Do Larger Health Insurance Subsidies Benefit Patients or Producers? Evidence from Medicare Advantage*

Marika Cabral† Michael Geruso‡ Neale Mahoney§

July 3, 2017

Abstract

A central question in the debate over privatized Medicare is whether increased government payments to private Medicare Advantage (MA) plans generate lower premiums for consumers or higher profits for producers. Using difference-in-differences variation brought about by a sharp legislative change, we find that MA insurers pass through 45% of increased payments in lower premiums and an additional 9% in more generous benefits. We show that advantageous selection into MA cannot explain this incomplete pass-through. Instead, our evidence suggests that market power is important, with premium pass-through rates of 13% in the least competitive markets and 74% in the most competitive.

*This paper was previously circulated under the title: "Does Privatized Health Insurance Benefit Patients or Producers? Evidence from Medicare Advantage". We thank Jason Abaluck, Christopher Afendulis, Michael Chernew, Amy Finkelstein, Matthew Grennan, Jonathan Kolstad, Amanda Kowalski, Timothy Layton, Matthew Notowidigdo, Robert Town, and seminar participants at the 2014 AHEC meeting, Duke Microeconomics Jamboree, Harvard, 2015 HEC Montreal Summer IO Conference, MIT, 2015 NBER Insurance/IO meeting, 2014 NBER Public Economics meeting, Penn State University, 2013 and 2014 RWJ Scholars in Health Policy Research Annual Meetings, UC Berkeley, UC Santa Barbara Health Economics Conference, UC Santa Cruz, University of Chicago, University of Texas at Austin, and Yale for helpful comments. We are grateful to Abhi Gupta, Mariel Schwartz, and Yin Wei Soon for excellent research assistance. Mahoney acknowledges the George J. Stigler Center for the Economy and State for financial support. Geruso acknowledges the Robert Wood Johnson Foundation. First version: July 2013. This version: July 3, 2017.

†University of Texas at Austin and NBER. Email: marikacabral@gmail.com

‡University of Texas at Austin and NBER. Email: mike.geruso@gmail.com

§Chicago Booth and NBER. Email: neale.mahoney@gmail.com

1 Introduction

Medicare is the second largest social insurance program in the United States and the primary source of health insurance for the elderly. In 2012, Medicare spent $572.5 billion on health care, a 4.8% increase over the previous year.[1] Given the large scale of the program and rapid growth in spending, reforming Medicare is a perpetual policy issue.

One commonly discussed proposal is adjusting subsidies to private Medicare Advantage plans.[2] Proponents of larger subsidies argue that increased payments will result in lower premiums or more generous benefits for Medicare beneficiaries. Opponents argue that such a move would lead to large profits for insurance companies and health care providers. Naturally, the lines of argument are reversed when a reduction in payments is proposed. At its core, these debates are about economic incidence: Does increasing government subsidies to private Medicare Advantage plans benefit patients or producers?

In most regions of the country, Medicare beneficiaries can choose to be covered by public fee-for-service Traditional Medicare or to obtain subsidized coverage through their choice of a private Medicare Advantage (MA) insurance plan. MA plans are differentiated from Traditional Medicare in having restricted provider networks, alternative cost-sharing arrangements, and additional benefits, such as vision and dental coverage. MA plans have historically been offered by health maintenance organizations (HMOs). Plans receive a capitation payment from Medicare for each enrolled beneficiary and often charge beneficiaries a supplemental premium.

We examine the incidence of subsidies to private Medicare Advantage plans by studying a sharp change in capitation payments brought about by the 2000 Benefits Improvement and Protection Act (BIPA). MA capitation payments vary at the county level. Prior to BIPA, payments were largely determined by historical Traditional Medicare expenditures in the county. BIPA reformed these payments by instituting a system of rural and urban payment floors that raised payments in 72% of counties. We show that MA capitation payments in the counties where these floors were binding were on parallel trends before the payment reform but increased by an average of about $600 per beneficiary per year or 12% when BIPA was implemented, providing us with a source of difference-in-differences

[1] Source: http://www.cms.gov/Research-Statistics-Data-and-Systems/Statistics-Trends-and-Reports/NationalHealthExpendData/NHE-Fact-Sheet.html.

[2] During our sample period, this private option was called Medicare Part C or Medicare+Choice. Since the passage of the Medicare Modernization Act in 2003, these plans have been called Medicare Advantage. We use the current naming convention throughout the paper.

variation.

Using this difference-in-differences variation, we find that MA plans passed through approximately half of their capitation payment increases. For each dollar in higher payments, consumer premiums were reduced by 45 cents at 3 years following the reform.[3] Using rich data on product characteristics, we find an additional 9 cents of pass-through in the actuarial value of plan benefits.[4] A 95% confidence interval allows us to rule out a combined pass-through rate outside of 37% to 71%. Difference-in-differences plots that flexibly allow the effect of the 2001 payment shocks to vary by year show no impacts in the pre-reform years, providing evidence in support of the parallel trends identifying assumption. Using monthly data, we show that the decline in premiums occurs precisely in the first month that these changes were permitted by the regulator.

We confirm the robustness of our findings by estimating difference-in-differences specifications that isolate subsets of the identifying variation. We obtain similar estimates when we isolate variation in the size of payment increases across urban and rural counties with the same pre-BIPA Medicare expenditure, reducing concerns that differential medical cost growth rates across high- and low-spending areas are biasing our results. We obtain similar estimates when we use complementary variation in the size of payment increases within the sets of urban and rural counties, reducing concerns about bias from separate urban and rural time trends.

The second part of the paper investigates why consumers receive only half of the marginal surplus from this increase in payments.[5] Drawing on prior work by Einav, Finkelstein and Cullen (2010) and Mahoney and Weyl (Forthcoming), we build a model that illustrates that the observed incomplete pass-through could potentially be explained by two factors: the degree of advantageous selection in the market and the market power of private MA insurance plans. If there is substantial advantageous selection into MA, then private plans will not pass through the increased payments in reduced premiums because lower premiums will attract enrollees who are differentially more costly

[3]Our preferred baseline 45 cents pass-through estimate comes from looking at the third year after the reform. Because we find that the pass-through estimates level off between years two and three after the reform, we focus on the estimate from the third year after the reform, as this estimate seems to represent the medium-run effect.

[4]Our product characteristics data include information on physician and specialist co-pays and supplemental benefits such as drug, dental, vision and hearing aid coverage. To ensure that our estimates capture pass-through on all relevant margins, we additionally analyze survey data from Medicare with subjective quality assessments of every Medicare Advantage plan. We estimate a precise zero effect on these subjective quality evaluations, indicating that there was no pass-through on unobservable plan quality.

[5]As shown in Weyl and Fabinger (2013), the incidence or ratio of consumer to producer surplus is given by $I = \frac{CS}{PS} = \frac{\rho}{1-(1-\theta)\rho}$ where ρ is the pass-through rate and $\theta \in [0,1]$ is an index of market power. Our baseline estimate of $\rho = 0.54$ allows us to bound the incidence between 0.54 and 1.17 and implies that consumers receive no more than approximately half the marginal surplus from the market.

on the margin. If firms have market power, then they may not face competitive pressure to pass through increased payments into lower premiums or more generous benefits.

We use the same difference-in-differences variation to estimate the degree of selection into MA. The BIPA-induced variation in payments creates variation in premiums and thereby generates quasi-exogenous variation in MA coverage. We use this variation in coverage, combined with administrative data on the near-universe of Traditional Medicare beneficiaries, to estimate the slope of the industry cost curve. Our estimates indicate there is limited advantageous selection into MA on the margin we study. Within our theoretical framework, the estimates imply that advantageous selection would reduce pass-through under the benchmark of perfect competition to 85%. Alternatively put, of the combined 46 cents in payments that is not passed through to beneficiaries, selection can account for 15 cents or about one-third of the shortfall.

We then provide evidence that suggests insurer market power is an important determinant of incomplete pass-through. Using our difference-in-differences variation, we estimate premium pass-through rates of 74% in the most competitive markets compared to 13% in the markets with the least competition. This heterogeneity is statistically significant and is robust to measuring market concentration by the pre-reform number of insurers in each market and the pre-reform insurance market Herfindahl-Hirschman Index (HHI).[6]

Our research contributes to a rich literature in public finance that examines the pass-through of government taxes and subsidies in health insurance. This includes work on health insurance mandates (Hackmann, Kolstad and Kowalski, 2015), physician and hospital payments (Clemens and Gottlieb, 2014; Dafny, 2005), Medicaid premium subsidies (Dague, 2014), and payments to Medicare Part D plans (Carey, 2014). In addition, our research complements a prior literature that uses discrete choice models to examine the relationship between market power and welfare in Medicare Advantage (Town and Liu, 2003; Dunn, 2010; Lustig, 2010; Curto et al., 2015).[7] Our finding of an average premium pass-through of 45%, with rates approaching 74% in the most competitive counties, sug-

[6]While we do not find evidence that BIPA affected market structure, splitting the sample by pre-BIPA market power is appropriate because the increase in payments could, at least in principle, affect the number of firms in each county.

[7]Our paper is also related to the broader literature on MA including Cawley, Chernew and McLaughlin (2005) who investigate the impacts of MA payment changes in 1997 on MA plan availability, Gowrisankaran, Town and Barrette (2011) who estimate the mortality effects of MA enrollment and MA drug coverage, and Duggan, Starc and Vabson (2014) who use cross-sectional variation in capitation payments between urban and rural counties to examine pass-through of Medicare Advantage subsidies. While we do not specify micro-foundations for consumer demand, our estimates of limited price sensitivity complement research by Stockley et al. (2014) on low premium transparency and Nosal (2012) on large switching costs in the MA market.

gests that private markets can efficiently provide Medicare benefits but that not all markets may be competitive enough to achieve this objective.

Our paper also contributes to a literature on selection in Medicare, with Brown et al. (2011) arguing that selection generates overpayments to MA plans and Newhouse et al. (2012) responding that selection has been mitigated by improved risk adjustment and other reforms. Prior studies have investigated selection by examining the cost of individuals who choose to switch from Traditional Medicare to MA or vice versa. Like these papers, we use data on Traditional Medicare costs to estimate selection into MA. Unlike these papers, our approach allows us to estimate selection using plausibly exogenous payment variation (Einav, Finkelstein and Cullen, 2010).[8] Our finding of little advantageous selection suggests that policies that aim to reduce selection, while perhaps worthwhile from a cost-benefit standpoint, would have limited scope to increase pass-through to consumers.[9]

Our estimates of pass-through are directly relevant for the $156 billion in MA payment reductions scheduled to take effect under the Affordable Care Act. Counter to claims made by some commentators, our results predict that the incidence of such payment reductions would fall only partially on Medicare beneficiaries, with a significant fraction of these cuts borne by the supply side of the market.[10,11]

More generally, we view our results as emphasizing the importance of market power in health insurance markets. The delivery of publicly funded health care in the United States has become increasingly privatized over the past 25 years, with Medicare, Medicaid, and the Affordable Care Act exchanges adopting managed competition to varying degrees.[12] Although evaluating the merits

[8]While the prior literature relies on the assumption that switching between MA and Traditional Medicare is unrelated to changes in health status, our study makes no such assumption as we rely on plausibly exogenous variation in prices to identify selection. Another advantage of the present study over the prior literature is that our design allows us to examine all enrollees, new and old. The prior switcher studies cannot examine new enrollees because effects can be estimated only among individuals that have at least one year of history in MA or Traditional Medicare prior to a switch in their coverage.

[9]Our results are not directly comparable to the selection results of Curto et al. (2015), who measure selection as an overall mortality rate difference between Traditional Medicare and MA, conditional on risk scores. This is both because we measure selection in dollars, as a marginal cost curve, and because we estimate selection that is marginal to premium variation, in the spirit of Einav, Finkelstein and Cullen (2010). Understanding selection based on premium variation that is driven by MA payment adjustments is especially policy relevant, as these types of payment adjustments are the primary policy tool—both historically and in current proposals—that are used to induce expansions and contractions of the MA program.

[10]Despite the growth in Medicare Advantage since our period of analysis, many Medicare Advantage markets remain highly concentrated today. The typical MA market (county) during our time period was highly concentrated (with a mean insurer HHI of 5,800 in 2000) and this remains true today (with a mean insurer HHI of 4,800 in 2014). To put this in some perspective, the DOJ thresholds for moderately and highly concentrated markets are 1,500 and 2,500 respectively. As of 2014, 88% of Medicare Advantage markets had insurer HHI values in excess of 2,500.

[11]For examples of opposition to the cuts on the basis that seniors bear the burden, see Millman (2014).

[12]This trend towards private provision extends beyond the context of Medicare. Many state Medicaid programs have transitioned to partial or complete private provision within the last several decades. Kuziemko, Meckel and Rossin-Slater

of specific policy proposals is outside the scope of our analysis, our estimates indicate that efforts to make insurance markets more competitive may be key to increasing consumer surplus in such settings.

The remainder of the paper proceeds as follows. Section 2 provides background information on MA payments and describes our data. Section 3 presents our empirical strategy. Section 4 reports estimates of pass-through. In Section 5 we present the model that allows us to investigate the determinants of pass-through. Section 6 empirically evaluates the role of selection in explaining incomplete pass-through. In Section 7 we examine the relationship between pass-through and market concentration. Section 8 concludes.

2 Background and Data

2.1 Medicare Advantage Payments

Private Medicare Advantage (MA) insurance plans are given monthly capitated payments for each enrolled Medicare beneficiary, equal to a base payment multiplied by the enrollee's risk score. Insurers can supplement these payments by charging premiums directly to enrollees. Base payments to MA plans are determined at the county level and are somewhat complex, reflecting the accumulation of legislation over the life of the program. Payments were originally intended to reflect the costs an individual would incur in Traditional Medicare (TM). Prior to 2001, base payments were largely determined by historical average monthly costs for the TM program in the enrollee's county of residence.[13]

Our source of identifying variation arises from the 2000 Benefits Improvement and Protection Act (BIPA). The historical context for BIPA was a contraction in the MA program in the late 1990s following the 1997 Balanced Budget Act (BBA). The BBA was designed to reduce variation in base payments across counties with different levels of Medicare spending. The legislation put in place a payment floor that increased base payments in counties with the lowest TM costs and mechanisms to

(2014) examine the transition to private provision within the Texas Medicaid program, and they find evidence that black-Hispanic infant health disparities widen as a result of this transition.

[13]Prior to 1998, MA capitation payments were set at 95% of the Average Adjusted Per Capita Cost (AAPCC), which was an actuarial estimate intended to match expected TM expenditures in the county for the "national average beneficiary." Beginning in 1998, county base payments were updated via a complex formula created by the Balanced Budget Act (BBA) of 1997. Specifically, plans were paid the maximum of (i) a weighted mix of the county rate and the national rate ("the blend"), (ii) a minimum base payment level implemented by BBA, and (iii) a 2% "minimum update" over the prior year's rate, applying in 1998 to the 1997 AAPCC. See Appendix A.1 for additional details.

limit the growth of payments in counties with high TM costs. As a result of this reform, enrollment growth in the MA program slowed, and between 1999 and 2000 the number of MA enrollees shrunk for the first time since the program's inception in 1985. Under pressure from insurers to reverse the payment cuts, Congress passed BIPA in December of 2000 (Achman and Gold, 2002).[14]

BIPA implemented two floors for county base payments in March 2001. These floors varied with whether the county was rural or urban and were scheduled to update over time.[15] Counties already receiving base payments in excess of the floors received a uniform 1% increase in their base payment rates in March 2001. Let j denote counties and t denote years. Base payments b_{jt} are given by

$$b_{jt} = \begin{cases} \widetilde{c}_{jt} & \text{if } t < 2001 \\ \max\left\{\widetilde{c}_{jt},\ \underline{b}_{u(j)t}\right\} & \text{if } t \geq 2001, \end{cases} \tag{1}$$

where $\widetilde{c}_{jt}$ is the base payment absent the BIPA floors and $\underline{b}_{u(j)t}$ is the relevant BIPA payment floor, which depends on the county's urban status, $u(j)$. In our main analysis, we use premium data from July of each year. Because BIPA modified payments in March 2001, and plans received special permission to adjust premiums and benefits packages in February 2001 (Committee on Ways and Means, 2004), we assign 2001 as the first post-reform year for all of our variables. We discuss the regulations that affected the precise timing of plan responses in more detail in Appendix A.2.

The final capitation payment received by MA insurers is determined by multiplying the county base payment rate by an individual risk adjustment factor to account for the relative costliness of MA versus TM enrollees. Prior to 2000, this adjustment was done using demographic information: age, sex, Medicaid status, working status, institutionalization status, and disability status. From 2000 to 2003, the risk adjustment formula additionally placed a small weight on inpatient diagnoses. Overall, the risk adjustment done prior to 2004 explained no more than 1.5% of the variation in medical spending (Brown et al., 2011).[16] Extensive risk adjustment of MA capitation payments was introduced in 2004 (see Brown et al., 2011; McWilliams, Hsu and Newhouse, 2012), after our study

[14] The bill was introduced in the House in October of 2000 in close to its final form and passed in December. According to Achman and Gold (2002), Congress passed BIPA in response to pressure from MA insurers to undo the cost-control provisions of BBA 1997, which constrained MA payment growth.

[15] Counties are designated urban if they are associated with an MSA with a population of 250,000 or greater. Rural counties are those not associated with an MSA, or associated with an MSA below the threshold.

[16] The purpose of this risk adjustment was not to correct for geographic variation in illness or utilization, which is fully captured in the local county average, but to address sorting between TM and MA. Following the prior literature, we focus solely on the demographic risk adjustment in our analysis.

period.

The Centers for Medicare and Medicaid Services (CMS) constructs the risk adjustment factors to equal 1.0 on average across the entire Medicare population. Because the risk adjustment factor averages 0.94 in our estimation sample, in the analysis that follows we multiply all county base payments by 0.94 to more accurately track average payments to plans.[17] To be consistent, we normalize the risk scores to have a mean of 1.0 in our sample when, in Section 6, we separately and explicitly estimate selection between MA and TM.

2.2 Data

We focus on the 7-year time period from 1997 to 2003, which provides us with 4 years of data from before the passage of BIPA and 3 years of data after the bill was signed into law. We end our sample in 2003 to avoid confounding factors introduced by the 2004 implementation of the Medicare Modernization Act of 2003 (MMA), which reformed the capitation payment system extensively.[18]

Most of our analysis relies on publicly available administrative data on the MA program. We combine data from several sources: MA rate books, which list the administered payment rates for each county in each year; the annual census of MA insurer contracts offered by county; county-level MA enrollment summaries; and plan premium data.[19] For 2000 to 2003, we are able to obtain information on the benefits (e.g., copayments, drug coverage) offered by each plan.[20] We supplement the data on plan characteristics with data on subjective consumer evaluations of all MA plans from the Consumer Assessment of Health Plans Survey (CAHPS) and clinical quality of care measures from Healthcare Effectiveness Data and Information Set (HEDIS). These data are available from 1999 to 2003.

To investigate the importance of selection, we use administrative data on costs and demographics

[17]The average risk score in our estimation sample is different than 1.0 for two primary reasons. First, our estimation sample excludes individuals that qualify for Medicare through Social Security Disability Insurance. Second, only a subset of the variables the regulator uses for calculating the demographic risk score are available to us in the administrative data. In particular, the regulator uses age, sex, Medicaid status, working status, and institutionalized status, and we do not have information on either working status or institutionalized status. Thus, we calculate demographic risk scores using information on age, sex, and Medicaid status, assuming individuals are non-institutionalized and non-working.

[18]MMA 2003 changed the formula by which the base payment is calculated substantially. In addition, the act introduced meaningful risk-adjustment applied on top of the base payment rate to calculate the overall capitation payment. Several prior papers examine the effects of various aspects of MMA 2003 reform including Brown et al. (2011), McWilliams, Hsu and Newhouse (2012), and Woolston (2012).

[19]Plan premium sources vary by year and include the Medicare Compare database, the Medicare Options Compare database, and an Out of Pocket Cost database provided by CMS.

[20]These detailed descriptions of plan benefits are sometimes referred to as Landscape Files or Plan Services Files.

for the near-universe of Medicare beneficiaries. We use the CMS Beneficiary Summary File from 1999 to 2003, which includes information on spending for the universe of Traditional Medicare beneficiaries. Additionally, we use the CMS Denominator File from 1999 to 2003, which provides demographic information for all Medicare beneficiaries.[21]

We conduct our analysis on a county-year panel dataset. We weight county-level observations by the number of Medicare beneficiaries in each county so that our findings reflect the experience of the average Medicare beneficiary. To construct county-level outcomes from plan-level data, we weight plan level attributes by the plan's enrollment share in that county. We inflation-adjust all monetary variables to year 2000 using the CPI-U.

Table 1 displays summary statistics for the pooled 1997 to 2003 sample. Panel A shows values for the full panel of 3,143 counties. Panel B shows summary statistics for plan characteristics, which require us to restrict the sample to county-years that have at least one MA plan. In 2000, the year just prior to the enactment of BIPA, MA plans were available in 680 out of 3,143 counties. These 680 counties collectively contain 67% of all Medicare beneficiaries (19.4 million individuals), and, by definition, 100% of Medicare beneficiaries who reside in a county with an available MA plan. In the pooled 1997-2003 panel, MA plans were available in 4,262 out of 22,001 county-years.[22] While not all Medicare beneficiaries had access to MA from 1997-2003, 64% of all Medicare beneficiaries resided within one of the counties in our primary estimation sample during our period of analysis. In Section 4, we show our source of identifying variation does not have a meaningful effect on entry or exit of counties from our primary estimation sample (i.e., county-year observations with at least one MA plan). Nevertheless, Appendix A.8 replicates all our analyses using the balanced panel of counties with at least one plan in each year between 1997 and 2003, and we show that the results are very similar.

Panel A shows that base payments average \$491 per month for all counties but range from \$223 to \$778 per month across the sample. Approximately 64% of Medicare beneficiaries live in a county with at least one plan. MA plans enroll 19% Medicare beneficiaries on average, although counties

[21] We accessed these data through the National Bureau of Economic Research. Pre-1999 data are not available through the data re-use agreement with CMS.

[22] Relative to the entire Medicare program, our effective sample size is much larger than the number of counties alone would suggest because counties served by an MA plan are on average much larger than counties without an MA plan: counties served by an MA plan during our time period have 30.3 thousand Medicare beneficiaries on average while counties without an MA plan have 4.0 thousand Medicare beneficiaries on average. Throughout the analysis, we weight county-year observations by the number of Medicare beneficiaries represented by the observations.

with the highest MA penetration rates have enrollment rates close to 70%. In the average county, TM beneficiaries cost $487 per month.

Panel B restricts the sample to counties with at least one plan. Premiums average $23 per month and vary substantially. The minimum premium within a county averages $15 per month and the maximum averages $32. Copayments for physician and specialists visits average $8 and $16, respectively. Approximately 70% of plans offer drug and vision coverage, 28% of plans offer dental coverage, and 38% cover hearing products. Beneficiaries in the restricted sample can choose among 2.3 plans on average, and enrollment is higher with an MA penetration rate of 29%. Average TM costs, at $522 per month, are somewhat higher as well.

3 Research Design

In this section we present the research design we use to examine the effects of the Benefits Improvement and Protection Act (BIPA). We start by showing descriptive evidence of the change in payments and then present our econometric model.

3.1 Identifying Variation

The top panel of Figure 1 plots payments for each county in the year before (x-axis) and after (y-axis) the BIPA payment floors came into effect. The bottom two panels plot histograms of the 2000 base payments, weighted by the county's Medicare population, for all counties (middle panel) and for counties with an MA plan in at least one year of the 1997-2003 study period (lower panel). The figure shows that BIPA led to a sharp increase in payments for a large share of counties, with urban counties having their base payment rates raised to a minimum of $525 per month and rural counties having their base payment rates raised to a minimum of $475 per month.

Figure 1 also illustrates the two key sources of variation that we use in our analysis. The first source of variation arises from the fact that counties with the same base payments prior to BIPA received different payment increases depending on their urban or rural status, with urban counties receiving increases of $50 per month more than rural counties with the same pre-BIPA base payment level. The second source of variation arises from the fact that counties with the same urban or rural status received different payment increases depending on their pre-BIPA base payment level.

For example, among urban counties affected by the floor, those with lower pre-BIPA base payments received relatively larger payment increases than those with higher pre-BIPA base payments.

Figure 2 presents maps that illustrate the variation. The shading in this figure corresponds to the magnitude of the treatment: the difference between the applicable payment floor and the base rate that would have applied absent the BIPA reform. This is the "distance-to-floor" variable that we define more precisely below. Counties are binned according to their tercile of distance-to-floor, and we separately map rural counties (Panel A) and urban counties (Panel B). Darker shading indicates a higher distance-to-floor (i.e. a larger payment shock), and counties for which the floors were not binding are shaded white. These maps show that the implementation of the BIPA payment floors, which were binding for 72% of counties, provides us with a large and geographically diverse source of identifying variation.[23,24]

Table 2 provides some basic statistics on the increase in payments. On average, the payment floors led to a 14.1% payment increase in affected rural counties and a 16.1% increase in affected urban counties. There was substantial variation, for example, with the bottom quartile of urban floor counties receiving a payment increase below 8.8% and the top quartile receiving an increase above 22.7%.

3.2 Econometric Model

We examine the effects of this payment change using a difference-in-differences research design that compares outcomes across counties that were differentially exposed to the BIPA payment floors. Let j denote counties and t denote years. We measure exposure to BIPA with a distance-to-floor variable, Δb_{jt}, which isolates the increase in payments solely due to the payment floors:

$$\Delta b_{jt} = \max\left\{\underline{\widetilde{b}}_{u(j)t} - \widetilde{c}_{jt}\,, \quad 0\right\}, \tag{2}$$

where $\widetilde{c}_{jt}$ is the monthly payment in the absence of the floor and $\underline{\widetilde{b}}_{u(j)t}$ is the relevant urban or rural payment floor. We define the instrument in all of the years in our sample so we can test for spurious responses prior to BIPA and any phased adjustment after the law comes into effect.

[23]In Appendix Figure A1, we show that this variation spans counties of varying population sizes. Overall, 53.7% of counties with an MA plan received an increase in payments. The figure shows that the percentage of "treated" counties is fairly stable across the distribution of county sizes.

[24]Appendix Figure A2 shows the baseline maps from Figure 2 along with an additional set of maps that conditions on the sample of counties with an MA plan in at least one year of the 1997-2003 study period.

Post-BIPA, we observe the actual county base payment but not the payment in the absence of the floor. During the post-period, non-floor counties received a 2% update each year. Therefore, to calculate counterfactual payments for floor counties, $\widetilde{c}_{jt}$, in the post-BIPA period, we simply update the pre-BIPA payments that we observe by 2% each year:[25]

$$\widetilde{c}_{jt} = \begin{cases} c_{jt} & \text{if } t \leq 2001 \\ c_{j,2001} \cdot 1.02^{(t-2001)} & \text{if } t > 2001, \end{cases} \tag{3}$$

where c_{jt} is the county base payment that we observe in the pre-BIPA period. Similarly, floors are observed in the post-BIPA period only. The law specified that floors be increased by 2% each year.[26] We define counterfactual floors, $\widetilde{\underline{b}}_{u(j)t}$, in the pre-BIPA period by deflating the 2001 floor by 2% per year:

$$\widetilde{\underline{b}}_{u(j)t} = \begin{cases} \underline{b}_{u(j),2001} \cdot 1.02^{(t-2001)} & \text{if } t < 2001 \\ \underline{b}_{u(j)t} & \text{if } t \geq 2001, \end{cases} \tag{4}$$

where $\underline{b}_{u(j)t}$ is the base payment floor that we observe during the post-BIPA period.

Our baseline econometric model is a difference-in-differences specification that allows the coefficient on the distance-to-floor variable, Δb_{jt}, to flexibly vary by year. Letting y_{jt} be an outcome in county j in year t, our baseline regression specification takes the form

$$y_{jt} = \alpha_j + \alpha_t + \sum_{t \neq 2000} \beta_t \times I_t \times \Delta b_{jt} + f(X_{jt}) + \epsilon_{jt}, \tag{5}$$

where α_j and α_t are county and year fixed effects, $f(X_{jt})$ is a flexible set of controls discussed in more detail below, and ϵ_{jt} is the error term. The β_ts are the coefficients of interest, and we use the summation notation to make explicit that separate coefficients are estimated for each calendar year. We normalize $\beta_{2000} = 0$ so that these estimates can be interpreted as the change in the outcomes relative to year 2000 when BIPA was passed. We consider β_{2003} to be our preferred estimate because the three-year horizon allows us to capture medium-run effects of the change in payments.

[25] Year 2001 is unique in that we observe both c_{jt} and $\underline{b}_{u(j)t}$, due to the implementation of the floors in March of that year. In our analysis, year 2001 always refers to the level of payments for March through December 2001. Since counties received an additional one-time 1% increase in March 2001, we define $c_{j,2001}$ as inclusive of this increase.

[26] There was an exception in the law for when medical inflation was particularly high, in which case the floors were updated by a larger amount. See Appendix A.1 for full details.

The identifying assumption for this difference-in-differences research design is the parallel trends assumption: in the absence of BIPA, outcomes for counties that were differentially affected by the payment floors would have evolved in parallel. We take two approaches to assess the validity of this assumption. Our first approach is to plot the β_t coefficients over time. This approach allows us to visually determine whether there is evidence of spurious pre-existing trends and to observe any anticipatory or delayed response to the BIPA payment increases.[27]

Our second approach is to estimate specifications that isolate the two key subsets of our identifying variation, each addressing a different class of potential confounders. Pre-BIPA base payments are not randomly assigned and reflect historical FFS costs, raising the possibility that time trends in relevant characteristics like population health, market structure, and healthcare spending could be correlated with the distance-to-floor. We address this potential concern by estimating an alternative specification which isolates variation in distance-to-floor due to urban or rural status while controlling for differential trends in the outcome variable by pre-BIPA base payments. Specifically, we include as controls quartiles of the base payment in year 2000 interacted with year indicators.[28] With this approach, the estimates are largely identified by differences in the payment increases between urban and rural counties with the same pre-BIPA base payments.

To isolate the complementary variation, we estimate a separate specification that includes as controls the urban status of the county interacted with year indicators. This complementary approach controls for differential time trends across urban and rural counties, and the estimates are identified by differences in the size of the payment increase within the sets of urban and rural counties.[29]

A recent paper by Duggan, Starc and Vabson (2014), conducted in parallel to our study, uses cross-sectional variation in capitation payments between urban and rural counties to estimate pass-through in MA. Using data from the post-BIPA time period, the authors estimate a premium pass-

[27] Our primary premium pass-through analysis is over-identified in the sense that we have four years prior to the reform of pre-period. During the period prior to the reform, 1997-2000, plots of the pre-reform coefficients reveal no evidence that counties differentially exposed to the reform had differential trends in premiums (see Figure 4). In addition, we report in the appendix supplemental monthly analysis that zooms into the period just surrounding the implementation of the reform. This additional evidence demonstrates that premiums sharply decrease in the first month that insurers were allowed to adjust premiums following the reform. (See Appendix Section A.2 for full analysis.)

[28] In principle, perfectly isolating the variation due to urban status would require completely non-parametric pre-BIPA payment rate $\times$ year fixed effect interactions. The choice of quartiles is a compromise between flexibility and over-parameterizing the model.

[29] This alternative specification controls flexibly for differential trends in the outcome variable across urban and rural areas by the inclusion of both the year fixed effects and urban $\times$ year fixed effects. These allow for fully non-parametric over-time differences in outcomes across urban and rural counties. In other words, the estimates from this specification come from isolating the variation within counties with the same urban or rural status.

through rate of zero, although their standard errors do not allow them to reject a relatively wide range of parameters (including our baseline estimate of 45% pass-through below). In contrast, our difference-in-differences strategy allows us to control for county fixed effects and to estimate specifications that control for differential time trends across counties. Given the importance of place-specific factors for medical spending (Finkelstein, Gentzkow and Williams, 2014), we see the ability to control for county fixed effects and differential time trends as a major advantage of our strategy.

As discussed in Section 2, Congress instituted several earlier payment reforms that affected payments during the pre-period. The most important of these was the payment floor established by the 1997 Balanced Budget Act (BBA) and an additional update to payments for some counties in 2000. To address any correlation between the effects of these payment reforms and BIPA, we explicitly control for these two events in all our regression specifications. We control for the BBA floor by constructing a distance-to-floor measure that is analogous to our BIPA distance-to-floor variable and interacting this variable with year fixed effects for 1998 onward. We control for the 2000 payment increases by constructing a variable defined as the difference between the 2% update and the actual update in 2000 and interacting this variable with year fixed effects for 2000 onward. See Appendix A.1 for more details on these payment changes.

Figure 3 shows the first stage effect of our constructed change in payments variable on actual monthly payment rates. It plots the coefficients on distance-to-floor $\times$ year interactions from the baseline difference-in-differences specifications (Equation 5) with base payments as the dependent variable. Table 3 presents parameter estimates from the corresponding regressions. Column 1 shows estimates from the baseline specification with county and year fixed effects. Column 2 adds controls for the base payment level in the year 2000 interacted with year indicators to isolate variation due to the difference between the urban and rural floor. Column 3 includes as controls an urban indicator interacted with year indicators to isolate variation due to differences in base payments conditional on urban or rural status. Standard errors in all specifications are clustered by county, with the capped vertical bars in the plot showing 95% confidence intervals.

Both the figure and table show that a dollar increase in our distance-to-floor variable translates one-for-one into a change in payments to plans at the county level. This first stage is very precisely estimated, with all specifications yielding a coefficient of 0.987 to 1.002 for each post-BIPA year and with standard errors no larger than 0.005. Because the first stage is one and precisely estimated, in the remainder of the paper, we interpret reduced form effects of distance-to-floor on outcomes, such as premiums and benefits, as resulting from a one-for-one change in monthly base payments.

4 Main Results

In this section, we examine the pass-through of the increase in payments. We start by presenting the effects on premiums. We then examine the pass-through into plan benefits, such as copayments and drug coverage. Finally, we examine impacts on plan availability.

4.1 Pass-Through into Premiums

Figure 4 examines the effect on premiums by plotting the coefficients on distance-to-floor $\times$ year interactions from the baseline difference-in-differences specifications (Equation 5) with county-level mean premiums as the dependent variable. County-level mean premiums are constructed from plan-level data by weighting by the number of enrollees in each plan. Table 4 presents parameter estimates from the corresponding regression, which includes year and county fixed effects. Table 4 also reports parameter estimates from additional specifications that isolate different subsets of the identifying variation described in Section 3. Standard errors in all specifications are clustered by county, with the capped vertical bars in the plots showing 95% confidence intervals.

The dashed horizontal line at zero in Figure 4 indicates no pass-through and the dashed horizontal line at -1 indicates full pass-through, which occurs when a dollar increase in payments translates one-for-one into a dollar decline in premiums. The plot shows no evidence of a trend in the period prior to the Benefits Improvement and Protection Act (BIPA), providing support for our parallel trends identifying assumption. In the first year following implementation, mean premiums decline by 30 cents for each dollar increase in payments and level off at a decline of approximately 45 cents in the third year after the reform. The size of effects in the third year are stable across specifications in Table 4, ranging from 32 to 45 cents—not statistically different from each other, and in all cases statistically different from zero (no pass-through) and from one (full pass-through). Difference-in-differences plots corresponding to the alternative specifications in columns 2 and 3 of Table 4 are displayed in Figures A3 and A4. Similar to the baseline result in Figure 4, these plots show no evidence of a differential trend in premiums prior to the reform. Our preferred estimate of mean pass-through is 45 cents, which is the 2003 estimate from the baseline specification shown in column 1.

Appendix Figure A5 illustrates the effect of this change in monthly payments on the median premium (Panel A), minimum premium (Panel B), and maximum premium (Panel C). Since the typical county has between two and three plans, these statistics provide an exhaustive characterization of the distribution of premiums in the typical county. The effects on these other statistics are similar to the effect on the mean, with the plots showing no evidence of a pre-BIPA effect and a sharp decline following implementation of the payment floors. The point estimates for these other statistics, shown in Appendix Table A1, are similar in magnitude to the mean effect, with the 2003 estimates ranging from 37 to 49 cents for the baseline specification. Like the effect on the mean, the results are robust to specifications that isolate different subsets of the identifying variation.

One factor that could affect our interpretation of the premiums and benefits pass-through estimates is the fact that plans could not set negative premiums during our time period.[30] In principle, a plan that was constrained from further reducing premiums would have an incentive to pass-through higher payments in the form of more generous benefits. Relative to an unconstrained setting, this would bias downward our estimate of premium pass-though and bias upward our estimate of pass-through into benefits, but might not impact on our combined pass-through estimate. In Appendix Section A.3, we examine this potential issue by estimating Tobit specifications that account for insurers' inability to set negative premiums. The magnitude of the Tobit estimates are very similar to, and statistically indistinguishable from, our baseline non-Tobit estimates, confirming that our baseline results are not driven by this feature of the market.

To summarize the premium pass-through results, we find that mean premiums decline by 45 cents for every dollar of increased monthly payments at 3 years following the reform. This result is robust to alternative specifications that isolate different subsets of our identifying variation, to other statistics describing the premium distribution (median, minimum, and maximum), and to Tobit specifications that explicitly account for the fact that plans could not give rebates (charge a negative premium) during our sample period. Appendix A.2 presents additional analysis using monthly premium data and a tight window around the passage of BIPA that illustrates that the decline in

[30] MA was changed after our sample period to allow plans to offer "rebates" that in effect operate as negative premiums. Examining data from this time period, Stockley et al. (2014) argue that firms do not pass-through higher payments in the form of rebates because the "Medicare Plan Finder" website does not prominently display this information, reducing the salience of these premium rebates at the time of purchase.

premiums occurs precisely in the first month that these changes were permitted by the regulator.[31]

4.2 Pass-Through into Benefits

In addition to lowering premiums, plans may have responded to the increased payments by raising the generosity of their coverage.[32] In the standard model of insurance demand, such a change in plan generosity would operate through an income effect. Consumers facing lower premiums would be richer and thus might demand more or less generous insurance coverage.[33]

We investigate pass-through on benefits using data on the main MA plan characteristics marketed to Medicare beneficiaries at the time of enrollment. Specifically, we examine the effect of BIPA on the mean county-level copayments for physician and specialist visits and the percentage of plans providing coverage for prescription drugs, dental, vision, and hearing aids. Figure 5 plots the coefficients on distance-to-floor $\times$ year interactions from difference-in-differences specifications (Equation 5) with measures of plan benefits as the dependent variable. To aid interpretation, we scale the coefficient on the distance-to-floor variable by \$50, which is approximately 10% of the \$511 mean pre-BIPA base payment. We have information on plan benefits for 2000 to 2003 and therefore only have one year of pre-BIPA data. These data are sufficient to identify the effect of BIPA but do not allow us to perform falsification tests for pre-existing trends, warranting more caution in interpreting the results. Table 5 displays parameter estimates from the corresponding difference-in-differences regressions where the coefficient is similarly scaled by \$50. The table shows coefficients from the baseline regression specification, with Appendix Table A2 showing the specifications that isolate different subsets of the identifying variation. Standard errors in all specifications are clustered by county and the capped vertical bars in the plots show 95% confidence intervals.

Panels A and B of Figure 5 show that the increase in payments had a sharp effect on mean personal physician and specialist copayments. By 2003, the \$50 increase in monthly payments reduced

[31] After the passage of BIPA in December 2000, the regulator required plans to submit new premiums and benefits by January 18, 2001, with the new premiums and benefits effective beginning February 2001 (Committee on Ways and Means, 2004). In Appendix Figure A6, we display a monthly sequence of our difference-in-differences coefficient estimates for premiums. The monthly plot shows a sharp drop in premiums in February 2001, consistent with plans responding in premium-setting at the first opportunity. We discuss the timing in full detail in Appendix A.2.

[32] In addition to varying premiums, insurers in the MA market often vary plan benefits such as copays and drug coverage across the different geographic markets they serve. Appendix A.4 provides more details on the within-insurer geographic variation in benefits and premiums.

[33] In the CARA specification that is used in much of the literature, there are no income effects, and we would therefore predict no change in plan generosity. Given that the premium changes are small relative to income, even in specifications with non-constant risk aversion, we might expect only small changes in plan generosity.

physician copayments by \$2.63 on a pre-BIPA base of \$7.29 and reduced specialist copayments by \$3.13 on a pre-BIPA base of \$11.13. The effects are highly statistically significant but modest in economic magnitude. The average Medicare beneficiary had 8 combined physician and specialist visits per year or two-thirds of a visit per month, implying that the \$50 increase in monthly payments reduced copayment spending by approximately \$2 per month.[34]

Panels C to F of Figure 5 show the effects on the percentage of plans offering drug, dental, vision, and hearing aid coverage. As before, the effects are scaled to a \$50 increase in monthly payments. The plots show that the increased payments have no effect on drug, dental, and vision coverage but a relatively large effect on the percentage of plans offering hearing aids.[35] By 2003, the parameter estimate for the effect on hearing aids, shown in column 6 of Table 5, indicates that the \$50 increase in payments raised the share of plans offering hearing aids by 23.8 percentage points on a base of 42.6%. Appendix Table A2 shows that the benefits effects are stable across our alternative specifications.

To quantify the actuarial value of the change in benefit generosity, we combine these estimates with data on utilization and payments from the 2000 Medical Expenditure Panel Survey (MEPS), restricting the sample to individuals who are 65 or older. For dental, vision, hearing aids, and drug coverage, we calculate the actuarial value of these benefits as the monthly costs paid by the insurance provider.[36] For copayments, we calculate the actuarial value of the insurer's share of costs by taking the negative of the copayment amount multiplied by the monthly number of visits.[37]

Figure 6 plots effects of a \$1 increase in payments on this measure of the actuarial value of benefits. The vertical axis offers the same pass-through interpretation as in the premium figures, where a coefficient of 1 corresponds to a dollar increase in plan benefits for a dollar increase in plan subsidies due to BIPA. Pass-through is small. The point estimate for 2003, shown in column 7 of the table, indicates a pass-through rate of 9 cents on the dollar and is marginally statistically significant with a p-value of 0.05.[38] Specifications that isolate alternative subsets of the identifying variation, shown in

[34]The number of provider visits is calculated using the 2000 Medical Expenditure Panel Survey (MEPS).

[35]Gowrisankaran, Town and Barrette (2011) find that higher MA payment rates increased the probability that plans offered drug coverage, using data spanning 1993—when drug coverage rates were low—through 2000. By our last pre-reform year, most plans had already adopted drug coverage, possibly accounting for our finding of no incremental effect of the reform in 2001.

[36]In particular, we estimate category-specific coinsurance rates among those MEPS respondents that report supplemental coverage. We then multiply these category-specific rates by the unconditional total monthly spending in each category, generating actuarial values of coverage for each supplemental benefit.

[37]By the envelope theorem, we can calculate the value to consumers of a small reduction in copayments without needing to account for any increase in medical utilization caused by the reduced cost-sharing. In addition, since medical utilization is relatively inelastic, any changes in utilization are likely to be small.

[38] If the actuarial value of the increase in plan benefits is larger in high out-of-pocket spending states of the world (where

Appendix Table A2, confirm the robustness of the finding that pass-through into benefits is at most small and in some specifications statistically insignificant.

One potential concern with all pass-through papers is that firms may change product characteristics that the researcher does not observe. We think this is a relatively minor concern in our setting for two reasons. First, we see all of the product characteristics (e.g., premiums, copayment, vision coverage) that the consumer sees when purchasing the plan. These are the characteristics that plans should be most likely to change as they are the most salient plan features and thus the most likely to affect enrollment. Second, for every Medicare Advantage plan, we also have data on the subjective plan evaluations of enrolled consumers. These survey data allow us to investigate unobservable (to the econometrician) changes in plan quality that might not be picked up by our analysis of product characteristics. In Appendix A.7, we show that our identifying variation has a precisely estimated zero effect on these evaluations and other measures of plan quality, including measures of clinical care quality and beneficiary-reported quality of care. This finding is consistent with other research which shows limited pass-through into plan characteristics that are not easily observed (e.g., Stockley et al., 2014; Agarwal et al., 2015, 2014).

Taken together, the premiums and benefits results for 2003 yield a combined pass-through estimate of 54 cents on the dollar. A 95% confidence interval allows us to rule out a combined pass-through effect outside the range of 37 cents to 71 cents.[39]

4.3 Plan Availability

If there are fixed costs of entry, then the increase in payments might have had an effect on plan availability. Figure 7 plots the coefficients on distance-to-floor $\times$ year interactions from difference-in-differences specifications (Equation 5) with different measures of plan availability as the dependent variable.[40] Table 6 shows the corresponding regression estimates, including alternative specifications

the marginal utility of consumption is higher) than in low out-of-pocket spending states of the world (where the marginal utility of consumption is lower), then the pass-through into benefits might have additional consumption-smoothing value to consumers which is not captured by the baseline actuarial value estimate. Additional analysis in Appendix A.6 illustrates that any additional consumption-smoothing value from the change in plan benefits is small in this setting (roughly 1 cent per dollar).

[39]This confidence interval is constructed by bootstrapping standard errors for the sum of our distance-to-floor coefficients from the premium and actuarial value of benefits regressions. The bootstrap calculation uses 200 random samples of counties drawn with replacement.

[40]There was a possible change to the regulator's reporting between 1999 and 2000 in terms of separating non-local plans in the enrollment files. Non-local plans are those purchased outside of the county in which the enrolled individual is observed, and these plans are characterized by very low enrollment in the county of observation. While unimportant for our main enrollment-weighted outcomes such as premiums, these may generate data artifacts in the pre-period trends of

that isolate different subsets of the identifying variation.

Panel A of Figure 7 shows the effect of a \$50 increase in payments on the percentage of counties with at least one plan. For this analysis, we use the entire balanced panel of county-years with non-missing information on base rates and Medicare beneficiaries during 1997 to 2003. This sample includes 21,504 of 22,001 county-years and more than 99.9% of all Medicare beneficiaries. The plot shows no evidence of an effect on the percentage of counties with at least one plan. The parameter estimates, shown in columns 1 to 3 of Table 6, are similar across alternative specifications.

One potential reason for this lack of an extensive margin effect is that BIPA had only a minor effect on the total revenue that could be earned in marginal counties, mainly because of the small number of Medicare beneficiaries in these areas. In particular, the average county with zero plans in year 2001 had only 4,278 Medicare beneficiaries, compared to an average of 32,172 in counties with at least one plan. This means that although BIPA raised payments by an average \$33 per month in these zero-plan counties, a plan capturing 5% of the Medicare beneficiaries would experience a total revenue increase of only \$84,704, which might not be enough to cause a detectable effect on entry or exit.

While these results are interesting in their own right, the plan existence results also offer reassurance that the identifying variation is not systematically related to entry and exit from our sample. The pattern of the coefficients in Panel A of Figure 7 indicates that changes to the number of counties with an MA plan are unlikely to be a source of bias in our main estimates. However, as a robustness test, we replicate all our analyses using a balanced sample of counties with an MA plan in each year between 1997 and 2003. These estimates, shown in Appendix A.8, are very similar and confirm that selection is not biasing the results.

The increase in payments may have also influenced market concentration within the set of counties that had at least one plan. Panel B of Figure 7 shows the effect of a \$50 increase in payments on the number of plans in each county conditional on there being at least one plan.[41] Panel C shows the effect on the Herfindahl-Hirschman Index (HHI) for the number of plans in each county, again conditional on there being at least one plan. The HHI is the standard measure of market power used for antitrust analysis and is similar to our other dependent variables in weighting plans based on

plan counts.

[41] Appendix Figure A7 includes additional plots examining the effect of the reform on the probability of ≥ 2 MA plans among the sample of counties with ≥ 1 MA plan.

their enrollment shares. These plots show no evidence of an effect of the increased payments in 2001 on these different measures of the number of plans, though the pre-BIPA trends are not completely flat. In contrast, the extensive margin of plan participation in a county (at least one plan) that defines our premium analysis sample is a robust and precisely estimated zero, with flat pre-trends across all specifications. Overall, these results indicate that BIPA did not have a meaningful impact on market concentration, consistent with Duggan, Starc and Vabson (2014) who show that their variation in payments is unrelated to insurer HHI.[42]

5 Model of Pass-Through

In the previous section, we showed that Medicare Advantage (MA) plans pass through approximately half of the increased capitation payments in the form of lower premiums and more generous benefits. In this section, we show that incomplete pass-through can possibly be explained by (i) advantageous selection into MA and (ii) market power among MA insurers and medical providers. To build intuition, we start by presenting simplified graphs that illustrate these potential mechanisms. We then present a model that, under assumptions on the nature of selection and competition, allows us to generate quantitative predictions on the relationship between pass-through and these underlying mechanisms. The model provides a framework for interpreting the empirical evidence that follows.

5.1 Graphical Analysis

Figure 8 presents this graphical analysis. We model demand for MA as linear, and we define the marginal cost of providing an MA plan to an individual as the expected cost of providing medical care net of the capitation payment from Medicare. Within this framework, we can depict the increase in capitation payments under BIPA as a downward shift of the marginal cost curve. Our graphical approach is closely related to that of Einav, Finkelstein and Cullen (2010), who examine selection in a perfectly competitive environment, and Mahoney and Weyl (Forthcoming), who examine the interaction of imperfect competition and selection.

[42]Immediately following the reform, it may have been easiest for firms to adjust premiums, with firm entry/exit decisions developing over a longer timeframe. Nonetheless, we see no meaningful effect on firm entry/exit in the medium-run, three years after the implementation of BIPA.

Panel A of Figure 8 examines the impact of selection on pass-through in a perfectly competitive market. In a perfectly competitive market, firms earn zero profits and the equilibrium is defined by the intersection of the demand and the average cost curves. When there is no selection, firms face a horizontal average cost curve, and a downward shift in the average cost curve translates one-for-one into a reduction in premiums, depicted by the transition from point A to point B in the figure. When there is advantageous selection, average costs are upward sloping as the marginal consumer is more expensive than the average. Panel A illustrates that under advantageous selection an identically sized downward shift in the average cost curve is not fully passed through as firms offset the higher costs of the marginal consumers with higher prices to maintain zero profits in equilibrium, depicted by the shift from point A to point C.

Panel B examines the impact of market power on pass-though in a market with no selection. To simplify the exposition, we consider the extremes of perfect competition and monopoly. As described above, when there is perfect competition and no selection, a downward shift in the marginal cost curve is fully passed through to consumers, moving the equilibrium from point A to point B. The monopolist sets the price such that marginal revenue is equal to marginal cost. With a linear demand curve, this leads to 50% pass-through, shifting the equilibrium from point C to point D in the figure. More generally, Bulow and Pfleiderer (1983) show that the pass-through of a small cost shock is determined by the ratio of the slope of the demand curve to the slope of the marginal revenue curve.

5.2 Model

We build on and generalize this graphical analysis by constructing a model of pass-through in imperfectly competitive selection markets, drawing upon previous work by Weyl and Fabinger (2013) and Mahoney and Weyl (Forthcoming). We direct the reader to these papers for technical details and micro-foundations that support the modeling choices.

Suppose individuals differ in their cost to firms, c_i, demographic risk score, r_i, and willingness to pay for insurance, v_i. Assume that insurance firms provide symmetric, although possibly horizontally differentiated, insurance products. At a symmetric equilibrium, all firms charge the same premium p. Aggregate demand at this price is given by $Q(p) \in [0, 1]$ and represents the fraction of the market with MA coverage. In addition to the premium, firms receive a risk-adjusted capitation payment equal to $b \cdot r_i$, where b is the county base payment. At a symmetric equilibrium, all plans receive enrollees with the same average risk adjustment factor so that average capitation payments to firms are $b \cdot AR(Q)$, where $AR(Q) = \frac{1}{Q}\int_{v_i \geq p^{-1}(Q)} r_i = \mathbb{E}[r_i | v_i \geq p^{-1}(Q)]$, where $p^{-1}(Q)$ is the inverse demand function.

In practice, risk adjustment is normed by the regulator to average to one in the overall Medicare population and is close to one in the MA segment. To avoid carrying extra notation in the derivation, we temporarily consider the case of no risk adjustment ($r_i = 1, \forall\, i$) but fully incorporate this term when presenting the final pass-through equation below.

Total costs for the industry are summarized by an aggregate cost function $C(Q) \equiv \int_{v_i \geq p^{-1}(Q)} c_i$, which is equal to the aggregate medical costs paid by MA plans when the prevailing premium is $p(Q)$. This specification rules out *firm-level* economies or diseconomies of scale, including fixed costs at the firm level.[43] Average costs for the industry are given by $AC(Q) \equiv \frac{C(Q)}{Q}$, and marginal costs are given by $MC(Q) \equiv C'(Q)$. Adverse selection at the industry level is indicated by decreasing marginal costs $MC'(Q) < 0$, and advantageous selection is indicated by increasing marginal costs $MC'(Q) > 0$. For the purposes of our discussion, we limit our attention to cases where $MC'(Q)$ and $AC'(Q)$ have the same sign.[44]

In a perfectly competitive equilibrium, firms earn zero profits and prices are equal to average costs net of payments from Medicare: $p = AC(Q) - b$. At the other extreme, a monopolist chooses the price to maximize profits:

$$\max_{p} \Big[p + b\Big] Q(p) - C(Q(p)). \tag{6}$$

Setting the first-order condition to zero yields the price-setting equation $p = \mu(p) + MC(Q) - b$, where $\mu(p) \equiv -\dfrac{Q(p)}{Q'(p)}$ denotes the standard absolute markup term and $MC(p) - b$ is the marginal (net of capitation payment) cost.

To allow for intermediate levels of competition, Mahoney and Weyl (Forthcoming) introduce a parameter $\theta \in [0, 1]$ that interpolates between the price-setting equations for perfect competition and

[43] This assumption is widely used in the literature (e.g., Einav, Finkelstein and Cullen, 2010; Bundorf, Levin and Mahoney, 2011) and broadly consistent with the structure of the industry. The model does allow for individual-specific loads related to the costs of administering the plan. In the next section, we calculate pass-through empirically restricting the cost of insuring an individual, c_i, to be an affine transformation of claim costs that we observe in the data.

[44] This restriction simply eases the discussion of selection. The derived pass-through equations are equally applicable if this restriction does not hold.

monopoly:

$$p = \theta\Big[\mu(p) + MC(Q)\Big] + (1-\theta)\Big[AC(Q)\Big] - b. \tag{7}$$

The model nests the extremes of perfect competition ($\theta = 0$) and monopoly ($\theta = 1$) along with a number of standard models of imperfect competition. Cournot competition is given by $\theta = 1/n$, where n is the number of firms. Mahoney and Weyl (Forthcoming) show that the model is a reduced-form representation of differentiated product Bertrand competition when $\theta \equiv 1 - D$, where $D \equiv -\dfrac{\sum_{j\neq i} \partial Q_i/\partial p_j}{\partial Q_i/\partial p_i}$ is the aggregate diversion ratio, the share of consumers that firm i diverts from rivals j when it lowers its price.[45]

5.3 Pass-Through

We are interested in how much of an increase in payments is passed through into lower health insurance premiums. For a small change in payments, pass-through is defined as the negative of the total derivative of premiums with respect to the capitation payment: $\rho \equiv -\frac{dp}{db}$. We will say there is full pass-through when $\rho = 1$ and no pass-through when $\rho = 0$. The model can accommodate pass-through greater than one under imperfect competition and some forms of demand, but as we show in Section 7, $\rho \leq 1$ is the empirically relevant case in our setting.[46]

First, consider the case of perfect competition. Setting $\theta = 0$ and differentiating Equation 7 with respect to b yields

$$\rho = \frac{1}{1 - \dfrac{dAC}{dp}}, \tag{8}$$

where we have suppressed arguments for notational simplicity. Under advantageous selection, average costs are decreasing in price $\left(\dfrac{dAC}{dQ} > 0 \text{ and } \dfrac{dQ}{dp} < 0 \Rightarrow \dfrac{dAC}{dp} < 0\right)$ and therefore pass-through is less than one. Consistent with Panel A of Figure 8, even in a perfectly competitive market, part of the increase in capitation payments must go to compensate insurers for costlier marginal enrollees,

[45] The differentiated product Bertrand representation also requires the symmetry assumption that all firms receive a representative sample of all consumers purchasing the product in terms of their cost and that a firm cutting its price steals consumers with a similarly representative distribution of costs from its competitors. See Mahoney and Weyl (Forthcoming) for details.

[46] Full pass-through ($\rho = 1$) is also the maximum in our benchmark case of perfect competition and advantageous selection. This benchmark case is the focus of our empirical analysis in Section 6.

explaining the lack of full pass-through.

In practice, Medicare risk adjusts payments to partially compensate insurers for selection. Incorporating risk rating yields the pass-through equation

$$\rho = \frac{AR}{1 - \left(\frac{dAC}{dp} - b\frac{dAR}{dp}\right)}, \tag{9}$$

which adds two terms to Equation 8 above. The $\left(\frac{dAC}{dp} - b\frac{dAR}{dp}\right)$ term in the denominator measures selection *net of any change in average risk adjustment payments*. The numerator is scaled by AR to reflect the fact that a dollar increase in base payments does not translate into a dollar increase in payments if MA enrollees have non-representative demographic risk $(AR(Q) \neq 1)$. MA enrollees have lower average demographic risk ($AR(Q) < 1$), which slightly lowers the predicted pass-through rate. See Appendix A.10 for a derivation of this pass-through formula.

Our model also provides predictions for pass-through under the more realistic assumption of imperfect competition ($\theta > 0$). Guided by our empirical results that payments have no effect on market structure, we assume that θ is constant.[47] Fully differentiating the pass-through equation yields

$$\rho = \frac{\theta MR + (1-\theta)AR}{1 - (1-\theta)\left(\frac{dAC}{dp} - b\frac{dAR}{dp}\right) - \theta\left(\frac{d\mu}{dp} + \frac{dMC}{dp} - b\frac{dMR}{dp}\right)}. \tag{10}$$

Increasing market power (higher θ) shifts optimal price-setting away from average cost pricing and toward marginal cost pricing, where both costs are net of risk adjustment. As in Equation 9, the net cost terms in the denominator $\left(\frac{dAC}{dp} - b\frac{dAR}{dp}, \frac{dMC}{dp} - b\frac{dMR}{dp}\right)$ are negative under advantageous selection, decreasing the pass-through rate. When there is no selection, the cost terms are zero and the pass-through formula simplifies to $\rho = \frac{1}{1-\theta\frac{d\mu}{dp}}$ and is decreasing in market power for many standard parameterizations of demand. For instance, linear demand implies $\frac{d\mu}{dp} = -1$ and simplifies the pass-through equation to $\rho = \frac{1}{1+\theta}$.[48]

[47] Because our estimates suggest that BIPA had little effect on product characteristics and no effect on market structure, the model takes these factors as given. This allows for a richer treatment of premium pass-through without making the model unnecessarily complex. It is important to emphasize that the aim of our model is to investigate mechanisms behind pass-through in the context of capitation payment changes within the MA market. Broad counterfactuals, such as analyzing the effect of alternative policies aimed at influencing plan entry/exit, are outside the scope of our analysis.

[48] More specifically, pass-through is decreasing in market power when demand is log-concave since $(\log q)'' = \mu'/\mu^2 <$

6 Selection

The objective of this section is to quantify the extent to which advantageous selection can explain our estimates of pass-through. If Medicare Advantage (MA) is advantageously selected, net of risk adjustment, then lower premiums draw in higher cost enrollees, and even a perfectly competitive market cannot pass through the full increase in payments.

6.1 Conceptual Approach

We estimate the reduction in pass-through that could be explained by selection and risk adjustment in a perfectly competitive market. Perfect competition is a natural benchmark because it implies a pass-through rate of one if there were no selection and no risk adjustment. In Appendix A.11 we show that under the assumptions of linear demand and cost curves, the main effects of selection and market power are proportionally separable. Thus, to a first order approximation, we can think about advantageous selection as scaling down the predicted pass-through for any given level of market power.

As shown in Section 5, pass-through in a perfectly competitive MA market is given by

$$\rho = \frac{AR^{MA}}{1 - \left(\dfrac{dAC^{MA}}{dp} - b\dfrac{dAR^{MA}}{dp}\right)}, \tag{11}$$

where AR^{MA} is the average risk adjustment factor, b is the base payment, and $\frac{dAC^{MA}}{dp} - b\,\frac{dAR^{MA}}{dp}$ is the change in the average costs net of any change in average risk adjustment payments. The superscript MA is added to the risk adjustment and cost terms to clearly distinguish these from risk and costs in the Traditional Medicare (TM) population, which we also discuss below. During our study period, risk adjustment was based on demographics, but the same formula could accomodate risk adjustment of any form, including the currently implemented diagnosis-based system or the type of diagnosis and drug utilization-based system being considered by CMS for future implementation.[49]

$0 \iff \mu' < 0$. When $\mu' > 0$, the pass-through rate can be greater than one and is increasing in market power. Fabinger and Weyl (2013) prove that $\mu' < 0$ if demand is linear or if it is based on an underlying willingness-to-pay distribution that is normal, logistic, Type I Extreme Value (logit), Laplace, Type III Extreme Value, or Weibull or Gamma with shape parameter $\alpha > 1$. They show that $\mu' > 0$ if demand is based on a willingness-to-pay distribution that is Pareto (constant elasticity), Type II Extreme Value, or Weibull or Gamma with shape parameter $\alpha < 1$. They show that μ switches from $\mu' < 0$ to $\mu' > 0$ for a log-normal distribution of willingness-to-pay.

[49]The formula in Equation (11) remains the same regardless of risk adjustment details because the term $\frac{dAR^{MA}}{dp}$ measures

We observe the average risk adjustment factor for MA plans in the data and can calculate AR^{MA} directly. Since we observe the risk adjustment factor, we can also estimate $\frac{dAR^{MA}}{dp}$. To do so, we estimate the reduced form effect of base payments on the average risk adjustment factor ($\frac{dAR^{MA}}{db}$) using our main difference-in-differences strategy and then divide by the effect of base payments on premiums ($\frac{dp}{db}$) from Section 4. This yields the effect of a change in premiums on the average risk adjustment factor $\left(\frac{dAR^{MA}}{dp} = \frac{dAR^{MA}/db}{dp/db}\right)$.

Estimating $\frac{dAC^{MA}}{dp}$ is more complicated because we do not observe data on MA costs. To overcome this issue, we follow the prior MA literature (e.g., Brown et al., 2011; Newhouse et al., 2012) and use TM costs to proxy for counterfactual costs under MA. Previous studies show that beneficiaries who switch from TM to MA and vice versa have low costs while in TM relative to other TM beneficiaries and interpret this fact as indicating that MA is advantageously selected. This "switcher" approach identifies selection in a relatively small sample of switchers and relies on the assumption that the choice of MA versus TM is exogenous to changes in health. In contrast, our strategy measures selection in a larger sample of beneficiaries that includes new enrollees, and our estimates are identified using plausibly exogenous variation. Since our identifying variation in payments affects premiums, we can use insights from Einav, Finkelstein and Cullen (2010), described below, to trace out the cost curve facing insurers and directly quantify the degree selection into MA.

Let $Q^{TM} = 1 - Q^{MA}$ denote the fraction of the market with TM coverage, and let AC^{TM} denote average TM costs. Assume (i) the costs of covering a given individual in MA and TM are proportionally constant so that $\frac{c_i^{MA}}{c_i^{TM}} = \phi$, $\forall i$, and (ii) the market average cost curves for both TM and MA are linear in quantity and therefore have a constant slope. These assumptions imply that the slopes of MA and TM average cost curves are of opposite sign and proportional:[50]

$$\frac{dAC^{MA}}{dQ^{MA}} = -\phi \, \frac{dAC^{TM}}{dQ^{TM}} \,. \tag{12}$$

This result, combined with the fact that a change in premiums has an equal and opposite effect on

only how average risk adjustment payments vary with premiums. Thus the denominator term in parentheses captures the slope of the insurer cost curve net of risk adjustment payments, whatever form those payments take. We also note that because premiums are required to be uniform within a market, the appropriate unit of analysis for selection on premiums would be the entire local market—even in a setting where risk adjustment is diagnosis-based. See Geruso and Layton (2017) for a full discussion of this point in the context of selection and risk adjustment in the ACA Exchanges.

[50] A proof is provided in Appendix A.12. Intuitively, the slopes of the MA and TM average cost curves are proportional because linearity implies that the slope of the average cost curves are half the slope of the marginal cost curves, and marginal costs are assumed to be proportional between MA and TM.

MA and TM quantity $\left(\frac{dQ^{MA}}{dp} = -\frac{dQ^{TM}}{dp}\right)$, implies that an increase in premiums has effects on TM and MA average costs that are of the same sign and proportional:[51]

$$\frac{dAC^{MA}}{dp} = \frac{dAC^{MA}}{dQ^{MA}} \frac{dQ^{MA}}{dp} = \left(-\phi \frac{dAC^{TM}}{dQ^{TM}}\right)\left(-\frac{dQ^{TM}}{dp}\right) = \phi \frac{dAC^{TM}}{dp}. \tag{13}$$

Intuitively, advantageous selection into MA implies that marginal enrollees are high cost relative to the MA average and low cost relative to the TM average. Therefore, if a decrease in MA premiums draws more individuals into MA and increases average MA costs, then the same decrease in premiums must lower TM enrollment and raise average costs among those who remain in TM.

This result allows us estimate $\frac{dAC^{MA}}{dp}$ up to the scaling parameter, ϕ, using the TM cost data. As before, we estimate the reduced form effect of base payments on average TM costs using our difference-in-differences strategy and then divide by our estimate of the effect of base payments on premiums from Section 4. The effect of a change in premiums on average MA costs is therefore $\frac{dAC^{MA}}{dp} = \phi \frac{dAC^{TM}}{dp} = \phi \frac{dAC^{TM}/db}{dp/db}$.[52]

For our baseline estimates, we make the conservative assumption that costs under MA and TM are equal ($\phi = 1$). This provides us an upper bound on the explanatory power of advantageous selection. If instead we follow a large literature that finds that costs are proportionally lower in managed care plans than in fee-for-service coverage ($\phi < 1$), our estimates of the explanatory power of selection would be reduced.[53]

6.2 Selection Estimates

Figure 9 presents the difference-in-differences estimates that allow us to recover the explanatory power of selection. The plots are identical to those that examine the effects on premiums (Figure 4) except with different dependent variables. For ease of interpretation, we scale the coefficient on the distance-to-floor variable by \$50, which is approximately 10% of the \$511 mean base payment

[51] The equality $\frac{dQ^{MA}}{dp} = -\frac{dQ^{TM}}{dp}$ simply follows from the fact that $Q^{MA} = 1 - Q^{TM}$.

[52] We observe only claims costs. Although we cannot rule out that increasing unobservable administrative costs could be an additional mechanism contributing to incomplete pass-through, any such effects are likely small. Non-claims costs (such as advertising, broker fees for customer acquisition, claims administration, and profits) combined typically account for less than 15 percent of the full premiums paid in these markets.

[53] We know from above that $\frac{dAC^{MA}}{dp} = \phi \frac{dAC^{TM}}{dp}$. Since $\frac{dAC^{MA}}{dp} < 0$ and $\frac{dAC^{TM}}{dp} < 0$ under advantageous selection into MA, $\phi < 1$ implies $0 > \frac{dAC^{MA}}{dp} > \frac{dAC^{TM}}{dp}$ and therefore that our estimates provide an upper bound on the explanatory power of advantageous selection.

in place prior to the Benefits Improvement and Protection Act (BIPA), and normalize the coefficient on year 2000 to zero so we can interpret the effects relative to the year before BIPA came into effect. Panel A of Table 7 displays parameter estimates from the corresponding difference-in-differences regressions, and Appendix Table A3 shows alternative specifications that isolate different subsets of the identifying variation.

Figure 9 Panel A shows the effect of a \$50 increase in monthly payments on MA enrollment. In terms of estimating the degree of selection, the effect on quantity can be thought of as a first stage. If payments had no effect on MA enrollment, there would be no identifying variation that would allow us to estimate the degree of selection. MA enrollment is slow to respond to the decline in premiums, consistent with inertia or switching costs (Handel, 2012). However, by 2003 the first stage is large, with a \$50 increase in payments raising enrollment by 4.7 percentage points on a pre-BIPA mean of 30.2%, and is highly significant with a p-value < 0.01.[54]

In addition to allowing us to estimate selection, the quantity effect is independently informative about the basic structure of the MA market. The 2003 estimate implies an enrollment elasticity with respect to payments of $1.6 = \frac{4.7\%}{30.2\%} / \frac{\$50}{\$511}$. If we assume that base payments affect enrollment only through premiums — so that base payments are a valid instrument for premiums — then the 2003 estimate implies a semi-elasticity of demand with respect to premiums of $-0.007 = \frac{(4.7\%/30.2\%)}{-0.45\times\$50}$, where the denominator is the change in premiums implied by a \$50 increase in the base payments. While this is a market-level elasticity, with individual firms facing more elastic residual demand curves, our low aggregate price elasticity estimate is similar to the -0.009 semi-elasticity estimate by Town and Liu (2003) and the -0.013 semi-elasticity estimate by Dunn (2010). The low elasticity is also consistent with work on limited premium transparency (Stockley et al., 2014) and large switching costs (Nosal, 2012) in the MA market.

Figure 9 Panel B shows the effect of a \$50 increase in payments on TM costs. To interpret the magnitude of the estimates, it is useful to divide by the effect on enrollment, which provides an estimate of the slope of the average cost curve ($\frac{dAC/db}{dq/db} = \frac{dAC}{dq}$). The 2003 point estimate of \$3.54, shown in column 2 of Table 7, divided by the 4.7% enrollment effect implies a \$75 slope of the average cost curve. Since average costs are \$483 per month, this indicates that individuals with the highest

[54]As discussed above, the enrollment effect can be viewed as a first-stage for the selection analysis. Because there is geographic variation across markets in the base payment change (as discussed in Section 3) and there may be variation across markets in the MA enrollment effect for a given base payment change, the selection estimates should be interpreted as a LATE that is valid for the subset of counties exposed to the BIPA-induced capitation payment variation.

willingness-to-pay for MA only cost about 16% less than the population on average. We cannot rule out the null hypothesis that the slope of the average cost curve is zero, with a 95% confidence interval that runs from -\$84 to \$233.[55] Appendix Section A.9 demonstrates that the selection estimates are qualitatively similar in specifications with alternative controls and specifications with alternative measures of utilization.

Figure 9 Panel C shows the effects on MA risk adjustment payments, which is the MA demographic risk score scaled by the year 2000 base payment. Since MA plan payments are scaled by an individual's risk score, increases in average demographic risk, holding costs fixed, result in greater pass-through. The plot shows evidence that demographic risk declines with MA penetration. While the magnitude is statistically significant, the estimate is small. Dividing the 2003 point estimate of -\$3.43, shown in column 3 of Table 7, by the enrollment effect indicates a slope of risk adjustment payments with respect to quantity of -\$72. Combining this estimate with our 2003 cost estimate yields a slope for the average cost curve net of risk adjustment ($\frac{dAC^{MA}}{dq} - b\,\frac{dAR^{MA}}{dq}$) of \$147.[56] We cannot reject that there is no net selection on the margin as the 95% confidence interval on this estimate runs from -\$13 to \$306.[57]

To calculate the explanatory power of selection, we combine these estimates with Equation 11, where the numerator of Equation 11, the average risk adjustment factor among MA beneficiaries AR^{MA}, is equal to 0.955 in our sample.[58] We calculate standard errors of the implied pass-through by bootstrapping over counties.[59] We estimate pass-through for each of the post-BIPA years. To increase power, we also construct a pooled pass-through estimate, which is calculated using regressions that specify a single post-BIPA coefficient for enrollment, demographic risk, costs, and premiums. These pooled estimates are shown in Panel B of Table 7. Column 5 of Table 7 shows the reduction in pass-

[55] This confidence interval is constructed by bootstrapping standard errors for the ratio $\frac{dAC/db}{dq/db}$. This bootstrap calculation relies on 200 random samples of counties drawn with replacement.

[56] The slope of the average cost curve net of risk adjustment ($\frac{dAC^{MA}}{dq} - b\,\frac{dAR^{MA}}{dq}$) is larger than the slope of the average cost curve alone ($\frac{dAC^{MA}}{dq}$) because our point estimates suggest that, on the margin, demographic risk adjustment reinforces rather than compensates for advantageous selection.

[57] This confidence interval is constructed by bootstrapping standard errors for the term $\frac{dAC^{MA}}{dq} - b\,\frac{dAR^{MA}}{dq}$. This bootstrap calculation relies on 200 random samples of counties drawn with replacement.

[58] As discussed in Section 2, we conduct our risk adjustment analysis with demographic risk adjustment factors normalized to one over our sample population. These normalized risk adjustment factors reflect the relative demographic risk scores across the MA and TM samples, where the average MA normalized risk adjustment factor is 0.955 and the average TM normalized risk adjustment factor is 1.02.

[59] We construct bootstrap standard errors by drawing a random sample of counties with replacement, estimating the effect on enrollment and costs for this sample, and using these estimates to construct a sample-specific pass-through rate. Our standard errors are based on calculating pass-through in this manner for 200 random samples.

through implied by our estimates of selection. The pooled estimates indicate that selection reduces pass-through to 85%. A 95% confidence interval allows us to rule out estimates lower than 0.73 or higher than 0.98. The yearly estimates similarly vary from 72% to 107%.[60]

Taken together, the results above indicate that selection is unable to explain our finding that only half of the increase in payments is passed through to consumers. We estimate that a perfectly competitive market would pass through 85 cents of each dollar in increased payments. Alternatively put, of the combined 46 cents in payments that is not passed through to consumers, our estimates indicate that selection can account for 15 cents or about one-third of the shortfall.[61]

7 Market Power

In this section, we examine the extent to which insurer market power is a mechanism that can explain our estimates of incomplete pass-through. In Section 5, we discussed how a monopolist facing a linear demand curve would pass through only half of an increase in payments (Panel B of Figure 8). More generally, we showed that for a range of functional form assumptions on the shape of the demand curve, pass-through in an imperfectly competitive market is declining in market power. In light of the evidence on limited selection, the model implies that much of the incomplete pass-through in our setting is due to market power.

We investigate the quantitative importance of insurer market power by splitting the sample by measures of insurer market power *prior to the 2000 Benefits Improvement and Protection Act (BIPA)* and estimating the pass-through rate separately in each sample.[62] It is important to emphasize that we view the following analysis as suggestive, since our research design isolates variation in payments to plans, not variation in pre-reform market power.

Figure 10 shows estimates of pass-through into mean premiums for different levels of competition. Panel A splits the sample by the year 2000 county-level insurer Herfindahl-Hirschman Index (HHI), with the highest HHI tercile corresponding to the most concentrated markets and the lowest

[60]In addition, we explore potential heterogeneity in the selection effect by pre-BIPA measures of insurer market power. Based on this analysis (reported in Appendix Table A4), we cannot reject the hypothesis that the selection effect is identical across markets with different pre-BIPA insurer market power.

[61]Because we find advantageous selection has little role in explaining the incomplete pass-through we observe during our period of study, it is unlikely that more recent refinements in risk adjustment have meaningfully affect the takeaways from our paper.

[62]While we do not find evidence that BIPA affected market structure, splitting the sample by pre-BIPA market power is appropriate because the increase in payments could, at least in principle, affect the number of firms in each county.

HHI tercile corresponding to the markets with the least market power.[63] Panel B splits the sample by whether the county had one, two, or three or more separate Medicare Advantage (MA) insurers in year 2000. The regression specifications used to construct these figures are identical to those used to construct the baseline pass-through plot (Panel A of Figure 4), applied to each subsample. We show coefficients for year 2003, which is the year with the largest pass-through of premiums, on average. Estimates for 2001 and 2002 are shown in Appendix Figure A8. As before, the vertical axes measure pass-through of payments, with the dashed horizontal line at zero indicating no pass-through and the dashed horizontal line at -1 indicating full pass-through.

Panel A of Figure 10 shows that the pass-through rate is monotonically decreasing in pre-BIPA insurer HHI. The pass-through rate is 13% in the most concentrated HHI tercile and 63% in the tercile with the lowest market power. Panel B shows that the pass-through rate is similarly increasing in the number of pre-BIPA insurers in the county. When there is a single insurer, pass-through is 13%. In counties with three or more firms, pass-through increases to 74%. This is consistent with our model, which predicts that pass-through is decreasing in market power for many standard parameterizations of demand.

Appendix Figure A8 shows the effects for each year in the post-BIPA period. The 2002 estimates are almost identical to the 2003 estimates and show that pass-through is monotonically increasing in both measures of competition. Consistent with the main results in Figure 4, pass-through rates are lower in 2001 and the relationship between pass-through and market power is less precise. The parameter estimates underlying these figures are shown in Appendix Table A5. The table also reports coefficients from full-sample regressions that interact pre-BIPA market power with the distance-to-floor variable. These confirm the statistical significance of the pattern in which pass-through declines with market power.[64]

The estimates of pass-through in the most competitive markets also provide us with an alternative approach to gauge the importance of selection as a mechanism for our findings. We estimate premium pass-through of 74 percent in markets with at least 3 plans. If we add in 9 percent benefits pass-through, then these results imply pass-through is at least 83 percent in the most competitive

[63] When stratify counties by HHI terciles, we do not weight by population. The reason is that there are a small number of urban counties (e.g., Miami-Dade) with very large populations and low HHI (because they have many plans). If we weighted by population, the lowest HHI tercile would not have enough counties to generate precise estimates.

[64] Because less populous counties have more concentrated MA markets on average, pass-through is also increasing in county population size.

markets, and therefore advantageous selection can explain no more than 17 percent of the reduction in pass-through (the difference between 100 percent and 83 percent), which can be compared to the directly estimated 15 percent selection parameter. While it is important to understand that each of these parameters has an associated confidence interval, it is comforting that the two alternate approaches yield consistent insights.

We conclude by noting that despite the growth in Medicare Advantage since our period of analysis, many MA markets remain highly concentrated today. As of 2014, 88 percent of Medicare Advantage markets had insurer HHI values in excess of 2,500, the Department of Justice standard for highly concentrated markets. Further, because MA market structure varies across geographic markets within a time period much more than in aggregate across time, it is likely that pass-through continues to be geographically heterogenous in the current MA program.

8 Conclusion

We examine the pass-through to consumers of payments in Medicare Advantage (MA) using difference-in-differences variation brought about by the Benefits Improvement and Protection Act (BIPA). We show that approximately half of the marginal spending on the MA program is passed through to beneficiaries in the form of lower premiums and more generous benefits. We find little evidence that selection of more costly beneficiaries into MA can account for this incomplete pass-through, suggesting the result is driven by supply-side market power. Consistent with this intuition, we find that the pass-through of payments varies greatly with insurer market concentration, with premium pass-through rates of 13% in the least competitive markets and 74% in the markets with the most competition.

Our estimates of pass-through are directly relevant for the $156 billion in MA payment reductions scheduled to take effect under the Affordable Care Act. Counter to claims made by some commentators, our results predict that the incidence of such payment reductions would fall only partially on Medicare beneficiaries, while a significant fraction of these cuts would be borne by the supply side of the market. Our study does not address the division of surplus among inframarginal MA consumers and therefore does not speak directly to the welfare effects of a more dramatic counterfactual, such as completely abolishing (or significantly expanding) privatized Medicare.

References

Achman, Lori, and Marsha Gold. 2002. *Medicare+ Choice 1999-2001: An Analysis of Managed Care Plan Withdrawls and Trends in Benefits and Premiums.* Commonwealth Fund.

Agarwal, Sumit, Souphala Chomsisengphet, Neale Mahoney, and Johannes Stroebel. 2014. "A Simple Framework for Estimating the Consumer Benefits from Regulating Hidden Fees." *Journal of Legal Studies*.

Agarwal, Sumit, Souphala Chomsisengphet, Neale Mahoney, and Johannes Stroebel. 2015. "Regulating Consumer Financial Products: Evidence from Credit Cards." *Quarterly Journal of Economics*.

Brown, Jason, Mark Duggan, Ilyana Kuziemko, and William Gui Woolston. 2011. "Can Risk Adjustment Reduce Selection in the Private Health Insurance Market? New Evidence from the Medicare Advantage Program." *NBER Working Paper*.

Bulow, Jeremy I, and Paul Pfleiderer. 1983. "A Note on the Effect of Cost Changes on Prices." *The Journal of Political Economy*, 91(1): 182–185.

Bundorf, M. Kate, Jonathan Levin, and Neale Mahoney. 2011. "Pricing and Welfare in Health Plan Choice." *American Economic Review*, 102(7): 3214–48.

Carey, Colleen. 2014. "Sharing the Burden of Subsidization: Evidence from a Payment Update in Medicare Part D." *Mimeo*.

Cawley, John, Michael Chernew, and Catherine McLaughlin. 2005. "HMO Participation in Medicare+Choice." *Journal of Economics & Management Strategy*, 14(3): 543–574.

Clemens, Jeffrey, and Joshua D Gottlieb. 2014. "Bargaining in the Shadow of a Giant: Medicare's Influence on Private Payment Systems." *NBER Working Paper*.

Committee on Ways and Means. 2004. *Green Book, 2004: Background Material and Data on Programs Within the Jurisdiction of the Committee on Ways and Means.* Government Printing Office.

Curto, Vilsa, Liran Einav, Jonathan Levin, and Jay Bhattacharya. 2015. "Can Health Insurance Competition Work? Evidence from Medicare Advantage." *Mimeo*.

Dafny, Leemore, and David Dranove. 2008. "Do report cards tell consumers anything they don't already know? The case of Medicare HMOs." *The RAND Journal of Economics*, 39(3): 790–821.

Dafny, Leemore S. 2005. "How Do Hospitals Respond to Price Changes?" *American Economic Review*, 95(5): 1525–1547.

Dague, Laura. 2014. "The effect of Medicaid premiums on enrollment: A regression discontinuity approach." *Journal of Health Economics*, 37: 1 – 12.

Duggan, Mark, Amanda Starc, and Boris Vabson. 2014. "Who Benefits When the Government Pays More? Pass-Through in the Medicare Advantage Program." *NBER Working Paper*.

Dunn, Abe. 2010. "The Value of Coverage in the Medicare Advantage Insurance Market." *Journal of Health Economics*, 29(6): 839–855.

Einav, Liran, Amy Finkelstein, and Mark R. Cullen. 2010. "Estimating Welfare in Insurance Markets Using Variation in Prices." *Quarterly Journal of Economics*, 125(3): 877–921.

Fabinger, Michal, and E. Glen Weyl. 2013. "Pass-Through and Demand Forms." *Mimeo*, 121(3).

Finkelstein, Amy, Matthew Gentzkow, and Heidi Williams. 2014. "Sources of Geographic Variation in Health Care: Evidence from Patient Migration." *NBNBER Working Paper 20789*.

Geruso, Michael, and Timothy J Layton. 2017. "Selection in Insurance Markets: Policy through the Lenses of Fixed and Endogenous Contracts." University of Texas Working Paper.

Gowrisankaran, Gautam, Robert Town, and Eric Barrette. 2011. "Managed Care, Drug Benefits and Mortality: An Analysis of the Elderly." *The BE Journal of Economic Analysis & Policy*, 11(2): 1–32.

Hackmann, Martin, Jonathan T. Kolstad, and Amanda E. Kowalski. 2015. "Adverse Selection and an Individual Mandate: When Theory Meets Practice." *American Economic Review*, 105: 1030–66.

Handel, Benjamin. 2012. "Adverse Selection and Inertia in Health Insurance Markets: When Nudging Hurts." *American Economic Review*, 103(7): 2643–2682.

Kuziemko, Ilyana, Katherine Meckel, and Maya Rossin-Slater. 2014. "Do IInsurer Risk-Select Against Each Other? Evidence from Medicaid and Implications for Health Reform." *NBER Working Paper 19198*.

Lustig, Joshua. 2010. "Measuring Welfare Losses from Adverse Selection and Imperfect Competition in Privatized Medi- care." *mimeo*.

Mahoney, Neale, and E. Glen Weyl. Forthcoming. "Imperfect Competition in Selection Markets." *Review of Economics and Statistics*.

McWilliams, J Michael, John Hsu, and Joseph P Newhouse. 2012. "New risk-adjustment system was associated with reduced favorable selection in Medicare Advantage." *Health Affairs*, 31(12): 2630–2640.

Millman, Jason. 2014. "Obama Administration Reverses Proposed Cut to Medicare Plans." *Washington Post*, April 4.

Newhouse, Joseph P., Mary Price, Jie Huang, J. Michael McWilliams, and John Hsu. 2012. "Steps to Reduce Favorable Risk Selection in Medicare Advantage Largely Succeeded, Boding Well for Health Insurance Exchanges." *Health Affairs*, 31(12): 2618–2628.

Nosal, Kathleen. 2012. "Estimating Switching Costs for Medicare Advantage Plans." *mimeo*.

Pope, Gregory C, Leslie M Greenwald, Deborah A Healy, John Kautter, Eric Olmsted, and Nathan West. 2006. "Impact of Increased Financial Incentives to Medicare Advantage Plans." RTI International.

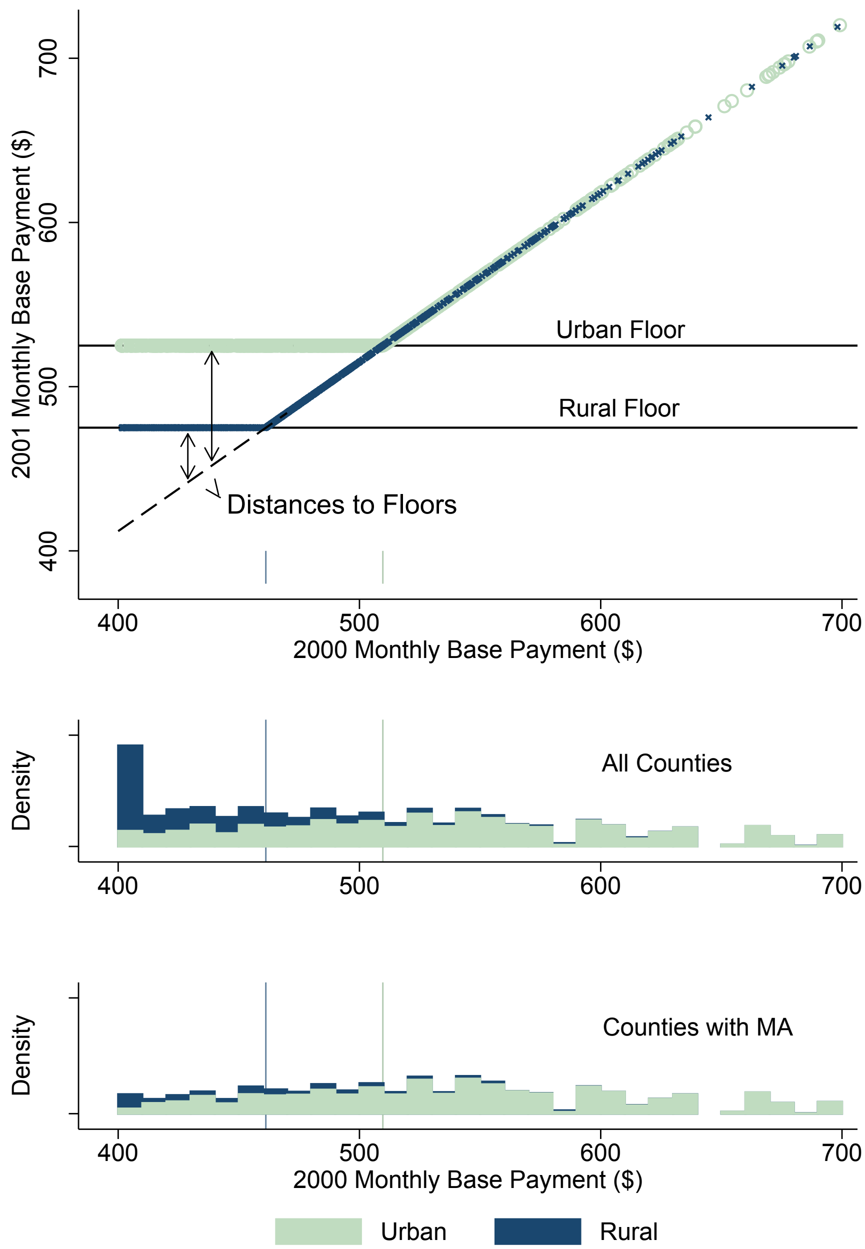
Stockley, Karen, Thomas McGuire, Christopher Afendulis, and Michael E. Chernew. 2014. "Premium Transparency in the Medicare Advantage Market: Implications for Premiums, Benefits, and Efficiency." *NBER Working Paper*.

Town, Robert, and Su Liu. 2003. "The Welfare Impact of Medicare HMOs." *RAND Journal of Economics*, 34(4): 719–736.

Weyl, E. Glen, and Michal Fabinger. 2013. "Pass-Through as an Economic Tool: Principles of Incidence under Imperfect Competition." *Journal of Political Economy*, 121(3): 528–583.

Woolston, William Gui. 2012. "Selection and the Manipulation of Price Signals: Evidence from Private Medicare Plans." *Mimeo*.

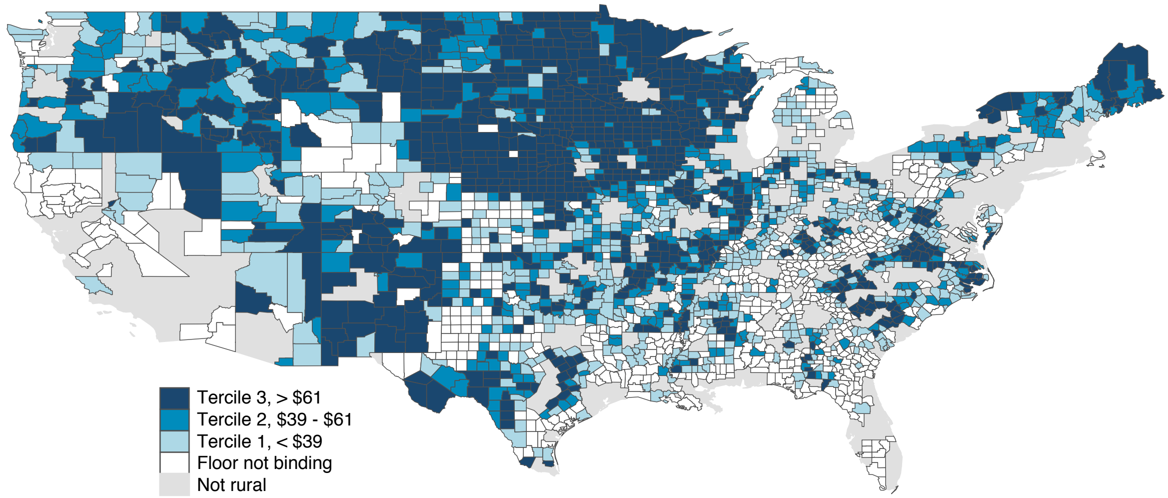
Figure 1: Payment Floors: Pre- and Post-BIPA Monthly Base Payments



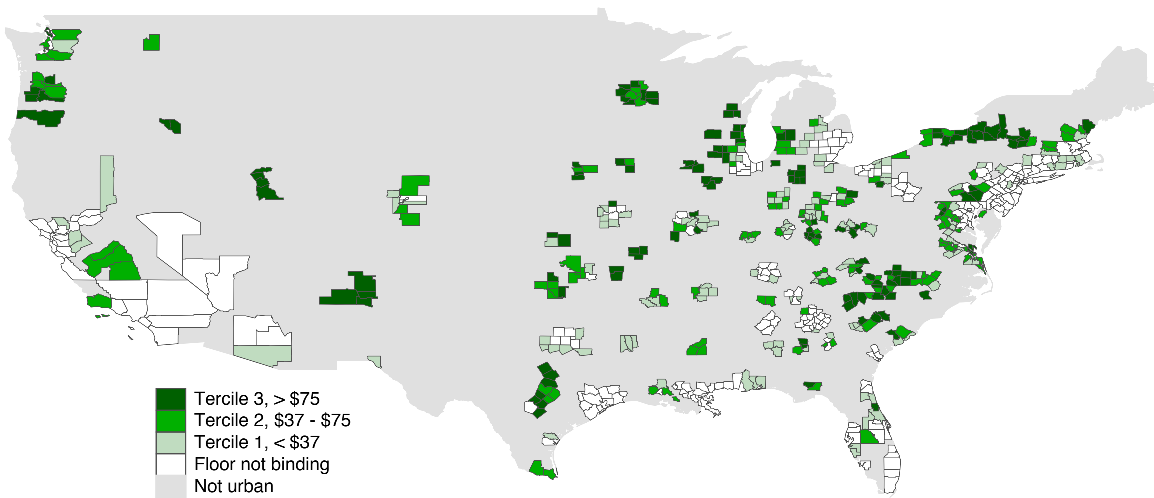
Note: Figure illustrates the identifying variation arising from BIPA. The top panel shows county base payments before (x-axis) and after (y-axis) the implementation of the BIPA urban and rural payment floors in 2001. Urban counties are represented in light green and rural counties in blue. The dashed line in the top panel indicates the uniform 3% increase that was applied to all counties between 2000 and 2001 and traces the counterfactual payment rule in absence of the floors. The distance to the floor defines our identifying payment variation and is a function of both the pre-BIPA base payment and a county's urban/rural classification. The bottom two panels plot histograms of the base payments in 2000, stacking rural and urban counties and weighting by county Medicare population, for all counties (middle panel) and for counties with an MA plan in at least one year of the 1997-2003 study period (bottom panel). All values are denominated in dollars per beneficiary per month. Base payments in this figure are not adjusted for inflation and are not normalized for the sample average demographic risk adjustment factor. The sample in the top two panels is 3,143 counties that include 100% of the Medicare population in 2000. The sample in the bottom is 880 counties that include 73% of the Medicare population in 2000.

Figure 2: Effect of BIPA on County Base Payments

(A) Floor Distance, Rural Counties

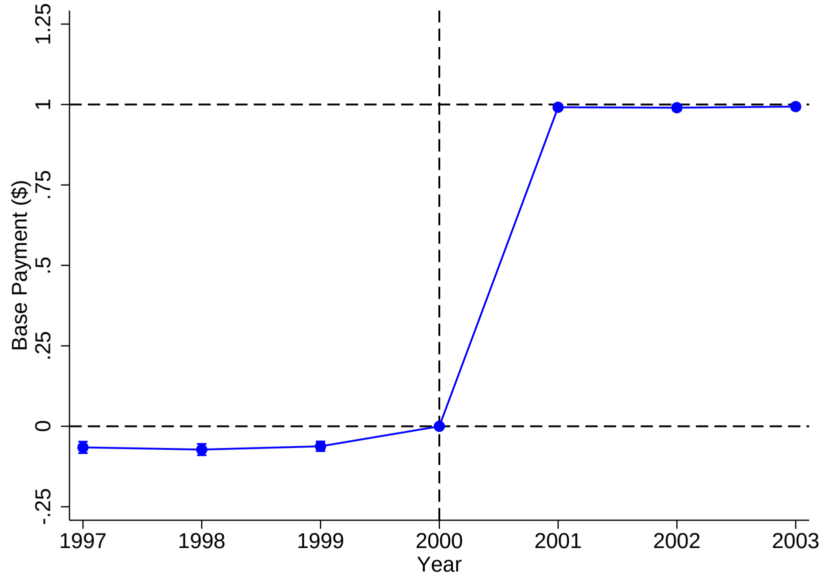


(B) Floor Distance, Urban Counties



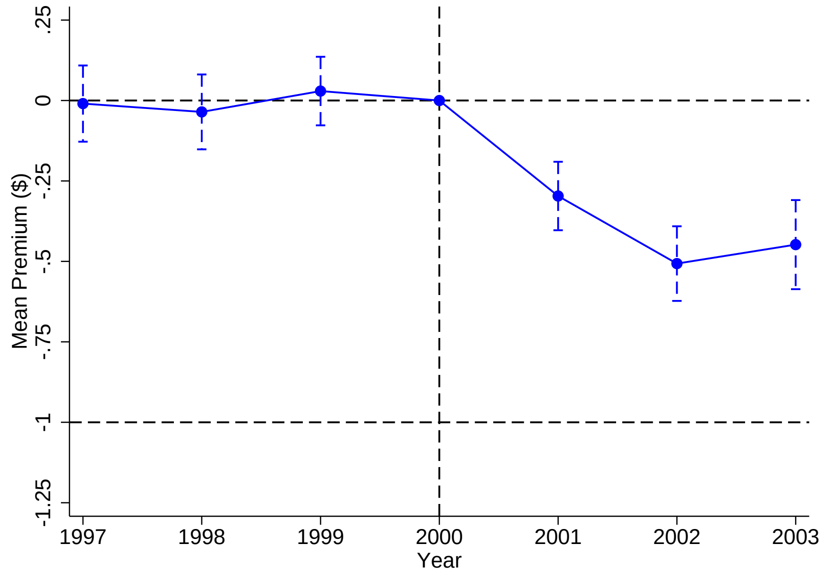
Note: Map shows the geography of the identifying variation across urban and rural counties. Counties are binned according to their tercile of distance-to-floor, separately for rural counties (Panel A) and urban counties (Panel B). Legends indicate the bin ranges, and counties for which the floors were not binding are shaded white. The distance-to-floor variable, which describes the payment shock between 2000 and 2001, is defined precisely in Equation (2) and is graphically illustrated in the top panel of Figure 1. Base payments in this figure are not adjusted for inflation and are not normalized for the sample average demographic risk adjustment factor. Alaska and Hawaii are excluded from these maps but included in all of the other analysis. Inclusive of AK and HI, the sample is 3,143 counties that include 100% of the Medicare population in 2000.

Figure 3: First Stage Effect on Base Payments: Impact of $1 Increase in Distance-to-Floor



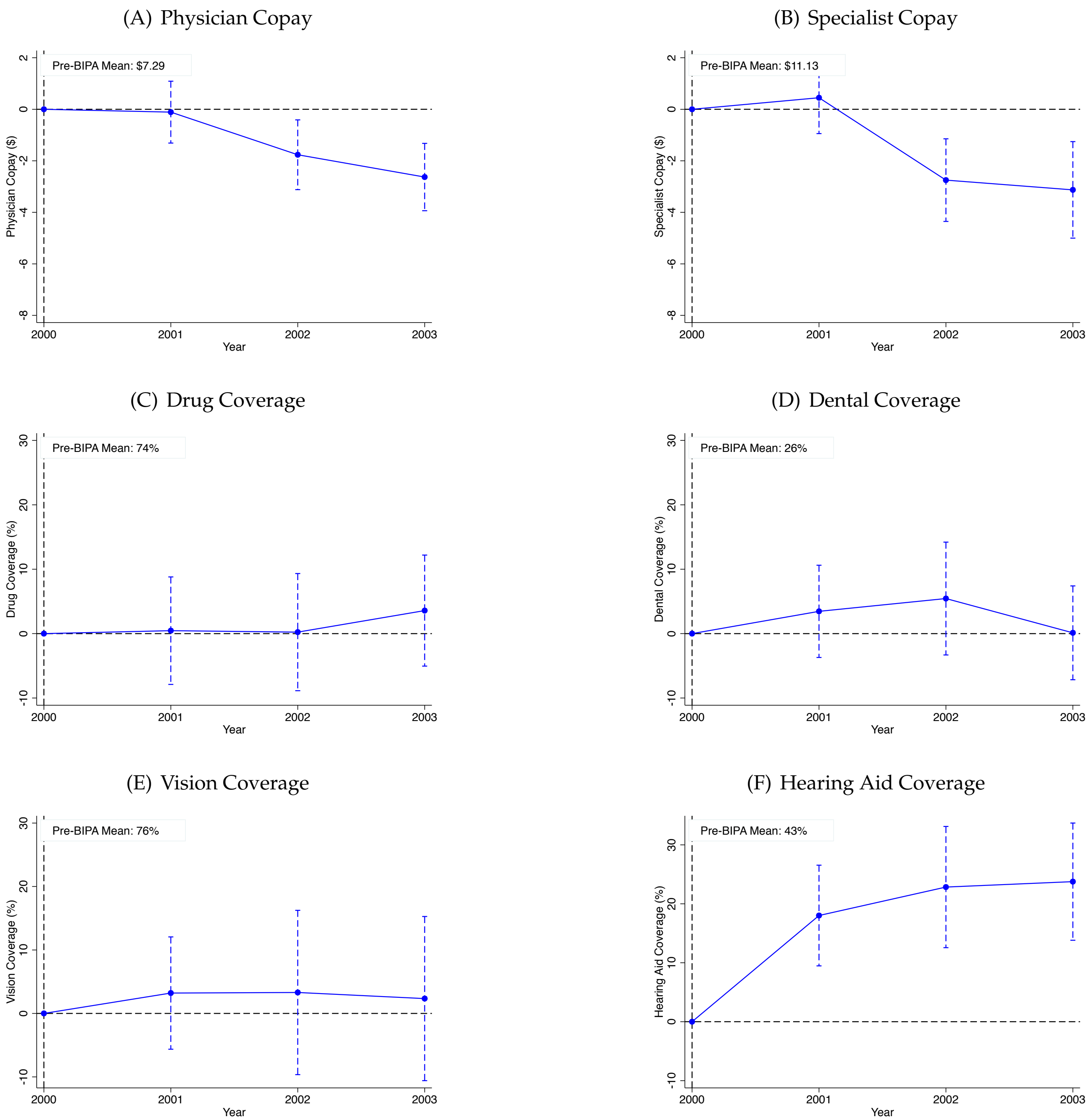
Note: Figure shows coefficients on distance-to-the-floor $\times$ year interactions from difference-in-differences regressions with the monthly base payments as the dependent variable. The unit of observation is the county $\times$ year, and observations are weighted by the number of beneficiaries in the county. The sample is the unbalanced panel of county-years with at least one MA plan over years 1997 to 2003. This sample includes 4,262 of 22,001 possible county-years and 64% of all Medicare beneficiaries. Controls include year and county fixed effects as well as flexible controls for the 1998 payment floor introduction and the blended payment increase in 2000. The capped vertical bars show 95% confidence intervals calculated using standard errors clustered at the county level. Year 2000, which is the year prior to BIPA implementation, is the omitted category and denoted with a vertical dashed line. Horizontal dashed lines are plotted at the reference values of 0 and 1.

Figure 4: Premium Pass-Through: Impact of $1 Increase in Monthly Payments



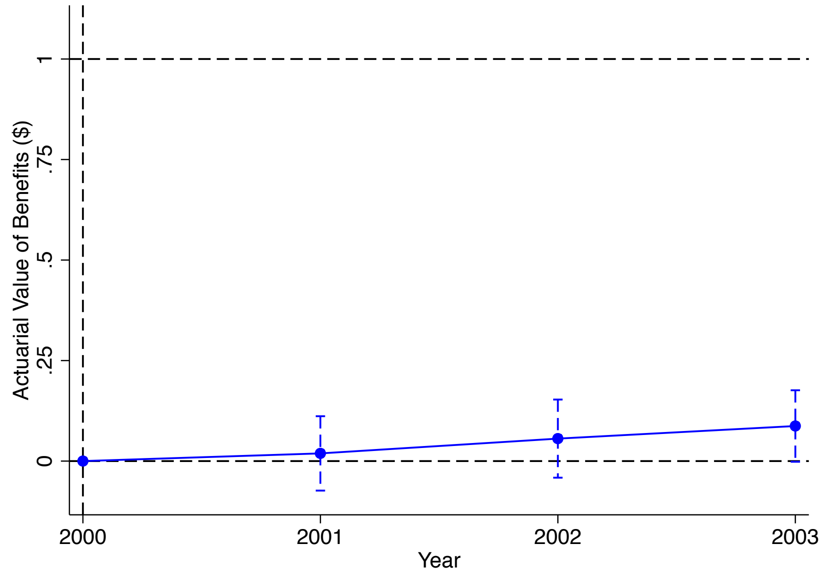
Note: Figure shows coefficients on distance-to-floor $\times$ year interactions from difference-in-differences regressions. The first-stage results displayed in Table 3 indicate that a $1 change in distance-to-floor translates into a $1 change in the monthly payments, so we can interpret the coefficients as the effect of an increase in monthly payments on a dollar-for-dollar basis. The dependent variable is the mean monthly premiums weighted by enrollment in the plan. The unit of observation is the county $\times$ year, and observations are weighted by the number of beneficiaries in the county. The county-level measures are constructed using plan-level data weighted by plan enrollment. The sample is the unbalanced panel of county-years with at least one MA plan over years 1997 to 2003. This sample includes 4,262 of 22,001 possible county-years and 64% of all Medicare beneficiaries. Controls are identical to those in Figure 3. The capped vertical bars show 95% confidence intervals calculated using standard errors clustered at the county level. Horizontal dashed lines are plotted at the reference values of 0 and -1, where -1 corresponds to 100% pass-through.

Figure 5: Benefits Generosity: Impact of $50 Increase in Monthly Payments



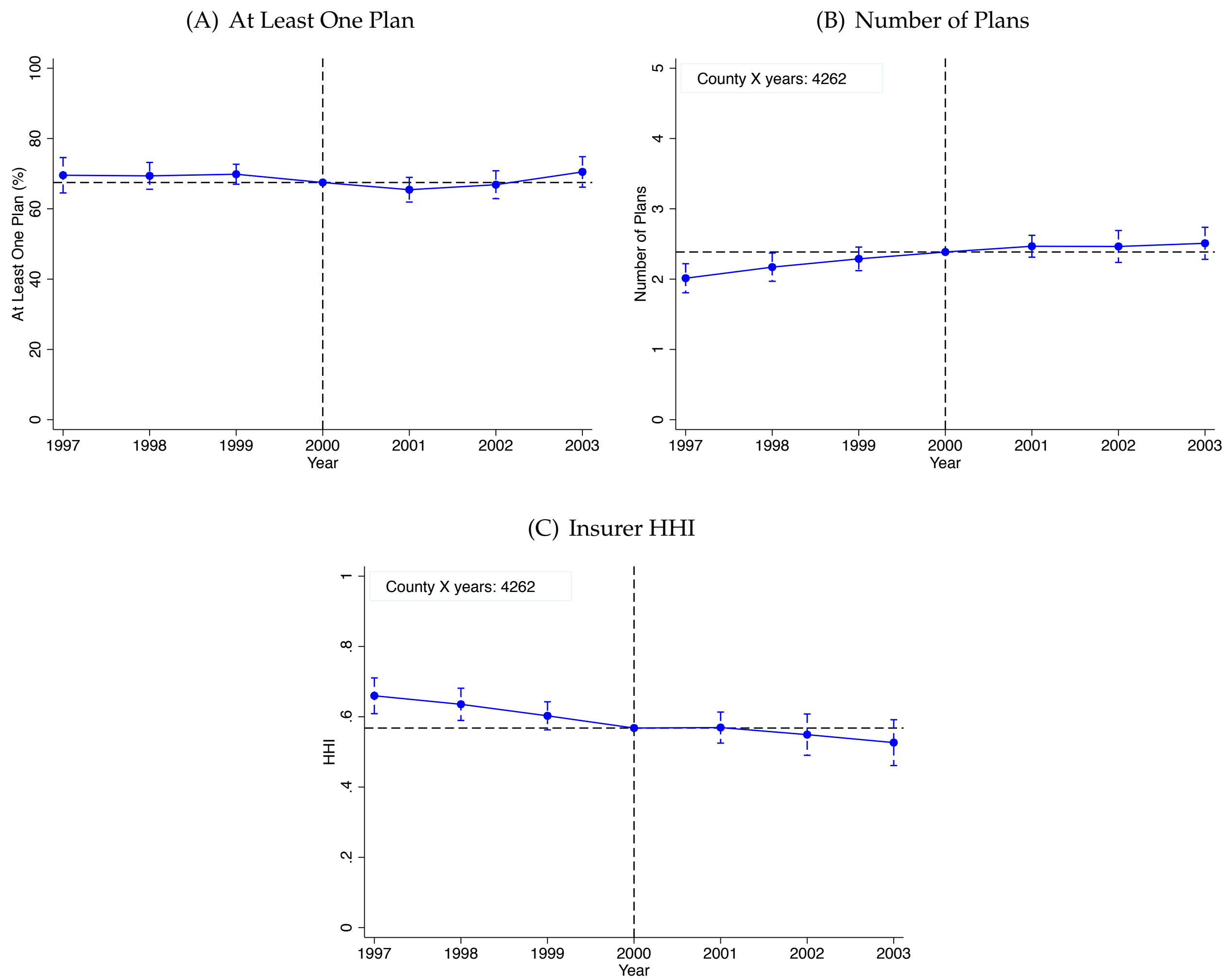
Note: Figure shows scaled coefficients on distance-to-floor $\times$ year interactions from difference-in-differences regressions. The first-stage results displayed in Table 3 indicate that a $1 change in distance-to-floor translates into a $1 change in the monthly payments, so we can interpret the coefficients as the effect of an increase in monthly payments on a dollar-for-dollar basis. The dependent variables are physician copays in dollars (Panel A), specialist copays in dollars (Panel B), and indicators for coverage of outpatient prescription drugs (Panel C), dental (Panel D), corrective lenses (Panel E), and hearing aids (Panel F). The unit of observation is the county $\times$ year, and observations are weighted by the number of beneficiaries in the county. The sample is the unbalanced panel of county-years with at least one MA plan over years 2000 to 2003. This sample includes 2,250 of 12,572 possible county-years and 62% of all Medicare beneficiaries. Controls are identical to those in Figure 3. In Panels A and B, the vertical axes measure the effect on copays in dollars of a $50 difference in monthly payments. In Panels C through F, the vertical axes measure the effect on the probability that a plan offers each benefit, again for a $50 difference in monthly payments. The capped vertical bars show 95% confidence intervals calculated using standard errors clustered at the county level. Year 2000, which is the year prior to BIPA implementation, is the omitted category. The horizontal dashed line is plotted at 0.

Figure 6: Actuarial Value of Benefits: Impact of $1 Increase in Monthly Payments



Note: Figure shows coefficients on distance-to-floor $\times$ year interactions from difference-in-differences regressions. The first-stage results displayed in Table 3 indicate that a $1 change in distance-to-floor translates into a $1 change in the monthly payments, so we can interpret the coefficients as the effect of a $1 increase in monthly payments. The dependent variable is the actuarial value of benefits, which is constructed based on observed plan benefits in our main analysis dataset and utilization and cost data from the 2000 Medical Expenditure Panel Survey. See text for full details. The unit of observation is the county $\times$ year, and observations are weighted by the number of beneficiaries in the county. The sample is the unbalanced panel of county-years with at least one MA plan over years 2000 to 2003. This sample includes 2,250 of 12,572 possible county-years and 62% of all Medicare beneficiaries. Controls are identical to those in Figure 3. The capped vertical bars show 95% confidence intervals calculated using standard errors clustered at the county level. Horizontal dashed lines are plotted at 0 and 1.

Figure 7: Plan Availability: Impact of $50 Increase in Monthly Payments

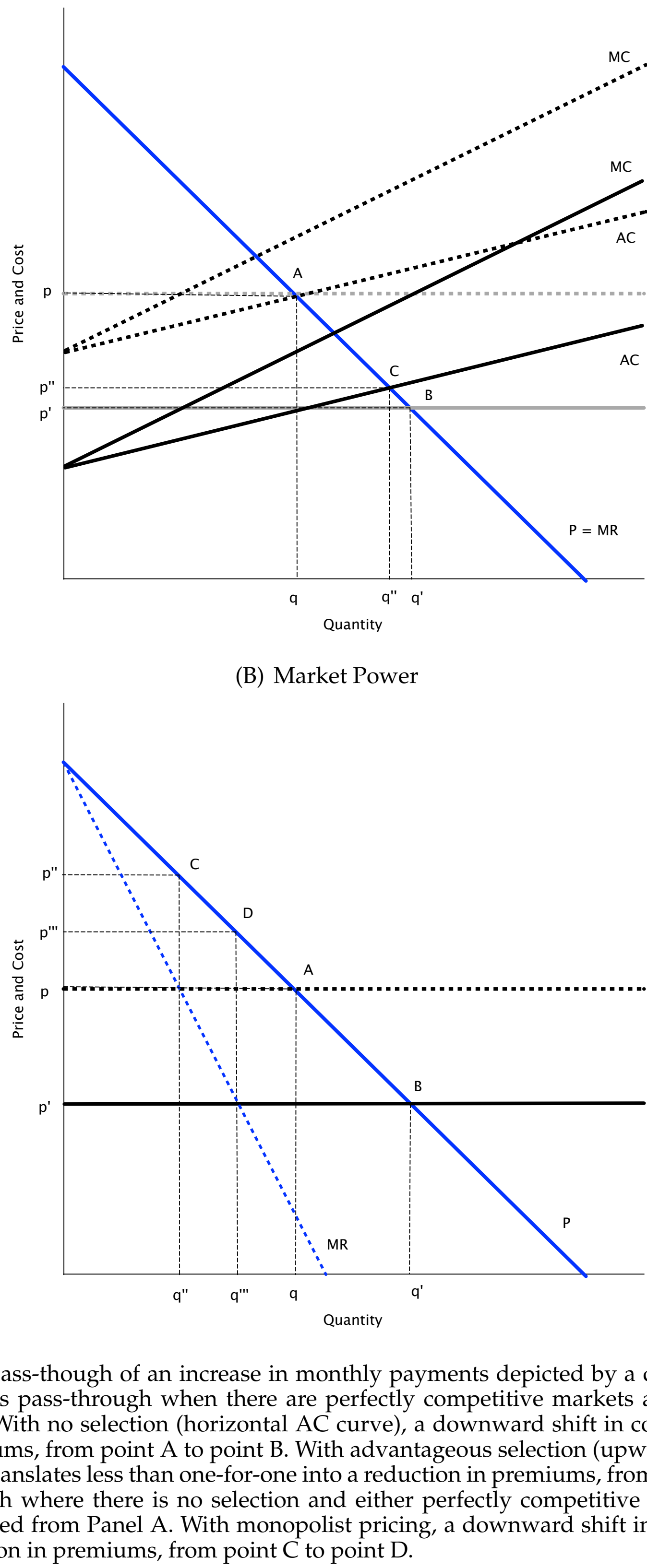


Note: Figure shows scaled coefficients on distance-to-floor × year interactions from difference-in-differences regressions. The first-stage results displayed in Table 3 indicate that a $1 change in distance-to-floor translates into a $1 change in the monthly payments, so we can interpret the coefficients as the effect of an increase in monthly payments on a dollar-for-dollar basis. Coefficients are scaled to reflect the impact of a $50 increase in monthly payments. The dependent variable in Panel A is an indicator for at least one plan, and the sample is the full sample of counties. Panels B and C restrict the sample to county × years with at least one plan. The dependent variable in Panel B is the number of plans conditional on at least one plan. The dependent variable in Panel C is a Herfindahl-Hirschman Index (HHI) with a scale of 0 to 1. The unit of observation is the county × year, and observations are weighted by the number of beneficiaries in the county. The sample in Panel A is the balanced panel of county-years with non-missing information on base rates and Medicare beneficiaries during 1997 to 2003. This sample includes 21,504 of 22,001 county-years and more than 99.9% of all Medicare beneficiaries. The sample in Panels B and C is the unbalanced panel of county-years with at least one MA plan over years 1997 to 2003. This sample includes 4,262 of 22,001 possible county-years and 64% of all Medicare beneficiaries. Controls are identical to those in Figure 3. The capped vertical bars show 95% confidence intervals calculated using standard errors clustered at the county level. The horizontal dashed lines are plotted at the sample means, which are added to the coefficients.

Figure 8: Determinants of Incomplete Pass-Through

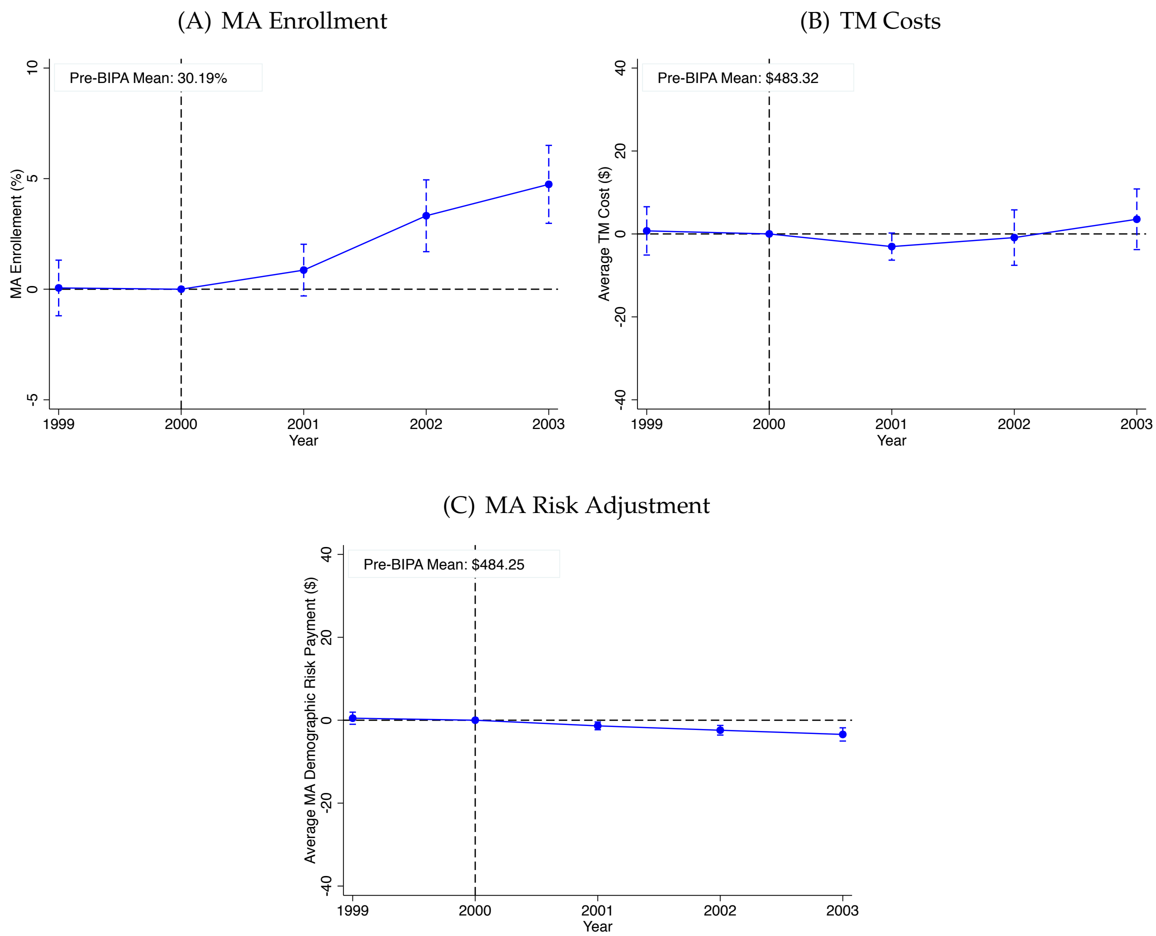
(A) Advantageous Selection

(B) Market Power



Note: Figure shows the pass-though of an increase in monthly payments depicted by a decrease in (net) marginal costs. Panel (A) examines pass-through when there are perfectly competitive markets and either no selection or advantageous selection. With no selection (horizontal AC curve), a downward shift in costs translates one-for-one into a reduction in premiums, from point A to point B. With advantageous selection (upward slopping AC curve), a downward shift in costs translates less than one-for-one into a reduction in premiums, from point A to point C. Panel (B) examines pass-through where there is no selection and either perfectly competitive markets or a monopolist. Points A and B are repeated from Panel A. With monopolist pricing, a downward shift in costs translates less than one-for-one into a reduction in premiums, from point C to point D.

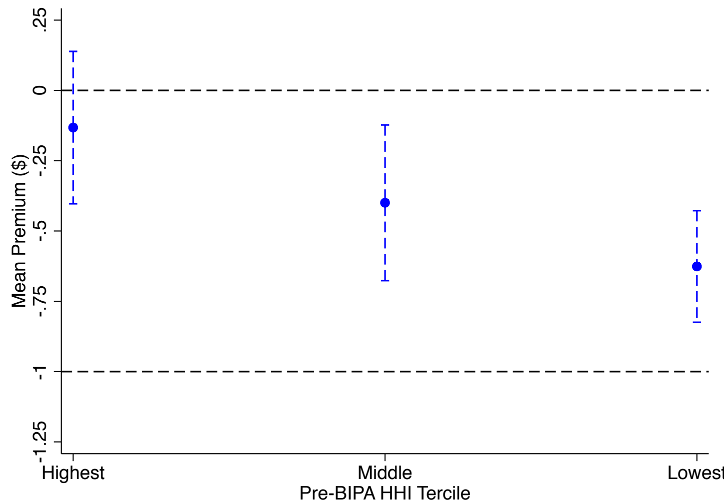
Figure 9: Selection: Impact of $50 Increase in Monthly Payments



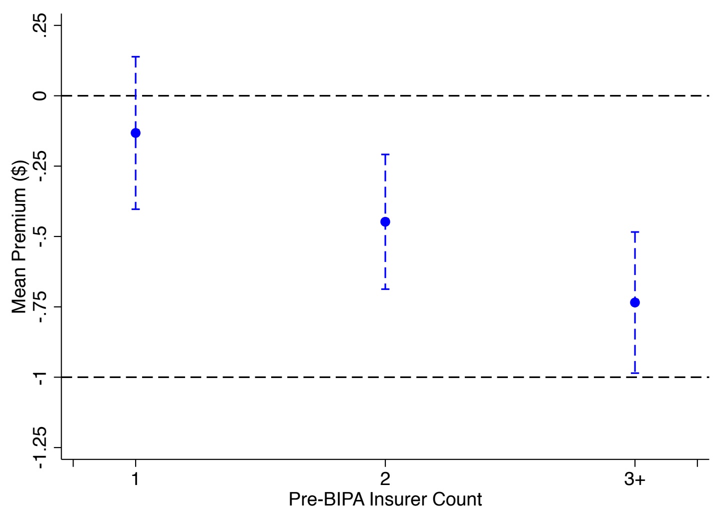
Note: Figure shows scaled coefficients on distance-to-floor × year interactions from difference-in-differences regressions. The first-stage results displayed in Table 3 indicate that a $1 change in distance-to-floor translates into a $1 change in the monthly payments, so we can interpret the coefficients as the effect of an increase in monthly payments on a dollar-for-dollar basis. Coefficients are scaled to reflect the impact of a $50 increase in monthly payments. The dependent variables are MA enrollment (Panel A), Traditional Medicare costs (Panel B), and mean demographic risk payments for MA enrollees (Panel C). The unit of observation is the county × year, and observations are weighted by the number of beneficiaries in the county. The sample is the unbalanced panel of county-years with at least one MA plan over years 1999 to 2003. This sample includes 2,892 of 15,715 possible county-years and 63% of all Medicare beneficiaries. Controls are identical to those in Figure 3. The capped vertical bars show 95% confidence intervals calculated using standard errors clustered at the county level. The horizontal dashed lines indicate zero effects.

Figure 10: Pass-Through and Market Concentration

(A) Insurer HHI



(B) Insurer Count



Note: Figure shows coefficients on distance-to-floor × year 2003 interactions from several difference-in-differences regressions. The dependent variable is the mean premium defined as in Figure 4. Each point represents a coefficient from a separate regression in which the estimation sample is stratified by market concentration in the pre-BIPA period. In Panel A, counties are binned according to the tercile of insurer HHI in plan year 2000. In Panel B, counties are binned according to the number of insurers operating in the county in plan year 2000. Competition increases to the right of both panels. The unit of observation is the county × year, and observations are weighted by the number of beneficiaries in the county. While the analysis is conducted on segments of the data, the underlying sample is the unbalanced panel of county-years with at least one MA plan over years 1997 to 2003. This sample includes 4,262 of 22,001 possible county-years and 64% of all Medicare beneficiaries. Controls are identical to those in Figure 3. The capped vertical bars show 95% confidence intervals calculated using standard errors clustered at the county level. Horizontal dashed lines are plotted at the reference values of 0 and -1, where -1 corresponds to 100% pass-through.

Table 1: Summary Statistics

	Mean	Std. Dev.	Min.	Max.
Panel A: All Counties, 1997 to 2003				
Base Payment ($ per month)	490.58	83.96	222.99	777.91
At Least One Plan	64.4%	47.9%	0%	100%
Number of Plans	1.46	1.33	0	6
MA Enrollment	19.0%	18.3%	0%	67.6%
TM Costs ($ per month)	486.53	103.94	136.87	940.08
Panel B: County X Years With At Least One Plan, 1997 to 2003				
County-Level Premium ($ per month)				
Mean	22.77	27.94	0	156.29
Min	15.47	26.35	0	156.29
Median	21.83	29.67	0	156.29
Max	31.73	33.23	0	194.47
County-Level Benefits*				
Physician Copay ($ per visit)	8.02	5.31	0	21.62
Specialist Copay ($ per visit)	15.62	7.10	0	95.72
Drug Coverage	68.7%	42.5%	0%	100%
Dental Coverage	28.0%	37.3%	0%	100%
Vision Coverage	68.1%	41.3%	0%	100%
Hearing Aid Coverage	38.2%	43.2%	0%	100%
Number of Plans	2.26	0.97	1	6
HHI	6,030	2,460	1,920	10,000
MA Enrollment	28.6%	16.2%	0.8%	67.6%
TM Costs ($ per month)	521.56	106.60	254.96	940.08

Note: Table shows county-level summary statistics for the pooled 1997 to 2003 sample. Panel A shows values for the full set of county $\times$ years ($N = 22,001$) that includes 100% of the Medicare population over this period. Panel B restricts the sample to county $\times$ years with at least one MA plan, which includes 4,262 county-years and 64% of all Medicare beneficiaries. The unit of observation is the county $\times$ year, and observations are weighted by the number of beneficiaries in the county. All monetary values are inflation adjusted to 2000 using the CPI-U.

*Benefits data are only available for 2000 to 2003.

Table 2: Effect of BIPA on County Base Payments

	Mean	Std. Dev.	Percentiles 25th	50th	75th
Non-Floor County (N = 886)					
Δ Base Payment	14.39	1.58	13.17	14.03	15.10
% Change in Base Payment	3.0%	0.0%	3.0%	3.0%	3.0%
Rural Floor County (N = 1,831)					
Δ Base Payment	52.94	17.16	39.67	62.59	67.18
% Change in Base Payment	14.1%	4.9%	10.0%	16.8%	18.3%
Urban Floor County (N = 426)					
Δ Base Payment	64.67	29.56	38.90	62.33	89.05
% Change in Base Payment	16.1%	8.4%	8.8%	14.9%	22.7%

Note: Table shows the effect of BIPA on base payments for non-floor counties and counties that were affected by the rural and urban floors. The "Δ Base Payment" rows show the difference between the 2001 base payment and the 2000 base payment in dollars per beneficiary per month. The "% Change in Base Payment" rows show this difference as a percentage of the 2000 base payment. The sample is the full set of counties in 2000 ($N = 3,143$) that includes 100% of the Medicare population. All monetary values are inflation adjusted to 2000 using the CPI-U. See text for additional information on data construction.

Table 3: First-Stage Effect on Base Payments: Impact of $1 increase in Distance-to-Floor

	Dependent Variable: Base Payment ($)		
	(1)	(2)	(3)
Δb X 2001	0.992	0.992	0.992
	(0.003)	(0.004)	(0.003)
Δb X 2002	0.990	0.999	0.987
	(0.005)	(0.005)	(0.005)
Δb X 2003	0.994	1.002	0.990
	(0.004)	(0.005)	(0.005)
Main Effects			
County FE	X	X	X
Year FE	X	X	X
Additional Controls			
Pre-BIPA Payment X Year FE		X	
Urban X Year FE			X
Pre-BIPA Mean of Dep. Var.	510.84	510.84	510.84
R-Squared	0.9998	0.9999	0.9999

Note: Table shows coefficients on distance-to-floor × year interactions from difference-in-differences regressions with monthly base payments as the dependent variable. Although the estimation includes distance-to-floor interactions for all the years in our sample, we display coefficients for the post-reform years (2001-2003) above for brevity. The unit of observation is the county × year, and observations are weighted by the number of beneficiaries in the county. The sample is the unbalanced panel of county-years with at least one MA plan over years 1997 to 2003. This sample includes 4,262 of 22,001 possible county-years and 64% of all Medicare beneficiaries. Year 2000, which is the year prior to BIPA implementation, is the omitted category. Additional controls in column 2 include quartiles of year 2000 county base payments interacted with year indicators and in column 3 include an indicator for urban status interacted with year indicators. Flexible controls for the 1998 payment floor introduction and 2000 blended payment increase are included in all specifications. All monetary values are inflation adjusted to 2000 using the CPI-U. Robust standard errors clustered at the county level are reported in parentheses.

Table 4: Premium Pass-Through: Impact of $1 Increase in Monthly Payments

	Dependent Variable: Mean Monthly Premium ($)		
	(1)	(2)	(3)
Δb X 2001	-0.297	-0.180	-0.308
	(0.054)	(0.093)	(0.055)
Δb X 2002	-0.507	-0.369	-0.519
	(0.059)	(0.122)	(0.059)
Δb X 2003	-0.448	-0.321	-0.451
	(0.071)	(0.126)	(0.072)
Main Effects			
County FE	X	X	X
Year FE	X	X	X
Additional Controls			
Pre-BIPA Payment X Year FE		X	
Urban X Year FE			X
Pre-BIPA Mean of Dep. Var.	12.58	12.58	12.58
R-Squared	0.71	0.71	0.71

Note: Table shows coefficients on distance-to-floor $\times$ year interactions from difference-in-differences regressions. Although the estimation includes distance-to-floor interactions for all the years in our sample, we display coefficients for the post-reform years (2001-2003) above for brevity. The first-stage results displayed in Table 3 indicate that a $1 change in distance-to-floor translates into a $1 change in the monthly payments, so we can interpret the coefficients as the effect of an increase in monthly payments on a dollar-for-dollar basis. The unit of observation is the county $\times$ year, and observations are weighted by the number of beneficiaries in the county. The county-level measures are constructed using plan-level data weighted by plan enrollment. The sample is the unbalanced panel of county-years with at least one MA plan over years 1997 to 2003. This sample includes 4,262 of 22,001 possible county-years and 64% of all Medicare beneficiaries. Year 2000, which is the year prior to BIPA implementation, is the omitted category. Controls are identical to those in Table 3. All monetary values are inflation adjusted to 2000 using the CPI-U. Robust standard errors clustered at the county level are reported in parentheses.

Table 5: Benefits Generosity: Impact of Increase in Monthly Payments

	Dependent Variable:						
	Physician Copay ($)	Specialist Copay ($)	Drug Coverage (%)	Dental Coverage (%)	Vision Coverage (%)	Hearing Aid Coverage (%)	Actuarial Value ($)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Δb X 2001*	-0.110	0.448	0.457	3.459	3.212	18.010	0.019
	(0.613)	(0.710)	(4.258)	(3.653)	(4.514)	(4.357)	(0.047)
Δb X 2002*	-1.765	-2.749	0.230	5.445	3.301	22.838	0.056
	(0.691)	(0.818)	(4.641)	(4.470)	(6.605)	(5.245)	(0.050)
Δb X 2003*	-2.630	-3.128	3.574	0.124	2.343	23.760	0.087
	(0.667)	(0.956)	(4.398)	(3.717)	(6.598)	(5.071)	(0.045)
Main Effects							
County FE	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X
Pre-BIPA Mean of Dep. Var.	7.29	11.13	73.62	25.77	75.68	42.58	n/a
R-Squared	0.67	0.70	0.83	0.67	0.74	0.83	0.82

Note: Table shows coefficients on distance-to-floor $\times$ year interactions from difference-in-differences regressions. Although the estimation includes distance-to-floor interactions for all the years in our sample, we display coefficients for the post-reform years (2001-2003) above for brevity. The first-stage results displayed in Table 3 indicate that a $1 change in distance-to-floor translates into a $1 change in the monthly payments, so we can interpret the coefficients as the effect of an increase in monthly payments on a dollar-for-dollar basis. In columns 1 to 6, the dependent variables are measures of benefit generosity, and the coefficient on distance-to-floor is scaled by $50. In column 7, the dependent variable is the monthly actuarial value of benefits, and the coefficient on distance-to-floor is not rescaled. See text for details on the construction of the monthly actuarial value of benefits. The unit of observation is the county $\times$ year, and observations are weighted by the number of beneficiaries in the county. The sample is the unbalanced panel of county-years with at least one MA plan over years 2000 to 2003. This sample includes 2,250 of 12,572 possible county-years and 62% of all Medicare beneficiaries. Year 2000, which is the year prior to BIPA implementation, is the omitted category. Controls are identical to those in Table 3. All monetary values are inflation adjusted to 2000 using the CPI-U. Robust standard errors clustered at the county level are reported in parentheses.

*Impact of $50 increase in columns 1 to 6. Effect of $1 increase in column 7.

Table 6: Plan Availability: Impact of $50 Increase in Monthly Payments

	Dependent Variable:								
	At Least One Plan (%)			Number of Plans			HHI		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Δb X 2001	-2.04	-3.35	-2.32	0.082	-0.104	0.103	0.001	0.038	-0.002
	(1.79)	(2.46)	(1.78)	(0.079)	(0.142)	(0.082)	(0.023)	(0.036)	(0.023)
Δb X 2002	-0.62	-6.57	-0.24	0.079	-0.114	0.092	-0.019	0.015	-0.024
	(2.02)	(3.13)	(2.04)	(0.116)	(0.191)	(0.119)	(0.030)	(0.046)	(0.031)
Δb X 2003	3.01	-2.60	3.39	0.124	-0.011	0.139	-0.041	-0.011	-0.048
	(2.21)	(3.54)	(2.23)	(0.116)	(0.202)	(0.119)	(0.033)	(0.051)	(0.033)
Main Effects									
County FE	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X
Additional Controls									
Pre-BIPA Payment X Year FE		X			X			X	
Urban X Year FE			X			X			X
Pre-BIPA Mean of Dep. Var.	67.5	67.5	67.5	2.39	2.39	2.39	0.57	0.57	0.57
R-Squared	0.86	0.86	0.86	0.70	0.69	0.70	0.73	0.72	0.73

Note: Table shows coefficients on distance-to-floor $\times$ year interactions from difference-in-differences regressions. Although the estimation includes distance-to-floor interactions for all the years in our sample, we display coefficients for the post-reform years (2001-2003) above for brevity. The first-stage results displayed in Table 3 indicate that a $1 change in distance-to-floor translates into a $1 change in the monthly payments, so we can interpret the coefficients as the effect of an increase in monthly payments on a dollar-for-dollar basis. Coefficients are scaled to reflect the impact of a $50 increase in monthly payments. The dependent variables are indicator for at least one plan (columns 1 to 3), number of plans conditional on at least one plan (columns 4 to 6), and Herfindahl-Hirschman Index (HHI) with a scale of 0 to 1 (columns 7 to 9). The sample in columns 1 to 3 is the balanced panel of county-years with non-missing information on base rates and Medicare beneficiaries during 1997 to 2003. This sample includes 21,504 of 22,001 counties and more than 99.9% of all Medicare beneficiaries. The sample in columns 4 to 9 is the unbalanced panel of county-years with with at least one MA plan over years 1997 to 2003. This sample includes 4,262 of 22,001 possible county-years and 64% of all Medicare beneficiaries. Year 2000, which is the year prior to BIPA implementation, is the omitted category. Controls are identical to those in Table 3. All monetary values are inflation adjusted to 2000 using the CPI-U. Robust standard errors clustered at the county level are reported in parentheses.

Table 7: Selection: Impact of $50 Increase in Monthly Payments

	Dependent Variable:				Implied Pass-Through with Selection (ρ)
	MA Enrollment (%)	TM Costs ($)	MA Risk Adjustment ($)	Mean Premiums* ($)	
	(1)	(2)	(3)	(4)	(5)
Panel A: Yearly BIPA Effect					
Δb X 2001	0.86	-3.05	-1.36	-0.32	1.07
	(0.60)	(1.67)	(0.48)	(0.05)	(0.16)
Δb X 2002	3.32	-0.88	-2.42	-0.48	0.90
	(0.83)	(3.41)	(0.59)	(0.06)	(0.14)
Δb X 2003	4.74	3.54	-3.43	-0.43	0.72
	(0.90)	(3.73)	(0.81)	(0.07)	(0.11)
Panel B: Pooled Post-BIPA Effect					
Δb X Post-BIPA	3.29	-0.05	-2.83	-0.47	0.85
	(0.71)	(2.80)	(0.59)	(0.05)	(0.09)
Controls: All Panels					
Main Effects					
County FE	X	X	X	X	
Year FE	X	X	X	X	
Pre-BIPA Mean of Dep. Var.	30.19	483.32	484.25	12.38	

Note: Columns 1 through 4 show coefficients on distance-to-floor $\times$ year interactions from difference-in-differences regressions. Although the estimation includes distance-to-floor interactions for all the years in our sample, we display coefficients for the post-reform years (2001-2003) above for brevity. The first-stage results displayed in Table 3 indicate that a $1 change in distance-to-floor translates into a $1 change in the monthly payments, so we can interpret the coefficients as the effect of an increase in monthly payments on a dollar-for-dollar basis. In columns 1 to 3 the coefficient on distance-to-floor is scaled by $50. The unit of observation is the county $\times$ year, and observations are weighted by the number of beneficiaries in the county. The sample is the unbalanced panel of county-years with at least one MA plan over years 1999 to 2003. This sample includes 2,892 of 15,715 possible county-years and 63% of all Medicare beneficiaries. Year 2000, which is the year prior to BIPA implementation, is the omitted category. Controls are identical to those in Table 3. All monetary values are inflation adjusted to 2000 using the CPI-U. Robust standard errors clustered at the county level are reported in parentheses. Column 5 reports the implied pass-through in a perfectly competitive market based on the estimates in the corresponding row (see Section 6 for more details). Standard errors for this implied pass-through estimate are calculated by the bootstrap method using 200 iterations.

*Impact of $1 increase in monthly payments shown in column 4.

Not For Publication

APPENDIX

A.1 Background on MA Capitation Payments

Medicare Advantage (MA) insurance plans are given monthly capitated payments for each enrolled Medicare beneficiary. These county-level payments are tied to historical Traditional Medicare (TM) costs in the county, although the exact formula determining payments varied over time.[65] Between the start of the MA program (formerly Medicare+Choice) in 1985 and the end of our study period, there were three distinct regimes determining capitation payments.

1. From 1985 to 1997, MA capitation payments were set at 95% of the Average Adjusted Per Capita Cost (AAPCC). The AAPCC was an actuarial estimate intended to match expected TM expenditures in the county. TM costs were adjusted for local demographic factors so that payments reflected local TM costs for the "national average beneficiary."

2. From 1998 to 2000, county payments were updated via a complex formula created by the Balanced Budget Act (BBA) of 1997. Specifically, plans were paid the maximum of (i) a blended rate, which was a weighted average of the county rate and the national rate, subject to a budget neutrality condition; (ii) a minimum payment floor implemented in the BBA and updated annually, and (iii) a 2% "minimum update" over the prior year's rate, applying in 1998 to the 1997 AAPCC rate. Because of a binding budget neutrality condition in 1998 and 1999, blended payments in practice applied only to year 2000.

3. From 2001 to 2003, county payments were set as the maximum of a 2% minimum update and a payment floor created by the Benefits Improvement and Protection Act (BIPA) of 2000. (For updating the 2001 rate only, there was an additional 1% increase mid-year.) Unlike the BBA 1997 floor, BIPA floors varied with each county's rural/urban status. The floors were indexed to medical expenditure growth via the national per capita Medicare+Choice growth percentage. For 2002 only, these Medicare+Choice growth percentage adjustments exceeded the 2% minimum update applied to the prior year's floors. For 2003, the 2% minimum update applied to the prior year's floors exceeded the floor levels determined by the Medicare+Choice growth percentage, and therefore the minimum update was the binding increase for floor counties.

After 1997, there was no explicit link between TM costs and MA payment updates. However, in practice, MA payments continued to be linked to historical TM costs since the rate that formed the basis to which all annual updates and floors were applied was the 1997 AAPCC.

The BBA payment floor referenced above was set at $387 in 1998. The floor impacted 1,098 mainly rural counties, most of which never had an MA plan during our time period. Among counties with an MA plan (which is the relevant sample for our analysis), the BBA floor impacted only 11.0% of counties and 3.2% of Medicare beneficiaries.

In addition to the formulas, the Balanced Budget Refinement Act (BBRA) of 1999 created a temporary system of bonuses (5% in the first year and 3% in the second) for plans entering "underserved" counties. Underserved counties were those in which an MA plan had not been offered since 1997 or from which, as of October 13, 1999 (the day prior to BBRA's introduction in Congress), all insurers had declared exit. Thus, plans reversing their exit decisions could receive the bonus. These payments did not directly affect capitation rates but rather provided temporary bonuses in addition to the capitation payments.

[65] Pope et al. (2006) provides a detailed description of the payment regimes.

A.2 Detailed Timing of Response to BIPA

Congress passed BIPA in December of 2000. In a typical year, plan characteristics including premiums, cost sharing, and supplemental benefits would have been submitted to the Secretary of HHS for approval by the middle of the year preceding the relevant plan year. Therefore, plan characteristics for 2001 would have been fixed prior to BIPA's passage in December 2000.

However, following the passage of BIPA in December 2000, the regulator *required* plans to submit new premiums and benefits to HHS by January 18, 2001. Any changes became effective in February 2001. From *Green Book, 2004: Background Material and Data on Programs Within the Jurisdiction of the Committee on Ways and Means*: "Because BIPA was enacted after the July deadline, there was a special timeline for 2001... Any M+C organization that would receive higher capitation payments as a result of BIPA was required to submit revised ACR information by January 18, 2001."

The annual data used in our main analysis are based on mid-year (July) premiums, and so it is this July-to-July change we measure in Figure 4, which shows a premium response in 2001. To demonstrate that the detailed timing of effects we measure is consistent with the policy, in Appendix Figure A6 we display a monthly sequence of our coefficient estimates on premiums. Monthly data are not available for all plan years that comprise our main analysis. Nonetheless, for 2000 to 2001, these data show a sharp drop in premiums in February 2001, consistent with plans responding in premium-setting at the first opportunity.[66] In contrast to the 2001 premium effects, the annual benefits data show no response in plan design until the 2002 plan year, suggesting that compressing a benefits redesign process from the typical months-long process into the few weeks following BIPA's passage in December 2000 wasn't feasible for most plans.

A.3 Robustness of Premium Pass-Through Estimates

A.3.1 Robustness Analysis: Tobit Estimation

In Section 4, we showed that the premium pass-through results are robust to specifications that isolate different subsets of the identifying variation and to specifications that examine effects on other moments of the premium distribution (median, minimum, maximum). In this section, we show that the premium pass-through results are robust to estimating Tobit specifications that explicitly account for the fact that plans could not give rebates (charge negative MA premiums to be credited to beneficiaries' Part B premiums) during our sample period.

Unlike the baseline specifications, which are estimated on data aggregated to the county $\times$ year level, the Tobit specifications are estimated on disaggregated plan-level data. Estimating a Tobit model on county-level means would be inappropriate because a county $\times$ year with at least one plan with a non-zero premium would have a non-zero mean and therefore seem unconstrained even if there were constrained plans in the county.

Table A6 shows the effect on premiums of dollar increase in payments using the plan-level data. Columns 1 to 3 show estimates from OLS specifications and columns 4 to 6 show estimates from the corresponding Tobit specifications. The OLS estimates are virtually identical to the baseline estimates (shown in column 1 to 3 of Table 4), and the Tobit estimates are only slightly larger. For example, the point estimate in column 4 indicates that three years after the reform, pass-through in a counterfactual setting where plans could offer rebates would have been 58 cents on the dollar. This is close to the OLS pass-through estimate of 45 cents on the dollar, and it is nearly equal to the combined pass-through point estimate of 54 cents on the dollar, which includes 9 cents in more generous benefits. In the counterfactual setting where premiums were not constrained, it could be the case that plans

[66] The monthly coefficients plotted in Figure A6 match the estimates in the main analysis when the annual sample is restricted to the same time period.

would have not adjusted plan generosity in response to the payment changes. Thus, these results suggest that the combined pass-through rate in this hypothetical unconstrained setting would lie between our combined pass-through estimate of 58 cents on the dollar and 67 cents on the dollar (the Tobit point estimate plus the change in benefit generosity we estimate).

The fact that the Tobit estimates are very similar to the non-Tobit estimates reveals that the non-negative premium constraint does not have a big impact on the results. To gain further intuition for why this is the case, Table A7 displays mean premiums by year for three subsets of counties: counties with no BIPA-induced payment change, counties with a payment increase of $1-$50 , and counties with a payment increase of greater than or equal to $51. There are two things to notice in the raw data. First, premiums are rapidly increasing over time. This means that our difference-in-differences analysis identifies the extent to which premiums *increased less* among counties marginal to the payment floors relative to other counties, rather than the extent to which premiums declined in absolute terms in these counties. Second, premiums are substantially higher in the markets that experienced the largest payment increases. Both of these facts imply that premiums for the "treated" counties in the "post" period are much larger than the mean premium in the pooled sample. For plans in counties with large payment increases, the mean premiums of $35 to $50 in the post period implies there is ample "room" for firms to pass-though additional premium cuts if they had chosen to do so. Thus, it is not surprising that the Tobit estimates are very similar to the non-Tobit estimates of premium pass-through.

A.3.2 Robustness Analysis: Including Additional Controls

Next, we investigate the robustness of our analysis to the inclusion of more controls. Specifically, we repeat our baseline pass-through estimation including contemporaneous per-capita TM costs as a control variable. The results are reported in Appendix Table A8. One can see that the addition of TM costs as a control has no meaningful impact on the pass-through estimate of interest. The fact that this addition does not matter is not surprising for a few reasons. First, in our analysis of selection, we find that the identifying variation is uncorrelated with contemporaneous TM costs when we look at contemporaneous TM costs as the outcome variable (see Figure 9 and Table 7 in the main text). Second, TM costs are quite persistent and all the cross-sectional variation in these costs is already soaked up by the county fixed effects included in all the specifications.

A.4 Within-Insurer Variation in Plan Characteristics

Table A9 describes the within-insurer variation in premiums and benefits across geography for the largest five insurers in the MA market in the year 2000. There is substantial within-insurer variation in premiums and copayments for specialists and physicians, and there is a moderate amount of within-insurer variation in the propensity to provide drug, dental, vision, and hearing aid coverage. Overall, the table indicates that it is common for insurers to vary premiums and benefits across geography in a given year.

A.5 Plan Benefits: Alternative Specifications

Section 4 describes the effect of BIPA on the generosity of plan benefits. Table 5 and Figure 5 display the results with only the baseline set of controls. Table A2 shows that these results are robust to including controls that isolate different subsets of the identifying variation. Odd columns in the table control for quartiles of the year 2000 base payment interacted with year fixed effects. Even columns control for urban status of the county interacted with year fixed effects.

A.6 Plan Benefits: Risk Smoothing

In Section 4, we showed that a \$1 increase in payments raised the actuarial value of benefits by 8.7 cents. However, unlike pass-through into premiums, the change in plan generosity might vary across states of the world. In particular, if the actuarial value of the increase in benefits is larger in high OOP spending states of the world (where the marginal utility of consumption is higher) than in low OOP spending states of the world (where the marginal utility of consumption is lower), then the pass-through into benefits might have additional consumption-smoothing value to consumers which is not captured by the baseline actuarial value estimate. To quantify the potential importance of an additional consumption-smoothing value from the increase in plan generosity, we re-estimate the pass-through into plan benefits separately for individuals with different levels of out-of-pocket spending and re-weighting the plan benefits pass-through estimates by the marginal utility of consumption across these states of the world.

As discussed in Section 4, we construct our measure of actuarial value using utilization data (e.g., number of office visits) on the elderly in the 2000 Medical Expenditure Panel Survey (MEPS). To allow the actuarial value to vary by the size of out-of-pocket (OOP) health shocks, we construct utilization measures for each quintile of the OOP spending distribution (e.g., number of office visits in the bottom quintile, second quintile, and so forth of overall OOP spending). We then re-estimate our actuarial value regression using these different utilization measures. In the following, Figure A9 shows plots of the effect by quintile; Table A10 shows the parameter estimates. At a three-year horizon, the effect on actuarial value ranges from 2.0 cents for the bottom quintile of realized utilization to 18.1 cents for the top. The increasing actuarial values indicate that individuals with higher out-of-pocket spending benefit more from, for example, a reduced copay or drug coverage.

The increasing actuarial values imply that the benefits expansion transfers resources from low OOP spending states of the world (where the marginal utility of consumption is lower) to high OOP spending states of the world (where the marginal utility of consumption is higher). This is valuable to risk averse individuals. If we assume that individuals have CRRA preferences, then the marginal utility of a benefits expansion at a given OOP spending quintile relative to that of receiving benefits expansion when you have average OOP spending is given by:

$$\text{Relative marginal utility of consumption} = \frac{\left(c - OOP_j\right)^{-\gamma}}{\left(c - \overline{OOP}\right)^{-\gamma}},$$

where c is consumption, OOP_j is out-of-pocket spending in quintile j, and $\overline{OOP}$ is average OOP spending.

Column 4 of Table A11 displays the relative marginal utility for each OOP spending quintile. We assume that the coefficient of relative risk aversion is $\gamma = 3$ and individuals have consumption of \$26,533, the mean consumption for elderly individuals in the 2000 Consumer Expenditure Survey. For individuals in the lowest out-of-pocket spending quintile, the marginal utility of consumption is about 11% less than for those with average out-of-pocket spending; for individuals in the highest quintile, the marginal utility of consumption is 30% more than for those with average out-of-pocket spending.

Given these parameters, we can account for risk aversion by calculating the weighted average of the actuarial value estimates across quintiles, where the weights are the relative marginal utilities of consumption. Re-weighting in this manner increases the actuarial value by just over 1 cent on the dollar, from 8.7 cents to 9.8 cents. While a one cent increase is a meaningful relative to the baseline effect on the actuarial value of pass-through in benefits of 8.7 cents, this increase is small compared to baseline total pass-through in premiums and plan benefits of 54 cents.

These effects are small because given the observed OOP sending dispersion and plausible assumptions about risk aversion, the marginal utility of money varies relatively little in the range of OOP spending we observe. Generating a meaningful increase in the value of plan benefits pass-through would require an implausibly high level of risk aversion. For instance, increasing the value by 4.5 cents (or 50% of the baseline actuarial value estimate) would require a risk aversion coefficient of 10, which is well above the range of estimates in the literature. Thus, we conclude that adjusting for risk aversion does not have a material effect on our results.

A.7 Plan Quality

In Section 4, we argue that focusing on premiums and benefits such as copays, drug, and dental coverage captures most of the quantitatively important changes in plan characteristics. In this section, we show that other observable measures of plan quality are not related to our identifying variation.

We begin by examining three measures of plan quality that were potentially the most salient because they were reported in the *Medicare & You* booklet that was mailed to Medicare eligibles on an annual basis during our time period (Dafny and Dranove, 2008). These are the percentage of enrollees that rate the quality of care received as a 10 out of 10, the percentage of enrollees who reported that the doctors in their plan always communicate well, and the mean mammography rate among eligible female enrollees. The first two measures are taken from an annual independent survey of Medicare beneficiaries known as the Consumer Assessment of Health Plans Survey (CAHPS). The third measure is taken from the Health Plan Employer Data and Information Set (HEDIS), which collects standardized performance measures that plans are required to report to CMS.

Following Dafny and Dranove (2008), we also create an "unreported quality composite" to capture plan quality not reported to Medicare beneficiaries. Specifically, this composite is the average z-score of three additional HEDIS measures collected by CMS but not reported to beneficiaries: the percentage of diabetic enrollees who had a retinal examination in the past year, the percentage of enrollees receiving a beta blocker prescription upon discharge from the hospital after a heart attack, and the percentage of enrollees who had an ambulatory visit or preventive care visit in the past year.

We are able to construct these plan quality measures for the years 1999 to 2003, with the exception of the mean mammography rate for which we have data going back to 1997. We repeat our main specification replacing the dependent variable with these measures of plan quality. The results are reported in Table A12 and Figure A10. For each of these measures of plan quality, we find there is no relationship with our identifying variation.

A.8 Baseline Estimation: Alternative Sample Definition

Our baseline estimates described in the text use the unbalanced sample of county-years with MA plans, including county fixed effects in all of our specifications. Figure 7, described in Section 4, illustrates that there is little evidence of systematic entry or exit from the sample based on our identifying variation. Still, as a robustness check, we repeat our analysis using the balanced sample of counties that have an MA plan in every year in our sample, 1997-2003. The balanced panel has 343 counties per year. Of the counties with MA at some point during our time period, 61% are in the balanced panel. The balanced panel covers 54% of Medicare beneficiaries and 89% of MA enrollees over the pooled sample period. The results of baseline regressions repeated on the balanced panel can be found in Figures A11, A12, A13, A14, A15, A16 and Tables A13, A14, A15, A16 and A17.

A.9 Selection: Alternative Specifications

Section 6 investigates the role of selection in explaining our incomplete pass-through estimates. Table 7 and Figure 9 display the results with the baseline set of controls. Table A3 shows that these results are robust to including controls that isolate different subsets of the identifying variation. Columns 2, 5, and 8 in the table control for quartiles of the year 2000 base payment interacted with year fixed effects. Columns 3, 6, and 9 control for urban status of the county interacted with year fixed effects. Columns 1, 4, and 7 display the baseline specifications for comparison.

In addition to investigating the impact of alternative controls, we also investigate robustness with respect to alternative measures of utilization. Figure A17 displays the difference-in-differences results for three alternative utilization measures: Part A hospital stays, Part A hospital days, and Part B physician line-item claims. The corresponding estimates are displayed in Table A18. The point estimates confirm the main finding that there is little selection, and the standard errors allow us to rule out meaningful degrees of selection in either direction. The effect of BIPA on Part A days and Part B line-item claims is statistically indistinguishable from zero in each year. The point estimate for Part A stays is statistically indistinguishable from zero in 2001 and statistically distinguishable from zero in 2002 and 2003; however, in all years, the magnitude is economically very small. For example, drawing on the estimates in columns 1, 4, and 7 of Table A18, the semi-elasticities of utilization with respect to MA enrollment for 2003 were 0.39 ($= \frac{0.0006}{0.0321}/4.74\%$) for Part A stays, 0.28 ($= \frac{0.003}{0.2249}/4.74\%$) for Part A days, and 0.21 ($= \frac{0.022}{2.187}/4.74\%$) for Part B claims. Overall, these elasticities are similar to the elasticity implied by our cost estimates discussed in the text.

A.10 Pass-Through Under Risk Adjustment

Equation 7 in Section 5 gives the first-order condition for price setting, ignoring risk adjustment. To incorporate risk adjustment, let us define the aggregate risk adjustment function $R(Q) = \int_{v_i \geq p^{-1}(Q)} r_i$, average risk adjustment $AR(Q) \equiv \frac{R(Q)}{Q}$, and marginal risk adjustment $MR(Q) \equiv R'(Q)$. The regulator sets the subsidy equal to $b \cdot AR(Q)$ so that total payments per capita are $p + b \cdot AR(Q)$. This generates the following monopolist problem:

$$\max_p \Big[p + b \cdot AR(Q(p))\Big] Q(p) - C(Q(p)), \tag{14}$$

$$\max_p \; pQ(p) + b \cdot R(Q(p)) - C(Q(p)), \tag{15}$$

where we have substituted $AR(Q(p)) \cdot Q(p) = R(Q(p))$ between the first and second lines.

The competitive pricing problem simply equates price with average net costs $(AC(Q) - b \cdot AR(Q))$. As in the main text, we use the parameter $\theta \in [0, 1]$ to interpolate between the price-setting equations for perfect competition and monopoly, yielding

$$p = \theta \Big[\mu(p) + MC(Q) - b \cdot MR(Q)\Big] + (1 - \theta) \Big[AC(Q) - b \cdot AR(Q)\Big], \tag{16}$$

where $\mu(p) \equiv -\dfrac{Q(p)}{Q'(p)}$ denotes the standard absolute markup term and $MC(Q) - b \cdot MR(Q)$ is marginal costs net of marginal risk adjustment. Totally differentiating and rearranging Equation 16 results in the pass-through formula in Equation 10.

A.11 Pass-through in Linear Model

Suppose costs are linear, risk adjustment curves are linear, and demand is linear. In this case, our main expression for pass-through in Equation 10 simplifies to

$$\rho = (AR + \theta(MR - AR)) \times \left(\frac{1}{1 - (\frac{dAC}{dp} - b\frac{dAR}{dp})} \right) \times \frac{1}{1+\theta}. \tag{17}$$

Putting aside the first term, which simply accounts for risk adjustment, the remaining two terms capture the main mechanisms that determine pass-through: the second term captures the degree of selection and the third term captures the degree of market power. Thus, in the linear case, we can think about the the degree of advantageous selection proportionally scaling down the predicted pass-through for any given level of market power.

A.12 Inferring MA Costs

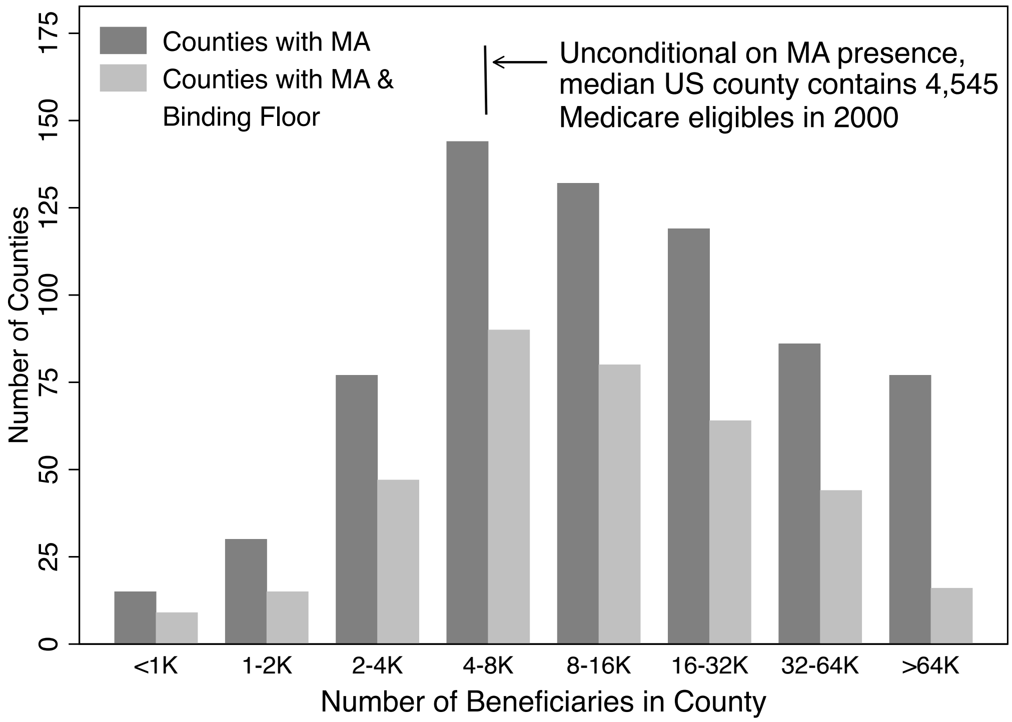
In Section 6, we claim that the slopes of MA and TM average cost curves are of opposite sign and proportional $\left(\frac{dAC^{MA}}{dQ^{MA}} = -\phi\frac{dAC^{TM}}{dQ^{TM}}\right)$ under the assumptions that (i) MA and TM costs are proportionally constant $\left(\frac{c_i^{MA}}{c_i^{TM}} = \phi\right)$ and (ii) average costs under both plans are linear in quantity.

The proof is as follows. The assumption that costs are proportional implies that the marginal individual in MA and TM are proportionally costly: $MC^{MA}(Q^{MA}) = \phi MC^{TM}(Q^{TM})$. This implies $\frac{dMC^{MA}}{dQ^{MA}} = \phi\frac{dMC^{TM}}{dQ^{TM}}\frac{dQ^{TM}}{dQ^{MA}} = -\phi\frac{dMC^{TM}}{dQ^{TM}}$, with the last equality from the fact that $Q^{TM} = 1 - Q^{MA}$. Linearity means we can translate between the slopes of the average and marginal cost functions to get $\frac{dAC^i}{dQ^i} = \frac{1}{2}\frac{dMC^i}{dQ^i}$ for $i \in \{MA, TM\}$. Combining this, we get $\frac{dAC^{MA}}{dQ^{MA}} = -\phi\frac{dAC^{TM}}{dQ^{TM}}$.

A.13 Pass-Through by Market Concentration: Alternative Specifications

Figure 10 in the main text displays heterogeneity in our pass-through estimates by pre-reform market concentration for 2003 only. Figure A8 repeats the same analysis for all of the post-reform years. The figure displays the pass-through point estimates as well as the 95% confidence intervals. Each point represents a separate regression performed over sub-samples defined by levels of pre-reform market concentration. Table A5 displays the corresponding regression results as well as results for full-sample regressions that interact the market concentration measures with our floor distance variables (Δb_{jt}). Overall, the coefficients show a statistically significant pattern of declining pass-through with market concentration.

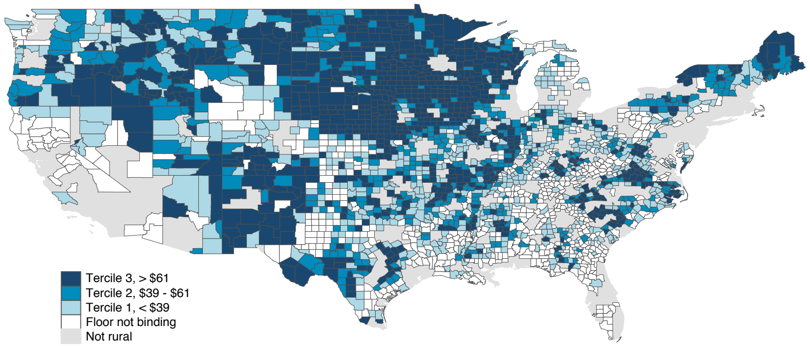
Figure A1: Distribution of Medicare Beneficiaries Across Counties



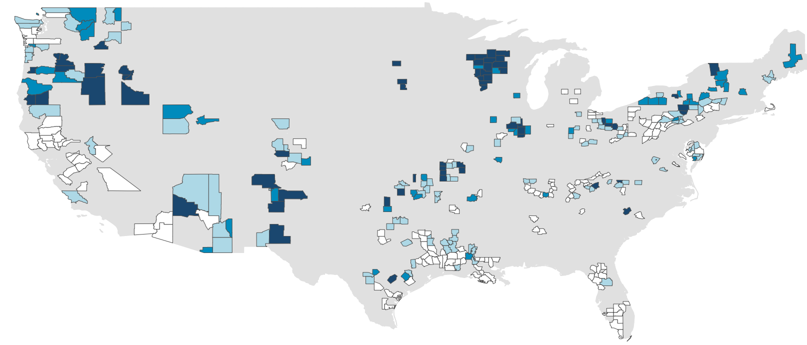
Note: Figure shows the distribution of the number of beneficiaries for counties with MA, and those additionally with binding BIPA floors. The sample is the 680 counties that include 67% of the Medicare population in 2000.

Figure A2: Effect of BIPA on County Base Payments

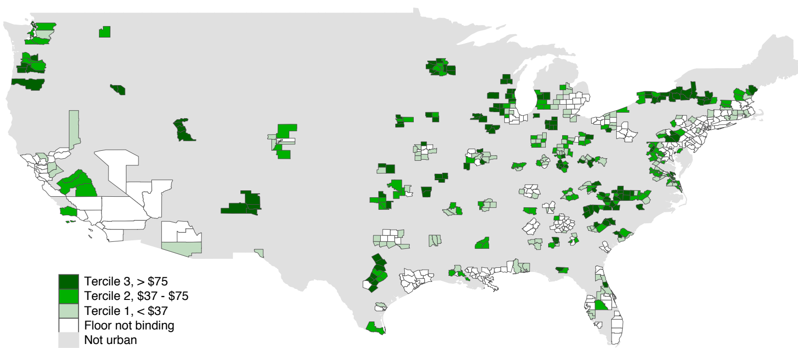
(A) Floor Distance, All Rural Counties



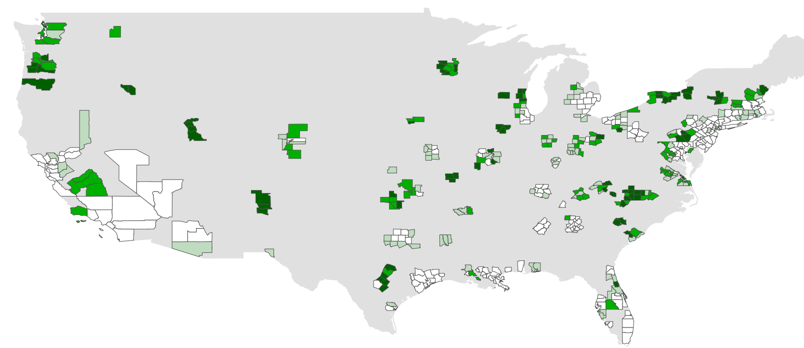
(B) Floor Distance, Rural Counties with MA



(C) Floor Distance, All Urban Counties

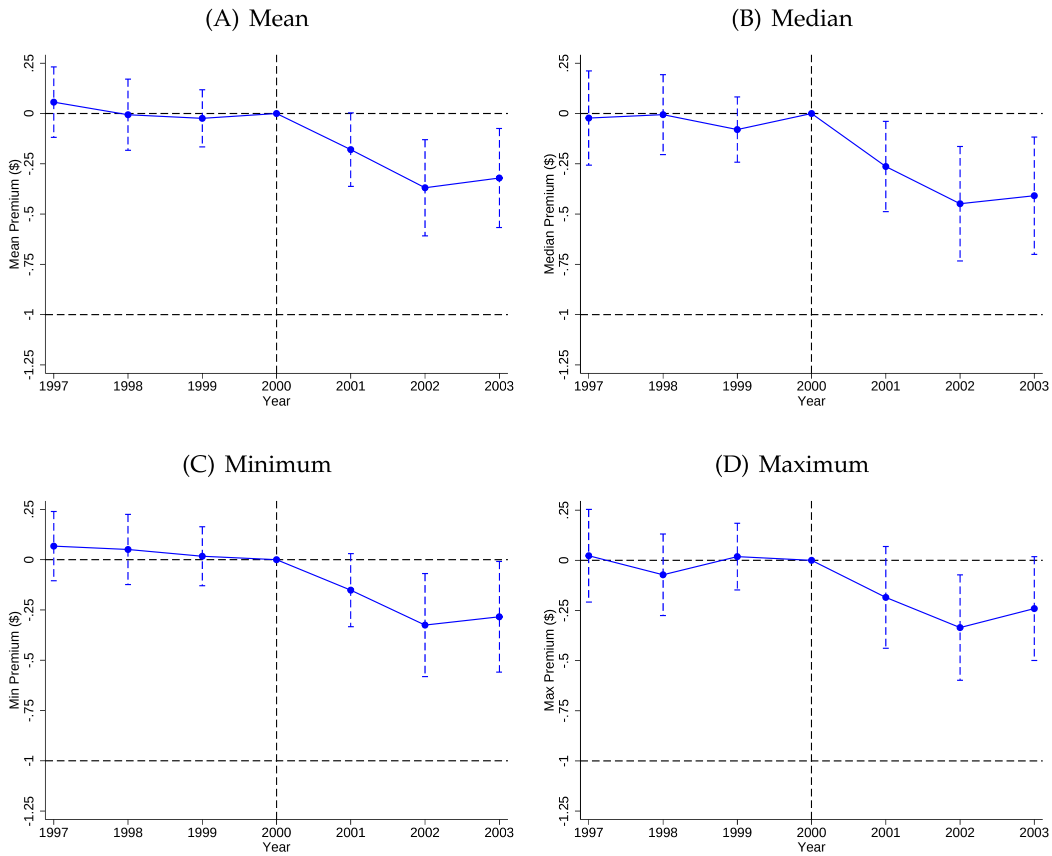


(D) Floor Distance, Urban Counties with MA



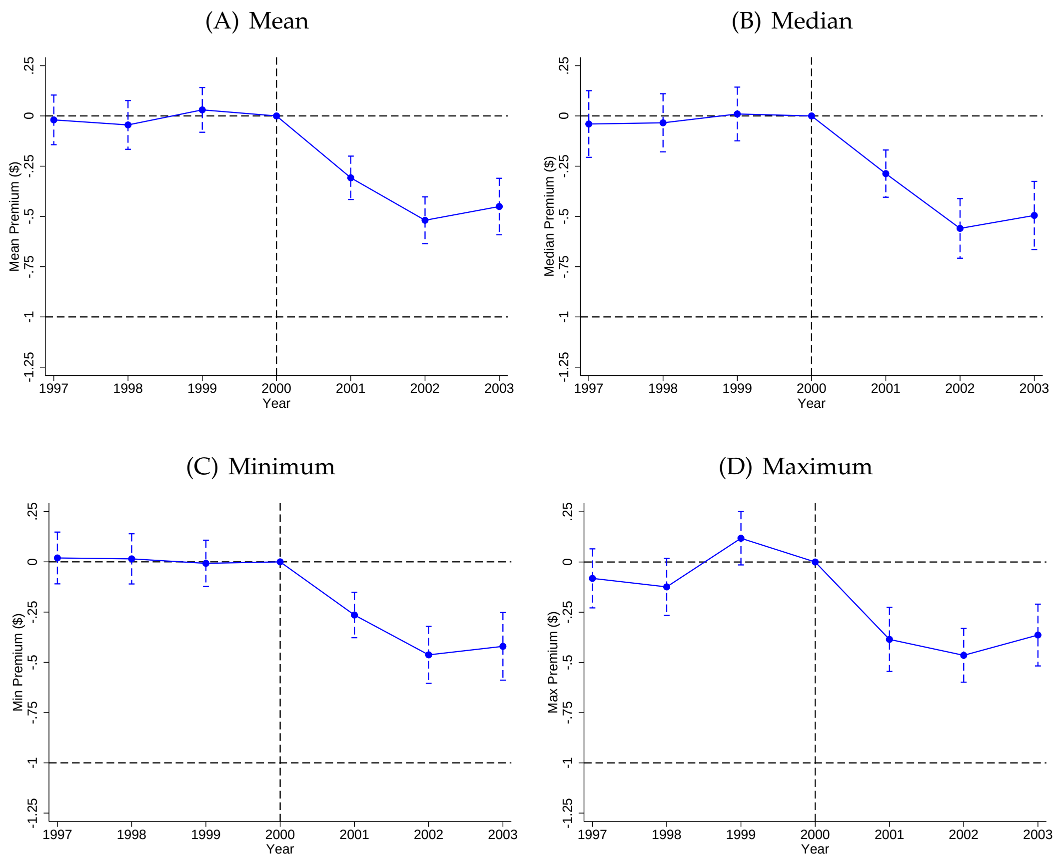
Note: Map shows the geography of the identifying variation across urban and rural counties. Counties are binned according to their tercile of distance-to-floor, separately for rural counties (Panels A and B) and urban counties (Panels C and D). Panels B and D condition on our main analysis sample, which includes counties with an MA plan in at least one year of the 1997-2003 study period. Legends indicate the bin ranges, and counties for which the floors were not binding are shaded white. The distance-to-floor variable, which describes the payment shock between 2000 and 2001, is defined precisely in Equation (2) and is graphically illustrated in the top panel of Figure 1. Base payments in this figure are not adjusted for inflation and are not normalized for the sample average demographic risk adjustment factor. Alaska and Hawaii are excluded from these maps but included in all of the other analysis. Inclusive of AK and HI, the sample in the left two panels is 3,143 counties that include 100% of the Medicare population in 2000. The sample in the right two panels is 880 counties that include 73% of the Medicare population in 2000.

Figure A3: Premium Pass-Through with Pre-BIPA Payment × Year Fixed Effects



Note: Figure is identical to Figure 4, except that all specifications include quartiles of year 2000 county base payments interacted with year indicators as additional controls. See Figure 4 note for more details.

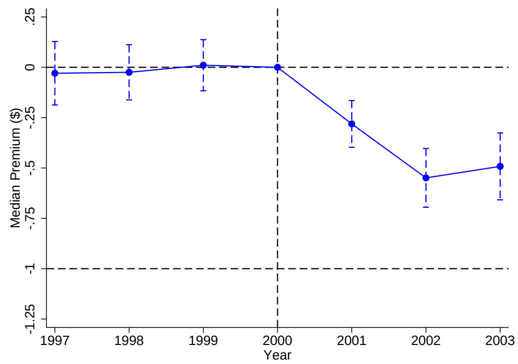
Figure A4: Premium Pass-Through with Urban × Year Fixed Effects



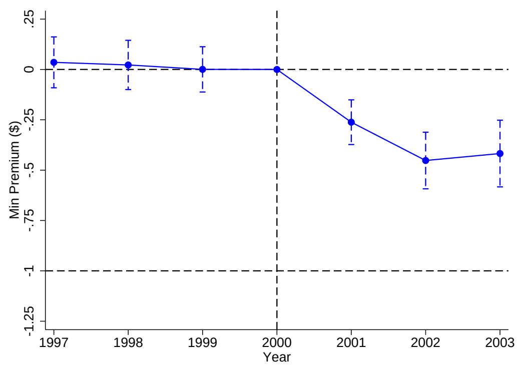
Note: Figure is identical to Figure 4, except that all specifications include urban status interacted with year indicators as additional controls. See Figure 4 note for more details.

Figure A5: Premium Pass-Through (Other Measures): Impact of $1 Increase in Monthly Payments

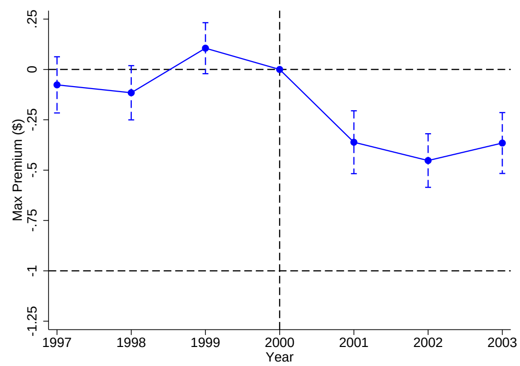
(A) Median



(B) Minimum

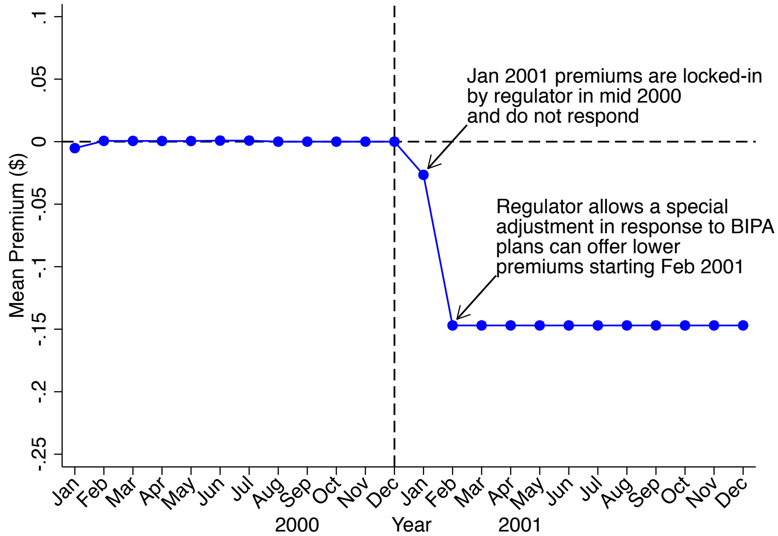


(C) Maximum



Note: Figure shows coefficients on distance-to-floor × year interactions from difference-in-differences regressions. The first-stage results displayed in Table 3 indicate that a $1 change in distance-to-floor translates into a $1 change in the monthly payments, so we can interpret the coefficients as the effect of an increase in monthly payments on a dollar-for-dollar basis. The dependent variables are median monthly premiums (Panel A), minimum monthly premiums (Panel B), and maximum monthly premiums (panel C). The unit of observation is the county × year, and observations are weighted by the number of beneficiaries in the county. The sample is the unbalanced panel of county-years with at least one MA plan over years 1997 to 2003. This sample includes 4,262 of 22,001 possible county-years and 64% of all Medicare beneficiaries. Controls are identical to those in Figure 3. The capped vertical bars show 95% confidence intervals calculated using standard errors clustered at the county level. Horizontal dashed lines are plotted at the reference values of 0 and -1, where -1 corresponds to 100% pass-through.

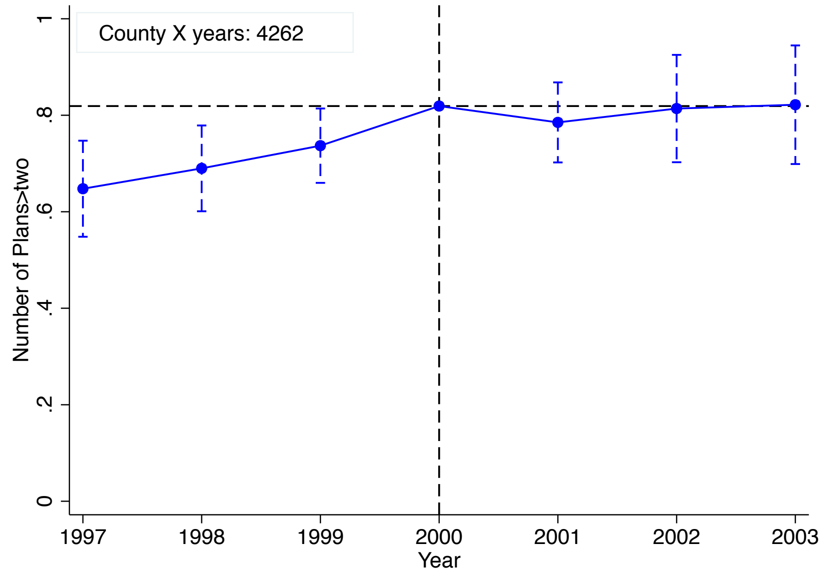
Figure A6: Premium Pass-Through: Detailed Timing of Effects



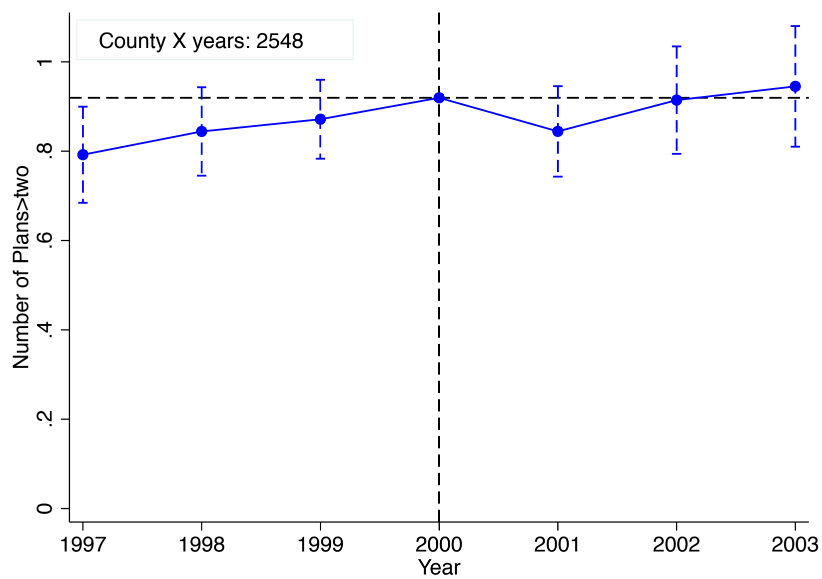
Note: Figure shows coefficients on distance-to-floor $\times$ month interactions from difference-in-differences regressions in which the dependent variable is mean premiums. The specification parallels that used in the county $\times$ year level analysis in Figure 4. The figure highlights January 2001, for which premiums were locked-in prior to the passage of BIPA in December 2000, and February 2001, for which the regulator permitted plans to revise premiums in response to BIPA. See Appendix Section A.2 for full details. The unit of observation is the county $\times$ month, and observations are weighted by the number of beneficiaries in the county. Monthly data are not available for all plan years that comprise our main analysis.

Figure A7: Availability of At Least Two Plans: Impact of $50 Increase in Monthly Payments

(A) Unbalanced Sample



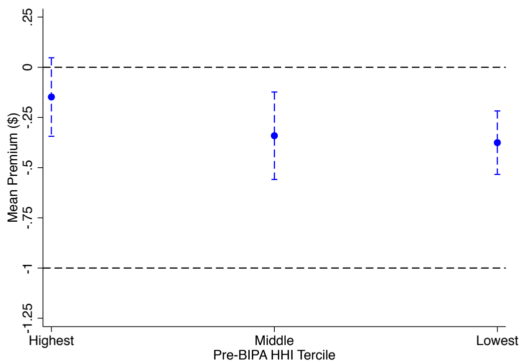
(B) Balanced Sample



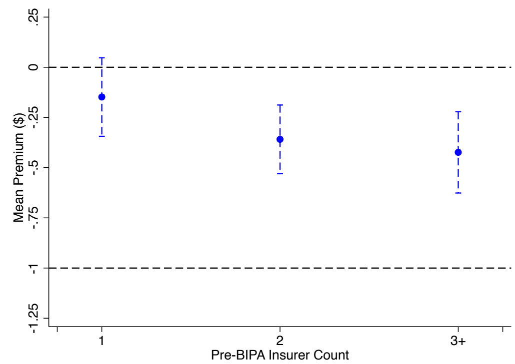
Note: Figure shows scaled coefficients on distance-to-floor $\times$ year interactions from difference-in-differences regressions. The first-stage results displayed in Table 3 indicate that a $1 change in distance-to-floor translates into a $1 change in the monthly payments, so we can interpret the coefficients as the effect of an increase in monthly payments on a dollar-for-dollar basis. Coefficients are scaled to reflect the impact of a $50 increase in monthly payments. The dependent variable in both panels is an indicator for at least two MA plans. The sample in Panel A is the unbalanced panel of county-years with at least one MA plan over years 1997 to 2003. This sample includes 4,262 of 22,001 possible county-years and 64% of all Medicare beneficiaries. The sample in Panel B is the balanced panel of county-years with at least one MA plan in each year between 1997 and 2003. This sample includes 2,548 of 22,001 possible county-years and 54% of all Medicare beneficiaries. Controls are identical to those in Figure 3. The capped vertical bars show 95% confidence intervals calculated using standard errors clustered at the county level. The horizontal dashed lines are plotted at the sample means, which are added to the coefficients.

Figure A8: Pass-Through and Market Concentration, 2001 to 2003

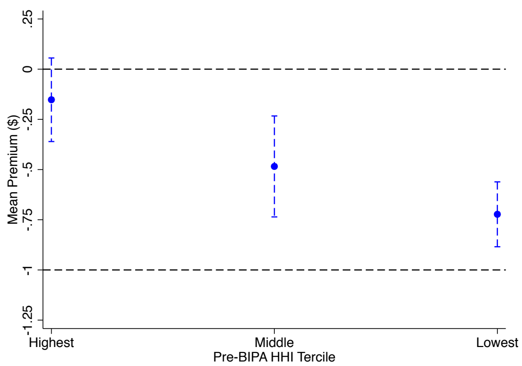
(A) By HHI, 2001



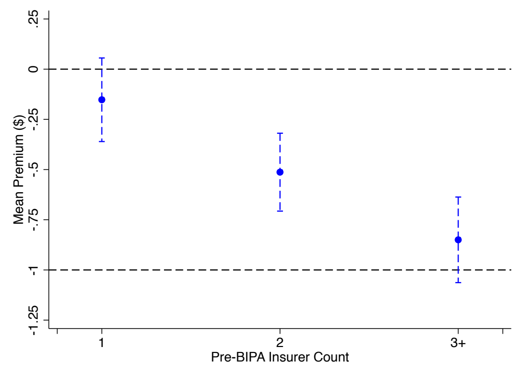
(B) By Insurer Count, 2001



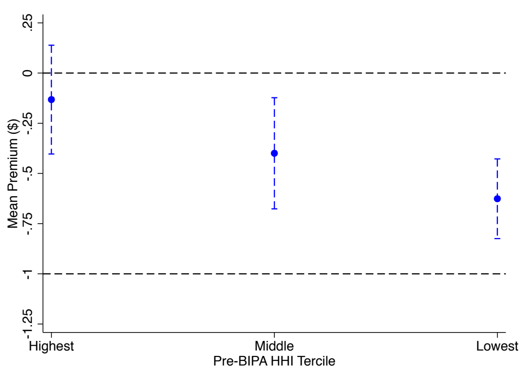
(C) By HHI, 2002



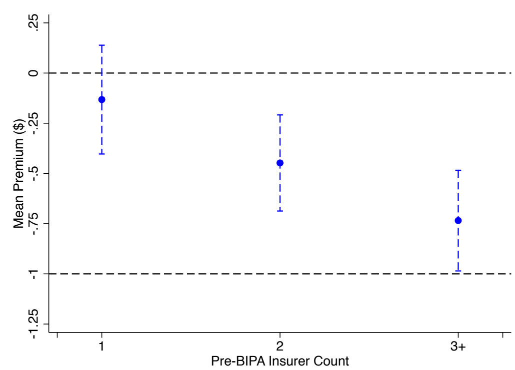
(D) By Insurer Count, 2002



(E) By HHI, 2003



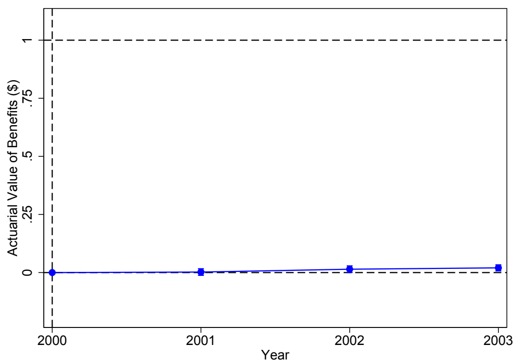
(F) By Insurer Count, 2003



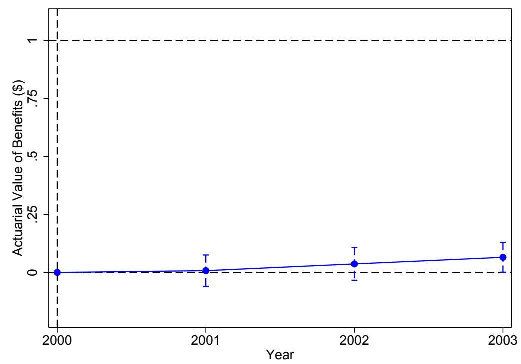
Note: Figure shows coefficients on distance-to-floor × year interactions for plan years 2001 through 2003 from several difference-in-differences regressions. The dependent variable is the mean premium defined as in Figure 4. Each point represents a coefficient from a separate regression in which the estimation sample is stratified by market concentration in the pre-BIPA period. In Panel A, counties are binned according to the tercile of insurer HHI in plan year 2000. In Panel B, counties are binned according to the number of insurers operating in the county in plan year 2000. Competition increases to the right of both panels. The unit of observation is the county × year, and observations are weighted by the number of beneficiaries in the county. While the analysis is conducted on segments of the data, the underlying sample is the unbalanced panel of county-years with at least one MA plan over years 1997 to 2003. This sample includes 4,262 of 22,001 possible county-years and 64% of all Medicare beneficiaries. Controls are identical to those in Figure A11. The capped vertical bars show 95% confidence intervals calculated using standard errors clustered at the county level. Horizontal dashed lines are plotted at the reference values of 0 and -1, where -1 corresponds to 100% pass-through.

Figure A9: Actuarial Value of Benefits: Impact of $1 Increase in Monthly Payments, by Quintile of Out-of-Pocket Spending

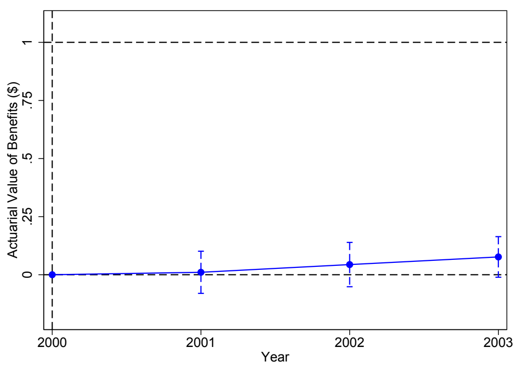
(A) Bottom Quintile



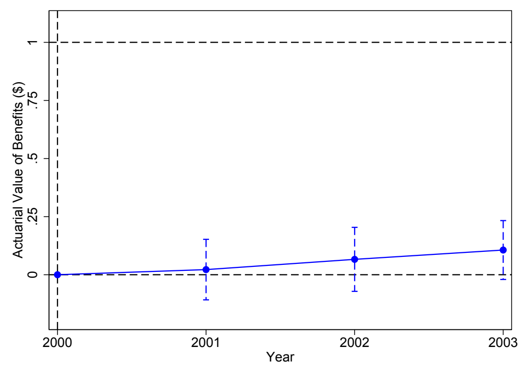
(B) Second Quintile



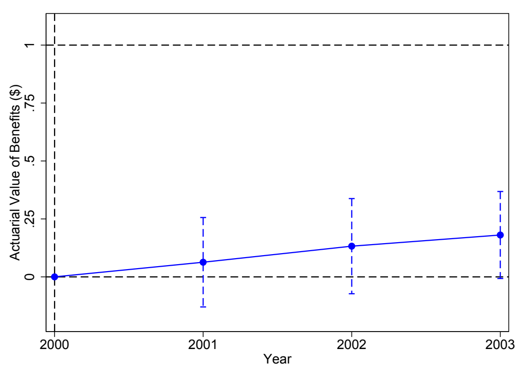
(C) Third Quintile



(D) Fourth Quintile

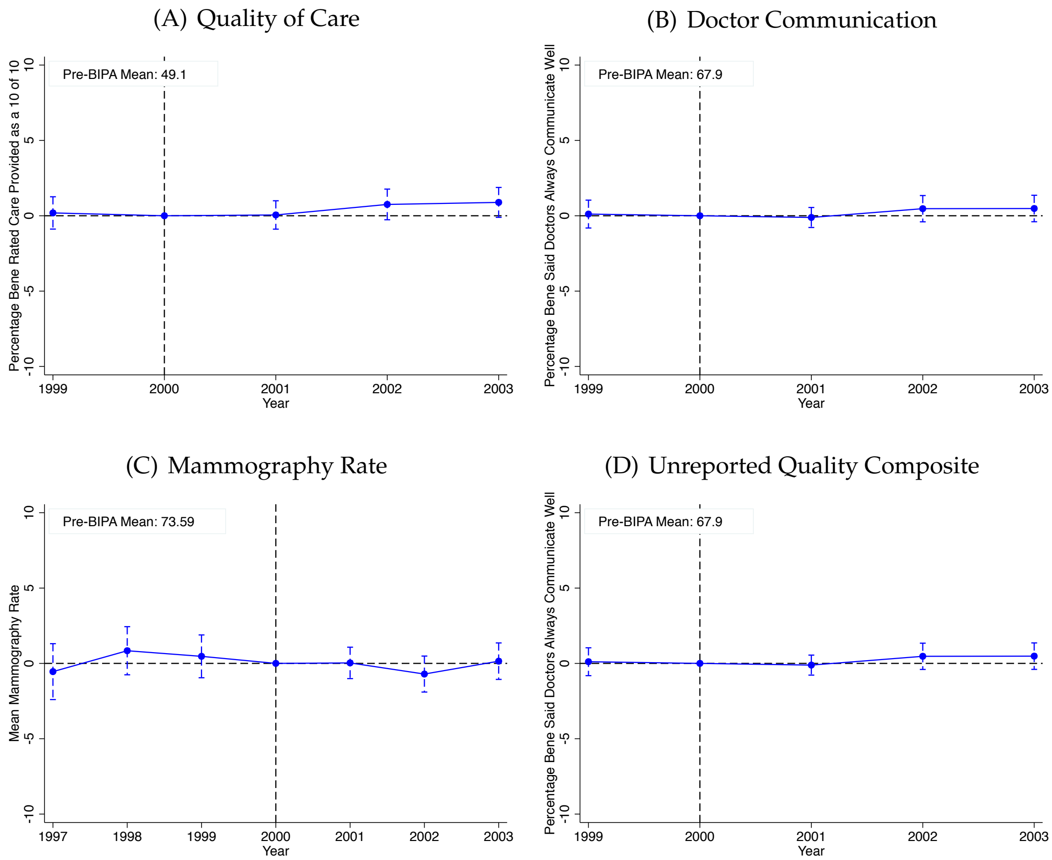


(E) Top Quintile



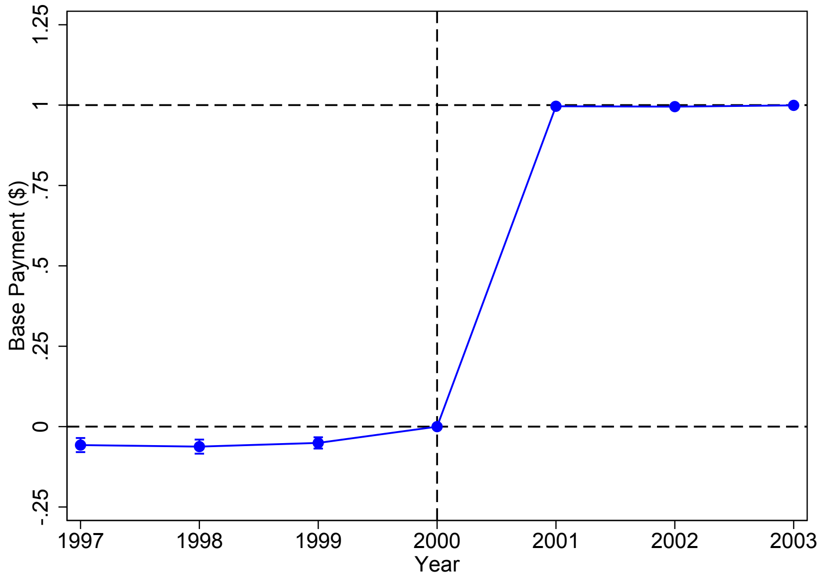
Note: Figure shows coefficients on distance-to-floor $\times$ year interactions from difference-in-differences regressions. The first-stage results displayed in Table 3 indicate that a $1 change in distance-to-floor translates into a $1 change in the monthly payments, so we can interpret the coefficients as the effect of a $1 increase in monthly payments. The dependent variable is the actuarial value of benefits for a given quintile of out-of-pocket spending, which is constructed based on observed plan benefits in our main analysis dataset and utilization and cost data from the 2000 Medical Expenditure Panel Survey. See text for full details. The unit of observation is the county $\times$ year, and observations are weighted by the number of beneficiaries in the county. The sample is the unbalanced panel of county-years with at least one MA plan over years 2000 to 2003. This sample includes 2,250 of 12,572 possible county-years and 62% of all Medicare beneficiaries. Controls are identical to those in Figure 3. The capped vertical bars show 95% confidence intervals calculated using standard errors clustered at the county level. Horizontal dashed lines are plotted at 0 and 1.

Figure A10: Plan Quality: Impact of $50 Increase in Monthly Payments



Note: Figure shows scaled coefficients on distance-to-floor × year interactions from difference-in-differences regressions. The first-stage results displayed in Table 3 indicate that a $1 change in distance-to-floor translates into a $1 change in the monthly payments, so we can interpret the coefficients as the effect of an increase in monthly payments on a dollar-for-dollar basis. The dependent variables are the mean percentage of beneficiaries that rate the quality of care received as a 10 out of 10 (Panel A), mean percentage of beneficiaries that report that the doctors in their plan always communicate well (Panel B), mean mammography rate (Panel C), and an unreported quality composite described in the text (Panel D). We have data on these measures from 1999 through 2003, with the exception of the mean mammography rate for which we have data going back to 1997. The unit of observation is the county × year, and observations are weighted by the number of beneficiaries in the county. In Panels A, B, and D, the sample is the unbalanced panel of county-years with at least one MA plan over years 1999 to 2003. This sample includes 2,892 of 15,715 possible county-years and 63% of all Medicare beneficiaries. In Panel C, the sample is the unbalanced panel of county-years with at least one MA plan over years 1997 to 2003. This sample includes 4,262 of 22,001 possible county-years and 64% of all Medicare beneficiaries. Controls are identical to those in Figure 3. In all the panels, the vertical axes measures the effect on the dependent variable of a $50 difference in monthly payments. The capped vertical bars show 95% confidence intervals calculated using standard errors clustered at the county level. Year 2000, which is the year prior to BIPA implementation, is the omitted category. The horizontal dashed line is plotted at 0.

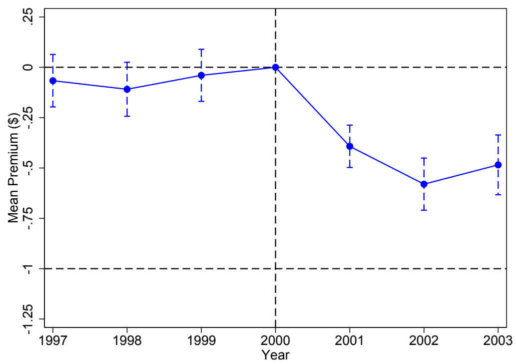
Figure A11: First-Stage Effect on Base Payments: Impact of $1 Increase in Distance-to-Floor, Balanced Sample of Counties



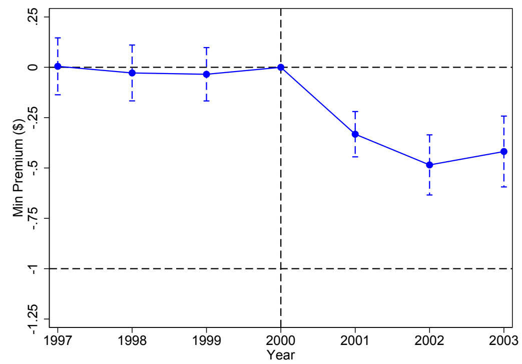
Note: Figure shows coefficients on the distance-to-floor × year interactions from difference-in-differences regressions with the monthly base payments as the dependent variable. The unit of observation is the county × year, and observations are weighted by the number of beneficiaries in the county. The sample is the balanced sample of county-years with at least one MA plan in each year between 1997 and 2003. This sample includes 2,548 out of 22,001 possible county-years and 54% of all Medicare beneficiaries. Controls include year and county fixed effects as well as flexible controls for the 1998 payment floor introduction and the blended payment increase in 2000. The capped vertical bars show 95% confidence intervals calculated using standard errors clustered at the county level. Year 2000, which is the year prior to BIPA implementation, is the omitted category and denoted with a vertical dashed line. Horizontal dashed lines are plotted at the reference values of 0 and 1.

Figure A12: Premium Pass-Through: Impact of $1 Increase in Monthly Payments, Balanced Sample of Counties

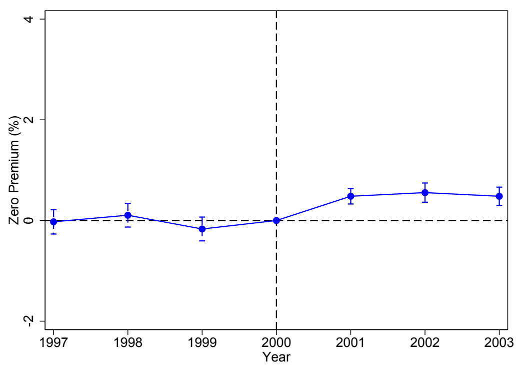
(A) Mean



(B) Minimum



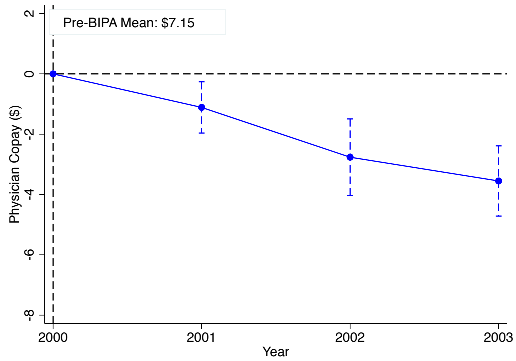
(C) Percent Zero



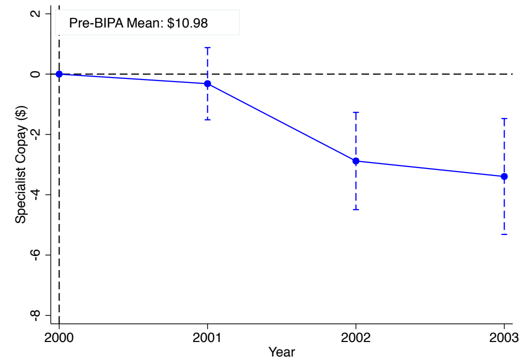
Note: Figure shows coefficients on distance-to-floor × year interactions from difference-in-differences regressions. The first-stage results displayed in Table A13 indicate that a $1 change in distance-to-floor translates into a $1 change in the monthly payments, so we can interpret the coefficients as the effect of an increase in monthly payments on a dollar-for-dollar basis. The dependent variables are mean monthly premiums weighted by enrollment in the plan (Panel A), minimum monthly premiums (Panel B), and the percentage of plans in the county with zero premiums (Panel C). The unit of observation is the county × year, and observations are weighted by the number of beneficiaries in the county. The sample is the balanced sample of county-years with at least one MA plan in each year between 1997 and 2003. This sample includes 2,548 out of 22,001 possible county-years and 54% of all Medicare beneficiaries. Controls are identical to those in Figure A11. The capped vertical bars show 95% confidence intervals calculated using standard errors clustered at the county level. Horizontal dashed lines in Panels A and B are plotted at the reference values of 0 and -1, where -1 corresponds to 100% pass-through.

Figure A13: Benefits Generosity: Impact of $50 Increase in Monthly Payments, Balanced Sample of Counties

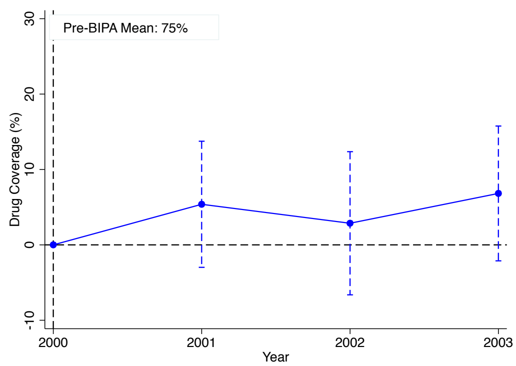
(A) Physician Copay



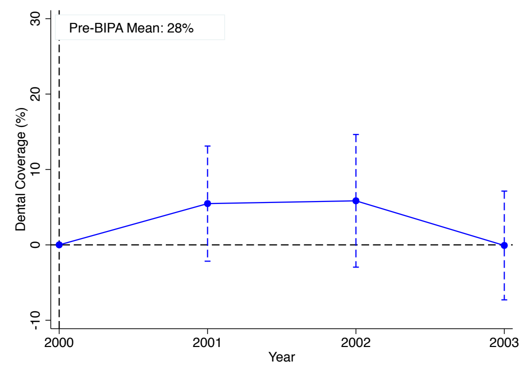
(B) Specialist Copay



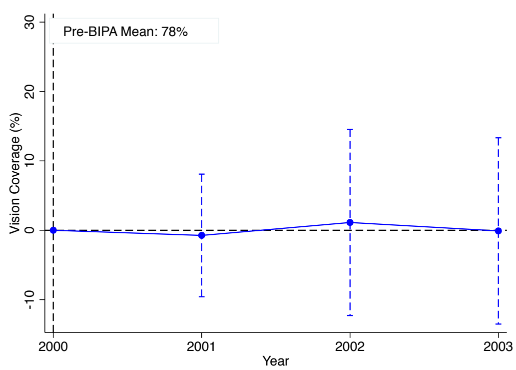
(C) Drug Coverage



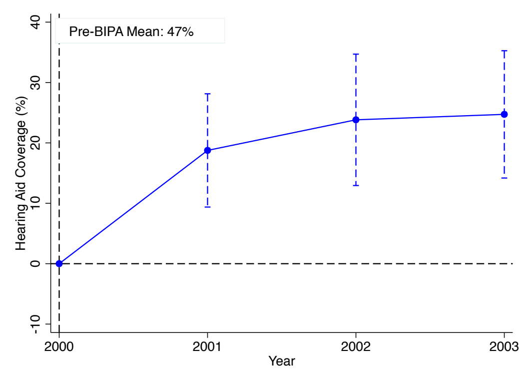
(D) Dental Coverage



(E) Vision Coverage

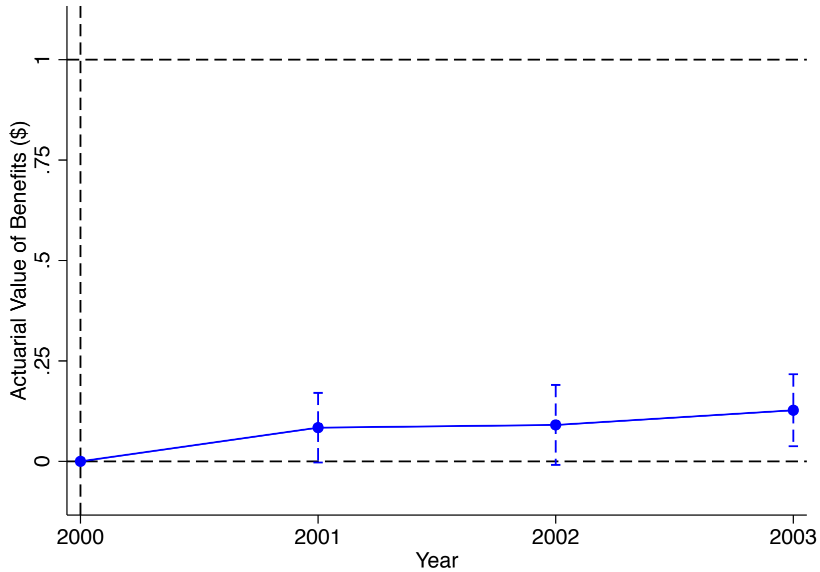


(F) Hearing Aid Coverage



Note: Figure shows scaled coefficients on distance-to-floor × year interactions from difference-in-differences regressions. The first-stage results displayed in Table A13 indicate that a $1 change in distance-to-floor translates into a $1 change in the monthly payments, so we can interpret the coefficients as the effect of an increase in monthly payments on a dollar-for-dollar basis. The dependent variables are physician copays in dollars (Panel A), specialist copays in dollars (Panel B), and indicators for coverage of: outpatient prescription drugs (Panel C), dental (Panel D), corrective lenses (Panel E), and hearing aids (Panel F). The unit of observation is the county × year, and observations are weighted by the number of beneficiaries in the county. The sample is the balanced sample of county-years with at least one MA plan in each year between 2000 and 2003. This sample includes 1,772 out of 12,572 possible county-years and 57% of all Medicare beneficiaries. Controls are identical to those in Figure A11. In Panels A and B, the vertical axes measure the effect on copays in dollars of a $50 difference in monthly payments. In Panels C through F, the vertical axes measure the effect on the probability that a plan offers each benefit, again for a $50 difference in monthly payments. The capped vertical bars show 95% confidence intervals calculated using standard errors clustered at the county level. Year 2000, which is the year prior to BIPA implementation, is the omitted category. The horizontal dashed line is plotted at 0.

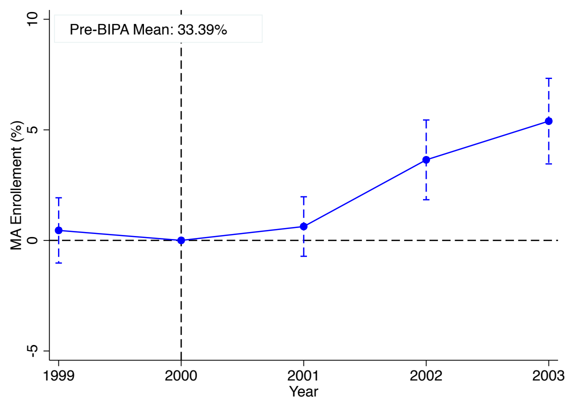
Figure A14: Actuarial Value of Benefits: Impact of $1 Increase in Monthly Payments, Balanced Sample of Counties



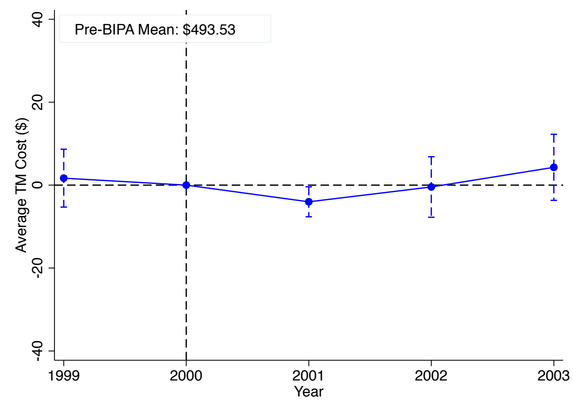
Note: Figure shows coefficients on distance-to-floor $\times$ year interactions from difference-in-differences regressions. The first-stage results displayed in Table A13 indicate that a $1 change in distance-to-floor translates into a $1 change in the monthly payments, so we can interpret the coefficients as the effect of an increase in monthly payments on a dollar-for-dollar basis. The dependent variable is the actuarial value of benefits, which is constructed based on observed plan benefits in our main analysis dataset and utilization and cost data from the 2000 Medical Expenditure Panel Survey. See text for full details. The unit of observation is the county $\times$ year, and observations are weighted by the number of beneficiaries in the county. The sample is the balanced sample of county-years with at least one MA plan in each year between 2000 and 2003. This sample includes 1,772 out of 12,572 possible county-years and 57% of all Medicare beneficiaries. Controls are identical to those in Figure A11. The capped vertical bars show 95% confidence intervals calculated using standard errors clustered at the county level. Horizontal dashed lines are plotted at 0 and 1.

Figure A15: Selection: Impact of \$50 Increase in Monthly Payments, Balanced Sample of Counties

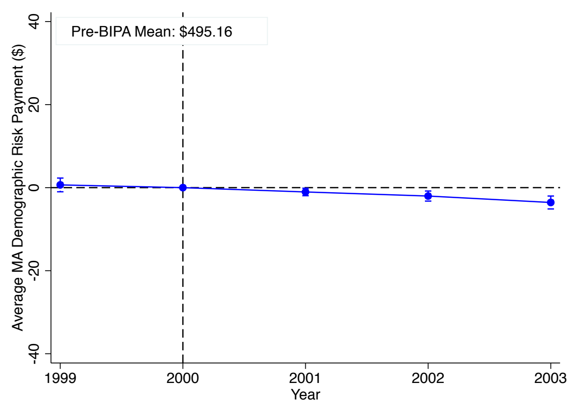
(A) MA Enrollment



(B) TM Costs



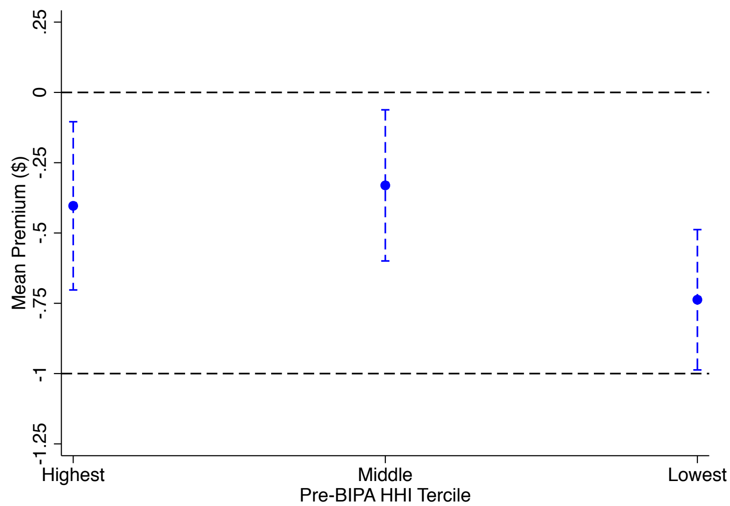
(C) MA Risk Adjustment



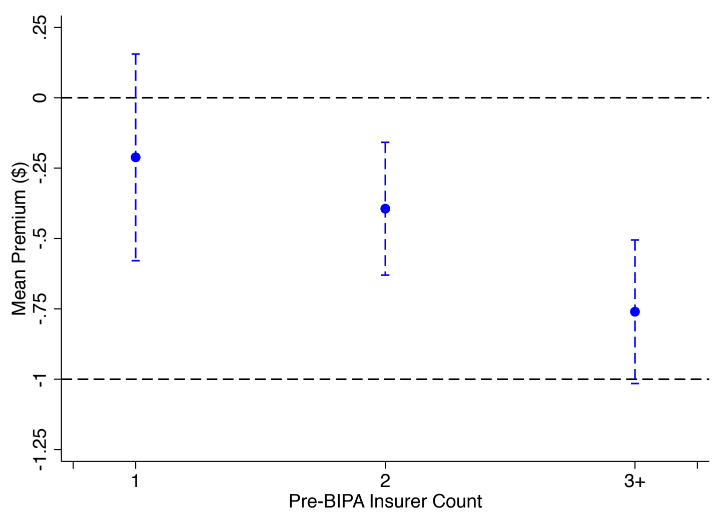
Note: Figure shows scaled coefficients on distance-to-floor × year interactions from difference-in-differences regressions. The first-stage results displayed in Table A13 indicate that a \$1 change in distance-to-floor translates into a \$1 change in the monthly payments, so we can interpret the coefficients as the effect of an increase in monthly payments on a dollar-for-dollar basis. Coefficients are scaled to reflect the impact of a \$50 increase in monthly payments. The dependent variables are MA enrollment (Panel A), Traditional Medicare costs (Panel B), and mean demographic risk payments for MA enrollees (Panel C). The unit of observation is the county × year, and observations are weighted by the number of beneficiaries in the county. The sample is the balanced panel of county-years with at least one MA plan in each year between 1999 and 2003. This sample includes 2,055 out of 15,715 possible county-years and 56% of all Medicare beneficiaries. Controls are identical to those in Figure A11. The capped vertical bars show 95% confidence intervals calculated using standard errors clustered at the county level. The horizontal dashed lines indicate zero effects.

Figure A16: Pass-Through and Market Concentration, Balanced Sample of Counties

(A) Insurer HHI



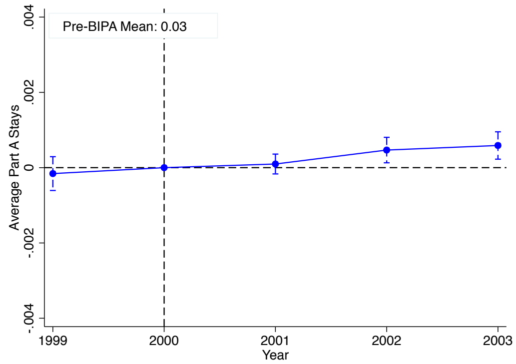
(B) Insurer Count



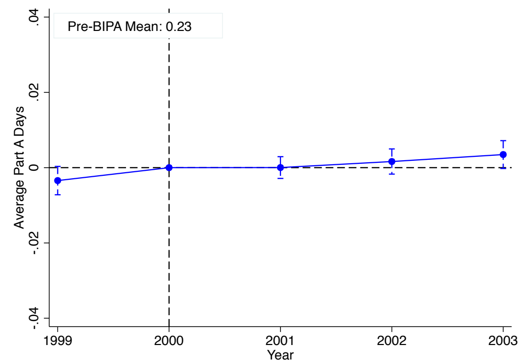
Note: Figure shows coefficients on distance-to-floor × year 2003 interactions from several difference-in-differences regressions. The dependent variable is the mean premium defined as in Figure 4. Each point represents a coefficient from a separate regression in which the estimation sample is stratified by market concentration in the pre-BIPA period. In Panel A, counties are binned according to the tercile of insurer HHI in plan year 2000. In Panel B, counties are binned according to the number of insurers operating in the county in plan year 2000. Competition increases to the right of both panels. The unit of observation is the county × year, and observations are weighted by the number of beneficiaries in the county. While the analysis is conducted on segments of the data, the underlying sample is the balanced panel of county-years with at least one MA plan in each year between 1997 and 2003. This sample includes 2,548 of 22,001 possible county-years and 54% of all Medicare beneficiaries. Controls are identical to those in Figure A11. The capped vertical bars show 95% confidence intervals calculated using standard errors clustered at the county level. Horizontal dashed lines are plotted at the reference values of 0 and -1, where -1 corresponds to 100% pass-through.

Figure A17: Utilization: Impact of $50 Increase in Monthly Payments

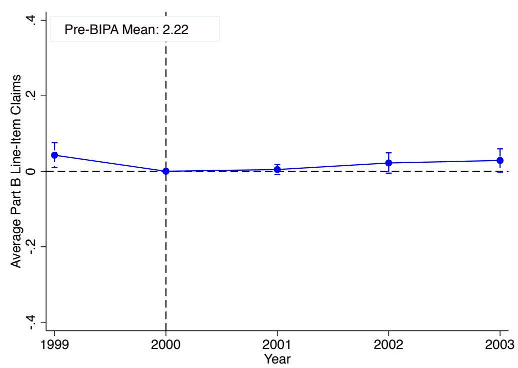
(A) Part A Stays



(B) Part A Days



(C) Part B Line-Item Claims



Note: Figure shows scaled coefficients on distance-to-floor $\times$ year interactions from difference-in-differences regressions. The first-stage results displayed in Table 3 indicate that a $1 change in distance-to-floor translates into a $1 change in the monthly payments, so we can interpret the coefficients as the effect of an increase in monthly payments on a dollar-for-dollar basis. Coefficients are scaled to reflect the impact of a $50 increase in monthly payments. The dependent variables are Part A hospital stays (Panel A), Part A hospital days (Panel B), and Part B physician line-item claims (Panel C). The unit of observation is the county $\times$ year, and observations are weighted by the number of beneficiaries in the county. The sample is the unbalanced panel of county-years with at least one MA plan over years 1999 to 2003. This sample includes 2,892 of 15,715 possible county-years and 63% of all Medicare beneficiaries. Controls are identical to those in Figure A11. The capped vertical bars show 95% confidence intervals calculated using standard errors clustered at the county level. The horizontal dashed lines indicate zero effects.

Table A1: Premium Pass-Through (Other Measures): Impact of $1 Increase in Monthly Payments

	Dependent Variable:								
	Median Monthly Premium ($)			Minimum Monthly Premium ($)			Maximum Monthly Premium ($)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Δb X 2001	-0.281	-0.264	-0.287	-0.262	-0.152	-0.264	-0.361	-0.185	-0.385
	(0.059)	(0.115)	(0.060)	(0.057)	(0.093)	(0.058)	(0.080)	(0.129)	(0.081)
Δb X 2002	-0.549	-0.449	-0.559	-0.452	-0.325	-0.463	-0.452	-0.335	-0.465
	(0.074)	(0.145)	(0.076)	(0.072)	(0.131)	(0.072)	(0.068)	(0.134)	(0.068)
Δb X 2003	-0.492	-0.409	-0.495	-0.417	-0.284	-0.420	-0.365	-0.241	-0.364
	(0.085)	(0.149)	(0.086)	(0.084)	(0.140)	(0.086)	(0.077)	(0.132)	(0.078)
Main Effects									
County FE	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X
Additional Controls									
Pre-BIPA Payment X Year FE		X			X			X	
Urban X Year FE			X			X			X
Pre-BIPA Mean of Dep. Var.	12.10	12.10	12.10	6.67	6.67	6.67	20.02	20.02	20.02
R-Squared	0.66	0.66	0.66	0.66	0.66	0.66	0.68	0.68	0.68

Note: Table shows coefficients on distance-to-floor $\times$ year interactions from difference-in-differences regressions. Although the estimation includes distance-to-floor interactions for all the years in our sample, we display coefficients for the post-reform years (2001-2003) above for brevity. The first-stage results displayed in Table 3 indicate that a $1 change in distance-to-floor translates into a $1 change in the monthly payments, so we can interpret the coefficients as the effect of an increase in monthly payments on a dollar-for-dollar basis. The unit of observation is the county $\times$ year, and observations are weighted by the number of beneficiaries in the county. The county-level measures are constructed using plan-level data weighted by plan enrollment. The sample is the unbalanced panel of county-years with at least one MA plan over years 1997 to 2003. This sample includes 4,262 of 22,001 possible county-years and 64% of all Medicare beneficiaries. Year 2000, which is the year prior to BIPA implementation, is the omitted category. Controls are identical to those in Table 3. All monetary values are inflation adjusted to 2000 using the CPI-U. Robust standard errors clustered at the county level are reported in parentheses.

Table A2: Benefits Generosity: Impact of Increase in Monthly Payments, Alternative Specifications

	Dependent Variable:													
	Physician Copay ($)		Specialist Copay ($)		Drug Coverage (%)		Dental Coverage (%)		Vision Coverage (%)		Hearing Aid Coverage (%)		Actuarial Value ($)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Δb X 2001*	0.11	-0.08	0.15	0.52	-4.30	0.67	-1.94	3.72	6.28	3.21	16.24	17.91	-0.04	0.02
	(0.93)	(0.62)	(0.98)	(0.72)	(9.57)	(4.27)	(5.28)	(3.79)	(8.33)	(4.59)	(5.75)	(4.46)	(0.10)	(0.05)
Δb X 2002*	-3.20	-1.96	-3.80	-2.81	-0.74	0.61	2.97	6.79	-0.82	3.35	18.28	22.65	0.05	0.06
	(1.12)	(0.70)	(1.18)	(0.83)	(8.38)	(4.76)	(7.22)	(4.54)	(11.09)	(6.65)	(6.85)	(5.40)	(0.09)	(0.05)
Δb X 2003*	-1.53	-2.82	-2.31	-3.35	-3.69	4.95	-2.64	1.70	0.28	2.43	22.18	23.68	-0.01	0.11
	(1.27)	(0.68)	(1.47)	(0.98)	(7.66)	(4.46)	(8.21)	(3.71)	(11.36)	(6.65)	(7.63)	(5.21)	(0.08)	(0.05)
Main Effects														
County FE	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Additional Controls														
Pre-BIPA Payment X Year FE	X		X		X		X		X		X		X	
Urban X Year FE		X		X		X		X		X		X		X
Pre-BIPA Mean of Dep. Var.	7.29	7.29	11.13	11.13	73.62	73.62	25.77	25.77	75.68	75.68	42.58	42.58	n/a	n/a
R-Squared	0.68	0.68	0.70	0.70	0.83	0.83	0.68	0.67	0.75	0.74	0.83	0.83	0.82	0.82

Note: Table shows scaled coefficients on distance-to-floor × year interactions from difference-in-differences regressions. Although the estimation includes distance-to-floor interactions for all the years in our sample, we display coefficients for the post-reform years (2001-2003) above for brevity. The first-stage results displayed in Table 3 indicate that a $1 change in distance-to-floor translates into a $1 change in the monthly payments, so we can interpret the coefficients as the effect of an increase in monthly payments on a dollar-for-dollar basis. In columns 1 to 12, the dependent variables are measures of benefit generosity, and the coefficient on distance-to-floor is scaled by $50. In columns 13 and 14, the dependent variable is the monthly actuarial value of benefits, and the coefficient on distance-to-floor is not rescaled. See text for details on the construction of the monthly actuarial value of benefits. The unit of observation is the county × year, and observations are weighted by the number of beneficiaries in the county. The sample is the unbalanced panel of county-years with at least one MA plan over years 2000 to 2003. This sample includes 2,250 of 12,572 possible county-years and 62% of all Medicare beneficiaries. Year 2000, which is the year prior to BIPA implementation, is the omitted category. Controls are identical to those in Table 3. All monetary values are inflation adjusted to 2000 using the CPI-U. Robust standard errors clustered at the county level are reported in parentheses.

*Impact of $50 increase in columns 1 to 12. Impact of $1 increase in columns 13 and 14.

Table A3: Selection: Impact of $50 Increase in Monthly Payments, Alternative Specifications

	Dependent Variable:								
	MA Enrollment (%)			TM Costs ($)			MA Risk Adjustment ($)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Yearly BIPA Effect									
Δb X 2001	0.86	1.74	0.86	-3.05	1.57	-3.36	-1.36	-0.58	-1.50
	(0.60)	(1.07)	(0.61)	(1.67)	(2.00)	(1.73)	(0.48)	(0.67)	(0.51)
Δb X 2002	3.32	2.88	3.61	-0.88	3.79	-1.11	-2.42	-2.88	-2.53
	(0.83)	(1.27)	(0.84)	(3.41)	(3.95)	(3.52)	(0.59)	(0.93)	(0.61)
Δb X 2003	4.74	3.72	5.12	3.54	4.71	3.56	-3.43	-4.60	-3.58
	(0.90)	(1.41)	(0.91)	(3.73)	(3.46)	(3.85)	(0.81)	(1.33)	(0.84)
Panel B: Pooled Post-BIPA Effect									
Δb X Post-BIPA	3.29	3.47	3.48	-0.05	4.11	-0.14	-2.83	-2.87	-2.98
	(0.71)	(1.22)	(0.72)	(2.80)	(2.60)	(2.91)	(0.59)	(0.91)	(0.62)
Controls: All Panels									
Main Effects									
County FE	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X
Additional Controls									
Pre-BIPA Payment X Year FE		X			X			X	
Urban X Year FE			X			X			X
Pre-BIPA Mean of Dep. Var.	30.19	30.19	30.19	483.32	483.32	483.32	484.25	484.25	484.25

Note: Table shows coefficients on distance-to-floor × year interactions from difference-in-differences regressions. Although the estimation includes distance-to-floor interactions for all the years in our sample, we display coefficients for the post-reform years (2001-2003) above for brevity. The first-stage results displayed in Table 3 indicate that a $1 change in distance-to-floor translates into a dollar-for-dollar change in the monthly payments, so we can interpret the coefficients as the effect of an increase in monthly payments on a dollar-for-dollar basis. Coefficients are scaled to reflect the impact of a $50 increase in monthly payments. The unit of observation is the county × year, and observations are weighted by the number of beneficiaries in the county. The sample is the unbalanced panel of county-years with at least one MA plan over years 1999 to 2003. This sample includes 2,892 of 15,715 possible county-years and 63% of all Medicare beneficiaries. Year 2000, which is the year prior to BIPA implementation, is the omitted category. Controls are identical to those in Table A13. All monetary values are inflation adjusted to 2000 using the CPI-U. Robust standard errors clustered at the county level are reported in parentheses.

Table A4: Selection: Impact of $50 Increase in Monthly Payments, by Pre-Reform Insurer Market Power

	MA Enrollment (%)	TM Costs ($)	MA Risk Adjustment ($)	Selection: Slope of AC Curve ($)	Selection: Slope of AC Curve Net of Risk Adjustment ($)
	(1)	(2)	(3)	(4)	(5)
Panel A: Baseline (Full Sample)					
Δb X 2003	4.74	3.54	-3.43	75	147
	(0.90)	(3.73)	(0.81)	(81.1)	(81.6)
Panel B: By HHI Tercile					
Highest Tercile (Most Concentrated)					
Δb X 2003	2.89	-7.42	-4.24	-257	-110
	(0.87)	(3.93)	(1.01)	(306.3)	(275.5)
Middle Tercile					
Δb X 2003	5.80	16.50	-4.41	284	361
	(1.40)	(10.15)	(1.42)	(214.3)	(218.1)
Lowest Tercile					
Δb X 2003	4.82	4.29	-1.94	89	129
	(1.68)	(5.13)	(1.31)	(128.6)	(134.2)
Panel C: By Number of Insurers					
One Insurer					
Δb X 2003	2.89	-7.42	-4.24	-257	-110
	(0.87)	(3.93)	(1.01)	(306.3)	(275.5)
Two Insurers					
Δb X 2003	3.54	12.13	-2.55	342	414
	(1.42)	(6.77)	(1.34)	(1640.0)	(1890.0)
Three or More Insurers					
Δb X 2003	7.20	0.90	-2.36	13	45
	(2.05)	(6.69)	(1.68)	(109.5)	(108.2)

Note: Columns 1 through 3 show coefficients on distance-to-floor $\times$ year interactions from difference-in-differences regressions, scaled to reflect the impact of a $50 increase in monthly payments. Although the estimation includes distance-to-floor interactions for all the years in our sample, we display coefficients for 2003 above for brevity. The first-stage results displayed in Table 3 indicate that a $1 change in distance-to-floor translates into a dollar-for-dollar change in the monthly payments, so we can interpret the coefficients as the effect of an increase in monthly payments on a dollar-for-dollar basis. The unit of observation is the county $\times$ year, and observations are weighted by the number of beneficiaries in the county. This sample includes 2,892 of 15,715 possible county-years and 63% of all Medicare beneficiaries. Year 2000, which is the year prior to BIPA implementation, is the omitted category. Controls are identical to those in Table A13. All monetary values are inflation adjusted to 2000 using the CPI-U. Robust standard errors clustered at the county level are reported in parentheses. Columns 4 reports the implied slope of the average cost curve before considering risk adjustment: $\frac{dAC/db}{dq/db}$. Column 5 reports the slope of the average cost curve incorporating risk adjustment: $\frac{dAC^{MA}}{dq} - b\,\frac{dAR^{MA}}{dq}$. Standard errors for the last 2 columns are calculated by the bootstrap method using 200 iterations.

Table A5: Pass-Through and Market Concentration, 2001 to 2003

	Dependent Variable: Mean Premium							
	Subsample, by 2000 HHI Tercile			Subsample, by 2000 Insurer Count			Full Sample	
	Q3	Q2	Q1	1	2	3 +		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δb X 2001	-0.148	-0.341	-0.375	-0.148	-0.359	-0.424	-0.104	-0.103
	(0.100)	(0.111)	(0.081)	(0.100)	(0.087)	(0.103)	(0.142)	(0.144)
Δb X 2002	-0.152	-0.484	-0.723	-0.152	-0.513	-0.850	0.106	0.155
	(0.106)	(0.128)	(0.082)	(0.106)	(0.099)	(0.109)	(0.150)	(0.158)
Δb X 2003	-0.132	-0.400	-0.626	-0.132	-0.448	-0.735	0.120	0.113
	(0.138)	(0.141)	(0.101)	(0.138)	(0.122)	(0.128)	(0.191)	(0.201)
Δb X 2001 X HHI Tercile							-0.095	
							(0.062)	
Δb X 2002 X HHI Tercile							-0.281	
							(0.065)	
Δb X 2003 X HHI Tercile							-0.254	
							(0.082)	
Δb X 2001 X Contract Count								-0.110
								(0.069)
Δb X 2002 X Contract Count								-0.332
								(0.075)
Δb X 2003 X Contract Count								-0.280
								(0.093)
Main Effects								
County FE	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X
Pre-BIPA Mean of Dep. Var.	19.53	11.60	10.47	19.53	11.56	10.20	12.58	12.58
R-Squared	0.70	0.71	0.72	0.70	0.70	0.75	0.72	0.72

Note: Table shows coefficients on distance-to-floor $\times$ year interactions from difference-in-differences regressions. The dependent variable throughout the table is mean premiums. In columns 1 through 6, each column represents the main specification applied to a different subsample defined by pre-BIPA market concentration. In columns 7 and 8, the full sample is used and HHI terciles and contract counts are interacted with the distance-to-floor variables as continuous measures. Although the estimation includes distance-to-floor interactions for all the years in our sample, we display coefficients for the post-reform years (2001-2003) above for brevity. The first-stage results displayed in Table 3 indicate that a \$1 change in distance-to-floor translates into a \$1 change in the monthly payments, so we can interpret the coefficients as the effect of an increase in monthly payments on a dollar-for-dollar basis. The unit of observation is the county $\times$ year, and observations are weighted by the number of beneficiaries in the county. While the analysis is conducted on segments of the data, the underlying sample is the unbalanced panel of county-years with at least one MA plan over years 1997 to 2003. This sample includes 4,262 of 22,001 possible county-years and 64% of all Medicare beneficiaries. Year 2000, which is the year prior to BIPA implementation, is the omitted category. Controls are identical to those in Table A13. All monetary values are inflation adjusted to 2000 using the CPI-U. Robust standard errors clustered at the county level are reported in parentheses.

Table A6: Premium Pass-Through: Plan-Level Analysis of Impact of $1 Increase in Monthly Payments

	Dependent Variable: Monthly Premium ($)					
	Linear Regression			Tobit Regression		
	(1)	(2)	(3)	(4)	(5)	(6)
Δb X 2001	-0.296	-0.292	-0.307	-0.417	-0.373	-0.445
	(0.054)	(0.089)	(0.055)	(0.010)	(0.015)	(0.010)
Δb X 2002	-0.505	-0.538	-0.517	-0.644	-0.661	-0.664
	(0.059)	(0.105)	(0.059)	(0.008)	(0.011)	(0.008)
Δb X 2003	-0.446	-0.450	-0.449	-0.575	-0.480	-0.585
	(0.070)	(0.118)	(0.071)	(0.009)	(0.009)	(0.009)
Main Effects						
County FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
Additional Controls						
Pre-BIPA Payment X Year FE		X			X	
Urban X Year FE			X			X
Pre-BIPA Mean of Dep. Var.	12.58	12.58	12.58	12.58	12.58	12.58
R-Squared	0.60	0.60	0.60	N/A	N/A	N/A

Note: Table shows coefficients on distance-to-floor $\times$ year interactions from difference-in-differences regressions. Although the estimation includes distance-to-floor interactions for all the years in our sample, we display coefficients for the post-reform years (2001-2003) above for brevity. The first-stage results displayed in Table 3 indicate that a $1 change in distance-to-floor translates into a $1 change in the monthly payments, so we can interpret the coefficients as the effect of an increase in monthly payments on a dollar-for-dollar basis. The unit of observation is the plan $\times$ year, and observations are weighted by the number of beneficiaries in the plan. The sample is the unbalanced panel of 7,386 MA plan-years over years 1997 to 2003. This sample includes 4,262 of 22,001 possible county-years and 64% of all Medicare beneficiaries. The final three columns display results from a Tobit regression, which explicitly takes into account the fact that plans could not give rebates (charge negative premiums) during our sample period. Year 2000, which is the year prior to BIPA implementation, is the omitted category. Controls are identical to those in Table 3. All monetary values are inflation adjusted to 2000 using the CPI-U. Robust standard errors clustered at the county level are reported in parentheses.

Table A7: Mean Premiums by Size of Payment Increase and Year

	Mean Premium ($)		
Year	No Change in Payment	$1-$50 Payment Increase	$\geq$ $51 Payment Increase
1997	6.44	10.18	25.81
1998	8.91	10.84	29.96
1999	1.54	6.04	23.79
2000	10.82	23.34	40.12
2001	27.74	35.14	42.76
2002	39.79	36.57	45.82
2003	41.59	41.39	50.52

Note: Table shows mean premiums by year in three subsets of counties: counties with no BIPA-induced payment change, counties with a payment increase of $1-$50 , and counties with a payment increase of greater than or equal to $51. While the summary statistics is displayed by subsets of the data, the underlying sample is the unbalanced panel of 7,386 MA plan-years over years 1997 to 2003. This sample includes 4,262 of 22,001 possible county-years and 64% of all Medicare beneficiaries.

Table A8: Premium Pass-Through: Impact of $1 Increase in Monthly Payments, Including FFS Costs Controls

	Dependent Variable: Mean Monthly Premium ($)					
	(1)	(2)	(3)	(4)	(5)	(6)
Δb X 2001	-0.305	-0.197	-0.316	-0.305	-0.196	-0.316
	(0.053)	(0.092)	(0.054)	(0.053)	(0.092)	(0.054)
Δb X 2002	-0.495	-0.371	-0.507	-0.495	-0.370	-0.506
	(0.058)	(0.119)	(0.058)	(0.058)	(0.119)	(0.058)
Δb X 2003	-0.436	-0.325	-0.438	-0.436	-0.325	-0.438
	(0.069)	(0.122)	(0.070)	(0.069)	(0.122)	(0.070)
Per capita FFS costs	-0.035	-0.043	-0.036			
	(0.037)	(0.036)	(0.037)			
Per capita FFS costs excluding IME and DSH				-0.031	-0.040	-0.032
				(0.038)	(0.038)	(0.038)
Main Effects						
County FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
Additional Controls						
Pre-BIPA Payment X Year FE		X			X	
Urban X Year FE			X			X
Pre-BIPA Mean of Dep. Var.	12.58	12.58	12.58	12.58	12.58	12.58
R-Squared	0.73	0.73	0.73	0.73	0.73	0.73

Note: Table shows coefficients on distance-to-floor $\times$ year interactions from difference-in-differences regressions. Although the estimation includes distance-to-floor interactions for all the years in our sample, we display coefficients for the post-reform years (2001-2003) above for brevity. The first-stage results displayed in Table 3 indicate that a $1 change in distance-to-floor translates into a $1 change in the monthly payments, so we can interpret the coefficients as the effect of an increase in monthly payments on a dollar-for-dollar basis. The unit of observation is the county $\times$ year, and observations are weighted by the number of beneficiaries in the county. The county-level measures are constructed using plan-level data weighted by plan enrollment. The sample is the unbalanced panel of county-years with at least one MA plan over years 1997 to 2003. This sample includes 4,262 of 22,001 possible county-years and 64% of all Medicare beneficiaries. Year 2000, which is the year prior to BIPA implementation, is the omitted category. Controls are identical to those in Table 3, in addition to per capita FFS costs. All monetary values are inflation adjusted to 2000 using the CPI-U. Robust standard errors clustered at the county level are reported in parentheses.

Table A9: Within-Insurer Variation in Plan Characteristics in Year 2000

	AETNA	CIGNA	Kaiser	Pacificare	United
Premiums ($)					
Mean	36.33	17.74	20.54	23.30	5.07
SD	31.49	19.14	30.38	24.49	11.32
Physician Copay ($)					
Mean	10.00	9.84	8.93	7.18	10.24
SD	0.00	0.90	3.02	2.26	6.16
Specialist Copay ($)					
Mean	16.10	16.61	11.30	7.76	12.07
SD	2.08	5.06	5.43	4.10	6.44
Drug Coverage (%)					
Mean	1.00	1.00	0.96	0.79	0.65
SD	0.00	0.00	0.04	0.17	0.23
Dental Coverage (%)					
Mean	0.02	0.13	0.35	0.18	0.01
SD	0.02	0.11	0.23	0.15	0.01
Vision Coverage (%)					
Mean	1.00	0.10	0.96	0.88	0.41
SD	0.00	0.09	0.04	0.10	0.24
Hearing Aid Coverage (%)					
Mean	0.70	0.16	0.09	0.37	0.11
SD	0.21	0.14	0.08	0.23	0.10

Note: Table shows the within-insurer variation in premiums and benefits for the largest five insurers in the MA market in year 2000.

Table A10: Actuarial Value of Benefits: Impact of $1 Increase in Monthly Payments, by Quintile of Out-of-Pocket Spending

	Dependent Variable: Actuarial Value ($), by Total OOP Expenditure				
	Bottom Quintile	Second Quintile	Third Quintile	Fourth Quintile	Top Quintile
	(1)	(2)	(3)	(4)	(5)
Δb X 2001*	0.002	0.008	0.010	0.022	0.063
	(0.007)	(0.034)	(0.046)	(0.066)	(0.098)
Δb X 2002*	0.014	0.037	0.044	0.066	0.132
	(0.007)	(0.036)	(0.049)	(0.070)	(0.105)
Δb X 2003*	0.020	0.065	0.077	0.106	0.181
	(0.007)	(0.033)	(0.045)	(0.065)	(0.096)
Main Effects					
County FE	X	X	X	X	X
Year FE	X	X	X	X	X
R-Squared	0.80	0.82	0.82	0.82	0.81

Note: Table shows coefficients on distance-to-floor $\times$ year interactions from difference-in-differences regressions. Although the estimation includes distance-to-floor interactions for all the years in our sample, we display coefficients for the post-reform years (2001-2003) above for brevity. The first-stage results displayed in Table 3 indicate that a $1 change in distance-to-floor translates into a $1 change in the monthly payments, so we can interpret the coefficients as the effect of an increase in monthly payments on a dollar-for-dollar basis. The dependent variable is the actuarial value of benefits for a given quintile of out-of-pocket spending, which is constructed based on observed plan benefits in our main analysis dataset and utilization and cost data from the 2000 Medical Expenditure Panel Survey. The unit of observation is the county $\times$ year, and observations are weighted by the number of beneficiaries in the county. The sample is the unbalanced panel of county-years with at least one MA plan over years 2000 to 2003. This sample includes 2,250 of 12,572 possible county-years and 62% of all Medicare beneficiaries. Year 2000, which is the year prior to BIPA implementation, is the omitted category. Controls are identical to those in Table 3. All monetary values are inflation adjusted to 2000 using the CPI-U. Robust standard errors clustered at the county level are reported in parentheses.

Table A11: Re-weighted Actuarial Value of Benefits: Impact of $1 Increase in Monthly Payments

OOP Spending Quintile	Actuarial Value of Benefits Expansions	Mean OOP Spending	Relative MU of Consumption	Reweighted Actuarial Value
	(1)	(2)	(3)	(4)
Bottom	0.020	$48	0.891	
Second	0.065	$282	0.915	
Third	0.077	$579	0.947	
Fourth	0.106	$1,135	1.011	
Top	0.181	$3,176	1.300	
Average	0.090	$1,044	1.000	0.098

Note: Table shows how the actuarial value of benefits changes when reweighted based on the marginal utility of consumption. Rows correspond to quintiles of the OOP spending distribution among the elderly in the 2000 MEPS. Column 1 reproduces the estimates from Table A10 for the year 2003. Column 2 lists the mean OOP spending in each quintile. Column 3 lists the marginal utility for each quintile, relative to marginal utility at the mean of OOP spending, given the assumptions on risk aversion and consumption described in Section A.6. Column 4 re-weights the overall actuarial value by applying the marginal utilities in column 3 to the actuarial values in column 1. See Section A.6 for additional details.

Table A12: Plan Quality: Impact of $50 Increase in Monthly Payments

	Dependent Variable:											
	Percentage beneficiaries report overall quality of care is 10 out of 10			Percentage beneficiaries report doctors always communicate well			Mean mammography rate			Unreported quality composite		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Δb X 2001	0.053	1.272	0.023	-0.113	0.647	-0.133	0.033	0.004	-0.005	0.219	0.149	0.225
	(0.481)	(0.817)	(0.492)	(0.339)	(0.615)	(0.344)	(0.532)	(0.793)	(0.560)	(0.090)	(0.138)	(0.091)
Δb X 2002	0.752	1.209	0.745	0.470	1.306	0.447	-0.707	-0.009	-0.651	0.042	0.054	0.034
	(0.519)	(0.893)	(0.525)	(0.445)	(0.730)	(0.450)	(0.609)	(1.058)	(0.614)	(0.072)	(0.119)	(0.073)
Δb X 2003	0.887	1.365	0.879	0.482	1.316	0.461	0.148	0.813	0.245	0.083	0.169	0.073
	(0.506)	(0.886)	(0.512)	(0.448)	(0.757)	(0.453)	(0.618)	(1.136)	(0.628)	(0.071)	(0.125)	(0.071)
Main Effects												
County FE	X	X	X	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X	X	X	X
Additional Controls												
Pre-BIPA Payment X Year FE		X			X			X			X	
Urban X Year FE			X			X			X			X
Pre-BIPA Mean of Dep. Var.	50.25	50.25	50.25	69.20	69.20	69.20	72.90	72.90	72.90	-0.34	-0.34	-0.34
R-Squared	0.92	0.92	0.92	0.90	0.90	0.90	0.69	0.69	0.69	0.84	0.85	0.84

Note: Table shows scaled coefficients on distance-to-floor $\times$ year interactions from difference-in-differences regressions. Although the estimation includes distance-to-floor interactions for all the years in our sample, we display coefficients for the post-reform years (2001-2003) above for brevity. The first-stage results displayed in Table 3 indicate that a $1 change in distance-to-floor translates into a $1 change in the monthly payments, so we can interpret the coefficients as the effect of an increase in monthly payments on a dollar-for-dollar basis. Coefficients are scaled to reflect the impact of a $50 increase in monthly payments. In columns 1 to 12, the dependent variables are measures of mean plan quality, and the coefficient on distance-to-floor is scaled by $50. See text for details on the construction of the unreported quality composite. The unit of observation is the county $\times$ year, and observations are weighted by the number of beneficiaries in the county. In columns 1 to 6 and 10 to 12, the sample is the unbalanced panel of county-years with at least one MA plan over years 1999 to 2003. This sample includes 2,892 of 15,715 possible county-years and 63% of all Medicare beneficiaries. In columns 7 to 9, the sample is the unbalanced panel of county-years with at least one MA plan over years 1997 to 2003. This sample includes 4,262 of 22,001 possible county-years and 64% of all Medicare beneficiaries. Year 2000, which is the year prior to BIPA implementation, is the omitted category. Controls are identical to those in Table 3. All monetary values are inflation adjusted to 2000 using the CPI-U. Robust standard errors clustered at the county level ($N = 662$) are reported in parentheses.

Table A13: Base Payments: Impact of $1 Increase in Distance-to-the-Floor, Balanced Sample of Counties

	Dependent Variable: Base Payment ($)		
	(1)	(2)	(3)
Δb X 2001	0.996	0.999	0.997
	(0.001)	(0.001)	(0.001)
Δb X 2002	0.995	1.000	0.992
	(0.003)	(0.003)	(0.003)
Δb X 2003	0.999	1.000	0.997
	(0.003)	(0.002)	(0.003)
Main Effects			
County FE	X	X	X
Year FE	X	X	X
Additional Controls			
Pre-BIPA Payment X Year FE		X	
Urban X Year FE			X
Pre-BIPA Mean of Dep. Var.	526.56	526.56	526.56
R-Squared	0.9999	0.9999	0.9999

Note: Table shows coefficients on distance-to-floor $\times$ year interactions from difference-in-differences regressions with the monthly base payments as the dependent variable. Although the estimation includes distance-to-floor interactions for all the years in our sample, we display coefficients for the post-reform years (2001-2003) above for brevity. The unit of observation is the county $\times$ year, and observations are weighted by the number of beneficiaries in the county. The sample is the balanced panel of county-years with at least one MA plan in each year between 1997 and 2003. This sample includes 2,548 out of 22,001 possible county-years and 54% of all Medicare beneficiaries. Year 2000, which is the year prior to BIPA implementation, is the omitted category. Year 2000, which is the year prior to BIPA implementation, is the omitted category. Additional controls in column 2 include quartiles of year 2000 county base payments interacted with year indicators and in column 3 include an indicator for urban status interacted with year indicators. Flexible controls for the 1998 payment floor introduction and 2000 blended payment increase are included in all specifications. These controls are identical to those in Table 3. All monetary values are inflation adjusted to 2000 using the CPI-U. Robust standard errors clustered at the county level ($N = 343$) are reported in parentheses.

Table A14: Premium Pass-Through: Impact of $1 Increase in Monthly Payments, Balanced Sample of Counties

	Dependent Variable:											
	Mean Monthly Premium ($)			Median Monthly Premium ($)			Minimum Monthly Premium ($)			Maximum Monthly Premium ($)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Δb X 2001	-0.392	-0.314	-0.412	-0.383	-0.256	-0.395	-0.332	-0.302	-0.342	-0.488	-0.374	-0.524
	(0.054)	(0.084)	(0.055)	(0.062)	(0.105)	(0.063)	(0.057)	(0.082)	(0.059)	(0.095)	(0.141)	(0.098)
Δb X 2002	-0.580	-0.402	-0.607	-0.647	-0.400	-0.672	-0.485	-0.353	-0.508	-0.523	-0.428	-0.550
	(0.066)	(0.109)	(0.066)	(0.087)	(0.126)	(0.089)	(0.076)	(0.119)	(0.077)	(0.081)	(0.138)	(0.082)
Δb X 2003	-0.485	-0.363	-0.497	-0.558	-0.333	-0.572	-0.418	-0.353	-0.428	-0.405	-0.405	-0.411
	(0.076)	(0.118)	(0.077)	(0.097)	(0.134)	(0.099)	(0.090)	(0.135)	(0.092)	(0.087)	(0.125)	(0.089)
Main Effects												
County FE	X	X	X	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X	X	X	X
Additional Controls												
Pre-BIPA Payment X Year FE		X			X			X			X	
Urban X Year FE			X			X			X			X
Pre-BIPA Mean of Dep. Var.	11.15	11.15	11.15	10.65	10.65	10.65	4.54	4.54	4.54	19.50	19.50	19.50
R-Squared	0.72	0.73	0.73	0.67	0.67	0.67	0.66	0.66	0.66	0.69	0.69	0.69

Note: Table shows coefficients on distance-to-floor $\times$ year interactions from difference-in-differences regressions. Although the estimation includes distance-to-floor interactions for all the years in our sample, we display coefficients for the post-reform years (2001-2003) above for brevity. The first-stage results displayed in Table A13 indicate that a $1 change in distance-to-floor translates into a $1 change in the monthly payments, so we can interpret the coefficients as the effect of an increase in monthly payments on a dollar-for-dollar basis. The unit of observation is the county $\times$ year, and observations are weighted by the number of beneficiaries in the county. The sample is the balanced panel of county-years with at least one MA plan in each year between 1997 and 2003. This sample includes 2,548 out of 22,001 possible county-years and 54% of all Medicare beneficiaries. Year 2000, which is the year prior to BIPA implementation, is the omitted category. Controls are identical to those in Table A13. All monetary values are inflation adjusted to 2000 using the CPI-U. Robust standard errors clustered at the county level ($N = 343$) are reported in parentheses.

Table A15: Benefits Generosity: Impact of Increase in Monthly Payments, Balanced Sample of Counties

	Dependent Variable:						
	Physician Copay ($)	Specialist Copay ($)	Drug Coverage (%)	Dental Coverage (%)	Vision Coverage (%)	Hearing Aid Coverage (%)	Actuarial Value ($)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Δb X 2001*	-1.112	-0.318	5.382	5.471	-0.744	18.757	0.084
	(0.434)	(0.611)	(4.267)	(3.896)	(4.509)	(4.784)	(0.044)
Δb X 2002*	-2.764	-2.881	2.867	5.841	1.118	23.822	0.091
	(0.648)	(0.823)	(4.844)	(4.489)	(6.839)	(5.551)	(0.051)
Δb X 2003*	-3.552	-3.395	6.822	-0.081	-0.100	24.724	0.127
	(0.594)	(0.980)	(4.561)	(3.680)	(6.850)	(5.377)	(0.046)
Main Effects							
County FE	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X
Pre-BIPA Mean of Dep. Var.	7.15	10.98	74.71	27.58	77.81	46.65	n/a
R-Squared	0.69	0.71	0.82	0.66	0.74	0.83	0.81

Note: Table shows the scaled coefficients on distance-to-floor $\times$ year interactions from difference-in-differences regressions. Although the estimation includes distance-to-floor interactions for all the years in our sample, we display coefficients for the post-reform years (2001-2003) above for brevity. The first-stage results displayed in Table A13 indicate that a $1 change in distance-to-floor translates into a $1 change in the monthly payments, so we can interpret the coefficients as the effect of an increase in monthly payments on a dollar-for-dollar basis. In columns 1 to 6, the dependent variables are measures of benefit generosity, and the coefficient on distance-to-floor is scaled by $50. In column 7, the dependent variable is the monthly actuarial value of benefits, and the coefficient on distance-to-floor is not rescaled. See text for details on the construction of the monthly actuarial value of benefits. The unit of observation is the county $\times$ year, and observations are weighted by the number of beneficiaries in the county. The sample is the balanced sample of county-years with at least one MA plan in each year between 2000 and 2003. This sample includes 1,772 out of 12,572 possible county-years and 57% of all Medicare beneficiaries. Year 2000, which is the year prior to BIPA implementation, is the omitted category. Controls are identical to those in Table A13. All monetary values are inflation adjusted to 2000 using the CPI-U. Robust standard errors clustered at the county level ($N = 343$) are reported in parentheses.

*Impact of $50 increase in columns 1 to 6. Effect of $1 increase in column 7.

Table A16: Plan Availability: Impact of \$50 Increase in Monthly Payments, Balanced Sample of Counties

	Dependent Variable:								
	At Least One Plan (%)			Number of Plans			HHI		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Δb X 2001	-2.044	-3.352	-2.325	0.019	-0.217	0.038	0.022	0.082	0.021
	(1.794)	(2.457)	(1.777)	(0.102)	(0.192)	(0.105)	(0.028)	(0.050)	(0.029)
Δb X 2002	-0.621	-6.569	-0.240	0.063	-0.186	0.078	-0.023	0.062	-0.029
	(2.022)	(3.130)	(2.041)	(0.135)	(0.199)	(0.140)	(0.034)	(0.049)	(0.035)
Δb X 2003	3.013	-2.601	3.388	0.122	-0.143	0.145	-0.053	0.031	-0.062
	(2.209)	(3.538)	(2.226)	(0.136)	(0.205)	(0.140)	(0.037)	(0.053)	(0.038)
Main Effects									
County FE	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X
Additional Controls									
Pre-BIPA Payment X Year FE		X			X			X	
Urban X Year FE			X			X			X
Pre-BIPA Mean of Dep. Var.	67.49	67.49	67.49	2.60	2.60	2.60	0.51	0.51	0.51
R-Squared	0.86	0.86	0.86	0.66	0.66	0.66	0.68	0.68	0.68

Note: Table shows scaled coefficients on distance-to-floor $\times$ year interactions from difference-in-differences regressions. Although the estimation includes distance-to-floor interactions for all the years in our sample, we display coefficients for the post-reform years (2001-2003) above for brevity. The first-stage results displayed in Table A13 indicate that a \$1 change in distance-to-floor translates into a \$1 change in the monthly payments, so we can interpret the coefficients as the effect of an increase in monthly payments on a dollar-for-dollar basis. Coefficients are scaled to reflect the impact of a \$50 increase in monthly payments. The dependent variables are indicator for at least one plan (columns 1 to 3), number of plans conditional on at least one plan (columns 4 to 6), and Herfindahl-Hirschman Index (HHI) with a scale of 0 to 1 (columns 7 to 9). The sample in columns 1 to 3 is the balanced panel of county-years with non-missing information on base rates and Medicare beneficiaries during 1997 to 2003. This sample includes 21,504 of 22,001 counties and more than 99.9% of all Medicare beneficiaries. The sample in columns 4 to 9 is the balanced panel of county-years with at least one MA plan in each year between 1997 and 2003. This sample includes 2,548 out of 22,001 possible county-years and 54% of all Medicare beneficiaries. Year 2000, which is the year prior to BIPA implementation, is the omitted category. Controls are identical to those in Table A13. All monetary values are inflation adjusted to 2000 using the CPI-U. Robust standard errors clustered at the county level are reported in parentheses.

Table A17: Selection: Impact of $50 Increase in Monthly Payments, Balanced Sample of Counties

	Dependent Variable:				Implied Pass-Through with Selection (ρ)
	MA Enrollment (%)	TM Costs ($)	MA Risk Adjustment ($)	Mean Premiums* ($)	
	(1)	(2)	(3)	(4)	(5)
Panel A: Yearly BIPA Effect					
Δb X 2001	0.63	-4.02	-1.04	-0.38	1.13
	(0.69)	(1.84)	(0.46)	(0.05)	(0.22)
Δb X 2002	3.64	-0.45	-2.02	-0.52	0.90
	(0.92)	(3.73)	(0.62)	(0.06)	(0.15)
Δb X 2003	5.39	4.29	-3.57	-0.44	0.70
	(0.99)	(4.06)	(0.79)	(0.07)	(0.12)
Panel B: Pooled Post-BIPA Effect					
Δb X Post-BIPA	3.49	-0.20	-2.82	-0.48	0.86
	(0.78)	(3.08)	(0.57)	(0.06)	(0.11)
Controls: All Panels					
Main Effects					
County FE	X	X	X	X	
Year FE	X	X	X	X	
Pre-BIPA Mean of Dep. Var.	33.39	493.53	495.16	10.52	

Note: Columns 1 through 4 of this table show coefficients on distance-to-floor × year interactions from difference-in-differences regressions. Although the estimation includes distance-to-floor interactions for all the years in our sample, we display coefficients for the post-reform years (2001-2003) above for brevity. The first-stage results displayed in Table A13 indicate that a $1 change in distance-to-floor translates into a $1 change in the monthly payments, so we can interpret the coefficients as the effect of an increase in monthly payments on a dollar-for-dollar basis. Coefficients are scaled to reflect the impact of a $50 increase in monthly payments. The unit of observation is the county × year, and observations are weighted by the number of beneficiaries in the county. The sample is the balanced panel of county-years with at least one MA plan in each year between 1999 and 2003. This sample includes 2,055 out of 15,715 possible county-years and 56% of all Medicare beneficiaries. Year 2000, which is the year prior to BIPA implementation, is the omitted category. Controls are identical to those in Table A13. All monetary values are inflation adjusted to 2000 using the CPI-U. Robust standard errors clustered at the county level are reported in parentheses. Column 5 reports the implied pass-through in a perfectly competitive market based on the estimates in the corresponding row (see Section 6 for more details). Standard errors for this implied pass-through estimate are calculated by the bootstrap method using 200 iterations.

*Impact of $1 increase in monthly payments shown in column 4.

Table A18: Utilization: Impact of $50 Increase in Monthly Payments

	Dependent Variable:								
	Part A Stays			Part A Days			Part B Line-Item Claims		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Δb X 2001	0.0002	0.0001	0.0002	0.001	-0.001	0.001	0.003	0.001	0.003
	(0.0001)	(0.0002)	(0.0001)	(0.001)	(0.002)	(0.001)	(0.006)	(0.007)	(0.007)
Δb X 2002	0.0005	0.0002	0.0005	0.002	0.0010	0.002	0.018	0.007	0.018
	(0.0002)	(0.0003)	(0.0002)	(0.002)	(0.002)	(0.002)	(0.012)	(0.012)	(0.013)
Δb X 2003	0.0006	0.0003	0.0006	0.003	0.002	0.003	0.022	0.011	0.022
	(0.0002)	(0.0003)	(0.0002)	(0.002)	(0.003)	(0.002)	(0.014)	(0.013)	(0.015)
Main Effects									
County FE	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X
Additional Controls									
Pre-BIPA Payment X Year FE		X			X			X	
Urban X Year FE			X			X			X
Pre-BIPA Mean of Dep. Var.	0.032	0.032	0.032	0.22	0.22	0.22	2.19	2.19	2.19
R-Squared	0.98	0.98	0.98	0.97	0.97	0.97	0.99	0.99	0.99

Note: Table shows coefficients on the coefficients on distance-to-floor $\times$ year interactions from difference-in-difference regressions. Although the estimation includes distance-to-floor interactions for all the years in our sample, we display coefficients for the post-reform years (2001-2003) above for brevity. The first-stage results displayed in Table 3 indicate that a $1 change in distance-to-floor translates into a dollar-for-dollar change in the monthly payments, so we can interpret the coefficients as the effect of an increase in monthly payments on a dollar-for-dollar basis. Coefficients are scaled to reflect the impact of a $50 increase in monthly payments. The unit of observation is the county $\times$ year, and observations are weighted by the number of beneficiaries in the county. The sample is the unbalanced panel of county-years with at least one MA plan over years 1999 to 2003. This sample includes 2,892 of 15,715 possible county-years and 63% of all Medicare beneficiaries. Year 2000, which is the year prior to BIPA implementation, is the omitted category. Controls are identical to those in Table A13. All monetary values are inflation adjusted to 2000 using the CPI-U. Robust standard errors clustered at the county level are reported in parentheses.