NBER WORKING PAPER SERIES

THE EFFECTS OF DISCLOSURE AND ENFORCEMENT ON PAYDAY LENDING IN TEXAS

Jialan Wang
Kathleen Burke

Working Paper 28765
http://www.nber.org/papers/w28765

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
May 2021

From 2012-2016, Jialan Wang was employed at the Consumer Financial Protection Bureau, which oversees a number of policies that are relevant to the paper. Kathleen Burke was employed at the CFPB from 2013-2014. Besides salary support, both received access to the data as employees of the CFPB. Jialan Wang worked on this paper as an unpaid visiting scholar at the CFPB from 2017 to 2020. The CFPB reviewed this paper prior to circulation for the purposes of maintaining consumer and lender confidentiality. We are grateful to Sean Hundtofte, Carlos Parra, and Victor Stango and participants at the American Law and Economics Association, CFPB, FDIC Consumer Research Symposium, National University of Singapore, and SFS Cavalcade for valuable comments. We are thankful to Jesse Leary for contributing to an earlier version of this paper, circulated as “Information Disclosure and Payday Lending in Texas,” and to Yucheng Zhou, Yunrong Zhou, Renhao Jiang, Deepti Kumari, Di (Freddy) Li, Bo Lyu, Prarthana Patel, Vikas Venkatapathy, Lihan Yu, Filipe Correia, and Peter Han for research assistance. The views expressed are those of the authors and do not necessarily represent those of the Consumer Financial Protection Bureau or the United States, and should not be interpreted as those of the Congressional Budget Office or the National Bureau of Economic Research. Any errors are our own.

The Effects of Disclosure and Enforcement on Payday Lending in Texas
Jialan Wang and Kathleen Burke
NBER Working Paper No. 28765
May 2021
JEL No. D12,D14,G23,G28,G41,G51

ABSTRACT

Inspired by the field experiment in Bertrand and Morse (2011), the state of Texas adopted an information disclosure for consumers taking out payday loans starting in January, 2012. The disclosure compares the cost of payday loans with other credit products, and presents their likelihood of renewal in easy-to-understand terms. Simultaneously, Austin and Dallas implemented stricter supply restrictions through city ordinances. We analyze both types of regulations, and find that the statewide disclosures led to a significant and persistent 13% decline in loan volume in the first six months after implementation. The city ordinances led to a 62% decline in loan volume in Austin and a 20% decline in Dallas, with the timing of the effect driven by the start of enforcement rather than the effective date of regulation. The results show that both behaviorally-motivated disclosures and city-level supply restrictions can have a significant impact on equilibrium loan quantities, with no effect on prices or evidence of evasive income falsification.

Jialan Wang
Department of Finance
University of Illinois at Urbana-Champaign
340 Wohlers Hall
1206 S. Sixth Street MC-706
Champaign, IL 61820
and NBER
jialanw@illinois.edu

Kathleen Burke
Congressional Budget Office
Ford House Office Building
Washington, DC 20515
Kathleen.Burke@cbo.gov

I Introduction

This paper examines the effects of disclosure regulation and the interaction between regulation, enforcement, and compliance in the payday loan market. One of the primary motivations for consumer protection regulation is the concern that markets do not provide consumers with the information necessary to make optimal choices (Campbell, Jackson, Madrian and Tufano 2011, Campbell 2016). As a result, mandatory disclosures are a core consumer protection policy tool, particularly in the areas of food and nutrition, energy efficiency, and financial services. Despite the prominence of disclosures in consumer financial protection regulation, there is limited evidence on whether, when, and for whom they are effective.

Disclosures primarily aim to provide information, but recent examples in consumer finance are also designed to counteract behavioral biases.[1] Research on payday loans suggests that behavioral biases such as present bias, over-optimism, and limited attention may cause consumers to borrow and repay in ways that are not in their best interest.[2] Motivated by this evidence, Bertrand and Morse (2011) conducted a field trial in which borrowers were given one of three behaviorally-informed disclosures on the cash envelope they received after taking out a payday loan. They found that the effects of these disclosures varied, with the most effective one leading to an 11% reduction in borrowing probability.[3]

In the summer of 2011, Texas legislators passed a law requiring that new disclosures based on the Bertrand and Morse study be given to consumers before every payday loan transaction. In this paper, we use a unique administrative dataset covering multiple states to analyze the impact of the Texas disclosures on payday borrowing. Based on a difference-in-differences research design, we find that the disclosures led to a 13% decline in loan volume that remains persistent for at

[1]For example, the CARD Act of 2009 implemented disclosures on credit card statements warning consumers that paying only the minimum payment would make it difficult to repay their balances.

[2]See for example Skiba and Tobacman (2008), Carter, Skiba, Liu and Sydnor (2019), Mann (2013), Allcott, Kim, Taubinsky and Zinman (2020), and Carvalho, Olafsson and Silverman (2019).

[3]The field trials tested three information treatments. The most effective (Dollar Treatment) showed borrowers the accumulated fees in dollar terms for a payday loan outstanding for different durations, compared with fees over equivalent periods for credit card borrowing. The authors found a 5.9 percentage point decline in the likelihood of borrowing per pay period (equivalent to an 11% decline relative to the control group) and a $55 decrease in amount borrowed. The other two treatments had statistically significant negative effects on amount borrowed but not the likelihood of borrowing. The APR treatment compared the prices of payday loans to that of other forms of consumer credit, and the refinancing treatment presented borrowers with distributions of typical repayment and refinancing behavior.

least six months after they were first implemented. The effects are driven by the extensive margin, suggesting that the disclosures discouraged a subset of consumers from taking out loans while having only a small effect on average loan size. Despite some key differences between Bertrand and Morse's field trial and the implementation in Texas, our findings are remarkably consistent with the magnitudes of their treatment effects.

In addition to confirming the overall effect on borrowing probabilities documented in Bertrand and Morse (2011), we are also able to examine broader market impacts. As opposed to a field experiment, which is a temporary change implemented with the cooperation of a lender, a marketwide mandate may generate incentives for lenders to evade or obfuscate disclosures, or to adjust prices to make up for lost revenue (Campbell 2016, Persson 2014, Jin, Luca and Martin 2018). Although some critics felt the disclosures would not be effective in preventing predatory lending, we find a significant and persistent impact, with no countervailing increase in prices.[4] Consistent with the extensive-margin interpretation, we find limited effects of the disclosures on renewal, delinquency or default rates. There are no clear patterns by income or other borrower characteristics, with every demographic group exhibiting a significant reduction in borrowing. Overall, our results suggest a decline in demand and reduction in lender revenue as a result of the disclosures.

We also examine the effects of city ordinances implemented in Austin and Dallas that placed explicit restrictions on the supply of credit, and provide evidence on the interaction between regulation, enforcement, and compliance with these rules. The city ordinances were passed because local officials viewed the statewide regulations as too lenient on payday lenders, and went into effect at the same time as the disclosure rule. Although most of the literature on government intervention in financial markets relies solely on the letter and effective dates of regulation, in reality there is a complex interplay between lenders and regulators in the interpretation of and compliance with the rules.[5]

[4]Wendy Davis, an outspoken opponent of the industry who represented Fort Worth in the Texas Senate, refused to vote on the licensing or the disclosure bill, and remarked after passage that "We haven't done anything in the state of Texas to help the people who are at the vulnerable end of this predatory practice." Thanh Tan, "Texas Senate Oks Bills Regulating Payday Lenders." *Texas Tribune* 23 May 2011.

[5]For example, both Ohio and Texas limit allowable interest rates for payday lenders to 30% APR or less. But firms in both states evade the rate restriction by lending under an entirely different set of rules - those intended for credit repair or credit access companies. See Taylor, Astra. *Why It's So Hard to Regulate Payday Lenders.* New Yorker, Aug 3, 2016. Similarly, many states technically ban "rollovers" of payday loans, but they have little impact on actual borrowing activity, as lenders simply create a new loan contract instead of rolling over an existing loan (Burke, Lanning, Leary and Wang 2014).

Both city ordinances became legally effective on January 1, 2012, but due to administrative delays, Austin did not begin enforcement until May 1, and Dallas did not begin until June 17. We find no effects of the ordinances at the regulation date, although they began binding immediately upon the enforcement dates. When correctly identifying the start of compliance on the enforcement dates, we find that the ordinances led to a 62% decline in loan volume in Austin and a 20% decline in Dallas, which are several times larger than the estimates when using the regulation dates. To our knowledge, this is some of the first direct evidence of strategic noncompliance with regulation by financial services firms. Despite the lag in compliance, we find no evidence of other forms of evasion such as income falsification or spillovers to neighboring stores outside of city limits.

This paper contributes to several literatures, on the effects of disclosures, the pass-through of regulation to prices, the use of payday loans, and the roles of enforcement and compliance in financial regulation. Disclosures are intended to improve consumer understanding of contract terms and reduce firms' ability to shroud these features, but their effects on equilibrium prices and quantities are typically believed to be small.[6] Moreover, theoretical and experimental studies suggest that lenders may undo the potential benefits of disclosures through obfuscation or price adjustment (Campbell 2016, Persson 2014, Jin, Luca and Martin 2018). Our results contrast with many of these previous studies. We find that in the context of payday loans, a simple product with only a few features, behaviorally-motivated disclosures can have a marked effect on equilibrium quantities without observable offsetting effects.

Our work also relates to the growing recent literature on the pass-through of financial regulation to prices, which paints an ambiguous picture of whether regulations designed to increase or decrease the revenues of financial institutions affect the ultimate prices consumers pay. Stango and Zinman (2011) find that weaker enforcement of Truth in Lending Act (TILA) disclosures widens the gap between prices for more- versus less-biased consumers, but that the absolute prices paid by biased consumers remain unchanged. Similarly, Melzer and Schroeder (2017) and Mukharlyamov and Sarin (2019) find that binding price ceilings are offset by increased costs on other margins, leaving consumers no better off. In contrast, Agarwal, Chomsisengphet, Mahoney and Stroebel (2014) and Gross, Kluender, Liu, Notowidigdo and Wang (forthcoming) find that changes in lender revenues

[6]See Lacko and Pappalardo (2010), Elizondo and Seira (2014), Agarwal, Chomsisengphet, Mahoney and Stroebel (2014), Keys and Wang (2016), Adams, Hunt, Palmer and Zaliauskas (2019), and Kulkarni (2020).

driven by credit card and bankruptcy regulation do translate into savings for consumers. Similar to the effects of the CARD Act, we find no offsetting changes in prices in response to either the disclosure or supply regulations in Texas, suggesting an overall decline in lender revenue.

This paper also contributes to the literature on the payday lending market. Since the Texas disclosures present no new information that isn't already publicly available in some form, a perfectly informed and rational consumer would exhibit no response to them. Thus, our results are consistent with previous evidence that some payday consumers are subject to behavioral biases such as present bias, over-optimism, and limited attention.[7] Given the combined nature of the Texas disclosures that could potentially target several different types of biases, we are unable to pinpoint the psychological mechanisms behind how consumers responded in our setting. However, our results suggest that at least some consumers believe they are better off taking fewer loans once they are exposed to easy-to-understand information. Since consumers across a range of demographic groups all exhibit significant responses to the Texas disclosures, our results caution that targeting biased consumers on the basis of simple metrics such as income level may prove challenging.

The extent to which regulation of part of the credit market spills over to other financial instruments, or the "regulatory whack-a-mole" effect, is critical to designing effective regulation, and has been an active area of study in the payday literature.[8] While we find that payday loan volume decreases both in response to statewide disclosures and city-mandated supply restrictions, we find no evidence of evasion in terms of income falsification or spillovers to stores just outside of city limits. Overall, given that a sizable but still minority-share of payday borrowers respond to disclosures, our findings do not contradict evidence that payday loans on average have positive, negative or neutral effects on consumer welfare.[9] Reconciling the disparate effects found in the literature is an important area for future research, and it is possible that a greater focus on measuring the

[7]See Skiba and Tobacman (2008), Carter, Skiba, Liu and Sydnor (2019), Mann (2013), Allcott, Kim, Taubinsky and Zinman (2020), and Carvalho, Olafsson and Silverman (2019)

[8]Morgan, Strain and Seblani (2012) and Gathergood, Guttman-Kenney and Hunt (2018) find that payday bans increase overdrafts and bounced checks. Carter (2015) finds that payday rollovers are complementary with pawnshop use, Bhutta, Goldin and Homonoff (2016) find that payday bans shift borrowers to other forms of high-cost credit, while Miller and Soo (2020) find a lack of substitution to mainstream credit when consumer credit scores increase exogenously. Desai and Elliehausen (2016) find small and largely insignificant changes in consumer credit delinquencies after payday bans.

[9]Zinman (2010), Morse (2011), Fitzpatrick and Coleman-Jensen (2014), and Zaki (2016) find benefits to payday loans; while Melzer (2011), Carrell and Zinman (2014), Melzer (2017), and Baugh (2016) find harms; and Bhutta, Skiba and Tobacman (2012), Bhutta (2014), and Carter and Skimmyhorn (2016) find null effects. Dobridge (2018) finds ambiguous effects depending on household circumstances.

interplay between regulation, compliance, and enforcement could shed light on the nature of this heterogeneity.

Our findings on the effects of municipal payday lending restrictions in Austin and Dallas are a first step toward understanding this interplay.[10] While a thorough investigation of enforcement and compliance throughout the payday industry is beyond the scope of our paper, we find that even municipalities with relatively few resources compared with state and federal regulators can effectively reduce loan volume at covered lenders. However, unlike a major federal regulation like the CARD Act, which induced large banks to fully comply on the exact effective dates of rules (Agarwal, Chomsisengphet, Mahoney and Stroebel 2014), lenders in Austin and Dallas strategically delayed compliance until enforcement became imminent. By directly measuring compliance at the loan level, we show that enforcement intensity matters dramatically for interpreting the effects of regulation.

The rest of this paper is organized as follows. Section II provides an overview of payday loans and payday regulations in Texas. Section III describes our data. Section IV describes our results. Section V describes the effects of city ordinances, and Section VI concludes.

II Background on Payday Loans and Texas Regulation

Payday loans are a popular source of short term credit among low- to middle-income Americans, with 9% of underbanked households reporting use of at least one payday loan in the past year (Burhouse, Chu, Goodstein, Northwood, Osaki and Sharma 2013). Payday loans are typically between \$300 and \$500 in principal and are structured as a single balloon payment of the amount borrowed and fees, timed to coincide with the borrower's next payday. Fees average \$10 to \$20 per hundred dollars borrowed. Although fees do not vary with loan duration, a flat \$15 per hundred fee annualizes to nearly 400% APR for a typical 14-day loan (Consumer Financial Protection Bureau, 2013).

[10] Most of the literature on public enforcement and compliance is in the context of environmental regulations, tax compliance, workplace safety, or criminal law (Heyes 2000, Slemrod 2019). In consumer finance, Bostic, Engel, McCoy, Pennington-Cross and Wachter (2008) find that the impact of state level anti-predatory-lending laws depends on the strength of the legal enforcement mechanisms. Benmelech and Moskowitz (2010) suggest that the strength of enforcement may reflect political capture by the regulated industry rather than the consumer welfare, and Stango and Zinman (2011) find that public enforcement intensity is crucial to the impact of TILA disclosure regulations.

The payday loan industry expanded through the 1990s and early 2000s, aided by the loosening of state usury laws through special carve-outs for payday loans and by partnership structures between payday lenders and banks to "import" regulations and rate restrictions across state lines, a practice the FDIC ended in the mid-2000s.[11] As it expanded, the industry attracted controversy and regulatory scrutiny due to the high annualized cost of the loans and the high frequency of loan renewals or "roll-overs." In recent years, state regulators have restricted lending with loan size caps, fee caps, limits on roll-over activity, cooling-off periods, and outright bans, among other measures (Kaufman 2013). In 2011, the Consumer Financial Protection Bureau became the industry's first federal regulator. While some regulators and consumer groups argue that lenders prey on consumers' desperation and confusion (Montezemolo 2013, Bourke, Horowitz, Lake and Roche 2013), these loans may also provide a valuable source of liquidity for households facing cashflow shocks.

Payday lending was lightly regulated in Texas before 2012. Although the state legislature created a new section of the Finance Code to govern payday lending in 2001, lenders circumvented the rate and fee schedules by partnering with out-of-state banks to originate loans. When this "rent-a-bank" model was ended by FDIC guidelines in 2005, lenders continued to offer payday loans outside of the payday lending section of the Finance Code by registering as Credit Access Businesses (CABs). As CABs, payday lenders charge borrowers fees to set up low-interest loans with a third party. From the borrower perspective, and for practical purposes, CABs act as payday lenders. We will use the terms synonymously to refer to payday lenders in Texas.

The Texas legislature reexamined its regulation of the payday loan industry during the 2011 session, passing two bills: one that brought CABs under the supervision of the Office of the Commissioner of Consumer Credit, with new licensing and reporting requirements (HB 2594), and another that mandated new disclosures, both through conspicuously posted fee schedules and a paper disclosure with a combination of the three Bertrand and Morse information treatments (HB 2592). The governor signed both into law on June 17, 2011, with an effective date of January 1, 2012.

The new licensing rules were not onerous for large lenders. Prior the effective date of the law requiring licensing, payday lenders operating as CABs were required to register with the Secretary

[11] The carve-outs for payday loans were sometimes based on model legislation crafted by the Consumer Financial Services of America. For more on the "rent-a-bank" model, see Mann and Hawkins (2007).

of the State of Texas for a nominal annual fee. The new law required these payday lenders to obtain a license to lend from the OCCC by showing sufficient assets for operation ($25,000 per location) and the financial responsibility and experience needed to command the confidence of the public. Anticipating a rush of applications during the transition period, the OCCC authorized 90-day provisional licenses for CAB applicants, allowing them to continue operations until their applications were fully processed. As of July 2012, 3,529 applications had been submitted to the OCCC and 2,957 were fully approved, with just 175 applications withdrawn and 13 denied. All of the lenders operating in Texas whose data were gathered by the Consumer Financial Protection Bureau and used in this study (see below) were granted licenses and continued to operate in Texas.

Figure 1 shows an example of the mandatory disclosures.[12] The top left corner emphasizes the escalating costs from long-term borrowing with payday loans in dollar terms, with accumulated payments from renewing the loan up to 3 months. Although APR is the standard measure of cost for loan disclosure documents under the Truth in Lending Act, dollars may be more salient for payday consumers because that is how loan fees are quoted. This information may also counteract the "peanuts effect," under which biased consumers fail to add up small repeated costs like the renewal fee. The bottom left provides information that consumers would not have with standard disclosures: typical repayment behavior from a nationwide survey. Informing borrowers that 4 out of 10 people will renew five or more times attempts to counteract over-optimism about their likelihood of renewal. Finally, the top right compares the APR of payday loans with other forms of prime and subprime credit. This gives consumers reference points for the how payday APRs compare with other forms of credit, potentially improving their understanding of the costs of debt.

Many payday loan opponents were disappointed with the perceived mildness of the new laws, because they did not change any characteristics of the product or limit the use of the loans. In response, Austin and Dallas passed municipal ordinances to limit the loans that could be offered within their city limits. These ordinances, passed in August and June of 2011, respectively, restricted loans to 20% of a borrower's gross monthly income and required 25% amortization of the principal of the original loan with each renewal. Although both took effect the same day as the

[12]Some lenders customized the disclosures to the size of the loan requested, but lenders could also fulfill their disclosure obligation by creating a template with typical loan sizes and having these sample disclosures available in the store. Source: Interview with state official in the Office of the Commissioner of Consumer Credit, February 2014

state law, on January 1, 2012, neither city began enforcement until the spring. This ordinance has since been adopted by over 40 cities in Texas.[13]

III Payday Loan Data

We use a unique multi-lender administrative dataset of payday loans that was collected by the Consumer Financial Protection Bureau through its supervisory process. For this study, we use a subsample of the CFPB's full supervisory dataset that includes information on payday loans extended from July 2011-June 2012, six months before and after the regulatory changes in Texas. Information on each loan includes the principal amount, total fees, origination date, due date, and actual repayment date. We exclude loans from states other than Texas that changed their payday lending laws during this time frame.[14]

The dataset includes anonymized customer identifiers that allow us to identify all loans made to the same consumer by a given lender during the observed time period. Payday lenders typically collect very limited information about borrowers and focus on information necessary to originate loans, such as income amount and the date of the borrower's next payday. For more information about the dataset, see Consumer Financial Protection Bureau (2013) and Burke, Lanning, Leary and Wang (2014).[15]

Panel A of Table 1 shows summary statistics for all loans, loans made in Texas, and loans made outside of Texas. Mean loan principal in the sample is \$474 and mean fees are \$54, for a total amount due of \$528. Principal amounts in Texas are similar to other states (\$472 vs. \$474). While many states place a cap on fees (typically \$15 per \$100 borrowed), Texas does not. As a result, payday loans in Texas are almost twice as expensive as in other states in our dataset, with mean fees of \$20 per hundred borrowed versus \$11 outside of Texas. Loan duration is typically a function of the pay frequency of borrowers, reflecting a mix between 14 days for borrowers paid biweekly

[13] See Texas Municipal League Payday Lending Clearinghouse (https://www.tml.org/312/Payday-Lending-Clearinghouse) for a list of the cities and information on the unified ordinance.

[14] Because the data are Confidential Supervisory Information, this paper only presents results that are aggregated and do not identify specific lenders, and as a further precaution we do not reveal how many lenders data are included in the analysis. There is no lender entry or exit in the sample, and only payday loans extended via storefront establishments are included (online loans are excluded). The states excluded due to other changes in their payday lending laws are Arizona, Colorado, Delaware, Illinois, Oregon, and Wisconsin.

[15] The data used in this analysis contain no direct consumer identifiers, such as name or address.

and 30 days for those paid monthly. The mean duration of loans in the sample is 18 days, and the median is 14 days.

Panel B describes borrower characteristics and loan usage per borrower over the 12-month sample. Average gross monthly income is $2,097, with borrower income in Texas ($2,459) being higher than in other states ($2,031). We divide consumers into three groups based on pay frequency.[16] Consistent with the median loan length of 14 days reported above, 51% of borrowers are paid either biweekly or semi-monthly, which we combine into the "biweekly" group. Seventeen percent of borrowers are paid weekly, and 32% are paid monthly. While the weekly and biweekly groups consist of income from employment, the monthly group includes income from employment, Social Security, and other benefits such as retirement, unemployment, and disability payments. Borrowers in our sample use about six loans per year on average, which corresponds to $2,725 in total principal originated, $311 in fees, and 116 days of indebtedness.[17] Overall, while loan costs are significantly higher in Texas compared to other states, consumer characteristics and usage patterns are similar between Texas and the rest of the sample.

IV Effects of Disclosures

In this section, we analyze the impacts of the disclosure requirement in Texas. We first describe the impacts of disclosures on overall loan volume and discuss dynamics, lender responses, and potential alternative interpretations. We then analyze the effects on loan renewals, delinquency, and default, and heterogeneity in the treatment effect.

IV.A Main Results

We estimate the effects of the Texas disclosures using a difference-in-differences strategy. Examining loan volume and other outcomes in state-month cells, we use states that did not experience regulatory changes as controls. The identifying assumption is that outcomes in Texas would have moved along a parallel trend as those in other states in the absence of the disclosure requirement.

[16] In order to obtain a payday loan, borrowers must present a pay stub from a regular income source, some details of which are recorded in our data.

[17] See Burke, Lanning, Leary and Wang (2014) for more details on loan usage among payday borrowers.

One way to validate this assumption is to see whether states were moving along parallel trends prior to the Texas law change.

Figure 2 provides an initial test of this assumption in the raw time series. The figure shows normalized loan volume for Texas and other states grouped by region. For Texas and each region, we calculate total loan volume per month and normalize by the average loan volume during the six months in 2011. The graph shows that while Texas moves along a very similar trend as states in the other three regions prior to 2012, loan volume is 10-15% lower in 2012. Moreover, states in other regions continue to move along very similar trends with each other in 2012.

Under the parallel trends assumption, we implement a regression framework of the following form to estimate the causal effect of the Texas disclosure requirement on outcomes within state-month cells:

$$y_{st} = \alpha_s + \alpha_t + \beta TexasPost + \epsilon_{st} \tag{1}$$

where α_s and α_t are state and month fixed effects and $TexasPost$ is an indicator for Texas during the six months after the reform. The coefficient of interest is β, which captures the effect of the disclosure regulation in Texas relative to control states. Standard errors in all specifications are robust to heteroskedasticity and are clustered at the state level.

Panel A of Table 2 shows our main results. Column (1) shows the effect of the disclosures on log monthly loan volume in dollars, which will be our key summary variable throughout the analysis. We find that over the six months following the disclosures, loan volume declines by 0.14 log points, equivalent to a 13% decrease in Texas relative to other states. The effect is highly statistically significant, with a p-value less than 1%.

The second column reports effects on the number of loans. The treatment effect is very similar to that for dollar volume, showing that the effect of disclosures is concentrated on the extensive rather than intensive margin. Disclosures seem primarily to discourage some consumers from taking loans, rather than encouraging them to take smaller loans. This is consistent with the overall message of the disclosures, which focus on the high costs of payday loans compared with other forms of credit instead of comparing the costs of larger versus smaller loans.

As an alternative method of inference, Figure 3 shows the results of a permutation test that assigns placebo treatments to states other than Texas for dates between September 2011 and March 2012 in the specification from equation (1), where the dependent variable is log dollar loan volume. The estimate for the date of the true disclosure reform in Texas, indicated by the red line, is larger in magnitude than nearly all of the placebo coefficients, confirming our interpretation of the causal effect of the Texas disclosure regulation. The p-value from this permutation test is 0.01.

We next test whether our results can be explained either by lender behavior or by the concurrent city ordinances in Austin and Dallas instead of through the consumer demand channel. Columns (3) and (4) of Table 2 test whether the decline in loan volume is caused by store closings. Column (3) shows a precisely-estimated null effect on the number of stores that are open in each month, and column (4) shows that the effects on loan volume are identical when keeping only loans made by stores that are open during the entire sample period. Persistent declines in loan volume may lead to more store closings in Texas over the long run compared with other states. However, the lack of store closings over our sample period is consistent with the interpretation that the drop in loan volume we observe is driven by consumer responses instead of lender strategy. Column (5) drops loans made by stores in Austin and Dallas for all months, and shows that the estimates for statewide loan volume is very similar, so are not driven by the concurrent ordinances in these cities. We devote Section V to exploring the effects of the Austin and Dallas ordinances in detail.

Another potential concern is that the decline in loan volume is due not to the disclosures, but to the licensing requirements implemented in Texas at the same time. There are several reasons why we believe this interpretation to be unlikely. As described in Section II, the new licensing requirements are likely to be most burdensome for small lenders, not the large multi-state lenders in our dataset. Given the relatively small number of licenses withdrawn or denied by the OCCC, the licensing requirements either did not bind or they discouraged unqualified lenders from applying. In contrast, none of the lenders in our dataset were denied licenses. Thus, we would expect supply responses driven by the licensing requirement to weakly increase borrowing volume at large lenders starting in January, the opposite of the result we find. To the extent that some loan volume shifted from unlicensed lenders to those in our dataset, our estimate of the treatment effect of disclosures would be biased downward.

In a final test of potential supply responses, panel B of Table 2 reports the effects of disclosures on average loan terms at origination. Sample means for Texas in the pre-reform period are shown in the first row. We might expect greater consumer awareness of costs to drive down prices. However, the results show a small decline in average loan principal and negligible changes in contract duration and price. Consistent with the regression results, Figure 4 shows the distribution of APRs in Texas for our sample period, with almost no change throughout the distribution. The lack of major changes in contract terms support the interpretation that the disclosures had few effects on price competition or lender strategy during the first six months, and that the effects on loan volume are driven by a decrease in demand.

IV.B Dynamics

We next examine the dynamics of the disclosure impacts using an event study specification of the following form:

$$Log(LoanVolume_{st}) = \alpha_s + \alpha_t + \sum_{t \neq 12/2011} \beta_t Texas_t + \epsilon_{st} \tag{2}$$

where the β_t's are the coefficients of interest. The coefficient is normalized to zero in the month prior to the disclosure implementation in December 2011, so the point estimates can be interpreted as changes relative to the month before the reform. We run this specification both including and excluding stores in Austin and Dallas, which were subject to more restrictive city ordinances.

Figure 5 presents the results. The solid blue line (circles) represents the coefficient estimates, and the short dashed lines represent 95% confidence intervals from standard errors clustered by state. The coefficients prior to disclosure implementation are small, ranging from 0.01 to 0.06 log points (1% to 6%) relative to the month prior to the reform, and they show no clear monotonic pattern. Only one coefficient is significantly different from zero at the 5% level. These results confirm that loan volume in Texas moved in a roughly parallel trend with other states prior to the disclosure implementation.

The coefficients for the six months after the reform illustrate the temporal pattern and persistence of the disclosure's effect. Instead of dropping suddenly, loan volume declines steadily over the six months after the reform, ending up at 15% below pre-reform levels in the last month. The gradual decline in loan volume could be due to a lag in compliance by lenders, although we are not aware of specific reports of such lags. A second explanation is that the dynamic pattern is caused by the compounded effects of repeated exposure to the disclosures. A third possibility is that disclosures discourage some consumers from using loans. If the disclosures cause a permanent decrease in the flow of new loans, the effects on total loan volume would take several months to stabilize to a new lower level, since loan sequences often span several pay cycles once they are started. While we cannot completely disentangle these explanations, the evidence that most of the effect comes from the extensive margin suggests that the third explanation may be the most likely one, and it is consistent with the rest of the evidence we present.

The event study results show no evidence of temporary or "Hawthorne" effects of the disclosure. It does not seem to be the case that consumers respond to the novelty of a new piece of information, only to revert back to previous borrowing patterns. Over the six months following the disclosure implementation, we see no evidence of reversal either due to borrower habituation or to lender efforts to counteract the effects of the disclosures. As shown in the solid red line (triangles) and long dashed lines, the dynamics when excluding stores in Austin and Dallas stabilize starting in April, suggesting that the drop in loan volume due to disclosures takes about four months to reach a steady state, and that the continued statewide drop in May and June is driven in part by the enforcement of local ordinances (see Section V for more details).

IV.C Loan Performance

Much of the policy concern about payday loans centers around the number of times loans are renewed or rolled over, which can lead to continuous sequences of borrowing that generate large accumulated fees. While a number of states prohibit renewals, and other states institute short waiting periods between loans, in practice over 80% of all payday loans are followed by another loan within 14 days, and prohibitions of contractual rollovers have little impact on the realized renewal rate due to lender tactics that evade the letter of the law (Burke, Lanning, Leary and Wang 2014).

Part of the disclosure explicitly targets renewals by showing the distribution of customers by how many times they renew, and showing the cumulative costs over different horizons of continuous borrowing. While this information could make borrowers less likely to renew conditional on taking out a loan, it could also discourage customers from taking out a loan altogether, keeping the conditional renewal rate the same. We examine the effects of disclosures on the likelihood of renewal within 30 days of the original due date, as well as delinquency and default in Table 3.

The first row shows the sample means for Texas in 2011, and panel A shows the results from the regression specification in equation (1), where the dependent variable is the fraction of loans that renew within 30 days, become delinquent, or default within each cell. By collapsing the data into state-by-month cells instead of using microdata regressions described in panels B and C, this specification yields more conservative standard errors, and suggests that the disclosures had very little effect on renewal, delinquency, or default rates in terms of either statistical or economic significance. All three point estimates and standard errors are smaller than one percentage point, allowing us to rule out economically large effects on loan performance.

Since loan performance could vary based on consumer characteristics and borrowing patterns that could be affected by the disclosures, panels B and C show the results of linear probability models using the loan-level microdata, where the dependent variables are 0/1 indicators for each loan outcome and standard errors are clustered by state. While panel B includes only state, month, and lender fixed effects similar to the collapsed model, panel C includes additional controls for paycheck frequency, indicators for each quintile of income and loan amount, and indicators for the number of prior loans in the sequence to account for changes in conditional loan performance based on the number of past renewals. As expected, the standard errors are smaller when moving to the microdata, and the point estimates continue to suggest small economic effects relative to baseline.

These results help shed light on the mechanisms behind the disclosures, in that customers seem to be discouraged from taking out new loan sequences as opposed to reducing the number of rollovers of existing loans. This is consistent with our overall interpretation that the disclosures primarily had an extensive-margin effect by reducing the number of new loan sequences, while keeping loan size and renewal and default rates fairly steady.

IV.D Heterogeneity

We next explore heterogeneity in the effects of the disclosures on different groups of consumers. Although we have limited information on consumer demographics, our data allow us to test for heterogeneous effects across paycheck frequency, income level, and distance between the customer's address and the store address. To get a better idea of the types of customers who respond most to disclosures, we also merge customer and store addresses to the 2010–2014 American Community Survey and test for heterogeneity by census tract demographics.

Table 4 replicates the results of the main specification with log loan volume as the outcome, stratifying by borrower characteristics. Panel A presents the heterogeneity by paycheck frequency, showing that the magnitude of the treatment effect is negatively correlated with paycheck frequency. Loan volume from weekly and biweekly borrowers declines by 0.10 and 0.12 log points (9% to 11%) following the disclosures, and these effects are statistically indistinguishable. Monthly borrowers, a group that includes those with income from employment, Social Security, and other types of benefits, reduce their loan volume significantly more, by 0.19 log points (17%). In results not shown in the table, the estimates are similar between those receiving employment versus benefit income (0.185 vs. 0.188 log points).

Since payday loan fees do not vary with loan duration, APRs are increasing in paycheck frequency. Thus, we find that consumers with lower APRs, based on having monthly pay frequencies, actually respond more to the disclosures. Because interest rates in Texas are much higher than in most other states in our sample, we are unable to directly test for heterogeneity in the treatment effect by APR due to the lack of common support across states. However, the heterogeneity by pay frequency provides suggestive evidence that consumers may be less responsive to the disclosed level of APRs than to other aspects of the information provided.

One potential explanation for this pattern is that responsiveness to disclosures is related to a consumer's propensity to enter into long-term debt cycles. As shown in Burke, Lanning, Leary and Wang (2014), borrowers paid monthly are much more likely to be in debt continuously for an entire year, and are more likely to have non-amortizing sequences of loans where no principal paydown occurs across successive cycles. Several components of the disclosures emphasize the costs of long-term borrowing. The top left portion of the example disclosure shown in Figure 1 shows how the costs of borrowing escalate over time, and the bottom left informs consumers of the likelihood of reborrowing after an initial loan. This interpretation is also consistent with the results from Bertrand and Morse (2011), whose most effective treatment compares the costs of payday loans with the costs of credit cards, showing the greatest differential cost for long-term loan sequences. Thus, the disclosures may help promote longer-term thinking and remind consumers about the overall costs of prolonged debt cycles among those most likely to enter them, causing some customers to avoid borrowing.

To further investigate the drivers of disclosure responsiveness, panels B and C show the effects on loan volume stratified by terciles of borrower income and distance to store. The point estimates by income tercile vary between -0.11 and -0.19 log points (-10% and -15%), with no clear monotonic pattern. The point estimates are larger for consumers who live further from stores, but the differences across distance terciles are small and insignificant. Table A1 presents similar stratifications by average census tract characteristics from the American Community Survey between 2010–2014, merged to either the consumer or store address. While there is very mild evidence that effects are stronger in areas with larger white populations based on both customer and store location, in general we find significant results of the disclosures across all demographic groups, and lack power to detect small differences between groups.

Our conclusion from this heterogeneity analysis is that the disclosures appeal widely to customers across the population of payday borrowers. To the extent that customers who respond to the disclosure represent those who self-identify as being harmed by payday loans after exposure to clear information about prices and reborrowing probabilities, these results highlight the difficulty of targeting interventions based on easily-observable characteristics, as harms may be spread out across demographic groups.

V The Effects of City Ordinances

We next turn to the impact of local ordinances in Austin and Dallas effective on the same date as the statewide disclosure requirement. As described in Section II, these ordinances were passed in order to provide stronger restrictions than the statewide laws, which some local officials viewed as too lenient on predatory practices.

Dallas's Ordinance No. 28287 was adopted on June 22, 2011, and Austin's Ordinance 20180818-75 was adopted on August 18, 2011. Instead of just providing information to consumers, both ordinances limit the size of payday loans to 20% of a borrower's gross monthly income, as measured by their paycheck or other similar documentation. They also require at least 25% amortization each time a loan is renewed or reborrowed within seven days of paying off a prior loan, such that the original principal is paid in full with a maximum of three renewals.[18] The ordinances do not limit or track borrowing across different store locations or different lenders by the same consumer.

The city ordinances allow us to analyze the impact and incidence of binding supply constraints that are similar in spirit to the more complex nationwide regulations proposed by the CFPB, and assess their impact on prices and demand spillovers to neighboring cities. It also provides some of the first direct evidence on enforcement and compliance in consumer finance, and highlights the important role of these dimensions of government oversight that have thus far been largely overlooked in the literature.

V.A Timing and Compliance

While the city ordinances were passed with effective dates of January 1, 2012, enforcement did not begin until several months later. Both cities were sued before the effective date by individual payday lenders and by the payday trade association Consumer Service Alliance of Texas (CSAT). The lawsuits did not prevent Austin or Dallas from enforcing their ordinances because neither case included a preliminary injunction (which would have legally required a pause in enforcement until the resolution of the court case), and neither city officially delayed the effective date of the ordinance. Nonetheless, Austin did not begin enforcement until May 1, and Dallas until June 17, 2012, based on official reports. These delays were likely driven by the administrative challenges of implementing a workable enforcement plan for the novel rules.[19]

[18] Austin and Dallas differ slightly in the amortization requirement. Dallas' ordinance requires that 25% of the principal be repaid with each renewal, while Austin's ordinance requires payment of 25% of the total transaction amount (principal and fees) upon renewal. Both of these requirements result in gradual declines in principal over a maximum of four total loans.

[19] The ordinances also required that lenders provide a valid state lending license in order to register with the city. When the state authorized 90-day provisional licenses for lenders in January 2012, the cities may have decided to push the beginning of enforcement until all lenders could be expected to have a full state license. The date of active enforcement in Austin was referenced in a slideshow from the Office of Telecommunications and Regulatory Affairs, which was responsible for enforcement. The City of Dallas voluntarily agreed to temporarily suspend enforcement of the ordinance for members of CSAT, but withdrew from the agreement in early March and informed CSAT that

Although the Austin and Dallas ordinances are identical, the cities created two very different enforcement strategies. Austin added a new full-time employee who monitors compliance with the specific ordinance by investigating consumer complaints and auditing lenders. Enforcement in Dallas is handled jointly by a small number of Department of Code Compliance employees who work on consumer protection issues more generally, and the Community Prosecution Team of the Dallas Police Department, which is composed of attorneys and enforcement officers who address quality of life issues through code enforcement. Enforcement of the Dallas ordinance was added as an additional duty for existing employees at these departments. The official penalty for violating the ordinance is the same in each city, a fine of $500 per day, but both enforcement strategies initially focused on bringing lenders into compliance through education rather than punitive action.

Our results for Dallas, and comparisons between Austin and Dallas, should be interpreted with significant caution because the enforcement date in Dallas was only two weeks before the end of our sample period. However, the more limited compliance in Dallas that we find and the longer delay until its enforcement date are broadly consistent with fewer resources devoted and weaker overall enforcement cited by media reports. While the penalty is the same, the likelihood of being penalized may have factored into the level of lender compliance.

While most studies of financial regulation rely solely on the effective dates of regulation as shocks to downstream outcomes, we examine changes in the payday loan market around both the effective and enforcement dates of the city ordinances, and are able to directly measure compliance in the loan data. We start by running an event study of log weekly loan volume for each store in Texas on indicators for each week of 2012 interacted with whether the store was located in Austin or Dallas:

$$Log(LoanVolume_{st}) = \alpha_s + \alpha_t + \sum_{t \neq 2011w52} [\beta_t^A Austin_t + \beta_t^D Dallas_t] + \epsilon_{st} \tag{3}$$

where α_s and α_t are store and week fixed effects and the β_t's are the coefficients of interest. The event study indicators are included for both cities in the same regression, and the last week of 2011 is omitted, so the point estimates can be interpreted as changes relative to that week. Importantly, only stores within Texas are included, so the effects of the statewide disclosures discussed above are

enforcement would begin on or after June 17, 2012.

absorbed into the time fixed effects. Thus, the coefficients measure the effects of the city ordinances over and above those of the disclosures.

Figure 6 presents the results. The solid blue lines represent the coefficient estimates, and the dotted lines represent 95% confidence intervals from standard errors clustered by store. The coefficients prior to the regulation date in January 2012 fluctuate around zero, but show no trend over time, confirming that loan volume in Austin and Dallas moved in roughly parallel trends with other areas of Texas prior to the regulation date. These trends continue to be flat between the regulation and enforcement dates, indicating that that there was no effect of the ordinances due to regulation alone. However, loan volume drops sharply in both cities precisely in the weeks that enforcement began.

These results suggest that even though the ordinances were legally in effect, ongoing lawsuits and administrative delays kept local regulators from enforcing them right away, and lenders strategically delayed compliance until they knew enforcement would begin. Nonetheless, while the sharp drops in loan volume at the exact enforcement dates are strongly indicative of direct causal effects of city ordinance enforcement, it could be the case that other factors rather than compliance with the ordinances themselves drove the change in loan volume.

To directly measure compliance with the rules, we examine the two loan features targeted by the ordinances: the loan-to-income ratio and principal amortization rate. Figure 7 compares the distributions of these features for loans made under three regimes: pre-regulation (prior to Jan. 1st, 2012), regulation with no enforcement (Jan. 1st through April 30th for Austin and Jan. 1st through June 16th for Dallas), and regulation with enforcement (starting May 1st for Austin and June 17th for Dallas). The pre-regulation distributions are shown the solid blue lines, while the distributions under regulation with no enforcement are shown in red dotted lines, and those under regulation with enforcement are in green dashed lines. Consistent with compliance and enforcement driving the loan volume declines shown in the event studies, the blue and red distributions are very similar, while the post-enforcement distributions in green are markedly different. Based on the largely overlapping blue and red distributions, there appeared to be very little effort to comply with the city ordinances prior to the enforcement dates.

Comparing the green dashed lines to the pre-enforcement distributions, subgraphs (a) and (b) show significant, though not complete, compliance with the provision limiting loan volume to 20% of a borrower's gross monthly income. Loan-to-income ratios shift down noticeably in Austin and Dallas and bunch just below the cutoff, although 10% of loans in Austin and 28% of those in Dallas still exceed the 20% limit after the enforcement dates. The significant changes in the distributions of gross monthly income around the 20% limit suggest that the income information we see is used by lenders in determining the loan-to-income ratio. But it is possible that nonconforming loans could be based on a different measure of income, or one that is more updated than the gross monthly income variable we are using (i.e. that we are measuring income and compliance with error, relative to how they would be treated by the regulators).

A limitation of our analysis is that we only have a static measure of income based on a borrower's initial application for credit with a lender. Since borrower income may change over time, our measures of compliance may not reflect the actual fraction of loans made in legal violation of the ordinances. However, given that borrowers whose incomes have risen since their initial application are likely to take out fewer subsequent loans, under-estimation of income is unlikely to fully explain the fraction of non-compliant loans. Since loan size comes directly from lenders' administrative data, it is unlikely that we are mis-measuring loan amounts.

Compliance with the provision requiring 25% amortization with each renewal is harder to measure, as we cannot identify the beginning of loan sequences for loans originated near the beginning of the sample period. We use the amortization rate across successive loans as a proxy for compliance with this provision, which is likely to produce an upper bound on the level of compliance.[20] Subgraphs (c) and (d) show that prior to enforcement of the ordinances, the vast majority of renewals had no amortization of principal. This is consistent with Burke, Lanning, Leary and Wang (2014), who found that more than 80% of multi-loan sequences had either zero or negative amortization. While the distributions shift downward after enforcement of the ordinances, 31% of renewals still amortize less than 25% of principal of the previous loan in Austin and 79% in Dallas during the post-enforcement period.

One potential concern with rules based on reported income is that lenders might evade the

[20] For example, if a borrower takes out three loans in a sequence with principal amounts of \$100, \$75, and \$50, they would exactly comply with the amortization rule. Under our measure, we would observe amortization rates of $25\% = 25/100$ and $33\% = 25/75$, so would over-estimate the degree of amortization and overall compliance with the rule.

loan-to-income restrictions by falsifying borrower incomes, leaving actual loan activity unchanged. More generally, it is useful to decompose the effects of the requirements on changes in reported income versus the loan amounts borrowed. To address these questions, we plot the distributions of loan size and income during the three regulation and enforcement regimes in Figure 8.

As shown in subgraph (a), the loan size distribution shifts down substantially in Austin, consistent with compliance with the loan-to-income and amortization requirements imposed by the ordinance. Subgraph (c) shows that the fraction of loans made to middle-income borrowers within the payday population (around \$1500 to \$2500 in gross monthly income) decreases somewhat, shifting mass toward the higher end of the income distribution. While this could result from some borrowers reporting false incomes in order to obtain loans, the overall income distribution shifts only modestly compared with loan size, which is unlikely to be affected by falsification because it comes directly from lenders' administrative data. These results suggest that compliance with the ordinances is driven by changes in loan size rather than income falsification or borrower composition. Subgraphs (b) and (d) show that Dallas experienced only a small downward shift in loan size and a slight upward shift in the income distribution, consistent with incomplete compliance during the short post-enforcement period we observe.

Overall, the descriptive results in this section show that the enforcement dates, not the effective date, drove changes in market outcomes in response to city ordinances in Austin and Dallas. We verify using loan-level data that the specific supply restrictions implemented by the ordinances were binding, and worked primarily by reducing loan size rather than changing the composition of borrowers or inducing income falsification.

V.B Regression Results

We next use a difference-in-differences framework to formally estimate the effects of the Austin and Dallas ordinances and test for spillovers to neighboring areas. In particular, we compare the estimated effects when using the regulation dates to those using the enforcement dates. While regulation dates are used in most of the literature, we showed above that the enforcement dates correspond to actual compliance with the law.

Table 5 shows the results of regressions of the following form, estimated on outcomes within each store-week cell within Texas:

$$y_{st} = \alpha_s + \alpha_t + \beta_A AustinPost + \beta_D DallasPost + \epsilon_{st} \tag{4}$$

where α_s and α_t are store and week fixed-effects, and $AustinPost$ and $DallasPost$ are indicators for the periods after either regulation or enforcement for stores in each city. The coefficients β_A and β_D reflect the differential change in outcomes for Austin and Dallas stores compared with other Texas stores, measuring the effects of the ordinances over and above the effects of the statewide disclosures. Robust standard errors are clustered at the store level.

To provide a benchmark for the expected effect of the ordinances on the equilibrium quantity of credit, the first two rows of panel A of Table 5 provide simulated estimates of their impact based on loans in 2011. For each loan we observe in 2011, we construct a simulated loan based on the maximum principal that can be achieved while complying with the loan-to-income and amortization requirements. Specifically, we define the simulated loan amount as the minimum of the observed loan amount, 20% of gross monthly income, and 75% of the loan amount of the prior loan in the sequence if the loan is a renewal. If the loan is part of a sequence that has already had more than three prior renewals, we set the simulated loan principal to zero.

Column (1) shows the estimates for the combined effect of the loan-to-income and amortization requirements in each city, by comparing the aggregate simulated loan volume to actual loan volume in 2011. Column (2) estimates the fraction of loans that would be completely eliminated due to the binding amortization requirements alone. The results show that dollar loan volume would be expected to decline by 45% in Austin and 51% in Dallas, while the number of loans would decline by 21 and 27%. Thus, based on ex ante lending patterns, the city ordinances are expected to bind significantly in both cities, with somewhat larger effects in Dallas.

These estimates have several limitations. First, as discussed above, our income variable includes potential measurement error, and the direction of bias is unclear. Second, because we cannot observe the beginning of loan sequences that are ongoing at the start of the sample, our calculations may underestimate the impact of the amortization restriction. Finally, because the simulated impacts are based on the six months before the ordinances were implemented, our estimates do not control for seasonality and do not take into account the concurrent effects of the disclosures.

Because we showed above that the disclosures did not significantly change the income or loan size distributions or renewal rates in Texas, we do not expect the latter bias to drive our results. Despite these limitations, the simulated impact calculations provide a useful benchmark for the expected magnitude of the effects under full compliance.

We next present the regression results using the regulation date of January 1, 2012, which are shown in the remaining rows of panel A. The results in columns (1) and (2) show that dollar loan volume declines by 0.24 log points (22%) in Austin, while the number of loans declines by 0.10 log points (10%). In columns (3) and (4), the dependent variables are the average fraction of loans in each cell that fail to comply with the loan-to-income and amortization requirements, based on our compliance proxies described above. The two rows above the regression estimates show the sample means in the 2011 period, indicating that 43–50% of loans exceed the loan-to-income threshold and 91% violate the amortization requirement in the pre-period.

Relative to these benchmarks, the regression estimates show that the fraction of loans exceeding the 20% loan-to-income threshold declines by 11 percentage points, and the fraction with less than 25% amortization declines by 22 percentage points in Austin. While these effects are statistically significant, they represent only about a quarter of the expected effect under full compliance. The remaining columns of panel A show that the Austin ordinance led to a $47 decrease in average loan size and a $150 increase in average income. Overall, the results suggest incomplete compliance in Austin, and the effects in Dallas are small and largely insignificant when using the regulation dates.

The estimates change dramatically when using the enforcement dates instead of the regulation dates, as shown in panel B. After enforcement began, dollar loan volume fell in Austin by 0.96 log points (62%), and the number of loans fell by 0.41 log points (33%). The fraction of loans exceeding the 20% loan-to-income ratio declined by 29 percentage points, and the fraction with less than 25% amortization declined by 71 percentage points, indicating that the majority of loans complied with the ordinances once enforcement began. Columns (5) and (6) show that average loan principal declined by $206 in Austin, representing a more than forty percent drop relative to the pre-period mean of $494. Average customer income increased by $92 based on a pre-period mean of $2,744 per month, but this result is not significant at the 10% level. Overall, the estimates in Austin show much larger effects when appropriately taking into account delayed compliance with the regulations.

Despite the very short two-week post-period in Dallas, the effects also increase and gain significance when measuring the enforcement as opposed to regulation dates. The results in panel B show that dollar loan volume dropped by 0.22 log points (20%) and the number of loans dropped by 0.16 log points (14%). The level of direct compliance with the loan-to-income and amortization requirements only increased slightly by 2 to 6 percentage points. We also find only moderate effects on loan size and average income in the two weeks after enforcement in Dallas. Nonetheless, the significant impacts on loan volume starting at the exact date of enforcement suggest that the Dallas ordinance was starting to have an impact immediately after the enforcement date, while these changes are washed out when relying on the regulation date.

Overall, we find that the effect sizes are very different depending on whether we use the regulation or enforcement dates. While regulation dates are used in the vast majority of academic studies, they would under-estimate the effects and lead to potentially misleading conclusions about the effects of regulation. If we had relied on the regulation dates alone, we may conclude that lenders were evading the regulation in Austin and Dallas. Instead, we show that the Austin ordinance was mostly complied with following the enforcement date, and that the changes in overall loan volume were on par with what would be expected under full compliance. While we observe a very short post-enforcement window in Dallas, we show the beginnings of significant compliance, while relying on regulation dates alone would generate a null effect of regulation.

An important concern with the interpretation of our results is that the effectiveness of the city ordinances may be over-estimated if borrowers simply cross city lines to avoid the loan restrictions. In this case, our observed reductions in loan volume would be mirrored by increases in lending outside of city limits. In particular, many lenders have multiple locations, and could direct a borrower to branches located outside the city for a loan or renewal that does not need to conform to the ordinance.[21] Much of the literature examining state-level payday loan restrictions estimates their effects using border regions between states that have different regulatory regimes, an approach that assumes borrowers will cross state lines to borrow. However, this is little direct evidence on

[21]Lender-directed evasion of city ordinances was a serious enough concern that the OCCC issued a bulletin in December 2012, cautioning lenders that the practice could be considered deceptive. The bulletin acknowledged that the state agency lacked the authority to enforce city ordinances, but it criticized the lack of transparency some lenders displayed in trying to circumvent local ordinances.

the willingness of borrowers to cross state or city limits to borrow when lending is restricted.

We test for the presence of geographic spillovers in panel C of Table 5, showing the results from of regressions of the following form:

$$\begin{aligned} y_{st} = \alpha_s + \alpha_t + \beta_A AustinPost + \beta_A^* AustinBordPost \\ + \beta_D DallasPost + \beta_D^* DallasBordPost + \epsilon_{st} \end{aligned} \tag{5}$$

where α_s and α_t are store and week fixed-effects, $AustinPost$ and $DallasPost$ are indicators for the post-enforcement periods in Austin and Dallas, and $AustinBordPost$ and $DallasBordPost$ are indicators for post-enforcement periods for stores within a 20 mile radius outside these cities. The coefficients β_A^* and β_D^* reflect the differential change in outcomes in the areas just outside of Austin and Dallas compared with other stores in Texas, measuring the potential spillover effects of the city ordinances. Columns (1) and (2) of panel C show insignificant effects of the ordinance on loan volume and number of loans in border areas, and the remaining columns show no change the characteristics of loans or customers in border regions.[22] Overall, we find no evidence of spillovers during the period after enforcement of the ordinances. While it is possible that payday consumers restricted from borrowing in Austin and Dallas resorted to other types of loans, payday stores just outside of these cities are likely to be one of the best substitutes for loans in Austin and Dallas. Thus, it is unlikely that the decline in loan volume is fully offset by increases in other types of credit.

A final question is whether the more restrictive ordinances in Austin and Dallas led to changes in prices. It is possible that pricing frictions would prevent adjustment due to the relatively modest effects of the statewide disclosures on equilibrium loan volume, while leading to significant price adjustment due to the more restrictive city ordinances that were expected to cut loan volumes in half in Austin and Dallas. However, as shown in Figure A1, there was almost no change across the entire distribution of prices in Austin or Dallas. Thus, in the short run at least, we find no price adjustment either to statewide mandatory disclosures or to more restrictive city ordinances in Texas, suggesting that the declines in loan volume caused by these regulations led to significant

[22] In unreported results, we have also tried different thresholds for border regions, and the choice of 20 miles does not make a significant difference in the results. The distributions of income and loan size are also similar between the affected cities and their bordering regions, suggesting no compositional shift toward loans for which the ordinances would be likely to bind.

declines in lender revenues.

VI Conclusion

This paper examines the effects of behaviorally-motivated disclosures and binding restrictions on loan size and amortization on the payday loan market in Texas. We find that statewide disclosures led to a 13% decline in loan volume. Despite differences in implementation in a marketwide context, these results are remarkably similar to the results of the field experiment by Bertrand and Morse (2011) that inspired the regulation. Although lenders may have had incentives to obfuscate or undo the effects of mandatory disclosures, we find no effects on prices or evidence of income falsification or other forms of evasion. While borrowers with monthly pay frequencies who pay lower APRs respond more to the disclosures than those with weekly or biweekly pay frequencies, we find significant responses to the disclosures across the distribution of borrower characteristics. Given that much of the prior literature casts doubt on the effectiveness of disclosures, our results contribute important evidence that behaviorally-motivated disclosures can have a significant impact on borrowing behavior.

We also contribute some of the first direct evidence on the interactions between regulation, enforcement, and compliance in consumer finance. Although Austin and Dallas passed concurrent ordinances that restricted the loan-to-income ratio and amortization rate of payday loans made within city borders, these rules had no effect until the cities announced they would begin enforcement. Examining the regulation date, as is done in most of the financial regulation literature, would significantly under-estimate the results and generate potentially misleading conclusions. When examining the post-enforcement periods, we find that the city ordinances led to a 62% decline in loan volume in Austin and a 20% decline in Dallas, and that the timing and degree of compliance by lenders may be related to differences in the nature and intensity of enforcement across these two cities. Although the ordinances caused a much larger decline in loan volume than the disclosures, we also find no short-run effects on prices, suggesting that the declines in loan volume for both types of regulation were fully passed through to lender revenues during our sample period.

Enforcement of and compliance with regulation have received much less attention in the literature than the letter of regulation, and both are important areas for future research into the effects of government oversight in finance. In particular, given very different enforcement regimes across states, these dimensions provide a potential avenue for reconciling the disparate estimates of the effects of payday loans on consumer welfare in the literature. Future researchers can study these areas by gathering data on the number of employees involved in enforcement (which can sometimes be gleaned from budget documents), the level of specialization of regulatory agents, the frequency of audits, and incentives for or roadblocks to borrower reporting of illegal activity. Enforcement action through the courts is often a last resort to stop repeated lender misbehavior, so the number of such actions might be a proxy for but not be fully representative of the spectrum of enforcement activity. Our study illustrates the importance of using loan-level administrative data to directly measure compliance with regulations and the extent of geographic spillovers associated with local rules.

References

Adams, Paul D, Stefan Hunt, Christopher Palmer, and Redis Zaliauskas, "Testing the Effectiveness of Consumer Financial Disclosure: Experimental Evidence from Savings Accounts," Working Paper 25718, National Bureau of Economic Research March 2019.

Agarwal, Sumit, Souphala Chomsisengphet, Neale Mahoney, and Johannes Stroebel, "Regulating Consumer Financial Products: Evidence from Credit Cards," *The Quarterly Journal of Economics*, 2014, pp. 111–164.

Allcott, Hunt, Joshua Kim, Dmitry Taubinsky, and Jonathan Zinman, "Are High-Interest Loans Predatory? Theory and Evidence From Payday Lending," Technical Report, Working paper 2020.

Baugh, Brian, "Payday borrowing and household outcomes: Evidence from a natural experiment," Technical Report, Working paper 2016.

Benmelech, Efraim and Tobias J Moskowitz, "The Political Economy of Financial Regulation: Evidence from U.S. State Usury Laws in the 19th Century," *The Journal of Finance*, June 2010, *65*, 1029–1073.

Bertrand, Marianne and Adair Morse, "Information Disclosure, Cognitive Biases, And Payday Borrowing," *The Journal of Finance*, 2011, *66* (6), 1865–1893.

Bhutta, Neil, "Payday Loans and Consumer Financial Health," *Journal of Banking & Finance*, 2014, *47*, 230–242.

____, Jacob Goldin, and Tatiana Homonoff, "Consumer Borrowing after Payday Loan Bans," *Journal of Law and Economics*, 2016, *59* (1), 225–259.

____, Paige Skiba, and Jeremy Tobacman, "Payday Loan Choices and Consequences," *SSRN Electronic Journal*, 10 2012, *47.*

Bostic, Raphael W., Kathleen C. Engel, Patricia A. McCoy, Anthony Pennington-Cross, and Susan M. Wachter, "State and local anti-predatory lending laws: The effect of legal enforcement mechanisms," *Journal of Economics and Business*, 2008, *60* (1), 47 – 66. Financing Community Reinvestment and Development.

Bourke, Nick, Alex Horowitz, Walter Lake, and Tara Roche, "Payday Lending In America: Policy Solutions," *Washington, DC: Pew Charitable Trusts*, 2013.

Bureau, Consumer Financial Protection, "Payday Loans and Deposit Advance Products: A White Paper of Initial Data Findings," Technical Report 2013.

Burhouse, Susan, Karyen Chu, Ryan Goodstein, Joyce Northwood, Yazmin Osaki, and Dhruv Sharma, "FDIC National Survey Of Unbanked And Underbanked Households," *Federal Deposit Insurance Corporation*, 2013.

Burke, Kathleen, John Lanning, Jesse Leary, and Jialan Wang, "CFPB Data Point: Payday Lending," Technical Report, Consumer Financial Protection Bureau Office of Research 2014.

Campbell, John, Howell Jackson, Brigitte Madrian, and Peter Tufano, "Consumer Financial Protection," *Journal of Economic Perspectives*, 02 2011, *25*, 91–114.

Campbell, John Y, "Restoring Rational Choice: The Challenge of Consumer Financial Regulation," *SSRN 2719330*, 2016.

Carrell, Scott and Jonathan Zinman, "In Harm's Way? Payday Loan Access and Military Personnel Performance," *Review of Financial Studies*, 2014, p. 28052840.

Carter, Susan and William Skimmyhorn, "Much Ado About Nothing? New Evidence on the Effects of Payday Lending on Military Members," *Review of Economics and Statistics*, 11 2016, *99.*

Carter, Susan Payne, "Payday Loan and Pawnshop Usage: The Impact of Allowing Payday Loan Rollovers," *Journal of Consumer Affairs*, 2015, *49* (2), 436–456.

___ **, Paige Marta Skiba, Kuan Liu, and Justin Sydnor**, "Time to Repay or Time to Delay? The Effect of Having More Time Before a Payday Loan is Due," Technical Report 2019.

Carvalho, Leandro, Arna Olafsson, and Dan Silverman, "Misfortune and Mistake: The Financial Conditions and Decision-making Ability of High-cost Loan Borrowers," Working Paper 26328, National Bureau of Economic Research September 2019.

Desai, Chintal and Gregory Elliehausen, "The Effect of State Bans of Payday Lending on Consumer Credit Delinquencies," *The Quarterly Review of Economics and Finance*, 07 2016, *64.*

Dobridge, Christine L, "High-cost Credit and Consumption Smoothing," *Journal of Money, Credit and Banking*, 2018, *50* (2-3), 407–433.

Elizondo, Alan and Enrique Seira, "Are Information Disclosure Mandates Effective? Evidence From The Credit Card Market," Technical Report, Banco de Mexico 2014.

Fitzpatrick, Katie and Alisha Coleman-Jensen, "Food on the Fringe: Food Insecurity and the Use of Payday Loans," *Social Service Review*, 2014, *88* (4), 553–593.

Gathergood, John, Benedict Guttman-Kenney, and Stefan Hunt, "How Do Payday Loans Affect Borrowers? Evidence from the U.K. Market," *Review of Financial Studies*, 06 2018.

Gross, Tal, Raymond Kluender, Feng Liu, Matthew J Notowidigdo, and Jialan Wang, "The Economic Consequences of Bankruptcy Reform," forthcoming.

Heyes, Anthony, "Implementing Environmental Regulation: Enforcement and Compliance," *Journal of Regulatory Economics*, 2000, *17* (2), 107–129.

Jin, Ginger Zhe, Michael Luca, and Daniel J Martin, "Complex Disclosure," Working Paper 24675, National Bureau of Economic Research June 2018.

Kaufman, Alex, "Payday Lending Regulation," *Finance and Economics Discussion Series*, 01 2013, *2013*, 1–38.

Keys, Benjamin J and Jialan Wang, "Minimum Payments and Debt Paydown in Consumer Credit Cards," Working Paper 22742, National Bureau of Economic Research 2016.

Kulkarni, Sheisha, "Removing the Fine Print: Standardization, Disclosure, and Consumer Outcomes," 2020.

Lacko, James M. and Janis K. Pappalardo, "The Failure and Promise of Mandated Consumer Mortgage Disclosures: Evidence from Qualitative Interviews and a Controlled Experiment with Mortgage Borrowers," *American Economic Review*, May 2010, *100* (2), 516–21.

Mann, Ronald, "Assessing The Optimism Of Payday Loan Borrowers," *Supreme Court Economic Review*, 2013, *21* (1), 105–132.

Mann, Ronald J and Jim Hawkins, "Just Until Payday," *UCLA Law Review*, 2007, *54.*

Melzer, Brian and Aaron Schroeder, "Loan Contracting in the Presence of Usury Limits: Evidence from Automobile Lending," *Consumer Financial Protection Bureau Office of Research Working Paper*, 2017, (2017-02).

Melzer, Brian T, "The Real Costs Of Credit Access: Evidence From The Payday Lending Market," *The Quarterly Journal of Economics*, 2011, *126* (1), 517–555.

——, "Spillovers from Costly Credit," *The Review of Financial Studies*, 12 2017, *31.*

Miller, Sarah and Cindy K Soo, "Does Increasing Access to Formal Credit Reduce Payday Borrowing?," Working Paper 27783, National Bureau of Economic Research September 2020.

Montezemolo, Susanna, "Payday Lending Abuses and Predatory Practices," *Center for Responsible Lending. December*, 2013.

Morgan, Donald P, Michael R Strain, and Ihab Seblani, "How Payday Credit Access Affects Overdrafts and other Outcomes," *Journal of Money, Credit and Banking*, 2012, *44* (2-3), 519–531.

Morse, Adair, "Payday Lenders: Heroes Or Villains?," *Journal of Financial Economics*, 2011, *102* (1), 28–44.

Mukharlyamov, Vladimir and Natasha Sarin, "The Impact of the Durbin Amendment on Banks, Merchants, and Consumers," *U of Penn, Inst for Law & Econ Research Paper*, 2019, (19-06).

Persson, Petra, "Attention Manipulation and Information Overload," *Behavioural Public Policy*, 2014, *2*, 78–106.

Skiba, Paige and Jeremy Tobacman, "Payday Loans, Uncertainty and Discounting: Explaining Patterns of Borrowing, Repayment, and Default," *Vanderbilt Law and Economics Research Paper*, 08 2008, *0833.*

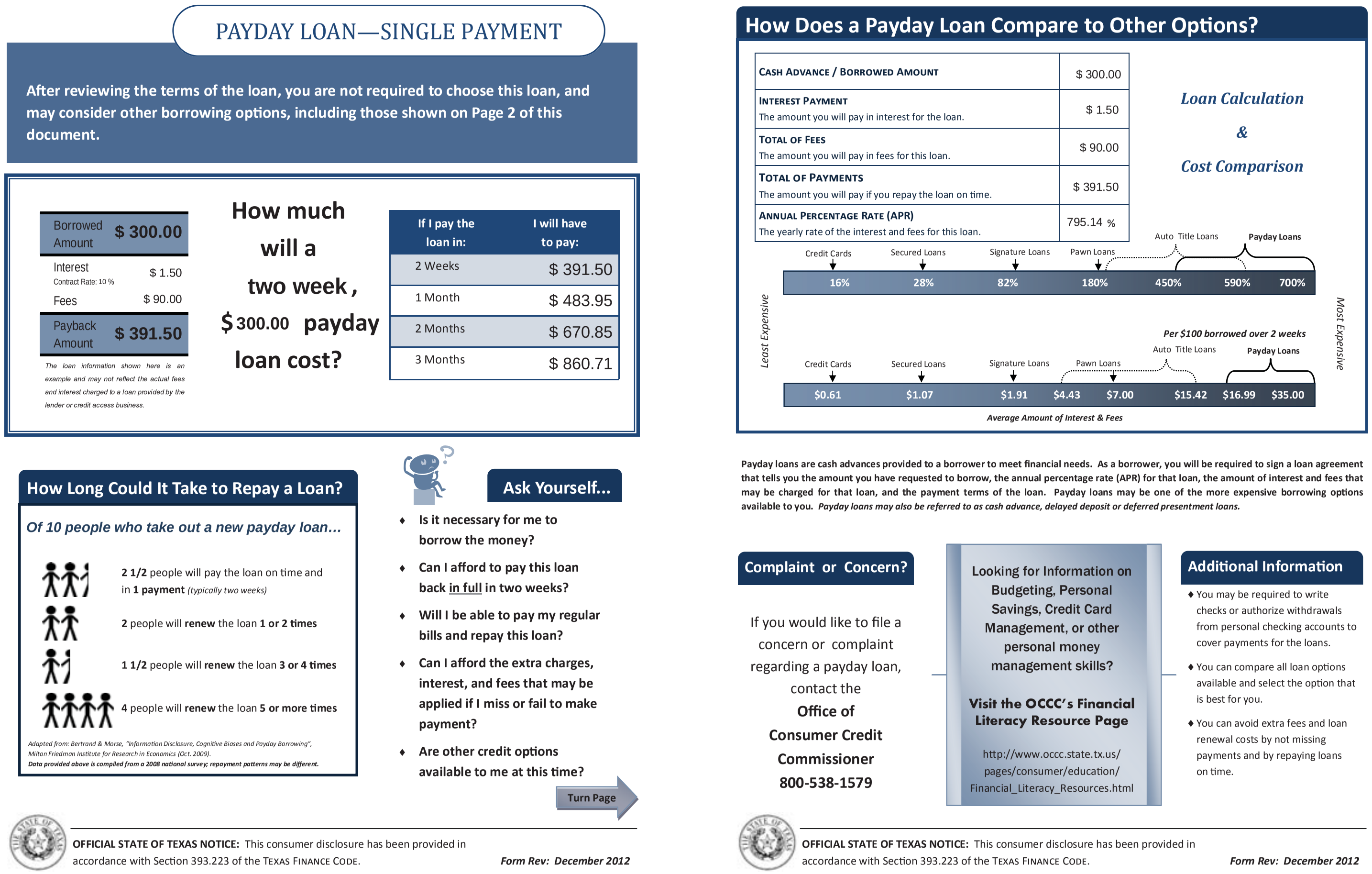
Slemrod, Joel, "Tax Compliance and Enforcement," *Journal of Economic Literature*, December 2019, *57* (4), 904–54.

Stango, Victor and Johnathan Zinman, "Fuzzy Math, Disclosure Regulation, And Market Outcomes: Evidence From Truth-In-Lending Reform," *The Review of Financial Studies*, 2011, *24* (2), 506–534.

Zaki, Mary, "Access to Short-term Credit and Consumption Smoothing Within the Paycycle," Working Paper 007.2016, FEEM 2016.

Zinman, Jonathan, "Restricting Consumer Credit Access: Household Survey Evidence On Effects Around The Oregon Rate Cap," *Journal of Banking & Finance*, 2010, *34* (3), 546–556.

Figure 1: Example of Texas Disclosure



PAYDAY LOAN—SINGLE PAYMENT

After reviewing the terms of the loan, you are not required to choose this loan, and may consider other borrowing options, including those shown on Page 2 of this document.

Borrowed Amount	$ 300.00
Interest (Contract Rate: 10 %)	$ 1.50
Fees	$ 90.00
Payback Amount	$ 391.50

The loan information shown here is an example and may not reflect the actual fees and interest charged to a loan provided by the lender or credit access business.

How much will a two week, $ 300.00 payday loan cost?

If I pay the loan in:	I will have to pay:
2 Weeks	$ 391.50
1 Month	$ 483.95
2 Months	$ 670.85
3 Months	$ 860.71

How Long Could It Take to Repay a Loan?

Of 10 people who take out a new payday loan…

- **2 1/2** people will pay the loan on time and in **1 payment** *(typically two weeks)*
- 2 people will **renew** the loan **1 or 2 times**
- **1 1/2** people will **renew** the loan **3 or 4 times**
- 4 people will **renew** the loan **5 or more times**

Adapted from: Bertrand & Morse, "Information Disclosure, Cognitive Biases and Payday Borrowing", Milton Friedman Institute for Research in Economics (Oct. 2009).
Data provided above is compiled from a 2008 national survey; repayment patterns may be different.

Ask Yourself…

- Is it necessary for me to borrow the money?
- Can I afford to pay this loan back <u>in full</u> in two weeks?
- Will I be able to pay my regular bills and repay this loan?
- Can I afford the extra charges, interest, and fees that may be applied if I miss or fail to make payment?
- Are other credit options available to me at this time?

Turn Page

OFFICIAL STATE OF TEXAS NOTICE: This consumer disclosure has been provided in accordance with Section 393.223 of the Texas Finance Code. *Form Rev: December 2012*

How Does a Payday Loan Compare to Other Options?

Cash Advance / Borrowed Amount	$ 300.00
Interest Payment The amount you will pay in interest for the loan.	$ 1.50
Total of Fees The amount you will pay in fees for this loan.	$ 90.00
Total of Payments The amount you will pay if you repay the loan on time.	$ 391.50
Annual Percentage Rate (APR) The yearly rate of the interest and fees for this loan.	795.14 %

Loan Calculation & Cost Comparison

Payday loans are cash advances provided to a borrower to meet financial needs. As a borrower, you will be required to sign a loan agreement that tells you the amount you have requested to borrow, the annual percentage rate (APR) for that loan, the amount of interest and fees that may be charged for that loan, and the payment terms of the loan. Payday loans may be one of the more expensive borrowing options available to you. *Payday loans may also be referred to as cash advance, delayed deposit or deferred presentment loans.*

Complaint or Concern?

If you would like to file a concern or complaint regarding a payday loan, contact the **Office of Consumer Credit Commissioner 800-538-1579**

Looking for Information on Budgeting, Personal Savings, Credit Card Management, or other personal money management skills?

Visit the OCCC's Financial Literacy Resource Page

http://www.occc.state.tx.us/pages/consumer/education/Financial_Literacy_Resources.html

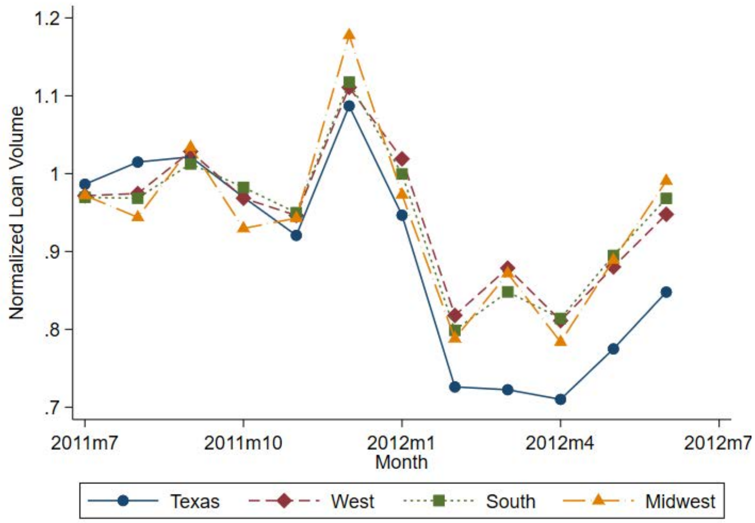
Additional Information

- You may be required to write checks or authorize withdrawals from personal checking accounts to cover payments for the loans.
- You can compare all loan options available and select the option that is best for you.
- You can avoid extra fees and loan renewal costs by not missing payments and by repaying loans on time.

OFFICIAL STATE OF TEXAS NOTICE: This consumer disclosure has been provided in accordance with Section 393.223 of the Texas Finance Code. *Form Rev: December 2012*

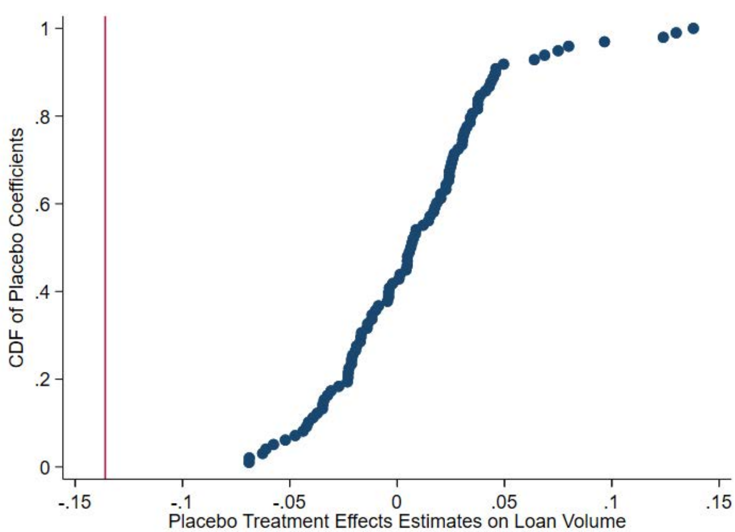
Source: https://www.mybridgeprovider.com/fees.html

Figure 2: Loan Volume in Texas vs. Other Regions



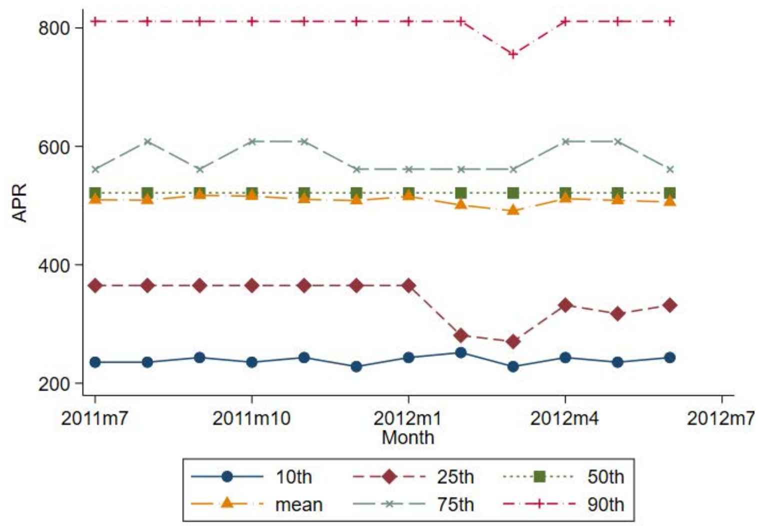
Note: The figure shows normalized loan volume originated in Texas and three other regions between July 2011 and June 2012. The western region of states that allow payday loans during this period includes CA, NV, UT, ID, WA, and WY. The southern region includes AL, FL, KY, LA, OK, TN, MS, SC, and VA. The midwestern region includes IN, MI, MO, OH, IA, KS, NE, and SD. Loan volume for each state or region is normalized by average volume between July 2011 and December 2011. Not all states listed are included in the underlying sample. We do not disclose the exact states used in order to preserve the confidentiality of lender identities.

Figure 3: Permutation Test of Difference-in-Differences Estimate



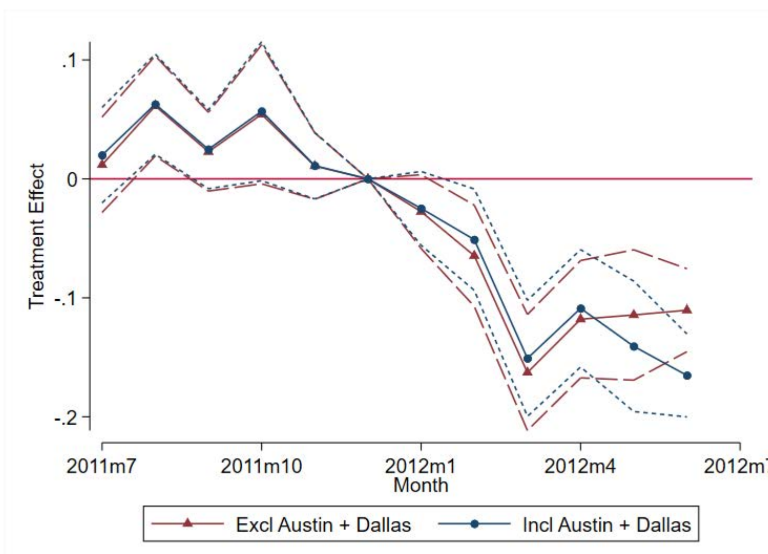
Note: The figure shows the distribution of difference-in-difference estimates from placebo treatment indicators for each state and each date between September 2011 and March 2012, where the dependent variable is the log of dollar loan volume. Treatment indicators for Texas are excluded. The red line indicates the estimated effect for Texas in January 2012.

Figure 4: Price Distribution in Texas



Note: The figure shows the distribution of APRs in Texas during the sample period.

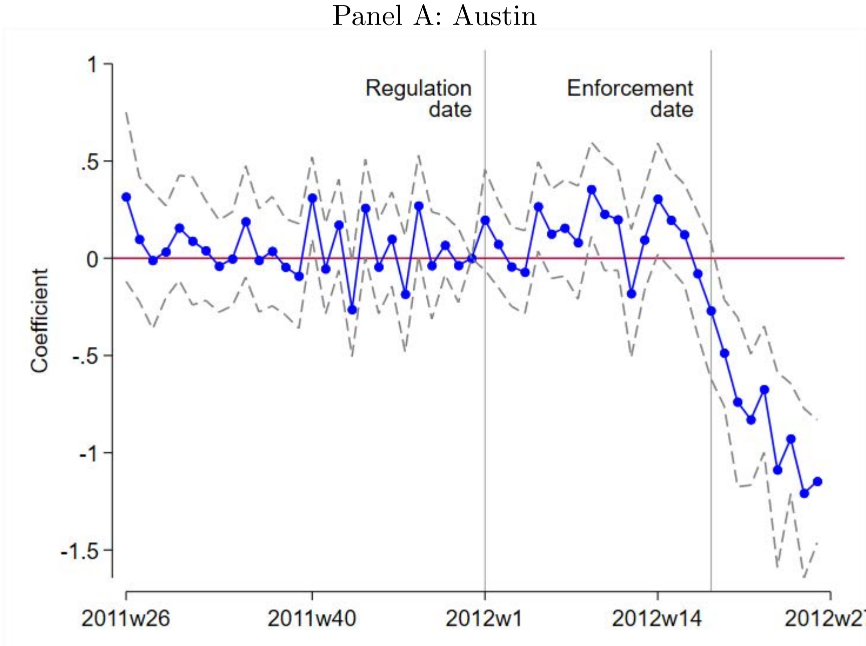
Figure 5: Persistence of Disclosure Impacts



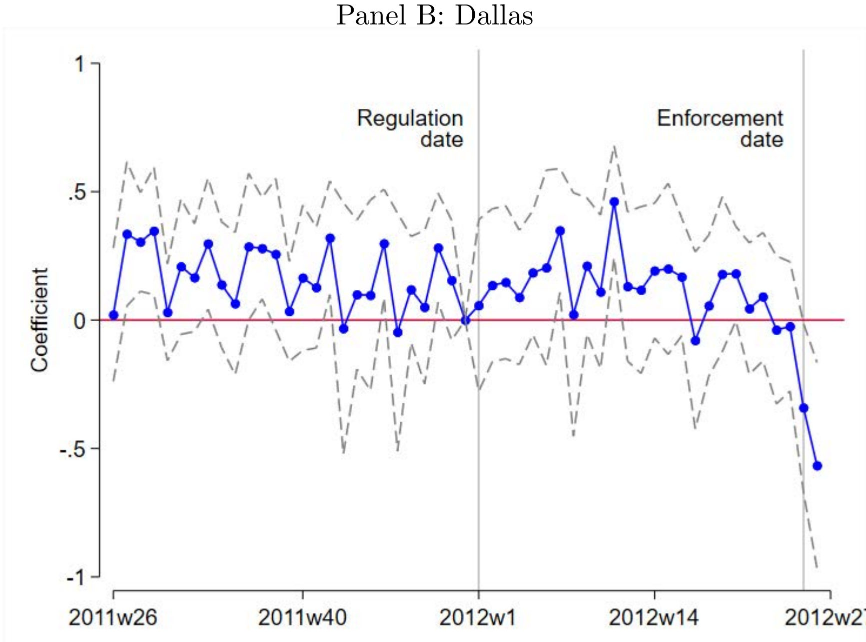
Note: The figure shows the coefficients from event study regressions of loan volume relative to the implementation of the disclosures in Texas in January 2012. The dependent variable is the log of total dollars lent in each state and month. The blue circles represent estimates when all loans in Texas are included. The red triangles represent estimates when loans in Texas exclude those made in the cities of Austin and Dallas. Dashed lines provide 95 percent confidence interval for each point estimate.

Figure 6: Effects of City Ordinances: Regulation vs. Enforcement

Panel A: Austin



Panel B: Dallas

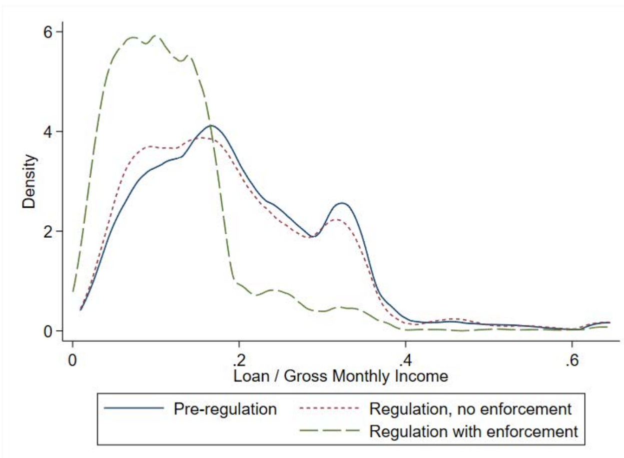


Note: The figure shows the coefficients from event study regressions of weekly loan volume relative to regulation and enforcement dates of city ordinances in Austin and Dallas. Both ordinances became effective on January 1st, 2012 (the regulation date), and the enforcement dates were May 1, 2012 for Austin and June 17, 2012 for Dallas. The dependent variable is the log of total dollars lent in each city and week within Texas. Dashed lines provide 95 percent confidence interval for each point estimate.

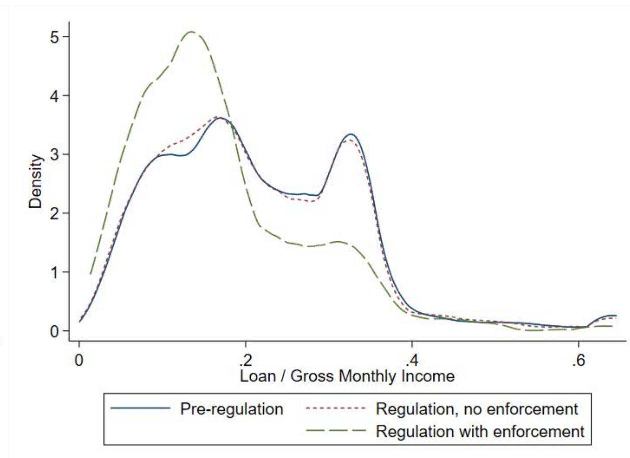
Figure 7: Compliance with City Ordinances After Enforcement Date

Panel A: Loan-to-income Ratio

(a) Austin

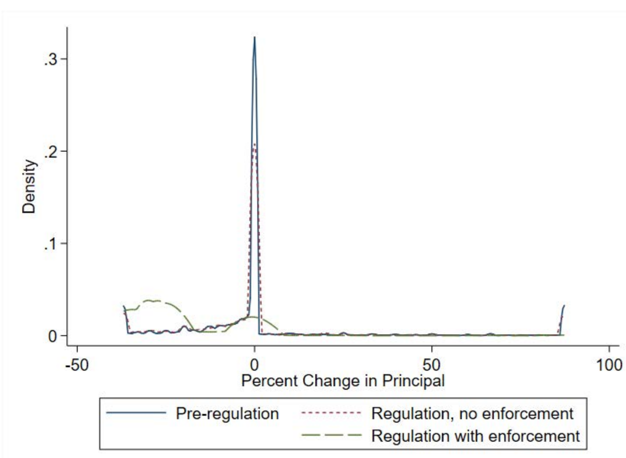


(b) Dallas

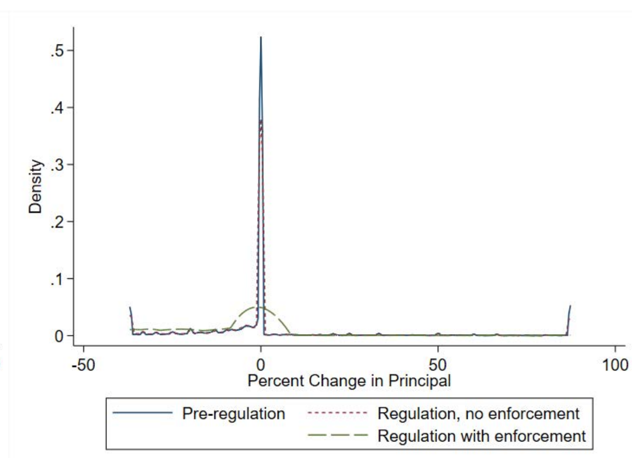


Panel B: Amortization Across Renewals Within Seven Days

(c) Austin



(d) Dallas

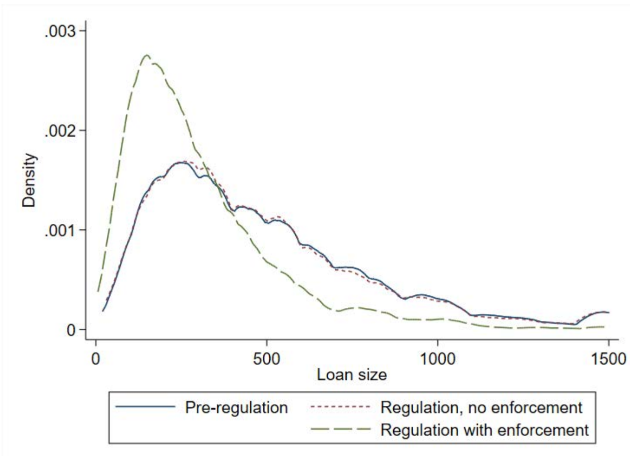


Note: The figure shows distributions of the loan to gross monthly income ratio and the percent change in loan principal for renewals taken within seven days of a prior loan, for three time periods based on the regulation and enforcement of the city ordinances. Both ordinances became effective on January 1st, 2012 (the regulation date), and the enforcement dates were May 1, 2012 for Austin and June 17, 2012 for Dallas. Loan-to-income ratios are winsorized at the 99th percentile, and amortization rates are winsorized at the 5th and 95th percentiles.

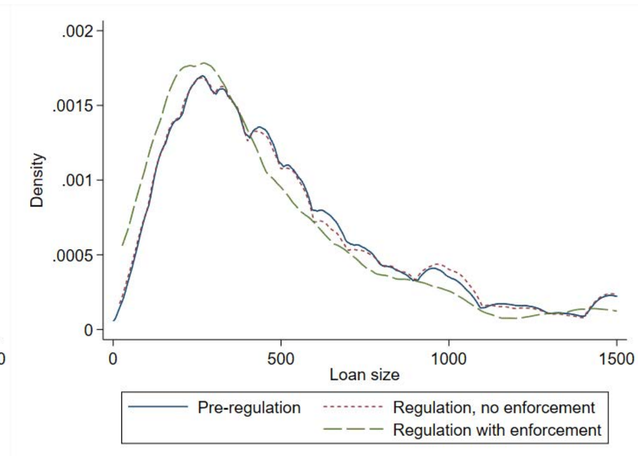
Figure 8: Changes in Loan Size and Income in Austin and Dallas

Panel A: Loan Size

(a) Austin

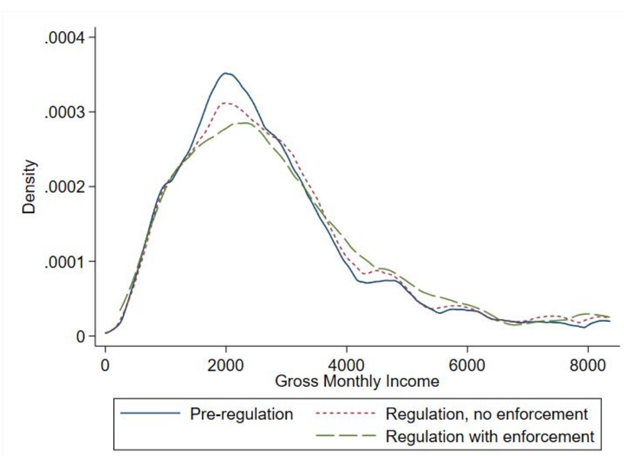


(b) Dallas

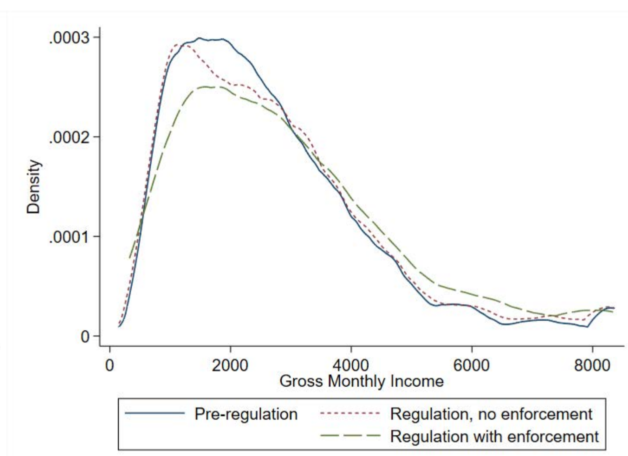


Panel B: Income

(c) Austin



(d) Dallas



Note: The figure shows distributions of loan size and gross monthly income for loans made in Austin and Dallas before and after enforcement of city ordinances. Both ordinances became effective on January 1st, 2012 (the regulation date), and the enforcement dates were May 1, 2012 for Austin and June 17, 2012 for Dallas. Both variables are winsorized at the 99th percentile.

Table 1: Summary Statistics

Panel A: Loan Characteristics

	All			Texas			Non-Texas		
	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.
Loan amount total	$528	$480	$328	$566	$480	$399	$521	$486	$313
Principal	$474	$400	$295	$472	$400	$332	$474	$425	$288
Finance charge	$54	$40	$41	$94	$80	$66	$47	$40	$29
APR	281%	221%	170%	508%	521%	210%	238%	205%	120%
Cost per $100	$12	$11	$5	$20	$20	$0	$11	$9	$4
Contract duration (days)	18.3	14	7.4	17.3	14	7.9	18.5	14	7.3
Renew within 30 days	81%			83%			81%		
Delinquency	36%			46%			34%		
Default	3%			6%			3%		

Panel B: Customer Characteristics and Loan Usage

	All			Texas			Non-Texas		
	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.
Monthly income	$2,097	$1,735	$12,463	$2,459	$1,872	$31,344	$2,031	$1,714	$1,912
Income frequency									
Weekly	17%			11%			18%		
Biweekly	51%			56%			50%		
Monthly	32%			33%			32%		
Total # of loan cycles	5.8	4	5.4	5.9	4	5.5	5.7	4	5.4
Total credit	$2,725	$1,400	$3,624	$2,763	$1,325	$3,893	$2,718	$1,405	$3,572
Total fees	$311	$153	$454	$553	$265	$779	$267	$143	$347
Total days indebted	116	81	101	115	76	104	116	82	100

Table 2: Effects of Disclosures

	(1)	(2)	(3)	(4)	(5)
	Panel A: Loan Volume and Number of Stores				
Sample:	All	All	All	Balanced	Excluding Austin+Dallas
Dependent variable:	$ Volume	# Loans	Stores	$ Volume	$ Volume
TexasPost	- 0.136	- 0.115	0.001	- 0.135	- 0.126
	(0.011)	(0.012)	(0.002)	(0.011)	(0.011)
	[0.000]	[0.000]	[0.672]	[0.000]	[0.000]
	Panel B: Loan Terms				
	Loan Principal	Contract Duration	Cost / $100	APR (1 = 1%)	
Sample mean:	$475	17.1	$20	511%	
TexasPost	- 8.961	- 0.144	- 0.007	0.812	
	(1.855)	(0.074)	(0.008)	(1.302)	
	[0.000]	[0.072]	[0.397]	[0.543]	

Note: The table shows coefficients on indicators for the interaction between Texas and post-2012 from difference-in-difference regressions. The dependent variables in panel A are log loan volume in dollars, log number of loans, and the log of the number of open stores within state-month cells. The dependent variable in column (4) of panel A is log dollar volume in stores that remained open during the entire sample period. The dependent variable in column (5) of panel A is log dollar volume excluding loans made by stores in Austin and Dallas. Dependent variables for panel B are mean loan terms in each state-month cell, and sample averages are reported for Texas prior to the disclosure implementation. Robust standard errors clustered at the state level are in parentheses, and p-values are in brackets.

Table 3: Effects of Disclosures on Loan Performance

	(1) Renewal	(2) Delinquency	(3) Default
Sample mean:	84%	46%	6%
	Panel A: Collapsed		
TexasPost	0.003	0.007	- 0.004
	(0.006)	(0.003)	(0.003)
	[0.605]	[0.044]	[0.250]
	Panel B: Microdata (few controls)		
TexasPost	0.020	0.002	- 0.007
	(0.003)	(0.001)	(0.001)
	[0.000]	[0.071]	[0.000]
	Panel C: Microdata (more controls)		
TexasPost	0.014	0.006	- 0.005
	(0.002)	(0.001)	(0.001)
	[0.000]	[0.000]	[0.000]

Note: The table shows coefficients on indicators for the interaction between Texas and post-2012 from difference-in-difference regressions. The dependent variables in columns (1) through (3) of panel A are the fraction of loans that are renewed within 30 days, delinquent, or defaulted within state-month cells. Panels B and C are linear probability regressions on loan-level microdata, where the dependent variables are 0/1 indicators for renewal, delinquency, and default. The regressions in panel B include only state and month fixed effects as in the collapsed model, and those in panel C include additional controls for paycheck frequency, indicators for each quintile of income and loan amount, and indicators for the number of prior loans in the sequence. Sample averages of the outcome variables are reported for Texas prior to the disclosure implementation. In all regressions, a coefficient of 0.01 represents a 1 percentage point change in the outcome probability. Robust standard errors clustered at the state level are in parentheses, and p-values are in brackets.

Table 4: Heterogeneity in Disclosure Impacts

	(1)	(2)	(3)
	Panel A: Paycheck Frequency		
	Weekly	Biweekly	Monthly
TexasPost	- 0.098	- 0.118	- 0.190
	(0.021)	(0.014)	(0.028)
	[0.000]	[0.000]	[0.000]
	Panel B: Income Level		
	First tercile	Second tercile	Third tercile
TexasPost	- 0.157	- 0.190	- 0.110
	(0.024)	(0.015)	(0.019)
	[0.000]	[0.000]	[0.000]
	Panel C: Distance to Store		
	First tercile	Second tercile	Third tercile
TexasPost	- 0.104	- 0.116	- 0.144
	(0.021)	(0.028)	(0.015)
	[0.000]	[0.001]	[0.000]

Note: The table shows coefficients on indicators for the interaction between Texas and post-2012 from difference-in-difference regressions that replicate the main results for different subsets of borrowers. The dependent variable for all regressions is log loan volume in dollars in each cell of state, month, and customer characteristic. Panel A splits customers by pay frequency, and panels B and C split customers by terciles of income and distance between home address and the address of the payday store. Robust standard errors clustered at the state level are in parentheses, and p-values are in brackets.

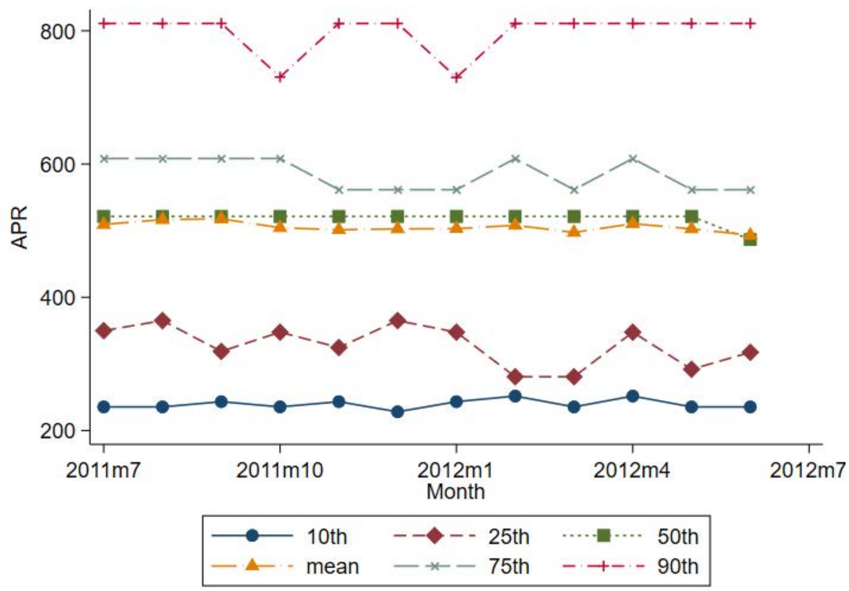
Table 5: Effects of Austin and Dallas Ordinances

	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Regulation Date			
Dependent variable:	$ Volume	# Loans	Loan / income > 20%	Amortization < 25%	Loan principal	Income
Sim. Impact (Austin):	-45%	-21%				
Sim. Impact (Dallas):	-51%	-27%				
Mean (Austin):			43%	91%	$494	$2,744
Mean (Dallas):			50%	91%	$510	$2,717
AustinPost	- 0.243	- 0.101	- 0.107	- 0.216	- 47.2	150.1
	(0.043)	(0.035)	(0.018)	(0.012)	(13.48)	(43.0)
	[0.000]	[0.005]	[0.000]	[0.000]	[0.001]	[0.001]
DallasPost	- 0.075	- 0.080	- 0.020	- 0.012	8.72	34.3
	(0.053)	(0.038)	(0.023)	(0.012)	(13.62)	(38.9)
	[0.156]	[0.037]	[0.369]	[0.317]	[0.522]	[0.379]
			Panel B: Enforcement Date			
AustinPost	- 0.955	- 0.405	- 0.291	- 0.707	- 205.8	91.6
	(0.083)	(0.073)	(0.015)	(0.012)	(17.68)	(70.7)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.196]
DallasPost	- 0.218	- 0.156	- 0.064	- 0.024	- 20.30	14.2
	(0.054)	(0.048)	(0.020)	(0.013)	(13.11)	(63.7)
	[0.000]	[0.001]	[0.002]	[0.057]	[0.123]	[0.824]
			Panel C: Enforcement Date - Geographic Spillovers			
AustinPost	- 0.887	- 0.391	- 0.297	- 0.617	- 196.0	112.8
	(0.068)	(0.063)	(0.016)	(0.014)	(17.72)	(71.7)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.117]
AustinBordPost	0.114	0.088	0.005	0.006	11.31	0.2
	(0.105)	(0.098)	(0.024)	(0.011)	(9.19)	(42.8)
	[0.282]	[0.372]	[0.821]	[0.599]	[0.219]	[0.997]
DallasPost	- 0.184	- 0.119	- 0.038	- 0.021	- 29.5	- 58.8
	(0.048)	(0.039)	(0.018)	(0.011)	(8.81)	(73.2)
	[0.000]	[0.002]	[0.035]	[0.053]	[0.001]	[0.422]
DallasBordPost	0.003	- 0.020	0.032	- 0.001	8.89	- 20.5
	(0.027)	(0.024)	(0.015)	(0.006)	(7.65)	(28.1)
	[0.916]	[0.407]	[0.032]	[0.903]	[0.246]	[0.467]

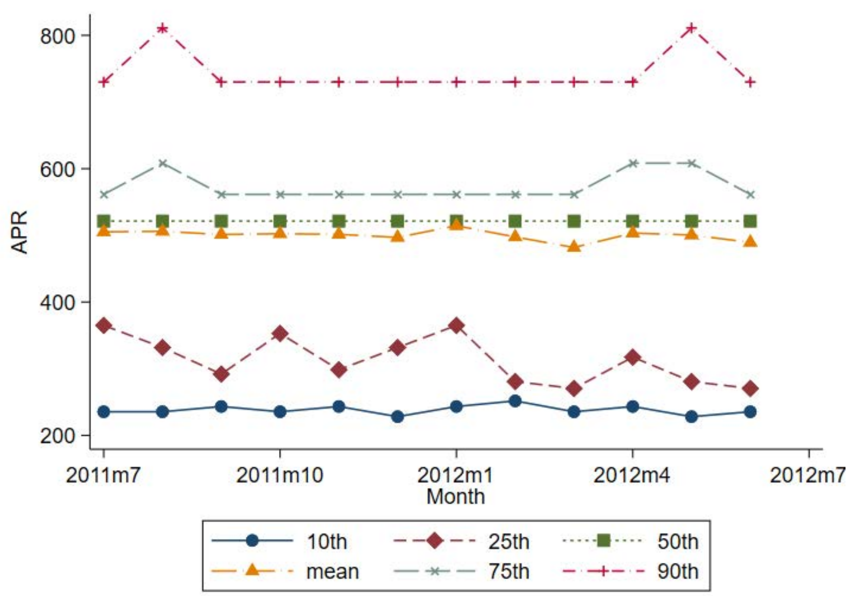
Note: The table shows coefficients on indicators for the interaction between stores in Austin and Dallas and the post-regulation or post-enforcement periods for regressions that include each store-week within Texas. The regulation date for both ordinances was January 1st, 2012, and the enforcement dates were May 1, 2012 for Austin and June 17, 2012 for Dallas. The regulation dates are used for panel A, the enforcement dates are used in panel B. Panel C uses the enforcement dates, while also including indicators and post-enforcement interaction terms for stores that are not in Austin or Dallas but within 20 miles of city borders. The dependent variables for each column in order are log weekly loan volume in dollars, log weekly number of loans, the fraction of loans with loan to gross monthly income ratio above 20%, the fraction of loans with less than 25% amortization, mean loan size, and mean customer income within store-week cells. Robust standard errors clustered at the store level are in parentheses, and p-values are in brackets.

Figure A1: Price Distribution in Austin and Dallas

(a) Austin



(b) Dallas



Note: The figure shows the distribution of APRs in Austin and Dallas during the sample period.

Table A1: Heterogeneous Effects By Census Tract Demographics

	(1)	(2)	(3)	(4)	(5)	(6)
	Consumer tract			Store tract		
	First tercile	Second tercile	Third tercile	First tercile	Second tercile	Third tercile
	Panel A: Percent college					
TexasPost	- 0.116	- 0.093	- 0.153	- 0.120	- 0.126	- 0.137
	(0.019)	(0.032)	(0.021)	(0.021)	(0.013)	(0.019)
	[0.000]	[0.012]	[0.000]	[0.000]	[0.000]	[0.000]
	Panel B: Percent white					
TexasPost	- 0.122	- 0.136	- 0.188	- 0.108	- 0.144	- 0.168
	(0.018)	(0.015)	(0.081)	(0.016)	(0.023)	(0.022)
	[0.000]	[0.000]	[0.038]	[0.000]	[0.000]	[0.000]
	Panel C: Average household income					
TexasPost	- 0.096	- 0.121	- 0.132	- 0.126	- 0.153	- 0.127
	(0.024)	(0.016)	(0.019)	(0.014)	(0.019)	(0.017)
	[0.001]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
	Panel D: Unemployment rate					
TexasPost	- 0.127	- 0.142	- 0.076	- 0.143	- 0.158	- 0.109
	(0.020)	(0.020)	(0.044)	(0.041)	(0.017)	(0.013)
	[0.000]	[0.000]	[0.105]	[0.004]	[0.000]	[0.000]

Note: The table shows coefficients on indicators for the interaction between Texas and post-2012 from difference-in-difference regressions that replicate the main results for different subsets of borrowers based on census tract demographics from the American Community Survey from 2010 to 2014. Columns (1) through (3) use the census tract of the customer's home address, while columns (4) through (6) use the census tract of the store's address. The dependent variable for all regressions is log loan volume in dollars in each cell of state, month, and demographic characteristic. Panel A splits census tracts by fraction of population with college education and above. Panel B splits by percent white population tercile, panel C splits by average household income, and panel D splits by the unemployment rate. Robust standard errors clustered at the state level are in parentheses, and p-values are in brackets.