Firm Leverage, Consumer Demand, and Unemployment during the Great Recession*

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Abstract

We argue that firms' balance sheets were instrumental in the propagation of consumer demand shocks during the Great Recession. Using establishment-level data, we show that establishments of more highly levered firms exhibit a significantly larger decline in employment in response to a drop in consumer demand. These results are not driven by firms being less productive, having expanded too much prior to the Great Recession, or being generally more sensitive to fluctuations in either aggregate employment or house prices. At the county level, we find that counties with more highly levered firms experience significantly larger job losses in response to county-wide consumer demand shocks. Thus, firms' balance sheets also matter for aggregate employment. Our research suggests a possible role for employment policies that target firms directly besides conventional stimulus.

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I. INTRODUCTION

The collapse in house prices during the Great Recession caused a sharp drop in consumer demand by households (Mian, Rao, and Sufi (2013)). This drop in demand in turn had severe consequences for employment: across U.S. counties, those with a larger decline in housing net worth experienced a significantly larger decline in employment, especially in the non-tradable sector (Mian and Sufi (2014a)).

What is conspicuously absent from this causal chain is any role for firms. After all, households do not lay off workers. Firms do. To explore the role of firms in the propagation of consumer demand shocks during the Great Recession, we construct a unique data set that combines employment data at the establishment level from the U.S. Census Bureau's Longitudinal Business Database (LBD) with balance sheet and income statement data at the firm level from Compustat and house price data at the ZIP code and county level from Zillow. Hence, our sample consists of small establishments—e.g., retails stores, supermarkets, or restaurants—that are matched to house prices in the establishment's ZIP code or county.

Our results show that *firm balance sheets* play a crucial role in the propagation of consumer demand shocks during the Great Recession. This is noteworthy, because both academic research and public policy have focused primarily on the role of either household or financial intermediary balance sheets.[1] In particular, our results show that establishments of firms with higher leverage at the onset of the Great Recession exhibit a significantly larger decline in employment in response to a drop in consumer demand during the Great Recession. The magnitude of this leverage effect is large. Imagine two establishments, one whose parent firm lies at the 90% percentile of the leverage distribution and another whose parent firm lies at the 10% percentile of the leverage distribution. Our

[1] For research focusing on the role of household balance sheets during the Great Recession, see, e.g., Mian and Sufi (2014a), Mian, Rao, and Sufi (2013), Guerrieri and Lorenzoni (2011), Hall (2011), Midrigan and Philippon (2011), and Eggertson and Krugman (2012). For research focusing on the role of financial intermediary balance sheets, and "lender health" in general, see, e.g., Chodorow-Reich (2014), Gertler and Kyotaki (2011), He and Krishnamurthy (2013), Brunnermeier and Sannikov (2014), and Moreira and Savov (2014). A notable exception is Gilchrist et al. (2014), who show that firms with weak balance sheets raise prices during the Great Recession, which may help explain why the U.S. economy experienced only a mild disinflation during that period.

estimates imply that the former establishment exhibits a three times larger elasticity of employment with respect to house prices. Importantly, firm leverage is uncorrelated with changes in house prices during the Great Recession. Thus, establishments of low- and high-leverage firms face the same consumer demand shocks—they merely react differently to these shocks.

The granularity of our data allows us to include a wide array of fixed effects in our cross-sectional regressions. Our tightest specification includes both firm and ZIP code $\times$ industry fixed effects. Hence, accounting for the possibility that low- and high-leverage firms experience differential job losses for reasons unrelated to changes in house prices, our empirical setting allows us to compare establishments in the same ZIP code and industry, where some establishments belong to low-leverage firms and others belong to high-leverage firms. Our establishment-level results are based on more than a quarter million observations and thus precisely estimated.

We also examine whether firms make adjustments at the extensive margin. Similar to what we find for employment, we find that establishments of more highly levered firms are significantly more likely to be closed down in response to consumer demand shocks. Moreover, and in line with prior research, we find no significant correlation between changes in house prices and changes in establishment-level employment in the tradable sector. By contrast, we find a positive and significant correlation in the non-tradable and "other" sectors—i.e., industries that are neither tradable nor non-tradable. Importantly, in both sectors, this correlation is significantly stronger among establishments of more highly levered firms.

Our results suggest that financial constraints play an important role in firms' decisions to engage in *labor hoarding*. The idea behind labor hoarding is that firms facing a temporary (e.g., cyclical) decline in demand choose to retain more workers than technically necessary so as to economize on the costs of firing, hiring, and training workers. This reduces the sensitivity of employment in response to changes in demand. Labor hoarding is costly, however. Effectively, firms must (temporarily) subsidize their workers' wages. Hence, firms with less financial slack face a tradeoff between long-run optimization—saving on firing, hiring, and training costs—and short-run liquidity needs. Our results suggest that firms with weaker balance sheets—and tighter financial constraints—are more apt to respond to this tradeoff by engaging in less labor hoarding.

In our sample, more highly levered firms indeed appear to be more financially constrained according to various measures. But do they also *act* like financially constrained firms in the Great Recession? To address this question, we turn to firm-level regressions. We find that, when faced with a drop in consumer demand, more highly levered firms are less apt (or able) to raise additional short- and long-term debt during the Great Recession. As a consequence, they experience more layoffs, are more likely to close down establishments, and cut back more on investment. Altogether, our results suggest that firms with higher leverage not only appear to be more financially constrained, but they also act like financially constrained firms in the Great Recession.

We explore alternative channels whereby more highly levered firms respond more strongly to consumer demand shocks not because they are more financially constrained, but rather because they are less productive or expanded to much in the years running up to the Great Recession. We also consider the possibility that more highly levered firms have more active investors, such as private equity funds or activist hedge funds. Further, we examine the possibility that more highly levered firms are merely "high-beta" firms that are *generally* more sensitive to either aggregate employment or house prices—i.e., for reasons unrelated to financial constraints. We find little evidence in support of any of these alternative channels.

In general equilibrium, output and workers may shift from high- to low-leverage firms. In an economy without frictions, this could imply that aggregate employment changes only little, or perhaps not at all. To empirically investigate whether the distribution of firm leverage also matters in the aggregate, we turn to county-level regressions. Imagine two counties, one with a smaller share of high-leverage firms and the other with a larger share. Suppose further that both counties exhibit a similar drop in house prices. If our previous results also hold in the aggregate, then the more highly levered county should experience a larger decline in employment. By contrast, if the distribution of firm leverage does not matter in the aggregate, then both counties should experience a similar decline in employment, irrespective of the level of "county leverage." Regardless of whether we consider total county-level employment by all firms in our sample or by all firms in the LBD, we find that more highly levered counties exhibit a significantly larger decline in employment in response to county-wide consumer demand shocks. Thus, our results are not undone by general equilibrium effects.

We conclude with a discussion of policy implications. That financial constraints may impair firms' ability to engage in labor hoarding suggests that it may be useful to think about policies that target firms directly besides conventional stimulus. To this end, we discuss the case of Germany, which has seen virtually no increase in unemployment despite being hit hard by the global recession of 2008-09. Many commentators attribute this resilience to large-scale labor hoarding, which is heavily subsidized in Germany. A central pillar of labor hoarding in Germany is the system of "short-time work" programs encouraging firms to adjust labor demand through hours reductions rather than layoffs. While a similar system also exists in many U.S. states ("work sharing" or "short-time compensation"), take-up rates have been extremely small due to financial disincentives for employers and workers, burdensome filing processes, program rigidity, and lack of employer outreach by state agencies.

In seminal work, Mian and Sufi (2011) and Mian, Rao, and Sufi (2013) show that rising house prices in the years running up to the Great Recession led to the build-up of household leverage, causing a sharp drop in consumer demand as house prices fell between 2006 and 2009. Mian and Sufi (2014a) examine the consequences of these consumer demand shocks for aggregate employment at the county level.[2] Our focus is at the establishment level. In particular, we show that establishments of more highly levered firms exhibit a larger decline in employment in response to a drop in consumer demand during the Great Recession.

The notion that firm balance sheets play an important role in the amplification and propagation of business cycle shocks goes back to Bernanke and Gertler (1989), Kiyotaki and Moore (1997), and Bernanke, Gertler, and Gilchrist (1999). Unlike a "standard" financial accelerator model, however, our focus is not on aggregate shocks to firms' net

[2] Charles, Hurst, and Notowidigo (2014) and Adelino, Schoar, and Severino (2015) examine the implications of rising house prices for employment in the years leading up to the Great Recession.

worth but rather on the *interaction* between heterogeneous demand shocks and firms' financial conditions. Caggese and Perez (2015) model precisely this interaction in a dynamic general equilibrium model with heterogeneous firms and households subject to financial and labor market frictions. When calibrating their model to U.S. data, the authors find interaction effects which, as they conclude, are consistent with those in our paper. Finally, Aghion et al. (2015) also explore the role of firm heterogeneity during the Great Recession. Using firm-level data from OECD countries, they find that decentralized firms fare significantly better than their centralized counterparts, especially in industries that were hit hard in the Great Recession.

The rest of this paper is organized as follows. Section II describes the data, variables, and summary statistics. Section III examines the interplay between consumer demand shocks, firm balance sheets, and employment at the establishment level. Section IV discusses the implications of financial constraints for labor hoarding. Section V explores alternative channels. Section VI considers aggregate employment at the county level. Section VII discusses policy implications. Section VIII concludes.

II. DATA, VARIABLES, AND SUMMARY STATISTICS

We construct a unique data set that combines employment data at the establishment level with balance sheet and income statement data at the firm level and house price data at the ZIP code and county level.

The establishment-level data are provided by the U.S. Census Bureau's Longitudinal Business Database (LBD). An establishment is a "single physical location where business is conducted" (Jarmin and Miranda (2003, p. 15)), e.g., a retail store, supermarket, restaurant, warehouse, or manufacturing plant. The LBD covers all business establishments in the U.S. with at least one paid employee.

The firm-level data are from Compustat. We exclude financial firms (SIC 60-69), utilities (SIC 49), and firms with missing financial data between 2002 and 2009. We match the remaining firms to establishments in the LBD using the Compustat-SSEL bridge maintained by the U.S. Census Bureau. Given that this bridge ends in 2005, we extend the match to 2011 using employer name and ID number (EIN) following the procedure described in McCue (2003). This leaves us with 327,500 establishments with non-missing employment data from 2007 to 2009.[3]

The house price data are from Zillow.[4] Of the 327,500 establishments, we are able to match 227,600 establishments to ZIP code-level house prices and 57,200 establishments to county-level house prices, leaving us with a final sample of 284,800 establishments for which we have both firm-level data and house price data.[5]

Our main analysis is at the establishment level. We regress the percentage change in employment at the establishment level between 2007 and 2009, Δ Log(Emp)$_{07-09}$, on the percentage change in house prices in the establishment's ZIP code or county (if the ZIP code information is missing) between 2006 and 2009, Δ Log(HP)$_{06-09}$, the level of firm leverage associated with the establishment's parent firm in 2006, Leverage$_{06}$, and the interaction term Δ Log(HP)$_{06-09}$ $\times$ Leverage$_{06}$. Our main focus is on the interaction term. Leverage is defined as the ratio of the sum of debt in current liabilities and long-term debt to total assets (from Compustat) and is winsorized between zero and one. In robustness checks, we also use other measures of debt capacity or financing constraints. While our main specification includes industry fixed effects, some of our specifications also include firm, ZIP code, or ZIP code $\times$ industry fixed effects. All regressions are weighted by the size of establishments, i.e., their number of employees. Standard errors are clustered at both the state and firm level.

The change in house prices between 2006 and 2009, Δ Log(HP)$_{06-09}$, is highly correlated with similar variables used in other research. For instance, the correlation at the MSA level with Δ Housing Net Worth 2006–2009, the main explanatory variable in Mian, Rao, and Sufi (2013) and Mian and Sufi (2014a), is 86.3%. Other papers, like Adelino,

[3]All sample sizes are rounded to the nearest hundred following disclosure guidelines by the U.S. Census Bureau.

[4]For the period from 2006 to 2009, the Zillow Home Value Index (ZHVI) is available for 12,102 ZIP codes and 1,048 counties. See www.zillow.com/research/data for an overview of the ZHVI methodology and a comparison with the S&P/Case-Shiller Home Price Index.

[5]Our results are similar if we use only the 227,600 establishments for which we have ZIP code-level house prices or if we use the full sample of 327,500 establishments by matching the remaining 327,500 - 284,800 = 42,700 establishments to state-level house prices constructed as population-weighted averages of available ZIP code-level house prices. See Table 1 of the Online Appendix.

Schoar, and Severino (2014) or Charles, Hurst, and Notowidigo (2014), use house price data from the Federal Housing Finance Agency (FHFA). The correlation at the MSA level between our house price variable, Δ Log(HP)$_{06-09}$, and the corresponding house price variable constructed from FHFA data is 96.4%.

Panel (A) of Table I provides summary statistics at the establishment level. The first and second column show the mean and standard deviation, respectively. The third column shows the correlation with Leverage$_{06}$. The last column shows the p-value of this correlation. As can be seen, establishments of more highly levered firms exhibit larger job losses during the Great Recession. Importantly, Leverage$_{06}$ is uncorrelated with both changes in house prices between 2006 and 2009 and housing supply elasticity. Hence, establishments of low- and high-leverage firms face the same consumer demand shocks; they merely react differently to these shocks. Notably, establishments of more highly levered firms are somewhat underrepresented in the non-tradable sector, while they are somewhat overrepresented in the "other" sector—i.e., industries that are neither tradable nor non-tradable. This is not a major concern, however. First, we perform separate analyses for each sector and obtain similar results for the non-tradable and "other" sector. Second, all our regressions include industry fixed effects.

Panel (B) provides firm-level summary statistics in 2006, at the onset of the Great Recession. As can be seen, more highly levered firms are less productive—they have a lower return on assets (ROA), lower net profit margin (NPM), and lower total factor productivity (TFP). Moreover, and not surprising, firms with higher leverage are more financially constrained according to the financial constraints indices of Kaplan and Zingales (1997) and Whited and Wu (2006).

Panel (C) includes the same firm-level variables as Panel (B). However, instead of showing their levels in 2006, it shows their changes between 2002 and 2006. Three results stand out. First, firms with higher leverage expand more in the years prior to the Great Recession. This holds irrespective of whether we consider growth in establishments, employees, or assets. Second, firms with higher leverage exhibit lower productivity growth between 2002 and 2006, which may explain the negative correlation between leverage and productivity in Panel (B). Third, firms with higher leverage experience an increase in leverage along with a tightening of financial constraints in the years prior to the Great Recession. While this last result is not surprising, the magnitude of the effect is large: the correlation between Leverage_{06} and the change in firm leverage between 2002 and 2006, Δ Leverage_{02-06}, is 37.9%. Accordingly, a substantial part of the cross-sectional variation in firm leverage in 2006, at the onset of the Great Recession, can be explained by changes in firm leverage in the years running up to the Great Recession.

We would like to caution that the various correlations with Leverage_{06} may not be independent of each other. Indeed, it is plausible that firms with higher leverage in 2006 increased their leverage between 2002 and 2006 *because* they needed to fund an expansion or a deficit arising from a productivity shortfall. But this raises the possibility that firms with higher leverage in 2006 respond more strongly to consumer demand shocks during the Great Recession not because they are more financially constrained, but rather because they expanded too much in the run-up period or were less productive at the onset of the Great Recession. We will address these and other alternative channels below.

III. FIRM LEVERAGE, CONSUMER DEMAND, AND UNEMPLOYMENT

III.A. Main Results

Figure I plots the percentage change in establishment-level employment between 2007 and 2009, Δ Log(Emp)_{07-09}, against the percentage change in house prices in the establishment's ZIP code or county (if the ZIP code information is missing) between 2006 and 2009, Δ Log(HP)_{06-09}, for various quartiles of firm leverage. For each percentile of Δ Log(HP)_{06-09}, the scatterplot shows the mean values of Δ Log(HP)_{06-09} and Δ Log(Emp)_{07-09}, respectively. In Panel (A), which depicts the lowest leverage quartile, there is a positive albeit weak relationship between changes in house prices and changes in employment at the establishment level, as illustrated by the solid trend line. In Panels (B) to (D), this relationship becomes successively stronger. In Panel (D), which depicts the highest leverage quartile, the elasticity of establishment-level employment with respect to house prices is 0.096, which is almost four times larger than the corresponding elasticity in the lowest leverage quartile.

Table II confirms this visual impression using regression analysis. All regressions are weighted by the size of establishments, i.e., their number of employees. As is shown, the average elasticity of establishment-level employment with respect to house prices is 0.066 and highly significant (column (1)). To put this number into perspective, imagine two establishments, one located in a ZIP code associated with a 10th percentile change in house prices and another located in a ZIP code associated with a 90th percentile change in house prices. An elasticity of 0.066 implies that the former establishment experiences an additional employment loss of 2.88 percentage points. Accordingly, changes in house prices during the Great Recession have a profound impact on changes in employment at the establishment level.

Columns (2) to (7) examine whether the elasticity of establishment-level employment with respect to house prices depends on the leverage of the establishment's parent firm. The main differences across the various columns are the fixed effects. Arguably, our "tightest" specification is column (7). While the inclusion of firm fixed effects accounts for any unobserved heterogeneity across firms, the ZIP code $\times$ industry fixed effects force comparison to be made between establishments in the same ZIP code and 4-digit NAICS industry. To this end, we should note that while our sample firms are in Compustat, their establishments are relatively small, with an average of 39 employees. Thus, accounting for the possibility that low- and high-leverage firms may exhibit differential job losses for reasons unrelated to changes in house prices, our empirical setting compares small establishments in the same ZIP code and industry, where some establishments belong to low-leverage firms and others belong to high-leverage firms.

Regardless of which fixed effects we include, we always find that the interaction term Δ Log(HP)$_{06-09}$ $\times$ Leverage$_{06}$ is positive and significant. Hence, establishments of more highly levered firms exhibit a significantly larger decline in employment in response to consumer demand shocks. The economic magnitude of this leverage effect is large. Imagine two establishments, one whose parent firm lies at the 90% percentile of the leverage distribution and another whose parent firm lies at the 10% percentile of the leverage distribution. Our estimates in column (3) imply that the former establishment has a three times larger elasticity of employment with respect to house prices.

The only fixed effects that have a noticeable impact on the coefficient associated with the interaction term are the firm fixed effects. Moving from columns (2) to (4) to columns (5) to (7), which include firm fixed effects, the coefficient associated with the interaction term drops markedly, albeit it remains significant at the 5% level. We should note, however, that including firm fixed effects may be "overcontrolling"—i.e., it may be "controlling away" some of the very effects we are trying to document. For instance, some firms in our sample are regionally concentrated firms that have most of their establishments in the same region. As the firm fixed effects force comparison to be made between different establishments within the same firm, this implies that, for regionally concentrated firms, there exists little within-firm variation in house price changes, making it difficult to identify the effects on employment changes. Given this issue, we use column (3) as our main specification. This specification has the further advantage that it also shows the coefficients associated with the main effects, Δ Log(HP)$_{06-09}$ and Leverage$_{06}$, respectively. That being said, the analysis in Table II shows that our main results hold under various fixed-effect specifications.

III.B. Other Measures of Debt Capacity

We obtain similar results when using other measures of firms' debt capacity. As Table III shows, all results are similar when using either net or market leverage, debt to EBITDA, and interest coverage, all measured in 2006 (columns (1) to (4)). They are also similar when using the change in leverage between 2002 and 2006 in lieu of the level of leverage in 2006 (column (5)). As discussed previously, these two variables are highly correlated, implying that firms with higher leverage in 2006 are to a large extent firms that increased their leverage in prior years. Finally, our results are similar when using the financial constraints indices of Kaplan and Zingales (1997) and Whited and Wu (2006) (columns (6) and (7)). Ultimately, all of the measures in Table III are proxies for the strength of firms' balance sheets.

III.C. Instrumenting House Price Changes

Unobserved heterogeneity may be driving both changes in house prices and changes in employment. We address this issue by instrumenting changes in house prices using the housing supply elasticity instrument from Saiz (2010). This instrument captures geographical and regulatory constraints to new construction. Accordingly, areas with inelastic housing supply are facing supply constraints due to their topography (steep hills and water bodies) as well as local regulations.

The instrumental variables (IV) results are shown in Table 2 of the Online Appendix. Similar to other studies, we find that housing supply elasticity is a strong predictor of changes in house prices during the Great Recession. Importantly, the results of the second-stage regression confirm that establishments of more highly levered firms respond significantly more strongly to consumer demand shocks. If anything, the IV estimates are slightly stronger than the OLS estimates. A possible concern with the housing supply elasticity instrument is that it also includes regulatory constraints, which may be driven by the same unobserved heterogeneity that also drives employment dynamics. To mitigate this concern, we repeat the analysis using only the part of the instrument that is based on an area's topology, "share of unavailable land."[6] All results remain similar.

III.D. Industry Sectors

The summary statistics in Table I show that establishments of more highly levered firms are somewhat underrepresented in the non-tradable sector, while they are somewhat underrepresented in the "other" sector.[7] While our establishment-level regressions include industry fixed effects, we can directly address concerns related to industry sector composition by performing separate analyses for each sector.

Figure II plots the relationship between changes in establishment-level employment between 2007 and 2009, Δ Log(Emp)$_{07-09}$, changes in house prices in the establishment's ZIP code (or county) between 2006 and 2009, Δ Log(HP)$_{06-09}$, and firm leverage separately for each industry sector. The scatterplots are constructed analogously to those in

[6] We are grateful to Albert Saiz for making the data available to us.

[7] Mian and Sufi (2014) classify an industry as tradable if imports plus exports exceed $10,000 per worker or $500M in total. Retail industries and restaurants are classified as non-tradable. We label industries that are neither tradable nor non-tradable as "other." The "other" sector is comprised of a diverse set of industries that includes, e.g., news and entertainment, transportation and trucking, healthcare and hospitals, and wholesale. Mian and Sufi also provide an alternative industry classification based on the geographical concentration of industries. Our results are similar when using this alternative classification. See Table 3 of the Online Appendix.

Figure I. As is shown, the non-tradable and "other" sectors look very similar. In both cases, there is a positive albeit weak relationship between changes in house prices and changes in employment in the lowest leverage quartile but a strongly positive relationship in the highest quartile (Panels (A) to (D)). In fact, the elasticities in the highest quartile are virtually identical in both sectors (0.089 versus 0.092). By contrast, there is no clear association between changes in house prices and changes in employment in the tradable sector (Panels (E) and (F)).

Panel (A) of Table IV confirms this visual impression using regression analysis. As is shown, there is no significant correlation between changes in house prices and changes in establishment-level employment in the tradable sector (column (3)). By contrast, there is a positive and significant correlation in the non-tradable sector (column (1)). Together, these findings confirm similar results by Mian and Sufi (2014a), who examine changes in employment at the county level. While differences in results across industries are sometimes a concern, the opposite is true here. Indeed, if changes in house prices affect local employment through changes in consumer demand, then variation in house prices should explain (geographical) variation in employment primarily in the non-tradable sector, where demand by households is local. By contrast, variation in house prices should not correlate strongly with variation in employment in the tradable sector, where demand is national or global. Given this evidence, as well as evidence in Mian, Rao, and Sufi (2013) and Mian and Sufi (2014a), we use "falling house prices" and "consumer demand shocks" interchangeably.[8]

Two further results in Panel (A) stand out. First, the correlation between changes in house prices and changes in establishment-level employment is positive and significant both in the non-tradable and in the "other" sector (column (3)). Indeed, the two elasticities are virtually identical (0.074 versus 0.075). Together, these two sectors account for

[8]The main alternatives are: i) falling house prices affect local employment by impairing the collateral value associated with local firms' commercial real estate, and ii) falling house prices affect local employment by affecting local credit supply—e.g., local banks cut lending after experiencing losses on their mortgage loan portfolios. In either case, however, it is unclear why employment in the tradable sector should remain unaffected; see Mian and Sufi (2014a) for a further discussion. Moreover, Mian, Rao, and Sufi (2013) provide direct evidence showing that U.S. counties with a larger decline in housing net worth exhibit a larger decline in consumer spending during the Great Recession.

97% of all establishment-level observations. Hence, there is no need to interact changes in house prices with sector dummies in our regressions.[9] Second, in both sectors, the correlation between changes in house prices and changes in employment is stronger among establishments of more highly levered firms. Accordingly, our results are not driven by industry sector composition effects.

Panel (B) lists the top ten industries in which house prices have the biggest impact on establishment-level employment. To construct this list, we have estimated column (1) of Table II separately for each 4-digit NAICS industry. At the top of the list are full-service restaurants (non-tradable) followed by building material and supplies dealers (other) and health and personal care stores (non-tradable). Interestingly, three of the top ten industries are auto-related: automotive repair and maintenance (#4, other), automotive parts, accessories, and tire stores (#7, non-tradable), and automobile dealers (#8, non-tradable). Not surprisingly, there is no tradable industry among the top ten.[10]

III.E. Establishment Closures

Does the drop in house prices between 2006 and 2009 also lead to adjustments at the extensive margin? To examine this question, we again estimate our baseline regression, except that the sample now also includes establishments that are closed down between 2007 and 2009. The dependent variable is a dummy indicating whether an establishment is closed during that period. As Table V shows, changes in house prices between 2006 and 2009 are negatively and significantly associated with establishment closures (column (1)). Moreover, and similar to our employment regressions, this effect is significantly stronger among establishments of more highly levered firms (column (2)). Hence, firms respond to falling house prices by making adjustments at both the intensive and the extensive margin.

[9] While the tradable sector accounts for 3% of all establishments, it accounts for 12% of total employment in our sample. In other words, firms in the tradable sector have relatively few but large establishments (e.g., manufacturing plants). Since all our regressions are employment-weighted, this implies that excluding tradable industries should make our results only stronger. Indeed, Table 4 of the Online Appendix shows that the results become slightly stronger if we exclude tradable industries.

[10] Cement and concrete product manufacturing (#10, other) is not classified as tradable, because its imports plus exports do not exceed $10,000 per worker or $500M in total. Due to high transportation costs, the market for cement and concrete manufacturing is largely local.

III.F. Compustat-LBD Sample versus Full LBD Sample

Our sample consists of establishments in the U.S. Census Bureau's Longitudinal Business Database (LBD)—e.g., retail stores, supermarkets, or restaurants—whose parent firms have a match in Compustat. Thus, our sample does not include establishments of private firms or, more importantly, single-unit establishments (e.g., "mom and pop shops").[11] Overall, our sample accounts for 12% of total LBD employment. In terms of industry sectors, our sample accounts for 26% of non-tradable employment, 18% of tradable employment, and 8% of "other" employment.

One might worry that our sample includes establishments that are especially responsive to consumer demand shocks. To explore this hypothesis, we regress the percentage change in establishment-level employment between 2007 and 2009, Δ Log(Emp)$_{07-09}$, on the percentage change in house prices between 2006 and 2009, Δ Log(HP)$_{06-09}$, separately for establishments in the matched Compustat-LBD sample and those in the full LBD sample. As Table 5 of the Online Appendix shows, the elasticity of establishment-level employment with respect to house prices in the full LBD sample is about 65% higher than in the matched Conpustat-LBD sample. Thus, if anything, our sample includes establishments that respond *less* strongly to consumer demand shocks.

The result that establishments in the matched Compustat-LBD sample have lower elasticities is consistent with firms in Compustat being less financially constrained. Indeed, several empirical studies provide evidence suggesting that private firms are more financially constrained than public firms, e.g., Brav (2009), Saunders and Steffen (2011), and Gilje and Taillard (2015). Notably, the lower elasticities of establishments in the matched Compustat-LBD sample are *not* due to these establishments being located in regions with a smaller decline in house prices: the correlation between Δ Log(HP)$_{06-09}$ and the employment shares of establishments in the matched Compustat-LBD sample at the ZIP code or county level is close to zero and insignificant (1.4% (p-value 0.321) and 1.2% (p-value 0.681), respectively). Likewise, a Kolmogorov-Smirnov test is unable to reject the null that the distribution of Δ Log(HP)$_{06-09}$ is identical for establishments in

[11] Our county-level analysis in Section VI is an exception. There, some regressions have *total* LBD employment as the dependent variable.

the matched Compustat-LBD sample and other establishments in the LBD. Thus, establishments in the matched Compustat-LBD sample and other establishments in the LBD experience a similar drop in house prices.

IV. FINANCIAL CONSTRAINTS AND LABOR HOARDING

The concept of "labor hoarding," which goes back to the early 1960s, posits that firms facing a *temporary* (e.g., cyclical) drop in demand choose to retain more workers than would be technically necessary so as to economize on the costs of firing, hiring, and training workers.[12] This reduces the sensitivity of employment in response to changes in demand. Direct evidence in support of labor hoarding comes from a survey of plant managers by Fay and Medoff (1985) asking detailed questions about the workforce retained during the plant's most recent downturn. The typical plant paid for about eight percent more blue-collar labor hours in a downturn than were technically necessary to meet production requirements. About half of this labor could be justified by other useful tasks—e.g., maintenance, cleaning, or training—leaving about four percent of the blue-collar hours paid for by the typical plant to be classified as truly "hoarded." By the 1980s and 90s, the concept of labor hoarding had become an "accepted part of economists' explanations of the workings of labor markets and of the relationship between labor productivity and economic fluctuations" (Biddle (2014, p. 197)).

Labor hoarding is costly, however. Effectively, firms must (temporarily) subsidize their workers' wages. Consequently, firms with little financial slack face a genuine tradeoff between long-run optimization—saving on the costs of firing, hiring, and (re-)training workers—and short-run liquidity needs. Our results suggest that firms with weaker balance sheets—and tighter financial constraints—are more apt to respond to this tradeoff by engaging in less labor hoarding. In other words, firms with weaker balance sheets cut more jobs in response to a decline in consumer demand than they (optimally) would have in the absence of financial constraints.

[12] Early contributions to the labor hoarding literature include Oi (1962) and Okun (1963). Biddle (2014) provides a comprehensive overview of the literature, including its historical origins.

In our sample, more highly levered firms indeed appear to be more financially constrained. According to the summary statistics in Table I, they score worse on popular measures of financial constraints, such as the indices by Kaplan and Zingales (1997) and Whited and Wu (2006). But do they also *act* like financially constrained firms during the Great Recession? To address this question, we turn to firm-level regressions. Precisely, we estimate the firm-level analogue of our baseline specification, where Δ Log(HP)$_{06-09}$ is the employment-weighted average percentage change in house prices between 2006 and 2009 across all of the firm's establishments. That is, Δ Log(HP)$_{06-09}$ is the average consumer demand shock faced by the firm. The dependent variable at the firm level is either the change in short-term debt, long-term debt, or equity, the change in employment or investment, or the fraction of establishments closed, all between 2007 and 2009. The first three dependent variables measure a firm's access to external finance during the Great Recession. The last three dependent variables measure if being financially constrained has real consequences at the firm level.

Table VI presents the results. When faced with consumer demand shocks during the Great Recession, more highly levered firms are less apt (or able) to raise additional short- and long-term debt (columns (1) and (2)).[13] As a consequence, they experience more layoffs, are more likely to close down establishments, and cut back more on investment (columns (4) to (6)). Overall, these results suggest that firms with higher leverage not only appear to be more financially constrained, but they also act like financially constrained firms in the Great Recession.

We should note that ours is not the first study to point to a link between financial constraints and labor hoarding. Using manufacturing firm-level data from 1959 to 1985, Sharpe (1994) shows that employment growth is more cyclical at more highly levered firms. Like we do, Sharpe concludes that financial constraints impair firms' ability to engage in labor hoarding. Survey evidence by Campello, Graham, and Harvey (2010) supports this conclusion. The authors asked 574 U.S. CFOs in 2008 whether their firms

[13] We are unable to reject the null that the sum of Δ Log(HP)$_{06-09}$ and Δ Log(HP)$_{06-09} \times$ Leverage$_{06}$ is zero in columns (1) and (2) (p-values 0.333 and 0.268, respectively). Consequently, firms with *very* high leverage do not, or cannot, raise *any* short- or long-term debt when faced with consumer demand shocks in the Great Recession.

are financially constrained and what they are planning to do in 2009. Firms classified as financially constrained said they would cut their employment by 10.9% in the following year, whereas firms classified as unconstrained said they would cut their employment only by 2.7%. While both studies suggest a link between employment growth and financial constraints over the business cycle, neither separates out the effects of *demand shocks* that lie at the heart of the labor hoarding concept.

V. ROBUSTNESS

According to the summary statistics in Table I, more highly levered firms are less productive and expand more in the years prior to the Great Recession. As discussed previously, this raises the concern that these firms respond more strongly to consumer demand shocks during the Great Recession not because they are more financially constrained, but rather because they are less productive or expanded too much in the years running up to the Great Recession. In what follows, we address these and other alternative channels by including additional controls Z and $Z \times$ Δ Log(HP)$_{06-09}$ in our regressions, where Z proxies for the alternative channel in question.[14]

V.A. Employment and Asset Growth

Table VII examines if our results are driven by firms expanding too much in the years prior to the Great Recession. In column (1), we include as additional controls the percentage change in employment between 2002 and 2006, Δ Log(Emp)$_{07-09}$, as well as its interaction with Δ Log(HP)$_{06-09}$. Column (2) is similar, except that we consider the percentage change in assets between 2002 and 2006, Δ Log(Assets)$_{07-09}$. As can be seen, including employment or asset growth has little effect in our regressions. While none of the additional controls are significant, the main coefficient of interest—that associated with the interaction term Δ Log(HP)$_{06-09} \times$ Leverage$_{06}$—remains significant and similar to that in our baseline regression in column (3) Table II.

[14] Our fixed-effect specifications in Table II already address some alternative hypotheses. For instance, we can firmly rule out alternative channels whereby low- and high-leverage firms differ along industry or geographical dimensions.

V.B. Productivity

Table VIII examines if our results are driven by firms having low productivity. In column (1), we include as additional controls the firm's return on assets, ROA_{06}, as well as its interaction with Δ $\mathrm{Log(HP)}_{06-09}$. Columns (2) and (3) are similar, except that we consider the firm's net profit margin, NPM_{06}, and total factor productivity, TFP_{06}, respectively. As is shown, the main effect of productivity is significant with the predicted sign: less productive firms experience larger job losses during the Great Recession. However, the interaction with Δ $\mathrm{Log(HP)}_{06-09}$, while having the predicted sign, is either insignificant (columns (1) and (2)) or only weakly significant (column (3)). Importantly, the coefficient associated with the interaction term Δ $\mathrm{Log(HP)}_{06-09} \times$ $\mathrm{Leverage}_{06}$, which is our main coefficient of interest, remains significant and stable in all regressions.

V.C. Sensitivity to Aggregate Employment and House Prices

Table IX examines if our results are driven by firms being generally more sensitive to either aggregate employment or house prices ("high-beta firms")—i.e., for reasons *unrelated* to financial constraints.[15] We can separately identify both effects because firms with weak balance sheets in the Great Recession may not have been firms with weak balance sheets in previous downturns. In columns (1) and (2), we include as additional controls the elasticity of firm-level employment with respect to aggregate (i.e., total LBD) employment over either a 10-year or 20-year period ending in 2006, $\mathrm{Elasticity}_{\mathrm{Emp,10\text{-}year}}$ and $\mathrm{Elasticity}_{\mathrm{Emp,20\text{-}year}}$, respectively, as well as its interaction with Δ $\mathrm{Log(HP)}_{06-09}$. Columns (3) and (4) are similar, except that we consider the elasticity of firm-level employment with respect to house prices over either a 10-year period ending in 2006 or during the 2002 to 2006 housing boom, $\mathrm{Elasticity}_{\mathrm{HP,10\text{-}year}}$ and $\mathrm{Elasticity}_{\mathrm{HP,02\text{-}06}}$, respectively.[16]

As is shown, the main effect of the elasticity of firm-level employment with respect to either aggregate employment or house prices is significant with the predicted sign:

[15] For example, more highly levered firms may have customers that are more apt to switch to alternative (e.g., cheaper) brands during downturns. In that case, firms with higher leverage may have a higher sensitivity to demand shocks, albeit for reasons unrelated to financial constraints.

[16] Table 6 of the Online Appendix considers the elasticity of firm-level employment with respect to aggregate employment over either a 15-year or 30-year period as well as the elasticity of firm-level employment with respect to either aggregate employment or house prices during the 2001 recession.

firms that had been previously more sensitive to either aggregate employment or house prices also experience greater employment losses during the Great Recession. However, the interaction with Δ Log(HP)$_{06-09}$, while having the predicted sign, is either only weakly significant (column (1)) or insignificant (columns (2) to (4)). Importantly, the main coefficient of interest—that associated with the interaction term Δ Log(HP)$_{06-09}\times$ Leverage$_{06}$—remains significant and stable in all regressions.

V.D. Activist Investors

Table VIII examines if our results are driven by firms being targeted by activist investors, such as activist hedge funds or private equity (PE) funds. The hedge fund data come from an extended version of the data set used in Brav et al. (2008).[17] The data are based on Schedule 13D filings, which investors must file with the SEC within ten days of acquiring more than 5% of any class of securities of a publicly traded company if they have an interest in influencing the management of the company. The PE data are obtained from Thomson Reuter's 13F database, which reports quarterly holdings for all institutional owners with an ownership stake of at least 5%. The names of the owners are matched to the list of PE firms obtained from VentureXpert using a fuzzy matching algorithm. All matches are reviewed by hand to ensure accuracy.

In column (1), we include as additional controls a dummy variable indicating whether a firm is targeted by an activist hedge fund in 2006, Hedge Fund$_{06}$, as well as its interaction with Δ Log(HP)$_{06-09}$. Column (2) is similar, except that we include a dummy variable indicating whether a firm has significant PE ownership in 2006, PE$_{06}$.[18] As can be seen, the main effect of activist investors has the predicted sign—firms with activist investors cut more jobs during the Great Recession—but is either only weakly significant (column (1)) or insignificant (column (2)). The interaction with Δ Log(HP)$_{06-09}$, while having the predicted sign, is always insignificant. Importantly, the coefficient associated with the interaction term Δ Log(HP)$_{06-09}\times$ Leverage$_{06}$, which is our main coefficient of interest, remains significant and stable in both regressions.

[17] We are grateful to Wei Jiang for making the data available to us.

[18] We obtain similar results if we include dummies indicating involvement by activist investors over the 2002 to 2006 period, Hedge Fund$_{02-06}$ and PE$_{02-06}$, respectively.

VI. COUNTY-LEVEL ANALYSIS

In general equilibrium, output and labor may shift from high- to low-leverage firms. The magnitude of this reallocation depends on how much output prices at low-leverage firms decline relative to those at high-leverage firms as well as the substitutability of goods between the firms. If prices are sticky or goods are imperfect substitutes, the magnitude of this reallocation will be limited, with the implication that the distribution of firm leverage matters also in the aggregate. If there is trade across regions, output and labor may not only shift to low-leverage firms but more generally to firms that are less prone to local demand shocks, such as firms in the tradable sector. Again, however, the magnitude of this reallocation will depend on how sticky prices are. Along those lines, Gilchrist et al. (2014), Mian and Sufi (2014a), and Chodorow-Reich (2014) all discuss how price stickiness or imperfect substitutability in the goods market limit the reallocation of output and labor in response to differential firm-level shocks.[19]

The extent of labor reallocation also depends on search and matching frictions as well as labor adjustment costs. Some evidence suggests that labor market frictions were particularly severe during the Great Recession. Davis (2011) and Davis, Faberman, and Haltiwanger (2013) find that both search intensity per unemployed worker and recruiting intensity per vacancy dropped sharply during the Great Recession. Likewise, Şahin et al. (2014) show that mismatch between job seekers and vacant jobs increased markedly during the Great Recession, explaining up to one third of the rise in unemployment. Overall, Foster, Grim, and Haltiwanger (2014) find that the intensity of labor reallocation *fell* rather than rose during the Great Recession, contrary to prior recessions. They conclude that "job reallocation (creation plus destruction) is at its lowest point in 30 years during the Great Recession and its immediate aftermath" (p. 10).

To empirically investigate whether the distribution of firm leverage matters in the aggregate, we turn to county-level regressions. Imagine two counties, one with a smaller (employment-weighted) share of high-leverage firms and the other with a larger share.

[19] Besides, if low-leverage firms engage in labor hoarding (see Section IV), this implies that their own workforce is underutilized—i.e., employed at below full capacity—making it less likely that these firms would hire additional workers from high-leverage firms.

Suppose further that both counties exhibit a similar drop in house prices. If our previous results also hold in the aggregate, then the more highly levered county should experience larger employment losses. By contrast, if the distribution of firm leverage does not matter in the aggregate, then both counties should experience a similar decline in employment, regardless of the level of "county leverage."

We measure county leverage by computing the employment-weighted average value of Leverage_{06} across all firms in our sample in a given county. Importantly, our measure of county leverage is uncorrelated with county-level changes in house prices between 2006 and 2009, Δ Log(HP)_{06-09}: the correlation is close to zero (0.8%) and highly insignificant (p-value 0.809). Accordingly, low- and high-leverage counties experience a similar drop in house prices on average.

Panels (A) and (B) of Figure III plot the percentage change in total county-level employment by all firms in our sample between 2007 and 2009, Δ Log(Emp)_{07-09}, against the percentage change in county-level house prices between 2006 and 2009, Δ Log(HP)_{06-09}, separately for counties in the lowest and highest leverage quartile. For each percentile of Δ Log(HP)_{06-09}, the scatterplot shows the mean values of Δ Log(HP)_{06-09} and Δ Log(Emp)_{07-09}, respectively. As can be seen, Panels (A) and (B) look very similar to Panels (A) and (D) of Figure I. In Panel (A), which depicts the lowest leverage quartile, there is a positive albeit weak relationship between changes in house prices and changes in county-level employment. In Panel (B), which depicts the highest leverage quartile, this relationship is strongly positive: the elasticity of county-level employment with respect to house prices is 0.095, which is more than seven times larger than the elasticity in the lowest quartile and, notably, virtually identical to the corresponding elasticity of 0.096 in our establishment-level analysis (Panel (D) of Figure I).

Table XI confirms this visual impression using regression analysis. To facilitate comparison with our establishment-level regressions, we include as controls the county-level employment shares of all 2-digit NAICS industries in 2006 (see Mian and Sufi (2014a, Table III)). As column (1) shows, the average elasticity of total county-level employment by all firms in our sample with respect to county-level house prices is 0.069 and highly significant. Indeed, this elasticity is very similar to the corresponding elasticity of 0.066

in our establishment-level analysis (column (1) of Table II).[20] Importantly, as column (2) shows, the interaction term Δ Log(HP)$_{06-09}$ $\times$ Leverage$_{06}$ is positive and highly significant, and its coefficient of 0.110 is very similar to the corresponding coefficient of 0.114 in our establishment-level analysis (column (3) of Table II). Thus, more highly levered counties exhibit a significantly larger decline in employment in response to county-wide consumer demand shocks.

Examining changes in total county-level employment by all firms in our sample captures any general equilibrium effects from labor reallocation between firms in our sample in a given county. However, it does not capture labor reallocation between firms in our sample and other firms in the LBD. Given that the latter firms are *more* affected by consumer demand shocks (see Table 5 in the Online Appendix), one would think that labor reallocation toward these firms is rather unlikely. Ultimately, however, this is an empirical question. For this reason, we shall now examine changes in total county-level employment by *all* firms in the LBD.

When examining changes in county-level employment by all LBD firms, one would expect that the differences between low- and high-leverage counties are less pronounced than those in column (2) or, likewise, Panels (A) and (B) of Figure III. This is because our measure of county leverage is based on the leverage of sample firms, which account only for a fraction of total LBD employment (see Section III.F). This introduces measurement error, leading to attenuation bias. That being said, if we (still) find that variation in county leverage can explain variation in county-level employment in response to consumer demand shocks, then this would provide compelling evidence that our establishment-level results are not undone by general equilibrium effects.

In columns (3) and (4), we focus on a subsample of counties in which the attenuation bias is likely to be limited: the top ten percent of counties with the highest employment shares by sample firms. As column (3) shows, the average elasticity of total county-level employment by all LBD firms with respect to county-level house prices is higher than

[20] The average elasticity of 0.066 in column (1) of Table II is estimated without industry fixed effects. The corresponding elasticity with industry fixed effects is 0.068; see column (1) of Panel (A) in Table 5 of the Online Appendix.

the corresponding elasticity in column (1), mirroring results in Table 5 of the Online Appendix showing that firms in the full LBD sample respond more strongly to consumer demand shocks than do firms in the matched Compustat-LBD sample. Moreover, as column (4) shows, the differences between low- and high-leverage counties are less pronounced than those in column (2): the main effect of Δ Log(HP)$_{06-09}$ is stronger, while its interaction with Leverage$_{06}$ is weaker. This is precisely what one would expect in light of the above discussion. Importantly, the coefficient associated with the interaction term Δ Log(HP)$_{06-09}\times$ Leverage$_{06}$ remains large and significant, reaffirming that our establishment-level results are not undone by labor reallocation between firms in our sample and other firms in the LBD.

In columns (5) and (6), we examine changes in total county-level employment by all LBD firms irrespective of the employment shares of sample firms. As column (5) shows, the average elasticity of total county-level employment by all LBD firms with respect to county-level house prices is larger than the corresponding elasticity in column (3). This is because i) the employment shares of sample firms are smaller than the corresponding employment shares in column (3), and ii) firms in the matched Compustat-LBD sample respond less strongly to consumer demand shocks than do firms in full LBD sample. To obtain a visual impression of how this elasticity depends on county leverage, Panels (C) and (D) of Figure III plot the relationship between changes in total county-level employment by all LBD firms, Δ Log(Emp)$_{07-09}$, and changes in county-level house prices, Δ Log(HP)$_{06-09}$, separately for counties in the lowest and highest leverage quartile. As expected, the differences between low- and high-leverage counties are less pronounced than those in Panels (A) and (B) of Figure III. And yet, county leverage matters: the elasticity of county-level employment by all LBD firms with respect to county-level house prices in the highest leverage quartile is about 50% larger than in the lowest leverage quartile (0.149 versus 0.098).

Column (6) of Table XI confirms this impression using regression analysis. Again, as one would expect, the differences between low- and high-leverage counties are less pronounced than those in columns (2) or (4): the main effect of Δ Log(HP)$_{06-09}$ is stronger, while its interaction with Leverage$_{06}$ is weaker. Importantly, however, the coefficient associated with the interaction term Δ Log(HP)$_{06-09}\times$ Leverage$_{06}$ remains large and significant. Thus, our establishment-level results are not undone by general equilibrium effects, implying that the distribution of firm leverage also matters in the aggregate.

VII. POLICY IMPLICATIONS

Our results show that job losses during the Great Recession arise from the interaction between declining consumer demand and weak firm balance sheets.[21] Mayer, Morrison, and Piskorski (2009), Posner and Zingales (2009), Agarwal et al. (2013), and Mian and Sufi (2014b, Chapter 10) all discuss policy measures targeted at (indebted) households facing a collapse in their home values. In what follows, we discuss policy measures targeted at (non-financial) firms.

As discussed in Section IV, financial constraints impair firms' ability to engage in labor hoarding. That is, firms with weak balance sheets would (optimally) like to retain more workers when facing a temporary decline in demand—so as to economize on the costs of hiring, firing, and training workers—but are unable to do so. To explore the role of policy in this context, we would like to draw attention to the case of Germany:

> "Consider, for a moment, a tale of two countries. Both have suffered a severe recession and lost jobs as a result—but not on the same scale. In Country A, employment has fallen more than 5 percent, and the unemployment rate has more than doubled. In Country B, employment has fallen only half a percent, and unemployment is only slightly higher than it was before the crisis. [...] This story isn't hypothetical. Country A is the United States, where stocks are up, G.D.P. is rising, but the terrible employment situation just keeps getting worse. Country B is Germany, which took a hit to its G.D.P. when world trade collapsed, but has been remarkably successful at avoiding mass job losses. Germany's jobs miracle hasn't received much attention in this country—but it's real, it's striking, and it raises serious questions about whether the U.S. government is doing the right things to fight unemployment" (Krugman (2009)).[22]

[21] In principle, our research implies that policy measures could target households, firms, or both. However, if there are diminishing marginal returns to policy intervention, targeting both households and firms is likely to be more effective than targeting either one alone.

[22] In fact, Germany took a much bigger hit to its GDP than did the U.S.: between 2008 and 2009, real GDP growth in Germany fell by 6.7 percentage points compared to 2.5 percentage points in the U.S. (Source: World Bank).

Why did German unemployment barely rise? According to many commentators, a primary reason is massive labor hoarding by German companies (e.g., Dietz, Stops, and Walwei (2010), Balleer et al. (2013), Rinne and Zimmermann (2013)). Important for our discussion, labor hoarding is heavily subsidized in Germany. A central institutional pillar of labor hoarding in Germany is the system of *short-time work* ("Kurzarbeit") programs encouraging firms to adjust labor demand through hours reductions rather than layoffs. Under this system, firms pay workers for the actual hours worked plus an additional 60 to 67 percent of the net income loss due to the hours reduction. Firms are later reimbursed for this additional pay through the unemployment insurance fund administered by the Federal Employment Agency. Hence, workers in a firm reducing hours by, say, 30 percent receive 88 to 90 percent of their original income.

During the Great Recession, the German government further promoted the use of short-time work by gradually expanding the maximum eligibility period from six to 15 months (2007) and later to 24 months (2009). In addition, the Federal Employment Agency assumed half of employers' social security contributions for the first six months of short-time work and the full amount thereafter. While other features of the German labor market—working time accounts, collective agreements, and firm-specific alliances for jobs—were also conducive to labor hoarding, short-time work has been credited to be the most important one (Rinne and Zimmermann (2013)). At the peak of the crisis, almost one in five firms with more than 500 employees was affected by short-time work. In those firms, short-time workers made up 8.8 percent of the total workforce (Brenke, Rinne, and Zimmermann (2013)).

What policy lessons can be drawn from this discussion? Arguably, the success of labor hoarding in Germany depends to some extent on specifics of the German labor market. However, on a broader level, this discussion suggests that it might be useful to think about employment policies that target firms directly besides conventional stimulus. As Krugman (2009) notes, "[h]ere in America, the philosophy behind jobs policy can be summarized as "if you grow it, they will come." That is, we don't really have a jobs policy: we have a G.D.P. policy. [...] Alternatively, or in addition, we could have policies that support private-sector employment. Such policies could range from labor rules that discourage firing to financial incentives for companies that either add workers or reduce hours to avoid layoffs."

In the aftermath of the Great Recession, various employment policy measures have been signed into law. For instance, the Hiring Incentives to Restore Employment (HIRE) Act of 2010 provides tax credits to employers hiring workers who were previously unemployed or only working part time. However, this Act does not support the retention of *existing* workers, which is central to the idea of labor hoarding. The Layoff Prevention Act—part of the Middle Class Tax Relief and Job Creation Act of 2012—updates and clarifies work-sharing provisions in federal law and provides temporary federal funding for states to adopt or expand work-sharing programs within the new federal guidelines. "Work sharing"—also known as "short-time compensation"—has existed in the U.S. since 1978, and 17 states had such programs in place during the Great Recession (27 states as of today). Essentially, they are similar to the German short-time work programs: employees with reduced hours receive pro-rated unemployment benefits to supplement their paychecks. However, take-up rates in the U.S. have been extremely small: participation in work-sharing programs peaked in 2009 at about 153,000 workers, which is just over 0.1 percent of U.S. payroll employment (Baker (2011)). Commentators have identified several reasons for the low take-up rates: financial disincentives for employers and workers, burdensome filing processes, rigid work-sharing schedules that do not allow employers to rotate the pool of affected workers or adjust the volume of hours depending on business activity, and lack of employer outreach by state agency officials. Most of these impediments are still in place today.[23]

Lastly, a word of caution. A potential drawback of German-style subsidies is that, while effective in a crisis situation, they may impede the efficient reallocation of workers in the long run. Labor hoarding may be an optimal response to a *temporary* decline in demand, but it is not a permanent solution. Accordingly, subsidies must be limited in

[23] See Baker (2011), Abraham and Houser (2014), and Wentworth, McKenna, and Minick (2014) for discussions. Employer outreach appears to matter a great deal. An often cited example is Rhode Island, where the state unemployment insurance agency pursued an aggressive marketing campaign during the Great Recession. In 2009, the year in which layoffs peaked, take-up rates in Rhode Island were by far the highest in the country and similar to those in Germany.

scope and used as temporary relief only. In a way, this is what the German government did when it gradually reversed the maximum eligibility period for short-time work beginning in 2010, although some observers argue that this reversal took unduly long due to political pressure and lobbying (Brenke, Rinne, and Zimmermann (2013)).

VIII. CONCLUDING REMARKS

This paper argues that firms' balance sheets play an important role in the propagation of consumer demand shocks during the Great Recession. Using establishment-level employment data from the U.S. Census Bureau, we show that establishments of more highly levered firms exhibit a significantly larger decline in employment in response to a drop in consumer demand. We find similar results when looking at the extensive margin: firms with higher leverage are significantly more likely to close down establishments in response to consumer demand shocks. These results are not driven by high-leverage firms being less productive, having expanded too much prior to the Great Recession, or being generally more sensitive to fluctuations in either aggregate employment or house prices. To examine whether the distribution of firm leverage also matters in the aggregate, we consider county-level employment. Similar to our establishment-level results, we find that counties with more highly levered firms experience significantly larger job losses in response to county-wide consumer demand shocks. Thus, firms' balance sheets also matter for aggregate employment. Our research suggests a possible role for employment policies that target firms directly besides conventional stimulus.

Our results have implications for macroeconomic modeling. In particular, they suggest that a model in which households', firms', and financial intermediaries' balance sheets interact might be a useful way to think about the Great Recession. Accordingly, falling house prices may erode the balance sheets of households, leading to a decline in aggregate consumer demand. The latter disproportionately affects firms with weak balance sheets, forcing them to downsize and reduce employment, as shown in this paper. At the same time, falling house prices may erode the balance sheets of financial intermediaries, impairing their capital and access to funding and therefore their ability or willingness to lend.

This tightening of lending standards in turn disproportionately affects firms with weak balance sheets ("flight to quality"), thereby reinforcing the adverse effects of aggregate consumer demand shocks.

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Figure I
Changes in House Prices and Employment at the Establishment Level

The figure plots the percentage change in establishment-level employment between 2007 and 2009, Δ Log(Emp)$_{07\text{-}09}$, against the percentage change in house prices in the establishment's ZIP code or county between 2006 and 2009, Δ Log(HP)$_{06\text{-}09}$, for various quartiles of firm leverage. For each percentile of Δ Log(HP)$_{06\text{-}09}$, the scatterplot depicts the mean values of Δ Log(HP)$_{06\text{-}09}$ and Δ Log(Emp)$_{07\text{-}09}$, respectively.

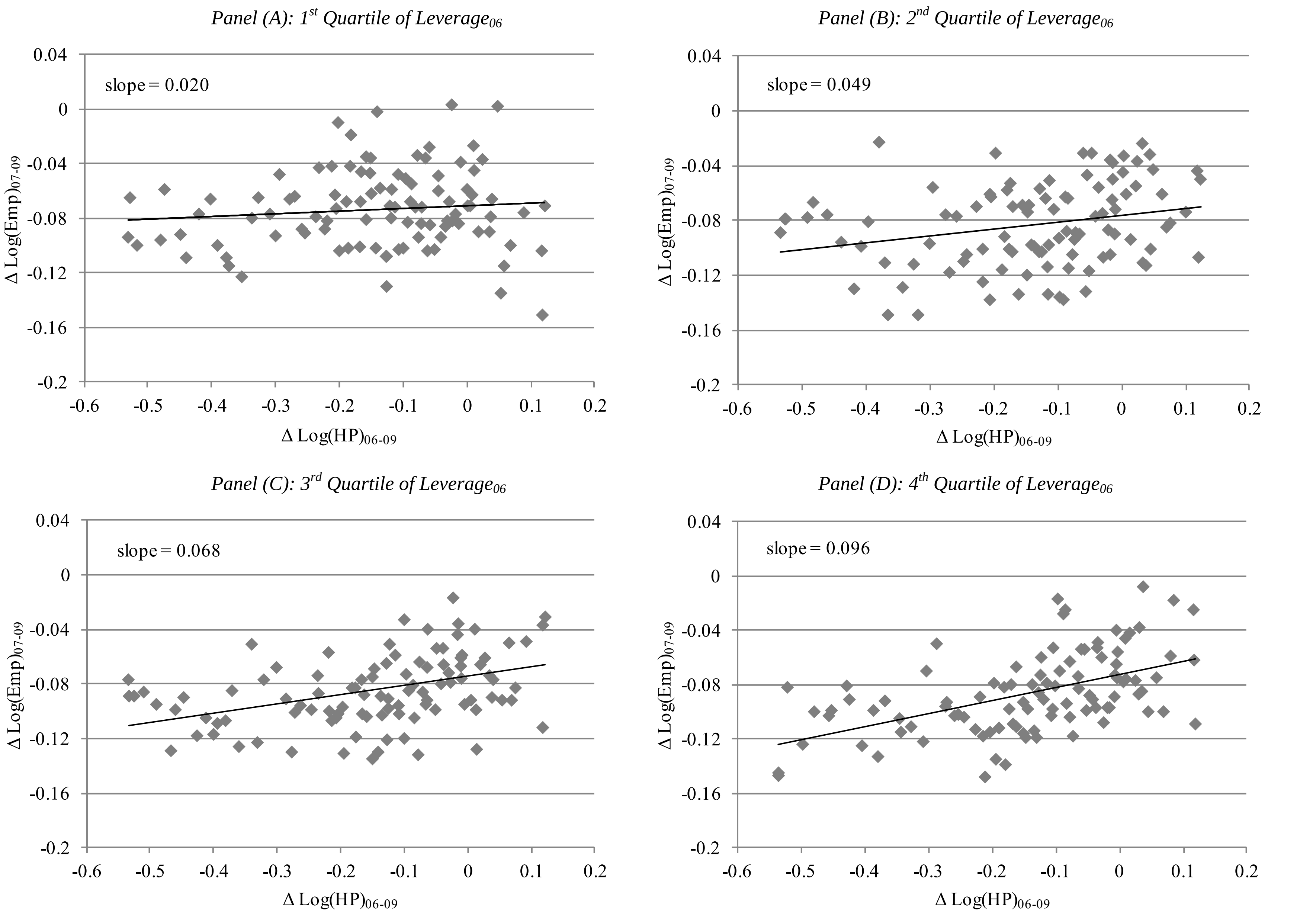
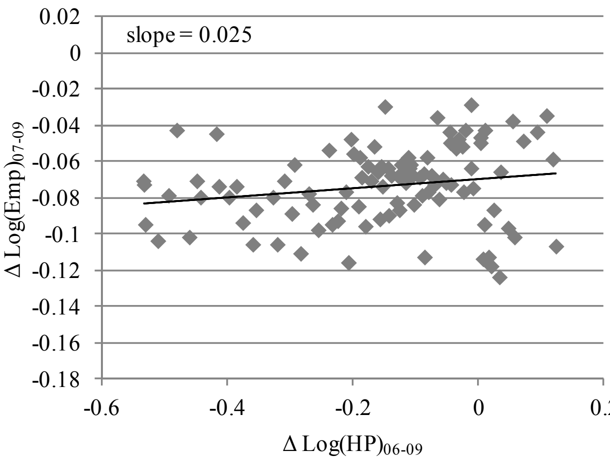


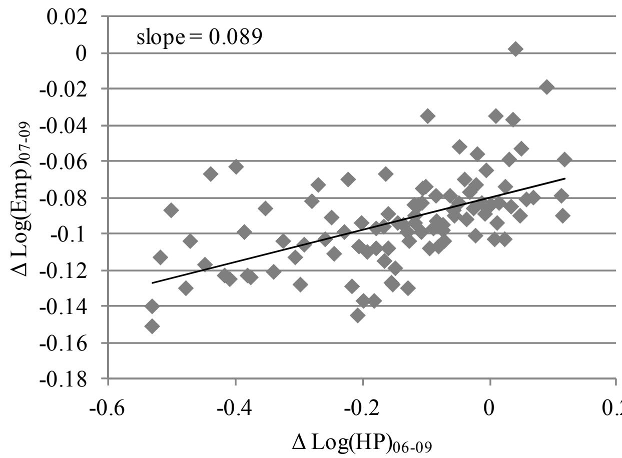
Figure II
Industry Sectors

The plots are similar to those in Panels (A) and (D) of Figure I, except that they pertain to non-tradable, “other,” and tradable industries.

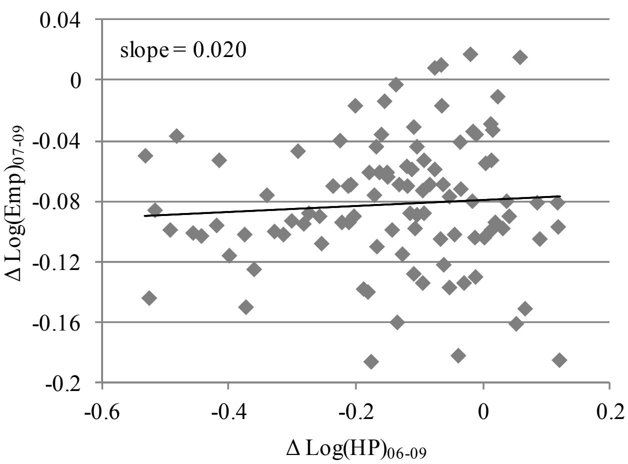
Panel (A): 1st Quartile of Leverage06 (Non-Tradable)



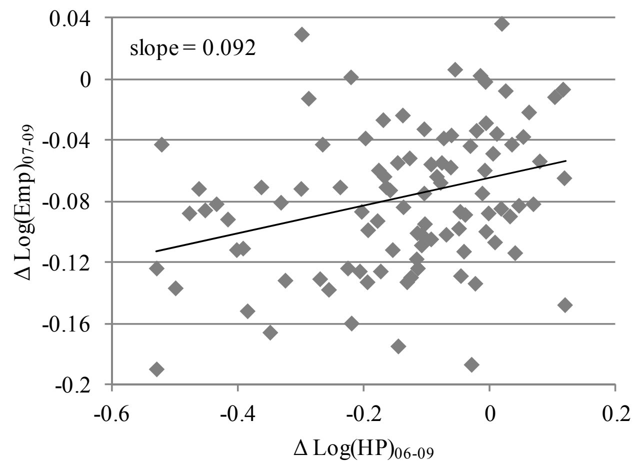
Panel (B): 4th Quartile of Leverage06 (Non-Tradable)



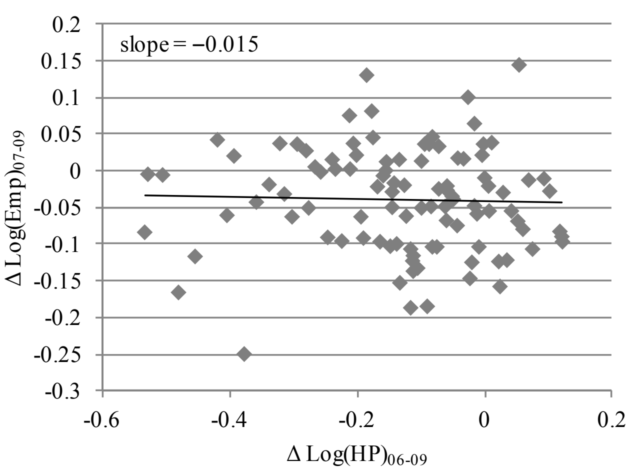
Panel (C): 1st Quartile of Leverage06 (Other)



Panel (D): 4th Quartile of Leverage06 (Other)



Panel (E): 1st Quartile of Leverage06 (Tradable)



Panel (F): 4th Quartile of Leverage06 (Tradable)

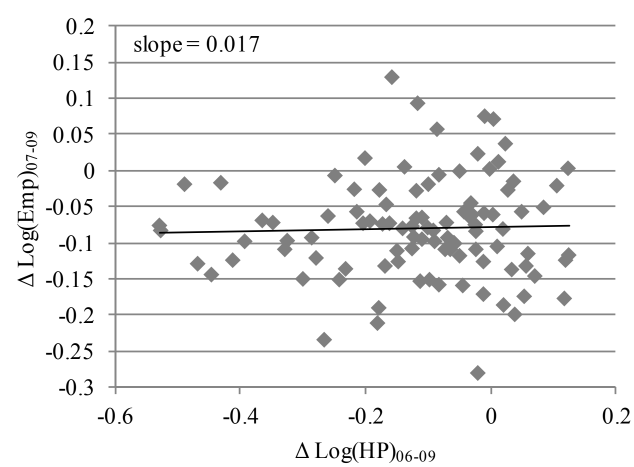
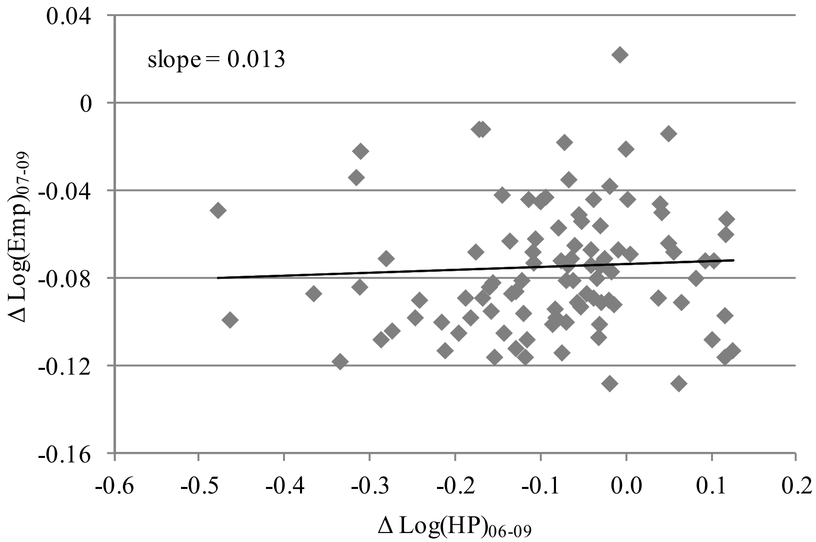


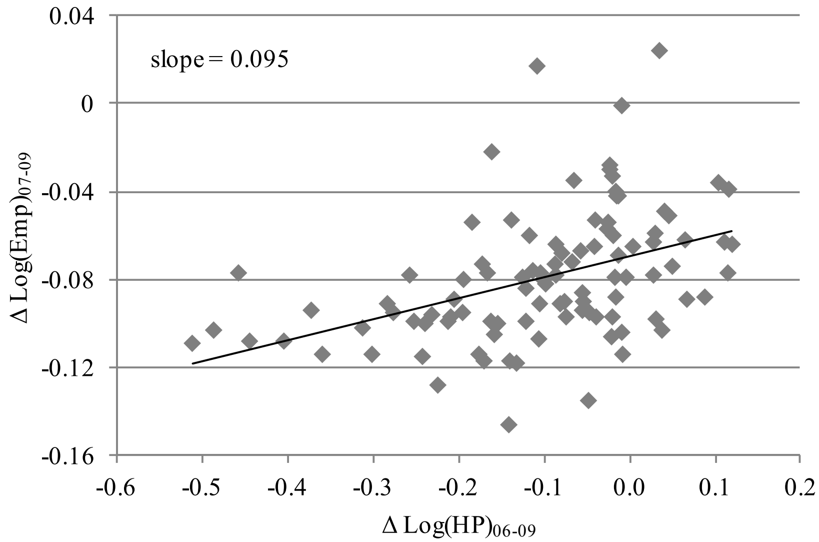
Figure III
Changes in House Prices and Employment at the County Level

The plots are similar to those in Panels (A) and (D) of Figure I, except that the analysis is at the county level. County-level leverage is based on the employment-weighted average value of firm leverage across all sample firms in a county. In Panels (A) and (B), county-level employment is total employment by all sample firms in a county. In Panels (C) and (D), county-level employment is total employment by all LBD firms in a county.

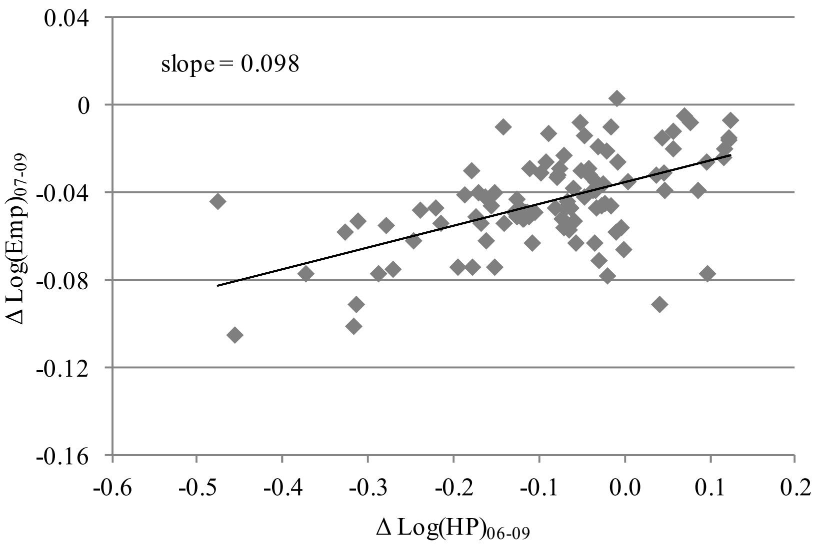
Panel (A): 1st Quartile of Leverage$_{06}$



Panel (B): 4th Quartile of Leverage$_{06}$



Panel (C): 1st Quartile of Leverage$_{06}$



Panel (D): 4th Quartile of Leverage$_{06}$

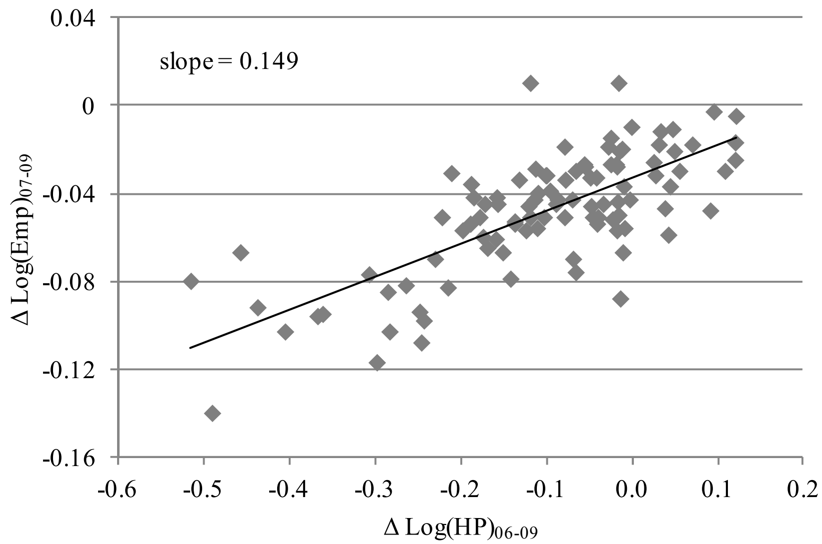


Table I
Summary Statistics

Panel (A) provides summary statistics at the establishment level. $Leverage_{06}$ is the ratio of the sum of debt in current liabilities and long-term debt to total assets associated with the establishment's parent firm in 2006. Δ $Log(Emp)_{07\text{-}09}$ is the percentage change in establishment-level employment from 2007 to 2009. Δ $Log(HP)_{06\text{-}09}$ is the percentage change in house prices in the establishment's ZIP code or county (if the ZIP code information is missing) from 2006 to 2009. Housing Supply Elasticity is described in Saiz (2010). Tradable and non-tradable industries are described in Mian and Sufi (2014a). "Other" industries are those that are neither tradable nor non-tradable. Panels (B) and (C) provide summary statistics at the firm level. Assets is the book value of total assets. ROA (return on assets) is the ratio of operating income before depreciation to total assets. NPM (net profit margin) is the ratio of operating income before depreciation to sales. TFP (total factor productivity) is the residual from a regression of log(sales) on log(employees) and log(PP&E) across all Compustat firms in the same 2-digit SIC industry. WW is the financial constraints index of Whited and Wu (2006). KZ is the financial constraints index of Kaplan and Zingales (1997). *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

Panel (A): Establishment Level (N=284,800)

	Mean	Std. Dev.	Correlation with $Leverage_{06}$	*p*-value of correlation
$Employees_{06}$	39	63	-0.028	0.283
Δ $Log(Emp)_{07\text{-}09}$	-8.2	24.2	-0.047**	0.020
Δ $Log(HP)_{06\text{-}09}$	-14.5	16.1	0.005	0.718
Housing Supply Elasticity	1.799	0.927	0.011	0.345
Census Regions				
Northeast	0.17	0.38	0.004	0.801
Midwest	0.21	0.41	-0.006	0.610
South	0.38	0.49	0.002	0.894
West	0.24	0.42	-0.000	0.978
Industry Sectors				
Tradable	0.03	0.18	0.001	0.954
Non-Tradable	0.44	0.5	-0.146**	0.014
Other	0.53	0.5	0.145**	0.013

Table I
(Continued)

Panel (B): Firm Level 2006 (N=2,800)

	Mean	Std. Dev.	Correlation with $Leverage_{06}$	*p* -value of correlation
$Establishments_{06}$	101	451	-0.015	0.495
$Employees_{06}$	4,005	16,384	-0.008	0.191
$Assets_{06}$	3,040	18,515	-0.003	0.655
ROA_{06}	0.045	0.177	-0.073***	0.003
NPM_{06}	0.024	0.28	-0.041**	0.032
TFP_{06}	-0.002	0.599	-0.083***	0.004
$Leverage_{06}$	0.227	0.253	1.000***	0.000
WW_{06}	-0.251	0.135	0.189***	0.000
KZ_{06}	-4.067	44.295	0.259***	0.000

Panel (C): Firm Level 2002-2006 (N=2,800)

	Mean	Std. Dev.	Correlation with $Leverage_{06}$	*p* -value of correlation
Δ $Establishments_{02\text{-}06}$	4.4	10.2	0.089***	0.000
Δ $Log(Emp)_{02\text{-}06}$	0.052	0.093	0.048***	0.009
Δ $Log(Assets)_{02\text{-}06}$	0.110	0.133	0.087***	0.000
Δ $ROA_{02\text{-}06}$	0.022	0.127	-0.061***	0.003
Δ $NPM_{02\text{-}06}$	0.020	0.225	-0.032**	0.015
Δ $TFP_{02\text{-}06}$	-0.001	0.569	-0.017	0.649
Δ $Leverage_{02\text{-}06}$	-0.023	0.153	0.379***	0.000
Δ $WW_{02\text{-}06}$	-0.006	0.08	0.085***	0.000
Δ $KZ_{02\text{-}06}$	-0.370	49.633	0.188***	0.000

Table II
Firm Leverage, Consumer Demand, and Unemployment

The dependent variable, Δ $Log(Emp)_{07\text{-}09}$, is the percentage change in establishment-level employment from 2007 to 2009. Δ $Log(HP)_{06\text{-}09}$ is the percentage change in house prices in the establishment's ZIP code or county (if the ZIP code information is missing) from 2006 to 2009. $Leverage_{06}$ is the ratio of the sum of debt in current liabilities and long-term debt to total assets associated with the establishment's parent firm in 2006. Industry fixed effects are based on 4-digit NAICS codes. All regressions are weighted by the size of establishments, i.e., their number of employees. Standard errors (in parentheses) are clustered at both the state and firm level. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Δ $Log(Emp)_{07\text{-}09}$						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Δ $Log(HP)_{06\text{-}09}$	0.066***	0.029	0.029		0.027		
	(0.019)	(0.022)	(0.019)		(0.019)		
Δ $Log(HP)_{06\text{-}09}$ × $Leverage_{06}$		0.111***	0.114***	0.113***	0.084**	0.076**	0.075**
		(0.039)	(0.040)	(0.038)	(0.035)	(0.031)	(0.038)
$Leverage_{06}$		-0.028**	-0.032**	-0.020**			
		(0.014)	(0.015)	(0.009)			
Industry Fixed Effects	No	No	Yes	Yes	Yes	Yes	-
Firm Fixed Effects	No	No	No	No	Yes	Yes	Yes
ZIP Code Fixed Effects	No	No	No	Yes	No	Yes	-
ZIP Code × Industry Fixed Effects	No	No	No	No	No	No	Yes
R-squared	0.00	0.00	0.04	0.13	0.17	0.25	0.31
Observations	284,800	284,800	284,800	284,800	284,800	284,800	284,800

Table III
Other Measures of Debt Capacity

This table presents variants of the regressions in Table II in which $Leverage_{06}$ is replaced by other measures of debt capacity. Net leverage is the ratio of debt in current liabilities plus long-term debt minus cash and short-term investments divided by total assets. Market leverage is the ratio of debt in current liabilities plus long-term debt divided by total assets minus the book value of equity plus the market value of equity (stock price multiplied by the number of shares outstanding). Debt to EBITDA is the ratio of debt in current liabilities plus long-term debt divided by operating income before depreciation. Interest coverage is the ratio of interest expense to operating income after depreciation. Δ $Leverage_{02\text{-}06}$ is the change in firm leverage from 2002 to 2006. WW and KZ are the financial constraints indices of Whited and Wu (2006) and Kaplan and Zingales (1997), respectively. Δ Leverage, WW, and KZ are net of their respective minimum values. Industry fixed effects are based on 4-digit NAICS codes. All regressions are weighted by the size of establishments, i.e., their number of employees. Standard errors (in parentheses) are clustered at both the state and firm level. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Net $Leverage_{06}$	Market $Leverage_{06}$	Debt to $EBITDA_{06}$	Interest $Coverage_{06}$	Δ $Leverage_{02\text{-}06}$	WW_{06}	KZ_{06}
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Δ $Log(HP)_{06\text{-}09}$	0.029	0.032	0.036*	0.040*	0.011	0.027	0.029
	(0.020)	(0.019)	(0.019)	(0.020)	(0.033)	(0.023)	(0.021)
Δ $Log(HP)_{06\text{-}09}$ × Debt Capacity	0.120***	0.130***	0.012***	0.127**	0.223***	0.059***	0.003**
	(0.041)	(0.044)	(0.004)	(0.056)	(0.070)	(0.015)	(0.002)
Debt Capacity	-0.038**	-0.054***	-0.003**	-0.063***	-0.038*	-0.009**	-0.003***
	(0.016)	(0.018)	(0.001)	(0.024)	(0.022)	(0.004)	(0.000)
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.04	0.04	0.04	0.04	0.04	0.04	0.04
Observations	284,800	284,800	284,800	284,800	284,800	284,800	284,800

Table IV
Industry Sectors

Panel (A) presents variants of the regressions in Table II in which the sample is partitioned into non-tradable, "other," and tradable industries. Tradable and non-tradable industries are described in Mian and Sufi (2014a). "Other" industries are those that are neither tradable nor non-tradable. Panel (B) lists the top ten industries in which house prices have the largest impact on establishment-level employment—i.e., those with the highest coefficients of Δ Log(HP)$_{06\text{-}09}$—based on estimating column (1) of Table II separately for each 4-digit NAICS industry. All regressions are weighted by the size of establishments, i.e., their number of employees. Standard errors (in parentheses) are clustered at both the state and firm level. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

Panel (A): Regressions by Industry Sector

	Δ Log(Emp)$_{07\text{-}09}$					
	Non-tradable	Other	Tradable	Non-tradable	Other	Tradable
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Log(HP)$_{06\text{-}09}$	0.074**	0.075***	0.009	0.029	0.030	-0.015
	(0.035)	(0.012)	(0.019)	(0.019)	(0.024)	(0.043)
Δ Log(HP)$_{06\text{-}09}$ × Leverage$_{06}$				0.131***	0.122***	0.037
				(0.034)	(0.047)	(0.120)
Leverage$_{06}$				-0.038**	-0.028**	-0.026
				(0.015)	(0.012)	(0.020)
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.04	0.04	0.03	0.04	0.04	0.03
Observations	124,100	150,800	9,900	124,100	150,800	9,900

Table IV
(Continued)

Panel (B): Industries in Which Consumer Demand Shocks Have the Biggest Impact on Employment

4-digit NAICS	NAICS Description	Sector
7221	Full-Service Restaurants	Non-tradable
4441	Building Material and Supplies Dealers	Other
4461	Health and Personal Care Stores	Non-tradable
8111	Automotive Repair and Maintenance	Other
4539	Other Miscellaneous Store Retailers	Non-tradable
4431	Electronics and Appliance Stores	Non-tradable
4413	Automotive Parts, Accessories, and Tire Stores	Non-tradable
4411	Automobile Dealers	Non-tradable
4482	Shoe Stores	Non-tradable
3273	Cement and Concrete Product Manufacturing	Other

Table V
Establishment Closures

This table presents variants of the regressions in Table II in which the sample also includes establishments that were closed between 2007 and 2009. The dependent variable is a dummy indicating whether an establishment is closed during that period. Industry fixed effects are based on 4-digit NAICS codes. All regressions are weighted by the size of establishments, i.e., their number of employees. Standard errors (in parentheses) are clustered at both the state and firm level. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Establishment Closure$_{07\text{-}09}$	
	(1)	(2)
Δ Log(HP)$_{06\text{-}09}$	-0.009***	-0.004
	(0.002)	(0.003)
Δ Log(HP)$_{06\text{-}09}$ $\times$ Leverage$_{06}$		-0.029**
		(0.011)
Leverage$_{06}$		0.043***
		(0.007)
Industry Fixed Effects	Yes	Yes
R-squared	0.02	0.03
Observations	338,100	338,100

Table VI
Firm-Level Analysis

This table presents firm-level analogues of the regressions in Table II. Short-Term (ST) Debt is the ratio of debt in current liabilities divided by total assets. Long-Term (LT) debt is the ratio of long-term debt divided by total assets. Equity is the ratio of the book value of equity divided by total assets. Establishment (Est.) closure is the number of establishments closed between 2007 and 2009 divided by the number of establishments in 2007. CAPEX is the ratio of capital expenditures divided by property, plant and equipment (PP&E). Δ Log(HP)$_{06\text{-}09}$ is aggregated at the firm level by computing the employment-weighted average value of Δ Log(HP)$_{06\text{-}09}$ across all of the firm's establishments. Industry fixed effects are based on 4-digit NAICS codes. Standard errors (in parentheses) are clustered at the state level. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	External Finance			Employment and Investment		
	Δ ST Debt$_{07\text{-}09}$	Δ LT Debt$_{07\text{-}09}$	Δ Equity$_{07\text{-}09}$	Δ Log(Emp)$_{07\text{-}09}$	Est. Closure$_{07\text{-}09}$	Δ CAPEX$_{07\text{-}09}$
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Log(HP)$_{06\text{-}09}$	-0.025**	-0.040**	0.005	0.020	-0.008	0.002
	(0.011)	(0.019)	(0.037)	(0.033)	(0.015)	(0.005)
Δ Log(HP)$_{06\text{-}09}$ × Leverage$_{06}$	0.035**	0.059**	-0.011	0.122***	-0.046**	0.014**
	(0.014)	(0.021)	(0.047)	(0.040)	(0.019)	(0.007)
Leverage$_{06}$	-0.011*	-0.019*	0.009	-0.024**	0.018**	-0.005*
	(0.006)	(0.011)	(0.021)	(0.010)	(0.008)	(0.003)
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.09	0.08	0.03	0.11	0.11	0.14
Observations	2,800	2,800	2,800	2,800	2,800	2,800

Table VII
Employment and Asset Growth

This table presents variants of the regressions in Table II in which a "horse race" is performed between $Leverage_{06}$ and either employment or asset growth. Δ $Log(Emp)_{02\text{-}06}$ is the growth in firm-level employment from 2002 to 2006. Δ $Log(Assets)_{02\text{-}06}$ is the growth in firm-level assets from 2002 to 2006. Industry fixed effects are based on 4-digit NAICS codes. All regressions are weighted by the size of establishments, i.e., their number of employees. Standard errors (in parentheses) are clustered at both the state and firm level. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Δ $Log(Emp)_{07\text{-}09}$	
	(1)	(2)
Δ $Log(HP)_{06\text{-}09}$	0.026	0.024
	(0.024)	(0.019)
Δ $Log(HP)_{06\text{-}09} \times Leverage_{06}$	0.111**	0.113***
	(0.047)	(0.040)
Δ $Log(HP)_{06\text{-}09} \times \Delta$ $Log(Emp)_{02\text{-}06}$	0.027	
	(0.034)	
Δ $Log(HP)_{06\text{-}09} \times \Delta$ $Log(Assets)_{02\text{-}06}$		0.012
		(0.009)
$Leverage_{06}$	-0.033**	-0.033**
	(0.016)	(0.015)
Δ $Log(Emp)_{02\text{-}06}$	0.008	
	(0.018)	
Δ $Log(Assets)_{02\text{-}06}$		0.005
		(0.003)
Industry Fixed Effects	Yes	Yes
R-squared	0.04	0.04
Observations	284,800	284,800

Table VIII
Productivity

This table presents variants of the regressions in Table II in which a "horse race" is performed between $Leverage_{06}$ and productivity measures at the firm level, all in 2006. ROA_{06} (return on assets) is the ratio of operating income before depreciation to total assets. NPM_{06} (net profit margin) is the ratio of operating income before depreciation to sales. TFP_{06} (total factor productivity) is the residual from a regression of Log(Sales) on Log(Employees) and Log(PP&E) across all Compustat firms in the same 2-digit SIC industry. Industry fixed effects are based on 4-digit NAICS codes. All regressions are weighted by the size of establishments, i.e., their number of employees. Standard errors (in parentheses) are clustered at both the state and firm level. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Δ Log(Emp)$_{07\text{-}09}$		
	(1)	(2)	(3)
Δ Log(HP)$_{06\text{-}09}$	0.030	0.031	0.031
	(0.019)	(0.035)	(0.019)
Δ Log(HP)$_{06\text{-}09}$ × $Leverage_{06}$	0.101**	0.122***	0.108***
	(0.040)	(0.041)	(0.039)
Δ Log(HP)$_{06\text{-}09}$ × ROA_{06}	-0.024		
	(0.110)		
Δ Log(HP)$_{06\text{-}09}$ × NPM_{06}		-0.038	
		(0.161)	
Δ Log(HP)$_{06\text{-}09}$ × TFP_{06}			-0.049*
			(0.026)
$Leverage_{06}$	-0.034**	-0.036**	-0.029**
	(0.015)	(0.016)	(0.014)
ROA_{06}	0.133***		
	(0.029)		
NPM_{06}		0.149***	
		(0.030)	
TFP_{06}			0.027*
			(0.016)
Industry Fixed Effects	Yes	Yes	Yes
R-squared	0.04	0.04	0.04
Observations	284,800	284,800	284,800

Table IX
Sensitivity to Aggregate Employment and House Prices

This table presents variants of the regressions in Table II in which a "horse race" is performed between $Leverage_{06}$ and the elasticity of firm-level employment with respect to either aggregate employment or house prices. The elasticity with respect to aggregate employment is computed by estimating a firm-level regression of Δ Log (Employment) on a constant and Δ Log (LBD Employment) using all available years from 1997 to 2006 ($Elasticity_{Emp,10\text{-}year}$) and 1987 to 2006 ($Elasticity_{Emp,20\text{-}year}$), respectively. The elasticity with respect to house prices is computed as the employment-weighted average elasticity of employment with respect to house prices across all of the firm's establishments. The latter is computed either by estimating an establishment-level regression of Δ Log(Employment) on a constant and Δ Log(HP) using all available years from 1997 to 2006 ($Elasticity_{HP,10\text{-}year}$) or as the percentage change in establishment-level employment divided by the percentage change in house prices during the 2002 to 2006 housing boom ($Elasticity_{HP,02\text{-}06}$). Industry fixed effects are based on 4-digit NAICS codes. All regressions are weighted by the size of establishments, i.e., their number of employees. Standard errors (in parentheses) are clustered at both the state and firm level. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Δ Log(Emp)$_{07\text{-}09}$			
	(1)	(2)	(3)	(4)
Δ Log(HP)$_{06\text{-}09}$	0.027	0.027	0.029	0.029
	(0.019)	(0.019)	(0.020)	(0.019)
Δ Log(HP)$_{06\text{-}09}$ × $Leverage_{06}$	0.109**	0.110**	0.119***	0.120***
	(0.044)	(0.044)	(0.044)	(0.044)
Δ Log(HP)$_{06\text{-}09}$ × $Elasticity_{Emp,10\text{-}year}$	0.006*			
	(0.004)			
Δ Log(HP)$_{06\text{-}09}$ × $Elasticity_{Emp,20\text{-}year}$		0.005		
		(0.004)		
Δ Log(HP)$_{06\text{-}09}$ × $Elasticity_{HP,10\text{-}year}$			0.006	
			(0.008)	
Δ Log(HP)$_{06\text{-}09}$ × $Elasticity_{HP,02\text{-}06}$				0.007
				(0.008)
$Leverage_{06}$	-0.027*	-0.028*	-0.030**	-0.030**
	(0.016)	(0.016)	(0.016)	(0.016)
$Elasticity_{Emp,10\text{-}year}$	-0.005***			
	(0.002)			
$Elasticity_{Emp,20\text{-}year}$		-0.005***		
		(0.002)		
$Elasticity_{HP,10\text{-}year}$			-0.004*	
			(0.002)	
$Elasticity_{HP,02\text{-}06}$				-0.004*
				(0.002)
Industry Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.04	0.04	0.04	0.04
Observations	284,800	284,800	284,800	284,800

Table X
Activist Investors

This table presents variants of the regressions in Table II in which a "horse race" is performed between Leverage_{06} and whether a firm has activist investors. Hedge Fund_{06} is a dummy indicating whether a firm is targeted by an activist hedge fund in 2006. Private Equity_{06} is a dummy indicating whether a firm has significant (i.e., more than 5%) private equity ownership in 2006. Industry fixed effects are based on 4-digit NAICS codes. All regressions are weighted by the size of establishments, i.e., their number of employees. Standard errors (in parentheses) are clustered at both the state and firm level. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Δ Log(Emp)$_{07\text{-}09}$	
	(1)	(2)
Δ Log(HP)$_{06\text{-}09}$	0.032	0.029
	(0.019)	(0.019)
Δ Log(HP)$_{06\text{-}09}$ × Leverage$_{06}$	0.110***	0.115***
	(0.039)	(0.040)
Δ Log(HP)$_{06\text{-}09}$ × Hedge Fund$_{06}$	0.036	
	(0.036)	
Δ Log(HP)$_{06\text{-}09}$ × Private Equity$_{06}$		0.009
		(0.056)
Leverage$_{06}$	-0.032**	-0.032**
	(0.015)	(0.015)
Hedge Fund$_{06}$	-0.015*	
	(0.008)	
Private Equity$_{06}$		-0.015
		(0.011)
Industry Fixed Effects	Yes	Yes
R-squared	0.04	0.04
Observations	284,800	284,800

Table XI
County-Level Analysis

This table presents county-level analogues of the regressions in Table II. County-level leverage is the employment-weighted average value of $Leverage_{06}$ across all sample firms in a county. In columns (1) and (2), county-level employment is total employment by all sample firms in a county. In columns (3) to (6), county-level employment is total employment by all LBD firms in a county. In columns (3) and (4), the sample is restricted to the top ten percent of counties with the highest employment share by sample firms. All regressions include the employment shares of all 23 two-digit NAICS industries as controls. Standard errors (in parentheses) are clustered at the state level. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Δ Log(Emp)$_{07\text{-}09}$					
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Log(HP)$_{06\text{-}09}$	0.069***	0.025	0.080***	0.049*	0.104***	0.085***
	(0.008)	(0.019)	(0.010)	(0.027)	(0.005)	(0.015)
Δ Log(HP)$_{06\text{-}09}$ × $Leverage_{06}$		0.110***		0.082**		0.045**
		(0.038)		(0.042)		(0.022)
$Leverage_{06}$		-0.029**		-0.028*		-0.023**
		(0.013)		(0.015)		(0.010)
Industry Controls	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.13	0.13	0.20	0.20	0.14	0.14
Observations	1,000	1,000	100	100	1,000	1,000

Online Appendix

Table 1
Matching House Prices to Establishments

This table presents variants of the regressions in Table II. In Panel (A), the sample is restricted to establishments with non-missing ZIP code-level house prices. In panel (B), establishments with missing ZIP code- or county-level house prices are assigned state-level house prices computed as population-weighted averages of available ZIP code-level house prices. Industry fixed effects are based on 4-digit NAICS codes. All regressions are weighted by the size of establishments, i.e., their number of employees. Standard errors (in parentheses) are clustered at both the state and firm level. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

Panel (A): Sample with Non-Missing ZIP Code- or County-Level House Prices

	Δ Log(Emp)$_{07\text{-}09}$	
Δ Log(HP)$_{06\text{-}09}$	0.064***	0.024
	(0.021)	(0.023)
Δ Log(HP)$_{06\text{-}09}$ × Leverage$_{06}$		0.119***
		(0.037)
Leverage$_{06}$		-0.026**
		(0.013)
Industry Fixed Effects	Yes	Yes
R-squared	0.04	0.04
Observations	227,600	227,600

Panel (B): Expanded Sample with Imputed State-Level House Prices

	Δ Log(Emp)$_{07\text{-}09}$	
Δ Log(HP)$_{06\text{-}09}$	0.079***	0.030
	(0.017)	(0.018)
Δ Log(HP)$_{06\text{-}09}$ × Leverage$_{06}$		0.129***
		(0.039)
Leverage$_{06}$		-0.022*
		(0.014)
Industry Fixed Effects	Yes	Yes
R-squared	0.02	0.02
Observations	327,500	327,500

Table 2
Geographical Concentration Index

This table presents variants of the regressions in Table IV in which the sample is partitioned into non-tradable, "other," and tradable industries based on the geographical concentration (GC) index of Mian and Sufi (2014a). Industries in the top quartile of the GC index are classified as tradable; those in the bottom quartile are classified as non-tradable. Industries in the second and third quartiles of the GC index are classified as "other." Industry fixed effects are based on 4-digit NAICS codes. All regressions are weighted by the size of establishments, i.e., their number of employees. Standard errors (in parentheses) are clustered at both the state and firm level. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Δ Log(Emp)$_{07\text{-}09}$					
	Non-Tradable	Other	Tradable	Non-Tradable	Other	Tradable
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Log(HP)$_{06\text{-}09}$	0.081***	0.048***	0.003	0.033	0.027	-0.007
	(0.031)	(0.017)	(0.039)	(0.031)	(0.121)	(0.055)
Δ Log(HP)$_{06\text{-}09}$ × Leverage$_{06}$				0.133***	0.103**	0.031
				(0.040)	(0.050)	(0.117)
Leverage$_{06}$				-0.036**	-0.021**	-0.032*
				(0.016)	(0.009)	(0.017)
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.07	0.02	0.08	0.07	0.02	0.08
Observations	130,700	138,200	15,800	130,700	138,200	15,800

Table 3
Excluding Tradable Industries

This table presents variants of the regressions in Table II in which tradable industries are excluded. Industry fixed effects are based on 4-digit NAICS codes. All regressions are weighted by the size of establishments, i.e., their number of employees. Standard errors (in parentheses) are clustered at both the state and firm level. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Δ Log(Emp)$_{07\text{-}09}$	
	(1)	(2)
Δ Log(HP)$_{06\text{-}09}$	0.071***	0.033
	(0.018)	(0.020)
Δ Log(HP)$_{06\text{-}09}$ $\times$ Leverage$_{06}$		0.120***
		(0.041)
Leverage$_{06}$		-0.031**
		(0.015)
Industry Fixed Effects	Yes	Yes
R-squared	0.04	0.04
Observations	274,900	274,900

Table 4
Instrumenting House Price Changes

This table presents variants of the regressions in Table II in which Δ $Log(HP)_{06\text{-}09}$ is instrumented with housing supply elasticity (columns (2) to (4)) and "share of unavailable land" (columns (6) to (8)), respectively. Both instruments are described in Saiz (2010). For brevity, the table only displays the first-stage regressions associated with columns (2) and (5). Industry fixed effects are based on 4-digit NAICS codes. All regressions are weighted by the size of establishments, i.e., their number of employees. Standard errors (in parentheses) are clustered at both the state and firm level. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Instrument: Housing Supply Elasticity			Instrument: Share of Unavailable Land		
	Δ $Log(HP)_{06\text{-}09}$	Δ $Log(Emp)_{07\text{-}09}$		Δ $Log(HP)_{06\text{-}09}$	Δ $Log(Emp)_{07\text{-}09}$	
	First Stage	IV		First Stage	IV	
	(1)	(2)	(3)	(4)	(5)	(6)
Housing Supply Elasticity	0.073***					
	(0.017)					
Share of Unavailable Land				-0.304***		
				(0.086)		
Δ $Log(HP)_{06\text{-}09}$		0.080***	0.036		0.078***	0.035
		(0.021)	(0.022)		(0.020)	(0.020)
Δ $Log(HP)_{06\text{-}09} \times Leverage_{06}$			0.130**			0.130**
			(0.052)			(0.054)
$Leverage_{06}$			-0.032**			-0.033**
			(0.017)			(0.017)
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.21	0.03	0.04	0.17	0.03	0.04
Observations	247,800	247,800	247,800	247,800	247,800	247,800

Table 5
Compustat-LBD Sample versus Full LBD Sample

This table presents variants of the regressions in Tables II and IV. In Panel (A), the sample is the matched Compustat-LBD sample. In Panel (B), the sample is the full LBD sample. Industry fixed effects are based on 4-digit NAICS codes. All regressions are weighted by the size of establishments, i.e., their number of employees. Standard errors (in parentheses) are clustered at both the state and firm level. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

Panel (A): Matched Compustat-LBD Sample

	Δ Log(Emp)$_{07\text{-}09}$			
	All	Non-tradable	Other	Tradable
	(1)	(2)	(3)	(4)
Δ Log(HP)$_{06\text{-}09}$	0.068***	0.074**	0.075***	0.009
	(0.018)	(0.035)	(0.012)	(0.019)
Industry Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.03	0.04	0.04	0.03
Observations	284,800	124,100	150,800	9,900

Panel (B): Full LBD Sample

	Δ Log(Emp)$_{07\text{-}09}$			
	All	Non-tradable	Other	Tradable
	(1)	(2)	(3)	(4)
Δ Log(HP)$_{06\text{-}09}$	0.108***	0.120***	0.110***	0.008
	(0.018)	(0.011)	(0.024)	(0.024)
Industry Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.04	0.02	0.04	0.04
Observations	4,542,300	910,300	3,449,600	182,400

Table 6
Sensitivity to Aggregate Employment and House Prices: Robustness

This table presents variants of the regressions in Table IX. The elasticity of firm-level employment with respect to aggregate employment is computed over either a 15-year period ($\text{Elasticity}_{\text{Emp,15-year}}$), 30-year period ($\text{Elasticity}_{\text{Emp,30-year}}$), or as the percentage change in firm-level employment divided by the percentage change in aggregate employment during the 2001 recession ($\text{Elasticity}_{\text{Emp,2001}}$). The elasticity of firm-level employment with respect to house prices is computed as the employment-weighted average percentage change in establishment-level employment divided by the percentage change in house prices during the 2001 recession ($\text{Elasticity}_{\text{HP,2001}}$). Industry fixed effects are based on 4-digit NAICS codes. All regressions are weighted by the size of establishments, i.e., their number of employees. Standard errors (in parentheses) are clustered at both the state and firm level. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Δ Log(Emp)$_{07\text{-}09}$			
	(1)	(2)	(3)	(4)
Δ Log(HP)$_{06\text{-}09}$	0.027	0.0300	0.031	0.029
	(0.019)	(0.019)	(0.019)	(0.020)
Δ Log(HP)$_{06\text{-}09}$ × Leverage$_{06}$	0.110**	0.106**	0.115***	0.119**
	(0.044)	(0.041)	(0.041)	(0.045)
Δ Log(HP)$_{06\text{-}09}$ × $\text{Elasticity}_{\text{Emp,15-year}}$	0.005			
	(0.004)			
Δ Log(HP)$_{06\text{-}09}$ × $\text{Elasticity}_{\text{Emp,30-year}}$		0.005		
		(0.003)		
Δ Log(HP)$_{06\text{-}09}$ × $\text{Elasticity}_{\text{Emp,2001}}$			0.014	
			(0.099)	
Δ Log(HP)$_{06\text{-}09}$ × $\text{Elasticity}_{\text{HP,2001}}$				0.005
				(0.008)
Leverage$_{06}$	-0.028*	-0.031**	-0.029**	-0.030**
	(0.016)	(0.015)	(0.015)	(0.016)
$\text{Elasticity}_{\text{Emp,15-year}}$	-0.005***			
	(0.002)			
$\text{Elasticity}_{\text{Emp,30-year}}$		-0.001		
		(0.001)		
$\text{Elasticity}_{\text{Emp,2001}}$			-0.071***	
			(0.021)	
$\text{Elasticity}_{\text{HP,2001}}$				-0.003
				(0.002)
Industry Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.04	0.04	0.04	0.04
Observations	284,800	284,800	284,800	284,800