Spillovers through Multimarket Firms: The Uniform Product Replacement Channel*

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Abstract

We study how regional housing market disruptions spill over across US local markets through intrafirm spatial networks created by multimarket firms. We identify spillovers by linking granular data on product-county-level prices and quantities with producer-level information and exploiting variation in firms' exposure to differential declines in local house prices. A firm's local sales decrease following a local housing price decline but do so more strongly to indirect exposure to the housing price decline originating in its other markets. The barcode-level data reveal a novel uniform product replacement mechanism behind the spillover: Firms replace higher-value products with lower-value products in response to the housing market disruptions, and such product replacements are synchronized across markets within each firm, including the markets with stable housing prices. These results have new implications for (i) firm-level returns to scale, (ii) intrafirm spillover mechanisms, and (iii) regional household consumption.

JEL Codes: E20, E32, F44, L11, L22, R32.

Keywords: Network, Spillover, Product Replacement, Great Recession, Housing Crisis.

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1 Introduction

The question of how and to what extent regional economic shocks spill over and affect other distant regions has been a longstanding interest in the macroeconomics and international economics literature. This topic has been extensively studied to understand the source of the interregional business cycle comovements and risk-sharing, as well as to appreciate the importance of economic networks in studying shock propagation and its macroeconomic implications. Such questions have become equally relevant in within-country contexts, especially during and in the aftermath of the Great Recession. As the crisis involved a large differential collapse in local housing markets followed by wide disparities in regional economic activity within the United States, previous papers have established a large effect of changes in local housing market conditions on local consumption and nontradable employment. The effect of such regional housing market disruptions may not be restricted to local markets of origin because the economy is highly connected across regions through various linkages; regional shocks could spill over and propagate and potentially reshape household welfare across locations.

What is notably not well understood in this study of spillovers is the role of spatial networks created by *multimarket firms*—producers selling their products in multiple counties and states that play an important role in US economic activities. Because these firms could make their product supply decisions at the firm level, the appearance of a negative demand shock in one market can cause them to change their product supply decision in another market. Three outcomes are possible: affected firms increase, decrease, or do not change the sales in the unaffected market. For example, the negative regional shock may decrease firms' total revenue substantially and make firms unable to cover their firm-wide operation costs, leading them to decrease the supply and sales in the unaffected markets. On the other hand, when firms face a negative demand shock and cannot sell their products in one market, they might sell their products in the other market to keep up their firm-level sales. In this case, a decrease in demand and sales in one market leads to an increase in sales in the other market. Also, it is possible that firms make their decision entirely at the local level and do not spill over the regional shock, as standard international macro and trade models with constant marginal costs predict (e.g., Backus et al. 1992; Melitz 2003).

This paper fills the gap by empirically establishing reduced-form causality of within-firm regional spillovers through multimarket firms and identifying a novel mechanism behind the spillover.

We construct highly detailed micro-level data that link product-county-level prices and quantities from the ACNielsen Retail Scanner database with various producer-level variables from the National Establishment Time-Series (NETS) database for the first time. Our combined dataset contains information on barcode-level product prices and quantities sold in each county produced by public and private firms and their establishment-level information in the United States. As a specific example, in 2007, Kraft Foods company generated sales from 2462 counties in 49 states, including Philadelphia County, Pennsylvania. In the data, we observe prices and quantities sold in Philadelphia and all other markets of the Kraft company separately for each barcode-level product (e.g., natural shredded part-skim organic mozzarella cheese) and Kraft's establishment location, primary industry code, and credit ratings. To investigate the consequence of a sudden differential collapse in regional house prices in 2007-09, we supplement our dataset with the county-, state-, and zip code-level house prices from the Zillow and Federal Housing Finance Agency (FHFA) data and county characteristics from the Census. We also incorporate three instrumental variables (IVs) used in recent literature for robustness analyses: house supply elasticity (Saiz 2010), house price sensitivity (Guren et al. 2021), and a nonlocal mortgage lending shock (García 2018).[1]

The unique combined data reveal four stylized facts, which point to the importance of multimarket firms and suggest their uniform product assortment decisions across markets. First, multimarket firms are prevalent and dominant in the US economy. More than 90% of consumer goods producers sold their products in multiple counties (markets), and these multimarket firms accounted for more than 99% of total consumer goods expenditures in 2007.[2] Second, multimarket

[1]We are grateful to the authors for either uploading their estimates on their websites or directly sharing their estimates with us.

[2]Defining a state as a market shows a similar empirical pattern (Appendix C.3). At the international level, only 36% of firms export to more than one market (country), but they account for 96.7% of total exports and 85.8% of total employment (Bernard et al. 2007).

firms sell to many markets; the mean and median number of markets are 670 and 155, respectively, indicating their influence on many counties. Third, firms do not make county-specific products in general, suggesting the national uniform product assortment decision. For instance, the median firm makes 12 products to serve 155 counties. Lastly, most of these firms produce a small number of product groups in a small number of plants, especially relative to the number of counties they serve. This result suggests the importance of studying spatial spillover operating through where firms sell their products relative to which product groups they sell or where they produce their products.

In providing direct evidence on spatial spillover, we exploit variations in multimarket firms' exposure to a sudden differential decrease in local house prices during the Great Recession. Our data reveal considerable variation in the amount of revenues firms generated from different markets before the housing market collapse; some firms created most of their sales from certain markets, whereas others did so from other markets. As a result, when specific markets experienced abrupt deterioration in their local economic condition due to falling property prices, those firms that initially earned a significant share of their revenues from these troubled markets suffered more than their counterparts. Exploiting these differential declines in product demand across markets, which are plausibly exogenous to firm characteristics, we analyze how firms change their local market sales when they face different degrees of indirect demand conditions originating from their other markets. For example, consider two firms that mainly sold cheese and earned similar revenues in 2007, Arla Foods Company and Swiss American Company, and a market where they jointly sold their products, Greenville, South Carolina. In 2007, much of the sales of Arla Foods came from California, where housing prices fell dramatically from 2007 to 2009. In the same initial year, the major markets for Swiss American were in Missouri, where housing prices were relatively stable during the crisis. Conditioning on the local economic conditions in Greenville, we compare how much Arla Foods changed their sales in Greenville relative to Swiss Americans.

Armed with detailed data and plausibly exogenous variations, we establish the regional spillover through multimarket firms: A firm's local sales decrease in response to direct local housing price fall but do so more strongly to the intrafirm indirect shock, which is the average negative shock originating in the firm's other markets. Strikingly, a firm's county-level sales growth decreases by 3.5 percentage points when it faces a 10 percentage point average decline in house price growth in other counties connected through its market network, while it only decreases by 0.6 percentage points given the same percentage point decrease in direct county house price growth. Comparing county-firm pairs that face extreme degrees of direct and indirect demand changes (the 95th-5th percentile ratio), the indirect shock decreases a local firm's sales by approximately 3 percentage points, which is more significant than the decrease from the direct shock. The relative effect suggests that the nonlocal firm-level decision, which has been overlooked in previous studies on local consumption, is a crucial determinant of the change in local firm sales during this period. The magnitude of the relative effect is intuitive, given that a typical firm sells to many markets other than the local market. For example, the median firm in our sample sells in 155 counties. When examining the local sales growth for this firm, the direct local shock captures the impact in that specific market, while the indirect shock reflects the average shock the firm faces across the other non-local 154 markets. Consistent with this intuition, we find a larger spillover effect when firms initially generate larger sales in other markets relative to the local market. Appendix B.1 visually illustrates the main empirical results.

We implement numerous supplementary analyses to address potential concerns related to the clustered shocks in nearby counties, retailer behaviors, spatial spillover interpretation, interindustry correlations, supply-side shocks, intranational and international trade linkages, similar household demographics across locations, the selection of firms into the indirect shock, product unit adjustment, shrinking variety effects, different samples, different measures of the dependent and independent variables, different definitions of markets, and alternative standard errors.

Our analyses reveal a novel uniform product replacement channel: The spillover effects occur because firms replace higher-value with lower-value products in response to adverse shocks, and within each firm, such product replacements are *synchronized* across many markets, including markets with stable product demand. As a result, firm product value and sales decline even in a local market that is not directly affected by the local shock, showing the spillover of demand shocks through the supply-side response. For a concrete example, consider Kraft company again. In 2007, it sold high-quality, organic, shredded, low-moisture, part-skim mozzarella cheese at $3.7 on average in 14 states and 119 counties. These counties include Philadelphia County in Pennsylvania, where housing prices were relatively stable during the crisis, and other counties that faced dramatic decreases in housing prices at the same time, such as Coos County in New Hampshire and Stafford County in Virginia. In 2009, Kraft withdrew this organic cheese in all 119 counties and instead introduced a new lower quality, nonorganic cheese with the same features (shredded, low-moisture, part-skim, mozzarella) at a lower price ($2.4 on average) in all 119 counties, including Philadelphia. As a result, Kraft generated lower sales in Philadelphia than its counterparts, which did not lower their product values. Appendix B.2 provides a narrative related to Kraft's product replacement behavior with its 10-K filing.

The three empirical findings with the granular barcode data strongly support the uniform product replacement channel, which cannot be identified from conventional, more aggregated product- or firm-level data or previous theoretical frameworks. First, by decomposing the spillover effect into the product replacement and the continuing product channels, we find that the entire intrafirm cross-market spillover effects arise from product replacement, specifically from those uniformly replaced products across multiple markets. The emphasis on the uniform product replacement channel contrasts with the conventional results that operate through the continuing product channel, which is confirmed when studying the traditional direct demand effect or using more aggregated data. Second, given the importance of uniform product replacement across markets, we additionally show that these newly replaced barcode-level products have lower value—sales per product, unit price, and organic share—compared to the value of exiting products and depress local sales. Lastly, we find that the spillover effect is more substantial for firms or counties where the uniform product replacement channel is more likely to be present: The effect is more significant for (i) firms that initially offered their products in multiple locations rather than tailoring their products to the local market, (ii) firms that initially sold to more homogeneous markets and have less incentive to tailor their products by market, (iii) firms that initially provided organic products and had the option to devalue their products during the crisis, and (iv) initially wealthy counties where lowering product quality would lead to a more significant loss of firm sales.

Implications in Related Literature. This paper connects several strands of literature in international economics, macroeconomics, financial economics, and industrial organization; Appendix A provides a more comprehensive discussion of the implications of our findings.

To the best of our knowledge, this paper is the first to study the question of spillover through multimarket producers (manufacturers) in an *intranational* setup, where detailed barcode-level data and the well-established identification strategy are available, and establish a positive sales correlation across markets within firms. Crucially, thanks to the barcode-level data, we identify a novel uniform product replacement channel, which stands in sharp contrast to the traditional models that either abstract away from the endogenous quality choice of firms or make firms target their product quality to local economic conditions. The new channel highlights the massive cost of product tailoring of firms to each market.

Our results indicate a critical difference between international and intranational multimarket firm behavior and the corresponding inference on the firm-level returns to scale. Specifically, since this paper is closely related to the study of international multimarket firms—exporters—that sell to domestic and foreign markets, our reduced-form results may appear at odds with Almunia et al. (2021). They show that Spanish exporters that face a negative demand shock originating from a housing market crisis in Spain *increase* their sales in their foreign markets by selling off their surplus, in contrast to our reduced-form empirical results, and infer the decreasing returns to scale at the firm level. The uniform product replacement channel, which leads to the firm-level increasing returns to scale, explains why we find the opposite results. For the multimarket firms to spill over the shocks through product replacement, they must initially sell the same barcode-level products across multiple markets; in our analyses, all the spillover effects arise from the products sold in multiple markets. Although we expect our results to apply to countries that likely share many barcode-level products, we do not expect firms to sell the same barcode-level products across countries in general.

Due to the vastly different customer characteristics, most international firms would tailor their products to each country because the resulting revenue gains would be larger than the costs of offering different products across countries.[3] As a result, other channels dictate the international spillover, such as the venting out surplus channel. In fact, although the uniform product replacement mechanism is dominant in studying the spillover within the US, we find empirical support for the venting out surplus channel emphasized in Almunia et al. (2021) among exporters (Table 6 and Appendix D.2).[4]

The uniform product replacement channel might appear surprising because it implies that financial friction, which has been at the heart of the within-entity linkage and spillover in the finance literature, is not necessary for the spillover.[5] Although we find weak yet supporting evidence of the financial constraint channel (Table 6), the granular data reveal that the most fundamental factor for the spillover is the uniform product replacement pattern and the associated changes in product attributes across markets. A simple theory built to match these empirical findings shows the spillover through the optimal decision-making firms in the absence of financial frictions (Appendix F). Instead, the model highlights the role of product scope and the fixed costs associated with producing and penetrating products into each market, which have been essential in understanding heterogeneous firm behavior in business cycles (Crouzet and Mehrotra 2020) and trade liberalization (Bernard et al. 2011). Note that one critical distinction that explains this difference from the previous literature emphasizing financial friction is that we analyze multimarket firms instead of multiplant firms, firms *producing* their goods and services in different plants located in multiple counties,

[3]This tradeoff multimarket firms face—the revenue gain from tailoring products to each market and the cost increase for producing and penetrating different products in different markets—is formally presented with the other channels in Appendix F.2 .

[4]Note that while we primarily compare our study with Almunia et al. (2021), others find a positive shock spillover through exporters with different samples (Berman et al. 2015). The results may differ because different samples feature different firm and market characteristics, which govern the spillover through product quantity and quality choices. The tradeoffs in theoretical analysis (Section F.2) and the related empirical evidence (Section 4 and Supplement E.17) show such characteristics, which affect the spillover via the uniform product quality choice. For the quantity choice, the heterogeneous slope of firm-level marginal cost across samples may be important, as discussed in Kim (2020a).

[5]For example, recent important work by Giroud and Mueller (2019) theoretically and empirically highlights the financial constraint of multiplant firms in generating a positive correlation of regional employment. Numerous other important papers, such as Cetorelli and Goldberg (2012); Gilje et al. (2016); Cortés and Strahan (2017), find the spillover effect via within-bank linkages through their liquidity positions.

states, or countries.[6] This seemingly minor difference is a first-order ingredient in several subfields of trade literature, from distinguishing between multinationals and exporters (Bernard et al. 2009; Antràs and Yeaple 2014) to understanding market concentration (Amiti and Heise 2021). In line with these studies, our paper calls for the careful distinction between multimarket and multiplant firms; unlike a multimarket firm network, Appendix E.4 shows no spillover via the multiplant firm network for tradable sectors we consider, consistent with Giroud and Mueller (2019).[7] We also confirm the robustness of the results by excluding counties where firms have their plants, providing reassurance that there is no confounding multiplant firm network effect (Appendix E.7).

In studying local policies and shocks, the detailed barcode-level and plant-level combined data underline the firms that produce the products (manufacturers) in addition to those repackaging them (retailers). Recent research spotlights the uniform pricing behavior of retailers in understanding the consequences of local economic shocks (Cavallo 2017, 2018; DellaVigna and Gentzkow 2019).[8] Given the importance of these papers, Section 3.1 explicitly allows for an intrafirm network of retailers in addition to that of multimarket firms (manufacturers) and finds that both networks independently spill over regional shocks. However, there is a stark asymmetry: Retailer spillover effects arise from continuing product sales, consistent with the uniform pricing mechanism, whereas the manufacturer spillover effect operates through uniform product replacement. Integrating both channels likely generates stronger regional shock propagation than the one with only retailers' uniform pricing behavior (Garcia-Lembergman, 2022; Daruich and Kozlowski, 2023). In the study of local consumption and welfare, we demonstrate the need for a more comprehensive analysis that integrates manufacturers' behaviors in addition to those of retailers. A back-of-the-envelope calculation shows that the identified effect on regional household consumption is

[6] Other studies also investigate this spillover. To name a few important papers, Cravino and Levchenko (2017); Bilir and Morales (2020); Gumpert et al. (2021); Bena et al. (2022) identify a positive interaction between parents and affiliates of multinational firms, and Chen et al. (2021) highlight the importance of the intraconglomerate network in understanding energy regulation policy. See also Ding (2020); Hyun et al. (2022) for the multiindustry intrafirm network related to a trade shock.

[7] Giroud and Mueller (2019) identify the spillover effect only based on the non-tradable sectors.

[8] Relatedly, in studying regional spillover through retailers, Butters et al. (2022) show that retailers respond asymmetrically to local cost and demand shocks, and Gopinath et al. (2011) identify the strong border effect. See also Gilbert (2017).

significant (Appendix F.3).

More broadly, this paper contributes to the rapidly growing literature that explores various networks that transmit and propagate micro-level shocks. The most prominent network is a supply chain network that propagates sector- and firm-specific shocks (e.g., Acemoglu et al. 2012, 2016; Barrot and Sauvagnat 2016; Acemoglu et al. 2020; Carvalho et al. 2021). In financial economics, several studies analyze the linkages created by interbank and intrabank networks (e.g., Cetorelli and Goldberg 2012; Gilje et al. 2016; Cortés and Strahan 2017; Baskaya et al. 2017; Mitchener and Richardson 2019; Doerr and Schaz 2021), internal capital markets (e.g., Shin and Park 1999; Giroud and Mueller 2015; Bartram et al. 2022; Dai et al. 2022), and social networks (Bailey et al. 2018). Our paper contributes to the literature by providing evidence of spatial spillovers via multimarket firms' within-firm market networks and their uniform product replacement behavior, which arises from the massive costs of producing and penetrating products to each market discussed in customer acquisition literature (Arkolakis, 2010; Drozd and Nosal, 2012; Afrouzi et al., 2021; Einav et al., 2021). Our identification strategy follows the literature analyzing the collapse of the housing market during the Great Recession. Previous studies document that a fall in house prices leads to a decline in local consumer spending (Mian et al. 2013; Kaplan et al. 2020a; Guren et al. 2021), price and price-cost markups (Stroebel and Vavra 2019), and employment (Mian and Sufi 2014; Giroud and Mueller 2017). We complement these studies by revealing the novel spillover effect arising from the decline in regional house prices. The product and firm turnover and quality changes have been emphasized in understanding the business cycle and price pass-through (Ghironi and Melitz, 2005; Jaimovich and Floetotto, 2008; Bernard et al., 2010; Broda and Weinstein, 2010; Nakamura and Steinsson, 2012; Jaimovich et al., 2019; Argente et al., 2018; Michelacci et al., 2022). For example, Granja and Moreira (2023) finds that firms introduce fewer novel products in response to supply-side factors. We show that such a pattern is uniform across markets, implying a new regional transmission mechanism.

The remainder of this paper is structured as follows. Section 2 describes the data, stylized facts, and construction of the key variables, Section 3 presents the main intrafirm spillover results, Section 4 provides empirical support for the underlying uniform product replacement mechanism. Section 5 concludes.

2 Data, Stylized Facts, and Measurement of Variables

2.1 Data

The dataset combines barcode-level prices and quantities sold in each county produced by public and private firms from the ACNielsen Retail Scanner database and various firm- and plant-level variables obtained from the GS1 and NETS data. The combined data allow us to construct a firm's county-specific sales and its connection to other counties where the firm generates sales, together with various pieces of firm-level information, including a firm's primary industry code, establishment location, and credit rating. We supplement our dataset with the county-level house prices from the Zillow database to measure local economic conditions.

The barcode-level price and quantity information in each county comes from the ACNielsen Retail Scanner database, which is made available by the Kilts Marketing Data Center at the University of Chicago Booth School of Business.[9] The data contain approximately 2.6 million barcode-level product prices and quantities recorded weekly from approximately 35,000 participating grocery, drug, mass merchandise, convenience, and liquor stores in all US markets. A barcode, a unique universal product code (UPC) assigned to each product, is used to scan and store product information. Participating retail stores use point-of-sale systems that record information whenever product barcodes are scanned during purchases. The data began in 2006 and ended in 2015, covering the Great Recession, the period of housing market collapse; we primarily focus on 2007 and 2009 to closely connect with the previous literature studying Great Recession and to avoid measurement errors associated with Nielsen's expanding sample coverage over time. The data mainly cover

[9] Researcher(s) own analyses calculated (or derived) based in part on data from The Nielsen Company (U.S.), LLC and marketing databases provided through the Nielsen Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business. The conclusions drawn from the Nielsen data are those of the researcher(s) and do not reflect the views of Nielsen. Nielsen is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein.

consumer packaged goods (CPG), such as food, nonfood grocery items, health and beauty aids, and general merchandise. According to Nielsen, the retail scanner covers more than half the total sales volume of US grocery and drug stores and more than 30 percent of all US mass merchandiser sales volume.

There are two notable advantages to using the ACNielsen Retail Scanner database when studying multimarket firm behavior. First, the database records product information at the barcode level, likely to be the most granular scale at which a product can be defined. This feature allows us to identify the uniform product replacement channel, which only works at the barcode level. Appendix D.5 explicitly shows that when using a conventional definition of a product—the aggregation of the barcode-level products into "product group"—the spillover effect arises entirely from the continuing products, and none of this effect works through uniform product replacement. Second, the database has fewer measurement error problems. For example, unlike most firm-level international trade data that infer market (domestic) sales by subtracting another market (international) sales from total firm sales, Nielsen collects sales information independently in each market. This feature prevents the mechanical regional sales correlation problem raised in Berman et al. (2015). Compared to similar data on consumer surveys, the Retail Scanner data directly records expenditures when consumers purchase and scan products at stores. Thus, our data suffer less from households' nonreporting or misreporting issues, which are common problems in survey data used in economic research (Einav et al. 2010; Meyer et al. 2015). One concern about the scanner data is that the products available on shelves but not purchased by any consumer may not be recorded. Although this could introduce some measurement errors in product creation and destruction, the existing literature using the scanner dataset documents that product creation and destruction are not driven by goods drifting in and out of the sample (see, e.g., Broda and Weinstein, 2010).[10] Another disadvantage specific to the Retail Scanner data is the sample selection since it likely covers more large retailers. Appendix E.11 confirms the robustness of our main intrafirm spillover results by leveraging the Homescan Panel data, which rely on household reporting but have a sample weight that can make the sample

[10]Broda and Weinstein (2010) report that less than two percent of products reappear after being classified as destroyed. In terms of value, these products account for less than 0.2 percent of the sample.

nationally representative.

We integrate each product's prices and quantities with producer information using the GS1 US Data Hub and the NETS. The GS1, a not-for-profit information standards organization, is the official source of barcodes for producers.[11] Their data record the company name and headquarters address for each barcode-level product. We use this information to link barcode-level product information to producer-level information from the NETS data. NETS is the US establishment-level longitudinal database made available by Walls & Associates. The data source is Dun and Bradstreet (D&B) archival data, which are collected primarily for marketing and credit scoring. The data allow us to identify each firm's establishment location, primary industry code defined at the SIC 4-digit level, and D&B credit and payment rating during the 1990-2014 period. We use this information to compare firms that operate in the same primary industry, analyze heterogeneous treatment effects, and address concerns related to the supply-side effect or collateral channel.[12] Note that our sample excludes the participating retailers in the Retailer Scanner data, as their UPCs are masked for confidentiality and cannot be combined with the GS1 data. Our baseline definition of a firm is based on the GS1 data, but using an alternative definition based on NETS data generates the same results. See Online Appendix C for further discussion of each dataset and the merging procedure.

We supplement the combined database with the county-level house price index and other regional housing price indexes from the Zillow and Federal Housing Finance Agency (FHFA) data. We also supplement the housing supply elasticity measure from Saiz (2010), the housing price sensitivity measure from Guren et al. (2021), and the mortgage lending information from García (2018) for our identification strategy. We further augment our data with the Rajan and Zingales (1998) industry-level external financial dependence index to explore the role played by financial frictions in spillovers. For the robustness check, we additionally use the NBER county distance

[11]GS1 provides a business with up to 10 barcodes for a $250 initial membership fee and a $50 annual fee. Firms that purchase larger quantities of barcodes enjoy significant discounts on the cost per barcode (see http://www.gs1us.org/get-started/im-new-to-gs1-us). See, Kim (2020b), for details.

[12]See, e.g., Neumark et al. (2011), Barnatchez et al. (2017), Rossi-Hansberg et al. (2018), and Asquith et al. (2019) for a more detailed discussion of the NETS data. According to Barnatchez et al. (2017), the NETS database is useful for studying cross-sectional business activities, but its information is limited for studying business dynamics. Thus, we only use the data for the prerecession period and abstain from using the data's panel structure.

database, NBER commodity flow survey database, and the Census county-level variables used in Mian and Sufi (2014), such as household population, income, and the debt-to-income ratio.

The final sample used in the main empirical analyses consists of 4,171 multimarket firms and covers 991 US counties from 2007 to 2009. As discussed in Appendix C.1, the final combined sample covers approximately 40% of all sales in the Nielsen data. The 991 US counties cover approximately 75% of the total US population. Appendix E.12 shows that the spillover results are robust to using all ACNielsen and GS1 combined data or including local, single-market firms in the analyses.

2.2 Stylized Facts about Multimarket Firms

Using the unique Nielsen-NETS combined data, this section documents four stylized facts about multimarket firms. These facts suggest that multimarket firms likely replace products uniformly across many markets, and such behavior is anticipated to be important in understanding regional household consumption.

To study multimarket firms, we first define the county as the primary definition of the market throughout our analyses, following the previous studies of the CPG sector (Hanner et al. 2015; Hottman 2021). Since consumers may travel across zip codes but do not likely travel across counties to purchase grocery goods, the county is a reasonable measure of a market for this sector. As a robustness exercise, we define a market by using alternative geographic codes available in the Retail Scanner data (3-digit zip code and state) in Appendix E.3. In using Homescan Panel data in Appendix E.11, we additionally use alternative geographic codes, including scantrack code, 5-digit zip code, and Census Division, as an alternative definition of the market. We also used product group (sector) x county as a market in Appendix E.6.[13] The spillover results are generally robust to these larger or smaller market definitions.

Given the market definition, the combined data reveal the first fact: Multimarket firms are

[13]We abstract away from product group in the main draft because a typical firm does not sell many product groups, as discussed in explaining the fourth stylized fact with Table 1.

prevalent and dominant in the US economy, showing the importance of studying their behavior. Figure 1 categorizes firms into five groups based on the number of markets (counties) they serve, aggregates the number (or sales) of firms in each group, and plots the histogram by each group. We find that more than 90% of firms sell to multiple markets, and these firms have more than 99.99% of the sales share. Specifically, most of the product market activities (more than 95% of the total sales) are attributed to approximately 25% of firms that sell to more than 500 markets. The pronounced nature of the multimarket firms echoes the international evidence on multimarket firms (exporters) reported in Bernard et al. (2007), who document that firms that export to more than one market (country) account for approximately 96.7% of total exports. The prevalence and dominance of multimarket firms are robust to using the state as an alternative definition of a market or using a broader ACNielsen sample, as shown in Appendix C.3.

The second fact from the data is that multimarket firms sell to many markets in general, highlighting the potentially important role of intrafirm spillover. The mean number of markets is 670, and the median number of markets is 155. Assuming that firms do not make product supply decisions entirely at the local level, economic conditions in other markets may substantially affect local sales because they arise from 669 markets on average. In contrast, the local market effect works through a single market.

The third feature of the data suggests the uniform product replacement channel: Firms do not tailor their product to each market. Panel A shows that the number of markets they serve exceeds the number of products they produce. For example, the median firm sells in 155 counties and 21 states, but it only makes 12 products in 2 product groups to serve all these markets.[14] Panel B shows this fact more precisely by using the UPC-level data and counting the number of markets per UPC. On average, each product is sold in 226 counties in 16 different states, suggesting that firms do not target each product in each market but instead provide the same product across multiple markets. For example, the Kraft company provided organic, shredded, low-moisture, part-skim mozzarella

[14]Comparing the median number of markets with the median number of plants, we observe a similar but even more extreme empirical pattern. The median firm serves 155 counties and 21 states from a single plant. Note that our sample excludes retailers that are likely to serve more counties per establishment.

cheese in 14 states and 119 counties in 2007 instead of making 119 products that serve 119 counties separately. A simple general equilibrium model in Appendix F rationalizes this empirical pattern by allowing the costs of developing and penetrating different products into different markets, which dominate the associated revenue gains.

Lastly, most of the firms in the data have a small number of plants and product groups.[15] The median firm has a single plant, and even the firms at the 90th percentile only have 13 plants. The fact that these firms own a small number of plants implies that their plant locations are concentrated in a small number of counties, helping us address potentially confounding multiplant network channels documented in previous studies. In Appendix E.7, we redo and confirm the primary analyses by excluding a small number of counties where firms have their plants. This robustness analysis rules out supply-side confounding factors associated with the decline in firms' plant value and local sales arising from the collapse of housing and land prices. Additionally, given the relatively small variation in product groups within firms, we abstract away from the product group dimension in our primary analysis and separately confirm the results by allowing the group dimension and the associated fixed effect in Appendix E.6.

Two other facts are worth mentioning. First, each county has many firms. On average, 848 firms sell their products in a county, and even for a county in the 10th percentile, 341 firms sell products. In terms of sales, the largest firm in a median county has a sales share smaller than 5% in the total Nielsen sample. Since each firm accounts for a small number and sales share in a county, it is unlikely that an individual firm could affect the local economic conditions, corroborating the validity of the indirect shock. Second, as documented in Hottman et al. (2016), there is extreme firm heterogeneity in the data. A firm in the 90th percentile of the distribution has approximately 3000 times more sales, produces about 55 times more products, and sells in approximately 160 times more counties than a firm in the 10th percentile of the distribution. We exploit this rich variation and confirm that the spillover effect is more substantial for larger firms, consistent with the previous

[15] Although our sample focuses on the CPG sector in the NETS data, the mean and standard deviation of the number of plants are comparable to those reported from the Census data in Giroud and Mueller (2019), who documents that the mean is 15.4 and the standard deviation is 132.

literature.

2.3 Measurement of Variables in Regression Analyses

Sales Growth and Decomposition. Our main dependent variable is county-firm sales growth. Let $\mathrm{S}_{cf,t}$ denote firm f's sales in county c at time t. We measure the county-firm-specific sales growth in 2007-2009 as

$$\tilde{\Delta}\mathrm{S}_{cf} \equiv \frac{\mathrm{S}_{cf,09} - \mathrm{S}_{cf,07}}{\overline{\mathrm{S}}_{cf}} \tag{2.1}$$

where $\overline{\mathrm{S}}_{cf} \equiv \frac{1}{2}(\mathrm{S}_{cf,07} + \mathrm{S}_{cf,09})$ is the simple average sales of firm f in county c in 2007 and 2009. This growth rate, which is a second-order approximation of the log difference growth rate around 0, follows previous papers that measure employment growth at the establishment level (e.g., Davis et al. 1996). This definition of the growth rate provides a symmetric measure around 0 and is bounded between -2 and 2. These features help limit the influence of outliers without arbitrarily winsorizing extreme observations. This growth rate can accommodate both the entry and exit of firms at the county level, and the main results are robust to such accommodations. Additionally, the qualitative results are robust to using the more conventional definition of sales growth in which the denominator equals 2007 sales. See Appendix E.14 for these analyses.

To understand the underlying mechanism behind the regional shock spillover, we follow Broda and Weinstein (2010) and exactly decompose the sales growth in Equation (2.1) into two margins: the intensive margin associated with products that are continuously offered in both the pre- and post-shock periods, and the extensive margin associated with product creation and destruction (i.e., net creation):

$$\tilde{\Delta}\mathrm{S}_{cf} = \tilde{\Delta}\mathrm{S}^{\mathrm{C}}_{cf} + \tilde{\Delta}\mathrm{S}^{\mathrm{R}}_{cf} \tag{2.2}$$

where $\tilde{\Delta}\mathrm{S}^{\mathrm{C}}_{cf} \equiv \frac{\mathrm{S}^{\mathrm{cont}}_{cf,09} - \mathrm{S}^{\mathrm{cont}}_{cf,07}}{\overline{\mathrm{S}}_{cf}}$ and $\tilde{\Delta}\mathrm{S}^{\mathrm{R}}_{cf} \equiv \frac{\mathrm{S}^{\mathrm{enter}}_{cf,09} - \mathrm{S}^{\mathrm{exit}}_{cf,07}}{\overline{\mathrm{S}}_{cf}}$. $\mathrm{S}^{\mathrm{cont}}_{cf,t}$ is the county-firm-time-specific sales from products that continuously offered in county c throughout the years 2007-2009, $\mathrm{S}^{\mathrm{exit}}_{cf,07}$ is the sales from products that sold in county c in 2007 but exited in 2009, and $\mathrm{S}^{\mathrm{enter}}_{cf,09}$ is the sales from products not offered in county c in 2007 but entered in 2009. Note that we use the following identity for the decomposition of sales growth: $S_{cf,07} = S^{cont}_{cf,07} + S^{exit}_{cf,07}$ and $S_{cf,09} = S^{cont}_{cf,09} + S^{enter}_{cf,09}$.

We further decompose the extensive margin into two subcomponents by classifying products as firms' global and local products to explore the uniform product replacement channel:

$$\tilde{\Delta}S^{R}_{cf} = \tilde{\Delta}S^{R,M}_{cf} + \tilde{\Delta}S^{R,L}_{cf} \tag{2.3}$$

where $\tilde{\Delta}S^{R,M}_{cf}$ and $\tilde{\Delta}S^{R,L}_{cf}$ are the sales growth measures originating from the products that are replaced in multiple markets and the local market, respectively. After establishing the positive spillover across counties within firms, we leverage the exactly decomposed margins of sales growth to quantify the source of the intrafirm regional spillover effect.

The Indirect Shock. Our main goal is to investigate whether a firm's local sales growth is affected by indirect shocks originating in the firm's other markets, conditional on the direct local change in economic condition. To this end, we define the county-firm-specific indirect shock as the average regional demand changes that a firm faces from its other markets, weighted by its initial sales share. This shift-share method is analogous to the specification proposed by Giroud and Mueller (2019) in studying the labor demand of multiplant firms in non-tradable sectors. Nonetheless, the identifying variation we describe below differs from the study of multiplant firms.

In measuring local consumer demand changes, we exploit the sharp differential decline in local house prices during the Great Recession following the large literature studying the consequences of housing market disruptions.[16] It is well known that there was a dramatic decline in housing prices in this period following the massive increase in housing prices in previous years, and the magnitude of the decline in housing prices varied widely across counties. The counties that faced a larger decrease in local housing prices experienced a greater decline in local consumption relative to their counterparts because of the lower household wealth or worsened credit conditions arising

[16]Our measure of house price changes using the Zillow data follows Giroud and Mueller (2017, 2019); Kaplan et al. (2020a). Giroud and Mueller (2017) document that at the MSA level, this measure is highly correlated with both the "housing net worth shock" used in Mian et al. (2013); Mian and Sufi (2014) (86.3%) and the measure of house price changes from the FHFA data used in Adelino et al. (2015); Charles et al. (2018) (96.4%).

from depressed collateral (housing) values (see, e.g., Mian et al. 2013; Kaplan et al. 2020b). The resulting depressed local consumption, in turn, lowers wages and employment in the county and further reduces local consumption (the local general equilibrium effect; see, e.g., Guren et al. 2020).

Let $\text{HP}_{c,t}$ denote the house price index in county c at time t. Consistent with the measure of sales growth, we define the county-specific house price growth in 2007-2009 as

$$\tilde{\Delta}\text{HP}_c \equiv \frac{\text{HP}_{c,09} - \text{HP}_{c,07}}{\overline{\text{HP}}_c} \tag{2.4}$$

where $\overline{\text{HP}}_c$ is a simple average of the housing price index values in county c in 2007 and 2009. We use $\overline{\text{HP}}_c$ as a denominator in defining housing price growth for our primary analyses to be consistent with the construction of the dependent variable, but Appendix E.14 shows that using conventional growth rate with the initial housing price as a denominator does not change the main results. We choose the initial period of housing price growth as 2007 for the main analyses to avoid a potentially confounding effect of housing price growth on the initial period of local firm sales growth. However, the results are robust to using a more conventional 2006-2009 housing price growth that exploits the entire period of housing market disruptions, similar to that of Giroud and Mueller (2019). Using alternative measures, such as housing net worth growth as in Mian et al. (2013); Mian and Sufi (2014), generates similar results. See Appendix E.15 for the analyses that consider these alternative measures of the local consumer demand changes.

Given the county-specific house price growth, the main independent variable is constructed in the following way. We take the weighted average of this local house price growth across all counties c' within a firm f, excluding the particular county c, so that we can interpret this measure as an indirect shock for county c:

$$\tilde{\Delta}\text{HP}_{cf}\ (\text{other}) \equiv \sum_{c' \neq c} \omega_{c'f} \times \tilde{\Delta}\text{HP}_{c'} \tag{2.5}$$

where $\omega_{c'f}$ is the initial sales share defined as $\frac{\text{Sale}_{c'f,07}}{\sum_{c' \neq c} \text{Sale}_{c'f,07}}$. The weight $\omega_{c'f}$ is firm f's initial sales share in county c', where shares are measured excluding county c. The weight measures the importance of each county to a firm, reflecting the idea that firms are more likely to be exposed to a consumer demand changes in county c' if they initially sold more in county c' than in other counties. Appendix E.14 shows that the main results are robust to using 2007-2009 averaged local sales share as a weight.

Figure 2 illustrates how the measure in Equation (2.5) is used to identify the indirect demand effect by plotting the 2007 sales share (ω_{cf}) across counties for two specific firms. Consider a particular local market, Greenville, South Carolina, and a firm, a Swiss American Company. Figure 2a shows that this firm sold its products not only in Greenville but also numerous other markets, and its sales were concentrated in the central states, where housing prices were stable in 2007-2009: its three largest markets were St. Louis in Missouri, Shelby County in Tennessee, and Smith County in Texas. As a result, this firm did not confront a large decrease in its product demand originating from the housing market collapse. In particular, the magnitude of the indirect shock it faced in Greenville was small. The indirect shock in Greenville is much larger for Arla Foods Incorporation, a comparable firm that sold the same major product group (cheese) and had similar overall sales. Although Arla Food had many overlapping markets with Swiss American, including Greenville, it earned much of its revenues from the western US, such as Maricopa County in Arizona and Los Angeles in California. Due to the dramatic decline in housing prices in these areas, Arla Foods suffered much more from consumer demand changes than Swiss American and faced an approximately twice as large negative indirect shock in Greenville. Such variation allows us to identify the indirect demand effect. As in this Greenville example, the main analyses allow the market fixed effects to compare across firms within a given market instead of comparing these firms across different markets.[17]

[17] This example also raises concerns. Although we compare two comparable firms with similar characteristics, such as major product group, sector code, and total initial sales, these two firms also have different characteristics. For example, Figure 2 shows that Arla Foods sells in a larger number of markets than Swiss American. Moreover, it highlights that these two firms concentrate on different markets, which may be correlated with other confounding characteristics and violate the identification assumption. Appendices E.10 and E.9 address such concerns by testing the selection of these characteristics and controlling for such characteristics in the analyses. Specific to this example, we compare firms that sold a similar number of counties and the same major market. Additionally, the rich micro-level data allow us to test the selection of unobserved firm characteristics concerning local housing price changes. As shown in Table 2, we do not find evidence of selection on potential unobserved characteristics.

One primary concern in treating the local housing price as an exogenous independent variable is that a price is an equilibrium object jointly determined by housing demand and supply and associated with various confounding county characteristics. Detailed county-firm data focusing on the intrafirm spillover effects ease this concern. Our tightest specification allows county-sector fixed effects to absorb all county and county-sector-specific characteristics potentially correlated with regional housing price changes. This specification, which compares firms within counties, aids the identification because the county-level housing prices are likely to be exogenous to our sample of firms. The narrative evidence and previous studies suggest that the regional housing price changes in this period originated from factors outside of product markets, especially the CPG market that this study considers. The two leading explanations for the decline in housing prices are the household credit expansion in the prerecession period (e.g., Mian and Sufi 2017) and a sudden change in household expectations (e.g., Adelino et al. 2018). We are not aware of any evidence that firms, particularly CPG firms, affect local housing prices by selling more or fewer products. Moreover, as discussed in Section 2, the firms' share in our sample is small in a county, and each firm would likely play no role in affecting local housing price changes.

Given that our sample of firms takes housing prices as given, another concern is that firms with specific characteristics may have initially chosen to serve counties that experienced disproportionately larger declines in house prices during the Great Recession. However, given the high persistence of regional market share in the CPG industry (see, e.g., Bronnenberg et al. (2009, 2012)), it is unlikely that firms selected into counties for reasons related to housing price changes during the Great Recession. One of our placebo tests reveals that the effect is muted when we only consider those firms entering the local market of interest at the beginning of the recession. We also test this possibility of selection by correlating the firms' initial observed characteristics with the average degree of housing price changes they face. As shown in Appendix E.10, we find that the firm size, scope, age, and financial stability, which are likely to affect firm local sales, are uncorrelated with the average regional shocks firms face in our sample. Appendix E.9 controls for these variables as well as other observed characteristics, such as the consumer demographics and the major market fixed effects, in the regression analyses and confirms the spillover results. Moreover, by utilizing the county and firm fixed effects in our regression specification, we test and confirm that firms are unlikely to initially select counties based on their unobserved characteristics.

In addition to the local change in housing prices, we add three other measures as IVs to confirm the findings: housing supply elasticity (Saiz 2010), housing price sensitivity (Guren et al. 2021), and nonlocal mortgage lending shock (García 2018). The housing supply elasticity measures the degree of difficulty in building new houses for a metropolitan area by exploiting the variation in the land's topology. The housing price sensitivity infers the housing supply elasticity by exploiting the systematic differences in the sensitivity of local house prices to broader regional house price variation, in which a larger estimate corresponds to a less elastic housing supply. Finally, the nonlocal mortgage lending shock exploits variation in changes to mortgage lender health—originating *outside* the county of interest—to generate exogenous variation in house prices in the local economy. With these measures at hand, we construct the corresponding leave-one-out weighted average IVs for the indirect shock following Equation (2.5). To ease potential concerns related to the initial share, we use a one-year-lagged weight for all of the instruments, similar to the construction of the instruments in Autor et al. (2013). Appendix D.1 reports the first-stage results. All three IVs—housing supply elasticity, housing price sensitivity, and nonlocal mortgage lending shock—are highly correlated with the main indirect shock.

Appendix C.2 presents detailed summary statistics and describes the sample distributions of the key variables.

3 The Spillover Results

We use the following equation as the main specification throughout the empirical analyses:

$$\tilde{\Delta}\mathrm{S}_{cf} = \beta_0 + \beta_1 \tilde{\Delta}\mathrm{HP}_c + \beta_2 \tilde{\Delta}\mathrm{HP}_{cf}\ (\text{other}) + \boldsymbol{X'_{cf}\beta_3} + \varepsilon_{cf} \tag{3.1}$$

where $\tilde{\Delta}\mathrm{S}_{cf}$ is county-firm level sales growth, and $\tilde{\Delta}\mathrm{HP}_c$ and $\tilde{\Delta}\mathrm{HP}_{cf}$ (other) are the local consumer shock and the indirect shock defined in Section 2.3, respectively. $\boldsymbol{X}'_{cf}$ is a vector of control variables. Most of our main empirical analyses adapt variants of Equation (3.1) to simply and coherently present our analyses. For the main reduced-form empirical results, we additionally consider tighter specifications that include county or county $\times$ sector fixed effects that absorb all county-level variation, including $\tilde{\Delta}\text{HP}_c$. This tighter specification allows us to compare local sales of firms *within* each market. Appendices 3.1 and E.6 also show that the results are robust to allowing more granular product group dimension or retail dimension with the associated fixed effects, which designs compare firms within product groups or retailers. All the regression analyses are weighted by initial county-firm sales, and standard errors are two-way clustered by state and 2-digit SIC sector. Given the shift-share structure of our main independent variable, Appendix E.16 considers alternative standard errors that account for the correlated errors arising from the shift-share structure following Adao et al. (2019).

Our central coefficient of interest is β_2. This coefficient measures a firm's local sales growth elasticity to the average regional shocks from the firm's other markets, conditional on the direct local shock. A priori, β_2 can have any sign; if the adverse regional shocks in other counties decrease (increase) the firm's local sales, then the sign of β_2 is positive (negative). β_2 is zero if firms make their decisions at the local level. On the other hand, β_1 measures the effect of direct regional housing price growth on a firm's local sales, which is similar to what is studied in Mian et al. (2013); Kaplan et al. (2020a). Our empirical analyses focus on the differential decline in housing prices by absorbing any nationwide changes, such as the aggregate decline in housing prices or the aggregate productivity changes, with the intercept β_0.

Figure 3 visualizes the direct and indirect spillover effects of regional demand changes by drawing binscatter plots based on Equation (3.1). As shown in Figure 3a, a firm's local sales growth decreases with respect to the decrease in local housing prices, confirming the results in Mian et al. (2013) and Kaplan et al. (2020a) at the county-firm level. Figure 3b plots our main intrafirm spillover results. We find that firms reduce their local sales when they face a negative shock originating from the other markets ($\beta_2 > 0$). Note that the local housing price growth dispersion is higher than that of the indirect shock, which averages across the local housing price changes within the firm. Appendix E.13 shows that the results are generally robust to excluding outliers.

Strikingly, Figure 3 reveals that the indirect demand elasticity arising from the intrafirm network on local firm sales is much larger than the direct demand elasticity; the slope of the linear line in Figure 3b is much steeper than the slope in Figure 3a. The relative importance of the indirect effect is intuitive because of the large number of markets a typical firm serves and, correspondingly, the relatively small sales share such firms initially earn from each market. For example, the median firm in our sample sells in 155 counties (Table 1), and the median initial sales share is 0.058% (Table OA.2). Given that local firm sales growth depends on the overall firm-level decisions, it is plausible that the effect of the demand changes originating from the other 154 markets, which account for more than 99.9% of the initial sales share, is larger than that of a negligible single market.

Table 2 column (1) confirms the positive and statistically significant direct and indirect effects of regional shocks on local consumption and that the indirect demand effect is stronger.[18] The direct and indirect effects are 0.06 and 0.35, respectively: A 10 percentage point decline in local house price growth leads to a 0.6 percentage point decrease in local firm sales growth, and the same decrease in the average of the other regional housing price growth reduces local firm sales growth by 3.5 percentage points.[19]

Note that this comparison between the direct and indirect effect is based on the *sensitivity* (or elasticity) of local sales growth with respect to regional demand changes and does not integrate the differences in the *magnitude* of the direct and indirect shocks. To account for this, we consider three alternative specifications, all of which confirm that the indirect effect is at least as large as the direct effect. First, we compare firms that face extreme degrees of demand changes by using the 95-5 percentile range, as shown in the bottom rows of Table 2. Although the overall direct

[18]Note that Table 2 does not include the variables from the NETS data, such as the measures of financial constraints and firm age, because they decrease the sample size. The effects of direct and indirect shocks are robust to including such controls, as shown in Appendix E.9.

[19]The magnitude of the estimated coefficient is smaller than the estimates reported in previous studies. For example, using the Nielsen Retail Scanner data, Kaplan et al. (2020a) show that the estimate is 0.207 at the county level, 0.239 at the MSA level, and 0.341 at the CBSA level. One key difference is the measure of the local shock. If we use household net worth instead of housing prices as in Kaplan et al. (2020a), our estimate of the local demand effect becomes 0.22. Appendix E.15 shows that the spillover results are robust to using this alternative measure of a shock.

demand effect becomes much larger (0.027) due to large variations in local housing prices, it is still smaller than the overall indirect demand effect (0.030). Second, we standardize the direct and indirect demand changes such that both estimated coefficients can be interpreted as the effect of a one-standard-deviation increase in the independent variables. A one-standard-deviation increase in the direct and indirect demand changes raises local firm sales by 0.008 and 0.014 percentage points, respectively. Lastly, we redefine the demand changes based on the indicator variables, which equal one if the demand change is larger than its median value and 0 otherwise.[20] Based on this specification, firms that face negative indirect demand changes lower their sales growth by twice as much as firms that face negative direct demand changes.[21] See Appendix E.15 for the last two exercises.

Columns (2)-(4) verify our empirical findings using the various fixed effects to test and exclude unobserved confounding factors, and columns (5)-(8) exploit the IVs. Column (2) includes the firm fixed effects instead of the firm-level variables and the indirect shock, the variation of which primarily arises from the comparison across firms. The local shock's quantitative effect remains the same with and without the firm fixed effects, suggesting no selection on unobserved firm characteristics into the local housing price changes conditional on control variables. If unobserved firm characteristics made some firms more exposed to the local shock, adding the firm fixed effects would correct the bias and change the coefficient.[22] Although the direct demand effect is not our primary interest, this validation exercise ensures the credibility of the indirect shock, which heavily relies on the variation in the local housing price changes. The indirect effect is stable across columns (1) and (3), similar to the direct effect reported in columns (1) and (2), suggesting that there is no selection on unobserved county characteristics in the indirect shock. Column (4) includes county times sector fixed effects that absorb all county-sector variation that might confound the results, such as the regional clustering of manufacturing sectors that could comove with the differential manufacturing sales across locations. The indirect intrafirm spillover effect remains strong in this

[20]The results are similar if we define the indicator variables based on the tercile or quartile.

[21]Note that, for both specifications, we cannot statistically reject the null hypothesis that the two effects are identical.

[22]This analysis is similar to the test of unobserved characteristics implemented in the banking literature. See, e.g., Khwaja and Mian (2008); Chodorow-Reich (2014).

specification. Columns (5)-(8) present the IV results and confirm the positive intrafirm spillover effects. As in previous literature studying the direct local effects, the IV estimates are larger than the OLS estimates, presumably due to the classical measurement errors that attenuate the coefficient size.[23] The first-stage F statistics are well above 10 in all specifications, and Hansen's J-statistics cannot reject the instruments' validity.

3.1 Retail Margin

A notable concern in our study of multimarket firm behavior is that sales information is recorded at the retail level, whereas our work focuses on nonretailers, primarily manufacturers in the economy. Although our main analyses aggregate retail dimensions within multimarket firms and compare the total sales generated by these firms across retailers, there may be confounding factors associated with retail characteristics that vary across the aggregated sample. For example, if large retailers contract with large manufacturers, retail size may appear as an omitted variable in the regression. As another example, the seminal paper by DellaVigna and Gentzkow (2019) shows that retailers choose a uniform price across regions. In this case, retailers may spill over regional shocks by choosing a uniform price across markets, and this within-retail network may confound the multimarket firm network in our regression analyses.

Given the concerns regarding retail characteristics and behavior, we explicitly include the retail dimension in the data and conduct exercises separately. Specifically, we investigate the role of within-retailer spillovers by explicitly measuring the indirect demand shock the retailers face, similar to Equation (2.5): $\tilde{\Delta}\text{HP}_{cr}$ (other) $\equiv \sum_{c' \neq c} \omega_{c'r} \times \Delta\text{HP}_{c'}$, where the subscript r denotes a retailer and $\omega_{c'r} \equiv \frac{\text{Sale}_{c'r,07}}{\sum_{c' \neq c} \text{Sale}_{c'r,07}}$. We include this measure in addition to the indirect demand shock faced by firms (producers) and study how the county-retail-firm-level sales growth responds to both shocks. We use the following regression specification that incorporates the indirect demand shock

[23] The sample size does not make a substantial difference in the magnitude of the coefficients. The estimates with the elasticity and sensitivity instruments are larger than the estimate with the lending instrument. One explanation for this difference is how different instruments affect different subsamples based on the underlying channel. The elasticity and sensitivity instruments are likely to contain all the effects of housing prices on local consumption discussed in previous studies. In contrast, the lending instrument would affect households only through household mortgage borrowing.

for retailers, which is an extension of Equation (3.1):

$$\tilde{\Delta}\mathrm{S}_{crf} = \gamma_0 + \gamma_1 \tilde{\Delta}\mathrm{HP}_c + \gamma_2 \tilde{\Delta}\mathrm{HP}_{cf}\ (\text{other}) + \gamma_3 \tilde{\Delta}\mathrm{HP}_{cr}\ (\text{other}) + \boldsymbol{X'_{crf}\gamma_4} + \varepsilon_{crf} \tag{3.2}$$

where $\tilde{\Delta}\mathrm{S}_{crf}$ is county-retail-firm-specific sales growth, and $\boldsymbol{X'_{crf}}$ is a set of county-retail-firm-specific control variables.

Table 3 columns (1)-(3) show the results. The indirect demand shocks for both multimarket firms and retailers separately generate the positive regional spillover, but the underlying mechanism is different. The multimarket firm spillover effect still entirely works through product replacement, but the retail spillover effect mainly works through continuing products. The importance of continuing products for retailers is fully consistent with the uniform pricing behavior documented in DellaVigna and Gentzkow (2019) and confirms the multiplant firm results documented in previous studies, such as Cravino and Levchenko (2017) and Garcia-Lembergman (2022) in our sample. Columns (4)-(6) consider the county $\times$ retailer fixed effect that absorbs all the county-retailer-specific variations. The estimated coefficients are stables across different specifications, showing the retail margin is not likely to confound the results.

Moreover, we additionally include the county-firm-retailer-level indirect demand shock, which is intended to capture the spillover through firm-retailer interaction. The county-firm-retailer indirect demand shock is measured as $\tilde{\Delta}\mathrm{HP}_{crf}\ (\text{other}) \equiv \sum_{c' \neq c} \omega_{c'rf} \times \Delta\mathrm{HP}_{c'}$, where $\omega_{c'rf} \equiv \frac{\mathrm{Sale}_{c'rf,07}}{\sum_{c' \neq c} \mathrm{Sale}_{c'rf,07}}$. This county-firm-retailer-level indirect demand shock measures the average housing market shock retailer r and firm f face from counties other than c. This shock captures the effect of the interaction of retailers and firms (producers) on their sales, given the retailer $\times$ county fixed effect. For example, consider Coke and Pepsi, which both sell to Whole Foods. Suppose that Coke generates smaller sales by losing shelf space to Pepsi in Whole Foods due to the negative indirect demand shock. In that case, given the retailer $\times$ county fixed effect, the county-firm-retailer-level indirect demand shock must reduce county-firm-retailer-level sales. As shown in Table 3 columns (7)-(9), including this county-firm-retailer-level indirect demand shock neither generates a meaningful effect nor confounds the main multimarket firm spillover effect. These results support the view that retailer behavior does not invalidate the spillover effect through multimarket firms.[24]

Appendix E.1 further evaluates the relative importance of retailers compared to producers in explaining local sales growth in our sample by (i) investigating the importance of retail variation in our sample and (ii) bringing in new data (Promodata) that directly record producer price information. In understanding the underlying variation, we regress county-retail-firm-level sales growth on either retail or firm fixed effects for each county and plot the associated R^2 across counties and find that the median R^2 associated with the firm (producer) is .63 while that associated with the retailer fixed effect is .07. This further confirms the importance of firms (producers) in explaining local sales dynamics. Second, using the producer price information, we still find empirical support for the main spillover results and the underlying uniform product replacement mechanism.

3.2 Additional Robustness Exercises

Among other concerns in identifying the spillover effects, one of the greatest threats is a clustered shock, which is often discussed in the international macroeconomics literature with more aggregate data (see, e.g., Kose et al. 2003). Although we already rule out any common shocks to the national, sector, or county-sector clusters, other geographically clustered shocks may exist within broader markets, such as spatially correlated housing price changes. Such shocks could correlate with the indirect shocks and confound the results if the intrafirm initial networks are also clustered in the same areas. Appendix E.2 presents various robustness exercises regarding the nearby markets and rules out the possibility of a confounding effect of clustered shocks. For example, we exclude all counties near the local county of interest in measuring the indirect shocks.[25] Appendix E.3 defines

[24]The interactions between manufacturers and retailers, including assortment and shelf space decisions, are complex and an interesting topic for future research. While our data do not allow us to fully explore these relationships, it is important to note that such decisions are typically governed by contracts that are renegotiated on a quarterly or semi-annual frequency. This periodic renegotiation suggests that real-time adjustments are limited in practice. Therefore, the frictions inherent in these relationships likely lead firms to respond more slowly to demand shocks. Our analysis, which focuses on a medium-term effect capturing the long difference between 2007 and 2009, should thus be interpreted with caution, as our estimates may underestimate the true effect in a friction-free environment.

[25]We consider radii of 50, 100, 150, and 1000 miles in measuring the indirect shocks that exclude nearby counties. We also calculate it by excluding all counties located within the same state of the county of interest.

a state and 3-digit zipcode as a geographic unit of analysis to exclude variation within the state or 3-digit zip code, which could be more vulnerable to clustered shocks. Moreover, to make the spatial spillover effect clear, we conducted additional exercises using only counties facing the negative housing price changes and only considering West Pacific Regions as shocked counties and how these counties influence other unaffected counties through multimarket firms (Appendix E.5). The indirect spillover effects remain strong regardless of excluding nearby markets, using a broader definition of markets, or considering particular counties as a source of the shocks.

More generally, the identification requires the only source of the estimated spillover effect to be the multimarket firm's intrafirm network. Appendix E.4 tests and confirms the validity of this requirement by considering other placebo networks that can potentially generate similar local sales correlations within firms. Specifically, we replace the initial local sales share, which represents the multimarket network, in Equation (2.5) with shares that capture other networks, such as household demographics, firm entry, and multiplant network. We also consider an equal weight across markets within each firm, a crude measure of the multimarket networks. Based on these alternative networks, the indirect effect on local firm sales growth is not significantly different from zero at conventional level of statistical significance. These results suggest that the spillover effect is muted unless one utilizes the precise measure of the multimarket firm network.

Appendix E considers numerous other robustness exercises. We address concerns related to the interindustry correlation by including the product group dimension and the associated product group fixed effect, as shown in Appendix E.6. This exercise also rules out a potential confounding effect that arises from the product market competition across firms in the nested CES demand system. The spillover results are robust to other concerns, such as supply-side shocks (Appendix E.7), international and intranational trade linkages (Appendix E.8), potentially different firm age, financial constraints, major markets, and customer demographics (Appendix E.9), the selection of firms into the indirect shock (Appendix E.10), different samples (Appendices E.11, E.12, and E.13), different measures of the dependent and independent variables (Appendices E.14 and E.15), and alternative standard errors (Appendix E.16).

4 The Uniform Product Replacement Channel

The reduced-form empirical results do not align with the predictions of the traditional models of international macroeconomics and trade, where firms make their decisions entirely at the market level. Without granular data, the most straightforward extension of the traditional model to match the reduced-form empirical facts would allow increasing returns to scale at the firm level associated with the product quantity, such as financial frictions at the firm level.

We propose the uniform product replacement channel, which goes beyond a simple extension of a traditional model: The firms that face a negative shock replace their high-valued products with low-valued products, and they do so uniformly in multiple markets—including markets that have no regional shocks—and generate the regional spillovers. This channel is plausible based on previous studies on extensive margins and barcode level data. The previous literature on endogenous product replacement indicates that firms replace products and change the product value (or quality) over the business cycle.[26] And when they replace products, the summary statistics in Table 1 and Table OA.2 suggest that they would do so uniformly across many markets.

This section provides three strong empirical supports for the proposed uniform product replacement mechanism, in addition to what is shown in previous literature and Tables 1 and OA.2. First, by using the exact decomposition, we show that almost all the spillover effects arise from the uniform product replacement channel, providing direct evidence of this channel. Second, conditioning on the indirect shock, we find that the newly introduced products have lower value—sales per product, unit price, and organic sales share—than the discontinued products, confirming product value downgrading through product replacement. Third, we exploit the richness of the data and confirm that the spillover effect is stronger for firms or counties where the uniform product replacement channel is likely to be stronger. Lastly, we formalize the mechanism by streamlining key results from a simple model in Appendix F, providing additional support to the proposed mechanism.

[26] See, e.g., Bernard et al. (2010) and Broda and Weinstein (2010) for product entry and exit, and Jaravel (2019) and Argente and Lee (2021) for product quality changes.

4.1 The Exact Decomposition

To empirically investigate the underlying mechanism, we use a simple regression framework to exactly decompose the spillover effect into the uniform product replacement effect and other effects. As described in Section 2, we decompose local sales into two parts: the intensive and extensive margins. The intensive margin is entirely conventional and refers to the local firm's sales growth from continuing products, which are offered in both the initial and end periods. The extensive margin is the local firm's sales growth that arises from product creation and destruction in a given market. To distinguish between the uniform product replacement across multiple markets and the idiosyncratic product replacement associated with market conditions, we further decompose the extensive margins into two parts related to uniform and idiosyncratic replacement. We regress each margin on the indirect shock to understand how firms that face a negative indirect shock change their local sales.

Figure 4 visualizes the essentiality of uniform product replacement in understanding the intrafirm spillover effect. Figure 4a plots the entirely conventional price and quantity effect of the intrafirm spillover, which arises from continuing products. Surprisingly, there is a near-zero linear relationship between sales growth and the indirect shock. Depending on how one treats the observations subject to the extreme indirect shock, there is at most a *negative* relationship. The baseline analyses are agnostic on these extreme values by including them but weight each observation with initial sales, but Appendix E.13 shows that the results are robust to excluding these values. On the other hand, Figure 4b shows the intrafirm spillover effect through product replacement. The figure presents a strong positive relationship and closely replicates the overall spillover effect visualized in Figure 3b. In particular, further decomposing the extensive margins shows that all the effects arise from products that are replaced across multiple markets. As shown in Table OA.2, the sales growth due to these single-market products, which are replaced only in a single market, is negligible and cannot generate the spillover effect identified in the data.

Table 4 validates the visualization. Columns (1)-(3) show a stark asymmetry: The direct demand effect works through the conventional intensive margin of price and quantity change, consistent with the continuing product price results in Stroebel and Vavra (2019), but the indirect demand effect works through product replacement. Column (1) replicates Table 2 column (1), and columns (2) and (3) exactly decompose the coefficients in column (1) into the continuing product effect and the product replacement effect. Approximately 83% (0.05/0.06) of the direct demand effect arises from continuing products, and approximately 92% (0.32/0.35) of the indirect demand effect occurs due to product replacement. This asymmetry of the direct and indirect effects ensures that the identified intrafirm spillover effect is unlikely to be confounded by the factors related to local housing price changes, which affect local firm sales through continuing products.

Columns (4) and (5) report that the importance of product replacement remains robust after including county times sector fixed effects. When comparing the firms that sell in the same local market and operate in the same sector, we still find that the firms that face a larger negative indirect shock decrease their local sales by replacing their products relative to their counterparts. Note that the use of the barcode as the definition of a product is vital in identifying the product replacement mechanism. Appendix D.5 considers alternative definitions of products, which are comparable to the product definition used in other aggregated data. In this case, the spillover results are fully attributed to the continuing products, suggesting that barcode-level product replacement happens within a narrow product category.

Columns (6) and (7) show that the intrafirm spillover effect results from the firms' uniform product replacement decisions across multiple markets, not the firms' market-specific product replacement decisions. The coefficients reported in columns (6) and (7) further decompose the coefficient in column (5) by classifying products that are replaced in multiple markets and those replaced only in the local market. The local-market product replacement effect reported in column (7) is effectively zero, suggesting that the entire effect arises from products that are replaced in multiple markets simultaneously. This decomposition result is intuitive since almost all barcode-level products are sold in more than one market, and more than 80% of barcode-level products are replaced in multiple markets simultaneously. The uniform product replacement result rules out the possibility that firms replace their products in each market separately when they face the indirect shock, potentially due to other firm-level costs.

Columns (8)-(9) further verify that the intrafirm spillover effects work through uniform product replacement within firms. We rerun the decomposition analyses by using all three different IVs: the leave-one-out weighted average of the regional Saiz (2010) housing supply elasticity, Guren et al. (2021) housing price sensitivity, and García (2018) nonlocal mortgage lending shock. Given that the local product replacement effects are close to zero in all specifications, these columns decompose the total spillover effects into the continuing and uniform product replacement effects. The uniform product replacement effects are economically and statistically significant at conventional levels, but the continuing product effects are negligible.

4.2 Changes in Product Value, Characteristic, and Variety

Having established that firms generate regional spillovers by replacing products across multiple markets, we investigate the associated changes in product characteristics. We only use replaced products and analyze whether entering products differ from the exiting products, conditional on the indirect shock. This analysis is tightly linked with the spillover effects in Table 2 by using the same regression specification (3.1) except for the dependent variable, which measures the difference in product characteristics through product replacement. Specifically, we aggregate the barcode-level sales, prices, and organic identifiers by county, firm, and entered (or exited) products and calculate the difference between entered and exited products within county and firm as:

$$\tilde{\Delta} v_{cf} \equiv \frac{v_{cf,09}^{\text{enter}} - v_{cf,07}^{\text{exit}}}{\overline{v}_{cf}} \tag{4.1}$$

where $\overline{v}_{cf} \equiv \frac{1}{2}(v_{cf,07} + v_{cf,09})$. $v_{cf,09}$ is the county-firm-level measure of product values and characteristics and $v_{cf,09}^{\text{enter}}$ and $v_{cf,09}^{\text{exit}}$ measure $v_{cf,09}$ by using only those entered and exited products by county and firm, respectively. To investigate the role of product variety changes emphasized in important previous studies (e.g., Mayer et al. (2014, 2021)), we similarly define the change in the number of products by county and firm as $\tilde{\Delta}N_{cf}$.[27] Following Equation (3.1), we regress the change in product values, characteristics, and variety on the indirect shock to investigate how firms spill over the regional shock by replacing their products.

Table 5 shows that the products that are newly introduced in the local market by firms that face the negative indirect shock have lower values—sales per product, price, and organic share—than the products that are destroyed by the same firm in the same market. Columns (1)-(6) consider different measures of values and characteristics by county, firm, and product entry and exit. Conditioning on the negative indirect shock, Column (1) shows that the sales of newly introduced products are lower than those of destroyed products.[28] Columns (2)-(4) use different weights and methods to construct the county-firm-level price index. Regardless of whether we fix the weight, vary the weight, or adjust for the product group mean, new products have a lower price than discontinued products due to the negative indirect shock. Appendix E.6 confirms the effect on prices within the finite product group category by adjusting for a more granular product module mean and by adjusting for the size or package of the product. Columns (5) and (6) consider the sales share and number of organic products. The negative indirect shock makes firms introduce fewer organic products and generate fewer sales from these new organic products in the local market. Columns (7) and (8) consider the effect on variety, but there is no effect on the number of products within the US in this period, regardless of whether we use the full sample or the restricted sample of counties in which firms replace their products. These results suggest that the product exit is not the primary channel that generates the within-firm spillover.

These analyses suggest that the negative indirect shock makes firms adjust their product value and characteristics, not price-cost markups or varieties. For firms to change their product features

[27] Specifically, $\tilde{\Delta}N_{cf} \equiv \frac{N_{cf,09}-N_{cf,07}}{\overline{N}_{cf}}$, where N_{cf} is the total number of products in county c and firm f, and $\overline{N}_{cf} \equiv \frac{1}{2}(N_{cf,07} + N_{cf,09})$.

[28] Note that the coefficient for the extensive margin change in sales per UPC is larger than that of our main spillover result for sales (Column (4) in Table 2) despite that the product variety does not change. The difference arises from the fact the dependent variable in Column (1) of Table 5 is well-defined only for firm-county observations that feature both positive product entry and positive product exit. If a firm has no product entry (exit) in a particular county, then the sales per UPC at the product entry (exit) margin in that firm-county is not well-defined. Once we restrict the sample to firm-county observations that have positive product entry and exit, our main spillover effect becomes stronger and similar to that of Column (1) in Table 5.

at the barcode level, they must replace their products, suggesting that firms change their product characteristics. It is difficult to entirely attribute our findings to the markup adjustment through product replacement since those affected firms lower both prices and sales in our data. If firms lower prices and markups due to the indirect shock as in studies of variable markup (e.g., Alessandria et al., 2010), they would generate *larger* sales and quantities conditional on the direct demand change given the well-known price elasticity of goods in these sectors (e.g., Broda and Weinstein, 2010; Hottman et al., 2016).[29] We rule out the variety adjustment channel because we do not find any supporting evidence, as shown in Table 5 columns (7) and (8). Therefore, through the lens of a theoretical model, these results are consistent with quality downgrading (see Footnote 34 and Appendix F for further discussions).[30] Note that these changes in product characteristics across barcode-level products ease a potential concern that a change in barcode may reflect a change in product labels and not a change in product characteristics; the product value declines following the replacement of barcode.

4.3 Heterogeneous Treatment Effect

To gain additional insights on the intrafirm spillover effect, we exploit the rich county and firm heterogeneity in the data to estimate the heterogeneous treatment effect. We allow the intrafirm spillover effect to differ across initial and lagged exposure variables by using the following regression specification:

$$\tilde{\Delta}\mathrm{S}_{cf} = \lambda_{cs} + \gamma_1 \tilde{\Delta}\mathrm{HP}_{cf}\ (\text{other}) \times Z_{cf} + \gamma_2 \tilde{\Delta}\mathrm{HP}_{cf}\ (\text{other}) + \gamma_3 Z_{cf} + \boldsymbol{X'_{cf}\gamma_4} + \varepsilon_{cf} \tag{4.2}$$

[29] We define the county-firm-level quantity and run the regression but do not find meaningful results.

[30] Although we report characteristics associated with intrinsic product values, firms may change the perceived product quality by changing their national advertising or marketing; our analyses are consistent as long as these changes apply to newly introduced products. Similarly, firms may resize and repackage products, which may not necessarily decrease the intrinsic quality of products. Appendix E.6 shows that replaced products still have lower prices relative to destroyed products after adjusting for the size and package of products. Generally, as long as the change in barcode-level products decreases the value of products and reduces firms' local sales, the results are consistent with the intrafirm spillover effect through uniform product replacement. Section F formalizes product replacement as a quality change, and quality is broadly defined as what changes the market share conditional on the product price.

where Z_{cf} is the initial or lagged exposure variable, and λ_{cs} is the county $\times$ 4-digit SIC sector fixed effects that are included in the tightest specification of our empirical analyses thus far. The key difference between this specification and Equation (3.1) is the variable Z_{cf}, which permits the effect of the indirect shock on local-firm sales to differ by county or firm characteristics. In particular, we allow eight different variables associated with the uniform product replacement channel to further test the proposed hypothesis.

Table 6 panel A provides additional empirical support for the uniform product replacement channel. Suppose that firms replace products across multiple markets and spill over the shock. In that case, the spillover effects must be larger for firms offering products in multiple locations than for firms that tailor their products to the local market. Column (1) tests and confirms this prediction by showing that the effect is indeed larger for firms that generate larger sales from products sold in multiple markets conditional on firm-level sales, consistent with the decomposition results in columns (6) and (7) of Table 4. Additionally, conditional on firm characteristics (number of markets), firms offering the local products to a larger number of nonlocal markets—where the indirect shock originates—would be more likely to uniformly replace these products and spill over the shock to the local market. Column (2) tests and confirms this idea by measuring the number of counties per UPC, which is normalized by the number of counties per firm. Columns (3) and (4) provide further evidence by showing that the spillover effect is stronger for firms that have more capacity to replace high- to low-valued products. If firms replace organic products with nonorganic products to spill over the shock, the effect would be concentrated in firms that originally generated larger sales from organic products since they have the option to replace their products with nonorganic products.[31] Appendix E.17 reports the robustness of the results in columns (1)-(4) by using alternative measures of Z_{cf} and controls.

Columns (5)-(8) of Panel A in Table 6 further support the uniform product replacement mechanism by sorting counties and firms based on initial market income. Column (5) shows that the

[31]As shown in Column (4), we find a positive yet statistically insignificant effect through the number of products, potentially because the number of products does not precisely measure the firm-level high-quality index. We find similar results by using the price index as a measure of firm-level quality, potentially because the price measure is confounded with other characteristics of firms.

effect is more substantial in counties where wealthy households dwell. In such counties, under the assumption that wealthy households prefer high-valued products, firms lose more sales when they offer low-valued (or low-quality) products instead of high-valued products.[32] This relationship is explicitly shown in Appendix G.2 Equation (G.29), in which we allow nonhomothetic preference in the model. Column (6) confirms the same intuition at the firm level: firms that face richer customers lose more sales when they replace high-valued products with low-valued products.[33] Columns (7) and (8) consider household income dispersion across counties within firms measured with the standard deviation and interquartile range, respectively. If customer income differs widely across markets, firms have more incentive to produce a larger variety of products and tailor each product to each market because the revenue gains from product tailoring could be larger than the costs of developing and penetrating such products. In this case, the spillover effect through uniform product replacement would be weaker because firms would not uniformly replace the products. Consistent with this intuition, the spillover effects are smaller when firms face a more heterogeneous market. These empirical results are formalized in Appendix F.2 Equation (F.15), highlighting the tradeoff between tailoring products to each market and choosing uniform products across markets.

Panel B in Table 6 shows the importance of other margins. We find that the intrafirm spillover effect is more substantial for (i) the local market of firms where they earn a smaller share of revenues, (ii) large firms, (iii) financially constrained firms, and (iv) nonexporters. Columns (1) and (2) additionally clarify why the intrafirm spillover effect is stronger than the direct effect, as shown in Figure 3 and Table 2. When a firm's initial market share is small, the indirect shock closely proxies for the total firm-level shock, strengthening the intrafirm spillover effect. Columns (3) and (4) indicate that the intrafirm effect is stronger for large firms, consistent with the direct effect results in Argente et al. (2020); it is likely easier for such firms to replace products because they produce more standardized products (Holmes and Stevens 2014) and have more scalability in general (Argente et al. 2020). Columns (5) and (6) show that the effect is marginally stronger

[32] See, e.g., Handbury (2021) for empirical evidence on the relationship between income and product value (quality).

[33] Another interpretation is that such firms originally sold high-valued products and have more capacity to downgrade their product quality, consistent with the results in columns (3) and (4). Both interpretations are consistent with the uniform product replacement channel.

for financially constrained firms, similar to the international evidence in Berman et al. (2015) and the multiplant network results in Giroud and Mueller (2019). Such firms likely incur higher costs in keeping up with high-valued or high-quality products when facing the negative indirect shock, generating a larger spillover effect. Column (7) shows that the effect is concentrated among nonexporters, potentially because exporters can sell off their surplus in the foreign market as shown in Almunia et al. (2021). See Appendix D.2 for more detailed analyses and discussions. Column (8) shows that the spillover effect is larger when the local market of interest is located farther away from the origin of the housing price changes. However, as shown in Appendix E.17, this distance effect is largely confounded by the size of firms; we do not find meaningful results when conditioning on firm size. This result is consistent with the dispersed nature of large firms documented in Bartelme and Ziv (2020). Note that the mechanisms associated with export and distance do not confound the spillover effect, as shown in Appendices E.8 and E.2.

4.4 Formalizing the Spillover Mechanism

Appendix F formalizes the uniform product replacement channel by providing a stylized model, which simplifies and adjusts the standard model environment in Melitz (2003) and Faber and Fally (2021) into a multimarket framework. In the model, firms that face a negative shock decrease their product quality and value, reflecting our empirical evidence that such companies replace high-valued products with low-valued products.[34] The model attributes the decrease in quality to both scale effects and nonhomothetic preferences. The scale effect means that firms that experience depressed demand do not have sufficient sales to recover the high fixed cost of producing high-quality products; for example, organic farming requires more land than conventional farming. Nonhomothetic

[34] We interpret the product replacement in the data as quality downgrading for two reasons: (i) this replacement leads to a decrease in the organic share, unit prices, and sales in the data and (ii) at the barcode level, changes in product attributes and intrinsic qualities must involve product replacements. Based on the decrease in prices, one might suspect that firms reduce their price-cost markups through product replacement instead of lowering their product quality. However, if firms reduce their markups, they must do so to increase their sales, given that the demand elasticities are generally larger than unity in the consumer packaged goods market (see, e.g., Broda and Weinstein, 2010; Hottman et al., 2016); we do not observe an increase in sales due to the decrease in product prices conditional on the indirect shocks.

preferences allow negatively affected, poorer consumers to switch their consumption toward lower quality goods, and as a result, firms have the incentive to supply more inferior quality products. In their quality downgrading process, firms downgrade uniformly across many markets—including markets that did not experience direct local shocks—as in the data, where almost all firms replace products uniformly across multiple markets. Firms do so to avoid the fixed cost of providing different quality products in different markets. Although tailoring product quality to each market may allow firms to generate an optimal amount of revenue from each market and large overall sales, such a benefit is dominated by the immense costs of producing and penetrating different qualities of products in different markets. The resulting uniform product quality changes by multimarket firms generate the intrafirm spillover effect observed in the reduced-form empirical analysis.

The baseline model in Appendix F.1 assumes that firms provide the same goods across multiple markets in 2007-2009, as shown in Tables 1 and OA.2, and shows how multimarket firms spill over the shocks by changing their product values and delivers an equation similar to the reduced-form regression equation (3.1). The extension in Appendix F.2 endogenizes the decision to provide the same goods across markets and highlights the tradeoff that firms face in changing their product values uniformly across markets. The counterfactual exercise in Appendix F.3 shows the identified intrafirm spillover leads to a new interregional shock transmission of the shocks and mitigates the regional consumption inequality.

5 Conclusion

This paper uses detailed data to study whether and how multimarket firms spill over regional shocks across US local markets through their intrafirm market network. We find that a firm's local sales decrease in response to the direct negative local shock but do so more strongly to indirect adverse local shocks, which affect its other markets. Our barcode-level data and the multiregion model identify a novel uniform product replacement mechanism behind the spillover: Firms that face a negative regional shock replace their high-valued products with low-valued products in multiple markets and spill over the shock.

Our work underscores that multimarket firm behavior, which has been neglected in studying local consumption, is crucial in understanding local firm sales variation. Integrating multimarket firms would deepen the understanding of regional economic activities and regional welfare distribution.

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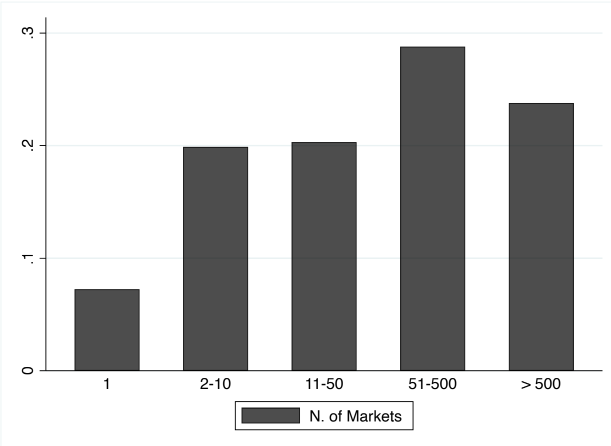
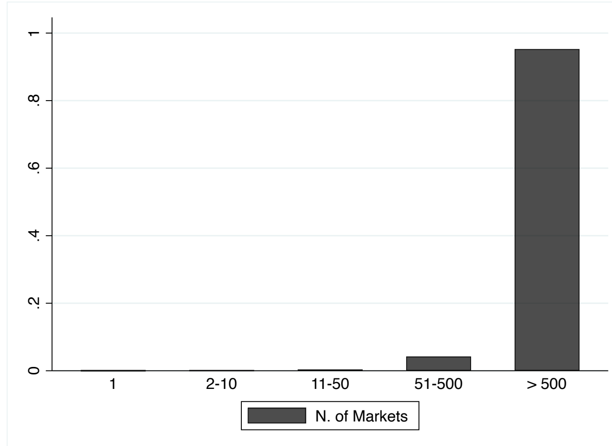
Figure 1: The Prevalence and Importance of Multimarket Firms



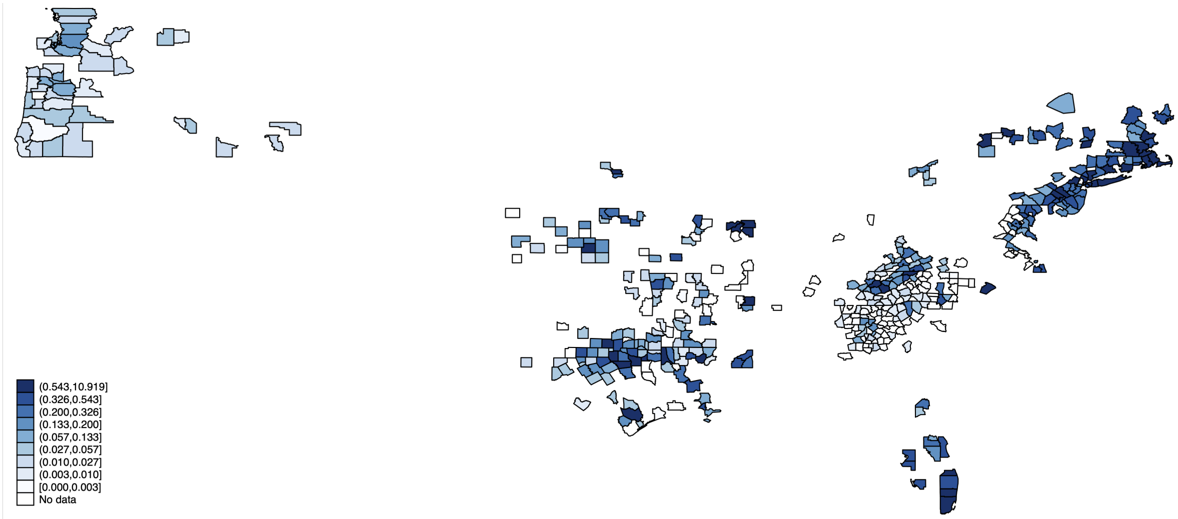
(a) Number of Firms (share, %)



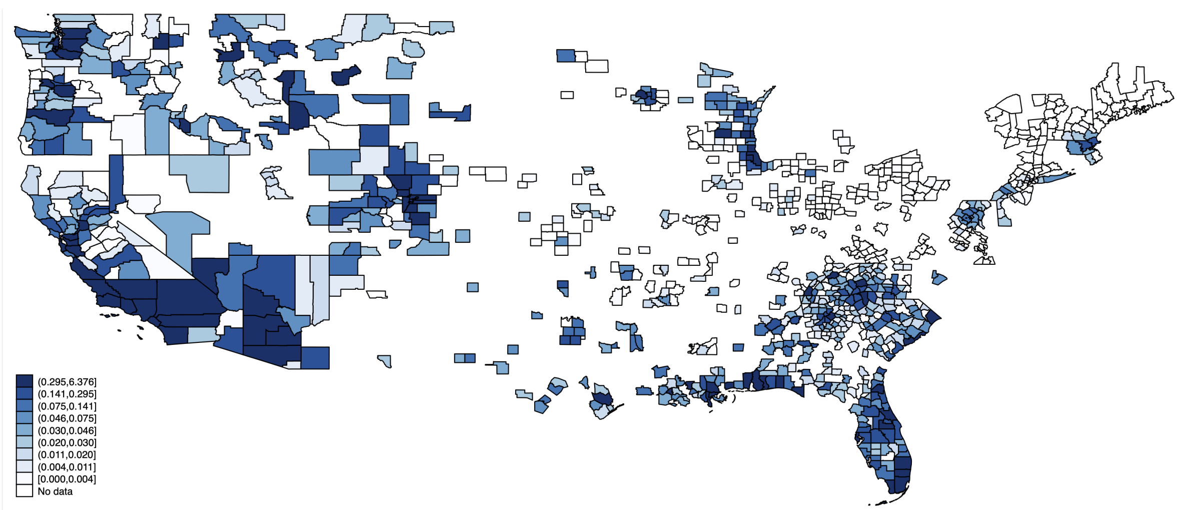
(b) Sales of Firms (share, %)

Note: Figure 1 plots the distribution of 5,597 firms that have nonmissing sales and number of plants information in ACNielsen and NETS combined data. We categorize these firms into five different groups based on the number of markets (counties). "1" on the X-axis denotes a single market firm, "2-10" denotes firms that sell in 2 to 10 markets, "11-50" denotes firms that sell in 11 to 50 markets, "51-500" denotes firms that sell in 51 to 500 markets, and "> 500" denotes firms that sell in more than 500 markets. Figure 1a shows the ratio of the number of firms in each group to total number of firms in the sample, and Figure 1b shows the sales share of the firms in each group.

Figure 2: An Example: Local Sales Share Dispersion within Firms



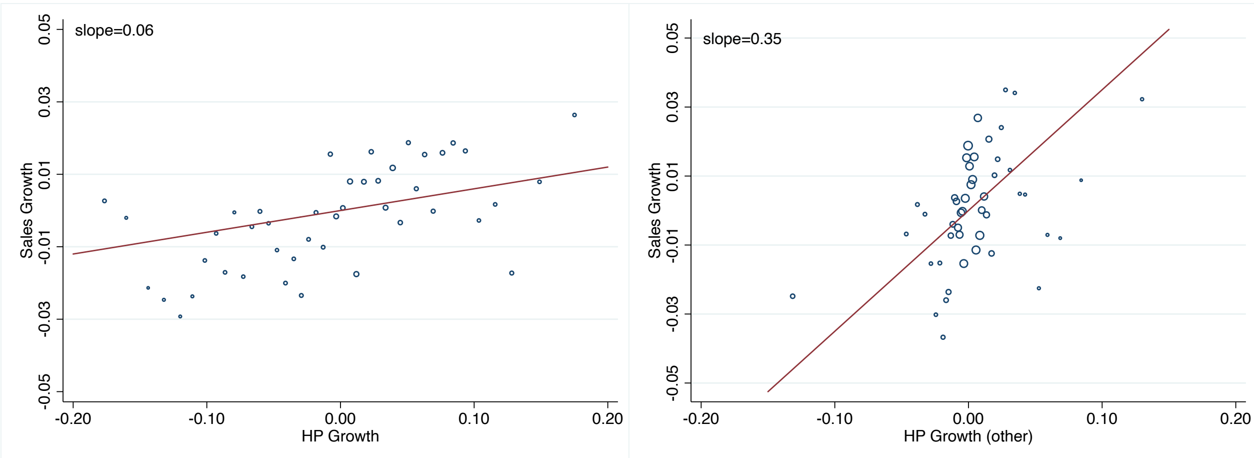
(a) Swiss-American



(b) Arla Foods

Note: Figure 2 plots the 2007 sales share (ω_{cf}, in percent) distribution across counties for two firms in the ACNielsen Retail Scanner data, Swiss-American and Arla Foods.

Figure 3: The Direct and Indirect Effects of Housing Market Disruptions



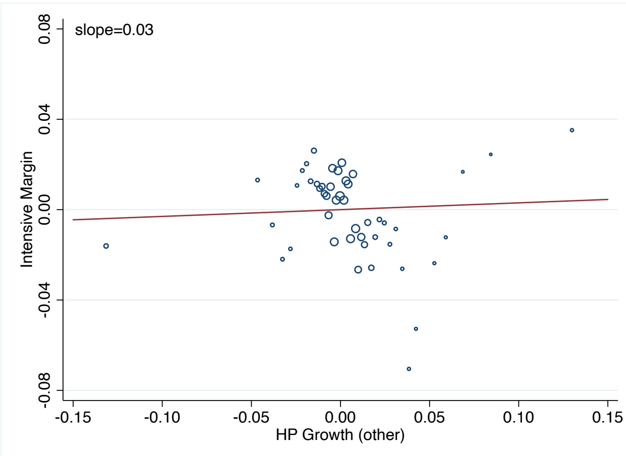
(a) Direct Effect

(b) Indirect Effect

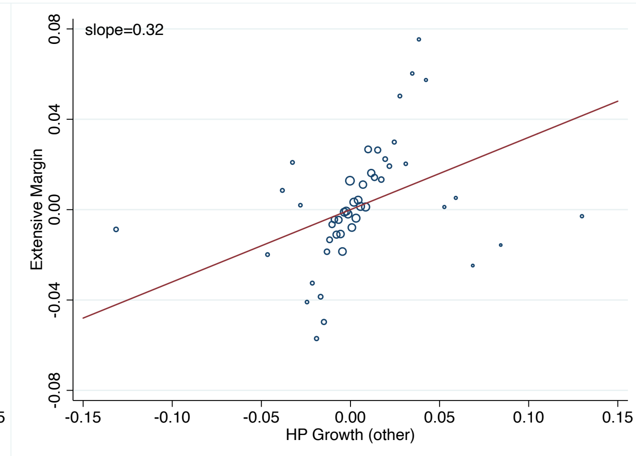
Note. Figure 3a plots the correlation between $\tilde{\Delta}\mathrm{S}_{cf}$ and $\tilde{\Delta}\mathrm{HP}_c$, and Figure 3b plots the correlation between $\tilde{\Delta}\mathrm{S}_{cf}$ and $\tilde{\Delta}\mathrm{HP}_{cf}$ (other). For all variables, we rely on the Frisch-Waugh theorem and partial out controls used in Table 2 column (1). We take a weighted average of each residualized variable by 50 equal-sized housing price growth bins, where the weight is the initial sales. The size of the circle denotes the total initial sales by each bin. The red linear lines and the reported slope coefficients are based on the underlying micro-level data, consistent with the results in Table 2 column (1). The bins with extreme values are excluded for visibility.

Figure 4: The Exact Decomposition of the Intrafirm Spillover Effect

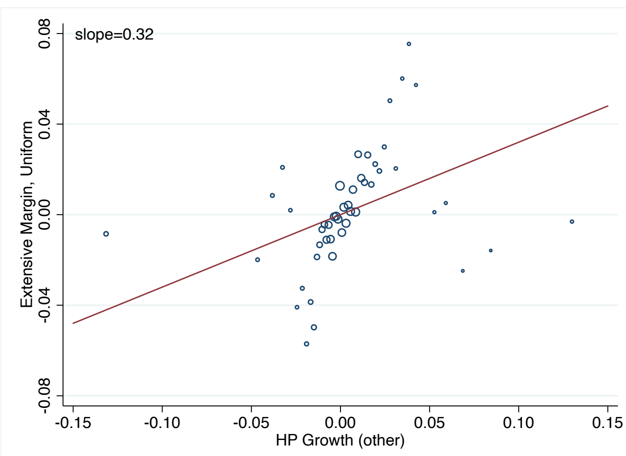
(a) Continuing Product



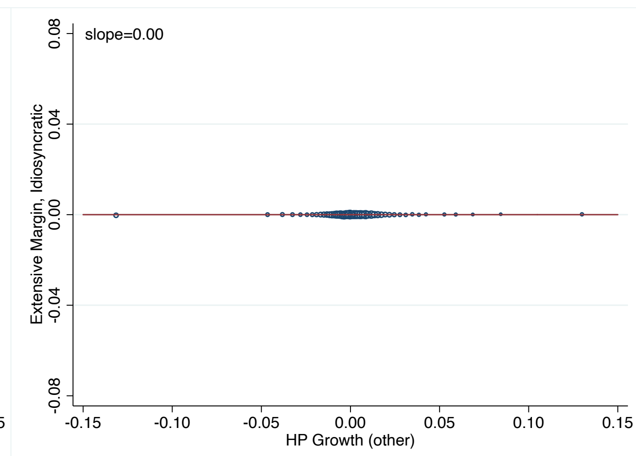
(b) Product Replacement



(c) Uniform Product Replacement



(d) Idiosyncratic Product Replacement



Note. Figure 4a and Figure 4b decompose the correlation between $\tilde{\Delta}\mathrm{S}_{cf}$ and $\tilde{\Delta}\mathrm{HP}_c$ (other) in Figure 3 into the correlation between $\tilde{\Delta}\mathrm{S}^{\mathrm{C}}_{cf}$ and $\tilde{\Delta}\mathrm{HP}_c$ (other) and the correlation between $\tilde{\Delta}\mathrm{S}^{\mathrm{R}}_{cf}$ and $\tilde{\Delta}\mathrm{HP}_{cf}$ (other), respectively. Similarly, Figure 4c and Figure 4d decompose the correlation between $\tilde{\Delta}\mathrm{S}^{\mathrm{R}}_{cf}$ and $\tilde{\Delta}\mathrm{HP}_c$ (other) in Figure 4b into the correlation between $\tilde{\Delta}\mathrm{S}^{\mathrm{R,M}}_{cf}$ and $\tilde{\Delta}\mathrm{HP}_c$ (other) and the correlation between $\tilde{\Delta}\mathrm{S}^{\mathrm{R,L}}_{cf}$ and $\tilde{\Delta}\mathrm{HP}_{cf}$ (other), respectively. All variables are partialled out based on Table 4 column (1) and are weighted averaged by 50 equal-sized housing price growth bins, where the weight is the initial sales. The size of the circle denotes the total initial sales by each bin. The red linear lines and the reported slope coefficients are based on the underlying micro-level data, consistent with the results in Table 4 columns (2) and (3).

Table 1: Extensive Margins: Markets, Products, Firms, and Plants

Variable	Obs	Mean	Std. Dev.	P10	P25	P50	P75	P90
			Panel A: Firm variables					
$N^{\text{counties}}_{f,07}$	4,171	513.243	669.991	10	35	155	808	1,655
$N^{\text{states}}_{f,07}$	4,171	24.278	18.682	2	6	21	47	49
$N^{\text{UPCs}}_{f,07}$	4,171	54.239	231.783	2	4	12	37	110
$N^{\text{groups}}_{f,07}$	4,171	2.701	3.421	1	1	2	3	6
$N^{\text{plants}}_{f,07}$	3,901	23.03	133.964	1	1	1	2	13
$\text{S}_{f,07}$	4,171	15.586	147.974	.005	.034	.278	2.13	14.677
			Panel B: UPC variables					
$N^{\text{counties}}_{u,07}$	226,230	225.577	415.986	1	5	34	203	789
$N^{\text{states}}_{u,07}$	226,230	15.791	16.953	1	2	7	27	47
			Panel C: County variables					
$N^{\text{firms}}_{c,07}$	991	848.316	353.868	341	616	876	1,110	1,306
$N^{\text{UPCs}}_{c,07}$	991	28,995.06	15382.66	7,994	17,404	28,730	40,399	49,854

Note. Panel A provides three separate extensive margins of firms in addition to the firm-level sales: a number of markets (counties and states), products (UPC and product groups), and plants (establishments). Panel B presents the number of counties and states each UPC serves, and Panel C shows how many firms and UPCs operate in each market. The subscript f denotes a firm, u denotes a UPC, and c denotes a county. N^{counties} is the number of counties, N^{states} is the number of states, N^{UPCs} is number of UPCs, N^{groups} is the number of product groups, N^{plants} is the number of plants (or establishments), N^{firms} is the number of firms, and S denotes sales, which is in millions of US dollars. The sample is restricted to those counties and firms used in the main regression analyses.

Table 2: The Direct and Indirect Effects of the Housing Market Disruptions

	$\tilde{\Delta}S_{cf}$, 2007-2009							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Ordinary Least Squares				IV Estimation Using			
					elasticity	sensitivity	lending	all
$\tilde{\Delta}HP_c$	0.06**	0.06**						
	(0.03)	(0.03)						
$\tilde{\Delta}HP_{cf}$ (other)	0.35***		0.34***	0.40***	0.60***	0.72***	0.41**	0.44**
	(0.11)		(0.12)	(0.10)	(0.14)	(0.25)	(0.20)	(0.22)
County-Firm Controls	✓		✓	✓	✓	✓	✓	✓
County Controls		✓						
Firm FE		✓						
Sector FE	✓		✓					
County FE			✓					
County x Sector FE				✓	✓	✓	✓	✓
First-stage F statistics					541.20	231.20	540.50	254.70
Hansen's J-stat p-value								0.24
R^2	0.20	0.61	0.24	0.39				
$E[\tilde{\Delta}S_{cf}:\tilde{\Delta}HP_{p95}-\tilde{\Delta}HP_{p5}]$	.027	.028						
$E[\tilde{\Delta}S_{cf}:\tilde{\Delta}HP_{p95,other}-\tilde{\Delta}HP_{p5,other}]$	.030		.030	.035	.053	.064	.036	.039
Observations	840,681	840,681	840,681	840,681	448,604	587,436	658,607	417,869

Note. All county-level control variables used in Mian et al. (2013) and firm-level variables that proxy for firm size and scope are included. County-firm controls are the initial log of the following variables: county-firm sales, firm sales, the firm's number of markets, and product groups. County controls are prerecession percentage white, median household income, percentage owner-occupied, the percentage with less than a high school diploma, percentage with only a high school diploma, the unemployment rate, the poverty rate, percentage urban, and employment share in a county for 2-digit industries. The sector is defined based on 4-digit SIC. The elasticity, sensitivity, and lending are the leave-one-out weighted average of the regional Saiz (2010) housing supply elasticity, Guren et al. (2021) housing price sensitivity, and García (2018) nonlocal mortgage lending shock, respectively. The regression is weighted by initial county-firm sales; standard errors are two-way clustered by state and sector. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: County-Firm-Retail-level Regression Analyses

	(1) $\tilde{\Delta}\text{S}_{crf}$	(2) $\tilde{\Delta}\text{S}^{\text{R}}_{crf}$	(3) $\tilde{\Delta}\text{S}^{\text{C}}_{crf}$	(4) $\tilde{\Delta}\text{S}_{crf}$	(5) $\tilde{\Delta}\text{S}^{\text{R}}_{crf}$	(6) $\tilde{\Delta}\text{S}^{\text{C}}_{crf}$	(7) $\tilde{\Delta}\text{S}_{crf}$	(8) $\tilde{\Delta}\text{S}^{\text{R}}_{crf}$	(9) $\tilde{\Delta}\text{S}^{\text{C}}_{crf}$
$\tilde{\Delta}\text{HP}_{cf}$ (other)	0.45***	0.43***	0.02	0.44***	0.43***	0.01	0.41***	0.41***	0.01
	(0.11)	(0.08)	(0.04)	(0.11)	(0.08)	(0.04)	(0.11)	(0.08)	(0.05)
$\tilde{\Delta}\text{HP}_{cr}$ (other)	0.38***	0.07***	0.31***						
	(0.04)	(0.01)	(0.06)						
$\tilde{\Delta}\text{HP}_{crf}$ (other)							0.12	0.10	0.03
							(0.15)	(0.13)	(0.06)
County-Retail-Firm Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
County x Sector FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
County x Retail FE				✓	✓	✓	✓	✓	✓
R^2	0.29	0.29	0.31	0.39	0.32	0.40	0.39	0.33	0.40
Observations	1,846,616	1,846,616	1,846,616	1,847,979	1,847,979	1,847,979	1,842,926	1,842,926	1,842,926

Note. The subscript c is a county, r is a retail chain, and f is a firm, defined as a producer. The sector is based on 4-digit SIC. $\tilde{\Delta}\text{S}_{crf}$ is county-retail-firm-specific sales growth, and $\tilde{\Delta}\text{S}^{\text{R}}_{crf}$ and $\tilde{\Delta}\text{S}^{\text{C}}_{crf}$ decompose $\tilde{\Delta}\text{S}_{crf}$ into net creation and continuing product sales growth. $\tilde{\Delta}\text{HP}_{cf}$ (other) is the indirect demand shock defined in Section 2, which is the initial county-firm-specific sales-weighted local house price growth excluding county c housing price growth. $\tilde{\Delta}\text{HP}_{cr}$ (other) is the initial county-retail-specific sales-weighted local house price growth excluding county c house price growth. $\tilde{\Delta}\text{HP}_{crf}$ (other) is the initial county-retail-firm-specific sales-weighted local house price growth excluding county c housing price growth. County-Retail-Firm controls are the initial log of the following variables: county-firm-retail sales, firm sales, the firm's number of markets and product groups, retail sales, the retailer's number of markets and product groups, and firm-retail sales; retail-specific variables are excluded when county x retail fixed effects are included due to collinearity. The regression is weighted by initial county-retail-firm sales; standard errors are three-way clustered by state, sector, and retailer. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 4: The Exact Decomposition of the Intrafirm Spillover Effect

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Ordinary Least Square, Decomposition						IV, Decomposition	
		$\tilde{\Delta}\mathrm{S}_{cf}$		$\tilde{\Delta}\mathrm{S}_{cf}$		$\tilde{\Delta}\mathrm{S}^{\mathrm{R}}_{cf}$		$\tilde{\Delta}\mathrm{S}_{cf}$	
	$\tilde{\Delta}\mathrm{S}_{cf}$	$\tilde{\Delta}\mathrm{S}^{\mathrm{C}}_{cf}$	$\tilde{\Delta}\mathrm{S}^{\mathrm{R}}_{cf}$	$\tilde{\Delta}\mathrm{S}^{\mathrm{C}}_{cf}$	$\tilde{\Delta}\mathrm{S}^{\mathrm{R}}_{cf}$	$\tilde{\Delta}\mathrm{S}^{\mathrm{R,M}}_{cf}$	$\tilde{\Delta}\mathrm{S}^{\mathrm{R,L}}_{cf}$	$\tilde{\Delta}\mathrm{S}^{\mathrm{C}}_{cf}$	$\tilde{\Delta}\mathrm{S}^{\mathrm{R,M}}_{cf}$
$\tilde{\Delta}\mathrm{HP}_c$	0.06**	0.05***	0.01						
	(0.03)	(0.02)	(0.01)						
$\tilde{\Delta}\mathrm{HP}_{cf}$ (other)	0.35***	0.03	0.32***	-0.02	0.42***	0.42***	0.00	0.08	0.37**
	(0.11)	(0.06)	(0.09)	(0.05)	(0.10)	(0.10)	(0.00)	(0.08)	(0.17)
County-Firm Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
Sector FE	✓	✓	✓						
Sector x County FE				✓	✓	✓	✓	✓	✓
First-stage F statistics								254.70	254.70
Hansen's J-stat p-value								0.20	0.82
R^2	0.20	0.22	0.28	0.43	0.41	0.41	0.27		
Observations	840,681	840,681	840,681	840,681	840,681	840,681	840,681	417,869	417,869

Note. The regression specifications in columns (1)-(3), (4)-(7), and (8)-(9) are the same as those in Table 2 columns (1), (4), and (8), respectively. All the dependent variables are formally defined in Section 2. The coefficients in columns (2)-(3) decompose the coefficients in column (1), the coefficients in columns (4)-(5) decompose the coefficients in Table 2 column (4), the coefficients in columns (6)-(7) decompose the coefficients in column (5), and the coefficients in columns (8)-(9) decompose the coefficients in Table 2 column (8). Columns (8)-(9) report the results by using all three instrumental variables: the leave-one-out weighted average of the regional Saiz (2010) housing supply elasticity, Guren et al. (2021) housing price sensitivity, and García (2018) nonlocal mortgage lending shock. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 5: Changes in Barcode-level Product Values, Characteristics, and Varieties

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$\tilde{\Delta} v_{cf}$						$\tilde{\Delta} N_{cf}$	
v_{cf} is	S per UPC	Price			Organic			
		Simple	Weight	Weight (d)	Sale	Number		
$\tilde{\Delta}\text{HP}_{cf}$ (other)	0.52**	0.73**	0.92**	0.70**	14.60**	12.78**	-0.04	-0.06
	(0.21)	(0.27)	(0.44)	(0.34)	(5.96)	(5.19)	(0.14)	(0.17)
County-Firm Controls	✓	✓	✓	✓	✓	✓	✓	✓
Sector x County FE	✓	✓	✓	✓	✓	✓	✓	✓
Sample Restriction								✓
R^2	0.40	0.51	0.41	0.42	0.38	0.36	0.35	0.40
Observations	464,423	461,672	461,672	461,672	27,930	27,930	840,681	464,423

Note. The regression specifications are the same as that in Table 2 column (4). S per UPC is defined as total sales per UPC. The simple and weighted price indexes in columns (2) and (3) are the simple and the sales-weighted geometric price across UPCs within the product group and firm. The simple index is the conventional price index component of the nested CES demand system in Hottman et al. (2016), and the weighted index is used to adjust for the importance of each UPC, as in the Cobb-Douglas utility function; all of them are sales-weighted averaged across groups. The weighted and demeaned price index in column (4) additionally subtracts the average product group price index following the quality index used in Argente et al. (2018). Columns (5) and (6) use the sales share and number of organic products, and columns (7) and (8) use the total number of products. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Heterogeneous Treatment Effect

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$\tilde{\Delta}\mathrm{S}_{cf}$							
	Panel A: The Uniform Product Replacement Channel							
	Using Firm Characteristics				Using Market Characteristics			
Z_{cf} is	$\mathrm{S}_f^{\mathrm{Multiple}}$	$\mathrm{N}_{u,f}^{\mathrm{Counties}}$	$\mathrm{S}_f^{\mathrm{Organic}}$	$\mathrm{N}_f^{\mathrm{Organic}}$	inc_c	inc_f	sd(inc_c)	iqr(inc_c)
$\tilde{\Delta}\mathrm{HP}_{cf,07-09}$ (other) x Z_{cf}	3.46***	0.98**	0.45**	1.00	0.13**	0.04***	-8.00***	-4.09***
	(1.00)	(0.43)	(0.20)	(1.08)	(0.05)	(0.01)	(2.86)	(1.10)
County-Firm Controls	✓	✓	✓	✓	✓	✓	✓	✓
Sector x County FE	✓	✓	✓	✓	✓	✓	✓	✓
R^2	0.40	0.39	0.73	0.71	0.39	0.39	0.39	0.39
Observations	840,677	840,681	74,323	74,323	840,681	840,681	840,681	840,681
	Panel B: Local Share, Size, Financial Constraint, Export, and Distance							
Z_{cf} is	local_{cf}	$\mathrm{D}_{cf}^{\mathrm{Local}}$	S_f	emp_f	paydex_f	RZ	$\mathrm{D}_f^{\mathrm{Export}}$	dist_{cf}
$\tilde{\Delta}\mathrm{HP}_{cf,07-09}$ (other) x Z_{cf}	-0.10***	-0.57***	0.28***	0.22***	2.15*	3.46	-0.42***	0.22***
	(0.03)	(0.13)	(0.05)	(0.08)	(1.20)	(2.14)	(0.10)	(0.08)
County-Firm Controls	✓	✓	✓	✓	✓	✓	✓	✓
Sector x County FE	✓	✓	✓	✓	✓	✓	✓	✓
R^2	0.39	0.39	0.39	0.40	0.38	0.36	0.39	0.39
Observations	840,681	840,681	840,681	838,508	771,840	571,795	840,681	840,681

Note. The regression specification is the same as that in Table 2, column (4) except that we include the exposure variable Z_{cf} and its interaction with the indirect shock $\tilde{\Delta}\mathrm{HP}_{07-09}$ (other) following Equation (4.2). We consider 14 alternative measures of Z_{cf}. $\mathrm{S}_f^{\mathrm{Multiple}}$ is the initial log sales of products that are sold in multiple markets per firm f, and $\mathrm{N}_{u,f}^{\mathrm{Counties}}$ is the sales-weighted average of the initial number of counties per UPC to the initial number of counties per firm f: $\mathrm{N}_{u,f}^{\mathrm{Counties}} \equiv \sum w_{uf}\mathrm{N}_u^{\mathrm{Counties}}/\mathrm{N}_f^{\mathrm{Counties}}$, where w_{uf} is the sales share of UPC u out of the total sales of firm f, $\mathrm{N}_u^{\mathrm{Counties}}$ is the initial number of counties per UPC u, and $\mathrm{N}_f^{\mathrm{Counties}}$ is the initial number of counties per firm f. $\mathrm{S}_f^{\mathrm{Organic}}$ is the initial log sales of organic products per firm f, and the $\mathrm{N}_f^{\mathrm{Organic}}$ is the initial log number of organic products per firm f. In columns (1) and (3), we additionally control for log firm sales and its decile fixed effects and their interaction with $\tilde{\Delta}\mathrm{HP}_{cf,07-09}$ (other) to compare firms generating similar revenue. In columns (2) and (4), we additionally control for the log number of products per firm and its decile fixed effects and their interaction with $\tilde{\Delta}\mathrm{HP}_{cf,07-09}$ (other) to compare firms with a similar number of products. inc_c is the initial log household median income in county c, and inc_f is the initial firm-level household median income, which is measured as the initial sales-weighted average of inc_c across counties (markets) c within firm f. sd(inc_c) and iqr(inc_c) are the standard deviation and interquartile range of inc_c across counties by firm, respectively. The local_{cf} is the log firm's initial local sales share, and $\mathrm{D}_{cf}^{\mathrm{local}}$ is a dummy variable equal to 1 if the initial local sales share is larger than the median value: $\mathrm{D}_{cf}^{\mathrm{local}} \equiv \mathbb{1}_{\{\mathrm{local}_{cf} > \mathrm{Median}(\mathrm{local}_{cf})\}}$. S_f denotes the initial log firm-level sales, emp_f is the initial log firm-level employment, Paydex_f is the 2002-2006 average numerical credit score given by Dun & Bradstreet, and RZ is the 2-digit SIC Rajan and Zingales (1998) external financial dependence index computed from Compustat data. The paydex_f is measured as ln(100-paydex) to facilitate interpretation. $\mathrm{D}_f^{\mathrm{export}}$ equals 1 if firms are exporters and 0 otherwise. dist_{cf} is the initial sales-weighted average of distance to the local market of interest across counties within all markets of firms. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.