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PAYDAY LENDERS: HEROES OR VILLAINS?

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Abstract

I ask whether access to high-interest credit (payday loans) exacerbates or mitigates individual financial distress. Using natural disasters as an exogenous shock, I apply a propensity score matched, triple difference specification to identify a causal relationship between access-to-credit and welfare. I find that California foreclosures increase after disasters, but the existence of payday lenders mitigates half (1.2 foreclosures per 1,000 homes). Lenders also mitigate 2.67 larcenies per 1,000 households with no effect on burglaries or vehicle thefts. My methodology demonstrates that my results apply to ordinary personal emergencies, with the caveat that not all payday loan customers borrow for emergencies.

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There is little debate that access to finance enhances value for *firms*.[1] A similar consensus does not exist as to whether access to consumer credit necessarily provides a benefit to *households*. If individuals have financial literacy shortcomings (Johnson, Kotlikoff and Samuelson, 2001; Stango and Zinman, 2007; Lusardi and Tufano, 2008) or engage in utility-destroying temptation consumption (O'Donoghue and Rabin, 2006), financial institutions may cater to these biases (Campbell, 2006), and access to finance may make borrowers worse off.

In this paper, I study the welfare effects of access to distress finance for credit constrained individuals around a community natural experiment. The primary providers of distress finance for constrained households are payday lenders, who offer short-term, small dollar advances intended to sustain individuals to the next payday. The fees charged in payday lending annualize to implied rates well over 400%. In this paper, I ask whether these 400+% loans mitigate or exacerbate the effect of financial distress on individuals' welfare as measured by foreclosures and small property crimes.

With up to 20% of U.S. residents financially constrained, the importance of knowing the welfare implications of payday lending is likely to be both timely and large. Fifteen percent of U.S. residents have borrowed from payday lenders in a market that now provides over $40 billion in loans each year.[2] Despite (or because of) the growing demand, State and Federal authorities are working towards regulating and curbing the supply of payday lending. Thus far, fifteen States prohibit payday lending.

From one perspective, payday lenders should help distressed individuals bridge financial shortfalls by enabling individuals to smooth liquidity shocks without incurring the larger costs of bouncing checks, paying late fees, having services suspended and reinstated, and getting evicted or foreclosed upon. As such, one view of payday lending is that it should be welfare-enhancing.

An opposite perspective is that payday lending destroys welfare. The availability

[1] e.g., Jayaratne and Strahan (1996); Rajan and Zingales (1998); Levine and Demirguc-Kunt (2001); Dahiya, John, Puri and Ramirez (2003); Guiso, Sapienza and Zingales (2004); Cetorelli & Strahan (2006); Paravisini (2006), etc.

[2] For a market overview, see Caskey (1994, 2005); Fannie Mae (2002); Barr (2004); Bair (2005).

of cash from payday loans may tempt individuals to over-consume. An individual who is likely to fall to temptation may prefer the discipline of lacking access to cash before temptation arises (Gul and Pesendorfer, 2001; 2004; O'Donoghue and Rabin, 2006). In this view, payday lending could be welfare-destroying.

To answer whether payday lending exacerbates or mitigates the welfare effect of distress, I use natural disasters as a community-level natural experiment. I perform the analysis at the zip code level for the State of California during 1996-2002. The difficulty in measuring how payday lending impacts welfare changes over time is in disentangling a causal payday lender effect from endogenous location decisions of lenders and from correlated community economic circumstances causing welfare outcomes. To overcome the endogeneities, I set up a matched triple difference framework. A simple derivation of the empirical model shows that once I match on financial constraints, the natural experiment is able to capture the general effect of financial distress on individual welfare and the role of lenders in mitigating or exacerbating the distress effect. The matching aligns communities on the propensity of residents to be financially constrained prior to the natural experiment. I generate these propensities at the zip code level by estimating the probability that an individual in the Survey of Consumer Finances (SCF) is financially constrained as a function of socioeconomic characteristics. I then project the relationship onto zip codes by apply the SCF coefficients to Census socioeconomic variables observed at the community level.

Matching alone does not solve the endogeneities of lender location decision, but does facilitate a counterfactual framework using a triple difference (difference-in-difference-in differences) specification. The key exogeneity assumption is that the non-disaster communities provide an unbiased benchmark of how lender and non-lender communities would have differed in welfare growth had they not been hit by a disaster. Thus, by subtracting this benchmark from the observed lender minus non-lender welfare growth for disaster communities, I can difference away endogeneities associated with the observed existence of a lender in a location.

There is one source of bias my matched triple difference specification may not overcome. It may be that there is something unique about communities where payday lenders locate that speaks to how resilient residents will be specifically during natural disasters. Although I am not sure why this would be the case, I address this argument by instrumenting the location of payday lenders using the count of surface (non-residential) intersections in a zip code, relying on the fact that payday lenders tend to cluster at focal points of traffic and commuting thoroughfares (U.S. Department of Treasury, 2000). Because I do the analysis in a matched changes over time framework, I am able to argue that intersections as an instrument meets the exogeneity assumption.

The results indicate that payday lenders offer a positive service to individuals in unexpected financial distress. Natural disasters induce an increase in foreclosures by 72%, but the existence of payday lenders significantly offsets half of this increase. In particular, I find that access to credit in distress times prevents 1.22 foreclosures per 1,000 homes.

The results also indicate that payday lenders alleviate individuals' need to resort to small property crimes in times of financial distress. I find significant and robust results, however, for only for larceny, the crime which carries the least sentencing of all property crimes. Natural disasters increase larcenies by 13% (nearly 9 larcenies per 1,000 households). Access to credit, however, mitigates 2.67 larcenies per 1,000 households, or 30% of the effect of the natural disaster.

My experimental design necessitates a caveat in how these results can be interpreted. Individuals may use payday loans in non-financial distress situations. In a survey of payday borrowers, Elliehausen and Lawrence (2001) report that 33% of loans are not for emergency needs. Some borrowers may habitually over-consume and use payday loans regularly to fill cash shortfalls. Skiba and Tobacman (2005) provide evidence consistent with the use of payday lending in such settings. The habitual over-consumers are those most likely to have negative welfare impacts of temptation consumption. Because I do not identify the net benefit of payday lending across the distribution of borrowers, my results can only be interpreted that payday lenders are providing a valuable service to individuals facing unexpected financial distress and cannot speak to the effect distilling to those habitually falling to temptation.

A number of other papers also cocurrently address the welfare implications of payday borrowing. On the surface, the results are conflicting, with Morgan and Strain (2007) showing a welfare improving role for lenders and Skiba and Tobacman (2007) and Melzer (2008) showing a welfare destroying role for lenders. However, I believe that what our results suggest is there is a pressing importance of understanding the heterogeneity of borrowers and the circumstances that they might face (Bertrand and Morse, 2009a) and mistakes that they might make (Brito and Harvey, 1995; Bernheim and Rangel, 2006; Skiba and Tobacman, 2009; Bertrand and Morse, 2009b).

The remainder of the paper proceeds as follows. Section I offers an overview of the market for payday loans. Section II develops the competing hypotheses of whether payday lending is welfare improving or diminishing. Section III outlines the triple differencing empirical methodology and presents the intermediate propensity score matching results. Section IV describes the data sources and summary statistics. Sections V and VI present the main empirical results for foreclosures and crimes, respectively, and Section VII concludes.

1 Payday Lending Market

Up to 20% of U.S. residents have been found to be credit constrained in recent decades (Hall and Mishkin, 1982; Hubbard and Judd, 1986; Zeldes, 1989; Jappelli, 1990; Gross and Souleles, 2002). Individuals restricted in access to credit offered by mainstream banking, mortgage companies and credit cards often resort to borrowing from high interest lenders. These high-cost financial institutions are only sparsely studied in the finance literature, despite the fact that payday lending alone provides the economy with over $40 billion in loans per year. Loans collateralized by car titles (title loans) and household assets (pawn shop loans) offer cheaper alternatives, but because these markets require clear ownership of valuable assets, they are much smaller in loan transaction volume.

The main alternatives to payday lending for individuals in distress are bank overdraft loans and bounced checks. Bouncing checks (or over-extending on debit cards) to buy a few days of float is still a very common way to borrow funds. Although the APR cost depends on the amount overdrawn and duration, bouncing checks is usually near to or more costly than taking out a payday loan, especially when adding an implied cost for a negative entry on one's credit history. Bank overdraft loans differ from bounced checks in that banks pre-agree to clear the overdraft check(s) for a fee. Overdraft loans are comparable in cost to payday loans: if they carry longer floats, they will be generally cheaper, but if multiple checks need clearing, they will be generally more expensive. The majority of my sample pre-dates widespread availability of overdraft loans, especially for the individuals with poor credit history and/or no direct deposit to whom the bank may not offer overdraft loans. Thus, for the majority of people in my sample, there are no obvious alteratives to a payday loan.[3]

How does payday lending work? An individual visits a payday loan store with her most recent paycheck, her checkbook and her bank statement. The unbanked and unemployed do not qualify. A typical loan is $300 with a fee of $50. In such a case, the borrower would write a check (or authorize a bank draw) for $350, post-dating it to her payday, usually 10-14 days hence. The payday lender verifies employment and bank information, but does not run a formal credit check. On payday, if the individual is not able to cover the check, which happens more often than not, she returns to the payday store and refinances the loan, incurring another $50 fee. The borrower typically is a repeat customer. According to the Center for Responsible Lending (2004), 91% of payday loans are made to individuals with five or more payday borrowings per year (with an average of 8-13 loans).

2 Competing Hypotheses

Individuals often experience some sort of personal emergency (e.g., medical expenses or car breakdowns) leaving them without cash for their short-term obligations. Banks do not provide credit for such situations, as the transaction costs of making small-scale, short-term loans are substantial, driving potential lenders into conflict with usury laws or the threat of greater regulation. Small-scale personal disasters lead to bounced checks,

[3] See the Appendix for a brief discussion of profitability of payday lenders to put context on why entry may not provide alternatives.

late fees, utility suspensions, repossessions, and, in some cases, foreclosures, evictions and bankruptcies. The $50 payday fee is likely to be as cheap as or cheaper than these alternatives, especially if payday borrowing evades delinquencies on multiple obligations. In these common scenarios, payday lenders can be heroes.

Consumer advocate groups argue that the problem of payday loans is not the single loan, but the revolving of loans when individuals cannot pay off the debt in a single pay cycle. This argument need not be always true. If an individual faces a short-term personal crisis, he may be willing to pay 400% for some time to weather the financial distress. Even for repeat borrowers, payday lending can be welfare improving to those in need.

On the other hand, the consumer advocates may be right. What if payday lending tempts individuals to over-consume? A large literature documents time-inconsistent preferences resulting in present-biased consumption (e.g., Jones, 1960; Thaler, 1990; Attanasio and Browning, 1995; Stephens, 2006) and a lack of saving (e.g., Thaler and Shefrin, 1981; Laibson, 1997; Laibson, Repetto, and Tobacman, 1998; Choi, Laibson and Madrian, 2005). Cash from payday lending may encourage present-biased consumption following the temptation and self-control models of Gul and Pesendorfer (2001; 2004), O'Donoghue and Rabin (2006), and Fudenberg and Levine (2006). In these models, temptation consumption in some intermediate period could be curbed if there were some ex ante self-control mechanism. In this case, if there were a ban on payday lending, cash for satisfying the temptations might be scarce.

To claim that the lack of a self-control mechanism destroys welfare requires taking a particular perspective. A revealed preference argument (e.g., Gul and Pesendorfer, 2001; 2004) would conclude that payday borrowers derive sufficient utility from a spontaneous purchase to offset the negative consequences of the cost to future consumption. Rather than taking this perspective, the villain hypothesis follows O'Donoghue and Rabin (2006) in viewing welfare in an ex ante, long-term sense. Viewed this way, temptation in these models lowers expected lifetime utility. If payday lending cash facilitates temptation consumption, welfare consequences are realized in lower future consumption.

This argument requires payday borrowers to be naïve to their lack of own self-control as in O'Donoghue and Rabin (2003) and DellaVigna and Malmendier (2004) or unable to find a commitment mechanism. If it were not so, individuals would themselves invest in self-control. Payday borrowers might be subject to both – naïve about their ability to resist spending payday cash and unable to commit not to consume under temptation with the knowledge that a payday loan is easily accessible. From the perspective of O'Donoghue and Rabin (2006), if long term welfare can be improved, the practices of payday lending should be banned.

A caveat is in order. Payday lenders may be both heroes and villains. It is likely that payday borrowers are of two types – those who face personal emergencies and those who borrow from payday lenders as an ordinary course of business. The ordinary course of business borrowers would naturally be those more likely to experience the negative welfare consequences of temptation consumption. Skiba and Tobacman (2005) show that the behavior of payday borrowers reflects behavior consistent with individuals reacting to consumption shocks as well as individuals expressing time-inconsistent preferences.

Since the focal point of my empirical design is an exogenous shock inducing financial distress, my results may fail to capture the negative consequences of temptation consumption. An argument could be made that in order to draw a conclusion as to whether payday lenders are heroes or villains, one must know the distribution of payday loans, i.e., what proportion of loans are made to assist people with interim finance in an emergency situation. Elliehausen and Lawrence (2001) show that 66% or survey respondents say they use payday loans for an emergency situation. Based on this information, one might conclude that my results apply to the two-thirds majority cases of payday loans. But, I prefer to interpret my results more modestly rather than to apply a social planner weighting of welfare. Personal emergencies are an ordinary fact of life, and I ask whether payday lenders are heroes or villains for individuals in financial distress because of such regular events. There is proportion of payday borrowers to whose welfare I cannot speak. I interpret policy implications accordingly.

3 Empirical Methodology

The goal of the analysis is to test to what extent the existence of a lender mitigates or exacerbates the effect of financial distress on welfare outcomes. I start with a fixed effects model of individual welfare growth in which financial distress (f) linearly affects welfare growth, and the existence of a high-interest lender (L) can mitigate or exacerbate the situation:

$$\Delta_t w_{izt} = \gamma_{iz} + \alpha_1 L_{zt} + \alpha_2 f_{izt} + \alpha_3 L_{zt} f_{izt} + \tau_t + \varepsilon_{izt} \tag{1}$$

$\Delta_t w_{izt}$ denotes changes in welfare outcomes for individual i in zip code z at time t, where Δ_t refers to a time first differencing. I refer to the linear time changes as welfare growth. Time dummy variables (τ_t) remove any economy-wide fluctuations in welfare growth so that the coefficient on individual financial distress (f_{izt}) captures individual-specific effects of distress for individual i at time t. Equation (1) removes the welfare growth fixed effect of individuals, γ_{izt}. Indicator variable L_{zt} is equal to one if the individual has access to a distress lender, where access is defined geographically at the community (zip code) level z since individuals generally do not travel far to go to a lender (Elliehausen and Lawrence, 2001). A zip code is on average 21,000 people. For densely-populated areas, the next community may only be a short distance away; thus, in estimation, I drop densely populated areas. If equation (1) could be estimated, the coefficient of primary interest, $\widehat{\alpha}_3$, would capture the influence of access to a lender on how financial distress affects welfare growth.

Three formidable problems exist with equation (1).[4] First, the variables necessary to measure welfare and financial distress are not readily available at the individual level. Second, the location of lenders is endogenous, potentially (probably) causing the estimator α_3 to be biased. Third, financial distress and welfare growth are simultaneously caused by economic conditions of the community, also implying that α_3 is likely to be biased. In what follows, I employ a series of transformations on (1) and set up a counterfactual framework to handle these concerns.

[4] Another problem is that the residuals can be serially correlated, but this problem can be handled with relatively more ease.

To make progress on the lack of data problem, I break financial distress (f_{izt}) into two types – personal emergency distress (f_{izt}^{pers}) and natural disaster distress (f_{izt}^{dis}).[5] Since it is possible to have both types of distress occurring at the same time, the appropriate indicator variable breakdown is: $f_{izt} = f_{izt}^{pers} + f_{izt}^{dis} - f_{izt}^{pers} f_{izt}^{dis}$. The benefit from this decomposition is that f_{izt}^{dis} is unrelated to the location decision of the lender. Specifically, the correlation between the occurrence of a disaster and the existence of a lender is 0.005.

The other data-solving step is to aggregate the model to the community (zip code) level and average over the community population n_{zt}. Aggregating facilitates two simplifications. Since (large) natural disasters hit areas as opposed to individuals and since zip codes are fairly small areas, I drop the individual subscript i on the natural disaster variable f_{zt}^{dis}, with only some concern of biasing tests toward finding no effects from disasters if the areas are too large. The other simplification comes from noticing that the average number of personal emergency distresses among community members is equivalent to the propensity of any individual in the community to be financially constrained due to personal emergencies. I denote this propensity by ρ_z, where $\rho_z \equiv \frac{1}{n_{zt}} \sum_{i=1}^{n_{zt}} f_{izt}^{pers}$. I assume that communities have a (medium-term) stable propensity for personal emergency distress (the time subscript disappears). Individuals can go in-and-out of distress, but on average the same number of individuals face personal emergency distress every time period in a given community. Over a longer time, this assumption will not be valid; thus, in the estimation I update ρ_z at a 3-year interval.

The two simplifications yield measures ρ_z and f_{zt}^{dis} that are either estimatable (ρ_z) or observable (f_{zt}^{dis}) with a little work described in the data section. Combining the simplifications with the aggregation yields a potential estimating equation for which all data are available:

$$\Delta_t W_{zt} = \gamma_z + \alpha_1 L_{zt} + \alpha_2(\rho_z + f_{zt}^{dis} - \rho_z f_{zt}^{dis}) + \alpha_3 L_{zt}(\rho_z + f_{zt}^{dis} - \rho_z f_{zt}^{dis}) + \tau_t + \varepsilon_{zt}, \quad (2)$$

where $\Delta_t W_{zt} \equiv \frac{\sum_{i=1}^{n_{zt}} \Delta_t w_{izt}}{n_{zt}}$ and $\varepsilon_{zt} \equiv \frac{\sum_{i=1}^{n_{zt}} \varepsilon_{izt}}{n_{zt}}$. The fixed effect γ_z is now the mean community welfare growth absent lenders and distress.

[5]In the empirical section, I allow the effect of distress to vary by by whether the distress results from a personal emergency or a natural disaster, but for now I assume that individuals are either cash constrained or they are not.

3.1 Counterfactual Framework

The distress decomposition and aggregation to the community level do not solve problems of lender location endogeneity and omitted variable bias. However, equation (2) does facilitate a counterfactual framework to solve these problems using a triple difference approach. The basic idea of the identification strategy is to use a matched difference-in-differences (DID) welfare estimator for non-disaster communities (DID dimensions: time and lender/no lender) as the counterfactual for what a similar DID estimator for natural disaster communities would look like in the absence of the random treatment of a natural disaster.

The counterfactual setup works as follows. I denote the communities that have been or will be hit by natural disaster with *treat*, and those not ever affected by a natural disaster with *cntrl*. I mark communities that have access to a lender with a subscript L, and those with no access, with N. For each control community with access to a lender, imagine choosing another control community with no lender, where the pair matches in time and on the propensity of the residents to be in personal emergency distress. Focusing on one particular pair, suppose $\rho_L^{cntrl} = \rho_N^{cntrl} \equiv \rho^*$. Differencing the matched pair using equation (2) gives a DID estimator for these control communities:

$$\left[\Delta_t W_{Lt}^{cntrl} - \Delta_t W_{Nt}^{cntrl} \mid \rho^*,\ f_{cntrl,t}^{dis} = 0\right] = \gamma_L^{cntrl} - \gamma_N^{cntrl} + \alpha_1 + \alpha_3 \rho^* + \varepsilon_{Lt}^{cntrl} - \varepsilon_{Nt}^{cntrl}. \quad (3)$$

An important note is that even if I average this DID over all matched control communities, I cannot interpret this estimator as a causal measure of the effect of lenders on welfare growth. The difference in welfare growth of communities with lenders as compared to those without may well be due to endogenous location decisions of lenders and other economic trends associated with observing lenders in a community.

The same matching exercise for a set of treatment communities yields a DID estimator:

$$\left[\Delta_t W_{Lt}^{treat} - \Delta_t W_{Nt}^{treat} \mid \rho^*,\ f_{treat,t}^{dis} = 1\right] = \gamma_L^{treat} - \gamma_N^{treat} + \alpha_1 + \alpha_3 + \varepsilon_{Lt}^{treat} - \varepsilon_{Nt}^{treat}. \quad (4)$$

As in the control case, I cannot interpret this DID estimator causally. Welfare growth may differ in locations with payday lender compared to locations without lenders for reasons unrelated to any financial distress caused by disasters. However, because equation (3) is a snapshot of how, on average, welfare growth differs in lender communities and non-lender communities, it can serve as the counterfactual for how the lender and non-lender communities would have differed in welfare growth on average had there been no natural disaster.

Following this intuition, the final differencing subtracts the DID estimate of equation (3) from the DID estimate of equation (4). Averaging over $m = 1,..., M$ matches of 4 communities, the resulting triple difference estimator $\Delta\Delta\Delta$ is:

$$\Delta\Delta\Delta \equiv \frac{1}{M}\sum_{m=1}^{M}\left[\left(\Delta_t W_{mL}^{treat} - \Delta_t W_{mN}^{treat}\right) - \left(\Delta_t W_{mL}^{cntrl} - \Delta_t W_{mN}^{cntrl}\right) \mid \rho_m,\ f_{cntrl}^{dis} = 0,\ f_{treat}^{dis} = 1\right]$$

$$= \frac{1}{M}\sum_{m=1}^{M}\alpha_3(1-\rho_m) + \varepsilon_m, \qquad (5)$$

where $\varepsilon_m = \left(\varepsilon_{mL}^{treat} - \varepsilon_{mN}^{treat}\right) - \left(\varepsilon_{mL}^{cntrl} - \varepsilon_{mN}^{cntrl}\right)$.[6]

What is essential is the conditional mean independence assumption:

$$E_m\left[\Delta_t W_{mL}^{treat} - \Delta_t W_{mN}^{treat} \mid \rho, f_{treat}^{dis} = 0\right] = E\left[\Delta_t W_{mL}^{cntrl} - \Delta_t W_{mN}^{cntrl} \mid \rho, f_{cntrl}^{dis} = 0\right], \quad (6)$$

which says that had there not been a natural disaster, the differential growth in welfare between lender and non-lender communities would have been the same in the treatment and control groups. The only property that this assumption relies on is that natural disasters hit randomly. The essence of the counterfactual framework is that although lender location endogeneity and omitted variables probably exist, they exist in the same way for matched disaster and non-disaster communities. Any biases from endogeneities are differenced out of the error terms.

The regression equation corresponding to equation (5) is:

$$\Delta_t W_{zt} = \alpha_1 L_{zt} + \alpha_2\rho + \alpha_3\rho_z L_{zt} + \alpha_2(1-\rho_z)f_{zt}^{dis} + \alpha_3(1-\rho_z)L_{zt}f_{zt}^{dis} + \varepsilon_{zt}. \qquad (7)$$

[6] In equation (5), I assume $\frac{1}{M}\sum_{m=1}^{M}\left(\gamma_{mL}^{treat} - \gamma_{mN}^{treat}\right) = \frac{1}{M}\sum_{m=1}^{M}\left(\gamma_{mL}^{cntrl} - \gamma_{mN}^{cntrl}\right)$ since the community fixed effects are not affected by disasters. This should hold as long as the sample is sufficiently large. Also I treat all four matches as occuring at the same point in time and thus drop the time subscripts. As long as I choose a disaster and non-disaster match at the same time, the time dummies drop out. I include time dummies in my estimation.

The estimating equation is constrained such that the coefficient on the second and fourth terms are equal and that on the third and fifth, reflecting the equal effect of financial distress coming from either personal emergencies or natural disasters. I relax this constraint in the empirical section to show my results hold generally. An estimate $\widehat{\alpha_2}$ measures the effect of distress on welfare, and $\widehat{\alpha_3}$ gauges to what extent either type of financial distress is mitigated or exacerbated by the existence of a lender. To handle the serial correlation discussed in Bertrand, Duflo and Mullainathan (2004), I collapse each zip code to one observation capturing the zip code change in welfare after the natural experiment event.

3.2 Resiliency and Instrumental Variables

One could make an argument that resiliency to disasters differs for communities with lenders compared to those without for reasons not causally related to the existence of a lender. An omitted variable of resiliency may only appear in the treated case, and thus the control counterfactual would not resolve the bias.[7] For this to be a valid concern, one needs to make an argument of why lender communities would react differently from matched non-lender communities. For example, lenders may locate in communities with more (or less) adhesive community or family ties that provide support during disasters. It is not obvious why this would be the case. Nevertheless, I address this issue (iteratively) first by inserting control variables into equation (7) that gauge resiliency directly and then by instrumenting the location of lenders.

For foreclosures, I measure resiliency with changes in commerce – the number of establishment and overall payroll paid in the community normalized by population – and changes in house prices (Campbell and Cocco, 2006). For small property crime, I measure resiliency with the same two changes in commerce variables plus changes in violent crime. I include the house price variable and violent crime variable to gauge impacts directly related to the foreclosures and small property crimes respectively.

After showing results with the covariates, I confirm my results using instrumental

[7] Technically, the previous section shows that $E\left[\varepsilon_m L_m|\rho_m\right]=0$ and $E\left[\varepsilon_m f_m^{dis}|\rho_m\right]=0$, but this does not rule out the possibility that $E\left[\varepsilon_m L_m f_m^{dis}|\rho_m\right]\neq 0$.

variables. My instrument is the count of intersections of surface (non-residential) roads in a zip code in the year 2006. A valid instrument must satisfy the usual two properties – being relevant in the first stage and meeting the exclusion restriction in the second stage. The relevance criterion is easily met. Payday lenders, like gas stations, locate at intersections according to survey results from the U.S. Department of Treasury (2000). Treasury's result is intuitive: lenders locate where people can easily access the service during regular commuting.

For the exclusion restriction to hold, it must be that intersections are unrelated to the unexplained portion of welfare growth. Working in a matched set of communities with a time first differenced dependent variable alleviates many concerns about violations to the exclusion restriction. For a violation to occur, it must be that a static measure of intersections predicts residual changes to community welfare. Nevertheless, one might worry about the relationship between intersections and population density. The post office adjusts the size of zip codes from time-to-time to realign zip codes with population targets. As a result, more densely populated zip codes have smaller land areas. It is not obvious on a set of matched communities with the same population whether bigger or smaller land mass areas would have more intersections. However, because I exclude the big cities in the analysis, I focus on comparably dense zip codes.

One argument could be that the existence of more intersections relates to growth in commercial activity. Because my measure of intersections is ex post (in 2006) to the analysis period, more intersections could have resulted from commercial growth in the zip code during the sample period. This is unlikely. The processes of roads changing from residential to commercial and of new surface roads being built are both very slow-moving. In addition, roads do not generally close down or lose commercial zoning when commercial activity declines. However, even if the ex post nature of the instrument is not a problem, it could be that a static quantity of infrastructure supports future commercial activity, which in turn could cause a decline in foreclosures and/or crime. Thus, in the IV estimations I control for growth in commercial activity by zip code using establishment and payroll data and estimate the IV statically as a cross-section at the end of the sample period.

3.3 Matching on Propensity to be Financially Constrained

The Survey of Consumer Finances (SCF) contains a number of measures that identify individuals who are constrained financially. Even if geographic identifiers were available for the SCF, the sample would not sufficiently large to be representative of individual communities. Thus, I estimate the relationship between individuals' socioeconomic attributes and their probability of being financially constrained using the SCF and then project the relationship onto the same socioeconomic information available at the zip code level from Census.

Table 1, panel C shows three measures of being financially constrained from the SCF. *AtLimit* is an indicator variable equal to one if the individual's outstanding balance on her credit card is within \$1,000 of her credit card limit, if she has credit card debt. Approximately 9% of respondents are within \$1,000 of their credit limits. *HiDebt*, is equal to one if the individual's credit card debt is equal to more than 10% of her yearly income. Twenty-eight percent of the sample have high debt. The final measure, *BehindPayments*, is equal to one is the individual responds affirmatively to the question if she is behind on any payments. Twelve percent of individuals are behind.

I use the 4,300 individuals in the 1998 SCF to estimate the probability an individual is financially constrained along each of these measures. The logistic estimations closely follow Jappelli (1990) and Calem and Mester (1995), who use the same procedure. I use their socioeconomic variables that are also available in Census files – wealth, income, age, education, marital status, race, sex, family size, home and car ownership, and shelter costs. To benefit from the distribution of socioeconomic characteristics and not just the means, I define variables in terms of whether a respondent falls in a range of values. For example, rather than using income as a variable, I use an indicator for whether income is between two ranges.

Table 2 presents the results of the logistic estimation of the probability of being financially constrained. The logistic estimates predict correctly whether an individual is financially constrained 89% of the time. The R-Squares run from 0.096 to 0.150, with the majority of the variance being explained by income and age. I only briefly highlight some of the results and refer interested readers to Jappelli (1990) and Calem and Mester (1995).

The coefficients in Table 2 should be interpreted as "compared to a wealthy, very educated, single male senior." For all three dependent variables, the probability of being financially constrained is highest in the $15,000 - $45,000 range. Survey data in Elliehausen and Lawrence (2001) finds that individuals in the $25,000 - $50,000 income range account for more than half of payday borrowers, suggesting that I am identifying a relevant profile of individuals. Constraints generally decline with age, after peaking somewhere between 18 and 34. Nonwhite persons and those with vehicles face more constraints. The other results vary by which dependent variable measure of financial constraints is used. Of these non-conclusive results, education is particularly interesting. Education has very little explanatory power once income is included except in the $BehindPayments$ specification in which those reaching but not finishing high school are more constrained.

I take the coefficients and project the linear relationship onto Census data for 1,762 California zip codes. In other words, I multiply each coefficient by the percentage of residents having that characteristic in a zip code and sum up. I do this for each of the three measures of being financially constrained and for each of the Census data years 1990, 1997 (an update with most socioeconomic variables), and 2000. I interpolate the in-between years to avoid jumps in my projections over time.

Since an argument could be made that each of these variables measures an important part of being constrained, I would like to form some combination of the measures once estimated. For simplicity and because I do not want to impose subjective assumptions, I will re-scale the predicted variables to have equal means which I fix to be equal to 0.10 for ease of exposition and take an average of the three measures for each zip code. As a check that I am not losing too much information by creating this index, I check the principle components of the three variables. The first principle component captures 80% of the variance space of the three measures (with an eigenvalue of 2.4). The factor loading weights are almost equal across the three measures, and the factor score is correlated over 0.95 with my equal weighted index.

With propensity scores in hand, I take the nearest neighbor match for communities that are hit by disasters from the pool of non-disaster communities, matched on access to a lender or not within a common support with replacements allowed. Because my foreclosure and crime data are not comprehensive in covering all zip codes and years, the number of observations and the match sample differ. My methodology section suggests that I should do a 4-way match (disaster/not and lender/not) all at once. However, because my pool of disasters is small relative to the pool of non-disaster communities, I am less likely to create a bias with a two-way match. In total, I use 899 zip codes disaster observations (at the year-quarter level) in the foreclosure matches and 492 zip code disaster observations (at the year level) in the crime matches. When a control group observation is chosen replicate times, I weight the observation accordingly such that the following four groups all have equal weight: non-disaster/non-lender communities, non-disaster/lender communities, disaster/non-lender communities, and disaster/lender communities. I run a Chi-Square test that the mean propensities of residents to be credit constrained are equal for all four sets of communities. The Bonferroni-adjusted p-value of 0.438 does not reject that the propensities are all the same.

4 Data and Summary Statistics

I limit the analysis to the State of California to make use of micro data available over time for payday lender locations and welfare variables and to isolate the analysis in a single regulatory environment. I drop the big city counties to focus on areas where crossing zip code lines is not done as a course of everyday business and on areas where my crime data are more precise (described below). In particular, I throw out 11 large citycounties (out of a total of 58) with a population over 800,000 people, all counties with populations equal to or greater than that of San Francisco.[8] The time period of the analysis is 1996-2002.

[8]The dropped counties are Los Angeles, San Diego, Orange, Riverside, San Bernardino, Santa Clara, Alameda, Sacramento, Contra Costa, Fresno, Ventura, and San Francisco.

4.1 Welfare Variables

For foreclosures to be a measure of welfare, it must be that individuals' utilities decline when their homes are foreclosed upon. Admittedly, having one's house foreclosed on can be efficient in some circumstances, even taking into account the large transaction costs involved. A general rule is that a foreclosure is inefficient if the present value of the homeowner's income is sufficient to cover the present value of consumption, including housing consumption, but the homeowner lacks access to credit to smooth consumption using future income as collateral. In my empirical design, the matched triple differences subtracts out the general pattern of foreclosures for similar communities (with the non-disaster areas) and the effect of disasters on forcing foreclosures (with the disaster, non-lender communities), thus isolating only financial distress-forcing foreclosures.

The dependent variable I use is quarterly residential foreclosures in a zip code recorded by the California Association of Realtors and available at RAND Statistics during each quarter from 1996-2002. As per my methodology, I work in foreclosure rates, normalizing foreclosures by the total number of owner-occupied housing in a zip code community available from the Census. Table 1 reports that, in the matched sample used in the estimations, foreclosures range from zero to 59 per quarter per zip code, with a mean (median) of 10.9 (6). In rates, this translates to a mean of 3.0 foreclosures per thousand owner-occupied housing units.

The second way I measure welfare is by small property crimes. California crime data are from the State of California Criminal Justice Statistics Center made available through RAND Statistics for 1996-2002 for each police jurisdiction. Since a police jurisdiction might be a county, city, town, or local authority (e.g., a university or railroad police force), I need to allocate crime in a meaningful way. I manually identify all zip codes covered by the police jurisdiction and allocate crimes by population weight within the covered zip codes. I then aggregate the crimes committed in a zip code across all police forces. This method is not perfect. The biggest bias would be in Los Angeles, because I allocate all crimes caught by the Los Angeles County and City police forces to the zip codes within L.A. based on population, reinforcing the need to throw out these big city counties. The problem is least severe for small towns, where the local police force is well defined within a zip code.

Among possible crime measures, I focus on small property crimes – larcenies (non-forceful theft, e.g., shoplifting), vehicle thefts and burglaries. I focus on these crimes because they are non-violent, and the link between relieving financial distress and criminal action is most direct. Since the intensity of the crime is, according to sentencing standards, monotonically increasing from larceny to vehicle theft to burglary, I can study to what degree individuals may use crime to relieve financial distress. Table 1 reports that the mean larcenies, vehicle thefts and burglaries are respectively 672, 145, and 232 per zip code. In the estimations, I normalize these by household units in the estimation.

4.2 Payday Lender and Intersections Data

The State of California Senate Bill #1959 legalized payday lending in 1996 and placed its licensing and regulation under the authority of the California Department of Corporations. The Department has license data for each payday store, with an original license date and date of suspension, if appropriate, for each active and non-active lender. One caveat with these data is that the payday stores are listed under two lending categories during the time period: California Finance Lender and Consumer Finance Lenders. I filter out insurance companies, auto loan companies, and realty lenders. What I am unable to fully distinguish are check cashiers with a licence to lend, who make only title loans or non-payday small consumer loans. However, according to my calculations, there were 2,160 payday stores, or 1 lender for every 16,000 people in the State, in 2002. This figure is almost exactly in line with the California figure cited in Stegman and Faris (2003) and those obtained from the Attorney General by Graves and Peterson (2005). A point of note is that a massive growth in payday lenders in California occurred between 2002 and 2005.

With the addresses for each payday lender, I plot the latitude and longitude coordinates of the address using GIS software (ArcView) and then collapse mapped data to zip code overlays from Census. Table 1 presents the community level summary statistics for payday lenders. The mean and median zip codes have 1.9 and 1 payday lenders. The empirical design is based on the yes/no question of whether there are any payday lenders in the zip code community, which is equivalent to being above or below median. Figure 1 depicts the mapping of 2002 payday locations to the zip codes, together with the propensities of communities to be credit constrained from section 3.3. The larger the dots on the zip code, the greater the density of lenders. The minimum size dot indicates no lenders are in the zip code, included for perspective. The zip code shadings reflect the credit constrained propensities; the higher the propensity to be credit constrained, the darker the color.

I instrument the location of payday lenders with the count of intersections in a zip code. I obtain detailed road data for 2006 from the California Department of Transportation which I use to calculate intersection density by counting up nodes in the GIS at which surface roads intersect. Surface roads are differentiated from expressways or residential streets. For State-designated rural areas, I allow expressways to be considered surface roads, as commerce often centers on expressway exits in non-urban areas. Mean and median intersections are 74.8 and 55.

The other statistics in Table 1, panel B are those of covariates. Descriptions of these zip-code level demographic variables appear in the table.

4.3 Natural Disaster Data

Natural disaster data come from the University of South Carolina's Sheldus Hazard database, which provides the location (by county), type (flood, wildfire, etc.), and magnitude (property damage) of natural disasters. Although disaster observations are at a county level, the comment field in the Hazard database contains more detailed location information, most often in the form of city names or NOAA (National Oceanic and Atmospheric Administration) Codes that identify the area hit by the disaster. For each line item, I manually attribute the disaster to the smallest area provided and then use the GIS program to overlay the disasters to zip code affiliations.

The Hazard database contains all natural disasters which cause more than $50,000 of property damage in a county. Because the focus of the paper is on foreclosures, I am concerned about the role of insurance, especially if insurance is held differentially across demographics. Thus, rather than try to control for the likely payments by insurance, I focus on disasters least likely to be covered by insurance, in particular, removing earthquakes, wind disasters and tornados from my sample. An added benefit from this is that these are the disasters most likely to invoke state or federal aid packages.

Table 1, panel B contains a breakdown of the disaster statistics by disaster type over the sample period 1996-2002. I aggregate sub-categories (e.g., hail into storms) to present these general statistics. Floods and landslides represent the most number of disasters (137 incidents) and communities affected (2,175). Eighty-two storms account for 1,381 zip code observations. Finally, 59 wildfires inflicted damage on 701 zip code communities. Not surprisingly, property damage inflicted (per incident) is much higher for wildfires and floods than storms. Since foreclosure and crime data are not inclusive of all zip codes, the estimations use only a subset of these disasters.

5 Foreclosure Results

Tables 3 and 4 present the matched sample foreclosure results. The dependent variable is the change in the quarterly rate of foreclosures for the zip code, where change is defined to be the average foreclosure rate in quarters 4 to 7 after the disaster (aligned for the match group) minus the average foreclosure rate in the 4 months prior to the disaster. I lag the post period three quarters to account for the average time in California for delinquency to culminate in foreclosure. There is a single collapsed observation for a zip code to eliminate the serial correlation concerns in differencing specifications highlighted by Bertrand, Duflo, and Mullainathan (2004).

The independent variables are those from equation (7) plus changes in zip code level house prices (quarterly averaged, as in the dependent variable), changes in payroll per population (yearly), and changes in the number of establishments (yearly). Columns 2 and 4 include the covariates interacted with *Disaster* to remove any resiliency effects of the extent to which the natural disaster impacted the community.

The estimations in columns 1 and 2 use constrained least squares, with robust standard errors and year dummies. The Table shows which variables are constrained to be equal with notation (c1) for the first constraint and (c2) for the second constraint. The first constraint forces the coefficient ($\widehat{\alpha_2}$) on ρ to be equal to the coefficient on $(1-\rho)*Disaster$. This coefficient is interpreted as the effect of financial distress on foreclosure rate changes. The second constraint forces the coefficient ($\widehat{\alpha_3}$) on $\rho * Lender$ to be equal to the coefficient on $(1-\rho)*Disaster*Lender$. This coefficient captures the interaction of both types of financial distress with $Lender$, interpreted as the additional effect of financial distress on foreclosure rate changes for communities with access to a lender.

Columns 1 and 2 show that financial distress, as captured by the coefficient for rows one (ρ) and four ($(1-\rho)*Disaster$), has a strong positive impact on foreclosures, as expected. However, access to a lender (rows two and five) mitigates this impact. The difference in the main coefficients of interest from the model do not vary much from column 1 to column 2, suggesting that the column 1 estimation is not just identifying omitted resiliency variables.

Before interpreting these coefficients, I repeat the estimations of 1 and 2 in an unconstrained framework, just using an OLS difference-in-differences approach (technically, it is a triple differencing since the dependent variables is in changes), presented in Columns 3 and 4. I unconstrain my estimations to ensure that I am identifying off the natural experiment of disasters and not the propensity of residents to be financial constrained. The impact of disasters on individual's welfare is very similar in columns 3 and 4 to that in columns 1 and 2. Thus, the main result is robust to relaxing the constraint that forces all types of distress to impact foreclosures equally.

Since columns 3 and 4 are more direct and conservative than the first two columns, I interpret the economic magnitude out of column 4. The pre-disaster mean number of foreclosures is 3.2 per quarter per 1,000 owner occupied housing. The constrained coefficient on $Disaster$ is 1.6, implying that a disaster or other distress causes 1.6 more foreclosures per 1,000 homes, a 50 percent increase. When individuals have access to lenders, all but a 0.3 increase in foreclosures is mitigated. Access to finance seems to mitigate 1.3 foreclosures per 1,000 homes that would have resulted from financial distress.

The covariate coefficients are in line with expectations. Zip codes with increasing house prices experience lower growth in foreclosures, as expected. Much of this effect is eliminated during disasters. More payroll is also associated with fewer foreclosures, an effect not impact by disasters. More commercialism in the community, as measured by changes in establishments, increases the growth of foreclosures. However, if communities are resilient to disasters in that they do not lose establishments, they experience a lower increase in foreclosures.

I now tun to the IV results. To ensure that I can make a causal claim on the relationship between lenders and foreclosures, Table 4 presents the results once I employ intersections as an instrument. I use a control function approach to instrumental variables in which the residuals from the first stage are included in the second stage. I do this because the need to interact the instrument with disasters in the second stage creates nonlinearities in way the instrument enters the second stage. Wooldridge (2001) suggests that the control function approach is preferable under such conditions.

The right hand side of Table 4 shows the first stage regression, using intersections as the instrument. I include area as a covariate in both stages to account for size differentials in zip codes. Intersections is significant at the 1% level in predicting whether a payday lenders exists in a location. All of the covariates except house prices are significant. More payroll growth, more establishment growth and less area predict the location of lenders. The first-stage F-statistic of 31.64 passes the threshold for instrument relevance. I take the predicted probability from this regression as the instrument for *Lender*.

I correct the second-stage residuals for the generated regressor by bootstrapping the first stage 500 times and then using the 500 different predicted values for IV_Lender in 500 new second stage estimations. I then add the variance created across the 500 new coefficient estimates to the parameter's robust variance from estimating the second stage as if IV_Lender were not a generated regressor (Petrin and Train, 2001). The second stage is estimated using OLS as a triple difference as in column 4 of Table 3. For brevity, I do not show all of the covariate coefficients; they are very similar to column 4 of Table

3.

Perhaps the most important change induced by the IV specification is that the magnitude of the estimates on the main variables (*Disaster*, *Lender*, and *Disaster**Lender*) are larger. In a manner, this is mechanical. The original variable *Lender* is an dummy variable with mean and standard deviation both equal to 0.5. IV_Lender is continuous, with the same mean, but a standard deviation equal to 0.20. Thus, the coefficient on *Lender*, and *Disaster**Lender* may just be a reflection the tighter deviation around the mean.

The key coefficient estimate on $IV\_Lender * Disaster$ is -3.09. The significance drops with the bootstrapping of the errors from the first stage, but the result is still interpretable. To compare the Table 4 result to Table 3, I consider the case of being two standard deviations more likely to have a lender in a community. (This would be comparable, in standard deviations, to moving from no lender to having a lender in Table 3.) When natural disasters hit, a two standard deviation higher likelihood of having a lender mitigate reduces foreclosures by 0.40 times -3.09, equalling 1.23. These results are very similar to the results in Table 3: the existence of a lender mitigates 1.23 foreclosures during distress per 1,000 homes in the community. Intuitively, once the IV is applied, I find that disasters cause foreclosures to increase by 72% (=2.3/3.2) and that lenders mitigate a little more than half (56%) of this increase in foreclosures following exogeneously-induced financial distress.

As robustness, I consider the popular view that payday lenders target military bases. (The federal government made lending to military personnel illegal in 2006.) Because there are many military bases in California and because military personnel may not follow a regular pattern of foreclosures, it could be that I am picking up a military effect. In order for this to explain my results it must be that lender communities with military bases are prevalent in areas hit by disasters and lender communities without military bases are prevalent in areas not hit by disasters (or vice versa). Nevertheless, to the extent that this is true, I re-run my tests throwing out all military communities. I measure a military community by whether there exists a military bank or its ATM in the zip code. Locations for military banks and ATMs are from the Army Bank, Navy Bank, Air Force Bank and

Bank of America Military Bank web pages. I find no change in my foreclosure results.

6 Small Property Crime Results

Table 5 reports the main results for the three small property crime variables – larceny, vehicle theft and burglary. The dependent variable is the change in annual crimes per household, where change is defined to be the average crimes in the year of the disaster (aligned for the match group) minus the average foreclosure rate in the year prior to the disaster. I include the same resiliency covariates – establishments and community payroll per capita,– but instead of house prices, I include violent crimes. All covariates are also interacted with the disaster indicator.

The estimations in columns 1-3 use constrained least squares, with robust standard errors and year dummies. The Table shows which variables are constrained to be equal with (c1) and (c2) notation, as in the foreclosure estimations. Columns 4-6 repeat the estimations for the three dependent variables with the simpler tripe difference specification.

Noticeable immediately is the fact that natural disasters do not impact vehicle thefts or burglaries. It could be that these crimes, the more serious of the three property crimes, reflect actions by more organized territorial or business-oriented crime operations. I do find, however, that distress identified by natural disasters increases larcenies with a statistical significant estimate of 11.20. To put this in context, an increase of 11.20 larcenies per household arises from pre-period mean of 68 larcenies per 1,000 households, an 18 percent increase. When individuals have access to a lender, all of larceny growth increase following the natural disaster is mitigated. It is worth noting that lender communities have a steeper growth profile in larcenies, with a higher growth of 9.98 larcenies relative to non-disaster communities. The results in the unconstrained triple difference specification of column 4 are very similar.

A brief look at the covariate resutls is also interesting. As expected, changes in violent crimes explain, positively, much of the variation in non-violent small property crime changes. Disasters increase the postive relationship between violent and small property crimes, almost doubling the sensitivity. The only other covariate that is significant is payroll changes and only for larcenies and vehicle thefts, and as in the foreclosure results, diasasters do not impact this sensitivity.

Table 6 reports the crime results for the instrumental variables specification, focusing on the triple difference specification to minimize the interactions with the instrument. Again vehicle thefts and burglaries are not impacted by natural disasters or access to credit. I find that access to credit has a mitigating effect of financial distress, but only for larcenies, even when instrumenting the location of lenders with intersections. The size of the coefficient on $IV\_Lender{*}Disaster$ is very similar to its parallel (column 4) in Table 5. However, as in the foreclosure results, I have to adjust interpretation to the fact that IV_Lender is a continuous variable with a tighter standard deviation. The resulting IV magnitudes are as follows: Natural disasters cause larcenies to increase by 8.766 or 13% (=8.766/68), and lenders mitigate 2.67 crimes (=13.36*0.2) or 30% of that increase in larcenies following exogeneously-induced financial distress. This result is significant, but only at the ten precent confidence level due to the extra variance of the generated regressor, as captured by the bootstrapped standard errors. Perhaps most interesting from the series of crime results is that financial distress and the benefit from access to credit matters only for the smallest of the small property crimes, where the connection between the need for cash and criminal action is arguably the most direct.

7 Conclusions

Taking advantage of the exogenous shock of natural disasters in a triple difference framework, I find that the existence of payday lending increases welfare for households who may face foreclosures or be driven into small property crime in times of financial distress. In particular, my results indicate that in times of distress, access to credit mitigates 1.22 foreclosures per 1,000 homes and discourages 2.67 larcenies per 1,000 households. The implication is that access to finance can be welfare improving at 400% APR.

My results speak to the benefits of local finance for individuals. Prior research documents the benefits of access to finance for aggregate growth (e.g., Jayaratne and Strahan (1996); Rajan and Zingales (1998); and Levine and Demirguc-Kunt (2001)), firm entrant growth (Guiso, Sapienza and Zingales (2004); Cetorelli & Strahan (2006); Paravisini (2006)) and corporate bankruptcy recovery (e.g., Dahiya, John, Puri and Ramirez (2003)), but little work has been done to gauge the benefit of access to finance in individuals-specific measures (Garmaise and Moskowitz, 2006). In addition, my work speaks to the community-level importance of resiliency. I find that financial institutions aid the resiliency of communities to financial downturns, a important topic not just for natural disaster recovery but aslo for planning for economic downturns and structural job shifts.

My results have important policy implications for payday lending. Fifteen States have recently banned payday lending, and legislation is pending in the many of the others. If the existence of payday lending is valuable for those facing personal disaster, then regulators should strive to make access to finance easier and more affordable, not to ban it. Payday lending may not be the best product conceivable, and that efforts should be focused on opening up the market for product innovation in high-risk and short-term personal finance.

There is an important caveat to my results. The results generalize to the common occurrence of personal emergencies. However, I do not capture the welfare impact of payday lenders on those borrowing in ordinary economic circumstances to fund temptation consumption. For this subset of the population, I am not able to capture the full negative implications to the temptation brought by payday lending.

That fact that finance may fodder temptation is an avenue for future research. Is it possible to document other cases, like payday lending for everyday users, in which access to finance has negative welfare consequences? If so, how much of consumer finance is servicing such consumption? Because consumer finance is the area of finance closest to consumption decisions, further empirical studies of household decision-making are likely to provide important insight even beyond the importance of the consumer finance market.

Appendix - Payday Lending Profitability

The $40 billion in payday loans generate an estimate of $5.4 billion in fee revenues (Center for Responsible Lending; 2004). Are these fees and the implied APR over 400% reasonable? Transaction costs per dollar of loan in the payday market are high. It is useful to think in terms of $50, rather than 400%. An initial payday loan takes on average fifteen minutes of labor and physical capital; subsequent loans take less. The lender subscribes to a banking account verification service as well as cash delivery services. Transactions records for North Carolina report a default of 6% per loan (Center for Responsible Lending, 2004), implying $18 in expected cost for a $300 loan. Adding up these costs leaves the question of profitability still unanswered. Flannery and Samolyk (2005) argue that payday lenders do become quite profitable, but not until the store has survived a number of years to establish a large clientele (also see Stegman and Faris, 2003). Skiba and Tobacman have work-in-progress on this topic directly. If correct, why would entry not drive out these rents, given that setup cost are minimal?

Two factors may be at work to impede entry. First, observed profit rates are different from their expected rate because there is a significant probability that State regulators will shut down payday stores altogether. Payday lending is now essentially illegal in fifteen States. In addition, entry may be deterred because the majority of payday borrowers are repeat customers, facing switching costs similar to those highlighted by Ausubel (1991) for the credit card industry. Those costs include the cost of shopping for lower rates, going through the application process, and foregoing any benefits of nurturing a favorable payment record with a lender. If Shui and Ausubel (2005) are correct in their characterization of the credit card market, borrowers may over-weight the short-term switching costs relative to long-term benefits of lower rates, especially if they procrastinate (Ravina, 2006) or fail to correctly incorporate the probability of not being able to pay off the loan in the next pay period as in Ausubel's (1991) credit card model.

The key points is that although payday lenders have acted in a near vacuum of household lending above 30% APR,competition has not eroded the 400% APR rates because of transactions cost involved in each small-scale loan and possibly because of entry deterrence caused by threat of abolishment of the industry and switching costs for borrowers.

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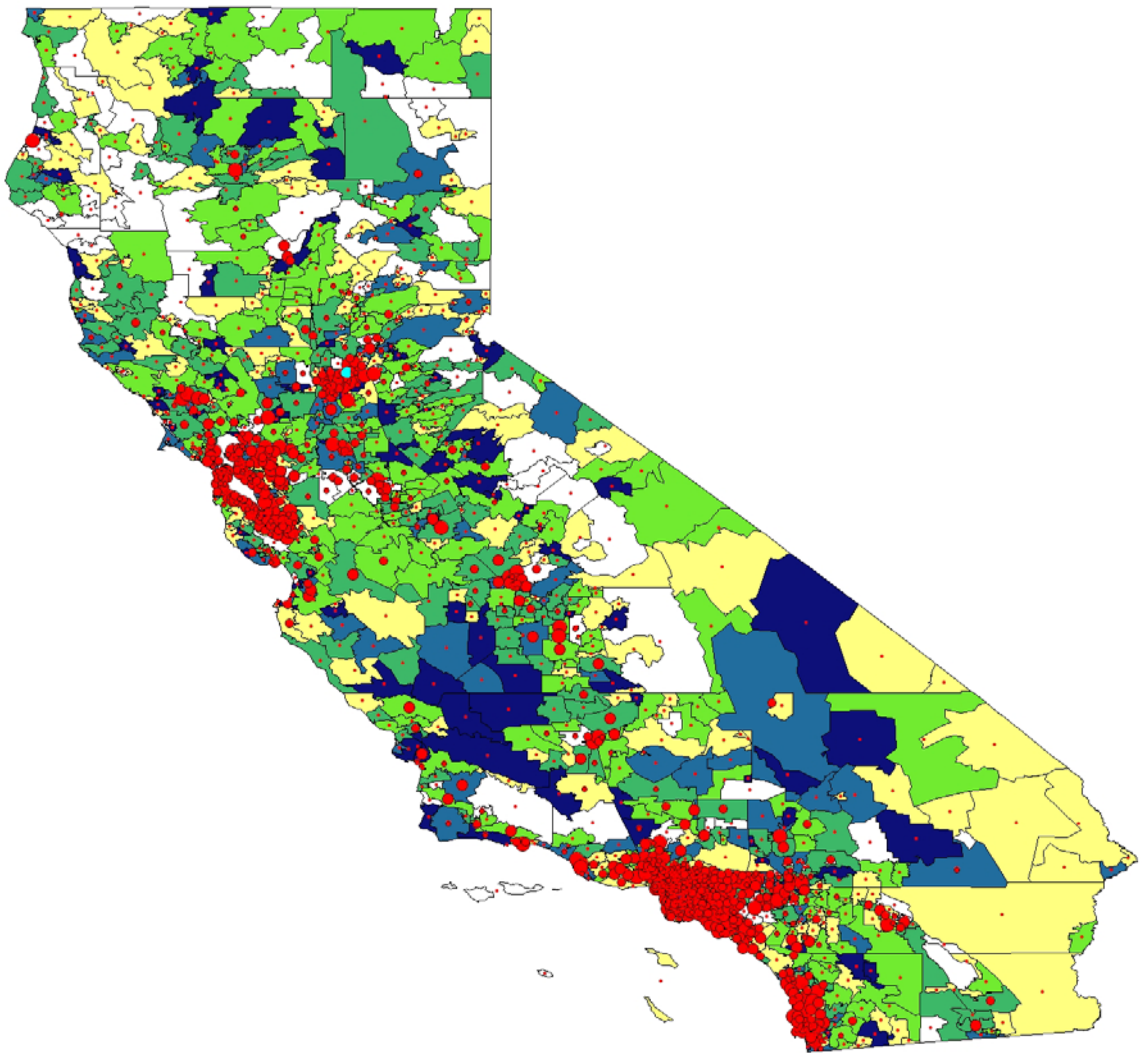

Figure 1: California Payday Lending Locations and Zip Code Propensities to be Constrained
The dots indicate the density of payday lenders in each zip code for 2002; larger dots indicate a higher quartile of payday lender counts. The minimum size dot indicates no lenders are in the zip code. The blocks shown are the 2001 zip code delineation from the postal service. The darker the shading on the zip code, the higher the propensity to be credit constrained is according to the matching methodology projections in section 3.3. The few zip codes with entirely white shading are those for which the post office altered during the sample or those of natural parks. These are not included in the analysis.

Table 1: Summary Statistics

Panel A: SCF Variables of Facing Financial Constraints

Below are the variables calculated in 1998 Survey of Consumer Finance (SCF) to measure socioeconomic factors associated with being credit constrained.

	Definition	*Mean*	*StdDev*
AtLimit	Indicator for being within $1,000 of limit on credit card	0.090	0.287
HiDebt	Indicator credit card debt > 10% yearly income	0.283	0.451
BehindPayments	Indicator for self-disclosed being behind on payments	0.123	0.334

The final matching measure of financial constraints ρ is created by simply averaging the three measures to be agnostic as to the rank of importance among of the three measures. The averaging is as follows, where the hat denotes the predicted value from the projection onto the zip code demographics:

$$\rho \equiv 10\% \cdot \frac{1}{3}\left[\frac{\hat{AtLimit}}{mean(AtLimit)} + \frac{\hat{HiDebt}}{mean(HiDebt)} + \frac{\hat{Behind}}{mean(Behind)}\right]$$

Panel B: Financial Institutions, Welfare & Control Variable Data

All variables are at the zip code level for 1996-2002. The sample for foreclosure and crime variables is the matched group used in the estimation. The sample for the other variables is the total pool of observations, since the matching chooses different samples for the different welfare measures. Quarterly housing prices and foreclosure counts are from the California Association of Realtors. Yearly crime data are from the State of California Criminal Justice Statistics Center. Yearly data on payday lending are from the State of California Department of Corporations. Intersections in 2006 are calculated from maps of the California Department of Transportation. The count of FDIC Banks is obtained by collapsing addresses from the FDIC database to zip codes. Population and number of owned housing units are from the U.S. Bureau of the Census for the 1990 or 2000 Census or the 1997 Update, depending on the year in question. Yearly establishment, payroll and employment data are from the Bureau of Labor Statistics.

		Mean	*Minimum*	*Median*	*Maximum*	*St. Dev.*
Foreclosures	(*quarterly*)	10.9	0	6	205	15.7
Foreclosures per Owner Occupied Housing		3.0	0	1.5	173	10.3
Larceny Theft (e.g., shoplifting)	(*yearly*)	672	0	259	11,003	1,172
Larcenies per Household		59.2	0	16.6	1,857	169
Vehicle Thefts	(*yearly*)	145	0	34	3,453	309
Vehicle Thefts per Household		10.9	0	2.38	361	31.4
Burglaries	(*yearly*)	232	0	109.5	3,475	374
Burglaries per Household		19.9	0	6.86	489	49.6
Payday Lenders	(*yearly*)	1.9	0	1	36	3.2
Intersections	(*yearly*)	74.8	0	55	660	80.4
Housing Prices ($)	(*quarterly*)	229,438	923	185,535	2,376,392	168,592
Violent Crimes	(*yearly*)	158	0	60	3,693	314
Establishments	(*yearly*)	1,505	3	293	14,158	2,103
Payroll per population	(*yearly*)	96.9	0	5.91	88,590	2,422

Panel C: Disaster Data

Natural disasters data for 1996-2002 are from the University of South Carolina's Sheldus Hazard database, which identifies the location, type and magnitude of natural disasters. Earthquakes and wind damage storms are removed, because of varying insurance implication across households. The Sheldus database measures disasters at a county level. Column 3 presents the count of counties hit by a disaster in the database. For each disaster in Sheldus, I locate the specific zip codes of the counties affected by the disaster using the comment field in the database, which often provides cities affected. The number of zip codes affected from this is in the last column.

	Mean Property Damage	*Median Property Damage*	*Count of Disasters*	*Communities Affected*
Flood/Landslide	12,501,720	2,000,000	137	2,175
Storm/Winter Weather/Coastal Weather	324,567	140,000	82	1,381
Wildfire	3,215,960	3,215,960	59	701
All	7,022,281	390,909	278	4,257

Table 2: SCF & Census Variables with Logit Estimations of Constraints

The first column presents the average across zip codes of the proportion of population (or households) in each variable category. For example, the first line is interpreted as 21.5% percent of the mean community have an income less than $15,000. The last three columns present the logistic estimation results for the dependent variables *AtLimit, High Debt/Income*, and *BehindPayments*. Standard errors are not presented in interest of space. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Variable	*Census: Zip Mean Proportion*	*SCF Logit: At Credit Card Limit*	*SCF Logit: High Debt/Income*	*SCF Logit: Behind on Payments*
$ 0 ≤ Household income < $ 15,000	0.215	2.183***	1.507***	1.158***
$ 15,000 ≤ Household income < $ 30,000	0.162	2.454***	1.978***	1.267***
$ 30,000 ≤ Household income < $ 45,000	0.274	2.472***	1.948***	1.263***
$ 45,000 ≤ Household income < $ 60,000	0.132	2.240***	2.059***	0.782***
$ 60,000 ≤ Household income < $ 75,000	0.082	2.111***	2.047***	0.730***
$ 75,000 ≤ Household income < $100,000	0.066	1.778***	1.594***	0.527*
$100,000 ≤ Household income < $125,000	0.031	1.805***	1.782***	0.879***
$125,000 ≤ Household income < $150,000	0.013	0.982	0.922***	0.616
$150,000 ≤ Household income	0.026	--	--	--
Unemployed Persons	0.082	-0.094	-0.197	-0.048
12 ≤ Persons' Age ≤ 17	0.093	--	--	--
18 ≤ Persons' Age ≤ 24	0.122	2.025***	1.703***	1.080***
25 ≤ Persons' Age ≤ 34	0.218	1.869***	1.791***	1.627***
35 ≤ Persons' Age ≤ 44	0.195	1.498***	1.705***	1.646***
45 ≤ Persons' Age ≤ 54	0.127	1.588***	1.647***	1.666***
55 ≤ Persons' Age ≤ 64	0.101	1.257***	1.280***	1.309***
65 ≤ Persons' Age ≤ 74	0.089	0.801*	0.805***	0.406
75 ≤ Persons' Age	0.056	--	--	--
Educated 0 – 8 years	0.110	0.199	0.218	-0.182
Educated 9 – 12 years, no degree	0.134	0.205	-0.015	0.418**
High School Graduate	0.236	0.304	0.282**	0.035
Attended Some College	0.225	0.326	0.542***	0.240
Associate Degree	0.075	0.083	0.583***	0.169
Bachelors Degree	0.142	0.128	0.187	0.037
Graduate Degree	0.077	--	--	--
Homeowning Households	0.204	0.080	0.218**	-0.313**
$ 0 ≤ Shelter Costs < $ 300	0.279	0.053	-0.533***	0.308*
$ 300 ≤ Shelter Costs < $ 500	0.173	0.262	-0.094	0.450**
$ 500 ≤ Shelter Costs < $ 750	0.185	0.273	0.210	0.555***
$ 750 ≤ Shelter Costs < $1,000	0.129	0.207	0.125	0.461**
$1,000 ≤ Shelter Costs	0.234	--	--	--
Owns 1+ Vehicles	0.922	0.354*	0.828***	0.244
Female Persons	0.470	0.182	0.341***	-0.108
Non-white Persons	0.158	0.379***	-0.112	0.229*
Person per Household = 1	0.234	--	--	--
Person per Household = 2	0.318	0.122	-0.016	-0.056
3 ≤ Person per Household ≤ 5	0.390	0.135	-0.087	0.291**
Person per Household ≥ 6	0.058	0.417	0.005	0.072
Married Persons	0.220	0.130	0.334***	-0.104
Observations in SCF		4305	4305	4305
Pseudo R-Square		0.104	0.150	0.096

Table 3: Effect of Lenders on Foreclosures during Distress

The dependent variable is the change in quarterly foreclosures per owner occupied home around the natural disaster or its match in time. The pre- period is the four quarters before the event and the post period is quarters 4-7 after the disaster or its match. The three quarter interim lag allows for the average time for foreclosures to happen in California. Year dummy variables are included but not shown. Columns 1 and 2 are estimated using constrained least squares. Columns 3 and 4 are estimated in unconstrained OLS. Coefficients constrained to be equal are marked with (c1) and (c2). Year dummy variables are included. ***, **, and * denote significance at the 1%, 5%, and 10% levels. Robust standard errors are reported in brackets.

Dependent Variable: Δt Quarterly Foreclosures per Owner Occupied Home					
		Constrained LS		*Unconstrained DDD*	
		(1)	*(2)*	*(3)*	*(4)*
ρ	c1	1.749**	1.753**		
		[0.711]	[0.718]		
Lender		1.412**	1.526**	1.268**	1.367**
		[0.678]	[0.659]	[0.602]	[0.587]
ρ *Lender	c2	-1.302*	-1.466**		
		[0.746]	[0.709]		
(1- ρ)*Disaster	c1	1.749**	1.753**		
		[0.711]	[0.718]		
(1- ρ)*Lender*Disaster	c2	-1.302*	-1.466**		
		[0.746]	[0.709]		
Disaster				1.556**	1.560**
				[0.632]	[0.639]
Lender*Disaster				-1.154*	-1.303**
				[0.664]	[0.631]
Δ House Price		-3.208**	-7.211***	-3.211**	-7.219***
		[1.324]	[2.669]	[1.324]	[2.669]
Disaster*Δ House Price			6.407**		6.416**
			[2.859]		[2.860]
Δ Payroll per Population		-0.154***	-0.169***	-0.153***	-0.169***
		[0.013]	[0.017]	[0.013]	[0.017]
Disaster*Δ Payroll /Pop.			0.033		0.033
			[0.023]		[0.023]
Δ Establishments		0.0018***	0.0022***	0.0018***	0.0022***
		[0.0005]	[0.0007]	[0.0005]	[0.0007]
Disaster*Δ Establishments			-0.0019**		-0.0019**
			[0.0009]		[0.0009]
Observations		1301	1301	1301	1301
R-Square		.	.	0.663	0.673

Table 4: IV Estimation of Effect of Lenders on Foreclosures during Distress

Because the IV is interacted in the second stage, I use a control function estimation, putting residuals from the first stage in the second stage estimation. Second stage standard errors are corrected by bootstrapping the first stage 500 times, using the 500 different predicted values for *IV_Lender* in 500 new second stage estimations, and then adding the variance created across the 500 new coefficient estimates to the parameter's robust variance from estimating the second stage as if *IV_Lender* were not a generated regressor (Petrin and Train, 2001). The second stage is estimated using OLS as in column 4 of Table 3. The second stage dependent variable is growth in foreclosures as in Table 3. Year dummy variables are included. All covariates from Table 3, column 4 are also included but not shown for brevity. Their results do not materially differ from those in Table3. ***, **, and * denote significance at the 1%, 5%, and 10% levels from the bootstrapped errors.

Second Stage *Dependent Variable: After-Before Foreclosures per Home*		*First Stage* *Dependent Variable: Existence of a Payday Lender*	
	OLS-DDD		*OLS*
IV_Lender	3.874**	Intersections	0.0023***
	[1.642]		[0.0002]
Disaster	2.294**	Δ House Price	0.1191
	[1.107]		[0.1320]
IV_Lender*Disaster	-3.090*	ΔPayroll per Pop	0.0051***
	[1.791]		[0.0017]
Area	-0.096	Δ Establishments	0.0006***
	[0.491]		[0.0002]
1st Stage Residuals	0.343	Area	-0.1014*
	[0.226]		[0.0574]
Observations	1301	Constant	0.2846
R-Square	0.674		[0.2163]
		Observations	1,108
Not shown: Year dummies, ΔHouse Price, Disaster*ΔHouse Price, ΔPayroll /pop, Disaster*ΔPayroll/pop., ΔEstablishments , Disaster*ΔEstablishments		R-Square	0.126
		F-Statistic	31.64

Table 5: Effect of Lenders on Small Property Crimes during Distress

The dependent variable is the change in annual crimes per zip code household. Change is calculated as the average crimes in the year of the disaster (aligned for the match group) minus the average foreclosure rate in the year prior to the disaster. I include the same resiliency covariates -- establishments and community payroll per capita – as in the foreclosure tests, but instead of house prices, I include violent crimes. Columns 1-3 are estimated with constrained LS, and 4-6 with OLS. Coefficients constrained to be equal are marked with (c1) and (c2). Year dummies are included but not shown. ***, **, and * denote significance at the 1%, 5%, and 10% levels. Robust standard errors are reported in parentheses.

		Constrained LS			*Unconstrained DDD*		
		Larceny	*Vehicle Theft*	*Burglaries*	*Larceny*	*Vehicle Theft*	*Burglaries*
		(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
ρ	c1	11.20*	0.862	0.141			
		[5.779]	[1.063]	[1.863]			
Lender		9.979**	1.426*	1.155	8.585**	1.318*	1.060
		[4.615]	[0.851]	[1.309]	[4.108]	[0.756]	[1.154]
ρ *Lender	c2	-12.70**	-0.933	-0.786			
		[6.165]	[1.174]	[2.074]			
(1- ρ)*Disaster	c1	11.20*	0.862	0.141			
		[5.779]	[1.063]	[1.863]			
(1- ρ)*Lender*Disaster	c2	-12.70**	-0.933	-0.786			
		[6.165]	[1.174]	[2.074]			
Disaster					9.923*	0.751	0.091
					[5.165]	[0.947]	[1.664]
Lender*Disaster					-11.28**	-0.818	-0.681
					[5.503]	[1.047]	[1.851]
Δ Violent Crime		0.105***	0.023***	0.039***	0.105***	0.023***	0.039***
		[0.013]	[0.002]	[0.004]	[0.013]	[0.002]	[0.004]
Disaster*ΔViolent Crime		0.122***	0.021***	0.059***	0.122***	0.021***	0.059***
		[0.037]	[0.006]	[0.013]	[0.037]	[0.006]	[0.013]
Δ Payroll per Population		1.972*	0.345*	0.067	1.966*	0.344*	0.066
		[1.081]	[0.184]	[0.330]	[1.080]	[0.183]	[0.330]
Disaster*ΔPayroll per Pop		-1.307	-0.363	0.148	-1.299	-0.361	0.151
		[3.053]	[0.602]	[1.235]	[3.053]	[0.602]	[1.235]
Δ Establishments		-0.0128	0.0005	-0.0037	-0.0129	0.0005	-0.0037
		[0.0120]	[0.0024]	[0.0042]	[0.0120]	[0.0024]	[0.0042]
Disaster*Δ Establishments		0.0104	-0.0012	0.0002	0.0106	-0.0012	0.0003
		[0.0207]	[0.0040]	[0.0077]	[0.0207]	[0.0041]	[0.0077]
Observations		764	764	764	764	764	764
R-Square		.	.	.	0.332	0.358	0.405

Table 6: IV Estimation of Effect of Lenders on Small Crime during Distress

Because the IV is interacted in the second stage, I use a control function estimation, putting residuals from the first stage in the second stage estimation. Second stage standard errors are corrected by bootstrapping the first stage 500 times, using the 500 different predicted values for *IV_Lender* in 500 new second stage estimations, and then adding the variance created across the 500 new coefficient estimate to the parameter's robust variance from estimating the second stage as if *IV_Lender* were not a generated regressor (Petrin and Train, 2001). The first stage is almost identical to that for foreclosures displayed in Table 4. The 2nd stage is estimated using OLS. Year dummies are included, as are all the covariates from Table5. The dependent variables are changes in property crimes as in Table 5. ***, **, and * denote significance at the 1%, 5%, and 10% levels from the bootstrapped errors.

	Larceny	*Vehicle Theft*	*Burglaries*
	(1)	*(2)*	*(3)*
IV_Lender	2.970	1.366	0.636
	[2.453]	[0.900]	[1.332]
Disaster	8.766*	1.055	0.949
	[4.552]	[1.100]	[1.879]
IV_Lender*Disaster	-13.36*	-1.513	-2.303
	[7.431]	[1.436]	[2.619]
Residuals / Control Function	0.734	0.759*	0.915
	[2.006]	[0.445]	[0.801]
Area (in 100 sq meters)	-0.267	0.021	-0.023
	[0.773]	[0.070]	[0.134]
Observations	759	759	759
R-Square	0.437	0.384	0.43

Not shown: Year dummies, ΔViolentCrime, Disaster*Δ ViolentCrime, ΔPayroll /pop, Disaster*ΔPayroll/pop., ΔEstablishments , Disaster*ΔEstablishments