Prices and Immigration: Firm-Level Evidence*

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Abstract

This paper studies how immigration affects consumer prices. Using scanner data and instrumenting county-level immigration with ancestry patterns, we find an inflow of 10,000 immigrants lowers four-year price growth by 0.66 percentage points. Measuring firm exposure separately through sales markets and production locations, we show the decline comes entirely from product demand, not labor costs: firms lower prices when immigrants arrive where they sell, not where they produce. We provide evidence that immigrants search more intensively, are more price elastic, pay less for identical products, and buy lower-appeal products. We rationalize these patterns with a model of heterogeneous demand elasticities.

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1 Introduction

Immigration and inflation stand as two paramount concerns for policymakers and the public alike, frequently dominating political discourse and economic analyses, particularly in recent years.[1] Considering immigration, public sentiment has recently leaned towards a desire for reduced immigration, often driven by concerns about its perceived effects on native wages. Yet, to fully grasp its economic impact, particularly on consumers, it is crucial to examine how immigration affects the prices they pay. Concurrently, inflation has surged as a major concern—particularly for everyday consumer goods—with popular debates pointing to both labor shortages and increased consumer demand as culprits. While the economic connection between immigration and inflation is frequently debated in media[2], their interplay remains an underexplored area in academic research.

Against this backdrop of pressing policy debates and a sparsely studied academic landscape, this paper asks two questions: Does immigration raise or lower consumer prices? And why?

The effect of immigration inflow on prices is ambiguous, mainly arising from two complexities. First, immigration operates through both labor supply and product demand. Media commentary stresses this dual effect of a larger population: a larger labor supply lowers labor costs and prices, while larger product demand pushes prices up. Second, immigrants possess different characteristics than natives and may affect prices differently than a simple increase in total population. For instance, immigrants may be more price sensitive and search more across products than natives, either because they have lower incomes or because they are new to the market and have not yet formed shopping habits. This behavior can compel firms to *lower* their prices even with the overall increased product demand from immigrants. Beyond these demand-side adjustments, immigration inflow can also raise wages, spur the entry of new establishments, or lead to production-side adaptations like firm scaling, innovation, or the introduction of new product varieties, depending on the type of immigrants. Which of these countervailing forces dominates is an empirical question.

To investigate the causal effect of immigration and its underlying mechanisms, we construct a detailed micro-level dataset by combining product-destination-level retail scanner prices with producer production sites, alongside county-specific historical and current immigration inflows and their origins. This dataset is built by matching the NielsenIQ Retail Scanner and Consumer Panel database with the GS1 database and the NETS establishment database in 2006-2018; these combined datasets are further matched to historical and current immigration data from the Census and the American Community Survey based on county information.[3] The matched data allow us to separately observe firms' product sales (destination) and their production (labor sites), which enables us to disentangle the product demand and labor supply channels through which immigration

[1] See Gallup polls in Appendix Figure B.1. As of summer 2024, 55% of Americans responded that immigration should be decreased (highest since 2001), and 41% named inflation the most important financial problem facing their family (highest recorded since poll started in 2005).

[2] For example, see the related articles in Wall Street Journal and The Washington Post

[3] The matching of retail scanner prices and firm production is done using firm names and address information via GS1 database. See Appendix for details.

affects prices. The household panel characteristics—such as frequency of purchases and trips—and granular product information allow us to investigate mechanisms beyond these direct demand and supply channels, including consumer search behavior and product variety effects. To ensure external validity and conduct additional robustness exercises, we supplement these data with additional county-level information from various datasets, including the Quarterly Census of Employment and Wages (QCEW), Bureau of Labor Statistics (BLS) City Price Index, and CoreLogic and Zillow Housing Price Indexes.

Armed with detailed data, we first show that immigration inflows *decrease* prices at the county level, with effects concentrated among low-income immigrants and disproportionately benefiting lower-income households. In this initial county-level analysis, we utilize geographic variation akin to the existing regional immigration literature. We consistently find that counties with more international immigrant inflows experience smaller price increases for consumer packaged goods (CPG), a result that holds whether using a chain-weighted county-level price index derived from barcode data or the official BLS price index for robustness.[4] To establish a causal impact and circumvent endogeneity concerns—such as immigrant self-selection into economically dynamic or more expensive regions—we instrument immigration with more than a century of ancestry and migration patterns (Burchardi et al., 2019; Terry et al., 2026). Prices fall most for the products that lower-income households buy, which overlap with those of newly arriving immigrants. These households, often thought to lose most from immigration through wage competition and more likely to oppose it (Scheve and Slaughter, 2001; Mayda, 2006), thus gain as consumers.

The effect is economically meaningful and holds across price measures and specifications. An increase of ten thousand new immigrants over a four-year period lowers CPG prices by approximately 0.66 percentage points, or about 9% of mean price growth, comparable in proportional terms to the wage effect that Terry et al. (2026) estimate with the same ancestry-based instrument. The estimate survives several identification tests. Shift-share-robust standard errors are comparable to our state-clustered baseline, the estimate is nearly unchanged when we drop any single origin from the instrument, randomly reassigning the instrument across counties rarely produces an estimate as strong as the actual one, and past inflation does not respond to contemporaneous immigration. The effect is also not an artifact of measurement: alternative index formulas also show a significant decline, as does measuring immigration relative to initial county population rather than in levels. Quantity rises, but significantly only for private-label goods, consistent with spending shifting toward cheaper goods. Beyond CPG, non-durable goods show a similar disinflationary response, durables and housing purchase prices show no significant change, and only housing rents rise.[5]

Having established a robust county-level reduced-form relationship, we next shift our unit of analysis to the firm to investigate how and why prices respond to immigrant inflows. We do so for

[4]Our retail scanner price index, based on CES utility (Sato, 1976; Vartia, 1976), is similar to the BLS methodology and presents a similar trend to that of the official BLS price index; see the Appendix for details.

[5]The net effect on the regional cost of living remains negative, because rent's small CPI share means that the non-durable price reductions outweigh the rental increase.

two reasons. First, price setting is largely a firm-level decision, and firm-specific shocks account for roughly 40% of the variance of aggregate inflation in advanced economies (Alvarez-Blaser et al., 2025). At the firm level we observe the decision makers themselves: the prices each firm charges, the households that buy its products, and how those households shop. Since a firm that sells in several counties sets its prices across all of them, a cross-county comparison might not capture a response common to those counties. Second, the county estimates capture the overall price response in a county, where the same immigrants are both consumers and workers, but cannot tell which of the two roles drives the decline. Because we observe where a firm sells as well as where it produces, at the firm level we can measure the product demand channel with immigrant inflows into the firm's sales locations and the labor supply channel with inflows into its production locations. For each exposure we study prices along with purchaser characteristics, shopping behavior, and prices paid.

Our firm-level analyses point to the product demand channel. Contrary to the common intuition that immigrant consumers raise prices and immigrant workers lower them, firms more exposed to immigration in their sales markets *lower* their prices, while their price response is weakly *positive* when immigrants arrive in their production sites. A one standard deviation increase in demand exposure lowers firm prices by 2.8 percentage points, three times the size of the supply-side response. Since immigrant demand expands the market, which under a scale effect would raise prices, the decline is consistent with markup compression as more price-sensitive consumers flatten the demand curve firms face. We also find that the demand-side price decline is concentrated among firms selling to low-income consumers, mirroring the county-level finding that price declines are concentrated among low-income immigrants and the products that low-income households buy. The decline remains when we control for retail wages and rents in sales markets, holds for individual barcodes rather than only in the expenditure-weighted firm index, and tracks the manufacturer's exposure in the markets where it sells but does not produce, even within retail chains, consistent with the demand channel.

Detailed purchase records suggest that search is one source of this greater price sensitivity. Firms facing a larger influx of immigrants in their sales markets, but not in their production locations, see an increase in purchasers who make more shopping trips and spend more days searching, along with an increase in the share of (cheaper) private-label products. These purchasers also pay below-average prices for identical products they buy from other firms, consistent with a customer base that is more responsive to prices.

We then extend our chain-weighted price index to a utility-based price index by structurally estimating a nested demand system. The extended index accounts for changes in product variety (Feenstra, 1994) and appeal (Hottman et al., 2016), and lets demand elasticities vary across markets. Three findings emerge. First, greater immigration inflows raise the estimated demand elasticities, consistent with the more price-sensitive demand that the search evidence above points to. Second, estimated appeal falls significantly with product demand exposure, consistent with price-sensitive immigrants buying the firm's cheaper, lower-appeal products. Third, the variety effect through the

product demand channel is largely muted, both in our structural variety index and in the number of products purchased per household, so variety-based competition is not what drives the price decline. Supply-side exposure instead raises prices and appeal, as well as patent and trademark filings, consistent with firms investing in innovation when immigrants arrive where they produce (Kerr and Lincoln, 2010; Terry et al., 2026).

We rationalize these findings with a parsimonious model in which immigrants are more price-sensitive than natives, a single primitive that can generate both the price decline and the fall in appeal. The difference operates through two margins, which are the two tiers of the demand system we estimate. First, substitution across firms changes markups. As immigrants become a larger share of a firm's buyers, the elasticity it faces rises and its markup compresses. That compression works against a scale effect that raises marginal cost as the market expands, and, taking the estimated rise in elasticities as given, markup compression dominates at our estimated parameters. Second, substitution within the firm changes the composition of sales: facing the same products at the same prices, immigrants spend more on the firm's cheaper, lower-appeal products. An inflow of immigrant consumers reallocates sales toward these products, lowering measured appeal even though the products themselves do not change.

Literature Review. Research on the economic effects of immigration has long focused on the labor market, with a large and diverse body of literature exploring its effects on nominal wages and employment.[6] The effects on consumer prices, and hence on real wages and the cost of living, remain comparatively little studied.

Papers that study prices directly find that the sign of the effect depends on the channel. On the supply side, Cortes (2008) documents that low-skilled immigration reduces the prices of immigrant-intensive nontradable services through lower wages.[7] On the demand side, immigrant spending raises local demand for services (Hong and McLaren, 2015) and rents and house prices in supply-constrained cities (Saiz, 2007; Sanchis-Guarner, 2023), although native out-migration can dampen or even reverse the price increase locally (Saiz and Wachter, 2011; Sá, 2015). By contrast, exploiting the sudden arrival in Israel in 1990 of Soviet immigrants, most of whom were not yet working, Lach (2007) finds that retail prices fell with the immigrant share, which he interprets as reflecting the new consumers' price sensitivity and search. In the US, McCully et al. (2025) find that immigrants raise the variety of imported goods available locally but leave the prices of imported

[6] For instance, while some studies find that immigration can harm the wages of low-skilled natives (Borjas, 2003; Monras, 2020), other papers report positive or negligible impacts, citing factors like imperfect substitutability, native worker specialization, and productivity gains (Card, 2009; Peri and Sparber, 2009; Ottaviano and Peri, 2012). Reviews of this literature suggest that the average wage effect is small (Peri, 2016; Clemens and Hunt, 2019), though estimates differ substantially across research designs and worker groups (Dustmann et al., 2016).

[7] Dustmann and Glitz (2015) find a parallel pattern in labor markets: local labor supply shocks lower wages in nontradable sectors, while firms in tradables absorb them by shifting their mix of workers. Burstein et al. (2020) show, in theory and in U.S. data, that immigration crowds native workers out of immigrant-intensive jobs in nontradables, while tradables adjust more through output than through prices.

varieties unchanged.[8] Aggregate studies find effects of either sign (Furlanetto and Robstad, 2019; Zachariadis, 2012), and recent work shows why the channels must be considered jointly: the two effects can offset (Cheremukhin et al., 2024), demand exposure raises native earnings distinctly from supply exposure (Galaasen et al., 2025), and both operate at once in general equilibrium (Cravino et al., 2026). In our setting, which spans over a decade of data across U.S. counties and in which both channels are present (immigrants being workers as well as consumers), immigration lowers CPG prices. To see which channel drives this decline, we let the data choose between them with micro data on the firms that set prices: firm-level exposures that separate where firms sell from where they produce, search behavior observed in the purchase records of each firm's customers, and structural estimates of elasticities and appeal. The demand channel dominates: prices fall where immigrants buy, not where they work, consistent with more price-sensitive consumers compressing markups.

Our evidence on consumer search relates to work showing that households with a lower opportunity cost of time shop more and pay less for identical goods (Aguiar and Hurst, 2007), to models that link consumer price sensitivity to firm markups and macroeconomic outcomes (Kaplan and Menzio, 2016), and to work on how consumer behavior changes with aggregate shocks such as recessions (Nevo and Wong, 2019). In particular, our results are parallel to the empirical findings that local shocks (e.g. house prices, incomes of other households) can affect consumer spending and price sensitivity, which in turn leads to changes in markups (Stroebel and Vavra, 2019; Sangani, 2024). Relatedly, our work complements cross-sectional studies on how prices vary with consumer characteristics (Jaravel, 2019; Handbury, 2021; Faber and Fally, 2022; Auer et al., 2024; Mongey and Waugh, 2025; Hu et al., 2025). We find that immigration lowers prices more for lower-income households, the reverse of the pattern documented for the Great Recession, growth and rising income inequality, and large devaluations, which raised the cost of living more for lower-income households (Argente and Lee, 2021; Jaravel, 2019; Cravino and Levchenko, 2017).

To estimate the causal effect of immigration, we build on the Bartik-type instruments developed by Card (2001) and refined by Burchardi et al. (2019) and Terry et al. (2026). To extend our price index, we estimate a nested demand system that allows for time-varying product variety and consumer preferences at the micro level, an approach widely used in macroeconomics, trade, and development (e.g., Broda and Weinstein, 2006, 2010; Handbury and Weinstein, 2015; Hottman et al., 2016; Atkin et al., 2018), and aggregate product prices into firm-level price indices, as in Hottman et al. (2016), Eslava et al. (2024), and Lenzu et al. (2026).

The paper is organized as follows. Section 2 discusses the various theoretical channels through which immigration can affect prices. Section 3 describes the data construction and empirical strategy. Section 4 presents our findings on how immigration affects prices at the county level. Section 5 investigates the underlying mechanisms using firm-level variation in exposure to immigration through sales and production locations. Section 6 estimates a nested demand system to decompose firm-level prices and to measure how demand elasticities respond to immigration. Section 7 develops

[8] For other effects of migration on available varieties, see Argente et al. (2026) and di Giovanni et al. (2015).

a parsimonious theoretical framework with household heterogeneity to rationalize these findings. Section 8 concludes.

2 The Link Between Immigration and Prices

Immigration can influence consumer prices through two primary channels: labor supply effects and product demand effects, with the net effect theoretically ambiguous. Table 1 summarizes the key mechanisms through which immigration affects prices based on our survey of the literature.

Table 1: Channels Through Which Immigration Affects Prices

Primary Channel		**Price**	**Description**	**Source**
Labor Supply	Lower Wages	Fall	Increased labor supply reduces wages and production costs.	Borjas 03, Cortes 08, Monras 20
	Productivity		Task specialization increases productivity, lowering unit costs.	Peri 12
	Entrepreneurship		Immigrant entrepreneurs increase firm entry and competition.	Azoulay et al. 22, Olney 13, Mahajan 24
	Skill Upgrading	Rise	Natives shift to more complex tasks, which pay more.	Peri Sparber 09, Foged Peri 16
	Innovation		Increased innovation and capital investment raise costs.	Kerr Lincoln 10, Burchardi et al. 19, Terry et al. 26
Product Demand	Demand Pull	Rise	Larger population increases overall demand.	Saiz 07
	Price Sensitivity	Fall	Search-intensive immigrants increase price competition.	Lach 07

Labor Supply Channel Immigration directly expands the local labor force, which can affect prices through multiple supply-side mechanisms. The most direct effect operates through labor costs: an influx of workers increases labor supply and can reduce wages, particularly for occupations where immigrants concentrate (Borjas, 2003; Cortes, 2008; Monras, 2020), although natives limit the decline by reallocating toward communication-intensive tasks (Peri and Sparber, 2009). Beyond wages, immigration raises total factor productivity, consistent with more efficient task specialization and the adoption of unskilled-efficient technologies (Peri, 2012), and immigrants are also concentrated in the largest, most productive firms, which amplifies the resulting cost and price declines (Brinatti and Morales, 2025). Immigrants also contribute to entrepreneurship (Azoulay et al., 2022) and increase the number of establishments (Olney, 2013; Mahajan, 2024), expanding competition and putting further downward pressure on prices.

On the other hand, several countervailing forces can lead to upward price pressure. Immigrants and natives are imperfect substitutes, so immigration raises average native wages, with gains concentrated among more educated workers (Ottaviano and Peri, 2012; Beerli et al., 2021; Terry et al., 2026) and, where natives move into more complex jobs, among less educated workers as well (Foged and Peri, 2016), which raises labor costs. Immigration can also stimulate innovation, though evidence is mixed: while some studies find positive effects on patents, product turnover, and firm performance (Kerr and Lincoln, 2010; Khanna and Lee, 2020; Clemens and Lewis, 2026;

Amuedo-Dorantes et al., 2023), others document negative spillovers in overlapping research fields (Borjas and Doran, 2012). Immigration also raises local innovation (Terry et al., 2026), and ancestry networks attract foreign investment (Burchardi et al., 2019), both of which can raise costs and prices when firms use them to upgrade the quality of their products.

Product Demand Channel On the demand side, immigration increases the local consumer base, creating upward pressure on prices through standard demand-pull mechanisms. This effect is most clearly documented in housing markets (Saiz, 2007), and immigrant spending also raises local demand for services (Hong and McLaren, 2015).

The effect also depends on who the new consumers are. Lach (2007) documents that a large influx of Soviet immigrants to Israel reduced prices, which he interprets as reflecting immigrants' greater price sensitivity and search intensity. New arrivals have lower incomes and no established brand or store loyalties, and the low opportunity cost of their time (Aguiar and Hurst, 2007) makes search cheap, so they have stronger incentives to search for better bargains, increasing effective price competition and reducing markups even as total demand expands.

Compositional shifts also alter the aggregate willingness to pay for quality. Handbury (2021) and Argente and Lee (2021) document that demand for quality is highly income-dependent, creating spatial and demographic variation in price indices. Consequently, a shift in the consumer base can induce a "trading down" effect (Jaimovich et al., 2019), or prompt firms to adjust product appeal to target specific demographic segments (Jaravel, 2019).

3 Data and Empirical Strategy

This section describes our data and empirical approach. We match retail scanner prices to producer locations and instrument local immigration with a shift-share instrument based on historical settlement patterns. The baseline specification in this section is at the county level, and Section 5 extends it to the firm.

3.1 Data

Retail Scanner Prices and Household Shopping Behavior Our primary data come from two NielsenIQ datasets that provide complementary information on prices and consumer behavior for CPG products. The Retail Scanner Dataset (RMS) contains weekly pricing, sales volume, and merchandising conditions from store point-of-sale systems across the United States from 2006 to 2019. The dataset includes approximately 35,000 participating stores in a balanced panel spanning drug stores, grocery stores, and mass merchandise retailers, covering roughly 2,500 counties, about 83% of all U.S. counties. Coverage is substantial: the data capture approximately 53-55% of national sales in food and drug stores and 32% of national sales in mass merchandise stores. Store locations are identified at the county level, which is the finest geographic unit available in the RMS data. The

dataset encompasses 2.6 million universal product codes (UPCs) organized into approximately 1,100 product modules, which aggregate further into 125 product groups covering consumer packaged goods (CPG), such as food, beverages, health and beauty products, and general merchandise. This rich product detail allows us to track prices of identical products across different counties and time periods.

The main unit of observation in our price data is a UPC in a county and year. We obtain it by taking quantity-weighted averages of weekly store prices, first within each store and year and then across stores in a county; modal prices give nearly identical results. Within each county we use only a balanced panel of stores, those present in every year of the 2006–2019 sample, so that store entry and exit do not mechanically drive our price measures. These county-UPC-year prices are the inputs to the price indices described in Section 3.3.

We complement the retail scanner price data with the NielsenIQ Consumer Panel, which provides detailed shopping behavior and demographics for approximately 55,000 households annually from 2004 to 2019. Panel participants record every shopping trip using handheld scanners or mobile apps. For each household-year, we observe the number of shopping trips made, the number of days spent shopping, the number of visits to different retail stores, the store brands purchased, and the purchases made at each retailer. The panel also includes household income, education, household size, race, and ethnicity. To this we add an immigrant-household probability estimated from household demographics and purchase histories, using observed birthplace reports for household heads in the 2008 Tell Us More About You (TUM) survey as training labels (Appendix A.2).[9] These behavioral differences are central to the price-sensitivity mechanism we document in Section 5.

Supplementary Price Data Across Sectors We also assemble price measures for other expenditure categories to test whether immigration's effects extend beyond CPG: county home price indices from CoreLogic and Zillow, county median gross rents from the ACS, and city-level BLS Consumer Price Index data for the 27 largest metro areas (261 counties). These measures have coarser geographic or product detail than the retail scanner data, but they let us gauge how general our findings are across the consumption basket.

Firm Production Locations A key strength of this paper is that we observe where each product is produced as well as where it is sold. We link firms in the retail scanner data to establishment-level information from the National Establishment Time-Series Database (NETS), which provides panel data on employment, sales, and locations for every establishment of private and public businesses across the United States from 2006 to 2018, and links each establishment to the headquarters of the firm that owns it. The link extends the match in Hyun and Kim (2026), built for the Great Recession, to the full 2006–2018 period and to all establishments of each matched firm.

[9] An alternative approach is to directly use households surveyed in the 2008 TUM module, as in McCully et al. (2025). Since our analysis focuses on new immigration inflows over 2006–2019, however, this would restrict the sample to households present in 2008 and miss immigrants who arrived in subsequent years.

The matching procedure uses business names and addresses from the GS1 Company Database, which maintains the registry of UPC codes and their associated manufacturers. GS1 is the global standards organization that administers the UPC system and assigns unique identifiers to products and companies. We use a multi-step algorithm to match GS1 companies to NETS firms, identified by their headquarters records. First, we match entities with identical names, states, and cities, which yields high-confidence matches for firms with unique names in specific locations. For remaining firms with the same name and state but different cities, often large national companies with multiple facilities, we use the Google Places API to verify matches based on common URL appearances in search results. Specifically, if a search for the firm name, city, and state returns a common root domain in the top results for both the GS1 and NETS entries, we classify these as successful matches. We manually review a random sample of 500 matches to validate the algorithm and find a success rate of 94%, suggesting that the procedure produces reliable linkages.

Once a GS1 company is matched to a NETS firm, we bring in all of the establishments that NETS assigns to that firm, together with the county and employment of each. The matched data thus let us observe, for each product sold in the retail scanner data, the firm that produces it, its industry (SIC code), the county of every establishment the firm operates, the number of workers at each establishment, and the counties where the product is sold. Appendix A.1 provides additional detail on the matching algorithm, its validation, and how each firm's establishments are identified.

Immigration Data We construct county-level immigration following Terry et al. (2026), using IPUMS microdata. For the historical periods behind the instrument we use the 1880, 1900–1930, and 1970–2000 census samples. For the main period we use the ACS, assigning each vintage to the immigration it best captures: the 2010 ACS for immigration through 2005, the 2014 ACS for 2005–2010, the 2018 ACS for 2010–2014, and the 2022 ACS for 2014–2018. We use later vintages because IPUMS pools five years of surveys after 2000, so the 2010 ACS alone would undercount recent arrivals. The 2022 ACS was the latest available at the data collection stage, which is why the sample ends in 2018.

Immigration is defined as the total number of foreign-born respondents by birthplace. We construct immigration flows (i.e. *new* immigrants) for multiple time periods using reported year of immigration. Ancestry is measured from respondents' reported ancestral origins, with country classifications harmonized across time using extended crosswalks based on Burchardi et al. (2019). We scale individual responses to county-level totals using IPUMS person weights and harmonize county codes to a consistent 1990 classification, yielding a balanced panel of 3,141 counties across all sample years. The ACS also reports immigrant characteristics such as education and income, which we use to examine heterogeneous effects by immigrant type.

Supplementary County-Level Data The Quarterly Census of Employment and Wages (QCEW) from the Bureau of Labor Statistics provides county-level annual data on average wages (deflated

by the PCE index), total employment, and number of establishments from 2006-2018. We obtain county GDP and population from the Bureau of Economic Analysis Regional Data and metropolitan status from the USDA Economic Research Service. These data provide controls for local economic conditions that could correlate with both immigration and prices, and let us examine whether immigration affects local outcomes such as wages and employment.

3.2 Baseline Empirical Specification

Our baseline specification examines how changes in county-level prices respond to immigration inflows:

$$\Delta \ln P_{dt} = \delta_t + \delta_s + \gamma I_{dt} + X'_{dt}\beta + \varepsilon_{dt}, \tag{3.1}$$

where $\Delta \ln P_{dt}$ is the log change in the price index for county d over period t, I_{dt} is the total number of new immigrants arriving in county d during period t, δ_t and δ_s are period and state fixed effects, and X_{dt} includes initial county characteristics interacted with period dummies: the urban indicator, GDP per capita, and the employment-to-population ratio. The coefficient γ captures the causal effect of immigration on local price inflation. We construct an instrument for I_{dt} to address endogeneity, discussed below.

We estimate this specification using a stacked long-difference approach covering three periods: 2006-2010, 2010-2014, and 2014-2018. We also report robustness checks using the full long-difference period (2006-2018). Pooling across periods focuses on medium-term adjustments, mitigates high-frequency noise in annual price changes, and allows time for firm-level adjustment. State and period fixed effects control for time-invariant state characteristics and common national shocks, isolating variation in immigration exposure across counties that deviates from national trends.

We estimate equation (3.1) separately for different price indices, using NielsenIQ CPG as our primary focus, validating it with the BLS non-durables index, and supplementing with durable goods, services, housing purchase prices, and housing rents, to analyze immigration's overall price impact across the consumption basket as fully as the data permit.

3.3 Construction of the CPG Price Index

We build a price index from the barcode data that is consistent with nested CES preferences and accounts for consumer substitution across products and firms. The index follows the exact price index literature (Sato, 1976; Vartia, 1976; Diewert, 1976) and covers continuing varieties only. We summarize the construction here and give the full detail in Appendix C.2. In Section 6, we extend this index with product variety and quality adjustments in a full structural demand model.

Let P_{udt} be the price of product (UPC) u in county d at time t, where u is made by firm f and belongs to product group g. We build the county index in three nested stages, each a weighted average of the price changes one level below. The first stage aggregates products within a firm,

product group, and county:

$$\Delta \ln P_{fgdt} = \sum_{u} w_{udt} \, \Delta \ln P_{udt}. \tag{3.2}$$

The second and third stages aggregate firms within a product group and county, and then product groups within a county:

$$\Delta \ln P_{dt} = \sum_{g} w_{gdt} \sum_{f} w_{fgdt} \, \Delta \ln P_{fgdt}. \tag{3.3}$$

We compute the index for each pair of consecutive years and chain the annual changes, so that a four-year change is the difference in the chained log index. Each sum includes only products, firms, and groups present in both years, so that entry and exit do not mechanically drive the measure. The weights w_{udt} and w_{fgdt} are Sato-Vartia weights, the logarithmic mean of each product's or firm's expenditure shares in the two years, which are exact for CES. The weights w_{gdt} are Törnqvist weights, the arithmetic mean of each group's expenditure shares in the two years, which give the Cobb-Douglas index when those shares are constant. In both cases, items with larger expenditure shares receive more weight. The baseline keeps only products sold in all fourteen years of 2006–2019 in a county; results are similar when products sold in at least two, six, or ten of those years are included (Appendix Table B.20).

The log change $\Delta \ln P_{dt}$ is the outcome in equation (3.1). By construction, it holds the set of products, firms, and groups fixed, so the price declines we document in Section 4 cannot come from immigrants buying different, cheaper products. That composition margin, along with product variety, is measured separately in Section 6. Appendix Table B.8 shows the results are robust to Jevons, Laspeyres, Paasche, and Törnqvist alternatives, and Figure B.2 shows the national aggregate of our index tracks the BLS food price index closely.

3.4 The Instrument

The immigration measure I_{dt} in equation (3.1) is likely endogenous. Immigrants select into counties on economic opportunities, wages, housing costs, and amenities, any of which also affect local prices. If immigrants are drawn to booming counties with rising prices, OLS is biased upward. If they sort into cheaper, declining areas, OLS is biased downward. Either way, isolating the causal effect requires an instrument.

We use the predicted-ancestry shift-share instrument of Terry et al. (2026), who refine the canonical instrument of Card (2001), and extend it through 2018. The idea is to predict where immigrants settle from more than a century of settlement history, going back to 1880, interacted with contemporary origin-country outflows. We summarize the instrument here; Appendix B.2 gives the full construction and works through an example.

Our instrument for immigration to county d in period t interacts two components and sums across origin countries o:

$$Z_{dt} = \sum_{o} \hat{b}_t \left(\hat{A}_{od,t-1} \times \tilde{I}_{o,-r(d),t} \right). \tag{3.4}$$

The first component, predicted ancestry $\hat{A}_{od,t-1}$, is a forecast of how many residents of origin-o descent live in county d. It uses only push-pull variation from 1880 up to the preceding period: in each earlier period, immigration from origin o to the United States outside d's census division (push) is interacted with the share of that period's European immigrants, an *unrelated* group, who settled in d (pull). Each push-pull term is residualized against the fixed effects and controls of the ancestry regression before it enters the forecast (Appendix B.2, equations (B.2) and (B.3)). The second component, the shifter $\tilde{I}_{o,-r(d),t}$, is contemporary immigration from origin o to census divisions other than d's, scaled to correct for differences in division size. The weights $\hat{b}_t$ come from the first stage that maps these interactions into actual immigration (Appendix equation (B.4)). Neither component uses immigration from origin o to county d's own division, and the only county-level information is where Europeans settled in earlier periods, so their interaction predicts where new immigrants arrive without using d's current economy.

The exclusion restriction is that these historical settlement forces and out-of-region contemporary flows affect current county prices only through current immigration. This is plausible because the economic, transportation, and institutional factors that shaped settlement, many of them more than a century ago, are unlikely to bear directly on 2006–2018 CPG prices once we condition on broad geographic fixed effects, and because the contemporary shifter uses immigration to other census divisions. We assess this restriction in Section 4.

Implementation and first-stage validation. We construct the instrument for the three periods in our sample, 2006–2010, 2010–2014, and 2014–2018, using only non-European origin countries, since European settlement is used to measure destination attractiveness. Non-European origins account for about 90 percent of immigrant inflows in our sample periods, so this is the policy-relevant margin. Predicted ancestry draws on historical migration back to 1880, while the first stage that produces the $\hat{b}_t$ is estimated over nine periods spanning 1980–2018 (Appendix B.2). Appendix Table B.7 reports first-stage estimates that closely replicate Terry et al. (2026) for overlapping years and extend them through 2018. Our constructed instruments correlate at 0.997 with theirs over the common sample, and the excluded-instrument F-statistic is 74 in our preferred specification, well above weak-instrument thresholds.

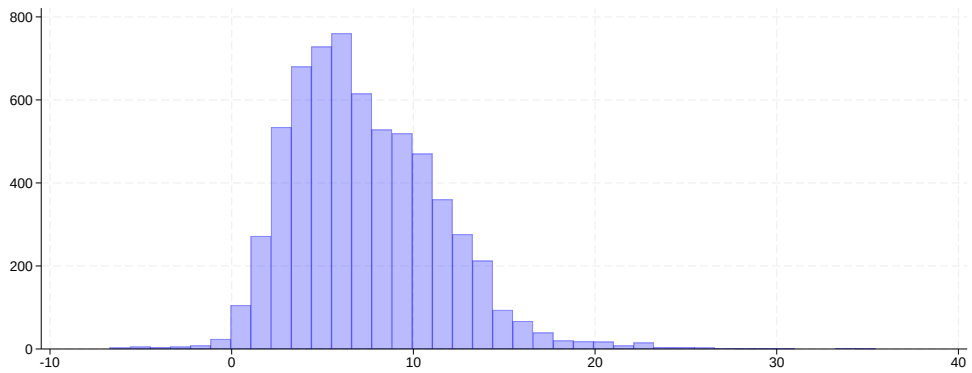
Finally, the predicted-ancestry approach produces immigration shocks in levels rather than population shares (Cortes, 2008; Peri and Sparber, 2009). Appendix Table B.11 shows similar estimates when immigration is measured relative to initial population and when we control for initial population interacted with period.

3.5 Summary Statistics

This section describes the main outcome and explanatory variables at the county level; Appendix Table B.6 reports summary statistics for all variables in the county and firm samples.

Price Dispersion Across Counties Figure 1 presents the distribution of county-level price changes for CPG products across four-year periods from 2006–2018, using our Sato-Vartia price index. Price changes vary considerably across counties. The median county experienced price increases of about 6.7% over four-year periods, and the interquartile range spans 5.6 percentage points, from 4.3% at the 25th percentile to 10% at the 75th, more than twice as high.

Figure 1: Distribution of County-Level Price Changes, Four-Year Periods 2006-2018



Notes: Histogram shows the unweighted distribution of price changes across counties over four-year periods from 2006-2018, measured using the Sato-Vartia price index for CPG products as described in Section 3.3. Each observation is a county-period (N=6,405). The price index is the baseline Sato-Vartia index over continuing products, holding the product set fixed while allowing expenditure shares to vary. Product entry and exit are handled separately via the Feenstra variety correction (Section 6).

This spatial price variation is essential for our analyses. If purchasing power parity held for these goods, local immigration could not affect local prices, because any gap would be arbitraged away through trade. In practice prices differ across locations for several reasons: firms price-discriminate across markets (Simonovska, 2015), local promotions and competition vary, and transportation and supply-chain costs create wedges. Because our index aggregates across products and firms, it reflects these differences in pricing across manufacturers and retail formats.[10]

Immigration Inflows Immigrant settlement is uneven across counties: the average county received 1,690 immigrants per four-year period, with a standard deviation of 4,970.[11] The distribution is skewed: the median county received 170 immigrants and the county at the 75th percentile 625. Predicted inflows are highest in the Southwest, California, and Florida but appear throughout the country and vary widely across counties within the same state (Appendix Figure B.4).

[10] Most US food, drug, and mass-merchandise chains set nearly uniform prices across their stores (DellaVigna and Gentzkow, 2019), but our index aggregates across manufacturers and retail formats, so it still captures cross-market variation. We return to the retail dimension in Section 5.

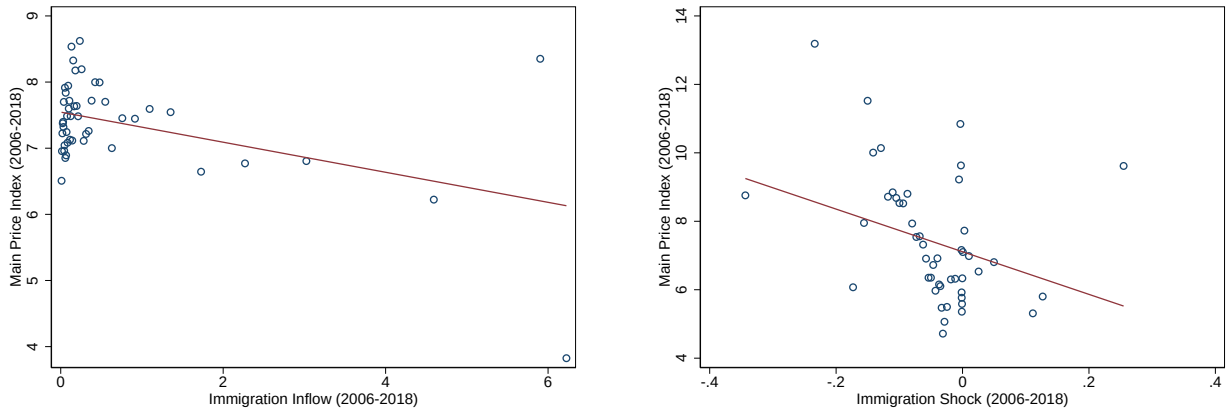
[11] These numbers use the 1% winsorization applied in the county-level analysis; weighted by initial sales, the standard deviation is about 12,500, comparable to Terry et al. (2026). The firm-level analysis uses unwinsorized inflows, since aggregating over a firm's counties already smooths extreme local shocks.

4 County-Level Results

This section shows that immigration lowers CPG prices at the county level, a decline that holds in every sample we examine, and then asks (i) where the effect is concentrated, (ii) how broad it is, and (iii) how other margins respond. The decline is driven by inflows of low-income, less-educated immigrants and is larger for products bought by low-income households. It extends to non-durable goods more broadly, such that the regional cost of living falls despite modest rent increases. Among other margins, immigration raises employment and the number of establishments without lowering wages, and quantity rises, though significantly only for private-label goods. These patterns are consistent with the arrival of price-sensitive consumers, a possibility Section 5 examines at the firm level, where demand and supply exposure can be measured separately.

4.1 Immigration and CPG Prices

Figure 2: Immigration and Price Changes Across Counties, 2006-2018



(a) OLS: Actual Immigration

(b) Reduced Form: Immigration Shock

Notes: Scatter plots show the relationship between CPG price changes (y-axis) and immigration measures (x-axis) at the county level, pooling the three stacked four-year periods (2006–2010, 2010–2014, 2014–2018). Panel (a) uses actual immigration inflows I_{dt}, while panel (b) uses predicted immigration shocks Z_{dt} from equation (3.4). Price changes are measured using the Sato-Vartia index described in Section 3.3. The immigration measures are winsorized at the 5% level within each period to reduce the influence of outliers. County-period observations are grouped into 50 equal-sized bins based on their immigration measure. Each point represents the mean price change and mean immigration level within that bin, without weights. Fitted lines are from bivariate regressions without controls or fixed effects.

Figure 2 shows the negative relationship between immigration and price changes in the raw data. Both panels plot county-level four-year price changes over the three periods, without controls or fixed effects. Against actual immigrant inflows (left panel), the relationship is weakly negative. Against the predicted inflow Z_{dt} (right panel), which uses only historical settlement patterns, it is clearly negative. The contrast hints that immigrants' location choices may attenuate OLS estimates.

Table 2 confirms the pattern in Figure 2. The OLS estimates in columns (1)-(3) are negative and modest, and the IV estimates in columns (4)-(6) are substantially larger, consistent with immigrants

Table 2: Immigration and CPG Prices: Baseline Results

	Δ $P^{SV,Stacked}$ (2006-2018)					
	(1)	(2)	(3)	(4)	(5)	(6)
$Immigr_{d,t}$	-0.037***	-0.025***	-0.026***	-0.070***	-0.067***	-0.066***
	(0.009)	(0.004)	(0.005)	(0.013)	(0.012)	(0.011)
Obs.	6,405	6,405	6,405	6,405	6,405	6,405
First-Stage F-stat				251.0	98.3	74.0
Fixed Effects	Time	State, Time	State, Time	Time	State, Time	State, Time
Method	OLS	OLS	OLS	IV	IV	IV
Controls			Yes			Yes

Notes: Table reports regressions of log changes in county-level CPG price indices on immigration, estimated using equation (3.1). The price index uses Sato-Vartia weights. Columns (1)-(3) report OLS estimates; columns (4)-(6) report IV estimates instrumenting immigration with predicted shocks from equation (3.4). Immigration is winsorized at top and bottom 1%. All specifications use stacked four-year differences (2006-2010, 2010-2014, 2014-2018). Column (1) includes only period fixed effects. Column (2) adds state and period fixed effects. Column (3) adds interactions of initial county GDP per capita, the employment-to-population ratio, and urban indicator with period dummies. Columns (4)-(6) follow the same structure with IV estimation. All columns include sales weights based on the CPG data. Standard errors clustered by state. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

selecting into counties with stronger economic conditions that would otherwise experience higher inflation, which biases OLS toward zero.

The estimated effect is economically large. Our preferred specification in column (6), with state and period fixed effects and controls for initial county characteristics interacted with period dummies, gives a coefficient of -0.066: an inflow of 10,000 immigrants lowers four-year CPG price growth by about 0.66 percentage points.[12] Relative to mean four-year CPG inflation of 7.34% in our sample, this is a reduction of about 9% ($\approx 0.66/7.34$). In proportional terms, the effect is comparable to the 8% effect on wage growth that Terry et al. (2026) find with the same instrument. For consumer packaged goods, the price response is thus about as large in proportional terms as the much-studied wage response, suggesting that studies that look only at nominal wages likely understate immigration's effect on real wages.

The price effect remains negative and significant across a wide range of specifications. In Table 2, this holds as we add state fixed effects and county-level controls in columns (4)-(6). Appendix Table B.8 shows that Jevons, Laspeyres, Paasche, and Törnqvist indices all yield statistically significant negative effects. Tables B.9–B.10 present further robustness checks, including long-difference specifications, controls for Mexican border proximity and local ICE enforcement, a test for anticipation and pre-trends through a one-period immigration lead, state-by-period fixed effects, removal of winsorization, and county fixed effects. Finally, Table B.11 shows similar results with initial population controls, including initial population interacted with period, controls for native population growth (Lach, 2007), and an instrument normalized by initial county population. The positive coefficient on native population growth is consistent with population and immigration

[12] See the discussion of Table B.11 in the Appendix for a comparison with existing estimates. Our estimates are within the range of existing studies finding negative price effects of immigration, though direct comparison is difficult given differences in settings, sectors, and specifications.

operating through distinct channels, as discussed in Section 7.

Shift-Share Diagnostics and Placebo Tests We report the standard diagnostics for shift-share designs. First, we compute the standard error on our preferred IV estimate using the Adão et al. (2019) method, capturing the shared exposure to a small set of origin shocks. The AKM standard error is somewhat above our baseline standard error (0.014 versus 0.011), remaining significant at the 1 percent level.[13] We also run permutation tests that randomly reassign the instrument across county-period observations. The permuted instrument has little effect on prices on average, and it rarely produces a reduced-form estimate as strong as the actual one (Appendix Table B.12).[14]

Second, following Goldsmith-Pinkham et al. (2020), we examine how much the estimate depends on individual origins. Table B.13 drops in succession each of the five origins with the largest Rotemberg weights from the instrument (Panel A), and also uses each origin alone (Panel B). The relationship is similar across origins.[15] Finally, in Appendix Table B.14 we report a placebo test that confirms that contemporaneous migrations have no effect on past inflation.

4.2 Heterogeneous Effects by Immigrant and Product Characteristics

We next examine heterogeneity in these effects along two dimensions: the characteristics of the immigrants and the characteristics of the products consumed.

Immigrant Characteristics The disinflationary effect is concentrated in places with lower-income, less-educated immigrants. We restrict the analysis to immigrants aged 25 and over to ensure accurate measurement of education and occupation, with the baseline result robust to this restriction (Table 3, Panel A, column 1). Column (2) examines this by interacting total immigration with the average years of education of incoming immigrants following Terry et al. (2026), finding that the disinflationary effect is weaker among counties receiving more educated immigrants, although the interaction is not statistically significant ($p = 0.12$). Columns (3) and (4) pursue an alternative approach, constructing separate instruments for distinct immigrant subgroups. Column (3) distinguishes immigrants by educational attainment: less-educated immigrants generate substantial price reductions, while more-educated immigrants show no significant effect. Column (4) performs a similar decomposition by occupation-based income. Because occupation is observed for fewer immigrants, the estimates are less precise: the coefficient is negative for lower-income immigrants and positive for high-income immigrants, though neither is statistically significant.

[13] We apply AKM to the stacked specification in column (6) of Table 2. Following their method, we decompose the instrument in equation (3.4) into the contribution of each origin, $\hat{b}_t \hat{A}_{od,t-1} \times \tilde{I}_{o,-r(d),t}$, and allow the shifters of each origin to be correlated across regions and periods. We replicate the coefficient of -0.066 and compute the AKM variance from the 2SLS residuals and each origin's contribution to the instrument, clustering by origin.

[14] Furthermore, we build placebo instruments from predicted-ancestry shares and random origin-period and division-period shocks, and regress actual non-European immigration on them. Across 1,000 simulations, the test rejects at the 5% level in 3.5% of draws.

[15] In Figure B.3 we also present the share of immigrants to the US across 9 major origins in our sample period. Mexico's share has decreased in the recent years, but note our IV is a multi-year weighted Bartik.

Table 3: Heterogeneous Effects by Immigrant Characteristics

	Δ P$^{\text{Sato-Vartia,Stacked}}$ (2006-2018)			
	(1)	(2)	(3)	(4)
Panel A: Heterogeneity by Immigrant Characteristics				
$\text{Immigr}_{d,t}$ (over25)	-0.117***	-0.084***		
	(0.021)	(0.027)		
$\text{Immigr}_{d,t} \times \text{EducYears}_{d,t}$		0.027		
		(0.017)		
$\text{Immigr}_{d,t}$ (No HS Degree)			-0.052**	
			(0.025)	
$\text{Immigr}_{d,t}$ (Some College)			0.014	
			(0.013)	
$\text{Immigr}_{d,t}$ (Low Inc. Occ.)				-0.043
				(0.030)
$\text{Immigr}_{d,t}$ (High Inc. Occ.)				0.050
				(0.056)
Obs.	6,405	6,405	6,405	6,405
First-Stage F-stat	74.1	5.9 / 5.7	52.1	15.7
Fixed Effects	State, Time	State, Time	State, Time	State, Time

Panel B: Heterogeneity by Product Basket	Low-Inc Goods	High-Inc Goods	High-Immigrant	Low Immigrant
	(1)	(2)	(3)	(4)
$\text{Immigr}_{d,t}$ (over25)	-0.137***	-0.098***	-0.127***	-0.116***
	(0.030)	(0.017)	(0.046)	(0.021)
Obs.	6,405	6,405	6,366	6,366
First-Stage F-stat	74.1	74.1	74.1	74.1
Fixed Effects	State, Time	State, Time	State, Time	State, Time

Notes: This table investigates heterogeneous effects of immigration on CPG prices across two dimensions: immigrant characteristics and product market segments. **Panel A** estimates effects by immigrant characteristics. Column (1) restricts the sample to immigrants aged 25+ since these are more relevant for educational attainment (though results are consistent throughout). Column (2) interacts immigration (over 25) with average years of education, following Terry et al. (2026)'s IV construction using the predicted inflows from each of their top-20 origin countries as instruments. For this column, the reported First-Stage F-statistics are the Montiel Olea–Pflueger effective F-statistics for the two endogenous regressors, each computed after partialling out the other regressor's first-stage fitted value. EducYears is the average years of education of non-European immigrants aged 25 and over, demeaned once over all county-periods, so that zero represents the average years of education among incoming immigrants. The interaction is the product of immigration and demeaned education, winsorized at 1% by period. Columns (3)-(4) construct separate instruments for mutually exclusive immigrant subgroups. Column (3) distinguishes immigrants without high school degrees vs. those with some college education. Column (4) distinguishes low-income (occupation score < 26) vs. high-income immigrants. **Panel B** examines heterogeneity by product type, constructing separate Sato-Vartia price indices for subsets of products. Columns (1) and (2) estimate the effect of total immigration among those aged 25 and over on separate price indices for products with customer income scores at or below and above the median, respectively. In columns (3) and (4), we use price indices for immigrant-intensive goods and for other goods. Immigrant-intensive goods are barcodes whose adjusted immigrant intensity exceeds the 90th percentile across scored barcodes. Appendix A.2 describes the household classification, expenditure weights, and adjustment used to construct these scores. These columns use the 2,122 counties for which the price index of immigrant-intensive goods can be constructed; column (4) is estimated on the same sample for comparability. All specifications are weighted by CPG sales, with state and period fixed effects and county controls. Immigration is winsorized at 1% in Panel A, columns (1) and (2), and in Panel B. Standard errors clustered by state. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Product Characteristics The price decline benefits lower-income households in particular. Panel B of Table 3 classifies products by who buys them in the NielsenIQ Consumer Panel. In columns (1) and (2), products are grouped by the average household income of their buyers: immigration lowers the prices of both groups, but more for low-income products (the difference has a p-value of 0.078).[16] Columns (3) and (4) compare immigrant-intensive products with the remaining products, using the adjusted expenditure-based classification described in the appendix (Appendix A.2). Prices fall in

[16] Consistent with the inflation inequality literature (Jaravel, 2019; Argente and Lee, 2021), we find that inflation in our low-income price index is about 40% higher on average than the high-income index throughout the sample period.

both groups.[17] Together, these results suggest that the gains follow buyers' income rather than their immigrant status, accruing to lower-income households, including native ones, rather than to immigrants alone.[18]

4.3 Effects Beyond the CPG Sector

Next, we examine how prices respond in other sectors of the consumption basket, using the sector price indices in Table 4. The response can differ between sectors because the labor supply and product demand channels do not necessarily have the same strength everywhere. For instance, an inflow of workers lowers wages in nontradable sectors but not in tradable ones (Dustmann and Glitz, 2015), while price responses may be dampened in tradable sectors because producers sell their extra output in other regions (Burstein et al., 2020). The sparse empirical evidence is mostly in nontradables, where lower wages reduce the prices of immigrant-intensive services (Cortes, 2008), while higher demand for housing raises rents (Saiz, 2007). The decline in CPG prices despite a weak supply channel in tradables points to the demand side, which Section 5 tests directly.

Table 4: Immigration Effects on Prices Across Sectors

	$\Delta \ln P^{\text{Dur}}$	$\Delta \ln P^{\text{Non-dur}}$	$\Delta \ln P^{\text{Svc}}$	$\Delta \ln P^{\text{Rent,BLS}}$	$\Delta \ln P^{\text{Rent,ACS}}$		$\Delta \ln HP^{\text{CL}}$	$\Delta \ln HP^{\text{Z}}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\text{Immigr}_{d,t}$	-0.006	-0.069***	-0.025	-0.038	0.063**	0.011	-0.184	-0.192
	(0.017)	(0.020)	(0.022)	(0.040)	(0.024)	(0.036)	(0.120)	(0.134)
Obs.	783	783	783	783	6,402	699	2,384	4,831
First-Stage F-stat	122.7	122.7	122.7	122.7	72.9	119.1	74.1	70.1
Fixed Effects	State, Time	State, Time	State, Time	State, Time	State, Time	State, Time	State, Time	State, Time
Method	IV	IV	IV	IV	IV	IV	IV	IV
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample						Cities		

Notes: Table reports IV estimates of immigration effects on prices across different sectors. Columns (1)-(4) use BLS city-level CPI data for 27 metropolitan areas (261 counties) covering durables, non-durables, services, and rent. Columns (5)-(6) use ACS median gross rent data: column (5) includes all counties, column (6) restricts to the 261 counties with BLS data. Columns (7)-(8) use housing purchase price indices: column (7) uses the CoreLogic index excluding distressed sales (short sales and real-estate-owned sales) (available 2006-2014 only), column (8) uses Zillow data (full period 2006-2018). All specifications instrument immigration with predicted shocks from equation (3.4) and include state and period fixed effects and county controls. All columns include population weights instead of sales weights, since weights on CPG products are not longer relevant. Standard errors clustered by state for ACS and housing price regressions, except column (6); heteroskedasticity-robust standard errors for BLS regressions and column (6) due to small number of states. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Goods and Services Column (2) shows that non-durable goods prices in the BLS data fall by about as much as CPG prices, an external check using official price measures rather than retail

[17] Immigrants do buy immigrant-intensive products disproportionately. In the 2008 Consumer Panel, among barcodes with at least 100 buyers, the immigrant share of spending averages 18.5 percent in the top decile of immigrant intensity, about three times the 6.8 percent elsewhere. Even on these products, non-immigrant households account for more than 80 percent of spending, and barcodes where immigrants account for a majority of spending make up less than 0.1 percent of spending. An inflow of immigrants is therefore unlikely to affect the two groups differently.

[18] Within income groups, immigrant and native households buy similar products. In the 2008 Consumer Panel, among households with incomes below $30,000, low-income products account for 55 percent of spending by immigrants and 60 percent by natives. The low-income immigrants behind the price decline (Panel A) are thus likely to buy what low-income natives buy.

scanner data. Durable goods and services, in columns (1) and (3), show small negative effects that are not statistically significant. Durables are bought infrequently and services are local and hard to compare across providers, which may leave less room for a demand-side effect.

Housing Rents rise with immigration. Using ACS rents across all counties, column (5) finds a positive and significant effect, consistent with higher housing demand. The BLS rent index and the ACS restricted to the 261 counties covered by the BLS, in columns (4) and (6), give smaller and insignificant estimates, suggesting that the rent increase is concentrated outside large metropolitan areas, in line with evidence that the effect of immigration on US shelter prices varies across locations (Cabral and Steingress, 2026). House purchase prices, in columns (7) and (8), show no significant response, consistent with newly arriving immigrants renting rather than buying.

Cost of Living To combine the sector results, we weight them by CPI expenditure shares (U.S. Bureau of Labor Statistics, 2018). An inflow of 10,000 immigrants lowers CPG prices by 0.66 percent (Table 2, column 6) and raises rents by 0.63 percent (Table 4, column 5). CPG products make up about 18 percent of the CPI basket (Leung, 2021), and rent of primary residence about 7 percent (U.S. Bureau of Labor Statistics, 2024). Setting the effects on all other categories to zero, an admittedly crude simplification but consistent with the broad sectors above, the net effect on the local cost of living is about -0.075 percentage points over four years ($= -0.66 \times 0.18 + 0.63 \times 0.07$).[19] This calculation is only approximate, since we do not estimate effects for every expenditure category and our estimates are partial equilibrium, but regressions on aggregate CPI indices also point to a decline.[20]

4.4 Other Margins

Beyond prices, we examine how local economic activity, wages, and quantities respond to immigration.

Local Activity and Wages Immigration expands local economic activity, raising both the number of establishments and total employment (Olney, 2013; Mahajan, 2024). Average wages do not fall in our baseline specification, including retail wages, which account for a significant share of retail costs (Stroebel and Vavra, 2019), and they rise more in counties receiving more educated immigrants, in line with the literature reviewed in Section 2 (Appendix Tables B.17 and B.18).[21] Wage responses

[19] The calculation sets the change in owners' equivalent rent (OER) to zero. The BLS imputes OER from the rents of rental units similar to owner-occupied homes, which are mostly single-family (U.S. Bureau of Labor Statistics, 2024). We find that rents of single-family rental units do not rise with immigration: using the average gross rent of single-family rental units in the ACS, with the specification of column (5) of Table 4, the effect is negative and insignificant (only available for about half of all counties).

[20] Table B.16 reports results for available aggregate inflation indices: the "All Items" CPI using BLS city-level data and the state-level inflation rates provided by Hazell et al. (2022). These provide limited variation, but the direction of aggregate prices in response to immigration is negative.

[21] The average wage effect is positive but not statistically significant, and it is sensitive to specification (Appendix Table B.18). The wage effect moves closer to that of Terry et al. (2026) as our specification and sample approach theirs. The retail wage effect remains close to zero across most specifications.

are a natural way to gauge the cost channel, but a county wage response combines immigrants' labor supply with the labor demand their spending creates, so on its own it cannot isolate how labor costs affect prices, a question we take up at the firm level in Section 5.

CPG Quantities Quantities show how spending adjusts as prices fall. They can rise simply because the market grows with more consumers and workers, and they can also shift toward cheaper products, for instance if new consumers are more price-sensitive. Our price index, which tracks price changes of the same products, does not capture this composition margin; Section 6 studies related changes in the composition of sales through firm-level appeal.

Immigration raises quantity, measured as revenue deflated by our price index, but significantly only for private-label goods,[22] pointing to a reallocation toward cheaper products (Appendix Table B.19). We also measure quantity in the household panel, which covers the discount, independent, and ethnic stores the retail scanner data miss, and find a similar but noisier pattern, likely reflecting its much smaller sample and coarser aggregation.[23] Prices, in contrast, fall in every sample, across data sources, product scope, balance windows, and retailer restrictions (Appendix Table B.20).

5 Firm-Level Analysis

To understand why prices fall, we turn to the level at which prices are set, the firm. Two features make the firm the natural unit. First, within a county, the same immigrant inflow raises product demand and labor supply at once, two forces that can push prices in opposite directions. A firm's customers and its workers are in different places, so the two can be measured separately at the firm level. Second, since a firm sells in many counties, its prices can respond to immigration across all of its markets, while a county regression relates the price in each county only to that county's immigration and so captures only part of this response.

We therefore build two exposures for each firm. The demand exposure averages county immigration shocks over the counties where the firm sells, and the supply exposure averages them over the counties where the firm produces. Measuring the two separately distinguishes the demand channel from the supply channel, subject to caveats we discuss below.

5.1 Firm-Level Specification and Identification

We estimate the following firm-level specification, where a firm's exposure to immigration varies separately across its sales locations (demand) and production locations (supply):

$$\Delta \ln P_{ft} = \beta_1 \mathrm{I}_{ft}^{\text{demand}} + \beta_2 \mathrm{I}_{ft}^{\text{supply}} + \mathbf{X}'_{ft}\gamma + \alpha_{s(f)} + \delta_t + \varepsilon_{ft}, \tag{5.1}$$

[22] Private-label goods are sold under retailers' brands and have no GS1 manufacturer prefix, so we study them at the county and state level.

[23] The measured response may understate the rise in physical quantities. Because the index deflates revenue by a price index over continuing products, shifts toward cheaper or lower-appeal products and changes in product variety can lower measured quantity even when consumers buy more.

where $\Delta \ln P_{ft}$ is a change in the log price index of firm f, $\mathrm{I}_{ft}^{\text{demand}}$ is the immigration inflow in the counties where the firm sells, $\mathrm{I}_{ft}^{\text{supply}}$ is the immigration inflow in the counties where it produces, $\mathbf{X}'_{ft}$ is the set of firm-specific control variables, and we control for industry (3-digit SIC) ($\alpha_{s(f)}$) and period (δ_t) fixed effects. The price index $\Delta \ln P_{ft}$ starts from the same first stage as the county index, equation (3.2). We then average these changes across the product groups the firm sells in each county, and then across the counties where it sells, with Törnqvist weights as in the last stage of the county index (Appendix C.2).

Firm-specific immigration shifters are measured as follows:

$$\mathrm{I}_{ft}^{\text{demand}} = \sum_d \omega_{fd}^{\text{sales}} \mathrm{I}_{dt}, \qquad \mathrm{I}_{ft}^{\text{supply}} = \sum_d \omega_{fd}^{\text{empl}} \mathrm{I}_{dt}, \tag{5.2}$$

where $\omega_{fd}^{\text{sales}}$ is the share of county d in firm f's sales and $\omega_{fd}^{\text{empl}}$ is its share in the firm's employment, both measured at the start of each period. We instrument the two exposures with the same averages of the county instrument Z_{dt} in equation (3.4), replacing I_{dt} with Z_{dt} in equation (5.2). The exposures could be further differentiated by product group through a crosswalk between 4-digit SIC industries and NielsenIQ product groups, but most firms sell in a single product group, so this adds little variation and the results are nearly identical.[24]

The coefficient β_1 is identified by the immigration that arrived in the counties where a firm sells, and β_2 by the immigration that arrived in the counties where it produces. Three features of these exposures shape identification. First, sales are dispersed while production is concentrated: the median firm sells in 18 counties but produces in one,[25] so the demand and supply exposures are far from collinear (their correlation across firms is about 0.4) and are separately identified. Second, both instruments are averages of the county instrument, so they inherit the exogeneity of the county design. Third, the fixed effects in equation (5.1) confine the comparison to firms in the same industry and period.

5.2 Demand vs. Supply Channels

Table 5 reports the effect of immigration on firm-level price changes, with demand and supply exposure entered separately. The outcome is the firm price index of continuing products described above. The exposure weights are rescaled to sum to one over counties with immigration data, and the regressions still control for the share of the firm's sales, and of its employment, in counties without immigration data, for whichever exposures they include. Columns (4)-(6) add interactions of period dummies with the firm's exposure to urban counties, to county GDP per capita, and to the county employment-to-population ratio, so that firms selling in urban and richer counties can

[24] We construct a crosswalk between NETS 4-digit SIC industries and NielsenIQ product groups based on our NielsenIQ-NETS firm-level matching, and verify that results are robust to allowing shocks to vary at the firm-and-group level with group fixed effects.

[25] Appendix Table A.3 reports the full distributions of sales and production. The median firm sells in 3 states and earns a third of its sales in its largest county.

Table 5: Firm-Level Prices: Separate Demand and Supply Side Immigration Exposure

	Δ lnP^{SV,Stack} (2006-2018)					
	(1)	(2)	(3)	(4)	(5)	(6)
Immigration$_{f,t}$ (D)	-0.073***	-0.058***		-0.062***	-0.065***	-0.051***
	(0.020)	(0.022)		(0.017)	(0.018)	(0.018)
Immigration$_{f,t}$ (S)	0.017***		0.007	0.021***	0.021***	0.016***
	(0.005)		(0.005)	(0.005)	(0.005)	(0.005)
Δ lnW (Retail, D)					0.050	0.058
					(0.215)	(0.215)
Δ lnP (Rent, D)					0.173	0.131
					(0.206)	(0.201)
Δ lnW (Retail, S)					0.068	0.067
					(0.070)	(0.070)
Δ lnP (Rent, S)					-0.030	-0.015
					(0.063)	(0.062)
Obs.	11,058	11,058	11,058	11,058	11,058	11,058
First-Stage F-stat	1,267.3	4,462.2	2,460.0	1,911.3	1,060.2	
Fixed Effects	Industry, Time	Industry, Time	Industry, Time	Industry, Time	Industry, Time	Industry, Time
Method	IV	IV	IV	IV	IV	OLS
Controls				Yes	Yes	Yes

Notes: This table reports the effect of immigration on firm-level prices, with demand-side and supply-side immigration exposure entered separately as described in the main text. The price index is the firm index of continuing products described in Section 5. All specifications use stacked four-year differences (2006-2010, 2010-2014, 2014-2018) and include industry (SIC 3-digit) and period fixed effects. The exposure weights are rescaled to sum to one over counties with immigration data; specifications still control for the share of the firm's sales (demand side) and of its employment (supply side) in counties without immigration data, for the side or sides of exposure that they include. Columns (1)-(5) report IV estimates; column (6) reports OLS estimates. Columns (4)-(6) add interactions of period dummies with the firm's exposure to the county urban indicator, GDP per capita, and employment-to-population ratio. Columns (5) and (6) further add retail wages and rents on both the demand and supply side. Observations are weighted by the firm's initial (2006) NielsenIQ sales, held fixed across the three stacked periods. Immigration is measured in thousands. Standard errors are clustered by SIC 3-digit industry. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

follow different price trends in each period.

Column (1) reveals the key finding: demand-side exposure significantly reduces prices, consistent with the county-level results, while supply-side exposure leads to a small but statistically significant price increase.[26] Columns (2) and (3) show that omitting either channel biases the remaining coefficient: omitting supply exposure attenuates the demand-side effect, as the positive supply pressure partially offsets the negative demand effect, while omitting demand exposure attenuates the supply-side effect for the same reason. Column (4) adds firm-level controls and results remain stable. If immigration lowers retail costs in the counties where the firm sells, the price decline would be hard to interpret as a demand-side effect, which is why in Column (5) we further add the changes in retail wages and rents as controls.[27] We caution that immigration itself can change local wages and rents, such that these controls may partly be outcomes of the immigration inflow and thus this is not a clean test. Still, the demand coefficient barely budges when they are included, which suggests that changes in rents or retail costs do not account for the price decline.

These results confirm that the disinflationary effect of immigration operates through the demand channel, consistent with immigrants' greater price sensitivity inducing firms to lower markups. The

[26] That the two exposures are positively correlated across firms yet produce opposite-signed effects is informative. The opposing signs point to two economically distinct channels rather than a common spurious factor.

[27] These are based on firm level exposure: wages and rents in the counties where firms sell and produce.

positive supply-side effect is consistent with firms taking on new tasks—such as innovation and brand building—as immigrants join their workforce, so that consumers value their products more highly and buy more of them. Still, the supply-side channel is quantitatively much less important than the demand channel. To gauge economic magnitude on a common scale, Appendix Table B.21 standardizes both exposures: a one-standard-deviation increase in demand exposure lowers firm prices by 2.8 percentage points, versus 0.9 for supply. Thus, the demand effect is three times the supply effect.

The demand-side result is robust to how we compute standard errors, how we construct the firm price index, the length of the differences, and firms' international activity. First, because the firm-level demand and supply exposures I_{ft}^{demand} and I_{ft}^{supply} are shift-share aggregates of county immigration shocks (equation (5.2)), we report exposure-robust standard errors following Borusyak et al. (2022). They are smaller than our SIC-3 clustered errors in all specifications (Appendix Table B.22). Second, aggregating UPC prices to the firm through alternative chain-weighted indices reproduces the baseline coefficient (Appendix Table B.23), and regressions estimated directly at the barcode level deliver comparable coefficients (Appendix Table B.24). Because these are price changes for identical barcodes, the decline does not reflect shifts in product composition or quality. Third, the results hold with 12-year long differences (Table B.25).[28] Finally, the main coefficients are virtually unchanged when controlling for exporter and importer status, when excluding internationally active firms entirely, and when allowing the effects to differ by international trade status, with no significant difference between international and domestic firms (Table B.26).

Retailers and Other Explanations The prices we observe reflect the decisions of both manufacturers and retailers. Which of the two lowers prices does not matter for our argument, as long as the decline reflects immigrants' demand rather than, for example, lower retail costs. We address this and other retail-side explanations in turn, with details in Appendix B.4. First, immigrants may concentrate at particular chains, or large manufacturers may sell disproportionately through large retailers. We re-estimate the price regression at the firm-retailer level and add demand-side immigration exposures built from the counties where a retailer operates and from the counties where a given firm sells through a given retailer (Table B.27).[29] The firm coefficient is stable throughout, while the coefficient on the retailer exposure is an order of magnitude smaller and the coefficient on the firm-by-retailer exposure is at most marginal. Second, a shock common to a retail chain, such as a change in its pricing policy or its costs, could lower prices. Absorbing retailer-by-period fixed effects, which remove any factor common to a chain in a period, leaves the firm effect essentially unchanged. Third, retail costs may fall in immigrant-heavy counties. Beyond the wage and rent controls in column (5) of Table 5, the demand coefficient is unchanged when these controls are

[28] A firm's exposure can be high in one period and low in another, so in the stacked specification the same firm can serve as more exposed in some comparisons and less exposed in others. The long difference uses each firm's cumulative exposure over 2006–2018, so each firm enters only once.

[29] Each exposure averages county immigration with sales weights, enters alongside the firm's demand-side exposure, and is instrumented by its shift-share counterpart.

measured at the firm-retailer level. Fourth, immigration may attract new stores, and incumbent stores may cut prices when they face more competition. The firm's demand-side exposure is not significantly related to its exposure to retail store entry, and controlling for this exposure, with the caveat that it may itself be an outcome of the immigration inflow, leaves the demand coefficient almost identical (Table B.28).[30] Together, these results suggest that retailer behavior unrelated to immigrants' demand does not account for the price decline.

Two other explanations could link immigration to lower prices. First, local demand may fall, either because more natives leave than immigrants arrive or because immigrants spend less locally than natives, for instance because they save more or send part of their income home. At the county level, controlling for the change in native population, measured over the same period or over the previous one, or instrumenting the former with the latter, leaves the immigration coefficient unchanged (Appendix Table B.11). Immigrant households spend about 88 percent of the average per household member (Appendix E), so their arrival still adds to local demand. Lower demand would also reduce quantities along with prices, but quantities rise instead, significantly so for private-label goods (Section 4.4). Second, firms may overestimate immigrant demand, build up inventories, and cut prices to clear them. Such mistakes would lower prices only temporarily, while the decline persists over 12-year differences at both the county and the firm level (Appendix Tables B.9 and B.25), and future immigration does not predict current price changes (Appendix Table B.10).

5.3 Consumer Search Behavior

Table 6 shows that firms whose sales markets receive more immigrants increasingly sell to households that shop more often and pay lower prices for the same goods. To construct firm-level measures of consumer search behavior, we link each firm to households that purchase from it using the NielsenIQ Consumer Panel. We measure each household's shopping behavior on its purchases of other firms' products to avoid mechanical correlation, and aggregate across households to the firm level, weighting by purchase intensity.

Columns (1) and (2) show that firms whose sales markets receive more immigrants are increasingly linked to households that make more shopping trips and spend more days searching, consistent with a more search-intensive consumer base. Column (3) shows a positive but insignificant increase in the number of stores visited, consistent with cross-retailer search behavior. Column (5) shows an increase in the store brand share, reflecting a shift toward lower-priced alternatives among these consumers. Notably, the number of products purchased (column 4) does not increase, suggesting that the effect is driven by search intensity rather than variety-seeking behavior.

The most direct evidence comes from column (6), which examines the relative prices paid by households linked to each firm for purchases made elsewhere. Following Aguiar and Hurst (2007),

[30] We build the exposure in the same way as the exposure to retail wages and rents, averaging the change in the number of retail establishments per resident from the QCEW across the counties where the firm sells, weighted by the firm's initial sales shares.

Table 6: Firm-Level Search Intensity

	Δ lnPurChar^{SV,Stack} (2006-2018)					
	(1) N. Trips	(2) N. Days	(3) N. Stores	(4) N. Products	(5) % Store Brand	(6) Rel. Price
Immigration$_{f,t}$ (D)	0.013**	0.010**	0.008	-0.002	0.009**	-0.003***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.001)
Immigration$_{f,t}$ (S)	-0.000	0.001	0.000	0.001	0.002	0.000
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.000)
Obs.	10,345	10,345	10,345	10,345	10,345	10,337
First-Stage F-stat	1,263.3	1,263.3	1,263.3	1,263.3	1,263.3	1,263.3
Fixed Effects	Industry, Time	Industry, Time	Industry, Time	Industry, Time	Industry, Time	Industry, Time
Method	IV	IV	IV	IV	IV	IV

Notes: Each observation is a firm × period, on stacked four-year differences (2006–2010, 2010–2014, 2014–2018); the outcomes measure the shopping behavior of the firm's customer base. For each firm we identify the NielsenIQ Consumer Panel households that purchase from it, measure each household's behavior on its purchases of other firms' products to avoid mechanical correlation, and aggregate to the firm level weighted by purchase intensity. The columns are: (1) the number of shopping trips; (2) the number of distinct shopping days; (3) the number of distinct retail chains visited; (4) the number of distinct products purchased; (5) the store-brand (private-label) expenditure share; and (6) the relative price paid, defined following Aguiar and Hurst (2007) as the price paid for a barcode divided by its national average price. All columns are log changes; column (5) is the log change in the store-brand share. The regressors are the firm's demand-side (D) and supply-side (S) immigration exposures, entered jointly and instrumented by their shift-share counterparts as in Table 5. All specifications include industry (3-digit SIC) and period fixed effects and control for the sales- and employment-share of counties with missing shocks on the demand and supply sides. Observations are weighted by initial firm sales, and standard errors are clustered by 3-digit SIC industry. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

we measure relative price as the price paid for a given barcode divided by the national average for that barcode. They show that households that shop more intensively pay less for the same goods, so a lower relative price indicates more search. A negative coefficient indicates that firms whose sales markets receive more immigrants are increasingly linked to households that pay below-average prices for identical products elsewhere, consistent with a shift in firms' consumer bases toward more price-sensitive shoppers. Supply-side exposure shows no significant effects across any outcome, further supporting the demand-side interpretation.

6 The Price Decomposition and Demand Elasticities

This section estimates a structural demand system that nests the price measure used so far. That measure, the price index of continuing products, is the special case with no product entry and exit and with firm appeal held fixed, both of which the demand system relaxes. Building on the search evidence above, the same estimates also show whether immigration makes demand more price-sensitive.

Specifically, we adopt the nested CES demand system of Hottman et al. (2016) and their method for estimating its elasticities. We extend their approach by estimating elasticities separately by state, product group, and time period, rather than for the nation as a whole, and by relating them to immigration inflows. With the estimated elasticities, we decompose the firm price index into a Sato-Vartia index for continuing varieties and a Feenstra (1994) variety adjustment for product entry and exit, and recover firm-level appeal. We sketch the main components here and refer the

reader to Appendix C for full details.

Demand System. Consider a nested demand system in each location, where the upper nest is across product groups, the middle nest is across firms within each product group, and the lower nest is across products within each firm. Household utility is a Cobb-Douglas aggregate of real consumption across product groups:

$$\ln U_{dt} = \int_{g \in \Omega_{dt}} \phi_{gdt}^{C} \ln C_{gdt} \, dg \tag{6.1}$$

where g is a product group, d is a county, t is time, and ϕ_{gdt}^{C} is the Cobb-Douglas expenditure share on product group g. Within each product group, households have nested CES preferences across firms and products:

$$C_{gdt} = \left[\sum_{f \in \Omega_{gdt}} \left(\phi_{fgdt}^{F} C_{fgdt} \right)^{\frac{\sigma_\ell^F - 1}{\sigma_\ell^F}} \right]^{\frac{\sigma_\ell^F}{\sigma_\ell^F - 1}}, \quad C_{fgdt} = \left[\sum_{u \in \Omega_{fgdt}} \left(\phi_{udt}^{U} C_{udt} \right)^{\frac{\sigma_\ell^U - 1}{\sigma_\ell^U}} \right]^{\frac{\sigma_\ell^U}{\sigma_\ell^U - 1}} \tag{6.2}$$

where ϕ_{fgdt}^{F} is the appeal of firm f, σ_ℓ^F is the elasticity of substitution across firms, ϕ_{udt}^{U} is the appeal of product u, and σ_ℓ^U is the elasticity of substitution across products within a firm. The subscript ℓ indexes a state, product group, and time period (2006–2010, 2011–2014, or 2015–2019), so elasticities vary along all three.

Inverting the CES demand for firm f within product group g, firm appeal is the part of the firm's market share that its relative price does not explain:

$$\phi_{fgdt}^{F} = \frac{P_{fgdt}}{P_{gdt}} \left(S_{fgdt} \right)^{\frac{1}{\sigma_\ell^F - 1}} \tag{6.3}$$

where S_{fgdt} is the firm's share of expenditure on product group g in county d and P_{gdt} is the price index of product group g in county d. A firm whose market share is larger than its relative price predicts has higher appeal, measured relative to the average firm in the county and product group.

Estimation. The key parameters to estimate are the demand elasticities σ_ℓ^F and σ_ℓ^U. Following Hottman et al. (2016), who build on Feenstra (1994) and Broda and Weinstein (2006, 2010), we identify them from the covariance between price and market share changes, imposing a supply side as well as a demand side (Online Appendix C.1). Firms set each price as a markup over marginal cost, $MC_u = a_u q_u^{\delta_\ell}$, where δ_ℓ governs returns to scale and is estimated jointly with the elasticities, restricted to be nonnegative. The markup is common across the products of a given firm, canceling out when we double-difference within a firm. The estimation is consistent with monopolistic competition, Bertrand competition with differentiated products, and Cournot competition.

Because we use the same demand system and estimator, our estimates are directly comparable

to those of Hottman et al. (2016), who estimate the same parameters by product group for the country as a whole (Appendix Table B.31). Both elasticities of substitution are higher in our data, 4.3 against 3.9 across firms and 15.6 against 6.9 across products within a firm at the median, as expected when demand is estimated within a state rather than for the nation. The elasticity of marginal cost with respect to output is comparable, 0.11 against 0.16.

Price Index Decomposition and Appeal. We construct three objects: the Sato-Vartia price index of continuing varieties, the variety adjustment for product entry and exit, and the change in firm appeal.[31] The first requires only observed prices and expenditure shares; the other two also require the estimated elasticities.

First, the Sato-Vartia price index of continuing varieties is the first stage of the index in Section 3.3. Writing Φ for a gross change between $t-1$ and t, $\ln \Phi^{SV}_{fgdt}$ is the log change $\Delta \ln P_{fgdt}$ in equation (3.2). Aggregating it across product groups and then counties gives the firm-level price index of Section 5.2, and aggregating it across firms and then product groups gives the county-level index of Section 4.

Second, the variety adjustment captures the contribution of product entry and exit to the cost of living:

$$\Phi^{Variety}_{fgdt} = \left(\frac{\lambda_{fgdt}}{\lambda_{fgd,t-1}} \right)^{\frac{1}{\sigma^U_\ell - 1}} \tag{6.4}$$

where λ_{fgdt} is the expenditure share on continuing products relative to all products. When entering products capture a larger share of period-t sales than exiting products held in period $t-1$, the ratio $\lambda_{fgdt}/\lambda_{fgd,t-1}$ falls below one. Since $\sigma^U_\ell > 1$, this implies $\Phi^{Variety}_{fgdt} < 1$, lowering the effective price index. The intuition is that consumers benefit from the availability of new varieties, even if prices of existing products are unchanged.

Combining these components, the firm-level utility-based price index decomposes as[32]:

$$\frac{P_{fgdt}}{P_{fgd,t-1}} = \Phi^{SV}_{fgdt} \cdot \Phi^{Variety}_{fgdt} \tag{6.5}$$

Third, the appeal index is the growth of firm appeal in equation (6.3):

$$\Phi^{Appeal}_{fgdt} \equiv \frac{\phi^F_{fgdt}}{\phi^F_{fgd,t-1}} = \frac{P_{fgdt}/P_{fgd,t-1}}{P_{gdt}/P_{gd,t-1}} \left(\frac{S_{fgdt}}{S_{fgd,t-1}} \right)^{\frac{1}{\sigma^F_\ell - 1}} \tag{6.6}$$

[31] All three are defined for each firm, product group, and county, and we aggregate them to the firm level in the same way as the firm price index (Appendix C.2).

[32] This decomposition is exact when the appeal of products within a firm is normalized with Sato-Vartia weights, so that the price index of continuing products contains no appeal term (Appendix C). The baseline variety term defines continuing products within each period, as those the firm sells at both its start and its end, so that a product counts as new only in the period in which it is introduced. The results are similar if the variety term instead uses the products sold in every year from 2006 to 2019 as the continuing products, the same set as in the Sato-Vartia index, which makes the decomposition exact.

We compute P_{fgdt} and S_{fgdt} in equation (6.6) over the firm's continuing products, those sold in every year from 2006 to 2019, which is the set used by the Sato-Vartia index (Appendix C.4). Appeal enters the full CES cost-of-living index as a utility shifter. We report it separately from the price index in equation (6.5), which combines the Sato-Vartia index and the variety adjustment.

Table 7: Demand Elasticities

	County-Level		State-Level	
	σ^F	σ^U	σ^F	σ^U
Immigration (100K)	0.053***	0.066***	0.021***	0.025***
	(0.016)	(0.022)	(0.007)	(0.008)
Obs.	422,016	422,016	9,141	9,141
County × product group FE	Yes	Yes		
State × product group FE			Yes	Yes
Period × product group FE	Yes	Yes	Yes	Yes
Median Outcome	4.27	15.67	4.31	15.50

Notes: This table reports IV estimates of the effect of immigration on demand elasticities, instrumenting immigration with the shift-share instrument of Section 3.4. Elasticities are estimated from barcode data by state, product group, and period (2006–2010, 2011–2014, 2015–2019), with sales and quantities aggregated to the state (Appendix C.1); each period's elasticities are matched to the immigration inflow of the corresponding four-year period (2006–2010, 2010–2014, 2014–2018). In columns (1)-(2), each county takes its state's estimate. In columns (3)-(4), immigration is aggregated to the state and the instrument is rebuilt at the state level with the same procedure. The outcome is the log of the estimated elasticity, winsorized at the 97.5th percentile within period, so coefficients are proportional changes. The median outcome is reported in levels. Observations are weighted by initial NielsenIQ sales, and standard errors are clustered by state, the level at which the elasticities are estimated. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Results: Demand Elasticities. We first examine whether immigration makes demand more price-sensitive. Table 7 reports results from regressions of estimated elasticities on immigration exposure. We consider two specifications: (i) county-group-period variation, where counties within the same state share the same elasticity estimate, and (ii) state-group-period variation, where immigration is aggregated to the state level. In the latter case, the instrument for state-wide immigration must be reconstructed; we do so following the same procedure described in Section 3.4 but using state-level immigration and ancestry data (Appendix B.2).

Both specifications show that immigration is associated with significantly higher demand elasticities, for both the elasticity across firms (σ^F) and across products within firms (σ^U). Within a location and product group, demand is more price-sensitive in periods when more immigrants arrive. In a framework where markups fall with the elasticity, as in the pricing structure we estimate, this implies lower markups.

Results: Price and Appeal Table 8 reports how immigration affects the firm-level CES price index, its two components, and firm appeal. Column (1) is the CES price index, which is the sum of the Sato-Vartia index of continuing varieties in column (2) and the variety term in column (3). Column (4) is firm appeal, which we report separately. The variety and appeal terms use the demand

Table 8: Firm-Level Prices: Decomposition of the CES Price Index and Appeal

	(1) CES price index	(2) Components of (1) Sato-Vartia	(3) Components of (1) Variety	(4) Appeal
Panel A: All firms				
$\text{Immigration}_{f,t}$ (D)	-0.062***	-0.062***	-0.000	-0.102***
	(0.018)	(0.017)	(0.003)	(0.025)
$\text{Immigration}_{f,t}$ (S)	0.019***	0.020***	-0.001	0.035***
	(0.005)	(0.004)	(0.001)	(0.012)
Obs.	10,692	10,692	10,692	10,692
Panel B: By customer income				
Low-income customers				
$\text{Immigration}_{f,t}$ (D)	-0.078**	-0.080**	0.001	-0.125***
	(0.036)	(0.035)	(0.003)	(0.041)
$\text{Immigration}_{f,t}$ (S)	0.012	0.013	-0.000	0.029
	(0.010)	(0.010)	(0.001)	(0.019)
Obs.	4,920	4,920	4,920	4,920
High-income customers				
$\text{Immigration}_{f,t}$ (D)	-0.042***	-0.041***	-0.001	-0.103***
	(0.011)	(0.010)	(0.004)	(0.028)
$\text{Immigration}_{f,t}$ (S)	0.016**	0.017***	-0.001	0.030**
	(0.006)	(0.005)	(0.001)	(0.012)
Obs.	5,734	5,734	5,734	5,734
Panel C: By customer nativity				
Immigrant-intensive customers				
$\text{Immigration}_{f,t}$ (D)	-0.046	-0.043	-0.003	-0.289***
	(0.031)	(0.030)	(0.003)	(0.060)
$\text{Immigration}_{f,t}$ (S)	-0.010	-0.011	0.001	0.030
	(0.013)	(0.013)	(0.001)	(0.038)
Obs.	683	683	683	683
Other customers				
$\text{Immigration}_{f,t}$ (D)	-0.056***	-0.056***	-0.000	-0.090***
	(0.018)	(0.018)	(0.003)	(0.025)
$\text{Immigration}_{f,t}$ (S)	0.020***	0.020***	-0.001	0.035***
	(0.005)	(0.004)	(0.001)	(0.013)
Obs.	9,971	9,971	9,971	9,971

Notes: This table reports IV estimates of the effect of immigration on the firm-level CES price index, its two components, and firm appeal. Column (1) is the CES price index (equation (6.5)). Columns (2) and (3) are its components, which sum to column (1): the Sato-Vartia price index of continuing varieties and the variety term for product entry and exit (equation (6.4)), both in logs. Column (4) is firm appeal (equation (6.6)). The variety and appeal terms use demand elasticities that vary across states, product groups, and periods. **Panel A** uses all firms. **Panel B** splits firms at the median of their customer income scores. **Panel C** compares firms whose immigrant intensity scores exceed the 90th percentile across scored firms with the remaining firms. Both firm scores are expenditure-weighted averages of the corresponding continuous barcode scores, constructed as described in Appendix A.2. The classifications are fixed over time. Within each sample, all four columns use the same firm-periods, those for which all four outcomes can be constructed. All specifications use stacked four-year differences (2006–2010, 2010–2014, 2014–2018), include industry (SIC 3-digit) and period fixed effects, and add interactions of period dummies with the firm's exposure to the county urban indicator, GDP per capita, and employment-to-population ratio. The exposure weights are rescaled to sum to one over counties with immigration data; all specifications still control for the share of the firm's sales (demand side) and of its employment (supply side) in counties without immigration data. Observations are weighted by the firm's initial (2006) NielsenIQ sales, held fixed across the three stacked periods. Immigration is measured in thousands. Standard errors clustered by SIC 3-digit industry. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

elasticities estimated above, which vary across states, product groups, and periods. Column (2) of Panel A reproduces the demand- and supply-side price effects of Table 5.[33]

For demand-side exposure, the variety term is small and not significant, so product entry and

[33]The demand-side coefficient happens to equal that in column (4) of Table 5, although the sample is slightly smaller in this table because the variety and appeal terms require estimated demand elasticities, which cannot be estimated for state, product group, and period combinations with too few observations. 3 percent of firm-periods in Table 5 are dropped, although less than 1 percent of sales.

exit contribute little to the price response. Firm appeal falls: the firm's market share falls by more than its relative price would predict. This is consistent with a customer base that shifts spending toward cheaper products: households that buy from more exposed firms spend a larger share on store brands and pay less for identical products in the rest of their shopping (Table 6), and at the county level, immigration raises the quantity of private-label goods (Section 4.4). The decline in appeal is separate from the price index, since it reflects which products consumers buy rather than the prices of the products the firm continues to sell.

Supply-side exposure has the opposite pattern. Besides the price increase of Table 5, appeal rises, while the variety term is again small. Prices and appeal rising together is most consistent with firms investing in stronger brands. Studies of immigrants as workers find that immigrants lead native workers to specialize in other tasks and raise innovation (Peri and Sparber, 2009; Kerr and Lincoln, 2010), and Terry et al. (2026) show that immigration raises patenting in the counties where immigrants settle. We find the firm-level counterpart: firms with greater *supply-side* exposure file more trademarks and patents (Appendix Table B.30). In particular, trademarks identify the source of a good and let consumers form expectations about its quality, which raises their willingness to pay (Alfaro et al., 2026). Because the firm sells many of its products under the same brand, a stronger brand also raises demand for the products the firm continues to sell (Wernerfelt, 1988; Erdem, 1998), and consumers pay more for the same good when it is sold under an established brand (Bronnenberg et al., 2015). Changes in local costs at production locations could also raise prices, for example if immigration increases prices of nontradables. However, controlling for changes in wages and rents in these locations leaves the coefficient unchanged (Table 5), and higher costs alone would not raise appeal.

Panels B and C split firms by the characteristics of their customers, paralleling the county-level product heterogeneity analyses of Table 3. Using the NielsenIQ Consumer Panel, we construct each firm's customer income and immigrant intensity scores by averaging its continuous barcode scores with expenditure weights. Panel B compares firms at or below and above the median customer income score. Panel C compares firms above the 90th percentile of immigrant intensity with the remaining firms. Both cutoffs are calculated across scored firms, and the classifications are fixed over time. The construction is described in Appendix A.2.

The demand-side decline in the Sato-Vartia index is concentrated among firms selling to low-income consumers, where it is about twice as large as among firms selling to high-income consumers.[34] By immigrant intensity, the difference for the Sato-Vartia index is not significant, while appeal falls by more for firms with the most immigrant-intensive customers. These firms account for less than 7 percent of firm-periods, and their price response is similar to that of other firms, even as appeal falls by more. The price declines therefore benefit mainly low-income consumers.

[34] When customer income enters continuously, standardized across firms and interacted with both exposures, the demand-side price decline is significantly smaller for firms whose customers have higher incomes: the interaction is 0.057 (standard error 0.023) for the Sato-Vartia index, per standard deviation of customer income (about $20,000).

7 Theoretical Framework

This section develops a model showing that a single primitive, immigrants' greater price sensitivity, can generate the two demand-side facts of Section 6: immigration in firms' sales markets lowers the price of continuing products, measured by the Sato-Vartia index Φ^{SV}, and lowers firm-level appeal, Φ^{Appeal}. Because immigrants are more price-sensitive than natives, a larger immigrant population compresses markups on continuing products and reallocates the firm's sales toward its cheaper and lower-appeal products. In the model, the two margins work through different elasticities: the markup on continuing products depends on substitution across firms, and the shift in the sales mix on substitution within the firm, the two tiers of the demand system estimated in Section 6.

The model focuses on this demand channel, which dominates in magnitude. To keep it as parsimonious as this system allows, it abstracts from production locations, and hence from the supply-side response that Section 6 interprets as innovation and brand building. The model is meant to show one mechanism transparently rather than to account for all of our results, and it relies on strong assumptions for that purpose, such as two consumer types and fixed variety and appeal. All derivations are in Online Appendix D.

7.1 Environment

Consumers. Preferences are nested CES with the same nesting as equations (6.1) and (6.2) of Section 6, with one such system for each of two consumer types. There are natives (n) and immigrants (i), with populations L_n and L_i. Upper-tier preferences are Cobb-Douglas between an outside good and the differentiated bundle, so a type-h consumer spends a constant fraction α_h of income on differentiated products, an amount we write E_h. Consumers substitute across firms with elasticity σ_h^F and across products within a firm with elasticity σ_h^U, where $\sigma_h^U > \sigma_h^F > 1$. Firm f has appeal ϕ_f and its product u has appeal ϕ_u and price p_u, with ϕ_f and ϕ_u common to the two types, so the types agree on appeal and differ only in how they substitute across prices.[35]

We assume immigrants are more price-sensitive at both margins, $\sigma_i^U > \sigma_n^U$ and $\sigma_i^F > \sigma_n^F$. Our structural estimates are consistent with this assumption, since both elasticities increase with immigration inflows (Table 7).[36] One reason to expect the gap is income. Lower-income consumers are more price-elastic (Handbury, 2021; Faber and Fally, 2022; Auer et al., 2024), and the variation our instrument isolates is non-European by construction (Section 3.4), largely drawn from origins with lower income per capita than the United States. In fact, the disinflationary effect is larger

[35]The appeal parameters here play the role of ϕ^F and ϕ^U of Section 6 but are not the same objects. In the model, appeal enters preferences as a taste weight rather than multiplying quantities as in equation (6.2), so the relation between the two involves the elasticity and is not type-free. Online Appendix D.6 explains this choice, and Online Appendix D sets out how the model relates to the estimated system.

[36]Each type's elasticities are fixed in the model. The estimation of Section 6 fits a single consumer for each product group, state, and period, so it recovers an average of the two types' elasticities weighted by their shares of the market. Among other reasons, this average can rise when immigrants become a larger share, even when neither type's price sensitivity changes. Setting $\sigma_i^U = \sigma_n^U$ and $\sigma_i^F = \sigma_n^F$ collapses the model to the estimated system exactly (Online Appendix D).

among less-educated immigrants and for low-income products (Table 3). A second reason, unrelated to income, is habit: brand preferences form through past consumption and persist after people move (Bronnenberg et al., 2012), so newly arrived immigrants are less attached to established brands. Both reasons imply that immigrants shop around more, since time is cheaper for lower-income households (Aguiar and Hurst, 2007) and weaker brand attachment makes switching less costly. Table 6 supports the search mechanism: firms whose sales markets receive more immigrants increasingly sell to households that shop more often and pay lower prices for the same goods. The results below require only that immigrants are more price-sensitive, not a particular source of this difference.

Firms. Each firm f produces a fixed set of products Ω_f with heterogeneous appeal, with the cost structure of Section 6: product u has marginal cost $MC_u = a_u q_u^{\delta}$, with $\delta \geq 0$ governing returns to scale. The model adds that, within a firm, the cost shifter is a function of appeal, $a_u = a(\phi_u)$ with elasticity $\varepsilon_a \equiv d\ln a / d\ln \phi_u \geq 0$, so that products with higher appeal, which includes quality, have weakly higher unit costs, as in the quality-upgrading literature (Verhoogen, 2008; Kugler and Verhoogen, 2012). Each firm internalizes substitution among its own products but is small relative to the market, so that it takes the aggregate indices P_h and aggregate expenditure as given, as in the standard multiproduct monopolistic competition setting.

We take product variety and appeal as fixed. Two features of our setting motivate this assumption on the demand side. First, we find no variety response on that channel, since the variety term does not respond to demand-side immigration exposure (Table 8). Online Appendix D.5 integrates variety explicitly: markup compression and demand expansion push a product's operating profit in opposite directions—a theoretically ambiguous net effect—consistent with the muted variety response we find. Second, a barcode is a fixed physical product, so its appeal does not change. Any change in a firm's average appeal therefore comes from the composition of what it sells. Because appeal is fixed, $a(\cdot)$ is not a technology for producing appeal but a description of a firm's existing product line: among the products it already sells, those with higher appeal cost more per unit. For the same reason, $a(\phi_u)$ does not change over time and therefore does not affect the double-differenced estimation of Section 6.

Demand. Type-h demand for product u of firm f is

$$c_{uh} = \phi_u \Big(\frac{p_u}{P_{fh}}\Big)^{-\sigma_h^U} \phi_f \Big(\frac{P_{fh}}{P_h}\Big)^{-\sigma_h^F} \frac{E_h}{P_h}, \qquad P_{fh} = \Big(\sum_{v \in \Omega_f} \phi_v p_v^{1-\sigma_h^U}\Big)^{1/(1-\sigma_h^U)}, \tag{7.1}$$

with total demand $q_u = \sum_h L_h c_{uh}$, and P_h the group-level CES price index aggregating the firm indices P_{fh} with weights ϕ_f, as in equation (6.2). A product's demand falls with its price relative to the firm's other products at rate σ_h^U, and with the firm's price index relative to other firms at rate σ_h^F, while appeal shifts demand at both levels. Write $s_{uh|f}$ for the within-firm expenditure share of a type-h consumer, the share of what it spends at firm f that goes to product u. These shares sum

to one over Ω_f:

$$s_{uh|f} = \frac{\phi_u \, p_u^{1-\sigma_h^U}}{\sum_{v \in \Omega_f} \phi_v \, p_v^{1-\sigma_h^U}}. \tag{7.2}$$

The shares in equation (7.2) depend on the type only through σ_h^U, so natives and immigrants divide their spending at firm f differently over the same products at the same prices. That gap between $s_{un|f}$ and $s_{ui|f}$ is the source of the composition mechanism below.

Equilibrium. Firms set prices to maximize profits. Since a firm owns the products that are closest substitutes to each other, raising one price diverts sales only within the firm, putting no pressure on its markup. Online Appendix D.2 shows that the price of product u is then a markup over marginal cost governed by the *across-firm elasticity*:

$$p_u = \frac{\sigma_{f,eff}^F}{\sigma_{f,eff}^F - 1} \cdot MC_u, \qquad MC_u = a_u q_u^{\delta}, \tag{7.3}$$

where

$$\sigma_{f,eff}^F \equiv \sum_h \theta_{fh} \, \sigma_h^F, \qquad \theta_{fh} \equiv \frac{L_h \sum_{u \in \Omega_f} p_u c_{uh}}{\sum_{u \in \Omega_f} p_u q_u}. \tag{7.4}$$

The markup therefore depends on how price-sensitive the firm's customer base is when choosing among firms. Immigration raises θ_{fi}, and so raises $\sigma_{f,eff}^F$ and compresses the markup. With a single consumer type, nested CES delivers a markup common to all of a firm's products (Hottman et al., 2016), as is the case in Section 6. With two types (equation (7.3)), the firm observes the composition of its customers at the firm level, through θ_{fh}, but not for each barcode, and so sets one markup rather than one tailored to the buyer mix of each product. Online Appendix D.2 solves the firm's problem without restricting markups across products and shows that the restriction does not change the direction of the response: in the unrestricted solution, the direct effect of immigration, evaluated at the initial prices, is to lower the markup on every product. The restriction suppresses the dispersion of markups across products, which is bounded by the across-firm elasticity gap.

7.2 Markup and Scale Effects

Immigration affects the price of continuing products through two opposing forces. Measuring the firm's price with the Sato-Vartia index, taking logs of equation (7.3), and differentiating with respect to the immigrant population, holding the Sato-Vartia weights fixed, gives the following decomposition.

Proposition 1 (Direct Price Forces)**.** *Immigration exerts two direct forces on the firm's price:*

$$\left.\frac{d\ln P_f^{SV}}{dL_i}\right|_{direct} = \sum_{u\in\Omega_f} \bar{s}_u \left[\underbrace{-\frac{1}{\sigma^F_{f,eff}(\sigma^F_{f,eff}-1)}\cdot\frac{\partial\sigma^F_{f,eff}}{\partial L_i}}_{Markup\ Effect\ (<0)} + \underbrace{\delta\cdot\frac{\theta_{ui}}{L_i}}_{Scale\ Effect\ (\geq 0)}\right] \tag{7.5}$$

where $\bar{s}_u$ *are Sato-Vartia weights and*

$$\frac{\partial\sigma^F_{f,eff}}{\partial L_i} > 0. \tag{7.6}$$

The proposition reports the direct effect: how immigration changes the right-hand side of equation (7.3), the firm's markup and marginal cost, evaluated at the initial prices. Because the markup and the quantity sold both depend on prices, prices then adjust further in equilibrium. Online Appendix D.3 decomposes the total derivative exactly and, when the two types share the within-firm elasticity, solves it in closed form.

The markup effect comes from immigration raising the effective across-firm elasticity, and its size depends on the gap $\sigma_i^F - \sigma_n^F$. The scale effect goes the other way: immigration raises quantity demanded, which raises marginal cost whenever $\delta > 0$. Which force dominates is a quantitative question. Prices fall when the two types differ enough in price sensitivity relative to how steeply marginal cost rises.[37] Our finding that Φ^{SV} falls with demand-side exposure is consistent with the markup effect dominating in our CPG setting. In Online Appendix E we evaluate the two forces at our estimated elasticities and returns to scale: taking the estimated rise in elasticities as given, markup compression dominates, and the implied price decline is similar in size to the decline we estimate in Table 5.

7.3 Appeal Composition

Immigration changes the appeal composition of what a firm sells, even though no price and no product appeal has changed. The reason is the within-firm margin, $\sigma_i^U > \sigma_n^U$: immigrants and natives split their spending across those products differently, with immigrants preferring more of the cheaper barcodes. As the immigrant share rises, the firm's sales shift toward the products immigrants favor. This shift appears in measured appeal, equation (6.6), which the model summarizes by $\bar{\phi}_f$ below.[38]

Define firm-level appeal as the within-firm expenditure-weighted average of product appeal,

$$\bar{\phi}_f \equiv \sum_{u\in\Omega_f} s_{u|f}\,\phi_u, \tag{7.7}$$

[37] With a common within-firm elasticity, Appendix D.3 states the condition as $(\sigma_i^F - \sigma_n^F)\theta_{fn} > \delta\,\sigma^F_{f,eff}(\sigma^F_{f,eff}-1)$.

[38] The appeal we measure in Section 6 is a residual from a demand system with a single consumer, so, like the estimated elasticities, it changes when the mix of buyers changes even though no primitive, including firm appeal ϕ_f, has changed (Online Appendix D.4).

where $s_{u|f}$ is product u's share of the firm's sales, averaging the two types,

$$s_{u|f} = \frac{L_n E_{fn}\, s_{un|f} + L_i E_{fi}\, s_{ui|f}}{L_n E_{fn} + L_i E_{fi}}, \qquad E_{fh} \equiv \phi_f \Big(\frac{P_{fh}}{P_h}\Big)^{1-\sigma_h^F} E_h, \tag{7.8}$$

with E_{fh} the expenditure of a type-h consumer on firm f.

Proposition 2 (Appeal Composition Effect)**.** *Provided the firm's products are not all of the same appeal and $\varepsilon_a + \delta > 0$, so that products with higher appeal sell at higher prices (Online Appendix Proposition 4), immigration reduces firm-level appeal under the pricing rule (7.3), evaluated at the initial prices:*

$$\frac{\partial \bar{\phi}_f}{\partial L_i} = \sum_{u \in \Omega_f} \frac{\partial s_{u|f}}{\partial L_i} \phi_u < 0. \tag{7.9}$$

The proof is in Online Appendix D.4. At a given price vector, a change in the buyer mix alone reshuffles the firm's sales across its products. Since immigrants substitute more readily on price, a high price costs a product more of immigrants' spending than of natives'. Products with higher appeal have higher prices in equilibrium, so as L_i rises the firm's sales shift toward its cheaper and lower-appeal products and $\bar{\phi}_f$ falls.

In the model, each measured object isolates one margin of immigrants' price sensitivity. The markup compression of Proposition 1 changes prices, but appeal nets out the firm's relative price, so it leaves measured appeal unchanged. The composition shift changes which products are bought, not their prices, so it leaves the Sato-Vartia index of continuing products unchanged. The two declines in Section 6 are separate implications, one of the gap in across-firm elasticities and one of the gap in within-firm elasticities, a separation that follows from the structure of the model.

8 Conclusion

This paper studies how immigration affects consumer prices through demand and supply channels. Linking product prices to the locations where firms sell and where they produce, we find that immigration lowers prices mainly through the demand channel. Immigrants are more price-sensitive than the consumers already in the market: firms whose sales markets receive more immigrants increasingly sell to households that shop more and pay less for the same goods. Their arrival raises the elasticity of the demand that firms face, as our estimated elasticities show, and firms lower their markups while their sales shift toward cheaper products. The price declines are concentrated among firms that sell to low-income households. For supply-side exposure, immigration is associated with higher prices and appeal and with more trademark and patent filings, consistent with firms investing in their brands.

Debates on immigration usually focus on the labor market, where low-income natives are often thought to lose. Our results point to a gain for the same households on the other side of the market. Less-skilled natives are also more likely to favor reducing immigration (Scheve and

Slaughter, 2001; Mayda, 2006). Because these natives tend to have lower incomes, part of the consumer gains we document accrues to a group more likely to oppose immigration. Contrary to the view that immigration raises the prices of consumer goods, it lowers them in our setting by making demand more price-sensitive, even as rents rise in the counties where immigrants arrive. Because our estimates use variation across counties and firms, they capture relative price changes rather than effects common to all markets, and our framework takes the market as given. How firm-level price responses aggregate to economy-wide inflation, and how they interact with monetary policy, remain open questions.

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