Firms' Internal Networks and Local Economic Shocks*

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Abstract

This paper shows that *firms* spread the adverse impacts of local employment shocks across regions through their internal networks of establishments. Linking confidential micro data at the establishment level from the U.S. Census Bureau's Longitudinal Business Database to ZIP code-level variation in house price changes during the Great Recession, we find that local establishment-level employment responds strongly to employment shocks in other regions in which the firm has establishments. Consistent with theory, the elasticity of establishment-level employment with respect to shocks in other regions is increasing with firms' financial constraints. Moreover, establishments belonging to more expansive firm networks exhibit smaller employment elasticities with respect to their own local shocks. To account for the impacts of general equilibrium adjustments, we examine aggregate employment at the county level. Similar to what we found at the establishment level, we obtain large elasticities of county-level employment with respect to employment shocks in other counties linked through firms' internal networks. Overall, our results suggest that firms play an important role in the provision of regional risk sharing and the propagation of local employment shocks across different U.S. regions.

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1 Introduction

How do firms respond to local economic shocks? Do they reallocate resources away from badly affected regions and toward less affected regions? Or do they smooth out local economic shocks by spreading their impacts across multiple production units, and thus effectively across multiple regions? These are important questions, the answers to which may improve our understanding of how risks are being shared across regions. As is well known, incomplete markets and credit constraints make it difficult to fully insure against local economic shocks.[1] And while factor mobility can, in principle, mitigate the impact of local economic shocks, there is mounting evidence that the movement of capital and labor across regions in the aftermath of shocks is sluggish and, at best, incomplete.[2] Accordingly, economists have broadly focused on the role of *public policy* in alleviating the adverse impacts of local economic shocks, including regional transfers, redistributive taxation, and "place-based" policies that directly target disadvantaged regions.[3] Little, if anything, is known about the role of *firms* in the provision of regional risk sharing, or how local shocks propagate across regions through firms' internal networks.

In this paper, we examine how firms respond to local economic shocks by building a complete network of the firm's internal organization using confidential micro data at the establishment level from the U.S. Census Bureau's Longitudinal Business Database (LBD). We obtain variation in local economic shocks by exploiting regional variation in house prices during the Great Recession. As prior research has shown, the collapse in house prices during the Great Recession caused a sharp drop in consumer spending by households (Mian, Rao, and Sufi (2013), Stroebel and Vavra (2014), Kaplan, Mitman, and Violante (2016)). This drop in consumer spending, in turn, led to large employment losses in the non-tradable industry sector: across different U.S. regions, those with larger declines in housing net worth experienced significantly larger declines in non-tradable employment (Mian and Sufi (2014), Giroud and Mueller (2017)).

[1] Beginning with Asdrubaldi, Sørensen, and Yosha (1996), a large regional risk-sharing literature rejects the null of perfect insurance across regions.

[2] See, e.g., Blanchard and Katz (1992), Notowidigo (2011), Autor, Dorn, and Hanson (2013, 2016), Autor et al. (2014), and Yagan (2016).

[3] See, e.g., Persson and Tabellini (1996a,b), Glaeser and Gottlieb (2008), Farhi and Werning (2012), Kline and Moretti (2014), Moretti (2014), Beraja (2016), and Hurst et al. (2016).

An important feature of non-tradable employment (e.g., restaurants, supermarkets, retail stores) is that it relies on *local* consumer demand. This makes it an ideal outcome variable to study the effects of local consumer demand shocks, such as those originating from falling house prices. The same feature also makes it an ideal outcome variable to study whether local consumer demand shocks spill over to other regions through firms' internal networks of establishments: while these shocks may directly affect non-tradable employment at the local level, they should *not directly* affect non-tradable employment in distant regions. Consequently, if a department store experiences a decline in employment in response to a local consumer demand shock in some other region in which the firm has establishments, then it is unlikely that this employment decline is due to a *direct* demand effect from that other region.

We find that local demand shocks affect non-tradable employment not only at the local level but also in other regions in which the firm has establishments. Both effects go in the same direction: a drop in local house prices leads to a decline in local non-tradable employment and a decline in non-tradable employment in other regions. We argue that these results are consistent with a view whereby drops in consumer spending constitute shocks to firms' budget constraints. Firms seeking to equate the marginal returns from investing across individual firm units respond to these shocks by spreading their impacts across multiple units, including units located in distant regions.

Based on our estimates, the elasticity of non-tradable employment with respect to house prices in other regions linked through firms' internal networks is between one fifth and one third of the elasticity with respect to local house prices. Thus, firms provide valuable insurance against local economic shocks. However, they do not provide full insurance. Local elasticities are between three and five times larger than elasticities with respect to house prices in other regions, implying that local firm units are (still) bearing the brunt of the shocks. This may be due to frictions inside the firm's internal resource reallocation process, such as lobbying or rent-seeking. Alternatively, it may be because firms view local drops in consumer spending in part also as shocks to local investment opportunities.

A main empirical challenge arises from separating regional spillovers through firms' internal networks from common shocks to all regions in which non-tradable firms have their establishments. We account for this possibility by including regional—precisely, ZIP code—fixed effects in all our specifications. Thus, we compare non-tradable establishments in the same ZIP code that are exposed to the same regional shock but that belong to different firms and hence to different firm networks. A possible concern is that regional shocks may differentially affect establishments in different industries. We address this concern by including ZIP code $\times$ industry fixed effects, where industries are measured at the 4-digit NAICS level. Our results remain virtually identical.

Regional shocks may differentially affect establishments even within a single 4-digit NAICS industry. For instance, different establishments within the same ZIP code and 4-digit NAICS industry may have different clienteles (e.g., high- versus low-end retail stores). To account for clientele effects, we control for the average income, age, and education in *other* ZIP codes in which the firm has establishments. Effectively, we thus compare establishments in the same ZIP code and industry that belong to firms catering to similar (demographic) segments of the population. Including these additional controls has no effect on our results.

In order to capture clientele effects, we additionally perform Placebo tests based on counterfactual firm networks. The idea is that if two firms in the same industry mutually overlap in almost all of their locations, then these firms are likely to cater to similar clienteles. To illustrate, suppose two firms in the same 4-digit NAICS industry mutually overlap in 90% of their ZIP codes (2 to 10), but firm A is additionally present in ZIP code 1, while firm B is additionally present in ZIP code 11. If our estimates are confounded by common shocks to firms' clienteles, then firm A's establishments—i.e., those in ZIP codes 1 to 10—should also be sensitive to changes in house prices in ZIP code 11, even though firm A itself has no presence in that ZIP code. Likewise, firm B's establishments should also be sensitive to changes in house prices in ZIP code 1. We find that this is not the case, suggesting that our estimates are unlikely to be confounded by common shocks to firms' clienteles.

Consumers may go to restaurants and grocery stores in neighboring regions. Thus, another empirical challenge arises from separating regional spillovers through firms' internal networks from potentially confounding *direct* demand effects from nearby regions. We find that direct demand spillovers have at best a modest effect. If we control for proximity-weighted house price changes in other ZIP codes, our estimates become only slightly weaker, and they remain highly significant. The same is true if we exclude all ZIP codes within a 50, 100, 150, or 250 mile radius.

We conclude our establishment-level analysis with some additional tests. Regional insurance through firms' internal networks implies that local firm units absorb some of the impact of shocks from other regions. The flip side is that firm units in other regions absorb some of the impact of local shocks. Accordingly, units belonging to firms with more expansive regional networks should be less sensitive to (their own) local economic shocks. Indeed, we find that the elasticity of employment with respect to *local* house prices is smaller for establishments belonging to more expansive regional networks. Second, theory predicts that the extent to which firms reallocate internal resources in response to local economic shocks should depend on their financial constraints. Consistent with this prediction, we find that establishments belonging to more financially constrained firms exhibit larger elasticities of employment with respect to house prices in other regions. In fact, for the least financially constrained firms in our sample, we find no evidence that firms reallocate internal resources in response to local economic shocks. Lastly, we examine if firm units in close proximity to headquarters are more insulated from economic shocks. This could be due to efficiency (e.g., information-based) reasons, lobbying, or simply because management feels more responsible for nearby firm units. Consistent with this notion, we find that establishments which are located closer to headquarters exhibit smaller elasticities of employment with respect to *both* local house prices *and* house prices in other regions in which the firm has establishments.

Regional spillovers through firms' internal networks may be irrelevant in general equilibrium if workers of multi-region firms that are laid off due to shocks in other regions are re-employed by (local) firms which are less exposed to these regions. To see whether the distribution of firm networks also matters in the aggregate, the final part of our paper examines aggregate non-tradable employment at the county level. Similar to what we found at the establishment level, we find sizable elasticities of non-tradable county-level employment with respect to house prices in other counties linked through firms' internal networks. That being said, relative to local elasticities, the magnitudes are somewhat smaller than in our establishment-level analysis, possibly reflecting the impacts of general equilibrium adjustments.

To account for the possibility of common shocks at the county level—which in turn are correlated with house price changes—we control for similarity-weighted house price changes in other counties based on similarities in income, age, education, household debt, and non-tradable employment shares. The idea is that counties which are more similar are more likely to be exposed to similar county-level shocks. We also consider a subset of counties in which house prices did *not* fall during the Great Recession. Linking such counties to other counties in which house prices fell sharply makes it less likely that spillovers across counties are the result of common county-level shocks. In all of these cases, the elasticity of county-level employment with respect to house prices in other counties linked through firms' internal networks remains highly significant.

To rule out potentially confounding *direct* demand effects from other counties, we proceed as in our establishment-level analysis. That is, we control for proximity-weighted house price changes in other counties, and we exclude all counties within a 50, 100, 150, or 250 mile radius. As in our establishment-level analysis, we find that our estimates become only slightly weaker, and they remain highly significant.

Lastly, we explore the possibility that local demand shocks may *indirectly* affect non-tradable employment in other counties via the trade channel. Specifically, they may lead to employment losses in other counties in the tradable sector, which may spill over to the non-tradable sector if workers that are laid off cut back on their local grocery shopping and restaurant visits. A necessary condition for this (trade) channel to explain our results is that changes in house prices affect tradable employment in other counties. As it turns out, however, changes in house prices have no significant effect on tradable employment, neither at the local level nor in other counties.

Our paper contributes to several strands of literature. As mentioned earlier, a large literature examines how *public policy* can mitigate the adverse impacts of local economic shocks through redistributive taxation, regional transfers, and "place-based" policies that directly target disadvantaged regions.[4] By contrast, our paper shows that *firms* play an

[4]Regional transfers may be implicit. For instance, Hurst et al. (2016) show that lack of regional variation in mortgage rates on loans secured by government-sponsored enterprises (GSEs) constitutes an implicit transfer to regions that are more likely to be hit by local economic shocks.

important role in the provision of risk sharing across regions.

Second, our paper is related to a recent literature in macroeconomics that studies how shocks propagate throughout the economy. This literature has focused on input-output networks (e.g., Acemoglu et al. (2012), Caliendo et al. (2014), Acemoglu, Akcigit, and Kerr (2016), Barrot and Sauvagnat (2016)), financial networks (e.g., Acemoglu, Ozdaglar, and Tahbaz-Salehi (2012), Cabrales, Gale, and Gottardi (2015)), and social networks (Bailey et al. (2016)). In contrast, little is known about whether, and how, firms' *internal* networks facilitate the propagation of local economic shocks, and how this affects macroeconomic aggregates. Also, an important benefit of using U.S. Census Bureau data is that we can completely characterize the entire network structure: the LBD includes the ZIP codes and firm affiliations of all (payroll) establishments in the U.S.

Lastly, our paper adds to a growing literature that studies the massive collapse in house prices in the Great Recession and its implications for consumer spending (e.g., Mian, Rao and Sufi (2013), Stroebel and Vavra (2014), Kaplan, Mitman, and Violante, (2016)) and non-tradable employment (e.g., Mian and Sufi (2014), Giroud and Mueller (2017)).[5] Our paper shows that local consumer demand shocks not only affect local non-tradable employment but also non-tradable employment in other regions linked through firms' internal networks. Indeed, we find sizable elasticities of non-tradable employment with respect to demand shocks in other regions, echoing a point made in Beraja, Hurst, and Ospina (2016) that it is difficult to draw inferences about aggregate economic activity based on local elasticities alone. In our empirical setting, local elasticities and those with respect to demand shocks in other regions have the same sign. Thus, accounting for spillovers from other regions strengthens the role of consumer demand in explaining the sharp decline in U.S. employment during the Great Recession.

The rest of this paper is organized as follows. Section 2 describes the data, variables, empirical strategy, and summary statistics. Section 3 documents how firms spread the impacts of local economic shocks across regions through their internal networks of establishments. Section 4 considers the implications of firm's redistributive policies for aggregate non-tradable employment at the county level. Section 5 concludes.

[5]See Berger et al. (2016) for a theory model that produces large consumption responses to house price changes in line with estimates found in empirical studies.

2 Data, Variables, and Summary Statistics

2.1 Data

We use confidential micro data at the establishment level from the U.S. Census Bureau's Longitudinal Business Database (LBD). An establishment is a "single physical location where business is conducted" (Jarmin and Miranda (2002, p. 5)), e.g., a restaurant, grocery store, gas station, or department store. The LBD covers all business establishments in the U.S. with at least one paid employee. Our data include information on employment, location, industry affiliation, and firm affiliation.

We focus on establishments in the non-tradable industry sector. An important feature of non-tradable employment is that it relies on *local* consumer demand. As explained in the Introduction, this makes it an ideal outcome variable to study whether local consumer demand shocks spill over to other regions through firms' internal networks: while these shocks may directly affect non-tradable employment at the local level, they should not directly affect non-tradable employment in distant regions. We classify industries as non-tradable using the classification scheme in Mian and Sufi (2014), which is based on restaurants and retail industries. Accordingly, there are 26 four-digit NAICS industries in the non-tradable sector. Among those, the largest ones in terms of U.S. employment shares are full-service restaurants (3.76%), limited-service eating places (3.40%), grocery stores (2.13%), department stores (1.36%), other general merchandise stores (1.12%), clothing stores (1.06%), automobile dealers (1.05%), health and personal care stores (0.89%), and gasoline stations (0.73%).

We match individual establishments to ZIP code-level house prices using house price data from Zillow. Our sample period is from 2006 to 2009.[6] 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 (2014), "Δ Housing Net Worth, 2006–2009." The correlation at the MSA level is 86.3 percent. They are also highly correlated with changes in house prices from 2006 to 2009 using data from the Federal Housing Finance Agency (FHFA). The correlation at the MSA level is 96.4 percent.

[6] Zillow house price data have been used in, e.g., Keys et al. (2014), Mian, Sufi, and Trebbi (2015), Kaplan, Mitman, and Violante (2016), Bailey et al. (2016), and Giroud and Mueller (2017).

In our establishment-level analysis, we focus on firms operating in multiple ZIP codes ("multi-region" firms). Our sample consists of 385,000 non-tradable establishments accounting for 64.7% of non-tradable U.S. employment in 2006.[7] The high employment share of multi-region firms is reflective of the prominent role of national restaurant and retail chains in the non-tradable industry sector. In our county-level analysis, we consider *total* non-tradable employment at the county level, that is, we include employment by single-region firms. Our sample consists of 1,000 counties representing 85.8 percent of non-tradable U.S. employment in 2006.

In some of our regressions, we additionally use data from the 2000 Decennial Census (population, age, education), the IRS (adjusted gross income per capita in 2006), and the FRBNY Consumer Credit Panel (household debt in 2006). In one regression, we compute measures of firms' financial constraints using data from Compustat (firm leverage, KZ index, WW index, all in 2006). To this end, we match establishments in the LBD to firms in Compustat using the Compustat-SSEL bridge maintained by the U.S. Census Bureau. As 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).

2.2 Variables and Empirical Specification

In our establishment-level analysis, we examine how non-tradable establishment-level employment during the Great Recession responds to changes in local house prices in the establishment's ZIP code as well as changes in house prices in other ZIP codes in which the firm has establishments. We estimate the following equation:

$$\Delta \text{ Log}(\text{Emp}_i)_{07-09} = \alpha + \eta_1 \; \Delta \text{ Log}(\text{HP}_k)_{06-09} + \eta_2 \; \textstyle\sum_{l \neq k} \omega_{j,k,l} \; \Delta \text{ Log}(\text{HP}_l)_{06-09} + \varepsilon_i, \quad (1)$$

where Δ Log(Emp$_i$)$_{07-09}$ is the percentage change in employment from 2007 to 2009 at establishment i of firm j in ZIP code k, Δ Log(HP$_k$)$_{06-09}$ is the percentage change in house prices from 2006 to 2009 in ZIP code k, and $\sum_{l \neq k} \omega_{j,k,l} \; \Delta$ Log(HP$_l$)$_{06-09}$ is the linkage-weighted percentage change in house prices from 2006 to 2009 in ZIP codes $l \neq k$.

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

For brevity, we write Δ Log(HP)$_{06-09}$ (other) in lieu of $\sum_{l \neq k} \omega_{j,l,k}\ \Delta\ \text{Log}(\text{HP}_l)_{06-09}$ in our figures and tables. The elasticities of interest are η_1 and, especially, η_2. All regressions are weighted by establishment size (number of employees) and include either industry, ZIP code, or ZIP code $\times$ industry fixed effects. Industries are measured at the 4-digit NAICS level. In regressions that include either ZIP code or ZIP code $\times$ industry fixed effects, $\Delta\ \text{Log}(\text{HP}_k)_{06-09}$ is absorbed by the fixed effects—they account for any common shock at the ZIP code or ZIP code $\times$ industry level, including those arising in general equilibrium through spillovers from other regions (e.g., price effects). Standard errors are double clustered at the firm and county level.

At the establishment level, the linkage weights $\omega_{j,k,l}$ specify the relative weight of house price changes in ZIP code l for an establishment of firm j in ZIP code k. We impose the minimal assumption that linkage weights be proportional to firms' non-tradable employment in a given ZIP code:

$$\omega_{j,k,l} = \frac{\text{Emp}_{j,l}}{\sum_{m \neq k} \text{Emp}_{j,m,}}.$$

Hence, a local economic shock in ZIP code l matters relatively more for an establishment of firm j in ZIP code k if the firm is relatively more exposed to ZIP code l, as measured by its employment in ZIP code l relative to other ZIP codes $m \neq k$.[8] Simply put, an establishment is relatively more exposed to a given ZIP code if its firm is relatively more exposed to that ZIP code. Naturally, a ZIP code has zero weight if the firm has no employees in that ZIP code.

In our county-level analysis, we examine how non-tradable county-level employment responds to changes in a county's own house prices as well as changes in house prices in other counties linked through firms' internal networks. Similar to above, we estimate the following equation:

$$\Delta\ \text{Log}(\text{Emp}_i)_{07-09} = \alpha + \eta_1\ \Delta\ \text{Log}(\text{HP}_i)_{06-09} + \eta_2\ \sum_{j \neq i} \lambda_{i,j}\ \Delta\ \text{Log}(\text{HP}_j)_{06-09} + \varepsilon_i,$$

where $\Delta\ \text{Log}(\text{Emp}_i)_{07-09}$ is the percentage change in non-tradable employment from 2007

[8] Bailey et al. (2016) use analogous linkage weights in the context of social networks.

to 2009 in county i, Δ Log(HP$_i$)$_{06-09}$ is the percentage change in house prices from 2006 to 2009 in county i, and $\sum_{j\neq i} \lambda_{i,j}$ Δ Log(HP$_j$)$_{06-09}$ is the linkage-weighted percentage change in house prices from 2006 to 2009 in counties $j \neq i$. Similar to above, we write Δ Log(HP)$_{06-09}$ (other) in lieu of $\sum_{j\neq i} \lambda_{i,j}$ Δ Log(HP$_j$)$_{06-09}$ for brevity. All regressions are weighted by county size (number of employees). Standard errors are clustered at the state level.

At the county level, the linkage weights $\lambda_{i,j}$ specify the relative weight of house price changes in county j for non-tradable employment in county i. They are obtained by taking the employment-weighted average of individual establishment-level linkage weights $\zeta_{h,i,j}$ within a given county:

$$\lambda_{i,j} = \sum_h \frac{\text{Emp}_{h,i}}{\sum_k \text{Emp}_{k,i}} \zeta_{h,i,j},$$

where $\sum_h \left(\text{Emp}_{h,i} / \sum_k \text{Emp}_{k,i}\right) = 1$. The establishment-level linkage weights $\zeta_{h,i,j}$ are constructed similarly to above, except that establishments are aggregated at the firm-county level and exposure is measured with respect to counties instead of ZIP codes. Hence, a local economic shock in county j matters relatively more for county i if its establishments are relatively more exposed to county j and these establishments have relatively high employment shares within county i.

2.3 Firms' Responses to Local Economic Shocks

How *should* firms respond to local economic shocks? The extant literature on within-firm resource reallocation, which is part of a larger literature in corporate finance studying the functioning of internal capital markets, emphasizes two main aspects: firms' financial constraints and the precise nature of the economic shock.[9]

2.3.1 Shocks to Investment Opportunities

Financially unconstrained firms

Suppose a firm has two establishments, one in region A and one in region B. Suppose further that region A experiences a decline in investment opportunities such that,

[9] See Stein (2003) for a comprehensive survey of the literature.

in a first-best world, it is optimal to reduce the scale of the firm's operations. In the firm's internal resource allocation (or capital budgeting) process, corporate headquarters (HQ) consequently allocates a smaller budget, and hence fewer resources (capital, labor), to region A. Since the firm faces no binding overall resource constraint, there are no implications for region B.

Financially constrained firms

If the firm is financially constrained, it allocates budgets across regions A and B to equate the marginal returns from investing across regions. This is illustrated in Panel (A) of Figure I where, for simplicity, labor is the only input in the firm's production process. Importantly, because the firm faces a binding resource constraint, its scale of operations in each region is below the first-best optimal level. If region A experiences a decline in investment opportunities, HQ allocates a smaller budget, and hence fewer resources, to region A, which frees up resources for region B. Hence, labor in region B expands. Precisely, HQ shifts resources from region A to region B until the marginal returns from investing across regions are again equalized. In equation (1), this implies a *negative* coefficient η_2 : a decline in house prices in ZIP codes $m \neq k$ leads to an *increase* in establishment-level employment in ZIP code k.[10,11]

2.3.2 Shocks to Firms' Budget Constraints

Financially unconstrained firms

Suppose now that region A experiences a shock to firm revenues, affecting the firm's overall budget constraint. If investment opportunities are unchanged, the firm simply raises new funds to make up for the budget shortfall. Nothing real changes, neither in region A nor in region B.

Financially constrained firms

If the firm is financially constrained, it optimally spreads the budget shock across

[10] See Stein (1997) for a theory model in which the firm reallocates resources towards units whose relative investment opportunities have increased and Giroud and Mueller (2015) for empirical evidence based on idiosyncratic shocks to plants' investment opportunities.

[11] The null hypothesis, $\eta_2 = 0$, implies that firms are either not financially constrained or, if they are, that they do not reallocate internal resources in response to local economic shocks.

multiple regions. This is illustrated in Panel (B) of Figure I, where the firm not only scales down its operations in region A but also in region B, effectively shifting resources from region B to region A ("cross-subsidization"). Consequently, employment in both regions declines. In equation (1), this implies a *positive* coefficient η_2 : a decline in house prices in ZIP codes $m \neq k$ leads to a *decrease* in establishment-level employment in ZIP code k.[12]

2.3.3 Local Consumer Demand Shocks in the Great Recession

Drops in local consumer spending during the Great Recession arguably tightened firms' budget constraints. Whether, and to what extent, firms perceived these drops also as shocks to investment opportunities is less clear. If firms view drops in local consumer spending as fundamental shocks to investment opportunities, then even unconstrained firms should make significant employment cuts (see Section 2.3.1). However, Giroud and Mueller (2017) find that while financially constrained firms made large employment cuts in response to local consumer demand shocks in the Great Recession, financially unconstrained firms adjusted their employment only little (Table II and Panel (A) of Figure I).[13] Further, and consistent with drops in local consumer spending representing shocks to firms' budget constraints, the authors find that financially unconstrained firms respond to these shocks by increasing their short- and long-term borrowing. By contrast, financially constrained firms are unable to borrow more and thus forced to downsize, lay off employees, and close down establishments. Ultimately, whether firms perceived drops in local consumer spending in the Great Recession primarily as budget shocks or as fundamental shocks to investment opportunities remains an empirical question, one which the sign of the coefficient η_2 in equation (1) can be informative about.

[12] See Inderst and Mueller (2003) for a theory model in which HQ smoothes out cash-flow shocks by cross-subsidizing firm units with resources from other units and Lamont (1997) for empirical evidence showing that in response to a decline in oil revenues, diversified oil companies cut investment not only in their oil segments but also in unrelated segments, such as chemicals, railroads, and shipbuilding.

[13] Survey evidence by Campello, Graham, and Harvey (2010) supports this view. 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%.

2.4 Summary Statistics

Table I provides basic summary statistics. In the top part of Panel (A), the sample consists of firms operating in multiple ZIP codes ("multi-region firms") and the unit of observation is at the establishment level, consistent with our empirical analysis in Section 3. As can be seen, non-tradable establishments have on average 28.9 employees and are linked to 812.9 other ZIP codes through their firms' internal networks (based on 2006 figures). This is sizable network, reflecting the prominent role of national restaurant and retail chains in the non-tradable sector. During the Great Recession, non-tradable establishments experience a drop in employment of 3.1 percent, while house prices at the ZIP code level fell by 14.5 percent.

The bottom part of Panel (A) provides basic county-level summary statistics. The sample consists of all non-tradable firms in a county, including single-region firms, consistent with our empirical analysis in Section 4. As is shown, the average county has 1,074 establishments and 18,490 employees in the non-tradable sector, accounting for 18.6 percent of total county-level employment. During the Great Recession, aggregate non-tradable county-level employment fell by 3.6 percent, which is slightly higher than the 3.1 percent reported above for multi-region firms.

To get a sense of whether, and how, firms' internal networks are correlated with demographic and other characteristics, Panel (B) of Table I shows correlations of the network-based linkage weights, ω and λ, with corresponding linkage weights based on proximity, population, income, education, age, and household debt. While most of these correlations are insignificant, those with proximity and population are significant. Both correlations are intuitive. For one, many non-tradable firms are regional firms. Second, national restaurant and retail chains are likely to have a bigger presence in regions with more potential customers. We address both correlations in our empirical analysis. As for population, we find that (counterfactual) networks based on population weights are unable to produce significant spillovers across regions (see Tables II and IX). As for proximity, we show that our estimates are robust to excluding proximate regions or controlling for proximity-weighted house price changes in other regions (see Tables V and X).

3 Firms' Internal Networks and Local Shocks

3.1 Main Establishment-Level Results

Figure II provides a visual impression by plotting the relationship between changes in establishment-level employment during the Great Recession and either changes in ZIP code-level house prices (top panel) or changes in house prices in other ZIP codes in which the firm has establishments (bottom panel). To filter out any confounding effects of Δ Log(HP)$_{06-09}$ (other) when plotting the relationship between Δ Log(Emp)$_{07-09}$ and Δ Log(HP)$_{06-09}$, we compute the residuals from a regression of Δ Log(Emp)$_{07-09}$ on a constant and Δ Log(HP)$_{06-09}$ (other). These residuals represent the component of Δ Log(Emp)$_{07-09}$ that is orthogonal to, and thus unexplained by, Δ Log(HP)$_{06-09}$ (other). For each percentile of Δ Log(HP)$_{06-09}$, the plot shows the mean values of the residuals and Δ Log(HP)$_{06-09}$, respectively. We proceed analogously in the bottom panel when plotting the relationship between Δ Log(Emp)$_{07-09}$ and Δ Log(HP)$_{06-09}$ (other).

As is shown in the top panel of Figure II, there is a positive relationship between changes in establishment-level employment and changes in local house prices at the ZIP code level. The elasticity of employment with respect to local house prices is 0.116, implying that a ten percent decline in local house prices is associated with a 1.16 percent decline in employment at the establishment level. (The average decline in house prices at the ZIP code level between 2006 and 2009 is 14.5 percent; see Table I.) The bottom panel shows the relationship between changes in establishment-level employment and changes in house prices in *other* ZIP codes in which the firm has establishments. Here, the elasticity of employment with respect to house prices in other ZIP codes is 0.029, which is about 25 percent of the elasticity with respect to local house prices. Thus, employment at the establishment level is highly sensitive not only to local house price changes but also to house price changes in other regions in which the firm has establishments.

Table II confirms this visual impression using regression analysis. All regressions include industry fixed effects. Industries are measured at the 4-digit NAICS level. In column (1), the elasticity of employment with respect to local house prices is 0.109, which is only slightly lower than in our graphical analysis. Accordingly, a ten percent decline in local house prices is associated with a 1.09 percent decline in employment at the establishment level. Column (2) includes the effect of changes in house prices in other ZIP codes in which the firm has establishments. While the coefficient associated with changes in local house prices, Δ Log(HP)$_{06-09}$, drops slightly, the coefficient associated with changes in house prices in other ZIP codes, Δ Log(HP)$_{06-09}$ (other), is highly significant. The elasticity of employment with respect to house prices in other ZIP codes is 0.028, which is about 30 percent of the elasticity with respect to local house prices. Both elasticities have the same sign, consistent with the graphical analysis in Panel (B) of Figure I, where firms reallocate resources from less affected regions to badly affected regions. Accordingly, firms provide valuable insurance against local economic shocks. However, they do not provide full insurance. The elasticity of employment with respect to local house prices is more than three times larger than the elasticity with respect to house prices in other regions, implying that local firm units are (still) bearing the brunt of the shocks. This may be due to frictions inside the firm's internal resource reallocation process, such as lobbying or rent-seeking. Alternatively, and consistent with the graphical analysis in Panel (A) of Figure I, it may be because firms view drops in local consumer spending in part also as shocks to local investment opportunities.

Perhaps employment at the establishment level responds *generically* to house price changes in other regions? To examine this possibility, we perform a number of Placebo tests. In column (3), we assign equal weight to all other ZIP codes. In columns (4) to (6), we replace the linkage weights ω with corresponding linkage weights based on population, income, and household debt.[14] Lastly, in column (7), we randomly select other ZIP codes. Precisely, for each establishment, we replace all ZIP codes the establishment is currently linked to ($\omega > 0$) with randomly selected ZIP codes. We then estimate equation (1) and store the coefficients and standard errors. We repeat this process 1,000 times. The results in column (7) display the average coefficients and standard errors, respectively, based on the 1,000 Placebo regressions. As can be seen, in all of the Placebo tests, the effect of house price changes in other regions is small and insignificant.

[14] Mian and Sufi (2011, 2014), Mian, Rao, and Sufi (2013), Keys et al. (2014), Berger et al. (2015), and Baker (2015) all emphasize the role of household debt in the Great Recession.

3.2 Common Regional Shocks

A main empirical challenge arises from separating regional spillovers through firms' internal networks from common shocks to regions in which non-tradable firms have their establishments. Such common regional shocks—if they are correlated with house price changes—could potentially explain why the elasticity of employment with respect to local house prices has the same sign as the elasticity with respect to house prices in other regions in which the firm has establishments.

Table III addresses confounding effects due to common regional shocks. In column (1), we include ZIP code fixed effects. These fixed effects account for any shock at the regional level as well as spillovers from one region to another, e.g., due to price or other general equilibrium adjustments. We thus compare non-tradable establishments in the same ZIP code that are exposed to the same regional shock but that belong to different firms and hence to different firm networks. As can be seen, the elasticity of employment with respect to house prices in other ZIP codes in which the firm has establishments is almost identical to our previous estimate in column (2) of Table II. A potential concern with this strategy is that regional shocks may differentially affect establishments in different industries. In column (2), we address this concern by including ZIP code $\times$ industry fixed effects. Industries are measured at the 4-digit NAICS level. As can be seen, our results remain virtually identical.

Regional shocks may differentially affect establishments even within a single 4-digit NAICS industry. For instance, different establishments within the same ZIP code and 4-digit NAICS industry may have different clienteles (e.g., high- versus low-end retail stores). To account for clientele effects, we control in columns (3) to (6) for the weighted average income, age, and education in the *other* ZIP codes in which the firm is present. All regressions include ZIP code $\times$ industry fixed effects. Effectively, we thus compare establishments in the same ZIP code and industry that belong to firms catering to similar (demographic) segments of the population. As can be seen, including these additional controls does not affect our results.

In Table IV, we account for clientele effects by conducting Placebo tests based on counterfactual firm networks. The idea is that if two firms in the same industry mutually overlap in almost all of their locations, then they are likely to cater to similar clienteles. This is illustrated in Figure III. Firm A has establishments in locations 1 to 10, while firm B has establishments in locations 2 to 11. Suppose firms A and B are in the same industry. Given that the two firms overlap in 90% of their locations, the counterfactual assumption is that—based on the firms' common clienteles—firm A *could have been* in location 11, while firm B *could have been* in location 1. Thus, if our estimates are confounded by common shocks to firms' clienteles, then firm A's establishments—i.e., those in locations 1 to 10—should also be sensitive to changes in house prices in location 11, even though firm A itself has no presence in that location. Likewise, firm B's establishments should also be sensitive to changes in house prices in location 1.

In our Placebo tests, we identify all non-tradable firms in the same industry that mutually overlap in at least either 75% or 90% of their locations. Location is defined either at the ZIP code or county level. Industries are measured either at the 3- or 4-digit NAICS level.[15] In the spirit of the above example, we estimate the elasticity of employment at the establishment level with respect to house prices in (counterfactual) locations in which the firm *could have been*. As can be seen, regardless of which specification we use, this elasticity is small and insignificant. Hence, our estimates are unlikely to be confounded by common shocks to firms' clienteles.

3.3 Direct Demand Spillovers

Consumers may go to restaurants and grocery stores in neighboring regions. Thus, another empirical challenge arises from separating regional spillovers through firms' internal networks from potentially confounding *direct* demand effects from nearby regions.[16] That is, falling house prices in ZIP code j may affect non-tradable establishment-level employment in ZIP code i not because of firms' internal networks but rather because consumers in ZIP code j cut back on their restaurant visits and grocery shopping in ZIP code i.

[15] To obtain strong counterfactuals, we restrict our sample to "pure industry" firms that have *all* of their establishments in a single industry. As it turns out, this sample restriction does not impose a serious limitation. In the non-tradable sector, 94.6% (90.9%) of multi-region firms have all of their establishments in a single 3-digit (4-digit) NAICS industry (based on 2006 figures).

[16] Likewise, house prices may be spatially correlated.

Table V addresses potentially confounding direct demand effects from nearby regions. In column (1), we directly control for proximity-weighted changes in house prices in other ZIP codes. While the coefficient associated with this control is (marginally) significant, the coefficient associated with Δ Log(HP)$_{06-09}$ (other) drops only slightly and remains highly significant. Hence, direct demand spillovers have at best a modest effect. In columns (3) to (5), we exclude all ZIP codes within a 50, 100, 150, or 250 mile radius based on the ZIP codes' geographical centroids. As is shown, the coefficient associated with Δ Log(HP)$_{06-09}$ (other) remains highly significant in all regressions.

3.4 Scope of Firms' Regional Networks

Regional insurance through firms' internal networks implies that local firm units absorb some of the impact of shocks from other regions. The flip side is that firm units in other regions absorb some of the impact of local shocks. Accordingly, if firms provide insurance across regions, units belonging to firms with more expansive regional networks should be less sensitive to (their own) local economic shocks.

We test this prediction in Table VI using different measures of the scope of firms' regional networks. In column (1), we use a dummy indicating whether a firm operates in multiple ZIP codes ("multi-region firm"). In column (2), we use the number of ZIP codes in which the firm operates. Lastly, in column (3), we use a firm-level Herfindahl-Hirschman Index (HHI) measuring the firm's geographical concentration based on its employment at the ZIP code level. (We use one minus the HHI to allow all three measures to have the same economic interpretation.) In column (1), our sample includes both multi- and single-region firms. In columns (2) and (3), we use our original sample of multi-region firms given that differences between single- and multi-region firms have already been captured in column (1). As is shown, regardless of how we measure the scope of firms' regional networks (RN), the interaction term Δ Log(HP)$_{06-09}$ $\times$ RN is always negative and highly significant. Thus, consistent with firms providing insurance across regions, establishments belonging to firms with more expansive regional networks exhibit lower elasticities of employment with respect to local house prices.[17]

[17] Firms with more expansive regional networks are typically larger. To account for the effects of firm

3.5 Financial Constraints

Theory predicts that the extent to which firms reallocate internal resources in response to local economic shocks depends on their financial constraints (see Section 2.3). In Table VII, we take this prediction to the data using different measures of firms' financial constraints. In column (1), we use firm leverage. This measure is based on Giroud and Mueller (2017), who argue that firms with higher leverage in 2006, at the onset of the Great Recession, were more financially constrained during the Great Recession. In columns (2) and (3), we use the Kaplan-Zingales index (Kaplan and Zingales (1997)) and the Whited-Wu index (Whited and Wu (2006)), respectively. Both indices are widely used in the finance literature. All three measures are only available for public firms. Accordingly, we restrict our sample to firms that have a match in Compustat.

As can be seen, regardless of how we measure firms' financial constraints (FC), the interaction term Δ Log(HP)$_{06-09}$ (other) $\times$ FC is always positive and highly significant. Hence, establishments of more financially constrained firms exhibit larger elasticities of employment with respect to house prices in other ZIP codes in which the firm has establishments. In fact, for the least financially constrained firms in our sample, we find no evidence that firms reallocate internal resources in response to local economic shocks. Finally, we find that establishments of more financially constrained firms exhibit larger elasticities of employment with respect to local house prices. Altogether, these results suggest that financial constraints matter, both for how firms respond locally to shocks and how they smooth out the impacts of these shocks across regions.

3.6 Proximity to Headquarters

An interesting question is whether firm units in close proximity to headquarters (HQ) are more insulated from economic shocks. For instance, it may be easier for such units to successfully lobby HQ, or management may simply interact more with, and hence feel more socially responsible for, units that are located closer to HQ. But proximity may

size, we have re-estimated columns (1) to (3) controlling for firm size (number of employees) in 2006 as well as its interaction with Δ Log(HP)$_{06-09}$. While the coefficient associated with Δ Log(HP)$_{06-09}$ $\times$ RN drops slightly, it remains highly significant in all regressions.

also facilitate information flows and monitoring, leading to higher marginal returns.[18] Accordingly, it may be efficient to be "protective" of proximate firm units.

Table VIII examines if establishments that are located closer to HQ are less sensitive to house price changes. We use three different measures of proximity. In columns (1) and (2), we use a dummy indicating whether the establishment and HQ are located in the same ZIP code and county, respectively. In column (3), we use the (inverse) geographical distance between the establishment and HQ. Two results stand out. First, establishments that are located closer to HQ exhibit smaller elasticities of employment with respect to local house prices. Second, establishments that are located closer to HQ also exhibit smaller elasticities of employment with respect to house prices in other ZIP codes in which the firm has establishments. Together, these results suggest that firm units in close proximity to HQ are more insulated from local economic shocks.

4 Aggregate Employment at the County Level

Regional spillovers through firms' internal networks may be irrelevant in general equilibrium if workers of multi-region firms that are laid off due to shocks in other regions are re-employed by (local) firms which are less exposed to these regions. In principle, such general equilibrium adjustments may be impaired by wage and price stickiness. In addition, the extent of labor reallocation may depend on search and matching frictions in the labor market as well as labor adjustment costs. Empirical evidence suggests that labor market frictions were particularly severe during the Great Recession (e.g., Davis, Faberman, and Haltiwanger (2013), Şahin et al. (2014)). Notably, Foster, Grim, and Haltiwanger (2014) find that the intensity of labor reallocation fell rather than rose during the Great Recession, in contrast to previous recessions. They conclude that "job reallocation (creation plus destruction) is at its lowest point in 30 years during the Great Recession and its immediate aftermath" (p. 10).

To see whether the distribution of firm networks also matters in the aggregate, we examine aggregate non-tradable employment at the county level. We consider non-tradable

[18] Using data on manufacturing plants from the U.S. Census Bureau, Giroud (2013) finds that proximity to HQ positively affects plant-level productivity.

employment by *all* firms in a county, including single-region firms. Hence, our setting accounts for the possibility that workers laid off due to shocks in other regions are re-employed either by other multi-region firms or by (local) single-region firms. Linkages across counties are based on firms' internal networks as described in Section 2.2.

4.1 Main County-Level Results

Figure IV provides a visual impression by plotting the relationship between changes in non-tradable county-level employment during the Great Recession and either changes in county-level house prices (top panel) or changes in house prices in other counties linked through firms' internal networks (bottom panel). As is shown in the top panel, the elasticity of employment with respect to a county's own house prices is 0.129. This is slightly larger than the corresponding elasticity in the top panel of Figure II as our sample now also includes small single-region firms. In the bottom panel, the elasticity of employment with respect to house prices in other counties linked through firms' internal networks is 0.030. Accordingly, non-tradable county-level employment is highly sensitive to changes in house prices in other counties, suggesting that spillovers through firms' internal networks also matter for aggregate employment.

Table IX confirms this visual impression using regression analysis. All regressions include demographic controls (income, age, education) as well as the county-specific employment shares of all 2-digit NAICS industries to account for the possibility that counties with exposure to certain industries are harder hit during the Great Recession (Mian and Sufi (2014)). In column (1), the elasticity of non-tradable county-level employment with respect to a county's own house prices is 0.122, which is only slightly lower than in our graphical analysis. Column (2) includes the effect of changes in house prices in other counties linked through firms' internal networks. While the coefficient associated with changes in a county's own house prices, Δ Log(HP)$_{06-09}$, drops slightly, the coefficient associated with changes in house prices in other counties, Δ Log(HP)$_{06-09}$ (other), is highly significant. The elasticity of non-tradable county-level employment with respect to house prices in other counties is 0.024, which is about 20 percent of the elasticity with respect to a county's own house prices. This is somewhat less than in our establishment-level analysis, possibly reflecting the impacts of general equilibrium adjustments.

A quick back-of-the-envelope calculation suggests that consumer demand shocks can explain a significant portion of the drop in non-tradable employment during the Great Recession. As the summary statistics in Table I show, non-tradable employment at the county level dropped by 3.6 percent, while county-level house prices fell by 14.5 percent. Given the elasticities of 0.115 and 0.024 in column (2), a drop in house prices of 14.5 percent implies a drop in non-tradable employment of $(0.115 + 0.024) \times 14.5\% = 1.95\%$, which is more than half of the overall decline in non-tradable employment during the Great Recession. Notably, a substantial fraction of the decline in non-tradable employment explained by consumer demand shocks ($0.024/0.139 = 0.173$) is due to shocks originating in other counties linked through firms' internal networks.

To address concerns that non-tradable county-level employment may be generically sensitive to house price changes in other counties, we perform the same Placebo tests as in Table II. In column (3), we assign equal weight to all other counties. In columns (4) to (6), we replace the linkage weights λ with corresponding linkage weights based on population, income, and household debt. Lastly, in column (7), we randomly select other counties. In all of these Placebo tests, the effect of house price changes in other counties is small and insignificant.

4.2 Common County-Level Shocks

As in our establishment-level analysis, a main empirical challenge arises from common shocks at the county level that are correlated with house price changes. In Table X, we account for this possibility by controlling for similarity-weighted house price changes in other counties. Similarity weights place more weight on other counties that are more "similar" (i.e., smaller absolute difference) to a given county. The underlying idea is that counties which are more similar are more likely to be exposed to similar county-level shocks. In columns (1) to (3), we examine similarities based on income, education, and age. In column (4), we examine similarities based on household debt. Lastly, in column (5), we examine similarities based on the share of non-tradable employment in a county. As is shown, in all of these regressions, the coefficient associated with house price changes in "similar" counties is small and insignificant. By contrast, our main coefficient of interest—that associated with Δ Log(HP)$_{06-09}$ (other)—is always highly significant. Thus, our estimates are unlikely to be confounded by common shocks at the county level that are correlated with house price changes.

In Table XI, we account for the possibility of common shocks at the county level in a different way. Precisely, we exploit the fact that not all counties experienced a collapse in house prices during the Great Recession. Linking such counties to other counties in which house prices fell sharply makes it unlikely that spillovers across counties are the result of common county-level shocks that are correlated with house price changes. In column (1), we focus on counties in which house prices *increased* during the Great Recession. In column (2), we focus on counties in which house prices changed only little, defined as changes of less than ± 2.5 percent. In both cases, the coefficient associated with Δ Log(HP)$_{06-09}$ (other) is highly significant. Thus, even though these counties did not experience house price shocks of their own, their employment is highly sensitive to house price changes in other counties linked through firms' internal networks.

4.3 Direct Demand Spillovers

Another empirical challenge arises from potentially confounding direct demand effects from nearby counties. Similar to our establishment-level analysis, we account for such direct demand effects by controlling for proximity-weighted changes in house prices in other counties and excluding counties within a certain radius. Table XII presents the results. In column (1), we control for promiximity-weighted changes in house prices in other counties. While the coefficient associated with this control is (marginally) significant, the coefficient associated with Δ Log(HP)$_{06-09}$ (other) drops only slightly, and it remains highly significant. Hence, direct demand spillovers from nearby counties have at best a small effect. In columns (2) to (5), we exclude all counties within a 50, 100, 150, or 250 mile radius based on the counties' geographical centroids. This rules out any direct demand spillovers from these counties by construction. As can be seen, the coefficient associated with Δ Log(HP)$_{06-09}$ (other) is always highly significant.

4.4 Trade Channel

We found no evidence that our results may be driven by direct demand spillovers from nearby counties. However, local demand shocks may also *indirectly* affect non-tradable employment in other counties, namely, through the trade channel. Intuitively, falling house prices in county j may lead to employment losses in county i's *tradable* sector, which in turn may spill over to county i's non-tradable sector if workers that are laid off cut back on their local grocery shopping and restaurant visits.[19]

We approach the trade channel hypothesis in two ways. We first test a necessary condition for this (trade) channel to explain our results: changes in county-level house prices must affect tradable employment in other counties. Figure V provides a visual impression. As can be seen, there is no association between changes in tradable county-level employment and either changes in a county's own house prices (top panel) or changes in house prices in other counties (bottom panel). Table XIII confirms this visual impression using regression analysis. As column (1) shows, neither changes in a county's own house prices nor changes in house prices in other counties have a significant effect on tradable employment at the county level.

The second approach considers a slightly more complex variant of the trade channel hypothesis. Accordingly, falling house prices in county j may lead to employment losses in county j's tradable sector, which in turn may spill over to county i's tradable sector via *tradable* firms' internal networks, which eventually may spill over to county i's non-tradable sector if workers that are laid off cut back on their local grocery shopping and restaurant visits. We find no support for this hypothesis. As column (2) shows, changes in house prices in other counties linked through tradable firms' internal networks have no significant effect on tradable employment at the county level. Ultimately, all of the results in Table XIII are reflections of the "tradable Placebo" in Mian and Sufi (2014), which posits that county-level variation in housing net worth shocks is unrelated to county-level variation in tradable employment.

[19] An industry is classified as as tradable if imports plus exports exceed \$10,000 per worker or \$500M in total (Mian and Sufi (2014)). Tradable industries are essentially manufacturing industries.

5 Conclusion

Local labor market shocks are difficult to insure against. Accordingly, economists have broadly focused on the role of public policy in mitigating the impacts of local shocks, including regional transfers, redistributive taxation, and "place-based" policies. Using confidential micro data from the U.S. Census Bureau's Longitudinal Business Database and exploiting ZIP code-level variation in house price changes during the Great Recession, we document that *firms* spread the adverse impacts of local employment shocks across regions through their internal networks of establishments. At the establishment level, we find that the elasticity of non-tradable employment with respect to house prices in other ZIP codes in which the firm has establishments is about one third of the elasticity with respect to local house prices. Similarly, at the county level, we find that the elasticity of non-tradable county-level employment with respect to house prices in other counties linked through firms' internal networks is about one fifth of the elasticity with respect to a county's own house prices. The smaller magnitudes at the county level are likely a reflection of the impacts of general equilibrium adjustments. Overall, our results suggest that firms play an important role in the provision of regional risk sharing and the propagation of local employment shocks across different U.S. regions.

Our work is consistent with prior literature arguing that firms provide valuable insurance to workers against idiosyncratic shocks, especially if the shocks are transitory (e.g., Guiso, Pistaferri, and Schivardi (2005), Ellul, Pagano, and Schivardi (2014)). Our research shows that one particular mechanism through which firms can provide insurance is regional diversification. If firms are regionally diversified, they can spread the adverse impacts of local employment shocks across multiple production units, and thus effectively across multiple regions. While we find that firms provide partial insurance to their workers, our results suggests that they do not provide full insurance: employment elasticities with respect to local shocks are between three and five times larger than elasticities with respect to shocks originating in other regions, suggesting that local workers are still bearing the brunt of local shocks.

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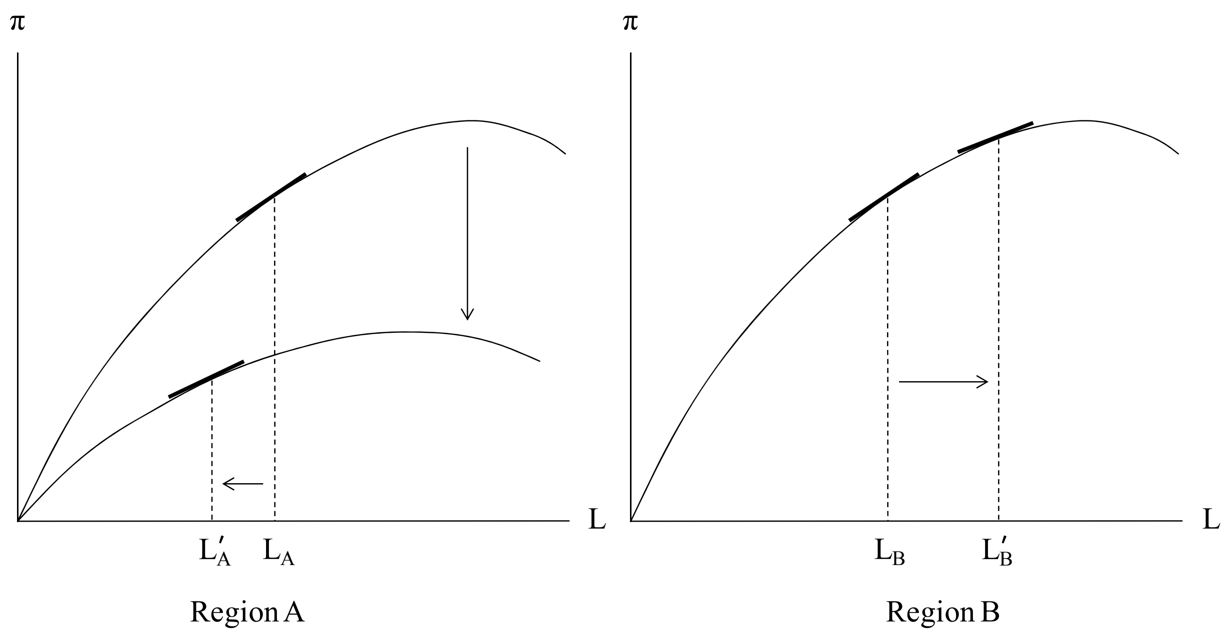
Figure I
Shocks to Investment Opportunities versus Shocks to Firms' Budget Constraints

This figure illustrates how a financially constrained firm with establishments in regions A and B adjusts employment in response to a given shock. For simplicity, labor (L) is the only input in the firm's production function. Profits at the establishment level are denoted by π. In Panel (A), region A experiences a decline in investment opportunities. In Panel (B), region A experiences a shock to firm revenues affecting the firm's overall budget constraint.

Panel (A): Shocks to investment opportunities



Panel (B): Shocks to firms' budget constraints

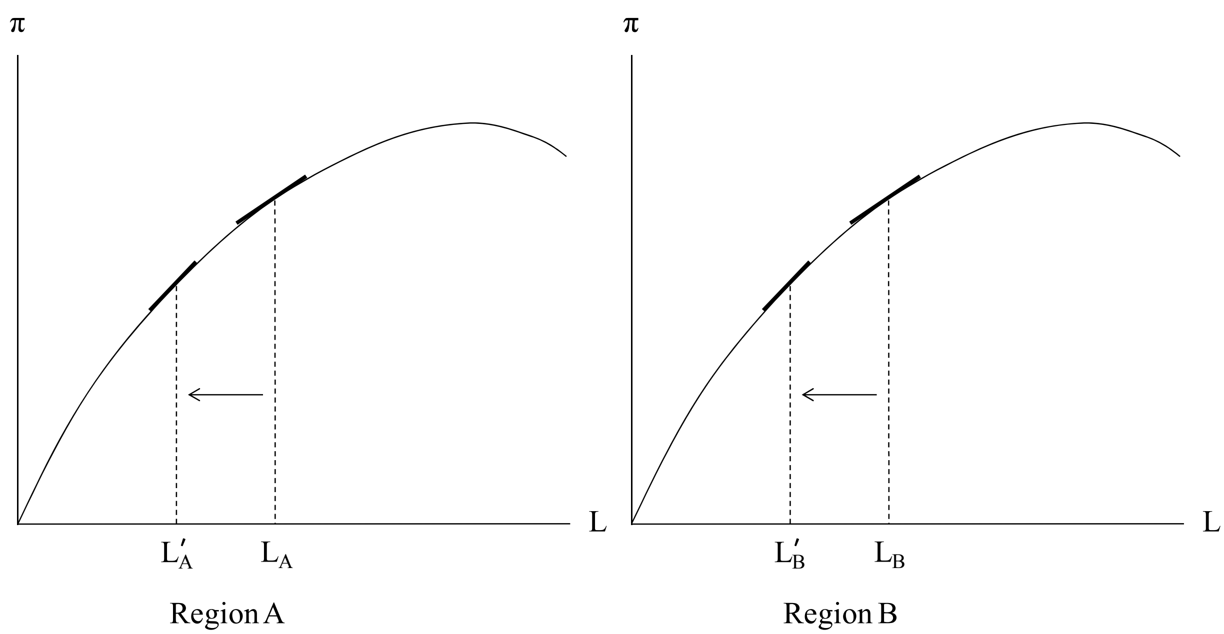


Figure II
Non-Tradable Establishment-Level Employment

This figure plots the relationship between changes in non-tradable establishment-level employment, Δ Log(Emp)$_{07\text{-}09}$, and either changes in house prices in the establishment's ZIP code, Δ Log(HP)$_{06\text{-}09}$, or changes in house prices in other ZIP codes in which the firm has establishments, Δ Log(HP)$_{06\text{-}09}$ (other). To filter out any confounding effects of Δ Log(HP)$_{06\text{-}09}$ (other) when plotting the relationship between Δ Log(Emp)$_{07\text{-}09}$ and Δ Log(HP)$_{06\text{-}09}$, we compute the residuals from a regression of Δ Log(Emp)$_{07\text{-}09}$ on a constant and Δ Log(HP)$_{06\text{-}09}$ (other). For each percentile of Δ Log(HP)$_{06\text{-}09}$, the plot in the top panel shows the mean values of the residuals and Δ Log(HP)$_{06\text{-}09}$, respectively. We proceed analogously in the bottom panel when plotting the relationship between Δ Log(Emp)$_{07\text{-}09}$ and Δ Log(HP)$_{06\text{-}09}$ (other).

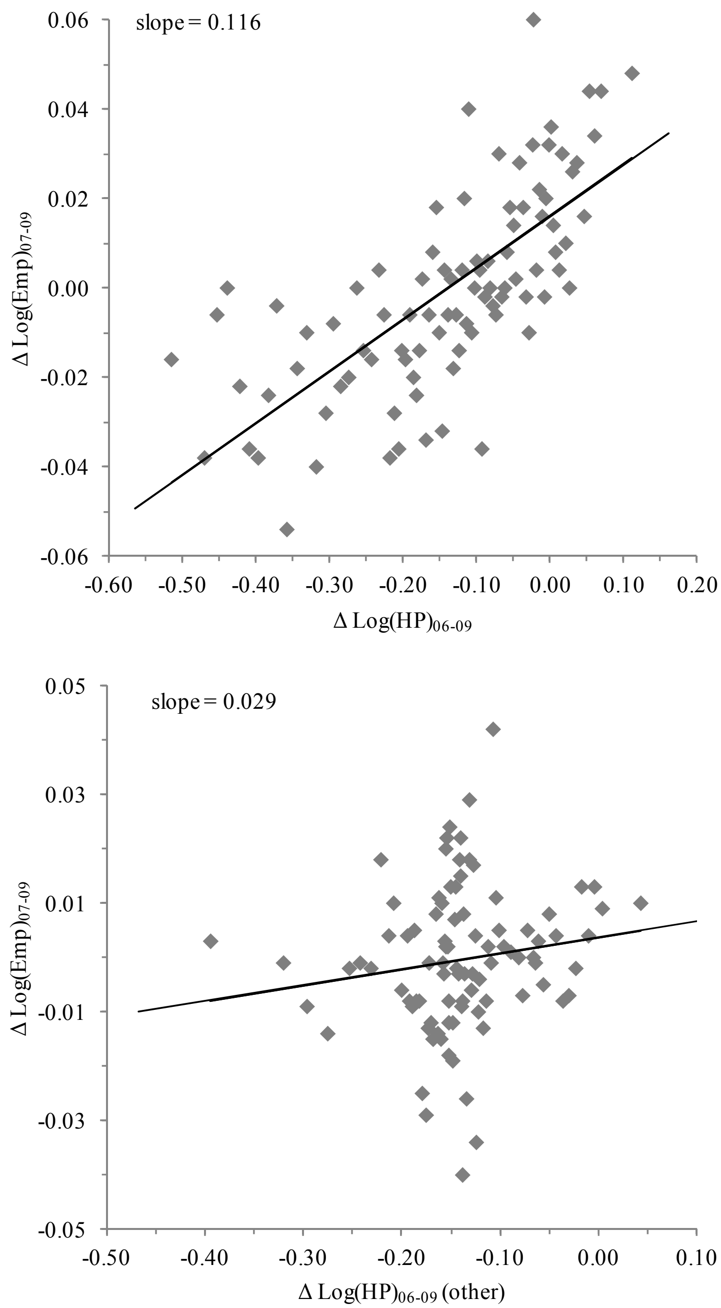


Figure III
Counterfactual Firm Networks

This figure illustrates the counterfactual firm networks used in the Placebo tests in Table IV. Firm A has establishments in locations 1 to 10, while firm B has establishments in locations 2 to 11. Moreover, firms A and B are in the same industry. Given that the two firms overlap in 90% of their locations, the counterfactual assumption is that, based on the firms' common clienteles, firm A *could have been* in location 11, while firm B *could have been* in location 1.

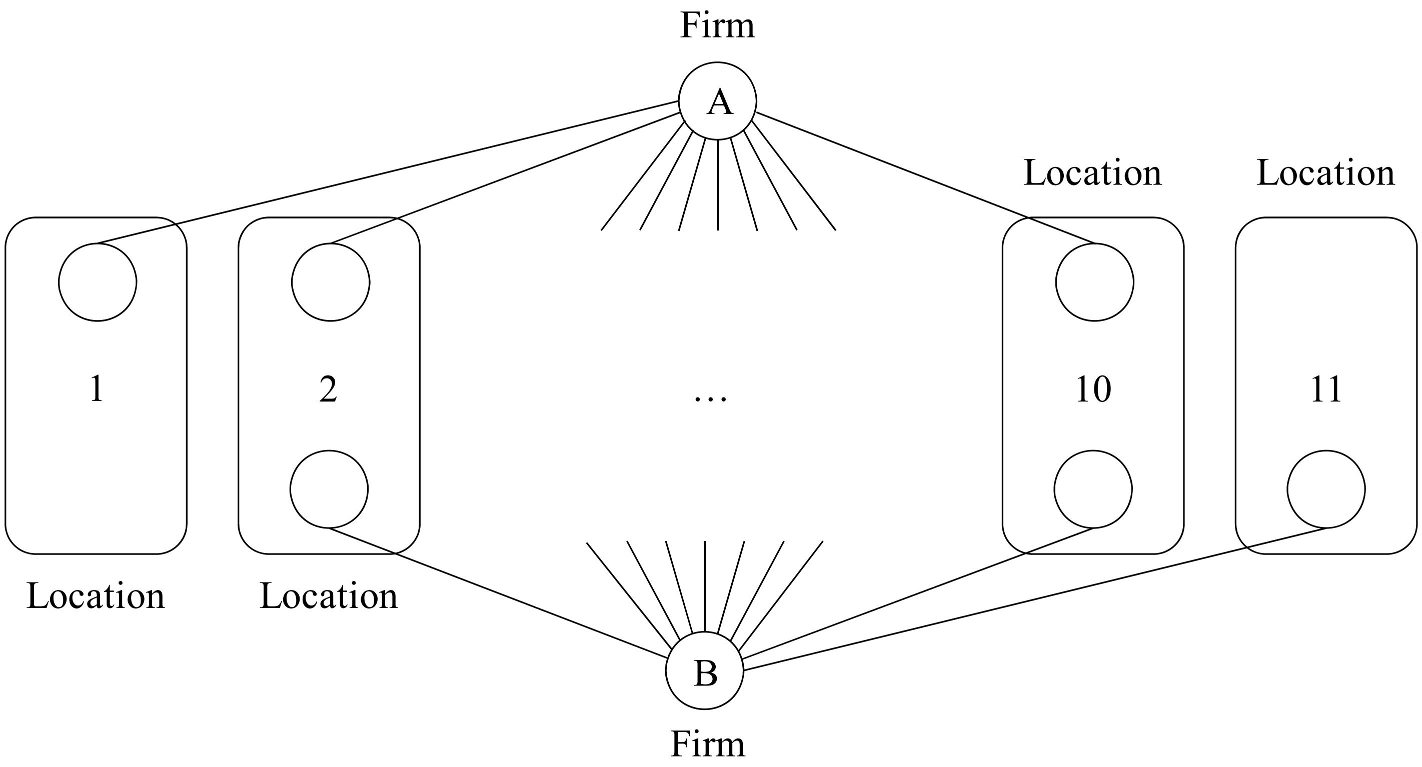
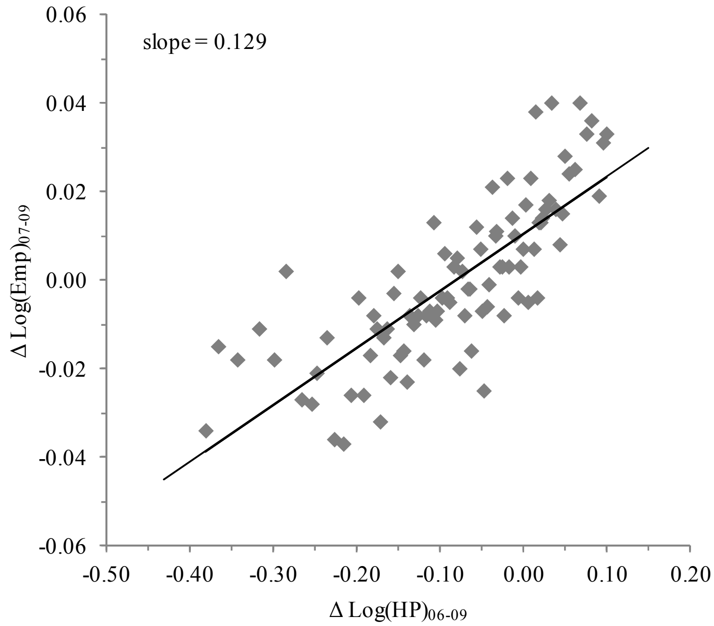


Figure IV
Non-Tradable County-Level Employment

This figure is similar to Figure I, except that it plots the relationship between changes in non-tradable county-level employment, Δ $Log(Emp)_{07\text{-}09}$, and either changes in county-level house prices, Δ $Log(HP)_{06\text{-}09}$, or changes in house prices in other counties linked through firms' internal networks, Δ $Log(HP)_{06\text{-}09}$ (other).



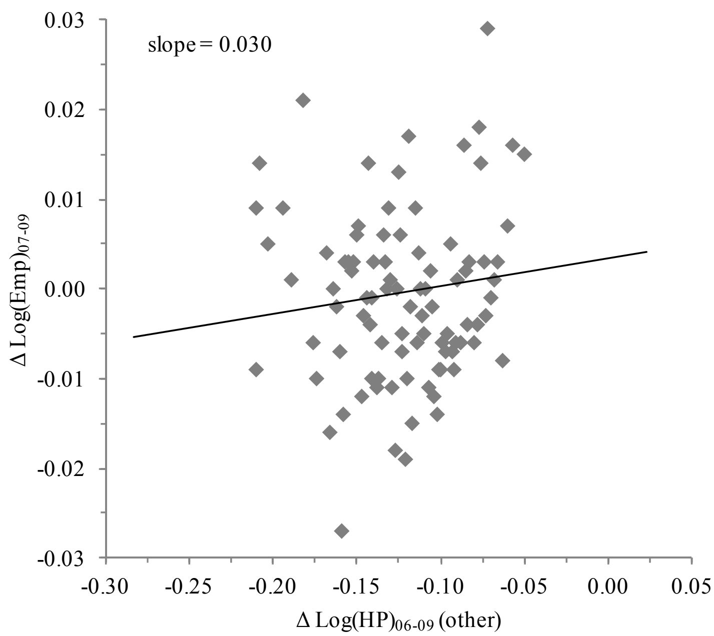
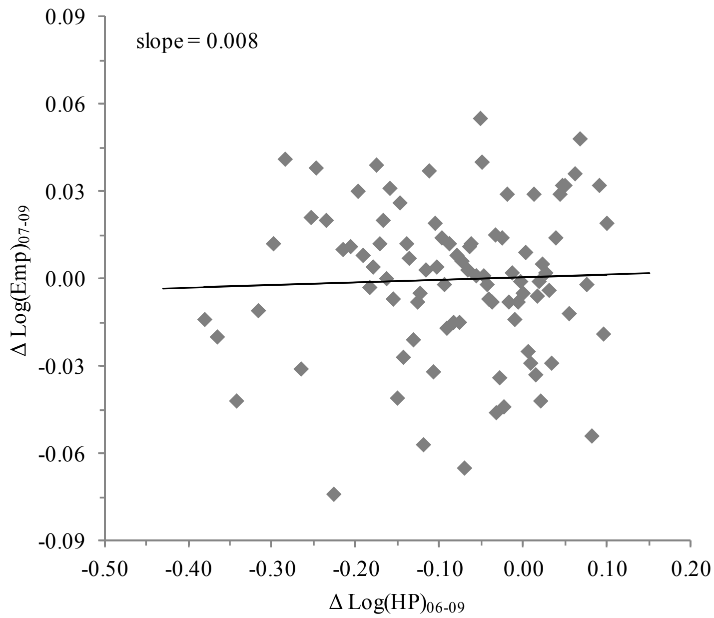


Figure V
Tradable County-Level Employment

This figure is similar to Figure IV, except that it describes tradable county-level employment.



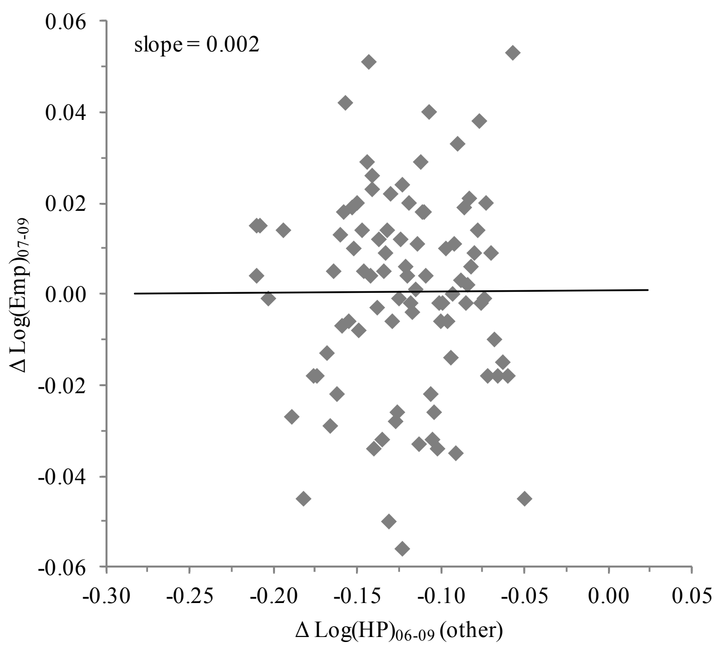


Table I
Summary Statistics

Panel (A) provides basic summary statistics. The establishment-level statistics pertain to non-tradable firms operating in multiple ZIP codes ("multi-region firms"). Employees is the number of employees in 2006. Δ $Log(Emp)_{07\text{-}09}$ is the percentage change in employment from 2007 to 2009. Δ $Log(HP)_{06\text{-}09}$ is the percentage change in house prices in the establishment's ZIP code from 2006 to 2009. # Linkages is the number of other ZIP codes in which the firm has establishments. The county-level statistics pertain to all non-tradable firms in a county, including single-region firms. Establishments and Employees is the number of establishments and employees, respectively, in 2006. Employment share is the ratio of non-tradable county-level employment to total county-level employment in 2006. Δ $Log(Emp)_{07\text{-}09}$ is the percentage change in non-tradable county-level employment from 2007 to 2009. Δ $Log(HP)_{06\text{-}09}$ is the percentage change in county-level house prices from 2006 to 2009. All percentage changes are employment-weighted. Panel (B) reports pairwise correlations of the network-based linkage weights ω and λ with corresponding linkage weights based on proximity, population, income, education, age, and household debt. The network-based linkage weights are described in Section 2.2. Proximity is the inverse of the geographical distance between regions' centroids. Population is recorded in 2000. Income is adjusted gross income per capita in 2006. Education is the percentage of adults in a county with a bachelor's degree or higher in 2000. Age is the median age among county residents in 2000. Household debt (mortgage, auto, and credit card debt) is per capita in 2006. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

Panel (A): Basic summary statistics

	N	Mean	Std. Dev.
Establishment level (multi-region firms)			
Employees	385,000	28.9	47.0
Δ $Log(Emp)_{07\text{-}09}$	385,000	-0.031	1.614
Δ $Log(HP)_{06\text{-}09}$	385,000	-0.145	0.193
# Linkages	385,000	812.9	1,085.9
County level (all firms)			
Establishments	1,000	1,074	2,174
Employees	1,000	18,490	38,227
Employment share	1,000	0.186	0.531
Δ $Log(Emp)_{07\text{-}09}$	1,000	-0.036	0.883
Δ $Log(HP)_{06\text{-}09}$	1,000	-0.145	0.189

Table I
(continued)

Panel (B): Correlation with network-based linkage weights

	Correlation with establishment-level linkage weights ω (*p* -value)	Correlation with county-level linkage weights λ (*p* -value)
Proximity	0.106***	0.103***
	(0.000)	(0.009)
Population	0.061***	0.073*
	(0.001)	(0.068)
Income	0.018	0.028
	(0.283)	(0.210)
Education	-0.027	-0.030
	(0.139)	(0.201)
Age	-0.019	-0.027
	(0.195)	(0.220)
Household debt	-0.006	-0.024
	(0.419)	(0.467)

Table II
Main Establishment-Level Results

The dependent variable is the percentage change in non-tradable establishment-level employment from 2007 to 2009, Δ Log(Emp)$_{07\text{-}09}$. Δ Log(HP)$_{06\text{-}09}$ is the percentage change in house prices in the establishment's ZIP code from 2006 to 2009. Δ Log(HP)$_{06\text{-}09}$ (other) is the linkage-weighted percentage change in house prices from 2006 to 2009 in the other ZIP codes in which the firm has establishments. The establishment-level network-based linkage weights ω are described in Section 2.2. In column (3), these linkage weights are replaced with equal weights. In columns (4) to (6), they are replaced with Placebo weights based on population, income, and household debt. In column (7), ZIP codes are randomly assigned using 1,000 bootstrap samples. All regressions are weighted by establishment-level employment. Standard errors (in parentheses) are double clustered at the firm and county level. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Δ Log(Emp)$_{07\text{-}09}$						
			Placebo tests				
			Equal weights	Population weights	Income weights	HH debt weights	Random ZIP codes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Δ Log(HP)$_{06\text{-}09}$	0.109***	0.091***	0.109***	0.109***	0.110***	0.109***	0.107***
	(0.020)	(0.023)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)
Δ Log(HP)$_{06\text{-}09}$ (other)		0.028***					
		(0.006)					
Δ Log(HP)$_{06\text{-}09}$ (other, placebo)			0.001	-0.001	-0.003	0.001	0.003
			(0.017)	(0.014)	(0.014)	(0.015)	(0.010)
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.02	0.02	0.02	0.02	0.02	0.02	0.02
Observations	385,000	385,000	385,000	385,000	385,000	385,000	385,000

Table III
Common Regional Shocks

This table presents variants of the specification in column (2) of Table II with alternative fixed effects and demographic controls. Average income (education, age) is the weighted average income (education, age) in the other ZIP codes in which the firm has establishments. All regressions are weighted by establishment-level employment. Standard errors (in parentheses) are double clustered at the firm and county level. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Δ Log(Emp)$_{07\text{-}09}$					
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Log(HP)$_{06\text{-}09}$ (other)	0.026***	0.025***	0.024***	0.024***	0.025***	0.024***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Average income			0.004*			0.004*
			(0.002)			(0.002)
Average education				0.006*		0.005
				(0.004)		(0.004)
Average age					0.001	-0.001
					(0.003)	(0.004)
Industry fixed effects	Yes	–	–	–	–	–
ZIP code fixed effects	Yes	–	–	–	–	–
ZIP code × industry fixed effects	No	Yes	Yes	Yes	Yes	Yes
R-squared	0.09	0.29	0.29	0.29	0.29	0.29
Observations	385,000	385,000	385,000	385,000	385,000	385,000

Table IV
Counterfactual Firm Networks

The dependent variable is the percentage change in non-tradable establishment-level employment from 2007 to 2009, Δ $Log(Emp)_{07\text{-}09}$. Δ $Log(HP)_{06\text{-}09}$ (other, placebo) is the average percentage change in house prices from 2006 to 2009 in (counterfactual) locations in which peer firms have establishments but the given firm has no establishments. Location is defined either at the county or ZIP code level. Peer firms are in the same 3- or 4-digit NAICS industry as the given firm and mutually overlap with the given firm in at least 75% or 90% of their locations. See Section 3.2 for a full description of the Placebo test. All regressions are weighted by establishment-level employment. Standard errors (in parentheses) are double clustered at the firm and county level. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Δ $Log(Emp)_{07\text{-}09}$							
	County				ZIP code			
	≥ 75% county overlap & same 3-digit NAICS	≥ 75% county overlap & same 4-digit NAICS	≥ 90% county overlap & same 3-digit NAICS	≥ 90% county overlap & same 4-digit NAICS	≥ 75% ZIP code overlap & same 3-digit NAICS	≥ 75% ZIP code overlap & same 4-digit NAICS	≥ 90% ZIP code overlap & same 3-digit NAICS	≥ 90% ZIP code overlap & same 4-digit NAICS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δ $Log(HP)_{06\text{-}09}$ (other, placebo)	0.002	-0.002	0.002	0.001	-0.002	0.001	0.001	0.002
	(0.009)	(0.011)	(0.011)	(0.016)	(0.015)	(0.017)	(0.018)	(0.020)
ZIP code × industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.40	0.65	0.57	0.62	0.77	0.79	0.65	0.65
Observations	39,900	28,600	16,200	9,900	4,500	2,900	2,500	1,200

Table V
Direct Demand Spillovers

This table presents variants of the specification in column (2) of Table III. Δ $Log(HP)_{06\text{-}09}$ (other, proximity) is similar to Δ $Log(HP)_{06\text{-}09}$ (other), except that the network-based linkage weights ω are replaced with weights based on the geographical distance between ZIP codes' centroids. Δ $Log(HP)_{06\text{-}09}$ (other, ZIP ≥ X miles) is similar to Δ $Log(HP)_{06\text{-}09}$ (other), except that the network-based linkage weights ω are set to zero for ZIP codes within an X-mile radius. All regressions are weighted by establishment-level employment. Standard errors (in parentheses) are double clustered at the firm and county level. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Δ $Log(Emp)_{07\text{-}09}$				
	(1)	(2)	(3)	(4)	(5)
Δ $Log(HP)_{06\text{-}09}$ (other, proximity)	0.011*				
	(0.007)				
Δ $Log(HP)_{06\text{-}09}$ (other)	0.020***				
	(0.006)				
Δ $Log(HP)_{06\text{-}09}$ (other, ZIP ≥ 50 miles)		0.022***			
		(0.005)			
Δ $Log(HP)_{06\text{-}09}$ (other, ZIP ≥ 100 miles)			0.022***		
			(0.005)		
Δ $Log(HP)_{06\text{-}09}$ (other, ZIP ≥ 200 miles)				0.020***	
				(0.004)	
Δ $Log(HP)_{06\text{-}09}$ (other, ZIP ≥ 250 miles)					0.019***
					(0.004)
ZIP code × industry fixed effects	Yes	Yes	Yes	Yes	Yes
R-squared	0.29	0.29	0.29	0.29	0.29
Observations	385,000	385,000	385,000	385,000	385,000

Table VI
Scope of Firms' Regional Networks

This table presents variants of the specification in column (2) of Table III in which Δ Log(HP)$_{06\text{-}09}$ is interacted with measures of the scope of firms' regional networks (RN) in 2006. In column (1), RN is a dummy variable indicating whether the firm operates in multiple ZIP codes ("multi-region firm"). The sample consists of all non-tradable firms in a county, including single-region firms. In column (2), RN is the number of ZIP codes in which the firm operates. In column (3), RN is one minus the Herfindahl-Hirschman index (HHI) measuring the extent of the firm's geographical concentration based on its non-tradable employment at the ZIP code level. All regressions are weighted by establishment-level employment. Standard errors (in parentheses) are double clustered at the firm and county level. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Δ Log(Emp)$_{07\text{-}09}$		
	Multi-ZIP	# ZIP	RN-HHI
	(1)	(2)	(3)
Δ Log(HP)$_{06\text{-}09}$ × RN	-0.027***	-0.013***	-0.522***
	(0.008)	(0.002)	(0.063)
RN	0.008***	0.005***	0.058**
	(0.001)	(0.001)	(0.024)
ZIP code × industry fixed effects	Yes	Yes	Yes
R-squared	0.20	0.29	0.29
Observations	910,300	385,000	385,000

Table VII
Financial Constraints

This table presents variants of the specification in column (2) of Table III in which Δ Log(HP)$_{06\text{-}09}$ and Δ Log(HP)$_{06\text{-}09}$ (other) are each interacted with measures of firms' financial constraints (FC) in 2006. In column (1), FC is firm leverage, which is the ratio of the sum of debt in current liabilities and long-term debt to total assets. In column (2), FC is the financial constraints index of Kaplan and Zingales (1997). In column (3), FC is the financial constraints index of Whited and Wu (2006). Both indices are net of their minimum values. In all columns, the sample consists of firms that have a match in Compustat. All regressions are weighted by establishment-level employment. Standard errors (in parentheses) are double clustered at the firm and county level. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Δ Log(Emp)$_{07\text{-}09}$		
	Leverage$_{06}$	KZ-index$_{06}$	WW-index$_{06}$
	(1)	(2)	(3)
Δ Log(HP)$_{06\text{-}09}$ × FC	0.130***	0.003**	0.051***
	(0.045)	(0.001)	(0.014)
Δ Log(HP)$_{06\text{-}09}$ (other)	0.009	0.008	0.010
	(0.012)	(0.010)	(0.016)
Δ Log(HP)$_{06\text{-}09}$ (other) × FC	0.038**	0.001**	0.013**
	(0.015)	(0.000)	(0.006)
FC	-0.038***	-0.003**	-0.008**
	(0.006)	(0.001)	(0.004)
ZIP code × industry fixed effects	Yes	Yes	Yes
R-squared	0.42	0.42	0.42
Observations	124,100	124,100	124,100

Table VIII
Proximity to Headquarters

This table presents variants of the specification in column (2) of Table III in which Δ $Log(HP)_{06\text{-}09}$ and Δ $Log(HP)_{06\text{-}09}$ (other) are each interacted with measures of geographical proximity to headquarters (HQ). In columns (1) and (2), Proximity to HQ is a dummy variable indicating whether the establishment and HQ are located in the same ZIP code and county, respectively. In column (3), Proximity to HQ is one divided by one plus the geographical distance between the establishment's and HQ's ZIP codes' centroids. All regressions are weighted by establishment-level employment. Standard errors (in parentheses) are double clustered at the firm and county level. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Δ $Log(Emp)_{07\text{-}09}$		
	Same ZIP code	Same county	Inverse distance
	(1)	(2)	(3)
Δ $Log(HP)_{06\text{-}09}$ × Proximity to HQ	-0.022**	-0.020**	-0.019**
	(0.010)	(0.010)	(0.009)
Δ $Log(HP)_{06\text{-}09}$ (other)	0.031***	0.032***	0.029***
	(0.008)	(0.008)	(0.010)
Δ $Log(HP)_{06\text{-}09}$ (other) × Proximity to HQ	-0.013**	-0.011**	-0.013**
	(0.006)	(0.006)	(0.005)
Proximity to HQ	0.017***	0.014***	0.023***
	(0.003)	(0.003)	(0.002)
ZIP code × industry fixed effects	Yes	Yes	Yes
R-squared	0.29	0.29	0.29
Observations	385,000	385,000	385,000

Table IX
Aggregate Employment at the County Level

The dependent variable is the percentage change in non-tradable county-level employment from 2007 to 2009, Δ Log(Emp)$_{07\text{-}09}$. Δ Log(HP)$_{06\text{-}09}$ is the percentage change in county-level house prices from 2006 to 2009. Δ Log(HP)$_{06\text{-}09}$ (other) is the linkage-weighted percentage change in house prices from 2006 to 2009 in other counties linked through firms' internal networks. The county-level network-based linkage weights λ are described in Section 2.2. The placebo tests in columns (3)-(7) are analogous to those in Table II. Demographic controls include income, education, and age at the county level. Industry controls are the county-specific employment shares of all 23 two-digit NAICS industries in 2006. All regressions are weighted by county-level employment. Standard errors (in parentheses) are clustered at the state level. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Δ Log(Emp)$_{07\text{-}09}$						
			Placebo tests				
			Equal weights	Population weights	Income weights	HH debt weights	Random counties
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Δ Log(HP)$_{06\text{-}09}$	0.122***	0.115***	0.123***	0.118***	0.122***	0.122***	0.122***
	(0.006)	(0.012)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Δ Log(HP)$_{06\text{-}09}$ (other)		0.024***					
		(0.007)					
Δ Log(HP)$_{06\text{-}09}$ (other, placebo)			0.007	0.009	0.002	0.001	0.002
			(0.041)	(0.010)	(0.015)	(0.013)	(0.028)
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.17	0.17	0.17	0.17	0.17	0.17	0.17
Observations	1,000	1,000	1,000	1,000	1,000	1,000	1,000

Table X
Common County-Level Shocks

This table presents variants of the specification in column (2) of Table IX. Δ Log(HP)$_{06\text{-}09}$ (other, *) is similar to Δ Log(HP)$_{06\text{-}09}$ (other), except that the network-based linkage weights λ are replaced with "similarity weights" that place more weight on counties that are more similar to the given county. Similarity is the absolute difference in either income, education, age, household debt, or the county-level share of non-tradable employment. All regressions are weighted by county-level employment. Standard errors (in parentheses) are clustered at the state level. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Δ Log(Emp)$_{07\text{-}09}$				
	(1)	(2)	(3)	(4)	(5)
Δ Log(HP)$_{06\text{-}09}$	0.112***	0.114***	0.108***	0.115***	0.114***
	(0.012)	(0.012)	(0.012)	(0.012)	(0.013)
Δ Log(HP)$_{06\text{-}09}$ (other)	0.025***	0.024***	0.029***	0.024***	0.022***
	(0.007)	(0.007)	(0.008)	(0.007)	(0.006)
Δ Log(HP)$_{06\text{-}09}$ (other, income)	0.003				
	(0.015)				
Δ Log(HP)$_{06\text{-}09}$ (other, education)		0.004			
		(0.010)			
Δ Log(HP)$_{06\text{-}09}$ (other, age)			0.003		
			(0.013)		
Δ Log(HP)$_{06\text{-}09}$ (other, household debt)				0.001	
				(0.013)	
Δ Log(HP)$_{06\text{-}09}$ (other, non-tradable share)					0.003
					(0.012)
Demographic controls	Yes	Yes	Yes	Yes	Yes
Industry controls	Yes	Yes	Yes	Yes	Yes
R-squared	0.17	0.17	0.17	0.17	0.17
Observations	1,000	1,000	1,000	1,000	1,000

Table XI
Counties in Which House Prices Did Not Fall

This table presents variants of the specifications in columns (1) and (2) of Table IX in which the sample is restricted to counties in which house prices either increased (columns (1) and (2)) or changed only little, defined as changes of less than ±2.5 percent (columns (3) and (4)). All regressions are weighted by county-level employment. Standard errors (in parentheses) are clustered at the state level. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Δ Log(Emp)$_{07\text{-}09}$			
	Δ Log(HP)$_{06\text{-}09}$ > 0		Δ Log(HP)$_{06\text{-}09}$ ± 0.025	
	(1)	(2)	(3)	(4)
Δ Log(HP)$_{06\text{-}09}$	0.018	0.014	0.003	0.003
	(0.050)	(0.051)	(0.012)	(0.012)
Δ Log(HP)$_{06\text{-}09}$ (other)		0.020**		0.022**
		(0.010)		(0.010)
Demographic controls	Yes	Yes	Yes	Yes
Industry controls	Yes	Yes	Yes	Yes
R-squared	0.18	0.19	0.22	0.23
Observations	200	200	200	200

Table XII
Direct Demand Spillovers

This table presents variants of the specification in column (2) of Table IX. Δ Log(HP)$_{06\text{-}09}$ (other, proximity) is similar to Δ Log(HP)$_{06\text{-}09}$ (other), except that the network-based linkage weights λ are replaced with weights based on the geographical distance between counties' centroids. Δ Log(HP)$_{06\text{-}09}$ (other, counties $\geq$ X miles) is similar to Δ Log(HP)$_{06\text{-}09}$ (other), except that the network-based linkage weights λ are set to zero for counties within an X-mile radius. All regressions are weighted by county-level employment. Standard errors (in parentheses) are clustered at the state level. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Δ Log(Emp)$_{07\text{-}09}$					
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Log(HP)$_{06\text{-}09}$	0.110***	0.115***	0.116***	0.116***	0.116***	0.116***
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
Δ Log(HP)$_{06\text{-}09}$ (other, proximity)	0.012*					
	(0.007)					
Δ Log(HP)$_{06\text{-}09}$ (other)	0.019***					
	(0.007)					
Δ Log(HP)$_{06\text{-}09}$ (other, counties $\geq$ 50 miles)		0.019***				
		(0.006)				
Δ Log(HP)$_{06\text{-}09}$ (other, counties $\geq$ 100 miles)			0.019***			
			(0.006)			
Δ Log(HP)$_{06\text{-}09}$ (other, counties $\geq$ 150 miles)				0.018***		
				(0.006)		
Δ Log(HP)$_{06\text{-}09}$ (other, counties $\geq$ 200 miles)					0.018***	
					(0.006)	
Δ Log(HP)$_{06\text{-}09}$ (other, counties $\geq$ 250 miles)						0.017***
						(0.006)
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry controls	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.17	0.17	0.17	0.17	0.17	0.17
Observations	1,000	1,000	1,000	1,000	1,000	1,000

Table XIII
Trade Channel

This table presents variants of the specification in column (2) of Table IX. In column (1), the dependent variable is the percentage change in tradable county-level employment from 2007 to 2009, Δ $Log(Emp)_{07-09}$. In column (2), Δ $Log(HP)_{06-09}$ (other, tradable network) is similar to Δ $Log(HP)_{06-09}$ (other), except that the network-based linkage weights λ are replaced with corresponding linkage weights based on tradable firms' internal networks. All regressions are weighted by county-level employment. Standard errors (in parentheses) are clustered at the state level. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Δ $Log(Emp)_{07-09}$	
	Tradable	Non-tradable
	(1)	(2)
Δ $Log(HP)_{06-09}$	0.011	0.120***
	(0.010)	(0.006)
Δ $Log(HP)_{06-09}$ (other)	0.003	
	(0.014)	
Δ $Log(HP)_{06-09}$ (other, tradable network)		0.004
		(0.010)
Demographic controls	Yes	Yes
Industry controls	Yes	Yes
R-squared	0.13	0.17
Observations	1,000	1,000