



Payday lenders: Heroes or villains? ☆

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ARTICLE INFO

Article history:

Received 23 September 2009

Received in revised form

19 August 2010

Accepted 7 September 2010

Available online 30 March 2011

JEL classification:

D14

G21

Keywords:

Payday lending

Access to credit

Natural disasters

Foreclosures

Welfare

ABSTRACT

Does access to high-interest credit (payday loans) exacerbate or mitigate individual financial distress. Using natural disasters as an exogenous shock, I apply a propensity score-matched, triple-difference specification to identify a causal relation between welfare and access to credit. California foreclosures increase by 4.5 units per 1,000 homes after a natural disaster. The existence of payday lenders mitigates 1.0–1.3 of them, with the caveat that not all payday loans are for emergency distress. Payday lenders also mitigate larcenies (but not burglaries or vehicle thefts). In a placebo test of disasters covered by homeowner insurance, payday lending has no mitigation effect.

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There is little debate that access to finance enhances value for firms.¹ A similar consensus does not exist, however, 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, 2011; Lusardi and Tufano, 2008) or engage in utility-destroying temptation consumption (O'Donoghue and Rabin, 2007), financial institutions might cater to these biases (Campbell, 2006), and access to finance could make borrowers worse off.

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

How can small shocks lead to such drastic outcomes? Individuals frequently experience some sort of personal emergency (e.g., an out-of-pocket medical expense or car breakdown) leaving them without cash for their short-term obligations. Without access to credit, these small-scale personal emergencies can lead to bounced checks, late fees, utility suspensions, repossessions, and, in some

☆ I greatly benefited from comments and suggestions during seminars at Berkeley, Columbia, Duke, the European University Institute, the FDIC, the Federal Reserve Bank of Cleveland, the Federal Reserve Bank of New York, Harvard Business School, MIT, New York University, Northwestern University, Ohio State University, UCLA, University of Chicago, University of Illinois, University of Maryland, University of Michigan, University of Southern California, Wharton, Yale, the WFA, and the European Summer Symposium in Financial Markets (Gerzensee). In addition, I would like to thank my committee E. Han Kim, Michael Barr, Fred Feinberg, Tyler Shumway, and Luigi Zingales as well as David Brophy, Alexander Dyck, Amiyatosh Purnanandam, and Amit Seru for their helpful comments.

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¹ See Jayaratne and Strahan (1996), Rajan and Zingales (1998), Levine and Demircug-Kunt (2001), Dahiya, John, Puri, and Ramirez (2003), Guiso, Sapienza, and Zingales (2004), Cetorelli and Strahan (2006), and Paravisini (2008).

cases, foreclosures, evictions, and bankruptcies. The United States works very much on a fee-based system for delinquencies, such that once low-margin individuals get into distress, they often end up in a cycle of debt. 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 \$50 billion in loans each year.² Despite (or because of) the growing demand, state and federal authorities are working toward 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 them to smooth liquidity shocks, a welfare-enhancing proposition. An opposite perspective is that payday lending destroys welfare. The availability of cash from payday loans might tempt individuals to overconsume. An individual who is likely to succumb to temptation might prefer the discipline of limited access to cash before temptation arises (Gul and Pesendorfer, 2001, 2004; O'Donoghue and Rabin, 2007; Fudenberg and Levine, 2006). A related argument is that because individuals might be naive about time-inconsistent preferences, they could spend with a bias toward the present moment (e.g., Jones, 1960; Thaler, 1990; Attanasio and Browning, 1995; Stephens, 2006) or be unable to save adequately (e.g., Thaler and Shefrin, 1981; Laibson, 1997; Laibson, Repetto, and Tobacman, 1998; Choi, Laibson, and Madrian, forthcoming). Cash (or access to cash) from payday lending might encourage either present-biased consumption or a lack of saving. In these views, payday lending can be welfare destroying.³

To determine 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 for the period 1996–2002. The difficulty in measuring how payday lending affects welfare over time lies in disentangling a causal payday lender effect from endogenous location decisions of lenders and from correlated community economic circumstances that cause welfare outcomes. To overcome the endogeneities, I set up a matched triple-difference framework. 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 U.S. Federal Reserve's Survey of Consumer Finances (SCF) is financially constrained as a function of socioeconomic characteristics. I then project the relation onto zip codes by applying the SCF coefficients to socioeconomic variables observed at the community level in the U.S. Census.

Matching alone does not solve the endogeneities of the lender location decision, but it 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.

The results indicate that payday lenders offer a positive service to individuals facing financial distress. Natural disasters increase foreclosures by 4.5 units per 1,000 homes in the year following the event, but payday lenders mitigate 1.0 to 1.3 units of this increase. In rate terms, natural disasters increase the rate of foreclosures per home from 0.972% to 1.5% in my sample of zip codes. (As a comparison, the 2009 foreclosure rate for California was 1.88% following the financial crisis.) Lenders mitigate 0.10–0.12% of the disaster-induced increase, after controlling for a number of different disaster resiliency stories. In a placebo test for natural disasters covered by homeowner insurance, I find no payday lending mitigation effect. The results also indicate that payday lenders alleviate individuals' need to resort to small property crimes in times of financial distress. I find significant results, however, only for larceny (shoplifting), the crime that carries the lightest sentencing of all property crimes.

My experimental design necessitates a caveat in how the results can be interpreted. Individuals can use payday loans in situations not caused by financial distress. In a survey of payday borrowers, Elliehausen and Lawrence (2001) report that 33% of loans are not for emergency needs. Some borrowers might habitually overconsume 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 overconsumers are those most likely to have negative welfare effects from temptation consumption. Because I do not identify the net benefit of payday lending across the distribution of borrowers, my results should be interpreted as payday lenders providing a valuable service to individuals facing unexpected financial distress (any type of unexpected financial distress) but do not speak to the effect on those habitually falling to temptation. In this sense, payday lenders can be both heroes and villains.

A set of concurrent papers also addresses 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 (forthcoming) showing a welfare-destroying role for lenders. However, I believe that these results suggest the pressing importance of understanding the heterogeneity of borrowers and the circumstances they might face (Bertrand and Morse, 2009), as well as the mistakes they might make (Brito and Hartley, 1995; Bernheim and Rangel, 2006; Skiba and Tobacman, 2009; Bertrand and Morse, forthcoming).

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

³ For temptation consumption, the argument of welfare destruction takes an ex ante lifetime consumption view, not a revealed preference one.

The remainder of the paper proceeds as follows. Section 1 offers an overview of the market for payday loans. Section 2 outlines the triple-differencing empirical methodology. Section 3 presents the intermediate propensity-score matching results. Section 4 describes the data sources and summary statistics. Section 5 presents the empirical results for foreclosures and crimes, and Section 6 concludes.

1. Payday lending market

To take out a payday loan, an individual visits a payday lender with his or her most recent paycheck stub and bank statement. (The unbanked and unemployed do not qualify.) A typical loan is \$350 with a fee of \$50. For a \$350 loan, the borrower writes a check (or authorizes a bank draw) for \$400, post-dating it to the next payday, usually 10–14 days hence. The fee is posted on the wall as a dollar fee per \$100 in loan. The implied annual interest rate is usually over 400%, which is disclosed at the closing of the transaction in the loan paperwork. The payday lender verifies the borrower's 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, he or she returns to the payday store and refinances the loan, incurring another \$50 fee, which is paid in cash.

To put payday borrowing in context, one has to consider why borrowers do not seek cheaper forms of finance. Research covering the last three decades finds that up to 20% of U.S. residents are credit constrained (Hall and Mishkin, 1982; Hubbard and Judd, 1986; Zeldes, 1989; Jappelli, 1990; Gross and Souleles, 2002). When expense or income shocks arrive, banks and credit cards usually do not provide these constrained borrowers with distress loans. Default risk and transaction costs make these loans infeasible without lenders coming into conflict with usury laws or the threat of greater regulation. Individuals restricted in access to credit resort to borrowing from high-interest lenders. These fringe financial institutions are only sparsely studied in the finance literature (see Caskey, 1994, 2005), despite the fact that payday lenders issue an estimated \$50 billion in loans per year (*Los Angeles Times*, December 24, 2008). Loans collateralized by car titles (title loans) and household assets (pawnshop loans) offer cheaper alternatives, but because these loans require clear ownership of valuable assets, the markets are much smaller.

The main alternatives to payday lending for individuals in distress are bank overdraft loans and bounced checks. Bouncing checks (or overextending on debit cards) to buy a few days of float is still a very common way to borrow funds. Although the implied interest rate depends on the duration and the number of checks bounced, the cost of bouncing checks is usually close to that of taking out a payday loan. Bouncing checks also adds an implicit cost via a negative entry on one's credit history, which does not happen with payday borrowing. Bank overdraft loans differ from bounced checks in that banks pre-agree to clear the overdraft check(s) for a fee. Overdraft loans are cheaper for the borrower than

bouncing checks since the borrower gains more time to repay the debt. Nevertheless, the overdraft fees can be quite high in annual percentage rate (APR) terms, especially if the checks overdrawn were for small face values. My sample largely predates the widespread availability of overdraft loans, especially for individuals with poor credit history and/or no direct deposit, to whom the bank is unlikely to offer overdraft loans. The upshot of this quick description of the market is that, for the majority of people in my sample, no obvious alternative to a payday loan exists.

2. Empirical methodology

The goal of the analysis is to test the extent to which the existence of a lender mitigates or exacerbates the effect of financial distress on individual welfare. Although I later aggregate to (and estimate at) a community level, the story is one of individuals in distress. Thus, I start with a fairly general depiction of individual distress and welfare.

Eq. (1) is an individual fixed-effects model of welfare in which financial distress (f , an indicator) linearly affects the change welfare, and the existence of a high-interest lender (L) can mitigate or exacerbate the situation:

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

where $\Delta_t w_{izt}$ denotes the change in welfare for individual i in zip code z at time t , and Δ_t refers to a first differencing from $t-1$ to t . I refer to the change over time as welfare growth. The γ_{iz} are the welfare growth fixed effects of individuals. Time dummy variables (τ_t) remove any economy-wide fluctuations in welfare growth. 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 .⁴ If Eq. (1) could be estimated, the estimates of primary interest, $\hat{\alpha}_2$ and $\hat{\alpha}_3$, would capture the extent to which distress affects welfare growth and the extent to which access to a payday lender mitigates or exacerbates the distress effect, respectively.

Three substantial problems exist with estimating Eq. (1). First, the variables necessary to measure welfare and financial distress are not available at the individual level. Second, the location of lenders is endogenous, potentially causing an ordinary least squares (OLS) estimate of α_3 to be biased. Third, financial distress and welfare growth are simultaneously caused by the economic conditions of the community, also implying that OLS estimates of α_2 and α_3 are likely to be biased. Another problem is that the residuals can be serially correlated, but this problem can be handled with relatively more ease. In what follows, I do a couple of transformations on (1) and set up a counterfactual framework to difference away these concerns.

I first break financial distress (f_{izt}) into two types: personal-emergency distress (f_{izt}^{pers}) and natural-disaster

⁴ Eliehausen and Lawrence's (2001) survey evidence finds that individuals do not travel far to go to a lender. For densely populated areas, the next community might only be a short distance away; thus, in estimation, I drop densely populated areas.

distress (f_{izt}^{dis}). For example, a personal emergency occurs when the transmission in one's car gives out, and one depends on the car to get to work but does not have the cash or credit to repair it. A natural-disaster distress example is when one's car floods, leaving a large repair bill.

Since it is possible for both types of distress to occur at the same time, the appropriate indicator-variable breakdown is: $f_{izt} = f_{izt}^{pers} + f_{izt}^{dis} - f_{izt}^{pers} f_{izt}^{dis}$. A benefit from this decomposition is that f_{izt}^{dis} is unrelated to the location decision of the lender. One might be speculate that lenders chase disasters or prefer disaster-prone areas, but this is not empirically supported: the correlation between the occurrence of a disaster and the existence of a lender is 0.005. Lenders depend on the profits created by borrowers with personal-emergency needs, not those whose needs for finance only occur after extreme events.

I next aggregate the model to the community (zip code) level and average over the community population n_{zt} . 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, which I denote by ρ_{zt} , where $\rho_{zt} \equiv (1/n_{zt}) \sum_{i=1}^{n_{zt}} f_{izt}^{pers}$.

Since natural disasters hit areas as opposed to individuals and since zip codes are fairly small areas, I treat the natural-disaster variable as a zip-code level observation (f_{zt}^{dis}) rather than an individual-level variable. If zip codes are much larger in area than disasters, the cost of this aggregation is in biasing my tests toward finding no effects from the disasters. The benefit from the aggregation is that measures ρ_{zt} and f_{zt}^{dis} are either estimatable (ρ_{zt}) or observable (f_{zt}^{dis}) with a little work described in later sections.

I am left with a potential estimating equation for which all data are available:

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

where $\Delta_t W_{zt} \equiv \sum_{i=1}^{n_{zt}} \Delta_t w_{izt} / n_{zt}$ and $\varepsilon_{zt} \equiv \sum_{i=1}^{n_{zt}} \varepsilon_{izt} / n_{zt}$. The fixed effect γ_z is now the mean community welfare growth absent lenders and distress. In the empirical section, I refer to $(\rho_{zt} + f_{zt}^{dis} - \rho_{zt} f_{zt}^{dis})$ as the variable *Distress_{zt}*, and thus Eq. (2) can be written:

$$\Delta_t W_{zt} = \gamma_z + \alpha_1 L_{zt} + \alpha_2 \text{Distress}_{zt} + \alpha_3 L_{zt} \text{Distress}_{zt} + \tau_t + \varepsilon_{zt}. \quad (2a)$$

2.1. Counterfactual framework

The distress decomposition and aggregation to the community level do not solve the problems of lender location endogeneity and omitted-variable bias inherent in Eq. (1). However, Eq. (2) does facilitate a counterfactual framework to solve these problems using a matching and differencing approach. A counterfactual framework, originating in the statistics and program evaluation studies of Neyman (1923) and Rubin (1974), is an experimental treatment design in which the treatment effect is assessed against an estimation of the counterfactual had the individual not been subject to the treatment. Framed this

way, the basic idea of my identification strategy is that I can use the difference in welfare growth for lender communities compared to non-lender communities in areas not hit by a disaster as a matched benchmark for what the lender-versus-non-lender welfare differential in a disaster area would have been had no disaster occurred.

To illustrate how the counterfactual setup works, I first need some labels. I denote by *treat* (for treated) the communities that have been or will be hit by a natural disaster, and by *cntrl* (for control) those not ever affected by a natural disaster. 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, with the pair matched in time and on the propensity of the residents to be in personal-emergency distress. Focusing on one particular pair of communities with no natural disaster, suppose $\rho_{Lt}^{cntrl} = \rho_{Nt}^{cntrl} \equiv \rho_t^*$. Differencing the matched pair using Eq. (2) gives a difference-in-differences (DID) estimator of the difference in welfare growth for lender versus non-lender areas for these control communities:

$$[\Delta_t W_{Lt}^{cntrl} - \Delta_t W_{Nt}^{cntrl} | \rho_t^*] = \gamma_L^{cntrl} - \gamma_N^{cntrl} + \alpha_1 + \alpha_3 \rho_t^* + \varepsilon_{Lt}^{cntrl} - \varepsilon_{Nt}^{cntrl}. \quad (3)$$

The left-hand side is the difference in welfare growth from $t-1$ to t between the community with a lender and the community with no lender, with communities matched on ρ_t^* and in time. The equation says that this difference is equal to $\gamma_L^{cntrl} - \gamma_N^{cntrl}$ (the difference in the community fixed effects) plus α_1 (the parameter measuring the welfare increase or decrease associated with being in a community with a lender) plus $\alpha_3 \rho_t^*$ (the welfare growth increase or decrease associated with the existence of a lender for those facing personal distress) plus the difference in error terms. An important feature to note in Eq. (3) is that the parameters remain associative in nature. The difference in welfare growth of communities with lenders as compared to those without could well be due to endogenous location decisions of lenders and other economic trends correlated with the existence of lenders in a community.

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

$$[\Delta_t W_{Lt}^{treat} - \Delta_t W_{Nt}^{treat} | \rho_t^*] = \gamma_L^{treat} - \gamma_N^{treat} + \alpha_1 + \alpha_3 + \varepsilon_{Lt}^{treat} - \varepsilon_{Nt}^{treat}. \quad (4)$$

As in the control case, I cannot interpret this DID estimator causally. Welfare growth could differ in locations with payday lenders compared to locations without lenders, for reasons unrelated to any financial distress caused by disasters. This does not rule out the possibility that the welfare reaction to disaster distress could be causally affected by access to a lender, but the DID estimate in Eq. (4) would capture both the endogenous and the causal effect of having a lender in the community.

The key identification insight is, however, that the control group in Eq. (3) is the counterfactual for how the lender and non-lender communities would have differed in welfare

growth had there been no natural disaster. A final differencing subtracts the DID estimate of Eq. (3) from the DID estimate of Eq. (4). After averaging $m=1, \dots, M$ matches of four communities at a point in time, the resulting triple difference of welfare growth $\Delta\Delta\Delta$ is

$$\Delta\Delta\Delta \equiv \frac{1}{M} \sum_{m=1}^M [(\Delta_t W_{mL}^{treat} - \Delta_t W_{mN}^{treat}) - (\Delta_t W_{mL}^{cntrl} - \Delta_t W_{mN}^{cntrl})] \rho_m] = \alpha_3(1-\bar{\rho}), \quad (5)$$

where $\bar{\rho} = (1/M) \sum_{m=1}^M \rho_m$ is the average propensity to be credit constrained across all sets of matches M , and $(1/M) \sum_{m=1}^M \varepsilon_m = (1/M) \sum_{m=1}^M [(\varepsilon_{mL}^{treat} - \varepsilon_{mN}^{treat}) - (\varepsilon_{mL}^{cntrl} - \varepsilon_{mN}^{cntrl})] \simeq 0$, which I defend in the next section. The community fixed effects $(\gamma_L^{treat} - \gamma_N^{treat}) - (\gamma_L^{cntrl} - \gamma_N^{cntrl})$ will cancel out if disasters hit randomly and as long as the sample is sufficiently large. Also, 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 in case there are any residual concerns about time effects.

The essence of the counterfactual framework is that although the lender locations are probably endogenous with respect to welfare growth, these endogeneities exist in the same way for matched disaster and non-disaster communities and can be differenced out during estimation. All that is left after the triple differencing is the impact of lenders mitigating or exacerbating distress following a disaster, α_3 , multiplied by the proportion of individuals not already in distress on average across the M sets of matched communities, $(1-\bar{\rho})$.

2.2. Regression framework

How is $\Delta\Delta\Delta = \alpha_3(1-\bar{\rho})$ estimated? An estimating equation similar to Eq. (2) provides the answer:

$$\Delta_t W_{zt} = \alpha_0 + \alpha_1 L_{zt} + \alpha_2 Distress_{zt} + \alpha_3 L_{zt} Distress_{zt} + \varepsilon_{zt}. \quad (6)$$

The only things new are that the community fixed effects are omitted, because I estimate a collapsed equation with only one observation of $\Delta_t W_{zt}$ per zip code in the time dimension and the data are limited to the matched sets of communities. I collapse each zip code to one observation, to handle the serial correlation discussed in [Bertrand, Duflo, and Mullainathan \(2004\)](#) by taking the average of the welfare variable in the four quarters after the event and subtracting out the average of four quarters before the event. We can now consider the properties of the OLS estimator.

The vector of right-hand side variables x_z (including the constant) for observation z is $[1, L_{zt}, Distress_{zt}, L_{zt} Distress_{zt}]$. For simplicity, consider a matched, four-observation dataset, all with the same ρ . The first two observations are from the control group and the latter two are from the treated group. Observations 2 and 4 have a lender in the community. The four observations imply a Y vector and an X matrix of

$$Y = \begin{bmatrix} \Delta_t W_{N,t}^{cntrl} \\ \Delta_t W_{L,t}^{cntrl} \\ \Delta_t W_{N,t}^{treat} \\ \Delta_t W_{L,t}^{treat} \end{bmatrix}, \quad X = \begin{bmatrix} x'_{cntrl,N,t} \\ x'_{cntrl,L,t} \\ x'_{treat,N,t} \\ x'_{treat,L,t} \end{bmatrix} = \begin{bmatrix} 1 & 0 & \rho & 0 \\ 1 & 1 & \rho & \rho \\ 1 & 0 & 1 & 0 \\ 1 & 1 & 1 & 1 \end{bmatrix}.$$

The OLS vector of estimates is $\hat{\alpha} = (X'X)^{-1}X'Y$. Solving for $\hat{\alpha}_3$ using the matrices above leads to $\hat{\alpha}_3 = (1/(1-\rho)) [(\Delta_t W_{L,t}^{treat} - \Delta_t W_{N,t}^{treat}) - (\Delta_t W_{L,t}^{cntrl} - \Delta_t W_{N,t}^{cntrl})]$. Rearranging and averaging over all M sets of four community matches brings us back to $\Delta\Delta\Delta = \hat{\alpha}_3(1-\bar{\rho})$.

This demonstrates that an OLS estimate of Eq. (6) gives the triple-differencing solution. It is now easy to show what is required for the estimate to be unbiased, i.e., for $E(\varepsilon|X) = 0$. Using the X matrix above, α_3 will be an unbiased estimator if $(1/(1-\rho))[(\varepsilon_{L,t}^{treat} - \varepsilon_{N,t}^{treat}) - (\varepsilon_{L,t}^{cntrl} - \varepsilon_{N,t}^{cntrl})] = 0$, or in terms of all possible matches

$$\frac{1}{M} \sum_{m=1}^M \frac{1}{1-\rho_m} [(\varepsilon_{L,m}^{treat} - \varepsilon_{N,m}^{treat}) - (\varepsilon_{L,m}^{cntrl} - \varepsilon_{N,m}^{cntrl})] = 0. \quad (7)$$

What this relies on is that any endogeneities between the lender location and the error term are same for disaster and non-disaster areas, or $\sum_{m=1}^M (\varepsilon_{L,m}^{treat} - \varepsilon_{N,m}^{treat}) = \sum_{m=1}^M (\varepsilon_{L,m}^{cntrl} - \varepsilon_{N,m}^{cntrl})$, which holds even if $\sum_{m=1}^M (\varepsilon_{L,m}^{cntrl} - \varepsilon_{N,m}^{cntrl}) \neq 0$ and even if $\sum_{m=1}^M (\varepsilon_{L,m}^{cntrl} - \varepsilon_{N,m}^{cntrl})$ correlates with the existence of a lender, L_z . If so, the estimates $\hat{\alpha}_2$ and $\hat{\alpha}_3$ causally measure the effect of distress on welfare and the extent to which financial distress is mitigated or exacerbated by the existence of a lender. The properties in a standard difference-in-differences setup for this to be true are now fairly innocuous: there must be a sufficiently large sample of zip codes, and natural disasters must hit randomly.

2.3. Possible disaster omitted variables and other robustness

The matched triple-difference framework leaves one possible dimension in which an omitted variable might remain. An argument might be made that reactions to a disaster differ across communities *only in the case of a natural disaster* in a way that could be correlated with the existence of a lender. For this to be a problem, it must be that this reaction is specific to disasters as opposed to personal emergencies. Three potential stories help to illustrate this argument, which I call disaster resiliency.

The first story is that lenders locate in communities with more (or less) adhesive community or family ties that provide support during disasters. This support during disasters would have to be different from the support during personal financial distresses, such as helping family or neighbors cope with health expenses or job losses. This story seems unlikely.

The other two stories emerge from the possibility that lenders locate in communities in which the commercial activity is up-and-coming rather than declining, and this characteristic of the community *only* differentiates a community during a natural disaster relative to its personal-emergency-matched community. Both stories build on the intuition that since payday lenders' largest expense is default and since default occurs more frequently in areas where people become unemployed, payday lenders prefer up-and-coming areas, all else equal.

The first of these stories concerns the effect of the disaster on property damage directly. Suppose that in up-and-coming communities, land quickly becomes valuable.

While people investing in existing properties upgrade the structures over time, this does not happen as rapidly as land price increases in desirable locations. This would imply that disasters affect the up-and-coming areas less, all else equal, because disasters presumably affect structures more than they affect land value.⁵

The other up-and-coming story concerns the type of economy that the community is. Suppose two communities have the same propensity to be financially constrained, but a lender chooses to locate in the one with a vibrant service sector rather than the one with a declining manufacturing sector. When a natural disaster strikes, the service sector might retrench quickly, whereas a manufacturing sector in decline might just face an accelerated demise.

To take the omitted-variable concern of these stories seriously, I implement two further steps in my specification. First, I construct multiple controls for the damage caused by the disaster, controlling both for the property damage of each disaster and for the damage to commercial activity in the communities. Second, I allow for a differential effect of disasters on lagged building permit values (to address story two) and the extent of the service orientation of the community (to address story three). I discuss these variables in more detail in the results section.

In addition to the concern about resiliency, the formulation requires that the predicted welfare impact from a natural disaster is higher for communities with lower initial levels of personal emergencies. To see this, imagine that I estimate a coefficient $\hat{\alpha}_2$. The predicted impact of a disaster for a non-lender community with a low propensity to be credit constrained, $\hat{\alpha}_2(1-\rho_{LOW})$, will be higher than that for one with a high propensity, $\hat{\alpha}_2(1-\rho_{HIGH})$, because more people in high personal-emergency areas were already in distress. Realistically, we would expect people in poorer areas to be more vulnerable to natural-disaster distress, not less.

However, this is not going to be much of a concern for interpreting my results since the vast majority of the variation in *Distress* comes from whether or not a disaster occurs (the standard deviation around ρ is much smaller than around f^{dis}), implying that the differential in effect between areas with high and low ρ is very small. Nevertheless, I show the robustness of my results to a standard difference-in-differences estimation around the natural disaster itself rather than around the *Distress* variable, i.e.,

$$\Delta_t W_{zt} = \gamma_z + \beta_1 L_{zt} + \beta_2 f_{zt}^{dis} + \beta_3 L_{zt} f_{zt}^{dis} + \tau_t + \varepsilon_{zt}. \quad (8)$$

3. Matching

The methodology section called for a matching of zip codes on ρ , the propensity of individuals in a community to be financially constrained. Databases such as the Survey of Consumer Finances contain a number of

measures that identify individuals who are constrained financially. However, even if geographic identifiers were available for the SCF, the observation counts are insufficient to be representative of individual communities. Thus, I estimate the relation between individuals' socioeconomic attributes and their probability of being financially constrained using the SCF and then project the relation onto the same socioeconomic information available at the zip-code level from the U.S. Census. This section describes the procedure and estimation.

I use three measures of financial constraints for the 4,300 individuals in the 1998 SCF. I use the 1998 SCF because it is the center of my 1996–2002 time period. *AtLimit* is an indicator variable equal to one if the individual's outstanding credit-card balance is within \$1,000 of the card limit, if the individual has credit-card debt. Approximately 9% of respondents are within \$1,000 of their credit limits, with a standard deviation of 0.287. *HiDebt* is equal to one if the individual's credit-card debt is equal to more than 10% of yearly income. Twenty-eight percent of the sample have high debt, with a standard deviation of 0.451. The final measure, *BehindPayments*, is equal to one if the individual responds affirmatively to a question about being behind on any payments. Twelve percent of individuals are behind, with a standard deviation of 0.334.

To project the relation between these measures and individual socioeconomic characteristics onto zip codes, I follow Jappelli (1990) and Calem and Mester (1995), employing the set of their explanatory variables also available in the U.S. Census files—wealth, income, age, education, marital status, race, sex, family size, home and car ownership, and shelter costs. Table 1 presents the logistic estimation of the probability of being financially constrained on these socioeconomic variables. The logistic estimates predict correctly whether an individual is financially constrained 89% of the time. I only briefly highlight some of the coefficients and refer interested readers to Jappelli (1990) and Calem and Mester (1995).

The coefficients in Table 1 should be interpreted as “compared to a wealthy, well 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. Non-white persons and those with vehicles face more constraints. The other results vary by which dependent-variable measure of financial constraints is used. Of these, 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 most constrained.

I take the coefficients and project the linear relation onto U.S. Census data for 1,762 California zip codes by multiplying each coefficient by the percentage of residents having that characteristic in a zip code and

⁵ I thank an anonymous referee for this suggestion.

Table 1

Survey of consumer finance estimations of financial constraints.

The first column presents the average across zip codes of the proportion of population (or households) in each category. For example, the first line shows that 21.5% percent of residents have an income of less than \$15,000. The last three columns present the logistic estimation results for the dependent variables *at credit card limit*, *high debt/income*, and *behind on payments*. Standard errors are not presented in the interest of space. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

	Census: Proportion in Zip Code	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		4,305	4,305	4,305
Pseudo R-Square		0.104	0.150	0.096

summing. I do this for each of the three measures of financial constraint and for each of the U.S. Census data years 1990, 1997 (an update containing most socioeconomic variables), and 2000. I interpolate the in-between years to avoid jumps in my projections over time.

Each of the three measures might capture an important part of being constrained. In the end, I would like a single measure that captures features of each. For simplicity and because I do not want to impose subjective assumptions, I rescale the predicted variables to have equal means, which I fix to be equal to 0.10 for ease of exposition. I then 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 examine the principal components of the three variables. The first principal component captures 80% of the variability 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 am ready to take the nearest neighbor match. Because my foreclosure and crime data are not equivalent in the coverage of zip codes and years (only a subset of counties report for each), I do a different matching for crime and foreclosures. Although my methodology section suggests that I should do a four-way match (disaster/not and lender/not) all at once, I have to deal with the fact that my pool of disasters is small relative to the pool of non-disaster communities. Thus, for communities that are hit by disasters I choose a matched-propensity community from the pool of non-disaster communities, on the basis of access to a lender or not within a common support. When a

control-group observation is chosen multiple times, I weight the observation accordingly. I run a chi-square test to see if 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 cannot 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 panel micro-data available for payday lenders 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 areas where my crime data are more precise (as described below). In particular, I throw out 11 large-city counties (out of a total of 58) with a population of over 800,000 people. I choose this threshold to drop all counties with populations equal to or greater than that of San Francisco County.⁶ The time period of the analysis is 1996–2002.

4.1. Natural-disaster data

Natural-disaster data come from the University of South Carolina's SHELUDS (Spatial Hazard and Loss Database for the United States), 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 SHELUDS contains more detailed location information, most often in the form of city names or NOAA (National Oceanic and Atmospheric Administration) codes that identify the specific local area hit by the disaster. For each line item, I manually attribute the disaster to the smallest area provided and then use a GIS (geographic information system) program to overlay the disasters to zip-code affiliations.

The Hazard database contains all natural disasters that cause more than \$50,000 in property damage. Table 2, Panel A, shows statistics for the 1,568 disasters in the sample period 1997–2002 for which I have all data and am able to successfully match. The average zip code incurs \$12.6 million in property damage, with the median being only \$391,000. (Note that if a disaster affects more than one zip code, the property damage is divided according to population.)

I include a breakdown of the disaster statistics according to the insurance coverage available and utilized in California.⁷ The category of earthquakes, floods, and landslides represents the largest number of communities affected (787) and the most damage (\$24 million per disaster on average). These types of disasters are almost never covered by insurance, especially for the profile of

individuals borrowing from payday lenders. Storms, wildfires, and coastal damage are often included in homeowner insurance, but the coverage is usually insufficient. Finally, my sample includes 350 communities hit by hail, lightning, tornadoes, or wind; these are smaller-impact natural-disaster categories that are covered by standard homeowner insurance. I expect that any disaster effects should be lower for this final category.

4.2. Payday lender 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 information for each payday store, with an original license date and date of suspension, if applicable, for each active and non-active lender. The categories containing the payday licenses during this time (California Finance Lenders and Consumer Finance Lenders) also contain other types of lenders, such as insurance companies, auto loan companies, and realty lenders, which I am able to filter out. What I am unable to fully remove are check cashiers with a license to lend who make only title loans or non-payday small consumer loans. However, my data tabulate to 2,160 payday stores in 2002, representing one lender for every 16,000 people in the state. This figure is almost exactly in line with the California figure cited in Stegman and Faris (2003) and the data obtained from the attorney general by Graves and Peterson (2005).

Table 2 presents the community-level summary statistics for payday lenders. The first row in Panel B shows that mean (median) number of payday lenders per zip code is 2.00 (1.00). The empirical design is based on the yes/no question of whether payday lenders exist in the zip code, which is equivalent to being above or below median. Fig. 1 depicts the mapping of payday locations to the zip codes for 2002, together with the propensities of communities to be credit constrained. The larger the dots on the zip code, the greater is the density of lenders. The minimum-size dot indicates that no lenders are in the zip code. The zip-code shadings reflect the credit-constrained propensities; the higher the propensity to be credit constrained, the darker is the color.

Because zip codes have varying sizes and densities of commercial activity, I use a second set of measures of payday loans for robustness. I construct the number of lenders within a radius of 10 or 20 miles from the center of the zip code using the GIS plotting of payday stores. As Table 2 presents, these are much larger numbers, with a mean (median) number of lenders of 72.6 (37.5) within 10 miles and 215.9 (101.5) within 20 miles. In the estimation, I use the log of these variables to offer a (non-skewed) continuous measure of payday density.

4.3. Welfare data

For foreclosures to be a measure of welfare, individuals' utilities must decline when their homes are foreclosed. Admittedly, having one's house foreclosed can be efficient in some circumstances, even taking into account the large transaction costs involved. A general rule is that a foreclosure

⁶ The dropped counties are Los Angeles, San Diego, Orange, Riverside, San Bernardino, Santa Clara, Alameda, Sacramento, Contra Costa, Fresno, Ventura, and San Francisco.

⁷ The breakdown of disasters that are effectively covered in California comes from a series of interviews with insurers from the Insurance Information Network of California, Milliman Agency, and walletpop.com/blog/category/insurance-home, all referenced as of 2/2010.

Table 2

Disaster, payday and welfare summary statistics.

<i>Panel A: Natural Disaster Property Damage Statistics (in \$1,000s)</i>						
	Mean	St. Dev.	Minimum	Median	Maximum	Count
Earthquakes, Floods, Landslides (No Insurance Usually)	24,227	86,721	50	1,600	719,300	787
Storms, Wildfires, Coastal Damage (Insufficient Insurance on Average)	1,303	3,863	50	215	66,900	431
Hail, Lightning, Tornados, Wind (Insured Usually in Homeowners' Policy)	169	138	50	164	550	350
All Disasters in Sample	12,556	62,600	50	391	719,300	1,568
<i>Panel B: Lender and Welfare Variable Statistics</i>						
	Mean	St. Dev.	Minimum	Median	Maximum	Count
Payday Lenders in Zip Code	2.00	3.92	0	1.00	44	2,306
Payday Lenders within 10 Miles	72.60	87.22	0	37.5	364	2,278
Payday Lenders within 20 Miles	215.9	251.8	0	101.5	914	2,278
(Log) Lender 10 Miles	3.10	1.93	0	3.65	5.90	2,278
(Log) Lender 20 Miles	4.30	1.86	0	4.63	6.82	2,278
Foreclosures (Quarterly)	10.52	15.52	0	5.00	243	2,306
Foreclosure Rate (/1,000 Owned Homes)	2.43	5.59	0	1.38	171.6	2,306
Change in Foreclosure Rate (Pre-to-Post)	−0.65	1.71	−5.72	−0.49	3.71	2,306
Larcenies per Household (Yearly)	60.8	172.9	0	16.6	1857	767
Change in Larcenies per Household	−1.93	44.7	−340.2	−0.01	312.7	767
Vehicle Thefts per Household (Yearly)	11.14	31.7	0	2.42	360.8	767
Change in Vehicle Thefts per Household	−0.75	9.18	−63.4	0	61.9	767
Burglaries per Household (Yearly)	20.4	50.9	0	6.90	489.3	767
Change in Burglaries per Household	−0.77	17.8	−125.3	−0.05	128.2	767

Notes:

1. All variables are at the zip code level for 1996–2002.
2. Natural disasters data are from the University of South Carolina's SHELDUS Hazard database, which identifies the location, type, and magnitude of natural disasters.
3. Yearly data on payday lending are from the State of California Department of Corporations. Payday Lenders is an indicator variable. Payday Lenders within 10 (20) Miles is the count of lenders within this distance from the center of the zip code. Log Lender 10 (20) miles is the log of this count + 1.
4. Foreclosure counts are from the California Association of Realtors.
5. Yearly crime data are from the State of California Criminal Justice Statistics Center.
6. Population and number of owned housing units (to normalize crime and foreclosures, respectively) 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.

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 subtract out the general pattern of foreclosures for similar communities (with the non-disaster areas) and the effect of disasters on foreclosures (with the disaster, non-lender communities), thus isolating only financial-distress-forcing foreclosures.

I use quarterly residential foreclosures in a zip code recorded by the California Association of Realtors, available at RAND Statistics, for each quarter over the period 1996–2002. Per my methodology, the dependent variable is changes in foreclosure rates. To get to rates, I divide foreclosures by the total number of owner-occupied dwellings in a zip-code community available from the U.S. Census. Table 2, panel B, reports that, in the matched sample used in the estimations, foreclosures range from zero to 243 per quarter per zip code, with a mean (median) of 10.5 (5). This translates to a mean rate of

2.43 foreclosures per thousand owner-occupied housing units. To get changes in these rates, I subtract the average of the four quarters prior to the natural disaster from the average of quarters 4 to 7 after the disaster. I leave three quarters to allow for the average processing time of a foreclosure in the State of California. I winsorize the changes in foreclosure rates to the middle 95 percentile density. I do this because the estimated coefficients are anticonservative with respect to the outliers. All significance and signs remain the same. The mean changes in foreclosures is negative, suggesting that foreclosures were declining in California at a rate of half a foreclosure for every 1,000 homes over a two-year period.

The second measure of welfare is small property crimes, following Garmaise and Moskowitz (2006). California crime data are from the State of California Criminal Justice Statistics Center made available through RAND Statistics for the period 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 to zip codes in a



Fig. 1. California payday lending locations and propensities to be credit constrained by zip code.

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 that 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 is the propensity to be credit constrained according to the matching methodology projections. The few zip codes with entirely white shading are those altered by the post office during the sample or those of natural parks. These are not included in the analysis.

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 admittedly 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 Los Angeles based on population. I 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 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 the degree to which individuals use crime to relieve financial distress.

Table 2 reports that the mean larcenies, vehicle thefts, and burglaries per household are 60.8, 11.14 and 20.4, respectively, in the matched sample. The summary statistics are winsorized, removing only 0.5% of the sample on each end, outliers which act anticonservatively in estimation.

5. Results

5.1. Baseline foreclosure results

Table 3 reports the baseline foreclosure results. The dependent variable is the change in the quarterly rate of foreclosures by zip code, where change is defined to be the average foreclosure rate in quarters 4–7 after the disaster (aligned for the matched group) minus the average foreclosure rate in the 4 months prior to the disaster. I use a single collapsed observation for a zip code to eliminate the serial correlation concerns in differencing specifications highlighted by Bertrand, Duflo, and Mullainathan (2004).

In the first column of Table 3, *Distress*, defined as $(\rho_{zt} + f_{zt}^{dis} - \rho_{zt}^{dis})$, is positive and significant; distress causes 1.228 more foreclosures per 1,000 homes. *Distress*Lender* is strongly significant with a coefficient of -0.503 . Column 2 shows that the coefficients on *Disaster* and *Disaster*Lender* are equivalent (up to the transformation) to those on *Distress*; I present the rest of the results using the *Disaster* variable because of the greater ease in interpreting the natural-disaster effect, and because using *Disaster* rather than *Distress* does not have the odd implication that disaster increases distress more in wealthy communities than in poor ones. In

Table 3
Baseline lender effect on foreclosures after disasters.

	1	2	3	4
Lender	0.164 [0.156]	0.110 [0.139]		
Distress	1.228*** [0.155]			
Lender*Distress	–0.503*** [0.186]			
Disaster		1.104*** [0.138]	1.315*** [0.239]	1.302*** [0.324]
Lender*Disaster		–0.450*** [0.166]		
Lenders 10 Miles			0.067 [0.049]	
Lenders 10 Miles*Disaster			–0.135** [0.056]	
Lenders 20 Miles				0.041 [0.052]
Lenders 20 Miles*Disaster				–0.099 [0.061]
Constant	–1.612*** [0.152]	–1.481*** [0.143]	–1.691*** [0.239]	–1.631*** [0.303]
Observations	2,306	2,306	2,278	2,278
R-squared	0.097	0.098	0.099	0.097

Notes:

1. The dependent variable is the change in quarterly foreclosures per owner-occupied home around the natural disaster or its match in time, where the pre-period is the four quarters before the event and the post period is quarters 4–7 after the disaster.

2. The analysis is quarterly at the zip-code level, with only one observation per disaster zip code (and its match).

3. *Distress* is equal to $(\rho + f^{dis} - \rho f^{dis})$, where ρ is the propensity of the community to be financially constrained and f^{dis} indicates a natural disaster. $Disaster = f^{dis}$.

4. The independent variable *Lender* is an indicator for a lender in the community. The dependent variables *Lender10 Miles* and *Lender20 Miles* are the log of the count of payday lenders within 10 or 20 miles from the center of the zip code.

5. Year dummy variables are included but not shown. ***, **, and * denote significance at the 1%, 5%, and 10% levels. Robust standard errors are reported in brackets.

Column 2, a natural disaster causes 1.1 more foreclosures per quarter (up from a baseline of 2.43) or 4.4 more foreclosures per year. How does this disaster effect compare to the literature on disasters? Murdoch, Singh, and Thayer (1993) and Bin and Polasky (2004) study housing values following the Loma Prieta earthquake and Hurricane Floyd, finding that the natural disaster reduced home values by 10% and 5.8%, respectively. More directly, Anderson and Weinrobe (1986) find that the 1971 San Fernando earthquake caused 31 more defaults from 372 afflicted houses than would have been predicted without an earthquake. This is a huge effect; 9% of afflicted homes defaulted. In zip-code terms, if there were an average of 4,328 owner-occupied homes per zip code; Anderson and Weinrobe's coefficient would be 7.16 more foreclosures per 1,000 homes. My natural-disaster effect looks somewhat small (4.4 versus 7.16) in comparison, but my natural disasters are much smaller on average than the San Fernando earthquake, which inflicted \$553 million in property damage according to the authors.

Returning to the question of disaster mitigation, Column 2 suggests that communities with lenders experience an overall increase in foreclosures after disasters but much less so (only $1.1 - 0.45 = 0.65$ more foreclosures) compared to matched communities experiencing disasters without access to a lender. In Column 3, the continuous variable (log of) *Lenders 10 Miles* also has

explanatory power, mitigating the effect of disasters. The mitigation effect is equally strong economically: -0.131 times the *Lenders 10 Mile* standard deviation of 1.93 is similar to -0.450 times the standard deviation of *Lenders* of 0.50. However, as one might expect, the results erode at the 20-mile radius in Column 4. Before jumping to interpretation of these effects, I first need to address resiliency.

5.2. Resiliency variables

One could make an argument that disaster resiliency is driving the disaster mitigation effect of payday lenders found in Table 3. Both of the up-and-coming stories presented earlier have a hypothesis consistent with the results. To address this concern, I first control for the disaster economic effect directly using the property damage caused by the storm (from SHELDDUS), the change in quarterly housing prices (from the California Association of Realtors), the change in yearly number of establishments per population (from the Bureau of Labor Statistics (BLS)), and the change in yearly payroll per population (from BLS). I include these variables alone and interacted with *Disaster* to remove the level effect and capture the economic damage of the disaster. The summary statistics and correlations of these variables are included in Table 4.

Table 4

Disaster resiliency variables summary statistics.

Panel A: Summary Statistics							
		Mean	St. Dev.	Minimum	Median	Maximum	Count
House Price	Quarterly \$mill.	0.172	0.166	−2.25	0.144	3.54	2292
Δ House Price		0.028	0.113	−1.90	0.013	3.39	2292
Payroll Paid per Population	Yearly \$1,000s	15.65	85.03	0.00	5.38	1883.3	2279
Δ Payroll Paid		0.002	0.014	−0.044	0.001	0.462	2292
Establishments per Population	Yearly 1,000s	2.38	2.03	0.011	1.87	12.91	2279
Δ Establishments		0.05	0.14	−1.70	0.02	0.93	2292
Residential Building Permit Value (lagged)	Yearly \$mill.	0.202	0.242	0.0	0.142	2.20	779
McDonald's	2007	0.98	1.17	0.0	1.00	6.00	2292

Panel B: Correlations							
	House Price	Change in House Price	Payroll Paid	Change in Payroll Paid	Establishments	Change in Establishments	Building Permit Value
House Price	1						
Δ House Price	0.412*	1					
Payroll Paid	0.028	0.009	1				
Δ Payroll	0.024	0.008	0.838*	1			
Establishments	0.314*	0.112*	0.180*	0.145*	1		
Δ Establishments	0.016	−0.115*	0.021	0.038	0.302*	1	
Building Permit Value	0.096*	0.015	0.548*	0.147*	0.227*	0.036	1
McDonald's	0.091*	0.020	0.033	0.024	0.495*	0.281*	0.052

Notes:

1. Variables are all defined in the data section of the paper.
2. All variable summaries represent statistics from the sample used, not the population in California.
3. In Panel B, * indicates that the correlation is significant at the 5% level.

Second, to directly address the structure-to-land value story (that disasters affect structures more and up-and-coming communities have relatively more land value), I collect the time series of zip-code level residential building value per permit issued from RAND. I use the lag of the average number of residential building permits by zip code in the matched sample to capture, all else equal, how much construction cost goes into a community. Presumably, up-and-coming areas have more money being invested in the community. (Because the sample is a matched one, I can include the level of house price in this estimation and the results remain the same.) Panel B of Table 4 shows that building permits are capturing something more than just house values. The correlation of building permit values with house prices is significant but small (0.096). Building permits is instead more positively correlated with payroll (0.548), changes in payroll (0.147) and the number of establishments (0.227). These correlations are at least consistent with building permits capturing some degrees of the up-and-coming nature of communities.

To address the service economy orientation of the zip code (that service communities retrench quickly), I proxy service orientation with McDonald's locations from the GIS website (<http://www.poi-factory.com>).⁸ The

justification for using McDonald's stores as a proxy comes from the fact that McDonald's targets locations in high-traffic corridors (stated in their policy) combined with the intuition that all else equal, high traffic corridors are likely to contain services, especially given that I have already controlled for other measures of commercial activity.⁹ Table 4, Panel B, shows that McDonald's locations are very, but not completely, correlated with number of establishments (0.495) and the growth in the number of establishments (0.281). The number of McDonald's locations is somewhat correlated with house prices but is not related to residential construction, probably because the commercial and residential aspects of a community are related in conflicting ways. The number of McDonald's stores is not related to payroll in either direction, which suggests that it is not capturing an income dimension.

5.3. Main results controlling for resiliency

Turning to the estimations, Column 1 of Table 5 examines the relation between foreclosures and the resiliency controls by themselves. As expected, foreclosures increase with the extent of property damage caused by the disaster. The property damage variable is in hundreds of millions; thus, to induce one more foreclosure per zip code per year (the mean number foreclosures per zip code is 10.52 quarterly or 42 yearly), the damage

⁸ The data are static as of 2007, but any lookback growth effect should be small in that most of McDonald's domestic growth pre-dates my sample.

⁹ I thank an anonymous referee for this suggestion.

Table 5

Main foreclosure results, with resiliency controls and tests.

	1	2	3	4	5	6	7	8	9
Disaster	0.873*** [0.097]	1.088*** [0.143]	1.383*** [0.250]	0.771*** [0.160]	1.108*** [0.234]	1.117*** [0.337]	1.087*** [0.112]	1.134*** [0.145]	1.456*** [0.255]
Lender		0.136 [0.139]			−0.006 [0.170]			0.154 [0.146]	
Lender*Disaster		−0.458*** [0.166]			−0.677*** [0.223]			−0.320* [0.174]	
Lenders 10 Miles			0.075 [0.049]			0.063 [0.075]			0.079 [0.048]
Lenders 10 Miles*Disaster			−0.143** [0.056]			−0.151* [0.089]			−0.125** [0.055]
Building Permit Value				0.455** [0.192]	0.360** [0.183]	0.208 [0.220]			
Building Permit*Disaster				−0.901*** [0.345]	−0.384 [0.315]	−0.268 [0.367]			
McDonald's							0.012 [0.063]	−0.029 [0.072]	−0.037 [0.066]
McDonald's*Disaster							−0.248*** [0.072]	−0.194** [0.082]	−0.196*** [0.076]
Property Damage	0.184*** [0.065]	0.179*** [0.061]	0.175*** [0.063]		0.128** [0.050]	0.123** [0.053]		0.189*** [0.049]	0.186*** [0.049]
Δ House Price	0.406 [0.546]	0.388 [0.545]	1.223 [1.352]		−0.579 [0.570]	−0.668 [0.579]		0.389 [0.543]	1.216 [1.346]
Δ House Price*Disaster	−0.321 [0.816]	−0.283 [0.802]	−1.011 [1.477]		0.476 [0.738]	0.773 [0.753]		−0.130 [0.807]	−0.871 [1.473]
Δ Payroll Paid	−10.28*** [2.372]	−10.58*** [2.404]	−10.39*** [2.372]		38.84*** [13.39]	31.81** [13.87]		−10.65*** [2.433]	−10.44*** [2.398]
Δ Payroll Paid*Disaster	16.07** [6.689]	17.81** [7.048]	19.06*** [6.834]		−26.80* [16.19]	−20.75 [16.32]		17.64** [7.766]	18.68** [7.708]
Δ Establishments	0.373 [0.442]	0.313 [0.441]	0.606 [0.503]		0.834* [0.502]	0.877 [0.557]		0.360 [0.466]	0.675 [0.531]
Δ Establishments*Disaster	−1.143** [0.512]	−0.879* [0.508]	−1.348** [0.564]		−0.610 [0.587]	−1.068* [0.611]		−0.242 [0.542]	−0.609 [0.599]
Observations	2292	2292	2278	773	773	773	2306	2292	2278
R-squared	0.105	0.110	0.113	0.067	0.110	0.090	0.106	0.120	0.124

Notes:

1. The dependent variable is the change in quarterly foreclosures per owner-occupied home around the natural disaster or its match in time, where the pre-period is the four quarters before the event and the post period is quarters 4–7 after the disaster. The analysis is quarterly at the zip-code level, with only one observation per disaster zip code (and its match).
2. The matching is redone in Columns 4–6 due to a smaller sample of zip codes covered in the building permit dataset.
3. The independent variable *Lender* is an indicator for a lender; *Lender10 Miles* is the log of the number of payday lenders within 10 miles of the center of the zip code area.
4. Year dummy variables are included but not shown. The other independent variables are defined in the robust variables section.
5. ***, **, and * denote significance at the 1%, 5%, and 10% levels. Robust standard errors are reported in brackets.

must be \$31 million more than the mean, or half a standard deviation from Table 2. In the presence of the property damage variable, the average house price in a zip code has no explanatory power. The more payroll that is paid in a zip code implies fewer foreclosures, but a disaster negates this negative relation. Finally, a disaster induces fewer foreclosures if there is concurrent growth in the number of establishments (or fewer establishment closings), but this effect is small in magnitude.

Columns 2 and 3 of Table 5 show that the inclusion of disaster resiliency controls does not reduce the magnitude or economic significance of payday lenders mitigating the disaster effect on foreclosures. The remaining columns explore whether the mitigation effect of payday lending can hold up when allowing the up-and-coming effect of communities to load on building permit values or McDonald's locations. Columns 4 and 7 show baseline disaster results with building permit values (Column 4) or McDonald's locations (Column 7) interacted with disaster.

I find strong support for both stories. All else equal, matched communities with higher building-permit values or more McDonald's stores experience lower growth in foreclosures after a disaster. The up-and-coming nature of a community seems to matter. Columns 5–6 and 8–9 show that the mitigating effect of payday lenders remains, albeit diminished in both significance and magnitude in one specification. Because Columns 8 and 9 are the most conservative columns, I use them to interpret the economic magnitudes of the payday lender effect.

Column 8 of Table 5 shows that disasters induce 4.5 (= 1.134 per quarter × 4 quarters) more foreclosures per 1,000 owner-occupied homes in a zip code in the year following the disaster. The existence of access to credit via a payday lender mitigates 1.3 (= −0.32 × 4) of them. Column 9's economic significance addresses the density of lenders, rather than the existence of a lender. A 20% higher number of payday lenders in a ten-mile radius of the zip code (about one standard deviation) mitigates one

foreclosure per 1,000 homes ($=0.125 \times 2 \times 4$ quarters) in the year following the disaster.

We can easily translate these to rates for comparison to current events. The sample mean annual foreclosure rate is 0.972% ($=0.243\% \times 4$ quarters) of homes. After a disaster, Column 9 implies that the rate increases to 1.55% of homes, and Column 8 implies that the rate increase to 1.43%. As a comparison, the 2009 foreclosure rate in California (from RealtyTrac) was 1.88%, larger than but quite comparable with a disaster effect. In my most conservative estimations, payday lenders mitigate from 0.10 percentage points to 0.13 percentage points of the foreclosure rate (1.0–1.3 foreclosures per 1,000 people) following a disaster.

At the risk of overqualifying, I want to emphasize how these numbers apply and do not apply to non-natural-disaster situations. My design uses disasters as a natural experiment for financial shocks inducing distress. I do not claim that payday lenders lower the foreclosure rate in general. Rather, among those individuals going to payday lenders following a financial shock (a personal emergency or natural disaster), lenders have a large mitigating effect in helping these individuals catch up with their obligations before facing foreclosure. Some individuals use payday lending as a very expensive form of ordinary medium-term finance for non-distress situations. My results do not apply to them.

5.4. Placebo test and instrumental variables robustness

As a robustness check, I first use the fact that I can categorize disasters into those that insurance is unlikely to cover (earthquakes, floods, landslides, storms, wildfires and coastal damage) and those that are often included in homeowners policies (hail, lightning, tornadoes and wind). I can use the insured disasters as a placebo; I should not find an effect of lenders. Table 6, Columns 2 and 3, shows exactly this: the payday lending variables interacted with *Disaster* are insignificant. Columns 3 and 4 are the subsample of insufficiently insured disasters for which the payday mitigation effect is apparent.

As a final test of robustness using the foreclosure dependent variable, I find an instrument for *Lenders 10 Miles*. I relegate this instrumental variables approach to robustness for two reasons. First, following the methodology section, I believe that my matched triple differencing approach with resiliency controls removes concerns about my results being driven by endogeneities. Second, it is hard to prove the validity of any instrument.

My instrument is the count of intersections per area of surface (non-residential) roads in a zip code in the year 2006, in quadratic form. 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). This 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 changes in foreclosures. Working in a matched set of

communities with a time first-differenced dependent variable alleviates many concerns about violations of the exclusion restriction. For a violation to occur, a static measure of intersections in levels must predict residual changes to foreclosures. Nevertheless, one might worry about the relation 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.

A second argument questioning the exclusion restriction is that the existence of more intersections relates to growth in commercial activity. Because my measure of intersections follows (in 2006) the analysis period, more intersections could have resulted from commercial growth in the zip code during the sample period. This is unlikely, but not impossible. 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.

Following Wooldridge (2001), 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 the way the instrument enters the second stage. I correct the second-stage residuals for the generated regressor by bootstrapping the first stage following Petrin and Train (2002). In the first-stage regression, the number of intersections is significant at the 1% level in predicting whether a payday lender exists in a location. The first-stage *F*-statistic of 19.8 passes the threshold for instrument relevance.

I take the predicted probability from this regression as the instrument for *Lender10 Miles*. Column 5 reports the instrumental variables results. The key result is that the coefficient on the instrument interacted with *Disaster* is negative and significant, consistent with the prior results.

As a final test (not shown), I consider the popular view that payday lenders target military bases (see, e.g., Carrell and Zinman, 2008). (The federal government made lending to military personnel illegal in 2006.) Because there are many military bases in California and because military personnel might 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 the case 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.

Table 6

Robustness of payday lender effect: placebo test of insurance coverage.

	Usually Insured 1	Usually Insured 2	Insufficient Insurance 3	Insufficient Insurance 4	IV-Insufficient Insurance 5
Lender	0.079 [0.220]		0.047 [0.169]		
Disaster	0.901*** [0.207]	1.014*** [0.377]	1.101*** [0.167]	1.417*** [0.288]	1.351*** [0.292]
Lender*Disaster	−0.195 [0.276]		−0.463** [0.197]		
Lenders 10 Miles		0.076 [0.071]		0.065 [0.061]	0.088 [0.068]
Lenders 10 Miles*Disaster		−0.037 [0.088]		−0.181*** [0.068]	−0.163** [0.076]
Constant	−1.502*** [0.199]	−1.773*** [0.334]	−1.446*** [0.233]	−1.666*** [0.292]	−1.689*** [0.316]
Observations	743	741	1843	1829	1829
R-squared	0.075	0.078	0.099	0.102	0.120

Notes:

1. The dependent variable is the change in quarterly foreclosures per owner-occupied home around the natural disaster or its match in time, where the pre-period is the four quarters before the event and the post period is quarters 4–7 after the disaster.
2. The analysis is quarterly at the zip-code level, with only one observation per disaster zip code (and its match).
3. The independent variable *Lender* is an indicator for a lender in the community; *Lender 10 Miles* is the log of the number of payday lenders within 10 miles of the center of the zip code area.
4. Disasters included in “Usually Insured” are earthquakes, floods, landslides, storms, wildfires, and coastal damage. Disasters in “Insufficient Insurance” are hail, lightning, tornadoes, and wind.
5. The intersections per area and its square are the instrument for the log of payday lenders within 10 miles in Column 5. The F-statistic in the first stage is 19.8.
6. Year dummy variables are included but not shown. ***, **, and * denote significance at the 1%, 5%, and 10% levels. Robust standard errors are reported in brackets.

5.5. Small property crime results

As an additional test, I turn now to the small property crime dependent variables, following [Garmaise and Moskowitz \(2006\)](#). Table 7 reports the 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 as a natural covariate.

Columns 1 and 2 in Table 7 show that natural disasters increase larcenies by about 12 crimes per 1,000 households per year, compared to a mean of 60 crimes per 1,000 households per year. The negative significant coefficient on *Lender*Disaster* implies that the disaster-driven increase in larcenies is mitigated when a lender is accessible. I qualify this result that the significance is weaker, but nevertheless, the larceny result supports the main foreclosure results. Payday lending seems to offer those in distress an option to weather financial distress.

Turning to the other small property crimes, the remaining columns report that payday lenders play no role in people's decisions to engage in vehicle thefts or burglaries. The only independent variables that explain any of the variation in the changes in these crimes are the change in violent crimes (very strongly) and payroll (weakly). Perhaps it is intuitive that in times of financial

distress, the benefit from access to credit matters only for the smallest of the small property crimes, e.g., shoplifting, where the connection between the need for cash and criminal action is arguably the most direct.

6. Conclusions

Taking advantage of the exogenous shock of natural disasters in a matched-triple-difference framework, I find that the existence of payday lending increases welfare for households that might face foreclosures or be driven into small property crime in times of financial distress. Specifically, the main result is that foreclosures increase dramatically (4.5 more foreclosures per 1,000 homes) in the year following a natural disaster; however, 1.0–1.3 of the 4.5 increase is mitigated by access to a lender. The implication is that access to finance can be welfare improving, even at a 400% APR. Payday lending also discourages shoplifting but does not factor into decisions of more serious crimes such as vehicle thefts and burglaries.

A qualification is that welfare improvement comes from the mitigating role of payday lenders following shock-driven distress. I do not capture the welfare impact of payday lenders on those borrowing as a regular process of balancing their budgets or as a means to fund temptation consumption. For this subset of the population, there could likely be negative implications to spending facilitated by payday loans ([Skiba and Tobacman, 2005, 2007](#); [Melzer, forthcoming](#)). However, my results should apply to the common occurrence of people facing personal

Table 7
Effect of payday lending on crime after a disaster.

	Larceny 1	Larceny 2	Vehicle Thefts 3	Vehicle Thefts 4	Burglaries 5	Burglaries 6
Lender	8.792 [5.431]	9.662* [5.151]	1.434* [0.857]	1.400* [0.832]	0.554 [1.398]	0.849 [1.264]
Disaster	11.64* [5.962]	12.28* [6.335]	0.545 [0.940]	0.733 [0.988]	–0.009 [1.624]	0.111 [1.678]
Lender*Disaster	–11.69* [6.473]	–12.35** [6.239]	–0.698 [1.039]	–0.595 [1.025]	–0.191 [1.840]	–0.307 [1.792]
Δ Violent Crime	0.127*** [0.023]	0.126*** [0.023]	0.0214*** [0.003]	0.021*** [0.003]	0.046*** [0.007]	0.045*** [0.007]
Δ Violent Crime*Disaster	1.678*** [0.612]	1.686*** [0.624]	0.502*** [0.082]	0.505*** [0.081]	0.999*** [0.139]	1.004*** [0.140]
Δ Payroll per Population		2.303* [1.360]		0.397* [0.215]		0.097 [0.335]
Δ Payroll*Disaster		–2.963 [2.452]		–0.760* [0.399]		–0.622 [0.753]
Δ Establishments		–11.56 [14.14]		0.272 [2.437]		–3.573 [4.540]
Δ Establishments*Disaster		10.84 [19.87]		0.310 [3.730]		2.637 [6.133]
Constant	–11.18 [8.075]	–11.86 [8.393]	–2.752** [1.335]	–2.895** [1.369]	–0.759 [1.404]	–0.846 [1.426]
Observations	767	767	767	767	767	767
R-squared	0.347	0.349	0.492	0.495	0.537	0.538

Notes:

1. The analysis is yearly at the zip-code level, with only one observation per disaster zip code (and its match).
2. The dependent variables are the change in annual crimes per zip code household. Change is calculated as the crimes in the year of the disaster (aligned for the match group) minus crime in the year prior to the disaster.
3. The resiliency covariates – establishments and community payroll per capita – are as in Table 5. Instead of house prices, I include violent crimes.
4. The independent variable Lender is an indicator for a lender.
5. Year dummy variables are included but not shown. ***, **, and * denote significance at the 1%, 5%, and 10% levels. Robust standard errors are reported in brackets.

emergencies, which, [Elliehausen and Lawrence \(2001\)](#) accounts for two-thirds of the self-reported reason for payday borrowing.

My results speak more generally 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](#); [Levine and Demircuc-Kunt, 2001](#)), firm entrant growth ([Guiso, Sapienza, and Zingales, 2004](#); [Cetorelli and Strahan, 2006](#); [Paravisini, 2008](#)) 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. (An exception similar in spirit to this work is [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 also for planning for economic downturns and structural job shifts.

The results have important policy implications for payday lending. Fifteen states have recently banned payday lending, and legislation is pending in 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 for those facing distress. This does not mean that payday lending is the best product conceivable, but it

does suggest that efforts should be focused on opening up the market for product innovation in (cheaper) high-risk and short-term personal finance to help those in need.

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