FEDERAL RESERVE BANK
PHILADELPHIA

Working Papers
RESEARCH DEPARTMENT

# Hospital Billing Regulations and Financial Well-Being

## Evidence from California's Fair Pricing Law


**Yaa Akosa Antwi**
Johns Hopkins University and
Federal Reserve Bank of Philadelphia
Consumer Finance Institute Visiting Scholar

**Marion Aouad**
University of California, Irvine and
Federal Reserve Bank of Philadelphia
Consumer Finance Institute Visiting Scholar

**Nathan Blascak**
Federal Reserve Bank of Philadelphia
Consumer Finance Institute



WP 25-39

PUBLISHED
December 2025

FRB CFI
PHILADELPHIA

**ISSN:** 1962-5361





**DOI:** https://doi.org/10.21799/frbp.wp.2025.39

# Hospital Billing Regulations and Financial Well-Being: Evidence from California's Fair Pricing Law*

Yaa Akosa Antwi† Marion Aouad‡ Nathan Blascak§

November 24, 2025

**Abstract**

We examine the financial consequences of the 2007 California Fair Pricing Law, which places a price ceiling on hospital bills for financially vulnerable individuals. Exploiting cross-sectional variation in exposure to the law, we estimate its impact on individual financial distress. We find that the law reduces the likelihood of incurring non-medical debt in collections by 19.8 percent and the number of non-medical collections by 39 percent for an individual living in a county with average exposure in California. In addition, we find suggestive evidence that the number of delinquent accounts decreased for exposed individuals. Our results suggest hospital billing regulations can improve targeted individuals' financial outcomes.

*Keywords*: financial distress, consumer credit, hospitals, health care

*JEL Codes*: G51, I18, H75

*The authors would like to thank Thanh Lu, Sarah Miller, Bob Hunt, Julia Cheney, and seminar participants at the University of Virginia Frank Batten School of Leadership and Public Policy, the University of Wisconsin-Madison Center for Financial Security, the Boulder Summer Conference on Consumer Financial Decision Making, the American Society of Health Economists Conference, the Southern Economic Association Annual Meeting, and the Association for Public Policy and Management Fall Research Conference for their helpful comments. Claire Jellison provided excellent research assistance.

†Johns Hopkins University, Carey School of Business, 100 International Drive, Baltimore, MD 21202, United States of America. Email: y.akosa.antwi@jhu.edu

‡University of California, Irvine, Department of Economics, 3151 Social Sciences Plaza, Irvine, CA 92697, United States of America. Email: marion.aouad@uci.edu

§Federal Reserve Bank of Philadelphia, Consumer Finance Institute, 10 Independence Mall, Philadelphia, PA 19106, United States of America. Email: nathan.blascak@phil.frb.org (corresponding author)

# 1 Introduction

Insurance contracts provide a form of income smoothing by allowing for the transfer of income across states of being (Rothshild and Stiglitz, 1976). Thus, in principle, health insurance should mitigate the financial risks associated with poorer health states and the costs associated with increases in health-care consumption. However, the link between insurance generosity and financial risk protection has become increasingly tenuous, given the growth in insurance products and health insurance reforms that increasingly expose individuals to a greater share of their health-care expenses (e.g., Brot-Goldberg et al. (2017), Aouad et al. (2019)). As such, there is a growing body of research that investigates the relationship between health-care utilization and financial outcomes (e.g., Dobkin et al. (2018), Gross and Notowidigdo (2011)).

Yet, there is relatively little known about the mediating effects of *legislation* that specifically aims to shield consumers from expensive medical events. This has become increasingly important to understand because unanticipated, expensive medical bills can have serious financial consequences for individuals residing in the US (Canilang et al. (2020)).[1] Furthermore, the estimated share of non-elderly US individuals who are uninsured, and therefore highly exposed to expensive medical events, remains high at approximately 11.8 percent as of 2022 (Lee et al., 2022) and is likely to further grow given proposed cuts to Medicaid funding (Euhus et al., 2025). Given the strong link between personal finances and measures of well-being, such as health (e.g., Schwandt (2018), Engelberg and Parsons (2016), and Lindahl (2005)), it is crucial to understand what policies, aside from health-insurance expansions, can impact financial well-being.

In this paper, we examine how the introduction of the *California's Hospital Fair Pricing Act*, commonly referred to as the California Fair Pricing Law (CA FPL), affects individuals' financial outcomes. Specifically, we trace the effects of exposure to this law among those most likely to be eligible across several measures of financial distress. The CA FPL, enacted in 2007, limits the amount that lower-income uninsured patients and lower-income insured

[1] For example, a Kaiser Family Foundation (KFF) analysis found that, "Adults who were uninsured for more than six months in a year are more likely to report having significant medical debt (13 percent) than those who were insured the full year or uninsured for half of the year or less (9 percent)." (Rae et al. (2022)).

patients experiencing "burdensome" medical bills face after visiting a hospital.[2] The law also requires the adoption of "formal, written financial assistance policies," which must be clearly advertised in the hospital (Melnick and Fonkych, 2013). While versions of the FPL exist in other states, our analysis focuses on California because of its population size and demographic background, both of which lend well to extrapolation to different settings.

To study the effect of the CA FPL on individual financial outcomes, we use the Federal Reserve Bank of New York Consumer Credit Panel/Equifax data (CCP), which is a 5 percent anonymized random sample of individuals with credit bureau records. We merge the CCP with a unique data set that contains detailed anonymized information on medical debt reported to Equifax by third-party debt collectors to examine several personal financial outcomes around the time of the policy's enactment.

Using data from 2003 to 2010, we estimate difference-in-difference-in-differences (DDD) models across California and its neighboring states, comparing, over time, individuals living in counties that have varying levels of pre-policy "exposure" to the FPL (i.e., pre-policy uninsured rates). Specifically, in California and its neighboring states, we compare outcomes for younger adults, ages 18 to 39, who live in areas with different rates of health-insurance coverage before and after the implementation of the FPL.[3]

We find that the CA FPL impacted *non*-medical debt in collections for younger adults. In particular, we find that exposure to the CA FPL decreases the probability of incurring any non-medical debt in collections in the past 12 months, by 2.1 percentage points (a relative 19.8 percent decrease), and decreases the number of non-medical accounts sent to collections by 0.057 accounts (a relative 39 percent decrease) for a young adult living a county in California with an average level of exposure. Alternatively, we can interpret these results as showing that a 10 percentage point increase in exposure leads to declines in the probability of having any non-medical collections and the number of non-medical collections by 4.7 percent and 9.3 percent, respectively. This is consistent with the findings from Argys et al. (2020), who also find changes in non-medical accounts after Tennessee's 2007 Medicaid

[2] In particular, if a medical bill exceeds 10 percent of an insured individual's annual income, if their income is at or below 350 percent of the federal poverty line, they are eligible for protections under the California Fair Pricing Law.

[3] This approach is similar to that of Finkelstein (2007), Miller (2012), Mazumder and Miller (2016), and Courtemanche et al. (2017).

reform. We also find that the introduction of the CA FPL did not have a significant effect on the likelihood that young adults incur any *medical* debt in collection nor on the likelihood that young adults have a $0 medical collection balance. We discuss possible reasons for these findings later in the Discussion section.

Consistent with the extensive margin results for medical collections, we find small, transitory effects of the law on the *number* of medical accounts sent to third-party collections in the past year. This suggests that the law operates by changing the distribution of medical debt *balances* rather than reducing the number of delinquent medical accounts for younger adults. We also examine three additional measures of financial health: credit scores, the percentage of all debt that is delinquent, and the total number of delinquent accounts.[4] After the introduction of the CA FPL, we find suggestive evidence that, for exposed individuals, credit scores improved within the first year of the law taking effect, and delinquencies decreased, consistent with our collection results. Taken together, these results imply that hospital billing legislation can provide financial protections to financially vulnerable populations and to consumers who have "thin" health-insurance coverage.

Our paper contributes to the growing literature that examines the relationship between health-care access/utilization and personal finances. The literature has examined how health-care utilization affects financial outcomes (Dobkin et al., 2018), the importance of liquidity for health-care utilization (Gross and Notowidigdo, 2011), and more recently, how the availability of public health-insurance options affect financial outcomes (e.g., Brevoort et al. (2020), Hu et al. (2018), Mazumder and Miller (2016), and Barcellos and Jacobson (2015)). These studies have found that interactions with the health-care system can be costly in both the shorter and longer run. The studies have also found that access to health insurance improves measures of financial well-being.

Additionally, several studies examine the effects of hospital billing policies on health-care utilization. For example, Batty and Ippolito (2017a), Bai (2015), and Melnick and Fonkych (2013) examine the impacts of fair pricing laws. Batty and Ippolito (2017a) find that fair pricing laws reduce the medical payments of the uninsured, resulting in lower

[4]The credit score measure we use is the Equifax Risk Score, a proprietary credit score produced by Equifax that is similar to other risk scores used in the industry.

provisions of health care by hospitals. Adams et al. (2022) examine the impact of Kaiser Permanente's financial assistance program on qualifying patients' health-care utilization and find that financial assistance programs can promote the consumption of high-value health care. In contrast to our work, these studies largely focus on health-care utilization effects (i.e., how much health care is provided by hospitals or consumed by patients after the policy is introduced) or price effects (i.e., what price do the uninsured face when visiting the hospital after the policy is introduced).

The novelty of our study is that we investigate how hospital billing regulations, such as the CA FPL, affect the shorter- to medium-run *financial outcomes* of those likely to be uninsured or financially vulnerable. Moreover, we examine a setting where the government, via legislation, directly affects the prices charged by health-care providers. This differs from the typical setting where an intermediary, such as a health insurer, negotiates prices with providers. Thus, we are able to observe the impacts of price ceilings set by the government and their effects on consumers' financial well-being. This examination may be useful for understanding the financial impacts of other policies, such as changes to the federal government's ability to negotiate drug prices for select medications as part of the 2022 *Inflation Reduction Act* (Cubanski et al., 2023).

Additionally, while a reduction in the prices paid for health care likely impacts financial outcomes, we quantify these effects over time. Furthermore, by making use of detailed individual-level credit bureau data, we can trace the impacts of the law over a number of relevant financial outcomes. This allows us to capture financial spillover effects of the law (e.g., reductions in other types of debt). As such, this is the first study, known to the authors, to examine how legislative changes to hospital billing regulations affect the personal financial outcomes of consumers.

The rest of our paper will proceed as follows: Section 2 discusses the law's background, Section 3 examines CA hospitals' revenues around the time of policy adoption, while Section 4 presents the conceptual framework. Section 5 describes our data, and Section 6 discusses our methods. We present our results and discussion in Sections 7 and 8 and conclude in Section 9.

## 2 Policy Background

The California Fair Pricing Law (CA FPL), Bill AB 774, was passed in 2006 and became effective on January 1, 2007. One of its main provisions restricted the amount hospitals could charge patients. Specifically, patients are protected by the law if their family income is at or below 350 percent of the federal poverty level and if they either i) are uninsured or ii) incur medical expenses that are greater than 10 percent of their household income (California Assembly, 2007).[5]

After the reform was passed, hospital charges were limited to the highest amounts that would be received from government programs, such as Medicare or the state Medicaid program, Medi-Cal (Melnick and Fonkych, 2013). The two groups were also to be made eligible for the hospital's charity care (California Assembly, 2007). Prior to the passage of the law, hospitals could charge uninsured patients the "chargemaster" price, which tended to be higher than the prices negotiated with health insurers (Batty and Ippolito, 2017b). Additionally, for the two eligible groups, hospitals could not "report adverse information to a consumer credit reporting agency or commence civil action against the patient for nonpayment at any time prior to 150 days after initial billing" (California Assembly (2007)).

In addition to these billing regulations, hospitals had to develop written, financial assistance policies that were clearly advertised. The law also put restrictions on hospital collection practices, for example, limiting the use of wage garnishments and sending unpaid bills to collection agencies for CA FPL-eligible patients who are making "good faith" attempts to pay their bills (Office of Statewide Health Planning and Development, 2021).[6]

We focus on the changes to billing regulations imposed by the law and the adoption/advertisement of financial assistance (charity) policies. Both policies will likely have large impacts on the uninsured and financially vulnerable population. The former likely includes many individuals between the ages of 18 and 39, given their traditionally lower rates of

[5] For context, in 2007, the federal poverty guidelines were, respectively, \$10,210, \$13,690, \$17,170, \$20,650, and \$32,470 for households of sizes one, two, three, four, and five (Office of the Assistant Secretary for Planning and Evaluation (2007)). Meanwhile, California 2006 median incomes were \$29,576, \$58,752, \$65,010, \$75,000, and \$65,000, for households of size one, two, three, four, and five, respectively (CA Demographic Research Unit (2009)).

[6] Further details about the provision of the law can be found in the California Health and Safety Code, Chapter 2.5 of Division 107.

health-insurance coverage, particularly during this time period. As such, this group is likely to be impacted by the price changes induced by the law since there are sizable differences in hospitals' chargemaster prices and the amount billed to insurers (e.g., Anderson (2007) and Bai and Anderson (2015)), but this group may also benefit from the charity care policies.

# 3 Hospital Revenue and Patient Payments

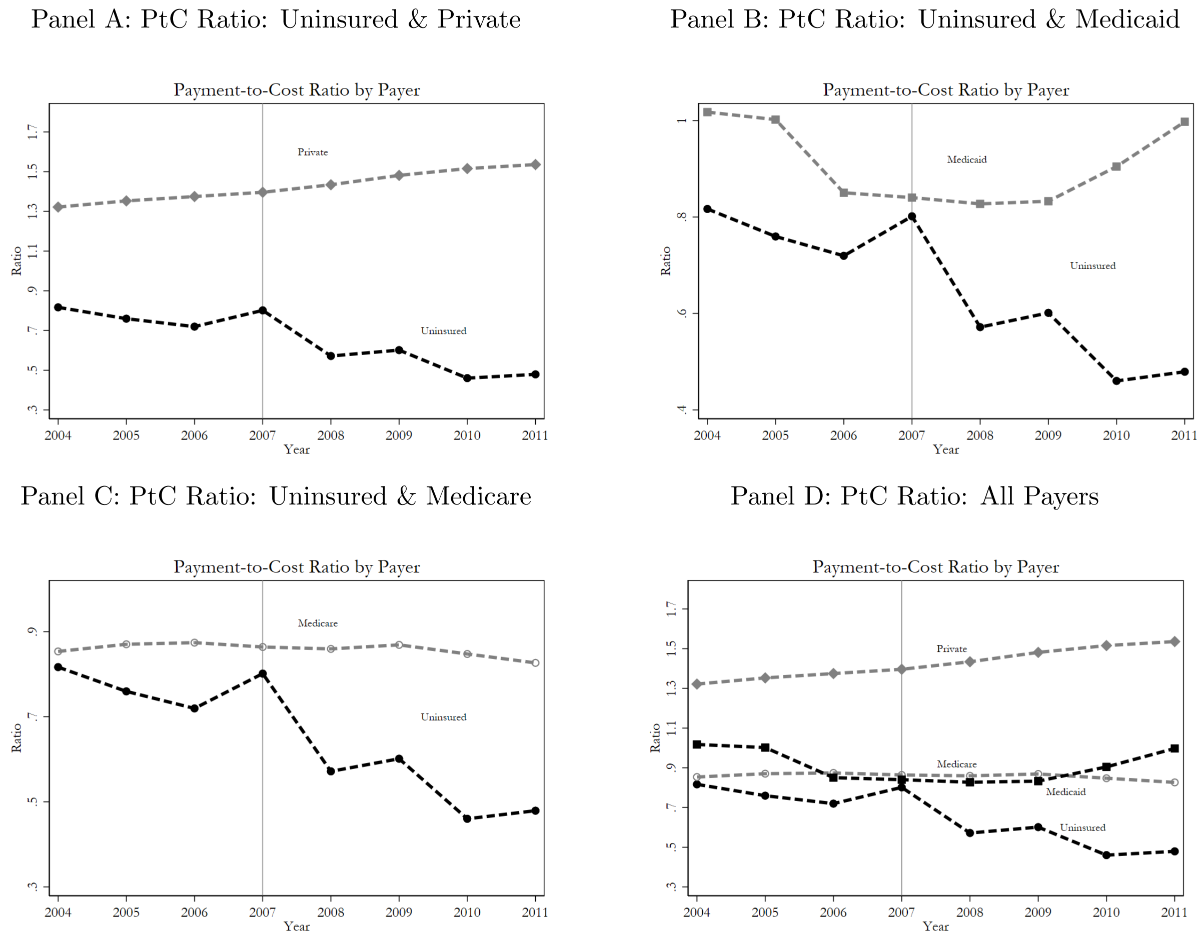
We first determine whether the uninsured paid relatively less for hospitalizations after the implementation of the CA FPL. To do this, we analyze annual hospital financial reports between 2004 and 2011 obtained from the California Health Care Access and Information (HCAI). We examine how payments from uninsured patients, relative to those from patients with other types of health insurance, changed after the CA FPL took effect.

To abstract away from differences in the quantity and intensity of hospital care received by patients with different types of health insurance, we examine the ratio of payments to total hospital expenses (e.g., see Batty and Ippolito (2017a)). The payment-to-cost ratio (PtC) measures the extent to which hospitals recover their treatment costs from different payer groups. For example, a PtC ratio equal to one indicates that hospitals were fully reimbursed for their costs, a ratio below one indicates that hospitals received payments below cost, and a ratio above one indicates that payments exceeded treatment costs.

Figure 1 compares PtC ratios across payers: uninsured versus privately insured patients (panel A), uninsured versus Medicaid patients (panel B), uninsured versus Medicare patients (panel C), and across all payers combined (panel D). Consistent with prior evidence that private insurers reimburse hospitals more generously than public payers, we find that the PtC ratio for private insurance exceeds one in all years. The PtC ratio for Medicare remains below one throughout the sample period, while Medicaid fluctuates around one, exceeding it in some years and falling below it in others. For all years, the PtC ratio for the uninsured is below one and consistently lower than other payer groups.

We observe a decline in the PtC ratio for the uninsured starting in 2007, falling from about 0.8 in 2007 to about 0.5 in 2011. We estimate that the average annual difference between gross revenue and net payments for uninsured patients was approximately $13.2

Figure 1: **Hospital Revenues and Patient Payments**




Note: This figure presents payment-to-cost (PtC) ratios for California hospitals between 2007 and 2011. A hospital's PtC ratio represents the hospital's net revenue to total hospital expense. It is calculated using data from the California Office of Statewide Health Planning and Development Data. Prices are adjusted to 2011 dollars.

million before the CA FPL (between 2004 and 2006). This difference increased to \$25.1 million annually after the CA FPL (between 2007 and 2011). This is consistent with Batty and Ippolito (2017a)'s finding that the price paid for hospital care by the average uninsured patient declined by 25 to 30 percent after the CA FPL. By contrast, the PtC ratios for private insurance, Medicaid, and Medicare remained stable or increased slightly after 2007.

Taken together, these findings suggest that the CA FPL lowered the hospital bills of the uninsured. This is demonstrated by the fact that hospitals experienced the largest relative decline in payments from uninsured patients between 2007 and 2011, whereas PtC ratios for other payers remained stable or improved. This pattern is consistent with the CA

FPL reducing the amounts hospitals could collect from uninsured patients, thereby lowering hospital bills for this group. Reduced hospital bills likely improved the financial well-being of individuals directly affected by the law, which we further investigate in the remainder of this paper.

# 4 Conceptual Framework

In this section, we discuss the possible pathways by which the law affects individuals' financial outcomes. While our analysis will not be able to pinpoint the specific mechanism by which our results occur, this discussion is useful for understanding which pathways are at work, in light of the results.[7]

The role of a program like the CA FPL is to limit the financial burden associated with health-care utilization for uninsured and underinsured individuals. Uninsured individuals typically face the full list price of care they receive and are at strategic disadvantage when bargaining with health-care providers for reduced bill amounts. Therefore, gaining access to this kind of financial protection should lower the prices they face and, consequently, the total medical expenditures that they incur for a given hospitalization. For instance, Batty and Ippolito (2017a) find that the price paid for hospital care by the average uninsured patient declines by 25 to 30 percent because of FPLs.

Lower medical expenditures, via reductions in price (through price caps or financial assistance programs), increase "real income," which may have positive financial spillovers. Having additional financial resources may allow individuals to pay down existing debt or prevent future delinquencies or the accumulation of future debt.[8] As shown by Brevoort et al. (2020), unpaid medical bills lead to substantial financial problems, including reduced access to credit via lower credit scores or higher costs of borrowing. As a result of the CA FPL, we may expect to see reductions in measures of financial distress, such as late medical bills, which may subsequently lead to improvements in other financial outcomes.[9]

[7] There are also potential supply-side and general equilibrium effects that may affect individuals' financial outcomes, but we primarily focus on demand-side effects in our analysis.

[8] Importantly, this effect is not restricted to just existing or new medical bills. Increased real income can be used to pay off any existing debt or can prevent the accumulation of any future debt.

[9] Late medical bills may also appear on an individual's credit as late credit card debt if an individual uses

The existence of the CA FPL may also allow uninsured and financially vulnerable individuals to engage in other types of financial activities they would not otherwise be able to do. This is because the law reduces the risk of incurring large medical expenses if an individual faces a negative health shock. For example, uninsured individuals covered by the CA FPL may be able to reduce the amount of precautionary savings held.

Lower prices via the CA FPL could also lead to changes in demand for hospital health-care services. If lower prices lead to increased consumption of health-care services, this may offset some of the 'income effect' mentioned above. This depends, however, on the elasticity of health-care consumption with respect to price. Additionally, if increased health-care consumption leads to improved health outcomes, this may allow a worker to better maintain employment or to be more productive. This in turn would strengthen the income effect.

Overall, the direction and magnitude of effects of the CA FPL are *a priori* ambiguous. The reduction in prices paid for health care and its associated income effect may lead to large reductions in financial distress if there are little changes in health-care consumption after the policy. However, if the consumption responses to the reduced hospital prices are large, this would result in smaller improvements to financial distress. If individuals borrow to finance this consumption, this would further mitigate these improvements.

# 5 Data

To examine the effects of the CA FPL on financial distress, we use consumer credit data from the Federal Reserve Bank of New York Consumer Credit Panel/Equifax data (CCP). The CCP data set is an anonymized, nationally representative 5 percent random sample of individuals with credit bureau records from 1999 to the present. Consumers must have at least one public record or credit account and a Social Security number (SSN) to be included in the CCP, and are followed at a quarterly frequency.[10] While the CCP contains extensive information regarding credit data, it does not contain any demographic information

a credit card to pay for health-care services. In support of this, Rae et al. (2022) find that 17 percent of survey respondents reported having any medical debt on a credit card.

[10]Individuals leave the CCP if they die, change their SSN, or have an extended period of credit market inactivity.

besides year of birth and census geography. In a given quarter, the CCP contains data on approximately 12 million different consumers.[11]

We create our analysis sample by merging the CCP to a unique data set that contains detailed information on medical bills placed for collection by third-party debt collectors. The medical collections data set is an approximately 40 percent anonymized random sample of individuals in the CCP with and without a medical collection on their credit report. The data set runs from 2003 to 2017 and consumers are observed at the end of the fourth quarter in each year. For each consumer, we have detailed information on up to 10 medical collection accounts, including the current balance on each account, the amount of debt that was initially sent to the debt collector, and information on the date the account was assigned to a debt collector. Importantly, this date is not necessarily the date the medical debt was incurred. The assignment date is, however, reported directly to Equifax by the debt collection agency. Because CCP data are at the individual level, we aggregate the medical collection account information to the consumer-level for each year. We refer to the unpaid medical bills that the debt collection agencies report to Equifax as "medical collections" or "medical debt."[12]

Of note, during our analysis period, there was no uniform standard for reporting unpaid medical bills to debt collectors, unlike other forms of debt, like credit card debt, that are reported as being 30, 60, 90, or 120 days late. This means that we observe medical debt only when it is reported by a collection agency to the credit bureau. The lack of a uniform reporting standard means that the date that we observe new medical collections appearing in the credit bureau data does not necessarily coincide with the date that the medical bill was actually accrued.[13] Also, the medical collections we observe do not capture the universe of medical debt. Medical bills paid using a credit card do not appear in our medical collections data. In addition, because hospitals may sue patients for unpaid medical bills (Cooper et al., 2021), if the outcome of such lawsuits is wage garnishment, the unpaid bill may not appear in the credit bureau data.[14]

[11] For a more comprehensive overview of the CCP, see Lee and van der Klaauw (2010).

[12] Note that not all debt in collections is actually sold to debt buyers/collectors. Firms collecting debt on behalf of the original creditor furnish the majority of collections to credit reporting agencies (CFPB, 2023).

[13] Additionally, we are unable to infer how delinquent the medical debt is from the reporting date.

[14] Though the unpaid bill may not appear in the credit report, the wage garnishment would appear as a civil judgment on the credit report.

We also use data on county characteristics obtained from several sources. County-level annual estimates of health-insurance coverage data come from the U.S. Census Bureau's Small Area Health Insurance Estimates (SAHIE) program. We also use data on the county-level housing price index from the Federal Housing Finance Agency (FHFA). The county-level unemployment rate is obtained from the Bureau of Labor Statistics (BLS). Finally, data on the share of county establishments that are manufacturing come from the U.S. Census Bureau County Business Patterns data set.

## 5.1 Outcomes

Our main outcomes of interest are measures of debt owed to third-party debt collectors, a frequently used measure of financial distress in the literature (Finkelstein et al., 2012). Accounts owed to these debt collectors are commonly referred to as *collections* and the amount of debt owed to debt collectors as *collection balances*. We observe two types of collection accounts in our merged data: medical and non-medical collections. Medical collections are categorized as delinquent accounts owed to a medical provider, such as a hospital or a doctor's office. Non-medical collections include both delinquent loans, such as car loans or credit cards, and late or unpaid bills, which can include those for utilities and telecommunication services. Both measures are for late bills or loans that are being pursued by a third-party debt collection firm.

To study the extensive margin effects of the CA FPL on collections, we analyze the probability of having any medical or non-medial collection accounts reported to Equifax in the past 12 months.[15] Additionally, we examine the probability of having a $0 medical or non-medical collection balance in the past 12 months.[16] These outcomes allow us to investigate if the CA FPL was able to protect individuals from financial distress due to the high costs of medical care. To study the intensive margins effects of the CA FPL on financial

[15]By choosing collections within the past 12 months, we are examining *flow* measures of financial distress instead of the total stock. This is because the stock of collections, especially medical collections, typically consists of very old accounts, which may not be indicative of an individual's current financial situation. See Gibbs et al. (2025) for more details.

[16]Having a $0 collection balance may indicate that an individual does not have a collection balance or it can be the result of paying off a collection balance. For the latter, an individual may still have a collection account on their credit report despite it being paid off.

outcomes, we examine the number of medical and non-medical collection accounts and the debt balances of medical and non-medical collections reported to the credit bureau in the past 12 months. We also explore the effects of the law on the distribution of medical and non-medical collections by binning individuals' collection balances within a range of values. We follow Mazumder and Miller (2016) and create four bins of balance ranges: \$0, \$1 – \$1,000, \$1,001 – \$2,000, and greater than \$2,000.

Along with our medical and non-medical collection variables, we also consider three additional financial outcomes that capture financial distress: an individual's credit score, the total number of delinquent accounts that an individual has, and the percent of all debt that is delinquent.[17] Accounts and debts are considered delinquent if they are at least 30 days past due.

### 5.2 Analysis Sample

Our analysis sample consists of individuals residing in California (CA) and in the neighboring states of Washington, Oregon, Nevada, and Arizona (WA, OR, NV, and AZ) who were 18-to-39 years old between 2003 and 2010. We focus on this group because it has traditionally had lower rates of health-insurance coverage, particularly during the study period, and is therefore likely to be most affected by the hospital price caps imposed by the law. Additionally, as we discuss in the next section, because the CCP does not contain information on health-insurance status, we proxy for an individual's likelihood of having health insurance by using their county's pre-CA FPL uninsured rate (Mazumder and Miller (2016)). We also restrict the last year of our analysis to 2010 since California expanded Medicaid in 2011.[18]

## 6 Methodology

We estimate the effect of the CA FPL on individual financial outcomes using a difference-in-difference-in-differences (DDD) design. We compare individuals who live in California with

[17] This credit score measure is the Equifax Risk Score, which is a proprietary credit score produced by Equifax. This score is similar to other risk scores used in the industry.

[18] Inclusion of the years beyond 2010 could result in biased treatment effect estimates if Medicaid expansions change the composition of the uninsured. For example, this could occur if there is selection into Medicaid participation among the uninsured.

those who live in border states before and after the implementation of the CA FPL. Similar to Mazumder and Miller (2016) and Bailey et al. (2025), who use individuals in nearby border states to form their control group, we take this approach under the assumption that individuals living in nearby border states are similar to those in California in their credit variables.

Our final difference uses county-level variation in the uninsured rate for low-income adults ages 18-to-39 in 2006 to identify individuals who are most likely to be impacted by the law (i.e., individuals who are more likely to be lower income and uninsured). Like Mazumder and Miller (2016), we refer to this variable as a measure of *exposure* to the law, as it provides across-county variation in the likelihood of an individual being exposed to the law. Specifically, our exposure variable is a continuous measure bounded between zero and one. For example, it equals one if an individual lives in a county with a 100 percent uninsured rate among low-income 18- to 39-year-olds in 2006 and it equals zero if the uninsured rate is 0 percent. Overall, the average county-level uninsured rate in 2006 for low-income 18- to 39-year-olds is 41.7 percent for states in our sample. Thus, our DDD specification allows us to control for shocks that differentially affect individuals living in counties with relatively higher uninsured rates and counties with relatively lower uninsured rates, and to avoid state-specific shocks that differentially affect outcomes of interest across California versus border states. Appendix Figure A1 documents the spatial variation in the uninsured rate for the states in our sample.

We estimate the following DDD equation:

$$\begin{aligned} Y_{it} = & \beta + \pi(CA_i \times Exposure_i \times Post_t) + \theta(CA_i \times Post_t) \\ & + \psi(Exposure_i \times Post_{it}) + X_{it}'\mathbf{\Omega} + C_{ct}'\mathbf{\Phi} + \delta_c + \delta_i + \delta_t + \epsilon_{it}. \end{aligned} \tag{1}$$

$Y_{it}$ refers to the financial outcomes of interest outlined in Section 5.1 for individual $i$ at time $t$. $CA$ takes the value one if an individual lives in California and is zero otherwise. $Post$ takes the value one for years after the law's introduction in 2007 and is zero otherwise.[19] We

[19]Similar to Courtemanche et al. (2017), $Post_t$ is not included in Equation 1 because it is perfectly collinear with the year fixed effects, and $CA_i$, $Exposure_i$, and $Exposure_i \times CA_i$ are all omitted because they are perfectly collinear with the county fixed effects.

include county fixed effects ($\delta_c$) to account for time-invariant county characteristics, $\delta_i$ to account for individual fixed effects, and $\delta_t$ to account for time fixed effects. $X_{it}$ contains our vector of individual-level controls, which include age and household-size fixed effects. $C_{ct}$ is a vector of county-level controls, which include variables for the county unemployment rate at time $t$, to account for the dynamic between employment and access to health insurance, as well as a county-level house price index and the county-level share of establishments that are manufacturing, since our sample spans the years of the Great Recession.[20]

The parameter of interest in Equation 1 is $\pi$, which captures the effect of the law on financial outcomes for individuals living in California for each additional unit change in the county uninsured rate for low-income 18- to 39-year-olds. Since our exposure measure is bounded between 0 and 1, a one unit increase in exposure is a 100 percentage point change in the uninsured rate.[21] Therefore, to aid in interpreting our results, we scale our estimates $\pi$ by 0.1, so the interpretations of $\pi$ are in terms of the effect of a 10 percentage point increase in the uninsured rate ($\pi \times 0.1$) on the outcomes of interest for individuals in CA after the reform. We also follow Courtemanche et al. (2017) and Argys et al. (2020) and interpret our results by scaling the estimates evaluated at the sample of mean of exposure (0.417), which tells us the impact on the average person in a county in CA with average exposure.

The identifying assumption for our DDD model is that in the absence of the CA FPL, the financial outcomes of interest would have followed similar trends across counties with differential exposure rates in California and border states. To test the validity of our parallel trends assumptions, we also estimate the following event study model:

$$\begin{aligned} Y_{it} = &\beta + \boldsymbol{\pi}(CA_i \times Exposure_i \times \delta_t) + \boldsymbol{\theta}(CA_i \times \delta_t) \\ &+ \boldsymbol{\psi}(Exposure_i \times \delta_t) + X'_{it}\boldsymbol{\Omega} + C'_{ct}\boldsymbol{\Phi} \\ &+ \delta_c + \delta_i + \delta_t + \epsilon_{it}. \end{aligned} \tag{2}$$

[20]We include the county manufacturing share in our estimating equation because the Great Recession may have had a differential economic impact on counties based on their exposure to the housing crisis and manufacturing.

[21]Alternatively, the point estimate can be interpreted as the effect of the FPL on an individual residing in a county with a 100 percent uninsured rate for low-income 18- to 39-year-olds ($\pi \times 1$) compared with another individual residing in a county with a zero percent uninsured rate for low-income 18- to 39-year-olds in CA.

All variables remain the same as in Equation (1) except we replace the variable *Post* with the vector of time indicator variables, $\delta_t$, to test if there is stability across our outcome variables of interest in the pre-FPL period.

# 7 Results

## 7.1 Summary Statistics

We present summary statistics in Table 1. In both CA and comparison states, individuals are more likely to have recent non-medical accounts in collection than medical accounts. This may be due to differences in how medical and non-medical debt are reported to credit agencies as discussed in Section 5. Prior to the passage of the law, the share of individuals with recent medical collections was 4 percent in California and 7 percent in comparison states; after the law as implemented, the shares increased to 5 percent and 10 percent respectively. We see similar patterns for recent non-medical collections: Border states had a slightly higher share of individuals with non-medical collections than CA (13 percent vs. 11 percent) and both groups saw the share slightly increase from the pre-law period to the post-law period.

Across CA and non-CA states, we observe a decline in the share with a $0 medical collection balance after 2007. In CA, the share declined by 1 percentage point and in non-CA states, the decline was 3 percentage points. The share with a $0 non-medical balance in both CA and comparison states declined by 1 percentage point from the pre-law period to the post-law period. The average number of medical collections is lower in CA, with individuals having 0.06 accounts in the pre-FPL period and 0.09 accounts after the FPL was enacted. In non-CA states, individuals had 0.12 accounts and 0.20 accounts in that time period.

Collection balances are also mostly lower in CA relative to other states, especially for medical collections, both in the pre- and post-periods. For context, we present Appendix Figure A2, which shows the county-level median collection balances for all counties in the U.S. in 2006. While there is significant geographic heterogeneity in median balance levels across the country in 2006, CA has lower levels than most western and midwestern states.[22] In

[22] This trend persists as California is currently among the states with the lowest levels of medical collections

Table 1: **CCP Summary Statistics**

| | Non-CA States | | California | |
|---|---|---|---|---|
| | Pre | Post | Pre | Post |
| Share(Medical Collections) | 0.07 | 0.10 | 0.04 | 0.05 |
| | (0.248) | (0.299) | (0.190) | (0.221) |
| Share(Non-medical Collections) | 0.13 | 0.14 | 0.11 | 0.12 |
| | (0.337) | (0.346) | (0.308) | (0.321) |
| Share($0 Medical Collection Balance) | 0.93 | 0.90 | 0.96 | 0.95 |
| | (0.250) | (0.298) | (0.191) | (0.220) |
| Share($0 Non-medical Collection Balance) | 0.87 | 0.86 | 0.89 | 0.88 |
| | (0.337) | (0.345) | (0.307) | (0.320) |
| Total Collection Balance | 463.64 | 684.06 | 430.68 | 583.26 |
| | (2294.6) | (3066.1) | (2535.1) | (3172.7) |
| Non-medical Collection Balance | 296.48 | 358.94 | 289.35 | 368.49 |
| | (1833.1) | (2144.5) | (1818.2) | (2463.7) |
| Medical Collection Balance | 167.16 | 325.12 | 141.33 | 214.77 |
| | (1321.1) | (2097.6) | (1745.9) | (1941.8) |
| Number of Medical Collections | 0.12 | 0.20 | 0.06 | 0.09 |
| | (0.588) | (0.818) | (0.389) | (0.484) |
| Number of Non-medical Collections | 0.20 | 0.21 | 0.15 | 0.16 |
| | (0.688) | (0.690) | (0.514) | (0.550) |
| Credit Score | 645.60 | 645.52 | 653.47 | 650.51 |
| | (98.75) | (104.0) | (99.04) | (104.1) |
| % of All Debt That Is Delinquent | 0.16 | 0.19 | 0.15 | 0.19 |
| | (0.344) | (0.368) | (0.339) | (0.372) |
| Number of Delinquent Accounts | 0.46 | 0.50 | 0.43 | 0.51 |
| | (1.138) | (1.244) | (1.128) | (1.283) |

Notes: Authors' calculation using Federal Reserve Bank of New York Consumer Credit Panel/Equifax and Census data. Sample includes individuals ages 18 − 39 from 2003 to 2010, which yields approximately 2.2 million observations and approximately 401,000 individuals. All collection variables are for accounts sent to a third-party debt collector in the past 12 months. Credit score is the Equifax Risk Score. An account is defined as delinquent if payment is at least 30 days late. Reported statistics are averages, unless otherwise stated.

Appendix Figure A3, we show the distribution of medical collections for CA and comparison states pre- and post-FPL. We see small shifts in the medical collections distribution, but overall, the distributions are fairly similar across time periods. In general during this time period, individuals in CA typically had lower medical collection balances than individuals living in other U.S. states.

## 7.2 Main Results

Our main results from the estimation of Equation 1 are presented in Table 2 with corresponding event study graphs in Figures 2 and 3. We do not find a statistically significant

in the U.S. See Kluender et al. (2021) for a detailed map for 2020.

effect of the FPL on the likelihood of having any medical debt in collections within the past 12 months. However, the event study results in panel A of Figure 2 show an immediate decline in the probability of having any medical debt in collections in the past 12 months following the passage of the law. This decline is transitory and is not sustained in subsequent years. Thus, the effects on medical debt appear to be transitory.

However, we find statistically significant effects of the law's passage on *non*-medical debt in collections. Specifically, a 10 percentage point increase in the uninsured rate leads to a 0.5 percentage point decline in the probability of having any non-medical debt in collections ($-0.05 \times 0.1$), a relative decrease of 4.7 percent. While this effect is modest, it suggests the CA FPL may have had a measurable effect on the extensive margin of non-medical debt accumulation. In panel C of Figure 2, we show that there are sustained reductions in the probability of having non-medical collections with larger declines in later years.

We next examine the effect of the law on the probability of having a \$0 balance in collections in the past 12 months. A 10 percentage point increase in the uninsured rate leads to a 0.03 percentage point decrease in the probability of having a \$0 medical collections balance, an estimate that is not statistically significant and similar in magnitude to our findings for the probability of having any medical debt in collections. Consistent with this result, panel B of Figure 2 shows an immediate post-reform increase in the probability of having a \$0 medical collections balance, followed by estimates that are near zero with large standard errors throughout the remainder of the sample period.

In contrast, the estimate for \$0 non-medical collection balance is positive and statistically significant. A 10 percentage point increase in the uninsured rate leads to a 0.53 percentage point increase in the probability of having a \$0 non-medical collections balance in the past 12 months after the passage of the CA FPL, suggesting that the law may have helped individuals to pay off their non-medical collection balances or prevented individuals from accruing non-medical collection balances in the first place.

Panel B of Table 2 presents estimates of the impact of the CA FPL on the number and balance of medical and non-medical accounts in third-party collections in the past 12 months. We do not find statistically significant effects of the law on the number of medical accounts sent to collections; however, for non-medical accounts, we estimate a statistically significant

Table 2: **Effect of FPL on Financial Distress**

| | Medical Collections | | Non-medical Collections | |
|---|---|---|---|---|
| | Any | $0 Balance | Any | $0 Balance |
| **Panel A** | | | | |
| Treat × Exposure × Post | 0.001 | -0.003 | -0.050** | 0.0532** |
| | (0.020) | (0.020) | (0.024) | (0.025) |
| Scaled Estimate (Exposure × 0.417) | 0.0004 | -0.0014 | -0.0210 | 0.0222 |
| Treatment Mean | 0.037 | 0.962 | 0.106 | 0.894 |
| $R^2$ | 0.362 | 0.358 | 0.390 | 0.392 |
| N | 2,206,398 | 2,258,553 | 2,206,398 | 2,203,415 |

| | Medical Collections | | Non-medical Collections | |
|---|---|---|---|---|
| | Number | Balance | Number | Balance |
| **Panel B** | | | | |
| Treat × Exposure × Post | 0.021 | 88.161 | -0.136*** | -46.106 |
| | (0.064) | (92.039) | (0.045) | (117.717) |
| Scaled Estimate (Exposure × 0.417) | 0.009 | 36.763 | -0.057 | -19.226 |
| Treatment Mean | 0.059 | 141.33 | 0.146 | 289.34 |
| $R^2$ | 0.391 | 0.249 | 0.368 | 0.279 |
| N | 2,206,398 | 2,203,415 | 2,206,398 | 778,484 |

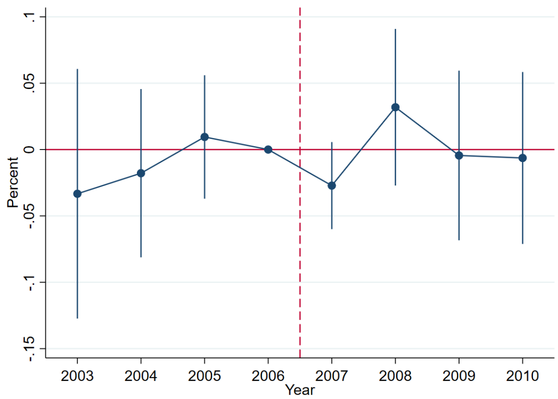
Notes: Authors' calculation using Federal Reserve Bank of New York Consumer Credit Panel/Equifax, Census, FHFA, and BLS data. All collection variables are for accounts sent to a third-party debt collector in the past 12 months. The *Any* collections variable = 1 if an individual has a positive number of accounts in collections, and the *$0 balance* variable = 1 if an individual has $0 collection balance during the *Post* period. *Treat* = 1 if an individual lives in CA, *Exposure* is a continuous measure of the county-level low-income young adult uninsured rate in 2006, and *Post* = 1 for the years 2007 to 2010. Sample includes individuals ages 18 – 39. Standard errors clustered at the county-level. Treatment mean is the pre-policy average for individuals living in CA counties. The scaled estimate is the product of the DDD coefficient and the pre-treatment mean uninsured rate. *** p<0.01, **p < 0.05, *p < 0.1.

reduction in the number of accounts sent to third-party collections. Our estimates suggest that a 10 percentage point increase in exposure leads to a decrease of 0.0136 accounts, which represents a 9.3 percent decrease relative to the pre-treatment mean.

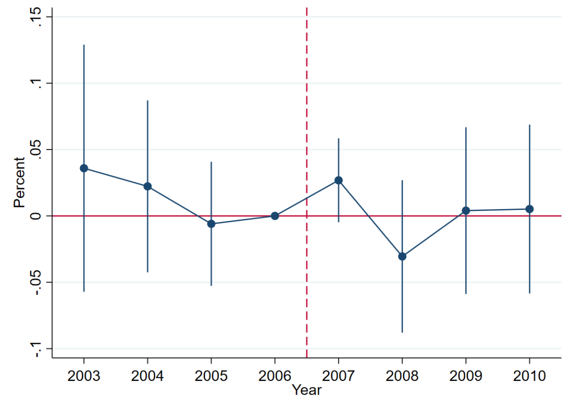
We also report "average effects" analogous to those reported by Argys et al. (2020), which are the DDD estimates scaled by the average pre-FPL county uninsured rate for low-income young adults (41.7 percent). For a young adult living in a county with an average exposure in CA, we estimate that the probability of having any non-medical debt in collections declined by 2 percentage points (a relative 19.8 percent decline), the probability of having a $0 non-medical collection balance increased by 2 percentage points (a relative

Figure 2: **Triple Difference Event Study Results: Extensive Margin**

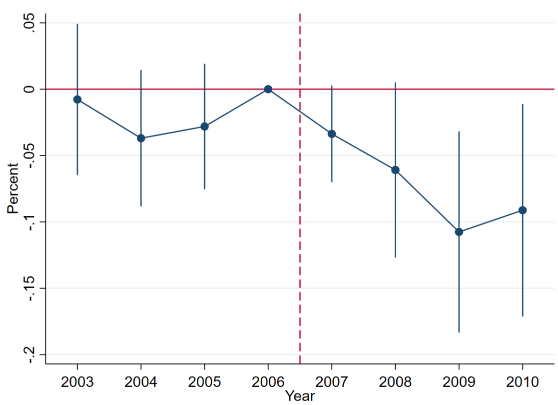
Panel A: Probability of having a medical account in third-party collections




Panel B: Probability of having a $0 medical collection balance




Panel C: Probability of having a non-medical account in third-party collections




Panel D: Probability of having a $0 non-medical collection balance

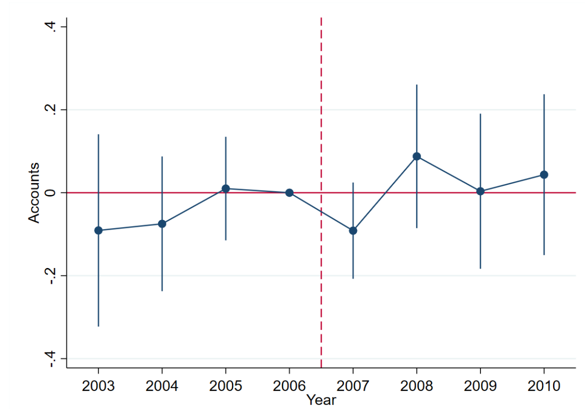

Note: Authors' calculations using Federal Reserve Bank of New York Consumer Credit Panel/Equifax, Census, FHFA, and BLS data. All collection variables are for accounts sent to a third-party debt collector in the past 12 months. Lines represent 95 percent confidence intervals. Sample includes individuals ages 18 – 39.

2.5 percent increase), and non-medical collections declined by 0.057 accounts (a relative 39 percent decline).

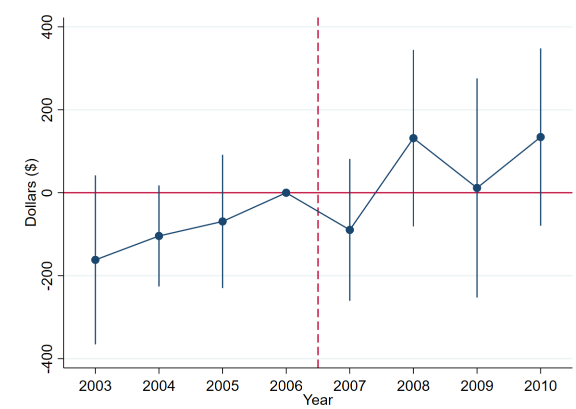
One explanation for the above findings is that the baseline balances and number of accounts in third-party collections were relatively low prior to the CA FPL, averaging $141 for medical collections and $289 for non-medical collections. Given this distribution, the absence of detectable effects may not be surprising. Also, as we show in Appendix Figure A3, the majority of the medical collection balance distribution did not change from pre- to

Figure 3: **Triple Difference Event Study Results: Intensive Margin**

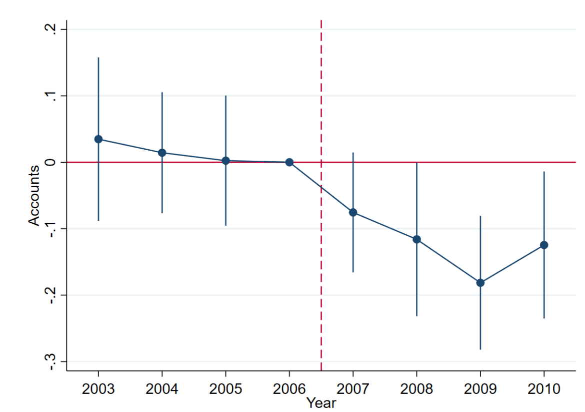
Panel A: Number of medical accounts sent to third-party collections in the past 12 months



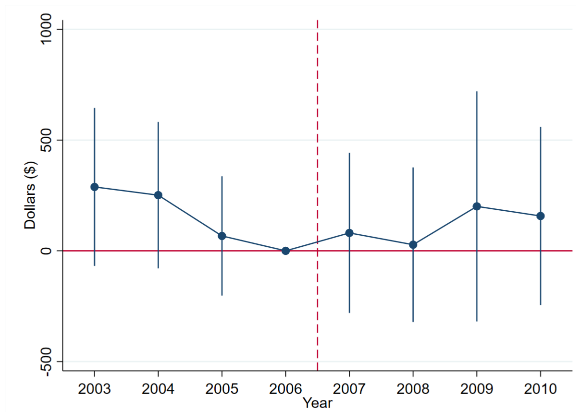

Panel B: Balance of medical accounts sent to third-party collections in the past 12 months




Panel C: Number of non-medical accounts sent to third-party collections in the past 12 months




Panel D: Balance of non-medical accounts sent to third-party collections in the past 12 months




Note: Authors' calculations using Federal Reserve Bank of New York Consumer Credit Panel/Equifax, Census, FHFA, and BLS data. All collection variables are for accounts sent to a third-party debt collector in the past 12 months. Lines represent 95 percent confidence intervals. Sample includes individuals ages 18 – 39.

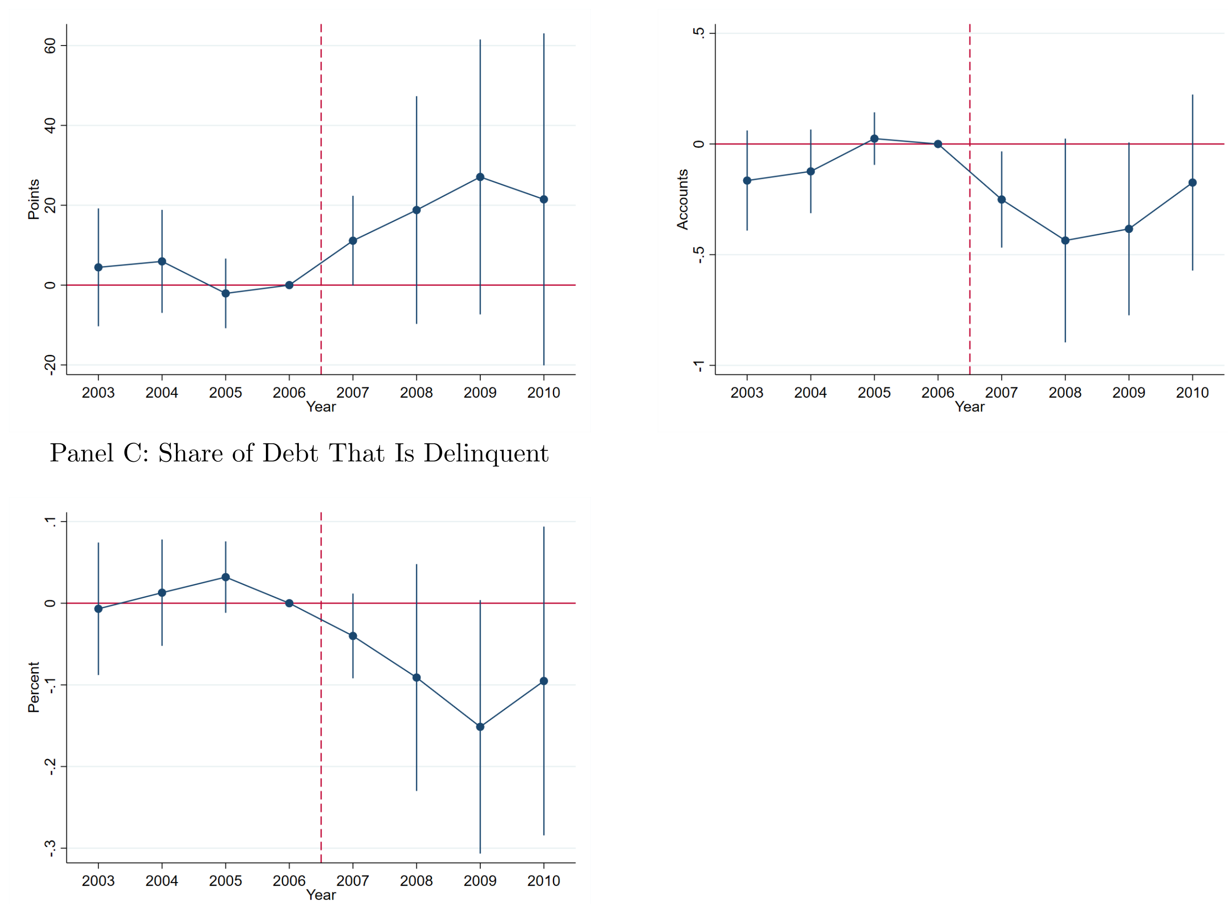
post-FPL in our sample, though there are small differences in the far right and left tails of the balance distribution. Therefore, it may be the case that the effects of the FPL varies over the collection distribution.

Lastly, we examine broader indicators of financial distress beyond medical and non-medical debt and collections. Figure 4 and Table 3 report estimates of the CA FPL on credit scores, the share of debt that is delinquent (at least 30 days past due) and the number of delinquent accounts (at least 30 days past due). The estimated effect on credit scores

Figure 4: **Triple Difference Event Study Results: Other Measures of Financial Distress**

Panel A: Risk Score

Panel B: Number of Delinquent Accounts




Note: Authors' calculations using Federal Reserve Bank of New York Consumer Credit Panel/Equifax, Census, FHFA, and BLS data. Lines represent 95 percent confidence intervals. Sample includes individuals ages 18 – 39. Accounts and debts are considered delinquent if they are at least 30 days past due.

is positive but not statistically significant, indicating that a 10 percentage point increase in exposure causes an increase of 2.2 points; the scaled "average effect" indicates that an individual in a CA county with average exposure saw an increase of 9.2 points. For our other two measures, we find suggestive evidence of improved financial well-being. Our scaled estimates indicate that the FPL led to a 5 percentage point decline in the share of all debt that is delinquent and a decline of 0.13 delinquent accounts. Using our preferred interpretation, our results suggest that a 10 percentage point increase in exposure leads to a decline in the share of delinquent debt by 0.012 percentage points (a relative 7.9 percent) and a decline in the number of delinquent accounts by 0.032 accounts (a relative 7.5 percent). Both estimates are statistically significant at the 10 percent level.

Table 3: **Effect of FPL on Other Measures of Financial Distress**

| | Risk Score | Share of Debt That Is Delinquent (%) | Number of Delinquent Accounts |
|---|---|---|---|
| Treat × Exposure × Post | 22.004 | -0.118* | -0.321* |
| | (14.268) | (0.066) | (0.174) |
| Scaled Estimate (Exposure × 0.417) | 9.176 | -0.049 | -0.134 |
| Treatment Mean | 653.5 | 0.152 | 0.428 |
| $R^2$ | 0.788 | 0.613 | 0.461 |
| N | 2,129,443 | 1,783,128 | 2,162,338 |

Note: Authors' calculations using Federal Reserve Bank of New York Consumer Credit Panel/Equifax, Census, FHFA, and BLS data. $Treat = 1$ if an individual lives in CA, $Exposure$ is a continuous measure of the county-level low-income young adult uninsured rate in 2006, and $Post = 1$ for the years 2007 to 2010. Sample includes individuals ages $18 - 39$. Standard errors clustered at the county-level. Treatment mean is the pre-policy average for individuals living in CA counties. Accounts and debts are considered delinquent if they are at least 30 days past due. *** p<0.01, **p $<$ 0.05, *p $<$ 0.1.

### 7.2.1 Effect on Collection Balances Distribution

To examine the impact of the CA FPL on the distribution of collection balances, we follow Mazumder and Miller (2016) and categorize collection balances into four groups: \$0, \$1 to \$1,000, \$1,001 to \$2,000, and over \$2,000, separately for medical and non-medical collections in the past 12 months.

Table 4 presents our distributional results. For medical debt, we estimate a negative and not statistically significant effect of the CA FPL on the probability of having a \$0 medical debt balance in collections. The estimated effects for medical balances between \$1 and \$1,000 and between \$1,001 and \$2,000 are negative, small, but not statistically different from zero. For medical debt exceeding \$2,000, we estimate a positive effect that is marginally statistically significant, suggesting a potential shift in the upper tail of medical debt distribution.

The effects on non-medical debt outcomes are consistent with improved financial outcomes. Panel B of Table 4 shows that the CA FPL increased the probability of having a \$0 non-medical collection balance; a 10 percentage point increase in exposure leads to a 0.053

Table 4: **Effect of FPL on Medical and Non-medical Collection Balance Distributions: Continuous Exposure**

| | Medical Debt Balance | | | |
|---|---|---|---|---|
| | $0 Balance | $1 to $1,000 | $1,001 to $2,000 | $2,001+ |
| **Panel A** | | | | |
| Treat × Exposure × Post | -0.003 | -0.011 | -0.002 | 0.016* |
| | (0.020) | (0.014) | (0.005) | (0.009) |
| Scaled Estimate (Exposure × 0.417) | -0.001 | -0.004 | -0.001 | 0.007 |
| Treatment Mean | 0.962 | 0.029 | 0.005 | 0.005 |
| $R^2$ | 0.358 | 0.290 | 0.210 | 0.255 |
| N | 2,258,553 | 2,258,553 | 2,258,553 | 2,258,553 |

| | Non-medical Debt Balance | | | |
|---|---|---|---|---|
| | $0 Balance | $1 to $1,000 | $1,001 to $2,000 | $2,001+ |
| **Panel B** | | | | |
| Treat × Exposure × Post | 0.053** | -0.043** | -0.006 | -0.004 |
| | (0.025) | (0.018) | (0.007) | (0.006) |
| Scaled Estimate (Exposure × 0.417) | 0.022 | -0.018 | -0.003 | -0.001 |
| Treatment Mean | 0.894 | 0.084 | 0.013 | 0.009 |
| $R^2$ | 0.392 | 0.341 | 0.231 | 0.217 |
| N | 2,203,415 | 2,203,415 | 2,203,415 | 2,203,415 |

Notes: Authors' calculation using Federal Reserve Bank of New York Consumer Credit Panel/Equifax, Census, FHFA, and BLS data. All collection variables are for accounts sent to a third-party debt collector in the past 12 months. The *$0 balance* variable = 1 if an individual has a $0 collection balance, while the other balance variables = 1 if an individual has a balance in that specific range during the *Post* period. $Treat$ = 1 if an individual lives in CA, *Exposure* is a continuous measure of the county-level low-income young adult uninsured rate in 2006, and *Post* = 1 for the years 2007 to 2010. Sample includes individuals ages 18 − 39. Standard errors clustered at the county-level. Treatment mean is the pre-policy average for individuals living in CA counties.*** p<0.01, **p < 0.05, *p < 0.1.

percentage point increase in the likelihood of having a $0 non-medical collection balance, a relative increase of 2.5 percent from the pre-period mean. For non-medical debt balances between $1 and $1,000, we estimate that a 10 percentage point increase in exposure leads to a 0.043 percentage point decline in collection balances in this range, a relative 5 percent decrease. For larger non-medical balances, we do not find statistically significant effects. Our scaled estimates imply that for individuals living in CA counties with average exposure, the probability having a $0 non-medical collection balance increased by 2.2 percentage points (a relative 2.5 percent increase) and the probability of having a non-medical collection balance between $0 and $1,000 decreased by 1.8 percentage points (a relative 21 percent decline).

Overall, we find evidence that the law improved financial well-being by reducing delinquent debt. Its effect on medical debt is negligible, which is somewhat surprising, given that the law explicitly capped hospital prices. By contrast, the law had a positive and economically meaningful impact on non-medical collections, reducing the probability of holding any non-medical debt, increasing the likelihood of having a zero non-medical debt balance, and decreasing balances in the $1 to $1,000 range.

## 7.3 Robustness Checks

### 7.3.1 Binary Measure of Exposure

In our main analyses, we used a continuous measure to define exposure to the policy. As a robustness check, we use an alternative exposure variable that equals one if an individual lives in a county with an uninsured rate greater than or equal to the median uninsured rate for low-income 18- to 39-year-olds across all states in the sample in 2006 (i.e., an individual lives in a *high exposure* county); it is zero, otherwise (i.e., an individual lives in a *low exposure* county). We re-estimate Equation 1 using this binary measure of exposure.[23] Tables 5 and 6 present robustness checks using this alternative measure of exposure to the CA FPL. These results are broadly consistent with our main findings based on the continuous uninsured rate.

Table 5 panel A shows that the law had no statistically significant effect on medical debt outcomes. By contrast, the effects on non-medical debt are marginally statistically significant and directionally consistent with our main findings. Individuals in high-exposure counties are 1.1 percentage points less likely to have any non-medical debt in collections and 1.2 percentage points more likely to have a $0 non-medical collections balance after the passage of the FPL. Table 5 panel B reports similar patterns for the number and balances of accounts in collections. We find no statistically significant effects on the number of medical accounts in collections or medical collection balances. For non-medical debt, however, the law is associated with a statistically significant reduction of 0.036 accounts in collections, while effects on non-medical balances are negative but imprecisely estimated.

Table 6 provides distributional evidence consistent with these results. The law had no statistically significant effects on the distribution of medical debt balances. In contrast, for

[23] The median uninsured rate for lower-income young adults across all counties in our data is 39.9 percent; the average uninsured rate for lower-income young adults is 41.7 percent.

Table 5: **Effect of the FPL on Financial Distress: Binary Exposure**

| | Medical Collections | | Non-medical Collections | |
|---|---|---|---|---|
| | Any | $0 Balance | Any | $0 Balance |
| **Panel A** | | | | |
| Treat × Exposure × Post | -0.003 | 0.002 | -0.011* | 0.012* |
| | (0.005) | (0.005) | (0.007) | (0.007) |
| Treatment Mean | 0.037 | 0.962 | 0.106 | 0.894 |
| $R^2$ | 0.362 | 0.358 | 0.390 | 0.392 |
| N | 2,206,398 | 2,258,553 | 2,206,398 | 2,20,3415 |

| | Medical Collections | | Non-medical Collections | |
|---|---|---|---|---|
| | Number | Balance | Number | Balance |
| **Panel B** | | | | |
| 6 Treat × Exposure × Post | -0.004 | 17.474 | -0.036*** | -18.20 |
| | (0.015) | (19.725) | (0.012) | (22.685) |
| Treatment Mean | 0.059 | 141.33 | 0.146 | 289.35 |
| $R^2$ | 0.391 | 0.249 | 0.368 | 0.279 |
| N | 2,206,398 | 2,203,415 | 2,206,398 | 778,484 |

Notes: Authors' calculation using Federal Reserve Bank of New York Consumer Credit Panel/Equifax, Census, FHFA, and BLS data. All collection variables are for accounts sent to a third-party debt collector in the past 12 months. The *Any* collections variable = 1 if an individual has a positive number of accounts in collections, and the *$0 balance* variable = 1 if an individual has $0 collection balance during the *Post* period. $Treat = 1$ if an individual lives in CA, $Exposure = 1$ if an individual lives in a county that is at or above the median young adult uninsured level, and $Post = 1$ for the years 2007 to 2010. Sample includes individuals ages $18 - 39$. Standard errors clustered at the county-level. Treatment mean is the pre-policy average for individuals living in CA counties. *** p<0.01, **p < 0.05, *p < 0.1.

non-medical collections, the probability of having a balance between $1 and $1,000 declined by a statistically significant 1.1 percentage points, while the probability of having a $0 non-medical collections balance increases by a marginally significant 1.2 percentage points. No significant effects are detected for larger non-medical balances.

Overall, our results using an alternative measure of exposure are consistent with our main results: The CA FPL had limited effects on medical debt but modest and statistically significant improvements in non-medical debt outcomes, particularly on the extensive margin of collections.

Table 6: **Effect of FPL on Medical and Non-medical Collection Balance Distributions: Binary Exposure**

| | Medical Debt Balance | | | |
|---|---|---|---|---|
| | $0 Balance | $1 to $1,000 | $1,001 to $2,000 | $2,001+ |
| **Panel A** | | | | |
| Treat × Exposure × Post | 0.002 | -0.004 | 0.001 | 0.002 |
| | (0.005) | (0.004) | (0.001) | (0.002) |
| Treatment Mean | 0.962 | 0.028 | 0.005 | 0.005 |
| $R^2$ | 0.358 | 0.290 | 0.210 | 0.255 |
| N | 2,258,553 | 2,258,553 | 2,258,553 | 2,258,553 |

| | Non-medical Debt Balance | | | |
|---|---|---|---|---|
| | $0 Balance | $1 to $1,000 | $1,001 to $2,000 | $2,001+ |
| **Panel B** | | | | |
| Treat × Exposure × Post | 0.012* | -0.011** | -0.0002 | -0.0004 |
| | (0.007) | (0.005) | (0.002) | (0.001) |
| Treatment Mean | 0.894 | 0.084 | 0.013 | 0.009 |
| $R^2$ | 0.392 | 0.341 | 0.231 | 0.217 |
| N | 2,203,415 | 2,203,415 | 2,203,415 | 2,203,415 |

Notes: Authors' calculation using Federal Reserve Bank of New York Consumer Credit Panel/Equifax, Census, FHFA, and BLS data. All collection variables are for accounts sent to a third-party debt collector in the past 12 months. The *$0 balance* variable = 1 if an individual has $0 collection balance, while the other balance variables = 1 if an individual has a balance in that specific range during the *Post* period. $Treat = 1$ if an individual lives in CA, $Exposure = 1$ if an individual lives in a county that is at or above the median young adult uninsured level, and $Post = 1$ for the years 2007 to 2010. Sample includes individuals ages 18 – 39. Standard errors clustered at the county-level. Treatment mean is the pre-policy average for individuals living in CA counties.*** p<0.01, **p < 0.05, *p < 0.1.

### 7.3.2 Excluding Oregon

In 2008, Oregon used a lottery system to increase Medicaid enrollment (Finkelstein et al., 2012). This additional enrollment targeted low-income, uninsured, non-disabled adults. Thus, including Oregon in our comparison group could bias our results toward zero since enrollment in Medicaid offers financial protections and because the population targeted by the CA law and the Oregon Medicaid program may be similar. To account for this potential source of bias, we test whether our results are sensitive to the inclusion of Oregon in our comparison states. We report the result in Appendix Tables A1 through A3.

In Appendix Table A1, we find no statistically significant effects on medical debt outcomes. However, the effects on non-medical debt remain statistically significant. A 10 percentage point increase in the uninsured rate leads to a 0.57 percentage point reduction in the probability of having any non-medical debt in collections and a 0.60 percentage point increase in the probability of having a $0 non-medical collections balance. There are also no statistically significant effects on medical debt accounts or balances. For non-medical collections, however, a 10 percentage point increase in the uninsured rate reduces the number of non-medical accounts in collections by 0.015 accounts (statistically significant at the 1 percent level). The estimated reduction in non-medical balances is negative but not statistically significant.

We report estimates on the distribution of balances in Appendix Table A2. Medical debt outcomes are not statistically significant across all balance categories. For non-medical collections, a 10 percentage point increase in the uninsured rate increases the probability of having a $0 balance by 0.60 percentage points and decreases the probability of having a balance between $1 and $1,000 by 0.48 percentage points, both statistically significant. We do not find statistically significant effects for larger non-medical balances.

Appendix Table A3 reports estimates for the other financial distress variables. The scaled estimates are similar to our scaled main results in Table 4, with credit scores improving by 11 points, delinquent debt falling by 6.2 percentage points, and the number of delinquent accounts declining by 0.166 accounts for a young adult living in a county with average exposure in CA. Using our preferred interpretation, these estimates suggest that a 10 percentage point increase in exposure improves credit score increases by 2.7 points, lowers the share of total debt that is delinquent by 0.015 percentage points (a relative 9.8 percent), and lowers the number of delinquent accounts by 0.04 accounts (a relative 9.3 percent). In sum, our results are robust to the inclusion of Oregon, given that our treatment effect estimates are not qualitatively different from our main results.

# 8 Discussion

We propose a few explanations for why we find muted effects of the policy for medical collections, but not for non-medical collections. First, as mentioned in Section 5, we observe medical collections only when they are reported to the credit bureaus, not when an individual actually accrues the medical debt. This is relevant because during our study period, hospitals varied substantially in how aggressive they pursued unpaid bills. This variation likely generates differences in the decisions of health-care providers to report unpaid medical bills to the credit bureaus or not, the timing of when medical collections were reported to credit reporting agencies, and which firms health-care providers used to collect unpaid medical bills (CFPB, 2014).

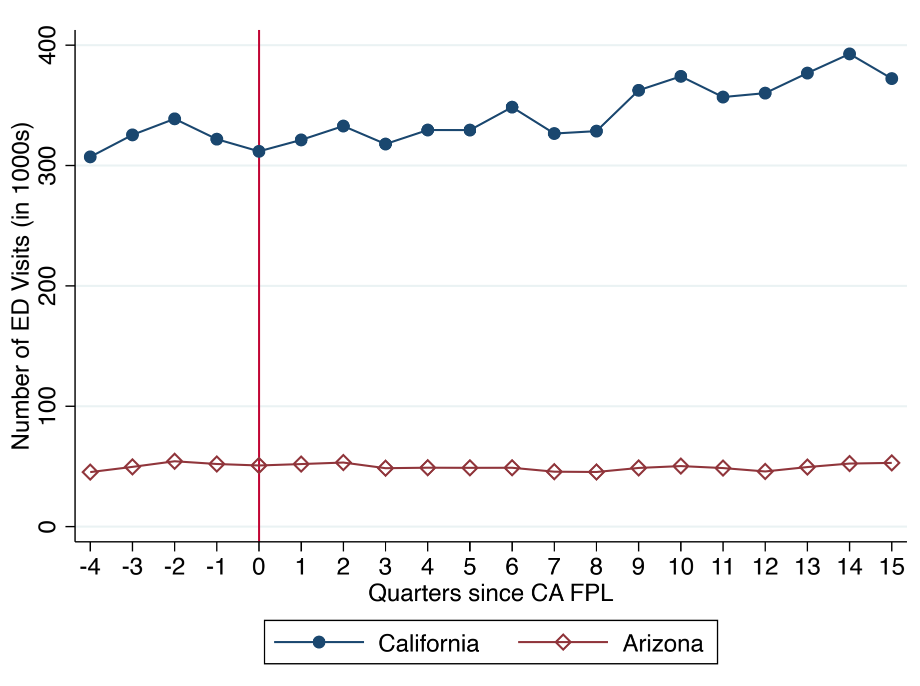
Second, medical collections may have been subject to informal negotiation or financial assistance programs before reaching collections. Even before the FPL, many hospitals offered charity care, income-based repayment plans, or write-offs for low-income patients which could have reduced the volume of debt formally recorded as delinquent even if households still carried unpaid balances (Adams et al., 2022). Additionally, while the FPL capped the size of hospital bills, it did not prevent providers from billing and collecting from patients. Without detailed information on prices paid, it is difficult to accurately know how much bills were lowered on average. Some of our distributional analyses seem to support this: We observe changes in debt balances at various points in the collection balance distribution, while finding no significant effects along the extensive margin.

For our significant non-medical collections results, one plausible mechanism is that the hospital price cap allowed individuals to re-optimize debt repayment. Since the law sets a price ceiling on hospital care for lower-income and uninsured individuals, this may lower individuals' medical bills, thereby freeing up liquidity and enabling them to meet other debt obligations in a timely manner. This interpretation is broadly consistent with prior research on household financial behavior following Medicaid dis-enrollments in Tennessee and Missouri, which found that individuals responded to *increased* medical expenses by delaying non-medical payments on things such as auto loan and other non-mortgage revolving credit or by financing consumption with revolving debt (Argys et al. (2020), Bailey et al. (2025)).

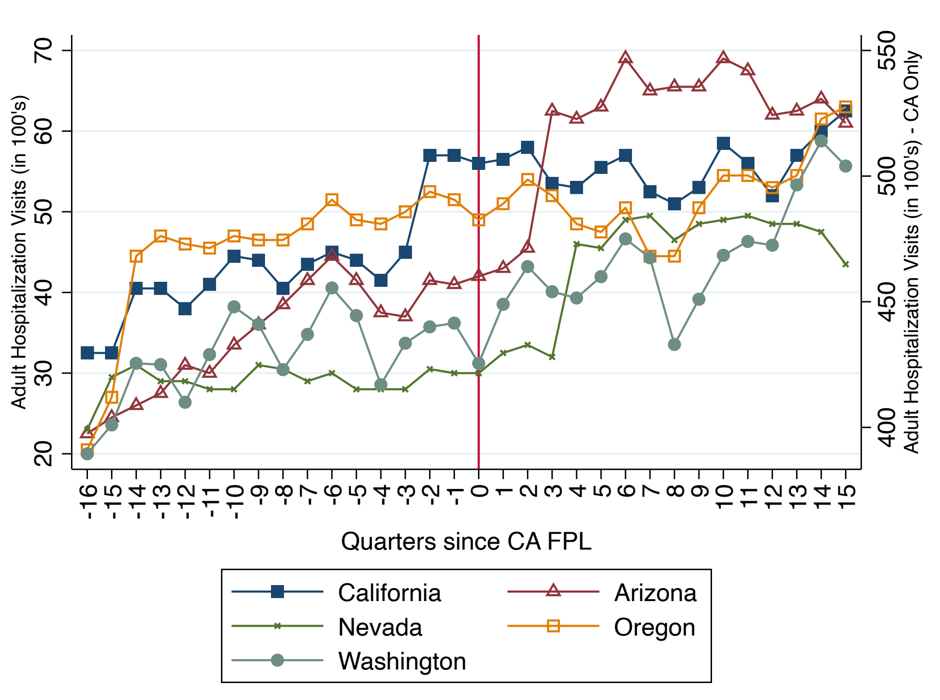
Lastly, an alternative explanation for our results is that there were differential changes in hospital utilization in CA and comparison states after 2007. For instance, Batty and Ippolito (2017a) find that hospitals responded to the CA FPL by decreasing the amount of care they provide to the uninsured. If the uninsured residing in comparison states experienced an increase in their hospital use *relative* to individuals in California in the post-2007 period,

Figure 5: **Trends in Utilization**

Panel A: Quarterly Emergency Department (ED) Visits for Self-Pay/No Charge Patients




Panel B: Quarterly Inpatient Hospitalizations for Self-Pay/No Charge Patients




Note: Authors' calculations using HCUP Fast Stats State Trends in Inpatient Stays by Payer and State Trends in Emergency Department Visits by Payer data (AHRQ (2020)).

this could trigger more medical and non-medical bills in collections for comparison states residents. We examine the validity of this concern by examining trends in adult inpatient hospitalizations and emergency department visits in California and comparison states over time, using state-level data from the Healthcare Cost and Utilization Project (HCUP).

In panel A of Figure 5, we find that quarterly emergency department visits for self-pay patients trended similarly, before and after 2007, for individuals visiting hospitals in California and Arizona.[24] Thus, emergency utilization behavior appears similar over time. Panel B of Figure 5 shows quarterly adult inpatient hospitalizations by uninsured patients in California and all comparison states four years before and after the introduction of the CA FPL. While we do not observe discernible changes in hospitalizations in California or comparison states in the quarter the law was implemented, we do observe increases in adult hospitalizations in Arizona and Nevada in 2008. However, any associated changes in debt/balances would likely bias our policy effects downward, which could, in part, explain the smaller effects of the law on collection balances.

# 9 Conclusion

We examine the impact of the California Fair Pricing Law on a broad set of individual-level financial outcomes. The law operates by enacting a price ceiling on hospital bills for lower-income uninsured and lower-income insured individuals and by requiring the advertisement of hospitals' charity policies. We make use of a large, nationally representative panel data set of anonymized credit bureau records from 2003 to 2010, merged with detailed information on medical collections to study the financial consequences of the law.

We find that the CA FPL had limited impacts on medical collection outcomes but led to improvements in non-medical collection outcomes. For medical collections, we estimate no statistically significant impact on the probability of having any medical debt or on the likelihood of having a $0 collections balance. However, we find that a 10 percentage point increase in the uninsured rate leads to a statistically significant 0.5 percentage point reduction

[24] Due to data limitations, we are able to show emergency department visits for California and Arizona only. For this figure, we have four quarters of data before and 15 quarters of data after the law.

in the probability of having any non-medical debt in collections (a 4.7 percent decrease) and a modest but statistically significant 0.53 percentage point increase in the likelihood of having a $0 non-medical collections balance.

Broader indicators of financial distress show similar results. While the estimated effect on credit score is positive, but not statistically significant, we find evidence of improvements in delinquency outcomes. Our results show that a 10 percentage point increase in the uninsured rate leads to the share of delinquent debt falling by 0.012 percentage points (7.7 percent), and the number of delinquent accounts declining by 0.032 accounts (7.5 percent) following the FPL's implementation. Taken together, these findings suggest that the law, whose goal is to protect uninsured and underinsured patients from excessive hospital charges, generated modest but meaningful improvements in individuals' financial well-being.

The consistent improvements in non-medical collections and delinquency outcomes suggest that reducing hospital bills may free up financial resources, alleviating liquidity constraints and enabling individuals to better manage other financial obligations. These findings underscore the broader welfare implications of health-related consumer protection policies: By reducing exposure to large medical expenses, such policies may be able to improve individuals' overall financial well-being.

# References


Adams, A., R. Kluender, N. Mahoney, J. Wang, F. Wong, and W. Yin (2022). The impact of financial assistance programs on health care utilization: Evidence from Kaiser Permanente. *American Economic Review: Insights 4*(3), 389–407.

AHRQ (2020). Fast stats. Healthcare cost and utilization project (HCUP). Available at `https://datatools.ahrq.gov/hcup-fast-stats` [Accessed: March 2022].

Anderson, G. F. (2007). From 'soak the rich' to 'soak the poor': Recent trends in hospital pricing. *Health Affairs 26*(3), 780–789.

Aouad, M., T. T. Brown, and C. M. Whaley (2019). Reference pricing: The case of screening colonoscopies. *Journal of Health Economics 65*, 246–259.

Argys, L. M., A. I. Friedson, M. M. Pitts, and D. S. Tello-Trillo (2020). Losing public health insurance: Tenncare reform and personal financial distress. *Journal of Public Economics 187*, 104202.

Bai, G. (2015). California's Hospital Fair Pricing Act reduced the prices actually paid by uninsured patients. *Health Affairs 34*(1), 64–70.

Bai, G. and G. F. Anderson (2015). Extreme markup: The fifty US hospitals with the highest charge-to-cost ratios. *Health Affairs 34*(6), 922–928.

Bailey, J., N. Blascak, and S. Mikhed (2025). Missouri's Medicaid contraction and consumer financial outcomes. *American Journal of Health Economics 11*(4), 529–564.

Barcellos, S. H. and M. Jacobson (2015). The effects of Medicare on medical expenditure risk and financial strain. *American Economic Journal: Economic Policy 7*(4), 41–70.

Batty, M. and B. Ippolito (2017a). Financial incentives, hospital care, and health outcomes: Evidence from fair pricing laws. *American Economic Journal: Economic Policy 9*(2), 28–56.

Batty, M. and B. Ippolito (2017b). Mystery of the chargemaster: Examining the role of hospital list prices in what patients actually pay. *Health Affairs 36*(4), 689–696.

Brevoort, K., D. Grodzicki, and M. B. Hackmann (2020). The credit consequences of unpaid medical bills. *Journal of Public Economics 187*, 104203.

Brot-Goldberg, Z. C., A. Chandra, B. R. Handel, and J. T. Kolstad (2017). What does a deductible do? The impact of cost-sharing on health care prices, quantities, and spending dynamics. *Quarterly Journal of Economics 132*(3), 1261–1318.

CA Demographic Research Unit (2009). State of California, Department of Finance, California current population survey report: March 2007.

California Assembly (2007). Ab 774 assembly bill. `www.leginfo.ca.gov/pub/05-06/bill/asm/ab_0751-0800/ab_774_bill_20060929_chaptered.html` [Accessed: August 2022].

Canilang, S., C. Duchan, K. Kreiss, J. Larrimore, E. A. Merry, and M. Zabek (2020). Report on the economic well-being of US households in 2019, featuring supplemental data from April 2020. Technical report, Board of Governors of the Federal Reserve System (US).

CFPB (2014). Consumer credit reports: A study of medical and non-medical collections. Technical report, Consumer Financial Protection Bureau.

CFPB (2023). Fair Debt Collection Practices Act: CFPB Annual Report 2023. Technical report, Consumer Financial Protection Bureau.

Cooper, Z., J. Han, and N. Mahoney (2021). Hospital lawsuits over unpaid bills increased by 37 percent in Wisconsin from 2001 to 2018: Study examines hospital lawsuits over unpaid bills in Wisconsin. *Health Affairs 40*(12), 1830–1835.

Courtemanche, C., J. Marton, B. Ukert, A. Yelowitz, and D. Zapata (2017). Early impacts of the Affordable Care Act on health insurance coverage in Medicaid expansion and non-expansion states. *Journal of Policy Analysis and Management 36*(1), 178–210.

Cubanski, J., T. Neumann, and M. Freed (2023). Explaining the Prescription Drug Provisions in the Inflation Reduction Act. Available at `https://www.kff.org/medicare/issue-brief/explaining-the-prescription-drug-provisions-in-the-inflation-reduction-act/` [Accessed: July 2023].

Dobkin, C., A. Finkelstein, R. Kluender, and M. J. Notowidigdo (2018). The economic consequences of hospital admissions. *American Economic Review 108*(2), 308–52.

Engelberg, J. and C. A. Parsons (2016). Worrying about the stock market: Evidence from hospital admissions. *Journal of Finance 71*(3), 1227–1250.

Euhus, R., E. Williams, A. Burns, and R. Rudowitz (2025). Allocating CBO's estimates of federal Medicaid spending reductions across the states: Enacted reconciliation package. Technical report, Kaiser Family Foundation.

Finkelstein, A. (2007). The aggregate effects of health insurance: Evidence from the introduction of Medicare. *Quarterly Journal of Economics 122*, 1–37.

Finkelstein, A., S. Taubman, B. Wright, M. Bernstein, J. Gruber, J. P. Newhouse, H. Allen, K. Baicker, and O. H. S. Group (2012). The Oregon health insurance experiment: Evidence from the first year. *Quarterly Journal of Economics 127*(3), 1057–1106.

Gibbs, C., B. Guttman-Kenney, D. Lee, S. Nelson, W. van der Klaauw, and J. Wang (2025). Consumer credit reporting data. *Journal of Economic Literature 63*(2), 598–636.

Gross, T. and M. J. Notowidigdo (2011). Health insurance and the consumer bankruptcy decision: Evidence from expansions of Medicaid. *Journal of Public Economics 95*(7-8), 767–778.

Hu, L., R. Kaestner, B. Mazumder, S. Miller, and A. Wong (2018). The effect of the Affordable Care Act Medicaid expansions on financial wellbeing. *Journal of Public Economics 163*, 99–112.

Kluender, R., N. Mahoney, F. Wong, and W. Yin (2021). Medical debt in the US, 2009-2020. *Journal of the American Medical Association 326*(3), 250–256.

Lee, A., J. Ruhter, C. Peters, N. De Lew, and B. D. Sommers (2022). National uninsured rate reaches all-time low in early 2022. US Department of Health and Human Services Office of the Assistant Secretary for Planning and Evaluation.

Lee, D. and W. van der Klaauw (2010). An introduction to the FRBNY Consumer Credit Panel. Staff Report 479, Federal Reserve Bank of New York.

Lindahl, M. (2005). Estimating the effect of income on health and mortality using lottery prizes as an exogenous source of variation in income. *Journal of Human Resources 40*(1), 144–168.

Mazumder, B. and S. Miller (2016). The effects of the Massachusetts health reform on household financial distress. *American Economic Journal: Economic Policy 8*(3), 284–313.

Melnick, G. and K. Fonkych (2013). Fair pricing law prompts most California hospitals to adopt policies to protect uninsured patients from high charges. *Health Affairs 32*(6), 1101–1108.

Miller, S. (2012). The effect of insurance on emergency room visits: An analysis of the 2006 Massachusetts health reform. *Journal of Public Economics 96*, 893–908.

Office of Statewide Health Planning and Development (2021). Hospital fair pricing policies. Available at `https://oshpd.ca.gov/data-and-reports/cost-transparency/hospital-fair-pricing-policies/` [Accessed: June 2021].

Office of the Assistant Secretary for Planning and Evaluation (2007). 2007 HHS poverty guidelines. `https://aspe.hhs.gov/2007-hhs-poverty-guidelines` [Accessed: November 2023].

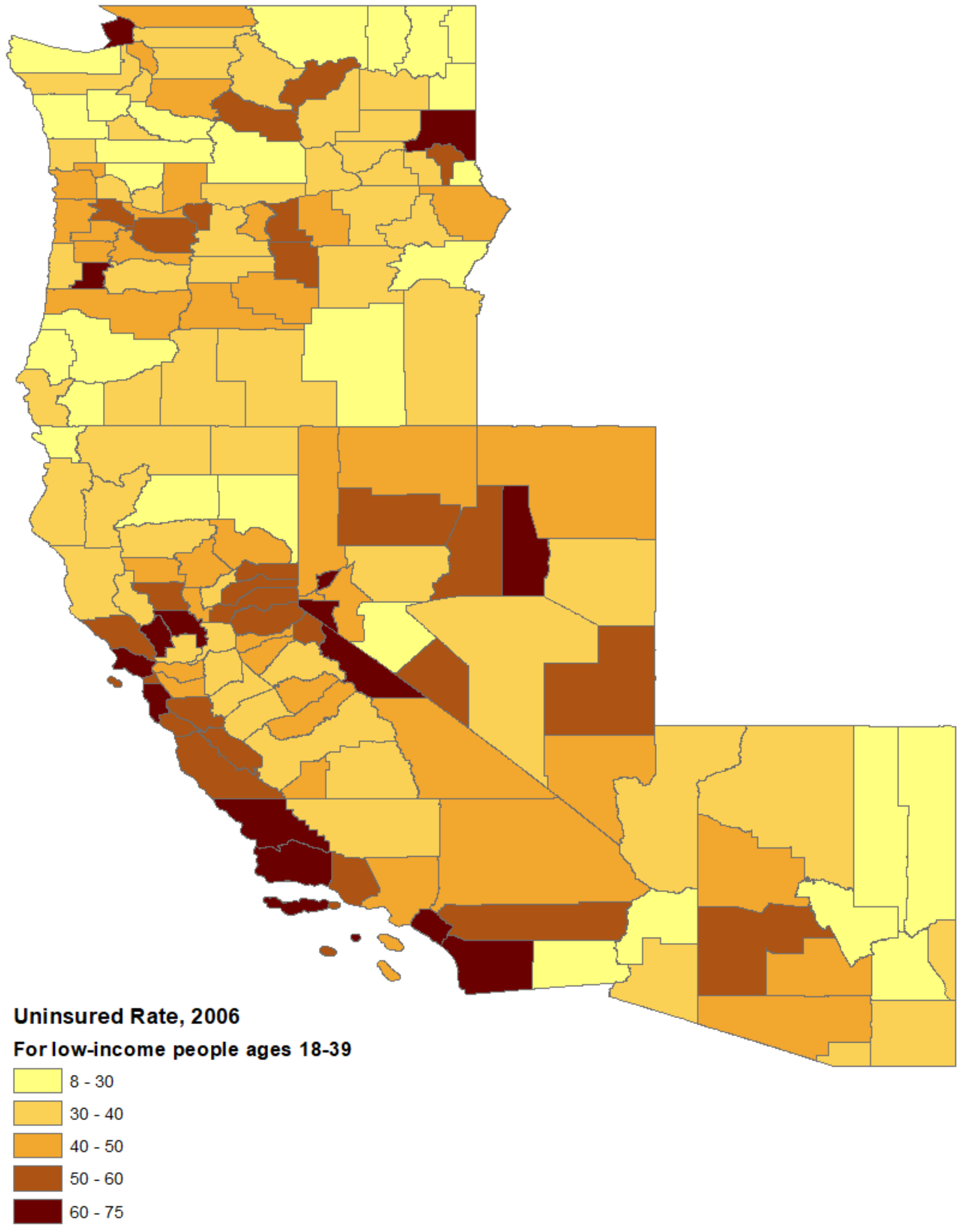
Rae, M., G. Claxton, K. Amin, E. Wager, J. Ortaliza, and C. Cox (2022). The burden of medical debt in the United States. Technical report, Kaiser Family Foundation.

Rothshild, M. and J. Stiglitz (1976). Equilibrium in competitive insurance markets: An essay on the economics of imperfect information. *Quarterly Journal of Economics 90*(4), 629–647.

Schwandt, H. (2018). Wealth shocks and health outcomes: Evidence from stock market fluctuations. *American Economic Journal: Applied Economics 10*(4), 349–77.

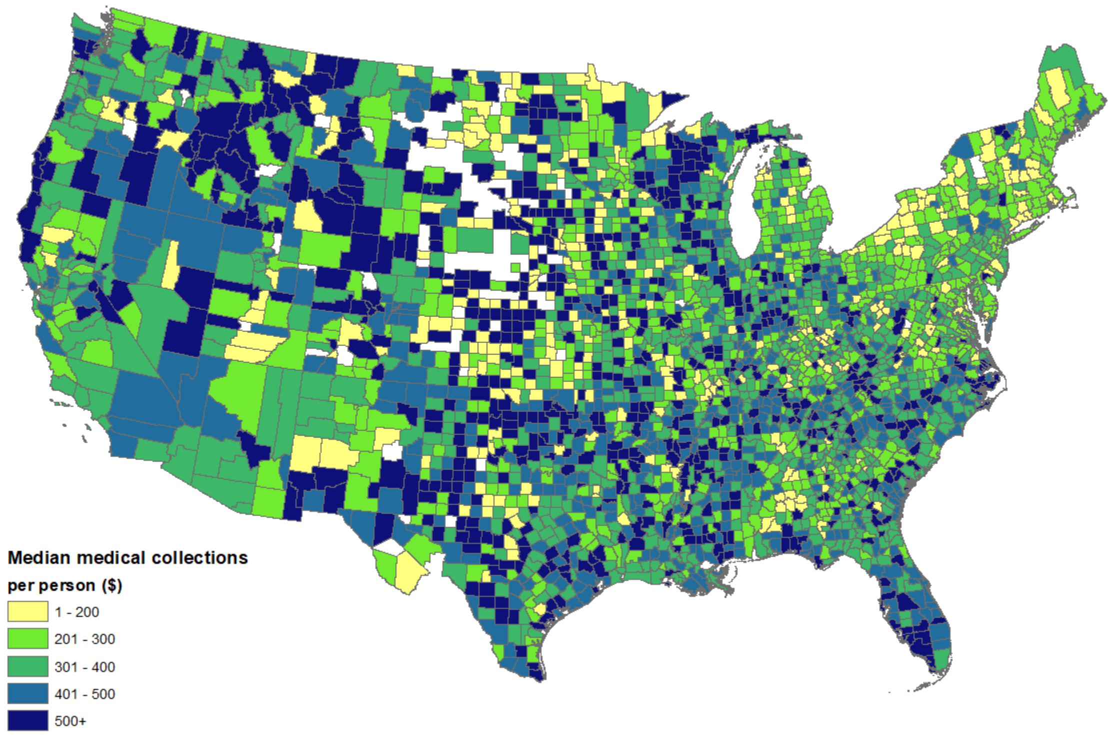
# ONLINE APPENDIX

Figure A1: **Uninsured Rate 2006**




Note: Authors' calculations using U.S. Census Bureau's SAHIE data. Uninsured rates are those for individuals ages 18 – 39 with incomes below 250% of the federal poverty level.

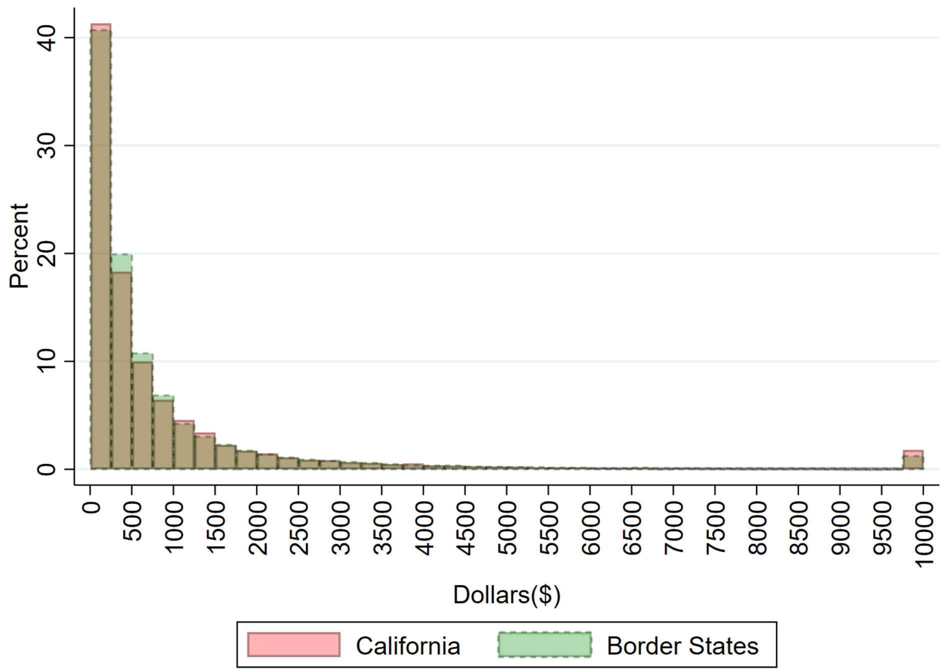
Figure A2: **Median Balance for Recent Medical Collections, 2006**



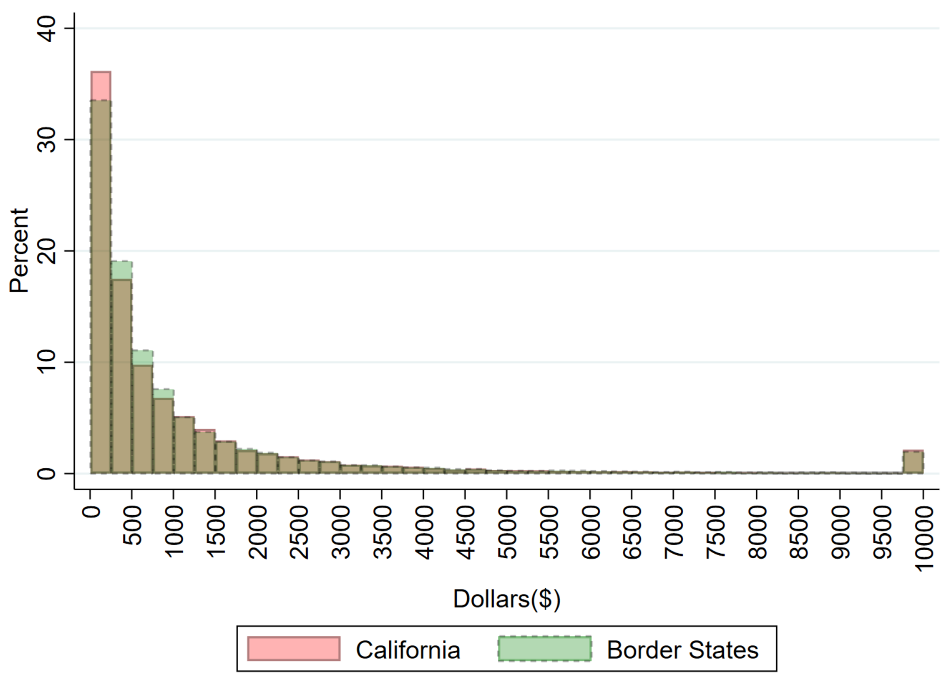

Note: Authors' calculations using Federal Reserve Bank of New York Consumer Credit Panel/Equifax data. Sample includes individuals ages 18 – 39. Recent medical collections are medical bills sent to a third-party debt collector in the past 12 months.

Figure A3: **Medical Collection Balance Distribution, Pre/Post FPL**

Panel A: Pre-FPL Period




Panel B: Post-FPL Period




Note: Authors' calculations using Federal Reserve Bank of New York Consumer Credit Panel/Equifax data. Sample restricted to individuals ages 18 – 39 with positive medical collection balances. Collection dollar amounts above $10,000 windsorized at $10,000.

Table A1: **Effect of FPL on Financial Distress: Exclude Oregon**

| | Medical Collections | | Non-medical Collections | |
|---|---|---|---|---|
| | Any | $0 Balance | Any | $0 Balance |
| **Panel A** | | | | |
| Treat × Exposure × Post | 0.009 | -0.013 | -0.057** | 0.060** |
| | (0.021) | (0.021) | (0.025) | (0.026) |
| Scaled Estimate (Exposure × 0.417) | .004 | -0.006 | -0.024 | 0.025 |
| Treatment Mean | 0.033 | 0.962 | 0.106 | 0.894 |
| $R^2$ | 0.362 | 0.358 | 0.392 | 0.394 |
| N | 2,055,570 | 2,103,946 | 2,055,570 | 2,052,934 |

| | Medical Collections | | Non-medical Collections | |
|---|---|---|---|---|
| | Number | Balance | Number | Balance |
| **Panel B** | | | | |
| Treat × Exposure × Post | 0.037 | 70.125 | -0.147*** | -30.449 |
| | (0.070) | (106.594) | (0.050) | (125.398) |
| Scaled Estimate (Exposure × 0.417) | 0.015 | 29.242 | -0.061 | -12.697 |
| Treatment Mean | 0.059 | 141.33 | 0.146 | 289.34 |
| $R^2$ | 0.393 | 0.248 | 0.371 | 0.281 |
| N | 2,055,642 | 2,053,008 | 2,055,642 | 723,407 |

Notes: Authors' calculation using Federal Reserve Bank of New York Consumer Credit Panel/Equifax, Census, FHFA, and BLS data. All collection variables are for accounts sent to a third-party debt collector in the past 12 months. The *Any* collections variable = 1 if an individual has a positive number of accounts in collections, and the *$0 balance* variable = 1 if an individual has $0 collection balance during the *Post* period. $Treat = 1$ if an individual lives in CA, $Exposure$ is a continuous measure of the county-level low-income young adult uninsured rate in 2006, and $Post = 1$ for the years 2007 to 2010. Sample includes individuals ages 18 – 39 and excludes individuals living in Oregon. Standard errors clustered at the county-level. Treatment mean is the pre-policy average for individuals living in CA counties. *** p<0.01, **p < 0.05, *p < 0.1.

Table A2: **Medical and Non-medical Collection Distributions: Exclude Oregon**

| | **Medical Debt Balance** | | | |
|---|---|---|---|---|
| | \$0 Balance | \$1 to \$1,000 | \$1,001 to \$2,000 | \$2,001+ |
| **Panel A** | | | | |
| Treat × Exposure × Post | -0.013 | 0.001 | -0.002 | 0.014 |
| | (0.020) | (0.015) | (0.005) | (0.010) |
| Scaled Estimate (Exposure × 0.417) | -0.005 | -0.0004 | -0.001 | 0.006 |
| Treatment Mean | 0.962 | 0.029 | 0.005 | 0.005 |
| $R^2$ | 0.358 | 0.290 | 0.210 | 0.257 |
| N | 2,104,018 | 2,104,018 | 2,104,018 | 2,104,018 |

| | **Non-medical Debt Balance** | | | |
|---|---|---|---|---|
| | \$0 Balance | \$1 to \$1,000 | \$1,001 to \$2,000 | \$2,001+ |
| **Panel B** | | | | |
| Treat × Exposure × Post | 0.076*** | -0.058*** | -0.0113 | -0.007 |
| | (0.024) | (0.016) | (0.007) | (0.006) |
| Scaled Estimate (Exposure × 0.417) | 0.032 | -0.024 | -0.005 | -0.003 |
| Treatment Mean | 0.894 | 0.084 | 0.013 | 0.009 |
| $R^2$ | 0.394 | 0.342 | 0.232 | 0.219 |
| N | 2,053,008 | 2,053,008 | 2,053,008 | 2,053,008 |

Notes: Authors' calculation using Federal Reserve Bank of New York Consumer Credit Panel/Equifax, Census, FHFA, and BLS data. All collection variables are for accounts sent to a third-party debt collector in the past 12 months. The *\$0 balance* variable = 1 if an individual has a \$0 collection balance, while the other balance variables = 1 if an individual has a balance in that specific range during the *Post* period. $Treat = 1$ if an individual lives in CA, *Exposure* is a continuous measure of the county-level low-income young adult uninsured rate in 2006, and $Post = 1$ for the years 2007 to 2010. Sample includes individuals ages $18 - 39$ and excludes individuals living in Oregon. Standard errors clustered at the county-level. Treatment mean is the pre-policy average for individuals living in CA counties. *** p<0.01, **p < 0.05, *p < 0.1.

Table A3: **Other Measures of Financial Distress: Exclude Oregon**

| | Risk Score | Share of Debt that is Delinquent (%) | Number of Delinquent Accounts |
|---|---|---|---|
| Treat × Exposure × Post | 27.235* | -0.149*** | -0.398** |
| | (14.428) | (0.055) | (0.157) |
| Scaled Estimate (Exposure × 0.417) | 11.357 | -0.062 | -0.166 |
| Treatment mean | 653.5 | 0.152 | 0.428 |
| $R^2$ | 0.787 | 0.613 | 0.460 |
| N | 1,985,259 | 1,663,094 | 2,016,103 |

Note: Authors' calculations using Federal Reserve Bank of New York Consumer Credit Panel/Equifax, Census, FHFA, and BLS data. $Treat = 1$ if an individual lives in CA, *Exposure* is a continuous measure of the county-level low-income young adult uninsured rate in 2006, and $Post = 1$ for the years 2007 to 2010. Sample includes individuals ages $18 - 39$ and excludes individuals living in Oregon. Standard errors clustered at the county-level. Treatment mean is the pre-policy average for individuals living in CA counties.*** p<0.01, **p < 0.05, *p < 0.1.