NBER WORKING PAPER SERIES

DOES PRIVATIZED HEALTH INSURANCE BENEFIT PATIENTS OR PRODUCERS? EVIDENCE FROM MEDICARE ADVANTAGE

Marika Cabral
Michael Geruso
Neale Mahoney

Working Paper 20470
http://www.nber.org/papers/w20470

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
September 2014

We thank Mark Duggan, Matthew Notowidigdo, and seminar participants at the UC Santa Barbara Health Economics Conference, the Chicago Junior Faculty Group, the 2013 and 2014 RWJ Scholars in Health Policy Research Annual Meetings, and the UT Austin Faculty Seminar for helpful comments. We are grateful to Abhi Gupta for excellent research assistance. Mahoney acknowledges the Stigler Center at the University of Chicago for financial support. First version: July 2013. This version: September 2014. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

Does Privatized Health Insurance Benefit Patients or Producers? Evidence from Medicare Advantage
Marika Cabral, Michael Geruso, and Neale Mahoney
NBER Working Paper No. 20470
September 2014
JEL No. D4,H22,I11,I13,L1

ABSTRACT

The debate over privatizing Medicare stems from a fundamental disagreement about whether privatization would primarily generate consumer surplus for individuals or producer surplus for insurance companies and health care providers. This paper investigates this question by studying an existing form of privatized Medicare called Medicare Advantage (MA). Using difference-in-differences variation brought about by payment floors established by the 2000 Benefits Improvement and Protection Act, we find that for each dollar in increased capitation payments, MA insurers reduced premiums to individuals by 45 cents and increased the actuarial value of benefits by 8 cents. Using administrative data on the near-universe of Medicare beneficiaries, we show that advantageous selection into MA cannot explain this incomplete pass-through. Instead, our evidence suggests that insurer market power is an important determinant of the division of surplus, with premium pass-through rates of 13% in the least competitive markets and 74% in the markets with the most competition.

Marika Cabral
Department of Economics
University of Texas at Austin
One University Station
BRB 1.116, C3100
Austin, TX 78712
and NBER
marika.cabral@austin.utexas.edu

Michael Geruso
University of Texas at Austin
Department of Economics
1 University Station C3100
Austin, TX 78712
and NBER
mike.geruso@austin.utexas.edu

Neale Mahoney
Booth School of Business
University of Chicago
5807 South Woodlawn Avenue
Chicago, IL 60637
and NBER
Neale.Mahoney@chicagobooth.edu

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 the privatization of Medicare. Proponents of privatization argue that it would reduce costs by encouraging competition among private insurers and would raise consumer surplus by allowing individuals to select coverage that better matches their preferences. Opponents of privatizing Medicare argue that such a move would lead to large profits for producers and the eventual erosion of insurance benefits. At its core, the debate is about economic incidence: Does privatized Medicare primarily generate consumer surplus for individuals or producer surplus for insurance companies and health care providers?

This paper investigates this question by studying an existing form of privatized Medicare called Medicare Advantage.[2] 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 traditionally 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. Recent proposals to privatize Medicare through a system of "premium supports" resemble an expansion of the current system of capitation payments. Like the current system, these privatization proposals typically include requirements for a minimum level of basic benefits and a traditional fee-for-service coverage option.[3]

We examine the incidence of privatized Medicare on consumer and producer surplus by studying a sharp change in capitation payments to MA insurers brought about by the 2000 Benefits Improve-

[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.

[3]For recent examples, see the 2012 Burr-Coburn plan or the 2014 Ryan proposal.

ment and Protection Act (BIPA). MA capitation payments are determined at the county level based on historical Traditional Medicare expenditures in the county. BIPA reformed this payment system 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 below these payment floors 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 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, we find that consumer premiums were reduced by 45 cents and that the actuarial value of plan benefits was increased by 8 cents in the 3 years following the reform. A 95% confidence interval allows us to rule out a combined pass-through rate outside of 35% to 71%. Difference-in-differences plots that flexibly allow the effect of the 2001 payment shocks to vary by year show no impacts on premiums in pre-reform years, providing evidence in support of the parallel trends identifying assumption.

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 privatized Medicare. Drawing on prior work by Weyl and Fabinger (2013) and Mahoney and Weyl (2014), 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 that are differentially high cost on the margin. If firms have market power, then they may not face pressure to pass through increased payments into lower premiums or more generous benefits.

We estimate the degree of advantageous selection into MA by estimating the slope of the Traditional Medicare cost curve using administrative spending data on the near-universe of Traditional Medicare beneficiaries and the same difference-in-differences empirical strategy.[4] Our estimates indicate there is limited advantageous selection into MA on the margin. 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 47 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. Premium pass-through rates approach 75% in the most competitive markets compared to approximately 10% in those 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).[5]

Our research is most closely related to a paper on pass-through in MA by Duggan, Starc and Vabson (2014) conducted in parallel to our study. Using a cross-sectional research design that compares capitation payments MA insurers receive in urban and rural counties, Duggan, Starc and Vabson (2014) estimate a premium pass-through rate of zero. In contrast, our difference-in-differences strategy yields premium pass-through estimates of 45% on average, with rates approaching 75% in the most competitive counties.[6] Our interpretation of this evidence is that private markets can efficiently provide Medicare benefits but that not all markets may be competitive enough to achieve this objective.[7]

Our paper also contributes to the literature on selection in Medicare, with Brown et al. (2011)

[4]We use comprehensive administrative data on costs of Traditional Medicare beneficiaries and demographics of all Medicare beneficiaries to investigate selection controlling for the demographic risk adjustment used during our sample time period. Details on this analysis can be found in Section 6.

[5]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 and thereby contaminate the estimates.

[6]Our work is also related to Curto et al. (2014) who study competition in MA, Town and Liu (2003) who estimate consumer and producer surplus generated by MA using a logit discrete choice framework assuming no selection, Dunn (2010) who uses a discrete choice framework to estimate the impact of plan generosity on consumer surplus, and Cawley, Chernew and McLaughlin (2005) who investigate the impacts of MA payment changes in 1997 on MA plan availability. 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.

[7]Our paper is more broadly related to research on market power in employer-sponsored health insurance (Dafny, 2010; Dafny, Duggan and Ramanarayanan, 2012) and Medicare Part D (Ho, Hogan and Morton, 2014) and research on the pass-through of Medicare payments (Clemens and Gottlieb, 2014).

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.

We view our results more generally 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. Although evaluating the merits of specific policy proposals are 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, and 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. In-

[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.

surers 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.[9]

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. The 1997 Balanced Budget Act (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 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).[10]

BIPA implemented two floors on county base payments in March 2001 that varied with whether the county was rural or urban and were scheduled to update over time. 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

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

where $\widetilde{c}_{jt}$ is the base payment absent the BIPA floors and $\underline{b}_{jt}$ is the relevant BIPA payment floor. Since BIPA was in place for most of 2001, we assign post-BIPA base payments to this year.[11]

[9]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.

[10]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.

[11]Although base payments changed mid-year in March 2001, plan offerings, benefits packages, and premiums were set only once, in late 2000.

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.[12] Extensive risk adjustment of MA capitation payments was introduced in 2004 (see Brown et al., 2011; McWilliams, Hsu and Newhouse, 2012), after our study period.

The Centers for Medicare and Medicaid Services (CMS) constructs the demographic risk adjustment factors to average to 1.0 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.[13] 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.[14]

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

[12] Between 2000 and 2003, 90% of the payment adjustment was based on sex and age, while 10% was based on inpatient diagnoses, if any. This mixture explained approximately 1.5% of the variation in medical spending (Brown et al., 2011), and its purpose 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.

[13] 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.

[14] 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).

each county in each year; the annual census of MA insurer contracts offered by county; county-level MA enrollment summaries; and plan premium data for every contract.[15] For 2000 to 2003, we are able to obtain information on the benefits (e.g., copayments, drug coverage) offered by each plan.[16] 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.[17]

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 $\times$ years that have at least one MA plan. In Section 4, we show our source of identifying variation does not have a meaningful effect on entry or exit of counties from the sample. Nevertheless, Appendix A.5 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.[18]

Panel A shows that base payments average $491 per month for all counties but range from $223 to $778 per month across the sample. More than 65% of Medicare beneficiaries live in a county with at least one plan. MA plans enroll 19% Medicare beneficiaries on average, although counties 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 roughly 52% of plans charge no premium to beneficiaries. Copayments for physician and specialists visits

[15] 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.

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

[17] 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.

[18] 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.

average \$8 and \$14, respectively. Approximately 70% of plans offer drug and vision coverage, 27% of plans offer dental coverage, and 40% cover hearing products. Beneficiaries in the restricted sample can choose among 2.8 plans on average, and enrollment is higher with an MA penetration rate of 29%. Average TM costs, at \$521 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

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 figure shows that BIPA led to a sharp increase in payments, 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, affected urban counties with lower base payments received relatively larger payment increases than affected urban counties with higher base payment levels prior to BIPA.

Figure 2 presents maps of base payments by county for the years before and after the implementation of the BIPA payment floors. Darker shading indicates higher payment levels, and the same shading scheme is used before and after the reform. Panel A shows the pre-BIPA geographic heterogeneity in payments, with low base payment counties spread over most of the map. Panel B shows the extent to which payment floors, which were binding for 72% of counties, truncated payments above the median of the pre-BIPA base payment distribution, providing us with a large and geographically diverse source of identifying variation.

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 8.4% 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 for counties that received payment increases due to the BIPA payment floors to counties that were unaffected by the reform. 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:

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

where $\widetilde{c}_{jt}$ is the monthly payment in the absence of the floor and $\underline{\widetilde{b}}_{jt}$ is the relevant urban or rural payment floor.

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 2001 pre-BIPA payments that we observe by 2% each year:[19]

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

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.[20]

[19] For payments, 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.

[20] 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.

We define counterfactual floors, $\widetilde{\underline{b}}_{jt}$, in the pre-BIPA period by deflating the 2001 floor by 2% per year:

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

where $\underline{b}_{jt}$ 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_j, to flexibly vary by year. Letting y_{jt} be an outcome in county j in year t, our baseline regression specification takes the form

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

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_t's are the coefficients of interest, and 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.

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 have two broad 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.

Our second approach is to estimate specifications that isolate the two key subsets of our identifying variation. To isolate variation due to urban or rural status, we include as a control the base payment in year 2000 interacted with a linear time trend. This approach controls for differential time trends across counties with different base payments, such as differential medical cost growth. With this approach, the estimates are largely identified by differences in the payment increases across urban and rural counties with the same pre-BIPA base payments. To isolate the complementary variation, we estimate a separate specification that includes urban status of the county interacted with a linear time trend as a control. This complementary approach controls for differential time trends across urban and rural counties, and the estimates are largely identified by differences in the size of the payment increase within the sets of urban and rural counties.

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 effect of our constructed change in payments variable on actual monthly payment rates, plotting 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 baseline specification with county and year fixed effects. Column 2 adds controls for the base payment level in the year 2000 interacted with a linear time trend to isolate variation due to the difference between the urban and rural floor, and column 3 includes as controls an urban indicator interacted with a linear time trend 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 0.98 to 1.02 for each post-BIPA year and with standard errors no larger than 0.004. 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 county monthly base payments.

4 Main Results

In this section, we examine the effects of the increase in payments on premiums and plan characteristics. We start by presenting the results on premiums. We then examine the effects on plan benefits, such as copayments and drug coverage, along with 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 measures of county-level premiums as the dependent variable. Table 4 presents parameter estimates from the corresponding baseline regressions, which include year and county fixed effects. In addition, Table 4 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.

Panel A of Figure 4 shows the effect on mean county-level premiums. The dashed horizontal line at zero 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. Following BIPA, premiums decline by approximately 50 cents for each dollar in higher payments. The point estimates, shown in columns 1 to 3 of Table 4, indicate the effects are stable across specifications, with the 2003 estimate ranging from 45 to 51 cents.

Panel B of Figure 4 shows the effect on the minimum county-level premium, which may be particularly relevant for the marginal Medicare Advantage (MA) enrollee. The effect on minimum premiums is similar to the effect on the mean, with the plot showing no evidence of a pre-BIPA effect, and a sharp decline following implementation of the payment floors. The point estimates, shown in columns 4 to 6 of Table 4, indicate that by 2003 the minimum premium fell by 42 to 48 cents for every dollar in increased payments and are robust to using different subsets of the identifying variation.

Panel C of Figure 4 shows the effect on the percentage of plans within a county with a premium of zero. Again, consistent with our identifying assumption, there is no evidence of a trend in the pre-BIPA period. Following BIPA, the plot indicates that a dollar increase in payments raised the share of plans with a zero premium by approximately 0.5 percentage points. The estimates are stable over time, statistically significant, and economically meaningful in magnitude. The estimates imply that a $50 increase in payments, which is approximately 10% of the $476 mean pre-BIPA base payment, raises the share of plans with a zero premium by 25 percentage points on a base of 65.1%. This effect is similarly robust to controls that isolate the different subsets of the identifying variation, which are shown in columns 7 to 9 of Table 4.

4.2 Pass-Through into Benefits

In addition to lowering premiums, plans may have responded to the increased base payments by raising the generosity of their coverage. MA insurers can differentiate their plans from Traditional Medicare (TM) by offering lower copayments and providing supplemental benefits, such as hearing, vision, dental, and drugs, which were not covered by TM during our study period. This channel is particularly relevant for plans setting their premium at zero since they could not further decrease premiums.

We examine the effect of BIPA on mean county-level copayments for physician and specialist visits and the percentage of plans providing coverage for prescription drugs, dental, vision, and hearing aids. These are the main benefits that were listed in Medicare's plan comparison website and are likely to be the most salient to consumers. While we cannot examine effects on other dimensions of plan quality (e.g, network breadth, quality of plan administration), most models of competition suggest that plans would be unlikely to raise the generosity of plan characteristics that consumers less readily observe.[21]

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 $476 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 our standard falsification tests for pre-existing trends. 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

[21] Further, characteristics, such as the quality of plan administration, are difficult to change rapidly. Thus, even if we had data on this outcome, we would be unlikely to observe effects during our sample period.

specification, with Appendix Table A1 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 physician copayments by \$1.98 on a pre-BIPA base of \$7.28 and reduced specialist copayments by \$3.01 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 on average by less than \$2 per month.[22]

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. 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 23.7 percentage points on a base of 44.4%. Appendix Table A1 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 each category of supplemental benefits (dental, vision, hearing aids, and drugs), we estimate category-specific coinsurance rates among those MEPS respondents that report supplemental coverage.[23] We then multiply these category-specific rates by the unconditional total annual spending in each category, generating actuarial values of coverage for each supplemental benefit. For copayments, we simply multiply the copayment amount by the average annual number of physician visits. Finally, we sum across all categories and divide the measure by 12, since the utilization and expenditure tallies in the MEPS are annual and our payment floor variation is in monthly payments. This procedure delivers a monetized measure of plan generosity that can be used to estimate changes in the actuarial value of the benefits.

[22] The number of provider visits is based on authors' calculations using the 2000 Medical Expenditure Panel Survey (MEPS).

[23] In practice, we estimate category-specific coinsurance rates by calculating the total spending and the insurer-covered portion among respondents with non-zero insurer claims for the specific category of supplemental coverage.

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 estimates for 2003, shown in column 7 of the table, indicate a pass-through rate of 8 cents on the dollar and is statistically insignificant with a p-value of 0.07. Specifications that isolate alternative subsets of the identifying variation, shown in columns 13 and 14 of Appendix Table A1, confirm the robustness of this finding. Taken together, the premiums and benefits results for 2003 yield a combined pass-through estimate of 53 cents on the dollar. A 95% confidence interval allows us to rule out a combined pass-through effect outside the range of 35 cents to 71 cents.[24]

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. Table 6 shows the corresponding regression estimates, including alternative specifications that isolate different subsets of the identifying variation. Due to a change in reporting on MA contracts between 1999 and 2000, we limit the sample to 2000 to 2003. As with the benefits analysis, the sample period is sufficient to identify the effect of BIPA but does not allow us to perform our standard falsification tests for pre-existing trends.

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 these specifications, we use the entire panel of 3,143 counties. The plot shows no evidence of an effect on the percentage of counties with at least one plan, with the exception of 2003 where there is a marginally significant uptick. The parameter estimates, shown in columns 1 to 3 of Table 6, are similar across alternative specifications.

Overall, this evidence suggests that BIPA had little effect on whether a county had at least one plan. 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.

[24] 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.

The pattern of the coefficients in Figure 7 indicates that the marginally significant increase in counties with an MA plan in year 2003 is unlikely to be a source of bias in our main estimates. The main premium and benefit effects emerge by 2002, before there is any evidence of a change in the number of counties with MA. 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.5, are very similar and confirm that selection is not biasing the results.

BIPA may have also influenced competitiveness within sample counties that had at least one plan. Panel B of Figure 7 shows the effect of a $50 increase in payments on a Herfindahl-Hirschman Index (HHI) for the number of plans in each county. The HHI is the standard measure of market power used for antitrust analysis. It is similar to our other dependent variables in weighting plans based on their enrollment shares. The plot shows no evidence of an effect of the increased payments on county-level HHI. The corresponding regression estimates in columns 4 to 6 of Table 6 show a stable non-effect across alternative specifications. This result, combined with the extensive margin finding above, indicate that BIPA did not seem to have a meaningful impact on market concentration.

5 Model of Pass-Through

In the previous section, we showed that Medicare Advantage (MA) plans pass through 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 forces. 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 these forces and pass-through. 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 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 (2014) who examine the interaction of imperfect competition and selection.

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 (2014). We direct the reader to these papers for technical details and microfoundations 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.[25] 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.[26]

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:

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

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

[25] 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.

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

(net of capitation payment) cost.

To allow for intermediate levels of competition, Mahoney and Weyl (2014) introduce a parameter $\theta \in [0, 1]$ that interpolates between the price-setting equations for perfect competition and monopoly:

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

The model nests the extremes of perfect competition ($\theta = 0$) and monopoloy ($\theta = 1$) 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 (2014) show that this equation is a reduced-form representation of differentiated product Bertrand competition if we make assumptions on the primitives such 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. In this case, differentiated product Bertrand is given when $\theta \equiv 1 - D$, where $D \equiv -\frac{\sum_{j \neq i} \partial Q_i / \partial p_j}{\partial Q_i / \partial p_i}$ is the aggregate division ratio, the share of consumers that firm i diverts from rivals j when it lowers its price.

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 $\rho = 0$.

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, 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.2 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. 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 $\rho = \frac{1}{1+\theta}$.[27]

[27] More specifically, pass-through is decreasing in market power when demand is log-concave since $(\log q)'' = \mu'/\mu^2 < 0 \iff \mu' < 0$. When $\mu'(p) > 0$, 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.

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 of one if there were no selection and no risk adjustment. As shown in Section 5, pass-through in a perfectly competitive MA market is given by

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

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.

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) in using 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 cost 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:[28]

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

This result, combined with the fact that a change in premiums has an equal and opposite effect on 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:[29]

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

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 base payments on premiums from Section 4. The effect of a change in premiums on average MA costs is therefore $\frac{dAC^{MA}}{dp} =$

[28] A proof is provided in Appendix A.3. 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.

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

$\phi \frac{dAC^{TM}}{dp} = \phi \frac{dAC^{TM}/db}{dp/db}$.

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.[30]

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 \$476 mean base payment 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 A7 shows alternative specifications that isolate different subsets of the identifying variation.

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 30.5% and is highly significant with a p-value < 0.01.

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 $1.5 = \frac{4.7\%}{30.5\%} \Big/ \frac{\$50}{\$476}$. If we assume that base payments affect enrollment only through premiums — so that base payments are a valid instrument for premiums — then the 2003

[30]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.

estimate implies a semi-elasticity of demand with respect to premiums of $-0.0068 = \frac{(4.7\%/30.5\%)}{0.45\times\$50}$, 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 -0.0129 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.

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 $3.76, shown in column 2 of Table 7, divided by the 4.7% enrollment effect implies a $80 slope of the average cost curve. Since average costs are $484 per month, this indicates that individuals with the highest willingness-to-pay for MA only cost about 17% 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 -$91 to $250.[31] Appendix Section A.6 demonstrates that the selection estimates are qualitatively similar in specifications with alternative controls and specifications with alternative measures of utilization.

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 -$3.24, shown in column 3 of Table 7, by the enrollment effect indicates a slope of risk adjustment payments with respect to quantity of -$69. Combining this estimate with our 2003 cost estimate yields a slope for the average cost curve net of risk adjustment ($\frac{dAC^{MA}}{dq} - b\,\frac{dAR^{MA}}{dq}$) of $149.[32] We cannot reject that there is no net selection on the margin as the 95% confidence interval on this estimate runs from -$9 to $307.[33]

To calculate the explanatory power of selection, we combine these estimates with Equation 11,

[31]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.

[32]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.

[33]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.

where the numerator of Equation 11, the average risk adjustment factor among MA beneficiaries AR^{MA}, is equal to 0.955 in our sample.[34] We calculate standard errors of the implied pass-through by bootstrapping over counties.[35] 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-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 0.66 or higher than 1.03. The yearly estimates similarly vary from 73% to 108%.

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 47 cents in payments that is not pass-through to consumers, our estimates indicate that selection can account for 15 cents or about one-third of the shortfall.

7 Market Power

In this section, we examine the extent to which insurer market power can explain our estimates of incomplete pass-through. In Section 5, we discussed how a monopolist facing a linear demand curve passed 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 less than one and declining in market power.

We investigate the quantitive 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. 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 and thereby contaminate

[34] 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.

[35] 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.

the estimates. Interpreting the estimates as the casual effect of market power on pass-through would require assuming that other factors that affect pass-through — the curvature of the demand and the degree of selection — are not correlated with these pre-exiting measures of 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 Herfindahl-Hirschman Index (HHI), with the highest HHI tercile corresponding to the most concentrated markets and the lowest HHI tercile corresponding to the markets with the least market power. 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 HHI. The pass-through rate is 13% in the most concentrated HHI tercile and 62% in the tercile with the lowest market power. Panel B shows the pass-through rate is similarly increasing in the number of pre-BIPA insurers in each county. When there is a single insurer, pass-through is 13%. In counties with three or more firms, pass-through increases to 74%.

Appendix Figure A8 shows the effects for each year in the post-BIPA period. The 2002 estimates are almost identical and show that pass-through is monotonically increasing in both measures of competition. Consistent with the main results in Figure 8, pass-through rates are lower in 2001 and the relationship between pass-through rate market power is less precise. The parameter estimates underlying these figures are shown in Appendix Table A9. 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.

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). The identifying variation in our study — variation in county-level base payments — resembles payment reductions scheduled to take effect under the Affordable Care Act. More broadly, the expansion of MA under BIPA can be seen as a step toward the systematic reforms proposed by some policymakers to more fully privatize the delivery of health care to seniors.

Our analysis shows that 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. In the context of the perpetual policy debate around privatizing Medicare, our findings indicate that efforts to make insurance markets more competitive may be key to providing the most value to beneficiaries.

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.

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.

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*.

Curto, Vilsa, Liran Einav, Jonathan Levin, and Jay Bhattacharya. 2014. "Can Managed Competition Work? The US Medicare Advantage Program." *Mimeo*.

Dafny, Leemore, Mark Duggan, and Subramaniam Ramanarayanan. 2012. "Paying a Premium on Your Premium? Consolidation in the US Health Insurance Industry." *The American Economic Review*, 102(2): 1161–1185.

Dafny, Leemore S. 2010. "Are Health Insurance Markets Competitive?" *The American Economic Review*, 100(4): 1399–1431.

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).

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

Ho, Kate, Joseph Hogan, and Fiona Scott Morton. 2014. "The Impact of Consumer Inattention on Insurer Pricing in the Medicare Part D Program." *Mimeo*.

Mahoney, Neale, and E. Glen Weyl. 2014. "Imperfect Competition in Selection Markets." *NBER Working Paper*.

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.

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.

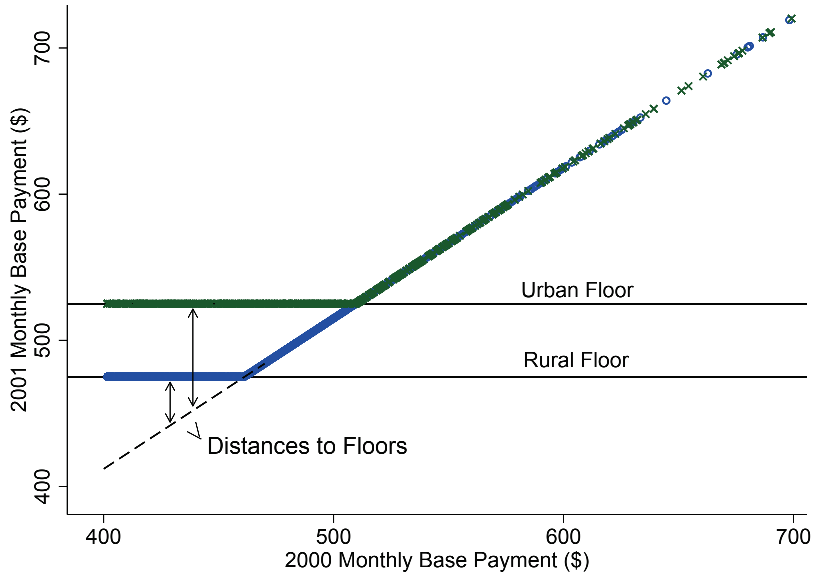
Stockley, Karen, Thomas McGuire, Christopher Afendulis, and Michael E. Chernew. 2014. "Premium Transparency in the Medicare Advantage Market: Implications for Premiums, Benefits, and Efficiency." *NBER Working Paper*.

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*.

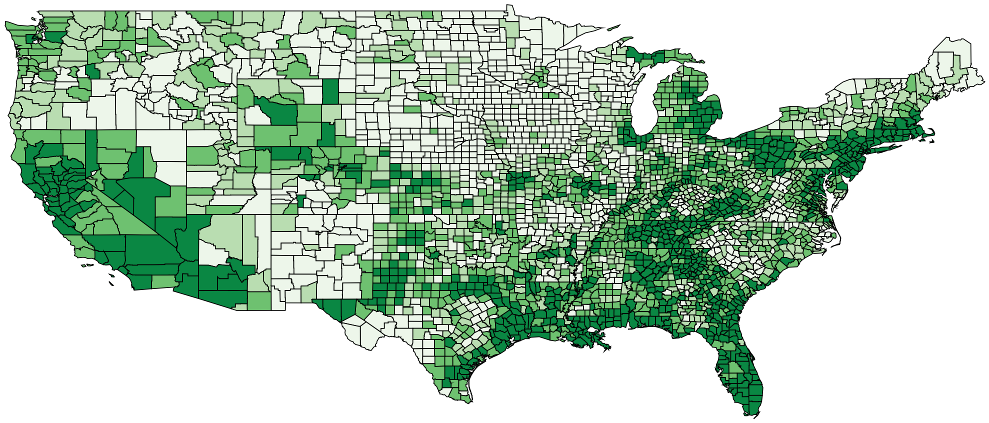
Figure 1: Payment Floors: Pre- and Post-BIPA Monthly Base Payments



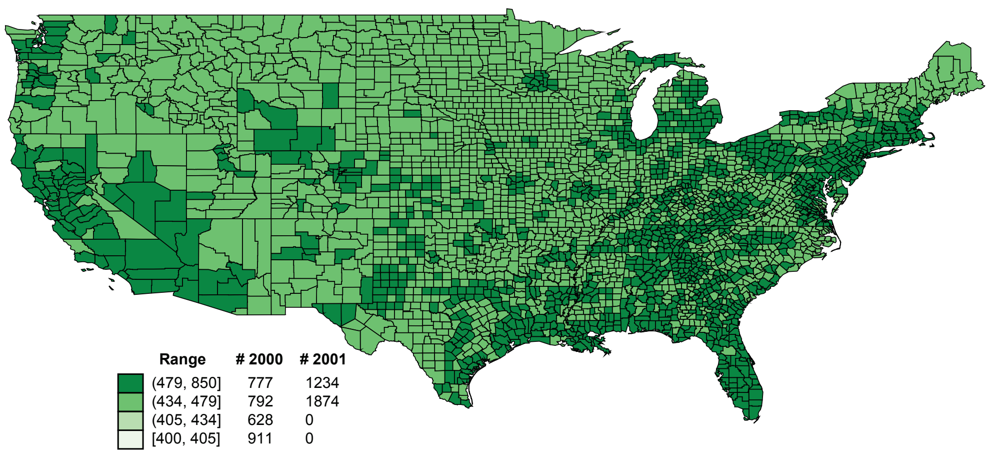
Note: Figure shows county base payments before (x-axis) and after (y-axis) the implementation of the BIPA urban and rural payment floors in 2001. Base payments in this figure are not adjusted for inflation and are not normalized for the sample average demographic risk adjustment factor. Urban counties are represented with a green "X" and rural counties with a blue "O". The dashed line 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. All values are denominated in dollars per beneficiary per month.

Figure 2: Effect of BIPA on County Base Payments

(A) Pre-BIPA, 2000

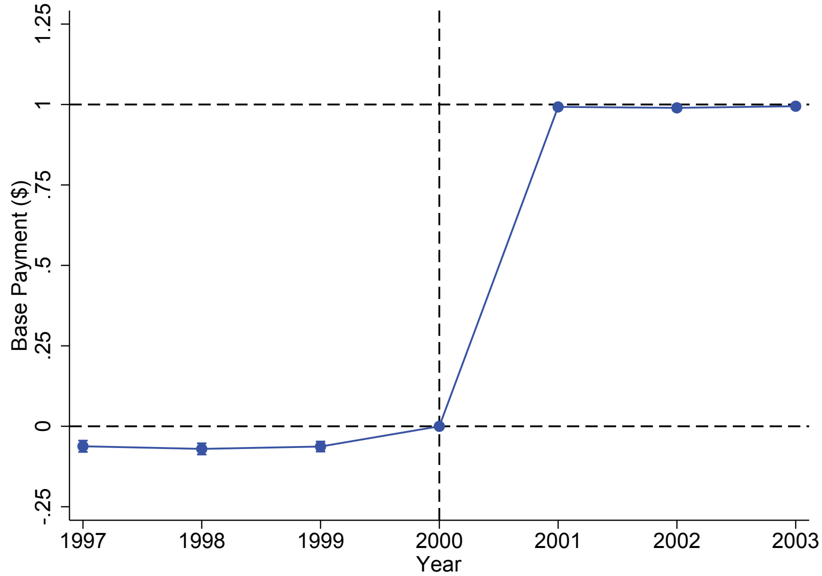


(B) Post-BIPA, 2001



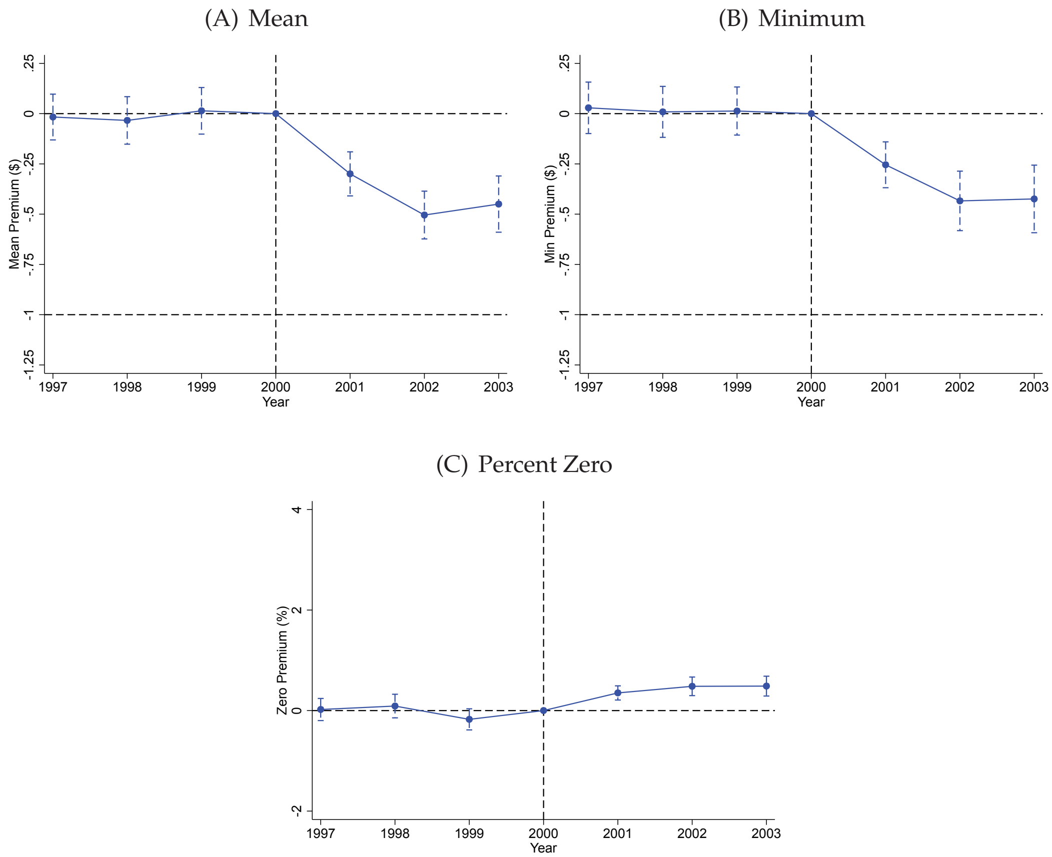
Note: Map shows base payments by county just before and just after the implementation of BIPA floors in early 2001. Base payments in this figure are not adjusted for inflation and are not normalized for the sample average demographic risk adjustment factor. Counties are binned according to their quartile of base payments in 2000. The legend lists bin ranges and the number of counties in each by year. Darker regions indicate larger payments. BIPA payment floors truncated payments above the median of the pre-BIPA distribution and were binding for 72% of counties.

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



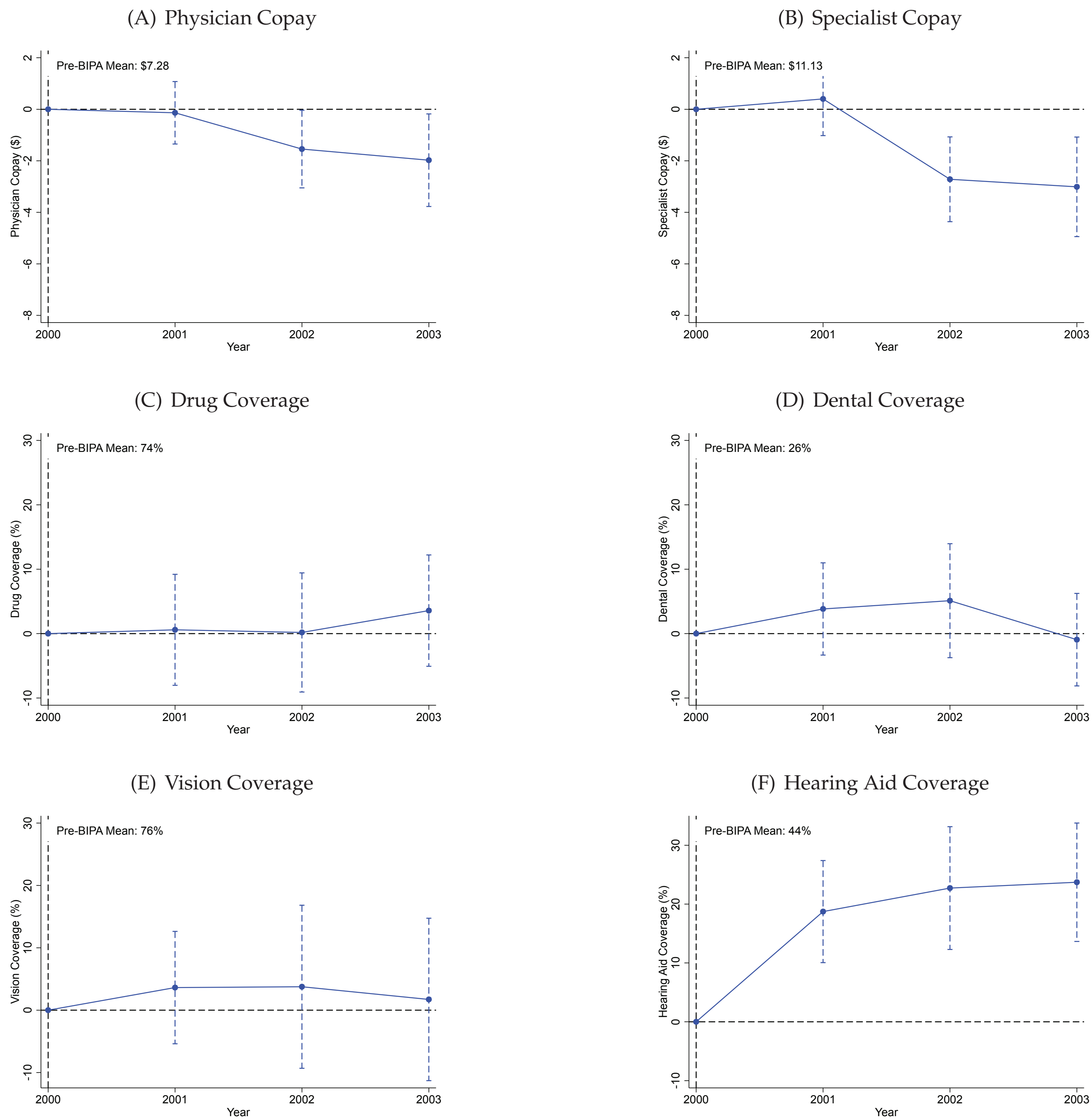
Note: Figure shows coefficients on distance-to-the-floor $\times$ year interactions from difference-in-differences regressions with the monthly base payments as the dependent variable. The unit of observation is the county $\times$ year, and observations are weighted by the number of beneficiaries in the county. 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



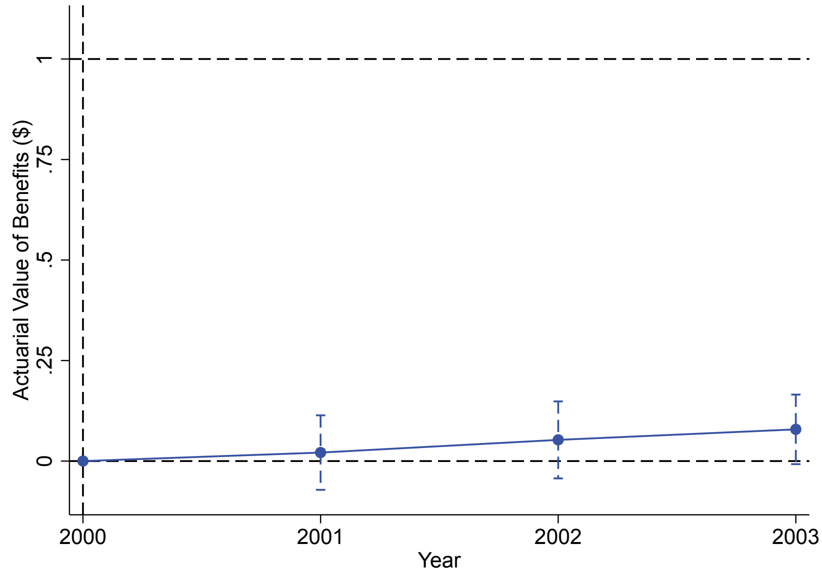
Note: Figure shows coefficients on distance-to-floor × year interactions from difference-in-differences regressions. The first-stage results displayed in Table 3 indicate that a $1 change in distance-to-floor translates into a $1 change in the monthly payments, so we can interpret the coefficients as the effect of an increase in monthly payments on a dollar-for-dollar basis. The dependent variables are 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. 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 in Panels A and B 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 × 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 × year, and observations are weighted by the number of beneficiaries in the county. 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 fringe 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.

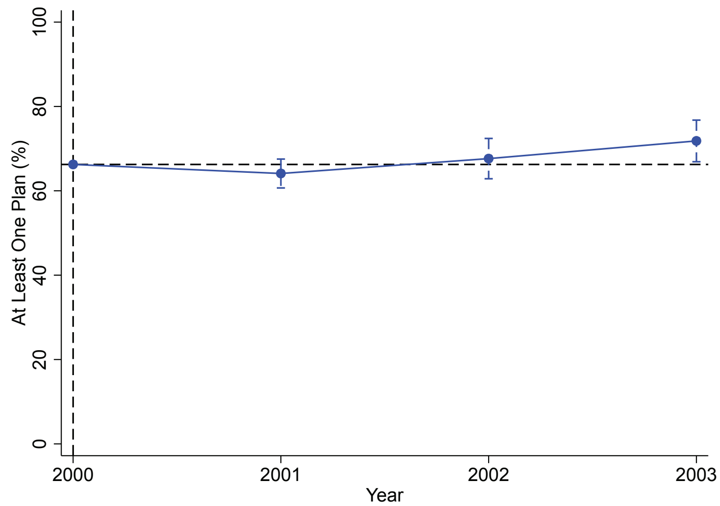
Figure 6: Actuarial Value of Benefits: Impact of $1 Increase in Monthly Payments



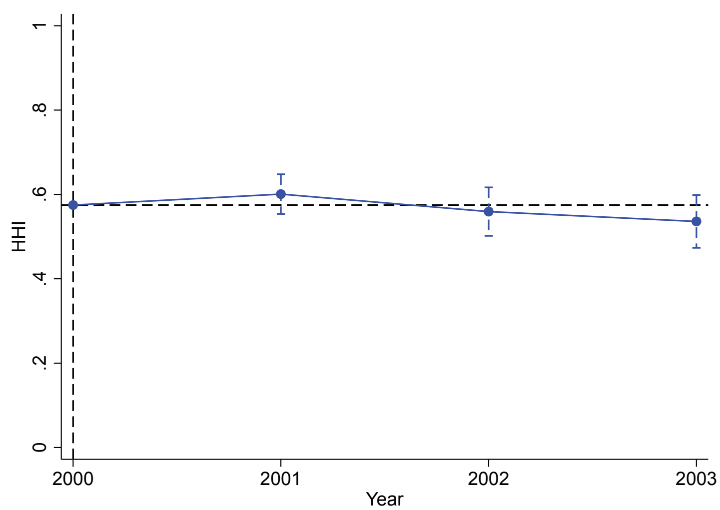
Note: Figure shows coefficients on distance-to-floor $\times$ year interactions from difference-in-differences regressions. The first-stage results displayed in Table 3 indicate that a $1 change in distance-to-floor translates into a $1 change in the monthly payments, so we can interpret the coefficients 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. 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

(A) At Least One Plan



(B) Insurer HHI*

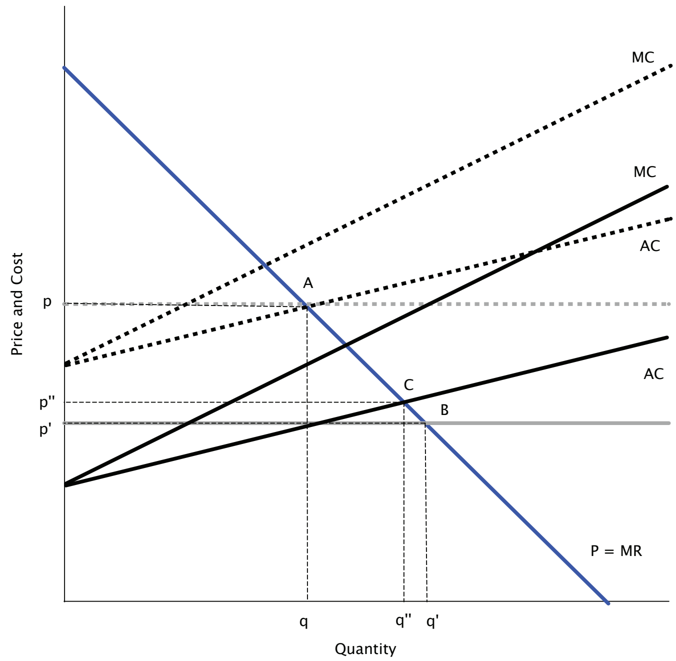


Note: Figure shows scaled coefficients on distance-to-floor × year interactions from difference-in-differences regressions. The first-stage results displayed in Table 3 indicate that a $1 change in distance-to-floor translates into a $1 change in the monthly payments, so we can interpret the coefficients 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 the presence of any plan (Panel A) and insurer HHI scaled from zero to one (Panel B). The unit of observation is the county × year, and observations are weighted by the number of beneficiaries in the county. 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.

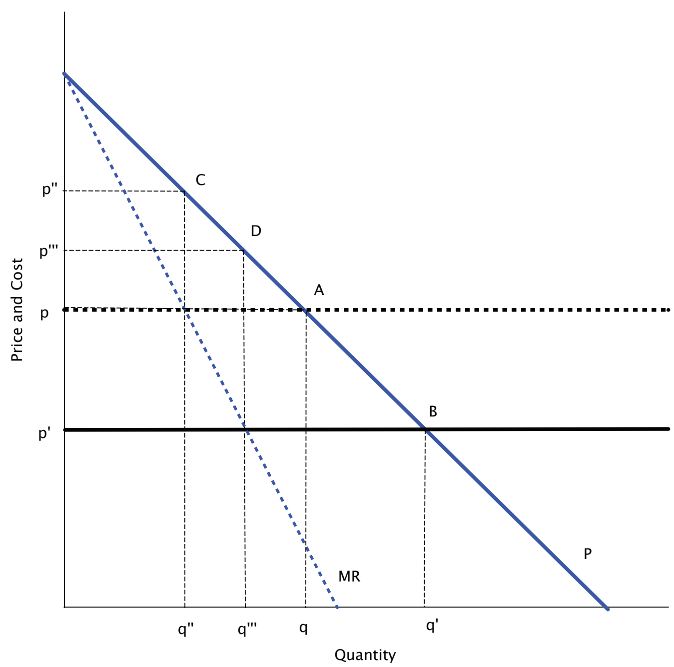
*Panel B restricts the sample to county × years with at least one plan.

Figure 8: Determinants of Incomplete Pass-Through

(A) Advantageous Selection



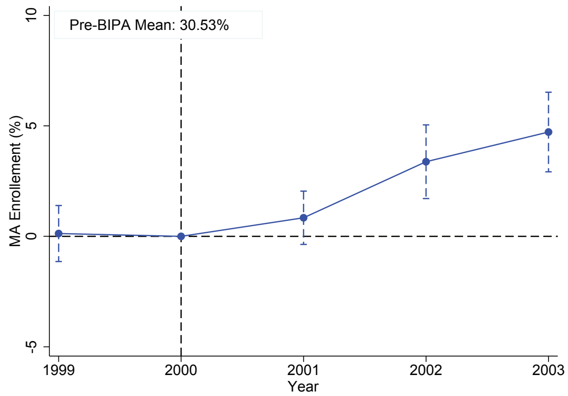
(B) Market Power



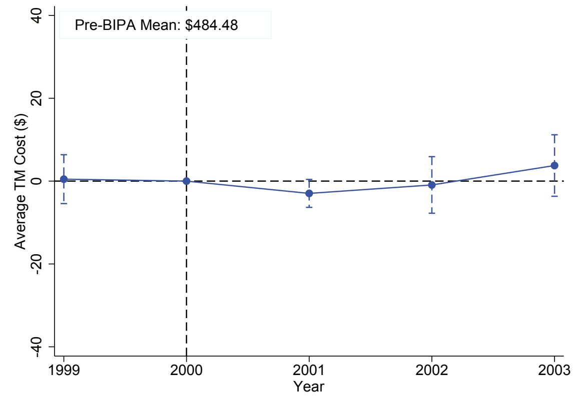
Note: Figure shows the pass-though of an increase in monthly payments depicted by a decrease in (net) marginal costs. Panel (A) examines pass-through when there are perfectly competitive markets and either no selection or advantageous selection. With no selection (horizontal AC curve), a downward shift 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

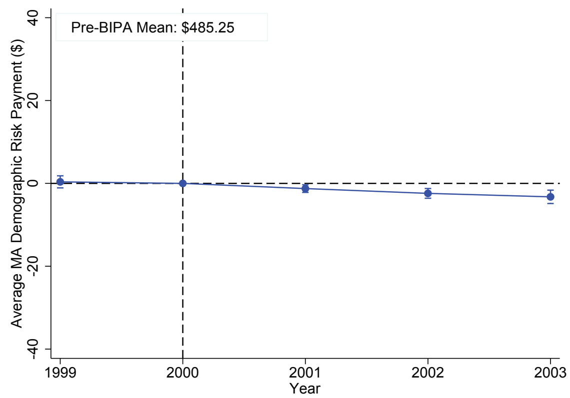
(A) MA Enrollment



(B) TM Costs



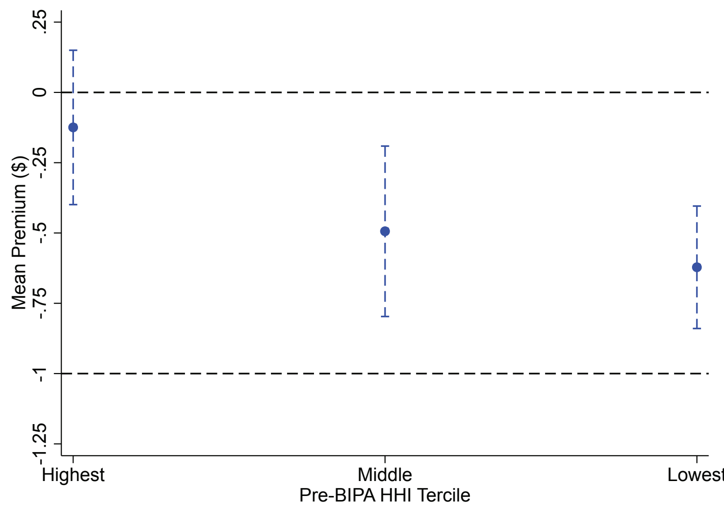
(C) MA Risk Adjustment



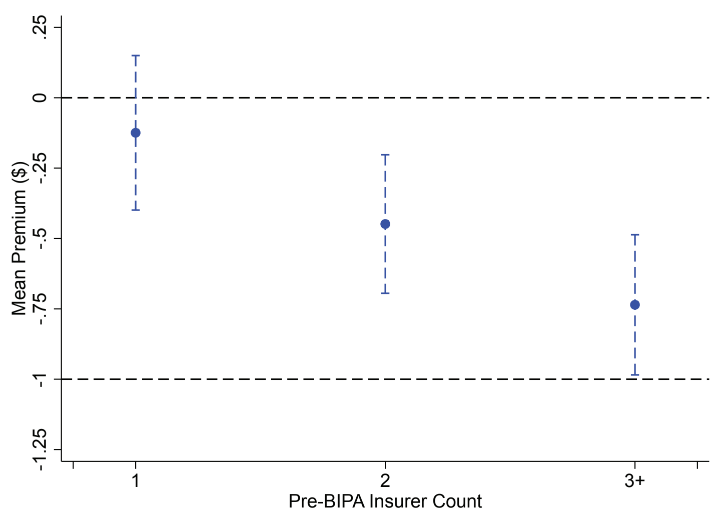
Note: Figure shows scaled coefficients on distance-to-floor $\times$ year interactions from difference-in-differences regressions.The first-stage results displayed in Table 3 indicate that a $1 change in distance-to-floor translates into a $1 change in the monthly payments, so we can interpret the coefficients 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 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 $\times$ year, and observations are weighted by the number of beneficiaries in the county. 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. 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	65.1%	47.7%	0%	100%
Number of Plans	1.78	1.73	0	7
MA Enrollment	19.1%	18.4%	0%	69.8%
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	22.71	27.82	0	156.29
Minimum	15.05	26.25	0	156.29
% With Zero Premium	52.1%	42.9%	0%	100%
County-Level Benefits*				
Physician Copay ($ per visit)	7.89	4.95	0	56.15
Specialist Copay ($ per visit)	14.39	6.79	0	95.72
Drug Coverage	70.5%	41.1%	0%	100%
Dental Coverage	27.4%	35.7%	0%	100%
Vision Coverage	69.9%	39.8%	0%	100%
Hearing Aid Coverage	40.0%	42.1%	0%	100%
Number of Plans	2.75	1.41	1	7
HHI	5,696	2,584	1,778	10,000
MA Enrollment	28.8%	16.1%	1.1%	67.6%
TM Costs ($ per month)	521.80	106.65	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 ($N = 22,001$). Panel B restricts the sample to county $\times$ years with at least one MA plan ($N = 3,961$). 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 percent of the 2000 base payment. 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	0.988	0.978	0.992
	(0.001)	(0.002)	(0.001)
Δb X 2002	1.015	1.001	1.009
	(0.003)	(0.003)	(0.003)
Δb X 2003	1.019	0.997	1.014
	(0.003)	(0.003)	(0.004)
Main Effects			
County FE	X	X	X
Year FE	X	X	X
Additional Controls			
Base Payment 2000 X Year Trend		X	
Urban X Year Trend			X
Pre-BIPA Mean of Dep. Var.	475.98	475.98	475.98
R-Squared	1.000	1.000	1.000

Note: Table shows coefficients on distance-to-floor $\times$ 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 $\times$ year, and observations are weighted by the number of beneficiaries in the county. Year 2000, which is the year prior to BIPA implementation, is the omitted category. 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 ($N = 787$) are reported in parentheses.

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

	Dependent Variable:								
	Mean Monthly Premium ($)			Minimum Monthly Premium ($)			Zero Monthly Premium (%)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Δb X 2001	-0.300	-0.341	-0.299	-0.254	-0.293	-0.252	0.351	0.405	0.349
	(0.056)	(0.056)	(0.056)	(0.058)	(0.059)	(0.059)	(0.072)	(0.073)	(0.072)
Δb X 2002	-0.504	-0.535	-0.504	-0.434	-0.463	-0.434	0.483	0.523	0.483
	(0.061)	(0.064)	(0.061)	(0.075)	(0.078)	(0.076)	(0.094)	(0.097)	(0.095)
Δb X 2003	-0.450	-0.510	-0.450	-0.424	-0.481	-0.424	0.487	0.566	0.486
	(0.071)	(0.077)	(0.071)	(0.086)	(0.092)	(0.086)	(0.101)	(0.107)	(0.101)
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									
Base Payment 2000 X Year Trend		X			X			X	
Urban X Year Trend			X			X			X
Pre-BIPA Mean of Dep. Var.	12.53	12.53	12.53	6.44	6.44	6.44	65.06	65.06	65.06
R-Squared	0.71	0.72	0.71	0.65	0.66	0.65	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 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. 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 ($N = 787$) 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*	-0.136	0.402	0.589	3.827	3.622	18.725	0.021
	(0.618)	(0.726)	(4.396)	(3.654)	(4.595)	(4.424)	(0.047)
Δb X 2002*	-1.544	-2.717	0.180	5.111	3.756	22.721	0.053
	(0.769)	(0.840)	(4.719)	(4.513)	(6.668)	(5.321)	(0.049)
Δb X 2003*	-1.976	-3.010	3.571	-0.939	1.721	23.712	0.079
	(0.917)	(0.986)	(4.410)	(3.664)	(6.643)	(5.132)	(0.044)
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.	7.28	11.13	74.20	26.11	75.84	44.44	n/a
R-Squared	0.66	0.70	0.83	0.68	0.75	0.85	0.83

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 unscaled. 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. 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.

*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 (%)			HHI		
	(1)	(2)	(3)	(4)	(5)	(6)
Δb X 2001	-2.146	-1.128	-2.204	0.026	-0.017	0.024
	(1.746)	(1.876)	(1.761)	(0.024)	(0.026)	(0.024)
Δb X 2002	1.389	2.468	1.830	-0.015	-0.050	-0.024
	(2.437)	(2.542)	(2.450)	(0.029)	(0.031)	(0.030)
Δb X 2003	5.577	7.420	6.056	-0.039	-0.105	-0.049
	(2.517)	(2.803)	(2.540)	(0.032)	(0.037)	(0.032)
Main Effects						
County FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
Additional Controls						
Base Payment 2000 X Year Trend		X			X	
Urban X Year Trend			X			X
Pre-BIPA Mean of Dep. Var.	66.2	66.2	66.2	0.57	0.57	0.57
R-Squared	0.91	0.91	0.91	0.80	0.80	0.80

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 variable in columns 1 to 3 is an indicator for at least one plan, and the sample is the full sample of counties. The dependent variable in columns 4 to 9 is a Herfindahl-Hirschman Index (HHI) with a scale of 0 to 1, and the sample is restricted to county $\times$ years with at least one plan. 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	0.84	-2.96	-1.25	-0.300	1.076
	(0.62)	(1.72)	(0.47)	(0.056)	(0.267)
Δb X 2002	3.38	-0.93	-2.41	-0.504	0.903
	(0.85)	(3.48)	(0.60)	(0.061)	(0.125)
Δb X 2003	4.72	3.76	-3.24	-0.450	0.732
	(0.92)	(3.79)	(0.82)	(0.071)	(0.103)
Panel B: Pooled Post-BIPA Effect					
Δb X Post-BIPA	3.27	0.21	-2.68	-0.44	0.845
	(0.73)	(2.86)	(0.60)	(0.05)	(0.095)
Controls: All Panels					
Main Effects					
County FE	X	X	X	X	
Year FE	X	X	X	X	
Pre-BIPA Mean of Dep. Var.	30.53	485.25	484.48	10.90	

Note: Columns 1 through 4 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 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 × year, and observations are weighted by the number of beneficiaries in the county. 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.

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.[36] 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.

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.

A.2 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 $R(Q) = \int_{v_i \geq p^{-1}(Q)} r_i$,

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

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.3 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 gives us that the marginal individual in MA and TM is proportionally expensive: $MC^{MA}(Q^{MA}) = \phi MC^{TM}(Q^{TM})$. 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.4 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 A1 shows that these results are robust to including controls that isolate different subsets of the identifying variation. Odd columns in the table control for the base payment in year 2000 interacted with a linear time trend. Even columns control for urban status of the county interacted with a linear time trend.

A.5 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 A1, A2, A3, A4, A5, A6 and Tables A2, A3, A4, A5 and A6.

A.6 Selection: Alternative Specifications

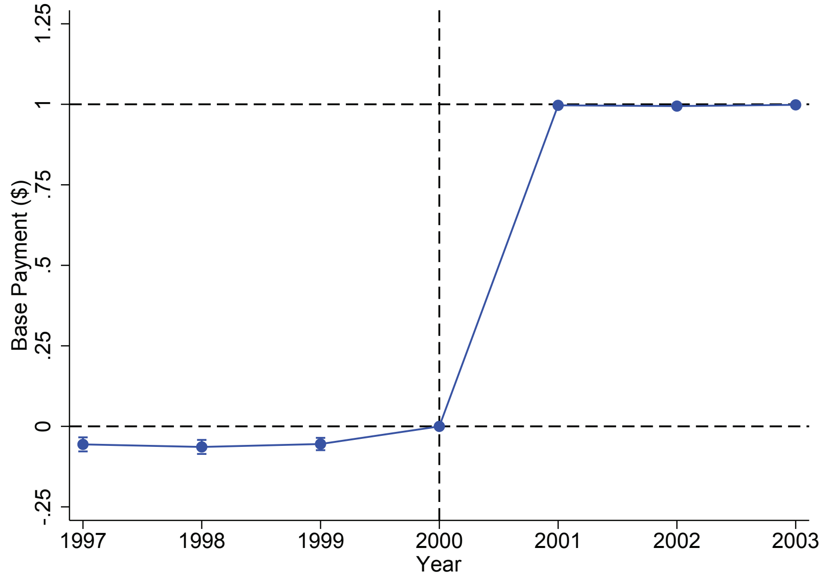
Section 6 investigates the role of selection in explaining our incomplete pass-through estimates. Table 7 and Figure 9 display the results with the baseline set of controls. Table A7 shows that these results are robust to including controls that isolate different subsets of the identifying variation. Columns 2, 5, and 8 in the table control for the base payment in year 2000 interacted with a linear time trend. Columns 3, 6, and 9 control for urban status of the county interacted with a linear time trend. 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 A7 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 A8. 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 column 1 of Table A8, the semi-elasticities of utilization with respect to MA enrollment for 2003 were 0.40 ($= \frac{0.00061}{0.03211}/4.7\%$) for Part A stays, 0.31 ($= \frac{0.00323}{0.2252}/4.7\%$) for Part A days, and 0.22 ($= \frac{0.0227}{2.1924}/4.7\%$) for Part B claims. Overall, these elasticities are similar to the elasticity implied by our cost estimates discussed in the text.

A.7 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 A9 displays the corresponding regression results as well as results for full-sample regressions that interact the market concentration measures with our floor distance variables ($\Delta b_j \times$ year). Overall, the coefficients show a statistically significant pattern of declining pass-through with market concentration.

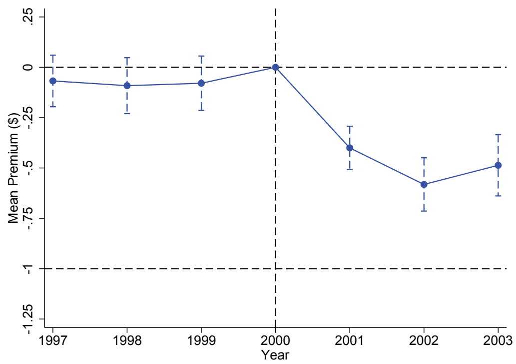
Figure A1: First-Stage Effect on Base Payments: Impact of \$1 Increase in Distance-to-Floor, Balanced Sample of Counties



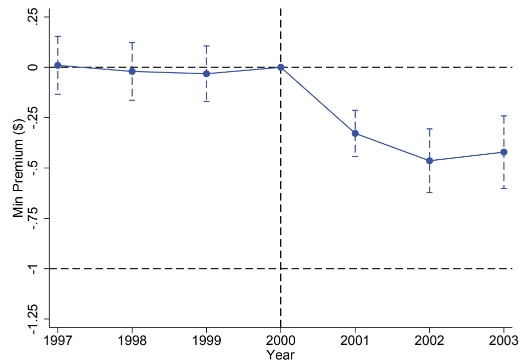
Note: Figure shows coefficients on the distance-to-floor × year interactions from difference-in-differences regressions with the monthly base payments as the dependent variable. The unit of observation is the county × year, and observations are weighted by the number of beneficiaries in the county. Controls 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 A2: Premium Pass-Through: Impact of $1 Increase in Monthly Payments, Balanced Sample of Counties

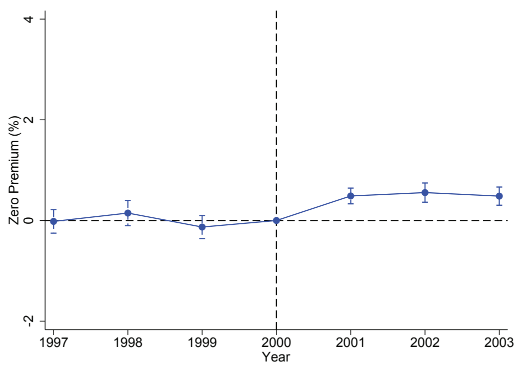
(A) Mean



(B) Minimum



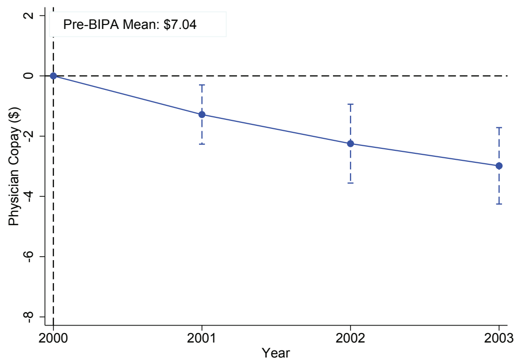
(C) Percent Zero



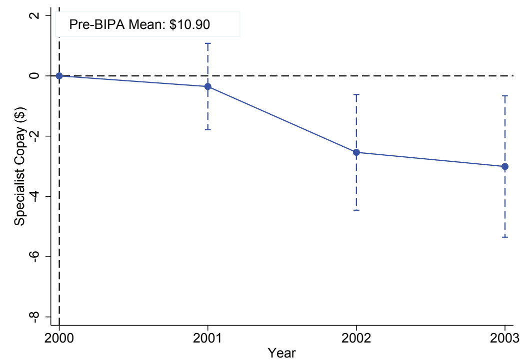
Note: Figure shows coefficients on distance-to-floor × year interactions from difference-in-differences regressions. The first-stage results displayed in Table A2 indicate that a $1 change in distance-to-floor translates into a $1 change in the monthly payments, so we can interpret the coefficients as 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. Controls are identical to those in Figure A1. 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 A3: Benefits Generosity: Impact of $50 Increase in Monthly Payments, Balanced Sample of Counties

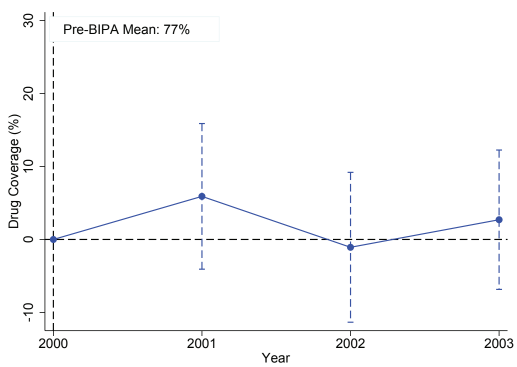
(A) Physician Copay



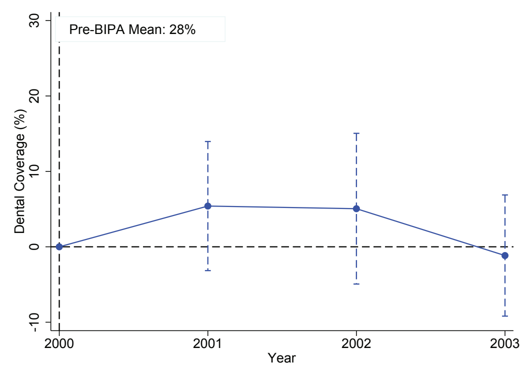
(B) Specialist Copay



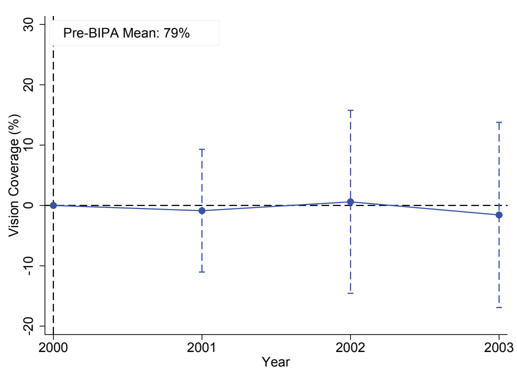
(C) Drug Coverage



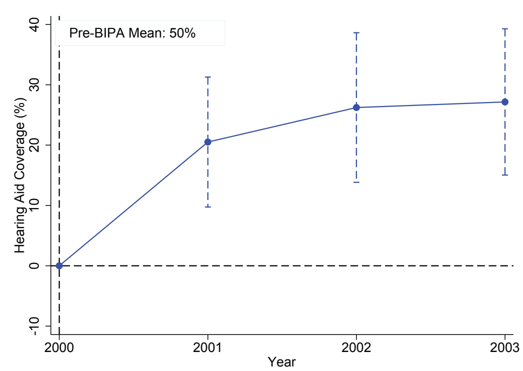
(D) Dental Coverage



(E) Vision Coverage

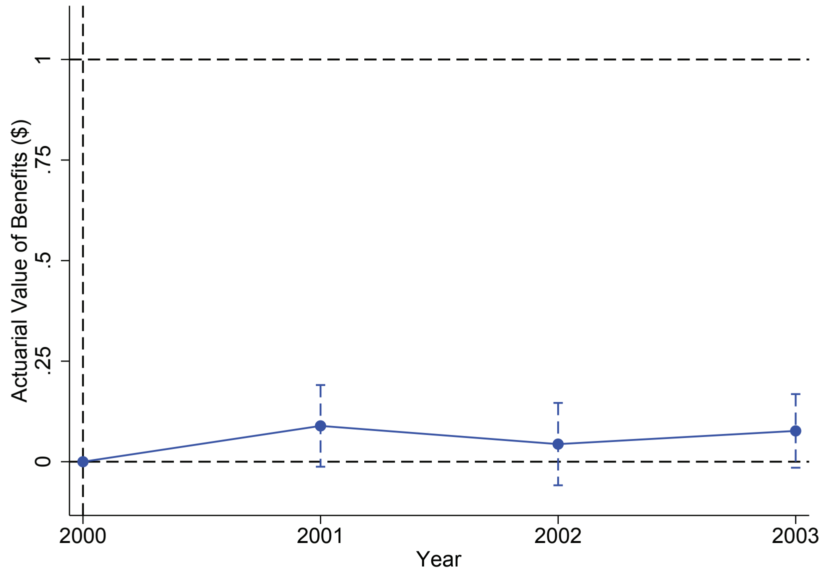


(F) Hearing Aid Coverage



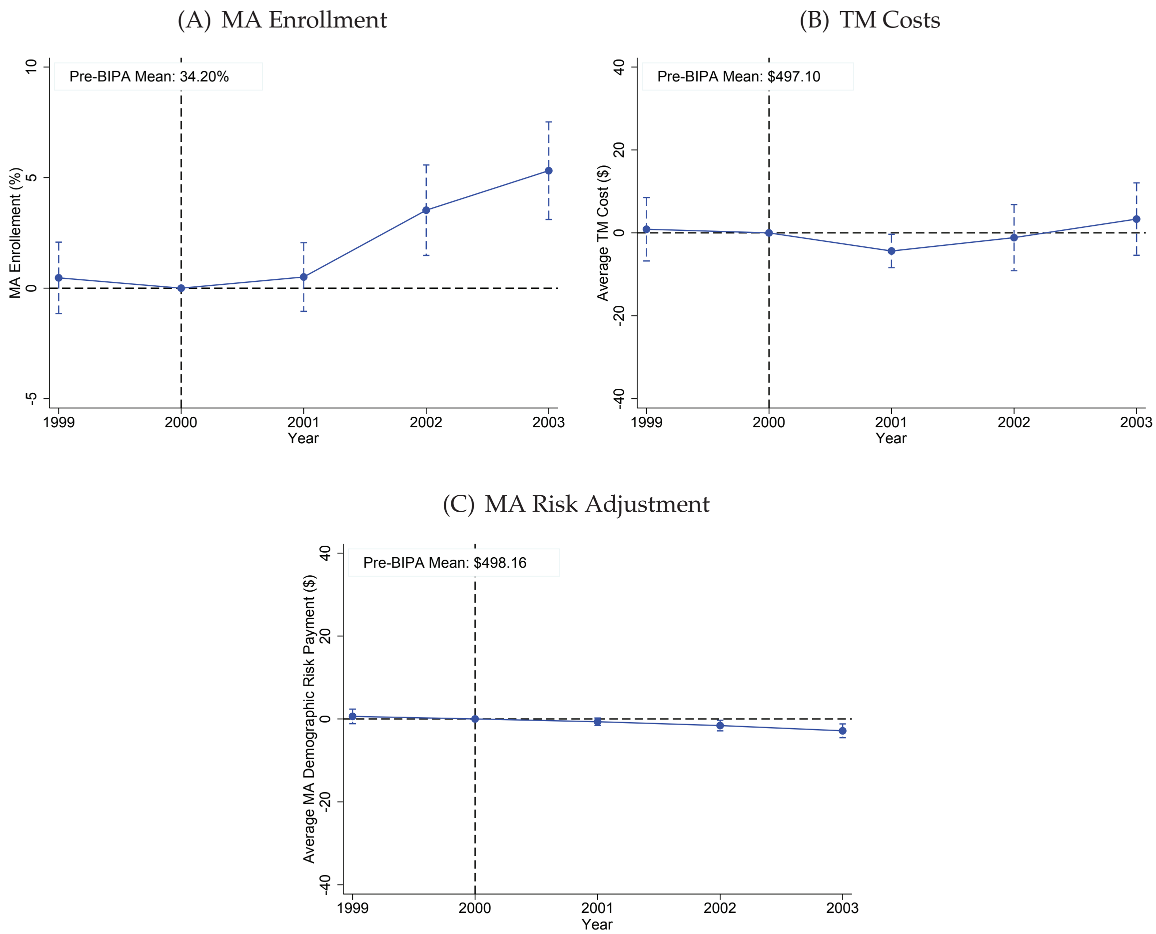
Note: Figure shows scaled coefficients on distance-to-floor × year interactions from difference-in-differences regressions. The first-stage results displayed in Table A2 indicate that a $1 change in distance-to-floor translates into a $1 change in the monthly payments, so we can interpret the coefficients 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. Controls are identical to those in Figure A1. 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 fringe 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 A4: Actuarial Value of Benefits: Impact of $1 Increase in Monthly Payments, Balanced Sample of Counties



Note: Figure shows coefficients on distance-to-floor × year interactions from difference-in-differences regressions. The first-stage results displayed in Table A2 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 × year, and observations are weighted by the number of beneficiaries in the county. Controls are identical to those in Figure A1. 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.

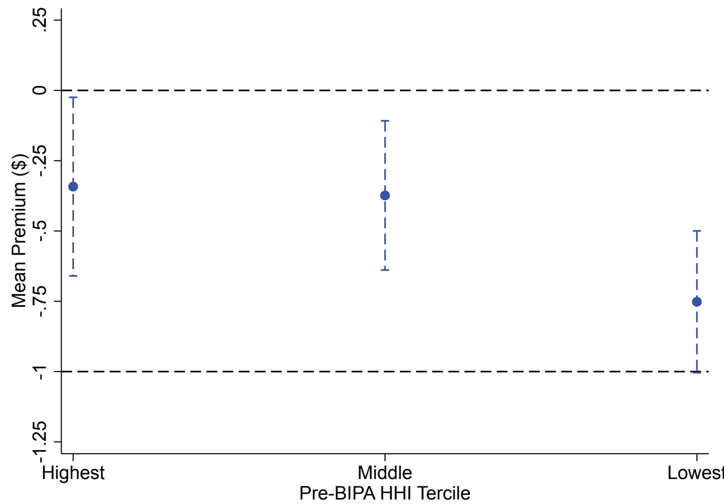
Figure A5: Selection: Impact of $50 Increase in Monthly Payments, Balanced Sample of Counties



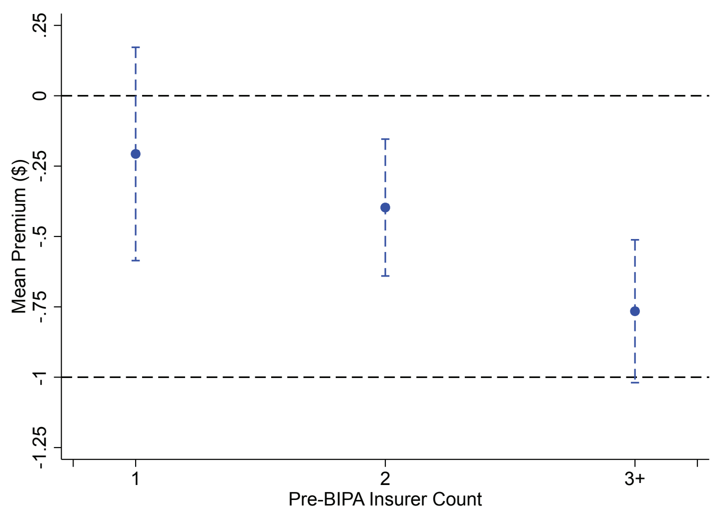
Note: Figure shows scaled coefficients on distance-to-floor × year interactions from difference-in-differences regressions. The first-stage results displayed in Table A2 indicate that a $1 change in distance-to-floor translates into a $1 change in the monthly payments, so we can interpret the coefficients 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. Controls are identical to those in Figure A1. 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 A6: Pass-Through and Market Concentration, Balanced Sample of Counties

(A) Insurer HHI



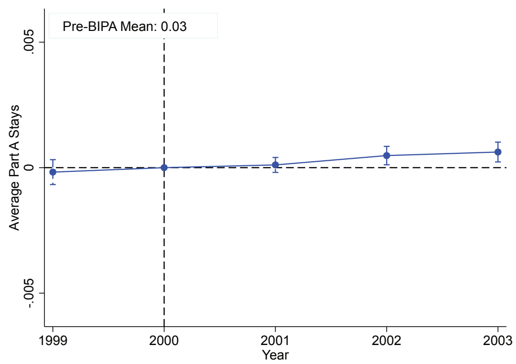
(B) Insurer Count



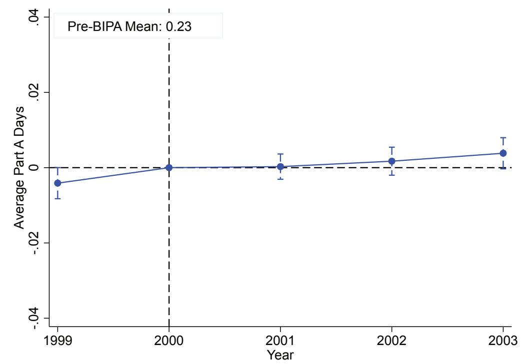
Note: Figure shows coefficients on distance-to-floor × year 2003 interactions from several difference-in-differences regressions. The dependent variable is the mean premium defined as in Figure 4. Each point represents a coefficient from a separate regression in which the estimation sample is stratified 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. Controls are identical to those in Figure A1. 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 A7: Utilization: Impact of $50 Increase in Monthly Payments

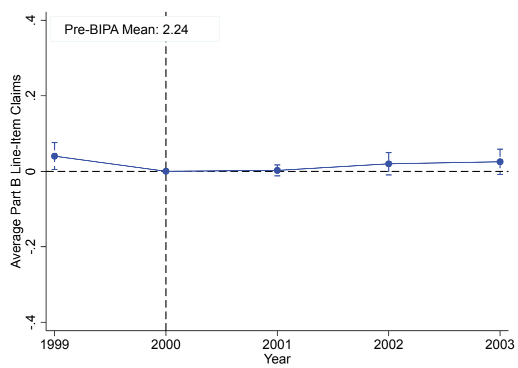
(A) Part A Stays



(B) Part A Days

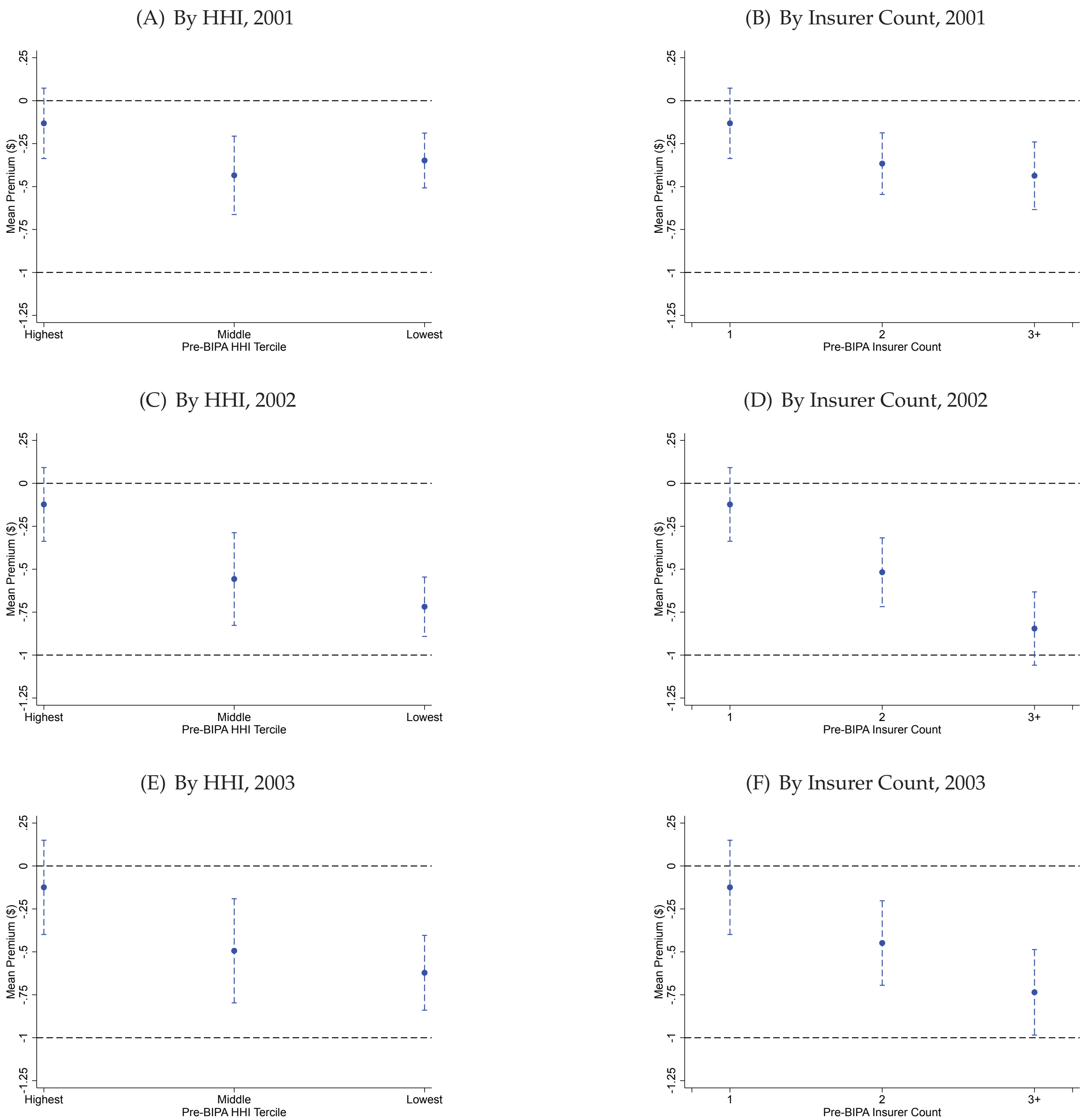


(C) Part B Line-Item Claims



Note: Figure shows scaled coefficients on distance-to-floor × 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 × year, and observations are weighted by the number of beneficiaries in the county. Controls are identical to those in Figure A1. 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 A8: Pass-Through and Market Concentration, 2001 to 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. Controls are identical to those in Figure A1. 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 A1: Benefits Generosity: Impact of Increase in 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*	-0.77	-0.17	0.22	0.37	0.57	0.82	5.29	4.14	9.14	3.63	18.87	18.74	0.03	0.02
	(0.63)	(0.62)	(0.83)	(0.73)	(4.57)	(4.39)	(4.16)	(3.61)	(4.89)	(4.61)	(4.83)	(4.42)	(0.05)	(0.05)
Δb X 2002*	-2.06	-1.69	-2.86	-2.89	0.16	1.28	6.29	6.58	8.22	3.80	22.83	22.79	0.06	0.07
	(0.81)	(0.77)	(0.96)	(0.85)	(4.86)	(4.78)	(4.96)	(4.53)	(6.77)	(6.70)	(5.93)	(5.44)	(0.05)	(0.05)
Δb X 2003*	-2.97	-2.15	-3.29	-3.21	3.54	4.82	1.34	0.73	10.34	1.77	23.93	23.79	0.10	0.10
	(0.97)	(0.93)	(1.29)	(1.01)	(4.79)	(4.48)	(5.41)	(3.66)	(7.13)	(6.69)	(6.64)	(5.26)	(0.05)	(0.04)
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														
Base Payment 2000 X Year Trend	X		X		X		X		X		X		X	
Urban X Year Trend		X		X		X		X		X		X		X
Pre-BIPA Mean of Dep. Var.	7.28	7.28	11.13	11.13	74.20	74.20	26.11	26.11	75.84	75.84	44.44	44.44	n/a	n/a
R-Squared	0.66	0.66	0.70	0.70	0.83	0.83	0.68	0.68	0.75	0.75	0.85	0.85	0.83	0.83

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. 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 unscaled. 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. 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.

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

Table A2: 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	0.997	0.992	0.998
	(0.001)	(0.002)	(0.001)
Δb X 2002	0.994	0.992	0.995
	(0.003)	(0.003)	(0.003)
Δb X 2003	0.998	0.992	0.999
	(0.003)	(0.003)	(0.003)
Main Effects			
County FE	X	X	X
Year FE	X	X	X
Additional Controls			
Base Payment 2000 X Year Trend		X	
Urban X Year Trend			X
Pre-BIPA Mean of Dep. Var.	527.44	527.44	527.44
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. Year 2000, which is the year prior to BIPA implementation, is the omitted category. 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 ($N = 343$) are reported in parentheses.

Table A3: Premium Pass-Through: Impact of $1 Increase in Monthly Payments, Balanced Sample of Counties

	Dependent Variable:								
	Mean Monthly Premium ($)			Minimum Monthly Premium ($)			Zero Monthly Premium (%)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Δb X 2001	-0.400	-0.442	-0.400	-0.328	-0.365	-0.326	0.487	0.547	0.481
	(0.055)	(0.055)	(0.055)	(0.059)	(0.059)	(0.059)	(0.080)	(0.083)	(0.081)
Δb X 2002	-0.582	-0.606	-0.582	-0.464	-0.485	-0.464	0.554	0.589	0.554
	(0.068)	(0.070)	(0.068)	(0.081)	(0.083)	(0.081)	(0.097)	(0.101)	(0.098)
Δb X 2003	-0.487	-0.538	-0.486	-0.421	-0.467	-0.420	0.484	0.558	0.481
	(0.078)	(0.083)	(0.078)	(0.092)	(0.097)	(0.092)	(0.092)	(0.100)	(0.093)
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									
Base Payment 2000 X Year Trend		X			X			X	
Urban X Year Trend			X			X			X
Pre-BIPA Mean of Dep. Var.	10.90	10.90	10.90	4.22	4.22	4.22	68.82	68.82	68.82
R-Squared	0.73	0.73	0.73	0.65	0.66	0.65	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 A2 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. Year 2000, which is the year prior to BIPA implementation, is the omitted category. Controls are identical to those in Table A2. 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 A4: 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*	-1.283	-0.353	5.907	5.403	-0.870	20.511	0.089
	(0.501)	(0.731)	(5.092)	(4.367)	(5.189)	(5.495)	(0.052)
Δb X 2002*	-2.249	-2.538	-1.073	5.054	0.597	26.231	0.044
	(0.668)	(0.980)	(5.239)	(5.098)	(7.736)	(6.325)	(0.052)
Δb X 2003*	-2.985	-3.007	2.705	-1.158	-1.576	27.155	0.077
	(0.647)	(1.197)	(4.872)	(4.099)	(7.829)	(6.183)	(0.047)
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.	7.04	10.90	76.91	28.36	79.28	49.74	n/a
R-Squared	0.68	0.72	0.82	0.65	0.74	0.84	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 A2 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 unscaled. 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. Year 2000, which is the year prior to BIPA implementation, is the omitted category. Controls are identical to those in Table A2. 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 A5: Plan Availability: Impact of $50 Increase in Monthly Payments, Balanced Sample of Counties

	Dependent Variable:					
	At Least One Plan (%)			HHI		
	(1)	(2)	(3)	(4)	(5)	(6)
Δb X 2001	-2.146	-1.128	-2.204	0.037	-0.008	0.035
	(1.746)	(1.876)	(1.761)	(0.030)	(0.032)	(0.030)
Δb X 2002	1.389	2.468	1.830	-0.001	-0.033	-0.012
	(2.437)	(2.542)	(2.450)	(0.034)	(0.036)	(0.035)
Δb X 2003	5.577	7.420	6.056	-0.030	-0.093	-0.044
	(2.517)	(2.803)	(2.540)	(0.037)	(0.041)	(0.038)
Main Effects						
County FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
Additional Controls						
Base Payment 2000 X Year Trend		X			X	
Urban X Year Trend			X			X
Pre-BIPA Mean of Dep. Var.	66.2	66.2	66.2	0.53	0.53	0.53
R-Squared	0.91	0.91	0.91	0.78	0.79	0.78

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 A2 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 $50 increase in monthly payments. The dependent variable in columns 1 to 3 is an indicator for at least one plan, and the sample is all counties ($N = 3,143$). The dependent variable in columns 4 to 6 the Herfindahl-Hirschman Index (HHI) on a scale of 0 to 1, and the sample is restricted to the balanced panel of counties with at least one plan in all years ($N = 343$). Year 2000, which is the year prior to BIPA implementation, is the omitted category. Controls are identical to those in Table A2. 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: 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	0.51	-4.36	-0.67	-0.400	1.17
	(0.79)	(2.05)	(0.46)	(0.055)	(0.18)
Δb X 2002	3.53	-1.14	-1.60	-0.582	0.94
	(1.04)	(4.07)	(0.65)	(0.068)	(0.15)
Δb X 2003	5.31	3.33	-2.86	-0.487	0.77
	(1.13)	(4.45)	(0.85)	(0.078)	(0.13)
Panel B: Pooled Post-BIPA Effect					
Δb X Post-BIPA	3.31	3.02	-1.64	-0.43	0.89
	(0.88)	(2.90)	(0.43)	(0.06)	(0.12)
Controls: All Panels					
Main Effects					
County FE	X	X	X	X	
Year FE	X	X	X	X	
Pre-BIPA Mean of Dep. Var.	34.20	498.16	497.10	10.90	

Note: Columns 1 through 4 of this table 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 A2 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 $\times$ year, and observations are weighted by the number of beneficiaries in the county. Year 2000, which is the year prior to BIPA implementation, is the omitted category. Controls are identical to those in Table A2. 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. 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 A7: 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	0.84	2.24	0.96	-2.96	0.93	-2.95	-1.25	-1.12	-1.30
	(0.62)	(0.65)	(0.62)	(1.72)	(1.93)	(1.75)	(0.47)	(0.56)	(0.48)
Δb X 2002	3.38	4.59	3.51	-0.93	2.47	-0.91	-2.41	-2.29	-2.47
	(0.85)	(0.88)	(0.86)	(3.48)	(3.80)	(3.54)	(0.60)	(0.80)	(0.60)
Δb X 2003	4.72	6.99	4.90	3.76	10.10	3.78	-3.24	-3.03	-3.32
	(0.92)	(1.01)	(0.93)	(3.79)	(4.51)	(3.86)	(0.82)	(1.19)	(0.83)
Panel B: Pooled Post-BIPA Effect									
Δb X Post-BIPA	3.27	5.54	3.49	0.21	6.67	0.23	-2.68	-2.32	-2.78
	(0.73)	(0.83)	(0.74)	(2.86)	(3.53)	(2.97)	(0.60)	(0.92)	(0.62)
Panel C: Pooled Post-BIPA Effect									
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									
Base Payment 2000 X Year Trend		X			X			X	
Urban X Year Trend			X			X			X
Pre-BIPA Mean of Dep. Var.	30.53	30.53	30.53	484.48	484.48	484.48	485.25	485.25	485.25

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 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. Year 2000, which is the year prior to BIPA implementation, is the omitted category. Controls are identical to those in Table A2. 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 A8: 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	0.00017	0.00015	0.00018	0.00098	-0.00068	0.00091	0.00316	0.03006	0.00256
	(.00013)	(.00013)	(.00013)	(.00138)	(.00145)	(.00139)	(.00647)	(.00936)	(.00664)
Δb X 2002	0.00049	0.00048	0.00050	0.00191	0.00046	0.00183	0.01832	0.04177	0.01761
	(.00016)	(.00018)	(.00016)	(.00161)	(.0018)	(.00162)	(.01247)	(.0158)	(.01264)
Δb X 2003	0.00061	0.00058	0.00062	0.00323	0.00052	0.00312	0.02270	0.06644	0.02178
	(.00017)	(.00022)	(.00017)	(.00178)	(.00218)	(.00179)	(.01446)	(.02179)	(.01471)
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									
Base Payment 2000 X Year Trend		X			X			X	
Urban X Year Trend			X			X			X
Pre-BIPA Mean of Dep. Var.	0.032	0.032	0.032	0.23	0.23	0.23	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. Year 2000, which is the year prior to BIPA implementation, is the omitted category. Controls are identical to those in Table A2. 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: 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)	(8)	(9)
Δb X 2001	-0.132	-0.435	-0.348	-0.132	-0.366	-0.437	-0.141	-0.086
	(0.105)	(0.116)	(0.081)	(0.105)	(0.091)	(0.100)	(0.136)	(0.150)
Δb X 2002	-0.123	-0.557	-0.718	-0.123	-0.518	-0.846	0.048	0.187
	(0.109)	(0.138)	(0.088)	(0.109)	(0.102)	(0.109)	(0.144)	(0.163)
Δb X 2003	-0.125	-0.494	-0.622	-0.125	-0.449	-0.736	-0.025	0.130
	(0.140)	(0.155)	(0.111)	(0.140)	(0.126)	(0.127)	(0.187)	(0.203)
Δb X 2001 X HHI Tercile							-0.065	
							(0.047)	
Δb X 2002 X HHI Tercile							-0.200	
							(0.048)	
Δb X 2003 X HHI Tercile							-0.156	
							(0.062)	
Δb X 2001 X Contract Count								-0.120
								(0.070)
Δb X 2002 X Contract Count								-0.343
								(0.077)
Δb X 2003 X Contract Count								-0.286
								(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.	18.86	10.71	10.73	18.86	11.48	10.06	12.10	12.10
R-Squared	0.70	0.72	0.73	0.70	0.70	0.76	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 7, each column represents the main specification applied to a different subsample defined by pre-BIPA market concentration. In columns 8 and 9, 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. Year 2000, which is the year prior to BIPA implementation, is the omitted category. Controls are identical to those in Table A2. All monetary values are inflation adjusted to 2000 using the CPI-U. Robust standard errors clustered at the county level are reported in parentheses.