

Forthcoming in the *Journal of Finance*

**Finance and Growth at the Firm Level:
Evidence from SBA Loans**

J. David Brown and John S. Earle*

July 2016

Abstract

We analyze linked databases on all SBA loans and lenders and on all U.S. employers to estimate the effects of financial access on employment growth. Estimation exploits the long panels and variation in ease of access to SBA lenders. The results imply an increase of 3 to 3.5 jobs for each million dollars of loans, suggesting real effects of credit constraints. Estimated impacts are stronger for younger and larger firms and when local credit conditions are weak, but we find no clear evidence of cyclical variation. We estimate taxpayer costs per job created in the range of \$21,000 to \$25,000.

* Brown (j.david.brown@census.gov): Center for Economic Studies–U.S. Census Bureau. Earle (earle@gmu.edu): School of Policy, Government, and International Affairs–George Mason University and IZA. An earlier version of this paper circulated as “Do SBA Loans Create Jobs?” We thank the National Science Foundation for support (Grant 1262269 to George Mason University) and participants in presentations at the International Monetary Fund, Southern Economic Association Annual Meetings, SGE-ASSA Annual Meetings, Comparative Analysis of Enterprise Data conferences in Nuremberg and Istanbul, George Mason University, Central European University, the U.S. Census Bureau, the Hungarian National Bank, the Small Business Administration, the Kauffman-Brandeis Entrepreneurial Finance and Innovation Conference, Wesleyan University, the Consumer Financial Protection Bureau, the Comptroller of the Currency, as well as Zoltan Acs, Emek Basker, David Hart, Deborah Lucas, Traci Mach, Javier Miranda, E.J. Reedy, Alicia Robb, Rebecca Zarutskie, two anonymous referees and Editor Michael Roberts for helpful discussions and comments. We thank Mee Jung Kim and Yana Morgulis for excellent assistance, and the SBA for providing the list of loans we use in the analysis. We have no conflicts of interest, but any opinions and conclusions expressed herein are ours only and do not necessarily reflect the views of the U.S. Census Bureau. All results have been reviewed to ensure that no confidential information on individual firms is disclosed.

“...[S]ome institutional solution to the problem of providing small business with long-term capital must be sought. In view of the special requirements of small business financial aid and the problems encountered in financing small business, it is doubtful if any of our existing financial institutions are equipped to perform this service.”

Schmidt (*Journal of Finance*, 1951)

The “special requirements and problems in financing small business” have been recognized since at least the early 1950s, when the Small Business Administration (SBA) was founded. SBA loans have since become one of the most significant interventions affecting firm-level access to finance and have recently reached “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 (SBA 2011, p. 1). Yet little is known about the programs’ outcomes.

In this paper, we estimate the firm-level impact of access to SBA loans on employment growth, which the SBA describes as its number one “strategic goal.”¹ Theoretically, the employment effects of SBA loans are ambiguous. On the one hand, easier access to 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 effect may be reduced if there are general equilibrium displacement effects (negative spillovers onto competing firms).² Empirically analyzing 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 micro-data have usually been unavailable.

Perhaps as a result of these problems – and despite the prominence of SBA programs and the high hopes in their power to stimulate business growth – there have been few attempts to measure their impacts using appropriate data and econometric methods. Unlike evaluations of job training programs, for example, research on SBA loan effects has had to rely on small samples, short time series, or aggregated data that do not permit the use of recent developments in

¹ See SBA (2012, p. 7). The first goal is “growing businesses and creating jobs,” while the other two (which would be still more difficult to investigate) 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 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 full range of assistance for small businesses, but we do show that recipients of SBA loans differ from the median in tending to grow prior to loan receipt, which is an important issue for our identification strategy.

econometrics (e.g., Imbens and Wooldridge, 2009). Most studies consist of simple comparisons before and after the interventions, with little use of comparison groups of non-recipients. The most common unit of observation in SBA studies is a geographic area such as the county or metropolitan statistical area, with outcomes measured as overall employment or per-capita income in the local area; Craig, Jackson, and Thomson (2009) review these studies. Many factors affect county-level outcomes, of course, making it difficult to isolate 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, based on summing up borrowers’ estimates on their loan applications of the number of jobs they anticipate creating or retaining as a result of the loan.³

Our research contributes to estimating these effects by using much richer data than were heretofore available and by applying econometric methods developed for estimating causal effects with such data. We link administrative data on every SBA 7(a) and 504 loan to long-panel data on all employers in the U.S. economy, and we use the linked data to implement a longitudinal matching estimator (e.g., Heckman, Ichimura, and Todd, 1997, 1998). The annual panels in our data from 1987 to 2012 permit us to select control firms based on size, age, industry, year, and several years of growth history, to control for fixed effects for time and treatment-control groups, and to measure the evolution of employment before and after the loans were awarded. To address potential correlation of SBA loan availability with local financial development and growth, we also add time-varying controls for the local employment share of banks, local volume of small business lending, and growth in the recipient’s county-industry cell during the estimation period.

To account for time-varying unobservables in the selection process into SBA loan receipt, we exploit institutional features of the SBA program as well as empirical regularities about small business credit markets. The SBA program works through partial (50-85 percent) loan guarantees provided to private lenders, some of which have a special status that reduces their administrative costs (preferred lender (PLP) status in the Preferred Lenders Program).⁴ As we show, these PLPs account for a large share of all SBA loans, particularly the larger loans, implying that participation in SBA loan provision is to some extent a function of corporate bank (firm-level) policy, such that

³ The figure is 583,737 for Fiscal Year 2010 (SBA 2011, p. 4). This number includes jobs supported not only by the 7(a) and 504 loan programs, but also the much smaller microloan and surety bond guarantee (SBG) programs.

⁴ Strictly speaking, PLP is the term used in the 7(a) loan program. The analogous status for 504 loans is called the Preferred Certified Lenders Program (PCLP). Hereafter we use the term PLP to refer to both PLP and PCLP status.

some banks participate a great deal in SBA lending, while others do little or none. Furthermore, the PLP banks are unevenly dispersed across the U.S. and have changed over time. These are key facts for SBA loan access, as they imply variation in the ease and likelihood of obtaining an SBA loan, especially when combined with the continuing importance of the local credit market for small businesses (e.g., Brevoort and Hannan 2006). Indeed, we find that only 3.2 percent of SBA loans are in counties where there was no presence of branches of PLP lenders in the previous year, implying that SBA borrowers also tend to rely on local lenders. At the same time, SBA loans are a tiny share of the total banking business, accounting for only about one percent of all small business loans which themselves are only a fraction of all banking business, suggesting that locational decisions of banks are unlikely to be based on SBA business.

Based on this reasoning, we construct instrumental variables that reflect the local presence of at least one branch of a PLP bank, defined by activity outside the local lending market. In alternative specifications, presence is measured as a dummy variable for any PLP branch, as the number of PLP banks with local branches, or as the local share of local bank branches affiliated with PLP lenders. In some specifications, the presence of a branch of a PLP bank is weighted by the prevalence of SBA lending by the parent bank, measured by the share of the bank's counties in which it provides PLP loans, again defined outside the local area. Our identifying assumptions for the instruments are, first, that bank entry into a new geographic (county) market is not based on the potential growth of SBA loan recipients in that area and year, and, second, that the decision to become a PLP lender is not based on the potential growth of SBA loan recipients in the local area. When the instrument is defined using the share of local branches which are part of PLP banks, rather than the presence or number of PLP banks, the first identifying assumption is weaker; the share is scaled by the total number of branches and thus controls for the size of the local banking industry. The assumption is then only that PLP banks do not choose their locations disproportionately, relative to non-PLP banks, based on the potential growth of SBA loan recipients. As a further robustness check, we define an instrument based only on PLP lenders operating in at least four states in a given year, to address the possibility that the corporate decision on SBA lending is driven by any particular local area.

The estimates are highly robust across all the variations in econometric method, implying an increase of 3 to 3.5 jobs in the first three years after loan receipt for each million dollars of loans. Provided our identification assumptions are valid, this implies that SBA loans did not

simply replace conventional loans, but that recipients were genuinely credit-constrained prior to the loans, and they used them to grow more than they would have otherwise.

We also examine several extensions to these basic results. We estimate the heterogeneity in estimated effects by firm size and age, for the 7(a) and 504 programs, and over the business and credit cycles. We provide some assessment of the degree to which displacement of non-treated firms may be a concern, distinguishing the non-tradable industries and comparing local controls with controls from other counties in which no SBA loans were given in the same industry, and a robustness analysis concerning firm exit. Finally, we calculate estimates of total job creation and the cost per job associated with SBA loans.

The paper builds on previous research on small business, finance, and government programs in several ways. It is important to clarify that much of the recent small business controversy in the U.S. has not concerned government interventions directly, but rather the empirical relationship between size and growth. Such a description of empirical regularities underlies Birch's (1987) claim that small businesses are responsible for most job creation and has been widely cited as the basis for government programs supporting this sector,⁵ although the underlying methods have been questioned by Davis, Haltiwanger, and Schuh (1996).⁶ How to measure the size-growth relationship is a different issue from the impact of government programs on small business growth, which is the question we address in this paper.

The research in this paper is relevant to the broader theoretical and empirical literature on finance and growth (Levine 2005; Clementi and Hopenhayn 2006). As emphasized in Beck's (2009) review, a standard identification problem involves 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. Many studies use aggregate country-level data (e.g., King and Levine 1993 and Demetriades and Hussein 1996). Financial constraints are notoriously difficult to measure at the firm level; see the controversy over the approach of analyzing the relationship between investment and cash flow (Hubbard 1998).⁷

⁵ 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).

⁶ See also Neumark, Wall, and Zhang (2011), Hurst and Pugsley (2011), and Haltiwanger, Jarmin, and Miranda (2013).

⁷ Our focus is not on distinguishing the different factors through which finance may affect growth, but the paper is also relevant to research on the finance-employment connection such as Pagano and Pica (2012), who use international industry-level data on financial development and employment growth.

Most closely related to our paper are some recent studies that have advanced this literature by employing firm-level data and innovative identification strategies. Banerjee and Duflo (2014) use changes in firm size eligibility for directed credit in India to identify the effects on firm growth, which they estimate to be strongly positive. Two studies, Lelarge, Sraer, and Thesmar (2010) and Bach (2014), take advantage of changes in sectoral eligibility for a French loan guarantee program. The former finds that the program is associated with positive growth effects, but also a higher probability of bankruptcy; the latter finds that the program has positive effects on credit growth with no evidence of substitution between subsidized and unsubsidized finance, and no increase in default risk. Our paper also studies financial access through a government program, but our identification strategy exploits geographic variation in access to the program, that is, in the geographic distribution of branches of PLP lenders.⁸

It is important to bear in mind a number of caveats. SBA loans are not randomly allocated, as in a randomized controlled trial, nor are we able to control directly for firm growth potential or for access to other sources of finance. Our instrumental variable approach aims to address this problem but requires the identifying assumptions discussed above and in greater detail below. Concerning external validity, incomplete linking of databases implies our estimates pertain only to the subsample of firms that are linked and have information on the key variables.⁹ More generally, while estimates of the average-treatment-effect-on-the-treated can be extrapolated to non-treated sub-populations only under strong assumptions, our analysis of the SBA case suggests a particular reason for caution. We find that SBA loan recipients tend to grow before they receive the loan, a pre-program trend that our matching procedure addresses to identify control firms with similar growth (and other characteristics) so that the analysis compares firms with similar growth histories. Thus, our results should not be interpreted as implying similar responsiveness to the program among all small firms, including the many that do not grow or want to grow (Hurst and Pugsley 2011), but they are relevant for the sub-group of small firms that shows growth for a significant period prior to receiving an SBA loan. Moreover, SBA lenders typically require

⁸ Whereas the Indian and French studies have implications for the effects of expanding or contracting a loan program along the firm size or sectoral dimensions, our focus on geographic variation in the ease of access is, we believe, a more relevant dimension for the SBA programs. As we document in this paper, local access to PLP lenders varies widely across counties and over time. Two recent papers using firm-level data and exploiting the great recession for identification to analyze financial constraints are Chodorow-Reich (2014) and Greenstone, Mas, and Nguyen (2015). Brown, Earle, and Lup (2005) analyze the impact of USAID-supported loans in Romania.

⁹ As discussed further below, incompleteness in linking also implies measurement error in the treatment variable.

collateral, so our estimates may not be relevant for firms lacking pledgeable assets. The estimates pertain to the treated sub-population, not to the full population.

Section I describes the SBA programs, and Section II describes the data. Section III explains the identification problem and provides basic methods and results. Section IV discusses our instrumental variable approach and corresponding results. Section V contains several extensions, including analyses of heterogeneity and estimates of the total job creation and cost per job. Section VI is a short summary. An Appendix contains details about the matching procedures and differences between the full and matched samples.

I. SBA Loan Programs

We focus on the largest two categories of SBA programs, 7(a) and 504. This section briefly describes their current characteristics.¹⁰ Most 7(a) loans (aside from special subprograms) have a maximum amount of \$5 million, with a maximum SBA guarantee rate of 85 percent for loans up to \$150,000 and 75 percent for higher amounts. Usually loans for working capital and machinery have a maturity of up to 10 years, while loans for real estate can have a term up to 25 years. The SBA sets maximum interest rates that decrease with loan amount and increase with maturity. To qualify, a business must be for-profit; meet SBA size standards;¹¹ show good character, management expertise, and a feasible business plan; and not have sufficient funds available from other sources. The SBA itself makes the final credit decisions for most of these loans.

Some 7(a) programs are more streamlined. In the Preferred Lender Program (PLP), the SBA delegates the 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 loans. In case of default, the PLP lender liquidates all assets before the SBA honors its guarantee. 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

¹⁰ SBA (2015) 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.

¹¹ The size standards vary by industry, with the criterion sometimes employment, sometimes revenue, and sometimes assets. Only a tiny fraction of loan recipients have a pre-loan size in the neighborhood of the industry threshold.

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 amount and 50 percent maximum SBA guaranty. Interest rates can be higher than on other 7(a) loans, but the SBA promises a decision on approval within 36 hours. PLPs also have an advantage here, as they may make eligibility determinations on their own.

The 504 Loan Program offers loan guarantees up to \$5.5 million, depending on the type of business. Typically a lender covers 50 percent of the project costs, a Certified Development Company (CDC) provides up to 40 percent of the financing (100 percent guaranteed by an SBA 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. Eligibility requirements for 504 loans are similar to those listed for 7(a) loans above. For approved lenders, the 504 Program has a streamlined loan processing process analogous to the 7(a) PLP program, called the Premier Certified Lender Program (PCLP), and we treat PCLP lenders the same as PLP lenders in our analysis.

For 7(a) loans, lenders must pay a guarantee fee that increases with maturity and guaranteed amount. For both programs they must sign the "Credit Elsewhere Requirement," which states "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." This requirement, also called the "Credit Elsewhere Test," must be accompanied by a detailed explanation why the loan would be unavailable on conventional terms. Both the fee and the requirement create costs of using SBA loan guarantees. In addition, there are administrative costs to the lender, including the specific bureaucratic formulae for loan application and SBA monitoring of lenders participating in the program. Probably because of scale economies in these costs, SBA loans tend to be concentrated in a relatively small number of lenders.¹² The concentration is especially large among PLP lenders.

To illustrate our points about SBA lenders and PLP lenders relative to all banks, Table I contains summary statistics for our database linking the SBA lenders to firms and establishments

¹² de Andrade and Lucas (2009) also note that relatively few banks originate large numbers of 7(a) loans in 2006.

in the Census Bureau's Longitudinal Business Database (LBD), as described in the next section. Statistics are shown for all banks (here defined as operating in the commercial banking sector or issuing an SBA loan), divided between non-SBA lenders, SBA but non-PLP lenders, and PLP lenders: there are 7,276, 1,493, and 276 of each of these, respectively. Measured as the number of branches, non-SBA lenders tend to be smaller than non-PLP SBA lenders, and both are smaller than PLP lenders; the same order is preserved in the number of counties in which each bank type is represented: 1.9, 4.1, and 42.5, on average. PLP lenders give SBA loans to borrowers in 12.9 counties of their counties with branches each year, on average, compared with only 1.3 for non-PLP SBA lenders. A PLP bank gives an average of 127.1 SBA loans each year, while the mean for non-PLPs is 6.6, and the PLP banks give larger loans. The mean PLP loan is \$460,822 while the mean SBA loan at non-PLP banks is \$286,288.

[Table I about here]

Further information on loan size is contained in Figure 1, with distributions of both all SBA and PLP loans for non-PLP and PLP lenders. Loan size varies substantially, implying that estimation should rely on this variation rather than a simple binary treatment dummy. PLP loans clearly tend to be larger, dominating all size ranges from \$150,000 on up. Access to a PLP lender therefore seems to raise both the probability and the likely size of an SBA loan.

[Figure 1 about here]

These are interesting facts about SBA loans and lenders that have not before been documented, to our knowledge. The measures of numbers of branches and county distribution are not possible with the SBA database alone, but require linking to a database like the LBD. The concentration of SBA lending among banks suggests that there are likely to be sizable fixed costs to be involved with SBA, such that many banks do not participate in the SBA programs at all, while others do much more fully. The various dimensions of this concentration, in conjunction with other facts about small business lending, are useful in developing instrumental variables for our identification strategy in Section IV below.

II. Data

We use a confidential database on all 7(a) and 504 loans guaranteed by the SBA from the fourth quarter of 1990 through the third quarter of 2009 to identify loan recipients, amounts, and time of receipt. We set the loan year on a fiscal year basis (October through September), using the

date the SBA approved the loan, so that the loan year is roughly centered on the Census Bureau's Longitudinal Business Database employment measure, which is the number of employees in the pay period including March 12. Our regression samples include loans starting in fiscal year 1992, because our instrumental variables (which also use information from the confidential loan database) are lagged one year relative to the firm's loan receipt. Loans to firms in U.S. territories are excluded, because of uneven coverage of other data sources. Cancelled loans are excluded from the treatment group.¹³ We aggregate cases where borrowers receive multiple SBA loans in the same year, and we analyze only the first year of SBA loans when borrowers receive them in multiple years.¹⁴

We link the confidential SBA 7(a) and 504 data as well as publicly available 7(a), 504, and disaster loan data covering loans since the inception of these programs to the Census Bureau's employer and non-employer business registers.¹⁵ Since both the confidential 7(a) and 504 data and the Census Bureau data contain Internal Revenue Service Employer Identification Numbers (EINs) and Social Security Numbers (SSNs), we first link by EIN and SSN.¹⁶ For confidential 7(a) and 504 records that cannot be linked by EIN or SSN, and for the publicly available data without EINs or SSNs, we probabilistically link records by different combinations of business name, street address, and zip code. As shown in Appendix Table AI, 86.9 percent of the confidential loan records are linked to a business register, of which 7.9 percent are linked only to a non-employer business register (i.e., they have no payroll employment). We exclude firms receiving a disaster loan before their first 7(a) or 504 loan, as well as firms receiving a 7(a) or 504 loan prior to 1992. Firms require an industry code, state (for those with 19 or fewer employees), and employment in

¹³ Note that cancelled loans cannot be used for a placebo test, because the cancellation is at the initiative of the borrower and thus could be correlated with later outcomes. Firms with cancelled loans are eligible to be control firms, however, and they make up about one percent of controls. For robustness we have estimated regressions without these controls, and the results are qualitatively similar.

¹⁴ Our loan amount variable is the amount disbursed, converted to real 2010 prices using the annual average Consumer Price Index. We use the total amount from loan financing, not just the amount guaranteed by the SBA. For 504 loans, we impute the total loan amount based on the guaranteed amount specified in the database, using the 504 program guidelines. The SBA-guaranteed portion is 40 percent, the equity share is 10 percent plus an additional 5 percent if a new business and/or an additional 5 percent if special use property, and the residual is a non-guaranteed bank loan. We are unable to observe if the project is for special use property; our imputations assume there is none. The database includes a third party loan amount, but it contains many implausibly high values.

¹⁵ The SBA has a separate disaster loan program, and we have names and addresses for recipients from 1953 to March 31, 2011. Because these loans are not our focus we drop them from both the treatment and potential control groups.

¹⁶ About three-fourths of the linked records are linked via EIN or SSN. We focus on 7(a) and 504 loans in the confidential database because the match rate to Census data is much higher with EINs and SSNs. The publicly-available 7(a) and 504 data are used only to exclude firms receiving such loans prior to 1992 from the analysis.

the year prior to loan receipt to be included in the matching process with LBD control firms, as described in the next section. Of these, we could not find any control firms meeting our matching criteria (discussed in the next section) for 19 percent of them. About 57,000 treated firms do not have employment in each of the next three years following loan receipt, which is necessary for the dependent variable in the main regression sample. About 6,400 additional treated firms cannot be included in the main regression sample, because none of their matched controls has employment in each of the next three years after the treated firm's loan receipt.

The LBD consists of longitudinally linked employer business registers (Jarmin and Miranda 2002) tracking all firms and establishments with payroll employment in the U.S. non-farm business sector on an annual basis in 1976 to 2012. The SBA loan link to employer business registers allows us to connect the SBA data to the entire LBD. Besides employment, the LBD contains annual payroll, establishment age (based on the first year the establishment appears in the dataset), state, county, zip code, industry code, and firm id. The industry code is a four-digit SIC code through the year 2001 and a six-digit NAICS code in 2002 to 2012.

We aggregate the LBD to the firm level, assigning each firm the location of its largest establishment by employment and its modal industry code. The firm birth year is defined as the earliest birth year among establishments belonging to the firm when it first appears in the LBD, and the firm exit year is the latest exit year among establishments belonging to the firm in the last year the firm appears in the LBD. Our firm employment measure is adjusted to focus on organic job creation. Employment in $t-1$, the year prior to the treatment year (defined for control firms as the matched treated firm's treatment year), is the base year (unadjusted) for treated firms and their matched controls. If a merger or acquisition occurs before the base year, the employment of the acquired establishments as of the merger year is included in the firm's employment in all pre-merger years, as if the establishments were always together. If a divestiture occurs before the base year, the employment of the divested establishments is not included in the firm's employment prior to divestment, as if the establishments were never together. If a merger, acquisition, or divestiture occurs after the base year, employment of divested establishments as of the year prior to divestment is included in all subsequent years, while that of the acquired establishments is not.¹⁷

¹⁷ For acquisitions prior to the base year and divestitures after the base year, we use a single employment value for the acquired or divested establishment, applied to all pre-acquisition years and post-divestment years, respectively, to avoid including employment changes occurring under other firms' ownership.

Table II contains summary statistics on basic characteristics of SBA loan recipients and non-recipients (treated and non-treated firms), calculated from the LBD. Recipients are smaller on average than non-recipients (14 versus 20 employees), but they are larger at the median (7 versus 4 employees). Recipients are younger (mean age of 8 versus 11 years, and median 6 versus 9). They are more likely to operate in certain industries, including manufacturing and trade. And, as we discuss further below, they grow faster on average during the pre-loan period.

[Table II about here]

To construct our instrumental variables, described in Section IV, we link SBA lenders, listed in the SBA loan database, to the LBD via name and address matching (no lender EINs or SSNs are included in the SBA data). We first perform a computer matching procedure like the one we use for loan recipients, then do a clerical match for the lenders remaining unlinked and which have issued at least five SBA loans. With these procedures, 74.5 percent of lenders and 84.9 percent of PLP lenders are linked to the LBD. The shares of all SBA loans issued by linked banks are higher – 91.9 percent of all SBA loans, 94.1 percent of PLP lenders’ SBA loans, and 90.1 percent of PLP lenders’ PLP loans. Our instrumental variables use information from the LBD on the locations of each of the establishments (branches) of the linked lenders.

Finally, Table III contains summary statistics on some county-level variables that are particularly important for our instrumental variable and heterogeneity analyses. One is employment growth in the county over the same time period as the dependent variable. A second county control is the county-year share of employment in bank branches, measuring the local extent of financial development. A third is the county-year CRA loan amount total subtracted by the SBA loan amount total (both for loans under \$1 million) and divided by employment, measuring the availability of conventional loans. The table shows means and standard deviations for these variables as well as how they vary across county-years with respect to two measures of SBA loan availability: below-median and above-median SBA loan volume per employee and the presence or non-presence of a PLP bank. County employment growth is higher where SBA loan intensity is larger, and conventional loan availability is greater in counties with PLP banks, but otherwise there is little difference across these categories. We control for these variables in most of the regression results reported below; in the case of employment growth we control for 4-digit-industry-county-year growth, measured at the same time as the firm-level growth dependent

variable, so the results can be interpreted as deviations from growth at other firms at the same time and in the same county and industry.

[Table III about here]

III. Basic Methods and Results

A. The Identification Problem

Ideally, we would like to estimate the causal effect of financial access on firm-level growth. The difficulties include the definitions of financial access and of growth and the fact that finance may take various forms, some of them unobserved. Most daunting is the problem that observed receipt of finance is a function of not only the availability of sources of finance but also of characteristics of the borrower including ability to repay and to benefit from more capital, which are again typically unobserved. In other words, we observe an equilibrium outcome determined jointly by supply and demand, both of them partially driven by unobservables.

Our approach in this paper is to focus upon the firm-level employment growth effects of a governmental intervention, the SBA loan programs. We choose employment growth as the outcome variable because it is the cleanest firm size measure in our data, and because the SBA itself states “growing business and creating jobs” as its top strategic goal. We focus on the SBA intervention because it is interesting in its own right and also because it provides the possibility of estimating the marginal value of this form of finance beyond that from other sources, including conventional loans and informal sources such as friends and family. We do not observe these other sources, so our estimates are not of the effects of expanding or contracting financial access more generally, which could be much more difficult to identify, but only of the effects of the programs. Given some identifying assumptions, however, the estimates can be interpreted as lower bounds on the effects of financial access for SBA loan recipients.

Although most of our estimation focuses on the impact of additional loan dollars, it is convenient to use the standard binary treatment notation for describing the identification problem. Let $TREAT_{it} \in \{0,1\}$ indicate whether firm i receives an SBA loan in year t , and let y_{it+s}^1 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_{it+s}^0 . The loan’s causal effect on employment growth for firm i at time $t+s$ is defined as $y_{it+s}^1 - y_{it+s}^0$, and the average effect of treatment on the treated as

$$E\{y_{it+s}^1 - y_{it+s}^0 | TREAT_{it} = 1\} = E\{y_{it+s}^1 | TREAT_{it} = 1\} - E\{y_{it+s}^0 | TREAT_{it} = 1\} \quad (1)$$

The standard evaluation problem is that y_{it+s}^0 is unobserved for recipients; thus $E\{y_{it+s}^0 | TREAT_{it} = 1\}$, the average employment outcome of loan recipients had they not received a loan, must be estimated. Identification relies on somehow constructing this counter-factual average using the average employment of never-treated firms, $E\{y_{it+s}^0 | TREAT_{it} = 0 \forall t\}$. In doing so, a number of issues arise from the nature of the data and sample, from the observable characteristics of firms that tend to receive treatment, from possible self-selection into the SBA loan programs based on unobservables, and from unobservable characteristics of non-treated firms. Each of these creates potential biases.

Beginning with the nature of the data and sample, our inability to link all SBA recipients in the SBA data to the LBD could lead to biases in either direction. The constructed control group may well contain unlinked treated firms whose treatment is unobserved. Indeed, because the controls are relatively high-growth firms (as a result of the matching to treated firms), the incidence of unobserved treatment among them is likely to be higher than in the whole population. 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 reduces our ability to pair them with otherwise similar firms, but if start-ups have a stronger (weaker) employment response to SBA loans than do existing firms, then this exclusion again implies a negative (positive) bias. Finally, firms with no employees prior to loan receipt are excluded; if such firms have a stronger (weaker) employment response, the effect will be to bias our estimates downward (upward).

A second set of issues arises from observable characteristics of the treated and non-treated firms. As shown in Table II, treated firms differ by size, age, and industry from non-treated firms. Perhaps most interesting is that firms receiving an SBA loan tend to have grown much faster prior to the loan than typical non-recipient firms in the U.S. economy. Mean employment growth from 3 years to 1 year prior to the loan of 0.116 for treated firms versus 0.023 for all other firms measured in all years in the LBD. This fact, which our research has uncovered for the first time, is not inconsistent with the regularity that most small firms exhibit low growth rates (e.g., Hurst and Pugsley 2011), but it suggests that a naïve comparison of post- minus pre-loan employment levels of treated versus non-treated firms is likely to yield an upward-biased estimate. We use matching and regression procedures, described in the next subsection, to account for such

differences in observables as well as unobservables that are time-invariant. Problems and solutions concerning time-varying unobservables are addressed in Section IV.

B. Matching and Regression Procedures

To take into account observable and fixed unobservable characteristics that may affect both the growth of the firm and the size and probability of an SBA loan, we estimate regression functions on a matched sample of treated and control firms. The Appendix describes our matching procedures and resulting sample in detail, and here we provide only a very brief summary. We restrict the sample to firms with no prior SBA loans and with no missing values for key variables necessary for matching, especially employment in the year before treatment. We select controls by first matching potential controls exactly on 4-digit industry, age categories, employment size categories, and year; firms with fewer than 20 employees are also matched exactly on state. In order to control for growth history, we carry out propensity score matching on several other variables, including four-year histories of employment, sales, and assets. We impose common support, and for each treated firm we select controls with a propensity score within a 10 percent bandwidth. Controls are weighted with a quadratic function inversely to the distance of their propensity score from the treated firm's so that closer matches are much more highly weighted.¹⁸

Evidence for the success of the matching procedures in almost entirely eliminating the observable pre-treatment differences between the treated and non-treated firms can be found in Appendix Table AIII, in the calculations of percentage reduction in the normalized difference for each variable. Treated firm employment and average wage are substantially larger than for non-treated firms prior to propensity score matching, and treated firms experience more employment growth in the four years prior to treatment. After matching, these differences are negligible.

Given the matched sample, one way to estimate the loan effect would be to simply compare the average outcomes for treated versus control firms. Another would compare outcomes for the data after differencing from the pre- to post-loan periods to eliminate unobserved, time-invariant differences; Heckman, Ichimura, and Todd (1997, 1998) recommend combining matching with difference-in-difference (DID) methods. For a number of reasons, we estimate regressions to

¹⁸ These procedures reduce the sample size significantly, and the Appendix contains an analysis of differences in characteristics between the original SBA loan list and the final matched sample, as well as much more detail on the implementation of matching.

implement DID methods using the matched sample. The regressions account for time-invariant unobservables in a natural way, and they permit us to estimate the effect of the size of the SBA loan, rather than the binary treatment. As we show in Figure 1, loan size varies quite considerably, and the analysis should allow for this variation.

Given our long time series, we find it useful to constrain the time frame around which we calculate employment growth to focus on the short- and medium-term effects of the loan. This puts all of the loan cohorts on an equal footing, so that each counts equally rather than having longer time series for the early cohorts and shorter for the later ones. The basic form of the regression therefore uses the change in employment as the dependent variable, as follows:

$$\Delta E_{ijt} = \boldsymbol{\rho}_t + \alpha_j + X_{ijt}\boldsymbol{\beta} + \theta_i\delta + u_{ijt} \quad (2)$$

where ΔE is the change in the number of employees from the pre-treatment to the post-treatment period, i indexes firms from 1 to I , j indexes from 1 to J the treated firms to which the firm is a control (for treated firms $i = j$), and t indexes loan years. $\boldsymbol{\rho}_t$ is a vector of year dummies for loan cohorts, α_j is a fixed effect for each group of treated firms and its matched controls (the treatment-control group), X_{ijt} is a set of other variables including firm age and age squared, and in some specifications variables representing demand conditions and finance availability; and u_{ijt} is an idiosyncratic error. θ_i is the amount of the SBA loan (which equals 0 for non-treated firms) received in year t , and δ is the loan effect of interest. In some specifications, we permit δ to vary by type of loan or other variables, as explained in Section V on extensions below.¹⁹

The dependent variable is defined in our main specification as the change in average employment from three years before to three years after the loan: $\Delta E_{ijt} = E_{ij,post} - E_{ij,pre}$, with $E_{ij,post} = (E_{ijt+1} + E_{ijt+2} + E_{ijt+3})/3$, and $E_{ij,pre,t} = (E_{ijt-1} + E_{ijt-2} + E_{ijt-3})/3$.²⁰ In other specifications, we define a base year as the year prior to the loan, $t-1$, and compute employment differences for each year from five before to five after the loan: $\Delta E_{ijt} = E_{ijt+s} - E_{ijt-1}$, $s = -5, -4, \dots, 4, 5$. The pre-loan coefficients are useful for diagnosing some forms of selection (they permit a “pre-program test” in the sense of Heckman and Hotz 1989, or a “pseudo-outcome” test in the sense of Imbens and Wooldridge 2009). The post-loan coefficients permit estimation of short- versus long-term effects.

¹⁹ We report results with robust standard errors adjusted for clustering on treatment-control firm groups. We have also estimated regressions with alternative clustering on county, county-year, and state-industry-year, with little difference in the standard errors.

²⁰ In cases of missing values for year $t-3$, we average employment in $t-2$ and $t-1$, and if year $t-2$ employment is missing, we use employment in $t-1$.

For robustness, we also investigate a specification in which both the dependent variable and loan amount are scaled by average employment in the three pre-loan years. This specification relates the employment growth rate, rather than absolute change in numbers of employees, to the loan amount per pre-loan employee, rather than simply the loan amount.

C. Matching and Regression Results

Table IV shows basic OLS results for the matched sample, including treatment-year age, age-squared, and treatment-control group fixed effects (FE), the α_j from Subsection III. B. In alternative specifications, we include a different county control for local growth or financial availability, either singly or all together. The final specification scales the dependent variable and loan amount by average employment in the three years before the loan.

[Table IV about here]

The five estimates of the “unscaled” SBA loan effect are almost exactly the same, implying 3.0 additional employees per million dollars of SBA loans. When scaled by pre-loan employment, the coefficient is slightly higher, at 3.15. Concerning the estimates for the controls, the negative coefficient on county-industry growth in the unscaled specifications seems surprising, but the mean of 0.034 and standard deviation of 0.132 (from Table III) implies that these are tiny magnitudes; the coefficient is positive but statistically insignificant in the scaled specification. The banking share coefficients are all statistically insignificant, while the coefficient for conventional loan availability is negative (although statistically insignificant in the scaled specification). Most important, these controls make no discernable difference for the loan amount estimate.²¹

Estimating the specification relative to $t-1$ for each other year from $t-5$ to $t+5$, the results are shown in Figure 2. The relative employment growth associated with SBA loan size grows during the post-loan period, implying an average of 4.2 extra employees by $t+5$. Prior to the loan year, however, there are only tiny, statistically insignificant differences associated with loan

²¹ The CRA data are available only for a shorter time period, from 1996 onwards, so to keep a consistent sample we impute the 1996 values in the years 1991 to 1995. As a robustness check to investigate whether results are sensitive to these imputations, we have estimated the regressions two additional ways. In both sets we include only the treatment years 1997 to 2009. In the first set of regressions, the actual CRA numbers are used in all years, while in the second set 1996 to 2000 values are imputed using 2001 values. The results are very similar.

amount. But this result does not completely allay all concerns about selection bias, as discussed in the next section.²²

[Figure 2 about here]

IV. Instrumental Variables Methods and Results

A. Motivation: Identification Problems due to Unobservables

Interpreting the results from the matching and regression estimates as causal requires an assumption of no correlated time-varying unobservables. This assumption would be incorrect if treated firms experience idiosyncratic demand, productivity, or cost shocks precisely during the treatment year. The shock could be positive, for instance an idea or a project to raise demand or productivity, or negative, when demand falls or costs rise for some unobserved reason. In either case, a firm experiencing the shock might be motivated to seek a loan, in the first case for growth and in the second for survival. The former implies positive selection into treatment, while the latter implies negative selection. In seeking to estimate a lower bound for loan effects, we are most concerned about the first case, but the second is also possible, so that a priori the direction of the estimation bias is indeterminate. In more formal discussion of the identification assumptions and potential biases below, we use the notation $S_{it} = 1$ for firm i receiving a shock to demand, cost, or productivity in year t ; we normalize the shock so that the expected effect on employment is positive. Defining variables as regression-adjusted to account for aggregate time effects, time-invariant unobservables, and other factors, treated firms receiving the shock will have average employment

$$E\{y_{it+s}|TREAT_{it} = 1, S_{it} = 1, X\} \quad (3)$$

and treated firms not receiving the positive shock will have

$$E\{y_{it+s}|TREAT_{it} = 1, S_{it} = 0, X\} \quad (4)$$

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.

²² The Appendix contains the results from some more basic specifications, including matching only on year and pre-loan employment and without FE; by contrast with Figure 2, these results show a pre-loan trend for treated firms.

A second type of unobservable is finance from a non-SBA source, such as informal credit or a conventional loan from a bank. Our data contain no firm-level information on any sources of capital aside from the SBA loan, so we cannot measure these, but it is useful to consider how they might affect the results. In general, if SBA loan recipients have more (less) additional financing in the SBA loan receipt year than controls, this might bias upward (downward) estimates of the effect of additional dollars in the SBA loan because they would then be reflecting the impact of all the additional credit, not just the amount of the SBA loan. However, there are several reasons to doubt that compared to controls, treated firms would be receiving larger amounts of fresh finance from non-SBA sources at the same time that they receive the SBA loan. To start with, “pecking-order theory” suggests that because of imperfect information and agency problems, firms should exhaust their internal and personal sources of finance, including informal borrowing from family and friends, before turning to the market (Myers and Majluf 1984), so SBA borrowers may be less likely to be freshly drawing upon these sources compared with controls.²³ Moreover, SBA loans are heavily if not fully collateralized, and the SBA loan itself lowers the firm’s remaining debt capacity, so treated firms are *ceteris paribus* less likely than controls to receive other loans in addition. Because the implicit subsidy created by the guarantee lowers the interest cost, the entrepreneur should prefer a larger SBA loan to adding another conventional loan, even assuming such a loan is available. Finally, the presence of significant financing from conventional loans received contemporaneously with an SBA loan would raise questions about whether the firm meets the “Credit Elsewhere Requirement” discussed above, possibly leaving the lender open to charges of fraud and abuse, resulting in potential fines and exclusion from SBA program participation. This consideration could be especially important for the PLP banks used in our IV strategy, since they would have the most to lose.

For these reasons, we focus on the possibility that control firms receive non-SBA finance. A comparison of loans less than \$1 million from the SBA database with those from Community Reinvestment Act (CRA) disclosure data on business loan originations under \$1 million for 1996 to 2009 (FFIEC 2015) implies that SBA loans account for 0.83 percent of the number and 4.48 percent of the volume of all such loans.²⁴ This suggests that many control firms receive non-SBA

²³ Also, to be eligible for an SBA loan, applicants should have already exhausted these sources.

²⁴ FFIEC (2015) contains CRA data only from 1996, and we calculate these ratios through 2009 because this is the last treatment year we study. Only about 0.06 percent of SBA loans exceed \$1 million during this period.

finance. If we use $K_{it} = 1$ to denote those firms receiving finance through a non-SBA source in year t , we may 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: $E\left\{y_{it+s} \mid \begin{matrix} TREAT_{it} = 0, \\ S_{it} = 0, K_{it} = 0 \end{matrix}\right\}$ for controls receiving neither a shock nor non-SBA finance, $E\left\{y_{it+s} \mid \begin{matrix} TREAT_{it} = 0, \\ S_{it} = 1, K_{it} = 0 \end{matrix}\right\}$ for those receiving the shock but no other finance, $E\left\{y_{it+s} \mid \begin{matrix} TREAT_{it} = 0, \\ S_{it} = 0, K_{it} = 1 \end{matrix}\right\}$ for those not getting the shock but receiving non-SBA finance, and $E\left\{y_{it+s} \mid \begin{matrix} TREAT_{it} = 0, \\ S_{it} = 1, K_{it} = 1 \end{matrix}\right\}$ 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 S and K are defined so that

$$E\{y_{it+s}|S_{it} = 1\} > E\{y_{it+s}|S_{it} = 0\} \quad (5)$$

and

$$E\{y_{it+s}|K_{it} = 1\} > E\{y_{it+s}|K_{it} = 0\} \quad (6)$$

implying 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 in 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 regression adjustments, that treatment is independent of the shock, so

$$E\{TREAT_{it}|S_{it} = 1, X\} = E\{TREAT_{it}|S_{it} = 0, X\} \quad (7)$$

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 still the causal effect of the program. If we assume either that K_{it} is received only by control firms or, alternatively, that more is received by controls than by treated firms (with the same marginal effect on employment growth), then our estimate will be a lower bound of the causal effect of finance on growth.

Relaxing the conditional independence assumption, selection bias reflects differences between the counterfactual outcome for treated firms and the observed outcome for the controls:

$$B(X) = E\{y_{it+s}^0 | TREAT_{it} = 1, X\} - E\{y_{it+s}^0 | TREAT_{it} = 0, X\} \quad (8)$$

If $E\{TREAT_{it} S_{it} | X\} > 0$, so that if treated firms would have performed better than non-treated even if the former had not been treated, then $B(X) > 0$. The magnitude of $B(X)$ in this case depends on the strength of the correlation between treatment and the shock. Our next step, therefore, will be to construct instrumental variables for SBA loan receipt that are functions only of SBA loan supply and therefore independent of the shock. As in the previous section, we also add variables to control for industry-county-specific growth and the availability of conventional finance in the locality.

B. Instrumental Variables

The matching and regression procedures account for observable differences and time-invariant unobservable differences between the treated and control firms. For the estimates from these methods to be interpreted as causal, the identifying assumption is that there is no systematically different shock at the time of the loan to the treated versus control firms' demand, productivity, costs, or access to capital from other sources. As discussed in the previous subsection, however, there are plausible reasons to expect that such shocks may occur, and there are several factors so that the direction of the bias is a priori unclear. The greatest concern for our attempt to estimate a lower-bound effect would be that treatment results from a positive shock, such as a new idea for business growth, and that the observed post-loan growth results from this shock rather than the access to capital provided by the SBA programs.

To account for such unobservables in the selection process by which firms receive SBA loans of varying sizes, we therefore develop an instrumental variables strategy based on some institutional features of the SBA program as well as on empirical regularities about small business credit markets. As discussed in Section I, the SBA program works through partial (50-85 percent) loan guarantees provided to private lenders, some of which are PLPs with a special status that reduces their administrative costs. As we showed, these PLPs account for a large share of all SBA loans, particularly the larger loans. The concentration of loans suggests that participation in SBA loan provision is to some extent a corporate bank (firm-level) decision, such that some banks participate a great deal in SBA lending, while others do little or none. Even within the PLPs, the average number and average size of SBA loans varies substantially, which may reflect differences in corporate lending policy among these banks. Furthermore, the PLP lenders are unevenly dispersed across the U.S. and have changed over time.

Another key fact is that SBA borrowers, like other small businesses, seem to rely heavily on local lenders. The importance of the local credit market for small business lending is well-documented in previous research.²⁵ While our SBA data contain headquarters rather than lending branch location, so that distance cannot be computed directly, indirect evidence of local lending for SBA loans comes from examining the incidence of SBA loans in counties in which no PLP lenders, nor any SBA lenders, are represented. The share of SBA loans in counties where there was no PLP branch in the previous year is 3.2 percent, and 0.3 percent for no SBA branch. The corresponding shares for PLP loans are 0.02 percent and 0.0003 percent respectively. Together, these facts imply geographic variation in the ease and likelihood for a small business to obtain an SBA loan and in the likely loan size. Our analysis defines the financial institutions in a county as the local credit market supply.²⁶

At the same time, SBA loans are a tiny share of the total banking business, even for lenders who do the most SBA lending, and it seems unlikely that banks choose to enter and exit local credit markets as a result of this line of business. An exact calculation of the share of SBA loans in all loans seems to be impossible with existing data sources, but we can combine several of them to produce a rough estimate. For the top ten SBA commercial bank lenders in 2012, for instance, we have combined information from the SBA database with CRA bank disclosure reports to calculate for all new loans under \$1 million the ratio of the total amount of SBA loans to all CRA loans, both measured at origination. This ratio is 0.050, implying that the total volume of SBA loans under \$1 million is 5 percent of the total volume of all loans under \$1 million. To assess the role of loans of this size in all bank lending, we must turn to FDIC call reports. For the same top ten SBA commercial bank lenders in 2012, we can calculate the ratio of the volume of loans in the categories of commercial and industrial loans and those secured by nonfarm nonresidential properties that were originally under \$1 million to the volume of all loans, in both cases on a stock basis, so that this refers to loans outstanding.²⁷ The ratio is 0.052, implying that small loans (under

²⁵ See, for example, Amel and Brevoort (2005), and Agarwal and Hauswald (2010), who report median distance of 2.4 to 4 miles from borrower to lending branch.

²⁶ The SBA itself uses only lenders in the same county as the borrower in its LINC (Leveraging Information and Networks to access Capital) “online referral tool to connect small business borrowers with participating SBA lenders.”

²⁷ The amount of outstanding loans that were less than \$1 million when originated is the sum of the June 30, 2012 FDIC call report variables for amount currently outstanding of loans secured by nonfarm residential properties and commercial and industrial loans (RCON5565, RCON5567, RCON5569, RCON5571, RCON5573, and RCON5575). Total loans outstanding is the June 30, 2012 FDIC call report variable total loans and leases, net of unearned income (RCON2122).

\$1 million in this measure) to nonfarm businesses only account for about 5 percent of all bank loans. Although these two ratios are on different bases (flow versus stock), if we were to multiply them, they would suggest that SBA loans in 2012 accounted for only 0.25 percent of all bank loans, even for these largest SBA lenders. Banks also have other lines of business – deposit collection, payment processing, asset management, financial advising, and investment banking – that are not accounted for here, but may be important determinants of branch locations.

Finally, in any given year, PLP lenders actually issue PLP loans in only a fraction of the counties where they operate: Table I shows that PLP banks have branches in an average of 42.5 counties during 1991 to 2009, while the mean number of counties in which borrowers received PLP loans from one of these banks is 6.3. For all these reasons, banks’ choices of counties in which to locate their branches seem quite unlikely to be related to the possibilities for SBA loan business.

Based on this reasoning, we construct instrumental variables that reflect the local presence of a branch of a PLP bank weighted by the pervasiveness of SBA lending activity for the parent bank, measured outside the local lending market. We present results for six instrumental variables that differ along two dimensions: first, they rely on different assumptions for the process by which small businesses search for loans and are potentially matched with an SBA lender; second, they either use all PLP banks or only those issuing PLP loans in at least four states in the given year, as discussed below. The identifying assumption for all the instrumental variables is that banks do not operate in particular local (county) markets based on the potential for SBA loan business.

In the simplest case, the variable is simply an indicator for the presence in county c of at least one branch of at least one bank b that made at least one PLP loan to a firm located in a different county $-c$ in year $t-1$:

$$PLP_{ct} = \mathbf{I}(1 | D_{cbt-1}P_{-cbt-1} = 1 \text{ for any } b) \quad (9)$$

where $D_{cbt-1} = 1$ if bank b has at least one branch in this county c in year $t-1$ and $P_{-cbt-1} = 1$ if bank b has at least one PLP loan in some other county $-c$ in year $t-1$ and t is the loan year. The assumption here, based on our analysis of the lender data in Section I, is that borrowers are much more likely to get an SBA loan and to receive a bigger SBA loan if they are located in a county where a lender offered a PLP loan in the previous year. We do not count PLP loans issued to firms in the same county as the borrower to avoid potential endogeneity of PLP loan availability in the same county.

A second variable weights the presence of different banks in a county by the strength of the corporate policy favoring SBA loans, measured by the pervasiveness of PLP loans in other counties in which the bank had branches in the previous year:

$$SumShares_{ct} = \sum_b [D_{cbt-1} \times [\sum_{-c} D_{-cbt-1} P_{-cbt-1} / \sum_{-c} D_{-cbt-1}]] \quad (10)$$

For instance, in a county with three banks present, two of which made PLP loans the previous year, one in half its counties and the other in one quarter, $SumSharesIV_{ct} = 0.75$. The assumption is that a borrower is more likely to get an SBA loan, and to get a bigger one, if there are more local branches of PLP banks for which the PLP loan business is pervasive across the other counties in which they operate.

We have no information on how small businesses search across lenders for a loan, and how they may be eventually matched with an SBA lender, but one possibility is that it is not the number of PLP lenders with local branches that matters, but rather the share of all local branches they account for. If borrowers search for loans randomly across local bank branches, then they have a higher probability of obtaining an SBA loan where a larger share of branches are part of PLP banks. Also, since $SumShares_{ct}$ is additive in the number of PLP lenders in the county, counties with more banks are likely to have larger values, which is also true of PLP_{ct} . While we control for measures of conventional small business lending in many of our specification, it is possible that the number of banks in a county could be correlated with unobserved factors contributing to small business growth. The next variable addresses these possibilities by weighting each locally represented PLP bank by the bank's share of the county's bank branches:

$$BranchShare_{ct} = \sum_b [[E_{cbt-1} / E_{ct-1}] \times D_{cbt-1} \times [\sum_{-c} D_{-cbt-1} P_{-cbt-1} / \sum_{-c} D_{-cbt-1}]] \quad (11)$$

where E_{cbt-1} is the number of establishments (branches) of bank b in county c in time $t-1$ and E_{ct-1} is the total number of banking establishments in county c in time $t-1$. The identification assumption of this instrument is that banks do not decide their share of branches in a county based on their expected returns from SBA lending. We believe this is very credible not only because, as we have demonstrated, SBA loans constitute a tiny fraction of all loans and all banking business even for the largest SBA lenders, but also because the share of any one bank in a county is a function of all other banks decisions. Moreover, this instrument is again not a function of banks issuing SBA loans in the specific county of the treated firm, but rather of the banks' PLP activity in other counties. For these reasons, $BranchShare_{ct}$ is our preferred instrument.

Even if the instruments avoid endogeneity issues with banks choosing to open branches where SBA lending is particularly profitable, identification could still be threatened if banks with particular portfolios of branches are more likely to become PLP banks. For example, a bank concentrated in a metropolitan area where SBA lending is common might decide to become a PLP bank. To address this concern, we have added IVs that are the same as those above except they include only those lenders issuing PLP loans in at least four states during the same year. Since they issue PLP loans over a wide geographic area, having branches in any area should have little effect on the bank's decision to apply for PLP status. These instruments have the same mnemonics as those above except for appending "4States" to indicate the restriction to lenders with PLP loans within at least four states: *PLP4States_{ct}*, *SumShares4States_{ct}*, and *BranchShare4States_{ct}*.

Table V contains summary statistics for the instruments and their components. All show substantial variation over the sample, with standard deviations about twice the mean. The correlations of these variables with the treatment variable loan amount are shown in the first-stage results in the next section.

[Table V about here]

Finally, even if conceptually these instruments seem to be independent of the incentives for banks to operate in particular counties, it is possible that the variables are correlated with other characteristics of counties. We would particularly worry about positive correlations with either growth or financial development that might bias the estimate SBA loan effect upward. Table III already provided some evidence on this issue, however, showing that county-years with a PLP bank present actually grow less rapidly (average employment growth of 0.032 compared to 0.040) and have a smaller share of total employment in banking (1.4 per cent versus 2.2 percent); however they do have a higher rate of conventional loans (\$1,706 per employee versus \$1,009).²⁸ The OLS results reported in Subsection III. C. found no difference in estimates when we controlled for these variables, but because our instruments vary with county-years they may matter more in an IV specification. We also consider specifications that permit the SBA loan effect to vary with these county characteristics, the results for which we report in our heterogeneity analysis in Section V.

²⁸ 34.5 percent of county-years have no PLP banks, 20.6 percent have exactly one, 25.0 percent have two to four, and 19.9 percent have five or more.

C. Instrumental Variable Estimates

Table VI contains results for the IV regression specifications using *BranchShare_{ct}*. The estimated coefficients on SBA loan amount are slightly larger than the OLS, at 3.5 in the unscaled specification and 3.3 when scaled. Standard errors are larger than for the OLS specifications, but all the coefficients are statistically significant at any conventional threshold. The coefficients on the county-level control variables are very similar to those of the OLS and they make little difference for the loan amount coefficient of interest, whether entered singly or jointly. The robustness of the coefficient of interest to these controls suggests that the estimates of the employment growth effect of SBA loans are not driven by local differences in either product demand or credit supply.

[Table VI about here]

The instrument is highly significant in the first-stage, with an F-statistic in the range of 838 to 909 (246 when scaled), implying a close relationship between the instrument and the loan amount treatment variable and easily clearing any conventional threshold for significance. The 1st-stage coefficients on the instrumental variables are also substantial economically, implying a meaningful relationship with SBA loan amount. For example, a one-standard-deviation increase in *BranchShare* is associated with a predicted \$40,000 larger loan. The F-statistics are little affected by the additional controls for growth and financial development. This is notable, because if PLP lenders were making location decisions based on small business lending, one would expect the inclusion of these controls to significantly reduce the explanatory power of the instrumental variables for local PLP lender presence.

Table VII shows results for the other five instruments in unscaled specifications with all control variables, since the latter make little difference for the loan amount coefficient. These estimates of the loan amount effects vary somewhat more widely, ranging from 2.4 to 4.4 jobs per million dollars of loans. The results for the control variables are not shown because they are very similar to those in Table VI. First-stage results are also more varied, with the F-statistic ranging from 199 to 931.

[Table VII about here]

Thus, the IV estimates bracket the OLS result of 3.0 and are quantitatively similar if usually slightly larger. They are also similar to the results from less complex methods such as the simple regression on the matched sample, which yielded a coefficient of 3.7, and not far from the largest

estimate of 4.1 based on exact but not propensity score matching, both of which we showed in Table AIV.

To help choose among these different specifications, it is useful to again examine the annual estimates relative to $t-1$ for each other year from $t-5$ to $t+5$. Figure 3 plots these results for the specifications with the *PLP*, *SumShares*, and *BranchShare* instruments.²⁹ The *PLP* specification shows statistically significant coefficients from $t-5$ to $t-3$ and a substantial negative pre-loan trend, while the *SumShares* has little overall pre-loan trend but does have a statistically significant coefficient in $t-2$. The most persuasive dynamics in showing no statistically significant coefficients in the pre-loan period and no evidence of a pre-existing trend is the *BranchShare* specification. Henceforward we focus on this instrument.

[Figure 3 about here]

The tendency for the IV point estimates to exceed the OLS suggests that selection bias in the OLS estimates tends to be negative rather than positive, so that firms receiving loans tend to have unobserved prospects that are relatively worse than controls. Note also that in nearly all cases, the post-loan dynamics show a steady upward trend in relative employment of treated versus control firms through $t+5$, suggesting that the loan produces longer term growth rather than just a temporary bump followed by a return to previous trend.

V. Extensions

In this section we explore the heterogeneity of the treatment effect by firm size and age, broad loan program category, cyclical conditions, and local conventional loan availability. We also provide an analysis of non-tradable industries within and across counties and estimates accounting for firm exit. Finally, we calculate the magnitude of overall job creation and the cost to the government per job that are implied by our estimated program impacts.

A. Estimates of Heterogeneous Effects

Patterns of firm growth by size and age have been the subject of considerable research, but as noted in the introduction, this research has not investigated the impact of governmental programs intended to support particular types of firms, such as the SBA programs. Table VIII contains estimates of SBA loan impacts by firm size and age. We split the regression sample into

²⁹ Results for the 4-state versions of these are similar to their unrestricted counterparts and are omitted to save space.

below-median and above-median firm employment and age groups and use the same methods as above, including all the control variables for growth and financial development. Both OLS and IV estimates are shown. The results imply somewhat larger loan effects on employment growth for firms with more than seven employees and for those younger than seven years old in the pre-loan year. These estimated differences are larger in the IV than in the OLS specification, but they are less precisely estimated in the IV results, and it is hard to conclude that the differences by size and age are statistically significant. Estimates for all size and age groups are statistically significantly different from zero, however, and they are mostly within the range of magnitudes seen under different specifications above. The one exception is the IV estimate of 6.4 for younger firms, although the standard error of 2.4 again suggests caution in concluding that the effect is outside the range estimated so far.³⁰

[Table VIII about here]

The 7(a) and 504 loan programs are similar in providing guarantees, but as noted above they also have differences. Table IX displays the results from separate regressions for each program. All estimates are statistically significantly different from zero and in the general range of magnitudes, but the OLS and IV specifications yield opposite conclusions about the ranking of the program impacts. The OLS implies slightly higher impact per million dollars for the 504 program, while the IV implies a larger impact for the 7(a) program. The difference between the loan amount coefficients in the IV regressions is not statistically significant, however.

[Table IX about here]

The extent to which SBA loan programs foster job creation may be of particular interest during times of high unemployment, when they might function as cyclical stabilizers. With this motivation, Table X contains results when we permit the loan amount coefficient to vary with two alternative measures of the business cycle: a dummy for years when the national unemployment rate is below the median during the treatment years 1992 to 2009, and a similar dummy for below-median state unemployment.³¹ The results are inconsistent across OLS and IV methods. The OLS results imply a counter-cyclical effect, insofar as the loans are estimated to have a smaller effect when unemployment is low and a larger effect when it is high; the difference is not large, however. The IV interactions with the low unemployment dummies are positive, implying a pro-cyclical

³⁰ Brown, Earle, and Morgulis (forthcoming) contains a more detailed analysis of firm age and size groups.

³¹ We define these variables in the same way as in Nakamura and Steinsson (2014).

effect, but they are statistically insignificant. One interpretation of the OLS-IV difference could be that the OLS reflects cyclically varying selection bias such that firms with relatively good unobserved prospects tend to receive SBA loans during recessions and those with relatively worse unobserved prospects tend to receive SBA loans during expansions.

[Table X about here]

A related question is how the loan amount effect varies with local credit conditions. We use the CRA measure of the volume of loans per employee in the county-year, which has been a control in all specifications going back to Table IV, interacted with SBA loan amount. The estimation results in Table XI suggest smaller employment effects of SBA loans when local credit is more plentiful, intuitively suggesting that the marginal benefit of the intervention is declining when credit is looser.

[Table XI about here]

An important concern about the estimates of employment changes at firms receiving loans is the implicit assumption the program has no effect on non-treated firms used as controls in the analysis.³² Because only a tiny fraction of firms in the U.S. receive SBA-backed loans, this assumption is plausible.³³ But it is nevertheless possible that even if treated firms grow as a result of loan receipt, 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 displace employment at non-treated firms competing 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 (e.g., Heckman, LaLonde, and Smith, 1999). Positive spillovers would imply that our estimates of the direct effect are lower than the total effect, and therefore we focus attention here on the possibility of negative displacement. If these spillovers result from product market competition, where loan receipt gives the beneficiary an advantage over its competitors, then negative effects should be larger for nearby firms in industries with a narrow geographic scope.

³² The program evaluation literature sometimes refers to this as the “stable unit treatment value assumption” (SUTVA) (Imbens and Wooldridge 2009).

³³ The annual number of disbursed SBA loans ranges from 22,586 in 1992 to 96,458 in 2007, compared to 4,689,051 and 5,520,998 firms with positive employment in the LBD in those years.

To assess this possibility, we distinguish a non-tradable sector and focus on treated firms for which we have two control groups: the first is matched controls within the same county as the treated firm, and the second consists of controls in counties where there are no firms in the same industry receiving SBA loans that year.³⁴ A within-county analysis does not permit us to use our IV strategy, so these equations are all estimated by OLS. While this is an important caveat to this analysis, we draw comfort from our finding through most of the paper that the OLS and IV results are similar.

Estimates for all industries and for the non-tradables are shown in Table XII. For the former, we find virtually identical coefficients with the two control groups. But the difference is much greater when we restrict attention to the non-tradable sector, where the employment growth associated with \$1 million of loans is 2.9 relative to within-county controls versus 2.6 relative controls from non-SBA county-industries (though the difference is not statistically significant). These differences are inconsistent with correlated demand shocks among firms in close proximity, but they could result from displacement, as nearby controls grow more slowly as a result of the loan. The difference is not large, implying only a small role for displacement – on the order of 10 percent of the employment effect.

[Table XII about here]

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, 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

³⁴ Recall that all controls are exact-matched on 4-digit industry; in the second control group they are included only when they are in counties where no firm in that same industry received an SBA loan that year, so they should not be influenced by a displacement effect. Following Mian and Sufi (2014), the non-tradable sector is defined to include the following retail trade industries: grocery stores; specialty food stores; beer, wine, and liquor stores; health and personal care stores; gasoline stations; clothing stores; shoe stores; jewelry, luggage, and leather goods stores; sporting goods, hobby, and musical instrument stores; book, periodical, and music stores; department stores; other general merchandise stores; florists; office supplies, stationery, and gift stores; used merchandise stores; other miscellaneous store retailers; full-service restaurants; limited-service eating places; special food services; drinking places (alcoholic beverages); automobile and other motor vehicle dealers; automobile parts accessories and tire stores; furniture stores; home furnishing stores; and electronics and appliance stores.

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 matched sample before imposing the constraint that the firm must have positive employment in $t+1$, $t+2$, and $t+3$, exit rates by $t+3$ are 24.2 percent for treated firms and 23.3 percent for controls (kernel weighted, as in the regressions, for greatest comparability). A crude comparison therefore suggests only a slight difference in 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 following exit and re-estimate. The loan amount coefficients, shown in Table XIII, are slightly smaller than those without the imputations, and the differences are not statistically significant, 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 in future research.

[Table XIII about here]

B. Estimates of Overall Job Creation and Costs per Job

Finally, we can provide rough calculations of the overall job creation and cost per created job that the estimates suggest are attributable to SBA loans. To estimate total employment effects over the entire period of our sample, we may apply the coefficients in Tables IV and VI to the total volume of loans over the period we study. For fiscal years 1992 to 2007, the total volume of 7(a) and 504 loans disbursed in the 50 states plus the District of Columbia is \$230.4 billion, in 2010 dollars. Multiplying the coefficients in the range of 3-3.5 yields estimated total job creation in the range of 690,000 to 813,000 over this period. Slightly smaller or larger estimates can be obtained with different instruments, but we find those alternatives less credible than the *BranchShare* that we emphasize.

For all the SBA loans over the same period, the applicants predicted they would create or retain 5.6 million jobs as a result of the loan.³⁵ Thus, our estimates are significantly less than the sum of those anticipated by applicants (and those reported by the SBA), but they are not trivial in magnitude. Note also that our year-by-year point estimates suggest that the job creation effects are sustained and even increase going out to five years after the loan: the OLS coefficient rises to 4 and the IV rises to 7. As we move out in event time, the coefficients are decreasingly well-estimated, however, especially for the IV where the 99 percent confidence interval includes 3 and 12 at five years out. If one accepts the relatively precisely estimated OLS estimate of 4, then the implied total job creation would be about 940,000. If we subtract 10 percent due to displacement (an upper bound in the sense that it applies to the non-tradable sector), net job creation would be about 620,000 to 850,000 over this period.

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, resulting 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. The main costs are default and administration, while SBA receives revenue from guaranty fees. Our own calculations from the SBA loan database yield \$18.934 billion (in 2010 dollars) in loan default chargeoffs on all 7(a) and 504 loans issued in 1992 to 2007, the years in our sample for the effects through $t+5$.³⁶ SBA (2010) reports \$138,601 million in administrative expenses for all its business loan programs in 2009. This cost is not available for earlier years, but if we assume that the administrative expenses were the same in real terms every year, the total administrative expenses in 1992 to 2007 are \$2.262 billion in 2010 dollars.³⁷ When we apply the SBA guaranty fees that have been in place since at least August 1, 2008 (the effective date of the earliest available Standard Operating Procedure

³⁵ The SBA's estimate of the number of jobs supported by the programs is based on the sum of the predicted jobs created and retained from the loan applications, but the exact methodology is not explained and has apparently changed over time. SBA (2011) reports a "change in methodology" and removal of cancelled loans from the calculations (p. 11); SBA (2012) reports "a filter applied to reduce outliers" (p. 4). We also exclude cancelled loans, but we do not know the SBA procedure regarding outliers.

³⁶ The chargeoff amount is the gross balance of the defaulted loan minus the gross amount that could be recovered through liquidation. These are defaults through May 31, 2015. Some of the loans still outstanding as of that date may later default.

³⁷ This is likely an overestimate, since the programs expanded significantly during the period, and some of the expenses were for other business loan programs besides 7(a) and 504 (e.g., the SBIC program and direct loans).

manual available on SBA's website) to the 1992 to 2007 loans, the total amount of fees is \$3.643 billion in 2010 dollars. Adding losses from defaults and administrative expenses and subtracting fees yields a total cost estimate of \$17.553 billion. Using the results from the OLS and IV specifications of Tables IV and VI implies a cost range from \$21,580 to \$25,450 per job created. By comparison, we estimate that the jobs created by the program pay an average of \$30,000 (whether estimated by OLS or IV), based on regressions where the dependent variable is the firm's wage bill rather than employment.³⁸ In addition, one might impute savings of unemployment insurance benefits not paid out to workers who found jobs, which are difficult to estimate without further information on individual workers and their wage and employment histories.

VI. Conclusion

Our estimates of the effects of the SBA loan programs 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. We use the matched sample to estimate program effects with treatment-control group fixed effects regressions and with instrumental variables based on arguably exogenous variation in the supply of SBA loans.

The estimation results imply that \$1 million of loans raises loan recipient employment by 3 to 3.5 jobs on average during the first three post-loan years. This basic result is robust across OLS and IV specifications and to controlling for detailed industry-county growth and county-level credit conditions. Estimating the effects farther out, at five years after loan receipt, the implied job creation ranges more widely, at 4 estimated by OLS, and from 5 to 7, depending on the particular IV specification, although the precision of these estimates decline with the number of post-loan years. The uniformly positive impact of increased capital access on employment suggests these factors of production are gross complements rather than substitutes.

³⁸ The regression coefficients (SEs) are 92.26(5.93) for the OLS and 105.31(59.65) for the IV, and the numbers in the text are obtained by dividing these coefficients by the corresponding coefficients from the employment regressions of 3.0 and 3.5, for the OLS and IV, respectively.

Extending these results to consider several dimensions of heterogeneity, our analysis finds somewhat larger point estimates for younger and larger firms, but little systematic evidence of differences between the estimated employment effects of the 7(a) and 504 programs or of variation over the business cycle. A clearer finding is that employment effects of SBA loans tend to be smaller where local credit is more plentiful, suggesting that the marginal benefit of the intervention is declining with general credit availability conditions. We also provide a preliminary investigation of possible displacement of competing firms by focusing on non-tradable industries, comparing estimated effects using controls in the same county with estimates using controls from counties in which there was no SBA loan in the same industry and county that year; the results imply a displacement effect on the order of 10 percent of the total. While our basic analysis considers only surviving firms (through the post-loan estimation period), survival and exit are clearly important issues, so, as a robustness check, we estimate similar equations with zeroes imputed for firms after exit; the results differ little, suggesting that our basic results are not driven by exit. All of these findings should be taken as preliminary with further research necessary to disentangle the many interesting sources of variation in the estimated SBA loan effects.

Finally, our rough estimates of the total job creation from the SBA 7(a) and 504 loans issued in 1992 to 2007 range from 620,000 to 850,000, with a cost per job created ranging from \$21,580 to \$25,450. This cost 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 to \$75,000.³⁹ A full assessment of costs should account for possible distortions, such as those highlighted by Hurst and Pugsley (2011), but these figures suggest that alleviating financial constraints through SBA loan programs may be a relatively low-cost tool to generate employment.

If these estimates are correct, they imply that credit constraints impeded growth of small businesses prior to receiving the SBA loans. The estimates pertain to the effect of the program, not

³⁹ Other estimates based on stimulus spending under the ARRA include Feyrer and Sacerdote (2012), and Wilson (2012), who report of the costs per job of \$107,000-\$400,000 and \$125,000, respectively. Chodorow-Reich et al. (2012) report an increase of 3.8 job years for additional \$100,000 in Medicaid spending under the ARRA through July 2010, implying a cost of \$26,000 per job year. If we restrict the horizon to the first post-loan year, for comparability, our estimates imply a cost of \$10,100 to \$20,800 per job year; of course we also show sustained employment increases beyond the first post-loan year.

directly to credit access more generally. As we have shown, however, the SBA loans are only a small fraction of all small business loans in the U.S. economy (less than 1 percent in number and less than 5 percent in volume). We have also argued that non-treated firms in the control group, which is constructed to match the pre-loan growth of treated firms, are more likely to receive these non-SBA loans. In this case, our estimates of the SBA loan impact can also be interpreted as lower bound estimates of the effect of financial access for similar credit-constrained firms more generally.

References

- Agarwal, Sumit, and Robert Hauswald, 2010, Distance and private information in lending, *Review of Financial Studies* 23, 2757-2788.
- Amel, Dean F., and Kenneth P. Brevoort, 2005, The perceived size of small business banking markets, *Journal of Competition Law and Economics* 1, 771-784.
- de Andrade, Flavio, and Deborah Lucas, 2009, Why do guaranteed SBA loans cost borrowers so much? Unpublished manuscript, Northwestern University.
- Bach, Laurent, 2014, Are small businesses worthy of financial aid? Evidence from a French targeted credit program, *Review of Finance* 18, 877-919.
- Banerjee, Abhijit V., and Esther Duflo, 2014, Do firms want to borrow more? Testing credit constraints using a directed lending program, *The Review of Economic Studies* 81, 572-607.
- Bartik, Timothy J., and George Erickcek, 2010, The employment and fiscal effects of Michigan's MEGA tax credit program, Upjohn Institute Working paper no. 10-164.
- Beck, Thorsten, 2009, The econometrics of finance and growth, in Terence C. Mills and Kerry Patterson, ed: *Palgrave Handbook of Econometrics* (Palgrave Macmillan).
- Brevoort, Kenneth P., and Timothy H. Hannan, 2006, Commercial lending and distance: Evidence from Community Reinvestment Act data, *Journal of Money, Credit, and Banking* 38, 1991-2012.
- Birch, David L., 1987, *Job Creation in America: How Our Smallest Companies Put the Most People to Work* (Free Press, New York, NY).
- Brown, J. David, John S. Earle, and Dana Lup, 2005, What makes small firms grow? Finance, human capital, technical assistance, and the business environment in Romania, *Economic Development and Cultural Change*, 54(1), 33-70.
- Brown, J. David, John S. Earle, and Yana Morgulis, forthcoming, Job creation, small vs. large vs. young, and the SBA," in John Haltiwanger, Erik Hurst, Javier Miranda, and Antoinette Schoar, eds.: *Measuring Entrepreneurial Businesses: Current Knowledge and Challenges*, Chicago and London: NBER and University of Chicago Press.
- Chodorow-Reich, Gabriel, 2014, The employment effects of credit market disruptions: Firm-level evidence from the 2008-9 financial crisis, *Quarterly Journal of Economics* 129(1), 1-59.
- Chodorow-Reich, Gabriel, Laura Feiveson, Zachary Liscow, and William Gui Woolston, 2012, Does state fiscal relief during recessions increase employment? Evidence from the American Recovery and Reinvestment Act, *American Economic Journal: Economic Policy* 4, 118-145.
- Clementi, Gian Luca, and Hugo A. Hopenhayn, 2006, A theory of financing constraints and firm dynamics, *Quarterly Journal of Economics* 121, 229-265.
- Congressional Budget Office, 2010, Estimated impact of the American Recovery and Reinvestment Act on employment and economic output from April 2010 through June 2010.
- Craig, Ben R., William E. Jackson III, and James B. Thomson, 2009, The economic impact of the Small Business Administration's intervention in the small firm credit market: A review of the research literature, *Journal of Small Business Management* 47, 221-231.
- Davis, Steven, John Haltiwanger, and Scott Schuh, 1996, *Job Creation and Destruction* (MIT Press, Cambridge, MA).
- Demetriades, Panicos O., and Khaled A. Hussein, 1996, Does financial development cause economic growth? Time-series evidence from 16 countries, *Journal of Development Economics* 51, 387-411.
- Federal Financial Institutions Examination Council, 2015, A guide to CRA data collection and reporting.

- Feyrer, James, and Bruce Sacerdote, 2012, Did the stimulus stimulate? Real time estimates of the effects of the American Recovery and Reinvestment Act, NBER Working paper no. 16759.
- Glennon, Dennis, and Peter Nigro, 2005, An analysis of SBA loan defaults by maturity structure, *Journal of Financial Services Research* 28, 77-111.
- Greenstone, Michael, Alexandre Mas, and Hoai-Luu Nguyen, Do credit market shocks affect the real economy? Quasi-experimental evidence from the great recession and 'normal' economic times, Working Paper, September 2015.
- Haltiwanger, John, Ron Jarmin, and Javier Miranda, 2013, Who creates jobs? Small vs. large vs. young, *Review of Economics and Statistics* 95, 347-361.
- Heckman, James, and Joseph V. Hotz, 1989, Choosing among alternative nonexperimental methods for estimating the impact of social programs: The case of manpower training, *Journal of the American Statistical Association* 84, 862-74.
- Heckman, James J., Hidehiko Ichimura, and Petra E. Todd, 1997, Matching as an econometric evaluation estimator: Evidence from evaluating a job training programme, *Review of Economic Studies* 64, 605-654.
- Heckman, James J., Hidehiko Ichimura, and Petra E. Todd, 1998, Matching as an econometric evaluation estimator, *Review of Economic Studies* 65, 261-294.
- Heckman, James J., Robert J. LaLonde, and Jeffrey A. Smith, 1999, The economics and econometrics of active labor market programs, in Orley Ashenfelter and David Card, ed: *Handbook of Labor Economics, Vol. 3A* (Elsevier Science B.V., Amsterdam).
- Hubbard, R. Glenn, 1998, Capital-market imperfections and investment, *Journal of Economic Literature* 36, 193-225.
- Hurst, Erik, and Benjamin Wild Pugsley, 2011, What do small firms do? *Brookings Papers on Economic Activity*, 73-118.
- Imbens, Guido W., and Jeffrey M. Wooldridge, 2009, Recent developments in the econometrics of program evaluation, *Journal of Economic Literature* 47, 5-86.
- Jarmin, Ronald S., and Javier Miranda, 2002, The Longitudinal Business Database, CES Working paper 02-17.
- King, Robert G. and Ross Levine, 1993, Finance and growth: Schumpeter might be right, *Quarterly Journal of Economics* 108, 717-737.
- Lelarge, Claire, David Sraer, and David Thesmar, 2010, Entrepreneurship and credit constraints: Evidence from a French loan guarantee program, in Josh Lerner and Antoinette Schoar, ed: *International Differences in Entrepreneurship* (University of Chicago Press, Chicago, IL).
- Levine, Ross, 2005, Finance and growth: Theory and evidence, in Philippe Aghion and Steven Durlauf, ed: *Handbook of Economic Growth* (Elsevier Science, Netherlands).
- Mian, Atif R., and Amir Sufi, 2014, What explains the 2007-2009 drop in employment, *Econometrica* 82, 2197-2223.
- Myers, Stewart C., and Nicholas Majluf, 1984, Corporate financing and investment decisions when firms have information that investors do not have, *Journal of Financial Economics* 13, 187-221.
- Nakamura, Emi, and Jon Steinsson, 2014, Fiscal stimulus in a monetary union: Evidence from US regions, *American Economic Review* 104, 753-792.
- Neumark, David, 2013, Spurring job creation in response to severe recessions: Reconsidering hiring credits, *Journal of Policy Analysis and Management* 32, 142-171.

- Neumark, David, Brandon Wall, and Junfu Zhang, 2011, Do small businesses create more jobs? New evidence for the United States from the National Establishment Time Series, *Review of Economics and Statistics* 93, 16-29.
- Pagano, Marco, and Giovanni Pica, 2012, Finance and employment, *Economic Policy* 27.
- Rosenbaum, Paul, and Donald B. Rubin, 1985, Constructing a control group using a multivariate matched sampling method that incorporates the propensity score, *The American Statistician* 39, 33-38.
- Small Business Administration, 2010, Agency financial report, Fiscal year 2010.
- Small Business Administration, 2011, Summary of performance and financial information, FY 2011.
- Small Business Administration, 2012, Agency financial report, Fiscal year 2012.
- Small Business Administration, 2015, Lender and development company loan programs.
- Whitehouse.gov*, 2010, Weekly address: President Obama calls for new steps to support America's small businesses, February 6.
- Wilson, Daniel J., 2012, Fiscal spending jobs multipliers: Evidence from the 2009 American Recovery and Reinvestment Act, *American Economic Journal: Economic Policy* 4, 251-282.

Table I
Summary Statistics on Banks

This table reports summary statistics on three different types of banks. Banks are defined as firms that have at least one establishment in the commercial banking sector or which issued at least one SBA loan in the year. *Non-SBA Banks* are those not issuing SBA loans, *Non-PLP SBA Banks* issue at least one SBA loan but no PLP loans, and *PLP Banks* issue at least one PLP loan in a given year. Only establishments with positive employment in the year are included. For banks that did not issue any SBA loans in the year, the establishment counts include only those in the commercial banking sector. The numbers are means across 1991-2009 annual averages. Loan amounts are in 2010 dollars. N.A. = not-applicable.

	Mean for Non-SBA Banks	Mean Across Non-PLP SBA Banks	Mean Across PLP Banks
Number of banks	7,253	1,493	276
Number of branches per bank	5.0	12.9	238.1
Number of counties where bank has ≥ 1 branch	1.9	4.1	42.5
Number of counties where ≥ 1 firm gets an SBA loan from this bank	0	1.3	12.9
Number of counties where ≥ 1 firm gets a PLP loan from this bank	0	0	6.3
Number of SBA Loans	0	6.6	127.1
Number of PLP Loans	0	0	36.4
Mean SBA Loan Amount	N.A.	286,288	297,010
Mean PLP Loan Amount	N.A.	N.A.	460,822

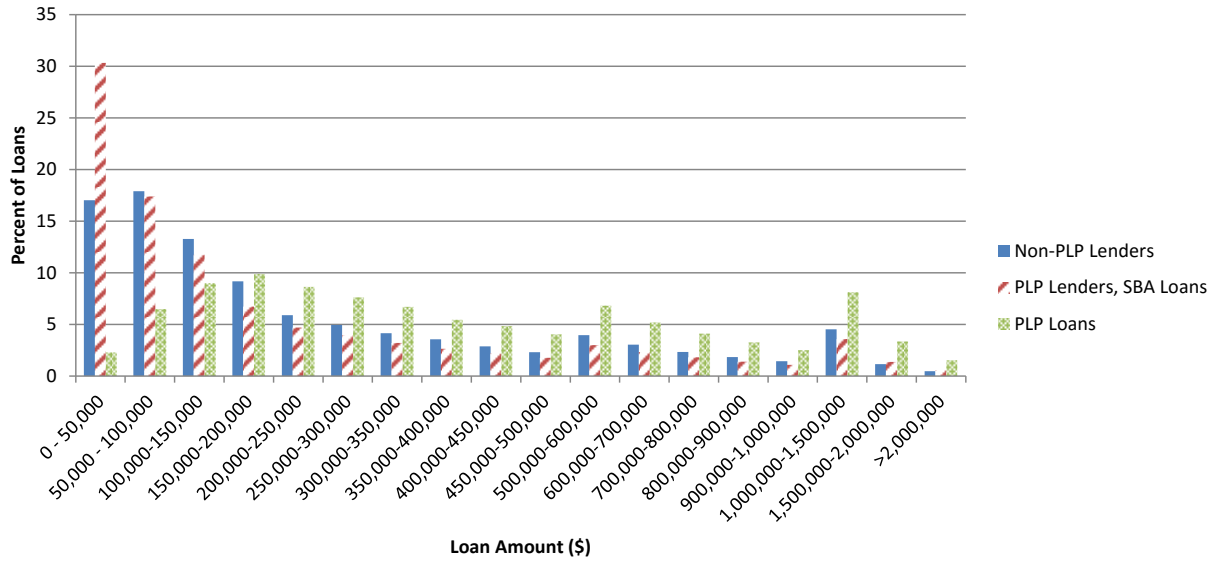


Figure 1. SBA and PLP Loan Size Distributions, Non-PLP vs. PLP Lenders. This figure shows the loan amount distributions in 1991-2009 in real \$2010, separately for SBA loans issued by non-PLP lenders, SBA loans issued by PLP lenders, and PLP loans.

Table II
Summary Statistics for Treated and Non-Treated Firms

This table reports summary statistics for treated and non-treated firms. Non-treated firms are included in each year they appear in the LBD between 1992 and 2009, while treated firms are included only in the treatment year. Year t is the treatment year for treated firms. The variables come from the LBD.

	All Non-Treated Firms	All Treated Firms
Employment _{$t-1$} Mean	19.85	14.33
Employment _{$t-1$} Standard Deviation	678.82	36.78
Employment _{$t-1$} Median	4	7
Emp. Growth from $t-3$ to $t-1$ Mean	0.023	0.116
Age Mean	10.91	8.04
Age Standard Deviation	8.34	7.21
Age Median	9	6
Percent by Sector:		
Construction	11.61	10.44
Manufacturing	5.57	10.33
Wholesale Trade	6.74	9.67
Retail Trade	16.07	20.70
Finance/Insurance/Real Estate	8.74	3.72
Services	45.83	40.17
Other	5.44	4.97

Table III**Summary Statistics for County-Level Measures of Growth and Financial Development**

This table reports summary statistics for county-level measures of growth and financial development. *County Employment Growth* is average county employment in $t+1$ through $t+3$ minus average employment in $t-3$ through $t-1$, divided by average county employment in these years. *Bank Branch Employment_{t-1}/Total County Employment_{t-1}* is the employment of establishments in the county in the commercial banking sector or which belonged to a firm issuing at least one SBA loan in the year, divided by total employment of establishments in the county, both computed from the LBD. *CRA Loan Amount/County Emp_{t-1} Ratio* is the $t-1$ dollar amount of Community Reinvestment Act (CRA) loans in the loan amount categories less than \$1 million, subtracting SBA loans under \$1 million, then converted to thousands of 2010 dollars and divided by $t-1$ county employment. *County SBA Loan Amount_{t-1} per Employee* is the total dollar amount of SBA loans in 2010 dollars distributed in $t-1$ to firms located in the county divided by total county employment that year. *PLP County* is defined as having at least one branch belonging to a bank issuing at least one PLP loan in another county in the year. The means reported in the table are calculated across county-years in 1992-2009.

	Mean (SD)	County SBA Loan Amount _{t-1} per Employee		Non-PLP County	PLP County
		< = median	> median		
County Employment Growth	0.034 (0.132)	0.023	0.046	0.040	0.032
Bank Branch Employment _{t-1} / Total County Employment _{t-1}	0.016 (0.010)	0.017	0.015	0.022	0.014
CRA Loan Amount _{t-1} /County Emp _{t-1} Ratio	1,486 (1,115)	1,385	1,582	1,009	1,706

Table IV
OLS Estimates of the SBA Loan Effect

This table reports *Loan Amount* and county-level control coefficients from OLS regressions where the dependent variable is average employment in $t+1$ through $t+3$ minus average employment in $t-3$ through $t-1$. *Loan Amount* is in millions of 2010 dollars. The “scaled specification” alters the dependent variable and loan amount by dividing them by average employment in $t-3$ through $t-1$. Treatment year *age*, *age squared*, and treated firm-control fixed effects are also included in the regressions. *County-Industry Employment Growth*, measured at the county-four-digit industry level (SIC industries for treatment years 1992-1998 and NAICS industries for treatment years 1999-2009), is the sum of LBD county-industry employment in years $t+1$, $t+2$, and $t+3$ minus county-industry employment in $t-3$, $t-2$, and $t-1$, divided by $\frac{1}{2}$ the sum of county-industry employment in years $t-3$, $t-2$, $t-1$, $t+1$, $t+2$, and $t+3$. It excludes the employment of the firm in the regression to which the variable refers. *Bank Branch Employment_{t-1}/Total County Employment_{t-1}* and *CRA Loan Amount \leq \$1m_{t-1}/County Emp_{t-1} Ratio* are defined in Table III. Standard errors clustered by treated firm-control groups are reported in parentheses. The regressions are kernel-weighted. The total number of firm-year observations is 4,581,000, and the number of SBA firms is 157,400, rounded to the nearest 100 for disclosure avoidance.

	Unscaled					Scaled
Loan Amount	2.994*	2.996*	2.995*	2.991*	2.994*	3.149*
	(0.101)	(0.101)	(0.101)	(0.101)	(0.101)	(0.193)
County-Industry Employment Growth		-0.360			-0.350	0.014
		(0.142)			(0.142)	(0.011)
Bank Branch Employment _{t-1} /Total County Employment _{t-1}			2.552		1.878	-0.068
			(3.886)		(3.890)	(0.347)
CRA Loan Amount \leq \$1m _{t-1} /County Emp _{t-1} Ratio				-	-	-0.007
				0.130*	0.125*	(0.005)
				(0.041)	(0.041)	

* denotes significance at the one percent level.

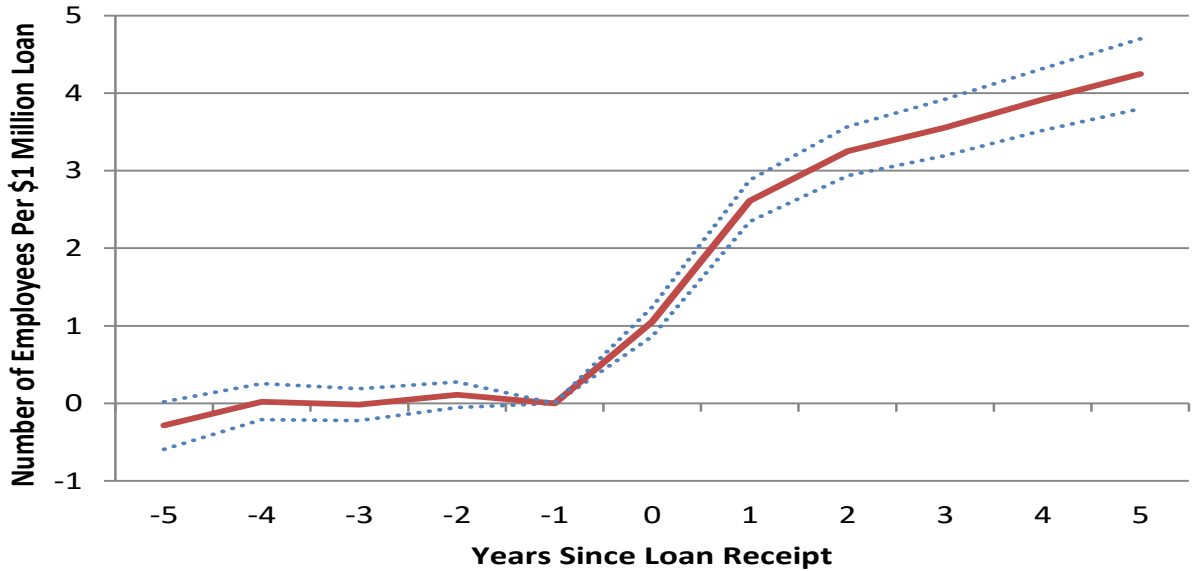


Figure 2. OLS Estimates of the SBA Loan Effect, Year-by-Year. This figure shows loan amount coefficients from OLS regressions where the dependent variable is the firm’s employment in the respective year minus employment in year $t-1$. *Loan Amount* is in millions of 2010 dollars. *County-Industry Employment Growth*, *Bank Branch Employment_{t-1}/Total County Employment_{t-1}*, *CRA Loan Amount \leq \$1m_{t-1}/County Emp_{t-1} Ratio*, *treatment year age*, *age squared*, and treated firm-control fixed effects are also included in the regressions. *Bank Branch Employment_{t-1}/Total County Employment_{t-1}* and *CRA Loan Amount \leq \$1m_{t-1}/County Emp_{t-1} Ratio* are defined in Table III, and *County-Industry Employment Growth* is defined in Table IV. The sample is the same in the regressions in years t , $t+1$, $t+2$, $t+3$, $t+4$, and $t+5$. To be in the samples for the pre-treatment years, treated firms and at least one control must have positive employment in the respective year, as well as in $t-1$ through $t+5$. The dotted lines are the bounds of the 99 percent confidence interval, based on standard errors clustered by treated firm-control groups.

Table V**Instrumental Variables and Components Descriptive Statistics, 1991-2008**

This table reports descriptive statistics for the instrumental variables and their components. A PLP bank is defined as a bank issuing at least one PLP loan during the year to a firm located in another county.

	Mean	Standard Deviation	50 th %
PLP Bank's Counties with PLP Loan as Share of Other Counties with Branch (Bank-County-Years)	0.146	0.163	0.10
PLP Bank's Share of County's Branches, Conditional on >0 (Bank-County-Years)	0.099	0.126	0.06
PLP Bank Average Loan Amount in Other Counties (2010 \$Millions), (Bank-County-Years)	0.324	0.224	0.26
<i>PLP_{ct-1}</i> (County-Years)	0.655		
<i>SumShares_{ct-1}</i> (County-Years)	0.414	0.860	0.12
<i>BranchShare_{ct-1}</i> (County-Years)	0.039	0.058	0.02
4+ State <i>PLP_{ct-1}</i> (County-Years)	0.550		
4+ State <i>SumShares_{ct-1}</i> (County-Years)	0.290	0.557	0.08
4+ State <i>BranchShare_{ct-1}</i> (County-Years)	0.029	0.045	0.01

Table VI**IV (*BranchShare_{ct-1}*) Estimates of the SBA Loan Effect**

This table reports *Loan Amount* and county-level control coefficients from IV regressions where the dependent variable is average employment in $t+1$ through $t+3$ minus average employment in $t-3$ through $t-1$. The IV is *BranchShare_{ct-1}*. The “scaled” specification alters the dependent variable and loan amount by dividing them by average employment in $t-3$ through $t-1$. *Loan Amount* is in millions of 2010 dollars. Treatment year *age*, *age squared*, and treated firm-control fixed effects are also included in the regressions. *Bank Branch Employment_{t-1}/Total County Employment_{t-1}* and *CRA Loan Amount <= \$1m_{t-1}/County Emp_{t-1} Ratio* are defined in Table III, and *County-Industry Employment Growth* is defined in Table IV. The regressions are kernel-weighted. Standard errors clustered by treated firm-control groups are reported in parentheses. The number of observations is 4,581,000, and the number of SBA firms is 157,400, rounded to the nearest 100 for disclosure avoidance.

	Unscaled					Scaled
Loan Amount	3.506*	3.491*	3.597*	3.472*	3.531*	3.253*
	(1.069)	(1.068)	(1.104)	(1.074)	(1.109)	(0.977)
County-Industry Employment Growth		-0.369*			-0.361	0.014
		(0.141)			(0.141)	(0.011)
Bank Branch Employment _{t-1} /Total County Employment _{t-1}			3.170		2.831	-0.057
			(3.918)		(4.129)	(0.361)
CRA Loan Amount <= \$1m _{t-1} /County Emp _{t-1} Ratio				-0.130*	-0.116*	-0.006
				(0.042)	(0.044)	(0.004)
1 st Stage <i>BranchShare_{ct-1}</i>	0.737*	0.737*	0.712*	0.734*	0.708*	0.057*
	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)	(0.004)
County-Industry Employment Growth		0.019*			0.020*	0.0025*
		(0.004)			(0.004)	(0.0005)
Bank Branch Employment _{t-1} /Total County Employment _{t-1}			-1.340*		-1.385*	-0.075*
			(0.126)		(0.126)	(0.022)
CRA Loan Amount <= \$1m _{t-1} /County Emp _{t-1} Ratio				-0.015*	-0.016*	-
				(0.001)	(0.002)	0.0011*
						(0.0003)
1 st Stage F Statistic	907.05	908.57	844.92	901.26	838.44	246.00

* denotes significance at the one percent level.

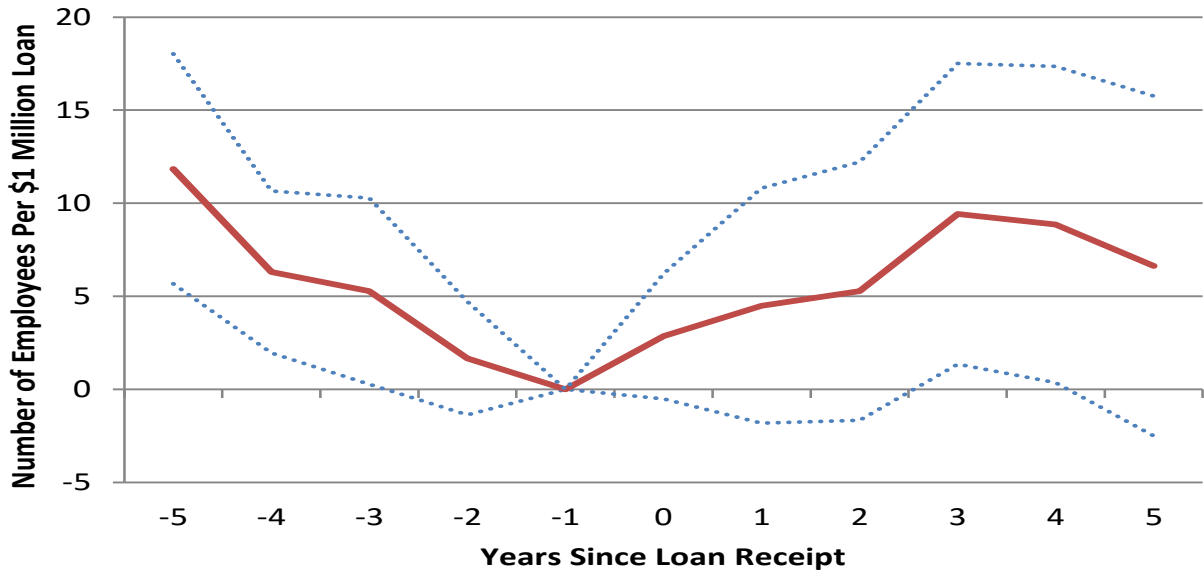
Table VII**Alternative Instrumental Variable Estimates**

This table reports *Loan Amount* coefficients from regressions with alternative IVs where the dependent variable is average employment in $t+1$ through $t+3$ minus average employment in $t-3$ through $t-1$. *Loan Amount* is in millions of 2010 dollars. *County-Industry Employment Growth*, *Bank Branch Employment_{t-1}/Total County Employment_{t-1}*, *CRA Loan Amount \leq \$1m_{t-1}/County Emp_{t-1} Ratio*, treatment year *age*, *age squared*, and treated firm-control fixed effects are also included in the regressions. *Bank Branch Employment_{t-1}/Total County Employment_{t-1}* and *CRA Loan Amount \leq \$1m_{t-1}/County Emp_{t-1} Ratio* are defined in Table III, and *County-Industry Employment Growth* is defined in Table IV. The regressions are kernel-weighted. Standard errors clustered by treated firm-control groups are reported in parentheses. The number of observations is 4,581,000, and the number of SBA firms is 157,400, rounded to the nearest 100 for disclosure avoidance.

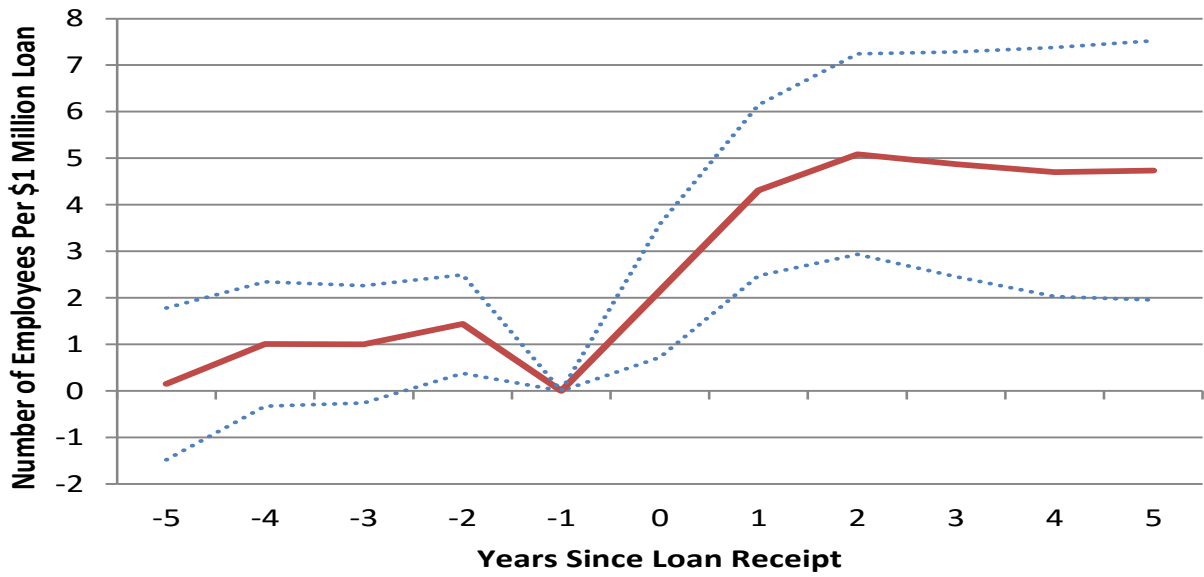
	Alternative Instruments				
	<i>PLP</i>	<i>SumShares</i>	<i>PLP4State</i>	<i>SumShares4State</i>	<i>BranchShare4State</i>
Loan Amount	4.078 (2.491)	2.793* (0.766)	4.094 (1.741)	2.360* (0.878)	4.396* (1.174)
1 st Stage <i>IV_{ct}</i>	0.068* (0.005)	0.0153* (0.0005)	0.075* (0.004)	0.0244* (0.0009)	0.802* (0.030)
F Statistic	198.93	931.03	387.30	695.76	702.24

* denotes significance at the one percent level.

IV: PLP_{ct-1}



IV: $SumShares_{ct-1}$



IV: $BranchShare_{ct-1}$

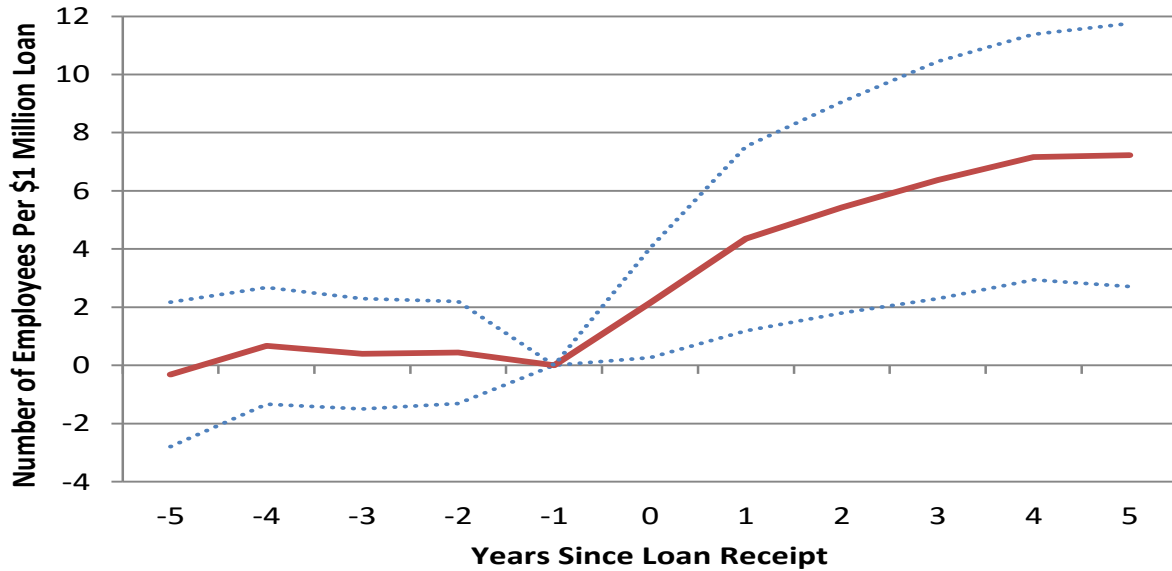


Figure 3. IV Estimates of the SBA Loan Effect, Year-by-Year. This figure shows *Loan Amount* coefficients from IV regressions where the dependent variable is the firm’s employment in the respective year minus employment in year $t-1$. *Loan Amount* is in millions of 2010 dollars. *County-Industry Employment Growth*, *Bank Branch Employment $_{t-1}$ /Total County Employment $_{t-1}$* , *CRA Loan Amount \leq \$1 m_{t-1} /County Emp $_{t-1}$ Ratio*, treatment year *age*, *age squared*, and treated firm-control fixed effects are also included in the regressions. *Bank Branch Employment $_{t-1}$ /Total County Employment $_{t-1}$* and *CRA Loan Amount \leq \$1 m_{t-1} /County Emp $_{t-1}$ Ratio* are defined in Table III, and *County-Industry Employment Growth* is defined in Table IV. The sample is the same in the regressions in years t , $t+1$, $t+2$, $t+3$, $t+4$, and $t+5$. To be in the samples for the pre-treatment years, treated firms and at least one control must have positive employment in the respective year, as well as in $t-1$ through $t+5$. The dotted lines are the bounds of the 99 percent confidence interval, based on standard errors clustered by treated firm-control groups. 1st Stage F Statistics: PLP_{ct-1} : 109.06 ($t-5$), 135.39 ($t-4$), 150.16 ($t-3$), 163.57 ($t-2$), 174.85 ($t...t+5$). $SumSharesIV_{ct-1}$: 653.20 ($t-5$), 709.05 ($t-4$), 747.26 ($t-3$), 793.28 ($t-2$), 851.08 ($t...t+5$). $BranchShare_{ct-1}$: 476.87 ($t-5$), 537.55 ($t-4$), 562.90 ($t-3$), 584.81 ($t-2$), 649.25 ($t...t+5$).

Table VIII**Estimate Effects by Firm Size and Age Groups**

This table reports *Loan Amount* coefficients separately by employment size and age groups from OLS and IV regressions where the dependent variable is average employment in $t+1$ through $t+3$ minus average employment in $t-3$ through $t-1$. *Loan Amount* is in millions of 2010 dollars. *County-Industry Employment Growth*, *Bank Branch Employment_{t-1}/Total County Employment_{t-1}*, *CRA Loan Amount \leq \$1m_{t-1}/County Emp_{t-1} Ratio*, treatment year *age*, *age squared*, and treated firm-control fixed effects are also included in the regressions. *Bank Branch Employment_{t-1}/Total County Employment_{t-1}* and *CRA Loan Amount \leq \$1m_{t-1}/County Emp_{t-1} Ratio* are defined in Table III, and *County-Industry Employment Growth* is defined in Table IV. The regressions are kernel-weighted. Standard errors clustered by treated firm-control group are reported in parentheses. The number of observations and SBA firms are rounded to the nearest 100 for disclosure avoidance.

	≤ 7 Emp _{t-1}	> 7 Emp _{t-1}	Age ≤ 6	Age > 6
OLS				
Loan Amount	2.636*	3.160*	3.141*	2.924*
	(0.087)	(0.121)	(0.158)	(0.128)
IV				
Loan Amount	2.706	3.628*	6.374*	2.292
	(1.316)	(1.277)	(2.447)	(1.196)
1 st Stage <i>BranchShare_{ct-1}</i>	0.205*	1.105*	0.452*	0.932*
	(0.019)	(0.041)	(0.029)	(0.038)
1 st Stage F Statistic	111.29	715.01	242.00	600.15
N	1,473,800	3,107,200	1,723,700	2,857,300
Number of SBA Firms	82,200	75,300	79,500	77,900

* denotes significance at the one percent level.

Table IX
Estimated Effects for 7(a) and 504 Programs

This table reports *Loan Amount* coefficients separately for 7(a) and 504 loans from OLS and IV regressions where the dependent variable is average employment in $t+1$ through $t+3$ minus average employment in $t-3$ through $t-1$. *Loan Amount* is in millions of 2010 dollars. *County-Industry Employment Growth*, *Bank Branch Employment_{t-1}/Total County Employment_{t-1}*, *CRA Loan Amount \leq \$1m_{t-1}/County Emp_{t-1} Ratio*, treatment year *age*, *age squared*, and treated firm-control fixed effects are also included in the regressions. *Bank Branch Employment_{t-1}/Total County Employment_{t-1}* and *CRA Loan Amount \leq \$1m_{t-1}/County Emp_{t-1} Ratio* are defined in Table III, and *County-Industry Employment Growth* is defined in Table IV. The regressions are kernel-weighted. Standard errors clustered by treated firm-control group are reported in parentheses. The number of observations and SBA firms are rounded to the nearest 100 for disclosure avoidance.

	7(a)	504
<hr/>		
OLS		
Loan Amount	2.541*	3.205*
	(0.157)	(0.128)
<hr/>		
IV		
Loan Amount	4.439	2.746
	(2.002)	(1.138)
1 st Stage	0.406*	1.983*
<i>BranchShare_{ct-1}</i>	(0.016)	(0.106)
<hr/>		
1 st Stage F Statistic	606.64	348.38
N	3,720,100	860,900
Number of SBA Firms	128,900	28,600
<hr/>		

* denotes significance at the one percent level.

Table X
Estimated Effects with Cyclical Variation

This table reports coefficients for *Loan Amount* and interactions with dummies for below-median state and national unemployment from OLS and IV regressions where the dependent variable is change in average employment ($t+1$ to $t+3$ minus $t-3$ to $t-1$). *Loan Amount* is in millions of 2010 dollars. *County-Industry Employment Growth*, *Bank Branch Employment_{t-1}/Total County Employment_{t-1}*, *CRA Loan Amount \leq \$1m_{t-1}/County Emp_{t-1} Ratio*, treatment year *age*, *age squared*, and treated firm-control fixed effects are also included in the regressions. *Bank Branch Employment_{t-1}/Total County Employment_{t-1}* and *CRA Loan Amount \leq \$1m_{t-1}/County Emp_{t-1} Ratio* are defined in Table III, and *County-Industry Employment Growth* is defined in Table IV. Unemployment rates are calculated over 1992-2009 and demeaned over the sample, using kernel weights. Regressions are kernel-weighted. Standard errors clustered by treated firm-control group are reported in parentheses. The number of observations is 4,581,000, and the number of SBA firms is 157,400, rounded to nearest 100.

	OLS	
Loan Amount	3.010*	3.006*
	(0.101)	(0.101)
Below-Median State Unemployment in Loan Year	0.817*	
	(0.266)	
Loan Amount*Below-Median State Unemployment	-0.782*	
	(0.202)	
Loan Amount*Below-Median National Unemployment		-0.673*
		(0.201)
	IV	
Loan Amount	3.420*	3.512*
	(1.108)	(1.105)
Below-Median State Unemployment in Loan Year	-0.700	
	(0.742)	
Loan Amount*Below-Median State Unemployment	4.131	
	(2.346)	
Loan Amount*Below-Median National Unemployment		1.219
		(2.134)
Loan Amount 1 st Stage <i>BranchShare_{ct-1}</i>	0.711*	0.717*
	(0.025)	(0.025)
Below-Median State/National Unemployment* <i>BranchShare_{ct-1}</i>	-0.042	-0.150*
	(0.048)	(0.049)
1 st Stage F Statistic	419.64	424.00
Loan Amount*Below-Median State/National Unemployment 1 st Stage <i>BranchShare_{ct-1}</i>	-0.030	-0.035*
	(0.012)	(0.012)
Below-Median State/National Unemployment* <i>BranchShare_{ct-1}</i>	0.665*	0.747*
	(0.022)	(0.025)
1 st Stage F Statistic	452.55	460.82

* denotes significance at the one percent level.

Table XI**Estimates with Variation by Local Conventional Loan Availability**

This table reports *Loan Amount* coefficients and their interactions with a proxy for local small business conventional loan availability from OLS and IV regressions where the dependent variable is average employment in $t+1$ through $t+3$ minus average employment in $t-3$ through $t-1$. *Loan Amount* is in millions of 2010 dollars. *County-Industry Employment Growth*, *Bank Branch Employment_{t-1}/Total County Employment_{t-1}*, *CRA Loan Amount \leq \$1m_{t-1}/County Emp_{t-1} Ratio*, treatment year *age*, *age squared*, and treated firm-control fixed effects are also included in the regressions. *Bank Branch Employment_{t-1}/Total County Employment_{t-1}* and *CRA Loan Amount \leq \$1m_{t-1}/County Emp_{t-1} Ratio* are defined in Table III, and *County-Industry Employment Growth* is defined in Table IV. *CRA Loan Amount \leq \$1m_{t-1}/County Emp_{t-1} Ratio* is demeaned over the sample, using the kernel weights. The regressions are kernel-weighted. Standard errors clustered by treated firm-control group are reported in parentheses. The number of observations is 4,581,000, and the number of SBA firms is 157,400, rounded to the nearest 100 for disclosure avoidance.

	OLS	IV
Loan Amount	3.050*	2.189
	(0.102)	(1.474)
CRA Loan Amount \leq \$1m _{t-1} /County Emp _{t-1} Ratio	0.003	3.422
	(0.039)	(1.626)
Loan Amount* CRA Loan Amount \leq \$1m _{t-1} /County Emp _{t-1} Ratio	-0.755*	-21.196
	(0.127)	(9.768)
Loan Amount 1 st Stage <i>BranchShare_{ct-1}</i>		0.720*
		(0.025)
CRA Loan Amount \leq \$1m _{t-1} /County Emp _{t-1} Ratio		-0.098*
		(0.019)
1 st Stage F Statistic		431.86
Loan Amount* CRA Loan Amount \leq \$1m _{t-1} /County Emp _{t-1} Ratio 1 st Stage <i>BranchShare_{ct-1}</i>		-0.058*
		(0.019)
CRA Loan Amount \leq \$1m _{t-1} /County Emp _{t-1} Ratio * <i>BranchShare_{ct-1}</i>		0.108*
		(0.036)
1 st Stage F Statistic		10.57

* denotes significance at the one percent level.

Table XII**Estimated Effects with Controls in Same Counties vs. Controls in County-Industries without SBA Loans in Year t**

This table reports *Loan Amount* coefficients from kernel-weighted OLS regressions using the same SBA loan recipients in both regressions, but with different sets of controls: ones in the same counties as the treated firms, and ones in county-industries without SBA loans in year t . The dependent variable is average employment in $t+1$ through $t+3$ minus average employment in $t-3$ through $t-1$. *Loan Amount* is in millions of 2010 dollars. Treatment year *age*, *age squared*, and treated firm-control fixed effects are also included in the regressions. Standard errors clustered by treated firm-control group are reported in parentheses. The number of observations and SBA firms are rounded to the nearest 100 for disclosure avoidance.

	All Sectors	Non-Tradable Sector
	Controls in Same Counties	
Loan Amount	2.427*	2.867*
	(0.124)	(0.458)
N	2,267,200	39,600
Number of SBA Firms	51,900	9,500
	Controls in County-Industries without SBA Loans in Year t	
Loan Amount	2.412*	2.599*
	(0.201)	(0.368)
N	244,300	594,500
Number of SBA Firms	51,900	9,500

* denotes significance at the one percent level.

Table XIII
Estimated Effects with Post-Exit Zeroes

This table reports *Loan Amount* coefficients from OLS and IV regressions where the dependent variable is altered to incorporate exit. The dependent variable is average employment in $t+1$ through $t+3$ minus average employment in $t-3$ through $t-1$. For employment in years $t+1$ through $t+3$, if a firm has exited from the LBD prior to that year, zero employment is imputed for the year. *Loan Amount* is in millions of 2010 dollars. *County-Industry Employment Growth*, *Bank Branch Employment_{t-1}/Total County Employment_{t-1}*, *CRA Loan Amount \leq \$1m_{t-1}/County Emp_{t-1} Ratio*, treatment year *age*, *age squared*, and treated firm-control fixed effects are also included in the regressions. *Bank Branch Employment_{t-1}/Total County Employment_{t-1}* and *CRA Loan Amount \leq \$1m_{t-1}/County Emp_{t-1} Ratio* are defined in Table III, and *County-Industry Employment Growth* is defined in Table IV. The regressions are kernel-weighted. Standard errors clustered by treated firm-control group are reported in parentheses. The number of observations is 7,010,900, and the number of SBA firms is 206,600, rounded to the nearest 100 for disclosure avoidance.

	Unscaled		Scaled	
	OLS	IV	OLS	IV
Loan Amount	2.866*	2.515	2.713*	2.698*
	(0.094)	(1.085)	(0.154)	(0.869)
1 st Stage <i>BranchShare_{ct-1}</i>		0.600*		0.052*
		(0.020)		(0.003)
1 st Stage F Statistic		902.45		284.99

* denotes significance at the one percent level.