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WP 23-20

PUBLISHED
September 2023

FRB CFI
PHILADELPHIA

ISSN: 1962-5361

DOI: https://doi.org/10.21799/frbp.wp.2023.20

I've Got 99 Problems But a Bill Ain't One: Hospital Billing Caps and Financial Distress in California*

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August 28, 2023

Abstract

We examine the financial consequences of the 2007 California Fair Pricing Law (FPL), a law that places a price ceiling on hospital bills for uninsured and financially vulnerable individuals. Using difference-in-difference-in-differences models, we exploit cross-sectional variation in exposure to the law to estimate the causal effects of the FPL on different measures of financial distress. We find that the law reduces the medical and non-medical debt burden of individuals targeted by the law, with the likelihood of incurring non-medical debt in collections declining by 14.5 percent and the number of non-medical collections declining by 31 percent. The law also reduces the probability of having medical and non-medical debt balances between \$1 and \$1,000 in collections by 16.5 percent and 40 percent, respectively. Our results suggest that hospital billing regulations have direct and indirect effects on the personal financial outcomes of uninsured and financially vulnerable individuals.

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

JEL Codes: G51, I18, H75

*The authors would like to thank Bob Hunt, Julia Cheney, and seminar participants at University of Virginia Frank Batten School of Leadership and Public Policy, the Boulder Summer Conference on Consumer Financial Decision Making, and the American Society of Health Economists Conference for their helpful comments. Claire Jellison provided excellent research assistance.

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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 devastating 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). Given the strong link between personal finances and numerous 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 distress. Specifically, we trace the effects of exposure to this law among those most likely to be uninsured or underinsured across several personal financial outcomes. The CA FPL, enacted in 2007, limits the amount that uninsured patients and insured patients experiencing "burdensome" medical bills face after visiting a hospital.[2]

[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))."

[2] In particular, if a medical bill exceeds 10 percent of an insured individual's annual income, they are

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. Further, while it may seem trivial that price restrictions on hospital bills and the increased advertisement of charity care policies should directly affect the financial responsibility of individuals, we are the first to empirically investigate this question and quantify the direct and broader effects on a number of personal financial outcomes.

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 from 1999 to the present. We merge the CCP with a unique data set that contains detailed anonymized information on medical debt owed to third-party debt collectors and examine several personal financial outcomes around the time of the policy's enactment.[3]

Our analysis proceeds in two steps. First, we estimate difference-in-difference-in-differences (DDD) models across California and its neighboring states, and within states, comparing (over time) counties that have varying levels of pre-policy "exposure" to the FPL (i.e., pre-policy uninsured rates). Specifically, in CA and its neighboring states, we compare outcomes for younger adults, ages 19 to 39 years, who live in areas with "lower" rates of health-insurance coverage to similarly aged individuals who live in areas with "higher" rates of health-insurance coverage, before and after the implementation of the FPL.[4] Second, we repeat the above exercise, but instead focus on individuals who are 65 years and older. In doing so, we aim to isolate and examine the impacts of the introduction of financial assistance policies prompted by the CA FPL, particularly among elderly individuals who may be financially vulnerable. Focusing on the 65 years and older group likely achieves this, as individuals above 65 years are likely to already have health insurance (i.e., Medicare); as such, these individuals will be most impacted by the changes

eligible for protections under the California Fair Pricing Law.

[3]Our analyses focus on both medical and non-medical debt owed to third-party debt collectors (i.e., third-party collections).

[4]This approach is similar to that of Finkelstein (2007), Miller (2012), and Mazumder and Miller (2016).

in the written adoption and advertisement of financial assistance policies brought on by the law, rather than price changes.

We find that the introduction of the CA FPL resulted in a marginally significant 33 percent decrease in the probability of incurring medical debt within three years of the law's enactment among younger adults. Moreover, this effect appears to be driven by reductions in medical debt balances between $1 and $1,000. In particular, we find that the enactment of the CA FLP resulted in a 1 percentage point decrease (a relative 40 percent reduction) in the probability of having a medical debt in collections between $1 and $1,000, within the first three years of the law's introduction. Interestingly, we only 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 reducing the size of medical debt *balances* rather than reducing the number of delinquent accounts for younger adults. We also find effects of the law on medical debt balances for adults older than 65. Specifically, for this group, we find that the CA FPL led to a 9 percent reduction in medical debt balances in collections, which implies that the impacts of the law are not limited to younger adults.

We also find 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 1.45 percentage points (a relative 14.5 percent decrease), and decreases the number of non-medical accounts sent to collections by 0.042 accounts (a relative 31 percent decrease). 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., Mazumder and Miller (2016) and Hu et al. (2018)). These studies have found that interactions with the health-care system can be costly both in 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 regulations, namely 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 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 setting by the government and its effects on consumers' financial well-being. This examination may be useful for understanding the financial impacts in other contexts, such as changes to the federal government's ability to negotiate drug prices (for select medications), as part of the *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 that occur as a result 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.

We also examine the financial impacts of the CA FPL on the 65 and older population.

This group is often presumed to be well insulated from the financial consequences of health-care utilization because of their near universal coverage through Medicare (e.g., Card et al., (2008) and Engelhardt and Gruber (2011)). As a result, there are fewer studies examining the financial strain of health-care use on this population (e.g., Caswell and Goddeeris (2020) and Barcellos and Jacobson (2015)), despite the documented financial hardships among the elderly. For example, Johnson et al. (2021) find that, "69 percent of older adults experience hardship for at least one year after age 65, and 53 percent experience hardship for at least three years." Thus, our study adds to the literature by examining how policies that affect hospital pricing and the advertisement of charity care can mitigate the financial costs of health-care utilization for the elderly.

The rest of our paper will proceed as follows: Section 2 discusses the law's background, while Section 3 presents the conceptual framework. Section 4 describes our data, and Section 5 discusses our Methods. We present our results and discussion in Sections 6 and 7 and conclude in Section 8.

2 Policy Background

The California Fair Pricing Law (i.e., 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 two groups of patients: i) uninsured patients who are at or below 350 percent of the federal poverty line, and ii) insured patients who incur medical expenses that are greater than 10 percent of their household income (California Assembly, 2007). Specifically, hospitals charges were now 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).[5]

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 19 and 39, given their traditionally lower rates of 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 very large and sizable differences in hospital's 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. In comparison to the billing caps imposed by the law, the 65 and older group will likely benefit most from the charity care aspect of the law since this group already has near universal insurance coverage through Medicare and already benefits from Medicare-negotiated prices.

3 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.[6]

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 indi-

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

[6]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.

viduals face the full list price of any care they receive and are at strategic disadvantage when bargaining with health-care providers for reduced bill amounts (Batty and Ippolito (2017a)). Therefore, gaining access to this kind of financial protection lowers the price they face and, consequently, the total medical expenditures that they incur, for a given hospitalization.

Lower medical expenditures, via reductions in price (through price caps or financial assistance programs), increases "real income," which may have positive financial spillovers. Having additional financial resources may allow individuals to pay existing debt or prevent future delinquencies or the accumulation of future debt.[7] 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.[8]

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

[7]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.

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

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.

4 Data

To explore 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) merged with a data set of unpaid medical bills owed to third-party debt collectors.[9] 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. Individuals are followed at a quarterly frequency until they die, change their SSN, or drop off due to an extended period of credit market inactivity. While the CCP contains extensive information regarding credit data, it does not contain any demographic information besides year of birth and census geography. In a given quarter, the CCP contains data on approximately 12 million different consumers.[10]

The medical collections data are an approximately 40 percent anonymized random sample of individuals in the CCP with and without an unpaid medical account owed to a third-party debt collector on their credit report. We have detailed information on up to 10 accounts per individual, 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 date is, however, reported directly to Equifax by the debt collection agency. We can observe medical collection information at the end of the fourth quarter of each year from 2003 to 2010. We refer to the unpaid medical bills that the collection agencies report to Equifax as "medical collections" or "medical debt."

We also use data on county characteristics obtained from several sources. County-

[9]The latter data set was obtained by the Federal Reserve Bank of Philadelphia's Consumer Finance Institute.

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

level annual estimates of health-insurance coverage status data comes from the U.S. Census Bureau's Small Area Health Insurance Estimates (SAHIE) program. We also use data on 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.

4.1 Outcomes

We consider two types of collection accounts in our analysis: medical and non-medical collections. We categorize medical collections as severely 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/unpaid bills, which can include those for utilities and telecommunication services. Both measures are for late bills and/or loans that are being pursued by a third-party debt collection firm.

During our period of study, there was no uniform standard for reporting unpaid medical bills to debt collectors, unlike other forms of debt that are reported as being 30, 60, 90, or 120 days late. This means that we observe medical debt only when it is sent to a collections agency and subsequently reported to the credit bureau. The lack of a uniform reporting standard means that the date that we observe new medical collections does not necessarily coincide with the date that the medical collection was actually accrued. Also, the medical debt we observe does not capture the universe of medical collections. This is because medical bills paid using a credit card will not appear in our medical collections data and 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 be reported in any credit database.

We first consider the extensive margin effects of the CA FPL on financial outcomes. This is done by analyzing the probability of having any medical or non-medical collections reported to Equifax in the past 12 months, as well as the probability of having a $0 medical or non-medical balance in collection in the past 12 months. These outcomes allow us to investigate if the CA FPL was able to protect individuals from adverse financial distress

due to the high cost of medical care. We also consider the intensive margin effects of the CA FPL on financial outcomes by evaluating its impact on the number of medical and non-medical collections owed to a third-party debt collector and on the amount of medical and non-medical debt in third-party collections in the past 12 months. We also explore the heterogeneous effects of the law on the distribution of medical and non-medical debt by binning individual's collection balances within a range of values. In particular, we create four bins of balance ranges: \$0, \$1 – \$1,000, \$1,001 – \$2,000, and greater than \$2,000.

4.2 Analysis Sample

Our main analysis sample consists of individuals from the aforementioned states who were 19-to-39 years old between 2003 and 2010. We focus on this group because it has had traditionally lower rates of health-insurance coverage, particularly in the years examined; as such, they will likely be the most impacted by the hospital price caps imposed by the CA FPL. 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 (e.g., see Mazumder and Miller (2016)). We also restrict the last year of our analysis to 2010 since California expanded Medicaid in 2011.[11]

We also explore the charity care impacts of the law by examining individuals who are 65 and older in 2007 when the law was implemented. For this population, we are interested in whether the adoption and advertisement of hospital financial assistance policy had an impact on their financial outcomes. We describe this older adult CCP sample in more detail in Section 6.3.

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

5 Methodology

Our identification strategy relies on leveraging across-county variation in uninsured rates for individuals residing in California (CA) to individuals living in neighboring states of Washington, Oregon, Nevada, and Arizona (WA, OR, NV, and AZ). We take this approach under the assumption that individuals living in nearby border states are similar to those in California in their propensity to experience debt/medical debt. This approach is similar to that of Mazumder and Miller (2016) and Blascak et al. (2021), who use individuals in nearby border states to form their control group.

We estimate the effect of the CA FPL on financial outcomes using a difference-in-difference-in-differences (DDD) design. Our DDD strategy begins by comparing individuals who live in California and those who live in border states before and after 2007, when the FPL was implemented. The final difference uses county-level variation in the uninsured rate for low-income adults. This constructed measure identifies individuals who are most likely to be impacted by the law (i.e., individuals who are more likely to be lower income and uninsured). Similar to Mazumder and Miller (2016), we refer to this variable as a measure of *exposure* to the FPL, as it provides within-state geographic variation on the likelihood of an individual being affected by (i.e., exposed to) to the FPL. Specifically, this variable 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 19-to-39 year olds across all states in the sample (i.e., CA, OR, WA, NV, and AZ) in 2006.[12] In 2006, this median uninsured rate was 39.9 percent.

We estimate the following DDD equation:

$$\begin{aligned} Y_{it} = \beta_0 + \pi(CA \times Exposure_i \times Post) + \theta(CA_i \times Post) + \psi(Exposure_i \times Post) \\ + \beta_1(CA_i \times Exposure_i) + \beta_2 CA + \beta_3 Exposure_i + \Gamma T_t + \mathbf{\Omega} X + C_c + I_i + \epsilon_{it} \quad (1) \end{aligned}$$

Y_{it} refers to the financial outcomes of interest outlined in Section 4.1, *Post* takes the value

[12]Defining exposure in this way differs from the Courtemanche et al. (2017) and Mazumder and Miller (2016) studies, which use a continuous measure of the uninsured rate instead of a binary variable. We prefer to use a binary exposure status as it allow us to move away from interpretations involving small, local movements at a specific exposure level. We conduct robustness tests using a continuous measure of our exposure variable in Section 6.4.

one for years occurring after the law's introduction in 2007 and is zero otherwise. We include county fixed effects (C_c) to account for time-invariant county characteristics, and I_i accounts for individual fixed effects. X contains our vector of controls, which includes age and household-size fixed effects. We also control 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.[13]

π is the parameter of interest in Equation 1. If our identifying assumptions hold, π, captures the added effect of an increased exposure to FPL on the debt outcomes of interest. The identifying assumption for our DDD model is that in the absence of the CA FPL, outcomes would have trended similarly for those individuals living in "higher exposure" counties versus those living in "lower exposure" counties across treatment and control groups. To test the validity of our parallel trends assumptions, we adjust Equation (1) and replace the variable *Post* with time dummy variable vector T_t to test if there is stability across our outcomes of interest in the pre-FPL period. Our estimating equation becomes:

$$Y_{it} = \beta_0 + \mathbf{\Pi}(CA \times Exposure_i \times T_t) + \mathbf{\Theta}(CA_i \times T_t) + \mathbf{\Psi}(Exposure_i \times T_t) + \beta_1(CA_i \times Exposure_i) + \beta_2 CA + \beta_3 Exposure_i + \Gamma T_t + +C_c + I_i + \mathbf{\Omega} X + \epsilon_{it} \quad (2)$$

6 Results

6.1 Summary Statistics

We present summary statistics of our outcome variables in Table 1. In both CA and comparison states, individuals are more likely to have non-medical accounts rather than medical accounts in collection. This may be due to differences in how medical and non-medical debt are reported to credit agencies as discussed in Section 4.1. Additionally, in CA prior to the introduction of the law, the share with medical debt in collections in

[13]We account for 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.

counties above (below) the median uninsured rate was 3 percent (6 percent); after the law was implemented, this share increases to 5 percent (8 percent). Analogous shares in comparison states are higher: In higher exposure counties in comparison states, the share with medical debt in collections was 6 percent before 2007, which increases to 9 percent after 2007. In lower exposure counties, the share with medical debt increases from 8 percent, prior to 2007, to 11 percent afterward. The share with non-medical debt increased from 10 (14) percent to 11 (16) percent in higher (lower) exposure counties in CA pre- and post-CA FPL, respectively. The same trend in the comparison states was mostly flat – in higher exposure counties, the share increased by 1 percentage point from 12 percent to 13 percent and stayed the same at 15 percent in lower exposure counties.

All counties, irrespective of exposure classification and state, experienced a decline in the share with a zero medical debt balance after 2007. In CA, both county types experienced a 2 percentage point decline, and in non-CA states, the decline was 3 percentage points. The share with a zero non-medical balance in higher exposure counties in both CA and comparison states are 68 percent before 2007 and 67 percent after 2007. There is a 2 percentage point drop to 65 percent for lower exposure counties in CA and a 1 percentage point increase to 66 percent for lower exposure counties in comparison states.

Overall, the number of medical accounts in collections is lower in CA with an average of 0.05 in higher exposure counties before the CA FPL and 0.07 after the CA FPL. Lower exposure counties also increase from 0.11 accounts to 0.15 accounts. In non-CA states, the average in higher exposure counties in CA increases from 0.11 accounts to 0.19 accounts and from 0.15 accounts to 0.23 accounts in lower exposure counties. Based on these statistics, it appears that differences in our main outcomes are relatively small when comparing the treatment and control states, as well as the higher versus lower exposure counties.

6.2 Main Results

Our main results from the estimation of Equation 1 are reported in Table 2 with corresponding event study graphs in Figures 1 and 2. The main coefficient of interest, π, captures the effect of an increased exposure to the FPL on the probability of owing any

medical or non-medical debt in the past 12 months.

In general, our estimates of π are negative for both medical and non-medical debt outcomes. Specifically, among younger adults, we find that the law reduces the probability of having any medical collections in the past 12 months by a marginally significant 1.1 percentage points, representing an approximately 33 percent decrease relative to the pre-period mean of the treatment group. Some of this decline may be due in part to the delayed medical bill reporting requirement provision of the law. However, the impact of this delayed reporting provision is likely limited, given our results in Figure 2 (panel A), which shows little change, relative to the pre-period when the reporting requirement had not been enacted, in the number of medical accounts sent to third-party collections. We also find that the passage of the CA FPL reduces the probability of having any non-medical collections in the past 12 months by a statistically significant 1.45 percentage points. Compared to the pre-CA FPL mean of 0.10 for our treated sample, this suggests a decrease of approximately 14.5 percent. These effects are sizable and suggest that the passage of the law has direct impacts on the extensive margin of debt accumulation for both medical and non-medical debt.

To further illustrate the impacts of the FPL, we examine our event-study estimates, which demonstrate the dynamic effects of the law. Panel A of Figure 1 shows an immediate decline in the probability of having any medical collections after the passage of the FPL. However, this decline is transitory and is not sustained in subsequent years. In panel C of Figure 1, we find that the introduction of the FPL leads to a reduction in the probability of having non-medical collections that precede the FPL law in 2005. However, estimates after 2007 are larger and more precise compared to estimates before 2007. In panel A, the plot provides strong graphical support for the parallel trends assumption, while in panel C, we note that there is one marginally statistically significant event study coefficient for the year 2005.[14]

We next examine the impact of the CA FPL on the probability of having a \$0 balance in collections for medical and non-medical debt in the past 12 months. As panel A of Table 2 shows, the probability of having a \$0 balance in collections for medical

[14] A Wald test of joint significance of all the pre-policy event study coefficients shows that they are not statistically different from zero.

debt increased by approximately 0.1 percentage point for individuals residing in treated counties, although the 95 percent confidence interval for this estimate narrowly includes zero. Similar to our results on the probability of having any medical debt, in panel B of Figure 1, we observe an immediate increase in this outcome followed by a decline that stays nearly the same for the rest of our sample period. Our results also indicate that the FPL increased the probability of having a $0 non-medical collections balance in the past 12 months for younger adults. Specifically, we find that the probability of having a $0 non-medical collection balance increased by a statistically significant 1.5 percentage points (1.7 percent) after the passage of the CA FPL.

Our next set of results are presented in panel B of Table 2 and show the effect 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 collections, we estimate that the CA FPL led to a statistically significant decline in the number non-medical accounts sent to collections in the past 12 months by 0.042 accounts. This represents a 31 percent decrease relative to pre-CA FPL mean. For the remaining outcomes (i.e., the amount of medical debt and non-medical debt in collections), our results are not statistically different from zero. This may be because of the relatively small balances and number of accounts held in third-party collections prior to the enactment of the law, which are $136 and $295, respectively. This stands in contrast to other studies, such as that of Finkelstein et al. (2012), who observe much larger balances in their sample. For example, Finkelstein et al. (2012) find that among the control patients observed in the Oregon Health Insurance Experiment (i.e., those who did not win the Medicaid lottery), approximately 28 percent had medical debt in collections, with approximately $2,000 owed, while the average amount owed in non-medical collections was approximately $2,740.

To examine the distributional impacts of the law, we explore whether the CA FPL affected the distribution of account balances. We use the same categories as Mazumder and Miller (2016) and examine collection balances of $0, $1 to $1,000, $1,001 to $2,000, and over $2,000 for both medical and non-medical collections in the past 12 months.

Table 3 presents our distributional results. For medical debt in collections, we find

that balances between $1 and $1,000 decreased by a large and statistically significant 1 percentage point, which represents a decrease of approximately 40 percent from the treatment group's pre-policy mean. Our estimates of the effect of the CA FPL on medical debt balances between $1,001 and $2,000 and over $2,001 are small and not statistically significant. However, for non-medical debt outcomes, we find negative effects of the law along the distribution of debt. Panel B of Table 3 shows that the CA FPL increases the probability of having a $0 non-medical debt balance by 1.5 percentage points, a relative increase of approximately 1.6 percent from the pre-period mean. Similarly, we find that the introduction of CA FPL decreased the probability of having non-medical debt balances in collection between $1 and $1,000 by 1.3 percentage points, a relative 16.5 percent decrease.

Overall, we find that the CA FPL resulted in declines in debt in third-party collections. For medical collections, we find effects that are marginally statistically significant and larger in relative terms when compared to estimates for non-medical debt. We note, however, that some results are suggestive because of larger standard errors. In addition, during this time period, hospitals varied in how aggressively they pursued such debts, likely yielding differences in when and how medical debt was reported to credit agencies (Consumer Financial Protection Bureau (2014)).[15] For *non*-medical collections, we find consistent evidence that the CA FPL reduced the number of non-medical accounts sent to debt collectors for individuals more likely to be exposed to the law. This result may be explained by people using credit cards (i.e., non-medical debt) to pay medical bills. For example, a recent survey by the Kaiser Family Foundation and the *New York Times* finds that about 34 percent of people with difficulty paying their medical bill increased their credit card debt to pay medical bills (Hamel et al., 2016), which is consistent with our findings.

6.3 Charity Care Effects of the CA FPL

In this subsection, we examine the charity care impacts of the CA FPL. To do this, we examine the effects of the law on the elderly (i.e., those 65 years and older). This

[15]For example, the CA FPL requires hospitals to wait before reporting an individual to collections if they are eligible for the law (see Section 1).

group is most likely impacted by the charity care aspects of the law since they are likely insured through Medicare, but they may still remain financially vulnerable. For example, in 2006, when the CA FPL law was passed, health-care spending for individuals 65 years and older in the US was more than four and a half times the spending of those between 19 and 44 years old at $16,400 versus $3,579, respectively (Centers for Medicare and Medicaid Services (2014)). Additionally, about 9.4 percent of persons 65 years and older live in poverty (Li and Dalaker (2022)).

To examine the charity care impacts, we reestimate Equation (1) with a modification to the exposure variable and sample. We use a new sample of the CCP data that comprises individuals over the age of 65 years in 2007 living in California and its neighboring states. To avoid measurement error induced by our inability to observe birth months, we drop those who are 65 and younger. We also make an adjustment to our exposure variable. Specifically, we create a new exposure measure using information on county-level poverty rates from the Census's Small Area Income and Poverty Estimates (SAIPE) program. We use the SAIPE data to construct this new exposure measure because it may better capture (proxy for) geographic areas that have older, financially vulnerable adults, than our previous measure, which is based on the uninsured rate of young adults. Our exposure measure is binary and now takes the value of one for individuals who live in a county with an overall poverty rate greater than or equal to the median poverty rate across all states in the sample (CA, OR, WA, NV, and AZ) in 2006 (14.4 percent), and is zero otherwise.

Our results from this analysis are reported in Tables 4 and 5. We generally find smaller effects of the charity care aspects of the law on financial outcomes for older adults compared to our main results. For example, panel A of Table 4 shows that the estimated effects of the law on the probability of having any medical or non-medical debt in collections are not statistically different from zero for individuals who are 65 years and older. However, a notable result is in panel B, where we find that the balance of medical debt in collections declined by a statistically significant $10.50 (9.1 percent). Our results from Table 5 suggests that the reduction in medical debt in collections ($10.50) may be driven by upper-end reductions, specifically coming from reductions in the $1,001 to $2,000 range.

In sum, there is limited evidence that the charity care aspects of the law were as

impactful as the billing caps imposed by the law. However, the relatively small effects found among the 65 and older population are consistent with the fact that Medicare enrollees already have generous insurance coverage in contrast to younger adults. As such, our examination does not rule out the possibility that younger adults benefited from both the pricing caps imposed by the law *and* the charity care policies. Unfortunately, we are not able to disentangle these program effects separately for younger adults.

6.4 Robustness Checks

6.4.1 Continuous Measure of Exposure

In our previous analyses, we used a binary measure to define our exposure measure. As a robustness check, we use a continuous measure of exposure. Specifically, we define our exposure measure as the pre-treatment (2006) uninsured rate for each county as done in prior work (e.g., Finkelstein (2007), Miller (2012), Mazumder and Miller (2016), and Courtemanche et al. (2017)). Our estimating equation is similar to Equation 1, but the exposure measure is now defined as the county-level uninsured rate for lower-income young adults. Because we use a continuous measure of young adult uninsured rate, we scale all coefficients by the mean pre-treatment uninsured rate.[16]

Tables 6 and 7 present our results and indicate that the point estimates are of the same sign and are generally larger than our main results. However, the standard errors for these estimates are larger than the estimates from our main results. We find that the probability of having any medical or non-medical debt in third-party collections declined after the law. The estimate for medical debt is not statistically different from zero, whereas the implied estimate of $-0.067 \times 0.417 = -0.028$ for non-medical debt at the mean pre-treatment uninsured rate is statistically significant at the 1 percent level. Relative to the pre-treatment mean of 0.109, this is a 25.7 percent decline. We also observe a $0.064 \times 0.417 = 0.027$ percentage point increase in the probability of having a zero balance in collections for non-medical debt after the law (a relative 3 percent increase). In sum, our estimates and standard errors are generally larger when using a continuous

[16] As mentioned previously, 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.

exposure measure, but the sign of the estimates remains consistent with our estimates that use a binary measure.

6.4.2 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 of states could bias our results toward zero since enrollment in Medicaid offers financial protections and because the population targeted by the CA FPL law and the Oregon Medicaid program are likely 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. The results are reported in Tables 8 and 9. The treatment effect estimates are not qualitatively different from our main results, which suggests that our main results are robust to the inclusion of Oregon in our analysis sample.

7 Discussion

We find that among 19-to-39 year olds living in higher exposure counties in California, the introduction of the CA FPL reduced the probability of having any medical debt in collections by 33 percent. The law also reduced the probability of having medical debt balance between $1 and $1,000 by 40 percent. In addition, the law reduced the probability of non-medical debt in collections in the past 12 months by 14.5 percent and decreased the number of non-medical accounts in collections in the past 12 months by 31 percent.

A possible explanation for the above results is an income effect induced by the CA FPL. This could occur since the law sets a price ceiling on hospital care for low-income and uninsured individuals. All else equal, uninsured individuals who use hospital care after the implementation of the law now face a lower hospital bill compared to those who used hospital care prior to the law.[17] In turn, lower hospital expenditures could affect the occurrence of debt in several ways. This could reduce the probability of having any

[17]For example, Bai (2015) finds that, "from 2004 to 2012 the net price actually paid by uninsured patients shrank from 6 percent higher than Medicare prices to 68 percent lower than Medicare prices."

medical debt, lower the credit balances for those who pay their medical bills with a credit card, and thereby increase income available for consumption and payment of other debts.

Another potential explanation for our results is that individuals in our comparison states and counties increased their credit utilization more than individuals in our treated counties due to reasons that are unrelated to hospital care. In fact, the CA FPL was passed just before the Great Recession. If the Great Recession led to higher unemployment and economic distress in comparison states and counties, individuals residing in these locations may have experienced differential financial distress, leading to more non-medical debt in collections and higher debt balances. Our results would then capture this differential financial distress rather than an impact of the CA FPL. However, our model accounts for this potential differential impact of the Great Recession by including county-level measures (i.e., unemployment rate, county housing price index, and the share of county establishments that are in manufacturing), thus limiting the strength of this channel.

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 a *relative* increase in their hospital use, as compared to individuals in California in the post-2007 period, this could trigger more medical and non-medical bills in collections for residents of comparison states. This would result in larger (biased) treatment effect estimates. We examine the validity of this concern by examining trends in adult hospitalizations and emergency department visits in California and comparison states over time.

Figure 3a shows quarterly adult hospitalizations by uninsured patients in California and all comparison states four years before and after the introduction of the CA FPL using state-level data from the Healthcare Cost and Utilization Project (HCUP). We do not observe any discernible change in hospitalizations in California or comparison states at or around the quarter when the law was implemented. Similarly, in Figure 3b, 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.[18] These

[18]Due to data limitations, we are able to show emergency department visits for California and Arizona

figures provide suggestive evidence that differential health-care utilization is not driving the observed effects.

8 Conclusion

We examine the impact of the California Fair Pricing Law (CA FPL) on a broad set of individual-level financial outcomes. The CA FPL operates by enacting a price ceiling on hospital bills for uninsured and financially vulnerable 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 debt owed to third-party debt collectors to study the financial consequences of the law.

Using difference-in-difference-in-differences (DDD) models, we find that the CA FPL has a large effect on *non*-medical debt in collections. Specifically, we find that the probability of incurring any non-medical debt in collections (in the past year) decreases by 14.5 percent, and the number of non-medical accounts in collections decreases by 31 percent. We also find that the law impacted medical debt. Specifically, we find a marginally significant 33 percent reduction in the probability of having any medical debt in collections after the law is enacted. This result appears to be driven by a reduction in medical debt balances between \$1 and \$1,000, as we find a 40 percent reduction in the probability of having a medical debt balance in collections in this dollar range after the CA FPL is introduced. Lastly, the charity care aspects of the law had limited effects on the 65 and older population. In particular, we observe small declines in medical debt balances in collections reported in the past year and declines in the probability of an individual having a medical debt balance between \$1,001 and \$2,000 in collections after the law took effect.

Our results suggest that the CA FPL reduces both the occurrence and balance of medical and non-medical debt in collections owed by individuals. To the extent that reductions in non-medical collections reflect broader improvements in financial well-being outside of an individual's medical bills, the law has both "direct" and "indirect" effects

only. For this figure, we have four quarters of data before and 15 quarters of data after the law.

on the occurrence and level of debt held by individuals. Understanding the impacts of such policies is important because the onset of medical debt may have downstream effects on other important outcomes. For example, debt due to medical care may prevent individuals from seeking timely health care, expose them to mental stress, and potentially increase their exposure to lawsuits, wage garnishments, or even bankruptcy (Consumer Financial Protection Bureau (2022)). Further, if the adoption of fair pricing laws does not induce a significant deterioration in the quality of health care provided by hospitals, the introduction of such laws may be a policy lever that can be used to protect the uninsured, underinsured, and financially vulnerable from expensive medical events.

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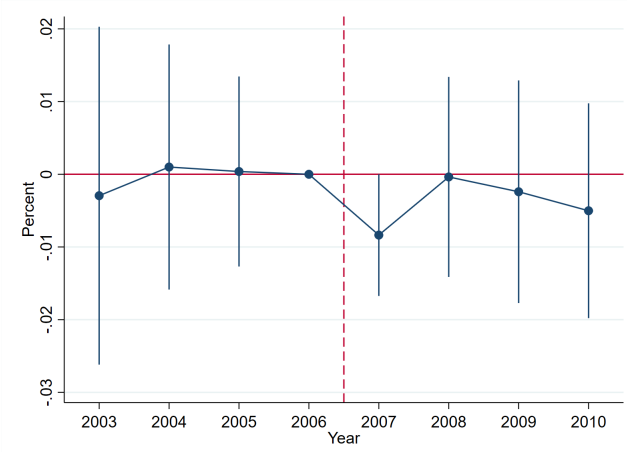
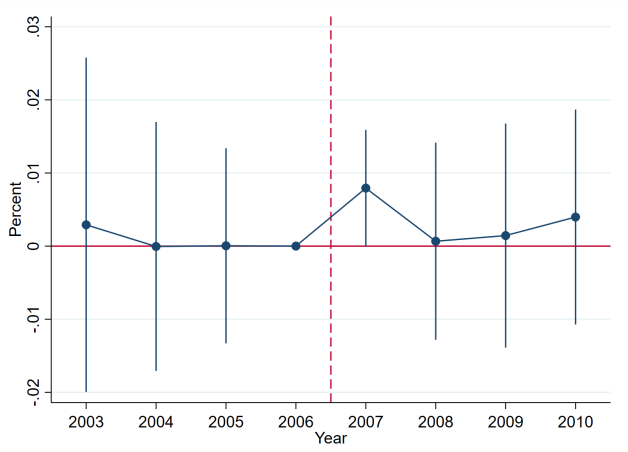
Figure 1: **Triple Difference Event Study Results**

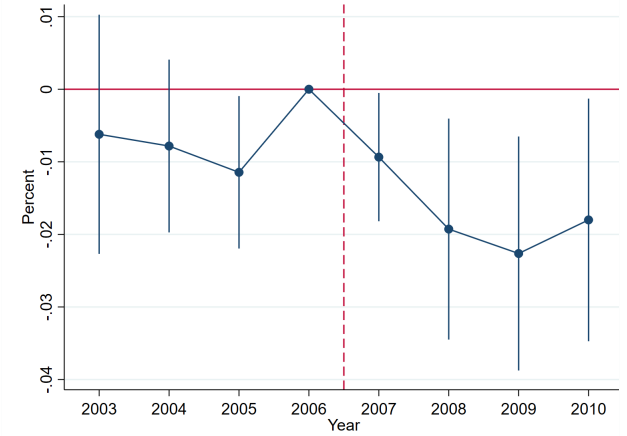
Panel A: Probability of having a medical account in third-party collections



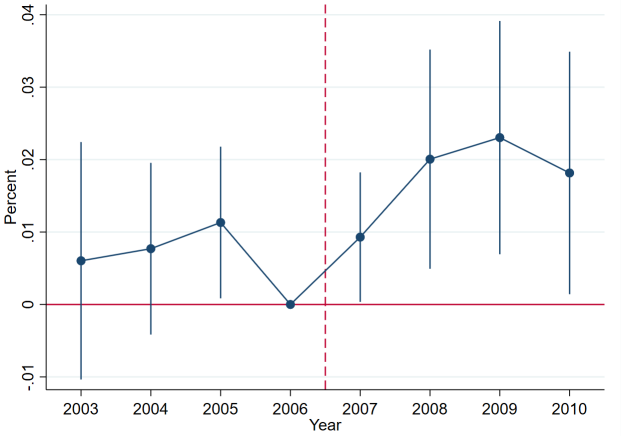
Panel B: Probability of having a $0 medical collection balance



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



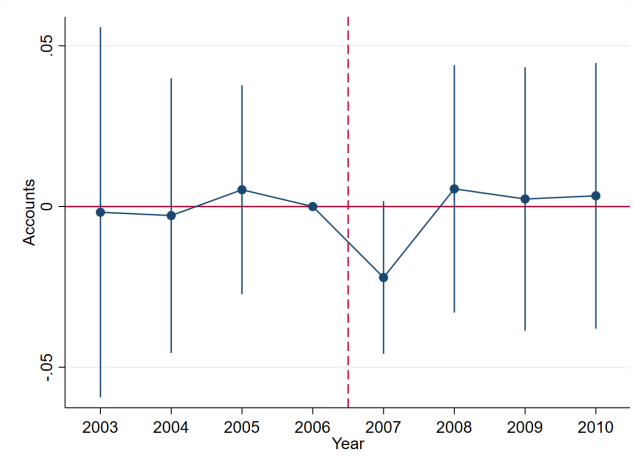
Panel D: Probability of having a $0 non-medical collection balance



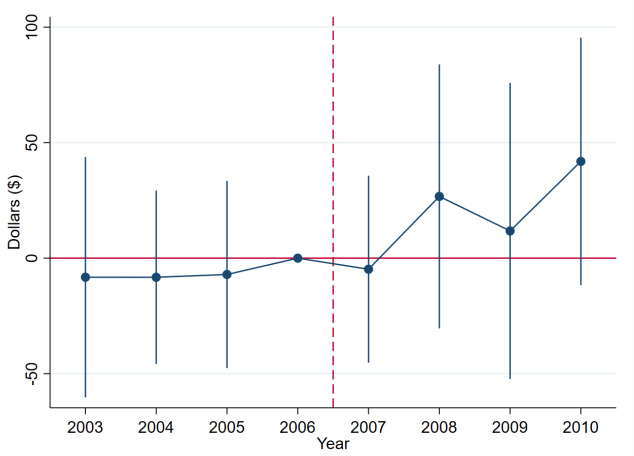
Note: Authors' calculations using Federal Reserve Bank of New York Consumer Credit Panel/Equifax, Census, FHFA, and BLS data. Lines represent 95 percent confidence intervals. Sample includes individuals ages 19-39.

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

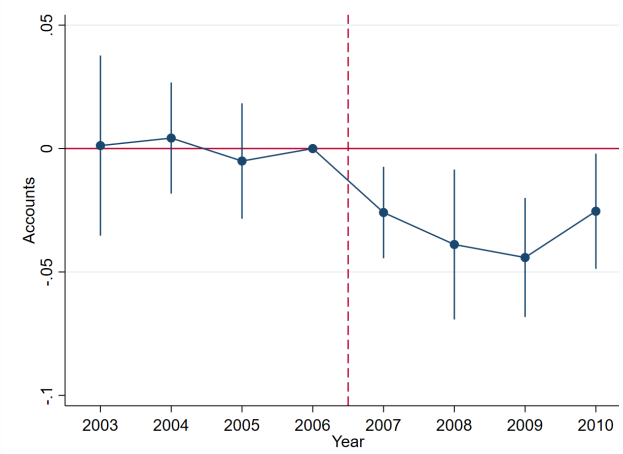
Panel A: Number of medical accounts sent to third-party collections in the past 12 months



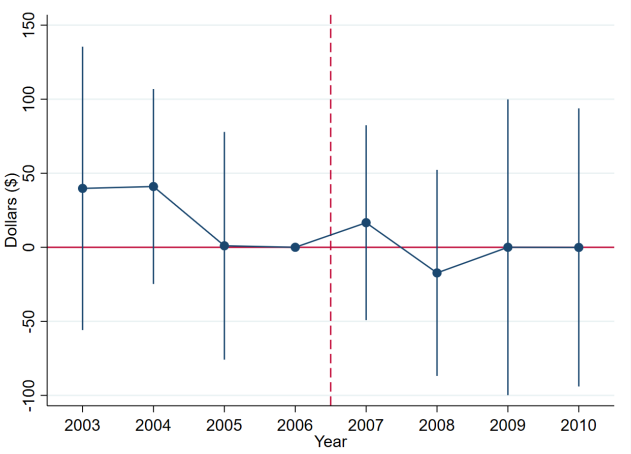
Panel B: Balance of medical accounts sent to third-party collections in the past 12 months



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



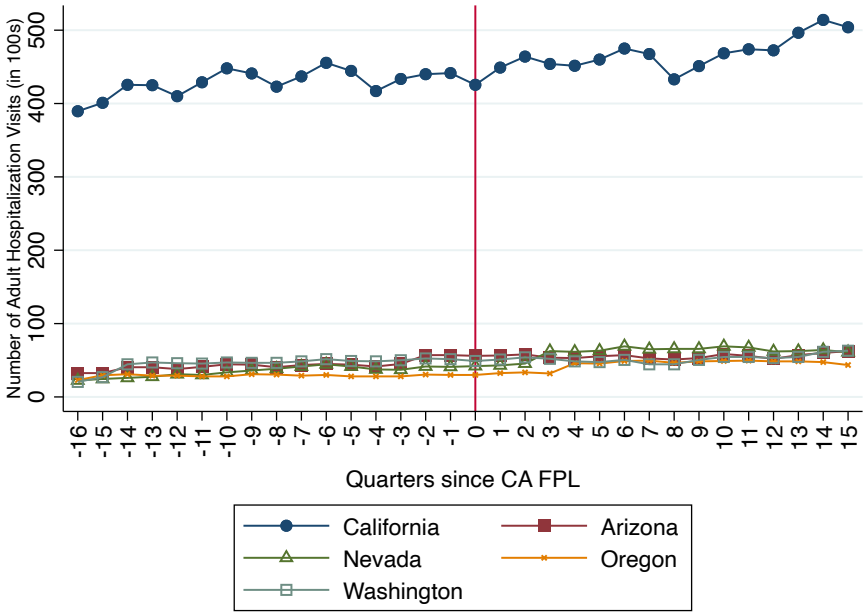
Panel D: Balance of non-medical accounts sent to third-party collections in the past 12 months



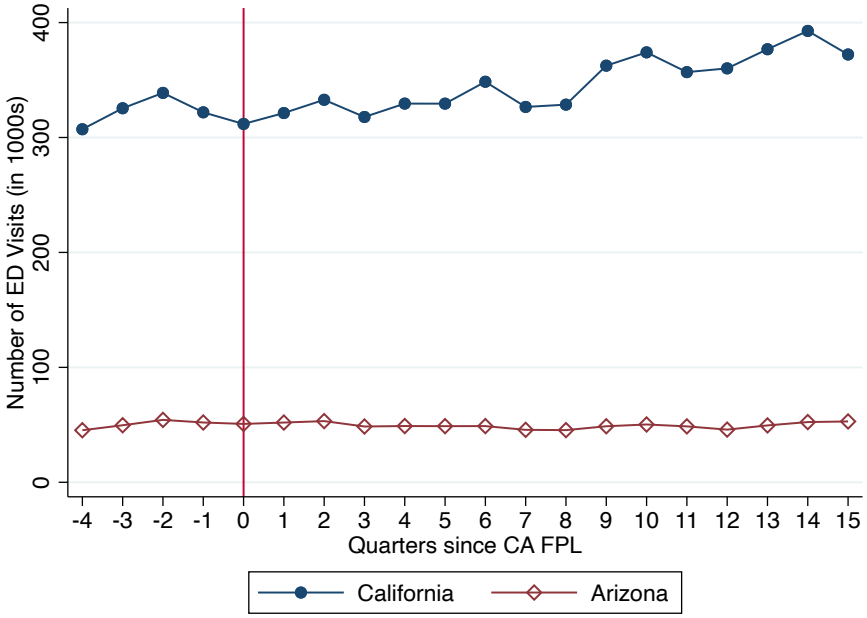
Note: Authors' calculations using Federal Reserve Bank of New York Consumer Credit Panel/Equifax, Census, FHFA, and BLS data. Lines represent 95 percent confidence intervals. Sample includes individuals ages 19-39.

Figure 3: **Trends in Utilization**

(a) Trends in Hospital Utilization



(b) Quarterly Emergency Department (ED) Visits for Self-Pay/No Charge Patients



Note: Data come from the Healthcare Cost and Utilization Project (HCUP) Fast Stats State Trends in Inpatient Stays by Payer and State Trends in Emergency Department Visits by Payer files (Healthcare Cost and Utilization Project (HCUP) Fast Stats (2020)).

Table 1: **CCP Summary Statistics**

	Non-CA States				California			
	Pre		Post		Pre		Post	
	Lower Exp	Higher Exp	Lower Exp	Higher Exp	Lower Exp	Higher Exp	Lower Exp	Higher Exp
Share(Medical Collections)	0.08	0.06	0.11	0.09	0.06	0.03	0.08	0.05
	(0.267)	(0.239)	(0.313)	(0.292)	(0.244)	(0.178)	(0.276)	(0.208)
Share(Non-medical Collections)	0.15	0.12	0.15	0.13	0.14	0.1	0.16	0.11
	(0.356)	(0.328)	(0.355)	(0.341)	(0.348)	(0.300)	(0.364)	(0.312)
Share($0 Non-medical Collection Balance)	0.65	0.68	0.66	0.67	0.67	0.9	0.65	0.67
	(0.477)	(0.465)	(0.472)	(0.471)	(0.469)	(0.465)	(0.476)	(0.470)
Share($0 Medical Collection Balance)	0.92	0.94	0.89	0.91	0.94	0.97	0.92	0.95
	(0.268)	(0.241)	(0.312)	(0.292)	(0.246)	(0.179)	(0.275)	(0.208)
Total Collection Balance	481.74	454.57	688.78	681.79	429.72	430.91	568.24	587.02
	(1827.8)	(2495.8)	(2670.8)	(3239.7)	(2020.1)	(2644.3)	(2819.4)	(3255.2)
Non-medical Balance	297.77	295.84	337.17	369.45	266.73	294.80	341.47	375.26
	(1301.1)	(2048.4)	(1520.1)	(2388.1)	(1164.6)	(1943.1)	(1941.5)	(2578.0)
Medical Balance	183.96	158.73	351.61	312.34	162.98	136.11	226.77	211.77
	(1244.9)	(1357.6)	(2109.6)	(2091.7)	(1637.1)	(1771.1)	(1971.2)	(1934.4)
# of Medical Collections	0.15	0.11	0.23	0.19	0.11	0.05	0.15	0.07
	(0.663)	(0.550)	(0.881)	(0.788)	(0.551)	(0.352)	(0.653)	(0.445)
# of Non-medical Collections	0.24	0.18	0.22	0.20	0.20	0.14	0.22	0.15
	(0.825)	(0.615)	(0.736)	(0.669)	(0.606)	(0.495)	(0.706)	(0.515)

Notes: Authors' calculation using Federal Reserve Bank of New York Consumer Credit Panel/Equifax and Census data. Sample includes individuals ages 19 – 39 from 2003 to 2010, which yields approximately 2.2 million observations and approximately 401,000 individuals.

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

	Medical Debt in Collections		Non-medical Debt in Collections	
	Any	$0 Balance	Any	$0 Balance
Panel A				
Treat × Exposure × Post	-0.0108*	0.0097	-0.0145**	0.015**
	(0.006)	(0.006)	(0.006)	(0.007)
Treatment Mean	0.033	0.967	0.1	0.9
R^2	0.362	0.358	0.390	0.393
N	2,206,325	2,258,480	2,206,325	2,203,340

	Medical Debt in Collections		Non-medical Debt in Collections	
	Number	Balance	Number	Balance
Panel B				
Treat × Exposure × Post	-0.020	6.89	-0.042***	-25.40
	(0.016)	(20.405)	(0.012)	(22.663)
Treatment Mean	0.05	136.11	0.136	294.80
R^2	0.391	0.249	0.367	0.279
N	2,206,398	2,303,415	2,206,398	778,484

Notes: Authors' calculation using Federal Reserve Bank of New York Consumer Credit Panel/Equifax, Census, FHFA, and BLS data. 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 19 – 39. Standard errors clustered at the county-level. Treatment mean is the pre-policy average for individuals living in high-exposure CA counties. *** p<0.01, **p < 0.05, *p < 0.1.

Table 3: **Effect of FPL on Medical and Non-medical Debt Balance Distributions**

	Medical Debt Balance			
	\$0 Balance	Between \$1 and \$1,000	\$1,001 to \$2,000	\$2,001+
Panel A				
Treat × Exposure × Post	0.0097	-0.0101**	-0.0003	0.0007
	(0.006)	(0.004)	(0.001)	(0.002)
Treatment Mean	0.967	0.025	0.004	0.004
R^2	0.358	0.290	0.210	0.256
N	2,258,480	2,258,480	2,258,480	2,258,480

	Non-medical Debt Balance			
	\$0 Balance	Between \$1 and \$1,000	\$1,001 to \$2,000	\$2,001+
Panel B				
Treat × Exposure × Post	0.015**	-0.013***	-0.001	-0.001
	(0.007)	(0.005)	(0.002)	(0.002)
Treatment Mean	0.9	0.079	0.012	0.009
R^2	0.393	0.341	0.231	0.218
N	2,203,340	2,203,340	2,203,340	2,203,340

Notes: Authors' calculation using Federal Reserve Bank of New York Consumer Credit Panel/Equifax, Census, FHFA, and BLS data. 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 19 – 39. Standard errors clustered at the county-level. Treatment mean is the pre-policy average for individuals living in high-exposure CA counties.*** p<0.01, **p < 0.05, *p < 0.1.

Table 4: **Effect of the FPL on Financial Distress for Individuals Older Than 65**

	Medical Debt in Collections		Non-medical Debt in Collections	
	Any	$0 Balance	Any	$0 Balance
Panel A				
Treat × Exposure × Post	0.0001	0.0003	0.0012	-0.0012
	(0.002)	(0.002)	(0.001)	(0.001)
Treatment Mean	0.01	0.99	0.021	0.979
R^2	0.331	0.312	0.342	0.346
N	907,932	1,004,523	907,932	906,286

	Medical Debt in Collections		Non-medical Debt in Collections	
	Number	Balance	Number	Balance
Panel B				
Treat × Exposure × Post	0.003	-10.49**	0.0004	-20.37
	(0.004)	(5.026)	(0.002)	(68.987)
Treatment Mean	0.015	112.63	0.027	250.15
R^2	0.363	0.160	0.350	0.362
N	907,976	906,327	907,976	92,007

Notes: Authors' calculation using Federal Reserve Bank of New York Consumer Credit Panel/Equifax, Census, FHFA, and BLS data. 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 county-level median poverty rate, and $Post = 1$ for the years 2007 to 2010. Sample includes individuals born in 1942 or earlier who are older than age 65. Standard errors clustered at the county-level. Treatment mean is the pre-policy average for individuals living in high-exposure CA counties. *** p<0.01, **p < 0.05, *p < 0.1.

Table 5: **Effect of the FPL on Medical and Non-medical Debt Balance Distributions for Individuals Older Than 65**

	Medical Debt Balance			
	\$0 Balance	Between \$1 and \$1,000	\$1,001 to \$2,000	\$2,001+
Panel A				
Treat × Exposure × Post	0.0003	0.0004	-0.0006*	-0.00004
	(0.002)	(0.001)	(0.0004)	(0.0004)
Treatment Mean	0.99	0.009	0.0008	0.0006
R^2	0.312	0.278	0.198	0.225
N	1,004,523	1,004,523	1,004,523	1,004,523

	Non-medical Debt Balance			
	\$0 Balance	Between \$1 and \$1,000	\$1,001 to \$2,000	\$2,001+
Panel B				
Treat × Exposure × Post	-0.001	0.000005	0.0006	0.0005
	(0.001)	(0.001)	(0.0004)	(0.0004)
Treatment Mean	0.979	0.016	0.002	0.003
R^2	0.346	0.317	0.223	0.207
N	906,286	906,286	906,286	906,286

Notes: Authors' calculation using Federal Reserve Bank of New York Consumer Credit Panel/Equifax, Census, FHFA, and BLS data. 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 county-level median poverty rate, and $Post = 1$ for the years 2007 to 2010. Sample includes individuals born in 1942 or earlier who are older than age 65. Standard errors clustered at the county-level. Treatment mean is the pre-policy average for individuals living in high-exposure CA counties. *** p<0.01, **p < 0.05, *p < 0.1.

Table 6: **Effect of FPL on Financial Distress: Continuous Exposure Measure**

	Medical Debt in Collections		Non-medical Debt in Collections	
	Any	$0 Balance	Any	$0 Balance
Panel A				
Treat × Exposure × Post	-0.034	0.031	-0.067***	0.064***
	(0.021)	(0.021)	(0.024)	(0.023)
Treatment Mean	0.033	0.967	0.1	0.9
R^2	0.362	0.358	0.390	0.393
N	2,206,325	2,258,480	2,206,325	2,203,340

	Medical Debt in Collections		Non-medical Debt in Collections	
	Number	Balance	Number	Balance
Panel B				
Treat × Exposure × Post	-0.051	43.77	-0.162***	-75.4
	(0.065)	(91.76)	(0.047)	(111.44)
Treatment Mean	0.051	136.11	0.137	294.80
R^2	0.391	0.249	0.367	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. 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 19 – 39. Standard errors clustered at the county-level. Treatment mean is the pre-policy average for individuals living in high-exposure CA counties.*** p<0.01, **p < 0.05, *p < 0.1.

Table 7: **Effect of FPL on Medical and Non-medical Debt Balance Distribution: Continuous Exposure**

	Medical Debt Balance			
	\$0 Balance	Between \$1 and \$1,000	\$1,001 to \$2,000	\$2,001+
Panel A				
Treat × Exposure × Post	0.031	-0.035**	-0.006	0.010
	(0.021)	(0.015)	(0.005)	(0.009)
Treatment Mean	0.967	0.025	0.004	0.004
R^2	0.358	0.290	0.210	0.256
N	2,258,480	2,258,480	2,258,480	2,258,480

	Non-medical Debt Balance			
	\$0 Balance	Between \$1 and \$1,000	\$1,001 to \$2,000	\$2,001+
Panel B				
Treat × Exposure × Post	0.067***	-0.052***	-0.010	-0.006
	(0.024)	(0.017)	(0.007)	(0.006)
Treatment Mean	0.9	0.079	0.012	0.009
R^2	0.393	0.341	0.231	0.218
N	2,203,340	2,203,340	2,203,340	2,203,340

Notes: Authors' calculation using Federal Reserve Bank of New York Consumer Credit Panel/Equifax, Census, FHFA, and BLS data. 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* 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 19 – 39. Standard errors clustered at the county-level. Treatment mean is the pre-policy average for individuals living in high-exposure CA counties.*** p<0.01, **p < 0.05, *p < 0.1.

Table 8: **Financial Distress Robustness Check: Exclude Oregon**

	Medical Debt in Collections		Non-medical Debt in Collections	
	Any	$0 Balance	Any	$0 Balance
Panel A				
Treat × Exposure × Post	-0.011	0.010	-0.016**	0.0175**
	(0.007)	(0.007)	(0.007)	(0.007)
Treatment Mean	0.033	0.967	0.1	0.9
R^2	0.361	0.357	0.391	0.394
N	2,055,570	2,103,946	2,055,570	2,052,934

	Medical Debt in Collections		Non-medical Debt in Collections	
	Number	Balance	Number	Balance
Panel B				
Treat × Exposure × Post	-0.025	-0.309	-0.047***	-29.59
	(0.019)	(23.464)	(0.014)	(25.12)
Treatment Mean	0.051	136.11	0.137	294.80
R^2	0.393	0.248	0.371	0.280
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. 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 19 – 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 high-exposure CA counties. *** p<0.01, **p < 0.05, *p < 0.1.

Table 9: **Medical and Non-medical Balance Distributions Robustness Check: Exclude Oregon**

	Medical Debt Balance			
	\$0 Balance	Between \$1 and \$1,000	\$1,001 to \$2,000	\$2,001+
Panel A				
Treat × Exposure × Post	0.0096	-0.0087	-0.0003	0.0007
	(0.007)	(0.005)	(0.001)	(0.002)
Treatment Mean	0.967	0.025	0.004	0.004
R^2	0.357	0.290	0.210	0.256
N	2,103,946	2,103,946	2,103,946	2,103,946

	Non-medical Debt Balance			
	\$0 Balance	Between \$1 and \$1,000	\$1,001 to \$2,000	\$2,001+
Panel B				
Treat × Exposure × Post	0.0175*	-0.015**	-0.001	-0.001
	(0.007)	(0.005)	(0.002)	(0.002)
Treatment Mean	0.9	0.079	0.012	0.009
R^2	0.394	0.342	0.232	0.219
N	2,052,934	2,052,934	2,052,934	2,052,934

Notes: Authors' calculation using Federal Reserve Bank of New York Consumer Credit Panel/Equifax, Census, FHFA, and BLS data. 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 19 – 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 high-exposure CA counties. *** p<0.01, **p < 0.05, *p < 0.1.