

# Health Insurance and High Cost Borrowing: The Effect of Medicaid on Pawn Loans, Payday Loans, and other Non-Bank Financial Products

Anne Fitzpatrick

University of Massachusetts Boston  
100 Morrissey Blvd.  
Boston, MA 02125.  
[Anne.Fitzpatrick@umb.edu](mailto:Anne.Fitzpatrick@umb.edu)

Katie Fitzpatrick

University of Delaware  
188A Graham Hall  
Newark, DE 19716  
[kfitzpat@udel.edu](mailto:kfitzpat@udel.edu)

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## **Abstract:**

Fringe banking products such as pawn loans and payday loans provide a crucial access to credit to vulnerable households, although empirical literature is mixed on whether household well-being is improved by using these “predatory lenders”. We test whether health insurance reduces the demand for these controversial services by increasing the ability of households to cope with medical expenditure risk. Specifically, we use plausibly exogenous variation in state choices to expand Medicaid to determine whether newly eligible households change their use of pawn loans, payday loans, check cashing outlets, and other non-bank financial products. We use a synthetic control methodology to account for differential trends of potentially confounding variables correlated with the Medicaid expansion. We find that Medicaid eligibility decreases utilization of any fringe banking credit services by 1.5 percentage points (15 percent), driven by reductions in pawn and payday loans. Low-income, low-education, and minority households decrease fringe banking credit use by between 1-4 percentage points, with similar large reductions in payday and pawn loans. This is driven by a decrease in the utilization of fringe credit providers, specifically pawn loans. We show that these fringe banking reductions are accompanied by large reductions in overall and out-of-pocket expenditures, suggesting that initiatives to reduce demand may be

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## Introduction

In 2017, more than one in five households – 28.6 million in total – reported using a fringe banking product (FDIC 2018). Fringe banks, non-bank financial services like payday lenders, pawn shops, and check cashers, play an important role in the financial lives of lower income households. These financial services are a large but often overlooked market with annual transactions totaling more than \$320 billion and annual revenues exceeding \$25 billion (Bradley et al. 2009; Rivlin 2010). Moreover, they are an important source of financial services in lower income communities. For example, the number of businesses offering payday loans and check cashing surpasses the number of McDonald’s and Starbucks combined while the number of pawn shops alone exceeds the number of credit unions and banks (Karger 2005).

Whether public policy should prohibit or otherwise restrict access to fringe-bank services is controversial. Opponents consider these services “predatory lenders” that impose excessive fees and usurious interest rates as high as 600 percent, potentially trapping vulnerable households in burdensome debt (Dobbie and Skiba 2013). Others argue that these lenders fill an unmet need for short-term credit, resolving cash shortages at a critical time (Mullainathan and Shafir, 2009). While empirical research is mixed on whether these lenders improve household well-being, the literature generally finds that these providers are used as lenders of last resort for households (see Barr 2004, Bhutta et al. 2015; Hogarth et al. 2004; Mullainathan and Shafir 2009). Policy initiatives to reduce reliance on fringe banking products has typically focused on restricting the supply of these products.<sup>1</sup> However, demand-side interventions aimed at reducing dependence on fringe banking services may improve household well-being while still allowing sufficient liquidity.

Improving the ability of lower income households to cope with financial shocks, especially unexpected medical expenditures, is one potential way to reduce reliance on fringe banks. Prior to the Affordable Care Act (ACA), approximately 41 percent of working-age individuals struggled with medical bills, medical debt, or both (Doty et al. 2008). The burden of medical debt was concentrated on lower income, uninsured households (Doty et al. 2008). The demographic characteristics of uninsured households overlap with the characteristics of households that use fringe banks services: lower education, lower income, minority, and unbanked households (Apaam et al. 2018; Barr 2004; Blank 2008; Bohn and Pearlman 2013; Goodstein and Rhine 2017). Similarly, the

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<sup>1</sup> Supply-side limits on fringe banks tend to occur at the state level rather than the federal level, with the exception of the 2007 federal law that made it illegal to charge members of the military more than a 36% APR interest rate and the limited authority of the Consumer Financial Protection Bureau (CFPB) in regulating fringe banks. States vary in their supply-side limits. According to Prosperity Now, in 2019, 17 states prohibit payday lending with either outright bans or a 36% APR cap on interest, 30 states prohibit auto-title loans with either outright bans or a cap of 36% APR, and 6 states ban high-cost installment loans with either outright bans or APR caps. Consumer advocates have also pursued state ballot initiatives to limit the industry in a number of states, successfully in Arizona (2008), Colorado (2018), Montana (2010), Ohio (2008), South Dakota (2016), and unsuccessfully in Missouri (2012) and Texas (2005).

ACA's Medicaid expansion resulted in approximately 9.6 million new enrollees (Carman et al., 2015), reducing the exposure of lower income households to medical risk. However, not all states chose to implement this expansion, resulting in similar lower income households that differ in their protection from medical expenditure risk based on their state of residence. We use the Medicaid expansion as a policy experiment to test whether health insurance coverage reduces demand for these controversial providers and, potentially, improves economic security.<sup>2</sup>

Our empirical approach employs four years of the FDIC-sponsored Unbanked and Underbanked Supplement to the Current Population Survey (FDIC-CPS) linked across time to data on health insurance status from the March CPS. This nationally representative data with both fringe bank use and health insurance coverage, spans the period before and after the Affordable Care Act's 2014 Medicaid expansion. To reduce bias from differential pre-period trends across expansion and non-expansion states, our methodology compares households in expansion states to those in non-expansion states with the synthetic control methodology of Abadie et al. (2010) extended to multiple treatment units by Cavallo et al. (2013).

We find households in Medicaid expansion states are 0.8 percentage points less likely to report any fringe bank use in the past 12 months, driven entirely by a decrease in fringe credit use (as opposed to fringe bank transaction product use). The Medicaid expansion is associated with a 1 percentage point decrease in pawn loans and 1.6 percentage point decrease in payday loans. Effects are nearly 3-4 times larger among households with demographic characteristics most likely to use fringe banking service: households that are either lower income (under 300% of the federal poverty level), without a college degree, or non-white households. We find that these financial outcomes are likely related to reduced medical debt. Lower income and non-college households in Medicaid expansion states report, on average, \$155-\$919 lower out-of-pocket expenditures. Lower income households report \$155 lower out-of-pocket. There are also large decreases in overall medical expenditures. Our results suggest that Medicaid expansion improved financial security by allowing for reductions in medical debt and corresponding medical expenses.

Our study builds upon a growing literature that suggests that households financially benefit from public health insurance expansions by improving their credit worthiness through reductions in medical debt. For example, recent work concludes that Medicaid lessens financial hardship by reducing out-of-pocket medical spending (Golberstein et al., 2015), unpaid medical bills sent to collections (Caswell and Waidmann 2017; Finkelstein

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<sup>2</sup> Theoretically, the effect of Medicaid on use of fringe bank credit could be ambiguous if Medicaid coverage induces moral hazard with associated increases in out-of-pocket health care expenditures. While states do have the option to charge premiums and impose cost sharing on Medicaid beneficiaries, federal regulations limit these premiums and fees. According to the Kaiser Family Foundation, as of January 2017, 30 states charge premiums or enrollment fees for Medicaid coverage, including those to lower income adults in 23 of the 32 states that implemented the Medicaid expansion (Artiga et al. 2017).

et al., 2012; Miller et al. 2018), credit scores (Caswell and Waidmann 2017; Miller et al. 2018) and bankruptcy filings (Caswell and Waidmann 2017; Gross and Notowidigdo, 2011; Miller et al. 2018). New research linking credit report records to Medicaid expansions similarly finds that Medicaid improves the credit worthiness of newly insured households (Allen and Gross 2016; Brevoort et al. 2017; Caswell and Waidmann 2017; Hu et al., 2016; Miller et al. 2018). Brevoort et al. (2017) find that improvements in credit worthiness stem from reductions in the incidence of unpaid medical debt, especially among households in the subprime market. These findings extend to insurance expansions other than Medicaid with Mazmuder and Miller (2016) concluding that the Massachusetts health reform reduced debt collections, improved credit scores, and reduced bankruptcy filings.

While important to understanding the role health insurance plays in long-term financial well-being, because it focuses on mainstream credit sources or relies on credit report records, this work omits both the fringe banking products used by low-income households, the population most affected by the ACA's Medicaid expansions (Barr 2004, Bhutta et al. 2014; Hogarth et al. 2004; Mullainathan and Shafir 2009). For example, in 2017, one in five households had no mainstream credit in the past year, with this proportion inversely related to income; more than half of households with incomes below \$15,000 had no mainstream credit in the past year (Aapam 2018). Nearly one-third of households – mostly lower income households – lack a credit card and are unlikely to be observed in credit report data (Burhouse et al. 2016).

This work also omits the short-term financial coping strategies of lower income households. Fringe banks do not report to consumer credit bureaus so their use will not be captured by credit report data. Use of fringe bank products is the option of last resort and their use may be strategic by, for example, using fringe banking products to avoid late bill payment that would appear in the credit report data (White 2017). Only Allen et al. (2017) who finds that the early Medicaid expansion in California reduced household reliance on payday lenders examines the relationship between health insurance and fringe bank credit.

We make four primary contributions to the literature. First, we provide new results using a plausible identification strategy on demand side interventions for fringe banking products, an important yet understudied market. Second, we link that literature to a growing literature on the impact of health insurance on financial outcomes, and specifically credit use and creditworthiness of lower income households. Third, we contribute to the evaluation of the ACA, ongoing state-level debates on the Medicaid expansion, and the larger debate over the role of public health insurance coverage in the US. Finally, our work highlights the interplay between health insurance and consumer finance for lower income households.

This paper proceeds as follows. Section 2 provides background on fringe banking products and the ACA's Medicaid expansion. Section 3 provides our conceptual framework. Section 4 presents the data and methods. Section 5 presents the results. Section 6 includes the discussion and conclusion.

## 2. Background

### *Fringe Banking: Pawn Shops, Payday Loans, and Other Non-bank Financial Products*

Products offered by the fringe banking industry include transaction products ranging from non-bank remittances, money orders, and check cashing services to credit products such as pawn loans, payday loans, refund anticipation loans, auto-title loans, and rent-to-own contracts. The common link of all fringe banking products is that 1) these products are financial services but they are not being offered by a bank; 2) these services tend to come with high costs, either through fees and/or interest. Fringe bank transaction products provide basic transaction services by converting checks into cash or income into payments for a fee. For example, check cashing services convert a check into cash for a fee ranging from 1 to 4 percent of the face value of the check (Bradley et al. 2009). Fringe bank credit products tend to be high-interest and high-fee, providing households that provide short-term access to credit and liquidity. For example, payday loans are short-term loans of \$100 to \$500. Loan fees average \$15 to \$20 per \$100 of principal, implying an annual percentage rate (APR) of over 400 percent. With high interest rates, payday loans have the potential to create debt while pawn loans and auto-title loans can result in the loss of an asset. As such, these products generate controversy.

Use of fringe bank products is related low or moderate income, unbanked status, education, and age; non-whites, single adults, households with children, and Hispanics are more likely to use these products (Barr 2004; Bohn and Pearlman, 2013; Caskey 1997; Caskey 2002; McKernan et al. 2003; Zinman 2010). State regulation and local characteristics of areas, such as location of banks, may influence the mix and type of fringe bank providers (Fowler et al. 2014; Graves 2003; Prager 2009).

Regulation of these products varies widely with some states choosing to tightly regulate or outright ban these products and others enacting little to no regulation.<sup>3</sup> Despite the creation of the Consumer Financial Protection Bureau (CFPB) and federal regulation of the banking industry, relatively little federal regulation exists.<sup>4</sup> Due to their implied annual percentage interest rates (APRs), fringe banking credit products would generally be prohibited by state usury limits (Barr 2004). Some states make special exemptions for some or all of these products; others place limits on interest rates or fees and a handful of states outright ban some or all of these products. Still, consumers can travel across state lines or use online providers to access these products so while state regulation reduces fringe

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<sup>3</sup> Changes in state regulation of these products largely occurred prior to 2009, our first year of data. The main area of federal regulation involves products sold to members of the armed forces.

<sup>4</sup> The main area of federal regulation involves transactions with members of the armed forces. The Talent-Nelson amendment to the John Warner National Defense Authorization Act for Fiscal Year 2007, Pub. L.No. 109-364, § 670(a), 120 Stat. 2083, 2266 (codified at 10 V.S.c.A. § 987(b)) states "A creditor ... may not impose an annual percentage rate of interest greater than 36 percent with respect to the consumer credit extended to a covered service member or a dependent of a covered service member." The greater likelihood that military members used payday loans and other high-cost credit prior to the ban was one reason for the ban (Knize 2009).

banking use, it is not binding. A new literature empirically examining the relationship between fringe banking use and economic well-being focuses on access to or use of payday loans. It finds mixed effects of payday loans on economic well-being (Campbell et al., 2008; Fitzpatrick 2016; Karlan and Zinman, 2010; Melzer, 2011; Melzer and Morgan, 2009; Morgan and Strain, 2008; Morse, 2009; Skiba and Tobacman, 2007; Zinman 2010).

### *The Affordable Care Act (ACA) and the Medicaid Expansion*

Prior to the ACA, lack of adequate health insurance was an important problem. For example, in 2009, nearly 50 million individuals lacked insurance and 31 million non-elderly adults were underinsured, meaning that their health insurance coverage did not adequately protect them from financial risk (Collins et al. 2015; DeNavas-Walt et al. 2011). Many of these individuals were not eligible for Medicaid. Federal requirements set minimum eligibility criteria based on income and age; no federal eligibility criteria existed for childless adults. States had the option of expanding these minimums which created considerable variation in Medicaid eligibility across states as many adopted more generous income eligibility limits, especially for children. This resulted in many lower income children being eligible for Medicaid, but few states provided coverage to adults, especially childless adults.

Signed into law on March 23, 2010, the ACA was the largest health insurance reform since the creation of Medicare and Medicaid in 1965. The ACA sought to achieve universal health insurance coverage through individual and employer mandates, subsidies to purchase health insurance, new regulations on insurance markets, state-based insurance exchanges, and Medicaid expansions. The ACA also set minimum requirements for health insurance and made other changes to industry practices. By law, these provisions were implemented over time, but the individual mandate, creation of state-based insurance markets, and Medicaid expansion began in 2014. As of 2017, the ACA increased health insurance to approximately 23 million people, including 9.5 million people through the Medicaid expansion (Carman et al, 2015). By 2017, Medicaid covered 22% of all non-elderly adults – many who may be unable to afford other health insurance – and paid for nearly one-sixth of national spending on health care.

The ACA's Medicaid expansion targeted those individuals likely unable to afford to purchase insurance in the individual market by requiring states to make all households with incomes up to 138% of the federal poverty line eligible for Medicaid. However, some states balked at the new eligibility requirements. In 2012, the Supreme Court decided in *National Federation of Independent Business v. Sebelius* to allow states to opt-out of the expansion. Originally 26 states elected not to expand their Medicaid programs; an additional 11 states have adopted the expansion to date.<sup>5</sup>

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<sup>5</sup> These states include: Alaska, Arizona, Arkansas, California, Colorado, Connecticut, Delaware, District of Columbia, Hawaii, Idaho, Illinois, Indiana, Iowa, Kentucky, Louisiana, Maine, Maryland, Massachusetts, Michigan, Minnesota, Montana, Nebraska, Nevada, New Hampshire, New Jersey, New Mexico, New

A growing body of work uses the state-level Medicaid expansion decision to estimate the impact of the ACA on health insurance coverage. Courtemanche et al. (2017) estimate that Medicaid expansion states experienced an average 5.9 percentage point increase in the insurance rate while states that did not expand Medicaid experienced a 2.8 percentage point increase. These estimates are in line with other studies of the effect of the ACA on health insurance rates (Long et al., 2014; Smith and Medalia, 2015; Courtemanche et al., 2016). The largest gains in insurance coverage occurred for those without a college degree, the young, single, and childless (Courtemanche et al. 2017).

### **3. Conceptual Framework**

Low-income individuals frequently lack bank accounts, limiting savings and the ability to cope with unexpected expenses. Health insurance may then either increase or decrease fringe banking use through two competing mechanisms. First, insured individuals may increase fringe banking use by increased healthcare utilization. Depending upon health insurance coverage, these medical expenses may or may not be covered and may therefore result in increased out-of-pocket expenses. In contrast, the counterfactual of a low-income uninsured individual may be less healthcare use, either due to lower healthcare access or anticipation of an inability to pay.

Second, however, health insurance with generous coverage—like Medicaid-- may decrease fringe banking services. Medicaid typically provides generous coverage with limited to no cost sharing.<sup>6</sup> As a result, individuals experiencing a negative health shock may be better equipped to financially cope due to their health care coverage, becoming less reliant on fringe banking services.

Regardless of the sign, we would expect that effects of health insurance on fringe-banking services are concentrated on non-bank credit products (i.e., payday loans; pawn loans; refund anticipation loans), as opposed to non-bank transaction products (i.e., remittances; money orders; etc.). This is because fringe bank credit provides needed liquidity in periods when expenditures exceed income. In contrast, fringe transaction services, while high fee, typically reflect a need to turn checks into cash or income into payments.<sup>7</sup>

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York, North Dakota, Ohio, Oregon, Pennsylvania, Rhode Island, Utah, Vermont, Virginia, Washington, and West Virginia. Idaho, Nebraska, and Utah adopted but not implemented the expansion.

<sup>6</sup> Artiga et al. (2017) find that 32 of the states that adopted the Medicaid expansion charge cost sharing, either through premiums or copayments, for expansion adults. Cost sharing is a barrier to utilization and maintaining Medicaid coverage among lower income households.

<sup>7</sup> Check cashing services can be used for liquidity for individuals because they clear checks immediately. In contrast, banks may require a customer to wait at least several days to clear a check, particularly if the customer has a history of overdrafts.

## 4. Data and Methods

### *Data*

We use five waves of the CPS (2009, 2011, 2013, 2015, and 2017) of the FDIC-CPS merged with the CPS's March Annual Social and Economic Supplement.<sup>8</sup> The FDIC-CPS queries respondents on the use of bank accounts and non-bank financial providers; the data provide a rare and detailed look at the types of financial services utilized by low-income households, as well as the frequency and reasons for use. The March CPS provides detailed information on the insurance status of each individual in the household, as well as detailed household income.

To create a dataset containing both fringe banking use and health insurance status, we link each year of the FDIC-CPS data to the March CPS data from the same year to analyze detailed information on the respondent's health insurance coverage and income.<sup>9</sup> Because the FDIC questions referred to the household-level, we use the characteristics of the most employed adult within the household and aggregate health insurance measures to the household level.

We exclude households with all members age 65 or older from our sample, because these households are likely insured through Medicare and thus unaffected by the ACA's insurance expansions. In order to examine the mechanisms and plausibility of our results, we also consider the effects of Medicaid expansion on populations likely affected by the health insurance expansions: households with reported incomes at or below 300 percent of the federal poverty guidelines, single adults without children, households headed by non-white adults, and households where the most employed adult has less than a 4-year college degree.

We then collapse the data by year and use state-level averages to estimate the effect of Medicaid expansion on financial outcomes using the synthetic control methodology described below. To account for the economic environment, we also merge on information on the state's unemployment rate.

### *Measures*

The FDIC-CPS does not contain a consistent series of questions related to fringe banking use over the different years.<sup>10</sup> We measure use of any fringe banking product by

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<sup>8</sup> The CPS consists of a rotating panel ("rotation group"), where a specific housing unit is in the survey for four months, spends eight months out of the sample, before returning for four months. In contrast to traditional longitudinal panels, the CPS randomly samples housing *units*, not *individuals*, the individuals and households completing the survey may change across time for the same identification number. As a result, a procedure was developed by Madrian and Lefgren (2000) in order to link individuals over time by using household identifiers and time-invariant characteristics (such as age, race, and sex) to determine if the individual is a likely match.

<sup>9</sup> The 2009 FDIC-CPS was in January while FDIC-CPS for all other years occurred in June. A total of 58,376 household observations were successfully merged across the FDIC-CPS and the March CPS.

<sup>10</sup> See Data Appendix. Over the four survey waves, the FDIC-CPS has modified their questions. In particular, in 2009 the survey asked about whether the household had ever used a non-bank check casher, non-bank money order, pawn lender, or rent-to-own agreement, and then asked about the frequency of use. ("at least a



determining if anyone in the household reports using any of the following products over the previous 12 months: non-bank money order, check casher, rent-to-own contract (RTO), pawn loan, or payday lender. Because our hypothesis is that households may require less liquidity due to less medical bills, we construct a measure of fringe banking credit use over the previous 12 months containing the products available in each survey: pawn loan, payday loans, tax refund anticipation loan (RAL), and rent-to-own contract. We also use a second AFS credit measure that contains the set of products available from 2011 through 2015: pawn loan, payday loan, RTO, and auto title loan. We then construct a measure of fringe banking transaction products that measures if anyone in the household used a non-bank money order or a check casher in the last 12 months. Finally, we measure if a household does not own a bank account because relaxing of budget constraints made possible by insurance coverage and improvements in credit scores can facilitate bank account ownership

In keeping with the household level measure of financial product use, we construct measures of health insurance status at the household level. Our measures include whether anyone in the household is enrolled in Medicaid; whether anyone in the household is enrolled in private health insurance; whether the entire household is insured/uninsured.<sup>11</sup>

### *Summary Statistics*

Summary statistics from the dataset are in Table 1; averages are for all pre-expansion years and disaggregated by Medicaid expansion status. Panel A presents demographic variables. Approximately 80 percent of households in expansion states are white; 10 percent are black; and 12 percent are Hispanic, with the remainder being of other races. The average age of the most employed adult in expansion states is 46 years old. The average household size is 1.37 people. The mean household income is \$57,792, although there is a wide variance in annual income.

Panel B demonstrates that use of a particular fringe credit provider within the past 12 months is relatively uncommon among the population as a whole. In expansion states, use of any individual fringe credit product varies from 1.7 percent using rent-to-own

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few times a year; once or twice a year; almost never”). In contrast, in all subsequent surveys the survey first asked about ever use, then followed up with whether the household had frequented that type of alternative financial service provider in the past 12 months. For the 2009 responses, we treat households that report using the AFS product “once or twice a year” or “at least a few times a year” as having used the product in the past 12 months. The two exceptions to this is for tax refund anticipation loans (RALs) where the 2009 survey asked if anyone in the household had taken one out in the past 5 years while 2011, 2013, and 2015 surveys asked about the past 12 months; for payday loans in 2009, the survey first asked if anyone ever used a payday loan and then queried the number of times in past 12 months anyone in the household used a payday loan. We treat households that used a RAL in the past 5 years as having used one in the past 12 months and we determine payday loan use in the previous year by the number of payday loans used in the past 12 months. FDIC-CPS did not include a question about remittances in the 2009 survey; it did not include a question about auto-title loans in the 2009 or 2011 surveys, so those types of providers are not in this analysis.

<sup>11</sup> Because financial behavior is measured at the household rather than the individual level, the subsample of single adults without children is useful because it allows us to determine who in the household used the financial product.

providers an auto title loan to just fewer than 3 percent for refund anticipation loans, pawn shops, or payday lenders.<sup>12</sup> To account for concerns of small sample bias, we combine these different types of providers into aggregate measures of fringe banking use that we use as our primary outcome variable. For example, Medicaid expansion states report an average fringe banking credit usage rate in the past 12 months of 6.7 percent. By combining these different services into one measure of access to alternative sources of credit, we gain statistical power.

One unique aspect of these merged data is not only financial behavior, but also detailed information on health insurance status and plan characteristics. Overall, expansion states report that on average 75 percent of households have all members insured, while 7 percent have all members uninsured. The remaining households are of mixed insurance status. Overall, union or employer-provided insurance (i.e., group health insurance) is the most common type of insurance- approximately 72 percent have at least one household member enrolled in this type of health insurance. Other types of insurance are far less common. Twenty five percent of households have at least one member on Medicaid and 14.7 percent of households have at least one member on Medicare. Only 9.1 percent of households contain at least one household member with insurance through the individual market. We now turn to examining whether the increased health insurance rates caused by the Medicaid expansion changed household reliance on fringe banking providers.

### *Empirical Strategy*

We utilize plausibly exogenous variation arising from the Medicaid expansion to identify the effect of Medicaid on fringe banking use. In order to establish a causal effect, one would like to compare average fringe banking use in Medicaid expansion states to a valid counterfactual. The ideal regression for examining the effect of Medicaid expansion on fringe banking services is:

$$FringeUse_{ist} = \beta_0 + \beta_1 MedicaidXPost + \beta_2 Post + \gamma_s + \delta'X + \epsilon$$

where *FringeUse* for household *i* from state *s* at time *t* is an indicator for whether the household had used various fringe banking services: money orders, check cashing services, pawn loans, payday loans, rent-to-own contracts, and retail auto loans.

Difference-in-difference designs use the time path of non-expansion states to create this counterfactual. However, the assumptions required for it to be an unbiased estimate require that the pre-periods trends must be similar between expansion states and non-expansion states. Figure A1 demonstrates that for several variables, including the proportion of residents with a high school degree or less, the proportion of unbanked

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<sup>12</sup> The FDIC-CPS did not include a question about remittances in the 2009 survey; it did not include a question about auto-title loans in the 2009 or 2011 surveys, so those types of providers are not in this analysis.

households, average household income, and any fringe credit use, this “parallel trends” assumption may not hold.

To address concerns that states that expanded Medicaid are different from those who did not, we utilize the synthetic control methodology developed by Abadie et al., (2010).<sup>13</sup> This technique has also been used by other researchers also examining the effect of the Medicaid expansion (see for example Hu et al, 2016; Kaestner et al. 2015; Courtemanche et al., 2017). While similar in spirit to difference-in-differences, the synthetic control methodology differs in substantive ways. For example, consider an expansion state such as Arkansas. While difference-in-difference weights all non-expansion states equally to form the counterfactual, the synthetic control approach instead estimates that the appropriate synthetic control for Arkansas is 8.8 percent Louisiana; 70.3 percent Tennessee; 20.9 percent Texas. All other states are not used in estimating the treatment effect for Arkansas. The degree to which bias is reduced depends upon whether the appropriate weights are constructed. Following the guidance from Cavallo et al., (2013), we calculate weights using the following characteristics related to both Medicaid enrollment and fringe banking use: race (white; black; Hispanic); education (high school or lower); age bins (20-29; 30-39); the state’s unemployment rate.<sup>14</sup> As demonstrated in Figure A2, this weighting procedure appears to generate a better counterfactual in the pre-period, and (by assumption) in the post-period as well.

In future versions of the paper, we plan to implement propensity score matching and simulated instruments to test the robustness of our results.

### *Inference*

While the synthetic control methodology is advantageous in reducing pre-intervention bias of selected variables, its small sample nature precludes standard techniques of statistical inference. While several approaches have been proposed, there is still some debate regarding the optimal method. We follow the original approach of Abadie et al. (2010), using a variant of randomization inference that uses the average of a series of placebo test results to calculate Type I error. We implement and conduct inference using the “synth\_runner” Stata module (Quistorff and Galiani, 2016); we report standardized p-values.

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<sup>13</sup> The original methodology developed in Abadie et al. (2010) is a response to concerns of bias in difference-in-difference when selected variables exhibit differential time trends prior to the intervention (i.e., when the parallel trends assumption is violated). In contrast to using an equally weighted average of all “control” units, Abadie et al., (2010) introduce a weighting procedure that instead optimally create weights of the “control” units that allow for a better estimate of the counterfactual (“synthetic control”). This approach was then extended to multiple treated groups by Cavallo et al. (2013). The extension of multiple treated units allows for a treatment effect estimate for each treated unit. The average treatment effect reported here, for example, is then an average of each of the 27 treatment effects.

<sup>14</sup> We report the weights used for the outcomes *Any AFS Use* and *Any AFS Credit Use* in Appendix Table A. Future versions of this paper will explore the sensitivity of the weights and results to the chosen variables.

### *Treatment Group Assignment*

Medicaid programs differ across states and the same is true of the Medicaid expansion. In fact, prior to the ACA, several states already expanded their Medicaid programs. This variation makes determining the relevant treatment and control groupings for the synthetic control methodology more difficult. For example, Hu et al. (2016) use two different classifications of treatment and control states based upon when states expanded Medicaid while Courtemanche et al. (2017) use a third classification. Therefore, we report primary results from our main classification of states that is very close to the preferred classification of Hu et al. (2016). We then examine the robustness of results with the additional classification of Hu et al. (2016) and those of Courtemanche et al. (2017).<sup>15</sup> A table of these various treatment assignments, with selected results, is presented in Appendix Table B; a list of our groupings is in Appendix C. Results are generally of the same sign, magnitude, and significance.

An alternative way to assess whether the groupings are reasonable is to examine whether the allocation of states to Expansion or non-Expansion results in comparable averages of key variables. In Table 1 we present both the unweighted averages (Column 2) and the averages using the synthetic control weights (Column 3) to demonstrate that weighting reduces the observable differences between Expansion and non-Expansion states. While the similarities among the variables for which the weights were generated is to be expected, the similarity among other variables provides some reassurance that the weights reduce bias in our estimates of the effects of Medicaid on financial outcomes.

## **4. Results**

### *Effects on Health Insurance, Fringe Banking Use, and Bank Accounts*

In Table 2 we present results on the effect of Medicaid expansion for state-level averages of all observations from the March CPS and the FDIC Supplements (Column 1) and state-level averages of all matched households (Column 2). Because the FDIC survey is conducted at the household level, there may be concerns of measurement error since fringe banking use may vary within a household. Therefore in Column 3 we use the state-level averages of single adult households, where the household is the same as the individual.

We begin by showing that our approach results in health insurance increases in line with the literature; estimates are in Panel A. Households in states that expanded Medicaid were 5.6-6.7 percentage points more likely to have anyone in the household enrolled in Medicaid, all significant at the 1 percent level. These estimates are similar to the results cited for individuals in expansion states of 5.9 percentage points in Courtemanche et al. (2017). We see similar gains in the likelihood that anyone in the household was enrolled

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<sup>15</sup> Our analysis excludes Indiana and Pennsylvania, because each state's Section 1115 waiver for the expansion make their classification challenging. Indiana launched an alternative Medicaid expansion plan that took effect in January 2015; Pennsylvania also undertook an alternative Medicaid expansion plan in 2014; following the election of Gov. Wolf in the 2014 the state undertook the traditional Medicaid expansion.

in employer-provided health insurance, potentially due to increase of take-up in employer-provided health or insurance or increase in employers offering health insurance because of the employer mandate. We find the Medicaid expansion significantly reduced the probability that everyone in household was uninsured by 2.1-3.3 percentage points. Results are substantially larger among the sample of childless adults. For these “individual households” Medicaid expansion increased their enrollment by 8 percentage points and decreased the likelihood of not being insured by 4.9 percentage points.

Turning to the effects of Medicaid expansion on fringe product use (Panel B), we find that Medicaid expansion states report a decrease in any fringe product use by 0.5-0.8 percentage points, although effects are only statistically significant in the matched sample. This decrease is driven by reductions in fringe credit use in the past 12 months by 0.8-1.5 percentage points, driven by a 1 percentage point decrease in pawn loans, and a 1.6 decrease in payday loans. Part of this increase may be driven by increases in access to the banking system. Medicaid expansion causes a 2 percentage point reduction in the likelihood a household is unbanked, possibly due to improvements of creditworthiness of households or increased savings from a reduction in health care expenditures. Childless adults are 2.6 percentage points less likely to use a fringe banking service, driven by a 3.6 percentage point decrease in payday loans (although a 0.7 percentage point increase in rent-to-own contracts). One advantage of the synthetic control methodology is that we are able to estimate a separate treatment effect for each treated unit; we present the treatment effects for these outcomes by state in Appendix Table D. In our estimation, Delaware’s Medicaid expansion is estimated to have had the largest decrease in fringe banking use by 8 percentage points, while New Mexico’s expansion is estimated to have increased fringe banking use by 6 percentage points. Therefore, our estimates are best interpreted as the average effect of Medicaid expansion; overall, these show that Medicaid expansion results in a small but statistically distinguishable reduction in fringe banking providers.

However, the impact of Medicaid expansion may be larger among households who are more likely to use fringe banking services: low income, low education, and minority households. Results in Panel A of Table 3 show that Medicaid expansion increased the likelihood that someone in the household was on Medicaid by 10-16 percentage points, with little change in employer-provided health insurance and a 4.2-6.4 reduction in the likelihood that the entire household was uninsured. Therefore, impacts on fringe banking use may be larger for groups that experienced larger increases in health insurance coverage.

We report in Panel B of Table 3 that although overall fringe banking use did not significantly change as a result of Medicaid expansion, fringe credit use decreased by 1.4-4.4 percentage points, driven by reductions in minority households. Low income and low education groups report 1.2-3 percentage point reductions in pawn loans. All groups report significant decreases in pawn loans by 1.8-5.3 percentage points, driven by minority households. Among low education and low income groups we also observe a 0.8 percentage point increase in rent-to-own contracts.

We also find that Medicaid expansion decreases the likelihood that a household lacks a bank account by 1.6-3.7 percentage points. This estimate provides evidence that credit is improving, echoing to the results of Hu et al., (2016). Overall, these point estimates suggest that Medicaid expansions improved credit and encouraged households to be less reliant on more expensive sources of credit.

### *Medical Expenditure Outcomes*

Our rich data can be used to further assess whether these financial behavior changes are directly due to reduced medical debt. The March CPS asked detailed questions on medical expenditures and select questions on subjective self-reported health status. On average, approximately 22 percent of households report a medical expenditure more than 5 percent of their disposable income; nearly 10 percent report a medical expenditure more than 10 percent of their disposable income (Baird, 2016). While those averages are confounded by income and health insurance—some individuals may elect to not seek treatment at all due to the cost—they are illustrative of the fact that other factors may also play into the relationship between health insurance and financial behavior.

In Table 3 we present results from the same variables used to construct the fringe banking and health insurance outcomes. We again show results from all households in the March CPS (Column 1), all households in the matched data (Column 2), childless adults (Column 3), low-income households (Column 4), low-education households (Column 5), and minority households (Column 6). For households in Medicaid expansion states, we find increases in the likelihood of reporting a fair or poor health status ranging from 0.01 in the matched sample to 2.9 percentage points in the low education sample. We also observe significant increases in the likelihood of a self-reported health disability that prevents working of 2.9 percentage points in the matched sample and 7.6 percentage points in the low income sample. These averages may reflect that newly insured individuals may be increasing visits to doctors and taking new information on their health status and conditions under advisement. While we lack data to directly test this channel, Glied et al., (2017) find that enrollment in health insurance through the marketplace decreased the likelihood of not receiving medical care by as much as 25 percent.

We do not find significant decreases in the average premium payment household face in any sample, further evidence that our estimates are driven by newly insured individuals as opposed to individuals switching from private to public insurance. However, fringe banking reductions accompany decreases in medical expenditures. Overall average out-of-pocket expenditures significantly decrease by \$155 for low income households, by \$531 among childless adults and \$919 in low education households. Impacts on other groups are large but not statistically significant. We find small reductions in over-the-counter expenditures for the matched sample and for low education. Overall the decrease in out-of-pocket expenditures is driven by an average reduction in medical expenditures of

\$389. Childless adults decrease medical expenditures by \$467. Most other groups also report large but statistically insignificant reductions. These results suggest that Medicaid expansion reduced fringe banking usage by reducing medical debt.

### *Causal Estimates*

We can create a back of the envelope calculation using estimates in Table 2 to try to establish a causal estimate of the effect of Medicaid on fringe banking products. With our estimated 6.7 percentage increase in households reporting Medicaid receipt after a state accepts the expansion and the 1.5 percentage point reduction in any fringe bank credit use, the constructed Wald estimator becomes  $\beta^{IV} = \frac{-0.015}{0.067} = -0.22$ . This Wald estimator, is the simplest of all instrumental variable estimators. In future versions of this paper, we will also explore IV estimates.

## **5. Discussion and Conclusion**

In this paper, we estimate the effect of the ACA's Medicaid expansion on the use of fringe banking providers, such as payday lenders, pawn shops, check cashing facilities and similar establishments. As a result of the creation of a detailed dataset, we are able to identify new barriers for low-income households to joining the banking system. With a synthetic control approach, results suggest that Medicaid eligibility reduced reliance on fringe banking providers, specifically pawn shops and payday loan outlets. Our results thus far indicate that Medicaid coverage provide risk-protection by reducing the demand for fringe banking products. The small positive to neutral effects on employment found in the literature cannot adequately explain the decrease in fringe banking providers (Goopta et al., 2016).

This research is timely as large-scale changes to the ACA are currently being proposed, including possible elimination of the Medicaid expansion, restructuring Medicaid as a block-grant, reducing the generosity of coverage on state exchanges, and/or expansions of high-deductible health plans. Most proposed changes are expected to increase both uninsurance and underinsurance rates. Quantifying whether and to the degree to which these individuals are better off compared to alternative insurance arrangements or remaining uninsured is important for policymakers. Overall, we seek to identify whether expanding public health insurance is also helpful for improving the well-being of low-income households.

At the same time, the federal government is emphasizing financial deregulation, including large changes to the Consumer Financial Protection Bureau (CFPB), which may increase the availability of fringe banking products. If Medicaid increases the reliance on fringe credit products, then there is a role for policy interventions to mitigate any detrimental financial effects of health insurance plan structures. These policies may range from more generous health insurance products to limiting the interest or usury rates on medical debt. Alternatively, the federal government could potentially work to coordinate

banks and hospitals to increase access to short-term credit, assisting families in creating feasible payment plans. Analyzing how policy can be used to reduce risk due to medical events, and the role of fringe banking providers in the financial behaviors of vulnerable populations, is key for improving the financial outcomes of low and moderate-income households.

This paper provides new evidence for policymakers on the relationship between public health insurance initiatives and the structure of the modern financial system. Specifically this paper has implications for the regulation of controversial fringe banking products. If Medicaid decreases the reliance on fringe credit products, then policymakers may be able to build on the Medicaid expansion to further increase the ability of households to participate in the modern financial system. Expanding health insurance may be an effective policy to improve financial outcomes for low-income households by encouraging less reliance on expensive sources of credit.

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**Table 1: Comparison of Pre-2014 Means for Treated and Synthetic Control States**

	Expansion State Means	Unweighted Non-Expansion State Means	Weighted Non-Expansion State Means
	(1)	(2)	(3)
<b>Panel A: Selected Demographics</b>			
White	0.804	0.812	0.841
Black	0.105	0.133	0.108
Hispanic	0.117	0.080	0.095
All Other Races	0.028	0.034	0.026
Age	46.31	45.69	45.848
Family Size	1.37	1.38	1.376
Total Family Income	57792	51095	51660
High School or Lower	0.400	0.422	0.406
Some College	0.274	0.298	0.299
Completed College or Higher	0.326	0.280	0.295
Household is Unbanked	0.062	0.073	0.059
<b>Panel B: AFS Product Type</b>			
Rent-To-Own	0.017	0.025	0.026
Refund Anticipation Loan	0.021	0.030	0.026
Pawn Shop	0.025	0.037	0.031
Payday Lender	0.023	0.033	0.030
Money Order	0.170	0.193	0.173
Check Cashier	0.067	0.084	0.077
Any AFS product	0.226	0.266	0.244
Any AFS credit product	0.067	0.095	0.088
<b>Panel C: Insurance Status</b>			
All Members Insured	0.745	0.708	0.708
All Members Uninsured	0.066	0.077	0.077
At Least One Household Member has..			
Medicaid	0.253	0.247	0.248
Medicare	0.147	0.140	0.150
Group (Employer) Insurance	0.715	0.669	0.666
Non-Group (Individual Market) Insurance	0.091	0.107	0.104

Notes: Above are state-level averages of selected variables prior to 2014. Individual characteristics refer to the most employed adult in the household. Respondents were not asked about remittances in 2009 and about auto title loans in 2009 or 2011. Total Household Income is not adjusted for inflation. Sample excludes households in which all members are 65 or older. Column 3 uses the weights of the synthetic control for the outcome "Any AFS Credit In Past 12 Months", constructed using the following variables: race, education (a dummy for GED/HS degree or lower), age bins (20-30 or 20-30), and the state-level unemployment rate.

**Table 2: Effects on Health Insurance and Fringe Banking Use**

	<i>All Households</i>		<i>All Matched Households</i>		<i>Childless Adults</i>	
<u>Panel A: Health Insurance Effects</u>	(1)		(2)		(3)	
Anyone in HH on Medicaid	0.056	***	0.067	***	0.080	***
<i>pvalue =</i>	<i>0.000</i>		<i>0.000</i>		<i>0.000</i>	
Anyone in HH on ER Ins	0.052	**	0.041	**	0.033	
<i>pvalue =</i>	<i>0.015</i>		<i>0.014</i>		<i>0.505</i>	
Entire HH Uninsured	-0.021	***	-0.033	***	-0.049	***
<i>pvalue =</i>	<i>0.000</i>		<i>0.000</i>		<i>0.000</i>	
<u>Panel B: AFS Usage</u>						
Any AFS 12 Month Usage	-0.005		-0.008	*	-0.021	
<i>pvalue =</i>	<i>0.500</i>		<i>0.096</i>		<i>0.552</i>	
Any AFS Credit 12 Month Usage	-0.008		-0.015	**	-0.026	***
<i>pvalue =</i>	<i>0.277</i>		<i>0.026</i>		<i>0.000</i>	
Money Order	-0.005		-0.003		-0.024	
<i>pvalue =</i>	<i>0.402</i>		<i>0.140</i>		<i>0.393</i>	
Check Cashing	-0.002		-0.011		0.002	
<i>pvalue =</i>	<i>0.553</i>		<i>0.226</i>		<i>0.536</i>	
Pawn Loans	-0.002		-0.010	**	-0.012	
<i>pvalue =</i>	<i>0.600</i>		<i>0.014</i>		<i>0.159</i>	
Rent to Own	-0.005	**	0.004		0.007	**
<i>pvalue =</i>	<i>0.036</i>		<i>0.173</i>		<i>0.016</i>	
Payday Loans	-0.009	***	-0.016	***	-0.036	***
<i>pvalue =</i>	<i>0.009</i>		<i>0.000</i>		<i>0.002</i>	
Retail Auto Loan	-0.001		-0.003		0.007	
<i>pvalue =</i>	<i>0.907</i>		<i>0.753</i>		<i>0.468</i>	
Unbanked	-0.001		-0.020	*	-0.011	
<i>pvalue =</i>	<i>0.531</i>		<i>0.098</i>		<i>0.544</i>	

Notes: Above are synthetic control estimates for insurance outcomes (Panel A) and fringe banking outcomes (Panel B) based upon our primary classifications of Treatment and Control groups. Groups are matched on state-level averages of race, age, education, and unemployment rate. Inference was conducted using the *synth\_runner* package in Stata with standardized p-values reported. Column 1 presents synthetic control results for all households; Column 2 presents results for the matched sample of households in both the March CPS and the FDIC Supplement. Column 3 presents synthetic control results for the matched sample of single adult households. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1



**Table 3: Effects of Medicaid Expansion on Health Insurance and Fringe Banking Use Among Selected Demographics**

	<i>Low Income Households</i>		<i>Low Education Households</i>		<i>Minority Households</i>	
<u>Panel A: Health Insurance</u>						
<u>Effects</u>	(1)		(2)		(3)	
Anyone in HH on Medicaid	0.160	***	0.105	***	0.100	**
<i>pvalue =</i>	<i>0.000</i>		<i>0.000</i>		<i>0.024</i>	
Anyone in HH on ER Ins	0.008		-0.012		-0.016	
<i>pvalue =</i>	<i>0.508</i>		<i>0.979</i>		<i>0.786</i>	
Entire HH Uninsured	-0.064	***	-0.042	***	-0.050	***
<i>pvalue =</i>	<i>0.001</i>		<i>0.000</i>		<i>0.004</i>	
<u>Panel B: AFS Usage</u>						
Any AFS 12 Month Usage	-0.038		0.002		-0.012	
<i>pvalue =</i>	<i>0.503</i>		<i>0.846</i>		<i>0.505</i>	
Any AFS Credit 12 Month Usage	-0.033	*	-0.014	**	-0.044	*
<i>pvalue =</i>	<i>0.097</i>		<i>0.015</i>		<i>0.072</i>	
Money Order	-0.035		0.009		-0.040	
<i>pvalue =</i>	<i>0.149</i>	***	<i>0.153</i>		<i>0.526</i>	
Check Cashing	-0.009		-0.011		-0.020	
<i>pvalue =</i>	<i>0.734</i>		<i>0.378</i>		<i>0.915</i>	
Pawn Loans	-0.020	**	-0.012	***	-0.018	
<i>pvalue =</i>	<i>0.021</i>		<i>0.004</i>		<i>0.501</i>	
Rent to Own	0.008	**	0.008	*	-0.009	
<i>pvalue =</i>	<i>0.507</i>		<i>0.099</i>		<i>0.215</i>	
Payday Loans	-0.036	***	-0.018	***	-0.053	***
<i>pvalue =</i>	<i>0.000</i>		<i>0.003</i>		<i>0.000</i>	
Retail Auto Loan	0.001		-0.004		-0.007	
<i>pvalue =</i>	<i>0.744</i>		<i>0.992</i>		<i>0.720</i>	
Unbanked	-0.037	***	-0.016	***	-0.030	**
<i>pvalue =</i>	<i>0.006</i>		<i>0.005</i>		<i>0.023</i>	

Notes: Above are synthetic control estimates for insurance outcomes (Panel A) and fringe banking outcomes (Panel B) based upon our primary classifications of Treatment and Control groups. Groups are matched on state-level averages of race, age, education, and unemployment rate. Inference was conducted using the synth\_runner package in Stata with standardized p-values reported. Column 1 presents synthetic control results for matched households making under \$30,000 per year; Column 2 presents results for the matched sample of households where the most employed adult has less than a bachelors degree. Column 3 presents synthetic control results for the matched sample of households where the most employed adult is not white and is not hispanic.\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Table 4: Health and Expenditure Effects of Medicaid Expansion**

	<i>All Households</i>	<i>Matched Households</i>	<i>Childless Adults</i>	<i>Low Income Households</i>	<i>Low Education Households</i>	<i>Minority Households</i>
	(1)	(2)	(3)	(4)	(5)	(6)
Health Condition is Fair/Poor	-0.003	0.010	0.014 ***	0.018	0.029 ***	0.028
<i>pvalue =</i>	<i>0.601</i>	<i>0.297</i>	<i>0.001</i>	<i>0.103</i>	<i>0.007</i>	<i>0.482</i>
Health Disability Reported	-0.001	0.029 *	0.036	0.076 ***	0.048 ***	0.025
<i>pvalue =</i>	<i>0.989</i>	<i>0.073</i>	<i>0.117</i>	<i>0.000</i>	<i>0.000</i>	<i>0.591</i>
Health Insurance Premiums	75.426	-19.610	-37.122	-63.386	-102.652	-94.457
<i>pvalue =</i>	<i>0.989</i>	<i>0.776</i>	<i>0.756</i>	<i>0.581</i>	<i>0.348</i>	<i>0.620</i>
Out Of Pocket Expenditures	-546.736	-570.275	-530.706 *	-155.105 ***	-919.416 ***	280.587
<i>pvalue =</i>	<i>0.262</i>	<i>0.711</i>	<i>0.072</i>	<i>0.001</i>	<i>0.005</i>	<i>0.274</i>
Over the Counter Expenditures	-22.420	-31.310 *	3.570	2.361	-44.066 **	25.898 *
<i>pvalue =</i>	<i>0.472</i>	<i>0.069</i>	<i>0.530</i>	<i>0.642</i>	<i>0.014</i>	<i>0.098</i>
Medical Expenditures	-316.089	-388.629 ***	-467.313 ***	-40.791	-715.645	283.369
<i>pvalue =</i>	<i>0.157</i>	<i>0.000</i>	<i>0.000</i>	<i>0.908</i>	<i>0.367</i>	<i>0.820</i>

Notes: Above are synthetic control estimates for various outcomes based upon our primary classifications of Treatment and Control groups. Estimates are above standardized p-values. Inference was conducted using the synth\_runner package in Stata. Column 1 presents synthetic control results for the state-level averages of all observations. Column 2 presents the same estimates for the matched sample. Column 3 uses the matched sample of single adult households without kids. Column 4 uses the matched sample of households earning less than \$30,000 per year. Column 5 uses the linked sample where the most employed adult has less than a college degree. Column 6 is the sample of all minority households. Out-of-pocket expenditures is defined as total family spending on medical care and items; it is composed of medical expenditures and over-the-counter expenditures. Medical expenditures is defined as total family spending on medical care and medical equipment, excluding over-the-counter items. Over-the-counter expenditures is defined as total family spending on over-the-counter health related products. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Data Appendix

Variable	Year	FDIC-CPS Question
Own a Bank Account in Past Year	2009	Do you or does anyone in your household currently have a checking or savings account? When was the last time you or anyone in your household had a checking or savings account, was it – within the last year or more than 1 year ago? (1) Within the last year (2) More than 1 year ago
	2011	Do you or does anyone in your household currently have a checking or savings account? When was the last time you or anyone in your household had a checking or savings account, was it – within the last year or more than 1 year ago?
	2013	Do you or does anyone in your household currently have a checking or savings account? When was the last time you or anyone in your household had a checking or savings account, was it – within the last year or more than 1 year ago?
	2015	Do you or does anyone in your household currently have a checking or savings account now? Have you or anyone in your household had a checking or savings account in the past 12 months, that is since June 2014?
Own a Checking Account in Past Year	2009	Not Available.
	2011	Do you or does anyone in your household currently have a checking or savings account? What type or types of accounts (do/does) (you/Name) have? (Asked for each adult (16+) in the household)
	2013	Do you or does anyone in your household currently have a checking or savings account? What type or types of accounts (do/does) (you/Name) have? (Asked for each adult (15+) in the household)
	2015	Do you or does anyone in your household currently have a checking or savings account? What type or types of accounts (do/does) (you/Name) have? (Asked for each adult (15+) in the household)
Own a Savings Account in Past Year	2009	Not Available.
	2011	Do you or does anyone in your household currently have a checking or savings account?

		What type or types of accounts (do/does) (you/Name) have? (Asked for each adult (16+) in the household)
	2013	Do you or does anyone in your household currently have a checking or savings account? What type or types of accounts (do/does) (you/Name) have? (Asked for each adult (15+) in the household)
	2015	Do you or does anyone in your household currently have a checking or savings account? What type or types of accounts (do/does) (you/Name) have? (Asked for each adult (15+) in the household)
Use a Check Casher in Previous 12 Months	2009	Have you or anyone in your household ever gone to a place other than a bank, a savings and loan or a credit union to cash a check that was received from someone else? How often do you or anyone in your household cash a check received from someone else at a place other than a bank? (“at least a few times a year; once or twice a year; almost never”). We treat households that report using the non-bank check casher product “once or twice a year” or “at least a few times a year” as having used the product in the past 12 months.
	2011	Have you or anyone in your household ever gone to a place other than a bank to cash a check that was received from someone else? In the past 12 months, did you or anyone in your household go to a place other than a bank to cash a check received from someone else?
	2013	Have you or anyone in your household ever gone to a place other than a bank to cash a check that was received from someone else? In the past 12 months, did you or anyone in your household go to a place other than a bank to cash a check received from someone else?
	2015	In the past 12 months, that is since June 2014, did you or anyone else in your household go to some place other than a bank to cash a check?
Use a Money Order in Previous 12 Months	2009	Have you or anyone in your household ever purchased a money order at a place other than a bank, a savings and loan or a credit union? How often do you or anyone else in your household purchase money orders at a place other than a bank, a savings and loan or a credit union?

		(“at least a few times a year; once or twice a year; almost never”). We treat households that report using a non-bank money order “once or twice a year” or “at least a few times a year” as having used the product in the past 12 months.
	2011	Have you or anyone in your household EVER gone to a place other than a bank to purchase a money order? In the past 12 months, did you or anyone in your household go to a place other than a bank to purchase a money order?
	2013	Have you or anyone in your household EVER gone to a place other than a bank to purchase a money order? In the past 12 months, did you or anyone in your household go to a place other than a bank to purchase a money order ?
	2015	In the past 12 months, did you or anyone else in your household go to some place other than a bank to purchase a money order?
Use a Rent-to-Own (RTO) Contract in Previous 12 Months	2009	Have you or anyone in your household ever rented or leased anything from a rent-to-own store because it couldn’t be financed any other way? How many times did you or anyone in your household do business at a rent-to-own store? (“at least a few times a year; once or twice a year; almost never”). We treat households that report using an RTO “once or twice a year” or “at least a few times a year” as having used the product in the past 12 months.
	2011	Have you or anyone in your household EVER rented or leased anything from a rent-to-own store because it couldn’t be financed any other way? In the past 12 months, did you or anyone in your household have a rent-to-own agreement?
	2013	Have you or anyone in your household EVER rented or leased anything from a rent-to-own store because it couldn’t be financed any other way? I am not talking about leasing cars or other installment payment plans that require credit check or layaway plans. In the past 12 months, did you or anyone in your household have a rent-to-own agreement?
	2015	Some stores allow people to rent to own items such as furniture or appliances. We do not mean stores that offer installment plans or layaway plans. In the past 12 months, did you

		or anyone else in your household rent anything from a rent-to-own store because it couldn't be financed any other way?
Use a Pawn Loan in Previous 12 Months	2009	Have you or anyone in your household ever sold items at a pawn shop? How often do you or anyone in your household sell items at pawn shops? (“at least a few times a year; once or twice a year; almost never”). We treat households that report using a pawn loan “once or twice a year” or “at least a few times a year” as having used the product in the past 12 months.
	2011	Have you or anyone in your household EVER pawned an item at a pawn shop because cash was needed, and not just to sell an unwanted item? In the past 12 months, have you or anyone in your household pawned an item because cash was needed? Again, do not count selling unwanted items.
	2013	Have you or anyone in your household EVER pawned an item at a pawn shop because cash was needed, and not just to sell an unwanted item? In the past 12 months, have you or anyone in your household pawned an item because cash was needed? Again, do not count selling unwanted items.
	2015	Did you or anyone else in your household pawn an item at a pawn shop in the past 12 months? Do not include selling an unwanted item to a pawn shop?
Use a Payday Lender in Previous 12 Months	2009	How many times in the last 12 months did you or anyone in your household use payday loan or payday advance services?
	2011	Have you or anyone in your household EVER taken out a payday loan? In the past 12 months, did you or anyone in your household have a payday loan?
	2013	Have you or anyone in your household EVER taken out a payday loan or payday advance at a place other than a bank? In the past 12 months, did you or anyone in your household have a payday loan or payday advance at a place other than a bank?

		In the past 12 months, have you or anyone in your household taken out a nonbank payday loan through the internet?
	2015	Did you or anyone else in your household take out a payday loan or payday advance from some place other than a bank in the past 12 months?
Use an Auto-Title Loan in Previous 12 Months	2009	Not Available.
	2011	Not Available.
	2013	Have you or anyone in your household EVER taken out an auto title loan at a place other than a bank? These are loans where a car title is used to borrow money for a short period of time. They are NOT loans used to purchase an automobile. In the past 12 months, did you or anyone in your household have an auto title loan?
	2015	Auto title loans use a car title to borrow money for a short period of time. They are NOT loans used to purchase a car. In the past 12 months, did you or someone else in your household take out an auto title loan?
Tax Refund Anticipation Loan (RAL) in Previous 12 Months	2009	In the past five years, have you or anyone in your household taken out a tax refund anticipation loan? The two exceptions to this is for tax refund anticipation loans (RALs) where the 2009 survey asked if anyone in the household had taken one out in the past 5 years while 2011, 2013, and 2015 surveys asked about the past 12 months. We treat households that used a RAL in the past 5 years as having used one in the past 12 months
	2011	Have you or anyone in your household EVER taken out a tax refund anticipation loan? Have you or anyone in your household taken one out in the past 12 months?
	2013	Have you or anyone in your household EVER taken out a tax refund anticipation loan, or used a tax preparation service to receive your tax refund faster than the IRS would provide it? Have you or anyone in your household received one of these loans or refunds in the past 12 months?
	2015	In the past 12 months, that is since June 2014, did you or anyone else in your household

		take out a tax refund anticipation loan, or use a tax preparation service in order to receive your tax refund faster than the IRS would provide it?
Remittance in Previous 12 Months	2009	Not available.
	2011	Have you or anyone in your household EVER gone to a place other than a bank to give or send money to relatives or friends living outside the U.S.? Please include all money for gifts or loans. In the past 12 months, did you or anyone in your household go to a place other than a bank to give or send money to relatives or friends living outside the U.S.?
	2013	Have you or anyone in your household EVER gone to a place other than a bank to give or send money to relatives or friends living outside the U.S.? Please include all money for gifts or loans. In the past 12 months, did you or anyone in your household go to a place other than a bank to give or send money to relatives or friends living outside the U.S.?
	2015	In the last 12 months, that is since June 2014, did you or someone else in your household send money to family or friends living outside of the US? In the last 12 months, did you or someone else in your household send money abroad using a bank? In the last 12 months, did you or someone else in your household send money abroad using a place other than a bank?



**Appendix Table A: State Weights for Synthetic Control,  
By Outcome**

State Abbreviation (1)	Any AFS Credit Usage (2)	Any AFS Credit (3)
AL	0.003	0.000
FL	0.153	0.130
GA	0.004	0.000
ID	0.012	0.017
KS	0.000	0.000
ME	0.128	0.122
MS	0.037	0.061
MO	0.000	0.000
NE	0.027	0.018
NC	0.022	0.031
OK	0.013	0.026
SC	0.002	0.000
SD	0.021	0.018
TN	0.081	0.086
TX	0.073	0.068
UT	0.172	0.175
VA	0.102	0.096
WI	0.031	0.028
WY	0.000	0.000

Notes: Above are states used as "control" states in the synthetic control methodology. Alaska, Louisiana, and Montana are counted as Treatment states for the relevant years. Above are the average weights for all treatment states for the selected outcomes.

**Appendix Table B: Effects of Medicaid Expansion on Fringe  
Banking Usage, Alternative Groupings**

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<u>Groupings:</u>	Anyone in Household on Medicaid Estimate (Standardized P-Value) (1)	Any Fringe Bank Usage Estimate (Standardized P-Value) (2)	Any Fringe Credit Usage Estimate (Standardized P-Value) (3)
Alt. Grouping 1: Courtemanche et al., (2017)	0.060*** (p=0.000)	-0.013*** (p=0.000)	-0.011* (p=0.078)
Alt. Grouping 2: Hu et al., (2016)'s Narrow Sample	0.021** (p=0.030)	0.001** (p=0.020)	-0.007* (p=0.066)
Alt. Grouping 3: Hu et al., (2016)'s Narrow Sample	0.014 (p=0.204)	0.007 (p=0.108)	-0.016*** (p=0.009)

Notes: Above are synthetic control estimates for various main and placebo outcomes based upon different classifications of Treatment and Control groups.  
\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Appendix C: Groupings of  
Treatment and Control States Used  
in Analysis**

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<u>Non-Expansion States</u>	<u>Expansion States</u>
Alabama	Alaska (2017 only)
Florida	Arizona
Georgia	Arkansas
Idaho	California
Kansas	Colorado
Maine	Connecticut
Mississippi	Delaware
Missouri	DC
Nebraska	Hawaii
North Carolina	Illinois
Oklahoma	Iowa
South Carolina	Kentucky
	Louisiana (2017 only)
South Dakota	Massachusetts
Tennessee	Michigan
Texas	Maryland
Utah	Minnesota
Virginia	Montana (2017 only)
Wisconsin	Nevada
Wyoming	New Hampshire
	New Jersey
	New Mexico
	New York
	North Dakota
	Ohio
	Oregon
	Rhode Island
	Vermont
	Washington
	West Virginia

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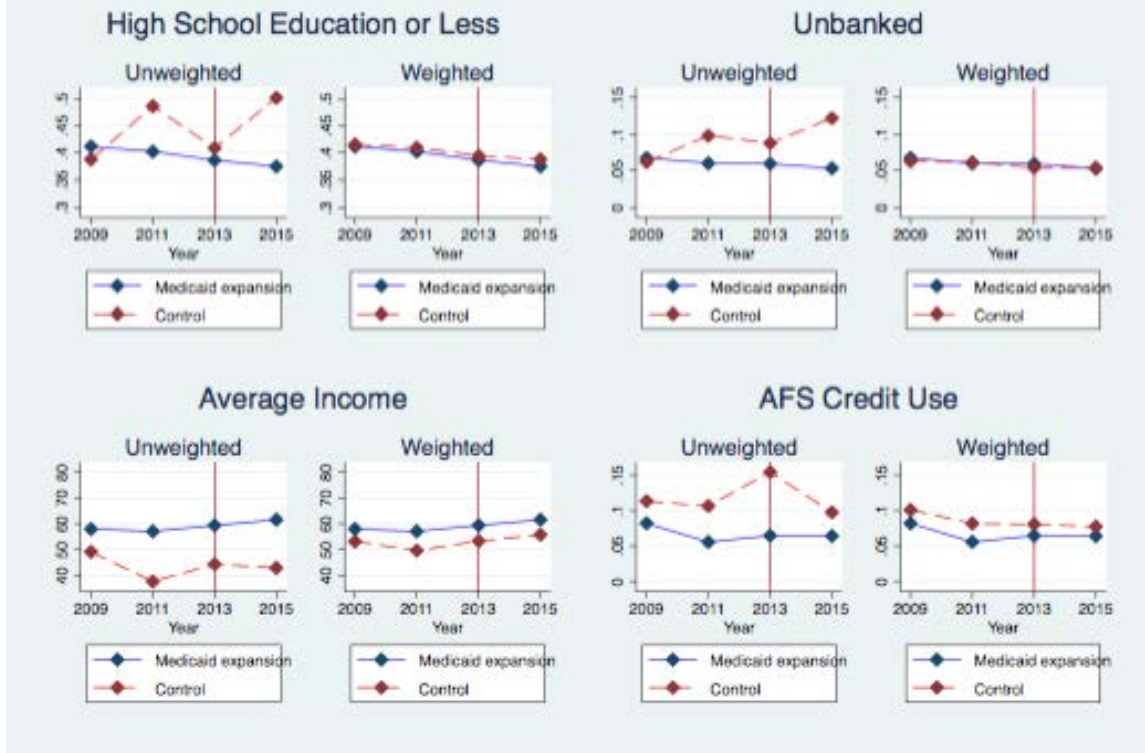
Notes: Indiana and Pennsylvania are excluded.

**Appendix Table D: Treatment  
Effects by State**

	Treatment Effect for Any AFS Product (1)	Treatment Effect for Any AFS Credit Product (2)
Arizona	-0.026	-0.023
Arkansas	0.046	0.003
California	-0.021	-0.009
Colorado	-0.039	-0.018
Connecticut	-0.045	-0.032
Delaware	-0.080	-0.041
Washington, D.C.	-0.011	-0.058
Hawaii	0.001	-0.014
Illinois	-0.055	-0.023
Iowa	-0.059	0.007
Kentucky	0.046	0.021
Maryland	0.003	0.004
Massachusetts	-0.012	-0.022
Michigan	0.002	0.017
Minnesota	-0.075	-0.025
Nevada	0.059	0.017
New Hampshire	-0.027	-0.019
New Jersey	-0.054	-0.037
New Mexico	0.067	0.024
New York	0.017	-0.017
North Dakota	-0.031	-0.031
Ohio	-0.007	-0.011
Oregon	0.040	-0.014
Rhode Island	-0.009	-0.034
Vermont	-0.055	-0.030
Washington	0.009	-0.002
West Virginia	0.013	0.007

Notes: Above are estimates for each Treatment state for the average rate of any AFS usage (Column B) and any AFS credit usage (Column C). Groups are matched on state-level averages of race, age, education, and unemployment rate.

Figure A1: Time Path of Selected Variables



Notes: Above are state-level means of selected variables taken from the merged FDIC-CPS data. All averages of non-expansion states are unweighted. HS Education or Less includes those with a GED. AFS or fringe banking credit products include payday loans, pawn shops, rent-to-own agreements, and refund anticipation loans; usage is within the past 12 months. Household income is based upon self-reported information in the March CPS. It is measured in 1000's of nominal dollars. The vertical line indicates the last data point prior to the implementation of the Medicaid expansion (2014)