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Job Creation, Small versus Large versus Young, and the SBA

J. David Brown, John S. Earle, and Yana Morgulis

9.1 Introduction

One of the few areas of recent consensus across all major political groups in the United States is the supposedly important role played by small businesses in job creation. The initiatives justified by this conviction include a variety of small business loan and support programs, largely through the Small Business Administration (SBA), as well as preferential treatment of small businesses in contracting and regulatory requirements.[1] The empirical basis for the belief goes back to Birch (1987), although the underlying methods and data were questioned by Davis, Haltiwanger, and Schuh (1996). More recently, Neumark, Wall, and Zhang (2011) have reconfirmed the Birch conclusion with improved data and methods, but Haltiwanger, Jarmin, and Miranda (2013; hereafter HJM) have shown that the size-growth relationship is not robust to controlling for age (as had Evans [1987] for a much smaller data set on manufacturing industries). Indeed, HJM find that

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1. A recent example is the JOBS Act, which loosens regulations on financing.

the relationship may even reverse signs, so that larger firms contribute more to job creation, once age is taken into account.

This research has attracted considerable attention both from scholars and journalists, and it is very useful as an empirical description of the economy, laying out the "facts" that may be juxtaposed against theories of firm and industry dynamics. Haltiwanger, Jarmin, and Miranda (2013, 360) infer from their results that "to the extent that policy interventions aimed at small businesses ignore the important role of firm age, we should not expect much of an impact on the pace of job creation." Strictly speaking, this inference requires the assumption that the patterns of responsiveness of employment to interventions across different categories of firms (defined by age and size) mimic the empirical regularities of employment dynamics in these categories more generally. While it could be the case that the categories with the strongest record of job creation also respond the most to a given intervention, it is also possible that there is no relationship. Potentially, the types of firms that typically create the fewest jobs might even benefit the most from supportive measures. More generally, empirical regularities have no necessary implications for the design of effective interventions.

Several studies provide indirect evidence that financial constraints on growth vary with firm size and age. Fort et al. (2013) suggest that financial constraints have the greatest impact on smaller, younger firms' growth, finding that their employment dynamics are more sensitive to housing price shocks. They state this could be due to such firms' greater dependence on home equity financing than other firms that can more easily obtain commercial loans. Adelino, Ma, and Robinson (2014) show that start-ups' higher responsiveness to investment opportunities is accentuated in local areas with better access to small business finance, implying that start-up job creation is curbed by financing constraints. Levenson and Willard (2000) supply survey evidence suggesting that younger, smaller US firms are more likely to be denied credit, and Canton et al. (2013) also report that younger, smaller firms across the European Union are more likely to perceive that bank loan accessibility is low. Note, however, that inability to obtain a bank loan does not by itself mean the firm's growth is constrained; the firm may not intend to use the loan for expansion. Hurst and Pugsley (2011) report that most small firms grow very little, entering at low employment levels and tending to remain small; survey evidence suggests the majority do not desire to grow, with the implication that they would not grow even if they had better financial access.[2]

This chapter more directly tests the variation by age and size in the association between financial access and firm growth, using the SBA's 7(a) and 504 loan guarantee programs. For this purpose, we have linked a complete list of

2. An alternative possibility is that small, mature firms are small due to lack of access to finance in the past, in which case they may benefit even more from SBA loans than other firms.

SBA 7(a) and 504 loans to the Census Bureau's employer and nonemployer business registers and to the Longitudinal Business Database (LBD), which tracks all firms and establishments in the US nonfarm business sector with paid employees on an annual basis in 1976–2012. We restrict the analysis to recipients of loans in 1991–2009 and their matched controls.

While our chapter is inspired to some extent by HJM, and we use some of the census data they developed, our question and therefore our methods are different. While HJM measure year-to-year growth in employment, our focus is the change in employment from the period before the SBA loan to the period after the loan is received, as well as on firm survival after loan receipt. Our estimation method involves construction of a control sample of firms based on age, industry, year, size in the year prior to loan receipt, and several years of growth history.

The estimation results suggest that both job creation and survival effects of a \$1 million loan decrease in age, controlling for size. Survival effects also decrease in size, but the association of size and job creation from loans is positive with or without age controls. This contrasts somewhat with HJM, who find positive size effects on job creation only when controlling for age. The fact that the job creation effect from loans is stronger in firms growing faster prior to loan receipt can help explain the positive association between the job creation effect from loans and size. Survival effects are strongest in the age-size categories most vulnerable to exit.

An important caveat to bear in mind when interpreting these estimates is the possibility of a contemporaneous shock to demand, costs, or productivity that could raise both the amount (or probability) of an SBA loan and the growth of employment. Our matching can only control for preloan growth and time-invariant characteristics. On the other hand, if the correlations of such shocks with loan probability and employment growth are similar across size and groups, then the relative magnitudes of the loan effects would also be similar.

The rest of the chapter is structured as follows. Section 9.2 describes the SBA programs we analyze. Section 9.3 describes the data, including the matched control samples. Section 9.4 outlines our methodology. Section 9.5 provides estimation results, and section 9.6 concludes.

9.2 SBA Loan Programs

The SBA has several small business loan guarantee programs. In this chapter, we focus on the largest two groups of programs, 7(a) and 504, and this section describes the programs' current characteristics.[3] Small businesses seeking financing apply to private lenders (generally not for SBA

3. SBA (2015) is the primary source for our description, and it contains further details. Brown and Earle (forthcoming) estimate separate job creation effects for 7(a) and 504 loans, finding similar magnitudes. In this chapter we do not distinguish separate effects by loan type.

loans in particular, but for any type of loan). The lenders then decide which applicants are denied, which receive conventional loans, and which of them are both eligible and good candidates for SBA loans. For subprograms where the SBA makes the final credit decision, the lender sends an application for a SBA loan to the SBA on behalf of the applicant, while for other subprograms the lender makes the final credit decision. Not all firms meeting program eligibility requirements receive loans—for example, the lender or the SBA could deny an application based on credit risk just as with conventional loans.

Most 7(a) loans (aside from special subprograms) have a $5 million maximum amount, with an 85 percent maximum SBA guarantee rate for loans up to $150,000 and 75 percent for higher amounts. Loans for working capital and machinery usually have a maturity of up to ten years, while the term for loans for purchase of real estate can be as long as twenty-five years. The SBA sets maximum loan interest rates, which decrease with loan amount and increase with maturity. To qualify, a business must be for-profit; meet SBA size standards;[4] show good character, management expertise, and a feasible business plan; not have funds available from other sources; and be an eligible type of business.[5] 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 final credit decision and most servicing and liquidation authority to PLP lenders, while the SBA's role is to check loan eligibility criteria. The SBA grants lenders PLP status based on their past record with the SBA, including proficiency in processing and servicing SBA-guaranteed loans. The PLP lender agrees to liquidate all business assets before asking the SBA to honor its guaranty in payment default cases. In the 7(a) Certified Lender Program (CLP), the SBA promises a loan decision within three working days on applications handled by CLP lenders. The SBA conducts a credit review, relying on the credit knowledge of the lender's loan officers rather than ordering an independently conducted analysis. Lenders with a good performance history may receive CLP status.

The express loan program is a final large category of 7(a). These have a 50 percent maximum SBA guaranty and a $350,000 maximum loan amount. Interest rates can be higher than on other 7(a) loans, but the SBA promises a decision on approval within thirty-six hours. The PLPs also have an advantage here, as they may make eligibility determinations on their own.

Depending on the type of business, the 504 Loan Program offers loan guarantees up to $5.5 million. Typically a lender covers 50 percent of the

4. The size standards vary by industry, with the criterion sometimes employment, sometimes revenue, and sometimes assets.

5. This includes engaging in business in the United States; possessing reasonable owner equity to invest; and using alternative financial resources, including personal assets, before seeking financial assistance.

project costs, a Certified Development Company (CDC) certified by the SBA provides up to 40 percent of the financing (100 percent guaranteed by an SBA-guaranteed debenture), and the borrower contributes at least 10 percent (the borrower is sometimes required to contribute up to 20 percent). The 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.[6] The 504 loan eligibility requirements are similar to those listed for 7(a) loans above.

Lenders must pay a guaranty fee that increases with maturity and guaranteed amount for 7(a) loans. 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 of why the loan would be unavailable on conventional terms.[7] Both the requirement and the fee 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. The SBA loans tend to be concentrated in a relatively small number of lenders (especially PLP lenders), probably because of scale economies in these costs.[8]

9.3 Data

We identify loan recipients, dates, and amounts with 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. We reset the loan year to be on a fiscal year basis (October of the previous calendar year through September of the current calendar year), using the date the SBA approved the loan so that the loan year is roughly centered on the Census Bureau's LBD (described below) employment measure, which is the number of employees in the pay period including March 12. As shown in table 9.1, loans to firms in US territories are excluded because of uneven coverage of other

6. The SBA loan data for 2006–2009 contain the amount of loan receipts devoted to each category of loan use. The shares of loans going to different uses vary by age and size, but not in a way that can help explain the job creation and survival effect patterns.

7. Examples of acceptable factors are that the business needs a larger loan or longer maturity than the lender's policy permits, or the collateral does not meet the lender's policy requirements.

8. As shown by Brown and Earle (forthcoming), PLP lender branches are not evenly distributed across the country, raising the potential concern that they may locate in areas with higher growth potential. Using a nearly identical sample to this chapter, Brown and Earle (forthcoming) find that the job creation effects of SBA loans are robust to the inclusion of a control for county-industry employment growth over the analyzed period.

Table 9.1 **Path from full SBA loan data set to treated firms in final matched regression sample**

	Number
Total SBA loans in 1991–2009	1,141,200
Except US territories	1,124,900
Except cancelled loans	979,600
After consolidating loans to the same borrower in the same year	947,300
Except loans not matched to any business register	824,200
Except loans matched to nonemployer business register	760,000
Except loans matched to a business register but not matched to the LBD firm data	701,500
Except SBA 7(a)/504 loans in years after the first loan year in the 1991–2009 period	518,200
Except firms with first SBA loan before 1991 or a SBA disaster loan at any time	486,200
Except firms with missing exact matching variables (employment in year before loan receipt, industry, or state [sample for control matching process])	459,600
Except firms without matched controls for three-year employment growth with exit zeroes	310,400
Except firms with greater than 249 employees in $t - 1$ (regression sample for tables 9.16 and 9.17)	309,700
Except firms missing three-year employment growth without exit	222,300
Except firms without matched controls for three-year employment growth without exit (regression sample for tables 9.8 and 9.9)	215,000

Note: Numbers are rounded to the nearest 100 for disclosure avoidance.

data sources. Since cancellations may occur at the initiative of the borrower, canceled loans are excluded. We aggregate loan amounts when borrowers receive multiple SBA loans in the same year.[9] We drop loans received in subsequent years to focus on the effects of the first treatment.

We match the confidential SBA 7(a) and 504 data and publicly available 7(a), 504, and disaster loan data covering loans since the inception of these programs to the Census Bureau's employer and nonemployer business registers.[10] We first link by Employer Identification Numbers (EINs) and Social Security Numbers (SSNs).[11] For confidential 7(a) and 504 records that can-

9. 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 nonguaranteed 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.

10. The SBA has a separate disaster loan program, and we have names and addresses for the recipients from 1953 through March 31, 2011. We have chosen to focus the analysis on 7(a) and 504 loans in the confidential database because the match rate to census data is much higher due to the presence of EINs and SSNs.

11. About three-fourths of the linked records are linked via EIN or SSN.

not 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. Table 9.1 shows that 87 percent of the confidential loan records are linked to a business register. Of these, 7.8 percent are linked only to a nonemployer business register (i.e., they are self-employed and 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 1991. Firms require an industry code, state (for those with nineteen or fewer employees), and employment in 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 32 percent of them. About 87,000 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 employment regression samples. About 7,300 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.[12]

The LBD is built from longitudinally linked employer business registers (Jarmin and Miranda 2002) tracking all firms and establishments with payroll employment in the US nonfarm business sector on an annual basis in 1976–2012. The SBA loan match to employer business registers allows us to link the SBA data to the entire LBD. The LBD contains employment, annual payroll, establishment age (based on the first year the establishment appears in the data set), state, county, ZIP Code, industry code, and firm ID. The industry code is a four-digit Standard Industrial Classification (SIC) code through the year 2001 and a six-digit North American Industry Classification System (NAICS) code in 2002–2012.

We aggregate the LBD to the firm level by assigning each firm the location of its largest establishment by employment and its modal industry code. Following HJM, we set the firm birth year to be 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 aggregates establishment employment in a way that focuses on organic job creation.[13] 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. The employment of the acquired establishments

12. Brown and Earle (forthcoming) provide comparisons between the matched and unmatched samples of firms receiving loans after start-up based on characteristics in the SBA loan recipient data.

13. Our method of calculating organic growth builds on HJM, but is more complicated because HJM consider growth only over one-year periods, while we estimate for several years before and after the loan.

as of the year of the merger is included in the firm's employment in all years prior to any mergers or acquisitions occurring before the base year, as if the establishments were always together. The employment of divested establishments is not included in the firm's employment prior to divestment, as if the establishments were never together, if a divestiture occurs before the base year. If a merger, acquisition, or divestiture occurs after the base year, employment of divested establishments measured in the year prior to divestment is included in all subsequent years, while that of the acquired establishments is not.[14]

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

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

Table 9.2 shows the number of loan recipients in the LBD that fall into each of the sixteen age-size categories. The numbers decline in size for the youngest continuers, while the most numerous size category is five to nineteen employees for the older age groups. The youngest continuers (age one to three) in the largest size (100–249 employees) group are a particularly small cell, suggesting caution in the interpretation of the results for this group.

How does the age-size distribution of recipients compare with nonrecipients? Table 9.3 shows the empirical probability of receiving an SBA loan in

14. For acquisitions prior to the base year and divestitures after the base year, we use a single employment value applied to all preacquisition years and postdivestment years, respectively, to avoid including employment changes occurring under other firms' ownership.

15. Among SBA loan recipients otherwise able to be in the regressions in tables 9.8 and 9.9, 0.3 percent have more than 249 employees in the year prior to loan receipt.

16. Start-up is defined as entry into the LBD, implying positive employment, and therefore employment in start-up firms is by definition zero in the year prior to start-up. We do not divide start-ups into size categories.

Table 9.2 **Number of SBA loan recipients in LBD by age and size**

	Employment in year $t-1$					
Age	1–4	5–19	20–49	50–99	100–249	Total
0						65,600
1–3	55,300	40,800	7,700	1,500	400	107,700
4–10	37,500	52,900	14,400	3,300	1,000	111,800
11+	20,700	40,200	16,600	5,500	2,300	86,700
Total	113,400	133,900	38,800	10,300	3,700	373,500

Notes: This sample includes SBA loan recipients with and without matched controls that either received a loan at start up or had 249 employees or fewer in $t-1$. The numbers are rounded to the nearest 100 for disclosure avoidance. Age is measured in the loan receipt year, and size is employment in the year prior to loan receipt.

Table 9.3 **SBA loan recipients as percent of all LBD firm years in 1991–2009**

	Employment in year $t-1$					
Age	1–4	5–19	20–49	50–99	100–249	Total
0						0.69
1–3	0.44	0.78	0.88	0.69	0.45	0.55
4–10	0.26	0.60	0.82	0.71	0.48	0.42
11+	0.13	0.29	0.42	0.40	0.30	0.23
Total	0.26	0.48	0.59	0.50	0.34	0.40

Notes: This sample includes SBA loan recipients with and without matched controls. The SBA loan recipients in the numerator are counted once. All SBA loan recipient and nonrecipient firm years are included in the denominator. Age is measured in the loan receipt year and size is employment in the year prior to loan receipt.

a particular year. For the sample as a whole the probability is 0.40 percent, and for start-ups the probability is 0.69 percent. Probabilities decline in age overall and for the size categories with up to forty-nine employees, while the age four to ten group has the largest probability for the two largest-size categories. The relationship with size is inverse-U shaped, with the twenty to forty-nine employee category having the highest probabilities. For every age group, the probability of receiving an SBA loan is higher for the 100–249 size group than for the one to four employee group. Thus, from a probability of receipt standpoint SBA loans are, in practice, allocated toward start-ups and younger firms but not toward the smallest size groups among the more mature small- to medium-sized firms that receive the loans. Nonetheless, a substantial share of all SBA loans goes to very small, mature firms.

Brown and Earle (forthcoming) report that SBA recipients' preloan growth rates tend to differ systematically from that of typical firms described

Table 9.4 **Mean employment growth between four years before and one year before loan receipt for SBA loan recipients by age and size**

	Employment in year $t-1$					
Age	1–4	5–19	20–49	50–99	100–249	Total
4–10	0.041	0.336	0.451	0.516	0.564	0.222
11+	–0.072	0.092	0.145	0.174	0.198	0.054
Total	0.003	0.232	0.288	0.305	0.306	0.150

Notes: This sample includes SBA loan recipients with and without matched controls. Growth is calculated using the Davis-Haltiwanger method $\{[2 \times (emp_{t-1} - emp_{t-4})] / (emp_{t-4} + emp_{t-1})\}$. Age is measured in the loan receipt year and size is employment in the year prior to loan receipt.

by Hurst and Pugsley (2011). Eslava and Haltiwanger (2014) show that young Colombian manufacturing firms that are also larger (often meaning they grew faster since birth) experience higher growth rates. One explanation for this pattern is that young firms with drive, managerial talent, and ambition grow faster from birth, and these factors persist in affecting growth rates later on.[17] Table 9.4 tabulates average employment growth rates from four years before the loan to one year prior to the loan for the SBA recipient sample by age-size categories, restricting attention to firms at least four years old. For comparison, table 9.5 contains the analogous computation for all nonrecipients in the LBD. The mean three-year preloan growth rate is higher among SBA recipients than nonrecipients in all age-size groups except very small, mature firms (age eleven or older with one to four employees). Mean three-year growth among SBA recipients is 0.150 compared to 0.019 among nonrecipients. Thus, while these results support Hurst and Pugsley's (2011) findings about the growth of typical small firms, they imply that many SBA firms belong to the atypical subset of small firms (including gazelles) that tend to grow strongly, even prior to loan receipt. Together with the other factors differentiating recipients and nonrecipients, this result highlights the importance of conditioning on prior growth. Below, we outline a matching approach to estimation where comparisons are carried out with controls experiencing similar past growth histories.

The analysis above is conditional on survival. But SBA loan receipt may also affect survival, which we discuss in a separate subsection below.

Finally, all of this analysis so far has implicitly treated SBA loan receipt as a binary treatment. The SBA loan amounts vary substantially, however. Table 9.6 displays mean loan amounts by age-size categories. Loan amounts increase monotonically in both age and size, except that start-ups receive slightly larger loans than one to four employee non-start-ups. While the

17. To the extent that initial success is due to luck or other transient factors, however, there could be reversion to the mean.

Table 9.5 **Mean pretreatment employment growth for all non-SBA LBD firms present in year *t* in 1991–2009 by age and size**

	Employment in year $t-1$					
Age	1–4	5–19	20–49	50–99	100–249	Total
4–10	0.007	0.267	0.342	0.382	0.421	0.066
11+	–0.064	0.072	0.104	0.122	0.135	–0.015
Total	–0.031	0.147	0.176	0.186	0.191	0.019

Note: Growth is calculated using the Davis-Haltiwanger method $\{[2 \times (emp_{t-1} - emp_{t-4})] / (emp_{t-4} + emp_{t-1})\}$. Age is measured in the loan receipt year and size is employment in the year prior to loan receipt.

Table 9.6 **Mean SBA loan size (2010 $US) with and without matched controls**

	Employment in year $t-1$					
Age	1–4	5–19	20–49	50–99	100–249	Total
0						259,362
1–3	220,059	389,783	648,111	895,688	902,995	327,228
4–10	250,075	471,787	800,836	1,008,725	1,116,431	456,831
11+	253,158	505,480	902,777	1,204,657	1,314,167	584,643
Total	236,005	456,907	814,098	1,096,969	1,219,093	445,995

Notes: This sample includes SBA loan recipients with and without matched controls that either received a loan at start up or had 249 employees or fewer in $t-1$. The numbers are rounded to the nearest 100 for disclosure avoidance. Age is measured in the loan receipt year and size is employment in the year prior to loan receipt.

grand mean across all SBA loans is $445,995 (in 2010 USD), the mean amount for the smallest-size group is about half that, and it is three times bigger for the largest-size group. This suggests that the treatments are very different across age-size groups, and the analysis allows for this variation by using loan amount rather than a simple treatment dummy.

9.4 Estimation Strategy

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

For this purpose, we have taken the following steps. As mentioned in section 9.3, we limit our treated sample to firms in the LBD receiving their first SBA 7(a) or 504 loan in 1991–2009 and those not receiving a SBA disaster loan prior to their first 7(a) or 504 loan. To be eligible to be a candidate control firm for a particular treated firm, a firm can never have received an SBA 7(a), 504, or disaster loan at any time between 1953 and 2009; it must be in the same four-digit industry in the treated firm's loan receipt year and be in the same firm-age category (one to two years old, three to five years old, six to ten years old, and eleven years or older) in the treated firm's loan receipt year and in the same employment category (one employee, two to four employees, five to nine employees, ten to nineteen employees, twenty to forty-nine employees, fifty to ninety-nine employees, and one hundred or more employees) in the year prior to loan receipt. For non-start-ups, the control must have nonmissing employment in the year prior to the treated firm's loan receipt. Among firms with nineteen or fewer employees in the previous year and start-ups, we also require the candidate control firm to be located in the same state (firms with one to nineteen employees are much more numerous than ones with more than nineteen employees, so we can afford to impose more restrictions on this group).[18] In addition, for non-start-ups we impose a restriction that the ratio of the treated firm's employment in the previous year to the control firm's previous year employment be greater than 0.9 and less than 1.1. This means that among firms with nine or fewer employees, employment must match exactly.

For the non-start-ups, we would also like to match on variables representing the growth history prior to treatment year, but it is difficult to design matching thresholds for each variable separately, so we reduce this dimensionality problem with propensity score matching. We estimate separate probit regressions using the sample of treated firms and their candidate controls (according to the exact-matching criteria) for different age-size categories (defined in the exact matching above).[19] The probit regresses a dummy for SBA loan receipt on cubic functions of the preloan year logs of employment, revenue, and assets, and their annual growth rates back four years prior to the loan; the log of payroll/number of employees in the preloan year; firm

18. Larger firms may well be in national markets, in which case matching on state would not be appropriate. Matching on geography below the state level even for the smallest firm categories would result in a large number of treated firms being left out of the analysis, potentially biasing the results.

19. Treated firms with no candidate controls are dropped at this point.

age; firm age squared; a multiunit firm dummy; and year dummies. For the lagged employment growth variables, all revenue and assets variables, and for the log of payroll/number of employees in the year prior to the treated firm's loan receipt, we also impute zeroes in place of missing values and include dummies for such cases. Conditioning on four years of lagged employment, revenues, and assets is intended to create a control group with very similar histories to the treated firms.

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

We limit the treated and nontreated firms in the employment and survival regression analysis to those within a common support, meaning that no propensity score of a treated (nontreated) firm that we use is higher than the highest nontreated (treated) firm propensity score, and no propensity score of a treated (nontreated) firm that we use is lower than the lowest nontreated (treated) firm propensity score. A nontreated firm is included as a control for a particular treated firm if the ratio of the treated to the nontreated firm's propensity score is at least 0.9 and not more than 1.1. Treated firms with no controls meeting all these criteria are not included in the employment and survival regression analysis. Nontreated firms appear in the regressions as many times as they have treated firms to which they are matched (i.e., this is matching with replacement). Kernel weights are applied to the controls.[20] In the employment and survival regressions, each control is assigned a final weight of their kernel weight divided by the sum of the kernel weights for all controls for a particular treated firm, and the treated firm is given a weight of 1. As a result, the treated firm and all its control firms together receive equal weight.

Propensity score matching relies on a strong assumption of "selection on observables." Since our data are longitudinal, for the non-start-ups we are also able to eliminate unobserved, time-invariant differences in employment through difference-in-differences (DID) regression specifications. This estimation strategy does not control for possible time-varying unobservables, such as systematically different demand, productivity, or cost shocks received by treated and control firms during the treatment year. Brown and Earle (forthcoming) address this possibility by using an instrumental variable (IV) strategy in addition to the ordinary least squares (OLS) strategy

20. The kernel weight is $1 - \{abs[(\text{propensity score}_{tr} / \text{propensity score}_{ntr}) - 1] / 0.1\}^2$ where tr is a subscript for the treated firm, and ntr is a subscript for the nontreated firm. See Imbens and Wooldridge (2009) for discussion of kernel weighting.

employed here. They estimate slightly stronger employment effects in IV specifications than in OLS specifications like those used here. We do not estimate IV specifications here, because the instrumental variables suffer from weak first-stage power in thin age-size cells.

For firms receiving an SBA loan at start-up (during the first year of positive employment in the LBD), the matching procedures involve exact matching on industry, year, age (start-ups are matched only with start-ups), state, but not propensity score matching. We do not exact match on start-up employment because that is influenced by the treatment. Without propensity score matching on growth history and exact matching on preloan employment level, treated and control start-ups may thus be less closely matched on observables than non-start-ups.

Our analysis focuses on the first SBA loan, as subsequent loan receipt (approximately 20–25 percent of the loan sample is subsequent loans) may be influenced by the outcome of the first one. Also, 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 series for the later ones. The basic form of the regression, therefore, uses the change in employment as the dependent variable as follows:

$$\Delta E_{ijt} = \alpha_j + X_{ijt}\beta + \theta_i\delta + u_{ijt},$$

where ΔE is the change in the number of employees over some 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 the loan years from 1 to T; 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 (only for the specifications used in figure 9.1); u_{ijt} is an idiosyncratic error; θ_i is the amount of the SBA loan (which equals 0 for nontreated firms) received in year t, and δ is the loan effect of interest.

The dependent variable is defined in one specification as change in average employment from three years before to three years after the loan: $\Delta E_{ijt} = E_{ij,\text{post}}\ E_{ij,\text{pre}}$, with $E_{ij,\text{post}} = (E_{ijt+1} + E_{ijt+2} + E_{ijt+3}) / 3$, and $E_{ij,\text{pre\_}t} = (E_{ijt-1} + E_{ijt-2} + E_{ijt-3})/3$.[21] In survival regressions the dependent variable is a dummy for survival through a particular year after the treated firm's loan receipt.

The reliability of propensity score matching depends on whether, conditional on the propensity score, the potential outcomes y^1 and y^0 are independent of treatment incidence. The assumption of independence conditional on observables depends on the pretreatment variables being balanced between the treated and control groups. We assess this in two ways—by

21. In cases of missing values in years prior to loan receipt, we average employment during the available years $t - 3$, $t - 2$, and $t - 1$.

Table 9.7 **Bias before and after propensity score matching**

	All nontreated	All treated	Control sample	Treated sample	Final standardized difference	Bias reduction (%)
Log emp_{t-1}	1.761	1.902	1.999	1.997	–0.143	98.69
Log (emp_{t-1} / emp_{t-2})	0.017	0.071	0.073	0.075	0.565	95.48
Log (emp_{t-2} / emp_{t-3})	0.024	0.072	0.070	0.073	0.735	93.57
Log (emp_{t-3} / emp_{t-4})	0.027	0.070	0.067	0.070	0.660	93.75
Log rev_{t-1}	6.396	6.611	6.710	6.691	–1.278	91.19
Log (rev_{t-1} / rev_{t-2})	0.031	0.091	0.093	0.111	3.892	70.13
Log (rev_{t-2} / rev_{t-3})	0.041	0.095	0.099	0.109	2.308	81.48
Log (rev_{t-3} / rev_{t-4})	0.052	0.098	0.094	0.102	2.076	82.03
Log $assets_{t-1}$	4.867	5.107	5.220	5.100	–6.533	50.22
Log ($assets_{t-1}$ / $assets_{t-2}$)	0.017	0.116	0.096	0.121	4.040	74.21
Log ($assets_{t-2}$ / $assets_{t-3}$)	0.030	0.073	0.077	0.088	1.803	74.87
Log ($assets_{t-3}$ / $assets_{t-4}$)	0.038	0.076	0.074	0.084	1.784	72.62
Log wage	3.042	3.048	3.079	3.088	1.074	–42.39
Age	10.706	8.324	8.863	8.773	–1.080	96.21
Multiunit	0.044	0.051	0.050	0.050	–0.381	88.90

Notes: For a given variable, say age, the standardized difference (percent bias) is $SDIFF(\text{age}) = \{100(1\ /\ N)\ \sum_{i\in A}[\text{age}_i - \sum_{j\in C} g(p_i, p_j)\text{age}_j]\}\ /\ \sqrt{[Var_{i\in A}(\text{age}) + Var_{j\in C}(\text{age})]\ /\ 2}$. The group before propensity score matching is treated and control firms satisfying exact matches on employment, treatment year age category, industry, year, and state (if it has nineteen or fewer employees in the prior year). Firms in the group after propensity score matching satisfy the propensity score bandwidth criterion, the common support criterion, and are in the regression samples for tables 9.8 and 9.9. The samples do not include firms receiving loans at start-up or their controls.

performing a standardized difference (or bias) test for the main variables included in the matching probit regressions, and by analyzing the pretreatment event-time dynamics. Table 9.7 reports the means of the main variables included in the matching probit regressions for four different samples: all treated firms, all nontreated firms, treated firms included in the employment regressions in tables 9.8 and 9.9, and controls included in those employment regressions. Treated firm employment is larger and age is younger than for nontreated firms prior to matching, and treated firms experience more employment growth in the four years prior to treatment. After matching, these differences are negligible. The standardized difference measures confirm this: employment, employment growth, and age biases are reduced by over 93 percent.[22] None of the biases are close to being large after matching.[23] Appendix table 9A.1 shows the means and percent bias after matching by age-size categories. Though none of the biases are large, the biggest ones are for larger young firms (the age one to three categories with fifty or more employees), which are the groups with the smallest loan recipient counts.

22. The mean age is very similar in the total treated and total nontreated samples, leaving little scope for improvement through matching.

23. Rosenbaum and Rubin (1985) consider a value of 20 to be large.

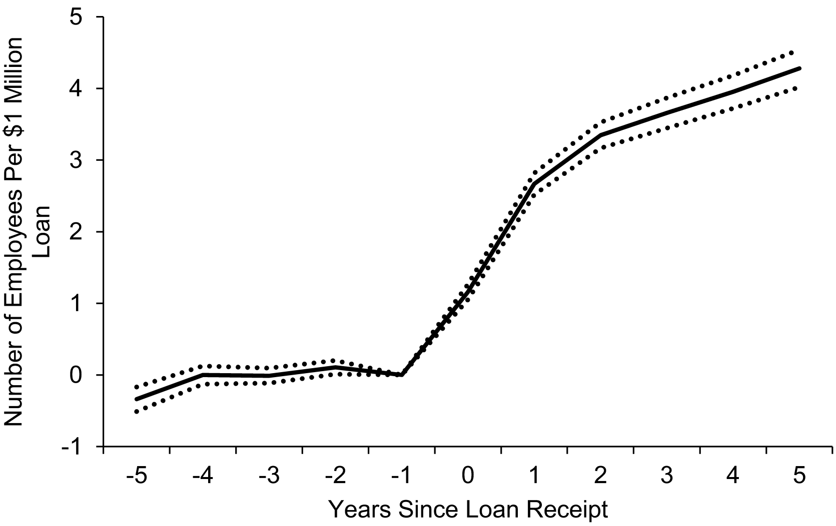

Fig. 9.1 Dynamics for number of employees per $1 million loan, firms receiving loans after start-up

Notes: These are loan amount coefficients from kernel-weighted OLS regressions with a dependent variable of the firm's employment in the respective year minus employment in year $t - 1$. Loan amount is in millions of 2010 dollars. Treatment year age, age squared, and treated firm-control fixed effects are included in the regressions. 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 pretreatment 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 firm.

These tests suggest the matching has achieved reasonable balance within each age-size category; treated firms are matched with controls that have had similar growth in the past.

The second test for how effectively the matching process has achieved balance between the treated and control groups uses estimates of the dynamic effects of SBA loan receipt on employment in our sample of non-start-up firms as a "preprogram test" in the sense of Heckman and Hotz 1989, or a "pseudo-outcome" test in the sense of Imbens and Wooldridge (2009). We define t as the loan year, and use the year prior to the loan, $t - 1$, as the base year, computing employment differences for each year from five before to five after the loan, so that $\Delta E_{ijt} = E_{ijt+s} - E_{ijt-1}(s = -5, -4, \ldots 4, 5)$. Figure 9.1 shows that pretreatment growth differences between future treated and control firms are negligible, so the matching appears to be effective at eliminating growth differences prior to $t - 1$. Non-start-up SBA loan recipient employment grows significantly more than that of controls starting in the loan year, and the gap steadily grows to 4.3 extra jobs per $1 million loan by five years after loan receipt.

Table 9.8 **Size effects on employment growth per $1 million loan by size categories, age categories**

	Loan amount coefficient	Number of observations	Number of treated firms
Emp. 1–4	2.434 (0.061)	1,284,000	59,700
Emp. 5–19	2.173 (0.043)	737,700	67,800
Emp. 20–49	3.115 (0.066)	2,517,600	28,400
Emp. 50–99	3.946 (0.206)	286,800	6,900
Emp. 100–249	5.873 (0.635)	56,900	2,400
Age 0	5.336 (0.195)	7,556,800	49,800
Age 1–3	3.130 (0.128)	1,282,100	53,100
Age 4–10	2.960 (0.100)	1,108,400	57,400
Age 11+	3.015 (0.081)	2,492,400	54,700

Notes: These are kernel-weighted OLS regressions, run separately for each size and age category. The dependent variable is average employment in $t + 1$ through $t + 3$ minus average employment in $t - 3$ through $t - 1$, including only firms that have positive employment in each of those years. Loan amount is in millions of 2010 dollars. Treated firm-control fixed effects are also included in the regressions. Standard errors, cluster-adjusted by firm, are in parentheses. The number of observations and SBA firms are rounded to the nearest 100 for disclosure avoidance.

9.5 Results

9.5.1 Employment Growth Estimates

We present estimates considering heterogeneity separately by size and age groups (table 9.8), followed by effects across age-size groups (table 9.9 and figures 9.2 and 9.3), to see how the effects differ with and without age-size interactions. For a decisionmaker interested in allocating loans to maximize the impact, an important question concerns the observability of variables used in targeting. Decisionmakers may have more reliable information on firm size than age (because age is more easily manipulated), so it is useful to know whether conditioning loans only on size reduces the efficiency of loan allocation.[24] The question is similar to HJM's analysis of the size-growth

24. Observed firm age might be easily manipulated, for example, by renaming and reregistering what is essentially the same company. Firm size might be manipulated through hiring decisions on the margin, but large changes in firm size are more difficult, although splitting up a large firm to make it eligible for small business preferences is hardly unheard of. If age

Table 9.9 **Employment growth regressions by age-size categories**

	Loan amount coefficient	Number of observations	Number of treated firms
Age 0	5.336 (0.195)	7,556,800	49,800
Age 1–3, emp. 1–4	3.356 (0.113)	622,500	28,500
Age 1–3, emp. 5–19	2.606 (0.130)	163,800	18,500
Age 1–3, emp. 20–49	3.738 (0.280)	467,400	5,000
Age 1–3, emp. 50–99	2.127 (0.876)	26,600	800
Age 1–3, emp. 100–249	8.712 (4.739)	1,800	200
Age 4–10, emp. 1–4	1.783 (0.067)	313,800	19,500
Age 4–10, emp. 5–19	2.244 (0.069)	201,100	25,100
Age 4–10, emp. 20–49	3.260 (0.119)	547,600	10,300
Age 4–10, emp. 50–99	4.587 (0.539)	41,000	1,900
Age 4–10, emp. 100–249	6.932 (2.131)	4,700	500
Age 11+, emp. 1–4	1.644 (0.120)	347,700	11,700
Age 11+, emp. 5–19	1.879 (0.047)	372,700	24,200
Age 11+, emp. 20–49	2.871 (0.072)	1,502,400	13,100
Age 11+, emp. 50–99	3.969 (0.217)	219,200	4,200
Age 11+, emp. 100–249	5.504 (0.625)	50,400	1,700

Notes: These are kernel-weighted OLS regressions, run separately for each age-size category. The dependent variable is average employment in $t + 1$ through $t + 3$ minus average employment in $t - 3$ through $t - 1$, including only firms that have positive employment in each of those years. Loan amount is in millions of 2010 dollars. Treated firm-control fixed effects are also included in the regressions. Standard errors, cluster-adjusted by firm, are in parentheses. The number of observations and SBA firms are rounded to the nearest 100 for disclosure avoidance.

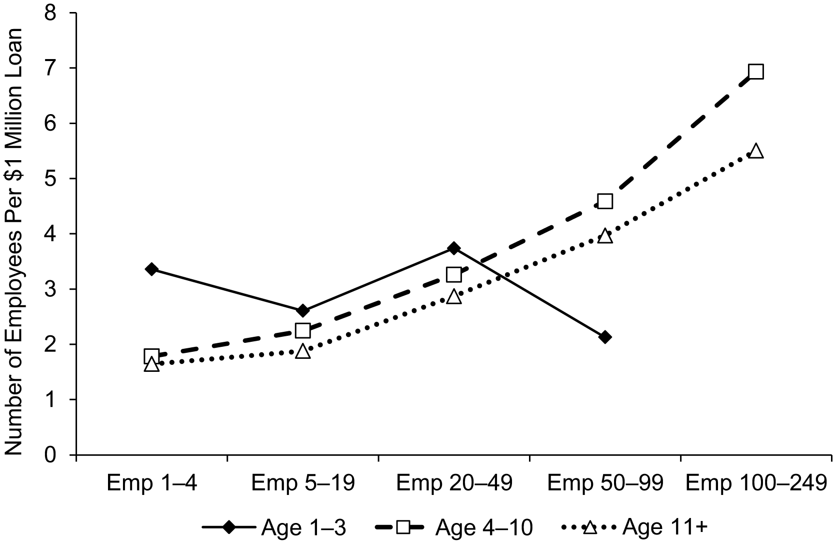

Fig. 9.2 Size effects on employment growth per $1 million loan for each age category
Note: These are plots of non-start-up firm loan amount coefficients reported in table 9.9, including only coefficients significant at the 5 percent level.

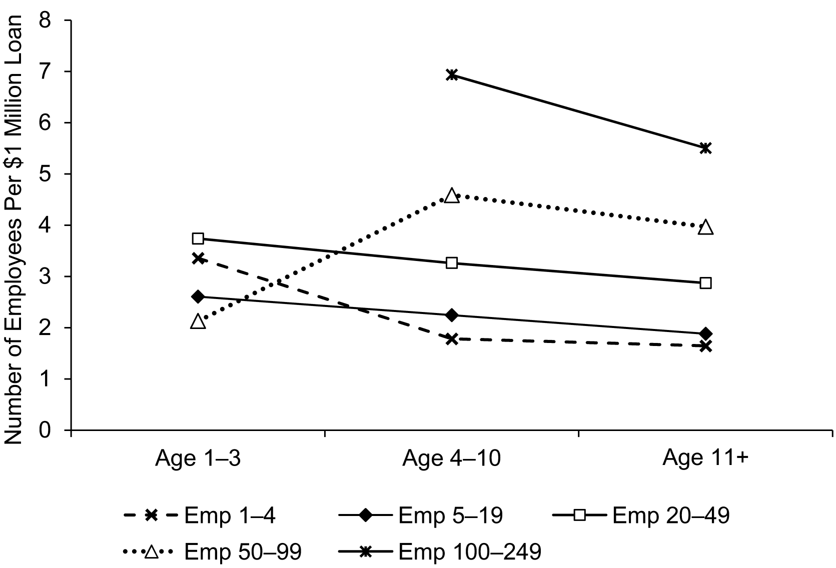

Fig. 9.3 Age effects on employment growth per $1 million loan for each size category
Note: These are plots of non-start-up firm loan amount coefficients reported in table 9.9, including only coefficients significant at the 5 percent level.

relationship with and without age controls; HJM report that controlling for age essentially eliminates the negative size-growth relationship found without age controls, and we can carry out a similar analysis for the effects of SBA loans.

Table 9.8 shows that the employment effects of the loans generally rise in size, varying from 2.2 jobs per $1 million loan for the five to nineteen employment category to 5.9 jobs for the 100–249 category. This pattern holds despite the likelihood that control firms in the larger size categories have access to more conventional financing than smaller controls. Start-up effects (5.3 jobs) are much larger than those in the other age categories (roughly flat at three jobs each).

The size analysis for non-start-ups with and without age controls in figure 9.4 shows that the size effects are virtually identical either way. Comparing the results in table 9.9 with the average preloan growth rates in table 9.4, we find a strong positive association between average past growth in the category and average job creation effects of SBA loans. The results in table 9.9 suggest that smaller, older firms do grow after loan receipt, but much less than other groups. Firms in the smaller, older age-size categories have a much lower propensity to receive SBA loans than other age-size categories (table 9.2),[25] but they still represent a significant fraction of total SBA loans (the three categories with fewer than two created jobs per $1 million loan represent 26 percent of all SBA loans).

Firms with a history of growth have both demonstrated the ability to grow and may be more likely to want to expand further in the future, which could explain their larger SBA loan effects. This is despite the possibility that growing firms have greater access to conventional financing, which should attenuate the effect of SBA-backed loans.[26]

The results above focus on organic growth. Appendix tables 9A.2 and 9A.3 show results that include firm boundary changes as employment changes. Boundary changes are most frequent in larger, more mature firms, especially those with 100–249 employees and that are age eleven years or older. The employment effects are a bit higher for firms with 100–249 employees than the effects solely using organic employment growth, suggesting larger treated firms are expanding more via acquisition than their matched controls.

is therefore less easily or reliably observed than size, then an important question is whether using information on size alone is at all useful, or if age is a crucial piece of information for targeting types of firms.

25. This could reflect either less need/desire for a loan or lower-quality loan applications.

26. The control firms in the age-size categories with higher past growth rates are more likely to receive conventional financing than controls in other categories, dampening the treatment effects if finance facilitates growth.

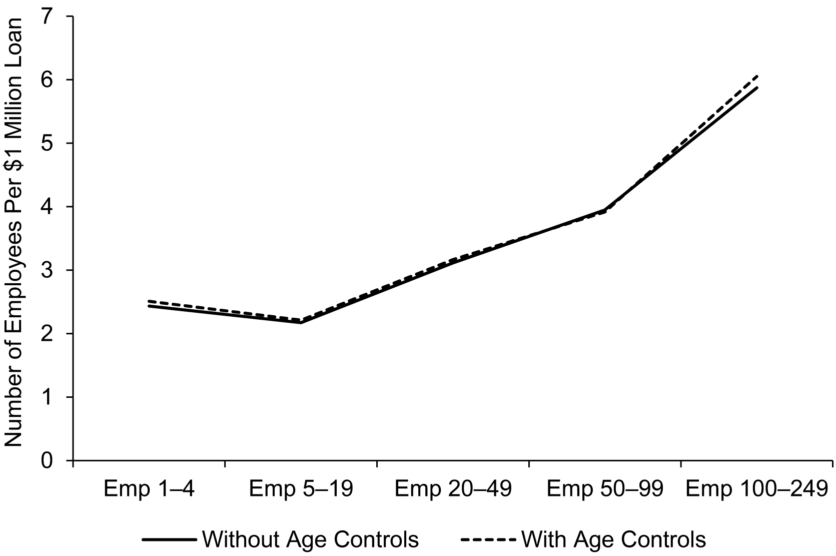

Fig. 9.4 Size effects on employment growth per $1 million loan, with and without age controls

Notes: These are plots of loan amount coefficients for non-start-up firms reported in tables 9.8 and 9.9. The numbers with age controls are averages of the coefficients for the size category across age categories, weighted by the number of treated firms in each cell.

9.5.2 Survival Estimates

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 overextension could create greater vulnerability. Nor is the measurement of survival unambiguous, and any disappearance from the database is classified as an exit. Though great effort has been made to link establishments across time in the LBD, we cannot always distinguish bankruptcy and other genuine shutdowns from buy-outs or reorganizations that lead to a change in the identifying code in the LBD. As some of these outcomes represent business failure, others reflect success, and some level of exit is a normal feature of a dynamic economy, the analysis of exit is thus also not as clear normatively as our analysis of employment effects.

With these qualifications in mind, we are nonetheless interested to ascertain how SBA loan receipt affects firm survival. In this section we estimate these effects using linear probability model (LPM) regressions for shorter-

(three-year) and longer-run (ten-year) survival. Again we examine the heterogeneity by age-size categories. Other than the dependent variable, the regressions are identical to the employment regressions in the previous section.

We include only firm exits occurring within the examined time period that have no surviving establishments (establishment sales to other firms) postexit. Firms that exit via sale of their establishments are ambiguous from a performance perspective—some may be cases where the entrepreneur is cashing in on a successful venture.

To provide a baseline for the estimated effects, the three- and ten-year survival rates in the regression sample for each age-size category are reported in tables 9.10 and 9.11. Average survival rates generally increase with both age and size, but for within age groups there is little difference in survival for size groups of five employees and greater.

Tables 9.12 and 9.13 and figures 9.5, 9.6, and 9.7 display the three-year survival regression results. The effects are sharply declining in age, ranging from a 14 percent higher propensity to survive per $1 million loan for start-ups to –1.2 percent for mature firms, and this pattern holds across all size classes (figure 9.6). The effects for non-start-ups vary little across employment categories, with the exception of a higher effect for the smallest firms (one to four employees), and this pattern holds with or without age controls (figure 9.7). The firms least likely to want to grow (small, mature firms) actually exhibit negative survival effects from loan receipt.

The ten-year survival effect (tables 9.14 and 9.15 and figures 9.8, 9.9, and 9.10) is much stronger for firms with nineteen or fewer employees than the three-year effect, while it is very similar for larger firms, resulting in a sharper decline of the effect with size. Loans have a larger effect on shorter-run than longer-run survival for start-up firms, suggesting that the loans are particularly beneficial while they are in the "valley of death." The effects are higher over the longer period for the other age categories, though. Across age-size

Table 9.10 **Three-year survival rates in survival regression samples (percent)**

	Employment in year prior to SBA loan					
Age	1 to 4	5 to 19	20 to 49	50 to 99	100 to 249	Total
0						62.27
1–3	68.17	72.50	71.80	71.88	71.00	70.00
4–10	74.57	80.31	80.86	79.76	79.47	78.26
11+	76.78	83.60	85.18	85.57	86.51	82.54
Total	71.76	79.05	80.95	81.97	83.44	72.90

Note: These numbers are calculated from the full survival regression samples for loans issued in 1991–2009, including both treated and control firms.

Table 9.11 **Ten-year survival rates in survival regression samples**

	Employment in year prior to SBA loan					
Age	1 to 4	5 to 19	20 to 49	50 to 99	100 to 249	Total
0						30.79
1–3	36.31	40.85	39.31	37.33	34.79	38.28
4–10	43.56	49.78	49.92	49.02	46.80	47.80
11+	45.83	56.13	58.47	57.82	58.53	54.99
Total	40.46	49.37	51.83	52.76	54.20	43.13

Note: These numbers are calculated from the full survival regression samples for loans issued in 1991–2002, including both treated and control firms.

Table 9.12 **Three-year survival regressions by size categories, age categories**

	Loan amount coefficient	Number of observations	Number of treated firms
Emp. 1–4	0.026	2,832,100	95,100
	(0.002)		
Emp. 5–19	0.007	1,220,600	94,900
	(0.001)		
Emp. 20–49	0.010	4,221,500	37,000
	(0.001)		
Emp. 50–99	0.012	423,400	8,900
	(0.002)		
Emp. 100–249	0.012	77,200	3,100
	(0.003)		
Age 0	0.140	24,686,200	83,400
	(0.002)		
Age 1–3	0.055	3,057,100	85,100
	(0.002)		
Age 4–10	0.020	1,953,200	81,700
	(0.001)		
Age 11+	–0.0124	3,764,600	72,200
	(0.0009)		

Notes: These are kernel-weighted OLS regressions, run separately for each size and age category. The dependent variable is a dummy for survival through $t + 3$. Loan amount is in millions of 2010 dollars. Treated firm-control fixed effects are also included in the regressions. Standard errors, cluster-adjusted by firm, are in parentheses. The number of observations and SBA firms are rounded to the nearest 100 for disclosure avoidance.

Table 9.13 **Three-year survival regressions by age-size categories**

	Loan amount coefficient	Number of observations	Number of treated firms
Age 0	0.140 (0.002)	24,686,200	83,400
Age 1–3, emp. 1–4	0.067 (0.004)	1,638,900	47,700
Age 1–3, emp. 5–19	0.052 (0.003)	350,000	28,600
Age 1–3, emp. 20–49	0.042 (0.004)	1,012,900	7,300
Age 1–3, emp. 50–99	0.069 (0.007)	51,600	1,200
Age 1–3, emp. 100–249	0.036 (0.022)	3,800	300
Age 4–10, emp. 1–4	0.017 (0.004)	608,900	30,100
Age 4–10, emp. 5–19	0.013 (0.002)	331,700	34,800
Age 4–10, emp. 20–49	0.025 (0.002)	939,300	13,400
Age 4–10, emp. 50–99	0.026 (0.005)	65,700	2,600
Age 4–10, emp. 100–249	0.023 (0.009)	7,600	700
Age 11+, emp. 1–4	–0.044 (0.005)	584,300	17,300
Age 11+, emp. 5–19	–0.026 (0.002)	538,900	31,500
Age 11+, emp. 20–49	–0.010 (0.001)	2,269,300	16,200
Age 11+, emp. 50–99	–0.0003 (0.0020)	306,200	5,100
Age 11+, emp. 100–249	0.0078 (0.0032)	65,800	2,100

Notes: These are kernel-weighted OLS regressions, run separately for each age-size category. The dependent variable is a dummy for survival through $t + 3$. Loan amount is in millions of 2010 dollars. Treated firm-control fixed effects are also included in the regressions. Standard errors, cluster-adjusted by firm, are in parentheses. The number of observations and SBA firms are rounded to the nearest 100 for disclosure avoidance.

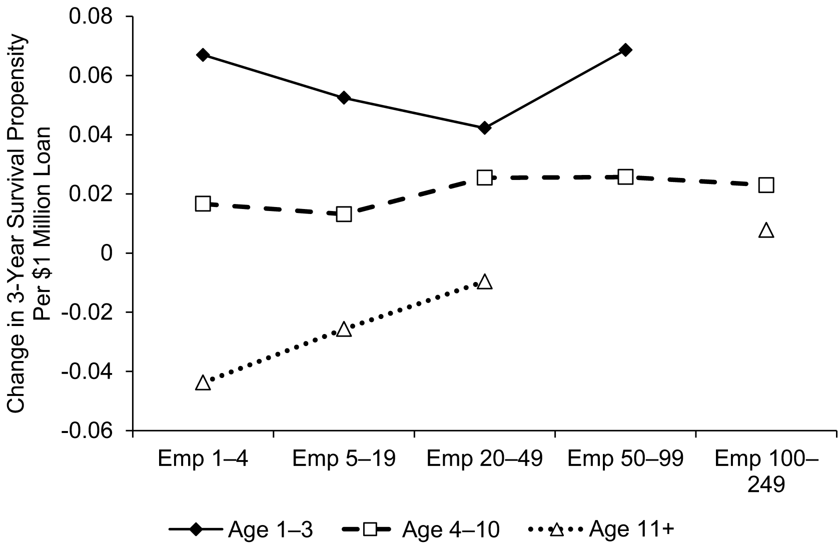

Fig. 9.5 Size effects on three-year survival propensity per $1 million loan for each age category

Note: These are plots of non-start-up firm results reported in table 9.13, including only coefficients significant at the 5 percent level.

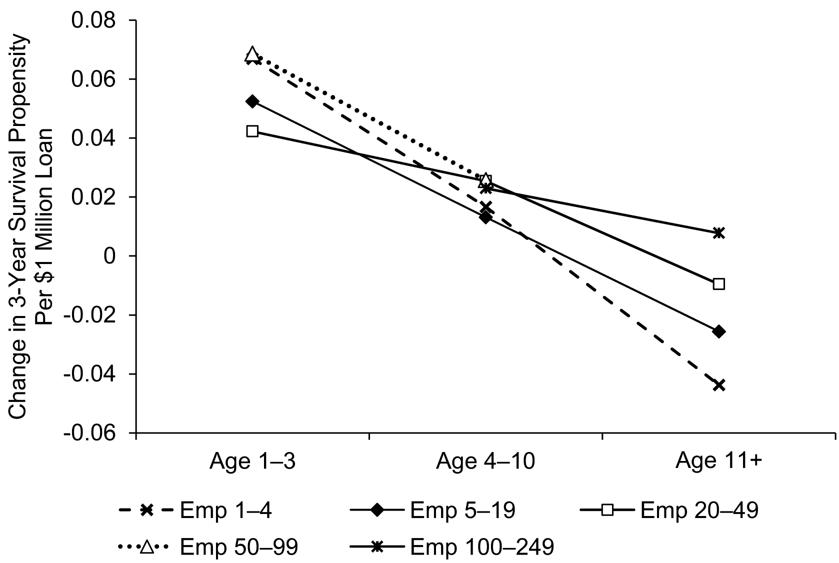

Fig. 9.6 Age effects on three-year survival propensity per $1 million loan for each size category

Note: These are plots of non-start-up firm results reported in table 9.13, including only coefficients significant at the 5 percent level.

Fig. 9.7 Size effects on three-year survival propensity per $1 million loan, with and without age controls

Note: These are plots of results for non-start-up firms reported in tables 9.12 and 9.13. The results with age controls are averages of the coefficients for the size category across age categories, weighted by the number of treated firms in each cell.

groups, the estimated survival effects are strongly negatively correlated with average survival rates in the corresponding subpopulations, suggesting SBA loans have the greatest survival benefits for firms that are particularly vulnerable to exit.

9.5.3 Employment Growth Estimates Incorporating Exit

If we assume exit represents job loss, the significant survival effects from SBA loan receipt suggest the employment growth analysis focusing on surviving firms in section 9.5.1 may be biased. We investigate this by imputing zero values for employment following exit and reestimating.[27] The patterns are somewhat sensitive to whether exit is taken into account: the estimates incorporating exit, shown in tables 9.16 and 9.17 and figures 9.11, 9.12, and 9.13, result in a stronger positive association between size and the employment effect, and the effect now declines with age: relative to their matched controls, fewer treated firms going through the “valley of death” destroy jobs via exiting.

27. Exit effects could be incorporated in many ways, as it is not conceptually clear how many years of zeros to impute postexit. These results can thus be viewed as giving an indication of the direction the exit effect exerts on employment estimates rather than some exact magnitude.

Table 9.14 **Ten-year survival regressions by size categories, age categories**

	Loan amount coefficient	Number of observations	Number of treated firms
Emp. 1–4	0.072	1,105,000	40,000
	(0.004)		
Emp. 5–19	0.035	659,900	48,400
	(0.003)		
Emp. 20–49	0.011	2,697,500	22,400
	(0.002)		
Emp. 50–99	0.012	286,500	5,600
	(0.004)		
Emp. 100–249	0.013	43,000	1,800
	(0.006)		
Age 0	0.117	8,729,600	37,700
	(0.003)		
Age 1–3	0.079	1,405,200	38,000
	(0.004)		
Age 4–10	0.046	1,197,300	42,100
	(0.003)		
Age 11+	–0.008	2,189,400	38,200
	(0.002)		

Notes: These are kernel-weighted OLS regressions, run separately for each size and age category. The dependent variable is a dummy for survival through $t + 10$. Loan amount is in millions of 2010 dollars. Treated firm-control fixed effects are also included in the regressions. Standard errors, cluster-adjusted by firm, are in parentheses. The number of observations and SBA firms are rounded to the nearest 100 for disclosure avoidance.

These patterns are again highly correlated with mean past growth rates in the age-size groups, consistent with the idea that firms demonstrating past growth are more likely to want to grow in the future and thus to use the loan for expansion.

9.5.4 Variation by Pretreatment Growth

The fact that the job creation loan effects are larger by size categories suggests there may be an association between pretreatment growth and the employment response to loan receipt, since there is a mechanical relationship between past growth and size. If so, then past employment growth could be used to inform loan allocation. A similar argument applies to survival: if past growth reflects not only a demonstrated desire to grow in the future, but also firm quality, then it should be associated with survival. We may test this directly by interacting pretreatment employment growth and loan amount in regressions by age categories (except start-ups).[28]

28. For firms with positive employment in both $t - 4$ and $t - 1$, pretreatment growth is the difference between employment in $t - 1$ and $t - 4$, divided by three so as to annualize it. If a firm does not have employment in $t - 4$, but has it in $t - 3$, pretreatment growth is the difference between employment in $t - 1$ and $t - 3$, divided by two. Firms without positive employment in $t - 4$

Table 9.15 **Ten-year survival regressions by age-size categories**

	Loan amount coefficient	Number of observations	Number of treated firms
Age 0	0.117	8,729,600	37,700
	(0.003)		
Age 1–3, emp. 1–4	0.094	563,600	19,400
	(0.007)		
Age 1–3, emp. 5–19	0.081	171,900	13,700
	(0.006)		
Age 1–3, emp. 20–49	0.065	635,500	4,100
	(0.006)		
Age 1–3, emp. 50–99	0.089	32,200	700
	(0.014)		
Age 1–3, emp. 100–249	0.002	2,000	100
	(0.034)		
Age 4–10, emp. 1–4	0.066	287,800	13,200
	(0.008)		
Age 4–10, emp. 5–19	0.044	197,100	18,600
	(0.005)		
Age 4–10, emp. 20–49	0.036	661,400	8,200
	(0.004)		
Age 4–10, emp. 50–99	0.057	46,100	1,600
	(0.008)		
Age 4–10, emp. 100–249	0.069	5,000	400
	(0.018)		
Age 11+, emp. 1–4	0.039	253,600	7,400
	(0.009)		
Age 11+, emp. 5–19	0.00002	290,900	16,200
	(0.00444)		
Age 11+, emp. 20–49	–0.020	1,400,700	10,100
	(0.003)		
Age 11+, emp. 50–99	–0.014	208,200	3,300
	(0.004)		
Age 11+, emp. 100–249	0.002	36,000	1,300
	(0.007)		

Notes: These are kernel-weighted OLS regressions, run separately for each age-size category. The dependent variable is a dummy for survival through $t + 10$. Loan amount is in millions of 2010 dollars. Treated firm-control fixed effects are also included in the regressions. Standard errors, cluster-adjusted by firm, are in parentheses. The number of observations and SBA firms are rounded to the nearest 100 for disclosure avoidance.

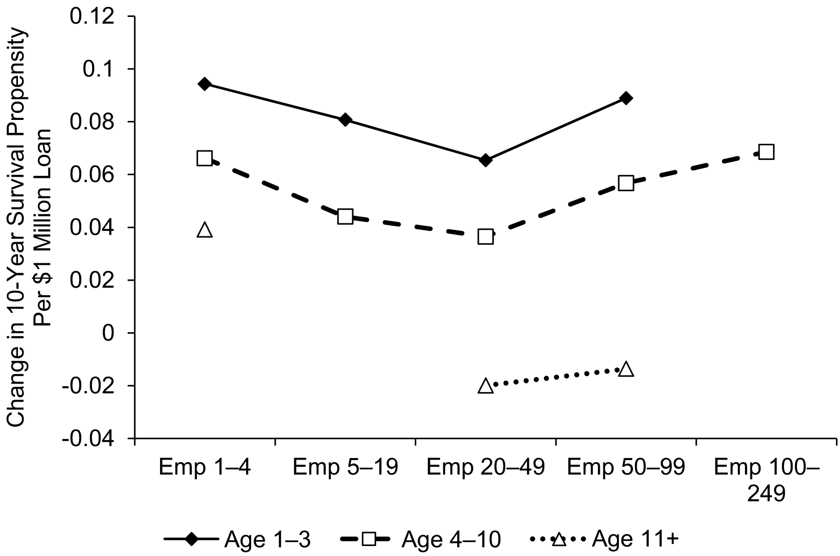

Fig. 9.8 Size effects on ten-year survival propensity per $1 million loan for each age category

Note: These are plots of non-start-up firm results reported in table 9.15, including only coefficients significant at the 5 percent level.

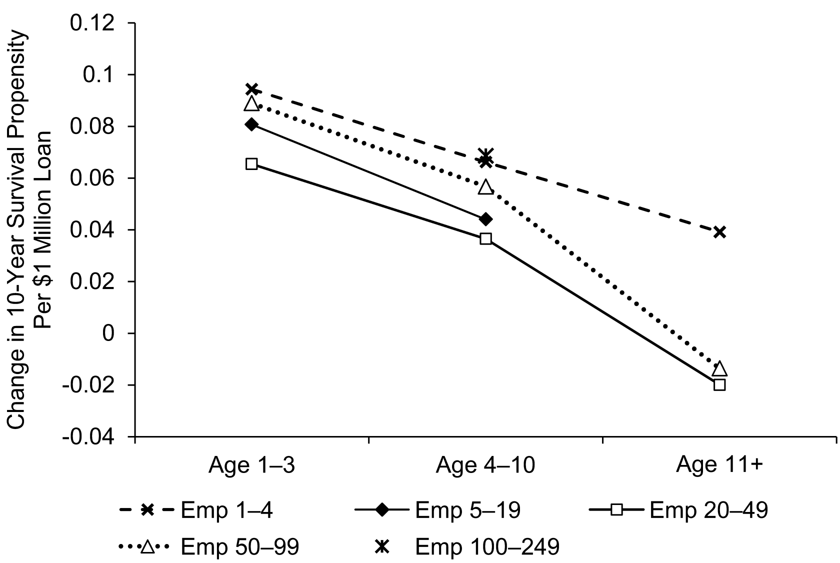

Fig. 9.9 Age effects on ten-year survival propensity per $1 million loan for each size category

Note: These are plots of non-start-up firm results reported in table 9.15, including only coefficients significant at the 5 percent level.

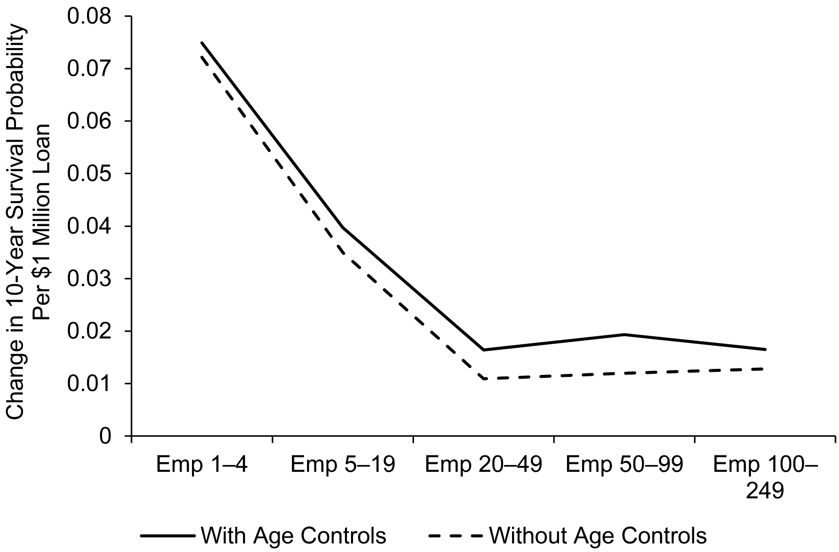

Fig. 9.10 Size effects on ten-year survival propensity per $1 million loan, with and without age controls

Notes: These are plots of results for non-start-up firms reported in tables 9.14 and 9.15. The results with age controls are averages of the coefficients for the size category across age categories, weighted by the number of treated firms in each cell.

Table 9.18 shows that the estimated relationship between pretreatment growth and the loan amount impact is positive for all three age groups, although it is statistically significant only for firms in the two older age categories, when exiting firms are excluded. When exit zeroes are incorporated, all three categories have positive associations and are statistically significant. The estimates suggest job creation per $1 million loan increases by about 0.1 for each additional employee added in the three years prior to the loan. The effects on survival are weaker, and the effect on longer-run survival is statistically insignificant for all three age categories. These results are consistent with pretreatment growth partly reflecting firm quality and partly a desire to expand further in the future.

9.6 Conclusion

Research on measures to support small businesses has been preoccupied with examining the basic proposition that small firms are disproportionate

and $t-3$, but with it in $t-2$, have pretreatment growth of the difference between employment in $t-1$ and $t-2$, and those only with positive employment in $t-1$ have pretreatment growth of employment in $t-1$ (since employment prior to that is assumed to be zero).

Table 9.16 **Employment growth regressions by size categories, age categories, accounting for exit**

	Loan amount coefficient	Number of observations	Number of treated firms
Emp. 1–4	2.094	2,633,400	90,700
	(0.050)		
Emp. 5–19	1.892	1,176,000	92,000
	(0.038)		
Emp. 20–49	2.995	4,036,100	35,800
	(0.067)		
Emp. 50–99	4.137	409,100	8,600
	(0.214)		
Emp. 100–249	6.947	73,800	2,900
	(0.702)		
Age 0	6.278	22,887,400	79,600
	(0.122)		
Age 1–3	3.537	2,860,300	81,500
	(0.113)		
Age 4–10	2.995	1,850,000	78,500
	(0.104)		
Age 11+	2.585	3,618,100	70,100
	(0.085)		

Notes: These are kernel-weighted OLS regressions, run separately for each size and age category. The dependent variable is average employment in $t + 1$ through $t + 3$ minus average employment in $t - 3$ through $t - 1$, including zeros for employment in years after exit. Loan amount is in millions of 2010 dollars. Treated firm-control fixed effects are also included in the regressions. Standard errors, cluster-adjusted by firm, are in parentheses. The number of observations and SBA firms are rounded to the nearest 100 for disclosure avoidance.

job creators. Although the proposition is practically an article of faith for many, HJM have recently shown that firm size and growth are essentially uncorrelated once the analysis accounts for firm age, and systematically larger job creation only comes from new entrants and very young firms. Whatever the nature of the firm age-size-growth relationships, however, the existing research does not address the question of whether and how job creation and survival per dollar of financing backed by the government varies across firms by size and age.

Our analysis matches firms with fewer than 250 employees receiving loans in the two largest loan guarantee programs of the Small Business Administration (the 7[a] and 504 programs) to nonrecipients that are essentially identical along every observable: preloan size, age, industry, year, and preloan growth history. For the results to be interpreted as causal, one must assume that there are no systematic time-varying differences between the loan recipients and control firms, such as differential demand, productivity, or cost shocks at the time of loan receipt. This is a potentially important caveat, as it is not difficult to imagine such shocks as the very motivation for

Table 9.17 **Employment growth regressions by age-size categories, accounting for exit**

	Loan amount coefficient	Number of observations	Number of treated firms
Age 0	6.278	22,887,400	79,600
	(0.122)		
Age 1–3, emp. 1–4	2.984	1,510,600	45,400
	(0.087)		
Age 1–3, emp. 5–19	2.643	334,400	27,600
	(0.368)		
Age 1–3, emp. 20–49	4.369	962,900	7,000
	(0.246)		
Age 1–3, emp. 50–99	5.362	48,800	1,200
	(0.907)		
Age 1–3, emp. 100–249	16.587	3,600	300
	(3.777)		
Age 4–10, emp. 1–4	1.554	567,100	28,700
	(0.058)		
Age 4–10, emp. 5–19	2.035	318,500	33,700
	(0.061)		
Age 4–10, emp. 20–49	3.400	894,600	13,000
	(0.116)		
Age 4–10, emp. 50–99	5.403	62,700	2,500
	(0.513)		
Age 4–10, emp. 100–249	7.488	7,200	700
	(2.329)		
Age 11+, emp. 1–4	1.143	555,700	16,700
	(0.115)		
Age 11+, emp. 5–19	1.301	523,100	30,700
	(0.046)		
Age 11+, emp. 20–49	2.341	2,178,600	15,800
	(0.081)		
Age 11+, emp. 50–99	3.516	297,700	4,900
	(0.232)		
Age 11+, emp. 100–249	6.232	63,100	2,000
	(0.678)		

Notes: These are kernel-weighted OLS regressions, run separately for each age-size category. The dependent variable is average employment in $t + 1$ through $t + 3$ minus average employment in $t - 3$ through $t - 1$, including zeros for employment in years after exit. Loan amount is in millions of 2010 dollars. Treated firm-control fixed effects are also included in the regressions. Standard errors, cluster-adjusted by firm, are in parentheses. The number of observations and SBA firms are rounded to the nearest 100 for disclosure avoidance.

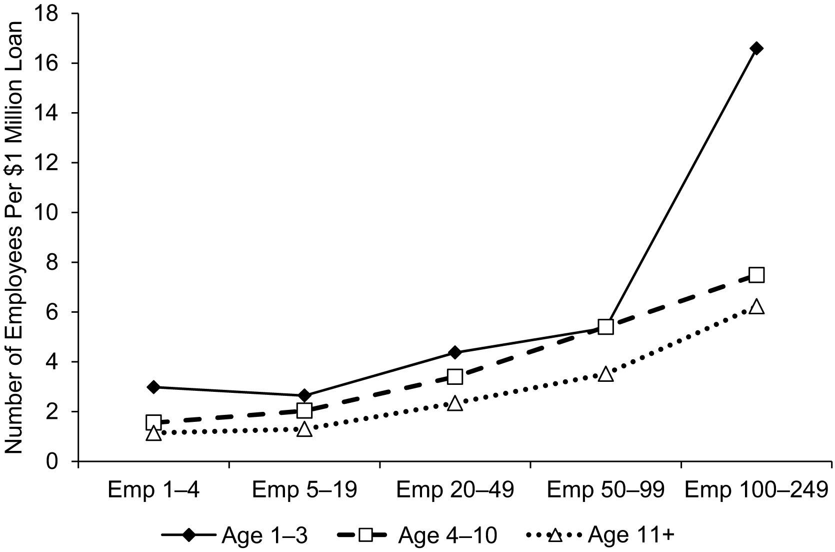

Fig. 9.11 Size effects on employment growth (accounting for exit) per $1 million loan for each age category

Note: These are plots of non-start-up firm results reported in table 9.17, including only coefficients significant at the 5 percent level.

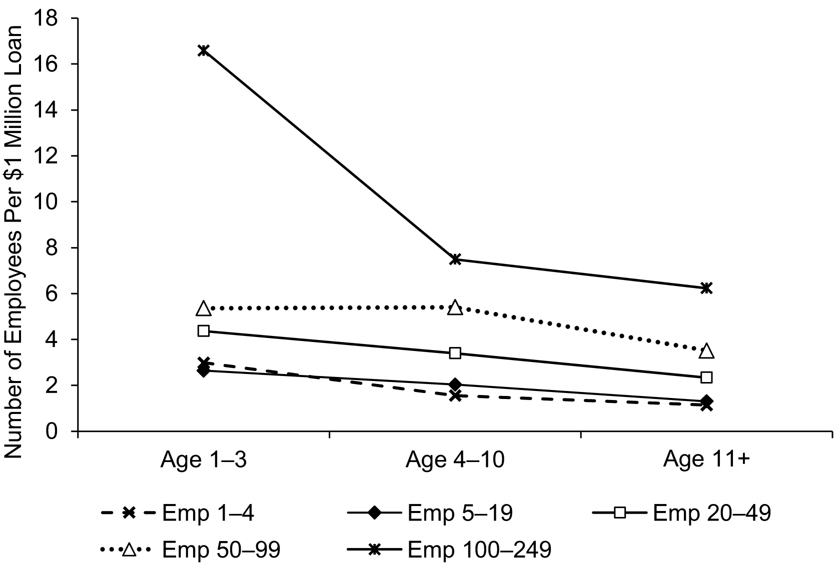

Fig. 9.12 Age effects on employment growth (accounting for exit) per $1 million loan for each size category

Note: These are plots of non-start-up firm results reported in table 9.17, including only coefficients significant at the 5 percent level.

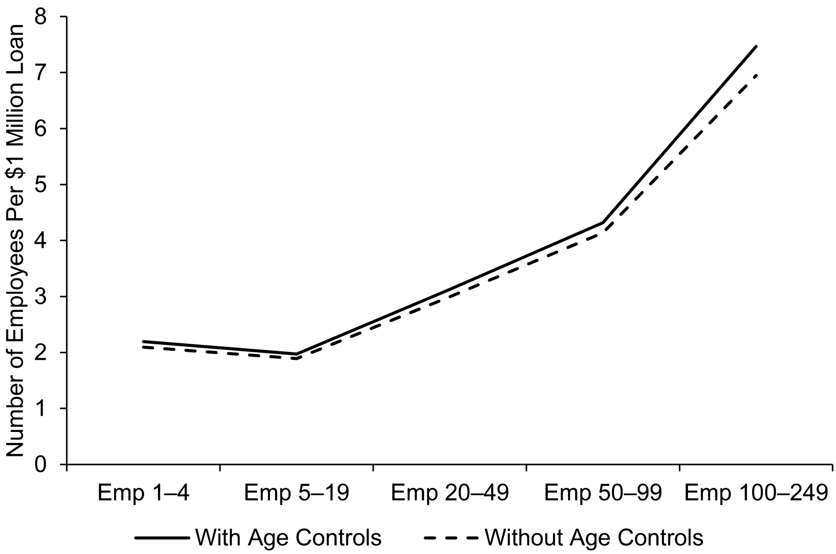

Fig. 9.13 Size effects on employment growth (accounting for exit) per $1 million loan, with and without age controls

Note: These are plots of results for non-start-up firms reported in tables 9.16 and 9.17. The results with age controls are averages of the coefficients for the size category across age categories, weighted by the number of treated firms in each cell.

applying for a loan. On the other hand, if the nature of this bias does not vary systematically across size and age groups, then the relative magnitudes of the loan effects would be unchanged.

Consistent with HJM's findings for overall job creation rates and with the literature on loan access and financial constraints on growth by firm age, we find strong employment and survival effects for start-up firms and both the employment and survival effects decline in age. Survival effects also decline in size, but within age categories employment effects increase with size. Unlike HJM, however, the employment effects increase with size even without age controls. We also find that the categories of firms most vulnerable to exit experience the largest survival effects from loan receipt. The loans are particularly helpful to young firms coping with the "valley of death."

The finding that small, mature firms expand the least and experience lower survival rates in response to SBA loan receipt suggests that though they may have difficulty obtaining loans (as suggested by the literature on loan access by firm size), their financial constraints only weakly affect growth. An interpretation of this result, consistent with Hurst and Pugsley's (2011) contention that most such firms are not motivated to grow, is that government support for them may therefore have a smaller impact. Though small, mature firms exhibit the lowest probability of receiving an SBA loan among all age-size categories and they still account for a substantial fraction of all SBA loans.

Table 9.18 **Regressions with pretreatment growth interactions by age categories**

	Loan amount	Pretreatment growth	Loan amt.* pretreatment growth	Number of obs.	Number of treated firms
			Employment growth		
Age 1–3	3.080	0.102	0.012	1,282,100	53,100
	(0.150)	(0.019)	(0.021)		
Age 4–10	2.611	0.060	0.120	1,108,400	57,400
	(0.107)	(0.035)	(0.034)		
Age 11+	2.786	0.210	0.093	2,492,400	54,700
	(0.078)	(0.024)	(0.021)		
			Employment growth with exit zeros		
Age 1–3	2.896	0.068	0.090	2,860,300	81,500
	(0.156)	(0.017)	(0.023)		
Age 4–10	2.696	0.114	0.102	1,850,000	78,500
	(0.115)	(0.030)	(0.038)		
Age 11+	2.406	0.297	0.076	3,618,100	70,100
	(0.082)	(0.027)	(0.018)		
			Three-year survival, 1991–2009 treatments		
Age 1–3	0.052	–0.00168	0.00029	3,057,100	85,100
	(0.002)	(0.00009)	(0.00013)		
Age 4–10	0.019	–0.00011	0.00016	1,953,200	81,700
	(0.001)	(0.00014)	(0.00017)		
Age 11+	–0.013	0.00053	0.00035	3,764,600	72,200
	(0.001)	(0.00010)	(0.00009)		
			Ten-year survival, 1991–2002 treatments		
Age 1–3	0.077	–0.0018	0.00015	1,405,200	38,000
	(0.004)	(0.0001)	(0.00022)		
Age 4–10	0.045	–0.0006	0.00029	1,197,300	42,100
	(0.003)	(0.0002)	(0.00039)		
Age 11+	–0.008	0.0007	–0.00001	2,189,400	38,200
	(0.002)	(0.0002)	(0.00020)		

Notes: These are kernel-weighted OLS regressions, run separately for each age category. The dependent variable for employment growth is average employment in $t + 1$ through $t + 3$ minus average employment in $t – 3$ through $t – 1$, including only firms that have positive employment in each of those years. The dependent variable for employment growth with exit zeros is average employment in $t + 1$ through $t + 3$ minus average employment in $t – 3$ through $t – 1$, including zeros for employment in years after exit. The dependent variable for three-year survival is a dummy for survival through $t + 3$. The dependent variable for ten-year survival is a dummy for survival through $t + 10$. Loan amount is in millions of 2010 dollars. Treated firm-control fixed effects are also included in the regressions. For firms with positive employment in both $t – 4$ and $t – 1$, pretreatment growth is the difference between employment in $t – 1$ and $t – 4$, divided by three so as to annualize it. If a firm does not have employment in $t – 4$, but has it in $t – 3$, pretreatment growth is the difference between employment in $t – 1$ and $t – 3$, divided by two. Firms without positive employment in $t – 4$ and $t – 3$, but with it in $t – 2$, have pretreatment growth of the difference between employment in $t – 1$ and $t – 2$, and those only with positive employment in $t – 1$ have pretreatment growth of employment in $t – 1$ (since employment prior to that is assumed to be zero). Standard errors, cluster-adjusted by firm, are in parentheses. The number of observations and SBA firms are rounded to the nearest 100 for disclosure avoidance.

Appendix

Table 9A.1 Bias after propensity score matching by age-size category

	Control sample	Treated sample	Standardized difference
	Age 1–3, emp. 1–4		
Log emp_{t-1}	0.703	0.703	0.000
Log (emp_{t-1} / emp_{t-2})	0.045	0.055	2.257
Log (emp_{t-2} / emp_{t-3})	0.136	0.135	–0.231
	Age 1–3, emp. 5–19		
Log emp_{t-1}	2.099	2.097	–0.138
Log (emp_{t-1} / emp_{t-2})	0.281	0.277	–0.867
Log (emp_{t-2} / emp_{t-3})	0.311	0.301	–2.343
	Age 1–3, emp. 20–49		
Log emp_{t-1}	3.331	3.328	–0.236
Log (emp_{t-1} / emp_{t-2})	0.345	0.358	2.881
Log (emp_{t-2} / emp_{t-3})	0.413	0.437	5.740
	Age 1–3, emp. 50–99		
Log emp_{t-1}	4.155	4.153	–0.126
Log (emp_{t-1} / emp_{t-2})	0.368	0.384	3.740
Log (emp_{t-2} / emp_{t-3})	0.492	0.547	12.913
	Age 1–3, emp. 100–249		
Log emp_{t-1}	4.947	4.945	–0.102
Log (emp_{t-1} / emp_{t-2})	0.481	0.518	8.626
Log (emp_{t-2} / emp_{t-3})	0.586	0.598	2.774
	Age 4–10, emp. 1–4		
Log emp_{t-1}	0.833	0.833	0.000
Log (emp_{t-1} / emp_{t-2})	–0.039	–0.038	0.161
Log (emp_{t-2} / emp_{t-3})	0.008	0.012	1.071
Log (emp_{t-3} / emp_{t-4})	0.042	0.040	–0.564
	Age 4–10, emp. 5–19		
Log emp_{t-1}	2.163	2.161	–0.118
Log (emp_{t-1} / emp_{t-2})	0.102	0.097	–1.144
Log (emp_{t-2} / emp_{t-3})	0.092	0.091	–0.274
Log (emp_{t-3} / emp_{t-4})	0.127	0.131	0.802
	Age 4–10, emp. 20–49		
Log emp_{t-1}	3.351	3.347	–0.322
Log (emp_{t-1} / emp_{t-2})	0.143	0.150	1.513
Log (emp_{t-2} / emp_{t-3})	0.143	0.149	1.391
Log (emp_{t-3} / emp_{t-4})	0.188	0.181	–1.683
	Age 4–10, emp. 50–99		
Log emp_{t-1}	4.169	4.166	–0.230
Log (emp_{t-1} / emp_{t-2})	0.163	0.166	0.664
Log (emp_{t-2} / emp_{t-3})	0.151	0.150	–0.292
Log (emp_{t-3} / emp_{t-4})	0.205	0.206	0.339

(*continued*)

Table 9A.1 (continued)

	Control sample	Treated sample	Standardized difference
	Age 4–10, emp. 100–249		
Log emp_{t-1}	4.923	4.923	0.062
Log $(\text{emp}_{t-1} / \text{emp}_{t-2})$	0.180	0.209	6.741
Log $(\text{emp}_{t-2} / \text{emp}_{t-3})$	0.153	0.157	1.069
Log $(\text{emp}_{t-3} / \text{emp}_{t-4})$	0.213	0.204	–2.368
	Age 11+, emp. 1–4		
Log emp_{t-1}	0.876	0.876	0.000
Log $(\text{emp}_{t-1} / \text{emp}_{t-2})$	–0.066	–0.064	0.376
Log $(\text{emp}_{t-2} / \text{emp}_{t-3})$	–0.040	–0.032	1.770
Log $(\text{emp}_{t-3} / \text{emp}_{t-4})$	–0.022	–0.015	1.766
	Age 11+, emp. 5–19		
Log emp_{t-1}	2.240	2.237	–0.260
Log $(\text{emp}_{t-1} / \text{emp}_{t-2})$	0.030	0.035	1.074
Log $(\text{emp}_{t-2} / \text{emp}_{t-3})$	0.021	0.026	1.278
Log $(\text{emp}_{t-3} / \text{emp}_{t-4})$	0.019	0.026	1.806
	Age 11+, emp. 20–49		
Log emp_{t-1}	3.384	3.378	–0.426
Log $(\text{emp}_{t-1} / \text{emp}_{t-2})$	0.056	0.061	1.263
Log $(\text{emp}_{t-2} / \text{emp}_{t-3})$	0.044	0.052	1.826
Log $(\text{emp}_{t-3} / \text{emp}_{t-4})$	0.044	0.046	0.479
	Age 11+, emp. 50–99		
Log emp_{t-1}	4.201	4.198	–0.232
Log $(\text{emp}_{t-1} / \text{emp}_{t-2})$	0.079	0.074	–1.121
Log $(\text{emp}_{t-2} / \text{emp}_{t-3})$	0.054	0.055	0.321
Log $(\text{emp}_{t-3} / \text{emp}_{t-4})$	0.053	0.055	0.530
	Age 11+, emp. 100–249		
Log emp_{t-1}	4.962	4.960	–0.180
Log $(\text{emp}_{t-1} / \text{emp}_{t-2})$	0.074	0.097	5.295
Log $(\text{emp}_{t-2} / \text{emp}_{t-3})$	0.055	0.054	–0.241
Log $(\text{emp}_{t-3} / \text{emp}_{t-4})$	0.056	0.071	3.538

Note: For a given variable, say age, the standardized difference (% bias) is $SDIFF(\text{age}) = \{100(1/N)\sum_{i\in A}[\text{age}_i - \sum_{j\in C} g(p_i, p_j)\text{age}_j]\} / \sqrt{[Var_{i\in A}(\text{age}) + Var_{j\in C}(\text{age})] / 2}$. These numbers are calculated for the regression samples in table 9.9.

Table 9A.2 **Unadjusted employment growth regressions by size categories, age categories**

	Loan amount coefficient	Number of observations	Number of treated firms	Percent of observations with boundary change
Emp. 1–4	2.431	1,284,000	59,700	0.01
	(0.062)			
Emp. 5–19	2.079	737,500	67,800	0.06
	(0.078)			
Emp. 20–49	3.180	2,516,300	28,400	0.34
	(0.083)			
Emp. 50–99	3.692	286,000	6,900	2.00
	(0.230)			
Emp. 100–249	6.234	55,700	2,400	13.65
	(0.706)			
Age 0	6.022	7,555,600	49,800	0.16
	(0.167)			
Age 1–3	2.978	1,282,100	53,100	0.08
	(0.181)			
Age 4–10	2.975	1,108,300	57,400	0.23
	(0.122)			
Age 11+	3.011	2,489,100	54,700	0.76
	(0.088)			

Notes: These are kernel-weighted OLS regressions, run separately for each size and age category. The dependent variable is average employment in $t + 1$ through $t + 3$ minus average employment in $t - 3$ through $t - 1$, including only firms that have positive employment in each of those years. Employment changes can be due to either organic growth or boundary changes. Loan amount is in millions of 2010 dollars. Treated firm-control fixed effects are also included in the regressions. Standard errors, cluster-adjusted by firm, are in parentheses. The share of observations with boundary changes is kernel weighted. The number of observations and SBA firms are rounded to the nearest 100 for disclosure avoidance.

Table 9A.3 **Unadjusted employment growth regressions by age-size categories**

	Loan amount coefficient	Number of observations	Number of treated firms	Percent of observations with boundary change
Age 0	6.022	7,555,600	49,800	0.16
	(0.167)			
Age 1–3, emp. 1–4	3.340	622,500	28,500	0.01
	(0.114)			
Age 1–3, emp. 5–19	2.158	163,800	18,500	0.03
	(0.339)			
Age 1–3, emp. 20–49	3.714	467,400	5,000	0.15
	(0.293)			
Age 1–3, emp. 50–99	2.168	26,600	800	0.70
	(0.920)			
Age 1–3, emp. 100–249	11.630	1,800	200	6.11
	(5.095)			
Age 4–10, emp. 1–4	1.797	313,800	19,500	0.01
	(0.068)			
Age 4–10, emp. 5–19	2.244	201,100	25,100	0.06
	(0.069)			
Age 4–10, emp. 20–49	3.480	547,900	10,300	0.27
	(0.180)			
Age 4–10, emp. 50–99	3.918	40,900	1,900	1.56
	(0.687)			
Age 4–10, emp. 100–249	6.823	4,700	500	7.85
	(2.255)			
Age 11+, emp. 1–4	1.638	347,700	11,700	0.01
	(0.120)			
Age 11+, emp. 5–19	1.881	372,600	24,200	0.08
	(0.047)			
Age 11+, emp. 20–49	2.857	1,501,100	13,000	0.43
	(0.077)			
Age 11+, emp. 50–99	3.813	218,400	4,100	2.25
	(0.219)			
Age 11+, emp. 100–249	5.852	49,300	1,700	14.47
	(0.712)			

Notes: These are kernel-weighted OLS regressions, run separately for each age-size category. The dependent variable is average employment in $t+1$ through $t+3$ minus average employment in $t-3$ through $t-1$, including only firms that have positive employment in each of those years. Employment changes can be due to either organic growth or boundary changes. Loan amount is in millions of 2010 dollars. Treated firm-control fixed effects are also included in the regressions. Standard errors, cluster-adjusted by firm, are in parentheses. The share of observations with boundary changes is kernel weighted. The number of observations and SBA firms are rounded to the nearest 100 for disclosure avoidance.

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