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Effect of the Small Business Administration on Small Business Lending Markets and Activity

Author
Morgulis, Yana

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Effect of the Small Business Administration on Small Business Lending Markets and Activity

A dissertation submitted in partial satisfaction of the requirements for the degree Doctor of Philosophy

in

Economics

by

Yana Morgulis

Committee in charge:

Professor Roger Gordon, Chair
Professor Thomas Baranga
Professor Julie Cullen
Professor Gordon Dahl
Professor Krislert Samphantharak

2015

The Dissertation of Yana Morgulis is approved, and it is acceptable in quality and form for publication on microfilm and electronically:

Chair

University of California, San Diego

2015

Dedication

This dissertation is dedicated to members of 9226 who made sure I take breaks and laugh uncontrollably over dinner and with whom there were countless dinner parties to which I looked forward; to my life partner who talked me through the cloudy days and celebrated with me on the sunny ones; to my mother who encouraged me to embark on every adventure that came my way, to my grandmother who made sure that neither the body nor the spirit was ever lacking in sustenance, and to my grandfather who shared his love for math with a silly, little girl.

Table of Contents

List of Figures

List of Tables

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Chapter 2, in part is currently being prepared for submission for publication of the material. Brown, David; Earle, John. The dissertation author was the primary investigator and author of this material.

Vita

2007	Bachelor of Arts, University of Chicago
2008-2009	Fulbright Research Fellow to Cambodia
2011	Master of Arts, University of California, San Diego
2015	Doctor of Philosophy, University of California, San Diego

Abstract of the Dissertation

Effect of the Small Business Administration on Small Business Lending Markets and Activity

by

Yana Morgulis

Doctor of Philosophy in Economics

University of California, San Diego, 2015

Professor Roger Gordon, Chair

In the first chapter, I take advantage of a regime change that introduced competition into a market initially defined by sanctioned monopolies to study the impact of changing incentives on firm behavior. The market is the 504 Loan Program administered by the Small Business Administration and defined by fixed prices which allow the analysis to study changes on other margins. The study focuses on changes in market concentration, loan volume, risk profile, and firm rents. Findings are consistent with a theoretical model of relaxing geographic boundaries and increasing competition.

Using a linked database based on a list of all Small Business Administration (SBA) loans in 1992 to 2011 and annual information on all U.S. employers from 1976 to 2012, the second chapter applies detailed matching and regression methods to estimate the variation in SBA loan effects on job creation across firm age and size groups. The firm-level proportional impact of loan receipt is estimated to fall with pre-loan firm size and age, and is largest for start-ups and very young and very small firms. The number of jobs created per million dollars of loans is also estimated to be highest for start-ups but otherwise generally increases with size and age. The estimated survival impact of loan amount is larger for smaller and younger firms.

The final chapter takes advantage of a unique investigation by a local utility company within a single metropolitan area to relate cultural norms of residents to identified instances of fraud. We base the cultural norms index on the country of birth and the associated country corruption index of residents and generate counts of location-based utilities fraud based on the data from the investigation. The analysis results in a large and persistent positive correlation between the cultural norms index and fraud variables suggesting that there exists a strong relationship between cultural norms and the likelihood of engaging in fraud.

Chapter 1: From Monopoly to Competition: the Impact of Market Structure on Lenders and Borrowers in Local Lending Markets.

Abstract

I take advantage of a regime change that introduced competition into a market initially defined by sanctioned monopolies to study the impact of changing incentives on firm behavior. The market is the 504 Loan Program administered by the Small Business Administration and defined by fixed prices which allow the analysis to study changes on other margins. The study focuses on changes in market concentration, loan volume, risk profile, and firm rents. Findings are consistent with a theoretical model of relaxing geographic boundaries and increasing competition.

1.1 Introduction

This paper studies and isolates the impact of competition in the financial industry. There are few instances in which theoretical implications of competition at large can be tested with real data. In fact, most modern cases take place in developing countries where other factors such as length of deregulation or lax governmental oversight may play a large role in the data and resulting findings. Tests of theories of competition in the financial industry are even rarer. Competition defines the financial industry in developed countries limiting the availability of relevant case studies and, at the same time, increasing the importance of such studies in testing theoretical implications of competition-based drivers. The regime change studied in this paper traces the impact of opening an industry to competition and offers a model test case for theories of competition in the financial industry.

The 504 Loan Program administered by the Small Business Administration (SBA) provides the setting for this study. The program offers small businesses that are unable to obtain conventional loans subsidized credit to finance fixed asset purchases. Certified Development Companies (CDCs, loan processors or loan intermediaries) work in partnership with banks and the SBA to identify potential lenders, process the 504 loan applications for approval by the SBA, and funnel funds to approved firms. Prior to 2003, CDCs were granted monopoly rights to operate at a specified geographic level (often one or more counties) within a state. In 2003, fearing that operators were not able to meet local demand for 504 loans, the SBA expanded CDC domain to the entire state. This increase in the potential domain of each CDC provides the backdrop for this study of competition, risk, and credit constraints. An interesting characteristics of this market is that prices charged by CDCs are fixed by the SBA allowing the analysis to study changes on other margins.

A large part of borrowers' default risk in this market is borne by the tax payer, a key part of government's subsidy for the program. Placing ultimate responsibility for risk with the tax payer seems to differentiate this market from others. Yet, government actions taken during the Great Recession demonstrated that the tax payer also bears the ultimate burden of default for large financial institutions. Therefore, the market at hand can be seen as a model for the standard financial market in which financial institutions have been shown to bear a limited amount of downside from future risk.

I developed a model which incorporates limited risk borne by the CDC to study the impact of the regime change. The model considers the relationships of all players in the market including CDCs, SBA, banks and borrowers. I establish theoretical predictions for the market post-regime change. They include a positive relationship between processing institution counts and loan volume and a negative relationship between institution counts and loan quality as well as rents. As predicted, findings show that the greater the number of CDCs, the greater the impact on loan volume. Loan quality falls as predicted but only in the form of a composite loan credit score. In contrast, there is no effect on default rates, suggesting that the fall in borrower quality resulting from the regime change does not have an effect on the underlying borrower quality and has no long-term impact. In line with the prediction of increasing competition between CDCs, rents to CDCs decrease especially in areas with greater potential for competition between CDCs.

This paper contributes to and fills gaps in several strands of literature. First, the paper contributes to research on the role of competition in risk-taking within financial institutions. Boyd and De Nicolo (2005) use a model to show that the evidence on the impact of competition on bank risk taking is, in fact, mixed due to the existence of risk-incentive mechanisms that work to counteract each other. Nevertheless, the majority of data-driven research has shown that competition increases risk taking in lending by financial institutions (Dinc 2000). Unlike the majority of the empirical literature, this paper supports the first argument. This paper produces limited evidence of increased risk taking and no change on other long-term risk measures. This finding points to neither mechanism dominating the other within the Boyd and De Nicolo model. However, unlike previous work, this paper suggests that the effect on risk is limited and offers empirical insight into ways in which competition and risk may be related.

More broadly, the paper contributes to the literature on the topic of competition in the financial industry. The paper also adds to the broader cross-industry, deregulation literature. Winston (1993) offers a review article on the topic. Measuring causal impact of competition has been a challenge as causal measurement requires an experimental setting. This paper comes closer to a causal estimate by measuring the effect of a regime change. Past empirical literature has approached such measurement issues in two ways. One strategy studies the effect of gradual deregulation at the state level that took place between 1970s and 1990s, while the other uses cross-country comparisons. Jith and Strahan (1997) describe the history of banking geographic deregulation. Although state-based deregulation of the banking industry would seem to lead to greater competition in a quasi-experimental setting, in fact, it led to large-scale consolidation in the industry.

Further, empirical studies of this period traced the source of resulting changes largely to consolidation (see Calem (1994), Chong (1991), Jith and Strahan (1996) and others). The other strategy of measuring the impact of competition is based on cross-country studies, yet this strategy presents a challenge in drawing causal arguments due to the cross-country nature of the empirical analysis.

Next, this paper adds additional evidence to a large body of literature which looks at the relationship between credit constraints and growth. Jayaratne and Strahan (1996) find evidence of faster growth due to deregulation but relate it to more efficient use of capital by banks rather than an increase in lending volume to alleviate credit constraints. Much of the rest of the literature relates credit constraints and growth without the convenience of a deregulation to test the theory (see Beck et al. (2006), Levine (1997), Levine (2005), and Rajan et al. (1998) among others). By studying the effect of deregulation on lending volumes, this paper also adds evidence to the relationship between credit constraints and growth.

Finally, the paper contributes to the literature surrounding privatization of Fannie Mae and Freddy Mac. Although the debate to privatize Fannie Mae and Freddy Mac has been ongoing for the past 25 years (see Reiss (2010) for an expansive bibliography), researchers know little about the impact of various market structures when tax payers carry associated risk. This paper sheds light on the effect of potential market structures. Oesterle (2010) proposes a public utilities model for Fannie Mae and Freddy Mac. This type of highly regulated market structure is consistent with the market structure in this paper pre-regime change. Other proposals call for almost complete privatization with limited tax payer risk sharing which is more consistent with the paper's post-regime change context. The study's contribution to understand the impact of market structure provides insight into proposed changes to Fannie Mae and Freddy Mac.

The next section outlines the model driving the analysis in this study and Section 3 describes the applied setting. Section 4 sets forth the estimation strategy, Section 5 describes the data, Section 6 presents estimation results and Section 7 concludes.

1.2 Model

First, I show the existence of credit constraints and rationing in lending markets following Stiglitz and Weiss (1981). This model is based on the assumption of perfect information on expected project returns, but imperfect information regarding project riskiness. Next, I apply the model to the SBA lending market. I relax the assumption of perfect information and incorporate uncertainty. I assume that expected return is known but with uncertainty and show that it is in a loan processor's interest to investigate expected returns provided low enough research costs.

Following Stiglitz and Weiss (1981), let $G(\theta)$ be the cumulative distribution of projects by riskiness, θ, and $\rho_m(\theta, r)$ be the expected return to the bank of a loan of risk θ, interest rate r, and mean return m. The bank can separate out groups of borrowers by mean return, m, but does not observe loan riskiness, θ. The mean return to the bank which lends at interest rate r to firms with mean return, m, is then,

$$\bar{\rho}_m(\hat{r}) = \frac{\int_{\theta_m(\hat{r})}^{\infty} \rho_m(\theta, \hat{r}) dG(\theta)}{1 - G(\theta)}$$

Stiglitz and Weiss show that for a given interest rate r, there is a critical value $\hat{\theta}$ such that a firm[1] borrows from the bank if and only if $\theta > \hat{\theta}$. Further, as the interest rate increases, the critical value of $\hat{\theta}$, below which individuals do not apply for loans, increases.

The interest rate at which a bank maximizes its returns from firms with mean return m is determined by the condition, $\frac{d\overline{\rho_m}}{d\hat{r}} = 0$. The bank lends to every group m as long as $\overline{\rho_m}(\hat{r}^*) - r_f \geq 0$ where $\hat{r}^*$ is the interest rate that maximizes bank returns and r_f is the risk-free interest rate or the cost of funds for the bank. Banks turn away groups m<m* for which the earlier condition does not hold for any value of $\hat{r}$. These borrowers are credit constrained.

Next, I introduce uncertainty into mean project return, m, and consider the SBA loan setting. I set up the uncertainty of project return. Then, I introduce the SBA and the CDC and the incentives each entity faces. The SBA determines a minimum mean return below which it also does not lend. A CDC is incentivized into a new set of behaviors based on project uncertainty and CDC's own costs. I consider the bank's role in SBA lending and confirm that it is always in the bank's interest to work with the SBA. Also for simplicity, I ignore loan riskiness in the exposition below.

The unobserved true return of a project, M, is composed of an expected return, m, and an idiosyncratic return, γ, drawn from a distribution with a common variance, Λ, and a standard deviation, λ. I assume that the covariance between expected and random returns is equal to zero or $corr(m, \gamma) = 0$. The borrowing firm will default if ex post

[1] There are several of assumptions of note. I assume equity financing is not available in order to focus on the debt financing decision in this model. In addition, since a portion of the loan is financed by the firm, itself, a condition exists which determines whether the firm continues to want to borrow. I focus on the firms for which this condition is non-binding. As a result, I assume that firms always want to borrow and invest.

value of the project is below the amount due on the loan or $m + \gamma < L(1 + r)$. I ignore variation in size of loan L across firms on the implicit assumption that this variation is uncorrelated with expected return, m.

The goal of the SBA program is to extend credit to firms just shut out of the conventional lending market (m<m*), while minimizing losses to its fund. However, the SBA cannot extend loans to all borrowers and limits its lending to a range of loans with expected value in the following range, $[m_{SBA}, m^*]$. The upper end of the range is determined by commercial lending as shown above. The lower end of the range is determined through an SBA maximization problem.

The SBA program faces a number of constraints. The first limitation that the SBA faces is that it is required to offer a single interest rate, r_{SBA}, to all firms to which it lends. The other limitation is that it is dependent on the size of the federal government subsidy, S, to cover its losses from lending. The last constraint is that the SBA takes second lien position on the full loan where the loan is composed of an SBA $(1 - f)$ and a bank (f) portion. The structure incentivizes banks to lend to typically credit constrained borrowers. The SBA maximizes its returns on loans (left-hand side of the equation below) with respect to the interest rate, r_{SBA}, subsidy level, S, and risk-free interest rate, r_f. The maximization problem determines the minimum mean project return (m_{SBAmin}) the SBA is able to finance. The break-even condition for the SBA is then,

$$\int_{m_{min}}^{m^*} (1 + r_{SBA})\, P_{\gamma > L(1 + f r_m + (1-f) r_{SBA}) - m} (1 - f) L dm$$

$$= (1 + r_f) \int_{m_{min}}^{m^*} (1 + f)\, L dm - S$$

where $P_{\gamma > L(1+fr_m+(1-f)r_{SBA})-m}$ is the probablity that idiosyncratic return is greater than the difference between loan payment due and mean return on the project.

A CDC processes loans on behalf of the SBA and submits loans to the SBA for approval. CDCs benefit from increasing the number of loans approved by the SBA while taking on costs of processing, $\pi = (a - c)L$ where L is the loan amount approved by the SBA, a is the percentage return on the loan to a CDC over time, and c is the cost of filing a loan application with the SBA on behalf of the borrower and conducting additional tests on the borrower's viability. These loans approved by the SBA must have expected returns within the SBA range, , $[m_{SBA}, m^*]$. In encountering a loan, a CDC can update the mean return for the loan by paying d. The updated mean return becomes m+p.

$$M = m + \gamma = m + p + e$$

where the error term, e, is uncorrelated with both m and p, while $var(\gamma) = var(p + e)$. When the SBA has a quality cut-off, m_{SBA}, the CDC will submit all applications from firms with $m > m_{SBAmin}$ without further examination. If $< m_{SBAmin}$, the CDC must judge whether it pays to conduct an examination costing d with the prospect of increasing the borrower's mean return to a level above the cut-off. The probability that this updated value for m is greater than m_{SBAmin} is equal to $1 - \phi(\frac{m_{SBAmin}-m}{\lambda})$. It pays to conduct this test if

$$(a - c)\left(1 - \phi\left(\frac{m_{SBAmin} - m}{\lambda}\right)\right) > dL$$

where $a = a' + E(bP(\ldots))$ or a fraction of the original loan size and an expected value of the future repaid loan. The lower is d , the lower is the minimum value of m at which the CDC conducts an examination, the greater the number of loans that have an updated mean value above the cut-off, and therefore the greater the number of loans successfully processed by the CDC. It follows that as d decreases, loans processed increases, while average mean return on loans falls toward m_{SBAmin}.

The bank finances a portion of the loan (fL). By lending only a piece of the loan, a bank's loan considerations change. A bank becomes willing to extend a loan to a lower mean value project than previously. The interest rate the bank receives on this portion of the loan is competitively determined by the following equation,

$$r_m P_{\gamma > fL(1+r_m)-m} = r_f$$

where $P_{\gamma > fL(1+r_m)-m}$ is the probability that the idiosyncratic return is greater that the difference between the repayment of the bank's portion of the loan, fL, and expected return on the full loan.

1.2.1 Model predictions

When county monopoly status is relaxed, CDCs are able to process loans in geographic areas to which they had not had access previously. Assuming that the monopoly-period CDC has the first mover advantage, other CDCs will enter the geography only if they can earn non-negative returns; that is, these CDCs must have lower examination costs, d. The greater the number of CDCs that have access, the greater the chance that another CDC has a lower examination cost and therefore can increase loan processing volume upon entry. Theory predicts a positive relationship between the number of CDCs that gain access to each county and volume of loans processed.

Theory outlined above also generates a testable prediction for average loan quality. A CDC with high examination costs will choose not to examine firms below or close to the SBA mean return cutoff, m_{SBAmin}, whereas CDCs with lower examination costs will find it worthwhile to examine these lower quality firms. As long as a CDC's examination costs are low enough, it is profitable for the CDC to examine lower return firms in order to identify a fraction of firms with updated mean returns above the cutoff. CDCs entering new geographies have lower examination costs and process loans that appear to have lower mean returns. Overall, theory predicts a negative relationship between the number of CDCs that gain access to the county and average loan quality.

The final prediction states that as a result of regime changes, CDC rents will decrease. Since loans are referred to CDCs by banks and banks are perfectly competitive, borrowers will begin to extract rents from competing CDCs after county monopoly status is relaxed.

1.3 504 Loan Program

The 504 Loan Program allows a credit-constrained small business to apply for and potentially receive a loan to finance fixed assets including construction, building, and machinery while meeting job creation or minority ownership criteria. Typically, a small business applies for a loan at a bank and is notified by the bank of its qualification for the 504 Loan Program. If the firm is interested, the bank then forwards the case to a CDC that can evaluate the firm and process its 504 Loan application. If the business chooses to apply for a loan under the program, then the CDC and bank work together to process the loan. While the bank evaluates the application internally, the CDC submits the loan application to the SBA for loan approval. The role of the CDC is to file documentation with the SBA to show that the business is qualified and is projected to repay the loan.

As the loan clearing-house, the SBA sets minimum borrowing standards. SBA loan officers enforce the borrowing standards by using objective and subjective measures to ensure that the projected repayment rate is above the threshold. Due to the fact that these loans are targeted at riskier borrowers with higher default rates and that the SBA holds a second lien position on each loan, default rates are expected to be higher than in conventional lending. Loans are priced to be as expensive as or more expensive than conventional loans (due to fees in addition to interest payments), discouraging firms who qualify for convention loans while attracting higher risk borrowers who are locked out of conventional loan markets.

If the loan is approved by both the SBA and the bank, the loan is financed. The 504 Program requires that at most 40% of the loan and up to a set maximum is funded by the SBA, 50% or more of the loan is funded by the bank, and at least 10% of the total is composed of borrower's equity. The 504 program reduces risk faced by the bank from each loan in two ways. First, in case of default, the bank occupies a first lien position, while the SBA takes a second lien position. As a result, the bank faces less risk from its portion of the loan than if the bank had lent the same amount independently of the 504 Program. In addition, the risk is reduced through a reduction in the bank's share of the loan to 50% as compared to a 65-75% share on a conventional loan.[2] Since the SBA is second in line to collect when a loan is in default, the SBA occupies a riskier position and faces a greater loss rate than banks.

The SBA's funds for the program are raised through a public sale of debt and are guaranteed by the SBA. Although these loans are guaranteed by US government they are sold for 200 to 300 interest points above the comparable treasuries rate. Other work has documented this phenomenon but has not put forth a convincing explanation as to

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[2] Conventional loans have higher equity requirements from borrowers than 504 Program loans.

possible reasons. The bank's portion is required to also be a fixed interest loan and therefore, like all other fixed interest loans, is tied to the 10-yr treasury note rate. When the rate on 10-yr treasury notes increases, variable interest rate loans become cheaper and thus more attractive to borrowers than fixed interest rate loans such as those offered through the 504 Program.

The 504 Loan Program designates the way in which a CDC is remunerated for its role in the underwriting process. A CDC receives a yearly payment proportional to the size of the loan as long as the loan is in the process of being financed or repaid. The CDC receives 1% of the loan at loan signing and an additional 0.5% once the project (construction or tenancy in a new building) is deemed complete. The fee structure ensures that CDCs benefit from loan closings. The CDC also receives yearly fees from loan servicing during the lifetime of the loan. This payment is conditioned on the loan being current. The payout system encourages the CDC to find borrowers who continue to pay down their loans, but does not punish the CDC for loan default. As a result, CDCs have financial incentive to grow loan volume, while paying limited attention to loan quality.

Prior to 2003, CDCs were granted monopoly rights to operate at a specified geographic level of one or more counties within a state. The original aim of the 504 Loan Program was to create loan processing organizations that would be best positioned at locating credit constrained businesses by being tuned into local economic and development conditions. Starting in 1986, a firm that applied to become a CDC under the 504 Loan Program was granted a geographic Area of Operations of one or more counties within a state where the CDC had monopoly lending status for the program. Multiple

Areas of Operations could overlap subject to approval from the oldest CDC in that Area of Operations.

In 2003, after over six months of deliberation, SBA implemented a regime change that eliminated all county-based boundaries, but left state boundaries in place. After the change, a CDC could lend anywhere within its state of operations. CDCs were no longer constrained to county allocations. Following the change, SBA conducted an internal study of the impact of geographic "liberalization." At the time, the SBA considered expanding the domain of CDCs beyond the state level. The internal study, since lost, found that post-geographic liberalization, average loan quality declined, while CDCs increasingly targeted high volume loan areas such as urban areas to the detriment of other more underserved locales. Liberalization beyond the state level did not take place.

Throughout this period, the SBA allowed CDC to process loans across counties or even state lines as long as the area belonged to the same MSA. That is, a CDC could operate in a contiguous economic area bisecting more than one state or temporarily expand its Area of Operations to include under-served areas with SBA's approval. From the SBA's perspective, this rule allowed for processing across dense population centers. Post-regime change, CDCs from either state could process loans across MSAs straddling a state boundary. Outside of these MSAs, CDCs were limited to processing loans in counties within their original state. This rule generates additional testing grounds to analyze the impact of regime change.

The Preferred Loan Program allows CDCs to approve 504 Program loans independently of the SBA. By approving the loan independently the CDC guarantees loan approval and decreases the time to process a loan. In exchange, the CDC takes on partial liability for the loan. The CDC is required to deposit 1% of the loan (equivalent to the loan fee the CDC receives after loan closing) into a separate account in case of loan default. Further, if default occurs, the CDC is liable for 10% of the loan. Qualification to process loans under the Preferred Loan Program is based on the CDC's multi-year record of default rates. The designation must be renewed every few years.

The Preferred Loan Program can be interpreted to be a rents transfer between a CDC and a bank. When a CDC processes a loan under the Preferred Loan Program, the loan approval decision circumvents SBA processing and allows the CDC to issue the loan decision. As a result, the bank faces decreased costs when a loan is processed by a CDC through the Preferred Loan Program, while the CDC faces additional financial obligations at the time of loan issuance as well as financial repercussions in case of loan default.

As mentioned above, a 504 loan is less costly to a bank when processed through the Preferred Loan Program. Since banks are perfectly competitive, all loan savings from the program are passed on to borrowers, decreasing the total cost of a 504 Program loan. This decrease in cost lowers the risk level at which a borrower is indifferent between a conventional and 504 loan. By qualifying for and lending under the Preferred Loan Program, a CDC incentivizes lower risk borrowers to borrow through the 504 program that would have otherwise borrowed through conventional means. In this way, a CDC can better compete with conventional loans and raise the quality and volume of 504 loans.

Post-regime change, the Preferred Loan Program can be used as a basis of competition between CDCs for bank loans. Multiple CDCs can now process loans in any one county. Given the choice of multiple CDCs, a bank will choose to share loans with the lowest cost CDC. One of the ways in which a CDC can lower 504 Program costs to a bank and, in turn, the borrower is by processing loans through the Preferred Loan Program. Therefore, post-regime change CDCs should begin to compete with one another for bank loans and gain loan share from other CDCs by processing more of their loans through the Preferred Loan Program.

1.4 Empirical Strategy

The empirical strategy is based on OLS, difference-in-difference estimation with controls added to help rule out alternative explanations and channels. The key independent variable is the CDC pool size at the state level interacted with regime change timing (pre and post). The CDC pool size is the number of CDCs active in the state pre-program change. Since the independent variable is calculated at the state level, regression standard errors are also clustered at the state level. Changes to the estimation strategy are made given additional data insights or limitations. These changes involve removing time or adding geographic interactions. Outcome measures include loan volume, loan quality, economic growth and competition measures. Each of the following sub-sections previews the estimation strategy specific to each of the four outcome measures.

1.4.1 Impact on loan volume

The main econometric specification measures the causal effect of the number CDCs on lending volume. Since lending is driven both by supply of and demand for loans, it is usually impossible to isolate the causal effect of credit supply on lending volume. In this study, the supply-side shock to lending is driven by regime change. Regime change dictated that intermediary lenders could expand lending from previously designated counties to the rest of the state. The number of intermediaries at the state level pre-regime change can be used to identify the portion of the increase in lending post-program change that was driven by the supply of intermediaries. For this analysis to produce causal estimates, I control for demand-side drivers and must assume that demand for loans does not change over this time period in a way for which I cannot control. The following assumption must also hold: the number of intermediaries in the state pre-regime change is not correlated with unobserved variables in the error term including any demand-side drivers. The second assumption will be violated if independent and dependent variables (lender pool size at state-level and lending volume at county level, respectively) are driven by state-specific, time-varying variables such as economic conditions not captured by time-varying controls included in the regression.

As mentioned above, exogenous variation is based on the interaction of a post-regime change dummy and state-level intermediary count pre-regime change. Using intermediary count pre-program change addresses the concern that the regime change also increased CDC entry. In reality, the number of CDCs changed little over this time period with less than 5% entry and exit rates nationwide. The estimation equation is at county level with the key independent variable, intermediary count, measured at state level and interacted with program timing dummies. The estimation equation is the following,

$$
\begin{aligned}
LogLoanVol_{ct} \\
&= \beta(Post_t)(LogNoCDC_s) + \delta_1 Post_t + \delta_2 LogNoCDC_s \\
&+ \sum X_{ct} + a_c + a_t + \varepsilon_{ct}
\end{aligned}
$$

where c indexes county, s state, and t year. $LogLoanVol_{ct}$ is the natural log of total intermediary-originated loan pool in county c and year t, $Post_t$ is a dummy which turns on post-regime change, $LogNoCDC_s$ is the log number of CDCs in the state pre-regime change, and X_{ct} includes controls for observed county-year characteristics that will be explained in the data section below. Other controls include county and time fixed effects. The regression is clustered at state level.

Program rules allowing for loan intermediation across state boundaries within an MSA offer an additional test case. Post-regime change, counties within MSAs and across state borders were able to have loans processed by CDCs based in either state, while counties outside of MSAs could only draw on own state intermediaries; this allows for a test based on a comparison of contiguous counties on state borders within and outside of MSAs. For this estimation to be valid, an additional assumption must hold that all unobserved error trends for contiguous counties are identical. For this analysis, the estimation equation above must be adjusted by interacting the independent variable with an outside of MSA identifier and limiting the sample to contiguous counties on state borders.

1.4.2 Impact on loan quality

The empirical set-up to measure the effect on loan quality mirrors the regression setup in Section 1.4.1 with adjustments for data constraints. Loan quality can only be reliably measured in the data starting in 2005. As a result, I can no longer interact the explanatory variable and a time variable in the estimation strategy. This data limitation causes additional identification issues. First, I can no longer use county fixed effects to eliminate constant differences among counties and must rely on other controls to do so.

More importantly, I cannot compare loan quality over time and must rely on other comparisons for identification. As a result, I use three comparisons to measure the impact of the intermediary pool on loan quality. Since relevant data is only available post-regime change, these comparisons are within the time period - not across time periods as with earlier estimations. The regression set-up takes the following form,

$$LoanQual_{ict} = \beta(LogNoCDC_s) + \sum X_{ct} + a_t + \varepsilon_{ct}$$

where i indexes the loan, c county, s state, and t year. $LoanQual_{ict}$ is quality metric for loan i issued in count c in year t. $LoanQual_{ict}$ is the log number of CDCs in the state pre-regime change, and X_{ct} includes controls for observed county-year that will be explained in the data section below. Other controls include time (a_t) fixed effects. The regression is clustered at state level. Loan quality can be measured using loan default rate and loan quality score obtained by the SBA from a third party.

The next set of comparisons use indicator variables to measure differences in loan quality between newly expanded portfolios and "home" portfolios first within intermediary and then within county. The first comparison is within intermediary and across counties. This analysis compares CDC portfolio in its "home" county or county in which it operated pre-regime change to its portfolio in the county to which it expands post-regime change. A "home" county is defined to be a county in which the CDC serviced the majority of loan volume pre-program or the CDC was one of two CDCs in the county to each service approximately 50% of loan volume. This analysis removes all time-invariant heterogeneity due to differences in CDCs, but requires the assumption that the difference in portfolios is not driven by unobservable county characteristics, county-CDC or time interactions. This regression set-up takes the following form,

$$Y_{ijct} = \beta H_{jc} + a_j + a_s + a_t + \varepsilon_{ijct}$$

where i indexes the loan, j CDC, c county, s state, and t year. Y_{ijct} is the loan credit score or default dummy where loan i is issued by CDC j in county c in year t, H_{jc} is a dummy equal to 1 if the county is not a "home" county for the CDC as defined above and a_j, a_s, a_t are CDC, state and time fixed effects, respectively.

The second comparison is across CDCs and within county. This analysis compares loan portfolios between a CDC in its "home" county and recently expanded CDCs to the same county. In contrast to the first analysis, this analysis allows us to control for heterogeneity based on time-variant county characteristics, but requires the assumption that the difference in portfolios is not driven by unobservable differences among CDCs or CDC-time interactions. This regression set-up takes the following form,

$$Y_{ijct} = \beta H_{jc} + a_c + a_t + \varepsilon_{ijct}$$

where H_{jc} is a dummy equal to 1 if the loan is made by a CDC not in its "home" county and a_c, a_t are county and time fixed effects, respectively.

1.4.3 Impact on competition

If CDCs began to compete with each other for bank loans post-regime change, then competition would allow banks to extract rents that previously belonged to the CDC. While there are many possible sources of rent, most cannot be measured. One observable source of rents is the method of loan processing chosen by the CDC. A bank can extract rents by demanding that loans are issued in the most favorible way for the bank.

Observing more intermediary-certified loans in the data signifies an increase in rent collection by banks. I follow Nickell (1996) in measuring potential for competition by counting the number of potential competitors. That is, the degree of potential competition is measured by the number of CDCs in the state. The regression is the following,

$$Y_{ict} = \beta LogNoCDC_t + \delta Post_t + \sum X_{ict} + \sum X_{ct} + a_t + \varepsilon_{ict}$$

where i is loan, c is county, and t is year. The dependent variable, Y_{ict}, is a dummy that turns on if loan i that is issued in county c in year t is certified, the independent variable, $LogNoCDC_t$ is the log of the number of CDCs at the state level, $\sum X_{ict}$ includes loan level controls such as loan amount and loan credit score and $\sum X_{ct}$ includes county-year controls. The regression also includes time (a_t) fixed effect. Due to data constraints, the explanatory variable cannot be interacted with a time dummy. As a result, county and state fixed effects must be left out of this model.

1.4.4 Impact on growth

An important question in the economics growth literature is whether credit constraints affect economic growth. Although this question has received a lot of attention, there have been few convincing answers. One of the reasons that a credible answer is so difficult to obtain is the challenge of isolating the impact of credit supply on overall economic growth. It is often the case that forces acting on the supply and demand side are impossible to pick apart. For example, a regression correlating lending volume and economic growth does not result in a causal estimate because lending volume is driven by not only supply of credit but also demand for credit.

Despite these drawbacks, I argue that a growth result can be estimated in this case in which loan volume grew due to supply side and demand side can be adequately controlled. I measure the impact of loan volume on county-wide growth. In order for the estimation to be consistent, in addition to the assumptions in Section 1.4.1, additional necessary assumptions include that the error term is not correlated with the explanatory variable. A caveat of this analysis is that the size of the 504 Program may be too small to measure impact from the decrease in credit constraints and increase in loan volume.

$$Growth_{ct} = \beta(Post_t)(LogLoanVol_{ct}) + \delta_1 Post_t + \delta_2 LogLoanVol_{ct} + \sum X_{ct} + a_c + a_t + \varepsilon_{ct}$$

where $Post_t$ is a dummy which turns on post-regime change, $LogLoanVol_{ct}$ is the total intermediary-originated loan pool at county-year level, and $\sum X_{ct}$ includes controls for observed county-year characteristics that will be explained in the data section below. $Growth_{ct}$ is economic growth at county-year level. Other controls include county (a_c) and time (a_t) fixed effects. The regression is clustered at county level in line with the independent variable's source of variation at the county-year.

1.5 Data

This paper makes use of 504 Loan Program data as well as several other datasets. Most of the analyses in this paper are based on 504 Loan Program data collected by the SBA. The Longitudinal Business Database (LBD) is the source of county-level yearly employment growth data. Community Reinvestment Act bank disclosure provides data on yearly commercial bank lending to small business. I also incorporate datasets that identify contiguous counties on state borders and counties in MSAs.

504 Loan Program data is composed of data on loans processed by each active CDC from the start of the program in 1986 through 2012, with the program change occurring in 2003. Loan-level data includes information on the borrower, the CDC, approval decision, size of loan, borrower credit score, loan repayment information, and the partner bank on the loan. The credit score is obtained by the SBA from a third party a month after the loan is finalized and becomes available for all processed loans starting in 2005. The single score is a composite of borrower and firm credit scores when available. The partner bank is the bank that processes and approves the loan in partnership with the CDC. Data on the partner bank is available starting in 2004. All other variables are available in all years that 504 loans were processed.

Community Reinvestment Act (CRA) data are made available by the Federal Financial Institutions Examination Council (FFIEC) on its website. The data are available starting in 1996 although data from a large swath of counties are not available until 1997. The data include various lending metrics for banks larger than a CRA-specified threshold. I use data on number of loans and dollar amount lent to businesses with under $1 million in revenue as it is the closest category to the category used by the 504 Loan Program.

1.5.1 Summary statistics

In this section, I describe the data for the analysis and begin to apply theoretical predictions to the data.

Table 1.1 and Table 1.2 present summary statistics for key variables. The most commonly used variable in the analysis is the number of CDCs at the state level before program change, measured over a two year span between 2001 and 2002 to increase count precision. 46 states have at least one CDC. Since the independent variable in most analyses is the logarithm of the number of CDCs at the state level, most regressions are limited to these 46 states. Log of loan counts at county-year level as proxy for loan volume in regressions limits the analyses to instances with positive loan counts. Nevertheless, there are over ten thousand county-years in which the loan count is positive. A county-year with a positive loan count has on average 5.8 504 program loans.

County-level growth variable is the percentage change in county employment in the following year. The mean and standard deviation for the growth variable are 1.7% and 27%, respectively. Default rates prior to 2005 are low (<5 percent), but rise dramatically between 2005 and 2007 to 19%. Given the relatively small number of loans made in any county, low default rates prior to 2005 make for an unreliable county-level comparison, therefore default data can be used only starting in 2005. Pattern of loan certification follows a similar pattern with 11% of all loans certified prior to 2005 and 17% between 2005 and 2007. Conveniently, credit scores for 504 Loan Program borrowers become widely available in 2005 as well. I also test the two available measures of loan quality (credit score and default) and find that they are closely related. I find that one standard deviation increase in a credit score almost doubles the probability of default. Finally, I choose 1997 and 2007 as start and end dates for most analyses for several reasons. 1997 is the first year that Community Reinvestment Act data are available for counties nationwide. Most analyses run through 2007 only as this was the last year preceding the Great Recession.

The variable measuring the number of CDCs at the state level is the key independent variable and is crucial to the analysis. Therefore, it is important to understand the source of variation in the variable. I investigate the number of CDCs at the state level and years to 504 Loan Program entry in Table 1.4 and Table 1.5. I use a number of geographic and firm-based variables to test how much of the variation can be predicted. These variables include number of high and low population density counties, average population density, average business density, part of country dummies, and number of years prior to regime change when the state received its fifth 504 loan. I use a Poisson regression because the mean and standard deviation of dependent variables are approximately equal. I find that I can predict 38% of the variation in the number of CDCs at the state (Table 1.4) and 59% of the variation in the number years to >5 loans at the county level (Table 1.5). These estimates are substantial despite the aggregation in the data. This analysis suggests that CDC presence can be explained by observable state and county characteristics that I am able to control for in the empirical section of the paper.

Next, I study descriptive patterns in the 504 Loan Program data. Figure 1.1, Figure 1.3, and Figure 1.4 display changes in counts of CDC, loans, and counties with loans over time. Most CDCs offer more than 50 loans over their lifetime, CDC entry and exit is low with each less than 3% (see Figure 1.2) and CDC counts do not change over the key period. Turnover rates are consistently in the low single digits throughout the period (not shown). The alternative explanation that the impact post-program change is due to a change in the number of CDCs can be ruled out. Although the number of CDCs does not change, the number of loans processed rises dramatically during the key period post-program change (Figure 1.3) and offers initial evidence that the program had impact on loan processing.

The key question is whether the program change had impact by altering CDC behavior. Figure 1.6 suggests that this is, in fact, the case. The most telling sign of the regime's bite is whether post-regime change CDCs took advantage of the change in rules to expand to new counties. Both tables show that starting from the time of program change in 2003, the number of counties with multiple CDCs grew quickly. The number of counties with two or more CDCs more than doubles, while the number of counties with three, four or five CDCs more than quadruple. The model above says that as the number of CDCs in each county expands, so does loan processing. This is in line with the hypothesis that the program had an impact on CDC behavior and loan processing. Regressions will need to show that the program had an impact on loan processing through a change in CDC behavior. Figure 1.7 studies CDC expansion into new counties. The number of new CDC-county pairs grows in the year the change is made and surpasses previous levels the year following program change. Those high levels are maintained until the start of the recession in 2008. Overall, these patterns suggest that program change had a substantial impact on CDC behavior and lending volume consistent with the outlined theory. More testing is required to determine the robustness of these results.

1.6 Results

1.6.1 Lending volume

Theory says that the greater the number of intermediary lenders that can enter a county's lending pool, the greater the credit supply in that county. That is, given all else equal, a state with more intermediary lenders will mean a larger number of draws from the examination cost distribution resulting in a lower minimum d. A lower d at the state level will lead to greater impact. A competing theory uses a different mechanism to get to the same prediction. This theory says that assuming all intermediaries specialize in processing loans for different firm-types, the greater the number of intermediaries, the greater the number of firm-types that can be served within each county. Both theories lead to the same prediction which says that a greater number of intermediaries at the state level will generate a greater program effect on lending volume.

I find a substantial and significant effect of the number of intermediary lenders on loan volume post-program change. Table 1.6 displays the estimates from the estimation based on the full sample of county data. We find that a one hundred percent increase in the number of CDCs at the state level will increase lending volume by fourteen percent. Since the average number of CDCs at the state level is 3.3 with a standard deviation of 3.1, a one hundred percent increase is a realistic measure. Additional controls do not change the estimate or its precision. Table 1.7 and Table 1.8 break out the estimates by year by interacting the independent variable with year dummies. This estimation shows that most of the effect comes from the years 2005 through 2007. Unlike in the earlier case, in this disaggregated estimation, coefficients are almost fifty percent smaller and become insignificant with the addition of all controls. This decrease in effect seems to result from a decrease in power that stems from disaggregating the independent variable.

Next, I take advantage of the MSA rule that I describe earlier in the paper. The rule, which held constant throughout the period of program change, allowed for CDCs to process loans within the same MSA even if the MSA crossed county and, most importantly, state boundaries. Post-program change, counties in MSAs but across state boundaries can benefit from CDCs in both states, while counties elsewhere can only have loans processed by CDCs within their state. On average, 4% to 5% of loans are processed by CDCs outside of their designated state between 1997 and 2003. Starting in 2004, this percentage begins to increase until it reaches a peak of 13% in 2007. The threat of CDCs from out of state coming in to process loans is what matters, rather than its actual application.

I use this rule to produce several key estimates. First, I conduct a placebo test to test the effectiveness of this rule. Next, I run a regression in which I use contiguous counties across state borders as controls for one another. That is, I compare contiguous counties outside of MSAs to contiguous counties in MSAs based on the number of CDCs at the state level interacted with time dummies. I expect that this will provide the most precise estimate of regime impact. Finally, I run a full regression, but exclude all counties contiguous to state boundaries that are in MSAs since these counties are affected by the rule. The results are consistent with theories and rules outlined above.

For a placebo test, I compare contiguous counties within an MSA but located different states. I look for an effect on the number of CDCs at the state level in the state within which the county is located interacted with a time dummy. Table 1.9 shows the results of the estimation. I find no effect of the number of CDCs on loan processing volume in the county within the same state. This result is consistent with the hypothesis above.

Second, I compare contiguous counties located across state borders outside of MSAs to such counties in MSAs in Table 1.11. The resulting estimate jumps as much as four-fold compared to the estimates based on full sample. I find that a one hundred percent increase in the number of CDCs at the state level will increase lending volume by as much as sixty percent. Estimates are robust to a variety of controls.

This set-up allows for a more precise estimation due to the use of contiguous counties on state borders. This is a common practice in measuring the impact of state-wide policies in trying to control for linear county-level trends over time and is especially prominent in measuring the impact of a minimum wage. Estimates can, however, be criticized for being specific to counties situated within or outside of MSA on state borders. Of course, this estimate is limited by the restricted sample size and applicable to counties outside of MSA zones. Finally, for comparison, we run the same regression with a sample of counties not on state border. When the sample is limited to contiguous counties, county fixed effects become county-pair fixed effects and a triple interaction is created using the MSA location dummy in the equation.

Overall, I find that a one hundred percent increase in the count of CDCs increases loan processing by an amount that falls in the fifteen to sixty percent range. Results are maintained within different data sub-samples and estimates are robust to the addition of numerous controls.

1.6.2 Loan quality

Theory says that loan quality decreases with an increase in the lender pool. First, I test this theoretical prediction using a similar set of data samples as in Section 1.6.1. I look at the overall sample of loans in all counties. Then, I compare contiguous counties across state borders within and outside of MSAs. Since both key variables are measured post-program change, the independent variable of the number of CDCs is no longer interacted with a time dummy. Next, I look within the state. I label CDC-county pairs that existed prior to program change as “home” portfolios and CDC-county pairs arise post-program change as “new” portfolios. Based on the model, we expect “home” portfolios to be of higher quality than portfolios in new counties of operations. I compare quality of loans within county between “new” and “home” CDCs and within CDC between “new” and “home” counties. I measure loan quality in two ways - borrower's credit score and loan default dummy and implement OLS and logit estimation strategies.

If instead we assume that CDCs specialize in some industry type or firm type and all have identical examination costs, then CDCs will enter additional markets and expand overall loan access without a decrease in observed loan quality.

I find that the number of CDCs at the state level is negatively correlated with loan credit scores, but has no relationship with default rates (see Table 1.12). A one hundred percent increase in CDC count is correlated with a decrease of 0.12 to 0.17 standard deviations in the credit score. The correlation with default rates is slightly negative, but small and insignificant.

The next set of comparisons use dummies to identify new CDC-county pairs and measure differences in loan quality first within the county and then within the CDC. I find similar estimates in both cases. Loans in a new CDC-county pairing as compared to a “home” CDC-county pairing in the same county process loans that are on average 0.04 standard deviations lower (Table 1.14). Similarly, loans in a new CDC-county pairing as compared to the same CDC's “home” county paring also processes loans that are on average 0.04 standard deviations lower (Table 1.15). The correlation with loan defaults is slightly negative, but small and insignificant. Overall, I find that CDCs expanding to new counties post-regime change lend to more low quality borrowers in these new counties.

An alternative explanation for findings in the second comparison is the theory common to the lending literature that lending entities not attuned to the local economic conditions will make worse lending decisions, but will learn to lend to similarly qualified borrowers as local lending entities over time. I test for this hypothesis in the robustness checks section. I do not see such improvement over time although the time period for which data are available is short so this is remains a possible explanation.

1.6.3 Competition

In the previous section, we did not discuss how loans are allocated among CDCs when multiple CDCs can process loans in a county. In this analysis I test for competition between CDCs for loans at the county level. I argue that observing more certified loans signifies more competition through an increase in rent collection by banks (see the data section above). Although several CDCs in a number of states had begun processing loans under the certification program prior to regime change, these were isolated uses of the program. Prior to regime change, each CDC's actions were limited in geographic scope and hence could not affect the type of loan processing used by CDCs in other counties. After regime change, one CDC's loan provision method could affect other CDCs' provision decisions within the state. Therefore, a competing CDC that was not willing to certify loans risked losing loan volume to other CDCs. Since the regime allowed competition at the state level, the effect would be limited to the state.

An alternative explanation is that by certifying loans and lowering the cost of lending in the 504 Loan Program for banks, a CDC can increase loan volume and continue to profit from these marginal loans. It can do so by lowering the cost of lending for banks and making banks indifferent between regular and 504 lending for higher quality bank loans. To decrease the likelihood that this explanation is driving the results, we can control for loan quality starting in 2005 when credit scores becomes available in the data.

Since the dependent variable is a dummy equal to one when the loan is certified, I estimate using OLS strategies (Table 1.17 and Table 1.18). I find that the number of CDCs at the state level as a proxy for the level of competition increases the proportion of certified loans. All estimates in the logit estimation are significant, though the magnitude of the estimate falls over two-fold when the data are constrained to the period post-regime change when the credit score variable is available. This pattern is in line with both theories presented above - a portion of loan certification was implemented to attract higher quality loans, while the remaining portion of loan certification can be attributed to competition between CDCs.

1.6.4 Growth

Theory says that in a credit-constrained setting, an increase in the supply of loans increases the number of loans made, thus driving growth. Therefore, I expect to see a positive coefficient on the explanatory variable. In this set of regressions, I look at the relationship between changes in the volume of loan processing and county-level growth. I find no effect on the response variable as shown in Table 1.19. This is not surprising due to the relatively small volume of 504 lending in any one county as compared to small business lending through other means as shown in summary statistics.

1.7 Conclusion

In this paper, I study a set of circumstances that offer unique insight onto the financial industry. The setting is the 504 Loan Program, one of several ways in which the Small Business Administration (SBA) lends to small enterprises in the US. The control which the SBA exercises over the program means that a substantial regime change can take place over a relatively short period of time; such a regime change is the focus of this study. The regime change altered geographic boundaries for institutions processing loans on behalf of the SBA from county to state. This change allowed different institutions to begin processing loans in overlapping geographic areas. Since this is a highly regulated market with regulation extending to prices paid in the market, the effect could not impact price directly and therefore could be measured on other margins.

Theory predicted that this regime change would bring many changes to loan processors as well as borrowers. Specifically, the model of the interaction between the SBA, processors and banks found that post-regime change loan volume would increase, while borrower quality and processor rents would both decrease. In fact, I find that loan processors took advantage of the regime change and expanded to process additional loans beyond their initial geographic boundaries. This allowed the 504 Loan Program to expand with a substantial boost to program loan volume. Although likely due to the relatively small size of the program, I do not find an impact on overall county-level growth resulting from the new regime. I go on to evaluate changes in borrower quality and look for evidence of competition between loan processors. Loan quality decreases were driven by processors with lower cost of doing business. In the meantime, rents that processors collect decrease signifying a presence of competition. Overall, I see an increase in volume, decrease in quality of borrowers and decrease in rents collecting to processors.

The intention of the regime change was to increase credit to small firms and alleviate credit constraints. A major tenet of the growth literature is that firms, and in particular small firms, experience lower growth due to the presence of credit constraints. Studies have documented that credit constrained firms operate below optimal size and have a firm-size distribution below that of unconstrained firms. Cross-country and cross-industry studies have shown that credit is an important prerequisite to growth. Nevertheless, injections of capital are rare. This is a great opportunity to study the impact on firms involved in the 504 Loan Program. Regarding the next research question as to whether there exists evidence that any of the following took place: increased lending benefited small businesses, competed these borrowers away from conventional means, or offered them better terms to reach a better outcome in the long-term.

I have shown that the regime change studied in this paper led to competition. The next question that arises is how to evaluate large-scale changes brought on by competition. Stephen Nickell (1996), in his paper on the topic of firm competition in the Journal of Political Economy, begins his analysis in the following way – “Most people believe that competition is a good thing.” It is generally accepted that competition diminishes profits, while stimulating growth, productivity, and innovation. Yet these analyses apply to a market cycle where the repercussions of under-performance or increased take-up of risk are bankruptcy and closure. However, the impact is far from clear when the ultimate risk bearer is the tax payer. For example, among financial institutions for which the government became the insurer of last resort during the Great Recession, did competition generate positive returns for the industry as well as tax payers? The question remains whether the benefits of competition outweigh the costs overall, in economic upturns as well as downturns. Future research may continue to explore which regime is less costly and which is more favorable to society.

1.8 Appendix

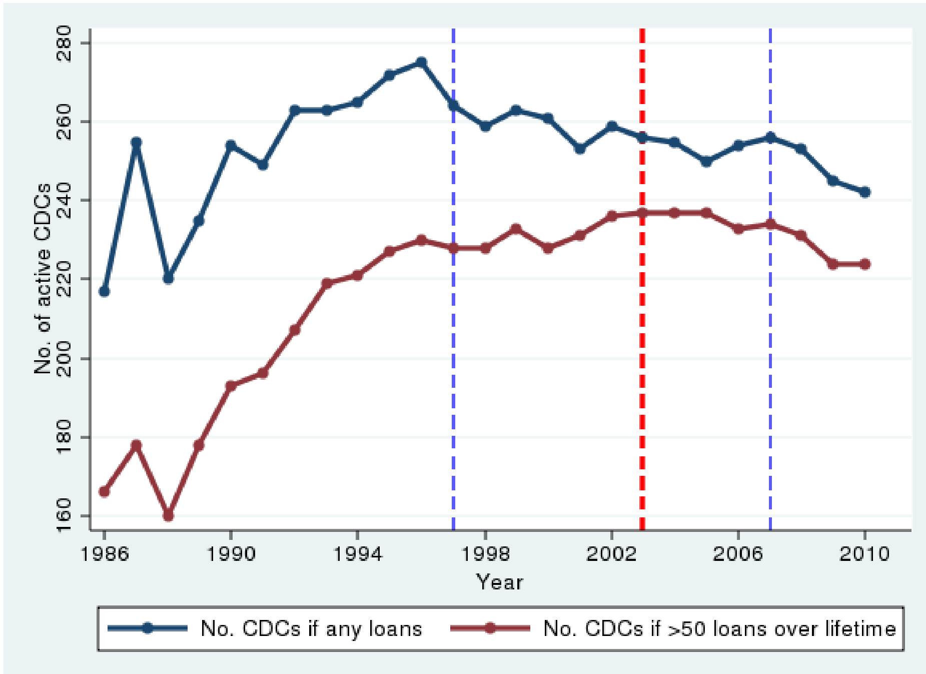

Figure 1.1 CDC Count

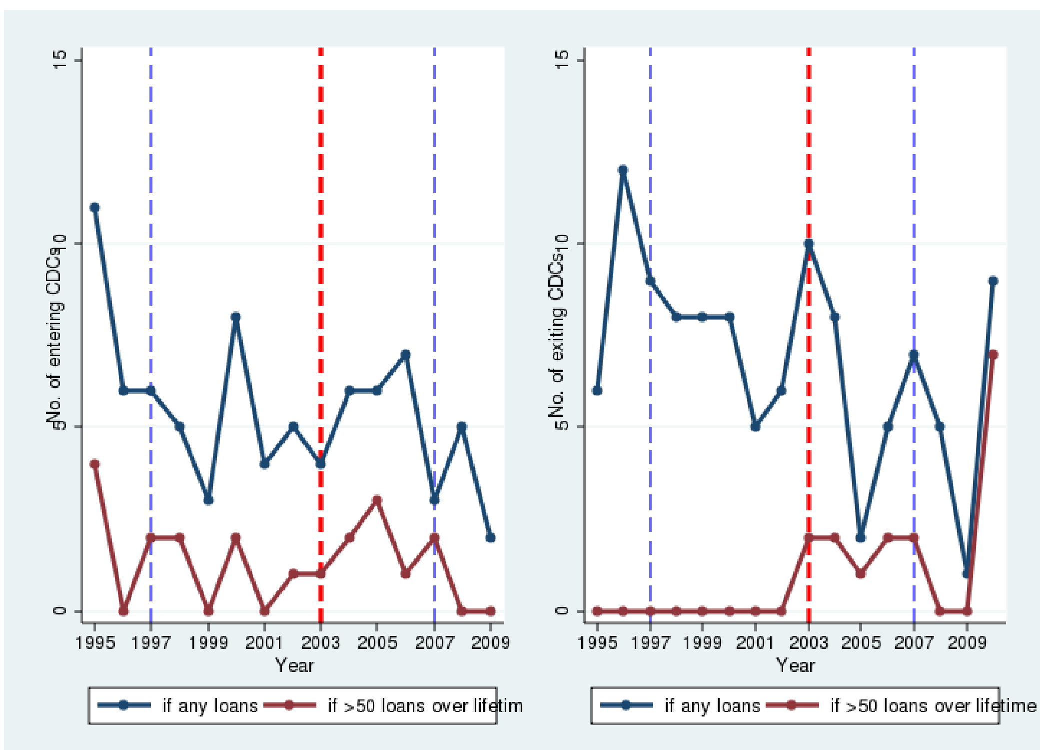

Figure 1.2 CDC Entry and Exit over Time

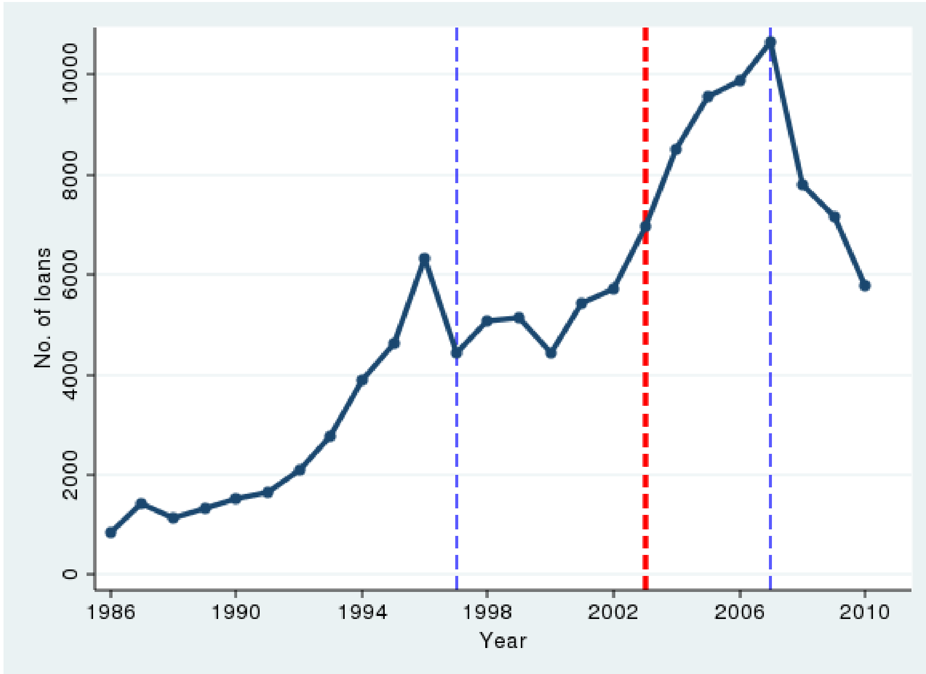

Figure 1.3 Loan Count over Time

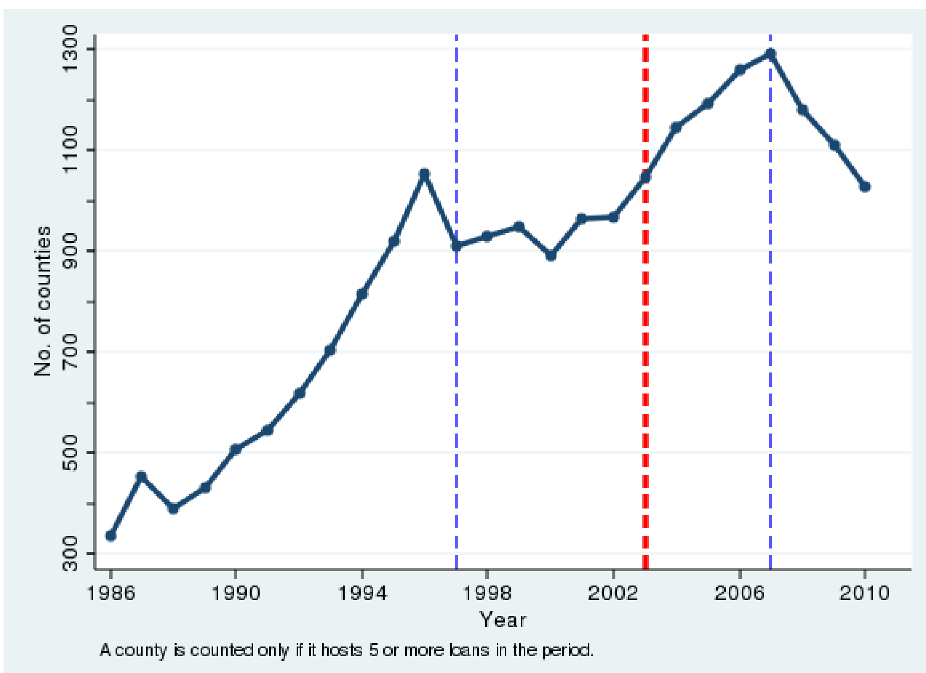

Figure 1.4 County Count over Time

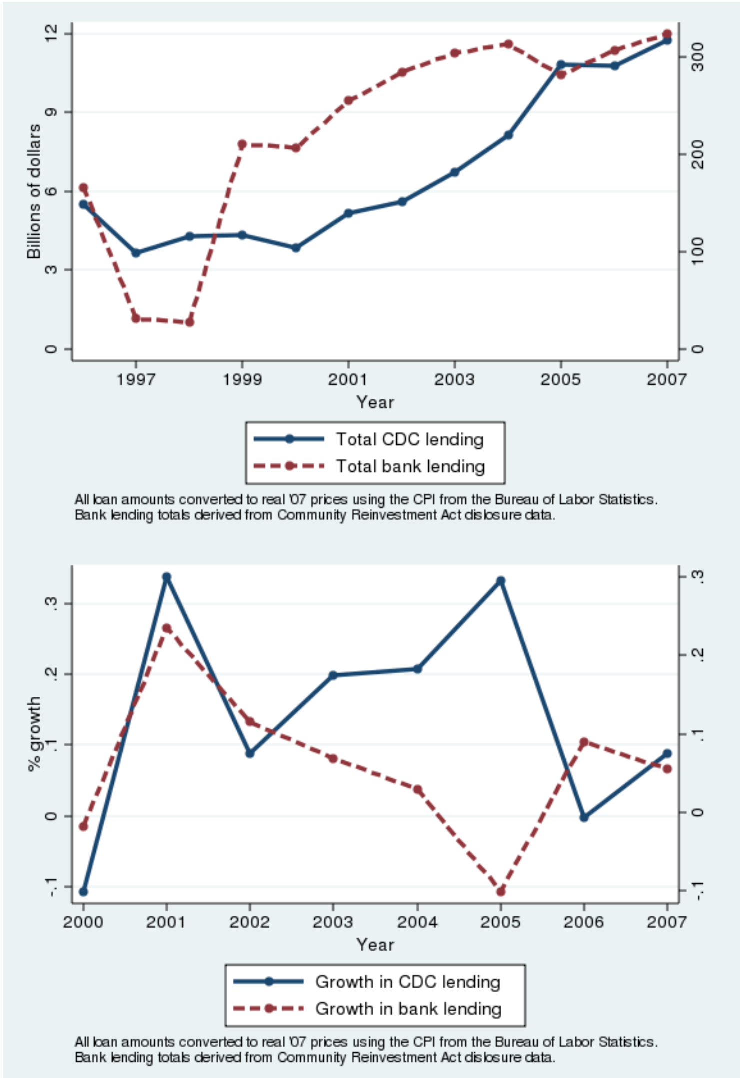

Figure 1.5 CDC and Bank Lending Comparison

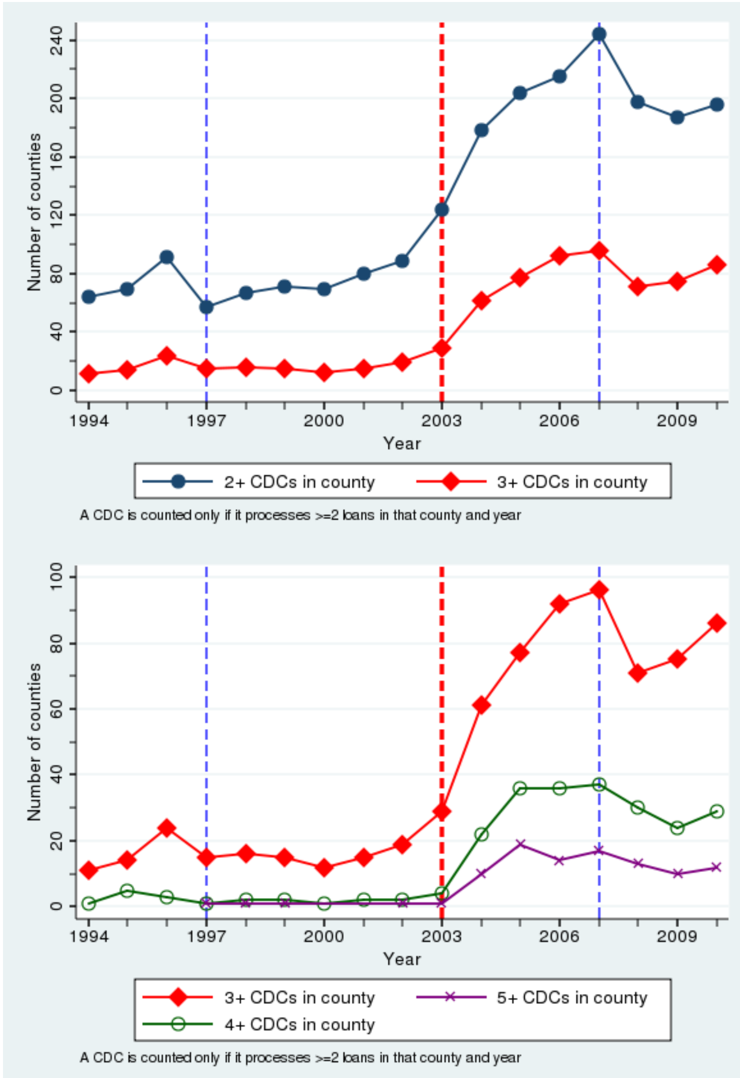

Figure 1.6 Counties by CDC Count

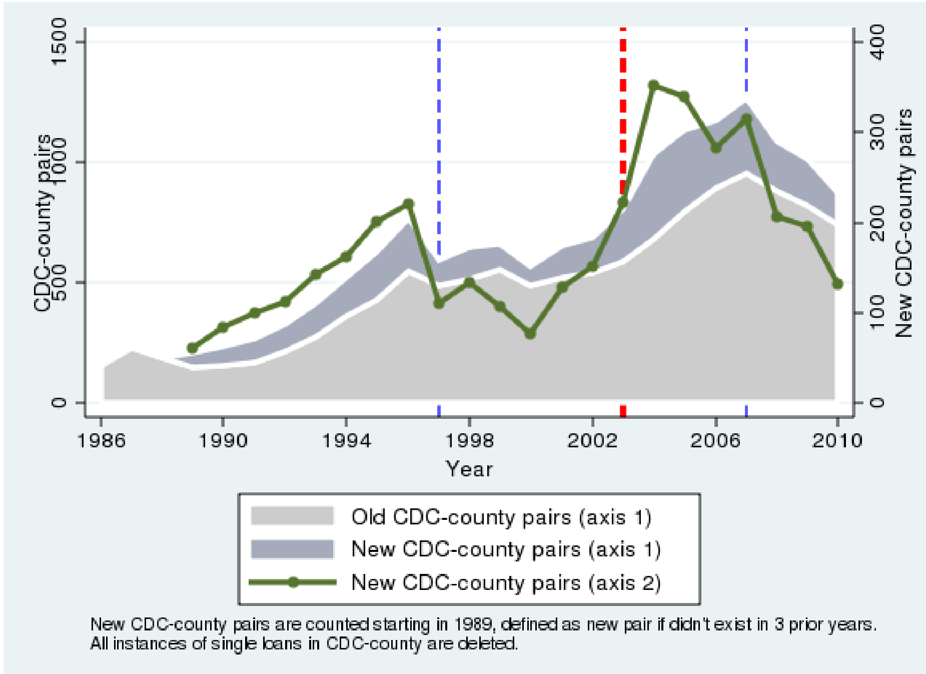

Figure 1.7 CDC-County Pairs

Table 1.1 Summary Statistics

	Count	Mean	SD	Min	Max
CDCcount(State)	49	3.1	3.1	0	18
CDCCnt(State if>0)	46	3.3	3.1	1	18
LoanCnt(if>0)	10,653	5.8	15.5	1	452
LoanSum($Mn, if>0)	10,653	6	21	0	817
PrivBanksLoanCnt	30,182	2,423	10,477	1	666,496
PrivBanksLoanSum($Mn)	30,182	80	301	0	15,840
GrowthRate(%)	30,182	0.02	0.28	-0.96	30.27

Summary statistics at county-year level unless otherwise noted.

Table 1.2 Summary Statistics

	Count	Mean	SD	Min	Max
LoanAmt($'000s)	63,103	1,079	866	52	9,000
Current('97-'04)	37,877	0.95	0.22	0	1
Current('05-'07)	25,226	0.81	0.39	0	1
CreditScore	22,835	190	25	75	246
CreditScoreSt	22,835	0	1.0	-4.5	2.2
Certified ('97-'04)	37,877	0.11	0.31	0	1
Certified ('05-'07)	25,226	0.17	0.38	0	1

Summary statistics at loan level unless otherwise noted.

Table 1.3 Loan Quality: Credit Score and Loan Non-Default

	(1)	(2)	(3)
CreditScoreSt	1.844^{***}	1.945^{***}	1.959^{***}
	(0.036)	(0.038)	(0.038)
Cnty/YrIndicators		X	X
CntyControls			X
N	19576	19461	19461

Logit estimation. Ceofficients are logit odds ratios. Errors clustered at county level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.4 Predicting CDC Count Pre-Regime Change at State Level

	(1)	(2)	(3)
CntyCnt	1.013***	1.002	1.000
	(0.002)	(0.002)	(0.003)
LogArea		1.105	1.037
		(0.134)	(0.147)
LogPopn		2.219***	1.087
		(0.226)	(0.857)
CntMSA			1.013
			(0.0125)
FirmCnt,<5Emp			0.698
			(0.439)
FirmCnt,5-50Emp			6.334
			(7.459)
FirmCnt,>50Emp			0.409
			(0.424)
GeogrIndicat(9)	X	X	X
N	50	50	50
Pseudo R^2	0.20	0.37	0.38

Poisson regressions. Ceofficients are incidence rate ratios. Robust SE. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.5 Predicting Years to 5+ CDC Loans at County Level

	(1)	(2)	(3)
LogDistCntytoMSA		0.682***	0.803***
		(0.026)	(0.031)
LogPopDens		1.660	1.025
		(0.057)	(0.041)
FirmCnt,<5Emp			0.281***
			(0.062)
FirmCnt,5-50Emp			8.369
			(2.276)
FirmCnt,>50Emp			0.928
			(0.107)
GeogrIndicat(9)	X	X	X
N	3089	3089	3089
Pseudo R^2	0.08	0.50	0.59

Poisson regressions. Ceofficients are incidence rate ratios. Robust SE. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.6 Growth in Loan Volume, Full Sample Regression

	(1)	(2)	(3)
PostXLogCntCDC	0.071*	0.095**	0.094**
	(0.037)	(0.038)	(0.037)
Post	0.169***		
	(0.048)		
LogCntCDC	0.095		
	(0.057)		
Cnty/YrIndicators		X	X
CntyControls			X
N	6826	6826	6826
R^2	0.023	0.784	0.784

State with outlier no. of CDCs and counties w/ <300 firms of size 5-50 in year 1996 are dropped.
Errors clustered at state level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.7 Growth in Loan Volume, CDC Cnt X Year interaction

	(1)	(2)
	LogLoanCnt	LogLoanCnt
LogCountCDCxYr1998	-0.066	0.013
	(0.094)	(0.102)
LogCountCDCxYr1999	-0.055	0.009
	(0.065)	(0.068)
LogCountCDCxYr2000	-0.027	0.067
	(0.079)	(0.090)
LogCountCDCxYr2001	-0.048	0.002
	(0.081)	(0.089)
LogCountCDCxYr2002	0.019	0.051
	(0.072)	(0.081)
LogCountCDCxYr2003	-0.006	0.038
	(0.072)	(0.084)
LogCountCDCxYr2004	0.008	0.045
	(0.086)	(0.096)
LogCountCDCxYr2005	0.038	0.094
	(0.080)	(0.091)
LogCountCDCxYr2006	0.074	0.122
	(0.071)	(0.078)
LogCountCDCxYr2007	0.136^{*}	0.194^{**}
	(0.078)	(0.092)
Cnty/YrIndicators		X
CntyControls		X
N	2882.000	2882.000
R^2	0.040	0.557

State with outlier no. of CDCs dropped Sample of counties with 300-900 firms of size 5-50 in year 1996. Errors clustered at state level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.8 Growth in Loan Volume, - CDC Cnt X Year Interaction

	(1) LogLoanCnt	(2) LogLoanCnt
LogCountCDCxYr1998	-0.115*	-0.084
	(0.060)	(0.066)
LogCountCDCxYr1999	-0.105	-0.081
	(0.078)	(0.063)
LogCountCDCxYr2000	-0.166	-0.092
	(0.105)	(0.085)
LogCountCDCxYr2001	-0.105	-0.026
	(0.087)	(0.083)
LogCountCDCxYr2002	-0.097	0.010
	(0.085)	(0.084)
LogCountCDCxYr2003	-0.117	-0.010
	(0.098)	(0.089)
LogCountCDCxYr2004	-0.120	-0.016
	(0.097)	(0.090)
LogCountCDCxYr2005	0.088	0.137*
	(0.077)	(0.081)
LogCountCDCxYr2006	0.021	0.109
	(0.095)	(0.091)
LogCountCDCxYr2007	-0.067	0.025
	(0.090)	(0.098)
Cnty/YrIndicators		X
CntyControls		X
N	3664	3664
R^2	0.067	0.786

State with outlier no. of CDCs dropped Sample of counties with 900+ firms of size 5-50 in year 1996. Errors clustered at state level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.9 Growth in Loan Volume, Placebo Test: Counties across State Borders within MSA

Log(loancount)	(1)	(2)	(3)
PostXLogCntCDC	-0.114*	-0.062	-0.065
	(0.067)	(0.048)	(0.054)
Post	0.404***	0.482***	
	(0.075)	(0.056)	
LogCntCDC	0.131	0.384***	
	(0.096)	(0.132)	
CntyPairIndicator		X	X
State & YrIndic, CntyControls			X
N	3583	3583	3583
R^2	0.023	0.661	0.738

State with outlier no. of CDCs and counties w/ <300 firms of size 5-50 in year 1996 are dropped. Errors clustered at state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.10 Growth in Loan Volume, Counties across State Borders

	(1)	(2)	(3)
PostXLogCntCDCXNonMSA	0.203**	0.295***	0.211***
	(0.082)	(0.075)	(0.077)
PostXLogCntCDC	-0.114*	-0.102**	-0.079
	(0.067)	(0.047)	(0.054)
LogCntCDCXNonMSA	-0.254*	-0.400***	-0.384***
	(0.127)	(0.083)	(0.100)
PostXNonMSA	-0.231**	-0.393***	-0.241***
	(0.098)	(0.084)	(0.086)
Post	0.404***	0.536***	
	(0.075)	(0.058)	
LogCntCDC	0.131	0.428***	
	(0.096)	(0.111)	
NonMSA	-0.454**		
	(0.181)		
Cnty Pair Indicator		X	X
State & Yr Indic, Cnty Controls			X
N	5171	5171	5171
R^2	0.122	0.664	0.726

State with outlier no. of CDCs and counties w/ <300 firms of size 5-50 in year 1996 are dropped. Errors clustered at state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.11 Growth in Loan Volume, Counties NOT in MSAs on State Borders

Log(loancount)	(1)	(2)	(3)
PostXLogCntCDC	0.197***	0.204***	0.202***
	(0.052)	(0.054)	(0.054)
Post	0.227***		
	(0.075)		
LogCntCDC	0.439		
	(0.078)		
Yr/CntyIndicators		X	X
CntyControls			X
N	2191	2191	2191
R^2	0.065	0.75	0.76

State with outlier no. of CDCs and counties with <5 loans/yr in all years prior to regime change are dropped. Errors clustered at state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.12 Loan Quality, Counties across State Borders

	(1) CrSc	(2) CrSc	(3) CrSc	(4) Curr	(5) Curr	(6) Curr
LogCountXNonMSA	-0.179	-0.253**	-0.229**	-0.00567	-0.0170	-0.00703
	(0.139)	(0.107)	(0.113)	(0.0445)	(0.0635)	(0.0606)
LogCountCDC	0.0440	0.0596	0.0868**	-0.0152	-0.0326**	-0.0267**
	(0.0377)	(0.0389)	(0.0428)	(0.0184)	(0.0130)	(0.0132)
NonMSA	0.222	0.239**	0.103	-0.0278	0.0330	0.0337
	(0.143)	(0.101)	(0.123)	(0.0443)	(0.0856)	(0.0762)
N	9335	9335	9335	9335	9335	9335
R^2	0.00178	0.0557	0.0572	0.0122	0.0649	0.0658

OLS estimation. State with outlier no. of CDCs dropped. Errors clustered at state level (43).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.13 Loan Quality, Counties across State Borders (Logit)

	(1) Curr	(2) Curr	(3) Curr
Current			
LogCountXNonMSA	0.978	0.892	0.896
	(0.247)	(0.325)	(0.294)
LogCountCDC	0.909	0.800**	0.833**
	(0.108)	(0.0700)	(0.0734)
NonMSA	0.836	1.227	1.322
	(0.224)	(0.608)	(0.556)
N	9335	9213	9213

Logit estimation. Ceofficients are logit odds ratios. State with outlier no. of CDCs dropped. Errors clustered at state level (43).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.14 Loan Quality Within County

	(1) CrSc	(2) CrSc	(3) CrSc	(4) Curr	(5) Curr	(6) Curr
New CDC (to county)	-0.0528**	-0.0416	-0.0439*	-0.0137	-0.0110	-0.0132
	(0.0240)	(0.0252)	(0.0244)	(0.0106)	(0.0103)	(0.0118)
Yr/CntyIndicators		X	X		X	X
CntyControls			X			X
N	15601	15601	15601	15601	15601	15601
R^2	0.00126	0.0378	0.0391	0.00635	0.0595	0.0613

OLS estimation. Counties with a yearly maximum of fewer than 10 loans are dropped. Errors clustered at county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.15 Loan Quality Within CDC

	(1) CrSc	(2) CrSc	(3) CrSc	(4) Curr	(5) Curr	(6) Curr
New CDC (to county)	-0.0528***	-0.0481**	-0.0370*	-0.0137	-0.00913	-0.0103
	(0.0197)	(0.0200)	(0.0201)	(0.0119)	(0.0115)	(0.0118)
Yr/CntyIndicators		X	X		X	X
CntyControls			X			X
N	15601	15601	15601	15601	15601	15601
R^2	0.00126	0.0345	0.0374	0.00635	0.0564	0.0589

OLS estimation. Counties with a yearly maximum of fewer than 10 loans are dropped. Errors clustered at CDC level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.16 Loan Quality within CDC (Measured by Loan Default) - Logit

	(1)	(2) Cnty	(3) Cnty	(4) CDC	(5) CDC
Current					
New CDC (to county)	0.918	0.935	0.927	0.943	0.940
	(0.0597)	(0.0606)	(0.0667)	(0.0692)	(0.0709)
Yr/CntyIndicators		X	X	X	X
CntyControls			X		
N	15601	15397	15397	15381	15381
R^2					

Logit estimation. Ceofficients are logit odds ratios. Counties with a yearly maximum of fewer than 10 loans are dropped. Errors clustered at county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.17 Competition within CDC (Measured by Certified Lending)

	(1)	(2)	(3)
PostXLogCntCDC	-0.0220	0.113**	0.0879**
	(0.0455)	(0.0552)	(0.0434)
LogCntCDC	0.0652	0.00288	0.00657
	(0.0545)	(0.0340)	(0.0340)
LoanAmt & Yr/CntyIndic.		X	X
CntyControls			X
N	41509	41509	41509
R^2	0.006	0.289	0.292

OLS estimation. State with outlier no. of CDCs and counties w/ <300 firms of size 5-50 in year 1996 are dropped. Weighted by loan count at county-year. Errors clustered at state level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.18 Competition as Measured by Certified Lending (Logit)

	(1)	(2)	(3)
Certif.Loan PostXLogCntCDC	0.793	8.799***	6.164***
	(0.317)	(5.780)	(2.529)
LogCntCDC	1.897	0.675	0.777
	(0.830)	(0.431)	(0.430)
LoanAmt & Yr/CntyIndic.		X	X
CntyControls			X
N	41509	16361	16361
R^2			

Coefficients are logit odds ratios. State with outlier no. of CDCs and counties w/ <300 firms of size 5-50 in year 1996 are dropped. Weighted by loan count at county-year level. Errors clustered at state level. Obs. loss is due to county FE. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.19 County Growth

	(1)
LogCountCDC	0.0013
	(0.001)
Cnty/YrIndicators	X
CntyControls	X
N	6826
R^2	0.20

State with outlier no. of CDCs and counties w/ <300 firms of size 5-50 in 1996 are dropped. Errors clustered at state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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Chapter 2: Job Creation, Small vs. Large vs. Young, and the SBA

Abstract

Using a linked database based on a list of all Small Business Administration (SBA) loans in 1992 to 2011 and annual information on all U.S. employers from 1976 to 2012, we apply detailed matching and regression methods to estimate the variation in SBA loan effects on job creation across firm age and size groups. The firm-level proportional impact of loan receipt is estimated to fall with pre-loan firm size and age, and is largest for start-ups and very young and very small firms. The number of jobs created per million dollars of loans is also estimated to be highest for start-ups but otherwise generally increases with size and age. The estimated survival impact of loan amount is larger for smaller and younger firms.

2.1 Introduction

One of the few areas of recent consensus across all major political groups in the US is the allegedly important role played by small businesses in job creation. The initiatives justified by this conviction include a variety of small business loan and support programs, largely through the Small Business Administration (SBA), as well as preferential treatment of small businesses in contracting and regulatory requirements.[3] The empirical basis for the belief goes back to Birch (1987), although the underlying methods and data were questioned by Davis, Haltiwanger, and Schuh (1996). More recently, Neumark, Wall, and Zhang (2011) have reconfirmed, using improved data and methods, the Birch conclusion, but Haltiwanger, Jarmin, and Miranda (hereafter HJM, 2013) have shown that the size-growth relationship is not robust to controlling for age (as had Evans (1987) for a much smaller data set on manufacturing industries). Indeed, HJM find that the relationship may even reverse signs, so that larger firms contribute more to job creation, once age is taken into account.

This research has attracted considerable attention both from scholars and journalists, and it is very useful as an empirical description of the economy laying out the "facts" that may be juxtaposed against theories of firm and industry dynamics. HJM infer from their results that "to the extent that policy interventions aimed at small businesses ignore the important role of firm age, we should not expect much of an impact on the pace of job creation." (p. 360) Strictly speaking, this inference requires the assumption that the patterns of responsiveness of employment to interventions across different categories of firms (defined by age and size) mimic the empirical regularities of

[3] A recent example is the JOBS Act, which loosens regulations on financing.

employment dynamics in these categories more generally. While it might be the case that the categories with the strongest record of job creation also respond the most to a given intervention, it is also possible that there is no relationship. Potentially, the types of firms that typically create the fewest jobs might even benefit the most from supportive measures. In general, empirical regularities have no necessary implications for the design of effective interventions.

This paper attempts to shed light on variation by age and size in the responsiveness of employment to interventions in financial access. The treatment we examine is the SBA's 7(a) and 504 loan guarantee programs. For this purpose, we have linked a complete list of SBA 7(a) and 504 loans to the Census Bureau's employer and non-employer business registers and to the Longitudinal Business Database (LBD), which tracks all firms and establishments in the U.S. non-farm business sector with paid employees on an annual basis in 1976-2012. We restrict the analysis to recipients of loans in 1992-2010 and their matched controls.

While our paper is inspired to some extent by HJM and we use some of the Census data they developed, our question and therefore our methods are somewhat different. While HJM measure year-to-year growth in employment, our focus is the change in level of employment from the period before the SBA loan to the period after the loan is received. Our estimation method involves construction of a control sample of firms in the same age-size class. We also impose the following "exact matching" requirements: employment within 10 percent of the recipient's employment in the year of loan receipt, the same four-digit industry, and the same state (the latter only for firms with 19 or fewer employees). Probit regressions for loan receipt are run on this set of recipients and potential controls, including employment in the year before loan receipt, annual employment growth three, two, and one year before loan receipt, average wage in the year before loan receipt, relative productivity in the year before loan receipt, firm age, and year dummies. A non-treated firm is included as a control for a particular treated firm if the ratio of the treated to the non-treated firm's propensity score is at least 0.9 and not more than 1.1. Kernel weights are applied to the controls in subsequent employment and survival regressions.

The employment panel regressions include post-loan period effects (common to the treated firm and its matched controls), year effects. One set of specifications includes firm fixed effects, and another omits them. In one specification, the dependent variable is the log of employment and a post-loan (treatment) dummy for the treated firm is the variable of interest. In other specifications, we use unlogged employment as the dependent variable and examine the results with a dummy treatment and the amount of the loan. We also study the dynamics of the effect by including separate dummies for each year before/after the treated firm's loan receipt. To study survival we estimate both Cox-Proportional Hazard models and 3-year and 10-year survival probits, including treatment year and sector effects along with a treatment dummy and loan amount, in alternative specifications.

The rest of the paper is structured as follows. Section 2 describes the SBA programs we analyze. Section 3 describes the data, including the matched control samples. Section 4 outlines our methodology. Section 5 provides employment estimation results, Section 6 provides survival estimation results, and Section 7 concludes.

2.2 SBA Loan Programs

The SBA has several small business loan guarantee programs. In this paper, we focus on the largest two categories of programs, 7(a) and 504, and this section briefly describes their current characteristics.[4]

SBA 7(a) loans not part of a special subprogram can be for an amount up to $5 million, with a maximum 85 percent SBA guarantee for loans up to $150,000, and a 75 percent maximum guarantee for higher amounts.[5] They are term loans that can be used for expansion/renovation; new construction; purchase of land, buildings, equipment, fixtures, and lease-hold improvements; working capital; debt refinancing for compelling reasons; seasonal line of credit; and inventory. Maturity depends on the ability to repay. Usually loans for working capital and machinery (not to exceed the life of equipment) have a maturity of 5-10 years, while loans for purchase of real estate can have a term up to 25 years. The SBA sets maximum loan interest rates, which decrease with loan amount and increase with maturity. Since December 8, 2004 SBA has charged a guaranty fee, which increases with maturity and loan amount.[6] To qualify, a business must be for-profit; meet SBA size standards;[7] show good character,[8] management expertise, and a

[4] This section draws heavily on Brown and Earle (2013).

[5] See http://www.sba.gov/content/7a-terms-conditions.

[6] The SBA subsidized the fee between 2009 and 2011 using stimulus program funding.

[7] The cut-offs for being a small business vary by NAICS industry. In some industries the criterion is the average number of employees, with a cut-off ranging from 50 to 1,500. In other industries it is average annual receipts, ranging from $750,000 to $35.5 million. For many types of financial institutions, the cut-off is $175 million in assets. See http://www.sba.gov/sites/default/files/files/Size_Standards_Table.pdf.

[8] The principals of each applicant firm provide a "Statement of Personal History", which the SBA uses to determine if they have shown a willingness and ability to pay their debts and abide by their community's laws. See http://www.sba.gov/content/standard-7a-evaluation-criteria.

feasible business plan; not have funds available from other sources;[9] and be an eligible type of business.[10] The SBA itself makes the final credit decisions for these loans.

Some 7(a) programs are more streamlined. Unlike with other 7(a) loans, in the 7(a) Preferred Lender Program (PLP) the SBA delegates the final credit decision and most servicing and liquidation authority to PLP lenders.[11] The SBA's role is to check loan eligibility criteria. The SBA selects lenders for PLP status based on their past record with the SBA, including proficiency in processing and servicing SBA-guaranteed loans. In payment default cases, the PLP lender agrees to liquidate all business assets before asking the SBA to honor its guaranty.

In the 7(a) Certified Lender Program (CLP), the SBA promises a loan decision within three working days on applications handled by CLP lenders.[12] Rather than ordering an independently conducted analysis, the SBA conducts a credit review, relying on the credit knowledge of the lender's loan officers. Lenders with a good performance history with SBA loans may receive CLP status.

SBA 7(a) Express loans have a $350,000 maximum loan amount and 50 percent maximum SBA guaranty.[13] Interest rates can be higher than on other 7(a) loans.

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[9] A review is made of both business and personal financial resources. When these resources are deemed excessive, the business is required to use them in place of part or all of the requested loan proceeds. See http://www.sba.gov/content/standard-7a-evaluation-criteria. In the lender's application for an SBA guaranty, the lender must sign the following statement "Without the participation of SBA to the extent applied for, we would not be willing to make this loan, and in our opinion the financial assistance applied for is not otherwise available on reasonable terms." See http://www.sba.gov/sites/default/files/SBA%20FORM%202301%20B.pdf. In practice, the lender's refusal to give the applicant a conventional loan is normally considered sufficient to meet this requirement.

[10] This includes engaging in, or proposing to engage in, business in the United States or its possessions; possessing reasonable owner equity to invest; and using alternative financial resources, including personal assets, before seeking financial assistance. See http://www.sba.gov/content/7a-eligibility.

[11] See http://www.sba.gov/content/steps-participating-plp.

[12] See http://www.sba.gov/content/steps-participating-clp.

[13] See http://www.sba.gov/content/become-express-lender and http://www.sba.gov/sites/default/files/files/

Qualified lenders may be granted authorization by the SBA to make eligibility determinations. The SBA promises a decision within 36 hours.[14]

The 504 Loan Program offers loan guarantees up to $5 million or $5.5 million, depending on the type of business.[15] Typically a lender covers 50 percent of the project costs without an SBA guarantee, a Certified Development Company (CDC) certified by the SBA provides up to 40 percent of the financing (100 percent guaranteed by an SBA-guaranteed debenture), and the borrower contributes at least 10 percent (the borrower is sometimes required to contribute up to 20 percent). CDCs are nonprofit corporations promoting community economic development via disbursement of 504 loans. Proceeds may be used for fixed assets or to refinance debt in connection with an expansion of the business via new or renovated assets. For-profit businesses with tangible net worth of no more than $15 million and average income of no more than $5 million after federal income taxes in the two years prior to application are eligible. Businesses must create or retain one job per $65,000 guaranteed by the SBA, with the exception of small manufacturers, which must create or retain one job per $100,000.

2.3 Data

We use a database on all 7(a) and 504 loans guaranteed by the SBA from inception in 1953 through 2011 to identify loan recipients, amounts, and time of receipt. We convert the SBA-approved loan amounts to real 2010 prices using the annual average

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Loan%20Chart%20Baltimore%20June%202012%20Version%202.pdf.

[14] There are several other smaller 7A programs not described here. See http://www.sba.gov/sites/default/files/files/Loan%20Chart%20Baltimore%20June%202012%20Version%202.pdf.

[15] See http://www.sba.gov/content/cdc504-loan-program.

Consumer Price Index from the Bureau of Labor Statistics.[16] Loan timing is based on the date the SBA approved the loan. In order to exclude any firms receiving a disaster loan before their first 7(a) or 504 loan from the analysis, we also use a database on all SBA disaster loans from inception through 2009.

We have matched the SBA 7(a), 504, and disaster loan data to the Census Bureau's employer and non-employer business registers. We use the following passes: The first is an exact match on Employer Identification Number (EIN). For those observations unmatched after this pass, the second pass is an exact match on 5-digit zip code, exact match on standardized street address, and exact match on standardized business name. The third pass is an exact match on 3-digit zip code, a standardized street address soundex (phonetic algorithm), and an exact match on standardized business name. The fourth pass is an exact match on 5-digit zip code, all of street address allowing for some fuzziness (70 percent sensitivity in SAS's DQMATCH software), and business name allowing for some fuzziness. The fifth pass is an exact match on 5-digit zip code and business name allowing for some fuzziness; and the sixth pass is place (city), business name allowing for some fuzziness, street name allowing for some fuzziness, and street number allowing for some fuzziness. A match from the first pass is prioritized over the second pass, which is prioritized over the third pass, etc. In a first series of passes, the SBA data are matched to business registers from the same year as the loan. Then they are matched to business registers in the subsequent year, and finally to business registers in the previous year.

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[16] This can be downloaded from ftp://ftp.bls.gov/pub/special.requests/cpi/cpiai.txt.

The Census Bureau's Longitudinal Business Database (LBD) consists of longitudinally linked employer business registers. The LBD tracks all firms and establishments in the U.S. non-farm business sector with paid employees on an annual basis in 1976-2012. The SBA loan match to employer business registers allows us to link the SBA data to the entire LBD. The LBD contains employment as of the pay period including March 12th, annual payroll, state, county, zip code, and industry code. The industry code is a four-digit SIC code through the year 2001 and a six-digit NAICS code in 2002-2012.

Following HJM (2013), we define firm birth as the first year any of the firm's establishments have positive employment. We assign firms to the county with the largest share of the firm's employment. We define firm industry as the modal industry, and ties are broken based on employment shares. For loan recipients with more than two consecutive years of inactivity post-start-up and prior to SBA loan receipt, we exclude the firm-years prior to the period of inactivity. If a loan recipient has more than two consecutive years of inactivity after loan receipt, we exclude the firm-years after the period of inactivity. Nonrecipient firms with more than two consecutive years of inactivity at any point are completely excluded. There is some question as to whether the entities before and after the inactivity are really the same firms.

Since our interest is organic job creation, we adjust firm employment for establishment ownership changes. When a firm purchases a pre-existing establishment, the establishment's employment in the year preceding the change is subtracted from the firm's employment in all subsequent years. Analogously, we add sold establishment employment in the year prior to sale to the firm's employment in all subsequent years, in cases where establishments are sold and continue to operate.[17]

We define employment outliers as instances where employment rises or falls by more than ten times between the start-up and second year or between the penultimate and final year; or where employment rises or falls by more than five times between interior years and in the immediate subsequent year falls or rises by more than five times. Non-treated firms with any employment outliers are excluded. Treated firm employment is set to missing in years with outliers, but their other years are kept in the analysis (provided the outlier is not in the year before the loan, which is critical for the matching process). The rationale is that we wish to keep as many treated firms in the analysis as possible so as to minimize bias from examining only a subset of treated firms. Since there are ample control firms for most treated firms, there is less cost to entirely excluding potential control firms with outliers.

As shown in Table 1, 87 percent of the SBA 7(a) and 504 loans have been matched to establishments in the business registers. Of these, 90 percent are found in the employer registry and thus in the LBD. Among firms receiving multiple SBA loans, we select the first 7(a) or 504 loan as the treatment.[18] We drop firms ever receiving a SBA disaster loan and those receiving their first 7(a) loan prior to 1992.[19] Our identification method for non-start-ups relies heavily on the value of employment in the year prior to

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[17] This procedure is analogous to HJM's inclusion of year-to-year growth of acquired establishments in the acquisition year, but in our analysis of multi-year pre- and post-loan periods, we need to make adjustments to measure organic growth over the entire observable firm life-cycle.

[18] We limit the analysis to the first loan, as subsequent SBA loans could be influenced by the first loan's effect.

[19] We choose to start the analysis in 1992 in order to measure firm age reliably (our oldest category is 11+ years) given that the LBD starts in 1976.

loan receipt, so we drop continuing firms missing this value in the LBD. We also drop firms receiving loans prior to the start-up year. Such firms could have differing lengths of history as nonemployers, and ideally nonemployer data should be used to match loan recipients and non-recipients based on that history, a task which we set aside for future research. Finally, we drop firms for which no suitable controls are found, according to our matching procedures. Table 1 reports the number of loans dropped as a result of each of these restrictions.

Following HJM and other analyses of size-age variation in firm growth, we form size-age categories. Only a tiny fraction of SBA loan recipients have more than 249 employees in the loan year, so we restrict attention to firms up to this threshold, with the following groupings: 1-4, 5-19, 20-49, 50-99, and 100-249 employees. As we show below, SBA recipients also tend to be young firms, and we group years of age as follows: 0 (start-up), 1-3, 4-10, and 11+.[20] We estimate separate effects for the 16 size-age groups defined as the intersection of these categorizations. As discussed in the next section, start-ups require a separate matching process (because of the lack of available history for matching), but they are also of special interest in light of the HJM findings on their great importance in job creation. Among the 15 non-start-up groups, the 1-3 year-old age category is of special interest, representing the "valley of death" – the period of high mortality among firms in their first few years. The 11+ age category corresponds to "mature" firms.

[20] Start-up is defined as entry into the LBD, implying positive employment, and therefore employment in start-up firms is by definition zero in the year prior to start-up.

We next turn to a description of the SBA loan recipients by age, size, and growth in comparison to non-recipients in the LBD. As discussed in Brown and Earle (2013), remarkably little is known about what types of firms get SBA loans and how recipients compare to non-recipients, so these results may be of broader interest to anyone interested in SBA programs.

Table 2 shows the number of loan recipients in the LBD that fall into each of the 16 age-size categories. The number declines monotonically with both age and size, and the youngest continuers (age 1-3) with the largest size (100-249 employees) is a particularly small cell (424 firms), suggesting particular caution in the interpretation of the results for this group.

How does the age-size distribution of recipients compare with non-recipients? Table 3 shows the empirical probability of receiving an SBA loan in a particular year. For the sample as a whole, the probability is 0.45 percent, and for start-ups the probability is 0.74 percent. Probabilities decline monotonically in age, but not in size: They nearly double from the 1-4 employee category to the 5-19 category and continue to rise to the 20-49 category (where they reach one percent for the age 1-3 and age 4-10 groups) before falling back somewhat for the largest two groups. For every age group, the probability of receiving an SBA loan is higher for the 100-249 size group than for the 1-4 employee group, and except for those age 1-3 the probability is at least double. Thus, SBA loans are in practice allocated towards start-ups and younger firms but not necessarily towards the smallest size groups among the small to medium sized firms that receive the loans.[21]

Brown and Earle (2013) report that SBA-recipients differ systematically from typical firms in several other ways, including the pace of growth prior to loan receipt. Hurst and Pugsley (2011), for example, report that most small firms grow very little, entering at low employment levels and tending to stay there. To examine growth for the categories used in this paper, Table 4 tabulates the pre-loan growth for the SBA recipient sample restricting attention to firms at least 4 years old to calculate growth from 4 years before the loan to one year prior to the loan. For comparison, Table 5 contains the analogous computation for all non-recipients in the LBD. The mean 3-year pre-loan growth rate is higher among SBA recipients for every age and size group than it is for the corresponding groups across all 3-year periods in the LBD. Mean 3-year growth among SBA recipients is 0.136, while it is 0.004 among non-recipients (the corresponding medians are 0.051 and 0.000, respectively). Thus, while these results support Hurst and Pugsley's (2011) contention about typical small firms, they imply that SBA firms belong to the atypical subset of small firms that tend to grow strongly, even prior to loan receipt. Together with the other factors differentiating recipients and non-recipients, this result suggests the importance of conditioning on prior growth. Below, we outline a matching approach to estimation where comparisons are carried out within this growing subset.

All of this analysis so far is conditional on survival. But SBA loan receipt may also affect exit behavior, which we discuss in a separate section below.

[21] Mean employment in the year prior to the loan is 16.6 for SBA recipients, while it is 22.0 across all firm years in the LBD. The corresponding medians are 8 and 16, respectively.

Finally, the analysis so far has implicitly treated SBA loan receipt as a binary treatment. As shown in Brown and Earle (2013), however, SBA loan amounts vary substantially. Table 6 displays mean loan amounts for the age-size categories we use in this paper. Loan amounts increase monotonically in both age and size, except that start-ups receive slightly large loans than non-start-ups in the 1-4 employee category. While the grand mean across all SBA loans is $421,267 (in 2010 USD), the mean amount for the smallest size group is about half that, and it is more than three times that size for the largest size group. This suggests the value of estimating the effect not only of loan receipt but also of loan amount, a specification we discuss in the following section.

2.4 Estimation Strategy

Estimating the causal effect of SBA loan receipt on employment faces typical identification challenges. Let $TREAT_{it} \epsilon \{0,1\}$ indicate whether firm *i* receives an SBA loan in year *t*, and let y^1_{it+s} be employment at time *t*+*s*, $s \geq 0$, following loan receipt. The employment of the firm if it hadn't received a loan is y^0_{it+s}. The loan's causal effect for firm *i* at time *t*+*s* is defined as $y^1_{it+s} - y^0_{it+s}$. The value of y^0_{it+s} is not observable, however. We define the average effect of treatment on the treated as $E\{y^1_{t+s} - y^0_{t+s} | TREAT_{it} = 1\} = E\{y^1_{t+s} | TREAT_{it} = 1\} - E\{y^0_{t+s} | TREAT_{it} = 1\}$. A counterfactual of the last term, i.e., the average employment outcome of loan recipients had they not received a loan, can be estimated using the average employment of non-recipients, $E\{y^0_{it+s} | TREAT_{it} = 0\}$. This approximation is valid as long as there are no uncontrolled contemporaneous effects correlated with loan receipt. To help control for such contemporaneous effects, we use matching techniques to select a control group.

For this purpose, we have taken the following steps. As mentioned in Section 3 above, we limit our treated sample to firms in the LBD receiving their first SBA 7(a) or 504 loan in 1992-2011 and those not receiving a SBA disaster loan prior to their first 7(a) or 504 loan. To be eligible to be a candidate control firm for a particular treated firm, a firm can never have received an SBA 7(a), 504, or disaster loan at any time between 1953-2009; it must be in the same four-digit industry (this is the four-digit SIC code through 2001 and the first four digits of the NAICS code in 2002-2012) in the treated firm's loan receipt year, and be in the same firm age category (as defined above) in the treated firm's loan receipt year. For non-start-ups, the control must have non-missing employment in the year prior to the treated firm's loan receipt. Among firms with 19 or fewer employees in the previous year, we also require the candidate control firm to be located in the same state (firms with 1-19 employees are much more numerous than ones with more than 19 employees, so we can afford to impose more restrictions on this group). In addition, for non-start-ups we impose a restriction that the ratio of the treated firm's employment in the previous year to the control firm's previous year employment be greater than 0.9 and less than 1.1. This means that among firms with nine or fewer employees, employment must match exactly.

For the non-start-ups, we would also like to match on variables representing the history of employment prior to treatment year, but it is difficult to design matching thresholds for each variable separately, so we reduce this dimensionality problem with propensity score matching. We estimate separate probit regressions using the sample of treated firms and their candidate controls (according to the exact matching criteria) for different age-size categories.[22] For each of the 15 size-age categories defined above for non-start-ups, a dummy for SBA 7(a) or 504 loan receipt is regressed on the log of employment in the year prior to the treated firm's loan receipt; the square of the log of employment in the year prior to the treated firm's loan receipt; the log of employment one year before minus log employment two years prior to the treated firm's loan receipt; the log of employment two years before minus log employment three years prior to the treated firm's loan receipt; the log of employment three years before minus log employment four years prior to the treated firm's loan receipt; the log of payroll/number of employees in the year prior to the treated firm's loan receipt; firm age; firm age squared; and year dummies. We also include dummies for missing values for the log of employment two years before minus log employment three years prior to the treated firm's loan receipt, the log of employment three years before minus log employment four years prior to the treated firm's loan receipt, and the log of payroll/number of employees in the year prior to the treated firm's loan receipt.[23]

The treated firm observations in the probit regressions are each assigned a weight of $\frac{(N-R)}{R}$, where N is the total number of firms in the regression and R is the number of treated firms in the regression. The non-treated firms are assigned a weight of 1. This equalizes the total weight of the treated firm and non-treated firm groups. The purpose of this weighting is to produce propensity scores that span a wider range, centered around 0.5 rather than near zero.

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[22] Treated firms with no candidate controls are dropped at this point.

[23] When a firm has a missing value for one of these variables, a zero is imputed.

We limit the treated and non-treated firms in the employment regression analysis to those within a common support, meaning that no propensity score of a treated (non-treated) firm that we use is higher than the highest non-treated (treated) firm propensity score, and no propensity score of a treated (non-treated) firm that we use is lower than the lowest non-treated (treated) firm propensity score. A non-treated firm is included as a control for a particular treated firm if the ratio of the treated to the non-treated firm's propensity score is at least 0.9 and not more than 1.1. Treated firms with no controls meeting all these criteria are not included in the employment regression analysis. Non-treated firms appear in the employment regressions as many times as they have treated firms to which they are matched (i.e., this is matching with replacement). Kernel weights are applied to the controls.[24] In the employment regressions, each control is assigned a final weight of their kernel weight divided by the sum of the kernel weights for all controls for a particular treated firm, and the treated firm is given a weight of 1. As a result, the treated firm and all its control firms together receive equal weight.

Propensity score matching relies on a strong assumption of "selection on observables." Since our data are longitudinal, for the non-start-ups we are also able to eliminate unobserved, time-invariant differences in employment through difference-in-differences (DID) regression specifications.

The employment regression specifications take the following form:

$$y_{ijt} = L_{ijt}\gamma + \boldsymbol{\rho_t} + \alpha_{ij} + \boldsymbol{\theta_{it}\delta} + u_{ijt},$$

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[24] The kernel weight is $1 - \left(\frac{abs\left(\frac{propensity\ score_{tr}}{propensity\ score_{ntr}} - 1\right)}{0.1}\right)^2$, where tr is a subscript for the treated firm, and ntr is a subscript for the non-treated firm.

where y is employment (logged or unlogged, in alternative specifications), i indexes firms from 1 to I, j indexes from 1 to J the treated firms to which the firm is a control,[25] and t indexes the years from 1 to T. $\boldsymbol{L_{it}}$ is a dummy common to treated firms and their matched controls equal to one in all years following the treated firm's loan receipt. ρ_t is a vector of year dummies, α_{ij} is a fixed effect for each firm i which also varies for controls matched to different treated firms j, and u_{ijt} is an idiosyncratic error.[26] In alternative specifications, y_{it} is the firm's employment and the natural logarithm of the firm's employment.

$\boldsymbol{\theta_{it}}$ is a vector of SBA loan treatment measures, and $\boldsymbol{\delta}$ are the loan treatment effects of interest. We estimate several alternative specifications of $\boldsymbol{\theta_{it}}$. The simplest specifications include a post-loan dummy, which for treated firms is equal to 1 in the year after receipt of the first SBA loan and in all subsequent years. Others include the post-loan dummy interacted with the amount of the first SBA loan, expressed in $millions.[27] Some specifications include the loan amount, and some also include the loan amount squared. We also estimate dynamic specifications including two sets of dummy variables for the years before and after first SBA loan receipt, one of which is common to both treated firms and their matched controls (replacing the $\boldsymbol{L_{it}}$), and the other specific to the treated firms (they are always zero for controls).

For firms receiving an SBA loan at start-up (during the first year of positive employment in the LBD or the prior year), the matching procedures involve only exact

[25] For treated firms, i=j.
[26] The standard errors are cluster-adjusted by firm. We have also bootstrapped some specifications, and the standard errors are similar to those reported here.
[27] If a firm received multiple SBA loans in that first year, the loan amounts are combined.

matching on industry, year, age (start-ups are matched only with start-ups), and county – a more exact geographic match because of the large volume of entry. This limitation, which is forced by the lack of history and thus observably *sui generis* nature of start-ups, somewhat reduces our confidence in their results compared to those for continuing firms, but we include them because of the considerable interest in their role in job creation.

Table 7 shows the resulting numbers of treated firm observations for the age-size categories used in this paper. Overall, about 83 percent of SBA recipients identified in the LBD are able to be matched. The ratio does not differ greatly across age-size groups, except for a somewhat lower match rate among start-ups. For the non-start-ups, there are typically several years of data on each treated and control firm before and after treatment, the former facilitating control for pre-treatment differences, and the latter allowing us to study long-run treatment effects.

Table 8 shows the numbers of treated firms, combinations of control firm and treated firms, pre-treatment and post-treatment firm-years for treated firms, and pre-treatment and post-treatment years for control firm-treated firm combinations. On average there are several years of data on each treated and control firm before and after treatment, the former facilitating control for pre-treatment differences, and the latter allowing us to study long-run treatment effects.

The reliability of propensity score matching depends on whether, conditional on the propensity score, the potential outcomes y^1 and y^0 are independent of treatment incidence. The assumption of independence conditional on observables depends on the pre-treatment variables being balanced between the treated and control groups. We assess this in two ways – by performing a standardized difference (or bias) test for the main variables included in the matching probit regressions, and by analyzing the pre-treatment event-time dynamics (see Section 5). Table 9 reports the means of the main variables included in the matching probit regressions for four different samples: all treated firms, all non-treated firms, treated firms included in the employment regressions, and controls included in the employment regressions. Treated firm employment is larger and age is younger than for non-treated firms prior to matching, and treated firms experience more employment growth in the four years prior to treatment. After matching, these differences are negligible. The standardized difference measures confirm this: employment and employment growth, and age biases are reduced by over 92 percent.[28] None of the biases are close to being large after matching.[29]

We also investigate variation in the impact of SBA loans on the probability of exit by size-age group. Our method is to use the matched samples, selected as defined above, to estimate proportional hazard functions including SBA loan receipt and amount. A hypothesis of particular interest is whether SBA loans reduce exit during the high mortality period of the "valley of death."

2.5 Empirical results

2.5.1 Employment Growth Estimates

We organize the results by starting with estimates of the average effects of SBA loan receipt on employment in our sample. Next we present estimates of heterogeneous effects across size-age groups, and then estimates where we consider heterogeneity for either size or age, separately, and how they differ from the interacted groups. Our

[28] The mean age is very similar in the total treated and total non-treated samples, leaving little scope for improvement through matching.

[29] Rosenbaum and Rubin (1985) consider a value of 20 to be large.

motivation here is that decision-makers may have more reliable information on firm size than age (because age is more easily manipulated), so it is useful to know whether conditioning loans only on size reduces the efficiency of loan allocation. A subsequent section presents estimates for exit and survival.

The estimates of average effects across the whole matched sample, not differentiating by size-age categories, are shown in Table 10. The eight specifications differ in the definition of the dependent variable (logged vs. unlogged employment), the right-hand-side variable of interest (binary treatment dummy vs. loan amount in 2010 USD), and whether firm fixed effects (FE) are included or not. The logged specification is more natural with a binary treatment and the unlogged with loan amount.

When log employment is the dependent variable, in Column (1) of the table, the OLS estimate implies that a loan is associated with approximately a 21 percentage point increase in employment on average across years following loan receipt, while the FE estimate is slightly lower at about 16 percent.[30] The OLS and FE estimates with unlogged employment as the dependent variable show a similar relationship with 3 jobs created by a loan under OLS and 2.5 with FE. Whether these differences reflect the attenuation in estimates with FE (either because of genuine heterogeneity or measurement error in the SBA data) or a larger effect among the start-ups (which contribute to the OLS but not the FE estimates) is unclear, but we will analyze the latter possibility when we consider heterogeneity of the effects across age-size groups.

Columns (3) and (4) focus on effects of the loan amount, measured in 2010 USD. In one case, we exclude the post-loan dummy and in the other case we include it, but the

[30] These results are slightly lower than reported in Brown and Earle (2013), who use a different sample.

implied effects of amount are similar: 15-16 jobs in the OLS and about 6 in the FE specification, with the latter result similar or slightly larger than reported by Brown and Earle (2013). While the OLS result for the post-loan dummy in Column (4) is large and negative, it is small in the FE result. One interpretation of this finding is that selection into the loan program does not appear, by itself, to raise employment, which is instead affected by the amount of the loan.

As a further specification check and to examine long- versus short-run consequences of the loans, Figures 1-6 show estimated dynamics of the employment effects, where the coefficient on SBA loan receipt is permitted to vary in event time around the in the start-up year. Figures 1 and 2 contain continuing firm OLS and FE results, respectively, with logged employment as the dependent variable, Figures 3 and 4 are the same with unlogged employment, and Figures 5 and 6 are results for start-ups only.

The logged employment specifications (Figure 1 and 2) show similar or slightly higher treated firm employment relative to their matched controls 2-4 years prior to loan receipt, but both have an insignificant difference in the year prior to receipt. A sharply positive effect appears in the loan year through two years after loan receipt, and this gap gradually widens to 20 log percentage points by ten years after loan receipt. In the unlogged employment dynamic specifications (Figures 3 and 4), post-start-up loan recipients are similar (OLS) or slightly larger (fixed effects) than their matched controls on average prior to loan receipt. Treated firm employment increases relative to that of controls by 2 to 2.5 jobs by two years after loan receipt, and it gradually increases to 2.5-3.5 jobs ten years after treatment. The insignificant employment differences in OLS regression pre-treatment period results and the similar post- vs. pre-treatment differences to those produced by fixed effects regressions suggest that the OLS specifications may be able to produce valid treatment effect inferences despite not controlling for time-invariant heterogeneity across treated and control firms.

Start-up loan recipients begin with slightly higher employment than their controls when measured as log employment (Figure 5), then a sharp gap forms between treated and control firms in the year following loan receipt. By ten years after start-up, the gap reaches 30 log percentage points higher than in the start-up year. When using unlogged employment (Figure 6), a gap of about two employees forms in the first year after loan receipt, and it widens to four employees ten years after loan receipt. Having a loan may enable firms to start up at a larger scale, or alternatively that firms planning to start up at a small scale may have less desire for financing. Since we do not have pre- start-up information about the firms, we are unable to distinguish between these two possibilities.

Turning to heterogeneity in estimated SBA loan effects across age-size groups, we first focus on Columns (1) and (3) of Table 11, considering the FE specifications that pertain only to continuing firms, not new entrants. The log specification with a binary loan dummy is informative about the impact on a recipient firm of the loan, while the unlogged specification with loan amount is more appropriate for calculating overall job creation effects. Figures 7-10 contain the basic results, as does Table 11. Each pair of Figures (7 and 8, and 9 and 10, respectively) contains the same set of coefficients but graphed differently to more clearly show the age and size profiles. The size profiles in Figure 7 are downward sloping for each age group, implying that the proportional impact of loan receipt is greater for smaller firms. The age profiles in Figure 8, however, are strongly downward sloping only for the smallest size group (1-4 employees) and mildly downward sloping for the next smallest (5-19 employees).

Figure 9 shows the size profiles for the unlogged employment specification with loan amount as the variable of interest. By contrast with the logged, dummy variable specification in Figure 7, the size profiles are upward sloping for older firms, but relatively flat for younger ones. The age profiles for loan amount in Figure 10 are downward sloping for the smallest firms, and the slope increases with size. In contrast to Figure 8, the effects are more diverse across size groups for older firms than they are for younger firms.

Thus, for the continuing firms in this analysis, if the main objective of the loan program is to affect firm behavior, then effects are biggest among small firms but have little variation with age, except for the smallest size group; the effect is estimated to be highest for the youngest and smallest group. However, if the main objective is job creation, then the number of jobs created for a given loan amount is increasing in firm size, a pattern that is most pronounced among older firms.

This analysis includes firm fixed effect and is valid for continuing firms only. If we instead estimate by OLS, then entrants can be included. Table 12 contains the results. In the logged, dummy specification, the SBA loan impact for entrants is estimated at 0.21, the highest among all categories except for the youngest, smallest continuers. In the unlogged, loan amount specification, the estimate is 8.2 (jobs per million loan dollars), again the second largest among the 16 age-size categories. These results provide some evidence that SBA loans may be particularly supportive of start-up job creation, though the inability to control for fixed effects limits the strength of this inference.[31]

To a decision-maker who may be interested in allocating loans where the impact is biggest, an important question concerns the observability of variables used in targeting. A problem with firm age is that it might be easily manipulated, for example by renaming and re-registering, even re-locating what is essentially the same company. Firm size may be manipulable through hiring decisions on the margin, but large changes in firm size are more difficult, although splitting up a large firm to make it eligible for small business preferences is hardly unheard of. If age is therefore less easily or reliably observed than size, then an important question is whether using information on size alone is at all useful, or if age is a crucial piece of information in determining which types of firms merit preferential support. The question is similar to HJM's analysis of the size-growth relationship with and without age controls. HJM report that controlling for age essentially eliminates the negative size-growth relationship found without age controls, and we can carry out a similar analysis for the effects of SBA loans.

The results with FE are shown in Figures 11 and 12. Figure 11 contains the logged, dummy variable specification and shows a negative relationship between size and loan effect that differs little depending on whether age controls are excluded or included. Figure 12 contains the unlogged, loan amount specification, and shows a positive

[31] Another caveat about the start-up analysis is that a firm's pre-treatment size is likely to influence its subsequent growth, but we are unable to match treated and control firms with pre-loan size information, which doesn't exist by definition. Recognizing that start-up employment is endogenous to loan receipt in the start-up year, we nevertheless match treated and control firms by start-up size (within +/- 10 percent, as done for continuers) as a robustness check. With that sample we obtain a higher postloan treatment coefficient in the log employment specification than that for the start-ups or any other category in Table 11; the loan amount coefficient in the unlogged employment specification is lower than that in Table 11, though still second highest among the age-size categories.

relationship between size and loan effect that is again little affected by the presence of age controls. Thus, while age is a particularly useful variable for the smallest size category (1-4 employees) and somewhat less so for the second smallest (5-19), the relationship between growth and size is generally little affected by including information on age. However, the size-growth relationship is very much affected by whether the focus is the proportional impact on individual firms or the employment gains from a particular volume of loans.

The analysis so far assumes no differences in survival rates between treated firms and controls, although the SBA frequently refers to business survival as a performance measure, and access to loans may well affect survival. The direction of the effect is not certain, however, because while more finance may help a business through hard times, the increased leverage and possible over-extension may create greater vulnerability. Nor is the measurement of survival unambiguous, and any disappearance from the database is classified as an exit. Great effort has been made to link establishments across time in the LBD, but less has been done with firm linking. We cannot always distinguish bankruptcy and other genuine shutdowns from buy-outs or reorganizations that lead to a change in the firm's identification code in the LBD. As some of these outcomes represent business failure, others reflect success, and some level of exit is a normal feature of a dynamic economy, the analysis of exit is thus also not as clear normatively as our analysis of employment effects.

With these qualifications in mind, we are nonetheless interested to ascertain the degree to which our results might be driven by exit effects. In Tables 14 and 15 (Figures 13-16) we re-estimate the specifications in Tables 11 and 12, adding one firm-year observation in the year after the firm's exit to each firm that exits prior to 2012, imputing the amount of employment in surviving establishments that had been under the firm's control in its final year. The value is zero if all the firm's establishments close.

The results for continuing firms in a FE specification can be seen in a comparison of Figures 7 and 8 to 13 and 14. The estimated effects of loan receipt on log employment are larger after accounting for exit among the larger firm sizes in the age 1-3 group, while they are diminished in the smaller size categories. The negative relationship of estimate loan impact with age is stronger in Figure 14 than Figure 8. The weak relationship with size remains. The results in the unlogged employment – loan amount specifications (Figures 15 and 16) are little changed. For the OLS specifications that permit estimates for start-ups, both the logged and unlogged specifications yield results in Table 15 that are very similar to those in Table 12, so taking into account survival has little implication for the start-ups. We study survival further in the next section.

2.5.2 Survival Estimates

In this section we estimate the effects of SBA loan receipt on firm survival using Cox-Proportional Hazard Models and probit regressions for short- (three-year) and longer-run survival. Again we examine the heterogeneity by size-age categories. The regressions include treatment dummies, loan amounts, treatment year dummies, and sector controls.

The control sample for these regressions is limited to the single nearest neighbor by loan receipt propensity score for each treated firm. Since propensity scores are not estimated for start-ups, we randomly select one of the treated firm's controls used in the employment analysis. Nearest neighbor matching is used here to avoid the need to weight the regressions, which is problematic for probit regressions.

We include only firm exits occurring within the examined time period (the full post-treatment period for the hazards and either three or ten years post-treatment for the probits) that have no surviving establishments (establishment sales to other firms) post-exit. Firms that exit via sale of their establishments are ambiguous from a performance perspective - some are cases where the entrepreneur is cashing in on past success.

To provide a baseline for the estimated effects, the three- and ten-year survival rates in the regression sample for each size-age category are reported in Tables 16 and 17. Average survival rates increase with both age and size.

Figures 17 and 18 (Table 18) plot the loan amount Cox-Proportional Hazard results, and Figures 19-22 (Tables 19-20) show the loan amount probit results. The effects per loan dollar decrease in both size and age. Loans thus have larger effects in size-age categories with higher exit rates. The effects are insignificant for the older, larger categories. Declining effects with size may be due to SBA loan amounts representing a much smaller share of larger firms' total capital requirements.

Loans have a larger effect on short-run than longer-run survival for start-up firms, suggesting that the loans are beneficial while the start-ups are in the "valley of death". The effects are higher over the longer period for nearly all the other age categories, though.

As with the employment results in the previous section, the size results are virtually unaffected by controlling for age, as shown in Tables 21-23 and Figures 23-25.

Finally, if we interpret the loan dummy coefficient in these specification with loan amount as reflecting selection into the loan program, then the results in Tables 18-20 imply negative loan selection effects for smaller firms in the Cox-Proportional Hazard regressions and older, small firms in the probit regressions. This may suggest that some categories are motivated to apply for an SBA loan in order to alleviate financial distress. Most of the dummy coefficients are small, however, so that, at the mean loan amount, the overall survival effect is positive.

2.6 Conclusion

Research on measures to support small businesses has been preoccupied with examining the basic proposition that small firms are disproportionate job creators. Although the proposition is practically an article of faith for many, Haltiwanger et al. (2013) have recently shown that firm size and growth are essentially uncorrelated once the analysis accounts for firm age, and systematically larger job creation only comes from new entrants and very young firms. Whatever the nature of the firm age-size-growth relationships, however, the existing research does not address either the question of whether programs have bigger impacts on employment in different types of firms – smaller, larger, younger, or older – or the question of whether and how job creation per dollar from the government varies across firms by size and age.

Somewhat surprisingly, our analysis finds different answers to these two questions in the context of the two largest loan guarantee programs of the Small Business Administration: the 7(a) and 504 programs. Our analysis matches firms with fewer than 250 employees receiving these loans to non-recipients that are essentially identical along every observable: pre-loan size, age, industry, year, and pre-loan growth history. We have more confidence in the results for continuing firms (non-start-ups at the time of the loan) than for entrants (firms starting up with an SBA-backed loan) because of our ability in the former case to match on pre-loan employment history and to control for fixed effects.

With respect to the sub-population of continuers, our results imply that the proportional impact of getting a loan on employment tends to be negatively related to firm size for all firm ages, while the impact is negatively related to firm age only in the smallest categories of firms (5-19 employees, and especially 1-4 employees). On the other hand, when we estimate the number of jobs created per million dollars of loans, we find a positive relationship with pre-loan firm size; it is also increasing with age for larger firms. Also different are the results for survival: we estimate that a given loan amount has a greater improvement in the survivability of the smaller and younger firms. Thus, depending on whether the desired outcome is to improve firm-level employment growth, to create the maximum number of jobs, or to increase firm survival, a different loan allocation across age-size groups produces larger effects.

With respect to start-up firms, interpreting the estimated loan coefficients as reflecting treatment impacts depends on stronger assumptions than for continuing firms. Essentially, every start-up is *sui generis* and we have no history or possibility of post-loan versus pre-loan comparison. We nevertheless find it noteworthy that the estimated impact on start-ups in an OLS regression is higher than for any of the other age-size groups save one. This result is consistent whether we focus on the proportional impact on the firm or on the number of jobs created per million dollars of loans.

Chapter 2, in part is currently being prepared for submission for publication of the material. Brown, David; Earle, John. The dissertation author was the primary investigator and author of this material.

2.7 Appendix

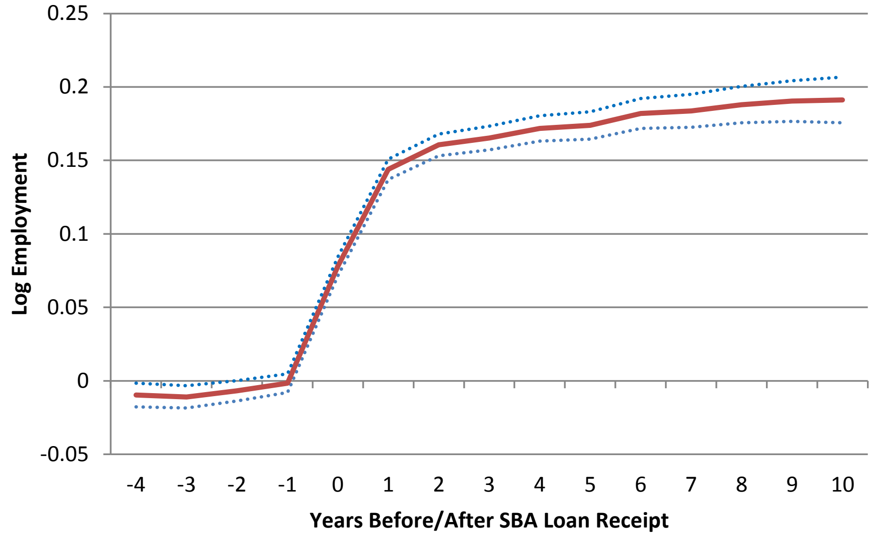

Figure 2.1 Dynamic Specification for SBA Loans After Start-Up, OLS

Note: OLS regression, also including dummies common to treated and control firms for years before/after the treated firm's treatment and year effects. Years prior to four years before treatment is the base category. Dummies for years after ten years after treatment are included, but not reported. The dotted lines are the boundaries of the 99 percent confidence interval.

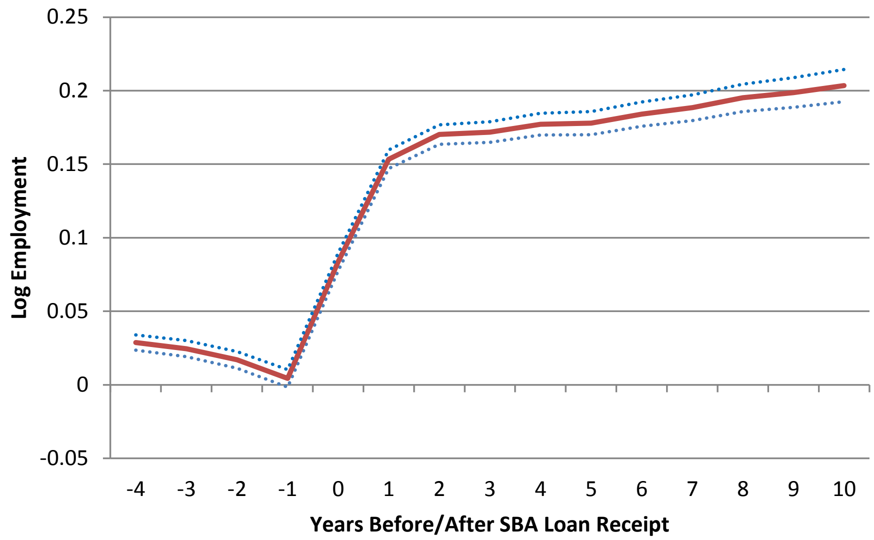

Figure 2.2 Dynamic Specification for SBA Loans After Start-Up, Fixed Effects

Note: fixed effects regression, also including dummies common to treated and control firms for years before/after the treated firm's treatment and year effects. Years prior to four years before treatment is the base category. Dummies for years after ten years after treatment are included, but not reported. The dotted lines are the boundaries of the 99 percent confidence interval.

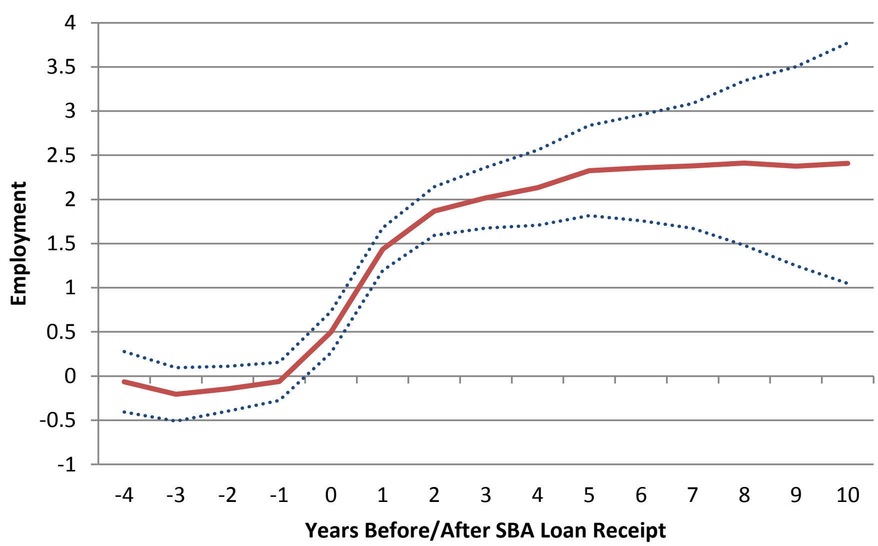

Figure 2.3 Dynamic Specification for SBA Loans After Start-Up, OLS

Note: OLS regression, also including dummies common to treated and control firms for years before/after the treated firm's treatment and year effects. Years prior to four years before treatment is the base category. Dummies for years after ten years after treatment are included, but not reported. The dotted lines are the boundaries of the 99 percent confidence interval.

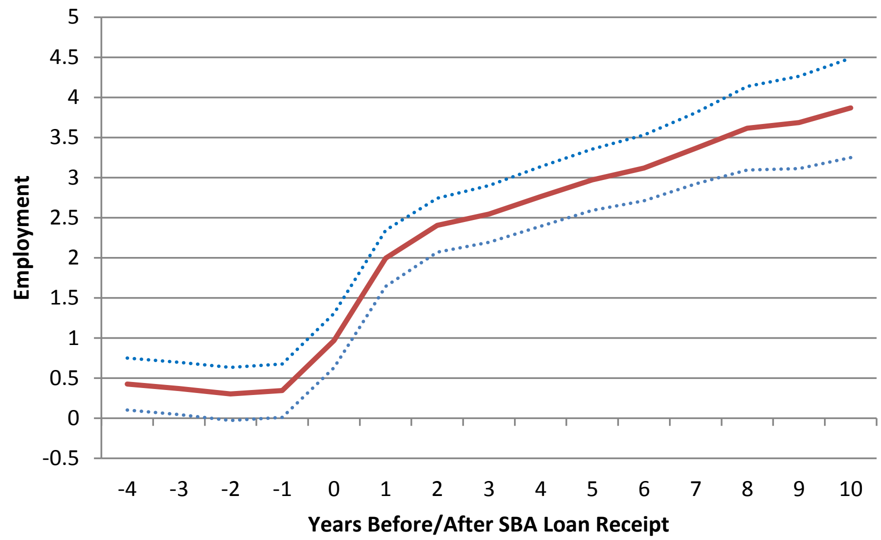

Figure 2.4 Dynamic Specification for SBA Loans After Start-Up, Fixed Effects

Note: fixed effects regression, also including dummies common to treated and control firms for years before/after the treated firm's treatment and year effects. Years prior to four years before treatment is the base category. Dummies for years after ten years after treatment are included, but not reported. The dotted lines are the boundaries of the 99 percent confidence interval.

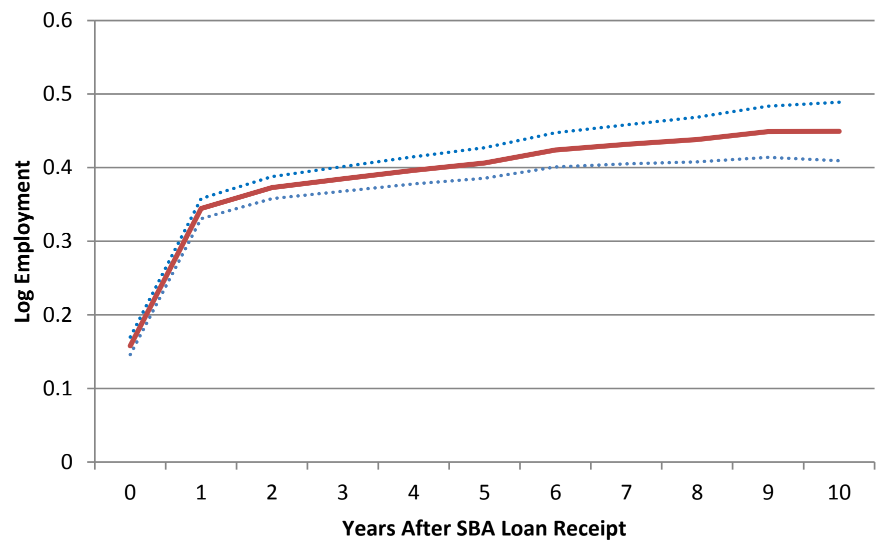

Figure 2.5 Dynamic Specification for SBA Loans in Start-Up Year

Note: OLS regression, also including dummies common to treated and control firms for years since the treated firm's treatment (which is also age) and year effects. The dotted lines are the boundaries of the 99 percent confidence interval.

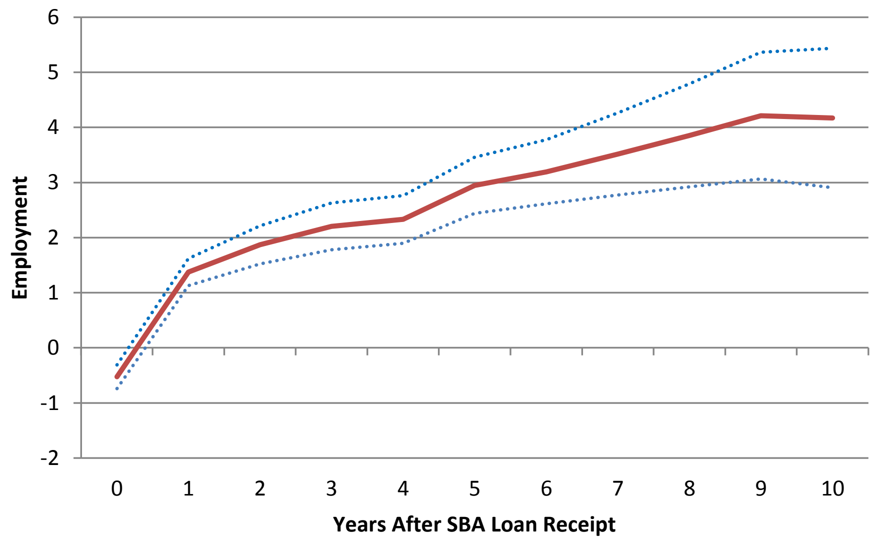

Figure 2.6 Dynamic Specification for SBA Loans in Start-Up Year

Note: OLS regression, also including dummies common to treated and control firms for years since the treated firm's treatment (which is also age) and year effects. The dotted lines are the boundaries of the 99 percent confidence interval.

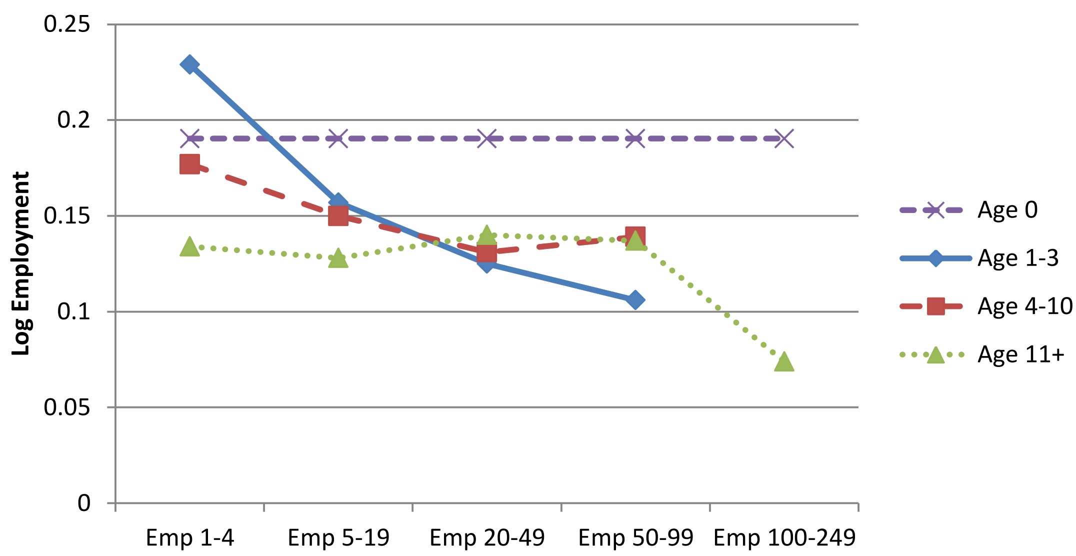

Figure 2.7 Size Effects Per Loan for Each Age Category, Fixed Effects Regressions

Note: These are plots of results reported in Table 11, including only coefficients significant at the 5 percent level.

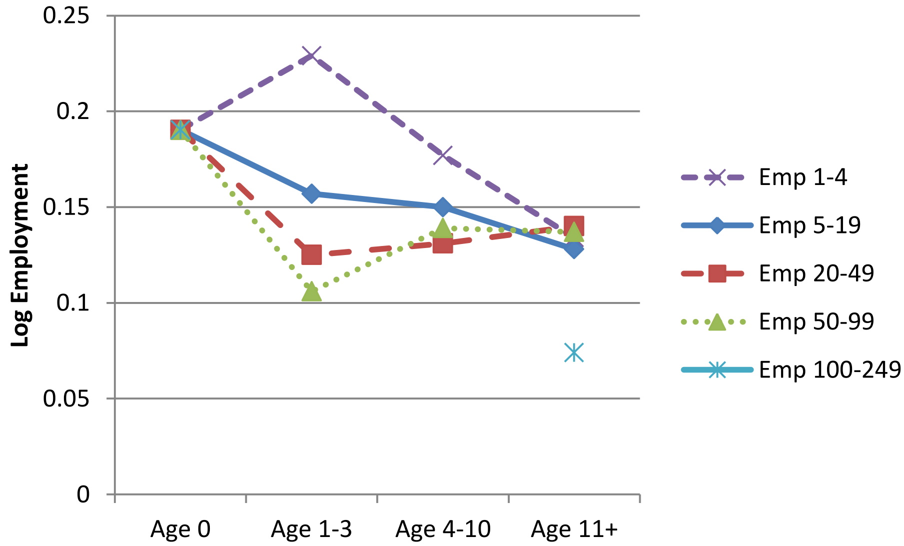

Figure 2.8 Age Effects Per Loan for Each Size Category, Fixed Effects Regressions

Note: these are plots of results reported in Table 11, including only coefficients significant at the 5 percent level.

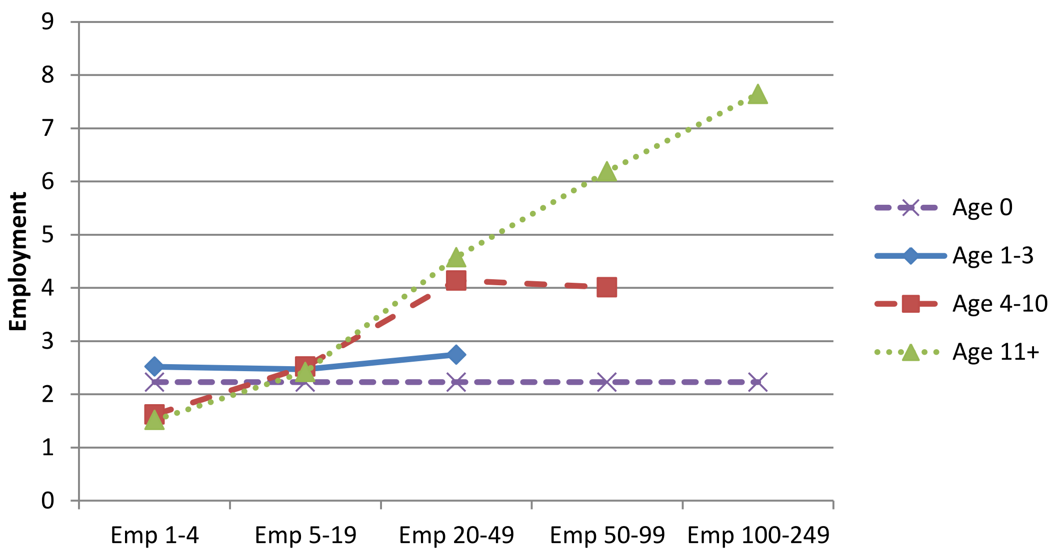

Figure 2.9 Size Effects per $Million Loan for Each Age Category, Fixed Effects Regressions

Note: these are plots of results reported in Table 11, including only coefficients significant at the 5 percent level.

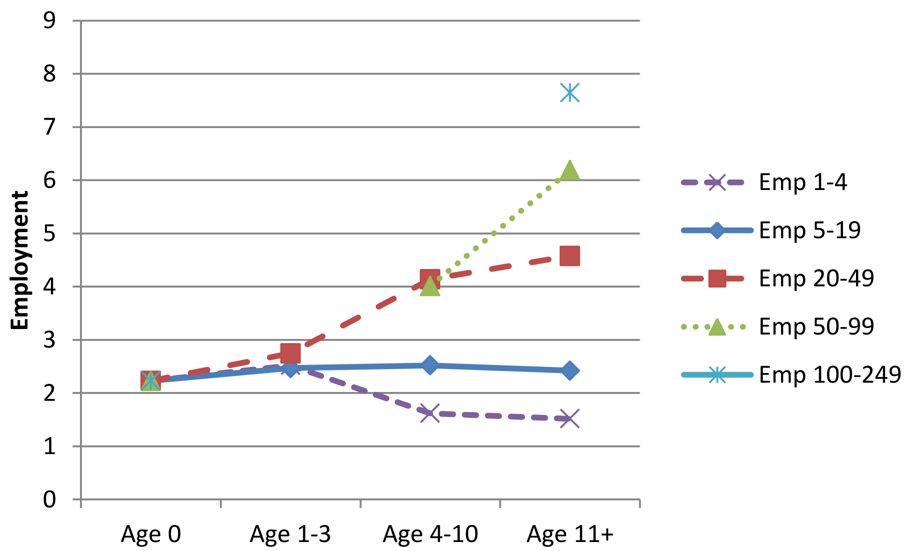

Figure 2.10 Age Effects Per $Million Loan for Each Size Category, Fixed Effects Regressions

Note: these are plots of results reported in Table 11, including only coefficients significant at the 5 percent level.

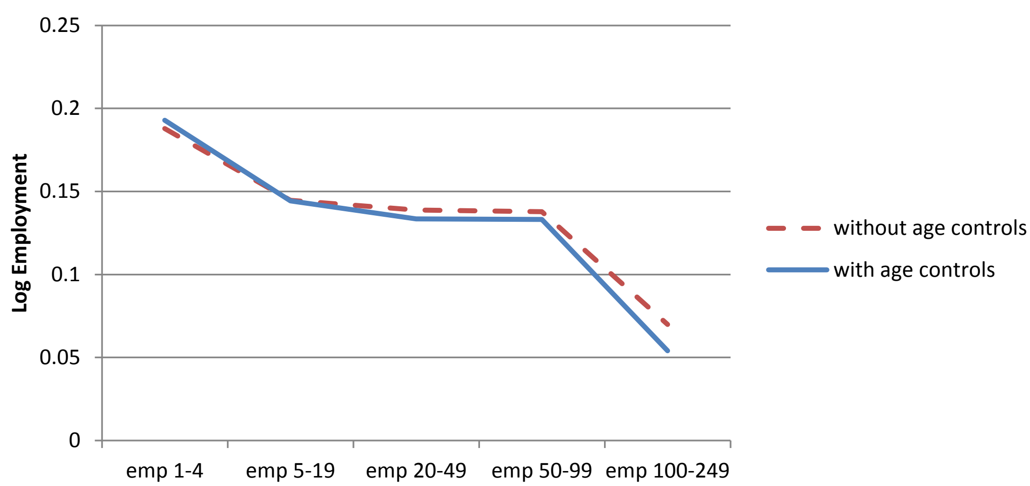

Figure 2.11 Effects Per Loan by Size, Fixed Effects Regressions

Note: these are plots of results reported in Table 13.

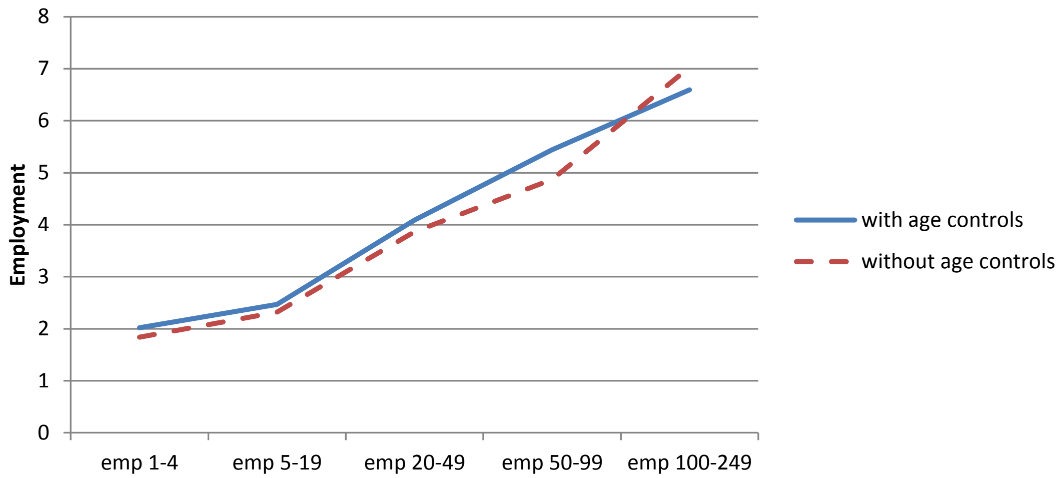

Figure 2.12 Effects Per $Million Loan by Size, Fixed Effects Regressions

Note: these are plots of results reported in Table 13.

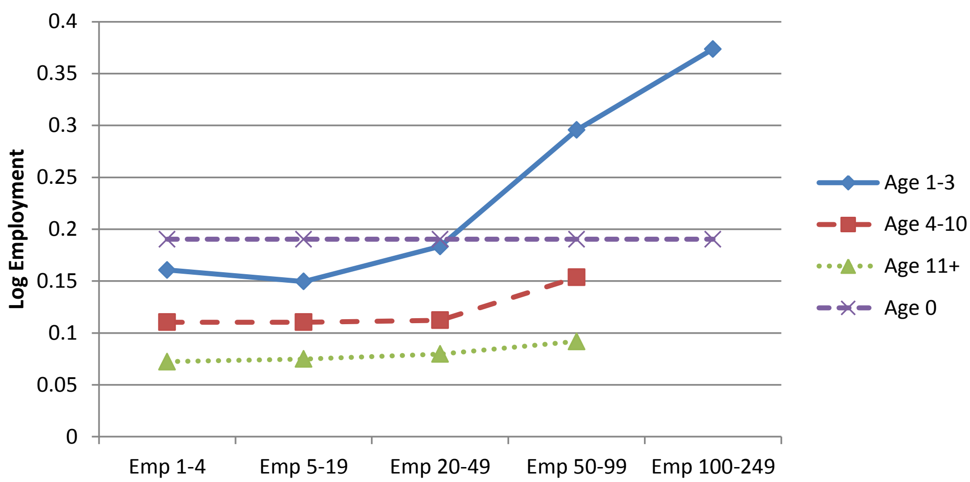

Figure 2.13 Size Effects Per Loan for Each Age Category, Fixed Effects Regressions, One Post-Exit Observation

Note: these are plots of results reported in Table 14, including only coefficients significant at the 5 percent level.

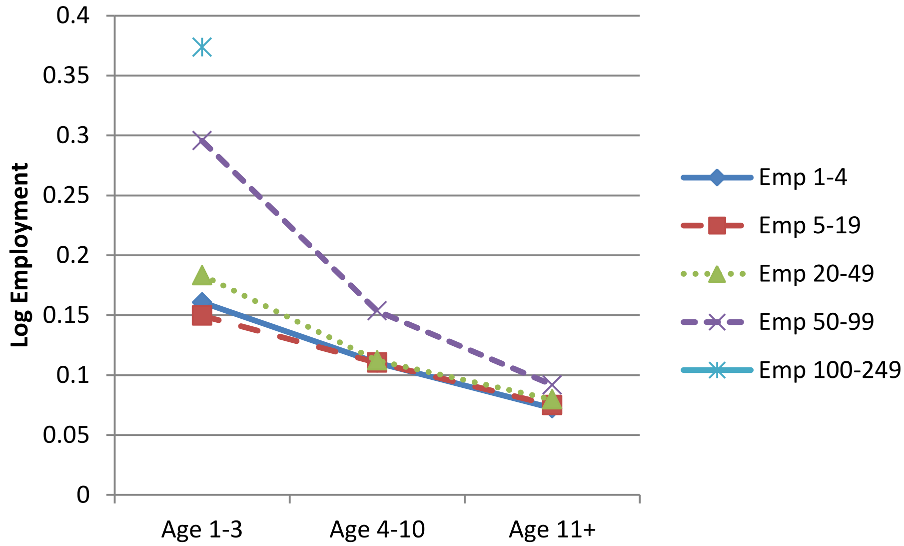

Figure 2.14 Age Effects Per Loan for Each Size Category, Fixed Effects Regressions, One Post-Exit Observation

Note: these are plots of results reported in Table 14, including only coefficients significant at the 5 percent level.

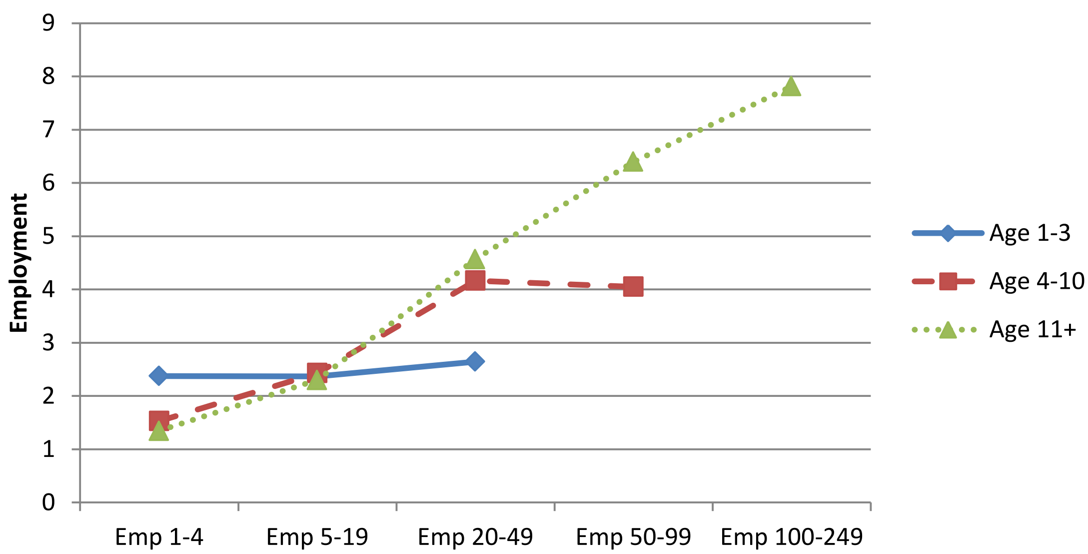

Figure 2.15 Size Effects per $Million Loan for Each Age Category, Fixed Effects Regressions, One Post-Exit Observation

Note: these are plots of results reported in Table 14, including only coefficients significant at the 5 percent level.

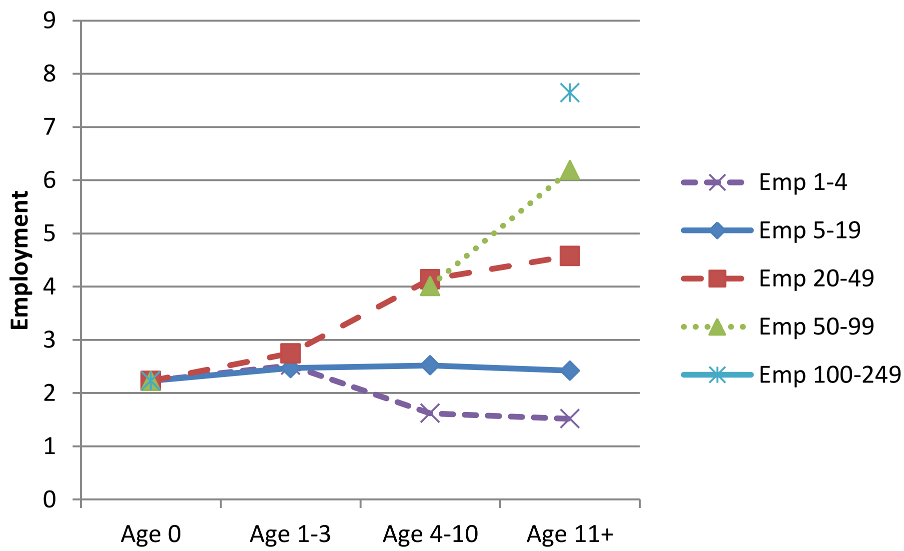

Figure 2.16 Age Effects Per $Million Loan for Each Size Category, Fixed Effects Regressions, One Post-Exit Observation

Note: these are plots of results reported in Table 14, including only coefficients significant at the 5 percent level.

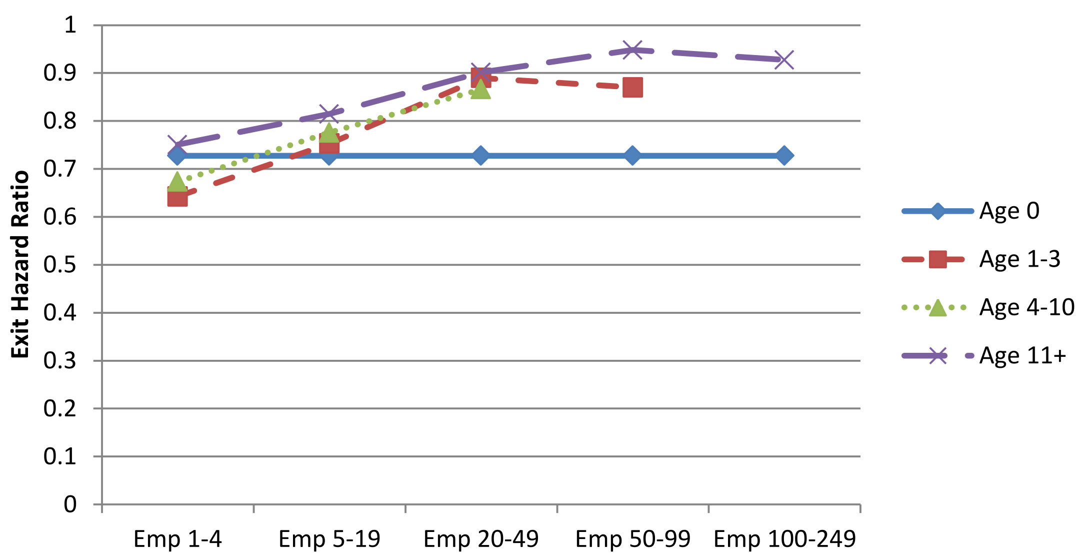

Figure 2.17 Survival Effects Per $Million Loan by Size for each Age Category, Exit Cox-Proportional Hazard Regressions

Note: these are plots of results reported in Table 18, including only coefficients significant at the 5 percent level.

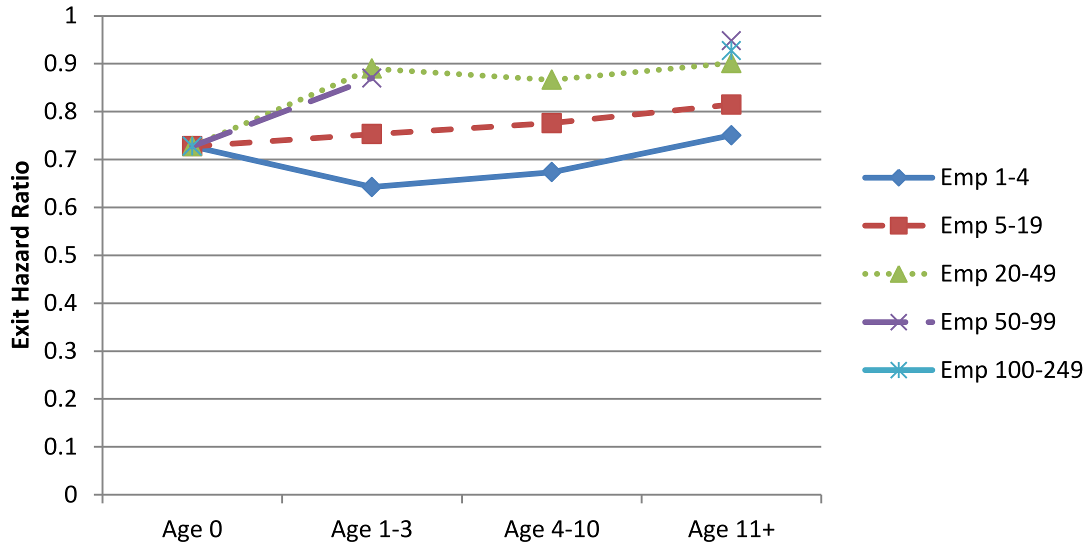

Figure 2.18 Survival Effects Per $Million Loan by Age for each Size Category, Exit Cox-Proportional Hazard Regressions

Note these are plots of results reported in Table 18, including only coefficients significant at the 5 percent level.

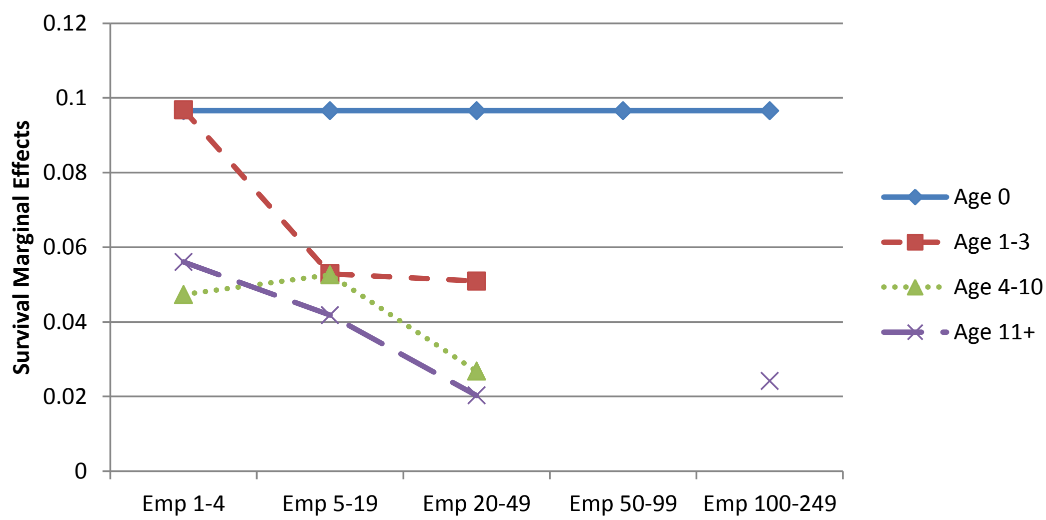

Figure 2.19 Survival Effects Per $Million Loan by Size for each Age Category, Probit Regressions for Survival Three Years After Loan Receipt

Note: these are plots of results reported in Table 19, including only coefficients significant at the 5 percent level.

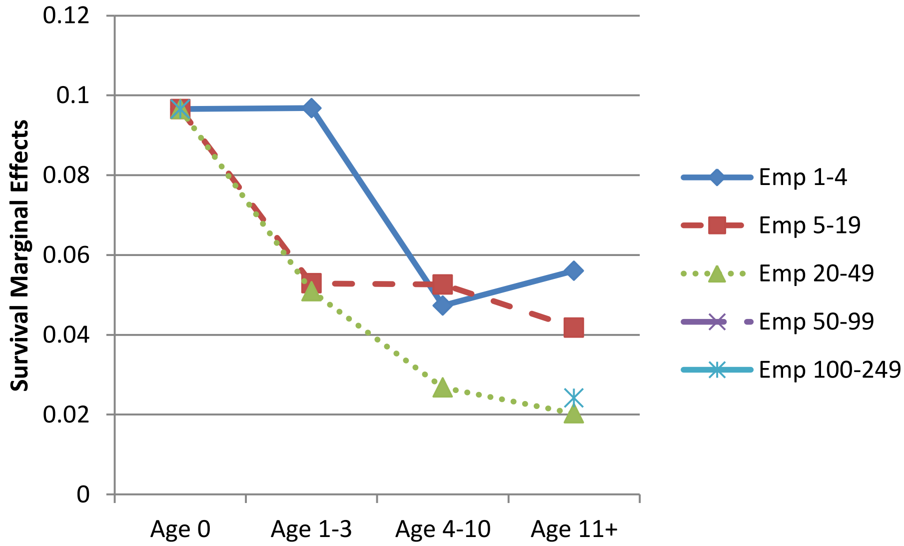

Figure 2.20 Survival Effects Per $Million Loan by Age for each Size Category, Probit Regressions for Survival Three Years After Loan Receipt

Note: these are plots of results reported in Table 19, including only coefficients significant at the 5 percent level.

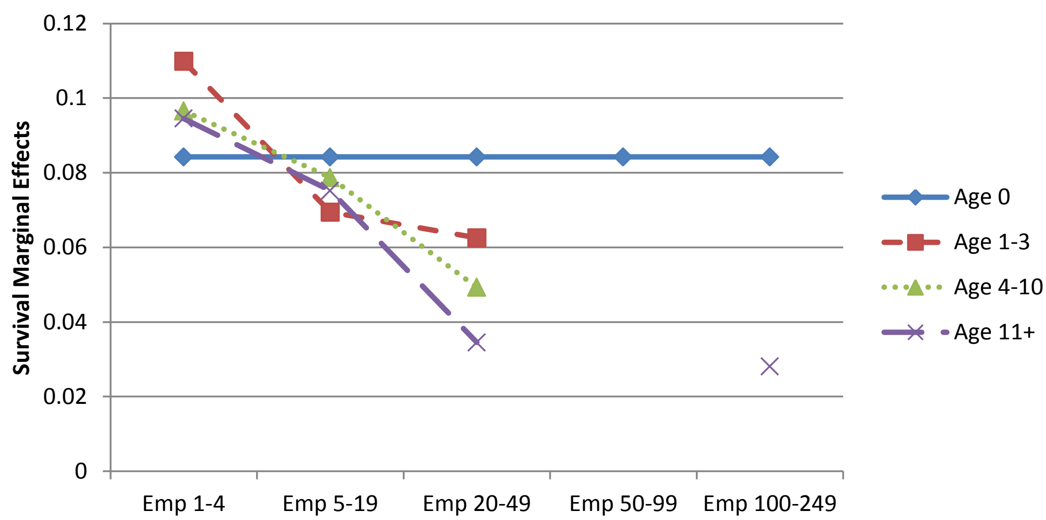

Figure 2.21 Survival Effects Per $Million Loan by Size for each Age Category, Probit Regressions for Survival Ten Years After Loan Receipt

Note: these are plots of results reported in Table 20, including only coefficients significant at the 5 percent level.

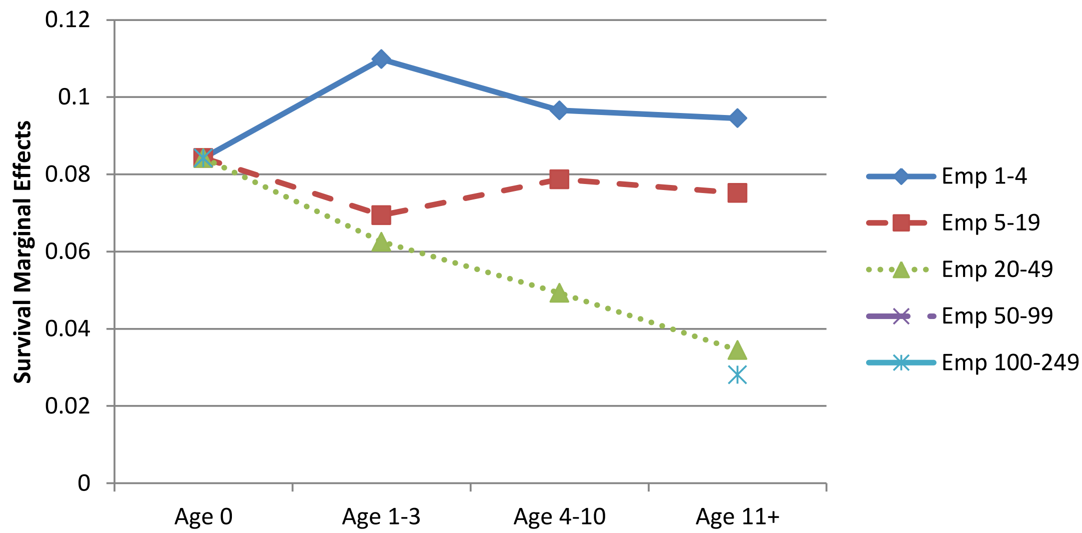

Figure 2.22 Survival Effects Per $Million Loan by Age for each Size Category, Probit Regressions for Survival Ten Years After Loan Receipt

Note: these are plots of results reported in Table 20, including only coefficients significant at the 5 percent level.

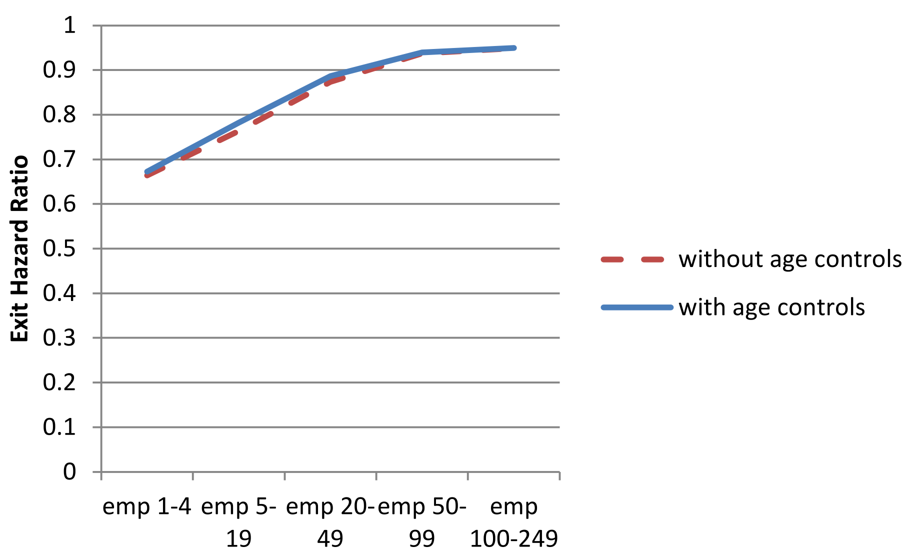

Figure 2.23 Survival Effects Per $Million Loan by Size, Exit Cox-Proportional Hazard Regressions

Note: these are plots of results reported in Table 21.

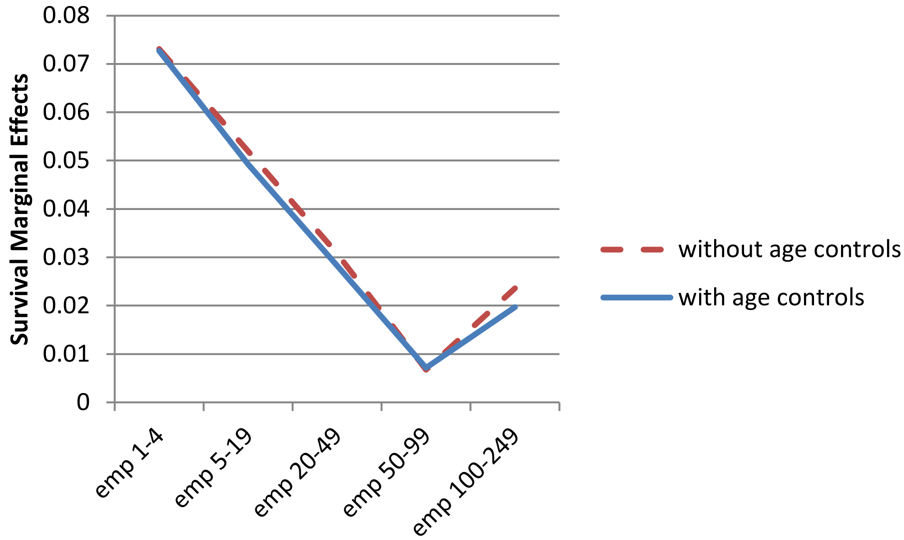

Figure 2.24 Survival Effects Per $Million Loan by Size, Probit Regressions for Survival Three Years After Loan Receipt

Note: these are plots of results reported in Table 2.22.

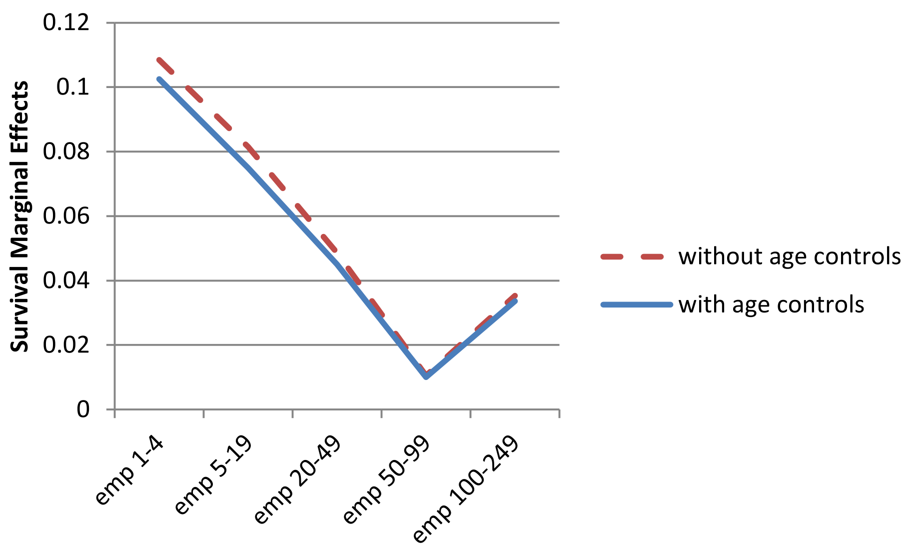

Figure 2.25 Survival Effects Per $Million Loan by Size, Probit Regressions for Survival Ten Years After Loan Receipt

Note: these are plots of results reported in Table 23.

Table 2.1 Path from Full SBA Loan Dataset to Treated Firms in Final Matched Regression Sample

	Number
Total SBA Loans in 1992-2011	986,090
Except loans not matched to any business register	856,447
Except loans matched to non-employer business register	766,609
Except SBA 7(a)/504 loans after the first loan	635,244
Except firms with first SBA loan before 1992	563,664
Except firms with SBA disaster loan	561,258
Except firms with more than two consecutive years of inactivity	558,789
Except firms without matched controls (main sample for analysis)	330,799

Table 2.2 Number of SBA Loan Recipients in LBD by Age

	Employment in Year Prior to SBA Loan					
Age	1-4	5-19	20-49	50-99	100-249	Total
0						69,007
1-3	59,083	43,036	8,160	1,540	424	112,243
4-10	40,510	56,661	15,206	3,546	1,057	116,980
11+	22,666	44,069	18,190	5,995	2,551	93,471
Total	122,259	143,766	41,556	11,081	4,032	391,701

This sample includes SBA loan recipients with and without matched controls. Loan year and employment size measured in year prior to loan.

Table 2.3 SBA Loan Recipients (counted once, in loan year) as Percent of All LBD Firm-Years between 1992-2011 (including all SBA firm-years)

	Employment in Year Prior to SBA Loan					
Age	1-4	5-19	20-49	50-99	100-249	Total
0						0.74
1-3	0.47	0.87	1.00	0.77	0.51	0.61
4-10	0.28	0.68	0.95	0.84	0.56	0.47
11+	0.15	0.33	0.52	0.51	0.38	0.27
Total	0.29	0.54	0.70	0.62	0.43	0.45

This sample includes SBA loan recipients with and without matched controls. Employment size measured in year prior to loan.

Table 2.4 Mean Employment Growth Between Four Years Before and One Year Before Loan Receipt for SBA Loan Recipients by Age

	Employment in Year Prior to SBA Loan					
Age	1-4	5-19	20-49	50-99	100-249	Total
4-10	0.038	0.326	0.428	0.484	0.536	0.211
11+	-0.077	0.076	0.123	0.150	0.175	0.039
Total	-0.001	0.217	0.262	0.274	0.276	0.136

This sample includes SBA loan recipients with and without matched controls. Growth is calculated using the Davis-Haltiwanger method $\left(\frac{2\times(emp_{t-1}-emp_{t-4})}{emp_{t-4}+emp_{t-1}}\right)$. Loan year and employment size measured in year prior to loan.

Table 2.5 Mean Employment Growth Between Year *t-4* and Year *t-1* for All Non-SBA LBD Firms Present in Year *t* in 1992-2012, by Age in Year *t* and Employment Size in Year *t-1*

	Employment in Year *t-1*					
Age in Year *t*	1-4	5-19	20-49	50-99	100-249	Total
4-10	-0.004	0.250	0.318	0.357	0.396	0.052
11+	-0.085	0.053	0.085	0.104	0.117	-0.031
Total	-0.046	0.128	0.156	0.169	0.175	0.004

Growth is calculated using the Davis-Haltiwanger method $\left(\frac{2\times(emp_{t-1}-emp_{t-4})}{emp_{t-4}+emp_{t-1}}\right)$.

Table 2.6 Mean SBA Loan Size (2010 $US)

	Employment in Year Prior to SBA Loan					
Age	1-4	5-19	20-49	50-99	100-249	Total
0						259,302
1-3	221,130	395,978	668,451	920,346	972,368	331,019
4-10	252,968	479,741	817,497	1,054,675	1,159,178	462,080
11+	254,621	512,747	928,310	1,260,069	1,408,985	599,904
Total	237,889	464,786	836,731	1,147,128	1,297,583	421,267

This sample includes SBA loan recipients with and without matched controls.

Table 2.7 Number of Firms with SBA Loans in LBD and Matched Sample by Size and Age

Age-Employment Category	No. Of Firms in LBD with SBA Loans	Number of Firms with SBA Loans Post Matching
Age 0	69,007	55,457
Age 1-3, Employment 1-4	59,083	54,854
Age 1-3, Employment 5-19	43,036	32,333
Age 1-3, Employment 20-49	8,160	7,733
Age 1-3, Employment 50-99	1,540	1,323
Age 1-3, Employment 100-249	424	337
Age 4-10, Employment 1-4	40,510	36,050
Age 4-10, Employment 5-19	56,661	41,431
Age 4-10, Employment 20-49	15,206	14,318
Age 4-10, Employment 50-99	3,546	2,983
Age 4-10, Employment 100-249	1,057	816
Age 11+, Employment 1-4	22,666	20,423
Age 11+, Employment 5-19	44,069	36,999
Age 11+, Employment 20-49	18,190	17,769
Age 11+, Employment 50-99	5,995	5,647
Age 11+, Employment 100-249	2,551	2,326
Total	391,701	330,799

Table 2.8 Number of Firms and Firm-Year Observations in Regressions with All Matches

	Number of Firms	Pre-Treatment Firm-Years	Pre-Treatment Years/Firm	Post-Treatment Firm-Years	Post-Treatment Years/Firm
Treated	330,799	2,313,109	7.0	1,773,278	5.4
Controls	16,372,951	142,541,955	8.7	88,047,895	5.4

Notes: The year of loan receipt is included with pre-treatment years in this table.

Table 2.9 Bias Before and After Propensity Score Matching

	Variable Mean					
	All Non-Treated	All Treated	Final Control Sample	Final Treated Sample	Final % Bias	% Bias Reduction
Log Emp t-1	1.749	1.892	1.898	1.895	-0.171	98.466
Log Emp t-1 sq.	4.966	4.979	5.015	5.003	-0.210	7.290
Log Emp t-1 – t-2	0.016	0.069	0.060	0.064	1.003	91.825
Log Emp t-2 – t-3	0.023	0.068	0.063	0.063	0.106	99.010
Log Emp t-3 – t-4	0.026	0.068	0.064	0.064	-0.007	99.935
Log Wage	3.044	3.050	3.039	3.060	2.551	-282.917
Age	10.755	8.409	8.507	8.504	-0.032	99.886

Notes: % bias is the standardized difference, which for a given variable, say age, is $SDIFF(age) = \frac{100\frac{1}{N}\sum_{i\epsilon A}[age_i - \sum_{j\in C} g(p_i,p_j)age_j]}{\sqrt{\frac{Var_{i\in A}(age)+Var_{j\in C}(age)}{2}}}$. The all non-treated group is included in all years they appear in the LBD. The other three groups are included only in the treatment year.

Table 2.10 Estimated Overall Employment Effects for SBA Loans

	(1) Log Employment	(2) Employment	(3) Employment	(4) Employment
	OLS			
Post-loan	0.204	2.903		-4.536
	(0.003)	(0.134)		(0.157)
Loan amount			14.532	15.948
			(0.261)	(0.287)
	Fixed Effects			
Post-loan	0.156	2.530		-0.671
	(0.002)	(0.090)		(0.109)
Loan amount			5.807	6.113
			(0.186)	(0.216)

The regressions also include a dummy common to treated and control firms for all years following the treated firm's treatment year, and year dummies. The OLS specifications also include a dummy for firms that are ever treated and 16 sector dummies, and the fixed effects specifications include firm fixed effects. Standard errors, cluster-adjusted by firm, are in parentheses. Loan amounts are in 2010 $millions. The OLS regressions include approximately 234 million firm-years, and the fixed effects regressions include about 224 million firm-year observations.

Table 2.11 Fixed Effects with Matched Controls

	(1) Log Employment	(2) Unlogged Employment	
	Post-loan Dummy	Post-loan Dummy	Loan Amount
Age 0	0.190	1.361	2.227
	(0.004)	(0.138)	(0.257)
Age 1-3, Emp 1-4	0.229	0.978	2.518
	(0.004)	(0.074)	(0.212)
Age 1-3, Emp 5-19	0.157	1.331	2.467
	(0.005)	(0.123)	(0.198)
Age 1-3, Emp 20-49	0.125	2.608	2.745
	(0.009)	(0.553)	(0.459)
Age 1-3, Emp 50-99	0.106	5.480	5.509
	(0.025)	(3.994)	(4.344)
Age 1-3, Emp 100-249	0.099	-0.926	-3.110
	(0.054)	(12.612)	(6.452)
Age 4-10, Emp 1-4	0.177	0.523	1.619
	(0.004)	(0.059)	(0.136)
Age 4-10, Emp 5-19	0.150	0.700	2.518
	(0.004)	(0.076)	(0.112)
Age 4-10, Emp 20-49	0.131	1.133	4.136
	(0.008)	(0.425)	(0.360)
Age 4-10, Emp 50-99	0.139	5.586	4.012
	(0.019)	(2.486)	(1.400)
Age 4-10, Emp 100-249	0.030	-7.402	13.527
	(0.038)	(8.342)	(7.273)
Age 11+, Emp 1-4	0.134	0.277	1.515
	(0.006)	(0.051)	(0.195)
Age 11+, Emp 5-19	0.128	0.316	2.421
	(0.004)	(0.074)	(0.117)
Age 11+, Emp 20-49	0.140	-0.333	4.576
	(0.007)	(0.386)	(0.268)
Age 11+, Emp 50-99	0.137	0.426	6.189
	(0.013)	(1.793)	(0.875)
Age 11+, Emp 100-249	0.074	1.947	7.645
	(0.022)	(6.167)	(3.199)

Separate regressions are run for each age-size category. The regressions also include a dummy common to treated and control firms equal to one for all years following SBA loan receipt, year dummies, and firm fixed effects. Standard errors, cluster-adjusted by firm, are in parentheses. Loan amounts are in 2010 $millions.

Table 2.12 OLS with Matched Controls, Age-Size Categories

	(1) Log Employment	(2) Unlogged Employment	
	Post-loan Dummy	Post-loan Dummy	Loan Amount
Age 0	0.214	0.754	8.213
	(0.006)	(0.227)	(0.409)
Age 1-3, Emp 1-4	0.269	1.216	3.517
	(0.005)	(0.123)	(0.280)
Age 1-3, Emp 5-19	0.170	1.152	4.033
	(0.006)	(0.166)	(0.279)
Age 1-3, Emp 20-49	0.118	2.581	3.108
	(0.012)	(0.801)	(0.627)
Age 1-3, Emp 50-99	0.091	5.551	6.257
	(0.031)	(4.612)	(3.765)
Age 1-3, Emp 100-249	0.085	-13.402	2.632
	(0.070)	(18.147)	(10.316)
Age 4-10, Emp 1-4	0.201	0.488	2.308
	(0.005)	(0.063)	(0.162)
Age 4-10, Emp 5-19	0.161	0.277	3.787
	(0.005)	(0.101)	(0.144)
Age 4-10, Emp 20-49	0.135	0.851	4.744
	(0.008)	(0.548)	(0.436)
Age 4-10, Emp 50-99	0.134	7.225	2.017
	(0.022)	(3.004)	(1.610)
Age 4-10, Emp 100-249	0.047	-5.613	20.251
	(0.042)	(11.855)	(10.107)
Age 11+, Emp 1-4	0.144	0.101	2.613
	(0.007)	(0.064)	(0.223)
Age 11+, Emp 5-19	0.127	-0.312	3.867
	(0.005)	(0.097)	(0.143)
Age 11+, Emp 20-49	0.135	-0.233	4.742
	(0.007)	(0.416)	(0.286)
Age 11+, Emp 50-99	0.132	0.983	4.947
	(0.014)	(2.507)	(0.780)
Age 11+, Emp 100-249	0.070	4.743	6.666
	(0.024)	(6.588)	(3.921)

Separate regressions are run for each age-size category. The regressions also include a dummy equal to one for treated firms in all years, a dummy common to treated and control firms equal to one for all years following SBA loan receipt, 16 sector dummies, and year dummies. Standard errors, cluster-adjusted by firm, are in parentheses. Loan amounts are in 2010 $millions.

Table 2.13 Effects by Size, Not Controlling for Age

	(1) Log Employment, Post-loan Dummy	(2) Unlogged Employment Post-loan Dummy	Loan Amount
Employment 1-4	0.188	0.644	1.840
	(0.003)	(0.037)	(0.105)
Employment 5-19	0.145	0.747	2.316
	(0.003)	(0.050)	(0.077)
Employment 20-49	0.139	0.963	3.865
	(0.005)	(0.268)	(0.200)
Employment 50-99	0.138	3.194	4.870
	(0.010)	(1.407)	(0.736)
Employment 100-249	0.070	2.326	7.028
	(0.019)	(4.923)	(2.760)

The regressions also include dummies common to treated and control firms equal to one for all years following SBA loan receipt, interacted with size categories; and year dummies. The OLS regression also includes uninteracted size category dummies. Standard errors, cluster-adjusted by firm, are in parentheses. Loan amounts are in 2010 $millions. The OLS regressions include approximately 234 million firm-year observations, and the firm fixed effects regressions include approximately 224 million firm-year observations.

Table 2.14 Fixed Effects with Matched Controls, Age-Size Categories, One Post-Exit Observation

	(1) Log Employment	(2) Unlogged Employment	
	Post-loan Dummy	Post-loan Dummy	Loan Amount
Age 0	0.182	1.863	1.327
	(0.004)	(0.120)	(0.230)
Age 1-3, Emp 1-4	0.161	0.785	2.375
	(0.003)	(0.063)	(0.191)
Age 1-3, Emp 5-19	0.150	1.202	2.365
	(0.005)	(0.109)	(0.184)
Age 1-3, Emp 20-49	0.183	2.836	2.644
	(0.011)	(0.489)	(0.412)
Age 1-3, Emp 50-99	0.296	7.933	5.352
	(0.033)	(3.342)	(3.441)
Age 1-3, Emp 100-249	0.374	9.871	-3.107
	(0.072)	(11.581)	(5.669)
Age 4-10, Emp 1-4	0.110	0.414	1.527
	(0.003)	(0.051)	(0.122)
Age 4-10, Emp 5-19	0.110	0.524	2.428
	(0.004)	(0.071)	(0.106)
Age 4-10, Emp 20-49	0.112	0.735	4.163
	(0.008)	(0.389)	(0.332)
Age 4-10, Emp 50-99	0.154	5.181	4.050
	(0.021)	(2.309)	(1.328)
Age 4-10, Emp 100-249	0.066	-4.949	12.117
	(0.044)	(7.580)	(6.427)
Age 11+, Emp 1-4	0.072	0.214	1.345
	(0.005)	(0.049)	(0.189)
Age 11+, Emp 5-19	0.075	0.144	2.301
	(0.004)	(0.071)	(0.112)
Age 11+, Emp 20-49	0.080	-0.965	4.570
	(0.007)	(0.382)	(0.261)
Age 11+, Emp 50-99	0.092	-1.076	6.369
	(0.013)	(2.081)	(0.992)
Age 11+, Emp 100-249	0.032	-0.075	7.816
	(0.024)	(5.904)	(3.069)

Separate regressions are run for each age-size category. The regressions also include a dummy common to treated and control firms equal to one for all years following SBA loan receipt, year dummies, and firm fixed effects. Standard errors, cluster-adjusted by firm, are in parentheses. Loan amounts are in 2010 $millions. An observation is added for each exiting firm in the year after exit, with employment equal to the sum of employment of the firm's exit-year establishments in the year after the firm's exit, which is zero if all establishments close. Log employment here is log of employment + 1 to incorporate zero values for employment.

Table 2.15 OLS with Matched Controls, Age-Size Categories, One Post-Exit Observation

	(1)	(2)	
	Log Employment	Unlogged Employment	
	Post-loan Dummy	Post-loan Dummy	Loan Amount
Age 0	0.178	0.711	7.953
	(0.005)	(0.202)	(0.373)
Age 1-3, Emp 1-4	0.192	1.030	3.429
	(0.004)	(0.110)	(0.256)
Age 1-3, Emp 5-19	0.149	1.004	3.973
	(0.005)	(0.152)	(0.260)
Age 1-3, Emp 20-49	0.137	2.583	3.073
	(0.011)	(0.734)	(0.578)
Age 1-3, Emp 50-99	0.183	6.496	6.372
	(0.031)	(4.237)	(3.475)
Age 1-3, Emp 100-249	0.222	-7.159	3.300
	(0.074)	(16.368)	(9.311)
Age 4-10, Emp 1-4	0.135	0.398	2.234
	(0.004)	(0.058)	(0.150)
Age 4-10, Emp 5-19	0.127	0.167	3.686
	(0.004)	(0.095)	(0.136)
Age 4-10, Emp 20-49	0.118	0.664	4.635
	(0.008)	(0.512)	(0.410)
Age 4-10, Emp 50-99	0.133	6.860	2.033
	(0.022)	(2.826)	(1.521)
Age 4-10, Emp 100-249	0.057	-4.323	18.455
	(0.045)	(10.996)	(9.277)
Age 11+, Emp 1-4	0.088	0.041	2.478
	(0.005)	(0.060)	(0.207)
Age 11+, Emp 5-19	0.085	-0.423	3.746
	(0.005)	(0.093)	(0.136)
Age 11+, Emp 20-49	0.092	-0.572	4.614
	(0.007)	(0.402)	(0.273)
Age 11+, Emp 50-99	0.101	0.534	4.700
	(0.014)	(2.478)	(0.746)
Age 11+, Emp 100-249	0.040	3.284	6.600
	(0.025)	(6.342)	(3.754)

Separate regressions are run for each age-size category. The regressions also include a dummy equal to one for treated firms in all years, a dummy common to treated and control firms equal to one for all years following SBA loan receipt, and year dummies. Standard errors, cluster-adjusted by firm, are in parentheses. Loan amounts are in 2010 $millions. An observation is added for each exiting firm in the year after exit, with employment equal to the sum of employment of the firm's exit-year establishments in the year after the firm's exit, which is zero if all establishments close. Log employment here is log of employment + 1 to incorporate zero values for employment.

Table 2.16 Three-Year Survival Rates (%) in Survival Regression Sample

	Employment in Year Prior to SBA Loan					
Age	1 to 4	5 to 19	20 to 49	50 to 99	100 to 249	Total
0						63.57
1-3	69.16	72.77	72.23	71.86	71.04	70.81
4-10	74.15	79.66	81.92	81.66	83.54	78.36
11+	75.34	82.73	86.94	87.34	87.42	82.80
Total	71.96	78.74	82.42	83.84	85.37	75.31

These numbers are calculated for the full survival regression sample, including both treated and control firms. The control samples are limited to the single nearest neighbor for each treated firm.

Table 2.17 Ten-Year Survival Rates (%) in Survival Regression Sample

	Employment in Year Prior to SBA Loan					
Age	1 to 4	5 to 19	20 to 49	50 to 99	100 to 249	Total
0						31.15
1-3	36.48	41.74	40.47	39.64	32.94	38.80
4-10	42.79	50.41	51.61	51.15	49.24	48.13
11+	45.18	56.42	59.22	58.31	59.29	55.06
Total	40.19	49.97	53.01	53.99	55.17	44.93

These numbers are calculated for the full survival regression sample, including both treated and control firms. The control samples are limited to the single nearest neighbor for each treated firm.

Table 2.18 Survival Effects by Age-Size Categories, Cox-Proportional Hazard Models

	Loan Recipient Dummy Hazard Ratio	Loan Amount Hazard Ratio
Age 0	1.040	0.728
	(0.010)	(0.013)
Age 1-3, Emp 1-4	1.096	0.643
	(0.011)	(0.016)
Age 1-3, Emp 5-19	1.082	0.753
	(0.015)	(0.016)
Age 1-3, Emp 20-49	1.023	0.890
	(0.030)	(0.023)
Age 1-3, Emp 50-99	0.874	0.870
	(0.064)	(0.050)
Age 1-3, Emp 100-249	0.782	0.872
	(0.113)	(0.094)
Age 4-10, Emp 1-4	1.125	0.673
	(0.015)	(0.020)
Age 4-10, Emp 5-19	1.139	0.776
	(0.016)	(0.014)
Age 4-10, Emp 20-49	1.143	0.866
	(0.026)	(0.016)
Age 4-10, Emp 50-99	0.967	0.952
	(0.049)	(0.031)
Age 4-10, Emp 100-249	0.954	1.045
	(0.094)	(0.056)
Age 11+, Emp 1-4	1.063	0.751
	(0.020)	(0.029)
Age 11+, Emp 5-19	1.165	0.815
	(0.019)	(0.016)
Age 11+, Emp 20-49	1.277	0.901
	(0.030)	(0.015)
Age 11+, Emp 50-99	1.154	0.948
	(0.049)	(0.023)
Age 11+, Emp 100-249	1.234	0.927
	(0.081)	(0.031)

These are hazard ratios from Cox-Proportional Hazard regressions, run separately for each age-size category. The regressions also include treatment year dummies and 16 sector dummies. Standard errors are in parentheses. The control samples are limited to the single nearest neighbor for each treated firm. Loan amounts are in 2010 $millions.

Table 2.19 Three-Year Survival Effects by Age-Size Categories, Probit Models

	Loan Recipient Dummy Marginal Effect	Loan Amount Marginal Effect
Age 0	0.085	0.097
	(0.005)	(0.010)
Age 1-3, Emp 1-4	0.029	0.097
	(0.005)	(0.013)
Age 1-3, Emp 5-19	0.044	0.053
	(0.006)	(0.010)
Age 1-3, Emp 20-49	0.067	0.051
	(0.013)	(0.013)
Age 1-3, Emp 50-99	0.129	0.029
	(0.033)	(0.028)
Age 1-3, Emp 100-249	0.115	0.016
	(0.064)	(0.052)
Age 4-10, Emp 1-4	-0.008	0.047
	(0.006)	(0.013)
Age 4-10, Emp 5-19	-0.014	0.053
	(0.005)	(0.007)
Age 4-10, Emp 20-49	0.014	0.027
	(0.008)	(0.007)
Age 4-10, Emp 50-99	0.056	0.010
	(0.019)	(0.014)
Age 4-10, Emp 100-249	0.048	0.006
	(0.035)	(0.025)
Age 11+, Emp 1-4	-0.065	0.056
	(0.008)	(0.015)
Age 11+, Emp 5-19	-0.063	0.042
	(0.005)	(0.006)
Age 11+, Emp 20-49	-0.034	0.020
	(0.006)	(0.005)
Age 11+, Emp 50-99	0.004	0.001
	(0.012)	(0.007)
Age 11+, Emp 100-249	-0.019	0.024
	(0.018)	(0.010)

These are marginal effects from probit regressions with a dependent variable equal to one if the firm survives three years after the treated firm's year of SBA loan receipt, run separately for each age-size category. The regressions also include treatment year dummies and 16 sector dummies. Standard errors calculated by the Delta method, are in parentheses. The control samples are limited to the single nearest neighbor for each treated firm. Loan amounts are in 2010 $millions.

Table 2.20 Ten-Year Survival Effects by Age-Size Categories, Probit Models

	Loan Recipient Dummy Marginal Effect	Loan Amount Marginal Effect
Age 0	0.045	0.084
	(0.005)	(0.008)
Age 1-3, Emp 1-4	0.013	0.110
	(0.005)	(0.011)
Age 1-3, Emp 5-19	0.021	0.069
	(0.007)	(0.010)
Age 1-3, Emp 20-49	0.026	0.063
	(0.014)	(0.013)
Age 1-3, Emp 50-99	0.113	0.028
	(0.035)	(0.028)
Age 1-3, Emp 100-249	0.111	0.061
	(0.065)	(0.053)
Age 4-10, Emp 1-4	-0.014	0.097
	(0.007)	(0.013)
Age 4-10, Emp 5-19	-0.024	0.079
	(0.006)	(0.008)
Age 4-10, Emp 20-49	-0.022	0.049
	(0.010)	(0.009)
Age 4-10, Emp 50-99	0.034	0.012
	(0.024)	(0.017)
Age 4-10, Emp 100-249	-0.009	0.043
	(0.047)	(0.032)
Age 11+, Emp 1-4	-0.029	0.095
	(0.009)	(0.017)
Age 11+, Emp 5-19	-0.067	0.075
	(0.007)	(0.008)
Age 11+, Emp 20-49	-0.067	0.035
	(0.009)	(0.007)
Age 11+, Emp 50-99	-0.011	0.006
	(0.018)	(0.011)
Age 11+, Emp 100-249	-0.065	0.028
	(0.027)	(0.015)

These are marginal effects from probit regressions with a dependent variable equal to one if the firm survives ten years after the treated firm's year of SBA loan receipt, run separately for each age-size category. The regressions also include treatment year dummies and 16 sector dummies. Standard errors, calculated by the Delta method, are in parentheses. The control samples are limited to the single nearest neighbor for each treated firm. Loan amounts are in 2010 $millions.

Table 2.21 Survival Effects by Size, Not Controlling for Age, Cox-Proportional Hazard Models

	Loan Recipient Dummy Hazard Ratio	Loan Amount Hazard Ratio
Employment 1-4	1.107	0.664
	(0.008)	(0.011)
Employment 5-19	1.148	0.763
	(0.010)	(0.009)
Employment 20-49	1.191	0.874
	(0.017)	(0.010)
Employment 50-99	1.061	0.938
	(0.031)	(0.017)
Employment 100-249	1.112	0.949
	(0.057)	(0.026)

These are hazard ratios from Cox-Proportional Hazard regressions run separately for each size category. The regressions include dummies for the treatment year and 16 sectors. The control samples are limited to the single nearest neighbor for each treated firm. Loan amounts are in 2010 $millions.

Table 2.22 Three-Year Survival Effects by Size, Not Controlling for Age, Probit Models

	Loan Recipient Dummy Hazard Ratio	Loan Amount Hazard Ratio
Employment 1-4	-0.001	0.073
	(0.004)	(0.008)
Employment 5-19	-0.016	0.052
	(0.003)	(0.004)
Employment 20-49	-0.000	0.031
	(0.005)	(0.004)
Employment 50-99	0.034	0.007
	(0.010)	(0.007)
Employment 100-249	0.003	0.024
	(0.016)	(0.010)

These are marginal effects from probit regressions with a dependent variable equal to one if the firm survives three years after the treated firm's year of SBA loan receipt, run separately for each size category. The regressions include dummies for the treatment year and 16 sectors. Standard errors, calculated by the Delta method, are in parentheses. The control samples are limited to the single nearest neighbor for each treated firm. Loan amounts are in 2010 $millions.

Table 2.23 Ten-Year Survival Effects by Size, Not Controlling for Age, Probit Models

	Loan Recipient Dummy Hazard Ratio	Loan Amount Hazard Ratio
Employment 1-4	-0.005	0.108
	(0.004)	(0.008)
Employment 5-19	-0.028	0.081
	(0.004)	(0.005)
Employment 20-49	-0.037	0.049
	(0.006)	(0.005)
Employment 50-99	0.016	0.011
	(0.013)	(0.009)
Employment 100-249	-0.043	0.035
	(0.023)	(0.013)

These are marginal effects from probit regressions with a dependent variable equal to one if the firm survives ten years after the treated firm's year of SBA loan receipt, run separately for each size category. The regressions include dummies for the treatment year and 16 sectors. Standard errors, calculated by the Delta method, are in parentheses. The control samples are limited to the single nearest neighbor for each treated firm. Loan amounts are in 2010 $millions.

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Chapter 3: Fraud and Cultural Norms: Evidence from Utilities Meter Fraud

Abstract

This paper takes advantage of a unique investigation by a local utility company within a single metropolitan area to relate cultural norms of residents to identified instances of fraud. We base the cultural norms index on the country of birth and the associated country corruption index of residents and generate counts of location-based utilities fraud based on the data from the investigation. The analysis results in a large and persistent positive correlation between the cultural norms index and fraud variables suggesting that there exists a strong relationship between cultural norms and the likelihood of engaging in fraud.

3.1 Introduction

Underlying reasons for corruption have not been well documented. For example, despite identical incentives, some individuals engage in corruption or fraud while others do not. One way to address these differences in outcomes is to differentiate among individuals. Influences that could contribute to the decision to commit fraud include one's interpretation of laws, morality, and cultural norms, however, these individual characteristics are difficult to measure.

It is possible to measure cultural norms for different countries, but comparing fraud outcomes across countries is problematic due to a variety of confounding variables such as strength of enforcement. However, one can study fraud outcomes for a single country by identifying a cultural norms variable across individuals of diverse origins living within the specified country, assuming that an individual's actions are influenced by cultural norms of his or her country of origin. In such a study, the hypothesis would state that greater tolerance of corruption in the home country would lead to an increase in the likelihood of engaging in fraud. This paper applies the outlined cultural norms logic to the topic of utility fraud in the United States. The cultural richness of the examined area allows us to link country of birth to frequency of fraud. We test the cultural norms hypothesis using 786 cases of utility fraud across one metropolitan area in the US.

Utility fraud examined in this paper may yield important insights into tax fraud as the two share important commonalities. Tax fraud is a prominent topic in the research literature in the US and elsewhere due to its importance for governments. Similarly to tax fraud, utilities fraud is uncommon because utilities data are rarely audited and fraud, even if identified, has limited consequences to the individual. While tax fraud is difficult to identify due to the complexity of the tax code and more limited data based on yearly observations, utility fraud is much easier to catch using utilities usage data that is collected on a continuous basis. In addition, tax evasion can take many forms from underreporting to misclassifying. In contrast, utilities fraud involves physical tampering that can be identified by an expert. Due to the similarities between the two types of fraud and the greater ease in identifying utilities fraud, results from this study may be applicable to other areas of fraud generally and tax evasion in particular.

In this study, we relate cultural norms and fraud across geographic areas within a single metropolitan area in the US. The cultural norms index is based on the local proportion of immigrants and corruption norms of the immigrant countries of origin. The key correlation is between this index and the number of identified fraud incidents at the census block group level. We use funds recovered by case as an additional outcome of interest.

Results signify that cultural norms associated with the country of origin play a role in determining both the probability of engaging in fraud and the size of the fraud. We find that, as predicted, cultural norms are significantly positively related to fraud. The range of estimates shows that a one standard deviation increase in the cultural norms index leads to a forecast of 1.2 times to 1.4 times the number of instances of fraud at the local geographic level. We also find evidence that the cultural index is positively correlated with the amount recovered per fraud case. In this case, a one standard deviation increase in the cultural norms index leads to $100 increase in the amount recovered.

An alternative explanation is that the findings are driven by the qualities of the neighborhoods into which immigrants select. In order to limit the persuasiveness of this argument, we include a wide range of neighborhood characteristics as controls, including immigrant population size, making it less likely that neighborhood characteristics should play a role. Nevertheless, the analysis is conducted at the cross-section and it is possible that other unobserved characteristics are driving neighborhood selection. If neighborhood characteristics do explain the findings, however, then it must be that immigrants are matched to neighborhoods based on unobserved neighborhood characteristics which are related to corruption norms of immigrants' countries of origin. Even then, this study shows that choices immigrants make are positively correlated to the cultural norms of the home country. The results of this analysis continue to support the relationship between cultural norms and behavior despite potential unobserved selection mechanisms.

The seminal paper in the field of cultural norms and corruption can be attributed to Fisman and Miguel (2007). In this paper, the authors use traffic parking ticket information of UN diplomats across 149 countries as a proxy for corruption in the sense of “abuse of entrusted power for private gain” and show that cultural norms are an important determinant of corruption. The data are collected at a time when diplomats were exempt from paying accumulated fines - that is, diplomats faced no obvious incentives to limit their traffic parking violations outside of cultural predispositions. Barr and Serra (2010) show that home-country cultural norms predicted the likelihood of offering or giving bribes in a laboratory game by undergraduate students. The result did not extend to the actions of graduate students. DeBacker et al. (2012) use IRS audit data to show that corporations from countries with higher tolerance for corruption engage in higher amounts of tax evasion in the US. This effect is strong for small corporations and decreases with the size of the corporation.

While Fisman and Miguel demonstrated the effect of cultural norms on corruption, this study shows that cultural norms can also have an effect on fraud. Fraud is defined as wrongful or criminal deception intended to result in financial or personal gain.

Further, this study complements Barr and Serra's findings by studying patterns in individual wrongdoing based on cultural norms of the birth country, but unlike Barr and Serra, this analysis does so outside of a laboratory setting. Finally, this study extends the findings of DeBacker et al. by suggesting that fraud may not only be dependent on the origin of the corporate entity, but also on the origin of the individuals involved.

The contribution of this paper is two-fold. First, previous papers focused on non-permanent and non-resident individuals, therefore lacking predictions for actions of permanently settled individuals. For example, Fisman and Miguel use data on non-resident UN diplomats, Barr and Serra perform their study in a lab with students, and DeBacker et al. study the impact of decision-making undertaken in foreign owned firms. The aforementioned studies do not offer any predictions for how long-term residents might act in their personal affairs. This study fills that gap by relating country of origin of long-term residents to probability and extent of fraud. Second, although tax fraud is one of the most consequential types of fraud, previous literature has little application to the topic. By studying household decisions in rarely enforced areas, this study generates predictions for the relationship between cultural norms and tax fraud. Use of these results, though, will be limited by legal restrictions on discrimination by country of origin.

Section 2 outlines the estimation procedure, Section 3 describes the setting and methods by which fraud was identified and describes the data used in this analysis, Section 4 presents estimation results and Section 5 details a number of robustness checks. Section 6 concludes.

3.2 Estimation Strategy

The main econometric specification looks at the correlation between a neighborhood cultural norms index and the number of fraud instances. The estimation is at the more granular block group census level, while the key explanatory variable, the cultural norms index, is at the larger census tract level due to data limitations. Errors are clustered at the track level. The estimation equation can be represented in the following way,

$$\#fraudcases_{\mathrm{i}} = \beta_1 + \beta_2 CultNormsInd._{\mathrm{j}} + \beta_3 \% ForeignBor\mathrm{n}_{\mathrm{j}} + \Sigma_{\mathrm{h}} \beta_{\mathrm{h}} Controls_{\mathrm{ih}} + \varepsilon_{\mathrm{ijh}}$$

where i denotes census block group and j stands for the larger census tract. The cultural norms index combines data on foreign births and US births and the relevant country's Kaufmann, Kraay, and Mastruzzi (2005) corruption index from 1998. The index is the sum of the fraction of group block residents by country of birth multiplied by the country corruption index and is a proxy for the cultural norms of the neighborhood as they are influenced by those born inside and outside of the United States. We define the cultural norms index in the following way,

$$\mathrm{CulturalNormsIndex}_{\mathrm{k}} = \Sigma_{\mathrm{k}} \frac{\#Personsfr/Countryk\ in\ Tractj}{Total\#Persons\ in\ Tractj} \mathrm{xCorruptionIndex}_{\mathrm{k}}$$

where k is a country outside of the US.

All controls ($\Sigma_{\mathrm{h}}\beta_{\mathrm{h}} Controls_{\mathrm{ih}}$) are at the more granular block group level rather than census tract level. Percent foreign born variable allows for the comparison between neighborhoods with similar ratios of immigrants. Additional controls include income and poverty variables, density and gas utility usage variables, as well as education and race variables.

The foreign born variable measures the percentage of individuals born outside of the country and is highly correlated with the independent variable and substantially affects the estimated coefficient of the cultural norms index, the key independent variable. The high correlation stems from the fact that the two variables are always jointly zero or positive. The greater the number of immigrants in the block group, the larger the fraction of immigrants. Yet the cultural norms index varies both by the fraction of immigrants and, equally importantly, by the magnitude of the corruption index. Fraction of immigrants variable allows for a comparison between neighborhoods that controls for the overall proportion of immigrants. Since differences in immigrant proportion across neighborhoods are large, this additional control, while highly correlated with the key independent variable, removes substantial, potentially endogenous variation across neighborhoods from the regression.

Since the dependent variable is the count of fraud cases in a census block group, we use a count model analysis. In particular, the estimation uses the negative binomial model because the variance of the dependent variable is almost twice the mean. The Poisson model, which assumes that the mean is equal to the variance, is easily rejected by the data. All coefficient results based on the negative binomial are presented as incidence rate ratios while standard errors have not been adjusted and will not match incidence rate ratios. Incidence rate ratios can be interpreted the following way – if the incidence rate ratio for a certain regression is 1.2, then the dependent variable changes 1.2 times for every unit change in the independent variable.

3.3 Data

I combine several datasets in this analysis. They include fraud, corruption, census and crime data.

Fraud data are based on an audit of residential gas utility customers that took place over 2010 and 2011 in a single metro area in the US. The goal of the audit was to identify customers who were underpaying the utility company for their gas usage and bill them for that usage. The audit was composed of two different data gathering sources. A portion of the cases were based on data-driven analysis conducted by a utility-contracted consulting firm. Consultants looked for unusual downward changes in usage or differences in usage between comparable properties using two years of data. The other data source was the utility company itself, which collected the data through a variety of means. Much of the data came from investigations of utility meters registering little or no usage based on similar methods to those used by the consulting firm, but with access to ten years of data. In addition, some cases were flagged due to reports of leaks, or problems visible to utility employees who happened to be passing by. In both sources of data, once locations were flagged, utility workers made visits to the physical locations to identify the cause of the flagged inconsistencies and noted their findings in a log.

Although there were a variety of reasons for why locations were flagged, in a surprisingly large fraction (786/1539) of total flagged cases, the reason for observed data inconsistencies was fraud. The cases flagged by an independent data firm yielded a 72 percent rate of tampering from 611 flagged cases, while the utility company had a 37 percent rate from 928 flagged cases.

Data on which the data audits were based consisted of usage and two numerical indices that recorded possible sources of interference with meter operation. Gas meters transmitting the data were located along the gas pipes in order to track gas usage for billing purposes, typically in the basements of properties. Almost all residential customers used a single gas meter type. Each gas meter had an electronic remote transmitter that allowed the utility company to collect monthly usage data by driving by the property without needing to enter the property (without visual inspection) installed by the utility company at some point during the 1990s and fixed or replaced on an as needed basis. In addition to logging and transmitting gas usage, meters also logged and transmitted interference data. Suspicious cases were first identified using algorithms that tagged unusual drop-offs in gas usage and movement on the indices. Below are three typical comments made by data analysts explaining why they flagged cases based on gas usage patterns as well as tampering index data.

“Consumption drops to 0 ccf in May after several [index] flags. Suspect tampering.”

“Multiple tampering [on the index], [gas usage] index frozen since January, when the first [index] tamper count was logged. Account has been targeted for field visit.”

“Bad [electronic remote transmitter].”

Utility employees then visited the flagged properties. Most common methods of tampering included using a tampering device or rewiring the meter. Below are three typical descriptions of fraud made by gas company employees based on field visits.

“Found [tampering device] on heater duct above meter.”

“There is dust/dirt on top of meter outlining what appears to be a [tampering device].”

“Found security caps drilled out.”

Not all investigators were able to enter the area where the gas meter was located on their first visit to the property allowing those who had used a tampering device to potentially remove incriminating evidence. As a result, we complement the conservative estimate of utility worker identified fraud cases with a count which includes all flagged cases as a robustness check.

Fraud data for residential customers within a single metropolitan area came from the local utility company. Fraud data were derived from sites flagged for meter maintenance based on abnormal utility meter readings. If the meter was found to be malfunctioning, the utility company fixed the utility meter and attempted to claim owed utility payments based on meter malfunction with future charges adjusted based on a newly functioning meter. The utility employee who visited the property noted down the probable cause of the issue.

As noted above, flagged data were compiled from two distinct sources - the utility company and the contracted consulting firm. Cases flagged by the utility were based on visual observation and call-in reports in addition to data source usage. The second source is a consulting company hired to identify suspect utility meters using data-based statistical methods only. The first data source may be influenced by employee perceptions of neighborhood demographics, previous experience and other factors. Although the first data source may be biased, the second should not be.

After data was flagged additional steps were taken to identify fraudulent cases. First, the utility company prioritized the largest and most promising loss cases; given staffing limitations, not all flagged sites were investigated when the data for this research was collected. Second, when a utility employee visited the flagged site, she created two sets of data - a written description of meter issues used for reference by other company employees and a numerical categorization system. The categorization standardized the probable cause of the malfunction into one of six categories. Utility company employees may have been subject to incentives or biases in investigating flagged cases. It is not known whether utility employees had financial or social incentives to flag suspicious sites or identify sites as being fraudulent.

It is logical to assume that utility workers faced a lower threshold for classifying a case as fraudulent than convincingly describing evidence of fraud. In order to decrease the potential bias injected by utility employees, we take a more conservative approach and categorize each case based on the notes written about the case.

The focus of the analysis is on documented fraud cases, though the flagged case count is used in robustness checks. There are 786 cases of documented fraud and a total of 1784 data-based flagged cases. Among 1,704 block groups in the metropolitan area, 1,221 have zero cases of documented fraud, 303 have one case, and the remaining 180 block groups with positive fraud have up to 9 cases of documented fraud each (see Figure 3.1 and Figure 3.2). On average, each block group has 0.5 cases of documented fraud. Of those, 0.2 was flagged originally by the utility itself and 0.3 by the consulting data company (see Table 3.2).

The data set also contains financial data tied to meter malfunction. When underpayment is identified, the utility company compiles a bill for the total amount owed to the utility company by the customers who underpaid their bills. Although some states have laws allowing criminal charges to be filed, few if any cases are pursued in court. It is then not surprising that only 490 of the 786 cases have a positive posted amount since only recovered amounts are logged in the data. If the customer had moved or participated in a program for low-income households, the customer was billed zero. Total dollar amount billed per fraud case is composed of two elements - length of time for which it could be ascertained that underpayment was taking place and amount of monthly underpayment during that period. A large billed amount could be a result of fraud that had been taking place for a long period of time and/or underpayment on large monthly gas bills. The average billed per theft case with a positive posted amount at block group level is $2,500 with a median of $1,400 and a maximum of over $25,000 (see Table 3.4).

Country-level corruption data are from "Aggregate and Individual Governance Indicators 1996-2008" compiled by Kaufmann, Kraay, and Mastruzzi (2009). Kaufman et al. and include indices on cross-country economic, political, and social characteristics. The cross-country corruption index is most relevant for this study. This corruption index resembles the first principal component of 35 different commonly used corruption indices. Currently, the oldest data available is for 1996, but 1998 data, the next year for which this data are available, includes a larger set of surveys substantially decreasing standard errors on country corruption estimates. As a result, 1998 country corruption data are the best and earliest source of cross-country corruption indices also used by Fisman and Miguel (2007).

The original corruption index is standardized to be mean zero, variance one with higher values signifying lower corruption. For ease of interpretation in this study, the index is reversed so that least corrupt countries are now associated with negative values. Since this analysis relates corruption data to country from which first generation immigrants came, only 73 countries identified by the US Census will be of relevance. The index is also available on a 100 scale created by Kaufman et al. for the 73 countries included in the U.S. Census in the appendix (see Table 3.1). The index ranges from -2.33 (1) for Sweden to a high of 1.83 (100) for Afghanistan. The mean for these 73 countries is -0.06 (48) and the median is 0.23 (50) for Peru. US has a normalized index value of -1.55 or 8 on a 100 point scale.

Data on the country of origin comes from the Decennial Census which contains geographically demarcated immigration data. The Census includes three questions that identify country of origin, year of immigration, and level of English proficiency. This analysis makes use of county of origin data only. Place of birth if outside of the US differentiates between 73 countries and includes "other" categories for each world region.

Most recent, publicly available immigration data come from the 2000 Census. While most relevant public Census data are available at the block group level, the lowest geographic level containing country of birth information for individuals in the public data are at the larger census tract level. Census tracts typically have anywhere between 2,000 and 8,000 individuals. In the county of interest, a census tract has a mean and a median of just over four thousand individuals whereas a block group area has a mean and a median of just under one thousand individuals. Data on income, poverty, education, population, race, and housing also come from the 2000 Census and are at the finer block-group level.

According to the 2000 census, the metropolitan area had a population of approximately 1.5 million. Of these, nine percent were born outside of the United States and are highly diverse in terms of region of origin. By region, the largest percentage of foreign-born persons were born in Southeast Asia (15%), followed by Eastern Europe (14%), the Caribbean (13%), and East Asia (11%). Within these regional breakdowns certain countries stand out. By country, the largest fraction (8%) were born in Vietnam. Ukraine, China, India and Jamaica, each account for approximately 5 percent of the foreign-born population.

A less important, but still very relevant question to this analysis is who, the resident or the landlord, pays the gas bill and is thus motivated to commit fraud as observed in the data. In apartments, oftentimes the landlord who may not live in the area pays the gas bill for the entire building whereas in flats and houses people living in the house pay the bills. Alternatively, meters in apartment buildings may all be located in one location so that apartment residents are prevented from tampering with only their own meters. A reasonable assumption is to say that residents in free-standing structures and two apartment buildings pay for their own bills, while the gas bill for apartment buildings may be aggregated and billed to landlords. As a result, we use Census to construct measures of apartment buildings and houses.

Crime data are a cross section of public crime data available for the metropolitan area. We focus on violent crimes and residential burglaries and leave out auto thefts, thefts from a vehicle since these latter crimes are less geography-specific. The crime variable is constructed by summing all relevant crimes committed within each block group area in 2007, several years prior to the start of fraud data. Number of crimes reported during the selected year ranges from 0 to 98 with a median of 13 and a mean of 16 crimes per block group.

3.3.1 Summary Statistics

Figure 3.7 breaks up mean incidence of fraud by tracts with highest density of individuals born in high corruption world regions that are well-represented in the sample. This sample is compared to fraud rates in all other block groups. The bar graph suggests that higher concentration tracts have higher incidence of fraud. As a visual control, figure 8 outlines mean incidence of fraud in neighborhoods with high concentration of a single race. In comparing the two figures, one can see that neighborhoods with high density of those born in the Caribbean, Central America or South America are comparable to neighborhoods with a high density of Hispanics with the highest fraud incidence of 1.3 to 1.22. Next, those born in Eastern Europe have a much higher incidence of corruption (1 versus 0.41). Finally, high concentrations of individuals born in Southeast Asia and South Asia have higher rates of fraud than neighborhoods with high density of Asians (0.69 and 0.84 as compared to 0.64). This comparison provides some evidence that country of birth matters. While these figures are far from conclusive, they are suggestive of a positive correlation between birth country cultural norms or, possibly, neighborhood selection and incidence of fraud.

Figure 3.9 graphs the cultural norms index normalized to mean 0, variance 1 against the incidence of fraud by block group. A positive relationship is apparent before any neighborhood controls are added. Regressions in the subsequent section will show that the positive correlation is sustained after all controls are included.

Table 3.5 looks at the correlations between the key dependent variable and the control variables. The cultural norms variable is dropped from this exercise. The coefficients are estimated using a negative binomial regression. The biggest predictors of fraud are number of housing structures (as opposed to apartments), crime rate, fraction of Asians and Hispanics. Within the ethnicity categories, fraction of Hispanics is a particularly large predictor of fraud. We include controls for the number of free-standing houses and number of apartment buildings.

A tract with all residents born in the United States has a normalized cultural norms index of approximately -0.88. 159 block groups have a normalized corruption norms index of -0.88 or lower meaning that residents of these geographic areas are mostly born in the US or countries with even lower corruption indices.

3.4 Results

In this paper, we relate the cultural norms index with counts of fraud. The analysis is based on a cross-section of data in time and across neighborhoods. The analysis adds additional neighborhood variables in order to control for geographic characteristics. Neighborhood controls include income, poverty, education levels, and self-employment. The estimate remains large and significant but does decrease slightly with the addition of neighborhood-level control variables. We also control for the racial make-up which also slightly decreases the estimate of interest. Next, these neighborhood characteristics are introduced into the analysis at the block group level. The variable of interest is calculated at the larger census tract level due to data limitations. By using finer neighborhood data, we are better able to describe and control for neighborhood characteristics while clustering at census tract level. The estimate remains robust to these controls, but there is a limit to the number of observed neighborhood characteristics, so unobserved neighborhood characteristics remain a concern.

To obtain an arguably more precise comparison across neighborhoods with immigrant populations, we include a variable measuring the percentage of immigrants by geographic unit. This control variable allows for comparison across areas with similar proportions of immigrant populations. In a simplified way, this means that the estimate compares an area with 5 percent population of immigrants from Vietnam (corruption index of 60), for example, to an area with 5 percent population of immigrants from Ukraine (corruption index of 94). A positive coefficient on the variable of interest means that given the 5 percent immigrant population, an area with immigrants from Ukraine will have more cases of fraud than an area with immigrants from Vietnam based on the differences in the corruption index between these countries. The positive relationship between cultural norms index and fraud continues to hold and increases in most instances when this control is included.

Next, we disaggregate fraud data by source in order to test potential issues of selection. Estimates stay largely consistent. Therefore, we do not see selection by data source as important in data interpretation. Finally, we use the financial variable to measure the correlation between the cultural norm index and recovered cost per fraud. We use the same set of controls to get a more precise estimate. Although we find a positive correlation, the estimate is weak and rarely significant.

3.4.1 Instances of Fraud

Table 3.6 exhibits the main result - the cultural norms index has a positive and significant correlation with fraud. More concretely, a one standard deviation increase in the normalized cultural norms index is forecasted to lead to 1.2 to 1.4 times the number of fraud cases. The estimate decreases from a high of 1.4 times but remains significant with additional controls which include income, poverty, neighborhood characteristics, education variables, and race. In the most conservative estimate, a one standard deviation increase in the normalized cultural norms index results in 1.2 times the number of fraud cases. The addition of an immigration variable, which is closely correlated with the cultural norms index, has the highest resulting coefficient along with a five-fold decrease in precision.

A question that has been of great interest is whether length of stay in the US decreases the influence of home-country cultural norms. However, this estimation in the context of utility fraud is limited by publicly available data. Data on year of entry is reported in three time spans and across four races for each census tract. The data does not capture the fact that each census tract could have a variety of immigration waves over a long time span within a single race category. That is, the data indicate how many Asians came in the 1980s versus the 1990s but not in which region they were born.

Next, we divide the data by source. Table 3.7 and Table 3.8 replicate the analysis above but use fraud cases based on flagged locations identified by data analysts as compared to those flagged by the utility company, respectively. In Table 3.7, results remain very similar although with the addition of the last control variable (fraction immigrant), the coefficient loses significance. Table 3.8 presents results based on cases flagged by the utility itself. Again, the sole substantial difference in estimates comes with the addition of the last control variable when the coefficient jumps to almost 2 times. The difference in significance may be due to differences in power between the regressions. Overall, results are consistent for all samples.

3.4.2 Cost of Fraud

Table 3.9 looks at the correlation between the cultural norms index and posted cost per fraud case. The coefficient although insignificant can be interpreted to signify a $100 increase in cost per fraud case with a one standard deviation increase in the cultural norms index. Nevertheless, the estimates are far too imprecise to establish a definitive correlation. When the percentage of individuals born outside of the country is added as a control, the coefficients and the standard errors jump so that the coefficients remain positive but insignificant. Some of the imprecision may be due to the fact that a bill is only logged if funds are paid back by the charged household. Results based on this dependent variable do not distinguish between the various reasons for a high bill charged by the utility company – cases of fraud that had been going on for a long period by average usage customers and cases of fraud committed by high usage customers for a short period both result in similar billed amounts. Using the available data, Table 3.9 shows a loose positive correlation between the cultural norms index dollar amount billed per theft case.

3.5 Robustness Checks

The utility company is likely to investigate cases where more fraud is expected, implicitly due to the immigrant composition. We rerun all regressions described in the results section using flagged data in order to look for selection between the flagged and fraud data sets. Estimates become more precise using flagged data as compared to fraud data in the results section above, yet we cannot reject the null hypothesis that the coefficients are equal. The greater number of observations in the flagged data allows for more precision and offers no sign of selection (see Table 3.10).

One may argue that the utility company may look to areas with higher immigrant composition even in flagging cases. This argument makes UtilityCo's flagged data biased since it was partially derived utility workers who are subject to these biases, while DataCo's flagged cases should not be biased as they were based on independent data-based analysis only. Nevertheless, these disaggregated results again show no substantial differences from findings in the results section. In an identical succession of regressions, we sort fraud data by source - DataCo and UtilityCo. Estimates are similar between the aggregate regressions in Table 3.10 and the disaggregated regressions in Table 3.11 and Table 3.12. With the addition of the immigration control variable, the estimates becomes less precise for data derived from DataCo, but remain significant unlike the estimate in the results section. With the addition of race and immigration variables, the estimates lose significance as above. Nevertheless, the coefficient remains consistent and large in magnitude despite becoming insignificant in both the results and robustness sections suggesting that lack of significance comes from low power in the estimation rather than bias. The estimate for data derived from UtilityCo jumps and becomes more imprecise but remains strongly significant as before. Overall, we see no evidence of selection in comparing estimates based on flagged and fraud datasets.

Finally, in line with the emerging pattern, the correlation between the cultural norms index and cost per flagged case grows in estimate and imprecision with additional controls. We cannot test for bias or selection that affected all datasets due to data limitations although there could be instances, even if unlikely, in which all datasets used in this study were subject to bias.

3.6 Conclusion

In this study, we find a positive correlation between cultural norms and instances of fraud. This implies that populations composed of residents from more corrupt home countries are associated with a higher likelihood of fraud. We do our best to control for neighborhood characteristics and therefore neighborhood selection throughout the analysis. Despite these efforts, many neighborhood unobservables may be present for which we cannot control. As a result, we cannot conclusively say that the results we find are driven by those born outside of the US. Neighborhoods into which immigrants select rather than immigrants themselves could very well be driving our findings. Although we cannot definitively identify the mechanism behind our findings, our results do establish that immigrants born in comparatively more corrupt countries after interacting with their neighborhood choice are correlated with an increase in instances of fraud in an urban environment in the US.

This finding is novel due to its focus on neighborhoods of permanently settled populations. In contrast, previous work in the culture norms literature studied the individual independent of her living environment as well as her interaction with it. This study shows that individuals and the neighborhoods with which they interact may be generating an environment for increased levels of fraud.

Of course, the results are also driven by the immigrant populations who select into cities. Due to large and growing city populations as well as high rates of world-wide immigration, this selection makes the results all the more exciting. The observed incidence of fraud and correlations are undoubtedly related to the dense urban environment whether due to selection into or interaction with the environment through social networks, for example. Data used for this study are just a subset of the entire data available across US geographic regions. Yet, we found that other geographic regions did not have nearly as big of a fraud issue as the one here. This observation gives rise to many additional questions regarding what else, in addition to culture norms, must be present to result in higher rates of fraud.

Although this study answers several important questions with respect to cultural norms and fraud, it raises many others. First, can we distinguish between individuals and the neighborhoods into which they select in the correlation with fraud? After all, this study is not conclusive on the mechanism which underlies the correlation. Next, is there a basis for a causal relationship such as between individual cultural norms and fraud? Finally, what generates large-scale fraud in one geography but not in another? If culture norms are at play, could strong social or ethnic networks play a role in the spread of the know-how and the impetus to partake?

3.7 Appendix

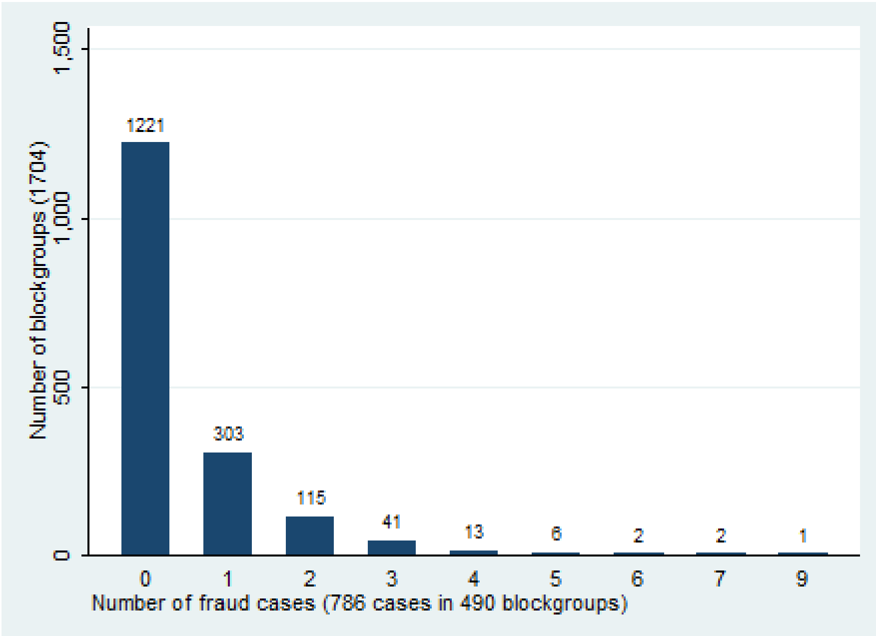

Figure 3.1 Incidence of Fraud

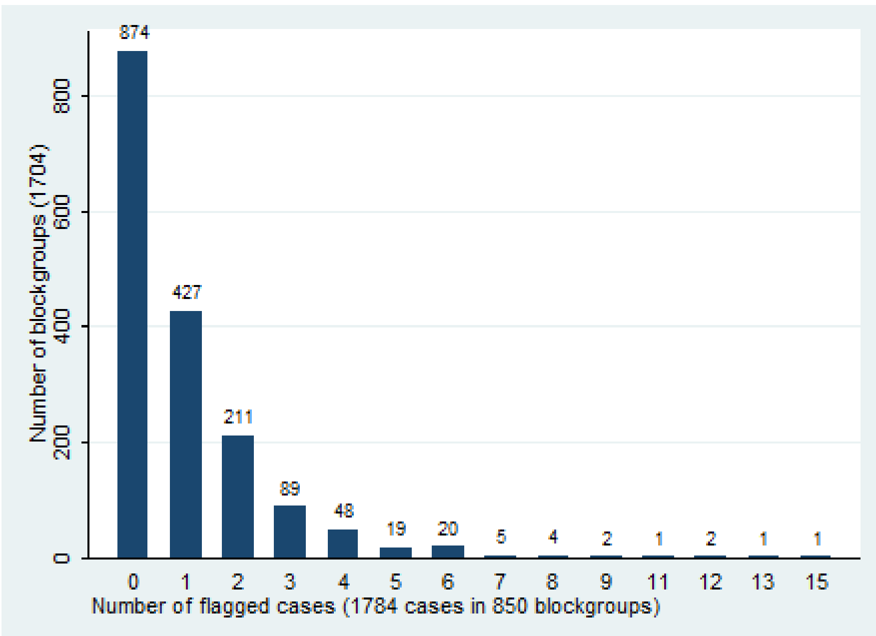

Figure 3.2 Flagged Sites

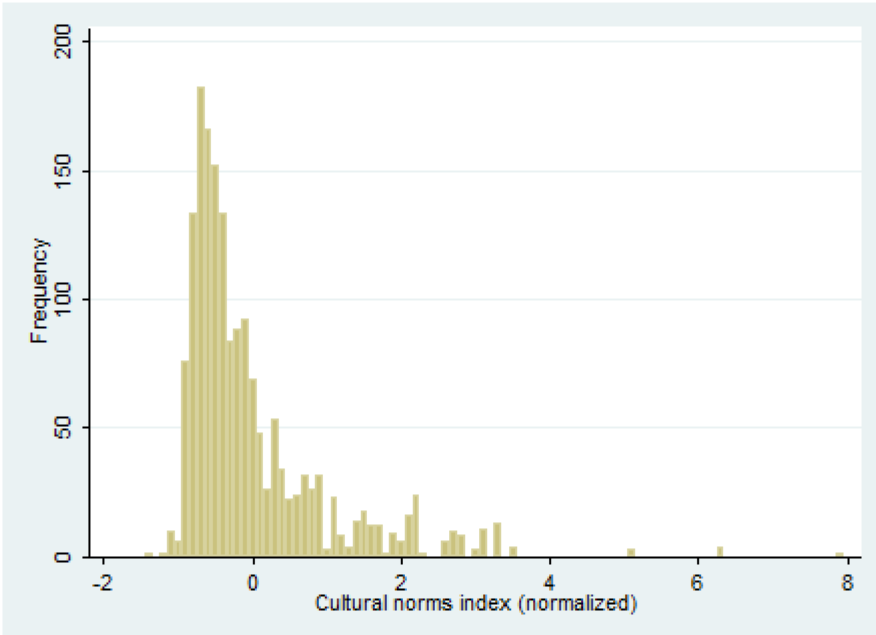

Figure 3.3 Cultural Norms Index

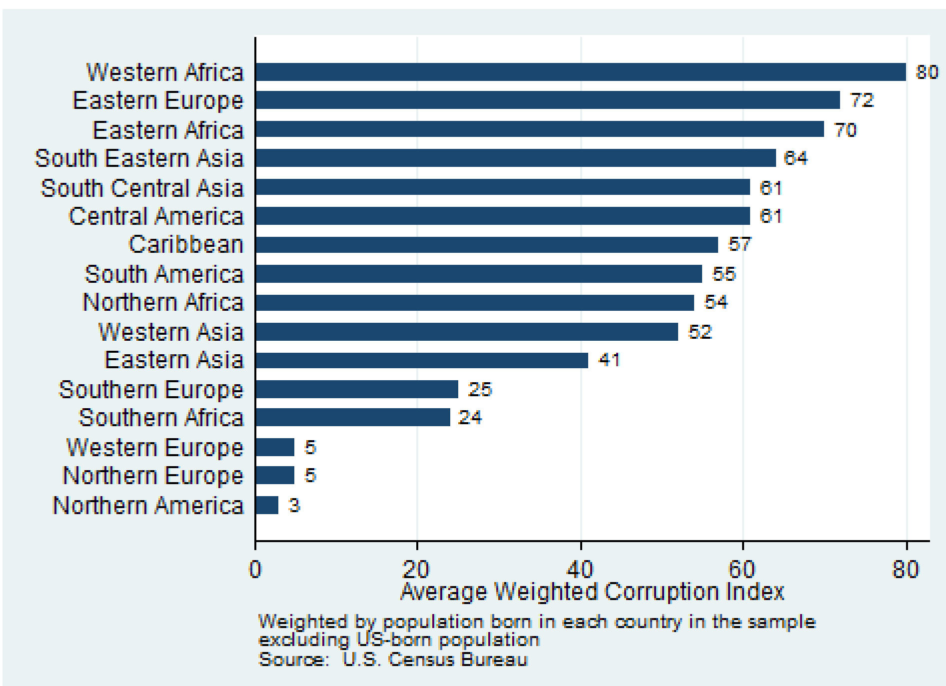

Figure 3.4 Average Corruption Index by Region

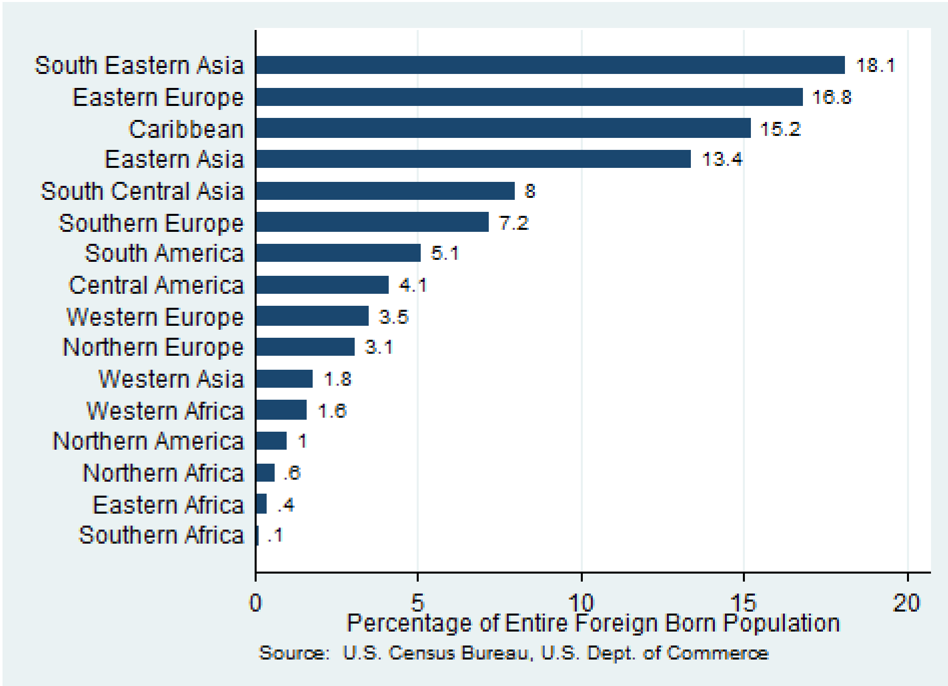

Figure 3.5 Region of Birth of Foreign Born Residents

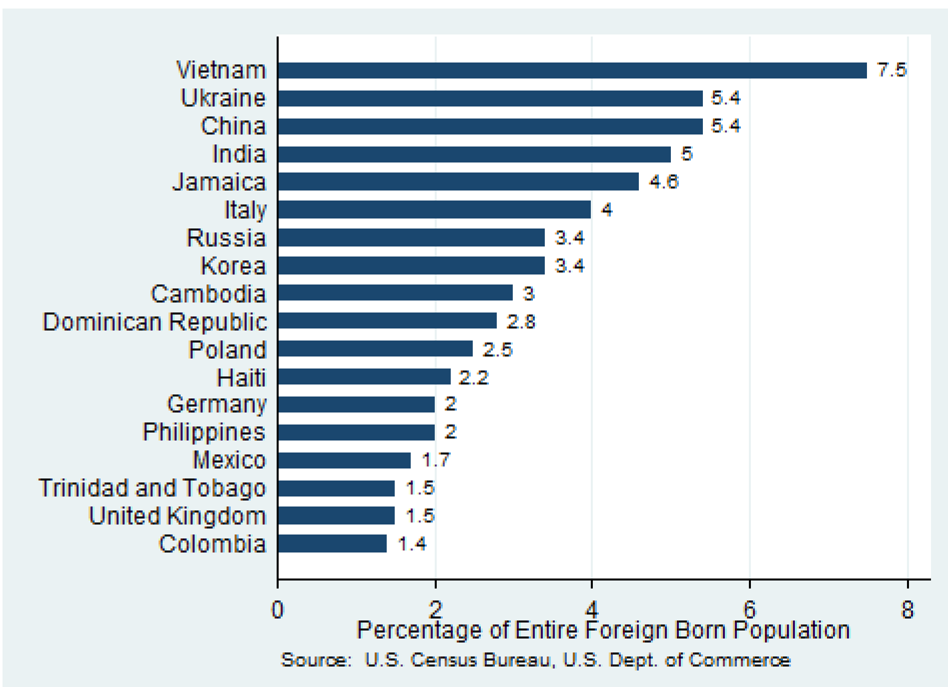

Figure 3.6 Country of Birth of Foreign Born Residents

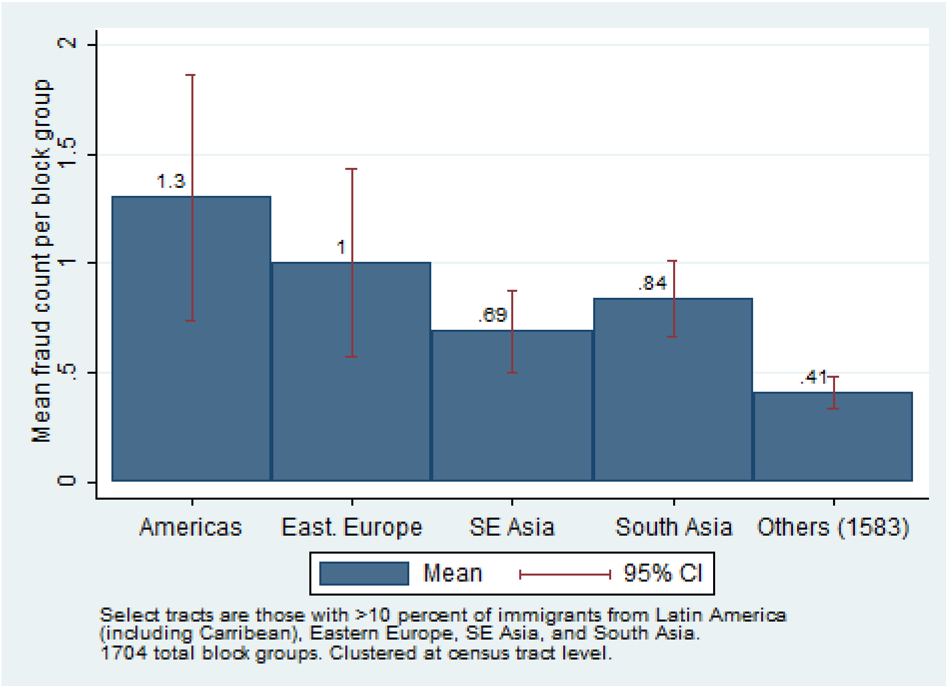

Figure 3.7 Frequency of Fraud by Birth Region Concentration

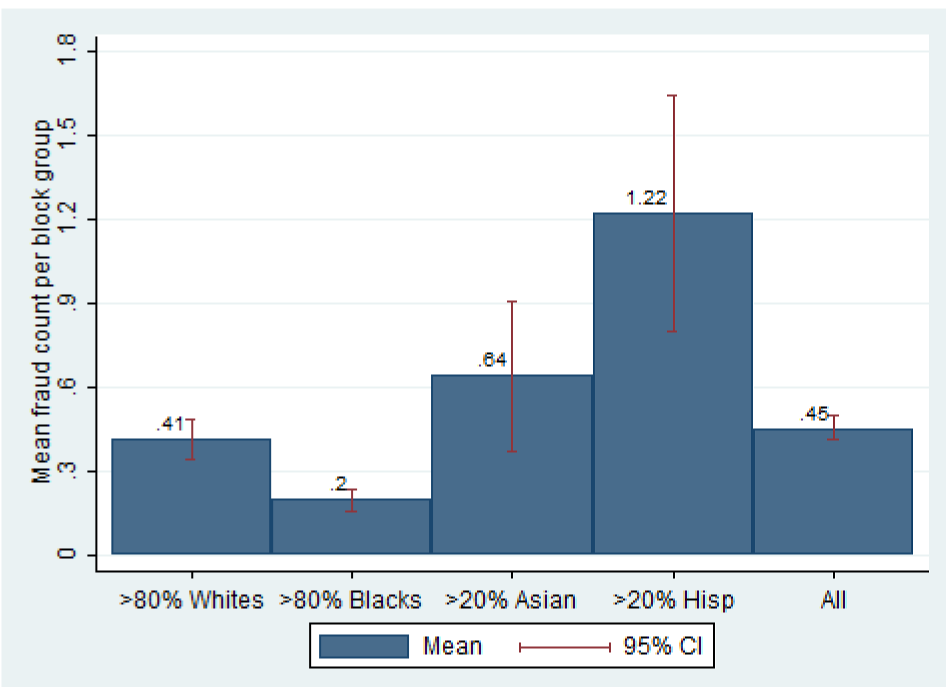

Figure 3.8 Frequency of Fraud by Ethnic Concentration

Figure 3.9 Fraud Frequency and the Cultural Norms Index

Table 3.1 Country Corruption Index List

Sweden	1	Hungary	24	Egypt	54	El Salvador	75
Netherlands	2	South Africa	24	China	54	Guatemala	76
Canada	3	Taiwan	25	Lebanon	55	Syria	77
UK	3	Malaysia	29	Bosnia	56	Nicaragua	78
Germany	5	Czeck Republic	30	Yugoslavia	56	Honduras	78
Austria	5	Cuba	30	India	56	Sierra Leon.	79
Ireland	7	Italy	31	Guyana	59	Russia	82
U.S.	8	Trinid. and Tobag.	31	Mexico	59	Venezuela	84
Hong Kong	8	Korea	35	Laos	60	Pakistan	84
France	9	Brazil	39	Vietnam	60	Ecuador	87
Barbados	9	Thailand	40	Colombia	60	Nigeria	90
Spain	12	Jordan	40	Bangladesh	61	Indonesia	90
Chile	12	Philippines	45	Turkey	66	Cambodia	91
Portugal	13	Panama	45	Belarus	67	Ukraine	94
Israel	15	Ghana	46	Romania	70	Iraq	96
Greece	16	Jamaica	47	Ethiopia	70	Haiti	97
Japan	17	Argentina	47	Dom. Repub.	71	Afghanistan	100
Costa Rica	20	Bolivia	49	Armenia	73		
Poland	23	Peru	50	Iran	74		

Table 3.2 Case Counts at Blockgroup Level

Variable	Mean	StdDev	Min	Max
No. fraud cases	0.5	0.9	0	9
No. fraud cases-UtilityCo	0.2	0.6	0	5
No. fraud cases-DataCo	0.3	0.6	0	6
No. flagged cases	1	1.6	0	16
No. flagged cases-UtilityCo	0.6	1.2	0	12
No. flagged cases-DataCo	0.4	0.7	0	8
N	1704			

Table 3.3 Billed Amounts by Blockgroup Level

Variable	Mean	StdDev	Min	Max	N
Bill per fraud site	2,522	2651	0	25,367	490
Bill per flagged site	1,601	2062	0	25,367	850
Bill for fraud cases	4,074	4922	0	47,497	490
Bill for flagged cases	3,521	5037	0	57,368	850

Table 3.4 Control Variables at Blockgroup Level

Variable	Mean	StdDev	Min	Max
Cultural norms index (norm.)	0	1	-1.4	7.9
Fraction of non-US born pers.	0.1	0.1	0	0.7
N	1704			

Table 3.5 Fraud and Control Variables Correlation

	(1)	(2)
No. fraud cases		
LOG Income per capita	0.71	0.73
	(0.15)	(0.15)
Fraction of persons in poverty	0.39*	0.47
	(0.20)	(0.23)
LOG no. house structures	1.93***	1.90***
	(0.18)	(0.17)
LOG no. apt structures	0.97	0.95
	(0.034)	(0.033)
Fract. of housing units with gas heat	1.42	1.30
	(0.63)	(0.56)
LOG no. crime cases	1.19***	1.21***
	(0.078)	(0.079)
Fraction self-employed	0.074	0.053
	(0.24)	(0.17)
Fract. of adults with high school educ.	1.31	1.51
	(1.32)	(1.53)
Fract. of adults with some college educ.	1.86	1.83
	(1.53)	(1.52)
Fract. of adults with college or more educ.	0.28	0.26
	(0.26)	(0.23)
Fraction of blacks	0.42***	0.45***
	(0.079)	(0.084)
Fraction of asians	3.87**	0.86
	(2.18)	(0.69)
Fraction of hispanics	1181.2***	642.6***
	(1265.9)	(710.1)
Fraction of non-US born persons		10.4***
		(7.91)
Observations	1704	1704

Exponentiated coefficients; Standard errors in parentheses
Coefficients are incidence rate ratios for the negative binomial regression.
Errors clustered by census tract (355 clusters).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.6 Fraud and Cultural Norms-Fraud Cases

No. fraud cases	(1)	(2)	(3)	(4)
Cultural norms index (normalized)	1.41*** (0.070)	1.35*** (0.055)	1.21*** (0.058)	1.43** (0.22)
Fraction of non-US born persons				0.074 (0.17)
Observations	1704	1704	1704	1704

Note: exponentiated coefficients; standard errors in parentheses; coefficients are incidence rate ratios for the negative binomial regression; errors clustered by census tract. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.7 Fraud and Cultural Norms Fraud Cases (DataCo)

No. fraud cases-DataCo	(1)	(2)	(3)	(4)
Cultural norms index (normalized)	1.45*** (0.077)	1.37*** (0.057)	1.20*** (0.064)	1.22 (0.20)
Fraction of non-US born persons				0.79 (2.04)
Observations	1704	1704	1704	1704

Note: exponentiated coefficients; standard errors in parentheses; coefficients are incidence rate ratios for the negative binomial regression; errors clustered by census tract. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.8 Fraud and Cultural Norms Fraud Cases (UtilityCo)

No. fraud cases-UtilityCo	(1)	(2)	(3)	(4)
Cultural norms index (norm.)	1.31***	1.30***	1.26***	1.87***
	(0.078)	(0.077)	(0.089)	(0.44)
Fraction of non-US born persons				0.0020*
				(0.0070)
Observations	1704	1704	1704	1704

Note: exponentiated coefficients; standard errors in parentheses; coefficients are incidence rate ratios for the negative binomial regression; errors clustered by census tract; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.9 Fraud and Cultural Norms-Cost per Fraud

	(1)	(2)	(3)	(4)
Cultural norms index (normalized)	96.7	97.6	164.0	508.6
	(84.2)	(87.2)	(121.2)	(345.1)
Fraction of non-US born persons				-5366.3
				(4670.4)
Observations	489	489	489	489

Note: estimated using OLS; top one percent of dependent variable observations dropped; sample limited to blockgroups with fraud cases. errors clustered by census tract; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.10 Fraud and Cultural Norms-Flagged Cases

No. flagged cases	(1)	(2)	(3)	(4)
Cultural norms index (normalized)	1.37*** (0.056)	1.30*** (0.044)	1.18*** (0.051)	1.42*** (0.17)
Fraction of non-US born persons				0.058* (0.100)
Observations	1704	1704	1704	1704

Note: exponentiated coefficients; standard errors in parentheses; coefficients are incidence rate ratios for the negative binomial regression; errors clustered by census tract; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.11 Fraud and Cultural Norms-Flagged Cases (DataCo)

No. flagged cases-DataCo	(1)	(2)	(3)	(4)
Cultural norms index (normalized)	1.42*** (0.065)	1.35*** (0.048)	1.22*** (0.055)	1.29* (0.19)
Fraction of non-US born persons				0.44 (1.07)
Observations	1704	1704	1704	1704

Note: exponentiated coefficients; standard errors in parentheses; coefficients are incidence rate ratios for the negative binomial regression; errors clustered by census tract; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.12 Fraud and Cultural Norms-Flagged Cases (UtilityCo)

No. flagged cases-UtilityCo	(1)	(2)	(3)	(4)
Cultural norms index (normalized)	1.36***	1.31***	1.20***	1.64***
	(0.069)	(0.059)	(0.075)	(0.27)
Fraction of non-US born persons				0.0071**
				(0.017)
Observations	1704	1704	1704	1704

Note: exponentiated coefficients; standard errors in parentheses; coefficients are incidence rate ratios for the negative binomial regression; errors clustered by census tract; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.13 Fraud and Cultural Norms -Cost per Flagged Case

	(1)	(2)	(3)	(4)
Cultural norms index (normalized)	98.9*	102.1*	123.8	380.7
	(59.6)	(57.7)	(81.3)	(242.2)
Fraction of non-US born persons				-3979.3
				(3266.7)
Observations	846	846	846	846

Note: estimated using OLS; top one percent of dependent variable observations dropped; sample limited to blockgroups with fraud cases. errors clustered by census tract; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.9 References

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