Financial Repercussions of SNAP Work Requirements

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Abstract

This paper considers individual-level credit responses after the implementation of work requirements for SNAP benefits. It does so by exploiting county-level variation in the reintroduction of work requirements after the Great Recession. We find that new SNAP work requirements lead more people to seek out new credit and lead to an increase in credit account openings. New work requirements also result in an increase in total outstanding credit balances as well as an increase in past due balances. These findings suggest that individuals are turning to credit and debt products to cover expenses after losing SNAP eligibility.

Keywords: Work Requirements; SNAP benefits; Consumer Credit

JEL Codes: G51, H31, H53, I38

Disclaimer. The results and opinions expressed in this paper reflect the views of the authors and should not be attributed to the Federal Reserve Board. Because the credit data used in this study are proprietary and are owned by Equifax and the Federal Reserve System, they are required to remain confidential and cannot be shared publicly. The results in this paper were reviewed prior to submission by Equifax to ensure that the data are properly described, and that no confidential data were compromised. The authors have no relevant financial interests that relate to the research described in this paper.

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1. Introduction

Since the welfare reforms enacted in the mid-1990s, the U.S. social safety net system has focused on promoting self-sufficiency in an approach that is often called "welfare to work." Frequently, this involves limiting or forbidding program benefits among those who are not working. For example, the Supplemental Nutrition Assistance Program (SNAP), the Temporary Assistance for Needy Families (TANF) program, and Medicaid program all include work requirements for some participants. Similarly, the Earned Income Tax Credit (EITC) requires labor earnings to be eligible for the credit. Recent policy proposals have also expanded and strengthened these work requirements by reducing existing exemptions (United States Department of Agriculture 2019) or expanding the age range for whom work requirements apply (United States Congress 2023). While research has long recognized that work requirements reduce SNAP caseloads (Ziliak, Gundersen, and Figlio 2003), the effects of these requirements on financial outcomes are still under debate.

After the imposition of new work requirements, non-workers who were receiving benefits have several potential channels of response. They may start working to earn money and make up for the lost benefits (Harris 2021). They may reduce consumption (Bruich 2014, Hoynes and Schazenbach 2009). Or they may find alternate channels to finance consumption – including using credit or debt.

This paper uses variation in work requirements for SNAP benefits to determine how individuals adjust their use of credit in response to newly imposed work requirements. There are two primary channels through with we may expect the loss of work requirements to affect credit use. The first is through an increase in credit demand among those who lose benefits that are not replaced by earnings.[1] Some may also seek credit after receiving notice of an upcoming loss of benefits to provide a financial buffer in case they need it, even if they later find employment. Additionally, a second way that work requirements can affect credit use is indirectly through increases in credit supply. If work requirements incentivize employment, banks may be more willing to lend to affected individuals who are now more creditworthy. However, since this credit supply response results from increased employment, the magnitude of this effect is contingent upon the size of employment effects, which recent research has suggested is limited in size (Gray et al. 2023). Of course, the implications of increases in credit from a demand effect versus a supply effect are quite different as the former reflects increased financial

[1] For context on the scale of benefits, when recipiency peaked in 2013, 23 million households, containing 47 million people, received an average of $274 worth of monthly benefits (United States Department of Agriculture 2020). Hastings and Shapiro (2018) demonstrate a high marginal propensity to consume food out of SNAP benefits, and these benefits have also previously been found to reduce food insecurity (Schmidt, Shore-Sheppard, and Watson 2016) and material hardship (Shaefer and Gutierrez 2013; McKernan, Ratcliffe, and Iceland 2018).

distress (or concerns about potential financial distress) while the latter reflects financial progress. Hence, it is also important to consider whether the changes in credit usage coincide with improvements in creditworthiness.

We explore these questions by using geographic variation in work requirements through a USDA waiver program since the Great Recession. In general, "able-bodied adults without dependents" (ABAWDs) under age 50 are subject to additional work requirements that limit them to three total months of benefits per three year period if they are not working. The waiver program allowed states to remove these additional work requirements for ABAWDs in specific counties. The timing and locations of waivers affect who is subject to work requirements each quarter and provides a natural experiment to explore the effects of these requirements—as has been done previously to consider how work requirements affect employment (Gray et al. 2023, Han 2022, and Harris 2021).

We use the variation in work requirements from the waiver program to look at credit responses through two frameworks: a difference-in-differences framework and a difference-in-discontinuities approach that leverages the age 50 ABAWD cutoff for identification. Using the FRBNY/Equifax Consumer Credit Panel (CCP), which contains data on individual credit use and balances, we consider four primary consumer responses to SNAP work requirements: (1) the rate at which consumers seek credit (inquiries) and obtain additional credit (new accounts), (2) total outstanding balances on credit card and retail card accounts, (3) the prevalence of past-due card debt, and (4) total outstanding balances and past due incidence on consumer finance accounts.[2] These measures were chosen to reflect that individuals who are subject to an income shock may be expected to seek credit that can be used for general spending and, subsequently, increase their credit utilization through their new or existing accounts. We also consider two measures of creditworthiness to help separate supply and demand effects – changes in their Risk Score and changes in the success rate of credit applications, i.e. the number of new accounts per inquiry.[3]

In our difference-in-differences design, those subject to work requirements in their county of residence represent the treated group. The comparison or control group is the set of individuals in

[2] Credit cards are also referred to as bankcards, which are credit card issued by a bank, a bankcard company, a national credit card company, or a credit union. Consumer finance loans are credit lines/loans originating from banks and credit unions with an initial credit limit of $20,000 or less. While less commonly studied than measures like credit cards, these products were included in case some consumers shifted to other unsecured debt products. We did not consider other products, such as auto lending or student loans, since those products must be used for specific large purchases that are less related to the loss of food benefits. We also did not consider products such as home equity loans, even though they can be used for general spending, since they require homeownership and are therefore less prevalent among the SNAP recipient population.

[3] Here and in all other instances in the paper, "credit score" refers to the Equifax Risk Score.

counties within the same commuting zone that do not have a work requirement in place. This approach restricts the control group to those counties that are most likely to share fundamental economic and policy attributes as well as common trends with the treatment group. Thus, this control group is most likely to satisfy the assumptions necessary for unbiased estimates in a difference-in-differences framework. We validate this approach with an event study model that traces out dynamic responses to work requirements and allows us to detect differential trends for those living in treatment and control areas prior to the imposition of work requirements.

Additionally, we employ a difference-in-discontinuities approach that leverages the age 50 cutoff for ABAWD status. Those over age 50 are never subject to the ABAWD work requirements. In this framework, we estimate a regression discontinuity model for each outcome at the age 50 ABAWD cutoff during the three years before work requirements went into effect and compare the same discontinuity during the three years after SNAP work requirements went into effect. The change in the discontinuity before and after the work requirements in treated counties provides us with a well-identified local average treatment effect of the work requirements for those near the age 50 cutoff.

Throughout this paper, our estimates are intent-to-treat estimates, rather than narrowly targeted estimates of effects on those subject to the treatment. Because we are unable to directly observe SNAP recipiency in the credit data, we focus our analysis on individuals who persistently do not have a mortgage who have had a Risk Score under 700 in the past decade, as these individuals are disproportionately likely to be SNAP recipients. However, even with these restrictions, we estimate that about 10 percent of our sample are ABAWD SNAP recipients, so estimates are closer to zero than if we were able to observe the effects only on those treated.

Our results indicate that SNAP work requirements significantly increase the likelihood that consumers apply for and use additional credit. Credit applications, total accounts, total credit card and retail card credit limits, and outstanding balances all increase significantly after the enactment of new work requirements, indicating that individuals subject to these requirements are seeking out and utilizing additional credit in response to the loss of benefits. Sullivan (2008) and Braxton, Herkenhoff, and Phillips (2020) previously observed that low-asset households increased borrowing in response to lost employment earnings, and our findings indicate that similar borrowing occurs in response to lost public benefits as well. Based on our difference-in-differences estimates for those with Risk Scores below 700 without a mortgage, in the 3 years following the imposition of work requirements, credit inquiries over a six month period months increased by 0.12 inquiries (13 percent), the probability of opening a new card account increased by 1.8 percentage points (7 percent), total credit accounts

increased by 0.08 accounts (5 percent), total combined credit limits increased by approximately $392 (10 percent), total card balances increased by approximately $132 (8 percent), and total consumer finance balances increased by approximately $16 (5 percent). Since these are intent-to-treat estimates, scaling up by the share of ABAWDs in our sample suggests that the increase in debt is large relative to the lost benefits. Much of the increase in credit seeking occurs in the first two quarters after the work requirement takes effect, indicating that there may be some credit seeking in anticipation of future needs, although the amount of borrowing accumulates over time. Additionally, we find a small decrease in credit scores and credit application success rates after the imposition of work requirements, suggesting that these effects are not coming primarily through increases in credit supply.

Our difference-in-discontinuities estimates around the age 50 cutoff show similar increases in credit seeking behaviors after the imposition of work requirements (approximately a 15 percent increase in inquiries and a 5 percent increase in the probability of opening a new account). However, perhaps because those near age 50 have already had more credit extended to them than our overall sample of non-mortgage holders with low credit, the differences-in-discontinuities does not show similar increases in credit limits. Nevertheless, the increase in credit and retail card balances was similar (6 percent) and the increase in consumer finance credit balances was greater than that seen from our difference-in-difference estimates.

In addition to the increases in credit seeking and borrowing, we find evidence that consumers are slightly more likely to be past due on their card payments and consumer finance account payments in response to the work requirements. In the difference-in-differences estimates, the prevalence of past due balances on card accounts increased by 0.5 percentage points (2 percent), although the prevalence of past due consumer finance accounts did not significantly change. Our difference-in-discontinuities design also observed increases in past due card payments, as well as past-due consumer finance accounts. Overall, despite differences in our empirical approaches measuring effects at slightly different margins among different age samples, the effects of the work requirements on the financial outcomes we measure are quite consistent.

These findings suggest that individuals are frequently turning to credit after work requirements go into effect. Recent research considering the effects of SNAP work requirements consistently documents a substantial drop in SNAP participation as work requirements are (re)introduced (Brantley, Pillai, and Ku 2020; Gray et al. 2023; Han 2022; Harris 2021; Ku, Brantley, and Pillai 2019; Stacy, Scherpf, and Jo 2018). There remains disagreement about the size of employment effects, as Harris (2021) observes that work requirements increase employment while other recent studies including Gray et. al

(2023) and Han (2022) have not.[4] Yet, all of these studies find increases in employment that are smaller than the disenrollment from SNAP. Hence, the credit responses we observe provide insights into how those who remain unemployed or who are slow to find employment respond to the policy change.

In addition to helping to understand the repercussions of work requirements on those who lose benefits, our findings have important implications for evaluating work requirements for public assistance programs more broadly. Numerous studies considered the employment and income effects of TANF work requirements (see e.g. Blank 2002; Falk 2018; and Ziliak 2016), Medicaid work requirements, (Sommers et al. 2020), and increasing the returns to work through programs such as the EITC (see e.g. Dahl, DeLeire, and Schwabish 2009; Eissa and Liebman 1996; Meyer and Rosenbaum 2001). A smaller literature considers how consumer credit responds to changes in public benefits programs (Bornstein and Indarte, 2022, Hsu Matsa, and Melzer 2018, Dodini 2023). Although our focus is on SNAP benefits, the results complement this work to provide guidance on how families may respond to the loss of social safety net benefits.

SNAP is an important source of consumption funding for many low-income Americans. As recipients lose access to benefits and are not offsetting lost benefits with increases in employment earnings, they use credit to make up the difference that could hamper potential asset and wealth accumulation. If the population affected by the work requirements falls behind on their payments with larger debt amounts, they may be subject to additional financial hardships and material deprivation in the future.

2. Institutional Details on SNAP and Work Requirements

Originally termed "Food Stamps," the Supplemental Nutrition Assistance Program has been one of the primary social safety net programs for low-income families for decades**.** Since 1990, at least 6 percent of the US population has received SNAP each year, rising to approximately 15 percent of the population in the years following the Great Recession (Ganong and Liebman 2018). Over the period of our analysis from 2010 through 2017, the maximum benefits that a single-person could receive in most states ranged from $194 to $200 per month and a two-person household could receive a maximum of $347 to $367 per month (see e.g. USDA 2014). These benefits are phased out as the recipient's income

[4] As earlier evidence of how SNAP benefits without work requirements can affect work decisions, Hoynes and Schanzenbach (2012) observed that there were employment declines when the food stamp program began prior to the introduction of work requirements. There may be differences in how consumers respond to adding a new benefit compared to taking away benefits, although this is beyond the scope of what we can explore with our data.

increases and eligibility is generally limited to households with income below 130 percent of the Federal Poverty Line. Additionally, in some states, there is also an asset limit of as low as $2,250.

Since the Farm Bill of 1996, the SNAP program has included a work requirement for ABAWDs – adults aged 18–49 without children under the age of 18 living in their home, who are not pregnant, disabled, or receiving unemployment benefits.[5] To receive benefits, ABAWDs must work for 80 or more hours per month or participate in a workforce program. This work requirement applies to ABAWDs who apply for benefits for more than three months in a three year period, and work status is certified each individual month in which they collect benefits.[6]

Importantly for our study, in the wake of the Great Recession, the American Recovery and Reinvestment Act (ARRA) of 2009 effectively suspended ABAWD work requirements for SNAP benefits nationwide in 2010. These work requirements were reintroduced in a staggered fashion over the subsequent years due to the interactions between pre-existing SNAP program rules and the 2008 Emergency Unemployment Compensation (EUC) program.

The US Department of Agriculture (USDA), which oversees the SNAP program, grants waivers allowing states to drop the work requirement in certain areas—normally counties—experiencing high unemployment or that are otherwise experiencing significant hardship in labor markets. Generally, states can exempt ABAWDs from work requirements if their local labor market had an elevated unemployment rate. However, states could also exempt ABAWDs from work requirements if their local labor market area qualifies for extended unemployment benefits, which was particularly important during the Great Recession and subsequent recovery.[7]

Because the 2008 Emergency Unemployment Compensation (EUC) program extended unemployment benefits nationwide, any state could request work requirement waivers for some or all counties while the extended benefits were in effect – although they were not required to do so. Due to extensions of the EUC program until 2013 and a subsequent lag in when work requirements had to

[5] Definitions of disability for the SNAP program include those that receive income from disability insurance, those with a statement from a medical profession that they cannot work due to a physical or mental concern, and those deemed unable to work by a state agency.

[6] For additional details on the characteristics of people likely affected by work requirements for SNAP and other programs, see Council of Economic Advisers (2018) and Bauer, Schanzenbach, and Shambaugh (2018).

[7] States could also apply for waivers for an area under other specific conditions such as being in a US Department of Labor "Labor Surplus Area," being described in an academic study as an area with a lack of jobs, having a low and falling employment-to-population ratio, qualifying for extended unemployment benefits, or experiencing declines in particular occupations or industries.

restart, states could extend waivers from work requirements through the end of 2015 based on this legislation.[8] The majority of waivers before 2016 were justified based on these EUC regulations.

Collectively the provisions in the ARRA and the EUC meant that there were no SNAP work requirements for ABAWDs at the beginning of 2010. Because of variation in when work requirement waivers ended for each county, there was a staggered reimplementation of work requirements over the subsequent years through 2016. Upon the reimplementation of work requirements, the USDA recommended to states that they notify ABAWDS of the policy change (USDA 2015), which means that affected individuals learned of their upcoming loss of benefits around the implementation date.[9] Additionally, several counties never reimposed work requirements prior to 2017 due to high local unemployment rates or by qualifying under the other special conditions to waive work requirements. Our analysis leverages variation in when work requirements returned across states and counties during this period to identify the effects of the work requirements on individual financial and credit outcomes. It also leverages variation in the age cutoff for ABAWDs, since individuals ages 50 and older are never subject to the ABAWD work requirements even after waivers end.

3. Data

To estimate the effects of SNAP work requirements on credit outcomes, we rely primarily on data from the FRBNY/Equifax Consumer Credit Panel (CCP) from 2010 to 2017. This is a large administrative panel dataset of credit records that contains consumer-level information on credit applications and credit usage for an anonymized random sample of all individuals with a social security number and a credit report in the U.S. From these data, we drew a random sample that contains 0.5 percent of individuals in the United States with a credit report. The CCP data is reported quarterly, evaluating credit records near the end of each calendar quarter (March, June, September, and

[8] States were then eligible for a 12-month waiver extension that began 12 months after the end of extended unemployment benefits, meaning that states could extend waiver eligibility through two-years after the 2013 expiration of the EUC program.

[9] Although new claimants would be subject to ABWAD rules immediately, the precise date that ABAWDs are affected by the reimplementation of work requirements will depend on their state's rules. States are required to identify potential ABAWDs prior to the waiver expiration, and in March 2015 the USDA strongly recommended that states notify ABAWDs of the new work requirements at least 30 days prior to the waiver ending (USDA 2015). Additionally, they stated that states cannot defer the work requirements until the next regular contact with the recipient at recertification. This guidance may therefore result in anticipatory effects of the expiration among ABAWDs and would accelerate the expiration of benefits relative to the next regular contact. However, as noted by Gray et al. (2023), at least some states that reintroduced work requirements prior to this notice had longer reintroduction periods by waiting until the next regular recertification meeting. These recertifications are typically every 6 to 12 months but can be shorter for some recipients including ABAWDS (CBPP 2018).

December). The quarterly reporting structure in the CCP allows us to examine the effects of work requirements as they are implemented and to trace out any dynamic effects over quarters.[10]

In the CCP, we focus on measures of credit-seeking behavior, debt on credit and retail cards, and debts on what we term in our analysis "consumer finance" products, which includes small loans and personal loans by bank and credit union lenders. The outcomes we consider are the number of credit inquiries on an individual's credit report in the past six months, the likelihood of opening a new account (of any type) in the past six months, the total number of credit and retail card accounts on the consumer's credit report, and total credit limit and balances on their card accounts.[11] We pay particular attention to these outcomes because credit and retail cards are the most commonly used types of credit for day-to-day purchases and consumption (Green and Stavins 2018) and because consumer finance loans could be a substitute for some credit card spending.[12]

These measures were chosen to reflect the progression of how individuals may adjust their credit behaviors in response to the loss of SNAP benefits after the imposition of work requirements if they are facing economic hardships. First, some individuals who have limited credit available may seek out additional credit that they can use to cover regular expenses. Second, individuals may utilize either their existing credit or this new credit by increasing their outstanding balances. An increase in balances is an indication that the individuals are taking on new debt, as would occur if their monthly income (including SNAP benefits and other support programs) is insufficient to cover their monthly expenses. Although many who incur additional debt can remain current on these bills, some may fall behind on the additional debt payments.[13] Hence, as a sign of substantial distress we also consider past due accounts.

[10] Work requirements typically begin at the start of calendar quarters. Hence, results for the 1st quarter of implementation may be a couple of months after the work requirement goes into effect.

[11] The CCP data represent a key source of information on credit outcomes. However, they are limited in that they do not contain any information that does not appear on credit records and we cannot merge individual-level credit records with SNAP recipiency data or datasets. As a result, we are unable to account directly in the data for gender, race/ethnicity, education, household structure, or other demographic characteristics besides age.

[12] The CCP contains credit data for those with a credit score or thin credit file but does not generally capture informal borrowing. Because the SNAP eligible ABAWD population may have particularly low incomes, they may turn to informal forms of credit not specified on a credit report. If affected individuals are also accelerating credit seeking in these other sectors, it may mean that our results understate the increasing of credit usage, particularly if the intensity of credit-seeking behavior is negatively correlated with income and a presence in the formal credit market. However, if that individuals who find employment after the work requirements shift from informal borrowing to formal borrowing, it would mean that our results overstate the increase in borrowing.

[13] Credit card accounts typically have a required minimum payment equal to the greater of a percent of the outstanding balance (such as 2-3 percent) or a dollar amount (such as $40). Hence, a borrower could be current on their outstanding credit card debt if they are able to pay at least the minimum amount due each month.

To track waivers from SNAP work requirements, we use county-level waiver data from Harris (2021), which he constructed based on official approval letters from the USDA in response to state waiver applications. Waivers typically take effect at quarterly breaks, so we consider a county to be "treated" if a work requirement waiver was not in place (meaning that a work requirement was in place) at the beginning of each quarter in the sample. Because waiver receipt is predicated upon a state's political leaders applying for the waivers, following Harris (2021), we include as controls in our analysis measures of party control of each state's governor's office and state legislature. County waivers in the final years of our study depend on the recent labor market conditions in the county, so we also include in our sample annual measures of lagged (1 year) county-level unemployment and labor force participation from the Local Area Unemployment Statistics (LAUS) from the Bureau of Labor Statistics.

The CCP does not contain information on individual demographic characteristics besides age, but we do include a set of controls of local neighborhood characteristics for each individual in the panel that may be correlated with individual characteristics. These variables come from 5-year estimates from the American Community Survey for each person's census tract, and we apply these estimates to middle year; for example, the 2011–2015 estimates are applied to the year 2013. These measures include population and population density, race/ethnicity shares, the share of adults with a bachelor's degree, the share of households with children under age 18, and the share of the population under age 18. We also include the lagged (1 year) local poverty rate.

Sample Restrictions

Ideally, we would examine changes in financial outcomes among just the SNAP-eligible or near-eligible ABAWD population in response to SNAP work requirements. The CCP does not contain individual information on income, receipt of public assistance, dependents, disability, or other determinants of SNAP eligibility. We therefore are left to approximate this population in the CCP based on factors that are in the data such as age, Risk Score, and mortgage status.

3.1. Difference-in-differences sample

For our difference-in-differences sample, we begin by limiting our sample to those ages 18–49 to match the ABAWD age definitions for SNAP. In order to approximate the income level of those in or near SNAP eligibility, we also limit our estimation sample to those whose lowest Equifax Risk Score was below 700 at any point during the sample period. We impose this restriction based on the correlation between income, Equifax Risk Score, and SNAP recipiency documented in other work. For example, in the Federal Reserve Board's 2019 Survey of Household Economics and Decisionmaking, the 22 percent of adults who reported that their credit was "fair" or "poor" represented 65 percent of SNAP recipients

(Federal Reserve Board 2020). It is also well documented that income and credit score are highly correlated (see, for example, Beer, Ionescu, and Li 2018). As the median Equifax Risk Scores for those classified as "Low Income, "Moderate Income," and "Middle Income" are 658, 692, and 735, respectively (Kramer-Mills, Landau, and Scally 2020), our choice of 700 as a sample cutoff allows us to better focus on low- and moderate-income consumers.

We further limit our sample to those who never had a mortgage in the CCP during our sample period. According to Kramer-Mills, Landau, and Scally (2020), less than 20 percent of low- and moderate-income adults have a mortgage. Based on ACS data from IPUMS (Ruggles et al. 2021), we observe that among those ABAWDs who received SNAP at least once in the prior year, 71 percent were renters or owned their home free and clear (no mortgage).[14] Taken together, our restrictions result in a sample that contains 45 percent of the age 18–49 population with a credit score.[15] Finally, since our comparison groups are based on Commuting Zones, we exclude those who are living outside of Commuting Zones.[16]

While our sample restrictions attempt to represent the population that is likely to be affected by changes to the SNAP program, because we cannot narrowly identify the SNAP eligible population, our estimates should be interpreted as broad "intent-to-treat" effects of SNAP work requirements. After our various restrictions, our final estimation sample covers 3,108 counties in 707 Commuting Zones with a total of approximately 8.7 million observations. When we limit the estimation sample to event windows of three years before and after the work requirement took effect, we have approximately 6.3 million observations.

3.2. Difference-in-discontinuities sample

For our difference-in-discontinuities sample, we take the above restrictions to those under a 700 Risk Score without a mortgage and adjust the age range to those aged 40 to 60, which grants us a ten-year age window on either side of the age 50 cutoff by which to measure structural breaks in each

[14] Although this group includes both renters and those who own their home without a mortgage, for ease of exposition, we refer to the group as renters through this paper.

[15] In the 2010-2017 CPS, 54 percent of renters ages 18 to 49 were able-bodied adults without dependents and between 5 and 6 percent were ABAWDs who received SNAP (the CPS does not have credit data to include that restriction). Because we restrict by Risk Score and to those who never have a mortgage between 2010 and 2017, our sample will be lower income than this group in the CPS and more likely to contain SNAP recipients. Nevertheless, even with the sample restrictions, a large share of our sample will be unaffected by the work requirements and the estimates would likely be larger were we able to isolate ABAWD SNAP recipients.

[16] In our difference-in-discontinuities approach, we relax this restriction and find similar results.

	Difference-in-Differences Sample		Difference-in-Discontinuities Sample	
	Mean	SD	Mean	SD
Age	32.6	8.6	49.4	6.0
Inquiries in Past 6 Months	0.97	2.30	0.87	2.26
Pr(Any New Inquiries)	0.41	0.49	0.38	0.48
Total Card Accounts	1.5	2.4	2.0	3.1
Any Card Total Balance	1,696	4,617	2,609	6,730
Total Card Credit Limit	4,044	10,858	6,200	16,357
Pr(Any Card Past Due)	0.26	0.44	0.26	0.44
Consumer Finance Total Balance	325	1,922	527	2,811
Pr(Consumer Finance Past Due)	0.04	0.19	0.04	0.20
Census Tract Characteristics				
Total Census Tract Population	5,273	2,728	5,154	2,532
% Under Age 18	0.23	0.07	0.23	0.06
% Non-Hispanic White	0.56	0.31	0.56	0.31
% Non-Hispanic Black	0.15	0.23	0.16	0.22
% in Poverty	0.17	0.12	0.17	0.12
% Bachelors+	0.28	0.18	0.27	0.17
% HH with Children Under 18	0.33	0.11	0.33	0.11
Population Density	6,641	14,588	6,583	15,391
County Labor Market Characteristics				
Total County Labor Force	636,593	1,037,553	587,203	978,818
Unemployment Rate	0.065	0.024	0.059	0.019
State Expanded Medicaid	0.36	0.48	0.40	0.49
N	8,665,475		2,453,054	

Table 1. Summary Statistics of Estimation Samples
Source: Authors' calculations using FRBNY Consumer Credit Panel/Equifax Data, the American Community Survey, and BLS Local Area Unemployment Statistics.

outcome as consumers cross the age 50 ABAWD threshold. After our this age restriction, our final estimation sample for the difference-in-discontinuities estimate includes approximately 2.5 million observations, roughly evenly split between those over and under age 50.

3.3. Descriptive Statistics

Table 1 presents summary statistics for our estimation samples used for the difference-in-differences and difference-in-discontinuities analyses. The average consumer in the 18-49 sample is 33 years old with an average of approximately one credit inquiry in the past six months. Approximately 28 percent of consumers opened a new account of some kind in the past six months. The average total card balance was approximately $1,700 and the average credit limit was approximately $4,000. About 26

percent of consumers in the sample had past due debts on a credit or retail card listed on their credit report. The average amount of consumer finance debt was $325. The average local unemployment rate experienced by consumers in the sample was 6.5 percent, which tracks the 6.8 percent national average calculated by the Bureau of Labor Statistics over this period, and their states of residence had expanded Medicaid for approximately 36 percent of the observations, the earliest of which began in 2014. The difference-in-discontinuities estimation sample has lower rates of flows of credit, that is, new inquiries and accounts. However, the number of accounts, card balances, total card credit limit, and balances on consumer finance accounts are all higher in this sample.

Figures 1 and 2 provide the first indication that individuals affected by new work requirements are seeking out additional credit. In these figures, which use our difference-in-differences sample under age 50, those who already had a work requirement that did not change status (i.e. "already treated") are included in the "no new work requirement" group, making these raw comparisons conservative estimates. While these figures are unconditional on other characteristics other than restricting the sample to renters, it is apparent in Figure 1 that lower-credit borrowers – and especially those with Risk Scores between 525 and 620 – are seeking out additional credit on the intensive margin in the year that new work requirements go into effect. In the raw data without controls, borrowers with Risk Scores in this range had approximately 5 percent more credit applications if living in an area with new SNAP work requirements than are those in areas where the work requirement rules did not change. For borrowers with either higher or lower Risk Scores, the gap in credit applications is smaller – and it disappears entirely among borrowers with a Risk Score over 680, who are less likely to be receiving or be eligible to receive SNAP benefits and thus be affected by the work requirements. As seen in Figure 2, which looks at new account openings of any kind, those living in areas with newly imposed work requirements also are more likely (12 percent) to have actually opened an account – especially if their Risk Score is in the 550 to 620 range. These figures provide further justification for our sample restriction to those with a minimum Risk Score under 700. Between these two figures, it is clear that those with higher Risk Scores, while seeking credit at roughly the same or lower probability as their lower-score counterparts, are far more successful in actually obtaining new credit. However, the gap between areas with new work requirements and those without new work requirements follows a similar pattern when considering both outcomes. This suggests that the gap is driven not by unobserved differences in the ability to get credit after applying conditional on Risk Score, but by differences in credit seeking.

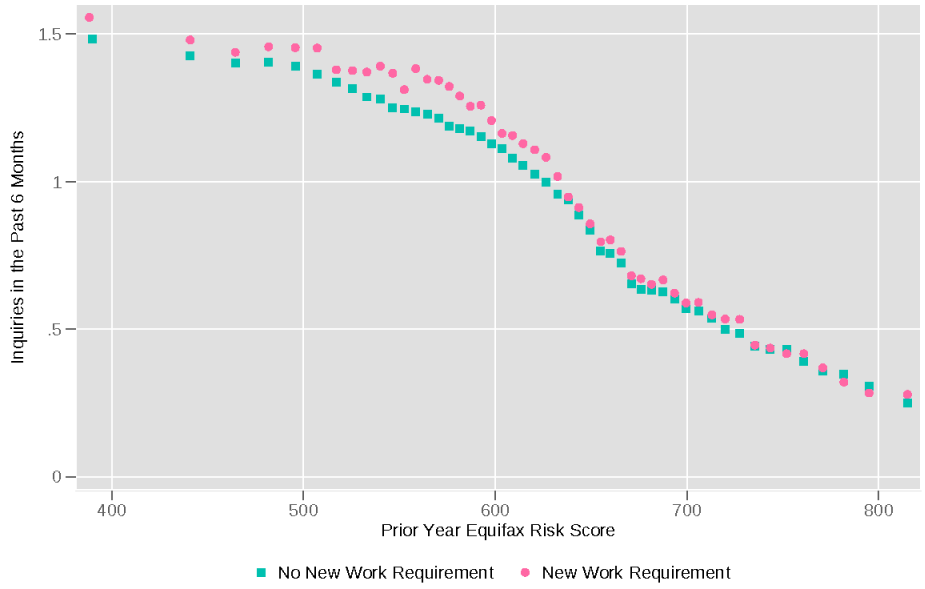


Figure 1. Inquiries in the past six months by credit score and presence of new work requirements
Source: Authors' calculations using FRBNY Consumer Credit Panel/Equifax Data.
Note: Among individuals without a mortgage from 2010 through 2017.

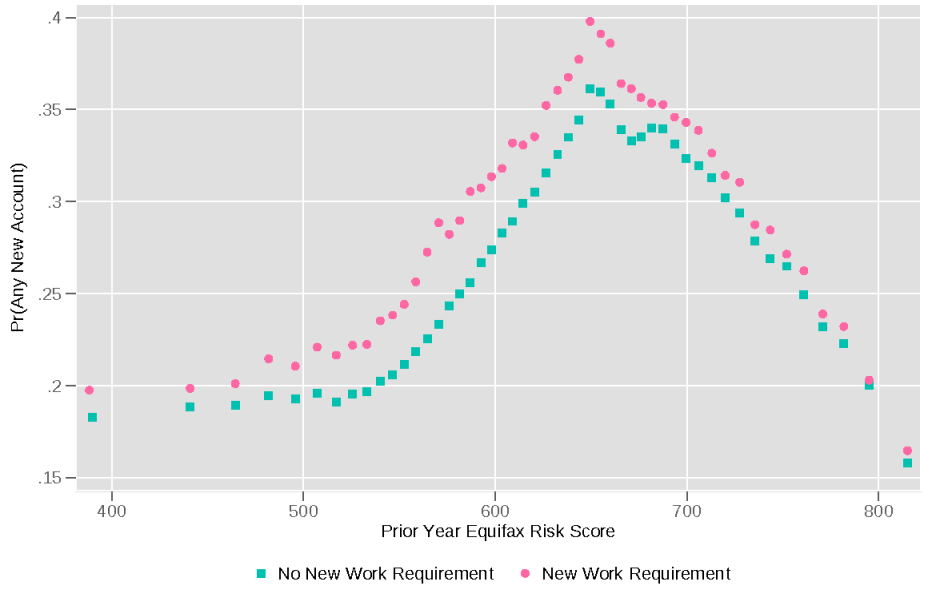


Figure 2. Probability of opening a new credit account by credit score and presence of new work requirements
Source: Authors' calculations using FRBNY Consumer Credit Panel/Equifax Data.
Note: Among individuals without a mortgage from 2010 through 2017.

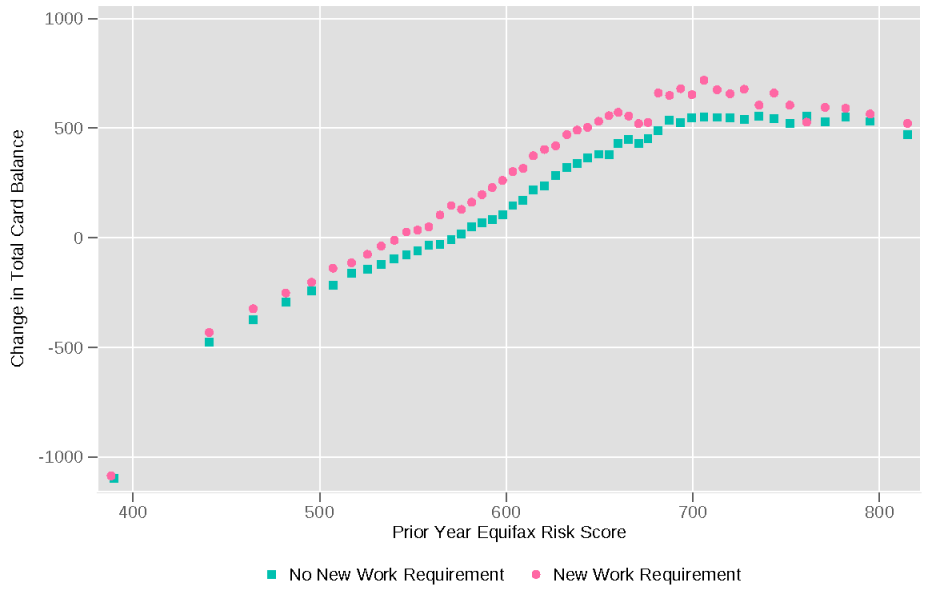


Figure 3. Change in total card balance by credit score and presence of new work requirements
Source: Authors' calculations using FRBNY Consumer Credit Panel/Equifax Data.
Note: Among individuals without a mortgage from 2010 through 2017

Figure 3 presents the same Risk Score distributions but shows the change in total debt balance on all bankcard and retail card accounts. This shows the realized change in balances, reflecting actual credit usage. Across the Risk Score distribution, changes in total balances across card accounts are notably higher in areas that implemented a new work requirement as a condition of SNAP recipiency for ABAWDs. Changes to reduce card balances were noticeably smaller (i.e. less negative) in these areas, while increases in balances were larger. Like the figures for credit-seeking behavior, these changes in balances are most pronounced for those with Risk Scores between 500 and 660, indicating that the balances are occurring subsequent to the opening of new credit lines for those most likely to be low-income earners.

While suggestive of how individuals may be seeking and relying on additional credit, Figures 1 through 3 do not control for other differences between areas that have newly imposed work requirements and those that do not. To incorporate these differences, we formally estimate the causal effects of the work requirements in a difference-in-differences framework and difference-in-discontinuities framework, which we describe below.

4. Empirical Strategy

4.1. Difference-in-differences

Our first strategy to measure the causal effects of SNAP work requirements for the SNAP-eligible ABAWD population is a difference-in-differences design. Our second strategy is a difference-in-discontinuities design. The strength of having both strategies is that they rely on very different assumptions for identification and involve different subsamples in the data, but, as we will show, they converge to remarkably similar estimates.

A core challenge in the difference-in-differences design is the selection of an adequate control group whose experiences with financial and labor market conditions are likely to satisfy the parallel trends assumption and where the potential outcomes for the untreated group are likely to closely match the actual treatment group before the policy change. With a staggered policy roll-out, the standard difference-in-differences model implicitly compares treated units to all already-treated units, never-treated units, as well as units that are not yet treated among the entire sample (Goodman-Bacon 2021). Given the uneven spatial distribution of the negative effects of the Great Recession and the uneven recovery after it, the entire sample of untreated counties may not closely match the experiences of the treated counties prior to the imposition of the work requirements in trends or in levels. This is particularly true if there are heterogeneous effects over time.

We use Commuting Zone definitions from the USDA in the year 2000 to construct control groups that allow us to compare individuals facing similar labor markets and financial markets before and after the policy change. Controlling for Commuting Zones in this way is necessary to ensure that the treatment and control groups have similar trends before implementation. The 709 Commuting Zone designations in the US are larger than counties and reflect places with similar commuting patterns, meaning they can be interpreted as a "local labor market." Recent research has frequently used Commuting Zones to capture place-based differences in economic outcomes (Chetty et al. 2014, Yagen 2019). Most Commuting Zones are centered around major metropolitan areas, but many extend into nonmetropolitan areas that are highly integrated economically. They also can cross state boundaries, increasing the variation in waiver status within a single Commuting Zone.[17] Importantly, SNAP work requirement waivers at the county level are based upon the residence of the prospective recipient. Two individuals may experience different SNAP work requirements depending on which county they live in while still experiencing a common labor market if they live in the same Commuting Zone. This commonality means we can control for spatially correlated differences in economic conditions. Consequently, unobserved differences in financial and labor market characteristics are less likely to

[17] Approximately 79 percent of people live in a commuting zone where there is variation in the timing of work requirements are reintroduced across counties.

confound our results because people living in different counties in the same Commuting Zone are participating in a common market that follow common trends.

With this framework in mind, we estimate the following difference-in-differences equation:

(1) $Y_{icgt} = \beta_0 + \beta_1 WorkReq_{ct} + X_{it}\,\beta_2 + Z_{ct}\beta_3 + \delta_i + \mu_c + \gamma_{gt} + \varepsilon_{icgt}$,

where *WorkReq* equals one beginning in the quarter-year *t* in which individual *i* encountered a work requirement while living in county *c (*in Commuting Zone *g).* The *X* vector contains individual-level controls from the CCP, which, in this case, is limited to age and age squared. The *Z* vector captures county-level controls that may influence financial outcomes, SNAP eligibility, and selection into work requirement status in the county, namely, the county unemployment and labor force participation rates. We select lagged labor market measures because contemporaneous measures like unemployment and labor force participation may be intermediate inputs to financial outcomes in the same period, and thus may be colliders if included in the regression. The vector also includes local neighborhood (Census tract) values of poverty rates in calendar year *t-1,* total population and population density, the share of the population under age 18, the share of the neighborhood's households with children at home, the share of the adult population with a bachelor's degree or more, and population shares that are non-Hispanic white, non-Hispanic black, and Hispanic. *Z* also includes state-level policy and political variables – indicators for if the state expanded Medicaid by January of the calendar year including quarter *t*, indicators for the party controlling the state legislature, and indicators for the party affiliation of the governor in that calendar year.

We include fixed effects for each individual (δ_i), each county (μ_c) and Commuting-Zone-by-quarter-year interacted fixed effects (γ_{gt}). These fixed effects control for time-invariant characteristics of each person, time-varying trends and shocks across commuting zones, and time-invariant differences in county characteristics (in the case of a move). Conditional on these fixed effects, the coefficient of interest, β_1, measures the average change in each outcome for individuals in counties after a work requirement took effect in relation to others in the same Commuting Zone that did not have a work requirement in the same quarter, which we argue represents the causal average treatment effect. We cluster our standard errors by counties because treatment varies at this level.

To examine dynamic effects of the work requirements, we estimate an event study model:

(2) $Y_{icgt} = \beta_0 + \sum_{e=-12;\, e \neq -1}^{12} \alpha_e I_e + X_{it}\,\beta_2 + Z_{ct}\beta_3 + \delta_i + \mu_c + \gamma_{gt} + \varepsilon_{icgt}$,

where each indicator *I* represents *e* time (in quarters) relative to when person *i* experienced the imposition of SNAP work requirements. The magnitudes of the *I* coefficients trace out pre-treatment trends in the treatment group as well as dynamic treatment effects in the treated counties in the same

Commuting Zone relative to the untreated or not-yet-treated counties across quarters. We estimate these models in three-year windows around the treatment date. This exercise can reveal if there are different trends across these groups that may violate the parallel trends assumption. The coefficients can also reveal if there is an anticipatory or time-varying response to the reimposition of work requirements. These dynamic effects are particularly important if work requirements generate an immediate financial shock that consumers must accommodate over time. These dynamic effects also provide information on how consumers affected by work requirements absorb short-term increases in out-of-pocket costs for food.

4.2. Difference-in-Discontinuities

To further test the effects of work requirements on credit outcomes, we conduct a separate analysis using a difference-in-discontinuities design. In this design, we estimate linear functions in age for each outcome in treated counties above and below the age 50 ABAWD cutoff before and after the work requirements came into effect:

$$(3)\quad Y_{it} = \beta_0 + \beta_1(age - 50) + \beta_2 Under50 + \beta_3 Under50 * (age - 50) + WorkReq[\alpha_1(age - 50) + \alpha_2 Under50 + \alpha_3 Under50 * (age - 50)] + \pi_t + \varepsilon_{it}$$

The β_1 and β_3 coefficients trace out first-order polynomial fits of the relationship between age and each outcome above and below age 50, respectively, before the work requirements took effect, and β_2 captures the measured discontinuity before the work requirements went into effect in treated counties. The α coefficients estimate the change in these parameters after the work requirements went into effect. The coefficient α_2, therefore, captures the change in the estimated discontinuity in after the work requirements went into effect net of any changes in the slopes of the relationships between age and each outcome (α_1 and α_3). We include quarter fixed effects (π_t) in order to control for common secular shocks across the sample such as the national recovery from the Great Recession and to account for differences in treatment timing. Like in our difference-in-differences model, we estimate this model for a window of 3 years before and after the treatment for those that experience a change in work requirement status.

Because our sample consists of a panel of people observed each quarter, individuals may age out of the treatment group and into the control group over the course of the estimation window. To avoid contaminating the control group, consistent with the approach in Gray et al. (2023), we estimate a "donut" discontinuity in which we eliminate from the estimation sample those age 47–50 who might age out of the treatment group as well as those over 50 that were previously exposed to work

requirements while they were under age 50.[18] Specifically, this donut is necessary to reflect that those who are just under the age-50 threshold may be treated briefly and have their credit affected, which can continue to have effects even after they age out of the work requirement and would therefore downwardly bias our results. In these estimates, we two-way cluster our standard errors: the level of the running variable (age) as well as county because both factors determine treatment status.

We estimate our models using a bandwidth of 10 years on either side of the age 50 cutoff. From a technical standpoint, because our dataset consists of discrete age units that do not vary with every quarter, the dataset suffers from a lack of continuous identifying variation over the running variable. This complicates the interpretation of the various optimal bandwidth selections.[19] Notwithstanding these limitations, it is still worth calculating these optimal bandwidths for comparison. When considering card balances, the mean squared error (MSE) optimal bandwidths in the Calonico, Cattaneo, and Titiunik (2014) battery of tests is 5.2 to 6 years, which gives us only 2-3 age years in our donut design to estimate the linear model, which we argue is too few. The coverage error rate (CER) optimal bandwidths range from 2.5 to 9.7. Given these results, a bandwidth of 10 years allows us to follow relatively closely to these tests—acknowledging their shortcomings in this context—while leveraging sufficient data to avoid misspecifying the linear model. Our choice of a 10-year bandwidth is, therefore, motivated by balancing the age profile observed in our sample and current best practices. However, as discussed in the results section and in the appendix, we find similar results with alternate bandwidths and when we relax the linearity assumption and broaden our bandwidth to 20 years.

One reason we use this difference-in-discontinuities framework is because other unobservable factors that influence an individual's financial life may also change at age 50 – including other public policies with thresholds at age 50. Looking at the age patterns in the outcomes before the implementation of work requirements, there is little evidence that such differences exist. This can also

[18] This includes those age 51, 52, and 53 during the first, second, and third years after the work requirements. We eliminate those age 50 because we cannot observe month or quarter of birth and are, therefore, left to infer age from the year of birth. Those turning 50 in that calendar year may age out of the ABAWD range in different quarters, which may attenuate our estimates.

[19] A discrete running variable is generally not a problem for estimation. In such cases, "a simple local linear extrapolation towards the cutoff may be a reasonable strategy," and if the number of unique points in the running variable is relatively small, "bandwidth selection methods will not be appropriate; in this case, the researcher may conduct linear parametric extrapolation globally, fitting the polynomial using all the observations… This runs counter to the local nature of the RD parameter, but it is essentially the only possibility for implementation if the goal is to estimate the canonical continuity-based RD parameter." (Cattaneo, Idrobo, and Titiunik, 2023).

be seen by looking at areas where work requirements were never implemented.[20] Nonetheless, the difference-in-discontinuities approach accounts for any unobserved factors because we are interested in the *change* in the relationship after the treatment. This approach delivers unbiased estimates of the causal effect of the treatment so long as the unobserved factors that change at age 50 do not systematically vary with county-level work requirements. Such a relationship is generally unlikely.

One important caveat to this approach is that the gap in outcomes between the general credit population at any particular age and our sample of those with a maximum Risk Score below 700 who do not have a mortgage is likely to be larger as age increases. Appendix Figures B7 and B8 provide some evidence that our selected sample is relatively more credit constrained, more disadvantaged, and relatively more reliant on consumer finance accounts at ages 40–60 than those in the general population that have a credit score. This is important for contextualizing some of our estimates.

5. Results

5.1. Difference-in-Differences Estimates

We first present the results of our difference-in-differences estimates in Table 2. Once controlling for the other local characteristics, our sample population of probable renters (and those who own their home free and clear) with low or moderate Risk Scores had 0.124 additional inquiries on their credit account in the past six months (Panel A) after the introduction of new work requirements. The mean number of inquiries in our sample is just under one, so the additional 0.124 inquires reflects a 12.9 percent increase at the sample mean. This effect includes effects at the extensive margin (having any inquiries) and intensive margin (number of inquiries conditional on any inquiries). As an additional outcome related to the "flow" of credit, we also consider the probability of opening a new account, which increased by 1.8 percentage points (6.7 percent at the sample mean).

Consistent with the increase in inquiries in areas with work requirements, we also see an increase in the average number of total card accounts, which represents one aspect of the "stock" of available credit. Individuals have 0.08 more open accounts on average in the 3 years after the introduction of new work requirements. At the sample mean, this reflects a 5.5 percent increase in total card accounts.

As consumers increase the number of accounts to which they have access, they also may seek additional credit at the intensive margin, meaning higher credit limits on existing accounts. We

[20] In Appendix Figures B1 and B2, we randomly assign the never-treated cohorts a placebo treatment date. There is no systematic change at age 50 after the placebo treatment date and no systematic discontinuities before or after this date across outcomes.

	Panel A			
VARIABLES	(1) Inquiries in Past 6 Months	(2) Pr(New Account)	(3) Total Card Accounts	(4) Total Card Credit Limit
Coefficient	0.124***	0.0182***	0.0827***	391.7***
	(0.0125)	(0.00334)	(0.0109)	(67.88)
Dep. Var. Control Mean	0.9642	0.2713	1.51	3,953.63
Pct Effect at Control Mean	12.86%	6.71%	5.47%	9.91%
Observations	6,175,994	6,280,242	6,280,242	6,280,242
R-squared	0.381	0.306	0.839	0.715

	Panel B			
VARIABLES	(1) Total Card Balance	(2) Pr(Any Card Past Due)	(3) Consumer Finance Total Balance	(4) Pr(Consumer Finance Past Due)
Coefficient	132.2***	0.00481***	15.77**	-0.000223
	(21.14)	(0.00186)	(7.692)	(0.000781)
Dep. Var. Control Mean	1,675.61	0.2562	339.29	0.0379
Pct Effect at Control Mean	7.89%	1.88%	4.65%	-0.59%
Observations	6,280,242	6,280,242	6,280,242	6,280,242
R-squared	0.785	0.577	0.58	0.583
Robust standard errors in parentheses				

Table 2. Difference-in-Differences Results
Source: Authors' calculations using FRBNY Consumer Credit Panel/Equifax Data.
Notes: Estimates correspond to Equation 1. Standard errors clustered at the county level.

therefore consider the total credit limit on all card accounts because this reflects the combination of extensive and intensive margin adjustments.[21] Our results show that SNAP work requirements increased the total credit limit of consumers by approximately $392 (9.9 percent).

[21] Mechanically, if a consumer opens an additional credit card account, their total credit limit will also increase. However, new credit accounts also exhibit different limit growth trajectory than established accounts. For borrowers who receive their first credit card, their total limit frequently more than doubles in during their first year with the card account (Livshits, 2022).

Consumers are also using this additional credit and increasing the balances they carry on their accounts. Our results in Panel B find that consumers increase their total balance by $132 (7.9 percent), indicating that these borrowers are carrying larger debt burdens as a result of the work requirements. As an indication that some people who are increasing their borrowing experience financial distress, there is also an increase in the share of borrowers who are past due after the imposition of work requirements. Column 2 of Panel B shows that consumers were 0.5 percentage points more likely to be past due on their card payments as a result of the work requirements, an increase of approximately 1.9 percent. In addition, in column 3, we see that consumers increased their balance of consumer finance debt by approximately $16 (4.7 percent).

Taken together, the results of our difference-in-differences estimates strongly point to consumers seeking out credit, obtaining credit, and carrying higher credit balances in order to make up for lost SNAP benefits. There is also a small uptick in the share of consumers that fall behind on their card payments, reflecting the financial vulnerability of their position.

The rise in being past due on their credit and retail cards is small relative to the size of the overall sample. However, it is instructive to understand exactly who runs the risk of falling behind on payments, and it is straightforward to hypothesize that it is the set of people induced to seek credit that may be most at risk. To explore this, we estimate our difference-in-differences model for past due card accounts but add an additional interaction between the work requirements variable and an indicator for whether or not the person had either opened a new account or else had an increase in their card credit limit of 25% or more in the prior 18 months. This interaction tells us if there is a differential effect on the likelihood of past due cards for those that had new credit in the recent past. The base effect is -0.0003 (s.e. = 0.0019), while the interaction has a coefficient of 0.0181 (s.e. = 0.00108). The increase in the likelihood of being past due, therefore, appears concentrated among those who had a recent increase in credit in the prior 18 months. This helps us to rule out the hypothesis that those obtaining new credit are substantially more creditworthy, which we also discuss below.

5.2. Event Studies

Using an event study framework, we can observe the dynamic treatment effects around the imposition of work requirements, doing so in Figures 4 and 5. These figures confirm the results of the difference-in-differences estimates and add additional context.

Panel A of Figure 4 show that there is a substantial increase in the number of inquiries that a consumer has on their credit report. There similarly is a substantial increase in the likelihood of opening a new account of any kind in the past six months (Panel B). However, the pattern at the extensive

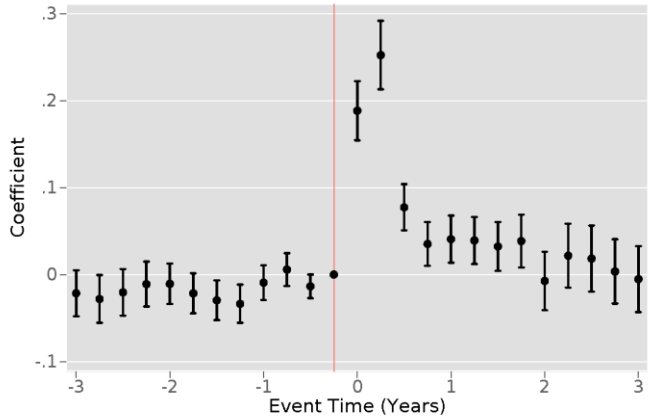
margin of new account openings is short lived, rising 4-5 percentage points (15 percent) in the first two quarters before falling marginally for the next two quarters and returning to the prior trend. Account openings are "sticky" in the sense that a new account now reduces the need for a new account tomorrow, which explains the brief fall in quarters 3-4. SNAP work requirements result in a short-term spike in new credit seeking behaviors in the "flow" of credit and generate higher numbers of inquiries each quarter for at least six months after the work requirements begin. Despite the relatively brief effect on new account openings, there is a lasting effect on the "stock" of credit accounts. Panel C shows this lasting increase in accounts after new SNAP work requirements (which can result from both the opening of new accounts or a decline in account closures). Panel D of Figure 4 shows that total credit limits on all credit and retail cards rises gradually as new accounts are opened and credit limits on existing accounts rise. This is instructive because this measure incorporates information on *all* margins of card credit: the existence of accounts and the credit available on those existing accounts.

Overall, Figure 4 provides strong evidence that the treatment effects of the work requirements appear within the first several months of the work requirements for the flow of credit.[22] This shows that the short-lived spike in credit inquiries is larger than it appears from the difference-in-differences estimates, which aggregate the entire "post" period. It is also clear that within commuting zones, the control counties appear to satisfy the parallel trends assumption, and any common shocks to the commuting zone would be controlled for with our quarter-year by commuting zone fixed effects.

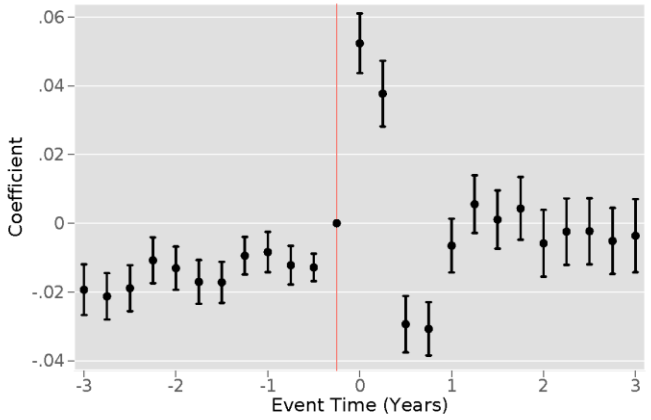
Figure 5 shows the results relating to the total amount of card debt, past due card balances, consumer finance debt, and past due consumer finance payments. Panel A of Figure 5 for total card balances follows a similar pattern as the credit limits in Panel D of Figure 4. Total balances exhibit a small change shortly after the work requirements, but total balances grow significantly over time during the first two years.[23] These growing balances may be an indication of individuals' budgets becoming strained after losing SNAP eligibility without an increase in labor earnings, resulting in increased debt.

[22] Since the USDA (2015) recommends that states notify ABAWDs about new work requirements one month prior to the waivers ending, this rapid response could reflect anticipatory credit seeking before individuals lose benefits.
[23] In some cases, the initial increase in credit seeking behavior and credit usage could reflect precautionary behaviors if uncertain about future resources, and these effects may appear in anticipation of failing to recertify for SNAP benefits. This would be consistent with precautionary borrowing behaviors observed previously by Telyukova (2013); Gorbachev and Luengo-Prado (2019); Druedahl and Jørgensen (2018). Since credit bureau dataset we use does not include any transaction-level information, we cannot speak to the type of spending and consumption that consumers are seeking credit for or using their credit on when increasing balances.

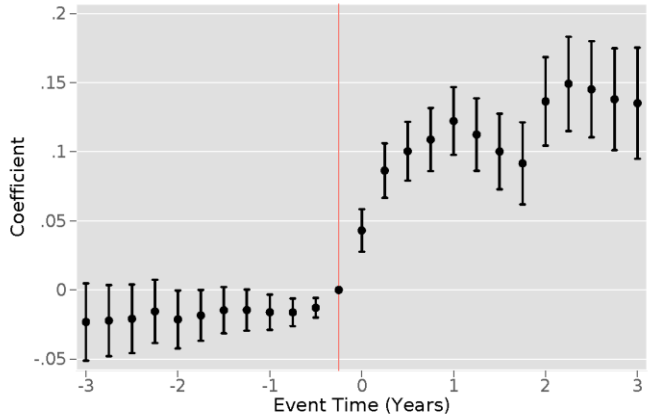
Panel A. Inquiries in the Last Six Months



Panel B. Pr(Any New Account in the Last Six Months)



Panel C. Total Number of Card Accounts



Panel D. Total Credit Limit on All Cards

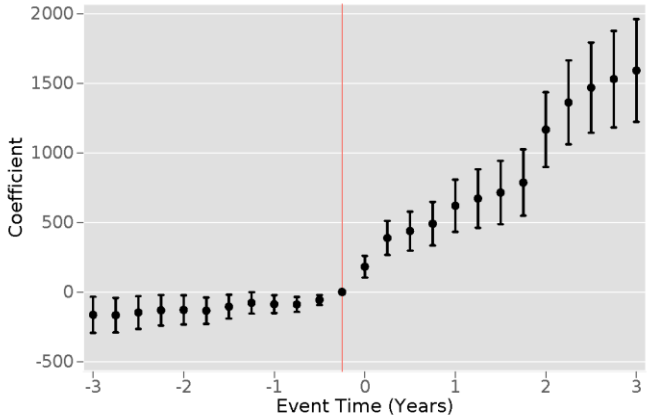
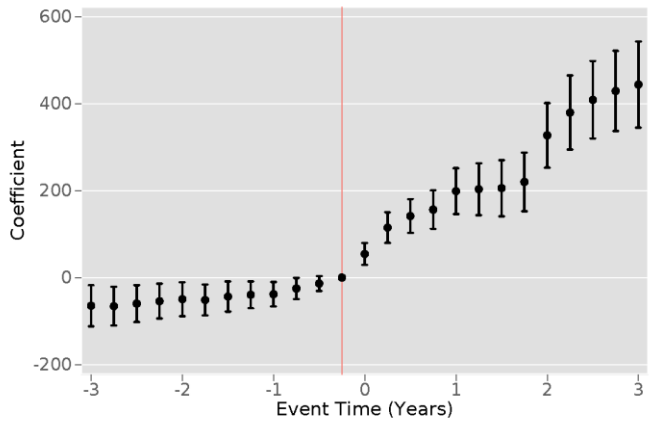
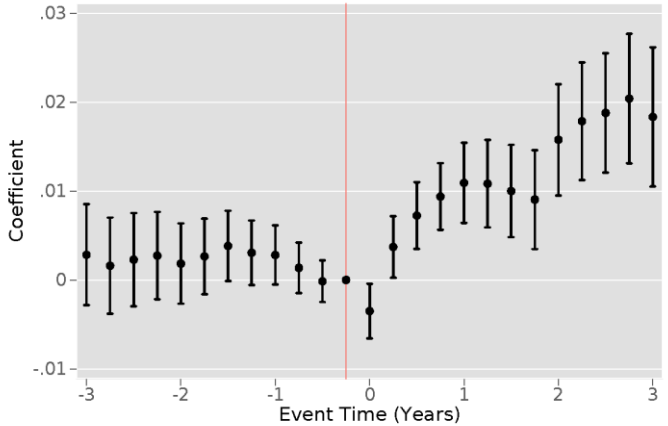


Figure 4. Event Study Results for Credit Inquiries, New Accounts, and Total Accounts
Source: Authors' calculations using FRBNY Consumer Credit Panel/Equifax Data.
Notes: Estimates correspond to Equation 2. Standard errors clustered at the county level.

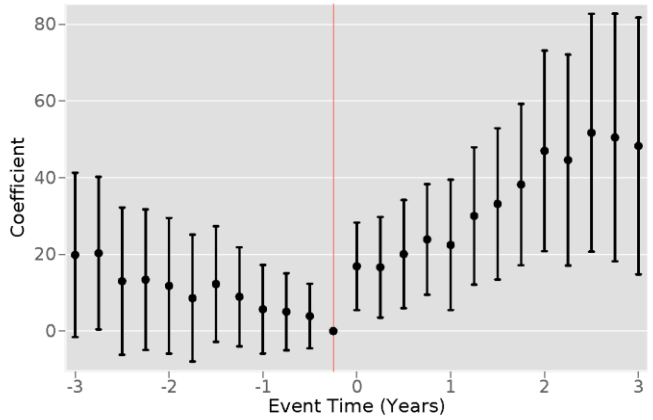
Panel A. Total Card Balance



Panel B. Pr(Any Card Past Due)



Panel C. Consumer Finance Total Balance



Panel D. Pr(Consumer Finance Past Due)

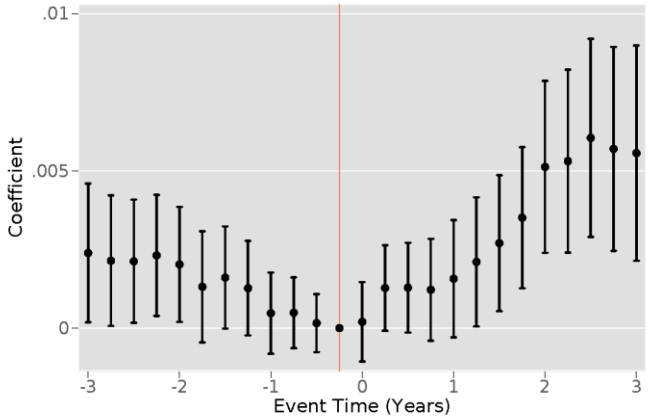


Figure 5. Event Study Results for Total Card Balance, Pr(Any Card Past Due), Consumer Finance Balance, and Pr(Consumer Finance Past Due)
Source: Authors' calculations using FRBNY Consumer Credit Panel/Equifax Data.
Notes: Estimates correspond to Equation 2. Standard errors clustered at the county level.

Because a borrower can remain current by making a minimum payment, the risk of past due debt may not necessarily track balances over time. Nevertheless, Panel B shows that the risk of having past due payments peaks after the first year and remains elevated for at least three years. The initial decline in past-due balances after work requirements go into effect likely reflects consumers prioritizing card payments prior to seeking new credit (Fulford and Stavins, 2022) and the fact that the new accounts must be open for a minimum of about two months prior to becoming past due.[24] However, the risk of being past due quickly increases over the subsequent three quarters, reaching levels as high as 1 percentage point higher (4 percent) by the end of the first year.

Similar to the patterns for credit and retail cards, balances on consumer finance products (Panel C) increase quickly over the first two years after the work requirements, reaching about $50 more by the end of the second year. In Panel D, we see that consumers also fall behind on their consumer finance payments, increasing their likelihood of past due payments by one half of a percentage point by the second year of the work requirements, which represents a nearly 14 percent increase relative to the control sample mean. Importantly, our base difference-in-differences estimates may not reflect the full picture of treatment effects for these two outcomes because consumer finance activity in treated counties was trending downward during the pre-treatment period relative to control counties in the same commuting zone, a trend that reversed after the work requirements came into effect.

5.3. Mechanisms

We argue that this rise in credit-seeking behaviors and card balances is likely the result of financial need on the part of prospective SNAP-eligible consumers. However, an alternate explanation is that there is an increase in credit supply associated with work requirements and more people working.

As a first test of whether credit supply or demand is primarily driving our results, we can also look at the change in Risk Scores and success rates for credit applications after the imposition of work requirements. As a proxy for success rates, we measure the number of new accounts per inquiry following Gross, Notowidigdo, and Wang (2020), which is the number of new accounts opened in the last six months divided by the number of inquiries in the last six months. If the increases in credit activities are attributable to increases in credit supply (if employment increases so borrowers are more creditworthy), we should expect to see increases in risk scores in areas with newly imposed work requirements and increases in the success rates of new credit applicants. However, as seen in Table 3, this did not occur. New work requirements are associated with small declines in risk scores and in

[24] Supporting the view that this temporary decline relates to consumer behavior around opening new accounts, when restricting the sample to individuals who did not open a new account, no decline is observed.

success rates for credit applications in our differences-in-differences regression.[25] Consequently, the increase in new accounts seen previously is a result consumers' seeking more credit during that time but these consumers are still relatively less likely than others to obtain credit from any given application. Consequently, it does not appear that lenders are increasing the supply of credit to borrowers in areas following new work requirements. Rather, this suggests that our findings are due to increases in consumers' demand for credit rather than an expansion of credit supply.

	(1)	(2)
VARIABLES	Equifax Risk Score	New Accounts Per Inquiry
Difference-in-Differences Estimate	-1.651***	-0.0101**
	(0.403)	(0.00471)
Observations (3 Years)	5,770,455	2,903,161
Dep. Variable Mean	613	0.608
Pct Effect at Mean	-0.27%	-1.66%

Table 3. Difference-in-Differences Coefficients - Risk Score and Success Rate
Source: Authors' calculations using FRBNY Consumer Credit Panel/Equifax Data.
Notes: Estimates correspond to Equation 1. Standard errors clustered at the county level.

As a second test of whether credit demand is driving our results, we consider the effects of asset limits on the trend in credit balances. These asset limits have previously been found to substantially reduce the level of assets of participants, including reducing their odds of having a bank account or $500 of emergency savings (Ratcliffe et al. 2016). If credit supply is driving the trends, because of their stronger financial situation those with more monetary assets prior to SNAP work requirements being imposed should exhibit similar (or even larger/faster) changes in balances to those with fewer available assets.[26] This is not the case.

To test this, we compare our estimated effects in states that had asset limits in place for SNAP recipients to estimates in states without asset limits. Larger relative changes in credit-seeking behavior and card balances in states with asset limits strengthens the case that these consumers are encountering new financial constraints that they cannot pay down with existing assets. Figure 6 presents our results for total balances when we separately estimate these effects in asset limit states versus

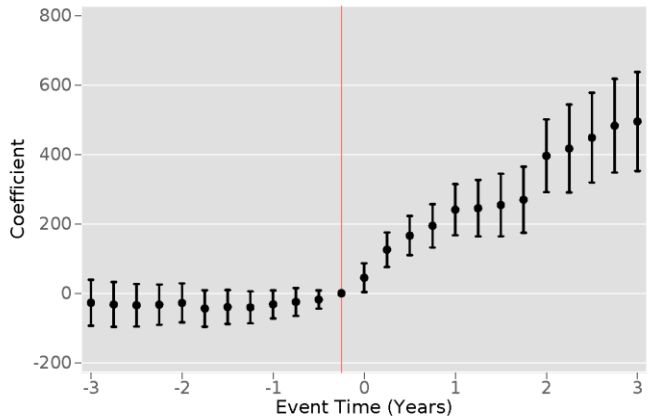
[25] Using the difference-in-discontinuities approach, we observed a larger decline in Equifax Risk Scores and no significant change in accounts per inquiry. See Appendix Table B5.
[26] However, we acknowledge, that since the credit supply effect is indirect through increases in employment, it is possible that the asset limits lead to more intensive job searching, which if successful could increase credit supply.

states without asset limits (Panels A and B) and when we use a full interacted model allowing for differential effects in states with asset limits compared to states without asset limits (Panel C).

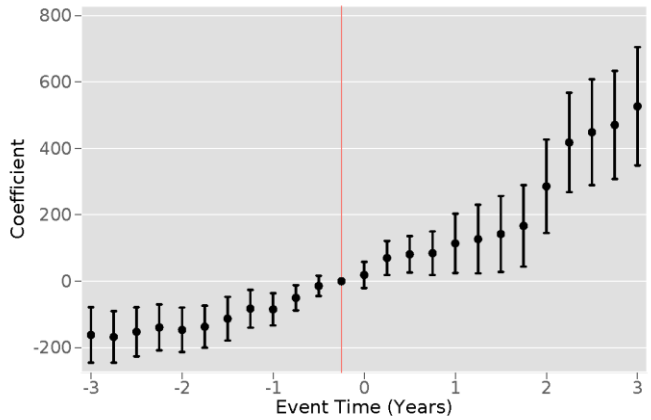
Panels A and B show that there is a larger increase in card balances in states with asset limits in the first few quarters after work requirements go into effect, and these balances increase for approximately two years after the work requirements. Those in states without asset limits for SNAP participation experience a smaller shock to total card balances in the initial quarters, and these balances remain stable for approximately two years before exhibiting a larger increase in later years. This is consistent with those who have more available assets spending down a portion of their assets instead of taking on costly card debts soon after work requirements are imposed. This is not consistent with a straightforward expansion in credit supply. Panel C makes it clear that the gap in the size of the effects is statistically significant from approximately the third quarter after the work requirements until the end of the second year, after which those in states without asset limits see their total balances catch up to those in states with asset limits. Taken together, these results suggest that the new debts taken on by those near SNAP eligibility are reflecting the need for new credit to finance consumption. Those that are disallowed from having more liquid assets for SNAP eligibility seek more credit, which is likely due to those with more available assets being able to partially finance their consumption out of existing assets rather than debt. These results also underscore the fact that asset limits for SNAP eligibility may increase pressure to take on credit card debts due to a lack of substantial savings.

Finally, although the prior literature has ruled out employment effects that fully counteract SNAP enrollment declines among those affected by work requirements (see e.g. Gray et al. 2023), if an increase in local employment or improving economic conditions is driving increased opportunities for credit among those in our sample, we would expect including contemporaneous controls for the labor force and unemployment rate to affect our estimates. As seen in Appendix Table B1, this is not the case. Our estimates are nearly identical with contemporaneous controls, suggesting that contemporaneous improvements in local labor markets that lead to an increase in credit supply are not driving our results.

Panel A. With Asset Limits: Total Balance



Panel B. Without Asset Limits: Total Balance



Panel C. Differences Between Asset Limit and No Asset Limit States – Joint Model

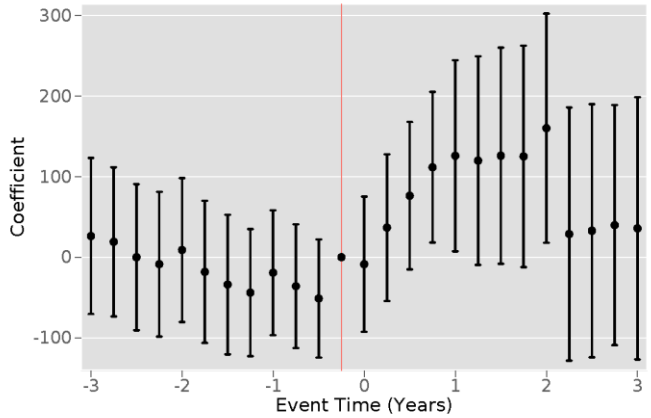


Figure 6. Comparison of Effects in States with and without Asset Limits
Source: Authors' calculations using FRBNY Consumer Credit Panel/Equifax Data.
Notes: Estimates correspond to Equation 2. Standard errors clustered at the county level.

5.4. Intent-to-Treat Effects and Non-ABAWDs

Our sample from the CCP cannot narrowly select the SNAP-eligible ABAWD population due to a lack of demographic information on dependents, family status, income, or disability. We therefore interpret our estimates as an intent-to-treat effect. Importantly, the effects of ABAWD work requirements for SNAP may not be isolated to only ABAWDs themselves. ABAWDs may share a household and financial responsibilities with others if cohabiting, married, or living with other individuals. Incomes among ABAWDs eligible for SNAP during time limit exemption periods are particularly low as documented in Han (2022), so if this low-income status is correlated with a propensity to have others in the home that share financial responsibilities, there may be spillovers within households. In addition, lost SNAP benefits among very low-income ABAWDs may necessitate financial help from others outside the household but who live nearby and are able to assist. Both mechanisms may drive an increase in credit-seeking and debt levels among lower-income adults who are not ABAWDs but are nonetheless affected by the work requirements indirectly. Furthermore, increases in administrative burdens from work requirements may reduce take-up rates of benefits even among those who are otherwise eligible.[27] However, with the available data, it is not feasible to test the extent to which results reflect the effects for the population directly affected versus those who may be indirectly affected in these ways. We therefore cannot rule out spillover effects.

ABAWDs likely represent at most 10 percent of our total CCP sample based on tabulations of public-use microdata samples from the American Community Survey. Therefore, the implied treatment-on-the-treated effects on total card balances for our difference-in-differences estimates suggests that, on average, there is an implied total increase in card balances of approximately $2,500 per ABAWD who has lost SNAP benefits during the first three years of the work requirement.[28] For comparison, based on average monthly benefits for households without children from CBO (2012), the annual value of ABAWD SNAP benefits is approximately $2,300 and three-years of benefits is just over $6,900.

[27] Researchers have long documented the large share of apparently eligible individuals who do not take up public benefits (Currie 2006). Homonoff and Somerville (2021) illustrate SNAP disenrollment effects from the administrative burdens of the SNAP recertification process. Finkelstein and Notowidigdo (2019) use a novel experiment to document both the information costs and transaction costs associated with SNAP enrollment. Bhargava and Manoli (2015) and Daigneault and Mace (2020) similarly observe that administrative burdens reduce take up rates of tax credits and public benefit programs.

[28] In order to estimate the effect per ABAWD who lost benefits, it requires knowing both the share of the sample who is an ABAWD and the share who likely lost benefits from work requirements. This calculation reflects the estimate from Table 2, scaled up by the fact that 10 percent of the sample are ABAWDS and, according to Gray et al. (2023) and other recent research, approximately half of ABAWDS lose snap benefits after the implementation of work requirements.

Several mechanisms may explain the large credit response effect beyond just the need to offset lost benefits. First, revolving balances and fees for missed payments can add a substantial amount to credit card balances, so the rise we find in past due risk may contribute to a rise in balances beyond the cash value of lost SNAP benefits.[29] Second, recent research suggests that when individuals seek and obtain additional credit, they incur higher debt balances as a behavioral response to the credit itself (Fulford and Schuh, 2017). This implies that as the work requirements induced the need for more credit, the additional credit itself may have partially induced higher balances.

Additionally, some of the increase in outstanding debt may simply reflect how people are using their credit cards. A share of borrowers may use their credit cards for normal purchases and pay off most or all of the balances in full each month. The credit balance for these "transactors" who pay off their balance each month will still appear in the data with their outstanding credit balance based on the day that the credit data is pulled.[30] The Federal Reserve (2020) finds that over half of people with a credit card do not have a persistent balance in any given month and Fulford and Shuh (2017) estimate that at least 35 percent of users ages 25-50 are "convenience users," meaning they do not carry revolving debts from month to month. If some people shift their spending from SNAP benefits to a credit card, average credit card balances will increase even if they pay it off each month.

Finally, to the extent that the work requirements increase employment, for those who find employment there may be a credit supply response in addition to the increase in credit demand. However, recent research from Gray et al. (2023) suggesting that the employment gains from new work requirements are small, as well as the flat to declining credit scores and success rates that we observe after new work requirements, provides evidence that this credit supply response may be small.

5.5. Difference-in-Discontinuities Estimates

In a second empirical design, we estimate a difference-in-discontinuities specification that leverages the age 50 cutoff for ABAWD status to identify the local average treatment effect of the work requirements. Before presenting our formal estimates, we first provide descriptive evidence for our approach in Figures 7 and 8, which both present binned sample means (conditional on quarter fixed

[29] In 2017, the average subprime interest rate on credit cards was approximately 25 percent and the deep subprime rate was approximately 30 percent (CFPB 2021). At a 25 percent interest rate, $2,000 of borrowing that is not paid off would grow to $3,125 of debt after 2 years and $3,900 after 3 years.

[30] Typically, the statement closing balance reported to the credit bureaus each billing cycle reflects a month of spending (per credit card). Individuals with multiple cards can have different reporting dates through the month.

effects) for each outcome for consumers by their age at the time the work requirements took effect. [31] We choose age at the time of the work requirement for ease of visualization of the omitted "donut" group. Those who were ages 47–50 at the time of the work requirement are omitted because they age across the age 50 threshold in the 3-year window, although we do show the outcomes of the omitted ages to show how heterogeneity in the length of exposure might influence the shape of the curve were the donut not imposed. In each panel, the black markers and lines represent outcomes during the period before the work requirements took effect, while the others represent outcomes after the work requirements took effect.

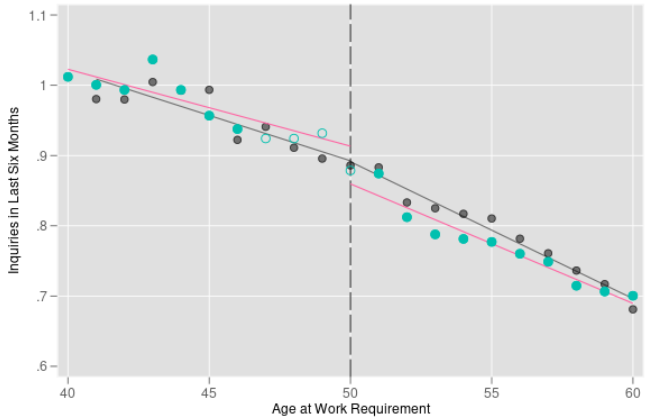
For each outcome, there is a significant change in the discontinuity in the outcome variable at age 50. As in our difference-in-differences estimates, we note that treatment effects grow when considering 1–2 year windows after the work requirements as the cumulative lost benefits increase over time. The discontinuity in a three-year window best approximates the difference-in-differences estimates for comparison.[32]

Table 4 shows our formal difference-in-discontinuities estimates for windows of one, two, and three years before and after the implementation of the work requirements among treated individuals. Columns 1 and 2 of Panel A suggest that, in the three years after the work requirements went into effect, the rate of inquiries and new account openings increased by 15.6 percent and 6.8 percent, respectively. In Column 3, total card accounts marginally increased by 4.2 percent. In contrast to our difference-in-differences results, the discontinuity in credit limits at age 50 did not change in a statistically significant way. Those near the age 50 cutoff in our sample of those with Risk Scores below 700 without a mortgage may be more disadvantaged or credit constrained relative to others their age compared to the full population (see Appendix Figures B7 and B8), which may explain this lack of expansion in the stock of credit.

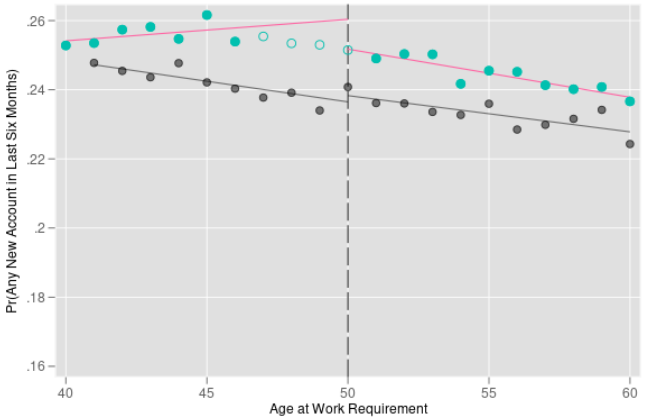
[31] We also use a placebo approach for untreated counties, randomly assigning a treatment year. When doing so, we observe that there are not significant changes in the discontinuity at age 50 for untreated counties, as there are for the treated counties. The graphs for this placebo test of "never-treated" counties are presented in Appendix Figures B1 and B2.

[32] We do not include those age 47-49 because we expect time-varying treatment effects and including those without the capacity to have been treated for the entire treatment window (3 years) would bias the estimates of the 3-year effects downward. To demonstrate the time-varying treatment effect, we include in Table 4 estimates for one and two-year exposure effects and narrow the excluded ages accordingly.

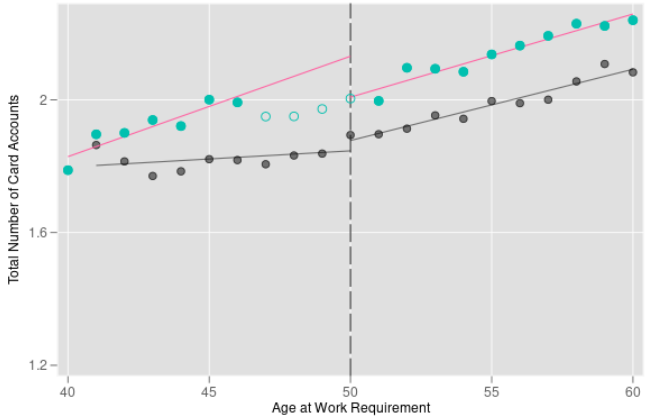
Panel A. Inquiries in the Last Six Months



Panel B. Probability of Opening a New Account



Panel C. Total Number of Card Accounts



Panel D. Total Card Credit Limit

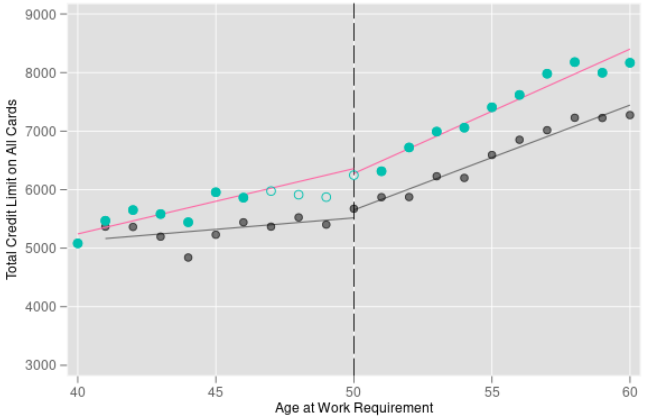
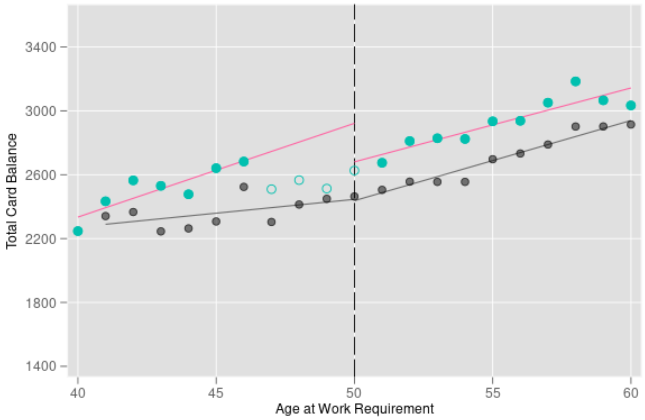
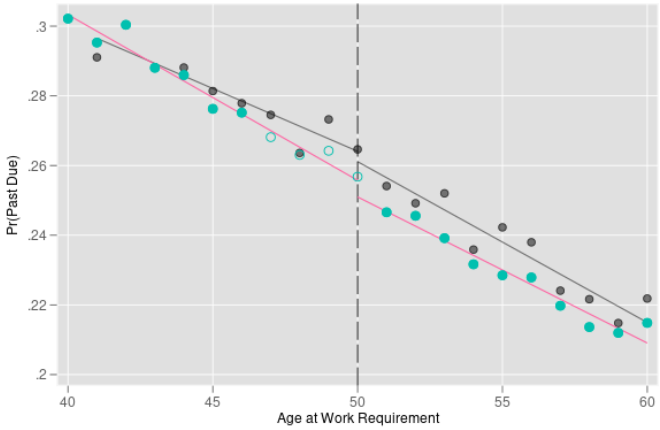


Figure 7. Age 50 Discontinuities Before and After Work Requirements for Credit Inquiries, Pr(New Account), Total Card Accounts, and Credit Limit
Source: Authors' calculations using FRBNY Consumer Credit Panel/Equifax Data.
Notes: Figures include quarter fixed effects. Hollow circles are for those age 47-50 at the time of the work requirement that would have aged into the control group by the end of the three-year post-treatment window. Black lines and dots represent the pre-treatment period.

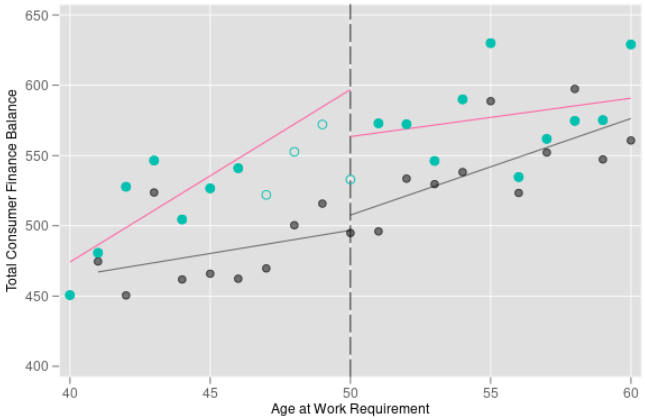
Panel A. Total Card Balance



Panel B. Pr(Any Card Past Due)



Panel C. Consumer Finance Total Balance



Panel D. Pr(Consumer Finance Past Due)

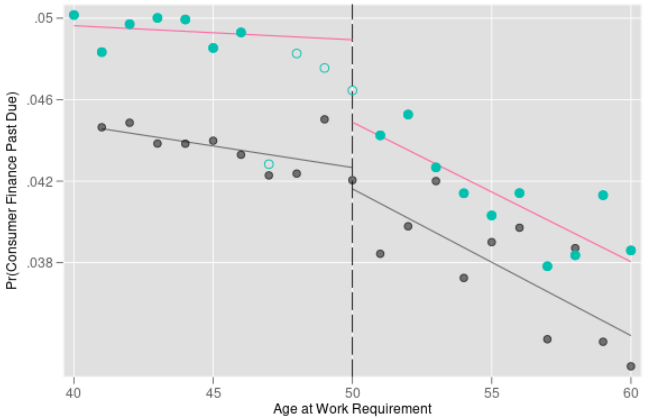


Figure 8. Age 50 Discontinuities Before and After Work Requirements for Total Card Balance, Pr(Any Card Past Due), Consumer Finance, and Pr(Consumer Finance Past Due)

Source: Authors' calculations using FRBNY Consumer Credit Panel/Equifax Data.

Notes: Figures include quarter fixed effects. Hollow circles are for those age 47-50 at the time of the work requirement that would have aged into the control group by the end of the three-year post-treatment window. Black lines and dots represent the pre-treatment period.

	Panel A			
	(1)	(2)	(3)	(4)
VARIABLES	Inquiries in Past 6 Months	Pr(New Account)	Total Card Accounts	Total Card Credit Limit
Difference-in-Discontinuity (3 Years)	0.122***	0.0166***	0.0883	99.86
	(0.0281)	(0.00449)	(0.0521)	(208.8)
Difference-in-Discontinuity (2-Years)	0.102***	0.00907**	0.0117	-173.0
	(0.0293)	(0.00361)	(0.0504)	(205.3)
Difference-in-Discontinuity (1-Year)	0.116***	0.0151***	-0.0335	-208.9
	(0.0254)	(0.00370)	(0.0446)	(185.5)
Observations (3 Years)	1,826,845	1,849,695	1,849,695	1,849,695
Dep. Variable Control Mean	0.7845	0.2454	2.12	7,232.28
Pct Effect at Mean (3 Years)	15.55%	6.76%	4.17%	1.38%

	Panel B			
	(1)	(2)	(3)	(4)
VARIABLES	Any Card Total Balance	Pr(Any Card Past Due)	Consumer Finance Total Balance	Pr(Consumer Finance Past Due)
Difference-in-Discontinuity (3 Years)	180.2**	0.0198***	114.3***	0.00973***
	(79.90)	(0.00535)	(34.76)	(0.00212)
Difference-in-Discontinuity (2-Years)	82.81	0.0162***	72.20*	0.00496
	(124.7)	(0.00432)	(37.26)	(0.00340)
Difference-in-Discontinuity (1-Year)	31.15	0.00829*	39.66	0.00139
	(108.0)	(0.00398)	(24.63)	(0.00251)
Observations (3 Years)	1,971,612	1,971,612	1,971,612	1,971,612
Dep. Variable Control Mean	2,888.39	0.2324	572.44	0.0418
Pct Effect at Mean (3 Years)	6.24%	8.52%	19.97%	23.28%
Robust standard errors in parentheses				
*** p<0.01, ** p<0.05, * p<0.1				

Table 4. Difference-in-Discontinuities Coefficients

Source: Authors' calculations using FRBNY Consumer Credit Panel/Equifax Data.

Notes: Estimates correspond to Equation 3. Standard errors two-way clustered at the age (running variable) and county level.

However, as shown in Panel B, balances on credit and retail cards among this population increased by $180 in the three years after the work requirements went into effect. This represents a 6.2 percent increase from the sample mean. This is a slightly larger dollar increase in card balances than our estimate in Table 2 ($132), but a slightly smaller percentage change at the sample mean since those near the age 50 cutoff have higher average balances. Yet given the difference in samples and estimation approaches, the results using the two approaches are quite consistent.

The likelihood of being past due on an account also increase by nearly 2 percentage points, or 8.5 percent relative to the control mean. Consumer finance balances and the likelihood of being past due on these payments also significantly increased. This increase is larger and more significant than our difference-in-differences estimates. Again, the relative disadvantage of our sample increases with age, meaning these consumers may be more likely to seek direct personal loans from a bank or credit union when other forms of credit like credit or retail cards are out of reach.

One important aspect of this difference-in-discontinuities design is that the change in the discontinuity will capture the local average treatment effect of the work requirements net of any spillovers that affect family and community members near the age 50 cutoff. This is because the spillovers will be captured by a change on the right side of the cutoff (over age 50) in a change in the slope, the intercept, or both.[33] The *differential* change on the left-hand side provides identification for the treatment effect net of any such spillovers to the non-ABAWD population over age 50.

5.6. Robustness of Results

There is an emerging econometric literature extoling the careful use of the difference-in-differences design when considering staggered treatments (e.g. Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021). One key insight from these recent papers is the importance of distinguishing between control units that are never treated, not yet treated, or already treated, particularly in the presence of heterogeneous treatment effects. In order to investigate this aspect of our design, we re-estimate our models following the adjustments suggested by Sun and Abraham (2021).[34] Though the estimates are somewhat noisier given constraints on computing power,

[33] These spillovers could be important if someone under age 50 lives with and shares resources with someone over age 50. The person over age 50 could be partially treated by because of the effect on the person they are living with. Additionally, the benefit rules could affect private transfers more broadly. Han, Meyer, and Sullivan (2022) observe that there is a substantial effect on transfers from family members after changes to public benefits program. To the extent that people over age 50 increase transfers to younger relatives affected by work requirements, these effects would downwardly bias our results.

[34] Because the number of interacted fixed effects and estimation subgroups is so large and results in infeasible computational requirements, we take a smaller random subsample of our main sample for these models.

the estimates in Appendix Table B2 are generally similar to our main approach, indicating that treatment effect estimates are not significantly affected by contamination of the earlier- to later-treated comparisons in the presence of heterogeneous treatment effects over time.[35] Appendix Figures B5 and B6 show the event study coefficients for these regressions and confirm that our results are similar under this approach as they are in our preferred specification. The general similarity ensures that negative weights from differential treatment timing are not leading to significant bias in our main estimates. Differences in magnitude may indicate that the earlier-treated to later-treated comparison groups contain information related to the timing of the effects. This is important because there are far fewer commuting zones containing never-treated counties. Importantly, the overall picture is very similar, and in most cases, our main estimates rest squarely within the confidence intervals of these alternative estimates.

When leveraging identifying variation around a discontinuity, there are two aspects to consider for robustness: estimation bandwidth and functional form. In our context, selection of both relates to the shape of the age profile for each outcome. In terms of bandwidth choices, we also attempted alternate bandwidths ranging from 6 to 15 years (see Appendix Figures B10 and B11 for details). In general, bandwidths from 8 to 12 show consistent estimates of the treatment effects across every outcome. At a narrow bandwidth of 6 years, which includes sparse coverage on the left side of the cutoff in our donut region, the estimates are smaller only for inquiries, past due cards, and consumer finance balances. When moving even one year further out to a bandwidth of 7 years in age, the estimates begin to converge. We see this as clear evidence of the robustness of our approach when there are enough points to specify a linear model. For some outcomes, the results attenuate when the bandwidth surpasses approximately 12-13 years. This is likely related to the nonlinear relationship between age and some of our outcomes between the ages of 30 and 50.

For nearly every outcome of interest, there is a significant change in the relationship between age and the outcome at around age 40 (see Appendix Figures B7 and B8 for details). While the relationship is largely linear beyond age 40, there is a clear curvature at earlier ages (see Appendix Figures B3 and B4). This curvature complicates estimates using bandwidths larger than ten years because wider bandwidths would then require the use of at least second-order polynomials.

[35] With the Sun and Abraham adjustments, the point estimates for total credit card limits and consumer finance total balances is higher, while the point estimates for inquiries, total card balance, and probability of new accounts (with the probability of having a new account no longer being statistically significant). Additionally, due to the larger standard errors with the Sun and Abraham adjustments, the likelihood of being past due is no longer significant, although the point estimate for this measure is nearly unchanged.

To address these dynamics, we estimate our difference-in-discontinuities models with second-order polynomials on either side of the age 50 cutoff and expand the bandwidth to 20 years. These results are in Appendix Table B3. The pattern of results matches closely those in Table 4, but with some additional noise.[36] Our results are, therefore, robust to expanding both polynomial degree and estimation bandwidth.

Finally, to ensure that other characteristics are not discontinuously changing at the age 50 cutoff, Appendix Figure B9 shows the pre-treatment relationship between age and a number of observable characteristics including the non-Hispanic white population share in the Census tract, tract poverty rate, county unemployment rate, and tract population. None of these shift significantly at the age 50 cutoff, making it unlikely that other unobserved compositional or location changes explain our results.

Taken together, across multiple specifications and identification strategies, our results point to the same conclusion: work requirements for SNAP increase credit-seeking in credit and retail cards, debt on these cards, and the incidence of past due debts among lower-income adults.

6. Conclusion

This paper advances our understanding of the financial repercussions of work requirements by measuring the causal effects of the SNAP work requirements on credit outcomes in high-quality administrative data. Most previous research has focused on the magnitude of employment gains from work requirements, and here we add to the literature by considering the costs of these policies on affected consumers.

Using both a difference-in-differences design and a difference-in-discontinuities design, we find evidence that SNAP work requirements increased credit-seeking behavior among lower-income renters. We find that borrowers in counties with work requirements applied for more new accounts, sought increases in credit limits, increased their total debt amounts on retail and credit cards, and experienced an elevated risk of having past due card debts. The effects are particularly pronounced and occur much faster in states that have asset limits attached to SNAP eligibility, pointing to a lack of liquid assets and the need to seek additional credit to fund consumption as the primary mechanism for the effects we find. In a similar fashion, after work requirements went into effect, those individuals near the age 50 cutoff that would have been subject to the work requirement increased their credit-seeking behavior, balances on card accounts and consumer finance accounts, and were more likely to fall behind on card

[36] Best practices generally advise against the use of more complex polynomials than necessary (Gelman and Imbens 2019), so we default to the use of the 10-year bandwidth with linear splines in our main strategy.

and consumer finance account payments compared to those over age 50 who were not subject to the work requirements.

These results provide evidence about a new dimension of the financial repercussions of work requirements beyond the employment effects commonly studied. They suggest that work requirements result in an increase in credit seeking behavior and outstanding debt as individuals lose access to public benefits. Our findings suggest that the individual-level harms for those who fail to quickly transition to employment or are hampered by new administrative burdens should be weighed against the benefits of encouraging work when considering the trade-offs of future work requirements for public benefits programs.

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Appendix to

Financial Repercussions of SNAP Work Requirements

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April 2023

A. Select features of the U.S. renter population ages 18 to 49

	Adults, 18 - 49, renters	Adults, age 18-49 without kids, renters
Gender		
Male	47.9%	55.3%
Female	52.1%	44.7%
Age		
18-24	24.6%	34.3%
25-29	21.8%	24.2%
30-34	17.1%	14.4%
35-39	13.6%	9.2%
40-44	12.0%	8.5%
45-49	11.0%	9.4%
Race/Ethnicity		
White, non-Hispanic	55.8%	58.9%
Black, non-Hispanic	19.9%	18.1%
Hispanic	17.0%	15.3%
Asian, non-Hispanic	3.7%	4.1%
Other race, non-Hispanic	3.6%	3.3%
Parental Status		
Has at least one child age <18	39.7%	--
Education		
Less than high school graduate	10.7%	8.9%
High school graduate	27.9%	25.6%
Some college/technical or associates degree	37.7%	37.1%
Bachelor's degree	17.3%	21.1%
Graduate or professional degree	6.4%	7.3%
Employment and earnings		
Employed	72.6%	73.5%
Employed for more than 20 hrs/week	69.1%	69.6%
Hours worked	31.0	31.2
Annual wage	$30,142	$30,172

Table A1. Characteristics of renters from the American Community Survey

Notes: Among renters. Dollar values are inflation adjusted to 2021 values.

Source: Authors' calculations using 2010-2017 1-year ACS PUMS samples from IPUMS USA.

B. Alternative Difference-in-Differences Specifications and Regression Discontinuity

	Panel A			
	(1)	(2)	(3)	(4)
VARIABLES	Inquiries in Past 6 Months	Pr(New Account)	Total Card Accounts	Total Card Credit Limit
Coefficient	0.125***	0.0185***	0.0818***	377.7***
	(0.0125)	(0.00336)	(0.0110)	(67.05)
Dep. Var. Control Mean	0.9642	0.2713	1.51	3,953.63
Pct Effect at Control Mean	12.96%	6.82%	5.41%	9.55%
Observations	6,175,994	6,280,242	6,280,242	6,280,242
R-squared	0.381	0.306	0.839	0.715

	Panel B			
	(1)	(2)	(3)	(4)
VARIABLES	Any Card Total Balance	Pr(Any Card Past Due)	Consumer Finance Total Balance	Pr(Consumer Finance Past Due)
Coefficient	129.5***	0.00486***	15.94**	-0.000289
	(21.03)	(0.00186)	(7.665)	(0.000784)
Dep. Var. Control Mean	1,675.61	0.2562	339.29	0.0379
Pct Effect at Control Mean	7.73%	1.90%	4.70%	-0.76%
Observations	6,280,242	6,280,242	6,280,242	6,280,242
R-squared	0.785	0.577	0.58	0.583

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B1. Difference-in-Differences Coefficients, Controlling for Contemporaneous Employment
Source: Authors' calculations using FRBNY Consumer Credit Panel/Equifax Data.
Notes: Estimates correspond to Equation 1 adding contemporaneous controls for labor force size and unemployment rate. Standard errors clustered at the county level.

Panel A

VARIABLES	(1) Inquiries in Past 6 Months	(2) Pr(New Account)	(3) Total Card Accounts	(4) Total Card Credit Limit
Coefficient	0.0687***	-0.003	0.0857***	462.89***
	(0.0194)	(0.0045)	(0.016)	(81.48)
Dep. Var. Control Mean	0.9642	0.2713	1.51	3,953.63
Pct Effect at Control Mean	7.12%	-0.11%	5.68%	11.71%
Observations	3,084,975	3,137,107	3,137,107	3,137,107
R-squared	0.392	0.310	0.842	0..828

Panel B

VARIABLES	(1) Any Card Total Balance	(2) Pr(Any Card Past Due)	(3) Consumer Finance Total Balance	(4) Pr(Consumer Finance Past Due)
Coefficient	77.912***	0.0049	28.61*	0.0001
	(29.26)	(0.0034)	(15.49)	(0.0012)
Dep. Var. Control Mean	1,675.61	0.2562	339.29	0.0379
Pct Effect at Control Mean	4.65%	1.91%	8.43%	0.26%
Observations	3,137,107	3,137,107	3,137,107	3,137,107
R-squared	0.787	0.581	0.596	0.585

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B2. Difference-in-Differences Coefficients - Sun and Abraham Adjustments
Source: Authors' calculations using FRBNY Consumer Credit Panel/Equifax Data.
Notes: Estimates correspond to Sun and Abraham (2021) adjusted estimates on a 50% random subsample of main estimation sample. Standard errors clustered at the county level.

	Panel A			
VARIABLES	(1) Inquiries in Past 6 Months	(2) Pr(New Account)	(3) Total Card Accounts	(4) Total Card Credit Limit
Difference-in-Discontinuity (3 Years)	0.0813**	0.0150***	0.0792	83.03
	(0.0323)	(0.00388)	(0.0636)	(275.1)
Difference-in-Discontinuity (2-Years)	0.0780***	0.00880***	0.0116	-161.4
	(0.0287)	(0.00324)	(0.0544)	(248.5)
Difference-in-Discontinuity (1-Year)	0.108***	0.0147***	-0.0403	-221.0
	(0.0269)	(0.00399)	(0.0459)	(226.7)
Observations	4,112,250	4,112,250	4,112,250	4,112,250
Dep. Variable Control Mean	0.7845	0.2454	2.12	7,232.28
Pct Effect at Mean (3 Years)	10.36%	6.11%	3.74%	1.15%

	Panel B			
VARIABLES	(1) Any Card Total Balance	(2) Pr(Any Card Past Due)	(3) Consumer Finance Total Balance	(4) Pr(Consumer Finance Past Due)
Difference-in-Discontinuity (3 Years)	170.8	0.0207***	46.69	0.00814***
	(104.3)	(0.00578)	(37.61)	(0.00285)
Difference-in-Discontinuity (2-Years)	56.32	0.0164***	38.24	0.00271
	(126.2)	(0.00498)	(34.86)	(0.00353)
Difference-in-Discontinuity (1-Year)	-5.593	0.00803*	24.35	-7.89e-05
	(111.2)	(0.00398)	(21.34)	(0.00269)
Observations	4,112,250	4,112,250	4,112,250	4,112,250
Dep. Variable Control Mean	2,888.39	0.2324	572.44	0.0418
Pct Effect at Mean (3 Years)	5.91%	8.91%	8.16%	19.47%
Robust standard errors in parentheses				
*** p<0.01, ** p<0.05, * p<0.1				

Table B3. Difference-in-Discontinuities Coefficients: Second-Order Polynomials, 20-Year Age Bandwidth

Source: Authors' calculations using FRBNY Consumer Credit Panel/Equifax Data

Notes: Estimates correspond to Equation 3 with the addition of second-order polynomials. Standard errors two-way clustered on age (running variable) and county.

VARIABLES	(1) Pr(Past Due Card)
Base Model	0.00481***
	(0.00186)
Controlling for Having New Account in Past 6 Months	0.00496***
	(0.00186)
Controlling for Limit Increase in Past 6 Months	0.00455**
	(0.00186)
Controlling for Both	0.00466**
	(0.00186)
Robust standard errors in parentheses	
*** p<0.01, ** p<0.05, * p<0.1	

Table B4. Difference-in-Differences Coefficients - Pr(Past Due), Controlling for Recent Increases in Credit

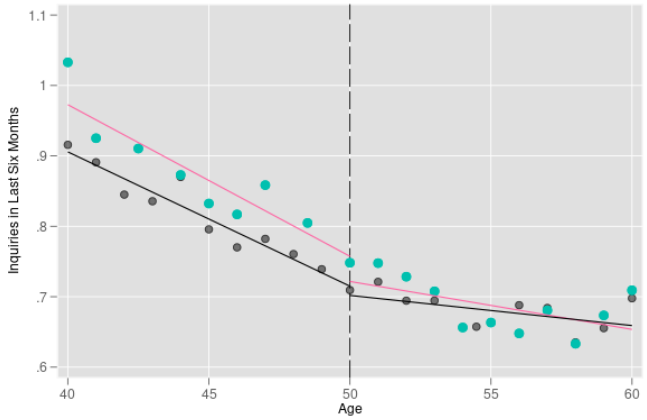
Source: Authors' calculations using FRBNY Consumer Credit Panel/Equifax Data

Notes: Estimates correspond to Equation 1 but include additional controls. Standard errors clustered at the county level.

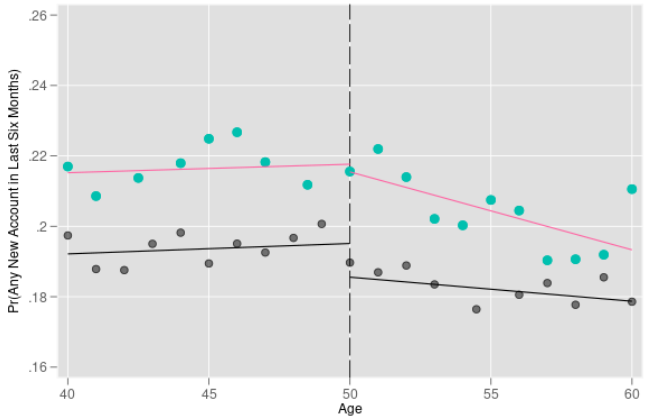
VARIABLES	(1) Equifax Risk Score	(2) New Accounts Per Inquiry
Difference-in-Discontinuity (3 Years)	-9.214***	0.000708
	(1.833)	(0.0136)
Difference-in-Discontinuity (2-Years)	-8.727***	-0.0149
	(1.972)	(0.0132)
Difference-in-Discontinuity (1-Year)	-6.723***	-0.0129
	(1.778)	(0.0156)
Observations (3 Years)	1,644,118	771,388
Dep. Variable Control Mean	632	0.658
Pct Effect at Mean (3 Years)	-1.46%	0.11%

Table B5. Difference-in-Discontinuities Coefficients - Risk Score and Success Rate
Source: Authors' calculations using FRBNY Consumer Credit Panel/Equifax Data.
Notes: Estimates correspond to Equation 3. Standard errors two-way clustered at the age (running variable) and county level.

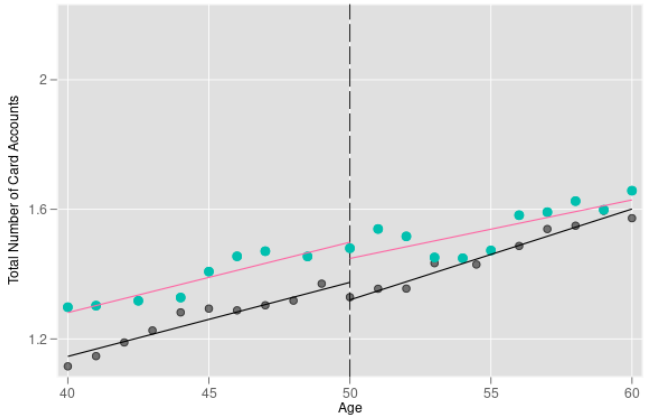
Panel A. Inquiries in the Last Six Months



Panel B. Pr(Any New Account in the Last Six Months)



Panel C. Total Number of Card Accounts



Panel D. Total Credit Limit on All Cards

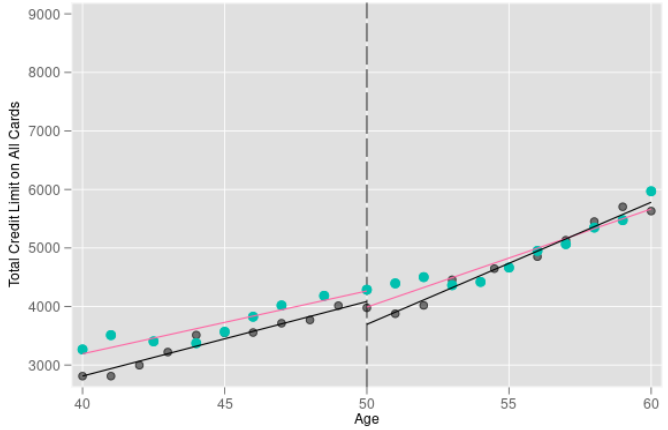
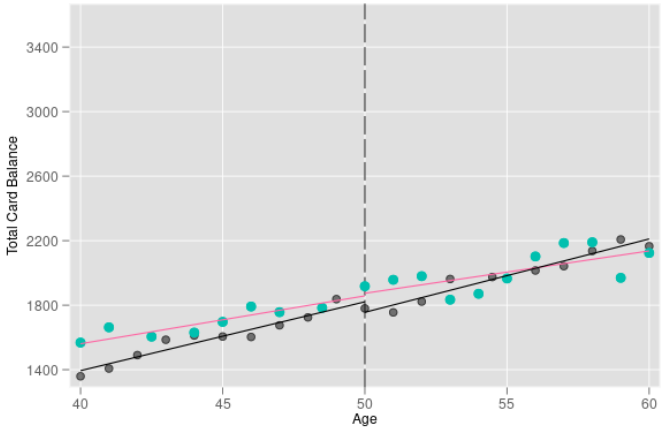


Figure B1. Age 50 Discontinuities for Credit Inquiries, New Accounts, Total Accounts, Total Credit Limit for Never-Treated Consumers with Placebo Treatment

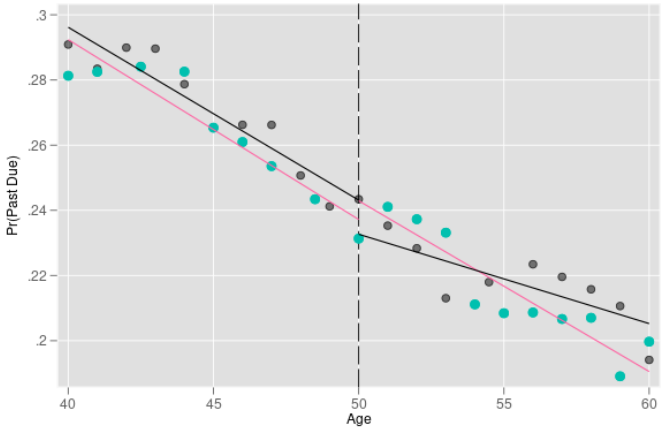
Source: Authors' calculations using FRBNY Consumer Credit Panel/Equifax Data.

Notes: Figures include quarter fixed effects. Placebo treatment quarters are randomly assigned to roughly match the distribution of treatment quarters in the treated sample. Black lines and dots are for the pre-treatment period, while colored lines and dots reflect the post-treatment period.

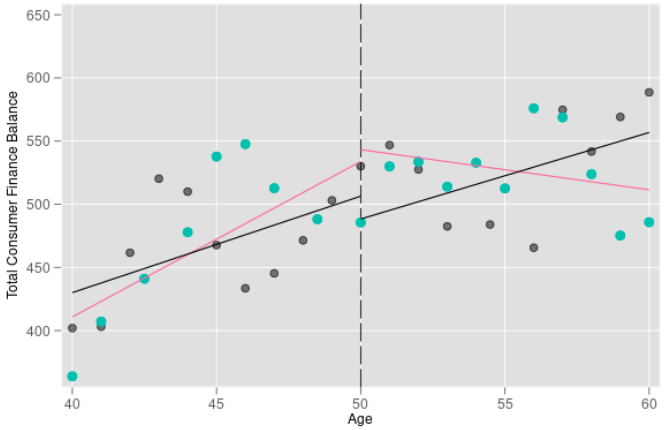
Panel A. Total Card Balance



Panel B. Pr(Any Card Past Due)



Panel C. Consumer Finance Total Balance



Panel D. Pr(Consumer Finance Past Due)

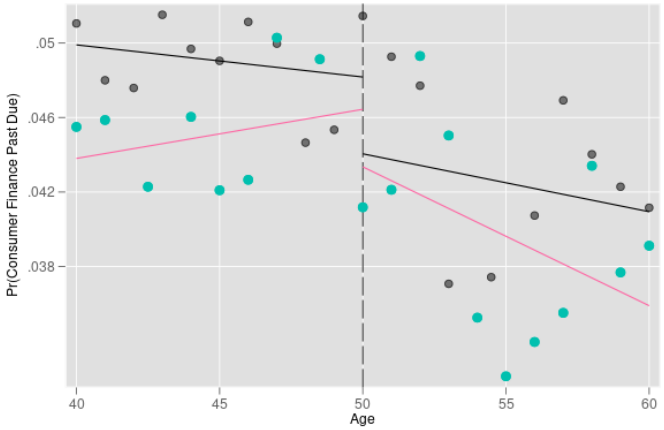
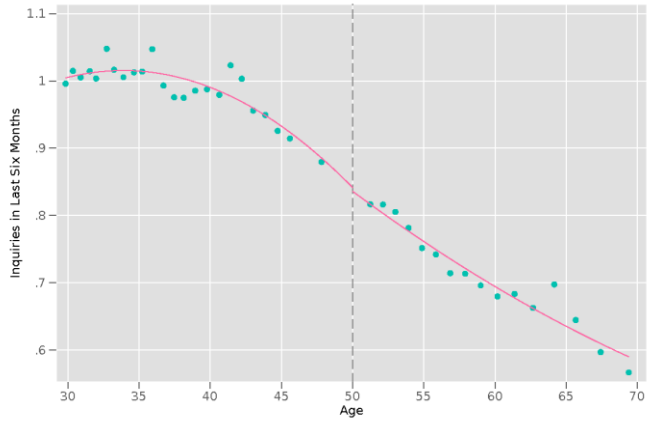


Figure B2. Age 50 Discontinuities for Total Card Balance, Pr(Any Card Past Due), Consumer Finance Balance, and Pr(Consumer Finance Past Due) Never-Treated Consumers with Placebo Treatment

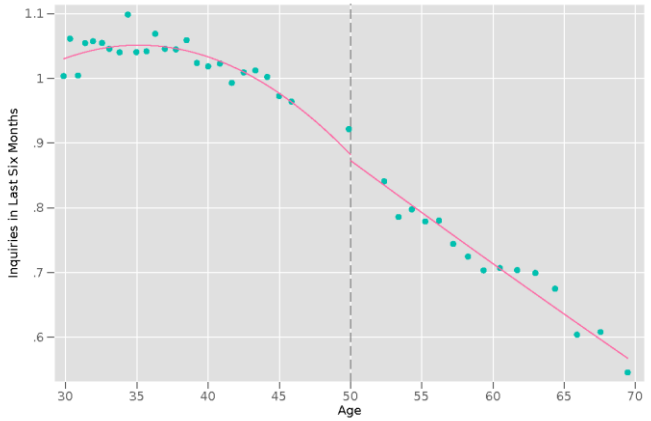
Source: Authors' calculations using FRBNY Consumer Credit Panel/Equifax Data.

Notes: Figures include quarter fixed effects. Placebo treatment quarters are randomly assigned to roughly match the distribution of treatment quarters in the treated sample. Black lines and dots are for the pre-treatment period, while colored lines and dots reflect the post-treatment period.

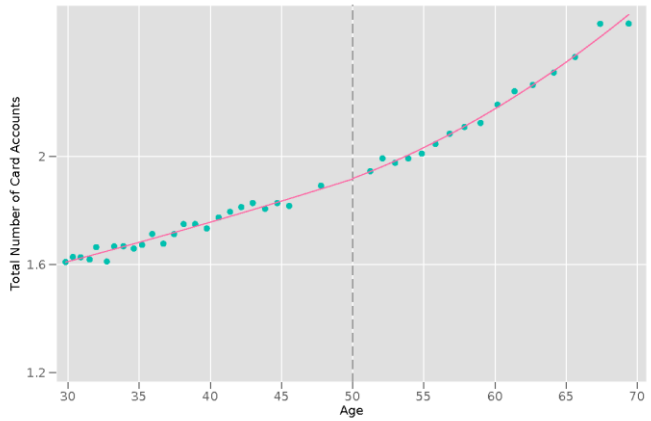
Panel A. Inquiries in the Last Six Months Before Treatment



Panel B. Inquiries in the Last Six Months After Treatment



Panel C. Total Number of Card Accounts Before Treatment



Panel D. Total Number of Card Accounts After Treatment

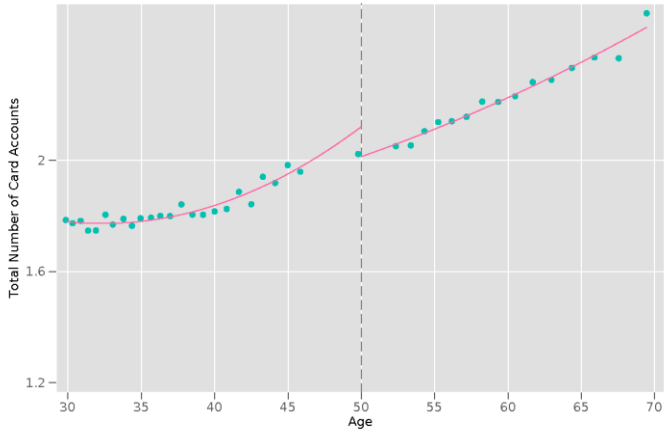
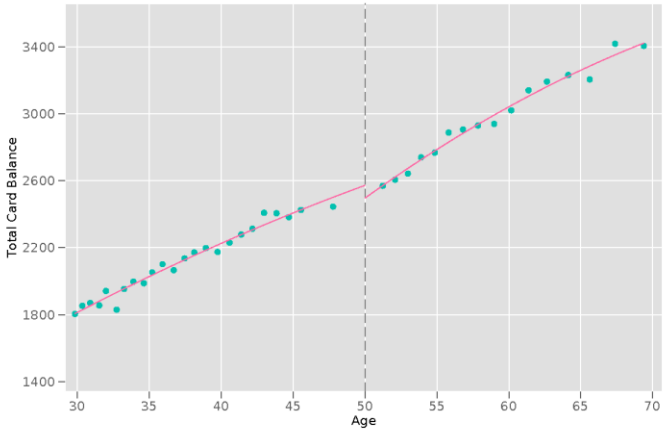
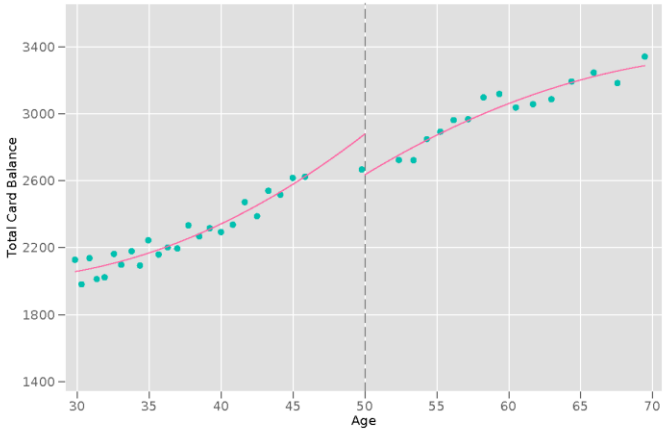


Figure B3. Age 50 Discontinuities for Credit Inquiries, Total Accounts, Quadratic Fits with 20 Year Window
Source: Authors' calculations using FRBNY Consumer Credit Panel/Equifax Data.
Notes: Figures include quarter fixed effects.

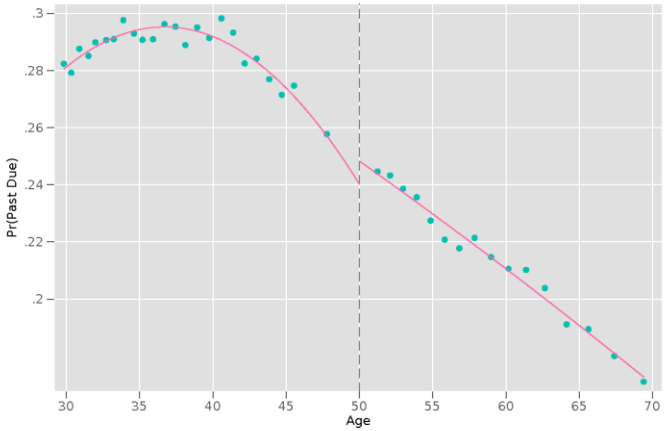
Panel A. Total Card Balance Before Treatment



Panel B. Total Card Balance After Treatment



Panel C. Pr(Card Past Due) Before Treatment



Panel D. Pr(Card Past Due) After Treatment

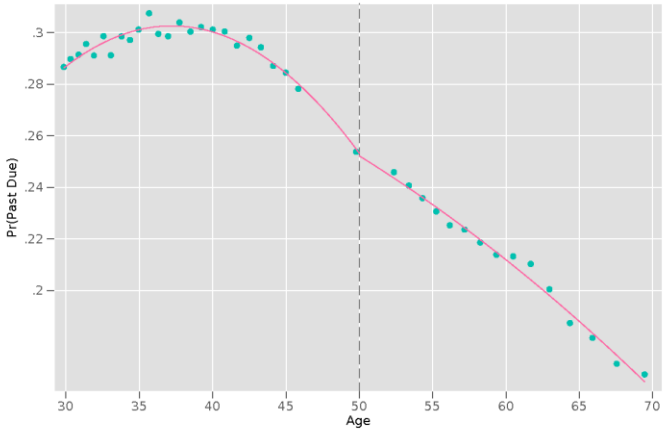
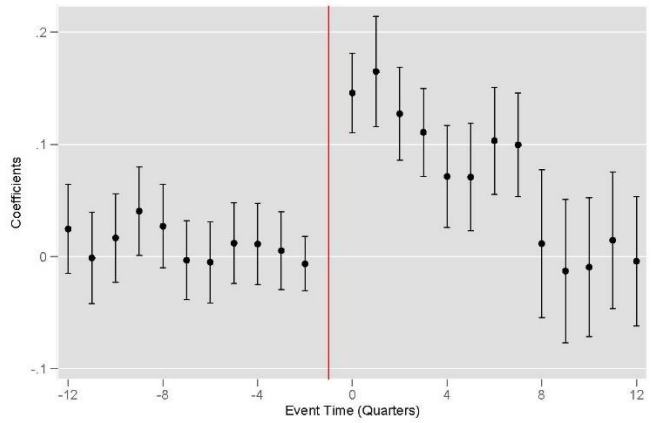
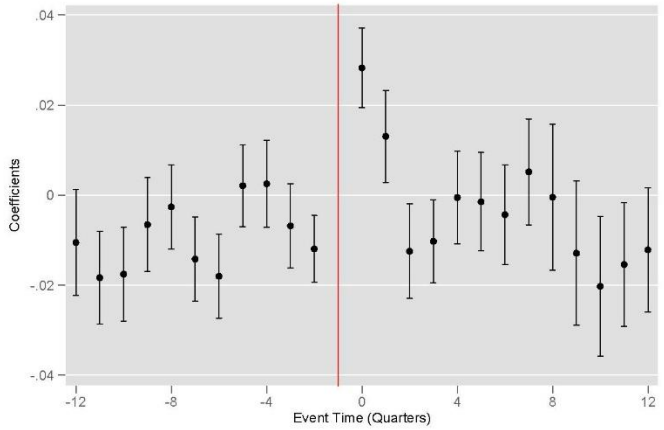


Figure B4. Age 50 Discontinuities for Total Card Balance, Pr(Any Card Past Due), Quadratic Fits with 20 Year Window
Source: Authors' calculations using FRBNY Consumer Credit Panel/Equifax Data.
Notes: Figures include quarter fixed effects.

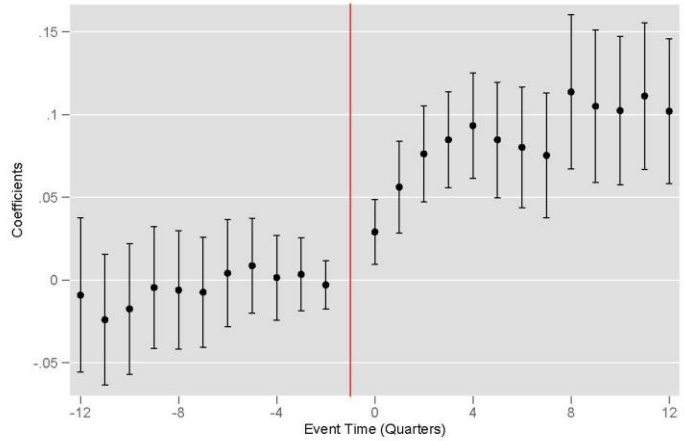
Panel A. Inquiries in the Last Six Months



Panel B. Pr(Any New Account in the Last Six Months)



Panel C. Total Number of Card Accounts



Panel D. Total Credit Limit on All Cards

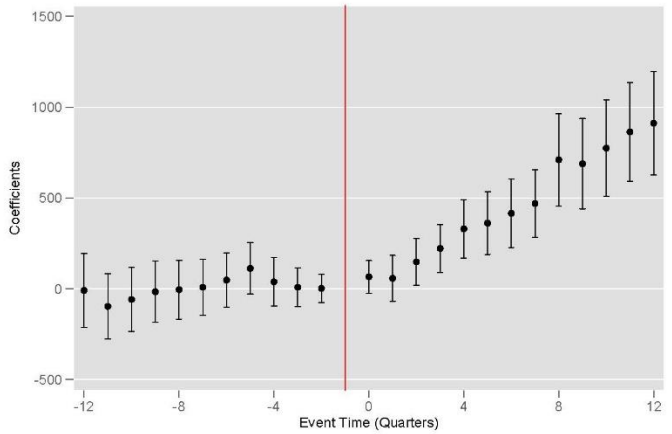
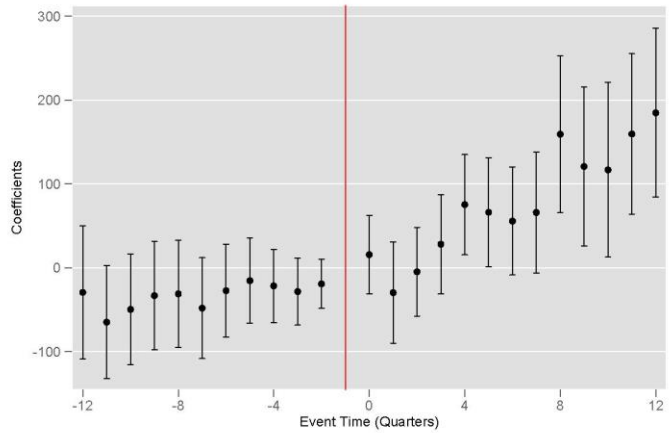
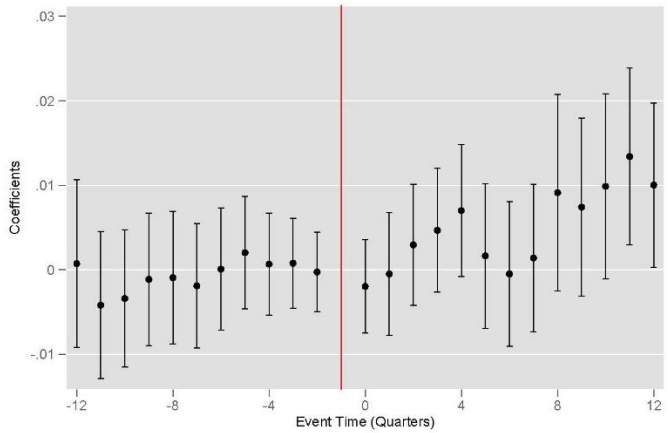


Figure B5. Event Study Results for Credit Inquiries, New Accounts, Total Accounts, Total Credit Limit following Sun and Abraham (2021)
Source: Authors' calculations using FRBNY Consumer Credit Panel/Equifax Data.
Notes: Estimates correspond to Equation 2 with additional adjustments for heterogeneous treatment effect and staggered rollout as in Sun and Abraham (2021). Standard errors clustered at the county level.

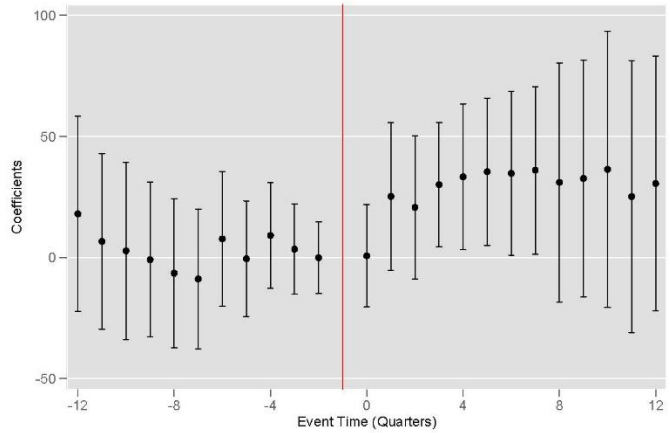
Panel A. Total Card Balance



Panel B. Pr(Any Card Past Due)



Panel C. Consumer Finance Total Balance



Panel D. Pr(Consumer Finance Past Due)

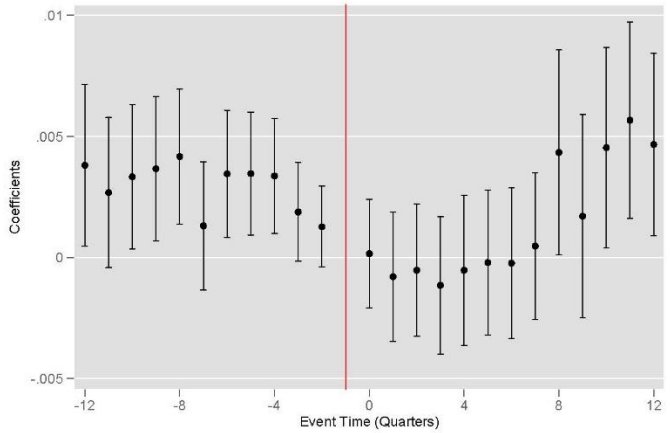
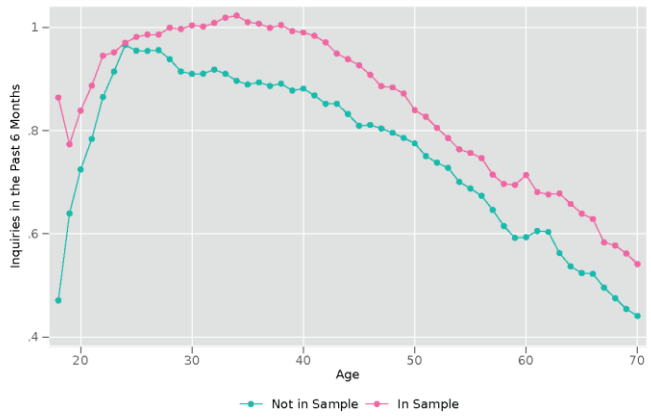


Figure B6. Event Study Results for Total Card Balance, Pr(Card Past Due), Consumer Finance Outcomes following Sun and Abraham (2021)

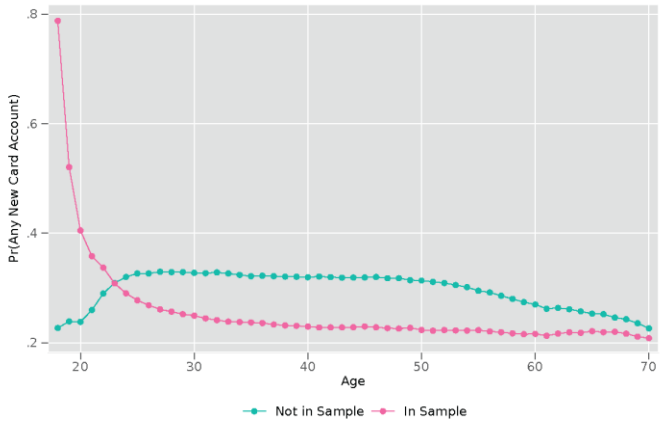
Source: Authors' calculations using FRBNY Consumer Credit Panel/Equifax Data.

Notes: Estimates correspond to Equation 2 with additional adjustments for heterogeneous treatment effect and staggered rollout as in Sun and Abraham (2021). Standard errors clustered at the county level.

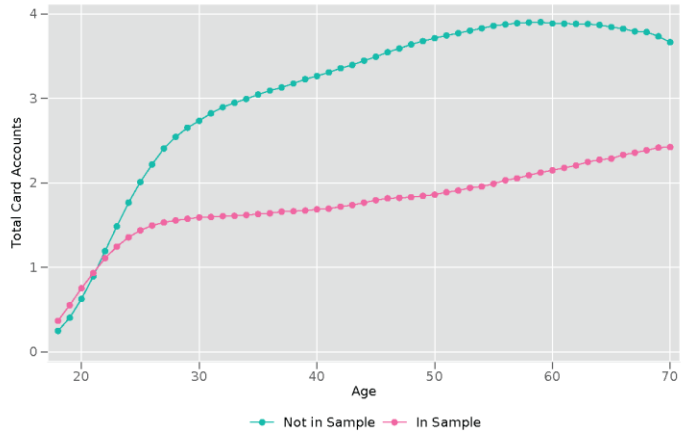
Panel A. Inquiries in the Last Six Months



Panel B. Pr(Any New Account in the Last Six Months)



Panel C. Total Number of Card Accounts



Panel D. Total Credit Limit on All Cards

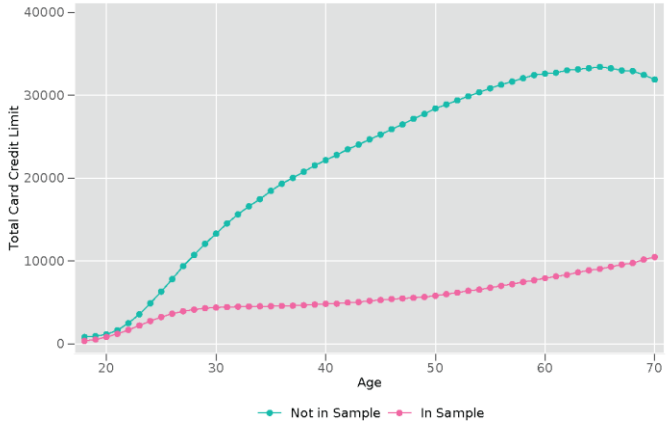
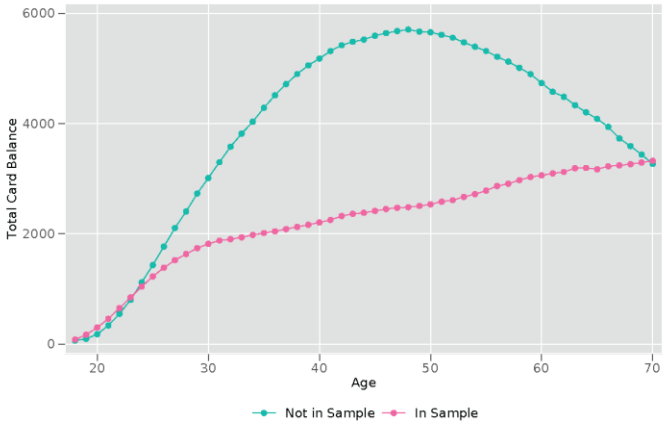
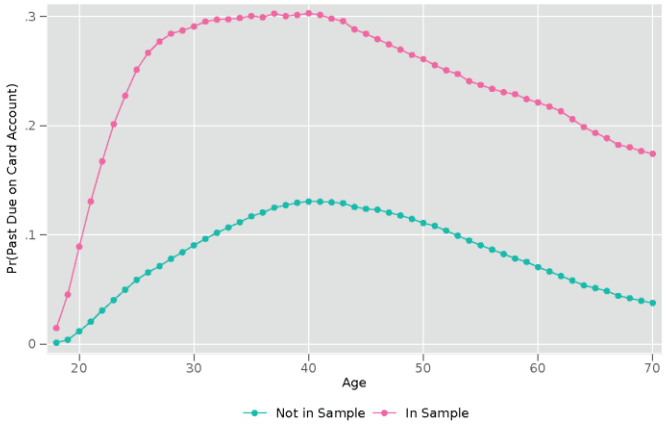


Figure B7. Age Profiles by Sample Status for Credit Inquiries, New Accounts, Total Accounts, Total Credit Limit
Source: Authors' calculations using FRBNY Consumer Credit Panel/Equifax Data.
Notes: Figures correspond to age mean outcomes for those included in our sample of those with a minimum Equifax risk score under 700 during the sample without a mortgage and those not included in that sample.

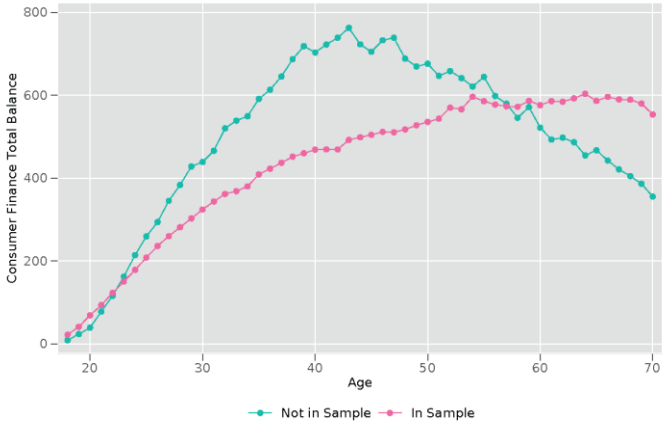
Panel A. Total Card Balance



Panel B. Pr(Any Card Past Due)



Panel C. Consumer Finance Total Balance



Panel D. Pr(Consumer Finance Past Due)

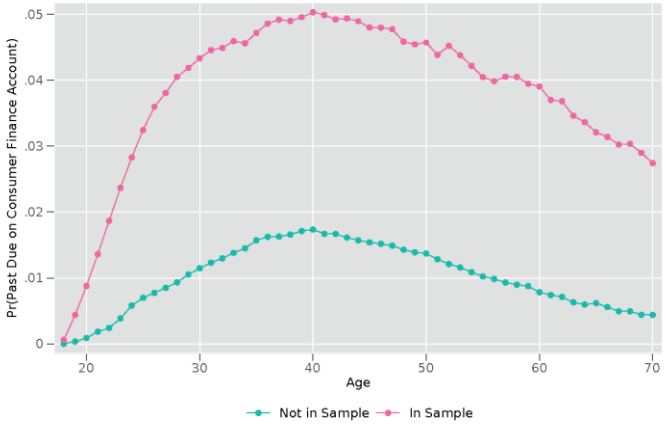
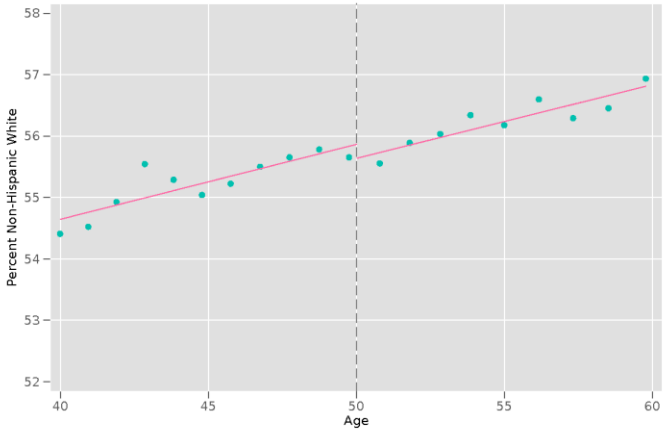


Figure B8. Age Profiles by Sample Status for Total Card Balance, Pr(Card Past Due), Consumer Finance Outcomes

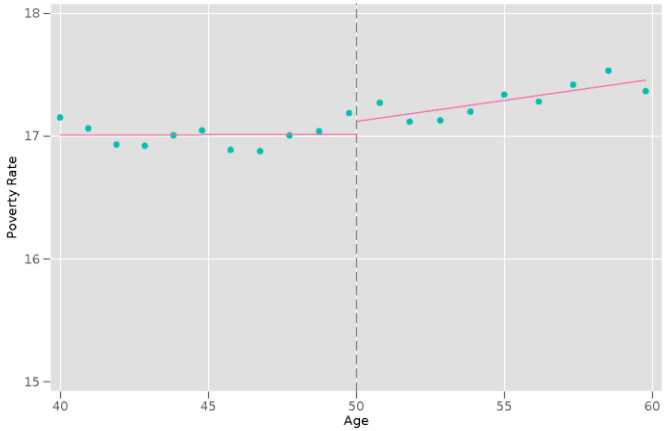
Source: Authors' calculations using FRBNY Consumer Credit Panel/Equifax Data.

Notes: Figures correspond to age mean outcomes for those included in our sample of those with a minimum Equifax risk score under 700 during the sample without a mortgage and those not included in that sample.

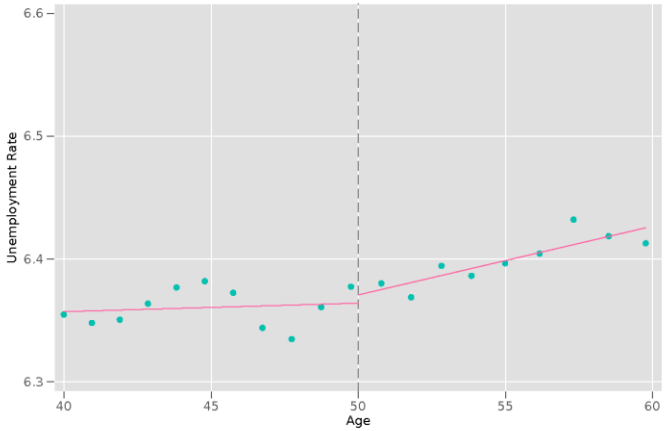
Panel A. Share Non-Hispanic White in Census Tract



Panel B. Poverty rate in Census Tract



Panel C. County Unemployment Rate



Panel D. Census Tract Population

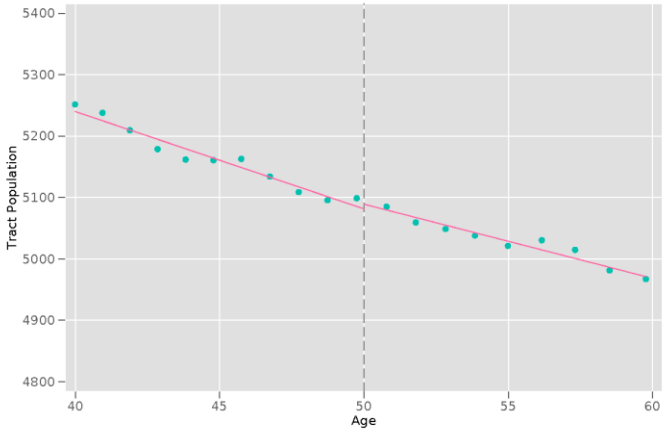
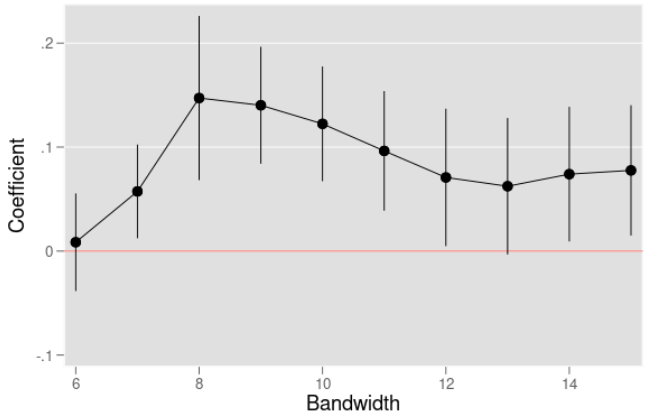


Figure B9. Age 50 Discontinuities for Various Characteristics Prior to the Implementation of Work Requirements

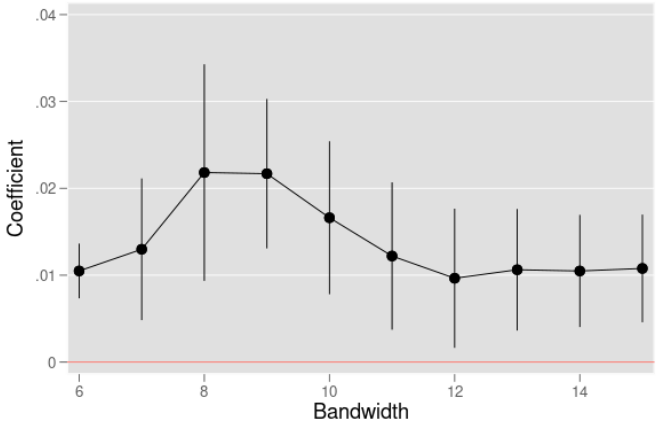
Source: Authors' calculations using Local Area Unemployment Statistics and American Community Survey data.

Notes: Figures include quarter fixed effects.

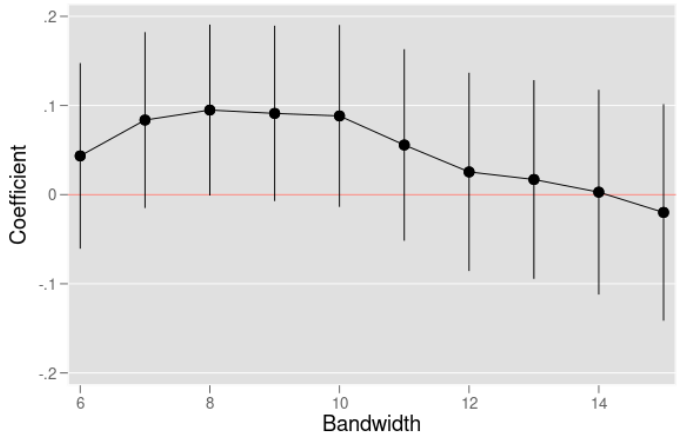
Panel A. Inquiries in the Last Six Months



Panel B. Pr(Any New Account in the Last Six Months)



Panel C. Total Number of Card Accounts



Panel D. Total Credit Limit on All Cards

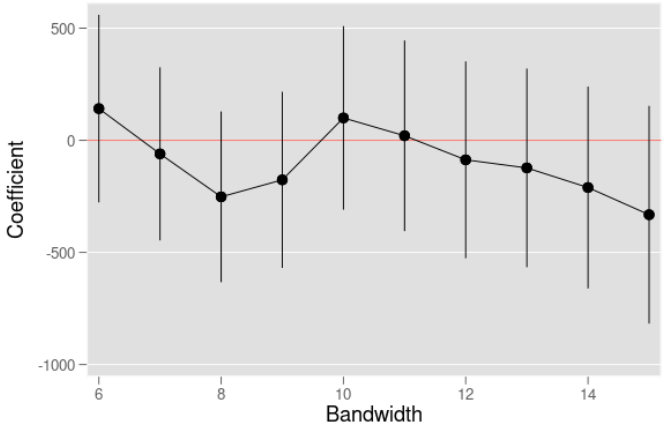
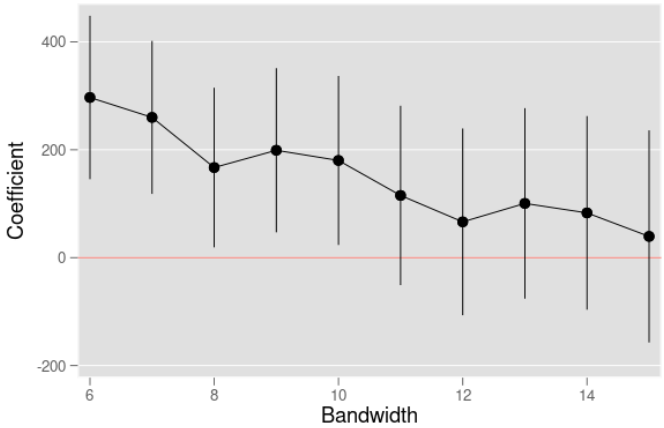
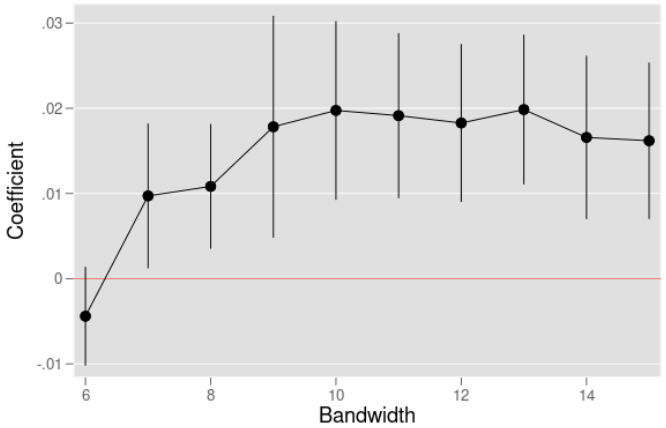


Figure B10. Difference in Discontinuity Estimates by Bandwidth: Credit Inquiries, New Accounts, Total Accounts, Total Credit Limit
Source: Authors' calculations using FRBNY Consumer Credit Panel/Equifax Data.
Notes: Estimates correspond to Equation 3 for bandwidth choices from age 50. Standard errors clustered at the county and age level.

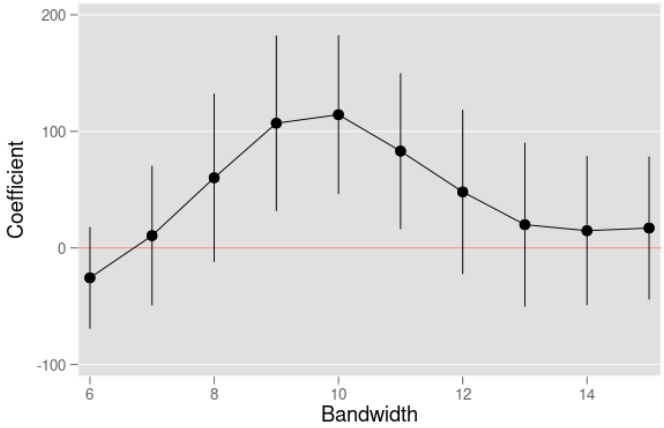
Panel A. Total Card Balance



Panel B. Pr(Any Card Past Due)



Panel C. Consumer Finance Total Balance



Panel D. Pr(Consumer Finance Past Due)

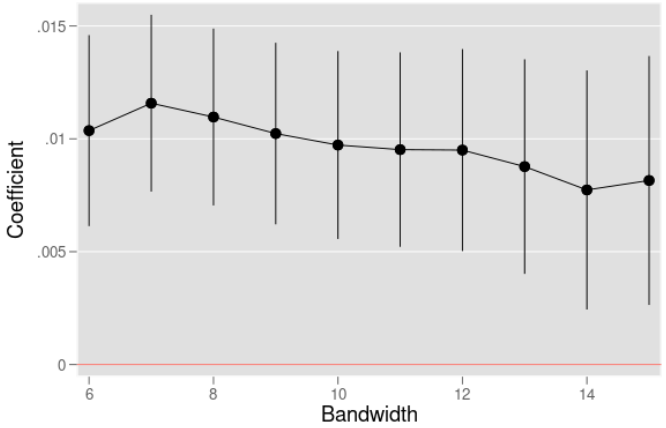


Figure B11. Difference in Discontinuity Estimates by Bandwidth: Total Card Balance, Pr(Card Past Due), Consumer Finance Outcomes

Source: Authors' calculations using FRBNY Consumer Credit Panel/Equifax Data.

Notes: Estimates correspond to Equation 3 for bandwidth choices from age 50. Standard errors clustered at the county and age level.