The Effects of Disclosure and Enforcement on Payday Lending in Texas *

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Abstract

Inspired by Bertrand and Morse (2011), Texas adopted an information disclosure for consumers taking out payday loans in January, 2012. The disclosures compare the costs of payday loans with other credit products and present their likelihood of renewal in easy-to-understand terms. Simultaneously, the cities of Austin and Dallas implemented stricter supply restrictions within their city limits. We find that the statewide disclosures led to a significant and persistent 13% decline in loan volume in the first six months after implementation, with no changes in prices or other detectable supply responses. The city-specific supply restrictions lead to a 62% decline in loan volume in Austin and a smaller drop in Dallas, with the timing of the effect driven by the start of enforcement rather than the date of enactment. The results suggest that some payday borrowers have imperfect expectations about the costs of borrowing, and that disclosures can have a significant impact on demand in a marketwide setting with no detectable evasion by lenders. We provide some of the first evidence on the significance of enforcement in consumer financial regulation.

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I Introduction

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 2016, Campbell, Jackson, Madrian and Tufano 2011). 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.

While disclosures primarily aim to provide information, recent examples in consumer finance are also designed to counteract consumer 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 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 Bertrand and Morse's study be given to consumers before every payday loan transaction. The law went into effect on January 1, 2012. 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 Texas disclosures led to a 13% decline in loan volume that remains persistent for at least six months after they were first implemented. The effects are driven by the extensive margin, suggesting that the disclosures discouraged a subset of

[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 and Sydnor (2013), and Mann (2013).

[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 APR of a payday loan to the APRs of other forms of consumer credit, and the refinancing treatment presented borrowers with typical repayment and refinancing behavior. A saving planner treatment was also tested alone and in combination with the information treatments but had no statistically significant effects on the likelihood of borrowing or amount borrowed.

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.

We test for heterogeneity in the treatment effect of disclosures across proxies for the costs of borrowing and consumer liquidity constraints, and find that borrower liquidity does not seem to be an important driver of responsiveness to the disclosures. However, borrowers with monthly loan cycles, who are more likely to enter into long-term, non-amortizing debt sequences, respond 50% more to the disclosures than those with biweekly loan cycles. We find no evidence that the effects of the disclosures are driven by changes in lender strategy or other supply responses as opposed to changes in demand.

The statewide effects are also not driven by concurrent city ordinances in Austin and Dallas that placed explicit restrictions on the supply of credit. While the effective date of the ordinances was also January 1, 2012, we do not see loan characteristics change to comply with the ordinances until enforcement begins in each city (May 1, 2012 for Austin, June 17, 2012 for Dallas). The ordinances reduce the supply of loans, without spillover of borrowing to neighboring areas.

Our study represents a joint test of the existence of imperfectly-informed consumers, the effectiveness of one particular set of disclosures, and the short-run responses of lenders. The results suggest that some payday borrowers under-estimate the costs of payday loans, and choose not to borrow once they are exposed to de-biasing information. Despite lender incentives for obscuring or counter-acting their effects, paper disclosures given to consumers at the time they take out a loan can have a significant impact on consumer decision-making. We find no effects on price competition.

This paper contributes to three strands of literature: on the effects of disclosures, on use of payday loans, and on the role of enforcement in compliance with financial regulation. Disclosures can reduce consumers' ignorance of contract terms and firms' ability to shroud the cost, but their effects on loan volume and prices are typically small. Examining one of the first federally mandated consumer financial disclosures, Stango and Zinman (2011), find that the 1968 Truth in Lending Act (TILA), which requires creditors to make written disclosures of finance charges in a standard format, reduces differences in price of credit between more- and less-biased consumers. A newer disclosure mandated by the CARD Act of 2009 requires that credit card bills display payoff duration

calculations. Agarwal, Chomsisengphet, Mahoney and Stroebel (2014) and Keys and Wang (2019) find providing this information leads to small increases in consumer payments. Consistent with Bertrand and Morse (2011), but at a market-wide level, we find a disclosure aimed at de-biasing payday borrowers results in a small but significant reduction in borrowing volume.

Many factors can limit the impact of disclosures, including a lack of consumer attention (Adams, Hunt, Palmer and Zaliauskas (2019)) or financial literacy (Lacko and Pappalardo (2010)), or manipulation of the disclosure by the lender or firm (Jin, Luca and Martin (2018) and Persson (2017)). Elizondo and Seira (2014) find no effect of APR disclosure or peer comparison messages on the delinquency of indebted credit card borrowers in Mexico. Kulkarni (2020) finds disclosures reduce the delinquency of consumer loans in Chile, but that the effect is much stronger for financially sophisticated borrowers. The reduction in payday borrowing we see from the disclosure in Texas may also have negative spillover effects that we do not capture. Medina (2018) finds that credit card payment reminders reduce the probability of late payments, but that prioritizing credit card payments led to an increase in overdraft fees for some borrowers, shifting them into other, potentially higher-cost credit.

Disclosures have also been shown to affect firm behavior. Stango and Zinman (2011) find that interest rates overall rose because of the costs of compliance with TILA, for an ambiguous net effect on the price of credit.[4] Unlike Stango and Zinman (2011), we find no change in prices over a six-month period following the payday disclosure regulation. This paper also contributes to the literature on the rationality of payday borrowers, the welfare effects of payday borrowing, and the impacts of regulation in the payday market. Our results are consistent with previous evidence documenting that some payday borrowers have imperfect expectations about the costs of borrowing, and their borrowing reflects behavioral biases.[5]

[4]For examples of the effect of disclosures on firm behavior outside of consumer finance, (Newell, Jaffe and Stavins 1999) find disclosures on the energy of household appliances raises the efficiency of appliances produced, and (Jin 2003) find disclosures on restaurant hygiene raise their compliance with health regulations.

[5]The choice to take a payday loan reflects bias if lower-cost credit is available. Agarwal, Skiba and Tobacman (2009) find that most payday borrowers have access to credit card liquidity on days they borrow. Similarly, Miller and Soo (2020) find that increased access to mainstream credit does not reduce payday loan usage among borrowers who have a bankruptcy flag removed. While Carvalho, Olafsson and Silverman (2019) find that payday borrowers in Iceland are illiquid before they get a loan, they also find that a subset of borrowers spend the bulk of their loan on inessential items. The repayment and default behavior of borrowers may also reflect bias. Carter et al. (2013) find that in violation of rational expectations, increasing the repayment period on a payday loan without changing the price has very little effect on rollover probability. Skiba and Tobacman (2008) find that rollover and default patterns reflect over-optimism about self-control or future shocks. Finally, Mann (2013) surveys borrowers and shows

Our findings do not contradict studies that find positive welfare effects or rational uses for some consumers.[6] Nor do our results confirm studies that find negative, or no, welfare effects.[7] Our results showing different effects of disclosures on subgroups of consumers suggest that the welfare effects of payday loans may be highly heterogenous. Such heterogeneity may explain the mixed evidence in the literature on whether payday loans on average have positive, negative or null effects on consumer welfare.

Our findings on the effect of municipal payday lending restrictions in Austin and Dallas contributes to literature on the effect of government enforcement on compliance with regulation. Much of this literature is in the area of environmental or labor regulation.[8] In the consumer finance context, 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 (2007) suggests that the strength of those mechanisms may reflect preferences of the regulated industry rather than the good of consumers; using penalties for usury as a proxy for enforcement, Benmelech finds that states change both allowable rates and enforcement intensity at the behest of private interests. Consistent with Burke, Lanning, Leary and Wang (2014), we find that restrictions on rollovers have little effect on the duration of loan sequences in Austin and Dallas.

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.

that heavy users of payday loans have poor ability to predict how long they will be in debt and are more likely to underestimate the costs of borrowing.

[6]Many researchers have found negative effects from payday lending bans or restrictions. Zinman (2010) finds that a restriction on payday loans in Oregon worsened the self-perceived financial outlook of borrowers compared with those in Washington, Morse (2011) finds that payday access is associated with lower rates of foreclosure and larceny after natural disasters, and Morgan, Strain and Seblani (2012) find that payday bans increase overdrafts and bounced checks.

[7] Melzer (2011) finds that payday access worsens consumers' ability to pay bills, and Carrell and Zinman (2014) find that payday access worsens the job performance of military airmen. Melzer (2017) find that payday access has spillover effects, increasing the likelihood of a household using food assistance and not receiving child support due from a non-resident parent. Gathergood, Guttman-Kenney and Hunt (2018) find that online payday loans in the United Kingdom cause consumers to overdraft in the months afterward. On the lack of welfare effect, Bhutta, Skiba and Tobacman (2012) and Carter and Skimmyhorn (2016) show that eligibility for payday loans has no effect on credit scores and that payday access has few measurable effects on service-members. Bhutta, Goldin and Homonoff (2016) find that bans on payday lead borrowers to simply shift to other high cost credit. Desai and Elliehausen (2016) finds minimal increases in delinquency after state payday loan bans.

[8]The literature is extensive; an example in the environmental context is Shimshack and Ward (2005), who find penalties for water pollution violations have a strong statewide deterrence effect. In the area of labor, Weil (2018) examines the effect on wage theft of a strategic enforcement approach, which incorporates a wide set of non-government enforcement tools.

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)).

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 or by a payday lender partnering with a bank based in a payday-allowing state to "import" the state's regulations and rate restrictions (a practice the FDIC ended in the mid-2000s).[9] 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 (Kaufman 2013), and 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,[10] 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 rebranding as Credit Access

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

[10] See Montezemolo (2013), or Bourke, Horowitz, Lake and Roche (2013)

Businesses (CABs). As CABs, payday lenders charge borrowers fees to set up low-interest loans with a third party; for practical purposes, CABs act as payday lenders.

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 HB 2594, payday lenders operating as CABs were required to register with the Secretary 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 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 (see below) were granted licenses and continued to operate in Texas.

Figure 1 shows an example of the mandatory disclosures.[11] 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 TILA, 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

[11]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. (From interview with state official in the Office of the Commissioner of Consumer Credit, February 2014)

repayment behavior from a nationwide survey. Informing borrowers that 4 out of 10 people will renew four times or more 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, since they did not change any characteristics of the product or limit the use of the loans.[12] In response, Austin and Dallas passed municipal ordinances to limit the loans that could be offered within their city limits. Austin and Dallas's ordinances, passed in June and August 2011 respectively, restricted loans to 20% of a borrower's gross monthly income and required 25% amortization of the principal with each renewal. Though both took effect the same day as the state law, 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.[14] 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 change in Texas.[15] Information on each loan includes the principal amount, total fees, the date the loan was originated, the date the loan was due to be repaid, and the date the loan was actually repaid. We exclude loans from states other than Texas that changed their payday lending laws during the time frame.[16]

The dataset includes anonymized customer identifiers that allow us to identify all loans made

[12] Wendy Davis, an outspoken opponent of the industry, 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.

[13] See Texas Municipal League Payday Lending Clearinghouse for a list of the cities and information on the 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.

[15] Only payday loans extended via storefront establishments are included in the dataset. Online loans are excluded.

[16] These states are Arizona, Colorado, Delaware, Illinois, Oregon, and Wisconsin.

to the same consumer by a given lender during the observed time period. Payday lenders typically collect very limited information about borrowers and restrict their collection to 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 Burke et al. (2014) and Consumer Financial Protection Bureau (2013).[17]

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 an average total repayment amount of $528. Principal amounts in Texas (mean of $472) are similar to other states ($474). While many states place a cap on fees (typically $15 per $100 borrowed), Texas does not. 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 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) higher than in other states ($2,031). We divide consumers into five groups that combine their reported income source and pay frequency.[18] Consistent with the median loan length of 14 days reported above, 51% of borrowers are employed and paid either biweekly or semi-monthly. Of the other 35% of borrowers who are employed, pay frequency is split evenly between weekly and monthly. Eleven percent of the borrowers receive Social Security income. The remaining three percent of borrowers receive income from other sources, which are mostly comprised of non-Social-Security retirement, unemployment, and disability benefits. Borrowers in Texas are more likely to be paid biweekly and less likely to be paid weekly, but the distribution across income source and pay frequency groups is otherwise similar to the sample average. Borrowers in our sample use about six loans per year on average, which corresponds to $2,725 in total principal, $311 in fees, and 116 days of

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

[18] Due to the nature of payday loans, both income source and pay frequency are precisely measured in our data. In order to obtain a payday loan, borrowers must present a pay stub from a regular income source. Loans are typically due on a borrower's next payday, so loan duration is a function of pay frequency.

indebtedness.[19]

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 the dynamics 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. 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 time series. The figure shows normalized loan volume for Texas and other states grouped by region. For each 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 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 parallel trends assumption, we implement a regression framework of the following form to estimate the causal effect of the Texas disclosure requirement:

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

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

[19] See Burke et al. (2014) for more details on loan usage among payday borrowers

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 our 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. 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. 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 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.[20] 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 effect on statewide

[20]Indeed, a major lender cited restrictive city ordinances as a cause of 36 store closings in 2013 (Cash America Annual Report, 2013)

loan volume is very similar. We devote the next section 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 prices to drive down the price. The results show a small decline in average loan principal and negligible changes in contract duration and price. Consistent with the regression results, Figure 5 shows the distribution of APR's in Texas for our sample period, with almost no change around the time of the reform. The lack of major changes in contract terms support the interpretation that the disclosures had few effects on 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 4 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 final 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. While we cannot disentangle these explanations completely, the evidence that most of the effect comes from the extensive margin suggests that the final explanation may be the most likely one, and it is consistent with the rest of the evidence we will 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 drop in May and June are driven in part by the enforcement of local ordinances (see Section V for more details).

IV.C Effects of Disclosures on 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 (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 compared to the microdata regressions described below, 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.

Since loan performance could vary based on consumer characteristics and borrowing history 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 and month fixed effects as in 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, 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 most impacted by 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 total loan volume as the outcome, stratifying by borrower characteristics. Panel A presents the heterogeneity by paycheck frequency, showing that 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 stastistically indistinguishable. Monthly borrowers, which include 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, APR's are decreasing in paycheck frequency. Thus, consumers with lower APRs based on having monthly pay frequencies actually respond the most to the disclosures. Since APR's 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 since there is a lack of common support across states. However, it provides suggestive evidence suggests 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.

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 more interventions, as harms may be spread out across demographic groups. Our results

caution against using a simple income-based metric to restrict loans to low-income consumers, as we find no evidence that they are the ones least-informed about the costs and consequences of payday loans.

V The Effects of City Ordinances

We next turn to the impact of local ordinances in Austin and Dallas effective 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 passed on June 22, 2011, and Austin's Ordinance 20180818-75 was passed on August 18, 2011. Instead of just providing information to consumers, both ordinances limite the size of payday loans to 20% of a borrower's gross monthly income, and require at least 25% principal amortization each time a loan is renewed or reborrowed within seven days of paying off a prior loan. Since 25% of the principal has to be repaid with each renewal, the amortization requirement effectively limits each loan sequence to a maximum of three renewals and four total loans.

The city ordinances allow us to analyze the impact and incidence of binding supply constraints that are similar in spirit to 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 regulation that have thus far been largely overlooked in the literature.

V.A Timing and Compliance

While the city ordinances were passed with an effective date of January 1st, 2012, enforcement did not begin until several months later. Both cities were sued before the effective date by the Consumer Service Alliance of Texas, a trade association composed of payday lenders. The lawsuits did not prevent Austin or Dallas from enforcing their ordinances because neither case included a

preliminary injunction (which would have required a pause in enforcement until the resolution of the court case).[21]

Nonetheless, Austin did not begin enforcement until May 1, and Dallas until June 17, 2012, based on official reports.[22] The organization and intensity of enforcement varies across these cities today. Austin has full-time employee who monitors compliance with the local ordinance by investigating consumer complaints and auditing lenders. Enforcement in Austin began promptly after May 1.[23] Enforcement in Dallas is handled jointly by two Department of Code Compliance employees who work on consumer protection issues more generally, and the Community Prosecution Team of the Dallas Police Department (composed of attorneys and enforcement officers who address quality of life issues through code enforcement). According to one of the ordinance's most vocal backers, Dallas City Councilperson Jerry Allen, enforcement in Dallas was slower to start.[24]

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 reduced ordinance impact in Dallas versus Austin is broadly consistent with fewer enforcement resources, and weaker overall enforcement. With penalties for violating the ordinance the same in each city ($500 per day), the likelihood of actually 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

[21] Cities in Texas classified as 'home rule' have wide latitude to pass regulations as long as the regulation is not specifically pre-empted or prohibited by the state. Both Dallas and Austin are home rule cities.

[22] 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 appeals court response to the suit against the City of Dallas referenced the date that the Dallas notified lenders the city would begin enforcement.

[23] Between May 2012 and December of 2013, Austin referred 4 complaints for prosecution by the city attorney. See https://www.houstonchronicle.com/news/houston-texas/houston/article/Houston-vows-speedy-enforcement-of-proposed-loan-5070223

[24] In December 2013, Allen said 'Without question, it's now time for enforcement.' Enforcement eventually picked up in Dallas: by July 2014 the city had conducted 72 code inspections and 6 financial audits of payday lenders, issued 34 citations, and referred one lender for prosecution. See https://www.houstonchronicle.com/news/houston-texas/houston/article/Houston-vows-speedy-enforcement-of-proposed-loan-5070223

or Dallas:

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

where 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 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 county. The coefficients prior to the regulation date in January 2012 fluctuate, 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, suggesting 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 show that even though the ordinances were legally in effect, ongoing lawsuits and the administrative effort required to create and implement a compliance strategy kept local regulators from enforcing them, and lenders strategically delayed compliance until they knew enforcement would begin. 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 law, we examine the two loan features targeted by the ordinances: the loan to income ratio and principal amortization rate. Figure 7 compares these distributions for loans made under three regimes: pre-regulation (prior to 1/1/2012), regulation with no enforcement (1/1 to 4/30 for Austin and 1/1 to 6/16 for Dallas), and regulation with enforcement (starting 5/1/12 for Austin and 6/17/12 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. Thus, 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 with some bunching 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 change in the distribution of gross monthly income around the 20% limit suggests that the income information we see is the one used by lenders in determining loan size. 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.

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.

Compliance with the provision requiring 25% amortization with each renewal is harder to measure, as we cannot always observe the beginning of loan sequences. 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.[25] 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 et al. (2014), which found that for more than 80% of loan sequences lasting more than one loan, the last loan is the same size or larger than the first.While the distributions shift down-

[25]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.

ward 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 in the post-enforcement period.

One concern is that lenders might evade the loan to income restrictions by falsifying borrower incomes, leaving actual loan activity unchanged. Taking reported income at face value, it is also useful to decompose the effects of the requirements on changes in the composition of customers versus the loan amounts borrowed. We plot the distributions of loan size and income in Figure 8 to verify whether the effects on loan to income ratios are due purely to changes in reported income or to shifts in loan amounts.

As shown in subgraph (a), the loan size distribution shifts down in Austin, consistent with both 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, 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, overall the income distribution shifts only modestly compared with loan size, which is not affected by falsification because it comes directly from lenders' administrative data. Overall, the results suggest that compliance with the ordinances is driven by changes in loan size rather than borrower composition, and we find no significant signs of evasion driven by income falsification. 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.

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

While we do not have enough information to look into the intensity of enforcement in Austin and Dallas, the different structure of the regulatory authority may explain some of the differences we see between the two cities in the effect of the identical regulation. In future research into the interplay of regulation, enforcement, and compliance, it may be interesting to gather 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 any incentives for (or

roadblocks to) the borrower reporting illegal activity. Enforcement action through the courts is often a last resort to stop repeated lender misbehavior, so the number of such actions might not provide a full picture of enforcement activity.

V.B Main Results

We next use a difference-in-differences framework to formally estimate the marketwide effects of the ordinances and test for spillovers to neighboring areas. In particular, we compare the estimated effects when using the regulation dates, as in most of the literature, with whose using the enforcement dates, which we showed above corresponded to actual compliance with the law.

Table 5 shows the results of regressions of the following form:

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

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 in each city. The coefficients β_A and β_D reflect the differential change in outcomes in 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 county 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 provide simulated estimates of their impact based on loans in 2011. For each loan we observe, we construct a simulated loan based on the maximum principal that can be achieved while complying with the loan to income and amortization requirements. If the loan already had three or more 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, by comparing the aggregate simulated loan amount to actual loan volume. Column (2) estimates the number of loans that would be completely eliminated by imposing only the amortization requirements. The results show that dollar loan volume would be expected to decline by 45 to 51%, while the number of loans would decline by 21 to 27%.

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 cannot 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 comparison for the expected magnitude of the effects if there were full compliance. The magnitude of the potential impact shows that the restrictions imposed by Austin and Dallas affect a large fraction of loans.

We next present the regression results using the regulation date of January 1st, 2012. The results show that dollar loan volume declines by 0.24 log points (22%), while the number of loans declines by 0.10 log points (10%) in Austin. In columns (3) and (4), the dependent variables are the average number 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 fraction of loans exceeding the 20% loan to income threshold declines by 11 percentage points, and fraction with less than 25% amortization declines by 22 percentage points. The two rows above the regression estimates show the sample means in the 2011 period, showing that 43% and 91% of loans exceeded these thresholds in the pre-period Austin, so the level of compliance was far from complete when using only the regulation dates. The results also show that loan principal declined by $47 and average customer income increased by $150 in the post-regulation period in Austin. The effects in Dallas are small and largely insignficant 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 with over 20% loan to income declined by 29 percentage points, and the fraction with less than 25% amortization declined by 71 percentage points, showing that the majority of loans complied with the ordinances after enforcement began. Columns (5) and (6) show that average loan principal declined dramatically by $206 in Austin, which was more than forty percent of the pre-period

mean of $494. Average customer income increased by $91 based on a pre-period mean of $2,744 per month, but this result is not significant at the 10% level. Overall, the effect sizes in Austin show dramatically 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 significant when appropriately using the enforcement 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 compared with Austin, but that could be the result of not observing a sufficient post period to allow lenders to adjust to enforcement of the regulations. We also find only moderate effects on loan size and average income in the two weeks after enforcement. 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.

A key result is that the effect sizes are dramatically 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 incorrect conclusions about the effects of regulation. If we had relied on the regulation dates alone, we may conclude that lenders were evading or failing to comply with the regulation in Dallas, and that compliance was at best incomplete in Austin. Instead, by using the enforcement dates we show that the ordinance was mostly complied with and the changes in overall loan volume were on par with what would be expected under full compliance.

Consistent with the lack of lender evasion, we find no evidence of mis-reported income, and find that most of the ordinances' effect comes through lower loan amounts that are unlikely to be mis-measured.

V.C Spillovers

One significant concern with both city-level policy and interpretation of our results is that the effectiveness of ordinances may be over-estimated if borrowers simply cross city lines to avoid the loan restrictions, in which case observed reductions in loan volume would be mirrored by increases

in lending outside of city limits. Additionally, many lenders have multiple locations and could direct a borrower to a branch location outside the city for a loan or renewal that does not need to conform to the ordinance.[26] There is some evidence that borrowers are willing to cross state lines to obtain a payday loan if their state does not allow it.[27] Whether consumers are willing to cross municipal lines for a payday loan with fewer restrictions than what is available in their hometown is not well-established.

We test for the presence of 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 the post-enforcement periods in Austin and Dallas, and $AustinBordPost$ and $DallasBordPost$ are indicators for a 20 mile radius outside the cities interacted with the post-enforcement periods. 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 D 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. Overall, the results suggest few spillovers during the period after enforcement of the ordinances.

VI Conclusion

This paper examines the effects of behaviorally-motivated payday loan disclosures. We find that disclosures implemented in Texas led to a 13% decline in loan volume. Borrowers paid on a monthly

[26] 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.

[27] Much of the literature examining the effects of payday loan restrictions at the state level estimates the effect using geographic distances in border areas, which requires an assumption that borrowers will cross lines. One example of this approach, Melzer (2011), justifies the assumption with an analysis that finds a higher number of payday loan stores on the border of states where such lending is banned

basis through employment or Social Security benefits respond most to the disclosures, suggesting that the information is most salient to consumers likely to enter into long-term, non-amortizing loan cycles. In contrast, the responsiveness to disclosures is negatively related to loan APRs, and has no clear relationship to borrower income.

Our results suggest that some payday borrowers have imperfect expectations about the costs of payday borrowing, and that disclosures can have an economically meaningful effect on consumer choices. However, it is possible that not all consumers who could benefit from new information change their behavior as a result of the particular set of disclosures that were implemented. Thus our results should be interpreted as a lower bound on both the fraction of imperfectly-informed consumers and on the potential effectiveness of disclosures.

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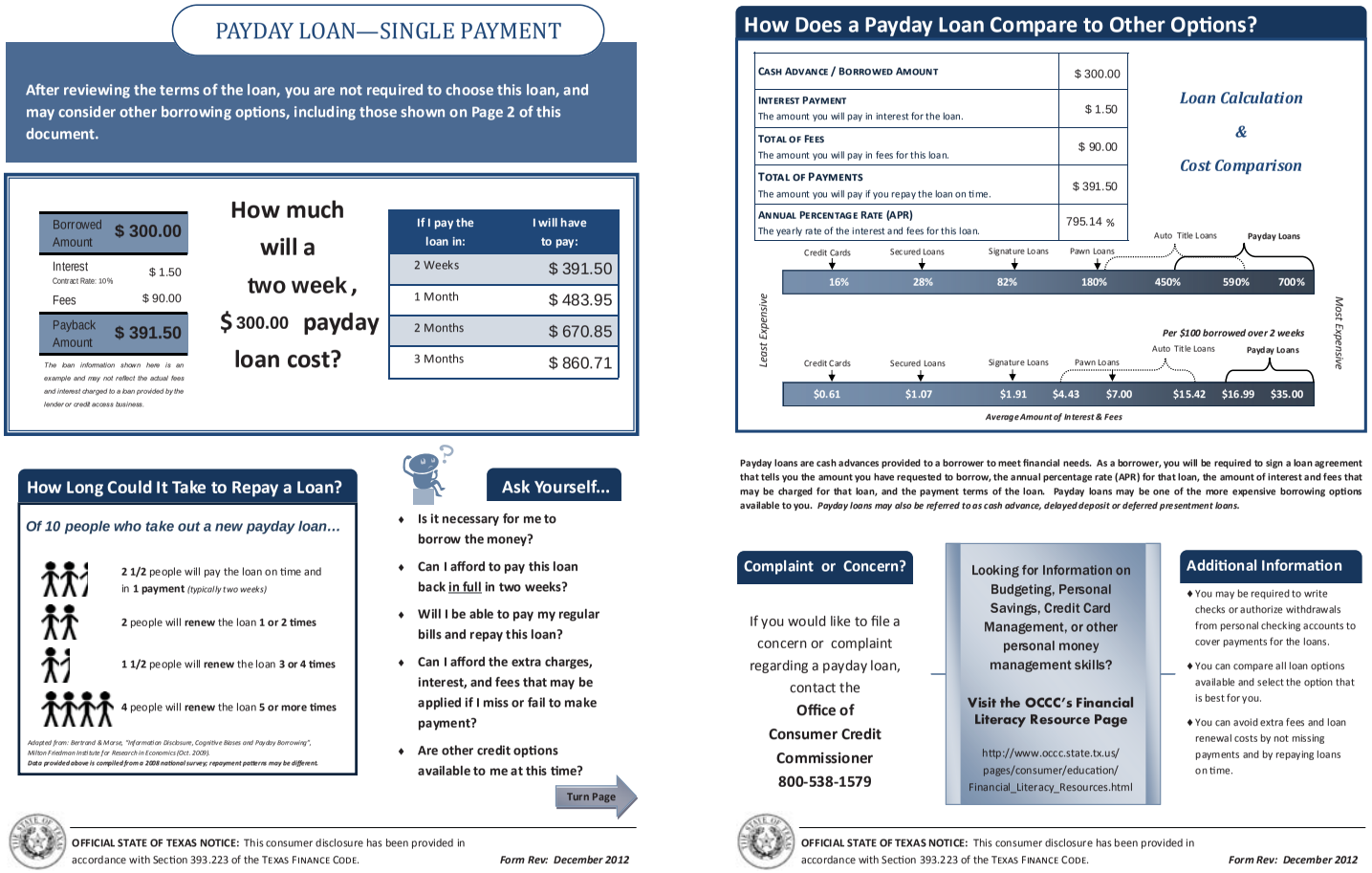
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Figure 1: Example of Texas Disclosure



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 in full 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

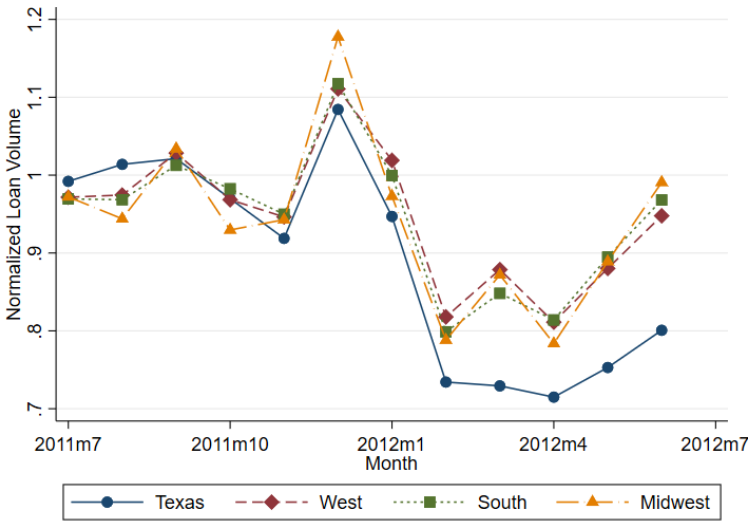
Additional Information

- 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.

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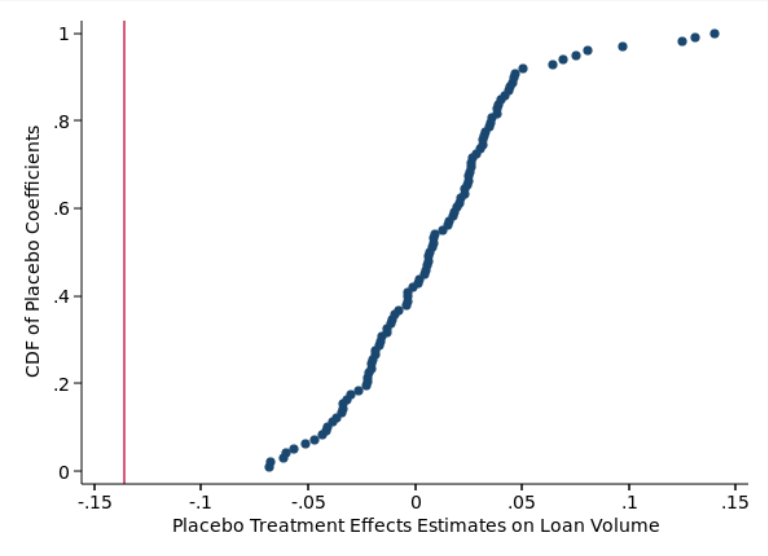
Source: https://www.mybridgeprovider.com/fees.html

Figure 2: Loan Volume in Texas vs. Other Regions



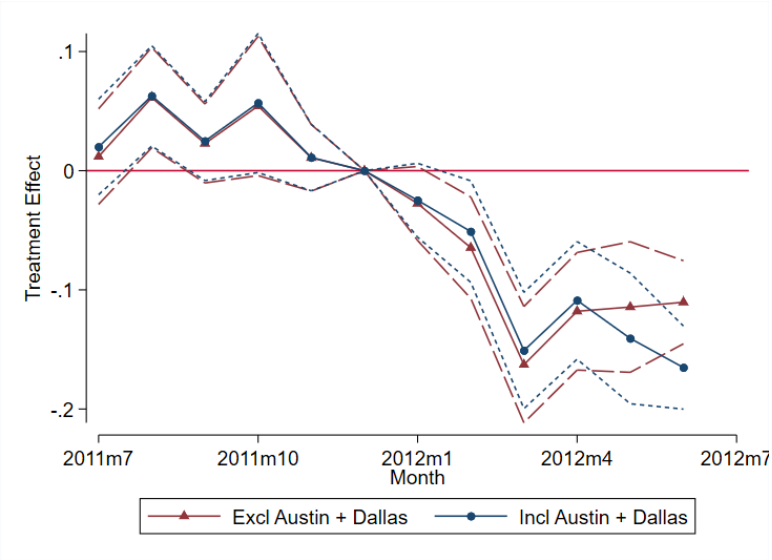
Note: The figure shows normalized loan volume originated in Texas and three other regions between July 2011 and June 2012. The western region of states that allow payday loans during this period includes CA, NV, UT, ID, WA, and WY. The southern region includes AL, FL, KY, LA, OK, TN, MS, SC, and VA. 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



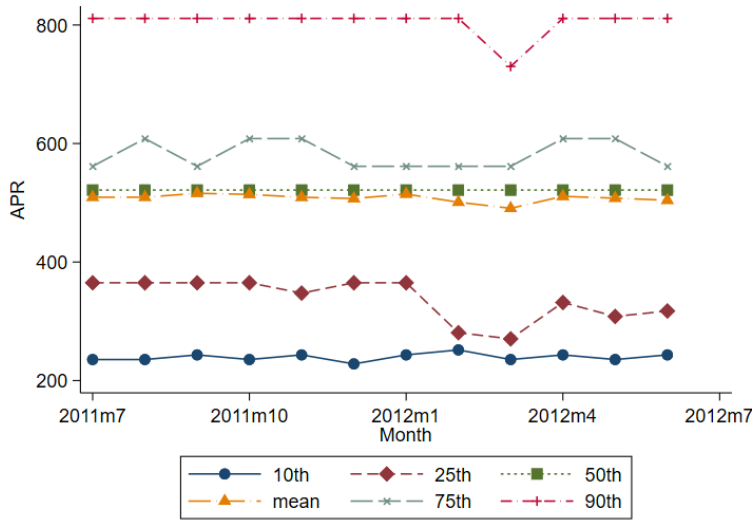
Note: The figure shows the distribution of difference-in-difference regressions with placebo treatment indicators for each state and each placebo date between September 2011 and March 2012. Treatment indicators for Texas are excluded. The red line indicates the estimated effect for Texas in January 2012.

Figure 4: 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.

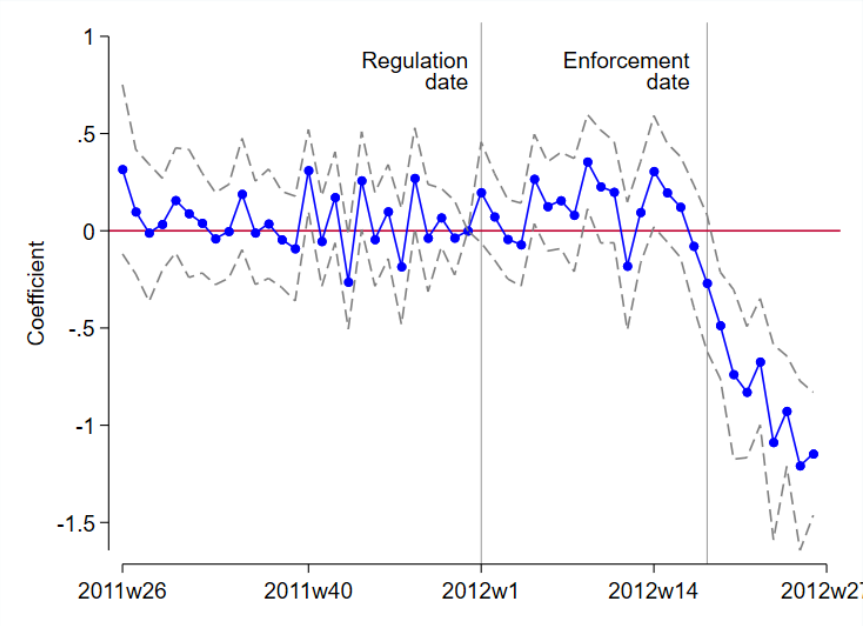
Figure 5: Price Distribution in Texas



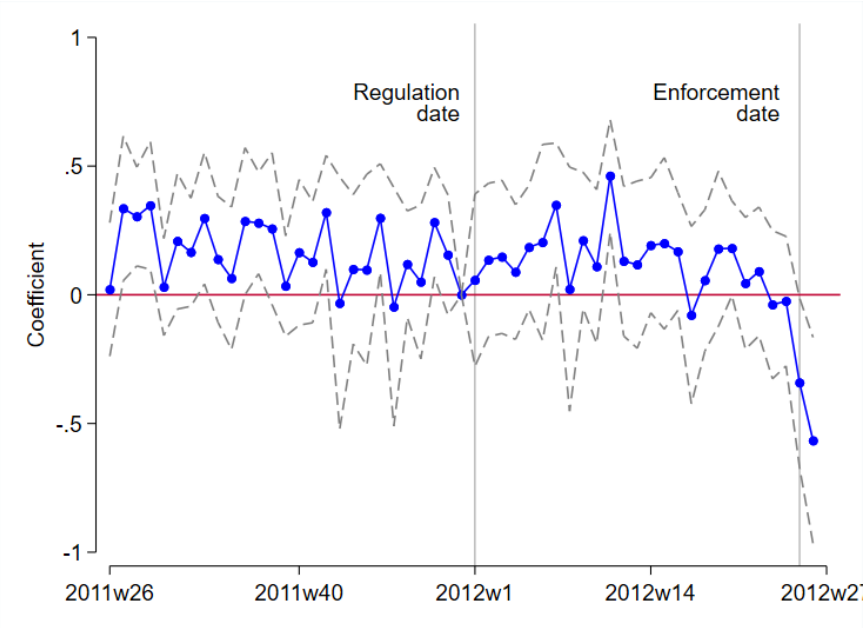
Note: The figure shows the distribution of APRs in Texas during the sample period.

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

Panel A: Austin



Panel B: Dallas

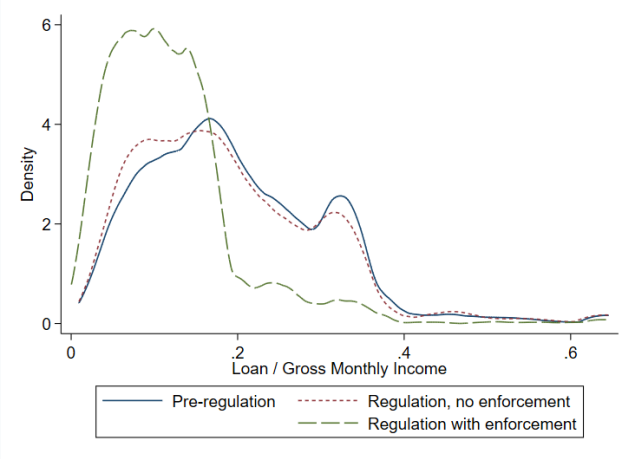


Note: The figure shows the coefficients from event study regressions of weekly loan volume relative to regulation and enforcement dates of city ordinances in Austin and Dallas. Both ordinances became effective on January 1st, 2012 (the regulation date), and the enforcement dates were May 1, 2012 for 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.

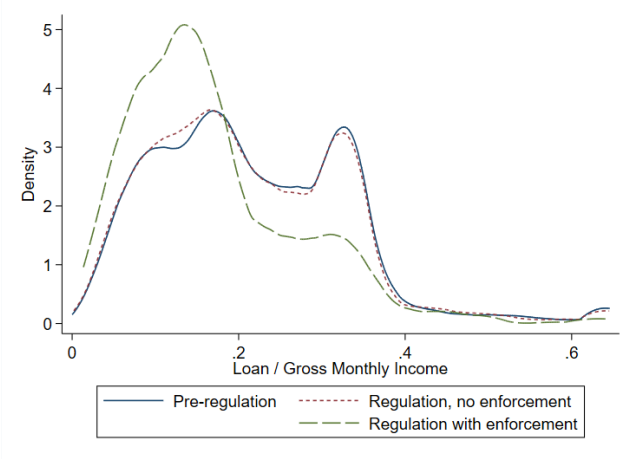
Figure 7: Compliance with City Ordinances After Enforcement Date

Panel A: Loan to Income Ratio

(a) Austin

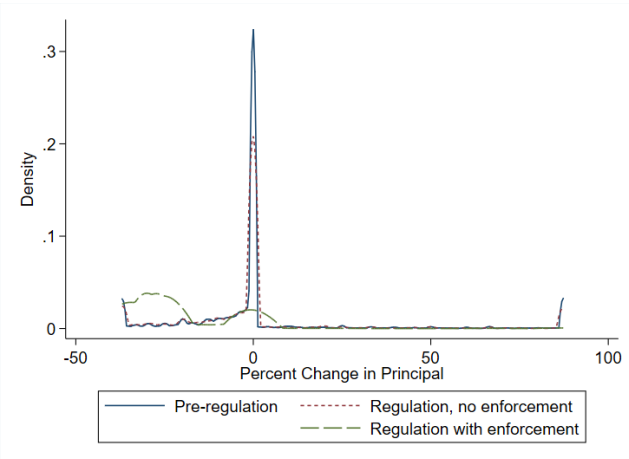


(b) Dallas

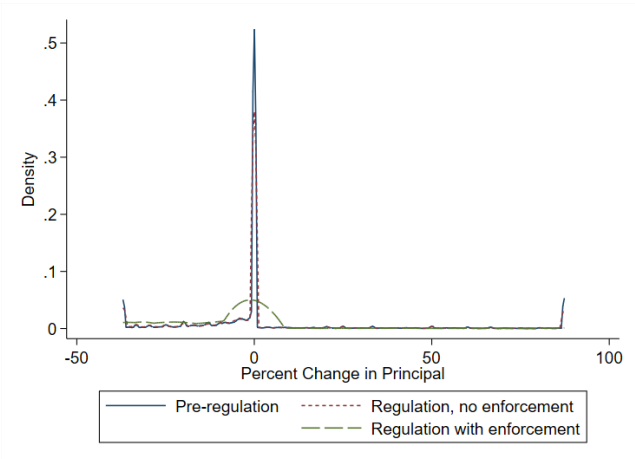


Panel B: Amortization Across Renewals Within Seven Days

(c) Austin



(d) Dallas

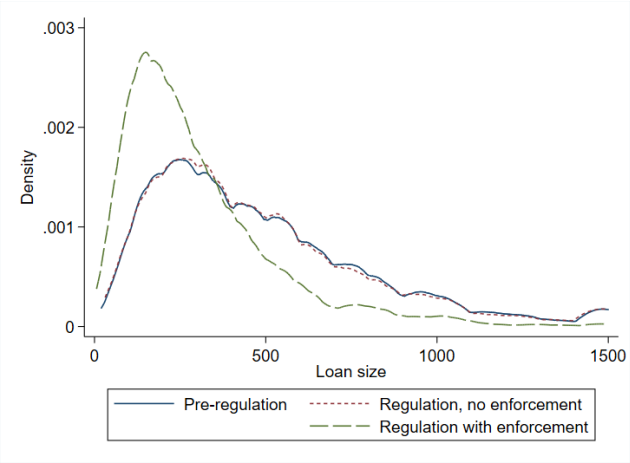


Note: The figure shows distributions of the loan to gross monthly income ratio and the percent change in loan principal for renewals taken within seven days of a prior loan, for three time periods based on the regulation and enforcement of the city ordinances. Both ordinances became effective on January 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.

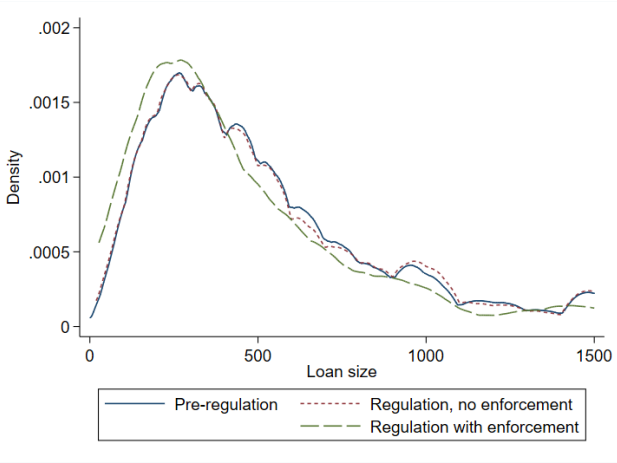
Figure 8: Changes in Loan Size and Income in Austin and Dallas

Panel A: Loan Size

(a) Austin

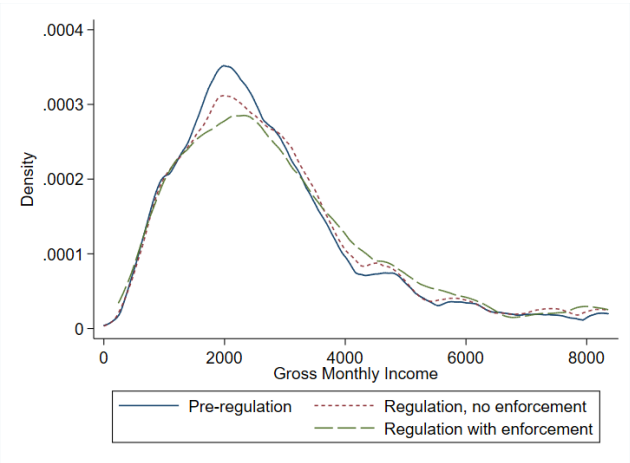


(b) Dallas

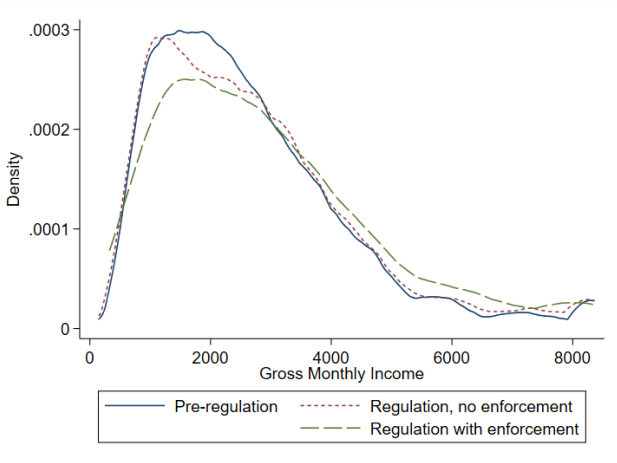


Panel B: Income

(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 Stores				
Sample:	All	All	All	Balanced	Excl. Dallas/Austin
Dep. Var:	$ 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 (no controls)		
TexasPost	0.020	0.002	- 0.007
	(0.003)	(0.001)	(0.001)
	[0.000]	[0.071]	[0.000]
	Panel C: Microdata (with 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. Panel B splits customers by nationwide income tercile. Panel C splits customers by the nationwide tercile in distance between their 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.

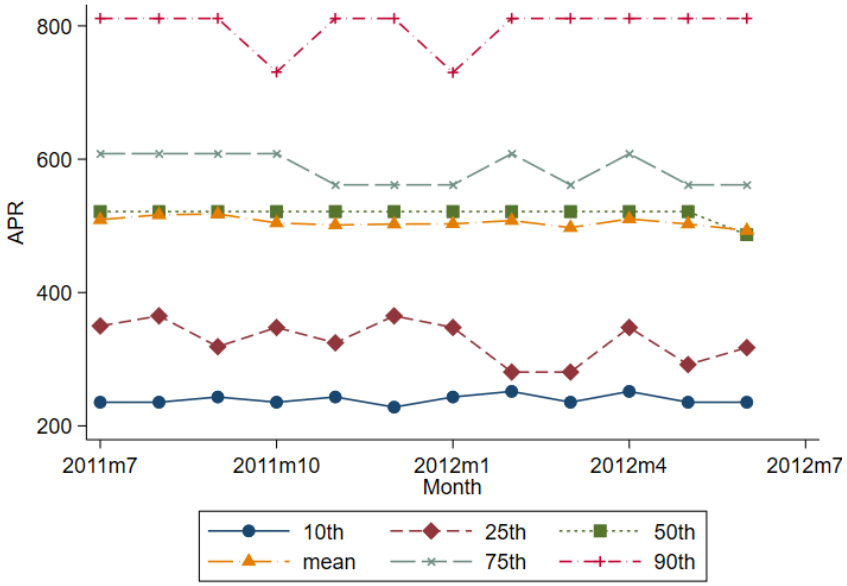
Table 5: Effects of Austin and Dallas Ordinances

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Effects of City Ordinances - Regulation Date					
Dep. Var:	$ Volume	# Loans	Loan / GMI > 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: Effects of City Ordinances - 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: Cross-city 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]
Austin border (20mi)	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]
Dallas border (20mi)	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]

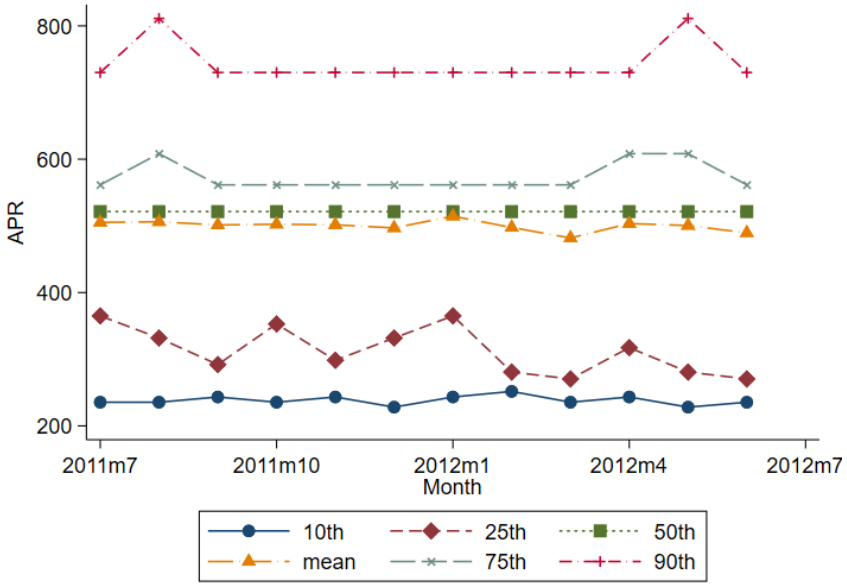
Note: The table shows coefficients on indicators for the interaction between stores in Austin and Dallas and the post-regulation or post-enforcement periods for regressions that include each store-week within Texas. The regulation date for both ordinances was January 1st, 2012, and the enforcement dates 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.