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PAYDAY LENDERS: HEROES OR VILLAINS?

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Abstract

I study the effect that the availability of exceptionally high-interest consumer loans (payday loans) has on people's welfare by using natural disasters as an exogenous shock to communities' financial condition. Utilizing a propensity score matched, triple difference approach, I find that communities with payday lenders show greater resiliency to natural disasters. For each of the welfare measures considered – foreclosures, births, deaths, and alcohol and drug treatment, – the estimates suggest that payday lending enhances the welfare of communities. I discuss whether this effect is limited to individuals facing personal disasters or applies in general.

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The 1906 San Francisco earthquake sparked hundreds of fires, leaving nearly 300,000 of the city's 410,000 residents homeless. Leading the recovery was A. P. Giannini, a smalltime banker who profited by providing distress finance (sitting on a wharf with a bag of gold) to the citizens of San Francisco. He emerged as the heroic founder of the Bank of America. If Giannini could be considered a hero for offering distress finance, why are distress lenders today considered villains?

There is little debate that access to finance enhances value for firms. Financial institutions of different shapes and sizes have always played an integral role in corporate finance by affording such access. There is not, however, a similar consensus whether all types of financial institutions, such as high-interest rate lenders, provide a benefit to households. If individuals suffer from time-inconsistent preferences as in Laibson (1997), financial institutions will cater to this bias (Campbell, 2006), and access to finance can make them worse off.

In this paper, I study the welfare effects of access to finance offered by a particular financial institution, payday lending. Payday loans are short-term, small dollar advances that sustain individuals to the next payday. The fees charged in payday lending annualize to an implied rate of 400%. Do these 400% loans contribute to individuals' resiliency to personal distress? I measure the outcome with foreclosures, death, drug and alcohol abuse and births.

With 20% of U.S. residents financially constrained,[2] 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 $40 billion in loans each year (Bair, 2005; Fannie Mae, 2002).[3] Despite (or because of) the growing demand, State and Federal authorities are working towards regulating and curbing the supply of payday lending. So far, fourteen States have banned payday lending outright, and most other States now regulate the fee structure and/or the process of revolving loans.

From one perspective, payday lenders should help distressed individuals to bridge financial shortfalls without incurring the greater expense of delinquency or default on

[2] See Hall and Mishkin (1982), Hubbard and Judd (1986), Zeldes (1989), Jappelli (1990), Calem and Mester (1995), and Gross and Souleles (2002).
[3] As a point of comparison, venture capitalists invested $21.7 billion in 2005 according to VentureXpert.

obligations. Acting in a vacuum of options for distress finance, payday loans should enable individuals to smooth liquidity shocks without incurring the larger costs of bouncing checks, paying late fees or facing service suspensions, evictions or foreclosures. As such, one view of payday lending is that it should be welfare-enhancing.

An opposite, more prevalent, perspective is that payday lending destroys welfare. The availability of cash from payday loans may tempt individuals to over-consume. An individual who is likely to fall to temptation would prefer that a self-control mechanism be set up before the temptation arises. In this case, if payday lending were banned, the temptation next period to over-consume with payday cash would be removed as per the models of Gul and Pesendorfer (2001; 2004).[4] In this view, payday lending can be welfare-destroying.[5]

To answer whether payday lending improves or destroys welfare, I use natural disasters as a community-level natural experiment. Natural disasters cause personal distress for at least some members of a community. The noise of having individuals in a community unaffected by the disaster biases my tests against finding any effects. I perform the analysis at the zip code level, focusing on the State of California during 1996-2005. I use positive and negative welfare measures to capture both the resiliency of lifestyle patterns during distress and the permanent consequences to distress. My positive welfare measure is births, and my negative measures are foreclosures, deaths, and alcohol and drug abuse.

The difficulty in measuring how payday lending impacts welfare is in disentangling the payday lending effect from correlated community economic circumstances that determine welfare outcomes. To overcome the endogeneities, I use a propensity score matched, triple difference (difference-in-difference-in-differences) framework. The role of the propensity score matching is to align communities on the likelihood that residents are financially constrained prior to the natural experiment of disasters. I generate propensity scores by estimating the probability that an individual in

[4] A large and growing literature documents the short-term patterns of consumption-savings behavior (e.g., Zeldes, 1989; Attanasio and Browning, 1995; Carroll, 1997; Thaler; 1994; Laibson, Repetto and Tobacman, 2005; O'Donoghue and Rabin, 2006).

[5] Consumer lobby groups argue that by expressly entrapping individuals who have little financial depth in a spiral of debt obligations, payday lenders permanently alter the well-being of borrowers (*USAToday*, August 31, 2006; Graves and Peterson (2005); Center for Responsible Lending (2004); Consumer Federation of America (2004); and Chin (2004)).

the Survey of Consumer Finances (SCF) is financially constrained as a function of socioeconomic characteristics and then projecting the estimates onto Census socioeconomic data observed at the zip code level.

The role of the triple differencings is to overcome other possible endogeneities. Differencing around natural disasters provides a set of two counterfactuals – the counterfactual of what a community hit by a disaster would have looked like if it did not have access to payday lending and the counterfactual of what a community with payday lending would have looked like if it had not been hit by a disaster. Natural disasters allow me to observe a situation in which demand for distress loans is not met by an endogenous supply. In addition, natural disasters provide an economically representative benchmark of what communities with payday lending that are subsequently hit by disasters would have looked like in the ex post period if a disaster had not happened.

Because my welfare variables are count variables, I estimate both a triple difference linear model and a triple interaction Poisson model. I measure welfare over two year windows before and after the disaster.

The results indicate that payday lenders offer a valuable service to community by providing credit in a very incomplete market. Natural disasters induce an increase in foreclosures, but the existence of payday lenders significantly offsets this increase. Communities with payday lenders are able to sustain their pre-disaster birth rates; other disaster-struck communities see a drop in births under the economic distress following disasters. Drug and alcohol treatments and deaths both fall in periods following disasters, consistent with prior research. The decrease in drug treatments is magnified in communities with payday lenders, and the drop in deaths only occurs for areas with access to payday lending.

Are banks substitutes for payday lenders? Because the role of financial institutions for natural disaster recovery is intrinsically important in its own right, I re-run all of the estimations using bank density in place of the existence of payday lenders. In only two of the sixteen specifications, do banks provide a valuable service of being a lender to individuals in distress in a similar way that payday lenders do. Finance is valuable for community resiliency, but for the most part, the value does not come from mainstream banking.

Implications to my results must be put in context of the experimental design. Individuals that use payday loans after natural disasters look just like people who use payday loans when faced with personal disasters, such a car breakdowns and health expenses. Since personal disasters happen all the time, my results can be extended to much of payday borrowing. However, I do not know exactly what 'much' means. There are likely to be payday borrowers who do not face disasters at all; rather they habitually over-consume and use payday cash to smooth cash cycles. Skias and Tobacman (2006) provide evidence consistent with the use of payday lending in such settings. Of course, the habitual over-consumers are those tempted by payday cash and thus those most likely to have negative welfare impacts. My results must be interpreted that payday lenders are providing a valuable service to communities, but do not speak to the net benefits distilling to those habitually falling to temptation. This suggests an agenda for future research which I discuss more fully in the conclusion.

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. Section IV describes the data sources and summary statistics. Section V presents the intermediate propensity score matching results, and Section VI presents the main empirical results showing the effect of payday stores on welfare. Section VII concludes.

I. Consumer Finance Institutions

Consumers have a variety of options for their borrowing needs. The typical consumer predominantly borrows from three – banks, mortgage institutions and credit cards. A number of individuals, however, have restricted access to credit at these institutions and resort to borrowing from high interest lenders. These additional 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.

The market for high-interest consumer loans divides generally into three segments. Credit cards provide the bulk of the liquidity for high-interest lending with rates up to 29.99%. (Of course, add-on fees charged by credit cards make the effective interest rates higher (e.g., Gabaix and Laibson, 2005; Massoud, Saunders and Scholnick, 2006)). The Supreme Court's *Marquette* ruling of 1978 took away the de facto power of State usury laws, making it possible for credit cards to offer higher interest credit, but they have not as a general rule crossed the threshold of 30%.[6] To our knowledge there is no decisive study of why this is so, but the threat of greater regulation undoubtedly concerns them (Knittel and Stango, 2003).[7]

From 30% to 400%, specialty markets offer collateralized loans (e.g., title lenders and pawn brokers), black market loans, and some new online instruments for the brave. These markets are very narrow and do not provide many options for consumers constrained at their debt capacity or those with poor or no credit histories.[8] As a result, it is unsurprising that prior research finds that approximately 20% of U.S. consumers are credit constrained (Hall and Mishkin, 1982; Hubbard and Judd, 1986; Zelders, 1989; Jappelli, 1990; Gross and Souleles, 2002).

All of this implies that for most individuals who have already maxed out their credit limits or who have poor or no prior credit history, the only option is to borrow at 400% APR from a payday lender.

How does payday lending work? An individual visits a payday loan store with a recent paycheck and checkbook. The typical loan given is approximately $300 with a fee of $50. In such a case, the borrower would write a check for $350, post-dating it to his payday, 10-14 days hence. The payday lender verifies employment and bank information, but does not run a formal credit check. (Un-banked and unemployed individuals do not qualify for payday loans; thus, the notion that payday stores lend to the poor-of-the-poor is not generally true. See, for instance, Barr (2004).) Two weeks hence, if the individual is not able to fund the check, which happens more often than not, he will return to the payday store and revolve the loan, incurring another $50 fee. The borrower typically is a

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[6] *Marquette National Bank v. First of Omaha Services Corp.* (1978) allowed credit cards to apply the State interest rate law of the corporate headquarters. Soon thereafter at the prompting of Citibank (*Frontline*, August 24, 2004), South Dakota lifted its usury ceiling, and all credit card companies could easily re-locate to South Dakota (or thereafter Delaware) and not be bound by usury limits.
[7] Since the early history of the United States, 36% has been the cap that States have applied as the high limit on defining usury (USA Today, August 31, 2006). By charging less than 30%, credit cards may be avoiding the perception that they are approaching the height of usury.
[8] For a more comprehensive survey of the market of pawn shops, title loans stores and informal lenders, see Caskey (1994; 2005), Bolton and Rosenthal (2005) and Barr (2004). Historically, usury laws' prohibitions have forced high-interest lending into the black market.

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).[9]

In 1993, the Alabama Supreme Court ruled that returns on payday loans should be deemed fees and not interest, thereby absolving payday lending from usury laws and inaugurating an explosive growth in the market. The $40 billion in payday loans generate an estimate of $5.4 billion in fee revenues per year based on ratios from the Center for Responsible Lending (2004). Are these fees and the implied APR over 400% reasonable? A consideration of the transaction costs of payday lending helps to put the fees in context.

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; subsequent loans take less. Thus, for $50, an individual buys a 10-14 day float, the capital and labor cost of ten minutes of service, the service charges for verifying bank account information, and the risk of default on $300. The default risk is surprisingly low for high-interest loans (e.g., 6% in North Carolina (Center for Responsible Lending, 2004)) because payday lenders receive a legal entitlement to pull funds from the borrower's active bank account, can charge for insufficient funds, and are privy to the exact timing of the borrowers wage payments.

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. In the last five years, fourteen States have made payday lending illegal.[10]

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: costs of shopping for lower rates, going through the application process, and foregoing any benefits of nurturing a favorable payment record

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[9] For overviews of payday services see, e.g., Stegman and Faris (2003), Center for Responsible Lending (2004), and Barr (2004).

[10] These States are Connecticut, Georgia, Maine, Maryland, Massachusetts, New Jersey, New York, North Carolina, Pennsylvania, Vermont, and West Virginia (Center for Responsible Lending, 2004). It is likely that payday operations do continue to operate in these States through other financial convenience stores, but the per-unit costs of running black market operations are undoubtedly higher.

with a lender.[11] If Shui and Ausubel (2005) are correct in their characterization of the credit card market, borrowers may over-weigh 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 of this section are twofold. Payday lenders act in a vacuum of household lending above 30% APR. In addition, payday lenders sustain 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.

II. Competing Hypotheses

The argument for why payday loans might increase welfare is straightforward. Individuals often experience some sort of personal disaster (e.g., medical expenses or car breakdowns) leaving them without cash for their short-term obligations. Banks and credit cards cannot provide relief, as the transaction costs of making small-scale, short-term loans are substantial, driving potential lenders into conflict with usury laws (for banks) or the threat of greater regulation (for credit cards). Small-scale personal disasters lead to bounced checks, late fees, utility suspensions, repossessions, and, in some cases, foreclosures, evictions and bankruptcies.[12] 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.

[11] The idea that individuals do not search further for a better price is also consistent with experimental evidence in Kogut (1990) that sunk costs are factored into search decisions. In addition, the distribution of search costs may imply that switchers are the least favorable customers (Calem and Mester, 1995).
[12] In 2003, banks generated $22 billion in non-sufficient fund fees and $57 billion in late fees (Bair, 2005).

On the other hand, the consumer advocates may be right. What if payday lending tempts individuals to over-consume? Amble literature shows 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). Cash from payday lending may encourage such behavior. The argument why individual choices may be welfare-destroying, lies in the temptation and self-control models of Gul and Pesendorfer (2001; 2004) and O'Donoghue and Rabin (2006). Temptation in some intermediate period may steal from consumption in a final period, thereby reducing the present value of lifetime consumption. In such a setting, having an ex ante control mechanism (in this case, a ban on payday lending) can improve overall welfare.[13]

The availability of payday loans for consumers tempted by cash (either cash-in-hand or the ease of access to cash) may encourage welfare-destroying over-consumption when temptation arrives. In such a case, payday lenders can be villains.

Whether payday lenders are heroes or villains is not necessarily a mutually exclusive question. It is likely that payday borrowers are of two types – those who face personal disasters and those who succumb to temptation. I leave the warranted task of disentangling of borrower types for my future work. However, I want to emphasize that repeat borrowers can be of either type. Banning the revolving of loans would impact the welfare of both types of borrowers.

In the empirical design, I use the exogenous shock of natural disasters to identify the demand for payday loans by individual facing personal disaster. My empirical design explicitly focuses on the marginal impact of lenders around disasters. By doing so, it is likely that I am removing the welfare implications to payday loan users who borrow in ordinary economic times. However, since natural disasters affect individuals in a similar way that personal disasters would, and since personal disasters are an ordinary fact of life, I can interpret my results broadly as long as I keep the caveat that there is an [unknown] proportion of payday borrowers to whose welfare I cannot speak. Importantly,

[13] DellaVigna and Malmendier's (2004) study self-control mechanisms in the interesting case of contract offerings. See Bernhein and Rangel (2006) for an overview of welfare analysis in light of behavioral findings.

the individuals I exclude in the measurement are theoretically likely to be those most negatively affected by payday borrowing.

III. Empirical Methodology

The identification of the payday effect faces two empirical challenges – measuring access to payday services and handling endogeneity concerns related to welfare effects. This section explains how the use of natural disasters helps us with both challenges and then describes the general estimating procedure.

A. Access to Payday Loans

Payday lenders decide where and when to open a store. If there is excess demand in an area, nothing prevents a new store from quickly opening to meet the demand. Because of this, it is difficult to observe a situation in which individuals would optimally choose to use payday services, but there is no provision of services. This matters because identifying a [causal] effect of payday lending requires addressing what an individual's welfare might have been if he had not used payday services, which requires identifying a situation in which the individual does not have access to payday loans. The solution in this paper to the endogeneity of access to payday services lies in natural disasters.[14]

Natural disasters are shocks to the demand for household finance. Without frictions, payday lenders should endogenously respond to these shocks by opening stores proximate to the new demand. The data collected for this study reveal that payday lenders do not open new stores to meet the increased demand from natural disasters. The reason is the same as the adverse selection effect of new credit cards attracting the worst customers (those who quickly pay off their debt float) (Shui and Ausubel, 2005). Payday borrowers who resort to payday loans only in the extreme case of natural disasters are not likely to meet the profile of repeat customers who generate the bulk of the profits for payday lenders. Thus, it is not surprising that payday lenders do not rush to meet the temporary demand following natural disasters. As a result, natural disasters enable us to

[14] An alternative solution would be to use varying regulatory environments (prohibitions) across States. The legislation changes are very recent, however, making it difficult at this point in time to measure welfare impacts. In addition, it is not clear how to interpret welfare effects when payday-type loans move to border cities across State lines or to the black market.

observe a counterfactual of individuals with demand for but without access to payday loans.

I measure access to payday services and conduct the analysis at a community level. Using community data rather than individual data avoids the difficult task of attributing individual welfare outcomes to payday borrowing orthogonal to all other events in individuals' lives. At the same time, community data is sufficiently fine in its granularity to capture welfare effects felt by individuals and to construct a relevant measure of access to lenders. In particular, the existence of a neighborhood payday lender captures individual access to payday loans. Survey data confirm that convenience is the foremost reason for borrowers to use payday services (Fannie Mae, 2002). Because payday loans are for small dollar amount over short time spans, the individual transaction costs of traveling long distances for the service become quickly salient. When I run empirical tests, I do so at a very small community measure (zip code) and at wider a measure (all adjacent zip codes), to ensure that I am not over-asserting the case that individuals do not travel to use payday services.

B. Welfare Endogeneity: Matched Triple Difference Model Intuition

The empirical model is a matched triple difference specification. This subsection explains the role of the matching and the differencings as solutions to possible endogeneities between the effect of payday lending and economic fundamentals in the community. Some very simple notation is helpful. I denote welfare by ω. Welfare has two subscripts identifying the community. The first subscript {D, N} indicates whether the community will be hit by a disaster (D) or not (N) in a future period. The second subscript {A, U} tells us whether payday loans are available (A) or unavailable (U) in the community.

To foster the intuition, it is useful to ignore natural disasters for the moment and consider on an urban up-and-coming community in Los Angeles booming with new services including a payday store. The first empirical step is to match communities on propensity of the residents to be financially constrained. Basing the analysis on a sample of communities matched on the propensity to be credit constrained eliminates the possibility that an effect attributed to payday lending is just capturing the location preferences of payday lenders for areas with more potential borrowers. Assume that the propensity score matching locates a similar up-and-coming urban community in Sacramento.

The next step is to take a first differencing of welfare over time to remove community-specific effects. Rather than searching for a way to normalize welfare measures, I remove the community heterogeneity by taking changes over time, denoted by $\Delta\omega$. Say the Los Angeles community has a larger, denser population than the one in Sacramento. By taking differences over time, the changes in community welfare will comparable across communities in an empirical model.

The second differencing contrasts areas with payday lenders to areas without payday lenders. Assume that there is a set of aging industrial communities in Los Angeles and Sacramento whose factories are struggling to survive. These aging communities have the same propensity for the individuals to be financially constrained as the urban communities, but payday stores are not to be found. The financially constrained individuals in the aging community are unemployed or retired, making them undesirable to payday lenders.

At this point we have a plausible model to identify the effect of payday lenders; namely, a propensity matched, difference-in-differences model. The model compares the welfare changes over time of communities with and without payday lenders according to: $\Delta\omega_{NA} - \Delta\omega_{NU}$. One can think of benefits of the matching and the differencings up to this point as balancing communities in terms of the financial constraints of individuals, removing community-specific heterogeneity, and benchmarking welfare against similar communities without access to payday lending.

The difference-in-differences model does not however address the concern that community-specific factors influence welfare for economic reasons that also cause the openings or closings of payday loan stores. The booming service industries in the urban communities, for example, might improve welfare (e.g., through job creation) and at the same time attract payday stores. A measurement of the effect of payday lending could just be an associative relationship of welfare changes with the location preferences of payday lenders. Herein is the role of natural disasters as an exogenous shock to the financial condition of individuals in a community that does not affect access to payday lending.

To complete the example, consider that a flood hits both the urban community and the aging community in Sacramento. For the people of Sacramento, financial distress following the natural disaster heightens the role of payday lending such that both the urban and the aging industrial communities have demand for loans. The flood allows us to observe the change in welfare following the disaster for the urban area that has access to interim finance as compared with the aging community that lacks payday lending. Furthermore, I can use the pattern of the difference-in-differences in Los Angeles as a benchmark of how the relationship between a payday community and a non-payday community evolves over time without a disaster. The triple difference removes any systematic pattern of welfare evolution by subtracting out a benchmark evolution of welfare in payday and non-payday communities ($\Delta\omega_{NA} - \Delta\omega_{NU}$) from the effect of experiencing a disaster in a payday versus non-payday community ($\Delta\omega_{DA} - \Delta\omega_{DU}$). The resulting triple differenced estimator ($\Delta\omega_{DA} - \Delta\omega_{DU}$) – ($\Delta\omega_{NA} - \Delta\omega_{NU}$) should be unrelated to both personal and community economic fundamentals.

A final point should be noted about the matched triple differencing framework. The empirical methodology identifies the effect of payday lending in crisis situations. Although I have no reason to expect everyday payday borrowing to affect individuals in a systematically different way, I cannot rule out this possibility. Thus, I interpret the results in this light.

B. General Specification

A general estimating framework averages the triple differences over the set of *M* four-community matches each denoted by *m*:

$$\Delta^3 = \tfrac{1}{M}\sum_m \left(\Delta\omega_{(DA)_m} - \Delta\omega_{(NA)_m}\right) - \left(\Delta\omega_{(DU)_m} - \Delta\omega_{(NU)_m}\right). \tag{1}$$

In this notation, Δ represents the change over time around the natural disaster event. For the matched non-disaster communities, the Δ change is around the fictitious disaster period defined by the disaster-hit community to which it is matched.

The triple differences specification can be reproduced in a regression framework. In this case, Δ^3 is identical to the estimate of β_7 from:

$$\omega_{it} = \beta_0 + \beta_1 D_{it} + \beta_2 Post_{it} + \beta_3 A + \beta_4 D_{it} Post_{it} + \beta_5 D_{it} A_{it} \\ + \beta_6 Post_{it} A_{it} + \beta_7 D_{it} Post_{it} A_{it} + \varepsilon_{it} \quad . \qquad (2)$$

The index *i* denotes individual communities. As in the four-community example, *D* indicates that community *i* is hit by a natural disaster, and *A* indicates that payday lenders are available in the community at the time of the disaster.

Timing is important in (2). The indicator variable *Post* equals to 1 for the post disaster period (and post disaster fictitious period) for the disaster community (and its match). To eliminate noise in welfare observations, a longer time series in the panel is included before (*Post* = 0) and after (Post = 1) the disaster. The analysis includes a set of *n* periods before the event and *n* after, where *n* varies depending on the granularity of time measurement of the welfare variable. I generally use time periods up to two years to capture medium-to-long term welfare consequences. Following Bertrand, Dufflo, and Mullainathan (2004), I handle the serial correlation in the within-community measures in the panel either with Newey-West standard errors or with a collapsing of the pre and post period effects into single pre and post observations.

Technically, the model in (2) assumes that a set of disasters should occur simultaneously for communities with and without payday stores. Disasters, however, do not all happen at the same time. In fact, this is a benefit. Since the non-disaster matches are timed to the disasters, and since I use a set of disasters that are spread randomly over 1996-2003, there should be no bias in the triple difference estimation. Instead, the time randomness of disasters creates a good variation for asserting that any effect I find cannot be attributable to some other event occurring at that point in time.

A final concern with (2), unrelated to the timing assumptions, is that although the communities are matched on ex ante fundamentals, the intra-period economies might diverge. In particular, disaster resiliency may be associated with payday availability but be caused by economic fundamentals. Continuing the example from before, if one community is an up-and-coming urban and another is an aging once-industrial town, disaster recovery might be stronger in the community with payday stores available simply because businesses in declining economic communities may be less willing and able to recover from a natural disaster than businesses in new growth areas. To handle this concern, where possible I add economic controls to the regression in (2) to capture the evolution of the communities.

IV. Data & Summary Statistics

I limit the analysis to the State of California because of the detailed micro-location data available over time for both the payday lenders and welfare variables and to isolate an analysis in a single regulatory environment. In this section, I describe the payday, disaster and welfare variables used in the analysis. I save a description of the data for matching communities for its own section.

A. Payday Data

The State of California Senate Bill 1959 legalized payday lending in 1996 and placed their licensing and regulation under the authority of the Department of Corporations. The Department has license data for each payday store, with the data containing the 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 three lending categories: Deferred Deposit Lenders, California Finance Lender, and Consumer Finance Lenders. Although the Deferred Deposit Lenders category is specifically for payday lenders, this category was not used until 2003. Thus, my measure of payday lenders includes entities in all three categories after filtering out banks, insurance companies, auto loan companies, and realty lenders. The largest group of non-payday entities left in the sample is the set of check cashers with lending licenses. Not all check cashers fall in this category, but most check cashers often offer a bundle of services, often including payday loans. Erring on including check cashers with lending licenses should be a benefit. It is possible that some payday stores are operating without explicit payday lending licenses through check cashing outlets. Including check cashers eliminates the possibility that I am biasing toward finding results by only having areas with heavy densities (some legal and some not) of payday stores.

The data identify 10,502 lenders operating at distinct locations at some point in time between 1995 and 2005 in the State of California. In 2002, there were 2,160 payday stores, or 1 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 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. By 2005, there were 6,194 lenders in California. Most of the analysis pre-dates this growth period.

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 2.2 and 1 payday lenders. The maximum number of lenders in a community is 59, in an area in Orange County. A total of eight high density lender communities have over 40 lenders in a zip code, all occurring in Orange, San Bernardino, and Los Angeles counties. The empirical design is based on the yes/no question of whether there are any payday lenders in the zip code community.

B. Natural Disaster Data

Natural disaster data come from the University of South Carolina's Sheldus Hazard database, which provides the location (by county), type (earthquake, wildfire, hail, tornado, 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 map 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 disasters which cause more than $50,000 of property damage in a county. The $50,000 threshold may be too small to inflict any financial distress on community individuals. Thus, I remove all disasters than incur less than half of a million in property damage to the area and $133 in property damage per person, the average per capita property damage from Katrina for all Gulf Coast counties (Burby, 2006). Because this filter may exclude sparsely populated areas hit with a very localized but significant disaster (e.g., a tornado), I include disasters in which the per person property damage is greater than $277, the average property damage incurred by residents of coastal counties in Mississippi during Katrina.

To put the filters in perspective, the population of zip codes varies from zero to 113,697 people, with a mean and median of 21,088 and 16,424 and a standard deviation of 21,063. The thresholds of $133 and even $277 may seem rather small, but it is important to realize that only a few disasters (e.g., a wildfire) hit all residents of the community. Identifying the effect of payday lenders at the community level using disasters is a stringent test in that the noise from all the individuals in the community not thrown into distress by a disaster works against my finding any results.

Table 2 contains a breakdown of the disaster statistics by disaster type over the sample period 1996-2005. The six primary disasters surviving the filters are floods (25 occurrences impacting 478 communities), wildfires (42 occurrences impacting 195 communities), severe storms (10 occurrences impacting 72 communities), landslides (2 occurrences impacting 49 communities), earthquakes (9 occurrences impacting 6 communities) and tornadoes (1 occurrence impacting 1 community). Wildfires have inflicted by far the most property damage to the State, both on a per-occurrence basis and an in-sum level. At the other end, the one tornado only hit one small-population community, but caused nearly $1,000 in damage per person.

The area hit by a natural disasters can be declared to be a disaster zone by the governor of the State, thereby entitling it to Federal Emergency Management Agency (FEMA) monetary support. FEMA support is slow to arrive and does not address the immediate concern for financial liquidity.[15] However, since there is a presumption of financial support in FEMA areas, I re-run all of the results for only the areas not declared to be FEMA disaster areas.

C. Welfare Data

Series of data at the zip code level are difficult to find. Fortunately, RAND California compiles micro-level data for a number of welfare indicators for the State. I use the RAND database for all of the welfare variables. Below, I describe the motivation for collecting the welfare variables as well as the original sources of the data.

[15] *Newsweek*, September 11, 2006 edition, “Relief When You Need It” by Silvia Spring.

I consider two types of welfare reactions to distress: measures of negative consequences and measures of resiliency. My dataset uses three measures of negative consequence and one of resiliency. I begin with the negative consequence variables. Ideally my variables should be direct measures of economic consequence. However, the availability of zip code level economic statistics limits my variable selection. I focus the majority of my results interpretation on the first negative consequence variable, *foreclosures*, for the ease of making the connection between distress and outcome. However, I do not want to discredit the other variables, just to mention that I expect to be able to measure the impact of disasters most precisely using the foreclosure measure of welfare.

Financial distress may lead to defaults on mortgage payments. The variable *foreclosures* is the sum all foreclosures in a community recorded by the California Association of Realtors during each quarter from 1996-2002. Individuals short of cash may resort to income-generating crime.

Rather than resorting to crime, individuals who are financially constrained may choose to ignore health issues needing costly medical treatments. There is a long literature in health economics that medical treatments are inferior for the poor and uninsured.[16] Unfortunately, I cannot observe medical community treatments. However, the culmination of the inferior medical attention for the financially constrained is an increase in mortality. Hadley's (2003) survey documents that the uninsured mortality rate is 4% to 25% higher than that of the insured. Supporting the evidence for the uninsured, Streenland et al (2004) find a monotonic decline in death rates with an increasing socioeconomic status. To capture the impact of medical choices, my second welfare variable is community *deaths* as measured by the California Center for Health Statistics for 1996 to 2004.

The final negative welfare variable measures a social outcome that results from the stress of economic crises. Evidence suggests that alcohol and drug abuse increases during all types of stress situations except natural disasters (North et al, 2004). By including drug and alcohol treatment, I study whether payday lending intensifies or mitigates the economic stress of disasters. My variable *drug and alcohol* comes from data

[16] See Hadley (2003) for a survey.

collected by the California Department of Alcohol and Drug Data Programs on the number of admissions into alcohol and drug treatments by zip code for 1996 to 2003

In addition to negative welfare variables, I investigate a positive welfare variable reflecting the possibility that payday loans may help individuals continue life as normal following a financial shock. The positive welfare variable is *births*, from the California Center for Health Statistics for 1996 to 2004. Economic constraints may discourage individuals from having children (Becker, 1981). I use *births* to test whether payday lenders aid community resiliency to distress.

Table 1 presents the overall mean, minimum, median, maximum and standard deviation for the four welfare variables as total counts and as rates, both by zip code. (The estimating samples are each different subsets of the data.) Comparing the means to the medians across variables shows that foreclosures and drug treatments have the largest right skew, measured both as counts and as rates. The skews in rate variables confirm our intuition that letting the model empirically determine the relationship with normalizing variables may be the optimal strategy to mimic the underlying count process of the data. The statistics all seem reasonable with intuition. Birth and deaths both have minimums at 5, but the mean number of births (509) is more than double the mean deaths (211), consistent with the fact that California's population is growing. The average number of foreclosures per community is 10 with a wide range from 0 to 300. Drug and alcohol treatments also show a wide range, from 1 to 2,711 cases.

The analysis incorporates four additional variables as controls. Similar summary statistics are collected for these variables and are presented in Table 1. The total number of owner-occupied housing in a zip code community, *owned housing units*, and the total population of the community, *population,* are normalizing variables available from the Census. *Housing prices* serves as an important covariate for foreclosures analysis. In addition, since housing prices are at least partially related to future income prospects (Campbell and Cocco, 2006) housing prices serve as a general control variable for the economic growth prospects of the region. Housing price data are available over quarters by zip code from the California Association of Realtors.

The final variable, *FDIC Banks (banks)*, measures the number of bank branches insured by the FDIC by zip code in the state of California. I use FDIC banks for two purposes – to control for the degree of commercialism in an area and to test whether the welfare impact of payday lending results are particular to payday lenders or are systematic of financial institutions at large. The FDIC bank data does not cover credit unions and state banks, and thus I interpret my results as reflecting effects of mainstream banking, not necessarily all financial institutions. The FDIC data contain addresses of each bank branch. I map branch locations, collapsing the variable *banks* to a count of branches in a zip code. The variable *banks* ranges from 0 to 43 in the sample, with a mean and median of 5.6 and 4.

V. Propensity Score Matching

The empirical methodology calls for matching communities hit with a disaster to communities similar in their financial profile but untouched by natural disasters. In this section, I describe the procedure, data and results for matching communities along the likelihood of their residents to be financially constrained.

A. Financially Constrained Data and Method

The Survey of Consumer Finances (SCF) contains a number of measures that identify individuals who are constrained financially. Since the sample of the SCF is not sufficiently large to be representative of individual communities, I combine what I can learn about socioeconomic attributes of constrained individuals from the SCF with detailed socioeconomic information that is available at the community level from the Census. The logic of the procedure is identical to a discrimination and classification analysis, whereby one finds a set of independent variables that discriminate whether an individual fits into one group or another and then applies the coefficients from the discriminant line (propensity score) to classify an out-of-sample group along the same set of independent variables.

I use the 4,300 individuals in the SCF to generate propensity scores of financial constraints for 1,762 zip code communities for the State of California. Because the matching of communities must occur prior to the event of disasters, I use the SCF of 1995 to characterize financially constrained individuals for the 1996-1998 years, and the SCF of 1995 for all years subsequent to 1998.

Jappelli (1990) and Calem and Mester (1995) estimate logistic relations between being financially constrained and socioeconomic predictors. I follow the same procedure using all SCF socioeconomic variables considered in Jappelli and Calem and Mester that are also available in Census files. I define two measures of being financially constrained. *AtLimit* is an indicator variable equal to one if the individual's outstanding balance on his credit card is within $1,000 of his credit card limit, if he has credit card debt.[17] Approximately 9% percent of respondents in both 1995 and 1998 were within $1,000 of their credit limits. The second measure of financial constraints, *Reject*, is equal to one if the individual has been rejected for a credit card or a credit card line increase in the last 5 years. In 1998, 18.5% said that they had been rejected for credit within the last five years; this is an increase from 17.3% in 1995.

The socioeconomic variables are wealth, income, age, education, marital status, race, sex, family size, home and car ownership, and shelter costs. To benefit from as much information as Census provides, I define variables in terms of whether a respondent falls in a range of values, in line with the Census definitions. For example, rather than using income as a variable, I use an indicator for whether income is between two ranges. The profile of a Census community will more closely approximate the SCF characteristics by capturing the distribution of socioeconomic variables rather than just centroid features. The exact variable definitions are provided in panel B, which presents the percentage of the community households (or individuals, as appropriate for the variable) that falls into the category at hand. The panel presents the means, medians, minimums and maximums of these variables for individuals in the SCF of 1998. The 1995 statistics are similar and are omitted for compactness.

A final data point is that the full Census data is only taken one time per decade. However, an update to most demographic variables is available from Census as of 1997. Thus, when doing the projection using the 1998 SCF estimates, I incorporate this update into the classification of communities.

[17] Stegman and Faris (2003) report that 91% of payday borrowers use other forms of consumer credit as well.

B. Financially Constrained Results and Community Matching

Table 4 presents the results of the logistic estimation of the probability of being financially constrained. The dependent variable is *AtLimit* in Panel A and *Reject* in panel B. I present both the 1995 and 1998 results across the columns of the Table. The coefficients in Table 4 should be interpreted as "compared to a wealthy, very educated, single male senior."

For both dependent variables, the probability of being financially constrained is highest at the $30,000 - $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.

Unemployment reduces financial constraints in two of the four estimations, perhaps indicating that the unemployed are more conservative in incurring debt. This result, however, must be interpreted after considering that total household income is already included in the model.

Less educated individuals more likely to be constrained in only one of the four estimations (only for 1995 for the *AtLimit* dependent variable). Even though other socioeconomic variables are already in the estimation, the result is curious in light of the literature on the actions of informed-versus-uninformed economic agents (Gabaix and Laibson, 2005; Campbell, 2006), but is consistent with the finding in Duflo and Saez (2003) that financial ignorance does not always explain particular deviations from seemingly rational behavior. In this case, education may simply have no role once income is controlled for simply because financial constraints are commonplace in the current environment.

Shelter costs have one common result: individuals whose rents and mortgages are less than $300 are likely to be less constrained. These are mainly homeowners who have paid off their mortgages. There is little to no direct effect for home or vehicle ownership, except that individuals with no vehicles seem to be less likely to find themselves constrained. The lack of a homeownership effect is again reassuring that financially constrained individuals are similar to payday borrowers. Surveys of payday borrowers find that slightly less than half of borrowers are homeowners (Barr, 2004).

There is some evidence that larger households are nearer to credit card limits but only in 1995 and only for households of 3-5 people. However, larger households are strongly associated with credit card rejections.

In all four specifications, younger individuals are more likely to be financially constrained. Women are less likely to have been rejected by credit cards, but women and men are equally likely to be near their credit card limits. Non-white individuals are more likely to be constrained in three of the four estimations. Finally, being married is good for not being rejected by credit cards but does not associate with being near credit limits.

In sum, the majority of the socioeconomic variables are significant in some ranges and consistent with prior predictions in all four logistic estimations. The R-Squares run from 0.106 to 0.147. Although there is much variation unexplained, the logistic estimates predict correctly whether an individual is financially constrained 85% of the time. This statistic suggests that I can feel a degree of confidence that the socioeconomic variables do in fact characterize constrained individuals.

C. Matching Propensity Scores

We project the coefficients from the logistic estimation onto the community-level Census data to create a projected propensity score for each zip code community. The propensity score captures the degree to which residents in the community are financial constrained.

The 1998 *AtLimit* propensity scores range from 0.03 to 0.22, with a mean and median both around 0.11. The 1998 *Reject* propensity scores range from 0.07 to 0.68, with a mean and median both approximately of 0.26.[18] The mean propensity scores from both *AtLimit* and *Reject* are generally in line with the overall probability of being constrained for the two variables, 9% and 18.5%, although the projections suggest that California has more constrained individuals than the national average.

Figure 1 maps the propensity score for *AtLimit* for each California zip code. The shadings on the map reflect the quintile of propensity scores; darker shadings indicate that a larger propensity of the community is credit constrained. On top of the shadings is a marker for the density of payday stores. Bigger markers indicate the existence of more

[18] The 1995 *AtLimit* propensity scores range from 0.03 to 0.38, with a mean and median both around 0.15. The 1995 *Reject* propensity scores range from 0.14 to 0.68, with a mean and median both approximately of 0.35.

payday stores. Because zip codes grow much smaller in more dense areas, I also include Figure 2, a blow-up picture of Los Angeles central areas.

The figures reveal that there is an association between payday stores and credit constraints, but there is association nowhere near complete. Many areas with financial constraints are not flush with payday lenders. Having a range in the degree that payday density and financial constraints overlap is important for the effectiveness of the propensity score matched identification strategy. Of course, we could consider correlations to see the same thing; but the Figure gives us more information. Financial constraints and payday store density cannot be considered solely urban versus rural or north versus south phenomena.

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. The matches use the propensity score from a time period (either 1995 or 1998) prior to the disaster. Since I have two measures of financial constraint, I have two propensity scores for each disaster community, and thus two separate set of matched data sets.

Before moving to the main results, I check whether the matching paired communities along similar welfare characteristics ex ante to any natural disasters. Table 5 presents t-test for differences in means of the welfare variables according to whether the communities have payday lenders or not and whether the communities will be hit by disasters or not. I would expect that there would be little difference in welfare for payday and non-payday communities since the matching is on the propensity of the residents to be financially constrained, a measure of demand for payday services. Column 3 shows that for all welfare variables, there is no significant difference in the means between payday and non-payday communities.

Stratifying welfare to disasters and non-disaster communities reveals a difference in welfare means for *foreclosures* and *drug and alcohol treatments*. Although the matching may pair communities with similar economic profiles, one possible interpretation for the difference in the means may be that disasters areas can be predicted with some probability. If areas prone to floods, for example, have residents who do not purchase flood insurance (which is not standard in housing contracts), then the market may compensate by requiring larger equity down payments. As a result, disasters areas may experience fewer foreclosures even in non-disaster times. Along the same lines, areas subject to disasters may attract more risk-loving individuals. As a result, drug and alcohol abuse may be higher in general.

These are only possible, but plausible, explanations for why there would be a systematic difference in the mean foreclosures and mean drug and alcohol abuse for disaster areas. For the purpose of this study, although it appears that the matching handles economic profiles well, any differences in welfare levels across disaster and non-disaster communities will be removed by including a disaster community dummy variable in the estimations.

VI. Results

The welfare variables {*foreclosures*, *deaths*, *drug treatment*, and *births*} are all natural count variables, suggesting that the empirical model should reflect the underlying count distribution of the data. Poisson estimation is a natural fit. However, one might argue that estimation properties of the triple difference specification derive from a linear concept. Therefore, I do two things to handle the count nature of the welfare variables.

First, I estimate the triple interaction model in (2) with Poisson regression. Theoretically, the Possion model should be a better fitting than the strictly defined linear triple difference fitting. In estimating the Poisson, I handle the serial correlation issue highlighted by Bertrand, Duflo and Mullainathan (2004) by collapsing the pre and post period observations into an average pre level of welfare and an average post level of welfare. By collapsing the data and reducing observations, the Poisson model becomes the most stringent test with which I approach the hypotheses.

Second, I estimate a linear triple difference specification as in (2) with Newey West standard errors. To do so, I need to normalize the count dependent variables to some community attribute, thereby viewing welfare as a rate concept. The natural normalization for the count of foreclosures in the community is the number of owner-occupied houses from Census data; for all other variables, population is a natural normalization.

One could simply create a rate measure of welfare, say foreclosures per owned houses, by dividing the welfare variable by the count of houses. However, given that each of the welfare variables is a count of discrete events governed by thresholds, modeling a count process directly as a rate may not capture the underlying forces affecting the realization of individual counts. (See, for example Grogger's (2002) study of community crime.) Therefore, I take a more general approach by allowing the appropriate relation between welfare and the normalization to be determined empirically. For the example of foreclosures, the natural logarithm of the rate of foreclosures can be expressed as: *ln*(*foreclosures*) – *ln*(*housing*). By bringing the log of housing to the right hand side of the estimation as a covariate, I allow the effect of equalizing communities by the number of housing to be incorporated empirically.

I estimate all of the models with both the *AtLimit* and *Reject* matchings of credit constraints. The measures capture different ideas. Having a history of being rejected by credit cards is indicative of individuals with expenses near the threshold of income as well as those with moderately good incomes, but poor cash management. Contrarily, being within $1,000 of one's credit card limit is most common for low income individuals who may manage cash well. Thus, the *Reject* variable (18% of the population) is more likely to pull together over-consumers as well as those very susceptible to personal disasters, and the *AtLimit* variable (9% of the population) likely focuses more directly on susceptibility.

A. Foreclosure Results

Foreclosures do not happen instantaneously. In the State of California, foreclosures have a required process time of 120 days and take a year on average (*Mortgage-Investments.com*). Since the foreclosure data are quarterly, I start my post period observations two quarters after the end of the disaster quarter to allow for quick foreclosures that close in six to nine months. I extend the post period six quarters forward to allow for the building process of financial distress to take its course and to be in line with my using two years forward as the post period for the other welfare variables, which are only observed at a yearly level.

Two independent variables are important to have in the estimation. The first is the natural log of house prices (in $1,000s) in the community. Housing prices should be negatively related to foreclosures. When the real estate market turns down, many individuals cannot sell their property at a price above their loan obligation to escape distress. Additionally, as interest rates rise, property values stagnate at the same time as overall debt obligations become more costly to service (Case, Shiller and Weiss, 2006).

The second added independent variable is the count of banks in the community, a measure of level of commercialism. Banks serve as a proxy control for gross product and employment, which are unavailable at a community level at anything smaller than census decade intervals. In addition, the opening and closing of banks controls for economic transitions of communities following disasters.

Table 6 presents the results with *foreclosures* as the welfare dependent variable. Columns 1-4 report results for the Poisson triple interaction model. In these estimations, *foreclosures*, *banks* and *houses* are expressed as their original count data. In all estimations, *housing prices* is expressed as a natural logarithm, to remove the skew from the distribution.

Column 1 and 3 report estimates from a Poisson treatment model of the effect of disasters on foreclosures, ignoring the effect of payday lending. (One can think of these columns as a difference-in-differences model of foreclosures on disasters.) The *Post*Disaster* interaction shows that disasters increase foreclosures in the Poisson model, but only significantly when the methodology matches communities along the propensity of the communities to be rejected by credit cards. The positive relationship between disasters and foreclosures is consistent with Anderson and Weinrobe (1986) who show that foreclosures significantly increased after the 1971 San Fernando earthquake.

Housing prices is only sometimes inversely related to foreclosures, as predicted. When the match is done with *Reject*, *housing prices* is strongly negatively related to foreclosures, but using the *AtLimit* match, foreclosures marginally associates positively with housing prices. The positive sign may result from not modeling the relationship between foreclosures and housing prices in a distributed lag framework (Case, Shiller and Weiss, 1996), complemented by the possibility that there should be a positive level association between house prices and foreclosures and a negative marginal effect.

Bank counts inversely associate with foreclosures. Whether banks actually play a role in preventing foreclosures or if the commercialism of areas with many banks implies greater cushions for escaping foreclosures, banks are an important control for the regressions.

The main result of Table 6 is in columns 2 and 4, which introduce the payday lending triple interaction (*Payday*Post*Disaster*). For communities matched on both the *At Limit* and *Reject* measures, payday lending decreases the number of foreclosures that result following a natural disaster. The estimates are significant at the 5% confidence level for the sample matched on the *AtLimit* measure of financial constraints.

Columns 5-8 repeat the analysis for the linear triple difference model. In these columns, *foreclosures* is expressed in natural logarithms. The covariate owner occupied housing counts (*houses*) is also expressed in logs, reflecting the approach of normalizing counts across communities by taking logarithms of the rate of foreclosures and moving the normalizing denominator to the right hand side.

For communities matched on the *At Limit* measure of financial constraints, payday lending decreases the number of foreclosures that result following a natural disaster. The triple interaction *Payday*Post*Disaster* is negative and significant, but only marginally so. For the *Reject* matching, the Linear Triple Difference model does not estimate a significant effect of the triple interaction in column 8.

Because the main result appears in the Poisson regressions, I focus my attention on these estimates, namely, column 2. How can we interpret the -0.701 coefficient? Ideally, we would like to be able to compare the overall increase in foreclosures after disasters (the coefficient 0.738 on *Post*Disasters*) with the -0.701 triple interaction. However, because Poisson estimates are interpreted as semi-elasticities and because the overall level of foreclosures is different in disaster areas compared to non-disaster areas, it is more natural to translate the coefficients into a matrix of predicted counts of foreclosures. Then, I can look at the triple differences impact on counts directly.

Panel B of Table 6 presents the predicted counts of foreclosures in the 2 x 2 x 2 box of payday areas or not, disaster or not, and pre or post periods. The predicted values vary widely, from an average count of 20.1 foreclosures in a non-payday, non-disaster pre community to 1.9 foreclosures in a post-disaster payday community. As the matching table suggested (Table 5), disaster areas have much lower counts of foreclosures.

The insight from the triple differencing framework comes from differencing out the level and single interaction effects. The (i) rows of Panel B show that foreclosures are declining over time for the average communities, particularly for non-disaster areas. When I difference out the effect of being in a payday area, the results become interesting. The (ii) rows suggest that compared to a benchmark of non-payday areas, the average change in foreclosures for payday communities is positive for communities not hit by disasters (2.3 increase in foreclosures) and negative for areas hit by disasters (2.8 decrease in foreclosures).

The triple result in row (iii) is the key result. Rows (ii) suggested that payday communities are generally increasing in foreclosures relative to non-payday areas in the benchmark non-disaster areas. But in disaster areas, foreclosures in payday areas are declining relative to non-payday areas. The simple differencing of these results in row (iii) reveals that the effect of payday lending on distress communities is a decrease in foreclosures by 5 units.

B. Death Results

Table 7 repeats the analysis for the death welfare variable. As in the foreclosure measurement, the pre and post periods are defined to be two years before and after a disaster. If anything, I expect disasters to increase death rates for one of two reasons. People experiencing natural disaster-induced Post-Traumatic-Stress-Disorder have negative health consequences (Karanci and Rustemli, 1995), and people in financial distress may postpone medical treatments if they cannot afford them.

In Column 1 of Table 7, I find the unintuitive result that death counts fall during the two years following disasters using the *AtLimit* measure of constraints. In particular, the interaction *Post*Disaster* is negative and significant. When the *Reject* constraint matching is used in column 3, there is no relationship between being in a post disaster environment and death outcomes.

Columns 2 and 4, however, provide some qualification to the negative association between disasters and death. The triple interaction *Payday*Post*Disaster* is negative and significant, and the double interaction *Post*Disaster* becomes positive and significant using both matching variables. In concert, the four columns suggest that economics matters in community reaction to disasters. Disasters increase the death count in some communities, but only in communities without distress finance.

This evidence supports that hypothesis that if disasters increase the financial constraints of community member, medical treatments may be foregone, drawing on the wide literature in health economics that medical treatments are chosen based on the ability of individuals to pay. The finding is consistent with Hadley (2003), who shows that mortality rates rise when individuals lack insurance.

In terms of the control variables, death rates are higher in areas with more banks, even after taking out the positive association between death and population counts. This result is consistent with the intuition in Phillimore and Reading's (1992) finding that death rates are lower in rural areas, even after controlling for socioeconomic factors.

The linear triple difference model estimates in the final four columns of Table 7 are not able to identify any significant effects of disasters or payday lenders on death welfare. The bank and population controls are consistent in sign with the Poisson model.

C. Drug and Alcohol Treatment Results

The third welfare variable is the count of drug and alcohol treatments. Again, I use a two year pre and post window to measure the effects. The majority of evidence suggests that alcohol and drug abuse does not change after natural disasters (North et al, 2004). Psychologist studying disasters conclude that disasters create a tendency for individuals to "take stock" of their lives (Cohan, 2002). Supporting this view, Shimuzu et al (2000) show that alcohol abuse falls in Japan following the Great Hanshin (Kobe) earthquake. At the same time, economic stress encourages alcohol consumption in general situations (Ruhm, 1995).

Table 8 presents the welfare estimations with drug and alcohol treatments as the dependent variable. In both the Poisson model and the Linear Triple Difference model (columns 1, 3, and 5), the *Post*Disaster* effect is negative and significant. Disasters decrease the need for drug and alcohol treatments, consistent with Cohan (2002) and Shimuzu et al (2000).

When I introduce payday lending interactions using the *AtLimit* matching, I find that drug and alcohol treatments fall even more for communities with payday lenders.

The coefficient from the triple interaction in column 1 (-0.222) suggests that two-thirds of the magnitude of the drug and alcohol treatments decline following disasters is in communities with payday lenders. Although the Reject matching is insignificant in both the Poisson (column 4) and Linear (column 8) models, the *AtLimit* result is again apparent in the Linear model of column 6, reinforcing this finding.

D. Birth Results

The final welfare variable is the count of *births*. Becker's (1981) seminal work on family life demonstrated that on average, people choose to have children based on economic motivations. Birth rates should fall after natural disasters in places succumbing to financial distress. Of course, I have to measure the post period beginning one year forward from the disaster (forward years 2 and 3) to account for pregnancy.

Columns 1 of Table 9 shows that birth rates fall in post disaster communities, consistent with Becker's intuition. The coefficient on *Post*Disaster* is negative and significant. The same results in columns 3, 5, and 7 are insignificant, which is a bit surprising give the strong significance in column 1. However, in three of the four cases in which *Payday*Post*Disaster* (columns 2, 4 and 8) is added to the model, the *Post*Disaster* coefficient returns to negative and significant. These results provide very suggestive evidence that the financial distress of disasters hinders childbearing decisions.

The main result of Table 9 is that the coefficients on *Payday*Post*Disaster* in both Poisson specifications and one of the Linear specification is positive and significant. Moreover the magnitudes of the birth result coefficients are very intuitive. In all cases, the negative impact of being in a post disaster period (the coefficients on *Post*Disaster*) is almost exactly negated for communities with payday lending (the coefficients on *Payday*Post*Disaster*).

E. Bank Results

In this subsection, I re-create the analysis replacing *Payday*, the existence of payday lenders, with *HiBankDensity*, an indicator variable capturing high bank density communities. I do this for two reasons. First, even after the propensity score matching and triple differencing and even after applying controls and finding consistent results across multiple welfare measures, I want to ensure that my result is not just a spurious association with some measure of commercial activity. Second, more fundamentally, I would like to know whether *any* financial institution can aid individuals in distress.

Table 10 presents a condensed set of results showing that bank density is a weak substitute for payday lenders. I re-run both the Poisson Triple Interaction and the Linear Triple Difference specifications for each of the four welfare measures and each of the *AtLimit* and *Reject* matching samples. Table 10 presents only the triple interaction (*HiBankDenisty*Post*Disaster*) coefficients from these regressions, where *HiBankDensity* takes a 1 when the density of banks per capita is above the mean.

Table10 shows that in only two of the 16 specifications does having a high bank density result in welfare improvements for communities under distress. No significant results emerged for the triple interaction using the Linear model. For the *Reject* matching sample with Poisson Regression, a higher bank density leads to fewer foreclosures after a disaster (but not for the *AtLimit* foreclosure sample). Higher bank density communities show a lower death rate (using the *AtLimit* matching only) after disasters. In both Poisson regressions of the drug welfare variable, higher bank density results in an increase in drug treatments following disasters, a welfare-deteriorating result. Likewise, higher bank density communities have a lower count of births following disasters in the *Reject* sample.

The payday variable (whose significance is presented in columns 2 and 4 for comparison) predicts welfare increases following disasters in 7 of the 8 Poisson specifications and 3 of the 8 Linear specifications. Bank density only shows welfare improvements for two cases, and is more likely to show welfare decreases following disasters. I conclude from Table 10 that bank density not a substitute for payday lending.

F. Future Work

In the preceding subsections, I provide evidence that the existence of payday lending in communities hit by disasters results in fewer foreclosures, less deaths, a fall in admissions to drug and alcohol treatments, and more births. The evidence implies that payday lending can be welfare-improving.

The work to verify this relationship is ongoing. In future drafts, I will add two more welfare variables – crime and community college enrollments. Germaise and Moskowitz (2006) show that more banks lead to less crime, supporting the role of economics in crime decisions. I have crime data at the city level and will run all my tests with this variable. In addition, an important resiliency variable is discretionary education choices. I have community college enrollments for each school in California and will test whether resiliency in education choices can be affected by distress finance.

The second addition in future work is to expand the meaning of community. I can spatially broaden a community's access to payday lenders to be the density of payday lenders in one's zip code and all adjacent zip codes. I will re-run all of my tests with this expansion to handle the robustness question that individuals are not so restricted in travel that limiting them to a zip code would be appropriate.

The final addition in future work is to tackle the final avenue for possible endogeneity. It may be that communities with payday lenders have a specific ability to handle disasters unrelated to their fundamentals of just being a payday community. I will instrument the location decisions of payday lenders with intersections. Survey data (U.S. Department of Treasury, 2000) confirms that payday lenders tend to cluster at major intersections. I have detailed road data from the California Department of Transportation which I will use to calculate intersection density. Intersections are indicative of economic activity, but not of the fundamental wealth of communities.

VII. Conclusions

Taking advantage of the exogenous shock of natural disasters, I find that the existence of payday lending increases welfare for all four outcome measures considered: foreclosures, death, drug and alcohol abuse and births. Access to finance is welfare improving at whatever cost.

My results speak to the benefits of local finance (e.g., Germaise and Moskowitz, 2006). Financial institutions aid the resiliency of communities to financial downturns. Resiliency is an important question in its own right, especially for developing countries, where distress finance is of primary importance for individuals and entrepreneurs. In the United States, resiliency has become a placard of the Department of Homeland Security since the devastation of New Orleans during hurricane Katrina. My results show that financial institutions contribute to the resiliency of communities.

My second main finding is that the different types of financial institutions are not substitutes for each other. In the majority of specifications, banks cannot serve the welfare-enhancing role for individuals in distress that payday lenders serve.

Combined, my two main results have important policy implications. Fourteen States have recently banned payday lending, and legislation is pending in the majority of others. If the existence of payday lending is valuable for those facing personal disaster in a way that other financial institutions cannot provide, then regulators should strive to make access to finance easier and more affordable, not ban it.

There is an important caveat to my results. My results generalize to the common occurrence of personal disasters. However, I do not capture the welfare impact of payday lenders on those borrowing in ordinary economic circumstances to fund over-consumption. For this subset of the population, I am not able to capture the full negative implications to the temptations brought by payday lending. However, if payday lending is welfare improving for at least some portion of the population, a move to ban payday lending is ill advised. Instead, alternative methods should be pursued to assist perpetual borrowers to stay out of debt traps.

That fact that finance may fodder temptation is an avenue for my future research. Can we document 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 itself.

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Figure 1: California Credit Constraint Propensities & Payday Densities

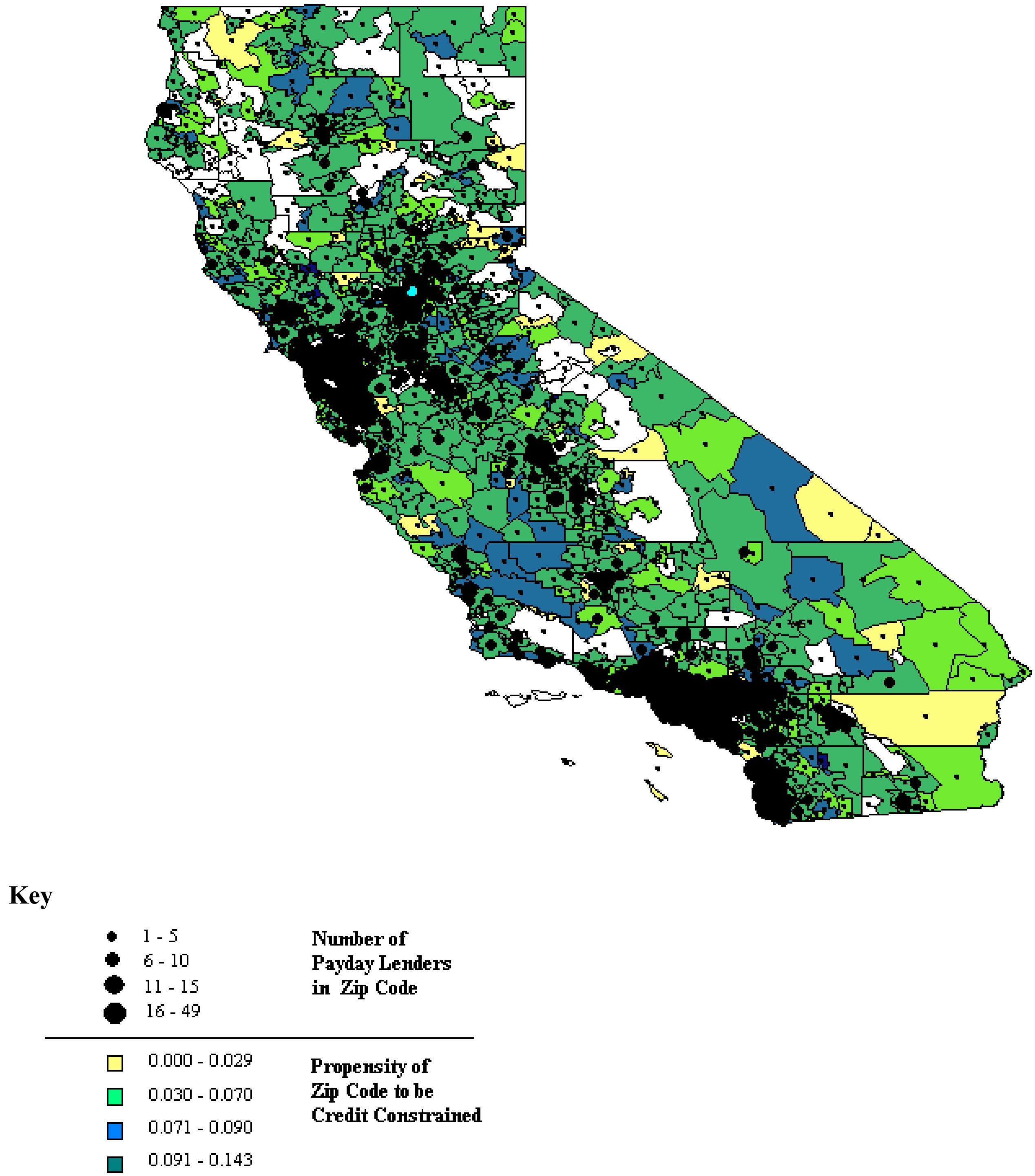
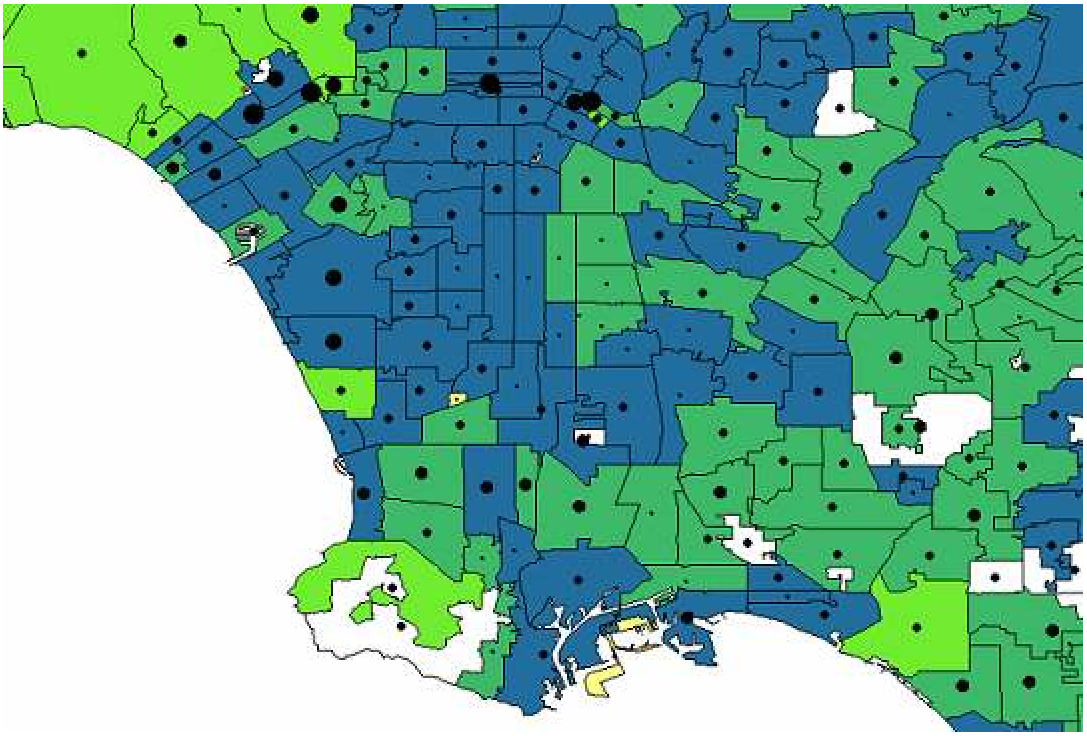


Figure 2: Payday Density in Los Angeles



Key

1 - 5 6 - 10 11 - 15 16 - 49	**Number of Payday Lenders in Zip Code**
0.000 - 0.029 0.030 - 0.070 0.071 - 0.090 0.091 - 0.143 0.144 - 0.363	**Propensity of Zip Code to be Credit Constrained**

Table 1: Payday Lender Data, Welfare Variables and Control Variables

Summary statistics are presented for payday lenders, welfare data, and control variables at the zip code level from 1996-2005. Data on payday lending are from the State of California Department of Corporations, for 1996-2005. The count of *FDIC Banks* is obtained by collapsing addresses from the FDIC database to zip codes for 1996-2005. *Population* and number of *Owned Housing Units* are from the U.S. Bureau of the Census for the 1990 Census or the 1997 Update, depending on the year in question. *Housing Prices* and *Foreclosures* count are from the California Association of Realtors from 1996-2002. Community *Deaths* and *Births* are from the California Center for Health Statistics for 1996-2004. *Drug & Alcohol Treatment* counts are from the California Department of Alcohol and Drug Data Programs 1996-2003. When viewing the welfare variables as rates, note that *Foreclosures* is normalized by housing whereas other welfare variables are normalized by population.

	Mean	*Minimum*	*Median*	*Maximum*	*St. Dev.*
Payday Data & Controls					
Payday Lenders	2.2	0	1	59	4.0
FDIC Banks	5.6	0	4	43	5.21
Population	21,088	0	16,424	113,697	20,063
Owned Housing Units	3,708	0	2,734	19,314	3,630
Housing Prices ($)	224,580	0	185,227	2,560,762	163,355
Welfare Variables					
Foreclosures	10.2	0	4	300	17.4
Deaths	210.7	5	199	1,047	129.5
Drug & Alcohol Treatments	105.7	1	58	2,711	148.2
Births	509.3	5	403	3,652	416.3
Welfare Variables as Rates					
Foreclosures per Owned Housing (1,000s)	2.19	0	0.93	129.9	4.63
Death per Population (1,000s)	7.49	0.12	7.11	36.7	3.05
Drug & Alcohol per Population (1,000s)	4.24	0.03	2.95	138.5	5.75
Birth per Population (1,000s)	16.41	0.09	15.22	84.4	7.46

Table 2: Natural Disaster Data

Summary statistics for Natural Disasters are taken from the University of South Carolina's Sheldus Hazard database, a collection of data identifying the location, type and magnitude of natural disasters from 1996-2005. Disasters below the Katrina thresholds of property damage across all Gulf Coast States are removed. For the six types of disasters, columns 1 and 2 represent the mean and median of property damage and columns 3 and 4 represent the mean and median of damage per capita. The Sheldus database measures disasters at a county level. Column 5 presents the count of county line items 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 commented information in the database, which often provides cities affected. Column 6 presents the total communities affected as measured by the total number of zip codes.

	Mean Property Damage	*Median Property Damage*	*Mean Damage per Capita*	*Median Damage per Capita*	*Count of Disasters*	*Communities Affected*
Flooding	37,360,145	36,670,000	1,602	346	25	478
Wildfire	629,043,804	1,000,000,000	4,162	966	42	195
Storm/Hail	24,125,000	20,000,000	629	307	10	72
Landslide	231,253,469	55,000,000	4,330	184	2	49
Earthquake	166,833,333	175,000,000	7,168	7,250	9	6
Tornado	150,000	150,000	949	949	1	1
All	192,997,180	36,670,000	2,345	461	89	801

Table 3: Socioeconomic Variables for Characterizing Financial Constraints

Panel A: Survey of Consumer Finance – Individual Characteristics for 1998

Panel A presents summary statistics of individual socioeconomic characteristics for respondents in the Survey of Consumer Finance for 1998.

Variable	*Description*	*Mean*	*Min*	*Median*	*Max*
Income	Household total income (in $1,000s)	430.4	0	49.4	176,892
Unemployed	Indicator for unemployment	0.036	0	0	1
Age	Respondent age	49.8	17	49	95
Education	Education years	13.7	1	14	17
Homeowner	Indicator for owning home	0.680	0	1	1
Shelter	Monthly housing cost = rent + mortgage	808.6	0	428.0	38,000
Vehicles	Number of vehicles in household	1.7	0	2	10
Female	Indicator for female	0.219	0	0	1
Nonwhite	Indicator for nonwhite race	0.187	0	0	1
PeopleHome	Persons in household	2.649	1	2	11
Married	Indicator for married	0.597	0	1	1
Separated	Indicator for separated	0.031	0	0	1
Divorced	Indicator for divorced	0.108	0	0	1
Widowed	Indicator for widowed	0.078	0	0	1
NeverMarried	Indicator for never married	0.130	0	0	1
AtLimit	Indicator for being within $1,000 of limit on credit card, if have credit card debt	0.090	0	0	1
Reject	Indicator for being rejected for credit card or credit increase within last 5 years	0.185	0	0	1

Panel B: Census – Zip Code Community Characteristics

Panel A presents U.S. Bureau of the Census data for the percentage of community residents (or households, depending on the variable) that are characterized by the variable. For example, the first line is interpreted as 21.5% percent of the mean community have an income less than $15,000. The minimum and maximum values of 0 and 1are observed for a few small population zip code areas.

	Percentage of Households or Individuals in Community with Characteristic			
Variable	*Description*	*Mean*	*Median*	*St. Dev*
Inc0015	$ 0 ≤ Household income < $ 15,000	0.215	0.188	0.134
Inc1530	$ 15,000 ≤ Household income < $ 30,000	0.162	0.161	0.078
Inc3045	$ 30,000 ≤ Household income < $ 45,000	0.274	0.278	0.088
Inc4560	$ 45,000 ≤ Household income < $ 60,000	0.132	0.134	0.069
Inc6075	$ 60,000 ≤ Household income < $ 75,000	0.082	0.082	0.050
Inc75100	$ 75,000 ≤ Household income < $100,000	0.066	0.055	0.050
Inc100125	$100,000 ≤ Household income < $125,000	0.031	0.019	0.038
Inc125150	$125,000 ≤ Household income < $150,000	0.013	0.006	0.017
Income150>	$150,000 ≤ Household income	0.026	0.010	0.048
Unemployed	Unemployed Persons	0.082	0.064	0.085
Age1217	12 ≤ Persons' Age ≤ 17	0.093	0.091	0.043
Age1824	18 ≤ Persons' Age ≤ 24	0.122	0.112	0.086
Age2534	25 ≤ Persons' Age ≤ 34	0.218	0.221	0.076
Age3544	35 ≤ Persons' Age ≤ 44	0.195	0.189	0.060
Age4554	45 ≤ Persons' Age ≤ 54	0.127	0.122	0.052
Age5564	55 ≤ Persons' Age ≤ 64	0.101	0.096	0.050
Age6574	65 ≤ Persons' Age ≤ 74	0.089	0.079	0.062
Age75>	75 ≤ Persons' Age	0.056	0.048	0.047
Educ_08	Educated 0 – 8 years	0.110	0.070	0.117
Educ_912	Educated 9 – 12 years, no degree	0.134	0.126	0.081
Educ_HS	High School Graduate	0.236	0.232	0.092
Educ_1316	Attended Some College	0.225	0.231	0.075
Educ_Assoc	Associate Degree	0.075	0.077	0.034
Educ_Bach	Bachelors Degree	0.142	0.127	0.089
Educ_Grad	Graduate Degree	0.077	0.056	0.078
Homeowner	Homeowning Households	0.204	0.217	0.091
Shltr000300	$ 0 ≤ Shelter Costs < $ 300	0.279	0.219	0.199
Shltr300500	$ 300 ≤ Shelter Costs < $ 500	0.173	0.134	0.130
Shltr500750	$ 500 ≤ Shelter Costs < $ 750	0.185	0.174	0.116
Shltr7501000	$ 750 ≤ Shelter Costs < $1,000	0.129	0.131	0.082
Shltr1000>	$1,000 ≤ Shelter Costs	0.234	0.195	0.199
Veh0	Household vehicles = 0	0.078	0.055	0.095
Veh1	Household vehicles = 1	0.319	0.321	0.115
Veh2	Household vehicles = 2	0.382	0.392	0.109
Veh3>	Household vehicles ≥ 3	0.221	0.212	0.113
Female	Female Persons	0.470	0.499	0.112
Nonwhite	Non-white Persons	0.158	0.111	0.152
PPH1	Person per Household = 1	0.234	0.220	0.108
PPH2	Person per Household = 2	0.318	0.318	0.090
PPH3_5	3 ≤ Person per Household ≤ 5	0.390	0.400	0.113
PPH6>	Person per Household ≥ 6	0.058	0.040	0.057
Married	Married Persons	0.220	0.220	0.065
Separated	Separated Persons	0.009	0.007	0.010
Divorced	Divorced Persons	0.034	0.029	0.037
Widowed	Widowed Persons	0.009	0.007	0.010
NeverMarried	Never Married Persons	0.126	0.114	0.074
Total Zip Code Observations: 1,762				

Table 4 – Estimates of Probability of Being Credit Constrained

Presented are the results of the logistic estimation of the probability of being financially constrained from Survey of Consumer Finances data. The dependent variable is *AtLimit* in Panel A and *Reject* in Panel B. *AtLimit* is defined as whether respondent is within $1,000 of credit card limit. *Reject* is defined as whether respondent has been turned down for credit. Results for 1995 and 1998 are presented across the columns. The reported coefficients should be compared to a highly educated single senior male living alone with income greater than $150,000 who does not own his home, but spends more than $1,000 per month on housing and owns at least three vehicles. *, **, *** indicate significance at the 10%, 5%, and 1% confidence interval.

Panel A: Dependent Variable: AtLimit = whether respondent is within $1,000 of credit card limit

	1995 SCF Estimation		*1998 SCF Estimation*	
Variable	*Coefficient*	*Standard Error*	*Coefficient*	*Standard Error*
Inc0015	2.529***	0.235	2.002***	0.235
Inc1530	2.801***	0.218	2.348***	0.218
Inc3045	2.876***	0.205	2.371***	0.205
Inc4560	2.580***	0.219	2.137***	0.219
Inc6075	2.110***	0.239	2.015***	0.239
Inc75100	1.662***	0.236	1.689***	0.236
Inc100125	1.460**	0.376	1.744***	0.376
Inc125150	1.386	0.661	0.959	0.661
Unemployed	-0.706**	0.412	-0.174	0.412
Age1824	1.884***	0.386	1.946***	0.386
Age2534	1.806***	0.386	1.820***	0.386
Age3544	1.575***	0.385	1.434***	0.385
Age4554	1.509***	0.389	1.545***	0.389
Age5564	1.367***	0.407	1.233***	0.407
Age6574	0.705*	0.321	0.802*	0.321
Educ_08	1.251***	0.210	-0.100	0.210
Educ_912	0.635**	0.156	-0.115	0.156
Educ_HS	0.897***	0.157	0.024	0.157
Educ_1316	0.941***	0.221	-0.673***	0.221
Educ_Assoc	0.558*	0.163	0.423	0.163
Educ_Bach	0.130	0.110	0.455*	0.110
Homeowner	-0.053	0.154	0.087	0.154
Shltr000300	-0.404*	0.169	-0.020	0.169
Shltr300500	-0.119	0.170	0.196	0.170
Shltr500750	-0.221	0.177	0.218	0.177
Shltr7501000	-0.452*	0.147	0.188	0.147
Veh0	-0.452*	0.161	-0.225	0.161
Veh1	-0.017	0.182	-0.209	0.182
Veh2	0.070	0.247	-0.141	0.247
Female	0.083	0.226	0.242	0.226
Nonwhite	0.580***	0.109	0.337**	0.109
PPH2	0.245	0.131	0.093	0.131
PPH3_5	0.58***	0.132	0.105	0.132
PPH6>	0.506	0.354	0.357	0.354
Married	-0.063	0.140	0.052	0.140
Separated	-0.724*	0.231	-0.122	0.231
Divorced	0.235	0.165	-0.140	0.165
Widowed	0.417	0.260	-0.236	0.260
Constant	-6.500***	0.539	-5.678***	0.539
R-Square	0.147		0.106	
Observations	4299		4305	

Panel B: Dependent Variable: Reject = whether respondent has been turned down for credit

	1995 SCF Estimation		*1998 SCF Estimation*	
Variable	*Coefficient*	*Standard Error*	*Coefficient*	*Standard Error*
Inc0015	1.094***	0.235	0.584**	0.235
Inc1530	1.173***	0.218	0.996***	0.218
Inc3045	1.274***	0.205	1.203***	0.205
Inc4560	1.072***	0.219	0.952***	0.219
Inc6075	0.532**	0.239	0.254	0.239
Inc75100	0.672***	0.236	0.258	0.236
Inc100125	-0.169	0.376	0.336	0.376
Inc125150	-0.838	0.661	0.429	0.661
Unemployed	0.000	0.412	-0.598***	0.412
Age1824	2.409***	0.386	4.139***	0.386
Age2534	2.408***	0.386	4.072***	0.386
Age3544	1.968***	0.385	3.597***	0.385
Age4554	1.829***	0.389	3.297***	0.389
Age5564	1.494***	0.407	3.076***	0.407
Age6574	0.911**	0.321	2.235***	0.321
Educ_08	-0.734**	0.210	-0.970***	0.210
Educ_912	-0.075	0.156	0.019	0.156
Educ_HS	-0.034	0.157	-0.346***	0.157
Educ_1316	0.307*	0.221	-0.490***	0.221
Educ_Assoc	-0.034	0.163	0.193	0.163
Educ_Bach	-0.363**	0.110	0.075	0.110
Homeowner	-0.161	0.154	-0.112	0.154
Shltr000300	-0.769***	0.169	-0.457***	0.169
Shltr300500	-0.569***	0.170	-0.178	0.170
Shltr500750	-0.440***	0.177	0.047	0.177
Shltr7501000	-0.235	0.147	-0.061	0.147
Veh0	-0.231	0.161	-0.304*	0.161
Veh1	0.187	0.182	-0.111	0.182
Veh2	0.020	0.247	-0.035	0.247
Female	-0.474***	0.226	-0.221*	0.226
Nonwhite	0.369***	0.109	0.062	0.109
PPH2	0.169	0.131	0.234*	0.131
PPH3_5	0.260**	0.132	0.292**	0.132
PPH6>	1.301***	0.354	1.025***	0.354
Married	-0.383***	0.140	-0.314**	0.140
Separated	0.386*	0.231	0.200	0.231
Divorced	0.049	0.165	0.452***	0.165
Widowed	0.020	0.260	-0.056	0.260
Constant	-2.837***	0.539	-4.445***	0.539
R-Square	0.137		0.147	
Observations	4,299		4,305	

Table 5: Welfare Variables Statistics by Match

The table presents T-tests for differences in means. T-tests are performed for all the normalized welfare variables according to whether or not they have payday lenders (in the first set of columns) and whether or not they will be hit by a disaster (in the second set of columns). All means are taken for matched samples of disaster and non-disaster communities prior to when the disaster occurs. The welfare variables are defined as in Table 1. *, **, *** indicate significance at the 10%, 5%, and 1% confidence interval.

Numbers: means	*Payday Lenders*		*Test for Diffferences*	*Disaster Community*		*Tests for Diffferences*
	No	Yes		No	Yes	
Foreclosures per Housing (1,000s)	1.81	1.85		2.32	0.86	***
Deaths per Population (1,000s)	7.94	7.66		8.13	8.07	
Drug & Alcohol per Pop (1,000s)	5.08	4.63		4.16	5.76	***
Births per Population (1,000s)	16.76	16.60		16.66	16.63	

Table 6: Impact of Payday Lenders on Foreclosures

The dependent is *Foreclosures* from the California Association of Realtors from 1996-2002. In Columns 1, 2, 5 and 6, the sample is the matching of zip code communities along the propensity of residents to be within $1000 of their credit card limit. The observation level is quarterly, and the measurement period is defined as 6 quarters before and after the disaster, starting at the third quarter following the disaster. In columns 3, 4, 7 and 8, the sample is the match according to the propensity of residents to be rejected from credit cards. In columns 1-4, the Poisson Triple Interaction Model is estimated in which the pre and post periods are collapsed to a single set of units. Columns 5-8 use a Linear Triple Difference estimation with Newey West Standard Errors to control for serial correlation. In the Linear Triple Difference columns, the dependent variables, *Banks* and *Houses* are all expressed in natural logarithms. All of the independent variables except *Houses*, *House Prices* and *Banks* are dummy variables. *Post* indicates a post-disaster period (or a false post-disaster period for non-disaster matches). *Disaster* indicates whether a community is hit by a disaster. *Payday* indicates whether a payday lender exists in the zip code. Pseudo R-Squares are reported for columns 1-4, and Adjusted R-Squares for 5-8; thus, they are not strictly comparable. *, **, *** indicate significance at the 10%, 5%, and 1% confidence interval. Standard errors are in parentheses.

Panel A: Estimation Results

	Poisson Triple Interaction Model *Dependent Variable: Foreclosures*				***Linear Triple Difference Model*** *Dependent Variable: Ln(Foreclosures)*			
	At Limit		Reject		At Limit		Reject	
	1	2	3	4	5	6	7	8
Post	-1.209***	-1.254***	-1.421***	-1.429***	-1.026***	-1.479***	-0.948***	-1.009***
	(0.071)	(0.123)	(0.084)	(0.109)	(0.191)	(0.361)	(0.173)	(0.251)
Disaster	-1.230***	-1.764***	-1.073***	-1.659***	-1.441***	-1.669***	-1.389***	-1.781***
	(0.071)	(0.158)	(0.072)	(0.135)	(0.180)	(0.342)	(0.175)	(0.277)
Post*Disaster	0.209	0.738***	0.471***	0.953***	-0.046	0.645	-0.257	-0.053
	(0.147)	(0.284)	(0.152)	(0.269)	(0.258)	(0.468)	(0.251)	(0.402)
Payday		-0.140*		-0.611***		-0.134		-0.623**
		(0.074)		(0.080)		(0.313)		(0.251)
Payday*Post		0.074		-0.080		0.688		-0.017
		(0.151)		(0.167)		(0.425)		(0.342)
Payday *Disaster		0.707***		0.935***		0.330		0.622*
		(0.178)		(0.163)		(0.408)		(0.360)
Payday*Post *Disaster		-0.701**		-0.595*		-1.048*		-0.224
		(0.333)		(0.334)		(0.561)		(0.517)
Houses	0.148***	0.140***	0.117***	0.127***	0.465***	0.454***	0.711***	0.746***
	(0.008)	(0.009)	(0.010)	(0.011)	(0.083)	(0.084)	(0.084)	(0.083)
House Prices	-0.245***	-0.241***	0.053	0.102**	-0.222***	-0.217***	-0.205**	-0.186**
	(0.036)	(0.036)	(0.045)	(0.047)	(0.078)	(0.078)	(0.082)	(0.084)
Banks	-0.053***	-0.050***	-0.047***	-0.030***	0.041	0.040	-0.110***	-0.074*
	(0.006)	(0.006)	(0.007)	(0.007)	(0.035)	(0.036)	(0.039)	(0.039)
Constant	2.526***	2.659***	2.417***	2.567***	-2.417***	-2.225***	-4.266***	-4.222***
	(0.067)	(0.084)	(0.065)	(0.067)	(0.671)	(0.762)	(0.682)	(0.711)
R-Square (Pseudo 1-4) (Adj. 5-8)	0.299	0.304	0.241	0.269	0.239	0.241	0.248	0.258
Obs.	169	169	166	166	1056	1056	1037	1037
Handling of Serial Corr.	Collapsed to a single pre and a single post period				Newey-West standard errors			

Panel B: Foreclosure Matrix of Predicted Counts: Triple Differencing Interpretation

The matrix of predicted counts is obtained by using the coefficient from Panel A, column 2 with the mean values of houses, house prices and banks. The differencing reading down the table take changes over time (post – pre), over payday availability (available (A) – unavailable (U)), and over being in a disaster area (D) or not (N).

Matrix of Predicted Counts From Panel A		No Disaster		Disaster	
		Pre	Post	Pre	Post
	No Payday	20.1	5.7	3.4	2.1
	Payday	17.5	5.4	6.1	1.9
(i) First Differencing Over Time: $\Delta\omega$ (Post – Pre)					
		No Disaster		Disaster	
	No Payday		-14.4		-1.4
	Payday		-12.1		-4.1
(ii) Difference-in-Differences over Time & Payday Availability: $\Delta\omega_A - \Delta\omega_U$					
		No Disaster		Disaster	
			2.3		-2.8
(iii) Tripe Differencing on Disasters: $(\Delta\omega_{DA} - \Delta\omega_{DU}) - (\Delta\omega_{NA} - \Delta\omega_{NU})$					
					-5.0

Table 7: Impact of Payday Lenders on Death

The dependent is Community *Deaths* are from the California Center for Health Statistics for 1996-2004. In Columns 1, 2, 5 and 6, the sample is the matching of zip code communities along the propensity of residents to be within $1000 of their credit card limit. In columns 3, 4, 7 and 8, the sample is the match according to the propensity of residents to be rejected from credit cards. In columns 1-4, the Poisson Triple Interaction Model is estimated in which the pre and post periods are collapsed to a single set of units. Columns 5-8 use a Linear Triple Difference estimation with Newey West Standard Errors to control for serial correlation. In the Linear Triple Difference columns, the dependent variables, *Banks* and *Houses* are all expressed in natural logarithms. Pseudo R-Squares are reported for columns 1-4, and Adjusted R-Squares for 5-8; thus, they are not strictly comparable. All of the independent variables except *Houses*, *House Prices* and *Banks* are dummy variables. *Post* indicates a post-disaster period (or a false post-disaster period for non-disaster matches). *Disaster* indicates whether a community is hit by a disaster. *Payday* indicates whether a payday lender exists in the zip code. The observation level is yearly, with the measuring period being defined as two years before and after the disaster. *, **, *** indicate significance at the 10%, 5%, and 1% confidence interval. Standard errors are in parentheses.

	Poisson Triple Interaction Model *Dependent Variable: Foreclosures*				***Linear Triple Difference Model*** *Dependent Variable: Ln(Foreclosures)*			
	At Limit		Reject		At Limit		Reject	
	1	2	3	4	5	6	7	8
Post	0.238***	-0.032	-0.011	-0.158***	-0.009	-0.007	-0.012	0.067
	(0.018)	(0.033)	(0.019)	(0.051)	(0.026)	(0.050)	(0.034)	(0.081)
Disaster	0.240***	-0.098***	-0.100***	-0.240***	0.058	0.016	0.014	0.068
	(0.018)	(0.033)	(0.019)	(0.050)	(0.038)	(0.065)	(0.039)	(0.075)
Post*Disaster	-0.264***	-0.033	-0.019	0.114**	-0.021	-0.021	-0.049	-0.156
	(0.020)	(0.039)	(0.022)	(0.054)	(0.041)	(0.095)	(0.045)	(0.108)
Payday		-0.351***		-0.081		-0.092		0.047
		(0.038)		(0.053)		(0.057)		(0.074)
Payday*Post		0.355***		0.176***		-0.006		-0.106
		(0.040)		(0.055)		(0.061)		(0.096)
Payday *Disaster		0.453***		0.170***		0.057		-0.072
		(0.040)		(0.054)		(0.076)		(0.088)
Payday*Post *Disaster		-0.300***		-0.157***		0.004		0.148
		(0.046)		(0.059)		(0.109)		(0.128)
Population	0.021***	0.022***	0.021***	0.021***	0.922***	0.926***	0.872***	0.873***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.031)	(0.031)	(0.035)	(0.035)
House Prices	0.045***	0.043***	-0.013**	-0.016***	0.026	0.027	0.004	0.005
	(0.004)	(0.004)	(0.005)	(0.005)	(0.024)	(0.024)	(0.022)	(0.021)
Banks	0.019***	0.018***	0.028***	0.025***	0.080***	0.091***	0.144***	0.147***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.020)	(0.020)	(0.024)	(0.025)
Constant	4.364***	4.632***	4.569***	4.661***	2.088***	2.127***	2.180***	2.139***
	(0.020)	(0.032)	(0.021)	(0.050)	(0.106)	(0.112)	(0.117)	(0.122)
R-Square (Pseudo 1-4) (Adj. 5-8)	0.534	0.5407	0.543	0.546	0.791	0.792	0.777	0.777
Obs.	420	420	367	367	1154	1154	1037	1037
Handling of Serial Corr.	Collapsed to a single pre and a single post period				Newey-West standard errors			

Table 8: Impact of Payday Lenders on Drug & Alcohol Abuse

The dependent is *Drug & Alcohol Treatment* counts are from the California Department of Alcohol and Drug Data Programs 1996-2003. In Columns 1, 2, 5 and 6, the sample is the matching of zip code communities along the propensity of residents to be within $1000 of their credit card limit. In columns 3, 4, 7 and 8, the sample is the match according to the propensity of residents to be rejected from credit cards. In columns 1-4, the Poisson Triple Interaction Model is estimated in which the pre and post periods are collapsed to a single set of units. Columns 5-8 use a Linear Triple Difference estimation with Newey West Standard Errors to control for serial correlation. In the Linear Triple Difference columns, the dependent variables, *Banks* and *Houses* are all expressed in natural logarithms. Pseudo R-Squares are reported for columns 1-4, and Adjusted R-Squares for 5-8; thus, they are not strictly comparable. All of the independent variables except *Houses*, *House Prices* and *Banks* are dummy variables. *Post* indicates a post-disaster period (or a false post-disaster period for non-disaster matches). *Disaster* indicates whether a community is hit by a disaster. *Payday* indicates whether a payday lender exists in the zip code. The observation level is yearly, with the measuring period being defined as two years before and after the disaster. *, **, *** indicate significance at the 10%, 5%, and 1% confidence interval. Standard errors are in parentheses.

	Poisson Triple Interaction Model *Dependent Variable: Foreclosures*				***Linear Triple Difference Model*** *Dependent Variable: Ln(Foreclosures)*			
	At Limit		Reject		At Limit		Reject	
	1	2	3	4	5	6	7	8
Post	0.119***	0.105***	0.025**	0.129***	0.092	-0.111	0.033	-0.028
	(0.010)	(0.019)	(0.012)	(0.026)	(0.056)	(0.122)	(0.067)	(0.116)
Disaster	0.432***	0.178***	0.528***	0.355***	0.316***	0.070	0.399***	0.185
	(0.011)	(0.025)	(0.012)	(0.028)	(0.103)	(0.181)	(0.105)	(0.176)
Post*Disaster	-0.295***	-0.101***	-0.201***	-0.136***	-0.199*	0.107	-0.167	-0.029
	(0.015)	(0.032)	(0.017)	(0.036)	(0.103)	(0.196)	(0.113)	(0.196)
Payday		0.082***		0.094***		-0.268**		-0.199*
		(0.017)		(0.022)		(0.119)		(0.120)
Payday*Post		0.023		-0.128***		0.264*		0.060
		(0.022)		(0.029)		(0.147)		(0.149)
Payday *Disaster		0.297***		0.209***		0.331		0.290
		(0.027)		(0.031)		(0.204)		(0.208)
Payday*Post *Disaster		-0.216***		-0.065		-0.435*		-0.178
		(0.036)		(0.041)		(0.249)		(0.258)
Population	0.034***	0.034***	0.039***	0.038***	1.017***	1.024***	0.909***	0.907***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.074)	(0.075)	(0.074)	(0.077)
House Prices	0.036***	0.034***	0.089***	0.090***	0.003	0.003	-0.011	-0.010
	(0.006)	(0.006)	(0.006)	(0.006)	(0.049)	(0.049)	(0.046)	(0.046)
Banks	-0.026***	-0.030***	-0.024***	-0.026***	-0.035	-0.022	0.037	0.055
	(0.001)	(0.001)	(0.001)	(0.001)	(0.054)	(0.056)	(0.060)	(0.062)
Constant	3.827***	3.822***	3.701***	3.679***	1.101***	1.262***	1.216***	1.348***
	(0.015)	(0.019)	(0.016)	(0.023)	(0.222)	(0.239)	(0.202)	(0.215)
R-Square (Pseudo 1-4) (Adj. 5-8)	0.327	0.334	0.337	0.341	0.459	0.461	0.423	0.423
Obs.	585	585	507	507	1166	1166	1003	1003
Handling of Serial Corr.	Collapsed to a single pre and a single post period				Newey-West standard errors			

Table 9: Impact of Payday Lenders on Birth

The dependent is Community *Births* are from the California Center for Health Statistics for 1996-2004. In Columns 1, 2, 5 and 6, the sample is the matching of zip code communities along the propensity of residents to be within $1000 of their credit card limit. In columns 3, 4, 7 and 8, the sample is the match according to the propensity of residents to be rejected from credit cards. In columns 1-4, the Poisson Triple Interaction Model is estimated in which the pre and post periods are collapsed to a single set of units. Columns 5-8 use a Linear Triple Difference estimation with Newey West Standard Errors to control for serial correlation. In the Linear Triple Difference columns, the dependent variables, *Banks* and *Houses* are all expressed in natural logarithms. Pseudo R-Squares are reported for columns 1-4, and Adjusted R-Squares for 5-8; thus, they are not strictly comparable. All of the independent variables except *Houses*, *House Prices* and *Banks* are dummy variables. *Post* indicates a post-disaster period (or a false post-disaster period for non-disaster matches). *Disaster* indicates whether a community is hit by a disaster. *Payday* indicates whether a payday lender exists in the zip code. The observation level is yearly, with the measuring period being defined as two years before and after the disaster. The post period is lagged forward one year to allow for gestation time. *, **, *** indicate significance at the 10%, 5%, and 1% confidence interval. Standard errors are in parentheses.

	Poisson Triple Interaction Model *Dependent Variable: Foreclosures*				***Linear Triple Difference Model*** *Dependent Variable: Ln(Foreclosures)*			
	At Limit		Reject		At Limit		Reject	
	1	2	3	4	5	6	7	8
Post	0.099***	0.340***	0.017	0.239***	-0.080***	-0.144**	-0.039	0.043
	(0.019)	(0.039)	(0.015)	(0.044)	(0.029)	(0.070)	(0.047)	(0.115)
Disaster	0.251***	0.305***	0.125***	0.257***	0.063	0.088	0.157***	0.198**
	(0.016)	(0.035)	(0.015)	(0.043)	(0.045)	(0.085)	(0.052)	(0.089)
Post*Disaster	-0.116***	-0.480***	0.007	-0.279***	0.074	0.061	0.010	-0.211
	(0.020)	(0.041)	(0.016)	(0.046)	(0.049)	(0.126)	(0.060)	(0.151)
Payday		0.126***		0.233***		0.041		0.052
		(0.039)		(0.046)		(0.072)		(0.085)
Payday*Post		-0.342***		-0.265***		0.091		-0.111
		(0.045)		(0.047)		(0.079)		(0.128)
Payday *Disaster		-0.059		-0.134***		-0.035		-0.049
		(0.039)		(0.046)		(0.097)		(0.106)
Payday*Post *Disaster		0.506***		0.352***		0.015		0.306*
		(0.047)		(0.049)		(0.143)		(0.172)
Population	0.024***	0.024***	0.026***	0.027***	1.012***	1.006***	1.035***	1.029***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.041)	(0.041)	(0.055)	(0.055)
House Prices	-0.010***	-0.013***	-0.009***	-0.011***	-0.139***	-0.152***	-0.152***	-0.155***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.024)	(0.024)	(0.025)	(0.026)
Banks	-0.034***	-0.026***	-0.046***	-0.042***	0.012	0.013	0.012	0.019
	(0.004)	(0.004)	(0.004)	(0.004)	(0.024)	(0.024)	(0.030)	(0.031)
Constant	5.202***	5.134***	5.181***	5.001***	2.939***	2.954***	2.787***	2.780***
	(0.017)	(0.035)	(0.016)	(0.043)	(0.147)	(0.155)	(0.200)	(0.193)
R-Square (Pseudo 1-4) (Adj. 5-8)	0.561	0.570	0.587	0.592	0.710	0.712	0.717	0.718
Obs.	259	259	364	364	1153	1153	1026	1026
Handling of Serial Corr.	Collapsed to a single pre and a single post period				Newey-West standard errors			

Table 10: Analysis with Banks

The table presents the β7 coefficient on the triple interaction variable HiBankDensity from the full specification:

$$Welfare = \beta_0 + \beta_1\, Post + \beta_2\, Disaster + \beta_3\, Post{*}Disaster + \beta_4\, HiBankDensity + \beta5\, HiBankDensity{*}Post + \beta_6\, HiBankDensity{*}Disaster + \beta_7\, HiBankDensity{*}Post{*}Disaster + \varepsilon$$

The rows represent the different welfare variable, with each welfare variable receiving two rows, one for each matching sample of *AtLimit* and *Reject*. Column 1 presents the results for the Poisson model, and column 3 for the Linear model. Columns 2 and 4 show the significance levels for the equivalent regressions in Tables 6-9 using *Payday* rather then *HiBankDensity*. *, **, *** denote significance at the 10%, 5% and 1% levels. Standard errors are in parentheses.

	Poisson Triple Interaction		***Linear Triple Difference***	
Welfare Variable	HiBankDensity* Post*Disaster	Significance of Payday*Post* Disaster	HiBankDensity* Post*Disaster	Significance of Payday*Post* Disaster
Foreclosure (AtLimit)	-0.219 (0.373)	Negative **	0.636 (0.513)	Negative *
Foreclosure (Reject)	-0.854** (0.382)	Negative *	0.088 (0.504)	
Death (AtLimit)	-0.583*** (0.042)	Negative ***	-0.025 (0.078)	
Death (Reject)	-0.030 (0.034)	Negative ***	0.031 (0.093)	
Drug (AtLimit)	0.059* (0.031)	Negative ***	-0.261 (0.215)	Negative *
Drug (Reject)	0.078** (0.034)		-0.352 (0.241)	
Birth (AtLimit)	0.020 (0.041)	Positive ***	-0.074 (0.103)	
Birth (Reject)	-0.083** (0.033)	Positive ***	-0.075 (0.123)	Positive *