Bankruptcy as Implicit Health Insurance[†]

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This paper examines the implicit health insurance that households receive from the ability to declare bankruptcy. Exploiting multiple sources of variation in asset exemption law, I show that uninsured households with a greater financial cost of bankruptcy make higher out-of-pocket medical payments, conditional on the amount of care received. In turn, I find that households with greater wealth at risk are more likely to hold health insurance. The implicit insurance from bankruptcy distorts the insurance coverage decision. Using a microsimulation model, I calculate that the optimal Pigovian penalties are three-quarters as large as the average penalties under the Affordable Care Act. (*JEL* D14, H51, I13, K35)

A large literature evaluates the effects of government policy on health insurance coverage. The question of why so many US households are uninsured is less well understood.[1] To better understand the insurance coverage decision, this paper examines a mechanism that has received little attention but may be important to households on the margin of insurance choice: implicit insurance from the ability to declare bankruptcy.

The implicit insurance from bankruptcy arises from the confluence of three factors. First, due to federal law, hospitals are required to provide emergency treatment on credit—and in most cases provide even nonemergency care without an upfront payment. Second, under Chapter 7 of the US bankruptcy code, households can discharge medical debt, giving up assets above exemption limits in return.[2] Third, because of the deadweight cost of the bankruptcy process, households and creditors have incentives to reach negotiated agreements that avoid formal bankruptcy filings.

[1]In a review of the literature, Gruber (2008, p. 581) concludes, "There are a variety of hypotheses for why so many individuals are uninsured, but no clear sense that this set of explanations can account for the 47 million individuals."

[2]The 2005 Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA) prevents some households from filing under Chapter 7 of the bankruptcy code. Below, I discuss how I use this source of over-time variation to test the predictions of the mechanism.

Bankruptcy, as a result, provides households with a form of high-deductible health insurance. Households are exposed to the financial risk from medical shocks up to the level of assets that can be seized in bankruptcy, and are insured against financial risk above this level. This implicit insurance affects the demand for health insurance. For households with fewer assets that can be seized in a bankruptcy filing, the ability to declare bankruptcy may crowd out conventional health insurance coverage. Health insurance is wealth insurance, to a certain degree, and is less valuable to those with fewer assets.

The main objective of this paper is to assess the quantitative importance of this mechanism. Hospitals have complex objective functions that may only place partial weight on profits. They may choose to provide charity care even when unconstrained by the threat point of bankruptcy. Households may view bankruptcy insurance as an incomplete source of coverage, better suited to acute health shocks than ongoing chronic conditions. They may worry about other costs—such as reduced access to credit markets—that arise from using this mechanism.

I assess the economic significance of the implicit insurance from bankruptcy with two sets of empirical analysis. First, I examine how out-of-pocket costs paid by the uninsured are affected by the assets these households would give up in bankruptcy. Second, I examine how insurance coverage is affected by a household's financial cost of filing.

Identifying the impact of bankruptcy is challenging because this implicit insurance is determined by household assets, which may partially reflect unobserved household-level variables, and bankruptcy laws, which may be correlated with other political economy factors. My main approach to addressing this issue is to isolate cross-state and within-state variation in the state-level asset exemption laws that specify the type and level of assets that can be seized in a Chapter 7 bankruptcy filing.[3] These laws vary considerably. Kansas, for example, allows households to exempt an unlimited amount of home equity and up to $40,000 in vehicle equity. Neighboring Nebraska allows households to keep no more than $12,500 in home equity or take a $5,000 wildcard exemption, which can be used for any type of asset.

I create simulated instrumental variables (Currie and Gruber 1996) to isolate different types of variation in these laws. I construct a cross-state instrument by calculating the mean financial cost of bankruptcy for a constant, nationally representative sample of households as though they lived in each state. This provides what Currie and Gruber (1996) call a "convenient parameterization" of the generosity of each state's asset exemption laws, purged of variation due to the characteristics of each state's actual residents.

I construct a within-state simulated instrument by partitioning the constant, nationally representative sample into demographic groups based on predetermined household characteristics. The mean financial cost for each demographic group in each state captures within-state heterogeneity in how asset exemption laws interact

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[3]Chapter 13 is the other bankruptcy option. Under Chapter 13, households make payments out of disposable income over the following three to five years.

with cross-group variation in wealth.[4] When I include controls for demographic groups and state fixed effects, this instrument isolates within-state variation in asset exemption law, addressing concerns that unobserved state-level factors might be driving the results. My preferred instrument combines both the cross- and within-state variation in what I term a pooled simulated instrument.

The identifying assumption for the cross-state instrument is that differences in seizable assets across states are uncorrelated with unobserved determinants of costs and coverage. For the within-state instrument, the identifying assumption is that differences in seizable assets across demographic groups within states are uncorrelated with unobserved determinants of the outcome variables. The within-state approach would be invalid, for example, if states that have relatively more generous bankruptcy exemptions for less educated demographic groups have other unobserved factors that differentially affect insurance coverage for these households.

Although I cannot incontrovertibly establish the validity of these assumptions, I take a number of steps to assess their validity in the data. One concern is that variation in asset exemptions might proxy for other economic or political economy factors that might also determine costs or coverage. I show that variation in asset exemptions is uncorrelated with economic conditions and partisan voting patterns. A second concern is that bankruptcy laws might partially reflect household risk preferences or employee bargaining weight, which might also impact health insurance coverage. If bankruptcy law reflects these types of factors, it should also influence other outcomes. I conduct falsification tests using pensions, income, and wages as the dependent variable and find no effect on these outcomes. Because these tests do not completely eliminate the potential for omitted variable bias, I interpret the results as indicative rather than definitive proof of the mechanism.

Using the cross- and within-states instruments and cost data from the Medical Expenditure Panel Survey (MEPS), I find that uninsured households with more seizable assets make greater out-of-pocket medical payments, conditional on the amount of care received. My preferred estimate indicates that a log increase in the financial cost of bankruptcy raises out-of-pocket payments by 34 percent for households with higher levels of medical utilization (more than $5,000 in annual charges). Consistent with the high-deductible nature of this insurance, I find no effect for households with lower levels of utilization (less than $5,000 in annual charges) and no effect on the extensive margin (positive charges).

Using the same sources of variation and data from the Survey of Income and Program Participation (SIPP), Panel Survey of Income Dynamics (PSID), and the MEPS, I find that households with a higher financial cost of bankruptcy are more likely to have health insurance. My preferred specification indicates that a log increase in the financial cost raises the probability of insurance coverage by 2.5–3.6 percentage points on a base of approximately 80 percent.

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[4] Among states with the same average level of asset exemption generosity, states with relatively larger vehicle and wildcard exemptions (and relatively smaller homestead exemptions) are more generous to demographic groups with a larger share of wealth in these assets (and a lower share of home equity). In particular, the slope of the relationship between wealth and seizable assets is steeper in states with relatively more generous vehicle and wildcard exemptions (and relatively smaller homestead exemptions), because low-wealth demographic groups have a large share of assets in these categories.

The magnitude of the coverage effect is economically significant. The estimates indicate that if the bankruptcy laws of the least debtor-friendly state of Delaware were applied nationally, approximately 8 percent of the uninsured would take up coverage. With a take-up semi-elasticity of -0.09 (Congressional Budget Office 2005), achieving the same increase would require a premium subsidy of 21 percent.

I use two additional sources of over-time variation, which in combination with the cross-state variation discussed above, allow me to provide evidence on the mechanism using a difference-in-differences strategy. The first is the 2005 Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA), which reduced the generosity of bankruptcy by restricting high-income households from filing under Chapter 7 under most conditions. Because the option to file under Chapter 7 is differentially valuable across states, the reform created difference-in-differences variation, with households in states with relatively more generous Chapter 7 asset exemptions experiencing larger declines in generosity than households in states where Chapter 7 was not particularly generous. I find similar effects on insurance coverage using this difference-in-differences identification strategy.

The second form of over-time variation is the 1986 Emergency Medical Treatment and Labor Act (EMTALA), which required hospitals to provide emergency care on credit and prohibited them from delaying treatment to inquire about insurance status or means of payment. Before EMTALA, households with a relatively low financial cost of bankruptcy had limited implicit health insurance because some hospitals might refuse to provide them with medical care. After EMTALA, these households had more generous implicit insurance because they were now guaranteed to receive medical care on credit. Consistent with this theory, I find that the coefficient on the financial cost of bankruptcy increases by about 50 percent after EMTALA.[5]

The implicit insurance from bankruptcy is inefficient because households do not face the full social cost of being uninsured, suggesting a system of "Pigovian penalties" that require households to pay for this externality.[6] I quantify the welfare effects of different penalty systems by calibrating a utility-based, microsimulation model of insurance choice for the sample of households without public insurance.[7] The optimal Pigovian penalties for this sample average \$334 and increase social surplus by \$58–\$113 per person. The penalties under the Affordable Care Act (ACA) average \$460 and increase surplus by \$28–\$50 per person. This shortfall is almost completely due to a negative correlation between the Pigovian and ACA penalties across households. While the Pigovian penalties are decreasing in the financial cost of bankruptcy, the ACA penalties, due to means testing, are increasing in this variable.

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[5] A natural question raised by the coverage result is how much households know about the implicit insurance from bankruptcy. A growing literature suggests that local information flows are important to the consumer bankruptcy decision (Gross and Souleles 2002; Fay, Hurst, and White 2002; Miller 2012). Households may have general impressions about financial risk from the news media or the experience of peers. I examine these perceptions with a web-based survey of individuals on the margin of insurance choice. More than 50 percent of the sample knows someone who has declared bankruptcy. I find that log increase in the financial cost of bankruptcy is associated with a 0.10 standard deviation increase in an index of perceptions of financial risk.

[6] This point is not novel. In discussing efficiency arguments for mandating that employers provide health insurance, Summers (1989, p. 178) cites the "externality that arises from society's unwillingness or inability to deny care completely to those in desperate need, even if they cannot pay."

[7] Governments typically encourage health insurance coverage for a number of non-Pigovian reasons. In this exercise, I abstract from these issues by focusing on the sample of households without public insurance or conditional access to Medicaid, and by assuming that health insurance is not subsidized through the tax code.

This paper is related to a number of strands of literature. By analyzing the interaction between implicit and conventional insurance, this paper is closely related to research by Brown and Finkelstein (2008) on long-term care insurance and the implicit insurance from spending down assets to qualify for Medicare. It is related to research by Andersen (2014) on Medigap insurance and the implicit insurance from the Medicaid Medically Needy Program. This paper is also closely related to Traczynski (2011), who shows that the implicit insurance from bankruptcy reduces the value of risk-sharing in marriage and leads to higher rates of divorce, and is more generally related to Dobbie and Song (2014), who quantify the beneficial effects of bankruptcy in raising future incomes and lowering rates of mortality.

But studying health insurance mandates, this paper complements recent work by Kolstad and Kowalski (2012). A key difference is that this paper is concerned with evaluating the case for increased coverage, whereas Kolstad and Kowalski (2012) focus on how to most efficiently increase coverage taking this goal as given. This paper shares similarities with a literature that examines the effect of medical debt on bankruptcy filings (Himmelstein et al. 2005; Dranove and Millenson 2006; Gross and Notowidigdo 2011). Unlike those papers, this study treats bankruptcy as a negotiation threat-point, not a dependent variable to be explained.

The rest of the paper proceeds as follows. Section I presents institutional background on personal bankruptcy and medical care. Section II provides an overview of the data. Sections III discusses the identification strategy. The main empirical results are presented in Section IV. Additional evidence using over-time variation from BAPCPA and EMTALA is presented in Section V. Section VI examines the implications of the mechanism for health insurance mandates. Section VII discusses additional implications. Section VIII concludes.

I. Institutional Background

A. *Bankruptcy as Implicit Health Insurance*

The implicit insurance from bankruptcy arises from the combination of three institutional features: federal EMTALA legislation that requires hospitals to provide medical care on credit even when repayment is unlikely, the ability of households to discharge this debt in bankruptcy, and the incentive for households and creditors to come to a negotiated solution that avoids the deadweight loss from a formal bankruptcy filing.

EMTALA was passed in April 1986 in response to a series of high-profile incidents of "patient dumping," chronicled in local newspapers and an episode of the television show *60 Minutes*.[8] EMTALA requires hospitals to treat patients with emergency medical conditions, and prohibits them from delaying treatment to inquire about insurance status or means of payment (USC. 42 §1395dd). As a matter of practice, most hospitals provide nonemergency medical care on credit as well. Hospitals generally lack the infrastructure to bill patients at the point of service (LeCuyer and Singhal 2007) and rarely deny service when repayment is unlikely.[9]

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[8] See Friedman (2011) for more details on the legislative history of this bill.

[9] In a survey of nonprofit hospitals, 90 percent reported never denying *any* medical services to patients with no insurance (IRS 2007). For-profit hospitals seem to operate similarly. For example, Duggan (2000) rejects the

Having received medical care on credit, households can, due to bankruptcy law, write off their medical debt and most other unsecured debt such as credit card debt and installment loans. Prior to the 2005 Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA), all households were eligible to file under either Chapter 7 or Chapter 13 of the bankruptcy code. Chapter 7 requires households to give up assets above their state's exemption limits, and accounted for approximately 70 percent of personal bankruptcy filings in the pre-BAPCA period (White 2007). Chapter 13 requires households to make payments out of disposable income over the following three to five years. By statute, these payments must be of at least the value that creditors would receive in Chapter 7.

BAPCPA reduced the generosity of the bankruptcy code by restricting Chapter 7 to households that passed either (i) a test based on income ("means test") or (ii) a test based on their ability to make payments over a five-year period ("repayment test"). Following BAPCPA, households with high incomes in unlimited homestead exemption states could no longer shield their home equity in a Chapter 7 filing.[10]

Online Appendix Table A1 shows Chapter 7 asset exemptions by state, taken from Elias (2007). Homestead exemptions exhibit substantial variation, ranging from $0 in 7 states to unlimited in 8 others; vehicle exemptions range from $0 in 15 states to at least $10,000 in 5 others; and wildcard exemptions, which can be applied to any asset, show a similar degree of variation. California residents can file under two different exemption systems, and residents of 14 other states can file under the federal exemption system if they choose.

This variation in asset exemptions has been remarkably stable over time. Homestead exemptions emerged in the second half of the nineteenth century as the result of an idiosyncratic set of historical circumstances. Since then, states have added vehicle and wildcard exemptions to keep up with changes in asset ownership but, as Skeel (2001) notes, most of the changes in asset exemptions in the twentieth century have been inflation updates. The last column of online Appendix Table A1 shows homestead exemptions from 1920 (Goodman 1993), and online Appendix Section A provides evidence on the stability of exemptions over time.

Households, however, do not have to formally declare bankruptcy to receive the implicit insurance it provides. Under the threat-point of bankruptcy, households and medical providers often resolve payments without an actual bankruptcy filing. There are multiple junctures where this occurs. Discounts on the list price of treatment—known as charity care—are offered at the point of service to the obviously indigent.[11] After treatment, many hospitals encourage financially strapped households to negotiate discounts, requiring the submission of information on income and assets (such as W-2s and mortgage payments) as part of their charity-care applications.[12]

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hypothesis that for-profit hospitals have a lower preference for charity care. Delgado et al. (2010) find that the majority of emergency departments offer preventative care to uninsured patients.

[10] Supporters of BAPCPA gave the examples of O. J. Simpson and WorldCom's Scott Sullivan, who both moved to Florida to take advantage of that state's unlimited homestead exemption, as symptomatic of the problems with the pre-BAPCPA bankruptcy system. See http://www.theatlantic.com/business/archive/2009/01/how-oj-simpson-may-help-keep-dick-fuld-from-stiffing-his-shareholders/4625/.

[11] Federal and state laws also influence charity-care provision. Nonprofits use charity care to meet their Community Benefit requirement. Some states subsidize care to the indigent through unpaid care pools. I account for these factors in the empirical analysis.

[12] When this information is not provided, hospitals run credit checks on indebted patients, filing suit if they find evidence of a mortgage or savings that could be claimed ("In Their Debt," *Baltimore Sun*, December 12–24, 2008).

Even when charity care is not provided, the lion's share of medical debt is charged off in the collection process. Despite contracting with debt collectors, providers recover only about 10–20 percent of bills submitted to the uninsured (LeCuyer and Singhal 2007).

B. *Public Insurance*

Bankruptcy is not the only source of implicit health insurance available to the uninsured. Households that receive negative health shocks can receive implicit insurance through the generosity of family members, friends, charities—and from public insurance programs. Below, I provide some background on implicit insurance from public programs and discuss how this insurance compares to the implicit insurance from bankruptcy.

The Medicaid program provides insurance coverage to individuals with income and assets below state-level thresholds deemed "categorically eligible": pregnant women, children, parents of Medicaid-eligible children, and the disabled. While the majority of Medicaid-eligible individuals take up their benefits, a sizable fraction do not enroll (Sommers et al. 2012). These eligible but not enrolled individuals are implicitly insured by Medicaid. After receiving care, hospitals and other medical providers have an incentive to enroll these individuals and bill Medicaid for the cost of care received. In my empirical analysis, I show that variation in the generosity of Medicaid across states is uncorrelated with variation in the generosity of the implicit insurance from bankruptcy, and I conduct a number of robustness checks to confirm that variation in Medicaid generosity is not contributing to the findings.

Categorically eligible individuals with incomes above state-level income thresholds can also qualify for insurance through the Medicaid Medically Needy (MN) program if their income *net of medical expenses* falls below state-level Medically Needy Income Limits (MNIL). As such, MN provides the categorically eligible with a form of safety net insurance for high-cost ongoing medical conditions, and provides all individuals with implicit insurance against disability, because disability makes individuals categorically eligible. Eligibility thresholds are extremely strict with a median income limit of 50 percent of the Federal Poverty Line (FPL) and asset limits of $2,000 or $3,000 in most states. The program is very small, covering 2.1 million non-elderly enrollees in 2009, or about 4.5 percent of total Medicaid enrollment (Kaiser Family Foundation 2012). In 2009, 34 states had MN programs. Controlling for the existence of this program has no impact on the empirical findings. See Kaiser Family Foundation (2012) for additional background on the MN program.

The Social Security Disability Insurance (DI) system provides a pathway to public insurance for individuals with ongoing disabilities. To qualify for DI, individuals must provide medical evidence of a disability that prevents them from engaging in "substantial gainful activity" and is expected to result in death or last for at least a year. Along with partial wage replacement, DI provides access to Medicare, the health insurance program for the elderly, but only after 24 months of enrollment. Therefore, access to Medicare through DI provides individuals with another form of safety net insurance for the onset of disabilities. Autor and Duggan (2006) report that 4.1 percent of 25-to-64-year-olds had DI in 2005. See Autor and Duggan (2006) for more on the program.

Taken together, public safety net insurance is an important form of implicit insurance but distinct from the implicit insurance from bankruptcy along at least two dimensions. First, bankruptcy is available to all households, and these programs are restricted to the disabled and non-disabled categorically eligible. Second, whereas individuals who are eligible but not enrolled in Medicaid are in effect retroactively covered for acute health shocks, individuals who are implicitly insured through MN or DI must first experience large levels of medical spending or undergo a 24-month waiting period to receive coverage. This delay can leave these individuals exposed to the full costs of acute health shocks and the initial costs of treating a disability or ongoing chronic condition.

II. Data

I conduct two sets of empirical analyses, first examining how the financial cost of bankruptcy affects the uninsured's out-of-pocket costs and second examining how this amount of wealth-at-risk affects insurance coverage. To examine the effect on out-of-pocket costs, I require information on medical costs, wealth, and state of residence. The only dataset I am aware of with this information is the MEPS, which is a rotating panel with survey waves introduced each year and followed for two-year periods. The MEPS has publicly available information on medical costs, and restricted-access information on wealth and state of residence. I use data from the start of the survey in 1996 to 2005, during which the survey waves covered 8,655–12,810 households per year.

To examine the effect on coverage, I require information on insurance coverage, wealth, and state of residence. I conduct this analysis using the MEPS and confirm the results using a number of additional surveys. My preferred dataset for examining the effect on coverage is the SIPP, which is composed of a series of multiyear panels. I use data from the 1996–2005 SIPP, which contains data from the 1996, 2001, and 2004 survey panels and covers 40,188–50,500 households per year. I prefer the SIPP to the MEPS because it has a substantially larger sample and externally validated information on wealth. For example, Wolff (1999) finds wealth in the SIPP tracks measures in the benchmark Survey of Consumer Finances (SCF) through the eightieth percentile of the wealth distribution, which is the relevant range for households on the margin of insurance coverage.[13] In contrast, MEPS estimates of the financial cost of bankruptcy are one-third lower than SIPP estimates across most of the distribution.[14]

I further confirm the coverage results using data from the PSID and the Current Population Survey (CPS). The PSID is a continuous panel with surveys conducted on a biennial basis. I use data from the 1999–2005 PSID, during which the survey covered approximately 7,000 households.[15] The PSID is weakly dominated

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[13] Wolff (1999) suggests that the SIPP understates wealth in the top quintile due to the oversampling of high-wealth individuals and a larger number of questions on investment-type assets in the SCF relative to the SIPP.

[14] A possible reason is that the MEPS asks substantially fewer questions about wealth than the SIPP or SCF (e.g., the SIPP collects data on eight types of financial assets, while the MEPS only collects data on three). This means that the MEPS may not capture some types of less-common assets. Since these less-common assets are typically seizable in bankruptcy, this can explain the lower financial cost of bankruptcy estimates in the MEPS. See the online Appendix to Bernard, Banthin, and Encinosa (2009) for more information on the MEPS wealth data.

[15] I start my sample in 1999 because information on wealth was not collected between 1996 and 1998.

by the SIPP, which has a larger sample size and similar quality data on household wealth. I also replicate the coverage findings using a two-sample instrumental variable research design (Angrist and Krueger 1992) that allows me to combine data on health insurance from the CPS and data on wealth from the SIPP. The CPS is a repeated cross-sectional survey of approximately 60,000 households per year, making it the largest dataset that I am aware of with information on insurance coverage and state of residence during my time period, but does not have information on household wealth. I describe my two-sample IV approach in more detail below.

The analysis in all datasets is conducted at the household level. Household-level wealth and medical costs are defined as the sum of individual-level wealth and medical costs, and household-level insurance coverage is defined as the fraction of household members with health insurance. I construct household-level survey weights, which I define as the sum of individual-level survey weights across household members. These weights have the desirable feature that average health insurance coverage calculated using these weights matches the population average.[16] I inflation-adjust monetary variables to 2005 US dollars using the CPI-U.

In all datasets, I exclude from the baseline sample households with one or more members with public insurance or a member who is eligible for Medicare (i.e., age 65 or older).[17] In the MEPS, I drop the first year of each survey wave because wealth information is only collected in the second year. After data restrictions, the MEPS has 61,405 households and 61,405 household-year observations, the SIPP has 103,313 households and 1,251,907 household-month observations, the PSID has 7,930 households and 20,774 household-year observations, and the CPS has 666,629 households and 666,629 household-year observations. Standard errors in all specifications are clustered at the level of the instrumental variable. By clustering above the household level, the number of observations per household does not impact the statistical inference.

A. *Financial Cost of Bankruptcy*

A key variable in the empirical analysis is the financial cost of bankruptcy. In the pre-BAPCPA period, I follow Fay, Hurst, and White (2002) and use the financial cost of Chapter 7 to characterize the financial cost under both chapters. Since debt payments under Chapter 13 can be no smaller than debt payments under Chapter 7, households face a weakly lower financial cost to filing under Chapter 7.[18]

Let $\mathbf{w}_i$ denote the vector of assets and debts for household i, and let $\mathbf{e}_j$ denote the vector of exemption laws in state j.[19] The pre-BAPCPA financial cost of bankruptcy

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[16] These weights contrast with the SIPP "family weights," which are defined as the individual-level weight of the household reference person. This measure will underweight large families and produce biased estimates of aggregate statistics. See the SIPP User's Guide for more information.

[17] In the MEPS, I drop the 3.6 percent of households with missing wealth variables. In the SIPP, I drop the 0.9 percent of observations where health insurance is imputed.

[18] Households often file under Chapter 13 to avoid losing their home in bankruptcy. While the financial costs of Chapter 13 must be weakly higher, filing under Chapter 13 is rational if the household places a large idiosyncratic value on retaining its home.

[19] Households with one adult are assigned the individual exemptions. Households with a married couple are assumed to file jointly and are assigned the joint-filer exemptions, which are twice the individual exemptions in most states.

is given by assets that can be seized minus debt that can be discharged plus filing costs:

$$w_{Pre}^S(\mathbf{w}_i, \mathbf{e}_j) = \text{Seizable Assets}(\mathbf{w}_i, \mathbf{e}_j) - \text{Dischargeable Debt}(\mathbf{w}_i) + \text{Filing Cost}.$$

Seizable assets are calculated as the sum of assets above the exemption level in each statutorily defined asset category:[20, 21]

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\begin{aligned}
\text{Seizable Assets}(\mathbf{w}_i, \mathbf{e}_j) = \ & \max\{\max[\text{Home Equity}_i - \text{Homestead Exemption}_j, 0] \\
& + \max[\text{Vehicle Equity}_i - \text{Vehicle Exemption}_j, 0] \\
& + \max[\text{Retirement Assets}_i - \text{Retirement Exemption}_j, 0] \\
& + \max[\text{Financial Assets}_i - \text{Financial Exemption}_j, 0] \\
& + \text{Other Assets}_i - \text{Wildcard}_j, 0\}.
\end{aligned}
$$

Dischargeable debt is defined as unsecured debt and is unaffected by state of residence. Filing costs, which include an estimate of legal fees, are set to $2,000, as estimated by Elias (2007). For households with multiple options (e.g., state or federal), I calculate the financial cost under each option and assign households the one that minimizes its financial cost of bankruptcy. For more details on the financial cost calculations, see online Appendix Section B.

As discussed in Section I, the main provision of BAPCPA was to restrict Chapter 7 to households that passed either a means or repayment test. Households pass the means test if their income is less than the state median income for households of their size. If households do have a low enough income, they may still qualify for Chapter 7 if they have a low enough *seizable income*. Following Elias (2007), I define seizable income as income minus expense allowances for food and clothing, mortgage payments or rent, home and cellular telephones, transportation, insurance, and taxes. Households also pass the means test if their seizable income is less than $110 per month. Households pass the repayment test if their seizable income is $110–$182.50 per month *and* payments of this amount would result in the payoff of less than 25 percent of the household's unsecured debt over a five-year repayment period.[22]

The post-BAPCPA financial cost of bankruptcy is given by

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[20] Calculating seizable assets by asset types ignores potential gains from reallocating wealth into asset categories with unused exemptions immediately before a bankruptcy filing. This seems appropriate, since such reallocation is explicitly prohibited under bankruptcy law and judges have broad discretion to root out this type of behavior (Elias 2007).

[21] Following the law, the formulation allows the wildcard exemption to be applied toward Other Assets and assets in excess of the exemption in the other asset categories.

[22] A strange feature of the repayment test is that it provides an incentive for households to accumulate additional debt so as to exceed the 25 percent threshold.

$$w_{Post}^S = \begin{cases} w_{Pre}^S & \text{if passes means or repayment test} \\ \max\left\{ 5 \times \text{Seizable Income} - \text{Dischargeable Debt} + \text{Filing Cost}, \ w_{Pre}^S \right\}. \end{cases}$$

Households that pass the means or repayment test make the same payments of w_{Pre}^S before and after BAPCPA. Households that do not qualify typically make payments of seizable income over five years and have a post-BAPCPA financial cost of $w_{Post}^S = 5 \times$ Seizable Income $-$ Dischargeable Debt $+$ Filing Cost. Since households must pay at least as much as they would pay under Chapter 7, their post-BAPCPA financial cost of bankruptcy is the maximum of this value and w_{Pre}^S. For more details on the calculation of seizable income, see online Appendix Section B.

B. *Summary Statistics*

Online Appendix Table A2 shows summary statistics on the pre-BAPCPA financial cost of bankruptcy by insurance status in the baseline SIPP, PSID, and MEPS datasets. Panel A data from the SIPP show that financial costs are right skewed with a median of $42,071 and a mean of $242,611 in the pooled sample. I find that 15 percent of households have a negative financial cost of bankruptcy, which lines up with White (1998), who finds that 15 percent of households would "financially benefit from bankruptcy" if factors such as stigma and the option value of being able to file again are not taken into account.

Seizable assets average $250,027. Due to the large homestead exemptions in many states, seizable home equity accounts for less than one-quarter of this amount. Dischargeable debt levels are small, averaging $9,572 per household. The financial cost summary statistics are similar in the PSID and somewhat lower in the MEPS. One potential reason for the lower values is that the MEPS assets supplemental survey is less extensive than the other surveys and may not fully capture all the components of household wealth.

The financial cost of bankruptcy diverges sharply by insurance status. Using SIPP data, I calculate that 56 percent of the uninsured would give up less than $5,000 in a bankruptcy filing and 64 percent would give up less than $10,000. On the other hand, 49 percent of households with private insurance would give up more than $50,000 and 66 percent would give up at least $10,000.

Online Appendix Table A3 shows summary statistics on medical costs in the baseline MEPS sample. Annual medical charges, defined as the list price of medical services used that year, average $7,113 per household. Total payments, defined as the sum of payments received, are substantially less than charges, because of discounts negotiated by insurance providers and charity care or bad debt. For the privately insured, total payments average $4,819 per household. Ninety-four percent of these payments are either out-of-pocket or made by private insurance providers. For the uninsured, total payments average $1,475 per household. Fifty percent of these payments are out-of-pocket. Miscellaneous payments (such as payments from charity-care pools, workers' compensation, or automobile insurance) account for most of the rest. For more details on the construction of the medical costs variables, see online Appendix Section B.

Online Appendix Table A4 shows insurance status in the baseline sample. In the SIPP, 19.4 percent of the sample is uninsured, 75.5 percent has insurance through an employer or union, and 5.1 percent has individually purchased coverage. These values are similar in the PSID and MEPS. Given the low rate of individually purchased coverage, I am unable to detect differential responses on this outcome.[23]

III. Empirical Strategy

In this section, I discuss the empirical strategy I use to test the central predictions of the mechanism: that households with a higher financial cost of bankruptcy faced increased out-of-pocket cost risk if uninsured and are more likely to hold conventional health insurance as a result. Estimating these effects poses a number of identification problems. First, outcome and the financial cost of bankruptcy may be jointly determined by unobserved factors (omitted variables).[24] Second, uninsured households have a strategic incentive to adjust their asset holdings to minimize financial losses in the event of a bankruptcy filing (reverse causality). Third, mismeasurement of assets may attenuate the estimates toward zero (measurement error). Fourth, state-level asset exemption laws may be correlated with unobserved state-level factors (endogenous asset exemption laws).

My main empirical strategy to address these issues is to construct simulated instrumental variables (Currie and Gruber 1996) that isolate cross- and within-state variation in the financial cost of bankruptcy due solely to legislative differences in asset exemption laws in the pre-BAPCPA time period.[25] I construct a *cross-state simulated instrument* by taking the entire sample of households and calculating the mean financial cost of bankruptcy as though this sample faced the asset exemption laws of each state. For state j, the instrument is given by

$$(1) \qquad z_j = \frac{1}{|\mathbb{N}|} \sum_{it \in \mathbb{N}} \ln \ w^S(\mathbf{w}_{it}, \mathbf{e}_j),$$

where $\mathbf{w}_{it}$ is wealth for household i in time period t, $\mathbf{e}_j$ is asset exemption laws in state j, $w^S(\mathbf{w}_{it}, \mathbf{e}_j)$ is the financial cost of bankruptcy, and $\mathbb{N}$ is the entire set of household $\times$ time periods in the data. This instrument provides what Currie and Gruber (1996) call a "convenient parameterization" of the generosity of each state's asset exemption laws, purged of variation due to the characteristics of each state's actual residents.

My preferred simulated instrument builds upon this cross-state instrument to additionally capture within-state variation in asset exemption laws. Among states with the same level of asset exemption generosity on average, states with relatively

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[23] While it is sufficient to view bankruptcy as a threat-point that does not necessarily occur in equilibrium, it would be interesting to examine effects on actual bankruptcy filings. Unfortunately, there is no dataset that I am aware of that permits this analysis. Bankruptcy is not recorded in the SIPP or MEPS. It was recorded during the 1996 wave of the PSID, but this wave does not have information on wealth.

[24] For instance, in the second-stage coverage equation, unobserved risk preferences could generate positive bias if more risk-averse households are more likely to accumulate precautionary savings and purchase insurance. Unobserved health shocks could generate negative bias by depleting assets and increasing preferences for coverage.

[25] Other than the variation induced by BAPCPA, there is very little panel variation in these exemptions. As I discuss in more detail below, real exemption levels have been remarkably stable over time since 1920. In particular, most of the changes since the Bankruptcy Reform Act of 1978 have been small updates to account for inflation. I thank Richard Hynes for sharing data that allowed me to examine this phenomenon.

larger vehicle and wildcard exemptions (and relatively smaller homestead exemptions) are relatively more generous to demographic groups with a larger share of wealth in these assets (and a lower share of home equity). In particular, the slope of the relationship between wealth and the financial cost of bankruptcy is steeper in states with relatively more generous vehicle and wildcard exemptions (and relatively smaller homestead exemptions), since lower-wealth demographic groups have a larger share of assets in these categories.

To construct this *pooled simulated instrument*, I divide the sample into $k = 1, \ldots, K$ demographic groups based on predetermined household characteristics. In particular, I use the full interaction of age group, race, education group, and family structure to define these groups.[26] As before, these groups aggregate across households that live in different states. For each demographic group k and state j, the instrument is given by

$$(2) \qquad\qquad z_{jk} = \frac{1}{|\mathbb{N}_k|} \sum_{it \in \mathbb{N}_k} \ln w^S(\mathbf{w}_{it}, \mathbf{e}_j) \quad \text{for } k = 1, \ldots, K,$$

where $w^S(\mathbf{w}_{it}, \mathbf{e}_j)$ is defined as before and $\mathbb{N}_k$ is the entire set of household $\times$ time periods in demographic group k. The instrument varies at the state $\times$ demographic group level, and I include a set of dummy variables for each demographic group $k = 1 \ldots K$ as controls in all specifications with this instrument to partial out cross-group variation in the financial cost of bankruptcy.

For households on the margin of insurance choice, the financial cost of bankruptcy exhibits substantial variation within demographic groups. Online Appendix Figure A2 presents histograms of the financial cost of bankruptcy for three selected demographic groups: (i) 18–34-year-old, white, high-school-educated single adults without children; (ii) 18–34-year-old, non-white, college-educated couples without children; and (iii) 35–44-year-old, non-white, high-school-educated couples with at least one child. I selected these groups by sorting the K demographic groups by their mean financial cost of bankruptcy and choosing the groups at the weighted fifth, twenty-fifth, and forty-fifth percentiles of this distribution. Since 23 percent of households are uninsured in my sample, this provides me with demographic groups that span a $+/-$ 20-percentage-point window of the marginal demographic group. In group (i) with the lowest financial cost of bankruptcy, 26 percent of households would give up more than \$16,000 in a bankruptcy filing; in group (iii) with the highest financial cost, 33 percent would give up less than \$4,000.[27] I view these histograms as showing there is sufficient variation in the financial cost of bankruptcy across the marginal demographic groups in the data.

By capturing both cross- and within-state variation, this pooled simulated instrument has a number of advantages over the cross-state instrument. First, it increases first-stage power by harnessing a greater amount of plausibly exogenous variation in the financial cost of bankruptcy across households. Second, by capturing a broader

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[26] Age groups are 18–34, 35–44, 45–54, and 55–64; race is white and non-white; education groups are high school or less, some college or college degree, and some graduate school or a graduate degree; and family structure is single, childless couple, single parent, and couple with one or more children.

[27] The group (i) households with a high financial cost are often homeowners in states with low homestead exemptions. The group (iii) households with a low financial cost are households in high-exemption states with all of their wealth in home equity.

amount of variation, the instrument identifies effects that are "local" to a larger share of the population and therefore closer to the parameter of interest for broad-based counterfactuals. Third, the instrument allows me to estimate models that isolate within-state variation in asset exemption law by adding state fixed effects to the regression specifications.

A. *Econometric Model*

The first-stage equation for household i that actually resides in state j in time period t is given by

$$(3) \qquad \ln w_{ijt}^S = \alpha_z z_{jk} + \alpha_j + \alpha_t + \alpha_k + f(x_{it}, \alpha_x) + \epsilon_{ijt},$$

where z_{jk} is the pooled instrument, α_j and α_t are state and year fixed effects, α_k are demographic group fixed effects, $f(x_{it}, \alpha_x)$ is a fourth-order polynomial in household income, and ϵ_{ijt} is the error term. I include the year and demographic group fixed effects in all specifications to control for unobserved determinants of the outcome variable. I include the polynomial in income in all specifications because determinants of outcome variables, such as hospital charity care, are sometimes directly determined by household income. In specifications with the cross-state instrument, I exclude the state fixed effects because there is no within-state variation. The second-stage equation for outcome y_{ijt} is given by

$$(4) \qquad y_{ijt} = \beta_w \ln w_{ijt}^S + \beta_j + \beta_t + \beta_k + f(x_{it}, \beta_x) + \nu_{ijt},$$

where β_j and β_t are state and year fixed effects, β_k are demographic group fixed effects, $f(x_{it}, \beta_x)$ is a fourth-order polynomial in household income, and ν_{ijt} is the error term.[28]

I examine the effect on costs by regressing log annual out-of-pocket costs on the log financial cost of bankruptcy and controls.[29] In some specifications, I also control for medical utilization with a polynomial in annual charges. This is potentially important because the sign of the unconditional effect of the financial cost of bankruptcy on out-of-pocket payments is theoretically ambiguous due to offsetting insurance and moral hazard effects.[30] My primary analysis focuses on the sample of uninsured households with positive medical utilization. I also examine whether bankruptcy impacts the extensive margin of whether households receive care.

I examine the effect on coverage by regressing insurance coverage on the log financial cost of bankruptcy and controls. I use a probit functional form in the preferred specification for the standard reason that the dependent variable is limited to the unit interval. I show that linear probability models produce similar estimates.

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[28] I take the log of seizable assets because of the long right tail of this variable in the data. In the preferred specification, I bottom-code seizable asset at the filing cost of $2,000 and include an indicator for bottom-coding as a control. I fail to reject this functional form compared to more flexible alternatives. The qualitative findings are robust to bottom-coding at other values and to a linear functional form.

[29] I set the dependent variable to zero when out-of-pocket payments are zero. This is rarely the case. In the sample analyzed, less than 4 percent of households make zero out-of-pocket payments.

[30] Controlling for charges raises its own problems if charges are endogenous to bankruptcy laws. The estimates are very similar with and without this control.

I exclude households with publicly provided insurance from the baseline sample, as these households are less likely to make active decisions about health insurance coverage, and I include these households in robustness checks to show that sample selection does not influence the estimates.

I use multiple observations per household to increase the precision of my estimates. Because the error term is correlated within households over time, an approach that does not account for this correlation would lead to downward-biased standard errors. My solution is to cluster the standard errors at the level of instrumental variable. Because the instrumental variable varies at the state or state × demographic group level, this approach allows for an arbitrary degree of correlation within households over time. I show the results are robust to specifications that restrict the sample to a single observation per household.

Together these instruments allow me to isolate variation in the financial cost of bankruptcy due solely to cross-state, within-state, or pooled variation in asset exemption laws. The first three identification concerns (omitted variables, reverse causality, measurement error) are addressed by all of the instruments. Similar results using the cross- and within-state identifying variation should alleviate concerns about the exogeneity of the demographic groups used to construct the pooled simulated instrument. The within-state strategy addresses the fourth concern (endogenous asset exemption laws) as the state fixed effects directly absorb any unobserved state-level variation that might be correlated with asset exemption laws and the outcome variable.

IV. Results

A. *First Stage*

I start by presenting estimates of the implied first stage. Table 1 shows estimates from ordinary least squares (OLS) regressions of the log financial cost of bankruptcy on the simulated instruments in the SIPP, PSID, and MEPS. Column 1 shows estimates from a specification that pools the within- and cross-state variation, column 2 shows estimates that isolate the within-state variation, and column 3 shows estimates that isolate the cross-state variation. Standard errors in this and all subsequent specifications are clustered at the level of the instrumental variable. The first stage is powerful with an *F*-statistic above 100 in most specifications. In the SIPP and PSID, the coefficient on the instrument is close to 1, which is consistent with zero correlation between asset exemption laws and wealth.[31] In the MEPS, the coefficient is somewhat lower, with point estimates ranging from 0.68 to 0.90. Online Appendix Figure A3 visually depicts the cross-state first stage.

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[31] This finding should not necessarily be interpreted as evidence against a casual effect of asset exemption laws on wealth, holding other factors equal. By simultaneously increasing interest rates (Gropp, Scholz, and White 1997) and raising the incentive to hold assets, higher asset exemptions could generate offsetting supply and demand effects that result in the zero net effect found here.

TABLE 1—FIRST STAGE: REGRESSIONS OF THE FINANCIAL COST OF BANKRUPTCY
ON THE SIMULATED INSTRUMENTS

	log financial cost of bankruptcy		
	Pooled IV (1)	Within-state IV (2)	Cross-state IV (3)
Panel A. SIPP			
Pooled simulated instrument	1.072*** (0.023)	1.179*** (0.053)	
Cross-state simulated instrument			1.056*** (0.074)
Controls			
Demographic controls	X	X	X
Year fixed effects	X	X	X
State fixed effects		X	
R^2	0.336	0.347	0.330
F-statistic on instrument	2,172	495	204
Panel B. PSID			
Pooled simulated instrument	0.903*** (0.041)	0.921*** (0.108)	
Cross-state simulated instrument			0.893*** (0.061)
Controls			
Demographic controls	X	X	X
Year fixed effects	X	X	X
State fixed effects		X	
R^2	0.436	0.449	0.432
F-statistic on instrument	485	73	214
Panel C. MEPS			
Pooled simulated instrument	0.799*** (0.015)	0.898*** (0.040)	
Cross-state simulated instrument			0.683*** (0.030)
Controls			
Demographic controls	X	X	X
Year fixed effects	X	X	X
State fixed effects		X	
R^2	0.539	0.544	0.531
F-statistic on instrument	2,837	517	518

Notes: Table shows the coefficient on the instrument from OLS regressions of the log financial cost of bankruptcy on the simulated instrument and controls. The cross-state simulated instrument is the mean log financial cost of bankruptcy for the entire sample of households as though this sample faced the asset exemption laws of each state. The pooled simulated instrument is similarly constructed by predetermined demographic group, where groups are defined by the full interaction of age group, race, education group, and family structure. Demographic controls are demographic-group dummies and a fourth-order polynomial in annual income. Pooled 1996–2005 SIPP, 1999–2005 PSID, and 1996–2005 MEPS, excluding households with public insurance or a member age 65 or older, inflation-adjusted to 2005 using the CPI-U. Sample size is 1,251,907 in the SIPP; 20,774 in the PSID; and 61,405 in the MEPS. Robust standard errors clustered at the level of the instrument are in parentheses.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

B. *Effect on Costs*

I next examine the effect of bankruptcy law on the financial risk faced by the uninsured. Panel A of Figure 1 shows the relationship between payments (*y*-axis)
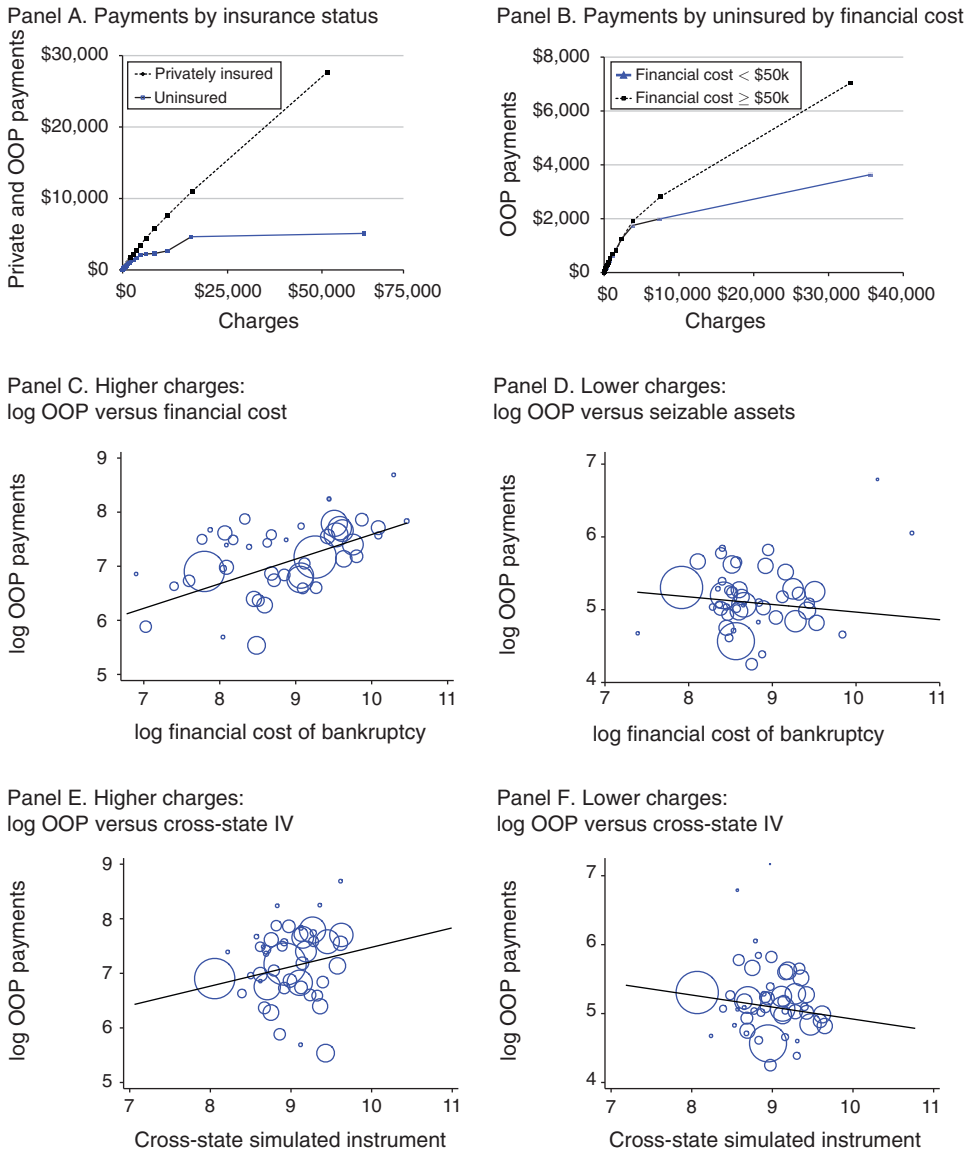
Panel A. Payments by insurance status

Panel B. Payments by uninsured by financial cost

Panel C. Higher charges:
log OOP versus financial cost

Panel D. Lower charges:
log OOP versus seizable assets

Panel E. Higher charges:
log OOP versus cross-state IV

Panel F. Lower charges:
log OOP versus cross-state IV

FIGURE 1. PLOTS OF THE EFFECT ON COSTS

Notes: Panel A shows payments against charges for privately insured and uninsured households. Payments are the sum of out-of-pocket payments and payments from private insurers. Panel B shows out-of-pocket payments against charges for uninsured households with a higher ($\geq$ \$50,000) and lower ($<$ \$50,000) financial cost of bankruptcy. Both plots are created by averaging payments and charges at twentieths of the charge distribution. Panels C and D plot log out-of-pocket payments against the log financial cost of bankruptcy averaged by state for households with higher ($\geq$ \$5,000) and lower ($<$ \$5,000) charges. Panels E and F plot log out-of-pocket payments against the cross-state simulated instrument averaged by state for households with higher ($\geq$ \$5,000) and lower ($<$ \$5,000) charges. The cross-state simulated instrument is the mean log financial cost of bankruptcy for the entire sample of households as though this sample faced the asset exemption laws of each state. The circles in panels C–F are proportional to the number of observations in each state. Pooled 1996–2005 MEPS, excluding households with public insurance or a member age 65 or older, inflation-adjusted to 2005 USD using the CPI-U.

and charges (x-axis) for households with private insurance and the uninsured. Charges are the list price of medical care and proxy for the level of medical utilization. Payments are the sum of out-of-pocket payments and payments from private insurance providers. (Payments by the uninsured are therefore simply out-of-pocket payments.) The plot was created by averaging payments and charges at twentieths of the charge distribution. Panel A shows that payments for the privately insured scale up proportionally with charges. The slope is approximately 60 percent reflecting the "negotiated discount" private insurers obtain off list prices. For households without coverage, payments scale up at the same rate to about $2,000 and then flatten out abruptly. Indeed, out-of-pocket payments made by the uninsured closely resemble those by an insured household with a high-deductible health plan.

Panel B examines how the relationship between out-of-pocket payments and charges varies across *uninsured* households with lower ($<$ \$50,000) and higher ($\geq$ \$50,000) financial costs of bankruptcy, and was similarly created by averaging payments and charges at twentieths of the charge distribution. The plot shows that for lower levels of medical utilization, all uninsured households make very similar out-of-pocket payments. For higher levels of utilization, out-of-pocket payments sharply diverge. Households with a lower financial cost of bankruptcy have their out-of-pocket payments truncated, whereas households with a higher financial cost have their out-of-pocket payments continue to increase with charges, albeit at a somewhat lower rate.

Panels C–F show the cross-state relationship between out-of-pocket payments (y-axis) and the financial cost of bankruptcy (x-axis) for uninsured households. To account for the high-deductible nature of this insurance, I split the sample into households with more or less than \$5,000 in annual medical charges. In these samples, mean out-of-pocket payments are \$1,268 and \$149, respectively. The data are averaged by state with circles proportional to the number of observations. Panels C and D show the raw correlation between log out-of-pockets payments and the log financial cost of bankruptcy. For households with higher charges (panel C), there is a robust upward-sloping relationship, consistent with bankruptcy as a form of high-deductible health insurance. For households with lower charges (panel D), the relationship is slightly downward sloping. Panels E and F show the graphical analogue to a reduced-form regression: log out-of-pocket payments against the cross-state simulated instrument. The reduced form paints a similar picture. There is a strong upward-sloping relationship for households with more than \$5,000 in annual charges (panel E) and a slightly downward-sloping relationship for households with lower charges (panel F).

Table 2 shows regression estimates of the effect on costs. Panels A and B show estimates from regressions of log out-of-pocket costs on the log financial cost of bankruptcy in the samples of uninsured households with more and less than \$5,000 in annual charges. Panel C examines extensive margin effects with linear probability model regressions of an indicator for positive charges on the log financial cost in the sample of all uninsured households. Odd-numbered columns show specifications that do not control for charges, and even-numbered columns show specifications that include a fourth-order polynomial in charges as a control. Controlling for charges is inappropriate if the amount of care provided is endogenous to the financial cost of bankruptcy. If charges are exogenous, then controlling for charges allows us to

TABLE 2—EFFECT ON COSTS: REGRESSIONS OF OUT-OF-POCKET COSTS ON THE FINANCIAL COST OF BANKRUPTCY

	OLS		Pooled IV		Within-state IV		Cross-state IV	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Dependent variable: log out-of-pocket payments								
Charges ≥ $5,000								
log financial cost	0.218***	0.189***	0.339**	0.335*	0.400***	0.405**	0.316	0.319
	(0.068)	(0.067)	(0.166)	(0.180)	(0.153)	(0.165)	(0.217)	(0.238)
Mean out-of-pocket payments	$1,268	$1,268	$1,268	$1,268	$1,268	$1,268	$1,268	$1,268
Panel B. Dependent variable: log out-of-pocket payments								
0 < Charges < $5,000								
log financial cost	0.135***	0.117***	−0.047	−0.140	0.041	−0.125	−0.096	−0.168
	(0.042)	(0.031)	(0.143)	(0.116)	(0.218)	(0.183)	(0.147)	(0.128)
Mean out-of-pocket payments	$149	$149	$149	$149	$149	$149	$149	$149
Panel C. Dependent variable: nonzero charge indicator								
All charges								
log financial cost	0.001		−0.003		−0.006		−0.002	
	(0.002)		(0.006)		(0.011)		(0.008)	
Probability of nonzero charge	0.761		0.761		0.761		0.761	
Instruments and controls: all panels								
Instrument								
Pooled simualted instrument			X	X	X	X		
Cross-state simulated instrument							X	X
Controls								
Charges		X		X		X		X
Demographic controls	X	X	X	X	X	X	X	X
Year fixed effects	X	X	X	X	X	X	X	X
State fixed effects					X	X		

Notes: Panel A shows estimates from regressions of log out-of-pocket payments on the log financial cost of bankruptcy in the sample of uninsured households with at least $5,000 in annual charges. Panel B shows estimates of the same regression in the sample of uninsured households with charges between $0 and $5,000 non-inclusive. Panel C shows estimates from regressions of an indicator for nonzero charges on the log financial cost of bankruptcy in the sample of all uninsured households. The cross-state simulated instrument is the mean log financial cost of bankruptcy for the entire sample of households as though this sample faced the asset exemption laws of each state. The pooled simulated instrument is similarly constructed by predetermined demographic group, where groups are defined by the full interaction of age group, race, education group, and family structure. Demographic controls are demographic-group dummies and a fourth-order polynomial in annual income. Charge polynomial is fourth-order as well. Samples sizes are 431; 2,002; and 3,201 across the three panels. Pooled 1996–2005 MEPS, excluding households with insurance or a member age 65 or older, inflation-adjusted to 2005 using the CPI-U. Robust standard errors clustered by state are in parentheses.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

interpret the coefficient of interest as an effect on the difference between the list price and out-of-pocket costs, sometimes referred to as the "discount" provided to the uninsured. All specifications include demographic controls, state controls, and year fixed effects. Moving from left to right, the table shows OLS specifications (columns 1 and 2) and two-stage least squares (2SLS) specifications (columns 3–8) that isolate the pooled, within-state, and cross-state variation in asset exemption law. Standard errors are clustered at the level of the instrument in all specifications.

Panel A provides evidence of an economically significant effect of the financial cost of bankruptcy on out-of-pocket costs for households with more than $5,000 in annual charges. The preferred pooled IV estimates (columns 3 and 4) indicate that a log increase in the financial cost raises out-of-pocket payments by 41 percent ($= \exp(0.34) - 1$) on a base of $1,268. The estimates are slightly lower ($0.23 = \exp(0.21) - 1$) in the OLS specifications and slightly higher in the specifications that use within-state variation ($0.49 = \exp(0.41) - 1$). The estimates are virtually identical with and without the charge controls. The cross-state estimates are similar to the preferred pooled IV estimates ($0.38 = \exp(0.32) - 1$) but less precisely estimated.

Panel B provides evidence of a flat if not slightly downward-sloping relationship between the financial cost of bankruptcy and out-of-pocket payments for households with less than $5,000 in annual medical charges. The preferred pooled IV estimates (columns 3 and 4) indicate that a log increase in financial cost reduces out-of-pocket payments by 5 percent ($= \exp(0.05) - 1$) to 15 percent ($= \exp(0.14) - 1$) on a base of $149. The standard errors are too large to rule out a nonzero effect. But even with greater precision, effects of this magnitude are unlikely to be economically significant given the low base level of spending. The within-state and cross-state IV estimates are similar to the preferred pooled IV specification and also statistically indistinguishable from zero. I interpret the positive OLS estimates as likely upward biased due to the potential correlation between unobserved household factors and the treatment and billing behavior of medical providers.

Panel C indicates no effect on the extensive margin. Using the pooled IV estimate (column 3), I can reject effects outside a -1.6–0.8 percentage point window on a base of 76.1 percent with a 95 percent confidence interval. The estimates are similar across specifications.

Online Appendix Table A6 replicates Table 2 with the dependent variable in levels instead of logs. The magnitudes are similar. For instance, the pooled IV specifications (columns 3 and 4) indicate that a log increase in the financial cost of bankruptcy raises out-of-pocket payments for households with more than $5,000 in annual charges by $552–$619 on a base of $1,268, or by 44–49 percent. The estimates are less precise due to the skewness of the out-of-pocket cost distribution.

To summarize, I find a strong positive relationship between the financial cost of bankruptcy and out-of-pocket payments for households with higher utilization, a slightly downward-sloping relationship for households with lower utilization, and zero effect on the extensive margin. Thus the impact of bankruptcy on financial risk is exactly what you would expect from a high-deductible health plan.[32,33]

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[32] There is reason to think the effect for households with higher utilization might be an underestimate of the long-run impact of bankruptcy insurance on exposure to financial risk. Medical providers sometimes allow households to make payments in multiple installments. The MEPS does a poor job capturing these payments as it only elicits out-of-pocket payments for medical events that occurred in the approximately five-month look-back period. If households with higher illiquid seizable assets (e.g., seizable home equity) are more likely to make installment payments, then out-of-pocket payments in the data would underestimate financial risk for higher-financial-cost households, making the estimated parameter a downward-biased measure of the effect of the insurance from bankruptcy on long-run exposure to financial risk.

[33] Although not statistically significant, the negative point estimates for lower-utilization households are consistent with a model in which medical providers partially offset the lower receipts from higher-charge households with more aggressive collections from households with lower charges. For instance, Gruber and Rodriguez (2007), using rich financial processing data for a group of physicians, provide evidence on cross-subsidization within the

C. *Effect on Coverage*

Having provided evidence that uninsured households with a higher financial cost of bankruptcy face greater financial risk, I now examine the crowd-out effects of this implicit insurance. Figure 2 presents visual evidence, plotting insurance coverage (*y*-axis) against the financial cost of bankruptcy (*x*-axis). The data are averaged by state with circles proportional to the number of observations. Plots in the left column use data from the SIPP; plots in the right column use data from the PSID. The top row (panels A and B) shows the raw data: insurance coverage against the log financial cost of bankruptcy. The bottom row (panels C and D) shows the analog to a reduced-form regression: insurance coverage against the cross-state simulated instrument. The plots confirm the crowd-out prediction: insurance coverage is substantially higher for households with more wealth at risk and lower for households with limited financial exposure. The outliers are predominately states with relatively few observations.

Table 3 presents the main coverage estimates. Panel A shows estimates in the SIPP; panel B shows estimates in the PSID; and panel C shows estimates in the MEPS. Column 1 shows the marginal effect calculated at the mean from a non-IV probit regression; column 2 adds state fixed effects to this specification; columns 3–5 show marginal effects from IV probit regressions that isolate the pooled, within-in-state, and cross-state variation in asset exemption law. All specifications include demographic controls, state controls, and year fixed effects.[34] Block bootstrap standard errors calculated using 200 draws clustered at the level of the instrument are shown in parentheses.

In the SIPP and PSID, the preferred pooled IV estimates (column 3) indicate that a log increase in the financial cost of bankruptcy raises insurance coverage by 2.5–3.6 percentage points on a base of approximately 80 percent. Both estimates are significantly different from zero at the 1 percent level. The within- and cross-state estimates are similar, ranging from 1.7–4.6 percentage points, and the non-IV estimates range from 1.8–2.4 percentage points. In the MEPS, the non-IV estimates are a similar 2.3–2.6 percentage points, while the IV estimates are a larger 3.8–6.0 percentage points. One explanation for the larger IV estimates is the smaller first stage. Indeed, since the first stage in the MEPS is one-fourth smaller (see Table 1), this difference by itself would imply one-third larger IV estimates. Because the MEPS estimates are moderate outliers, I refer to the pooled SIPP and PSID results as my preferred estimates of the effect on coverage.

Online Appendix Section C examines the sensitivity of the effect on coverage. I show the results are similar when I control for "conditional access" to Medicaid and when I use a two-sample IV strategy that combines data on health insurance from

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uninsured. In this context, the offset is far from complete. Combining the estimates for high- and low-utilization households, the pooled IV estimates imply that a log increase in the financial cost of bankruptcy raises out-of-pocket payments on net by 21–24 percent.

[34] The demographic controls are demographic-group dummies (full interaction of age group, race, education group, and family structure) and a fourth-order polynomial in annual income. State controls are for individual market insurance regulations from Kowalski, Congdon, and Showalter (2008) (count of mandated benefits and indicators for any wiling pharmacist, any willing provider, community rating, and guaranteed issue regulations), hospital ownership structure (nonprofit share of beds, for-profit share of beds), Disproportionate Share Hospital (DSH) payments per capita, Federally Qualified Health Centers (FQHC) per capita, and the presence of a charity-care pool or fund.
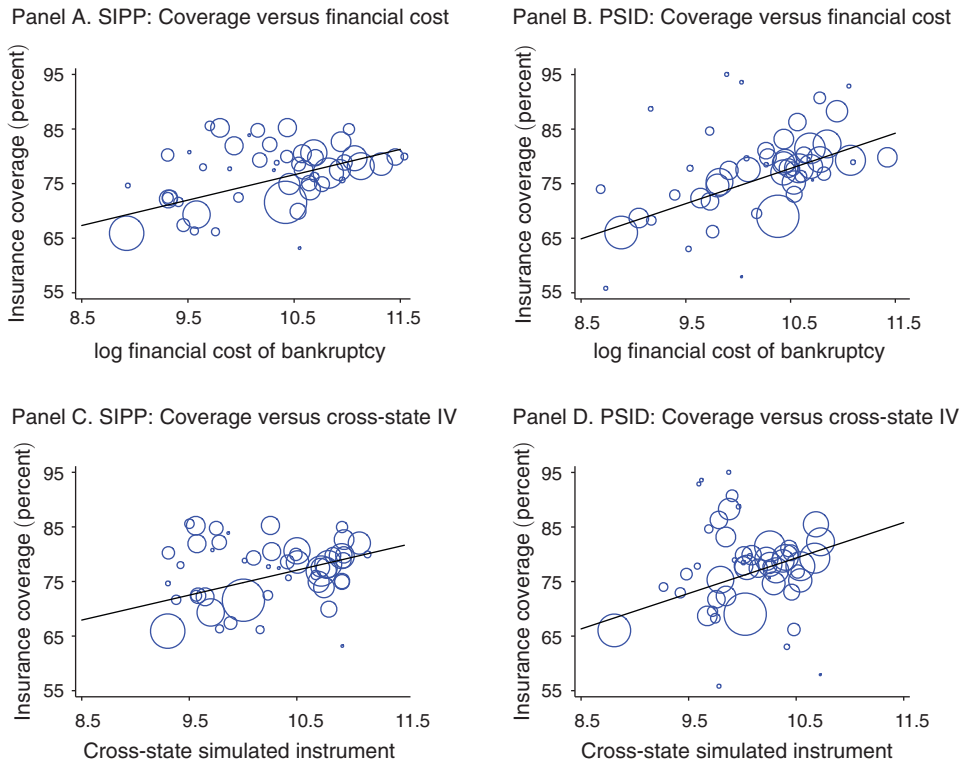
Panel A. SIPP: Coverage versus financial cost

Panel B. PSID: Coverage versus financial cost

Panel C. SIPP: Coverage versus cross-state IV

Panel D. PSID: Coverage versus cross-state IV

FIGURE 2. PLOTS OF THE EFFECT ON COVERAGE

Notes: Panel A plots insurance coverage against the log financial cost of bankruptcy averaged by state using data from the SIPP. Panel B shows the same plot using data from the PSID. Panel C plots the reduced form: insurance coverage against the cross-state simulated instrument averaged by state using data from the SIPP. Panel D shows the same plot using data from the PSID. The cross-state simulated instrument is the mean log financial cost of bankruptcy for the entire sample of households as though this sample faced the asset exemption laws of each state. The circles in each plot are proportional to the number of observations in each state. Pooled 1996–2005 SIPP and 1999–2005 PSID, excluding households with public insurance or a member age 65 or older, inflation-adjusted to 2005 using the CPI-U.

the CPS and data on wealth from the SIPP. I also show specifications that address concerns that arise from the use of panel data that pools multiple observations for a given household over time. The bottom line is that the effect on coverage is stable across a broad range of alternative specifications.

To put the magnitude of the estimates in context, I conduct the counterfactuals of applying nationwide the exemption laws of the most and least debtor-friendly states. I stress that these counterfactuals are intended for illustrative purposes only. Changing asset exemption laws could have broad consequences for credit markets that are not examined in this paper, and as I discuss below, policies that increase insurance coverage may not necessarily be welfare enhancing. Texas has the most debtor-friendly asset exemption laws, allowing households to exempt the full value of their homestead and take a $60,000 wildcard exemption. If these laws were applied nationally, the log financial cost of bankruptcy would decline by 1.2 on average, and the preferred pooled IV estimates indicate that the fraction of uninsured households would rise by 2.4–4.2 percentage points, or approximately 12.5–21.0 percent.

TABLE 3—EFFECT ON COVERAGE:
REGRESSIONS OF INSURANCE COVERAGE ON THE FINANCIAL COST OF BANKRUPTCY

	Insurance coverage				
	Non-IV		Pooled IV	Within-state IV	Cross-state IV
	(1)	(2)	(3)	(4)	(5)
Panel A. SIPP					
log financial cost	0.018***	0.018***	0.025***	0.017**	0.027**
	(0.002)	(0.002)	(0.006)	(0.009)	(0.013)
Panel B. PSID					
log financial cost	0.024***	0.023***	0.036***	0.023	0.046**
	(0.002)	(0.002)	(0.015)	(0.034)	(0.022)
Panel C. MEPS					
log financial cost	0.026***	0.023***	0.060***	0.038***	0.058***
	(0.001)	(0.001)	(0.006)	(0.011)	(0.019)
Instrument and controls: all panels					
Controls					
Demographic controls	X	X	X	X	X
State controls	X	X	X	X	X
Year fixed effects	X	X	X	X	X
State fixed effects		X		X	
Instrument					
Pooled simulated instrument			X	X	
Cross-state simulated instrument					X

Notes: Table shows marginal effects calculated at the mean of a log increase in the financial cost of bankruptcy on insurance coverage from non-IV and IV probit regressions. The cross-state simulated instrument is the mean log financial cost of bankruptcy for the entire sample of households as though this sample faced the asset exemption laws of each state. The within-state simulated instrument is similarly constructed by predetermined demographic group, where groups are defined by the full interaction of age group, race, education group, and family structure. Demographic controls are demographic-group dummies and a fourth-order polynomial in annual income. State controls are for individual market insurance regulations (see text for details), hospital ownership structure, DSH payments and FQHC per capita, and the presence of a charity-care pool or fund. Pooled 1996–2005 SIPP, 1999–2005 PSID, and 1996–2005 MEPS, excluding households with public insurance or a member age 65 or older, inflation-adjusted to 2005 using the CPI-U. Sample size is 1,251,907 and mean insurance coverage is 80.6 percent in the SIPP. Sample size is 20,774 and mean insurance coverage is 83.6 percent in the PSID. Sample size is 61,405 and mean insurance coverage is 81.8 percent in the MEPS. Block bootstrap standard errors calculated using 200 draws clustered at the level of the instrument are in parentheses.

  ***Significant at the 1 percent level.
  **Significant at the 5 percent level.
  *Significant at the 10 percent level.

Delaware has the least debtor-friendly laws, with no homestead exemption and a $500 wildcard exemption. Applying Delaware asset exemptions nationwide would increase the log financial cost of bankruptcy by approximately 0.6, and reduce the fraction of uninsured households by 1.7–1.9 percentage points, or approximately 8.5–9.5 percent. This magnitude is economically significant. To see this, consider the premium subsidy required to increase coverage by the same 1.7–1.9 percentage points. A central estimate of the premium semi-elasticity of insurance take-up is −0.084 (Congressional Budget Office 2005).[35] Using this estimate, inducing the same increase in coverage requires a premium subsidy of 20.3–22.5 percent.

[35] The Congressional Budget Office (2005) estimate is identified off premium variation due to state-level community rating and premium compression regulations. The estimate is central to the small number of estimates in the

D. *Falsification Tests*

I further explore the exogeneity of the identifying variation by conducting falsification tests using pensions, income, and wages as dependent variables. The motivation for these tests is the concern that a third variable—such as household risk preferences or employee bargaining weights—might be simultaneously determining asset exemption laws and the availability of health insurance. Since this type of factor might also affect pensions, income, and wages, a finding that asset exemption laws predicts higher values for these outcomes would raise concerns about the validity of the identification strategy.[36]

To conduct the falsification tests, I estimate the baseline coverage specifications (equation (4)) with pensions, income, or wages as the dependent variable. That is, I estimate 2SLS regressions of the dependent variable on the log financial cost of bankruptcy, using the standard instrumental variables to isolate the pooled, within-, and cross-state variation. I define the pensions dependent variable as an indicator for whether the household head has a pension or tax-deferred savings account and the wage and income dependent variables as the natural logarithm of annual household wages and income. The controls are identical to those in the coverage regressions (Table 3) except that the wage and income regressions exclude the fourth-order polynomial in income.

Table 4 shows the results of these falsification tests. I show both the parameter estimates and the estimates normalized by the standard deviation of the dependent variable, which can be more easily compared across specifications. As a point of reference, the effect on coverage normalized by the standard deviation of the dependent variable is $0.059 \ (= 0.025/0.421)$ in the SIPP and $0.086 \ (= 0.036/0.419)$ in the PSID. Table 4 shows no systematic correlation between the financial cost of bankruptcy and pensions, income, or wages. The effect on pensions is statistically and economically insignificant. The wage and income effects, taken together, show no correlation. If anything, the pooled IV specification in the SIPP points to a slightly negative relationship, although the effect is not replicated in the PSID or the other SIPP specifications.

V. Additional Evidence on the Effect on Coverage

The estimates in the previous section rely on the exogeneity of the cross- and within-state variation in the financial cost of bankruptcy. A potential concern with this identification strategy is unobserved state-level factors that are correlated with the instruments and health insurance coverage. In this section, I present evidence on the effect on coverage using difference-in-differences variation from the 2005

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literature. It is smaller than the estimate of Gruber and Poterba (1994), who use the introduction of a tax subsidy for insurance purchases by the self-employed and is larger than the estimate from Marquis and Long (1995). See Liu and Chollet (2006) for a review of estimates in the literature.

[36] The theory of compensating differentials suggests that the financial cost of bankruptcy and non-health insurance compensation should be negatively correlated. If a high financial cost of bankruptcy increases take-up of employer-sponsored health insurance, then employers should offset these costs with lower compensation along other dimensions, potentially reducing pensions, wages, and income.

Table 4—Falsification Tests: Regressions of Pensions, Income, and Wages on the Financial Cost of Bankruptcy

	Dependent variable: pension plan			Dependent variable: log wage income		
	Pooled IV (1)	Within-state IV (2)	Cross-state IV (3)	Pooled IV (4)	Within-state IV (5)	Cross-state IV (6)
Panel A. SIPP						
log financial cost	0.006 (0.007)	0.024 (0.015)	0.003 (0.014)	−0.065** (0.030)	0.011 (0.067)	−0.077 (0.058)
Normalized by SD of dependent variable	0.012	0.048	0.006	−0.020	0.003	−0.024
Panel B. PSID						
log financial cost	−0.013 (0.015)	−0.011 (0.029)	−0.010 (0.013)	−0.022 (0.068)	0.134 (0.136)	−0.058 (0.060)
Normalized by SD of dependent variable	−0.026	−0.022	−0.020	−0.005	0.028	−0.012
Instrument and controls: all panels						
Controls						
Demographic controls	X	X	X	X	X	X
State controls	X	X	X	X	X	X
Year fixed effects	X	X	X	X	X	X
State fixed effects		X			X	
Instrument						
Pooled simulated instrument	X	X		X	X	
Cross-state simulated instrument			X			X

(*Continued*)

Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA) and the 1986 Emergency Medical Treatment and Labor Act (EMTALA).

A. *BAPCPA*

As discussed in Section I, BAPCPA reduced the generosity of the bankruptcy code by restricting Chapter 7 to households that passed either a means or repayment test. For households that passed one of these tests, the financial cost of bankruptcy was the same before and after BAPCPA. For households that did not pass, the pre-BAPCPA variation in Chapter 7 asset exemptions created difference-in-differences variation, with households with relatively more generous pre-BAPCPA Chapter 7 asset exemptions experiencing larger increases in the financial cost of bankruptcy from the requirement to file under Chapter 13.

I exploit this variation by estimating versions of the baseline coverage specification that isolate differences-in-differences variation in the financial cost of bankruptcy, generated by applying the appropriate pre- or post-BAPCPA financial cost of bankruptcy formula. The identifying assumption is that changes in the financial cost of bankruptcy are uncorrelated with changes in the error over time. The assumption would be violated if, for example, states that experienced larger shifts in the financial cost of bankruptcy also experienced larger shifts in coverage for unobserved reasons. I address this concern by showing the estimates are stable to the inclusion of state-specific time trends.

TABLE 4—FALSIFICATION TESTS: REGRESSIONS OF PENSIONS, INCOME,
AND WAGES ON THE FINANCIAL COST OF BANKRUPTCY (*Continued*)

	Dependent variable: log total income		
	Pooled IV (7)	Within-state IV (8)	Cross-state IV (9)
Panel C. SIPP			
log financial cost	−0.047	−0.013	−0.053
	(0.016)	(0.035)	(0.049)
Normalized by SD of dependent variable	−0.027	−0.007	−0.030
Panel D. PSID			
log financial cost	−0.039	0.168	−0.105
	(0.043)	(0.093)	(0.060)
Normalized by SD of dependent variable	−0.033	0.140	−0.088
Instrument and controls: all panels			
Controls			
Demographic controls	X	X	X
State controls	X	X	X
Year fixed effects	X	X	X
State fixed effects		X	
Instrument			
Pooled simulated instrument	X	X	
Cross-state simulated instrument			X

Notes: Table shows estimates of the effect of the financial cost of bankruptcy on pensions, wages, and income from IV regressions. Pensions is an indicator for whether the household head has a pension or tax-deferred savings account; log wage income and log total income are annualized household-level values. The cross-state simulated instrument is the mean log financial cost of bankruptcy for the entire sample of households as though this sample faced the asset exemption laws of each state. The pooled simulated instrument is similarly constructed by predetermined demographic group, where groups are defined by the full interaction of age group, race, education group, and family structure. Demographic controls are demographic-group dummies. The pensions specifications include a fourth-order polynomial in annual income. State controls are for individual market insurance regulations (see text for details), hospital ownership structure, DSH payments and FQHC per capita, and the presence of a charity-care pool or fund. Pooled 1996–2005 SIPP and 1999–2005 PSID, excluding households with public insurance or a member age 65 or older, inflation-adjusted to 2005 using the CPI-U. Sample size is 1,251,907 in the SIPP and 20,774 in the PSID. Standard errors clustered at the level of the instrument are in parentheses.

  ***Significant at the 1 percent level.
   **Significant at the 5 percent level.
    *Significant at the 10 percent level.

Although this specification exploits difference-in-differences variation in the financial cost of bankruptcy, it does not isolate variation solely due to BAPCPA. For example, if the financial cost of bankruptcy increases because of BAPCPA and macroeconomic trends, this specification would capture the effect of both of these factors. To isolate the variation due to BAPCPA, I follow the approach discussed in Section III and construct simulated instruments. I construct a *cross-state × BAPCPA* instrument by taking the entire sample of households and calculating their mean financial cost of bankruptcy separately under the pre- and post-BAPCPA laws in each state. I construct a *pooled × BAPCPA* instrument that captures state × demographic group variation over time by dividing the sample into $k = 1, \ldots, K$ demographic groups based on the standard predetermined household characteristics and calculating the mean financial cost of bankruptcy for each of these groups under the pre- and post-BAPCPA laws in each state.

Online Appendix Table A15 presents summary statistics on the change in the financial cost of bankruptcy using data from the 1996–2011 SIPP. The first row shows the household-level change in the log financial cost, constructed by calculating

the financial cost of bankruptcy for each household in its actual state of residence under pre- and post-BAPCPA laws. BAPCPA increased the log financial cost of bankruptcy by approximately 0.5, with a standard deviation of 1.16, indicating significant heterogeneity. The second and third rows show the change in the pooled and state-level instruments. The change in the pooled simulated instrument captures about one-third of the household-level variation (standard deviation of 0.32) and the state-level instrument captures about one-quarter of the household-level change (standard deviation of 0.24).

Online Appendix Table A15 presents the first-stage estimates of the log financial cost of bankruptcy on the simulated instruments, state and year fixed effects, and additional demographic controls. Column 1 shows the coefficient on the pooled × BAPCPA simulated instrument; column 2 adds state-specific time trends as an additional control; column 3 shows the coefficient on the instrument that captures difference-in-differences variation at the state level. Across specifications, the first stage has substantial power. The coefficient on the instrument is slightly greater than 1, indicating a modest positive correlation between changes in asset holdings and the BAPCPA-induced legal variation.

Table A16 presents the main estimates. Column 1 shows the marginal effect calculated at the mean from a probit regression of health insurance on the financial cost of bankruptcy that isolates difference-in-differences variation by including state and year fixed effects, along with the standard demographic controls. Column 2 adds state-specific time trends to this specification. Columns 3 and 4 repeat the specifications in columns 1 and 2, instrumenting with the pooled × BAPCPA simulated instrument to isolate variation due to BAPCPA. Column 5 repeats specification 1 using the cross-state × BAPCPA simulated instrument.

The non-IV estimates indicate that a unit increase in the log financial cost of bankruptcy raises health insurance coverage by 1.2 percentage points. The effects are statistically distinguishable from zero at the 1 percent level and similar to the baseline non-IV estimates of 1.8 percentage points (columns 1 and 2 of Table 3). The IV estimates range from 0.8–1.8 percentage points, and two of the three estimates are statistically significant at the 1 percent level. The estimates are somewhat smaller than the baseline IV estimates, which range from 1.7–2.7 percentage points (columns 3 to 5 of Table 3), although I cannot reject the null hypothesis that the estimates are identical.[37]

B. *EMTALA*

As discussed in Section I, EMTALA required hospitals to provide emergency care on credit and prohibited them from delaying treatment to inquire about insurance status or means of payment. Before EMTALA, households with a relatively low financial cost of bankruptcy had only moderately more generous implicit health insurance than households with a higher financial cost, because some hospitals

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[37] In particular, the pooled IV estimate in column 3 of Table 3 is statistically indistinguishable from the pooled × BAPCPA estimate in column 3 of Table 5; the within-state IV estimate in column 4 of Table 3 is statistically indistinguishable from the pooled × BAPCPA estimate with state-specific time trends in column 4 of Table 5; and the cross-state IV estimate in column 5 of Table 3 is statistically indistinguishable from the cross-state × BAPCPA estimate in column 5 of Table 5.

TABLE 5—EFFECT ON COVERAGE: USING VARIATION FROM BAPCPA

| | Insurance coverage | | | | |
| | Non-IV | | IV | | |
	(1)	(2)	(3)	(4)	(5)
log financial cost	0.012***	0.012***	0.017***	0.018***	0.008
	(0.001)	(0.001)	(0.005)	(0.005)	(0.007)
Controls					
Demographic controls	X	X	X	X	X
Year fixed effects	X	X	X	X	X
State fixed effects	X	X	X	X	X
State-specific time trends		X		X	
Instrument					
Pooled × BAPCPA simulated instrument			X	X	
Cross-state × BAPCPA simulated instrument					X

Notes: Table shows marginal effects calculated at the mean of an increase in the financial cost of bankruptcy on insurance coverage. Columns 1 and 2 show effects from non-IV specifications that isolate difference-in-differences variation in the financial cost of bankruptcy. Columns 3–5 show effects from specifications that use instruments to further isolate difference-in-differences variation solely due to BAPCPA. The cross-state × BAPCPA simulated instrument is constructed by calculating the mean log financial cost of bankruptcy for the entire sample of households under the pre- and post-BAPCPA laws of each state. The pooled × BAPCPA simulated instrument is similarly constructed by predetermined demographic group, where groups are defined by the full interaction of age group, race, education group, and family structure. Demographic controls are demographic-group dummies and a fourth-order polynomial in annual income. Pooled 1996–2011 SIPP, excluding households with public insurance or a member age 65 or older, inflation-adjusted to 2005 using the CPI-U. Robust standard errors clustered at the level of the instrument are in parentheses.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

might refuse to provide them with medical care. After EMTALA, these households should have seen the generosity of their implicit insurance differentially increase, since they were now guaranteed to receive care and have the costs discharged.

I examine this prediction using data from the 1979–2005 CPS. The CPS is the only dataset I am aware of with state-level information on insurance coverage over this time period. Because the CPS does not include detailed information on wealth, I cannot estimate instrumental variables specifications with these data. Instead, I merge the CPS with the instruments from the 1996–2005 SIPP and estimate reduced-form specifications of the effect on coverage.[38]

Figure 3 shows estimates of the effect on health insurance coverage where I allow the coefficient of interest to vary by year. In particular, the plot shows marginal effects calculated at the mean from a probit regression of health insurance coverage on the pooled simulated instrument, state and year fixed effects, and the standard demographic controls. Standard errors are clustered by state. The figure shows a

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[38] Recall that the instrument is constructed using data pooled across years and therefore does capture time-series variation in assets. Therefore, after inflation adjustment, merging the instrument with the CPS data from earlier years does not raise particularly troubling issues. In online Appendix C, I restrict the sample to the baseline 1996–2005 period and show that two-sample IV estimates of the effect on coverage in the merged CPS-SIPP data are similar to the baseline estimates from Table 3.
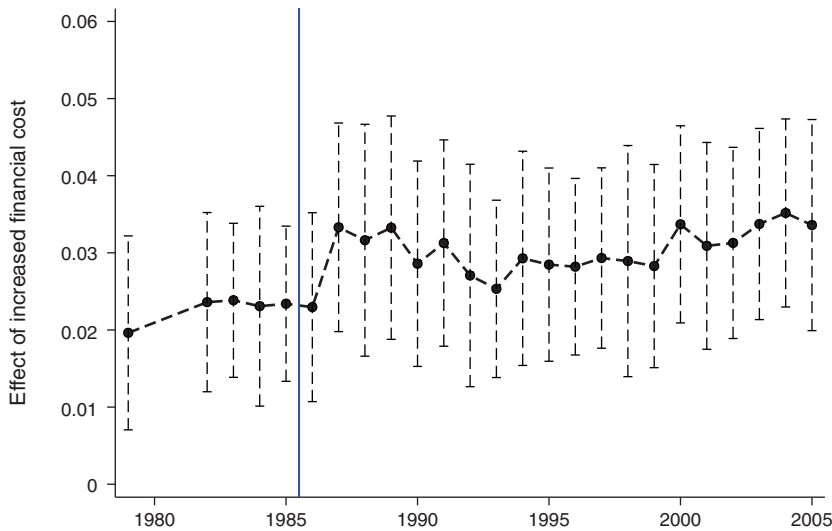
FIGURE 3. EMTALA AND THE EFFECT ON COVERAGE

Notes: Figure shows marginal effects calculated at the mean from a reduced-form probit regression of health insurance on the pooled simulated instrument, year and state fixed effects, and demographic controls, where the coefficient on the instrument is allowed to vary by year. The dashed vertical lines show 95 percent confidence intervals, calculated using standard errors clustered by state. The solid vertical line denotes the 1986 implementation of EMTALA. The pooled simulated instrument is the mean log financial cost of bankruptcy for a nationally representative sample of households in each predetermined demographic group as though they lived in each state. Demographic groups are defined by the full interaction of age group, race, education group, and family structure. Demographic controls are demographic-group dummies and a fourth-order polynomial in annual income. Health insurance and demographics are from the 1980–2006 CPS and are lagged by one year because questions ask about health insurance coverage in the previous year. The pooled simulated instrument is from the 1996–2005 SIPP. Both samples exclude households with public insurance or a member age 65 or older, and monetary values are inflation-adjusted to 2005 using the CPI-U.The years 1980 and 1981 are missing because the CPS did not ask about health insurance coverage in these years.

sharp, persistent increase in the effect following EMTALA, with the coefficient of interest rising by approximately 1 percentage point on a base of 2 percent.

To increase precision, I also show estimates from reduced-form difference-in-differences specifications with a single post-EMTALA coefficient. Letting y_{ijt} be an outcome for household i in state j in year t, I estimate regressions of the form

$$(5) \qquad y_{ijt} = \delta_z z_{jk} + \delta_{Post} z_{jk} \times Post_t + \delta_j + \delta_t + \delta_k + f(x_{it}, \delta_x) + \epsilon_{ijt},$$

where z_{jk} is the pooled simulated instrument, $Post_t$ is an indicator for the post-EMTALA period, δ_j and δ_t are state and year fixed effects, δ_k are demographic group fixed effects, $f(x_{it}, \delta_x)$ is a fourth-order polynomial in household income, and ϵ_{ijt} is the error term. The coefficient on the $z_{kj} \times Post_t$ interaction captures the incremental effect of EMTALA. Standard errors are clustered by state.

Table 6 shows marginal effects from reduced-form probit regressions that isolate the pooled, within-state, and cross-state variation in bankruptcy law. Column 1 shows that EMTALA increased the coefficient on the pooled simulated instrument by just under 1 percentage point. Column 2 shows this effect is stable to the inclusion of state fixed effects. A standard concern with difference-in-differences

TABLE 6—EFFECT ON COVERAGE: USING VARIATION FROM EMTALA

	Insurance coverage			
	Pooled IV (1)	Within-state IV		Cross-state IV (4)
		(2)	(3)	
Pooled simulated instrument × post EMTALA	0.009*** (0.001)	0.008*** (0.001)	0.010*** (0.001)	
Pooled simulated instrument	0.022*** (0.002)	0.008*** (0.004)	0.007* (0.004)	
Cross-state simulated instrument × post EMTALA				0.002 (0.002)
Cross-state simulated instrument				0.026*** (0.005)
Controls				
Demographic controls	X	X	X	X
Year fixed effects	X	X	X	X
State fixed effects		X	X	
State-specific time trends			X	
Observations	1,005,427	1,005,427	1,005,427	1,005,427

Notes: Table shows marginal effects calculated at the mean from reduced-form probit regressions of health insurance on the simulated instrument interacted with a post-EMTAA indicator and controls. The post-EMTALA period is the years after 1986. The cross-state simulated instrument is the mean log financial cost of bankruptcy for the entire sample of households as though this sample faced the asset exemption laws of each state. The pooled simulated instrument is similarly constructed by predetermined demographic group, where groups are defined by the full interaction of age group, race, education group, and family structure. Demographic controls are demographic-group dummies and a fourth-order polynomial in annual income. Health insurance and demographics are from the 1980–2006 CPS and are lagged because questions ask about coverage in the previous year. Simulated instruments are from the 1996–2005 SIPP. Both samples exclude households with public insurance or a member age 65 or older; monetary values are inflation-adjusted to 2005 using the CPI-U. Standard errors clustered at the level of the instrument are in parentheses.

   ***Significant at the 1 percent level.
    **Significant at the 5 percent level.
     *Significant at the 10 percent level.

research designs is the parallel-trends assumption. Figure 3 shows little evidence of a preexisting trend. Column 3 confirms this finding by adding state-specific time trends to the specification shown in column 2. Column 4 isolates the cross-state variation and has an effect that is smaller, and statistically indistinguishable from zero. All other estimates are statistically significant at the 1 percent level.

To put this estimate in context, a comparison the effect of EMTALA to the baseline coverage effect is useful. EMTALA increased the coefficient on the financial cost of bankruptcy by 1 percentage point or approximately one-third of 2.5–3.6 percentage point baseline coverage effect. This suggests the legal requirement that hospitals provide care on credit is quantitatively important but is not a necessary condition for households to receive implicit insurance from bankruptcy. This is consistent with hospitals providing some medical care on credit even before EMTALA.

The main estimates of the effect on coverage were subject to the concern that unobserved state-level factors might be biasing the results. This section addresses this concern by using difference-in-differences variation from BAPCPA and EMTALA. The effect on an increase in the financial cost of bankruptcy is similar when estimated using difference-in-differences variation from BAPCPA. Consistent with the theory

of bankruptcy as health insurance, the estimates indicate that EMTALA increased the coefficient on the financial-cost instruments.

VI. Health Insurance Mandates

While the implicit insurance from bankruptcy protects uninsured households from financial risk, the social cost of this insurance is borne by other users of the medical system. Forgoing health insurance therefore imposes an externality on others, and health insurance mandates can be motivated as "Pigovian penalties" that expose households to the full social cost of this decision. I construct a microsimulation model to quantify the socially optimal mandates. My simulation abstracts from other factors that influence health insurance coverage. I exclude households with public insurance or conditional access to Medicaid, and do not model tax exemptions for employer-sponsored insurance or the premium subsidies or Medicaid expansions under the ACA, not because these policies are unimportant, but because Medicaid expansions and means-tested subsidies are typically motivated on redistributive grounds and not because of externalities.[39]

A. Model

The microsimulation model has households and medical providers. Households have a representative agent with expected utility preferences over wealth w. They face medical shocks with list price m drawn from a distribution F with weakly positive support and choose whether to purchase health insurance to protect against this financial risk. Medical providers are obligated to provide medical services m and then attempt to recover the costs. I assume that household wealth is common knowledge and that medical providers face a small (possibly nonpecuniary) cost to pushing households into formal bankruptcy.

Model timing proceeds as follows: (i) households decide whether to purchase health insurance, (ii) households receive medical shock m, (iii) medical providers submit bill s, and (iv) households decide whether to declare bankruptcy. I solve the model in reverse order. Conditional on receiving medical bill s, households either do not declare bankruptcy (yielding wealth $w - s$) or declare bankruptcy (yielding wealth $w - w^S$). Maximizing wealth, households declare bankruptcy if and only if $s > w^S$.[40] Conditional on a medical shock with list price m, medical providers submit a bill $s \leq m$ to households. With a cost of pushing households into bankruptcy, the optimal bill is given by $s^* = \min\{m, w^S\}$, which is simply the cost m truncated by the financial cost of bankruptcy w^S.

This simple model captures the main empirical findings. Out-of-pocket payments are increasing in the financial cost of bankruptcy, with an effect that is concentrated among households with higher medical charges where the cap on out-of-pocket

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[39] I also do not model public programs that provide health insurance to individuals with high, ongoing medical costs, such as the Medicare Medically Needy program and non-aged Medicare eligibility through Social Security Disability Insurance.

[40] Fay, Hurst, and White (2002) find empirical support for this strategic model of bankruptcy in contrast to a non-strategic model where households file due to unanticipated adverse events.

spending is more likely to bind. Holding wealth constant, insurance coverage is increasing in the financial cost of bankruptcy. To see this, consider a stylized health insurance contract with deductible $\bar{m}$ and no other features. Under this contract, households are exposed to medical costs up to deductible $\bar{m}$ and insured above this level.[41] Under bankruptcy, households are exposed to medical costs up to the financial cost of bankruptcy w^S and insured above this amount. Thus, conventional and bankruptcy insurance are substitutes, and a higher financial cost make makes conventional insurance more valuable.

The coverage prediction is robust to natural extensions of the model. For example, allowing insured households to receive more or better medical treatment (Doyle 2005) increases the incentive to purchase coverage, but households with fewer seizable assets are still *relatively* less likely to insure. Similarly, increasing the cost of bankruptcy to account for factors such as stigma (Gross and Souleles 2002) or reduced access to credit (Musto 1999) does not affect the basic prediction. Finally, extending the model to endogenize the financial costs of bankruptcy would strengthen this relationship because households that choose to forgo coverage have an additional incentive to reduce their financial cost of filing.

B. *Simulation*

The microsimulation model is based on the sample of households in the 2005 PSID, excluding households with public insurance, conditional access to Medicaid, or a member age 65 or older. I assume each household is represented by a single member with constant relative risk-aversion (CARA) utility and show results for different degrees of risk aversion.[42] I construct the financial cost of bankruptcy for each of these households using the formula in Section II and construct medical cost distributions using realized cost distributions for households with the same demographic factors in the 2005 MEPS. I simulate the model by separately calculating a willingness to pay (WTP) and premium for each household and assign insurance coverage to households with WTP greater than the premium.[43] See online Appendix Section D for more details.

C. *Results*

Table 7 shows the welfare effects of the optimal Pigovian and ACA penalties. For each penalty system, I allow households to choose between conventional insurance at the simulated premiums and bankruptcy insurance at the cost of the penalty. I show results when the model is calibrated to low, moderate, and high levels of risk aversion which generate baseline coverage rates of 63, 81, and 88 percent, respectively.

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[41] In practice, health insurers negotiate discounts off medical charges. However, as shown in panel A of Figure 1, uninsured households seem to receive these discounts as well. To account for discounts in the model, one could replace *m* with discounted costs with no impact on the predictions.

[42] Using a CARA specification avoids the problems associated with nonpositive wealth. Calibrations with CRRA utility and a consumption floor generate stronger results.

[43] Implicit in this formulation is the assumption that households with employer-sponsored insurance pay for this coverage with a wage offset. Summarizing the empirical literature, Gruber (2000) concludes that the costs of health care are fully shifted to wages on average, justifying this approach.

Table 7—Microsimulation: Welfare Effects of Health Insurance Mandates

	Penalty	Take-up percent	Δ WTP	Δ Cost	Δ Surplus
Panel A. Pigovian penalty					
Low risk aversion	$333.92	95.0	$364.23	$306.64	$57.60
Moderate risk aversion	$333.92	95.9	$244.09	$131.11	$112.99
High risk aversion	$333.92	90.3	$171.89	$81.95	$89.94
Panel B. ACA penalty					
Low risk aversion	$460.36	35.2	$164.55	$136.91	$27.64
Moderate risk aversion	$460.36	40.6	$94.82	$50.17	$44.66
High risk aversion	$460.36	43.4	$89.33	$39.11	$50.21

Notes: Microsimulation estimates of insurance take-up, willingness to pay (WTP), costs, and social surplus from Pigovian and ACA penalties relative to a baseline in which households can choose bankruptcy at no cost. The Pigovian penalty is the household-specific social cost of the implicit insurance from bankruptcy. The ACA penalty is the inflation-adjusted, fully phased-in penalty under the ACA, defined as the greater of $625 or 2.5 percent of income, up to a maximum of $2,085 per household. Take-up is the percentage of uninsured individuals that take up coverage. WTP is calculated using CARA utility with parameters of 2.5×10^{-5} (low risk aversion), 5.0×10^{-5} (moderate risk aversion), and 7.5×10^{-5} (high risk aversion). Baseline insurance coverage rates with these risk-aversion parameters are 63, 81, and 88 percent, respectively. Microsimulation based on the financial cost of bankruptcy in the 2005 PSID and distributions of medical costs in the 2005 MEPS. Sample excludes households with public insurance, conditional access to Medicaid, or a member age 65 or older. Household-level estimates weighted to be nationally representative at the individual level.

Actual insurance coverage approximately 80 percent in the sample. The results are shown relative to a baseline in which households can choose bankruptcy at no cost.

Panel A shows coverage and welfare under the optimal Pigovian penalties, defined as expected costs for each household in excess of the financial cost of bankruptcy. The optimal penalties average $334 per person but exhibit substantial heterogeneity. Across risk-aversion levels, the penalties induce more than 90 percent of the uninsured to take up coverage. As indicated by the higher WTP and higher costs, these households purchase more generous coverage than they had from bankruptcy. The net effect is an increase in surplus of $58–$113 per person. The effects are nonmonotonic with risk aversion because this parameter affects both the baseline coverage rate and the responsiveness of households to the penalty.

Panel B of Table 7 shows the welfare effects of the ACA penalties. When fully implemented in 2016, these penalties will equal the greater of $625 or 2.5 percent of income per household, up to a maximum of $2,085. Deflated to 2005 levels, assuming trend inflation, the ACA penalties average $460 per person, about one-third larger than the optimal Pigovian penalties of $334 per person. Under these penalties, take-up ranges from 35–43 percent. WTP and costs rise by less than under the optimal penalties. The net effect is an increase in surplus of $28–$50 per person or 40–56 percent of the optimum.

This shortfall is almost completely due to the negative correlation between the ACA and optimal penalties. Equating the mean level of the ACA and optimal penalties has virtually no effect on net surplus. Because the ACA penalties are increasing in income while the optimal penalties are decreasing in the financial cost of bankruptcy, the ACA and optimal penalties are negatively correlated ($\rho = -0.34$). When it comes to mandates, progressivity and Pigovian efficiency directly conflict.

As previously discussed, the estimates are based on the sample of households without public insurance or conditional access to Medicaid. Online Appendix Table A19 examines the sensitivity of the estimates to these sample restrictions.

Panel A expands the Table 7 sample to include households that are conditionally eligible for Medicaid. Panel B alternatively restricts the Table 7 sample by dropping households with incomes below 138 percent of the Federal Poverty Line, which is the income threshold for Medicaid coverage in states that fully implement the ACA Medicaid expansions. Expanding the sample slightly raises the optimal Pigovian penalties, since the conditionally eligible have a lower financial cost of bankruptcy, and further restricting the sample slightly lowers the optimal penalties. With these alternative samples, the optimal Pigovian penalties continue to be three-quarters as large as the average ACA penalties, and the take-up and welfare effects are also similar.

VII. Discussion

The main focus of this paper is to examine how the implicit insurance from bankruptcy affects out-of-pocket payments and the level of insurance coverage. Given the large number of uninsured and the vigorous debate over policies to increase coverage, this seems like the primary question of interest. However, the implicit insurance from bankruptcy has a number of more nuanced implications. I briefly discuss some of them below.

High-Deductible Health Plans.—High-deductible health plans (HDHP) were intended by their proponents to expand insurance coverage.[44] The idea was that by offering low premiums, these plans would be expand coverage among the young and healthy who are more likely to be uninsured. Yet despite a concerted effort by policymakers and insurance companies, they have not been successful in this regard (Fronstin and Collins 2008). The implicit insurance from bankruptcy is an appealing explanation for this failure. Because more than one-half of the uninsured have a financial cost of bankruptcy of less than $5,000, HDHPs are the type of health plan that is most crowded out by this mechanism.

"Mini-Med" Plans.—A related issue is the popularity of "mini-med" plans. These are plans with annual caps on coverage of a few thousand dollars and low monthly fees. For example, McDonald's "McCrew Care" in Montana provides its employees up to $2,000 in annual benefits for $56 per month.[45] Many have questioned whether these plans are actually "insurance," since they provide essentially no coverage for large health shocks. Yet if mini-med-plan enrollees have a low financial cost of bankruptcy, this is exactly the type of insurance that theory implies they should demand, because it fills in the gap below the "deductible" of the implicit insurance from bankruptcy.

The Insurance Generosity Gap.—In his review of the literature, Gruber (2008) asks why most US households appear to be underinsured or overinsured but are rarely in between. Implicit insurance from bankruptcy can explain this finding. Without bankruptcy, households face a standard continuous trade-off between insurance generosity and other goods. Implicit insurance generates a notch: households receive some

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[44] In 2005, qualifying HDHP were required to have deductibles of $2,000–$5,250 for a family and $1,000–$2,650 for an individual.
[45] See http://www.mcmontana.com/11844/1370/McCrew-Care-Health-Insurance/ for details.

implicit insurance without giving up other goods. Convex preferences give rise to an insurance generosity gap, with households sorting into more-generous conventional health insurance and less-generous implicit insurance from bankruptcy. Bankruptcy insurance can explain why there are many households with first-dollar or no coverage, and few households with $10,000 deductible plans.

*Rising Risk, Falling Coverage.—*Chernew, Cutler, and Keenan (2005) show that more than one-half of the decrease in insurance coverage during the 1990s can be explained by rising premiums. Yet as the authors explain, from the standpoint of economic theory, this finding is counterintuitive. With standard risk preferences, rising costs should lead to *increased* coverage. Taking bankruptcy into account, however, reverses this intuition. The decrease in coverage can be explained by households substituting away from conventional health insurance and choosing bankruptcy insurance that is increasing in actuarial value without increasing in price.

VIII. Conclusion

Understanding why households are uninsured is fundamental to positive and normative analysis of health insurance policy—yet the insurance-coverage decision is not well understood. The objective of this paper is to examine how the implicit insurance from bankruptcy bears on this decision.

In the first part of the paper, I argue that the fact that most medical care is provided on credit, coupled with the fact that this debt can be discharged in bankruptcy, provides households with implicit high-deducible health insurance.

I next evaluate the quantitative importance of this mechanism. Exploiting multiple sources of variation in asset exemption law, I show that uninsured households with a higher financial cost of bankruptcy make greater out-of-pocket medical payments, conditional on the amount of care received. In turn, I find that households with greater wealth-at-risk are more likely to hold health insurance coverage. Health insurance is wealth insurance, to a certain degree, and is less valuable to those with fewer assets.

The final part of the paper examines ways in which the implicit insurance from bankruptcy might inform the design of health insurance policy. Because households do not pay for bankruptcy insurance, too many households choose to be uninsured on the margin. Using a utility-based, microsimulation model of insurance choice, I estimate that the optimal Pigovian penalties are three-quarters as large as the average penalties under the Affordable Care Act (ACA).

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