Health insurance, medical debt, and financial well-being

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We study the financial protection provided by health insurance through two natural experiments—the Affordable Care Act's Under 26 provision and Medicare eligibility. In both cases, the coverage expansion sharply reduces medical debt in collections, but does not systematically improve credit outcomes not directly related to medical care. This is consistent with the infrequent repayment rate and lack of persistence on credit reports that we document for medical collections. These results help clarify the role of health insurance in broader financial health and suggest medical debts in collection may more often be a symptom rather than a cause of wider financial distress.

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Americans hold $81 billion of unpaid medical bills in collections (Batty, Gibbs and Ippolito, 2018), which is on par with debt collections from all other sources combined (Consumer Financial Protection Bureau, 2014). These bills are widely considered to be a significant source of financial strain, and a possible trigger of broad deterioration of consumers' finances. Indeed, the prevalence of medical debt has popularized the concept of "medical bankruptcies." In addition to improving access to health care and health outcomes, mitigating the financial consequences of health events is often cited as motivation for expanding health insurance.

We study the degree to which health insurance helps avert negative financial outcomes by analyzing two policy-induced changes in insurance coverage. First, we track financial outcomes for young adults before and after the implementation of the Affordable Care Act's dependent care mandate in 2010, which required that insurers offer dependent care coverage up to age 26. We focus on this population because young adults both incur the most medical collections (Batty, Gibbs and Ippolito, 2018) and may be more reliant on the credit they are just beginning to build. We supplement this analysis by studying financial outcomes as people become eligible for Medicare at age 65 to show that these effects likely also apply to a broader population.

Our focus on these interventions is particularly informative to the growing literature on financial protection provided by health insurance because, like the coverage considered in this study, most Americans have health insurance with cost sharing. Surveys indicate that expenses of several hundred dollars, which are common under the cost-sharing of Medicare and most private plans, could pose challenges for many families (Federal Reserve Board, 2018). In contrast, Medicaid eliminates virtually all financial risk from health expenses for enrollees, and has been the focus of much of the prior literature.[1] As a result, our study helps inform discussions of coverage expansion where proposals, such as lowering

[1] See Finkelstein et al. (2012), Gross and Notowidigdo (2011), Brevoort, Grodzicki and Hackmann (2020), and Mazumder and Miller (2016).

the Medicare eligibility age, increasing support for the ACA individual market or employer-sponsored market, or further expanding Medicaid, often differ substantially in the degree of financial risk to which insureds are exposed. In addition, compared to the existing literature on the financial protection provided by health insurance, we are able to focus more closely on distinguishing between unpaid medical bills in collections and broader financial consequences of health expenses.[2] Surprisingly, the coverage expansions we study lower medical collections sharply, but do not ameliorate broader measures of financial well-being observable from credit records. Finally, we also document facts about medical collections that help interpret the results presented here and elsewhere in the literature. Namely, reported repayment rates of medical collections are low, and these collections remain on records for a relatively short time. This may help explain why some coverage expansions that result in lower medical collections do not appear to spill over onto other financial outcomes.

As noted above, we find that the effects of insurance coverage on credit outcomes are relatively narrow in both settings. Insurance coverage produces sharp declines in medical debt in collections, but there is little evidence of improvement for a range of broader measures such as credit scores, credit limits, on-time payment of bills, or bankruptcy. When we focus on populations for which the effects of extensive coverage increases are likely the greatest, such as geographic areas with lower incomes or lower rates of insurance coverage, we find greater reductions in medical collections, but still no effects on the other credit outcomes. Although the outcomes we study do not provide a comprehensive view of financial well-being, they correlate with the consumers' ability to smooth consumption using credit and with their success meeting financial obligations. Thus, they are quite informative about deterioration in financial conditions.

There are several reasons why the consequences of unpaid medical bills appear to be less severe than are found by some other studies (or than is asserted in

[2]See Barcellos and Jacobson (2015) and Goldsmith-Pinkham, Pinkovskiy and Wallace (2020)

some policy discussions). First, we show that few medical debts in collections are ultimately repaid.[3] Thus, few consumers appear to be directly diverting payments from other debt obligations to cover these bills once they are reported as collections. Second, we show that most medical collections disappear from credit reports within two years, well before they would be removed by the consumer reporting agencies. The low persistence of medical collections could limit any lasting damage done by their presence on credit reports. Third, medical collections have been shown to be less predictive of future payment difficulties than other collection accounts (Brevoort and Kambara, 2015), which has prompted some credit scoring models to downplay the importance of medical collections in recent versions.[4] Fourth, the recourse available to debt collectors may be somewhat limited. Since medical care is not an asset that can be repossessed and, unlike many other accounts that show up in collections, access to future medical care is often not dependent on payment history, collectors may see little value in pursuing judgements that may result in bankruptcies when repayment is unlikely. Finally, it is important to note that the insurance interventions we study do more to reduce the size of a typical unpaid medical collection than the frequency of such collections. Interventions which more fully reduce the rate at which medical collections (of any size) are accrued, such as with Medicaid, may result in more substantial spillovers to other credit outcomes.

While the existence of medical collections on credit reports reflects some level of financial strain, our findings suggest that policies which lead to decreases in medical collections may not directly lead to improvements in other credit outcomes. However, this does not diminish the other benefits of health insurance or the possibility of improvements to measures of financial strain that are not observable on credit records.

[3] This is consistent with evidence from Consumer Financial Protection Bureau (2014) and Brevoort, Grodzicki and Hackmann (2020).

[4] For example, see the description of FICO 9 here: http://www.fico.com/en/blogs/risk-compliance/impact-medical-debt-fico-scores/

I. Related Literature

A substantial literature has developed on the effect of health insurance on health (see Sommers, Gawande and Baicker (2017) for a recent overview). In the last several years, this work has increasingly focused on health care and financial well-being. For example, several high profile papers have debated the role of medical bills in personal bankruptcy. Himmelstein et al. (2009) has been influential arguing that medical bills play a large role, but subsequent work has generally found much smaller effects. Using the same data but different methods, Dranove and Millenson (2006) estimate medical bills are a contributing factor in 17 percent of bankruptcies. In addition, Dobkin et al. (2018) estimate that hospitalizations cause four percent of personal bankruptcies among non-elderly Americans. Overall, the extent to which medical bills cause wider financial harm is still under debate.

Prior work on the ACA dependent care mandate suggests the mandate lowers the health care costs borne by young adults. For example, it decreases the number of young adults who report delaying or not receiving care because of cost (Sommers et al., 2012), and lowers annual out-of-pocket spending (Chua and Sommers, 2014), particularly among those with higher health care spending (Chen, Vargas-Bustamante and Novak, 2017; Busch, Golberstein and Meara, 2014). In a working paper Blascak and Mikhed (2019) find reductions in out-of-pocket spending and debt collections, but their analysis cannot distinguish collections owing to medical bills versus those from other sources. We provide a more comprehensive study of the dependent care mandate's effect on credit outcomes by showing that the reduction in collections is restricted to those stemming from health care, and that there is little evidence of improvement for other outcomes that describe borrowers' access to credit and difficulties repaying other debts.

A larger literature has developed on the financial benefits of Medicare. Finkelstein and McKnight (2008) show that the creation of Medicare in 1965 led to a reduction in the risk of large out-of-pocket expenses for older Americans. Engel-

hardt and Gruber (2011) come to a similar conclusion about the prescription drug benefit Medicare Part D passed in 2003. Barcellos and Jacobson (2015) find a 33 percent reduction in out-of-pocket spending upon becoming eligible for Medicare (concentrated among large expenses), and large reductions in self-reported financial strain related to medical bills.[5] Caswell and Goddeeris (2020) present a targeted analysis of Medicare's effects on medical collections and find reductions, particularly among large bills. Meanwhile, Goldsmith-Pinkham, Pinkovskiy and Wallace (2020) find reductions in total collections upon reaching Medicare age and highlight the distribution of these effects across geographies and populations. Our data reveal that the entire reduction in collections comes from medical bills in collections. We also explore variation that generates larger reductions in medical collections upon becoming eligible for Medicare, but still find no evidence of improvement in other credit outcomes. Thus, we present relatively comprehensive evidence that despite being protected from larger medical bills, gaining insurance coverage under Medicare does not meaningfully improve broader credit outcomes. Moreover, we highlight empirical facts about the lack of persistence of medical collections on credit reports as a potential mechanism to help explain these results.

The financial benefits of Medicaid have also been the subject of several recent studies, and researchers have amassed more evidence that its financial benefits extend beyond reductions in medical collections. Finkelstein et al. (2012) find large reductions in unpaid medical collections using the Oregon Health Insurance Experiment, and Brevoort, Grodzicki and Hackmann (2020) find the same using states' decision to expand Medicaid under the ACA. However, Finkelstein et al. (2012) find no other changes in access to credit or delinquencies, while Brevoort, Grodzicki and Hackmann (2020) find greater access to credit and a reduction in delinquencies. Gross and Notowidigdo (2011) argue that state-level Medicaid

[5]The reduction in expenses comes mostly from lower prices negotiated by Medicare, as opposed to differences in the amount of care received or the generosity of the insurance.

expansions in the early 1990s led to significant reductions in bankruptcy filings. Mazumder and Miller (2016) study the 2006 Massachusetts health reform, which, much like the ACA, combined Medicaid expansion with an insurance mandate and subsidies to purchase private coverage. They find substantial improvements in credit outcomes, including credit scores and bankruptcies, but, surprisingly, find relatively moderate reductions in unpaid bills sent to collections. Finally, Miller et al. (2018) find that Michigan's Medicaid expansion led to improvements in a very broad set of financial outcomes, including several credit and non-credit outcomes (such as evictions and foreclosures).

In addition to explicitly targeting a low-income population, Medicaid requires very little cost-sharing from enrollees which may limit its comparability with other insurance programs.[6] For example, approximately half of all consumers with new medical bills reported in collections in a given year owed less than $600 (Batty, Gibbs and Ippolito, 2018). Thus, it is plausible these bills would be covered by Medicaid but would still be the responsibility of the patient under Medicare or private insurance.[7] As a result, the minimal cost sharing within Medicaid may be crucial to its effects on credit outcomes not tied to medical bills.

II. Data

Our primary data source is the Consumer Financial Protection Bureau's Consumer Credit Panel (CCP) from 2007 to 2018. The CCP is a 1-in-48 random sample of de-identified credit reports from a nationwide consumer reporting agency. It contains account-level information for a variety of forms of credit for the same individuals each period. Critically, beginning in 2012, it allows us to identify unpaid medical collections separately from other types of bills that are in collec-

[6] Most Medicare enrollees have supplemental coverage that lowers what they pay out-of-pocket. In Appendix B we use data from the Health and Retirement Study to investigate how this varies across individuals. We conclude that while most Medicare enrollees have supplemental coverage of some kind, the majority of the formerly uninsured do not and thus are exposed to meaningful cost sharing.

[7] See Gabel et al. (2015) for a recent analysis of trends in cost sharing in individual and employer market health insurance plans.

tions.[8] We observe the opening date and opening balance of each collection.[9] For every individual, we calculate the total dollar value of new medical collections for the year, and create an indicator for whether they have any new medical collections. By focusing on the flow rather than the stock of medical collections we isolate the failure to pay a medical bill from how long the debt collector continues to report that bill to the consumer reporting agency which we discuss in more detail toward the end of this study.[10]

We only observe medical debt once it has become sufficiently delinquent to be sent to a third-party collection agency. While this captures an important measure of medical debt, it is not necessarily a comprehensive measure of all unpaid medical bills. Although many unpaid medical bills appear in credit record data, a provider or debt collector may choose not to report this information to a nationwide consumer reporting agency. We also do not observe when a medical bill is financed with a credit card or personal loan and the balance is not fully repaid, or when patients become delinquent on other bills after paying a medical bill before it is sent to collections. These outcomes are observable in credit reports but would not be explicitly tied to a medical bill so we cannot isolate them.

In addition to medical debt in collections, we study a broad array of general financial outcomes observable in the CCP. These include changes in non-medical bills that have been sent to collections, the frequency with which credit accounts become increasingly delinquent (30 days past due to 60 days, 60 to 90, and 90 to 120, excluding bills sent to collections), total credit card limit, the share of that

[8]The CCP begins in 2001, but we cannot separately identify medical and non-medical collections until September 2011 and so do not use a measure of medical collections until 2012, the first full year in which we can separate medical collections. In order to maintain anonymity of consumers in the panel, the data do not contain any information about the type of care provided or the identity of the provider.

[9]There is some challenge in determining when a medical collection is legitimate and not the result of a problem with insurance billing. For example, a medical bill may initially be sent to collections, only to be removed from the consumer's credit report when it is later discovered that the bill should have been covered by insurance. However, this appears to have a limited effect on the medical collections reported. See Appendix A of Batty, Gibbs and Ippolito (2018) for a more detailed discussion.

[10]Furnishers are not required to report these collections for a specific length or time or to report them at all. Conditional on reporting on these accounts, furnishers are required to report accurately under the Fair Credit Reporting Act, and the derogatory item will only show up on a consumers account for up to seven years after the delinquency is incurred.

limit in use, credit score, and whether the individual has filed for bankruptcy or been identified in another public record.[11] Together, these outcomes provide a useful summary of consumers' ability to pay bills on time, and the potential consequences of failing to do so.

To understand insurance transitions by age, we use national insurance coverage by age from the American Community Survey (ACS) 1-year PUMS for 2008-2018. To study heterogeneous effects on credit outcomes, we merge the CCP data with county-level income and insurance coverage. Median household income comes from the Census Bureau's Small Area Income and Poverty Estimates for 2013-2017. County-level insurance coverage for ages 40-64 is taken from the Census's Small Area Health Insurance Estimates (SAHIE) for 2012-2017.

III. The Effect of the ACA Dependent Coverage Provision

A. Coverage Changes From the Dependent Coverage Provision

We begin by studying young adults who gained coverage through the ACA dependent care mandate. This population both incurs the most medical debt in collections and is likely to be more dependent on credit than older Americans (Batty, Gibbs and Ippolito, 2018). The dependent care mandate became the first major provision of the ACA to take effect in September 2010. It allows dependent children to be covered on a parent's health insurance plan through age 25, an increase from the previous limit of age 18 for many plans.[12]

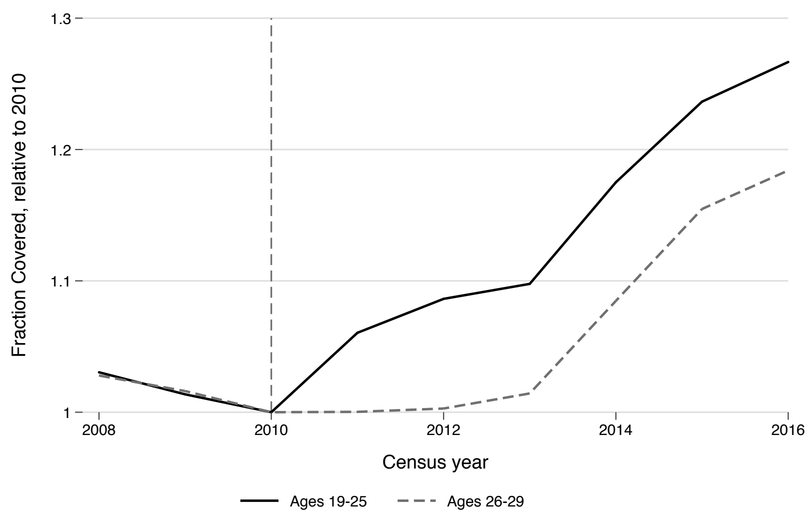
This provision had a substantial effect on coverage rates for 19- to 25-year-olds. Figure 1 shows health insurance coverage for those affected (ages 19-25), and those just above the age cutoff (ages 26-29). For clarity, we show coverage rates as a fraction of levels in 2010, which were 67.6 percent for those 19-25 and 71.3 percent for those 26-29. Coverage trends evolved similarly prior to enactment but diverge

[11] These public records include a variety of judgments and tax liens.

[12] As discussed in Akosa Antwi, Moriya and Simon (2013), prior to the ACA several states had policies to allow children stay on their parents' insurance above age 18, but the authors find no evidence that the coverage increases under the ACA dependent care provision were affected by these state-level policies.

markedly in 2011, the first year after enactment. By 2012, coverage among 19- to 25-year-olds increased about 9 percent (5.9 percentage points), while coverage rates for 26-29 year olds were unchanged. Coverage rates for both groups rose in 2014 as other major ACA provisions took effect.

Figure 1. : Health Insurance Coverage by Age Group, 2008−2016



Note: This figure shows the fraction of individuals with any insurance coverage (at the time of survey) by age in the American Community Survey (ACS) surrounding the introduction of the dependent coverage mandate. This includes public and private insurance, but does not include insurance from the Indian Health Service. For clarity, we show coverage relative to a baseline year of 2010. Those under 26 were eligible for coverage on a parent's plan after 2010.

B. Empirical Analysis of the Dependent Coverage Mandate

As suggested by Figure 1, our approach is to compare the credit outcomes of young adults who are and are not eligible for dependent care before and after the enactment of the provision. The fact that the dependent care mandate was enacted in the early stages of the recovery from a severe recession complicates the analysis because college-aged students, people just entering the workforce, and those more established in the workforce may have experienced the recession in different ways which could be reflected in credit report outcomes (Bell

and Blanchflower, 2011). As shown in Appendix A, younger adults experienced steeper proportional declines in employment and income, and their recovery began approximately one year later than those in their mid to late 20s. Therefore, we segment those affected by the dependent care mandate into three age groups. As shown in Appendix A and in the results below, we find that those within three years above the age eligibility cutoff are a valid control group for those within three years below. The comparison is somewhat less precise, but still informative, for those four or five years below the cutoff. However, the recession appears to have differentially affected those in their early versus late 20s to the degree that the older group no longer provides a useful control group. We formalize this analysis by estimating the following event study specification for an individual i in year t:

(1)

$$Y_{it} = \alpha + \beta Young_{it} + \kappa Middle_{it} + \sum_{\tau \in K} (\delta_t Young_{it}\gamma_t + \theta_t Middle_{it}\gamma_t + \eta_t\gamma_t) + \epsilon_{it}$$

$$\text{where } K = \{2007, 2008, 2009, 2011, 2012, 2013\}.$$

$Young_{it}$ and $Middle_{it}$ are indicator variables equal to one if individual i is between the ages of 22 and 23 in year t and 24 and 26 in year t, respectively, while γ_t is indicator variable equal to one in year t. The coefficients of interest are δ_t and θ_t, which measure the difference between the outcome for each group eligible for dependent care in year t and in 2010 (the year before the enactment of the dependent care provision) relative to the difference between the outcome for slightly older adults who are not eligible in year t and in 2010. That is, δ_t and θ_t prior to 2010 assess the degree to which outcomes for younger and slightly older adults were trending similarly before the enactment of the ACA dependent care provision, and δ_t and θ_t after 2010 estimate the provision's effect. Finally, β and κ measure the level differences between the two treated groups and the control group in the omitted year, 2010, and the η_t coefficients measure year effects. We

use data from 2007 through 2013, which gives us several years on either side of enactment and insulates the analysis from the enactment of other ACA provisions in 2014.

Since the CCP contains birth year but not precise age, we omit the age at which age-based dependent care is ambiguous. Although most young adults become ineligible for dependent care at age 26, we instead omit the year in which people turn 27 due to the lag between when a medical event occurs and when the unpaid bill appears on a credit report.[13] We are unaware of any data on the time between care delivery and reporting an unpaid bill, but our review of available anecdotal evidence suggests it is measured in months, and perhaps close to a year.[14] This suggests that medical collections reported in the CCP in the year a patient turns 26 likely stem from care given before they lost eligibility under the dependent care mandate. Age 27, however, likely represents a mix of bills stemming from care both before and after this change.

Our data do not separately identify medical versus non-medical collections until 2012. Thus, we estimate Equation 1 on total collections. However, we use the fact that young adults "age out" of the dependent care provision to distinguish between effects on medical versus non-medical collections after 2012. The effect of this "age out" changes as other ACA provisions went into effect. Coverage rates drop at age 26 by about four percentage points in the years prior to the other ACA provisions, and by about two percentage points in the years after, before returning to their prior level by age 29. We examine whether medical collections evolve differently over this "aging out" threshold than non-medical collections.

[13]For example, a medical provider will typically attempt to collect payment for an extended period before ultimately turning the bill over to a third-party debt collector who may report the delinquency to a consumer reporting agency.

[14]Review of several hospital policies suggest that 120 days is a common lower bound.

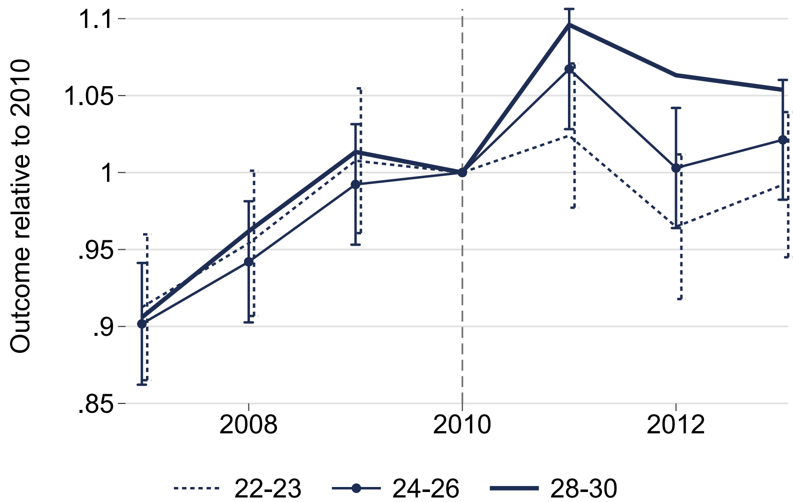
C. Dependent Coverage Mandate Results

Below we plot the results of our event study specification. Rather than showing only the δ_t and θ_t, we plot these coefficients relative to the year effects (which we label 28-30 since they capture the control group). Each set of coefficients is scaled so the omitted year of 2010 equals one. That is, the line for the youngest group plots $(\delta_t + \eta_t)/(\delta_{2010} + \eta_{2010})$, the line for the middle group plots $(\theta_t + \eta_t)/(\theta_{2010} + \eta_{2010})$, and the line for the control group plots η_t/η_{2010}. We select this presentation because it also shows how the recession and recovery affects the outcomes of interest.

Effects on Medical Debt in Collections. — Figure 2 shows a clear reduction in collections surrounding the enactment of the under-26 provision, with larger effects for the younger treated group. By 2012, the average of the event study estimates for the two treated age groups is a 7.9 percent reduction in collections relative to the control. Figure 3 splits that result into the fraction of individuals incurring a collection (panel a) and the average annual amount of new collections for those that occur (panel b). The fraction who incurred a collection trended very similarly for all groups before the mandate went into effect. Immediately after enactment, the fraction of those aged 22-23 with a new collection fell by three percent. However, those aged 24-26 saw no reduction. Prior to enactment, the dollar value of collections (conditional on having one) trended similarly across age groups, but clearly diverged in the post-enactment period for all treated ages. By 2012, the size of incurred collections increased by over seven percent for those above 26 but less than one percent for the treated groups (corresponding to event study coefficients indicating just under seven percent reduction). Together we take this as evidence that the under-26 provision led to substantial reductions in the size of collections, and suggestive, but not conclusive, evidence that it also reduced the frequency of collections.

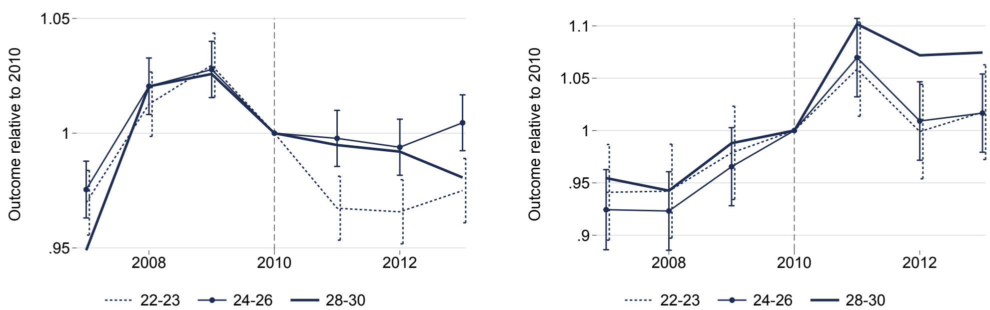
Although we cannot distinguish between medical and other collections until

Figure 2. : Unconditional Mean Dollar Value of Total Collections Surrounding Dependent Care Provision, 2007-2013



Note: This figure shows unconditional mean value of total collections for years 2007-2013 by age group. Those aged 22-23 and 24-26 are affected by the introduction of provision while those aged 28-30 are not. Data are from the CFPB CCP.

Figure 3. : Total Collections Surrounding Dependent Care Provision



(a) Fraction with Any Collections, Relative to 2010 Levels

(b) Conditional Mean Dollar Value of Total Collections, Relative to 2010 Levels

Note: Coefficients from Equation 2 are plotted relative to 2010 and to the control group of ages 28-30. Data are from the CFPB CCP for years 2007-2013. Vertical bars represent 95 percent confidence intervals.

after the dependent care mandate was implemented, the large changes in coverage rates after young adults age-out of the dependent care mandate protections

provide insight. Figure 4 shows that through age 25, coverage rates are stable, and medical and non-medical collections trend together. Once young adults are no longer eligible for dependent care coverage at 26, health insurance coverage declines sharply, and then rebounds over the next three years as people gain insurance through other sources. During the period of volatile coverage rates, non-medical collections are largely stable, but medical collections spike as coverage rates decline, and then fall as coverage rates rebound.[15] This suggests that the eight percent reduction in total collections we estimate upon implementation of the dependent care mandate is dominated by a reduction in medical collections. If we assume it comes entirely from medical collections, and since medical collections account for approximately half of total collections, the 5.9 percentage point (9 percent) increase in insurance coverage would translate to a 16 percent ($40) decline in medical collections.

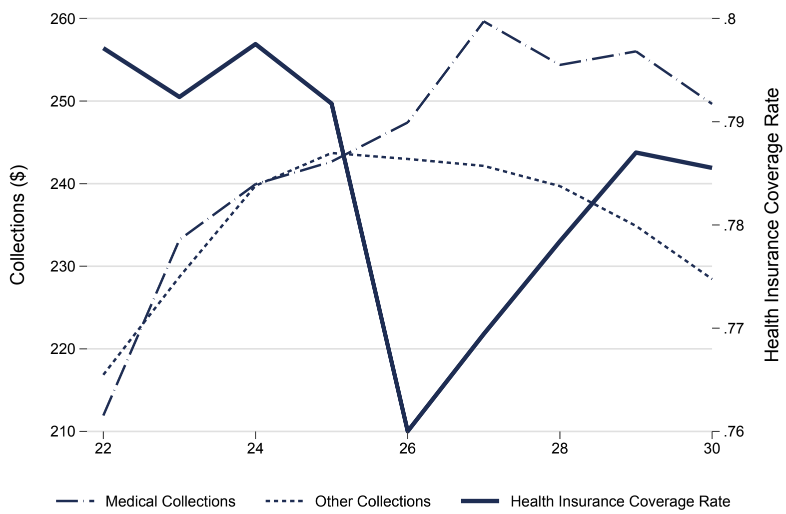
Effects on Broader Financial Outcomes. — Besides medical collections, we also study a variety of other ways in which receiving a large medical bill might affect measures of financial well-being in credit report data. First, as patients pay all or a portion of a medical bill, they may fall further behind paying other bills. This would be captured in the CCP by revolving or installment loans that are at least 60 days past due.[16] Thus, we look at effects on the share of consumers who became more delinquent on an account in a given year (e.g., from 30 days past due to 60 days, 60 to 90, 90 to 120, etc.).

Second, medical bills might affect access to credit (as measured by credit score and credit limits) and how much other debt consumers are using. A credit score is a numerical assessment of a borrower's creditworthiness based upon factors such

[15] Note, medical collections increase slightly at age 26, but most of the increase occurs at age 27. This is consistent with the fact that the CCP records the year in which a person reaches an age (meaning 26 represents a mixture of 25 and 26), and there being a time lag between receiving care and medical collections appearing on a credit report.

[16] We choose a 60-day threshold because a substantial share of 30-day delinquencies are resolved before they reach 60 days, and thus the shorter threshold is likely a less reliable indicator of significant financial distress. In addition, the 60-day threshold aligns well with other sources, such as the Survey of Consumer Finances.

Figure 4. : Unconditional Mean Dollar Value of Medical versus Non-Medical Collections, 2012−2018



Note: This figure shows medical versus other collections (both unconditional) by age from the 2012-2018 CFPB CCP. It also shows the fraction with health insurance by age from the 2012-2017 ACS.

as existing debt load and payment history. Although it is generally acknowledged that unpaid medical collections factor negatively in many credit scoring models, the algorithms are proprietary.[17] In addition to credit score, we consider two measures related to access to credit and realized borrowing: total credit card limit and how much of that limit the borrower is using.[18] Changes in credit limit can be requested by the borrower, but they can also be unilaterally imposed by the creditor (for example by cutting a limit or closing an account). Credit limit and utilization reveal the amount of available credit, but they also speak to the frequency with which patients might incur shadow medical debt by paying for medical bills with credit cards which they could later struggle to repay. For

[17]Research has shown that medical collections are less predictive of future payment behavior than other collection accounts (Brevoort and Kambara, 2015) which has prompted some models to reduce the influence of medical collections in their most recent versions (e.g., see the description of FICO 9 here: http://www.fico.com/en/blogs/risk-compliance/impact-medical-debt-fico-scores/).

[18]We also consider effects on total revolving account limits (including general purpose credit cards, retail cards, and lines of credit) and utilization and find similar results, in part because credit cards make up a large share of total revolving limits and balances (not shown).

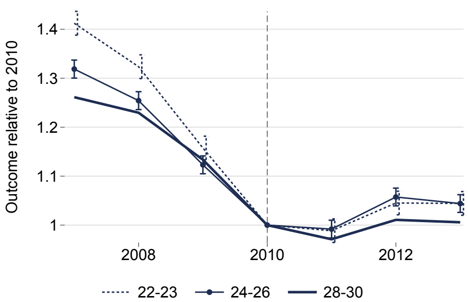
example, insurance that lowered the frequency or size of such bills could directly lower utilization.

Finally, the CCP also records more severe consequences of a borrower's failure to pay, such as public records and bankruptcies. Public records include instances when a creditor wins a judgment for wage garnishment, asset seizure (e.g., the contents of a bank account), or a lien placed on property. Bankruptcy can be a way to resolve debt collection efforts, possibly including judgments, so we primarily view them as an indicator that unpaid bills are causing problems for the borrower. We measure the rate at which new declarations of bankruptcy and new public records appear on credit reports each year.

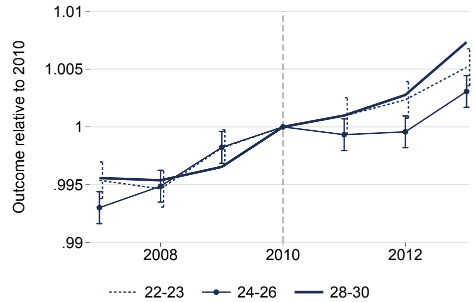
Figure 5 shows the results for these outcomes. We find very little evidence that the increase in coverage led to meaningful improvements in objective measures of financial well-being not directly related to medical debt in collections. If anything, delinquencies (panel a) for the treated ages appear to have increased slightly after the dependent care mandate came into effect. We find few signs that access to credit increased. Credit scores (panel b) did not improve, and again deteriorated slightly for those 24-26. Credit utilization (panel c) for those 24-26 trended closely with those 28-30 before the dependent care provision and by 2013 was nearly 2.2 percent lower. This may stem from fewer medical bills being paid for with credit cards, but we can rule out an effect of more than 3.4 percent. Credit limits (panel d) for those aged 24-26 moved closely in line with the control group over the entire period. Total limits slightly increased for those 22-23, but these results are also consistent with the pattern of a steeper recession-related decline and recovery as documented in Appendix A. Public records (panel e) for those 24-26 show no sign of decline relative to those 28-30, while estimates for those 22-23 are slightly, but insignificantly, lower. Finally, bankruptcy rates are slightly higher for treated ages in the post period, though there is some difference leading up to the policy enactment. Taken together, the data do not provide evidence the dependent care mandate systematically improved the credit records of young adults beyond

outcomes directly tied to medical bills (in fact, some outcomes like credit score or delinquencies appear to deteriorate).

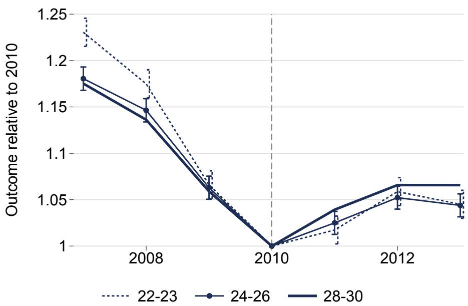
Figure 5. : Other Financial Outcomes Surrounding Dependent Care Provision



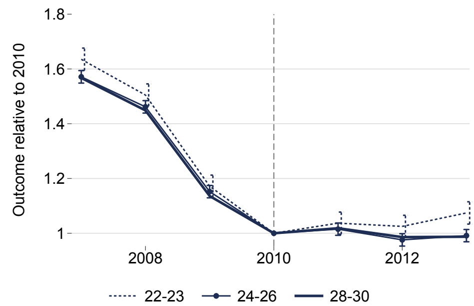
(a) Increased Severity of Delinquency



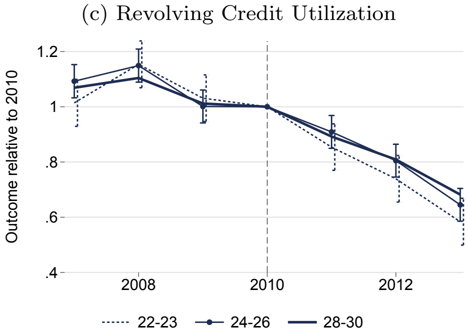
(b) Credit Score



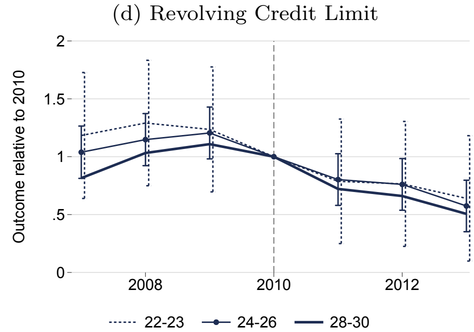
(c) Revolving Credit Utilization



(d) Revolving Credit Limit



(e) Public Records



(f) Bankruptcy

Note: This figure shows non-collections outcomes surrounding the enactment of the ACA's dependent coverage mandate in 2010. Data are from the CFPB CCP for the year 2007-2013. Coefficients from Equation 2 are plotted relative to 2010 and to the control group of ages 28-30.

Since health insurance status is not recorded in credit data, our results combine

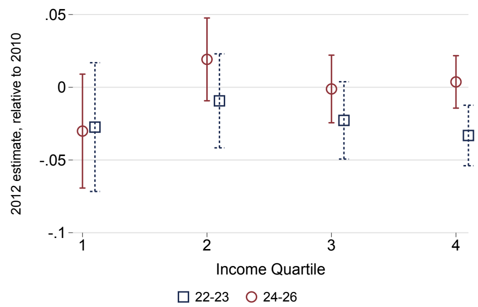
individuals who do and do not gain coverage under the dependent care mandate. We focus on subgroups that are most affected, but appropriately segmenting the population is not straightforward. By definition, dependent coverage is only available to young adults whose parents have insurance that can be extended to their adult children and are therefore more affluent than average. However, children of more affluent parents are more likely to already have coverage on their own absent the dependent coverage provision. We see these tensions at play in the ACS. For example, coverage increases under the dependent care provision are largest in counties in the third quartile of household income, followed by the top quartile, then the second, and then the first.

In figure 6 we investigate whether estimates for income quartiles mirror coverage increases across quartiles. To ease comparison, we show event study estimates for one year for each treated group relative to the omitted control group. We choose 2012 because it is well into the post-period. To adjust for level differences in the outcomes across groups, we have normalized the coefficients by the 2010 level for each group.

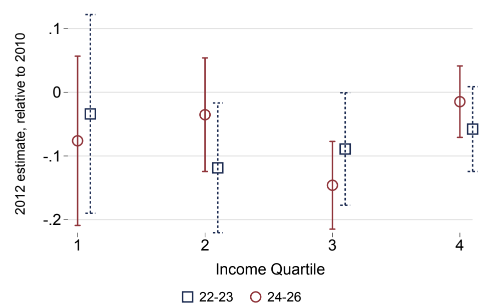
If results mirror insurance coverage gains, we would expect the largest improvements among young adults in the third quartile and smallest among those in the first. The effects on the fraction with any collections are similar across groups (panel a). However, consistent with insurance coverage increases, we find that reductions in the dollar value of collections, conditional on incurring at least one, appear to be larger for the middle to upper portion of the income distribution (panel b). Across other measures, we do not see evidence of clear improvements concentrated in one part of this distribution, nor a consistent pattern between the size of coverage increase and the size or direction of the other estimates.

Taken together these results show that the dependent coverage mandate reduced medical debt in collections for those affected by the provision, but there is little evidence that it directly improved—and possibly worsened—other financial outcomes seen in our credit record data. Although our ability to focus on

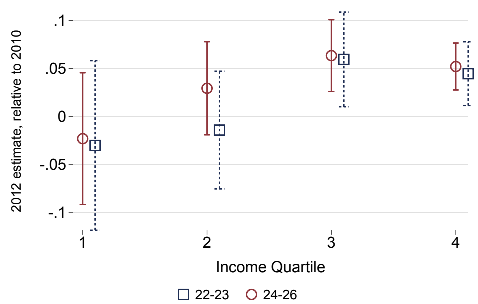
Figure 6. : Effects of the Dependent Care Provision by Income Quartile



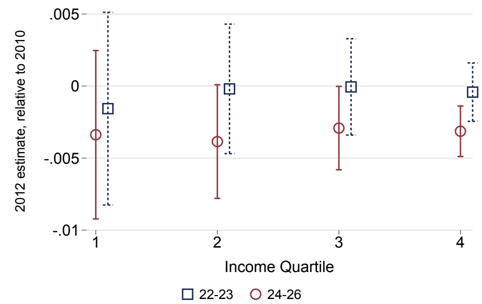
(a) Fraction with Any Collections



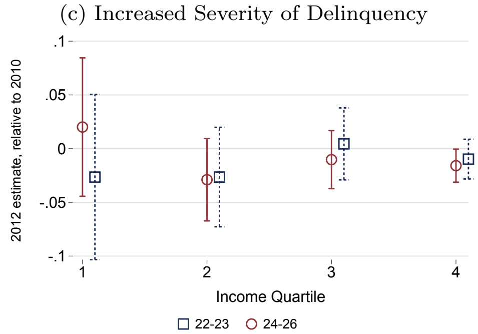
(b) Conditional Value of Collections



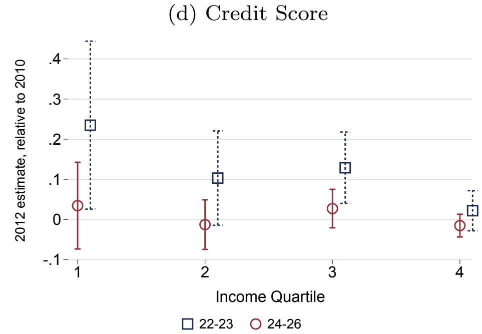
(c) Increased Severity of Delinquency



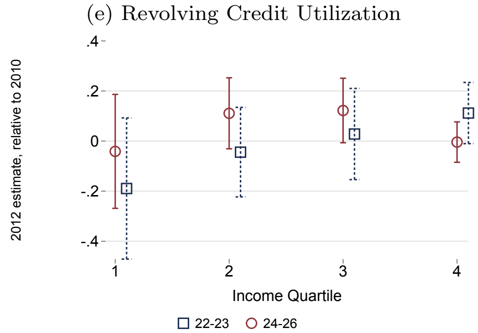
(d) Credit Score



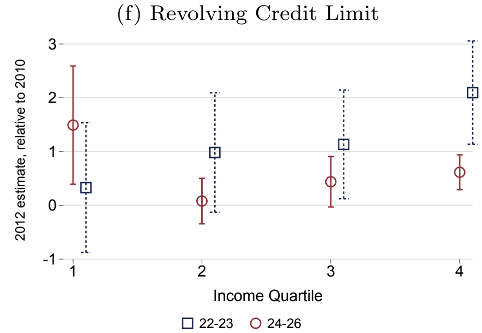
(e) Revolving Credit Utilization



(f) Revolving Credit Limit



(g) Public Records



(h) Bankruptcy

Note: This figure shows the 2012 event study estimate from Equation 2 as a fractional change from the 2010 level, by quartile of county-level income. Coverage gains were largest in the third quartile of income and lowest in the first quartile. Credit data are from the CFPB CCP. Income data are from the Census' SAIPE for 2012-2017.

populations directly affected by the dependent care mandate is limited, below we show very similar results for Medicare eligibility where we can more readily differentiate between types of people who gain insurance coverage.

IV. The Effect of Medicare Eligibility

A. Coverage Changes Surrounding Medicare

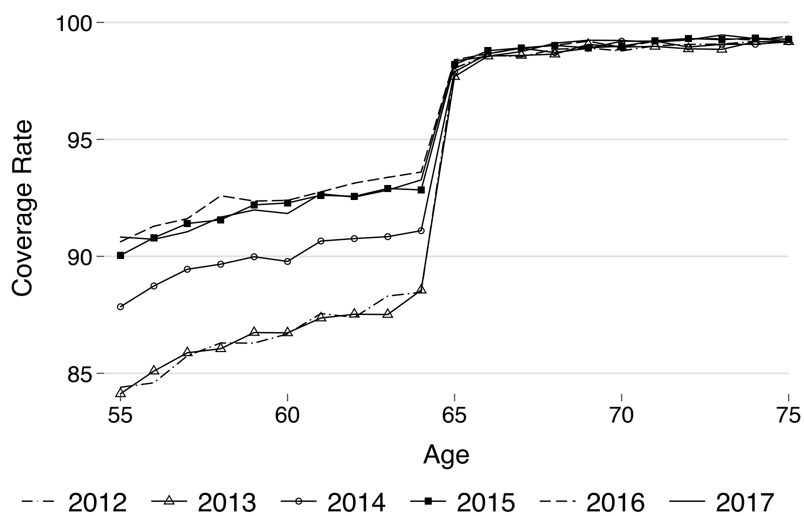
We identify the effect of health insurance coverage on older consumers' financial outcomes using the discrete change in health insurance coverage at age 65.[19] Figure 7 shows the change in coverage separately for 2012 through 2017. Both 2012 and 2013 are prior to the enactment of key provisions of the ACA (other than the under-26 provision, which would not have affected these age groups), and in both years health insurance coverage increases by nearly 10 percentage points (to near 100 percent) at age 65. This discontinuous jump began to erode in 2014 with the introduction of the ACA individual market and the primary wave of state Medicaid expansions. As a result, coverage increased by 7 percentage points in 2014 and only 5 percentage points in 2015-17.

B. Regression Discontinuity Analysis

We assume that in the absence of Medicare, outcomes would evolve smoothly over the ages surrounding 65, and thus any shift or trend break that occurs at Medicare eligibility age is the result of the program. Like prior studies, we do not find evidence of discrete changes or trend breaks in employment rates, social security claiming, and personal income around the age 65 threshold (see Appendix C). Evidence on whether health care use changes at Medicare eligibility is mixed. Card, Dobkin and Maestas (2008) find moderate increases in self-reported utilization in both the National Health Interview Survey (NHIS)

[19]To be eligible for Medicare, an individual or their spouse must have paid Medicare payroll tax for at least ten years. People who are covered by Social Security Disability Insurance or who have end-stage renal failure can receive Medicare before age 65.

Figure 7. : Health Insurance Coverage by Age, 2012−2017



Note: This figure shows the share of individuals with any insurance coverage (at the time of survey) by age in the American Community Survey (ACS). This includes public and private insurance but does not include insurance from the Indian Health Service. We show data separately for years 2012−2017.

and administrative data from hospital discharge records. Meanwhile, Barcellos and Jacobson (2015) find no evidence of changes in utilization in the Medical Expenditure Panel Study (MEPS). Because prior studies have shown clear reductions in out-of-pocket spending across the coverage discontinuity, increases in utilization upon Medicare eligibility would not preclude finding improvements in financial outcomes. Our preferred specification for the regression discontinuity (RD) framework is a polynomial in age that allows both a discontinuity and a trend break after Medicare eligibility:

$$Y_{it} = f(age_{it}) + \beta Post_{it} + g(age_{it})Post_{it} + \alpha + \epsilon_{it}. \tag{2}$$

Y_{it} is the outcome of interest for individual i in year t, $Post$ is an indicator for being above the Medicare eligibility age threshold, $f(\cdot)$ and $g(\cdot)$ are polynomials of the same order, α is a constant term, and ϵ is an error term. The coefficients

of interest are β, which shows the level shift upon becoming eligible for Medicare, and the difference between the coefficients on the polynomials $f(\cdot)$ and $g(\cdot)$, which show the changes in any age-related trends. Following Gelman and Imbens (2017), we avoid higher-order polynomials and present baseline results using either linear or quadratic polynomials, depending upon whether the time series exhibit material curvature. Appendix D shows that our key results are robust to bias adjustment proposed by Dong (2015) to account for the discrete nature of our running variable and to employing a local linear regression discontinuity model. We estimate the models from ages 55 through 74, but in some cases also test robustness with shorter windows around the discontinuity. We also split the sample using county-level characteristics to test for heterogeneous effects across pre-65 insurance coverage rates and household income.

As in the dependent care mandate analysis, we omit year in which individuals turn 66 because it is unclear whether medical collections reported in this year stem from care that was delivered before or after Medicare eligibility. Because effects on some credit outcomes might appear faster on credit records than medical collections we also test specifications that omit age 65 or include age 66 and find our results to be robust (estimates not shown, but we do include age 66 in figures for comparison).

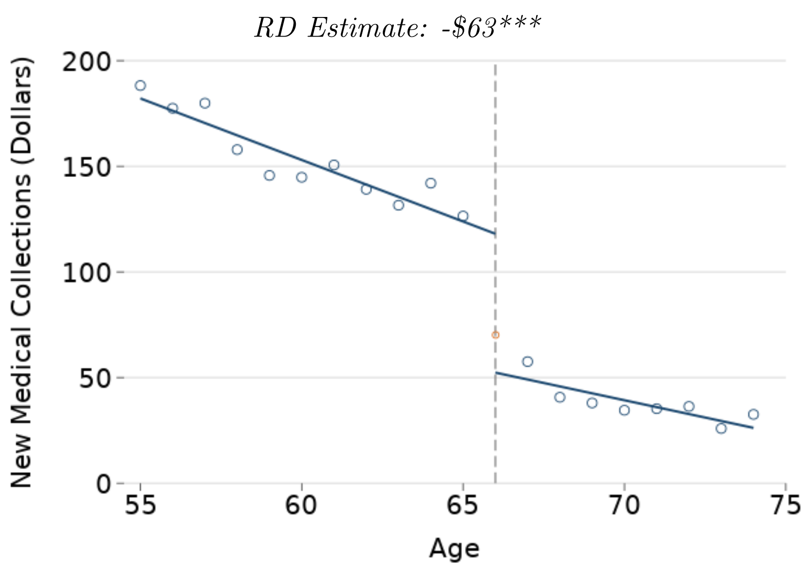
C. Medicare Regression Discontinuity Results

For our primary results we show RD estimates from 2013, the year preceding the enactment of provisions of the ACA relevant to this age group when changes in the share of people with insurance coverage before Medicare eligibility began to change.

Effects on Medical Collections. — Figure D1 shows a $63, or 56 percent, reduction in the unconditional dollar value of medical collections. This is similar to the reduction in medical collections from the dependent care mandate: $40 for

a 5.9 percentage point gain (\$6.72 per percentage point gain) for dependent care versus \$63 for a 9.6 percentage point gain (\$6.56 per percentage point gain). If we assume that the effect stems entirely from people who gain insurance coverage under Medicare, as opposed to lower cost sharing or lower prices negotiated by Medicare compared to the pre-65 insurer, this would represent a \$658 per person reduction in medical collections. In Appendix D we show that the estimated reductions in medical collections are concentrated among larger bills and are larger for areas with larger increases in insurance rates following Medicare eligbility. Reductions in medical collections are smaller in areas with higher baseline insurance rates and over time as the ACA reduces extensive margin increases at age 65. As with the under-26 provision, we find particularly large and and clear effects on the size of medical collections and a more modest effect on the number of consumers incurring any medical debt in collections.

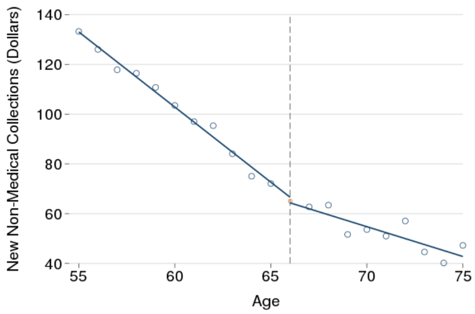
Figure 8. : Mean Dollar Value of Medical Collections, 2013



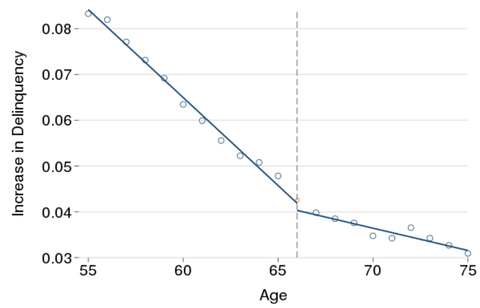
Note: Figure and regression discontinuity estimate are generated from evaluating Equation 2 linear polynomial in age. We omit consumers who turn 66 in a year when calculating polynomials on either side, but we illustrate the age 66 value in our figures as a red circle. Data are from the CFPB CCP for 2013. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Effects on Broader Financial Outcomes. — Seeing clear reductions in the amount of medical debt in collections after Medicare eligibility, we now ask whether this translates to an improvement in broader measures of financial health as with the dependent care mandate analysis. We start with non-payment of bills not directly related to medical expenses by looking at effects on the annual flow of non-medical collections and on changes in delinquencies on credit products. In stark contrast to medical collections, figure 9 shows little evidence that timely payment of other bills increases after Medicare eligibility. Discontinuities are virtually nonexistent in 2013 for non-medical collections (panel a), and standard errors rule out more than small declines. Panel b also suggests there is not a discontinuous drop in delinquencies on credit products, but this is somewhat sensitive to the specification and year. Models that look at smaller windows around the discontinuity (not shown), or at the bias-adjusted specifications in Appendix D, sometimes suggest a small decrease in delinquencies around Medicare eligibility. Appendix figures D5 and D7 show little evidence that these effect sizes change with variation in extensive margin increases in insurance coverage. Additionally, in appendix figure D6 we split the sample by county-level income rather than insurance coverage rates and, again, do not find evidence of effects concentrated in poorer areas. Taken together, this suggests Medicare eligibility may result in a small decrease in delinquencies on credit products, but this does not appear to be tied to the increase in coverage as we saw with the incidence of medical collections where the change is also larger and more sustained.

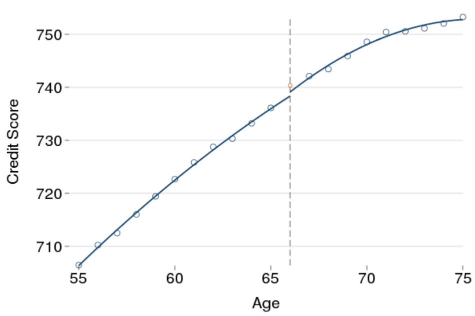
Figure 9. : Non-Medical Collections Outcomes



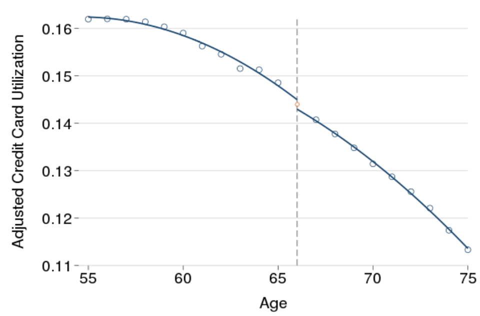
(a) Unconditional Mean Dollar Value of Non-Medical Collections, 2013. RD Estimate: -$1.01



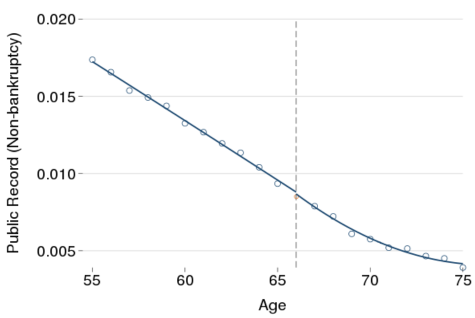
(b) Fraction Becoming More Delinquent on Debt in a Year, 2013. RD Estimate: -0.002



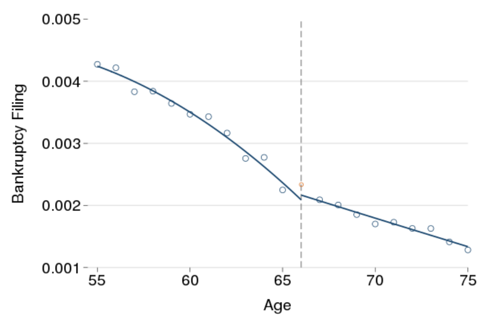
(c) Credit Score, 2013. RD Estimate: 0.42



(d) Credit Card Utilization, 2013. RD Estimate:



(e) Annual Rate of Public Records, 2009-2013. RD Estimate: -0.00007



(f) Annual Rate of Bankruptcy Filing, 2009-2013. RD Estimate: 0.00012

Note: This figure shows CFPB CCP outcomes on credit reports that are not directly related to medical care. Delinquencies are any increase in delinquency status conditional on at least 30 days of delinquency on installment or revolving credit accounts. Vertical bars indicate 95 percent confidence intervals. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Panels (c) and (d) of figure 9 show results for credit score and utilization (Ap-

pendix D shows results for credit limit that are qualitatively similar).[20] Credit score and credit utilization evolve smoothly across the Medicare coverage discontinuity, and confidence intervals rule out more than very small improvements (see panels (c) and (d) of appendix figure D7). Because credit score reflects payment history and other past credit events, and credit utilization may reflect past accumulation of debt, we might expect these metrics to improve gradually after 65. However, improvement in credit scores instead modestly slows after Medicare eligibility, and there is very little trend break for credit utilization. We also see little evidence of meaningful differences in either result when focusing on areas with high rates of uninsurance or low household income Appendix D. The implementation of the ACA does not substantially alter our estimates for credit scores, and in all cases we can rule out meaningful increases relative to the average score of 65 year-olds (around 740). The same is true for credit utilization.

Similarly, panels (e)-(f) show very little evidence of changes to public records or bankruptcy. The RD estimates for public records are essentially zero both overall and for the targeted populations (Appendix D). Note that we pool 2009-2013 to help overcome the noise in the data for these relatively rare events[21] and our analysis of public records ends in 2016 because the reporting standards for civil judgments and tax liens changed starting in 2017.[22]

Taken together, we see little evidence that Medicare eligibility, and subsequent increases in insurance coverage, lead to improvements in credit outcomes outside of medical collections.

[20]Credit limit and credit utilization exhibit persistent variation across some birth cohorts that interfere with isolating the effect of Medicare eligibility. See Appendix D6 for relevant examples. To address this, we first pool data from 2012-2018 to estimate utilization and limit as functions of age fixed effects and birth cohort fixed effects. We then estimate Equation 2 each year on a version of the outcome variable that has been stripped of these cohort effects.

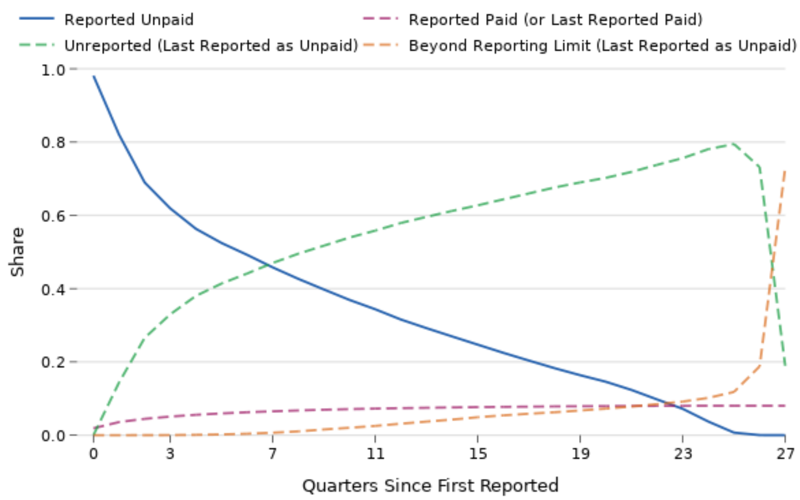
[21]In general, we do not show results for other outcomes for earlier years because the data first distinguish between medical collections and other collections in 2012.

[22]Due to a settlement with a large group of State Attorneys to improve the accuracy of information in credit reports, the major credit reporting bureaus dramatically reduced the reporting of civil judgements and tax liens in 2017 and stopped reporting them in 2018 (Clarkberg and Kambara, 2018).

V. Persistence of Medical Debts in Collections

We conclude by exploring a potential mechanism to explain the apparent lack of spillovers between medical collections and other credit outcomes. Specifically, we investigate how long medical collections actually appear on credit records before they are either removed or reported as paid. In order to show the full potential reporting period, we restrict our analysis to collections reported as stemming from non-payment in 2011, 2012, and the first half of 2013.[23]

Figure 10. : Persistence of Medical Collections on Credit Reports



Note: This figure shows the status of medical debt in collections that was incurred in 2011 through the first half of 2013 by quarter since it was first reported on a consumer's credit record in the CFPB CCP. The solid blue line represents the share of those medical collections that are still reported as unpaid. This share falls over time as bills are reported as paid (purple dashed line), they reach the seven year reporting threshold (orange dashed line), or they have not been paid but are no longer reported to the consumer reporting agency for other reasons (green dashed line).

As shown in Figure 10, most medical collections are never reported as paid.

[23]To account for any transfers of collections between different furnishers, we group all collections based on the original collection amount and when the collection opened for each consumer. Using a broader, but overly inclusive, definition of same collection where we group all medical collections for a consumer within the same month shows patterns like those in Figure 10 (not shown) but the share of medical collections ever reported as paid is about one percentage point higher.

Only about eight percent of medical collections are ever reported as paid, and the majority of those are paid within the first year or two. Instead of remaining on credit records until they are required to be removed, however, the still unpaid medical collections begin dropping off relatively quickly. More than half no longer appear in the credit data within seven quarters of first appearing, and only one percent ultimately reach the seven-year threshold for removal.[24] To confirm this pattern is not specific to 2011-2013, we also consider more recent collections. Specifically, we follow collections opened in 2014-2016 for four years and find a similar pattern, although with slightly lower payment rates and decreased persistence for unpaid medical collections (not shown).

The low payment rates mute one channel through which insurance coverage expansions, and their subsequent effects on medical collections, could improve other credit outcomes directly. The low persistence of medical collections suggests that the damage done from their presence on credit reports may be relatively short-lived, so perhaps it is unsurprising we do not find large effects for other credit outcomes.

VI. Conclusion

We have shown that two exogenous increases in health insurance sharply reduce medical debt in collections but result in few additional improvements to financial distress observable in credit reports. Despite covering populations with different healthcare needs and financial resources who gain different types of coverage, we find very similar results around both the dependent care mandate and Medicare eligibility. This may, among other things, reflect broadly similar cost sharing for patients who gain coverage through the policies studied here in contrast to Medicaid where the literature has found broader improvements in financial well-

[24]Some of these collections that are never reported as paid may no longer be reported because the furnisher determined the collection was inaccurate. Additionally, while some of these unreported collections may simultaneously be paid and no longer reported, this analysis still suggests a large share of medical collections are not ultimately paid, which is consistent with shorter-term analysis of repayment of collections of various types and furnishing practices (Consumer Financial Protection Bureau, 2014).

being on credit records.

To provide context for these results, and others in the literature, we provide new evidence about medical collections on credit reports. Namely, reported repayment rates of medical collections are generally low and their presence on credit reports is relatively short-lived. This may reflect a host of factors, including the relative priority placed on these debts by consumers in the face of other obligations or the limited recourse available to debt collectors. Regardless of the underlying causes, these facts could mute one important channel through which insurance coverage expansions, and their subsequent effects on medical collections, directly affect the financial outcomes studied here.

The set of policy options that could mitigate the financial consequences of health events is likely quite broad. Health insurance with low cost sharing would limit most of the financial risk for patients, and studies of Medicaid have generally found wider financial benefits. Alternatively, policymakers could aim to increase the amount of financial risk individuals can bear. Policies that increase income, promote emergency savings, or improve broader personal finances may prove at least as important as health insurance in this regard. For instance, Dobkin et al. (2018) argue that lost income following an adverse health event may play a larger role than medical bills in subsequent financial hardship, suggesting that policies like unemployment insurance could play a role in avoiding financial distress. Of course, the relative merits of these proposals involve considerations that extend far beyond the scope of this paper, as do the potential health, psychological, and other benefits of these insurance expansions.

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Appendix A: Effects of the Great Recession Differ by Age

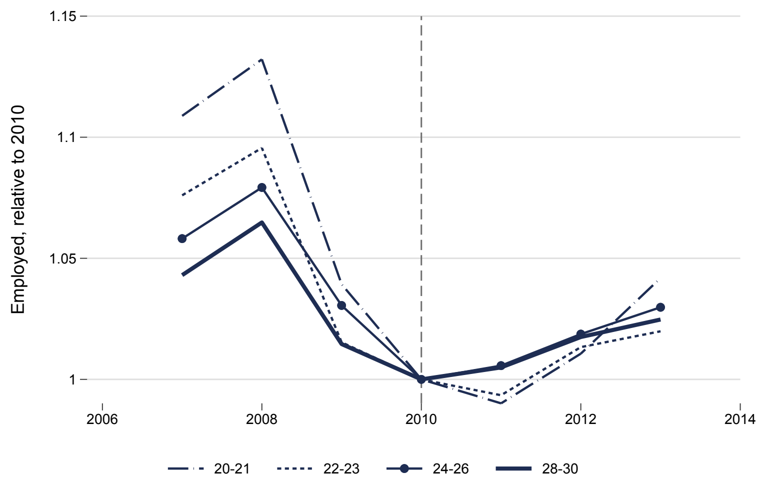
Our analysis of the dependent care mandate is complicated by the fact that it was implemented during the early stages of the recovery from the Great Recession. We turn to data from the ACS to understand how the recession and subsequent recovery affected key economic outcomes of young adults. This exercise helps us ensure that we select reasonable treatment and control groups for our CCP analysis. Because the data we use do not contain minors, they are not a control candidate for the youngest treated group.

Figure A1 uses ACS data to show trends in employment and income (relative to 2010) for young adults in 2007 through 2013. We exclude those aged 27 for consistency with our analysis of the CCP. Employment and income trends differ substantially across ages, especially for the youngest adults we analyze. In particular, younger adults experienced larger relative declines in employment and income, and the trough of the recession occurred one year later. In short, those aged 28-30 (the closest possible control group in the CCP) are likely a very poor control for the youngest adults affected by the dependent coverage mandate. Even without the recession, many fewer of the youngest adults are in the labor force and financially independent, so intuitively they are not an ideal match for people approaching 30. For this reason we exclude 20-21 from our CCP analysis.

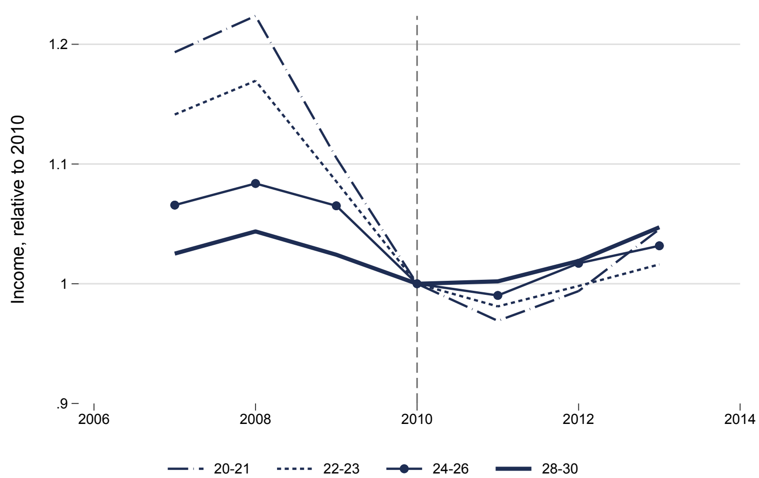
However, the declines in employment and income leading up to 2010 are considerably more similar for those aged 24-26 and those aged 28-30. In addition, the pace of recovery in the subsequent years is nearly identical. Thus, we primarily focus on a comparison of outcomes for these two age groups to understand the effects of the under-26 provision. The employment and income trends of 22-23 year-olds fall in between those of the ages on either side. Because the trends are not wildly different from the control group, we include 22-23 as a separate treatment group and allow the trends in CCP outcomes before the dependent care provision to speak to the validity of the comparison. In general, we find the comparison to be informative, although we temper the interpretation in places.

Figure A1. : Employment and Income by Age over the Great Recession

Panel A: Fraction Employed by Age, 2007-2013



Panel B: Average Income by Age, 2007-2013



Note: This figure shows the fraction employed and the average income by age during and after the Great Recession. Data are from the 2007-2013 American Community Survey 1-year estimate person record PUMS.

Appendix B: Insurance Coverage Transitions at Medicare Eligibility

Medicare causes a stark change in the insurance coverage of Americans at age 65, both in terms of coverage rate and coverage type. In this appendix, we illustrate insurance coverage changes at 65 in more detail and discuss our estimates presented in the main text. We conclude that although the majority of the Medicare population has supplemental coverage, most who were uninsured before Medicare do not. As a result, we believe the effects are driven by those transitioning from uninsured to insurance with non-trivial cost sharing.

Insurance changes at age 65 on both the intensive and extensive margins. The formerly uninsured experience extensive margin increases in coverage, while the majority of the formerly insured change their coverage to Medicare. While some new Medicare beneficiaries face the full cost sharing schedule ($1,300 part A deductible, 20 percent part B coinsurance, and no limit on out-of-pocket spending), most enrollees have supplemental coverage that reduces cost sharing. Supplemental coverage comes from a variety of sources, including former employers or unions, Medicaid, and privately purchased Medigap plans. Those who enroll in Medicare Advantage plans also typically have coverage for additional services and altered cost sharing structures. The amount of additional protection varies from covering some subset of additional services (e.g., Medicare Advantage) to effectively eliminating cost sharing (e.g., those dual-eligible for Medicare and Medicaid).

We are particularly interested in the post-65 coverage of those who were uninsured prior to age 65. Evidence from the introduction of the ACA (shown in Figure D4) and cross-sectional variation in pre-65 coverage rates strongly suggests that those gaining insurance coverage at age 65 drive the effects. To provide context for these results, it is important to understand what kind of cost sharing these formerly uninsured face under Medicare. If, for example, nearly all became dual-eligible for Medicare and Medicaid, we might expect the effects of turning 65 would be more similar to those of Medicaid expansions studied in prior work.

To understand these transitions, we use data from the Health and Retirement

Study (HRS), which is a longitudinal panel study of Americans over the age of 50.[25] Individuals included in the HRS (and their spouses) are interviewed every two years and asked a number of questions about health insurance coverage. Because of the panel structure of the HRS, we can identify individuals who were uninsured before age 65 and track their coverage once they become Medicare eligible. We include spouses in our analysis and use data from waves 6 through 12, corresponding to years 2002-2014.

Individuals are deemed uninsured if they report no public coverage, have zero private insurance plans, and do not have any “other” coverage. Because the HRS surveys individuals every two years, those without coverage at age 63 or 64 are considered to have been uninsured immediately preceding Medicare eligibility. Twelve percent of our sample was uninsured when observed at age 63 or 64. We further segment this group into those who also reported no health insurance in the prior survey wave (i.e. at ages 62 or 63) to capture those with more-sustained spells of uninsurance.

For context, we show uninsured rates of each group, by age, in panel (a) of Figure B1. Note that we define groups based on their coverage at ages 63 or 64, so the very high and low uninsured rates at those ages are mechanical. Unsurprisingly, once individuals reach age 65, uninsured rates drop precipitously for the formerly uninsured. However, it is worth noting that around half of those uninsured immediately preceding Medicare eligibility have coverage in the prior waves. It is possible that those with shorter and longer spells of uninsurance before Medicare eligibility experience different coverage transitions at 65.

Coverage rates across these groups rapidly converge at age 65, but substantial differences in the types of coverage persist. In panel (b) of Figure B1 we show the fraction of individuals with at least two reported sources of insurance (Medicare, Medicaid, private plan(s), or other insurance). For those with insurance prior to

[25] Specifically, we use the RAND HRS Longitudinal File, which includes a substantial amount of data cleaning and processing.

Medicare, roughly 60 percent report at least two sources of coverage after 65.[26] That number is under 30 percent for those without coverage at 64, and still lower for those who reported longer spells of uninsurance.

We can further disentangle the alternative sources of coverage to shed light on the likely cost sharing faced by these different groups. In Figure B2 we show the evolution of Medicaid and private coverage surrounding Medicare eligibility. We take particular interest in Medicaid coverage because of its lack of cost sharing. Panel (a) shows very stable rates of Medicaid coverage among the previously insured, but a clear increase at age 65 for those formerly uninsured. The same is true if we restrict the sample to those who were uninsured for at least two waves. We note, however, that the magnitude is fairly modest—about 12 percent of those previously without insurance have Medicaid at age 66.

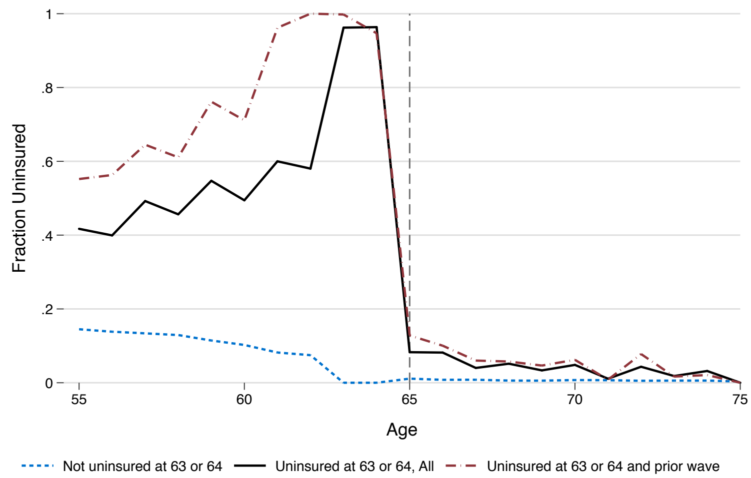
Unsurprisingly, those who lacked coverage before Medicare also had fewer private insurance plans at other ages (panel b). Some previously uninsured report private insurance plans but the rate is considerably lower than those who had coverage. On average, formerly uninsured have 0.2 private insurance plans while on Medicare.

Overall, only about a quarter of the newly insured population have supplemental coverage beyond Medicare. More importantly, only half of those are covered by Medicaid. Thus, the modal newly insured Medicare enrollee is of a patient exposed to significant cost sharing, making the increase in coverage materially different from Medicaid expansions.

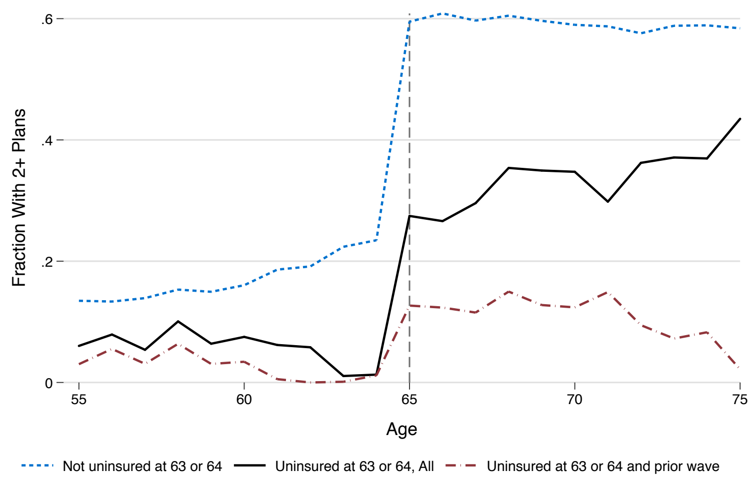
[26] This is similar to the trends from the NHIS shown in Card, Dobkin and Maestas (2008).

Figure B1. : Insurance Coverage by Pre-65 Uninsured Status

Panel A: Fraction without Coverage



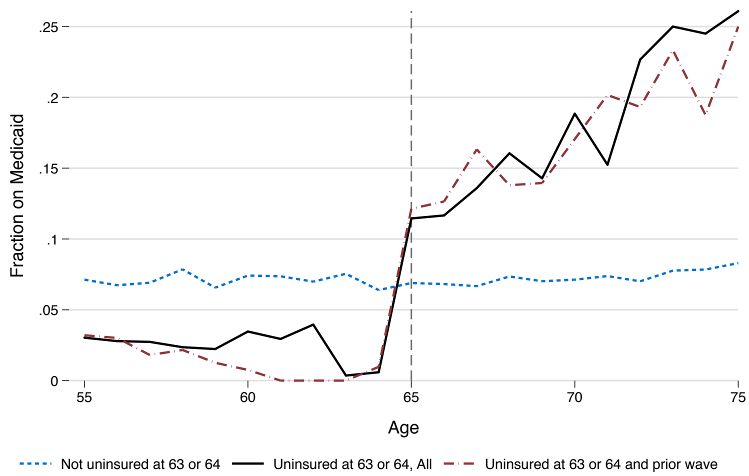
Panel B: Fraction with Multiple Sources of Coverage



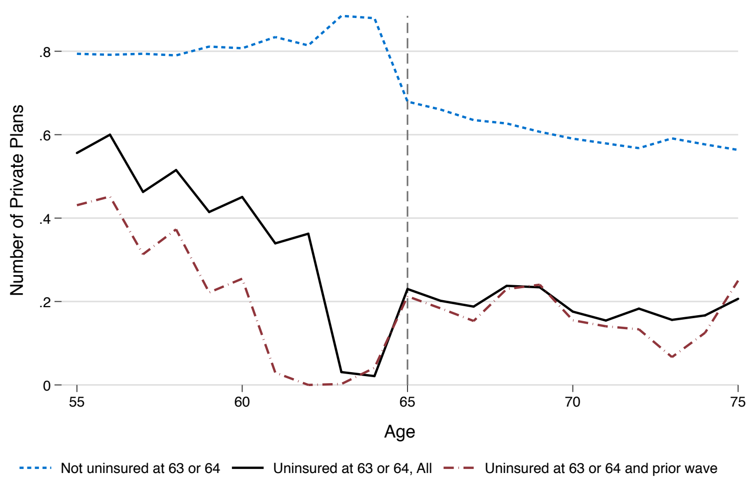
Note: This figure shows uninsured rates by age for groups defined by their rates of uninsurance preceding Medicare eligibility (at age 63 or 64). Data are from the Health and Retirement Study for years 2002-2014 and includes spouses (if applicable). Individuals are uninsured if they report no public coverage, have zero private insurance plans, and do not have any "other" coverage.

Figure B2. : Private and Medicaid Coverage by Pre-65 Uninsured Status

Panel A: Medicaid Coverage



Panel B: Number of Private Plans



Note: This figure shows uninsured rates by age for groups defined by their rates of uninsurance preceding Medicare eligibility (at age 63 or 64). Data are from the Health and Retirement Study for years 2002-2014 and includes spouses (if applicable) .

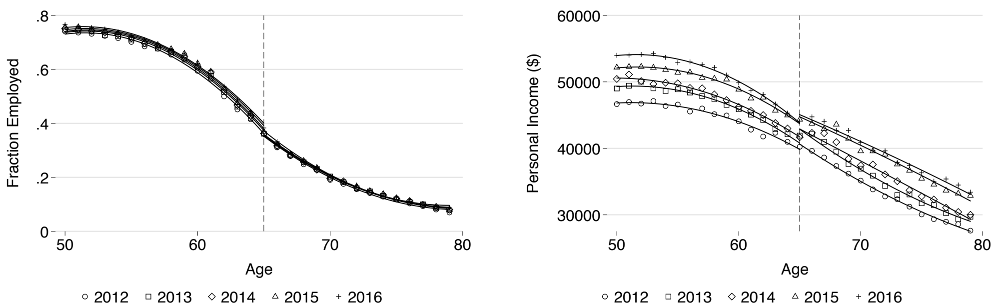
Appendix C: Trends in Confounding Variables across Medicare Eligibility

The validity of our regression discontinuity research design relies on the assumption that there are no other abrupt changes at age 65 beyond Medicare that could affect the outcomes we study. Prior work has generally found little evidence of such changes (Card, Dobkin and Maestas, 2008; Barcellos and Jacobson, 2015), but we reproduce similar analyses testing for discontinuities in potential confounding variables with more recent data. Given the credit outcomes we focus on, any changes in financial resources present the most likely threat to identification. If Medicare eligibility is coincident with other improvement (or deterioration) in financial health, we would expect to see improvement (or deterioration) in credit outcomes absent Medicare.

For this exercise we use data from the American Community Survey for years 2012-2016, accessed through IPUMS (Ruggles et al., 2018). In both cases, we use quadratic age polynomials interacted with an indicator for being over 65, and cluster on age. Figure C1 presents trends in two key variables: the fraction of people who are employed (panel a) and average personal income (panel b) by age for 2012-2016. Visual inspection shows that, while incomes and employment change substantially across this age range, they have relatively minor changes at age 65.

Table C1 reports our regression discontinuity estimates for these (and other) outcomes by year. In general, we estimate small decreases in most years for employment, labor force participation, and hours worked, with slight jumps in income. This leads us to believe that the change in coverage is the dominant discrete change upon turning 65, which supports our identifying assumption.

Figure C1. : Employment and Income by Age, 2012−2016



(a) Fraction Employed

(b) Personal Income

Note: This figure shows the fraction of adults reporting current employment (panel a) and average personal income (panel b) by age for 2012-2016. Data are from the American Community Survey.

Table C1—: RD Estimates for Employment Measures Surrounding Age 65

	2012	2013	2014	2015	2016
Fraction Employed	0.015	0.007	-0.0006	-0.002	-0.002
	(0.0118)	(0.0085)	(0.008)	(0.012)	(0.007)
Personal Income ($)	419.53	1,802.70	691.17	1,085.61	1,145.19
	(415.22)	(581.88)	(653.25)	(365.16)	(509.64)
Hours Worked/Week	0.384	0.339	0.001	-0.040	-0.023
	(0.397)	(0.307)	(0.269)	(0.329)	(0.265)
Not in Labor Force	-0.019	-0.009	-0.0009	-0.0009	0.0005
	(0.013)	(0.009)	(0.008)	(0.012)	(0.008)

Note: This table shows regression discontinuity estimates at age 65 from models including quadratic polynomials in age, interacted with a dummy for age 65 or higher. Each cell is the RD estimate at age 65 for a different regression. Data are from the American Community Survey for years 2012-2016, accessed through IPUMS (Ruggles et al., 2018). Sample sizes in each year are between 1.06 million and 1.14 million. Standard errors, shown in parentheses, are clustered by age.

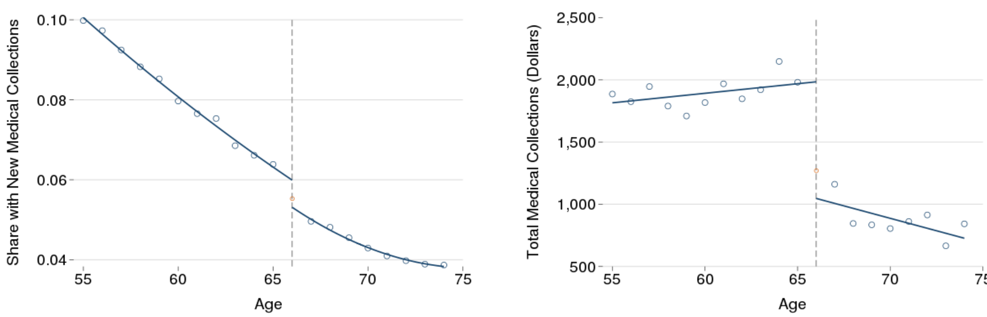
Appendix D: Additional Medicare Analysis

This section includes additional analysis surrounding Medicare eligibility. We disentangle the extensive and intensive margin contributions to medical collections results, explore cross sectional and temporal variation in insurance increases after age 65, and consider alternative specifications for our results.

D1. Medical Collections Results

Figure D1 disaggregates the overall effect on average medical collection flows into its two parts: the fraction of consumers who incur a collection (panel a), and the mean dollar value of collections, conditional on incurring one (panel b). Panel (a) shows an estimated decrease of 0.7 percentage points in the fraction of people with at least one new medical collection in 2013, which corresponds to a 12 percent decrease from the pre-Medicare trend. Panel (b) shows the mean dollar value of new medical collections, conditional on having any decreased by \$903, or 47 percent.

Figure D1. : Medical Collections, 2013



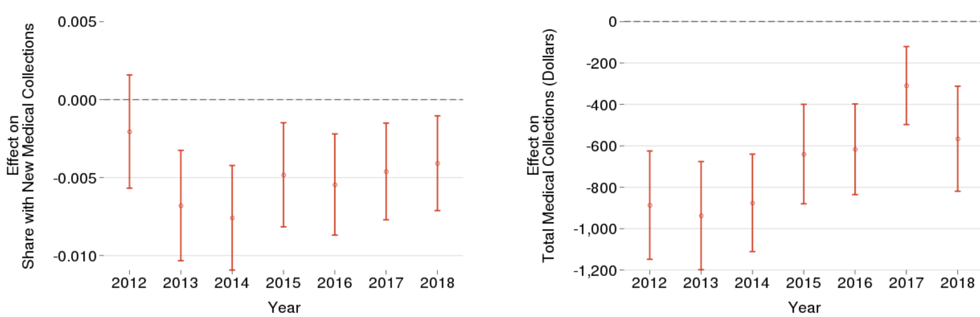
(a) Fraction with Any Medical Collections. *RD Estimate: -0.0068****

(b) Mean Dollar Value of Non-zero Medical Collections. *RD Estimate: -\$937****

Note: Figures and regression discontinuity estimates are generated from evaluating Equation 2 with quadratic (panel a) and linear polynomials (panel b) in age. We omit consumers who turn 66 in a year when calculating polynomials on either side, but we illustrate the age 66 value in our figures as a red circle. Data are from the CFPB CCP for 2013. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure D2 shows RD estimates for both metrics by year. Panel (a) shows that the effect of Medicare on the fraction of adults with a medical collection. While the estimate is unusually small in 2012, the remaining years follow a broadly consistent pattern. Estimated effects decline around 30 percent from 2013 to the post-ACA years, though they are not distinguishable across years. Panel (b) illustrates a very similar trend to that of the unconditional mean of medical collections. Before the ACA, we estimate that Medicare eligibility reduced the average size of medical collections by \$937, but after the ACA's implementation, this effect fell to an average of \$497 in 2016-2018, a reduction of 47 percent. Taken together, this shows that the increase in coverage under the ACA by those approaching 65 reduces the size of medical collections, but has a smaller effect on the fraction incurring them.

Figure D2. : Effects of Medicare Eligibility by Year



(a) Fraction with Any Medical Collections

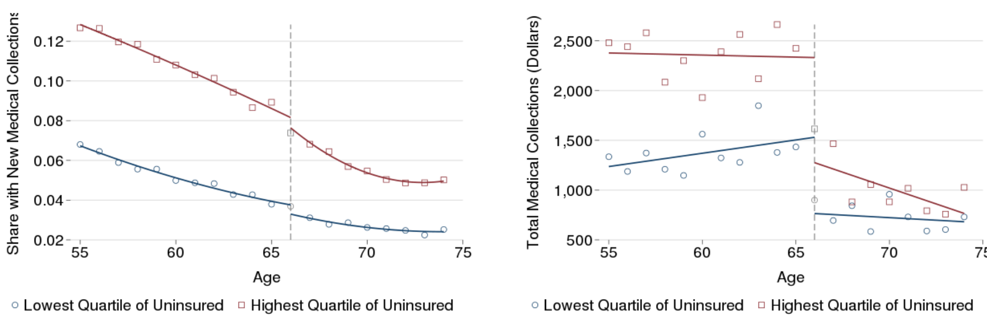
(b) Mean Dollar Value of Non-zero Medical Collections

Note: Figures and regression discontinuity estimates are generated from evaluating Equation 2 with quadratic (panel a) and linear polynomials (panel b) in age. Credit data are from the CCP for years 2012−2018. Vertical bars indicate 95 percent confidence intervals.

In Figure D3 we show the effects of Medicare eligibility on medical collections in counties where either a high or low fraction of the under-65 population is uninsured. Panel (a) shows that in 2013, the estimated reduction in the frequency of medical collections is slightly larger in counties with the most uninsured patients, but it is not significantly different from either zero or the estimate for the

high coverage counties. In other years we observe larger point estimates for areas with high uninsured rates relative to those with low uninsured rates, though confidence intervals generally overlap. Panel (b) shows that both high and low coverage counties experience sharp declines in the average size of medical collections (conditional on having a medical collection). In this case, the difference between high and low coverage counties is clear, with the effect being 40 percent larger for the latter. Our bias-adjusted results (shown later in this section) similarly show small differences between high and low coverage counties on the extensive margin for medical collections but significantly larger decreases on the intensive margin for relatively low coverage counties.

Figure D3. : Medical Collections in Top and Bottom Quartile of Uninsured Rates, 2013



(a) Fraction with Any Medical Collections–Quartile of Uninsured. RD Estimates (Top and Bottom Quartiles): -0.0052 and -0.0046

(b) Conditional Mean Dollar Value of Medical Collections–Quartile of Uninsured. RD Estimates (Top and Bottom Quartiles): -\$1,055*** and -\$765**

Note: Figures and regression discontinuity estimates are generated from evaluating Equation 2. We split the sample into the top and bottom quartiles of county uninsured rate. Collections data are from the CFPB CCP for year 2013, and insurance coverage are from the Census's SAHIE. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure D4 shows how the effect of Medicare on the unconditional average level of medical collections changes as the ACA begins providing coverage to people under 65. We estimate the model each year from 2012 to 2018 and plot the resulting

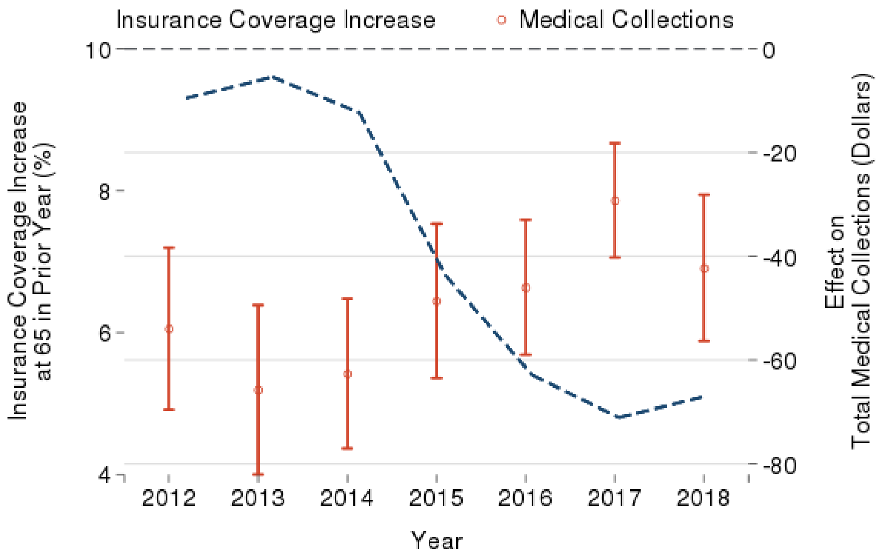
estimates along with the increase in insurance coverage for the prior year.[27] We estimate between a $55 and $65 decrease in the average dollar amount of medical collections upon becoming eligible for Medicare in 2012 through 2014, and then the effects decrease in subsequent years in step with the coverage increase at age 65. Once the ACA-driven coverage increase for this population stabilized after 2015, we find an average reduction in medical collections of $39, representing a roughly 40 percent decrease from our baseline estimate in 2013. This is broadly similar to the 47 percent decrease in the coverage discontinuity at 65 over the same time period and is further suggestive evidence that coverage increases at Medicare eligibility, as opposed to changes in type of coverage, drive the decline in medical debt in collections.

We also note that the reductions in medical debt in collections are concentrated among the highest values of debt. Table D1 shows the size distribution of total medical collections incurred in 2013 for people between ages 61-71. Most of the reduction in the frequency of medical collections comes from the larger amounts. For example, the share of 67 year-olds with total collections up to $200 is only 5 percent lower than for 65 year-olds, but the gap increases to 25 percent for total medical collections between $200 and $1,000, 31 percent for total medical collections between $1,000 and $3,000, and 57 percent lower for total medical collections above $3,000.

Finally, in the main text we emphasize results using data from 2013 using our full sample of credit records. In this section we include two additional sets of results, both of which we reference in the main text. Figure D5 shows how our estimates differ for counties with high and low uninsured rates for those below 65. If extensive margin increases in coverage trigger meaningful spillovers to non-medical collection measures, we expect them to be biggest in the areas with

[27]We align estimates generated using the CCP for a given year with ACS coverage rates from the year prior. For example, the ACA Medicaid expansion and individual market provisions began increasing pre-65 coverage rates in 2014, but we show this decline in the coverage increase at 65 aligned with the 2015 CCP outcomes. This is analogous to omitting age 66 rather than 65 in our regression models because of the lag between care delivery and reporting an unpaid bill.

Figure D4. : Effects of Medicare Eligibility on the Unconditional Mean Dollar Value of Medical Collections, by Year



Note: This figure shows the regression discontinuity estimates from Equation 2 for unconditional mean medical collections, by year using linear age polynomials. Credit data are from the CFPB CCP for years 2012-2018. The figure also includes the coverage increase each year at Medicare eligibility. Coverage data are from the ACS for years 2011-2017 and shifted one year later in this figure to align with the delay required for a collection to show up on a credit report. Vertical bars represent 95 percent confidence intervals.

Table D1—: Size Distribution of Medical Collections, 2013

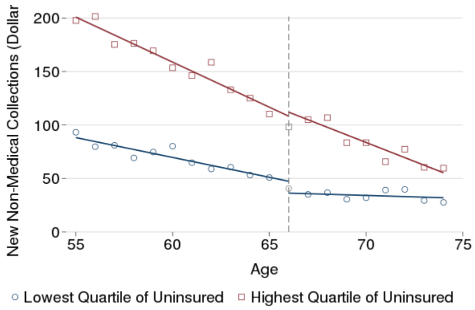
Annual Medical	Percent of Consumers by Age					
Collections (Dollars)	61−63	64	65	67	68	69−71
0	92.7	93.4	93.6	95.0	95.2	95.7
1−199	2.2	2.0	2.1	2.0	2.0	1.9
200−999	2.8	2.5	2.4	1.8	1.7	1.5
1,000−2,999	1.4	1.3	1.3	0.9	0.9	0.7
3,000+	0.8	0.8	0.7	0.3	0.2	0.2

Note: Data are from the CFPB CCP for 2013.

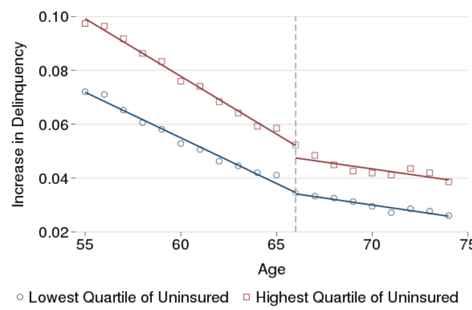
lower baseline rates of coverage. We also split the sample by income to focus on where the effects may be particularly large (figure D6). Although there is overlap between counties with high and low insurance and median household income, the two cuts are distinct. For example, some areas have low income but

comparatively higher rates of coverage due to Medicaid. In Figure D6 we see no evidence that effects for broader financial outcomes are systematically different for areas in the top and bottom quartiles of median household income. In figure D7, we report the results of re-estimating our RD specification separately for 2012-2018. Beginning in 2014, the ACA increased coverage among non-elderly Americans, thereby decreasing the size of coverage increases after age 65. If Medicare eligibility leads to improvements in these credit measures, we expect them to be largest prior to the ACA (and for effects to decrease over time). Instead, we generally see muted effects in 2013 with little evidence that they systematically vary with the size of the discontinuity in coverage across years.

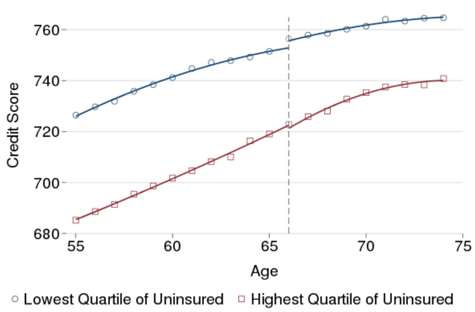
Figure D5. : Non-Medical Collection Outcomes, by Uninsured Quartile



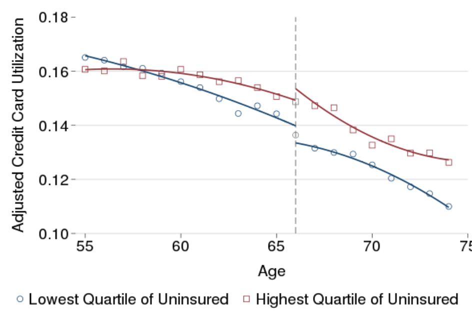
(a) Unconditional Mean Dollar Value of Non-Medical Collections, 2013. RD Estimates (Top and Bottom Quartile of Uninsured): \$3.77 and -\$11.10



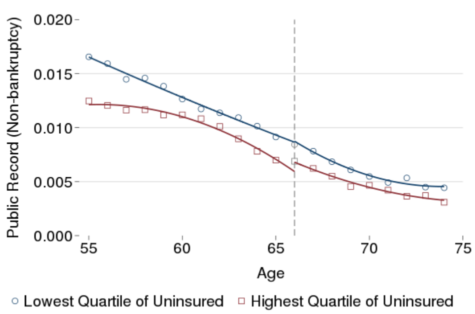
(b) Fraction Becoming More Delinquent on Debt in a Year, 2013. RD Estimates (Top and Bottom Quartile of Uninsured): -0.0046* and -0.0005



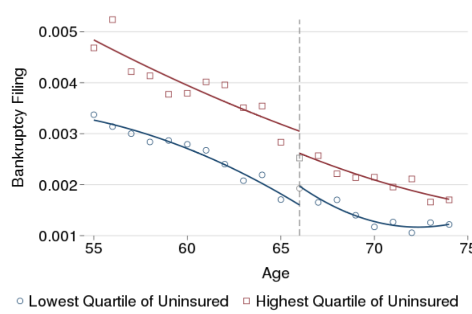
(c) Credit Score, 2013. RD Estimates (Top and Bottom Quartiles of Uninsured): 2.79* and -1.21



(d) Credit Card Utilization, 2013. RD Estimates (Top and Bottom Quartiles of Uninsured): 0.004 and -0.006*



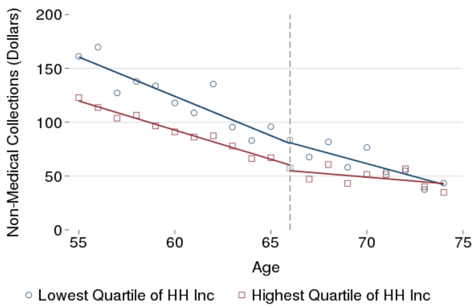
(e) Annual rate of Public Records, 2009−2013. RD Estimates (Top and Bottom Quartiles of Uninsured): 0.0009 and 0.00009



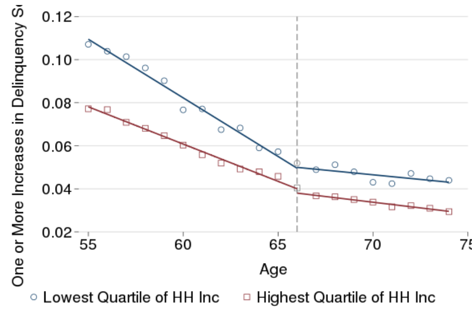
(f) Annual rate of Bankruptcy Filing, 2009−2013. RD Estimates (Top and Bottom Quartiles of Uninsured): -0.0004 and 0.0004

Note: This figure shows CFPB CCP outcomes on credit reports that are not directly related to medical care by county uninsured rate. Data are from the CCP for the years 2012-2018. Insurance data are from the Census's SAHIE. Vertical bars indicate 95 percent confidence intervals. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

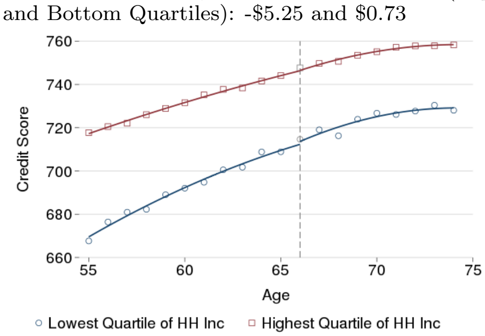
Figure D6. : Non-Medical Collection Outcomes, By Median Household Income



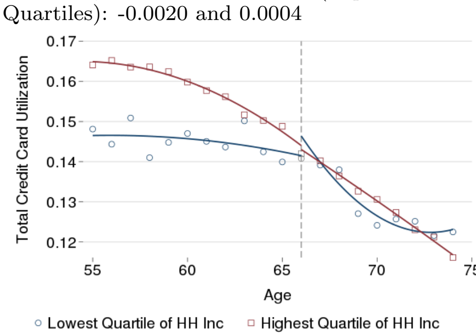
(a) Unconditional Mean Dollar Value of Non-Medical Collections, 2013. RD Estimates (Top and Bottom Quartiles): -\$5.25 and \$0.73



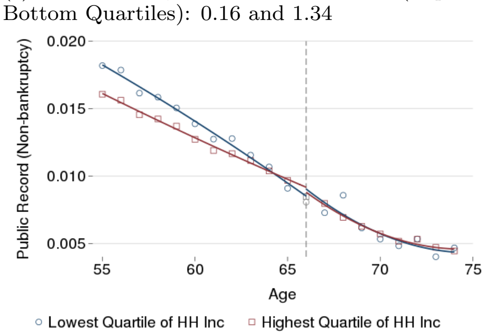
(b) Fraction Becoming More Delinquent on Debt in a Year, 2013. RD Estimates (Top and Bottom Quartiles): -0.0020 and 0.0004



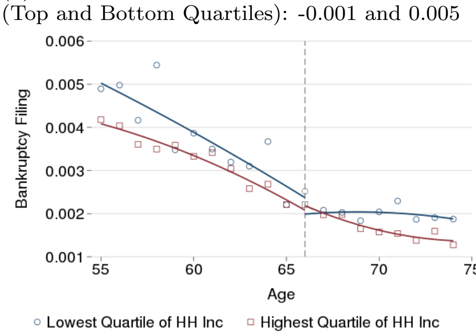
(c) Credit Score, 2013. RD Estimates (Top and Bottom Quartiles): 0.16 and 1.34



(d) Credit Card Utilization, 2013. RD Estimates (Top and Bottom Quartiles): -0.001 and 0.005



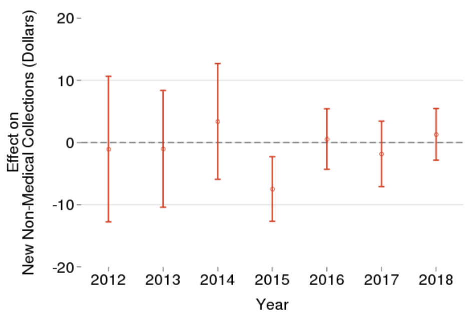
(e) Annual rate of Public Records, 2009-2013. RD Estimates (Top and Bottom Quartiles): -0.0004 and 0.0005



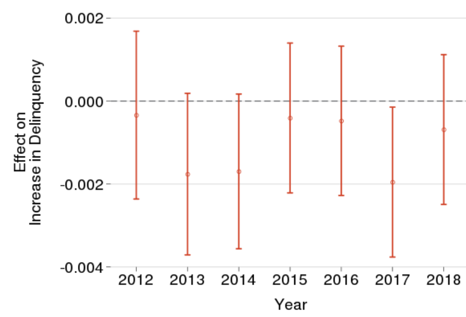
(f) Annual rate of Bankruptcy, 2009-2013. RD Estimates (Top and Bottom Quartiles): 0.0001 and -0.0004

Note: This figure shows non-medical collections outcomes in the CCP. Data are from the CFPB CCP for the years 2012-2018. We have omitted consumers who turn 66 in a year because evidence suggests credit outcomes reported in this year stem from events that occurred both before and after Medicare eligibility. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

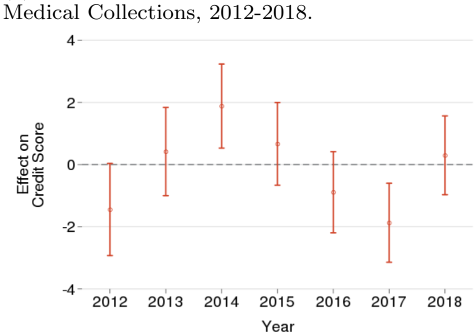
Figure D7. : Non-Medical Collection Outcomes, by Year



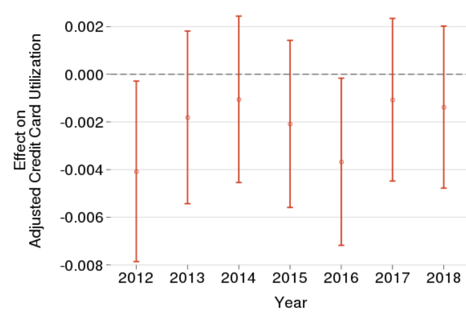
(a) Unconditional Mean Dollar Value of Non-Medical Collections, 2012-2018.



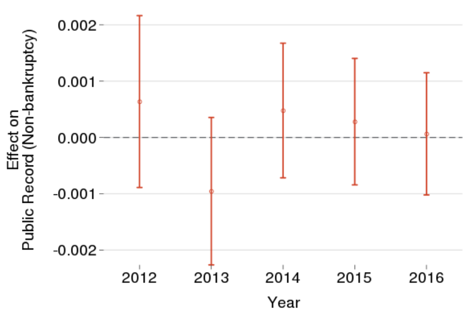
(b) Fraction Becoming More Delinquent on Debt in a Year, 2012-2018.



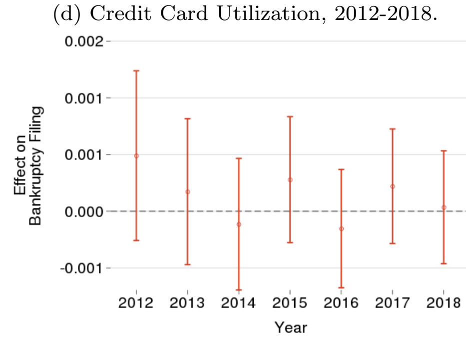
(c) Credit Score, 2012-2018.



(d) Credit Card Utilization, 2012-2018.



(e) Annual rate of Public Records, 2012-2016.



(f) Annual rate of Bankruptcy Filing, 2012-2018.

Note: This figure shows CFPB CCP outcomes on credit reports that are not directly related to medical care by year. Delinquencies are any increase in delinquency status conditional on at least 30 days of delinquency on installment or revolving credit accounts. Data are from the CCP for the years 2012-2018. Insurance data are from the Census's SAHIE. Vertical bars indicate 95 percent confidence intervals. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

D2. Bias Adjustment for Regression Discontinuity with Rounded Data

Since we observe year of birth rather than date of birth we must estimate discontinuities at Medicare eligibility by extrapolating from the nearest integer ages. As noted in Dong (2015) and Lee and Card (2008), rather than applying non-parametric techniques, standard practice for regression discontinuity designs with rounded data is to fit low-order polynomials both above and below the cut-off of the running variable. However, Dong (2015) also describes how these estimates can be biased if the moments of the data are materially different above and below the discontinuity. Tables D2 and D3 show that our main results are robust to the bias correction proposed by Dong (2015). These models are estimated assuming births are uniformly distributed throughout the year, and like our main results, use a 10-year age window above and below the Medicare eligibility discontinuity, though a shorter six-year window has largely similar results (not shown).

Table D2—: Bias-Adjusted RD Estimates, Around Age 65

	2012	2013	2014	2015	2016	2017	2018
Medical Collections							
Fraction with Any Medical Collections	-0.00133	-0.00677***	-0.00770***	-0.00422*	-0.00444*	-0.00374*	-0.00241
	(0.00212)	(0.00205)	(0.00193)	(0.00193)	(0.00187)	(0.00178)	(0.00174)
Mean Dollar Value of Non-Zero Medical Collections	-872.7***	-909.4***	-855.6***	-626.8***	-603.1***	-287.3**	-563.1***
	(141.6)	(141.1)	(127.0)	(129.5)	(118.0)	(101.6)	(137.0)
Mean Unconditional Dollar Value of Medical Collections	-55.85***	-66.97***	-63.90***	-50.05***	-47.10***	-30.00***	-43.80***
	(8.325)	(8.681)	(7.657)	(7.889)	(6.876)	(5.833)	(7.474)
Non-Medical Outcomes							
Mean Unconditional Dollar Value of Non-Medical Collections	-2.576	-2.633	1.937	-8.670**	-1.057	-2.939	0.121
	(6.245)	(4.993)	(4.937)	(2.756)	(2.575)	(2.784)	(2.202)
Increased Severity of Delinquency	-0.00184	-0.00322**	-0.00306**	-0.00164	-0.00161	-0.00325***	-0.00195*
	(0.00108)	(0.00104)	(0.000990)	(0.000958)	(0.000954)	(0.000958)	(0.000957)
Credit Score	-1.694	0.0220	1.922*	0.631	-0.868	-2.352**	0.122
	(0.868)	(0.822)	(0.781)	(0.771)	(0.753)	(0.730)	(0.728)
Credit Limit	-300.0	-176.8	-20.71	-71.97	-107.4	-503.7	-237.6
	(385.5)	(366.4)	(359.6)	(370.1)	(381.9)	(374.8)	(385.8)
Public Records	0.000862	-0.000995	0.000435	0.000327	-0.0000495		
	(0.000892)	(0.000758)	(0.000690)	(0.000651)	(0.000625)		
Bankruptcy	0.000490	0.000119	-0.000117	0.000263	-0.000134	0.000168	-0.00000229
	(0.000437)	(0.000373)	(0.000335)	(0.000322)	(0.000301)	(0.000289)	(0.000286)

Note: This table shows regression discontinuity estimates at age 66 from models based on Dong (2015). Data are from the CFPB CCP for years 2012-2018.
*p<0.05, **p<0.01, ***p<0.001

Table D3—: Bias-Adjusted RD Estimates, Around Age 65, by County Uninsured Rate

	Quartile of uninsured	**2012**	**2013**	**2014**	**2015**	**2016**	**2017**
Medical Collections							
	Low	-0.00142	-0.00437	-0.00112	0.000749	0.000157	0.000296
Fraction with Any		(0.00299)	(0.00289)	(0.00272)	(0.00278)	(0.00263)	(0.00244)
Medical Collections	*High*	-0.00322	-0.00338	-0.00690	-0.00852	-0.0119*	-0.00246
		(0.00512)	(0.00494)	(0.00501)	(0.00585)	(0.00563)	(0.00549)
	Low	-444.8	-747.4**	-838.5***	-371.8*	-158.8	-252.0
Mean Dollar Value of		(264.2)	(238.5)	(234.7)	(164.1)	(148.6)	(170.7)
Non-Zero Medical Collections	*High*	-1393.9***	-1025.7***	-1060.5***	-607.5**	-927.3**	-127.0
		(315.1)	(310.4)	(267.9)	(216.7)	(289.7)	(245.8)
	Low	-16.60	-33.18***	-35.15***	-16.25*	-8.089	-13.44*
Mean Unconditional Dollar		(9.757)	(9.220)	(9.057)	(6.477)	(5.738)	(6.308)
Medical Collections	*High*	-124.8***	-108.2***	-115.3***	-97.34***	-117.3***	-49.55*
		(24.72)	(25.34)	(22.78)	(21.00)	(26.16)	(23.12)
Non-Medical Outcomes							
	Low	-10.91	-12.69*	2.030	-1.369	5.097	3.124
Mean Unconditional Dollar		(7.619)	(6.453)	(6.085)	(3.978)	(4.002)	(5.329)
of Non-Medical Collections	*High*	-5.749	3.099	-28.49*	-16.60	-10.97	-5.057
		(14.11)	(14.91)	(13.65)	(8.997)	(6.822)	(7.037)
	Low	-0.00121	-0.00164	-0.00152	-0.00148	-0.000472	-0.000438
Increased Severity		(0.00170)	(0.00164)	(0.00156)	(0.00154)	(0.00147)	(0.00148)
of Delinquency	*High*	-0.00471	-0.00622*	-0.00639**	0.00123	-0.00213	-0.00605*
		(0.00255)	(0.00243)	(0.00243)	(0.00264)	(0.00263)	(0.00265)
	Low	-0.238	2.458	2.443*	0.638	-2.561*	-4.541***
Credit		(1.383)	(1.307)	(1.239)	(1.257)	(1.193)	(1.153)
Score	*High*	-2.600	-1.680	2.219	0.0589	-0.101	-2.280
		(1.943)	(1.841)	(1.838)	(2.006)	(1.946)	(1.894)
	Low	-305.5	-552.0	-684.9	-125.6	-836.4	-2229.9***
Credit		(717.6)	(657.3)	(642.5)	(673.6)	(677.7)	(639.8)
Limit	*High*	659.8	947.4	910.6	-622.6	636.0	1598.1
		(818.3)	(754.1)	(815.8)	(854.6)	(908.5)	(902.3)
	Low	0.00145	-0.00162	-0.000605	0.00102	-0.000129	
Public		(0.00152)	(0.00132)	(0.00113)	(0.00107)	(0.00101)	
Records	*High*	0.00225	-0.000501	0.00111	-0.00170	-0.000799	
		(0.00163)	(0.00136)	(0.00128)	(0.00138)	(0.00137)	
	Low	0.00106	-0.00000445	-0.000349	0.000315	-0.000335	0.000692
Bankruptcy		(0.000662)	(0.000554)	(0.000483)	(0.000498)	(0.000443)	(0.000439)
	High	0.000234	0.000535	-0.000408	-0.000934	-0.000370	0.000178
		(0.000982)	(0.000826)	(0.000805)	(0.000830)	(0.000797)	(0.000731)

Note: This table shows regression discontinuity estimates at age 66 from models based on Dong (2015). Models are estimated separately for counties in the highest and lowest quartile of uninsured rate. Each cell reports the RD estimate at age 66 for a different regression and associated standard error (in parentheses). Data are from the CFPB CCP for years 2012-2018. *p<0.05, **p<0.01, ***p<0.001

D3. Local Linear Regression Discontinuity

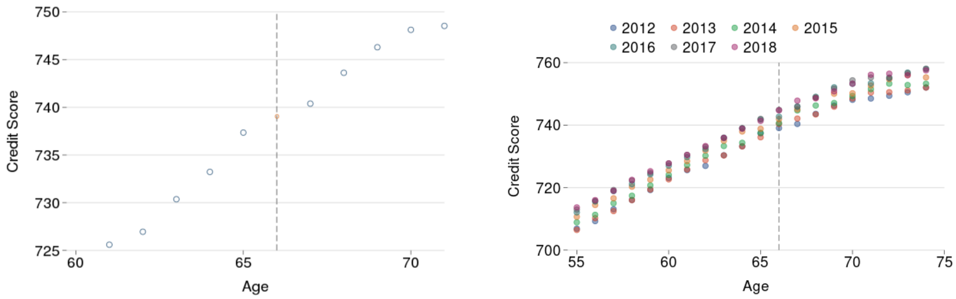
Although discontinuities are not identified non-parametrically because no data are observed immediately above and below the cut-off (regardless of the sample size), for robustness we include estimates from local linear models that extrapolate from the nearest integer ages. For these models we employ the standard MSE-minimizing bandwidth selection procedure and the robust biased-corrected confidence intervals.

Table D4 shows that the estimates surrounding Medicare eligibility in the main text are generally similar. There are reductions in measures of medical collections and little evidence of systematic spillovers to the other credit outcomes we can measure. For completeness, we also show estimates separately for counties with high and low levels of uninsurance (table D5).

While the local linear results are generally similar to our primary specification, there are some key differences that are worth exploring further. For example, this method estimates a positive effect on credit scores in 2012 (table D4). This estimate for 2012 reflects the selection of an extremely narrow bandwidth of one age on each side of the discontinuity using a mean-square error optimal bandwidth selector. Upon visual inspection, however, the raw data show there is a meaningful increasing trend in credit scores as individuals approach age 65 (see figure D8). Though there is some variation across years (such as small differences in levels), plots of the raw data suggest there is not a consistent discontinuity around age 65 for this sample. Parametric estimates (or a local linear with a wider bandwidth) would instead suggest a small reduction in level at Medicare eligibility. Additionally, the direction and magnitude of these credit score estimates are not driven by areas with high uninsured rates (table D5) and become negative when age 66 is included (not shown).

Similarly, the local linear effects for the average size of non-zero medical collections differ meaningfully (in magnitude, not direction) from the main linear models in some years. The local linear estimates suggest a drop in medical collec-

Figure D8. : Raw Data Surrounding Medicare Eligibility: Credit Scores



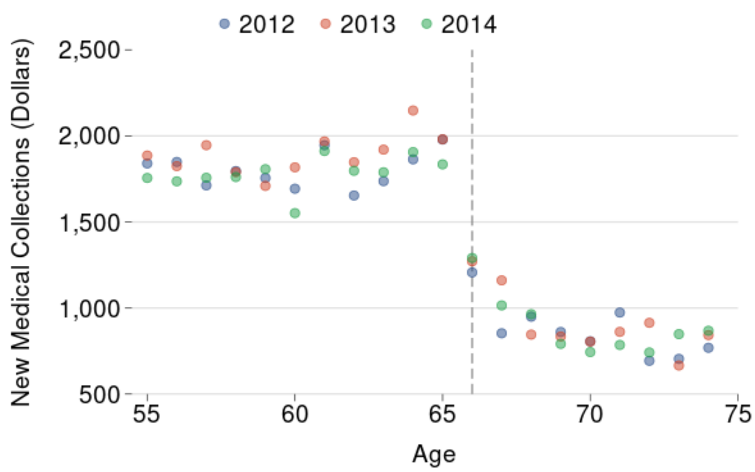
(a) Credit Score, 2013

(b) Credit Score, 2012-2018

Note: This figure shows the average credit score of consumers, by age. Data are from the CFPB CCP.

tions on the intensive margin in 2013 that is one-third to one-half the estimated size in 2012 or 2014. To better understand why, figure D9 plots average non-zero medical collection size by age for each of 2012 through 2014. Visual inspection suggests a substantial decrease in all years starting at age 66, as is the case in our parametric specification in the main text. Figure D9 shows the smaller local linear estimate for 2013 is driven by relatively high average medical collection amounts at ages 64 and 67 in 2013 but does not suggest a meaningful difference in the overall change in medical collections on the intensive margin. As in the above example with credit score, including age 66 attenuates this difference and results in an estimated decrease for 2013 of $829 (not shown), in between 2012 ($927) and 2014 ($630).

Figure D9. : Raw Data Surrounding Medicare Eligibility: Non-Zero Medical Collections, 2012-2014



Note: This figure shows the average dollar value of medical collections, conditional on having a non-zero amount, by age. Data are from the CFPB CCP for years 2012-2014.

Table D4—: RD Estimates using Local Linear, Around Age 65

	2012	2013	2014	2015	2016	2017	2018
Medical Collections							
Fraction with Any	-0.0021	-0.0105***	-0.0099***	-0.0009	-0.0055*	-0.0036	0.0003
Medical Collections	(0.0022)	(0.0029)	(0.0023)	(0.0027)	(0.0027)	(0.0026)	(0.00255)
Mean Dollar Value of Non-	-1,256.60***	-337.87	-748.95*	-722.63***	-172.48	-245.86	-971.3**
Zero Medical Collections	(251.31)	(364.02)	(243.07)	(166.71)	(351.52)	(194.51)	(375.82)
Mean Unconditional Dollar	-71.22***	-36.47	-56.64***	-49.01***	-18.04	-14.66	-54.84***
Value of Medical Collections	(13.319)	(21.959)	(14.014)	(9.8879)	(20.791)	(13.178)	(16.267)
Non-Medical Outcomes							
Mean Unconditional Dollar	9.40	4.01	4.53	-17.98*	2.33	-0.12	1.55
Value of Non-Medical Collections	(17.93)	(6.29)	(6.58)	(7.56)	(5.56)	(7.76)	(5.83)
Increased Severity of	0.0030	-0.0040	-0.0055**	-0.0022	-0.0013	-0.0018	-0.0044**
Delinquency	(0.0028)	(0.0028)	(0.0025)	(0.0017)	(0.0021)	(0.0021)	(0.0016)
Credit Score	3.034***	1.46	2.36*	1.64	-1.72	-1.98	3.25**
	(0.56)	(0.96)	(1.15)	(1.13)	(1.11)	(1.09)	(1.09)
Public Records	0.0023	-0.0009	0.0007	-0.0007	-0.0001		
	(0.0012)	(0.0007)	(0.0008)	(0.0006)	(0.0005)		
Bankruptcy	0.0006	0.0003	-0.0004	0.0004	-0.0005*	0.0003	0.0001
	(0.0005)	(0.0004)	(0.0004)	(0.0003)	(0.0003)	(0.0002)	(0.0003)

Note: This table shows regression discontinuity estimates at age 66 from models using local linear regressions. Each cell reports the RD estimate at age 66 for a different regression and associated standard error (in parentheses) using the RDRobust package from Calonico et al. (2017). Data are from the CFPB CCP for years 2012-2018. *p<0.05, **p<0.01, ***p<0.001

Table D5—: RD Estimates using Local Linear, Around Age 65, by County Uninsured Rate

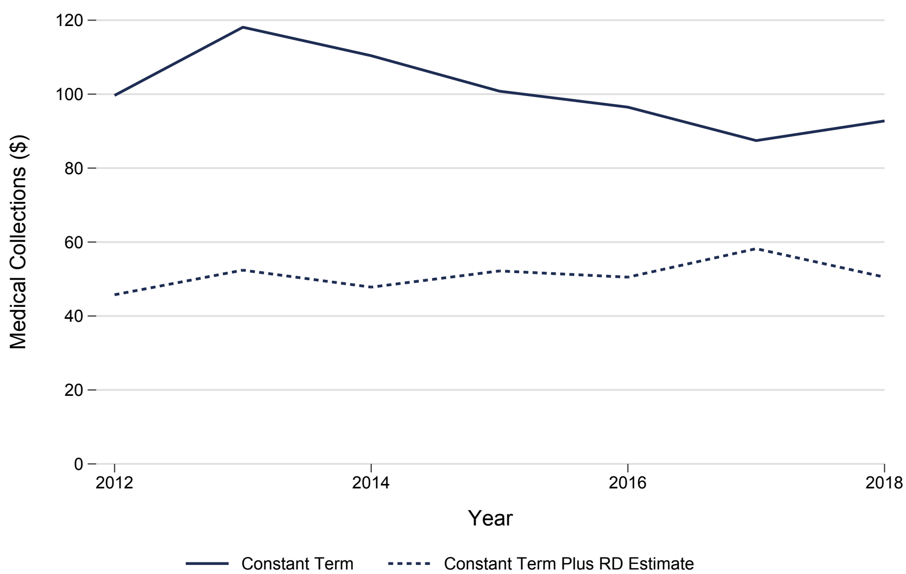
	Quartile of uninsured	**2012**	**2013**	**2014**	**2015**	**2016**	**2017**
Medical Collections							
Fraction with Any Medical Collections	*Low*	-0.0014	-0.0031	-0.0064	-0.0007	0.0000	-0.0002
		(0.0028)	(0.0023)	(0.0033)	(0.0032)	(0.0031)	(0.0022)
	High	-0.0096*	-0.0112**	-0.0099	-0.0094	-0.0078	-0.0164***
		(0.0040)	(0.0043)	(0.0053)	(0.0070)	(0.0062)	(0.0037)
Mean Dollar Value of Non-Zero Medical Collections	*Low*	150.29	-533.24	-661.66**	-408.81	-127.48	-427.08
		(344.25)	(337.28)	(231.35)	(213.88)	(214.24)	(286.75)
	High	-1,632.8***	-518.11	-759.31	-1238.7*	-1017.2***	763.87
		(326.24)	(618.84)	(486.29)	(549.48)	(282.23)	(668.97)
Mean Unconditional Dollar Medical Collections	*Low*	6.12	-29.98	-8.54	-15.38	-4.43	-12.41
		(12.99)	(18.40)	(13.12)	(8.29)	(8.48)	(11.54)
	High	-144.80***	-100.81*	-85.34	-160.04*	-97.06**	84.47
		(36.85)	(45.60)	(43.88)	(63.59)	(36.43)	(64.12)
Non-Medical Outcomes							
Mean Unconditional Dollar of Non-Medical Collections	*Low*	-13.63	-9.49	20.09	-0.28	8.02	7.69
		(13.13)	(8.68)	(13.31)	(6.74)	(9.57)	(8.14)
	High	11.23	12.98	-14.76	-34.13	-13.04	18.94
		(28.75)	(29.07)	(11.45)	(24.66)	(12.16)	(15.19)
Increased Severity of Delinquency	*Low*	0.0100*	-0.0054	-0.0050	-0.0019	0.0015	0.0024
		(0.0043)	(0.0032)	(0.0040)	(0.0040)	(0.0039)	(0.0033)
	High	-0.0049	-0.0049	-0.0071	0.0018	-0.0017	-0.0021
		(0.0054)	(0.0051)	(0.0047)	(0.0039)	(0.0051)	(0.0058)
Credit Score	*Low*	-5.26**	3.53*	4.55*	1.27	-3.21	-3.16
		(1.95)	(1.53)	(1.81)	(1.51)	(1.74)	(1.70)
	High	-3.41	-0.19	1.92	1.82	-1.90	-6.32*
		(2.87)	(2.10)	(2.27)	(2.45)	(2.16)	(2.90)
Public Records	*Low*	0.0039	-0.0026	-0.0012	0.0004	0.0003	
		(0.0021)	(0.0014)	(0.0009)	(0.0009)	(0.0010)	
	High	0.0045*	-0.0003	0.0020	-0.0019	-0.0001	
		(0.0023)	(0.0015)	(0.0012)	(0.0012)	(0.0011)	
Bankruptcy	*Low*	0.0009	0.0004	-0.0004	0.0001	-0.0012	0.0006
		(0.0007)	(0.0005)	(0.0004)	(0.0005)	(0.0006)	(0.0004)
	High	0.0004	0.0015	-0.0012	-0.0025*	-0.0011	0.0008
		(0.0009)	(0.0011)	(0.0008)	(0.0011)	(0.0009)	(0.0008)

Note: This table shows regression discontinuity estimates at age 66 from models using local linear regressions. Models are estimated separately for counties in the highest and lowest quartile of uninsured rate. Each cell reports the RD estimate at age 66 for a different regression and associated standard error (in parentheses) using the RDRobust package from Calonico et al. (2017). Data are from the CFPB CCP for years 2012-2018. *p<0.05, **p<0.01, ***p<0.001

D4. Decomposing Effects on Medical Collections

In Figure D10 we show the results of estimating Equation 2 on the unconditional average level of medical collections (i.e. including both those who do and do not have a medical collection that year). We estimate the model for each year from 2012 to 2018 separately and show that the effect of Medicare eligibility on medical collections decreased by 45 percent as the ACA eroded the coverage increase at age 65.

Figure D10. : Coefficients from the Effects of Medicare Eligibility on the Unconditional Mean Dollar Value of Medical Collections, by Year



Note: This figure shows estimates from evaluating Equation 2 for the overall unconditional mean medical collections, by year. Specifically, it plots the constant term, which represents the modeled annual medical collections by age as it approaches the Medicare discontinuity from below, and the constant term plus the RD estimate, which represents the modeled annual medical collections as it approaches the Medicare discontinuity from above. We re-estimate the model for each year and use linear age polynomials in all regressions. Credit data are from the CFPB CCP for years 2012-2018.

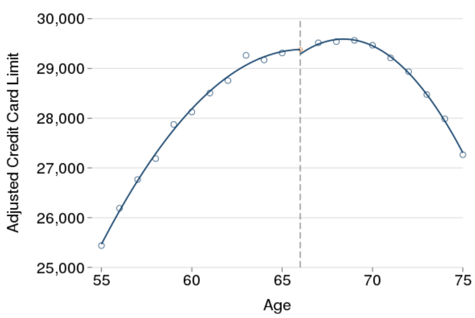
Figure D10 further decomposes the estimates of the RD effect on medical collections shown in Figure D4. Specifically, it plots the constant term from the regression each year, which represents the model-predicted annual medical collections

approaching 65 from below, and the constant term plus the RD estimate, which represents the model-predicted annual medical debt in collections approaching 65 from above. The decline in the RD estimate over time comes almost entirely from falling medical collections for the under 65 population, which is consistent with ACA-related coverage increases reducing medical collections.

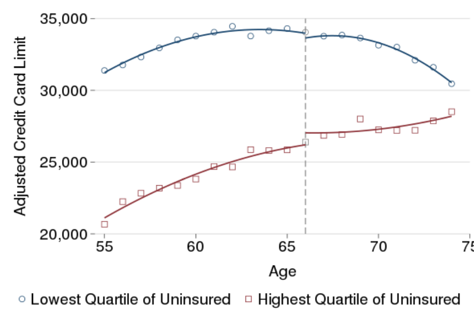
D5. Effects on Revolving Credit Limit

In Figure 9 we show that credit scores and credit card utilization evolve smoothly across the age 65 threshold, and note that the results for credit limit are similar. Figure D11 shows that credit limit also evolves smoothly across the age 65 discontinuity, and confidence intervals rule out economically meaningful effects. None of the subgroups stand out, and the trend across years does not suggest the ACA had a material effect.

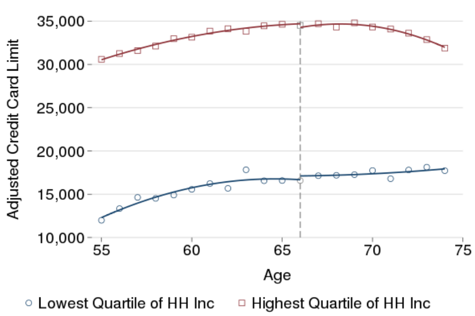
Figure D11. : Credit Card Limit



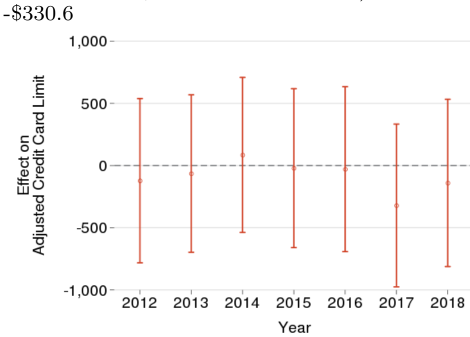
(a) Credit Card Limit, 2013. RD Estimate: -$64.09



(b) Credit Card Limit, 2013. RD Estimates (Top and Bottom Quartiles of Uninsured): $833.1 and -$330.6



(c) Credit Card Limit, 2013. RD Estimates (Top and Bottom Quartiles of HH Income): -$421.3 and $440.3



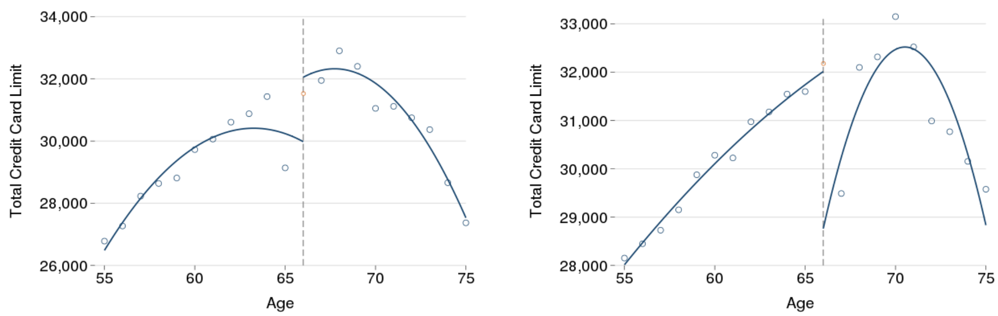
(d) Credit Card Limit, 2012-2018.

Note: This figure shows average revolving credit limit. Data are from the CFPB CCP for the years 2012-2018. We have omitted consumers who turn 66 in a year because evidence suggests credit outcomes reported in this year stem from events that occurred both before and after Medicare eligibility. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

D6. Cohort Effects for Credit Limit and Utilization

As described above, the reported values of credit card utilization and credit card limit for the 1950 birth cohort in each CCP year are consistently lower than those of the surrounding birth cohorts. There is no indication that data errors or outliers are the cause, but failing to adjust for this variation violates the premise of the regression discontinuity because those born in 1950 would not be representative of either prior year outcomes for those born in 1949, or next year outcomes for those born in 1951. Figure D12 shows how this distorts the RD estimates for credit limit in 2015 and 2017, the years before and after the 1950 birth cohort cross the discontinuity of Medicare eligibility.

Figure D12. : Unadjusted Credit Limit, 2015 and 2017



(a) 2015 (1950 cohort turns 65). RD Estimate: $2,190.3

(b) 2017 (1950 cohort turns 67). RD Estimate: -$3,820.9

Note: This figure shows revolving credit limit in 2015 and 2017 before persistent cohort effects have been removed. Data are from the CFPB CCP. Figures and regression discontinuity estimates are generated from evaluating Equation 2 with quadratic polynomials in age. We have omitted consumers who turn 66 in a year because evidence suggests credit outcomes reported in this year stem from events that occurred both before and after Medicare eligibility.