Credit Cycles, Firms, and the Labor Market

Michael Blank and Omeed Maghzian*

November 10, 2023

Please click here for latest version

Abstract

We use administrative data from the U.S. Census Bureau to estimate the causal effects of loose credit conditions on firm employment and worker earnings. To obtain quasi-random variation in firms' exposure to credit booms, we exploit the segmentation of high-yield (BB+ rated) versus investment grade (BBB- rated) firms in credit markets. Loose credit conditions generate cyclical fluctuations in employment: high-default risk firms create jobs during the credit boom, but then experience financial distress and destroy these jobs during the ensuing bust. We show that these firm-level boom-bust dynamics are transmitted to individual workers. To obtain quasi-random variation in workers' exposure to boom-induced job creation, we exploit the importance of parental connections in determining where labor market entrants are first employed. We find that recent high-school graduates with parents at high-yield (BB+) firms can more easily find high-paying jobs during credit booms, compared to graduates with parents at investment-grade (BBB-) firms. But ten years later, graduates whose parents were at BB+ firms have substantially lower earnings. The magnitude of these negative long-term effects is comparable to the effect of entering the labor market during a recession. Our results suggest that loose credit market conditions lead firms to create short-lived jobs that impede workers' long-run accumulation of human capital.

*Michael Blank: Harvard University, mblank@g.harvard.edu. Omeed Maghzian: Harvard University, omaghzian@g.harvard.edu. We thank our advisors Gabe Chodorow-Reich, Sam Hanson, Jeremy Stein, and Adi Sunderam for their invaluable guidance and support. We also thank Adrien Bilal, John Campbell, Veronica De Falco, Adriano Fernandes, Nathan Hendren, Spencer Kwon, Max Miller, David Scharfstein, Matt Staiger, Ludwig Straub, Johnny Tang, Boris Valleé, Hanbin Yang, and participants in the Harvard finance and macro lunches for useful comments. We are grateful to Jacob Sørensen for providing us with historical bond price data, to Nicolas Crouzet and Neil Mehrotra for sharing cleaning code for the Quarterly Financial Report, and to Matt Staiger for providing advice on implementing the parental connections design. We gratefully acknowledge support from the Molly and Domenic Ferrante Economics Research Fund and the Chae Family Economics Research Fund. This research uses data from the Census Bureau's Longitudinal Employer Household Dynamics Program, which was partially supported by the following National Science Foundation Grants SES-9978093, SES-0339191 and ITR-0427889; National Institute on Aging Grant AG018854; and grants from the Alfred P. Sloan Foundation. Any views expressed are those of the authors and not those of the U.S. Census Bureau. The Census Bureau has reviewed this data product to ensure appropriate access, use, and disclosure avoidance protection of the confidential source data used to produce this product. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2590, (CBDRB-FY23-P2590-R10522).

1 Introduction

The credit cycle features periods where lenders demand less compensation for bearing default risk (Greenwood and Hanson, 2013; Baron and Xiong, 2017; Krishnamurthy and Muir, 2020; Sorensen, 2021). Past work has found that when credit conditions are loose, households borrow more, setting off a boom-bust cycle in residential investment and consumption (Mian et al., 2013; Di Maggio and Kermani, 2017; Mian et al., 2020; Benguria and Taylor, 2020). Less is known about the impact that loose conditions have on households' *labor market* outcomes through the borrowing decisions of *firms*. Cheap credit could allow constrained firms to create valuable, long-lasting jobs. In contrast to credit boom-induced consumption, such boom-induced job creation could benefit households over both the short- and long-runs, providing a silver lining to the future effects of booms.

We use administrative micro-data from the U.S. Census bureau to show that credit booms lead firms to create significantly more jobs than they otherwise would have, and that these jobs affect the long-run earnings of the workers who take them. But instead of providing workers with the opportunity to improve their long-run labor market prospects, these boom-induced jobs reduce workers' future earnings potential. This is because credit booms primarily incentivize high default-risk firms to expand their real operations and employment. Since the temporary availability of cheap credit does not shore up risky firms' balance sheets, they destroy the jobs that they had created during the boom once credit conditions tighten. The workers who had taken these jobs are laid off and experience a persistent reduction in earnings.

To establish these results, we must confront a fundamental identification challenge: the firms and workers that create and take jobs during booms have endogenously chosen to do so. Periods of loose credit are, on average, followed by economic contractions (Jorda et al., 2013; Lopez-Salido et al., 2017; Mian et al., 2017). To the extent that recessions disproportionately affect the cash flows or financial health of risky firms, their employment growth would fluctuate more than that of less risky firms over the credit cycle even if credit conditions have no causal effect. Moreover, even if some firms were randomly more able to access cheap credit during booms, the workers who take jobs at these firms may have fewer other labor market opportunities. The resulting selection means that the workers who take boom-induced jobs would have lower future earnings even absent any causal impact of the job itself.

We overcome these challenges by exploiting segmentation in the credit market and the labor market. In the credit market, firms that issue high-yield versus investment-grade debt face markedly different investor bases (Chernenko and Sunderam, 2012). In the labor market, young workers are especially likely to take jobs at fast-growing firms where their parents work (Staiger, 2023). As we show, this segmentation helps determine which firms can access cheap credit during booms, and which workers can take the jobs that these firms create, for reasons that are unrelated to fundamentals. To exploit this quasi-random firm and worker variation, we construct a novel dataset with administrative U.S. Census data. This dataset links the credit ratings and bond issuance of the universe of public firms to the employment at these firms' establishments from 1978-2020 and to the labor market outcomes of these firms' workers in 24 states from 1998-2020.

We start in Section 2 by examining the dynamics of risky firms' employment over the credit cycle. We link the default risk of Compustat firms to the Longitidunal Business Database (LBD), a Census dataset in which we observe the employment of all establishments over 1978-2020. We then run regressions of establishment-level employment growth on firm default risk, interacted with a measure of aggregate credit market conditions that captures how aggressively default risk is priced in the corporate bond market (Sorensen, 2021). A 1 standard deviation increase in this measure implies that a firm in the highest quintile of risk experiences a 125 basis point greater reduction in its credit spread compared to a firm in the first quintile. Across the full distribution of risk, we estimate that a risky firm whose spread goes down by 100 basis points more than that of a less risky firm increases its employment growth by 2 percentage points more. But over the next five years, this firm destroys these jobs, leaving its employment at or even below its original level. Our estimates are robust to using variation in firm risk within establishments that lie in the same four-digit industry-by-city cell; these establishments are plausibly subject to similar types of aggregate shocks (eg. demand shocks), other than credit market ones.

In Section 3, we exploit segmentation in the bond market to confirm that risky firms' boom-bust employment dynamics are causally driven by credit market conditions. Regulatory frictions drive a time-varying wedge in the supply of capital directed towards high-yield (HY) versus investment-grade (IG) corporate bonds (Lemmon and Roberts, 2010; Chernenko and Sunderam, 2012). In bond-level data, we show that this creates a stark discontinuity at the HY/IG rating threshold: in response to a 1 standard deviation increase in our in credit conditions measure, spreads on the highest-rated HY bonds (BB+) go down by 45 basis points more than the spreads on bonds that are only rated one-notch higher (BBB-). We utilizes this quasi-random variation by comparing the employment growth of establishments with BB+ parent firms to the growth of an establishment that lies in the same MSA and four-digit industry but has a BBB- parent firm. In response to a credit boom that decreases the spreads of BB+ firms by 100 basis points more than BBB- firms, the establishments of BB+ firms initially experience 5 percentage point higher employment growth. This growth is then reversed over the next five years. We show that the relatively large magnitude of these estimates reflects the higher treatment effects associated with firms that face borrowing constraints, as BB+ firms do through restrictive covenants (Green, 2018). These results confirm that temporarily loose credit conditions cause risky firms to create jobs that they soon destroy.

We then turn to estimating the effects of boom-induced job creation on the short- and long-run earnings of workers. In Section 4, we first characterize which types of workers actually take these jobs. To do so, we merge our firm-level risk measures into matched worker-firm data from the Census's Longitudinal Employer-Household Dynamics (LEHD) program, which allows us to observe the jobs and quarterly earnings for the near-universe of workers in 24 states over 1998-2020. We show that young workers with little previous labor market experience are more than twice as likely to fill the jobs created by risky firms during booms than the average worker. Though the future earnings potential of such workers could be profoundly affected by taking boom-induced jobs, it is not clear which direction this should go. Even short-lived jobs could positively affect workers who have few other opportunities to gain labor market experience. On the other hand, boom-induced jobs may leave these disadvantaged workers more vulnerable to the recessions that tend to follow credit booms.

In Section 5, we show that the latter is the case for the credit cycles in our 1998-2020 LEHD sample. Workers who choose to move to risky Compustat firms experience boom-bust dynamics in their relative earnings. A worker hired by a firm undergoing a 100 basis points greater decrease in spreads initially experiences 2 to 5% higher earnings growth compared to a worker hired by a lower-risk firm. Three years after the hire, this effect plunges to -4 to -6%, before starting a slow, incomplete recovery. This crash in earnings reflects the transmission of risky firms' financial distress to workers: the crash occurs at the same time as the reversal in firm employment growth, and is associated with an upwards spike in the probability that the worker is laid off. We find these boom-bust dynamics for workers even when we non-parametrically control for a rich set of worker demographics and previous labor market outcomes, or when we compare workers that move to BB+ firms to observably similar workers that move to BBB- firms.

To ensure that the negative long-term effects of boom-induced jobs on earnings are not driven by the selection of particular workers into these jobs, we turn to our final, most rigorous specification. We develop a design that approximates the ideal experiment in which the spreads of some firms are randomly more responsive to aggregate credit conditions, and only some randomly-selected workers are able to take the jobs that these firms create. Our design is based off a striking feature of the labor market: 5% of workers in the LEHD obtain their first full-time job at a firm at which one of their parents works.

Staiger (2023) shows that workers are especially likely to join their parent's firm if it happens to be hiring at the time of their high-school graduation. This, combined with the fact that BB+ firms are more exposed to credit booms than BBB- firms, suggests an instrument for if a worker takes a boom-induced job: whether a recent high-school graduate's parents work at a BB+ firm or a BBB- firm. Even though this variation does not condition on workers' actual labor market decisions, it is a strong predictor of which workers are hired by BB+ firms. During a credit boom in which a BB+ firm experiences a 100 basis points greater decrease in spreads than a BBB- firm, a recent graduate with a BB+ parent is 400 basis points more likely to obtain a job at their parent's firm than a graduate with a BBB-. Our IV estimates then imply that starting one's career at a boom-induced job causes earnings to be 5% higher two years after high-school graduation, but 8% lower ten years after graduation.

In Section 6, we conclude by fleshing out two important implications of these results. First, we discuss how the credit booms in our sample caused risky firms to create jobs that led young workers to, in effect, borrow against their future labor income. Over our 1998-2020 sample period, the future consequences of taking a boom-induced job (−8%) are around the same as the long-run effects associated with graduating high school during a recession (Schwandt and von Wachter, 2019). The higher initial earnings from boom-induced jobs came at the expense of impeding workers' accumulation of valuable skills and job security, leaving them almost as exposed to recessions as workers without any experience. Second, we present findings that suggest that the credit booms in our sample had negative long-term effects for the average worker in the economy, not just those who took the jobs created by risky firms. We discipline the aggregate implications of our results by applying our cross-sectional firm and worker estimates to two exercises: a partial equilibrium aggregation procedure (Chodorow-Reich, 2014) and a comparison to estimates from a regional design (Mian et al., 2022). We find little evidence that temporary credit booms induce low-risk firms to create jobs, or allow some workers to find more stable jobs.

1.1 Related work

Our paper shows the connection between two important, but largely separate, strands of literature. The first is the literature that documents the predictive power of measures of credit market looseness for future downturns and financial crises .[1] The second strand is work that has estimated large firm-level em-

[1] This includes Schularick and Taylor (2012) Borio et al. (2016), Lopez-Salido et al. (2017), Mian et al. (2017), Baron and Xiong (2017), Kirti (2018), Krishnamurthy and Muir (2020), Greenwood et al. (2022), and Müller and Verner (2023). Most related to our work, Borio et al. (2016) find that booms in aggregate credit tend to reallocate labor towards lower-productivity sectors, and Müller and Verner (2023) find that credit booms only forecast future declines in real activity when the credit goes towards households or firms that produce non-tradable goods. Our work builds on these two papers by evaluating the causal link between boom-time reallocation and future costs. Our micro data allows us to establish an important role for the impact that

ployment effects of negative shocks to credit supply,[2] with some papers also finding evidence of negative effects on the firms' previous workers (Ponticelli et al., 2022; Graham et al., 2023; Zullig, 2022). Our paper shows that the exposure of workers to deteriorating credit supply conditions is in part due to the labor demand and supply choices made during the boom phase of the credit cycle. This provides evidence for a novel, human capital-based mechanism by which booms can cause persistent future downturns, and shows that financial distress-driven labor market shocks partially reflect increased risk-taking by both firms and workers. In this regard, the paper that relates the most to ours is Giroud and Mueller (2021), which finds, also using the Census's LBD dataset, that growth in a firm leverage is associated with initial employment growth that is later reversed. Our work builds on this finding by showing that for risky firms, these boom-bust cycles are synchronized across historical credit cycles; are causally driven by a loosening of credit supply; and impose future costs on the workers who are hired during the boom phase.

Our work also relates to the empirical macro-finance literature that tests models of financial acceleration – financially-constrained firms amplifying business cycles through greater responsiveness to aggregate shocks (eg. Bernanke et al., 1999) – which has yielded inconclusive results. While some papers find that the investment, sales, and/or employment of more constrained firms[3] are more sensitive to monetary shocks, other papers find evidence of a *lower* responsiveness of small firms.[4] Crouzet and Mehrotra (2020), in the first paper to use the micro-data for a representative set of private and public firms in the U.S. (the QFR), find little evidence for a relationship between any of these proxies and a firm's cyclicality. Our paper's results can help make sense of the contrasting findings in this literature. Because these papers use cyclical measures such as GDP growth or monetary shocks that commingle nonfinancial and credit market conditions, if constrained firms react less strongly to nonfinancial conditions, the pooled estimate of constrained firm sensitivity could be positive, negative, or zero. Our work thus points to the empirical, and potentially also theoretical, value of distinguishing between fluctuations in nonfinancial

credit cycles have on worker-level human capital distinct from the general equilibrium-based reallocation frictions in models of capital inflows, such as Kalantzis (2015) and Schmitt-Grohé and Uribe (2016), that could explain these papers' cross-country findings.

[2]This includes Chodorow-Reich (2014),Falato and Liang (2016),Huber (2018),Bentolila et al. (2017),Berton et al. (2018),Benmelech et al. (2019), and Benmelech et al. (2021)

[3]This includes papers in which the degree to which a firm is constrained is proxied by small size (Gertler and Gilchrist, 1994), young age (Cloyne et al., 2023), high leverage (Sharpe, 1994; Chava and Hsu, 2019), usage of bank debt (Ippolito et al., 2018), or low liquidity (Jeenas, 2018)

[4]This includes papers in which the degree to which a firm is constrained is proxied by small size (Gopinath et al., 2017) or high-leverage (Ozdagli, 2017; Ottonello and Winberry, 2020) Contrasting results are found even among studies that focus on the differential cyclicality of just employment over the U.S. business cycle with respect to firm size: Moscarini and Postel-Vinay (2012) estimate that large firms' employment growth is more sensitive to aggregate conditions, while Fort et al. (2013) and Haltiwanger et al. (2018) estimate that, conditional on age and using different cyclical indicators, it is smaller firms that are more sensitive.

vs. credit market conditions.[5]

Finally, our paper contributes to the macro-labor literature on worker reallocation and displacement over the business cycle. Past work has found that economic expansions promote movements into the labor force up the "job ladder" – labor reallocation towards high-paying, productive firms (Moscarini and Postel-Vinay, 2018; Haltiwanger et al., 2018, 2021) – especially for younger workers (Haltiwanger et al., 2018) and when the unemployment rate is already low (Aaronson et al., 2019; Bergman et al., 2020). Our findings suggest that when macroeconomic strength is in part driven by increased risk-taking in credit markets, such benefits may be temporary, and actually impose longer-run costs on younger workers, via increasing workers' exposure to the large adverse consequences of entering the labor market (Schwandt and von Wachter, 2019; von Wachter, 2020) or being laid off (Jacobson et al., 1993; Davis and von Wachter, 2011) during a recession.[6] In suggesting that loose credit conditions have such intertemporal effects on workers, our findings are also relevant for the literature on optimal macroprudential monetary and regulatory policies, for which the extent and persistence of real output costs caused by increased risk taking is an important input.[7]

2 Firm employment dynamics over credit cycles

In this section, we describe the employment dynamics of risky firms over credit cycles. In Section 2.1, we present a reduced-form framework in which credit booms can directly affect firms' employment only by lowering their credit spreads. In Section 2.2, we show that during credit booms, risky firms issue significantly more debt in response to reductions in their spreads. This motivates our cross-sectional empirical design, in which we compare the employment growth of risky firms to the growth of less risky firms as credit conditions fluctuate. After describing our establishment-level LBD data in Section 2.3, in Section 2.4 we state the orthogonality conditions that default risk must satisfy in order to identify the causal impact of credit booms. Since default risk is plausibly related to other determinants of employment growth over credit cycles, we refer to the results in this section as our OLS estimates. We first show results for Compustat firms in Section 2.5, and then show estimates obtained on a representative sample of public

[5] This relates to the classic distinction made in the literature between the balance sheet and bank lending, as discussed in Bernanke and Gertler (1995), as well as models of credit cycles, such as Greenwood et al. (2019), which emphasize and explain the lack of perfect synchronization between business and credit cycles

[6] This contribution is also related to the findings of Laeven and Popov (2016) and Charles et al. (2018) that the early 2000s housing boom made high school graduates more likely to forgo higher education and instead take housing-related jobs. Our work differs from these papers in that it focuses on the effect of credit conditions in particular over several historical credit cycles, and quantifies both boom-time gains and future losses using administrative panel data.

[7] Papers in this literature include Svensson (2017), Gourio et al. (2018), Kashyap and Stein (2023), Dávila and Walther (2023), Simsek and Caballero (2023), Fontanier (2022)

and private manufacturing firms in Section 2.6. In Section 2.7, we discuss the economic magnitudes and mechanisms of our results.

2.1 Empirical framework

Environment In our framework, firms are heterogeneous and differ in their exposure to the credit cycle and the business cycle. The state of the credit cycle is in a year t is given by c_t, with higher values corresponding to looser credit conditions (lower risk premia). The nonfinancial (business cycle) state z_t captures all aggregate conditions outside of the credit market that can influence real firm outcomes, such as the level of aggregate demand. Firms are indexed by f, with state variables x_{ft}. The firm's credit spread, $s(c_t, x_{ft})$, is defined as the interest rate that the firm must pay on a marginal unit of debt that it can issue in excess of the rate on safe (default-free) debt of the same maturity. The credit spread depends on the interaction between aggregate credit market conditions and firm-specific characteristics.[8]

Firm labor dynamics We denote $g_{ft}^{(h)}$ the firm's employment growth rate from $t-1+h$ to $t+h$, for $h \geq 0$. A firm's employment growth can be determined by its credit spread, idiosyncratic firm-level shocks λ_{ft}, and nonfinancial conditions z_t:

$$g_{ft}^{(h)} \equiv g^{(h)}\Big(x_{f,t-1}, s[x_{f,t-1}, c_t], z_t\Big) + \lambda_{ft}^{(h)} \tag{1}$$

Credit conditions c_t can affect growth either through the firm's credit spread, or through feedback to nonfinancial conditions z_t.

Object of interest We are interested in the causal effect of fluctuations in c_t on the path of a firm's employment growth. Denote $\delta_{ft} \equiv -\frac{\partial s[x_{f,t-1}, c_t]}{\partial c_t}$ the sensitivity of a given firm's credit spread to c_t; it is multiplied by -1, so that firms whose spreads fall more when c_t increases have higher values of δ_{ft}. Starting from the historical means of c_t and z_t, both normalized to zero, consider a first-order perturbation to the state of the economy at year t. A firm's employment growth is approximately equal to

$$g_{ft}^{(h)} \approx \overline{g}_{ft}^{(h)} + \beta^{(h)} \times (\delta_{ft} \cdot c_t) + \Gamma_{ft}^{(h)} \cdot z_t + \lambda_{ft}^{(h)} \tag{2}$$

[8] If the firm faces hard borrowing constraints due to, for example, covenants on its outstanding debt, one can interpret $s(\cdot)$ as the actual credit spread that the firm pays plus the shadow price of these constraints.

for $\Gamma_{ft}^{(h)}$ the effect of nonfinancial conditions on the firm's employment.[9] Our object of interest is $\beta^{(h)} \equiv \frac{\partial g_{ft}^{(h)}}{\partial s_{ft}}$: the causal response of employment growth over horizon h to the change in credit spreads ($\delta_{ft} \times c_t$) induced by an increase in aggregate credit conditions. Holding constant firms' sensitivities to nonfinancial conditions and realizations of idiosyncratic shocks, firms with higher spread sensitivities (δ_{ft}) respond more to fluctuations in credit markets.

2.2 Firm default risk, credit spreads, and debt issuance over the credit cycle

Measuring c_t and δ_{ft} δ_{ft} is the change in the credit spread that a firm would pay on a marginal unit of debt as aggregate conditions vary, holding constant its characteristics $x_{f,t-1}$. We do not observe the spreads on debt in certain markets (eg. small bank loans) or on the debt that a firm counterfactually could have issued but chose not to. This, in addition to the inability to hold constant firm characteristics while varying aggregate conditions, makes this object fundamentally unobservable. We therefore construct a proxy for δ_{ft} by estimating the secondary market pricing function for senior unsecured corporate bonds, a type of credit that has readily-available price data and that is a key source of marginal credit for many firms.

Constructing our proxy for δ_{ft} entails two steps that we briefly describe here, relegating full details to Appendix Section B.1. First, drawing on Gilchrist and Zakrajšek (2012) and Sorensen (2021), we take the near-universe of corporate bonds issued by Compustat firms from 1978-2020 and estimate the time-varying relationship between a firm's default risk $\pi_{f,t-1}$ and the credit spread on its bonds. We proxy $\pi_{f,t-1}$ with the (negative of) the Merton (1974) distance-to-default measure, as constructed by Bharath and Shumway (2008). This provides our annual measure of c_t. We multiply it by -1, so that c_t is high in years during which investors require relatively little additional yield to hold the bonds of risky firms. Second, we set a firm's sensitivity to c_t equal to proxies for its default risk, $\delta_{ft} = \pi_{f,t-1}$. This allows us to extrapolate the relationship between firm risk and spreads in the bond market to the spreads for all firms in our sample, even those that have never issued bonds.

Variation in c_t over time The dashed line line in Figure 1 shows c_t over our 1978-2020 sample. For this figure, we scale c_t by the average value of $\pi_{f,t-1}$ for Compsutat firms that are in the first quintile of $\pi_{f,t-1}$, minus the average for firms in the fifth quintile. The series is thus interpreted as the difference in average spreads of the least and most risky firms. While risky firms must always pay a higher spread – the series

[9] That is, $\Gamma_{ft}^{(h)} \equiv \frac{\partial g_{ft}^{(h)}}{\partial z_t}$. The object $\overline{g}_{ft}^{(h)}$ denotes the firm's expected employment growth at the economy's initial state.

is strictly negative in all years – their relative cost of credit significantly fluctuates over the credit cycle. In the early 2000s, for example, the riskiest firms go from facing spreads that are 500 basis points higher in 2002, to less than 100 basis points higher in 2006, and then back to 600 basis points higher in 2008. Overall, our sample period contains five distinct boom-bust episodes in which the the relative spreads of the riskiest firms are reduced by 100 to 500 basis points, before spiking back up. A 1 standard deviation increase in c_t corresponds to a roughly 125 basis point reduction in the credit spreads of firms in the fifth quintile relative to those of firms in the first quintile.

c_t and the cost of credit for risky firms We show in Appendix Section C that our measure of c_t captures variation in the cost of credit for a broad set of markets and contractual terms. First, Appendix Table A1 shows that higher values of c_t forecast significantly lower expected returns on corporate bonds (Greenwood and Hanson, 2013; Sorensen, 2021), especially those issued by risky firms. Second, Appendix Table A2 confirms that when c_t is high, risky firms face relatively lower spreads across several different credit markets, including those for commercial paper, and syndicated loans. Third, Appendix Table A3 shows that higher values of c_t are also associated with a relaxation of borrowing constraints imposed on risky firms; this includes looser bank lending standards and less stringent covenants on newly-issued bonds and syndicated loans.

Debt issuance of risky firms over the credit cycle Table A4 show that, in response to cheaper credit during booms, risky Compustat firms issue more debt. A risky firm that experiences a 100 basis points greater decrease in spreads increases its annual net debt issuance by 0.9 percentage points more as a share of assets than a less-risky firm; this effect is as strong for syndicated loan borrowing as for bond issuance. In addition, Table A5 shows that when c_t is high, risky firms refinance their outstanding debt to extend its maturity and decrease its spread. It is thus a priori not clear whether loose credit conditions should improve or further weaken risky firms' financial health.

2.3 Data on default risk and employment growth

To estimate risky firms' employment dynamics over the credit cycle, we construct an annual dataset that links an establishment's employment growth to the financial characteristics of its controlling firm.

Establishment employment data We obtain annual establishment-level employment from the Longitudinal Business Database (LBD), an administrative dataset maintained by the U.S. Census Bureau. By combining Census survey data and IRS tax records, the LBD provides the annual number of employees for the universe of nonfarm private sector establishments with at least one paid employee.

We use establishment-level employment growth in our analysis, as opposed to aggregated firm-level growth, for two main reasons. First, whereas firm-level growth is partially driven by the "relabeling" of where jobs are located due to organizational changes like mergers, establishment-level growth reflects the actual creation or destruction of jobs. Moreover, by running regressions at the establishment level, we can estimate the effects that loose credit conditions have on the future job destruction of a risky firm even if it downsizes or closes.[10] Second, with the LBD, we can estimate whether establishments that lie in the same narrow region and industry, but that are controlled by firms with different levels of default risk, have divergent employment dynamics over credit cycles. As discussed below, this will help purge default risk of determinants of employment growth other than exposure to credit market conditions.

Concretely, for a given establishment e, we use the year-over-year growth rates from $t+h-1$ to $t+h$ introduced by Davis and Haltiwanger (1992), equal to the second-order approximation of the log change in employment,

$$g_{et}^{(h)} \equiv \frac{emp_{e,t+h} - emp_{e,t+h-1}}{.5 \times (emp_{e,t+h} + emp_{e,t+h-1})}$$

for $emp_{e,t+h}$ employment at the end of of year $t+h$.[11] This growth rate is bounded between -2 and $+2$ and is well-defined for establishments that either enter ($+2$) or exit (-2) during year $t+h$.

Firm samples We link an establishment's employment growth to the default risk $\pi_{f,t-1}$ of its controlling firm f as of the end of year $t-1$. We have the financial information necessary to construct default risk proxies for two samples of firms. The first sample consists of establishments that are controlled by a public firm in Compustat. For these firms, we proxy default risk with the (negative of) Merton (1974)'s distance-to-default measure, as constructed by Bharath and Shumway (2008).[12] Our second sample is made up of establishments that are controlled by a firm that, as of year $t-1$, has been recently surveyed in the manufacturing portion of the Quarterly Financial Report (QFR). The QFR is a survey conducted by the U.S. Census Bureau that provides financial information for a representative sample of both public and private manufacturing firms over our full sample period (1978-2020). To our knowledge, the only

[10] The distressed sales of establishments may impose significant job losses (Arnold, 2021) that should be attributed to credit cycle sensitivity of the original firm (Chodorow-Reich, 2014).

[11] In the LBD, the employment counts are as of the payroll period containing March 12. For expositional simplicity, we refer to year $t+h$ as the time between March of year $t+h$ to March of year $t+h+1$. For our employment growth regressions , we annualize all other data (eg. c_t) using quarters two to four of the year t and the first quarter of $t+1$.

[12] We drop firms in the finance or utilities sectors, as well as firms with zero leverage, for which distance to default cannot be defined. We elaborate on the latter choice, as well as provide details on how link Compustat firm identifiers to LBD establishments, in Appendix Section A.1.

other paper that has used the QFR micro-data is Crouzet and Mehrotra (2020). For firms in the QFR sample, we proxy default risk with book leverage. We use book leverage, instead of alternative proxies that require income statement variables (eg. the interest coverage ratio or Altman's z-score), due to the fact that we only have access to the QFR in certain non-adjacent years.[13]

2.4 Methodology

Visualization of risky firm employment growth over credit cycle Figure 1 presents initial descriptive evidence on the employment dynamics of risky firms over credit cycles. The solid green line plots, for the set of manufacturing establishments controlled by a Compustat firm,[14] the annual employment growth of the establishments of firms in the fifth quintile of $\pi_{f,t-1}$ minus the growth of establishments controlled by firms in the first quintile. Recall that the dashed red line shows the difference in credit spreads on the debt that first quintile versus fifth quintile firms can issue given aggregate conditions c_t. For the credit episodes since 1978, as risky firms' relative spreads decrease by 100 to 500 basis points, their relative employment growth also increases, with the magnitude around twice the spread decrease. But as the credit boom turns into a bust, risky firms' employment growth spikes downward as their spreads spike upward.

Specification To characterize the association between firm risk and employment dynamics, we run annual establishment-level regressions. For an establishment e controlled by a f in year t, we estimate

$$g_{et}^{(h)} = \alpha^{(h)} + \eta^{(h)} \cdot \pi_{f,t-1} + \gamma^{(h)} \times \left(\pi_{f,t-1} \cdot c_t\right) + \psi_t + \boldsymbol{X}_{eft} + \epsilon_{eft}^{(h)} \tag{3}$$

for $\boldsymbol{X}_{eft}$ a vector of time-varying establishment- and/or firm-level controls. We double cluster standard errors at the firm and year level to account for the fact that our data is at the establishment level and that c_t is a time-series object. We weight the regressions by the establishment's average employment over $t+h-1$ and $t+h$, divided by the sum of these weights across all establishments in the given year.[15]

With the inclusion of year fixed effects ψ_t, the estimate $\gamma^{(h)}$ has a difference-in-difference interpreta-

[13]In particular, we can only obtain the QFR data in so-called Economic Census years–those that end in 2 or 7. Appendix Section A.2 details how we interpolate book leverage between these years, as well as approximate the sampling weights used in the actual QFR survey.

[14]For this figure, we zoom in on a single sector, manufacturing, to avoid complications from the changing sectoral composition of public firms over time.

[15]Average employment is the weight that aggregates the establishment-level Davis and Haltiwanger (1992) growth rates. We normalize the weights by the total sum across all observations in the given year to ensure that secular employment growth, as well as trends in the economy-wide importance of public firms, do not allow particular years or credit cycle episodes to drive the results.

tion: it tells us how, as aggregate credit conditions loosen, the employment growth of risky firms changes compared to that of less risky firms. This identifies the causal object $\beta^{(h)}$ from (2) if the following condition holds:

Identification condition I: firm employment growth. *The OLS estimate of $\gamma^{(h)}$ in (3) identifies the causal employment effect of spread changes induced by credit market fluctuations, $\beta^{(h)}$ in (2), if and only if, conditional on controls,*

$$\mathbb{E}\Big[c_t \times \mathbb{E}[\pi_{f,t-1} \cdot \epsilon^{(h)}_{eft} | t]\Big] = 0 \tag{4}$$

where, with $\Gamma^{(h)}_{eft}$ is the firm's sensitivity to nonfinancial conditions z_t and $\lambda^{(h)}_{eft}$ an idiosyncratic shock,

$$\epsilon^{(h)}_{eft} = \Gamma^{(h)}_{eft} \cdot z_t + \lambda^{(h)}_{eft}$$

Assume that $\mathbb{E}[\pi_{f,t-1} \cdot \Gamma^{(h)}_{eft} | t]$ is time-invariant. Then, the above condition (4) is satisfied if and only if either of the following two conditions hold:

1. ***Credit conditions orthogonal to nonfinancial conditions:*** $\mathbb{E}_t[c_t \cdot z_t] = 0$
2. ***Sensitivity to credit orthogonal to sensitivity to nonfinancial conditions:*** $\mathbb{E}_t[\pi_{f,t-1} \cdot \Gamma^{(h)}_{eft} | t] = 0$

The causal effect is identified if and only if risky firms do not, when credit is booming, experience systematically different employment growth dynamics due to factors outside of the credit market. This condition holds if either (i) c_t is orthogonal to nonfinancial conditions z_t, or (ii) risky firms do not on average have different sensitivities to z_t. This first condition is strongly counterfactual: our measure of c_t has a 0.45 correlation with contemporaneous real GDP growth.[16] As such, identification will boil down to whether risky firms respond differently to aggregate nonfinancial conditions.

2.5 Baseline OLS estimates, Compustat sample

Contemporaneous effects Table 1 presents the estimates for our Compustat sample over 1978-2020 when we run regression (3) for contemporaneous employment growth ($h = 0$). In row one, we show the estimate $\eta^{(0)}$ on firm default risk. We scale $\eta^{(0)}$ so that it can be interpreted as the "long-run" semi-elasticity of employment growth to spreads: the difference in the growth of a risky firm that, when c_t is at its sample mean, faces spreads that are 100 basis points higher than those of a a less risky firm. Row two presents $\gamma^{(0)}$, the main estimate of interest: the semi-elasticity of a risky firm's employment growth to a 100 basis point relative reduction in spreads driven by a higher value of c_t. Column (1) shows that when

[16] This is consistent with theories of endogenous credit conditions in which strong recent economic performance is what drives lower spreads, through behavioral distortions (Bordalo et al., 2018), financial frictions (He and Krishnamurthy, 2013), or the interaction of the two (Maxted, 2023). It is also consistent with evidence of causal feedback effects from credit supply to aggregate demand over the credit cycle (Di Maggio and Kermani, 2017; Mian et al., 2020; Benguria and Taylor, 2020)

we include just establishment and year fixed effects, $\gamma^{(0)}$ is estimated to be around 2. This implies that a 100 basis point decrease in a risky firm's spread is associated with the firm increasing its employment growth by 2 percentage points, compared to a less risky firm.

In Columns (2)-(5), we add controls designed to absorb potential sources of correlation between a firm's default risk and its sensitivity to nonfinancial conditions. Conceptually, there are two potential sources. The first is that risky firms may systematically participate in input or product markets that load more heavily on the business cycle; this may indeed be an important reason why investors perceive these firms to be risky. To address this identification threat, starting in Column (2) of Table 1, we add granular year t-region r-industry k fixed effects, ϕ_{rkt}, for r the establishment's MSA and k the establishment's four-digit NAICS. The estimate of $\gamma^{(0)}$ is virtually unchanged between Columns (1) and (2). To the extent that establishments in the same tightly-defined industry-by-region pair are subject to similar nonfinancial shocks (eg. aggregate demand), omitted-variables bias from differential business cycle exposure may thus not be a significant concern in practice.

Even if the nonfinancial conditions faced by firms are effectively held constant in this way, there remains a second identification threat: risky firms may respond differently to the exact same conditions, as implied by financial accelerator theories (eg. Bernanke et al., 1999).[17] To eliminate this threat, we must utilize variation in credit conditions that is orthogonal to nonfinancial shocks. We attempt to do by effectively running cross-sectional Jordá (2005) local projections. In particular, we add controls for the interaction of default risk with two lags of c_t in Column (3); interactions of risk with contemporaneous and lagged GDP growth in Column (4); and interactions of risk with the contemporaneous and lagged level of the unemployment rate level in column (5).[18] The estimates of $\gamma^{(0)}$ are reasonably stable as these cyclical interaction controls are added; this is consistent with the fact, discussed in Section 1.1, that existing work has not found robust empirical support for the prediction that risky firms systematically respond to nonfinancial shocks differently. These controls are admittedly crude and do not address the potential for credit conditions to causally influence aggregate nonfinancial conditions in the same year. Still, they provide some reassurance that the potential correlation between firm risk and sensitivity to business cycles is not driving the estimates in Table 1.

[17] As formalized and discussed by Ottonello and Winberry (2020), these theories are generally ambiguous regarding whether riskier firms should react more or less strongly to an aggregate shock.

[18] We consider both GDP growth and the unemployment rate level based off past work, such as Fort et al. (2013), that finds non-trivial differences in the cyclicality of firms (by age or size) when using one measure versus the other. In unreported results, we also estimate similar values for $\gamma^{(0)}$ if we control for interactions of business cycle conditions in the establishment's region and industry or, owing to the potential relevance of intra-firm capital markets (Giroud and Mueller, 2021), the employment-weighted average of conditions across the firm's different regions and industries.

Dynamic effects We now turn to estimating whether the initial increase in risky firms' employment during credit booms persists or reverts. Figure 2 shows the estimates of $\gamma^{(h)}$ when we run the baseline regression (3) for annual horizons $h = 0$ to $h = 4$. We focus on the specification from Column (4) of Table 1, which includes year-region-sector fixed effects and the interaction of default risk with lags of c_t and GDP growth.[19] The large, positive contemporaneous response of employment growth to loose credit conditions reverts around the same time that credit conditions themselves do: the estimated year-over-year employment effect becomes negative starting in year $t+2$ and goes down to a statistically significant level of around -1.4 percentage points at $t+3$. As shown by the dotted black line, which cumulates the estimated coefficients up to and including each horizon h, this negative growth brings the year $t+4$ employment of a risky firm slightly below its initial level. Our baseline OLS estimates thus imply that while risky firms experience significantly higher employment growth during credit booms, over the next few years this growth is reversed, and even overshoots its return to its pre-boom level.

2.6 Estimates in the QFR sample

We now consider whether risky firms in the representative sample of manufacturing firms from the QFR also exhibit boom-bust dynamics in their employment growth. A priori, it is not clear. Smaller firms rely less on market-based sources of credit, potentially making fluctuations in the bond market less relevant to their cost of credit. On the other hand, smaller, private firms are more likely to be bound by credit constraints, which Appendix Table A3 indicates are relaxed during credit booms.

Figure 3 shows the estimates of $\gamma^{(h)}$ when we estimate regression (3) over horizons $h = 0$ to $h = 4$ on our QFR sample, using book leverage as the proxy for default risk.[20] We scale $\gamma^{(h)}$ by the relationship among Compustat firms between book leverage and distance to default, which allows us to directly compare the estimated value of $\gamma^{(h)}$ from Figure 3 (QFR sample) to the estimates from Figure 2 (Compustat sample). The average risky firm in the QFR experiences a boom-bust cycle in employment growth that is even more pronounced than for the average Compustat firm: the contemporaneous ($h = 0$) effect is larger at 3 percentage points, while the subsequent reversal starts sooner and is significantly larger. The cumulative $h = 4$ estimate in Figure 3 implies that, following a 100 basis point decline in its relative cost of credit in year t, by year $t+5$, a risky firm's employment is 4% lower than its initial level.

[19] Note that the coefficient for $h = 0$ in Figure 2 differs slightly from the estimate from Column (4) of Table 1. This is because for the dynamic regressions, we drop observations after 2015 so that the same is kept the same as the horizon h increases.

[20] Appendix Table A6 shows how the contemporaneous estimates change as we add in additional controls, analogous to Table 1 for the Compustat estimates.

2.7 Mechanism of risky firms' employment dynamics over credit cycles

Contemporaneous effects Our estimates imply a semi-elasticity of employment to credit spread changes of around 2 percentage points. To interpet this magnitude, it useful to consider two underlying mechanisms by which changes in firms' credit spreads could affect employment.

First, consider a neoclassical setting in which there are no direct interactions between a firm's cost of credit and their employment. If risky firms' marginal financing comes entirely from debt, we can interpret our estimate as the elasticity of employment to the user cost of capital.[21] Curtis et al. (2022) combine quasi-experimental estimates from tax changes with a structural model to find that, due to capital-labor complementarity, the elasticity of labor to the user cost of capital is around 3 to 4 percentage points.[22] The lower magnitude of our estimate could perhaps be explained by the fact that credit boom-induced spread changes are not long-lasting, dampening their impact on capital investment relative to tax changes. Indeed, Appendix Figure A2 uses data from the Census's Annual Survey of Manufacturers to show that the response of risky firms' physical capital growth to c_t is muted, both overall and relative to the response of employment.

Second, suppose risky firms are bound by borrowing constraints that affect their labor demand, either directly or through production complementarity with capital. If loose credit conditions relax these constraints, then we can interpret the employment elasticity that we estimate as partially arising from risky firms' ability to issue more debt. In Appendix Table A4, we estimate that a risky firm with a 100 basis point reduction in spreads increases its relative debt issuance as a share of assets by 0.9 percentage points. Our employment estimate thus implies a quasi-elasticity of employment growth to asset-normalized debt issuance of around 2.2. This is comparable to, though a bit lower than, the estimate of Benmelech et al. (2021) of the impact of a firm being forced to de-lever when its long-term debt matures. This indicates that, while the employment effects that we estimate may be in-part driven by risky firms' borrowing constraints varying with aggregate credit conditions, the cost of capital mechanism discussed

[21] The assumption of 100% debt financing is arguably inconsistent with a neoclassical setting in which the cost of credit can only affect labor demand via capital-labor production complementarity. Loosely, we are considering a firm that is constrained in its ability to issue equity but not debt. Relatedly, one could argue that to compute the elasticity, we should use the relationship between c_t and the actual return on risky firms' debt, rather than just the change in spreads. As Appendix Table A1 shows, a credit boom-induced 100 basis point decrease in spreads is associated with a larger decrease (4% to 8%, depending on the horizon) in the returns of risky firms' corporate bonds. Using these returns would imply an employment elasticity well below unity. We do not emphasize this number, though, since we view the implicit assumption that we would make in computing the elasticity with bond returns – that risky firms can time their debt issuance and repurchase behavior to take advantage of the reversion of booming credit conditions – as inconsistent with the 100% debt financing assumption.

[22] The headline numbers that Curtis et al. (2022) discuss, 0.6 to 0.8 percentage points, are significantly lower than this. This is because they emphasize estimates of the employment elasticity that utilize a structural estimate of the effect of the tax bonus depreciation policy on firms' cost of capital that is inclusive of the effects of the policy on alleviating firms' financial constraints. Our estimate reflects the reduced-form effect of changes to credit spreads, and so we compare it to the reduced-form estimates of the tax policy reported in Curtis et al. (2022).

above may also be relevant. As discussed in Section 3.5, we find suggestive evidence that for especially risky or financially-constrained firms, the cost of capital and borrowing constraint mechanisms complement each other's effects on employment over credit cycles.

Reversal of boom-time employment growth What leads risky firms to destroy the jobs that they created during booms as credit conditions normalize? We find suggestive evidence that the lower credit spreads that risky firms experience during credit booms delay, but do not prevent, the arrival of acute financial distress. Appendix Figure A7 shows estimates from annual firm-level local projections in which we regress indicators for various types of distress on the interaction between default risk and c_t. Loose credit conditions at year t are associated with lower probabilities that risky firms file for bankruptcy, default on outstanding corporate bonds, or are in violation of a loan covenant. But by year $t+2$, as aggregate credit conditions revert, all of these events becoming significantly more likely. Risky firms' propensity to engage in mass layoffs or close establishments follows the same inverse boom-bust pattern, as shown in Appendix Figures A1 and Figure A3 for, respectively, our Compustat and QFR samples. These results suggest that even though credit booms allow risky firms to reduce the interest burden on their outstanding debt (Appendix Table A5), the beneficial effects on their financial health are counteracted by their increased issuance of new debt (Appendix Figure A4). When loose credit conditions revert and/or the macroeconomy declines (Lopez-Salido et al., 2017), risky firms experience pent-up financial distress that is associated with the destruction of the jobs that they created during the boom.

3 Exploiting random variation across firms to aggregate credit conditions

In this section, we show that the boom-bust employment dynamics of risky firms over credit cycles reflect the causal effect of credit conditions. We do so by exploiting institutional features of the corporate bond market, discussed in Section 3.1, that lead to segmentation of bonds with high-yield (HY) versus investment-grade (IG) credit ratings. We show in Section 3.2 that this segmentation leads the spreads of HY bonds to be significantly more sensitive to aggregate conditions than those of IG bond, even among bonds at the HY/IG rating threshold with similar observable characteristics. We exploit this discontinuity to design a matching procedure in Section 3.3 in which we compare firms with BB+ (HY) ratings to firms with BBB- (IG) ratings. We use this procedure in Section 3.4 to estimate the causal effect of a credit boom-induced reduction in spreads on employment dynamics. We find that the IV estimates are larger in magnitude than the OLS estimates from the previous section. In Section 3.5,

3.1 Institutional details behind HY/IG threshold

We exploit two features of U.S. credit markets. First, due to a number of regulatory and institutional frictions, the U.S. bond market is segmented by whether a bond has a HY or IG rating. Past work has found that the supply of capital available to the issuers of HY bonds is more sensitive to market disruptions (Lemmon and Roberts, 2010; Chernenko and Sunderam, 2012). Second, the exact rating that credit rating agencies assign to a given firm is a noisy signal of the firm's true creditworthiness. This is, in part, due to the explicit goals of rating agencies to incorporate news about fundamentals in a gradual, autocorrelated way (Altman and Kao, 1992) and to disregard the effects of temporary aggregate conditions, i.e."look through the cycle" (Cornaggia and Cornaggia, 2013). Together, these two features suggest an instrument for the sensitivity of a firm's cost of credit to aggregate conditions: whether a firm is rated BB+ (the highest HY rating) or BBB- (the lowest IG rating).[23] Noise in credit ratings implies that some BB+ firms will have the same fundamental default risk and exposure to nonfinancial conditions as some BBB- firms. But due to market segmentation, the credit spreads on the bonds that BB+ firms can issue may be far more responsive to fluctuations in aggregate credit conditions.

3.2 Bond-level first stage

We first confirm the premise that the prices of BB+ and BBB- bonds respond differently to aggregate credit market conditions. Letting s_{bt} denote the spread of a bond b in quarter t, we run quarterly bond-level regressions of the bond's spread s_{bt} on an interaction of credit conditions c_t with a dummy for the bond's rating.[24] Define $1\{rating_{b,t-1} = BB+\}$ as a dummy variable that equals one if the bond is rated $BB+$. On the sample of bonds that, as of quarter $t-1$, are rated either BB+ or BBB-, we run the quarterly bond-level regression

$$s_{bt} = \alpha + \eta \cdot 1\{rating_{b,t-1} = BB+\} + \delta_{BB+} \cdot 1\{rating_{b,t-1} = BB+\} \times c_t + \phi_t' \times \boldsymbol{X}_{bt} + \epsilon_{bft} \tag{5}$$

where $\phi_t' \times \boldsymbol{X}_{bt}$ denotes the interaction between a quarter fixed effect ϕ_t a the unique value of a vector of binned bond-level characteristics $\boldsymbol{X}_{bt}$. Controls for bond-level characteristics are motivated by the fact that we want δ_{BB+} to capture the effect of c_t on spreads that reflect the fresh trading prices that a firm would face when issuing new bonds during quarter t. Because past work has found that HY bonds have

[23] Throughout the paper, we discuss this measure using the S&P rating convention, even for bonds/firms for which the underlying ratings data comes from Moody's.

[24] We run the bond-level regressions at the quarterly frequency, as opposed to the annual frequency of our employment regressions, to maximize the power of our first-stage estimates. For this subsection, the time index t denotes a quarter rather than year. For the sake of expositional simplicity, we do not introduce additional time notation.

significantly lower trading liquidity than IG bonds (Lin et al., 2011), $\boldsymbol{X}_{bt}$ includes bucketed proxies for liquidity that should partially absorb such differences.[25]

Figure 4 shows the estimate and 95% confidence interval of δ_{BB+}, scaled to be interpreted as the effect of a 1 standard deviation increase in c_t. The spreads of BB+ bonds are, compared to BBB- bonds, around 45 basis points more responsive to a 1 standard deviation loosening of conditions compared. This effect is large relative to both the mean (217 basis points) and standard deviation (137 basis points) of spreads for BB+ and BBB- bonds in our sample. Consistent with this effect being driven by segmentation frictions, the estimate is essentially unchanged when we add a control for the interaction of the firm's default risk with c_t (dashed orange estimates). Moreover, the differential sensitivity of BB+ versus BBB- bonds is not present for adjacent rating pairs below or above the HY/IG threshold. We show this by estimating an identical regression for each rating notch n between BB- and AA and plotting the estimates in the same figure. The greater sensitivity of BB+ bonds compared to BBB- bonds is a result of segmentation frictions, not differences in the fundamental risk of the firms issuing the bonds.

3.3 Firm-level rating measure and methodology

Firm-level instrument To utilize the bond-level variation in sensitivity to aggregate conditions for our employment regressions, we must aggregate this variation up to the firm level. We follow Chernenko and Sunderam (2012) in classifying a firm as rated BB+ or BBB- based off the long-term issuer rating it is given by S&P and available via Compustat. Because this rating type is based off a notion of unsecured credit risk, and is available for rated firms regardless of if they currently have any bonds outstanding, it reflects the rating on the marginal bond that the firm could issue, abstracting from covenants on existing debt. Since data on this is available only starting in 1986, from 1978-1985 we classify firms as BB+ or BBB- based off the rating on their senior unsecured bonds, for those firm-years that have such bonds outstanding.

Characteristics of BB+ vs. BBB- firms Among the firms with a credit rating of either BB+ or BBB-, the assignment of which of the two ratings an agency gives is not random: the average BB+ firm is smaller, more levered, and riskier than the average BBB- firm (Chernenko and Sunderam, 2012). This lack of balance is not surprising–there should be *some* signal in the decision of agencies to grant a firm a BB+ issuer rating as opposed to a BBB- rating. Importantly, it does not invalidate using the sharp differences in the sensitivity of BB+ versus BBB- bonds to obtain quasi-random variation in firm exposure to aggregate

[25] Following Coppola (2022), $\boldsymbol{X}_{bt}$ includes seven bins for duration, five buckets for bond age, and deciles of amount outstanding

credit conditions; it just requires that when we condition on observable characteristics when comparing the employment dynamics of BB+ and BBB- firms. Intuitively, conditioning on observables, such as proxies for default risk, allows the noise in credit rating agencies' selection of firms into BB+ versus BBB- to be separated from the signal. Because, as shown in Figure 4, the residual noise in agencies' assignment of BB+ versus BBB- ratings has a large effect on the sensitivity of spreads to aggregate conditions, this still yields a powerful instrument.

Matching methodology We extend the firm-level matching procedure of Chernenko and Sunderam (2012) to our establishment-level LBD data. For each establishment e with a BB+ parent firm in a given year, we attempt to find an establishment of a BBB- parent firm that has similar observable characteristics, denoted $m(e)$.[26] We match on three types of features: (i) the market in which the establishment operates (the exact four-digit NAICS-by-MSA pair); (ii) firm-level determinants of default risk, including distance to default; and (iii) the firm's overall exposure to shocks across its different markets.[27] Distance to default is a crude proxy for default risk that may not capture all of the fundamental information embedded in rating agencies' choices. But combined with the direct measures of exposure to nonfinancial shocks, it is hard to see why matched BB+ and BBB- establishments should systematically respond in different ways to aggregate conditions, other than through credit market fluctuations.

We implement a nearest-neighbor matching procedure in which we select the BBB- establishment $m(e)$ as the one that minimizes the Mahalanobis norm across the continuous matching variables. We only keep matches where, across all continuous matching variables, the BBB- establishment's value is within 1 standard deviation of the BB+ establishment's value. For each successfully-matched BB+ establishment, we then run the annual regression

$$(g_{et}^{(h)} - g_{m(e)t}^{(h)}) = \alpha^{(h)} + \gamma^{(h)} \times \left(\delta_{BB+} \cdot c_t\right) + (\boldsymbol{X}_{eft} - \boldsymbol{X}_{m(e)m(f)t}) + \epsilon_{eft}^{(h)} \tag{6}$$

We scale c_t by δ_{BB+}, the differential sensitivity of BB+ bonds to credit conditions relative to BBB- bonds that we estimated in Figure (4). Thus, $\gamma^{(h)}$ captures how the employment growth of a BB+ establishment

[26] Mechanically, we run the matching procedure at a more aggregated level – firm-by four-digit NAICS-by MSA – which is convenient when dealing with establishments of certain types of firms that might have many different physical locations in the same market (eg. coffee shops) . This does not throw away any variation or prevent us from using any matching variables compared to matching at the establishment level, since our most granular controls are at the firm-industry-region level. We continue to use notation as if the procedure is run at the establishment level for expositional convenience.

[27] For (i), we include distance to default, book leverage, Tobin's Q, assets, and the liquidity ratio. In (iii), we include Bartik instruments for the mean and volatility of employment growth across a firm's different industries and regions, along with potential determinants of how intensely shocks in other markets may be transmitted within the firm to the given establishment. Appendix Section B.6 provides the exact definitions and construction details of these matching variables.

responds to a 100 basis point reduction in credit spreads compared to its matched BBB- establishment. Formally, if the average sensitivity to nonfinancial conditions of BB+ establishments is the same as that of matched BBB- establishments, $\gamma^{(h)}$ identifies the causal parameter $\beta^{(h)}$ when interpreted as the average treatment effect on the treated (ATT) for BB+ firms.

3.4 Estimated employment dynamics

Table 2 presents the contemporaneous estimate $\gamma^{(0)}$ across specifications in which we vary the included controls. Column (1) does not contain any controls, while Column (2) adds controls for the deviation of the continuous matching variables between the establishment e and its match $m(e)$; this accounts for potential finite-sample bias from an inability to perfectly match observations (Imbens, 2015). Column (3) adds lags of c_t, while Column (4) adds contemporaneous and lagged GDP growth. Across (1)-(4), the estimated semi-elasticity $\gamma^{(0)}$ is, at between three and five percentage points, large relative to the OLS estimates.

One may be concerned about how insightful these estimates are regarding the employment dynamics over credit cycles for typical risky firms. The estimates' large magnitudes may be driven by shocks to the relative employment growth of BB+ firms in years with market crashes like in $2008-09$, during which market segmentation may be especially severe. To test whether the estimates suffer from a lack of external validity, Column (5) shows estimates in which we allow the estimate to vary for NBER expansion versus recession years. Reassuringly, the estimates are similar for recessions and expansions, indicating that they are informative about employment dynamics over both phases of the credit cycle.

We now estimate whether BB+ firms, like risky firms, later reverse the job creation that they engage in during credit booms. Figure 5 shows 95% confidence intervals on the estimates $\gamma^{(h)}$ run over horizons $h=0$ to $h=4$. Similar to the dynamic OLS estimates, the employment growth of BB+ firms turns sharply negative in years $t+2$ and $t+3$, with a magnitude that is similar to, and slightly greater than, the positive effects in year t; Appendix Figure A4 shows that this is the case for both expansions and recessions. Thus, when credit conditions are loose, the establishments of BB+ firms experience a boom-bust cycle in employment growth, as compared to the growth of establishments of BBB- firms that lie in the same local market and have similar observable characteristics.

Support for identification conditions As discussed above, it is not clear what could lead the establishments of BB+ firms to experience different employment dynamics over the credit cycle than matched establishments of BBB- firms, other than differential sensitivity to credit conditions. To provide further reassurance that the effects are not driven by a differential sensitivity of BB+ firms to nonfinancial conditions, we run a placebo test in which we implement the exact same matching procedure for rating pairs to the left and right of the HY/IG threshold. These rating pairs exhibit a lack of balance with respect to certain characteristics, like default risk and size, that is similar to that for the BB+/BBB- pair (Chernenko and Sunderam, 2012). This, along with the fact that the bonds of rating pairs to the left and right of the HY/IG threshold respond similarly to aggregate credit conditions (Figure 4), is the basis for the placebo test. If the above employment estimates are driven by the signal in credit agencies' rating choices that our matching procedure does not strip out, we should obtain similar estimates for these other pairs.

Figure 6 shows that this is not the case. It plots the contemporaneous estimates $\gamma^{(0)}$ across different rating pairs. These are based off estimating equation (6) using the BB+/BBB- first-stage estimate δ_{BB+}, and pooling the sample of matched establishments across multiple adjacent pairs; the left-most estimate in the figure, for example, runs separate matching procedures on each pair from B-/B to B+/BB-, but then pools the resulting observations to estimate a single $\gamma^{(0)}$.[28] For rating pairs both below and above the BB+/BBB- pair, the estimates are statistically insignificant. More importantly, these other estimates are small in magnitude, with none of their 95% confidence intervals covering the BB+/BBB- estimate.

3.5 Comparison between OLS and IV estimates

Accounting for difference between IV and OLS estimates It is perhaps counter-intuitive that the rating design produces estimates of the semi-elasticity $\gamma^{(0)}$ that, at 3 to 5 percentage points, are roughly twice as large as those from the OLS specification in Table 1. Heterogeneous treatment effects – a greater responsiveness of the employment growth of some firms to a decrease in credit spreads of the same size – can help reconcile the differences between the IV and OLS estimates. Table 3 shows estimates from a non-parametric version of the OLS specification (3) in which we replace the continuous default risk variable with a categorical variable, the firm's quintile of risk. The regression also allows the effect of credit conditions on employment growth to vary with whether the firm has a high yield credit rating, an investment grade rating, or is unrated.

Panel (a) shows that only a particular set of firms – high yield and unrated firms in the fourth and fifth risk quintiles – have a significantly different employment response to c_t than the omitted group, investment grade firms in the first quintile. Panel (b) scales the estimates so that they can be interpreted as the response of employment growth to a 100 basis points greater reduction in spreads. The estimated

[28] We pool estimates in this way because of the disclosure burdens that we would face in trying to disclose RDC output specific to each single non-BB+/BBB- pair, given that several of these pairs do not contain many firms. This restriction has the benefit, though, of increasing the power for our placebo tests.

semi-elasticity of high-yield firms in the fourth quintile of risk is around 3.5 percentage point.[29] Since the average BB+ firm in our matching sample lies in the fourth risk quintile, this treatment effect heterogeneity can explain the relatively large magnitude of the IV estimates. Following a given reduction in spreads, risky high-yield firms increase their employment by more than the average Compustat firm does.

Source of treatment effect heterogeneity What is the underlying source of treatment effect heterogeneity with respect to risk and rating category? According to our estimates, the semi-elasticity of employment growth to credit spreads is relatively large (4 to 5 percentage points) for risky high-yield and unrated firms (Table 3), as well as the average firm in the representative QFR sample (Figure 3). These firms are all more likely than the average (employment-weighted) Compustat firm[30] to face binding borrowing constraints: smaller, private firms and unrated public firms rely on bank credit, while high-yield firms can only issue bonds with covenants that restrict their future debt issuance (Green, 2018). This heterogeneity suggests that the interaction between lower credit spreads and looser borrowing constraints can help account for the large employment effects of credit booms on risky firms that we estimate.

4 Worker exposure to boom-induced jobs

We now ask whether the jobs that risky firms create during credit booms have important effects on the future earnings of the workers who take them, despite being short lived. In Section 4.1, we develop a conceptual framework that explains how this could occur. Boom-induced jobs must be filled by workers whose longer-run labor market prospects are sensitive to their short-run employment outcomes. After describing our LEHD data in Section 4.2, we show in Section 4.3 that risky firms' boom-time employment growth is driven by the formation of full-time, stable employment relationships with new workers. These relationships could have long-run consequences for workers, either good (eg. if they serve as a jumping-off point) or bad (eg. if their value is tied to the firm's health). In Section 4.4, we estimate whether the types of workers who may be subject to such path dependency, such as young inexperienced workers, actually take boom-induced jobs.

[29] For Panel (b), we scale the estimates for HY firms by summing (a) the predicted effect of c_t on spreads due to the differences in average default risk between firms in the given quintile and firms in the omitted first quintile with (b) the effect of c_t on HY spreads due to bond market segmentation, taking the estimate based off the BB+/BBB- discontinuity from Figure 4. This could lead the estimates in Panel (b) to understate the semi-elasticity of HY firms, to the extent that the spreads of HY bonds away from the BB+/BBB- threshold are not as strongly affected by changes in aggregate credit conditions (due to, for example, lower liquidity). If we only scale the HY estimates by (a), the estimated semi-elasticity for HY firms in the fourth quintile is substantially higher (above 6).

[30] In our Compustat sample, investment grade firms make up just 12% of all firms but around 60% of total employment.

4.1 Conceptual framework

Environment We start from the framework of Section 2.1 in which there are firms whose current and future employment growth depends on their exposure to time-varying aggregate credit and nonfinancial conditions. Denote $\boldsymbol{g}_{ft}$ the vector of the current and future employment growth rates of firm f written in (1). We add in workers, indexed by i, who work at firm $f(i,t)$ at the end of t. Workers have a vector of state variables $\boldsymbol{v}_{it}$ relevant to their productivity, such as formal education and work experience. The present discounted value (PDV) of the worker's labor earnings, W_{it}, is the sum of current earnings plus the value of the worker's human capital:

$$W_{it} \equiv \underbrace{w_{i,f(i,t),t}}_{\text{Current earnings}} + \mathcal{H}\Big(\underbrace{v_{it}(w_{i,f(i,t),t})}_{\text{Worker productivity}}, \underbrace{z_t}_{\text{Aggregate conditions}}, \underbrace{\boldsymbol{g}_{f(i,t),t}}_{\text{Current firm}} \Big) \tag{7}$$

The function $\mathcal{H}(\cdot)$ allows the worker's current job to have path-dependent effects, in two different ways. First, the worker's job can bolster their long-run productivity by helping them accumulate *general* experience and skills. For simplicity, we assume that the capacity of a job to provide these benefits is summarized by the amount that it pays, $w_{i,f(i,t),t}$. Second, the worker's future earnings can be partially determined by the employment demand of the firm at which they currently work. This would be the case if the worker accumulates *firm-specific* skills or if search frictions make finding a new job difficult.

Exposure to boom-induced jobs Consider a worker i who, at the beginning of time t, is unemployed and deciding between taking a job at different firms. When credit conditions c_t are loose, firms whose spreads are especially sensitive to c_t (high δ_{ft}) create a relatively large amount of new jobs. Conceptually, if looser conditions prompt the worker to join a firm with a higher δ_{ft}, we say that the worker has chosen to take a boom-induced job. Formally, we thus denote the worker's propensity to take a boom-induced job as δ_{it}, where

$$\delta_{it} \equiv \frac{\partial \delta_{f(i,t),t}}{\partial c_t} \tag{8}$$

To derive an expression for how the decision to take a boom-induced jobs affects the worker's earnings, consider a first-order perturbation around the historical means of c_t and z_t. Define $\overline{W}_{it}$ as the PDV of the worker's earnings if this perturbation did not affect which job the worker chose to take. This is the worker's outside option: the earnings the worker would get in lieu of taking a boom-induced job. Taking a first-order approximation of (7), we can then write the impact on the worker of take up as[31]

$$W_{it} - \overline{W_{it}} \approx (\beta_i^{ret} \cdot \delta_{f(i,t),t}) \times c_t + \Gamma_{it}^z \times z_t \tag{9}$$

where β_i^{ret} is the return that the worker receives by choosing to take a boom-induced job:

$$\beta_i^{ret} \equiv \frac{\partial W_{it}}{\partial \delta_{f(i,t),t}} \tag{10}$$

This object is analogous to the expected excess returns on a financial investment in a firm with a high δ_{ft}, in that it reflects the causal effect of a worker taking a job in such a firm relative to their outside option. However, there are two important difference. First, whereas for financial investors there exists a homogeneous outside option, the safe interest rate, workers have heterogeneous outside options. Second, even the gross return on taking a boom-induced job can in principle depend on a worker's type. In particular, using the expression (7) for W_{it}, we can write β_i^{ret} as

$$\beta_i^{ret} = \underbrace{\frac{\partial w_{i,f(i,t),t}}{\partial \delta_{f(i,t),t}}}_{\text{Short-run effect}} \times \left(1 + \underbrace{\frac{\partial v_{it}}{\partial w_{i,f(i,t),t}} \cdot \frac{\partial \mathcal{H}}{\partial v_{it}}}_{\text{Long-run effect, general}}\right) + \underbrace{\frac{\partial \boldsymbol{g}_{ft}}{\partial \delta_{ft}} \cdot \frac{\partial \mathcal{H}}{\partial \boldsymbol{g}_{f(i,t),t}}}_{\text{Long-run effect, firm-specific}} \tag{11}$$

The worker's return to taking a boom-induced job is has three components. The first is the short-run boost in earnings that the worker may receive by going to a firm that is more exposed to the credit boom. The second is the beneficial long-run effect that higher short-run earnings may have on the worker's general skills and experience. The third component is the long-run effect of becoming attached to a firm that, based off the results of Sections 2 and 3, may fire its newly-hired workers once the credit boom recedes. As such, boom-induced jobs can, even if short lived, still have important long-term effects on workers. For this to be the case, boom-induced jobs must be taken by workers whose future labor market prospects can be shaped by the potentially lucrative, but unstable, opportunities that these jobs provide.

4.2 Data and measurement

Measuring the creation of firm-worker relationships We want to measure whether risky firms fill the jobs that they create during booms by forming employment relationships with new workers. Employment relationships are characterized by jobs that could either allow the worker to accumulate valuable

[31] For this expression, we define Γ_{it}^z as the effect of a higher value of nonfinancial conditions z_t has on the worker via affecting their choice of job. Recalling from equation (2) that Γ_{ft} is the sensitivity of the employment growth of firm f to z_t, Γ_{it}^z is the same as the term that multiples c_t, $\beta_i^{ret} \cdot \delta_{f(i,t),t}$, except that the firm's spread sensitivity δ_{ft} is replaced with Γ_{ft}.

working experience and/or make the worker worse off should the job go away

In principle, risky firms could increase their employment growth during credit booms without creating jobs that have these properties. This is for two reasons. First, as an accounting identity, the employment growth rate of a firm during a given period equals its hiring rate minus its separation rate, i.e. the inflow of new workers net of the outflow of workers who previously worked at the firm (eg. through layoffs). During credit booms, risky firms could increase their employment growth by just laying off fewer workers, rather than enticing new workers to join the firm. Second, in the U.S., it is frequently the case that firms' hiring activity reflects the creation of part-time or temporary jobs (Hyatt et al., 2014) or the recall of previously-employed workers (Fujita and Moscarini, 2017). These hires would not be expected to have important path-dependent effects on the involved workers.

To measure the rate at which risky firms create new employment relationships in the data, we therefore only consider a subset of firms' employment growth. In particular, we consider employment growth that comes from hiring a worker into a job that (a) is at a firm where the worker has never before worked, (b) is taken on a full-time basis, and (c) is stable, in the sense that for an extended period of time, it is the only job that the worker holds.

LEHD data We construct the rate at which firms create new employment relationships using quarterly firm-worker matched data from the Census Bureau's Longitudinal Employer Household Dynamics (LEHD) data. The LEHD provides panel data on workers' earnings and employment histories from state unemployment insurance records. We have access to the LEHD data for 24 states, with the bulk of states' data starting by 1997. We observe the quarterly earnings and job characteristics for all workers in these states.

We focus on the set of Compustat firms that, at some point, employ workers in our LEHD dataset. Because we cannot observe establishments in the LEHD data, we measure the creation of new employment relationships, $create_{rkft}$, at the region r-by industry k-by firm f level, where the region r is the job's MSA and the industry k is the job's four-digit NAICS. In line with the above discussion, we construct $create_{rkft}$ by counting up the number of prime-age workers that the firm hires during quarter t into full-time, stable positions, and dividing by the firm-region-industry observation's initial employment count. As detailed in Appendix Section B.5, we follow previous work in deducing whether a new job is full-time and stable based off the worker's employment status in the quarters surrounding the hire.

4.3 Creation of firm-worker relationships over credit cycles: all workers

Figure 7 visualizes the year-by-year relationship between the rate at which the firm creates new employment relationships and its default risk. It plots the coefficient on a regression run each year of $create_{rkft}$ on $\pi_{f,t-1}$, including fixed effects for the job's region r-industry k pair. The figure makes clear that over the credit cycle, risky firms create a significantly higher amount of new employment relationships. This is particularly noticeable during the booming conditions of the early 2000s.

To quantify the relationship between credit conditions and risky firms' creation rate, we run a quarterly regression at the firm-region-industry level of the form

$$create_{rkft} = \alpha + \eta \cdot \pi_{f,t-1} + \gamma \times (\pi_{f,t-1} \cdot c_{t-3,t}) + \phi_{rkt} + \theta_{rkf} + \epsilon_{rkft}$$

where $c_{t-3,t}$ is the average value of c_t in the four quarters leading up to (and including) c_t; we use smoothed credit conditions to address seasonality in firms' hiring activity, as well as to allow for a lag between when credit conditions loosen and when risky firms hire more. We report the estimate in the first column of panel (a) in Table 4. When credit conditions are loose, a risky firm with a 100 basis point greater decrease in spreads increases the rate at which it creates new employment relationships by 1 percentage point. The creation of new relationships thus accounts for around half of this risky firm's 2 percentage point increase in employment growth during booms. From the worker's perspective, the estimate implies that when c_t increases by 1 standard deviation, the likelihood that a worker enters an employment relationship with a firm in the fifth quintile of risk increases by 1.25 percentage points, relative to the likelihood of starting a relationship with a firm in the first quintile.[32]

4.4 Creation of firm-worker relationships over credit cycles: worker heterogeneity

We now consider *which* workers choose to enter employment relationships with risky firms–i.e, to what extent there is selection of certain types of workers into boom-induced jobs. We are interested in whether the types of workers who may be subject to important path-dependent effects of taking boom-induced jobs are actually the workers who do so. If, for example, boom-induced jobs are predominantly taken by workers who already attached to the labor market, or older workers who are nearing retirement, there would be limited scope for these jobs to affect workers' long-run labor market outcomes. This would

[32] This follows from the fact that we weight the regression by the initial employment of each firm-region-industry observation, along with the relationship between c_t and the credit spreads of fifth-quintile versus first-quintile firms shown in Figure 1. Technically, this statement also assumes that the workers who enter into new employment relationships previously worked at a firm that is also in our Compustat-LEHD sample (rather than coming from a firm outside of this sample or from non-employment).

not be the case if boom-induced jobs were disproportionately taken by younger workers with little labor market experience, whose future labor market prospects are plausibly more affected by their short-term outcomes.

To see which of these two starkly-different possibilities is closer to reality, Table 4 shows estimates of specification (4.3) when we measure the creation rate, $create_{rkft}$, for different subsets of workers. Panel (a) shows estimates of the rate at which risky firms create employment relationships with workers who, in the previous quarter, were employed at a different firm (Column 1) versus workers who were not employed (Column 2). The estimate γ on the interaction term between default risk and credit conditions is essentially the same for workers that were previously employed versus not employed. This shows that that these two types of workers account for the same fraction of the increase in risky firms' creation of new employment relationships during booms. However, it is the number of new relationships as a fraction of workers in each given category – not as a fraction of the firm's employment – that captures the propensity δ_i of different types of workers to boom-induced jobs. Since there are more employed than non-employed workers in the population, γ understates the likelihood that a given non-employed worker takes a boom-induced jobs. To turn the estimates into per-worker rates, we normalize the value of γ estimated for each worker type by that type's population share.[33] The second row of the table shows that with this normalization, our estimates imply that non-employed individuals are far likelier to enter new employment relationships with risky firms during credit booms.

Non-employed workers' greater tendency to take boom-induced jobs could be a reflection of younger workers starting their careers at these jobs, or of older, marginally attached workers using the jobs as a temporary employment stopgap. In Panel (b) of Table 4, we try to distinguish between these two stories by running specification (4.3) when we split $create_{rkft}$ by worker age. The per-worker estimates of δ_i in the second row show that individuals who are of high-school graduation age (18-20) have by far the highest propensities to take boom-induced jobs. Following a 1 standard deviation increase in c_t, the likelihood that a recent high-school graduate enters an employment relationship with a firm in the fifth risk quintile increases by 3 percentage points more than the probability that they enter a relationship with

[33] Specifically, for the estimate for previously non-employed workers, we use data from the CPS to divide γ by the ratio of the non-employed worker population (the number of prime-age, non-institutionalized individuals who are either not in the labor force or who are unemployed) to total U.S. nonfarm employment. We conduct the analogous normalization for the employed worker estimate. Seemingly, a more straightforward approach would have been to do this renormalization using the share of workers in our LEHD sample that are of each worker type. Or, more simply, we could have estimated the type-specific rate by running regressions at the worker group level rather than at the aggregated firm-region-industry level. However, these more direct approaches are unfortunately not feasible. This is because we only observe an individual in our sample in a given quarter if they have strictly positive earnings (in our sample's 24 states). If a non-negligible amount of boom-driven hires for certain groups come via inducing more labor force participation – as turns out to be the case – running a worker type-specific hiring rate regression would bias upwards the estimate of δ_i.

a firm in the first quintile. This is likely composed of workers who enter the labor market directly after high school since, by construction in the data, entering an employment relationship entails working at a job full time for multiple quarters. This indicates that credit boom-induced jobs are disproportionately taken by workers who have relatively little formal education or labor market experience. A priori, it is plausible that the future labor market prospects of these workers could be significantly affected by taking boom-induced jobs, either positively or negatively.

5 Impact of boom-induced jobs on worker earnings

In this section, we present our estimates for the effect of taking a boom-induced job on the worker's future earnings, β_i^{ret}. After going over worker-level sample and variables that we use from the LEHD in Section 5.1, we present graphical evidence of the impact of moving to a high-risk firm during periods of cheap credit in Section 5.2. We then state the formal conditions to consistently estimate β_i^{ret} in Section 5.3. The ideal variation is based off quasi-random variation in the propensity of workers to take jobs at firms whose spreads are quasi-randomly more exposed to aggregate conditions. We start in Section 5.4 by using variation that falls short of this idealized form, in that it is based off the actual decision of workers to move to either high-risk firms or to BB+ versus BBB- firms. To more plausibly satisfy the conditions to identify causal earnings effects, we present in Section 5.5 estimates from a design that exploits workers' parental connections to BB+ versus BBB- firms.

5.1 Data

Worker samples Our sample consists of all workers in our LEHD data, described in Section 4.2, that take a job at (or are predicted to take a job at) a Compustat firm between 1998-2012. The exact subset of these workers in our sample depends on the firm- and worker-level variation underpinning our different designs. To estimate earnings effects on a panel of workers for whom we can observe current and future earnings, we must drop certain workers (eg. those that leave the labor force); see Appendix Section A.6 for the precise details on these filters.

Worker outcome variables We focus on two worker-level outcome variables, described in detail in Appendix Section B.7. The first is $w_{it}^{(h)}$, the log of the worker's total earnings over quarters $t+h$ to $t+h+3$. Taking the log of earnings over four quarters smooths out volatile high-frequency changes, and also allows us to include workers in the sample who occasionally have quarters with zero earnings (eg. unemployment spells). Second, to understand the labor market dynamics underlying movements in $w_{it}^{(h)}$, we construct an indicator variable $displace_{it}^{(h)}$ that proxies for whether, over quarters $t+h$ to $t+h+3$, the worker becomes involuntary displaced from their job. We set the variable equal to one if the worker permanently separates from a (previously) full-time, stable job, and, as of the start of the next quarter, is not yet employed at a new firm. The latter condition makes it more likely that workers for whom $displace_{it}^{(h)} = 1$ leave their job due to being laid off, exposing them to a potentially costly spell of unemployment (Flaaen et al., 2019).

5.2 Graphical evidence

Before formally estimating β_i^{ret}, we present graphical evidence in Figure 8 of whether the average worker that takes a boom-induced job experiences different earnings dynamics. The left-hand side plots average annual earnings for the set of workers hired in a given year by a risky firm (fifth quintile of risk, in blue) versus a low-risk firm (first quintile, in red), normalized by earnings in the year prior to the hire. The right-hand side plots the fraction of the workers in each quintile that are displaced. The three panels show this plot for hiring years that correspond to the peak of the late 1990s credit cycle, 1998 (panel a), the peak of the pre-GFC cycle, 2006 (panel b), and the start of the post-GFC cycle, 2010. (panel c).

Each of the three episodes share three noteworthy features. First, and consistent with the selection effects documented in Section 4.4, the workers who join risky firms appear to always be relatively worse off. Even immediately after the hire takes place, the earnings growth of these workers is lower, and the displacement rate higher, than workers who join the least-risky firms. Second, there is a kink in the relative earnings growth of risky firm workers, along with an increase in the relative displacement rate, around the years that credit conditions spike downwards for each episode (2001, 2008, and 2015). Third, the 10% to 20% earnings differences across risky versus less-risky workers that open up during periods of credit market stress do not seem to close even after credit markets have recovered.

5.3 Specification and identification condition

To estimate the return to taking a boom-induced on a worker's year-h earnings, $\beta^{(h,ret)}$, we run quarterly worker-level regressions of a worker's outcomes on whether they take a boom-induced job. Among the set of workers who take a new job, as defined in Section 4.2, we run quarterly worker-level local projection-style regressions of their earnings on a function of the risk $\pi_{f(i,t),t}$ of the worker's new firm, interacted with aggregate credit conditions:

$$w_{it}^{(h)} = \alpha^{(h)} + \eta^{(h)} \cdot \pi_{f(i,t),t} + \gamma^{(h)} \times (\pi_{f(i,t),t}) \cdot c_{t-3,t}) + \psi_t + \boldsymbol{X}_{it} + \boldsymbol{X}_{f(i,t),t} + \epsilon_{it}^{(h)} \qquad (12)$$

The inclusion of quarter fixed effects means that $\gamma^{(h)}$ is estimated by comparing the future earnings dynamics of workers that move to risky vs. less-risky firms during the same quarter. This variation allows us to identify the causal object $\beta^{(h,ret)}$ from (9) if the following condition holds:

Identification condition II: worker human capital returns. *The OLS estimate of $\gamma^{(h)}$ in (12) identifies the causal return to worker earnings of taking a boom-induced job, $\beta^{(h,ret)}$ in (9), if and only if, conditional on controls, the following two conditions both hold:*

1. ***Firm-level identification condition holds for new firm $f(i,t)$:*** *Condition I is satisfied under $\pi_{f(i,t),t}$*
2. ***No differential worker selection into risky firms over the credit cycle:***

$$\mathbb{E}\big[c_t \cdot \mathbb{E}[\pi_{f(i,t),t} \cdot \overline{W_{it}}|t]\big] = 0$$

If $\pi_{f(i,t),t}$ is correlated with the sensitivity of the worker's new firm to aggregate nonfinancial conditions, violating the first condition in II, then $\gamma^{(h)}$ will pick up the effects of more than just the reduction of the new firm's spreads. Even if the second condition is satisfied, such that $\gamma^{(h)}$ reflects the causal impact of taking a job at the firm on the worker's earnings, it cannot be interpreted as the causal impact of taking a credit boom-induced job in particular. The second condition in II is violated if there is time-varying selection of which workers take jobs at risky firms that is correlated with the state of the credit cycle. This would be the case if either the economic expansions empirically associated with credit booms (Mian et al., 2017), or the causal impact of credit booms on the attractiveness of jobs at risky firms, lead different types of workers to take jobs at risky firms relative to normal times. For example, if workers perceive boom-induced jobs to be unduly risky, only workers with low outside options ($\overline{W_{it}}$) may choose to take them.

As such, to identify $\beta^{(h,ret)}$, it is necessary that we utilize variation that exhibits randomization in two senses: first, firm-level randomization in exposure to credit conditions, and second, worker-level randomization in the decision to move to highly-exposed firms.

5.4 Earnings dynamics associated with taking a boom-induced job

We start in this subsection by estimating earnings effects using the endogenous measure of take-up based off workers' actual decisions to move to risky firms. Because they embed worker selection, these estimates may not satisfy the conditions (II) necessary to interpret $\gamma^{(h)}$ as the causal effect of credit booms on short- and longer-run worker outcomes, even once we control for a rich set of worker-level characteristics. Still, the estimates are informative about how closely tied over the credit cycle the earnings dynamics of the workers who take boom-induced jobs are to the employment dynamics of risky firms themselves.

To mitigate the identification concerns discussed in 5.3, we include both worker-level controls $\boldsymbol{X}_{it}$ and controls for the hiring firm $\boldsymbol{X}_{f(i,t),t}$. We use the same firm-level controls as in the employment growth regressions (Table 1): region-by-industry quarter fixed effects for the worker's new job and interactions of the firm's risk quintile with lags of credit conditions and GDP growth. At the worker level, we non-parametrically control for a rich set of proxies for the worker's characteristics and outside labor market option. Specifically, we define a vector of binned values of variables related to the worker's demographics, observed labor market state at the start of quarter t, and past labor market outcomes.[34] The unique values of these worker-level vectors are then interacted with the industy-by-MSA quarter fixed effects. This means that $\gamma^{(h)}$ is estimated by comparing workers with observably similar characteristics and recent labor market histories that during credit booms move to firms with different risk.

As discussed in Section 3.5, only the economy's riskiest firms engage in a significant degree of boom-induced job creation. We thus focus on the results, shown in Figure 9, from a non-parametric specification in which we estimate earnings effects associated with taking a job at a firm in the fifth quintile of risk.[35] Workers who move to a risky firm experience boom-bust dynamics in earnings that are quantitatively and qualitatively similar to the firm employment estimates in Figure 2. During a credit boom, a worker who takes a job at a fifth-quintile firm experiencing a 100 basis point reduction in its spread has 1% higher earnings growth over the next eight quarters. But two years later, this effect sharply decreases, winding up at −1% before slowly converging back to zero. As seen in the right-hand plot, which shows the estimates when using $displace_{i,t+k}$ as the dependent variable in (12), the reversion in earnings occurs alongside a spike upwards in the probability of displacement.

Estimates using random firm variation As an intermediate step towards estimating earnings effects using randomization across both firms *and* workers, we first briefly consider estimates when utilizing just the random firm-level variation embedded in whether a worker takes a job at a BB+ or BBB- firm. Because BB+ and (matched) BBB- firms have similar labor demand dynamics over the business cycle absent changes in credit conditions, these estimates plausibly satisfy the first condition in (II). Additionally, there a priori should be less severe selection among observationally-equivalent workers that take jobs at

[34] The demographic variables are age, race, and sex; the labor market state variables are employment status and, if applicable, job quality and employment growth of the previous full-time job; and the labor market experience and outcome variables are years since full-time entry and average quarterly employment and earnings over the previous three years. The full details of exactly how we define and construct these variables, and the number of bins we form for each, are provided in Appendix Section B.8 .

[35] Appendix Figure A6 also shows the non-parametric estimates for the second, third, and fourth quintiles, while Appendix Figure A5 shows the estimates from a linear specification.

BB+ versus BBB- minus firms, compared to the selection with respect to firm default risk. This follows from the fact that workers seemingly do not perceive large differences in the value of jobs at BB+ versus BBB- firms, leading to less selection of individuals with different outside options into the relatively more-exposed BB+ firms.

To continue to control for observable worker characteristics, we adapt the establishment-level matching procedure developed in Section 3.4 to match observations at the worker-by-hiring firm level. For each worker in our sample that takes a new job at a firm with a BB+ credit rating, we try to find a worker with similar characteristics that, in the same quarter, takes a job that is in the same region-industry pair but at a firm with a BBB- rating. For worker-level characteristics, we require exact matches on the non-parametric controls used for Figure 9. For each BB+ mover, we then choose among the eligible potential BBB- movers by minimizing the Mahalanobis norm across the same (hiring) firm-level matching variables as described in (3.3), applying the same requirement that each of these variables be within 1 standard deviation for a match to be usable. For each BB+ worker i with a valid matched BBB- worker $m(i)$, we then estimate

$$(w_{it}^{(h)} - w_{m(i)t}^{(h)}) = \alpha^{(h)} + \gamma^{(h)} \times (\delta_{BB+} \cdot c_{t-3,t}) + (\boldsymbol{X}_{f(i,t)t} - \boldsymbol{X}_{f(m(i),t)t}) + \epsilon_{it}^{(h)} \tag{13}$$

where $\boldsymbol{X}_{f(i,t)t}$ includes the predicted effect of deviations of the firm-level matching variables for the treated versus control worker as well as lags of c_t and contemporaneous and lagged GDP growth.

Figure 10 plots the estimates for log earnings on the left-hand side and job displacement on the right-hand side. The estimated effects of moving to a firm with a 100 basis point lower spread reduction follow the same boost-bust pattern as in Figure 9. Moreover, the estimates in Figure 10 imply greater persistence in the post-boom earnings decline of workers that move to exposed firms, with a complete lack of recovery eight years after the hire.

5.5 Instrumenting for decision to take boom-induced job with parental connections

Even when comparing workers that move to firms with plausibly-random variation in exposure to aggregate credit conditions, the second condition of (II) may be violated. This would be the case if during credit booms, the marginal worker hired by a BB+ firm is different than that of a BBB- firm. This could be due to the selection produced by BB+ moving up their labor supply curves, or becoming more or less attractive (higher wages versus greater risk) to different types of workers, during credit booms. We thus now estimate the earnings effects of boom-induced jobs based off variation that exploits both firm- *and* worker-level variation. To do so, we need variation in the *ability or willingness* – not the *actual choice* – of workers to move to the jobs created by BB+ firms during credit booms. This variation must be sufficiently strong to induce highly-exposed workers to move to take BB+ jobs, while still being orthogonal to determinants of the worker's outside labor market options. We obtain such variation by exploiting a particular source of segmentation in the labor market: the firms at which an inexperienced worker's parents are employed.

Background on parental connections Staiger (2023) finds that in the LEHD, young individuals who have not yet fully entered the labor force – in the sense of never having held a full-time, stable job – are disproportionately likely to start their careers at a firm where one of their parents currently works. Around 5% of workers' first full-time job is at the same firm that currently employs one of their parents, with a strikingly-high 30% of all individuals working at their parent's firm at some point by the time they turn 30. Moreover, a worker is much more likely to utilize these connections when one of their parent's firms is temporarily increasing its hiring rate around the quarter in which the worker graduates from high school; Staiger (2023) estimates that such a fortunate concurrence of events leads the worker's earnings to be 20% than their otherwise-identical peers. Overall, parental connections are an important driver of whether young individuals take advantage of the job creation of certain firms when starting their careers.

Instrument construction We exploit the significance of parental connections to create an instrument for taking a boom-induced job based off whether an individual's parents work for a BB+ or a BBB- firm. As fully detailed in Appendix Section A.8, to construct the instrument, we follow Staiger (2023) in linking the LEHD to the 2000 and 2010 Decenniel Census micro-data, in which we can observe the parents of the universe of individuals who are under 18 as of 2000 or 2010. For each such individual, we take the firm associated with their parent's full-time job as of the their imputed high-school graduation quarter (the third quarter of the year of their 19^{th} birthday), and keep only individuals for which this firm has a credit rating of BB+ or BBB-.

We construct the instrument based off the parent's job as of an individual's high school graduation year instead of, say, the years in which a given individual actually enters the labor market or graduates college, for three reasons. First, an individual's decision of when to enter the labor market may be endogenous to their opportunity set; for example, a recent high school graduate who can more easily join a fast-growing firm may be less likely to pursue additional education. Using imputed graduation years based off the worker's birth year prevents this from driving variation in the instrument. Second, because the oldest individual in our sample is of four-year college graduation age only by 2004 or 2005, there is limited variation in aggregate credit conditions in the pre-GFC period, a significant portion of the years in our post-2000 sample. Third, Staiger (2023) estimates that parental connections are most important in driving the initial career decisions of individuals who are relatively less educated, presumably since these are the young workers whose labor market connections are the most limited outside of their parents.

As detailed in Appendix Section A.8, we apply two additional sample filters: we (a) require that the worker works full-time at a job within two years of their imputed high school graduation quarter, and (b) require that the worker has a full-time job as of the third quarter of the year in which they turn 28. These filters allow us to measure homogeneously-timed short- and longer-run labor market outcomes for potential entrants, while not requiring workers to have an uninterrupted spell of earnings at the start of their careers. The exclusion of workers who do not work at a full-time job in the eight quarters after high school graduation effectively drops workers from our sample who attend a four-year college. Because the majority of workers who actually take advantage of parental connections are those with little formal education (Staiger, 2023), this sample restriction should have little impact on our results. It also allows our estimates to capture the worker type that, as shown in Section 4.4, has the highest propensity to take boom-induced jobs.

Matching specification As with the HY/IG design considered in Section 5.4, we implement a matching procedure at the worker-by-firm level. But instead of matching on the characteristics of the BB+/BBB- firm that the worker actually moves to, we now match on the characteristics of the BB+/BBB- firm that the parent is employed at as of the entrant's high-school graduation year. These consist of the same firm- and firm-by region-by industry level variables as for the firm employment growth estimates (3.3), including requiring an exact match on the four-digit NAICS-MSA pair of the parent's job. Because the characteristics of the firm(s) at which the entrant actually works do not drive variation in the instrument, we need not condition on any aspect of the worker's endogenous labor market choices.

In addition to the characteristics of the parent's firm, we match on parent- and entrant-level variables meant to alleviate remaining potential concerns, minor as they likely are, about the selection of workers into BB+ versus BBB- firms whose children have possibly different outside options upon starting their careers. As detailed in Appendix Section B.9, these include variables that capture the demographics of the entrant and parent, including the parent's level of education; variables that capture the past labor market outcomes of the parent; and a proxies for how much internal firm power parents may have in order to give their children jobs (their earnings rank within the firm and the number of years they have been in their job). For a potential labor market entrant i with a BB+ parent, once we find a suitable match to an entrant with a BBB- parent, we run the worker-level matching regression as in the previous section (13), though with different left-hand side outcome variables, as we detail below.

Estimates Table 5 presents the estimates from this matching specification, for outcome variables that are precisely defined in Appendix Section B.10. Columns (1)-(3) show estimates for dependent variables that measure whether during credit booms, entrants with BB+ parents are more likely to become employed in their first full-time, stable job within eight quarters high-school graduation, both overall (column 1) and at their parent's BB+ firm in particular (column 2). These estimates are interpreted as the first-stage, in that they reveal the extent to which entrants that the instrument predicts should participate more in the boom actually do so. The estimate in Column (1) implies that in response to a 100 basis point decrease in spreads brought on by loose credit conditions, potential entrants are 4.9 percentage points more likely to enter a full-time, stable job in the eight quarters that follow their high-school graduation. The estimate in Column (2), of how much likelier these workers are to find such a job at their parent's firm in particular, is essentially the same number.[36] [37] As such, individuals with parental connections to the boom-induced jobs of BB+ firms are more likely not only to take these jobs, but also to find full-time employment more quickly, as compared to individuals with BBB- parents.

In columns (4)-(5) of Table 5, we estimate the short- and long-run earnings implications of workers taking boom-induced jobs. Specifically, we implement two-stage least-squares using the estimates in Column (2) as the first stage. The estimates can thus be interpreted as the treatment effect of a worker starting their career at a BB+ firm during a credit boom. Unsurprisingly, column (4) shows that the short-run effect – annualized log earnings over the eight quarters after high-school graduation – is large and positive; more quickly joining the labor market via taking a boom-induced job leads to a 4.4% increase in early-career earnings. But column (5) shows that, consistent with the evidence presented in Section 5.4, these initially-positive effects reverse over the long run: during the eight-quarter period that starts nine years after high school graduation, the workers annualized earnings are 8.2% lower.

The estimates in Table 5 show that while taking boom-induced jobs allow workers to more quickly

[36]Though an apples-to-apples comparison is not straightforward, these estimates are somewhat larger than those in Staiger (2023) that are based off the quarterly hiring rate of the parent's firm. Conceptually, there are two key differences in the labor demand shocks based off credit cycle exposure that we utilize versus those used by Staiger (2023): first, Staiger (2023)'s shocks are based off all sources of a firm's labor demand, nonfinancial and financial; and second, Staiger (2023)'s shocks are by design meant to be very transitory ones that may not even involve an increase in net employment growth.

[37]Column (3), for which the dependent variable is the number of quarters that it takes the worker to enter such a job, provides another way to quantify this: a 100 basis point decrease in the spreads of entrants with BB+ parents leads them to, on average, find their first job 0.8 quarters sooner after graduation.

find stable work at the start of their careers, this come at the expense of significantly reducing the value of their human capital for as long as 10 years after labor market entry.

6 Implications of results

In this section, we discuss the implications of the findings from the previous sections. In Section 6.1, we contextualize the overall earnings effects and the intertemporal tradeoff that our estimates imply boom-induced jobs impose on workers. In Section 6.2, we discuss what aggregate implications we can draw from our cross-sectional estimates.

6.1 Intertemporal tradeoff of boom-induced jobs

Net present value of taking boom-induced job Our estimates imply that boom-induced jobs provide workers with short-run benefits at the expense of longer-run costs. To understand how large the future losses of boom-induced jobs are relative to the initial gains, we compute the net present value (NPV) of the job's estimated impact on earnings. Cumulating over the period-by-period effects $\beta^{(h-ret)}$ and letting ρ be the worker's discount rate, the NPV is given by

$$NPV = \sum_{h=0}^{\infty} \frac{\beta^{(h-ret)}}{(1+\rho)^{h+1}} \tag{14}$$

By plugging our estimates $\gamma^{(h)}$ into this expression, we can compute the NPV under two assumptions: first, that the earnings effects of boom-induced jobs do not last more than nine years (the end date of our estimates), and second, that our estimates $\gamma^{(h)}$ identify the causal parameter $\beta^{(h-ret)}$. We use the estimates from the specification that exploits variation in whether workers join a BB+ or BBB- rated firm (Figure 10). This is because, as discussed in Section 3.5, the effect of credit booms on BB+ firms' relative employment growth is comparable to the effect that booms have on risky, financially-constrained firms. Since it is these firms that significantly increase job creation during booms (Table 3), we can measure the NPV of boom-induced jobs by using the earnings estimates associated with jobs at BB+ firms.[38]

Concretely, consider the NPV of a worker taking a job at a BB+ firm that experiences the reduction in credit spreads, relative to a BBB- firm, that we estimate occurred for the three distinct credit cycles in our 1998 – 2020 sample. Across the three cycles, the average deviation of c_t from its sample mean to its

[38]Unfortunately, we cannot estimate the NPV using the estimates from Section 5.5 that are based off quasi-random variation in which workers take jobs at BB+ firms. As discussed in that section, data constraints make it feasible to only estimate snapshots of the short- and long-run earnings effects, rather than the year-by-year estimates that equation (14) requires.

peak was around half a standard deviation, which corresponds to a relative decrease in BB+ spreads of 25 basis points. We multiply the earnings estimates $\gamma^{(h)}$ by 0.25 and plug them into equation (14), and set the discount rate ρ equal to the average risk-free rate over our LEHD sample period (2%), to obtain $NPV \approx -3.8\%$.[39] This means that, for the credit cycles in our LEHD sample, the average worker who took a boom-induced job at a BB+ firm received an NPV of −3.8%, relative to a worker who took a job at a BBB- firm.

There is an important caveat to this NPV calculation: it only applies in an *ex post* (equivalently, perfect foresight) sense with respect to the realization of future aggregate conditions. This is because our LEHD sample allows us to estimate worker-level effects over a relatively small time series (1998-2020) with just three distinct credit cycle episodes. While our estimates are informative about what value boom-induced jobs ended up providing workers for the credit booms in our sample, they may not necessarily reflect the value of these jobs in an *ex ante* (full information rational expectations) sense. One cannot, for example, conclude that workers who moved to BB+ firms during the booms in our sample made an ex ante "mistake" in doing so.

Magnitude of intertemporal tradeoff Our estimates reveal that boom-induced jobs imposed a substantial intertemporal tradeoff on workers during the credit cycles in our sample. The estimates of Section 5.5 that utilize quasi-random variation across both workers and firms imply that, for the average credit boom in our sample, a boom-induced job at a BB+ firm caused the earnings of a recent high-school graduate to be 8.2% lower. This is comparable in magnitude to the long-term effects that past work has found are associated with finishing school and entering the labor market during a recession. Based off Schwandt and von Wachter (2019), a high-school graduate who entered the labor market during the troughs of the two recessions in our sample, 2001 and 2007-09, had, respectively, 4% and 13% lower earnings seven years later.[40]

[39] In calculating this number, there are two particular choices we make that, arguably, make $NPV \approx -3.8\%$ a lower bound in absolute magnitude. First, we consider the effect of jobs created by the set of high-yield firms with the highest sub-investment grade credit rating, BB+. While the semi-elasticity of employment that we estimate for BB+ firms is comparable to that for the average high-yield firm (Table 3), the estimated impact of credit booms on the spreads of BB+ firms is mechanically lower. If, for example, we compute the NPV by using the same earnings estimates but multiplying them by the average reduction in spreads experienced by firms in the upper quintile, relative to firms in the first quintile, we obtain $NPV \approx -9.5\%$. Second, setting the discount rate ρ equal to the risk-free rate neglects the possibility that taking a boom-induced job increase the idiosyncratic or aggregate risk in a worker's labor income. The higher risk associated with boom-induced jobs could imply that the worker attaches a higher discount rate to the job's short-run benefits and a lower discount rate to the job's future costs. For example, Graham et al. (2023) structurally estimate that, given the composition of the average household's portfolio of financial wealth and human capital, it is appropriate to use a −7.4% discount rate to value the earnings losses from a worker's firm going bankrupt.

[40] We compute this number by first computing peak-to-trough increase in the unemployment rate over the two recessions. Based off the unemployment rate chronology of Dupraz et al. (2023), this is approximately 2% for the 2001 recession (inclusive of the "jobless recovery" after the NBER-dated recession itself ended) and 6.5% for the 2007-09 recession. We then multiply these

We consider the seven year effect for a particular reason: it is the number of years between when risky firms' boom-time employment growth starts to reverse (year three) and the horizon of our worker estimates (year ten). The similarity between the seven year effect of recessions and the ten year effect that we estimate suggests a particular economic mechanism behind the future costs of boom-induced jobs. When credit conditions are loose, workers with negligible labor market experience or formal skills take the jobs that risky firms create and enjoy a period of relatively high earnings. When these jobs are destroyed, they go back to their initial state – essentially, turning back into a new labor market entrant – but at a time at which it is even more costly to be in this state than before, a recession.

This discussion brings into focus the intertemporal tradeoffs that the credit booms in our sample generated in the labor market. The jobs that risky firms were driven to create effectively caused young workers to borrow against their future labor market income. This reveals that the boom-bust dynamics associated with credit cycles extend far beyond financial markets and debt-financed consumption.

6.2 Aggregate implications

We now briefly consider the aggregate implications of our cross-sectional firm and worker results. A full quantitative analysis of these implications is outside the scope of our paper. We focus on suggestive evidence from analyses that we describe in fuller detail in Appendix Section H.

Mapping cross-sectional estimates to aggregate effects From an aggregate perspective, we care about the causal impact that loose credit conditions have on the *average* worker via job creation. Our cross-sectional estimates do not necessarily capture this effect, due to two "missing intercept" problems. First, our finding that risky firms create short-lived jobs during credit booms is based off cross-firm variation in exposure to credit conditions. If credit booms also cause low-risk firms to create jobs – either because of direct financial market effects or general equilibrium effects – and these jobs have more favorable long-run consequences on the workers who take them compared to the jobs created by risky firms, our estimates may overstate the intertemporal tradeoff that credit booms impose. Second, even if low-risk firms do expand their employment during booms, risky firms' job creation could allow workers to more easily obtain jobs at low-risk firms. For example, suppose that, prior to a credit boom, there are more recent graduates trying to obtain employment than there are vacant jobs. Then, when a boom induces

increases by the year-seven estimate from Figure 8 of Schwandt and von Wachter (2019) of the earnings effects of a high-school graduate entering the labor market when the unemployment rate is 1 pp higher, which is around −2%. Note that while we focus on the results of Schwandt and von Wachter (2019), since these are based off U.S. data from 1976 to 2016, the magnitudes that they estimate are similar to those from other papers in the literature (von Wachter, 2020).

risky firms to create jobs, there will be less competition in the labor market for low-risk jobs. As a result, the workers who take low-risk jobs during booms – the control group in our cross-sectional worker regressions – will have indirectly benefited from loose credit conditions. Below, we discuss suggestive evidence that, in practice, these missing intercept concerns do not change the conclusion of our results.

Missing intercept I: job creation of low-risk firms We present three different results that, together, suggest that loose credit conditions do not cause low-risk firms to engage in a significant amount of job creation.

First, low-risk Compustat firms do not appear to increase their level of real investment during credit booms. Appendix Figure A8 shows estimates for the response of firms in all five risk quintiles to c_t. While firms in the first quintile issue significantly more debt when conditions are loose, they do so in order to increase equity repurchases; as a result, their total amount of external financing and assets do not significantly change. This is consistent with the theoretical prediction of Stein (1996) that the real investment of financially-unconstrained firms should, under certain conditions, not be affected by variation in investors' valuation of their liabilities; it also accords with empirical finding of Ma (2019) that low credit spreads lead large firms to engage in financing arbitrage across debt and equity markets.

Second, the relatively high growth of risky firms during booms can entirely account for the empirical covariance between c_t and aggregate employment growth. We conduct a partial equilibrium exercise similar to Chodorow-Reich (2014), in which we impose the assumption that the employment growth of firms in the first quintile of risk is not affected by changes in c_t. For establishments controlled by riskier Compustat firms in each year, we compute the growth rate that our estimates imply would have prevailed had c_t been at its sample average. Appendix Table A7 shows that the aggregate growth rate under these counterfactual rates only weakly covaries with c_t, in contrast to the strong relationship of c_t with the actual aggregate growth rate.

Third, in regions that are more highly exposed to the boom-induced job creation of risky firms, there do not appear to be significant general equilibrium spillovers to other firms. At the MSA level, we construct a Bartik instrument, similar to Giroud and Mueller (2021), in which exposure to c_t is predicted based off the ex ante employment shares of Compustat firms with different levels of risk. Appendix Table A8 shows that the estimated impact on total MSA-level employment (public and private firms) of a 100 basis point reduction in the (weighted) average spread faced by the MSA's firms is only slightly higher than the firm-level effect of 2 (Table 1). This suggests that risky firms' job creation is associated with a limited degree of either general equilibrium crowd-out or crowd-in (Mian et al., 2022).

Missing intercept II: ability to obtain existing low-risk jobs Even if the amount of job creation by lower-risk firms is not affected by credit booms, workers may find it easier to obtain such jobs in booms. This would be the case if the labor market features strong congestion effects: the workers who take newly-created jobs at risky firms would compete less aggressively for already-existing jobs at lower-risk firms.[41] Indeed, the regional effects of exposure to boom-induced job creation that we estimate in Appendix Table A8 suggest that this is the case: risky firms create jobs without crowding out the employment growth of other firms. In principle, then, boom-induced job creation could positively affect some workers, even in the future.

We present two preliminary results in the appendix that suggests that these positive indirect effects, while potentially important, are dominated by the direct negative effects of boom-induced job creation that we estimate. First, while risky firms' job creation does not appear to initially crowd out the job creation of other firms, there appear to be significantly negative spillovers in future years. This is shown in Appendix Figure A9, which, using our MSA-level Bartik instrument, plots the estimated dynamic effects of regional exposure to boom-induced job creation. The cumulative four-year effect that we estimate at the MSA level (-1.75% of initial employment) is well below the four-year Compustat firm effect (less than -0.5%, Figure 2). Second, and more directly, workers who graduate high school in an MSA that is highly exposed to boom-induced job creation experience boom-bust dynamics in their earnings. For recent high-school graduates in our LEHD sample over 2000 – 2012, Appendix Figure A10 shows the estimated short- and long-run earnings effects associated with MSA-level exposure to boom-induced job creation. In MSAs that are more exposed to the job creation of risky firms during booms, the average high-school graduate's earnings – without conditioning on whether they actually take a boom-induced job – exhibit strong boom-bust dynamics. Consistent with the back-of-the-envelope exercise conducted in Section 6.1, the magnitude of the long-term future effects is around a quarter of the estimated effects of graduating high school in an MSA with an elevated unemployment rate (Appendix Figure A11). We view these results as preliminary evidence that the positive labor market spillovers of boom-induced job creation are, for the credit booms in our sample, dominated by the direct negative effects that we estimate, as well as potential sources of negative spillovers (eg. future decreases in aggregate demand).

[41] In other work (Blank and Maghzian, 2023), we find evidence that such congestion effects are important: the amount of workers who are unemployed at a given point in time is an important determinant of a given unemployed worker's ability to find a high-paying job.

7 Conclusion

We use administrative U.S. Census data to estimate the causal impact of aggregate credit booms on firm job creation, and the causal short- and longer-run effects that boom-induced jobs have on the workers who take them. We find that loose conditions lead risky firms to engage in a significant amount of job creation, but to later destroy these jobs when they experience financial distress. These jobs are disproportionately taken by young, inexperienced workers with little formal education. We estimate that these workers obtain higher short-run earnings as a causal effect of taking boom-induced jobs, at the expense of significantly lower long-run earnings. The future costs implied by our estimates are large and, based off preliminary regional evidence, appear to exist even for the average young worker in the economy.

Our finding that credit booms can lead workers to effectively borrow against their future labor income may have important policy implications that we plan to explore in future work. Monetary policy may have limited ammunition to support strong and inclusive labor markets, to the extent that increased risk-taking in credit markets is an important transmission mechanism of loose policy (Bauer et al., 2023; Kashyap and Stein, 2023). But it is conceivable that the negative long-term effects that occur during our sample period could be mitigated by policies that more actively support the workers of distressed firms, or the functioning of credit markets, when credit conditions start to revert. To make progress, we must understand what motivates workers to take boom-induced jobs – including whether they, like lenders, sometimes neglect to consider the risks associated with credit booms – and whether there are circumstances in which loose credit market conditions support the creation of productive, sustainable jobs.

References

Aaronson, S. R., M. C. Daly, W. L. Wascher, and D. W. Wilcox (2019). Okun revisited: Who benefits most from a strong economy? Brookings Papers on Economic Activity, 333–375.

Abadie, A. and J. Spiess (2022). Robust post-matching inference. Journal of the American Statistical Association 117(538), 983–995.

Altman, E. I. and D. L. Kao (1992). The implications of corporate bond ratings drift. Financial Analysts Journal 48(3), 64–75.

Arnold, D. (2021). Mergers and acquisitions, local labor market concentration, and worker outcomes. Working paper.

Baron, M. and W. Xiong (2017). Credit expansion and neglected crash risk. Quarterly Journal of Economics 132, 713–764.

Bauer, M. D., B. S. Bernanke, and E. Milstein (2023, February). Risk appetite and the risk-taking channel of monetary policy. Journal of Economic Perspectives 37(1), 77–100.

Becker, B. and V. Ivashina (2014). Cyclicality of credit supply: Firm level evidence. Journal of Monetary Economics 62, 76–93.

Benguria, F. and A. M. Taylor (2020, December). After the panic: Are financial crises demand or supply shocks? evidence from international trade. American Economic Review: Insights 2(4), 509–26.

Benmelech, E., N. Bergman, and A. Seru (2021, 05). Financing Labor. Review of Finance 25(5), 1365–1393.

Benmelech, E., C. Frydman, and D. Papanikolaou (2019). Financial frictions and employment during the great depression. Journal of Financial Economics 133(3), 541–563.

Bentolila, S., M. Jansen, and G. Jiménez (2017, 08). When Credit Dries Up: Job Losses in the Great Recession. Journal of the European Economic Association 16(3), 650–695.

Bergman, N., D. A. Matsa, and M. Weber (2020). Heterogeneous labor market effects of monetary policy. Working paper.

Bernanke, B. S. and M. Gertler (1995, December). Inside the black box: The credit channel of monetary policy transmission. Journal of Economic Perspectives 9(4), 27–48.

Bernanke, B. S., M. Gertler, and S. Gilchrist (1999). Chapter 21 the financial accelerator in a quantitative business cycle framework. Volume 1 of Handbook of Macroeconomics, pp. 1341–1393. Elsevier.

Berton, F., S. Mocetti, A. F. Presbyter, and M. Richiardi (2018, 01). Banks, Firms, and Jobs. The Review of Financial Studies 31(6), 2113–2156.

Bharath, S. T. and T. Shumway (2008). Forecasting default with the merton distance to default model. The Review of Financial Studies 21(3), 1339–1369.

Billett, M. T., T.-H. D. King, and D. C. Mauer (2007). Growth opportunities and the choice of leverage, debt maturity, and covenants. The Journal of Finance 62(2), 697–730.

Blank, M. and O. Maghzian (2023). The labor market spillovers of job destruction. Working paper.

Bordalo, P., N. Gennaioli, and A. Shleifer (2018). Diagnostic expectations and credit cycles. The Journal of Finance 73(1), 199–227.

Borio, C., E. Kharroubi, C. Upper, and F. Zampolli (2016). Labour reallocation and productivity dynamics: financial causes, real consequences. BIS Working Papers.

Borusyak, K., P. Hull, and X. Jaravel (2021, 06). Quasi-Experimental Shift-Share Research Designs. The Review of Economic Studies 89(1), 181–213.

Charles, K. K., E. Hurst, and M. J. Notowidigdo (2018, October). Housing booms and busts, labor market opportunities, and college attendance. American Economic Review 108(10), 2947–94.

Chava, S. and A. Hsu (2019, 11). Financial Constraints, Monetary Policy Shocks, and the Cross-Section of Equity Returns. The Review of Financial Studies 33(9), 4367–4402.

Chernenko, S. and A. Sunderam (2012, 7). The Real Consequences of Market Segmentation. The Review of Financial Studies 25(7), 2041–2069.

Chodorow-Reich, G. (2014, 10). The Employment Effects of Credit Market Disruptions: Firm-level Evidence from the 2008–9 Financial Crisis. The Quarterly Journal of Economics 129(1), 1–59.

Chow, M., T. C. Fort, C. Goetz, N. Goldschlag, J. Lawrence, E. R. Perlman, M. Stinson, and T. K. White (2021, 3). Redesigning the longitudinal business database. Census Working Papers CES-21-08.

Cloyne, J., C. Ferreira, M. Froemel, and P. Surico (2023, 03). Monetary Policy, Corporate Finance, and Investment. Journal of the European Economic Association, jvad009.

Coppola, A. (2022). In Safe Hands: The Financial and Real Impact of Investor Composition Over the Credit Cycle. Working paper.

Cornaggia, J. and K. J. Cornaggia (2013, 09). Estimating the Costs of Issuer-Paid Credit Ratings. The Review of Financial Studies 26(9), 2229–2269.

Crouzet, N. and N. R. Mehrotra (2020, November). Small and large firms over the business cycle. American Economic Review 110(11), 3549–3601.

Curtis, E. M., D. G. Garrett, E. Horn, K. A. Roberts, and J. C. S. Serrato (2022). Capital investment and labor demand. Working paper.

Davis, S. J. and J. Haltiwanger (1992). Gross job creation, gross job destruction, and employment reallocation. The Quarterly Journal of Economics 107(3), 819–863.

Davis, S. J. and T. von Wachter (2011). Recessions and the costs of job loss [with comments and discussion]. Brookings Papers on Economic Activity, 1–72.

Di Maggio, M. and A. Kermani (2017, 06). Credit-Induced Boom and Bust. The Review of Financial Studies 30(11), 3711–3758.

Ding, X., T. C. Fort, S. J. Redding, and P. K. Schott (2022). Structural change within versus across firms: Evidence from the united states. Working paper.

Dupraz, S., E. Nakamura, and J. Steinsson (2023). A plucking model of business cycles. Working Paper.

Dávila, E. and A. Walther (2023, July). Prudential policy with distorted beliefs. American Economic Review 113(7), 1967–2006.

Falato, A. and N. Liang (2016). Do creditor rights increase employment risk? evidence from loan covenants. The Journal of Finance 71(6), 2545–2590.

Fallick, B., J. C. Haltiwanger, E. McEntarfer, and M. Staiger (2021). Job displacement and job mobility: The role of joblessness. Working paper.

Fama, E. F. and K. R. French (1993). Common risk factors in the returns on stocks and bonds. Journal of Financial Economics 33(1), 3–56.

Flaaen, A., M. D. Shapiro, and I. Sorkin (2019, April). Reconsidering the consequences of worker displacements: Firm versus worker perspective. American Economic Journal: Macroeconomics 11(2), 193–227.

Fontanier, P. (2022). Optimal policy for behavioral financial crises. Working paper.

Fort, T. C., J. Haltiwanger, R. S. Jarmin, and J. Miranda (2013). How firms respond to business cycles: The role of firm age and firm size. IMF Economic Review 61(3), 520–559.

Fujita, S. and G. Moscarini (2017, December). Recall and unemployment. American Economic Review 107(12), 3875–3916.

Gertler, M. and S. Gilchrist (1994, 05). Monetary Policy, Business Cycles, and the Behavior of Small Manufacturing Firms*. The Quarterly Journal of Economics 109(2), 309–340.

Gilchrist, S. and E. Zakrajšek (2012). Credit spreads and business cycle fluctuations. American Economic Review 102, 1692–1720.

Giroud, X. (2013, 03). Proximity and Investment: Evidence from Plant-Level Data. The Quarterly Journal of Economics 128(2), 861–915.

Giroud, X. and H. M. Mueller (2019, October). Firms' internal networks and local economic shocks. American Economic Review 109(10), 3617–49.

Giroud, X. and H. M. Mueller (2021). Firm leverage and employment dynamics. Journal of Financial Economics 142(3), 1381–1394.

Gopinath, G., S. Kalemli-Özcan, L. Karabarbounis, and C. Vellegas-Sanchez (2017, 06). Capital allocation and productivity in south europe. The Quarterly Journal of Economics 132(4), 1915–1967.

Gourio, F., A. K. Kashyap, and J. W. Sim (2018, 3). The tradeoffs in leaning against the wind. International Monetary Fund 66, 70–115.

Graham, J. R., H. Kim, I. Li, and J. Qiu (2023). Employee costs of corporate bankruptcy. The Journal of Finance 78(4).

Green, D. (2018). Corporate refinancing, covenants, and the agency cost of debt. Working paper.

Greenwood, R. and S. G. Hanson (2013). Issuer quality and corporate bond returns. Review of Financial Studies 26, 1483–1525.

Greenwood, R., S. G. Hanson, and L. J. Jim (2019). Reflexivity in credit markets. Working paper.

Greenwood, R., S. G. Hanson, A. Shleifer, and J. A. Sorensen (2022). Predictable financial crises. The Journal of Finance 77(2), 863–921.

Gürkaynak, R., B. Sack, and J. Wright (2007). The u.s. treasury yield curve: 1961 to the present. Journal of Monetary Economics 54(8), 2291–2304.

Haltiwanger, J., H. Hyatt, and E. McEntarfer (2018). Who moves up the job ladder? Journal of Labor Economics 36(S1), S301–S336.

Haltiwanger, J., H. Hyatt, E. McEntarfer, and M. Staiger (2021). Cyclical worker flows: Cleansing vs. sullying. Working paper.

Haltiwanger, J. C., H. R. Hyatt, L. B. Kahn, and E. McEntarfer (2018, April). Cyclical job ladders by firm size and firm wage. American Economic Journal: Macroeconomics 10(2), 52–85.

He, Z. and A. Krishnamurthy (2013, April). Intermediary asset pricing. American Economic Review 103(2), 732–70.

Huber, K. (2018, March). Disentangling the effects of a banking crisis: Evidence from german firms and counties. American Economic Review 108(3), 868–98.

Hyatt, H., E. McEntarfer, K. McKinney, S. Tibbets, and D. Walton (2014, September). Job-to-job (j2j) flows: New labor market statistics from linked employer-employee data. CES Working Paper Series (14-34).

Imbens, G. W. (2015). Matching methods in practice: Three examples. The Journal of Human Resources 50(2), 373–419.

Ippolito, F., A. K. Ozdagli, and A. Perez-Orive (2018). The transmission of monetary policy through bank lending: The floating rate channel. Journal of Monetary Economics 95, 49–71.

Jacobson, L. S., R. J. LaLonde, and D. G. Sullivan (1993). Earnings losses of displaced workers. The American Economic Review 83(4), 685–709.

Jeenas, P. (2018). Monetary Policy Shocks, Financial Structure, and Firm Activity: A Panel Approach. SSRN Electronic Journal, 1–65.

Jordá, Ó. (2005, March). Estimation and inference of impulse responses by local projections. American Economic Review 95(1), 161–182.

Jorda, O., M. Schularick, and A. M. Taylor (2013). When credit bites back. Journal of Money, Credit and Banking 45(s2), 3–28.

Kacperczyk, M. and P. Schnabl (2010, March). When safe proved risky: Commercial paper during the financial crisis of 2007-2009. Journal of Economic Perspectives 24(1), 29–50.

Kalantzis, Y. (2015, 03). Financial fragility in small open economies: Firm balance sheets and the sectoral structure. The Review of Economic Studies 82(3), 1194–1222.

Kashyap, A. K. and J. C. Stein (2023, February). Monetary policy when the central bank shapes financial-market sentiment. Journal of Economic Perspectives 37(1), 53–76.

Kirti, D. (2018). Lending standards and output growth. IMRF Working Paper.

Klimek, S. and T. Fort (2018). The effects of industry classification changes on us employment composition. CES Working Paper.

Krishnamurthy, A. and T. Muir (2020). How credit cycles across a financial crisis. Working paper.

Laeven, L. and A. Popov (2016, May). A lost generation? education decisions and employment outcomes during the us housing boom-bust cycle of the 2000s. American Economic Review 106(5), 630–35.

Lemmon, M. and M. R. Roberts (2010). The response of corporate financing and investment to changes in the supply of credit. The Journal of Financial and Quantitative Analysis 45(3), 555–587.

Lin, H., J. Wang, and C. Wu (2011). Liquidity risk and expected corporate bond returns. Journal of Financial Economics 99(3), 628–650.

Lopez-Salido, D., J. Stein, and E. Zakrajsek (2017, 8). Credit-market sentiment and the business cycle. Quarterly Journal of Economics 132, 1373–1426.

Ma, Y. (2019). Nonfinancial firms as cross-market arbitrageurs. The Journal of Finance 74(6), 3041–3087.

Maxted, P. (2023, 03). A macro-finance model with sentiment. The Review of Economic Studies, rdad023.

Merton, R. C. (1974). On the pricing of corporate debt: The risk structure of interest rates. The Journal of Finance 29(2), 449–470.

Mian, A., K. Rao, and A. Sufi (2013). Household balance sheets, consumption, and the economic slump. The Quarterly Journal of Economics 128(4), 1687–1726.

Mian, A., A. Sarto, and A. Sufi (2022). Estimating credit multipliers. Working paper.

Mian, A., A. Sufi, and E. Verner (2017, 11). Household debt and business cycles worldwide. Quarterly Journal of Economics 132, 1755–1817.

Mian, A., A. Sufi, and E. Verner (2020). How does credit supply expansion affect the real economy? the productive capacity and household demand channels. The Journal of Finance 75(2), 949–994.

Moscarini, G. and F. Postel-Vinay (2012, May). The contribution of large and small employers to job creation in times of high and low unemployment. American Economic Review 102(6), 2509–39.

Moscarini, G. and F. Postel-Vinay (2018). The cyclical job ladder. Annual Review of Economics 10(1), 165–188.

Müller, K. and E. Verner (2023). Credit allocation and macroeconomic fluctuations. Working paper.

Nini, G., D. C. Smith, and A. Sufi (2012). Creditor control rights, corporate governance, and firm value. The Review of Financial Studies 25(6), 1713–1761.

Ottonello, P. and T. Winberry (2020). Financial heterogeneity and the investment channel of monetary policy. Econometrica 88(6), 2473–2502.

Ozdagli, A. K. (2017). Financial Frictions and the Stock Price Reaction to Monetary Policy. The Review of Financial Studies (617).

Ponticelli, J., M. Tsoutsoura, S. Lagaras, F. Moraes, A. Araujo, and R. Ferreira (2022). The labor cost of judicial bias in bankruptcy. Working paper.

Schmitt-Grohé, S. and M. Uribe (2016). Downward nominal wage rigidity, currency pegs, and involuntary unemployment. Journal of Political Economy 124(5), 1466–1514.

Schularick, M. and A. M. Taylor (2012). Credit booms gone bust: Monetary policy, leverage cycles, and financial crises, 1870-2008. American Economic Review 102, 1029–1061.

Schwandt, H. and T. von Wachter (2019). Unlucky cohorts: Estimating the long-term effects of entering the labor market in a recession in large cross-sectional data sets. Journal of Labor Economics 37(S1), S161–S198.

Sharpe, S. A. (1994). Financial market imperfections, firm leverage, and the cyclicality of employment. The American Economic Review 84(4), 1060–1074.

Simsek, A. and R. Caballero (2023). A monetary policy asset pricing model. Working paper.

Sorensen, J. (2021). Risk neglect in the corporate bond market. Working paper.

Spletzer, J. R. (2014, June). Inequality statistics from the lehd. CES Working Paper Series.

Staiger, M. (2023). The intergenerational transmission of employers and the earnings of young workers. Working paper.

Stein, J. C. (1996). Rational capital budgeting in an irrational world. The Journal of Business 69(4), 429–455.

Strebulaev, I. A. and B. Yang (2013). The mystery of zero-leverage firms. Journal of Financial Economics 109(1), 1–23.

Svensson, L. E. (2017, 10). Cost-benefit analysis of leaning against the wind. Journal of Monetary Economics 90, 193–213.

Tello-Trillo, C. and S. Streiff (2020, September). Matching compustat data to the business register 1976-2016. CES Technical Notes Series 20(07).

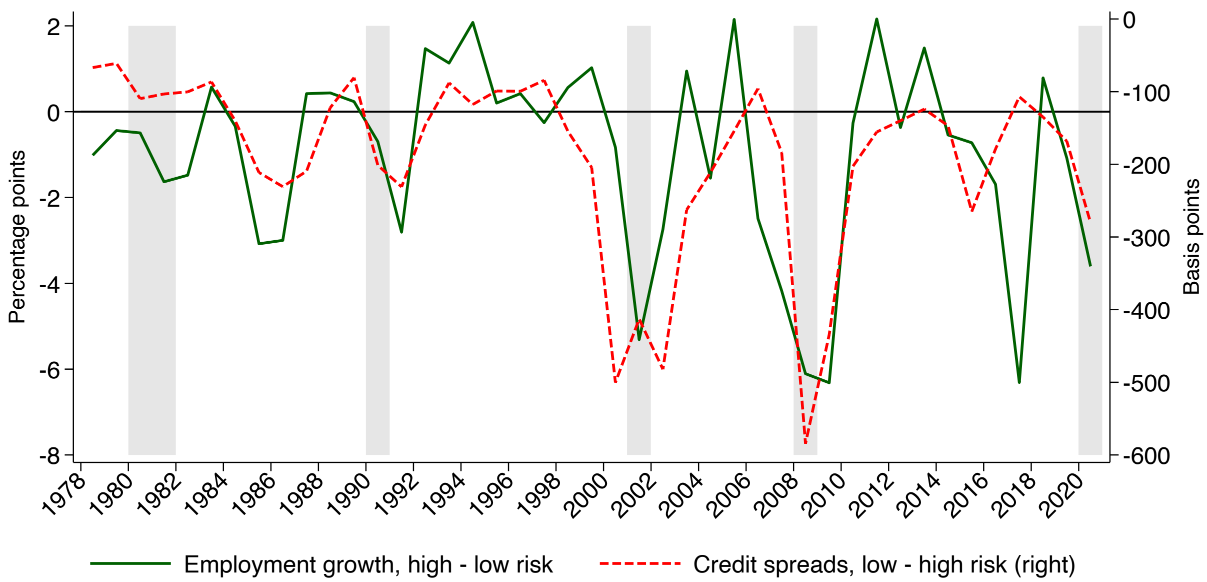
von Wachter, T. (2020, November). The persistent effects of initial labor market conditions for young adults and their sources. Journal of Economic Perspectives 34(4), 168–94.

Yagan, D. (2019). Employment hysteresis from the great recession. Journal of Political Economy 127(5), 2505–2558.

Zullig, G. (2022). Heterogeneous employment effects of firms' financial constraints and wageless recoveries. Working paper.

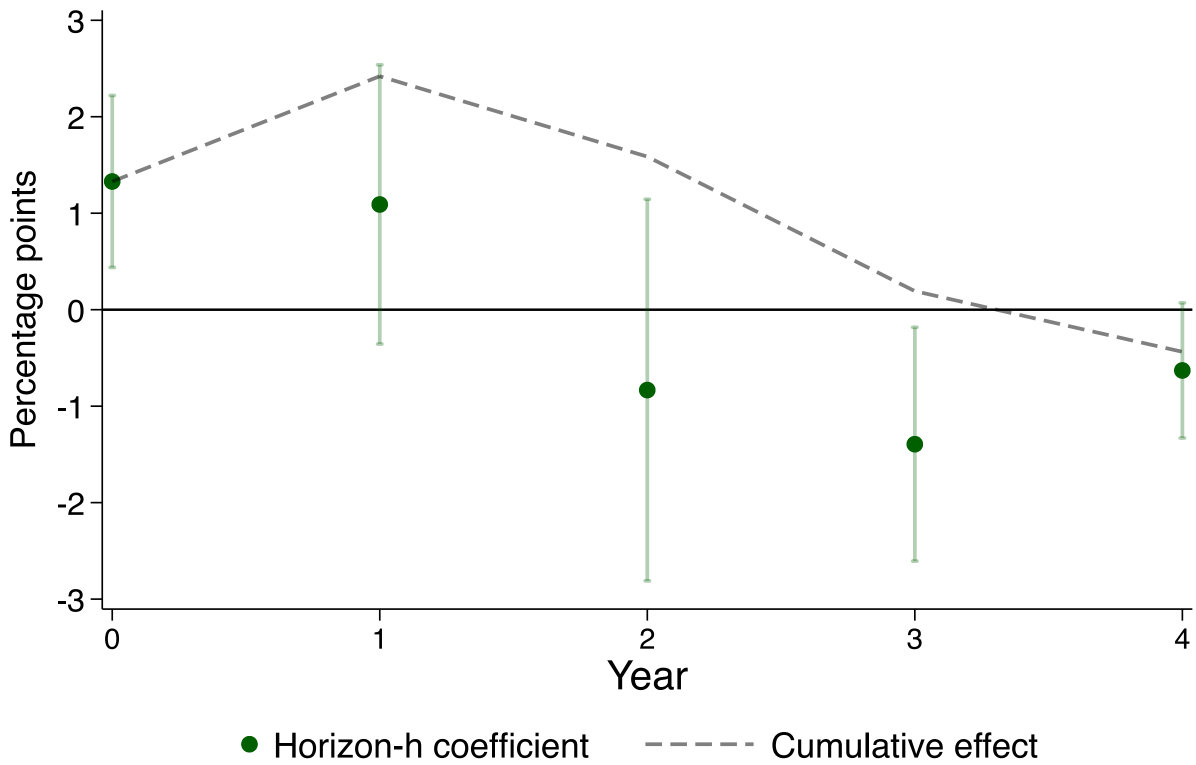
Figures

Figure 1: Relative employment growth and credit spreads of high- vs. low-risk firms



Notes: This figure plots the annual employment growth and predicted credit spreads of firms with high default risk relative to firms with low default risk. The sample consists of manufacturing establishments that, at the start of a given credit cycle episode (1978, 1983, 1992, 2003, 2010, or 2016), are controlled by a public firm. At the start of each episode, firms in the sample are put into quintiles of default risk π_{ft}. Default risk is proxied by the negative of Merton (1974) distance to default, constructed following Bharath and Shumway (2008) (see Appendix Section B.2). The solid green line (left y-axis) plots the weighted-average employment growth rate of establishments controlled by firms in the fifth quintile of risk ("high-risk firms") minus the growth rate of establishments controlled by firms in the first quintile of risk ("low-risk firms"). Employment growth is measured using the symmetric growth rate of Davis and Haltiwanger (1992) given by Equation (2.3). The dashed red line (right y-axis) plots the difference in predicted credit spreads between low- and high-risk firms. This is computed from the measure c_t of aggregate credit conditions that is based off the year-specific relationship between credit spreads and π_{ft} in the bond market (see Appendix Section B.1). The dashed red line shows c_t after it is multiplied by the difference in π_{ft} between the average firm in the first quintile and the average firm in the fifth quintile. This allows one to interpret the dashed red line as the predicted difference in credit spreads between low- and high-risk firms. The solid green line's value is large when high-risk firms' employment growth is high relative to low-risk firms' growth, while the dashed red line's value is large when there is a less negative difference in the credit spreads of low-risk firms relative to high-risk firms. The correlation between the credit spread series and the employment growth series is, leading the employment series by $h \geq 0$ years, 0.54 for $h = 0$, 0.35 for $h = 1$, -0.09 for $h = 2$, -0.34 for $h = 3$, $-.07$ for $h = 4$. The correlation between the credit spread series and the capital growth series is, leading the capital series by $h \geq 0$ years, -0.07 for $h = 0$, 0.31 for $h = 1$, 0.48 for $h = 2$, 0.37 for $h = 3$, 0.21 for $h = 4$.

Figure 2: Local projections of response of annual establishment-level employment to credit conditions, Compustat sample

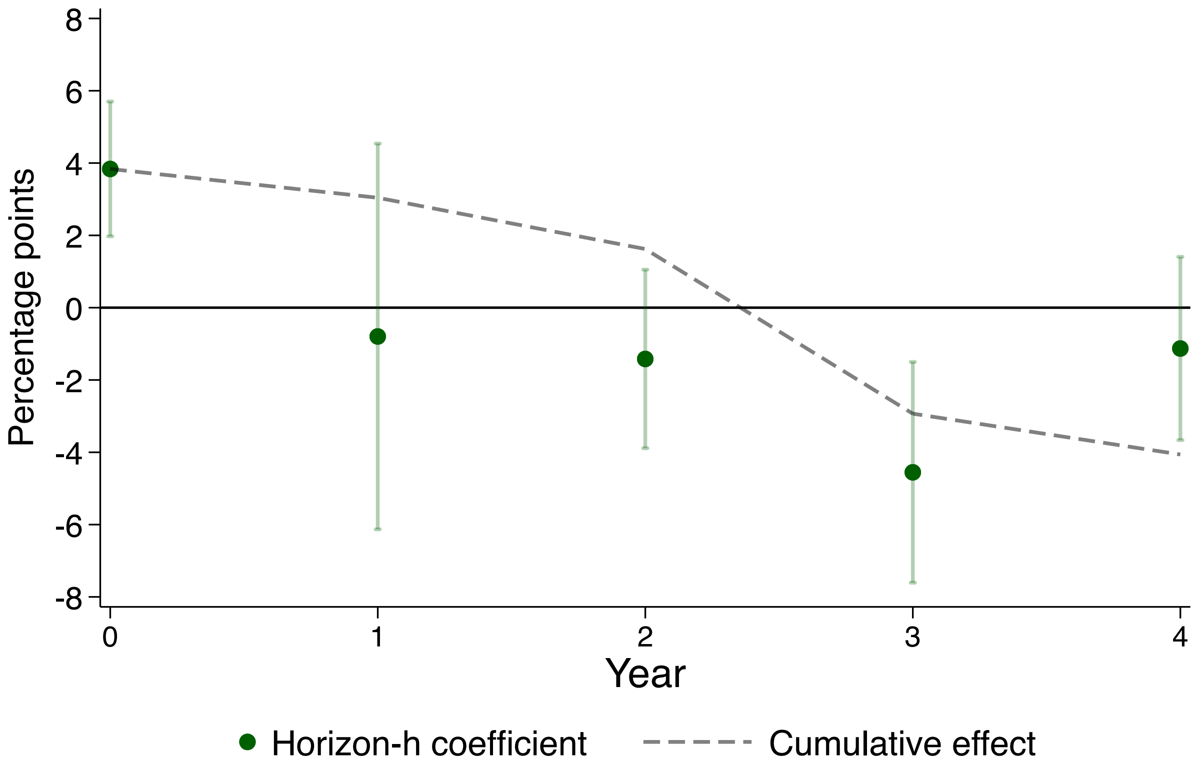


Notes: This figure plots estimates of the relative employment growth of risky firms when credit conditions are loose. For an establishment e controlled by a firm f at the start of year t, it shows 95% confidence intervals of $\gamma^{(h)}$ from annual establishment-level Jordá (2005) local projections given by

$$g_{et}^{(h)} = \alpha^{(h)} + \eta^{(h)} \cdot \pi_{f,t-1} + \gamma^{(h)} \times \left(\pi_{f,t-1} \cdot c_t\right) + \psi_{rkt} + \phi_e + \boldsymbol{X}_{eft} + \epsilon_{eft}^{(h)}$$

for annual horizons $h = 0$ to $h = 4$. The sample is the set of establishment-years in the LBD from 1978-2016 that are controlled by a Compustat firm (see Appendix Section A.1). The left-hand side variable $g_{et}^{(h)}$ is employment growth from year $t+h-1$ to $t+h$, measured using the symmetric growth rate of Davis and Haltiwanger (1992) (see Equation 2.3). The key right-hand side variable is the interaction between firm-level default risk $\pi_{f,t-1}$ and aggregate credit conditions c_t. Default risk is proxied by the negative of Merton (1974) distance to default, constructed following Bharath and Shumway (2008) (see Appendix Section B.2). Credit conditions are measured based off the year-specific relationship between credit spreads and default risk in the bond market (see Appendix Section B.1). Each regression includes (a) establishment fixed effects ϕ_e; (b) year fixed effects ψ_{rkt} that are specific to the establishment's MSA r-by-four-digit NAICS k pair; (c) interactions of two lags of c_t with $\pi_{f,t-1}$; and (d) interactions of two lags of real GDP growth, as well as year t GDP growth, with $\pi_{f,t-1}$. The regressions are weighted by the establishment's average level of employment between years $t+h-1$ and $t+h$, divided by the sum of these weights across all observations in the given year. Standard errors are double clustered on firm and year. The coefficients are interpreted as the estimated effect on an establishment's employment growth of a risky firm's credit spread being reduced by 100 basis points more as credit conditions loosen, relative to an establishment controlled by a less risky firm. The bashed black line shows the cumulative effect (as a percent of year $t-1$ employment) of these year-over-year estimates, which is the sum of the $\gamma^{(h)}$ estimates up to and including horizon h.

Figure 3: Local projections of response of annual establishment-level employment to credit conditions, Quarterly Financial Report (QFR) sample

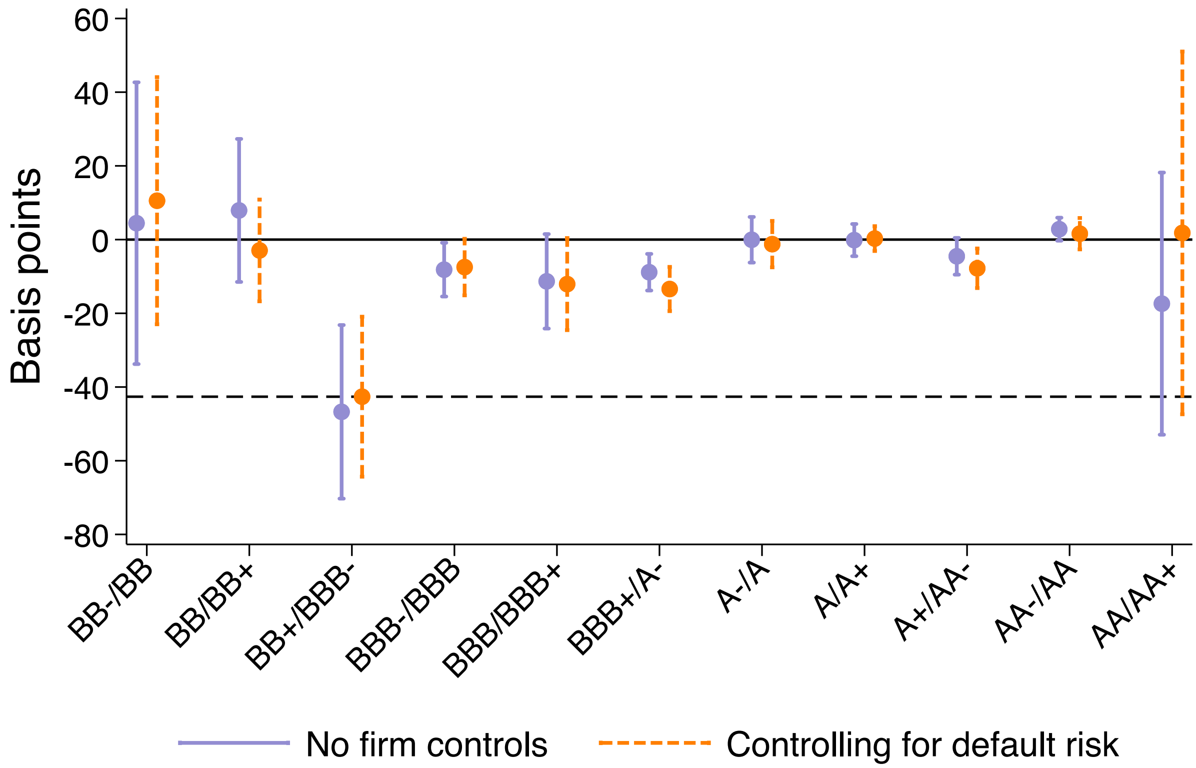


Notes: This figure plots estimates of the relative employment growth of risky firms when credit conditions are loose. For an establishment e controlled a firm f at the start of year t, it shows 95% confidence intervals of $\gamma^{(h)}$ from annual establishment-level Jordá (2005) local projections given by

$$g_{et}^{(h)} = \alpha^{(h)} + \eta^{(h)} \cdot \pi_{f,t-1} + \gamma^{(h)} \times \left(\pi_{f,t-1} \cdot c_t\right) + \psi_{rkt} + \phi_e + \boldsymbol{X}_{eft} + \epsilon_{eft}^{(h)}$$

for annual horizons $h = 0$ to $h = 4$. The sample is the set of establishment-years in the LBD from 1978-2016 that are controlled by a manufacturing firm that was sampled in the most recent Economic Census version of the QFR (see Appendix Section A.2). The left-hand side variable $g_{et}^{(h)}$ is employment growth from year $t+h-1$ to $t+h$, measured using the symmetric growth rate of Davis and Haltiwanger (1992) 2.3). The key right-hand side variable is the interaction between firm-level default risk $\pi_{f,t-1}$ and aggregate credit conditions c_t. Default risk is proxied by book leverage, scaled by the estimated relationship among Compustat firms between the negative of Merton (1974) distance to default and book leverage (see Appendix Section B.2). This makes the units of the estimates the same as those in Figure 2. Credit conditions are measured based off the year-specific relationship between credit spreads and default risk in the bond market (see Appendix Section B.1). Each regression includes (a) establishment fixed effects ϕ_e; (b) year fixed effects ψ_{rkt} that are specific to the establishment's MSA r-by-four-digit NAICS k pair; (c) interactions of two lags of c_t with $\pi_{f,t-1}$; and (d) interactions of two lags of real GDP growth, as well as year t GDP growth, with $\pi_{f,t-1}$. The regressions are weighted by the establishment's average level of employment between years $t+h-1$ and $t+h$, divided by the sum of these weights across all observations in the given year. These weights are then multiplied by the parent firm's QFR sample weight, such that the estimate of $\gamma^{(h)}$ reflects the behavior of the average (employment-weighted) manufacturing firm in the economy. Standard errors are double clustered on firm and year. The coefficients are interpreted as the estimated effect on an establishment's employment growth of a risky firm's credit spread being reduced by 100 basis points more as credit conditions losen, relative to an establishment controlled by a less risky firm. The dashed black line shows the cumulative effect (as a percent of year $t-1$ employment) of these year-over-year estimates, which is the sum of the $\gamma^{(h)}$ estimates up to and including horizon h.

Figure 4: Relative sensitivity of bond spreads to aggregate credit conditions of lower rung in adjacent credit rating pairs

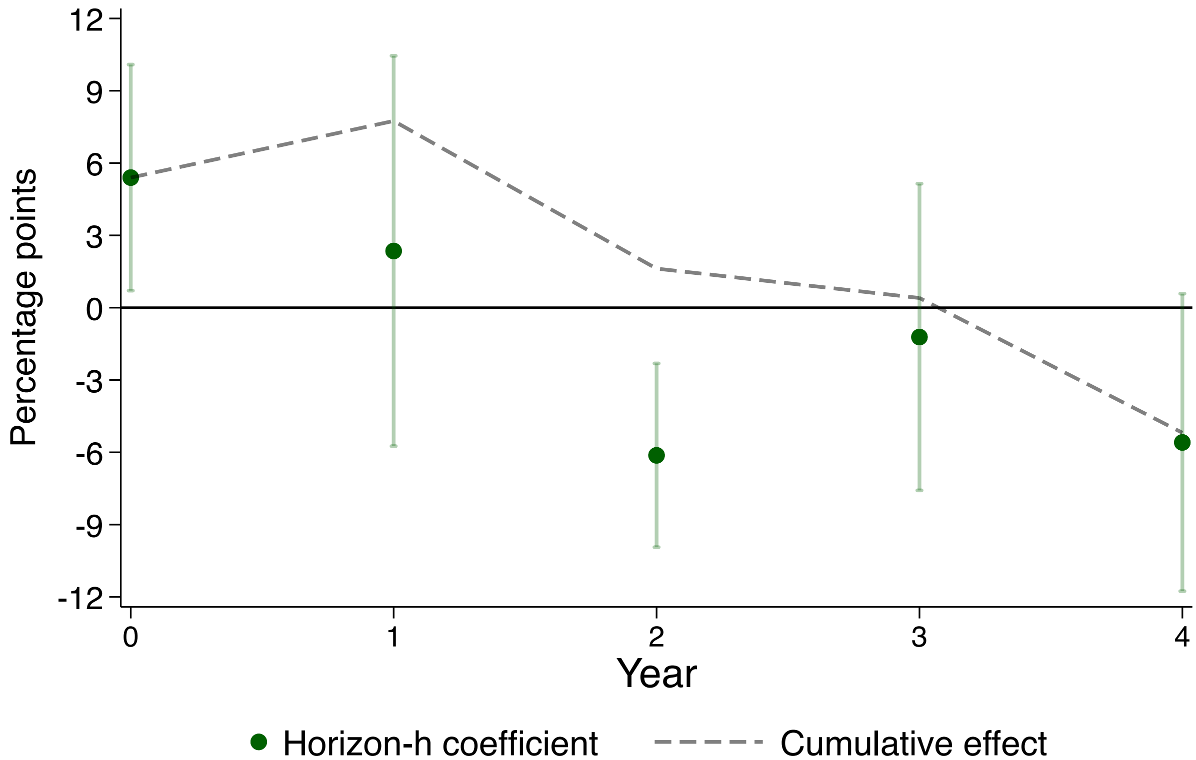


Notes: This figure plots estimates of the sensitivity of credit spreads to aggregate credit conditions for bonds in the lower rung of adjacent credit rating pairs. For a bond b issued by a firm f that, as of the end of quarter $t-1$, has a credit rating of either n or one notch above, it shows 95% confidence intervals of δ_n from quarterly bond-level regressions of the form

$$s_{bt} = \alpha + \eta \cdot 1\{rating_{b,t-1} = n\} + \delta_n \cdot 1\{rating_{b,t-1} = n\} \times c_t + \phi_t' \times \boldsymbol{X}_{bt} + \epsilon_{bft}$$

run separately for each notch n from BB- to AA. The sample is the set of bonds between 1978 – 2020 that have non-missing price data in the given quarter and that are issued by a firm in Compustat (see Appendix Section A.3). The left-hand side variable s_{bt} is the credit spread based off the bond's quarter t trading price, net of the component explained by duration and prepayment risk (see Appendix Section B.4). The key right-hand side variable is the interaction between aggregate credit conditions c_t and a dummy variable $1\{rating_{b,t-1} = n$ that equals one if the bond's $t-1$ rating is n and zero if the $t-1$ rating is one notch above n. Credit conditions are measured based off the year-specific relationship between credit spreads and default risk in the bond market (see Appendix Section B.1). Each regression includes a quarter fixed effect ϕ_t that is interacted with the unique value of a vector of binned bond-level characteristics. These bond-level characteristics include (a) seven bins for duration; (b) five buckets for bond age; and (c) deciles of amount outstanding. The solid purple lines show confidence intervals of δ_n when the regression does not include any controls for firm-level characteristics. The dashed orange lines show confidence intervals when the regression includes controls for the firm's default risk $\pi_{f,t-1}$ and its interaction with c_t. Default risk is proxied by the negative of Merton (1974) distance to default, constructed following Bharath and Shumway (2008) (see Appendix Section B.2) The regressions are weighted by the bond's amount outstanding, divided by the total amount outstanding across all bonds in the sample during the given quarter. Standard errors are double clustered by issuing firm and quarter. The coefficients are interpreted as the estimated sensitivity (in basis points) of a bond's spread following a 1 standard deviation increase in c_t, as compared to the sensitivity of the bond rated one notch above. The horizontal dashed black line marks the estimate δ_{BB+} – the estimated sensitivity of BB+ bonds relative to BBB- bonds – for the specification that controls for default risk.

Figure 5: Sensitivity of employment growth to aggregate conditions of BB+ vs. matched BBB- establishments

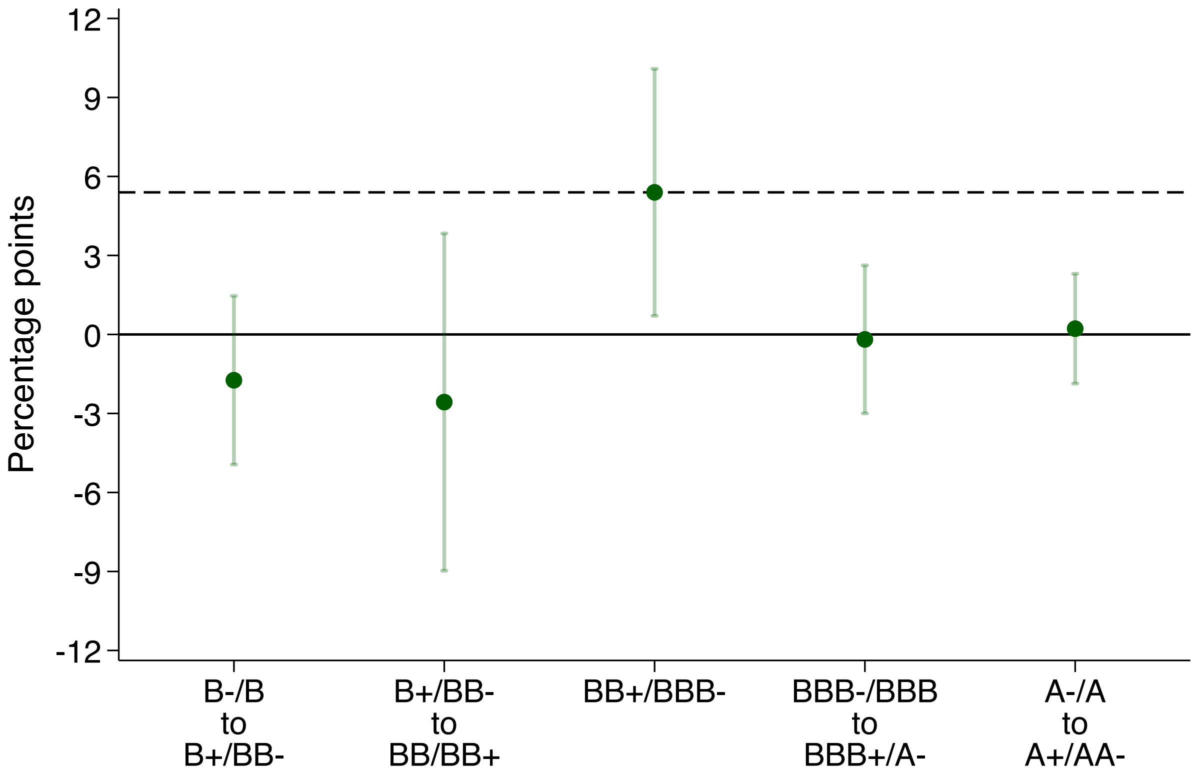


Notes: This figure plots estimates of the effect of loose credit conditions on the employment growth of the establishments of firms with a high-yield BB+ rating, relative to matched establishments of investment-grade firms with a BBB- rating. For an establishment e that as of year t is controlled by a firm f that has a BB+ rating, it shows 95% confidence intervals of $\gamma^{(h)}$ from annual establishment-level regressions given by

$$(g_{et}^{(h)} - g_{m(e)t}^{(h)}) = \alpha^{(h)} + \gamma^{(h)} \times \left(\delta_{BB+} \cdot c_t\right) + (\boldsymbol{X}_{eft} - \boldsymbol{X}_{m(e)m(f)t}) + \epsilon_{eft}^{(h)}$$

for annual horizons $h = 0$ to $h = 4$. The sample is the set of establishment-years in the LBD from 1978-2016 that are (a) controlled by a Compustat firm that is rated BB+; (b) not their firm's headquarters (see Appendix Section Appendix Section A.4), and (c) can be matched to an establishment $m(e)$ that lies in the same MSA and four-digit NAICS and is controlled by a firm with observably similar default risk but that is rated BBB-. See Section 3.3 for the details of the nearest-neighbor matching procedure. The left-hand side variable the difference between $g_{et}^{(h)}$, the employment growth from year $t+h-1$ to $t+h$ of the BB+ establishment, and $g_{m(e)t}^{(h)}$, growth of the matched BBB- establishment. Employment growth is measured using the symmetric growth rate of Davis and Haltiwanger (1992) (see Equation 2.3). The key right-hand side variable is aggregate credit conditions c_t, scaled by δ_{BB+}, the estimate from the bond-level regressions shown in Figure 4 of how much more sensitive the spreads of BB+ bonds are to c_t compared to spreads of BBB- bonds. All regressions include the following controls: (a) the differences $\boldsymbol{X}_{eft} - \boldsymbol{X}_{m(e)m(f)t}$ in the values of of the continuous matching variables between e and its matched establishment $m(e)$, (b) two lags of c_t, and (c) two lags of real GDP growth, along with year t GDP growth. The regressions are weighted by the BB+ establishment's average level of employment between years $t+h-1$ and $t+h$, divided by the sum of these weights across all observations in the given year. Standard errors are triple clustered on BB+ firm, year, and, following Abadie and Spiess (2022), matched BBB- firm. The coefficients are interpreted as the estimated effect on an establishment's employment growth when its controlling BB+ firm experiences a 100 basis point greater reduction in spreads as credit conditions loosen, relative to the growth of the matched BBB- establishment. The dashed black line shows the cumulative effect (as a percent of year $t-1$ employment) of these year-over-year estimates, which is the sum of the $\gamma^{(h)}$ estimates up to and including horizon h.

Figure 6: Relative sensitivity of employment growth to aggregate conditions of lower rung in credit rating pair

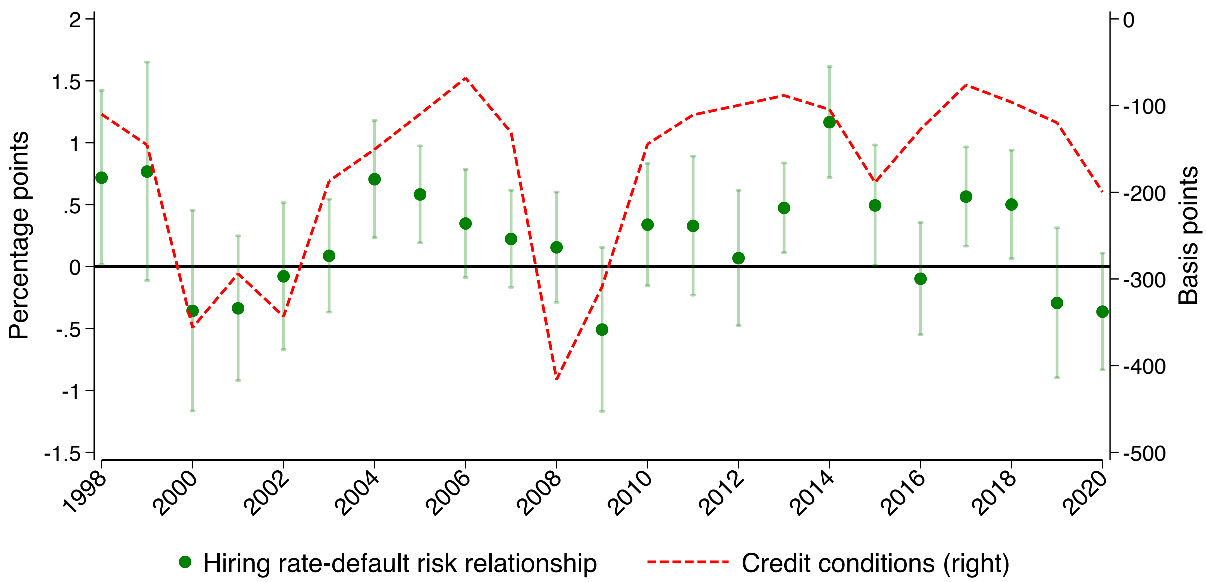


Notes: This figure plots estimates of the change in the contemporaneous employment growth of the establishments of firms with a certain credit rating, relative to matched establishments of firms rated one notch higher, when credit conditions loosen. For an establishment e that as of year t is controlled by a firm f that has a credit rating of notch n, it shows 95% confidence intervals of $\gamma^{(0)}$ from the annual establishment-level regression

$$(g_{et}^{(0)} - g_{m(e)t}^{(0)}) = \alpha^{(0)} + \gamma^{(0)} \times \left(\delta_{BB+} \cdot c_t\right) + (\boldsymbol{X}_{eft} - \boldsymbol{X}_{m(e)m(f)t}) + \epsilon_{eft}^{(0)}$$

We conduct separate matching procedures for each notch, but pool the samples to run the regressions in the way indicated on the x-axis. For example, the first coefficient shows the result of a regression that pools observations from three separate matching procedures: B-/B, B/B+, and B+/BB-. For each of the regressions, the sample is the set of establishment-years in the LBD from 1978-2016 that, as of the start of the given year, are (a) controlled by a Compustat firm with rating n; (b) not their firm's headquarters (see Appendix Section Appendix Section A.4), and (c) can be matched to an establishment $m(e)$ that lies in the same MSA and four-digit NAICS and is controlled by a firm with observably similar default risk but that is rated one notch higher than n. See Section 3.3 for the details of the nearest-neighbor matching procedure. The left-hand side variable is the difference between $g_{et}^{(0)}$, the employment growth from year $t-1$ to t of the notch-n establishment, and $g_{m(e)t}^{(0)}$, the growth of the higher-rated matched establishment. Employment growth is measured using the symmetric growth rate of Davis and Haltiwanger (1992) (see Equation 2.3). The key right-hand side variable is aggregate credit conditions c_t, scaled by δ_{BB+}, the estimate from the bond-level regressions shown in Figure 4 of how much more sensitive the spreads of BB+ bonds are to c_t compared to spreads of BBB- bonds. All regressions include the following controls: (a) the differences $\boldsymbol{X}_{eft} - \boldsymbol{X}_{m(e)m(f)t}$ in the values of of the continuous matching variables between e and its matched establishment $m(e)$, (b) two lags of c_t, and (c) two lags of real GDP growth, along with year t GDP growth. The regressions are weighted by the notch-n establishment's average level of employment between years $t-1$ and t, divided by the sum of these weights across all observations in the given year. Standard errors are triple clustered on the notch-n firm, year, and, following Abadie and Spiess (2022), the higher-rated matched firm.

Figure 7: Annual relationship between firm default risk and creation rate of new employment relationships

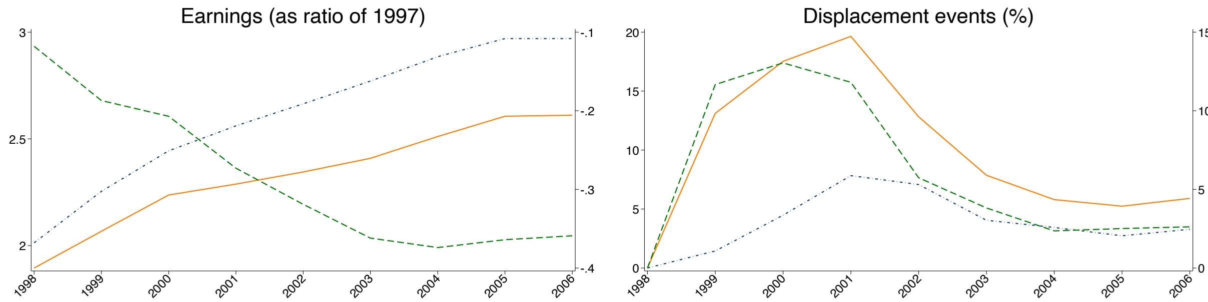


Notes: This plot shows the relationship over time between the rate at which a firm creates new employment relationships and its default risk. For establishments of a firm f operating during quarter t in MSA r and four-digit NAICS k, it shows 95% confidence intervals of η from the quarterly firm-by industry-by region regression

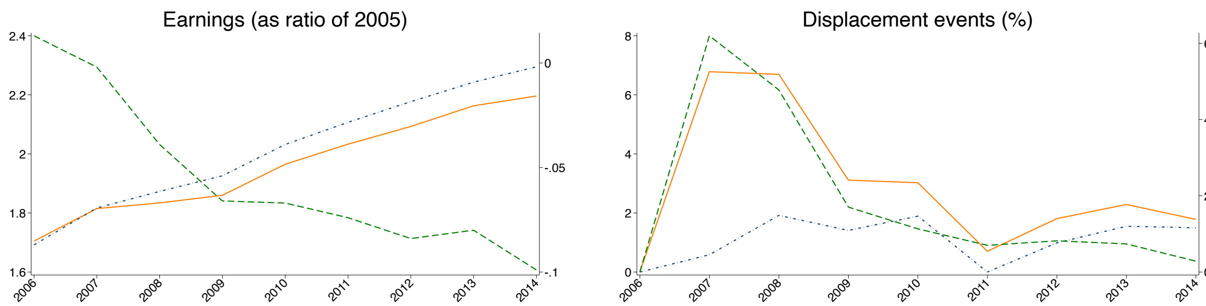
$$create_{rkft} = \alpha + \eta \cdot \pi_{f,t-1} + \psi_{rkt} + \epsilon_{frkt}$$

run separately for each year from 1998 to 2020. The sample is the set of firm-region-industry observations that are in our LEHD sample over 1998-2020 and are controlled by a Compustat firm (see Appendix Section A.5). The left-hand side variable $create_{rkft}$ is the number of new employment relationships that the firm-industry-region observation creates during quarter t, as a fraction of its employment at the start of the quarter. An employment relationship is created when the firm hires a worker that it had never before employed into a full-time, stable job, as detailed in Appendix Section B.5. Firm-level default risk $\pi_{f,t-1}$ is proxied by the negative of Merton (1974) distance to default, constructed following Bharath and Shumway (2008) (see Appendix Section B.2). Each annual regression includes quarter-by industry-by region fixed effects. Standard errors are clustered by firm. The regressions are weighted by the firm-industry-region observation's employment at the beginning of quarter t. The graph also shows the average value during the year of quarterly credit conditions c_t (dashed red line), which are measured based off the year-specific relationship between credit spreads and default risk in the bond market (see Appendix Section B.1). Both series are multiplied by the difference in default risk $\pi_{f,t-1}$ between the average firm in the fifth quintile of $\pi_{f,t-1}$ and the average firm in the first quintile of $\pi_{f,t-1}$. The η point estimates are thus interpreted as the predicted difference in employment relationship creation rates between a fifth- minus first-quintile firm, while the credit conditions series shows the predicted difference in credit spreads between a first-quintile minus fifth-quintile firm. The correlation between the two series is 0.66.

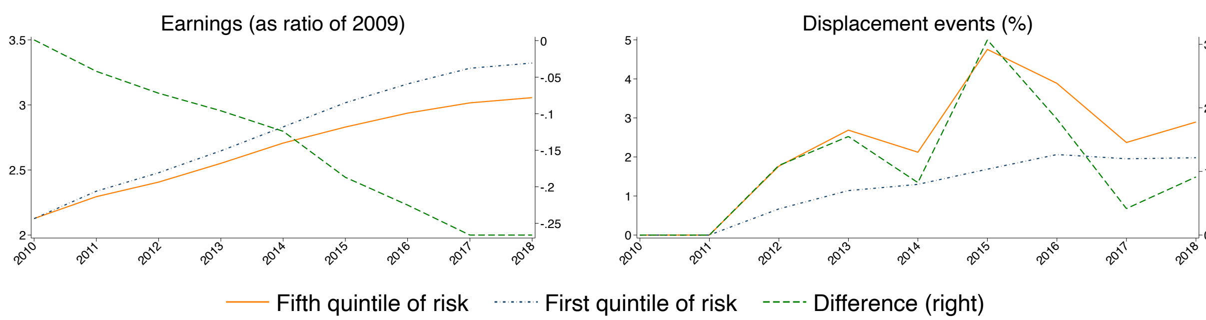
Figure 8: Mean earnings ratios and displacement rates of workers hired by firm in fifth and first quintiles of default risk



(a) 1998 hires



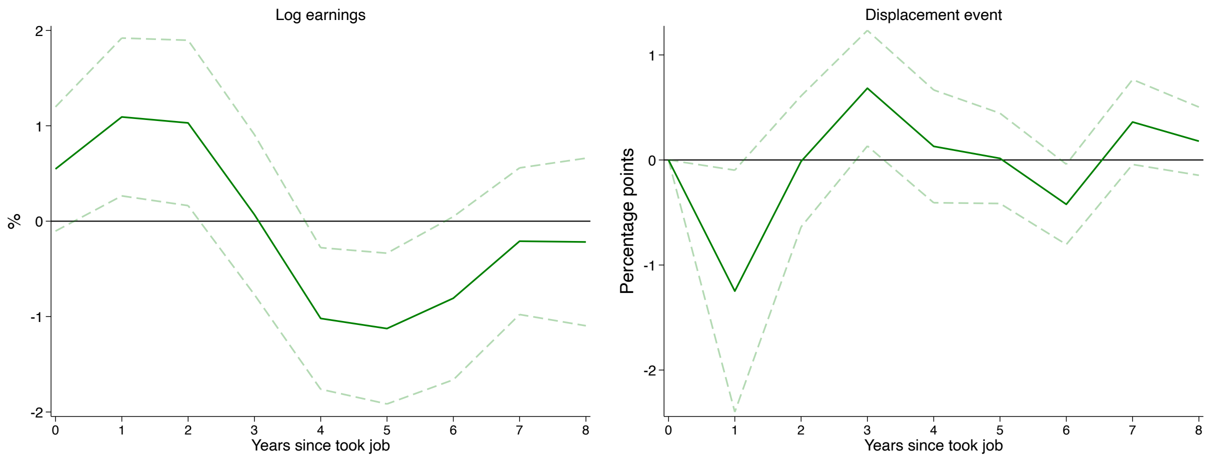
(b) 2006 hires



(c) 2010 hires

Notes: This figure shows the average outcomes of workers hired into a new employment relationship by high- versus low-risk firms during the peak of the first two credit cycles in our LEHD sample (1998 and 2006) and the first year of the final cycle in our sample (2010). It is based off the sample of prime-age workers in the our LEHD dataset that have positive earnings over each of the next eight years (see Appendix Section A.6). The solid orange lines plot average outcomes for workers that, in the year associated with the panel, (a) take a full-time, stable job at a firm at which they had previously never been employed (see Appendix Section B.5), where (b) the firm is in Compustat and, in the previous quarter, had default risk in the fifth quintile. The short-dash navy lines shows average outcomes for workers that take jobs at firms in the first quintile of risk. Firm-level default risk is proxied by the negative of Merton (1974) distance to default, constructed following Bharath and Shumway (2008) (see Appendix Section B.2). For each panel, the left-hand graphs show average earnings over each of the next eight years after the worker takes the job. The first year does not include earnings in the hiring quarter that come from the worker's previous job (if applicable). The right-hand graphs show the fraction of workers in each sample that, in each of the given years, experience a displacement event (separation into non-employment) from any full-time job during one of the quarters (see Appendix Section B.7). For both the earnings and displacement plots, the dashed green line shows the difference between the fifth quintile and first quintile series. Note that due to data aggregation required by RDC disclosure standards, the series can reflect earnings / displacement events that occur in in the year after that indicated on the x-axis. For example, in panel (b), the high displacement rate of fith-quintile workers in 2007 partially reflects the displacement of workers who took the fifth-quintile job in the third or fourth quarter of 2006 and are displaced in the first or second quarter of 2008.

Figure 9: Outcomes of workers hired by risky firms during credit booms

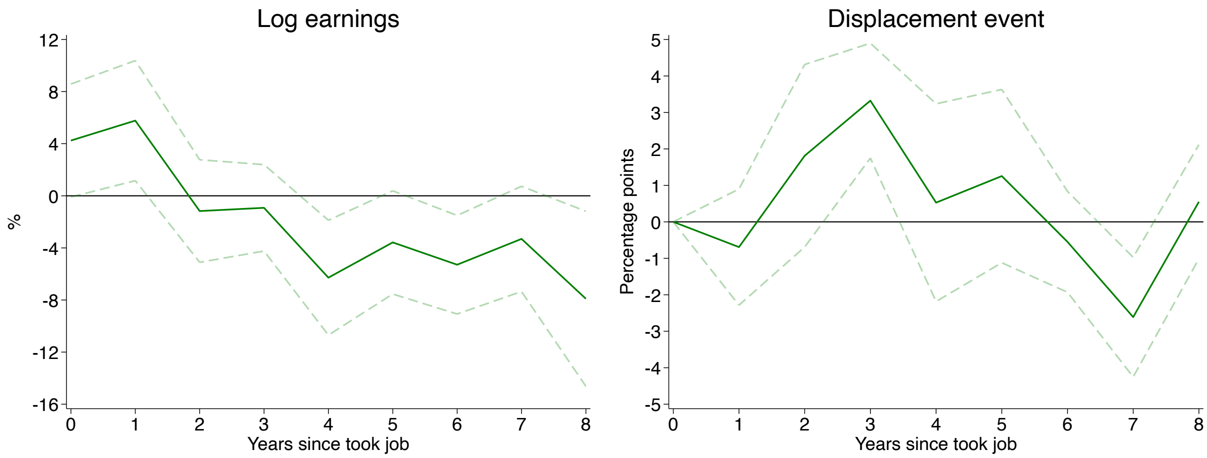


Notes: This figure plots estimates of the effect on worker outcomes of taking a job at a risky firm when credit conditions are loose. For a worker i who takes a job during quarter t at an establishment controlled by a firm f operating in MSA r and four-digit NAICS k, the figure shows 95% confidence intervals of $\gamma_5^{(h)}$ from the quarterly worker-level regression

$$y_{it}^{(h)} = \sum_{j=2}^{5} \left(\alpha_j^{(h)} + \gamma_j^{(h)} \times c_{t-3,t}\right) \cdot 1\{\text{risk quintile}_{f(i,t),t} = j\} + \phi'_{rkt} \times \boldsymbol{X}_{it} + \theta_{rkf} + \boldsymbol{X}_{f(i,t)t} + \epsilon_{ift}^{(h)}$$

run in four-quarter increments $h = 0, 4, ..., 32$. This figure only shows estimates associated with jobs at firms in fifth quintile of default risk $\gamma_5^{(h)}$; see Appendix Figure A6 for estimates for $j = 1, 2, 3, 4$. The sample is the set of prime-age workers in our LEHD sample that take a full-time, stable job at a Compustat firm during quarter t (see Appendix Section A.6). For the left-side plot, the dependent variable is $earn_{it}^{(h)}$, which is the log of total earnings over the four quarters starting with $t + h \cdot 4$. For the right-side plot, the dependent variable is $\text{displace}_{it}^{(h)}$, a dummy variable that equals one if the worker experiences a experience a displacement event (separation into non-employment) from any full-time job during one of the four quarters starting with $t + h \cdot 4$ (see Appendix Section B.7). The key right-hand side variable is the interaction the quintile of the new firm's default risk $\pi_{f,t-4}$ as of quarter $t - 4$ with average credit conditions between quarters $t - 3$ and t, $c_{t-3,t}$. Firm-level default risk is proxied by the negative of Merton (1974) distance to default, constructed following Bharath and Shumway (2008) (see Appendix Section B.2). Credit conditions are measured based off the year-specific relationship between credit spreads and default risk in the bond market (see Appendix Section B.1). All regressions include time-invariant firm-region-industry fixed effects, as well quarter-region-industry fixed effects interacted with the unique value of a vector of binned worker-level characteristics. As detailed in Appendix Section B.8, these characteristics include variables for the worker's demographics, observed labor market state at the start of quarter t, and past labor market outcomes. The regressions also control for interactions of $\pi_{f,t-4}$ with one- and two-year lags of $c_{t-3,t}$, as well as with lagged and contemporaneous GDP growth. The regressions are equal weighted. Standard errors are double clustered by quarter and new firm. We divide $\gamma_j^{(h)}$ by the average default risk between firms in the fifth risk quintile and firms in the first risk quintile. The coefficients are thus interpreted as the effect on workers' outcomes of taking a job at a fifth quintile firm that is experiencing a 100 basis points greater reduction in its spread, relative to a worker who takes a job at a first quintile firm.

Figure 10: Outcomes of workers hired by BB+ firms during credit booms



Notes: This figure plots estimates of the effect on worker outcomes of taking a boom-induced job at a firm with a BB+ credit rating. The estimates are obtained by matching workers who take jobs at BB+ firms to workers who take jobs at BBB- firms. For a worker i who takes a new job at a BB+ firm during quarter t, the figure shows 95% confidence intervals of the estimate $\gamma^{(h)}$ from the quarterly worker-level regressions given by

$$(y_{it}^{(h)} - y_{m(i)t}^{(h)}) = \alpha^{(h)} + \gamma^{(h)} \times (\delta_{BB+} \cdot c_{t-3,t}) + (\boldsymbol{X}_{f(i,t)t} - \boldsymbol{X}_{f[m(i,t)],t}) + \epsilon_{it}^{(h)}$$

run in four-quarter increments $h = 0, 4, ..., 32$. The sample is the set of prime-age workers in our LEHD sample that take a full-time, stable job at a Compustat firm during quarter t that is rated BB+ as of quarter $t-4$ (see Appendix Section A.6) and can be matched to a worker $m(i)$ that takes a job in the same quarter, four-digit NAICS, and MSA but at a firm with a BBB- rating. See Section 5.4 for details on the nearest-neighbor matching procedure. The dependent variable for the left-side plot is the difference between i and $m(i)$ in $earn_{it}^{(h)}$, which is the log of total earnings over the four quarters starting with $t + h \cdot 4$. For the right-side plot, the dependent variable for the right-side plot is the difference in $\text{displace}_{it}^{(h)}$, a dummy variable that equals one if the worker experiences a experience a displacement event (separation into non-employment) from any full-time job during one of the four quarters starting with $t + h \cdot 4$ (see Appendix Section B.7). The key right-hand side variable is average credit conditions between quarters $t-3$ and t, $c_{t-3,t}$, scaled by δ_{BB+}, the estimate from the bond-level regressions shown in Figure 4 of how much more sensitive the spreads of BB+ bonds are to credit conditions compared to spreads of BBB- bonds. All regressions include the following controls: (a) the differences $\boldsymbol{X}_{f(i,t)t} - \boldsymbol{X}_{f[m(i,t)],t}$ in the values of of the continuous matching variables between i and its matched worker $m(i)$, and (b) one- and two-year lags of $c_{t-3,t}$, as well as lagged and contemporaneous GDP growth. Standard errors are triple clustered on the BB+ firm of worker i, the quarter t, and, following Abadie and Spiess (2022), the BBB- firm of the matched worker $m(i)$. The coefficients are interpreted as the effect on workers' outcomes of taking a job at a BB+ firm that is experiencing a 100 basis points greater reduction in its spread, relative to a matched worker who takes a job at a BBB- firm.

Tables

Table 1: Establishment employment growth and firm risk as credit conditions vary, Compustat sample

	(1)	(2)	(3)	(4)	(5)
$\pi_{f,t-1}$	-3.061***	-3.066***	-2.687***	-1.812**	-3.230***
	(0.2449)	(0.2734)	(0.3630)	(0.8637)	(1.004)
$\pi_{f,t-1} \times c_t$	2.034***	2.001***	1.612***	1.945***	1.864**
	(0.4016)	(0.5159)	(0.4182)	(0.7067)	(0.7650)
Establishment FEs	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	-	-	-	-
Year-Industry-Region FEs	No	Yes	Yes	Yes	Yes
Credit condition lags	No	No	Yes	Yes	Yes
GDP growth interaction controls	No	No	No	Yes	No
Unemployment rate interaction controls	No	No	No	No	Yes
Number of establishment-years (N)	11,990,000	11,990,000	11,990,000	11,990,000	11,990,000
Number of firm-years	83,500	83,500	83,500	83,500	83,500
Adjusted R^2 (within estab.)	.002296	.1116	.1116	.1116	.1116
Within R^2	.0008303	.0005079	.0005171	.0005372	.0005237

Notes: This table shows estimates of the contemporaneous employment growth of risky firms when credit conditions are loose, relative to less risky firms. For an establishment e controlled by a firm f at the start of year t, we run different variants of the regression

$$g_{et}^{(0)} = \alpha^{(0)} + \eta^{(0)} \cdot \pi_{f,t-1} + \gamma^{(0)} \times \left(\pi_{f,t-1} \cdot c_t\right) + \psi_t + \phi_e \boldsymbol{X}_{eft} + \epsilon_{eft}^{(0)}$$

The sample is the set of establishment-years in the LBD from 1978-2020 that are controlled by a Compustat firm (see Appendix Section A.1). The left-hand side variable $g_{et}^{(0)}$ is employment growth from year $t-1$ to t, measured using the symmetric growth rate of Davis and Haltiwanger (1992) (see Equation 2.3). The key right-hand side variable is the interaction between firm-level default risk $\pi_{f,t-1}$ and aggregate credit conditions c_t. Default risk is proxied by the negative of Merton (1974) distance to default, constructed following Bharath and Shumway (2008) (see Appendix Section B.2). Credit conditions are measured based off the year-specific relationship between credit spreads and default risk in the bond market (see Appendix Section B.1). All regressions include establishment fixed effects. For additional controls, Column (1) includes year and establishment fixed effects. Column (2) includes year fixed effects ψ_{rkt} that are specific to the establishment's MSA r-by-four-digit NAICS k pair. Column (3) adds controls for the interaction of $\pi_{f,t-1}$ with two lags of c_t, giving the regressions a local projection interpretation (Jordá, 2005). Column (4) adds controls for the interaction of two lags of real GDP growth, as well as year t GDP growth, with $\pi_{f,t-1}$. Column (5) replaces the GDP growth interaction controls with analogous controls that use the level of the unemployment rate rather than GDP growth. The regressions are weighted by the establishment's average level of employment between years $t-1$ and t, divided by the sum of these weights across all observations in the given year. Standard errors are double clustered on firm and year. The coefficients on $\pi_{f,t-1}$ are interpreted as the estimated effect on an establishment's employment growth of being controlled by a risky parent firm that, when c_t is at its sample mean, faces predicted credit spreads 100 basis points higher than the establishment controlled by a less risky firm. The coefficients on the interaction term $\pi_{f,t-1} \cdot c_t$ are interpreted as the estimated effect on an establishment's employment growth of their risky firm's credit spread being reduced by 100 basis points more as credit conditions loosen, relative to an establishment controlled by a less risky firm. All sample sizes are rounded to the nearest hundred following disclosure guidelines by the U.S. Census Bureau. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels, respectively.

Table 2: Differential employment growth of BB+ establishments to matched BBB- establishments as credit conditions vary

	(1)	(2)	(3)	(4)
$\delta_{BB+} \cdot c_t$	3.334**	3.344**	5.396 **	
	(1.353)	(1.544)	(2.393)	
$\delta_{BB+} \cdot c_t \times 1\{\text{NBER Expansion}_t\}$				5.106**
				(2.614)
$\delta_{BB+} \cdot c_t \times 1\{\text{NBER Recession}_t\}$				4.314
				(4.246)
Match variable deviation controls	No	Yes	Yes	Yes
Credit condition lags	Yes	Yes	Yes	Yes
GDP growth control	No	No	Yes	Yes
Number of firm-industry-MSA years (N)	3,000	3,000	3,000	3,000
Number of firm-years	500	500	500	500
Adjusted R^2	.00942	.01979	.01954	.02106

Notes: This table shows estimates of the change in the contemporaneous employment growth of the establishments of firms with a high-yield BB+ rating, relative to matched establishments of investment-grade firms with a BBB- rating, when credit conditions loosen. For an establishment e that as of year t is controlled by a firm f that has a BB+ rating, we run different variants of the regression

$$(g_{et}^{(0)} - g_{m(e)t}^{(0)}) = \alpha^{(0)} + \gamma^{(0)} \times \left(\delta_{BB+} \cdot c_t\right) + (\boldsymbol{X}_{eft} - \boldsymbol{X}_{m(e)m(f)t}) + \epsilon_{eft}^{(0)}$$

The sample is the set of establishment-years in the LBD from 1978-2016 that are (a) controlled by a Compustat firm that is rated BB+; (b) not their firm's headquarters (see Appendix Section Appendix Section A.4), and (c) can be matched to an establishment $m(e)$ that lies in the same MSA and four-digit NAICS and is controlled by a firm with observably similar default risk but that is rated BBB-. See Section 3.3 for the details of the nearest-neighbor matching procedure. The left-hand side variable is the difference between $g_{et}^{(0)}$, the employment growth from year $t-1$ to t of the BB+ establishment, and $g_{m(e)t}^{(0)}$, the growth of the matched BBB- establishment. Employment growth is measured using the symmetric growth rate of Davis and Haltiwanger (1992) (see Equation 2.3). The key right-hand side variable is aggregate credit conditions c_t, scaled by δ_{BB+}, the estimate from the bond-level regressions shown in Figure 4 of how much more sensitive the spreads of BB+ bonds are to credit conditions compared to spreads of BBB- bonds. For controls, Column (1) includes two lags of c_t; Column (2) adds the differences $\boldsymbol{X}_{eft} - \boldsymbol{X}_{m(e)m(f)t}$ in the values of of the continuous matching variables between e and its matched establishment $m(e)$; and Columns (3) and (4) add two lags of real GDP growth, along with year t GDP growth. Column (4) shows the regression when saturated by dummy variables for whether year t contains at least one quarter in which there is an NBER-defined recession ($1\{\text{NBER Recession}_t\} = 1$) or not ($1\{\text{NBER Expansion}_t\} = 1$). The regressions are weighted by the BB+ establishment's average level of employment between $t-1$ and t, divided by the sum of these weights across all observations in the given year. Standard errors are triple clustered on BB+ firm, year, and, following Abadie and Spiess (2022), matched BBB- firm. The coefficients are interpreted as the estimated effect on an establishment's employment growth when its controlling BB+ firm experiences a 100 basis point greater reduction in spreads as credit conditions loosen, relative to the growth of the matched BBB- establishment. All sample sizes are rounded to the nearest hundred following disclosure guidelines by the U.S. Census Bureau. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels, respectively.

Table 3: Sensitivity of employment growth to credit conditions, split by risk quintile and credit rating category

(a) Response to 1 standard deviation ↑ c_t				**(b) Semi-elasticity**			
Risk quintile	HY	Unrated	IG	**Risk quintile**	HY	Unrated	IG
1	0.144	2.469		1			
	(2.294)	(1.916)					
2	1.273	0.5099	0.6831	2	1.582	1.417	1.900
	(1.185)	(1.085)	(1.438)		(1.472)	(3.015)	(4.000)
3	1.823	1.816	1.357	3	1.934	3.650	2.728
	(1.195)	(1.375)	(1.59)		(1.268)	(2.764)	(3.916)
4	3.748***	2.938**	0.6076	4	3.518***	4.736**	0.9794
	(0.7717)	(1.311)	(1.81)		(0.7243)	(2.113)	(2.918)
5	4.167***	3.698***	3.001	5	3.374***	4.682***	3.800
	(1.29)	(1.357)	(2.019)		(1.045)	(1.718)	(2.556)

Notes: This table shows estimates of the contemporaneous response of employment growth to loose credit conditions of firms in different risk quintile-rating category pairs. The two panels (a) and (b) show the estimates of a single non-parametric regression. For an establishment e controlled by a firm f at the start of year t, the table shows the estimates $\gamma_{j,IG}$, $\gamma_{j,HY}$, and $\gamma_{j,NR}$ from the annual establishment-level regression

$$
\begin{aligned}
g_{et}^{(0)} = & \sum_{j=2}^{5} (\alpha_{j,IG} + \gamma_{j,IG} \times c_t) \cdot 1\{\text{risk quintile}_{f,t-1} = j\} \cdot 1\{\text{investment grade}_{f,t-1} = 1\} \\
& \sum_{j=1}^{5} (\alpha_{j,HY} + \gamma_{j,HY} \times c_t) \cdot 1\{\text{risk quintile}_{f,t-1} = j\} \cdot 1\{\text{high yield}_{f,t-1} = 1\} \\
& \sum_{j=1}^{5} (\alpha_{j,NR} + \gamma_{j,NR} \times c_t) \cdot 1\{\text{risk quintile}_{f,t-1} = j\} \cdot 1\{\text{not rated}_{f,t-1} = 1\} + \psi_{rkt} + \boldsymbol{X}_{eft} + \epsilon_{eft}^{(0)}
\end{aligned}
$$

The sample is the set of establishment-years in the LBD from 1978-2020 that are controlled by a Compustat firm (see Appendix Section A.1). The left-hand side variable $g_{et}^{(0)}$ is employment growth from year $t-1$ to t, measured using the symmetric growth rate of Davis and Haltiwanger (1992) (see Equation 2.3). $\gamma_{j,IG}$, $\gamma_{j,HY}$, and $\gamma_{j,NR}$ are the estimates on the interaction terms between credit conditions c_t and the firm's default risk quintile j, estimated separately by credit rating category (investment grade, high yield, or not rated). Credit conditions are measured based off the year-specific relationship between credit spreads and default risk in the bond market (see Appendix Section B.1). Firm-level default risk $\pi_{f,t-1}$ is proxied by the negative of Merton (1974) distance to default, constructed following Bharath and Shumway (2008) (see Appendix Section B.2). The credit rating categories are constructed using ratings on outstanding bonds from 1978-1985 and long-term issuer ratings from 1986-2020 (see Appendix Section B.2). The omitted category in the regression is investment grade firms in the first risk quintile. The estimates in panel (a) show employment growth response of firms in the given risk quintile-by-rating category to a 1 standard deviation increase in c_t, relative to IG firms in the first quintile. The estimates in panel (b) are multiplied by the difference in the average risk $\pi_{f,t-1}$ of firms in the given risk quintile-by-rating category minus the average of IG firms in the first quintile. The high-yield estimates are in addition multiplied by δ_{BB+}, the estimate from the bond-level regressions shown in Figure 4 of how much more sensitive the spreads of BB+ bonds are to credit conditions compared to spreads of BBB- bonds. The estimates in panel (b) are thus interpreted as semi-elasticities: the response of employment growth when firms in the risk quintile-by-rating category experience a 100 basis points greater reduction in spreads than IG firms in the first quintile. The regression includes (a) establishment fixed effects, (b) year fixed effects ψ_{rkt} that are specific to the establishment's region r-by-industry k pair (MSA-by-four digit NAICS); (c) interactions of two lags of c_t with the risk quintile-by rating category dummies; and (d) interactions of two lags of real GDP growth, as well as year t GDP growth, with the dummies. The regressions are weighted by the establishment's average level of employment between years $t-1$ and t, divided by the sum of these weights across all observations in the given year. Standard errors are double clustered on firm and year. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels, respectively.

Table 4: Response of risky firms' creation rate of new employment relationships to credit conditions, by worker type

	Overall	Employment status	
	(1)	(2)	(3)
		Prev. employed	Not prev. employed
$\pi_{f,t-4} \times c_{t-3,t}$	1.091***	0.5136**	0.5006***
	(0.3505)	(0.2189)	(0.1683)
δ_i		0.66	2.32
Quarter-Industry-Region FEs	Yes	Yes	Yes
Credit condition lags and GDP controls	Yes	Yes	Yes
Number of firm-LLM-quarters (N)	7,226,000	7,226,000	7,226,000

(a) By worker's previous labor market state

	Overall	Age bucket				
	(1)	(2)	(3)	(4)	(5)	(6)
		18-20	21-24	25-34	35-44	45-54
$\pi_{f,t-4} \times c_{t-3,t}$	1.091***	0.123**	0.07137	0.3367***	.2980***	.2521***
	(0.3505)	(0.06971)	(0.04847)	(0.1198)	(0.08013)	(0.05278)
δ_i		2.40	0.54	1.25	1.09	0.93
Quarter-Industry-Region FEs	Yes	Yes	Yes	Yes	Yes	Yes
Credit condition lags and GDP controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of firm-LLM-quarters (N)	7,226,000	7,226,000	7,226,000	7,226,000	7,226,000	7,226,000

(b) By worker's age

Notes: This table shows estimates of the creation rate of new firm-worker employment relationships by risky firms when credit conditions are loose. We estimate rates at the firm-industry-region level and then scale the estimates to obtain per-worker rates δ_i for workers of a particular type. For establishments of a firm f operating during quarter t in MSA r and four-digit NAICS k, we run quarterly firm-by industry-by region regressions of the form

$$create_{rkft} = \alpha^{(g)} + \eta \cdot \pi_{f,t-4} + \gamma^{(g)} \times (\pi_{f,t-4} \cdot c_{t-3,t}) + \phi_{rkt} + \theta_{rkf} + \boldsymbol{X}_{rkft} + \epsilon_{frkt}$$

The sample is the set of firm-industry-region observations that are in our LEHD sample over 1998-2020 and are controlled by a Compustat firm (see Appendix Section A.5). The left-hand side variable $create_{rkft}$ is the number of new employment relationships that the firm-industry-region observation creates during quarter t, as a fraction of its employment at the start of the quarter. An employment relationship is created when the firm hires a worker that it had never before employed into a full-time, stable job, as detailed in Appendix Section B.5. The key right-hand side variable is the interaction between firm-level default risk as of quarter $t-4$, $\pi_{f,t-4}$, and average credit conditions between quarters $t-3$ and t, $c_{t-3,t}$. Firm-level default risk $\pi_{f,t-1}$ is proxied by the negative of Merton (1974) distance to default, constructed following Bharath and Shumway (2008) (see Appendix Section B.2). Credit conditions are measured based off the year-specific relationship between credit spreads and default risk in the bond market (see Appendix Section B.1). Column (1) of both panels shows estimates from a regression in which the creation rate includes all worker types. Panel (a) also shows regressions in which the creation rate is split by whether the worker was employed at another firm during quarter $t-1$ (Column 2) or whether the worker was not employed at $t-1$ (Column 3). Panel (b) shows regressions in which the creation rate is split by the worker's age as of quarter t, according to the age range indicated in the column titles. The second row in each panel shows the estimated propensity to take boom-induced jobs δ_i for the particular worker type whose creation rate is the dependent variable of that regression. We compute δ_i computed by multiplying γ by the total number of workers of the given type as a share of total employment. Each regression includes (a) time-invariant firm-region-industry fixed effects, (b) quarter-region-industry fixed effects, and (c) interactions of $\pi_{f,t-4}$ with one- and two-year lags of $c_{t-3,t}$, as well as with lagged and contemporaneous GDP growth. Standard errors are double clustered by firm and quarter. The regressions are weighted by the firm-industry-region's share of initial employment in quarter t. All sample sizes are rounded to the nearest hundred following disclosure guidelines by the U.S. Census Bureau. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels, respectively.

Table 5: Differential outcomes over credit cycle of labor market entrants with parental connection to BB+ firm vs. matched worker with parental connection to BBB- firm

	HS graduation year entry outcomes			Age 19 – 20	Age 28 – 29
	(1)	(2)	(3)	(4)	(5)
	Stable job	Parent job	Qs until entry	Earnings	Earnings
$\delta_{BB+} \cdot c_{t-3,t}$	4.891 **	4.960***	-0.7928**		
	(2.3875)	(1.770)	(0.4576)		
$\Delta boom\_\widehat{induced}\_job_{it}$				2.186**	-8.206**
				(1.4763)	(3.5916)
Match deviation controls	Yes	Yes	Yes	Yes	Yes
GDP growth control	Yes	Yes	Yes	Yes	Yes
# of BB+ parent entrants (N)	2,600	2,600	2,600	2,600	2,600
# of BB+ firm-years	800	800	800	800	800

Notes: This table shows estimates of the effect on worker outcomes of taking a boom-induced job based off quasi-random worker and firm variation. The estimates are obtained by instrumenting for the worker's decision to take a boom-induced job with whether one of the worker's parents works at a firm with a BB+ rating or a BBB- rating. For a worker i who is imputed to graduate high-school during quarter t, has a parent at a BB+ firm, and can be matched to another high-school graduate $m(i)$ whose parent works at a BBB- firm, we estimate the specification

$$(y^{(h)}_{it} - y^{(h)}_{m(i)t}) = \alpha^{(h)} + \gamma^{(h)} \times x_{it} + (\boldsymbol{X}_{f(i,t)t} - \boldsymbol{X}_{f[m(i,t)],t}) + \epsilon^{(h)}_{it}$$

for two snapshots h, one for the short-run (two years after t) and another for the long run (nine to ten years after t). The sample, detailed in Appendix Section A.8, consists of recent high-school graduates in our LEHD sample from 2000 to 2012 who have a parent at a BB+ firm and can be matched to a graduate during the same quarter whose parent works at a BBB- firm in the same four-digit NAICS and MSA. See Section 5.5 for details on the nearest-neighbor matching procedure. Columns (1)-(3) show first-stage estimates that characterize how exposure to a BB+ firm when credit conditions are loose influences the graduate's labor market entry outcomes. The key right-hand side variable is $c_{t-3,t}$, average credit conditions between quarters $t-3$ and t, scaled by δ_{BB+}, the estimate from the bond-level regressions shown in Figure 4 of how much more sensitive the spreads of BB+ bonds are to credit conditions compared to spreads of BBB- bonds. The dependent variable is the difference between worker i and their match $m(i)$ of, for Column (1) a dummy variable for whether the worker obtains a stable full-time job within two years of graduation; for Column (2) a dummy variable for whether the worker obtains a full-time job at one of their parent's firms; and for Column (3) the number of quarters from the worker's high-school graduation until they obtain their first full-time job (see Appendix Section B.10 for precise definitions). We use the estimate in Column (2) – which implies that a graduate is 4.96 percentage points more likely to work at their parent's firm when it is a BB+ firm that experiences a 100 basis point greater reduction in spreads – as an instrument for whether the worker obtains a boom-induced job. Columns (4) and (5) show two-stage least squares estimates in which the dependent variable is the difference between worker i and their match $m(i)$ of, for Column (4), the annualized log of total earnings in the eight quarters after high school graduation; and for Column (5), the annualized log of total earnings in the eight quarters that start nine years after graduation. All regressions include the following controls: (a) the differences $\boldsymbol{X}_{f(i,t)t} - \boldsymbol{X}_{f[m(i,t)],t}$ in the values of of the continuous matching variables between graduate i and its match $m(i)$, and (b) contemporaneous GDP growth. Standard errors are double clustered by MSA-quarter and, following Abadie and Spiess (2022), the BBB- firm at which the parent of graduate i's match works. All sample sizes are rounded to the nearest hundred following disclosure guidelines by the U.S. Census Bureau. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels, respectively.

Appendix

Michael Blank and Omeed Maghzian

November 13, 2023

Contents (Appendix)

A Sample construction

In this section, we provide details on the composition and construction of the samples used to produce our main results. For convenience, we put the table and figure numbers corresponding to each sample in the section header.

A.1 Compustat sample, OLS employment regressions (Table 1, Table 3 , and Figure 2)

We describe here the establishment-level Compustat sample used in the employment growth regressions of Section 2. We first describe the filters that we apply to quarterly Compustat data to define the baseline set of firms, and then detail how we link Compustat firm identifiers to a establishments in the LBD.

Sample of Compustat firms For each year t from 1978 to 2020, we take the set of all firms in Compustat-CRSP as of the first quarter of t. We define our base sample of Compustat firm-years by taking all firms that pass the following filters:

1. The firm's headquarters is in the U.S.
2. The firm's primary industry, as assigned in Compustat, is not in FIRE or utilities(corresponding to an SIC code of 49 or between 60-69), as is standard in the corporate finance literature
3. The firm has been in Compustat for at least two consecutive years. This is meant to address the survival bias in the Compustat fundamentals database
4. As of the first quarter of t, the firm has strictly positive book leverage (i.e., either $\texttt{dlcq} > 0$ or $\texttt{dltt} > 0$). This restriction on leverage drops around 10% of firms in Compustat and is not strictly necessary: although we cannot define distance-to-default for such firms, we could augment the default risk measure used in our regressions with a dummy variable for having zero leverage and set distance-to-default to some arbitrary number for zero-leverage firms. We instead exclude these firms in our baseline regressions for the sake of parsimony. In robustness checks, we find that our results on the employment sensitivity of risky firms to credit conditions are if anything more pronounced when adding in zero-leverage firms. This is consistent with the findings of Strebulaev and Yang (2013) that zero-leverage firms appear to be financially unconstrained – paying high dividend rates and being more likely to have investment-grade credit ratings – combined with the results, discussed in Section 3.5, that the employment growth of IG firms is relatively unresponsive to aggregate credit conditions

Linking to establishments in LBD For each Compustat firm f selected above, we then take the set of establishments in the LBD that are, if in operation (strictly positive employment) by March of year t, controlled by the firm as of March of year t; we also keep establishments whose first year of strictly positive employment comes after March of year t if their initial controlling firm is firm f. Because the creation of establishments represents a margin by which firms may organically increase employment (or reallocate employment from previous establishments, eg. move to a new headquarters), it is appropriate to include these establishments in the sample; note that their entry is accommodated by the Davis and Haltiwanger (1992) growth rates described in Section 2.3, assigning them a year-over-year growth rate of +2 for the year of entry.

The LBD provides the firm identifier that `lbdfid` that, based off the Census's Business Register, allow one to observe each establishment's controlling firm over time.[1] To link Compustat firm identifiers (`gvkey`) to these LBD identifiers, we execute two steps. First, in the LBD, we create a time-consistent firm identifier series, which we generically name `firmid_tc`. This series deals with features of the LBD that may lead an economically-unique firm's `lbdfid` to inappropriately change over time. There are two sources of such changes that our procedure addresses.

1. First, there could be artificial changes, in which the `lbdfid` of the same firm changes over time. The most common reason for this, as discussed by Ding et al. (2022), is that a firm is assigned a new `lbdfid` when it switches from being a single-establishment firm to a multi-establishment firm (or vice versa). Such artificial changes are straightforward to undo: if (a) a given `lbdfid` f is last observed in year t, (b) all of f's establishments are assigned the same `lbdfid` f' in year $t+1$, (c) f' is first observed in year $t+1$, and (d) at $t+1$, f' only controls establishments that are either newly created at $t+1$ or were controlled by firm f at t, then for `firmid_tc`, we replace f' with f for $t+1$ and all future years

2. Second, as noted by Chow et al. (2021), in producing the LBD, no attempt is made to harmonize the way in which genuine structural changes are reflected in the `lbdfid` series. We adjust `firmid_tc` for structural changes by applying a simple rule for which `lbdfid`, if any, should continue to exist when two or more firms combine at $t+1$ in a way that destroys one or more `lbdfid` values that existed at t. We infer which `lbdfid` corresponds to the firm that economically continues to exist based off the composition of the new, combined firm. If, among establishments with positive employment at t, more than half of the combined firm's $t+1$ employment come from an `lbdfid` that exits at t, we set the combined firm's `firmid_tc` to this `lbdfid`. If no `lbdfid` meets the 50% threshold, then we set `firmid_tc` to a new generic value.

With our time-consistent `firmid_tc` series in-hand, we then merge in the `gvkey` associated with a given year-`firmid_tc` using the Compustat-Business Register bridge, available in the RDC and described by Tello-Trillo and Streiff (2020). Specifically, for each `gvkey`, we take the latest year for which a match to a `lbdfid` is in the bridge. Given the `firmid_tc` associated with this `lbdfid` in that year, we then assign this `gvkey` to all establishments that, in previous years, have the same `firmid_tc`. We do this, rather than merging in the Compustat-Business Register bridge year-by-year, for two reasons. First, the bridge is based off matching a given `gvkey` to firms in the LBD year-by-year, using information such as the firm's EIN, name, and headquarters location; because of imperfections in the matching, this leads to occasional jumps back and forth in which `gvkey` an `lbdfid` in the LBD is associated with. Second,

[1] The `lbdfid` identifiers are an index of the `firmid` identifiers, except for the fact that the `lbdfid` identifiers correct for the recycling of the same value of `firmid` over time (eg. from one firm exiting and a future firm being assigned the same value).

because the bridge is in-part constructed using information that comes from the Compustat company table, and this table provides "header" (last date available) information, the most recent `gvkey` match in the bridge is one to which we attach the most confidence.

Additional establishment-level filters, contemporaneous regressions For the contemporaneous regressions of Table 1, we apply one additional filter to the set of LBD establishments matched to a Compustat firm: we drop establishments that provide temporary help services (six-digit NAICS of 561320) or payroll services (six-digit NAICS 541214). Inspection reveals that in the LBD, establishments in these sectors are sometimes incorrectly given the total employment of the firms to which these services are provided, rather than the establishments' own employment.

Additional establishment-level filters, dynamic regressions For the dynamic regressions of Figure 2, we keep all of the establishment-years that are in the contemporaneous sample described above, except observations that lie between 2016 and 2020. We drop these late-year observations to ensure that the sample stays the same as we vary the horizon length in Figure 2.

A.2 QFR sample, OLS employment regressions (Figure 3)

We describe here the establishment-level QFR sample used in the employment growth regressions of Section 2.

Constructing annual firm-level dataset As described in Section 2.3, the QFR is a quarterly survey of a representative sample of firms for which manufacturing for which manufacturing is the primary production activity.[2] While the QFR data is available at a quarterly frequency to researchers (Crouzet and Mehrotra, 2020), we only have access to snapshots of the QFR from "Economic Census (EC) years," defined as years that end in 2 or 7.[3] To use the QFR for our employment growth analyses, in non-EC years we must infer our variable of interest – book leverage – for firms using the available data in EC years. The vast majority of firms in the QFR are small firms that, in a given quarter, are sampled with a low probability; for example, firms with assets below \$250 million are only sampled with $\approx 5-10\%$ probability. Because most of these firms are not in consecutive EC years, we must extrapolate their value of book leverage observed in an EC year to other years.

To do this extrapolation, for a given year t we simply take a firm's value of book leverage in the last EC year in which it is available; this corresponds to the actual value when t is an EC year, and a previous year when t is not. To ensure that the data is not too stale, and that firms included in the sample for a given year have comparable data, we only keep firm-years in which the firm is sampled in the QFR for the most recent EC year. When t is an EC year, the sample is just the set of firms in the QFR in at least

[2]The QFR also contains data on private firms for sectors outside of manufacturing, including retail and wholesale trade. However, the sampling frame for these sectors imposes a much higher minimum asset threshold (\$50 million) than for the manufacturing sector, increasing the overlap with Compustat in terms of both identify and type of firms covered. We thus do not include results run on these other industries in this paper.

[3]The Census only allows researchers to access the full quarterly version of the QFR if their project does not have access to the LBD.

one quarter of t; when t is not an EC year, the sample is the set of firms that were in the QFR during the closest previous EC year (eg. for 2004, the 2002 QFR).

While this is a crude way to extrapolate the data in non-EC years, the distribution of book leverage across firms is reasonably stable over time; among Compustat firms, year t leverage has a 0.65, 0.56, 0.49, and 0.45 correlation with leverage in years $t+1$, $t+2$, $t+3$, and $t+4$, with the correlations between a firm's year t rank of leverage amongst all firms and its future rank even higher (0.86, 0.76, 0.69, and 0.62). This is why we do not other proxies for default risk, such as the interest coverage ratio, that utilize more volatile income statement variables. The crudeness of the default risk proxy for the QFR sample should also lead to attenuated estimates of the effect of credit conditions on the relative employment growth of risky firms.

Sample of QFR firms From this firm-level panel of firms in the QFR, we take all firms that pass the following two filters:

1. The firm's QFR data must be of a sufficiently high quality. We base this off whether we can the sum of the granular asset fields available in the survey equals the sum of the granular liability fields

2. The majority of the firm's employment must be in establishments that, according to the LBD, engage in manufacturing (two-digit NAICS from 31 – 33) as their primary activity. This filter keeps only firms that, according to the LBD's industrial classifications, should be included in the manufacturing portion of the QFR. It services two purposes. First, as described below, we impute the QFR's sampling weights by using the distribution of employment among such firms in the LBD; firms that are not majority manufacturing in the LBD do not contribute towards the aggregate employment weight under this calculation. Second, this filter drops some QFR-sampled firms that genuinely do operate in manufacturing, but that are, according to the Business Register's legal ownership information, controlled by a higher-up firm as part of a diversified conglomerate. Because the balance sheets of such subsidiary firms potentially do not reflect the exposure of credit spreads to aggregate conditions, it is appropriate to drop these firms.

Linking to establishments in LBD With this sample of firms, we construct the establishment-level panel used in our QFR employment growth regressions by merging the firms to establishments in the LBD, based off the EIN of the firm. As with the Compustat sample, and as described in Appendix Section A.1, we take all establishments that a firm controls as of year t or creates in some year after t, defining firms with the time-consistent identifier `firmmid_tc`. We keep establishments that are not classified by the LBD as primary engaged in manufacturing, since such establishments are in principle part of the total operations of a manufacturing firm (eg. warehouses).

Constructing sample weights The version of the QFR to which we have access is missing the QFR's sample weights for most years. Since the value of the QFR data, relative to Compustat, is its representativeness, we manually reconstruct the sampling weights. Following Crouzet and Mehrotra (2020), we do this by, for each QFR sample wave, taking the number of total firms in each sampling stata reported on the Census's website[4] and creating weights for the firms in our QFR sample by the ratio of the number of firms in that observation's strata to the number of firms in that strata available in our sample. In some years, our raw QFR data does not contain the field for observations' sample strata. For these years, we impute the sample strata of each observation based off the assets reported for that observation in the QFR. To account for the possibility that firms that are sampled as part of one strata have assets that would put it in another strata once actually included in the QFR survey – due to a large change in their assets between when the QFR's sample frame is set and when the firm is actually surveyed – we merge in firms' employment growth over the previous year from the LBD. Applying the relationship between asset and employment growth estimated on our Compustat sample, we put firms in a higher-asset (lowet-asset) strata if we observe their employment to have significant increased (decreased) over the past year.

The sample weights that we construct apply for the Economic Census years to which we have access to the QFR. Thus, the regressions shown in Figure 3 should be interpreted as being representative of the manufacturing industry as of each Economic Census year. If firms in some asset size strata have relatively high between-Economic Census year rates of switching between strata (eg. firms that start in the stratum for low-asset firms are more likely to expand and reach strata for higher-asset firms), then our between-Economic Census year sampling weights may slightly diverge from the sampling weights for the actual QFR surveys in these years.

Additional establishment-level filters, contemporaneous regressions For the regressions shown in of Figure 3, we drop establishments that provide temporary help services (six-digit NAICS of 561320) or payroll services (six-digit NAICS 541214). Inspection reveals that in the LBD, establishments in these sectors are sometimes incorrectly given the total employment of the firms to which these services are provided, rather than the establishments' own employment. We also drop observations that lie between 2016 and 2020. We drop these late-year observations to ensure that the sample stays the same as we vary the horizon length.

A.3 Bond sample (Figure 4)

Base sample of bonds We construct our quarterly sample of corporate bonds, which is used both to estimate the first stage of our credit rating IV regressions (Figure 4), as well as the construct our measure of aggregate credit conditions, following past work by Gilchrist and Zakrajšek (2012) and Sorensen (2021). We build a quarterly bond-level panel of prices on the near-universe of publicly-traded corporate bonds in the U.S. by combining data from three sources that collectively span our sample period, January 1978-December 2020:

1. The Lehman Brothers Bond Indices (1978-1996) (the "Lehman-Warga database")
2. The ICE Fixed Income platform (1997-2002)
3. The TRACE standard and enhanced databases (2003-2020)

Note that this panel contains both investment grade and high-yield bonds over the entire time period.

[4] https://www.census.gov/econ/qfr/historic.html

Sample restrictions We then merge in bond-level characteristics from Mergent, financial information for publicly-traded firms from Compustat, and stock price-derived variables from CRSP. We keep only senior, unsecured bonds issued by publicly-traded firms for which we can observe financial characteristics via Compustat. We also impose additional selection criteria, similar to that employed by Gilchrist and Zakrajšek (2012) and Sorensen (2021), to drop bond-quarter observations for which the price data may be noisy. Specifically, we drop: observations with par value of less than \$1 million, less than one year left until maturity, or extreme levels of credit spreads (lower than .05% or higher than 35%). We also exclude bonds with non-standard characteristics that may distort the mapping from firm default risk to the bond's observed price: bonds that have variable rate schedules or sinking fund provisions, that are convertible or putable, or that are issued by firms in the financial or utility sectors (firms with SIC codes in Compustat of 49 or between 60-69).

Finally, we drop the handful of bonds associated with firms that, for the given quarter, do not have positive debt (according to the Compustat fields `dlcq` and `dlttq`). The literal reason we drop these bonds is that we cannot construct the Merton (1974) distance to default measure for their issuing firms. The fact that there is an inconsistency between Compsutat reporting that they have no debt and them having outstanding bonds in our database also makes it sensible to drop these bonds for data quality reasons.

A.4 Credit rating IV employment regressions (Table 2 , Figure 5, and Figure 6)

Base sample of LBD establishment-years We start with the base sample of establishment-years in the LBD that satisfy two conditions:

1. The establishment-year is in the Compustat sample used for the OLS employment regressions, as described in Appendix Section A.1.

2. We can observe the credit rating of the establishment's controlling firm as of the end of year $t-1$. This includes firms that (a) starting in 1986, have a long-term issuer rating from S&P available in S&P Ratings Direct, or (b) that, at the end of $t-1$, have an outstanding senior unsecured bond in the bond-level dataset described in Appendix Section A.3. We drop the establishments of the small number of firm-years where the firm has a both a long-term issuer rating and a rating on outstanding senior unsecured bonds, but the two ratings are not the same as each other.

Restrictions on establishments eligigble for matching For the matching procedure used to implement our employment growth IV regressions, we keep all establishment-years in the above base sample, other than those that appear to be *auxiliary* establishments: establishments that exist to provide services to other establishments controlled by the same firm. A prominent example is the physical headquarters of a firm whose main business activities are done in decentralized locations (eg, an airlines or a large manufacturing company with plants not attached to the head office). The rationale for this filter is that the industry codes available in the LBD for these establishments do not capture the markets to which they are exposed; for example, headquarters that are separate from the actual operations of the company are given the code associated with "Administrative and Support Services" (NAICS 561000), rather than the codes associated with the actual sectors (eg. air travel) on which the employment decisions headquarters are seemingly based. The logic of our matching procedure's requirement that treated (BB+) and control (BBB-) establishments be in the same region and industry – that such establishments are plausibly exposed to the same markets and nonfinancial conditions – does not apply to auxillary establishments. We note that the assignment of industry codes that describe the services that auxillary establishments provide, rather that the markets that they ultimately help produce in, is a feature of how NAICS are defined (production based, instead market based like SIC codes) rather than how the LBD assigns NAICS.

To proxy for which establishments in the sample are auxiliary, we utilize the internal Census data sources that, as noted in the appendix of Fort et al. (2013), mark establishments as auxiliary based off information from various sources, including: the Business Register, the `fk_naics_aux` datasets compiled by Klimek and Fort (2018), the Census of Auxiliaries (AUX), the Census of Services (CSR), the Census of Transportation, Communications, and Utilities (CUT).

A.5 Firm hiring / worker take-up regressions (Figure 7 and Table 4)

Linking Compustat firm identifiers to the LEHD From our raw LEHD data, we want to keep all quarterly worker-job observations in which the job is at an establishment that is controlled by a Compustat firm included in the Compustat-LBD sample (as described in Appendix Section A.1). For the available years over 1998-2020 in the 24 states in our LEHD sample, our initial LEHD dataset contains the universe of distinct quarterly worker-job pairs where the worker makes strictly positive earnings that are covered by UI. To merge in Compustat firm identifiers, we first link LEHD observations to firms in the LBD, and then apply the Compustat-LBD firm identifier link described in Appendix Section A.1. We conduct the LBD-LEHD link by merging on the EIN reported in the LEHD. Specifically, we consider a firm in the LBD – as defined by the time-consistent identifiers that we construct, `firmid_tc` – to be successfully merged to an LEHD observation if the LEHD's observations EIN is one with which the `firmid_tc` is associated (based off the Business Reigster).

The only exception is in the case of LBD-LEHD EIN merges in which the majority of the LBD employment for the `firmid_tc` are in the temporary help service (six-digit NAICS of 561320) or payroll service (six-digit NAICS 541214) sectors. These merges likely reflect cases where the firm associated with the LEHD observation uses an external payroll processing company to handle its administrative UI reporting tasks. In these cases, the LEHD lists the EIN of the external processing company rather than of the firm actually associated with the job's establishment. Because few Compustat firms outsource their UI reporting in this way, as well as the difficulty in using information other than EINs (such as name and address) to link the LEHD and LBD, we simply exclude thse LEHD observations from the sample.

Additional worker-level filters We drop worker-quarters from the sample where either (a) the worker has a job in a state for which the starting quarter of LEHD data collection was less than four quarters ago, or (b) the worker has strictly positive earnings over the previous four quarters in a state not included in our 24 state LEHD sample. We cannot observe the full recent employment history for these workers, preventing us from inferring if they take a new job at a particular firm in the given quarter. For example, suppose that there is a worker-quarter observation in which the worker receives positive earnings from a particular firm for the first time in our dataset, but this worker has received earnings outside of our sample states over the past year. It could be the case that this worker was previously employed by the same firm, but at an establishment in a state not included in our sample, as it making a within-firm job transfer rather than taking a new job at the firm altogether.

A.6 Worker earnings regressions, firm risk (Figure 8 and Figure 9)

Base sample of quarterly worker observations: We start with the quarterly sample of workers who receive positive earnings at jobs controlled by a Compustat firm, as described in Appendix Section A.5. We then keep only the workers that, during the quarter, take a new full-time, initially stable job at the Compustat firm, as defined in Appendix Section B.5.

Additional worker-level filters: We want to estimate the earnings dynamics of workers who take jobs during quarter t at Compustat firms with different levels of default risk. Our analyses consider the worker's earnings for each year from the quarter of the new job, t, to eight years after the hire. To ensure that the results of these analyses are not affected by changes to the sample, we only keep workers that do not leave the sample at any point over this eight-year horizon. This entails applying two filters.

1. *No earnings in states outside of LEHD sample:* We drop workers if, during any quarter over the next eight years, we observe them making strictly positive earnings from a job in a state that is not in the set of 24 states in our LEHD sample. We can only observe whether workers have positive earnings or not in these non-sample states, but not the actual amount of these earnings. This is an arguably innocuous filter given the muted migration response of workers to the largest source of worker displacement in our sample, the 2008-09 recession (Yagan, 2019).

2. *Positive earnings over next eight years:* We drop workers who, for any quarter between t and eight years after t, go four consecutive quarters or more without making any UI-covered earnings (i.e., that are not included in our dataset with positive earnings for those quarters). This can occur for several economically-distinct reasons – such as the worker moving into self employment, becoming displaced and then discouraged in searching for new work, or retiring – which makes imputing missing earnings (eg. setting to zero) problematic. Dropping workers who leave the LEHD dataset for an extended period of time is a common practice when using the LEHD to estimate dynamic earnings effects associated with labor market events, such as mass layoffs at a worker's initial firm (Flaaen et al., 2019). This filter should, if anything, attentuate estimated effects of taking a boom-induced job on future worker displacement. If, for example, a worker is causally more likely to drop out of the labor force for an extended period of time after being laid off from their boom-induced job, their drop in earnings will not be reflected in our estimates

A.7 Worker earnings regressions, firm credit rating (Figure 10)

For our analyses of the worker earnings effects of taking jobs at Compustat firms with certain credit ratings, we start with the sample used for the analyses that estimate earnings effects based on firm default risk, as described in Appendix Section A.6. In this quarterly sample of workers who take new full-time, initially stable jobs, we only keep the observations associated with jobs associated with firms included in the firm-level sample used for the credit rating IV regressions, as described in Appendix Section A.4. Because we cannot directly link establishments in the LBD data to the jobs in our LEHD dataset, we apply the auxiliary establishment filter detailed in Appendix Section A.4 by dropping quarter-worker observations if the new job is at a firm- by MSA-by four-digit NAICS pair that is associated with an auxiliary establishment in the LBD.

A.8 Worker earnings regressions, firm credit rating and parental connections (Table 5)

Base sample of recent high-school graduates We start from the sample of individuals who are of high-school graduation age and can be linked to parents who, as observed in the LEHD, have jobs at Compustat firms with credit ratings of BB+ or BBB-. Following Staiger (2023), we start from Summary File 3 – the file that contains information on the 100% sample of U.S. households – of the 2000 and 2010 Decennial Censuses. Our sample includes all individuals in either of the two Censuses who meet each of the following restrictions:

1. *Age 19 between* 2000 – 2012*:* The year in which the individual turns 19 – which we refer to as the individual's imputed high school graduation year – must lie between 2000 and 2012. 2000 is the earliest year in which we can link children to their parents via the Decenniel Census, and 2012 is the latest year for which we can observe an individual's earnings 9 years after their high school graduation year

2. *Valid child-parent relationship in Decenniel Census:* We keep individuals who are reported in a housing unit in which there is a valid "parent" that is the head of the household; this can be the child's actual parent, or legal guardian (eg. grandparent). Following Staiger (2023) drop individuals that are part of a household that is listed as containing more than 15 different people, since these households may reflect multiple families living in the same household

3. *At least one parent and child can be linked to unique identifier in LEHD:* We use the Decenniel Census-PIK crosswalk available in the RDC to link individuals in the Decennial Census to the `pik` identifiers that are uniquely assigned to workers in the LEHD. A fair fraction of individuals (around a fifth) either cannot be linked via this crosswalk to a unique `pik`, or do not have a parent that can be linked to a unique `pik`

4. *Parents' joint income sufficiently high:* Following Staiger (2023), we drop individuals associated with parents whose average joint annual income is less than $15,000 (in 2016 dollars). The parents of these individuals are either do not work full-time, or earn a significant fraction of their earnings at jobs not covered by UI (eg. self employment)

5. *At least one parent works at BB+ or BBB- rated firm in LEHD sample at time of individual's imputed high school graduation:* To be included in our matching procedure, a high school graduate must have at least one parent who, in our LEHD data, (a) has positive earnings during the third quarter of the year in which the worker turns 19 from an establishment controlled by a Compustat firm with a credit rating of either BB+ or BBB-. To ensure that this job represents the parent's full-time job, we also require that (b) the parent has earnings at this job that exceed the minimum wage (assuming a 35 hour work week) and (c) does not receive earnings from any other job during the quarter.

6. *The parent has not recently taken a job at the BB+/BBB- firm:* To ensure that our instrument's variation is not affected by a high school graduate's parents selecting into working at a BB+ or BBB- firm given the current state of credit conditions, we require that the parent has worked full-time at this job for at least the four quarters prior to the third quarter of the year in which the individual turns 19. In addition to mitigating the possibility that our instrument is affected by endogenous parental selection, this requirement increases our instrument's power, since it is only defined for parents with a stable connection to the BB+/BBB- firm

Additional worker-level filters For the sample of high school graduates with parents at BB+ or BBB- firms defined above, we apply three additional filters that are necessary to estimate the short- and long-run earnings effects of exposure to boom-induced job creation:

1. *Works full-time within two years of high school graduation:* Over the eight quarters following the third quarter of the year where the worker turns 19, the worker must work full time – proxied by having earnings during a quarter that exceeds the minimum wage (under a 35 hour work week) – for at least two consecutive quarters. This allows us to consider workers who are likely not attending a four-year college for whom employment opportunities in the two years following high-school graduation are relevant

2. *Works full-time as of third quarter of year that turn* 28*:* The worker must work full time, using the same proxy as for the above condition, as of the third quarter of the year in which they turn 28. This ensures that we are estimating long-run earnings effects for workers who, at the time at which we measure future earnings, are in the labor force

3. *No non-LEHD state earnings:* The worker cannot have positive earnings from a job that outside our 24 state LEHD sample in either (a) any of quarter in the eight quarters after their imputed high school graduation quarter (third quarter of the year they turn 19) or (b) any quarter of the eight quarters that start from the third quarter of the year in which the worker turns 28. As the quarter ranges specified in (a) and (b) are the horizons over which we estimate short- and long-run earning effects, respectively, this ensures that our estimates are not affected by measurement error from earnings that we cannot observe in our LEHD data

B Variable definitions

In this section, we provide details on the data sources and construction behind the variables used to produce our main results.

B.1 Aggregate variables

Timing convention for employment regressions We initially construct all cyclical variables at the quarterly frequency. For our annual employment analyses, we aggregate these variables to the annual frequency by, for a given variable in year t, taking the simple average of the variable across four quarters: quarters $2-4$ of year t and quarter 1 of year $t+1$. We use this timing convention, following standard practice in the literature (eg. Fort et al., 2013), due to the fact that the LBD's annual employment variable is taken from the pay period that contains March 12. Fort et al. (2013). Our employment growth variables are thus roughly based off the change in employment from the end of the first quarter of year t to the end of the first quarter of year $t+1$.

Credit market conditions (c_t): Following Gilchrist and Zakrajšek (2012) and Sorensen (2021), we measure credit market conditions c_t based off the observed relationship in the bond market between a bond's spread and the default risk of the issuing firm. As noted above, we first construct the variable at the quarterly level, and so in this section, describe the procedure for constructing a quarterly measure. For notational simplicity, we still use the time index t here, even though it refers to the quarter, not the year

To proxy Compustat firms' default risk, we use Merton (1974) distance to default, as constructed by Bharath and Shumway (2008). We denote this measure, normalized by -1 such that a higher value corresponds to a higher default risk, as $\pi^{dtod}_{f,q}$; see Appendix Section B.2 for construction details. On the bond-level sample described in Appendix Section A.3, we then estimate c_t quarter-by-quarter using spreads s_{bt} that are purged of the component of yield that is accounted for the bond's duration and prepayment risk (see Appendix Section B.4). This entails running, for each quarter from 1978-2020, the regression

$$s_{bt} = a_t - c_t \cdot \pi^{dtod}_{f,t-1} + \boldsymbol{X}_{bt} + \epsilon_{bft} \tag{1}$$

for $\boldsymbol{X}_{bt}$ a vector of bond characteristics related to non-price terms or liquidity. Following Gilchrist and Zakrajšek (2012), these controls include: the log of the bond's amount outstanding, age, and duration, as well as the bond's coupon rate. Given the sign normalization in this expression, the estimate c_q is high for quarters in which the sensitivity of spreads to default risk is low.

Note that c_t is highly related to the measures of Sorensen (2021) and Gilchrist and Zakrajšek (2012). It corresponds to the (negative of) "yield for risk" (YFR) measure devised by Sorensen (2021), though estimated via an expanded sample that includes high-yield bonds in addition to investment grade bonds. The estimate a_t in (1) is related to but not the same as the "excess bond premium" (EBP) measure of Gilchrist and Zakrajšek (2012), in that EBP imposes the constraint that c_t is time invariant.

GDP growth With the FRED series `GDPC1` based off quarterly BEA data, we use quarterly real GDP growth for our quarterly regressions, and construct year-over-year GDP growth from March of year t to March of year $t+1$ for our annual regressions.

Unemployment rate With the FRED series `UNRATE` based off monthly BLS data, we use the average level of the unemployment rate over the three months of a given quarter for our quarterly regressions, and use the average level of the twelve months from March of year t to March of year $t+1$ for our annual regressions.

B.2 Firm-level financial variables

Default risk ($\pi_{f,t-1}$), Compsutat sample For firms in our Compustat sample, we proxy default risk $\pi_{f,t-1}$ with the naive distance-to-default measure $\pi^{dtod}_{f,t-1}$ developed by Bharath and Shumway (2008). We construct this variable at the quarterly frequency. For our LBD-based employment growth analyses, we take its value as of March of the given year.

For a given firm-quarter observation, we construct the quarterly variable π^{dtod}_{ft} by computing:

$$\pi^{dtod}_{ft} \equiv \frac{-1}{\sigma_{ft}} \cdot \left(-\ln[\ell^M_{ft}] + \mu_{ft} - .5 \times \sigma^2_{ft} \right)$$

These variables are defined as follows:

- ℓ^M_{ft} is the firm's market leverage, equal to the market value of debt D^M_{ft}, divided by the sum $D^M_{ft} + E^M_{ft}$, for E^M_{ft} the market value of equity, equal to the product of the firm's stock price (the absolute value of CRSP field `prc`) and shares outstanding (CRSP field `shrout`) as of the final trading date of the quarter. D^M_{ft} is proxied by the full amount of the firm's current liabilities (Compustat field `dlcq`) plus half the amount of the firm's long-term liabilities (Compustat field `dltt`)

- σ_{ft} is the estimated volatility of the firm's assets given its recent stock price volatility and leverage. It is based off first computing the volatility of the firm's stock price, σ^E_{it}, equal to the standard deviation of annualized monthly returns over the twelve months leading up (not including) the final month in the given quarter. We then compute

$$\sigma_{ft} = \frac{E^M_{ft}}{E^M_{ft} + D^M_{ft}} \times \sigma^E_{iq} + \frac{D^M_{ft}}{E^M_{ft} + D^M_{ft}} \times (0.05 + 0.25 \cdot \sigma^E_{it})$$

- μ_{ft} is the estimated expected growth of the firm's assets given its recent stock returns. It is based off the realized return on the firm's stock on the holding period of the twelve months leading up (not including) the final month in the given quarter.

Default risk ($\pi_{f,t-1}$), QFR sample Our QFR sample contains both public and private firms, the latter of which do not have the trade data necessary to compute the components of distance-to-default or Altman's z-score. We thus simply use the firm's book leverage as the default risk proxy for the QFR sample.

It is convenient to put book leverage into the same units as the distance-to-default proxy used for the Compustat sample. To do this, we take firms in our Compustat sample whose primary sector is manufacturing (SIC codes 2000-3999) and run the following firm-level quarterly regression:

$$\pi_{ft}^{dtod} = \alpha + \beta \ell_{ft}^{book} + \phi_t + \epsilon_{ft}$$

for ℓ_{ft}^{book} the firm's book leverage, computed by summing the Compustat current liabilities variable `dlcq` with the long-term liabilities variable `dlttq`. In regressions that use the QFR, we multiply a given firm's book leverage by the estimate β. As such, if the relationship between distance-to-default and book leverage is the same among manufacturing firms in the QFR as among Compustat manufacturing firms, then the QFR estimates are in the same units as the Compustat estimates.

B.3 Establishment-level variables

Employment (emp_{et}): Our annual establishment-level employment variable is based off the raw employment variable from the LBD, which equals the total number of employees at the given establishment as of the March 12 pay period in year t. We specifically use the `bds_emp` variable from the LBD that is used to produce the BDS. As described by Chow et al. (2021), `bds_emp` applies certain adjustments to the raw employment variable (eg. take out implausibly volatile year-over-year changes in employment) to reduce noise in the data.

Employment growth ($g_{et}^{(h)}$): For a given establishment e, we use the year-over-year growth rates from $t+h-1$ to $t+h$ introduced by Davis and Haltiwanger (1992), equal to the second-order approximation of the log change in employment,

$$g_{et}^{(h)} \equiv \frac{emp_{e,t+h} - emp_{e,t+h-1}}{.5 \times (emp_{e,t+h} + emp_{e,t+h-1})} \tag{2}$$

Establishment industry: In regressions in which we include annual fixed effects that are specific to a given establishment's four-digit NAICS-by-MSA, we use the time-consistent NAICS series developed by Klimek and Fort (2018).

B.4 Bond-level variables

Credit spread purged of duration and prepayment risk (s_{bt}): For our aggregate credit conditions measure c_t, as well as the bond-level regressions that constitute the first stage of our employment credit rating IV design, we construct a credit spread variable that is not affected by the bond's duration or prepayment risk. To purge the raw spreads available in the data of the pricing of duration and prepayment risk, we follow the procedures developed by Gilchrist and Zakrajšek (2012).

To strip out duration risk, we subtract the yield on a synthetic zero-coupon security that has no credit risk but that has the same timing of cash flows as the given corporate bond. As in Gilchrist and Zakrajšek (2012), we construct this synthetic security based off the zero-coupon Treasury yield curve estimated by

Gürkaynak et al. (2007). Specifically, for a bond b at t that matures in T periods and has promised cash flows $cf_{b,t+h}$ for each $h \in [1, T]$, we compute the price of the synthetic bond given zero-coupon Treasury yields r_{t+h} as

$$\sum_{h=1}^{T} cf_{b,t+h} \times \exp(-r_h \cdot h)$$

We then transform this price into the yield of the synthetic Treasury security.

To strip out the contribution of prepayment risk to the yields of bonds that are callable, we residualize the yields of these bonds against time-varying determinants of prepayment risk, including interaction of the bond's callability with the level and slope of the yield curve, recent interest rate volatility, and firm default risk. An alternative way to strip out prepayment risk – directly computing the value of the embedded call option(s) for each bond – would require more specific data on each bond's callability (eg. strike prices) than is available over our full sample period.

B.5 Firm hiring / worker take-up variables

Firm-by-local labor market take-up rate $create_{rkft}$ We construct the rate at which firms create employment relationships with new workers at the quarterly firm-region-industry level. In our quarterly worker-level LEHD data, we first must define when we consider a firm-worker pair to have started an employment relationship. We consider a worker i to form an employment relationship with firm f in quarter t when three conditions are each satisfied:

1. *Worker is employed at firm for the first time:* First, the quarter must be the first one in which the worker has strictly positive earnings from f. We use the LEHD's "Successor-Predecessor Flows" (SPF) table – which infers changes in firm identifiers based off the fraction of workers who previously worked at a given firm f' that, in the next quarter, are in the LEHD data as working at the same firm f – to disregard this occurring due to spurious changes in firm identifiers (for example, from mergers). Note that this definition excludes recalls – workers being re-hired by a firm at which they previously worked and were temporarily separated from (eg. due to being temporarily laid-off) – and intra-firm transfers (eg. moving from a firm's office in one region to an office of the same firm in a different region). The fact that we cannot observe the past employment histories of workers who previously had jobs in states and/or quarters not in our sample is a source of measurement error in imposing these filters.

2. *The job is stable:* Second, the job must be stable, in that the worker must have at least one full, uninterrupted quarter of earnings – quarter $t+1$ – from firm f. For this, we adopt the notion of "full quarter employment" from Hyatt et al. (2014), in which a worker is imputed to have worked at a firm in the days surrounding quarter $t+1$ if the worker has strictly positive earnings from the firm in all three of quarter t, quarter $t+1$, and quarter $t+2$.

3. *The job is full-time:* Third, the job must be full-time, which we impute by imposing the requirements that (a) the worker does not receive positive earnings during quarter $t+1$ from any other job and (b) earnings during $t+1$ are at least as large as earnings in a hypothetical full-time minimum

wage job; following Staiger (2023), we define the minimum-wage threshold for quarterly earnings as \$3,200 (in 2014 dollars), which equals the federal minimum wage in our sample period times 35 hours.

After applying this definition to obtain the set of workers who are hired by firm f in MSA r and four-digit NAICS k, we construct $create_{rkft}$ by taking the number of newly-hired workers during quarter t and dividing by number of workers that are employed at the firm-region-industry at the beginning of quarter t. This denominator is proxied by taking the number of workers at the firm-by-LLM that, according to the above definition, are full-quarter employed by it during quarter $t-1$. This is the appropriate denominator, given that a hire is only included in the numerator if it adds to the stock of full-quarter employed workers.

B.6 Firm credit rating design matching variables

We use the following continuous variables for the matching procedure described in Section 3.3:

Firm-level financial variables These variables are all computed from Compustat-CRSP

- Distance to default: The Bharath and Shumway (2008) measure described in Appendix Section B.2
- Assets: The quarterly assets field `atq` from Compustat
- Book leverage: The sum of current liabilities and long-term debt in Compustat, `dlcq` + `dltt`, divided by assets, `atq`
- Liquidity ratio: The ratio of cash and short-term investments (Compustat field `cheq`) to assets
- Tobin's Q: The sum of the market value of debt and equity, $E^M_{ft} + D^M_{ft}$ (as defined in Appendix Section B.2) divided by the book value of debt plus the book value of equity, $E^B_{ft} + D^B_{ft}$. D^B_{ft} is the sum of current liabilities and long-term debt in Compustat, `dlcq+dltt`. Book equity E^B_{ft} is constructed following Fama and French (1993). It is defined as the book value of shareholder equity, plus balance sheet deferred taxes, plus investment tax credits, minus the book value of preferred stock. If possible, we compute this using the quarterly Compustat files, with the book value of shareholder equity given by `seq`, the par book value of preferred stock given by `pstkq`, and deferred taxes plus investment credits given by `txditc`. If these fields are not available in the quarterly Compustat file, we take the value of book equity computed in the annual file, taking the year that lies closest to (but not after) the given quarter. To construct annual book equity, we use the same fields as with quarterly book equity, except that for the value of preferred stock, we use the redemption value (`pstkrv`) if available; if not, the liquidation value (`pstkl`) if available; and if not, the par value (`pstk`).

Firm-level business cycle exposure variables These variables are all computed from the establishment-level LBD-Compustat sample described in Appendix Section A.1. For a given year t, they are proxies for the exposure of the firm to future realized aggregate shocks – as proxied by the first and second moments over a five-year horizon – given the firm's distribution of employment across different regions (MSAs) and industries (four-digit NAICS) as of $t-1$. These controls are motivated by the evidence in Giroud and Mueller (2019) that financially-constrained firms transmit shocks from establishments in shocked regions to establishments in healthier regions; such within-firm transmission makes the establishment-level variables used in the matching procedure (exact matching on region-by-industry pair) potentially insufficient to find a BBB- establishment with the same shock exposure sa the BB+ establishment.

- Regional employment growth Bartik: A Bartik instrument for average employment growth from t to $t+5$ accross the MSAs to which the firm is exposed given its $t-1$ employment shares. For a given firm f, we compute this variable as follows. For each LBD establishment (Compustat or not) in an MSA r, we compute the MSA-level aggregate growth rate, that leaves out the establishments of the firm f itself, as

$$g_{rt5}^{loo} = \sum_{k=1}^{5}\left(\sum_{\forall e:r(e)=r,f(e)\neq f}\frac{(emp_{e,t+h-1}+emp_{e,t+h})}{\sum_{\forall e:r(e)=r,f(e)\neq f}(emp_{e,t+h-1}+emp_{e,t+h})}\times g_{et}^{(h)}\right) \tag{3}$$

  for $g_{et}^{(h)}$ the year-over-year employment growth rates defined in Section 2.3; note that because this growth rates are the second-order approximation formulation of Davis and Haltiwanger (1992), their average is computed by using the average levels of employment between one year and the next as weights. The Bartik instrument is then defined as

$$\sum_{\forall r}\frac{\sum_{\forall e:f(e)=f,r(e)=r}emp_{e,t-1}}{\sum_{\forall e:f(e)=f}emp_{e,t-1}}\times g_{rt5}^{loo} \tag{4}$$

- Industry employment growth Bartik: This variable is constructed exactly as the regional employment growth Bartik is, but using four-digit NAICS in place of MSA

- Regional employment growth volatility Bartik: A Bartik instrument for five-year employment volatility across the MSAs to which the firm is exposed given its $t-1$ employment shares. For a given firm f, MSA-level employment growth volatility is computed by taking the standard deviation of year-over-year, aggregate growth rates that leave out the firm f itself, i.e. the standard deviation g_{rth}^{loo} from $h=0$ to $h=4$, where g_{rth}^{loo} is calculated as in (3). The Bartik variable is then constructed by plugging these MSA-level standard deviations into (4) in place of g_{rt5}^{loo}

- Industry employment growth volatility Bartik: This variable is constructed exactly as the regional employment growth volatility Bartik is, but using four-digit NAICs in place of MSA

Firm-by-market variables These variables are all computed from the establishment-level LBD-Compustat sample described in Appendix Section A.1. They capture potential determinants of the transmission of firm-level shocks to the industries and/or regions in which establishments operate, as well as the transmission of shocks from the establishment's industry and/or region to the entire firm

- Share of firm's $t-1$ employment in region: For a firm f and the establishment's MSA r, we compute

$$\frac{\sum_{\forall e: f(e)=f, r(e)=r} emp_{e,t-1}}{\sum_{\forall e: f(e)=f} emp_{e,t-1}}$$

- Share of firm's $t-1$ employment in industry: For a firm f and the establishment's four-digit NAICS m, we compute

$$\frac{\sum_{\forall e: f(e)=f, m(e)=m} emp_{e,t-1}}{\sum_{\forall e: f(e)=f} emp_{e,t-1}}$$

B.7 Worker-level outcome variables

Log earnings ($w_{it}^{(h)}$): $w_{it}^{(h)}$ is the log of total earnings over quarters $t+h$ to $t+h+3$. The LEHD's earnings variable includes all components of wages that firms report to states for purposes of UI administration. This includes "UI-gross wages and salaries, bonuses, stock options, tips and other gratuities, and the value of meals and lodging, where supplied" (Spletzer, 2014). It excludes compensation items, such as non-vested options, that are not covered by UI. Taking the log four-quarter sum of earnings allows us to include workers in the sample who, at some point over a four-quarter stretch, have quarters with zero earnings. As described in Appendix Section A.6, this also means that we exclude workers from the sample who go four consecutive quarters without any earnings in our LEHD data.

Displacement indicator ($displace_{it}^{(h)}$): We set $displace_{it}^{(h)} = 1$ if, for some quarter over the four quarters from $t+h$ to $t+h+3$, the worker permanently separates from a previously full-time, stable job, and has at least one full quarter of non-employment. This definition departs from the standard measure of displacement in the literature based off mass layoffs (eg. Davis and von Wachter, 2011), in that we do not condition on the worker having a long tenure at the separating firm, nor do we condition on the separating firm's overall separation rate being very negative. The latter deviation is partly based off the finding of Fallick et al. (2021) that, in the LEHD, the change in a worker's long-run earnings following a separation event is sharply negative when it is accompanied by at least one quarter of non-employment, regardless of whether the separation occurs alongside a mass layoff or not. The lack of a tenure restriction reflects the fact that our estimate of displacement effects is meant to capture a different channel – the potential consequences of being displaced from an early-career job – than the one typically studied in the displaced worker literature.

B.8 Worker-level controls

Using the LEHD, we construct the worker-level control variables across three broad categories: demographics, observed labor market state at the start of quarter t, and past labor market experience and outcomes. We split these variables into bins, as specified below:

Demographics

- Age: Based off a worker's age as of the first day of the quarter, we split workers into five bins. They are: 18-20, 21-24, 25-34, 35-44, and 45-54
- Race: Based off the worker's race (available in the LEHD from Decennial Census data), we divide workers into five bins. They are: non-Hispanic white, non-Hispanic black, Hispanic, Asian, and other
- Sex: Based off the worker's sex (available in the LEHD from Decennial Census data), we divide workers into two bins, one for male and one for female

Labor market state

- Employment status: Based off the worker's earnings around the given quarter t, we define three employment categories. The first is *employed*, for workers who are full-quarter employed (as defined in Appendix Section B.5) at a job during quarter $t-1$. For workers hired into a new job in quarter t, this corresponds to an employment-to-employment (EE) transition (the "within quarter EE" flow definition in Haltiwanger et al. (2018)). The second is *non-employed, short,* for workers who have a full-time (above minimum wage) job during quarter $t-1$ that does not correspond to a full-quarter job. For workers hired into a new job in quarter t, this corresponds to a transition from a previous full-time job, into a short non-employment spell, and then into the new job; this could either correspond to an employment-unemployment-employment (EUE) transition, or a delay between making an EE transition (the "within/between EE" flow definition in Haltiwanger et al. (2018)). The third is *non-employed,long*, for workers who do not have a full-time job in quarter $t-1$. These workers either are non-employed as of quarter t, or are employed a part-time job.
- Employment growth of previous full-time job: If the worker had a full-time job at $t-1$, we compute the associated firm's employment growth rate – constructed by summing net flows from the LEHD across all observations attached to the firm – over the four quarters surrounding t ($t-2$, $t-1$, t, and $t+1$). We then assign each worker their quintile of this variable. We put workers without a full-time job at $t-1$ into a generic sixth bin.
- Employment quality of previous full-time job: Based off Haltiwanger et al. (2018), we proxy the quality of the worker's quarter $t-1$ full-time job (if applicable) by computing the average earnings of all workers with a full-time job at one of the firm's establishments as of $t-1$, and then take the normalized rank (0 to 1) among all firms (public and private) in the LEHD. We then assign each worker their quintile of this variable. We put workers without a full-time job at $t-1$ into a generic sixth bin.

Labor market experience and past outcomes

- Years since full-time entry: The number of years since a worker has obtained their first full-time, full-quarter job in the LEHD. We then assign each worker their quintile of this variable. For workers who are left-censored in our data – those who are present in the first year in which data is in our LEHD sample for the given state – we assign the fifth quintile; if the difference between the worker's age and the 80^{th} percentile of years since full-time entry is less than 18, we assign the worker to the fourth quintile, and so on.

- Employment rate over last three years: The fraction of quarters over the twelve quarters leading up to, but not including, quarter t in which the worker has strictly positive earnings in the quarter. We then assign each worker their quintile of this variable.

- Average earnings over last three years: The average quarterly earnings (across all jobs) of the worker in the twelve quarters leading up to, but not including, quarter t. We do not include quarters in which the worker has zero earnings in the average, so that this variable reflects the "intensive margin" of labor income. We then assign each worker their quintile of this variable.

B.9 Worker parent-firm credit rating design matching variables

We match on the following variables for the worker BB+/BBB- parent firm matching design described in Section 5.5:

Demographics of high-school graduates

- Race (exact): We require that the graduates have the same race, where we bin the Decennial Census race variable into five categories: non-Hispanic white, non-Hispanic black, Hispanic, Asian, and other

- Sex: We require that the graduates are of the same sex

Characteristics of parents' firms For the BB+ and BBB- firms of the graduates' parents, we use the same continuous matching variables as for the firm employment matching design, as detailed in Appendix Section B.6. We also require exact matches on the four-digit NAICS and MSA associated with the parents' jobs

Demographics of parents

- Race (exact): We require that the parents have the same race, where we bin the Decennial Census race variable into five categories: non-Hispanic white, non-Hispanic black, Hispanic, Asian, and other

- Sex (exact): We require that the parents are of the same sex

- Age (exact): We require that the parents have the same broad age, based off five bins: 18-20, 21-24, 25-34, 35-44, and 45-54

- Education (exact): We require that the parents have the same broad level of education, based off the education variable in the LEHD. This variable takes one of four values: less than high-school, high school, some college, and Bachelors degree or above. It is based off actual data for workers observed in the Decenniel Census's long-form survey during 2000 or 2000, or in the American Community Survey. For other workers, it is imputed during the production of the LEHD, based off the workers' demographics and historical earnings

Labor market status of parents

- Average earnings (continuous): The average annual earnings of the graduate's parents (summed together if the graduate has two parents in the Decenniel Census), adjusted for age and gaps in available LEHD data via the procedure developed by Staiger (2023)

- Number of quarters at firm (continuous): The number of quarters at which the parent has been employed at the BB+/BBB- firm

- Relative earnings at firm (continuous): The parent's normalized earnings rank (using earnings over the past year) among all workers at the given MSA-four-digit NAICS of the BB+ or BBB- firm

B.10 Recent high-school graduate outcome variables

Note that we refer to the third quarter of the year in which a worker turns 19 as the worker's "high-school graduation quarter." We construct the following worker-level outcome variables for the regressions in Table 5:

Stable job: A dummy variable that equals one if, for some quarter in the eight quarters starting from the worker's high-school graduation quarter, the worker has a full-time, stable job that represents entry into the labor market. This requires that the worker has a job at some quarter t for which the following three conditions are all met:

1. *The job is stable:* First, the job must be stable, in that the worker must have at least one full, uninterrupted quarter t of earnings from firm f. For this, we adopt the notion of "full quarter employment" from Hyatt et al. (2014), in which a worker is imputed to have worked at a firm in the days surrounding quarter t if the worker has strictly positive earnings from the firm in all three of quarter $t-1$, quarter t, and quarter $t+1$.

2. *The job is full-time:* Second, the job must be full-time, which we impute by imposing the requirements that (a) the worker does not receive positive earnings during quarter t from any other job and (b) earnings during t are at least as large as earnings in a hypothetical full-time minimum wage job; following Staiger (2023), we define the minimum-wage threshold for quarterly earnings as $3,200 (in 2014 dollars), which equals the federal minimum wage in our sample period times 35 hours.

3. *The job represents labor force entry:* Third, the job must be associated with the worker entering the labor force full-time. We proxy this as the worker's earnings during three consecutive quarters – either $t-1$, t, and $t+1$ or t, $t+1$, and $t+3$ exceeding the minimum wage (under a 35 hour work week) for all three quarters

Parent job: A dummy variable that equals one if two conditions are met the worker obtains a stable job at the BB+/BBB- firm at which their parent works. This requires that two conditions are both met:

1. The above dummy for stable job equals one
2. The job that induces the stable job dummy to equal one is a job at the BB+ or BBB- firm at which the graduate's parent worked as of their high-school graduation quarter. Note that we set the parent job dummy to one even if (a) the graduate's parent no longer works at the firm by the time that the graduate starts to work there or (b) the graduate joins an establishment of the firm that is not the same one at which their parent works

Quarters until entry: The number of quarters between the graduate's high-school graduation quarter before the worker obtains their first stable job.

Age 19-20 earnings: The log of the sum of earnings in the eight quarters that follow the worker's high-school graduation quarter

Age 28-29 earnings: The log of the sum of earnings in the eight quarters that start in the third quarter of the year in which the worker turns 28

C Bond market price of credit risk

C.1 Bond forecasting regressions

In this sub-section, we quantify the extent to which the reduction in risky firms' credit spreads when our measure c_t is high correspond to these firms' bonds have low excess returns. We run quarterly bond-level forecasting regressions of the form

$$r_{bt}^{(h)} = \alpha_0 \cdot \pi_{f,t-1}^{dtod} + \alpha_1 \times (a_t + \pi_{f,t-1}^{dtod} \cdot c_t) + \boldsymbol{X}_{bt} + \epsilon_{bft} \tag{5}$$

where $r_{bt}^{(h)}$ is the realized return on bond b in excess of the realized return on the same duration-matched synthetic Treasury bond used in the construction of credit spreads B.4. The variable a_t is the time-varying intercept estimated in the quarterly bond-level regressions (1) that we run to estimate c_t. As such, $a_t + \pi_{f,t-1}^{dtod} \cdot c_t$ is just the predicted value of a bond's spread in quarter t, given the risk of the firm that issued the bond along with the aggregate level and risk sensitivity of credit spreads in the corporate bond market during t.

The sample and controls are the same as for the construction of c_t, as described in Appendix Section B.1. To deal with bonds leaving the sample, generally either due to them maturing or being called by the issuing firm, we assume that an investor into a bond that has matured reinvests the principal into a Treasury bond with the same duration; this ensures corporate bonds in the sample do not affect the forecasting regression once they have matured. The coefficient α_1 estimates whether, at a horizon of $t+h$ quarters, looser conditions predict differential excess returns, in both the time series – whether quarters with low values the predicted spread $a_t + \pi^{dtod}_{f,t-1} \cdot c_t$ predict lower average returns – and in the interaction of the time series with the cross section – whether these quarters forecast especially low returns for the bonds of relatively risky firms.

Appendix Table A1 shows estimates of α_1 across one to five year horizons ($h = 4, 8, 12, 16$ quarters) and for a specification with and without quarter fixed effects. We display the estimates of $-\alpha_1$, such that the estimates are interpreted as the impact of looser quarter t conditions on excess returns. Consistent with past work (eg. Greenwood and Hanson, 2013; Sorensen, 2021), periods of observably cheap credit forecast persistently low future excess returns in both the time series and cross section. For example, Column (4) implies that a a bond that, based on aggregate conditions and the firm's default risk, has a credit spread that is 100 basis points lower than would be the case under the sample means of a_t and c_t – forecasts a 8% lower excess return five years out. The estimate of α_1 is little-changed in the regressions in columns (5)-(8) that include quarter fixed effects; this means that in quarters with a relatively flat relationship between spreads and default risk, the future excess returns of the bonds of high-risk firms have significantly lower expected excess returns compared to the bonds of lower-risk firms (Sorensen, 2021).

Our empirical design is predicated on there being significant heterogeneity across firms in how shifts in aggregate credit supply affect the cost of debt financing. The fact that the estimated effect of $(a_t + \pi^{dtod}_{f,t-1} \cdot c_t)$ on future excess returns is largely insensitive to the inclusion of quarter fixed effects suggests that this is the case. To more precisely quantify the importance of cross-firm variation, the bottom rows of Columns (5)-(8) of Appendix Table A1 show the results of a simple decomposition that calculates the fraction of the forecasting power of $(a_t + \pi^{dtod}_{f,t-1} \cdot c_t)$ on excess returns that is explained by variation in the quarterly mean versus the within-quarter deviations of $(a_t + \pi^{dtod}_{f,t-1} \cdot c_t)$. It is based off comparing the OLS estimates of excess returns on $\widehat{s}_{ft}$ when these two variables are transformed into the quarterly mean versus the deviation from the mean, scaled by the variance of $(a_t + \pi^{dtod}_{f,t-1} \cdot c_t)$ under each transformation. Over a three-year forecasting horizon, within-quarter variation in spreads explains around half of the forecasting power.

D Correlation of c_t with spreads in other credit markets

We consider whether our measure c_t, based off the price of risk in the bond market, also captures meaningful cross-sectional variation in the cost of debt in other credit markets. It is not a priori clear that fluctuations in c_t should be strongly associated with fluctuations in the spreads on the debt that all firms view as their marginal source of credit, for two reasons. First, bond-issuing firms tend to be larger and less credit constrained than the average economy-wide or even public firm, with bank-intermediated credit being a more prominent financing source for the average firm. Given the stark deviations in the supply of bank loans versus bonds that has occurred in certain historical episodes like the 2008 crisis (Becker and Ivashina, 2014), the underlying drivers of aggregate loan supply may be quite different than those of bonds. Second, even for bond-issuing firms, shorter-term debt instruments that similarly have market-specific drivers of aggregate supply, such as commercial paper (Kacperczyk and Schnabl, 2010), may represent the source of marginal financing relevant for labor demand.

Appendix Table A2 presents suggestive evidence that periods of lower risk premia in the bond market are also times in which relatively risky firms experience lower spreads in other markets. The table shows estimates from simple bi-variate quarterly time-series regressions in which the right-hand side variable is c_t and the dependent variables are spreads on three different credit types: small business bank loans, syndicated bank loans, and short-term (60 day) commercial paper. All variables in the regressions are normalized to have standard deviation one. Specifically, the spreads that we consider are:

- The dependent variables in columns (1) and (2) are based off interest rates small business loans, which come from the Federal Reserve's Survey of Terms of Business Lending (STBL) , available over 1997-2017. The STBL provides average rates on C&I loans made by commercial banks, split by loans with "low risk" vs. "moderate risk." The low risk spread is constructed by taking the "low risk" rate net of the five-year Treasury rate, and the high vs. low risk spread by subtracting the "low risk" rate from the "moderate risk" rate

- The dependent variables in Columns (3) and (4) are based off syndicated loan rates from Dealscan. For each quarter from 1994-2020, we estimate a pricing equation for the all-in drawn spread in loan-level data similar to the bond-level regression (1). For the taking the set of loans b in Dealscan to a nonfinancial public firm borrower f in quarter t, we run quarter-by-quarter regressions of the all-in drawn spread y_{bt} on the borrower's distance to default $\pi^{dtod}_{f,t-1}$:

$$y_{bt} = -\alpha_t - \beta_t \pi^{dtod}_{f,t-1} + \phi_{lt} + \psi_{kt} + \boldsymbol{X}_{bt} + \epsilon_{bt} \tag{6}$$

  for ϕ_{lt} a fixed effect for the leading bank in the syndicate, ψ_{kt} a fixed effect for the borrowing firm's two-digit SIC, and $\boldsymbol{X}_{bt}$ a vector of loan-level characteristics: whether the loan is a term loan or credit line, the share of the lead lender, the number of lenders in the syndicate, and dummy variables for the loan's purpose. The dependent variable of Column (3) is α_t, while the dependent variable of Column (4) is β_t

- The commercial paper rate series are based off Federal Reserve data. The low risk rate spread is constructed by subtracting the six month Treasury bill rate from the rate on 60-day nonfinancial AA commercial paper. The high vs. low spread comes from subtracting the AA rate from the rate on 60-day nonfinancial A2/P2 commercial paper. Panel (b) shows regressions in which the dependent variable x_t is a measure of non-price terms across different credit markets.

. The relationship between c_t and the high-low risk spread is relatively weak for small business loans.

Appendix Table A3 shows that c_t is more closely tied to lending standards for small businesses than it appears to be for the prices of the loans.

E Tightness of borrowing constraints over credit cycles

Appendix Table A3 considers the relationship between c_t and measures of hard constraints small business bank loans, syndicated loans, and bonds. The table shows estimates from simple bi-variate quarterly time-series regressions in which the right-hand side variable is c_t and the dependent variables are proxies of the constraints imposed in different credit markets:

- The dependent variable in Column (1) is bank small business lending standards – the fraction of banks reporting that they are loosening their standards on C&I loans to small firms – taken from from the Federal Reserve's Senior Loan Officer Opinion Survey on Bank Lending Practices (SLOOS).

- The dependent variable in Columns (2)-(3) is a measure of syndicated loan covenant tightness – the minimum allowed interest coverage ratio – constructed from Dealscan data. It is based off taking the set of loans b in Dealscan to a nonfinancial public firm borrower f that contain a covenant for the minimum interest coverage ratio, y_{bt}, and running quarter-by-quarter regressions of y_{bt} on the borrower's distance to default $\pi^{dtod}_{f,t-1}$:

$$y_{bt} = \alpha_t + \beta_t \pi^{dtod}_{f,t-1} + \phi_{lt} + \psi_{kt} + \boldsymbol{X}_{bt} + \epsilon_{bt} \tag{7}$$

  for ϕ_{lt} a fixed effect for the leading bank in the syndicate, ψ_{kt} a fixed effect for the borrowing firm's two-digit SIC, and $\boldsymbol{X}_{bt}$ a vector of loan-level characteristics: whether the loan is a term loan or credit line, the share of the lead lender, the number of lenders in the syndicate, and dummy variables for the loan's purpose. Note that a higher value of y_{bt} corresponds to a less restrictive covenant. the dependent variable in Column (2) is α_t, while the dependent variable in Column (3) is β_t

- The dependent variable in Columns (4)-(5) is a measure of covenant tightness in the bond market. Specifically, for the set of newly-originated bonds at quarter t issued by nonfinancial firm f, to we construct a bond-level index y_{bt}, devised by Billett et al. (2007), of the number of unique covenants (among 15 broad categories) specified in FISD data. We then run the regression

$$-y_{bt} = \alpha_t + \beta_t \pi^{dtod}_{f,t-1} + \boldsymbol{X}_{bt} + \epsilon_{bt} \tag{8}$$

  where X_{bt} is the same set of bond-level characteristics used in the regressions (1) that we run to construct c_t. Note that y_{bt} is multipled by -1, normalized so that a higher value corresponds to a less restrictive set of covenants in the bond. The dependent variable in Column (4) is α_t, while the dependent variable in Column (5) is β_t

F Loose credit conditions and risky firms' debt issuance

Appendix Tables A4 and A5 shows quarterly firm-level regressions for the debt issuance of risky firms as credit conditions c_t vary. For the set of nonfinancial Compustat firms, each column shows the results of a quarterly regression of the form

$$\Delta D_{ft} = \alpha + \eta \cdot \pi_{f,t-1} + \gamma \pi_{f,t-1} \times c_t + \phi_f + \phi_{kt} + \boldsymbol{X}_{ft} + \epsilon_{ft} \tag{9}$$

for $\pi_{f,t-1}$ the negative of the Merton (1974) distance to default, constructed as in Bharath and Shumway (2008), $\phi_f$are firm fixed effects, ϕ_{kt} are two-digit SIC-by-quarter fixed effects, and X_{ft} is a vector of the interaction between $\pi_{f,t-1}$ and lags of c_t and GDP growth. ΔD_{ft} is the amount of issuance of some kind of debt over the next four quarters t to $t+3$, scaled by the firm's assets as of $t-1$. For Appendix Table A4, ΔD_{ft} is defined as follows:

- Columns (1) and (2): ΔD_{ft} is the net issuance of long-term debt, calculated by by constructing net debt growth each quarter t – the change in the stock of debt, based off the sum of Compustat fields `dlttq` and `dlcq` – summing up the amounts over t to $t+3$, and dividing by assets as of $t-1$

- Columns (3) and (4): ΔD_{ft} is the change in principal amount of the firm's bonds outstanding, computed based off FISD data for each quarter t. We sum the quarterly changes across t to $t+3$, and divided by assets as of $t-1$

- Columns (5) and (6): ΔD_{ft} is the net borrowing of credit lines in the syndicated loan market, calculated by summing up the amount of new credit line loans in Dealscan for quarter t, net of the amount of previously-arranged credit lines that mature early. We sum the quarterly changes across t to $t+3$, and divided by assets as of $t-1$

- Columns (7) and (8): ΔD_{ft} is the net borrowing of term loans in the syndicated loan market, calculated by summing up the amount of new credit line loans in Dealscan for quarter t, net of the amount of previously-arranged term that mature early. We sum the quarterly changes across t to $t+3$, and divided by assets as of $t-1$

Panel (a) of Appendix Table A5 shows regressions in which the dependent variables are the same as in Appendix Table A4, but where the net debt flow variables are split up into the sum of net flows accounted for by gross inflows (Columns 2, 5, 8, and 11) versus gross outflows (Columns 3, 6, 9, and 12). Specifically:

- Columns (1)-(3): Net long-term debt issuance, where inflows are Compustat field `dltis` and outflows are Compustat field `dltr`)

- Columns (4)-(6): Net bond issuance, where inflows are computed based off the new amount of bond issuance in FSID and outflows are computed based off the amount of bonds maturing or being called in the FSID amount outstanding table

- Columns (7)-(9): New credit line syndicated loan borrowing, where inflows are computed based off the origination of new credit lines in Dealscan and outflows are computed based off the maturity of credit lines in Dealscan

- Columns (10)-(12): New term loan syndicated loan borrowing, where inflows are computed based off the origination of new term loans in Dealscan and outflows are computed based off the maturity of term loans in Dealscan

Appendix Table A5 shows regressions in which the dependent variables describe the maturity of net debt issuance within different types of debt:

- Columns (1)-(2): Debt of less than one year is based off the change in Compustat field `dlcq`, while debt of greater than one year is based off the change in Compustat field `dlcq`

- Columns (3)-(6): Debt of (a) < 1 year, (b) $1-3$ years, (c) $3-5$ years, and (d) > 5 years are based off, respectively, Compustat field `dd1`, the sum of Compustat fields `dd2` and `dd3`, the sum of Compustat fields `dd4` and dd5, and the difference between Compustat field `dltt` and the sum of `dd1`, `dd2`, `dd3`, and `dd4`

- Columns (7)-(10): Based off the time to maturity of outstanding bonds in FSID Mergent

- Columns (11)-(14): Based off the time to maturity of outstanding credit lines in Dealscan, accounting for ammendments and refinancings that change the maturity date

- Columns (15)-(17): Based off the time to maturity of outstanding term loans in Dealscan, accounting for ammendments and refinancings that change the maturity date

G Credit cycles and risky firms' financial distress

Appendix Figure A7 is based off the set of nonfinancial Compustat firms. It shows the results quarterly firm-level regressions of the form

$$1\{\text{Distress}^{(h)}_{ft} = \alpha + \eta \cdot \pi_{f,t-1} + \gamma \pi_{f,t-1} \times c_t + \phi_f + \phi_{kt} + \boldsymbol{X}_{ft} + \epsilon_{ft} \tag{10}$$

run over $h = 0$ to $h = 4$ for $\pi_{f,t-1}$ the negative of the Merton (1974) distance to default, constructed as in Bharath and Shumway (2008), ϕ_f firm fixed effects, ϕ_{kt} two-digit SIC-by-quarter fixed effects, and X_{ft} a vector of the interaction between $\pi_{f,t-1}$ and lags of c_t and GDP growth. The dependent variables $1\{\text{Distress}^{(h)}_{ft}\}$ are dummy variables for whether the firm experiences a distress event over the four-quarter period given by $t + h \cdot 4$ to $t + h \cdot 4 + 4$. For the four panels in the figure, this variable is based off:

- Panel (a): Whether the firm files for bankruptcy, based off data from UCLA-LoPucki Bankruptcy Research Database (BRD). The BRD is a database that tracks the set of public firm bankruptcies since October 1979, covering firms with assets of at least $100 million (in 1980 dollars)

- Panel (b): Whether the firm files for bankruptcy, based off CapitalIQ events data that starts in 1997. We set the variable equal to one if, in the given quarter, the firm has has an announcement in the CapIQ events database that it is filing for bankruptcy

- Panel (c): Whether the firm defaults on an outstanding bond, based off the FISD default table

- Panel (d): Whether the firm is in violation of a covenant on its outstanding debt, based off data collected by Nini et al. (2012) using SEC filings from 1996 to 2007

Appendix Figure A1 shows local projections that are of the same form and on the same establishment-level Compustat-LBD sample as in Figure 3, but where we put an indicator for whether the establishment engages in a mass layoff on the left-hand side rather than employment growth. Specifically, the figure shows estimates of $\gamma^{(h)}$ from the establishment-level annual regression

$$1\{\text{Mass\_Layoff}_{et}^{(h)}\} = \alpha^{(h)} + \eta^{(h)} \cdot \pi_{f,t-1} + \gamma^{(h)} \times \left(\pi_{f,t-1} \cdot c_t\right) + \psi_{rkt} + \phi_e + \boldsymbol{X}_{eft} + \epsilon_{eft}^{(h)}$$

for annual horizons $h = 0$ to $h = 4$. $\{\text{Mass\_Layoff}_{ft}^{(h)}\}$ equals one if the employment growth rate $g_{et}^{(h)} * 100$ is less than 30%, including cases in which the establishment closes completely (corresponding to $g_{et}^{(h)} = -2$). Appendix Figure A3 shows the results of the same regression, but on our LBD-QFR sample

H Aggregation of cross-sectional estimates

H.1 Debt issuance of low-risk firms

Appendix Figure A8 shows the results of quarterly firm-level regressions. For the set of nonfinancial Compustat firms, each column shows the results of a quarterly regression of the form

$$y_{ft}^{(h)} = \sum_{j=1}^{5} \left(\alpha_j^{(h)} + \gamma_j^{(h)} \times c_t\right) \cdot 1\{\text{risk quintile}_{f,t-1} = j\} + \phi_f + \boldsymbol{X}_{ft} + \epsilon_{ft} \tag{11}$$

where $\{\text{risk quintile}_{f,t-1} = j\}$ is the firm's risk quintile (based off its distance to default) as of quarter $t-1$. The regressions do not include quarter fixed effects, allowing $\gamma_j^{(h)}$, the interaction term between credit conditions c_t and the indicator variable for the j^{th} quintile, to be identified for each quintile. All regressions include firm fixed effects and interaction terms of the quintile indicators with lags of c_t and real GDP growth. The dependent variables $y_{ft}^{(h)}$ are flow variables, normalized by assets in $t-1$, that we calculate by taking the sum of the given variable y_{ft} over the four-quarter horizon $t+h\cdot 4$ to $t+h\cdot 4+4$, and dividing by assets as of quarter $t-1$. The flow variable y_{ft} used to construct to the dependent variable in each of the panels is as follows:

- Panel (a): Net issuance of long-term debt growth, where where inflows are Compustat field `dltis` and outflows are Compustat field `dltr`)

- Panel(b): Net issuance of equity, where inflows are Compustat field `sstkq` and outflows are Compustat field `prstkcq`

- Panel (c): the sum of net debt issuance and net equity issuance from Panels (a) and (b)
- Panel (d): The growth in log assets, based off Compustat field `atq`

H.2 Partial equilibrium aggregation exercise

We conduct a simple partial equilibrium aggregation exercise based off the one in Chodorow-Reich (2014). The exercise is based off computing an aggregate employment growth series among the set of Compustat firms in our LBD-Compustat sample (see Appendix Section A.1) under the counterfactual in which c_t (aggregate credit conditions) is at its sample mean for each year t over 1978 – 2020, rather than the actual value it took in year t. We compute this counterfactual by making three assumptions:

1. *Relationship between c_t and relative employment growth of risky firms is causal:* The estimate $\gamma^{(0)}$ from Column (4) of Table 1 identifies the causal effect of c_t on the relative employment growth of risky firms
2. *No direct effects on low risk firms:* Defining low-risk firms as of year t as firms with default risk $\pi_{f,t-1}$ in the first quintile, the employment of these firms is not directly affected (through changes to the spreads on the firm's debt) by fluctuations in c_t
3. *No general equilibrium effects:* There are no general equilibrium spillovers of the employment growth of one firm to the employment growth of other firms

Under assumption (1), for each firm with default risk above the first quintile, we can compute the counterfactual Davis and Haltiwanger (1992) growth rate of the firm's establishments as the growth rate that would be obtained if c_t were set to its sample mean as the sum of its actual growth minus the growth that, according to our estimate from Table 1, is attributed to the level of credit conditions during the year:

$$g_{et}^{(0-cf)} = \begin{cases} g_{et} - (\pi_{f(e),t-1} - \pi_{t-1}^{(low-risk)}) \times (c_t - \overline{c}) \times \gamma^{(0)} & \text{if } \{\text{risk quintile}_{f(e),t-1} \neq 1\} \\ g_{et} & \text{if } \{\text{risk quintile}_{f(e),t-1} = 1\} \end{cases} \tag{12}$$

for $\pi_{t-1}^{(low-risk)}$ the 20^{th} percentile of default risk as of year $t-1$ and $\overline{c}$ the mean of c_t over our sample period, 1978 – 2020. We then (a) take the weighted average of establishment-level log employment growth for each year t to obtain the firm-level counterfactual growth rate $g_{ft}^{(0-cf)}$, (b) apply the transformation described in Chodorow-Reich (2014) to turn $g_{ft}^{(0-cf)}$, which is the second-order approximation to log employment growth, into actual firm-level log employment growth, and finally (c) take the weighted average of these firm-level log employment growth rates for each year t to obtain the aggregate conterfactual growth for our Compustat sample.

Appendix Table A7 shows how the relationship between the actual aggregate growth rate in our Compustat sample with c_t compares to the relationship between the counterfactual series and c_t. The right-hand side variables in the regression are normalized by their standard deviations. Column (2) shows that, even controlling for GDP growth (and lags of c_t and GDP growth), a 1 standard deviation increase in c_t is associated with a 0.68 percentage point increase in the actual rate of aggregate employment growth in our sample. The counterfactual series shown in Columns (3) and (4), on the other hand, has a statistically-insignificant relationship with c_t that is essentially zero when we control for GDP growth. Mechanically, then, the economically-meaningful relationship between aggregate growth and c_t is explained by the difference between the actual and counterfactual growth rate series, shown in Columns (5) and (6). Note that the high R^2 and t-stats in Columns (5) and (6) are not meaningful, since the left-hand side variable for the regressions of these columns is increasing in c_t itself

H.3 Regional employment dynamics

Construction of Bartik instrument To get a sense of the general equilibrium spillovers that the employment growth of risky firms during credit booms may induce, we develop a regional design. The design is based off constructing an MSA-level Bartik instrument using the establishments in our Compustat-LBD sample (see Appendix Section A.1). For each MSA r and year t between $1978-2016$, we construct the instrument as, denoting $\mathcal{C}$ the set of establishments controlled by a Compustat firm in our Compustat-LBD sample, $\mathcal{E}$ the set of all establishments in the LBD, and $r(e)$ the MSA of an establishment e,

$$\pi_{r,t}^{bartik} = \sum_{\forall e \in \mathcal{C}: r(e)=r} \left(\frac{emp_{e,t-1}}{\sum_{\forall e' \in \mathcal{E}: r(e')=r} emp_{e',t-1}} \right) \cdot \pi_{f(e),t-1} \tag{13}$$

The instrument $\pi_{r,t}^{bartik}$ for year t is the weighted average of the default risk of establishments' controlling firms, with the weights corresponding to establishments' employment shares as of year $t-1$. Note that the shares are computed using the employment of all establishments in the denominator, including establishments not in the Compustat-LBD sample $\mathcal{C}$. This ensures that MSA-level variation in the instrument has the same interpretation as establishment-level variation in the instrument: when the Bartik instrument is interacted with c_t, the effect on the MSA's aggregate employment growth of a 100 basis point decrease in the spreads faced by the employment-weighted average establishment in the region. An MSA with low a employment share of public firm establishments will have low values of the instrument, which is appropriate given that there are few public firms that can affect such an MSA's aggregate employment growth. To account for the potential correlation of an MSA's share of public firms with other determinants of employment growth, in our regressions, we always directly control for this share (as well as its interaction with c_t), as suggested by Borusyak et al. (2021).

Contemporaneous MSA-level effects of exposure to risky firms during credit booms Appendix Table A8 shows estimates of how MSA-level exposure to credit spread reductions during credit booms transmits to its overall employment growth rate. For $g_{rt}^{(0)-agg}$ the MSA r's aggregate growth rate from $t-1$ to t – based off the employment of all establishments in the LBD, including establishments controlled by private firms – we run the regression

$$g_{rt}^{(0)-agg} = \alpha^{(0)} + \eta^{(0)} \cdot \pi_{r,t}^{bartik} + \gamma^{(0)} \cdot \left(\pi_{r,t}^{bartik} \times c_t \right) + \psi_t + \phi_r + \boldsymbol{X}_{rt} + \epsilon_{rt}^{(0)} \tag{14}$$

where ψ_t is a vector of year fixed effects, ϕ_r is a vector of MSA fixed effects, and $\boldsymbol{X}_{rt}$ includes controls for the share of firms included in $g_{rt}^{(0)-agg}$ (Borusyak et al., 2021), the interaciton of the share of public firms with c_t, the interaction of the Bartik instrument and the share of public firms with two lags of c_t and with contemporaneous and lagged GDP growth, and the predicted growth rate of the MSA based off its $t-1$ shares across four-digit NAICS (ie, a Bartik instrument for the MSA based off its industry composition).

The estimate in Column (1) implies that when the employment-weighted average firm in an MSA experiences a 100 basis point reduction in spreads during a credit boom, that MSA's employment growth is 2.36 percentage points higher than an MSA that is exposed to less risky firms. Columns (2)-(4) consider variants of the Bartik instrument $g_{rt}^{(0)-agg}$ that address potential concerns regarding how it could be correlated with determinants of MSA-level employment growth other than exposure to credit conditions. Column (2) constructs $\pi_{r,t}^{bartik}$ using firm-level default risk $\pi_{f,t-1}$ that is first demeaned at the four-digit SIC level. This is an alternative way (the MSA industry Bartik control being the baseline) to address the potential endogeneity of $\pi_{r,t}^{bartik}$ to the industrial composition of the region; the instrument is then explicitly based off whether a given MSA is relatively exposed to firms that, even relative to other firms in their industry, have high default risk. Column (3) addresses the possibility of reverse causality – that high growth in an MSA induces higher default risk in the firms that operate there, for example by encouraging them to increase investment by borrowing more – by only constructing $\pi_{r,t}^{bartik}$ with the establishments of firms who have less than 5% of their total employment in that MSA. We refer to this instrument as the "non-headquarter" establishment instrument, since it effectively is based off the MSA's exposure to establishments that are not located in the region in which the firm has a significant portion of its employment. Column (4) constructs $\pi_{r,t}^{bartik}$ by applying both of these modifications to the baseline instrument.

Dynamic MSA-level effects of exposure to risky firms during credit booms Appendix Figure A9 shows the effect of MSA-level exposure to credit conditions during year t on the dynamics of its employment growth by running Jordá (2005) local projections of the form

$$g_{rt}^{(h)-agg} = \alpha^{(h)} + \eta^{(h)} \cdot \pi_{r,t}^{bartik} + \gamma^{(h)} \cdot \left(\pi_{r,t}^{bartik} \times c_t\right) + \psi_t + \phi_r + \boldsymbol{X}_{rt} + \epsilon_{rt}^{(h)} \tag{15}$$

for annual horizons $h = 0$ to $h = 4$. We use the baseline specification from Column (1) of Appendix Table A8.

H.4 Regional worker earnings effects

Sample of high school graduates We want to get an initial sense of whether the labor market dynamics associated with risky firms' job creation during credit booms have spillovers to workers not directly affected by these jobs. To do so, use the MSA-level Bartik instrument described in Appendix Section H.3 to estimate the effects on a potential labor market entrant of being exposed to an MSA that itself is relatively exposed to risky firms' job creation during credit booms. In our LEHD data from 2000 – 2012, we take the set of all individual-years from the 2000 and 2010 Decenniel Censuses in which we (a) impute that the individual is graduating high school in year t based off turning 19 in that year and (b) can link the individual to their parents via the Decennial Censuses (as described in Appendix Section A.8), where (c) the MSA of their parents residence as of the high school graduation year t is is available in our LEHD data. We then keep only those individuals who (d) have strictly positive earnings in the LEHD for each of the next eight years starting in their high-school graduation year t. This last restriction allows us to estimate dynamic earnings effects for a consistent sample of workers, and ensures that we are considering workers that work immediately after high school. The sample restriction has the downside of condintioning on high-school graduates' choice of working immediately after high school or not, which could be endogeneous to the conditions of the MSA in which they graduate high school. We plan to address the potential effects of this sample selection in the future.

Design Appendix Figure B.7 shows our estimates for the effect on contemporaneous and future earnings of being exposed to a market with a lot of boom-induced job creation upon graduating high school. For each high school graduate in our sample, we run annual worker-level regressions in which we regress $w_{it}^{(h)}$ – the log of total earnings of the four quarters in year $t+h$ (where year t is the four quarters from the third quarter of year t to the second quarter $t+1$, and so on) – on the Bartik instrument $\pi_{r(i)t}^{bartik}$ from equation (13) interacted with c_t:

$$w_{it}^{(h)} = \alpha^{(h)} + \eta^{(h)} \cdot \pi_{r(i)t}^{bartik} + \gamma^{(h)} \cdot \left(\pi_{r(i)t}^{bartik} \times c_t\right) + \psi_t' \cdot \boldsymbol{X}_{it} + \phi_{r(i)} + \boldsymbol{X}_{r(i)t} + \epsilon_{it}^{(h)} \tag{16}$$

where $\pi_{r(i)t}^{bartik}$ is the credit shock Bartik of the MSA of the high-school graduate's parents, $\phi_{r(i)}$ is a vector of MSA fixed effects, $\boldsymbol{X}_{it}$ is and $\boldsymbol{X}_{r(i)t}$ includes the same MSA-level controls as in Appendix Figure A9 (other than the lags of c_t and GDP growth, given the short panel). The object $\psi_t' \cdot \boldsymbol{X}_{it}$ is a year fixed effect specific to the high-school graduate's type, as proxied by a set of binned characteristics that include: the graduate's race and sex, the education of the graduate's parents, the lifetime earnings quintile of graduate's parents, and earnings quintile of the graduate's parents over the previous year.

Comparison to regional unemployment rate exposure Appendix Figure A11 shows estimates of the a specification similar to that of Appendix Figure B.7, except that the credit shock Bartik $\pi_{r(i)t}^{bartik} \cdot c_t$ is replaced with the MSA-level unemployment rate as of year t:

$$w_{it}^{(h)} = \alpha^{(h)} + \gamma^{(h)} \cdot ur_{r(i),t} + \psi_t' \cdot \boldsymbol{X}_{it} + \phi_{r(i)} + \boldsymbol{X}_{r(i)t} + \epsilon_{it}^{(h)} \tag{17}$$

I Appendix Tables

Table A1: Quarterly bond-level regressions of realized excess returns on predicted credit spreads

	Without quarter FEs					With quarter FEs				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1 year	2 year	3 year	4 year	5 year	1 year	2 year	3 year	4 year	5 year
π_{ft}	1.007	3.987***	6.152***	7.797***	8.473***	0.914	3.501***	5.883***	7.411***	8.241***
	(1.22)	(4.28)	(7.54)	(10.22)	(8.94)	(1.04)	(4.24)	(7.21)	(8.27)	(7.96)
$(a_t + \pi^{dtod}_{f,t-1} \cdot c_t)$	-4.846***	-7.870***	-8.628***	-10.10***	-10.80***	-4.203***	-7.362***	-8.028***	-8.215***	-8.384***
	(-3.53)	(-5.44)	(-6.60)	(-6.13)	(-7.12)	(-3.23)	(-5.75)	(-8.63)	(-10.74)	(-11.75)
Quarter FEs	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes
N	36634	36634	36634	36634	36634	36634	36634	36634	36634	36634
Adjusted R2	0.151	0.204	0.249	0.290	0.283	0.444	0.434	0.405	0.410	0.401
Fraction of bonds active	0.955	0.917	0.885	0.855	0.824	0.955	0.917	0.885	0.855	0.824
Within R2						0.0349	0.0750	0.107	0.133	0.149
% variance: across quarter						0.493	0.468	0.497	0.562	0.569
% variance: within quarter						0.507	0.532	0.503	0.438	0.431

Notes: This table shows the results of quarterly bond-level regressions of realized excess returns on predicted credit spreads, based off the firm's default risk and the price of default risk estimated across all bonds for the given quarter. The regressions take the form

$$r^{(h)}_{bt} = \alpha_0 \cdot \pi^{dtod}_{f,t-1} + \alpha_1 \times (a_t + \pi^{dtod}_{f,t-1} \cdot c_t) + \boldsymbol{X}_{bt} + \epsilon_{bft}$$

run over annual forecasting horizons $h = 0$ to $h = 4$ See Appendix Section C.1 for details on the sample and variables. Standard errors are Driscoll-Kraay with lag length $4 \cdot h + 4$. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels, respectively.

Table A2: Relationship between c_t and risky firm credit spreads in other credit markets

	Bank small biz. lending		Syndicated loans		Commercial paper	
	(1)	(2)	(3)	(4)	(5)	(6)
	Low risk	High-low risk	Low risk	High-low risk	Low risk	High-low risk
c_t	0.0415	0.0931	0.136**	0.122*	0.0673	0.536**
	(0.54)	(1.01)	(2.01)	(1.66)	(0.58)	(2.43)
N	81	81	171	117	95	91
R^2	0.00280	0.0141	0.0185	0.0198	0.00660	0.420

Notes: This table shows the results of quarterly bivariate regressions of the form

$$y_t = \alpha + \beta c_t + \epsilon_t$$

where y_t is a measure of aggregate spreads in a certain credit market. All variables are normalized by their standard deviation. See Appendix Section D for details on the dependent variables. Standard errors are Newey-West with four lags. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels, respectively.

Table A3: Relationship between c_t and aggregate credit constraints

	Bank small biz. lending	Syndicated loan covenants		Bond covenants	
	(1)	(2)	(3)	(4)	(5)
	Loosening standards	Low risk	High-low risk	Low risk	High-low risk
c_t	0.592***	0.0970*	0.149**	-0.147	0.225***
	(9.63)	(1.85)	(2.04)	(-1.57)	(2.63)
N	122	97	97	137	137
R^2	0.449	0.0413	0.0428	0.0252	0.0586

Notes: This table shows the results of quarterly bivariate regressions of the form

$$y_t = \alpha + \beta c_t + \epsilon_t$$

where y_t is a measure of credit standards/constraints in different credit markets. All variables are normalized by their standard deviation. See Appendix Section E for details on the dependent variables. Standard errors are Newey-West with four lags. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels, respectively.

Table A4: Quarterly firm-level regressions of debt issuance on credit conditions and default risk

	Overall		Bonds		Syndicated loans: credit line		Syndicated loans: term loan	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\pi_{f,t-1}$	-3.821***	-3.396***	-1.483***	-2.539***	-1.293***	-0.471	-1.078***	0.0694
	(-18.18)	(-7.70)	(-4.74)	(-3.01)	(-5.12)	(-1.06)	(-3.56)	(0.07)
$\pi_{f,t-1} \times c_t$	0.884***	0.845***	0.607***	0.556*	0.547**	0.749***	0.305	0.413
	(5.27)	(4.56)	(2.89)	(1.90)	(2.16)	(3.34)	(1.15)	(1.09)
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter-sector FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Credit lag interactions	No	Yes	No	Yes	No	Yes	No	Yes
GDP growth interactions	No	Yes	No	Yes	No	Yes	No	Yes
N	326606	326606	52706	52706	118280	118280	65883	65883

Notes: This table quarterly firm-level regressions for the debt issuance of risky firms as credit conditions c_t vary. For the set of nonfinancial Compustat firms, each column shows the results of a quarterly regression of the form

$$\Delta D_{ft} = \alpha + \eta \cdot \pi_{f,t-1} + \gamma \pi_{f,t-1} \times c_t + \phi_f + \phi_{kt} + \boldsymbol{X}_{ft} + \epsilon_{ft}$$

where ΔD_{ft} is the firm's net debt issuance in different credit markets. See Appendix Section F for details on the sample and variables. Standard errors are Driscoll-Kraay with lag length 4. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels, respectively.

Table A5: Quarterly firm-level regressions of debt issuance, split by inflow vs. outflow or maturity, on credit conditions and default risk

	LT Debt (>1 year)			Bonds			Syndicated loans: credit line			Syndicated loans: term loan		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Net	In	Out	Net	In	Out	Net	In	Out	Net	In	Out
$\pi_{f,t-1}$	-3.695***	4.850***	8.673***	-2.539***	-2.381**	0.181	-0.471	-0.426	0.0310	0.0694	0.136	0.0557**
	(-3.87)	(3.84)	(9.04)	(-3.01)	(-2.42)	(0.29)	(-1.06)	(-0.99)	(1.45)	(0.07)	(0.14)	(2.26)
$\pi_{f,t-1} \times c_t$	1.356***	1.263**	-0.119	0.556*	1.095***	0.521**	0.749***	0.677***	-0.0636***	0.413	0.417	0.0135
	(3.04)	(2.04)	(-0.37)	(1.90)	(2.93)	(2.19)	(3.34)	(3.13)	(-6.00)	(1.09)	(1.08)	(1.50)
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter-sector FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Credit lag interactions	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
GDP growth interactions	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	280515	280515	280515	52706	52706	52706	118280	118280	118280	65883	65883	65883

(a) By inflows vs. outflows

	All		LT Debt				Bonds				Syndicated loans: credit line				Syndicated loans: term loan			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
	<1 yr	>1 yr	<1 yr	1-3 yr	3-5 yr	>5 yr	<1 yr	1-3 yr	3-5 yr	>5 yr	<1 yr	1-3 yr	3-5 yr	>5 yr	<1 yr	1-3 yr	3-5 yr	>5 yr
$\pi_{f,t-1}$	-1.203***	-2.144***	-0.0742	-0.181	-0.261*	-0.568**	0.139	-0.886***	0.930	-2.756***	0.397	-0.259	-1.204**	0.346	0.771**	-0.114	-0.990*	-0.329
	(-5.51)	(-5.45)	(-0.95)	(-1.24)	(-1.72)	(-2.05)	(1.51)	(-3.05)	(1.54)	(-3.60)	(0.91)	(-0.38)	(-2.53)	(1.42)	(2.45)	(-0.22)	(-1.92)	(-0.91)
$\pi_{f,t-1} \times c_t$	0.0591	0.747***	0.00599	0.0511	0.0952	0.481***	0.00871	-0.337***	-0.0333	0.886***	-0.157	0.208	0.272	0.284***	0.0358	-0.109	-0.0146	0.279**
	(0.85)	(4.05)	(0.17)	(0.61)	(1.39)	(3.84)	(0.17)	(-2.96)	(-0.12)	(3.02)	(-1.13)	(0.84)	(1.43)	(3.43)	(0.41)	(-0.62)	(-0.08)	(2.03)
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter-sector FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Credit lag interactions	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
GDP growth interactions	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	326606	326606	324146	324146	324146	324112	52706	52706	52706	52706	131595	126647	129468	131302	122102	94171	110311	128753

(b) By maturity

Notes: This table quarterly firm-level regressions for the debt issuance of risky firms as credit conditions c_t vary. For the set of nonfinancial Compustat firms, each column shows the results of a quarterly regression of the form

$$\Delta D_{ft} = \alpha + \eta \cdot \pi_{f,t-1} + \gamma \pi_{f,t-1} \times c_t + \phi_f + \phi_{kt} + \boldsymbol{X}_{ft} + \epsilon_{ft}$$

where ΔD_{ft} is debt issuance across different markets or of different security types. Panel (a) shows regressions in which ΔD_{ft} is split up into gross inflows vs. outflows. Panel (b) shows regressions in which ΔD_{ft} is split up into the net issuance of securities in different maturity buckets. See Appendix Section F for details on the sample and variables. Standard errors are Driscoll-Kraay with lag length 4. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels, respectively.

Table A6: Regressions of establishment-level employment growth on firm-level credit condition exposure in QFR sample

	(1)	(2)	(3)	(4)	(5)
$\pi_{f,t-1}$	-5.284***	-5.761***	-7.513***	-8.776***	-7.622***
	(0.5996)	(0.4263)	(0.4218)	(1.126)	(2.636)
$\pi_{f,t-1} \times c_t$	2.464	2.345**	3.838***	3.556***	3.607***
	(1.748)	(1.105)	(0.9461)	(0.9467)	(1.133)
Year FEs	Yes	No	No	No	No
Year-Industry-Region FEs	No	Yes	Yes	Yes	Yes
Credit condition lags	No	No	Yes	Yes	Yes
GDP growth interaction controls	No	No	No	Yes	No
Unemployment rate interaction controls	No	No	No	No	Yes
Number of establishment-years (N)	1,482,000	1,482,000	1,482,000	1,482,000	1,482,000
Number of firm-years	233,000	233,000	233,000	233,000	233,000

Notes: This table shows estimates of the contemporaneous employment growth of risky firms when credit conditions are loose, relative to less risky firms. For an establishment e controlled by a firm f at the start of year t, we run different variants of the regression

$$g_{et}^{(0)} = \alpha^{(0)} + \eta^{(0)} \cdot \pi_{f,t-1} + \gamma^{(0)} \times \left(\pi_{f,t-1} \cdot c_t\right) + \psi_t + \phi_e \boldsymbol{X}_{eft} + \epsilon_{eft}^{(0)}$$

The sample is the set of establishment-years in the LBD from 1978-2020 that are controlled by a manufacturing firm that was sampled in the most recent Economic Census version of the QFR (see Appendix Section A.2). The left-hand side variable $g_{et}^{(0)}$ is employment growth from year $t-1$ to t, measured using the symmetric growth rate of Davis and Haltiwanger (1992) (see Equation 2.3). The key right-hand side variable is the interaction between firm-level default risk $\pi_{f,t-1}$ and aggregate credit conditions c_t. Default risk is proxied by book leverage, scaled by the estimated relationship among Compustat firms between the negative of Merton (1974) distance to default and book leverage (see Appendix Section B.2). This makes the units of the estimates the same as those in Figure 2. Credit conditions are measured based off the year-specific relationship between credit spreads and default risk in the bond market (see Appendix Section B.1). All regressions include establishment fixed effects. For additional controls, Column (1) includes year and establishment fixed effects. Column (2) includes year fixed effects ψ_{rkt} that are specific to the establishment's MSA r-by-four-digit NAICS k pair. Column (3) adds controls for the interaction of $\pi_{f,t-1}$ with two lags of c_t, giving the regressions a local projection interpretation (Jordá, 2005). Column (4) adds controls for the interaction of two lags of real GDP growth, as well as year t GDP growth, with $\pi_{f,t-1}$. Column (5) replaces the GDP growth interaction controls with analogous controls that use the level of the unemployment rate rather than GDP growth. The regressions are weighted by the establishment's average level of employment between years $t-1$ and t, divided by the sum of these weights across all observations in the given year. These weights are then multiplied by the parent firm's QFR sample weight, such that the estimate of $\gamma^{(h)}$ reflects the behavior of the average (employment-weighted) manufacturing firm in the economy. Standard errors are double clustered on firm and year. The coefficients on $\pi_{f,t-1}$ are interpreted as the estimated effect on an establishment's employment growth of being controlled by a risky parent firm that, when c_t is at its sample mean, faces predicted credit spreads 100 basis points higher than the establishment controlled by a less risky firm. The coefficients on the interaction term $\pi_{f,t-1} \cdot c_t$ are interpreted as the estimated effect on an establishment's employment growth of their risky firm's credit spread being reduced by 100 basis points more as credit conditions loosen, relative to an establishment controlled by a less risky firm. All sample sizes are rounded to the nearest hundred following disclosure guidelines by the U.S. Census Bureau. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels, respectively.

Table A7: Partial equilibrium aggregation exercise

	Actual growth		Counterfactual growth		Actual - Counterfactual	
	(1)	(2)	(3)	(4)	(5)	(6)
c_t	1.054***	0.682***	0.382	0.0401	0.672***	0.642***
	(3.91)	(3.73)	(1.67)	(0.19)	(6.18)	(9.71)
GDP growth		0.772***		0.651**		0.121**
		(2.78)		(2.20)		(2.11)
Credit condition lags	Yes	Yes	Yes	Yes	Yes	Yes
GDP growth lags	No	Yes	No	Yes	No	Yes
N	40	40	40	40	40	40
Adjusted R2	0.328	0.557	0.140	0.374	0.712	0.759

Notes: This table shows the results of the partial equilibrium aggregation exercise described in Appendix Section H.2. It shows annual time series regressions of the form

$$g_t^{agg-x} = \alpha + \beta c_t + \boldsymbol{X_t} + \epsilon_t$$

where g_t^{agg-x} is an aggregate growth rate (for firms in our Compustat-LBD sample) for year t, c_t is aggregate credit conditions, and $\boldsymbol{X}$ is a vector that contains two lags of c_t and, for Columns 2, 4, and 6, year t GDP growth as well as two lags of GDP growth. As described in Appendix Section H.2, g_t^{agg-x} is the actual aggregate growth rate in the Compustat-LBD sample in Columns (1) and (2); the counterfactual aggregate growth rate when c_t is at its sample mean in each year in Columns (3) and (4); and the difference between the actual and counterfactual growth rates in Columns (5) and (6). Standard errors are Newey-West with two lags. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels, respectively. All sample sizes are rounded to the nearest hundred following disclosure guidelines by the U.S. Census Bureau. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels, respectively.

Table A8: Contemporaneous response of regional employment to credit market shock

	(1) Baseline	(2) Using demeaned $\pi_{f,t-1}$	(3) Using only non-HQ estabs	(4) Both
$\pi_{r,t}^{bartik} \cdot c_t$	2.361^{***}	2.090^{***}	2.061^{***}	3.445
	(0.6818)	(0.8340)	(0.8157)	(3.025)
MSA FEs	Yes	Yes	Yes	Yes
Credit condition lags	Yes	Yes	Yes	Yes
GDP growth interactions controls	Yes	Yes	Yes	Yes
Industry employment growth Bartik	Yes	Yes	Yes	Yes
Number of MSA-years (N)	34,000	34,000	34,000	34,000
Within R^2	.05243	.05113	.005228	.05161

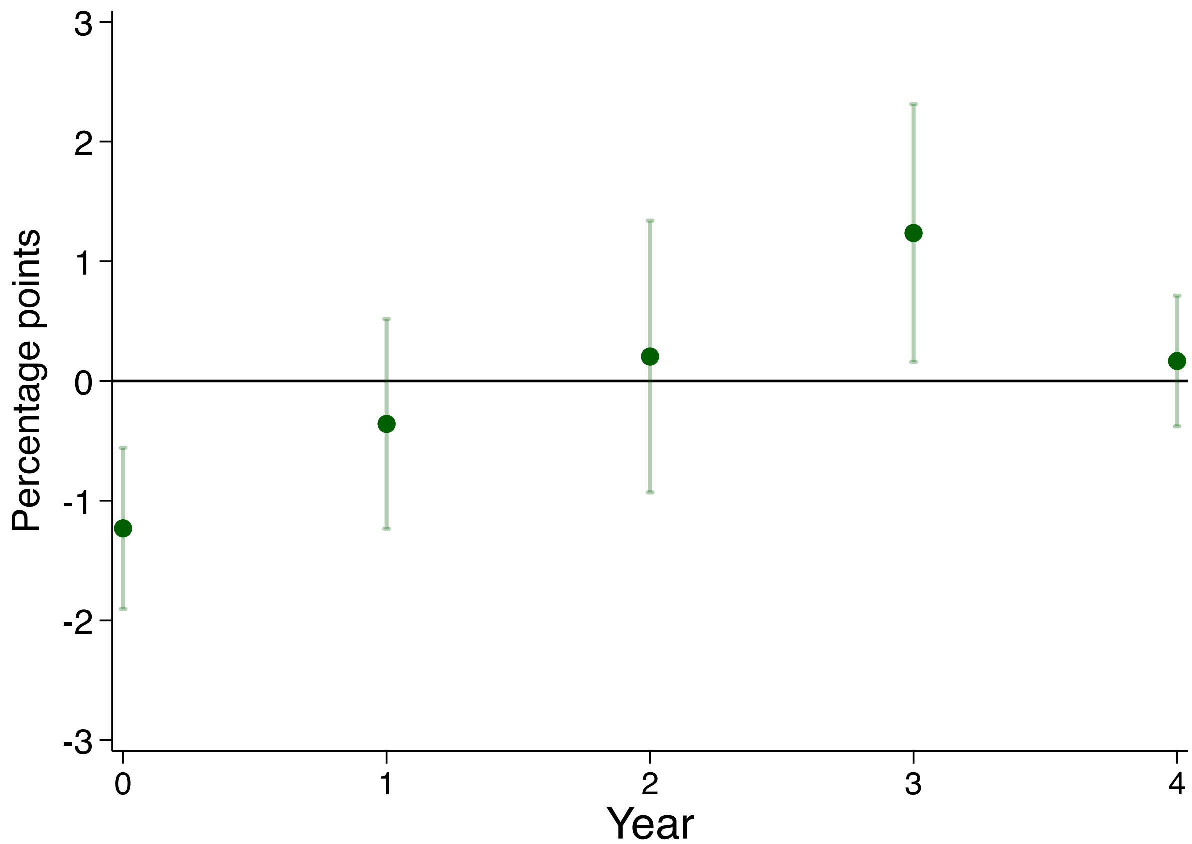
This table shows estimates of $\gamma^{(0)}$ from the annual MSA-level regression

$$g_{rt}^{(0)-agg} = \alpha^{(0)} + \eta^{(0)} \cdot \pi_{r,t}^{bartik} + \gamma^{(0)} \cdot \left(\pi_{r,t}^{bartik} \times c_t\right) + \psi_t + \phi_r + \boldsymbol{X}_{rt} + \epsilon_{rt}^{(0)}$$

where $g_{rt}^{(0)-agg}$ is aggregate MSA-level employment growth (including the establishments of both private and public firms) from $t-1$ to t. $\pi_{r,t}^{bartik}$ is the MSA-level Bartik instrument that is based off the default risk of public firms with establishments in the MSA, and these establishments' year $t-1$ employment shares, given by equation (13). See Appendix Section H.3 for a description of the controls included in the regression. The different columns correspond to different variants of the construction of the Bartik instrument, as explained in Appendix Section H.3. The regressions are weighted by MSA-level employment as of $t-1$. Standard errors are double clustered by year and MSA. The magnitudes are interpreted as effect on the MSA's aggregate employment growth when the employment-weighted average firm in an MSA experiences a 100 basis point reduction in spreads during a credit boom, relative than an MSA that is exposed to less risky firms. All sample sizes are rounded to the nearest hundred following disclosure guidelines by the U.S. Census Bureau. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels, respectively.

J Appendix Figures

Figure A1: Response of mass layoff events to credit conditions in Compustat sample

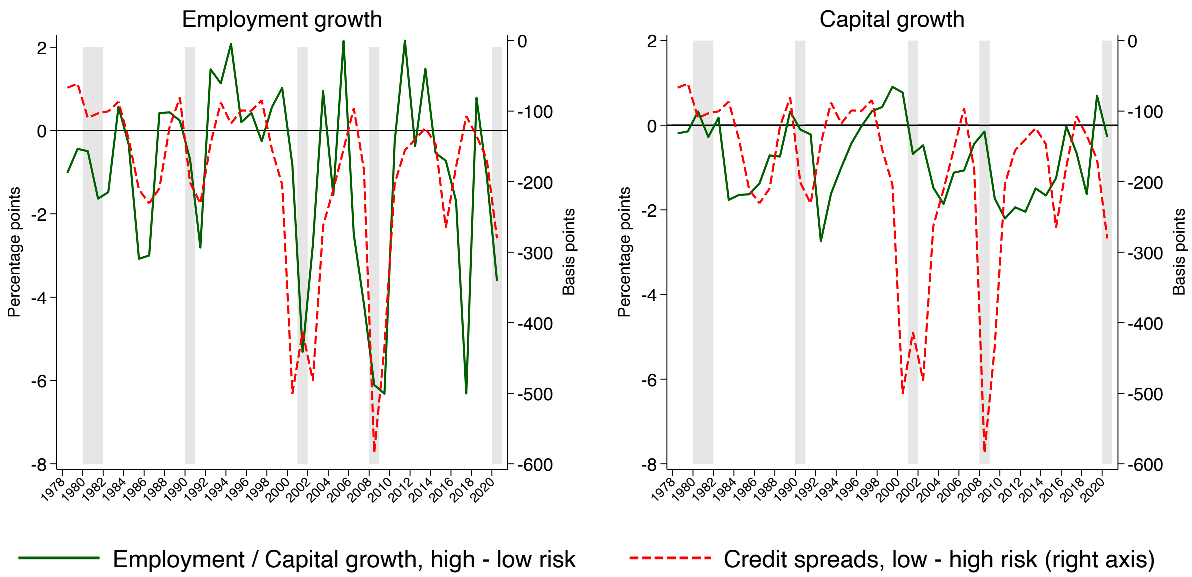


Notes: This figure plots estimates of the propensity of risky firms' establishments to engage in mass layoffs when credit conditions are loose. For an establishment e controlled by a firm f at the start of year t, it shows 95% confidence intervals of $\gamma^{(h)}$ from annual establishment-level Jordá (2005) local projections given by

$$1\{\text{Mass\_Layoff}_{et}^{(h)}\} = \alpha^{(h)} + \eta^{(h)} \cdot \pi_{f,t-1} + \gamma^{(h)} \times \left(\pi_{f,t-1} \cdot c_t\right) + \psi_{rkt} + \phi_e + \boldsymbol{X}_{eft} + \epsilon_{eft}^{(h)}$$

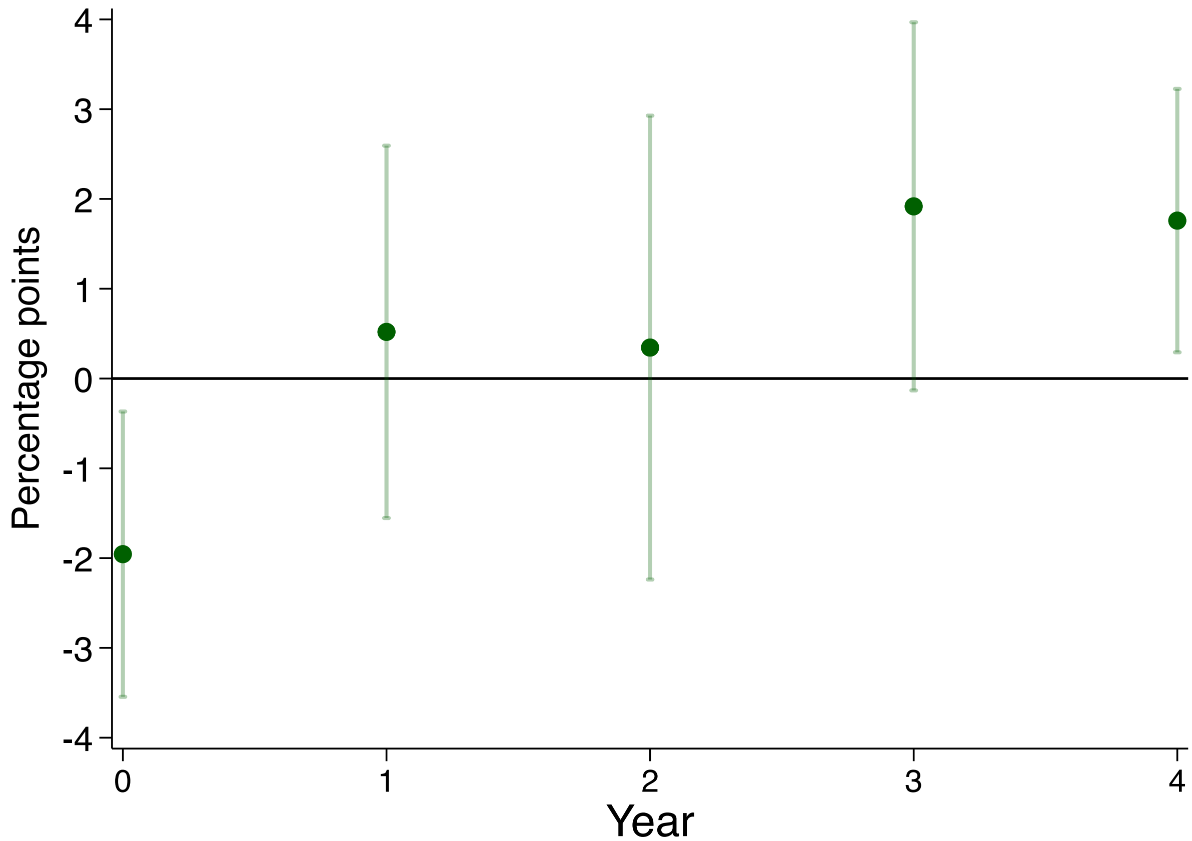
for annual horizons $h = 0$ to $h = 4$. The sample is the set of establishment-years in the LBD from 1978-2016 that are controlled by a Compustat firm (see Appendix Section A.1). The left-hand side variable is $1\{\text{Mass\_Layoff}_{ft}^{(h)}\}$, a dummy variable variable that equals one if the employment growth rate $g_{et}^{(h)} * 100$ is less than 30%, including cases in which the establishment closes completely (corresponding to $g_{et}^{(h)} = -2$). Default risk is proxied by the negative of Merton (1974) distance to default, constructed following Bharath and Shumway (2008) (see Appendix Section B.2). Credit conditions are measured based off the year-specific relationship between credit spreads and default risk in the bond market (see Appendix Section B.1). Each regression includes (a) establishment fixed effects ϕ_e; (b) year fixed effects ψ_{rkt} that are specific to the establishment's MSA r-by-four-digit NAICS k pair; (c) interactions of two lags of c_t with $\pi_{f,t-1}$; and (d) interactions of two lags of real GDP growth, as well as year t GDP growth, with $\pi_{f,t-1}$. The regressions are weighted by the establishment's average level of employment between years $t+h-1$ and $t+h$, divided by the sum of these weights across all observations in the given year. Standard errors are double clustered on firm and year. The coefficients are interpreted as the estimated effect on an establishment's mass layoff propensity of a risky firm's credit spread being reduced by 100 basis points more as credit conditions loosen, relative to an establishment controlled by a less risky firm.

Figure A2: Employment and capital growth of risky firms over credit cycles



This figure plots the annual employment /capital growth and predicted credit spreads of firms with high default risk relative to firms with low default risk. The sample consists of manufacturing establishments that, at the start of a given credit cycle episode (1978, 1983, 1992, 2003, 2010, or 2016), are controlled by a public firm. At the start of each episode, firms in the sample are put into quintiles of default risk π_{ft}. Default risk is proxied by the negative of Merton (1974) distance to default, constructed following Bharath and Shumway (2008) (see Appendix Section B.2). In the left panel, the solid green line (left y-axis) plots the weighted-average employment growth rate of establishments controlled by firms in the fifth quintile of risk ("high-risk firms") minus the growth rate of establishments controlled by firms in the first quintile of risk ("low-risk firms"). Employment growth is measured using the symmetric growth rate of Davis and Haltiwanger (1992) given by Equation (2.3). In the right panel, the solid green line (left y-axis) plots the same series, but for capital growth. Capital growth is based off cumulating capital expenditure flows, as in Giroud (2013), obtain from the Census's Annual Manufacturing Survey (ASM) and Census of Manufacturers (CMF). The dashed red line (right y-axis) plots the difference in predicted credit spreads between low- and high-risk firms. This is computed from the measure c_t of aggregate credit conditions that is based off the year-specific relationship between credit spreads and π_{ft} in the bond market (see Appendix Section B.1). The dashed red line shows c_t after it is multiplied by the difference in π_{ft} between the average firm in the first quintile and the average firm in the fifth quintile. This allows one to interpret the dashed red line as the predicted difference in credit spreads between low- and high-risk firms. The solid green line's value is large when high-risk firms' employment / capital growth is high relative to low-risk firms' growth, while the dashed red line's value is large when there is a less negative difference in the credit spreads of low-risk firms relative to high-risk firms. The correlation between the two series is 0.54.

Figure A3: Response of mass layoff events to credit conditions in QFR sample

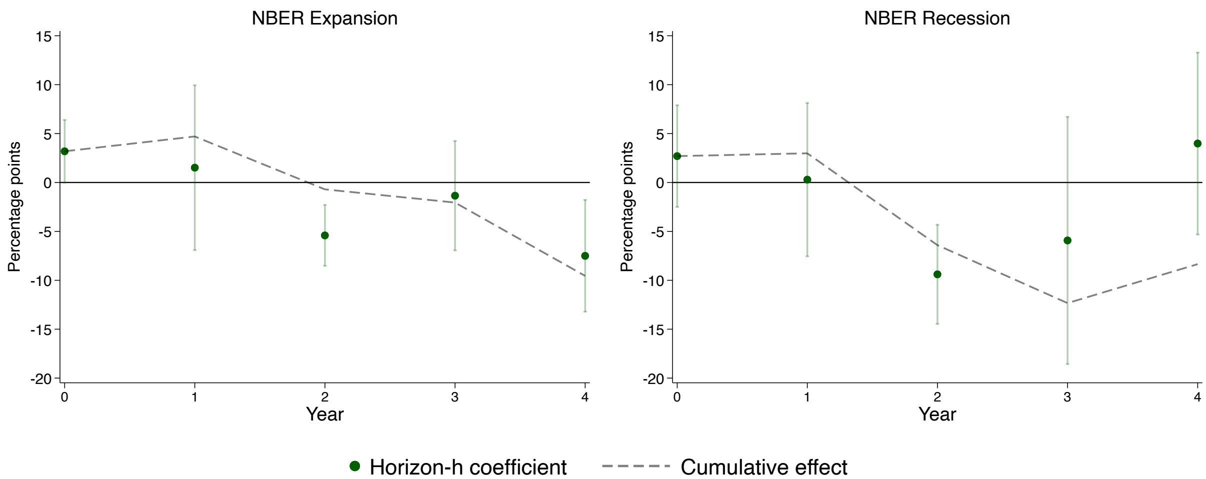


Notes: This figure plots estimates of the propensity of risky firms' establishments to engage in mass layoffs when credit conditions are loose. For an establishment e controlled a firm f at the start of year t, it shows 95% confidence intervals of $\gamma^{(h)}$ from annual establishment-level Jordá (2005) local projections given by

$$1\{\text{Mass\_Layoff}_{ft}^{(h)}\} = \alpha^{(h)} + \eta^{(h)} \cdot \pi_{f,t-1} + \gamma^{(h)} \times \left(\pi_{f,t-1} \cdot c_t\right) + \psi_{rkt} + \phi_e + \boldsymbol{X}_{eft} + \epsilon_{eft}^{(h)}$$

for annual horizons $h = 0$ to $h = 4$. The sample is the set of establishment-years in the LBD from 1978-2016 that are controlled by a manufacturing firm that was sampled in the most recent Economic Census version of the QFR (see Appendix Section A.2). The left-hand side variable is $1\{\text{Mass\_Layoff}_{ft}^{(h)}\}$, a dummy variable variable that equals one if the employment growth rate $g_{et}^{(h)} * 100$ is less than 30%, including cases in which the establishment closes completely (corresponding to $g_{et}^{(h)} = -2$). The key right-hand side variable is the interaction between firm-level default risk $\pi_{f,t-1}$ and aggregate credit conditions c_t. Default risk is proxied by book leverage, scaled by the estimated relationship among Compustat firms between the negative of Merton (1974) distance to default and book leverage (see Appendix Section B.2). This makes the units of the estimates the same as those in Figure 2. Credit conditions are measured based off the year-specific relationship between credit spreads and default risk in the bond market (see Appendix Section B.1). Each regression includes (a) establishment fixed effects ϕ_e; (b) year fixed effects ψ_{rkt} that are specific to the establishment's MSA r-by-four-digit NAICS k pair; (c) interactions of two lags of c_t with $\pi_{f,t-1}$; and (d) interactions of two lags of real GDP growth, as well as year t GDP growth, with $\pi_{f,t-1}$. The regressions are weighted by the establishment's average level of employment between years $t+h-1$ and $t+h$, divided by the sum of these weights across all observations in the given year. These weights are then multiplied by the parent firm's QFR sample weight, such that the estimate of $\gamma^{(h)}$ reflects the behavior of the average (employment-weighted) manufacturing firm in the economy. Standard errors are double clustered on firm and year. The coefficients are interpreted as the estimated effect on an establishment's mass layoff propensity of a risky firm's credit spread being reduced by 100 basis points more as credit conditions losen, relative to an establishment controlled by a less risky firm.

Figure A4: Sensitivity of employment growth to aggregate conditions of BB+ vs. matched BBB- establishments, split by NBER expansion vs. recession

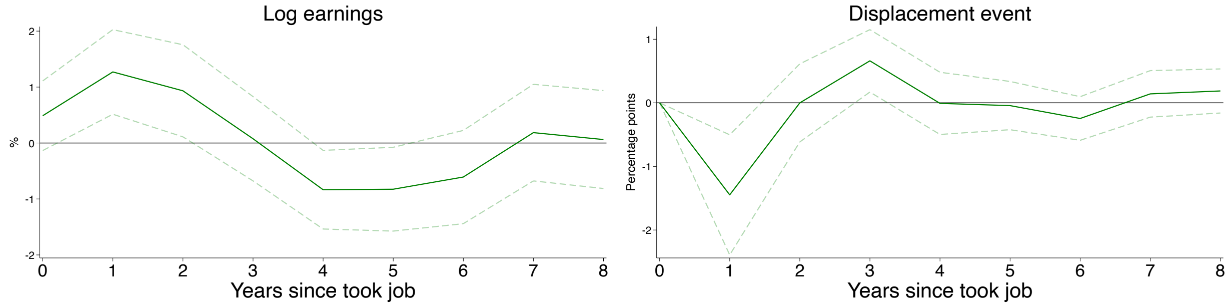


Notes: This figure plots estimates of the effect of loose credit conditions on the employment growth of the establishments of firms with a high-yield BB+ rating, relative to matched establishments of investment-grade firms with a BBB- rating. For an establishment e that as of year t is controlled by a firm f that has a BB+ rating, it shows 95% confidence intervals of $\gamma^{(h)}$ from annual establishment-level regressions given by

$$(g_{et}^{(h)} - g_{m(e)t}^{(h)}) = \alpha^{(h)} + \gamma^{(h)} \times \left(\delta_{BB+} \cdot c_t\right) + (\boldsymbol{X}_{eft} - \boldsymbol{X}_{m(e)m(f)t}) + \epsilon_{eft}^{(h)}$$

for annual horizons $h = 0$ to $h = 4$ and run when saturated with dummy variable for whether ($1\{\text{NBER Recession}_t\} = 1$) or not ($1\{\text{NBER Expansion}_t\} = 1$). The sample is the set of establishment-years in the LBD from 1978-2016 that are (a) controlled by a Compustat firm that is rated BB+; (b) not their firm's headquarters (see Appendix Section Appendix Section A.4), and (c) can be matched to an establishment $m(e)$ that lies in the same MSA and four-digit NAICS and is controlled by a firm with observably similar default risk but that is rated BBB-. See Section 3.3 for the details of the nearest-neighbor matching procedure. The left-hand side variable the difference between $g_{et}^{(h)}$, the employment growth from year $t+h-1$ to $t+h$ of the BB+ establishment, and $g_{m(e)t}^{(h)}$, growth of the matched BBB- establishment. Employment growth is measured using the symmetric growth rate of Davis and Haltiwanger (1992) (see Equation 2.3). The key right-hand side variable is aggregate credit conditions c_t, scaled by δ_{BB+}, the estimate from the bond-level regressions shown in Figure 4 of how much more sensitive the spreads of BB+ bonds are to c_t compared to spreads of BBB- bonds. All regressions include the following controls: (a) the differences $\boldsymbol{X}_{eft} - \boldsymbol{X}_{m(e)m(f)t}$ in the values of of the continuous matching variables between e and its matched establishment $m(e)$, (b) two lags of c_t, and (c) two lags of real GDP growth, along with year t GDP growth. The regressions are weighted by the BB+ establishment's average level of employment between years $t+h-1$ and $t+h$, divided by the sum of these weights across all observations in the given year. Standard errors are triple clustered on BB+ firm, year, and, following Abadie and Spiess (2022), matched BBB- firm. The coefficients are interpreted as the estimated effect on an establishment's employment growth when its controlling BB+ firm experiences a 100 basis point greater reduction in spreads as credit conditions loosen, relative to the growth of the matched BBB- establishment. The dashed black line shows the cumulative effect (as a percent of year $t-1$ employment) of these year-over-year estimates, which is the sum of the $\gamma^{(h)}$ estimates up to and including horizon h.

Figure A5: Outcomes of workers hired by risky firms during credit booms

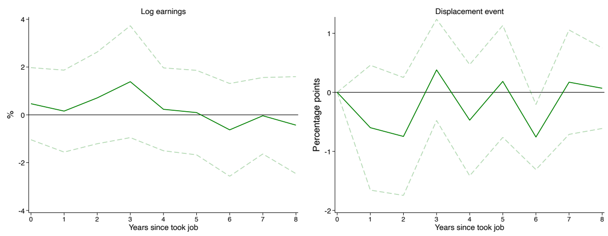


Notes: This figure plots estimates of the effect on worker outcomes of taking a job at a risky firm when credit conditions are loose. For a worker i who takes a job during quarter t at an establishment controlled by a firm f operating in MSA r and four-digit NAICS k, the figure shows 95% confidence intervals of $\gamma(h)$ from the quarterly worker-level regression

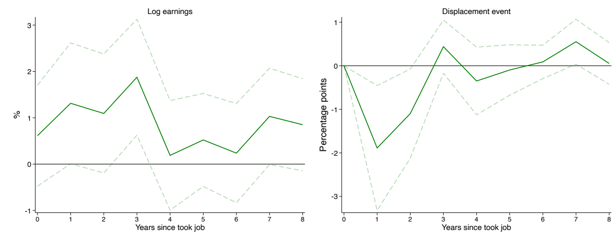
$$y_{it}^{(h)} = \alpha^{(h)} + \eta^{(h)} \cdot \pi_{f(i,t),t} + \gamma^{(h)} \times \left(pi_{f(i,t),t} \times c_{t-3,t}\right) + \phi'_{rkt} \times \boldsymbol{X}_{it} + \theta_{rkf} + \boldsymbol{X}_{f(i,t)t} + \epsilon_{ift}^{(h)}$$

run in four-quarter increments $h = 0, 4, ..., 32$. The sample is the set of prime-age workers in our LEHD sample that take a full-time, stable job at a Compustat firm during quarter t (see Appendix Section A.6). For the left-side plot, the dependent variable is $earn_{it}^{(h)}$, which is the log of total earnings over the four quarters starting with $t + h \cdot 4$. For the right-side plot, the dependent variable is $\text{displace}_{it}^{(h)}$, a dummy variable that equals one if the worker experiences a experience a displacement event (separation into non-employment) from any full-time job during one of the four quarters starting with $t + h \cdot 4$ (see Appendix Section B.7). The key right-hand side variable is the new firm's default risk $\pi_{f(i,t),t}$ as of quarter $t-4$ with average credit conditions between quarters $t-3$ and t, $c_{t-3,t}$. Firm-level default risk is proxied by the negative of Merton (1974) distance to default, constructed following Bharath and Shumway (2008) (see Appendix Section B.2). Credit conditions are measured based off the year-specific relationship between credit spreads and default risk in the bond market (see Appendix Section B.1). All regressions include time-invariant firm-region-industry fixed effects, as well quarter-region-industry fixed effects interacted with the unique value of a vector of binned worker-level characteristics. As detailed in Appendix Section B.8, these characteristics include variables for the worker's demographics, observed labor market state at the start of quarter t, and past labor market outcomes. The regressions also control for interactions of $\pi_{f,t-4}$ with one- and two-year lags of $c_{t-3,t}$, as well as with lagged and contemporaneous GDP growth. The regressions are equal weighted. Standard errors are double clustered by quarter and new firm. The coefficients are thus interpreted as the effect on workers' outcomes of taking a job at a risky firm that is experiencing a 100 basis points greater reduction in its spread, relative to a worker who takes a job at a less risky firm.

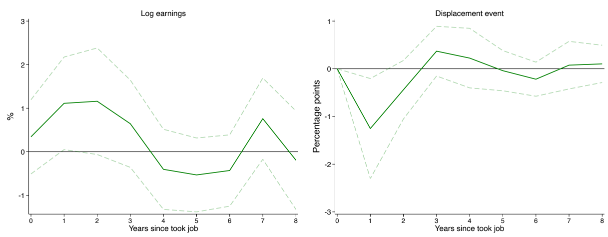
Figure A6: Worker earnings after taking job at firms in different risk quintiles during credit booms



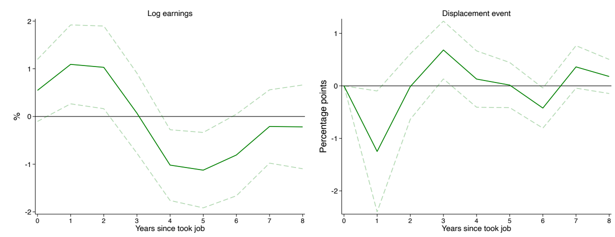
(a) Quintile 2



(b) Quintile 3



(c) Quintile 4



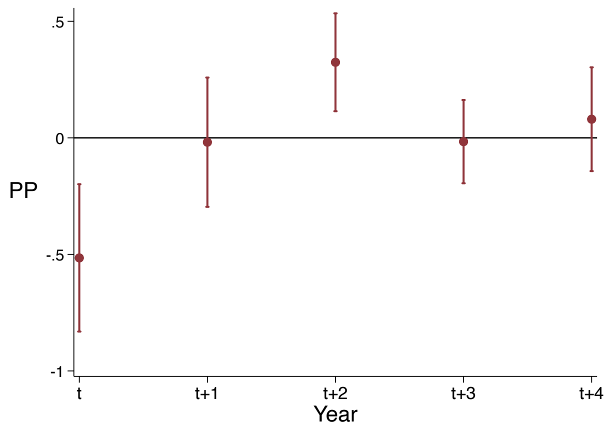
(d) Quintile 5

Notes: This figure plots estimates of the effect on worker outcomes of taking a job at a risky firm when credit conditions are loose. For a worker i who takes a job during quarter t at an establishment controlled by a firm f operating in MSA r and four-digit NAICS k, the figure shows 95% confidence intervals of $\gamma_j^{(h)}$ from the quarterly worker-level regression

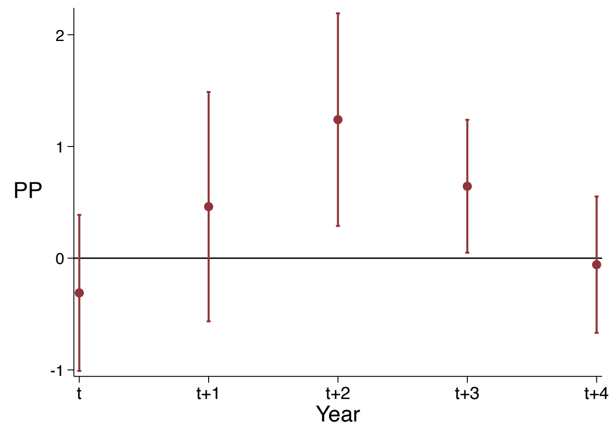
$$y_{it}^{(h)} = \sum_{j=2}^{5}\left(\alpha_j^{(h)} + \gamma_j^{(h)} \times c_{t-3,t}\right) \cdot 1\{\text{risk quintile}_{f(i,t),t} = j\} + \phi'_{rkt} \times \boldsymbol{X}_{it} + \theta_{rkf} + \boldsymbol{X}_{f(i,t)t} + \epsilon_{ift}^{(h)}$$

run in four-quarter increments $h = 0, 4, ..., 32$. The sample is the set of prime-age workers in our LEHD sample that take a full-time, stable job at a Compustat firm during quarter t (see Appendix Section A.6). For the left-side plot, the dependent variable is $earn_{it}^{(h)}$, which is the log of total earnings over the four quarters starting with $t + h \cdot 4$. For the right-side plot, the dependent variable is $\text{displace}_{it}^{(h)}$, a dummy variable that equals one if the worker experiences a experience a displacement event (separation into non-employment) from any full-time job during one of the four quarters starting with $t + h \cdot 4$ (see Appendix Section B.7). The key right-hand side variable is the interaction the quintile of the new firm's default risk $\pi_{f,t-4}$ as of quarter $t-4$ with average credit conditions between quarters $t-3$ and t, $c_{t-3,t}$. Firm-level default risk is proxied by the negative of Merton (1974) distance to default, constructed following Bharath and Shumway (2008) (see Appendix Section B.2). Credit conditions are measured based off the year-specific relationship between credit spreads and default risk in the bond market (see Appendix Section B.1). All regressions include time-invariant firm-region-industry fixed effects, as well quarter-region-industry fixed effects interacted with the unique value of a vector of binned worker-level characteristics. As detailed in Appendix Section B.8, these characteristics include variables for the worker's demographics, observed labor market state at the start of quarter t, and past labor market outcomes. The regressions also control for interactions of $\pi_{f,t-4}$ with one- and two-year lags of $c_{t-3,t}$, as well as with lagged and contemporaneous GDP growth. The regressions are equal weighted. Standard errors are double clustered by quarter and new firm. We divide $\gamma_j^{(h)}$ by the average default risk between firms in the fifth risk quintile and firms in the first risk quintile. The coefficients are thus interpreted as the effect on workers' outcomes of taking a job at a j^{th} quintile firm that is experiencing a 100 basis points greater reduction in its spread, relative to a worker who takes a job at a first quintile firm.

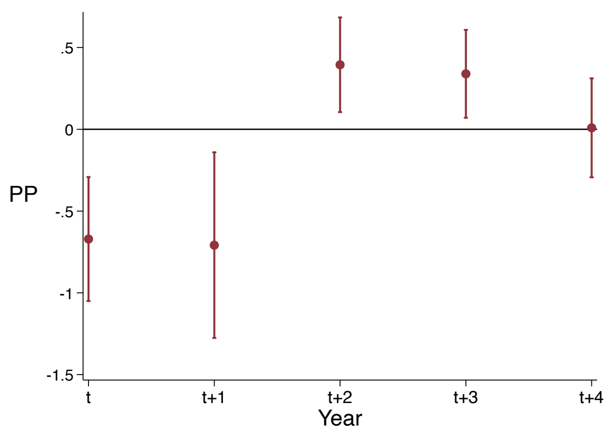
Figure A7: Response of risky firms' financial distress to loose credit conditions



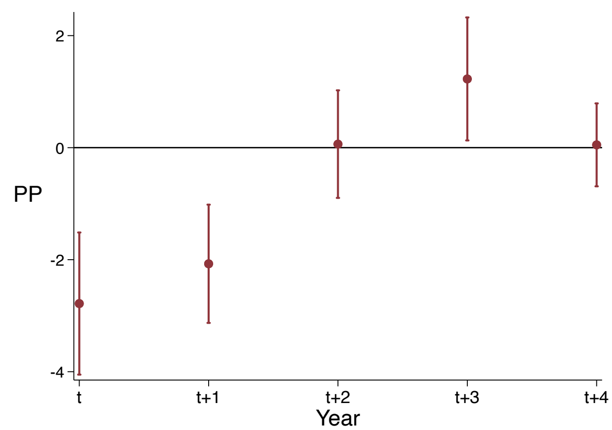
(a) Bankruptcy (BRD)



(b) Bankruptcy (Capital IQ)



(c) Bond default



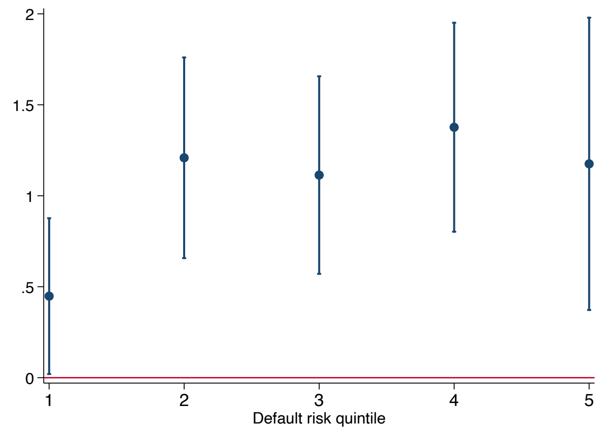
(d) Covenant violation

Notes: This figure shows the results of quarterly firm-level regressions of indicators for firm financial distress on the interaction of default risk with credit conditions c_t. For the set of nonfinancial Compustat firms, each panel plots 95% confidence intervals on $\gamma(h)$ obtained from regressions of the form

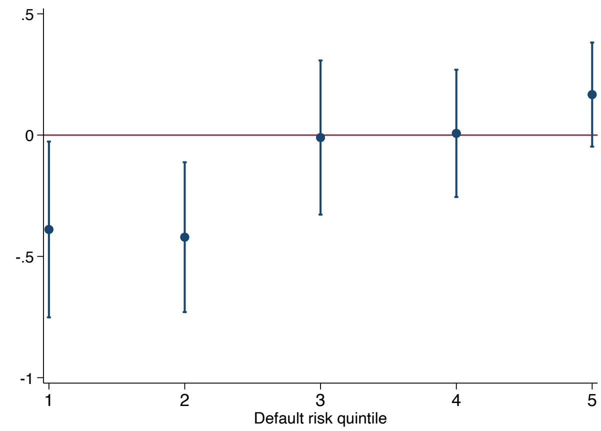
$$1\{\text{Distress}_{ft}^{(h)} = \alpha + \eta \cdot \pi_{f,t-1} + \gamma \pi_{f,t-1} \times c_t + \phi_f + \phi_{kt} + \boldsymbol{X}_{ft} + \epsilon_{ft}$$

run over horizons $h = 0$ to $h = 4$. $1\{\text{Distress}_{ft}^{(h)}$ is a dummy variable for whether the firm experiences a financial distress event – bankruptcy (panels and b), a bond default (panel c), or a covenant violation (panel d) – over the four-quarter horizon from $t + h \cdot 4$ to $t + h \cdot 4 + 4$, divided by assets as of quarter $t - 1$. See Appendix Section G for further details on the dependent variables. Standard errors are Driscoll-Kraay with lag length $4 \cdot h + 4$

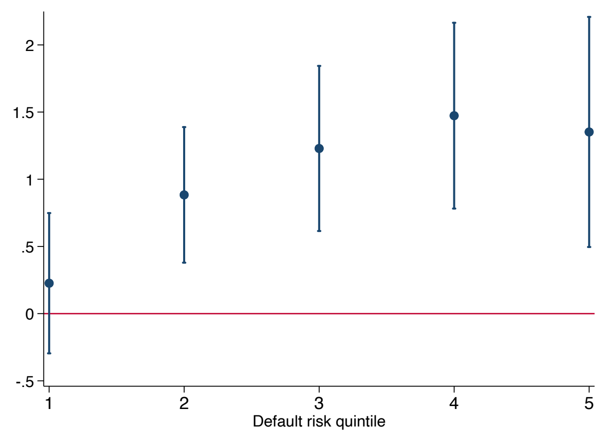
Figure A8: Loading of external financing flows and assets on credit conditions, by default risk quintile



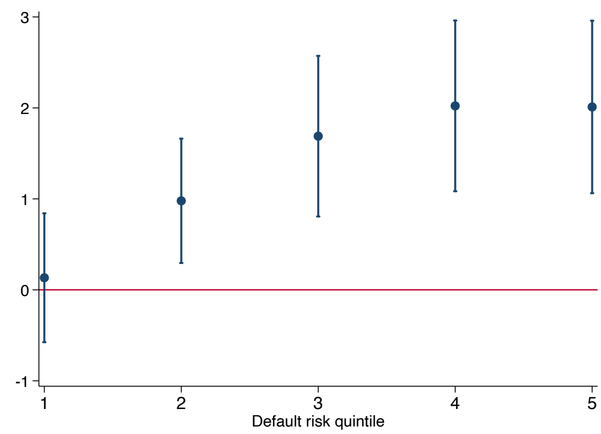
(a) Net debt growth



(b) Net equity growth



(c) Net debt + equity growth



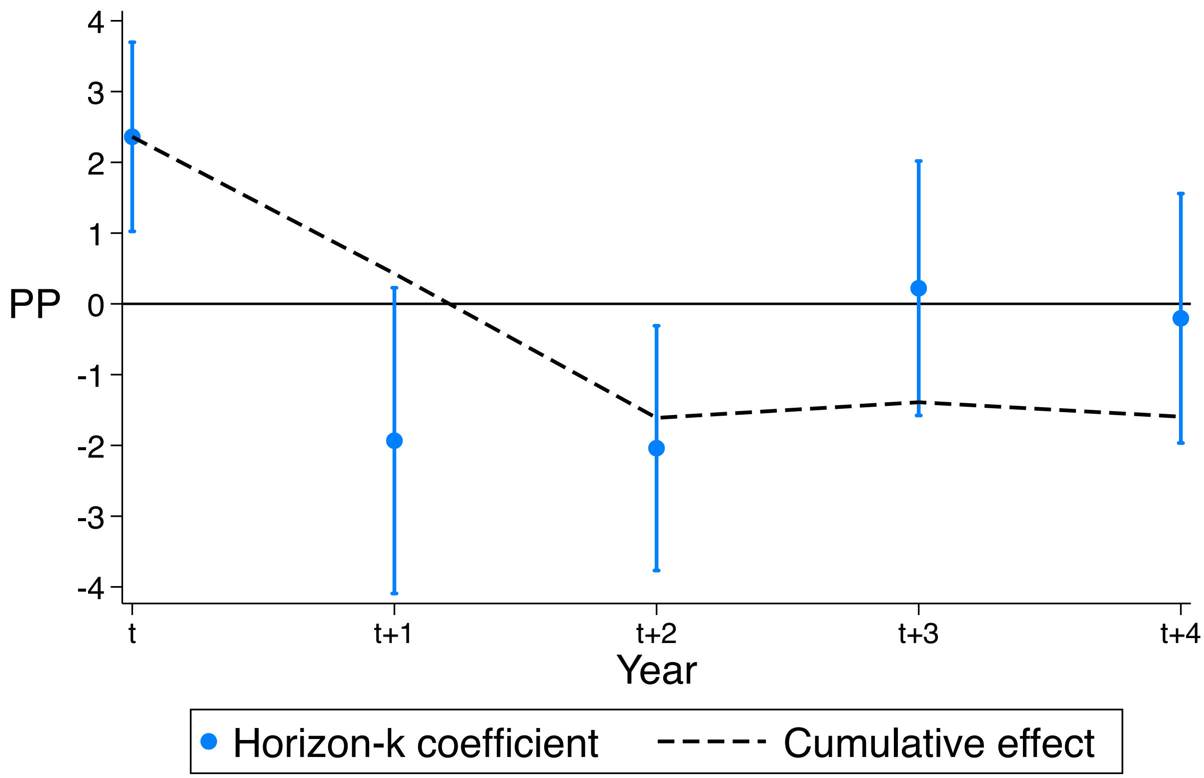
(d) Asset growth

Notes: This figure shows the results of quarterly firm-level regressions of financing or asset growth on credit conditions c_t, split by quintile of default risk. For the set of nonfinancial Compustat firms, each panel plots 95% confidence intervals on $\gamma_j^{(h)}$ for each default risk quintile j, obtained from regressions of the form

$$y_{ft}^{(h)} = \sum_{j=1}^{5} \left(\alpha_j^{(h)} + \gamma_j^{(h)} \times c_t\right) \cdot 1\{\text{risk quintile}_{f,t-1} = j\} + \phi_f + \boldsymbol{X}_{ft} + \epsilon_{ft}$$

run over horizons $h = 0$ to $h = 4$. The dependent variable $y_{ft}^{(h)}$ is the sum of financing flows (Panels a to c) or asset growth (Panel d) over the four-quarter horizon from $t + h \cdot 4$ to $t + h \cdot 4 + 4$, divided by assets as of quarter $t - 1$. See Appendix Section H.1 for further details on the variables. Standard errors are Driscoll-Kraay with lag length $4 \cdot h + 4$

Figure A9: Response of MSA employment to credit cycle shock

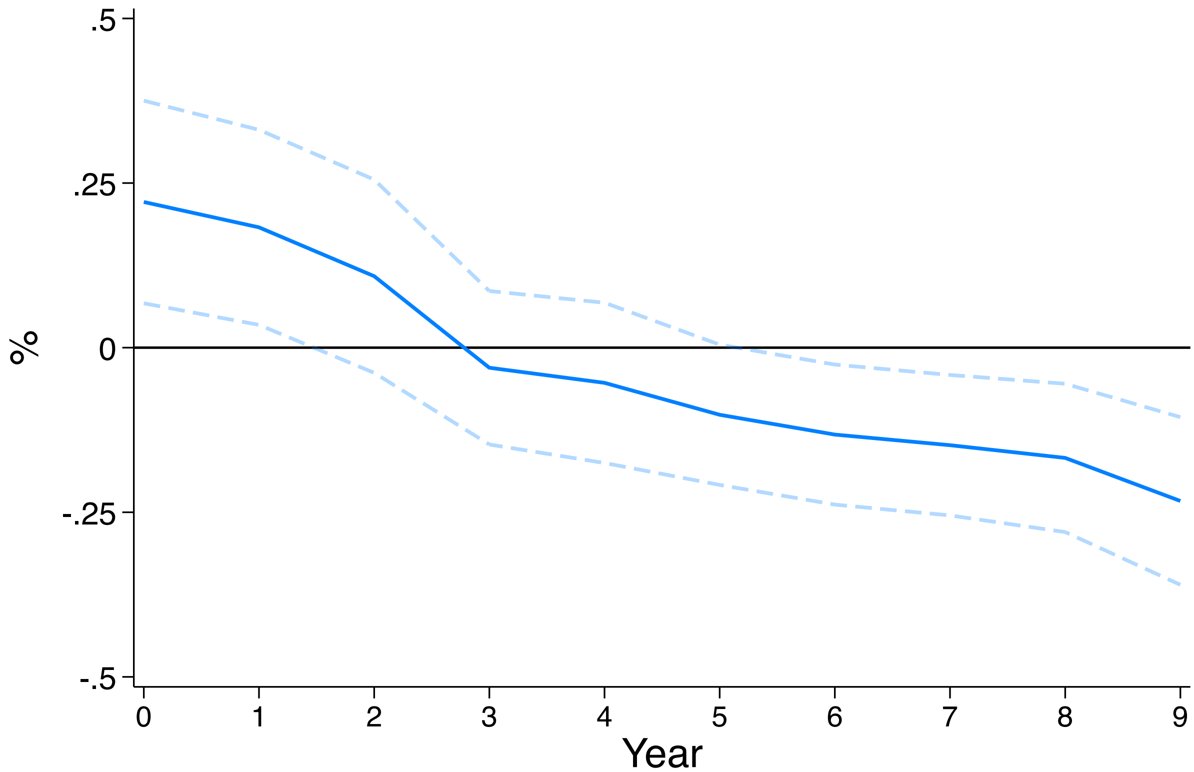


This figure plots 95% confidence intervals of estimates of $\gamma^{(h)}$ from the annual MSA-level Jordá (2005) local projection

$$g_{rt}^{(h)-agg} = \alpha^{(h)} + \eta^{(h)} \cdot \pi_{r,t}^{bartik} + \gamma^{(h)} \cdot \left(\pi_{r,t}^{bartik} \times c_t\right) + \psi_t + \phi_r + \boldsymbol{X}_{rt} + \epsilon_{rt}^{(h)}$$

run for annual horizons $h = 0$ to $h = 4$, where $g_{rt}^{(h)-agg}$ is aggregate MSA-level employment growth (including the establishments of both private and public firms) from $t + h - 1$ to $t + h$. $\pi_{r,t}^{bartik}$ is the MSA-level Bartik instrument that is based off the default risk of public firms with establishments in the MSA, and these establishments' year $t - 1$ employment shares, given by equation (13). The specification is the same as Column (0) of Appendix Table A8. The regressions are weighted by MSA-level employment as of $t - 1$. Standard errors are double clustered by year and MSA. The magnitudes are interpreted as effect on the MSA's aggregate employment growth during year $t + h$ when the employment-weighted average firm in an MSA experiences, at year t, a 100 basis point reduction in spreads during a credit boom, relative than an MSA that is exposed to less risky firms.

Figure A10: Effect of MSA-level exposure to credit boom-induced job creation on high school graduates' earning dynamics

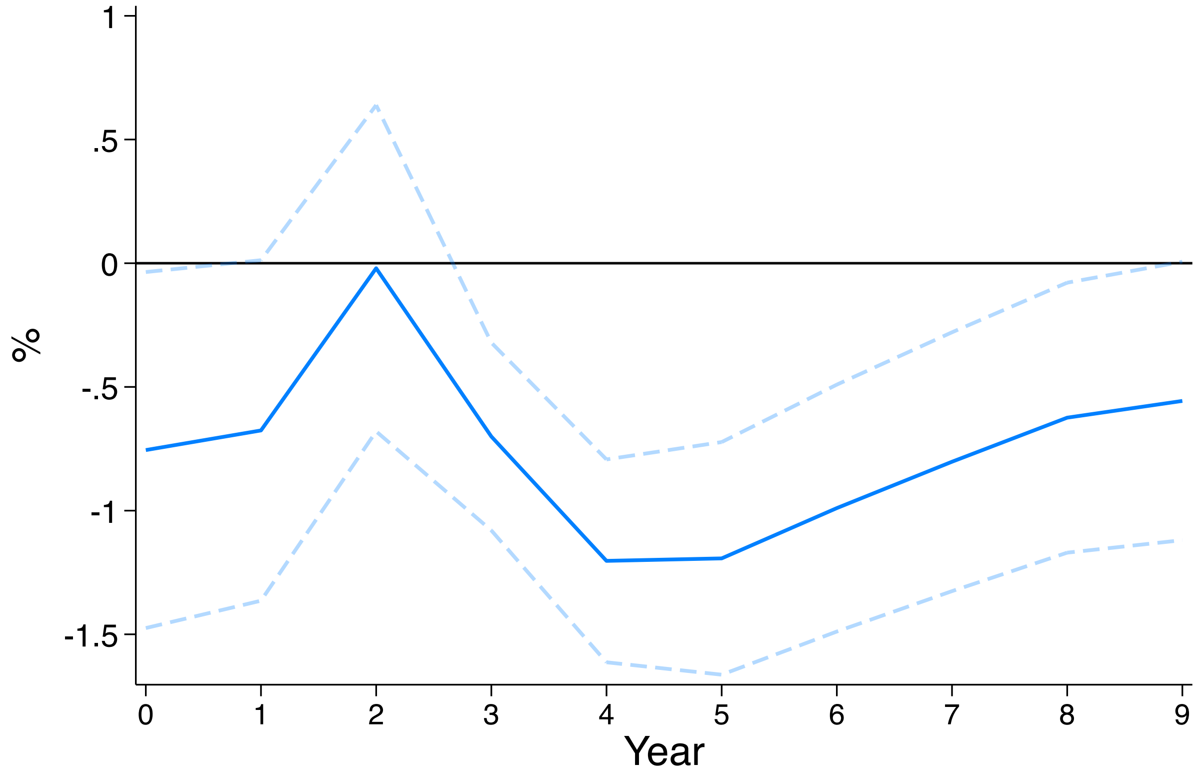


Notes: This figure plots 95% confidence intervals of estimates of $\gamma^{(h)}$ from the annual worker-level Jordá (2005) local projection

$$w_{it}^{(h)} = \alpha^{(h)} + \eta^{(h)} \cdot \pi_{r(i)t}^{bartik} + \gamma^{(h)} \cdot \left(\pi_{r(i)t}^{bartik} \times c_t\right) + \psi_t' \cdot \boldsymbol{X}_{it} + \phi_{r(i)} + \boldsymbol{X}_{r(i)t} + \epsilon_{it}^{(h)}$$

run for annual horizons $h = 0$ to $h = 4$, where $w_{it}^{(h)}$ is the log earnings of high-school graduate i during year $t + h$, $\pi_{r,t}^{bartik}$ is the MSA-level Bartik instrument – based off the MSA of the graduate's parents as of the high school graduation year t – that is based off the default risk of public firms with establishments in the MSA, and these establishments' year $t - 1$ employment shares, given by equation (13). Appendix Section H.4 provides details on the sample construction. The right-hand side variables are the same as Column (0) of Appendix Table A8. The regressions are unweighted. Standard errors are clusted by MSA-year. The magnitudes are interpreted as effect on the graduate's log earnings h years after high-school graduation when the employment-weighted average firm in an MSA experiences, at year t, a 100 basis point reduction in spreads during a credit boom, relative than to high school graduates in an mSA that is exposed to less risky firms.

Figure A11: Effect of MSA-level unemployment rate on high school graduates' earning dynamics



This figure plots 95% confidence intervals of estimates of $\gamma^{(h)}$ from the annual worker-level Jordá (2005) local projection

$$w_{it}^{(h)} = \alpha^{(h)} + \gamma^{(h)} \cdot ur_{r(i)t} + \psi'_t \cdot \boldsymbol{X}_{it} + \phi_{r(i)} + \boldsymbol{X}_{r(i)t} + \epsilon_{it}^{(h)}$$

run for annual horizons $h = 0$ to $h = 4$, where $w_{it}^{(h)}$ is the log earnings of high-school graduate i during year $t+h$ and $ur_{r(i)t}$ is the unemployment rate of the MSA in which the graduate's parents live as of their high-school graduation year t. Appendix Section H.4 provides details on the sample construction. The right-hand side variables are the same as Column (0) of Appendix Table A8. The regressions are unweighted. Standard errors are clusted by MSA-year. The magnitudes are interpreted as effect on the graduate's log earnings h years after high-school graduation when the MSA's unemployment rate is 1 percentage point higher than that of an MSA with a relatively stronger labor market.