American Economic Journal: Applied Economics 2019, 11(2): 37–73
https://doi.org/10.1257/app.20160350

Externalities and Taxation of Supplemental Insurance: A Study of Medicare and Medigap†

By Marika Cabral and Neale Mahoney*

Most health insurance uses cost-sharing to reduce excess utilization. Supplemental insurance can blunt the impact of this cost-sharing, increasing utilization and exerting a negative externality on the primary insurer. This paper estimates the effect of private Medigap supplemental insurance on public Medicare spending using Medigap premium discontinuities in local medical markets that span state boundaries. Using administrative data on the universe of Medicare beneficiaries, we estimate that Medigap increases an individual's Medicare spending by 22.2 percent. We calculate that a 15 percent tax on Medigap premiums generates savings of $12.9 billion annually with a standard error of $4.9 billion. (*JEL* G22, H24, H51, I13, J14)

Health insurance policies typically include cost-sharing, such as coinsurance, co-payments, and deductibles. By partially exposing beneficiaries to the marginal price of care, optimal cost-sharing strikes a balance between the risk-smoothing benefits of insurance and the excess utilization from moral hazard (Zeckhauser 1970). However, in many settings individuals can purchase supplemental insurance, reducing their exposure to this cost-sharing and potentially exerting a negative externality on the primary insurance provider.

A leading example of this phenomenon is the interaction between public Medicare insurance and private Medigap supplemental insurance. Most elderly Americans have health insurance through Medicare, which controls utilization with a deductible of approximately $1,000 for each hospital admission and coinsurance

* Cabral: University of Texas Austin, 1 University Station BRB 1.116, C3100 Austin, TX 78712, and NBER (email: marikacabral@gmail.com); Mahoney: Chicago Booth, 5807 S. Woodlawn Ave. Chicago, IL 60637, and NBER (email: neale.mahoney@gmail.com). Ilyana Kuziemko was coeditor for this article. We thank Jeff Clemens, Mark Duggan, Tom Davidoff, Liran Einav, Josh Gottlieb, Caroline Hoxby, Erin Johnson, Amanda Kowalski, Jonathan Levin, and David Powell for helpful comments. We also thank seminar participants at UC Berkeley, University of Chicago Junior Faculty Group, University of Chicago Harris School, University of Houston, UT Austin Applied Micro Lunch, Rice University, Stanford University, Texas A&M University, the 2013 RWJ Annual Meeting, the 2013 Austin-Bergen Labor Workshop, the 2013 National Tax Association Annual Conference, the 2013 Annual Health Economics Conference, and the 2013 and 2015 ASSA Meetings for their comments and suggestions. We thank Can Cui and Hanbin Yang for excellent research assistance. We are grateful to John Robst for sharing Medigap premium data and to Frances McCarty for help with the restricted access NHIS data. Cabral gratefully acknowledges financial support from a National Science Foundation Fellowship. Mahoney gratefully acknowledges financial support from a Kapnick Fellowship, Ric Weiland Fellowship, and the Robert Wood Johnson Foundation. All errors are our own.

† Go to https://doi.org/10.1257/app.20160350 to visit the article page for additional materials and author disclosure statement(s) or to comment in the online discussion forum.

of 20 percent for physician office visits.[1] In addition to these features, Medicare has no annual or lifetime out-of-pocket maximum, leaving beneficiaries exposed to substantial out-of-pocket risk. Although most private insurance prohibits the purchase of supplemental insurance, Medicare allows its beneficiaries to purchase private supplemental insurance called Medigap. This supplemental insurance covers essentially all of Medicare's cost-sharing, potentially leading to excess utilization and exerting a negative externality on Medicare.[2] Taxing the purchase of Medigap to account for this externality may be a promising avenue for controlling Medicare costs and increasing overall efficiency.

Researchers have long been aware that supplemental insurance may impose a fiscal externality on Medicare, and policymakers have issued a number of proposals to tax or regulate Medigap.[3] Yet despite this policy interest, considerable uncertainty remains about the effects of such a policy. Estimating the causal impact of Medigap is difficult because supplemental insurance coverage may be correlated with unobserved determinants of medical utilization. Previous studies, which have examined this relationship with regressions of medical spending on an indicator for Medigap coverage, admit that adverse or advantageous selection may bias the results.

This paper uses plausibly exogenous variation to estimate the externality that Medigap imposes on the Medicare system and to estimate how a corrective tax on Medigap would impact Medicare costs and welfare. Medicare costs, and thus the costs financed through supplemental Medigap insurance, exhibit considerable within-state variation due to geographic variation in factors ranging from household incomes to local physician practice styles to the supply of medical resources. Yet despite this local variation in the determinants of health care spending, within-state variation in Medigap premiums is very limited. This means that on opposite sides of state boundaries, otherwise identical individuals who belong to the same local medical market can face very different Medigap premiums, solely due to the costs of individuals elsewhere in their state.

An example is the Hospital Service Area (HSA) centered on Bennington, Vermont, which spans the border between southwest Vermont and upstate New York. On the Vermont side of the border, Medigap premiums are \$1,058 per year. On the New York side of the border, premiums are \$1,504 per year or about 40 percent higher. The reason for this premium difference is that New York state has New York City in the south, a region with substantially higher Medicare costs than the northern part of the state. It is the high-spending metropolitan south, combined with the limited within-state variation in premiums, that inflates Medigap premiums in upstate New York, creating a plausibly exogenous source of premium variation.

We isolate this variation by "zooming in" on HSAs that straddle state borders and instrumenting for premiums in these border-spanning HSAs with costs elsewhere

[1] All dollar values are inflation-adjusted to 2005 values using the CPI-U. The Part A deductible was \$912 in 2005 and has been raised by \$27 (nominal dollars) on average per year since 2000.

[2] Because Medicare pays for a large fraction of the care provided on the margin, if beneficiaries increase spending due to Medigap enrollment, then Medicare pays for a large fraction of this excess care.

[3] For example, President Barack Obama's 2013 budget proposed a 15 percent tax on Medigap premiums.

in the state. HSAs are defined by the Dartmouth Atlas as sets of adjacent ZIP codes in which residents receive most of their routine hospital care at the same facilities. HSAs are roughly the size of a county, and approximately 250 of the 3,436 HSAs cross state lines, accounting for 11 percent of the individuals in our sample. We isolate premium variation within border-spanning HSAs with a "leave-out costs" instrumental variable, which we define as the average uncovered Medicare spending for all Medicare beneficiaries outside an individual's HSA but within their state of residence; uncovered Medicare spending refers to the portion of Medicare-eligible spending that is the responsibility of the beneficiary and is paid either by the beneficiary or the beneficiary's supplemental insurer. Leave-out costs differ by at least \$64 per year in 50 percent of cross-border HSAs and by at least \$166 per year in 20 percent of the cross-border HSAs in our sample. Our first-stage regression of premiums on leave-out costs and HSA fixed effects is highly predictive with an R^2 ranging between 0.84 and 0.93 across specifications and a p-value on the instrument of less than 0.01.

We use this variation in premiums to estimate the price sensitivity of Medigap demand. Our preferred instrumental variable estimates indicate a demand elasticity of -1.5 to -1.8. These estimates are stable across alternative specifications and different approaches to measuring Medigap coverage in our data. Our empirical strategy also allows us to examine potential substitution into alternative forms of coverage, and we find no evidence of substitution into Medicare Advantage or Medicaid based on our variation in premiums.

Using administrative data on the universe of Medicare beneficiaries, we use this same instrumental variable strategy to examine the impact of Medigap on medical utilization and Medicare costs. Our estimates can be interpreted as local average treatment effects for individuals who are marginal to variation in premiums—presumably the same individuals who would respond to a tax on premiums. We find that Medigap increases Part B physician claims by 33.7 percent and Part A hospital stays by 23.9 percent. Summing across all categories of spending, we find that Medigap increases overall Medicare costs by \$1,396 per year on a base of \$6,291 or by 22.2 percent. This effect averages over individuals with higher spending due to moral hazard and any individuals with potentially lower spending due to increased use of preventative care (Chandra, Gruber, and McKnight 2010). We show that our results are robust to alternative specifications, and we conduct several falsification tests using individuals and procedures that should be unaffected by the variation in premiums.

We combine our demand and cost estimates to calculate the impact of taxing Medigap. Our estimates indicate that a 15 percent tax on Medigap premiums, with full pass-through, would decrease Medigap coverage by 13 percentage points on a base of 48 percent and reduce net government costs by 4.3 percent per Medicare beneficiary, with a standard error of 1.7 percentage points. About 35 percent of this savings would come from tax revenue while the remainder would come from lower Medigap enrollment. A tax equal to the full \$1,396 per year externality requires us to extrapolate outside the premium variation in the data. To a first-order approximation, our estimates indicate that such a tax would eliminate the Medigap market and decrease Medicare costs by 10.7 percent per beneficiary.[4] We conclude by discussing optimal Medigap taxation and welfare.

Our paper builds on an older literature that assesses the impact of Medigap with regressions of medical spending on an indicator for Medigap coverage, controlling for selection into Medigap with available covariates. The key challenge with this type of analysis is disentangling moral hazard from selection. Given this identification challenge, it is perhaps not surprising that prior studies have arrived at a wide range of estimates for the Medigap externality depending on the included set of controls, with estimates of the effect of Medigap on Medicare spending spanning the range of 10 percent to nearly 100 percent.[5] We contribute to this literature by using plausibly exogenous variation paired with comprehensive administrative data on Medicare spending, allowing us to overcome the classic identification concern and isolate the externality induced by Medigap.[6]

Our paper contributes to the literature in a number of ways. First, to the best of our knowledge, our paper is the first to estimate the fiscal externality from Medigap using a quasi-experimental source of variation. Second, by using premium variation to identify the effect of Medigap on Medicare spending, we are able to quantify the cost savings and welfare effects of taxing Medigap. Third, many public insurance programs throughout the world allow policyholders to purchase private supplemental insurance.[7] Thus, we think that our approach can be applied to studying how to reduce costs and increase surplus from public insurance in a broad range of settings.

The remainder of the paper proceeds as follows. Section I presents background on Medicare and Medigap and describes our data sources. Section II outlines our empirical strategy and Section III presents summary statistics and evidence on the

[4] While our estimates directly address the savings from taxing Medigap, broader reforms could potentially further increase efficiency. For example, Gruber (2013) suggests restructuring Medicare's cost-sharing in addition to levying a tax on supplemental insurance. A nuanced restructuring of Medicare's cost-sharing may aim to influence not only the level of medical spending, but also what individuals spend money on, encouraging the use of high-value care and discouraging the use of low-value care (Baicker, Chandra, and Skinner 2012; Baicker, Mullainathan, and Schwartzstein 2015). Using cost-sharing to curb moral hazard, Medicare could also use supply-side policies to limit the overuse of medical care (e.g., Einav, Finkelstein, and Mahoney 2017).

[5] Estimates of the Medigap externality range from Medigap increases Medicare spending by approximately 10 percent (Ettner 1997) to nearly 100 percent (GAO 2013), with several studies suggesting estimates between these extremes (e.g., Wolfe and Goddeeris 1991, Khandker and McCormack 1999, Hurd and McGarry 1997). While prior studies acknowledge the bias that selection has on these estimates, prior studies do not agree on the magnitude and direction of the bias due to selection. Lemieux, Chovan, and Heath (2008) argues that selection is probably adverse, leading these studies to overstate the impact of Medigap. Finkelstein (2004) finds evidence consistent with adverse selection in the Medigap market. Fang, Keane, and Silverman (2008) finds evidence of advantageous selection into Medigap, though this advantageous selection disappears once they condition on a wider set of covariates.

[6] Our paper is also related to Chandra, Gruber, and McKnight (2010), which studies the effects of a change in the generosity of the retiree supplemental insurance provided to California state employees through the CalPERS system. The authors' main finding is that CalPERS drug coverage can reduce hospitalizations among the chronically ill. These results do not have direct relevance to this setting because Medigap does not typically include drug coverage.

[7] In France, more than 92 percent of the population holds private supplemental insurance to protect against the substantial coinsurance payments (10 to 40 percent) of the universal public health insurance system. In Austria, about a third of the population has a supplemental private insurance plan that covers additional charges not covered under the basic health insurance benefits. About 30 percent of Belgians carry private supplemental health insurance policies. Approximately 30 percent of the population of Denmark purchases Voluntary Health Insurance (VHI) in order to cover the costs of statutory copayments of the universal health care coverage package. See Kaiser Permanente International (2008) and Tanner (2008) for more details. Parallel public and private coverage is also common in the setting of disability insurance. Leveraging variation in private disability insurance, Cabral and Cullen (2018) uses a revealed preference approach to analyze the welfare associated with public disability insurance.

Table 1—Medicare Cost-Sharing

Part A. Hospital expenditures			Part B. Physician expenditures		SNF	
	Per day copay					Per day copay
Deductible	Days 61–90	Days 91–150	Deductible	Coinsurance	Deductible	Days 21–100
$912	$228	$456	$110	20%	$0	$114

Notes: This table shows FFS Medicare cost-sharing for 2005. Part A cost-sharing is applied separately to each benefit period, which begins upon a hospital or Skilled Nursing Facility (SNF) admission and ends when the patient has been out of the hospital or SNF for 60 days. Medicare only pays for Part A hospitalizations in excess of 90 days through the drawdown of 60 lifetime reserve days. Part B cost-sharing is applied on an annual basis. SNF cost-sharing is applied separately in each benefit period, and Medicare provides no coverage for SNF stays longer than 100 days. Dollar values are inflation-adjusted to 2005 using the CPI-U.

validity of our identifying assumption. Section IV presents the main results and Section V examines the robustness of these results to a number of specification checks and placebo tests. Section VI presents policy counterfactuals. Section VII concludes.

I. Background and Data

This section provides background on Medicare and Medigap, and describes our data sources.

A. *Background*

Medicare beneficiaries can choose to receive coverage from publicly administered fee-for-service (FFS) Medicare or from a private Medicare Advantage plan.[8] FFS Medicare allows beneficiaries to choose their doctors and see a specialist without a referral. To control costs, FFS Medicare uses cost-sharing, partially exposing beneficiaries to the marginal cost of care. Medicare Advantage policies have premiums subsidized by Medicare. Relative to FFS Medicare, Medicare Advantage plans typically have more generous cost-sharing but place restrictions on provider choice. During our sample period, which runs from 1999 to 2005, 85 percent of Medicare beneficiaries selected FFS Medicare coverage, with the remaining 15 percent choosing coverage from private Medicare Advantage plans.

The details of FFS Medicare coverage for 2005 (the last year of our sample) are shown in Table 1. For hospital visits, which are covered by Part A of the Medicare program, beneficiaries face a deductible of nearly $1,000 and additional cost-sharing for long hospital stays. For physician expenditures, which are covered by Part B of the Medicare program, beneficiaries pay a small annual deductible and 20 percent coinsurance. A key feature of FFS Medicare is that there is no annual or lifetime out-of-pocket maximum, so individuals are exposed to significant financial risk. Figure 1 shows the distribution of annual uncovered Medicare spending, which is defined as Medicare-eligible spending for which the patient is responsible. The

[8] Throughout the paper, we refer to Medicare Part C as Medicare Advantage, even though it was called "Medicare + Choice" during the beginning of our sample period.

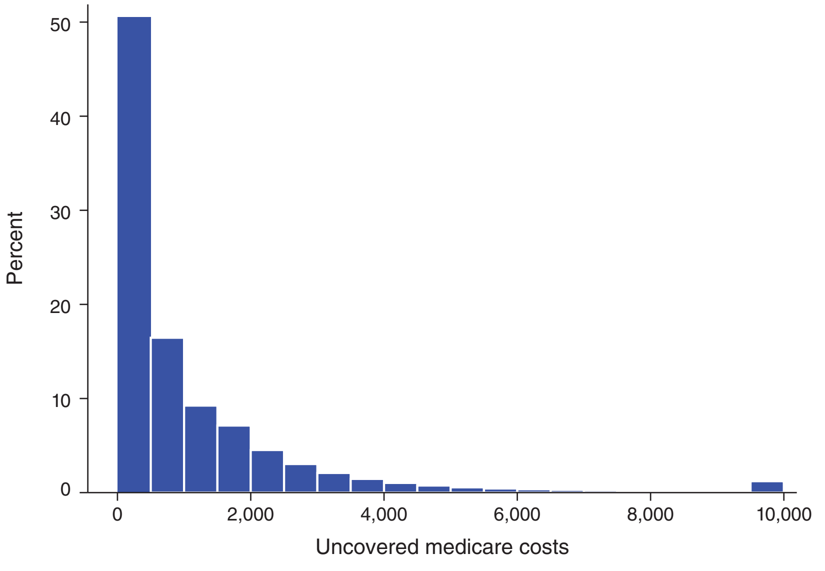

Figure 1. Annual Uncovered Medicare Costs

Notes: This figure shows a histogram of annual uncovered Medicare spending, defined as Medicare-eligible spending that is the responsibility of the beneficiary, and is thus paid out-of-pocket by the beneficiary, paid by supplemental insurance, or absorbed by the medical provider as bad debt. The figure is constructed using data from the 2005 CMS Beneficiary Summary File and covers the universe of aged, FFS Medicare, and non-Medicaid beneficiaries ($N = 22{,}196{,}098$). Uncovered Medicare costs are top-coded at \$10,000. Approximately 3.8 percent of beneficiaries have uncovered Medicare spending greater than \$5,000, and approximately 1 percent of beneficiaries have uncovered Medicare spending greater than \$10,000. Dollar values are inflation-adjusted to 2005 using the CPI-U.

mean uncovered spending is \$1,186 per year, and 3.8 percent of individuals in each year have uncovered expenditures in excess of \$5,000.

To protect against the financial risk, 86 percent of FFS Medicare beneficiaries carry supplemental insurance. Approximately 13 percent of FFS beneficiaries qualify for supplemental insurance at no cost through the government Medicaid program. Other beneficiaries may choose to purchase supplemental insurance offered by a former employer, and everyone has the option to purchase private Medigap coverage. Among FFS beneficiaries, 42 percent purchase Medigap coverage and approximately 40 percent purchase supplemental insurance through a former employer.[9]

The federal government regulates both the form of Medigap insurance and the purchase of Medigap policies. Individuals are restricted to choose from a standardized set of plans, all of which cover the same basic benefits.[10] These basic benefits include coverage of the Part A deductible, Part A copays, and Part B coinsurance.

[9] According to the MCBS estimates, approximately 10 percent of FFS beneficiaries carried both Medigap and Retiree Supplemental Insurance coverage during our sample period.

[10] There are three states in which the Medigap market is different. Massachusetts, Wisconsin, and Minnesota standardized their plans prior to federal regulation and have continued their own offerings. We exclude these three states from our analysis. The Medicare Prescription Drug, Improvement, and Modernization Act of 2003 introduced plans K and L and eliminated the sale of Medigap plans with drug benefits (H, I, and J). These changes took effect after our sample period.

Beyond the basic benefits, there is some variation across plans in the remaining coverage, though most of this variation is for less common expenses such as travel emergencies and home health care. Online Appendix A shows enrollment by plan and discusses Medigap plan characteristics in detail.

In this paper, we focus on the extensive margin of whether an individual has Medigap, rather than the effect of one plan compared to another, for two reasons. First, the basic benefits that are likely to have the greatest effect on the marginal price of care are common to all plans. Thus, the extensive margin is likely to be the primary driver of the marginal cost of care.[11] Second, our aim is to investigate the effect of a tax on Medigap policies. Because the Medigap tax proposals under consideration do not discriminate across plans, the extensive margin is more policy-relevant than substitution between Medigap plans.

In addition to regulating the form of Medigap policies, the federal government regulates the purchase of policies. Medigap beneficiaries typically purchase Medigap insurance within 6 months of turning 65 years old and signing up for FFS Medicare, during what is called the "open enrollment period." Medigap policies purchased during this open enrollment period are guaranteed renewable as long as Medigap enrollees pay plan premiums each year.[12] Individuals in this market typically sign up for a Medigap plan during their open enrollment period, and renew their policy each year.[13] During this open enrollment period, individuals cannot be legally denied coverage for any reason, and pricing is limited to a small set of characteristics (gender, location, and smoking status). In practice, premium variation is much more limited than what is legally allowed, and companies rarely vary premiums for a given plan within a state.[14] The beneficiary-weighted average annual premium of Medigap policies is $1,779, though the premium varies substantially across states.[15] In Section III, we discuss the Medigap premium variation in more detail.

Some individuals obtain supplemental coverage through a former employer. Unlike Medigap coverage during our sample period, Retiree Supplemental Insurance (RSI) policies typically covered prescription drugs and provided less generous coverage (or sometimes no coverage) of medical services. The average annual premium for an individual RSI policy in 2004 was $3,144, and retirees on average contributed approximately 39 percent or $1,212 of this premium. Unlike individual Medigap policies, RSI coverage is often available to both the retiree and his or her spouse for

[11] Although coverage for the Part B deductible is available only for some plans, since most beneficiaries spend more than the $110 Part B deductible, this coverage has little impact on the marginal cost of care.

[12] The federal government regulates how Medigap policy prices can evolve. In particular, when an individual enrolls in a Medigap plan, he is choosing an age-price profile that may be adjusted with medical inflation but may not be contingent on his current or future health status. Thus, along with the contemporaneous benefits, Medigap coverage provides insurance against reclassification risk in future periods. Since the evolution of premiums over time is set by federal standards, throughout the paper we focus on the premium charged to a 65-year-old during the open enrollment period.

[13] Medicare's website provides beneficiaries with information on selecting a Medigap policy and encourages beneficiaries to select a policy as if they will annually renew the policy, because dropping coverage would mean that they would face risk-rating should they wish to re-enroll. In the Medicare Current Beneficiary Survey (MCBS), 87 percent of individuals renew their Medigap plan across years.

[14] In practice, smoking status and gender are rarely priced, and although plans are legally allowed to vary prices at the ZIP code level, there tends to be very limited variation in company-plan level premiums within a state.

[15] The beneficiary-weighted premium is calculated using the baseline sample, as described in Table 2.

a higher premium contribution. This background information on RSI is drawn from KFF (2004).

B. *Data Sources*

We use data from several sources. The primary medical spending and utilization information comes from the Centers for Medicare and Medicaid Services (CMS) and covers the years 1999 through 2005. The CMS Denominator file contains data on the universe of Medicare enrollees, and includes information on sex, age, Medicaid status, Medicare Advantage enrollment, and ZIP code of residence. To investigate beneficiary-level spending and utilization, we combine the CMS Denominator file with the CMS Beneficiary Summary File, which covers the universe of FFS Medicare beneficiaries. The Beneficiary Summary File data contain information on health care spending (Medicare spending and beneficiary spending), utilization by category of care (e.g., hospitalizations, Part B claims), and chronic conditions.[16]

To further investigate which types of utilization are elastic to Medigap enrollment, we also examine Medicare claims data. Outpatient claims data are available in the CMS Carrier data file that contains outpatient claims for a 20 percent random sample of FFS Medicare beneficiaries. Inpatient claims data are available in the CMS MedPAR data file, which contains inpatient claims for 100 percent of FFS Medicare beneficiaries.

The CMS administrative data do not contain information on Medigap enrollment.[17] Thus, we must rely on survey data to estimate the demand for Medigap. To maximize statistical power, we combine estimates from two surveys: the Medicare Current Beneficiary Survey (MCBS) from 1992 to 2005 and the National Health Interview Survey (NHIS) from 1992 to 2005. Both surveys ask questions regarding supplemental insurance coverage among Medicare beneficiaries and contain similar demographic and health information. Online Appendix B describes how we construct the key variables from each survey.

Our premium data come from Weiss Ratings and contain Medigap premiums for policies purchased during the open-enrollment period for year 2000.[18] Prior work reveals that within-state premium variation in plan-level Medigap premiums is very limited (Robst 2006; Maestas, Schroeder, and Goldman 2009). In practice, firms do not tend to vary premiums across localities within a state, and firms rarely price gender or smoking status. For the analysis in this paper, we use premium data aggregated to the state-plan-firm level.

Our premium data is far from perfect. Individuals typically buy a Medigap plan during their one-time open enrollment period when they are first Medicare eligible, and then renew their plans at a guaranteed age-premium profile. Since we only have premium data for year 2000, we cannot match most individuals to the premium

[16] Data on spending, utilization, and chronic conditions are available only for FFS Medicare beneficiaries (no data are available for those on Medicare Advantage). Thus, it is key that we show that individuals do not switch to Medicare Advantage to be able to interpret our results.

[17] The lack of CMS data on Medigap is perhaps not surprising since Medigap enrollment does not affect Medicare's reimbursement formulas, so claims can be processed without this information.

[18] We thank John Robst for sharing these data.

menu they faced during their respective open enrollment periods. We view our year 2000 premium data as a noisy proxy for the relevant measure of premiums each individual faced when considering a Medigap plan. This measurement error motivates the instrumental variables strategy we describe in Section II.

As we describe in detail in Section II, the empirical strategy focuses on isolating variation in premiums within local medical markets that span state boundaries. Geographic crosswalks from the Dartmouth Atlas are used to match localities with their associated local medical markets. Our baseline definition of a local medical market is a Hospital Service Area (HSA). HSAs are defined by the Dartmouth Atlas as sets of adjacent ZIP codes in which residents receive most of their routine hospital care at the same facilities. HSAs are approximately the size of a county: there are 3,436 HSAs and 3,140 counties in the United States. However, unlike counties, HSAs often span state boundaries, reflecting the fact that local medical markets are not aligned with political boundaries. Using within-HSA variation provides us with a convenient way of ignoring state border areas where geographic barriers, or sharp differences in socioeconomic factors, lead to natural breaks in the providers from which medical care is received, allowing us to identify the effect of Medigap among those individuals who receive care from the same medical providers.

We combine these datasets with supplemental data from several other sources. ZIP code-level demographic and income data are obtained from the Census of Populations and Housing 2000 Special Tabulation on Aging (available through ICPSR). Additional ZIP code-level income data are obtained from the 2001 IRS aggregate income statistics. Medicare reimbursement rates vary geographically due to geographic adjustment factors. Although our research design focuses on individuals within local medical markets who (by definition) tend to use the same providers, our baseline analysis controls for Medicare geographic price adjustment factors (Part A OWI and Part B GAF) which are obtained from CMS.

II. Empirical Strategy

This section provides an overview of our empirical strategy and presents our estimating equations.

A. *Overview*

Our empirical approach is to use exogenous variation in Medigap premiums to identify the price sensitivity of the demand for Medigap and the fiscal externality of Medigap on Medicare costs. Medical costs exhibit considerable within-state variation due to factors ranging from household incomes to local physician practice styles to the supply of medical resources.[19] Yet despite this local variation, within-state premium variation is highly limited. Maestas, Schroeder, and Goldman (2009) show that, while firms are allowed to vary premiums at the ZIP code level, there is very little within-state variation in the Medigap premiums for a given plan offered by a

[19] See Cutler and Sheiner (1999); Cutler et al. (2013); Wennberg (1999); Wennberg, Fisher, and Skinner (2002); and MedPAC (2003), among others.

given insurance company. The authors cite state-level reporting requirements and regulations as a potential explanation.[20] Whatever the cause, the fact that premiums do not vary means that on opposite sides of state boundaries, otherwise identical individuals who belong to the same local medical markets can face very different premiums for Medigap, solely due to the costs of individuals elsewhere in their state. We isolate this variation by "zooming in" on HSAs that span state borders and instrumenting for premium variation in these border spanning HSAs with costs elsewhere in the state.

Figure 2 provides a concrete example of our empirical strategy. Panel A shows a map of per capita uncovered Medicare spending in New York and Vermont by HSA; panel B shows Medigap premiums in the same area. We define "uncovered Medicare spending" as the Medicare-eligible spending that is the responsibility of the beneficiary and is paid for either out-of-pocket or by a supplemental insurance plan. Two HSAs, centered on Bennington, Vermont and Cambridge, New York, straddle the New York-Vermont border. Each of these HSAs had average per capita uncovered Medicare spending around \$900, typical of the other HSAs in the upstate New York and Vermont area.[21] However, within these cross-border HSAs, there are sharp differences in Medigap premiums. Premiums on the New York side of the border are \$1,504 per year versus \$1,058 on the Vermont side.[22] The reason for this premium difference is that New York state has New York City in the south, a region with substantially higher Medicare costs than the northern part of the state.[23] It is the high-spending metropolitan south, combined with the limited within-state variation in premiums, that inflates Medigap premiums in upstate New York, creating a plausibly exogenous source of premium variation. Figure 3 shows the analogous data for the continental United States. Like the case of New York and Vermont, much of the premium variation within these cross-border HSAs is driven by within-state variation in costs outside the relevant HSAs.

As mentioned above, we isolate this variation by zooming in on cross-border HSAs and by instrumenting for premiums with the average uncovered costs of individuals who live within the state but outside of the HSA of interest. There are two reasons why we augment a "borders" approach with an instrumental variables strategy. First, on a given side of a border-spanning HSA, premiums are partially determined by the behavior of individuals on that side of the cross-border medical market. For example, if individuals on the New York side of the Bennington, Vermont HSA have higher average spending than those on the Vermont side, we would expect premiums to be mechanically higher in New York to account for this higher utilization, holding all else equal. Our "leave-out costs" instrument isolates

[20] This type of coarse pricing is not uncommon. Ericson and Starc (2015) shows that health insurance plans set premiums by five-year age bands on the Massachusetts health insurance exchange, even though plans were allowed to set prices that vary at the yearly level. Agarwal, et al. (2018) shows evidence of coarse pricing in consumer lending, and DellaVigna and Gentzkow (2017) shows evidence of coarse pricing in the retail sector.

[21] Per capita uncovered Medicare spending in 2000 was \$902 and \$927 in the Bennington HSA and Cambridge HSA, respectively.

[22] The average premium cited here is the average premium of all plans offered in the year 2000 by United Healthcare and Mutual of Omaha, the two largest Medigap insurers.

[23] The maximum HSA-level uncovered Medicare spending is \$1,585 in the south versus \$1,087 in upstate New York.

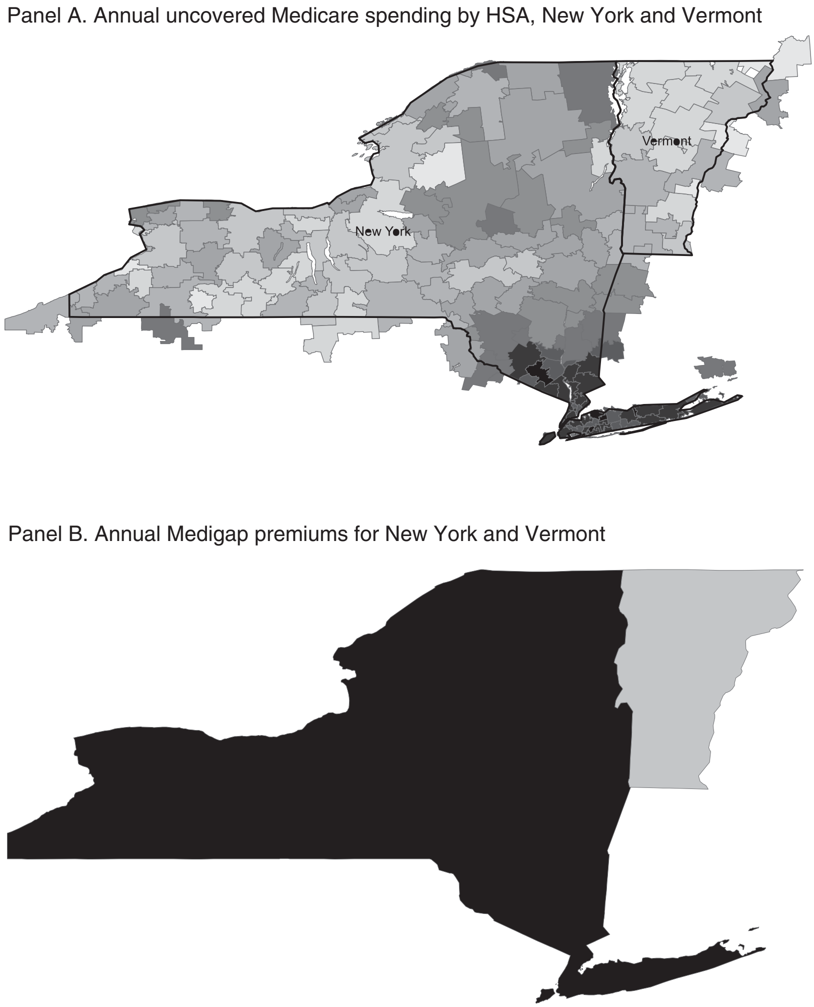

Figure 2. Annual Uncovered Medicare Expenditures and Medigap Premiums in New York and Vermont

Notes: Panel A displays average annual uncovered Medicare spending by HSA in New York and Vermont. Uncovered Medicare spending is defined as Medicare-eligible medical spending that is the responsibility of the beneficiary, and is thus paid out-of-pocket by the beneficiary, paid by supplemental insurance, or absorbed by the medical provider as bad debt. The map is based on data from the 2000 CMS Beneficiary Summary File for aged, FFS Medicare beneficiaries residing in New York and Vermont ($N = 1{,}415{,}957$). Uncovered Medicare spending ranges from \$766 per capita in the HSA centered on Lowville, New York (a village in upstate New York) to \$1,585 in the HSA centered on Far Rockway, New York (a neighborhood of New York City). Among HSAs within these two states, the fifth percentile of uncovered Medicare spending is \$843, the tenth percentile is \$847, the median is \$956, the ninetieth percentile is \$1,296, and the ninety-ninth percentile is \$1,404. Panel B displays annual average state-level Medigap premiums for the two largest insurers, United Healthcare and Mutual of Omaha, based on data from Weiss Ratings for 2000. The average annual Medigap premium is \$1,504 in New York and \$1,058 in Vermont. Dollar values are inflation-adjusted to 2005 using the CPI-U.

variation that is due to the fact that Medigap premiums on the New York side of the border are driven up by the high costs in New York City, hundreds of miles to the south. It is worth noting that in practice this endogeneity problem shrinks substantially as we narrow the focus of the analysis to those in very close proximity to the boundary who make up a very small fraction of any state.

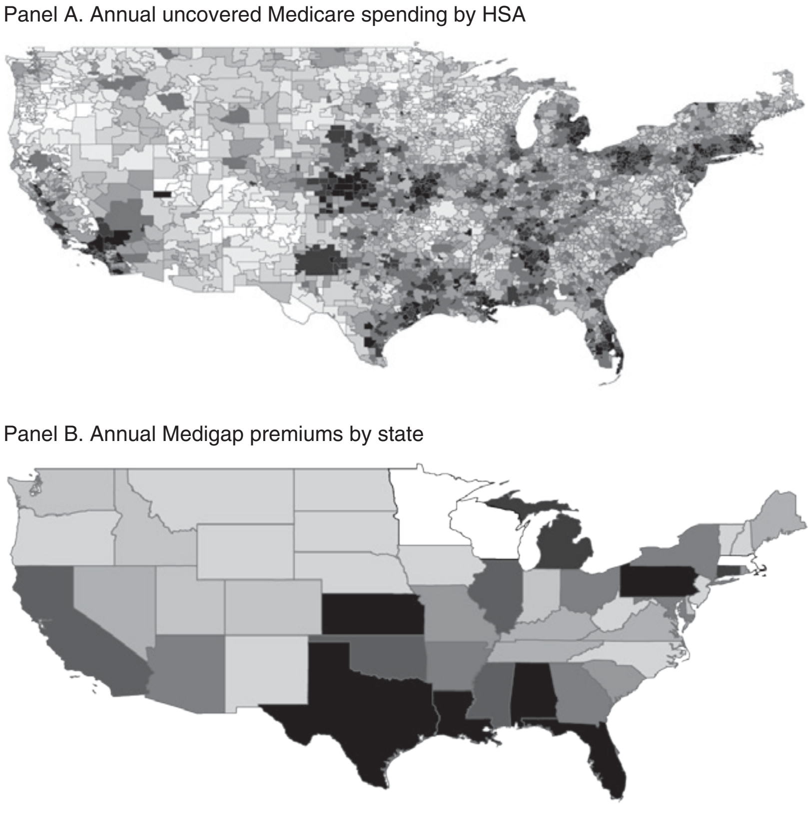

Figure 3. Annual Uncovered Medicare Spending and Medigap Premiums

Notes: Panel A displays average annual uncovered Medicare spending by HSA for the continental United States. Uncovered Medicare spending is defined as Medicare-eligible medical spending that is the responsibility of the beneficiary, and is thus paid out-of-pocket by the beneficiary, paid by supplemental insurance, or absorbed by the medical provider as bad debt. The map is based on data from the 2000 CMS Beneficiary Summary File for aged, FFS Medicare beneficiaries ($N = 20{,}492{,}806$). The fifth percentile of HSA-level uncovered Medicare spending is \$705, the tenth percentile is \$801, the median is \$944, the ninetieth percentile is \$1,131, and the ninety-ninth percentile is \$1,360. Panel B displays annual average state-level Medigap premiums for the two largest insurers, United Healthcare and Mutual of Omaha, based on data from Weiss Ratings for 2000. Premium data do not exist for Wisconsin, Massachusetts, and Minnesota, since these states do not have standardized Medigap policies. The average Medigap premium is \$1,456 and the median is \$1,448. The ninetieth percentile is \$1,772 and the tenth percentile is \$1,232. Dollar values are inflation-adjusted to 2005 using the CPI-U.

The second, and more important, reason for our instrumental variables strategy is that we do not observe the relevant measure of premiums each individual faced when considering a Medigap plan. As discussed in Section I, our premium data cover all Medigap plans offered during the 2000 open enrollment period. This introduces two sources of measurement error. First, we cannot match individuals perfectly to the premium menu they faced during their respective open enrollment periods (during the first six months they were Medicare eligible).[24] Second, collapsing an entire

[24] As discussed in Section IA, individuals typically buy a Medigap plan during their one-time, open-enrollment period when they are first Medicare eligible, at which point they can purchase a plan without facing medical underwriting. In subsequent years, individuals tend to renew their plans at a guaranteed age-premium profile. As

menu of premiums into a single premium measure would require strong, untestable assumptions on the underlying model of demand.[25] Indeed, we cannot even calculate market-share, weighted average premiums, which would arguably be the most natural aggregate premium proxy, as we do not observe enrollment for each plan.[26]

The instrumental variables approach allows us to overcome these limitations. The first stage relates the leave-out cost instrument (defined precisely below) to the available premium data, implying that the instrument is a powerful shifter of premiums across the full menu of Medigap plans available at a given point in time. We estimate the price sensitivity of the demand for Medigap as the ratio of the reduced form effect of leave-out costs on Medigap coverage and the first-stage effect on premiums. We estimate the effect of Medigap on costs (and utilization) as the ratio of the reduced form effect of our instrument on costs and the reduced form effect of our instrument on coverage.

Of course, our instrumental variables strategy requires that the leave-out cost instrument is uncorrelated with observed determinants of costs and utilization. In Section IIIB, we discuss the exclusion restriction in detail and present a number of pieces of evidence in support of our identifying assumption.

B. *Estimating Equations*

Let i denote individuals, j denote states, k denote HSAs, and l denote Medigap plans. Assume, to a first approximation, that Medigap premiums in a given state for a given plan are proportional to the uncovered Medicare spending of individuals within that state, $p_{jl} = \alpha_l E_{i \in I_j}[c_i^u]$, where c_i^u is the uncovered Medicare spending of individual i and the expectation is taken over I_j, the set of individuals in state j. For a HSA-state pair, we can decompose the determinants of premiums into the uncovered spending of individuals within and outside of the given HSA: $p_{jkl} = \alpha_l \Pr[i \in I_{j,k} | i \in I_j] \times E_{i \in I_{j,k}}[c_i^u] + \alpha_l \Pr[i \in I_{j,-k} | i \in I_j] \times E_{i \in I_{j,-k}}[c_i^u]$, where $I_{j,k}$ denotes the set of individuals in state j and HSA k. We define our leave-out cost instrument as the average uncovered Medicare spending of those who reside outside of the HSA but within the state of interest scaled by the fraction of the state's Medicare beneficiaries who make up this sample:

$$\textit{Leave-out costs}_{jk} = \Pr[i \in I_{j,-k} | i \in I_j] \times E_{i \in I_{j,-k}}[c_i^u]. \tag{1}$$

discussed in Section IB, our premium data cover plans offered during the 2000 open enrollment period. Thus, we cannot match most individuals to the premium menu they faced during their respective open enrollment periods.

[25] As we note above, we only have premium data for plans offered in the year 2000, and we do not have data on plan market share. Thus, it is not possible to estimate the OLS analogue of the instrumental variables specification using a market-share, weighted mean premium measure. In online Appendix Table C3, we report OLS demand estimates using some feasible premium measures.

[26] In settings where measurement error is the sole rationale for an instrument, parametric adjustment, to account for attenuation bias may be an alternative to an instrumental variables strategy provided that there is a reliable way to estimate the degree of measurement error. In our setting, such adjustments are infeasible, as we are unable to estimate the degree of measurement error. Further, such an adjustment would not help us with the mechanical correlation between utilization and premiums, which also motivates our instrumental variables strategy.

We estimate the first-stage effect using the following regression:

$$(2) \qquad p_{jkl} = \alpha_c Leave\text{-}out\ costs_{jk} + \alpha_k + X'_{jk}\alpha_X + \alpha_{0(l)} + \alpha_{1(l)} + \epsilon_{jkl},$$

where α_k is a vector of HSA fixed effects, X_{jk} are covariates, $\alpha_{0(l)}$ is a vector of Medigap insurer fixed effects, $\alpha_{1(l)}$ is a vector of Medigap plan letter fixed effects, and ϵ_{jkl} is the error term. Including HSA fixed effects implies that the coefficient on leave-out costs α_c is identified by variation in leave-out costs within border-spanning HSAs.

We estimate the reduced form effect on Medigap enrollment using individual-level survey data. Let q_{ijk} be an indicator that takes a value of one if the individual reports having Medigap and zero otherwise. The reduced form regression takes the form

$$(3) \qquad q_{ijk} = \beta_c Leave\text{-}out\ costs_{jk} + \beta_k + X'_{ijk}\beta_X + \nu_{ijk},$$

where β_k is a vector of HSA fixed effects, X_{ijk} are covariates, and ν_{ijk} is the error term. The implied instrumental variable impact on Medigap enrollment of an increase in premiums is given by the ratio of the reduced form and first-stage coefficients, β_c/α_c. We can explore the sensitivity of our results by using α_cs from alternative specifications of the first-stage regression.

We estimate the effect on Medicare costs (and utilization) using individual-level claims data. Let y_{ijk} be a measure of costs. The costs regression takes the form

$$(4) \qquad y_{ijk} = \gamma_c Leave\text{-}out\ costs_{jk} + \gamma_k + X'_{ijk}\gamma_X + \mu_{ijk},$$

where γ_k are HSA fixed effects, X_{ijk} are covariates, and μ_{ijk} is the error term. As mentioned above, the effect of Medigap on costs is given by the ratio of the reduced-form costs and enrollment effects, γ_c/β_c. The effect on costs of an increase in premiums is given by the effect on Medigap coverage of an increase in premiums (β_c/α_c) multiplied by the effect on utilization of an increase in coverage (γ_c/β_c). This simplifies to γ_c/α_c and implies that our estimate of the effect of a premium increase on utilization is invariant to our estimate of the demand elasticity. To account for the fact that determinants of medical care may be related within local medical markets, we calculate robust standard errors clustered at the HSA level in each stage of the estimation. We also examine sensitivity to different levels of clustering in online Appendix Table H1, including clustering at the HSA $\times$ state level and multiway clustering on HSAs and states.

III. Summary Statistics and Identifying Variation

A. *Summary Statistics*

Table 2 presents summary statistics. Panel A shows summary statistics for all Medicare beneficiaries continuously enrolled within a calendar year. Panel B shows statistics for our baseline sample, defined as the universe of FFS Medicare beneficiaries continuously enrolled within a calendar year, excluding those who are

Table 2—Summary Statistics

	All HSAs	Cross-Border HSAs (11.0%)
Panel A. All beneficiaries		
Medicare type (denominator file, 1999–2005)		
Traditional Medicare (FFS), Non-Medicaid	73.6%	82.7%
Medicare advantage	15.3%	6.7%
Medicaid (dual-eligible)	11.1%	10.6%
Panel B. Baseline sample: FFS Medicare, Non-Medicaid beneficiaries		
Supplemental insurance* (MCBS, 1992–2005)		
Medigap	47.9%	50.0%
Retiree supplemental insurance	46.3%	45.9%
None	15.8%	14.1%
Annual Medigap premiums (Weiss ratings, 2000)	$1,779	$1,727
Annual costs (beneficiary summary file, 1999–2005)		
Part A payments	$3,021	$2,776
Part B payments	$2,648	$2,395
SNF payments	$399	$337
Total Medicare payments	$6,291	$5,760
Annual utilization (beneficiary summary file, 1999–2005; carrier claims file, 1999–2005)		
Part A days	2.10	2.06
Part A stays	0.34	0.34
Part B events	25.81	24.01
Part B RVUs	70.77	64.86
SNF days	1.37	1.25
SNF stays	0.06	0.06
Demographics (denominator file, 1999–2005)		
Sex		
Male	41.6%	41.6%
Race		
White	92.2%	92.6%
Black	5.6%	5.8%
Other	2.1%	1.7%
Age		
65–74	50.1%	51.7%
75–84	37.4%	36.7%
85+	12.5%	11.7%

Notes: Panel A displays the type of insurance coverage. The data source is the pooled 1999–2005 CMS Denominator File and the sample is restricted to individuals who are enrolled for the entire year and meet the geographic restrictions described in Section II ($N = 222{,}390{,}439$). Panel B displays summary statistics for the baseline sample of FFS Medicare, non-Medicaid beneficiaries. In addition to the panel A sample restrictions, the sample excludes beneficiaries who also have coverage from Medicaid (dual-eligibles), beneficiaries who originally qualified for Medicare through SSDI, and beneficiaries with Medicare Advantage coverage. The utilization and payment information comes from the pooled 1999–2005 CMS Beneficiary Summary File ($N = 130{,}895{,}953$) for all variables except the Part B RVU variable, which comes from the pooled 1999–2005 CMS Carrier Claims File ($N = 23{,}708{,}295$). Demographics are based on the 1999–2005 CMS Medicare Denominator File ($N = 130{,}895{,}953$) and the insurance coverage variables come from the Medicare Current Beneficiary Survey ($N = 86{,}229$). The Medigap premium information comes from the Weiss Ratings, and the premium measure is the average premium for all plans offered by United Healthcare and Mutual of Omaha, the two largest insurers, during the 2000 open enrollment period. Dollar values are inflation-adjusted to 2005 using the CPI-U.

*Percentages add up to more than 100 percent because some individuals report holding both RSI and Medigap coverage.

simultaneously enrolled in Medicaid (known as dual-eligibles) and those who qualify for Medicare before age 65 due to disability. We restrict the sample to FFS beneficiaries since we do not observe costs or utilization for individuals with Medicare Advantage coverage. We drop dual-eligibles because they received supplemental insurance through Medicaid, and we drop non-elderly Medicare beneficiaries qualifying through Social Security Disability Insurance (SSDI) because they are in a different risk pool for Medigap insurance.[27,28]

The first column of Table 2 displays summary statistics for all HSAs, including border-spanning and non-border-spanning HSAs. Among all beneficiaries (panel A), 73.6 percent have coverage from FFS Medicare without Medicaid, 15.3 percent have coverage from a Medicare Advantage plan, and 11.1 percent are dual-eligibles with coverage from both Medicare and Medicaid. Within the baseline sample of FFS non-Medicaid beneficiaries (panel B), 47.9 percent hold a Medigap policy, 46.3 percent hold an RSI policy, and 15.8 percent have no supplemental coverage. These numbers sum to greater than 100 percent because some individuals report having both Medigap and RSI coverage. Medigap premiums have a mean value of \$1,779 per year. Within the baseline sample, total Medicare payments average \$6,291, and approximately 56 percent of payments are for inpatient care. On average, Medicare beneficiaries spend two days in a hospital annually and have 26 Part B events, where an event is defined as a line item claim.

The second column of Table 2 presents the same summary statistics for the 11 percent of beneficiaries who reside in HSAs that span state boundaries. This sample is of particular interest as variation in our instrument among these individuals identifies the demand and utilization elasticities. In the cross-border sample, individuals are 9 percentage points more likely to have FFS Medicare without Medicaid and are about 9 percentage points less likely to have Medicare Advantage. This is because the border-spanning sample is more rural and Medicare Advantage penetration was lower in rural areas during our time period. The cross-border sample is very similar in terms of the percentage of dual eligibles, enrollment in supplemental insurance, and demographics (age, sex, and race). The cross-border sample has slightly lower Medigap premiums, Part A days, and Part B events. Taken together, these statistics indicate that the border-spanning sample is broadly similar to the sample of all HSAs.

Our main regression analysis focuses on the baseline sample. While the natural sample of interest, our estimates would be biased if selection into the baseline sample is correlated with our identifying variation.[29,30] We examine this threat to validity with regressions of ZIP code-level measures of coverage type on our instrument and HSA fixed effects. Specifically, letting y_{zjk} indicate the percentage of individuals with a given coverage type in ZIP code z, HSA j, and state k, we run regressions of the form

$$y_{zjk} = \delta_c Leave\text{-}out\ costs_{jk} + \delta_k + \nu_{zjk}, \tag{5}$$

[27] Because the premium data we have are for Medigap plans available to elderly Medicare beneficiaries, our identification strategy and the instrument are inappropriate for this sample.

[28] In both the samples described in panels A and B, we make a few geographic exclusions. We exclude the District of Columbia from our analysis because more than 99 percent of the individuals in this region belong to a single HSA. We also exclude beneficiaries from the three states that do not have standardized Medigap products (Wisconsin, Massachusetts, and Minnesota). Lastly, we exclude a small number of HSA-states where the remainder of the state accounts for less than 80 percent of the state Medicare population.

[29] As discussed in Section I, data on spending and utilization are available for the universe of Medicare FFS beneficiaries; analogous data on utilization and spending are not available for Medicare Advantage beneficiaries.

[30] As explained by Slemrod and Yitzhaki (2001) and Hendren (2016), from an optimal policy perspective, whether an individual received coverage from FFS Medicare or Medicare Advantage only matters if the type of coverage imposes a fiscal externality on the government.

Table 3—Identifying Variation: Insurance Status and Individual Characteristics

	Coefficient on leave-out cost (hundreds)			
Dependent variable	Est.	SE	p-value	Mean of dep. var.
Panel A. Identifying variation and insurance status				
Medicare administrative data				
All beneficiaries				
Part B coverage	0.001	(0.001)	0.54	0.92
Original medicare eligibility through SSDI	0.002	(0.003)	0.39	0.07
Medicare advantage	−0.003	(0.006)	0.62	0.15
Medicaid (dual-eligibles)	0.008	(0.005)	0.15	0.11
Panel B. Identifying variation and individual characteristics				
Census 2000, special tabulation of elderly population				
High school degree, 65+	−0.015	(0.013)	0.26	0.65
Bachelors, 65+	−0.012	(0.011)	0.28	0.15
Veteran, male 65+	−0.014	(0.010)	0.15	0.65
Veteran, female 65+	−0.001	(0.001)	0.26	0.02
Labor force participation, female 65–69	−0.001	(0.006)	0.84	0.20
Labor force participation, male 65–69	−0.004	(0.011)	0.71	0.30
Income < 100% FPL, age 65+	−0.001	(0.005)	0.85	0.10
log median income, age 65–74	−0.017	(0.030)	0.56	10.35
log median income, age 75+	−0.020	(0.027)	0.47	10.02
log mean house value	−0.033	(0.045)	0.46	11.78
Renters, age 65+	−0.018	(0.007)	0.01	0.22
Move homes, 65–74	−0.009	(0.009)	0.33	0.29
Move homes, 55–64	−0.007	(0.011)	0.52	0.38
IRS aggregate income statistics				
Mean adjusted gross income (AGI)	600.2	(2,672.1)	0.82	46,012.30
AGI < \$10,000	0.014	(0.012)	0.27	0.23
\$10,000 < AGI < \$25,000	0.000	(0.011)	0.98	0.29
\$25,000 < AGI < \$50,000	0.012	(0.014)	0.39	0.28
AGI > \$50,000	0.021	(0.015)	0.15	0.29
Medicare administrative data, baseline sample				
Predicted annual medicare spending	35.3	(34.5)	0.31	6,291

Notes: This table shows estimates from regressions of outcome variables on leave-out costs and HSA fixed effects (see Section III, equation (5)). Panel A is based on the pooled 1999–2005 CMS Denominator File with the sample restrictions described in panel A of Table 2. The first section of panel B is based on data from the 2000 Census Special Tabulation on Aging (available from ICPSR). The second section of panel B is based on the IRS Aggregate Income Statistics for 2001. For these regressions, the data are aggregated to the ZIP code level and the observations are weighted by the Medicare population residing in those ZIP codes. In the final row of panel B, the dependent variable is predicted Medicare spending, based on a linear regression of individual-level Medicare payments on the census demographics listed in the table and controls used in our baseline specifications (age, sex, race, year, and health risk). The fitted value is then regressed on the leave-out costs instrument and HSA fixed effects using the baseline sample described in Table 2, panel B. Dollar values are inflation-adjusted to 2005 using the CPI-U.

where δ_k are HSA fixed effects and ν_{zjk} is the error term.

Panel A of Table 3 shows the results of these regressions. The dependent variables are the Part B coverage rate, the fraction of beneficiaries originally qualifying for Medicare through SSDI, the fraction of beneficiaries covered by Medicare Advantage (MA), and the fraction of beneficiaries dually eligible for both Medicare and Medicaid. Each of these measures is constructed using data on the universe of

Medicare beneficiaries from the CMS Denominator file. The results reveal that none of these measures are related to our identifying variation.[31]

In addition to addressing concerns over sample selection bias, the results indicate that our identifying variation does not induce substitution between Medigap and Medicare Advantage or Medicaid. This is not surprising given the institutional setting. Medicaid provides supplemental insurance, of similar generosity as Medigap, to poor beneficiaries for no premium, so it would be strange if variation in Medigap premiums had an impact on Medicaid coverage. During the time period we analyze, Medicare Advantage plans were typically organized as Health Maintenance Organizations (HMOs). The lack of substitution into Medicare Advantage is consistent with other evidence on limited substitution between HMO and FFS insurance plans in the non-elderly context (e.g., Bundorf, Levin, and Mahoney 2012).

B. *Identifying Variation*

Having defined our baseline sample, we next examine our identifying variation. Figure 4 plots a histogram of the leave-out costs instrument in cross-border HSAs net of the mean of the instrument within each HSA. The instrument is constructed using data on the baseline sample from the 2000 CMS Beneficiary Summary File.[32] Leave-out costs exhibit substantial dispersion, with an interquartile range of \$64 and a 90-10 percentile range of \$166. This implies a jump of at least \$64 in 50 percent of the cross-border regions, or 7.2 percent of the mean leave-out cost value in cross-border HSAs of \$886. In 20 percent of the regions, there is a jump of at least \$166 or 18.7 percent of the mean.

The identification assumption is that the within-HSA variation in leave-out costs (i.e., uncovered Medicare spending of individuals within the state but outside of the border-spanning HSA) affects the dependent variable of interest (e.g., Medigap enrollment, medical utilization) only through Medigap premiums. Although we cannot test this assumption directly, we provide several pieces of empirical evidence that support the identifying assumption. In this section, we show that the instrument does not covary with individual and local characteristics (potential omitted variables) within cross-border HSAs. In Section V, we further examine the robustness of our results by examining the stability of the estimates when we control for potential

[31] It is important to note that the MA enrollment point estimate is small in terms of magnitude. The point estimate indicates that a \$100 increase in annual Medigap premiums is associated with a 0.3 percentage point reduction in Medicare Advantage coverage. To put this magnitude in context, consider the MA-FFS cost difference required to explain the entire Medigap effect through selection. Specifically, let C_0 represent the mean costs on FFS Medicare initially, and C_1 represent the mean costs on FFS Medicare after the premium increase. Let N_0 be the fraction of total beneficiaries on FFS Medicare initially, let N_S be the fraction of total beneficiaries switching to FFS Medicare from MA after the premium increase, and let $N_1 (= N_0 + N_S)$ be the fraction of beneficiaries on FFS Medicare after the premium increase. Let C_S represent the average cost on FFS Medicare for those individuals who switch coverage from MA to FFS after the premium increase. If there is no Medigap effect, then we can express the mean FFS costs after the premium increase as $C_1 = C_0 \frac{N_0}{N_1} + C_s \frac{N_S}{N_1}$. We can calculate the implied value of C_S that makes this expression hold using the mean values of the variables in our data, along with our regression estimates ($C_0 = 6{,}291$; $C_1 = 6{,}291 - 67$; $N_0 = 0.85$; $N_S = 0.003$; $N_1 = 0.853$). This calculation yields that $C_S = -12{,}760$. That is, switchers would need to have mean claim costs well below zero (which is obviously not possible) for selection to explain our entire effect. The intuition is that the effect on FFS costs is large relative to the effect on MA enrollment, so an implausibly large selection effect would be required to explain the result.

[32] We use Beneficiary Summary File data from 2000 because our premium data are also from this year.

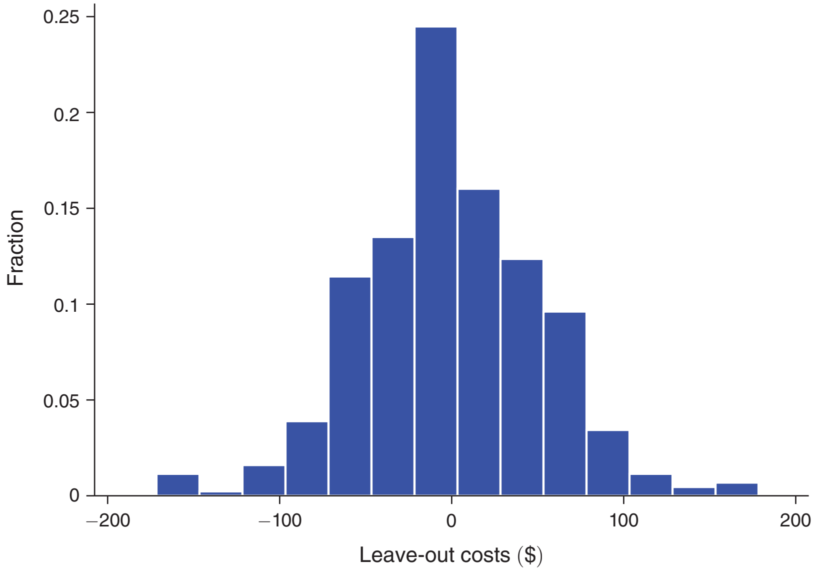

Figure 4. Leave-Out Costs

Notes: This figure shows a histogram of the leave-out costs instrument net of mean leave-out costs within the 437 border-spanning HSAs. The leave-out costs instrument is defined using data from the 2000 CMS Beneficiary Summary File ($N = 20{,}492{,}806$). See Section II for more details. Dollar values are inflation-adjusted to 2005 using the CPI-U.

confounding factors and conducting falsification tests on outcomes and individuals that should not be affected by our source of variation.

Finally, while we think our variation is valid, it is worth pointing out that the most likely threat to validity would bias us against our bottom-line finding that Medigap increases Medicare spending. To see this, suppose that demand for medical care is correlated within a state even after controlling for local demand with HSA fixed effects. If this were the case, then this residual demand effect would generate a positive correlation between leave-out costs and Medicare spending. Our main result is that higher leave-out costs, by raising the premium and reducing enrollment in Medigap, generate a negative correlation with Medicare spending. Thus, any bias is likely to attenuate our estimates of the effect of Medigap and work against our main finding that Medigap imposes a fiscal externality on the government.

To examine whether our instrument is correlated with individual and local characteristics (potential omitted variables) in cross-border HSAs, we run versions of equation (5) with individual and local characteristics as the dependent variables. Panel B of Table 3 shows the results of these regressions. The top section examines the correlation with ZIP code-level characteristics in the Census 2000 Special Tabulation on Aging. Nearly all of the ZIP code-level census demographics have a statistically insignificant relationship with the leave-out costs instrument.[33] The

[33] Although there is one exception (the coefficient on Renters among those 65+), the reported standard errors are not corrected for multiple hypothesis testing, and if we were to do so, many corrections would lead us to conclude that we could not reject the hypothesis that all the coefficients are statistically indistinguishable from zero.

second section of panel B examines the correlation with ZIP code-level IRS 2001 statistics on adjusted gross income. The IRS income statistics have both strengths and weaknesses relative to the Census Special Tabulation income measures for our purposes. While the IRS data likely have less measurement error, the Census Special Tabulation data focus specifically on the elderly population, our population of interest, rather than all households. Like the census measures, the IRS measures show no correlation.

A limitation of these regressions is that the magnitude of the estimates does not have a natural interpretation. To address this limitation, we estimate a specification that aggregates across these different dependent variables based upon their importance in predicting Medicare spending. Specifically, we construct a measure of predicted Medicare spending based on linear regression of individual-level annual Medicare spending on the census demographic variables and the controls in our baseline sample. Then we examine whether there is a within-HSA correlation between this predicted annual Medicare spending measure and our instrument using equation (5). As shown in the bottom row of Table 3, the estimated coefficient from this exercise is statistically indistinguishable from zero with a p-value of 0.31. Overall, this evidence suggests that observables plausibly related to medical spending are unrelated to the identifying variation.

IV. Results

This section presents the baseline estimates. We start by showing that the leave-out costs instrument is a powerful predictor of premiums. We then use variation in leave-out costs to estimate the demand for Medigap and the effect of Medigap on Medicare utilization and spending.

A. *Premiums*

Table 4 presents estimates of the first-stage regression of annual Medigap premiums on the leave-out costs instrument, HSA fixed effects, and controls (see Section II, equation (2)). The first column displays results for a plan-level specification that includes all plans offered by United Healthcare and Mutual of Omaha, the two largest insurance companies with a combined market share of 69 percent.[34] The second and third columns of Table 4 examine the sensitivity of our estimates by restricting attention to Plan C and Plan F, the most popular plans sold by these insurance companies. The coefficient on the instrument ranges from 1.12 to 0.94 across specifications, indicating that the instrument shifts premiums on an approximately one-for-one basis. The coefficient on the instrument is precisely estimated with p-values of less than 0.01 across the specifications. The specifications explain

[34] This number is taken from Starc (2014), which summarizes data from the National Association of Insurance Commissioners. We do not have plan-level enrollment, so we cannot construct an enrollment-weighted measure of premiums. Using an unweighted measure places excess weight on plans with low enrollment shares but does not materially impact our results.

Table 4—Regressions of Annual Medigap Premiums on Leave-Out Costs

	Dep. variable: annual Medigap premiums		
	Plans A–J (1)	Plan C (2)	Plan F (3)
Leave-out costs	1.118 (0.111)	0.937 (0.153)	0.974 (0.155)
HSA FE	X	X	X
Insurer FE	X	X	X
Plan FE	X		
R^2	0.926	0.841	0.877
Mean of dependent variable	1,536	1,429	1,475
Observations	45,129	6,298	6,449

Notes: This table shows estimates from regressions of annual Medigap premiums on the leave-out costs instrument, HSA fixed effects, plan fixed effects, insurer fixed effects, and controls for GAF/OWI adjustment factors (see Section II, equation (2)). The first column displays results from a specification that includes all plans offered by United Healthcare and Mutual of Omaha, the two largest insurers. The second and third columns restrict attention to the most popular plans offered by these companies, Plan C and Plan F, respectively. Observations are at the HSA-state-plan-company level. Standard errors are clustered at the HSA level. Dollar values are inflation-adjusted to 2005 using the CPI-U.

much of the premium variation within cross-border HSAs, with the R^2 ranging from 0.84 to 0.93.

Panel A of Figure 5 depicts this relationship using a scatter plot. The vertical axis displays the residuals from a regression of premiums for all plans sold by the top two insurers on HSA fixed effects and the same controls as the regression described above. The horizontal axis displays the residuals from a regression of leave-out costs on HSA fixed effects and the same controls. Each point shows the mean values for an HSA-state. The axes are rescaled by adding the means of the vertical and horizontal axis variables to ease interpretation. The plot confirms the strong relationship between premiums and leave-out costs.

B. *Demand*

We estimate the demand for Medigap with regressions of coverage indicators on the leave-out costs instrument, HSA fixed effects, and controls (see Section II, equation (3)). We use data from two surveys: the 1992 to 2005 Medicare Current Beneficiary Survey (MCBS) and the 1992 to 2005 National Health Interview Survey (NHIS). The MCBS sample contains 114,561 observations and the NHIS sample contains 121,009 observations.

Our ability to precisely measure Medigap coverage varies across the datasets. In the MCBS, we have a relatively accurate measure of Medigap coverage, and we use this measure as an outcome variable. In contrast, the NHIS survey questions make it more difficult to distinguish Medigap from other forms of supplemental insurance.[35] We therefore estimate the effect in the NHIS using a broader measure

[35] The MCBS survey asks several questions regarding the source of coverage that we can use to cross-validate responses. In addition, the MCBS makes some effort to check Medicare Advantage and Medicaid enrollment

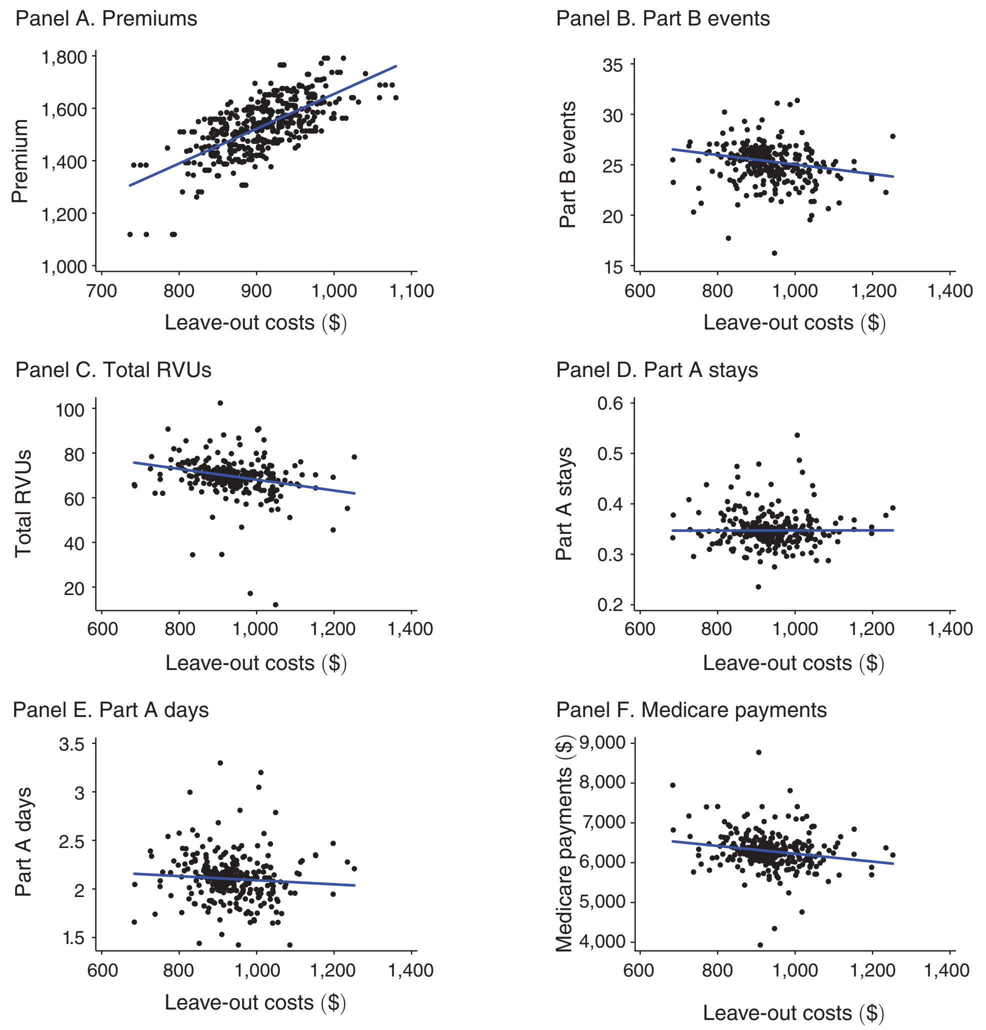

Figure 5. Outcomes versus Leave-Out Costs Instrument

Notes: This figure displays scatter plots of key outcome variables against the leave-out costs instrument. The vertical axis displays the residuals from a regression of the outcome variable on HSA fixed effects and the controls from the baseline specification. The horizontal axis displays the residuals from a regression of leave-out costs on HSA fixed effects and the same controls. Each point shows the mean value for an HSA × state. The axes are rescaled by adding the means of the vertical and horizontal axis variables. Panel A uses data on annual premiums for plans sold by the two largest insurers in the year 2000 (and is analogous to column 1 of Table 4). See Table 4 for more on the premium specification, Table 6 for more on the utilization specifications, and Table 7 for more on the Medicare payments specification. Dollar values are inflation-adjusted to 2005 using the CPI-U.

of supplemental insurance that captures whether the individual has any supplemental insurance, including Medigap but also Medicare Advantage, Medicaid, and RSI. Because our results using the administrative data indicate that the identifying

against administrative records. In contrast, the NHIS contains very few questions regarding sources of coverage, and responses are not checked against administrative records.

variation does not cause substitution into Medicare Advantage or Medicaid, we estimate our demand specification using the "All beneficiaries" sample described in Table 2, panel A, and we interpret the effect on the broad measure as reflecting the response of Medigap coverage to leave-out costs.[36]

In both surveys, our estimates are identified by cross-border HSAs in which we observe individuals on both sides of the state border. Of the 259 total cross-border HSAs, we observe individuals on both sides of state borders in 27 HSAs in the MCBS and 37 HSAs in the NHIS. This means that the HSA-level estimates are identified using 2,903 of the 114,561 observations in the MCBS and 5,690 of the 121,009 observations in the NHIS. To increase the precision of our estimates, we also estimate the same specifications using a more aggregate definition of local medical markets called a Hospital Referral Region (HRR). The Dartmouth Atlas defines an HRR as the set of adjacent ZIP codes in which individuals use the same hospitals for major medical care (such as cardiovascular surgery). While there are 3,436 HSAs across the nation, there are only 306 HRRs. Of the 140 total cross-border HRRs, we have observations on opposite sides of state borders in 66 HRRs in the MCBS and 70 HRRs in the NHIS. In these HRR-level specifications, the estimates are identified by 32,915 of the 114,561 observations in the MCBS and 39,060 of the 121,009 observations in the NHIS.[37]

Table 5 presents the results of these regressions. The estimates in the MCBS indicate that a \$100 increase in leave-out costs reduces Medigap demand by 6.6 to 9.0 percentage points. The estimates are similar whether we use variation at the HSA or HRR level and whether we measure Medigap coverage using the narrow Medigap coverage variable or the broader measure of supplemental insurance coverage. In the NHIS, where we only have the broader measure of Medigap coverage, we find that a \$100 increase in leave-out costs lowers Medigap coverage by 1.0 to 3.1 percentage points depending on whether we use variation at the HSA or HRR level.[38]

Our preferred estimates combine the point estimates from the MCBS and the NHIS using the Delta Method to construct the appropriate standard errors.[39] These estimates indicate that a \$100 increase in leave-out costs reduces our broad measure of Medigap by 4.0 to 4.8 percentage points. The HSA-level estimate is statistically distinct from zero with a p-value of 0.04, and the HRR-level estimate is statistically distinguishable from zero with a p-value of 0.01. Since the Medigap market-share is

[36] The prior literature has traditionally assumed there is no substitution between Medigap and RSI, and the results presented in Table 5 are consistent with no substitution into RSI based on our variation.

[37] We normalize the HRR-level demand coefficients in Table 5 by the HRR-level first-stage effect so that estimates are comparable with the HSA-level coefficients. See Table 5 for details.

[38] Online Appendix C illustrates that the demand estimates are robust to the inclusion of fewer or more controls than in these baseline specifications. The baseline specifications in Table 5 include year fixed effects, local medical market fixed effects, basic demographic controls, and controls for geographic price indexes (GAF and OWI).

[39] Let β_i, se_i, and n_i denote the point estimate, standard error, and sample size in dataset i. The combined point estimate is constructed as the sample-size weighted average of the point estimates in the two samples:

$$\beta_{Combined} = \frac{n_{MCBS}\beta_{MCBS} + n_{NHIS}\beta_{NHIS}}{n_{MCBS} + n_{NHIS}}.$$

Using the Delta Method and assuming that the point estimates are uncorrelated, the standard error of the combined estimate is given by

$$se_{Combined} = \frac{\sqrt{n_{MCBS}^2 se_{MCBS}^2 + n_{NHIS}^2 se_{NHIS}^2}}{n_{MCBS} + n_{NHIS}}.$$

Table 5—Regressions of Insurance Coverage on Leave-Out Costs

	Leave-out costs (hundreds)			Mean of dep. var.
	Est.	SE	p-value	
MCBS Alone ($N = 114{,}561$)				
Supplemental coverage (HSA level)	−0.066	(0.038)	0.08	0.90
Supplemental coverage (HRR level)	−0.071	(0.028)	0.01	0.90
Medigap (HSA level)	−0.083	(0.060)	0.17	0.36
Medigap (HRR level)	−0.090	(0.049)	0.07	0.36
NHIS alone ($N = 121{,}009$)				
Supplemental coverage (HSA level)	−0.031	(0.027)	0.26	0.79
Supplemental coverage (HRR level)	−0.010	(0.016)	0.51	0.79
Combined MCBS + NHIS				
Supplemental coverage (HSA level)	−0.048	(0.023)	0.04	0.85
Supplemental coverage (HRR level)	−0.040	(0.016)	0.01	0.85

Notes: This table shows estimates from regressions of insurance coverage indicators on leave-out costs, HSA or HRR fixed effects, and controls for age, sex, and GAF/OWI adjustment factors (see Section II, equation (3)). The analysis uses the MCBS and NHIS data from 1992 to 2005, using a sample definition analogous to panel A of Table 2. The dependent variable in the Supplemental Coverage specifications is an indicator for Medigap, Medicare Advantage, Medicaid, or RSI coverage. The HRR-level first-stage ranges from 0.24 to 0.25 across specifications (Appendix Table C2) and we scale the HRR demand estimates by 4 to make them comparable to the HSA-level estimates, which have first-stage of 0.94 to 1.1 across specifications. Standard errors are clustered at the HSA or HRR level depending on the specification. Dollar values are inflation-adjusted to 2005 using the CPI-U.

47.9 percent in the MCBS baseline sample and the mean inflation-adjusted premium is \$1,779, these estimates translate into a demand elasticity of −1.5 to −1.8.

Although the demand estimates vary across specifications, the tax policy counterfactuals that motivate our analysis are not particularly sensitive to the exact value of the demand elasticity. Because our instrument affects premiums much like a tax would, the direct cost-savings from taxing Medigap can be calculated from the reduced form relationship between premiums and Medicare spending, a relationship we can more precisely estimate with the universe of spending data. The role of the demand estimates is to calculate the revenue raised from taxing Medigap, which turns out to be a small share of the total budgetary savings.

C. *Utilization and Spending*

We examine the effect on utilization and spending with regressions of these measures on the leave-out costs instrument, HSA fixed effects, and controls (see Section II, equation (4)). The main source of data is the pooled 1999 to 2005 Beneficiary Summary Files, which provide us with annual beneficiary-level cost and utilization data for the universe of FFS Medicare beneficiaries. We also use the 1999 to 2005 Carrier File for analysis that requires claim-level data. For these data, we have information on a randomly selected 20 percent sample of individuals.

Utilization.—Table 6 presents estimates of the effect on annual utilization. The first column displays the dependent variable, and each row shows results from a

TABLE 6—REGRESSIONS OF ANNUAL MEDICARE UTILIZATION ON LEAVE-OUT COSTS

	Leave-out costs (hundreds)			Mean of	Implied Medigap effect	
Dependent variable	Est.	SE	p-value	dep. var.	Level	Percent
Part B events	−0.4180	(0.1810)	0.021	25.81	8.71	33.7
Imaging events	−0.0812	(0.0323)	0.012	3.99	1.69	42.4
Testing events	−0.4090	(0.1470)	0.005	11.41	8.52	74.7
Total RVUs	−1.2900	(0.4960)	0.009	70.77	26.88	38.0
Part A days	−0.0621	(0.0188)	0.001	2.10	1.29	61.6
Part A stays	−0.0040	(0.0021)	0.065	0.34	0.08	23.9
SNF days	0.0120	(0.0201)	0.552	1.37	−0.25	−18.2
SNF stays	0.0003	(0.0008)	0.761	0.06	−0.01	−8.8

Notes: Table displays estimates from regressions of annual Medicare utilization on leave-out costs, HSA fixed effects, and controls for age, sex, race, health risk, and GAF/OWI adjustment factors (see Section II, equation (4)). Each row displays the results from a separate regression. The implied Medigap effect is calculated by dividing the estimate by the coefficient on leave-out costs from the baseline demand specification. This analysis draws on data from the pooled 1999–2005 CMS Beneficiary Summary File, CMS Denominator File, and CMS Carrier File (for RVU analysis). This analysis uses the baseline sample described in panel B of Table 2 ($N = 23{,}708{,}295$ for the RVU measure; $N = 130{,}895{,}953$ for all other measures). Standard errors are clustered at the HSA level. Dollar values are inflation-adjusted to 2005 using the CPI-U.

separate regression. The baseline specifications include controls for demographics (age, sex, and race), geographic price indexes (GAF and OWI), and chronic conditions.[40] Standard errors are clustered at the HSA level.[41]

Panels B to E of Figure 5 depict the relationship between our key utilization measures and leave-out costs using scatter plots. The vertical axis displays the residuals from a regression of our utilization measures on HSA fixed effects and the same controls as the regression described above. The horizontal axis displays the residuals from a regression of leave-out costs on HSA fixed effects and the same controls. Each point shows the mean values for an HSA-state. The axes are rescaled by adding the means of the vertical and horizontal axis variables.

Table 6 shows that most categories of utilization are decreasing in leave-out costs—implying that Medigap coverage increases Medicare utilization. The first row shows that a \$100 increase in leave-out costs reduces annual Part B events (line-item claims) by 0.42, and this estimate is statistically significant with a p-value of 0.02. Given the one-for-one relationship between the instrument and premiums (Table 4), we can interpret this coefficient as the effect of a \$100 increase in Medigap premiums. We also translate the estimates into an implied effect of Medigap by dividing this estimate by the coefficient on leave-out costs from the preferred HSA-level demand specification of −0.048. Dividing by the demand coefficient implies that Medigap increases Part B events by 8.7 or 33.7 percent of the average number of events.

The second and third rows of Table 6 examine subcategories of Part B events that are often considered more discretionary and may be more elastic to variation

[40] Online Appendix D displays the full list of chronic health condition controls. Online Appendix Table E1 shows that the exclusion of chronic conditions controls has a statistically indistinguishable effect on the estimates.

[41] In Online Appendix Table H1, we examine the sensitivity of our utilization estimates to different levels of clustering, including clustering at the HSA $\times$ state level and multiway clustering on HSAs and states.

in cost-sharing.[42] We find that a \$100 increase in leave-out costs reduces imaging events (e.g., X-rays, CT scans, MRIs) by 0.08, implying a Medigap effect of 1.7 or 42.4 percent of the average. We find that a \$100 increase reduces testing events (e.g., glucose tests, bacterial cultures, EKG monitoring) by 0.41, implying a Medigap effect of 8.5 or 74.7 percent of the average.[43]

We also use the 20 percent sample of claims data from the CMS Carrier File to examine effects on other measures of Part B utilization. For each claim, these data provide the relative value units (RVUs) of the care provided. An RVU is a measure constructed by CMS that is intended to reflect relative input intensity, and CMS scales this measure to determine Medicare payments. The estimates indicate that a \$100 increase in leave-out costs reduces RVUs by 1.3, implying a Medigap effect of 26.9 or 38.0 percent of the average. The effect is statistically significant with a p-value less than 0.01. Panel C of Figure 5 depicts this relationship using a scatter plot.

The next two rows show the effects of the instrument on annual Part A hospital utilization. The estimates indicate that a \$100 increase in the instrument reduces the number of Part A hospital stays by 0.004 with an implied Medigap effect of 23.9 percent. A \$100 increase in leave-out costs reduces the number of Part A hospital days by 0.06, for an implied Medigap effect of 1.3 or 61.6 percent. The associated p-values of these estimates are 0.065 and 0.001, respectively. Panels D and E of Figure 5 show these relationships using scatter plots.

There is suggestive evidence that the reduction in Part A hospital utilization may be due in part to substitution away from Part A hospital care to Skilled Nursing Facility (SNF) care. SNFs provide care to recently discharged patients who need skilled medical and rehabilitative care. Although receiving Part A care requires significant cost-sharing, Medicare provides complete coverage for SNF care with no deductible for the first 20 days per benefit period.[44] Thus, patients without Medigap have an incentive to obtain this care at an SNF. We find suggestive evidence that an increase in leave-out costs *raises* SNF Days and SNF Stays. While the estimates are not statistically distinguishable from zero, the point estimate for SNF Days suggests that substitution to SNF may explain 19.3 percent $(= 0.012/0.062)$ of the decline in Part A Days caused by Medigap.

Medicare Payments.—Table 7 presents estimates of the effect on annual Medicare payments. The table layout is identical to Table 6. The first column displays the dependent variable, and each row shows results from a separate regression. We show the coefficient on leave-out costs (measured in hundreds of dollars) and the implied

[42] Prior research suggests that testing and imaging claims are more elective than general physician claims. For instance, Clemens and Gottlieb (2014) finds that testing and imaging claims are more responsive to changes in provider payments than evaluation and management claims, and Finkelstein, Gentzkow, and Williams (2016) finds that imaging and testing claims are more responsive to place based factors than other types of care using a "movers" design.

[43] As indicated in the Beneficiary Summary File data documentation, imaging events are defined as claims with a line BETOS code that starts with the letter "I." Testing events are claims with a line BETOS code that starts with the letter "T."

[44] To qualify for SNF coverage during a benefit period, beneficiaries must have a qualifying hospital stay of 3 days or longer and enter the SNF within 30 days of hospital discharge for services related to the hospital stay.

Table 7—Regressions of Annual Medicare Payments on Leave-Out Costs

	Leave-out costs (hundreds)			Mean of	Implied Medigap effect	
Dependent variable	Est.	SE	p-value	dep. var.	Level	Percent
Medicare payments	−67.02	(33.11)	0.043	6,291	1,396.25	22.2
Part A payments	−47.59	(22.76)	0.037	3,021	991.54	32.8
Part B payments	−21.80	(15.90)	0.159	2,648	454.23	17.2
SNF payments	3.44	(5.25)	0.513	399	−71.61	−17.9

Notes: This table displays estimates from regressions of annual Medicare payments on leave-out costs, HSA fixed effects, and controls for age, sex, race, health risk, and GAF/OWI adjustment factors (see Section II, equation (4)). Each row displays the results from a separate regression. The implied Medigap effect is calculated by dividing the estimate by the coefficient on leave-out costs from the baseline demand specification. This analysis draws on data from the pooled 1999–2005 CMS Beneficiary Summary File and CMS Denominator File. This analysis uses the baseline sample described in panel B of Table 2 ($N = 130{,}895{,}953$). All dependent variables are top-coded at $64,000. Standard errors are clustered at the HSA level. Dollar values are inflation-adjusted to 2005 using the CPI-U.

effect of Medigap. These baseline specifications include controls for demographics (age, sex, and race), geographic price indexes (GAF and OWI), and chronic conditions.[45] Standard errors are clustered at the HSA level.[46]

The top row of Table 7 shows the effect on total annual Medicare payments. A $100 increase in leave-out costs reduces total Medicare payments by $67.02, and this estimate is statistically significant with a p-value of 0.043. This estimate implies that Medigap increases Medicare payments by $1,396 on a mean of $6,291 or 22.2 percent.[47] Panel F of Figure 5 depicts the relationship between total Medicare payments and leave-out costs using a scatter plot, constructed in the same manner as the other panels in the figure.

The remaining rows of Table 7 show that a $100 increase in leave-out costs reduces annual Part A payments by $47.59 and annual Part B payments by $21.80. These estimates imply that Medigap raises Part A payments by $992 or 32.8 percent and Part B payments by $454 or 17.2 percent. Similar to the utilization results, we find that SNF payments are decreasing in leave-out costs, although the estimate lacks statistical precision. The point estimate for SNF payments suggests that a $100 increase in leave-out costs raises SNF spending by $3.44. The implied Medigap effect is −$72 or a reduction of 17.9 percent.

Our preferred estimate—that Medigap increases Medicare payments by 22.2 percent—implies a price elasticity similar to standard estimates in the literature. As emphasized by Aron-Dine, Einav, and Finkelstein (2013), summarizing the effect of health insurance with a single elasticity parameter is difficult because nonlinear health insurance contracts do not exhibit a well-defined, out-of-pocket

[45] Online Appendix D displays the full list of chronic health condition controls. Online Appendix Table E2 shows that the exclusion of chronic health condition controls has a statistically indistinguishable effect on the payment estimates.

[46] In Online Appendix Table H1, we examine the sensitivity of our payment estimates to different levels of clustering, including clustering at the HSA × state level and multiway clustering on HSAs and states.

[47] Given the sizable effects on utilization and Medicare payments, one might be interested in testing whether Medigap reduces mortality. Online Appendix G shows results consistent with Medigap having no effect on mortality. Specifically, Online Appendix G demonstrates that the age distribution (conditional on reaching age 65) is unrelated to the identifying variation.

"price" for medical care. This is particularly true for Medicare since cost-sharing is nonlinear in the level of utilization (e.g., Part A deductible, copays) and cost-sharing varies across categories of medical care (e.g., Part A, Part B, SNF). If we assume, as an approximation, that Medigap reduces cost-sharing from 20 to 0 percent, then our preferred estimate that Medigap increases utilization by 22.2 percent implies an arc-elasticity of −0.11, which is in the same range as the classic RAND estimate of −0.2 (Keeler and Rolph 1988).[48] Our elasticity estimate is also similar to the −0.16 elasticity estimated by Chandra, Gruber, and McKnight (2014) in the context of Massachusetts health care reform.

V. Robustness

The basic threat to our identification strategy is that there may be omitted variables that are correlated with both our leave-out costs instrument and Medicare utilization. In Section IIIB, we showed that ZIP code-level demographic characteristics such as income, labor force participation, and education are not correlated with our instrument. Below, we present two additional pieces of evidence in support of our identification strategy. First, we show that our baseline results are robust to the inclusion of additional control variables. Second, we conduct falsification tests that demonstrate that omitted factors that change sharply at state boundaries are unlikely to be driving our results. In online Appendix J, we present estimates from additional alternative specifications and placebo border analysis, which further suggest that our results are not driven by unrelated spatial trends in medical spending.

A. *Alternative Specifications*

Table 8 shows that our results are robust to the inclusion of additional control variables. The first row displays the baseline Medicare payments result for reference. The second row displays the results when ZIP code-level census demographic variables are added to the baseline specification. The third row displays the results when we include fully interacted HSA-by-year fixed effects, instead of the additively separable HSA and year fixed effects in the baseline specification. The point estimates are stable across all the specifications, with an implied Medigap effect ranging from \$1,396 to \$1,157. Online Appendix Table F1 illustrates that estimates are broadly similar when we re-estimate the baseline specification separately by year.

B. *Falsification Tests*

It could be a problem for the identification strategy if there are omitted factors related to Medicare spending that are also correlated with the within-HSA variation in the leave-out cost instrument. For example, if the underlying health of the population changed sharply at state boundaries in a way that was correlated with our

[48] Let q_1 and p_1 be the quantity and price without supplemental insurance and let q_2 and p_2 be the price and quantity with Medigap. The arc elasticity is given by $\epsilon_{arc} = \frac{q_2 - q_1}{(q_2 + q_1)/2} / \frac{p_2 - p_1}{(p_2 + p_1)/2}$.

TABLE 8—ROBUSTNESS CHECKS

	Leave-out costs (hundreds)			Mean of dep. var.	Implied Medigap effect	
	Est.	SE	p-value		Level	Percent
Baseline specification						
Annual Medicare payments	−67.02	(33.11)	0.043	6,291	1,396.25	22.2
Alternative specifications						
(dep. var. is annual Medicare payments)						
Census ZIP code-level controls included	−59.96	(30.16)	0.047	6,291	1,249.09	19.9
Region-year fixed effects included	−55.54	(31.74)	0.085	6,291	1,157.02	18.4
Unaffected procedures						
Urgent RVUs	5.44e-02	(6.76e-02)	0.421	4.274	−1.13	−26.5
(Clemens & Gottlieb Def'n)						
Urgent admissions	−1.31e-03	(1.03e-03)	0.201	0.077	0.03	35.4
(Card, Dobkin, & Maestas Def'n 1)						
Urgent admissions	−6.89e-04	(1.03e-03)	0.505	0.125	0.01	11.5
(Card, Dobkin, & Maestas Def'n 2)						
Unaffected individuals						
Non-elderly adults in NHIS						
Hospital days	0.042	(0.048)	0.378	0.342	−0.88	−257.1
Hospital stays	0.011	(0.006)	0.060	0.086	−0.23	−266.2
Physician office visits (indicator for ≥ 2)	0.019	(0.012)	0.106	0.528	−0.39	−74.6
Self-reported health	0.040	(0.042)	0.340	1.999	−0.83	−41.4

Notes: This table displays estimates from regressions of spending and utilization measures on leave-out costs, HSA fixed effects, and controls for age, sex, race, health risk, and GAF/OWI adjustment factors (see Section II, equation (4)). Each row displays the results from a separate regression. The implied Medigap effect is calculated by dividing the estimate by the coefficient on leave-out costs from the baseline demand specification. This analysis draws on data from the pooled 1999–2005 CMS Beneficiary Summary File, CMS Denominator File, CMS Carrier File ("Urgent RVU" analysis), NHIS ("Unaffected Individuals" analysis), and CMS MedPAR ("Urgent Admissions" analysis). Aside from the NHIS, for each of these datasets we use a sample definition analogous to the baseline sample described in panel B of Table 2. The "Unaffected Individuals" analysis utilizing the NHIS data focuses on the sample of non-elderly adults, excluding those with Medicare coverage. Standard errors are clustered at HSA level. Dollar values are inflation-adjusted to 2005 using the CPI-U.

instrument, our results may simply reflect this health differential and not the effect of Medigap. We present the results of two tests below that help to alleviate this concern, drawing on data on procedures that are very urgent (and thus should be unaffected by our instrument) and health outcomes for individuals younger than 65 who are ineligible for Medigap. Together, these tests indicate that factors affecting utilization in general (for example, the underlying health of the population) are not driving the results.

Urgent Procedures.—We investigate the relationship between our instrument and urgent procedures using definitions of urgent procedures from the literature. First, we examine the effect on urgent Part B RVUs using the characterization of Clemens and Gottlieb (2014), which is based on the BETOS code classification. Second, we investigate urgent hospital admissions based on the methodology of Card, Dobkin, and Maestas (2009), which defines urgent hospitalizations as those with similar daily frequencies on weekdays and weekends.[49] We consider two variants of this definition of urgent hospitalizations. We investigate the ten most common non-deferrable

[49] This analysis is done using the CMS MedPAR files that contain hospital claims data for 100 percent of FFS Medicare beneficiaries.

conditions identified by Card, Dobkin, and Maestas (2009) in their data and we use the Card, Dobkin, and Maestas (2009) methodology to characterize the set of urgent hospitalizations with our data (the CMS MedPAR data). Online Appendix I describes all three characterizations of urgent procedures in detail.

Table 8 presents the results of these regressions, which repeat the baseline specification replacing the dependent variable with the number of urgent procedures based on the characterizations described above.[50] Across the different classifications, there is no evidence of an effect of leave-out costs on urgent procedures. The point estimates vary greatly in terms of magnitude and sign and none of the estimates are statistically significant (p-values ranging from 0.20 to 0.51). These results suggest that it is unlikely that discontinuities in other health-related factors are driving the main results.

Non-Elderly Individuals.—Next, we investigate the relationship between our instrument and outcomes for non-elderly individuals (aged 18–64) using data from the NHIS. We examine effects on utilization measures including hospital stays, hospital days, and physician office visits. In addition, we examine the effect on self-report health, measured with a Likert Scale that runs from 1 to 5, with 1 indicating "Excellent" and 5 indicating "Poor."

Table 8 presents the results of these regressions, which repeat the baseline specification replacing the dependent variable with these measures of utilization and health status among the non-elderly. Across the four measures, the coefficient on leave-out costs is statistically indistinguishable from zero. Although the limited sample size of the NHIS prevents us from ruling out effects, these falsification tests show no evidence of any negative correlation between health care utilization and our instrument for individuals younger than 65 who are ineligible for Medigap.

VI. Policy Counterfactuals

A natural policy to address the externality from Medigap is a tax on Medigap premiums. The idea of taxing Medigap premiums is not new. For example, the Obama Administration's 2013 budget proposal called for a 15 percent tax on Medigap policies. In *Budget Options Volume I: Health Care*, the Congressional Budget Office (CBO) considered a 5 percent excise tax on Medigap premiums. Below, we investigate the effect of corrective taxation on Medicare's budget and welfare.

A. *Medicare's Budget*

A tax presents two sources of savings for the Medicare program. First, a tax discourages some individuals from enrolling in Medigap, which reduces their Medicare

[50] As in the baseline specification, these regressions are run at the individual-year level, so the measure of urgent procedures is also at the individual-year level. The Clemens and Gottlieb (2014) measure is based on the 20 percent of individuals for which Part B claims data are available (in the CMS Carrier file). The two Card, Dobkin, and Maestas (2009) measures are created using the CMS MedPAR data available for 100 percent of beneficiaries.

spending by removing the externality estimated above. Second, tax revenues are raised from those remaining Medigap purchasers.

We use the results from Section IV to produce estimates of the effect of a tax on Medigap premiums in the following manner. First, the counterfactual Medigap market share is calculated using the estimated demand elasticity, assuming the tax is fully passed through to consumers.[51] The demand curve used for these calculations has a slope equal to $\partial q_{ijk}/\partial \textit{Leave-out costs}_{jk} = -0.048$ (as the coefficient in the premium regressions was approximately one) and an intercept pinned down by the national Medigap market share of 48 percent and the national average premium of \$1,779. The tax revenue raised is then calculated by multiplying the tax by the resulting Medigap market share. Medicare cost savings are determined by applying the Medigap externality calculated above to all those who drop their Medigap coverage due to the tax (the change in the Medigap market share). Importantly, the cost savings estimate does not depend on our estimate of the Medigap demand curve, and instead relies on the reduced form Medicare cost estimate in Table 7 that uses the administrative cost data.[52] The total budgetary impact is simply the sum of the tax revenue raised and Medicare cost savings from Medigap disenrollment. The parameters used in this calculation are estimated using local variation in premiums, and the projected effects of larger taxes should be viewed with appropriate caution.

Table 9 shows the results of this exercise. Each row displays the results for a different tax rate; the columns display the tax revenue raised, the Medicare cost savings obtained through Medigap disenrollment, and the total budgetary impact on the Medicare program. The per capita numbers presented in this table refer to the non-dual eligible, FFS Medicare population (the estimation sample). A 15 percent tax on Medigap premiums would raise \$94 per beneficiary in tax revenue and reduce Medicare costs per beneficiary by \$179 for a total savings of \$273 per beneficiary or 4.3 percent of per capita costs.

Online Appendix Table K1 shows that this estimate varies from 4.0 to 4.8 percent using all of the alternative demand estimates in Table 5.[53] As discussed in Section IVB, these savings effects are quite stable because the demand estimates are only used to calculate the revenue from taxing Medigap, which turns out to be a small share of the total budgetary savings. Combining the standard errors associated with our demand and cost estimates, we calculate that the standard error of our baseline estimate of 4.3 percent total savings is 1.7 percentage points.

[51] The calculations in Table 9 assume that the tax is fully passed through to consumers. If the pass-through rate is ρ, it would take a tax of size x/ρ percent to achieve the Medicare budgetary impact we calculate for an x percent tax.

[52] To see this, note that a \$100 tax on Medigap generates per capita cost savings of γ_c, the coefficient in column 1 of Table 7. Alternatively, this cost savings could be calculated as the savings for each person who drops Medigap coverage, the Medigap externality $\left(\gamma_c/\beta_c\right)$, multiplied by the fraction of people who drop Medigap coverage from a \$100 tax (β_c). These procedures are equivalent and are both valid for a small tax.

[53] Online Appendix Table K1 displays the projected total savings and standard errors associated with a 15 percent tax using the various demand estimates. To calculate the standard error on the total savings, we first separately calculate the standard error on the tax revenue raised (from the corresponding demand estimate) and the standard error from the Medicare cost savings from Medigap disenrollment (from the reduced form). We then obtain the standard error on the total savings by aggregating these standard errors using the Delta Method assuming no covariance between the demand and cost estimates.

Table 9—Counterfactuals: Taxing Medigap

Tax (%)	Medigap Market share (%)	Tax revenue (per beneficiary)($)	Medicare savings (per beneficiary)($)	Total budgetary impact (per beneficiary)($)	Total budgetary impact Percent
0	48	0	0	0	0
5	44	39	60	99	1.6
10	39	70	119	189	3.0
15	35	94	179	273	4.3
20	31	110	238	348	5.5
30	22	119	358	477	7.6
40	14	99	477	575	9.1
Pigouvian tax	0	0	670	670	10.7

Notes: The first column lists the tax as a percentage of the $1,779 average Medigap premiums. The second column lists the implied Medigap market share assuming full pass-through of the tax. The linear demand curve used in these calculations has a slope equal to $\partial q_{ijk}/\partial \textit{Leave-out costs}_{jk} = -0.048$ (as the coefficient on leave-out costs in the premium regressions was is approximately one) and an intercept pinned down by the equilibrium average price and quantity ($p = 1{,}779$ and $q = 0.48$). The remaining columns list the tax revenue, cost savings from Medigap disenrollment, and total budgetary impact, respectively. These results are based on the estimated $1,396 Medigap externality. Dollar values are inflation-adjusted to 2005 using the CPI-U.

We can translate this calculated per capita savings into the aggregate savings for the current Medicare program. In 2012 dollars, the per capita savings from a 15 percent tax for non-Medicaid eligible, FFS Medicare enrollees is $321. By law, Medicare Advantage payments are set to be a function of the local FFS Medicare spending. Thus, if we assume that Medicare Advantage capitation payments are reduced by the same amount as the FFS Medicare spending, then the per capita savings for Medicare Advantage beneficiaries is also $321. There are roughly 27.4 million FFS, non-Medicaid eligible Medicare beneficiaries and 12.7 million Medicare Advantage enrollees (KFF 2012). Summing across these beneficiaries, the total savings for the Medicare program from a 15 percent tax is estimated to be $12.9 billion, with a standard error of $4.9 billion.

Table 9 shows that a Pigouvian tax that fully accounts for the estimated externality would completely eliminate the Medigap market, saving the Medicare program $670 per capita or 10.7 percent of total Medicare costs. When we adjust for inflation and assume that the savings are internalized by the Medicare Advantage program, this translates into total savings for the Medicare program of roughly $31.6 billion in 2012 dollars.

B. *Welfare*

The cost-savings to Medicare from taxing Medigap calculated in the prior section should not be thought of as a pure efficiency gain. That is, while Medigap exerts a negative externality on the Medicare system, it also generates surplus for consumers who value the risk protection benefits it provides and, to some extent, the additional care they demand as a result of the increased coverage. One way to measure how much consumers value the benefits of Medigap is through their willingness-to-pay or the demand curve for Medigap. Below we compare the cost savings and the efficiency gains from taxation using our estimates of the Medigap externality and Medigap demand curve.

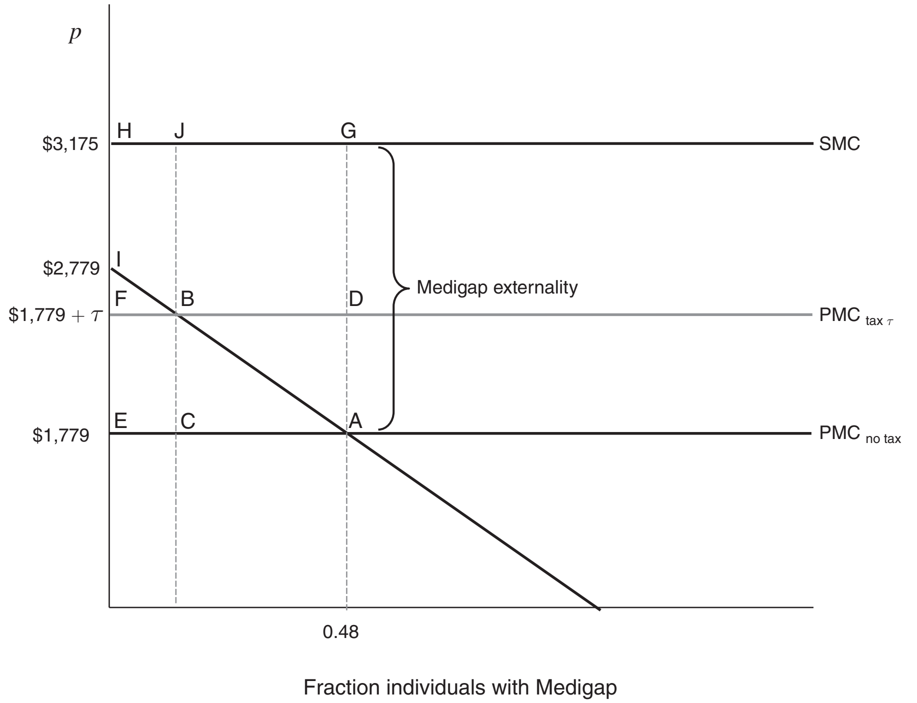

FIGURE 6. WELFARE UNDER TAXATION

Notes: This figure shows the welfare effects of taxing Medigap. The demand curve has a slope equal to $\partial q_{ijk}/\partial \textit{Leave-out costs}_{jk} = -0.048$ (as the coefficient on leave-out costs in the premium regressions is approximately one) and an intercept pinned down by the equilibrium average price and quantity ($p = 1{,}779$ and $q = 0.48$). The private marginal cost curve is the horizontal line at the observed average annual premium ($1,779). The social marginal cost curve is the private marginal cost curve shifted upward by the Medigap externality. The deadweight loss from Medigap is the trapezoid AIHG. The figure also displays the private marginal cost curve under a tax of τ. Dollar values are inflation-adjusted to 2005 using the CPI-U.

Figure 6 displays supply and demand in the Medigap market under the assumption of perfect competition and constant marginal costs. Under these assumptions, we have the standard "price equals marginal costs" equilibrium condition, and the private marginal cost curve can be approximated by a horizontal line at the average Medigap premium of $1,779. The social marginal cost curve is the sum of private costs and the externality and is depicted in the figure by the horizontal line at $3,175 (= $1,396 + $1,779). The equilibrium with no tax is represented by point A, the intersection of the private marginal cost curve and the demand curve. The social optimum result is the elimination of the Medigap market. The deadweight loss from the fiscal externality of Medigap is given by the trapezoid AIHG. In this figure, the net efficiency gain from a Pigouvian tax is 64 percent of the total impact on Medicares budget; the remaining 36 percent is a transfer of surplus from individuals who otherwise would have purchased Medigap to taxpayers.

Figure 6 also illustrates the private marginal cost curve in the case of a smaller tax τ that does not cause the Medigap market to disappear. The effect of a tax τ on

Medicare's budget is depicted by the sum of two rectangles: CEFB (the tax revenue raised) and ACJG (the cost savings from Medigap dis-enrollment). The net efficiency gain is represented by the deadweight loss trapezoid ABJG. Comparing this welfare gain to the overall impact on the Medicare budget shows that only a fraction of the impact on the Medicare budget is a net welfare gain. The remainder of Medicare's total savings comes from transfers from Medigap purchasers and individuals deterred from purchasing Medigap because of the tax. These transfers are represented in the figure by the rectangle CEFB (tax revenue raised from Medigap purchasers under the tax) and the triangle ACB (consumer surplus forgone by individuals discouraged from purchasing Medigap because of the tax).

There are at least two caveats to these calculations. First, our analysis focuses on evaluating the effect of a tax on Medigap premiums taking the form of Medigap and Medicare as given. Although the first-best policy to address the Medigap externality may involve broader changes to Medigap or Medicare coverage, taxing Medigap premiums is a commonly discussed policy and our identifying premium variation gives us a unique opportunity to evaluate the effect of a tax on Medigap premiums.[54]

Second, the welfare discussion above abstracts from market power. To the extent that Medigap insurers have market power, the resulting markups already act as an implicit tax, raising the price relative to the social marginal cost.[55] It turns out that our estimate of the Medigap externality is large enough that an optimal tax would substantially reduce the size of the Medigap market regardless of the degree of market power.[56] Of course, the exact welfare effect of such a tax would need to be measured relative to the correct equilibrium and cost curves (which, in the case of market power, would differ from those depicted in Figure 6).[57]

VII. Conclusion

Medicare includes cost-sharing to reduce unnecessary utilization. Since beneficiaries can purchase supplemental insurance from Medigap, they are able to reduce

[54] See Pauly (2000) for a theoretical discussion of the efficiency trade-offs involved in the simultaneous public and private provision of insurance within the Medicare context.

[55] Starc (2014) estimates that markups are substantial in this market, on the order of 30 percent.

[56] Let us assume firms face constant marginal costs equal to the observed average uncovered Medicare spending of \$911 in our data. (Note that this number is likely conservatively low relative to insurer average costs if there is either adverse selection in the Medigap market or administrative costs associated with Medigap policies.) In this case, regardless of the structure of competition, a Pigouvian tax would bring the after-tax premium to a minimum of \$2,307 (= \$911 + \$1,396), as insurers will avoid making losses; the implied Medigap market share at a premium of \$2,307 is approximately 23 percent. In other words, our estimated Medigap externality is high enough that an optimal Pigouvian tax would cause the Medigap market to shrink by at least 50 percent of its current size regardless of the form of competition.

[57] A third potential caveat is that this analysis uses our estimated uncompensated demand curve, while the ideal welfare analysis would use a compensated demand curve. However, there are a few reasons why the uncompensated demand curve may be a good local approximation of the compensated demand curve in this setting. The change in income associated with a small to moderate tax on Medigap is very small: a 15 percent tax on Medigap would amount to roughly \$200 annually, or less than 0.5 percent of average annual household income in the over 65 population. In addition, prior estimates suggest the elasticity of health care spending with respect to income is small. A treatment arm in the RAND health insurance experiment, which provided participants an unanticipated increase in income, found no effect on health care utilization (Newhouse and the Insurance Experiment Group 1993). Using oil price shocks and geographic variation in exposure to these shocks, Acemoglu, Finkelstein, and Notowidigdo (2013) finds health care expenditures have an elasticity of approximately 0.7 with respect to income.

their exposure to this cost-sharing, potentially increasing utilization and imposing a negative externality on the Medicare system. Using Medigap premium discontinuities that occur at state boundaries and an estimated demand elasticity of -1.8, we find that Medigap increases overall Medicare costs by \$1,396 per year on a base of \$6,291 or by 22.2 percent.

Our estimates indicate that a 15 percent tax on Medigap premiums, with full pass-through, would decrease Medigap coverage by 13 percentage points on a base of 48 percent and reduce net government costs by 4.3 percent per Medicare beneficiary or \$12.9 billion in 2012 dollars. About 35 percent of these savings would come from tax revenue while the remainder would come from lower Medigap enrollment. A Pigouvian tax requires us to extrapolate outside the premium variation in the data. To a first approximation, such a tax would generate combined savings of 10.7 percent per beneficiary or \$31.6 billion in 2012 dollars.

In closing, we want to emphasize that taxing Medigap is not the only way to address the externality from Medigap. Although taxing Medigap has received substantial attention, such a tax is a fairly blunt instrument for increasing Medicare's efficiency, and other policies may lead to even larger efficiency gains.

REFERENCES

Acemoglu, Daron, Amy Finkelstein, and Matthew J. Notowidigdo. 2013. "Income and Health Spending: Evidence from Oil Price Shocks." *Review of Economics and Statistics* 95 (4): 1079–95.

Agarwal, Sumit, Souphala Chomsisengphet, Neale Mahoney, and Johannes Stroebel. 2018. "Do Banks Pass through Credit Expansions to Consumers Who Want to Borrow?" *Quarterly Journal of Economics* 133 (1): 129–90.

Aron-Dine, Aviva, Liran Einav, and Amy Finkelstein. 2013. "The RAND Health Insurance Experiment, Three Decades Later." *Journal of Economic Perspectives* 27 (1): 197–222.

Baicker, Katherine, Amitabh Chandra, and Jonathan S. Skinner. 2012. "Saving Money or Just Saving Lives? Improving the Productivity of US Health Care Spending." *Annual Review of Economics* 4 (1): 33–56.

Baicker, Katherine, Sendhil Mullainathan, and Joshua Schwartzstein. 2015. "Behavioral Hazard in Health Insurance." *Quarterly Journal of Economics* 130 (4): 1623–67.

Bundorf, M. Kate, Jonathan D. Levin, and Neale Mahoney. 2012. "Pricing and Welfare in Health Plan Choice." *American Economic Review* 102 (7): 3214–48.

Cabral, Marika, and Mark R. Cullen. 2018. "Estimating the Value of Public Insurance Using Complementary Private Insurance." National Bureau of Economic Research (NBER) Working Paper 22583.

Cabral, Marika, and Neale Mahoney. 2019. "Externalities and Taxation of Supplemental Insurance: A Study of Medicare and Medigap: Dataset." *American Economic Journal: Applied Economics*. https://doi.org/10.1257/app.20160350.

Card, David, Carlos Dobkin, and Nicole Maestas. 2009. "Does Medicare Save Lives?" *Quarterly Journal of Economics* 124 (2): 597–636.

Chandra, Amitabh, Jonathan Gruber, and Robin McKnight. 2010. "Patient Cost-Sharing and Hospitalization Offsets in the Elderly." *American Economic Review* 100 (1): 193–213.

Chandra, Amitabh, Jonathan Gruber, and Robin McKnight. 2014. "The Impact of Patient Cost-Sharing on Low-Income Populations: Evidence from Massachusetts." *Journal of Health Economics* 33: 57–66.

Clemens, Jeffrey, and Joshua Gottlieb. 2014. "Do Physicians' Financial Incentives Affect Medical Treatment and Patient Health?" *American Economic Review* 104 (4): 1320–49.

Cutler, David M., and Louise Sheiner. 1999. "The Geography of Medicare." *American Economic Review* 89 (2): 228–33.

Cutler, David, Jonathan Skinner, Ariel Dora Stern, and David Wennberg. 2013. "Physician Beliefs and Patient Preferences: A New Look at Regional Variation in Health Care Spending." National Bureau of Economic Research (NBER) Working Paper 19320.

DellaVigna, Stefano, and Matthew Gentzkow. 2017. "Uniform Pricing in US Retail Chains." National Bureau of Economic Research (NBER) Working Paper 23996.

Einav, Liran, Amy Finkelstein, and Neale Mahoney. 2017. "Provider Incentives and Healthcare Costs: Evidence from Long-Term Care Hospitals." http://faculty.chicagobooth.edu/neale.mahoney/research/papers/Mahoney_LTCHs_Jan_2017.pdf.

Ericson, Keith M. Marzilli, and Amanda Starc. 2015. "Pricing Regulation and Imperfect Competition on the Massachusetts Health Insurance Exchange." *Review of Economics and Statistics* 97 (3): 667–82.

Ettner, Susan L. 1997. "Adverse Selection and the Purchase of Medigap Insurance by the Elderly." *Journal of Health Economics* 16 (5): 543–62.

Fang, Hanming, Michael P. Keane, and Dan Silverman. 2008. "Sources of Advantageous Selection: Evidence from the Medigap Insurance Market." *Journal of Political Economy* 116 (2): 303–50.

Finkelstein, Amy. 2004. "Minimum Standards, Insurance Regulation and Adverse Selection: Evidence from the Medigap Market." *Journal of Public Economics* 88 (12): 2515–47.

Finkelstein, Amy, Matthew Gentzkow, and Heidi Williams. 2016. "Sources of Geographic Variation in Health Care: Evidence from Patient Migration." *Quarterly Journal of Economics* 131 (4): 1681–1726.

Government Accountability Office. 2013. *Medicare Supplemental Coverage: Medigap and Other Factors Are Associated with Higher Estimated Health Care Expenditures.* United States Government Accountability Office. Washington, DC, September.

Gruber, Jonathan. 2013. "An Enduring Social Safety Net: Proposal 3: Restructuring Cost Sharing and Supplemental Insurance for Medicare." Hamilton Project. http://www.hamiltonproject.org/assets/legacy/files/downloads_and_links/THP_15WaysFedBudget_Prop3.pdf.

Hendren, Nathaniel. 2016. "The Policy Elasticity." *Tax Policy and the Economy* 30 (1): 51–89.

Hurd, Michael D., and Kathleen McGarry. 1997. "Medical Insurance and the Use of Health Care Services by the Elderly." *Journal of Health Economics* 16 (2): 129–54.

Kaiser Family Foundation, and Hewitt. 2004. "Current Trends and Future Outlook for Retiree Health Benefits: Findings from the Kaiser/Hewitt 2004 on Retiree Health Benefits." Kaiser Family Foundation and Hewitt Associates. https://www.kff.org/medicare/current-trends-and-future-outlook-for-retiree/.

Kaiser Permanente International. 2008. "Selected European Countries' Health Systems and the United States." http://xnet.kp.org/kpinternational/iinternational/2008eurohealthsyscomp.pdf (accessed 9/10/2009).

Kaiser Family Foundation. 2012. "State Health Facts." Kaiser Family Foundation. https://www.kff.org/statedata/.

Keeler, Emmett B., and John E. Rolph. 1988. "The Demand for Episodes of Treatment in the Health Insurance Experiment." *Journal of Health Economics* 7 (4): 337–67.

Khandker, Rezaul K., and Lauren A. McCormack. 1999. "Medicare Spending by Beneficiaries with Various Types of Supplemental Insurance." *Medical Care Research and Review* 56 (2): 137–55.

Lemieux, Jeff, Teresa Chovan, and Karen Heath. 2008. "Medigap Coverage and Medicare Spending: A Second Look." *Health Affairs* 27 (2): 469–77.

Maestas, Nicole, Mathis Schroeder, and Dana Goldman. 2009. "Price Variation in Markets with Homogeneous Goods: The Case of Medigap." National Bureau of Economic Research (NBER) Working Paper 14679.

MedPAC. 2003. *Report to the Congress: Variation and Innovation in Medicare*. Medicare Payment Advisory Commission. Washington, DC, June.

Newhouse, Joseph P., and the Insurance Experiment Group. 1993. *Free for All? Lessons from the RAND Health Insurance Experiment.* Cambridge: Harvard University Press.

Pauly, Mark V. 2000. "The Medicare Mix: Efficient and Inefficient Combinations of Social and Private Health Insurance for U.S. Elderly." *Journal of Health Care Finance* 26 (3): 26–37.

Robst, John. 2006. "Estimation of a Hedonic Pricing Model for Medigap Insurance." *Health Services Research* 41 (6): 2097–2113.

Slemrod, Joel, and Shlomo Yitzhaki. 2001. "Integrating Expenditure and Tax Decisions: The Marginal Cost of Funds and the Marginal Benefit of Projects." *National Tax Journal* 54 (2): 189–201.

Starc, Amanda. 2014. "Insurer Pricing and Consumer Welfare: Evidence from Medigap." *RAND Journal of Economics* 45 (1): 198–220.

Tanner, Michael D. 2008. "The Grass Is Not Always Greener: A Look at National Health Care Systems around the World." Cato Institute Policy Analysis 613.

Wennberg, John E. 1999. "Understanding Geographic Variations in Health Care Delivery." *New England Journal of Medicine* 340 (1): 52–53.

Wennberg, John E., Elliott S. Fisher, and Jonathan S. Skinner. 2002. "Geography and the Debate over Medicare Reform." *Health Affairs* 21 (2).

Wolfe, John R., and John H. Goddeeris. 1991. "Adverse Selection, Moral Hazard, and Wealth Effects in the Medigap Insurance Market." *Journal of Health Economics* 10 (4): 433–59.

Zeckhauser, Richard J. 1970. "Medical Insurance: A Case Study of the Tradeoff between Risk Spreading and Appropriate Incentives." *Journal of Economic Theory* 2 (1): 10–26.

This article has been cited by:

1. Menghan Shen, Wen He, Linyan Li. 2020. Incentives to use primary care and their impact on healthcare utilization: Evidence using a public health insurance dataset in China. *Social Science & Medicine* **255**, 112981. [Crossref]
2. David Powell. 2020. Does Labor Supply Respond to Transitory Income? Evidence from the Economic Stimulus Payments of 2008. *Journal of Labor Economics* **38**:1, 1-38. [Crossref]
3. Amitabh Chandra, Craig Garthwaite. 2019. Economic Principles for Medicare Reform. *The ANNALS of the American Academy of Political and Social Science* **686**:1, 63-92. [Crossref]