# Debtor Protection and Health Insurance: Evidence From Personal Bankruptcy Reform*

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Paolo Nicola Barbieri[1], Laura Bottazzi[2], and Giuseppe Di Giacomo[3]

[1]Università di Bologna and Prometeia
[2]Università di Bologna, IGIER and CEPR
[3]ifo Institute and LMU Munich

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**Abstract**

We investigate how the use of bankruptcy as an implicit health insurance varies across households, focusing on heterogeneity by asset holdings, race, marital status, and educational attainment. Using a difference-in-differences design based on the 2005 bankruptcy reform, we find that the reform modestly increased health insurance coverage among middle-income households unlikely to lose assets under Chapter 7, with stronger effects for married and less educated households. The reform primarily affected White households, suggesting racial disparities in bankruptcy use. Treated households also showed increased healthcare utilization and spending. These heterogeneous effects highlight how the reform may have deepened existing health and financial inequalities.

**Keywords:** health insurance, bankruptcy reform, consumer protection.

**JEL Codes:** D18, H51, I13, K35

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paolonicola.barbieri@unibo.it; l.bottazzi@unibo.it; g.digiacomo@lmu.de.

# 1 Introduction

In the United States, federal law requires hospitals to provide emergency medical care regardless of a patient's ability to pay. At the same time, the US Bankruptcy Code provides households with mechanisms to manage unsecured debts such as medical bills, either by discharging them entirely under Chapter 7, or by repaying a portion of them through a court-approved plan under Chapter 13.[1] This legal framework suggests that bankruptcy protections may serve as a form of implicit health insurance, potentially contributing to the relatively low levels of health insurance coverage in the country (Mahoney, 2015). Yet this implicit insurance is unlikely to be uniform: households differ markedly in their ability to manage health and financial shocks.

This paper studies how the value of bankruptcy as implicit health insurance varies across the population and how restricting access to bankruptcy protection affects households' health insurance and medical decisions. We focus on the 2005 *Bankruptcy Abuse Prevention and Consumer Protection Act* (BAPCPA), that introduced an income-based means test restricting eligibility for Chapter 7 debt discharge and increased the cost of filing for bankruptcy.

Because some groups were disproportionately reliant on Chapter 7 before the reform, BAPCPA created differential exposure across households. We exploit this heterogeneity in a difference-in-differences framework to estimate the causal effect of reduced bankruptcy protection on health insurance coverage, medical expenditures, healthcare utilization, and preventive care. Variation in exposure — driven by pre-reform differences in the likelihood of filing for bankruptcy along dimensions such as asset holdings, race, marital status, and educational attainment — allows us to compare outcomes for highly versus less exposed households.

Understanding these effects and their heterogeneity is crucial. If bankruptcy protections substitute for health insurance, restricting access may shift financial risk back onto households. Moreover, if the impact varies across socioeconomic groups, such a policy can exacerbate existing inequalities in both health and financial outcomes. Disadvantaged households may be pushed further into debt or forgo necessary care out of fear of medical costs.

Bankruptcy serves as a vital safety net for individuals in financial distress. Sullivan et al. (2020) and Keys (2018) find that negative events such as job loss, illness, injury, and divorce frequently precede bankruptcy filings. They provide evidence that approximately half of pre-BAPCPA bankruptcy filers cited medical expenses as a contributing factor in their financial distress.[2] While bankruptcy protection offers relief, it may also incentivize consumers to deliberately

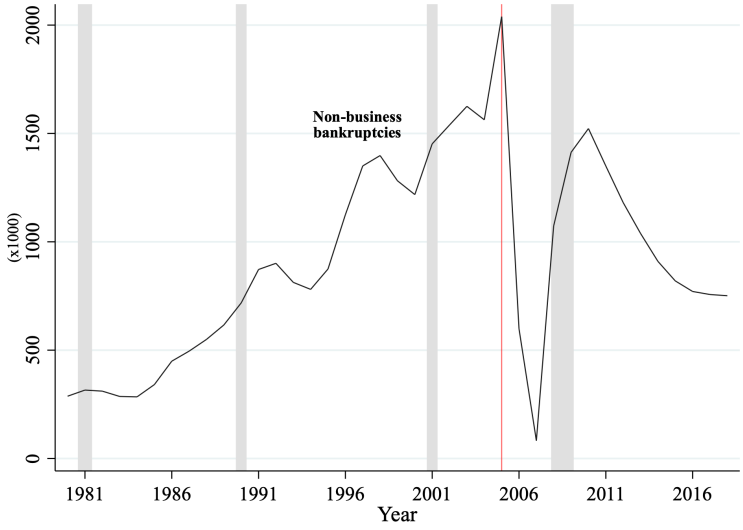
[1] This obligation was established by the Emergency Medical Treatment and Labor Act (EMTALA) in 1986. EMTALA requires hospitals to provide a medical screening examination and, if an emergency medical condition is found, to offer stabilizing treatment regardless of the individual's insurance status or ability to pay.

[2] However, Fay et al. (2002) fails to find substantial evidence supporting the idea that divorce, unemployment, or health shocks significantly influence the likelihood of filing for bankruptcy.

accumulate debt to seek financial discharge through bankruptcy.

The widespread use of bankruptcy as a financial relief mechanism led to a sharp increase in filings. Between the 1980s and 2000s, the number of non-business bankruptcy filings rose five-fold, reaching 1.5 million per year by 2004 (Figure 1).[3] In response, BAPCPA introduced an income-based means test to limit bankruptcy protections for higher-income households to discourage *abusive* Chapter 7 filings. Because the upcoming changes to the bankruptcy laws were widely known to both the legal community and the public, many individuals rushed to file before the new law took effect.[4] Some filers likely accelerated their decision to declare bankruptcy, while others, who might not have done so otherwise, may have chosen to file. The broad discussion about the bankruptcy reform, which did increase public awareness of its implications, makes BAPCPA a compelling case for studying how individuals adjusted their behavior and decision-making immediately after the law was signed (Sather, 2006a), even before its implementation.

**Figure 1:** Yearly non-business bankruptcy filings in the US




*Notes:* This figure illustrates the number of non-business filing from 1980s until 2016.

After 2005, the number of yearly bankruptcies stabilized at around 60 percent of the pre-reform level (Figure 1), and the likelihood of receiving bankruptcy relief after hospitalizations for the uninsured decreased by 70 percent post-reform (Gross et al., 2021).

Our empirical analysis draws on two main data sources: the Survey of Income and Program Participation (SIPP) and the Medical Expenditure Panel Survey (MEPS). The SIPP provides de-

[3] Moss and Johnson (1999) defined this period as the "revolution in consumer credit and consumer bankruptcy."

[4] BAPCPA was the culmination of nearly a decade of legislative efforts. The reform was under discussion and development for approximately eight years before its enactment in April 2005. Among many sources, see (Sather, 2006a).

tailed information on health insurance coverage, income, and wealth, collected every four months for up to four years. We use the 2004 panel (2004–2007) to conduct a longitudinal analysis of the reform's effect on health insurance coverage around the reform period. In contrast, MEPS offers repeated cross-sectional data, which we use to examine how BAPCPA influenced medical expenditure and healthcare utilization, and health insurance coverage in the longer run. The analysis is carried out at the household level to better reflect intra-household risk-sharing behavior and financial decision-making, with household wealth and income aggregated from individual responses and insurance coverage measured as the proportion of insured household members.

During a period of declining health insurance coverage since the early 2000s (Long et al., 2016), likely driven by reductions in employer-sponsored insurance and rising premiums, and before the implementation of the Affordable Care Act in 2014, our findings show that the reform led to a relative increase in coverage among households prevented from filing under Chapter 7.[5] We refer to a relative increase because the effect is driven primarily by a decline in coverage within the control group, rather than by a marked rise among treated households. Most of this adjustment occurs immediately after the signing of the reform in April 2005, anticipating its implementation in October. This decline in private coverage, especially among households still eligible for Chapter 7, is consistent with an anticipation effect. As documented by Sather (2006b), over 400,000 households rushed to file bankruptcy between April and October 2005, roughly 13 times the usual rate, taking advantage of the last months of the pre-BAPCPA regime. National statistics show that private insurance coverage also fell slightly between 2004 and 2005 (U.S. Census Bureau, 2006, Kaiser Family Foundation, 2006), while health-insurance premiums continued to rise (Agency for Healthcare Research and Quality, 2007). Together, these patterns suggest that some households may have temporarily relied on bankruptcy to manage medical expenses rather than renewing coverage. This interpretation is consistent with the surge in bankruptcy filings documented in Figure 1 and supports the idea of a reform *advertising effect*, by drawing public attention to the tightening of bankruptcy protection, BAPCPA induced some households to adjust their behavior, either by filing for bankruptcy under the old rules or by retaining (and in some cases purchasing) private health insurance in anticipation of reduced access to bankruptcy relief.

Our results show that households that were more likely to file for bankruptcy before the reform are responsible for most of the adjustment. Indeed, we find that this effect is concentrated among middle-class households with a low level of seizable assets. Furthermore, we observe a greater impact among married couples, who could benefit from the doubled exemption threshold available in many states, and among households without a college education, who more frequently relied on Chapter 7 protection in the pre-reform period (Fisher, 2019).

[5] As documented in a report from the U.S. Census Bureau (DeNavas-Walt and Lee, 2006), the percentage of uninsured Americans rose in 2005, continuing a trend that started around 2000. See Long et al. (2016) for trends in ESI.

Since access to bankruptcy is very different by race, with Black middle-class families being three times more likely to file for bankruptcy than White families (Warren et al., 2020, Van Loo, 2009, Braucher et al., 2012), we investigate the heterogeneity by race to explore whether bankruptcy can play a role in explaining the different attitudes towards health insurance among various racial groups. Our results show that the regulatory change affected the insurance coverage decisions of White households, while Black households appear unaffected. This pattern aligns with existing research highlighting racial disparities in bankruptcy. Specifically, despite the higher likelihood of bankruptcy filings among Black middle-class families, they have higher rates of case dismissals without debt relief in both Chapter 13 and Chapter 7 cases (Argyle et al., 2023). An additional potential mechanism is that Black households may have had fewer affordable insurance options available during the reform period. Since this occurred before the implementation of the Affordable Care Act, insurers were allowed to apply medical underwriting, which could result in higher premiums or outright denial of coverage.

Then, we analyze the consequences of these changes in health insurance on medical expenditure, utilization, and preventive care. We observe an increase in both the frequency of healthcare utilization and total medical charges. In particular, we find an increase in the number of individuals, from households who were prevented from using Chapter 7, undergoing screening for preventive medical interventions recommended by the US Preventive Services Task Force (USPSTF). Regarding households' healthcare expenditures, we observe a significant increase in total payments within the treatment group, largely driven by the surge in contributions from private health insurance rather than out-of-pocket expenditures. We show that the utilization gap between treatment and control groups widens post-reform, experiencing a notable increase of approximately 60 percent. Hence, our results corroborate existing evidence highlighting the role of insurance as a mechanism for managing financial risk and underscore that having health insurance holds the potential to improve financial security (Allen et al., 2010).

Our contribution to the existing literature is manifold. First, in a seminal paper, Mahoney (2015) using a simulated instrument highlights the existence of a causal link between seizable assets and health insurance coverage of the household, suggesting that the option of declaring bankruptcy can substitute the need for conventional health insurance. We expand upon these results on coverage in several ways: (i) Using a different identification strategy, which exploits a change in bankruptcy eligibility, we show that the effect estimated by Mahoney (2015) is mostly driven by a specific subgroup of the population, namely median-income households who would not lose assets under Chapter 7; (ii) we investigate the heterogeneity of the effect by race finding that the regulatory changes significantly influence the insurance coverage decisions of middle income White households, while Black households appear unaffected. Furthermore, we observe a larger impact among married couples and households without a college education.

Second, we further explore the implications of the reform, showing that households that increase their health coverage also tend to expand their use of medical services and preventive care. These findings align with previous literature indicating that health insurance coverage correlates with higher healthcare utilization and improved self-reported health (Finkelstein et al., 2012, Aron-Dine et al., 2013, Mazumder and Miller, 2016, Sommers et al., 2017). We also analyze households' healthcare expenditures and observe a significant increase in total payments for households that were prevented from using bankruptcy after the reform, driven by the surge in contributions from private health insurance rather than out-of-pocket expenditures.

Finally, our research contributes to the existing literature on the consequences of the reform. Although BAPCPA was implemented almost twenty years ago, the discussion on its effects is still relevant in the political debate. Advocates of the reform argue that it effectively tackled the problem of excessive bankruptcy filings attributed to abuse, with consumers deliberately accumulating debt only to seek financial relief through bankruptcy. Conversely, critics of the reform contend that it could adversely affect individuals experiencing sudden financial shocks, such as medical emergencies or job loss, suggesting that bankruptcy primarily stems from unfortunate circumstances rather than irresponsible actions (Warren and Tyagi, 2005). Gross et al. (2021) show that BAPCPA reduced the likelihood of uninsured hospitalizations leading to bankruptcy relief, lowered credit card interest rates, and narrowed the interest rate gap between prime and subprime consumers. In addition, Albanesi and Nosal (2025) document that the reform significantly reduced Chapter 7 filings while increasing household insolvency and foreclosure, highlighting that its effects extended well beyond filing behavior. We contribute to this evidence by showing that the reform influenced the health insurance choice, medical utilization, and preventive care decisions of a specific, vulnerable segment of American households, therefore extending far beyond its original scope.

The rest of the paper is organized as follows. Section 2 describes the institutional background before and after the BAPCPA and provides a simple conceptual framework that clarifies how changes in bankruptcy regulation can shape individuals' insurance choices. Section 3 describes our data sources and sample construction. Section 4 discusses our empirical strategy. Section 5 reports the results, and Section 6 concludes.

# 2 Institutional background and theoretical framework

## 2.1 Institutional framework

In the United States, the Emergency Medical Treatment and Active Labor Act (EMTALA) legislation mandates hospitals to offer emergency medical care on credit, even in situations where

repayment seems unlikely.[6] After receiving bills, households have the option to discharge their medical debt, along with most other unsecured debts, such as credit card debt and installment loans, by leveraging bankruptcy regulations. In 2015, approximately 795,000 consumer debtors declared personal bankruptcy, leading to the cancellation of about $47 billion in debt (U.S. Courts, 2015).[7]

Even the mere possibility of bankruptcy can influence financial negotiations. Many hospitals, recognizing the financial burden of medical bills, actively encourage financially distressed families to apply for charity care, discounted or fully covered treatment based on income and assets (Mahoney, 2015). In instances where charity care is not a viable option, a considerable proportion of medical debt is discharged during the collection process. Debt collectors typically recover only about 10–20 percent of bills owed by the uninsured (Pellathy and Singhal, 2010). Therefore, bankruptcy can be seen as a form of implicit health insurance (Mahoney, 2015).

Before 2005, American households benefited from an exceptionally lenient bankruptcy system. Those filing for bankruptcy had the choice between two main procedures: Chapter 7, known as the *fresh start* option, and Chapter 13. With Chapter 7, debtors could discharge their unsecured debt by giving up nonexempt assets (seizable assets) while holding onto their current and future income. On the other hand, Chapter 13 required filers to restructure their debts, repaying them gradually through an installment plan using their future income.

The asset exemptions for Chapter 7 bankruptcy filings varied significantly by state, as detailed in Table A1 in the Appendix and documented by Mahoney (2015). These exemptions, such as those for home property, vehicles, and financial assets, showed substantial heterogeneity, ranging from zero to unlimited, depending on the state. Additionally, residents of 14 states have the option to file under federal legislation instead of state laws. These asset exemptions have remained relatively consistent since their introduction in the latter part of the nineteenth century (Skeel, 2001).

Despite the relatively lenient regulations in place at the time, the number of cases filed annually remained relatively low, with approximately 300,000 cases being filed at the beginning of the 1980s. However, in 1978, the US legalized advertising by bankruptcy attorneys, and the Supreme Court allowed banks to export their home interest rates, effectively bypassing state usury laws (Gross et al., 2021). These changes led to a surge in unsecured borrowing in the following decades (White, 2007). By 2004, annual bankruptcy filings had skyrocketed to 1.5 million, prompting creditors to push for a reform of the bankruptcy code.

[6] EMTALA was implemented in April 1986 and mandates hospitals to treat patients with emergency medical conditions without delay, regardless of their insurance status or means of payment. In practice, many hospitals also extend credit for nonemergency medical care.

[7] By comparison, in 2013, Medicaid provided coverage for around 72.8 million individuals and expended approximately $461 billion, translating to an average government expenditure of $6,300 per beneficiary (U.S. Department of Health and Human Services, 2014).

The surge in bankruptcy filings led to the passage of the Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA) in 2005, enjoying bipartisan support. The initial impact of the passage of the reform, however, was a dramatic rush to file before the bankruptcy code was changed. In the six months before the law was implemented, the filing rate increased dramatically: more than 400,000 households declared bankruptcy, roughly 13 times the typical weekly caseload (Figure 1). The public's immediate reaction in April 2005, when the law was signed, is understandable given the long history of the reform process. Initial drafts date back to 1997, with the House passing a version titled the Bankruptcy Reform Act of 1999 and the Senate approving a slightly different version in 2000. Although Congress eventually reconciled these differences and passed the Bankruptcy Reform Act of 2000, President Clinton used a pocket veto to prevent it from becoming law. In the years that followed, new versions of the bill were introduced in each session of Congress but were repeatedly delayed due to disagreements over amendments. The reform only began to gain real momentum after the 2004 elections. Specifically, the reform allowed access to Chapter 7 for households that passed either the *means test*, based on income, or the *repayment test*, evaluating their ability to make payments over five years.

Debtors pass the *means test* if their average income over the six months preceding filing is below the state median income for a household of their size, or if their disposable income is less than \$110 per month.[8] Households pass the *repayment test* if their disposable income ranges between \$110 and \$182.50 per month, but paying this amount would cover less than 25 percent of the household's unsecured debt over five years.[9] These features generate a difference-in-differences variation that we exploit in our empirical analysis. Debtors who do not qualify for Chapter 7 may turn to Chapter 13, which entails a higher repayment obligation. Even in this scenario, BAPCPA reduced the advantages of filing. Before the reform, debtors could propose their repayment plans, whereas post-2005, filers are mandated to allocate all their disposable income over five years to repay their debts (Gross et al., 2021). Additionally, the reform constrained the discharge of certain debts and restricted the practice of *exemption shopping*: individuals relocating to another state were now required to wait two years before becoming eligible to use the exemptions of their new state of residence.[10] Moreover, BAPCPA increased the complexity and the expenses of the filing process: court fees rose, and filers must now complete credit counseling and financial management courses before their debts can be discharged. With increased filing requirements came higher at-

[8] As defined by Elias and Bayer (2013), disposable income is the income after deductions for necessities such as food and clothing, mortgage or rent payments, telephones, insurance, transportation, and taxes.

[9] This structure provides motivations for households to lower their income deliberately to fall below the qualifying threshold for Chapter 7 or to minimize their repayment responsibilities within Chapter 13 (Gross et al., 2021). Moreover, the design of the repayment test encourages households to deliberately accumulate more debt to surpass the 25 percent threshold. (Mahoney, 2015).

[10] Additionally, the reform prolonged the waiting period from six to eight years since the most recent Chapter 7 filing, and from six months to two years since the last Chapter 13 filing.

torney costs. Consequently, filers experienced a rise in the average financial burden of filing, from $868 to $1,309 for Chapter 7 and from $2,260 to $2,861 for Chapter 13 (Lupica, 2012).

## 2.2 Conceptual framework

In Section A2 of the Appendix, we develop a theoretical framework that motivates our empirical analysis. The model clarifies the complex dynamics of personal bankruptcy and health insurance choices, as it shows how regulatory reforms can shape individuals' insurance choices.

Following Wang and White (2000), we model Chapter 7 and Chapter 13 into a consolidated personal bankruptcy process in which debtors are required to cover a portion of their medical expenses post-bankruptcy while taking into consideration exemptions on assets.

The model features two periods. In the initial period, individuals make a critical decision: how much health insurance coverage to secure for potential health shocks in the subsequent period. In the second period, individuals face the consequences of their decisions and assess their wealth and income. If they are faced with significant medical expenses, individuals must consider whether to file for bankruptcy or to bear the costs themselves.

Individuals optimize their utility over both periods, determining the optimal level of insurance coverage that aligns with their preferences, wealth, and income.

The 2005 reform introduced a means test, restricting bankruptcy eligibility based on income thresholds. Post-reform, the model predicts a notable increase in the optimal level of insurance coverage for individuals who do not pass the means test and whose seizable assets are zero. This shift emphasizes the reform's impact on individuals' risk management behaviors, shedding light on how policy changes can ripple through financial decision-making processes.

# 3 Data and summary statistics

Our empirical analysis is based on two datasets, the Survey of Income and Program Participation (SIPP) and the Medical Expenditure Panel Survey (MEPS). The study is conducted at the household level to capture risk-sharing dynamics and decisions regarding bankruptcy, with household wealth and income aggregated from individual data, and insurance coverage calculated as the fraction of insured household members.[11]

**SIPP** – The SIPP is a Census Bureau-conducted survey that represents U.S. households on a national scale. It is composed of multi-year panels, including around 50,000 households annually. This survey collects data on health insurance coverage, income, and wealth of American households. Participating households undergo an initial in-person interview and subsequent phone or

[11] In many states, filing as a married couple doubles the exemption limits.

in-person interviews every four months for a maximum duration of four years. Our focus lies on the 2004 panel, spanning from 2004 to 2007, enabling us to conduct a longitudinal analysis around the reform period.

Following recommendations from the Census Bureau (U.S. Census Bureau, 2008a,b) and research by Graves and Mishra (2016), we generate household survey weights by summing individual-level survey weights across household members, corresponding to the first spell of an individual's sample period.[12] We adjust monetary variables to 2005 US dollars using the Consumer Price Index for All Urban Consumers (CPI-U) and exclude households from the initial sample if any members are covered by public insurance. Such households, as noted by Mahoney (2015), are less likely to decide on health insurance coverage actively. Additionally, we exclude group quarters, households that undergo structural changes during our sample period, or those transitioning from treated to control status in the post-BAPCPA period.[13] This filtering results in approximately 471,438 observations. Table A12 in Appendix A4 shows that our results remain robust when we relax the sample selection criteria.

**MEPS** – We supplement this information with the Medical Expenditure Panel Survey (MEPS), a household survey administered by the Agency for Healthcare Research and Quality. MEPS data are constituted by an overlapping panel design by enrolling a new 2-year panel annually, derived from the previous year's National Health Interview Survey (Cohen, 1997).

Using MEPS data enables us to investigate the impact of BAPCPA on medical expenditure and the utilization of medical services.[14] Similarly to our primary dataset, we aggregate data at the household level, and as previously suggested by Mahoney (2015), we calculate household survey weights as the sum of individual-level survey weights across household members.[15] We exclude households with missing wealth variables and, as previously mentioned, we exclude households with members covered by any form of public insurance.

**Summary statistics** – Table 1 presents summary statistics for the pre-reform period based on SIPP data. Columns 1 and 2 provide information for the entire sample, while Columns 3 to 6 distinguish between households that fail the means test and are prevented from using Chapter 7 after October 2005, our treatment group, and those unaffected by the reform, our control group. In the Appendix, Table A2 offers a corresponding set of statistics using MEPS data.

As indicated in Table 1, American households in our sample have, on average, 81 percent of

[12] As per Graves and Mishra (2016), this approach maximizes sample sizes while considering the sampling design.

[13] Changes in household composition can mechanically affect the share of members covered by health insurance — or any other outcome variable — without reflecting active household choices. We also exclude households that move from the treated to the control group, as this would require assumptions about the persistence of treatment effects beyond the treatment period.

[14] MEPS data contains restricted-access information regarding wealth and state of residence.

[15] The insurance coverage calculated using these weights aligns with the population average (Mahoney, 2015).

their members covered by private health insurance. This proportion increases to 94 percent for the treatment group, as richer individuals are more insured, while remaining at 70 percent for the control group. Notably, the majority of individuals across all categories obtain health insurance through their primary employer or union.

**Table 1:** SIPP pre-reform summary statistics

| | All | | Control | | Treated | |
|---|---|---|---|---|---|---|
| | Mean | Sd | Mean | Sd | Mean | Sd |
| | [1] | [2] | [3] | [4] | [5] | [6] |
| Priv. health insurance | 81.2 | 34.8 | 70.3 | 40.8 | 94.0 | 19.2 |
| Employer/union HI | 78.1 | 41.3 | 67.6 | 46.7 | 90.4 | 29.3 |
| Age | 42.3 | 10.2 | 40.6 | 10.7 | 44.3 | 9.2 |
| White | 79.4 | 40.3 | 74.2 | 43.7 | 85.6 | 35.0 |
| Black | 11.0 | 31.3 | 14.0 | 34.8 | 7.3 | 26.1 |
| Hispanic | 11.8 | 32.3 | 16.9 | 37.5 | 5.8 | 23.4 |
| Single | 16.1 | 36.8 | 18.6 | 38.9 | 13.2 | 33.8 |
| Married | 83.8 | 36.8 | 81.3 | 38.9 | 86.7 | 33.8 |
| Less than college | 67.3 | 46.9 | 78.7 | 40.9 | 53.9 | 49.8 |
| College | 32.6 | 46.9 | 21.3 | 40.9 | 46.0 | 49.8 |
| Cost of Ch.7 | 149.0 | 1048.0 | 107.0 | 1375.0 | 199.0 | 398 |
| Unsecured debt | 9.6 | 30.0 | 8.1 | 25.1 | 11.4 | 34.8 |
| Household income | 77.3 | 68.4 | 50.8 | 46.6 | 108.0 | 76.2 |
| Employed | 82.6 | 37.8 | 78.7 | 40.9 | 87.3 | 33.2 |
| Private-employed | 57.3 | 49.4 | 56.6 | 49.5 | 58.3 | 49.3 |
| Public-employed | 14.2 | 34.9 | 11.1 | 31.5 | 17.8 | 38.3 |
| Self-employed | 11.9 | 32.4 | 12.2 | 32.7 | 11.5 | 31.9 |
| Unemployed | 17.2 | 37.7 | 21.1 | 40.8 | 12.6 | 33.1 |

*Notes:* This table presents summary statistics for our sample during the pre-reform period, utilizing SIPP data comprising 471,438 observations. Percentages are multiplied by 100, and monetary values are denominated in thousands of 2005 dollars.

The analysis of socio-demographic characteristics reveals an overrepresentation of White individuals in the treatment group, accompanied by a lower proportion of Black and Hispanic individuals compared to the overall averages. Moreover, the treatment group reports a significantly higher percentage of college-educated individuals than the U.S. population in our sample. In contrast, marital status, a factor relevant for determining asset exemption levels in Chapter 7 filings, shows a similar distribution across groups and aligns with the overall sample.

The table also presents statistics on household income, bankruptcy costs, and employment. The average household income is $77,000, with the treatment group earning nearly double the average income of the control group. The average cost of bankruptcy, measured by the value of seizable assets, is approximately $150,000, while households carry around $10,000 in dischargeable debts.[16] Notably, the treatment group tends to have higher levels of unsecured debt compared to the control group, possibly because of easier access to credit. Additionally, the table highlights higher unemployment rates in the control group compared to the overall average, and the treatment group

[16] Refer to Section A3 in the Appendix for details on the construction of seizable assets.

includes a larger proportion of individuals employed in the public sector.

These statistics show significant disparities between the two groups. However, as elaborated in the following section, our identification strategy does not rely on similarities in levels but rather on the two groups following similar trends over time. Moreover, we will focus on a group of treated and controls that are more similar.

# 4 Empirical strategy

As outlined in Section 2, BAPCPA restricted access to Chapter 7 bankruptcy by making it harder for some individuals, particularly those with higher income,to qualify. Thus, to assess the impacts of the reform on our outcome variables, we employ a difference-in-differences (DID) strategy. This approach leverages variation introduced by the reform by comparing outcomes between affected households (treatment group) and those that remained eligible for Chapter 7 post-reform (control group). However, as bankruptcy filings primarily affect a specific socio-demographic group, an imprecise definition of the treatment group might bias our estimated effect toward zero. Many households above the income threshold would not have used bankruptcy even before the reform, and are therefore effectively never treated. It is thus essential to focus on households with a higher pre-reform likelihood of filing for Chapter 7.

To identify this demographic group, we refer to Fisher (2019), who provides a detailed description of the socio-demographic characteristics of bankruptcy filers based on an extensive micro dataset of over 10 million administrative bankruptcy records linked to Census and American Community Survey data.

His analysis shows that, relative to the general population, filers are disproportionately middle-income, middle-aged, non-immigrants, and are more likely to be Black, veterans, and employed full-time. The 25th percentile of income for the distribution of filers is approximately $30,000, while the 75th percentile is around $75,200 when adjusted to 2005 dollars.[17] More importantly, only five percent of Chapter 7 filers report positive seizable assets, a sharp contrast to the 94.5 percent observed in the broader U.S. population.

We can leverage this additional variation within the treated by conducting the following triple-difference-in-differences (DDD) regression:

$$y_{im(t)} = \beta_1\, treated_i \times post_{m(t)} + \beta_2\, post_{m(t)} \times highExp_i + \beta_3\, treated_i \times post_{m(t)} \times highExp_i + \alpha_i + \alpha_{m(t)} + \psi X_i + \varepsilon_{im(t)} \quad (4.1)$$

[17] Fisher (2019) presents the data in 1999 dollars, which we adjusted to 2005 dollars using the Consumer Price Index (CPI) adjustment factor: dollars 2005 = dollars 1999 × (195.3 / 166.6).

where $y_{im(t)}$ represents the outcome variable of interest for household $i$ in month $m$ of year $t$. The dummy variable $treated_i$ defines the treated households as those who fail to pass the means test in the post-reform period, while the dummy variable $post_{m(t)}$ equals one after April 2005, when the reform was signed.[18] The choice of April rather than October as the threshold is motivated by the sizable anticipation of the reform, as shown in Figure 1. As explained in Section 2 the reform process had a long history. Initial drafts date back to 1997, with the House passing a version of the reform in 1999 and the Senate approving a slightly different version in 2000. After President Clinton used a pocket veto to prevent it from becoming law, new versions of the law were introduced in each session of Congress but were repeatedly delayed due to disagreements over amendments. However, in Section A4, we conduct robustness checks by excluding the pre-reform period and using October 2005 as the threshold. The dummy variable $highExp_i$ defines high exposure households as those most likely to rely on bankruptcy pre-reform, following Fisher (2019): middle-income households (25th–75th percentile of income, \$25,000–\$75,000 in 2005 dollars) with no seizable assets (i.e., the typical Chapter 7 filer profile). In our data, $HighExp_i$ includes roughly 14 percent of the sample, and $treated_i \times highExp_i$ includes about 4 percent of the sample. According to our theoretical framework, these are the households that should be most affected by the reform.[19] Finally, $\alpha_i$ and $\alpha_{m(t)}$ denote household and month-year fixed effects, respectively, and $X_i$ contains a set of time-varying controls that vary across the robustness checks that we conduct.[20] Robust standard errors are clustered at the state level.

In Equation 4.1, the coefficient $\beta_3$ captures the differential change in the outcome variable among highly exposed households, comparing those who are treated with those who are in the control group.[21] Therefore, the key identifying assumption is that, in the absence of treatment, the treatment and control groups would have followed similar trends over time within each category of the third dimension (high- vs. low-exposed).

To shed light on the timing of adjustment and the validity of the parallel trend assumption, we also compute a dynamic version of the estimating equation:

$$y_{im(t)} = \sum_{m(t)\neq 0} month_{m(t)}[treated_i(\beta_1 + \beta_3\ highExp_i) + \beta_2\ highExp_i] + \alpha_i + \alpha_{m(t)} + \varepsilon_{im(t)} \quad (4.2)$$

[18] The 2004 panel of SIPP collects information on assets and debt only in the 2004 and 2005 waves. Consequently, we cannot measure the amount of debt in the post-reform period and therefore cannot assess the *repayment test*. Hence, treatment assignment here relies solely on the *means test*. As we know from MEPS data, where the repayment test can be assessed, only a small fraction of households fail the means test and pass the repayment test. Therefore, we believe that this limitation will not affect our results.

[19] Note that the correlation between *treated* and *highExp* is $-13\%$, reflecting that most treated households have high income and substantial seizable assets.

[20] It is worth noting that household and month-year fixed effects already capture the levels of post, treated, and *highExp*.

[21] In other words, $\hat{\beta}_3 = [(\bar{y}_{HE,T,2} - \bar{y}_{HE,T,1}) - (\bar{y}_{HE,C,2} - \bar{y}_{HE,C,1})] - [(\bar{y}_{LE,T,2} - \bar{y}_{LE,T,1}) - (\bar{y}_{LE,C,2} - \bar{y}_{LE,C,1})]$ where $HE = highExp$, $LE = non\text{-}highExp$, $T = treated$, $C = control$, $2 = post$, $1 = pre$.

where we estimate a series coefficients $\beta_{m(t)}$, that correspond to the interaction between the variables $treated_i$ and $highExp_i$, alongside a set of dummy variables $month_{m(t)}$ that take value of one in each month $m$. In the regression, we exclude the coefficient corresponding to April 2005 ($m(t) = 0$), the month when the reform was approved.

Before presenting the results, it is important to clarify that our difference-in-differences identification strategy enables us to estimate the causal effect of the reform on health insurance coverage. We limit the sample period from 2004 to 2007, as in the following years there were several important health insurance reforms and efforts aimed at improving access to healthcare, expanding coverage, and reducing costs of insurance in the United States.[22] Moreover, the SIPP panel ends in 2007, preventing a longer-term longitudinal analysis. Estimates for the post-2007 period are instead obtained using MEPS data, as reported in Appendix A4.

# 5 Results

We start by presenting the empirical findings on health insurance coverage and examining the heterogeneity of the effects. Next, we evaluate the reform's impact on healthcare expenditures and utilization.

## 5.1 Health insurance coverage

**Dynamic Specification** – Figure 2 illustrates the estimates of the trend in average household health insurance coverage for both the treatment and control groups, conditional on household fixed effects. Panels A and B report the estimates of the month-year fixed effects for the entire sample and for the highly exposed households only, respectively. Panel C shows the difference between treatment and control groups estimated using Equation 4.2.

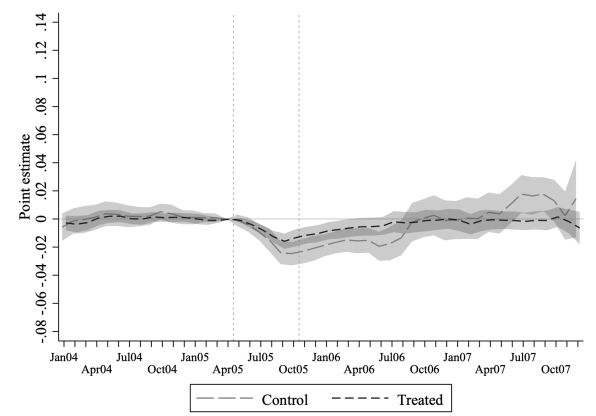
From Panel A, we can see that both groups exhibited a similar (parallel) trend before the approval of the reform in April 2005, reinforcing the interpretation of our results. Between the approval of BAPCPA in April 2005 and its implementation in October 2005, insurance coverage declined but returned to pre-reform levels afterward. This pattern, along with the rush to file documented in Figure 1, suggests that the relevant threshold for the reform is its approval in April, rather than its actual implementation in October.[23] It also indicates that there is no substantial

[22] The most notable of these was the Affordable Care Act (ACA), passed in 2010, and the Children's Health Insurance Program Reauthorization Act (CHIPRA) of 2009, which expanded the Children's Health Insurance Program (CHIP) to provide health coverage for more low-income children.

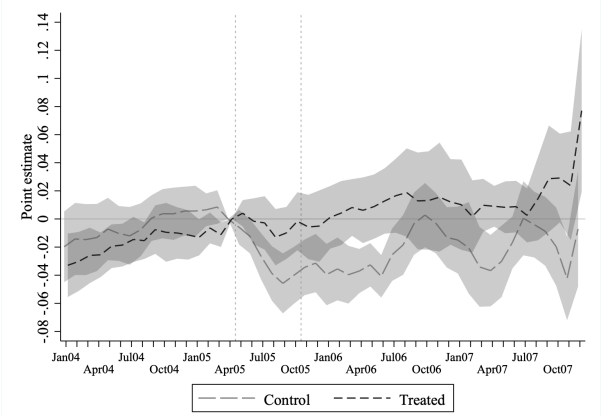
[23] Table A15 in the robustness checks section shows that using October as the threshold does not affect our results. We also find that the effect remains consistent when excluding the turbulent pre-reform period from April to October 2005.

**Figure 2:** Trends in health insurance coverage and dynamic DDD

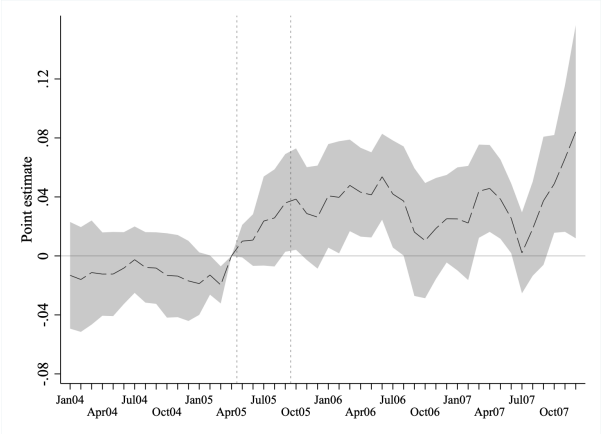
Panel A: Entire sample




Panel B: High-exposed




Panel C: Dynamic DDD




*Notes:* The figure reports the trend in insurance coverage for the treatment and the control group conditional on household fixed effects, using SIPP data. The reference period is set to April 2005, when the reform was signed. Panel A illustrates the data for the whole sample, while Panel B focuses on the subsample of highly exposed households. Panel C illustrates the dynamic of the effect of BAPCPA on PHIC using SIPP data. The coefficients in the graph report the sum of the $\beta_1$ and $\beta_3$ from Equation 4.2 for each $month_{m(t)}$. Standard errors are robust against heteroskedasticity and allow for clustering at the state level. Confidence intervals are computed at a 95% level.

difference in the pattern between the broadly defined treatment and control group. This supports the relevance of examining heterogeneous effects by pre-reform propensity to use Chapter 7.

Panel B focuses on high-exposed households only. While the overall dynamics resemble those in Panel A, a clear divergence emerges between treated and control households within this group. Specifically, following the approval of the reform in April 2005, PHIC levels dropped less sharply for the treated group than for the control group. This differential decline is unique to high-exposed households, for whom bankruptcy was a particularly salient and valuable option before the reform. After the reform's implementation in October 2005, PHIC slowly increased by about two percentage points among treated households, whereas it remained stable among control households.

This difference in the dynamic is highlighted in Panel C, which reports the sum of $\beta_1$ and $\beta_3$ for each $month_{m(t)}$. This sum captures the difference between the treatment and control groups for highly exposed households relative to the average insurance coverage during the baseline period. In simpler terms, it reflects the month-by-month intention-to-treat (ITT) effect for the highly exposed group. In Section A4 in the Appendix, we repeated this analysis using MEPS data to examine the dynamics over a longer time frame, from 2000 to 2011. The results are highly consistent with the evidence reported in Figure 2.

**Interpretation of the coefficients** – The post-reform gap in average coverage between high-exposed treated and control households stems mainly from the smaller decline in PHIC among the treated group, rather than from a pronounced increase in their coverage, as one might have expected. This evidence complicates the causal interpretation of the coefficients. Specifically, the larger decline in PHIC within the high-exposed control group could be attributable to several factors: (i) a broader aggregate trend affecting the entire population; (ii) increased awareness of the insurance benefits of bankruptcy among the control group following the reform approval; (iii) a trend specific to highly exposed individuals in the control group. If the latter hypothesis holds, it would call into question the causal interpretation of the coefficients.

Although these scenarios are observationally equivalent, there is suggestive evidence favoring the exclusion of the third hypothesis. Notably: (i) The decline is uniform across subgroups — non-high-exposed treated, non-high-exposed control, and high-exposed control (see Figures 2 and A1) — except for high-exposed treated households for which the bankruptcy regulation was salient; (ii) the graph shows parallel trends before the reform and the drop in the control group coverage aligns with the legislative process leading to reform implementation; (iii) as detailed in the following paragraphs, the lack of decline is primarily linked to demographic groups (by race, marital status, and education) that were more inclined to use Chapter 7 in the pre-reform period.[24] Moreover, national data also show a contemporaneous, though modest, drop in private insurance coverage between 2004 and 2005 (U.S. Census Bureau, 2006), suggesting that our control-group decline aligns with broader economic trends.

In light of the evidence presented and the absence of factors indicating a specific decline in insurance coverage for the high-exposed control group only, we can reasonably assume that, in the absence of the reform, the treatment group would have exhibited behavior similar to that of the control group. Consequently, the coefficients calculated will be interpreted as deviations from the coverage the treated households would have experienced in the absence of the treatment and, therefore, as the causal effect of the reform.

[24] Holahan (2011) reported that both the number and share of the population without health coverage have grown since 2000. The drop in the rates of employer-sponsored coverage has not been offset by the increase in public coverage through Medicaid or the Children's Health Insurance Program (CHIP).

**Static specification** – We can now assess the impact of the reform on PHIC, as delineated in Equation 4.1. Table 2 reports the results using SIPP data. Regressions are weighted by household weights in the first spell, and standard errors are clustered at the state level.

**Table 2:** The effect of the reform on health insurance coverage

| | [1] | [2] | [3] | [4] |
|---|---|---|---|---|
| Post × Treated | 0.001 | 0.001 | -0.005 | -0.003 |
| | (0.005) | (0.005) | (0.005) | (0.005) |
| Post × HighExp | -0.015** | -0.015** | -0.018*** | -0.018*** |
| | (0.007) | (0.007) | (0.006) | (0.006) |
| Post × Treated × HighExp | 0.042*** | 0.042*** | 0.042*** | 0.040*** |
| | (0.010) | (0.010) | (0.010) | (0.010) |
| Observations | 471,438 | 471,438 | 471,438 | 471,438 |
| *Covariates*: | | | | |
| Household FE | ✓ | ✓ | ✓ | ✓ |
| Time FE | ✓ | ✓ | ✓ | ✓ |
| Employment | | ✓ | ✓ | ✓ |
| $\text{Income}^2$ | | | ✓ | ✓ |
| $\text{Income}^4$ | | | | ✓ |

*Notes:* This table illustrates the estimates of the effect of the reform on health insurance coverage using SIPP data. Treated equals one if the household fails to pass the means test. HighExp equals one for households with no seizable assets and income between $25000 and $75000. Column 1 incorporates household and month-year fixed effects. In Column 2, a dummy variable for employment is introduced. Column 3 adjusts for a second-degree polynomial in income, while Column 4 includes controls for a fourth-degree polynomial in income. Standard errors are robust against heteroskedasticity and allow for clustering at the state level. Coefficients with ***, ** and * are significant at the 1%, 5% and 10% confidence level.

Column 1 presents our baseline estimates of the impact of BAPCPA on PHIC, accounting for household and month-year fixed effects only. Columns 2 to 4 extend the analysis by incorporating additional covariates to address potential confounding factors that might influence our estimates. Specifically, we control for employment status and income up to a fourth-order polynomial degree. Notably, the addition of these controls does not result in any substantial alterations to our point estimates. Therefore, we opt for the parsimonious baseline model as our preferred specification.[25]

According to our preferred specification, the reform does not seem to affect the PHIC of low-exposed households within the treatment group, as indicated by the coefficient of the double interaction term. This coefficient is nearly zero and is not statistically significant. Conversely, a positive impact is evident when analyzing the high-exposed group. The triple interaction term shows a positive and statistically significant coefficient, indicating that the reform acted as an incentive for these households to maintain their insurance coverage. To be specific, the treatment results in an approximate four percentage point rise in PHIC compared to the counterfactual.[26]

[25] Mahoney (2015) suggested that controlling for income is important, as hospital charity care is directly linked to household income. However, we prioritize the simplicity of our baseline specification as our primary model. This choice reflects concerns that income and employment may adjust endogenously to the treatment, potentially acting as *bad controls* and introducing bias into our estimates.

[26] This number is obtained by summing the coefficients from $Post \times Treated$ and $Post \times Treated \times HighExp$.

Are these effects large or small, and how do they compare to Mahoney (2015)? A direct comparison is complicated by differences in research design, but we can obtain a rough benchmark using the change in the average cost of bankruptcy for the highly exposed households in our sample before and after the reform. Mahoney (2015) estimates, using SIPP data, that a one-unit increase in the log of seizable assets raises the probability of health insurance coverage by about 2.5 percentage points. In our setting, the log cost of bankruptcy for treated households increases from 7.6 to 11.57 between the pre- and post-period, a change of 3.97 log units. Applying Mahoney (2015)'s semi-elasticity to this change implies an increase in coverage of roughly $3.97 \times 2.5 \approx 9.9$ percentage points, whereas our difference-in-differences estimate is four percentage points. Our estimated effect is therefore less than half the magnitude predicted by mechanically applying Mahoney (2015)'s coefficient. Moreover, in our case, the entire response occurs within a specific subgroup of the population with a higher pre-reform propensity to use bankruptcy.

**Robustness** – In Section A4 in the Appendix, we perform several robustness checks on this specification. Specifically, (i) we report the estimates of the effect of BAPCPA on insurance coverage using MEPS data and extending the analysis to 2011 (see Table A9 and Figure A5). Similarly to the results obtained with the SIPP database, treatment is associated with a rise in PHIC; (ii) we show that our results remain consistent, though slightly noisier, when matching households in the treated and control groups. This is done using a combination of propensity score matching and difference-in-differences; (iii) we show that the sample selection operated in our analysis does not drive our results; (iv) we further report the results of the dynamic regression using the method of Sun and Abraham (2021) to account for the fact that while the reform was simultaneously introduced throughout the US, the change in Chapter 7 eligibility could impact each household at different points in time, resulting in a *de facto* staggered implementation; (v) we also report our results using a different control group, high-income households, which are *de facto* never treated; (vi) we show that both the median income and the zero seizable assets criteria are relevant in determining the high-exposed group. Our results survive all the robustness checks we perform; Finally, (vii) Table A15 shows that using October as the threshold, or excluding the turbulent pre-reform period from April to October 2005, does not affect our results.

## 5.2 Heterogeneity of the effect on coverage

We now investigate the heterogeneity of the effect of the reform on PHIC to understand the differences in the use of bankruptcy as a source of implicit health insurance across different demographic groups.

**Race** – Table 3 displays the estimates derived from Equation 4.1 for households headed by individuals who are White (Column 1) or Black (Column 2). In Table A4, we replicate the estimates

for the high-exposure subsample, interacting $post \times treated$ with race dummies to enable a direct comparison of coefficients across racial groups.[27] While the dynamic version of the regression for these groups, as well as for Hispanic and the residual category, is reported in Figure A2 in the Appendix.

**Table 3:** Heterogeneity by race of the effect on health insurance coverage

| | White | Black |
|---|---|---|
| | [1] | [2] |
| Post × Treated | 0.008 | -0.002 |
| | (0.006) | (0.015) |
| Post × HighExp | -0.019* | 0.015 |
| | (0.010) | (0.015) |
| Post × Treated × HighExp | 0.050*** | -0.008 |
| | (0.013) | (0.024) |
| Observations | 375,028 | 53,957 |
| *Fixed effects*: | ✓ | ✓ |

*Notes:* This table presents estimates of the reform's impact on health insurance coverage using SIPP data. Treated equals one if the household fails to pass the means test. HighExp equals one for households with no seizable assets and income between $25000 and $75000. Column 1 provides estimates for the subsample of White households, and Column 2 for Black households. Standard errors are robust against heteroskedasticity and allow for clustering at the state level. Coefficients with ***, ** and * are significant at the 1%, 5% and 10% confidence level.

Our findings suggest that only White households modify their level of PHIC after the reform, with Black households showing no response. Put differently, the decline in PHIC observed within the control group primarily stems from a reduction in coverage among White households, with Black households showing a flat trend in the reform period (see Figure A3 in the Appendix).

The lack of response from Black households should not come as a surprise. Existing research on bankruptcy preferences among different racial groups indicates that Black households tend to favor Chapter 13 over Chapter 7, at a rate approximately three times higher than that of White households (Van Loo, 2009).[28] More recent evidence shows that Black filers are statistically more likely to experience dismissals without debt relief in both Chapter 13 and Chapter 7 cases, making bankruptcy less beneficial for this group (Argyle et al., 2023). Therefore, the lack of effect on the insurance decisions of Black households can be understood by acknowledging that BAPCPA primarily altered eligibility requirements for Chapter 7, rather than Chapter 13, in addition to the preference of Black households for Chapter 13.

These findings also confirm the presence of a race gap in formal and informal health insurance. While White households, who were more likely to be insured before BAPCPA, experienced a

[27] As a robustness check, we repeat a similar analysis using MEPS data. Results are reported in Table A5.

[28] This trend can be attributed to various factors, including the higher stigma associated with a bankruptcy within more religious communities or the protection of assets such as driving licenses and vehicles under Chapter 13, which are often essential for commuting to work (Morrison et al., 2020) and may steer Black households towards filing under Chapter 13 (Braucher et al., 2012).

reduction in coverage that could slightly narrow the racial gap, the limited responsiveness of Black households highlights deeper systemic barriers within the bankruptcy and financial systems. These barriers limit Black households' ability to adapt to policy changes, potentially leaving them more exposed to financial and health-related risks compared to their White counterparts.

Finally, an additional potential mechanism is that Black households may have had fewer affordable insurance options available during the reform period. Since this occurred before the implementation of the Affordable Care Act, insurers were allowed to apply medical underwriting, which could result in higher premiums or outright denial of coverage.

**Marital status and education** – In Table 4, we further decompose the effect by marital status and education. The table reveals that the effect is positive and larger for married couples. These households can double the exemption limits in several states, thus benefiting more from Chapter 7 protection. Moreover, in line with the findings of Fisher (2019), we observe that the impact is positive and significant only for households without a college education, which were more likely to use Chapter 7 before the reform.

**Table 4:** Heterogeneity of the effect on health insurance coverage

| | Marital status | | College Education | | |
|---|---|---|---|---|---|
| | Single | Married | No-degree | Voc./Asso. | Bachelor's |
| | [1] | [2] | [3] | [4] | [5] |
| Post × Treated | -0.002 | 0.003 | 0.004 | 0.004 | -0.002 |
| | (0.006) | (0.006) | (0.008) | (0.011) | (0.006) |
| Post × HighExp | 0.005 | -0.015** | -0.020** | 0.001 | -0.011 |
| | (0.019) | (0.007) | (0.010) | (0.022) | (0.023) |
| Post × Treated × HighExp | 0.013 | 0.042*** | 0.055*** | 0.032 | 0.011 |
| | (0.014) | (0.013) | (0.017) | (0.030) | (0.021) |
| Observations | 150,065 | 321,373 | 223,164 | 98,280 | 157,994 |
| *Fixed effects*: | ✓ | ✓ | ✓ | ✓ | ✓ |

*Notes:* This table displays estimates of the reform's effect on health insurance coverage using SIPP data. Treated equals one if the household fails to pass the means test or the repayment test. HighExp equals one for households with no seizable assets and income between $25000 and $75000. Column 1 presents estimates for households where the householder is unmarried, Column 2 for households where the householder is married, Column 3 reports results for non-college graduates, Column 4 for households where the householder has vocational training or an associate degree, and Column 5 reports the results for households where the householder has at least a bachelor's degree. Standard errors are robust against heteroskedasticity and allow for clustering at the state level. Coefficients with ***, ** and * are significant at the 1%, 5% and 10% confidence level.

## 5.3 Healthcare utilization and expenditure

Using MEPS data, we expand our investigation to incorporate variables linked to medical expenses and usage. This analysis reinforces the findings on coverage and provides insights into the extent to which bankruptcy influences the health-related decisions of American households. Building on our earlier findings, we focus solely on *high-exposed* households for the rest of the paper. While MEPS data offers valuable insights into the reform's impact on various health-related

factors, its panel component cannot be fully used in this context. The panels within this dataset are relatively short (two years), and a significant portion of the information is gathered only once. As a result, for the remainder of the analysis, the data will be structured in the form of repeated cross-sections. Hence, we run the following regression:

$$y_{it} = \beta_1 \, treated_i + \beta_2 \, treated_i \times post_t + \alpha_d + \alpha_{st} + \varepsilon_{it} \tag{5.1}$$

where $y_{it}$ is the outcome variable of interest for household $i$ in year $t$. The dummy variable $treated_i$ takes value one if household $i$ fails both means and repayment tests, and the dummy variable $post_t$ takes value one after 2005. Finally, $\alpha_d$ is the full interaction of marital status, race, age, and education dummy, and $\alpha_{st}$ is the full interaction of state and year fixed effects.[29] Robust standard errors are clustered at the state level.[30]

**Medical utilization and expenditure** – We examine the impact of BAPCPA on medical utilization and expenditure. The estimates derived from Equation 5.1 are presented in Table 5.

In Column 1, we outline the impact on medical charges, which represent the list price of medical care and act as a proxy for medical utilization, as suggested by Mahoney (2015). Columns 2 and 3 illustrate the effect of the reform on out-of-pocket (OOP) and private health insurance payments (PIP) used to cover medical expenses. As medical expenditure and utilization distributions often display right-skewness, largely because a minority of individuals incur exceptionally high expenses, we examine these variables in logarithmic form rather than their original levels.[31] Finally, Columns 4 and 5 outline the effect on the share of total payments covered by OOP and PIP, respectively.

The table shows that the utilization gap between the treated and control groups widens after the reform, showing a significant increase of about 60 percent (Column 1).[32] Table A6 in the Appendix reveals that the higher utilization is primarily attributed to a higher number of physician visits for the treatment group, in both office-based and hospital outpatient visits. This outcome aligns with the findings of Finkelstein et al. (2012), illustrating how health insurance coverage correlates with more healthcare utilization.[33]

Additionally, the table reveals a rise in the contribution of private health insurance compared to the counterfactual, both in terms of the contribution amount (i.e., Total Payments and PIP) and

[29] Note that year fixed effects already capture the level of post.

[30] When clustering standard errors at the state level, above the household level, the number of observations for each household does not affect the statistical inference (Mahoney, 2015).

[31] To account for households with zero expenditure or utilization, we compute $log(1+x)$.

[32] These figures are calculated as $(e^{(0.472)} - 1) * 100 = 60.3\%$

[33] Additionally, a marginal positive coefficient is estimated for emergency room accesses, consistent with the observations of Card et al. (2008), who show that an increase in coverage, in the case of Medicare eligibility at age 65, also leads to a rise in emergency room access.

**Table 5:** Effect on utilization and expenditure

| | Utilization | Expenditure | | | | |
|---|---|---|---|---|---|---|
| | Medical charges | Total payments | Out-of-pocket | Priv. insurance payments | OOP/Tot. payments | PIP/Tot. payments |
| | [1] | [2] | [3] | [4] | [5] | [6] |
| Treated | -0.106 | 0.007 | 0.099 | 0.187 | 0.016 | 0.006 |
| | (0.108) | (0.078) | (0.071) | (0.132) | (0.018) | (0.019) |
| Post × Treated | 0.472*** | 0.351*** | 0.137 | 0.684*** | -0.076*** | 0.067** |
| | (0.127) | (0.109) | (0.152) | (0.169) | (0.026) | (0.026) |
| Observations | 3,275 | 3,275 | 3,275 | 3,275 | 2,948 | 2,948 |
| *Fixed effects*: | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |

*Notes:* This table reports the effect of BAPCPA on medical utilization and expenditure for highly exposed households. The estimates are derived from Equation 5.1 using MEPS data. Treated equals one if the household fails to pass the means test or the repayment test. Column 1 details the effect on medical charges, Column 2 the effect on total medical payments, and Columns 3 and 4 depict the impact on out-of-pocket (OOP) and private health insurance payments (PIP). Dependent variables in Columns 1 to 4 are expressed as log transformations. Columns 5 and 6 outline the effect on the share of total payments covered by OOP and PIP, respectively. Standard errors are robust against heteroskedasticity and allow for clustering at the state level. Coefficients with ***, ** and * are significant at the 1%, 5% and 10% confidence level.

as a fraction of total medical payments. We find that the significant increase in total payments for the treatment group seems to be related to the increases in private health insurance contributions rather than in out-of-pocket spending, in line with the estimated effect on PHIC. Finally, table A7 in the Appendix reports the same estimates, dividing by Black and White households. Again, our results are driven by the response of White households.

In summary, Table 5 highlights an increase in the significance of private health insurance payments, both in terms of absolute contribution and as a percentage of total medical payments, when comparing the treatment and control groups, aligning with the observed impact on PHIC.

**Preventive care** – The US Preventive Services Task Force (USPSTF) provides recommendations for preventive care tailored to specific demographic groups. These recommendations, summarized by Holden et al. (2015), are presented in Table A8 in the Appendix.[34]

Our analysis focuses on six preventive services, including cancer screening procedures (breast, colon, and cervical), hypertension and blood cholesterol screening, and routine physical checkups. The dependent variables were constructed following USPSTF guidelines, with each representing the fraction of individuals within the household's target population who underwent the recommended screening within the specified time frame.

The outcomes of our analysis are detailed in Table 6. Although positive coefficients are observed throughout the table, only more general tests, such as blood pressure checks and routine doctor visits, show statistically significant results. This result suggests that the increase in coverage translates into an increase in utilization, and in particular in preventive care.

[34] Our Table is derived from Table 1 of Holden et al. (2015).

**Table 6:** Effect on preventive care

| | Pap smear | Mammography | Cholesterol check | Blood pressure test | Routine check | Colonoscopy |
|---|---|---|---|---|---|---|
| | [1] | [2] | [3] | [4] | [5] | [6] |
| Treated | 0.040 | 0.133*** | 0.046 | 0.026 | 0.019 | -0.015 |
| | (0.025) | (0.040) | (0.039) | (0.017) | (0.024) | (0.100) |
| Post × Treated | 0.040 | 0.049 | 0.026 | 0.060*** | 0.092** | 0.019 |
| | (0.058) | (0.059) | (0.064) | (0.014) | (0.038) | (0.104) |
| Observations | 2,484 | 1,138 | 1,790 | 3,250 | 3,249 | 281 |
| *Fixed effects*: | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |

*Notes:* The presented table outlines the impact of BAPCPA on preventive care. The estimates are obtained from Equation 5.1 using MEPS data. Each dependent variable reflects the proportion of individuals within the household's target population who underwent the recommended screening within the specified timeframe. Treated equals one if the household fails to pass the means test or the repayment test. Standard errors are robust against heteroskedasticity and allow for clustering at the state level. Coefficients with ***, ** and * are significant at the 1%, 5% and 10% confidence level.

# 6 Conclusions

This paper investigates how the use of bankruptcy as an implicit health insurance varies across households, focusing on heterogeneity by asset holdings, race, marital status, and educational attainment.

To explore this hypothesis, we exploit the 2005 BAPCA, which significantly reduced debtor protections. Using a difference-in-differences approach, we examine how this legislative change influenced private health insurance coverage decisions across different sociodemographic groups. Understanding this heterogeneity is crucial, as it reveals who is most vulnerable to financial and health shocks, how the reform redistributed burdens, and which policy interventions could more effectively target those at risk. Identifying group-specific responses also allows us to assess whether the reform exacerbated existing inequalities in health and financial outcomes. For example, disadvantaged individuals may forgo necessary care or be pushed deeper into poverty due to fear of medical debt.

We find that the reduction in bankruptcy protections led to an increase in health insurance coverage among white, middle-class households without seizable assets — a group that, as noted in the literature, was particularly likely to file for bankruptcy before the reform. While this finding supports the mechanism proposed by Mahoney (2015), our results suggest that the potential for bankruptcy to serve as implicit health insurance is limited to a narrow segment of the population. Consequently, reducing asset exemptions in bankruptcy is unlikely to substantially increase average private health insurance coverage nationwide.

In our heterogeneity analysis, we find that the reform's effects were driven primarily by married households and those without a college degree, consistent with pre-reform bankruptcy patterns and the stronger protections available to married couples. Furthermore, the increase in private health

insurance coverage occurred mainly among white households, with Black households showing little response. This aligns with existing evidence of racial disparities in both the accessibility and utilization of the bankruptcy system and points to a broader racial gap in access to both formal and implicit health insurance.

To further assess the effects of bankruptcy reform on health-related behavior, we also analyze changes in health expenditures and service utilization. Beyond increased PHI coverage, the reform led to higher medical spending and greater use of healthcare services, including preventive care, among affected households. In line with these findings, we observe a decline in the share of medical costs paid out-of-pocket by treated households. These results underscore the importance of insurance coverage in enhancing financial protection and expanding access to healthcare.

Our study contributes to the broader understanding of how bankruptcy protection laws influence insurance coverage decisions. We extend the existing literature and inform the ongoing policy debate on how best to balance financial risk protection with access to essential healthcare services for American households.

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# Appendix:

## A1 Additional figures and tables

**Table A1:** State asset exemptions

| State | Homestead | Vehicle | Retirement | Financial assets | Wildcard | Wildcard no-homestead | Federal available |
|---|---|---|---|---|---|---|---|
| Alabama | 10,000 | 0 | Unlimited | 0 | 6,000 | 6,000 | no |
| Alaska | 67,500 | 7,500 | Unlimited | 3,500 | 0 | 0 | no |
| Arizona | 150,000 | 10,000 | Unlimited | 300 | 0 | 0 | no |
| Arkansas | Unlimited | 2,400 | 40,000 | 0 | 500 | 500 | yes |
| California 1 | 75,000 | 4,600 | Unlimited | 1,825 | 0 | 0 | no |
| California 2 | 0 | 2,975 | Unlimited | 0 | 19,675 | 19,675 | no |
| Colorado | 90,000 | 6,000 | Unlimited | 0 | 0 | 0 | no |
| Connecticut | 150,000 | 3,000 | Unlimited | 0 | 2,000 | 2,000 | yes |
| DC | Unlimited | 5,150 | Unlimited | 0 | 17,850 | 17,850 | yes |
| Delaware | 0 | 0 | Unlimited | 0 | 500 | 500 | no |
| Florida | Unlimited | 2,000 | Unlimited | 0 | 2,000 | 2,000 | no |
| Georgia | 10,000 | 7,000 | Unlimited | 0 | 11,200 | 11,200 | no |
| Hawaii | 40,000 | 5,150 | Unlimited | 0 | 0 | 0 | yes |
| Idaho | 50,000 | 6,000 | Unlimited | 0 | 1,600 | 1,600 | no |
| Illinois | 15,000 | 2,400 | Unlimited | 0 | 4,000 | 4,000 | no |
| Indiana | 0 | 0 | Unlimited | 0 | 20,000 | 20,000 | no |
| Iowa | Unlimited | 1,000 | Unlimited | 0 | 200 | 200 | no |
| Kansas | Unlimited | 40,000 | Unlimited | 0 | 0 | 0 | no |
| Kentucky | 10,000 | 5,000 | Unlimited | 0 | 2,000 | 2,000 | no |
| Louisiana | 25,000 | 0 | Unlimited | 0 | 0 | 0 | no |
| Maine | 70,000 | 10,000 | Unlimited | 0 | 12,800 | 12,800 | no |
| Maryland | 0 | 0 | Unlimited | 0 | 22,000 | 22,000 | no |
| Massachusetts | 1,000,000 | 1,400 | Unlimited | 1,250 | 0 | 0 | yes |
| Michigan | 7,000 | 0 | Unlimited | 0 | 0 | 0 | no |
| Minnesota | 200,000 | 7,600 | Unlimited | 0 | 0 | 0 | yes |
| Mississippi | 150,000 | 0 | Unlimited | 0 | 10,000 | 10,000 | no |
| Missouri | 15,000 | 6,000 | Unlimited | 0 | 1,250 | 1,250 | no |
| Montana | 200,000 | 5,000 | Unlimited | 0 | 0 | 0 | no |
| Nebraska | 12,500 | 0 | Unlimited | 0 | 0 | 5,000 | no |
| Nevada | 400,000 | 30,000 | 1,000,000 | 0 | 0 | 0 | no |
| New Hampshire | 200,000 | 8,000 | Unlimited | 0 | 8,000 | 8,000 | yes |
| New Jersey | 0 | 0 | Unlimited | 0 | 2,000 | 2,000 | yes |
| New Mexico | 60,000 | 8,000 | Unlimited | 0 | 1,000 | 4,000 | yes |
| New York | 20,000 | 0 | Unlimited | 0 | 10,000 | 10,000 | no |
| North Carolina | 13,000 | 3,000 | Unlimited | 0 | 8,000 | 8,000 | no |
| North Dakota | 80,000 | 2,400 | 200,000 | 0 | 0 | 15,000 | no |
| Ohio | 10,000 | 2,000 | Unlimited | 800 | 800 | 800 | no |
| Oklahoma | Unlimited | 6,000 | Unlimited | 0 | 0 | 0 | no |
| Oregon | 33,000 | 3,400 | 15,000 | 15,000 | 800 | 800 | no |
| Pennsylvania | 0 | 0 | Unlimited | 0 | 600 | 600 | yes |
| Rhode Island | 200,000 | 20,000 | Unlimited | 0 | 0 | 0 | yes |
| South Carolina | 10,000 | 2,400 | Unlimited | 0 | 0 | 2,000 | yes |
| South Dakota | Unlimited | 0 | 50,0000 | 0 | 4,000 | 4,000 | no |
| Tennessee | 7,500 | 0 | Unlimited | 0 | 8,000 | 8,000 | no |
| Texas | Unlimited | 0 | Unlimited | 0 | 60,000 | 60,000 | yes |
| Utah | 40,000 | 5,000 | Unlimited | 0 | 0 | 0 | no |
| Vermont | 150,000 | 5,000 | Unlimited | 1,400 | 8,400 | 8,400 | yes |
| Virginia | 0 | 4,000 | 35,000 | 0 | 32,000 | 32,000 | no |
| Washington | 40,000 | 5,000 | Unlimited | 0 | 4,000 | 4,000 | yes |
| West Virginia | 0 | 4,800 | Unlimited | 0 | 51,600 | 51,600 | no |
| Wisconsin | 40,000 | 0 | Unlimited | 2,000 | 10,000 | 10,000 | yes |
| Wyoming | 20,000 | 4,800 | Unlimited | 0 | 0 | 0 | no |

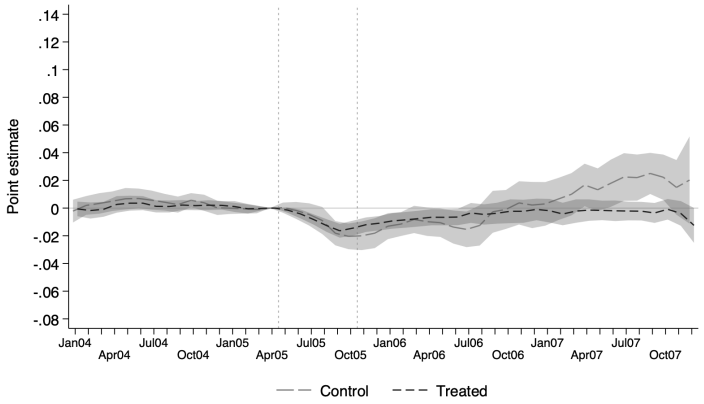
Note: Exemptions for couples filing jointly from Mahoney (2015). California residents can choose between two regimes, and households can choose federal exemptions in states where it is available. Wildcard no-homestead exemption can be used when not using the homestead exemption.

**Table A2:** MEPS pre-reform summary statistics

| | All | | Control | | Treated | |
|---|---|---|---|---|---|---|
| | Mean | Sd | Mean | Sd | Mean | Sd |
| | [1] | [2] | [3] | [4] | [5] | [6] |
| Priv.health insurance | 83.5 | 32.8 | 73.0 | 40.2 | 91.2 | 23.2 |
| Employer/union HI | 79.7 | 40.1 | 67.6 | 46.7 | 88.6 | 31.7 |
| Age | 41.7 | 10.4 | 39.4 | 10.9 | 43.3 | 9.7 |
| White | 74.1 | 43.7 | 64.9 | 47.7 | 80.9 | 39.2 |
| Black | 9.3 | 29.0 | 12.2 | 32.7 | 7.2 | 25.8 |
| Hispanic | 10.9 | 31.2 | 17.0 | 37.6 | 6.5 | 24.6 |
| Single | 19.7 | 39.8 | 25.5 | 43.6 | 15.5 | 36.2 |
| Married | 80.2 | 39.8 | 74.4 | 43.6 | 84.4 | 36.2 |
| Less than college | 56.5 | 49.5 | 72.3 | 44.7 | 45.0 | 49.7 |
| College | 43.4 | 49.5 | 27.7 | 44.7 | 54.9 | 49.7 |
| Cost of Ch.7 | 186.0 | 512.0 | 104.0 | 404.0 | 246.0 | 571.0 |
| Unsecured debt | 7.0 | 29.2 | 6.6 | 27.7 | 7.4 | 30.2 |
| Household income | 63.9 | 44.3 | 31.6 | 16.2 | 87.4 | 43.5 |
| Employed | 88.8 | 31.4 | 84.9 | 35.7 | 91.7 | 27.5 |
| Self-employed | 4.5 | 20.8 | 4.1 | 19.9 | 4.8 | 21.4 |
| Unemployed | 11.1 | 31.4 | 15.0 | 35.7 | 8.2 | 27.5 |

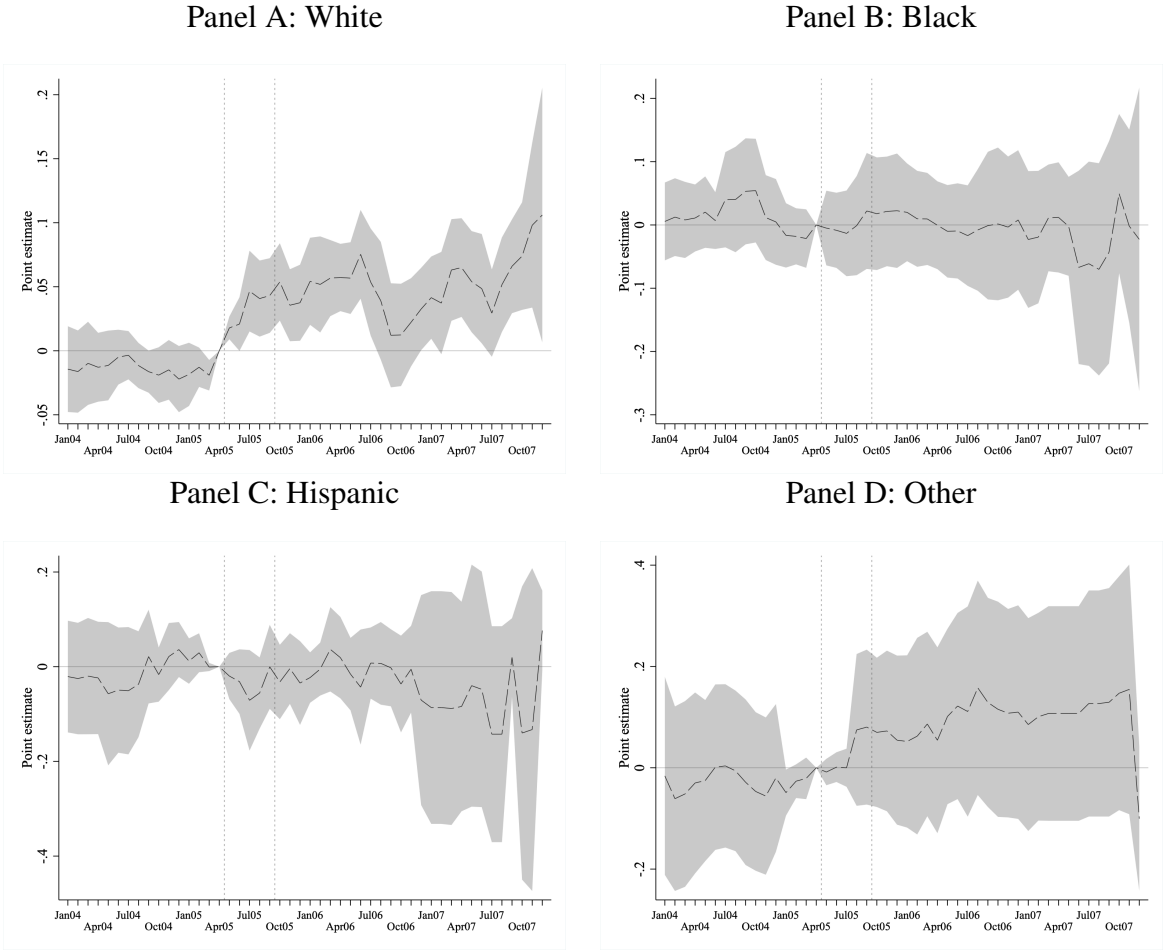
*Notes:* This table presents summary statistics for our sample during the pre-reform period, utilizing MEPS data comprising 76,926 observations. Percentages are multiplied by 100, and monetary values are denominated in thousands of 2005 dollars.

**Figure A1:** Trends in health insurance coverage for non-high-exposed households



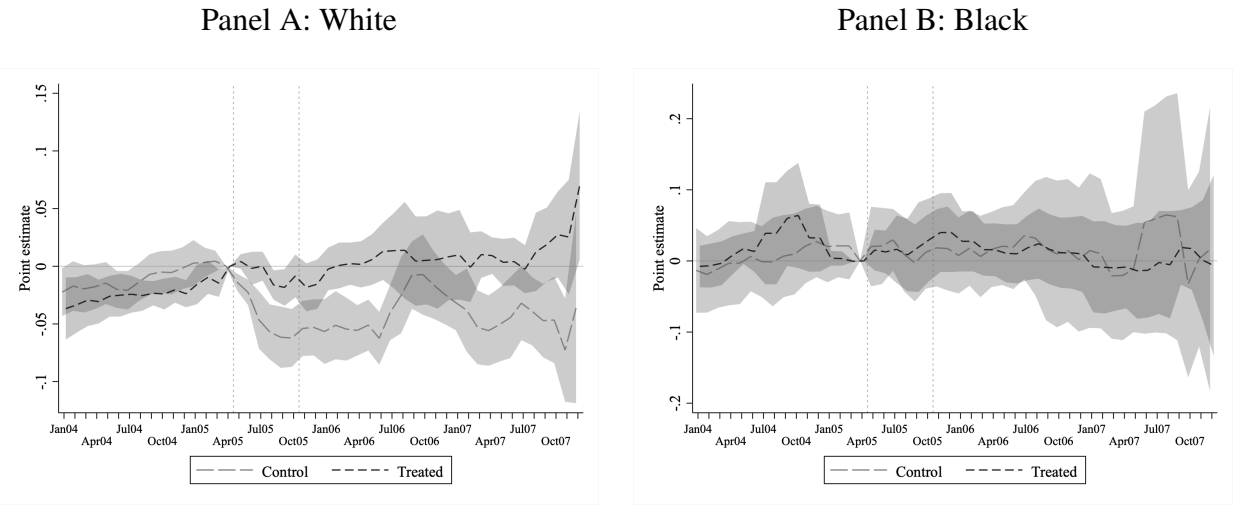

*Notes:* The figure reports the trend in insurance coverage among non-high-exposed households for the treatment and the control group conditional on household fixed effects, using SIPP data. The reference period is set to April 2005, when the reform was signed. Treated equals one if the household fails to pass the means test. Standard errors are robust against heteroskedasticity and allow for clustering at the state level. Confidence intervals are computed at a 95% level.

**Figure A2:** The effect of the reform on PHIC by race group




*Notes:* This figure illustrates the dynamic of the effect of BAPCPA on PHIC using SIPP data. The coefficients in the graph report the sum of the $\beta_1$ and $\beta_3$ from Equation 4.2 for each $month(t)$. Panel A provides estimates for the subsample of White households, Panel B for Black households, Panel C for Hispanic households, and Panel D for the residual category. Standard errors are robust against heteroskedasticity and allow for clustering at the state level. Confidence intervals are computed at a 95% level.

**Figure A3:** Trends in health insurance for high-exposed households by race




*Notes:* The figure reports the trend in insurance coverage for the treatment and the control group conditional on household fixed effects, using SIPP data. The reference period is set to April 2005, when the reform was signed. Treated equals one if the household fails to pass the means test. Panel A illustrates the data for White households, while Panel B focuses on Black households. Standard errors are robust against heteroskedasticity and allow for clustering at the state level. Confidence intervals are computed at a 95% level.

**Table A3:** Balancing table for high exposed households

| | Control | | | Treatment | | | |
|---|---|---|---|---|---|---|---|
| | n | mean | sd | n | mean | sd | Diff |
| Priv. health insurance | 22075 | 0.67 | 0.40 | 7855 | 0.86 | 0.30 | 0.184*** |
| Employer/union HI | 22075 | 0.71 | 0.46 | 7855 | 0.84 | 0.36 | 0.136*** |
| Age | 22075 | 37.36 | 9.79 | 7855 | 39.93 | 10.01 | 2.577*** |
| White | 22075 | 0.68 | 0.47 | 7855 | 0.78 | 0.42 | 0.095*** |
| Black | 22075 | 0.18 | 0.38 | 7855 | 0.14 | 0.34 | -0.042** |
| Hispanic | 22075 | 0.27 | 0.44 | 7855 | 0.16 | 0.37 | -0.106*** |
| Single | 22075 | 0.15 | 0.36 | 7855 | 0.25 | 0.43 | 0.103*** |
| Married | 22075 | 0.85 | 0.36 | 7855 | 0.75 | 0.43 | -0.103*** |
| Less than college | 22075 | 0.86 | 0.34 | 7855 | 0.75 | 0.43 | -0.116*** |
| College | 22075 | 0.14 | 0.34 | 7855 | 0.25 | 0.43 | 0.116*** |
| Cost of Ch.7 | 22075 | -3.92 | 17.71 | 7855 | -6.87 | 19.69 | -2.945** |
| Unsecured debt | 22075 | 7.20 | 16.80 | 7855 | 10.08 | 18.92 | 2.873** |
| Household income | 22075 | 45.59 | 11.57 | 7855 | 56.33 | 11.01 | 10.744*** |
| Employed | 22075 | 0.85 | 0.35 | 7855 | 0.90 | 0.30 | 0.043*** |
| Private-employed | 22075 | 0.69 | 0.46 | 7855 | 0.67 | 0.47 | -0.023 |
| Public-employed | 22075 | 0.12 | 0.33 | 7855 | 0.18 | 0.38 | 0.056*** |
| Self-employed | 22075 | 0.05 | 0.23 | 7855 | 0.05 | 0.23 | 0.000 |
| Unemployed | 22075 | 0.15 | 0.35 | 7855 | 0.10 | 0.30 | -0.043*** |

*Notes:* This table shows averages for the baseline period using SIPP data for the subsample of high-exposed households. The Diff column is the coefficient of a simple regression of treatment status on the variable. Standard errors are robust against heteroskedasticity and allow for clustering at the state level. Coefficients with $^{***}$, $^{**}$ and $^{*}$ are significant at the 1%, 5% and 10% confidence level.

**Table A4:** Heterogeneity of the effect on PHIC by race with interaction in the high-exposed sample

| | RaceGroup = White | RaceGroup = Black |
|---|---|---|
| | [1] | [2] |
| Post × Treated | 0.008 | 0.053*** |
| | (0.017) | (0.008) |
| Post × RaceGroup | -0.038*** | 0.042** |
| | (0.014) | (0.016) |
| Post × Treated × RaceGroup | 0.049** | -0.063*** |
| | (0.018) | (0.022) |
| Observations | 61,539 | 61,539 |
| *Fixed effects*: | ✓ | ✓ |

*Notes:* This table presents estimates of the reform's impact on health insurance coverage using SIPP data on the subsample of high-exposed households. Treated equals one if the household fails to pass the means test. Column 1 provides estimates for RaceGroup equal to one for White households, and Column 2 for RaceGroup equal to one for Black households. Standard errors are robust against heteroskedasticity and allow for clustering at the state level. Coefficients with ***, ** and * are significant at the 1%, 5% and 10% confidence level.

**Table A5:** Heterogeneity by race of the effect on PHIC, high-exposed sample in MEPS data

| | White | Black |
|---|---|---|
| | [1] | [2] |
| Treated | 0.039** | 0.091* |
| | (0.018) | (0.045) |
| Post × Treated | 0.104** | -0.070 |
| | (0.045) | (0.070) |
| Observations | 4,446 | 1,659 |
| *Fixed effects*: | ✓ | ✓ |

*Notes:* This table presents estimates of the reform's impact on health insurance coverage using MEPS data on the subsample of high-exposed households. Treated equals one if the household fails to pass the means test or the repayment test. Column 1 provides estimates for the subsample of White households, and Column 2 for the subsample of Black households. Standard errors are robust against heteroskedasticity and allow for clustering at the state level. Coefficients with ***, ** and * are significant at the 1%, 5% and 10% confidence level.

**Table A6:** Effect of the reform on utilization

| | Office based visits | | | Hospital outpatient visits | | | ER & hospital inpatient | |
|---|---|---|---|---|---|---|---|---|
| | Total | Physician | Non-Physician | Total | Physician | Non-Physician | ER | Inpatient |
| | [1] | [2] | [3] | [4] | [5] | [6] | [7] | [8] |
| Treated | -0.067 | -0.069 | -0.014 | 0.007 | -0.027* | 0.037 | -0.056** | -0.044** |
| | (0.046) | (0.049) | (0.066) | (0.036) | (0.015) | (0.031) | (0.022) | (0.019) |
| Post × Treated | 0.185* | 0.142 | 0.098 | 0.086** | 0.089*** | 0.011 | 0.056* | 0.057** |
| | (0.095) | (0.087) | (0.098) | (0.036) | (0.024) | (0.035) | (0.033) | (0.027) |
| Observations | 3,275 | 3,275 | 3,275 | 3,275 | 3,275 | 3,275 | 3,275 | 3,275 |
| *Fixed effects*: | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |

*Notes:* This table reports the effect of BAPCPA on medical utilization. The estimates are obtained using Equation 5.1 on MEPS data. Treated equals one if the household fails to pass the means test or the repayment test. Columns 1 to 3 detail the effect on office-based visits, distinguishing among total, physician, and non-physician. Columns 4 to 6 report the effect on hospital outpatient visits, distinguishing among total, physician, and non-physician. Column 7 shows the effect on emergency room entries, and Column 8 reports results for hospital inpatient visits. Dependent variables are expressed as log transformations. Standard errors are robust against heteroskedasticity and allow for clustering at the state level. Coefficients with ***, ** and * are significant at the 1%, 5% and 10% confidence level.

## Table A7: Effect on utilization and expenditure by race

| | Utilization | Expenditure | | | | |
|---|---|---|---|---|---|---|
| | Medical charges | Total payments | Out-of-pocket | Priv. insurance payments | OOP/Tot. payments | PIP/Tot. payments |
| | [1] | [2] | [3] | [4] | [5] | [6] |
| Panel A: White households | | | | | | |
| Treated | -0.378** | -0.225** | -0.121 | -0.130 | 0.031 | -0.011 |
| | (0.167) | (0.096) | (0.094) | (0.145) | (0.026) | (0.026) |
| Post × Treated | 0.682*** | 0.561*** | 0.392* | 0.844*** | -0.073 | 0.065* |
| | (0.237) | (0.179) | (0.218) | (0.296) | (0.046) | (0.038) |
| Observations | 1,438 | 1,438 | 1,438 | 1,438 | 1,316 | 1,316 |
| Panel B: Black households | | | | | | |
| Treated | 0.669*** | 0.581 | 0.803 | 0.699 | 0.049 | -0.003 |
| | (0.234) | (0.426) | (0.477) | (0.534) | (0.054) | (0.063) |
| Post × Treated | 0.118 | 0.085 | -0.306 | -0.370 | -0.028 | -0.036 |
| | (0.423) | (0.633) | (0.592) | (0.527) | (0.042) | (0.070) |
| Observations | 511 | 511 | 511 | 511 | 457 | 457 |
| *Fixed effects*: | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |

*Notes:* This table reports the effect of BAPCPA on medical utilization and expenditure by race. Panel A reports results for the subset of White households, while Panel B for Black households. The estimates were derived from Equation 5.1 using MEPS data. Treated equals one if the household fails to pass the means test or the repayment test. Column 1 details the effect on medical charges, Column 2 the effect on total medical payments, and Columns 3 and 4 depict the impact on out-of-pocket (OOP) and private health insurance payments (PIP). Dependent variables in Columns 1 to 4 are expressed as log transformations. Columns 5 and 6 outline the effect on the share of total payments covered by OOP and PIP, respectively. Standard errors are robust against heteroskedasticity and allow for clustering at the state level. Coefficients with ***, ** and * are significant at the 1%, 5% and 10% confidence level.

## Table A8: Screening recommendations USPSTF

| **Screening** | **Recommended Population** | **Frequency** | **Survey Question(s)** |
|---|---|---|---|
| Pap Smear | Women aged 21-65 years | Every 3 years | How long since last Pap smear test? |
| Mammography | Women aged 40-74 years | Every 2 years | How long since the last mammogram? |
| Cholesterol Check | Men over 35 and Women over 45 years | Every 5 years | How long since last blood cholesterol check by a doctor or health professional? |
| Blood Pressure Test | Adults over 18 | Every 2 years | How long since last blood pressure check? |
| Routine Check | Adults over 18 | Yearly | How long since last routine checkup by doctor or other health professional for assessing overall health? |
| Colonoscopy | Adults aged 50-75 years | Every 10 years | When was last colonoscopy? |

*Notes:* This Table illustrates the list of check-ups from Holden et al. (2015) and recommended by the US Preventive Services Taskforce (USPSTF).

# A2 Theoretical model

Before the 2005 reform, the bankruptcy system under Chapter 7 required debtors to liquidate assets above certain exemptions to pay off creditors, while their future earnings were not affected. On the other hand, Chapter 13 allowed debtors to submit a court-approved repayment plan that allocated a portion of their future income toward settling their debts. Debtors had the freedom to choose between these two options. Following Wang and White (2000), we model these chapters into a consolidated personal bankruptcy process. In this framework, debtors are required to cover a portion of their medical expenses post-bankruptcy, while taking into consideration exemptions on assets.

The model has two periods. In period one, a representative consumer has to decide the share $\alpha$ of health insurance to buy to cope with a possible health shock in period two, which occurs with probability $p$. In period one, the consumer has wealth $W_1$ and income $Y_1$. The values of $W_1$ and $Y_1$ are known in advance and certain. Consumption in period one is $C_1 = W_1 + Y_1 - P\alpha$ where $P = p \times M(1+\lambda)$ is the insurance premium: $M$ is the medical expenditure in case of health shocks and $\lambda$ measures the mark-up over the actuarial fair premium, $p \times M$. Period two consumer's wealth $W_2$ and income $Y_2$ are distributed according to a density function $g(W_2)$ and $f(Y_2)$, respectively.

At the beginning of period two, the consumer learns her wealth, $W_2$, and her income $Y_2$. She also learns if she is subject to a health shock that determines a health expenditure of $M(1-\alpha)$ where $\alpha$ is the insurance coverage that she had bought in period one.

After learning about her wealth, the consumer decides whether to file for bankruptcy. The wealth exemption, denoted by $E$, can take any non-negative value. The post-bankruptcy repayment plan leads to the remittance of a fraction $\Phi$ of medical expenditure. The cost of filing for bankruptcy is a fixed amount, $K$, that includes lawyers' and filing fees. It is reasonable to assume that $K < M(1-\alpha)(1-\Phi)$ so that, in case the individual declares bankruptcy, the gain in terms of medical expenses discharged is higher than the cost of bankruptcy. The most favorable bankruptcy policy for debtors is $\Phi = 0$ and $E = \infty$.

In period two, if the consumer does not experience a health shock, she enjoys $C_2 = W_2 + Y_2$. Alternatively, if the consumer experiences a health shock that implies a disbursement of $M$ in medical expenditure, she can decide to file for bankruptcy. In this case, she has to pay $K$ and keep all her wealth if $W_2 < E$ while giving up her nonexempt wealth, $W_2 - E$, and paying a fraction $\Phi$ of $M(1-\alpha)$ of medical expenditures if $W_2 > E$. If the consumer decides not to file for bankruptcy, she will have to repay the whole $M(1-\alpha)$. Therefore, for a level of $W_2 < E$, the consumer will always file for bankruptcy and will have no incentive to buy health insurance. For $W_2 > E$, we can compute the level of wealth that makes the consumer indifferent between filing or non-filing for

bankruptcy. That occurs whenever:

$$W_2 + Y_2 - M(1-\alpha) = E + Y_2 - K - \Phi M(1-\alpha), \tag{A2.1}$$

that is $\bar{W}_2 = E + M(1-\alpha)(1-\Phi) - K$. The higher the cost of bankruptcy $K$, the lower the health expenditure net of insurance coverage as well as the wealth exemption, the lower is the level of wealth that makes the consumer indifferent between filing and not filing for bankruptcy. In other words, for a given health expenditure $M(1-\alpha)$,a higher wealth exemption increases the incentive for consumers to file for bankruptcy. Conversely, wealthier individuals, for whom $\bar{W}_2 < W_2$, will choose not to file for bankruptcy and instead purchase health insurance. Importantly, the threshold level of wealth that determines this filing decision does not depend on income. Therefore, it is reasonable to assume that the effect of any income-based bankruptcy reform on health insurance coverage will also depend on the individual's wealth level, as wealth influences the incentive to purchase insurance.

The representative consumer's utility function is assumed to depend positively on consumption and, since consumers are risk-averse, the marginal utility of consumption declines as consumption increases.

In period one, the consumer will choose the optimal insurance coverage to maximize utility in the two periods:

$$\begin{aligned}
\max_{\alpha}\ & U(Y_1 + W_1 - \alpha P) + (1-p)\int_0^{\infty}\int_0^{\infty} U(Y_2 + W_2) f(Y_2) g(W_2) dY_2 dW_2 \\
& + p\int_0^{\infty}\int_0^{E} U(W_2 + Y_2 - M\Phi - K) f(Y_2) g(W_2) dY_2 dW_2 \\
& + p\int_0^{\infty}\int_E^{\bar{W}_2} U(E + Y_2 - M(1-\alpha)\Phi - K) f(Y_2) g(W_2) dY_2 dW_2 \\
& + p\int_0^{\infty}\int_{\bar{W}_2}^{\infty} U(W_2 + Y_2 - M(1-\alpha)) f(Y_2) g(W_2) dY_2 dW_2,
\end{aligned} \tag{A2.2}$$

taking the first order conditions w.r. to $\alpha$ we get:

$$\begin{aligned}
PU'(Y_1 + W_1 - \alpha P) = & p\left[\int_0^{\infty}\int_E^{\bar{W}_2} U'(E + Y_2 - \Phi M(1-\alpha) - K)\Phi M f(Y_2) g(W_2) dY_2 dW_2 \right. \\
& \left. + \int_0^{\infty}\int_{\bar{W}_2}^{\infty} U'(W_2 + Y_2 - M(1-\alpha)) M f(Y_2) g(W_2) dY_2 dW_2\right].
\end{aligned} \tag{A2.3}$$

The first term, on the l.h.s, is the marginal utility cost of buying one unit of insurance, while the first term on the r.h.s. is the marginal utility of buying one unit of insurance in the higher-wealth bankruptcy region. The second term on the r.h.s. measures the marginal utility of health insurance in the no-bankruptcy wealth interval. To determine the optimal health insurance coverage, we need

to consider whether the above expression has an interior solution.

Since the marginal cost of buying one unit of health insurance is increasing in $\alpha$, while the marginal utility of health insurance in the second period is decreasing in $\alpha$, for a positive probability of a health shock occurring in period two, equation A2.3 admits an interior solution and an optimal $\alpha$ in the interval (0,1). Therefore, as Figure A4 shows, it is optimal for the consumer to buy health insurance in period one.

Finally, let's consider the 2005 bankruptcy reform that introduces a level of income above which individuals cannot file for bankruptcy (means test). Define $\bar{Y_2}$ the threshold above which individuals cannot file for bankruptcy.[35] In that case, we can rewrite the problem as:

$$\begin{aligned}\max_{\alpha}\ & U(Y_1+W_1-\alpha P)+(1-p)\int_0^{\infty}\int_0^{\infty}U(Y_2+W_2)f(Y_2)g(W_2)dY_2dW_2 \\ &+p\int_0^{\bar{Y_2}}\int_0^{E}U(W_2+Y_2-M\Phi-K)f(Y_2)g(W_2)dY_2dW_2 \\ &+p\int_0^{\bar{Y_2}}\int_E^{\bar{W_2}}U(E+Y_2-M(1-\alpha)\Phi-K)f(Y_2)g(W_2)dY_2dW_2 \\ &+p\int_0^{\bar{Y_2}}\int_{\bar{W_2}}^{\infty}U(W_2+Y_2-M(1-\alpha))f(Y_2)g(W_2)dY_2dW_2 \\ &+p\int_{\bar{Y_2}}^{\infty}\int_0^{\infty}U(W_2+Y_2-M(1-\alpha))f(Y_2)g(W_2)dY_2dW_2,\end{aligned} \tag{A2.4}$$

and the first order conditions w.r. to $\alpha$ :

$$\begin{aligned}PU'(Y_1+W_1-\alpha P)=p\Bigg[&\int_0^{\bar{Y_2}}\int_E^{\bar{W_2}}U'(E+Y_2-\Phi M(1-\alpha)-K)\Phi Mf(Y_2)g(W_2)dY_2dW_2 \\ &+\int_0^{\bar{Y_2}}\int_{\bar{W_2}}^{\infty}U'(W_2+Y_2-M(1-\alpha))Mf(Y_2)g(W_2)dY_2dW_2 \\ &+\int_{\bar{Y_2}}^{\infty}\int_0^{\infty}U'(W_2+Y_2-M(1-\alpha))Mf(Y_2)g(W_2)dY_2dW_2\Bigg],\end{aligned}$$

or equivalently

[35] We are making things easier as, in reality, the 2005 reform allowed individuals whose income is below the median income to file for bankruptcy under the chapter 7 rule and, in case they cannot repay their debt, to file for bankruptcy under chapter 13 for an income above the medial income, repaying all the debt with future income

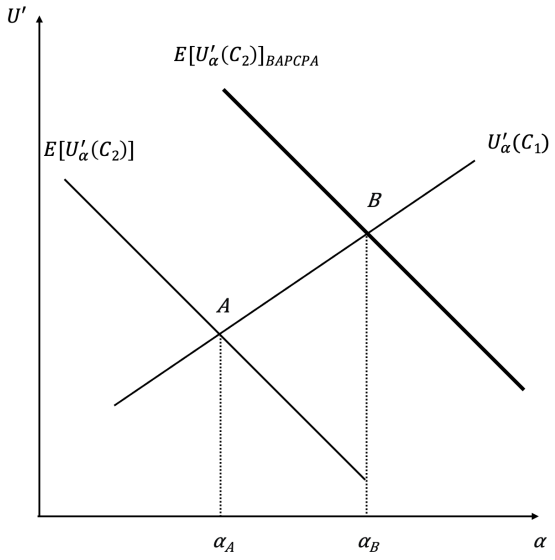
$$
\begin{aligned}
PU'(Y_1+W_1-\alpha P) = p\Bigg[&\int_0^{\bar{Y}_2}\int_E^{\bar{W}_2} U'(E+Y_2-\Phi M(1-\alpha)-K)\Phi M f(Y_2)g(W_2)dY_2dW_2 \\
&+\int_0^{\infty}\int_{\bar{W}_2}^{\infty} U'(W_2+Y_2-M(1-\alpha))Mf(Y_2)g(W_2)dY_2dW_2 \\
&+\int_{\bar{Y}_2}^{\infty}\int_0^{\bar{W}_2} U'(W_2+Y_2-M(1-\alpha))Mf(Y_2)g(W_2)dY_2dW_2\Bigg].
\end{aligned}
\tag{A2.5}
$$

To understand whether the reform affects and increases the share of insurance that is optimal for the individual, we need to compare the right-hand side of Equation A2.5 and Equation A2.3 and take the difference between the two:

$$
\begin{aligned}
&-\int_{\bar{Y}_2}^{\infty}\int_E^{\bar{W}_2} U'(E+Y_2-\Phi M(1-\alpha)-K)\Phi M f(Y_2)g(W_2)dY_2dW_2 \\
&+\int_{\bar{Y}_2}^{\infty}\int_0^{\bar{W}_2} U'(W_2+Y_2-M(1-\alpha))Mf(Y_2)g(W_2)dY_2dW_2\Bigg] = 0,
\end{aligned}
\tag{A2.6}
$$

which is positive.

**Figure A4:** Equilibrium level of $\alpha$ pre and after the BAPCPA




*Notes:* This figure illustrates the optimal insurance coverage demanded by the consumer in the case of no bankruptcy reform (point *A*) and after BAPCPA (*B*)

Therefore, following the reform, the marginal utility of purchasing health insurance rises for individuals with income exceeding $\bar{Y}_2$ and whose seizable assets are zero. The reform primarily impacts these individuals, and in the paper, we test this prediction in the data. Our findings are

illustrated in Figure A4, where the marginal expected utility curve for insurance shifts rightward after the reform. The shift causes the equilibrium point to move to $B$, which is associated with a higher optimal level of insurance coverage, denoted by $\alpha_B$.

# A3 Data and cleaning

This section provides further details about the data cleaning process and the construction of the variables used in the analysis.

**Seizable assets** – Following Mahoney (2015), we characterize home and vehicle equity using specific variables. Retirement assets include the value held in IRA, Keogh, or 401K plan. Financial assets are defined to include equity in farms or businesses, equity in other real estate, equity in a second home, equity in recreational vehicles, the value of CDs, stocks, government or corporate bonds, mutual funds, as well as the value in checking or savings accounts, and other assets.

After the reform, households may qualify for Chapter 7 if their seizable income falls below a certain threshold. According to the definitions provided by Elias and Bayer (2013) and Mahoney (2015), seizable income is calculated by subtracting expense allowances for food and clothing, mortgage payments or rent, home and cellular telephones, transportation, insurance, and taxes from household income. The Department of Justice website provides details on these expense allowances. Food and clothing expenses are adjusted based on household size, while transportation expenses vary according to the number of vehicles and the region of residence. Mortgage payments and rent expense allowances depend on household size and county of residence. However, since we lack information on the county of residence, we assign each household the average expense allowance for their state of residence.[36]

**Financial cost of bankruptcy** – Since debt repayment under Chapter 13 must be higher than in Chapter 7, households encounter a lower financial cost when filing under Chapter 7 (Mahoney, 2015). Consequently, we can use the financial cost associated with Chapter 7 to characterize Chapter 13 as well, as suggested by (Fay et al., 2002). In line with Mahoney (2015), we formulate the pre-reform financial cost of bankruptcy as:

$$w_{pre}(w_i t, e_s) = \text{seizable assets}(w_i, e_s) - \text{dischargeable debt}(w_i) + \text{filing cost} \tag{A3.1}$$

In the equation, $w_{it}$ represents the assets (and debts) for household $i$ in year $t$, and $e_s$ denotes the exemptions applicable in state $s$. The filing cost, inclusive of an estimate for attorney fees, is fixed at \$2,000 according to (Elias and Bayer, 2013). The seizable assets are calculated as the total of

[36] For updated data on allowances and median income, refer to https://www.justice.gov/ust/means-testing/means-testing-cases-filed-between-january-1-2008-and-january-31-2008-inclusive.

assets exceeding the exemption level in each specific asset category.[37]

The post-BAPCPA financial cost of bankruptcy is instead a function of the mean test as described in Section 2:

$$w_{post} = \begin{cases} w_{pre} \quad \text{if passes means or repayment test} \\ max\{w_{pre}, w^f_{post} = 5 \times \text{seizable income} - \text{dischargable debt} + \text{filing cost}\} \end{cases} \tag{A3.2}$$

households meeting the means or repayment test face the same financial costs before and after the reform. Conversely, households ineligible for these tests make payments equivalent to their disposable income over five years, resulting in a post-BAPCPA financial cost denoted as $w^f_{post}$. As households must pay at least as much as they would under Chapter 7, their post-BAPCPA financial cost of bankruptcy becomes the higher value between $w_{pre}$ and $w^f_{post}$.

# A4 Robustness checks

This section presents a set of robustness checks and additional results in support of our preferred specification.

**Insurance coverage on MEPS data** – We start by reporting the estimates of the effect of BAPCPA on insurance coverage using MEPS data. Table A9 reports the results.

**Table A9:** The effect of the reform on PHIC of high-exposed households using MEPS data

| | [1] | [2] | [3] | [4] |
|---|---|---|---|---|
| Treated | 0.080*** | 0.086*** | 0.085*** | 0.068*** |
| | (0.017) | (0.017) | (0.018) | (0.020) |
| Post × Treated | 0.086*** | 0.085*** | 0.084*** | 0.085*** |
| | (0.024) | (0.028) | (0.028) | (0.028) |
| Avg. Treated | 80.5 | 80.5 | 80.5 | 80.5 |
| Observations | 9,943 | 9,943 | 9,943 | 9,943 |
| *Covariates*: | | | | |
| Demographics FE | ✓ | ✓ | ✓ | ✓ |
| State FE | ✓ | ✓ | ✓ | ✓ |
| Year FE | ✓ | ✓ | ✓ | ✓ |
| State-Year FE | | ✓ | ✓ | ✓ |
| Employment | | | ✓ | ✓ |
| Income$^2$ | | | | ✓ |

*Notes:* This table illustrates the estimates of the effect of the reform on health insurance coverage using MEPS data on the subsample of high-exposed households. Treated equals one if the household fails to pass the means test or the repayment test. Column 1 incorporates year, state, and demographics fixed effects. Column 2 introduces state-year fixed effects. Column 3 controls for employment status, while Column 4 adjusts for a second-degree polynomial in income. Standard errors are robust against heteroskedasticity and allow for clustering at the state level. Coefficients with ***, ** and * are significant at the 1%, 5% and 10% confidence level.

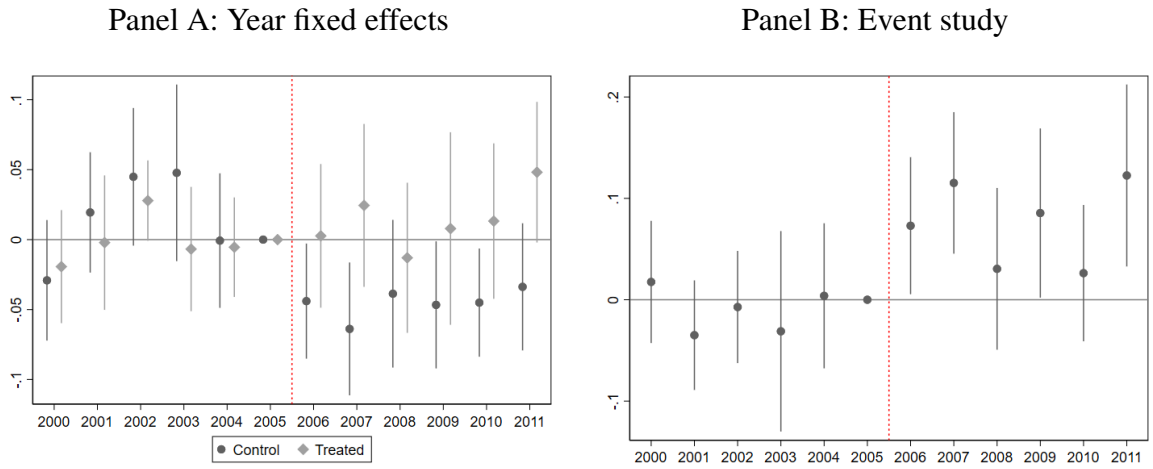
[37] For more details on the construction of seizable assets see Mahoney (2015).

Focusing on our preferred specification in Column 2, the treatment is associated with a rise in PHIC by approximately 8.5 percentage points. In comparison to the estimates outlined in Table 2, this effect is roughly twice as large. The relative size of the two estimates aligns with the variance in magnitude observed in Mahoney (2015) between SIPP and MEPS data regarding the impact of seizable assets on PHIC.

Overall, the two datasets provide a useful robustness check, but there are several reasons why they may yield different magnitudes. Both SIPP and MEPS offer nationally representative measures of health insurance coverage; however, estimates from the two sources can differ due to differences in survey design and data collection methods (Graves and Mishra, 2016). SIPP typically follows respondents for a longer period but collects insurance information less frequently, historically every four months. In contrast, MEPS interviews respondents five times over two years and records their monthly insurance status. Variations in panel length, follow-up frequency, validation procedures, and weighting adjustments can therefore lead to systematic differences in measured coverage rates and spell durations across the two datasets (Graves and Mishra, 2016).

Figure A5 exploits the repeated cross-section structure of the data to extend the analysis over a more extended time frame (2000-2011). The dynamic shows that the effect is persistent over time, and also in this case, it is mostly driven by a drop in PHIC in the high-exposed control group (Panel A).

**Figure A5:** Health insurance coverage for high-exposed households using MEPS data




*Notes:* The chart depicts the insurance coverage trend among high-exposed households in the treatment and control groups, using MEPS data. Treated equals one if the household fails to pass the means test or the repayment test. Panel A displays year fixed effects. Panel B presents the coefficient of the interaction $Treated \times Year$. The reference period is defined as 2005, corresponding to the signing of the reform. Standard errors are robust against heteroskedasticity and allow for clustering at the state level. Confidence intervals are computed at a 95% level.

**Matched sample** – A potential concern to our primary approach is the dissimilarity in sociodemographic characteristics and initial insurance coverage levels between households in the

treatment and control groups (as shown in Table A3). Consequently, the assumption that both groups experienced the same aggregate shock, with corresponding parallel outcomes in the absence of treatment, becomes less plausible.

**Table A10:** Balancing table for high-exposed in our matched sample

| | Control | | | Treatment | | | |
|---|---|---|---|---|---|---|---|
| | n | mean | sd | n | mean | sd | Diff |
| Priv. health insurance | 4959 | 0.84 | 0.30 | 7855 | 0.89 | 0.27 | 0.044** |
| Employer/union HI | 4959 | 0.90 | 0.30 | 7855 | 0.87 | 0.34 | -0.032 |
| Age | 4959 | 42.48 | 11.25 | 7855 | 40.20 | 10.47 | -2.282 |
| White | 4959 | 0.78 | 0.42 | 7855 | 0.79 | 0.41 | 0.010 |
| Black | 4959 | 0.16 | 0.36 | 7855 | 0.14 | 0.35 | -0.017 |
| Hispanic | 4959 | 0.05 | 0.22 | 7855 | 0.09 | 0.28 | 0.033 |
| Single | 4959 | 0.24 | 0.42 | 7855 | 0.44 | 0.50 | 0.206*** |
| Married | 4959 | 0.76 | 0.42 | 7855 | 0.56 | 0.50 | -0.206*** |
| Less than college | 4959 | 0.55 | 0.50 | 7855 | 0.68 | 0.46 | 0.134 |
| College | 4959 | 0.45 | 0.50 | 7855 | 0.32 | 0.46 | -0.134 |
| Cost of Ch.7 | 4959 | -6.54 | 24.77 | 7855 | -7.30 | 23.12 | -0.757 |
| Unsecured debt | 4959 | 10.94 | 23.42 | 7855 | 10.64 | 22.21 | -0.299 |
| Household income | 4959 | 45.55 | 11.31 | 7855 | 54.79 | 11.37 | 9.243*** |
| Employed | 4959 | 0.91 | 0.29 | 7855 | 0.91 | 0.29 | 0.003 |
| Private-employed | 4959 | 0.69 | 0.46 | 7855 | 0.70 | 0.46 | 0.008 |
| Public-employed | 4959 | 0.19 | 0.39 | 7855 | 0.18 | 0.38 | -0.015 |
| Self-employed | 4959 | 0.03 | 0.17 | 7855 | 0.04 | 0.20 | 0.010 |
| Unemployed | 4959 | 0.09 | 0.29 | 7855 | 0.09 | 0.29 | -0.002 |

*Notes:* This table shows averages for the baseline period using SIPP data for the matched sample of high-exposed households. The Diff column is the coefficient of a simple regression of treatment status on the variable. Standard errors are robust against heteroskedasticity and allow for clustering at the state level. Coefficients with $^{***}$, $^{**}$ and $^{*}$ are significant at the 1%, 5% and 10% confidence level.

To address this concern, we employed a combination of propensity score matching and difference-in-differences techniques. Specifically, we conducted a nearest neighbor matching process during the pre-2005 period and then reweighted the observations to create treatment and control groups that exhibit greater similarity in sociodemographic characteristics. The summary statistics for the matched sample are presented in Table A10. Notably, due to our methodology, we observe a greater alignment in most characteristics across the two groups, and the average insurance coverage is now also more comparable.

Table A11 presents the resulting estimates. The impact of the reform remains positive and quantitatively consistent with the findings in Table 2, albeit with larger standard errors, primarily attributed to a consistent reduction in sample size.

**Sample robustness** – In this robustness check, we examine how the sample selection that we operate in our analysis affects our results. Table A12 presents the estimates of Equation 4.1 under different sample conditions. Specifically, we report the estimates in Column 1 when we exclude observations from the states of Massachusetts and Vermont, where mandatory insurance coverage was introduced around BAPCPA period. In Column 2, we include households with members covered by public health insurance in the sample. Column 3 includes households that transition in and out of the treatment group, while Column 4 incorporates households that experience changes

**Table A11:** BAPCPA and health insurance coverage, matched sample

| | [1] | [2] | [3] | [4] |
|---|---|---|---|---|
| Post × Treated | 0.007 | 0.006 | -0.000 | 0.000 |
| | (0.005) | (0.005) | (0.005) | (0.004) |
| Post × HighExp | 0.002 | 0.001 | -0.001 | -0.002 |
| | (0.028) | (0.028) | (0.029) | (0.029) |
| Post × Treated × HighExp | 0.020 | 0.020 | 0.021 | 0.021 |
| | (0.028) | (0.028) | (0.029) | (0.029) |
| Observations | 275,181 | 275,181 | 275,181 | 275,181 |
| *Covariates*: | | | | |
| Household FE | ✓ | ✓ | ✓ | ✓ |
| Year FE | ✓ | ✓ | ✓ | ✓ |
| Employment | | ✓ | ✓ | ✓ |
| Income$^2$ | | | ✓ | ✓ |
| Income$^4$ | | | | ✓ |

*Notes:* This table illustrates the estimates of the effect of the reform on health insurance coverage using our matched sample. Treated equals one if the household fails to pass the means test. Column 1 incorporates household and month-year fixed effects. In Column 2, a dummy variable for employment is introduced. Column 3 adjusts for a second-degree polynomial in income, while Column 4 includes controls for a fourth-degree polynomial in income. Standard errors are robust against heteroskedasticity and allow for clustering at the state level. Coefficients with $^{***}$, $^{**}$ and $^{*}$ are significant at the 1%, 5% and 10% confidence level.

in composition during the sample period.

**Table A12:** Effect on health insurance coverage: Sample robustness

| | Excluding | Including households | | |
|---|---|---|---|---|
| | MA & VT | W/ Pub. ins. | Not-always tr. | # person change |
| | [1] | [2] | [3] | [4] |
| Post × Treated | 0.000 | -0.010$^{***}$ | 0.002 | 0.008 |
| | (0.005) | (0.003) | (0.005) | (0.005) |
| Post × HighExp | -0.017$^{**}$ | -0.014$^{**}$ | -0.015$^{**}$ | 0.005 |
| | (0.007) | (0.007) | (0.007) | (0.008) |
| Post × Treated × HighExp | 0.045$^{***}$ | 0.037$^{***}$ | 0.055$^{***}$ | 0.020$^{*}$ |
| | (0.010) | (0.010) | (0.012) | (0.011) |
| Observations | 458,546 | 816,916 | 530,979 | 667,456 |
| *Fixed effects*: | ✓ | ✓ | ✓ | ✓ |

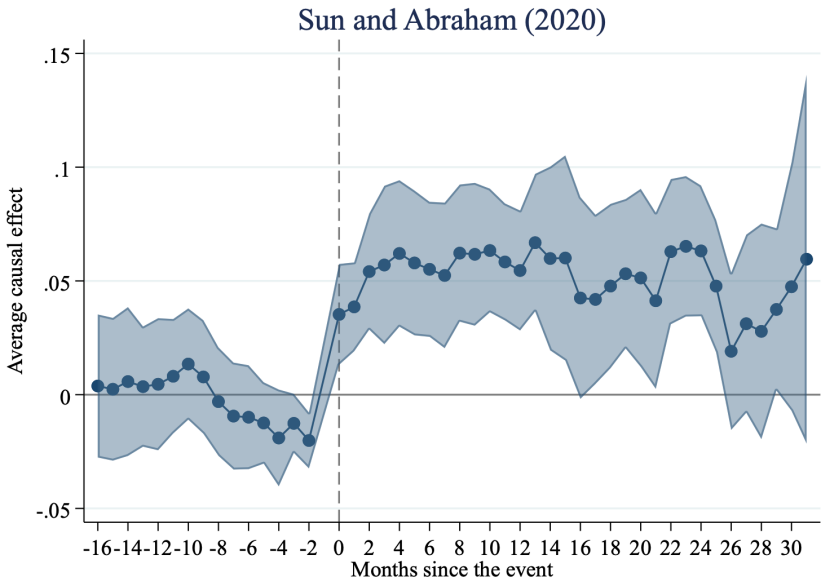
*Notes:* This table presents estimates depicting the impact of the reform on health insurance coverage, utilizing SIPP data with various sample selections. Treated equals one if the household fails to pass the means test. HighExp equals one for households with no seizable assets and income between $25000 and $75000. In Column 1, the states of Massachusetts and Vermont are excluded. Column 2 incorporates households with members covered by public health insurance, while Column 3 includes households that experience fluctuations around the means test threshold in the post period. Column 4 includes households that alter their composition during our sample period. Standard errors are robust against heteroskedasticity and allow for clustering at the state level. Coefficients with $^{***}$, $^{**}$ and $^{*}$ are significant at the 1%, 5% and 10% confidence level.

The table shows that the estimates align with the findings presented in Table 2. In all columns, the coefficient for high-exposure households remains consistently positive and statistically significant.[38]

[38] Notably, the final column incorporates households that undergo structural changes over time, and this variation might be correlated with the measurement of coverage calculated at the household level.

**Staggered diff-in-diff** – While the reform was simultaneously introduced throughout the United States, the alteration in Chapter 7 eligibility could impact households at various points in time, resulting in a *de facto* staggered implementation. Figure A6 presents the event-study analyzing the impact of Chapter 7 non-eligibility on health insurance coverage, utilizing the methodology outlined by Sun and Abraham (2021). As illustrated in the graph, the findings align closely with those reported in Figure A6.

**Figure A6:** BAPCPA and coverage of high-exposed households (Sun and Abraham, 2021)




*Notes:* This figure illustrates the dynamic of the effect of BAPCPA on PHIC using SIPP data. The graph is obtained employing the methodology outlined by Sun and Abraham (2021). Standard errors are robust against heteroskedasticity and allow for clustering at the state level. Confidence intervals are computed at a 95% level.

**Different control group** – In this paragraph, we repeat our analysis of the effect of the reform on health insurance coverage, focusing only on the treatment group, hence households that fail the means test. Treatment and control groups in this case will be defined on the intensity of the treatment measured by our *highExp* dummy, which will be one for the treated and zero for the control. Table A13 shows the results. Also in this case, the coefficient is positive and significant at 5% throughout the table.

**Income and seizable assets** – Table A14 reports the estimates using different definition of high exposed. Column 1 reports the definition adopted throughout the paper. Column 2 refines this definition by including only households falling within the income bracket as defined by Fisher (2019), provided they possess zero net seizable assets (calculated as seizable assets minus dischargeable debt). Columns 3 and 4 display the outcomes when high exposure is determined exclusively based

**Table A13:** BAPCPA and insurance coverage: Other control group.

| | [1] | [2] | [3] | [4] |
|---|---|---|---|---|
| Post × HighExp | 0.027*** | 0.027*** | 0.025*** | 0.023*** |
| | (0.007) | (0.007) | (0.008) | (0.008) |
| Observations | 234,749 | 234,749 | 234,749 | 234,749 |
| *Covariates*: | | | | |
| Household FE | ✓ | ✓ | ✓ | ✓ |
| Time FE | ✓ | ✓ | ✓ | ✓ |
| Employment | | ✓ | ✓ | ✓ |
| Income$^2$ | | | ✓ | ✓ |
| Income$^4$ | | | | ✓ |

*Notes:* This table presents estimates depicting the impact of the reform on health insurance coverage, utilizing SIPP data on the subsample of treated households (households who fail the means test). HighExp equals one for households with no seizable assets and income between $25000 and $75000.Standard errors are robust against heteroskedasticity and allow for clustering at the state level. Coefficients with ***, ** and * are significant at the 1%, 5% and 10% confidence level.

on income or seizable assets, respectively. Also in this case, the results are consistent with the analysis reported in the main text.

**Table A14:** Effect on health insurance coverage: High exposure selection

| | Median inc. & gross SA | Median inc. & net SA | Median inc. only | Gross SA only |
|---|---|---|---|---|
| | [1] | [2] | [3] | [4] |
| Post × Treated | 0.001 | 0.003 | -0.005 | 0.001 |
| | (0.005) | (0.005) | (0.005) | (0.005) |
| Post × HighExp | -0.015** | -0.016 | -0.015*** | -0.005 |
| | (0.007) | (0.016) | (0.004) | (0.005) |
| Post × Treated × HighExp | 0.042*** | 0.035* | 0.023*** | 0.023*** |
| | (0.010) | (0.019) | (0.006) | (0.008) |
| Avg. Treated | 94.1 | 94.1 | 94.1 | 94.1 |
| Avg. HighExp | 85.8 | 85.8 | 85.8 | 85.8 |
| Observations | 471,438 | 471,438 | 471,438 | 471,438 |
| *Fixed effects*: | ✓ | ✓ | ✓ | ✓ |

*Notes:* able A5 reports the estimates using different definition of high exposed.Treated equals one if the household fails to pass the means test. Column 1 reports the definition adopted throughout the paper. Column 2 refines this definition by including only households falling within the income bracket as defined by Fisher (2019), provided they possess zero seizable assets. Columns 3 and 4 display the outcomes when high exposure is determined exclusively based on income or seizable assets, respectively. Standard errors are robust against heteroskedasticity and allow for clustering at the state level. Coefficients with ***, ** and * are significant at the 1%, 5% and 10% confidence level.

**Changing the baseline period** – Table A15 reports regression coefficients, using October 2005 as the time threshold in Column 1 and excluding the turbulent pre-reform period (April–October 2005) in Column 2. The results indicate that our estimates are highly consistent with those from the main specification.

**Table A15:** The effect of the reform on health insurance coverage using different time thresholds

| | Post = 1 after October 2005 | Excluding April to October 2005 |
|---|---|---|
| | [1] | [2] |
| Post × Treated | 0.001 | 0.003 |
| | (0.005) | (0.006) |
| Post × HighExp | -0.011 | -0.018** |
| | (0.007) | (0.008) |
| Post × Treated × HighExp | 0.034*** | 0.049*** |
| | (0.009) | (0.012) |
| Observations | 471,438 | 377,186 |
| *Covariates*: | | |
| Household FE | ✓ | ✓ |
| Time FE | ✓ | ✓ |

*Notes:* This table illustrates the estimates of the effect of the reform on health insurance coverage using SIPP data. Treated equals one if the household fails to pass the means test. HighExp equals one for households with no seizable assets and income between $25000 and $75000. Column 1 post equals one after October 2005. In Column 2, we exclude observations between April and October 2005. All regressions incorporate household and month-year fixed effects. Standard errors are robust against heteroskedasticity and allow for clustering at the state level. Coefficients with ***, ** and * are significant at the 1%, 5% and 10% confidence level.