Do SBA Loans Create Jobs?

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Abstract

Small Business Administration (SBA) loans are among the most significant firm-level policy interventions in the U.S., but little is known about their outcomes. We estimate employment effects using 1976-2010 data on all U.S. employers. Our methods combine fixed-effect regressions with matching on exact firm age, industry, year, and pre-loan size, and on propensity scores as a function of employment history. The results imply a 25 percent average effect on loan recipient employment and an increase of 5.4 jobs for each million dollars of loans. We also estimate total job creation by the SBA and the cost per job created.

JEL codes: D04, G21, G28, H32, H81, J23, L53

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1. Introduction

The number one "strategic goal" of the Small Business Administration (SBA) is "growing businesses and creating jobs."[1] The urgency of the employment objective has increased during the recent period of high unemployment in the U.S. Nearly all political groups have reached a rare consensus that small businesses are the primary source of job creation, and the budget of the SBA has been steadily increased, reaching "all-time records in the Agency's history, with over $30 billion in lending support to 60,000 small businesses in its top two lending programs — 7(a) and 504" during fiscal year 2011.[2]

Do SBA loan programs actually raise employment? Theoretically, the answer is ambiguous. Easier access or lower cost of capital may enable expansion, a scale effect. But it may also induce capital-labor substitution, and it would reduce employment if capital and labor are gross substitutes. Moreover, even if the scale effect dominates, so that the factors are gross complements, the employment rise may be attenuated if the program crowds out other sources of capital, and the aggregate employment effect may be reduced if there are general equilibrium displacement effects (negative spillovers onto competing firms).[3] An empirical analysis of the programs is also difficult for several reasons: many factors influence employment and growth, loan receipt may be subject to selection bias (positive or negative), and appropriate firm-level microdata have usually been unavailable.

Perhaps as a result of these problems – and despite the prominence of SBA programs, their large size and high costs, and the many hopes vested in their power to stimulate business growth – there have been few attempts to evaluate them using appropriate data and econometric methods. Unlike job training programs, for example, where researchers have long estimated employment and wage impacts using appropriate micro data and program evaluation methods, analysts of SBA loan effects have had to rely on small samples, short time series, or aggregated data that do not permit the use of recent developments in econometrics (e.g., Imbens and

[1] See SBA (2013a). This goal is the first of three; the other two (which would be still more difficult to evaluate) are "building an SBA that meets the needs of today's and tomorrow's small businesses" and "serving as the voice for small business."

[2] SBA programs have received strong support both from congress and all recent presidential administrations, and small businesses are frequently cited as "…the places where most new jobs begin" (e.g., Whitehouse.gov, President's Weekly Address, February 6, 2010). For the budget figures, see SBA 2013b.

[3] Hurst and Pugsley (2011) have recently criticized SBA programs on the basis that typical (median) small firms neither grow nor report wanting to grow, and thus that the emphasis on small businesses is misplaced. Our paper does not analyze the broader set of policies towards small businesses, but we do show that recipients of SBA loans differ from the median in tending to grow prior to loan receipt – an important issue for our identification strategy.

Wooldridge, 2009). Most previous evaluations of small business programs consist of simple comparisons before and after the policy interventions, with little use of comparison groups of nonrecipients. The most common unit of observation in SBA studies is a geographic area such as the county, with outcomes measured as overall employment or per-capita income in the local area; Craig et al. (2009) review these studies. Many factors affect county-level employment and income, of course, which makes it difficult to disentangle the effects of a program that is small relative to the local economy. The SBA itself reports a "performance indicator" – the number of "jobs supported," reported in recent years at over 0.5 million.[4] Although the exact calculation of this indicator is unclear, it seems to be based on summing up the borrowers' statements on loan applications concerning their intentions to create or retain jobs.

Our research aims to contribute to estimating these employment impacts by using much better data than were heretofore available and by applying recent econometric methods developed for estimating causal effects with such data. We link administrative data on every SBA 7(a) and 504 program loan to long-panel data on the universe of employers in the U.S. economy, and we use the linked data to implement a longitudinal matching estimator (e.g., Heckman et al., 1997, 1998). The annual panels in our data run from 1976 to 2010 and permit us to select comparator firms based on age, industry, and several years of employment history, to control for time and firm-fixed effects, and to measure the evolution of employment before and after the loans were awarded. We use multiple control groups, differentiated by distance from the loan-recipients, to assess possible general equilibrium (displacement) effects of the loans.

The paper builds on previous research on small business, finance, and government policy in several ways. Much of the recent small business controversy in the U.S. has actually not concerned policy directly, but rather the empirical relationship between business size and employment growth. Birch's (1987) claim that small businesses were responsible for most job creation is widely cited as the basis for government programs supporting this sector, although the underlying methods have been questioned by Davis, Haltiwanger, and Schuh (1996) (See also Neumark, Wall, and Zhang 2011, Hurst and Pugsley 2011, and Haltiwanger, Jarmin, and Miranda 2013). But the size-growth relationship is a different issue from the impact of the programs on business growth and performance, which is the question relevant for policy and the one we address in this paper.

[4]The figure is 583,737 for Fiscal Year 2010 (the most recent provided) in SBA 2013c, Appendix 3.

Evaluating the effects of SBA loans on job creation is also related to macroeconomic debates on the size of the government spending multiplier (e.g., Ramey 2011). As in this paper, some of the recent literature on that question uses micro-data (e.g., Parker 2011 and Parker et al. 2011). Our analysis of potential displacement effects is relevant for the question whether government spending merely reallocates resources across economic agents or whether it also has an aggregate effect.

Finally, the paper is relevant for the broader theoretical and empirical literature on finance and growth (Levine 2005; Clementi and Hopenhayn 2006). As emphasized in Beck's (2009) review of the econometric research, a standard identification problem is determining the direction of causality between growth and finance, and despite a long list of empirical studies, the degree to which financial development promotes economic growth remains controversial. Most studies use aggregate (typically country-level) data. Those using firm-level data frequently employ country-level measures of financial development, because of the difficulty of measuring financial constraints at the firm level; see the controversy over the approach of analyzing the relationship between investment and cash flow (Hubbard 1998). By contrast, in this paper we are able to analyze a specific policy intervention varying at the firm level, which may be a unique contribution to this literature.[5]

Section 2 describes the SBA programs we analyze. Section 3 describes the data, including the matched control samples. Section 4 outlines our evaluation methodology. Section 5 provides results for several alternative estimators and reports analyses of robustness and displacement. Section 6 summarizes the caveats and implications, including estimates of the total job creation and cost per job associated with the SBA loan programs.

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.[6] Most 7(a) loans (those not part of a special subprogram) have a maximum amount of $5 million, with a maximum 85 percent SBA guarantee for loans up to

[5] Beck (2009) concludes his review of this literature with a call for firm-level studies evaluating the growth effects of finance by analyzing specific policy interventions, which is our purpose in this paper.

[6] SBA (2013d) is the primary source for our description, and it contains further details. Glennon and Nigro (2005) and de Andrade and Lucas (2009) also describe the 7(a) program; they focus on defaults and on costs to borrowers, respectively, and they do not analyze the effects of the loans on employment or other outcomes.

$150,000, and a 75 percent maximum guarantee for higher amounts. They purposes are listed as expansion/renovation, new construction, land, buildings, equipment, working capital, debt refinancing for compelling reasons; seasonal line of credit; and inventory. Maturity is supposed to depend 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 (subsidized under the recent stimulus program), which increases with maturity and loan amount. To qualify, a business must be for-profit; meet SBA size standards;[7] show good character, management expertise, and a feasible business plan; not have funds available from other sources;[8] and be an eligible type of business.[9] 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. 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. 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 may receive CLP status.

A final large category of 7(a) is the express loan program. These have a $350,000 maximum loan amount and 50 percent maximum SBA guaranty. Interest rates can be higher

[7] The size standards vary by 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.
[8] 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." In practice, the lender's refusal to give the applicant a conventional loan is normally considered sufficient to meet this requirement.
[9] This includes engaging in business in the United States; possessing reasonable owner equity to invest; and using alternative financial resources, including personal assets, before seeking financial assistance.

than on other 7(a) loans. Qualified lenders may be granted authorization by the SBA to make eligibility determinations. The SBA promises a decision within 36 hours.

The 504 Loan Program offers loan guarantees up to $5 million or $5.5 million, depending on the type of business. 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.

3. Data

We use a database on all 7(a) and 504 loans guaranteed by the SBA from inception in 1953 through 2009 to identify loan recipients, amounts, and time of receipt. Loan amounts are converted to real 2010 prices using the annual average Consumer Price Index. Loan timing is based on the date the SBA approved the loan. In order to exclude 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 match the SBA 7(a), 504, and disaster loan data to the Census Bureau's employer and non-employer business registers. As shown in Table A1 we use the following passes: The first is an exact match on 5-digit zip code, exact match on standardized street address, and exact match on standardized business name. For those observations unmatched after this pass, the second 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 third 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 fourth pass is an exact match on 5-digit zip code and business name allowing for some fuzziness; and the fifth pass is place (city) soundex, 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, which is prioritized over the third, 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 registers in the following year, and finally to registers in the previous year.

The Census Bureau's Longitudinal Business Database (LBD) consists of longitudinally linked employer business registers (Jarmin and Miranda 2002). The LBD tracks all firms and establishments in the U.S. non-farm business sector with paid employees on an annual basis in 1976-2010. 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, establishment age (calculated based on the first year the establishment appears in the dataset), 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 2002-2010. We assign each establishment the latitude and longitude of the centroid of its 5-digit zip code using centroids for the decennial census years of 1990, 2000, and 2010, applying the 1990 centroids to the years 1976-1990, and linearly interpolating the centroids for 1991-1999 and 2001-2009.

As shown in Table 1, 55.39 percent of the SBA 7(a) and 504 loans could be matched to business registers.[10] In this study we focus on single-establishment employer businesses receiving an SBA loan after their first year in operation.[11] Among firms receiving multiple SBA loans, we select the first 7(a) or 504 loan as the treatment. We drop firms with an SBA disaster loan prior to their first 7(a) or 504 loan and those receiving their first 7(a) loan prior to 1977.[12] Our identification method uses employment in the year prior to loan receipt, so we drop firms that do not have it in the LBD. Finally, we drop firms for which no suitable controls are found. Table 1 reports the number of loans dropped as after imposing each of these restrictions.

Potential biases can result from the fact that not all single-establishment employer businesses receiving an SBA loan after start-up are included in the regression analysis. To get some feel for the nature of the possible bias, in Table 2 we display descriptive statistics from the

[10] Among loans issued to firms identifying themselves on the loan application as existing businesses, the match rate is somewhat higher, at 59.26 percent.

[11] We drop loans issued to an entity that is part of a multi-establishment firm in the loan year or any earlier year. Though the effects of SBA loans on multi-establishment firms and start-ups are of interest, they require different identification methods, so we leave them for future research.

[12] The first 504 loans were issued in 1986. Our identification methods require at least one year of data prior to receipt of the loan, and the LBD starts in 1976, necessitating dropping 7(a) loans prior to 1977.

SBA loan applications for four different samples: those reporting to be an existing business and not linked to any business register, those not in the final sample due to missing employment in the year prior to the loan, those not in the final sample because no suitable control firms were found, and the main regression sample. Those not linked or missing LBD employment tend to be smaller firms relative to those in the regressions, and more of them are minority-owned and sole proprietorships or partnerships. In contrast, fewer recipients without suitable control firms are minority-owned or sole proprietorships or partnerships, and they are generally larger. More of those without controls are in manufacturing and fewer are in construction or services. There are nearly nine times as many loans in the not matched to any business register and missing LBD employment groups as there are in the group without suitable controls, so overall, the regression sample has higher average employment than the other groups.

Table 3 shows descriptive statistics using variables from the LBD for matched SBA firms (treated firms), as well as all other LBD firms (excluding multi-establishment firms and those ever in a multi-establishment firm in the past). The standard deviation of employment for firms not receiving SBA loans (i.e., non-treated firms) is much larger, reflecting the fact that large firms are ineligible for SBA programs. Treated firm median employment is higher and mean employment is about the same as for non-treated firms, however, suggesting that SBA loan recipients tend to be larger firms within the small business sector. Treated firms are younger on average. More treated firms are in manufacturing and wholesale and retail trade compared to non-treated firms. These differences could affect employment growth, so a simple comparison of treated and non-treated firm employment growth is likely to be misleading.

4. Empirical Strategy

4.1 Estimation Methods

Our goal is to estimate the causal effect of SBA loan receipt on employment. 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 had not received a loan is y^0_{it+s}. The loan's causal effect for firm *i* at time *t*+*s* is defined as $y^1_{it+s} - y^0_{it+s}$, and the average effect of treatment on the treated as $E\{y^1_{it+s} - y^0_{it+s}|TREAT_{it} = 1\} = E\{y^1_{it+s}|TREAT_{it} = 1\} - E\{y^0_{it+s}|TREAT_{it} = 1\}$. The standard evaluation problem is that y^0_{it+s} is unobserved for non-recipients; thus $E\{y^0_{it+s}|TREAT_{it} = 1\}$, the average employment outcome of loan recipients had they not received a loan, must be estimated. The principal identification approach constructs this counter-factual average using the average employment of never-treated firms, $E\{y_{it+s}^{0}|TREAT_{it}=0\ \forall\ t\}$.[13] As we showed in the previous section, however, treated and nontreated firms differ in several important characteristics, motivating us to use matching techniques to select a control group.

Our matching procedures include sample restrictions, exact matching on several firm characteristics, and propensity score matching on several other variables, including a four-year history of employment. As discussed in Section 3 above, we limit our treated sample to firms in the LBD that have been single-establishment firms since birth, ones that are at least one year old when receiving their first SBA loan, those receiving their first SBA 7(a) or 504 loan in 1977-2009, those not receiving an SBA disaster loan prior to their first 7(a) or 504 loan, with non-missing employment in the LBD in the year before loan receipt, and with no employment outliers in the LBD throughout the 1976-2010 period.[14] We symmetrically restrict the non-treated sample so that a firm eligible to be a candidate control firm for a particular treated firm must have non-missing employment in the year prior to the treated firm's loan receipt (which also means it isn't a new start-up in the year of loan receipt) and no employment outliers in the LBD; it can never have received an SBA 7(a), 504, or disaster loan at any time between 1953-2010; and it can never have been a part of a multi-establishment firm through the year of loan receipt for the treated firm.

The exact matching requirements for control firms include industry, age-group, size-group, and year. Specifically, for a firm to be a candidate control firm for a particular treated firm 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 2002-2009) in the treated firm's loan receipt year. It must be in the same firm age category (1-2 years old, 3-5 years old, 6-10 years old, and 11 or more years old) in the treated firm's loan receipt year. It must be in the same firm employment category (1 employee, 2-4 employees, 5-9 employees, 10-19 employees, 20-49 employees, 50-99 employees, and 100 or more employees) in the year before the treated firm's loan receipt year. Among firms with 19 or fewer employees in the previous year, we also require the candidate

[13] An alternative we consider briefly is $E\{y_{it-\mathrm{r}}^{0}|TREAT_{it}=1\}$, r>0, the pre-treatment values for the treated firms.
[14] We define the following cases as outliers: an employment increase or decrease of more than ten times between the first and second year of life or the second-to-last and last year of life; or an employment increase (decrease) of more than five times followed in the next year by an employment decrease (increase) of more than five times.

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, 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 ten or fewer employees, a majority of the sample, employment must match exactly.

The exact matching creates a set of controls essentially identical in pre-treatment size, industry, and age. But there are other variables that we would like to match on, especially employment history and wage for which it is difficult to design matching thresholds for each variable separately, so we reduce this dimensionality problem with propensity score matching. After dropping treated firms with no candidate controls based on the sample restrictions and exact matching conditions, we estimate separate probit regressions for each age-size group using the sample of treated firms and their candidate controls. There are 28 age-size groups, based on the 4 age groups and 7 size groups defined above. The probit regresses a dummy for SBA loan receipt on the following variables: 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. For the three lagged employment growth variables and for the log of payroll/number of employees in the year prior to the treated firm's loan receipt, we also impute zeroes in place of missing values and include dummies for such cases. Conditioning on four years of lagged employment is intended to create a control group with very similar employment histories to the treated firms, and it follows Heckman et al.'s (1997, 1998, 1999) recommendations for evaluating the outcomes of labor market training programs.

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.

We limit the treated and non-treated firms in the final sample 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.[15] 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.[16] 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, we are also able to eliminate unobserved, time-invariant differences in employment through difference-in-differences (DID) regressions, again following Heckman et al.'s (1997, 1998, 1999) recommendations to combine matching with DID methods, although our data contain much longer time series than in the typical labor market program evaluation upon which they focus.

The employment regressions take the form $y_{it} = \boldsymbol{L_{ij\tau}\gamma_{ij\tau}} + \boldsymbol{\rho_t} + \alpha_{ij} + \boldsymbol{\theta_{it}\delta} + u_{ijt}$, where *y* is employment, *i* indexes firms from 1 to *I*, *j* indexes from 1 to *R* the treated firms to which the firm is a control,[17] and *t* indexes the years from 1 to *T*. $\boldsymbol{L_{ij\tau}}$ is a 1 x 66 vector of event time dummies. Designating τ as the index of event time, the number of years since the treated firm received its first SBA loan, $\tau = \ldots -3, -2, -1, 0, 1, 2, 3 \ldots$ such that $\tau < 0$ in the pre-loan

[15] We have also carried out the analysis with a much smaller bandwidth (0.98 to 1.02) and obtained very similar results.

[16] 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.

[17] For treated firms, *i*=*j*.

years, $\tau = 0$ in the year of loan receipt, and $\tau > 0$ in the post-loan years.[18] ρ_t is a 1 x 35 vector of year dummies, α_{ij} is a fixed effect for each firm for each treated firm to which it is matched, and u_{ijt} is an idiosyncratic error.[19] In alternative specifications, y_{it} is the firm's employment and the natural logarithm of the firm's employment. The log specification has several advantages: it accounts for the skewed distribution of employment, the impact of SBA loan receipt is assumed proportionate to employment rather than absolute, and problems of heteroskedasticity are mitigated, while the unlogged specification permits more direct estimates of the effects of receiving an SBA-backed loan and of receiving different loan amounts on the number of jobs created.

$\boldsymbol{\theta_{it}}$ 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 amount of the SBA loan in the post-loan period (equal to zero for non-recipients in all periods), expressed in $millions.[20] Some specifications include only the loan amount, and some also include the loan amount squared. We also estimate dynamic specifications including treated-firm-specific dummy variables for the years before and after first SBA loan receipt. For treated firms, these dummy variables take on identical values to the event time dummies described above, except that we pool all years earlier than 4 years prior to treatment into a single base category, while for non-treated control firms, they are always zero.

Table 4 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. Note that treated firms have more post-treatment years on average, indicating a higher survival rate.

4.2 Specification Tests

[18] These event time dummies, which are sometimes non-zero for all firms, in conjunction with θ_{it}, which are sometimes non-zero only for treated firms, are necessary to make these DID regressions.

[19] The standard errors are cluster-adjusted by firm. We have also bootstrapped some specifications, and the standard errors are similar to those reported here.

[20] If a firm is reported to have received multiple SBA loans in the year, the loan amounts are combined.

The reliability of these methods for estimating an average treatment effect on the treated depends on whether, conditional on the variables in the exact and propensity score matching and the fixed effects in the regressions, the potential outcomes y^0 are independent of treatment incidence (e.g., Imbens and Wooldridge 2009). This unconfoundedness assumption cannot be tested directly, but we can evaluate it partially in three ways. First, the matching literature typically checks the balance of pre-treatment variables between the treated and control groups, and for this purpose we perform standardized difference (or bias) tests for the main variables included in the matching probit regressions. Table 5 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 and average wage are substantially larger 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, employment growth, and wage biases are reduced by over 89 percent, while age bias is reduced by 38 percent.[21] None of the biases are close to being large after matching.[22]

Second, we analyze the pre-treatment event time dynamics of employment for treated versus control firms. The "pre-program test" of Heckman and Hotz (1989) involves a comparison of the level of the outcome variable, typically in a single pre-treatment period, but we are equally interested in assessing the extent to which the two groups display diverging trends prior to treatment. If the estimated program effects reflect improved access to finance, rather than selection into the program, then there should be no pre-treatment divergence between treated firms and controls, and the presence of such a divergence would provide evidence of selection bias. For this purpose, we study the dynamics of loan effects on employment in event time, estimating separate effects by years normalized around the loan year. Grouping together all years five and more years before the loan as the base period (a normalization is necessary because of the inclusion of firm fixed effects), we permit the estimated coefficient to vary for each year from four years before to 33 years after the loan. Examining the dynamics of the estimates prior to the loan provides a Heckman-Hotz (1989) "pre-program test" of the

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

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

specification: if we observe large differences between the treated and control firms prior to the loan, and particularly if we observe differing trends, then this would be symptomatic of residual selection bias, even conditioning on our matching and regression procedures.

Finally, we construct a "placebo test" that is also motivated by the finance versus selection mechanisms that may underlie a measured correlation of employment with loan receipt. The test uses information not only on whether a firm receives an SBA loan but also on the size of the loan. If SBA loans raise employment because they improve access to capital, then it stands to reason that the effect should rise with the size of the loan, and there should be no effect of loan receipt per se (essentially, of receiving a loan of size zero), once loan amount is controlled for. On the other hand, if program participation is estimated to affect employment growth regardless of loan size, then this would provide evidence of biased selection into the program. A simple way to implement this test is to include both the post-loan dummy and loan amount in the same equation; because a significant coefficient on the dummy might reflect nonlinearities in the effect of loan amount, we also consider specifications with a quadratic form for loan amount.

4.3 Potential Sources of Bias

Even for estimators satisfying these specification tests, there are potential biases arising from the characteristics of the data and selection of the sample, from heterogeneity in the effects, and from possible self-selection into the SBA loan programs. We consider each of these in turn. Beginning with the nature of the data and sample, a first issue is that the SBA data available to us do not distinguish disbursed from cancelled loans. The treated sample therefore contains an unknown number of cases of cancellations, but Dilger (2013) reports that 7-10 percent are cancelled each year. Assuming the effect of SBA loans is larger for firms actually receiving them than for those not, the inclusion of untreated firms in the treatment group leads to a negative bias on our estimate of the causal effect on employment. Second, our inability to match all SBA recipients in the SBA data to the LBD implies that the constructed control group may well contain treated firms; we cannot estimate the magnitude of this problem, but if the true SBA effect is positive, then misallocating loan recipients to the controls implies a downward bias. Other potential biases stem from restrictions on the sample necessary for convincing identification. Start-ups are excluded because the lack of employment history prevents us from matching, but if start-ups have a stronger employment response to SBA loans than do existing firms, then this exclusion again implies a negative bias. Multi-establishment firms are excluded because of the difficulties of isolating the establishment that gains from the loan and of using geographic criteria in matching, but if loans are frequently used to set up new establishments and these involve above-average levels of job creation, then the result is a downward bias in our estimates. Finally, firms with no employees prior to loan receipt are excluded, because of lack of information on non-employers, but again if such firms have a stronger employment response, the effect will be to bias our estimates downward. Thus, these sources of potential bias all tend to work in the same direction, implying under plausible assumptions that our estimates are lower bounds on the true causal effect.[23]

A second category of issues about our estimates is the potential for heterogeneity in causal effects, while we estimate an average for all treated firms. The dimensions of heterogeneity are many: firm size, age, industry, and region; program characteristics such as guarantee rate, interest rate, and term; and calendar time and business cycle. The average effects we have estimated need not be simple or weighted averages of effects estimated for groups defined along these dimensions separately, but the direction of the difference is unknown, and we leave examination of heterogeneity to future research.

The final set of issues concerns potential selection bias remaining even after the matching and regression procedures. For a specification passing the tests described in the previous sub-section, any remaining selection bias must be time-varying, reflecting for example a positive demand shock received by treated firms – but not by controls – precisely during the treatment year. In what follows, we use the notation S_{it} = 1 for firms receiving this positive shock (or having an idea or a project) in year *t*. Defining variables as demeaned by firm, calendar year, and event year (the adjustments taken into account by the regressions), treated firms receiving the demand shock will have average employment $E\{y_{it+s}|TREAT_{it}=1,\ S_{it}=1,X\}$ and treated firms not receiving the positive shock will have $E\{y_{it+s}|TREAT_{it}=1,\ S_{it}=0,X\}$, where *t* is again the treatment year, *s* is the number of years since treatment, and *X* is the set of matching variables. The average among all treated firms is then a weighted average of these two groups, where the weights are proportions of firms in each group: $E\{S_{it}|TREAT_{it}=1\}$ and 1-$E\{S_{it}|TREAT_{it}=1\}$, respectively.

[23] Most of the assumptions are not testable, but we have discussed the multi-unit issue above and carried out some preliminary analysis suggesting that start-ups with SBA loans may indeed grow faster than those without, an issue we plan to address more extensively in future research.

In addition to demand shocks, another issue crucial for non-treated firms is whether they receive finance without SBA support, such as a conventional loan from a bank or informal credit. We have no information on any sources of capital aside from the SBA loan, so we cannot distinguish these in the data, but it is useful to consider these possibilities. K_{it} = 1 denotes those firms receiving finance through a non-SBA source in year *t*. We can distinguish four types of controls according to whether they receive the shock and whether they get non-SBA finance, with the following average employment in year *s* after the loan: $E\{y_{it+s}|TREAT_{it} = 0,\ S_{it} = 0, K_{it} = 0\}$ for controls receiving neither a positive shock nor non-SBA finance, $\{y_{it+s}|TREAT_{it} = 0,\ S_{it} = 1, K_{it} = 0\}$ for those receiving the shock but no other finance, $E\{y_{it+s}|TREAT_{it} = 0,\ S_{it} = 0, K_{it} = 1\}$ for those not getting the shock but receiving non-SBA finance, and $E\{y_{it+s}|TREAT_{it} = 0,\ S_{it} = 1, K_{it} = 1\}$ for those getting both the shock and other finance. The average among all controls is a weighted average of these, with weights determined by the proportions with $S_{it} = 1$ and $K_{it} = 1$ among the controls. If $E\{y_{it+s}|S_{it} = 1\} > E\{y_{it+s}|S_{it} = 0\}$ and $E\{y_{it+s}|K_{it} = 1\} > E\{y_{it+s}|K_{it} = 0\}$ so that positive shocks and increased finance lead to higher employment, then the estimator $E\{y_{it+s}|TREAT_{it} = 1\}$ - $E\{y_{it+s}|TREAT_{it} = 0\}$ will be increasing in the share of treated firms receiving positive shocks and decreasing in the share of controls receiving shocks and the share of non-SBA finance.

This analysis clarifies both some potential sources of bias and the interpretation of our estimates. Assuming, conditional on our matching procedures and firm fixed effects, that treatment is independent of the shock, so that $E\{TREAT_{it}|S_{it} = 1, X\} = E\{TREAT_{it}|S_{it} = 0, X\}$ and if in addition, there is no non-SBA finance ($K_{it} = 0$), then our estimate can be interpreted as the causal effect of finance on growth. Maintaining the conditional independence of treatment, but permitting non-SBA finance, implies that our estimate is the causal effect of the program. Since K_{it} is unobserved, the latter interpretation is the appropriate one for our context.

Relaxing the conditional independence assumption, selection bias reflects differences between the counterfactual outcome for treated firms and the observed outcome for the controls: *B(X)* = $E\{y^0_{it+s}|TREAT_{it} = 1, X\}$ - $E\{y_{it+s}|TREAT_{it} = 0, X\}$. If $E\{TREAT_{it} * S_{it}|X\} > 0$, so that treated firms would have performed better than non-treated even if the former had not been treated, then *B(X)* > 0. The magnitude of *B(X)* depends on the strength of the correlation between treatment and demand shock. We try to assess this below by considering contexts where arguably all firms have strong demand shocks and by using alternative control groups.

5. Results

5.1 Estimates from Alternative Specifications

In order to understand the data and the nature of the evaluation problem, it is useful before presenting the results from our preferred estimator to take a step back and consider some alternative empirical strategies, which include some approaches that are more traditional in program evaluations. The alternatives include simple after-before differences and regressions for treated firms only, and – for treated and non-treated firms – matching without regression, regression without matching, and a combined matching and regression estimator (our preferred approach). We assess these alternatives on two criteria: a placebo test of participation in the loan program versus the effect of the size of the loan, and a pre-program test of loan effects prior to loan receipt. We also report short- and long-run dynamics of the estimated effects.

A first alternative, frequently used by program evaluation offices of agencies with access to administrative data, is the after-before estimator (AB), which uses treated firms only to compare mean employment in the post- and pre-treatment periods. The top panel of Table 6 shows mean employment is 11.9 pre-treatment, and it rises to 20.9 post-treatment, an increase of 9 jobs per loan. Of course, this estimator makes no use of control groups, so the counter-factual to post-treatment employment is simply the continuation of pre-treatment employment; nor does it take into account loan size. Mean employment among all firms in the LBD except those we can identify as treated is 15.69; this is the average across all years, since a treatment year is undefined for untreated firms in the absence of matching.

Figure 1 shows the corresponding evolution of employment in event-time for each year before and after loan receipt among treated firms (the matched controls, also plotted in this figure, will be discussed shortly). “0” in the figure is the treatment (loan) year, “-7” refers to 7 years before treatment, and “10” is the 10^{th} year after treatment. The plot shows employment in treated firms is rather stable at 10-10.5 employees for several years prior to loan receipt, but from 3 years before begins to rise, and it continues to rise post-treatment. The pre-treatment growth is consistent with the interpretation that treated firms display an interest in expansion before they receive the loan that may enable further expansion. SBA-recipients therefore may be somewhat atypical of small firms, most of which do not grow (Hurst and Pugsley 2011). This is an issue to address through matching, on which we will report shortly.[24] For the moment, the important point is that the existence of pre-treatment growth among treated firms undermines the AB as measuring a causal effect, because post-treatment growth is difficult to distinguish from mere continuation of a pre-existing trend.

Further progress requires a genuine comparison group, which the bottom panel of Table 6 provides in the form of mean pre- and post-treatment employment for the matched controls constructed according to the procedures described in the previous section. The AB estimator for matched treated firms in this panel, at 9.3, differs only slightly from that for all treated firms in the upper panel, but the panel shows that the matched controls also grow by 6.5. A simple difference-in-differences estimator (DD) that assumes the control firm growth is the counterfactual, without any other adjustments, is derived from the reported mean employment post- and pre-treatment. The DD implies a gain of 2.8 jobs in the treated relative to control firms, substantially smaller than that implied by the AB, but still a non-trivial estimated effect. Figure 1 shows the annual evolution of mean control firm employment in event time. The close alignment with the treated firms in the pre-treatment period provides evidence of the success of our matching methods. In the treatment year, the two plots begin to diverge and a gap of about 3 jobs opens up within two years post-treatment.

These versions of the AB and DD estimators are based on unadjusted differences of means, but using regressions allows year effects that account for aggregate shocks on employment, event-timing effects that normalize event time around the loan year, and firm fixed effects that account for time-invariant heterogeneity across firms. Regressions provide a framework for controlling for these factors in an examination of growth, not just levels, of employment both pre- and post-treatment. A further, major advantage of regressions is the possibility to measure loan receipt not only as a binary treatment but also as the size of the loan – a continuous variable that reflects the financial channel through which the SBA programs may have a genuine causal effect. As a consequence, regressions also allow us to use additional specification checks: the placebo and pre-program tests described in the previous section.

[24] Table 5 clearly quantifies these differences: in the pre-treatment period, future SBA recipients grow twice as fast each year as do all non-recipients in all years; the matched sample is selected to eliminate this (and other differences) between treated and control firms.

Before presenting results from the combined matching-regression estimator, we consider two other alternative estimators that use regression but not matching. One is an AB estimator using only treated firms without any control group, and a second, at the other extreme, includes all non-treated firms as controls. Results for both are shown in the two panels of Table 7. Specifications include Column (1) that uses log(employment) as the dependent variable and Columns (2)-(4) with unlogged employment; Columns (3) and (4) include not only the Postloan Dummy, but also Loan Amount, in Column (4) in quadratic form. The results are rather similar in the two panels, both implying employment rises by about a third (in the log specification) or by 3.6 jobs (in the unlogged specification) after loan receipt. When we include Loan Amount, the estimates imply a gain of 5.5 jobs per million dollars of loans in Column (3). Including the quadratic term in Column 4 shows the relationship is only very slightly concave. In both of these specifications, and for both the AB estimator in Panel A and the full LBD in Panel B, the coefficient on the Postloan Dummy remains substantial and highly statistically significant, indicating that these estimators fail the placebo test, inasmuch as they imply that the effect of a zero (infinitesimal) loan would be positive.

We can also diagnose potential selection bias in these specifications by estimating the dynamics of the loan effects in event time, and the results are shown in Figures 2 and 3 for these two specifications. Both figures show substantial pre-loan effects of loan receipt with a strong positive trend. The upward rise continues in the postloan period, but this analysis implies that the results without matching are likely plagued by too much selection bias to allow reliable inferences about the impact of these programs.

Thus, we turn to the combined matching with regression estimator, where we use similar regressions to those just described, but the sample follows the matching procedures described in the previous section. The results in Table 8 are organized similarly to those in Table 7, with log(employment) and unlogged employment as dependent variables. The estimates imply somewhat smaller effects than the previous results: with log(employment) as dependent variable we estimate an average effect of 24 log points increase in employment associated with receiving the loan. For unlogged employment, the estimate implies an average treatment effect on the treated firms of 3.1 additional jobs. Columns (3) and (4) again permit the effect to vary with Loan Amount, with coefficients rather similar to those we saw in Table 7: slightly smaller in Column (3) and slightly larger in Column (4). Very different, however, are the estimated coefficients on the Postloan Dummy in these two specifications, which are much smaller and statistically insignificantly different from zero, the magnitude declining in the quadratic specification to the tiny value of 0.20. This implies that the employment gain from loan receipt is associated only with the amount of the loan, not with selection into the treatment group, evidence that our matching procedures may be working to reduce selection bias in the estimates.[25] The combined matching-regression estimator passes this "placebo test."

Figure 4 contains the results from using the combined matching-regression procedure to estimate the event-time dynamics of the loan effects. The pre-loan period allows us to assess the "pre-program test," and by contrast with the other estimators, here we find only tiny, statistically insignificant differences between the treated and control firms during this period. In the post-loan period, we observe big jumps in employment in both the loan year and year following: about 20 percent total. The jump in the loan year may be explained by anticipatory hiring or receipt of the loan early in the calendar year. After two years, the growth diminishes, but the estimates imply the employment effect never falls over the 10-year period we observe. An interpretation of these results is that the SBA loan, rather than crowding out alternative sources of finance, may "crowd in" by making it possible for firms to develop a credit history and gain regular access to formal financial markets.

The combined matching-regression method is our preferred approach because, unlike the estimators based on only treated firms, or using only matching or only regression methods, the combined method satisfies the balancing, placebo, and pre-program tests. The tests results support, but of course cannot definitively prove, the basic identifying assumption that the combined method has eliminated unobserved differences in demand for loans by firms that are correlated with differences in their growth potential. If this assumption is invalid, then it might be the case that the effects we estimate reflect selection bias in which types of firms are loan recipients. Note that the inclusion of firm fixed effects in the regressions implies that such a residual selection bias must be time-varying. Furthermore, the dynamics results in Figure 4 imply that there would have to be a demand shock exactly in the loan year and following year. Any other form of selection bias, such as a more rapid trend growth rate prior to loan receipt, would have been reflected as such in Figure 4.

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[25] Estimates from a quartic specification in Loan Amount produced a small negative coefficient on the Postloan Dummy, which taken literally would imply negative selection into the SBA loan programs.

5.2 Robustness and Displacement

As noted in the previous sub-section, our inclusion of firm fixed effects and our analysis of dynamics showing no differences in the level or trend of employment between treated and untreated firms in the pre-treatment period imply that any selection bias must be time-varying, reflecting for example a positive demand shock received by treated firms – but not by controls – precisely during the treatment year.[26] As discussed in Section 4.3, the identifying assumption for our estimates to be interpreted as causal is that treatment is independent of a contemporaneous demand shock. One way of assessing this possibility is to focus on situations where all firms face a strong increase in demand and thus have good growth possibilities.

For this purpose, we focus on unusually rapid growth environments, which we define two alternative ways. The first is based on high growth in the same county and year that a treated firm receives a loan. We define high growth as county-years in the top decile of county-level employment growth rates over the whole sample; the average employment growth in these cases is 22.2 percent, and the minimum (i.e., the 90th percentile) is 10.9 percent – compared with a county-year average of 0.18 percent. The second approach narrows the definition still farther by focusing on industry-county-years in the top decile of all industry-county-years in the data; in this case, the minimum industry-county-year employment growth of the decile is 36.4 percent and the mean within the top decile is 67.5 percent. We restrict both the treated firms and controls to come from these unusually high growth situations. If loan receipt is just reflecting a greater opportunity for growth among treated firms, then the estimate with this high-growth-context sample should be zero, or at least attenuated compared to the full matched sample estimates in Table 8. The results shown in Table 9, however, are rather similar to those for the full data: slightly smaller for the coefficients on Postloan in the county-year definition of high growth in Panel A, and slightly larger for the industry-county-year definition and for the Loan Amount specifications. The dynamics of the Postloan coefficient in loan-event time, shown in Figures 5 and 6, are also qualitatively similar, with only tiny differences in treated and control

[26] It is also noteworthy that we found that SBA loan recipients tend to grow for several years prior to the loan, and we have therefore selected a control group exhibiting similar growth; thus, the distinction between SBA recipients and non-recipients is not that some "want to grow" and others do not: in our estimates, both groups are growing prior to the loan at the same rate, but beginning immediately afterwards the recipients grow distinctly faster. The fact that the matched controls are also growing makes it more likely that they receive conventional loans, without SBA support, than if we were comparing to the whole population of non-treated firms.

firms prior to loan receipt and large sustained jumps immediately afterward. Because these samples are smaller, the 99 percent confidence intervals are wider, of course. Overall, there is no evidence from these analyses that differences in demand conditions drive our results.

Another approach to assessing the unconfoundedness assumption is to compare results with different control groups. If the selection bias into treatment differs across these groups, then the estimated effects should differ depending on which is used (Imbens and Wooldridge 2009). In our context, we may define alternative control groups geographically, by distance from the treated firm. Nearby controls (in the same narrow industry and size and age groups) are more likely to be subject to the same or similar demand shocks as the treated firms, while those far-away are less so. To implement this procedure, we divide the controls within the kernel bandwidth according to the distance from treated firm and estimate separately for nearby and faraway controls. Distance is defined two different ways: in the first, controls are included if they are up to 10 miles away to constitute the "nearby" group, which is compared to a "faraway" group more than 50 miles distant from any SBA loan recipient; in the second procedure, we simply take the nearest four controls for the "nearby" group and the furthest four as "faraway."

We also have an additional motivation for this geographic analysis. Our methods are designed to estimate the "average treatment effect on the treated" (ATT), the direct effect on firms receiving loans, and they assume the program has no effect on non-treated firms used as controls in the analysis.[27] Because only a tiny fraction of firms in the U.S. receive SBA-backed loans, this assumption is plausible. But it is nevertheless possible that even if treated firms grow as a result of loan receipt that the program creates general equilibrium effects, or spillovers on other firms. Spillovers may be positive if the loan enables innovation that is somehow copied or imitated by other firms, or if suppliers or customers benefit together with the loan recipient. They could also be negative if they are displacement effects that reduce employment at non-treated firms that compete with the treated in product and labor markets. In either case, the total job creation – including these indirect effects as well as the direct effect – would differ from the direct effect we have estimated.

Estimating such general equilibrium effects is intrinsically difficult, and it is largely ignored in the program evaluation literature (Heckman et al., 1999). Positive spillovers would

[27] The program evaluation literature sometimes refers to this as the "stable unit treatment value assumption" (SUTVA) (Imbens and Wooldridge 2009).

imply that our estimates of the direct effect are lower than the total estimate, and therefore we focus attention here on the possibility of negative displacement effects. If these result from product market competition, where loan receipt gives the beneficiary an advantage over its competitors, then we should look for negative effects within industries. If the degree of competition is related to geographic distance, then we should look for larger negative effects nearer to treated firms than farther away. In turn, this implies that the estimated ATT should be larger when the controls are drawn from close by than when they are far away.

Results are shown in Table 10 and dynamics of the estimated effects in Figure 7. In all cases, we find slightly larger coefficients using nearby controls than we do with faraway controls. These differences are inconsistent with correlated demand shocks among firms in closer geographic proximity, but the pattern could result from displacement, however, as nearby controls grow more slowly as a result of the loan. But the differences are slight, implying only a small role for displacement – on the order of 0.5 percentage points of the employment effect or 0.1 jobs from the unlogged specification, on average per loan.

A more direct way to compare the nearby and faraway controls, and thus to measure the displacement effect, is through a "pseudo-outcome test" (Imbens and Wooldridge 2009). The procedure uses the nearby control group as the treated group, estimating the difference between them and the faraway controls, defined as above. The results, reported in Table 11, show that the nearby control group has about two percent fewer jobs (0.26 fewer jobs in the unlogged specification of Panel B) relative to the faraway control group, though only the coefficient in the log specification is statistically significant. This again suggests a small displacement effect.

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. Though great effort has been made to link establishments across time in the LBD, we cannot always distinguish bankruptcy and other genuine shutdowns from buy-outs or reorganizations that lead to a change in the identifying 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. For the final matched sample, exit rates (by 2010, the last year in the LBD) are 45.94 percent for treated firms and 53.62 percent for controls (kernel weighted, as in the regressions, for greatest comparability). A crude comparison therefore suggests a higher survival rate associated with treatment, but this does not take into account the timing of exit or the size of firms exiting. Assuming exit represents job loss, then if exit is more common among loan recipients, our earlier results are overstated in ignoring the employment decline associated with exit. On the other hand, if SBA-backed loans raise survival, our earlier results could be understated. To distinguish these alternatives, we impute a zero value for employment two years following exit and re-estimate the specifications in Table 8. The results, shown in Table 12, are slightly larger but qualitatively similar to those without the imputations, so we conclude that different patterns of exit are unlikely to play an important role in our results. Further analysis of exit effects, including the characteristics of survivors and exitors, could be of considerable interest, but we leave it for future research.

6. Conclusion

Our estimates of the effects of the SBA loan programs on employment in this paper are based on an unusual linking of administrative and census data and an application of econometric methods originally designed for evaluating job training interventions. We exploit the large size and completeness of the data to combine matching and regression methods. The first step is to match exactly on firm age, industry, year, and pre-loan size, and the second is to carry out kernel-based matching on propensity scores estimated as a function of four years of employment history and other variables. Having constructed the matched sample, we estimate program effects using firm fixed effect regressions.

The estimation results show positive average effects on loan recipient employment of about 25 percent or 3 jobs at the mean. Including loan amount, the results imply an increase of about 5.4 jobs for each million dollars of loans. Examining situations where most small firms should have excellent growth potential, defined as high growth county-years (average growth of 21.2 percent) or industry-county-years (average growth of 67.5 percent), we find similar effects, implying that the estimates are not driven by differential demand conditions across firms. Results are also similar regardless of distance of control from recipient firms, suggesting only a small role for displacement effects. In all these cases, the results from our preferred specification combining matching with regression pass a "pre-program" specification test, where controls and treated firms look similar in the pre-loan period, as well as a "placebo" test, where we find no effect of program participation *per se* but an effect increasing with loan size; both of these are consistent with an important role for finance rather than selection in driving the different evolution of employment in treated compared to control firms in the post-treatment period. Because we cannot identify other sources of finance, in particular non-SBA finance received by the control firms, our estimates may be interpreted as program effects, and possibly as lower bounds on the effects of the availability of finance more generally. Other specifications, such as those using only the treated firms or using only matching or only regression methods imply somewhat higher effects, but they fail the pre-program and placebo tests.

Clearly, these estimates are averages that take no account of a number of dimensions of heterogeneity. For example, the literature on firm growth, age, and size suggests the interesting question of whether small versus large versus young firms are more responsive to an easing of credit constraints. Together with industry and regional characteristics, results for these variables may provide useful information for targeting firms in future loan programs. Another interesting set of questions concerns the heterogeneity with respect to program characteristics such as interest rate, term, and SBA program design. A final example concerns the economic environment, including the state of the business cycle, with its relevance for the role of loans in macroeconomic stabilization. This paper has ignored all of these dimensions of heterogeneity.

Nevertheless, our estimates of average effects allow rough calculations of the overall job creation attributable to SBA loans. For this purpose, let us assume the coefficients in Table 7, which are estimated over the period of 1976-2010, can be applied to the "$30 billion in lending support to 60,000 small businesses" reported by the SBA for fiscal year 2011. One estimate uses the specification in column (1) to multiply the Postloan dummy coefficient of 3.083 increase in employment per loan times the 60,000 small businesses receiving loans to obtain an estimate of 184,980. The second uses column (2) and multiplies 60,000 by 0.708 plus 30 billion dollars times 5.385 jobs per million dollars for a total of 204,030. The two estimates are rather close, and although they are significantly less than the claimed half-million or more "jobs created and retained" by the SBA each year, they are not in an entirely different order of magnitude. On the other hand, these estimates are for 2011, which saw an unusually large volume of both number and value of SBA loans; for most other years, the estimated job creation would be lower.

It is important to remember a number of caveats to our estimates. As we have discussed, the inability to distinguish disbursed from non-disbursed loans and to fully link the SBA data with the LBD imply measurement error in the treatment variable. We have excluded start-ups, non-employers, and multi-establishment firms (because of lack of history on which to match and of identifying the establishment benefitting from the loan). Under plausible assumptions, which we have described, these imply our estimates may be downward biased. On the other hand, the estimates could also be further refined by permitting heterogeneous effects for different groups, regions, and time periods, which could lead to lower or higher estimates of overall job creation. Pursuing these extensions is a high priority for future research.

An additional caveat concerns external validity. In general, estimates of the average-treatment-effect-on-the-treated can be extrapolated to non-treated sub-populations only under strong assumptions, but our analysis demonstrates a particular reason for caution. We find that SBA loan recipients tend to grow for some time before they receive the loan, a pre-program trend that our matching procedure must address in identifying similar controls. Our analysis of bias in the covariates suggest the matching procedure has been successful in identifying controls with similar growth (and other characteristics), and thus that the analysis compares growing treated with growing control firms, not with the many non-treated small firms that never grow. Therefore, our results should not be interpreted as implying similar responsiveness to policy among all small firms, including the many who do not grow and do not want to grow (Hurst and Pugsley 2011), but they may be relevant for the sub-group of small firms that show growth for a significant period prior to receiving an SBA loan.

It bears emphasis that our study is not a cost-benefit analysis. It does not estimate the full benefits of the program, which would include producer surplus of borrowers, lenders, and workers; possible consumer surplus (if loans help firms to produce at lower cost and result in lower prices); possible positive spillovers into other sectors; and any external effects of increased employment for society or the government budget. Our estimates do permit us, however, to calculate a rough range on the cost per job created by the SBA programs. Assuming an average guarantee rate of 75 percent, an overall default rate of 18 percent taking place at an average balance of 80 percent with an average recovery rate of 30 percent, and using our estimate of 5.4 jobs per million dollars of loans implies a cost per job of about $14,000.[28] Applying a 99 percent confidence interval (based on the standard error of 0.8) around the point estimate of 5.4 yields a range of $9,200 to $18,800 cost per job created. This range is far to the left of the usual estimated cost per job of government programs. Neumark (2013), for instance, reports the cost from fiscal stimulus under the American Recovery and Reinvestment Act (based on estimates from Congressional Budget Office 2010) to lie in the range of $158,000 to $407,000, and the cost per job from a hiring credit under the New Jobs Tax Credit (based on Bartik and Erickcek 2010) at $37,500-$75,000. A full assessment of costs should account for possible distortions, such as those highlighted by Hurst and Pugsley (2011), and opportunity costs of the funds, but these figures suggest that compared with the alternatives SBA loan programs may be a relatively low-cost policy to generate employment.

[28] The formula is ($1mln/5.4)*(guarantee rate)*(default rate)* loan balance*(1-recovery rate). The guarantee rate is from our data, the default and balance rates are from Glennon and Nigro (2005), and the recovery rate is based on de Andrade and Lucas (2009, Figure 4). The calculation does not consider lender fees to SBA (about $10mln in 2011) or SBA administrative costs (127mln for both 7(a) and 504 programs, or less than $1000 per job in 2011). The formula implies a total cost to the SBA of $2.6-2.8mln, not very different from the "fair market value" calculation of CBO (2012), although significantly higher than the official procedures under the Federal Credit Reform Act, which would imply still lower cost estimates per job created through the SBA programs.

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Table 1
Path from Full SBA Loan Dataset to Treated Firms in Final Matched Regression Sample

	Number
Total SBA Loans in 1977-2009	1,378,501
Except loans not matched to any business register	763,583
Except loans matched to non-employer business register	604,023
Except firms with first SBA loan before 1977 or a SBA disaster loan before first SBA 7(a)/504 loan	528,753
Except SBA 7A/504 loans after the first loan	493,116
Except start-ups	337,148
Except multi-establishment firms prior to loan	318,207
Except firms with missing employment in year before loan receipt	269,623
Except firms without matched controls (main sample for analysis)	216,023

Table 2
Sample Comparisons among SBA Loan Recipients

	Not in Final Sample			Final Sample
	Not Linked, Non-Start-Up	Missing Employment	No Control Firms Found	
Total Number	398,088	48,584	53,600	216,023
Mean Employment	11.87	8.86	16.73	14.80
Mean Loan Amount	339,469	285,527	415,857	400,164
Median Loan Amount	138,789	138,978	250,256	196,329
Percent Sole Proprietorships	39.09	20.02	13.47	18.74
Percent Partnerships	5.35	4.10	3.33	3.49
Percent Minority	28.00	22.96	13.51	21.29
Percent Female	27.59	27.68	22.25	26.17
Percent Veteran	11.23	10.83	13.79	11.47
Percent By Sector:				
Construction	6.39	9.40	5.73	9.19
Manufacturing	5.50	6.22	14.36	7.16
Wholesale Trade	4.70	4.93	6.38	5.39
Retail Trade	12.90	11.08	10.33	11.66
Finance/Insurance/ Real Estate	2.69	2.51	1.37	2.36
Services	33.89	29.98	17.52	31.18
Other/Unknown	33.93	35.88	44.31	33.06

Notes: The variables come from the SBA loan recipient database and generally refer to the year before the loan. "Not linked" means we were unable to match these firms to any business register. Loan amounts are in constant 2010 US$.

Table 3
Descriptive Statistics

	All Non-Treated Firms	All Treated Firms
Employment Mean	12.54	12.46
Employment Standard Deviation	129.70	26.91
Employment Median	4	6
Age Mean	8.20	6.74
Age Standard Deviation	7.61	6.59
Age Median	6	4
Percent by Sector:		
Construction	11.15	10.80
Manufacturing	5.36	12.62
Wholesale Trade	6.21	10.83
Retail Trade	8.22	12.58
Finance/Insurance/Real Estate	2.55	1.87
Services	55.47	44.93
Other	11.05	6.37

Notes: This excludes multi-establishment firms and establishments that were ever in a multi-establishment firm in the past. Non-treated firms are included in each year they appear in the LBD, while treated firms are included only in the treatment year. For treated firms, employment is measured in the year prior to treatment. The variables come from the LBD.

Table 4
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	216,023	2,051,524	9.5	1,353,043	6.3
Controls	3,508,245	30,404,010	8.7	15,756,467	4.5

Notes: The year of loan receipt is included with pre-treatment years in this table. Controls are included within a 10% bandwidth around the treated firm's propensity score.

Table 5
Bias Before and After Propensity Score Matching

	Variable Mean					
	All Non-Treated	All Treated	Final Treated Sample	Final Control Sample	Final % Bias	% Bias Reduction
Log Emp t-1	1.307	1.702	1.847	1.879	-2.473	92.050
Log Emp t-1 sq.	3.455	4.374	4.696	4.791	-1.781	89.739
Log Emp t-1 – t-2	0.107	0.229	0.228	0.220	1.198	93.708
Log Emp t-2 – t-3	0.095	0.186	0.198	0.192	0.909	94.176
Log Emp t-3 – t-4	0.082	0.151	0.164	0.162	0.449	96.474
Log Wage	2.151	2.625	2.963	2.985	-1.641	95.297
Age	7.496	7.160	8.030	8.239	-2.928	37.624

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 6
Mean Employment in the Unmatched and Matched Samples

<table>
<tr><th></th><th>Pre-Treatment</th><th>Post-Treatment</th></tr>
<tr><td>Without Matching:</td><td></td><td></td></tr>
<tr><td>Treated</td><td>11.91</td><td>20.90</td></tr>
<tr><td>Non-Treated</td><td colspan="2">15.69</td></tr>
<tr><td>With Matching:</td><td></td><td></td></tr>
<tr><td>Treated</td><td>10.84</td><td>20.17</td></tr>
<tr><td>Non-Treated (Controls)</td><td>12.44</td><td>18.96</td></tr>
</table>

Notes: The year of loan receipt is included as a pre-treatment year. The post-treatment years for matched non-treated firms are the post-treatment years of the treated firms to which the non-treated firms are matched; the treatment year is undefined for untreated firms without matching, so their mean (15.69) is shown over the entire observation period.

Table 7
Estimates from Regressions without Matching

	(1)	(2)	(3)	(4)
Dependent variable	Log Employment	Employment	Employment	Employment
		A. Treated Firms Only		
Independent variables:				
Postloan Dummy	0.328	3.615	1.325	0.965
	(0.002)	(0.085)	(0.260)	(0.107)
Loan Amount			5.463	6.394
			(0.600)	(0.195)
Loan Amount^2				-0.061
				(0.004)
		B. Full LBD used as Controls		
Postloan Dummy	0.340	3.648	1.197	0.814
	(0.088)	(0.088)	(0.271)	(0.096)
Loan Amount			5.526	6.469
			(0.604)	(0.197)
Loan Amount^2				-0.062
				(0.004)

Notes: Each column of each panel presents results from a separate regression. The regressions also include year dummies and firm fixed effects. Standard errors, cluster-adjusted by firm, are in parentheses. Loan amounts are in 2010 $millions.

Table 8
Estimates from Combined Matching and Regression using the Final Sample

	(1)	(2)	(3)	(4)
Dependent variable	Log Employment	Employment	Employment	Employment
Independent variables:				
Postloan Dummy	0.242	3.083	0.708	0.203
	(0.002)	(0.101)	(0.357)	(0.113)
Loan Amount			5.385	6.613
			(0.803)	(0.232)
Loan Amount^2				-0.063
				(0.004)

Notes: Each column presents results from a separate regression. Other regressors include event time dummies common to treated firms and their matched controls, year dummies, and firm fixed effects (for control firms, there are separate fixed effects for each treated firm to which they are matched). Loan amount measured in millions of 2010 dollars. Matched sample includes loan recipients and all control firms meeting the exact matching criteria and with propensity scores lying within a 10% bandwidth. Standard errors, cluster-adjusted by firm, are in parentheses. Total observations = 49,565,044; treated firm observations = 3,404,567.

Table 9
Estimates from Combined Matching and Regression for Samples in Rapidly Growing Counties and Industries

	(1)	(2)	(3)	(4)
Dependent variable	Log Employment	Employment	Employment	Employment
Independent variables:				
	A. Top Decile of County-Year Growth			
Postloan Dummy	0.261	2.817	-0.381	-0.811
	(0.012)	(0.376)	(0.413)	(0.449)
Loan Amount			7.226	8.601
			(1.011)	(1.316)
Loan Amount^2				-0.427
				(0.169)
	B. Top Decile of Industry-County-Year Growth			
Postloan Dummy	0.310	4.000	1.446	-0.275
	(0.019)	(0.591)	(0.662)	(0.674)
Loan Amount			6.228	13.440
			(1.202)	(2.333)
Loan Amount^2				-3.356
				(1.131)

Notes: Results based on the specifications in Table 8, including matching, but with samples restricted to treated firms and controls in high-growth environments during the loan year. In Panel A, a high-growth environment is defined as county-years in the top decile of employment growth from the year before loan receipt to the year of receipt; the mean growth rate is 21.2 percent for these county-years; there are 6,206,928 observations, of which 129,845 are from treated firms, in these regressions. In Panel B, a high-growth environment is defined as industry-county-years in the top decile of employment growth from the year before loan receipt to the year of receipt; the mean growth rate is 67.5 percent for these industry-county-years; there are 2,207,589 observations, of which 59,442 are from treated firms, in these regressions (industry is defined at the 2-digit level). As in Table 8, all regressions also include event time dummies common to both treated firms and their matched controls, year dummies, and firm fixed effects (for control firms, there are separate fixed effects for each treated firm to which they are matched), but geography is not used for exact matching among treated and control firms in these samples. Loan amounts are in millions of 2010 dollars. Standard errors, cluster-adjusted by firm, are in parentheses.

Table 10

Regressions with Control Groups Defined by Geographic Distance from Matched Treated Firms

Control Groups	<10 miles	>50 miles	Nearest 4	Furthest 4
		Log Employment		
1. Postloan Dummy	0.209	0.204	0.203	0.194
	(0.007)	(0.007)	(0.004)	(0.004)
		Unlogged Employment		
2. Postloan Dummy	1.663	1.501	2.277	2.150
	(0.128)	(0.130)	(0.134)	(0.146)
3. Postloan Dummy	0.529	0.364	0.444	0.320
	(0.134)	(0.135)	(0.166)	(0.175)
Loan Amount	3.524	3.470	4.540	4.532
	(0.354)	(0.352)	(0.221)	(0.221)
4. Postloan Dummy	0.187	0.022	-0.001	-0.123
	(0.131)	(0.132)	(0.182)	(0.190)
Loan Amount	5.119	5.003	6.143	6.131
	(0.484)	(0.475)	(0.320)	(0.320)
Loan Amount^2	-0.543	-0.521	-0.405	-0.404
	(0.124)	(0.121)	(0.051)	(0.052)
Total Obs.	1,419,964	2,617,849	4,961,474	4,955,124
Treated Firm Obs.	400,854	400,854	1,079,342	1,079,342

Notes: Results based on the specifications in Table 8 (numbered 1-4, as in that table, but for convenience here arrayed with axes rotated), including matching, but with samples of controls restricted based on their distance from the matched treated firm. The first control group contains controls located no more than 10 miles away. The second control group contains those more than 50 miles away from any SBA loan recipient in the treated firm's year of loan receipt. The third and fourth control groups are the nearest and furthest four firms from the treated firms to which they are matched. Only treated firms that have controls in both control groups are included in the regressions. The regressions also include event time dummies common to both treated firms and their matched controls, year dummies, and firm fixed effects (for control firms, there are separate fixed effects for each treated firm to which they are matched). Standard errors, cluster-adjusted by firm, are in parentheses. Loan amounts are in millions of 2010 $.

Table 11

Evidence on Displacement: Pseudo-Outcome Tests with Nearby Controls Acting as Treated Relative to Far-Away Controls

	Log Employment	Unlogged Employment
	A. Closest 4 Relative to Furthest 4	
Postloan Dummy	-0.009	-0.079
	(0.003)	(0.096)
	B. < 10 Miles Relative to > 50 Miles Away	
Postloan Dummy	-0.019	-0.264
	(0.006)	(0.141)

Notes: The regressions also include event time dummies common to both treated firms and their matched controls, year dummies, and firm fixed effects (for control firms, there are separate fixed effects for each treated firm to which they are matched). Standard errors, cluster-adjusted by firm, are in parentheses. In Panel A, the pseudo-treatment group is the nearest four non-treated firms to the matched treated firm. The control group is the furthest four firms from the matched treated firm. Only firms whose matched treated firm appears in the regressions with the nearest four non-treated firms and furthest four treated firms are included here; there are 7,757,914 observations of which 3,882,132 are pseudo-treated. In Panel B, the pseudo-treatment group is non-treated firms located no more than 10 miles away from the matched treated firm. The control group is firms more than 50 miles away from any SBA loan recipient in the treated firm's year of loan receipt. Only firms whose matched treated firm appears in the regressions with non-treated firms no more than 10 miles away and those more than 50 miles away are included here.

Table 12

Unlogged Employment Regressions with Matched Sample, Imputing Zero Employment in First Two Years After Exit

	(1)	(2)	(3)
Postloan Dummy	3.305	1.716	1.220
	(0.082)	(0.349)	(0.092)
Postloan Dummy*Loan Amount		3.701	4.920
		(0.811)	(0.204)
Postloan Dummy*Loan Amount^2			-0.045
			(0.002)
Total Obs.		55,883,482	
Treated Firm Obs.		3,704,108	

Notes: The regressions also include event time dummies common to both treated firms and their matched controls, year dummies, and firm fixed effects (for control firms, there are separate fixed effects for each treated firm to which they are matched). Standard errors are cluster-adjusted by firm. The loan amount is in $millions. Zero employment is imputed in the first two years after exit. The percent of firms exiting prior to 2010 is 45.9 percent for treated firms and 53.6 percent for their matched controls.

Table A1
Passes Used for Matching SBA Loan Data to Census Bureau Employer and Non-Employer Business Registers

	Pass 1	Pass 2	Pass 3	Pass 4	Pass 5
Exact match on 5-digit zip code	X		X	X	
Exact match on standardized street address	X				
Exact match on standardized business name	X	X			
Exact match on 3-digit zip code		X			
Standardized street soundex (phonetic algorithm)		X			
Street address allowing for fuzziness (70% sensitivity in SAS DQMATCH)			X		
Business name allowing for fuzziness (70% sensitivity in SAS DQMATCH)			X	X	X
Place (city) soundex					X
Street name allowing for fuzziness (70% sensitivity in SAS DQMATCH)					X
Street number allowing for fuzziness (70% sensitivity in SAS DQMATCH)					X

Figure 1. Mean Firm Employment by Year Before/After Loan

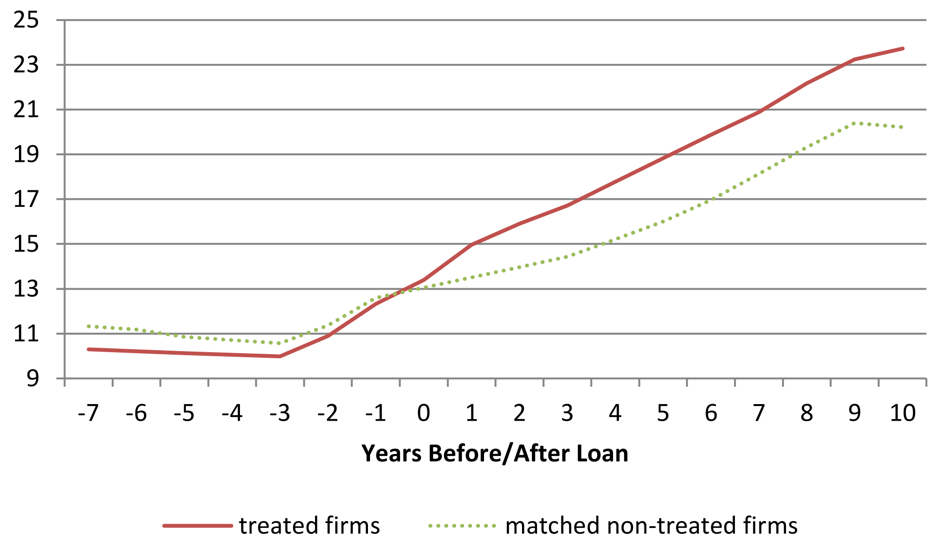

Figure 2. Estimated Dynamics of Loan Effects from Regressions with Treated Firms Only

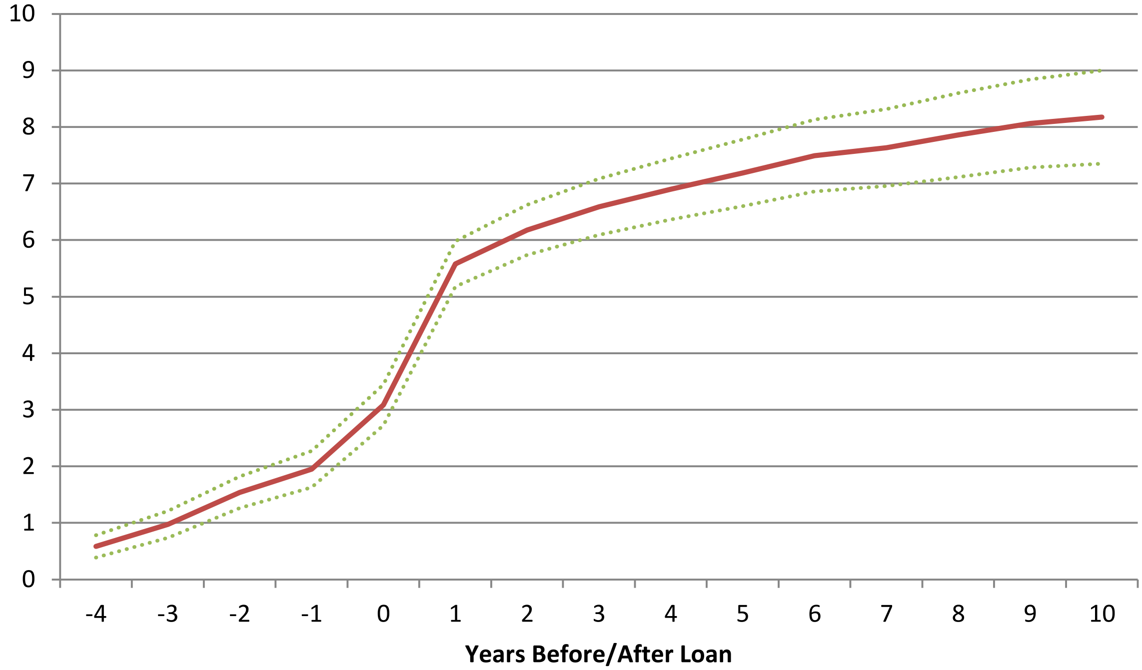

Figure 3. Estimated Dynamics of Loan Effects from Regressions with All LBD (no matching)

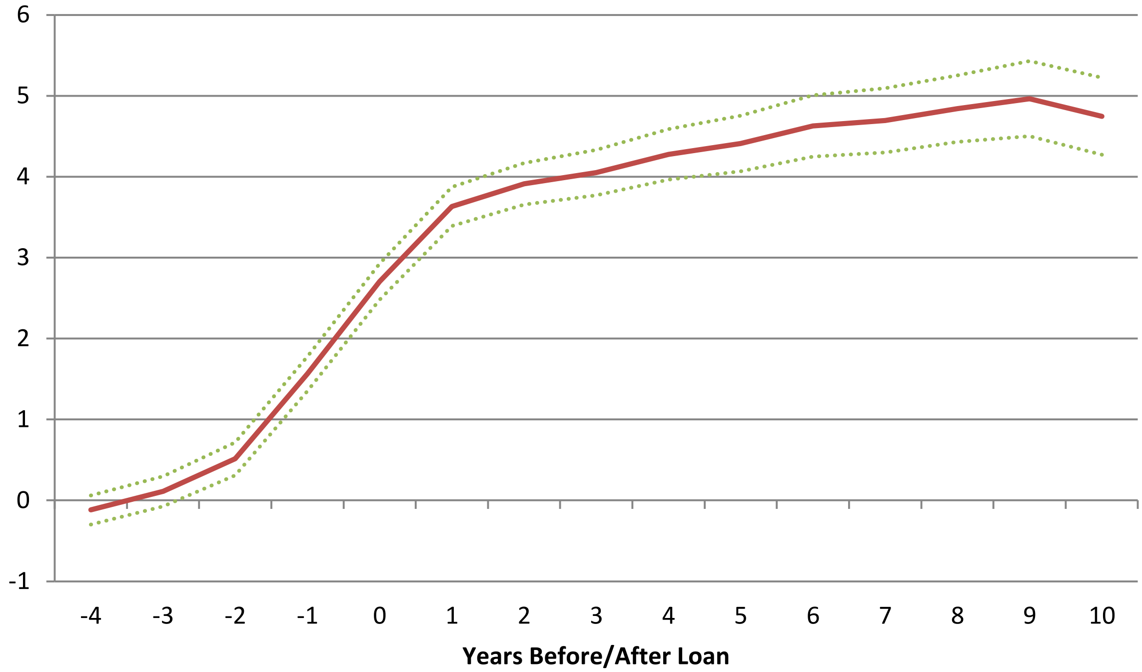

Figure 4. Estimated Dynamics of Loan Effects from Regressions with Final Matched Sample

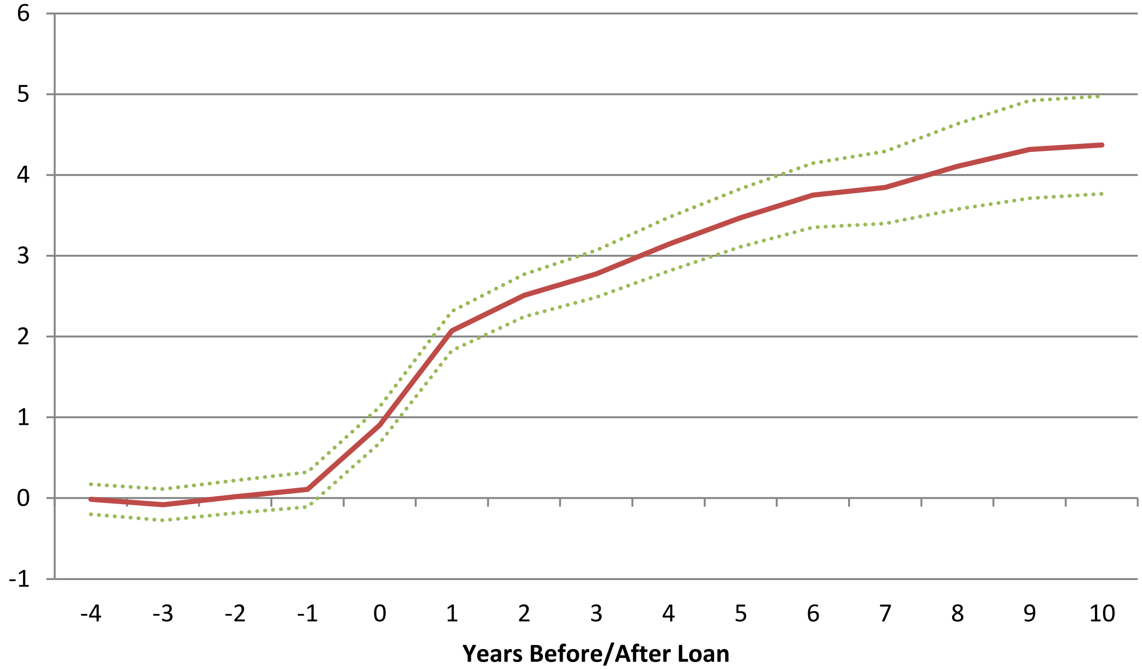

Figure 5. Estimated Dynamics of Loan Effects from Regressions with Matched Sample Restricted to County-Years in the Top Decile of Employment Growth Rates

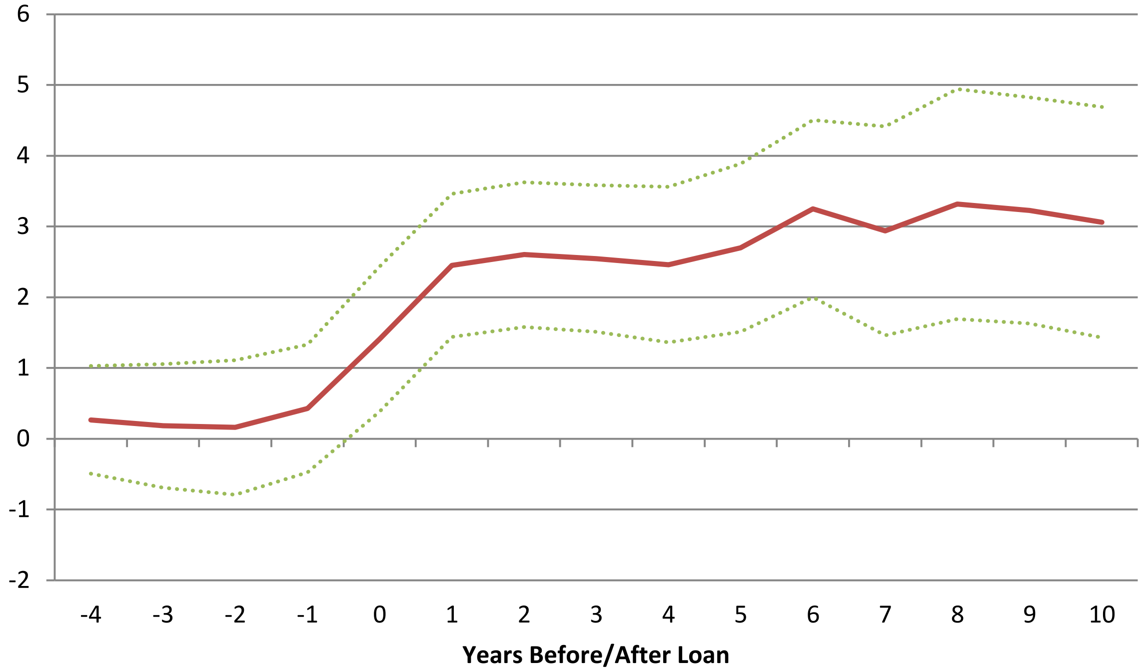

Figure 6. Estimated Dynamics of Loan Effects from Regressions with Matched Sample Restricted to Industry-County-Years in the Top Decile of Employment Growth Rates

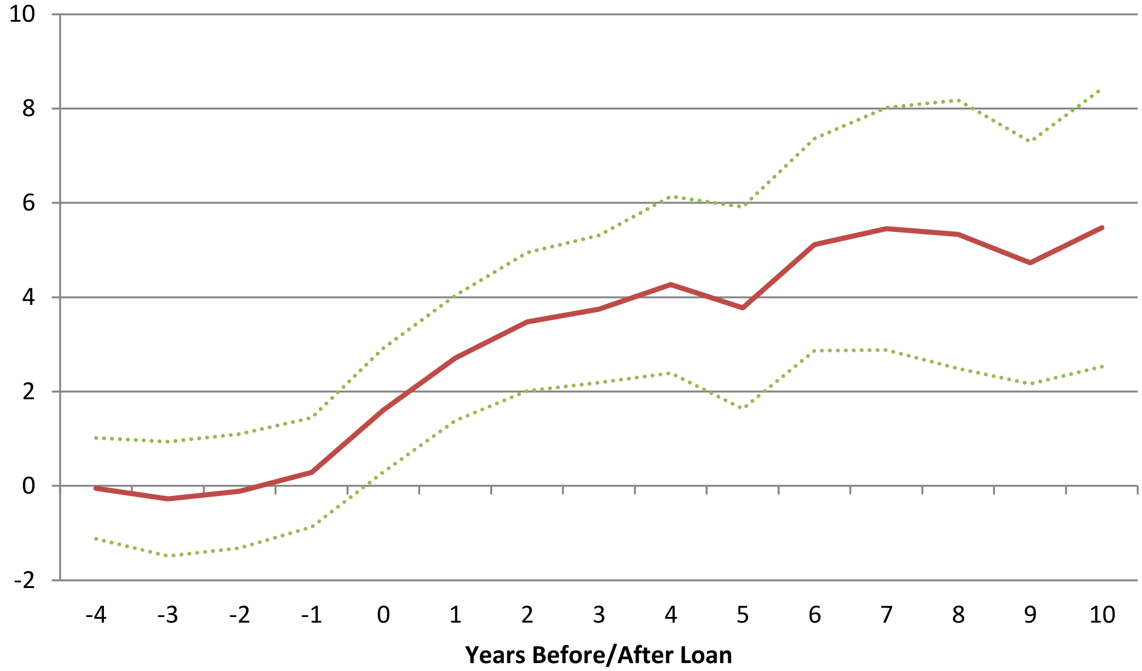

Figure 7. Estimated Dynamics of Loan Effects from Regressions with Matched Sample Restricted by Treated-Control Distance