Federal Reserve Bank of Philadelphia

Working Papers
Research Department

WP 18-03
January 2018
https://doi.org/10.21799/frbp.wp.2018.03

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ISSN: 1962-5361

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Abstract

We analyze whether the passage of the Affordable Care Act's dependent coverage mandate in 2010 reduced financial distress for young adults. Using nationally representative, anonymized consumer credit report information, we find that young adults covered by the mandate lowered their past due debt, had fewer delinquencies, and had a reduced probability of filing for bankruptcy. These effects are stronger in geographic areas that experienced higher uninsured rates for young adults prior to the mandate's implementation. Our estimates also show that some improvements are transitory because they diminish after an individual ages out of the mandate at age 26.

Keywords: Affordable Care Act, health insurance, consumer credit, financial distress
JEL Codes: D14, I13, I18

*email: nathan.blascak@phil.frb.org. The authors would like to thank Robert M. Hunt, Julia Cheney, Andrew Kish, Daniel Grodzicki, Marieke Bos, Catherine Maclean, and Doug Webber for their helpful comments and Anna Tranfaglia for her valuable research assistance. We also thank seminar participants at the Federal Reserve Bank of Philadelphia, Temple University, the 2017 APPAM Student Conference, the 2017 Boulder Summer Conference on Consumer Financial Decision Making, the 2017 Western Economic Association Conference, the 2017 New Perspectives on Consumer Behavior in Credit and Payments Markets Conference, and the 2017 APPAM Annual Fall Research Conference for their helpful comments.

1 Introduction

Prior to the passage of the Affordable Care Act (ACA), young adults were surprisingly exposed to high levels of medical expenditure risk due to low health-insurance coverage and high levels of indebtedness. From 2006 to 2009, data from the U.S. Census Bureau show that the uninsured rate of adults ages 19–25 was over 35 percent, approximately 75 percent higher than the rate of middle-age adults.[1] Over this same period, young adults carried significant amounts of debt and may have had difficulties accessing additional sources of credit. Data from the Federal Reserve Board indicate that approximately 80 percent of young individuals ages 19–31 had some type of debt balance, and more than 40 percent reported being credit constrained (Dettling and Hsu, 2014).[2] This lack of insurance coverage, combined with the limited ability to acquire additional credit, implies that even small medical shocks could have serious negative financial consequences for these young individuals during this period. However, relatively little research has been done to assess if policies extending health-insurance coverage to young adults have mitigated this risk and reduced financial distress.

While young adults faced this high level of medical expenditure risk across the latter half of the 2000s, policies aimed at improving health insurance for young adults prior to the ACA were implemented only at the state level. The scope of these laws varied greatly, with each mandate covering different ages and targeting specific groups of young adults, such as students. Implemented in September 2010, the dependent coverage mandate of the ACA streamlined these laws, allowing young adults to remain on their parents' health insurance plans until the age of 26. While existing research has shown that the mandate led to decreases in annual health-care expenditures and overall out-of-pocket (OOP) expenditures,[3] we use this expansion of health-insurance coverage for young adults to assess how the mandate more broadly reduced financial distress for adult dependents covered by the law.

To estimate the effect of the mandate on measures of financial distress, we use a standard difference-in-differences (DID) framework and compare young adults born in either 1985 or 1986

[1] The uninsured rate for adults ages 36–45 was 20.8 percent from 2006 to 2009. Both rates are based on the authors' calculations from the Current Population Survey (CPS).

[2] The average balance of this debt ranged from \$13,000 to \$20,000, depending on what kind of debt is measured (Dettling and Hsu, 2014; Brown, Grigsby, van der Klaauw, Wen, and Zafar, 2016).

[3] Chua and Sommers (2014) and Chen, Vargas-Bustamante, and Novak (2015) estimate that the dependent coverage mandate led to a 14 percent decrease in annual health-care expenditures and an 18–21 percent decrease in out-of-pocket (OOP) expenditures.

(who were ages 24 or 25 at the passage of the mandate in 2010) with individuals born in 1982 or 1983 (who were 27 or 28 in 2010). Similar to other recent studies on the financial effects of health-insurance policy changes, we use individual-level credit and debt information on a 5 percent sample of U.S. adults with a credit report. These data provide a unique perspective for assessing financial distress; they contain detailed longitudinal records of these individuals' financial information, allowing us to track their performance accurately over time. Also, because of the nature of the mandate, we can measure the effect of losing eligibility for parental health-insurance coverage under the mandate. In particular, since we observe cohorts of individuals who age out of the mandate at 26, we can estimate the long-run or age-out effect of the mandate.

Our results indicate that the mandate had an immediate effect of reducing the financial distress of young individuals. In particular, it reduced the probability of new 120 days past due occurrences on all accounts and the number of major derogatory events on revolving accounts. We also find that it lowered the probability that a young adult would file for personal bankruptcy during the pre-implementation period from March 2010 to September 2010. During this period, the probability that a young adult would file for bankruptcy fell by 21 percent, or 0.03 percentage point. A rough back-of-the-envelope calculation translates this drop to a decline of 1.2 bankruptcies per 1,000 young adults. We obtain similar estimates for the short-run effect of the mandate after its implementation. During the two years immediately following the passage of the mandate, from the fourth quarter of 2010 to the fourth quarter of 2012, the probability of having an account 120 days past due declined by 0.4 percentage point or 16.7 percent.

We also find that declines in debt past due and debt in third-party collections are primarily concentrated in the far-right tails of their respective distributions. Results from quantile regressions indicate that declines in either kind of debt occur at the 99th percentile and are economically significant. Our estimates indicate that the 99th percentile of debt in third-party collections declined by $3,500, and the 99th percentile of total debt past due declined by $6,000 by the beginning of 2013. Placebo tests on cohorts of older individuals do not show evidence that our results are driven by a secular trend in a reduction of financial distress for young adults. Our long-run estimates indicate that some improvements observed in the two years immediately after the mandate was implemented disappear after an individual ages out of the mandate's coverage at age 26. This result is consistent with Dahlen (2015), who found a 15.4 percent increase in the share of young adults with worse health-insurance coverage after they aged out of the mandate.

As prior research has noted, there is likely significant heterogeneity in our results. To examine if there are differential effects of the law due to geography, we test if individuals living in areas most likely to be affected by the mandate experienced greater improvements than those living in areas that were less likely to be affected. To do this, we follow Mazumder and Miller (2016) and estimate a triple-difference model by combining data on the uninsured rate and the unemployment rate for young adults and form an ex-ante "exposure" indicator. Point estimates from this triple-difference specification indicate that young adults living in high exposure areas experienced greater improvements across a broad set of measures of financial distress compared with those living in low exposure areas. We also examine if individuals with subprime risk scores prior to the mandate's implementation, those most likely to be considered credit constrained and financially vulnerable, experienced relatively larger decreases in distress than those with prime risk scores.[4] We find that young adults who were subprime prior to the passage of the mandate experienced the largest declines in financial distress.

This paper contributes to an emerging literature that has used large, nationally representative data sets of credit bureau information to examine the effect of other health-insurance policies on financial well-being of various groups (Argys, Friedson, Pitts, and Tello-Trillo, 2017; Brevoort, Grodzicki, and Hackmann, 2017; Hu, Kaestner, Mazumder, Miller, and Wong, 2016; Mazumder and Miller, 2016).[5] We add to this literature by examining how the expansion in coverage due to the dependent coverage mandate affected financial outcomes of young adults.

In addition to examining the effects of providing health insurance to young individuals, this paper makes three contributions to the existing literature. First, we add to a growing body of literature that has analyzed the effects of the ACA's dependent coverage mandate on a number of different margins, including employment (Bailey and Chorniy, 2016; Heim, Lurie, and Simon, 2017), self-employment (Bailey, 2017), and health and health-care utilization outcomes (Akosa Antwi, Moriya, and Simon, 2015; Barbaresco, Courtemanche, and Qi, 2015). Second, unlike previous studies that have used credit report information, we analyze the effect of a government-mandated

[4] We define subprime as having a risk score below 620 in the first quarter of 2008. We use the Equifax Risk Score, which is similar to other risk or credit scores used in the industry.

[5] Other studies focusing on the effect of health insurance on financial outcomes include Barcellos and Jacobson (2015) and Gross and Notowidigdo (2011).

expansion in private insurance coverage on financial outcomes of intended beneficiaries. Unlike the policy changes used in the previous studies, the expansion in coverage because of the dependent coverage mandate relies on an increase in private insurance, not public insurance programs such as Medicaid or subsidies important for the ACA's marketplaces or the Massachusetts health-care reform. Given the policy uncertainty surrounding health care, we believe it is important to assess the effects of various health-insurance policies implemented in recent years on the financial outcomes of their target populations. Third, we estimate not only the effect of enrollment in health-insurance coverage but also the consequences of an automatic disenrollment from insurance coverage after young adults age out of the mandate at age 26.

This paper also adds to the literature on the effect of public policies on the financial outcomes of young adults. While there are recent studies examining the effects of financial education mandates (Brown et al., 2016) and credit card restrictions (Debbaut, Ghent, and Kudlyak, 2016) on financial outcomes of young individuals, the financial response of this population group to health-insurance coverage is not well understood. However, given that young adults in the U.S. still experience high levels of uninsurance and are financially vulnerable because of low income and high unemployment, it is important to understand this response.

Overall, our results indicate that the ACA's dependent coverage mandate improved the financial well-being of young adults. This is consistent with other recent literature that has shown that assessments of welfare effects of health-care policy should account for the effect on individuals' personal finances as well as such factors as labor market outcomes. It is important to note that, because we observe only if young adults are eligible to be covered by the mandate, not if they actually gained health-insurance coverage, our estimates measure the intent-to-treat (ITT) effects of the dependent coverage mandate. This implies that our estimates of the effect of the mandate on the treated individuals are more conservative than the treatment effects for individuals who actually received health insurance through the mandate. This is because we will be averaging effects across eligible individuals who actually received health insurance through their parents' plans and those who did not.

2 Background and Framework

2.1 Young Adult Financial Health

Prior to the implementation of the ACA in 2010, the financial health of young adults could be characterized by three stylized facts. First, young adults were both asset and savings poor. Data from the 2010 Survey of Consumer Finances (SCF) show that the median bank deposits[6] for young adults ages 19–25 was approximately \$1,072, and the average amount of money in savings accounts was \$3,000, with a median of \$0. Median financial assets for this age group were approximately \$1,558, while median total assets, including real estate, were only \$11,163. Second, young adults during this time held fairly high levels of debt. Based on data from the CCP, average debt balances in 2009 for 19- to 25-year-olds were approximately \$13,470, with a median of \$7,430. Individuals who were 25 years of age experienced even higher levels of indebtedness, with an average debt balance of \$18,708. Third, young adults generally had low rates of financial literacy. Surveys conducted by the Jump\$tart Coalition show that high school students consistently score poorly on tests of financial literacy, correctly answering only half of the test questions since 2000.[7] College students who took the test in 2008 fared better than high school students, with an average score of 62.2 (Mandell, 2008). Taking all of these facts into account, young adults in this time period were low on income, assets, and financial literacy, and they had high debt levels.

One important implication of these resource constraints is how they affect an individual's ability to smooth consumption and cover medical expenses. Although young adults are healthier on average than the rest of the population, they spend a relatively high amount of money for health-care out of their own pockets. According to data from the Medical Expenditure Panel Survey, approximately 17.5 percent of all health-care expenditures in 2009 by young adults ages 19–25 were paid out of pocket. Approximately 48.7 percent of these individuals paid more than 50 percent of their total yearly medical expenses out of pocket, and 37.8 percent paid their entire yearly medical expenses out of pocket. Given that young adults have relatively little in savings and assets and hold higher amounts of debt, there is a higher probability that they would face financial hardship in the face

[6]These include checking, savings, and money market mutual fund accounts.

[7]The Jump\$tart Coalition administered biannual surveys from 1998 to 2008 to a nationally representative sample of high school seniors. In 2008, Jump\$tart administered the financial literacy test to college students.

of an expensive medical bill.[8]

2.2 Dependent Coverage Mandates and Health-Insurance Coverage of Young Adults

2.2.1 State-Level Dependent Coverage Mandates

Prior to the ACA's dependent coverage mandate, laws requiring private health-insurance plans to cover young adults were passed at the state level. Starting with Utah in 1995, these laws become more popular over time, with 20 additional states passing some type of dependent coverage mandate by 2008 (Monheit, Cantor, DeLia, and Belloff, 2011). The passage of these laws was in part a response to the fact that, historically, young adults have been the most uninsured age demographic in the United States. While estimates vary based on the definition of health-insurance coverage, there is broad consensus that the rate was in excess of 30 percent, with some estimates as high as 37 percent. However, despite these state-level mandates, young adult uninsurance remained persistently high, raising questions regarding their efficacy (Cantor, Belloff, Monheit, and Koller 2012b; Levine, McKnight, and Heep, 2011; Monheit et al., 2011). Among the many reasons for this is that the state laws were fairly narrow in scope, with eligibility varying by student status, marriage status, and type of insurance plan. Prior research also suggested that availability and eligibility criteria were not always clear or readily available to consumers (Cantor, Belloff, Monheit, DeLia, and Koller, 2012a). Another reason why the uninsured rate remained high is that self-insured plans, which account for more than half of all private health-insurance plans, were typically not covered by these state-level dependent coverage mandates because the Employee Retirement Income and Security Act (ERISA) exempted them from these regulations.

2.2.2 The ACA's Dependent Coverage Mandate

The ACA was enacted by Congress on March 21, 2010, and signed into law by President Barack Obama on March 23, 2010. While a majority of the law's components did not take effect until 2014, the dependent coverage mandate was one of the first provisions to be implemented, taking effect in late September 2010. The mandate standardized dependent coverage across all states, requiring

[8] Statistics are based on the authors' calculations using data from the Medical Expenditure Panel Survey (MEPS). For more information on the MEPS, see www.meps.ahrq.gov.

all family health-insurance plans to offer coverage for dependent children until they reached 26, regardless of student or marital status. Subsequent analysis on the coverage effects of the mandate have shown that the uninsured rate for adult dependents dropped significantly, with 2 million to 3 million young adults receiving dependent coverage through parental insurance by the end of 2011 (Akosa Antwi et al., 2013; Cantor et al., 2012b; Sommers et al., 2013).

However, this increase in young adult insurance coverage via parental employer-sponsored insurance (ESI) was not solely from the previously uninsured population. Akosa Antwi et al. (2013) estimate that there was 1.7 percent decline in own-name ESI for young adults as a result of the mandate. This implies that own-name ESI coverage was crowded out by parental ESI, an important spillover effect from the mandate. Despite this crowd-out problem, the uninsured rate for young adults 19–25 has declined dramatically to 14.5 percent, more than a 50 percent decline from the pre-ACA levels (Collins, Gunja, and Beutel, 2016).

Importantly, there was significant heterogeneity in where these declines in the uninsured rate occurred. To demonstrate this, we show counties with the largest declines in young adult uninsurance in Figure 1.A and 1.B. As shown in Figure 1.A, there was significant variation in the uninsured rate across the U.S. prior to the passage of the dependent coverage mandate in 2009, with many parts of the Southwest and Southeast, as well as some counties in the Northwest, with uninsured rates in excess of 30 percent. These rates declined substantially after the mandate implementation, especially in the Southwest and Western states as shown in Figure 1.B. Similar to Mazumder and Miller (2016), we use this variation in the uninsured rate across U.S. counties in 2009 to examine if individuals in the regions most affected by the policy change experienced more pronounced changes in financial distress.

2.3 Conceptual Framework

The main hypothesis of this paper is that providing health insurance to young adults through their parents' plans should lessen financial distress. The most direct effect is on uninsured individuals who previously were delinquent on or unable to pay their medical bills. Because delinquent and/or unpaid medical bills can lead to substantial financial problems (Brevoort et al., 2017), increasing insurance coverage should directly lead to a lower incidence of these events and, in turn, reduce financial distress. There are also additional mechanisms through which the provision of health insurance can reduce financial distress. For those individuals uninsured before the passage of the mandate, receiving coverage likely results in an "income effect" that improves financial standing. For example, receiving coverage through parental ESI reduces OOP medical expenditures on preventative medical services and bills incurred because of an adverse medical shock, resulting in increased availability of financial resources. Increased financial resources may allow individuals to pay down existing delinquent debt or prevent future delinquencies.

As previously mentioned, a sizable number of young adults who had own-name ESI switched to a parent's plan in response to the mandate. For these individuals, the decision to switch to a parent's plan is likely driven by differences in OOP expenditures and/or plan generosity. If we assume young adults derive utility from insurance coverage and there are nonzero switching costs for shifting to parental ESI from an own-name plan, then individuals must receive higher utility either through increased plan generosity or through lower OOP costs to incentivize a switch to parental ESI. Similar to the uninsured, individuals who switch insurance plans because of lower OOP costs also experience an income effect through this OOP reduction. Overall, we expect the income effect for these individuals who switched from own-name to parental ESI to be less than that of the uninsured group, since the switching group includes individuals that switched because of net utility gains through increased benefits and because they incurred lower OOP costs.

Another mechanism through which the mandate may operate is a risk mechanism in which insurance coverage reduces the risk of incurring large medical expenditures. A reduction in this kind of risk may allow young adults to take on risks in other areas, such as investing (Goldman and Maestas, 2013; Ayyagari and He, 2016), or may reduce the need to hold precautionary savings (Lee, 2016; Blascak, Mikhed, and Bailey, 2017). These kinds of activities can also lead to improvements in financial distress. Newly covered individuals receive the largest reduction in this type of risk, while young adults who switched from own-name to parental ESI likely experience smaller decreases. However, given that younger people are healthier on average, we expect to see relatively smaller effects from this mechanism than previously seen for more general populations.

It is also possible that the mandate indirectly affected young adults' financial well-being through channels in the real economy. For example, if individuals experience "job-lock"[9] due to health

[9] *Job lock* is the reluctance to change jobs due to the fear of losing ESI.

insurance, health insurance through parental ESI may remove the incentive to stay at a job. In the short run, this may result in a temporary increase in job search and subsequent changes in income but may also increase entrepreneurial or college-going opportunities. While our data do not allow for us to test the impact of this real economy channel directly, we acknowledge that this specific channel may play a role in either exacerbating or mitigating financial distress for marginal individuals.

If individuals operate within this basic conceptual framework, we hypothesize that the mandate should reduce financial distress. Given that severe health shocks are less likely for this population, we predict that improvements will come from extreme cases such as bankruptcy, delinquency, or other major derogatory events in which the insurance protection is greatest.

3 Data

3.1 Consumer Credit Data

The consumer credit data used in the analysis come from the Federal Reserve Bank of New York/Equifax Consumer Credit Panel (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. Individuals are followed at a quarterly frequency until they die, change their SSN, or drop off because of an extended period of credit market inactivity. While the CCP contains extensive information regarding credit information, 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]

One concern with our data is that not all individuals, especially young adults, have a credit bureau file. Works by Lee and van der Klaauw (2010) and Brown et al. (2016) compare the CCP with data from the SCF and the American Community Survey (ACS) and they provide strong evidence that the young adult population covered in the CCP is representative of other measures of this age population. Analysis by Brevoort, Grimm, and Kambara (2016) show that, while approximately 62 percent of consumers ages 18–19 do not have a credit bureau file, this number

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

drops to nearly 10 percent for consumers ages 25–29.

3.2 Sample Selection

When constructing our sample, there are two important considerations to make. First, because the mandate's effects are determined by age, we restrict the data to include the credit files of individuals born in the years 1982–1983 and 1985–1986. The individuals born in 1985 and 1986 serve as a treatment group since they would have been 24 and 25, respectively, when the mandate took effect in 2010. Individuals born in 1982 and 1983 are never treated by the mandate since they would have been 27 or 28 when it was implemented and therefore serve as the control group.[11] Limiting the analysis to four birth-year cohorts also helps increase comparability within the groups because both levels and trends in credit data exhibit substantial time-varying cohort effects (Debbaut et al., 2016; Fulford and Schuh, 2016).[12] Including more birth years in the treatment and control groups would likely cause the two groups to trend differently and cast doubt on the identifying assumption of the DID framework. Therefore, by limiting the analysis to four birth years, we increase the likelihood that the control group properly accounts for other unobserved factors that affect the financial outcomes of young adults.

Additionally, the Credit Card Accountability and Disclosure (CARD) Act was passed by Congress in 2009. While the majority of the CARD Act was aimed at regulating different aspects of the credit card industry, one of the provisions of the law restricted lenders' offerings of credit cards to young adults under the age of 21.[13] The timing and demographic target of this portion of the CARD Act provides additional motivation to restrict the sample to older young adults.

We take a 50 percent subsample from Q1:2008 to Q4:2013 of all consumers from the four birth-year cohorts and drop any consumers who have fewer than four total observations across the

[11] Because the CCP only contains information on birth year, the possible age range of individuals in the treated group could be from 23–25. We exclude individuals born in 1984 because it is possible they would have turned 26 prior to the passage of the mandate.

[12] Our approach also addresses the issue raised by Slusky (2017) regarding previous research that used difference-in-differences strategies to study the dependent coverage mandate. He found that previous studies often failed placebo tests because the age bandwidth for treatment groups was too wide and argued that future studies should reduce the age bandwidth to control for the age-structure of health insurance markets.

[13] In particular, credit card companies were not allowed to solicit students within 1,000 feet of college campuses or events, were barred from sending preapproved card offers to individuals under 21, and were not allowed to issue a credit card for individuals under 21 unless they provided written proof of ability to repay their debt. For more details, see Debbaut et al. (2016).

sample period. We do not observe all individuals in each time period because many young adults typically have "thin" credit files[14] and therefore do not have continuously present credit bureau files.[15] Because of this, we do not restrict the sample to consist of balanced panels because this would introduce a sampling bias into our analysis by arbitrarily omitting individuals with either fewer observations over time or without a continuous credit report over the sample period. Instead, we allow for unbalanced panels to avoid this bias. Our final sample includes approximately 440,000 individuals and 8.6 million observations.

To analyze financial distress, we use a number of different measures as outcome variables. To capture different levels of financial distress, we use the number of 90 days past due (DPD) occurrences on revolving accounts,[16] a binary variable indicating the presence of a new 120 DPD occurrence, the number of revolving accounts with a major derogatory event, a binary indicator variable for a new declaration of bankruptcy, and a binary indicator for the presence of a new account in third-party collections.[17] We also include measures of the total amount of debt past due and the amount of debt in third-party collections. These variables measure different levels and kinds of financial distress, with bankruptcy and major derogatory events indicating the most extreme type of financial distress. Table 1 presents summary statistics for these variables in the treatment and control groups.

3.3 Control Data

Although we cannot control for individual- or household-level insurance status, we account for county-level differences in insurance status prior to the passage of the mandate using the Small Area Health Insurance Estimates (SAHIE) data from the U.S. Census Bureau. The SAHIE data are produced by a hierarchal Bayesian model that estimates health-insurance coverage for every county in the United States. This model combines data from multiple sources, including the ACS,

[14] Credit bureau records with only one or two trades, or accounts, are considered "thin."

[15] Young people have thin credit files typically because they have little need or opportunity for credit activity (Lee and van der Klaauw, 2010).

[16] Revolving debt includes debt from credit cards, charge cards, department store cards, and home equity lines of credit.

[17] An account is sent to third-party collections when the party that the debt is initially owed to is unable to collect it from the debtor and contracts an outside party to collect the debt.

the CPS, and data from the Medicaid and SNAP programs.[18] Following Mazumder and Miller (2016), we include uninsured rates for two age groups, 18- to 39-year-olds and 18- to 64-year-olds.

To control for local labor market conditions, we use yearly data from the ACS. We use state-level unemployment data for three different age groups: 20- to 24-year-olds, 25- to 34-year-olds, and the total unemployment rate for the state. We also use county-level poverty data from the Small Area Income and Poverty Estimates (SAIPE). Similar to SAHIE, the SAIPE model uses a large number of data sources to make regression-based predictions on the number of people living in poverty at the state, county, and school district level.

4 Methodology

4.1 Parallel Trends Tests

To identify the effect of the mandate on financial distress, we use a standard difference-in-differences (DID) framework to separate the effects of the policy change from other contemporaneous changes in the financial distress of young adults. The DID framework relies on the assumption that the trends in the financial variables would be the same for the treatment and control groups in the absence of the mandate. While we cannot test if the treatment and control groups would have trended similarly in the post-mandate period in the absence of the mandate, we can evaluate if the two groups had similar trends in the pre-mandate period. To assess if the trends in our financial variables are comparable across the treated and control groups, we plot mean values of these variables over time in Figures 2 and 3. The four panels of Figure 2 show average values of the number of 90 days past due occurrences on revolving accounts, the probability of new 120 days past due occurrences on all accounts, the number of revolving accounts with major derogatory events, and the probability of a new bankruptcy. The four panels of Figure 3 show average values for amount of debt in third-party collections, the probability of having a new account in third-party collections, amount of past due debt on all accounts, and total balance on bankcards. For all these variables, the trends in the treatment and control groups appear parallel prior to the law's enactment in the second quarter of 2010.

The graphs in Figures 2 and 3 suggest that the number of major derogatory events and the

[18] For more information on the SAHIE data, see www.census.gov/did/www/sahie/index.html.

amount in third-party collections started to decrease in the treatment group compared with the control group after the law was enacted and implemented. Similarly, the number of revolving accounts with 90 days past due occurrences seems to trend down in the treatment group compared with the control group. These preliminary results are suggestive of a potential reduction in some measures of financial distress as a result of the dependent coverage mandate.

As a more formal test of the equality of trends in the treatment and control groups in the pretreatment period, we estimate the following model on data from the first quarter of 2008 to the first quarter of 2010:

$$y_{it} = \delta_0 + \delta_1 Treated_i \times Time_t + \delta_2 Treated_i + \delta_3 Time_t + \mathbf{X_{it}}\mathcal{B} + \nu_{it} \tag{1}$$

where $Treated_i$ is a dummy variable equal to one if an individual was born in 1985 or 1986, and X_{it} is a vector of control variables that includes state fixed effects, state linear time trends, county-level uninsured and poverty rates, state-level unemployment rates for the 19- to 25-year-old age group and the 26- to 34-year-old age group, state-level unemployment rate interacted with the treatment dummy variable, age, and age interacted with birth-year cohort fixed effects. We follow Bertrand, Duflo, and Mullainathan (2003) and bootstrap the standard errors.[19] The variable of interest in Equation (1) is the interaction between the treatment dummy variable $Treated_i$ and the linear time trend $Time_t$. If there is a difference in trends in the pre-mandate period for the treatment and control groups, we would expect δ_1 to be statistically significant and nonzero. A coefficient of zero indicates that there is no difference in trends between the two groups. Results for the credit variables of interest are presented in Table 2. Based on the results, we conclude that there are no significant differences in trends for the variables of interest prior to the passage of the mandate at the 5 percent level of significance.

[19] Bootstrapping standard errors is preferred over clustering at either the individual or state level as it is unlikely that the assumptions of independence across clusters but correlation within clusters would be satisfied.

4.2 Main Difference-in-Differences Model

To estimate the causal impact of the passage of the mandate on financial distress for young adults, in principle, we would estimate the following DID model:

$$y_{it} = \alpha_0 + \alpha_1 Treated_i \times Post_t + \alpha_2 Treated_i + \alpha_3 Post_t + \mathbf{X_{it}}\mathcal{B} + \mu_i + T_t + \epsilon_{it} \tag{2}$$

where $Treated_i$, X_{it}, and T_t are as defined above in Equation (1). μ_i is an individual fixed effect and $Post_t$ is a dummy variable equal to one for observations starting in the fourth quarter of 2010, the first quarter that the mandate was officially in place, to the end of the sample period in 2013. However, several health insurers announced their intention to implement the mandate prior to the required implementation date in September 2010 (Akosa Antwi et al., 2013; Federal Register, 2010).

To address the staggered nature of the implementation of the mandate, we follow the approach widely used in the previous literature and create a number of time-period dummy variables to allow for differential effects of the mandate across different points in the timeline of the implementation. $Enact_t$ is a dummy variable equal to one for observations that span the enactment period of the mandate, from the second to third quarters of 2010 (March 2010–September 2010). To analyze the effects of being covered by the mandate and aging out of the coverage on financial distress, we divide the post-implementation period into two separate intervals. In particular, we define the implementation or covered stage to span the fourth quarter of 2010 to the fourth quarter of 2012 and specify the aging-out period to run from the first quarter of 2013 to the fourth quarter of 2013. Thus, the model to be estimated is now:

$$\begin{aligned} y_{it} = \alpha_0 &+ \alpha_1 Treated_i \times Enact_t + \alpha_2 Treated_i \times Implement_t + \\ &\alpha_3 Treated_i \times AgeOut_t + \alpha_4 Treated_i + \alpha_5 Enact_t + \alpha_6 Implement_t + \\ &\alpha_7 AgeOut_t + \mathbf{X_{it}}\mathcal{B} + \mu_i + T_t + \epsilon_{it} \end{aligned} \tag{3}$$

The coefficients of interest are α_1, α_2, and α_3, which capture the immediate, short-run, and long-run effects of the mandate on financial distress for treated individuals. If access to health insurance only improves financial outcomes while young adults are covered, then we would expect the α_2 coefficient to be statistically significant. If enough health-insurance companies began implementation of the mandate before it went into effect, α_1 may also be negative and significant. The sign and significance of α_3 is ex-ante ambiguous because there are many potential mechanisms that could drive certain effects after individuals have aged out of the mandate. For example, if young adults age out of the mandate and do not regain health-insurance coverage, we may expect financial distress to increase again, or any improvements made while being insured may be reversed. Although we include $Treated_i$ in Equations (2) and (3), its coefficient is not estimated because this variable is collinear with the individual fixed effects μ_i.

We also considered two alternative methodologies but ultimately decided they were inappropriate for this analysis. First, it is possible to form treatment and control groups by age instead of by birth year. This strategy would potentially result in a number of problems. Because we observe only the year of birth and not the exact birth date, an individual's age would automatically change in the first quarter of a given year within our sample, inducing attenuation bias due to the measurement error in age. Also, use of age in the definition of the treatment and control groups introduced "migration" problems because of the length of the panel. Specifically, by using age instead of birth-year cohorts, individuals could either enter or exit the treatment and control group each year after the implementation date, resulting in treated individuals having varying number of years of coverage under the mandate.[20] The combined problems of treatment/control group "migration" and the potential attenuation bias due to all individuals changing age in the first quarter of each year imply that the use of age to define treatment status would introduce a number of analytical problems. Therefore, we decided that using age would be inappropriate, and we instead used birth-year cohort to define treatment status. Second, we tried applying a regression discontinuity (RD) approach at age 26. However, because we do not observe the precise birth date of an individual, only birth year, everyone in a cohort "turns" the same age on January 1 of each year. Thus the usefulness of an RD design is limited.

[20]For example, a 24-year-old entering the sample in 2012 would already have two years of eligibility under the mandate, with an additional two more years of coverage, while a 24-year-old in 2010 would have two years of coverage and then would have aged out of the mandate.

5 Results

5.1 Main Results

The main DID results from Equation (3) are presented in Tables 3 and 4. Each coefficient represents the average effect of the mandate on the measures of financial distress in each period. The first row of Table 3 provides evidence that the mandate had an immediate effect on financial distress during the enactment period. The number of 90 days past due occurrences and the number of major derogatory events on revolving accounts (which include charge-offs and first-party collections) both declined by 0.007 occurrences, a 2 percent reduction from their pre-mandate means. The probability of a new bankruptcy declaration also declined, by a statistically significant 0.03 percentage point, which roughly translates to an annual reduction of 1.2 bankruptcies per 1,000 individuals. Observing these improvements in financial distress during the enactment period is not unexpected, given that there was a 10 percent increase in dependent health-insurance coverage under parental ESI during this time (Akosa Antwi et al., 2013).

The effects of the mandate for the two years after its implementation are presented in the second row of Table 3. The estimates indicate that the mandate reduced the number of 90 days past due incidents on revolving accounts by 0.01 occurrences, or 4 percent, and the probability of a new 120 days past due occurrence on all debt by 0.4 percentage point or 16.7 percent. We also find that the number of major derogatory events on revolving accounts declined by 0.011 occurrences, or 5 percent. Similar to the enactment period, we observe a decrease of 0.03 percentage point in the probability of declaring bankruptcy, but this decrease is not statistically significant. Because this time period coincides with the years that the treatment group is covered by the dependent coverage mandate, we would expect to find the strongest effects during this time period.

The third row of Table 3 shows the long-run effects of the mandate for the treated group, after they have aged out of the mandate. With the exception of the probability of a new 120 days past due occurrence, all coefficients are not statistically different from zero. These estimates imply that positive effects of the mandate may disappear after an individual ages out of the mandate. This result is consistent with the findings of Dahlen (2015), who finds that young adults who age out of the mandate at age 26 report a 15.4 percent increased probability of having worse insurance coverage from the previous year (when they were covered by the mandate). Importantly, this decrease in quality occurred even though uninsured rates did not increase at age 26, with both the offer of ESI coverage (7.9 percent) and the purchase of non-ESI coverage (5.1 percent) increasing after an individual's 26th birthday. Since non-ESI coverage is typically more expensive and provides less generous benefits than ESI coverage, it is unsurprising that our estimated improvements in financial distress diminish but do not revert to pre-mandate levels, as young adults age out of the mandate.

As shown in Table 4, across all three time periods, we observe no statistically significant changes in the new incidence of third-party collections, the amount of debt in third-party collections, or the amount of debt past due on all accounts. This result is not entirely unexpected, as we may expect to see declines in indebtedness for only those individuals with debt totals in the far right tail of the distribution. The last column of Table 4 indicates that total balances on bank card accounts actually increased during and after the mandate's implementation. This result may show that covered individuals used their credit cards more because of the reduction in risk of unexpected health expenditures. These effects are explored in detail in Blascak, Mikhed, and Bailey (2017).

5.2 Triple-Difference Model Results

Although the average effects estimated previously strongly suggest that the dependent coverage mandate led to improvements in financial distress, because our estimates are ITT, they likely understate the actual effects on individuals that received health insurance. To more directly test how the dependent coverage mandate affected eligible individuals, we exploit the geographic variation in the uninsured rate for young adults and follow Mazumder and Miller (2016) and Brevoort et al. (2017) by implementing a triple-difference empirical framework. We do this by combining county- and state-level information on uninsured and unemployment rates for young adults during the pre-mandate period to create a measure of ex-ante exposure to the law. Using data from the ACS and SAHIE, we create an indicator variable equal to one if an individual was living in a county that was at or above the 75th percentile of the uninsured rate and a state that was at or above the 75th percentile of the unemployment rate for young adults in 2009.[21] We then interact this dummy

[21] According to the SAHIE data, the weighted county-level young adult uninsured rate at the 75th percentile from 2008–2009 was 30.5 percent. The 75th percentile of the young adult unemployment rate in 2009 was 16 percent.

variable, labeled $Exposure_c$, with the DID specification in Equation (3):

$$
\begin{aligned}
y_{itc} = \lambda_0 &+ (Exposure_c \times Treated_i \times (Enact_t + Implement_t + AgeOut_t))\mathbf{\Phi} + \\
&+ Exposure_c \times (Enact_t + Implement_t + AgeOut_t)\mathbf{\Psi} \\
&+ Treated_i \times (Enact_t + Implement_t + AgeOut_t)\mathbf{\Omega} \\
&+ \lambda_1 Exposure_c \times Treated_i + \lambda_2 Exposure_c + \lambda_3 Treated_i + \lambda_4 Enact_t \\
&+ \lambda_5 Implement_t + \lambda_6 AgeOut_t + \mathbf{X_{it}}\mathcal{B} + \mu_i + T_t + \epsilon_{it} \qquad (4)
\end{aligned}
$$

where all control variables are as defined previously. The interaction terms of $Exposure_c \times (Enact_t + Implement_t + AgeOut_t)$ control for any trends in high uninsured areas that are common between the treatment and control groups after the mandate was passed. The terms in $Treated_i \times (Enact_t + Implement_t + AgeOut_t)$ control for any trends in the treatment group that are common across all geographic areas in the post-mandate periods. The coefficients of interest are in the vector $\mathbf{\Phi}$, which are the triple interactions of $Exposure_c$, $Treated_i$, and each of the time periods analyzed previously. We can interpret these coefficients as the effect of the ACA's dependent coverage mandate on financial distress for young adults within geographic areas that experienced high levels of uninsurance for each time period. If the passage of the mandate reduces financial distress for young adults living in geographic areas more exposed to the mandate, we would then expect the coefficients in $\mathbf{\Phi}$ to be negative.

Results from this triple-difference specification are presented in Tables 5 and 6. The estimates in Table 5 show that each of our measures of financial distress declined as a result of the dependent coverage mandate. Young adults living in more exposed areas experienced a 14 percent decline in the number of 90 days past due occurrences, a 4 percent decline in the probability of a new 120 days past due occurrence, and a 17.6 percent decline in the number of major derogatory events. The probability of a new bankruptcy declaration, the most severe type of delinquency, also decreased after implementation of the mandate by 25 percent.

The results in Table 6 also show that individuals living in areas most exposed to the effect of the mandate experienced statistically significant decreases in the probability of a new account in third-party collections and the amount of debt in third-party collections and the total amount of debt past due on all accounts. On average, the amount in third-party collections declined by $48 or 6.5 percent (relative to the mean of $740 in the treatment group), and the amount past due on all accounts decreased by $199 or 24 percent (relative to the mean of $825). These estimates indicate that young adults living in areas with greater "exposure" to the mandate experienced statistically significant declines in both the incidence of financial distress and amounts of debt past due or in collections.

To test whether our measure of exposure to the mandate is sensitive to the definition of the exposure variable, we also tried alternative definitions of this variable. In particular, we define our new exposure variable to be equal to the actual young adult uninsured rate in 2009. This variable provides a direct measure of the level of heterogeneity across counties. The rest of the model is the same as in Equation (4). The results of this analysis are presented in Tables 7 and 8.

While some coefficients in these tables lose statistical significance, overall, the effects of the mandate on financial distress are very similar to the results in Tables 5 and 6. It is worth noting that the interpretation of these coefficients is different than that for our earlier results. While the results in Tables 5 and 6 show the change in financial distress for treated individuals in high young adult uninsurance and high unemployment areas, the results in Tables 7 and 8 should be interpreted as changes in financial distress from 1 percent of additional uninsurance among young adults in 2009.

In Table 7, we see that a 1 percent increase in the uninsured rate prior to the mandate is associated with 0.0026 fewer 90 days past due occurrences and 0.002 fewer accounts with a majority derogatory event. In Table 8, our estimates show that a 1 percent increase in ex-ante exposure leads to $2.75 fewer dollars in third-party collections and $6.55 fewer dollars of total debt past due. These results imply that a treated adult dependent living in a county at the 90th percentile of the young adult uninsured rate prior to the mandate, when compared with a treated individual living in a county at the 10th percentile of uninsured rate, would have experienced an additional decrease of $131 in past due debt and a decrease of $55 in debt in third-party collections.[22]

[22] The 90th percentile of the young adult uninsured rate was approximately 39 percent in 2009, while the 10th percentile was 19 percent.

5.3 Heterogeneity Analysis

In addition to the geographic variation explored previously, there are likely other dimensions along which we would expect to see heterogeneous effects. In Table 9 and Appendix Table A1, we report results for two subsamples divided based on the average uninsured rate in the individual's county of residence from 2008 to 2009. Specifically, we separate the sample based on whether a county was in the 75th percentile of the young adult uninsured rate in 2008–2009,[23] the period before the mandate was enacted, and then estimate Equation (3) on each group separately. The top panel of Table 9 presents the results for counties that were in the 75th percentile or higher for the young adult uninsured rate, and the lower panel presents results for counties below the 75th percentile. The probability of a new bankruptcy filing declined by 0.05 percentage point for individuals living in high uninsured counties, a decrease that is approximately five times larger than for those living in low uninsured counties. We also observe much larger magnitudes of declines in the number of 90 days past due occurrences, major derogatory event occurrences, and in the probability of having a new 120 days past due event among individuals in high uninsured counties. As can be seen in Table A1, amounts past due and in collections change little after the mandate's implementation in high and low uninsured counties.

Another dimension on which we may expect to see significant differences in the effects of the mandate is an individual's risk score status prior to the mandate's implementation. It is likely that individuals with a subprime risk score are credit constrained and cannot borrow as much as they need to cover their unexpected expenses. Thus, they may benefit from health insurance more than individuals with prime risk scores. To test this hypothesis, we employ the same method used for the uninsured rate and divide the sample based on the individual's subprime status in the first quarter of 2008, using a risk score of 620 as the cutoff.[24] Results for the regressions on the subprime and prime samples are presented in Table 10 and Appendix Table A2, with subprime individuals in the top panel, and prime individuals in the bottom panel.

The results in Table 10 show that the mandate had the largest effects among subprime individuals, who are most likely to be credit constrained. In particular, this population experienced

[23] Estimates for the young adult uninsured rate are generated from the SAHIE data.

[24] The risk score used is the Equifax Risk Score.

reductions in the number of accounts 90 days past due and major derogatory events on revolving accounts as well as declines in the probability of a new 120 days past due occurrence. These improvements in various measures of delinquency are present in the enactment, implementation, and post-implementation periods. We also observe a statistically significant decline in the probability of a new bankruptcy filing for subprime young adults eligible for health-insurance coverage under the mandate. The results for prime individuals in Table 10 indicate that the mandate did not affect individuals who were less likely to be credit constrained. Although there are large differences in the effects of the law on the incidence and the number of accounts delinquent among subprime and prime consumers, Table A2 shows that the mandate had little differential effects on the amounts past due and in third-party collections for subprime and prime borrowers.

In our main results in Table 4, we do not find any statistically significant decreases in total debt in third-party collections and total debt past due. These results are not entirely unexpected; there may be heterogeneous effects in different parts of the distribution for these variables. In particular, given that the individuals most likely to be affected by the mandate are those who actually experience a health shock and incur a large amount of debt, we hypothesize that reductions will come from those individuals in the far right tail of the distributions of these variables. To examine this potential source of heterogeneity, we estimate an event study model using a conditional quantile regression specification similar to Dobkin, Finkelstein, Kluender, and Notowidigdo (2018):

$$Y_{it} = \gamma_0 + \sum_{e=Q1:2009}^{Q4:2013} \gamma_{e\rho} T_e \times Treated_i + \sum_{e=Q1:2009}^{Q4:2013} \theta_e T_e + \gamma_1 Treated_i + \mathbf{X_{it}} \mathcal{B} + \epsilon_{it} \qquad (5)$$

where T_e is a vector of dummy variables for each calendar quarter from Q1:2009 to Q4:2013 and the control variables in X_{it} are the same as the previous equations.[25] The coefficients of interest in Equation (5) are the $\gamma_{e\rho}$'s on the interaction of the calendar time dummy variables and the treatment dummy variable. We estimate $\gamma_{e\rho}$ for three different quantiles ρ in the far right tail of the distribution: the 90th, 95th, and 99th quantiles. Because we are estimating conditional quantile regression models, each $\gamma_{e\rho}$ will be interpreted as the change in debt balances at quantile ρ in time T_e relative to balances in 2008. Results from the regressions are presented graphically in Figure 4, with statistically significant time period estimates represented as circles.

[25] We exclude all four quarters of 2008 and use them as the omitted time period.

As can be seen in Panel A of Figure 4, changes in debt in third-party collections are not statistically significant in the post period except at the 99th percentile. We estimate that debt at the 99th percentile for third-party collections was reduced by approximately \$3,500 one year after the mandate, and this decline persisted for the remainder of the sample period. These results imply that consumers in the far right tail of the distribution of debt in third-party collections experience deep declines in the amount in third-party collections.

Panel B of Figure 5 shows that total debt past due at the 90th, 95th, and 99th percentiles all experienced statistically significant declines after the implementation of the mandate. We see that levels of debt at the 90th percentile were persistently lower throughout the post-mandate period, with average declines between \$300 and \$600. The 95th percentile of total debt past due declined even more dramatically between \$2,000 and \$3,000, relative to the omitted period of 2008. Declines in total debt past due at the 99th percentile, while only statistically significant at certain points in the post-mandate period, represent significant declines relative to 2008 levels, with point estimates showing reductions as deep as \$6,300. Combined, these results show large declines in the far-right tail of the distribution of the amount of debt past due. This finding is consistent with health-insurance coverage protecting consumers from extreme forms of medical shocks and accompanying financial debt and distress.

5.4 Placebo Tests

One concern we may have is that the improvements we observe in the previous sections are the result of a secular change affecting the financial variables of all young adults, instead of the improvements being the results of the dependent coverage mandate. To test if our results can be attributed to changes in the economic environment for young people unrelated to the passage of the mandate, we conduct a series of placebo tests by estimating Equation (3) for individuals who should not have been affected by the mandate. Specifically, we compare individuals born in 1980–1981 (placebo treatment) with those born in 1978–1979 (placebo control). If our main analysis is capturing the effect of the mandate on financial distress, and not simply a secular trend in improvement in financial distress for young adults, then estimates from Equation (3) using older cohorts should produce statistically insignificant results.

Results from the placebo regressions are reported in Appendix Tables A3 and A4. All these placebo results are statistically insignificant. For placebo tests for older young adults (birth years 1980–1981 and 1978–1979), we find no statistically significant differences in the probability of new bankruptcy filings, incidence and number of delinquent accounts, or major derogatory events as well as amounts past due or in third-party collections.

6 Discussion and Conclusion

The results from this analysis contribute to the growing body of studies that leverage large consumer credit data sets to analyze the effects of health-insurance policy on financial distress. Using anonymized credit report data for young adults affected by the ACA's dependent coverage mandate (born in 1985–1986) and not affected by the law (born in 1982–1983), we find that the increased access to insurance reduced financial distress for adult dependents. In particular, we observe reductions in financial distress immediately after the law was enacted in the second quarter of 2010 and during the two years after its implementation, from the fourth quarter of 2010 to the fourth quarter of 2012. Although results for some measures of financial distress do not appear to be significantly affected by the mandate, we find that the number of delinquent accounts, the incidence of major derogatory events, and the probability of a new bankruptcy filing all fell after the mandate's enactment and implementation. Regression results also suggest that once individuals aged out of the mandate after age 26, some improvements disappeared, indicating that young adults did not receive the same amount of financial protection when transitioning to individual health insurance plans or losing their health insurance.

Examining the heterogeneity in the effects of the mandate, we find that declines in the amount of debt past due and the amount of debt in third-party collections are concentrated in the far right tails of their distributions. This result is consistent with the idea that health-insurance coverage protects individuals from extraordinary medical debt. We also find that individuals living in counties with high rates of young adult uninsurance and those with subprime risk scores prior to the enactment of the mandate experienced greater improvements in financial distress than persons living in counties with relatively better local economic conditions or those who had prime risk scores. These results suggest that the mandate was effective in the geographical areas and among populations that had the highest likelihood of being affected. Most importantly, the results as a whole strongly indicate that providing health insurance to the young can reduce their financial distress.

Our estimates can provide context when evaluating the welfare aspects of the ACA's dependent coverage mandate. Specifically, we are able to evaluate the effect of a *private* health insurance expansion on financial distress, which represents an important departure from the existing literature, as a majority of other studies that have examined the effect of health-insurance policy on financial distress have focused on *public* health insurance expansions. This distinction has direct implications on the welfare effects of this law, as the efficiency of the mandate, as opposed to public finance considerations, will dictate the incidence of its cost. Depew and Bailey (2015) show that while family plan premiums increased by 2.5 percent to 2.7 percent after the mandate was implemented, employee contributions did not experience a statistically significant increase. This implies that while employers saw increased costs, they did not pass the cost of the coverage on to workers. Since employee contributions did not change, it is possible then that employers passed on the costs through wage reductions instead of increased insurance contributions.

While young adults may not bear the entire cost of the mandate, the benefits from reducing their financial distress can be significant. Brevoort et al. (2017) provide a theoretical framework that shows how reductions in delinquent medical debt can improve consumer welfare. Reductions in financial distress for young adults may also reduce the financial burden of parents who provide financial support to their children. Because these individuals are at the beginning stages of the life cycle, reducing financial burdens or decreasing the probability of incurring large amounts of medical debt may have significant long-run implications, especially if the shocks persist over time. Our results provide an important first step in understanding these dynamics by empirically identifying these effects.

The results of the analysis have important policy implications. We contribute to the growing body of evidence that the provision of health insurance may generate important, welfare-enhancing benefits beyond providing access to health care or reducing out-of-pocket costs. If policymakers are to properly assess the expansion or contraction of health insurance, they need to consider the effect of providing or removing health insurance on the financial distress of individuals, not just measures of physical health and access to health care.

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Figure 1.A: 2009 U.S. Uninsurance Rates for Young Adults, by County

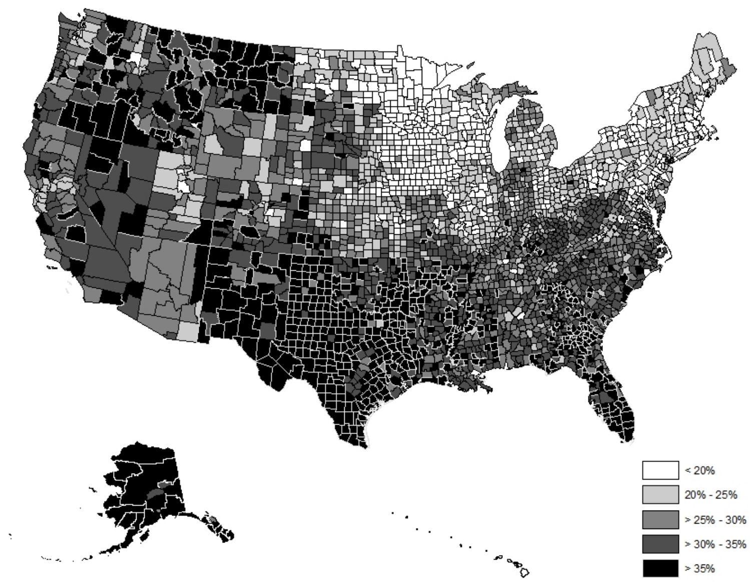
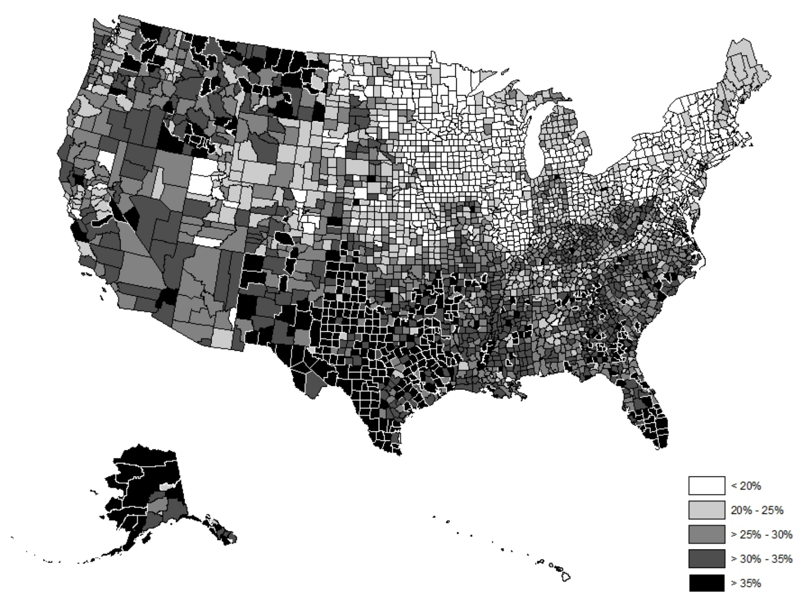


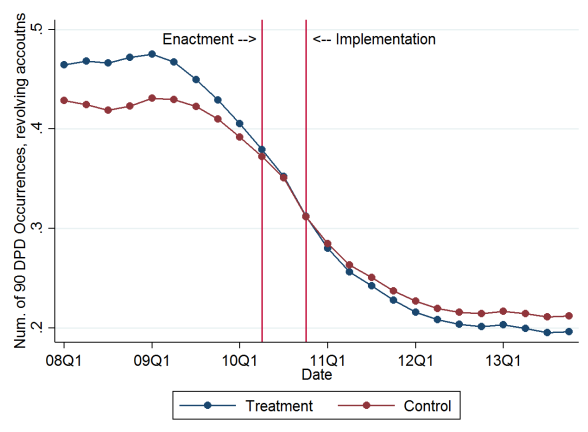
Figure 1.B: 2013 U.S. Uninsurance Rates for Young Adults, by County



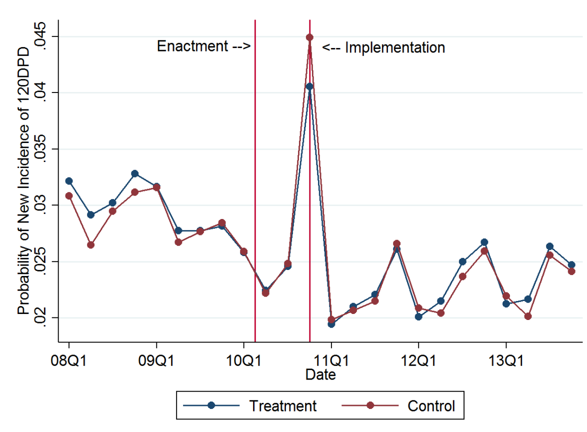
Note: Authors' calculations using data from the U.S. Census Bureau's Small Area Health Insurance Estimates Program.

Figure 2: Differences in Trends in the Treatment and Control Groups

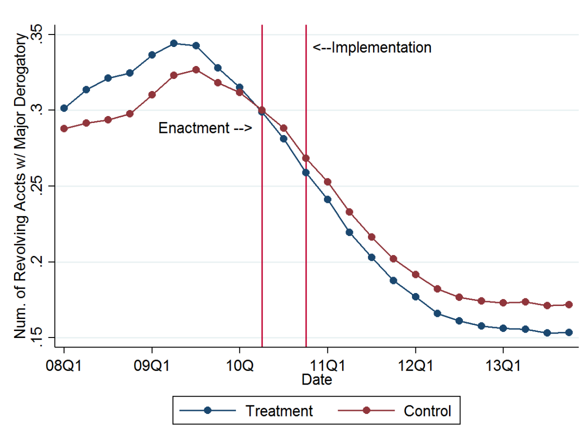
Panel A: Number of 90 days past due occurrences on revolving accounts



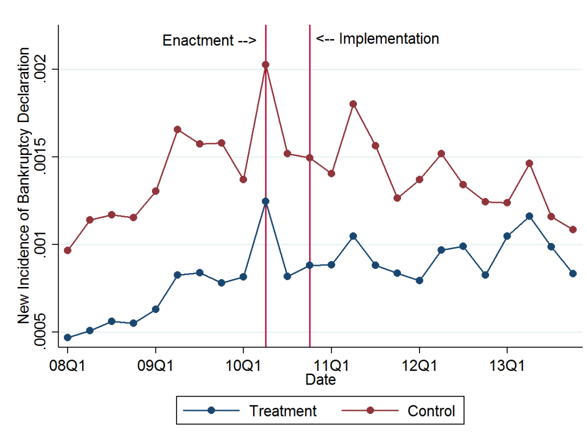
Panel B: Probability of new 120 days past due occurrence on all accounts



Panel C: Number of revolving accounts with major derogatory event



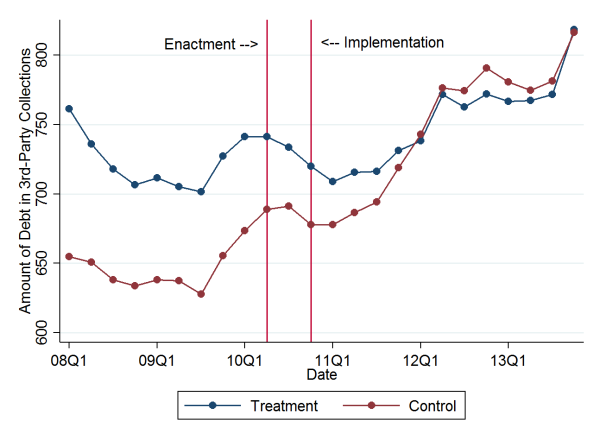
Panel D: Probability of new bankruptcy declaration



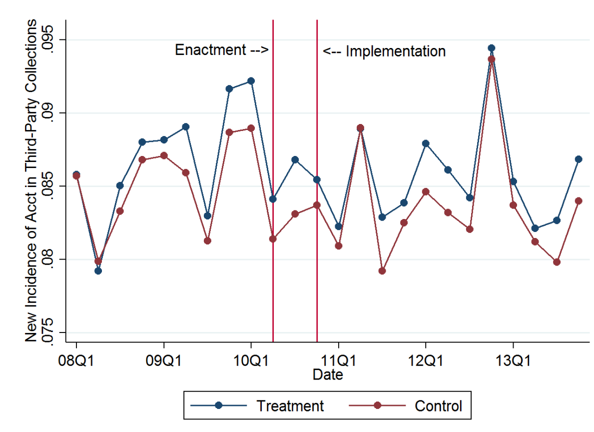
Note: Authors' calculations using data from Federal Reserve Bank of New York/Equifax Consumer Credit Panel.

Figure 3: Differences in Trends in the Treatment and Control Groups

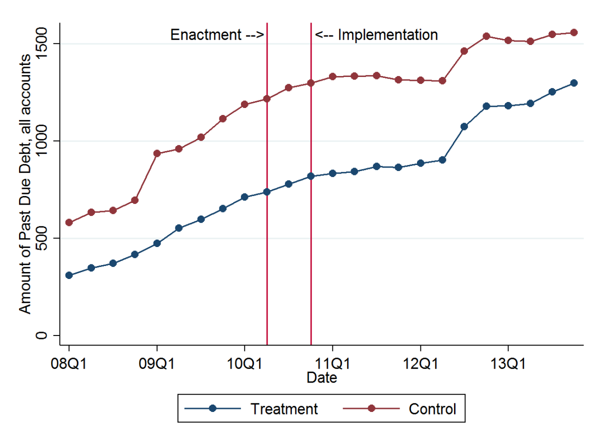
Panel A: Amount of debt in third-party collections ($)



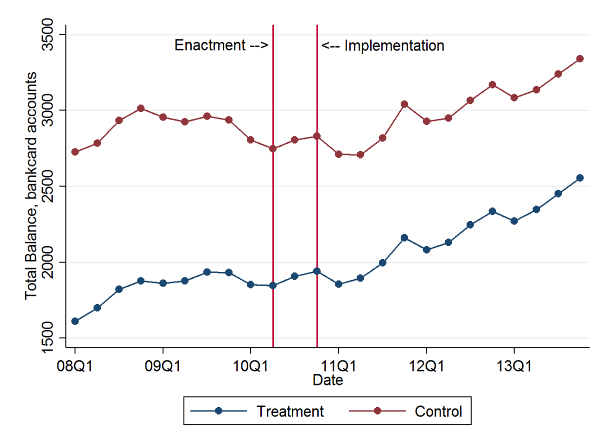
Panel B: Probability of new account sent to third-party collections



Panel C: Amount of past due debt on all accounts ($)



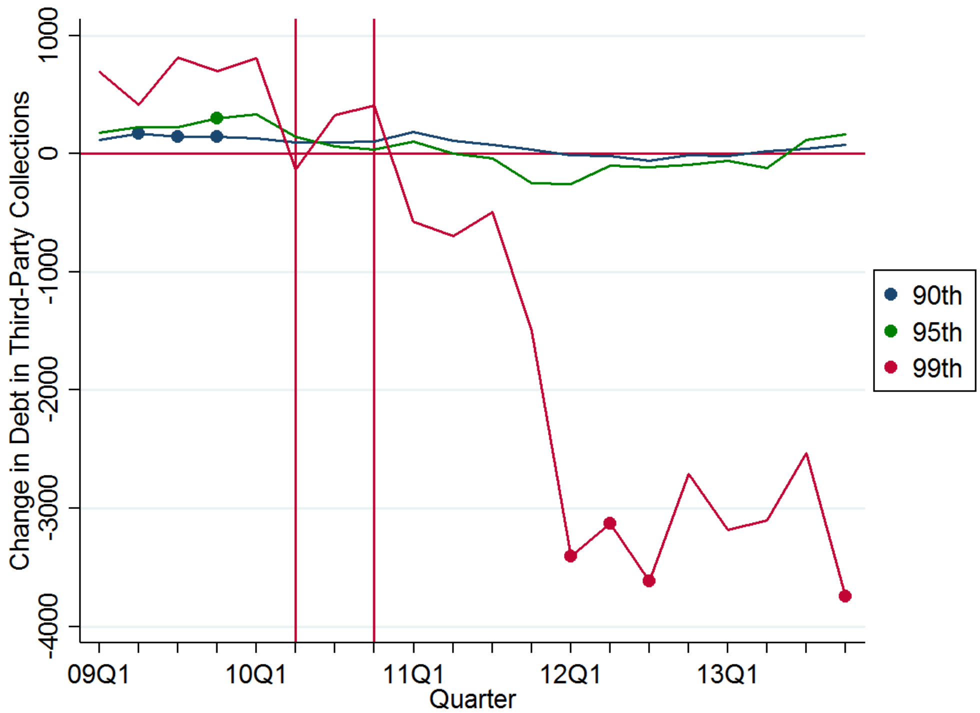
Panel D: Total balances on bankcards ($)



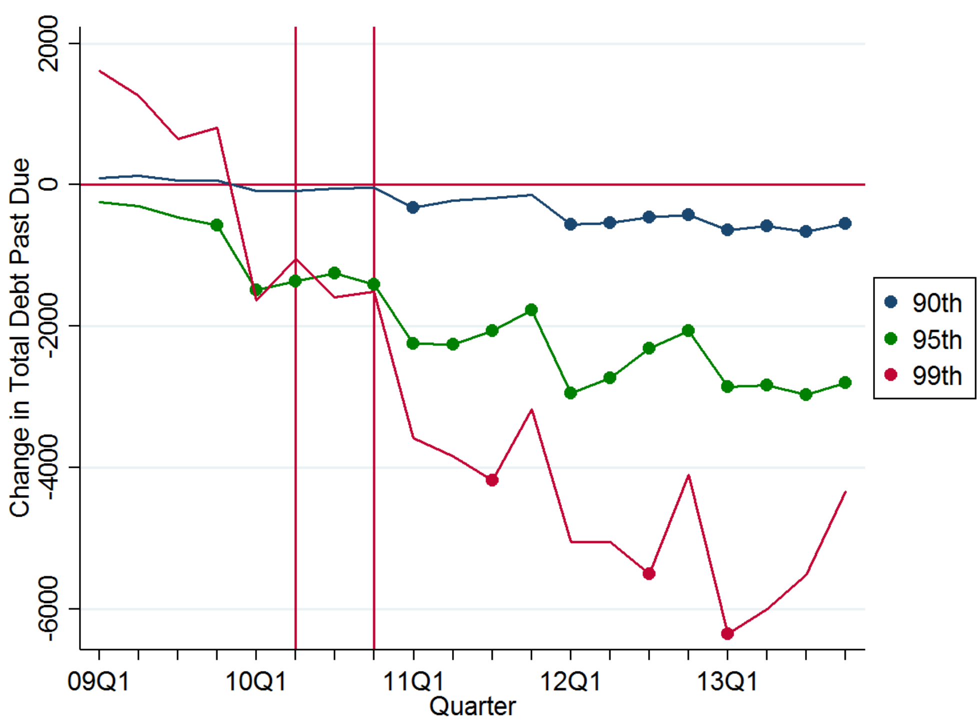
Note: Authors' calculations using data from Federal Reserve Bank of New York/Equifax Consumer Credit Panel.

Figure 4: Quantile Regression Event Study

Panel A: Debt in third-party collections



Panel B: Debt past due, all accounts



Note: Statistically significant coefficients are marked with circles. Authors' calculations using data from Federal Reserve Bank of New York/Equifax Consumer Credit Panel.

Table 1: Financial Distress Summary Statistics

	Treatment Group			Control Group		
Variable	**Mean**	**Std. Dev.**	**Obs.**	**Mean**	**Std. Dev.**	**Obs.**
Number of 90 DPD occurrences, revolving accounts	0.321	1.068	3,608,550	0.316	1.129	3,800,182
Probability of new 120 DPD occurrence	0.026	0.160	4,235,221	0.026	0.159	4,400,462
Number of accounts with major derogatory event, revolving accounts	0.245	0.809	3,609,612	0.247	0.865	3,802,262
Probability of having new account in third-party collections	0.086	0.281	4,374,994	0.084	0.278	4,540,177
Probability of new bankruptcy filing	0.0008	0.029	4,219,725	0.0014	0.037	4,472,387
Amount in third-party collections (in $)	739.95	2,574.78	1,989,740	701.43	2,644.65	2,162,367
Total amount of debt past due, all accounts (in $)	825.48	4,152.193	3,902,530	1,212.31	14,995.93	4,035,477
Total balance, bankcard accounts (in $)	2,044.46	3,186.79	2,184,590	2,957.21	4,584.495	2,373,940
Number of individuals	440,533					
Number of new bankruptcy filings	9,789					

Notes: Authors' calculations using data from the Federal Reserve Bank of New York/Equifax Consumer Credit Panel. Data are for full sample period, Q1:2008–Q4:2013.

Table 2: Difference in Linear Trends Between Treatment and Control Groups in Prepolicy Period (Q1:2008–Q1:2010)

Variable	**Coefficient**	**Obs.**
Number of 90 DPD occurrences, revolving accounts	-0.0027*	2,706,656
	(0.001)	
Probability of new 120 DPD occurrence, all accounts	-0.0002	3,195,608
	(0.000)	
Number of accounts with major derogatory event, revolving accounts	-0.0009	2,707,738
	(0.001)	
Probability of new account sent to third-party collections	0.0005*	3,289,756
	(0.000)	
Probability of new bankruptcy declaration	0.00003	3,221,050
	(0.000)	
Amount of debt past due, all accounts	8.784	2,290,579
	(14.724)	
Amount of debt in third-party collections	-4.5248	1,499,225
	(3.896)	
Total balance, bankcard accounts	4.7588	1,674,565
	(7.467)	

Notes: Standard errors bootstrapped. ***, **, * - denote significance at the 1%, 5%, and 10% level, respectively. Authors' calculations using data from the Federal Reserve Bank of New York/Equifax Consumer Credit Panel.

Table 3: The Effect of the Dependent Coverage Mandate on Financial Distress: DID Results (Q1:2008–Q4:2013)

Coefficient	**Number of 90 DPD Occurrences on Revolving Accounts**	**Probability of New 120 DPD Occurrence**	**Number of Revolving Accounts with Major Derogatory**	**Probability of New Bankruptcy Filing**
Enactment	-0.007**	0.0001	-0.007**	-0.0003**
(Q2:2010–Q3:2010)	(0.003)	(0.001)	(0.003)	(0.000)
Implementation	-0.010***	-0.004***	-0.011***	-0.0003
(Q4:2010–Q4:2012)	(0.004)	(0.001)	(0.003)	(0.000)
Age-Out	-0.006	-0.005***	-0.004	0.00001
(Q1:2013–Q4:2013)	(0.006)	(0.001)	(0.004)	(0.000)
Average in Enactment	0.365	0.024	0.293	0.0014
Average in Treatment	0.244	0.025	0.205	0.0012
Average in Age-Out	0.207	0.023	0.164	0.0011
R^2	0.366	0.089	0.360	0.136
Number of Observations	7,396,593	8,622,075	7,399,732	8,678,030

Notes: Standard errors bootstrapped. All regressions include individual, time, and state fixed effects. ***, **, * - denote significance at the 1%, 5%, and 10% level, respectively. Authors' calculations using data from the Federal Reserve Bank of New York/Equifax Consumer Credit Panel.

Table 4: The Effect of the Dependent Coverage Mandate on Financial Distress: DID Results (Q1:2008–Q4:2013)

Coefficient	**Probability of New Account in Third-Party Collections**	**Amount of Debt in Third-Party Collections**	**Total Amount of Debt PD, All Accounts**	**Total Balance, Bankcard Accounts**
Enactment	0.0001	0.792	-17.305	55.645***
(Q2:2010–Q3:2010)	(0.001)	(15.370)	(25.056)	(10.036)
Implementation	-0.0012	4.982	4.445	65.298***
(Q4:2010–Q4:2012)	(0.001)	(14.453)	(39.027)	(12.444)
Age-Out	-0.0010	38.780*	63.436	116.10***
(Q1:2013–Q4:2013)	(0.002)	(20.829)	(57.930)	(21.046)
Average in Enactment	0.084	709.61	1003.21	2360.95
Average in Treatment	0.085	731.90	1150.24	2525.44
Average in Age-Out	0.083	780.95	1398.59	2824.47
R^2	0.161	0.278	0.360	0.650
Number of Observations	8,901,138	4,147,303	7,399,732	4,548,988

Notes: Standard errors bootstrapped. All regressions include individual, time, and state fixed effects. ***, **, * - denote significance at the 1%, 5%, and 10% level, respectively. Authors' calculations using data from the Federal Reserve Bank of New York/Equifax Consumer Credit Panel.

Table 5: The Effect of the Dependent Coverage Mandate on Financial Distress: Triple-Difference Specification (Q1:2008–Q4:2013)

Coefficient	**Number of 90 DPD Occurrences on Revolving Accounts**	**Probability of New 120 DPD Occurrence**	**Number of Revolving Accounts with Major Derogatory**	**Probability of New Bankruptcy Filing**
Treated × Exposure × Enactment (Q2:2010–Q3:2010)	-0.031*** (0.005)	-0.0002 (0.001)	-0.030*** (0.005)	-0.0004* (0.000)
Treated × Exposure × Implementation (Q4:2010–Q4:2012)	-0.036*** (0.004)	-0.0010** (0.001)	-0.036*** (0.003)	-0.0003*** (0.000)
Treated × Exposure × Age-Out (Q1:2013–Q4:2013)	-0.040*** (0.004)	-0.0013 (0.001)	-0.031*** (0.004)	-0.0003* (0.000)
R^2	0.367	0.089	0.360	0.136
Number of Observations	7,396,593	8,622,075	7,399,732	8,678,030

Notes: Standard errors bootstrapped. All regressions include individual, time, and state fixed effects. ***, **, * - denote significance at the 1%, 5%, and 10% level, respectively. Authors' calculations using data from the Federal Reserve Bank of New York/Equifax Consumer Credit Panel.

Table 6: The Effect of the Dependent Coverage Mandate on Financial Distress: Triple-Difference Specification (Q1:2008–Q4:2013)

Coefficient	**Probability of New Account in Third-Party Collections**	**Amount of Debt in Third-Party Collections**	**Total Amount of Debt PD, All Accounts**	**Total Balance, Bankcard Accounts**
Treated × Exposure × Enactment (Q2:2010–Q3:2010)	-0.003* (0.002)	-22.506 (19.148)	-187.25** (91.051)	47.723** (20.672)
Treated × Exposure × Implementation (Q4:2010–Q4:2012)	-0.002** (0.001)	-47.975*** (14.492)	-198.80*** (63.196)	97.970*** (14.686)
Treated × Exposure × Age-Out (Q1:2013–Q4:2013)	0.001 (0.002)	-14.487 (16.687)	-62.829 (76.358)	125.27*** (24.836)
R^2	0.161	0.278	0.348	0.650
Number of Observations	8,901,138	4,147,303	7,925,340	6,548,988

Notes: Standard errors bootstrapped. All regressions include individual, time, and state fixed effects. ***, **, * - denote significance at the 1%, 5%, and 10% level, respectively. Authors' calculations using data from the Federal Reserve Bank of New York/Equifax Consumer Credit Panel.

Table 7: Effect of the Dependent Coverage Mandate on Financial Distress: Triple-Difference Specification Using the Pre-policy County Uninsured Rate for Young Adults (Q1:2008–Q4:2013)

Coefficient	**Number of 90 DPD Occurrences on Revolving Accounts**	**Probability of New 120 DPD Occurrence**	**Number of Revolving Accounts with Major Derogatory**	**Probability of New Bankruptcy Filing**
Treated × Uninsured × Enactment (Q2:2010–Q3:2010)	-0.0025***	-0.0001**	-0.002***	-0.00002**
	(0.000)	(0.000)	(0.000)	(0.000)
Treated × Uninsured × Implementation (Q4:2010–Q4:2012)	-0.0026***	-0.0000	-0.002***	-0.00001
	(0.000)	(0.000)	(0.000)	(0.000)
Treated × Uninsured × Age-Out (Q1:2013–Q4:2013)	-0.0025***	-0.0000	-0.002***	-0.00000
	(0.000)	(0.000)	(0.000)	(0.000)
R^2	0.367	0.089	0.360	0.136
Number of Observations	7,396,593	8,622,075	7,399,732	8,678,030

Notes: Standard errors bootstrapped. All regressions include individual, time, and state fixed effects. ***, **, * - denote significance at the 1%, 5%, and 10% level, respectively. Authors' calculations using data from the Federal Reserve Bank of New York/Equifax Consumer Credit Panel.

Table 8: Effect of the Dependent Coverage Mandate on Financial Distress: Triple-Difference Specification Using the Pre-policy County Young Adult Uninsured Rate (Q1:2008–Q4:2013)

Coefficient	**Probability of New Account in Third-Party Collections**	**Amount of Debt in Third-Party Collections**	**Total Amount of Debt PD, All Accounts**	**Total Balance, Bankcard Accounts**
Treated × Uninsured × Enactment (Q2:2010–Q3:2010)	-0.0001	-0.177	-5.608***	0.256
	(0.000)	(0.861)	(1.825)	(1.210)
Treated × Uninsured × Implementation (Q4:2010–Q4:2012)	-0.0001	-2.749***	-6.548***	1.388*
	(0.000)	(0.517)	(2.131)	(0.748)
Treated × Uninsured × Age-Out (Q1:2013–Q4:2013)	0.0001	-0.850	-1.719	2.046*
	(0.000)	(0.876)	(3.146)	(1.125)
R^2	0.161	0.278	0.348	0.650
Number of Observations	8,901,138	4,147,303	7,925,340	4,548,988

Notes: Standard errors bootstrapped. All regressions include individual, time, and state fixed effects. ***, **, * - denote significance at the 1%, 5%, and 10% level, respectively. Authors' calculations using data from the Federal Reserve Bank of New York/Equifax Consumer Credit Panel.

Table 9: The Effect of the Dependent Coverage Mandate on Financial Distress: Heterogeneous Effects by Young Adult Uninsured Rate (Q1:2008–Q4:2013)

Coefficient	**Number of 90 DPD Occurrences on Revolving Accounts**	**Probability of New 120 DPD Occurrence**	**Number of Revolving Accounts with Major Derogatory**	**Probability of New Bankruptcy Filing**
High Uninsurance Rate				
Enactment	-0.014**	-0.001	-0.014**	-0.0005**
(Q2:2010–Q3:2010)	(0.007)	(0.001)	(0.006)	(0.000)
Implementation	-0.020***	-0.006***	-0.024***	-0.0005*
(Q4:2010–Q4:2012)	(0.006)	(0.002)	(0.007)	(0.000)
Age-Out	-0.013	-0.007***	-0.014	-0.0001
(Q1:2013–Q4:2013)	(0.010)	(0.002)	(0.009)	(0.000)
R^2	0.357	0.086	0.353	0.137
Number of Observations	2,524,569	3,018,701	2,525,572	3,042,260

Low Uninsurance Rate				
Enactment	-0.004	0.001	-0.003	-0.0001
(Q2:2010–Q3:2010)	(0.005)	(0.001)	(0.003)	(0.000)
Implementation	-0.005	-0.003***	-0.004	-0.0001
(Q4:2010–Q4:2012)	(0.005)	(0.001)	(0.004)	(0.000)
Age-Out	-0.003	-0.004***	0.001	-0.0001
(Q1:2013–Q4:2013)	(0.007)	(0.001)	(0.006)	(0.000)
R^2	0.372	0.091	0.363	0.136
Number of Observations	4,872.024	5,603,374	4,874,160	5,635,770

Notes: Standard errors bootstrapped. All regressions include individual, time, and state fixed effects. ***, **, * - denote significance at the 1%, 5%, and 10% level, respectively. Authors' calculations using data from the Federal Reserve Bank of New York/Equifax Consumer Credit Panel.

Table 10: The Effect of the Dependent Coverage Mandate on Financial Distress: Heterogeneous Effects by Subprime/Prime Status (Q1:2008–Q4:2013)

Coefficient	**Number of 90 DPD Occurrences on Revolving Accounts**	**Probability of New 120 DPD Occurrence**	**Number of Revolving Accounts with Major Derogatory**	**Probability of New Bankruptcy Filing**
Subprime				
Enactment	-0.023***	0.000	-0.0194***	-0.0001**
(Q2:2010–Q3:2010)	(0.008)	(0.001)	(0.007)	(0.000)
Implementation	-0.034***	-0.007***	-0.031***	-0.0002
(Q4:2010–Q4:2012)	(0.010)	(0.001)	(0.008)	(0.000)
Age-Out	-0.042***	-0.009***	-0.032***	0.0003
(Q1:2013–Q4:2013)	(0.010)	(0.002)	(0.006)	(0.000)
R^2	0.362	0.079	0.344	0.161
Number of Observations	2,868,925	3,501,170	2,871,466	3,446,966
Prime				
Enactment	0.002	-0.0000	0.001	0.0001
(Q2:2010–Q3:2010)	(0.004)	(0.001)	(0.003)	(0.000)
Implementation	0.003	0.0002	0.001	-0.0003*
(Q4:2010–Q4:2012)	(0.004)	(0.001)	(0.003)	(0.000)
Age-Out	0.010*	-0.0002	0.009*	-0.0002
(Q1:2013–Q4:2013)	(0.006)	(0.001)	(0.005)	(0.000)
R^2	0.375	0.088	0.366	0.093
Number of Observations	4,107,125	4,595,477	4,107,674	4,644,163

Notes: Standard errors bootstrapped. All regressions include individual, time, and state fixed effects. ***, **, * - denote significance at the 1%, 5%, and 10% level, respectively. Authors' calculations using data from the Federal Reserve Bank of New York/Equifax Consumer Credit Panel.

APPENDIX

Table A1: The Effect of the Dependent Coverage Mandate on Financial Distress: Heterogeneous Effects by Young Adult Uninsured Rate (Q1:2008–Q4:2013)

Coefficient	**Probability of New Account in Third-Party Collections**	**Amount of Debt in Third-Party Collections**	**Total Amount of Debt PD, All Accounts**	**Total Balance, Bankcard Accounts**
High Uninsurance Rate				
Enactment	-0.001	-3.109	-62.096	72.758***
(Q2:2010–Q3:2010)	(0.002)	(21.105)	(87.307)	(21.199)
Implementation	-0.001	-21.111	3.052	105.31***
(Q4:2010–Q4:2012)	(0.002)	(23.618)	(78.094)	(27.343)
Age-Out	0.001	12.546	114.21	190.39***
(Q1:2013–Q4:2013)	(0.003)	(35.418)	(180.219)	(40.380)
R^2	0.151	0.273	0.381	0.644
Number of Observations	3,133,574	1,687,069	2,722,539	1,435,502
Low Uninsurance Rate				
Enactment	0.001	3.415	7.164	47.560***
(Q2:2010–Q3:2010)	(0.001)	(12.209)	(17.746)	(11.519)
Implementation	-0.001	22.893	5.785	46.598***
(Q4:2010–Q4:2012)	(0.001)	(17.642)	(18.203)	(13.787)
Age-Out	-0.002	55.544**	34.281	83.092***
(Q1:2013–Q4:2013)	(0.002)	(27.419)	(31.987)	(19.994)
R^2	0.166	0.283	0.258	0.653
Number of Observations	5,767,564	2,460,234	5,202,801	3,113,486

Notes: Standard errors bootstrapped. All regressions include individual, time, and state fixed effects. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Authors' calculations using data from the Federal Reserve Bank of New York/Equifax Consumer Credit Panel.

Table A2: The Effect of the Dependent Coverage Mandate on Financial Distress: Heterogeneous Effects by Subprime/Prime Status (Q1:2008–Q4:2013)

Coefficient	**Probability of New Account in Third-Party Collections**	**Amount of Debt in Third-Party Collections**	**Total Amount of Debt PD, All Accounts**	**Total Balance, Bankcard Accounts**
Subprime				
Enactment	0.002	-10.623	-23.606	26.387
(Q2:2010–Q3:2010)	(0.002)	(13.717)	(83.026)	(20.159)
Implementation	-0.002	-10.241	34.804	19.957
(Q4:2010–Q4:2012)	(0.003)	(17.481)	(86.519)	(23.529)
Age-Out	-0.003	29.890	116.07	12.401
(Q1:2013–Q4:2013)	(0.004)	(25.118)	(113.348)	(36.534)
R^2	0.108	0.258	0.335	0.649
Number of Observations	3,610,783	2,817,620	3,090,811	1,016,355
Prime				
Enactment	-0.002*	25.355	-13.514	62.664***
(Q2:2010–Q3:2010)	(0.001)	(24.970)	(13.989)	(16.127)
Implementation	-0.001	33.372	-12.354	73.348***
(Q4:2010–Q4:2012)	(0.001)	(32.546)	(14.423)	(18.748)
Age-Out	-0.001	75.860	49.344**	140.51***
(Q1:2013–Q4:2013)	(0.002)	(48.962)	(22.527)	(29.758)
R^2	0.175	0.310	0.466	0.646
Number of Observations	4,751,249	1,147,147	4,319,835	3,242,659

Notes: Standard errors bootstrapped. All regressions include individual, time, and state fixed effects. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Authors' calculations using data from the Federal Reserve Bank of New York/Equifax Consumer Credit Panel.

Table A3: Placebo Tests for Measures of Financial Distress

Coefficient	Number of 90 DPD Occurrences on Revolving Accounts	Probability of New 120 DPD Occurrence	Number of Revolving Accounts with Major Derogatory	Probability of New Bankruptcy Filing
Placebo Treatment Group: 1980–1981				
Placebo Control Group: 1978–1979				
Enactment	-0.006	0.000	-0.003	-0.0000
(Q2:2010–Q3:2010)	(0.005)	(0.001)	(0.004)	(0.000)
Implementation	-0.007	-0.002*	-0.005	-0.0002
(Q4:2010–Q4:2012)	(0.005)	(0.001)	(0.004)	(0.000)
Age-Out	0.007	-0.002	-0.002	-0.0001
(Q1:2013–Q4:2013)	(0.006)	(0.001)	(0.005)	(0.000)
R^2	0.371	0.090	0.356	0.155
Number of Observations	7,735,983	8,876,613	7,741,544	8,933,378

Notes: Standard errors bootstrapped. All regressions include individual, time, and state fixed effects. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Authors' calculations using data from the Federal Reserve Bank of New York/Equifax Consumer Credit Panel.

Table A4: Placebo Tests for Measures of Financial Distress

Coefficient	Probability of New Account in Third-Party Collections	Amount of Debt in Third-Party Collections	Total Amount of Debt PD, All Accounts	Total Balance, Bankcard Accounts
Placebo Treatment Group: 1980–1981				
Placebo Control Group: 1978–1979				
Enactment	-0.0004	-18.205	3.526	18.195
(Q2:2010–Q3:2010)	(0.001)	(12.498)	(30.922)	(15.739)
Implementation	-0.0006	-6.013	-39.847	2.767
(Q4:2010–Q4:2012)	(0.001)	(15.532)	(31.373)	(20.449)
Age-Out	-0.0014	-9.398	-70.995	33.886
(Q1:2013–Q4:2013)	(0.002)	(20.218)	(70.860)	(32.106)
R^2	0.163	0.220	0.366	0.673
Number of Observations	9,128,992	4,286,672	8,132,624	4,845,667

Notes: Standard errors bootstrapped. All regressions include individual, time, and state fixed effects. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Authors' calculations using data from the Federal Reserve Bank of New York/Equifax Consumer Credit Panel.