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Firms' Internal Networks and Local Economic Shocks†

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Using confidential establishment-level data from the US Census Bureau's Longitudinal Business Database, this paper documents how local shocks propagate across US regions through firms' internal networks of establishments. Consistent with a model of optimal within-firm resource allocation, we find that establishment-level employment is sensitive to shocks in distant regions in which the establishment's parent firm is operating, and that the elasticity with respect to such shocks increases with the firm's financial constraint. At the aggregate regional level, we find that aggregate county-level employment is sensitive to shocks in distant counties linked through firms' internal networks. (*JEL* D22, G32, L14, L22, R23, R32)

A prominent feature of resource allocation within firms is that individual business units must compete for scarce resources. As Williamson (1975, p. 147) notes when describing the advantages of the multidivisional (M-form) organization, "cash flows in the M-form firm are not automatically returned to their sources but instead are exposed to an internal competition." Such competition naturally creates an interdependence among otherwise unrelated business units. When a business unit experiences a negative shock to its cash flows, e.g., due to a drop in local consumer demand, corporate headquarters does not simply cut resources in the affected business unit. Rather, corporate headquarters optimally "spreads" the cash-flow shock across multiple business units in an effort to equate their marginal revenue products (normalized by factor input prices).[1] As a result, local consumer demand shocks

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[1] Section I presents a model of optimal within-firm resource allocation in which financially constrained firms spread cash-flow shocks across multiple business units. Similarly, in Inderst and Mueller (2003), financially constrained firms smooth out cash-flow shocks to individual business units by withdrawing resources from other

not only lead to employment declines at local business units but also at business units in distant regions. Our paper shows empirically how local consumer demand shocks spill over to distant regions through firms' internal networks, and how such spillovers affect aggregate employment in distant regions.

To examine how local consumer demand shocks propagate across regions through firms' internal networks, we construct a spatial network of the firm's internal organization using confidential data at the establishment level from the US Census Bureau's Longitudinal Business Database (LBD). We obtain regional variation in consumer demand shocks by exploiting the collapse in house prices during the Great Recession. As prior research has shown, the collapse in house prices caused a sharp drop in consumer spending (Mian, Rao, and Sufi 2013; Kaplan, Mitman, and Violante 2016; Stroebel and Vavra 2019), leading to massive employment losses. As a result, US regions with larger declines in housing net worth experienced significantly larger declines in non-tradable employment (Mian and Sufi 2014, Giroud and Mueller 2017).

A defining feature of non-tradable industries (e.g., restaurants, supermarkets, retail stores) is that they rely on local consumer demand. This makes non-tradable employment particularly well-suited to study the effects of local consumer demand shocks, such as those originating from falling house prices. The same feature also makes non-tradable employment particularly well-suited to study how local consumer demand shocks spill over to distant 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. Consequently, if a supermarket experiences an employment decline in response to a local consumer demand shock in some other region in which the parent firm is operating, then it is unlikely that this employment decline is due to a direct demand effect from that other region.

We find that non-tradable establishment-level employment is sensitive to consumer demand shocks in other regions in which the parent firm is operating. Controlling for local house price changes, a 10 percent drop in house prices in other regions translates into a 0.28 percent decline in local establishment-level employment. Importantly, what matters is that establishments are linked to other regions in which the parent firm is operating, not other regions in general. If we link establishments to other regions using equal weights, population weights, income weights, or household debt weights, or if we link them to randomly selected regions, their elasticity of employment with respect to house prices in other regions is close to zero and highly insignificant.

A main empirical challenge is to separate regional spillovers through firms' internal networks from common shocks to regions in which the parent firm is operating. We account for such common shocks by saturating our empirical model with highly granular zip code $\times$ industry fixed effects, where industries are measured at the 4-digit NAICS code level. We thus effectively compare non-tradable establishments

business units, and in Stein (1997), financially constrained firms reallocate scarce resources toward business units whose relative investment opportunities have increased. On the empirical side, Lamont (1997) and Shin and Stulz (1998) both find that investment by divisions of multi-segment firms depends on the cash flows of the firm's other divisions, while Giroud and Mueller (2015) find that productivity shocks at the plant level affect investment and employment at the firm's other plants.

in the same zip code and 4-digit NAICS code industry that are exposed to the same local shock but that belong to different firms and hence are exposed to different shocks in other regions.

Regional shocks may differentially affect establishments even within a given 4-digit NAICS code industry. A classic example are clientele effects. A low-end department store may be affected differently by a regional shock than a high-end department store, even though both are in the same industry (NAICS 4522). We account for clientele effects in various ways, e.g., through placebo tests based on counterfactual firm networks. The idea is that if firms in the same industry mutually overlap in almost all of their locations, they are likely to cater to similar clientele. To illustrate, suppose firms A and B are in the same 4-digit NAICS code industry and mutually overlap in 90 percent 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 clientele effects, then firm A's establishments—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. All our placebo estimates are insignificant, suggesting that our results are unlikely to be confounded by differential shocks to firms' clientele.

We additionally account for common regional shocks by considering establishments that switch firm affiliation. If firms' networks merely cluster around regions that are affected by common shocks, then individual establishments should remain sensitive to those regions even if their firm affiliation changes (since their location does not change). However, we find that establishments that switch firm affiliation are no longer sensitive to house prices in regions that were part of their original firm network.

Consumers may go to restaurants and grocery stores in neighboring regions. Hence, another empirical challenge is to separate spillovers through firms' internal networks from possible confounding *direct* demand effects from nearby regions. We account for the possibility of direct demand spillovers in different ways. For instance, we control for changes in house prices in nearby regions, exclude regions within a 500 mile radius, and exclude regional firms from our sample. We also aggregate establishments at either the firm-county or firm-state level. By construction, this accounts for any direct demand spillovers within a county and state, respectively.

Prior research shows that falling house prices in the Great Recession caused a drop in local consumer spending, thereby affecting non-tradable employment.[2] But changes in house prices may affect local employment also through another channel, namely, by affecting the collateral value of firms' real estate. Under this "collateral channel," firms' internal networks still matter, but not because they propagate local consumer demand shocks. Rather, they matter because they propagate shocks to firms' collateral value. We examine the collateral channel in two ways. First, we consider tradable industries. While the collateral channel should matter in these industries, the local demand channel should not matter, because demand for tradable goods is national or global. Second, we consider a setting where it is unlikely that

[2] Berger et al. (2018) construct a model that can generate large consumption responses to changes in house prices in line with the estimates found in empirical studies.

firms own their real estate: restaurants and retail stores in shopping malls. While the local demand channel should matter in this setting, the collateral channel should not matter. Based on these tests, we conclude that our results are unlikely to be explained by the collateral channel.

We conclude our establishment-level analysis with additional tests. Theory predicts that local consumer demand shocks should only spill over to other regions if the firm is financially constrained. Consistent with this prediction, we find that establishments of more financially constrained firms have relatively larger elasticities of employment with respect to house prices in other regions in which the parent firm is operating. In fact, for the least financially constrained firms within our sample, we find no evidence that establishment-level employment is sensitive to changes in house prices in other regions. Theory also predicts that establishments of multi-region firms should be less sensitive to (their own) local shocks than single-region firms. Consistent with this prediction, we find that establishments of multi-region firms exhibit relatively smaller elasticities of employment with respect to local house prices. Lastly, we find that establishments that are located closer to headquarters have relatively smaller elasticities of employment with respect to both local house prices and house prices in other regions in which the firm is operating, suggesting that they are more insulated from economic shocks.

Regional spillovers through firms' internal networks may not matter in the aggregate if workers of multi-region firms that are laid off due to shocks in other regions are re-employed by (local) firms that are less exposed to those regions. To account for the possibility of worker reallocation within a given region, we consider total non-tradable employment by all firms in a county, including single-region firms. Similar to what we found at the establishment level, we find that non-tradable county-level employment is sensitive to consumer demand shocks in distant counties linked through firms' internal networks. Accordingly, regional spillovers through firms' internal networks matter for aggregate regional employment.

Our paper contributes to several strands of literature. A growing literature in urban, macro, and financial economics studies how shocks propagate throughout the economy. This literature focuses on input-output networks (e.g., Acemoglu et al. 2012; Jacobson and von Schedvin 2015; Acemoglu, Akcigit, and Kerr 2016; Barrot and Sauvagnat 2016; Bigio and La'O 2016; Caliendo et al. 2018), financial networks (e.g., Acemoglu, Ozdaglar, and Tahbaz-Salehi 2012; Cabrales, Gale, and Gottardi 2016), and social networks (Bailey, Cao, Kuchler, and Stroebel 2018). Relatedly, a literature in banking documents how shocks in distant regions affect local bank lending (e.g., Peek and Rosengren 1997, 2000; Schnabl 2012; Gilje, Loutskina, and Strahan 2016). By contrast, little is known about whether and how shocks propagate across regions through firms' internal networks of establishments. In this regard, an important benefit of using US Census Bureau data is that we are able to construct a network of the firm's *entire* internal organization: the LBD includes the zip codes and firm affiliations of *all* (payroll) establishments in the United States.

Second, our paper contributes to a recent literature that studies the collapse in house prices in the Great Recession and its implications for consumer spending (Mian, Rao, and Sufi 2013; Stroebel and Vavra 2019; Kaplan, Mitman, and Violante 2016) as well as employment (Mian and Sufi 2014, Giroud and Mueller 2017). Our paper shows that local consumer demand shocks not only affect local non-tradable employment but also non-tradable employment in other regions. Indeed, we find large 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.

Third, a large literature in urban, macro-, and public economics focuses on the role of public policy in redistributing resources across regions through a federal system of tax and transfer policies, including "place-based" subsidy and investment programs (e.g., Glaeser and Gottlieb 2008; Kline and Moretti 2013, 2014a, 2014b; Moretti 2011; Nakamura and Steinsson 2014; Farhi and Werning 2017; Beraja 2018).[3] By contrast, our empirical study focuses on the role of *firms* in redistributing resources across regions via their internal networks of establishments.[4]

The rest of this paper is organized as follows. Section I presents a simple model of within-firm resource reallocation. Section II describes the data, variables, empirical specification, and summary statistics. Section III contains our main establishment-level results. Section IV accounts for common regional shocks. Section V discusses the role of regional firms. Section VI distinguishes between the consumer demand channel and the collateral channel. Section VII explores cross-sectional heterogeneity. Section VIII considers aggregate regional employment. Section IX concludes.

I. Resource Reallocation in Multi-Region Firms

This section presents a model of optimal within-firm resource allocation to illustrate how financially constrained firms operating in multiple regions allocate internal resources in response to a regional shock. Consider a firm operating in n regions. Each regional firm unit produces output using labor input according to the production function $f_i(L_i)$ satisfying the conditions $f_i'(L_i) > 0, f_i''(L_i) < 0, f_i(0) = 0$, $\lim_{x\to 0} f_i'(L_i) = \infty$, and $\lim_{x\to\infty} f_i'(L_i) = 0$, where $i = 1, \ldots, n$.[5] Regional firm units may differ in their labor productivity, as indicated by the subscript i in the production function f_i. Each regional firm unit takes output prices p_i and factor input prices w_i as given. Labor input in period t generates output in period $t+1$, which is discounted using the per-period discount factor δ. Factor input costs are funded out of the firm's cash flows. (We discuss the role of external funds below.) Importantly, factor input choices and funding decisions are made centrally by the firm's headquarters, which has authority to move budgets across firm units so as to maximize overall firm value (e.g., Williamson 1975; Gertner, Scharfstein, and Stein 1994; Stein 1997). Thus, the relevant budget constraint is at the overall firm level, not at

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

[4] While factor mobility can, in principle, mitigate the adverse impacts of regional 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 (e.g., Blanchard and Katz 1992; Notowidigo 2011; Autor, Dorn, and Hanson 2013, 2016; Autor et al. 2014; Yagan forthcoming).

[5] We focus on labor input because our empirical analysis considers employment changes within multi-region firms. That being said, the model can be extended to include both labor and capital input. See online Appendix Section A.1 for a model with both labor and capital input based on the Cobb-Douglas production function.

the individual firm unit level. Let C_i denote the cash flow generated by regional firm unit i. The firm's budget constraint is then $\sum_i w_i L_i \leq \sum_i C_i$.

The firm solves

$$\max_{L_i,\lambda} \delta \sum_i p_i f_i(L_i) - \sum_i w_i L_i + \lambda\Big[\sum_i C_i - \sum_i w_i L_i\Big],$$

where λ denotes the Lagrange multiplier associated with the budget constraint.

The Kuhn-Tucker conditions are

$$\delta p_i f_i'(L_i) = (1+\lambda) w_i, \quad \forall i, \tag{1}$$

$$\sum_i w_i L_i \leq \sum_i C_i, \tag{2}$$

and

$$\lambda\Big[\sum_i C_i - \sum_i w_i L_i\Big] = 0; \quad \lambda \geq 0.$$

From equation (1), it follows that for any two regional firm units i and j it must hold that

$$\frac{\delta p_i}{w_i} f_i'(L_i) = 1 + \lambda = \frac{\delta p_j}{w_j} f_j'(L_j), \tag{3}$$

implying that a marginal dollar of funds must have the same value at each regional firm unit.

As a benchmark, suppose that the firm is financially unconstrained, so that the budget constraint (2) is slack ($\lambda = 0$). In that case, equation (3) implies that headquarters allocates labor input to each regional firm unit up to the point where the (discounted) marginal revenue product of labor, $\delta p_i f_i'(L_i)$, equals the wage, w_i. Consequently, labor input at each regional firm unit is at the first-best optimal level.

Suppose next that the firm is financially constrained, so that the budget constraint (2) binds ($\lambda > 0$). By equation (3), a marginal dollar of funds must have the same value at each regional firm unit. However, this (shadow) value now strictly exceeds one—in contrast to the financially unconstrained case, where it was equal to one—implying that the marginal revenue product of labor now strictly exceeds the wage. Consequently, labor input at each regional firm unit is below the first-best optimal level.

Importantly, what matters is only whether the firm's budget constraint binds or is slack at the optimum, not whether the firm has access to external funds. The firm could have no access to external funds, yet the budget constraint could be slack if the firm's internal funds are sufficient to attain the first-best optimal level of production. Conversely, the firm could have access to external funds, yet the budget constraint could bind at the optimum if the sum of the firm's internal and external funds are insufficient to attain the first-best optimal level of production. Hence, access to external funds is neither a necessary nor a sufficient condition for the firm's budget constraint to be slack.

Consider now a negative cash-flow shock in region j. The question we are interested in is whether and how this shock affects the firm's labor input choices in regions $i \neq j$. Intuitively, a negative cash-flow shock in region j raises the shadow value of a marginal dollar of funds, $1 + \lambda$. As a result, headquarters will adjust production in each region to ensure that the optimality condition (3) remains satisfied. Given that regional firm units exhibit decreasing returns to scale, $f_i''(L_i) < 0$, this implies that labor input must decline in *all* regions—including regions $i \neq j$ that are not directly affected by the shock. Formally, differentiating equations (1) and (2) with respect to C_j yields

$$\frac{d\lambda}{dC_j} = \frac{1}{\sum_i \frac{w_i^2}{\delta p_i f_i''(L_i)}} < 0$$

and

$$\frac{dL_i}{dC_j} = \frac{w_i}{\delta p_i f_i''(L_i)} \frac{d\lambda}{dC_j} = \frac{\frac{w_i}{\delta p_i f_i''(L_i)}}{\sum_i \frac{w_i^2}{\delta p_i f_i''(L_i)}} > 0, \quad \forall i.$$

Hence, a negative cash-flow shock in one region leads to a decline in labor input in all regions, including those that are not directly affected by the shock. Also, this decline is larger the tighter is the firm's financial constraint, as expressed by the sensitivity of the shadow value of a marginal dollar to the cash-flow shock, $d\lambda/dC_j$.

Let us briefly comment on the nature of the regional shock. Prior research has shown that the collapse in house prices in the Great Recession is associated with large drops in consumer spending (Mian, Rao, and Sufi 2013; Kaplan, Mitman, and Violante 2016; Stroebel and Vavra 2019). Our model captures a salient feature of consumer demand shocks: drops in consumer spending negatively impact firms' cash flows. An alternative view is that falling house prices and drops in consumer spending are associated with regional *productivity* shocks.[6] A negative productivity shock lowers the first-best optimal level of production, implying that even financially unconstrained firms should cut their employment. However, contrary to this prediction, Giroud and Mueller (2017) find that while financially constrained firms make large employment cuts in response to consumer demand shocks, financially unconstrained firms make no significant employment cuts. Moreover, given a negative productivity shock in one region, firms should allocate a smaller budget to, and hence cut employment in, the affected region. This frees up scarce funds, which financially constrained firms can use for other regions, as production in these regions is below the first-best optimal level. Hence, for financially constrained firms, employment in other regions should *increase*, contrary to the evidence provided in

[6] See online Appendix Section A.2 for a model of optimal within-firm resource allocation in which multi-region firms reallocate resources across regional firm units in response to a regional productivity shock.

this paper. Our model of regional cash-flow shocks, on the other hand, implies that (i) only financially constrained firms should make employment cuts, and (ii) given a negative consumer demand shock in one region, firm-level employment in other regions should decrease. Both of these predictions are consistent with the evidence.

Our analysis illustrates a key implication of centralized resource allocation: to ensure that the optimality condition (3) remains satisfied, regional firm units must absorb some of the impacts of shocks in other regions. The flip side is that regional firm units become less sensitive to (their own) local shocks. Consider a single-region firm operating in region j. Differentiating the firm's budget constraint with respect to C_j, we obtain

$$\frac{dL_j}{dC_j} = \frac{1}{w_j}.$$

By contrast, for a regional firm unit in region j that is part of a multi-region firm, the sensitivity of labor input to a local cash-flow shock is given by

$$\frac{dL_j}{dC_j} = \frac{1}{w_j}\frac{\frac{w_j^2}{\delta p_j f_j''(L_j)}}{\sum_i \frac{w_i^2}{\delta p_i f_i''(L_i)}},$$

which is strictly less than the corresponding sensitivity for single-region firms.

Let us summarize the main predictions of our model. First, our model implies that local cash-flow shocks spill over to other regions in which the firm is operating. As a result, firm-level employment not only declines in the affected region but also in other regions. Financial constraints are crucial for this: if the firm's budget constraint is slack, local cash-flow shocks do not spill over to other regions. Second, the magnitude of the regional spillover depends on how tight the firm's financial constraint is. The tighter is this constraint, the more sensitive is regional firm-level employment to cash-flow shocks in other regions. Third, while regional firm units absorb some of the impacts of shocks in other regions, the flip side is that firm units in other regions absorb some of the impacts of local shocks. Consequently, regional firm units that belong to multi-region firms are less sensitive to (their own) local shocks than single-region firms.

II. Data, Variables, and Summary Statistics

A. *Data*

Our main data source is the LBD, which covers all business establishments in the United States with at least one paid employee. An establishment is a "single physical location where business is conducted" (Jarmin and Miranda 2002, p. 5), e.g., a restaurant, grocery store, supermarket, or department store. We have information on employment, industry and firm affiliation, payroll, and location at the individual establishment level.

We focus on establishments in the non-tradable sector. A defining feature of non-tradable industries is that they rely on local consumer demand. As we discussed in the introduction, this makes non-tradable employment well-suited to study how local consumer demand shocks spill over to distant regions through firms' internal networks: while these shocks affect non-tradable employment at the local level, they should not directly affect non-tradable employment in distant regions. We classify industries based on the classification scheme in Mian and Sufi (2014). Accordingly, there are 26 non-tradable industries. Among those, the largest ones in terms of US employment shares are full-service restaurants (3.76 percent), limited-service eating places (3.40 percent), grocery stores (2.13 percent), department stores (1.36 percent), other general merchandise stores (1.12 percent), clothing stores (1.06 percent), and automobile dealerships (1.05 percent).

We match establishments to zip code-level house prices using data from Zillow. Our sample period is from 2006 to 2009.[7] 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.

Our establishment-level analysis focuses on firms operating in multiple zip codes ("multi-region firms"). Our sample includes 385,000 establishments in the non-tradable sector representing 64.7 percent of US non-tradable employment in 2006.[8] The high employment share of multi-region firms in the non-tradable sector is reflective of the prominent role of regional and national restaurant and retail chains. In our county-level analysis, we consider *total* county-level employment, that is, we include employment by single-region firms. Our county-level sample consists of 1,000 counties representing 85.8 percent of US non-tradable employment in 2006.

We use control variables from various data sources, including the 2000 Decennial Census (population), 2006 American Community Survey (age, education, race, gender), Internal Revenue Service (income per capita in 2006), Federal Reserve Bank of New York (Consumer Credit Panel; household debt in 2006), and Facebook (Social Connectedness Index).[9] Moreover, we compute measures of firms' financial constraints using data from Compustat (firm leverage, Kaplan-Zingales index (Kaplan and Zingales 1997), Whited-Wu index (Whited and Wu 2006), all in 2006). To this end, we match establishments in the LBD to firms in Compustat using the Compustat-SSEL bridge maintained by the Census Bureau. As this bridge ends in 2005, we extend the match to 2009 using employer name and ID number following the procedure described in McCue and Jarmin (2005).

[7] Zillow house price data have been used in, e.g., Mian, Sufi, and Trebbi (2015); Kaplan, Mitman, and Violante (2016); Di Maggio et al. (2017); Giroud and Mueller (2017); and Bailey, Cao, Kuchler, and Stroebel (2018).

[8] All sample sizes are rounded to the nearest hundred following Census Bureau disclosure guidelines.

[9] The Social Connectedness Index is described in Bailey, Cao, Kuchler, Stroebel, and Wong (2018). We thank Mike Bailey from Facebook for providing us with the data.

B. *Variables and Empirical Specification*

We examine the sensitivity of non-tradable establishment-level employment during the Great Recession to changes in local house prices in the establishment's zip code and to changes in house prices in other zip codes in which the establishment's parent firm is operating. We estimate the following equation:

$$\Delta \log(Emp_{i,j,k})_{07-09} = \alpha + \eta_1 \Delta \log(HP_k)_{06-09} + \eta_2 \sum_{l \neq k} \omega_{j,k,l} \Delta \log(HP_l)_{06-09} + \varepsilon_{i,j,k},$$

where $\Delta \log(Emp_{i,j,k})_{07-09}$ is the percentage change in employment from 2007 to 2009 at establishment i of firm j in zip code k, $\Delta \log(HP_k)_{06-09}$ 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 network-weighted percentage change in house prices from 2006 to 2009 in zip codes $l \neq k$ based on 2006 firm network weights.[10] For brevity, we write $\Delta \log(HP)_{06-09}$ (other) in lieu of $\sum_{l \neq k} \omega_{j,l,k} \Delta \log(HP_l)_{06-09}$. The elasticities of interest are η_1 and, especially, η_2. Our model in Section I predicts that $\eta_2 > 0$.[11] All regressions are weighted by establishment size (number of employees) and include either industry, zip code, or zip code $\times$ industry fixed effects. When zip code or zip code $\times$ industry fixed effects are included, $\Delta \log(HP_k)_{06-09}$ is absorbed by the fixed effects. Industries are measured at the 4-digit NAICS code level. Standard errors are double clustered at the firm and county level.

The firm network weights $\omega_{j,k,l}$ specify the relative weight of changes in house prices in zip code l for an establishment of firm j in zip code k. We impose the minimal requirement that these weights be proportional to firms' non-tradable employment in a given zip code:

$$\omega_{j,k,l} = \frac{Emp_{j,l}}{\sum_{m \neq k} Emp_{j,m}}.$$

Accordingly, a local economic shock in zip code l matters more for an establishment of firm j in zip code k if the firm is more exposed to zip code l, as measured by its employment in zip code l relative to zip codes $m \neq k$. Simply put, an establishment is more exposed to a zip code if its parent firm is more exposed to the zip code. Naturally, a zip code has zero weight if the parent firm has no employees in the zip code.

Our main identifying assumption is that—*in the absence of firms' internal networks*—changes in establishment-level employment would be uncorrelated with

[10] The timing convention (changes in house prices from 2006 to 2009 and changes in employment from 2007 to 2009) follows Mian and Sufi (2014) and Giroud and Mueller (2017). In those studies, as well as here, house prices are measured in December, while (LBD) employment is measured in March. Online Appendix Table B9 considers an alternative timing convention in which changes in house prices and changes in employment are both measured from March 2007 to March 2009.

[11] In our model, firms are financially constrained and optimally reallocate internal resources across regions in response to local shocks. Under the null hypothesis, $\eta_2 = 0$, firms are either not financially constrained or, if they are, do not reallocate internal resources in response to local shocks.

changes in house prices in distant regions in which the establishment's parent firm is operating. There are many challenges to our identification strategy, notably common shocks to regions in which the parent firm is operating and direct demand spillovers from nearby regions. We address these challenges in Sections IV to VI.

In the final part of our analysis, we consider aggregate non-tradable employment at the county level. Specifically, we examine the sensitivity of non-tradable county-level employment to changes in county-level house prices and to changes in house prices in other counties linked through firms' internal networks. Analogous to our establishment-level analysis, we estimate the following equation:

$$\Delta \log(Emp_i)_{07-09} = \alpha + \eta_1 \Delta \log(HP_i)_{06-09} + \eta_2 \sum_{j \neq i} \lambda_{i,j} \Delta \log(HP_j)_{06-09} + \varepsilon_i,$$

where $\Delta \log(Emp_i)_{07-09}$ is the percentage change in employment from 2007 to 2009 in county i, $\Delta \log(HP_i)_{06-09}$ is the percentage change in house prices from 2006 to 2009 in county i, and $\sum_{j \neq i} \lambda_{i,j} \Delta \log(HP_j)_{06-09}$ is the network-weighted percentage change in house prices from 2006 to 2009 in counties $j \neq i$ based on 2006 county network weights. Similar to above, we write $\Delta \log(HP)_{06-09}$ (other) in lieu of $\sum_{j \neq i} \lambda_{i,j} \Delta \log(HP_j)_{06-09}$ for brevity. All regressions are weighted by county size (number of employees). Standard errors are clustered at the state level.

The county network weights $\lambda_{i,j}$ specify the relative weight of changes in house prices in county j for county i and are computed as the employment-weighted average of the firm network weights $\zeta_{h,i,j}$ within a county:

$$\lambda_{i,j} = \sum_h \frac{Emp_{h,i}}{\sum_k Emp_{k,i}} \zeta_{h,i,j},$$

where $\zeta_{h,i,j}$ is 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 more for county i if its establishments are more exposed to county j and these establishments have a relatively higher employment share within county i.

C. *Descriptive Statistics*

Table 1 provides summary statistics. The top part of panel A pertains to multi-region firms. Non-tradable establishments have on average 28.9 employees, and their parent firms have on average 15.4 establishments with 445 employees and operate in 1.9 states, 5.9 counties, and 12.7 zip codes. That said, the firm-size distribution is highly skewed due to the presence of some large national restaurant and retail chains in our sample. In our empirical analysis, we show that our results continue to hold if we exclude either the largest or smallest firms in our sample, or if we divide our sample into terciles based on firm size (see Table 3 as well as online Appendix Tables B1 and B2). During the Great Recession, non-tradable employment at the establishment level declined by 3.1 percent, while house prices at the zip code level fell by 14.5 percent. The bottom part of panel A pertains to all non-tradable firms within a county, including single-region firms. The average county has 1,074

Table 1—Descriptive Statistics

	Observations	Mean	SD
Panel A. Summary statistics			
Establishment level (multi-region firms)			
Employees	385,000	28.9	47.0
$\Delta\log(Emp)_{07\text{–}09}$	385,000	−0.031	1.614
$\Delta\log(HP)_{06\text{–}09}$	385,000	−0.145	0.193
Firm level (multi-region firms)			
Establishments	25,000	15.4	132.0
Employees	25,000	445	7,182
States	25,000	1.9	4.2
Counties	25,000	5.9	32.1
Zip codes	25,000	12.7	95.3
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
$\Delta\log(Emp)_{07\text{–}09}$	1,000	−0.036	0.883
$\Delta\log(HP)_{06\text{–}09}$	1,000	−0.145	0.189

	Correlation with firm network weights ω	Correlation with county network weights λ
Panel B. Correlation with network weights		
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)

Notes: Panel A provides summary statistics at the establishment, firm, and county level. All figures are from 2006 unless otherwise noted. All percentage changes are employment-weighted. The establishment-level summary statistics pertain to non-tradable establishments of firms operating in multiple zip codes (multi-region firms). $\Delta\log(Emp)_{07\text{–}09}$ is the percentage change in employment from 2007 to 2009. $\Delta\log(HP)_{06\text{–}09}$ is the percentage change in house prices in the establishment's zip code from 2006 to 2009. The firm-level summary statistics pertain to the establishments' parent firms. States, counties, and zip codes refer to the number of regions in which the firm is operating. The county-level summary statistics pertain to all non-tradable establishments in a county, including single-region firms. Employment share is the ratio of non-tradable county-level employment to total county-level employment. $\Delta\log(Emp)_{07\text{–}09}$ is the percentage change in non-tradable county-level employment from 2007 to 2009. $\Delta\log(HP)_{06\text{–}09}$ is the percentage change in county-level house prices from 2006 to 2009. Panel B shows correlations of the firm and county network weights, ω and λ, respectively, with corresponding linkage weights based on proximity, population, income, education, age, and household debt. Proximity is the inverse of the geographical distance between regions' centroids. Population is based on 2000 figures. Income is adjusted gross income per capita in 2006. Education is the percentage of adults with a bachelor's degree or higher in 2000. Age is the median age in 2000. Household debt (mortgage, auto, and credit card debt) is measured per capita in 2006.

non-tradable establishments with 18,490 employees representing 18.6 percent of total county-level employment. During the Great Recession, non-tradable employment at the county level declined by 3.6 percent, which is slightly higher than the 3.1 percent decline for multi-region firms shown above, as single-region firms experienced relatively larger declines in employment.

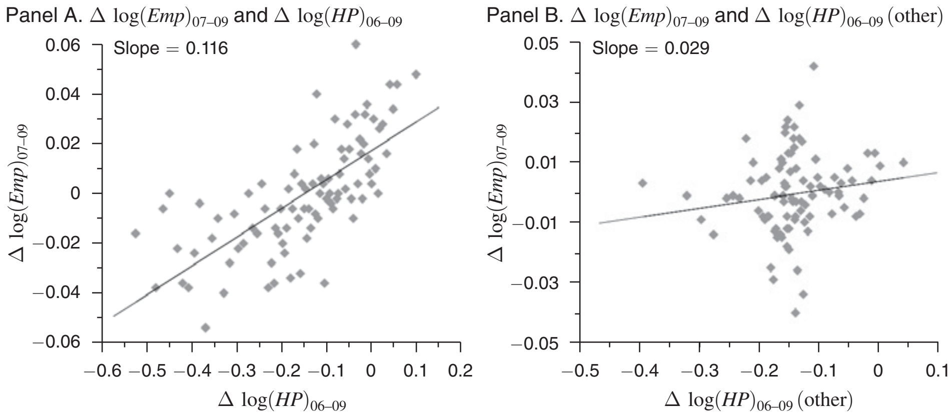

FIGURE 1. NON-TRADABLE ESTABLISHMENT-LEVEL EMPLOYMENT

Notes: The bin scatterplots show the relationship between changes in non-tradable establishment-level employment during the Great Recession, $\Delta \log(Emp)_{07-09}$, and either changes in house prices in the establishment's zip code, $\Delta \log(HP)_{06-09}$ (panel A), or changes in house prices in other zip codes in which the establishment's parent firm is operating, $\Delta \log(HP)_{06-09}$ (other) (panel B). Zip codes are sorted into percentile bins based on their value of $\Delta \log(HP)_{06-09}$ (panel A) and $\Delta \log(HP)_{06-09}$ (other) (panel B), respectively. The sample includes 16,600 zip codes; each bin therefore represents 166 zip codes. To filter out confounding effects of $\Delta \log(HP)_{06-09}$ (other) when plotting the relationship between $\Delta \log(Emp)_{07-09}$ and $\Delta \log(HP)_{06-09}$ in panel A, we estimate the residuals from a regression of $\Delta \log(Emp)_{07-09}$ on a constant and $\Delta \log(HP)_{06-09}$ (other). For each percentile bin, the bin scatterplot shows the mean values of (residual) $\Delta \log(Emp)_{07-09}$ and $\Delta \log(HP)_{06-09}$. We proceed analogously in panel B when plotting the relationship between $\Delta \log(Emp)_{07-09}$ and $\Delta \log(HP)_{06-09}$ (other).

Panel B reports pairwise correlations of the firm and county network weights with corresponding weights based on geographical proximity, population, income, education, age, and household debt. While most of the correlations are insignificant, those with proximity and population are significant. Both are intuitive. First, some firms in our sample are regional firms. Second, national restaurant and retail chains are likely to have more establishments 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 generate significant spillovers across regions (see Tables 2 and 12). As for proximity, we show that our empirical estimates are robust to excluding nearby regions and controlling for proximity-weighted changes in house prices in other regions (see Table 7 and online Appendix Table B23).

III. Propagation of Local Demand Shocks across Regions

Figure 1 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 (panel A) or changes in house prices in other zip codes in which the establishment's parent firm is operating (panel B). To filter out confounding effects of $\Delta \log(HP)_{06-09}$ (other) when plotting the relationship between $\Delta \log(Emp)_{07-09}$ and $\Delta \log(HP)_{06-09}$, we estimate the residuals from a regression of $\Delta \log(Emp)_{07-09}$ on a constant and $\Delta \log(HP)_{06-09}$ (other). These residuals capture the variation in $\Delta \log(Emp)_{07-09}$ that is orthogonal to, and thus unexplained by, $\Delta \log(HP)_{06-09}$ (other). For each percentile bin of

Table 2—Propagation of Local Demand Shocks across Regions

	Δ log$(Emp)_{07–09}$						
			Placebo tests				
			Equal weights	Population weights	Income weights	HH debt weights	Random zip codes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\Delta\log(HP)_{06–09}$	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)
$\Delta\log(HP)_{06–09}$ (other)		0.028					
		(0.006)					
$\Delta\log(HP)_{06–09}$ (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^2	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

Notes: The dependent variable, $\Delta\log(Emp)_{07–09}$, is the percentage change in non-tradable 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 from 2006 to 2009. $\Delta\log(HP)_{06–09}$ (other) is the network-weighted percentage change in house prices from 2006 to 2009 in other zip codes in which the establishment's parent firm is operating based on 2006 firm network weights. The firm network weights are described in Section IIB. In column 3, the firm network weights are replaced with equal weights. In columns 4 to 6, the firm network weights are replaced with placebo weights based on population, income, and household debt, respectively. All three variables are described in Table 1. In column 7, zip codes to which the establishment is linked ($\omega > 0$) are replaced with randomly assigned zip codes. The sample consists of non-tradable establishments of firms operating in multiple zip codes (multi-region firms). All regressions are weighted by establishment-level employment. Standard errors (in parentheses) are double clustered at the firm and county level.

$\Delta\log(HP)_{06-09}$, the bin scatterplot provides the mean values of the residuals and $\Delta\log(HP)_{06-09}$. We proceed analogously in panel B when plotting the relationship between $\Delta\log(Emp)_{07-09}$ and $\Delta\log(HP)_{06-09}$ (other).

In panel A, there is a positive association between changes in establishment-level employment and changes in (local) house prices at the zip code level. The elasticity of establishment-level employment with respect to local house prices is 0.116, implying that a 10 percent drop in house prices is associated with a 1.16 percent drop in establishment-level employment. As panel B shows, there is also a positive association between changes in establishment-level employment and changes in house prices in other zip codes in which the establishment's parent firm is operating. The elasticity of establishment-level employment with respect to house prices in other zip codes is 0.029, implying that a 10 percent drop in house prices is associated with a 0.29 percent drop in establishment-level employment. Accordingly, establishment-level employment is sensitive not only to local house prices but also to house prices in other regions in which the establishment's parent firm is operating.

Table 2 confirms this visual impression. All regressions include industry fixed effects. As column 1 shows, the elasticity of establishment-level employment with respect to local house prices is 0.109, which is only slightly lower than the corresponding elasticity in Figure 1. In column 2, we additionally include house prices in other zip codes in which the establishment's parent firm is operating. While the coefficient associated with local house prices, $\Delta\log(HP)_{06-09}$, drops slightly, the coefficient associated with house prices in other zip codes, $\Delta\log(HP)_{06-09}$ (other), is highly significant. The elasticity of establishment-level employment with respect to house prices in other zip codes is 0.028, which is almost identical to the corresponding elasticity in Figure 1.

What matters is that establishments are linked to other regions in which the parent firm is operating, not other regions in general. To illustrate this, we present placebo tests. In column 3, we assign equal weight to all other zip codes. In columns 4 to 6, we replace the firm network weights ω with corresponding weights based on population, income, and household debt, respectively. In column 7, we randomly select other zip codes. Precisely, for each establishment, we replace all zip codes to which the establishment is currently linked $(\omega > 0)$ with randomly selected zip codes. We then run our main establishment-level regression and store the coefficients and standard errors. We repeat this process 1,000 times. The results in column 7 show the average coefficient and standard error based on the 1,000 regressions. As can be seen, in all of these placebo tests, house prices in other regions have no significant effect on establishment-level employment.

IV. Common Regional Shocks

A. *Within-Zip Code Analysis*

A key challenge is to separate regional spillovers through firms' internal networks from common shocks to regions in which a firm is operating. To filter out confounding effects due to common regional shocks, we add zip code fixed effects in our regression. These fixed effects absorb any common variation within a zip code that is due to a regional shock, regardless of whether the shock is region-specific or correlated with shocks in other regions. They also account for spillovers from one region to another, e.g., due to price or other general equilibrium effects. We thus compare non-tradable establishments in the same zip code that are exposed to the same local shock but that belong to different firms and therefore are exposed to different shocks in other regions.

Table 3 shows the results. As column 1 shows, the elasticity of establishment-level employment with respect to house prices in other regions is similar to the corresponding elasticity without zip code fixed effects in Table 2. A potential concern is that regional shocks may differentially affect establishments in different industries. We address this concern in column 2 by including highly granular zip code $\times$ industry fixed effects. Relative to column 1, which includes both zip code fixed effects and industry fixed effects, the coefficient on $\Delta \log(HP)_{06-09}$ (other) remains virtually unchanged. Thus, our results are not driven by within-zip code variation across industries or within-industry variation across zip codes, which is consistent with non-tradable industries being a fairly homogeneous group of industries that are widely dispersed across zip codes. In the remainder of the paper, we use the specification with zip code $\times$ industry fixed effects as our baseline establishment-level specification.

Columns 3 to 8 show that our main results are not driven by outliers. In column 3, we exclude the largest 10 percent of firms in our sample. Given that many of these firms are restaurant and retail chains, the number of observations drops by more than 10 percent. In column 4, we exclude the smallest 10 percent of firms in our sample. As these firms have only relatively few establishments, the number of observations

Table 3—Within-Zip Code Analysis

	Δ log$(Emp)_{07–09}$							
			Excluding outliers (top or bottom 10 percent)					
			Firms		Zip codes			
			Largest	Smallest	Largest house price decline	Smallest house price decline	Largest employment decline	Smallest employment decline
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δ log$(HP)_{06–09}$ (other)	0.026	0.025	0.022	0.025	0.024	0.027	0.023	0.022
	(0.006)	(0.006)	(0.010)	(0.006)	(0.009)	(0.008)	(0.005)	(0.006)
Industry FE	Yes	—	—	—	—	—	—	—
Zip code FE	Yes	—	—	—	—	—	—	—
Zip code × industry FE	–	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.09	0.29	0.52	0.29	0.28	0.29	0.27	0.26
Observations	385,000	385,000	82,700	380,000	351,500	343,700	378,900	379,600

Notes: This table presents variants of the specification in column 2 of Table 2 in which the industry fixed effects are replaced with either industry and zip code fixed effects (column 1) or zip code × industry fixed effects (columns 2 to 8). $\Delta\log(HP)_{06–09}$ is absorbed by the fixed effects. Columns 3 and 4 exclude the top and bottom 10 percent of firms based on their employment in 2006. Columns 5 to 8 exclude the top and bottom 10 percent of zip codes based on their value of either $\Delta\log(Emp)_{07–09}$ or $\Delta\log(HP)_{06–09}$. All regressions are weighted by establishment-level employment. Standard errors (in parentheses) are double clustered at the firm and county level.

drops by less than 10 percent. Lastly, in columns 5 to 8, we exclude the top and bottom 10 percent of zip codes in our sample based on either changes in house prices or changes in employment. As can be seen, our results are always similar.

The online Appendix contains additional robustness checks. Table B1 is similar to columns 5 to 8 of Table 3, except that the relevant cutoff is at the 5 percent level. In Table B2, we divide our sample into terciles based on firm size. In Table B3, we focus on 4-digit NAICS code industries that are "especially" non-tradable, in the sense that it is difficult to move inventory across locations: full-service restaurants, limited-service eating places, drinking places, special food services, grocery stores, and specialty food stores. In Table B4, we divide our sample into census regions. In Table B5, we control for establishment-level size (number of employees) and past employment volatility. In Table B6, we weigh observations based on zip code, county, state, or industry employment. In Table B7, we use distance-adjusted network weights, which place less weight on nearby zip codes within the firm's internal network. In Table B8, we exclude coastal states. In Table B9, we measure both changes in house prices and changes in employment from March 2007 to March 2009.

Two robustness checks deserve special mention. Table B10 focuses on the housing boom prior to the Great Recession. While the results are qualitatively similar to our baseline results, the economic magnitudes are smaller: both employment elasticities—with respect to local house prices and house prices in other zip codes—are only about half as large as the corresponding elasticities in Tables 2 and 3. Accordingly, firms' responses to consumer demand shocks are relatively stronger during the housing bust, which is consistent with firms being more financially constrained in the Great Recession. In Table B11, we consider how wages adjust to changes in house prices in the Great Recession. Both wage elasticities—with respect to local house prices and house prices in other zip codes—are positive but insignificant. Thus, consistent with Keynesian wage stickiness, wages at non-tradable establishments do not appear to fall in response to consumer demand shocks, in contrast to employment.[12]

B. *Clientele Effects*

Common regional shocks may differentially affect establishments even within a given 4-digit NAICS code industry. A classic example are clientele effects. A low-end department store may be affected differently by a regional shock than a high-end department store, even though both are in the same industry (NAICS 4522).[13] To account for clientele effects, we include as controls the average income, education, age, race, gender, and population density in the other zip codes in which the establishment's parent firm is operating. Effectively, we thus compare establishments in the same zip code and 4-digit NAICS code industry that belong to parent firms catering to similar demographic segments of the population.

Table 4 shows that including these controls has little effect on our results. The effects of demographics may arguably differ across locations. For example, young people in urban areas may have been harder hit during the Great Recession than young people in rural areas. In online Appendix Table B13, we include "more flexible" controls by interacting income, education, age, race, and gender each with population density. In Table B14, we re-estimate columns 6 and 7 of Table 4 and column 1 of Table B13 using an "urban" (i.e., MSA) dummy in lieu of population density. And in Tables B15 and B16, we interact all demographic controls in separate regressions with either population density or the urban dummy.

We further account for clientele effects by estimating placebo regressions based on counterfactual firm networks. The idea is that if firms in the same industry mutually overlap in almost all of their locations, then they likely cater to similar clientele. To illustrate, suppose firms A and B are in the same industry and mutually overlap in 90 percent of their locations (2 to 10), but firm A is additionally present in location 1, while firm B is additionally present in location 11 (see Figure 2). Since the two firms mutually overlap in 90 percent of their locations, the counterfactual assumption is that—based on their common clientele—firm A *could have been* in location 11, and firm B *could have been* in location 1. Hence, if our estimates are confounded by differential shocks to firms' clientele, then firm A's establishments—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.

[12] One reason may be that workers' pay in many restaurant and retail jobs is already at or near the minimum wage. Online Appendix Table B24 presents similar results at the county level.

[13] During the housing boom that preceded the Great Recession, firms with a higher sensitivity to consumer demand may have expanded into regions in which house prices, and hence consumer demand, increased more strongly. In many instances, these were also regions in which house prices fell more strongly during the housing bust. Hence, firms with a higher sensitivity to regional shocks may have expanded into regions with larger negative shocks during the Great Recession. To see whether our results are driven by firms' expansions during the housing boom, we estimate our baseline specification using firms' internal networks in 2001 instead of 2006. As online Appendix Table B12 shows, all our estimates remain similar, albeit the coefficient on $\Delta \log(HP)_{06-09}$ (other) is slightly attenuated as firms' internal networks in 2001 constitute a noisy proxy of their (true) internal networks in the Great Recession.

Table 4—Clientele Effects

	Δ log(Emp)$_{07\text{–}09}$						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Δ log(HP)$_{06\text{–}09}$ (other)	0.024	0.024	0.025	0.025	0.025	0.025	0.024
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Income	0.004						0.004
	(0.002)						(0.002)
Education		0.006					0.004
		(0.004)					(0.005)
Age			0.001				−0.001
			(0.004)				(0.004)
Nonwhite				−0.002			−0.001
				(0.005)			(0.006)
Male					0.001		−0.000
					(0.002)		(0.002)
Population density						0.002	0.001
						(0.003)	(0.003)
Zip code × industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.29	0.29	0.29	0.29	0.29	0.29	0.29
Observations	385,000	385,000	385,000	385,000	385,000	385,000	385,000

Notes: This table presents variants of the specification in column 2 of Table 3 with demographic controls. Income is the weighted average income in other zip codes in which the establishment's parent firm is operating. Zip code-level weights are based on the fraction of the firm's employment in the zip code relative to the firm's total employment. The other controls (education, age, nonwhite, male, and population density) are constructed analogously. Income, education, and age are described in Table 1. Nonwhite and male are measured in 2006. Population density is measured in 2000. All regressions are weighted by establishment-level employment. Standard errors (in parentheses) are double clustered at the firm and county level.

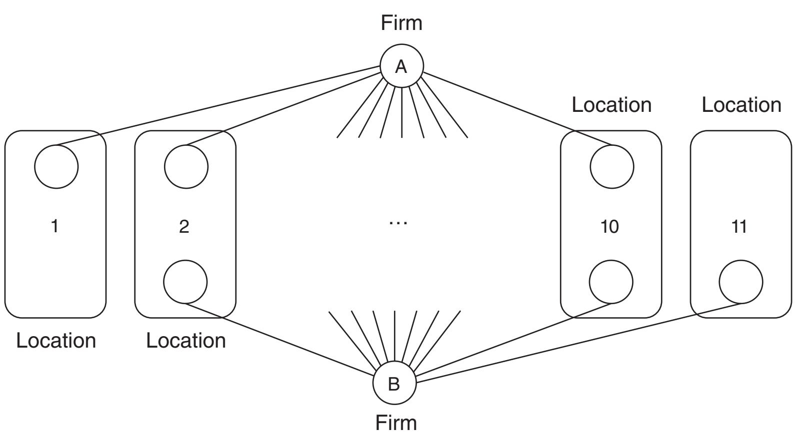

Figure 2. Counterfactual Firm Networks

Notes: This figure illustrates how the counterfactual firm networks used in the placebo tests described in Section IVB are formed. In the figure, firms A and B are in the same industry and mutually overlap in 90 percent of their locations (2 to 10), but firm A is additionally present in location 1, while firm B is additionally present in location 11. The counterfactual assumption is that, based on the firms' common industry and mutual overlap of their locations, firm A *could have* been in location 11, while firm B *could have* been in location 1.

Table 5—Counterfactual Firm Networks

	Δ log$(Emp)_{07–09}$							
	County				Zip code			
	≥75% overlap 3-digit NAICS (1)	≥75% overlap 4-digit NAICS (2)	≥90% overlap 3-digit NAICS (3)	≥90% overlap 4-digit NAICS (4)	≥75% overlap 3-digit NAICS (5)	≥75% overlap 4-digit NAICS (6)	≥90% overlap 3-digit NAICS (7)	≥90% overlap 4-digit NAICS (8)
Δ log$(HP)_{06–09}$	0.002	−0.002	0.002	0.001	−0.002	0.001	0.001	0.002
(other, placebo)	(0.009)	(0.011)	(0.011)	(0.016)	(0.015)	(0.017)	(0.018)	(0.020)
Zip code × industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	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

Notes: This table presents variants of the specification in column 2 of Table 3 in which Δ log$(HP)_{06–09}$ (other) is replaced with Δ log$(HP)_{06–09}$ (other, placebo) representing the mean value of Δ log$(HP)_{06–09}$ (other) in counterfactual locations in which peer firms have establishments but the given firm has no establishment. Peer firms are firms in the same 3- or 4-digit NAICS code industry that mutually overlap with the given firm in at least 75 percent or 90 percent of their locations. Location is defined either at the county or zip code level. See Section IVB for a full description of the placebo tests. All regressions are weighted by establishment-level employment. Standard errors (in parentheses) are double clustered at the firm and county level.

In the placebo regressions, we restrict our sample to non-tradable firms in the same industry that mutually overlap in at least 75 or 90 percent 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 code level.[14] As discussed above, we estimate the elasticity of establishment-level employment with respect to house prices in (counterfactual) locations in which the firm could have been.[15] Table 5 shows that this elasticity is always small and insignificant, suggesting that our results are unlikely to be confounded by differential shocks to firms' clientele.

C. *Changes in Firm Affiliation*

If firms' networks merely cluster around regions which are affected by common shocks, then individual establishments should remain sensitive to shocks in those regions even if their firm affiliation changes (since their location does not change). To test this hypothesis, we restrict our sample to establishments that switch firm affiliation between 2002 and 2005. We focus on sales of individual establishments, and not mergers between firms, because in a merger the "old" (pre-merger) and "new" (post-merger) networks overlap. Altogether, there are 15,600 sales of non-tradable establishments associated with multi-region firms between 2002 and 2005.

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

[15] Online Appendix Table B17 shows that firms' counterfactual locations are observationally equivalent to their actual locations based on employment or house price changes, income, education, age, race, gender, and population density.

Table 6—Changes in Firm Affiliation

	$\Delta \log(Emp)_{07–09}$			
	Switched firm affiliation		Did not switch firm affiliation	
	2001 network	2006 network	2001 network	2006 network
	(1)	(2)	(3)	(4)
$\Delta \log(HP)_{06–09}$ (other)		0.022	0.021	0.025
		(0.010)	(0.007)	(0.006)
$\Delta \log(HP)_{06–09}$ (other, placebo)	0.004			
	(0.007)			
Zip code $\times$ industry fixed effects	Yes	Yes	Yes	Yes
R^2	0.79	0.79	0.29	0.29
Observations	15,600	15,600	369,400	369,400

Notes: This table presents variants of the specification in column 2 of Table 3 in which the sample is restricted to establishments that either switched (columns 1 and 2) or did not switch (columns 3 and 4) firm affiliation between 2002 and 2005. In columns 1 and 3, $\Delta \log(HP)_{06–09}$ (other, placebo) and $\Delta \log(HP)_{06–09}$ (other) are based on establishments' firm networks in 2001. In columns 2 and 4, $\Delta \log(HP)_{06-09}$ (other) is based on establishments' firm networks in 2006. All regressions are weighted by establishment-level employment. Standard errors (in parentheses) are double clustered at the firm and county level.

Table 6 presents the results. As is shown in column 1, establishments that switch firm affiliation between 2002 and 2005 are no longer sensitive to house prices in regions that were part of their original 2001 network. However, as is shown in column 2, they *are* sensitive to house prices in regions that are part of their current 2006 network. By comparison, columns 3 and 4 consider all (remaining) establishments that did not switch firm affiliation between 2002 and 2005. As can be seen, the results are similar regardless of whether we consider 2001 or 2006 networks.

V. Regional Firms

Consumers may go to restaurants and grocery stores in neighboring regions. Therefore, another challenge is to separate regional spillovers through firms' internal networks from possibly confounding *direct* demand effects from nearby regions. Accounting for direct demand effects is important, because some firms in our sample are "regional firms," which exhibit a relatively high degree of geographical concentration. Besides, for regional firms, $\Delta \log(HP)_{06-09}$ (other) may be correlated with $\Delta \log(HP)_{06-09}$, thus potentially reflecting local consumer demand shocks. Across all firms in our sample, the correlation between $\Delta \log(HP)_{06-09}$ and $\Delta \log(HP)_{06-09}$ (other) is 12.1 percent. However, as is shown below, this correlation is (almost) entirely driven by regional firms.

In Table 7, we address the issue of regional firms in different ways. In panel A, we directly control for changes in house prices in nearby regions. In panel B, we exclude nearby regions when computing $\Delta \log(HP)_{06-09}$ (other) or include only establishments when all of the firm's other establishments are out of state. In panel C, we exclude regional firms from our sample. Since most of our observations are *not* from regional firms, the sample size remains always large. Importantly, in all of those samples, the correlation between $\Delta \log(HP)_{06-09}$ and $\Delta \log(HP)_{06-09}$ (other) is extremely small, ranging from 1.2 percent to 2.9 percent.

TABLE 7—REGIONAL FIRMS

	$\Delta \log(Emp)_{07–09}$				
	(1)	(2)	(3)	(4)	(5)
Panel A. Controlling for house prices in nearby regions					
$\Delta \log(HP)_{06–09}$	0.076	0.077	0.089	0.073	
	(0.029)	(0.028)	(0.024)	(0.029)	
$\Delta \log(HP)_{06–09}$ (other)	0.024	0.026	0.027	0.021	0.020
	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)
$\Delta \log(HP)_{06–09}$ (other, 100 miles)	0.018				
	(0.010)				
$\Delta \log(HP)_{06–09}$ (other, 200 miles)		0.014			
		(0.010)			
$\Delta \log(HP)_{06–09}$ (other, 300 miles)			0.005		
			(0.009)		
$\Delta \log(HP)_{06–09}$ (other, proximity)				0.013	0.011
				(0.008)	(0.007)
Industry fixed effects	Yes	Yes	Yes	Yes	—
Zip code × industry fixed effects	—	—	—	—	Yes
R^2	0.02	0.02	0.02	0.02	0.29
Observations	385,000	385,000	385,000	385,000	385,000

	$\Delta \log(Emp)_{07–09}$				
	≥100 miles (1)	≥250 miles (2)	≥500 miles (3)	Out-of-state (4)	No other in-state est. (5)
Panel B. Excluding nearby regions					
$\Delta \log(HP)_{06–09}$ (other)	0.022	0.021	0.019	0.020	0.021
	(0.005)	(0.005)	(0.007)	(0.007)	(0.010)
Zip code × industry fixed effects	Yes	Yes	Yes	Yes	Yes
R^2	0.29	0.30	0.31	0.31	0.74
Observations	365,100	340,800	310,700	295,000	8,900

	$\Delta \log(Emp)_{07–09}$				
	≥10 states (1)	≥15 states (2)	≥20 states (3)	All census regions (4)	Lowest 5% correlation (5)
Panel C. Excluding regional firms					
$\Delta \log(HP)_{06–09}$ (other)	0.022	0.025	0.026	0.028	0.026
	(0.010)	(0.011)	(0.011)	(0.012)	(0.009)
Zip code × industry fixed effects	Yes	Yes	Yes	Yes	Yes
R^2	0.34	0.34	0.36	0.39	0.33
Observations	226,600	210,700	197,700	170,900	247,500

Notes: This table presents variants of the specifications in column 2 of Table 2 and column 2 of Table 3. In panel A, columns 1 to 3 control for changes in house prices within a 100, 200, or 300 mile radius around the establishment's zip code. Columns 4 and 5 control for proximity weighted changes in house prices in other zip codes, where relatively more weight is placed on nearby zip codes. In panel B, zip codes within a 100, 250, or 500 mile radius around the establishment's zip code (columns 1 to 3) or within the same state (column 4) are excluded when computing $\Delta \log(HP)_{06–09}$ (other). In column 5, the sample is restricted to establishments whose parent firms have no other establishments in the same state. In panel C, the sample is restricted to firms operating in at least 10, 15, or 20 states (columns 1 to 3) or in all four census regions (column 4). In column 5, the sample is restricted to firms whose correlation between $\Delta \log(HP)_{06–09}$ and $\Delta \log(HP)_{06–09}$ (other) lies in the bottom 5 percent across all firms in the sample. All regressions are weighted by establishment-level employment. Standard errors (in parentheses) are double clustered at the firm and county level.

In columns 1 to 3 of panel A, we control for changes in house prices within a 100, 200, or 300 mile radius around the establishment's zip code. Including this control mainly affects the coefficient on $\Delta \log(HP)_{06-09}$—it is smaller the tighter is the radius around the establishment's zip code—while the coefficient on $\Delta \log(HP)_{06-09}$ (other) remains largely unaffected. Accordingly, $\Delta \log(HP)_{06-09}$ (other) is *not* mainly picking up the effects of house prices from nearby regions. In columns 4 and 5, we control for proximity-weighted changes in house prices in other regions. While this control is marginally significant, the coefficient on $\Delta \log(HP)_{06-09}$ (other) drops only slightly and remains highly significant. Next, in columns 1 to 4 of panel B, we exclude all zip codes within a 100, 250, or 500 mile radius or within the same state when computing $\Delta \log(HP)_{06-09}$ (other). Although the coefficient on $\Delta \log(HP)_{06-09}$ (other) is highly significant, it becomes weaker as we exclude larger parts of the firm's internal network. In column 5, we only include establishments if all of the firm's other establishments are out of state. Thus, as in column 4, $\Delta \log(HP)_{06-09}$ (other) is comprised of out-of-state house prices. Finally, in columns 1 to 4 of panel C, we require that firms operate in at least 10, 15, or 20 states or in all four census regions, while in column 5, we rank firms based on their correlation between $\Delta \log(HP)_{06-09}$ and $\Delta \log(HP)_{06-09}$ (other) and only include the bottom 5 percent with the lowest correlations.

In Table 8, we aggregate establishments at either the firm-county or firm-state level. By construction, this aggregation accounts for any direct demand spillovers within a county and state, respectively.[16] Besides, in the state-level aggregation, $\Delta \log(HP)_{06-09}$ (other) exclusively consists of out-of-state house prices, alleviating concerns that it may capture local consumer demand shocks. To include region $\times$ industry fixed effects, we restrict the sample to "pure industry firms" that have all of their establishments in a single 4-digit NAICS code industry. As is shown, both elasticities—with respect to local house prices and house prices in other regions in which the parent firm is operating—are remarkably stable as we increase the level of regional aggregation.

VI. Consumer Demand versus Collateral Channel

Prior research shows that changes in house prices affect local consumer demand (Mian, Rao, and Sufi 2013; Kaplan, Mitman, and Violante 2016; Stroebel and Vavra 2019) and thus employment in the non-tradable sector (Mian and Sufi 2014, Giroud and Mueller 2017). But changes in house prices may affect employment also through another channel, namely, by affecting the collateral value of firms' real estate (Chaney, Sraer, and Thesmar 2012; Adelino, Schoar, and Severino 2015; Ersahin and Irani 2018). Under this "collateral channel," firms' internal networks still matter, but not because they propagate shocks to local consumer demand. Rather, they matter because they propagate local shocks to firms' collateral values.

[16] Based on the firm-county level aggregation, online Appendix Table B18 examines if firms' internal networks are correlated with either banking or social networks. Banking networks are constructed in the same way as (non-tradable) firm networks by aggregating establishments of commercial banks (NAICS code 522110) at the firm-county level. Social networks are based on the county-level Facebook network in Bailey, Cao, Kuchler, Stroebel, and Wong (2018) (Social Connectedness Index).

TABLE 8—AGGREGATION AT THE FIRM-COUNTY AND FIRM-STATE LEVEL

	Δ log(Emp)$_{07–09}$			
	Firm-county level		Firm-state level	
	(1)	(2)	(3)	(4)
Δ log(HP)$_{06–09}$	0.091		0.085	
	(0.016)		(0.022)	
Δ log(HP)$_{06–09}$ (other)	0.026	0.024	0.023	0.021
	(0.009)	(0.008)	(0.011)	(0.010)
Industry fixed effects	Yes	—	Yes	—
County $\times$ industry fixed effects	—	Yes	—	—
State $\times$ industry fixed effects	—	—	—	Yes
R^2	0.01	0.22	0.05	0.14
Observations	110,300	110,300	38,500	38,500

Notes: This table presents variants of the specifications in column 2 of Table 2 and column 2 of Table 3 in which establishments are aggregated at either the firm-county (columns 1 and 2) or firm-state level (columns 3 and 4), the firm network weights ω are replaced with either firm-county or firm-state network weights, and changes in house prices are measured at either the county or state level. All regressions are weighted by firm-county (firm-state) employment. Standard errors (in parentheses) are double clustered at the firm and county (state) level.

Such "collateral shocks" affect firms' budget constraints in ways that are similar to the cash-flow shocks in our model in Section I.

In Table 9, we examine the collateral channel hypothesis in two different ways. In panel A, we consider tradable industries.[17] While the collateral channel should matter in these industries, the local demand channel should not matter, because demand for tradable goods is national or global. We construct tradable firms' internal networks in the same way as we construct non-tradable firms' internal networks. As columns 1 and 2 show, tradable establishment-level employment is not sensitive to changes in local house prices, echoing similar findings by Mian and Sufi (2014) at the county level. Moreover, as columns 2 and 3 show, tradable establishment-level employment is also not sensitive to changes in house prices in other regions in which the firm is operating. Altogether, these results are inconsistent with the collateral channel.

One reason why the collateral channel may be less important is that non-tradable firms—especially large restaurant and retail chains—often rent or lease their real estate rather than owning it. In panel B, we consider a setting where it is unlikely that firms own their real estate: establishments that are located in shopping malls. We identify a given location as a shopping mall if (i) at least five non-tradable establishments are located at the same address (same street name and number), or (ii) the establishment's address field contains "MALL," "SHOPPING CENTER," or "SHOPPING CTR." To ensure that shopping malls constitute an important part of the firm's internal network, we restrict our sample to firms that have at least 75 or 90 percent of their establishments in shopping malls. Moreover, we only include firms' actual shopping mall locations in $\Delta \log(HP)_{06-09}$ (other). Hence, the coefficient on $\Delta \log(HP)_{06-09}$ (other) measures the elasticity of establishment-level employment with respect to house prices in locations where it is unlikely that firms

[17] Industries are classified 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.

Table 9—Consumer Demand versus Collateral Channel

	$\Delta\log(Emp)_{07\text{–}09}$		
	(1)	(2)	(3)
Panel A. Tradable industries			
$\Delta\log(HP)_{06\text{–}09}$	0.010	0.009	
	(0.019)	(0.022)	
$\Delta\log(HP)_{06\text{–}09}$ (other)		0.002	0.001
		(0.016)	(0.017)
Industry fixed effects	Yes	Yes	—
Zip code × industry fixed effects	—	—	Yes
R^2	0.05	0.05	0.35
Observations	29,700	29,700	29,700

	$\Delta\log(Emp)_{07\text{–}09}$	
	≥75% in malls	≥90% in malls
	(1)	(2)
Panel B. Shopping malls		
$\Delta\log(HP)_{06\text{–}09}$ (other)	0.024	0.025
	(0.011)	(0.012)
Zip code × industry fixed effects	Yes	Yes
R^2	0.38	0.41
Observations	83,800	56,900

Notes: Panel A presents variants of the specifications in columns 1 and 2 of Table 2 and column 2 of Table 3 in which the sample is restricted to establishments in tradable industries. Panel B presents variants of the specification in column 2 of Table 3 in which the sample is restricted to firms that have at least 75 percent or 90 percent of their establishments in shopping malls, and where only firms' shopping mall locations are included when computing $\Delta\log(HP)_{06\text{–}09}$ (other). Shopping mall locations are described in Section VI. All regressions are weighted by establishment-level employment. Standard errors (in parentheses) are double clustered at the firm and county level.

own their real estate. Under the collateral channel, this coefficient should be insignificant. As is shown, however, the coefficient is significant and of the same magnitude as the corresponding coefficient in column 2 of Table 3. Accordingly, our results are unlikely to be explained by the collateral channel.

VII. Cross-Sectional Heterogeneity

A. *Financial Constraints*

Our model in Section I predicts that local consumer demand shocks spill over to other regions only if the firm is financially constrained. The magnitude of the spillover depends on how tight the firm's financial constraint is. The tighter is this constraint, the more sensitive is regional firm-level employment to demand shocks in other regions. In Table 10, we take these predictions 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 show that firms with higher leverage in 2006, at the onset of the Great Recession, are more financially constrained in the Great Recession. In columns 2 and 3, we use the Kaplan-Zingales (Kaplan

Table 10—Financial Constraints

	Δ log(Emp)$_{07–09}$		
	Leverage (1)	KZ-index (2)	WW-index (3)
Δ log(HP)$_{06–09}$ $\times$ FC	0.130	0.003	0.051
	(0.045)	(0.001)	(0.014)
Δ log(HP)$_{06–09}$ (other)	0.009	0.008	0.010
	(0.012)	(0.010)	(0.016)
Δ log(HP)$_{06–09}$ (other) $\times$ 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 $\times$ industry fixed effects	Yes	Yes	Yes
R^2	0.42	0.42	0.42
Observations	124,100	124,100	124,100

Notes: This table presents variants of the specification in column 2 of Table 3 in which $\Delta\log(HP)_{06–09}$ and $\Delta\log(HP)_{06–09}$ (other) are interacted with measures of firms' financial constraints (FC) in 2006. In column 1, FC is firm leverage (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. The sample is restricted to 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 Zingales 1997) and Whited-Wu (Whited and Wu 2006) index, respectively. Both indices are widely used in the finance literature. All three measures are only available for publicly listed firms. Accordingly, we restrict our sample to firms in the LBD that have a match in Compustat.

As can be seen, regardless of how we measure firms' financial constraints (FC), the interaction term $\Delta\log(HP)_{06-09}$ (other) $\times$ FC is always positive and highly significant. Thus, establishments of more financially constrained firms exhibit larger elasticities of employment with respect to house prices in other regions in which the parent firm is operating. In fact, for the least financially constrained firms in our sample, we find no evidence that establishment-level employment is sensitive to changes in house prices in other regions—the (stand-alone) coefficient on $\Delta\log(HP)_{06-09}$ (other) is insignificant. Finally, we find that establishments of more financially constrained firms have larger employment elasticities with respect to *local* house prices. Overall, these results suggest that financial constraints matter, both for how consumer demand shocks spill over to other regions and how employment responds locally to these shocks.[18]

[18] Survey evidence by Campello, Graham, and Harvey (2010) supports the notion that firms' financial constraints matter during the Great Recession. The authors asked 574 US CFOs in 2008 whether their firms are financially constrained and what they are planning to do in the following year. Firms classified as financially constrained based on tangible measures—credit rationing, high costs of borrowing, and difficulties in initiating or renewing a credit line—said they would cut employment by 10.9 percent. By contrast, financially unconstrained firms said they would cut employment only by 2.7 percent.

Table 11—Geographic Dispersion

	Δ log$(Emp)_{07–09}$		
	Multi-region	Number of zip codes	GD-HHI
	(1)	(2)	(3)
$\Delta\log(HP)_{06–09} \times$ GD	−0.023	−0.010	−0.501
	(0.011)	(0.003)	(0.095)
GD	0.006	0.004	0.049
	(0.002)	(0.002)	(0.029)
$\Delta\log(HP)_{06–09} \times$ firm size	−0.002	−0.002	−0.003
	(0.001)	(0.001)	(0.001)
Firm size	0.003	0.004	0.006
	(0.001)	(0.001)	(0.001)
Zip code × industry fixed effects	Yes	Yes	Yes
R^2	0.20	0.29	0.29
Observations	910,300	385,000	385,000

Notes: This table presents variants of the specification in column 1 of Table 2 in which $\Delta\log(HP)_{06–09}$ is interacted with measures of firms' geographic dispersion (GD) in 2006, and the industry fixed effects are replaced with zip code × industry fixed effects. In column 1, GD is a dummy variable indicating whether the firm operates in multiple zip codes (multi-region firm). The sample consists of all non-tradable establishments, including single-region firms. In column 2, GD is the number of zip codes in which the firm operates. In column 3, GD is one minus the Herfindahl-Hirschman index (HHI) measuring the firm's geographic dispersion based on its employment at the zip code level. All three columns control for firm size (log number of employees in 2006) and $\Delta\log(HP)_{06–09}$ × firm size. All regressions are weighted by establishment-level employment. Standard errors (in parentheses) are double clustered at the firm and county level.

B. *Geographic Dispersion*

There are two sides to being part of a multi-region firm. One is that local firm units absorb some of the impacts of shocks in other regions. The flip side is that firm units in other regions absorb some of the impacts of local shocks. Accordingly, our model in Section I predicts that establishments of multi-region firms should be less sensitive to (their own) local shocks than single-region firms.

Table 11 provides suggestive evidence showing that establishments of multi-region firms—and firms which are generally more geographically dispersed—are less sensitive to local shocks. We use three different measures of firms' geographic dispersion. In column 1, we use a dummy indicating whether the firm operates in multiple zip codes (multi-region firm). In column 2, we use the number of zip codes in which the firm operates. In column 3, we construct a Herfindahl-Hirschman Index (HHI) measuring the firm's geographic dispersion 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 all three cases, we control for firm size and $\Delta\log(HP)_{06-09}$ × firm size to account for the possibility that more geographically dispersed firms may also be larger in general. As is shown, regardless of how we measure firms' geographic dispersion (GD), the interaction term $\Delta\log(HP)_{06-09}$ × GD is always negative and significant.

While the results in Table 11 show that establishments of multi-region firms are less sensitive to local shocks than single-region firms, our previous results show that they are sensitive to shocks in distant regions. Whether or not establishments of multi-region firms exhibit overall smoother employment depends on the net effect. The online Appendix shows that establishments of multi-region firms exhibit lower 10- and 20-year employment volatility than single-region firms.[19] Table B19 compares raw means, while Table B20 shows a regression with zip code $\times$ industry fixed effects and firm size as a control. In both tables, the difference between multi- and single region firms is significant at the 1 percent level. We need to caution, however, that these are only partial equilibrium results and therefore do not imply that aggregate employment volatility—or, likewise, aggregate employment responses to regional shocks—would be lower in a (counterfactual) world with only multi-region firms. Drawing such inferences requires estimating counterfactual elasticities in a structural general equilibrium model in which multi-region firms can be effectively "switched on and off."

C. *Proximity to Headquarters*

In online Appendix Table B21, we explore whether establishments which are located closer to headquarters are more insulated from shocks. For instance, it may be easier for such establishments to lobby headquarters, or headquarters may simply care more about nearby establishments. Alternatively, proximity may facilitate information flows and monitoring, leading to higher productivity, in which case it may be efficient to favor nearby establishments.[20] We use three different measures of proximity: geographical distance and dummy variables indicating whether the establishment and headquarters are located in the same zip code and county, respectively. We find that establishments which are located closer to headquarters exhibit smaller employment elasticities with respect to both local house prices and house prices in other regions in which the firm is operating, suggesting that they are more insulated from shocks.[21]

VIII. Aggregate Regional Employment

Regional spillovers through firms' internal networks may not matter for aggregate employment if workers of multi-region firms that are laid off due to shocks in other regions are re-employed by (local) firms that are less exposed to those regions. To account for the possibility of worker reallocation within a given region, we consider total non-tradable 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 by either multi-region firms or (local) single-region firms.

[19] Guiso, Pistaferri, and Schivardi (2005) provide empirical evidence showing how firms provide risk-sharing to their workers by smoothing output shocks intertemporally.

[20] Giroud (2013) shows that proximity to headquarters positively affects plant-level productivity.

[21] Online Appendix Table B22 includes financial constraints (firm leverage, Kaplan-Zingales index, Whited-Wu index), geographic dispersion (number of zip codes in which the firm operates), and proximity to headquarters (same zip code) in a single multivariate regression.

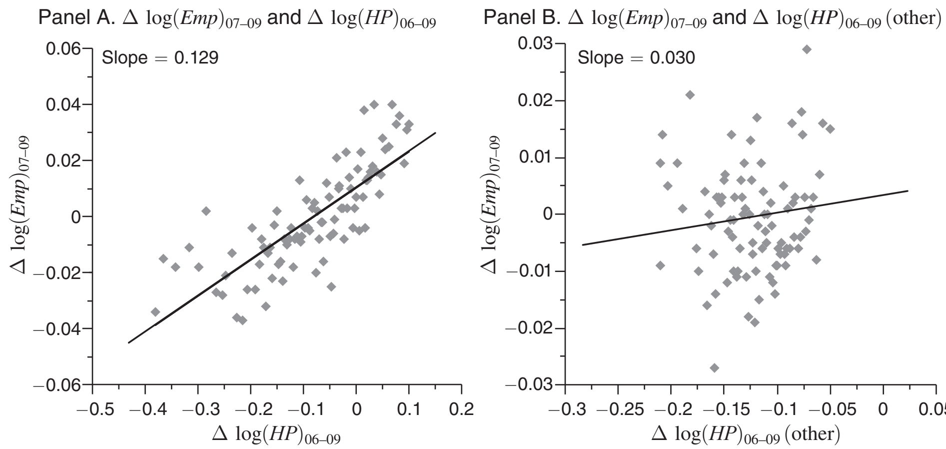

Figure 3. Non-Tradable County-Level Employment

Notes: The bin scatterplots are constructed analogously to those in Figure 1. Panel A depicts the relationship between changes in non-tradable county-level employment, $\Delta \log(Emp)_{07–09}$, and changes in county-level house prices, $\Delta \log(HP)_{06–09}$. Panel B depicts the relationship between changes in non-tradable county-level employment, $\Delta \log(Emp)_{07–09}$, and changes in house prices in other counties linked through firms' internal networks, $\Delta \log(HP)_{06–09}$ (other).

A. *Main County-Level Results*

Figure 3 provides a visual impression by plotting the relationship between changes in non-tradable county-level employment and either changes in county-level house prices (panel A) or changes in house prices in other counties linked through firms' internal networks (panel B). As can be seen, the bin scatterplots look similar to those in Figure 1. In both panels, there is a positive relationship between changes in county-level employment and changes in county-level house prices.

Table 12 offers suggestive empirical evidence. All regressions include demographic controls (income, age, education) as well as county-specific employment shares of all 2-digit NAICS industries to account for the possibility that counties with exposure to particular industries are harder hit during the Great Recession (see Mian and Sufi 2014). As column 1 shows, the elasticity of county-level employment with respect to local house prices is 0.122. This is somewhat larger than in our establishment-level analysis, reflecting the fact that our sample now also includes single-region firms, which tend to respond more strongly to local shocks.[22] In column 2, the elasticity of county-level employment with respect to house prices in other counties linked through firms' internal networks is 0.024, which is only slightly lower than in our establishment-level analysis. We may thus conclude that regional spillovers through firms' internal networks affect aggregate regional employment. Finally, in columns 3 to 7, we perform the same placebo tests as in Table 2. In all

[22] Online Appendix Table B25 confirms that single-region firms are relatively more sensitive to local shocks. A likely reason is that single-region firms—which include small mom-and-pop shops—are more financially constrained. See Giroud and Mueller (2017, Section III.F) for a related discussion.

Table 12—Aggregate Employment at the County Level

	$\Delta\log(Emp)_{07\text{–}09}$						
			Placebo tests				
			Equal weights	Population weights	Income weights	HH debt weights	Random counties
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\Delta\log(HP)_{06\text{–}09}$	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)
$\Delta\log(HP)_{06\text{–}09}$ (other)		0.024					
		(0.007)					
$\Delta\log(HP)_{06\text{–}09}$ (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^2	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

Notes: The dependent variable, $\Delta\log(Emp)_{07\text{–}09}$, is the percentage change in non-tradable county-level employment from 2007 to 2009. County-level employment is based on all firms in a county, including single-region firms. $\Delta\log(HP)_{06\text{–}09}$ is the percentage change in county-level house prices from 2006 to 2009. $\Delta\log(HP)_{06\text{–}09}$ (other) is the network-weighted percentage change in house prices from 2006 to 2009 in other counties linked through firms' internal networks based on 2006 county network weights. The county network weights are described in Section IIB. The placebo tests in columns 3 to 7 are analogous to those in Table 2. Demographic controls include income, education, and age. All three variables are described in Table 1. Industry controls are the county-specific employment shares of all 23 2-digit NAICS code industries in 2006. All regressions are weighted by county-level employment. Standard errors (in parentheses) are clustered at the state level.

of these placebo tests, house prices in other counties have no significant effect on county-level employment.[23]

B. *Discussion and Extensions*

Labor Market Frictions.—Labor market frictions may prevent workers of multi-region firms that are laid off due to shocks in other regions from being re-employed by local firms. Empirical evidence suggests that labor market frictions were particularly severe during the Great Recession. Davis (2011) and Davis, Faberman, and Haltiwanger (2013) find a sharp drop in both search and recruiting intensity, and Şahin et al. (2014) find a significant increase in mismatch between job seekers and vacant jobs. Overall, Foster, Grim, and Haltiwanger (2016) note that the intensity of labor reallocation fell rather than rose in the Great Recession, contrary to previous recessions. The authors 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, p. S305).

In addition to search and matching frictions, wage rigidity constitutes an important labor market friction. For local firms to absorb additional labor, wages would have to decline. However, as online Appendix Table B24 shows, the elasticity of

[23] Online Appendix Table B23 shows that our county-level results are robust to controlling for proximity-weighted changes in house prices in other counties, excluding all counties within a 500 mile radius or within the same state, and forming county-level networks based on large firms operating in at least 20 states or in all four census regions.

non-tradable county-level wages with respect to both local house prices and house prices in other counties linked through firms' internal networks is small and insignificant. This is true regardless of whether we consider wages at single-county firms, multi-county firms, or all firms within a county.[24]

Local Spillover Effects.—In online Appendix Table B25, we estimate our county-level regression in column 2 of Table 12 separately for single- and multi-county firms. Two results stand out. First, single-county firms are highly sensitive to local consumer demand shocks. Their elasticity of employment with respect to local house prices is 0.161, which is almost twice as large as the corresponding elasticity for multi-county firms. Second, layoffs at multi-county firms due to consumer demand shocks in other regions seem to negatively impact (local) single-county firms. One possible channel is that workers of multi-county firms which are laid off cut back on their local restaurant and retail spending.[25] Indeed, we can almost fully explain the elasticity of county-level employment with respect to house prices in other counties shown in column 2 of Table 12 as the sum of this local spillover effect and the original effect on multi-county firms. In Table B25, the coefficient on $\Delta \log(HP)_{06-09}$ (other) is 0.031 for multi-county firms and 0.011 for single-county firms. Since multi-county firms account for 61.2 percent of non-tradable county-level employment, this implies a weighted average elasticity of county-level employment with respect to house prices in other counties of 0.023 $\left(= 0.031 \times 61.2\% + 0.011 \times 38.8\%\right)$, which is almost identical to the elasticity of 0.024 in column 2 of Table 12.

Counties in Which House Prices Did Not Fall.—Online Appendix Table B26 focuses on counties in which house prices did *not* fall during the Great Recession, but which are linked (through firms' internal networks) to other counties in which house prices fell sharply. The underlying assumption is that counties in which house prices did not fall and those in which house prices fell sharply were unlikely hit by common regional shocks—or else they would have displayed more similar patterns with respect to house prices. Regardless of whether we consider counties in which house prices increased or changed only a little ($\pm$2.5 percent), we find that the elasticity of county-level employment with respect to house prices in other counties is similar to the corresponding elasticity in Table 12.[26]

Trade Channel.—Local consumer demand shocks may indirectly affect non-tradable employment in other counties, namely, through the trade channel. Precisely, they may lead to employment losses in other counties' *tradable* sector, which may ultimately spill over to the non-tradable sector. In online Appendix Table B28, we test this hypothesis in two ways. First, we consider tradable

[24] As we noted in Section IVA, one reason for the downward wage rigidity may be that workers' pay in many restaurant and retail jobs is already at or near the minimum wage.

[25] Moretti (2010), Huber (2018), and Bernstein et al. (forthcoming) all find significant spillover effects on local non-tradable employment.

[26] As online Appendix Table B27 shows, wages in these counties did not fall in response to consumer demand shocks in other counties, in contrast to employment. Thus, wage frictions can possibly explain why workers of multi-county firms are not re-employed by local firms, even though the county itself experienced no consumer demand shock of its own.

county-level employment. Second, we consider non-tradable county-level employment but form county-level linkages based on tradable firms' internal networks. In both of these placebo tests, the elasticity of county-level employment with respect to house prices in other counties is small and insignificant. Thus, our results are unlikely to be explained by the trade channel hypothesis.

IX. Conclusion

Using confidential establishment-level data from the US Census Bureau's Longitudinal Business Database, this paper documents how local shocks propagate across US regions through firms' internal networks of establishments. Consistent with a simple model of optimal within-firm resource allocation, we show that (i) establishment-level employment is sensitive to shocks in distant regions in which the parent firm is operating; (ii) the elasticity with respect to such shocks increases with firms' financial constraints; and (iii) establishments of multi-region firms are less sensitive to (their own) local shocks than single-region firms. What matters is that establishments are linked to other regions in which the parent firm is operating, not other regions in general. If we link establishments to other regions using equal weights, population weights, or income weights, or if we link them to randomly selected regions, their elasticity of employment with respect to shocks in other regions is close to zero and insignificant.

We account for common shocks to regions in which the parent firm is operating by saturating our model with highly granular zip code $\times$ industry fixed effects. We also conduct placebo tests using counterfactual firm networks and focus on establishments whose firm affiliation, but not their location, has changed. To account for direct demand spillovers from nearby regions, we exclude all regions within a certain radius or within the same state, exclude regional firms from our sample, and aggregate establishments at either the firm-county or firm-state level. Finally, to account for the possibility of worker reallocation within a given region, we consider aggregate employment by all firms in a county. Similar to what we previously found at the establishment level, we find large elasticities of county-level employment with respect to shocks in other counties linked through firms' internal networks. Accordingly, regional spillovers through firms' internal networks matter for aggregate regional employment.

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