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How does labor market size affect firm capital structure? Evidence from large plant openings[☆]

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ABSTRACT

I examine how the labor market in which firms operate affects their capital structure decisions. Based on US Census Bureau data and information on companies' decisions to locate their new operations, I use a large plant opening as an abrupt increase in the size of a local labor market. I find that a new plant opening leads to an increase of 2.5–3.9 percentage points in the debt-to-capital ratio of existing firms in the "winner" county relative to the "runner-up" choice. This result is consistent with the argument that larger labor markets make job loss less costly, which in turn reduces the indirect costs of financial distress. Notably, this spillover effect is larger for firms that employ the same type of workers as the new plant in the affected county.

1. Introduction

The size of the US labor market varies considerably across geographic areas, economic sectors, and time. The Rust Belt, which has undergone great losses in manufacturing jobs in recent decades (e.g., Kahn, 1999), was formerly home to America's chief manufacturing cities. Conversely, Sun Belt cities have recently observed a considerable expansion in manufacturing labor markets. Interestingly, firms that operate in larger labor markets have significantly higher leverage, controlling for firm and year fixed effects as well as other covariates (see Online

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Appendix A).[1] What might explain the association between labor market size and firm capital structure?

This paper examines whether and how the size of labor markets in which firms operate affects their capital structure decisions. To do so, I build a simple model that illustrates the economic forces that link labor market size and firms' capital structure (Section 2). The premise is that large labor markets facilitate workers in their job search, which makes losing their jobs less costly (e.g., Petrongolo and Pissarides, 2006). Therefore, in such markets, when firms use more debt, the marginal compensating premium for increased risk of job loss is smaller (e.g., Titman, 1984; Agrawal and Matsa, 2013). Given these smaller indirect costs of financial distress, firms operating in lager labor markets may use more debt in their capital structure.

I test this prediction by exploiting an empirical setting in which the size of a local labor market increases abruptly due to a large plant opening. Building on Greenstone and Moretti (2004) and Greenstone, Hornbeck, and Moretti (2010, GHM hereafter), my empirical approach uses local counties that either win a larger manufacturing plant ("winners") or lose the competition ("runners-up"). Under the assumption that firms operating in the winner and runner-up counties are comparable before the large plant opening, I estimate the effect of an increase in labor market size on corporate capital structure by comparing changes in leverage between the two groups of firms.[2] I use a large manufacturing plant opening as an increase in manufacturing labor market size under the plausible assumption that a significant portion of skills are specific to the manufacturing sector.[3]

To implement the quasi-natural experiment, I hand-collect information on rankings of potential large manufacturing plant sites from the corporate real estate journal Site Selection (Greenstone and Moretti, 2004; GHM, 2010). I merge the set of new manufacturing plants with the Census Bureau's plant-level data sets and identify other incumbent plants that operate in either the winner or runner-up counties. I then match these existing plants with Compustat firms and define firms that employ a significant fraction of their workforces in the winner and runner-up counties as the treated and control groups.

I find that before a large plant opens, the difference in characteristics between firms operating in winner counties and runner-up counties, including asset size, profitability, and market-to-book ratios, is not statistically significant. Importantly, the trends in the debt-to-capital ratio, defined as total debt (long-term plus short-term debt) divided by the sum of total debt and book equity, are statistically not different from each other between firms in winner and runner-up counties, providing evidence for a parallel trend in leverage. I also show that the difference in the number of existing manufacturing plants and their key operating characteristics, including employment and hours worked, is not statistically significant between winner and runner-up counties before the new plant enters the winner county. Combined with evidence in Greenstone and Moretti (2004) that county-level characteristics are similar between winners and runners-up before a plant opening, these findings support the identifying assumption that firms in winner and runner-up counties are valid counterfactuals of each other.

A typical new manufacturing plant in the sample leads to a 22.4% increase in the county's manufacturing labor market relative to the runner-up. I find that two years after the plant opening, manufacturing firms operating in the winner county increase their leverage ratios by 2.3 percentage points. In contrast, manufacturing firms in the runner-up county do not change their leverage during the same period. Four years after the plant opening, firms in the winner county increase their leverage by 3.9 percentage points relative to those in the runner-up county. This baseline result is robust to controlling for a difference in firm leverage between the winner and runner-up counties before a plant opening. In contrast, when the new plant and existing firms are in different industry sectors (e.g., manufacturing versus nonmanufacturing), opening a new plant leads to an insignificant change in leverage of existing firms.

I propose that an increase in the local labor market size, which reduces workers' job loss costs, leads firms to increase the use of debt in their capital structure. The leverage increase could also be consistent with other mechanisms, nonetheless. In particular, Greenstone and Moretti (2004) and GHM (2010) show that the new plants lead to increases in total factor productivity (TFP), wages, and property values in the winner county relative to the runner-up county, plausibly due to agglomeration spillovers. They argue and show evidence that labor market size, proximity to suppliers and buyers, and knowledge spillovers are likely mechanisms for those effects.

In turn, I attempt to examine the extent to which these and other related mechanisms, including expanded labor market size, explain the leverage increase. First, I measure similarity of skills using the frequency of worker moves from the industries of the existing plants to those of the new plants observed from the Census Bureau's Longitudinal Employment and Household and Dynamics (LEHD) data. Using this measure, I find that the effect of a plant opening is more pronounced for existing manufacturing firms operating in industries that employ workers who could potentially move to the industry of the new plant in the affected county. Second, I employ measures of how the industries of the new and existing plants are related along other mechanisms of agglomeration economies, especially proximity to suppliers and buyers and knowledge spillovers. I do not find evidence that incumbent firms that operate more related plants in the winner county experience a larger increase in leverage after the plant opening. Third, inconsistent with a collateral channel in which the new plant makes the incumbent firms' assets more redeployable, which in turn increases their debt usage, I find

[1] Large labor markets are often referred to as "thick labor markets." I use "large labor markets" for consistency.

[2] I define labor markets at the county level because workers concentrate their job searches in local areas (e.g., Moretti, 2011; Manning and Petrongolo, 2017).

[3] Consistent with this assumption, a large literature on displaced workers (e.g., Jacobson, LaLonde, and Sullivan, 1993; Couch and Placzek, 2010) finds that displaced manufacturing workers who move to nonmanufacturing industries are among those who experience the greatest earnings loss.

that existing firms increase their leverage whether or not they operate in industries that use assets highly similar to the assets used in the industry of the new plant in the winner county. Taken together, these analyses provide further evidence that labor market size is a plausible mechanism for the documented leverage increase while casting doubt on alternative channels.

Finally, I test the key presumption of the paper's empirical approach that large labor markets reduce the costs of job loss. I estimate how labor market size, measured by manufacturing employment at the county level, affects displaced workers' earnings loss by tracking individual workers after a mass layoff in the LEHD data. I find that a one standard deviation increase in labor market size is associated with a reduction in present-value earnings losses that is 41% of predisplacement annual earnings after a displacement.

The main contribution of this paper is to show evidence that search frictions in the labor market, proxied by labor market size measured at the local and sector level, shape corporate capital structure decisions. Previous research examining related questions includes Agrawal and Matsa (2013), which uses state-level changes in unemployment insurance benefits to show that employees' unemployment risk affects firms' capital structure choice. This paper also adds to the large literature on agglomeration spillovers by showing that firms' increased debt usage is another significant benefit of firm clustering in local areas due to, for example, improved incentives for managers (Harris and Raviv, 1990) and interest tax deductions (Graham, 2000).[4] A back-of-the-envelope calculation (see Online Appendix B) suggests that an increase in firm value owing to the increase in debt usage can amount to 0.06% to 0.18% of the aggregate market value of incumbent firms in the average winner county ($25.3 billion). Last, this paper provides novel evidence that labor market size is an important determinant of worker earnings after a job loss.

2. Simple model of labor market size and capital structure

This section illustrates conceptual links between the size of labor markets and corporate leverage policy using a simple static trade-off model of capital structure with labor as a production input. The framework for production is adapted from Acemoglu (1997) and that for capital structure is from Titman (1984) and Berk et al. (2010). Let L be a firm's leverage ratio, which is defined as the ratio of debt to the sum of equity and debt. Suppose that L *[0, 1]* generates a tax benefit $T(L)$, which is an increasing, concave function of L (Graham, 2000). L also increases the probability of financial distress, $p(L)$, and the loss given financial distress, $C_F(L)$,[5] where $p(L)$ and $C_F(L)$ are increasing, convex functions of L.

The firm hires labor (in size 1) to produce output by paying the wage W, and the labor invests in required skills for which the cost to the worker (e.g., effort) is K. For simplicity, I assume that investments in skills have a positive present value to the worker, S, beyond the current period if the worker (1) stays within the current firm or (2) moves to a firm that uses the same skills, but have zero value at all other firms. Importantly, I assume that the worker faces search frictions in the local labor market (Diamond, 1982). As a result, if the worker is laid off from the current firm, he or she is not able to find another job using current skills with probability u (<1). For simplicity, I assume that if a firm becomes financially distressed (with a probability $p(L)$), the worker is laid off with a probability of one.[6]

The firm solves the following maximization problem by choosing the optimal level of leverage L[7]:

$$\max_L \; T(L) - p(L)\cdot C_F(L) - W \tag{1}$$

$$\text{s.t. } W + [1 - p(L)\cdot u]\cdot S \geq K + e(o). \tag{2}$$

When a large employer (e.g., plant) enters the local labor market, there may be a short-run imbalance between labor demand and supply (i.e., excess demand) to the extent that labor supply is not perfectly elastic. I capture this effect on the worker's outside option and wages with $e(o)$, which takes the value of a constant $e > 0$ if a large plant opens ($o = 1$), and zero otherwise ($o = 0$). Eq. (2) represents the worker's participation constraint (PC) condition: the wage W (assumed to be paid up front for simplicity) must exceed the cost of effort K plus a potential increase in the worker's outside option due to an entry $e(o)$, net of the future expected benefits from investing in skills. Therefore, the firm offers the following take-it-or-leave-it wage offer to the worker, who would accept it:

$$W = K - [1 - p(L)\cdot u]\cdot S + \; e(o) \tag{3}$$

To derive the firm's optimal leverage choice, substituting W in Eq. (3) into Eq. (1) and taking the derivative of the equation with respect to L gives the following first-order condition:

$$T'(L) = p'(L)\cdot(\mathrm{u}\cdot\mathrm{S} + \mathrm{C}_F(L)) + p(L)\cdot C_F'(L). \tag{4}$$

Given that $p(.)$ and $C_F(.)$ are increasing and convex and $T(.)$ is concave, the optimal leverage ratio is decreasing in u. Therefore, an increase in labor market size (i.e., smaller u) would lead to an increase in the optimal leverage ratio, L^* (Main prediction).[8] In addition, Eq. (3) shows determinants of wages, W. First, K represents compensation for the

[4] See, e.g., Glaeser and Gottlieb (2009) for a review of the agglomeration economies literature. Commonly mentioned benefits of agglomeration include high productivity (GHM, 2010), wages (Glaeser and Maré, 2001), and innovative outputs.

[5] $C_F(L)$ includes traditional costs of financial distress such as those due to the bankruptcy process, loss of market share, and asset fire sales. However, $C_F(L)$ does not include the costs of distress due to search frictions in the labor market, which I introduce later.

[6] A similar result will be obtained if I assume a probability of a job loss conditional on financial distress that is less than one. See Agrawal and Matsa (2013) and Graham et al. (2019) for evidence that employees of distressed firms face a significant risk of job and earnings losses.

[7] I assume that the firm produces a fixed amount of output given the worker's skill investment K. The firm's objective function thus does not include the value of output.

[8] An implicit assumption of this and other predictions of the model is that employees can perceive the effect of the firm's financial distress on their job security. See Brown and Matsa (2016) for evidence that job seekers accurately perceive the financial strength of potential employers and reduce their labor supply to firms experiencing financial difficulties.

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worker's effort to acquire skills (or human capital). Second, $p(L)\cdot u\cdot S$ captures compensating differentials for the risk of job loss conditional on the firm's financial distress (Abowd and Ashenfelter, 1981; Agrawal and Matsa, 2013). Third, $-S$ represents the present value of compensation beyond the current period. Last, $e(o)$ represents a wage premium due to short-term excess labor demand caused by a large plant opening. Hence, a reduction in labor search costs (u) driven by a plant opening would decrease the compensating differentials, other things held constant. However, given the result above that a decrease in u leads to an increase in L, which pushes up the compensating differentials, and that the new plant leads to short-term excess labor demand ($e(o)$), the net impact of reduced search costs due to plant opening on equilibrium wages is ambiguous (Wage prediction).

3. Empirical approach: a quasi-natural experiment

I use the opening of a large manufacturing plant in a given county as an abrupt expansion of the local labor market in the sector.[9] I define labor markets at the local level given that workers concentrate their job searches in local areas (e.g., Moretti, 2011; Manning and Petrongolo, 2017). This increase in labor market size reduces the costs of financial distress stemming from frictions associated with workers finding a similar employer, which, all else being equal, leads the existing firms in the local labor market to increase debt usage. One difficulty with the empirical approach I employ is that plant opening decisions are driven by economic forces and thus could be endogenous. For example, a county that ultimately attracts a large plant might have had a faster-growing economy than another county. Then, it is possible that the incumbent firms in the winner county have a larger debt capacity and thus can increase leverage caused by fast economic growth or by an increased supply of credit even in the absence of the new plant.

To avoid these endogeneity concerns, I follow Greenstone and Moretti (2004) and GHM (2010) and hand-collect rankings of potential large manufacturing plant sites from the corporate real estate journal Site Selection.[10] The regular feature titled "Million dollar plants" (MDP)[11] provides information on the site selection process for notable large plants, including the new plant's identity and characteristics and which localities were under consideration. Importantly, the MDP articles identify both (1) the county that succeeded in attracting the new plant (the winner) and (2) the county that was a final candidate but narrowly lost out (the runner-up). I search for an empirical link between labor market size and capital structure by using firms in the runner-up county as a counterfactual of those in the winner county.

Greenstone and Moretti (2004) and GHM (2010) provide evidence that the winner and runner-up counties, as well as plants therein, are comparable, validating the comparison of winner versus runner-up. Although they use a similar data set of large plant opening events, these two papers examine the agglomeration spillover effects on real outcomes for existing plants and local economies, including TFP, wages, and property values. This paper complements them by showing that a plant opening in a local market has an important impact on firm-level financial decisions, particularly its capital structure.

Using information on winner and runner-up counties of new plants to identify a valid counterfactual has several advantages. First, both winner and runner-up counties have survived a nationwide site selection process that usually involves dozens of initial candidates and can take several years.[12] And the runner-up is one of the two or three final candidates to survive this process. It is thus plausible that the runner-up lost narrowly, which is a key identifying assumption for my research design.[13] Therefore, it is reasonable to argue that both counties satisfy most of the important specifications for the new site, such as labor availability, transportation infrastructure, and employee quality of life, all of which are generally unobservable to the econometrician (GHM, 2010). In addition, before the plant opening, the difference in observable characteristics of firms and plants in the winner and runner-up counties, including plant output, its growth rate, and determinants of capital structure, is statistically insignificant, further validating the comparison (see Section 4.2).

Last, focusing the analysis on the manufacturing sector has two important advantages. First, prior research shows that industry- or sector-specific human capital is more important for manufacturing workers. For example, Jacobson et al. (1993) and Couch and Placzek (2010) show that workers who are displaced from a manufacturing industry and subsequently find a job in a nonmanufacturing industry experience a significantly larger earnings loss relative to other displaced workers who move from nonmanufacturing to manufacturing sectors. It is thus likely that a plant opening will lead to greater variation in the cost of job loss in manufacturing. The evidence suggests, moreover, that labor markets are likely segmented between manufacturing and nonmanufacturing, and thus the introduction of a new employer in the manufacturing sector

[9] The size of a labor market depends not only on the overall number of firms and workers but also, crucially, on worker skills. For example, a manual worker at a chemical plant and a clerk in a retail store in an identical city could face quite different labor markets.

[10] The journal has varying titles—Site Selection, Industrial Development, Site Selection & Industrial Development—depending on the year of publication. I use Site Selection for consistency.

[11] The feature title varies ("Million dollar plants," "Million dollar facilities," "Location reports"). I use "Million dollar plants" for consistency.

[12] The case of Tesla Motors illustrates this competition among US localities to attract large plants. See, e.g., "Tesla confirms Nevada to get battery factory," The Wall Street Journal, 09/04/2014. The article reports that sites in Arizona, California, Nevada, New Mexico, and Texas were competing for the new plant before a Nevada site near Reno was finally chosen. It quotes Tesla CEO Elon Musk's comment that the decision between Nevada and the other states was "tight."

[13] Quotations from articles in "Million dollar plants" illustrating this assumption include: "We found the three locations equally suitable" (TRW); "Yamaha officials stressed that any of the four final areas under consideration would have been an excellent location for their new facility" (Yamaha Motors); and "Jacksonville [a runner-up] was certainly a prime candidate for the center. We just had to choose between two excellent candidates" (MCI Communications).

may not significantly affect job search costs for workers in the nonmanufacturing sector. This paper therefore studies only other manufacturing firms' capital structure changes in response to the opening of a manufacturing plant. Second, given that manufacturing industries generally produce nationally (as opposed to locally) tradable goods (Glaeser and Kohlhase, 2004), I can avoid alternative explanations related to local product market competition.

4. Data and descriptive statistics

This section describes the data sets used in the empirical analysis, sample selection procedures, and resulting samples.

4.1. Data sources and sample construction

I hand-collect data on the opening of large manufacturing plants from "Million dollar plants" (MDP) articles from 1980 to 1995 (Site Selection stopped publication of this feature after 1995). The sample period is similar to those used in Greenstone and Moretti (2004) and GHM (2010), respectively, 1982–1993 and 1981–1993. When not available from the journal, the data are supplemented with information from Greenstone and Moretti (2004). The MDP articles provide information on the location (city and county) that the firm chose for the new plant site as well as on one or two runner-up locations considered by the firm. The analysis focuses on the impact of the plant opening on other existing firms in the county.

I first match each new manufacturing plant from Site Selection with a plant in the Census Bureau's Standard Statistical Establishment List (SSEL) and Longitudinal Business Database (LBD) using the parent company name, state, county, opening year, and industry.[14] If a new plant is not matched to a plant in the SSEL or LBD, I drop the case from the sample.[15] Second, I identify all establishments in the winner and runner-up counties that are owned by firms other than the firm opening the new plant by using location information in the two census databases. Third, I match these existing plants to parent Compustat firms in the manufacturing industries (Standard Industrial Classification (SIC) codes 2000–3999) using a bridge file created by the Census Bureau. I obtain firm-level variables for leverage and financial controls from Compustat. My identification strategy relies on the within-event comparability of the winner and runner-up firms, but applying the matching procedure often leads to highly unbalanced numbers of firms between the counties for some events. To avoid potential biases in the estimate, I drop a plant opening event if the ratio of the selected firms in the winner county to those in the winner or runner-up counties is too small or too large: less than 0.05 or larger than 0.95.[16]

This sample selection procedure yields 40 manufacturing plant opening cases from 1980 to 1995, similar to prior research (GHM, 2010).[17] Fig. 1 shows the location of the winner and runner-up counties in these plant opening cases. I define the treatment window as four years before and after a plant opening for each event. I require parent firms in the sample to have at least 3% of their employees located in either the winner or the runner-up county.[18] Last, I require each firm-year in the sample to have key control variables used in the analysis, including book assets, tangibility, market-to-book, and return on assets (ROA), all of which are lagged by one year relative to leverage. This selection procedure yields the final sample of approximately 5900 firm-year observations from 1976 to 1999. In part of the analysis (Online Appendix Table 4), I examine robustness of the main results by employing an extended sample that adds approximately 46,000 firm-years from Compustat that are not affected by those events. Observation numbers are rounded to the nearest hundred or thousand to follow the Census Bureau's disclosure rules.

In addition, I obtain data on plant observations from the Census of Manufacturers (CMF) and the Annual Survey of Manufacturers (ASM) maintained by the Census Bureau. The CMF covers all manufacturing plants with at least one employee for years ending "2" or "7" (census years), including approximately 300,000 plants in each census. The ASM covers about 50,000 plants for noncensus years. Plants with more than 250 employees are always included in the ASM, whereas those with fewer employees are randomly sampled, with the probability increasing in size. Both data sets provide information on plant operation, including total value of shipments, labor hours, and wage bills, which I use to estimate the impact of a plant opening on wages and labor productivity.

I also use the employer-employee matched data from the Census Bureau's LEHD program to examine the implications of local labor market size for the magnitude of displaced workers' earnings loss. The LEHD data sets are based on the state unemployment insurance (UI) records and track individual workers across firms over time, covering about 96% of private sector employment in 30 states. They provide information on earnings, employers, locations, and industries for each employment relation and on such individual characteristics as age and sex from 1985 to 2008. I winsorize all potentially unbounded variables at the 1% and 99% tails.

[14] The plant opening year is recorded as the earliest of the year of publication in Site Selection and the year in which the matched new plant first appears in the SSEL or LBD (GHM, 2010). The locations are recorded in Site Selection mostly at the city level. Given that plant location is available only at the county level in the SSEL and LBD, I convert cities into counties.

[15] The SSEL contains the Census Bureau's most complete data for US business establishments, and the LBD tracks more than five million manufacturing and nonmanufacturing establishments annually, essentially covering the entire US economy.

[16] This issue arises essentially owing to the limited number of public firms that have plants in some of the counties.

[17] I match the events to Compustat data in addition to the census plant-level data sets, whereas GHM (2010) match to the census data sets only.

[18] Section 5.2 examines whether the impact of a plant opening is stronger for firms with a larger fraction of employees in the winner or runner-up counties.

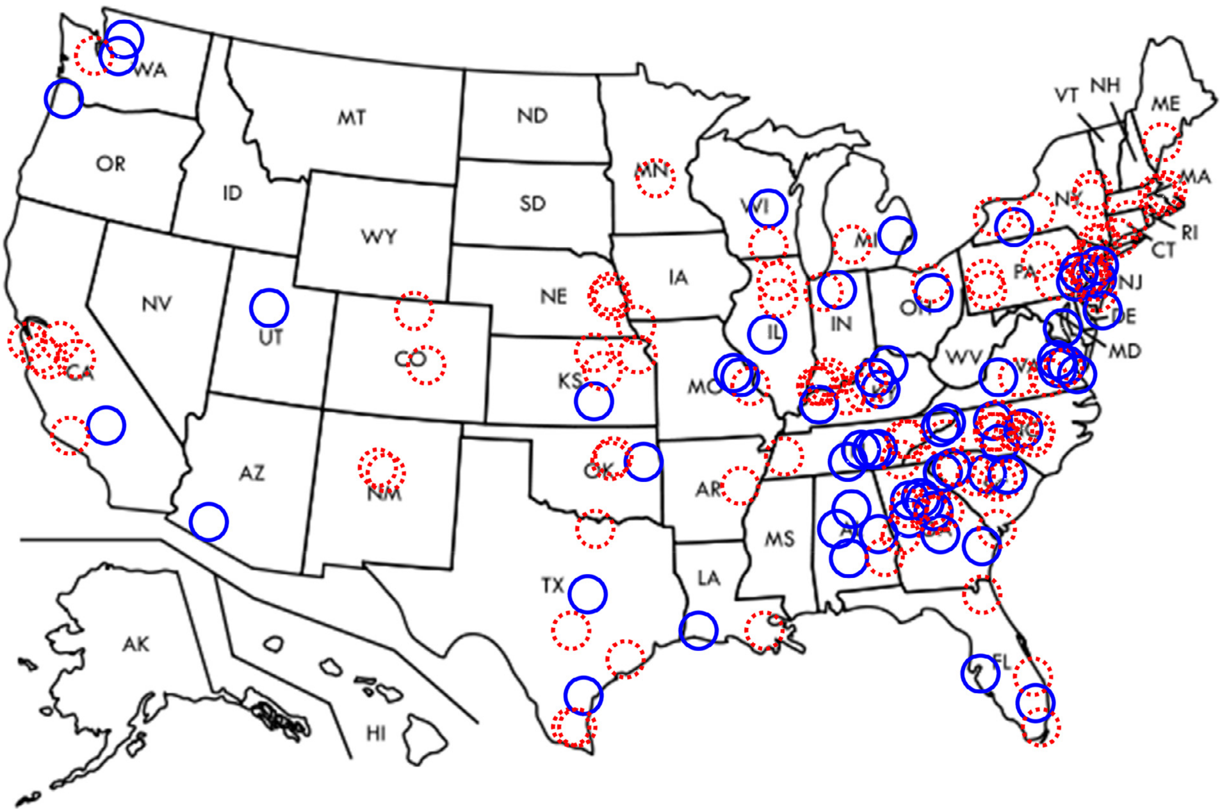

Fig. 1. Location of manufacturing plant opening events.
This figure shows the approximate location of the counties in which new manufacturing plants opened ("winners"; blue solid circles) and the plants' runner-up choices ("runners-up"; red dotted circles). There are 60 events represented in the figure, drawn from issues of Site Selection from 1980 to 1995, among which 40 are matched with the US Census Bureau's establishment-level data sets. A detailed list of the 60 events is available on request. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

4.2. *Descriptive statistics—similarity of winner versus runner-up*

Table 1 provides descriptive statistics on the 40 manufacturing plant opening events used in the analysis. Panel A shows that there are 43 winner and 59 runner-up counties represented, implying that a few cases have more than one winner or runner-up localities. The cases are equally distributed across the first and second halves of the sample period. The distribution of winner and runner-up counties shows that the winners are concentrated in the South and West, while the runner-up counties are more often located in the Northeast and Midwest, consistent with Fig. 1 and GHM (2010).[19] I later examine whether the unbalanced geographical distribution introduces biases into estimates.

Importantly, the average new plant in the sample accounts for 41% of manufacturing labor forces of the winning county. Given the potential time lag between when a plant opens and when it reaches full operating capacity, employment at the new plant is measured four to eight years (whichever year appears first in the ASM or CMF database) after the opening, while the workforce at existing plants is measured one year before the opening (GHM, 2010). The relatively large employment by the new plant suggests that its opening significantly expands the local market for manufacturing jobs and reduces search costs for manufacturing workers in the winner county.

Table 2 shows firm-level characteristics for samples of public (i.e., Compustat) manufacturing firms operating plants in the winner and runner-up counties (columns 1 and 2). All firm characteristics are measured one year before the plant opening. First, the first row shows that these firms have, on average, 25% of their workforces in the winner or counterfactual counties. Second, the comparison of column 1 with column 2 shows that observable firm characteristics, including asset size, market-to-book, and sales growth, are well balanced between the firms in the two groups. In particular, *t*-statistics in column 5 indicate that most of the differences between firms in the winner and runner-up counties are statistically insignificant at a conventional level. Only the differences in leverage and tangibility of assets are marginally significant, but the economic magnitude is small. In Section 5.4, I provide evidence that the baseline result is robust to this pre-event difference in leverage.

This result contrasts with the significant differences between firms in the winner county and those in neither

[19] Census Bureau disclosure rules prevent a more detailed breakdown of the distribution of the winners and runners-up.

Table 1
Summary statistics on manufacturing plant opening events.
This table presents descriptive statistics on the events of manufacturing plant openings drawn from issues of Site Selection that are matched with the US Census Bureau's establishment-level data sets from 1980 to 1995. It shows the numbers of plant opening events and the winner and runner-up counties that could be matched to establishment-level census data for the full sample, by time period, and by census region. Census confidentiality rules prevent more detailed presentation of the distribution of events. *Total employees* is the new plant's employment as an average fraction (and its standard deviation in parentheses) of manufacturing employment of the winner counties as a whole obtained from the ASM and CMF databases. Employment of the new plant is measured four to eight years (whichever year appears first in the data) after the opening, and that of the winner county is measured one year before the opening.

	(1)
Total number of events	40
Total number of winner counties	43
Total number of runner-up counties	59
Distribution of events by year:	
1980–1987	20
1988–1995	20
Distribution of winner counties by region:	
Northeast and Midwest	10
South and West	33
Distribution of runner-up counties by region:	
Northeast and Midwest	25
South and West	34
New plants relative to winner counties:	
Total employees	0.41
	(1.01)

winner nor runner-up counties in column 3. In fact, column 6 shows that the differences between the two groups are significantly different from zero for most variables. Hence, the descriptive analysis illustrates the advantage of using firms in the runner-up counties, rather than all other firms in Compustat, as a control group. In addition, Online Appendix Table 3 shows that the difference in key plant-level characteristics is also statistically insignificant between the winner and runner-up counties before the plant opening, further supporting the identifying assumption that the two counties are similar.

5. Empirical analysis

This section provides baseline estimates for the effect of large plant openings on firm capital structure, examines robustness of the baseline results to potential firm selection in the winner and runner-up counties, explores mechanisms, and verifies a key assumption underlying the research design.

5.1. Baseline results

I estimate the effects of the new manufacturing plant opening, which increases the size of the manufacturing labor market in a county, on the capital structure of incumbent manufacturing firms using the following difference-in-difference approach:

$$Leverage_{ijet} = \alpha_i + \alpha_{jt} + \alpha_e + \beta_1 After_{et} \times Winner_{ie} + \beta_2 After_{et} + \beta_3 Winner_{ie} + \gamma' X_{it-1} + \varepsilon_{ijet}, \quad (5)$$

Table 2
Summary statistics on firm observations.
This table provides descriptive statistics for firm-year observations from Compustat in the manufacturing industries (SIC codes 2000–3999) from 1976 to 1999. Columns 1 and 2 show the means for firms that operate (i.e., have plant(s) and at least 3% of employees) in the winner and runner-up counties one year before the plant opening, respectively, and columns 3 and 4 show the means and standard deviations for all firm-years not located in the winner or runner-up counties. The firms that own the new plants are excluded from both the winner and runner-up groups. *% employees in winner/runner-up counties* represents the fraction of the firm's total workforce located in the winner or runner-up counties; *Leverage* is the debt-to-capital ratio defined as total debt (long-term plus short-term debt) divided by the sum of total debt and book value of equity; *Cash holdings* is cash and equivalents divided by total assets; *Log assets* is log book assets in million dollars; *Tangibility* is net value of plant, property, and equipment divided by total assets; *Market-to-book* is total assets minus book equity plus market equity scaled by total assets; *Return on assets* is operating income before depreciation and amortization scaled by lagged assets; *Labor intensity* is computed as the number of total employees divided by real assets in constant 2000 dollars; *Capex* is capital expenditure scaled by lagged assets; *R&D* is research and development expenses scaled by lagged assets; *Sales growth* is the growth rate of sales. Column 5 (6) shows *t*-statistics for mean differences in variables between the winner and runner-up (all other) firms. *t*-statistics are based on standard errors clustered at the plant opening event level.

	(1)	(2)	(3)	(4)	(5)	(6)
Sample:	Winner	Runner-up	All other firms		(1)–(2)	(1)–(3)
Statistic:	Mean	Mean	Mean	SD	*t*-statistic	*t*-statistic
% employees in winner/runner-up counties	0.25	0.25	–	–	0.20	–
Leverage	0.33	0.36	0.33	0.27	−1.99	0.14
Cash holdings	0.11	0.10	0.15	0.19	1.14	−3.32
Log assets	5.48	5.30	4.56	2.16	0.94	6.66
Tangibility	0.32	0.30	0.28	0.17	1.75	4.41
Market-to-book	1.83	1.66	2.14	2.25	1.41	−2.46
Return on assets	0.14	0.14	0.10	0.23	−0.30	4.52
Labor intensity	8.72	8.91	8.22	6.82	−0.37	1.20
Capex	0.27	0.25	0.35	0.42	0.56	−3.45
R&D	0.04	0.03	0.06	0.11	1.05	−4.79
Sales growth	0.07	0.08	0.10	0.32	−0.50	−1.85
Observations	276	413	44,599	–	–	–

where i indexes firm, j industry, e plant opening event, and t year; α_i is firm fixed effects; α_{jt} is industry-by-year fixed effects; α_e is event fixed effects; and $Leverage_{ijet}$ is the debt-to-capital ratio defined as total debt (long-term plus short-term debt) divided by the sum of book value of equity and total debt.[20] I use the debt-to-capital instead of the debt-to-assets ratio, a commonly used measure of leverage given that the latter essentially treats operating liabilities as "equity" (Welch, 2011).[21] $After_{et}$ is an indicator variable equal to one if the new plant opening has been announced for event e by year t, and zero otherwise, $Winner_{ie}$ is an indicator variable equal to one if firm i in event e operates plants in the winner county, and zero otherwise,[22] X_{it-1} is a set of one-year lagged firm-level control variables, and ε_{ijet} is the residual. In the main specification, I include the plant opening event fixed effects (i.e., indicators for the 40 events) to control for any fixed differences in leverage across events. I also estimate a version of Eq. (5) that interacts event fixed effects with year fixed effects, which controls for year-specific variations in leverage within each event. This specification estimates the pairwise comparison of the winner and runner-up firms within events. The time-varying firm-level control variables include (log) assets, tangibility of assets, market-to-book, and ROA as defined in Table 2 (Rajan and Zingales, 1995). Standard errors are clustered at the plant opening event level.

Table 3, Panel A shows estimation results for Eq. (5). Column 1 presents the baseline difference-in-difference estimates in which firms in the winner and runner-up counties are included. The column excludes the event fixed effects and firm-level controls. The coefficient on *After* × *Winner* shows that the leverage ratios of firms in the winner county increase by 3.09 percentage points after a plant opening relative to the leverage ratios of incumbent firms in the runner-up county.[23] The coefficient is statistically significant at the 1% level. The relative increase in leverage ratio amounts to nearly 10% of the pre-event average leverage ratio (33%).

Column 2 includes the firm-level controls and plant opening event fixed effects. Adding these controls does not significantly alter the coefficient estimate on *After* × *Winner*, which is 2.85 and statistically significant at the 1% level. This result indicates that the effect of a plant opening on leverage is unlikely to be driven by concurrent changes in the firm-level determinants of leverage or heterogeneity across events. To control for time-varying industrywide shocks, the specification in column 3 adds two-digit SIC industry-by-year fixed effects. The coefficient estimates and their statistical significance are very similar to those in column 2, indicating that industry-level shocks are an unlikely driver of the result. Online Appendix Table 4 reports results of estimating Eq. (5) using a sample that includes manufacturing firms not affected by plant openings (approximate $N = 52{,}000$). Across the columns, the results are quantitatively similar to those in Table 3, Panel A.

Panel B of Table 3 reestimates Eq. (5) by including event-by-year fixed effects. This specification controls for year-specific leverage changes for firms in a given MDP event, which enables the coefficient on *After* × *Winner* to be interpreted as leverage changes for firms in a given winner county relative to changes for firms in the associated runner-up counties. Columns 1 through 3 of Panel B, which include the same controls as the corresponding columns in Panel A (other than event-by-year fixed effects), show estimates that are quantitatively similar to those in Panel A. For example, the coefficients on *After* × *Winner* are 3.27 and 3.98 and significant at the 1% and 5% levels in column 3 of Panels A and B. Column 4 of Panel B includes event-by-industry-by-year fixed effects and shows a similar estimate for *After* × *Winner* (3.86).

How big is the economic magnitude of the plant opening effect on leverage ratios? Online Appendix Table 5 compares the impact of a typical plant opening in the sample on the leverage of incumbent firms with the association between a one standard deviation change in other determinants and leverage. The caveat of this calculation is that the estimates on financial characteristics do not permit causal interpretation. Based on the coefficient estimates in Table 3, Panel A, column 2, the table shows that a one standard deviation change in each common determinant of financial leverage is associated with a change in leverage ratio of 1.83 to 6.74 percentage points in absolute value. In comparison, a typical new plant in the sample leads the leverage of firms in the winner county to increase by 2.85 percentage points relative to the runner-up. The magnitude of this effect suggests that it could be comparable to the correlation between a one standard deviation change in log assets, tangibility, or market-to-book and a leverage change.

5.2. Treatment intensity: effects of plant opening conditional on fraction of affected firm employees

Next, I examine whether the magnitude of the effect varies by the fraction of the incumbent firm's employees located in the winner or runner-up counties (i.e., treatment intensity). In particular, the indicator variable *Large* (*Small*) is defined as equal to one if the fraction of workers in the affected counties is larger than (smaller than or equal to) the top 40%, and zero otherwise.[24] The cutoff is relatively high because the fraction is quite small for a bulk of firms. I then estimate the following regression, which augments Eq. (5) with the interactions between the indicator variables *Large* and *Small* and indicators in

[20] I obtain qualitatively similar results using total debt divided by the sum of market value of equity and total debt as an alternative measure of leverage.

[21] On this ground, Welch (2011) recommends using the debt-to-capital ratio as a more appropriate measure of financial leverage than the debt-to-assets ratio.

[22] Eq. (5) estimates the coefficient on the indicator *Winner* as well as firm fixed effects because some firms switch in and out of the winner group across the plant opening events.

[23] In Table 3 and other tables, "NR" represents estimates that are not reported owing to Census Bureau disclosure rules concerning sample size.

[24] The Census Bureau disclosure rules do not permit researchers to disclose percentiles.

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Table 3
Effect of new manufacturing plants on leverage of existing manufacturing firms.
This table presents the effect of the opening of a manufacturing plant on the leverage of existing manufacturing firms (Compustat SIC codes 2000–3999) that operate (i.e., have plant(s) and at least 3% of employees) in the winner county compared to those in the runner-up county from 1976 to 1999. The firms that own the new plants are excluded from both the winner and runner-up groups. The table presents estimates using a sample of firm-years that are in either the winner or runner-up counties. Panel A (B) presents estimates excluding (including) plant opening event-by-year fixed effects. *Leverage (%)* is defined as total debt (long-term plus short-term debt) divided by the sum of total debt and book value of equity in percentage; *After* is an indicator variable equal to one if the firm operates in the winner or runner-up counties after the opening of a manufacturing plant, and zero otherwise; *Winner* is an indicator variable equal to one if the firm operates in the winner county, and zero otherwise. Other control variables are defined in Table 2. "NR" represents estimates that are not reported due to Census Bureau disclosure rules concerning sample size. Standard errors adjusted for sample clustering at the plant opening event level are reported below coefficient estimates in parentheses. Numbers of observations are rounded to the nearest hundred per Census Bureau disclosure rules.

Panel A: Baseline results

	(1)	(2)	(3)
Dependent variable:		Leverage (%)	
After × Winner	3.089	2.852	3.267
	(1.034)	(1.039)	(0.997)
After	NR	NR	NR
	NR	NR	NR
Winner	−2.489	−2.404	−2.658
	(1.011)	(1.045)	(0.944)
log(assets)	–	1.411	0.856
	–	(1.167)	(0.991)
Tangibility	–	22.770	21.780
	–	(5.593)	(5.694)
Market-to-book	–	0.815	0.666
	–	(0.591)	(0.677)
ROA	–	−29.290	−25.370
	–	(3.309)	(3.353)
Firm fixed effects	Y	Y	Y
Year fixed effects	Y	Y	
Event fixed effects		Y	Y
Industry × year fixed effects			Y
Observations	5900	5900	5900
R^2	0.7314	0.7436	0.7791

Panel B: Using event-by-year fixed effects

	(1)	(2)	(3)	(4)
Dependent variable:		Leverage (%)		
After × Winner	3.415	3.329	3.977	3.860
	(1.570)	(1.544)	(1.388)	(2.037)
Winner	−2.635	−2.643	−3.058	−4.847
	(1.232)	(1.245)	(1.097)	(2.831)
Firm-level controls		Y	Y	Y
Firm fixed effects	Y	Y	Y	Y
Event × year fixed effects	Y	Y	Y	
Industry × year fixed effects			Y	
Event × industry × year fixed effects				Y
Observations	5900	5900	5900	5900
R^2	0.7413	0.7535	0.7891	0.8716

Eq. (5):

$$\begin{aligned} Leverage_{iet} = \alpha_i + \alpha_t + \alpha_e + (\beta_1 After_{et} \times Winner_{ie} \\ + \beta_2 After_{et} + \beta_3 Winner_{ie}) \times Large_{ie} \\ + (\beta_4 After_{et} \times Winner_{ie} + \beta_5 After_{et} \\ + \beta_6 Winner_{ie}) \times Small_{ie} + \gamma' X_{it-1} + \varepsilon_{iet}. \quad (6) \end{aligned}$$

Essentially, this specification separately estimates the effect of a plant opening on leverage for firms with relatively high and low treatment intensities. Table 4 shows that the effect of a plant opening is indeed statistically and economically significant for firms with a large fraction of their employees in the affected counties, whereas the effect is insignificant for firms with a small fraction of their labor in the counties. The first row shows that the estimate of *After* × *Winner* × *Large* is 4.59 and statistically significant at the 1% level. The estimate of *After* × *Winner* × *Small* is 0.30 and statistically insignificant. The difference between these two coefficients is significant at the 10% level (t-stat = 1.83).

5.3. Dynamic effects of plant opening on firm leverage

I estimate the dynamic effects of a manufacturing plant opening on the capital structure of existing manufacturing

Table 4
Fraction of firm employees in affected counties and effect of new manufacturing plants on leverage of existing manufacturing firms.
This table presents the effect of the opening of a manufacturing plant on the leverage of existing manufacturing firms that operate plants in the winner versus runner-up counties conditional on the fraction of the existing firm's employees located in the winner or runner-up counties. The sample is the same as that in Table 3. *Leverage (%)* is defined as total debt (long-term plus short-term debt) divided by the sum of total debt and book value of equity in percentage. *Large* (*Small*) is an indicator variable equal to one if the fraction of employees in the affected (i.e., winner or runner-up) counties is larger than (smaller than or equal to) the top 40%, and zero otherwise. Other independent variables are defined in Table 3. Standard errors adjusted for sample clustering at the plant opening event level are reported below coefficient estimates in parentheses. The number of observations is rounded to the nearest hundred per Census Bureau disclosure rules.

	(1)	(2)
Fraction of employees:	Large	Small
Dependent variable:	Leverage (%)	
After × Winner	4.592	0.304
	(1.673)	(1.379)
After	−2.000	0.542
	(1.061)	(1.129)
Winner	−1.860	−2.687
	(1.519)	(1.213)
Firm-level controls	Y	
Firm fixed effect	Y	
Year fixed effects	Y	
Event fixed effects	Y	
Observations	5900	
R^2	0.7444	
After × Winner × (Large − Small)	4.288	
t-statistic	1.83	

Table 5
Dynamic effect of new plants on leverage of existing manufacturing firms.
This table presents the dynamic effect of the opening of a manufacturing plant on the leverage of existing manufacturing firms that operate plants in the winner versus runner-up counties. The sample is the same as that in Table 3. *Leverage (%)* is defined as total debt (long-term plus short-term debt) divided by the sum of total debt and book value of equity in percentage. *d[t + k]*, −4 ≤ *k* ≤ 4, is an indicator variable equal to one if the firm is in either the winner or runner-up counties from four years before and after the new plant opening, and zero otherwise. *d[t−1]* is zero by construction. Column 1 (2) shows the coefficients on *d[t + k]* × *Winner* (*d[t + k]* × *Runner-up*), −4 ≤ *k* ≤ 4, and column 3 shows the difference between columns 1 and 2. Standard errors adjusted for sample clustering at the plant opening event level are reported below coefficient estimates in parentheses. The number of observations is rounded to the nearest hundred per Census Bureau disclosure rules.

	(1)	(2)	(3)
Dependent variable:		Leverage (%)	
Coefficient:	Winner	Runner-up	[Winner − Runner-up]
d[t−4]	−0.778	−0.604	−0.174
	(0.997)	(0.804)	(1.148)
d[t−3]	−0.651	−0.638	−0.013
	(1.011)	(0.664)	(1.087)
d[t−2]	−0.620	−0.109	−0.512
	(0.765)	(0.672)	(1.015)
d[t−1]	0.000	0.000	0.000
	–	–	–
d[t]	0.436	−0.163	0.599
	(0.448)	(0.536)	(0.669)
d[*t* + 1]	2.401	−1.340	3.740
	(0.756)	(0.656)	(1.006)
d[*t* + 2]	2.258	−0.763	3.021
	(0.939)	(0.649)	(1.159)
d[*t* + 3]	1.385	−1.100	2.485
	(0.919)	(0.877)	(1.276)
d[*t* + 4]	2.551	−1.382	3.933
	(1.332)	(0.896)	(1.631)
Firm-level controls		Y	
Firm fixed effect		Y	
Year fixed effects		Y	
Event fixed effects		Y	
Observations		5900	
R^2		0.7441	

firms using the following specification:

$$\begin{aligned} Leverage_{iet} = {} & \alpha_i + \alpha_t + \alpha_e + \sum_{k=-4}^{-2} \beta_k^W Winner_{ie} \times d[t+k]_{et} \\ & + \sum_{k=0}^{4} \beta_k^W Winner_{ie} \times d[t+k]_{et} \\ & + \sum_{k=-4}^{-2} \beta_k^R Runner-up_{ie} \times d[t+k]_{et} \\ & + \sum_{k=0}^{4} \beta_k^R Runner-up_{ie} \times d[t+k]_{et} \\ & + \beta^W Winner_{ie} + \gamma' X_{it-1} + \varepsilon_{iet} \end{aligned} \tag{7}$$

This specification is similar to that in Eq. (5) except that the indicator variable *After* is replaced with the eight indicator variables *d[t + k]*, $-4 \le k \le -2$ or $0 \le k \le 4$, which are equal to one for firm *i* that operates in the winner or runner-up counties in four years before and after a new plant opening.[25]

Table 5 shows the results of estimating Eq. (7). All estimates are from one regression equation, but the coefficients on the event time indicators interacted with *Winner* and *Runner-up* are presented separately in columns 1 and 2, and column 3 shows the differences. The coefficients on *Winner* × *d[t + k]* ($-4 \le k \le -2$) show that there is no significant pattern of leverage for the winner firms before the plant opening ("year *t*-1" is the baseline year, and thus d[t - 1] is equal to zero by construction; see also GHM, 2010, Table 4). Similarly, the coefficients on *Runner-up* × *d[t + k]* ($-4 \le k \le -2$) show an insignificant change for the runner-up firms before the plant opening. In column 3, I cannot reject the null hypothesis that each of the differences between the coefficients on *Winner* × *d[t + k]* and *Runner-up* × *d[t + k]* ($-4 \le k \le -2$) is equal to zero. These estimates suggest that the trends in leverage ratios of the winner and runner-up firms were statistically not different from each other before the plant decided to open. This evidence lends credibility to the identifying assumption that firms operating plants in the two counties are similar ex ante.

In contrast, the coefficients on *Winner* × *d[t + k]* ($0 \le k \le 4$) show that the leverage ratios of winner firms begin to increase from the year of the plant opening ("year *t*"). For example, one year after the plant opening,

[25] Note that an indicator for "year *t*-1" is omitted in the estimation, and all event time indicators thus represent leverage ratios relative to one year before the plant opening.

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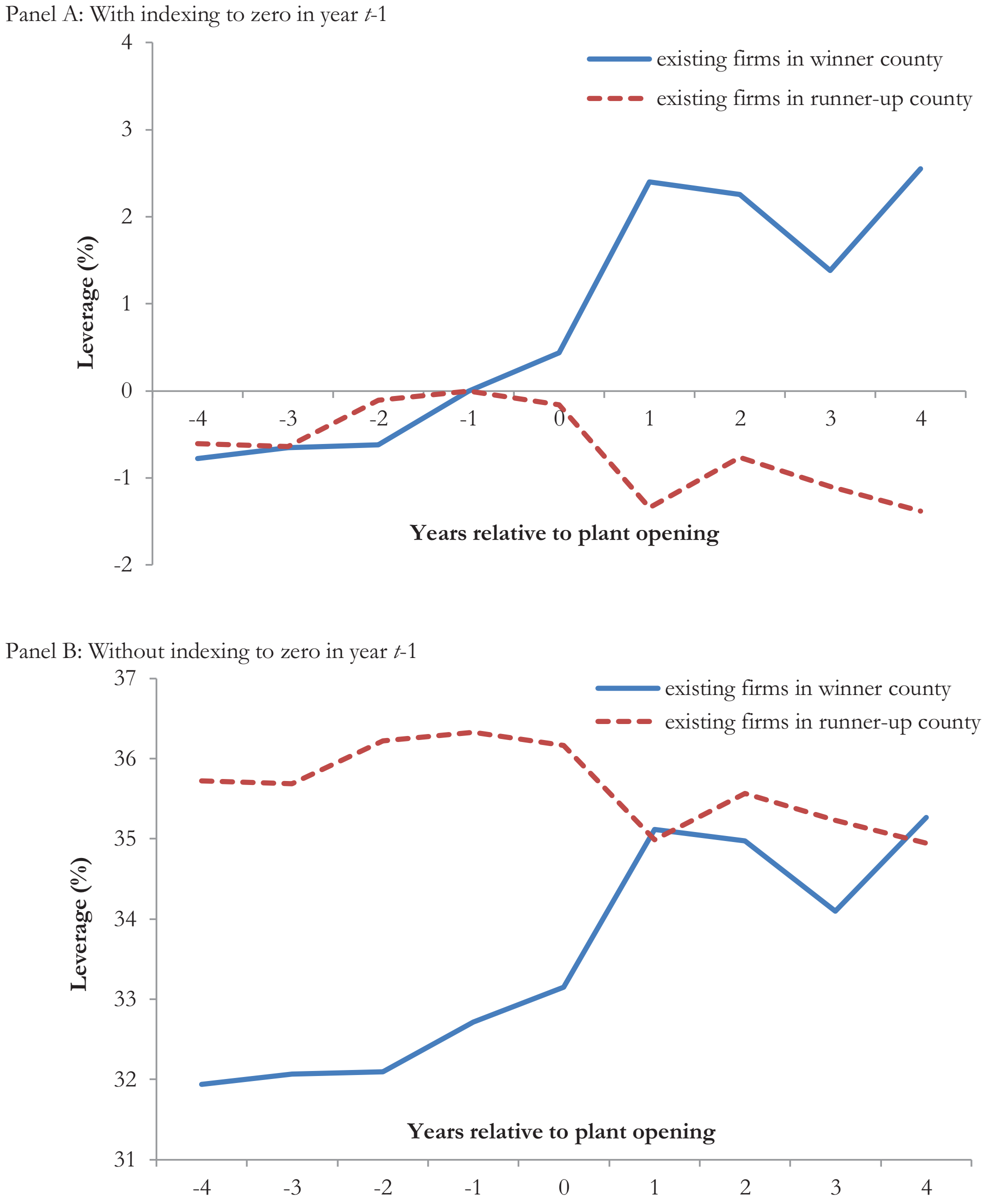

Fig. 2. Dynamic effects of manufacturing plant opening on leverage of existing manufacturing firms.
This figure shows the dynamics of leverage ratios for firms operating in the counties in which new manufacturing plants opened ("winner") and firms operating in the counties that were top candidates for the plant sites but lost the competition ("runner-up") from four years before and after the plant opening. The firms that are the owners of the new plants are excluded from both the winner and runner-up groups. Panel A (B) presents the dynamics for the winner and runner-up groups with (without) indexing the graphs to zero in year *t*-1.

the leverage of the winners increases by 2.40 percentage points, on average, compared to one year before the opening (significant at the 1% level). The coefficients on *(Winner – Runner-up)* $\times$ *d[t + k]* ($1 \leq k \leq 4$) show that the leverage ratios of the winners increase significantly relative to those of the runners-up after the plant opening. Fig. 2 depicts the dynamics of leverage based on the estimates in Table 5. Panel A, which presents the Table 5 estimates (with indexing leverage to zero in year *t*-1), shows that there is no pre-trend in leverage between firms in the winner and runner-up counties. Panel B shows the dynamics for firms in the winner and runner-up counties without indexing the leverage ratio to zero in year *t*-1. The panel shows that the effect of a new plant opening represents the closing of a preexisting difference between the winner and runner-up counties.

5.4. Alternative explanations: selection and reversion to mean

Results in Sections 5.1 through 5.3 suggest that a large plant opening leads to a sizable increase in leverage of existing firms, consistent with the prediction that these firms use more debt given a reduced cost associated with it. One concern with the baseline results is that they could be due to a pre-event difference in leverage between firms in the winner and runner-up counties. In particular, the leverage ratio of firms in the winner counties is about 3 percentage points lower than that in runner-up counties (see Table 2), suggesting that selection (on leverage or other correlated, unobservable characteristics) or mean reversion in leverage might explain the relative increase in leverage for the winner firms after the plant opening.

I address this concern empirically using two approaches. The first uses a subset of MDP events for which the average difference in leverage between the winner and runner-up groups is less than 10% in absolute value one year before a plant opening. The difference in leverage ratios is not statistically significant between firms in these winner and runner-up counties (difference = −0.59; *t*-stat = −0.53). Using this subset of events, I reestimate Eq. (5) and report the results in Table 6, Panel A. Across the specifications, I find quantitatively similar results to those in Table 3, which uses the full sample of MDP events. For example, the coefficient estimates for *After* × *Winner* are 2.95 and significant at the 1% level in column 2 of Table 3, Panel A, and 2.38 and significant at the 10% level in column 2 of Table 6, Panel A. This similar magnitude of the effect alleviates concerns regarding selection or reversion to the mean as alternative explanations for the baseline results.

In the second approach, for a given firm in the winner county, I find a matched firm in the runner-up county with a leverage ratio that is closest to that of the winner firm one year before the plant opening. I find that pre-event, firms in the winner counties and the matched firms in the runner-up counties have leverage ratios that are statistically not different from each other, with a difference of −1.09 and an associated *t*-statistic of −0.72. I reestimate Eq. (5) using this matched sample and report the estimation results in Panel B of Table 6. Across the columns, the coefficient on *After* × *Winner* ranges from 3.66 to 4.23 and is significant at the 5% to 10% level. Again, the finding based on a sample matched on pre-event leverage indicates that the baseline results are unlikely due to a preexisting difference in leverage between the treated and control groups.

5.5. *Mechanisms for the effects*

This section explores the mechanisms that drive the effects of large plant openings on firm leverage.

5.5.1. *Labor market size versus other mechanisms of agglomeration*

Previous research on labor mobility and specific human capital shows that some skills are not transferrable across different employers (e.g., Jacobson et al., 1993; Lazear, 2009). Thus, if the effects I find are driven by an expansion of the local labor market, a plant opening would have a larger impact on existing firms that employ workers who have similar skills with the new plant.[26]

To test this prediction, I measure the similarity of labor skills between two industries using movements of workers. Specifically, for each ordered pair of two-digit SIC industries within manufacturing, I compute the fraction of worker flows from a job in one industry to another conditional on a job displacement (see Section 5.6) using the LEHD data (for a similar approach, see GHM, 2010; and Tate and Yang, 2016). This fraction is assigned to an existing plant in the winner or runner-up county (“origin”)-new plant (“destination”) pair based on their two-digit SIC industries. Using this measure, I aim to capture the potential ability of existing plants' workers to move to the new plant. The firm's plants located outside the winner or runner-up counties are assigned zero.

Then, for a given existing firm, the weighted average fraction of worker flows is computed using the employment of plants owned by the firm divided by the firm's total employment as the weight. I estimate the effect of a plant opening separately for firms with the weighted average fraction of worker movements above and below or equal to the top 40% of the distribution.[27] Table 7, columns 1 and 2 show that the effect on leverage is significant only when workers could potentially move from the industries of the plants owned by existing firms in the affected county to the new plant (*After* × *Winner* = 5.84; *t*-stat = 2.84). The difference between firms with relatively high versus low levels of labor flows is significant at the 10% level (*t*-stat = 1.97).

The finding that a new plant affects the capital structure of local firms that share similar workers in the affected county is consistent with the role of the “thick labor markets” channel as a micro-mechanism of agglomeration economies.[28] Moreover, this paper documents for the first time that increased debt usage in large labor markets is another benefit of firm clustering due to, for example, improved incentives for managers (Harris and Raviv, 1990) and interest tax deductions (Graham, 2000). However, given that the measure of similarity of labor skills could be correlated with other relations between the industries of the new plant and existing firms, I interpret this result with caution.

One related concern is that other mechanisms of agglomeration spillovers could drive the increase in leverage. In particular, the agglomeration economies literature identifies two main mechanisms other than labor market size: proximity to suppliers and buyers and knowledge spillovers (e.g., GHM, 2010; Moretti, 2011). For example, if the new plant is a buyer of intermediate goods produced by existing firms, then their products will become less unique after the plant opening, which could lead to higher optimal leverage (e.g., Titman, 1984). I examine whether the effect of the plant opening is more pronounced when

[26] In the model in Section 2, a plant opening represents a decrease in the parameter u for firms that use similar skills with the new plant.

[27] A relatively higher cutoff is preferred to the median given that the fraction approaches zero for a bulk of firms.

[28] See Glaeser and Gottlieb (2009) for a review of the literature on agglomeration economies.

Table 6
Robustness of baseline results to preexisting difference in leverage.
This table presents the effect of the opening of a manufacturing plant on the leverage of existing manufacturing firms that operate plants in the winner versus runner-up counties using a subset of firms that have similar leverage between the winner and runner-up counties before the plant opening. The firms that are the owners of the new plants are excluded from both the winner and runner-up groups. The sample is the same as that in Table 3, except the following. Panel A presents estimates excluding MDP events in which the difference in leverage ratios between firms in the winner and runner-up counties one year before a plant opening is greater than 10% in absolute value. Panel B presents estimates using a matched sample in which, for each firm in the winner county, a firm with the closest leverage ratio in the associated runner-up county before a plant opening is matched. All variables are defined in Table 3. “NR” represents estimates that are not reported due to Census Bureau disclosure rules concerning sample size. Standard errors adjusted for sample clustering at the plant opening event level are reported below coefficient estimates in parentheses. Numbers of observations are rounded to the nearest hundred per Census Bureau disclosure rules.

Panel A: Excluding MDP events with large pre-event difference in leverage

	(1)	(2)	(3)	(4)
Dependent variable:	Leverage (%)			
After × Winner	2.832	2.377	2.293	2.125
	(1.225)	(1.216)	(1.108)	(1.758)
After	−1.882	−1.533	−1.236	–
	(0.685)	(0.718)	(0.616)	–
Winner	−0.289	0.370	0.034	0.444
	(1.287)	(1.142)	(1.063)	(1.284)
Firm-level controls		Y	Y	Y
Firm fixed effects	Y	Y	Y	Y
Year fixed effects	Y	Y		
Event fixed effects		Y	Y	
Industry × year fixed effects			Y	
Event × year fixed effects				Y
Observations	4000	4000	4000	4000
R^2	0.7393	0.7541	0.7950	0.7621

Panel B: One-to-one matched sample on pre-event leverage

	(1)	(2)	(3)	(4)
Dependent variable:	Leverage (%)			
After × Winner	4.229	3.662	4.006	3.776
	(2.313)	(2.138)	(2.110)	(2.121)
After	NR	NR	NR	–
	NR	NR	NR	–
Winner	−1.774	−1.012	−0.933	−1.066
	(1.755)	(1.404)	(1.612)	(1.440)
Firm-level controls		Y	Y	Y
Firm fixed effects	Y	Y	Y	Y
Year fixed effects	Y	Y		
Event fixed effects		Y	Y	
Industry × year fixed effects			Y	
Event × year fixed effects				Y
Observations	4500	4500	4500	4500
R^2	0.7245	0.7457	0.8037	0.7757

the existing firms and new plant are in industries that (1) ship goods to each other or (2) share similar technologies. Specifically, I follow Ellison et al. (2010) and calculate (1) the input–output relation between every pair of three-digit SIC industries using the Bureau of Economic Analysis (BEA) 1987 Benchmark Input–Output Accounts and (2) the extent to which technologies are related between every pair of three-digit SIC industries using the NBER Patent Database. If the observed increase in leverage is caused by these two mechanisms, the estimates would be more pronounced when the new plant-existing firm pair is more “connected” in these dimensions in the winner county. Inconsistent with these channels, however, neither input–output relation nor technological similarity is significantly associated with the effect of the plant opening on leverage (Table 7, columns 3–6). The economic magnitudes of the effect are similar between firms that have relatively high and low degrees of similarities with the new plant on these dimensions, and the differences are statistically insignificant.

Another concern for the finding that the effect is stronger for firms that employ similar labor with the new plant in the affected local labor market is that those firms share similar assets. If so, a large new plant could increase those firms’ asset redeployability, which in turn would increase their collateral value and leverage. I address this alternative mechanism by constructing a measure of asset similarity between every pair of industries. Specifically, I use the BEA capital flow table, which shows 180 categories of physical assets (i.e., plant, property, and equipment) used by 123 BEA industries. For each of the 123 × 123 industry pairs, I compute the “asset similarity score” as the

Table 7
Labor market size versus other mechanisms of agglomeration spillovers.
This table presents heterogeneous effects of plant openings on the leverage ratios of existing manufacturing firms that operate plants in the winner versus runner-up counties, conditional on how the industries of the new and existing plants in the affected (i.e., winner or runner-up) counties are related. The sample is the same as that in Table 3. Columns 1 and 2 present the effect conditional on the frequency of worker flows from the two-digit SIC manufacturing industries of the existing to new plants in the affected counties, measured using the LEHD data. Columns 3 and 4 present the effect conditional on the extent to which the three-digit SIC industries of the new and existing plants in the affected counties buy (input) or sell (output) goods with each other. Columns 5 and 6 present the effect conditional on the extent to which the three-digit SIC industries of the new and existing plants in the affected counties cite patents of each other. Columns 7 and 8 present the effect conditional on whether the Bureau of Economic Analysis (BEA) industries of the new and existing plants in the affected counties use similar assets with each other, measured using the BEA capital flow table. Groups are sorted at the top 40% of each variable. All other variables are defined in Table 3. Standard errors adjusted for sample clustering at the plant opening event level are reported below coefficient estimates in parentheses. Numbers of observations are rounded to the nearest hundred per Census Bureau disclosure rules.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Group:	High labor flow	Low labor flow	High input-output	Low input-output	High citation	Low citation	High asset similarity	Low asset similarity
Dependent variable:	Leverage (%)							
After × Winner	5.843	0.825	3.146	2.644	3.385	2.544	3.477	2.355
	(2.061)	(1.241)	(1.519)	(1.196)	(1.869)	(1.179)	(1.588)	(1.333)
Firm-level controls	Y		Y		Y		Y	
Firm fixed effect	Y		Y		Y		Y	
Year fixed effects	Y		Y		Y		Y	
Event fixed effects	Y		Y		Y		Y	
Observations	5900		5900		5900		5900	
R^2	0.7445		0.7448		0.7439		0.7439	
After × Winner × (High − Low)	5.019		0.502		0.841		1.122	
t-statistic	1.97		0.28		0.39		0.56	

fraction of asset categories that are used in both industries.[29] This score is assigned to a new plant-existing plant pair based on their BEA industries, in concordance with SIC industries. Again, the firm's plants located outside the winner or runner-up counties are assigned zero scores. Then, for a given existing firm, I compute the weighted average fraction of assets commonly used with the new plant's industry. Table 7, columns 7 and 8 show that when firms are sorted on this measure, those with a high similarity of assets with the new MDP in the local market do not experience a significantly larger increase in leverage relative to those with low similarity (*t*-statistic for the difference = 0.56). This result suggests that similarity of assets and particularly a collateral channel do not account for the observed increase in leverage.[30]

5.5.2. How does labor market size affect costs of financial distress?

The results in the previous sections are consistent with the argument that an increase in labor market size reduces the cost of financial distress, which in turn increases the optimal leverage ratio. What might drive this cost of financial distress? The conceptual framework in Section 2 suggests that the compensation premium for the risk of job loss is the cost of financial distress, affecting leverage. For example, when a worker has invested in specific skills, the worker would require a premium for earnings loss risk if the firm is highly levered (Titman, 1984; Agrawal and Matsa, 2013). Given that a large market reduces this risk (see Section 5.6), the wage premium will decrease after a plant opening. After the plant opening, however, firms increase leverage, which tends to increase wage premia (Graham et al., 2019). The net effect of market size on wages, therefore, is ambiguous and ultimately an empirical question (see Eq. (3)).

To examine this question empirically, I estimate the effect of a plant opening on wages and labor productivity using the ASM and CMF databases and the following difference-in-difference approach:

$$Outcome_{ijet} = \alpha_i + \alpha_{jt} + \alpha_e + \beta_1 After_{et} \times Winner_{ie} + \beta_2 After_{et} + \gamma' X_{it} + \varepsilon_{ijet}, \tag{8}$$

where α_i is plant fixed effects; α_{jt} is three-digit SIC industry-by-year fixed effects; α_e is plant opening event fixed effects; $Outcome_{ijet}$ includes the average annual pay of workers, per hour wages, and labor productivity, defined as output scaled by total work hours, and all indicators are defined as in Eq. (5);[31] X_{it} is a set of plant-level control variables, including the log numbers of plants in a given firm and segment in the firm; and ε_{ijet} is the residual for plant i in industry j, event e, and year t. Plant-year observations from four years before to five after a large plant

[29] For example, if there are only two categories of assets (1 and 2) and both industries A and B use asset 1 but only industry A uses asset 2, the asset similarity score would be 0.5 for industry pair (A, B).

[30] Another heterogeneity prediction from the model in Section 2 is that firms with higher marginal tax benefits of debt (higher $T'(L)$ in Eq. (4)) would lever up more in response to a reduction in the costs of financial distress (a decrease in u). Consistent with this prediction, I find that firms with marginal tax rates (MTRs) equal to the statutory rate increase leverage ratios by 3.69 percentage points (significant at the 1% level), while the effect is only 1.06 percentage points and insignificant for firms with lower MTRs. However, the difference is not statistically significant (*t*-stat = 1.36).

[31] Eq. (8) does not include the *Winner* indicator because it is collinear with plant fixed effects.

Table 8
Labor market size, wages, and labor productivity.
This table presents the effect of a new manufacturing plant opening on wages and productivity of workers at existing plants in the winner relative to runner-up counties. The new plants are excluded from both the winner and runner-up groups. The sample includes plant-year observations from four years before to five after the new plant opening. The plants are required to exist in the ASM and CMF databases for at least eight years before the new plant opening. All dependent variables are in log scale. *Average annual pay* is computed as total wage bills divided by the number of total employees; *Wage per hour* is total production worker wage bills divided by total production labor hours; *Labor productivity* is defined as output divided by total labor hours; *Plant per segment* is the number of plants in a given three-digit SIC industry segment of a given firm; *Plant per firm* is the total number of plants of a given firm. All other variables are defined in Table 3. Standard errors adjusted for sample clustering at the plant opening event level are reported below coefficient estimates in parentheses. Numbers of observations are rounded to the nearest thousand per Census Bureau disclosure rules.

	(1)	(2)	(3)
Sample:		Plants	
Dependent variable:	Average annual pay	Wage per hour	Labor productivity
After × Winner	−0.001	0.003	0.032
	(0.009)	(0.011)	(0.016)
After	−0.007	0.006	−0.023
	(0.006)	(0.005)	(0.01)
log(plant per firm)	0.007	0.015	0.014
	(0.004)	(0.005)	(0.009)
log(plant per segment)	0.002	0.006	0.007
	(0.003)	(0.005)	(0.014)
Plant fixed effects	Y	Y	Y
Industry × Year fixed effects	Y	Y	Y
Event fixed effect	Y	Y	Y
Observations	19,000	19,000	19,000
R^2	0.8211	0.7996	0.9169

opening are included. I require that the plants exist in the ASM and CMF databases for at least eight years before the large plant opening (GHM, 2010). Standard errors are clustered at the plant opening event level.

Table 8 presents estimation results for Eq. (8). Columns 1 and 2 show that after a plant opening, there is no significant change in wages at the plants in winner versus runner-up counties. In contrast, column 3 shows that labor productivity increases significantly by 3.2% at existing plants in the winner county relative to the runner-up county. This insignificant wage change, combined with significant productivity gains, is consistent with the scenario that various forces driving wages are offsetting one another (see Section 2). On the one hand, workers would require smaller compensating differentials for earnings loss risk in a larger market, which decreases wages. On the other hand, firms increase leverage after a labor market expands, which in turn increases wage premia. In addition, short-term excess labor demand due to a plant opening would increase equilibrium wages. Although not explicitly modeled in this paper, a larger labor market could also incentivize workers to invest in skills and be more productive (Rotemberg and Saloner, 2000; Almazan et al., 2007). Online Appendix C discusses how these estimates for the wage effect of an MDP can be reconciled with those from GHM (2010).

5.6. Labor market size and earnings loss of displaced workers

This section tests an important assumption underlying the paper's research design: the larger the local labor market, the smaller the earnings losses experienced by workers after a job loss due to reduced search costs. Following the literature (e.g., Gibbons and Katz, 1991; von Wachter, Song, and Manchester, 2011), I define a job displacement as a long-tenure male worker's change in employer in "mass layoffs" due to (near) plant closings, which are identified using the LBD.[32] Consistent with the earlier analysis of plant opening events, I focus on a sample of workers displaced from manufacturing plants located in the winner or runner-up counties (see Table 1) from 1991 to 2005 and use manufacturing employment in a given county from the LBD as a proxy for labor market size (see, e.g., Petrongolo and Pissarides, 2006; Harmon, 2015). Using this sample of displaced workers allows me to link the economic magnitudes of earnings loss estimates from this analysis to those of corporate leverage changes from the earlier analysis (see Online Appendix D).

I obtain worker-level earnings and characteristics information from the LEHD data and construct a control group of workers by choosing a 1% random sample of the LEHD male workers in manufacturing who are not displaced (Jacobson et al., 1993). I require that both displaced and nondisplaced workers have six or more years of tenure one year before a displacement (or random selection). I estimate the earnings dynamics around the year of

[32] The crosswalk between the Census LEHD and LBD databases is only available at the firm-four-digit SIC industry-county level, making it infeasible to exactly match individual workers from the LEHD to plants in the LBD. Thus, I define (near) plant closures as instances in which a firm's employment in a given four-digit SIC industry-county declines by at least 70% due to plant closure. Job losses initiated by a plant closing are considered exogenous in that workers' abilities or skills are much less likely to affect firing decisions in mass layoffs (Gibbons and Katz, 1991).

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job displacement relative to nondisplaced workers as follows:

$$\log(earn)_{ijt} = \alpha_i + \alpha_{jt} + \sum_{k=-3}^{5} d[t+k]_{it}\delta_k + \sum_{k=-3}^{5} d[t+k]_{it} \times DS_i\theta_k + \sum_{k=-3}^{5} d[t+k]_{it} \times DS_i \times Market\ size_i\ \beta_k + \gamma' X_{it} + \varepsilon_{ijt}, \quad (9)$$

where α_i is worker fixed effects; α_{jt} is two-digit SIC industry-by-year fixed effects; $\log(earn)_{ijt}$ is log annual real earnings (adjusted using the consumer price index); $d[t+k]_{it}$, $-3 \le k \le 5$ is an indicator variable equal to one if worker i is in three years before to five years after a job displacement (or, for the control group of workers, it is an indicator variable equal to one if the worker is in three years before to five years after a randomly selected year), and zero otherwise; DS_i is an indicator variable equal to one if worker i is displaced from a job, and zero otherwise; *Market size*$_i$ represents the log number of workers in the manufacturing sector in worker i's county measured in the event year; X_{it} is an interaction term between education and age; and ε_{ijt} is the residual for worker i in industry j and year t.[33] Standard errors are adjusted for sample clustering at the (predisplacement) county level. The sample includes worker observations from four years before to five years after a job displacement (or random selection) to estimate Eq. (9). Thus, given that the event indicator variables begin in t-3, earnings in t-4 are used as an implicit benchmark.

Table 9 shows that in an average local labor market (*Market size* = 10.09), in the year of and one year after job displacement, the earnings loss for displaced workers amounts to 44.2% (= 1 − exp(−0.583)) and 12.8% of their predisplacement annual earnings, measured four years before the displacement. These magnitudes are comparable with previous research (e.g., Jacobson et al., 1993; Couch and Placzek, 2010). Using a real discount rate of 5.07% (the average BBB-rated bond yield minus inflation, 1991–2008), the present value (PV) of earnings losses from the year of displacement to five years afterward amounts to 47.3% of predisplacement annual earnings. Importantly, the positive estimates for *d[t + k]* × *DS* × *Market size* ($1 \le k \le 5$) show that the earnings loss is smaller when the relevant local labor market is larger (though the coefficient is not statistically significant for $k = 3$). A one standard deviation (0.581) increase in local market size leads to a 40.8 percentage point reduction in PV earnings losses for displaced workers from the year of a job displacement to year $t + 5$. Interestingly, the positive estimates for *d[t + k]* × *DS* × *Market size* ($-2 \le k \le -1$) indicate that workers who are ultimately displaced in larger local labor markets have higher earnings relative to those displaced in smaller markets, even before a job displacement, which might be due to selection.

[33] I do not include education, age, or market size as standalone variables because they are collinear with worker fixed effects and (industry ×) year fixed effects.

Table 9
Labor market size and displaced workers' earnings loss.
This table presents earnings patterns of male displaced workers around mass layoffs due to (near) closure of manufacturing plants located in the winner or runner-up counties (see Table 1) from 1991 to 2005. (Near) plant closures are identified using the LBD as instances in which a firm's employment in a given four-digit SIC industry-county declines by at least 70% due to plant closure. A control group of workers includes a 1% random sample of the LEHD male, nondisplaced workers in manufacturing. Both displaced and nondisplaced workers are required to have six or more years of tenure one year before a displacement. The dependent variable is log real annual earnings (adjusted using the consumer price index) from the LEHD. *DS* is an indicator variable equal to one for workers who are displaced in a mass layoff, and zero for randomly selected control workers; *Market size* is the log number of workers in a given manufacturing sector and county from the LBD; *Time-varying control* includes the interaction between education and age from the LEHD. "NR" represents estimates that are not reported due to Census Bureau disclosure rules concerning sample size. Standard errors adjusted for sample clustering at the pre-displacement county level are reported below coefficient estimates in parentheses. The number of observations is rounded to the nearest thousand per Census Bureau disclosure rules.

Dependent variable:	(1) log(earnings)
d[t−3] × DS	NR
	NR
d[t−2] × DS	−0.523
	(0.302)
d[t−1] × DS	−0.146
	(0.351)
d[t] × DS	−1.296
	(0.814)
d[t + 1] × DS	−2.065
	(0.837)
d[t + 2] × DS	−1.837
	(0.710)
d[t + 3] × DS	−0.527
	(0.570)
d[t + 4] × DS	−0.885
	(0.546)
d[t + 5] × DS	−1.655
	(1.031)
d[t−3] × DS × Market size	NR
	NR
d[t−2] × DS × Market size	0.056
	(0.030)
d[t−1] × DS × Market size	0.019
	(0.035)
d[t] × DS × Market size	0.071
	(0.081)
d[t + 1] × DS × Market size	0.190
	(0.081)
d[t + 2] × DS × Market size	0.181
	(0.070)
d[t + 3] × DS × Market size	0.057
	(0.056)
d[t + 4] × DS × Market size	0.092
	(0.054)
d[t + 5] × DS × Market size	0.167
	(0.100)
Controls for d[t−3] to d[t + 5]	Y
Individual fixed effects	Y
Industry × year fixed effects	Y
Time-varying control	Y
Observations	75,000
R^2	0.6816

In sum, the evidence suggests that the size of a local labor market provides significant variation in workers' personal cost of job loss, which in turn affects the indirect costs of financial distress. In Online Appendix D, I use the estimates of the impact of labor market size on workers' earnings loss after a job loss to show that the economic magnitudes of the effect of plant openings on firm leverage is consistent with the following mechanism: a reduction in earnings loss risk encourages firms to take on more financial leverage.[34]

6. Alternative explanations and robustness

This section examines the validity of leading alternative explanations and the robustness of the baseline results using complementary empirical approaches.

6.1. Bargaining with employees

One might argue that a plant opening increases employees' bargaining power by improving their local job opportunities. If so, firms in the winner county could increase their financial leverage to improve their bargaining position in setting wages (Bronars and Deere, 1991; Matsa, 2010). However, it is not theoretically clear whether a plant opening would increase workers' bargaining power, which is considered to be driven by such things as (threat of) unionization, uniqueness of labor skills, and importance of labor to the firm (Mishel, 1984; Katz et al., 2008). Rather, a plant opening is more likely to increase employees' outside options by reducing labor market slack. In fact, rent-sharing models of wages typically consider workers' outside option as a separate force from the bargaining power in wage determination.[35] Consistent with this theoretical difficulty of the bargaining channel as an alternative explanation of the results, I do not find support for the channel. Specifically, in an untabulated analysis that separately estimates the effect of a plant opening for firms with relatively high and low employee bargaining power (e.g., using unionization rates as a proxy) or rents to be shared (using profitability as a proxy), I find that the effect is statistically not different between these groups.

6.2. Alternative measure of corporate leverage

In this section, I estimate the effect of a large plant opening by using the debt-to-assets ratio as an alternative measure of corporate leverage. Online Appendix Table 6, Panel A shows that the plant opening leads to a 0.96 to 1.31 percentage point increase in debt-to-assets ratio for average firms in the winner versus runner-up counties (the latter is significant at the 10% level). In Panel B, I find that existing firms with greater exposure to the affected local labor markets experience a 2.5 percentage point leverage increase (significant at the 5% level), whereas firms with smaller exposure experience an insignificant decrease in leverage (the difference between the two groups is significant at the 5% level). Panels C and D show that all the other estimates are qualitatively similar to my estimates using the debt-to-capital ratio as the dependent variable, although statistical significance is generally lower when the debt-to-assets ratio is employed. These results are consistent with the argument that the debt-to-capital ratio is a cleaner measure of corporate leverage (Welch, 2011).

6.3. Geographical distribution of winner and runner-up counties

Table 1 and Fig. 1 show that runner-up localities are relatively concentrated in the Northeast and Midwest and winners are clustered in the South and West. This uneven spatial distribution could raise the concern that omitted economic factors specific to regions is driving firm capital structure decisions. To address this concern, I estimate the effect of a plant opening conditional on whether both the winner and runner-up counties in a given event are in the same census region, or in the (1) Northeast or Midwest regions, or (2) South or West regions. Online Appendix Table 7 shows that there is no significant difference in the estimates for *After* $\times$ *Winner* whether the winner and runner-up counties are in the same regions or not. The difference between the two groups in columns 1 and 2 (3 and 4) is insignificant with an associated t-statistic of 0.54 (0.09).

6.4. Increased labor cost

An alternative explanation is that increased labor demand in the winner county leads firms to increase their debt usage in part to finance higher wage bills (assuming that local labor supply is upward-sloping). However, as shown in Section 5.5.2, there is no significant change in wages at plants in the winner versus runner-up counties. Moreover, at a theoretical level, debt is less suitable than equity to finance wages because a higher labor cost increases the firm's operating leverage (e.g., Schmalz, 2015). Given the trade-off between operating and financial leverages (Mauer and Triantis, 1994), an increase in expected wages would lead to a decrease, as opposed to an increase, in optimal financial leverage.

6.5. County-wide shocks

Another concern is that local-level economic or financial shocks that coincide with the plant opening is driving the increase in leverage. I address this concern by estimating the effect of a manufacturing plant opening on the leverage of incumbent nonmanufacturing firms in the county and vice versa. If countywide shocks drive the effect, then it would likely hold independent of the sector. Online Appendix Table 8 shows that both the effect of a large manufacturing plant opening on existing nonmanufacturing firms and that of a large nonmanufacturing establishment opening on existing manufacturing firms are negative and insignificant. This result suggests that a new establishment has no impact on the leverage of incumbent

[34] I thank the referee for encouraging the exploration of this issue.

[35] For example, see Mortensen (2003) for a bilateral bargaining model, which shows equilibrium wages, $w(p) = b + \beta(p - b)$, where b and β represent the worker's outside option and bargaining power, respectively, and p represents productivity.

firms in different economic sectors, plausibly due to a difference in skills. However, in an untabulated analysis, I find that the opening of a large nonmanufacturing establishment leads to an (insignificant) increase in leverage for existing nonmanufacturing firms, although the estimated effect is insignificant partly due to the small event sample size.[36]

7. Conclusion

This paper investigates how the size of labor markets affects the capital structure choices of firms through changes in the costs of financial distress arising from workers' limited ability to move across jobs. To examine this relation, I use the opening of a large manufacturing plant in a given county, combined with plant-level data from the Census Bureau, as an abrupt increase in local market size for manufacturing labor. My estimates indicate that after a plant opening, existing manufacturing firms in the county increase their leverage ratios by 2.5 to 3.9 percentage points relative to firms in an otherwise comparable county. Additional analysis shows that plant openings have a larger impact on firms that are more likely to use the same type of labor as the new plant. Tracking displaced workers in the LEHD data reveals that the costs of job loss are significantly mitigated in larger local labor markets. Overall, the evidence suggests that the size of labor markets in which firms operate is an important determinant of the costs of using financial leverage and hence the capital structure decisions of firms.

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[36] To perform these analyses, I collect 18 cases of large nonmanufacturing establishment openings, such as retail warehouses and operation centers, from Site Selection and the SSEL and LBD databases following the procedure described in Section 4.1. Thus, one caveat of the analyses is that the small event sample size relative to the main sample of manufacturing MDPs (18 versus 40) implies that these tests may have low power.