FEDERAL RESERVE BANK
PHILADELPHIA

Working Papers
RESEARCH DEPARTMENT

Health Insurance and Young Adult Financial Distress

Nathan Blascak
Federal Reserve Bank of Philadelphia
Consumer Finance Institute

Vyacheslav Mikhed
Federal Reserve Bank of Philadelphia
Consumer Finance Institute

WP 19-54

PUBLISHED
DECEMBER 2019

REVISED
AUGUST 2022

FRB CFI PHILADELPHIA

ISSN: 1962-5361

DOI: https://doi.org/10.21799/frbp.wp.2019.54

Health Insurance and Young Adult Financial Distress

Nathan Blascak and Vyacheslav Mikhed*
Federal Reserve Bank of Philadelphia

August 2022

Abstract

We study how health-insurance eligibility affects financial distress for young adults using the Affordable Care Act's (ACA) dependent coverage mandate — the part of the ACA that requires private health-insurance plans to cover individuals up to their 26th birthday. We examine the effects of both gaining *and* losing eligibility by exploiting the mandate's implementation in 2010 and its automatic disenrollment mechanism at age 26. Our estimates show that increasing access to health insurance lowers young adults' out-of-pocket medical expenditures and debt in third-party collections. However, reductions in financial distress are transitory, as they diminish after an individual loses access to parental insurance when they age out of the mandate at age 26.

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

---

* Blascak: Consumer Finance Institute, Federal Reserve Bank of Philadelphia, 10 Independence Mall, Philadelphia, PA, 19106 (email: nathan.blascak@phil.frb.org). Mikhed: Consumer Finance Institute, Federal Reserve Bank of Philadelphia, 10 Independence Mall, Philadelphia, PA, 19106; (email: slava.mikhed@phil.frb.org). The authors would like to thank Robert M. Hunt, Julia Cheney, Andrew Kish, Daniel Grodzicki, Marieke Bos, Catherine Maclean, Steven Hill, Doug Webber, Thanh Lu, Craig Garthwaite, and Matthew Notowidigdo for their helpful comments and Anna Tranfaglia and Peter Psathas for their valuable research assistance. We also thank seminar participants at the Federal Reserve Bank of Philadelphia, Temple University, the APPAM Student Conference, the Boulder Summer Conference on Consumer Financial Decision Making, the Western Economic Association Conference, the New Perspectives on Consumer Behavior in Credit and Payments Markets Conference, the APPAM Annual Fall Research Conference, the FDIC Consumer Research Symposium, and the CEAR-RSI Household Finance Workshop for their helpful comments. A previous version of this paper was circulated as "Did the ACA's Dependent Coverage Mandate Reduce Financial Distress for Young Adults?"

INTRODUCTION

Although younger individuals are one of the healthiest demographic groups in the U.S., with a low incidence of chronic diseases and declining rates of tobacco and alcohol use,[1] they are a population at high risk of financial distress because of medical expenditures. This is because young adults (YAs) have one the highest uninsured rates of any age group in the U.S. (15% in 2019 for individuals ages 19–25), and they own few assets while also holding relatively high levels of nonmortgage debt. This combination of low insured rate and limited ability to borrow implies that even a small medical shock could have serious negative financial consequences, further straining young adults' balance sheets. While numerous studies have shown that health insurance improves financial outcomes for low-income and older adults, less is known how insurance coverage affects younger adults' financial outcomes.

This paper addresses this gap in the literature by exploiting the Affordable Care Act's (ACA) dependent coverage mandate (DCM) to assess the impact of having access to health insurance on the financial outcomes of YAs. The DCM is a unique policy change in that it (1) requires that all *private* health-insurance plans offering dependent coverage policies in the U.S. cover older children up to the age of 26, and (2) makes YAs automatically ineligible for dependent coverage *after* they turn age 26. Because of the specific features of the DCM, we can observe individuals both becoming eligible and ineligible for health insurance during our sample period. This age-based eligibility criteria allows us to use different empirical approaches to examine how financial distress changes as individuals vary in their eligibility for health insurance.

To study how health insurance affects younger adults' financial outcomes, we first use the passage of the mandate and its age eligibility rule to formulate a standard differences-in-differences (DID) event study design to estimate the financial effects of *gaining* access to health insurance. We use this DID framework and compare the financial outcomes of YAs ages 23–25 to outcomes of individuals ages 27–29 before and after the DCM's implementation in 2010.[2] Second, to estimate the effects of *losing* insurance access under the DCM, we exploit the aspect of the mandate that limits insurance coverage to dependents up to their 26th birthday. Because of the introduction of a national age-based cutoff, we may expect to see differences in the age dynamics in financial outcomes before and after the implementation of the DCM. We take advantage of this age-based mechanism and compare financial outcomes across ages before and after the ACA's implementation.

Similar to other recent studies on the financial effects of health-insurance policy changes (Argys, Friedson, Pitts, and Tello-Trillo, 2020; Brevoort, Grodzicki, and Hackmann, 2020; Hu, Kaestner,

[1] According to the Substance Abuse and Mental Health Services Administration (SAMHSA), 19.1% of young people ages 18–25 reported cigarette use in the past month, and 55.1% reported alcohol use in the past month in 2018. Both of these figures represent the lowest-recorded usage rates since 2002. Opioid misuse is also down, with 5.6% of young adults reporting opioid misuse in 2018, a 3.1% decline since 2015. For more information, see SAMHSA (2019).

[2] We intentionally omit individuals who are age 26 because they will be partially treated (i.e., eligible to be covered by the DCM) in the year they turn 26.

Mazumder, Miller, and Wong, 2018; Mazumder and Miller, 2016),[3] we use anonymized individual-level credit report data. These data are well suited for assessing financial distress; they contain detailed records of these individuals' financial information over time. We also use data from the Medical Expenditure Panel Survey (MEPS) to assess the effect of access to health insurance on medical expenditures, out-of-pocket (OOP) expenditures, and the probability of incurring large OOP medical expenditures. The MEPS has been previously used by Chua and Sommers (2014) and Chen, Vargas-Bustamante, and Novak (2017) to assess the effect of the passage of the ACA's DCM on OOP expenses, showing that its implementation led to a 14% decrease in annual health-care expenditures and an 18%–21% decrease in OOP expenditures.

The results in the first part of the paper show that access to health insurance lowers financial distress for young individuals. Using the MEPS data, we confirm previous studies' results that have found that OOP medical expenditures declined for individuals covered by the mandate. We also find that these individuals have a lower probability of incurring very large OOP medical expenditures, which is consistent with health insurance limiting medical expenditure risk. Results from the credit report data show that the introduction of the mandate reduced the probability of having debt in third-party collections, the number of third-party collections, and the amount of debt in third-party collections. By 2012, YAs eligible to be covered by the DCM experienced a 0.9 percentage point (7.4%) decline in the probability of having an account in collections and a $75 decline (10.5%) in the amount of debt in collections.

These results are consistent with our heterogeneity analysis findings that declines in third-party collections are primarily concentrated in the far-right tail of the collection balance distribution. Results from estimating conditional quantile regression event study models indicate that declines in debt in third-party collections start at the 95th percentile and are economically significant. At the 99th percentile of debt in third-party collections, we estimate a decline of $728 by 2013.We also find that YAs living in counties with high uninsured rates prior to the passage of the mandate experienced the largest declines in financial distress. This result is in-line with recent research showing that place-based factors play a role in the incidence of financial distress (Goldsmith-Pinkham, Pinkovskiy, and Wallace, 2020; Keys, Mahoney, and Yang, 2020).

Our results in the second half of the paper indicate that after an individual ages out of the mandate's coverage at age 26, financial distress increases along a number of different measures. Using MEPS data, we observe that the percent of total medical expenses paid OOP are 5 percentage points higher for individuals who turn age 26 in the postmandate period (after 2010), compared with individuals who turn age 26 and older in the premandate period (before 2010). Results from the credit report data show that individuals turning age 26 and older in the postmandate period are 4% more likely to have an account in collections and have more accounts sent to third-party debt collectors. We also see that collection balances in the right tail of the distribution increase in size for YAs older than 26, with collection balances at the 99th percentile increasing by over $300. These results are consistent with findings from Dahlen (2015), who finds a 15.4 percentage point increase in the share of YAs with worse health-insurance coverage after they aged out of the mandate, and Batty, Gibbs, and Ippolito (2018), who document that

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

medical bills in collections are highest for YAs in their late 20s. Our findings may also suggest that the quality of health insurance, in addition to access, is important for financial outcomes.

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, 2018), 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 of changes in health-insurance policies that have used credit report information and public health-insurance expansions, we analyze the effect of a government-mandated expansion in *private* insurance coverage on financial outcomes of intended beneficiaries, not public insurance programs such as Medicaid 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 both the effect of enrollment in health-insurance coverage and 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 YAs. While there are recent studies examining the effects of financial education mandates (Brown, Grigsby, van der Klaauw, Wen, and Zafar, 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 documented. 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 expanding health insurance to young adults leads to material improvements in their financial well-being, while losing eligibility worsens it. This is consistent with the 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 DCM. This implies that our estimates of the mandate's effect on treated individuals are more conservative than the treatment effects for individuals who actually received health insurance through the mandate. This is because we average the effects across eligible individuals who actually received health insurance through their parents' plans and those who did not.

BACKGROUND AND FRAMEWORK

Young Adult Financial Health

The financial health of young adults can be characterized by three stylized facts. First, YAs are both asset and savings poor. Data from the 2016 Survey of Consumer Finances (SCF) show that the median bank deposits[4] for young adults ages 19–25 is approximately $1,500, and the average

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

amount of money in savings accounts was less than $3,000, with a median of less than $100. Median financial assets for this age group were approximately $2,400, while average total assets, including real estate, were only $12,800.[5] Second, YAs have fairly high levels of debt. Based on data from the Federal Reserve Bank of New York Consumer Credit Panel/Equifax Data (CCP), average debt balances in 2016 for 19- to 25-year-olds were approximately $18,800, with a median of $8,600. Individuals who are 25 years of age experienced even higher levels of indebtedness, with an average debt balance of $30,300.

These numbers are relatively similar to those for the period prior to the implementation of the ACA's DCM in 2010. Data from the 2010 SCF show that for YAs ages 19–25, median total bank deposits were approximately $1,072, and the median amount of money in savings accounts was $0. Median financial assets for this age group were approximately $1,558, and median total assets were $11,163. Average debt levels for individuals ages 19–25 in 2009 were approximately $13,400, while the median was $7,400.

Third, YAs generally have had low rates of financial literacy. Surveys conducted by the Jump$tart Coalition show that high school students consistently score poorly on a test of financial literacy, correctly answering only half of the test questions since 2000.[6] 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, YAs in this time period had low levels of income and assets, had lower financial literacy, and had high levels of debt.

It is important to consider how these resource constraints affect a young individual's ability to smooth consumption and cover medical expenses. Although YAs 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 the MEPS data, approximately 17.5% of all health-care expenditures in 2009 by young adults ages 19–25 were paid OOP. Approximately 48.7% of these individuals paid more than 50% of their total yearly medical expenses OOP, and 37.8% paid their entire yearly medical expenses OOP. Given that YAs have relatively little in savings and assets and have higher amounts of debt, there is a higher probability that they would face financial hardship with an expensive medical bill.[7]

Dependent Coverage Mandates and Young Adult Health Insurance

State-Level Dependent Coverage Mandates

In the years before 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 became 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

[5] Total assets include financial assets, such as stocks and bonds, and nonfinancial assets, such as real estate and vehicles.

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

[7] Statistics are based on the authors' calculations using data from the MEPS. For more information on the MEPS, see www.meps.ahrq.gov.

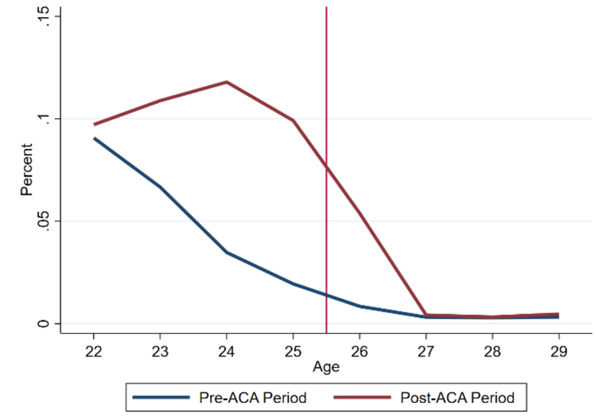
of these laws was in part a response to the fact that, historically, YAs have been the most uninsured age demographic in the United States. While estimates vary based on the definition of health-insurance coverage, there is broad consensus that the rate exceeded 30%, with some estimates as high as 37%.

However, despite these state-level mandates, YA uninsured rates 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 state laws were fairly narrow in scope, with eligibility varying by student status, marriage status, and type of insurance plan. Prior research also suggests 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.

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 DCM was one of the first provisions to be implemented, taking effect in late September 2010. The mandate standardized dependent coverage across all states, requiring all family health-insurance plans to offer coverage for dependent children until they reached age 26, regardless of student or marital status. Subsequent analysis on the coverage effects of the mandate using data from the Survey of Income and Program Participation (SIPP) and the Current Population Survey (CPS) have shown that the uninsured rate for adult dependents dropped significantly, with 2 million to 3 million YAs receiving dependent coverage through parental insurance by the end of 2011 (Akosa Antwi et al., 2013; Cantor et al., 2012b; Sommers et al., 2013). **Figure 1** shows the probability that a YA is covered by a health-insurance policyholder living outside of the household by age before and after the DCM using data from the MEPS. Consistent with the previous literature, we see that for individuals who are eligible for coverage (those younger than 26), there is a substantial increase in this probability in the post-ACA DCM period, with young adults ages 24 and 25 seeing an approximately 7 percentage point increase in having this kind of coverage, relative to YAs of the same age prior to the DCM.

Figure 1: Probability of Being Covered by a Policyholder Outside of the Household



Notes: Based on authors' calculations using data from the MEPS. *Pre-ACA* is defined as the years 2005–2009, while post-ACA is defined as the years 2011–2015.

However, this increase in YA 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 a 1.7% decline in own-name ESI for YAs 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 YAs ages 19–25 has declined dramatically to 15%, more than a 50% 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. As shown in **Appendix A Figure A1**, there was a significant variation in the uninsured rate across the U.S. prior to the passage of the DCM 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%. These rates declined substantially after the mandate's implementation, especially in the Southwest and Western states as shown in **Appendix Figure A2**.

Conceptual Framework

Gaining Insurance

The fundamental purpose of health insurance is to reduce the financial risk of health-care spending. For YAs, who typically are healthier and have a lower incidence of chronic illnesses, we would expect that insurance does *not* function as a means for financing *regular* medical spending. Instead, it should act more in accordance with the standard model of insurance, mitigating financial uncertainty in the future. This is in contrast to older adults, who are relatively less healthy than YAs and suffer from chronic illnesses at a higher rate;[8] for them, health-care costs can be very high and are regularly incurred. Because of these persistent and high costs, older adults frequently use health insurance as a tool to finance the use of regular and predictable medical spending, regardless of being in either a healthy or sick state. Using health

[8] Note: 60% of adults ages 50–64 have at least one chronic condition (Fox, Duggan, Rainie, and Purcell, 2013).

insurance in this way is a deviation from the standard model, in which insurance smooths income across different states of health. This distinction in the use of health insurance between the two age groups is important; it suggests that we may find differential financial effects from policies that expand insurance coverage to younger individuals compared with older individuals.

Acquiring health insurance improves financial outcomes through two primary mechanisms: a risk effect and an income effect. The risk effect improves financial outcomes by lowering expected medical expenditures in the case of adverse health shocks. This reduction in risk may allow for investing (Goldman and Maestas, 2013; Ayyagari and He, 2016) or reduce the need to have precautionary savings (Kotlikoff, 1989; Gruber and Yelowitz, 1999; Lee, 2016). Because of the differences in the incidences of chronic disease and other adverse health issues between older and younger adults, the transition from being uninsured to having health insurance should lower medical expenditure risk *less* for YAs, all else equal. In addition, YAs have significantly less income and assets, which implies that the level of medical expenditures that would trigger default is lower, leaving them vulnerable to smaller shocks. However, the overall effect of this risk reduction on financial outcomes depends on additional factors, including health-risk status and individual preferences for insurance (Cutler, Finkelstein, and McGarry, 2008).

For the income effect, the financial effects of gaining coverage depend on prior insurance status. For uninsured individuals who gain coverage, having insurance likely results in an income effect that improves financial standing. Because access to health insurance directly lowers total OOP medical expenses through co-pays and co-insurance, insured individuals will have increased availability of financial resources, via higher real incomes or increased credit availability. Having additional financial resources may also allow individuals to pay down existing delinquent debt or prevent future delinquencies. Delinquent and/or unpaid medical bills can lead to substantial financial problems (Brevoort et al., 2020); therefore, increasing insurance coverage should directly lead to a lower incidence of these events and, in turn, reduce financial distress. The presence of moral hazard will reduce the magnitude of the income effect, as any increased health-care utilization after receiving coverage would increase OOP expenses. For young adults receiving coverage through parental ESI, the magnitude of the income effect would depend on the distribution of the premium and the cost sharing between parents and dependents.

Parental ESI

A potential caveat when thinking about how insurance improves financial health for YAs is that the DCM induced crowd out, leading some dependents to switch from own-name ESI plans to their parents' plans (Akosa Antwi et al., 2013). 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 YAs 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 who switched because of net utility gains through increased benefits and because they incurred lower OOP costs.

Losing Health Insurance

While expanding insurance coverage can improve financial outcomes, the financial effects of *losing* insurance are more ambiguous. While many individuals will regain access through alternative means upon losing DCM eligibility (e.g., COBRA, the ACA marketplaces), some will experience an uninsured spell. Loss of insurance will likely lower medical-care access and utilization (Anderson, Dobkin, and Gross, 2012, 2014; Ghosh and Simon, 2015; Tello-Trillo, 2021), leading to lower OOP expenditures while increasing medical expenditure risk. This, in turn, may lead to worse financial outcomes (Argys et al., 2020).[9] If an individual's physical health worsens as a result of losing health insurance, this could also lead to worse financial outcomes. Uninsured (or underinsured) individuals may have to seek care at an emergency room, which is expensive relative to primary care and can lead to serious negative financial consequences (Dobkin, Finkelstein, Kluender, and Notowidigdo, 2018). Once again, because of the health of younger adults and their patterns of utilization of care, we expect that the medical expenditure risk is lower for younger adults than older adults when losing health insurance.

In summary, we form the following hypotheses regarding the effects of the ACA's DCM:
(1) For young adults who *gain* access to health insurance, we expect financial distress to *decrease*. The magnitude of the decline is ambiguous.[10]
(2) If young adults are able to reinsure at the same generosity level after the automatic disenrollment at age 26, we would expect no change in financial distress after age 26.
(3) For young adults who *lose* health insurance via the aging-out mechanism, we expect financial distress to *increase*. The magnitude of the increase is ambiguous.
(4) For young adults who are able to reinsure, but with lower generosity plans, we expect financial distress to *increase*. While the magnitude of the increase is ambiguous, it would be smaller than for young adults who become uninsured after the auto disenrollment.
(5) We expect the effects of losing health insurance access because of the aging-out mechanism of the DCM to be *asymmetric* with the effects of gaining access to health insurance, if young adults are able to reinsure.

DATA

Medical Expenditure Panel Survey Data

To examine how the ACA's DCM affected the health-care expenditures of young adults, we use data from the 2005–2015 waves of the Household Component of the MEPS. The MEPS is a yearly nationally representative survey of approximately 40,000 individuals and families. Participants in the MEPS are surveyed five times over two years about their households' use of health services, insurance status, and health-care expenditures. The survey also collects detailed information on the characteristics of all members in the household, including demographic information, income, and employment.

---

[9] Tello-Trillo (2021) discusses how an individual who used health care prior to disenrollment, and gained health capital (such as information on a chronic illness), would not lose this health capital when becoming uninsured.

[10] Given that younger adults are healthier, we expect the majority of the effect to be because of the insurance value of health coverage, not subsidizing predictable health-care expenditures (which may be the case for other groups such as older adults).

We limit our sample to individuals ages 22–29 because we are primarily interested in examining the age dynamics of medical expenses before and after the implementation of the DCM. **Table 1** provides summary statistics for our medical spending variables, and we report summary statistics for our demographic variables in **Appendix B**.

Table 1: MEPS Summary Statistics, 2006–2013

	Treatment Group Ages 23–25			**Control Group Ages 27–29**		
	Overall	Pre-ACA (2006–2009)	Post-ACA (2011–2013)	Overall	Pre-ACA (2006–2009)	Post-ACA (2011–2013)
Total OOP medical spending	328.68	344.90	319.27	382.30	382.76	387.65
	(14.30)	(20.08)	(22.99)	(18.53)	(27.12)	(25.86)
Total medical spending	1,979.04	1,693.51	2,429.55	2,118.37	1,929.83	2,471.33
	(164.04)	(96.65)	(393.11)	(101.53)	(106.70)	(193.51)
% of total medical spending paid OOP†	0.275	0.297	0.236	0.272	0.282	0.264
	(0.005)	(0.007)	(0.007)	(0.006)	(0.007)	(0.008)
Number of obs.	11,300	5,284	4,761	11,233	5,252	4,681
Fraction with OOP medical expenses	0.544	0.57	0.515	0.586	0.614	0.558

Notes: Authors' calculations using data from the Medical Expenditure Panel Survey (MEPS) from 2006–2013. The sample is limited to individuals ages 22–29. Standard deviations reported in parentheses. All calculations made with sample weights. Expenditure variables deflated to 2010.
†: conditional on having any OOP expenses.

YAs in our treatment group average approximately $1,979 in total medical expenditures per year and $328 in OOP expenses. In comparison, individuals ages 30– 64 averaged $4,176 in total medical expenditures over the same period.[11] While total medical expenditures for YAs have increased over time, the percent of expenditures that have been paid OOP have decreased in the post-ACA period. We also observe a decrease in the percent of individuals who have any OOP medical expenditures at all. These trends are consistent with the decrease in the uninsured rate for this population and the increase in insurance generosity because of the passage of the ACA's dependent coverage mandate.

In addition to total OOP medical expenditures, we also look at an alternative measure of OOP expenditures. Because of a methodology change in the calculation of expenditures for prescription medications in the MEPS in 2006, total OOP expenditures for individuals of certain ages in the years 2005–2006 differ significantly from the other years in our sample. To address this issue, we subtract OOP prescription drug spending from total OOP spending to form a "net"

[11] For a closer comparison, individuals ages 30–39 average $2,649 in total medical expenditures.

OOP medical expenditure variable. Details of the methodology change and summary statistics for our net OOP expenditure variables are reported in **Appendix C**. While netting out prescription expenditures minimizes the effect of the methodology change on our variables of interest (to the degree that the dependent coverage mandate affects prescription drug spending), omitting these expenditures from our analyses may produce results that are less informative of the effects of the policy.

Consumer Credit Data

The consumer credit data used in the analysis come from the Federal Reserve Bank of New York Consumer Credit Panel/Equifax Data (CCP). The CCP data set is an anonymized, nationally representative 5% 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 data, it does not contain any demographic information besides the year of birth and census geography. In a given quarter, the CCP contains data on approximately 12 million different consumers.[12]

One possible concern with our data is that not all individuals, especially YAs, 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); they provide strong evidence that the young adult population covered in the CCP is representative of other measures of this age population. An analysis by Brevoort, Grimm, and Kambara (2016) shows that, while approximately 62% of consumers ages 18–19 do not have a credit bureau file, this number drops to nearly 10% for consumers ages 25–29. We conduct our own analysis in **Appendix D** by comparing aggregated population counts from the CCP with population estimates from the census and conclude that the YA population covered in the CCP is representative.

We use these credit bureau data for two types of analysis. First, we use them to measure the effect of the ACA's DCM's implementation in 2010 on financial outcomes using a differences-in-differences (DID) research design, in which we compare DCM-eligible young adults with young adults who are too old to be covered. Second, we rely on credit bureau data for a broader set of YAs to estimate the effect of disenrollment from the DCM at age 26. We describe the age-out CCP sample in more detail in the age-out analysis section later in the paper, while the CCP sample used to analyze the DCM's implementation is discussed next.

When constructing our CCP sample for the analysis of the implementation of the DCM in 2010, there are two important considerations. First, because the mandate's effects are determined by age, we restrict the data to include the credit files of individuals who were ages 23–29 from 2006 to 2013. We exclude individual observations for age 26 because some individuals may be still eligible for coverage under the DCM, while others would not. Because the CCP only contains information on birth year, we include credit data for the fourth quarter of each year (as of December 31), when everyone in both groups is the correct age.

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

Table 2: Financial Distress Summary Statistics, 2006–2013

	Treatment Group Ages 23–25			Control Group Ages 27–29		
	Overall	Pre-ACA (2006–2009)	Post-ACA (2011–2013)	Overall	Pre-ACA (2006–2009)	Post-ACA (2011–2013)
Probability of debt in third-party collections	0.237	0.234	0.239	0.231	0.227	0.236
	(0.425)	(0.423)	(0.426)	(0.421)	(0.419)	(0.425)
Number of accounts in third-party collections	0.494	0.478	0.507	0.496	0.474	0.524
	(1.350)	(1.302)	(1.384)	(1.411)	(1.335)	(1.505)
Amount in third-party collections ($)	714.98	649.37	802.39	670.77	596.93	770.55
	(2,545.33)	(2,410.19)	(2,699.64)	(2,635.14)	(2,338.57)	(2,865.43)
Num. of obs.	1,752,032	898,089	635,791	1,847,958	930,078	686,663
Num. of individuals	1,140,934					

Notes: Authors' calculations using data from the CCP. Data are for the sample period 2006–2013.

Second, we do not observe all individuals in each time period because many young adults have *thin* credit files[13] and therefore do not have continuously present credit bureau files.[14] 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 history over the sample period. Instead, we allow for unbalanced panels to avoid this bias. We then take a random subsample of the CCP data from 2006 to 2013 to improve computational efficiency and drop any individuals who have fewer than four total observations across the sample period. Dropping these individuals reduces the likelihood that we include either duplicate observations or fraudulent accounts in our analysis. Our final sample includes 1,140,000 individuals and 3.7 million observations.

To examine financial distress, we follow the recent literature that analyzes the financial effects of insurance policy (e.g., Hu et al., 2018; Dobkin et al., 2018) and focus on the presence of third-party collections as a measure of financial health.[15] In most cases, unpaid medical bills are sent to third-party collections instead of being reported as delinquent, which is typical for other types of debt in consumer credit reports. To capture different margins of adjustment in third-party

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

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

[15] 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.

collections, we use the number of accounts in third-party collections, the probability of having an account in third-party collections, and the amount of debt in third-party collections. The probability of having a third-party collection is an extensive margin measure (whether an individual has an account in collections or not), while the number of accounts and the amount of debt in third-party collections are intensive margin measures that show how many bills and the amount of debt owed change. We also consider bankruptcy, one of the most extreme forms of financial distress, in **Appendix Section E**. Table 2 presents summary statistics for these variables in the treatment and control groups.

Control Variables Data

To control for local economic conditions, we use annual county-level data on the percent of population in poverty, unemployment rate, and median income. Although we cannot observe or control for individual- or household-level insurance status, in our heterogeneity analysis (described in the next section), we use data on county-level differences in insurance status prior to the passage of the mandate. These data come from the Small Area Health Insurance Estimates (SAHIE) from the U.S. Census Bureau. The SAHIE data are produced by a hierarchical Bayesian model that estimates health-insurance coverage for every county in the United States. This model combines data from multiple sources, including the ACS, the CPS, and data from Medicaid and SNAP programs.[16] Following Mazumder and Miller (2016), we use uninsured rates for 18- to 39-year-olds in our models.[17]

THE EFFECT OF THE DCM ON OOP EXPENSES

To examine if the implementation of the ACA's DCM reduced financial distress for young adults, we first establish that the mandate was successful in reducing medical expenditures and medical expenditure risk. To do this, we use MEPS data and estimate an event-study model using a standard DID strategy and compare young adults eligible to be covered by the law (under the age of 26) with young adults unaffected by the law (older than 26) before and after the DCM took effect. Because we cannot observe changes in the individual health insurance status before or after the mandate's implementation, our estimates are intent-to-treat (ITT) only.[18] We restrict our MEPS sample to the years 2007–2013 to focus on the three years before and after its implementation.[19] Our DID estimating equation is then:

---

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

[17] While the ACS has more disaggregated data on uninsured rate by geography and age, the 2010 ACS only has county-level uninsured information for less than a quarter of all counties because of sampling. The SAHIE, while having less disaggregated data, has complete geographic coverage. We therefore choose to use the SAHIE data as our uninsured data source.

[18] Although insurance status is available in MEPS data, we cannot track the health insurance of every individual in our sample before and after the DCM's implementation. This is because we only observe two years of data for each individual because of the MEPS rotating panel structure. Also, we cannot observe whether individuals gained insurance because of the DCM or other reasons. Therefore, we are not able to provide treatment-on-the-treated (TOT) estimates with these data.

[19] Because of the MEPS methodology change mentioned in the Data section, we omit 2005 and 2006 from our event study analyses. We test the inclusion of these years in our analysis by using our alternative measure of OOP

$$y_{it} = \beta_0 + \lambda Treatment_i \times \gamma_t + \beta_1 Treatment_i + \gamma_t + X_{it}\Omega + \epsilon_{it}, \quad (1)$$

where γ_t is a vector of year dummy variables. We define $Treatment = 1$ for young adults between the ages of 23 and 25 (treatment group), and $Treatment = 0$ for young adults ages 27–29 (control group). Because there are some individuals who appear in our sample more than once, we adjust our treatment variable so that it is coded as missing if a treated individual becomes *ineligible* upon aging out of the DCM.[20] Coding the treatment variable in this way prevents previously treated individuals from being included in the control group. Restricting our treatment and control groups to this relatively narrow bandwidth of ages helps to ensure the demographic composition of both groups remains stable across time.[21] The vector X includes a rich set of demographic control variables, including race, income, level of education, family size, marital status, student status, census region, and age. We summarize these demographic variables for our treatment and control groups in the pre- and postmandate period in Appendix B. The coefficients of interests are in the vector λ, which are the interactions of $Treatment_i$ with the vector of year dummy variables. These coefficients are estimated relative to the excluded time period of 2009. We compute heteroskedasticity robust standard errors.[22]

Our dependent variables of interest are four different measures of medically related financial outcomes: total medical expenditures, total OOP medical expenditures, the percent of total medical expenditures paid OOP, and the probability that an individual has high OOP expenses (which we define as having at least $2,000 in OOP medical expenses). Results from the event study model are presented in **Figure 2**.

expenditures where we net out prescription drug expenditures. More details regarding the methodology change and summary statistics and results using this net measure are presented in Appendix C.

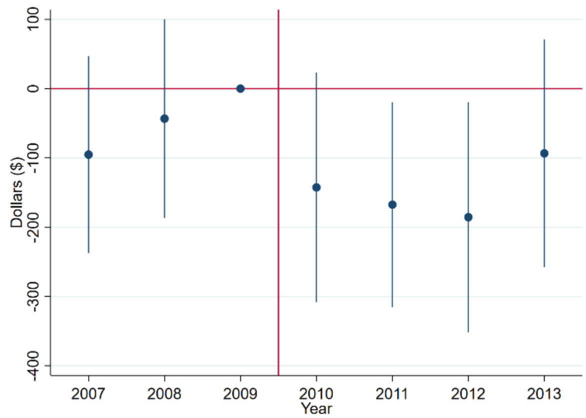
[20] Because of the way we define our sample and the treatment and control groups, only 27% of individuals have a second observation.

[21] Our approach also addresses the issue raised by Slusky (2017), regarding previous research that used DID strategies to study the dependent coverage mandate. He finds 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.

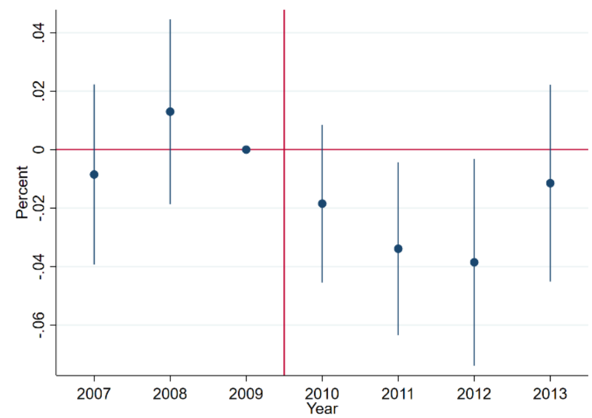
[22] As a robustness check, we cluster our standard errors at the individual level. Regression results using this level of clustering are nearly identical to our main results.

Figure 2: Medical Spending Event Study Results

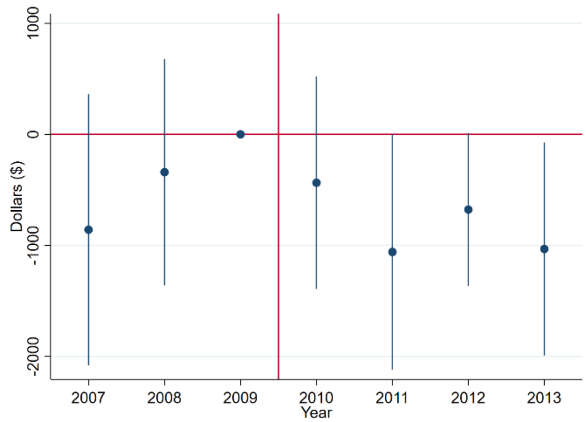
Panel A: OOP medical expenditures



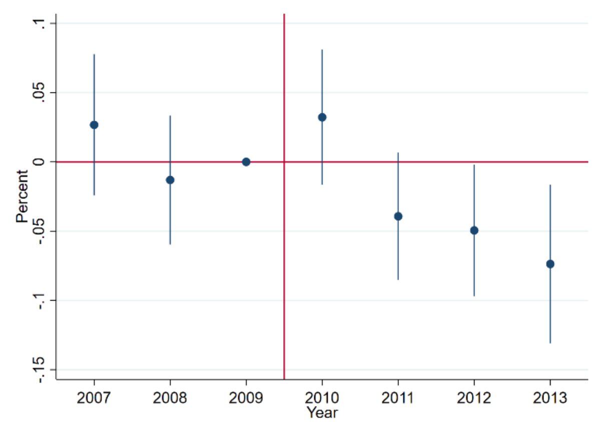
Panel B: Probability of having more than $2,000 in OOP expenditures



Panel C: Total medical expenditures



Panel D: Percent of total medical expenditures paid OOP



Notes: Based on authors' calculations using MEPS data and sample weights. Dots represent event-study coefficient estimates, while lines show 95% confidence intervals. These confidence intervals are based on Huber-White standard errors.

Panel A of Figure 2 shows that OOP medical expenditures for young adults decreased after the implementation of the ACA's DCM. We find that total OOP expenditures decreased by $93–$185, or 28%–56% relative to the pre-DCM mean. In Panel B, we find that the probability that a young adult incurs high OOP expenses declines by 4 percentage points. This result is consistent with the DCM lowering medical expenditure risk for young adults. Part of this reduction may be related to the decline in total medical expenditures, shown in Panel C, but we only observe one year in the post-ACA DCM period, where we find statistically significant declines in total medical expenditures. However, Panel D shows that the share of total medical expenditures paid OOP declined by 3.9 to 7.3 percentage points in the first three full years of the post-ACA DCM period (2011–2013). This indicates that although both OOP and total medical expenditures are declining, OOP expenditures are declining at a faster rate.

To test if these results are driven by changes in utilization by young adults, we examine multiple measures of health-care consumption in the MEPS. Results are reported in **Appendix F**. Overall, we do not find any evidence that our results are driven by changes in health-care utilization, which is consistent with the findings from Chua and Sommer (2014) and Barbaresco, Courtemanche, and Qi (2015), who also report that health-care consumption did not increase for YAs in the post-DCM period.

Our medical expenditure results are consistent with the Chua and Sommers (2014) and Chen, Vargas-Bustamante, and Novak (2017) findings that the DCM reduced annual OOP health-care expenditures by 18%–21% and reduced the percentage of total medical expenditures paid OOP by 3.7 percentage points. We find larger-sized effects for our expenditure measures than both of these studies because we use a tighter age bandwidth to define our treatment and control groups and we include an additional year of data in the post-ACA DCM period.[23] Overall, the results in Figure 2 demonstrate a broad pattern of reduction in medical expenditure risk for young adults after the implementation of the ACA's dependent coverage mandate.

THE EFFECT OF THE DCM ON FINANCIAL DISTRESS

Main Results

To identify the effect of the DCM on financial distress of young adults, we adjust the event study model we specified in Equation (1) and form a new DID estimating equation:

$$y_{it} = \beta_0 + \lambda Treatment_i \times \gamma_t + \beta_1 Treatment_i + \gamma_t + \mu_i + X_{it}\Omega + \epsilon_{it}. \qquad (2)$$

Because of the panel data structure of the CCP, we follow the same methodology we outlined in the MEPS analysis section and define the $Treatment$ dummy variable so that it is equal to one, if a YA is between the ages of 23 and 25, equal to zero if a YA is between the ages of 27 and 29, and is coded as missing for treated individuals in the years after they become ineligible for the DCM. Because the CCP data do not contain any demographic information other than an individual's year of birth and census geography of residence, we include individual fixed effects μ_i in Equation (2), along with an alternative set of control variables in the vector X_{it} that include state and state-by-year fixed effects, age fixed effects, and county-level median income, unemployment rate, and percent of population at or below the federal poverty level. We cluster standard errors at the state level since we believe geographic variation plays an important role in the likelihood that a given individual is affected by the implementation of the mandate (Abadie, Athey, Imbens, and Wooldridge, 2017).[24]

Figure 3 plots the estimated effects of the DCM on the financial distress of young adults over the course of our sample. The three panels of Figure 3 show the effects for the probability of having debt in third-party collections, the number of accounts in third-party collections, and the

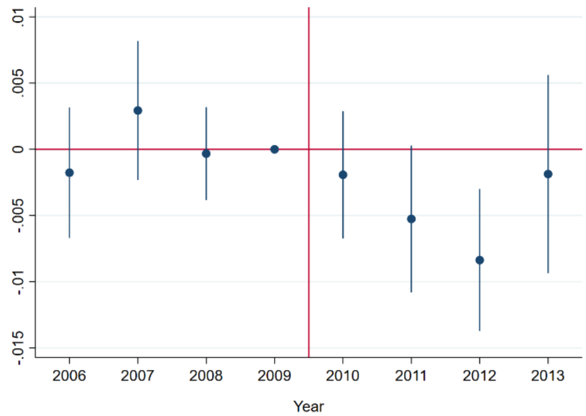
[23] Chua and Sommers (2014) use MEPS data from 2002 to 2011, while Chen et al. (2017) use MEPS data from 2008–2012.

[24] As a robustness check, we use two other standard errors methods when estimating Equation (2): heteroskedasticity robust standard errors and standard errors clustered at the individual level. Results from either approach are very similar to those reported in Figure 3 and **Table 3**.

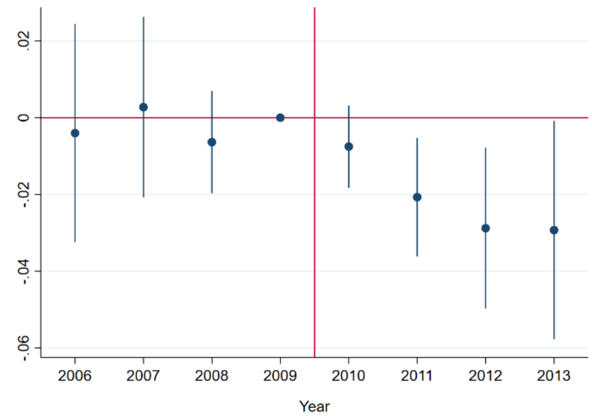
amount in third-party collections. For all these variables, the trends in the treatment and control groups appear parallel, prior to the law's implementation in 2010.

Figure 3: Financial Distress Event Study Results

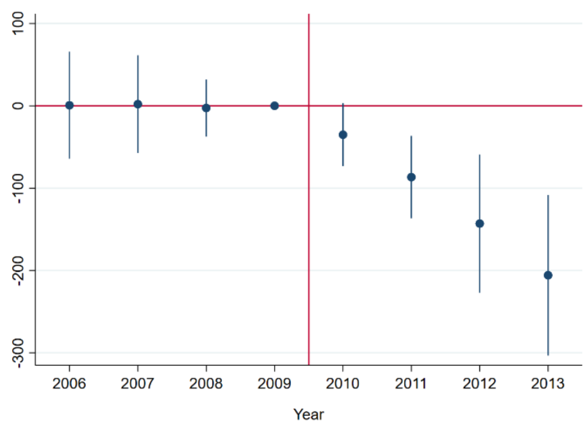
Panel A: Probability of a third-party collection



Panel B: Number of third-party collections



Panel C: Amount in third-party collections



Note: Authors' calculations using data from Federal Reserve Bank of New York Consumer Credit Panel/Equifax Data. Results in Panel A are measured in percentages, while Panel C is measured in dollars. Lines represent 95% confidence intervals.

The results in this figure show that for DCM-eligible young adults, the probability of third-party collections, the number of collections, and the amount in collections all declined relative to individual in the control group after the law was implemented. In particular, we see that in the postmandate period, the probability of having any accounts in collections declines by 0.2–0.8 percentage points (1%–4% relative to the pre-ACA mean), the number of third-party collections declines by 0.007–0.03 accounts (0.8%–5%), and the amount of debt in third-party collections declines by $35–$200 (5%–30%). Generally, effect sizes for all three variables grow in magnitude over time, except for the probability of a third-party collection in 2013, which is consistent with other studies that have shown that young adults' insurance enrollment increased over time after the DCM took effect (Akosa Antwi et al., 2013). These patterns are also consistent with the idea that the insurance effect on financial distress compounds over time as consumers are protected from unexpected medical expenses each year they are covered by health insurance.

In addition to the event-study estimates in Figure 3, we provide standard DID coefficients in **Table 3** to summarize the effects across the entire postmandate period. To estimate these effects, we modify Equation (2) by replacing the vector of year dummy variables γ_t with a single $Post_t$ indicator variable that is equal to 1 for the years 2011–2013. The estimates in Table 3 show that all of our measures of financial distress declined as a result of the dependent coverage mandate. In the post-DCM period, we find that eligible young adults experienced a 0.5 percentage point reduction in the probability of debt in third-party collections (2% decline relative to the pre-DCM sample average), a 3% decline in the number of accounts in third-party collections (decrease of 0.016 accounts), and a 9% decline in the amount in collections ($59).

Table 3: The Effect of the Dependent Coverage Mandate on Financial Distress

	(1) Probability of 3rd-Party Collections	(2) Number of 3rd-Party Collections	(3) Amount in 3rd-Party Collections
$Treatment \times Post\ DCM$	-0.004*	-0.016***	-58.87**
	(0.002)	(0.005)	(20.61)
R^2	0.58	0.58	0.39
N	3,167,262	3,167,262	1,413,351

Notes: Authors' calculations using data from the CCP. Standard errors clustered at the state level reported in parentheses. $Post_t = 1$ for the years 2011–2013. ***, **, * - denote significance at the 1%, 5%, and 10% level, respectively.

We use these estimates of the ITT effects of the mandate to calculate the implied treatment-on-the-treated effects. To do this, we divide the ITT effects by the change in the uninsured rate that resulted from the passage of the DCM, which we estimate to be 5.4% from the MEPS data.[25] Using this estimated decline in the uninsured rate, we estimate that treated individuals saw a reduction in accounts in collections of $\frac{0.015}{0.054} = 0.3$ accounts and a reduction in the amount of debt in collections of $\frac{59}{0.054} = \$1{,}092$ in the postmandate period from 2011 to 2013. While these implied effects are large, they are in-line with estimates from previous studies that have focused on national policy changes. Hu et al. (2018) estimated that the passage of the ACA's Medicaid expansion reduced debt in collections by $1,140, while Brevoort et al. (2020) find that *medical* debt in collections declined by $1,231, implying that decline in overall third-party collections could be even larger. Studies that have focused on specific states have found smaller effects: For example, Finkelstein et al. (2012) documented a decline of $390 for individuals who received

[25] We calculate this number using Equation (1) and an indicator variable equal to one if any individual has any insurance coverage.

Medicaid coverage in the Oregon health-insurance experiment, while Miller et al. (2021) found that individuals who enrolled in Medicaid in Michigan saw a decline of $515.

As previously discussed in the CCP data section, we use data from the fourth quarter of each year. If there is seasonality in the credit bureau data because of the U.S. holiday shopping season, using the fourth quarter may overstate financial distress. To address this issue, we conduct a robustness check in which we use data from the third quarter of every year instead of the fourth quarter. Results from using the third quarter instead of the fourth quarter are quantitatively very similar.

As an additional robustness check, we use a different version of our data set and provide results using a triple-difference empirical strategy similar to that of Mazumder and Miller (2016). In this specification, we define our treatment and control groups by year of birth instead of by age and use the geographic variation of the young adult uninsured rate. Details of the alternate methodology and results are reported in **Appendix G**. Generally, we find similar results to those reported in Figure 3 and Table 3: After the DCM was passed, eligible young adults experienced lowered measures of financial distress.

Quantile Regressions

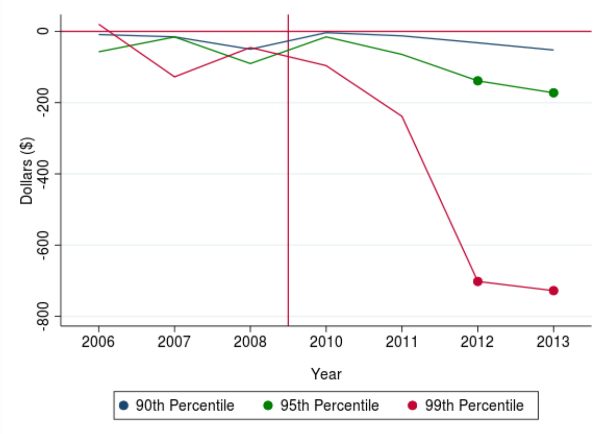
It is likely that the effects of access to health insurance on financial outcomes will vary over the distributions of these variables. In particular, given that the individuals most likely to be affected by the mandate are those who actually experience a health shock and incur high medical expenses, we hypothesize that reductions in financial distress will come from the far right tail of the distribution of debt in third-party collections. To test this hypothesis, we estimate a quantile regression event study model similar to the one specified in Dobkin et al. (2018), though we depart from their original specification and instead estimate a *conditional* quantile regression model. Our estimating equation takes the following form:

$$Y_{it} = \beta_0 + \theta_\rho Treatment_i \times \gamma_t + \beta_1 Treatment_i + \gamma_t + X_{it}\Omega + \epsilon_{it}, \tag{3}$$

where γ_t is a vector of dummy variables for years 2006–2013, while the $Treatment_i$ dummy variable and the control variables in X_{it} are the same as in Equation (2).[26] The coefficients of interest in Equation (3) are the θ_ρ's on the interactions of the calendar time dummy variables with the $Treatment_i$ variable. Specifically, we estimate θ_ρ at three different quantiles: $\rho = 90$, $\rho = 95$, and $\rho = 99$. Because we are estimating conditional quantile regression models, each θ_ρ will be interpreted as the change in debt balances at quantile ρ in time t relative to balances in 2009. Results from the regressions are presented graphically in **Figure 4**, with circles marking estimates statistically significant at 5% level.

[26] We exclude individual fixed effects from the quantile regressions specification.

Figure 4: Quantile Regression Event Study — Debt in Third-Party Collections



Note: Statistically significant (at 5%) coefficients are marked with circles. Authors' calculations using data from the CCP.

As can be seen in Figure 4, changes in debt in third-party collections are not statistically significant in the years after the DCM at the 90th percentile but are significant at both the 95th and 99th percentiles. We estimate that in 2012 and 2013, the second and third full years after the DCM was implemented, debt in collection at the 90th percentile declined by \$150–\$175 and by approximately \$700 at the 99th percentile. These results imply that debt amounts in the far right tail of the distribution of third-party collections were greatly reduced after the DCM's implementation. This finding is consistent with health insurance reducing the risk of extreme financial distress from extraordinary medical expenses.

Heterogeneous Effects by Geography

As shown in Appendix A, there is substantial geographic heterogeneity in young adult uninsured rates prior to the DCM's implementation. We exploit this premandate variation in the uninsured rates to identify individuals who may have been the most likely to benefit from the health-insurance expansion under the DCM. We follow Mazumder and Miller (2016) and hypothesize that individuals living in counties with high premandate uninsured rates are more likely to be affected by the passage of the mandate (i.e., are more likely to be uninsured and subsequently receive insurance after the passage of the mandate) compared with individuals in areas with low premandate uninsured rates. Thus, the effects of the mandate for individuals residing in high uninsured-rate areas should be larger than the effects for consumers in low uninsured-rate areas.[27] To test this hypothesis, we modify the DID empirical strategy used in Table 3 and split the sample based on the individual's county-level uninsured rate in 2008–2009.[28]

[27] We note that unlike in Mazumder and Miller (2016), using the uninsured rate in our context is an imperfect way to measure exposure to the DCM because in order to benefit from the DCM expansion, uninsured YAs also need to have their parents covered by private insurance plans. Unfortunately, the SAHIE data do not contain information on the insurance status of other family members. Despite this fact, our exposure measure should bias our heterogeneity analysis toward finding no difference between the two groups because we may include counties in the high-exposure group that should be in the low-exposure group.

[28] We use the SAHIE median uninsured rate in 2008–2009, which is equal to 25%.

Table 4: The Effect of the DCM on Financial Distress, by County Uninsured Rate

	(1) Probability of 3rd-Party Collections	(2) Number of 3rd-Party Collections	(3) Amount in 3rd-Party Collections
Above the median premandate county uninsured rate			
$Treatment \times Post\ DCM$	-0.008*	-0.03***	-83.81**
	(0.003)	(0.007)	(28.94)
R^2	0.58	0.58	0.39
N	1,617,545	1,617,545	817,348
Below the median premandate county uninsured rate			
$Treatment \times Post\ DCM$	-0.001	-0.004	-28.73
	(0.003)	(0.007)	(26.56)
R^2	0.59	0.59	0.40
N	1,493,821	1,493,821	574,099

Notes: Authors' calculations using data from the CCP. Standard errors reported in parentheses. Uninsured rate is for young adults ages 19–39 using the census' SAHIE data for 2008–2009. All regressions include individual, state, and state by year fixed effects. $Post_t = 1$ for the years 2011–2013. ***, **, * - denote significance at the 1%, 5%, and 10% level, respectively.

Results for the regressions on the high-uninsured and low-uninsured samples are presented in **Table 4**, with individuals living in high-uninsured areas in the top panel and individuals living in low-uninsured areas in the bottom panel. Our estimates show that declines in financial distress are almost entirely concentrated among individuals living in the *high* premandate uninsured counties. In particular, these individuals experienced statistically significant reductions in the probability and the number of accounts in third-party collections and in the amount of debt in collections. Unsurprisingly, the magnitude of the declines are all larger than those reported for the entire population in Table 3.

Point estimates in the bottom panel of Table 4 show that individuals living in low-uninsured areas also experienced reduced financial distress, but these declines are smaller than those reported in the top panel of Table 4 and are not statistically significant. Overall, these results are consistent with our hypothesis that individuals living in high YA–uninsured areas were more likely to be impacted by the mandate. Our results are also consistent with other research that shows that health insurance plays an important role in the geography of financial health (Goldsmith-Pinkham, Pinkovskiy, and Wallace, 2020).

THE EFFECT OF LOSING INSURANCE: AGING-OUT ANALYSIS

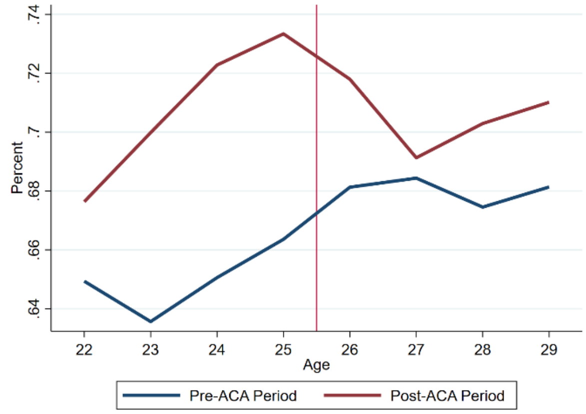
Unlike most other national health policies in the U.S., the ACA's DCM has a unique age-based cutoff in which YAs become automatically ineligible for dependent coverage after their 26th birthday. This implies that all YAs "age out" of eligibility to be enrolled on their parents' insurance plans at age 26 and must subsequently transition to enrolling in their own plans. Because of this aging out mechanism, the DCM provides us with a unique setting to also study the financial effects of *losing* health-insurance eligibility, as prior studies of the mandate have shown that the uninsured rate for YAs increases immediately after turning 26 (Yoruk, 2018) and that YAs who transition to their own plans have less generous coverage (Dahlen, 2015).

To examine the financial effects of losing eligibility for dependent coverage, we depart from our previous analytical framework. Instead of focusing on treatment and control groups of similar YAs, we are now interested in examining how financial distress changes from age to age (i.e., age dynamics) for YAs in their 20s. This strategy allows us to flexibly examine how financial distress changes from one age to the next without placing any parametric assumptions on the relationship between financial distress and age. We then trace out these age dynamics for both the pre- and post-DCM periods and estimate a linear regression, interacting age dummy variables with a post-DCM indicator to see if the age dynamics in the two periods are statistically different. Absent the DCM, we would expect that the differences in financial distress between each age would have been consistent across the two time periods. Although this analysis is not a classic DID, examining the age dynamics of financial distress around the age 26 cutoff before and after the DCM's implementation allows us to estimate the effects of losing parental health insurance. Our analyses in the previous sections do not allow us to directly estimate these effects.[29]

To motivate this analysis, we first show how insurance coverage changes for YAs in their 20s in both the pre- and post-DCM periods using MEPS data. **Figure 5** plots the percent of young adults with health insurance by age before and after the ACA.

[29] For the age-out analysis, in the post-DCM period, all YAs are either eligible for the DCM or were previously eligible for the DCM and subsequently aged out. In our previous analysis of the passage of the DCM, our empirical design compares YAs who are currently eligible for the DCM (between the ages of 23 and 25) and YAs who are not eligible for the DCM (between the ages of 27 and 29). We also drop observations of YAs who were previously eligible for the DCM, but subsequently age out of the DCM. Therefore, by construction, the previous analysis does not include observations in which a previously eligible YA ages out of the DCM. Because of this, we cannot use this empirical framework to study the aging-out effect.

Figure 5: Percent of Individuals Having Insurance by Age; Pre/Post-ACA



Notes: Based on authors' calculations using MEPS data using sample weights. Pre-ACA is defined as the years 2005–2009, and post-ACA is defined as the years 2011–2015.

From the figure, it is clear that the insured rate for young adults is higher at all ages in the post-DCM years compared with the pre-DCM years, even with the dip at age 26. This is consistent with the prior literature that the DCM was successful in improving insurance coverage for YAs. When examining the years prior to the implementation of the DCM, we do not observe any changes in the probability of being insured as YAs turn age 26. However, in the years after the DCM was implemented, we observe a strong dip in the insured rate as YAs age out of the mandate, declining from 73.3% at age 25, to 69.1% by age 27. Because YAs become uninsured or have less-generous insurance coverage after aging out of the mandate, this change in insurance status likely results in higher OOP medical expenses, which in turn may lead to the reversal of the improvements in financial outcomes we document for individuals who received coverage.

MEPS Age-Out Analysis

To examine if YAs pay higher OOP medical expenses after turning 26, we first plot the averages of our expenditure variable for each age, which will allow us to examine the evolution of OOP expenditures across ages before and after ACA's implementation. To do this, we expand the data set used in the previous MEPS analysis to include information from 2005 to 2006 and from 2014 to 2015. Including these additional years of data allows us to sample more individuals who turn each age in each of the pre- and post-DCM periods, which is necessary for us to properly estimate the age dynamics for each period.[30] An important caveat of expanding the sample is that we now include the 2014 ACA Medicaid expansion, which may lead us to observe smaller effects of aging out of the DCM in 2014 and 2015 than in the years prior to 2014. Because we include the years 2005 and 2006, we use the net OOP expenditure variable described in the MEPS data section and in **Appendix C**.

[30] Restricting the years in the sample reduces the number of birth year cohorts used to calculate age effects for each time period. For example, the only individuals that turn 25 in the pre-ACA period for a sample of data from 2007 to 2009 would have been born in 1982, 1983, or 1984.

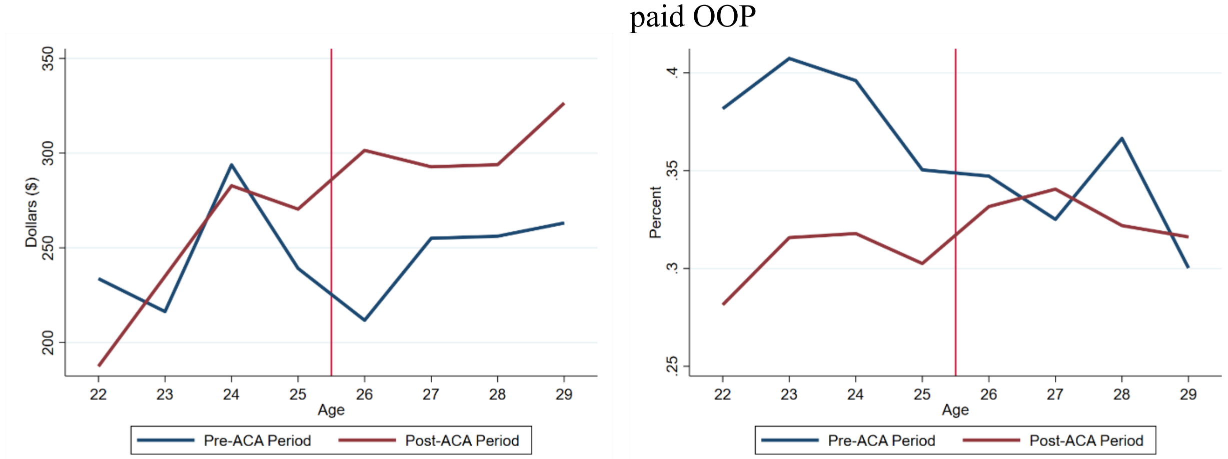
Panel A of **Figure 6** shows the raw age trends in net OOP expenditures in the pre- and post-DCM years from 2005 to 2015, and Panel B shows the percentage of net medical expenditures paid OOP. In the pre-DCM years (2005–2009), net OOP medical spending increases from ages 22 through 24, declines through age 26, and then rebounds slightly through age 29. In the post-DCM period, net OOP expenditures follow the pre-DCM period age dynamic through age 24, but *increase* at age 26 and stay elevated through age 29. This increase at age 26 in the post-DCM period suggests that changes to young adult health-insurance coverage at the DCM's age cutoff may result in higher net OOP medical expenses. Interestingly, there is little difference in the levels of net OOP medical expenditures for ages 22–25 between the two periods.

In Panel B of Figure 6, we show trends in the percentage of total net medical expenditures that are paid OOP in the pre- and post-DCM periods. For ages under 26, it can be clearly seen that in the post-DCM period, young individuals pay a significantly lower percentage of their net medical expenses OOP compared with their similarly aged counterparts in the pre-DCM period. For ages 26 and older, we see the percentage of medical costs paid OOP are very similar across the two periods. For YAs in the post-DCM period, the percentage of expenses paid OOP are actually *higher* as individuals turn age 26; in the pre-DCM period, the percent of expenses paid OOP generally fall as individuals get older. That this spike in the percentage of expenses paid OOP at age 26 is only present in the post-DCM period is additional suggestive evidence that aging out of the DCM may result in additional net OOP expenses.

Figure 6: Differences in Net OOP Medical Spending by Age; Pre/Post-ACA

Panel A: Net OOP medical expenditures

Panel B: Percent of net medical expenditures paid OOP



Notes: Based on authors' calculations using MEPS data from 2005 to 2015. All calculations made using sample weights. Net OOP expenditures calculated by subtracting OOP prescription drug expenditures from total OOP expenditures. *Pre-ACA* is defined as the years 2005–2009, and *post-ACA* is defined as the years 2011–2015. Figures for all OOP expenditures are presented in **Appendix H**.

To more rigorously test if these age-out effects are unique to the post-DCM period, we estimate a regression model in which we interact each age dummy variable with a dummy variable equal to one for the years after the passage of the ACA in 2010. This allows us to exploit variation in the effects of each age across time to examine if differences in age dynamics between the pre-DCM and post-DCM periods are statistically different from each other. In other words, our specification allows us to compare how OOP expenses change across ages over the two time periods. Because the medical expenditure variables in the MEPS are for the entire year, we exclude the year 2010 because we are not able to distinguish what fraction of the medical expenditures were incurred prior to the implementation of the ACA's DCM and which were incurred afterward. Our estimating equation takes the following form:

$$y_{it} = \beta_0 + \sum_{age=22}^{29} \alpha_{age} D_{age} \times PostACA + \sum_{age=22}^{29} \nu_{age} D_{age} + \beta_1 PostACA + X_{it}\Lambda + \epsilon_{it}. \quad (4)$$

We can interpret each coefficient α_{age} as the difference in being a specific age before the DCM went into effect and being of this age after the DCM was in effect. Since the reference group in our estimating equation consists of individuals at age 25 in the pre-DCM period, our coefficient estimates are relative to a YA not eligible for parental coverage in the pre-DCM period.

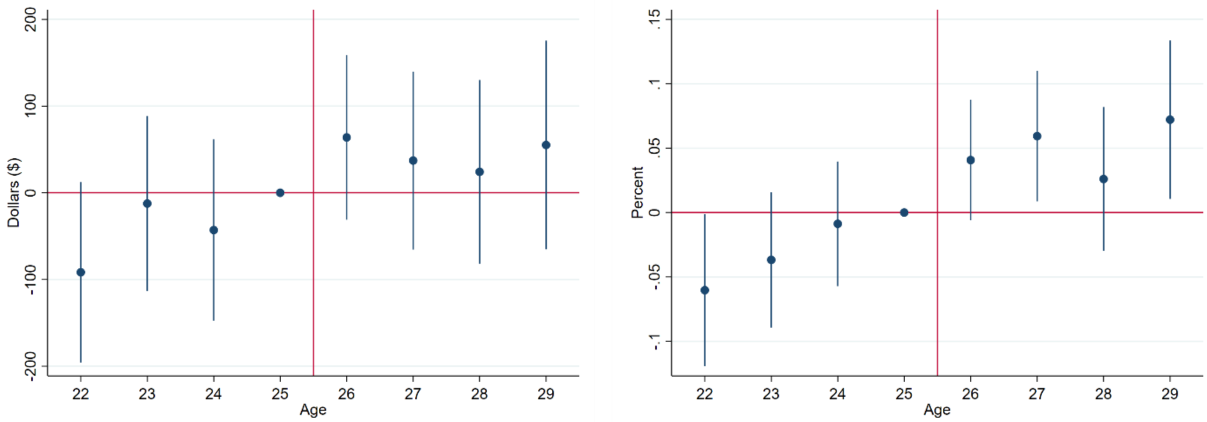
If the DCM has no effect on age dynamics, we would expect to see all coefficient estimates of α_{age} to be close to zero, which would indicate that the effect of being a specific age did not change from the pre-DCM period to the post-DCM period. However, given that our previous analyses have shown that the DCM lowered OOP expenses, we should expect to see differences in the age dynamics. For ages 22 to 25, we should see negative α_{age} coefficients for these ages, which indicate that YAs in the post-DCM period have lower OOP medical spending than YAs of the same age in the pre-DCM period.

For ages 26–29, the summary statistics reported in Figure 6 strongly suggest that we should expect to see differences in age dynamics for these older ages. In contrast to the younger ages, we should see *positive* estimates for α_{age}, indicating that the effects for each age are higher in the post-DCM period relative to the pre-DCM period.

Figure 7: Differences in Net OOP Medical Spending by Age; Pre/Post-ACA

Panel A: Net OOP medical expenditures

Panel B: Percent of net medical expenditures paid OOP



Notes: Based on authors' calculations using MEPS data. Includes years 2005–2009 and 2011–2015. Age 25 is the omitted category. Net OOP expenditures are calculated by subtracting OOP prescription drug expenditures from total OOP expenditures. Lines represent 95% confidence intervals.

Figure 7 shows that both net OOP medical expenditures and the percentage of total net medical expenditures that are paid OOP increase for individuals at age 26 and older in the postmandate period. Panel A shows that young adults who turn age 26 in the post-ACA DCM period have approximately $70 more in net OOP medical expenditures than individuals who turn age 26 in the pre-ACA DCM period, though this increase is not statistically significant. We also observe that net OOP spending is lower for younger ages in the post-DCM period, which we would expect if the DCM reduces exposure to net OOP medical expenditure for eligible individuals. Results for total OOP expenditures, reported in Panel A of **Appendix I**, show similar age dynamics to Figure 7, though the increases in expenditures are larger ($110 versus $70) and statistically significant at age 26.

For the percent of total net medical expenses that are paid OOP, we observe that YAs at age 26 and older in the post-ACA DCM period pay approximately 4.5 percentage points more OOP than YAs at age 26 and older in the pre-ACA DCM period. Similar to the net OOP expenditure results, we also observe negative coefficients for the younger ages, indicating that individuals under age 26 in the post-DCM period pay a lower percentage of their total net medical expenditure OOP than similarly aged individuals in the pre-DCM period. As was the case for total OOP expenditures, results for the percent of total expenditures paid OOP (shown in Panel B of Appendix I) exhibit similar age dynamics to our net expenditure measure but are larger in magnitude for the older ages. These results together imply that YAs who turn 26 and age out of the DCM incur more OOP expenses and pay a higher percentage of their medical expenses OOP compared with individuals turning age 26 in the pre-DCM period. In other words, the effects after age 26 are unique to the postmandate period and are not indicative of a persistent change in medical expenditures at age 26 across time.

CCP Age-Out Analysis

We now examine how financial distress changes after individuals age out of the mandate by examining how the age dynamics for financial distress changed in the post-DCM period. Our previous MEPS results, combined with the findings in Batty et al. (2018), who show that medical bills in collections peak at age 27, suggest that we may expect to see increased financial distress as individuals lose insurance eligibility at age 26.

To estimate the age effects on our measures of financial distress, we make two important adjustments to our original CCP sample: (1) we extend the sample frame to include data from 2004 to 2016, and (2) we include individuals born from 1974 to 1995. Similar to the MEPS age-out analysis, we extend the sample in these ways to ensure that there are enough birth-year cohorts that turn each age before and after the ACA goes into effect to properly examine the changes in the age dynamics.[31] Using this updated sample, we estimate a model very similar to Equation (4) by interacting a vector of age dummy variables with a dummy variable that is equal to one for the years in the post-DCM period:

$$y_{it} = \beta_0 + \sum_{age=22}^{29} \alpha_{age} D_{age} \times PostACA + \sum_{age=22}^{29} \nu_{age} D_{age} + \beta_1 PostACA + \mu_i + X_{it}\Omega + \epsilon_{it}.$$
(5)

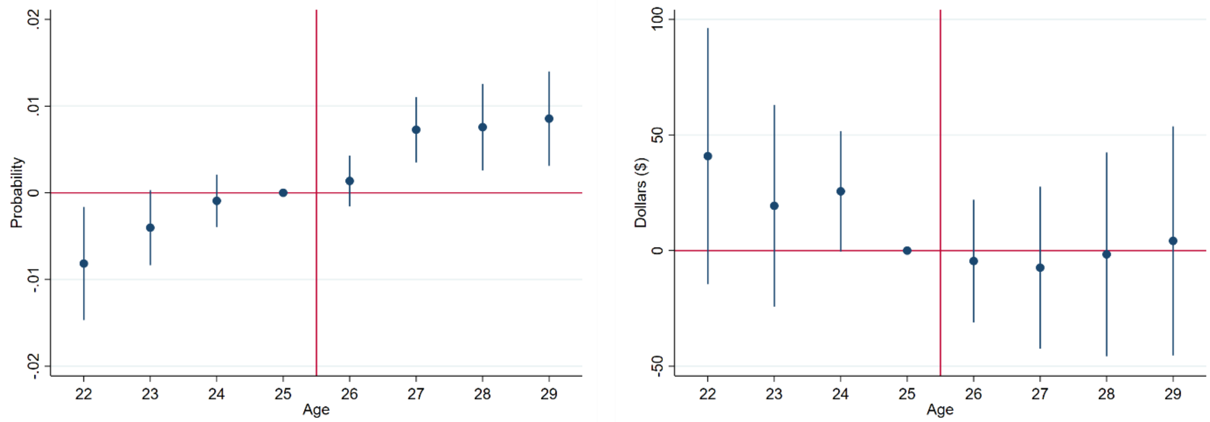
Along with individual fixed effects, we include age, state, year, and state by year fixed effects in our model. We also include a second-order polynomial in birth year and controls for the cohorts of young adults who potentially would have been affected by the Credit Card Accountability Responsibility and Disclosure (CARD) Act. Similar to Equation (4) for the previous MEPS analysis, this model allows us to examine how the age dynamics of financial distress changed from the pre- to post-DCM period. Our coefficients of interest are the α_{age}'s, which indicate if individuals of a specific age in the post-ACA DCM period have more debt in collections than young adults of the same age in the pre-ACA DCM period. Our results for two measures of debt in collections are displayed in **Figure 8**.

[31] For example, to estimate the age 26 dummy in the pre-ACA period 2004–2009, we need to include individuals born in 1983 (26 in 2009), 1982 (26 in 2008), 1981 (26 in 2007), and 1980 (26 in 2006).

Figure 8: Differences in Debt in Collections by Age; Pre/Post-ACA

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

Panel B: Amount in third-party collections



Notes: Authors' calculations using data from the CCP. Includes years 2004–2009 and 2011–2016.

Panel A of Figure 8 shows that the probability of having an account in collections from ages 27 to 29 is approximately 1 percentage point (a relative 4.4%) higher for YAs in the post-ACA period compared with 27- to 29-year-olds in the pre-ACA period. This result indicates that upon aging of the mandate (in the post-DCM period), YAs are more likely to have a collections account on their credit report. We also see that YAs under age 26 in the post-ACA period have a lower probability of having an account in collections relative to similarly aged individuals in the pre-ACA period. This result is consistent with our MEPS results and provides additional evidence that the DCM improves financial health for YAs eligible for insurance coverage.

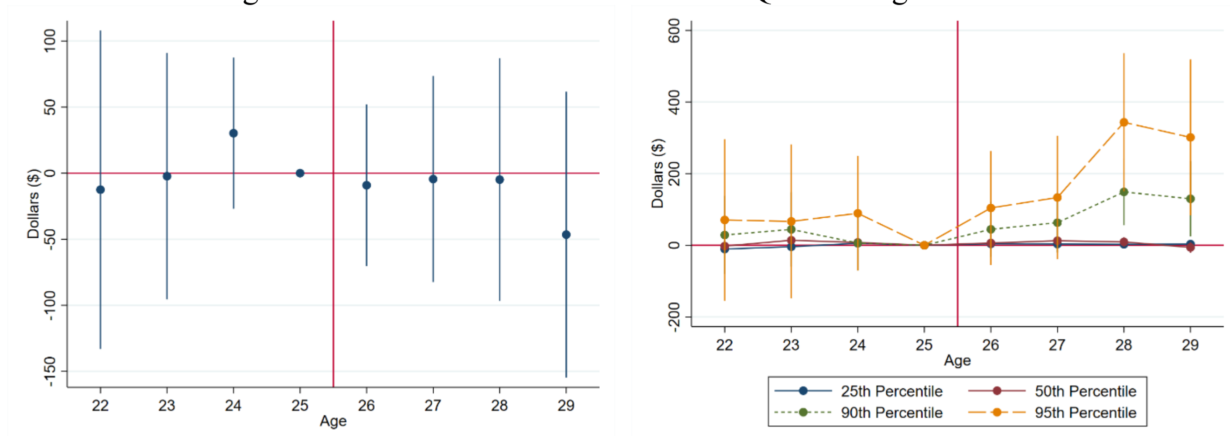
In Panel B of Figure 8, we see little effect of aging out on the total amount of debt in third-party collections. This result is surprising, as our result in Panel A of Figure 8 shows that older YAs in the post-ACA period have a higher probability of having an account in collections and more overall accounts in collections upon losing access to parental health insurance via the DCM (shown in **Appendix J**). One possible explanation could be that more YAs report owing $0 of debt in collections, which would lead to the average amount of debt in collections being closer to $0. To examine how positive collection amounts change after individuals age out of the DCM, we reestimate Equation (5), but we restrict our sample to only include observations with a positive amount of debt in collections. Results from this regression are in Panel A of **Figure 9**. As can be seen, even when restricting the sample to positive balances, we still find no changes in debt in collections for older YAs when they age out of the mandate.

A more likely explanation for this null result for collection amounts is that the effect of aging out of the mandate on collections debt varies over the collections distribution. For example, YAs who maintain insurance coverage after aging out but transition to a plan of worse quality than their parents' may have relatively small bills sent to collections, while a YA who becomes uninsured after aging out is at a higher risk of having large bills sent to a debt collector. To test this hypothesis, we follow the empirical framework we outlined in Equation (3) in the heterogeneity section and estimate a series of conditional quantile regression models at different points in the debt-in-collections distribution. Similar to our analysis in Panel A of Figure 9, we restrict our sample to observations with positive collection balances. Regression results for the 25th, 50th, 90th, and 95th percentiles are presented in Panel B of Figure 9.

Figure 9: Distributional Changes in Debt in Third-Party Collections; Pre/Post-ACA

Panel A: Linear regression

Panel B: Quantile regressions



Notes: Samples for both figures are restricted to individuals with positive amounts of debt in third-party collections. Authors' calculations using data from the CCP; includes years 2004–2009 and 2011–2016.

For balances in the left tail and middle of the collections distribution, we see no changes in the age dynamics from the pre-DCM to the post-DCM period. However, for collection balances at the 95th and 99th percentiles, debt in collections increases for ages 27–29 in the post-DCM period relative to the pre-DCM period by \$60 to \$150 at the 95th percentile and by \$100 to \$340 at the 99th percentile. In other words, the age dynamics for smaller balances did not change across the two periods, but the age dynamics for larger balances did change after the DCM was implemented. These results are consistent with the nonsignificant *average* effect on collections balances we found in Panel B of Figure 8: The null effect for the majority of the distribution from ages 26 to 29 counteracts the large increases at the 95th and 99th percentiles, resulting in the imprecise zero average result. These results also make sense in the context of how insurance coverage changes when YAs turn 26. Because most individuals remain insured after aging out, although some do so with worse quality coverage, we should expect to see little to no increase in the amount of debt in collections for the majority of individuals; rather, since only a small fraction of individuals are uninsured after aging out of the mandate, and subsequently face elevated risk of incurring catastrophic medical expenditures, we should expect to see increases in the far right tail of the collections distribution.

DISCUSSION AND CONCLUSION

The results from this analysis contribute to the growing body of studies that leverage consumer credit and medical expenditure data sets to analyze the effects of health-insurance policy on financial outcomes. Using the implementation of the ACA's DCM in 2010 and its automatic disenrollment mechanism at age 26 to identify the effects of health insurance on young adults, we find that the increased access to health insurance reduced financial distress for young adult dependents. In particular, we observe declines in the probability of having debt in collections, the number of accounts and balance in collections, the amount of OOP medical expenses, and in the probability of incurring very large medical expenditures. We also find that individuals living in areas with high uninsured rates prior to the enactment of the mandate experienced greater declines in financial distress than those who lived in low uninsured-rate counties. These results suggest that the mandate was effective in geographic areas with the highest percentage of individuals likely to be affected.

Our results in the second half of the paper suggest that financial distress increases once individuals age out of the DCM after age 26, with the probability of having debt in collections and the size of large collection balances both increasing. These increases are not surprising given that many young adults either transition to lower quality plans or become uninsured after losing access to parental insurance. That financial distress worsens upon losing parental insurance coverage indicates that young adults (1) receive material financial protections from health insurance, although they are relatively healthy and use health-care services at lower rates, and (2) did not receive the same amount of financial protection when transitioning from the DCM to either their own individual health-insurance plans or losing their coverage entirely. In addition, the magnitude of the effects we estimate imply that the financial effects of gaining and losing access to health insurance for young adults are not symmetric. These findings also suggest that the quality of health insurance plays an important role in the financial protection of covered individuals.

Our estimates provide additional context when evaluating the welfare aspects of the ACA's DCM. Specifically, we are able to evaluate the effect of a *private* health-insurance expansion on financial distress, which represents an important contribution to 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%–2.7% after the DCM 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 that employers passed on the costs through other means, such as wage reductions, instead of increased insurance contributions. If this is the case, this would decrease the total welfare benefit of the DCM.

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. (2020) 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. Finally, since these young adults are at the beginning stages of the life cycle, reducing current financial burdens and/or the probability of incurring large amounts of medical debt may have significant long-run implications. Our results provide an important first step in understanding these dynamics by empirically identifying these effects.

The results of this analysis also 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 OOP 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 outcomes of individuals, not just measures of physical health and access to health care.

References

Abadie, A., S. Athey, G. W. Imbens, and J. Wooldridge. (2017). "When Should You Adjust Standard Errors for Clustering?" National Bureau of Economic Research Working Paper 24003.

Akosa Antwi, Y., A. S. Moriya, and K. I. Simon. (2013). "Effects of Federal Policy to Insure Young Adults: Evidence from the 2010 Affordable Care Act's Dependent-Coverage Mandate." *American Economic Journal: Economic Policy* 5(4), 1–28, https://doi.org/10.1257/pol.5.4.1.

Akosa Antwi, Y., A. S. Moriya, and K. I. Simon. (2015). "Access to Health Insurance and the Use of Inpatient Medical Care: Evidence from the Affordable Care Act Young Adult Mandate." *Journal of Health Economics* 39, 171–187, https://doi.org/10.1016/j.jhealeco.2014.11.007.

Anderson, M. L., C. Dobkin, and T. Gross. (2012). "The Effect of Health Insurance Coverage on the Use of Medical Services." *American Economic Journal: Economic Policy* 4(1), 1–27.

Anderson, M. L., C. Dobkin, and T. Gross. (2014). "The Effect of Health Insurance on Emergency Department Visits: Evidence from an Age-Based Eligibility Threshold." *The Review of Economics and Statistics* 96(1), 189–195.

Argys, L.M., A. I. Friedson, M. M. Pitts, and D. S. Tello-Trillo. (2020). "Losing Public Health Insurance: TennCare Reform and Personal Financial Distress." *Journal of Public Economics* 187 (article 104202), https://doi.org/10.1016/j.jpubeco.2020.104202.

Ayyagari, P., and D. He. (2016). "The Role of Medical Expenditure Risk in Portfolio Allocation Decisions." *Health Economics* 26(11), 1447–1458, https://doi.org/10.1002/hec.3437.

Bailey, J. (2017). "Health Insurance and the Supply of Entrepreneurs: New Evidence from the Affordable Care Act." *Small Business Economics* 49(3), 627–646, https://doi.org/10.1007/s11187-017-9856-8.

Bailey, J., and A. Chorniy. (2016). "Employer-Provided Health Insurance and Job Mobility: Did the Affordable Care Act Reduce Job Lock?" *Contemporary Economic Policy* 34(1), 173–183, https://doi.org/10.1111/coep.12119.

Barbaresco, S., C. J. Courtemanche, and Y. Qi. (2015). "Impacts of the Affordable Care Act Dependent Coverage Provision on Health-Related Outcomes of Young Adults." *Journal of Health Economics* 40, 54–68, https://doi.org/10.1016/j.jhealeco.2014.12.004.

Barcellos, S. H., and M. Jacobson. (2015). "The Effects of Medicare on Medical Expenditure Risk and Financial Strain." *American Economic Journal: Economic Policy* 7(4), 41–70, https://doi.org/10.1257/pol.20140262.

Batty, M., C. Gibbs, and B. Ippolito. (2018). “Unlike Medical Spending, Medical Bills in Collections Decrease with Patients’ Age.” *Health Affairs* 37(8), 1257–1264, https://doi.org/10.1377/hlthaff.2018.0349.

Brevoort, K., P. Grimm, and M. Kambara. (2016). “Credit Invisibles and the Unscored.” *Cityscape* 18(2), 9–34.

Brevoort, K., D. Grodzicki, and M. Hackmann. (2020). “The Credit Consequences of Unpaid Medical Bills.” *Journal of Public Economics* 187 (article 104203), https://doi.org/10.1016/j.jpubeco.2020.104203.

Brown, M., J. Grigsby, W. van der Klaauw, J. Wen, and B. Zafar. (2016). “Financial Education and the Debt Behavior of the Young.” *The Review of Financial Studies* 29(9), 2490–2522, https://doi.org/10.1093/rfs/hhw006.

Cantor, J. C., D. Belloff, A. C. Monheit, and M. Koller. (2012a). “Expanding Dependent Coverage for Young Adults: Lessons from State Initiatives.” *Journal of Health Politics, Policy and Law* 37(1), 99–128, https://doi.org/10.1215/03616878-1496056.

Cantor, J. C., D. Belloff, A. C. Monheit, D. DeLia, and M. Koller. (2012b). “Early Impact of the Affordable Care Act on Health Insurance Coverage of Young Adults.” *Health Services Research* 47(5), 1773–1790, https://doi.org/10.1111/j.1475-6773.2012.01458.x.

Chen, J., A. Vargas-Bustamante, and P. Novak. (2017). “Reducing Young Adults’ Health Care Spending Through the ACA Expansion of Dependent Coverage.” *Health Services Research* 52(5), 1835–1857, https://doi.org/10.1111/1475-6773.12555.

Chua, K. P., and B. D. Sommers. (2014). “Changes in Health and Medical Spending Among Young Adults Under Health Reform.” *Journal of the American Medical Association* 311(23), 2427–2439, https://doi.org/10.1001/jama.2014.2202.

Collins, S. R., M. Z. Gunja, and S. Beutel. (2016, September). “New U.S. Census Data Show the Number of Uninsured Americans Dropped by 4 Million, with Young Adults Making Big Gains.” To the Point. (The Commonwealth Fund).

Cutler, D. M., A. Finkelstein, and K. McGarry. (2008). “Preference Heterogeneity and Insurance Markets: Explaining a Puzzle of Insurance.” *American Economic Review: Papers & Proceedings* 98(2), 157–162, http://www.aeaweb.org/articles.php?doi=10.1257/aer.98.2.157.

Dahlen, H. M. (2015). “‘Aging Out’ of Dependent Coverage and the Effect on US Labor Market and Health Insurance Choice.” *American Journal of Public Health* 105(S5), S640-S650, https://doi.org/10.2105/AJPH.2015.302791.

Debbaut, P., A. Ghent, and M. Kudlyak. (2016). “The CARD Act and Young Borrowers: The Effects and the Affected.” *Journal of Money, Credit and Banking* 48(7), 1495–1513, https://doi.org/10.1111/jmcb.12340.

Depew, B., and J. Bailey. (2015). "Did the Affordable Care Act's Dependent Coverage Mandate Increase Premiums?" *Journal of Health Economics* 41, 1–14, https://doi.org/10.1016/j.jhealeco.2015.01.004.

Dobkin, C., A. Finkelstein, R. Kluender, and M. Notowidigdo. (2018). "The Economic Consequences of Hospital Admissions." *American Economic Review* 108(2), 308–352.

Elul, R., N. Souleles, S. Chomsisengphet, D. Glennon, and R. Hunt. (2010). "What 'Triggers' Mortgage Default?" *American Economic Review* 100(2), 490–494.

Federal Register. 2010. "Group Health Plans and Health Insurance Issuers Providing Dependent Coverage of Children to Age 26 Under the Patient Protection and Affordable Care Act." 75(92), 27122–27140.

Finkelstein, A., S. Taubman, B. Wright, M. Bernstein, J. Gruber, J. P. Newhouse, H. Allen, K. Baicker, and Oregon Health Study Group. "The Oregon Health Insurance Experiment: Evidence from the First Year." *The Quarterly Journal of Economics* 127(3), 1057–1106, https://doi.org/10.1093/qje/qjs020.

Fox, S., M. Duggan, L. Rainie, and K. Purcell. (2013). *Pew Research Center, The Diagnosis Difference*.

Ghosh, A., and K. Simon. (2015). "The Effect of Medicaid on Adult Hospitalizations: Evidence from Tennessee's Medicaid Contraction." National Bureau of Economic Research Working Paper 21580.

Goldman, D., and N. Maestas. (2013). "Medical Expenditure Risk and Household Portfolio Choice." *Journal of Applied Econometrics* 28, 527–550, https://doi.org/10.1002/jae.2278.

Goldsmith-Pinkham, P., M. Pinkovskiy, and J. Wallace. (2020). "The Great Equalizer: Medicare and the Geography of Consumer Financial Strain." Working Paper 2102.02142, arXiv.org, retrieved from https://paulgp.github.io/papers/GPW_compressed.pdf.

Gross, T., and M. J. Notowidigdo. (2011). "Health Insurance and the Consumer Bankruptcy Decision: Evidence from the Expansions of Medicaid." *Journal of Public Economics* 95, 767–778, https://doi.org/10.1016/j.jpubeco.2011.01.012.

Gruber, J., and A. Yelowitz. (1999). "Public Health Insurance and Private Savings." *Journal of Political Economy* 107(6), 1249–1274, http://dx.doi.org/10.1086/250096.

Heim, B., I. Lurie, and K. Simon. (2018). "Did the Affordable Care Act's Young Adult Provision Affect Labor Market Outcomes: Analysis Using Tax Data." *Industrial and Labor Relations Review,* forthcoming, https://doi.org/10.1177/0019793917744176.

Hu, L., R. Kaestner, B. Mazumder, S. Miller, and A. Wong. (2018). "The Effect of the Affordable Care Act Medicaid Expansions on Financial Wellbeing." *Journal of Public Economics* 163, 99–112, https://doi.org/10.1016/j.jpubeco.2018.04.009.

Keys, B. K., N. Mahoney, and H. Yang. (2020). “What Determines Consumer Financial Distress? Place- and Person-Based Factors.” National Bureau of Economic Research Working Paper 26808.

Kotlikoff, L. J. (1989). *What Determines Savings?* Cambridge, Massachusetts: MIT Press.

Lee, D. (2016). “Effects of Dependent Coverage Mandate on Household Precautionary Savings: Evidence from the 2010 Affordable Care Act.” *Economics Letters* 147, 32–37, https://doi.org/10.1016/j.econlet.2016.08.002.

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

Levine, P. B., R. McKnight, and S. Heep. (2011). “How Effective Are Public Policies to Increase Health Insurance Coverage to Young Adults?” *American Economic Journal: Economic Policy* 3(1), 129–156, https://doi.org/10.1257/pol.3.1.129.

Mandell, L. (2008). *The Financial Literacy of Young American Adults: Results of the 2008 National Jump$tart Coalition Survey of High School Seniors and College Students*, available at www.jumpstart.org/assets/files/2008SurveyBook.pdf.

Mazumder, B., and S. Miller. (2016). “The Effects of the Massachusetts Health Reform on Household Financial Distress.” *American Economic Journal: Economic Policy* 8(3), 284–313, https://doi.org/10.1257/pol.20150045.

Miller, S., L. Hu, R. Kaestner, B. Mazumder, and A. Wong. (2021). “The ACA Medicaid Expansion in Michigan and Financial Health.” *Journal of Policy Analysis and Management* 40(2) 348–375, https://doi.org/10.1002/pam.22266.

Monheit, A. C., J. C. Cantor, D. DeLia, and D. Belloff. (2011). “How Have State Policies to Expand Dependent Coverage Affected the Health Insurance Status of Young Adults?” *Health Services Research* 46(1), 251–267, https://doi.org/10.1111/j.1475-6773.2010.01200.x.

Slusky, D. (2017). “Significant Placebo Results in Difference-in-Differences Analysis: The Case of the ACA’s Parental Mandate.” *Eastern Economic Journal* 43(4), 580–603, https://doi:10.1057/eej.2015.49.

Sommers, B. D., T. Buchmueller, S. L. Decker, C. Carey, and R. Kronick. (2013). “The Affordable Care Act Has Led to Significant Gains in Health Insurance and Access to Care for Young Adults.” *Health Affairs* 32(1), 165–174, https://doi.org/10.1377/hlthaff.2012.0552.

Substance Abuse and Mental Health Services Administration. (2019). “Key Substance Use and Mental Health Indicators in the United States: Results from the 2018 National Survey on Drug Use and Health.” *HHS Publication PEP19-5068, NSDUH Series H-54*. Rockville, MD: Center for Behavioral Health Statistics and Quality, Substance Abuse and Mental Health Services Administration. Retrieved from https://www.samhsa.gov/data/.

Tello-Trillo, D. S. (2021). “Losing Public Health Insurance on Preventative Care, Health, and Emergency Department Use: Evidence from the TennCare Disenrollment.” *Southern Economic Journal,* Forthcoming, https://doi.org/10.1002/soej.12504.

Willage, B. (2020). “Unintended Consequences of Health Insurance: Affordable Care Act’s Free Contraception Mandate and Risky Sex.” *Health Economics* 29(1), 30–45, https://doi.org/10.1002/hec.3967.

Yoruk, B. K. (2018). “Health Insurance Coverage and Health Care Utilization: Evidence from the Affordable Care Act’s Dependent Coverage Mandate.” *Forum for Health Economics & Policy* 21(2), https://doi.org/10.1515/fhep-2017-0032.

APPENDIX A

Figure A1: 2009 U.S. Uninsured Rates for Young Adults, by County

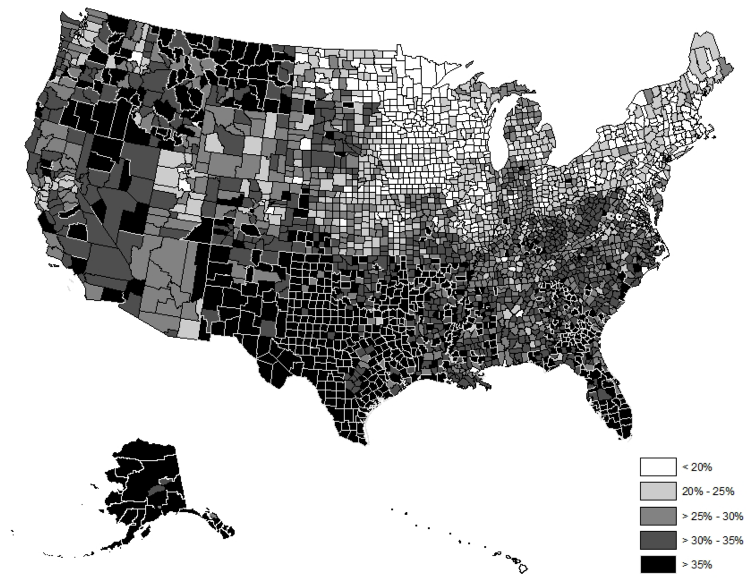
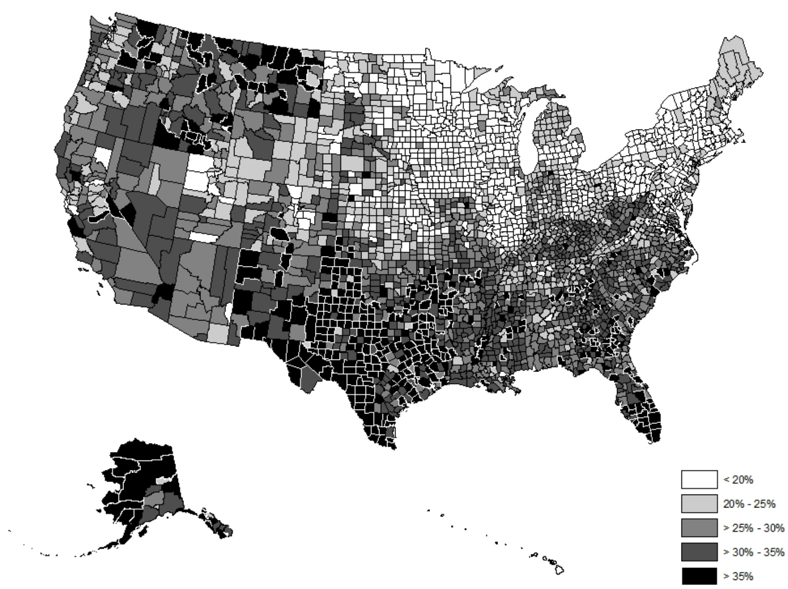


Figure A2: 2013 U.S. Uninsured Rates for Young Adults, by County



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

APPENDIX B

Table B1: Summary Statistics for MEPS Covariate: 2005–2015

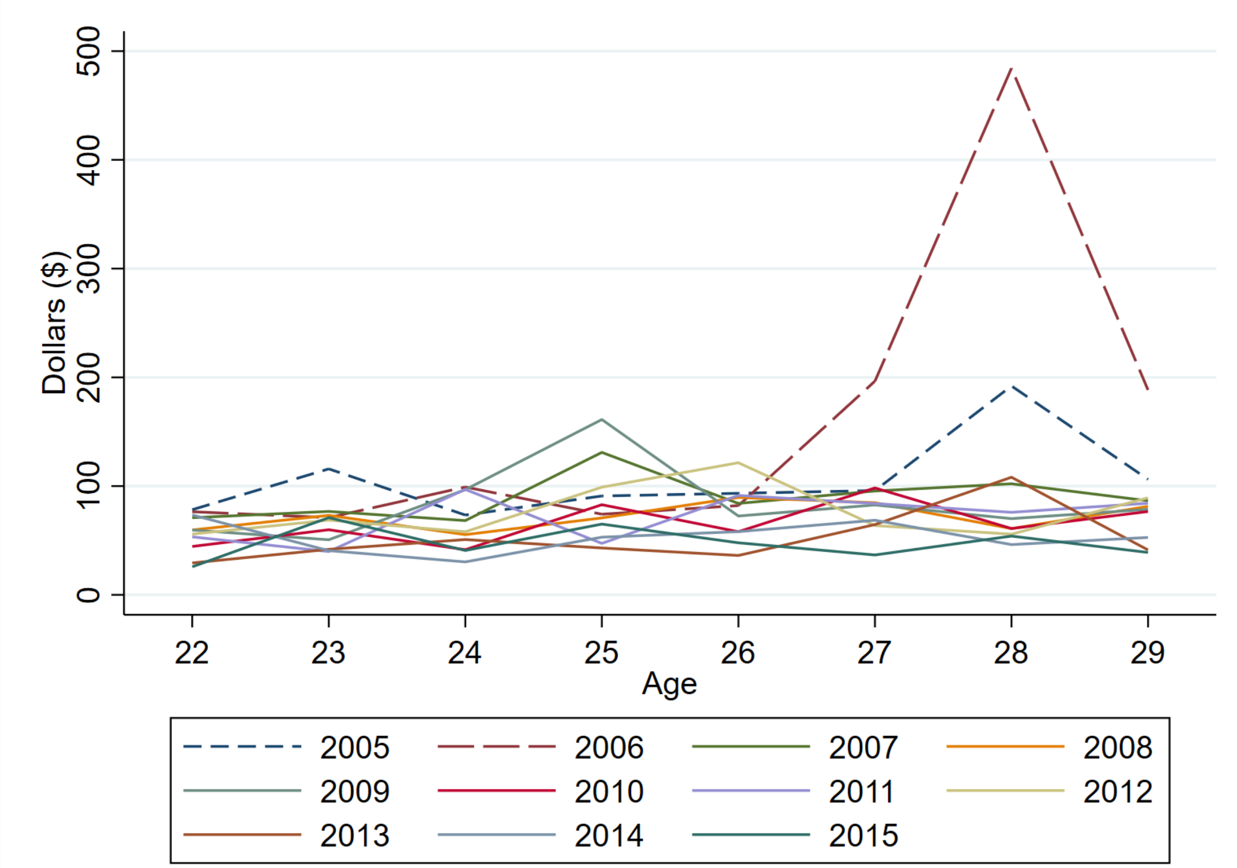
	Pre-ACA		Post-ACA	
Variable	Mean	Std. Dev.	Mean	Std. Dev.
Treatment Group				
% White	0.609	0.012	0.568	0.016
% Female	0.492	0.009	0.508	0.010
Family Size	2.73	0.038	2.79	0.047
Marital Status	0.244	0.01	0.194	0.009
Wage ($)	20,416.91	391.6	20,054.63	381.34
Control Group				
% White	0.599	0.012	0.575	0.014
% Female	0.517	0.009	0.507	0.008
Family Size	2.87	0.038	2.79	0.037
Marital Status	0.478	0.012	0.405	0.011
Wage ($)	28,402.59	637.82	30,841.34	587.71

Note: Authors' calculations using data from the Medical Expenditure Panel Survey (MEPS) from 2005–2009 and 2011–2015. Sample is limited to individuals ages 23–25 and ages 27–29. MEPS sample weights applied.

APPENDIX C: Alternate MEPS OOP Expenditure Definition

Because of a change in methodology of how expenditures on prescribed medicines were edited starting in 2007,[32] OOP prescription drug expenditures for individuals ages 27, 28, and 29 are unusually high relative to expenditures in later years. **Appendix Figure C1** shows the OOP prescription drug expenditure trends by age for each year of the MEPS data in our sample. As can be clearly seen, OOP prescription drug expenditures are 2 to 4 times higher for these ages in 2005 and 2006, compared with all other years in the sample.

Figure C1: OOP Prescription Drug Expenditures



Notes: Authors' calculations using data from the Medical Expenditure Panel Survey (MEPS) from 2005 to 2015. All calculations made using sample weights.

To address this issue, we create an alternate measure of our OOP medical expenditures by subtracting prescribed medicine OOP expenditures from overall OOP expenditures for each individual in our sample. Summary statistics for this "net" measure of OOP expenditures are provided in **Appendix Table C1**. While this new measure addresses data issues related to the methodology change for the years prior to 2007, prescription drug OOP expenditures are an important driver of OOP medical expenditures, making up 20%–30% of total yearly OOP

[32] For more details, see https://meps.ahrq.gov/data_files/publications/mr29/mr29.shtml.

expenditures. As mentioned previously, if the DCM affects prescription drug spending in a significant way, omitting these expenditures from overall OOP expenditures likely produces results that are less informative of the effects of the policy on overall OOP medical spending.

Table C1: MEPS Summary Statistics for Net Medical Expenditures, 2005–2015

	Overall	Pre-ACA (2005–2009)	Post-ACA (2011–2015)
Total Net OOP Medical Spending	253.33	241.93	264.94
	(6.84)	(8.65)	(10.35)
Total Net Medical Spending	1,618.49	1,397.45	1,862.66
	(44.66)	(38.24)	(79.18)
% of total net medical spending paid OOP†	0.339	0.365	0.313
	(0.004)	(0.005)	(0.005)
N	46,845	19,933	22,970
Fraction with OOP medical expenses	0.467	0.494	0.444

Notes: Authors' calculations using data from the Medical Expenditure Panel Survey (MEPS) from 2005 to 2015. Standard deviations reported in parentheses. All calculations made with sample weights. Expenditure variables deflated to 2010 dollars.

†: conditional on having any OOP expenses.

APPENDIX D: Coverage of Young Adults in the CCP Data

As mentioned in the Data section, previous work by Lee and van der Klaauw (2010) and Brown et al. (2016) has shown that the young adult population covered in the Federal Reserve Bank of New York Consumer Credit Panel/Equifax Data (CCP) is representative when compared with other measures of the young adult population. We conduct a more detailed comparison in the following section to provide additional evidence that CCP's coverage of young adults (YAs) is nationally representative and to show that the passage of the dependent coverage mandate did not change the rate at which individuals appear in the CCP.

To do this, we first use the annual resident population estimates produced by the U.S. Census Bureau to calculate the number of individuals of each age in the U.S. from 2004 to 2016. These estimates are for the resident population of the U.S., defined as persons that are "usually resident" in one of the 50 states or Washington, D.C., and exclude U.S. citizens living outside the U.S.[33]

We then calculate the number of individuals in each year-age cell in the full version of the CCP. Similar to our main analysis, we focus on the fourth quarter of each year from 2004 to 2016. To avoid double counting and other potential data quality problems that may inflate our CCP population counts, we drop any individual that was ever recorded as being deceased and drop any records that may be *fragment files*. Fragment files occur when an individual has multiple credit bureau files on record, which can happen if individuals change their name or SSN.[34] For this analysis, we define fragment files as an individual with four or fewer total quarters of data from 2004 to 2016. Since the CCP is a nationally representative 5% sample of the U.S. population, we multiply these counts by 20 to reach a national estimate of each age group in each year.

To compare the two counts, we simply divide the CCP population count by the census's population count for each age and year and express the result in percentage terms. A value of 100 implies that the CCP has full coverage (i.e., the CCP accounts for all individuals that the census counts), while values of less than 100 indicate that the CCP has less than full coverage. It is possible for this ratio to have values over 100, which indicates that the CCP data have *more* individuals in an age-year cell than estimated by the census. While the removal of fragments should limit the amount of double counting that occurs, it does not guarantee that we have removed all potential duplicates. To address this issue, we also calculate a coverage measure that drops individuals with only inquiries on their credit report (and no accounts),[35] along with fragments. We graph both of these ratios over the time period of 2004 to 2016 in **Appendix Figure D1** for ages 23–25 and 27–29.

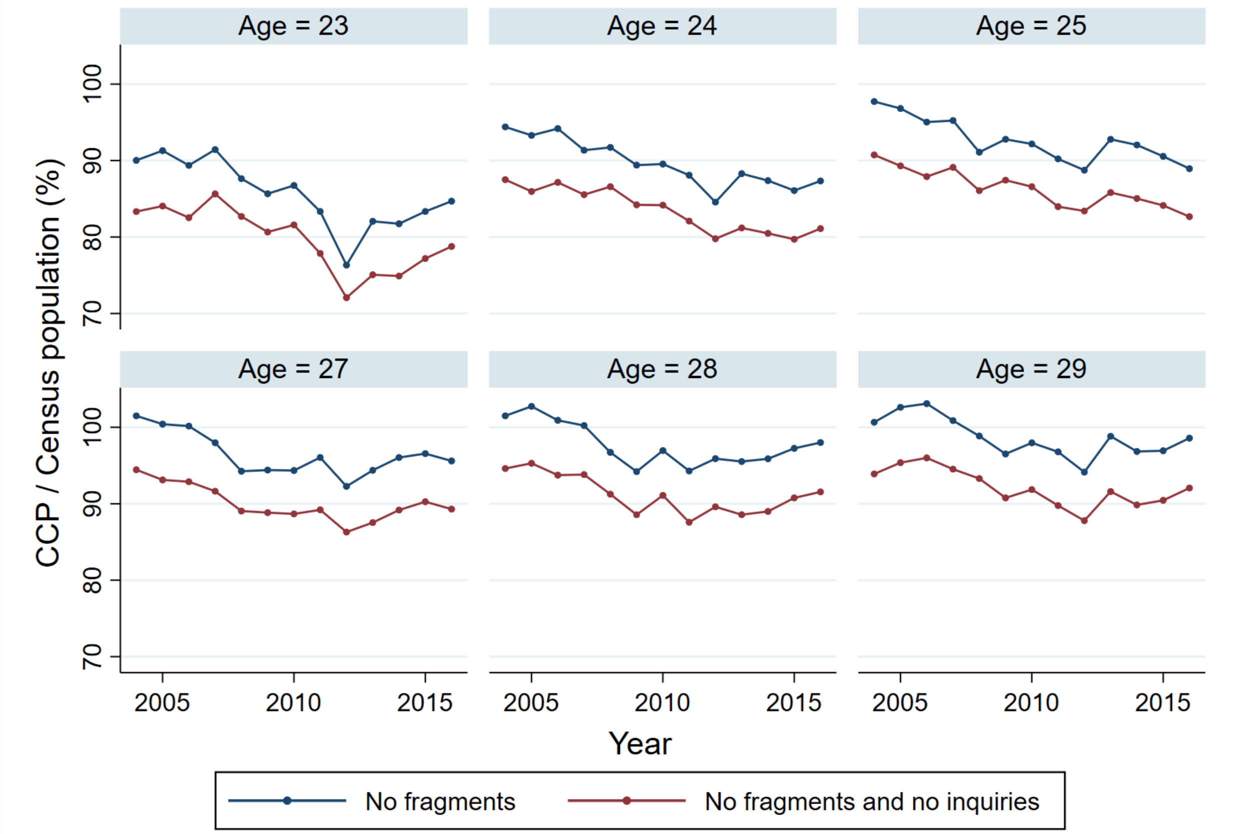
---

[33] For more information on the census's methodology, see https://www2.census.gov/programs-surveys/popest/technical-documentation/methodology/2000-2009/2009-nat-meth.pdf.

[34] For more information on fragment files, see https://files.consumerfinance.gov/f/201212_cfpb_credit-reporting-white-paper.pdf.

[35] An *inquiry* is a request for credit that results in a financial institution officially requesting to examine an individual's credit file. Files with inquiries only may be more prone to errors because they may not have full information to link them to the appropriate individual.

Figure D1: CCP to Census Population Ratios by Age, 2004–2016



Notes: Authors' calculations using data from the CCP from 2004 to 2016. *Fragments* are defined as those individuals with four or fewer total quarterly observations during the sample frame.

Figure D1 shows that the CCP has very good coverage of young adults, with CCP-Census ratios varying from 70% to 100% for each age. When we only exclude fragment files to calculate the CCP population, we do end up with some coverage ratios that are slightly over 100. Restricting the sample and dropping individuals with only inquiries along with fragments leads to more realistic coverage rates. Coverage rates are much higher for older young adults (ages 27–29) than younger young adults, which is expected as older YAs are more likely to be credit visible (Brevoort, Grimm, and Kambara, 2016). While the coverage of young adults in the CCP decreases over time, this trend is present for all age groups, and it does not change substantially with the passage of the DCM. Most importantly for our empirical methodology, we do not find evidence that the treatment and control groups' coverages in the CCP diverge with the DCM implementation.

APPENDIX E: Bankruptcy Results

In addition to third-party collections, we create a binary indicator variable that is equal to one in the year of an individual's first personal bankruptcy filing, is equal to zero in the years where an individual has not yet declared bankruptcy, and is coded as missing in the years after bankruptcy declaration. While coding the variable in this way leads to individuals falling out of the sample when they declare bankruptcy, it allows us to examine how the DCM affects the probability that an individual files for bankruptcy for the first time in our data.[36] This definition of the bankruptcy variable also accounts for the institutional fact that individuals cannot file for another bankruptcy for many years after the first bankruptcy filing. Setting our bankruptcy variable to zero after the first instance of filing would imply that an individual can file for another bankruptcy immediately after the first bankruptcy declaration, which is not allowed in practice. Prior studies argue that personal bankruptcy is one of the last resort options used by individuals to deal with unpaid medical bills (Gross and Notowidigdo, 2011). Therefore, this variable may be able to capture extreme financial distress that could be related to medical expenditures. Summary statistics for this bankruptcy measure are reported in **Appendix Table E1**.

Table E1: Bankruptcy Summary Statistics

	Treatment Group Ages 23–25			**Control Group Ages 27–29**		
	Overall	Pre-ACA (2006–2009)	Post-ACA (2011–2013)	Overall	Pre-ACA (2006–2009)	Post-ACA (2011–2013)
First personal bankruptcy filing	0.003	0.003	0.002	0.006	0.006	0.005
	(0.051)	(0.054)	(0.044)	(0.075)	(0.078)	(0.069)
Num. of obs.	1,752,032	898,089	635,791	1,847,958	930,078	686,663
Num. of individuals	1,140,934					

Notes: Authors' calculations using data from the CCP. Data are for the sample period 2006–2013.

To measure the effect of the passage of the DCM on bankruptcy, we follow Elul, Souleles, Chomsisengphet, Glennon, and Hunt (2010) and specify a dynamic logit model of bankruptcy of the following form:[37]

$$Pr(y_{it}) = F(\beta_0 + \lambda Treatment_i \times \gamma_t + \beta_1 Treatment_i + \gamma_t + X_{it}\Omega), \qquad (E1)$$

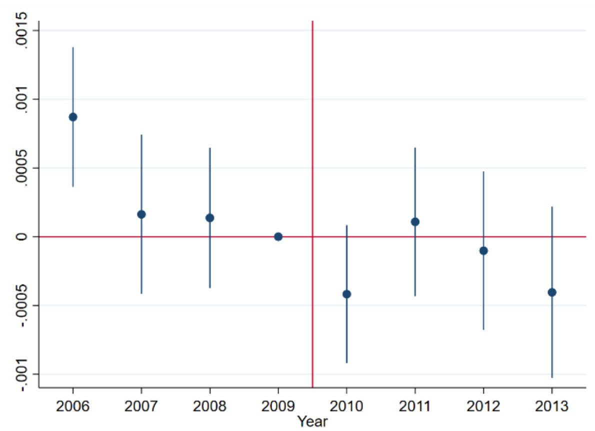
where all variables are specified as in Equation (2). Our dynamic logit model specification does not include individual fixed effects because there is little variation in our bankruptcy variable for

[36] In addition, coding it in this way also leads us to exclude individuals who had already declared bankruptcy prior to the implementation of the DCM from our sample.

[37] Elul et al. (2010) argue that dynamic logit models of default (bankruptcy) can be used instead of discrete duration models in contexts similar to ours.

the majority of individuals in our sample and our data set contains relatively short panels.[38] We also omit state by year fixed effects from the vector X_{it} as their inclusion leads to both estimation problems and biased standard errors. Because we omit individual FEs from our estimating equation, our bankruptcy results may suffer from an omitted variable bias as our model may include individual, time-invariant unobservable factors. Because of these issues, we interpret our bankruptcy results with caution.

Figure E1: First Bankruptcy Declaration



Notes: Authors' calculations using data from the CCP. Results are predicted probabilities. Lines represent 95% confidence intervals.

For the bankruptcy filing variable in **Appendix Figure E1**, we report the marginal effects for the DID coefficients on the probability of first bankruptcy declaration as specified in Equation (E1). Unlike our other variables of interest, we see there is a spike in the preperiod in 2006 for the predicted probability of bankruptcy filing. This spike may be explained by unusual bankruptcy activity related to the passage of the 2005 Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA), which led to large-scale changes in the U.S. bankruptcy system. In the post-DCM period, we observe a small decline in the probability of filing for bankruptcy immediately after the DCM is implemented in 2010, though it is not statistically significant. Overall, we see little effect of the DCM on the probability of filing for bankruptcy.

We also report results from the standard DID regression with a single dummy variable equal to the one for the years in the post-DCM period in Column (1) of **Appendix Table E2**. These results imply that the mandate decreased the probability of personal bankruptcy by 0.02 percentage point or 6.7% of the sample mean, though this decrease is not statistically significant. Columns (2) and (3) of Table E2 report heterogeneity results for our bankruptcy variable by splitting our sample in high- and low-uninsured counties. We observe very small declines in the marginal effect of the DCM on bankruptcy declaration in both high- and low-uninsured rate

[38] Less than 10% of the individuals in our sample have any variation in the bankruptcy variable during the sample frame. Additionally, estimation of the individual fixed effects by including dummy variables in the regression results in incorrect standard errors because of the incidental parameters problems.

counties. These declines are not statistically significant, which is consistent with our other bankruptcy results.

Table E2: The Effect of the Dependent Coverage Mandate on Bankruptcy

	Whole Sample	Above the median pre-mandate county uninsured rate	Below the median pre-mandate county uninsured rate
$Treatment \times Post\ DCM$	-0.0002	-0.0002	-0.00016
	(0.0002)	(0.0002)	(0.0002)
R^2	0.04	0.04	0.04
N	3,570,587	1,904,689	1,745,479

Notes: Authors' calculations using data from the CCP. The dependent variable is first bankruptcy filing. Standard errors clustered at the state level reported in parentheses. Uninsured rate is for young adults ages 19–39 using data from the Census' Small Area Health Insurance Estimates for 2008–2009. All regressions include state and state by year fixed effects. $PostDCM_t = 1$ for the years 2011 to 2013. ***, **, * - denote significance at the 1%, 5%, and 10% level, respectively.

APPENDIX F: MEPS Health-Care Utilization Results

In this section, we examine if the changes to OOP medical expenditures are driven by changes in health-care utilization by young adults because of the passage of the ACA's DCM. Previous studies have shown that DCM had fairly limited effects on health-care utilization of young adults. If young adults have relatively elastic demand for health care, any changes in OOP expenditures may be due to both changes in insurance coverage (insurance now covering utilization that was previously financed OOP) *and* changes in utilization of health care (possibly because of moral hazard or pent-up demand).[39]

To examine changes in young adults' health-care utilization after the implementation of the DCM, we consider a number of intensive and extensive margin measures. In particular, we create dummy variables for individuals if they had any medical provider visits,[40] any non-hospital medical provider visits, any emergency room (ER) visits, and any office visits specifically for physicians in the past year. We also examine the total number of nonhospital (e.g., office) visits. We use the same estimating equation as specified in Equation (1). Results are presented in **Appendix Figure F1**.

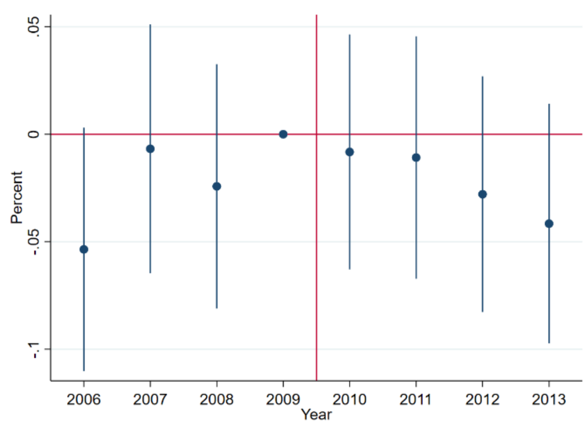
We do not observe any meaningful changes in our measures of health-care utilization after the passage of the DCM. These results are consistent with Chua and Sommers (2014) and Barbaresco, Courtemanche, and Qi (2015), who find little effects of the mandate on preventative care utilization, primary care utilization, or drug utilization. Our results differ from Akosa Antwi et al. (2015), who found increases in utilization of hospital inpatient care for mental illness, though our measures of utilization do not specifically look at the nature of hospitalizations.

[39] Young adults are potentially susceptible to ex ante moral hazard in health insurance. See Barbaresco, Courtemanche, and Qi (2015) and Willage (2020).

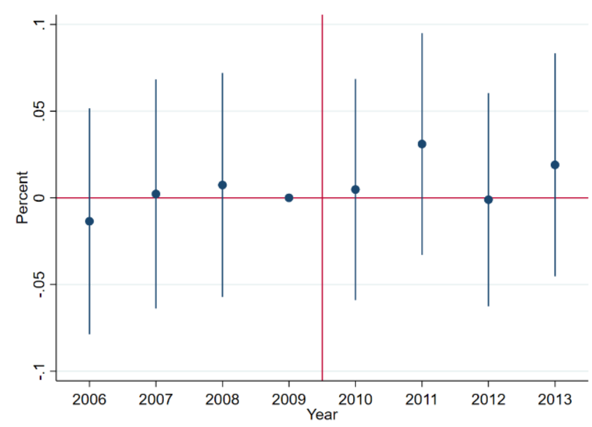
[40] We include any visits to physicians, specialists, hospitals, and filling a prescription.

Figure F1: Effect of Dependent Coverage Mandate on Health-Care Utilization

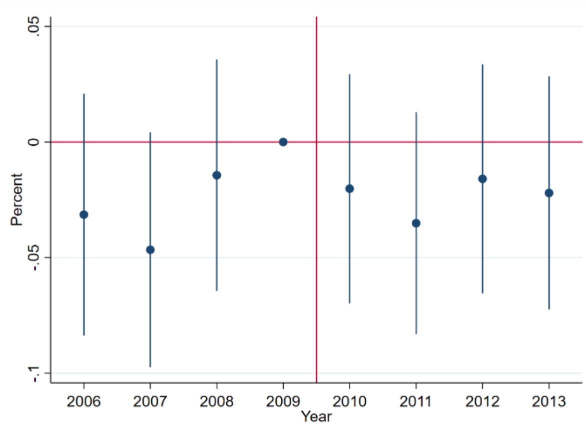
Panel A: Probability of any medical provider visit



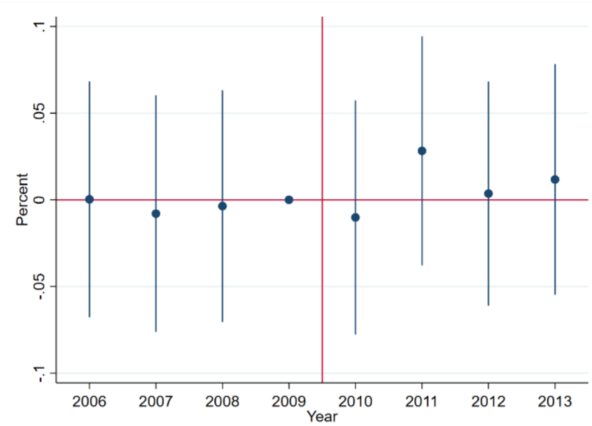
Panel B: Probability of any nonhospital visit



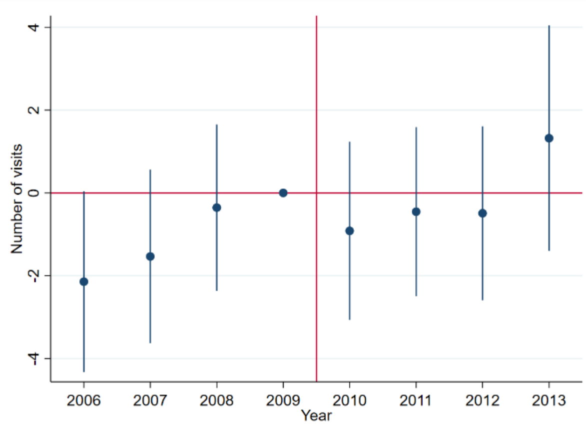
Panel C: Probability of an emergency room visit



Panel D: Probability of any office visit



Panel E: Number of nonhospital visits



Notes: Authors' calculations using data from the Medical Expenditure Panel Survey (MEPS) from 2006 to 2013. All calculations made with sample weights.

APPENDIX G: Triple-Difference Empirical Strategy for Estimating the Effect of the DCM on Financial Distress

In the main CCP analysis, we use age to define the treatment and control groups in our standard DID strategy to identify the financial effects of gaining health insurance for eligible young adults. In this section, we use an alternative strategy that defines the treatment and control groups using an individual's year of birth (instead of age). By using year of birth, we can follow cohorts of individuals who were eligible for health insurance under the DCM and cohorts that just missed being eligible. We also use a triple-difference empirical framework similar to Mazumder and Miller (2016) and Brevoort et al. (2020).[41] This strategy allows us to test how the DCM affected eligible individuals by exploiting the geographic variation in the uninsured rate and unemployment rate for young adults and combine them in implementing a triple-differences (DDD) empirical framework. We do this by combining county- and state-level information on uninsured and unemployment rates for young adults during the premandate 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.[42] We label this indicator variable $Exposure_c$, where subscript c denotes county-level variation we exploit.

When constructing our CCP sample for our DDD analysis of the implementation of the DCM in 2010, 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. We use *quarterly* data of all consumers from the four birth-year cohorts from Q1:2008 to Q4:2013 and drop any consumers who have fewer than four total observations across the sample period. Our final sample includes 866,781 individuals and 18 million observations.

To estimate the causal effect of the passage of the mandate on financial distress for young adults, in principle, we would estimate the following triple-differences model:

$$\begin{aligned} y_{it} = \alpha_0 &+ \alpha_1 Exposure_c \times Treated_i \times Post_t + \alpha_2 Exposure_c \times Treated_i \\ &+ \alpha_3 Exposure_c \times Post_t + \alpha_4 Treated_i \times Post_t + \alpha_5 Treated_i + \alpha_6 Post_t \\ &+ \alpha_7 Exposure_c + X_{it}B + \mu_i + T_t + \epsilon_{it}\,, \qquad (G1) \end{aligned}$$

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 fixed effects interacted

---

[41] Similar to our main results, we cannot observe an individual's insurance status before or after the mandate's implementation and, therefore, interpret our estimates as intent-to-treat (ITT) only. Thus, they likely understate the actual effects on individuals who received health insurance.

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

with time fixed effects, state-level unemployment rates for the 19- to 25-year-old age group and the total unemployment rate for the state, and age fixed effects. We also include in this equation time fixed effects T_t and individual fixed effects μ_i. $Post_t$ 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 postimplementation 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:

$$\begin{aligned} y_{itc} = \lambda_0 &+ \big(Exposure_c \times Treated_i \times (Enact_t + Implement_t + AgeOut_t)\big)\Phi \\ &+ Exposure_c \times (Enact_t + Implement_t + AgeOut_t)\Psi \\ &+ Treated_i \times (Enact_t + Implement_t + AgeOut_t)\Omega \\ &+ \lambda_1 Exposure_c \times Treated_i + \lambda_2 Exposure_c + \lambda_3 Treated_i + \lambda_4 Enact_t \\ &+ \lambda_5 Implement_t + \lambda_6 AgeOut_t + X_{it}B + \mu_i + T_t + \epsilon_{it}\,, \qquad (G2) \end{aligned}$$

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 postmandate periods.

The coefficients of interest are in the vector Φ, which are the triple interactions of $Exposure_c$, $Treated_i$, and each of the time periods defined. 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 access to health insurance only improves financial outcomes while young adults are covered, then we would expect the coefficient on $Exposure_c \times Treated_i \times Implement_t$ to be statistically significant. If enough health-insurance companies began implementation of the mandate before it went into effect, the coefficient on $Exposure_c \times Treated_i \times Enact_t$ may also be negative and significant. The sign and significance of the coefficient on $Exposure_c \times Treated_i \times AgeOut_t$ 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 (G1) and (G2), the coefficient is not estimated because this variable is collinear with the individual fixed effects μ_i. In these regressions, standard errors are clustered at the county level because we use county-level variation in the uninsured rate.

Triple-Difference Event Study Results

The DDD framework outlined in the previous section 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 postmandate period in the absence of the mandate, we can evaluate if the two groups had similar trends in the premandate period. To assess if the trends in our financial variables are comparable across the treated and control groups, we estimate the following event study model based on our triple-difference specification:

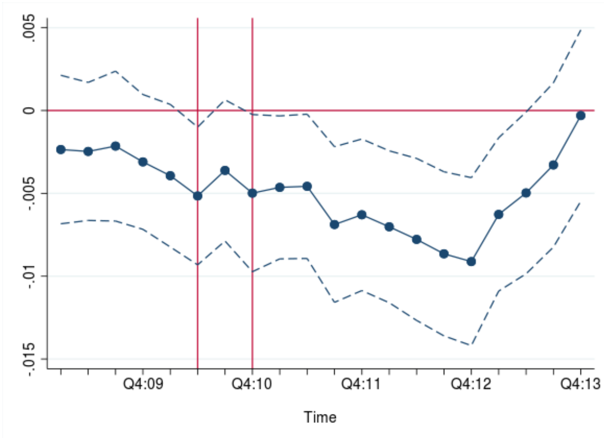
$$y_{itc} = \gamma_0 + \sum_{e=Q1:2009}^{Q4:2013} \gamma_{e\rho} T_e \times Exposure_c \times Treated_i + \cdots + X_{it}B + \mu_i + T_t + \epsilon_{it}, \qquad (G3)$$

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 in the previous equations. Also included in this equation are all double interactions and the single terms of $Exposure_c$, $Treated_i$, and T_e (though we omit them the text for brevity). The coefficients of interest in Equation (G3) are the $\gamma_{e\rho}$'s on the interaction of the calendar time dummy variables, exposure dummy, and the treatment dummy variable. These coefficients show the differences in financial outcomes in the treatment and control groups for quarters prior to and after the mandate implementation. These coefficients are estimated relative to the excluded period — all quarters in 2008. We cluster standard errors at the county level.

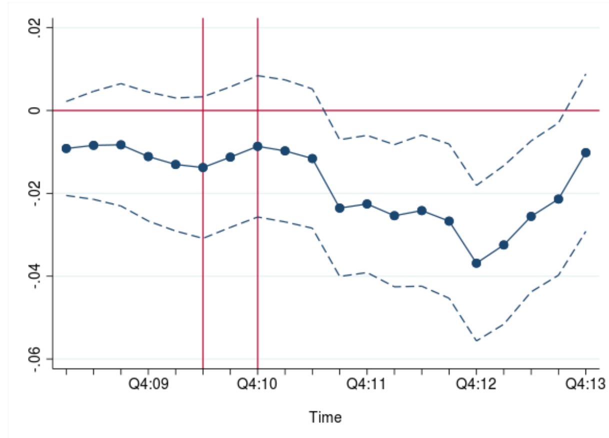
Our left-hand side variables of interest are the same as in main body of the paper, except for our bankruptcy measure. Instead of using a dummy variable that is equal to one for the first bankruptcy declaration we observe in the data for each individual, we use an indicator variable that is equal to one if an individual has recently filed for bankruptcy in the past 24 months. We also use the same estimating equation (Equation G3) for both our bankruptcy and nonbankruptcy measures. We make both of these changes because we encountered multiple quantitative challenges attempting to estimate a triple-difference dynamic logit model and its marginal effects.

Figure G1: Effect of Dependent Coverage Mandate on Financial Distress

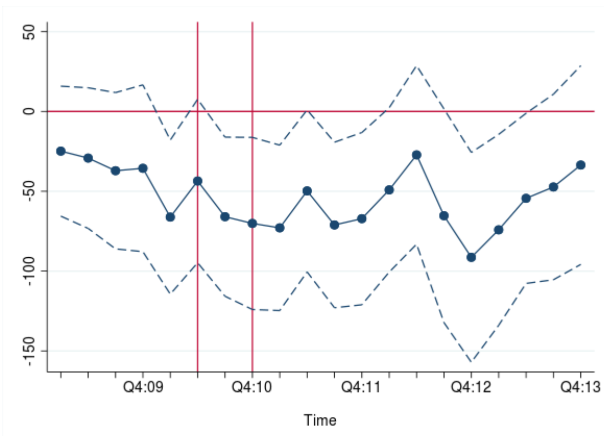
Panel A: Probability of a third-party collection



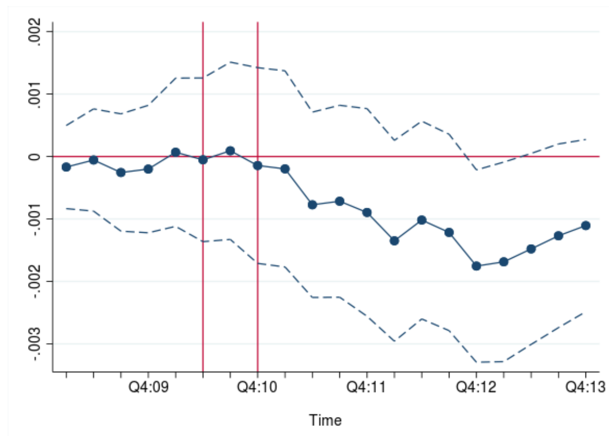
Panel B: Number of third-party collections



Panel C: Amount in third-party collections



Panel D: Bankruptcy declaration in the last 24 months



Note: Authors' calculations using data from the CCP. The first vertical red line indicates when the DCM was enacted while the second vertical red line indicates when the DCM was officially implemented.

Appendix Figure G1 plots the estimated effects of the DCM on financial distress of young adults over the course of our sample. The four panels of Figure G1 show effects for the probability of having debt in third-party collections, the number of accounts in third-party collections, the amount in third-party collections, and the indicator of a bankruptcy declaration in the last 24 months. 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 results in this figure also suggest that the probability of third-party collections, number of collections, and amount in collections declined in the treatment group, compared with the control group after the law was enacted and implemented. There is also a decline in personal bankruptcy filings, which becomes statistically significant in the second half of 2012, when treated individuals are covered by the mandate. After 2012, all cohorts of treated individuals age out of the mandate. In this period, the event study graphs indicate that the improvements in financial outcomes start to diminish and they become statistically insignificant. These preliminary results are consistent with our main results and are suggestive of a potential reduction in some measures of financial distress as a result of the dependent coverage mandate and a potential reversal of these improvements after the automatic disenrollment at age 26.

Triple-Differences Results

Results from the triple-differences specification in Equation (G2) are presented in Table G1. The estimates in Table G1 show that three of our measures of financial distress declined as a result of the dependent coverage mandate. In the implementation period, eligible YAs experienced a 0.5 percentage point reduction in the probability of debt in third-party collections, which is a 2% decline relative to the average in the treatment group. The number of accounts in third-party collections decreased by 0.015, which is a 3% reduction. The amount in collections dropped by $39, or 5% of the mean. Finally, the effect of the mandate on the bankruptcy indicator is negative, but it is not statistically significant at the conventional levels.

We use these estimates of the ITT effects of the mandate to calculate the implied treatment-on-the-treated effects. To do this, we divide the ITT effects by the change in the uninsured rate that resulted from the passage of the DCM, which was approximately 5.4%. Using this rate, we estimate that treated individuals saw a reduction in accounts in collections of $\frac{0.015}{0.054} = 0.278$ and a reduction in the amount of debt in collections of $\frac{39.16}{0.054} = \$725.1$ during the implementation period. While these implied effects are larger than those we estimate in our main analysis, they are still in-line with estimates from previous studies by Hu et al. (2018) and Brevoort et al. (2020).

Table G1: The Effect of the Dependent Coverage Mandate on Financial Distress: Triple-Differences Specification (Q1:2008–Q4:2013)

Coefficient	**Probability of Accounts in 3rd-Party Collections**	**Number of 3rd-Party Collections**	**Amount in 3rd-Party Collections**	**Bankruptcy in the Last 24 Months**
Treated × *Exposure* × *Enactment* (Q2:2010–Q3:2010)	-0.002815*	-0.006854	-30.926*	0.00008722
	(0.002)	(0.007)	(17.927)	(0.001)
Treated × *Exposure* × *Implementation* (Q4:2010–Q4:2012)	-0.005066***	-0.01525**	-39.157**	-0.0008241
	(0.002)	(0.006)	(18.687)	(0.001)
Treated × *Exposure* × *Age-Out* (Q1:2013–Q4:2013)	-0.00213	-0.01669**	-29.096	-0.001310*
	(0.002)	(0.008)	(23.288)	(0.001)
R^2	0.4794	0.4893	0.2869	0.3366
Number of Observations	17,905,532	17,905,532	8,241,707	17,941,047

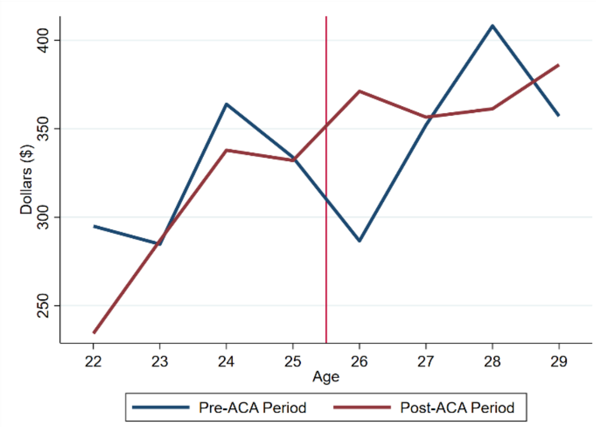
Notes: Authors' calculations using data from the CCP. Standard errors are clustered at the county level. All regressions include individual, time, and state fixed effects. ***, **, * - denote significance at the 1%, 5%, and 10% level, respectively.

The third row of results in **Appendix Table G1** shows the age-out effects of the mandate for the treated group. In this period, the effect of the mandate on the probability of having debt in third-party collections increases (becomes less negative) in magnitude and becomes statistically insignificant. Similarly, the effect on the amount in third-party collections declines in absolute value and becomes insignificant. While the number of accounts in third-party collections still shows a statistically significant effect, overall, the improvements in financial outcomes from the mandate seem to diminish after an individual ages out of the mandate. This result is consistent with our main findings in the paper that show that aging out of DCM eligibility is associated with worse financial outcomes.

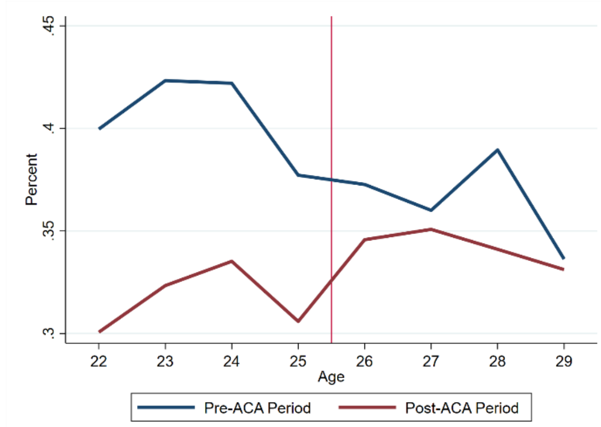
APPENDIX H

Figure H1: Differences in Trends in OOP Medical Spending by Age; Pre/Post-ACA

Panel A: OOP medical expenditures



Panel B: Percent of medical expenditures paid OOP

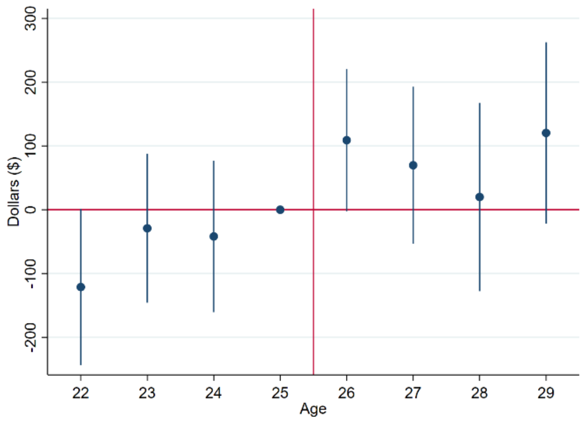


Notes: Based on authors' calculations using MEPS data. All calculations made using sample weights. Pre-ACA is defined as the years 2005–2009 and post-ACA is defined as the years 2011–2015.

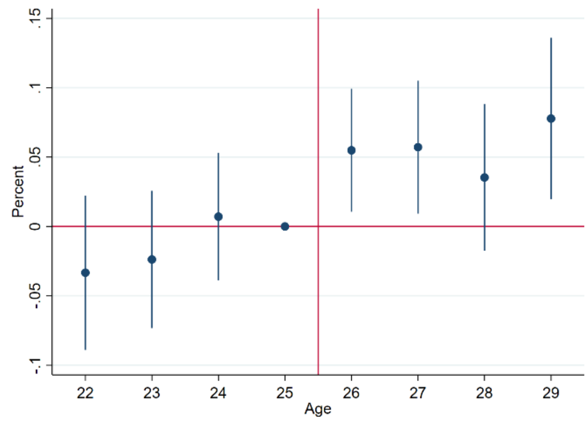
APPENDIX I

Figure I1: Differences in OOP Medical Spending by Age; Pre/Post-ACA

Panel A: OOP medical expenditures



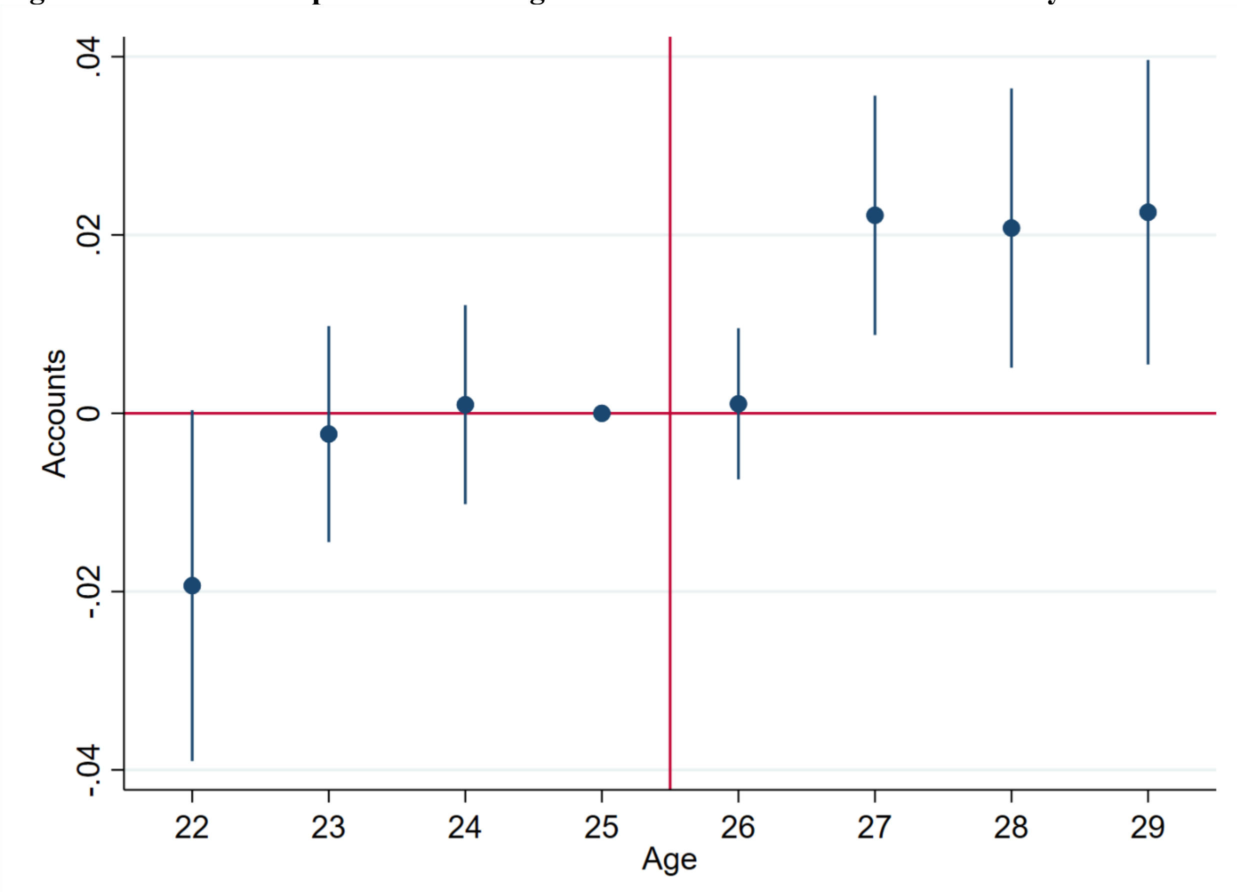
Panel B: Percent of medical expenditures paid OOP



Notes: Based on authors' calculations using MEPS data. Includes years 2005–2009 and 2011–2015. Age 25 is the omitted category.

APPENDIX J

Figure J1: Effect of Dependent Coverage Mandate on Number of Third-Party Collections



Note: Authors' calculations using data from the CCP. Includes years 2004–2009 and 2011–2016. Age 25 is the omitted category.