Payday borrowing and household outcomes; Evidence from a natural experiment *

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Abstract

This paper examines the effect of restricting payday credit to payday users. Using administrative banking data from over ten thousand online payday borrowers, I exploit a natural experiment surrounding a 2013 U.S. Department of Justice initiative known as Operation Choke Point (OCP) that unexpectedly shut down dozens of unlicensed online payday lenders. Using a difference in differences framework, I find a persistent reduction in payday borrowing of treated households, those with a pre-existing relationship with a lender that is shut down. Relative to control households, treated households reduce borrowing by $136 per month, reduce the number of bounced checks by 17%, and increase consumption by 3%. These effects are persistent and observable six quarters after treatment. A cross-sectional analysis reveals that the positive outcomes following restricted payday loan access are concentrated among the heaviest pre-treatment borrowers. I conclude by analyzing what types of purchases payday loans are financing and find that about half of abnormal spending occurs in predictable categories such as mortgages, car loans, and insurance. Surprisingly, I find evidence of abnormal gambling activity the week following payday borrowing.

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1 Introduction

Does restricting access to payday credit improve or worsen household outcomes? The answer to this question is not obvious and is the source of much debate.

A nascent literature assesses the effects of payday loan access on household well-being. To date, the empirical evidence is mixed, with some papers concluding that payday borrowing does more harm than good (Melzer (2011), Carrell and Zinman (2014), Campbell et al. (2012), Skiba and Tobacman (2011)), others concluding the opposite (Morse (2011), Morgan and Strain (2008), Morgan et al. (2012), Zaki (2013), Zinman (2010)), and still others concluding that payday loans have little to no effect on household outcomes (Bhutta (2014), Bhutta et al. (2015)). Though surprising, these mixed empirical results need not be contradictory (Zingales (2015)). Rather, the mixed empirical results could illustrate underlying heterogeneity in both household characteristics and how payday loans are used. Further, the mixed results could partly be driven by data limitations in previous studies.

In this paper, I use a new administrative dataset and identification strategy that allow me to significantly extend the prior literature. The dataset contains transactional household-level banking data of over ten thousand payday users, from which I extract household-level information on payday borrowing, consumption, income, and financial distress proxies. My identification strategy exploits a natural experiment following the unexpected closure of dozens of unlicensed online payday lenders in a 2013 Department of Justice initiative known as Operation Choke Point. Using a difference in differences framework, I compare treated households, those with pre-existing relationships with lenders who are shut down, to control households, those with pre-existing relationships with lenders who are not shut down.

I begin by showing a large and persistent treatment effect of Operation Choke Point. Treated households persistently reduce the amount of payday borrowing relative to control households by $109 per month. However, when I restrict treated households to those living in states where payday lending is illegal, making it much more difficult for borrowers to substitute to other lenders, the magnitude of the treatment effect increases. In this restricted sample, treated households reduce borrowing by $136 per month relative to control households. Treated households experience a 17% reduction in the frequency of bounced checks, and a 3% increase in the level of consumption. These effects are persistent and remain economically significant over a year after treatment. Following payday bans, households exhibit no change in consumption volatility.

To better understand the underlying mechanisms, I perform a cross-sectional analysis and find that the positive outcomes following restricted payday loan access vary substantially across groups. The benefits of restricted access to payday credit are concentrated among the heaviest pre-treatment borrowers. Such households experience a 20% reduction in the frequency of bounced checks, a 5% reduction in the frequency of overdrafts, a 4% increase in the level of consumption, and no change in consumption volatility. These effects are non-existent for the group of light pre-treatment borrowers. I observe no difference in the response of high- and low-income borrowers.

I conclude the analysis by investigating the types of purchases that payday loans are financing. I find that approximately half of payday loan purchases are financing expenditures in categories that are predictable such as mortgages, auto loans, and insurance payments. Surprisingly, I find evidence of abnormal gambling activity the week following payday borrowing.

My paper adds to the literature in four ways. First, I use a new identification strategy that relies on the direct and immediate treatment effect caused by Operation Choke Point rather than a state-level change in payday lending laws. Whereas changes in state lending laws might slowly lead to lending closures or openings, the natural experiment I exploit is unexpected, immediate, and directly observable. Second, I directly observe not only household-level payday borrowing across dozens of lenders, but also high-frequency measures of consumption and financial distress. Third, unlike most prior studies that cannot discern from population data who uses payday loans, I restrict my analysis to the subset of the population that uses online payday loans. By doing so, I have more power in making causal statements about the population of interest. Fourth, my paper is the first to empirically decompose what types of purchases payday loans are financing across a wide variety of consumption categories including mortgages, auto loans, credit card payments, and ATM withdrawals at casinos.

The paper proceeds as follows. Section 2 provides background information. Section 3 describes the data. Section 4 explains the identification strategy. Sections 5 and 6 contains the pooled and cross-sectional analyses surrounding Operation Choke Point, respectively. Section 7 analyzes abnormal spending activity surrounding payday borrowing. Section 8 concludes.

2 Background

Payday and installment loans are common types of high-interest credit utilized by households. Payday loans are typically small loans (around \$500) that are repaid in full at the time of the borrower's next paycheck, while installment loans offered by payday lenders are slightly larger loans (around \$1,500) that are repaid over several paychecks. Interest rates on both payday and installment loans are very high, with effective annual rates (EARs) ranging from 400% to well over 1,000%.[1] Since the interest rates on both payday and installment loans offered by payday lenders are similar, I will hereafter refer to both types of loans simply as "payday loans." Despite the high interest rate of payday loans, 12 million U.S. households borrow from payday lenders every year, corresponding to five percent of the adult population

[1] For example, the EAR of a 14-day payday loan with a 15% fee is 3,686%, while the EAR of a 1-month payday loan with a 15% fee is 435%. In contrast, the EAR of a 14-day payday loan with a 25% fee is 32,987%, while the EAR of a 1-month payday loan with a 25% fee is 1,355%. In practice, fees and average loan maturity vary substantially across lenders and households.

(Pew (2014)).

Historically, payday loans were obtained through brick-and-mortar locations in which the borrower enters a storefront and provides a post-dated check as collateral in exchange for cash. However, in recent years, payday loans are increasingly obtained through internet lenders in which the loans and repayments are distributed electronically via direct deposit. The percentage of high-interest loans originating from online lenders is growing at a rapid pace. Stephens (2013) estimates that online payday loan volume grew from 10% of payday loans in 2006 to 33% of payday loans in 2013.

While traditional payday loans are controversial, online payday loans are even more so as online lenders often circumvent state laws by incorporating abroad or as tribal entities (Pew (2014)). During the application process, households provide online payday lenders proof of income along with their checking account and routing numbers. The lender distributes the loan through an electronic Automated Clearing House (ACH) transfer directly into the borrower's checking account. When the repayment date arrives, the lender will withdraw the agreed-upon amount irrespective of whether the borrower has the required amount in her checking account. Insufficient funds at the time of repayment result in an overdraft; multiple overdrafts may occur since the lender will continue attempting withdrawals until repaid.

Given the triple- to quadruple-digit EARs, the controversy surrounding payday lending is easy to understand. Opponents of payday lending argue that the availability of high-interest credit tempts financially unsophisticated or myopic households to borrow, potentially resulting in a debt trap (CFPB (2015)). On the other hand, proponents of payday lending argue that it provides necessary emergency financing to the financially constrained.[2] Empirically testing which of these arguments best explains borrower behavior is not only important from a policy standpoint, but is also important in understanding how households make borrowing decisions.

[2]For example, "CFPB Sets Sights on Payday Loans," *Wall Street Journal*, January 4, 2015.

Regulation on payday lending has fallen largely to the states. As of 2015, payday lending is illegal in 15 states.[3] Recently, however, the federal government has intervened on a few occasions. First, in 2007 the federal government passed the Military Lending Act, which effectively banned payday loans to military personnel. Second, the Department of Justice shut down dozens of online payday lenders in an initiative known as Operation Choke Point (OCP) in 2013. Finally, the Consumer Financial Protection Bureau is in the midst of designing new federal payday lending laws (CFPB (2015)).

3 Data

Aggregation of financial accounts is a popular service that allows households to easily monitor financial activities from across multiple financial institutions into a single web-page or smart-phone app. Account aggregation services often allow features such as budgeting, expense tracking, etc. Dozens of companies currently provide such services and my data comes from one of these services.

Once the user initially signs up for the free service, she is given the opportunity to provide the service with usernames and passwords to a variety of financial accounts (i.e. checking, savings, credit card, brokerage, retirement, mortgage, student loan, etc.) from any financial institution. However, my particular dataset is limited to bank and credit card transactions. After signing up, the service automatically and regularly pulls data from the user's financial institutions. The dataset contains transaction-level data similar to those typically found on monthly bank or credit card statements, containing the amount, date, and description of each transaction. As a result, I have high-fidelity data on consumption and income for over a million households. There is very little attrition in my sample.

I identify online payday loan transactions through a simple process. I first identify which

[3]See http://www.pewtrusts.org/en/multimedia/data-visualizations/2014/state-payday-loan-regulation-and-usage-rates and www.online-payday-loans.org.

transaction descriptions are most frequently leading to overdrafts. I manually identify which of these transactions are associated with online payday lenders.[4] I next visit each lender's website to determine if the lender also participates in other forms of lending such as auto title loans, debt consolidation, or mortgage refinancing. I exclude such lenders since these alternative loans offer larger loans with lower interest rates than payday loans since they are collateralized with physical assets. This process leaves me with 39 online payday lenders.

I focus exclusively on online payday loans in this paper since I cannot identify brick and mortar payday loans in my dataset. In a brick and mortar payday loan, the borrower receives the loan in cash and repays with a check. I observe checks in my dataset, but there is not sufficient information to differentiate a check to a grocer from a check to a payday lender.

In addition to online payday loans, I extract income, consumption, credit card payment, mortgage, car payments, student loans, the number of bounced checks and overdrafts. Further, I construct indicator variables for whether households have transactions with investment accounts, receive unemployment or Social Security payments, or make ATM withdrawals in casinos. All variables are winsorized at the 99$^{\text{th}}$ percentile.

4 Identification Strategy

To determine the causal impact of payday borrowing on household outcomes, a carefully constructed identification strategy is required since a naïve research design will suffer from the selection bias surrounding payday loans. For example, regressing household outcomes on payday loan usage is inappropriate since this would omit the unobserved demand shock which led to payday borrowing such as a broken car or hospital visit. A valid identification strategy, therefore, will need to exploit exogenous variation in payday loan demand or supply

[4] An alternate method of identifying payday transactions involves using internet searches and subsequent keyword searches to identify online payday lenders. The two methods produce an identical list of payday lenders, though the mapping of payday lenders to transactions found on bank statements is much simpler with the former process.

that is uncorrelated with unobserved demand shocks.

To date, the majority of the literature has achieved identification by relying on state-level changes to payday lending laws (for example, Melzer (2010) and Morgan et al. (2012)). Several papers have relied on state-level changes plus additional sources of variation. Carrell and Zinman (2014) use the additional variation of random assignment of the location of servicemen, while Dobrige (2014) relies on the additional variation of weather as demand shocks. Morse (2011) is one of the few papers to achieve identification without reliance on state-level changes in payday lending laws. She achieves identification through demand shocks, which occur exogenously in the form of earthquakes to different regions, and through the additional variation of the geographic location of lenders.

Online payday loans are illegal in 15 states, while in the remaining 35 states, payday loans are generally only legal if the lender is licensed to lend within the state. Seldom is this the case. In a given state, there are generally no more than five licensed lenders,[5] meaning that much of online payday lending is operating in an unregulated and illegal manner. Despite operating illegally, these lenders are difficult for regulators to reign in since many are either incorporated abroad or as tribal entities. Further, the lack of physical locations has been another barrier inhibiting regulators from intervening.

Without warning, the Department of Justice introduced Operation Choke Point around August of 2013, an initiative that shut down many of these unlicensed online payday lenders that were operating illegally. The Department of Justice pressured U.S. banks to terminate banking relationships with unlicensed payday lenders. Without bank accounts, affected payday lenders lost the ability to distribute loans or collect payment via ACH transfers. As a result, these lenders were effectively and immediately closed. This program was first mentioned in a *Wall Street Journal* article published on August 7, 2013.[6]

[5] www.online-payday-loans.org/state-licensed-lenders/

[6] "Probe Turns Up Heat on Banks; Prosecutors Target Firms That Process Payments for Online Payday Lenders, Others," *Wall Street Journal*, written August 7, 2013.

I determine which lenders close during the 2013-2014 observation window by observing the date that each lender stops lending. Inferring the shut down dates follows easily from the data, as illustrated in Figure 1. Panel A shows lending activity for three affected lenders in the sample, while Panel B shows aggregate lending activity across all lenders in the sample. As shown in Panel B, the majority of lenders were shut down during the two-month period beginning in August of 2013. Obtaining shutdown dates through any other manner would be impossible due to the opacity of Operation Choke Point and the relative obscurity of most online payday lenders. Despite the lack of public information surrounding OCP closures, my dates align well with the few closure dates I found from affected households as reported on several internet forums.[7]

The resulting list of lenders is found in Table 1. The second column contains the number of payday loan transactions in my dataset from each lender in the six-month period before OCP, from January 2013 to June 2013. The third column contains the estimate shutdown date. As shown in the table, the majority of payday lending is concentrated among a few lenders. For example, the top 5 lenders in my sample are CashnetUSA (41,472 pre-OCP transactions), Plain Green (27,176 pre-OCP transactions), Mobiloans (18,911 pre-OCP transactions), Ameriloan (16,183 pre-OCP transactions), and United Cash Loans (15,088 pre-OCP transactions). The bottom 5 lenders in my sample are Regions Bank (278 pre-OCP transactions), LiquidCash (927 pre-OCP transactions), Netcredit (1,023 pre-OCP transactions), Dollar Premier (1,185 pre-OCP transactions), and Fedfinsvcs (1,206 pre-OCP transactions).

As shown in Table 1, the three largest lenders in my sample were unaffected by Operation Choke Point. Further, 26 of the 39 lenders were shut down, leaving only 13 unaffected by Operation Choke Point. Table 2 and Figure 2 formally investigate pre-treatment differences

[7]For example, the following is a comment reported by a user after the closure of Ameriloan. On Sept. 23, 2013 a user wrote: "I have used them before and not had a major problem. But I am wondering now why their computers are down and have been for 3 weeks??????" www.consumeraffairs.com/finance/ameriloan.html

between lenders who remained open during OCP and those who were shut down. Prior to OCP, lenders who remain open issued 97,720 loans and collected 304,155 payments from borrowers. In contrast, lenders who were shut down issued 23,916 loans and collected 141,642 payments from borrowers. Clearly, the largest lenders were not the targets of Operation Choke Point.

The difference in the size of targeted lenders begs the question of whether they differ on other dimensions. Table 2 shows that the average sized loan of a lender who remained open during OCP is $478 while the average sized loan of a lender who is shut down during OCP is $546. The median loan of a lender who remained open during OCP is $255 while the median sized loan of a lender who is shut down during OCP is $500. This difference, also illustrated in Figure 2, is driven primarily by the $255 per loan limit imposed by California state payday lending laws.[8] Lenders who were not shut down during OCP adhered to state regulations such as California's $255 loan limit, while lenders who were shut down did not adhere to such limits. The second to last row indicates the percentage of transactions that were debits for the two types of lenders. A value of 50% would indicate that lenders are all traditional payday lenders (1 credit and 1 debit per loan), while a value greater than 50% would indicate that some of the lenders are involved with installment lending. As shown, lenders that were shut down were more likely to participate in installment lending.

In my analysis of Operation Choke Point in Sections 5 and 6, I restrict the sample to households that have at least one payday loan transaction from January 2013 to June 2013, the approximate six-month period before OCP begins. Treated households are those who borrowed during the six month pre-OCP window from any lender that is subsequently shut down. Control households are those who, during the six month pre-OCP window, borrowed exclusively from lenders who are not subsequently shut down. After applying the above filters, I am left with 13,353 households, 7,070 of which are in the treatment group, and

[8] "What You Need to Know About Payday Loans," *California Department of Business Oversight.*

6,283 of which are in the control group.

I plot average monthly payday borrowing for treated and control households in Panel A of Figure 3. Before Operation Choke Point, treated households borrowed approximately $250 per month from payday lenders while control households borrowed $180 per month. After Operation Choke Point, treated households borrowed $50 per month, while control households borrowed $100 per month. Clearly, Operation Choke Point was successful in deterring borrowing of treated households.

The fact that there is a sizable treatment effect in Panel A of Figure 3 implies that there are frictions preventing households from substituting to different lenders after Operation Choke Point. Upon finding out that their lender is closed, it may seem surprising that treated households do not seamlessly navigate to a new website to get another loan. However, there are many practical reasons which would prevent a household from doing so. One such reason is the lack of substitute lenders, which is especially pronounced for borrowers living in states where payday lending is illegal. In Panel B of Figure 3, I restrict the sample to treated households living in states where payday lending is illegal and control households to those living in states where payday lending is legal. In this restricted sample, I am left with 7,335 households, 1,928 of which are in the treatment group, and 5,407 of which are in the control group.

Panel B shows that treated households in this restricted sample borrow approximately $250 per month before Operation Choke Point. After OCP, treated households in the restricted sample reduce borrowing to $10 per month. Whereas the precise friction preventing households from substituting is ambiguous in the unrestricted subsample in Panel A, the friction preventing households from substituting to other lenders is well defined in Panel B. Such households would have had extreme difficulty finding lenders who remained operational through OCP and continued issuing illegal loans to households residing in states where payday lending is illegal.

My main analysis in Section 5 contains the results for both the unrestricted and restricted subsamples. However, when I explore cross-sectional differences in Section 6 I use the unrestricted sample to increase the statistical power of the tests.

In the remainder of this section I evaluate the differences between treated and control households during the pre-treatment period. I explore this in Panels B and C of Table 3 using data from January of 2013 to June of 2013. But first, in Panel A of Table 3, I explore the question of how payday borrowers differ from the general population. To answer this question, I use all available data from 2010 to 2015. In Panel A, payday borrowers are defined as any household that has any observed online payday transaction over the entire sample period.

In each panel, the first two columns compare the average monthly value of the respective groups, while the third computes the percentage difference between the two. Columns 4 and 5 compute the percentage of households who have ever had non-zero values for the given category, while Column 6 computes the percentage difference between the the two.

Given that I have no explicit demographic information for each household, I must infer information from bank and credit card statements. In Table 3 I present a variety of statistics on income, spending, and other observed behaviors such as the receipt of unemployment benefits to compare the different groups of households.

Income is the sum of observed monthly income. *ATM and Check* is the sum of ATM withdrawals and checks written in a given month. *Consumption* is the sum of observed credit card and debit transactions during the month. *Credit Card Payments* is the sum of credit card payments during the month. *Mortgage*, *Car Payment*, *Misc. Bills*, and *Student Loans* are the sum of the respective categories. *Misc. Bills* contains transactions from merchants for which over 20% of transactions within a household are for the exact same amount, and contains payments to cell phone and television providers. *Bounced Checks* and *Overdrafts* are the count of observed bounced checks and overdrafts, respectively.

$I(Investment)$ is an indicator variable which takes the value of one during months with observed transfers to or from brokerage accounts. $I(Unemployment)$ and $I(Soc.\ Sec.)$ are indicator variables which takes the value of one during months where unemployment benefits or Social Security payments are observed, respectively. It should be noted that not all states distribute unemployment benefits through direct deposit, which will bias these values towards zero. $I(Casino)$ is an indicator variable which takes the value of one during months with observed ATM transactions from casinos.

Panel A analyzes how payday borrowers differ from the general population. Payday borrowers have an average monthly income of $3,975, which is 9% lower than the average monthly income of non-payday borrowers. Payday borrowers have average ATM and check monthly outflows of $1,372 per month, which is 26% lower than those of non-payday borrowers. I observe an average of $1,447 in monthly consumption from payday borrowers, which is 25% higher than the $1,159 observed consumption of non-payday borrowers. I find that payday borrowers make $737 in credit card payments per month, compared to $1,483 for non-payday borrowers, a difference of 50%. Payday borrowers pay an average of $310 per month in mortgages as compared to $569 per month for non-payday borrowers, a difference of 46%. Payday borrowers and non-payday borrowers have similar car payments of $155 and $147 per month, respectively. Likewise, payday borrowers and non-payday borrowers have similar student loan payments of $40 and $50 per month, respectively. Payday borrowers earn an average of $0.69 per month in bank interest, whereas non-payday borrowers earn an average of $2.79 per month, a difference of 75%. Payday borrowers bounce 0.12 checks per month, as compared to non-payday borrowers who bounce 0.02 checks per month, a difference of 494%. Payday borrowers incur 0.64 overdrafts per month, as compared to 0.19 overdrafts per month for non-payday borrowers, a difference of 240%. Payday borrowers invest 1.9% of months, while non-borrowers invest 2.5% of months, a difference of 24%. Payday borrowers receive unemployment benefits 1.7% of months, while non-borrowers receive unemployment benefits 1.1% of months, a difference of 63%. Payday borrowers receive social security payments 4.2% of months, while non-borrowers receive unemployment benefits 3.2% of months, a difference of 30%. Finally, I observe ATM withdrawals in casinos 1.5% of months for payday borrowers and 0.5% of months for non-borrowers, a difference of 181%.

Columns 4 though 6 report the percentage of households with non-zero values in each respective category. Payday borrowers are particularly different from non-borrowers in a several categories. Payday borrowers are 122% more likely to have ever bounced a check, 37% more likely to have ever overdrafted, 80% more likely to have ever received unemployment benefits, 44% more likely to have ever received social security benefits, and 67% more likely to have ever made an ATM withdrawal at a casino. Clearly, payday borrowers are substantially different from the general population.

Panel B compares pre-OCP values of treated and control households for the unrestricted sample. In contrast to Panel A, the two groups presented in Panel B are similar on many dimensions. However, they are different on a few dimensions. Treated households incur an average of 58% more bounced checks. Further, treated households are 33% more likely to have visited a casino. Finally, treated households borrow 40% more than control households and have 68% more borrowing relationships (2.05 versus 1.22).

Panel C compares pre-OCP values of treated and control households using the restricted sample of treated households living in states where payday lending is illegal and control households living in states where payday lending is legal. Similar to Panel B, the two groups presented in Panel C are similar on many dimensions. However, they are different on a few dimensions. Treated households incur an average of 67% more bounced checks. Further, treated households are 56% more likely to receive unemployment benefits and 31% less likely to have visited a casino in a given month. Finally, treated households borrow 35% more than control households and have 59% more borrowing relationships (1.97 versus 1.23).

Even though treated and control households are similar on many dimensions, I control for differences between them by using household fixed effects in every empirical specification.

5 Pooled Operation Choke Point Analysis

This section analyzes the average response to the restriction of credit. Section 5.1 performs the analysis on the unrestricted sample. Section 5.2 explores whether the observed effects are driven by windfall gains. Section 5.3 performs the analysis on the restricted sample. Section 5.4 explores the persistence of the response to the restriction of payday credit.

Since the purpose of the paper is to determine changes to household well-being following exogenous reductions in the availability of payday credit, one challenge is identifying relevant household well-being measures that are identifiable in my dataset. I identify four variables to allow for the evaluation of household well-being. The first is the frequency with which a household bounces checks. The second is the frequency with which a household experiences account overdrafts. The third is the level of household consumption, measured as the sum of observed debit card and credit card purchases. Finally, I calculate the volatility of consumption over six-month periods beginning in January and July of each year.

5.1 Average response to the restriction of payday credit

To understand the average responses to OCP, I begin with the specification shown in Equation (1):

$$\begin{aligned} Y_{h,t} = {} & \beta_1 \, Treated * MA1_{h,t} + \beta_2 \, Treated * MA2_{h,t} + \beta_3 \, Treated * MA3_{h,t} \\ & + \beta_4 \, Treated * QA2\_6_{h,t} + \beta_5 \, Income_{h,t} + \beta_6 \, Income_{h,t-1} \\ & + FE_t + FE_h + \epsilon_{h,t}. \end{aligned} \tag{1}$$

The data is collapsed by household month. $Y_{h,t}$ is the dependent variable of interest, with subscripts h indicating household and t indicting time (in terms of month). The dependent variables I analyze are the dollar amount of online payday borrowing ($Payday\ Borrow_{h,t}$), the dollar amount of online payday repayment ($Payday\ Repay_{h,t}$), the number of days a household is in financial distress ($Financial\ Distress_{h,t}$), and total household consumption ($Consumption_{h,t}$). $Treated * MAZ_{h,t}$ is an interaction term of $Treated_h$ and MAZ_t. Likewise, $Treated * QA2\_6_{h,t}$ is an interaction term of $Treated_h$ and $QA2\_6_t$. $Treated_h$ is an indicator that takes the value of 1 when household has a pre-existing relationship with a lender that is shutdown during OCP. MAZ_t is an indicator that takes the value of 1 the Z^{th} month after treatment. $QA2\_6_t$ is an indicator that takes the value of 1 during the second through sixth quarters after treatment. $Treated_h$, MAZ_t, and $QA2\_6_t$ are collinear with household and date fixed effects and are dropped from the regression. $Income_{h,t}$ is household income in dollars and $Income_{h,t-1}$ is lagged household income in dollars. FE_t and FE_h represent time and household fixed effects, respectively. I cluster standard errors by household.[9]

The regression results are found in Panel A of Table 4. Column (1) formally confirms what was illustrated simply in Panel A of Figure 3. Treated households initially reduce total payday borrowing by \$73 (corresponding to a 29% reduction relative to the pre-treatment mean of treated households) during the month after treatment, and this number grows to a reduction of \$109 (corresponding to a 43% reduction relative to the pre-treatment mean of treated households) per month during the second through sixth quarters after treatment. The coefficient is highly statistically and economically significant. Clearly, OCP was effective in changing household borrowing behavior.

Columns (2) and (3) evaluate whether the frequency of financial distress changes as a result of restricted payday credit, as proxied by the number of bounced checks and over-

[9]Similar results are obtained if standard errors are clustered by time, or by household and time.

drafts incurred in a given month, respectively. Column (2) shows that treated households initially reduce the number of bounced checks incurred by 0.02, corresponding to an 8% reduction relative to the pre-treatment mean of treated households, and this number grows in magnitude to a reduction of 0.04, corresponding to a 18% reduction. Column (3) likewise shows that treated households reduce the long-term number of overdrafts incurred by 0.04, corresponding to a 4% reduction relative to the pre-treatment mean of treated households.

Finally, Columns (4) and (5) investigate the effect of the restricted payday access on consumption and consumption volatility, respectively. Column (4) shows that treated households increase monthly consumption by $38 in quarters 2 through 6 after treatment, corresponding to a 3% increase relative to the pre-treatment mean of treated households. Column (5) shows that treated households experience no significant change in consumption volatility following restrictions in payday credit.

Since these specifications have household and time fixed effects, the resultant income should be interpreted as abnormal income. For example, if a household has constant income every month, the household fixed effect would simply transform the income stream to zeros. Payday borrowing is positively correlated with household income. Though surprising, this is consistent with the fact that proof of income is required for loans. Following a job loss, for example, a household would have greater difficulty qualifying for loans and thus the coefficient on income and lagged income would be positive in the regressions. Income is positively correlated with bounced checks and overdrafts, but economically trivial. Both lagged and current income are positively correlated with consumption. Consumption will increase (decrease) by $10 for every $100 increase (decrease) in current income for households in my sample. Likewise, consumption will increase (decrease) by $6 for every $100 increase (decrease) in lagged income for households in my sample. Finally, consumption volatility is increasing in both income and lagged income, which is consistent with households increasing (decreasing) consumption following increases (decreases) in abnormal income.

The results are robust to the exclusion of income controls in my regressions. However, since consumption is highly correlated with income, I am able to achieve more precise estimates when controlling for income.

5.2 Are the results driven by windfall gains?

Due to the abrupt closure of lenders effected by Operation Choke Point, a small percent of households in my sample ended up receiving windfall gains. Such households borrowed immediately before a particular lender was shut down and therefore did not repay the loan. Panel A of Table 4 provides evidence of positive outcomes following the closure of payday lenders, and a natural question is whether these positive outcomes are driven by windfall gains rather than restricted access to payday credit.

To explore this potential explanation, I replicate the pooled results of Panel A of Table 4 after excluding households that borrowed in July of 2013, approximately one month before OCP was implemented. The rationale is that households who happened to have borrowed in July of 2013 may potentially have benefited from windfall gains through the closure of lenders. 29% of households meet this criteria. With these households excluded, I rerun the analysis of Section 5. The results are presented in Panel B of Table 4.

The results of Panel B of Table 4 are substantially equivalent to those of Panel A Table 4, both in terms of economic magnitude and statistical significance. The results of Panel B support the conclusion that the restriction of credit which is driving the results rather than windfall gains received by a subset of the population.

5.3 Restricted Subsample

In the remainder of this section, I restrict treated households to those living in states where payday lending is illegal and control households to those living in states where payday lending is legal. For such households, the treatment effect is large due to the inability to substitute to other lenders.

Regression results are found in Panel C of Table 4. Column (1) shows that treated households initially reduce total payday borrowing by \$103 (corresponding to a 41% reduction relative to the pre-treatment mean of treated households) during the month after treatment, and this number grows to a reduction of \$137 (corresponding to a 55% reduction relative to the pre-treatment mean of treated households) per month during the second through sixth quarters after treatment. The coefficient is highly statistically and economically significant. As expected, the treatment effect is larger for the restricted sample in Panel C than the original sample in Panel A.

Columns (2) and (3) evaluate whether the frequency of financial distress changes as a result of restricted payday credit, as proxied by the number of bounced checks and overdrafts incurred in a given month, respectively. Column (2) shows that treated households temporarily increase the number of bounced checks during the third month after treatment by 0.03, corresponding to an increase of 15% relative to the pre-treatment mean. However, the long-term response for treated households is a 0.04 reduction in bounced checks, corresponding to a 17% reduction relative to the pre-treatment mean. Column (3) shows that there is no change in the number of overdrafts incurred by treated households.

Finally, Columns (4) and (5) investigate the effect of the restricted payday access on consumption and consumption volatility, respectively. Column (4) shows that treated households decrease consumption by \$17 during the first month after treatment, though this is not statistically significant. In quarters 2 through 6 after treatment, treated households increase consumption by \$31 per month, corresponding to a 3% increase relative to the pre-treatment mean of treated households. Column (5) shows that treated households experience no significant change in consumption volatility following restrictions in payday credit.

Overall, Table 4 illustrates that Operation Choke Point dramatically reduced the amount of payday borrowing for treated households. The reduction in borrowing is more pronounced for the treated households in the restricted subsample in Panel C. Treated households in this restricted subsample reduce long-term overdrafts and increase long-term consumption and exhibit no change in consumption volatility. Panel B illustrates that these results are not driven by windfall gains of treated households.

5.4 Persistence of the response to the restriction of payday credit

A natural follow-on question is whether the observed treatment effects observed in Sections 5.1 though 5.3 are persistent or only temporary. To answer this question, I introduce the following specification that allows for a comparison of short- versus long-term responses:

$$\begin{aligned} Y_{h,t} = {} & \beta_1\ Treated * MA1_{h,t} + \beta_2\ Treated * MA2_{h,t} + \beta_3\ Treated * MA3_{h,t} \\ & + \beta_4\ Treated * QA2_{h,t} + \beta_5\ Treated * QA3_{h,t} + \beta_6\ Treated * QA4_{h,t} \\ & + \beta_7\ Treated * QA5_{h,t} + \beta_8\ Treated * QA6_{h,t} + \beta_9\ Income_{h,t} \\ & + \beta_{10}\ Income_{h,t-1} + FE_t + FE_h + \epsilon_{h,t}. \end{aligned} \tag{2}$$

The only difference between this specification and the one described previously is the fact that the $Treated * QA2\_6_{h,t}$ variable is broken up into quarters after transition. I define quarters as 3-month periods after the treatment effect. To illustrate, consider a household who had a pre-existing relationship with a lender who was closed during OCP on August 1, 2013. This household would be assigned the value of 1 for $Treated * QA2_{h,t}$ for the months of December 2013, January 2014, and February 2014 and 0 otherwise.

As in Panel C of Table 4, I restrict the sample to treated households residing in states where payday lending is illegal and control households residing in states where payday lending is legal. The regression results are found in Table 5. Column (1) shows that treated households initially reduce borrowing by $103 per month during the first quarter after treatment, and this number monotonically grows in magnitude to a reduction of $152 per month in the sixth quarter after treatment. The coefficients are highly statistically and economically significant. This column clearly shows that the treatment effect was persistent.

Columns (2) and (3) evaluate whether the frequency of financial distress changes as a result of restricted payday credit, as proxied by the number of bounced checks and overdrafts incurred in a given month, respectively. Column (2) shows that treated households increase the number of bounced checks per month by 0.03 during the third month after treatment, corresponding to a 16% increase relative to the pre-treatment mean of treated households. However, the number of bounced checks decreases in quarters two through six after treatment. The reduction begins in quarter two a 0.02 (corresponding to a 11% reduction relative to the pre-treatment mean of treated households), reaches a peak reduction in the fourth quarter after treatment of 0.04 (corresponding to a 21% reduction relative to the pre-treatment mean of treated households). Column (3) shows that treated households experience no change in the number of overdrafts over the observation window.

Finally, Columns (4) and (5) investigate the effect of the restricted payday access on consumption and consumption volatility, respectively. Column (4) shows that treated households experience no statistically significant change in consumption until the fourth quarter after treatment. During the fourth quarter after treatment, treated households increase consumption by $43 per month (corresponding to a 4% increase relative to the pre-treatment mean of treated households), but this number reaches a peak value of $74 during the sixth quarter after treatment (corresponding to a 7% increase relative to the pre-treatment mean of treated households). Column (5) shows that the restriction of payday credit did not change the volatility of consumption in any quarter after treatment.

Table 5 shows that the treatment effect from Operation Choke Point was large and persistent. Further, Table 5 reveals that household responses of bounced checks, and consumption were likewise persistent. Given the persistence of the responses, subsequent tables will utilize the specifications utilized in Section 5.1.

Overall, the pooled results are difficult to reconcile with standard neoclassical models of human behavior, which would predict that restrictions in credit would weakly worsen household outcomes. Rather, the pooled results are consistent with behavioral models. In the following section I exploit cross-sectional variation to better understand the underlying mechanisms at play.

6 Cross-Sectional Operation Choke Point Analysis

To date, few papers have explored how heterogeneity in household characteristics matter in this setting. Carrell and Zinman (2014) find that negative outcomes associated with payday loan access are concentrated among inexperienced and unsophisticated airmen, while Dobrige (2014) finds that borrowers who borrow in "bad" states of the world, such as hurricanes and blizzards, exhibit positive outcomes of consumption smoothing.

Given the richness of the dataset, I am able to explore how heterogeneity in household characteristics influences the response to restrictions in payday credit. I explore heterogeneity in pre-treatment borrowing behavior in Section 6.1 and heterogeneity in income in Section 6.2.

In each of these subsections, I utilize the specification set forth in Equation (1) in Section 5.1. In order to maximize the power of the cross-sectional results, I use the unrestricted sample used in Panel A of Table 4. The cross-sectional results hold for the restricted subsample, but are weakened due to the reduced sample size.

6.1 Do heavy borrowers respond differently than light borrowers to the restriction of payday credit?

In this section, I test whether the heaviest payday borrowers respond differently to the restriction of payday credit than those who use payday loans more sparingly. If heavy payday loan users are simply unlucky and have had a series of unfortunate events, then the restriction of payday credit may harm these households. On the other hand, if heavy payday loan users are more prone to borrowing unnecessarily, then the restriction of payday credit may disproportionately benefit these households.

I proceed by dividing the sample into two groups based on the count of payday loans transactions in the six-month period from January 2013 to June 2013. I refer to households above the median amount borrowed as "heavy borrowers" and those below the median amount borrowed as "light borrowers." I use the specification outlined in Equation (3) to understand how each group responds. Results for the subsample of heavy borrowers is found in Panel A of Table 6 while the results for the subsample of light borrowers is found in Panel B of Table 6.

Consider first the response of heavy borrowers in Panel A of Table 6. Column (1) shows that treated households, relative to control households, dramatically reduce payday borrowing. The reduction in borrowing begins at \$94 per month (corresponding to a 27% reduction from the pre-treatment mean) in the first month after treatment and ends at \$138 per month in quarters two through six after treatment (corresponding to a 39% reduction from the pre-treatment mean). The results are highly statistically significant throughout the observation window.

Columns (2) and (3) evaluate whether the frequency of financial distress of heavy borrowers changes as a result of restricted payday credit, as proxied by the number of bounced checks and overdrafts incurred in a given month, respectively. Column (2) shows that treated households reduce the number of bounced checks by 0.02 per month during the first month after treatment, corresponding to an 10% reduction from the pre-treatment mean. In quarters two through six after treatment the treated households reduce the number of bounced checks by 0.05 per month, corresponding to a 20% reduction from the pre-treatment mean. Column (3) shows evidence of a reduction in the number of overdrafts of treated households in quarters two through six after treatment, though it is only marginally significant.

Finally, Columns (4) and (5) investigate the effect of the restricted payday access on consumption and consumption volatility of heavy borrowers, respectively. Column (4) shows that treated households initially experience no change in consumption during the three months after treatment, though in quarters two through six treated households increase consumption by \$54 per month (corresponding to a 4% increase relative to the pre-treatment mean of treated households). Column (5) shows that treated households experience no significant change in consumption volatility following restrictions in payday credit.

I next evaluate the response of light borrowers in Panel B of Table 6. Column (1) shows that treated households, relative to control households, reduce payday borrowing. The reduction in borrowing begins at \$32 per month (corresponding to a 26% reduction from the pre-treatment mean) in the first month after treatment and ends at \$42 per month for quarters two through six after treatment (corresponding to a 34% reduction from the pre-treatment mean). The results are highly statistically significant throughout the observation window.

Columns (2) and (3) evaluate whether the frequency of financial distress of light borrowers changes as a result of restricted payday credit, as proxied by the number of bounced checks and overdrafts incurred in a given month, respectively. Columns (2) and (3) show that treated households experience no short- or long-term change in the number of bounced checks or overdrafts, respectively.

Finally, Columns (4) and (5) investigate the effect of the restricted payday access on consumption and consumption volatility of light borrowers, respectively. Column (4) shows that treated households experience no short- or long-term change in the level of consumption. Column (5) likewise shows that treated households experience a very short-lived reduction in consumption volatility during the first month after treatment.

I proceed by testing for differences in coefficients across the two panels. Heavy borrowers reduce payday borrowing more than light borrowers throughout the sample period, and this difference is significant at the 1% level. Further, heavy borrowers reduce bounced checks more than light borrowers in quarters 2 through 6 after treatment, and this difference is significant at the 1% level. Finally, heavy borrowers increase consumption more than light borrowers in quarters 2 through 6 after treatment, and this difference is significant at the 1% level. Overall, the results of Table 6 paint a clear picture that most of the positive outcomes following restrictions of payday credit accrue to the heaviest borrowers.

6.2 Do high-income households respond differently than low-income households to the restriction of payday credit?

Next, I proceed by asking how income effects household responses to the restriction of payday credit. On the one hand, payday loans may be more useful to lower-income households since lower-income households are more likely to be financially constrained. On the other hand, payday loans may be more harmful to low-income households since low-income households are less able to afford the interest incurred through payday borrowing if they are borrowing excessively.

I proceed by dividing the sample into two groups based on the sum of observed income in the six-month period from January 2013 to June 2013. I refer to households above the median income as "high-income" and those below the median income as "low-income." I use the specification outlined in Equation (3) to understand how each group responds. Results for the subsample of high-income borrowers is found in Panel A of Table 7 while the results for the subsample of low-income borrowers is found in Panel B of Table 7.

Consider first the response of high-income borrowers in Panel A of Table 7. Column (1) shows that treated households, relative to control households, dramatically reduce payday borrowing. The reduction in borrowing begins at \$92 per month (corresponding to a 31% reduction from the pre-treatment mean) in the first quarter after treatment and ends at \$137 per month for the remaining quarters after treatment (corresponding to a 46% reduction from the pre-treatment mean). The results are highly statistically significant throughout the observation window.

Columns (2) and (3) evaluate whether the frequency of financial distress of high-income borrowers changes as a result of restricted payday credit, as proxied by the number of bounced checks and overdrafts incurred in a given month, respectively. Column (2) shows that treated households reduce the number of bounced checks by 0.04 per month in quarters two through six after treatment (corresponding to a 17% reduction from the pre-treatment mean). Likewise, Column (3) shows that high-income households reduce the number of overdrafts by 0.06 per month (corresponding to a 6% reduction from the pre-treatment mean).

Finally, Columns (4) and (5) investigate the effect of the restricted payday access on consumption and consumption volatility of high-income borrowers, respectively. Column (4) shows that treated household experience an increase in consumption of \$33 per month (corresponding to a 2% increase from the pre-treatment). Column (5) shows that treated households experience no change in consumption volatility following restrictions in payday credit.

I next evaluate the response of low-income borrowers in Panel B of Table 7. Column (1) shows that treated households, relative to control households, reduce payday borrowing. The reduction in borrowing begins at \$51 per month (corresponding to a 26% reduction from the pre-treatment mean) in the first month after treatment and ends at \$73 per month during quarters two through six after treatment (corresponding to a 37% reduction from the pre-treatment mean). The results are highly statistically significant throughout the observation window.

Columns (2) and (3) evaluate whether the frequency of financial distress of low-income borrowers changes as a result of restricted payday credit, as proxied by the number of bounced checks and overdrafts incurred in a given month, respectively. Column (2) shows that treated households experience short-term reductions in the number of bounced checks beginning the first month after treatment. Eventually, this reduction grows to 0.03 fewer bounced checks per month, corresponding to an 18% reduction from the pre-treatment mean. Column (3) shows that treated households experience no short- or long-term change in the number of overdrafts.

Finally, Columns (4) and (5) investigate the effect of the restricted payday access on consumption and consumption volatility of low-income borrowers, respectively. Column (4) shows that treated households experience an increase in consumption of \$32 beginning the third month after treatment (corresponding to a 4% increase from the pre-treatment mean) and growing to \$39 in quarters two through six after treatment (corresponding to a 5% increase from the pre-treatment mean). Column (5) shows that treated households experience no change in consumption volatility following restrictions in payday credit.

I proceed by testing for differences in coefficients across the two panels. High-income borrowers reduce payday borrowing more than low-income borrowers throughout the sample period, and this difference is significant at the 1% level. However, none of the remaining coefficients are statistically different across the two panels.

Overall, the results of Table 7 suggest that benefits of restricted access to payday credit accrue to both high- and low-income households, though the difference between these groups is negligible.

7 What types of purchases payday loans are financing?

Given the high rate of interest on payday loans, households should only borrow for extraordinary circumstances. Recent survey evidence suggests that this might not be the case (Pew (2015)). Rather, payday loans might be financing regular purchases such as groceries and rent rather than unexpected purchases such as car repairs and hospital bills. In this section, I empirically analyze what types of purchases payday loans are financing.

I employ the following specification to calculate abnormal spending surrounding borrowing:

$$Y_{h,t} = \sum_{Z=-10}^{11} \beta_Z \; Treated * WAZ_{h,t} + \beta_{22} \; Income_{h,t} + \beta_{23} \; Income_{h,t-1} \\ + FE_t + FE_h + \epsilon_{h,t}. \tag{3}$$

Due to the short maturity of these loans, I collapse the data by household week in this section. The indicator variable WAZ takes the value of 1 during the Z^{th} week after borrowing and 0 otherwise. For example, the week before borrowing, the indicator variable $WA\ -1$ would take the value of 1, and the week after borrowing the indicator variable WA 1 would take the value of 1. WA 10 takes the value of 1 for any week greater than 10 weeks after payday borrowing. The omitted dummy is any week 11 or more weeks before borrowing, so coefficients should be interpreted as abnormal spending relative to this period. As with the previous specifications, I include household and time fixed effects. Standard errors are clustered by household and time.

In this section, I analyze a more precise set of spending categories than in previous sections. *Payday Repay* is the dollar amount spent on repaying payday loans in a given week. *Cash* is the observed cash withdrawals from banks and ATMs. *Recurring Consumption* is the dollar amount of credit and debit card spending spent on recurring merchants, while

Nonrecurring Consumption is the dollar amount of credit and debit card spending spent on nonrecurring merchants. A merchant is classified as recurring (nonrecurring) if more (less) than 20% of the household's purchases at the merchant occur in adjacent months. Retailers, grocers, gas stations, and restaurants are the types of merchants which end up in the recurring consumption category, whereas transactions such as airline tickets and automotive repairs end up in the nonrecurring consumption category. *Insurance* is the sum of observed insurance payments. *Nonrecurring Check* and *Recurring Check* are nonrecurring and recurring check payments, respectively. Recurring check payments are checks repeated within a household at a regular interval for the exact amount, such as a series of $1,000 rent payments. *Credit Card Payment* are the sum of payments to credit cards. *Mortgage* is the sum of mortgage payments. *Misc. Bills* contains transactions for which over 20% of transactions within a household are for the exact same amount, and contains payments to cell phone and television providers. *Car Payments* are payments of car loans. *Casino* is the dollar amount of ATM withdrawals from casinos.

Regression results are presented in Table 8. For brevity, I suppress any coefficient outside of the 8-week window surrounding borrowing as well as the income controls. With the exception of Column (1), the remainder of the columns are sequenced in descending order based on the magnitude of the coefficient during the week of borrowing. Column (1) shows the timing of payday repayments as a function of weeks after borrowing. Payday repayments reach a peak 2 weeks after borrowing at $68 per week, though repayments remain positive and significant for subsequent weeks after borrowing. Interestingly, payday repayments of $36 per week are made the week during borrowing (0 weeks after borrow), which could reflect one of a few scenarios. The loan could be rolled over (taken out after repaying the loan), or the household could be borrowing from one lender to repay another, or finally, the loan could have been repaid within a week of borrowing. Column (2) shows that cash withdrawals is the category with the highest abnormal activity during the week of borrowing, with an increase in cash withdrawals of \$43 per week during the week of payday borrowing. Column (3) shows an increase of \$29 in the category of recurring consumption during the week of payday borrowing. Column (4) shows abnormal expenditures on insurance spending peaks at \$20 during the week of borrowing. Nonrecurring and recurring checks follow in Columns (5) and (6) with peak abnormal spending of \$18 and \$16, respectively. Column (7) follows and shows a \$13 increase in credit card payments the week of borrowing. Column (8) shows that mortgage payments increase by \$10 during the week of borrowing. Column (9) shows that spending in the miscellaneous bills category goes up by \$9 during the week of borrowing. Column (10) shows that spending in the category of nonrecurring consumption increases by \$9. Column (11) shows that car payments, not to be confused with automotive repairs which would show up in the category of nonrecurring consumption, increase by \$4 during the week of borrowing. Finally, Column (12) shows that ATM withdrawals at casinos increase by \$1 during the week of borrowing. Interestingly, this category is the only category observed for which abnormal spending consistently increases for several weeks before borrowing.

I do not include income as a dependent variable in this table since it is a control variable in these regressions. Nonetheless, when I run this analysis with income as a dependent variable and omit the income controls as independent variables, I find no negative income shocks surrounding payday borrowing.

Overall, the results of Table 8 provide a surprising view of how these loans are used. Whereas these products are marketed as products to help households to cover unexpected expenses, about half of the spending occurs in categories which are fully predictable.

8 Conclusion

Whether access to payday loans is "good" or "bad" is a question that will continue to be debated by policymakers, individuals, and lenders for years to come. Further, there is no single policy which can be implemented that will not hurt someone. If payday loans are banned, this will harm those borrowers with unexpected expenses which require short term financing before the next paycheck. If payday loans are allowed, households who lack discipline or financial sophistication may make unwise borrowing decisions.

In this paper I find, on average, positive outcomes surrounding restrictions in payday credit in terms of reductions in financial distress and an increase in consumption. However, these results are driven primarily from heavy borrowers in the sample. A deeper analysis on household spending surrounding payday borrowing reveals substantial heterogeneity in how payday loans are used. About half of abnormal spending the week of payday borrowing is spent on predictable categories such as mortgages, car loans, and insurance. Surprisingly, I find evidence of abnormal gambling activity immediately preceding and following payday borrowing.

In general, many of the results are difficult to reconcile with standard neoclassical models of human behavior. A fruitful area for future research would be to investigate why so many households do not have emergency (or non-emergency) savings as models of precautionary savings would suggest.

Figure 1: This figure shows how Operation Choke Point affected payday lending activity. Panel A shows the monthly count of loans issued for three affected lenders, while B shows aggregate count of loans issued across all lenders.

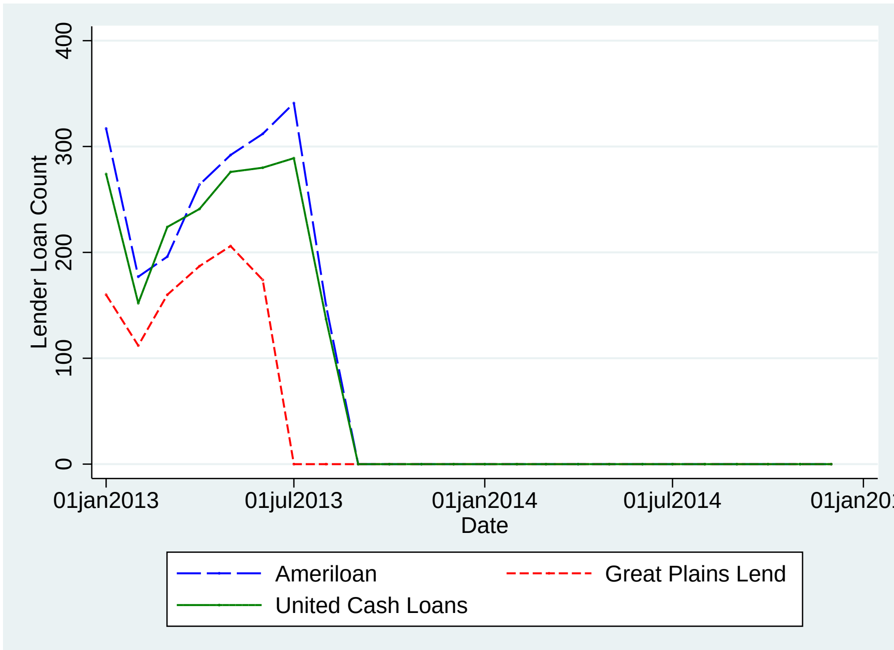

Figure 1: Panel A

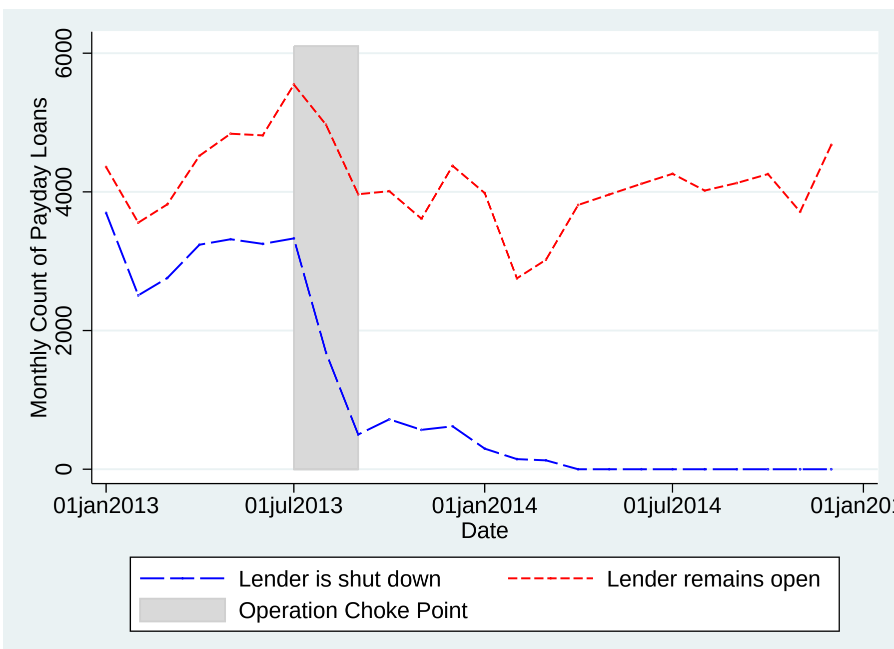

Figure 1: Panel B

Figure 2: This figure compares histograms of payday activity between lenders who remain open through Operation Choke Point and lenders who are shut down. Panel A shows differences in the dollar amount of loans issued while Panel B shows differences in the dollar amount of payments collected.

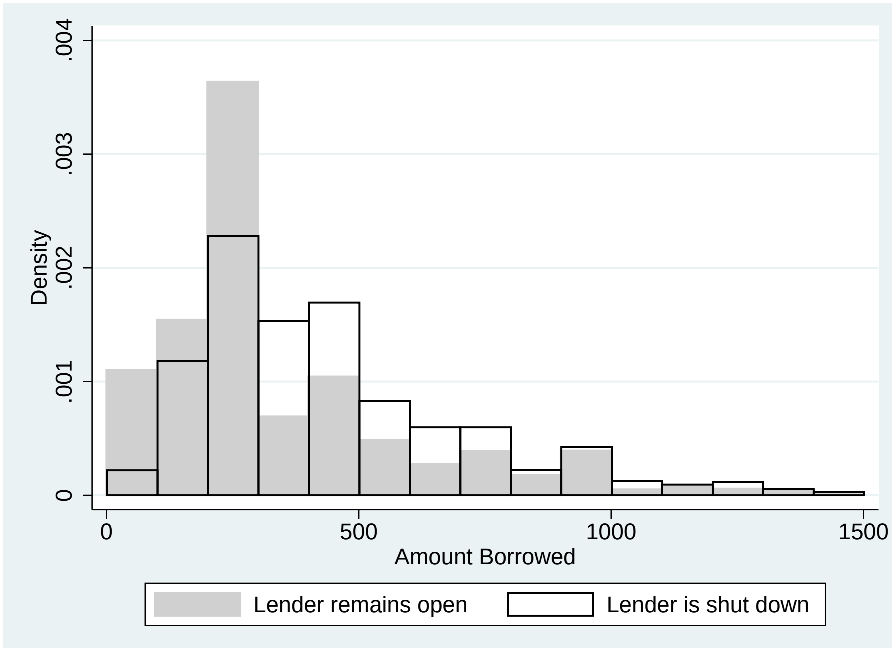

Figure 2: Panel A

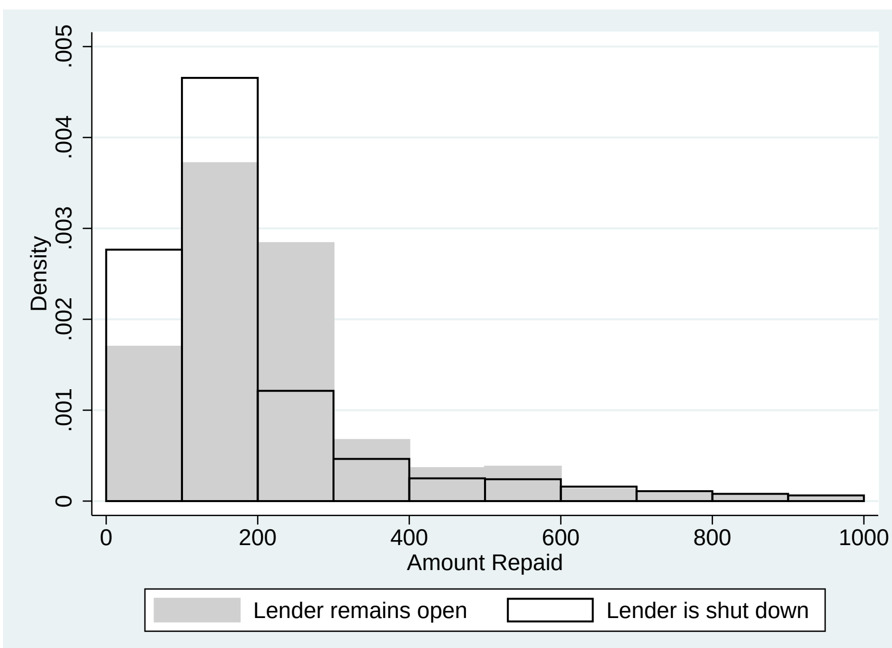

Figure 2: Panel B

Figure 3: This figure illustrates the treatment effect by plotting average monthly household-level payday borrowing for both treatment and control households. Treated households are those with pre-existing relationships with lenders that are shut down, while control households are those with pre-existing relationships with lenders that are not shut down. Panel A presents plots for the entire sample, whereas Panel B restricts treated households to those living in states where payday lending is illegal and control households to households living in states where payday lending is legal.

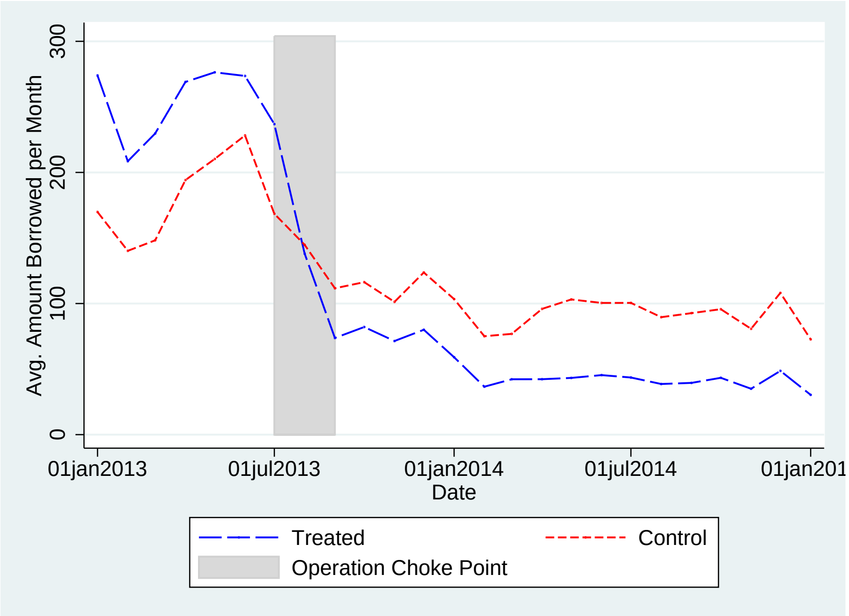

Figure 3: Panel A

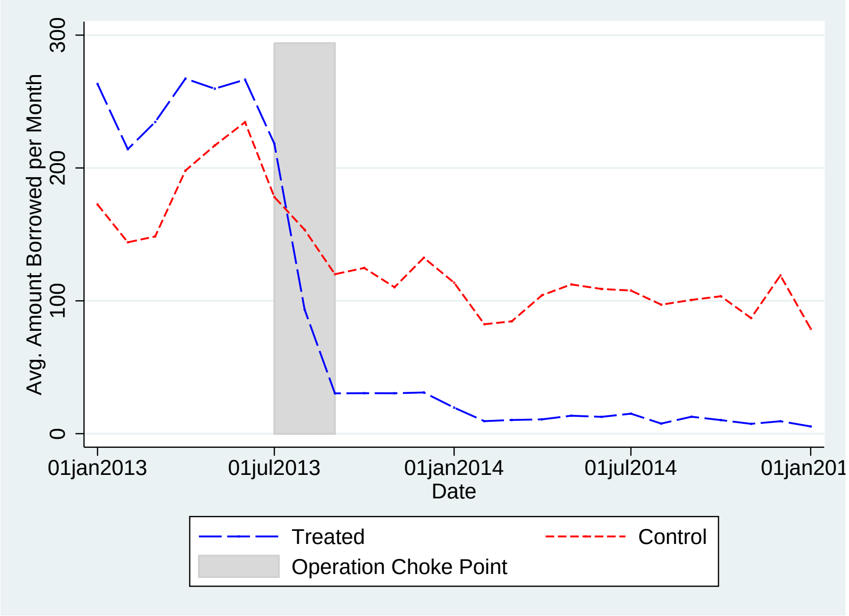

Figure 3: Panel B

Table 1: Summary of payday lenders in sample. The second column contains the number of observed pre-OCP transactions over the six-month period prior to OCP, from January 2013 to June 2013. The third column lists the date of lender shutdown as identified in the data as when the lender stopped lending. The table is sorted in descending order by transaction count in Column (2).

Lender Name	Pre-OCP Transaction Count	Date of Shutdown
CashnetUSA	41,472	
Plain Green	27,176	
Mobiloans	18,911	
Ameriloan	16,183	15-Aug-13
United Cash Loans	15,088	14-Aug-13
Mycashnow	13,214	12-Aug-13
Oneclickcash	11,884	12-Aug-13
Fastcash	10,633	16-Aug-13
Zip Com	10,150	6-Jan-14
Ace Cash Express	9,653	
Greatplainslend	9,560	25-Jun-13
Paydaymax	8,519	9-Aug-13
Castlepayday	7,521	
Cash Central	6,947	
Cash Jar	6,062	2-Aug-13
Viploanshp	6,040	15-Oct-13
Hydra Fund	5,743	2-Aug-13
Bdpdlservices	4,656	15-Oct-13
Americanwebloan	4,086	15-Oct-13
Pdo	3,240	13-Jun-13
Golden Valley	2,795	
Silvercloud Fin	2,710	27-Jul-13
Spot On Loans	2,657	7-Jun-13
Starcashprcssng	2,092	26-Jun-13
Spotloan	1,939	
Actionpdl	1,923	15-Oct-13
Magnum Cash	1,816	3-Aug-13
Vip Cash	1,757	
Cash In A Wink	1,601	30-Aug-13
Fifththird	1,597	
Integrity	1,448	15-Oct-13
Lendingbooth	1,437	29-Aug-13
Nxtdaycash	1,369	16-Oct-13
Fast Efunds	1,228	8-Jul-14
Fedfinsvcs	1,206	30-Oct-13
Dollar Premier	1,185	
Netcredit	1,023	
Liquidcash	927	8-Oct-13
Regions	278	

Table 2: This table compares lenders who remain open through Operation Choke Point to lenders who are shut down. The first two columns contain summary statistics on the loans issued and the payments collected from lenders who remain open, respectively. The last two columns contain summary statistics for lenders who are shut down.

	Lender Remains Open		Lender is Shut Down	
	Loan Issued	Payment Collected	Loan Issued	Payment Collected
Count	97,720	304,115	23,916	141,642
Average	\$478	\$232	\$546	\$182
P25	\$200	\$114	\$300	\$60
P50	\$255	\$159	\$500	\$120
P75	\$500	\$282	\$700	\$200
Count Payments / Total Count	0.76		0.86	

Table 3: This table provides summary statistics for households in the sample. Panel A contains summary statistics for households that use payday loans (Denoted "PD") and those households who do not (Denoted "Non PD"). Panel B compares treated and control payday users. The unit of observation is the household month. The first three columns summarize the (unconditional) average monthly values for the given variable. The next three columns summarize the percentage of months that the given the variable is observed in a given month. *Income* is the sum of observed monthly income. *Consumption* is the sum of observed credit card and debit transactions during the month. *Credit Card Pay.* is the sum of credit card payments during the month. *Student Loan* and *Mortgage* are the sum of student loan and mortgage payments, respectively. # *Bounced Checks* and *Overdrafts* are the count of observed bounced checks and overdrafts, respectively. $I(Investment)$ is an indicator variable which takes the value of one during months with observed transfers to or from brokerage accounts. $I(Unemployment)$ and $I(Soc\,Sec.)$ are an indicator variables which takes the value of one during months where unemployment benefits or Social Security payments are observed, respectively. $I(Casino)$ is an indicator variable which takes the value of one during months with observed ATM transactions from Casinos. Panel B contains the additional variables of *Payday Borrow* and # *Pre OCP Rel.*, which represent the sum of monthly payday loans borrowed and the number of pre-OCP lending relationships, respectively. Panel A contains summary statistics from 2011-2015, whereas Panels B and C contain summary statistics from Jan. 2013 to Jun. 2013, the six-month period before OCP.

Table 3: Panel A - Payday vs. Non-Payday Population.

	Average			% Ever Observed		
	PD	Non PD	% Diff	PD	Non PD	% Diff
Income	$3,975	$4,361	-8.8%	99.9%	95.2%	4.9%
ATM and Check	$1,372	$1,852	-25.9%	99.8%	96.2%	3.8%
Consumption	$1,447	$1,159	24.9%	100.0%	98.5%	1.5%
Credit Card Pay.	$737	$1,483	-50.3%	98.7%	95.2%	3.7%
Mortgage	$310	$569	-45.5%	39.1%	46.5%	-15.9%
Car Payment	$155	$147	5.2%	66.9%	52.5%	27.3%
Misc. Bills	$281	$192	45.9%	100.0%	98.4%	1.6%
Student Loan	$40	$50	-19.9%	50.8%	39.5%	28.8%
Interest Earned	$0.69	$2.79	-75.3%	81.6%	84.9%	-3.9%
# Bounced Checks	0.12	0.02	494.2%	50.5%	22.8%	121.8%
# Overdrafts	0.64	0.19	240.0%	81.7%	59.5%	37.4%
I(Investment)	1.9%	2.5%	-23.5%	11.9%	12.2%	-2.6%
I(Unemployment)	1.7%	1.1%	63.0%	12.3%	6.8%	79.9%
I(Soc. Sec.)	4.2%	3.2%	30.3%	7.5%	5.2%	43.9%
I(Casino)	1.5%	0.5%	180.6%	18.1%	10.8%	67.0%

Table 3: Panel B - Treated vs. Control Households.

	Average			% Ever Observed		
	Treated	Control	% Diff	Treated	Control	% Diff
Income	$4,535	$4,207	7.8%	100.0%	100.0%	0.0%
ATM and Check	$1,508	$1,299	16.1%	99.9%	99.9%	0.1%
Consumption	$1,503	$1,560	-3.7%	100.0%	100.0%	0.0%
Credit Card Pay.	$688	$713	-3.5%	99.3%	99.6%	-0.2%
Mortgage	$292	$280	4.3%	39.3%	38.6%	1.8%
Car Payment	$167	$173	-3.3%	68.2%	70.8%	-3.7%
Misc. Bills	$359	$335	7.2%	100.0%	100.0%	0.0%
Student Loan	$44	$42	3.9%	55.5%	53.9%	3.0%
Interest Earned	$0.43	$0.48	-10.4%	80.5%	83.1%	-3.2%
# Bounced Checks	0.20	0.13	57.5%	56.6%	49.4%	14.7%
# Overdrafts	0.95	0.88	7.6%	82.1%	85.5%	-4.0%
I(Investment)	2.4%	1.9%	24.7%	12.4%	12.2%	1.1%
I(Unemployment)	1.4%	1.6%	-15.4%	12.6%	12.9%	-2.7%
I(Soc. Sec.)	5.5%	4.8%	13.6%	8.8%	8.3%	5.6%
I(Casino)	2.6%	2.0%	33.4%	18.6%	19.1%	-2.6%
Payday Borrow	$253	$180	40.4%			
# Pre-OCP Rel.	2.05	1.22	67.8%			

Table 3: Panel C - Treated vs. Control Households (Restricted Sample).

	Average			% Ever Observed		
	Treated	Control	% Diff	Treated	Control	% Diff
Income	$3,779	$4,299	-12.1%	99.9%	100.0%	0.0%
ATM and Check	$1,517	$1,300	16.7%	100.0%	99.9%	0.1%
Consumption	$1,149	$1,605	-28.4%	100.0%	100.0%	0.0%
Credit Card Pay.	$510	$748	-31.8%	99.3%	99.5%	-0.2%
Mortgage	$242	$284	-14.9%	34.6%	39.0%	-11.3%
Car Payment	$134	$178	-24.8%	57.6%	71.7%	-19.7%
Misc. Bills	$319	$341	-6.4%	100.0%	100.0%	0.0%
Student Loan	$40	$43	-6.3%	50.7%	53.9%	-5.9%
Interest Earned	$0.22	$0.52	-57.7%	75.2%	84.0%	-10.5%
# Bounced Checks	0.21	0.13	66.7%	59.4%	49.6%	19.7%
# Overdrafts	0.71	0.88	-20.0%	74.4%	85.7%	-13.2%
I(Investment)	1.8%	2.1%	-15.9%	9.6%	12.8%	-24.5%
I(Unemployment)	2.1%	1.4%	55.6%	16.0%	12.1%	32.7%
I(Soc. Sec.)	4.2%	4.9%	-13.5%	6.6%	8.4%	-21.3%
I(Casino)	1.5%	2.1%	-31.3%	11.9%	20.0%	-40.5%
Payday Borrow	$249	$184	35.1%			
# Pre-OCP Rel.	1.97	1.23	59.3%			

Table 4: This table explores household outcomes following Operation Choke Point. Panel A performes the analysis on the entire sample. In order to rule out potential windfall effects, Panel B excludes those who borrowed in July of 2013, approximately one month before OCP was implemented. Panel C restricts the sample to treated households living in states where payday lending is illegal and control households to those living in states where payday lending is legal. The regression specification is: $Y_{h,t} = \beta_1\ Treated * After_{h,t} + \beta_2\ Income_{h,t} + \beta_3\ Income_{h,t-1} + FE_t + FE_h + \epsilon_{h,t}$, where $Y_{h,t}$ is the dependent variable of interest, with subscripts h indicating household and t indicting time. The unit of observation is household month. Dependent variables analyzed in this table include $Payday\ Borrow_{h,t}$ (the dollar amount of online payday borrowing), $Bounced\ Checks_{h,t}$ (the number of bounced checks), $Overdrafts_{h,t}$ (the number of overdrafts), $Consumption_{h,t}$ (the dollar amount of household debit and credit card transactions), and $Consumption\ Volatility_{h,t}$ (the six month volatility of $Consumption_{h,t}$). $Treated * After_{h,t}$ is an interaction term of $Treated_h$ and $After_t$. $Treated_h$ is an indicator that takes the value of 1 when household has a pre-existing relationship with a lender that is shutdown during OCP. $After_t$ is an indicator that takes the value of 1 after treatment and 0 otherwise. Both $Treated_h$ and $After_t$ are collinear with household and date fixed effects and are dropped from the regression. $Income_{h,t}$ is current household income and $Income_{h,t-1}$ is lagged household income. FE_t represent household time fixed effects and FE_h represent date fixed effects. Standard errors are clustered by household. t-statistics are reported in parentheses.

Table 4: Panel A - Whole Sample.

	Payday Borrow (1)	*Bounced Checks* (2)	*Overdrafts* (3)	*Consumption* (4)	*Consumption Volatility* (5)
*Treated * MA1*	-73.116***	-0.016**	-0.015	7.327	-1.753
	(-15.46)	(-2.17)	(-0.83)	(0.69)	(-0.40)
*Treated * MA2*	-91.803***	-0.016**	-0.021	2.538	0.933
	(-19.74)	(-2.16)	(-1.12)	(0.23)	(0.19)
*Treated * MA3*	-96.702***	-0.013*	-0.030	17.570	1.728
	(-21.15)	(-1.70)	(-1.52)	(1.52)	(0.31)
*Treated * QA2_6*	-108.506***	-0.035***	-0.040**	37.616***	2.796
	(-29.20)	(-5.94)	(-2.37)	(3.78)	(0.46)
Income	0.001***	0.000***	0.000***	0.100***	0.012***
	(4.20)	(8.84)	(6.21)	(68.74)	(29.15)
Lagged Income	0.001**	-0.000	-0.000***	0.062***	0.010***
	(2.34)	(-0.20)	(-2.62)	(53.71)	(24.55)
N	319032	319032	319032	319032	319032
R-sq	0.22	0.31	0.44	0.69	0.56
Pre-OCP Mean of Treated	$252.95	0.205	0.949	$1,502.53	$657.37

Table 4: Panel B - Excludes Recent Borrowers.

	Payday Borrow (1)	*Bounced Checks* (2)	*Overdrafts* (3)	*Consumption* (4)	*Consumption Volatility* (5)
*Treated * MA1*	-38.236***	-0.015*	-0.026	2.880	-3.835
	(-8.39)	(-1.76)	(-1.17)	(0.23)	(-0.74)
*Treated * MA2*	-50.288***	-0.016*	-0.037*	6.420	-1.579
	(-15.62)	(-1.93)	(-1.66)	(0.49)	(-0.27)
*Treated * MA3*	-57.264***	-0.012	-0.048**	13.150	-1.386
	(-14.74)	(-1.40)	(-2.07)	(0.96)	(-0.21)
*Treated * QA2_6*	-62.230***	-0.031***	-0.040**	33.779***	4.765
	(-19.76)	(-4.49)	(-1.99)	(2.85)	(0.66)
Income	0.001***	0.000***	0.000***	0.098***	0.012***
	(3.29)	(7.17)	(5.36)	(56.10)	(24.44)
Lagged Income	0.001***	0.000	-0.000	0.063***	0.011***
	(3.50)	(0.25)	(-1.39)	(45.32)	(21.53)
N	226062	226062	226062	226062	226062
R-sq	0.16	0.29	0.44	0.69	0.56
Pre-OCP Mean of Treated	$196.44	0.210	1.008	$1,479.64	$659.02

Table 4: Panel C - Restricted Subsample.

	Payday Borrow (1)	*Bounced Checks* (2)	*Overdrafts* (3)	*Consumption* (4)	*Consumption Volatility* (5)
*Treated * MA1*	-102.731***	0.010	-0.001	-16.696	1.435
	(-14.30)	(0.67)	(-0.04)	(-0.98)	(0.19)
*Treated * MA2*	-115.981***	0.009	0.001	1.579	2.883
	(-15.72)	(0.63)	(0.03)	(0.09)	(0.35)
*Treated * MA3*	-130.749***	0.032**	0.000	-0.039	8.058
	(-19.07)	(2.09)	(0.01)	(-0.00)	(0.91)
*Treated * QA2_6*	-136.498***	-0.035***	0.027	31.485**	14.399
	(-21.54)	(-3.54)	(1.14)	(2.25)	(1.60)
Income	0.001***	0.000***	0.000***	0.099***	0.012***
	(3.97)	(4.87)	(4.51)	(48.04)	(20.95)
Lagged Income	0.001*	-0.000**	-0.000*	0.061***	0.010***
	(1.80)	(-2.27)	(-1.78)	(36.94)	(18.12)
N	175275	175275	175275	175275	175275
R-sq	0.20	0.29	0.44	0.69	0.56
Pre-OCP Mean of Treated	$248.71	0.212	0.706	$1,148.75	$559.29

Table 5: This table explores how household outcomes change over time following Operation Choke Point. The regression specification is: $Y_{h,t} = \sum_{Z=1}^{6} \beta_Z \ Treated * QAZ_{h,t} + \beta_7 \ Income_{h,t} + \beta_8 \ Income_{h,t-1} + FE_t + FE_h + \epsilon_{h,t}$, where $Y_{h,t}$ is the dependent variable of interest, with subscripts h indicating household and t indicting time. The unit of observation is household month. Dependent variables analyzed in this table include $Payday \ Borrow_{h,t}$ (the dollar amount of online payday borrowing), $Bounced \ Checks_{h,t}$ (the number of bounced checks), $Overdrafts_{h,t}$ (the number of overdrafts), $Consumption_{h,t}$ (the dollar amount of household debit and credit card transactions), and $Consumption \ Volatility_{h,t}$ (the six month volatility of $Consumption_{h,t}$). $Treated * QAZ_{h,t}$ is an interaction term of $Treated_h$ and QAZ_t. $Treated_h$ is an indicator that takes the value of 1 when household has a pre-existing relationship with a lender that is shutdown during OCP. QAZ_t is an indicator that takes the value of 1 the Z$^{\text{th}}$ quarter after treatment and 0 otherwise. Both $Treated_h$ and QAZ_t are collinear with household and date fixed effects and are dropped from the regression. $Income_{h,t}$ is current household income and $Income_{h,t-1}$ is lagged household income. FE_t represent household time fixed effects and FE_h represent date fixed effects. Standard errors are clustered by household. t-statistics are reported in parentheses.

	Payday Borrow (1)	*Bounced Checks* (2)	*Overdrafts* (3)	*Consumption* (4)	*Consumption Volatility* (5)
*Treated * MA1*	-102.852***	0.011	-0.002	-16.564	1.378
	(-14.32)	(0.69)	(-0.06)	(-0.98)	(0.18)
*Treated * MA2*	-115.981***	0.010	0.000	1.250	2.616
	(-15.72)	(0.67)	(0.01)	(0.07)	(0.32)
*Treated * MA3*	-130.691***	0.033**	-0.000	-0.431	7.776
	(-19.04)	(2.11)	(-0.01)	(-0.02)	(0.87)
*Treated * QA2*	-134.005***	-0.024**	0.017	18.315	7.895
	(-20.93)	(-2.06)	(0.65)	(1.21)	(0.86)
*Treated * QA3*	-130.032***	-0.037***	0.032	19.329	10.620
	(-20.26)	(-3.43)	(1.26)	(1.18)	(1.05)
*Treated * QA4*	-138.531***	-0.044***	0.034	43.229**	17.535
	(-20.85)	(-3.69)	(1.17)	(2.44)	(1.64)
*Treated * QA5*	-138.965***	-0.036***	0.036	33.244*	22.323*
	(-20.58)	(-2.91)	(1.13)	(1.72)	(1.92)
*Treated * QA6*	-152.734***	-0.035**	0.005	74.340***	21.698
	(-19.30)	(-2.18)	(0.12)	(2.94)	(1.56)
Income	0.001***	0.000***	0.000***	0.099***	0.012***
	(3.98)	(4.87)	(4.51)	(48.04)	(20.95)
Lagged Income	0.001*	-0.000**	-0.000*	0.061***	0.010***
	(1.81)	(-2.26)	(-1.78)	(36.93)	(18.12)
N	175275	175275	175275	175275	175275
R-sq	0.20	0.29	0.44	0.69	0.56
Pre-OCP Mean of Treated	$248.71	0.212	0.706	$1,148.75	$559.29

Table 6: This table explores how household outcomes vary across pre-OCP borrowing groups following Operation Choke Point. Heavy borrowers are those whose count of payday loan transactions in the six-month period before OCP from January 2013 to June 2013 was above the median, while light borrowers are those below the median. Panel A presents the results of the subsample of heavy pre-treatment borrowers, while Panel B presents the results of the subsample of light pre-treatment borrowers. The regression specification is: $Y_{h,t} = \beta_1\, Treated{*}QA1_{h,t} + \beta_2\, Treated{*}QA2\_6_{h,t} + \beta_3\, Income_{h,t} + \beta_4\, Income_{h,t-1} + FE_t + FE_h + \epsilon_{h,t}$, where $Y_{h,t}$ is the dependent variable of interest, with subscripts h indicating household and t indicting time. The unit of observation is household month. Dependent variables analyzed in this table include $Payday\ Borrow_{h,t}$ (the dollar amount of online payday borrowing), $Bounced\ Checks_{h,t}$ (the number of bounced checks), $Overdrafts_{h,t}$ (the number of overdrafts), $Consumption_{h,t}$ (the dollar amount of household debit and credit card transactions), and $Consumption\ Volatility_{h,t}$ (the six month volatility of $Consumption_{h,t}$). $Treated_h$ is an indicator that takes the value of 1 when household has a pre-existing relationship with a lender that is shutdown during OCP. $QA1_t$ is an indicator that takes the value of 1 the first quarter after treatment and 0 otherwise, while $QA2\_6_{h,t}$ is an indicator that takes the value of 1 during the second through sixth quarters after treatment 0 otherwise. Both $Treated_h$, $QA1_t$, and $QA2\_6_{h,t}$ are collinear with household and date fixed effects and are dropped from the regression. $Income_{h,t}$ is current household income and $Income_{h,t-1}$ is lagged household income. FE_t represent household time fixed effects and FE_h represent date fixed effects. Standard errors are clustered by household. t-statistics are reported in parentheses.

Table 6: Panel A - Heavy Pre-Treatment Borrowers.

	Payday Borrow (1)	*Bounced Checks* (2)	*Overdrafts* (3)	*Consumption* (4)	*Consumption Volatility* (5)
*Treated * MA1*	-94.101***	-0.024**	-0.019	19.034	6.498
	(-12.46)	(-2.19)	(-0.76)	(1.34)	(1.11)
*Treated * MA2*	-118.444***	-0.019*	-0.038	7.373	5.342
	(-15.45)	(-1.70)	(-1.47)	(0.50)	(0.82)
*Treated * MA3*	-124.130***	-0.018	-0.024	19.313	4.851
	(-16.44)	(-1.62)	(-0.86)	(1.26)	(0.64)
*Treated * QA2_6*	-138.475***	-0.048***	-0.044*	53.704***	6.066
	(-21.96)	(-5.22)	(-1.77)	(3.88)	(0.71)
Income	0.001**	0.000***	0.000***	0.098***	0.011***
	(2.35)	(6.42)	(4.77)	(49.47)	(19.51)
Lagged Income	0.001***	0.000	-0.000**	0.060***	0.010***
	(3.08)	(0.25)	(-2.35)	(37.98)	(15.55)
N	152928	152928	152928	152928	152928
R-sq	0.24	0.32	0.47	0.70	0.56
Pre-OCP Mean of Treated	$351.64	0.244	0.980	$1,497.14	$638.08

Table 6: Panel B - Light Pre-Treatment Borrowers.

	Payday Borrow (1)	*Bounced Checks* (2)	*Overdrafts* (3)	*Consumption* (4)	*Consumption Volatility* (5)
*Treated * MA1*	-31.560***	-0.007	-0.008	-14.896	-15.899**
	(-6.52)	(-0.67)	(-0.30)	(-0.94)	(-2.35)
*Treated * MA2*	-42.698***	-0.011	0.001	-7.960	-10.371
	(-10.62)	(-1.13)	(0.05)	(-0.47)	(-1.37)
*Treated * MA3*	-43.235***	-0.005	-0.037	6.772	-8.120
	(-10.58)	(-0.56)	(-1.32)	(0.38)	(-0.98)
*Treated * QA2_6*	-41.496***	-0.012	-0.032	7.501	-10.465
	(-13.99)	(-1.55)	(-1.42)	(0.52)	(-1.18)
Income	0.001***	0.000***	0.000***	0.101***	0.012***
	(3.57)	(6.10)	(4.00)	(48.00)	(21.77)
Lagged Income	-0.000	-0.000	-0.000	0.064***	0.011***
	(-1.21)	(-0.76)	(-1.39)	(38.09)	(19.25)
N	166104	166104	166104	166104	166104
R-sq	0.16	0.28	0.41	0.69	0.56
Pre-OCP Mean of Treated	$122.50	0.153	0.909	$1,509.67	$682.87

Table 7: This table explores how household outcomes vary across income groups following Operation Choke Point. High-income borrowers are those whose sum of income in the six-month period before OCP from January 2013 to June 2013 was above the median, while low-income borrowers are those below the median. Panel A presents the results of the subsample of high-income borrowers, while Panel B presents the results of the subsample of low-income borrowers. The regression specification is: $Y_{h,t} = \beta_1\ Treated * QA1_{h,t} + \beta_2\ Treated * QA2\_6_{h,t} + \beta_3\ Income_{h,t} + \beta_4\ Income_{h,t-1} + FE_t + FE_h + \epsilon_{h,t}$, where $Y_{h,t}$ is the dependent variable of interest, with subscripts h indicating household and t indicting time. The unit of observation is household month. Dependent variables analyzed in this table include $Payday\ Borrow_{h,t}$ (the dollar amount of online payday borrowing), $Bounced\ Checks_{h,t}$ (the number of bounced checks), $Overdrafts_{h,t}$ (the number of overdrafts), $Consumption_{h,t}$ (the dollar amount of household debit and credit card transactions), and $Consumption\ Volatility_{h,t}$ (the six month volatility of $Consumption_{h,t}$). $Treated_h$ is an indicator that takes the value of 1 when household has a pre-existing relationship with a lender that is shutdown during OCP. $QA1_t$ is an indicator that takes the value of 1 the first quarter after treatment and 0 otherwise, while $QA2\_6_{h,t}$ is an indicator that takes the value of 1 during the second through sixth quarters after treatment 0 otherwise. Both $Treated_h$, $QA1_t$, and $QA2\_6_{h,t}$ are collinear with household and date fixed effects and are dropped from the regression. $Income_{h,t}$ is current household income and $Income_{h,t-1}$ is lagged household income. FE_t represent household time fixed effects and FE_h represent date fixed effects. Standard errors are clustered by household. t-statistics are reported in parentheses.

Table 7: Panel A - High-Income Borrowers.

	Payday Borrow (1)	*Bounced Checks* (2)	*Overdrafts* (3)	*Consumption* (4)	*Consumption Volatility* (5)
*Treated * MA1*	-92.306***	-0.010	0.005	11.864	-3.570
	(-12.53)	(-0.86)	(0.20)	(0.72)	(-0.54)
*Treated * MA2*	-110.112***	-0.009	-0.032	-4.622	-2.037
	(-14.97)	(-0.85)	(-1.16)	(-0.27)	(-0.28)
*Treated * MA3*	-125.253***	-0.018*	-0.037	2.728	-1.100
	(-17.71)	(-1.68)	(-1.27)	(0.15)	(-0.13)
*Treated * QA2_6*	-136.593***	-0.039***	-0.060**	33.126**	-2.343
	(-23.58)	(-4.45)	(-2.42)	(2.09)	(-0.25)
Income	0.001**	0.000***	0.000***	0.092***	0.010***
	(2.48)	(7.11)	(3.72)	(57.77)	(22.55)
Lagged Income	0.000	-0.000	-0.000***	0.058***	0.009***
	(0.05)	(-0.71)	(-4.66)	(45.28)	(18.68)
N	171368	171368	171368	171368	171368
R-sq	0.23	0.32	0.46	0.66	0.51
Pre-OCP Mean of Treated	$298.54	0.236	1.063	$2,008.55	$787.90

Table 7: Panel B - Low-Income Borrowers.

	Payday Borrow (1)	*Bounced Checks* (2)	*Overdrafts* (3)	*Consumption* (4)	*Consumption Volatility* (5)
*Treated * MA1*	-50.575***	-0.025***	-0.045*	1.093	-0.207
	(-9.31)	(-2.62)	(-1.88)	(0.09)	(-0.04)
*Treated * MA2*	-69.356***	-0.024**	-0.013	13.124	3.599
	(-13.69)	(-2.52)	(-0.51)	(1.05)	(0.57)
*Treated * MA3*	-61.990***	-0.006	-0.026	32.495**	3.648
	(-11.58)	(-0.57)	(-1.01)	(2.46)	(0.52)
*Treated * QA2_6*	-72.757***	-0.029***	-0.015	39.052***	8.675
	(-17.31)	(-3.82)	(-0.68)	(3.53)	(1.15)
Income	0.001***	0.000***	0.000***	0.136***	0.020***
	(3.00)	(5.98)	(6.08)	(41.37)	(21.65)
Lagged Income	0.002***	0.000	0.000***	0.077***	0.017***
	(4.33)	(1.03)	(3.41)	(31.89)	(19.13)
N	147664	147664	147664	147664	147664
R-sq	0.21	0.27	0.41	0.59	0.54
Pre-OCP Mean of Treated	$196.15	0.166	0.807	$872.10	$494.74

Table 8: This table explores what types of purchases payday loans finance. The $Y_{h,t} = \sum_{Z=-10}^{11} \beta_Z \; Treated * WAZ_{h,t} + \beta_{22} \; Income_{h,t} + \beta_{23} \; Income_{h,t-1} + FE_t + FE_h + \epsilon_{h,t}$, where $Y_{h,t}$ is the dependent variable of interest, with subscripts h indicating household and t indicting time. The unit of observation is household week. Dependent variables analyzed in this table include the following. $Payday\ Repay_{h,t}$ is the dollar amount spent on repaying payday loans in a given week. $Cash$ is the observed cash withdrawals from banks and ATMs. $Recurring\ Consumption$ is the dollar amount of credit and debit card spending spent on recurring merchants, while $Nonrecurring\ Consumption$ is the dollar amount of credit and debit card spending spent on nonrecurring merchants. A merchant is classified as recurring (nonrecurring) if more (less) than 20% of the household's purchases at the merchant occur in adjacent months. A merchant is classified as recurring (nonrecurring) if more (less) than 20% of the household's purchases at the merchant occur in adjacent months. Retailers, grocers, gas stations, and restaurants are the types of merchants which end up in the recurring consumption category, whereas transactions such as airline tickets and automotive repairs end up in the nonrecurring consumption category. $Insurance_{h,t}$ is the sum of observed insurance payments. $Nonrecurring\ Check_{h,t}$ and $Recurring\ Check_{h,t}$ are nonrecurring and recurring check payments, respectively. Recurring check payments are checks repeated within a household at a regular interval for the exact amount, such as a series of $1,000 rent payments. $Credit\ Card\ Payment_{h,t}$ are the sum of payments to credit cards. $Mortgage_{h,t}$ is the sum of mortgage payments. $Misc\ Bills_{h,t}$ contains transactions for which over 20% of transactions within a household are for the exact same amount, and contains payments to cell phone and television providers. $Car\ Payments_{h,t}$ are payments of car loans. $Casino_{h,t}$ is the dollar amount spent on casinos. $Treated * WA`Z_{h,t}$ is an interaction term of $Treated_h$ and WAZ_t. $Treated_h$ is an indicator that takes the value of 1 when household has a pre-existing relationship with a lender that is shutdown during OCP. WAZ_t is an indicator that takes the value of 1 the Z^{th} week after borrowing and 0 otherwise. Both $Treated_h$ and $Treated * WA`Z_{h,t}$ are collinear with household and date fixed effects and are dropped from the regression. $Income_{h,t}$ is current household income and $Income_{h,t-1}$ is lagged household income. FE_t represent household time fixed effects and FE_h represent date fixed effects. Standard errors are clustered by household and by time. t-statistics are reported in parentheses.

Week After Borrow	*Payday Repay.* (1)	*Cash* (2)	*Recur. Cons.* (3)	*Ins.* (4)	*Nonrecur. Check* (5)	*Recur. Check* (6)
-8	-3.129***	0.673	1.409**	-1.501**	-3.299***	3.580***
	(-3.85)	(0.86)	(2.49)	(-2.47)	(-2.69)	(5.33)
-7	1.195	-0.114	3.484***	1.343**	-3.945***	-1.057*
	(1.59)	(-0.16)	(5.58)	(2.13)	(-3.28)	(-1.76)
-6	4.543***	1.263**	5.494***	3.771***	-5.928***	-1.004*
	(6.15)	(2.00)	(9.63)	(6.35)	(-4.94)	(-1.73)
-5	12.864***	3.223***	5.772***	6.504***	-6.410***	2.879***
	(14.44)	(4.51)	(8.92)	(10.84)	(-5.55)	(4.07)
-4	2.144**	2.130***	-0.067	1.904***	-3.234***	8.310***
	(2.46)	(2.98)	(-0.12)	(3.43)	(-2.87)	(11.21)
-3	0.946	-2.621***	-1.509**	-1.123*	-7.784***	0.332
	(1.19)	(-3.64)	(-2.48)	(-1.84)	(-6.91)	(0.52)
-2	14.952***	0.561	2.717***	2.933***	-8.770***	-1.848***
	(16.14)	(0.79)	(4.08)	(4.59)	(-8.17)	(-3.28)
-1	41.527***	-1.646**	-0.633	3.245***	-8.534***	-1.394**
	(31.08)	(-2.02)	(-0.89)	(5.08)	(-7.39)	(-2.16)
0	38.382***	47.873***	32.376***	23.376***	15.029***	16.542***
	(27.47)	(42.66)	(32.41)	(32.29)	(12.32)	(20.04)
1	0.510	7.605***	12.314***	7.111***	4.639***	3.945***
	(0.54)	(10.59)	(17.68)	(12.10)	(3.99)	(6.03)
2	77.155***	3.007***	3.909***	7.969***	-3.416***	-4.126***
	(45.68)	(3.90)	(6.36)	(13.11)	(-2.82)	(-6.23)
3	65.659***	-3.215***	-4.226***	2.341***	-5.993***	-5.575***
	(42.63)	(-4.79)	(-6.74)	(3.79)	(-5.44)	(-8.48)
4	70.061***	5.947***	-2.637***	10.425***	-2.701**	1.253*
	(44.72)	(7.81)	(-4.28)	(16.26)	(-2.08)	(1.84)
5	21.221***	-3.883***	-10.957***	-1.451**	-3.057***	1.043*
	(25.70)	(-5.28)	(-16.30)	(-2.40)	(-2.64)	(1.67)
6	46.045***	1.913***	-2.571***	6.558***	-0.784	-2.477***
	(40.39)	(2.71)	(-3.87)	(9.57)	(-0.69)	(-3.98)
7	26.910***	-1.650**	-6.011***	2.544***	-2.458**	-5.994***
	(25.64)	(-2.22)	(-9.39)	(4.10)	(-2.01)	(-9.81)
8	45.462***	5.354***	0.979	10.910***	-0.626	-0.911
	(37.62)	(6.96)	(1.41)	(17.48)	(-0.51)	(-1.42)
N	14091801	14091801	14091801	14091801	14091801	14091801
Mean	$22.60	101.165	246.513	$125.24	$106.73	$67.79

Week After Borrow	*CC Pay.* (7)	*Mort.* (8)	*Misc Bills* (9)	*Nonrecur. Cons.* (10)	*Car Paym.* (11)	*Casino* (12)
-8	-1.049	-0.145	-0.120	-1.650***	-1.043***	0.023
	(-1.16)	(-0.19)	(-0.43)	(-4.85)	(-3.15)	(0.21)
-7	-3.568***	-1.686**	0.992***	-0.593*	-0.072	0.058
	(-3.92)	(-2.19)	(3.68)	(-1.68)	(-0.22)	(0.56)
-6	-0.263	0.333	1.817***	0.065	0.910***	0.200*
	(-0.28)	(0.47)	(6.57)	(0.20)	(3.12)	(1.77)
-5	-3.583***	1.103	2.370***	-0.140	1.524***	0.176
	(-3.97)	(1.49)	(9.11)	(-0.40)	(4.48)	(1.48)
-4	-4.986***	0.909	0.418	-2.460***	-0.572*	0.164
	(-5.14)	(1.14)	(1.56)	(-6.71)	(-1.85)	(1.58)
-3	-5.904***	-1.357*	-0.361	-3.082***	-1.053***	0.248**
	(-6.88)	(-1.83)	(-1.35)	(-8.92)	(-3.29)	(2.13)
-2	-5.201***	-3.341***	1.461***	-1.841***	0.148	0.326***
	(-5.62)	(-4.74)	(5.38)	(-4.89)	(0.44)	(2.75)
-1	-8.618***	0.367	1.141***	-3.401***	0.339	0.300***
	(-9.03)	(0.47)	(4.36)	(-9.47)	(1.00)	(2.72)
0	14.212***	10.814***	10.841***	8.365***	4.373***	1.173***
	(14.58)	(12.93)	(31.44)	(17.19)	(12.39)	(7.67)
1	3.208***	0.877	4.138***	4.937***	1.025***	0.227**
	(3.14)	(1.19)	(13.91)	(11.92)	(3.03)	(2.01)
2	-1.464	-4.672***	3.180***	0.316	0.410	-0.041
	(-1.45)	(-6.47)	(12.15)	(0.87)	(1.24)	(-0.38)
3	-5.521***	-3.253***	0.644**	-3.063***	-0.232	-0.013
	(-5.51)	(-4.53)	(2.30)	(-8.39)	(-0.74)	(-0.13)
4	-0.900	-0.299	2.054***	-3.024***	0.869***	0.038
	(-0.95)	(-0.41)	(7.32)	(-8.28)	(2.68)	(0.36)
5	-2.403**	0.069	-1.333***	-5.131***	-1.511***	-0.296***
	(-2.54)	(0.09)	(-5.19)	(-13.11)	(-4.85)	(-2.62)
6	-0.157	0.171	1.682***	-1.863***	0.838**	-0.115
	(-0.17)	(0.23)	(6.10)	(-5.28)	(2.37)	(-1.16)
7	-4.112***	-0.998	0.332	-2.698***	0.039	-0.075
	(-3.99)	(-1.39)	(1.22)	(-7.14)	(0.12)	(-0.79)
8	-0.728	0.354	2.586***	-0.383	1.267***	-0.025
	(-0.77)	(0.45)	(9.11)	(-0.95)	(3.48)	(-0.27)
N	14091801	14091801	14091801	14091801	14091801	14091801
Mean	$141.12	$59.63	$63.60	$79.67	$34.69	$1.62

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