FIRM LEVERAGE, CONSUMER DEMAND, AND EMPLOYMENT LOSSES DURING THE GREAT RECESSION*

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This article argues that firms' balance sheets were instrumental in the transmission of consumer demand shocks during the Great Recession. Using micro-level data from the U.S. Census Bureau, we find that establishments of more highly levered firms experienced significantly larger employment losses in response to declines in local 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. Likewise, at the county level, we find that counties with more highly levered firms experienced significantly larger declines in employment in response to local consumer demand shocks. Accordingly, firms' balance sheets also matter for aggregate employment. Our results suggest a possible role for employment policies that target firms directly besides conventional stimulus. *JEL Codes:* E24, E32, G32, J21, J23, R31.

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 consumer demand, in turn, had severe consequences for employment: across U.S. counties, those with larger declines in housing net worth experienced significantly larger declines in employment, especially in the nontradable sector (Mian and Sufi 2014a).

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What is conspicuously absent from this causal chain is any role for firms. After all, households do not lay off workers. Firms do. To investigate the role of firms in the transmission 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 individual establishments—for example, retail stores, supermarkets, or restaurants—that are matched to house prices in the establishment's zip code or county.

Our results suggest that firm balance sheets played a crucial role in the transmission of consumer demand shocks during the Great Recession. This is noteworthy because both academic research and public policy have hitherto primarily focused on 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 experienced significantly larger employment losses in response to declines in local consumer demand during the Great Recession. The magnitude of this leverage effect is large. Imagine two establishments, one whose parent firm lies at the 90th percentile of the leverage distribution and another whose parent firm lies at the 10th percentile of the leverage distribution. Our estimates imply that the former establishment exhibits a three times larger elasticity of employment with respect to local house prices. Importantly, the correlation between firm leverage and changes in house prices during the Great Recession is virtually 0. Thus, establishments of low- and high-leverage firms face similar local 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 regressions. Our tightest specification

1. For research focusing on the role of household balance sheets in the Great Recession, see, for example, Hall (2011), Eggertson and Krugman (2012), Mian, Rao, and Sufi (2013), Mian and Sufi (2014a), Guerrieri and Lorenzoni (2015), and Midrigan and Philippon (2016). For research focusing on the role of financial intermediary balance sheets, and "lender health" more generally, see Gertler and Kyotaki (2011), He and Krishnamurthy (2013), Brunnermeier and Sannikov (2014), Chodorow-Reich (2014), and Moreira and Savov (2016). A notable exception is Gilchrist et al. (2016), 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.

includes both firm and zip code × industry fixed effects. Hence, accounting for the possibility that low- and high-leverage firms experience differential employment losses for reasons unrelated to changes in consumer demand, 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 are thus precisely estimated.

We also examine whether firms make adjustments at the extensive margin. Similar to our employment results, establishments of more highly levered firms are significantly more likely to be closed down in response to declines in local consumer demand. Furthermore, 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 positive and significant correlations in the nontradable and "other" sector—that is, industries that are neither tradable nor nontradable. Importantly, in both sectors, this correlation is significantly stronger among establishments of more highly levered firms.

Our results are consistent with financial constraints impairing firms' ability to engage in labor hoarding. The idea behind labor hoarding is that firms facing a temporary (e.g., cyclical) decline in demand retain more workers than technically necessary so as to economize on the costs of firing, hiring, and training workers. Labor hoarding is costly, however. Effectively firms must (temporarily) subsidize workers' wages. Hence, firms with little financial slack face a genuine trade-off 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 trade-off by engaging in less labor hoarding.

In our sample, more highly levered firms indeed appear to be more financially constrained based on 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. Indeed, we find that more highly levered firms are less apt (or able) to raise additional short- and long-term debt in response to a decline in local consumer demand. 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 also act like financially constrained firms during the Great Recession.

An important concern is that more highly levered firms respond more strongly to local consumer demand shocks not because they are more financially constrained but because of other reasons unrelated to financial constraints. Although we cannot rule out this possibility in general, we can address specific alternative stories. For instance, more highly levered firms may be less productive, have expanded too much prior to the Great Recession, or have more active investors, such as private equity funds and activist hedge funds. Or high-leverage firms may simply be "high-beta" firms that are generally more sensitive to either aggregate employment or house prices—that is, for reasons unrelated to financial constraints. We find little evidence in support of these alternative stories. Furthermore, there is virtually no correlation between firm leverage and either housing supply elasticity or changes in house prices during the Great Recession. Hence, more highly levered firms do not respond more strongly to consumer demand shocks because they are located in regions with stronger shocks. In fact, given our fixed-effect specification, we can rule out any alternative story in which low- and high-leverage firms differ along either geographical or industry dimensions.

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 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 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 county-level employment. By contrast, if the distribution of firm leverage does not matter in the aggregate, then both counties should experience similar declines in county-level employment, irrespective of the level of county leverage. Regardless of whether we consider county-level employment by all firms in our sample or by all firms in the LBD, we find that more highly levered counties exhibit significantly larger employment losses in response to local consumer

demand shocks. Thus, our results are not undone by general equilibrium effects.

As in our establishment-level analysis, we also examine alternative channels at the county level. We find no evidence that more highly levered counties respond more strongly to consumer demand shocks during the Great Recession because their firms are less productive, have expanded too much in prior years, or have more activist investors, such as private equity funds and activist hedge funds, or because employment in these counties is generally more sensitive to either aggregate employment or house prices.

We conclude with a discussion of policy implications. That financial constraints may impair firms' ability to engage in labor hoarding suggests 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 rise in unemployment despite being hit hard by the global recession of 2008–2009. Many commentators attribute this resilience to massive labor hoarding, which is heavily subsidized in Germany. A central pillar of German labor hoarding is the system of "short-time work" programs encouraging firms to adjust labor demand through hours reductions rather than layoffs. Although a similar system also exists in many U.S. states ("work-sharing" programs), take-up rates have been extremely low due to burdensome filing processes, program rigidity, and financial disincentives for employers and workers.

In seminal work, Mian and Sufi (2011) and Mian, Rao, and Sufi (2013) document that rising house prices in the years prior to the Great Recession led to the build-up of household leverage, causing a sharp drop in consumer demand as house prices plummet during the Great Recession. 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 individual establishment level. In particular, we find that establishments of more highly levered firms exhibit significantly larger employment losses in response to declines in local consumer demand during the Great Recession.

The notion that firm balance sheets play an important role in the transmission of business cycle shocks goes back to Bernanke

2. Charles, Hurst, and Notowidigdo (2015) and Adelino, Schoar, and Severino (2015) examine the role of rising house prices for employment growth in the years leading up to the Great Recession.

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 worth but rather on the interaction between heterogeneous demand shocks and firm balance sheets. Caggese and Perez (2016) 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, they find interaction effects which, as they conclude, are in line with those found in our article. Aghion et al. (2015) also explore the role of firm heterogeneity during the Great Recession. Using firm-level data from Organisation for Economic Co-operation and Development (OECD) countries, they find that decentralized firms fare significantly better than their centralized counterparts, especially in industries that were hit hard by the Great Recession.

The rest of this article 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 financial constraints and 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 LBD. An establishment is a "single physical location where business is conducted" (Jarmin and Miranda 2002, p. 5), for example, a retail store, supermarket, restaurant, warehouse, or manufacturing plant. The LBD covers all business establishments in the United States 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. Because this bridge ends

in 2005, we extend the match to 2009 using employer name and ID number (EIN) following the procedure described in McCue (2003). This leaves us with 327,500 establishments with nonmissing 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 establishment-level employment between 2007 and 2009, $\Delta\text{Log(Emp)}_{07-09}$, 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, $\Delta\text{Log(HP)}_{06-09}$, the level of firm leverage associated with the establishment's parent firm in 2006, Leverage_{06}, and the interaction term $\Delta\text{Log(HP)}_{06-09} \times \text{Leverage}_{06}$. 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 0 and 1. In robustness checks, we also use other measures of firms' balance sheet strength. Although 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, that is, their number of employees. Standard errors are clustered at both the state and firm level.

Changes in house prices from 2006 to 2009 based on Zillow data are highly correlated with the "housing net worth shock" in Mian, Rao and Sufi (2013) and Mian and Sufi (2014a), ΔHousing Net Worth, 2006–2009. The correlation at the metropolitan statistical area (MSA) level is 86.3%. Other studies, like Adelino, Schoar,

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 http://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.

and Severino (2015) and Charles, Hurst, and Notowidigdo (2015), use house price data from the Federal Housing Finance Agency (FHFA). The correlation at the MSA level between changes in house prices from 2006 to 2009 based on Zillow data and FHFA data is 96.4%.

Table I, Panel A provides summary statistics at the establishment level. The first and second columns 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, Leverage_{06} is uncorrelated with changes in house prices between 2006 and 2009 and housing supply elasticity. Thus, establishments of low- and high-leverage firms face similar consumer demand shocks during the Great Recession—they merely react differently to these shocks. Interestingly, establishments of more highly levered firms are underrepresented in the nontradable sector and overrepresented in the "other" sector—that is, industries that are neither tradable nor nontradable. This is not a major concern, however. First, we perform separate analyses for each industry sector and find similar results in the nontradable 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 perhaps not surprising, more highly levered firms 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 establishment, employment, or asset growth. 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 before the Great Recession. Although this latter result is not surprising, the magnitude is large: the correlation between Leverage_{06} and the change in

TABLE I

SUMMARY STATISTICS

	Mean	Std. dev.	Correlation with $Leverage_{06}$	p-value of correlation
Panel A: Establishment level (N = 284,800)				
$Employees_{06}$	39	63	−0.028	.283
$\Delta Log(Emp)_{07-09}$	−8.2	24.2	−0.047**	.020
$\Delta Log(HP)_{06-09}$	−14.5	16.1	0.005	.718
Housing supply elasticity	1.799	0.927	0.011	.345
Census regions				
Northeast	0.17	0.38	0.004	.801
Midwest	0.21	0.41	−0.006	.610
South	0.38	0.49	0.002	.894
West	0.24	0.42	−0.000	.978
Industry sectors				
Tradable	0.03	0.18	0.001	.954
Nontradable	0.44	0.5	−0.146**	.014
Other	0.53	0.5	0.145**	.013
Panel B: Firm level 2006 (N = 2,800)				
$Establishments_{06}$	101	451	−0.015	.495
$Employees_{06}$	4,005	16,384	−0.008	.191
$Assets_{06}$	3,040	18,515	−0.003	.655
ROA_{06}	0.045	0.177	−0.073***	.003
NPM_{06}	0.024	0.28	−0.041**	.032
TFP_{06}	−0.002	0.599	−0.083***	.004
$Leverage_{06}$	0.227	0.253	1.000***	.000
WW_{06}	−0.251	0.135	0.189***	.000
KZ_{06}	−4.067	44.295	0.259***	.000

TABLE I
(CONTINUED)

	Mean	Std. dev.	Correlation with $Leverage_{06}$	p-value of correlation
Panel C: Firm level 2002–2006 (N = 2,800)				
$\Delta Establishments_{02-06}$	4.4	10.2	0.089***	.000
$\Delta Log(Emp)_{02-06}$	0.052	0.093	0.048***	.009
$\Delta Log(Assets)_{02-06}$	0.110	0.133	0.087***	.000
ΔROA_{02-06}	0.022	0.127	−0.061***	.003
ΔNPM_{02-06}	0.020	0.225	−0.032**	.015
ΔTFP_{02-06}	−0.001	0.569	−0.017	.649
$\Delta Leverage_{02-06}$	−0.023	0.153	0.379***	.000
ΔWW_{02-06}	−0.006	0.08	0.085***	.000
ΔKZ_{02-06}	−0.370	49.633	0.188***	.000
Panel D: 10th and 90th percentiles of $\Delta Log(HP)_{06-09}$ and $leverage_{06}$				
	10th	90th		
$\Delta Log(HP)_{06-09}$	−0.402	0.035		
$Leverage_{06}$	0.000	0.568		

Notes. 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. $\Delta Log(Emp)_{07-09}$ is the percentage change in establishment-level employment from 2007 to 2009. $\Delta Log(HP)_{06-09}$ 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 nontradaable industries are described in Mian and Sufi (2014a). "Other" industries are those that are neither tradable nor nontradable. 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 two-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). Panel D shows the 10th and 90th percentiles of $\Delta Log(HP)_{06-09}$and $Leverage_{06}$, respectively, used in counterfactual analyses in Section III.A. **, and *** denotes significance at the 5%, and 1% levels, respectively.

firm leverage between 2002 and 2006, $\Delta\text{Leverage}_{02-06}$, is 37.9%. Hence, a substantial portion 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 leading up to the Great Recession.

Panel D shows the 10th and 90th percentiles of $\Delta\text{Log(HP)}_{06-09}$ and Leverage_{06}. We make use of these percentiles below to perform counterfactual analyses.

Last, we 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 in the Great Recession not because they are more financially constrained but because they are less productive or expanded too much in the years prior to the Great Recession. We will address these and other alternative stories below.

III. Firm Leverage, Consumer Demand, and Employment

III.A. Main Results

To obtain a visual impression, Figure I plots the percentage change in establishment-level employment between 2007 and 2009, $\Delta\text{Log(Emp)}_{07-09}$, 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, $\Delta\text{Log(HP)}_{06-09}$, for different quartiles of firm leverage. For each percentile of $\Delta\text{Log(HP)}_{06-09}$, the plot shows the mean values of $\Delta\text{Log(HP)}_{06-09}$ and $\Delta\text{Log(Emp)}_{07-09}$, respectively. In Panel A, which represents the lowest leverage quartile, there is a positive albeit weak relationship between changes in house prices and changes in establishment-level employment, as is illustrated by the solid trend line. In Panels B to D, this relationship becomes successively stronger. In Panel D, which represents the highest leverage quartile, the elasticity of establishment-level employment with respect to house prices is 0.096, which is more than four times larger than the elasticity in the lowest leverage quartile.

Table II confirms this visual impression using regression analysis. All regressions are weighted by the size of

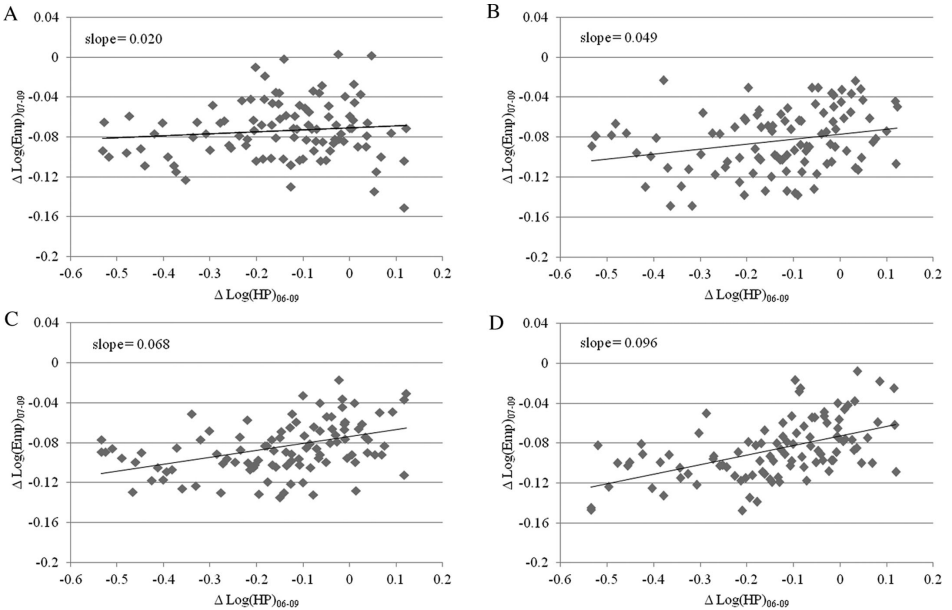

FIGURE I

Firm Leverage, House Prices, and Employment at the Establishment Level

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

TABLE II

FIRM LEVERAGE, CONSUMER DEMAND, AND EMPLOYMENT

	$\Delta\text{Log(Emp)}_{07-09}$						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\Delta\text{Log(HP)}_{06-09}$	0.066***	0.029	0.029		0.027		
	(0.019)	(0.022)	(0.019)		(0.019)		
$\Delta\text{Log(HP)}_{06-09} \times \text{Leverage}_{06}$		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

Notes. The dependent variable, $\Delta\text{Log(Emp)}_{07-09}$, is the percentage change in establishment-level employment from 2007 to 2009. $\Delta\text{Log(HP)}_{06-09}$ 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 four-digit NAICS codes. All regressions are weighted by establishment size. Standard errors (in parentheses) are clustered at both the state and firm level. **, and *** denote significance at the 5%, and 1% levels, respectively.

establishments. Standard errors are clustered at the state and firm level.[6] As can be seen, the average elasticity of establishment-level employment with respect to house prices is 0.066 (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 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.[7] 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. Each column has a different set of fixed effects. Arguably, our "tightest" specification is that in 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 four-digit NAICS industry. Note that although our sample firms are in Compustat, their establishments are relatively small, with an average size 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 relatively small establishments in the same zip code and four-digit NAICS 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 $\Delta\text{Log(HP)}_{06-09} \times \text{Leverage}_{06}$ is positive and significant. Hence, establishments of more highly levered firms exhibit significantly larger declines in employment in response to local consumer demand shocks. The magnitude of this leverage effect is large. Imagine two establishments, one whose parent firm lies at the 90th percentile and another whose parent firm lies at the 10th percentile of the leverage distribution. Our estimates in column (3) imply that the former establishment

6. Table 2 of the Online Appendix considers alternative clustering methods.

7. See Panel D of Table I: $0.066 \times (0.035 - (-0.402)) = 0.0288$, where 0.035 and -0.402 represent the 10th and 90th percentile, respectively, of $\Delta\text{Log(HP)}_{06-09}$.

exhibits a three times larger elasticity of employment with respect to house prices.[8]

The only fixed effect that has a noticeable impact on the coefficient associated with the interaction term is the firm fixed effect. 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, although it remains significant at the 5% level. Note, however, that including firm fixed effects may be "overcontrolling"—that is, it may be "controlling away" some of the very effect we are trying to document. For instance, some firms in our sample have most of their establishments in the same region. As the inclusion of firm fixed effects forces comparison to be made between different establishments within the same firm, this implies that for regionally concentrated firms, there exists only little within-firm variation in house price changes, making it difficult to identify the effect on employment changes. Alternatively, internal capital market flows may level out differences in employment losses across establishments within the same firm. If the firm's headquarters engages in cross-subsidization, establishments in less affected regions may subsidize those in more affected regions, reducing the within-firm variation in the sensitivity of employment with respect to local consumer demand shocks. Given these issues, we use column (3) as our main specification. This specification has the additional advantage that it also shows the coefficients associated with the main effects, $\Delta\text{Log(HP)}_{06-09}$ and Leverage_{06}, respectively. That being said, the analysis in Table II has shown that our main results hold under various fixed-effect specifications.

III.B. Other Measures of Firm's Balance Sheet Strength

We obtain similar results when using other measures of firms' debt capacity or balance sheet strength. 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

8. See Panel D of Table I: $0.029 + 0.114 \times 0.568 = 0.094$ versus $0.029 + 0.114 \times 0.000 = 0.029$, where 0.568 and 0.000 represent the 90th and 10th percentile, respectively, of Leverage_{06}. Although these counterfactual elasticities are based on a linear specification, they compare reasonably well to the elasticities in the highest and lowest leverage quartiles in Figure I.

TABLE III

OTHER MEASURES OF FIRMS' BALANCE SHEET STRENGTH

	$\Delta\text{Log(Emp)}_{07-09}$						
	Net leverage$_{06}$ (1)	Market leverage$_{06}$ (2)	Debt to EBITDA$_{06}$ (3)	Interest coverage$_{06}$ (4)	$\Delta\text{Leverage}_{02-06}$ (5)	WW$_{06}$ (6)	KZ$_{06}$ (7)
$\Delta\text{Log(HP)}_{06-09}$	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)
$\Delta\text{Log(HP)}_{06-09} \times$ 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

Notes. This table presents variants of the regressions in Table II in which Leverage$_{06}$ is replaced by other measures of firms' debt capacity or balance sheet strength. 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. $\Delta\text{Leverage}_{02-06}$ is the change in firm leverage from 2002 to 2006. WW and KZ are the financial constraints indexes of Whited and Wu (2006) and Kaplan and Zingales (1997), respectively. ΔLeverage, WW, and KZ are net of their respective minimum values. Industry fixed effects are based on four-digit NAICS codes. All regressions are weighted by establishment size. Standard errors (in parentheses) are clustered at both the state and firm level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

in 2006 (column (5)). As discussed previously, these two variables are highly correlated, implying that firms with higher leverage in 2006 are largely firms that increased their leverage in previous years. Finally, our results are similar when using the financial constraints indexes 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 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 face supply constraints due to their topography (steep hills and water bodies) as well as local regulations. The Saiz instrument has been widely used in the literature as a source of exogenous variation in house price changes (e.g., Mian and Sufi 2011, 2014a; Mian, Rao, and Sufi 2013; Adelino, Schoar, and Severino 2015; Baker 2015; Berger and Vavra 2015; Stroebel and Vavra 2015; Kaplan, Mitman, and Violante 2016).

The instrumental variables (IV) results are provided in Table 3 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 more strongly to local 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." All results remain similar.

III.D. Industry Sectors

The summary statistics in Table I show that establishments of more highly levered firms are underrepresented in the nontradable sector and overrepresented in the "other" sector—that

is, industries that are neither tradable nor nontradable.[9] While all our establishment-level regressions include industry fixed effects, we can directly address any concerns related to industry sector composition by performing separate analyses for each industry sector.

Figure II plots the relationship between changes in establishment-level employment, changes in house prices, and firm leverage separately for the nontradable and tradable sectors.[10] The plots for the nontradable sector are similar to those in Figure I. In the lowest leverage quartile, there is a positive albeit weak relationship between changes in house prices and changes in establishment-level employment (Panel A). In the highest leverage quartile, this relationship is strongly positive (Panel B). On the other hand, there is no clear association between changes in house prices and changes in establishment-level employment in the tradable sector (Panels C and D).

Table IV confirms this visual impression using regression analysis. As is shown, there is a positive and significant correlation between changes in house prices and changes in establishment-level employment in the nontradable sector (column (1)). By contrast, there is no significant correlation in the tradable sector (column (2)). Together, both results confirm similar results by Mian and Sufi (2014a), who examine changes in aggregate employment at the county level. Although 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 (regional) variation in employment primarily in the nontradable sector, where demand by households is local. By contrast, variation in house prices should not correlate strongly with variation in employment in the tradable

9. Mian and Sufi (2014a) 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 nontradable. We label industries that are neither tradable nor nontradable as "other." The "other" sector comprises a diverse set of industries that includes, for example, news and entertainment, transportation and trucking, health care 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 4 of the Online Appendix.

10. The plots for the "other" sector are similar to those for the nontradable sector. See Figure 1 of the Online Appendix.

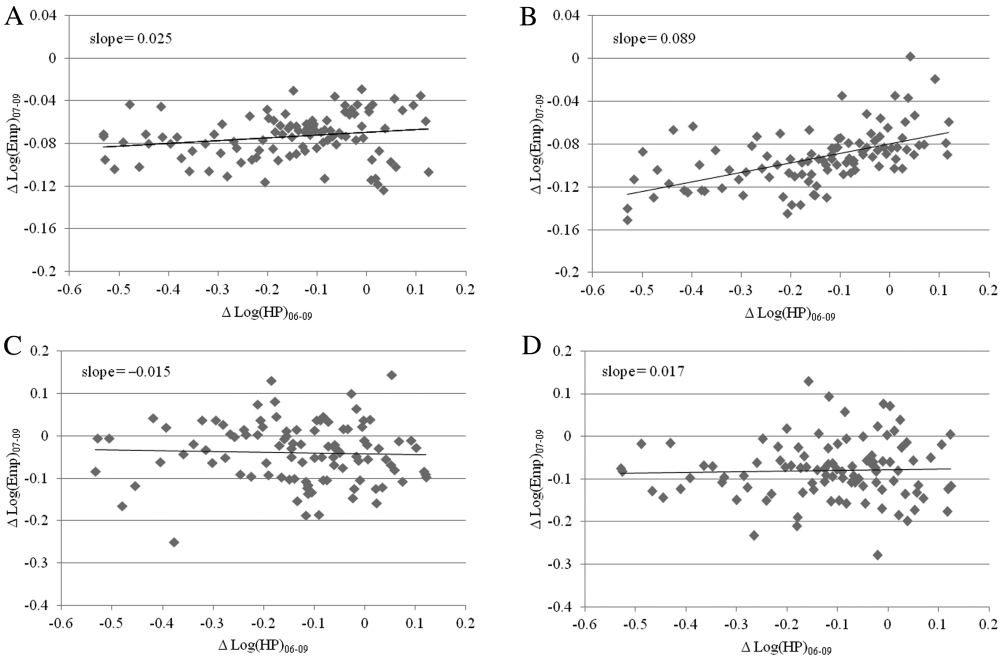

FIGURE II

Nontradable and Tradable Employment at the Establishment Level

The plots are similar to those in Panels A and D of Figure I, except that the sample is restricted to nontradable and tradable industries, respectively.

TABLE IV
TRADABLE AND NONTRADABLE INDUSTRIES

	$\Delta\text{Log(Emp)}_{07-09}$			
	(1) Nontradable	(2) Tradable	(3) Nontradable	(4) Tradable
$\Delta\text{Log(HP)}_{06-09}$	0.074**	0.009	0.029	−0.015
	(0.035)	(0.019)	(0.019)	(0.043)
$\Delta\text{Log(HP)}_{06-09} \times$ Leverage$_{06}$			0.131***	0.037
			(0.034)	(0.120)
Leverage$_{06}$			−0.038**	−0.026
			(0.015)	(0.020)
Industry fixed effects	Yes	Yes	Yes	Yes
R-squared	0.04	0.03	0.04	0.03
Observations	124,100	9,900	124,100	9,900

Notes. This table presents variants of the regressions in Table II in which the sample is restricted to tradable and nontradable industries, respectively. Tradable and nontradable industries are described in Mian and Sufi (2014a). All regressions are weighted by establishment size. Standard errors (in parentheses) are clustered at both the state and firm level. **, and *** denote significance at the 5%, and 1% levels, respectively.

sector, where demand is national or global. Given the evidence in Table IV, as well as further evidence in Mian, Rao, and Sufi (2013), Mian and Sufi (2014a), Stroebel and Vavra (2015), and Kaplan, Mitman, and Violante (2016), we use "falling house prices" and "consumer demand shocks" interchangeably.[11]

Table 5 of the Online Appendix considers the "other" industry sector. As can be seen, results are similar to those for the nontradable sector. Indeed, the elasticity of establishment-level employment with respect to house prices is virtually identical in both sectors (0.074 versus 0.075). Together, the nontradable and "other" sector account for 97% of all establishment-level observations. Hence, there is no need to interact changes in house

11. In principle, falling house prices could affect local employment through various channels. For instance, they could impair the collateral value associated with local firms' commercial real estate or affect local credit supply—for example, local banks reduce lending after experiencing losses on their mortgage loan portfolios. Either way, however, this would imply that falling house prices should affect local employment also in the tradable sector, contrary to what is observed in the data. See Mian and Sufi (2014a) for a further dicussion. In addition, Mian, Rao, and Sufi (2013) and Kaplan, Mitman, and Violante (2016) provide direct evidence showing that counties or core-based statistical areas (CBSAs) experiencing larger declines in housing net worth exhibit larger declines in consumer spending during the Great Recession. Likewise, Stroebel and Vavra (2015) find that homeowners become more price-sensitive and cut back more on their retail spending in zip codes experiencing larger drops in house prices.

prices with sector dummies in our regressions.[12] Importantly, in both sectors, the correlation between changes in house prices and changes in employment is significantly stronger among establishments of more highly levered firms. Accordingly, our results are not driven by industry sector composition effects.

Table 7 of the Online Appendix lists the top 10 industries in which house prices have the biggest impact on establishment-level employment. To construct this list, we estimated column (1) of Table II separately for each four-digit NAICS industry. At the top of the list are full-service restaurants (nontradable), building material and supplies dealers ("other"), and health and personal care stores (nontradable). Interestingly, 3 of the top 10 industries are auto-related: automotive repair and maintenance (#4, "other"); automotive parts, accessories, and tire stores (#7, nontradable); and automobile dealers (#8, nontradable). Not surprisingly, there is no tradable industry in the top 10.[13]

III.E. Establishment Closures

Does the drop in house prices between 2006 and 2009 cause firms to make adjustments at the extensive margin? To examine this question, we include in our sample 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 is shown in Table 8 of the Online Appendix, changes in house prices between 2006 and 2009 are negatively and significantly associated with establishment closures (column (1)). Moreover, as in 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 extensive margin.

12. While tradable industries account for 3% of all establishments, they account for 12% of total employment in our sample. In other words, firms in tradable industries have relatively few but large establishments (for example, manufacturing plants). Since all our regressions are employment-weighted, this implies that excluding tradable industries should make our results stronger. Indeed, Table 6 of the Online Appendix shows that our results become slightly stronger if we exclude tradable industries.

13. 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 excessively 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 LBD—for example, 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 important, single-unit establishments (e.g., "mom and pop shops").[14] Overall, our sample accounts for 12% of total LBD employment. In terms of industry sectors, our sample accounts for 26% of nontradable employment, 18% of tradable employment, and 8% of "other" employment.

One might worry that our sample consists of establishments that are particularly responsive to local consumer demand shocks. In Table 9 of the Online Appendix, we estimate the elasticity of establishment-level employment with respect to house prices separately for establishments in the matched Compustat-LBD sample and those in the full LBD sample. As is shown, the elasticity is 59% larger in the full LBD sample.[15] Thus, if anything, our sample includes establishments that respond less strongly to local consumer demand shocks.

That establishments in the matched Compustat-LBD sample have lower elasticities is consistent with Compustat firms being less financially constrained. Indeed, several empirical studies provide evidence suggesting that public firms are less financially constrained than private firms, for example, Brav (2009) and Gilje and Taillard (2015). Notably, the lower elasticities are not due to Compustat firms being located in regions with smaller house price drops: the correlation between $\Delta\text{Log(HP)}_{06-09}$ and the employment share of Compustat firms at either the zip code or county level is close to zero and insignificant (1.4% [p-value: .321] and 1.2% [p-value: .681]). Likewise, a Kolmogorov-Smirnov test is unable to reject the null that the distribution of $\Delta\text{Log(HP)}_{06-09}$ is identical for establishments in 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 similar declines in house prices.

14. Our county-level analysis in Section VI constitutes an exception. There, some of our regressions have total LBD employment as the dependent variable.

15. A main difference between the two samples is firm size: Compustat firms are much larger. Indeed, if we reweight our regressions using firm size instead of establishment size—thus giving more weight to establishments of larger firms—the elasticity in the full LBD sample is only 24% larger than in the matched Compustat-LBD sample. See Panel C of Table 9 in the Online Appendix.

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. 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 8% 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—for example, maintenance, cleaning, or training—leaving about 4% of the blue-collar hours paid for by the typical plant to be classified as truly "hoarded." By the 1980s and 1990s, 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).[16]

Labor hoarding is costly, however. Effectively, firms must (temporarily) subsidize workers' wages. Hence, firms with little financial slack face a genuine trade-off 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 weak balance sheets—and tighter financial constraints—are more apt to respond to this trade-off by engaging in less labor hoarding. In other words, firms with weak balance sheets cut more jobs in response to a decline in consumer demand than they (optimally) would have in the absence of financial constraints.

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 indexes 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 $\Delta \text{Log(HP)}_{06-09}$ is now the employment-weighted average

16. Biddle (2014) provides a comprehensive overview of the labor hoarding literature.

percentage change in house prices between 2006 and 2009 across all of the firm's establishments. In other words, $\Delta\text{Log(HP)}_{06-09}$ 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 V presents the results. When faced with consumer demand shocks in the Great Recession, more highly levered firms are less apt (or able) to raise additional short- and long-term debt (columns (1) and (2)).[17] 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 also act like financially constrained firms during the Great Recession.

We should note that ours is not the first publication to point to a link between financial constraints and labor hoarding.[18] Using manufacturing firm-level data from 1959 to 1985, Sharpe (1994) found that employment growth is more cyclical at more highly levered firms. As 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 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. By contrast, firms classified as unconstrained said they would cut their employment only by 2.7%. Although both studies suggest a link between employment growth and financial constraints

17. We are unable to reject the null that $\Delta\text{Log(HP)}_{06-09} + \Delta\text{Log(HP)}_{06-09} \times \text{Leverage}_{06}$ evaluated at $\text{Leverage}_{06} = 1$ is 0 in columns (1) and (2) (p-values: .333 and .268, respectively). Accordingly, 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.

18. We are unaware of theory models linking financial constraints and labor hoarding. For theory models of labor hoarding per se—that is, absent financial frictions—see Oi (1962), Clark (1973), Rotemberg and Summers (1990), and Burnside, Eichenbaum, and Rebelo (1993).

TABLE V

FIRM-LEVEL ANALYSIS

	External finance			Employment and investment		
	ΔST debt$_{07-09}$ (1)	ΔLT debt$_{07-09}$ (2)	ΔEquity$_{07-09}$ (3)	ΔLog(Emp)$_{07-09}$ (4)	Est. Closure$_{07-09}$ (5)	ΔCAPEX$_{07-09}$ (6)
ΔLog(HP)$_{06-09}$	−0.025**	−0.040**	0.005	0.020	−0.008	0.002
	(0.011)	(0.019)	(0.037)	(0.033)	(0.015)	(0.005)
ΔLog(HP)$_{06-09}$ × 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

Notes. This table presents firm-level variants 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 to property, plant and equipment (PP&E). ΔLog(HP)$_{06-09}$ is aggregated at the firm level by computing the employment-weighted average value of Δ Log(HP)$_{06-09}$ across all of the firm's establishments. Industry fixed effects are based on four-digit NAICS codes. Standard errors (in parentheses) are clustered at the state level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

over the business cycle, neither study separates out the effects of demand shocks that lie at the very heart of the labor hoarding concept.

V. Robustness

An important concern is that more highly levered firms respond more strongly to local consumer demand shocks not because they are more financially constrained but rather because of other reasons unrelated to financial constraints. Although we cannot rule out this possibility in general, we can address specific alternative stories.[19] For instance, according to the summary statistics in Table I, more highly levered firms are less productive and expanded more in the years before the Great Recession. However, 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 because they are less productive or expanded too much in previous years. We examine these and other alternative stories in Table VI by including additional controls Z and $\Delta\text{Log(HP)}_{06-09} \times Z$ in our regressions, where Z stands for the alternative hypothesis in question. For brevity, Table VI only shows the coefficients associated with the main variables of interest, $\Delta\text{Log(HP)}_{06-09} \times \text{Leverage}_{06}$ and $\Delta\text{Log(HP)}_{06-09} \times Z$. The full sets of coefficients are provided in Tables 10 to 14 of the Online Appendix.

V.A. Employment and Asset Growth

Panel A 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, $\Delta\text{Log(Emp)}_{02-06}$, as well as its interaction with $\Delta\text{Log(HP)}_{06-09}$. Column (2) is similar, except that we consider the percentage change in assets between 2002 and 2006, $\Delta\text{Log(Assets)}_{02-06}$. As it turns out, including past

19. Some alternative stories can be ruled out a priori. For instance, Table I shows that there is virtually no correlation between firm leverage and either housing supply elasticity or changes in house prices during the Great Recession. Hence, more highly levered firms do not respond more strongly to consumer demand shocks because they are located in regions with stronger shocks. Indeed, given our fixed-effect specification in Table II, we can rule out any alternative story whereby low- and high-leverage firms differ along either geographical or industry dimensions.

TABLE VI

ROBUSTNESS

	(1)	(2)	(3)	(4)
Panel A: Employment and asset growth				
$\Delta\text{Log(HP)}_{06-09} \times \text{Leverage}_{06}$	0.111**	0.113***		
	(0.047)	(0.040)		
$\Delta\text{Log(HP)}_{06-09} \times \Delta\ \text{Log(Emp)}_{02-06}$	0.027			
	(0.034)			
$\Delta\text{Log(HP)}_{06-09} \times \Delta\ \text{Log(Assets)}_{02-06}$		0.012		
		(0.009)		
Panel B: Productivity				
$\Delta\text{Log(HP)}_{06-09} \times \text{Leverage}_{06}$	0.101**	0.122***	0.108***	
	(0.040)	(0.041)	(0.039)	
$\Delta\text{Log(HP)}_{06-09} \times \text{ROA}_{06}$	−0.024			
	(0.110)			
$\Delta\text{Log(HP)}_{06-09} \times \text{NPM}_{06}$		−0.038		
		(0.161)		
$\Delta\text{Log(HP)}_{06-09} \times \text{TFP}_{06}$			−0.049*	
			(0.026)	
Panel C: Sensitivity to aggregate employment and house prices				
$\Delta\text{Log(HP)}_{06-09} \times \text{Leverage}_{06}$	0.109**	0.110**	0.119***	0.120***
	(0.044)	(0.044)	(0.044)	(0.044)
$\Delta\text{Log(HP)}_{06-09} \times \text{Elasticity}_{\text{Emp,10-year}}$	0.006*			
	(0.004)			
$\Delta\text{Log(HP)}_{06-09} \times \text{Elasticity}_{\text{Emp,20-year}}$		0.005		
		(0.004)		
$\Delta\text{Log(HP)}_{06-09} \times \text{Elasticity}_{\text{HP, 10-year}}$			0.006	
			(0.008)	
$\Delta\text{Log(HP)}_{06-09} \times \text{Elasticity}_{\text{HP, 02-06}}$				0.007
				(0.008)

TABLE VI
(CONTINUED)

	(1)	(2)	(3)	(4)
Panel D: Activist investors				
$\Delta\text{Log(HP)}_{06-09} \times \text{Leverage}_{06}$	0.110***	0.115***		
	(0.039)	(0.040)		
$\Delta\text{Log(HP)}_{06-09} \times \text{Hedge Fund}_{06}$	0.036			
	(0.036)			
$\Delta\text{Log(HP)}_{06-09} \times \text{Private Equity}_{06}$		0.009		
		(0.056)		

Notes. This table presents variants of the regressions in Table II in which Z and ΔLog(HP)06–09 $\times$ Z are included as additional controls. For brevity, the table only shows the coefficients associated with the main variables of interest, ΔLog(HP)06–09 $\times$ Leverage06 and ΔLog(HP)06–09 $\times$ Z. The full sets of coefficients are provided in Tables 10 to 14 of the Online Appendix. In Panel A, Z is either the growth in firm-level employment from 2002 to 2006, $\Delta\,\text{Log(Emp)}_{02-06}$ (column (1)), or the growth in firm-level assets from 2002 to 2006, $\Delta\text{Log(Assets)}_{02-06}$ (column (2)). In Panel B, Z is either the firm's return on assets, ROA_{06} (column (1)), the firm's net profit margin, NPM_{06} (column (2)), or the firm's total factor productivity, TFP_{06} (column (3)), all measured in 2006. ROA_{06} is the ratio of operating income before depreciation to total assets, NPM_{06} is the ratio of operating income before depreciation to sales, and TFP_{06} is the residual from a regression of Log(Sales) on Log(Employees) and Log(PP&E) across all Compustat firms in the same two-digit SIC industry. In Panel C, Z is either the 10-year elasticity of firm-level employment with respect to aggregate employment, $\text{Elasticity}_{Emp,10\text{-}year}$(column (1)), the 20-year elasticity of firm-level employment with respect to aggregate employment, $\text{Elasticity}_{Emp,20\text{-}year}$ (column (2)), the 10-year elasticity of firm-level employment with respect to house prices, $\text{Elasticity}_{HP,10\text{-}year}$ (column (3)), or the elasticity of firm-level employment with respect to house prices during the 2002 to 2006 housing boom, $\text{Elasticity}_{HP,02-06}$ (column (4)). $\text{Elasticity}_{Emp,10\text{-}year}$ and $\text{Elasticity}_{Emp,20\text{-}year}$ are computed by estimating a firm-level regression of ΔLog (Employment) on a constant and ΔLog(LBD Employment) using all available years from 1997 to 2006 and 1987 to 2006, respectively. $\text{Elasticity}_{HP,10\text{-}year}$ and $\text{Elasticity}_{HP,02-06}$ are computed as the employment-weighted average elasticity of employment with respect to house prices across all of the firm's establishments, where the latter is computed either by estimating an establishment-level regression of ΔLog(Employment) on a constant and ΔLog(HP) using all available years from 1997 to 2006 ($\text{Elasticity}_{HP,10\text{-}year}$) or as the percentage change in establishment-level employment divided by the percentage change in house prices during the 2002 to 2006 housing boom ($\text{Elasticity}_{HP,02-06}$). In Panel D, Z is either a dummy indicating whether a firm is targeted by an activist hedge fund, Hedge Fund_{06} (column (1)), or a dummy indicating whether a firm has significant (i.e., more than 5%) private equity ownership, $\text{Private Equity}_{06}$ (column (2)), all measured in 2006. Industry fixed effects are based on four-digit NAICS codes. All regressions are weighted by establishment size. Standard errors (in parentheses) are clustered at both the state and firm level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

employment or asset growth has no effect on our results. Although none of the additional controls are significant, the main coefficient of interest—that associated with the interaction term $\Delta\text{Log(HP)}_{06-09} \times \text{Leverage}_{06}$—is significant and of similar magnitude as in our main specification in Table II.

V.B. Productivity

Panel B examines if our results are driven by firms having lower productivity. In column (1), we include as additional controls the firm's return on assets, ROA_{06}, as well as its interaction with $\Delta\text{Log(HP)}_{06-09}$. 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. All three productivity measures are measured in 2006. In all three cases, 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 $\Delta\text{Log(HP)}_{06-09}$, 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 $\Delta\text{Log(HP)}_{06-09} \times \text{Leverage}_{06}$ is significant and stable in all three regressions.

V.C. Sensitivity to Aggregate Employment and House Prices

Panel C examines if our results are driven by firms being generally more sensitive to either aggregate employment or house prices ("high-beta firms")—that is, for reasons unrelated to financial constraints.[20] 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 prior downturns. In columns (1) and (2), we include as additional controls the elasticity of firm-level employment with respect to total LBD employment over either a 10-year or 20-year period ending in 2006, $\text{Elasticity}_{\text{Emp, 10-year}}$ and $\text{Elasticity}_{\text{Emp, 20-year}}$, respectively, as well as its interaction with $\Delta\text{Log(HP)}_{06-09}$. 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

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

period ending in 2006 or during the 2002 to 2006 housing boom, $\text{Elasticity}_{\text{HP,10-year}}$ and $\text{Elasticity}_{\text{HP,02-06}}$, respectively.[21]

As is shown, the main effect of the elasticity of firm-level employment with respect to either aggregate employment or house prices is always significant with the predicted sign: 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 $\Delta\ \text{Log}(\text{HP})_{06-09}$, while having the predicted sign, is either only weakly significant (column (1)) or insignificant (columns (2) to (4)). Importantly, our main coefficient of interest—that associated with the interaction term $\Delta\text{Log}(\text{HP})_{06-09} \times \text{Leverage}_{06}$—is significant and stable in all four regressions.

V.D. Activist Investors

Panel D 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). The data are based on Schedule 13D filings, which investors must file with the SEC within 10 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 $\Delta\text{Log}(\text{HP})_{06-09}$. Column (2) is similar, except that we include a dummy variable indicating whether a firm has significant PE ownership in 2006, PE_{06}.[22] As is shown, the main effect of activist

21. Table 13 of the Online Appendix additionally 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.

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

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 $\Delta\text{Log(HP)}_{06-09}$, while having the predicted sign, is always insignificant. Importantly, the coefficient associated with the interaction term $\Delta\text{Log(HP)}_{06-09} \times \text{Leverage}_{06}$ is significant and stable in both regressions.

VI. County-Level Analysis

VI.A. General Equilibrium Adjustments

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 is 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, the magnitude of this reallocation will depend on how sticky prices are. Along those lines, Gilchrist et al. (2016), Mian and Sufi (2014a), and Chodorow-Reich (2014) all discuss how sticky prices or imperfect substitutability in the goods market may limit the reallocation of output and labor in response to differential firm-level shocks.[23]

The extent of labor reallocation also depends on search and matching frictions and 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 the search intensity per unemployed worker and the recruiting intensity per vacancy dropped sharply during the Great Recession. Likewise, Şahin et al. (2014) find 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

23. Besides, if low-leverage firms engage in labor hoarding (see Section IV), this means their own workforce is employed at below full capacity, making it less likely that these firms would want to hire additional workers from high-leverage firms.

(2016) 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" (Foster, Grim, and Haltiwanger 2016, p. S305).

VI.B. Firm Leverage, House Prices, and Employment at the County Level

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. 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 county-level employment. By contrast, if the distribution of firm leverage does not matter in the aggregate, then both counties should experience similar declines in county-level employment, regardless of the level of county leverage.

We proceed as in our establishment-level analysis. County-level employment is total employment by all firms in our sample within a county. County-level leverage is the employment-weighted average value of Leverage_{06} across all firms in our sample within a county. Importantly, county-level leverage is uncorrelated with changes in county-level house prices between 2006 and 2009, $\Delta\text{Log}(\text{HP})_{06-09}$: the correlation is 0.8% and highly insignificant (p-value: .809). Thus, low- and high-leverage counties experience similar consumer demand shocks during the Great Recession.

To obtain a visual impression, Figure III plots the percentage change in county-level employment between 2007 and 2009, $\Delta\text{Log}(\text{Emp})_{07-09}$, against the percentage change in county-level house prices between 2006 and 2009, $\Delta\text{Log}(\text{HP})_{06-09}$, separately for counties in the lowest and highest leverage quartile. For each percentile of $\Delta\text{Log}(\text{HP})_{06-09}$, the scatterplot shows the mean values of $\Delta\text{Log}(\text{HP})_{06-09}$ and $\Delta\text{Log}(\text{Emp})_{07-09}$, respectively. The plots in Panels A and B (all industry sectors) are similar to those at the establishment level (see Figure I, Panels A and D). In Panel A, which represents the lowest leverage quartile, there is a positive but weak relationship between changes in house prices and changes in county-level employment. In Panel B, which represents

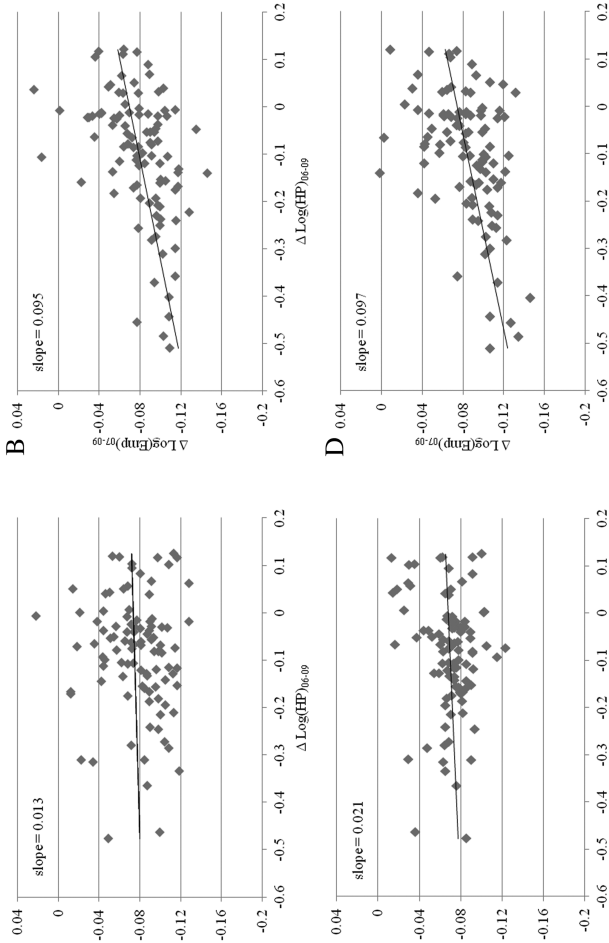

FIGURE III

Firm Leverage, House Prices, and Employment at the County Level

The plots are similar to those in Panels A and D of Figure I and Panels A to D in Figure II, except that the analysis is at the county level. County-level employment is total employment by all firms in our sample within a county. County-level leverage is the employment-weighted average value of firm leverage across all firms in our sample within a county.

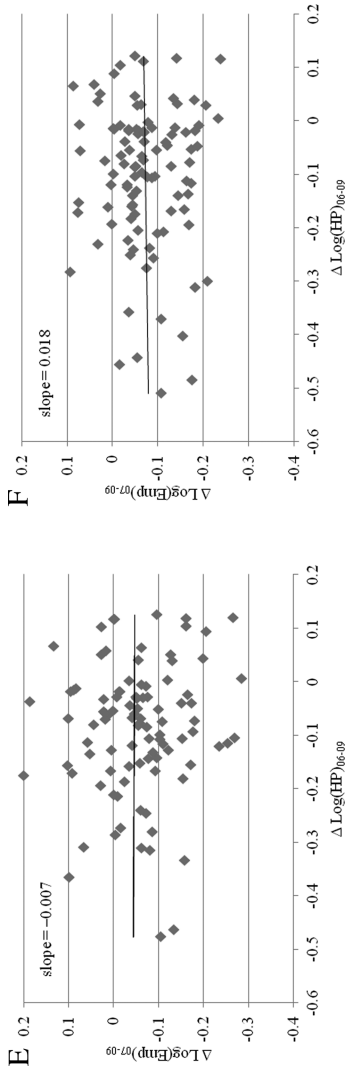

FIGURE III
(CONTINUED)

the highest leverage quartile, this relationship becomes strongly positive. The elasticity of county-level employment with respect to house prices in the highest leverage quartile is 0.095, which is more than seven times larger than the elasticity in the lowest leverage quartile and virtually identical to the corresponding elasticity of 0.096 at the establishment level (see Figure I, Panel D). The plots in Panels C to F are also similar to those at the establishment level (see Figure II, Panels A–D). In Panels C and D (nontradable sector), there is a positive but weak relationship between changes in house prices and changes in county-level employment in lowest leverage quartile and a strongly positive relationship in the highest leverage quartile. By contrast, in Panels E and F (tradable sector), there is no clear association between changes in house prices and changes in county-level employment.

Table VII 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 two-digit NAICS industries in 2006 (see Table III of Mian and Sufi 2014a). Columns (1) and (2) examine county-level employment across all industry sectors. In column (1), the average elasticity of county-level employment with respect to house prices is 0.069, which is similar to the elasticity of 0.066 in our establishment-level analysis (see Table II, column (1)).[24] Importantly, in column (2), the interaction term $\Delta \text{Log(HP)}_{06-09} \times \text{Leverage}_{06}$ is highly significant, and its coefficient of 0.110 is similar to the coefficient of 0.114 at the establishment level (see Table II, column (3)). Thus, more highly levered counties exhibit significantly larger declines in employment in response to consumer demand shocks, implying that the distribution of firm leverage matters also in the aggregate. Columns (3) and (4) examine county-level employment in the nontradable sector. In column (3), the average elasticity of nontradable county-level employment with respect to house prices is 0.076, which is similar to the elasticity of 0.074 in our establishment-level analysis (see Table IV, column (1)). Moreover, in column (4), the interaction term $\Delta \text{Log(HP)}_{06-09} \times \text{Leverage}_{06}$ is highly significant, and its coefficient of 0.134 is similar to the coefficient of 0.131 at the

24. The 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, which is almost identical to the county-level elasticity of 0.069 (see Table 9, Panel A, column (1) of the Online Appendix).

TABLE VII

COUNTY-LEVEL ANALYSIS

	$\Delta\text{Log(Emp)}_{07-09}$					
	All industries		Nontradable		Tradable	
	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta\text{Log(HP)}_{06-09}$	0.069***	0.025	0.076***	0.026	0.010	−0.015
	(0.008)	(0.019)	(0.012)	(0.020)	(0.010)	(0.039)
$\Delta\text{Log(HP)}_{06-09} \times \text{Leverage}_{06}$		0.110***		0.134***		0.034
		(0.038)		(0.036)		(0.151)
Leverage_{06}		−0.029**		−0.039**		−0.028*
		(0.013)		(0.020)		(0.015)
County × industry controls	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.13	0.13	0.18	0.19	0.09	0.09
Observations	1,000	1,000	1,000	1,000	1,000	1,000

Notes. This table presents county-level variants of the regressions in Table II and IV. County-level employment is total employment by all firms in our sample within a county. County-level leverage is the employment-weighted average value of Leverage_{06} across all firms in our sample within a county. County × industry controls are the county-specific employment shares of all 23 two-digit NAICS industries in 2006. All regressions are weighted by county size. Standard errors (in parentheses) are clustered at the state level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

establishment level (see Table IV, column (3)). Finally, columns (5) and (6) examine county-level employment in the tradable sector. Regardless of county-level leverage, there is no significant association between changes in house prices and changes in county-level employment, confirming similar results in our establishment-level analysis (see Table IV, columns (2) and (4)).

Tables 15 to 19 of the Online Appendix provide the same robustness tests as in Section V, except that the analysis is at the county level. There is no evidence that more highly levered counties respond more strongly to consumer demand shocks during the Great Recession because their firms are less productive, have expanded too much prior to the Great Recession, or have more activist investors, or because employment in these counties is generally more sensitive to either aggregate employment or house prices.

VI.C. Expanding the Sample: County-Level Employment by All Firms in the LBD

Examining changes in county-level employment by all firms in our sample captures any general equilibrium effects from labor reallocation between firms in our sample within a given county. However, it does not capture labor reallocation between firms in our sample and other firms in the LBD, such as small private firms and mom and pop shops. Given that the latter firms exhibit even larger employment losses in response to consumer demand shocks (see Table 9 of the Online Appendix), one would think that labor reallocation toward these firms is unlikely. Ultimately, however, this is an empirical question. In the remainder of this section, we thus examine changes in county-level employment by all firms in the LBD.

Expanding the sample to include all firms in the LBD entails two main changes. The first change is that firms become more sensitive to changes in house prices. As discussed in Section III.F, the average firm in the LBD is more financially constrained than the average firm in the matched Compustat-LBD sample. Accordingly, we would expect that the average elasticity of county-level employment with respect to house prices increases. Indeed, as column (1) of Table VIII shows, the average elasticity of county-level employment with respect to house prices is now 0.104, which is about 50% larger than the average elasticity of 0.069 in the matched Compustat-LBD sample (see column (1) of Table VII). Note that

TABLE VIII

EXPANDING THE SAMPLE: COUNTY-LEVEL EMPLOYMENT BY ALL FIRMS IN THE LBD

	ΔLog(Emp)$_{07-09}$					
	All industries		Nontradable		Tradable	
	(1)	(2)	(3)	(4)	(5)	(6)
ΔLog(HP)$_{06-09}$	0.104***	0.085***	0.122***	0.096***	0.008	−0.003
	(0.005)	(0.015)	(0.006)	(0.019)	(0.016)	(0.034)
ΔLog(HP)$_{06-09}$ × Leverage$_{06}$		0.045**		0.058***		0.022
		(0.022)		(0.020)		(0.105)
Leverage$_{06}$		−0.023**		−0.028**		−0.019*
		(0.010)		(0.014)		(0.010)
County × industry controls	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.14	0.14	0.16	0.16	0.13	0.13
Observations	1,000	1,000	1,000	1,000	1,000	1,000

Notes. This table presents variants of the regressions in Table VII in which county-level employment is total employment by all firms in the LBD within a county. County-level leverage is the employment-weighted average value of Leverage$_{06}$ across all firms in our sample within a county. County × industry controls are the county-specific employment shares of all 23 two-digit NAICS industries in 2006. All regressions are weighted by county size. Standard errors (in parentheses) are clustered at the state level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

these numbers almost perfectly match those obtained at the establishment level. There, the average elasticity of county-level employment with respect to house prices in the full LBD sample was 0.108 versus 0.068 in the matched Compustat-LBD sample (see Table 9 of the Online Appendix).

The second change is that county-level leverage is measured with error. Unlike our previous analysis, county-level employment and county-level leverage are no longer based on the same sample of firms. While county-level employment now includes all firms in the LBD, county-level leverage is (still) based on firms in the matched Compustat-LBD sample, because leverage is computed using data from Compustat. Hence, we only observe $\text{Leverage}_{06} = \text{Leverage}^{*}_{06} + \eta$, where Leverage^{*}_{06} is "true" county-level leverage based on all firms in the LBD. As is shown in the Online Appendix, this implies that the coefficients associated with Leverage_{06} and $\Delta\text{Log(HP)}_{06-09} \times \text{Leverage}_{06}$ will be biased downward (attenuation bias). By contrast, the coefficient associated with the main effect of house prices, $\Delta\text{Log(HP)}_{06-09}$, is biased upward and approaches the coefficient from a univariate regression of $\Delta\text{Log(Emp)}_{07-09}$ on $\Delta\text{Log(HP)}_{06-09}$ from below as $\sigma^2_{\eta} \to \infty$. In conjunction with the first change discussed above, this implies that the coefficient associated with $\Delta\text{Log(HP)}_{06-09}$ increases for two reasons: it approaches the average elasticity of county-level employment with respect to house prices (i.e., the univariate coefficient) from below, while at the same time the average elasticity itself increases because firms become more sensitive to changes in house prices.[25]

As Table VIII, column (2) shows, all coefficients change in the way discussed above.[26] While the coefficient associated with $\Delta\text{Log(HP)}_{06-09}$ increases, the coefficients associated with Leverage_{06} and $\Delta\text{Log(HP)}_{06-09} \times \text{Leverage}_{06}$ both decrease (all relative to the coefficients in column (2) of Table VII). Importantly,

25. Whether the coefficient associated with $\Delta\text{Log(HP)}_{06-09}$ approaches the univariate coefficient from below or above depends on the sign of the (unbiased) coefficient associated with the interaction term and the sign of the mean value of county-level leverage. If both are positive, as is the case here, the coefficient associated with $\Delta\text{Log(HP)}_{06-09}$ approaches the univariate coefficient from below, meaning it increases. See the proof in the Online Appendix for details. Table 20 of the Online Appendix simulates measurement error in county-level leverage, empirically validating the results in the proof.

26. Figure 2 of the Online Appendix provides a visual impression similar to Figure III, except that county-level employment includes all firms in the LBD.

the coefficient associated with $\Delta \text{Log}(\text{HP})_{06-09} \times \text{Leverage}_{06}$ remains large and significant. Thus, despite the downward bias in this coefficient, we (still) find that variation in county-level leverage can successfully explain variation in county-level employment in response to consumer demand shocks, reaffirming that our prior results are not undone by general equilibrium adjustments. Columns (3) and (4) examine county-level employment in the nontradable sector. The results are similar to those in columns (1) and (2). Finally, columns (5) and (6) confirm prior results that there is no significant association between changes in house prices and changes in county-level employment in the tradable sector.

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. Mayer, Morrison, and Piskorski (2009), Agarwal et al. (2013), and Mian and Sufi (2014b, Chapter 10) discuss policy measures targeted at indebted households. In what follows, we discuss policy measures targeted at (nonfinancial) firms.[27]

As we discussed in Section IV, financial constraints impair firms' ability to engage in labor hoarding. That is, firms with weak balance sheets cut more jobs in response to a decline in consumer demand than they would have in the absence of financial constraints. 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.

27. 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.

> 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).

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).[28] Importantly, labor hoarding is heavily subsidized in Germany. A central pillar of German labor hoarding is the system of short-time work ("Kurzarbeit") programs encouraging firms to adjust labor through hours reductions rather than through layoffs. Under this system, firms pay workers for the actual hours worked plus an additional 60–67% 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. Arguably, the success of labor hoarding in Germany may depend on specifics of the German labor market. On a broader level, however, this discussion suggests that it might be useful to think about employment policies that target firms directly besides conventional stimulus. As Krugman (2009) argues, "here 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 example, 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 key 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

28. For discussions of Germany's "labor market miracle" in the Great Recession and its aftermath, see Burda and Hunt (2011), Dustmann et al. (2014), and Hoffmann and Lemieux (2016). The latter paper also provides a comparison with Canada and the United States.

federal funding for states to adopt or expand work-sharing programs. Such programs are similar to German-style short-time work programs: employees with reduced hours receive pro-rated unemployment benefits to supplement their paychecks. However, although work-sharing programs had been in place in 17 U.S. states during the Great Recession, take-up rates were extremely low.[29] According to commentators, the primary reasons are financial disincentives for employers and workers, burdensome filing processes, rigid work-sharing schedules, and lack of employer outreach by state agency officials (e.g., Baker 2011; Abraham and Houseman 2014; Wentworth, McKenna, and Minick 2014).

Last, a word of caution. A potential drawback of German-style subsidies is that, while effective in a crisis situation, they may impede the efficient allocation 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 scope and used as temporary relief only.

VIII. Conclusion

This article argues that firms' balance sheets played an important role in the transmission of consumer demand shocks during the Great Recession. Using establishment-level data from the U.S. Census Bureau, we find that establishments of more highly levered firms exhibit significantly larger declines in employment in response to drops in local consumer demand. We find similar results at the extensive margin: firms with higher leverage at the onset of the Great Recession are significantly more likely to close down establishments in response to local consumer demand shocks. 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. Likewise, 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.

29. Participation in work-sharing programs peaked in 2009 at about 153,000 workers, which is just over 0.1% of U.S. payroll employment (Baker 2011).

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 consumer demand. The latter disproportionately affects firms with weak balance sheets, forcing them to downsize and reduce employment, as is shown in this article. 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 to lend. This tightening of lending standards, in turn, disproportionately affects firms with weak balance sheets ("flight to quality"), reinforcing the adverse effects of consumer demand shocks.

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SUPPLEMENTARY MATERIAL

An Online Appendix for this article can be found at *The Quarterly Journal of Economics* online.

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