Job Creation, Small vs. Large vs. Young, and the SBA

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Abstract

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

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Introduction

One of the few areas of recent consensus across all major political groups in the US is the allegedly important role played by small businesses in job creation. The initiatives justified by this conviction include a variety of small business loan and support programs, largely through the Small Business Administration (SBA), as well as preferential treatment of small businesses in contracting and regulatory requirements.[2] The empirical basis for the belief goes back to Birch (1987), although the underlying methods and data were questioned by Davis, Haltiwanger, and Schuh (1996). More recently, Neumark, Wall, and Zhang (2011) have reconfirmed, using improved data and methods, the Birch conclusion, but Haltiwanger, Jarmin, and Miranda (hereafter HJM, 2013) have shown that the size-growth relationship is not robust to controlling for age (as had Evans (1987) for a much smaller data set on manufacturing industries). Indeed, HJM find that the relationship may even reverse signs, so that larger firms contribute more to job creation, once age is taken into account.

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

This paper attempts to shed light on variation by age and size in the responsiveness of employment to interventions in financial access. Building on our previous paper estimating the average effects (Brown and Earle, 2013), the treatment we examine is the SBA's 7(a) and 504 loan guarantee programs. For this purpose, we have linked a complete list of SBA 7(a) and 504 loans to the Census Bureau's employer and non-employer business registers and to the Longitudinal Business Database (LBD), which tracks all firms and establishments in the U.S. non-farm business sector with paid employees on an annual basis in 1976-2012. We restrict the analysis to recipients of loans in 1992-2011 and their matched controls.

While our paper is inspired to some extent by HJM and we use some of the Census data they developed, our question and therefore our methods are somewhat different. While HJM measure year-to-year growth in employment, our focus is the change in level of employment from the period before the SBA loan to the period after the loan is received. Our estimation

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

method involves construction of a control sample of firms in the same age-size class. We also impose the following "exact matching" requirements: employment within 10 percent of the recipient's employment in the year of loan receipt, the same four-digit industry, and the same state (the latter only for firms with 19 or fewer employees). Probit regressions for loan receipt are run on this set of recipients and potential controls, including employment in the year before loan receipt, annual employment growth three, two, and one year before loan receipt, average wage in the year before loan receipt, relative productivity in the year before loan receipt, firm age, and year dummies. A non-treated firm is included as a control for a particular treated firm if the ratio of the treated to the non-treated firm's propensity score is at least 0.9 and not more than 1.1. Kernel weights are applied to the controls in subsequent employment and survival regressions.

The employment panel regressions include post-loan period effects (common to the treated firm and its matched controls), and year effects. Specifications focused on continuing firms included firm fixed effects, while those focused on start-ups do not (because of the absence of a pre-treatment period). In one specification, the dependent variable is the log of employment and a post-loan (treatment) dummy for the treated firm is the variable of interest. In other specifications, we use unlogged employment as the dependent variable and examine the results with a dummy treatment and the amount of the loan. We also study the dynamics of the effect by including separate dummies for each year before/after the treated firm's loan receipt. To study survival we estimate both Cox-Proportional Hazard models and 3-year and 10-year survival probits, including treatment year and sector effects along with a treatment dummy and loan amount, in alternative specifications.

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

2. SBA Loan Programs

The SBA has several small business loan guarantee programs. In this paper, we focus on the largest two categories of programs, 7(a) and 504, and this section briefly describes their current characteristics.[3]

SBA 7(a) loans not part of a special subprogram can be for an amount up to $5 million, with a maximum 85 percent SBA guarantee for loans up to $150,000, and a 75 percent maximum guarantee for higher amounts.[4] They are term loans that can be used for expansion/renovation; new construction; purchase of land, buildings, equipment, fixtures, and lease-hold improvements; working capital; debt refinancing for compelling reasons; seasonal line of credit; and inventory. Maturity depends on the ability to repay. Usually loans for working capital and machinery (not to exceed the life of equipment) have a maturity of 5-10 years, while loans for

[3] This section draws heavily on Brown and Earle (2013).
[4] See http://www.sba.gov/content/7a-terms-conditions.

purchase of real estate can have a term up to 25 years. The SBA sets maximum loan interest rates, which decrease with loan amount and increase with maturity. Since December 8, 2004 SBA has charged a guaranty fee, which increases with maturity and loan amount.[5] To qualify, a business must be for-profit; meet SBA size standards;[6] show good character,[7] management expertise, and a feasible business plan; not have funds available from other sources;[8] and be an eligible type of business.[9] The SBA itself makes the final credit decisions for these loans.

Some 7(a) programs are more streamlined. Unlike with other 7(a) loans, in the 7(a) Preferred Lender Program (PLP) the SBA delegates the final credit decision and most servicing and liquidation authority to PLP lenders.[10] The SBA's role is to check loan eligibility criteria. The SBA selects lenders for PLP status based on their past record with the SBA, including proficiency in processing and servicing SBA-guaranteed loans. In payment default cases, the PLP lender agrees to liquidate all business assets before asking the SBA to honor its guaranty.

In the 7(a) Certified Lender Program (CLP), the SBA promises a loan decision within three working days on applications handled by CLP lenders.[11] Rather than ordering an independently conducted analysis, the SBA conducts a credit review, relying on the credit knowledge of the lender's loan officers. Lenders with a good performance history with SBA loans may receive CLP status.

SBA 7(a) Express loans have a $350,000 maximum loan amount and 50 percent maximum SBA guaranty.[12] Interest rates can be higher than on other 7(a) loans. Qualified lenders may be granted authorization by the SBA to make eligibility determinations. The SBA promises a decision within 36 hours.[13]

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

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

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

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

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

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

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

[12] See http://www.sba.gov/content/become-express-lender and http://www.sba.gov/sites/default/files/files/Loan%20Chart%20Baltimore%20June%202012%20Version%202.pdf.

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

The 504 Loan Program offers loan guarantees up to $5 million or $5.5 million, depending on the type of business.[14] Typically a lender covers 50 percent of the project costs without an SBA guarantee, a Certified Development Company (CDC) certified by the SBA provides up to 40 percent of the financing (100 percent guaranteed by an SBA-guaranteed debenture), and the borrower contributes at least 10 percent (the borrower is sometimes required to contribute up to 20 percent). CDCs are nonprofit corporations promoting community economic development via disbursement of 504 loans. Proceeds may be used for fixed assets or to refinance debt in connection with an expansion of the business via new or renovated assets. For-profit businesses with tangible net worth of no more than $15 million and average income of no more than $5 million after federal income taxes in the two years prior to application are eligible. Businesses must create or retain one job per $65,000 guaranteed by the SBA, with the exception of small manufacturers, which must create or retain one job per $100,000.

3. Data

We use a database on all 7(a) and 504 loans guaranteed by the SBA from inception in 1953 through 2011 to identify loan recipients, amounts, and time of receipt. We convert the SBA-approved loan amounts to real 2010 prices using the annual average Consumer Price Index from the Bureau of Labor Statistics.[15] Loan timing is based on the date the SBA approved the loan. In order to exclude any firms receiving a disaster loan before their first 7(a) or 504 loan from the analysis, we also use a database on all SBA disaster loans from inception through 2009.

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

The Census Bureau's Longitudinal Business Database (LBD) consists of longitudinally linked employer business registers. The LBD tracks all firms and establishments in the U.S. non-farm business sector with paid employees on an annual basis in 1976-2012. The SBA loan

[14] See http://www.sba.gov/content/cdc504-loan-program.
[15] This can be downloaded from ftp://ftp.bls.gov/pub/special.requests/cpi/cpiai.txt.

match to employer business registers allows us to link the SBA data to the entire LBD. The LBD contains employment as of the pay period including March 12th, annual payroll, state, county, zip code, and industry code. The industry code is a four-digit SIC code through the year 2001 and a six-digit NAICS code in 2002-2012.

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

Since our interest is organic job creation, we adjust firm employment for establishment ownership changes. When a firm purchases a pre-existing establishment, the establishment's employment in the year preceding the change is subtracted from the firm's employment in all subsequent years. Analogously, we add sold establishment employment in the year prior to sale to the firm's employment in all subsequent years, in cases where establishments are sold and continue to operate.[16]

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

As shown in Table 1, 87 percent of the SBA 7(a) and 504 loans have been matched to establishments in the business registers. Of these, 90 percent are found in the employer registry and thus in the LBD. Among firms receiving multiple SBA loans, we select the first 7(a) or 504 loan as the treatment.[17] We drop firms ever receiving a SBA disaster loan and those receiving their first 7(a) loan prior to 1992.[18] Our identification method for non-start-ups relies heavily on the value of employment in the year prior to loan receipt, so we drop continuing firms missing

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

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

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

this value in the LBD. Finally, we drop firms for which no suitable controls are found, according to our matching procedures. Table 1 reports the number of loans dropped as a result of each of these restrictions.

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

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

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

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

Brown and Earle (2013) report that SBA-recipients differ systematically from typical firms in several other ways, including the pace of growth prior to loan receipt. Hurst and Pugsley (2011), for example, report that most small firms grow very little, entering at low

[19] 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.

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

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

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

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

4. Estimation Strategy

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

For this purpose, we have taken the following steps. As mentioned in Section 3 above, we limit our treated sample to firms in the LBD receiving their first SBA 7(a) or 504 loan in 1992-2011 and those not receiving a SBA disaster loan prior to their first 7(a) or 504 loan. To be

eligible to be a candidate control firm for a particular treated firm, a firm can never have received an SBA 7(a), 504, or disaster loan at any time between 1953-2009; it must be in the same four-digit industry (this is the four-digit SIC code through 2001 and the first four digits of the NAICS code in 2002-2012) in the treated firm's loan receipt year, and be in the same firm age category (as defined above) in the treated firm's loan receipt year. For non-start-ups, the control must have non-missing employment in the year prior to the treated firm's loan receipt. Among firms with 19 or fewer employees in the previous year, we also require the candidate control firm to be located in the same state (firms with 1-19 employees are much more numerous than ones with more than 19 employees, so we can afford to impose more restrictions on this group). In addition, for non-start-ups we impose a restriction that the ratio of the treated firm's employment in the previous year to the control firm's previous year employment be greater than 0.9 and less than 1.1. This means that among firms with nine or fewer employees, employment must match exactly.

For the non-start-ups, we would also like to match on variables representing the history of employment prior to treatment year, but it is difficult to design matching thresholds for each variable separately, so we reduce this dimensionality problem with propensity score matching. We estimate separate probit regressions using the sample of treated firms and their candidate controls (according to the exact matching criteria) for different age-size categories.[21] For each of the 15 size-age categories defined above for non-start-ups, a dummy for SBA 7(a) or 504 loan receipt is regressed on the log of employment in the year prior to the treated firm's loan receipt; the square of the log of employment in the year prior to the treated firm's loan receipt; the log of employment one year before minus log employment two years prior to the treated firm's loan receipt; the log of employment two years before minus log employment three years prior to the treated firm's loan receipt; the log of employment three years before minus log employment four years prior to the treated firm's loan receipt; the log of payroll/number of employees in the year prior to the treated firm's loan receipt; firm age; firm age squared; and year dummies. We also include dummies for missing values for the log of employment two years before minus log employment three years prior to the treated firm's loan receipt, the log of employment three years before minus log employment four years prior to the treated firm's loan receipt, and the log of payroll/number of employees in the year prior to the treated firm's loan receipt.[22]

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

We limit the treated and non-treated firms in the employment regression analysis to those within a common support, meaning that no propensity score of a treated (non-treated) firm that we use is higher than the highest non-treated (treated) firm propensity score, and no propensity

[21] Treated firms with no candidate controls are dropped at this point.
[22] When a firm has a missing value for one of these variables, a zero is imputed.

score of a treated (non-treated) firm that we use is lower than the lowest non-treated (treated) firm propensity score. A non-treated firm is included as a control for a particular treated firm if the ratio of the treated to the non-treated firm's propensity score is at least 0.9 and not more than 1.1. Treated firms with no controls meeting all these criteria are not included in the employment regression analysis. Non-treated firms appear in the employment regressions as many times as they have treated firms to which they are matched (i.e., this is matching with replacement). Kernel weights are applied to the controls.[23] In the employment regressions, each control is assigned a final weight of their kernel weight divided by the sum of the kernel weights for all controls for a particular treated firm, and the treated firm is given a weight of 1. As a result, the treated firm and all its control firms together receive equal weight.

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

The employment regression specifications take the following form:

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

where *y* is employment (logged or unlogged, in alternative specifications), *i* indexes firms from 1 to *I*, *j* indexes from 1 to *J* the treated firms to which the firm is a control,[24] and *t* indexes the years from 1 to *T*. α_{ij} is a fixed effect for each firm *i* which also varies for controls matched to different treated firms *j*, $\boldsymbol{L_{it}}$ is a dummy common to treated firms and their matched controls equal to one in all years following the treated firm's loan receipt, ρ_t is a vector of year dummies, and u_{ijt} is an idiosyncratic error.[25] In alternative specifications, y_{it} is the firm's employment and the natural logarithm of the firm's employment.

$\boldsymbol{\theta_{it}}$ is a vector of SBA loan treatment measures, and $\boldsymbol{\delta}$ are the loan treatment effects of interest. We estimate several alternative specifications of $\boldsymbol{\theta_{it}}$. The simplest specifications include a post-loan dummy, which for treated firms is equal to 1 in the year after receipt of the first SBA loan and in all subsequent years. Others include the post-loan dummy interacted with the amount of the first SBA loan, expressed in $millions.[26] Some specifications include the loan amount, and some also include the loan amount squared. We also estimate dynamic specifications including two sets of dummy variables for the years before and after first SBA loan receipt, one of which is common to both treated firms and their matched controls (replacing the $\boldsymbol{L_{it}}$), and the other specific to the treated firms (they are always zero for controls).

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

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

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

[26] If a firm received multiple SBA loans in that first year, the loan amounts are combined.

For firms receiving an SBA loan at start-up (during the first year of positive employment in the LBD or the prior year), the matching procedures involve only exact matching on industry, year, age (start-ups are matched only with start-ups), and county – a more exact geographic match because of the large volume of entry. We also estimate regressions but cannot include firm fixed effects because firms receiving SBA loans during the start-up year have no untreated observations. These limitations, which are forced by the lack of history and thus observably *sui generis* nature of start-ups, somewhat reduce our confidence in their results compared to those for continuing firms, but we include them because of the considerable interest in their role in job creation.

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

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

The reliability of propensity score matching depends on whether, conditional on the propensity score, the potential outcomes y^1 and y^0 are independent of treatment incidence. The assumption of independence conditional on observables depends on the pre-treatment variables being balanced between the treated and control groups. We assess this in two ways – by performing a standardized difference (or bias) test for the main variables included in the matching probit regressions, and by analyzing the pre-treatment event-time dynamics (see Section 5). Table 9 reports the means of the main variables included in the matching probit regressions for four different samples: all treated firms, all non-treated firms, treated firms included in the employment regressions, and controls included in the employment regressions. Treated firm employment is larger and age is younger than for non-treated firms prior to matching, and treated firms experience more employment growth in the four years prior to treatment. After matching, these differences are negligible. The standardized difference measures confirm this: employment and employment growth, and age biases are reduced by over 92 percent.[27] None of the biases are close to being large after matching.[28]

We also investigate variation in the impact of SBA loans on the probability of exit by size-age group. Our method is to use the matched samples, selected as defined above, to

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

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

estimate proportional hazard functions including SBA loan receipt and amount. A hypothesis of particular interest is whether SBA loans reduce exit during the high mortality period of the "valley of death."

5. Employment Growth Estimates

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

The estimates of average effects across the whole matched sample, not differentiating by size-age categories, are shown in Table 10. The eight specifications differ in the definition of the dependent variable (logged vs. unlogged employment), the right-hand-side variable of interest (binary treatment dummy vs. loan amount in 2010 USD), and whether firm fixed effects (FE) are included or not. The logged specification is more natural with a binary treatment and the unlogged with loan amount. Excluding firm fixed effects permits start-ups to contribute to identifying the loan effect, but of course it also removes the control on time-invariant heterogeneity.

When log employment is the dependent variable, in Column (1) of the table, the OLS estimate implies that a loan is associated with approximately a 21 percentage point increase in employment on average across years following loan receipt, while the FE estimate is slightly lower at about 16 percent.[29] The OLS and FE estimates with unlogged employment as the dependent variable show a similar relationship with 3 jobs created by a loan under OLS and 2.5 with FE. Whether these differences reflect the attenuation in estimates with FE (either because of genuine heterogeneity or measurement error in the SBA data) or a larger effect among the start-ups (which contribute to the OLS but not the FE estimates) is unclear, but we will analyze the latter possibility when we consider heterogeneity of the effects across age-size groups.

Columns (3) and (4) focus on effects of the loan amount, measured in 2010 USD. In one case, we exclude the post-loan dummy and in the other case we include it, but the implied effects of amount are similar: 15-16 jobs in the OLS and about 6 in the FE specification, with the latter result similar or slightly larger than reported by Brown and Earle (2013). While the OLS result for the post-loan dummy in Column (4) is large and negative, it is small in the FE result. One interpretation of this finding is that selection into the loan program does not appear, by itself, to raise employment, which is instead affected by the amount of the loan.

As a further specification check and to examine long- versus short-run consequences of the loans, Figures 1-6 show estimated dynamics of the employment effects, where the coefficient on SBA loan receipt is permitted to vary in event time around the in the start-up year. Figures 1

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

and 2 contain continuing firm OLS and FE results, respectively, with logged employment as the dependent variable, Figures 3 and 4 are the same with unlogged employment, and Figures 5 and 6 are results for start-ups only.

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

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

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

Figure 9 shows the size profiles for the unlogged employment specification with loan amount as the variable of interest. By contrast with the logged, dummy variable specification in Figure 7, the size profiles are upward sloping for older firms, but relatively flat for younger ones. The age profiles for loan amount in Figure 10 are downward sloping for the smallest firms, and

the slope increases with size. In contrast to Figure 8, the effects are more diverse across size groups for older firms than they are for younger firms.

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

This analysis includes firm fixed effect and is valid for continuing firms only. If we instead estimate by OLS, then entrants can be included. Table 12 contains the results. In the logged, dummy specification, the SBA loan impact for entrants is estimated at 0.21, the highest among all categories except for the youngest, smallest continuers. In the unlogged, loan amount specification, the estimate is 8.2 (jobs per million loan dollars), again the second largest among the 16 age-size categories. These results provide some evidence that SBA loans may be particularly supportive of start-up job creation, though the inability to control for fixed effects limits the strength of this inference.[30]

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

The results with FE are shown in Figures 11 and 12. Figure 11 contains the logged, dummy variable specification and shows a negative relationship between size and loan effect that differs little depending on whether age controls are excluded or included. Figure 12 contains the unlogged, loan amount specification, and shows a positive relationship between size and loan effect that is again little affected by the presence of age controls. Thus, while age is a particularly useful variable for the smallest size category (1-4 employees) and somewhat less so for the second smallest (5-19), the relationship between growth and size is generally little

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

affected by including information on age. However, the size-growth relationship is very much affected by whether the focus is the proportional impact on individual firms or the employment gains from a particular volume of loans.

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

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

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

6. Survival Estimates

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

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

Nearest neighbor matching is used here to avoid the need to weight the regressions, which is problematic for probit regressions.

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

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

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

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

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

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

7. Conclusion

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

Somewhat surprisingly, our analysis finds different answers to these two questions in the context of the two largest loan guarantee programs of the Small Business Administration: the

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

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

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

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

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

Table 2. Number of SBA Loan Recipients in LBD by Age in Loan Year and Employment Size in Year Before Loan

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

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

Table 3. SBA Loan Recipients (counted once, in loan year) as Percent of All LBD Firm-Years between 1992-2011 (including all SBA firm-years)

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

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

Table 4. Mean Employment Growth Between Four Years Before and One Year Before Loan Receipt for SBA Loan Recipients by Age in Loan Year and Employment Size in Year Before Loan

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

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

Table 5. Mean Employment Growth Between Year *t-4* and Year *t-1* for All Non-SBA LBD Firms Present in Year *t* in 1992-2012, by Age in Year *t* and Employment Size in Year *t-1*

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

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

Table 6. Mean SBA Loan Size (2010 $US)

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

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

Table 7. Number of Firms with SBA Loans in LBD and Matched Sample by Size and Age

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

Table 8. Number of Firms and Firm-Year Observations in Regressions with All Matches

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

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

Table 9. Bias Before and After Propensity Score Matching

	Variable Mean					
	All Non-Treated	All Treated	Final Control Sample	Final Treated Sample	Final % Bias	% Bias Reduction
Log Emp t-1	1.749	1.892	1.898	1.895	-0.171	98.466
Log Emp t-1 sq.	4.966	4.979	5.015	5.003	-0.210	7.290
Log Emp t-1 – t-2	0.016	0.069	0.060	0.064	1.003	91.825
Log Emp t-2 – t-3	0.023	0.068	0.063	0.063	0.106	99.010
Log Emp t-3 – t-4	0.026	0.068	0.064	0.064	-0.007	99.935
Log Wage	3.044	3.050	3.039	3.060	2.551	-2.829
Age	10.755	8.409	8.507	8.504	-0.032	99.886

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

TABLE 10. Estimated Overall Employment Effects for SBA Loans

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

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

TABLE 11. Fixed Effects with Matched Controls

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

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

TABLE 12. OLS with Matched Controls, Age-Size Categories

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

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

TABLE 13. Effects by Size, Not Controlling for Age

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

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

TABLE 14. Fixed Effects with Matched Controls, Age-Size Categories, One Post-Exit Observation

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

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

TABLE 15. OLS with Matched Controls, Age-Size Categories, One Post-Exit Observation

	(1)	(2)	
	Log Employment	Unlogged Employment	
	Post-loan Dummy	Post-loan Dummy	Loan Amount
Age 0	0.181	0.629	8.438
	(0.005)	(0.205)	(0.373)
Age 1-3, Emp 1-4	0.194	1.038	3.427
	(0.004)	(0.111)	(0.257)
Age 1-3, Emp 5-19	0.150	1.033	3.928
	(0.005)	(0.153)	(0.261)
Age 1-3, Emp 20-49	0.136	2.489	3.267
	(0.011)	(0.747)	(0.587)
Age 1-3, Emp 50-99	0.182	7.352	5.621
	(0.031)	(4.440)	(3.588)
Age 1-3, Emp 100-249	0.234	1.753	-7.593
	(0.073)	(16.735)	(9.406)
Age 4-10, Emp 1-4	0.134	0.390	2.268
	(0.004)	(0.058)	(0.151)
Age 4-10, Emp 5-19	0.126	0.183	3.640
	(0.005)	(0.096)	(0.137)
Age 4-10, Emp 20-49	0.117	0.577	4.736
	(0.008)	(0.514)	(0.413)
Age 4-10, Emp 50-99	0.141	7.504	1.655
	(0.022)	(2.870)	(1.544)
Age 4-10, Emp 100-249	0.066	-0.533	15.321
	(0.046)	(11.351)	(9.068)
Age 11+, Emp 1-4	0.088	0.037	2.499
	(0.005)	(0.061)	(0.208)
Age 11+, Emp 5-19	0.085	-0.451	3.805
	(0.005)	(0.093)	(0.137)
Age 11+, Emp 20-49	0.089	-0.701	4.717
	(0.007)	(0.405)	(0.273)
Age 11+, Emp 50-99	0.095	1.039	4.271
	(0.014)	(2.279)	(0.715)
Age 11+, Emp 100-249	0.041	6.430	4.479
	(0.026)	(6.410)	(3.758)

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

Table 16. Three-Year Survival Rates (%) in Survival Regression Sample

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

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

Table 17. Ten-Year Survival Rates (%) in Survival Regression Sample

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

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

TABLE 18. Survival Effects by Age-Size Categories, Cox-Proportional Hazard Models

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

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

TABLE 19. Three-Year Survival Effects by Age-Size Categories, Probit Models

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

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

TABLE 20. Ten-Year Survival Effects by Age-Size Categories, Probit Models

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

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

TABLE 21. Survival Effects by Size, Not Controlling for Age, Cox-Proportional Hazard Models

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

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

TABLE 22. Three-Year Survival Effects by Size, Not Controlling for Age, Probit Models

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

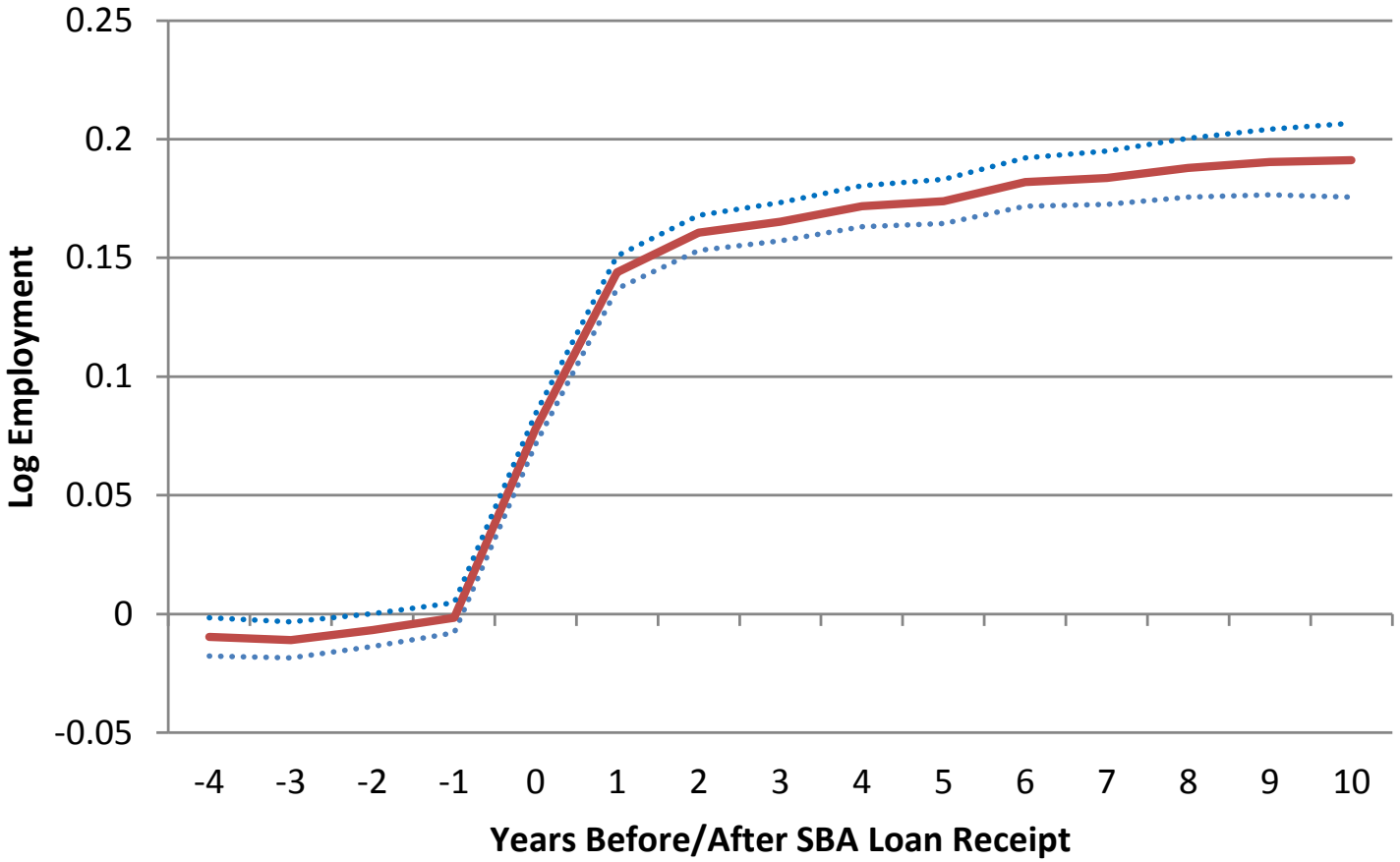
These are marginal effects from probit regressions with a dependent variable equal to one if the firm survives three years after the treated firm's year of SBA loan receipt, run separately for each size category. The regressions include dummies for the treatment year and 16 sectors. Standard errors,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

TABLE 23. Ten-Year Survival Effects by Size,
Not Controlling for Age, Probit Models

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

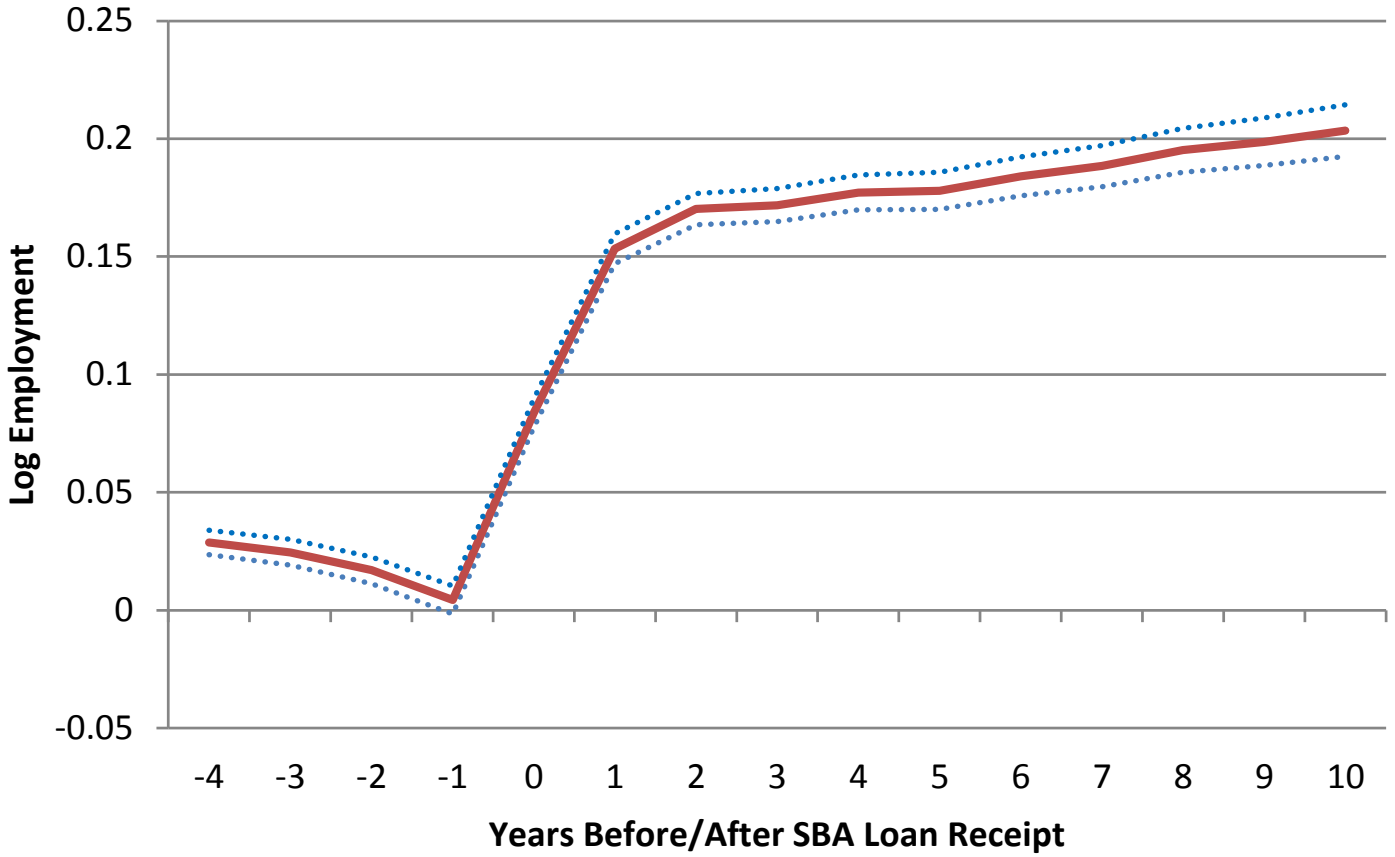
These are marginal effects from probit regressions with a dependent variable equal to one if the firm survives ten years after the treated firm's year of SBA loan receipt, run separately for each size category. The regressions include dummies for the treatment year and 16 sectors. Standard errors, calculated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

Figure 1. Dynamic Specification for SBA Loans After Start-Up, OLS



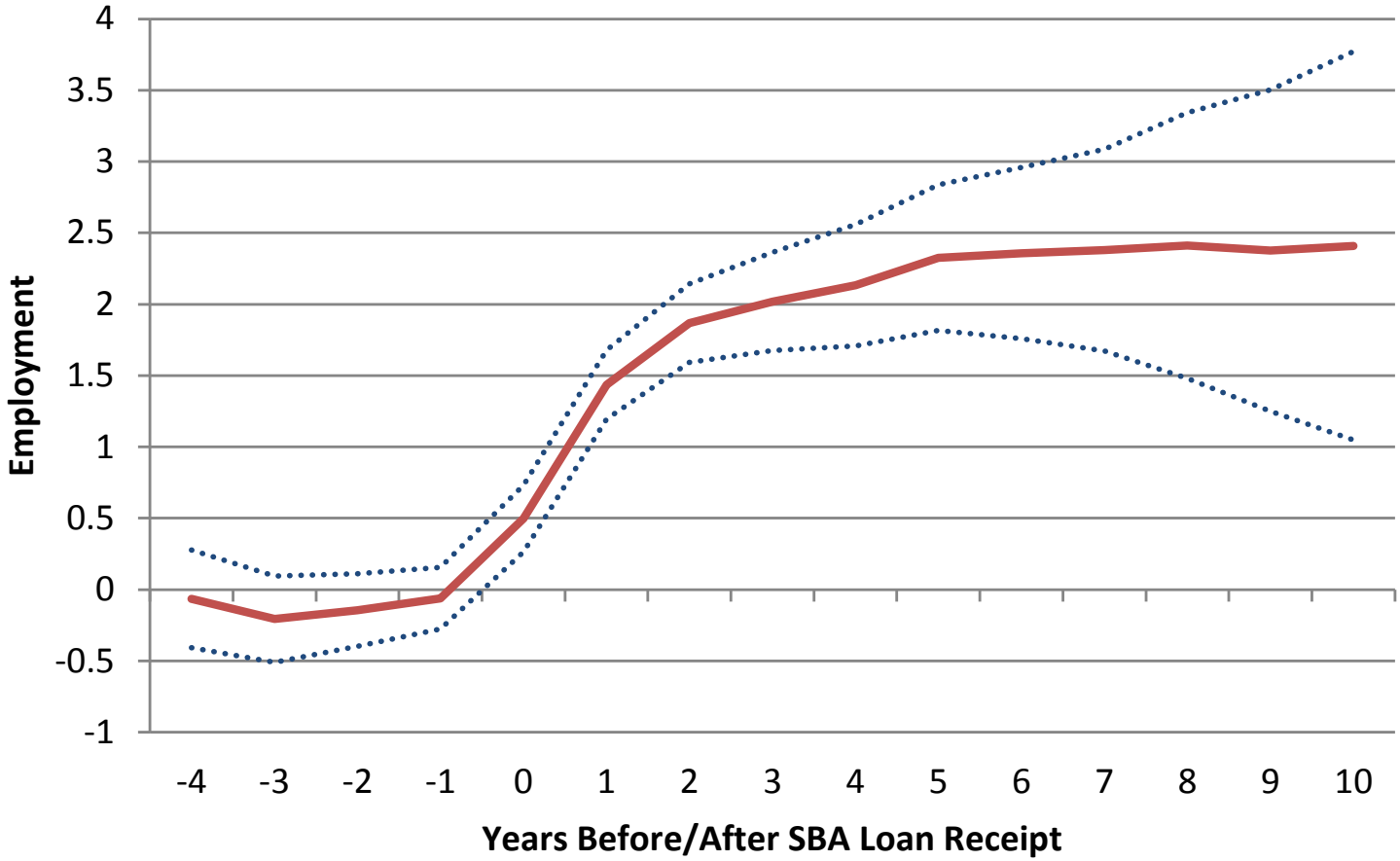
OLS regression, also including dummies common to treated and control firms for years before/after the treated firm's treatment and year effects. Years prior to four years before treatment is the base category. Dummies for years after ten years after treatment are included, but not reported. The dotted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

Figure 2. Dynamic Specification for SBA Loans After Start-Up, Fixed Effects



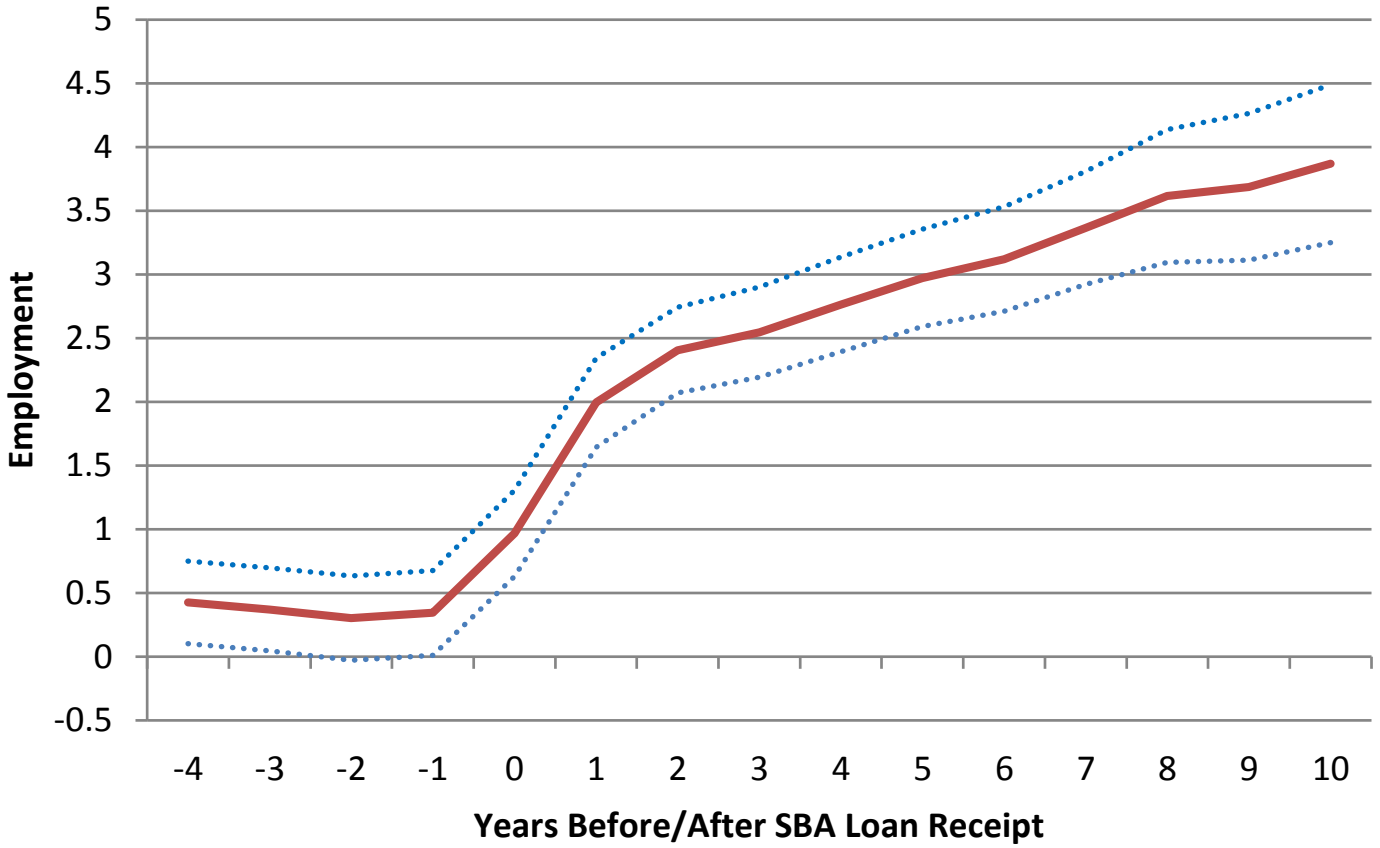
Fixed effects regression, also including dummies common to treated and control firms for years before/after the treated firm's treatment and year effects. Years prior to four years before treatment is the base category. Dummies for years after ten years after treatment are included, but not reported.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

Figure 3. Dynamic Specification for SBA Loans After Start-Up, OLS



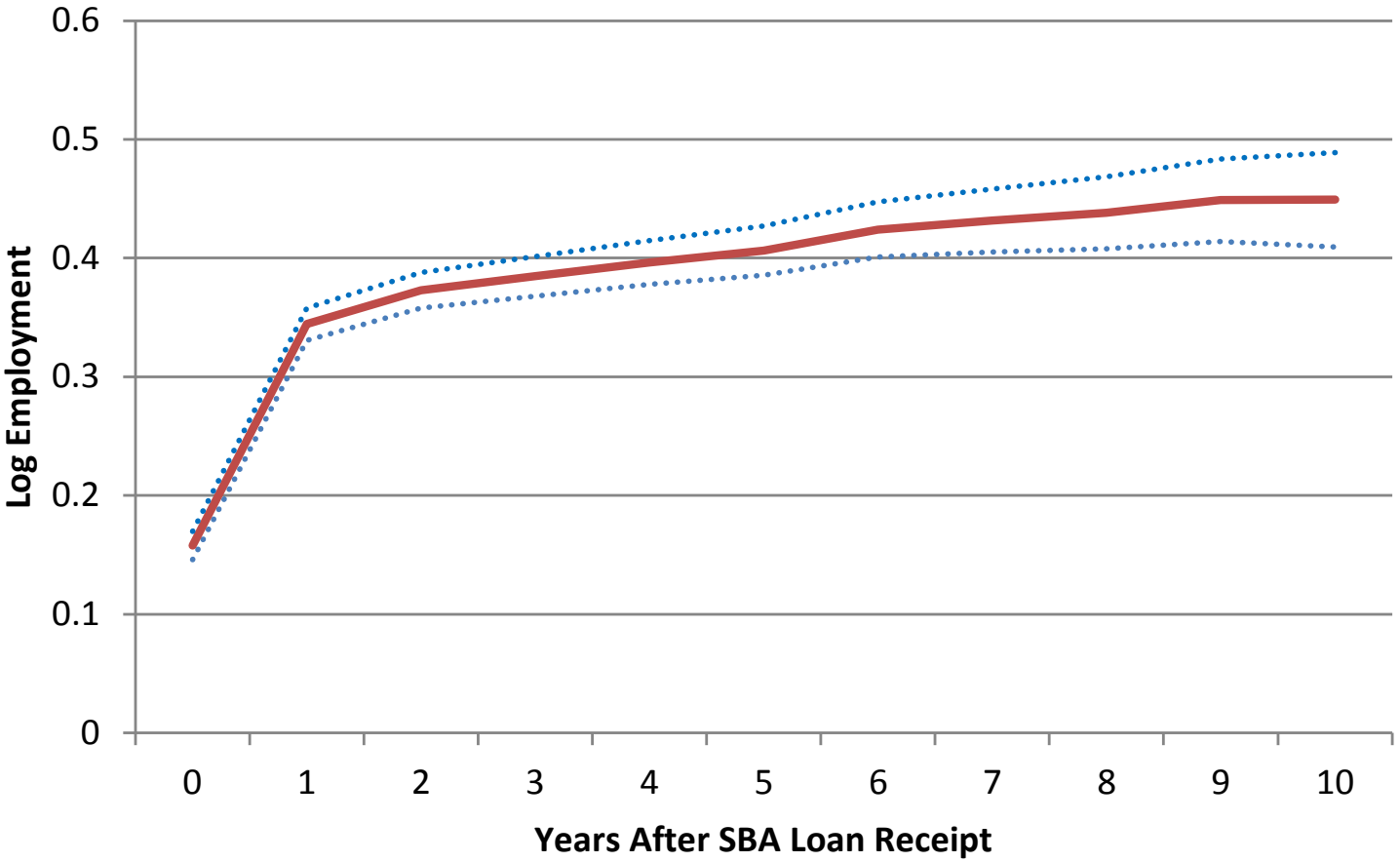
OLS regression, also including dummies common to treated and control firms for years before/after the treated firm's treatment and year effects. Years prior to four years before treatment is the base category. Dummies for years after ten years after treatment are included, but not reported. The dotted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

Figure 4. Dynamic Specification for SBA Loans After Start-Up, Fixed Effects



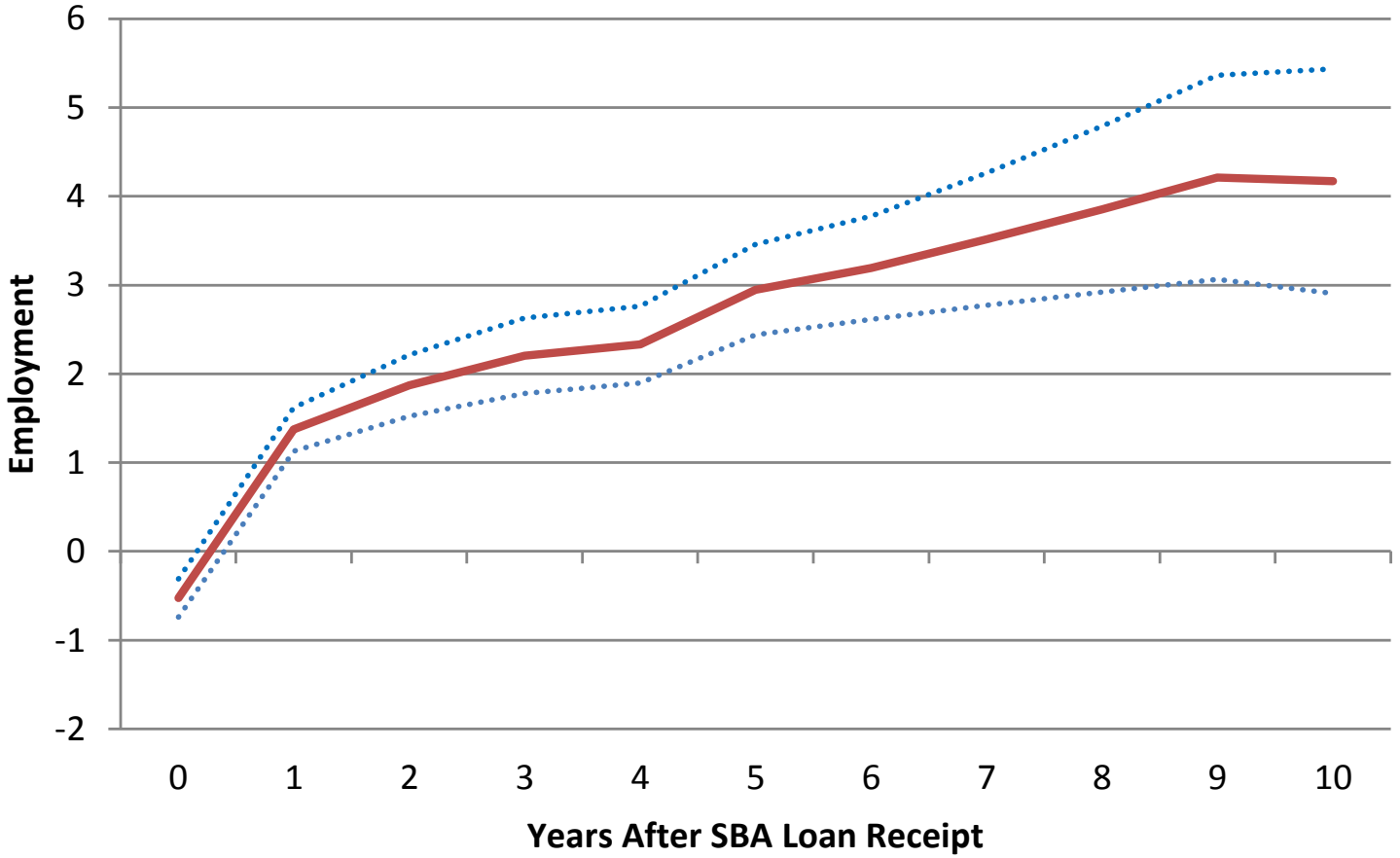
Fixed effects regression, also including dummies common to treated and control firms for years before/after the treated firm's treatment and year effects. Years prior to four years before treatment is the base category. Dummies for years after ten years after treatment are included, but not reported.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

Figure 5. Dynamic Specification for SBA Loans in Start-Up Year



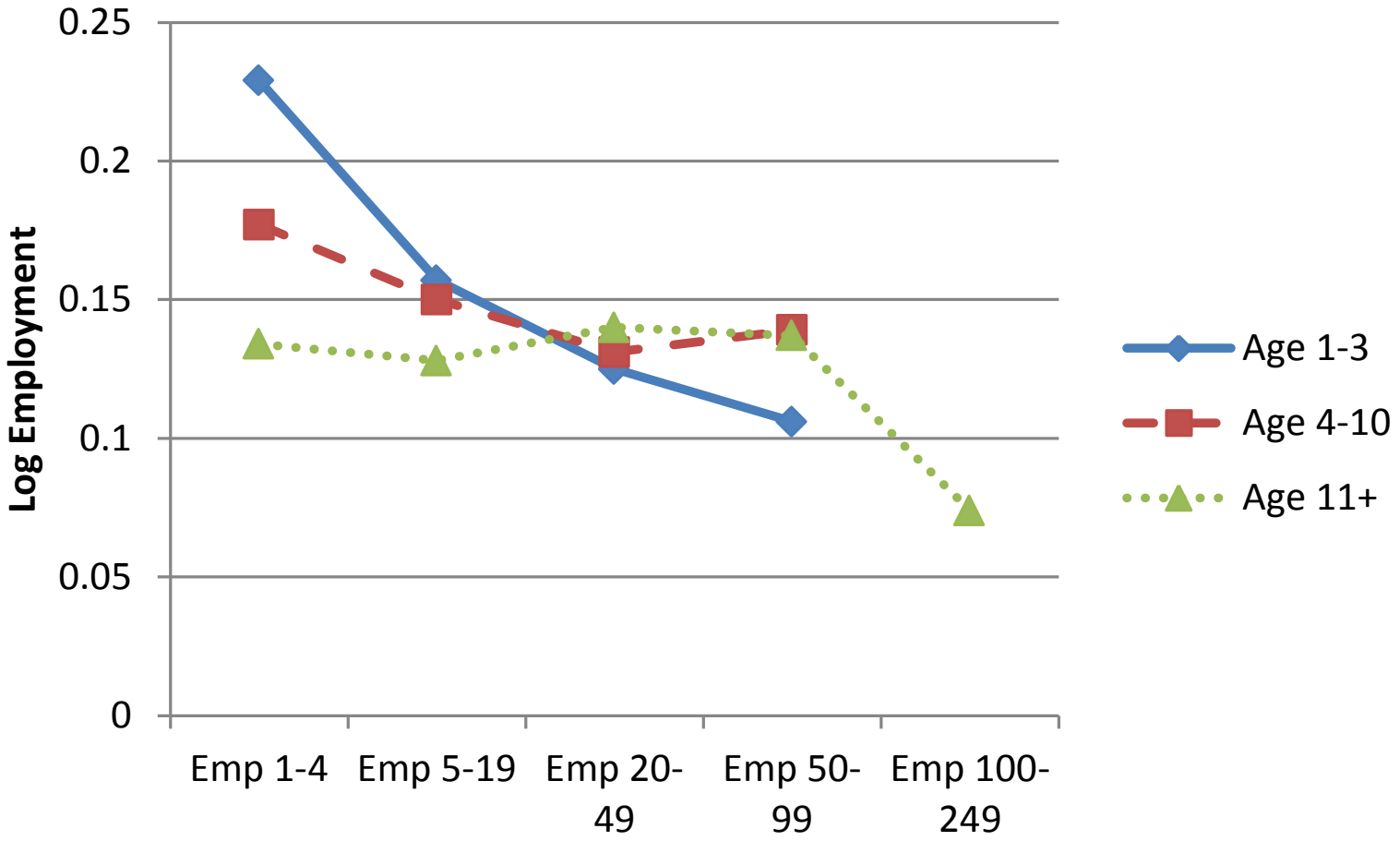
OLS regression, also including dummies common to treated and control firms for years since the treated firm's treatment (which is also age) and year effects. The dotted lines are the boundaries of the 99 percent confidence interval.

Figure 6. Dynamic Specification for SBA Loans in Start-Up Year



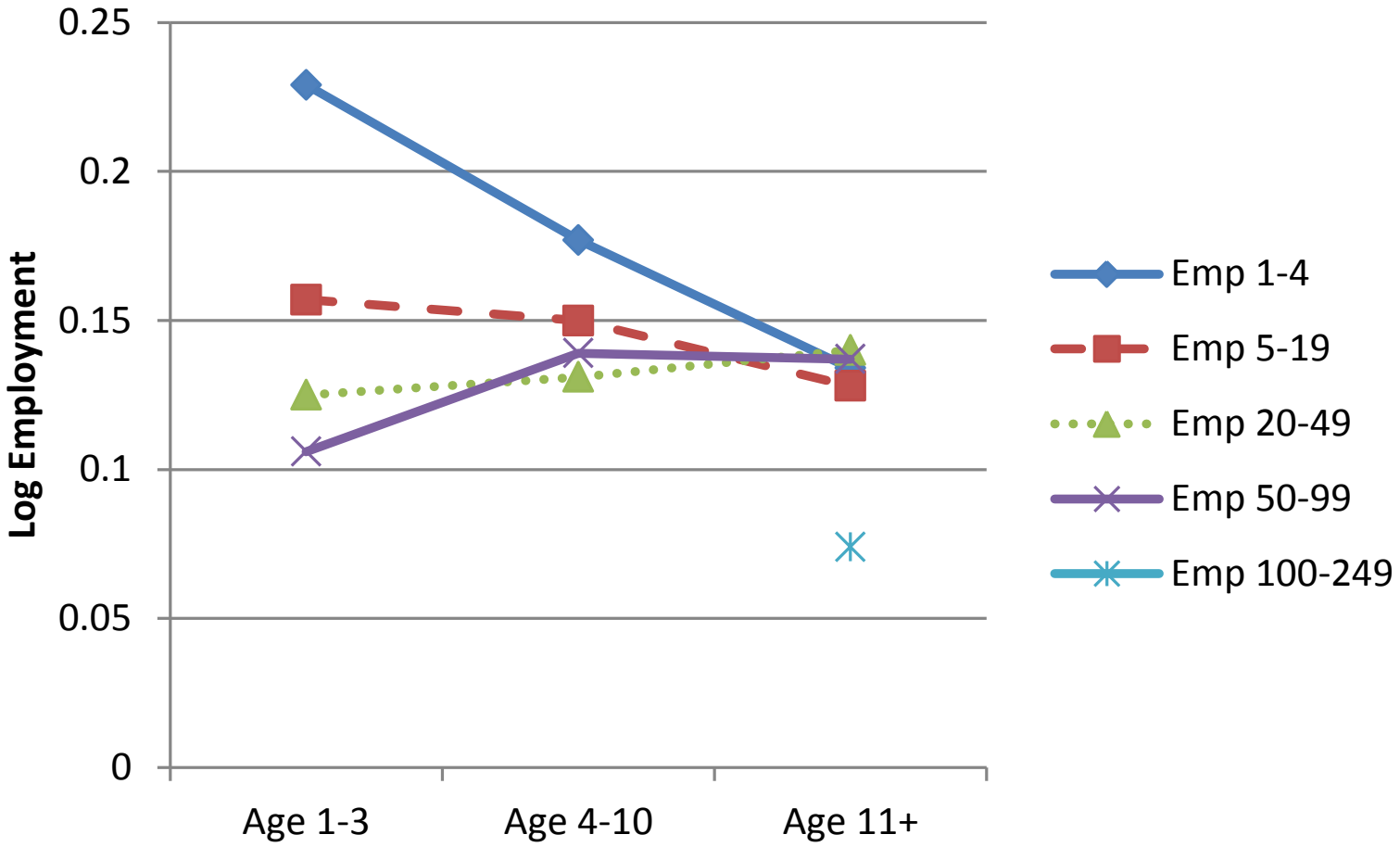
OLS regression, also including dummies common to treated and control firms for years since the treated firm's treatment (which is also age) and year effects. The dotted lines are the boundaries of the 99 percent confidence interval.

Figure 7. Size Effects Per Loan for Each Age Category, Fixed Effects Regressions



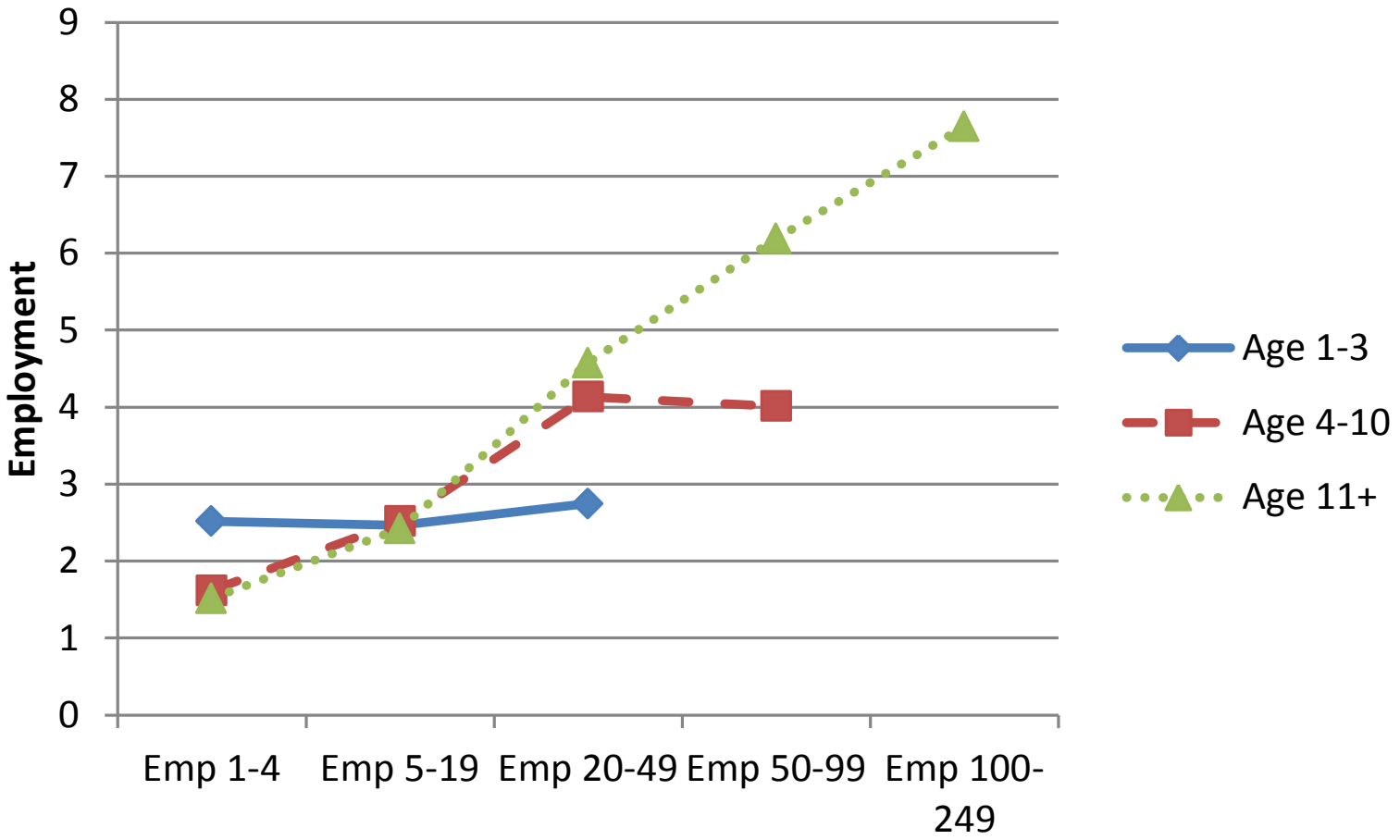
These are plots of results reported in Table 11, including only coefficients significant at the 5 percent level.

Figure 8. Age Effects Per Loan for Each Size Category, Fixed Effects Regressions



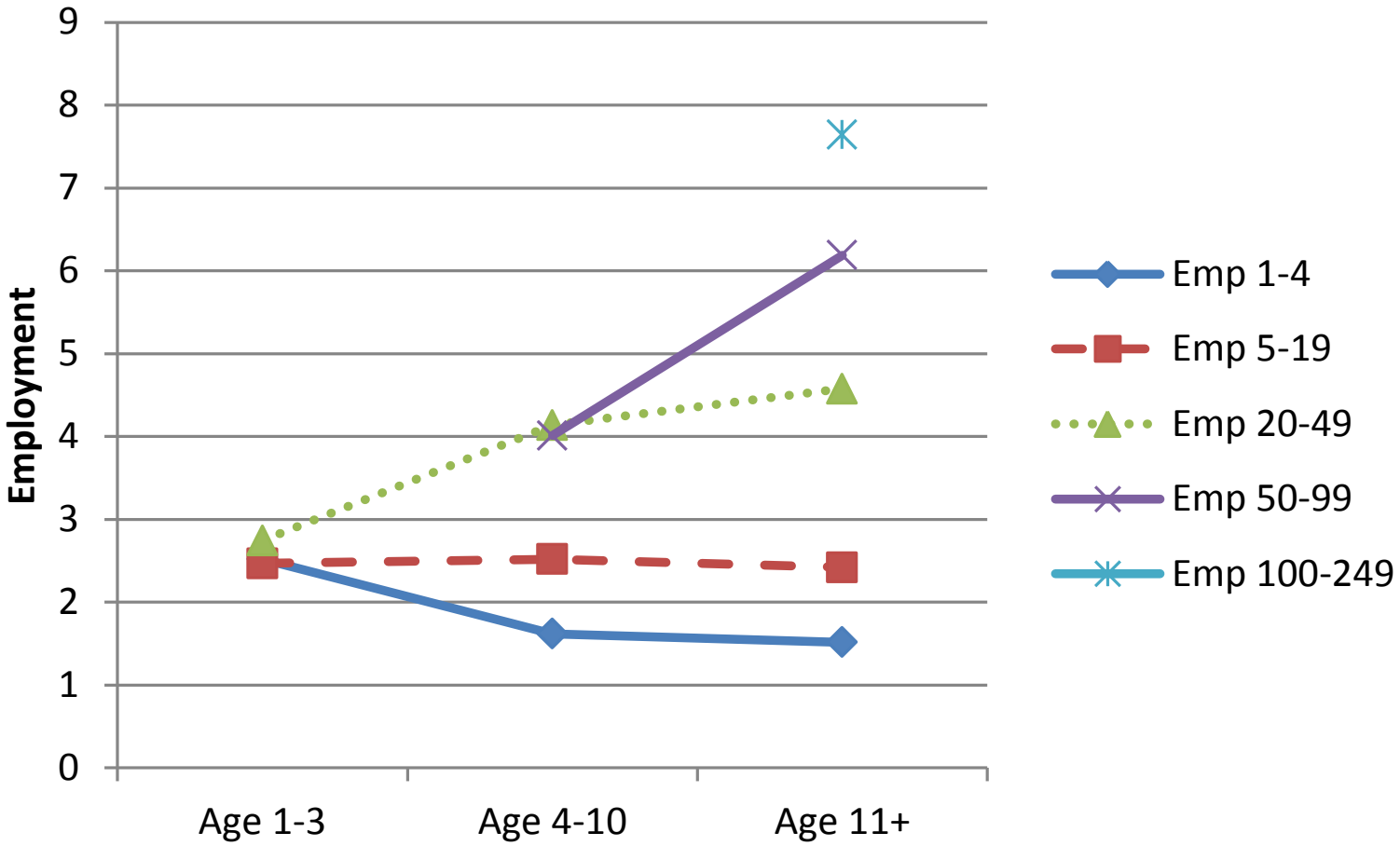
These are plots of results reported in Table 11, including only coefficients significant at the 5 percent level.

Figure 9. Size Effects per $Million Loan for Each Age Category, Fixed Effects Regressions



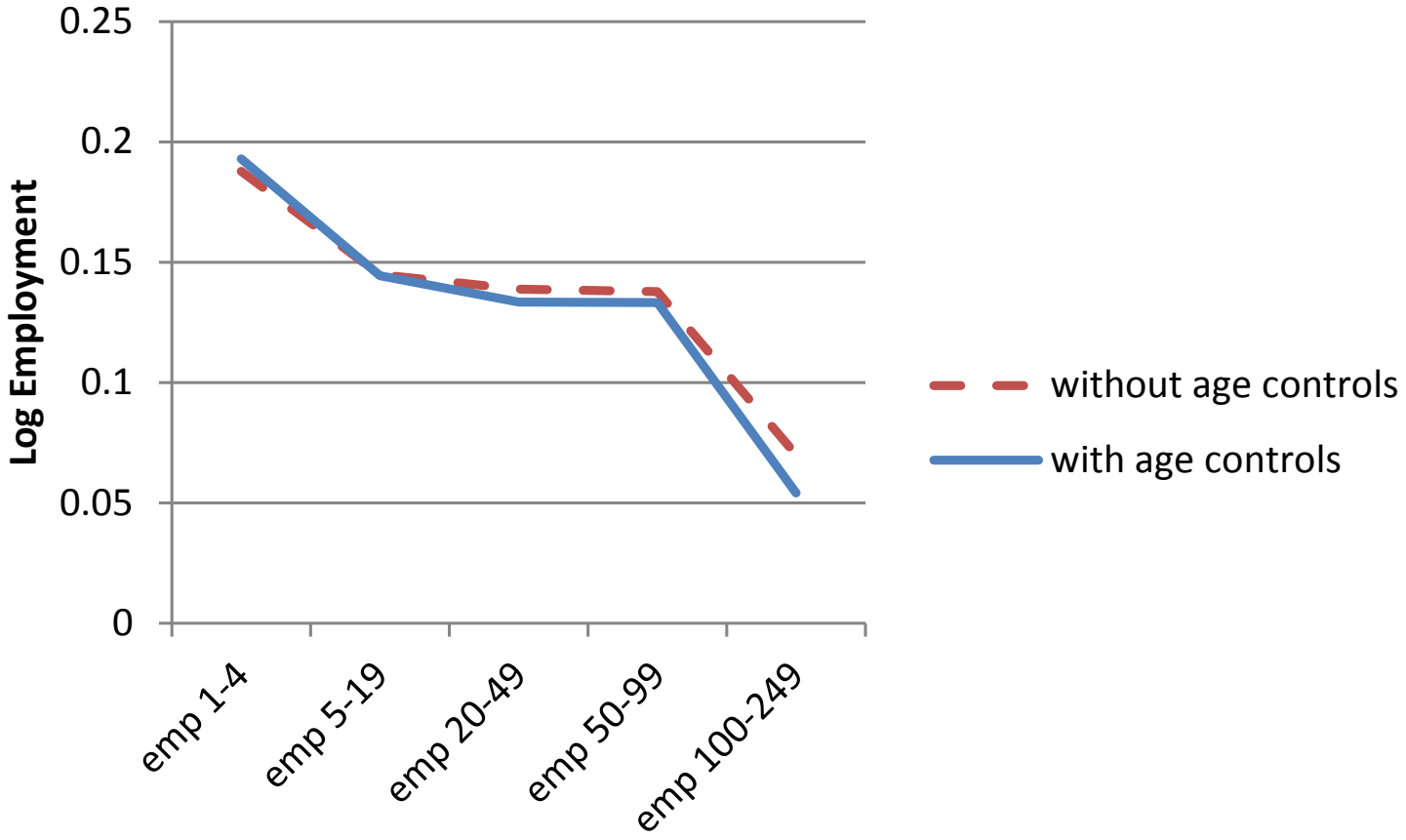
These are plots of results reported in Table 11, including only coefficients significant at the 5 percent level.

Figure 10. Age Effects Per $Million Loan for Each Size Category, Fixed Effects Regressions



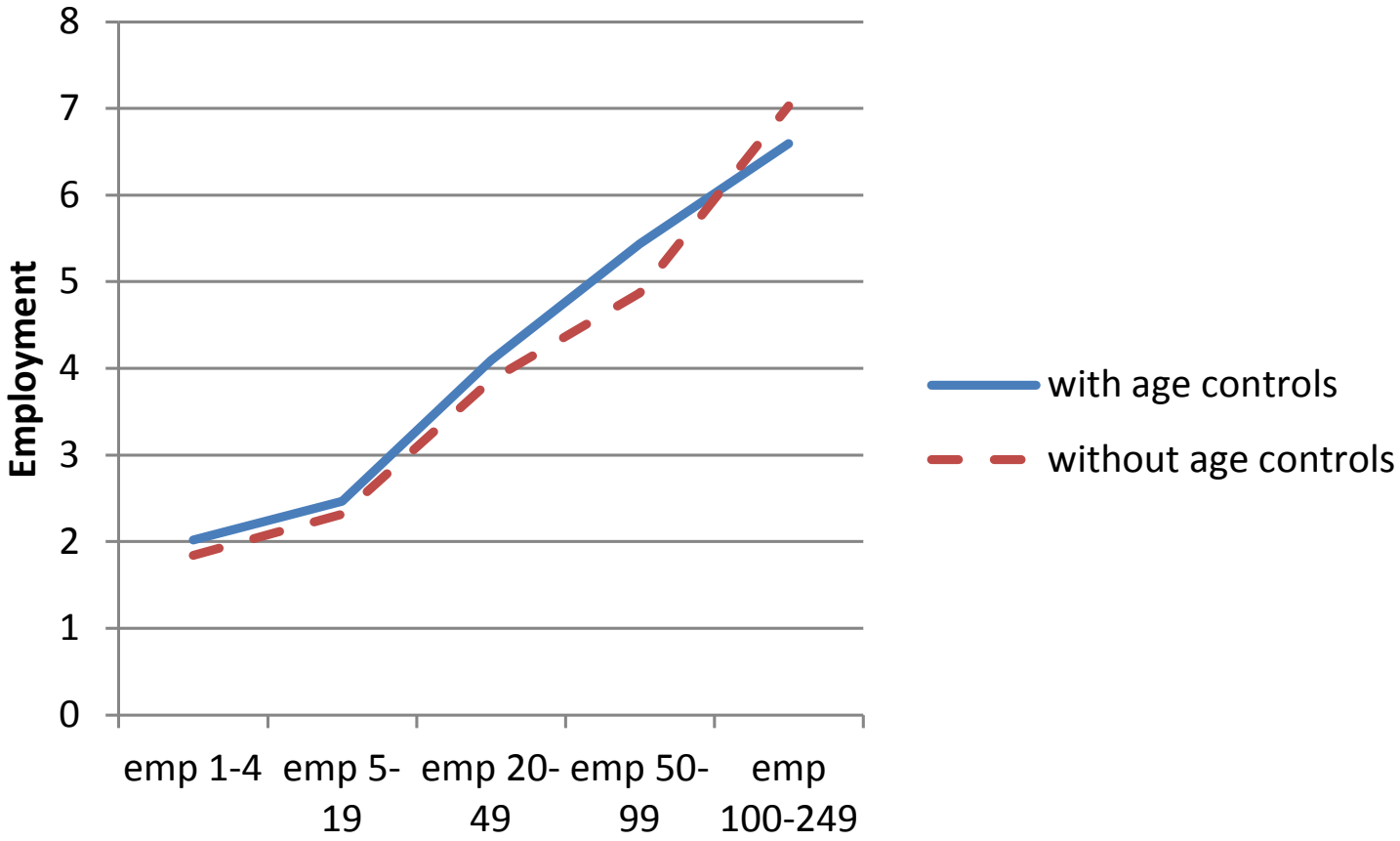
These are plots of results reported in Table 11, including only coefficients significant at the 5 percent level.

Figure 11. Effects Per Loan by Size, Fixed Effects Regressions



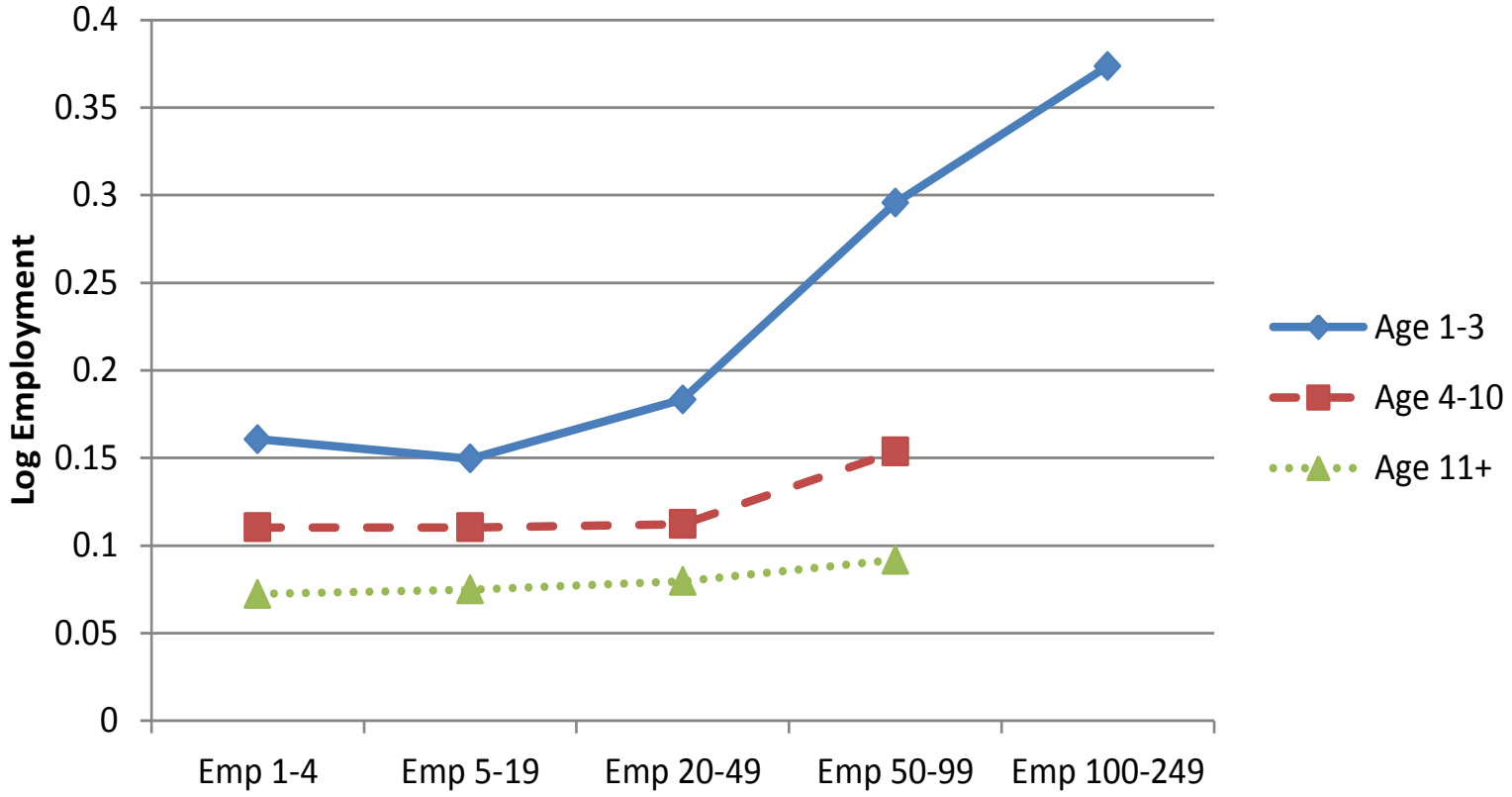
These are plots of results reported in Table 13.

Figure 12. Effects Per $Million Loan by Size, Fixed Effects Regressions



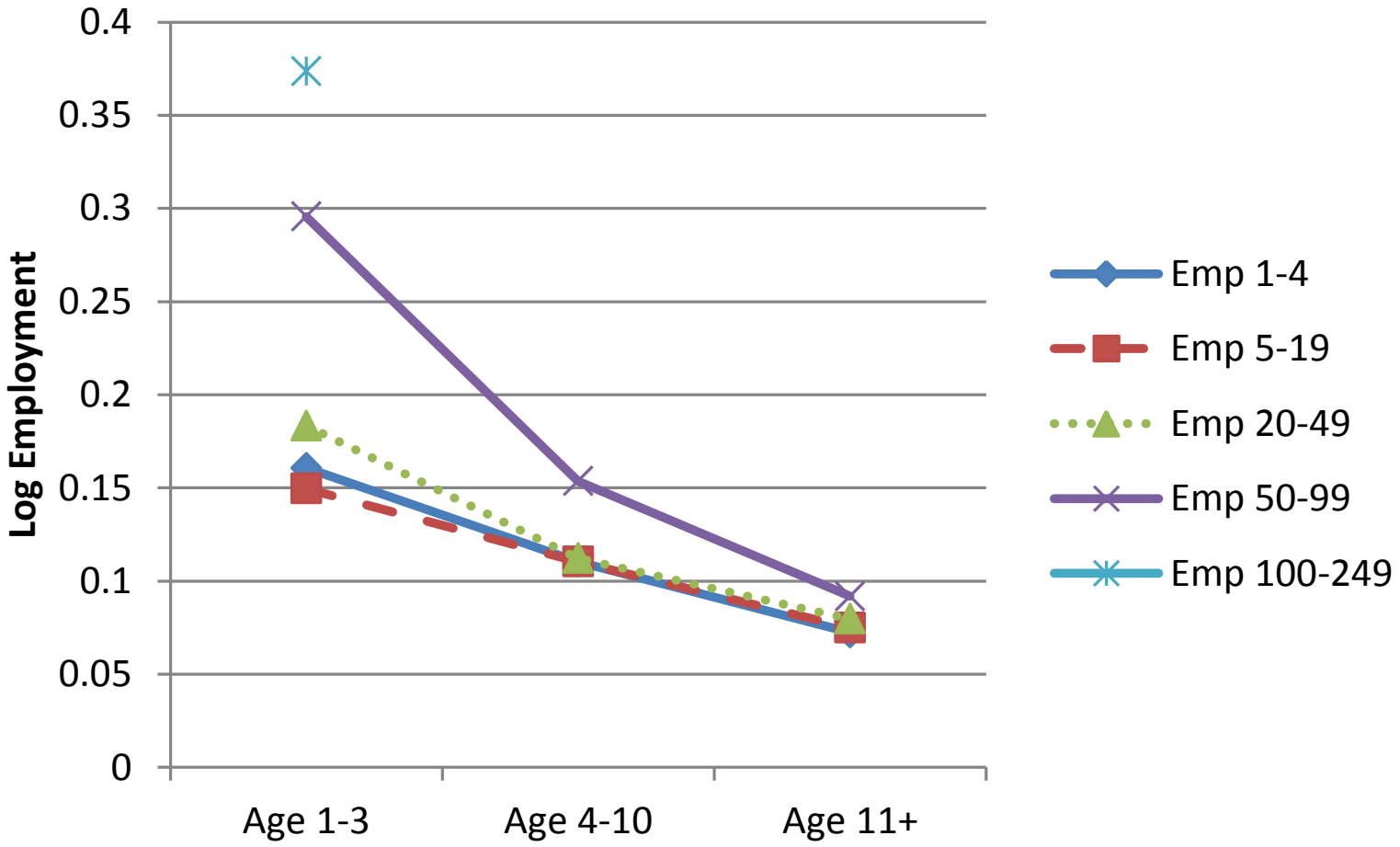
These are plots of results reported in Table 13.

Figure 13. Size Effects Per Loan for Each Age Category, Fixed Effects Regressions, One Post-Exit Observation



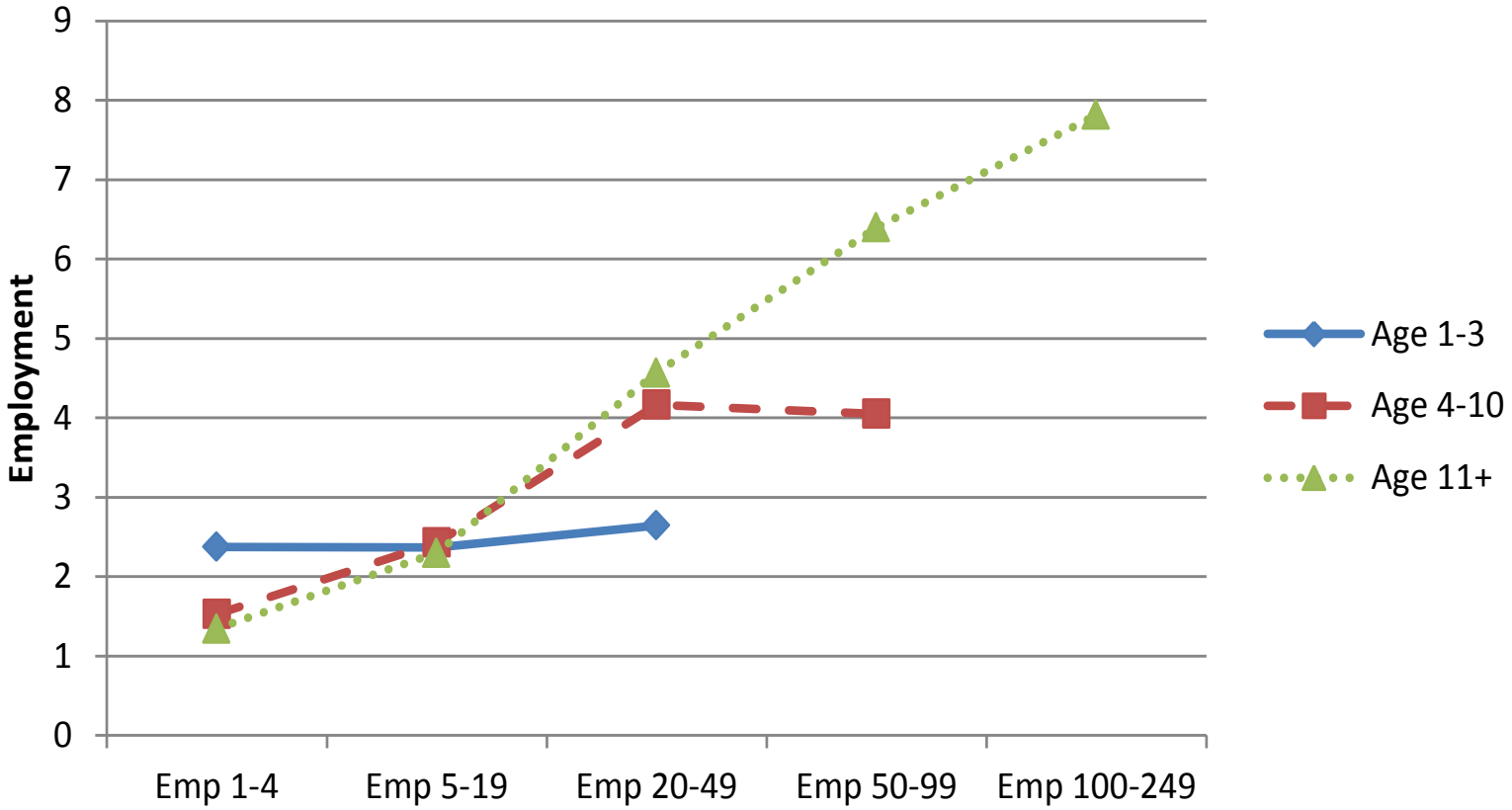
These are plots of results reported in Table 14, including only coefficients significant at the 5 percent level.

Figure 14. Age Effects Per Loan for Each Size Category, Fixed Effects Regressions, One Post-Exit Observation



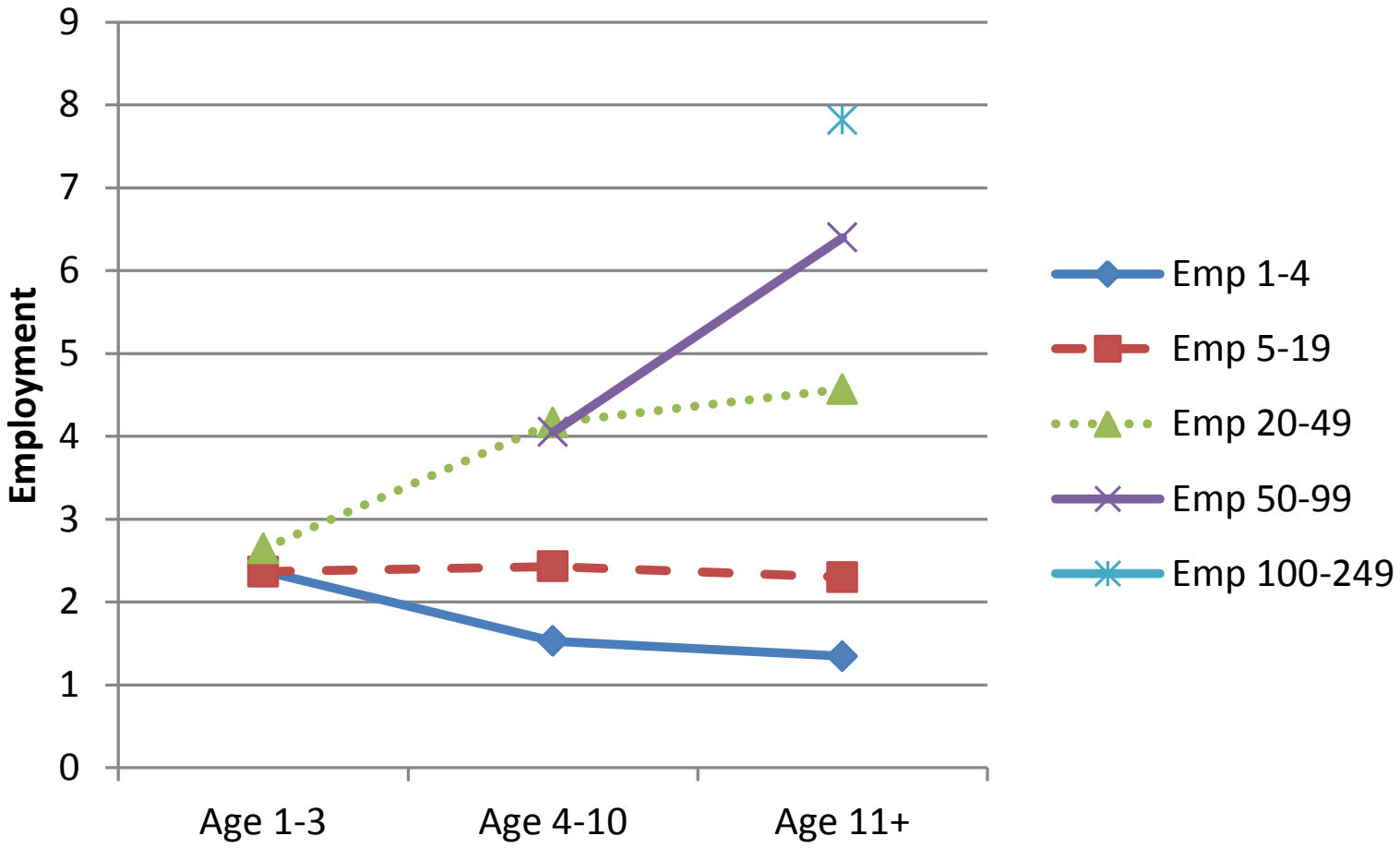
These are plots of results reported in Table 14, including only coefficients significant at the 5 percent level.

Figure 15. Size Effects per $Million Loan for Each Age Category, Fixed Effects Regressions, One Post-Exit Observation



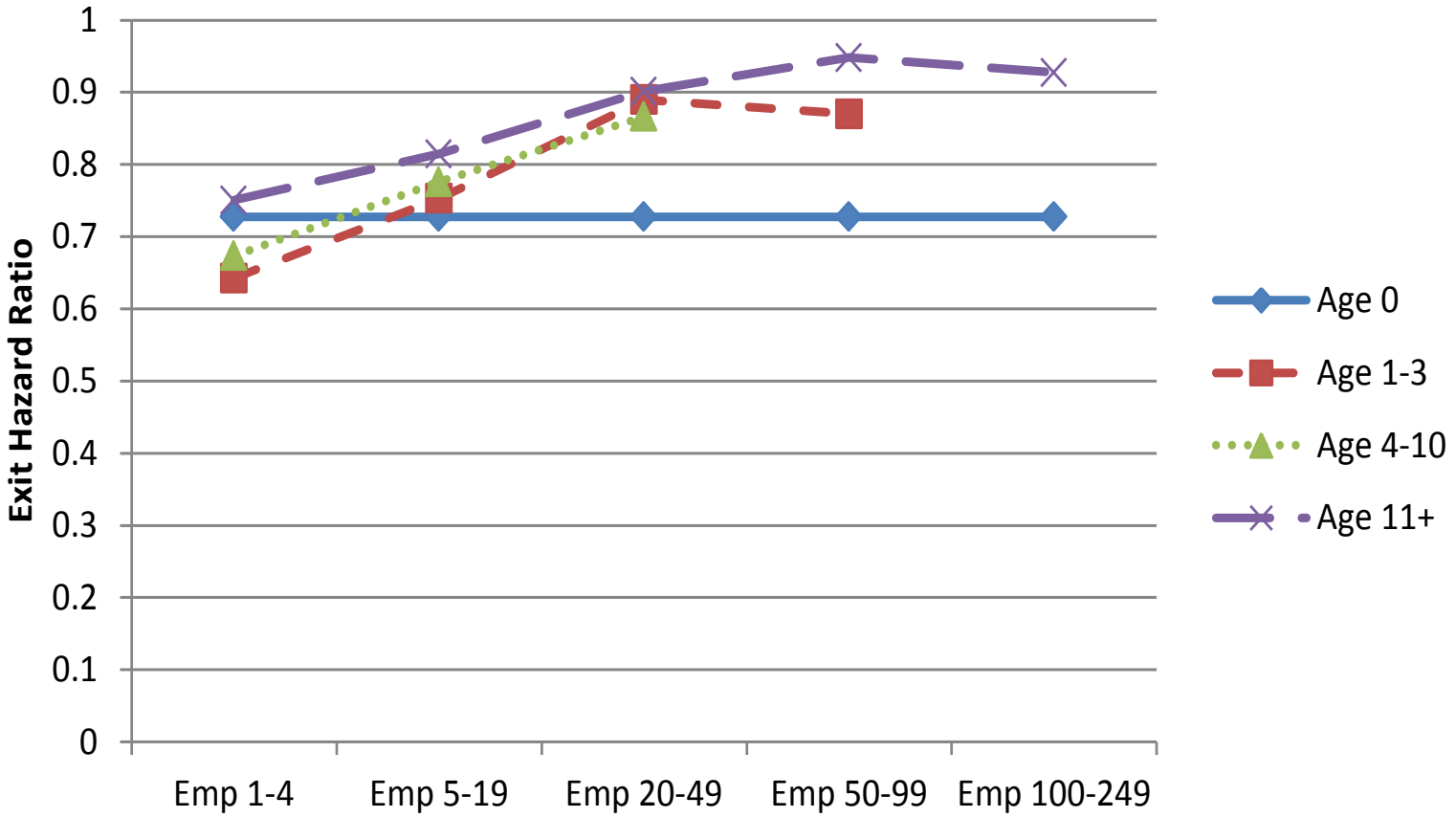
These are plots of results reported in Table 14, including only coefficients significant at the 5 percent level.

Figure 16. Age Effects Per $Million Loan for Each Size Category, Fixed Effects Regressions, One Post-Exit Observation



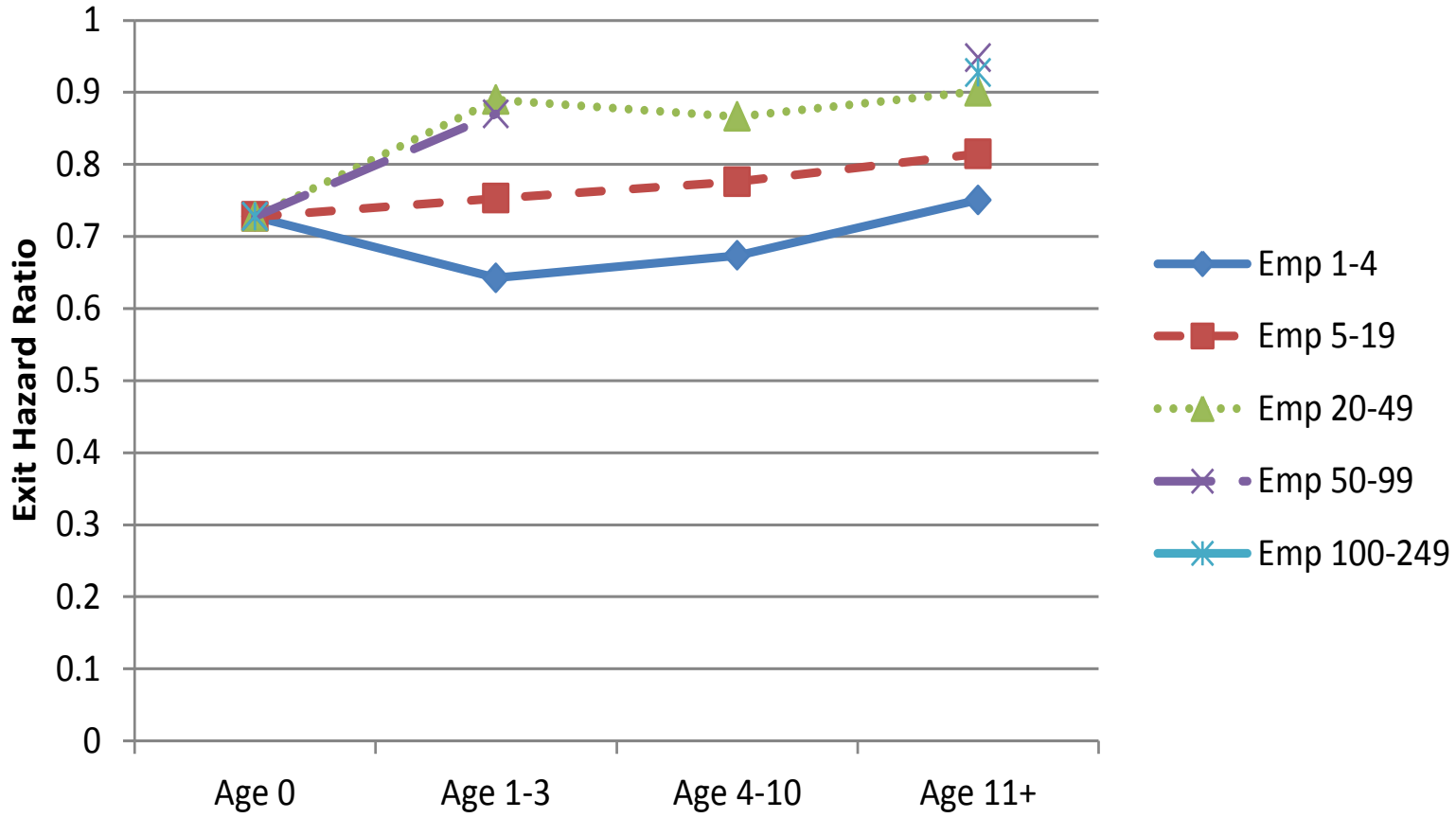
These are plots of results reported in Table 14, including only coefficients significant at the 5 percent level.

Figure 17. Survival Effects Per $Million Loan by Size for each Age Category, Exit Cox-Proportional Hazard Regressions



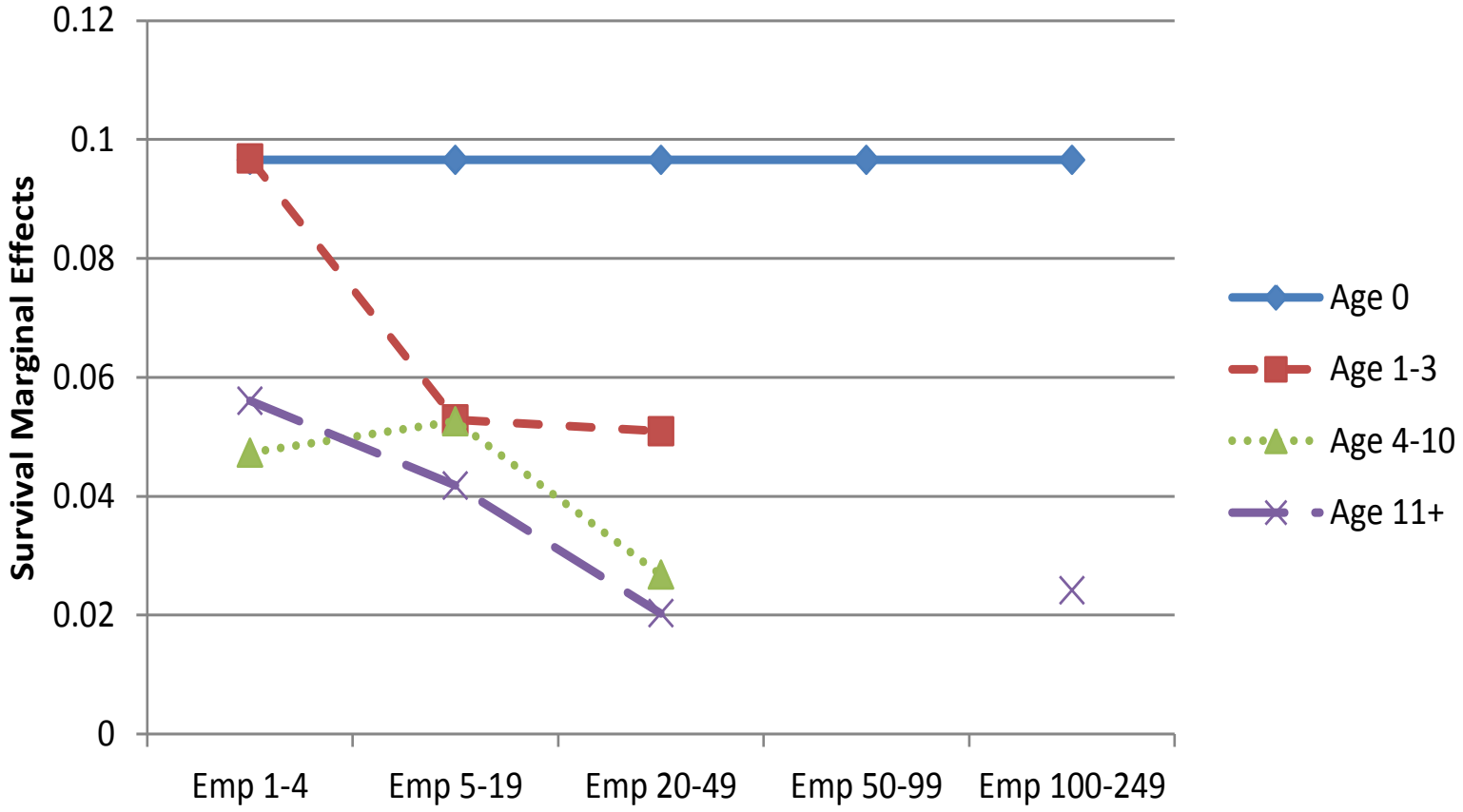
These are plots of results reported in Table 18, including only coefficients significant at the 5 percent level.

Figure 18. Survival Effects Per $Million Loan by Age for each Size Category, Exit Cox-Proportional Hazard Regressions



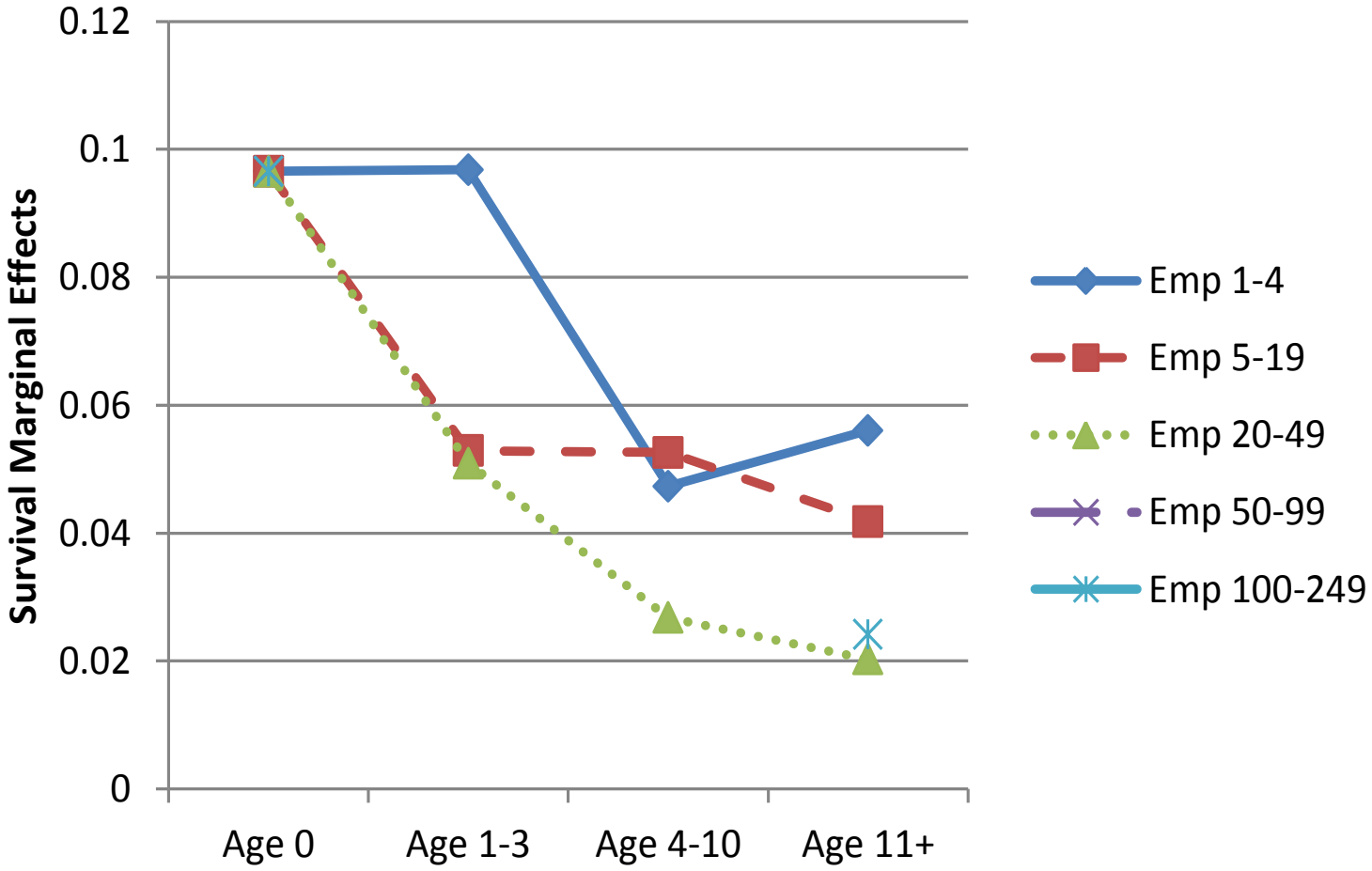
These are plots of results reported in Table 18, including only coefficients significant at the 5 percent level.

Figure 19. Survival Effects Per $Million Loan by Size for each Age Category, Probit Regressions for Survival Three Years After Loan Receipt



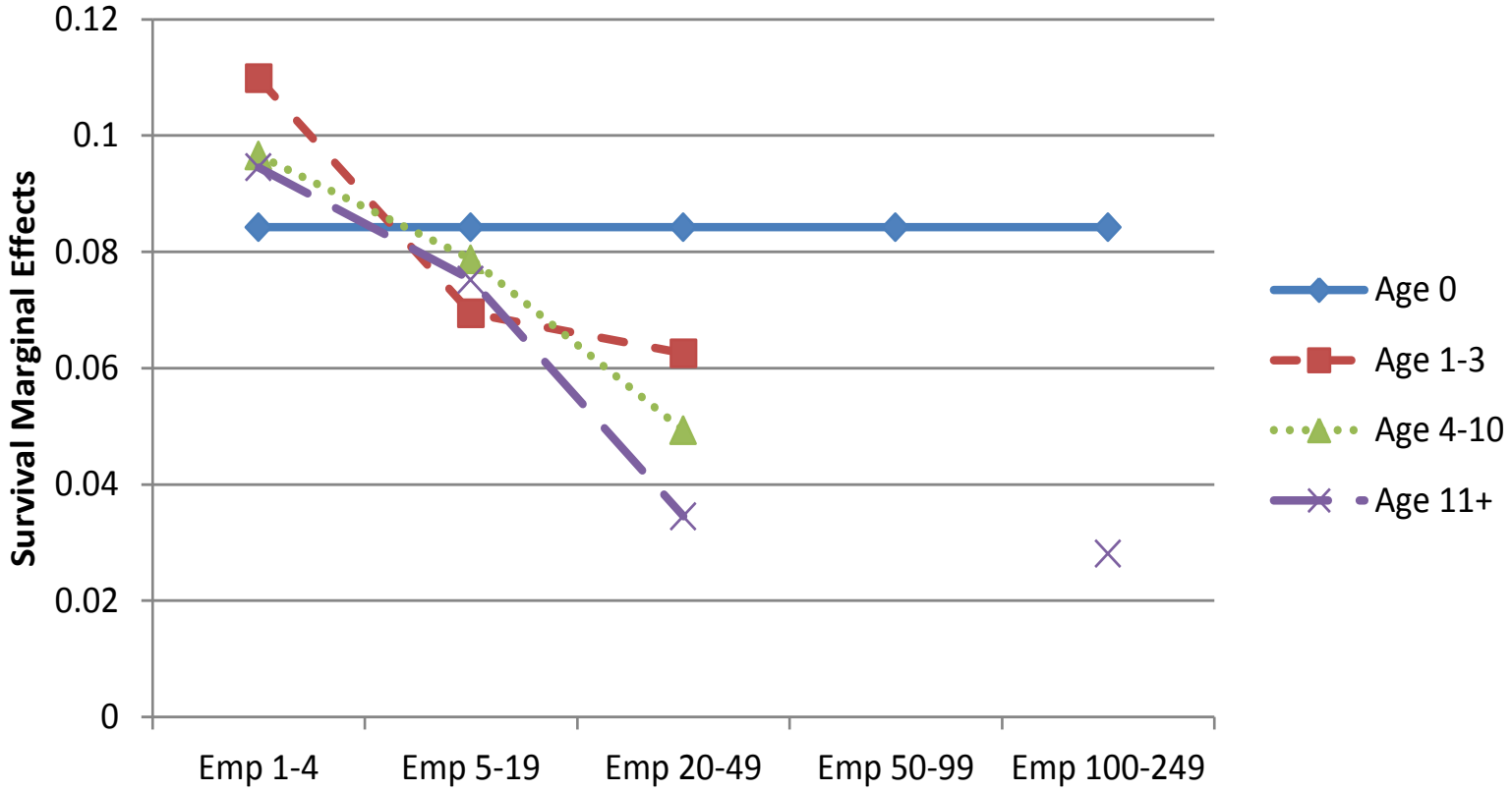
These are plots of results reported in Table 19, including only coefficients significant at the 5 percent level.

Figure 20. Survival Effects Per $Million Loan by Age for each Size Category, Probit Regressions for Survival Three Years After Loan Receipt



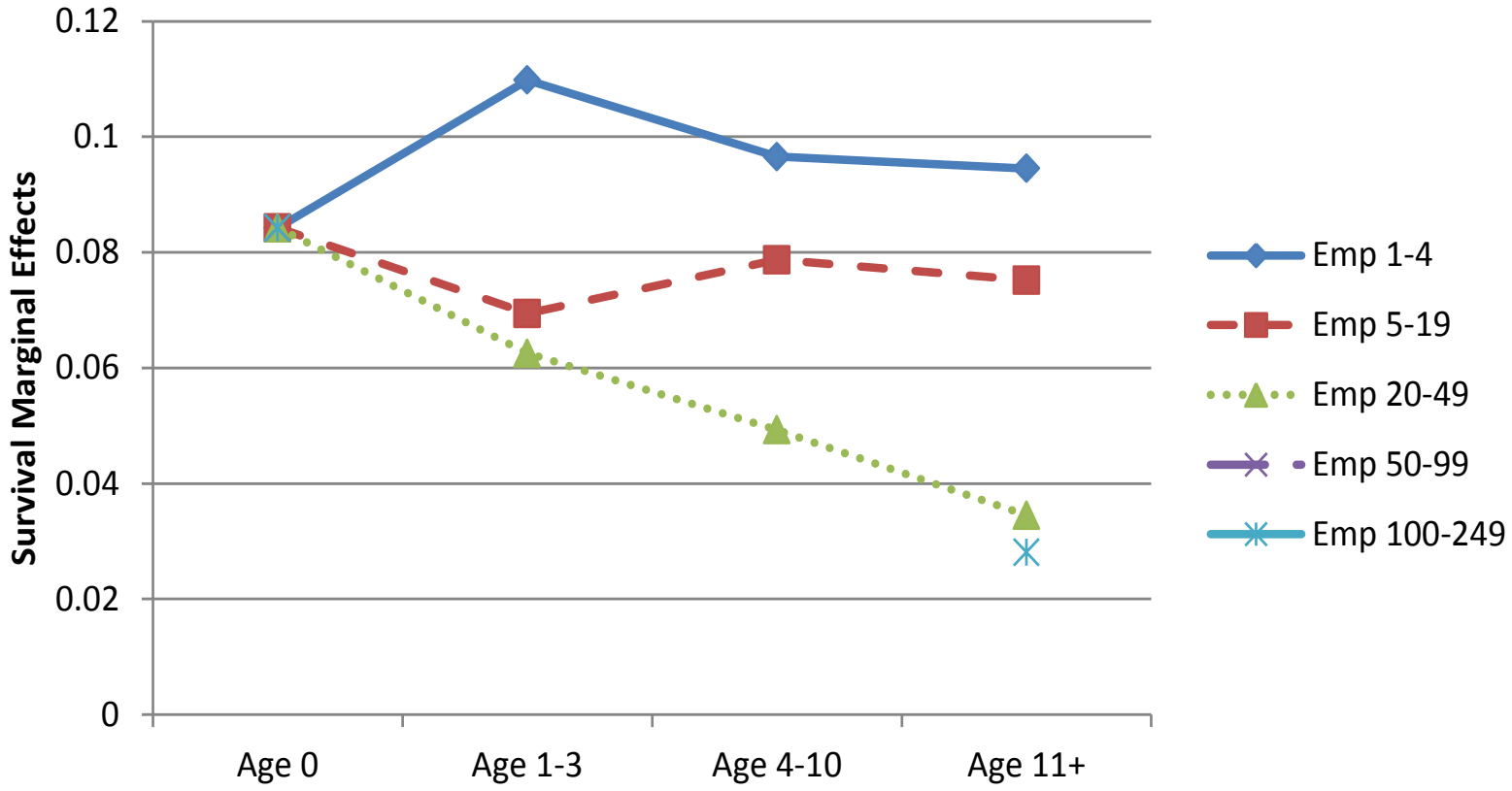
These are plots of results reported in Table 19, including only coefficients significant at the 5 percent level.

Figure 21. Survival Effects Per $Million Loan by Size for each Age Category, Probit Regressions for Survival Ten Years After Loan Receipt



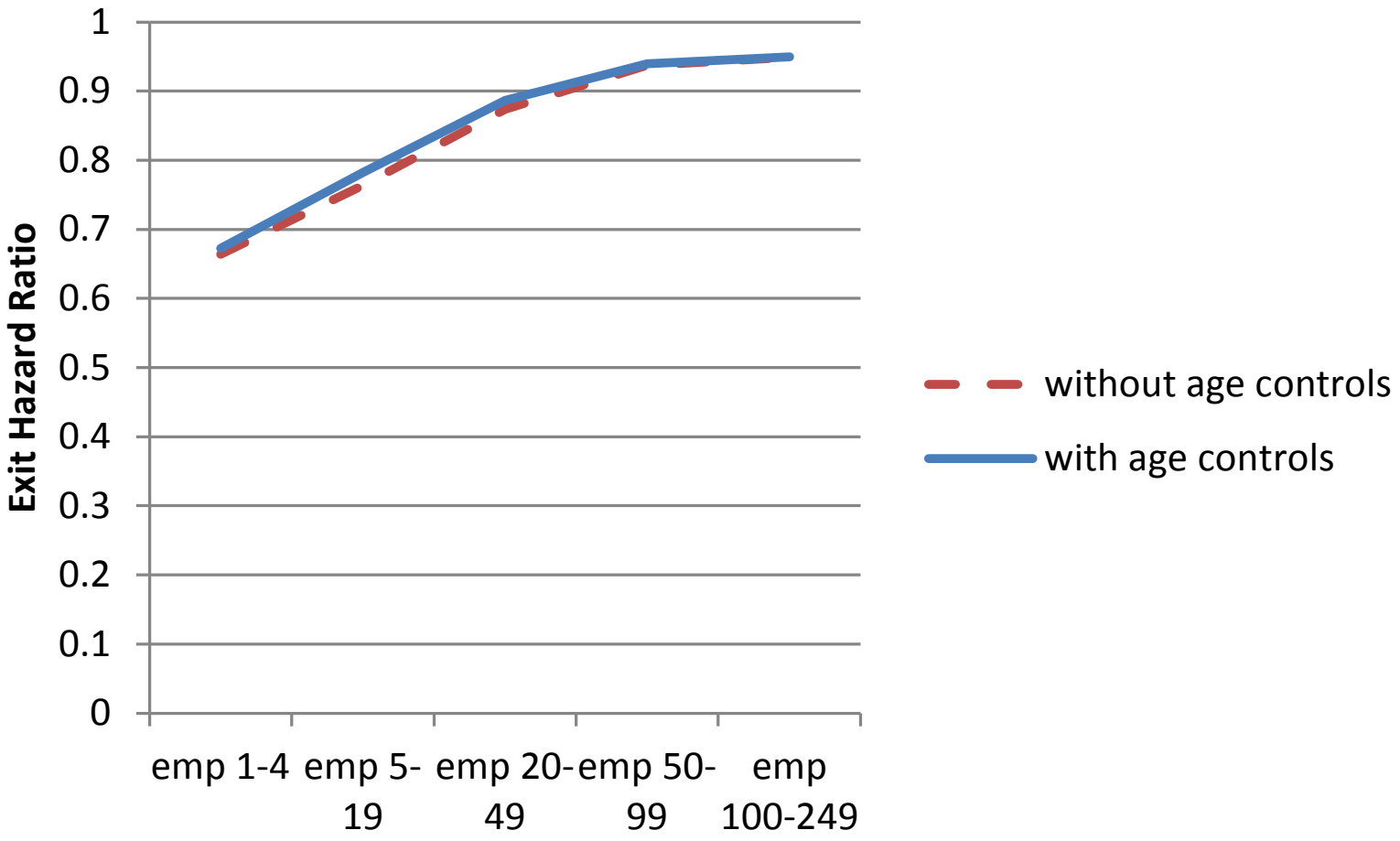
These are plots of results reported in Table 20, including only coefficients significant at the 5 percent level.

Figure 22. Survival Effects Per $Million Loan by Age for each Size Category, Probit Regressions for Survival Ten Years After Loan Receipt



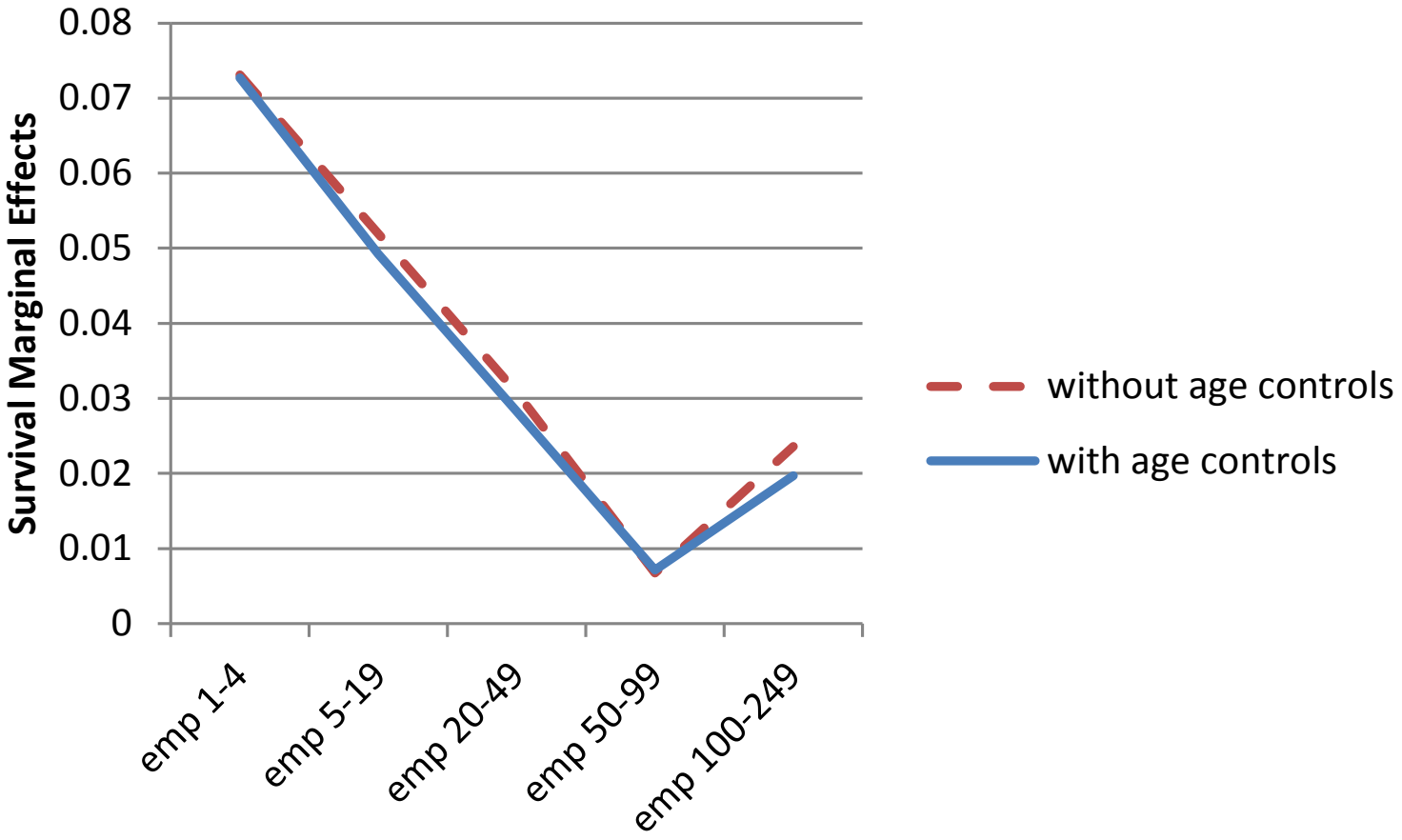
These are plots of results reported in Table 20, including only coefficients significant at the 5 percent level.

Figure 23. Survival Effects Per $Million Loan by Size, Exit Cox-Proportional Hazard Regressions



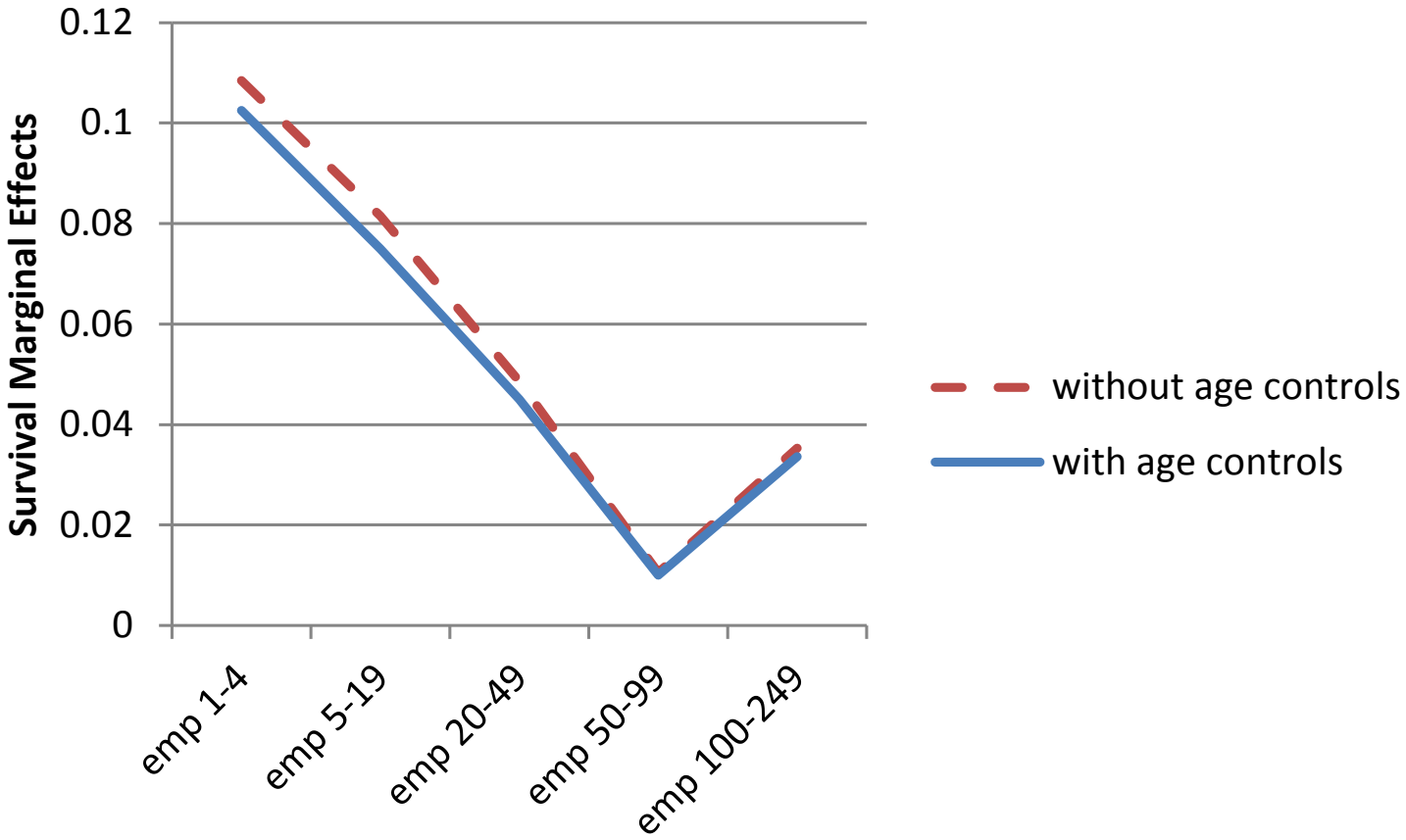
These are plots of results reported in Table 21.

Figure 24. Survival Effects Per $Million Loan by Size, Probit Regressions for Survival Three Years After Loan Receipt



These are plots of results reported in Table 22.

Figure 25. Survival Effects Per $Million Loan by Size, Probit Regressions for Survival Ten Years After Loan Receipt



These are plots of results reported in Table 23.