Unintended Consequences of Public Procurement: Evidence from the WIC Program*

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Abstract

The Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) provides infant formula to low-income households through state-level exclusive supply contracts that cover roughly half of U.S. infants. Although these contracts directly govern purchases by WIC participants, they also affect households that receive no WIC benefits. We study these spillovers using household-level in-store and online transaction data matched to WIC contract changes across states from 2018 to 2025. When a manufacturer becomes the WIC supplier, its market share among non-WIC households rises by roughly 30 percentage points for formula in sizes provided by the WIC program. The response is concentrated in physical stores, where the new supplier's market share rises by roughly 50 percentage points, while we find little systematic response online. This contrast points to the physical retail environment as a central mechanism. Additional evidence across WIC-authorized and unauthorized retailers shows that the spillover extends beyond retailers directly participating in WIC, including purchase locations where WIC labels are absent. Outside of the retail environment, we find limited evidence that hospital formula practices impact WIC-brand choice. The results show that public procurement can substantially reshape demand among untreated consumers by changing the retail environments in which choices are made.

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Keywords: Public procurement; Demand spillovers; Retail environment; Shelf space; WIC; Infant formula

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1 Introduction

Government subsidy programs are typically designed to affect the consumption behavior of targeted households. However, when such programs are large and interact with manufacturers and retailers, they can reshape the broader consumer choice environment, influencing the behavior of untargeted consumers. In particular, these programs may alter how products are marketed and presented—through changes in product visibility and association with trusted institutions or experts—thereby affecting consumers' consideration sets and perceived product quality. Such spillover effects arise in a wide range of settings, including Medicaid, the Supplemental Nutrition Assistance Program (SNAP), school meal programs, and the 340B Drug Pricing Program.

In this paper, we study "demand" spillovers from the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC), a large federal program that provides free infant formula to low-income households and covers nearly half of all infants born in the United States.[1] Under WIC, each state awards an exclusive contract to a single manufacturer through a procurement auction, granting the winner the exclusive right to supply formula to WIC participants. In 2011, winning this contract led to a dramatic increase in market share from roughly 10% to as much as 90% in a relevant market, as reported by the United States Department of Agriculture (USDA) (Oliveira et al., 2011). While part of this increase is mechanically driven by purchases from WIC participants, a substantial portion reflects a demand spillover: households that are ineligible for WIC shift their purchases toward the contract brand, despite receiving no price subsidy or direct benefit from the program.

We first measure this spillover directly using household-level purchase data from non-WIC households. This isolate demand changes among non-WIC households from mechanically-induced changes driven by WIC participants.

We then examine the mechanisms underlying this spillover. In particular, we study how the WIC program may alter the information and retail environments faced by consumers. When consumers make purchasing decisions—especially for products they buy infrequently or for the first time—they often face incomplete information about product availability and quality, and may therefore rely on cues provided by retailers, institutions, or experts. The WIC program may affect these cues through several channels. First, when a manufacturer wins the WIC contract, the product may become more prominent and visible in stores because WIC-authorized retailers are required to stock

[1] https://www.ers.usda.gov/topics/food-nutrition-assistance/wic-program/

the contract brand and may also adjust shelf allocation, product placement, inventory, or other merchandising decisions to accommodate increased demand from WIC participants. Second, labels identifying WIC-eligible products in WIC-authorized retailers may signal government endorsement or regulatory approval. Third, hospitals may influence early product choices by providing new parents with discharge packs containing formula samples.

To identify the mechanisms underlying these spillovers, we combine multiple data sources that allow us to observe consumer purchases across different marketing environments. Our primary data come from household-level purchase records from Numerator, which track both in-store and online transactions and include detailed information on prices, quantities, retailers, and household demographics. Importantly, we observe whether households use WIC benefits. We merge these data with state-level WIC contract information from 2018–2025, which determines the manufacturer receiving preferential treatment in retail environments. In addition, we incorporate hospital-level data from the CDC's mPINC survey, which provides information on infant formula practices such as formula sample provision and WIC-related support. Throughout the analysis, we focus on households that are income-ineligible for WIC and never use WIC benefits in our transaction data, allowing us to directly measure demand spillovers among households that are not directly treated by the program.

Our empirical strategy exploits institutional features of the WIC program that generate variation in consumers' exposure to different marketing channels. First, WIC contracts change discretely across states and over time, creating plausibly exogenous shifts in which the selected manufacturer receives increased retail prominence. Second, because WIC benefits can be redeemed almost exclusively in physical stores, consumers shopping online are not exposed to in-store marketing inputs such as shelf allocation or WIC labeling. This distinction allows us to compare purchasing behavior across online and offline channels to isolate the role of retail environments in shaping demand.

We further examine how broadly the retail effect propagates by leveraging variation in retailer authorization. WIC-authorized retailers are directly affected by the program through stocking requirements, WIC purchases, and WIC-related labeling, whereas unauthorized retailers are not directly subject to these program-specific retail features. Comparing the two environments allows us to assess whether the spillover is confined to retailers directly participating in WIC or extends more broadly across the retail market. Finally, we use variation in hospital provision of formula discharge packs to examine whether early exposure to formula through hospitals contributes to subsequent demand.

We document three main findings. First, when a manufacturer becomes the WIC supplier, its market share among households ineligible for WIC increases by roughly 30 percentage points for formula in sizes provided by the WIC program, which we refer to as "regular" size and accounts for roughly one-third of total sales. Importantly, within the non-WIC population, consumers who purchase regular-size formula tend to be lower income, less educated, and more likely to be Black, indicating that the spillover disproportionately affects more vulnerable households. The effect emerges immediately after the contract change and remains stable over time. It persists after controlling for prices and restricting attention to large retailers that are more likely to carry all major national brands, suggesting that neither price changes nor differences in product availability fully explain the result. While the aggregate market impact is more moderate, the magnitude of the response within the regular-size segment suggests that changes in the marketing environment can substantially reshape demand in policy-relevant product categories.

Second, market shares respond sharply to WIC contract changes in physical stores, while online responses are less precisely estimated. This contrast is consistent with an important role for the physical retail environment, including product availability, shelf prominence, and WIC labeling. The online response suggests spillovers may also result from influences shared across shopping channels, such as recommendations from pediatricians, friends, or family.

Third, substantial spillovers also arise among households shopping exclusively at unauthorized retailers, where WIC logos are absent, although some pre-contract differences warrant caution in attributing the entire shift to the contract change. Direct exposure to WIC logos at the point of purchase is therefore not necessary for the observed reallocation. Separately, our analysis provides little evidence that hospital provision of formula discharge packs generates substantial subsequent demand for national formula brands. Together, these findings are consistent with a role for broader retail visibility, without separately identifying the contributions of shelf prominence and WIC logos.

Our findings yield both managerial and policy implications. From a managerial perspective, the physical retail environment emerges as a powerful demand lever. Gaining product prominence through public procurement or other institutional channels can generate large and persistent demand spillovers even among consumers who are not directly targeted. Importantly, these spillovers extend to retail environments in which consumers are not directly exposed to WIC labeling, suggesting that the effects of procurement extend beyond institutional endorsement at the point of purchase. Because these effects propagate across retailers , the returns to shelf space investments can be substantially larger than previously measured, benefiting manufacturers even in outlets

where shelf space was not initially expanded. These results suggest that managers should view shelf space not merely as a point-of-sale tactic, but as a strategic tool for shaping demand and building market-wide brand presence.

Our findings have two implications for current policy discussions surrounding the WIC program and the infant formula market. First, they speak directly to concerns about market concentration and supply resilience raised by the Federal Trade Commission (FTC) following the 2022 formula shortage. A recall affecting one manufacturer left retail shelves empty and families desperate, with other manufacturers unable to sufficiently increase production. The FTC investigation sought information about families' experience with fraud, deception, scams, or reselling at exorbitant prices; and retailers' experiences with customer stockpiling.[2] In a concentrated market, a supply disruption affecting a single manufacturer can have substantial consequences for consumers. Our results show that the current WIC procurement system can reinforce this concentration by concentrating purchases among both program participants and non-participants. We also show that retail visibility is an important mechanism to consider in reducing market concentration among program non-participants.

Second, our findings identify specific retail levers through which ongoing reforms to WIC food delivery may affect these broader market consequences. Current efforts to expand online ordering and other alternative shopping arrangements have focused primarily on improving convenience, access, and equity for WIC participants. Our results show that the design of these channels may also affect demand among non-participating households. In particular, because the spillover is strongly associated with the physical retail environment and remains substantial even in stores without WIC labeling, expanding purchasing channels that reduce the contract brand's preferential visibility in physical stores could attenuate the spillover. Conversely, similar effects could emerge online if WIC-designated products receive preferential placement in search results or digital displays. As WIC shopping options are currently being redesigned, our findings therefore identify concrete features of the retail environment that policymakers can consider when evaluating how program delivery affects competition in the broader infant formula market.

Literature Review Our paper contributes to several strands of literature on how policy interventions interact with marketing environments to shape consumer demand and market outcomes. In particular, we relate to work on demand spillovers from government programs, as well as research

[2] `https://www.ftc.gov/system/files/ftc_gov/pdf/infant_formula_report_final.pdf`

on how retail environments and product visibility influence consumer choice.

Our paper contributes to a growing literature on the spillover effects of government programs. While these programs are designed to affect targeted populations, they often generate broader market-wide effects through equilibrium responses of firms and consumers. In the context of government procurement, Duggan and Scott Morton (2006) show that Medicaid procurement affects pharmaceutical prices and market outcomes beyond program beneficiaries. In the context of food assistance, Handbury and Moshary (2021) show that expansions of the National School Lunch Program reduce grocery demand and induce price adjustments by retailers, generating indirect welfare effects for non-participating households. Similarly, Leung and Seo (2023) document that increases in SNAP benefits raise grocery prices, implying that transfer programs can affect non-recipients through price adjustments in retail markets. In healthcare, Baicker et al. (2014) and related work study the broader impacts of Medicaid expansions on labor supply and participation in other social programs, while more recent work documents spillovers to privately insured patients through changes in provider behavior (Lee, 2025). These studies highlight that large-scale government programs can affect untargeted consumers through equilibrium price responses. We make two contributions to this literature. First, we study a setting where changes in market concentration impact untargeted consumers even in the absence of meaningful price changes. By concentrating demand around a small number of firms, procurement may increase consumers' exposure to supply disruptions, such as recalls, pandemic-related production constraints, or trade shocks. Second, we identify retail visibility of the contract brand as a key driver of increased concentration to aid policymakers in improving supply chain resilience and reducing unintended consequences of the program.

Our paper is also related to the literature on shelf space management, where there is mixed evidence on the impact of the number of products on the shelf (facings) and product position. Empirical work generally finds that greater shelf space can increase product sales, although the magnitude of the effect varies substantially across products and retail settings (Chandon et al., 2009; Corstjens and Doyle, 1981; Desmet and Renaudin, 1998; Dreze et al., 1994). Earlier work reports relatively modest average shelf-space elasticities, while more recent studies emphasize substantial heterogeneity and nonlinear effects. In particular, Dreze et al. (1994) find that additional facings have limited effects once a product receives sufficient shelf space, suggesting diminishing marginal returns to further shelf allocation. Chandon et al. (2009), by contrast, show that greater shelf presence can increase consumer attention and purchase intentions, highlighting that shelf space

may affect demand not only through product availability but also through visibility. We make three contributions to this literature. First, we provide evidence from a natural experiment rather than a controlled manipulation of shelf facings, allowing us to study retail visibility at market scale. Second, we show that the demand effects associated with changes in retail visibility are substantially larger than the elasticities reported in the existing shelf-space literature. Third, by comparing online and offline purchases and exploiting variation in WIC authorization, we show that WIC-induced demand shifts are closely associated with the physical retail environment and extend beyond retailers directly participating in the program. Differences between authorized and unauthorized retailers also provide suggestive evidence on additional WIC-specific retail mechanisms, including labeling.

Lastly, this paper adds to the literature on the US infant formula markets and WIC. Using retail scanner data, Oliveira et al. (2011) document that a manufacturer's market share increases substantially after winning a WIC contract. Building on this evidence, Huang and Perloff (2014), Choi et al. (2020), and Rojas and Wei (2019) provide evidence that these market share gains extend beyond WIC participants to non-participating households. More recently, An et al. (2023) develop a structural model of procurement auctions and pricing competition to quantify how WIC contracts affect consumer demand, manufacturer pricing, and government expenditures. We contribute to this literature in three ways. First, using household-level purchase data, we directly measure demand spillovers among non-WIC households rather than inferring them from aggregate scanner data. Second, we provide evidence on the mechanisms underlying these spillovers. We show that WIC-induced demand shifts are strongly associated with the physical retail environment and extend beyond WIC-authorized retailers, indicating that WIC-specific point-of-sale features such as labeling cannot fully account for the spillover. At the same time, the larger response at authorized retailers is consistent with additional WIC-specific mechanisms, potentially including labeling.

The remainder of the paper is structured as follows. Section 2 provides background on the infant formula market and the WIC program, Section 3 introduces the data, Section 4 presents potential mechanisms behind the spillover effects, Section 5 presents the analysis on the discrete choice model, and Section 6 concludes.

2 Background

2.1 Infant Formula Market

Infant formula is an essential product with limited substitutability.[3] Although the American Academy of Pediatricians recommends exclusively breastfeeding infants for the first year of life,[4] many women do not produce enough milk or lack the support needed to breastfeed. 6 month breastfeeding rates are 59% for Asian, 45% for white and Hispanic, and 28% for black mothers.[5] Thus, households that purchase infant formula typically have limited outside options: if breast milk is not available, formula cannot be easily substituted or produced at home because a baby's nutritional needs are highly specific and any contamination or imbalance in vitamins and minerals can cause serious health consequences.[6]

Due to the importance of the product, the infant formula market is heavily regulated. Infant formula falls under section 412 of the Federal Food, Drug, and Cosmetic Act. The act lays out minimum standards for the nutrient content, quantity, and quality of infant formula, along with requirements for recordkeeping and recall practices. The act also requires all infant formula manufacturers to register with the US Food and Drug Administration (FDA). The act also gives the FDA the authority to set and adjust requirements for nutrient quality control standards, submission requirements, labeling, and nutrient specifications.[7] All formula sold legally in the US is reviewed by the FDA, which regularly inspects formula products and the manufacturing facilities where they are made.[8] Heavy regulations that differ from those in other countries and import tariffs have together limited imports of formula into the US.

As a consequence, the infant formula market in the US has been highly concentrated. Only four manufacturers together produce 90% of infant formula sold in the US, and only 2% is imported[9]. Among the four manufacturers, the three companies, Abbott (Similac), Mead Johnson (Enfamil), and Nestle (Good Start), produce national brands, while the fourth one, Perrigo, is the private label manufacturer that produces generic infant formula and its products are sold through all

[3] Commonly available infant formulas contain mostly cow's milk with purified whey and a mixture of vitamins and minerals.

[4] https://www.cdc.gov/nutrition/infantandtoddlernutrition/breastfeeding/recommendations-benefits.html

[5] https://www.ncbi.nlm.nih.gov/pmc/articles/PMC4410446/

[6] https://www.cdc.gov/nutrition/infantandtoddlernutrition/formula-feeding/choosing-an-infant-formula.html

[7] https://www.ibisworld.com/united-states/market-research-reports/infant-formula-manufacturing-industry/

[8] https://www.cdc.gov/nutrition/infantandtoddlernutrition/formula-feeding/choosing-an-infant-formula.html

[9] https://www.theguardian.com/us-news/2022/may/18/baby-formula-shortage-why-is-there-none-what-to-do-causes-explained

major retailers.[10] In November 2022, Perrigo acquired Nestlé's U.S. infant formula manufacturing facility and the U.S. and Canadian rights to the Good Start brand. Good Start continued to be sold as a national brand, while Nestlé continued to fulfill its existing WIC contracts, with Perrigo supplying the product.

Finally, infant formula products are typically available in two different sizes, regular size (12-13 oz) and bulk size (48-50 oz). The three national brands offer both regular and bulk-size products, and Perrigo offers mostly bulk-size products. This distinction is important because policy interventions such as WIC apply only to the regular-size segment, while bulk-size products serve as a natural comparison group in our empirical analysis.

2.2 The WIC Program and Supplier Contracts

The Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) is a federal assistance program administered by the Food and Nutrition Service (FNS) of the United States Department of Agriculture (USDA) that aims to supplement the health of at-risk low-income pregnant, postpartum, and breastfeeding women, infants, and children up to age five.[11] Eligibility for WIC is determined based on three criteria: (i) categorical eligibility (pregnant, postpartum, or breastfeeding women, infants, or young children), (ii) income eligibility, typically defined as household income below 185% of the federal poverty level, and (iii) nutritional risk, as assessed by a healthcare professional. Individuals who participate in certain other assistance programs, such as Medicaid, SNAP, or TANF, are automatically considered income-eligible.

It is the third largest food and nutrition assistance program and served about 6.2 million participants per month in fiscal year 2020, including almost half of all infants born in the United States.[12] The WIC program provides certain foods to eligible participants, including powdered infant formula, infant cereal, and other dietary supplements for children and mothers. Based on estimates by the USDA, in 2017, 2.2 million infants (56 percent of all infants in the United States) were eligible for WIC and 79% of eligible infants participated in the program.[13] WIC participants are given vouchers or EBT cards for eligible products so that they can redeem them at retail stores, who then bill the federal government at their retail price for each redeemed voucher.

This paper focuses on milk-based powder formula, which accounts for 72% of all infant formula

[10] `https://www.perrigopediatrics.com/about/default.aspx`
[11] https://www.fns.usda.gov/wic/about-wic
[12] https://www.ers.usda.gov/topics/food-nutrition-assistance/wic-program/
[13] https://www.fns.usda.gov/wic-2017-eligibility-and-coverage-rates

in dollar sales to WIC and non-WIC consumers.[14] Importantly, WIC benefits apply to standard ("regular-size") formula products that are eligible for voucher redemption, rather than larger bulk-size products. As a result, both the program's direct effects and the commonly cited evidence on market share dynamics—such as the USDA event study discussed below—primarily reflect outcomes within the regular-size segment of the market.

WIC is an expensive program, with annual costs totalling about $6 billion.[15] A large share of these costs is driven by infant formula, which is the single most expensive food category in the program. Since the late 1980s, WIC State agencies have attempted to control costs of infant formula procurement by holding auctions in which infant formula manufacturers offer discounts in return for the exclusive right to supply the state's WIC program.[16]

To determine the brand that supplies WIC, state agencies conduct auctions in which manufacturers bid a rebate paid to the government for each redeemed voucher, along with a wholesale price that puts the rebate into context. The contract is awarded to the manufacturer offering the lowest net price (wholesale price minus rebate). Historically, these auctions have been dominated by a small number of national manufacturers, reflecting the highly concentrated structure of the U.S. infant formula market.

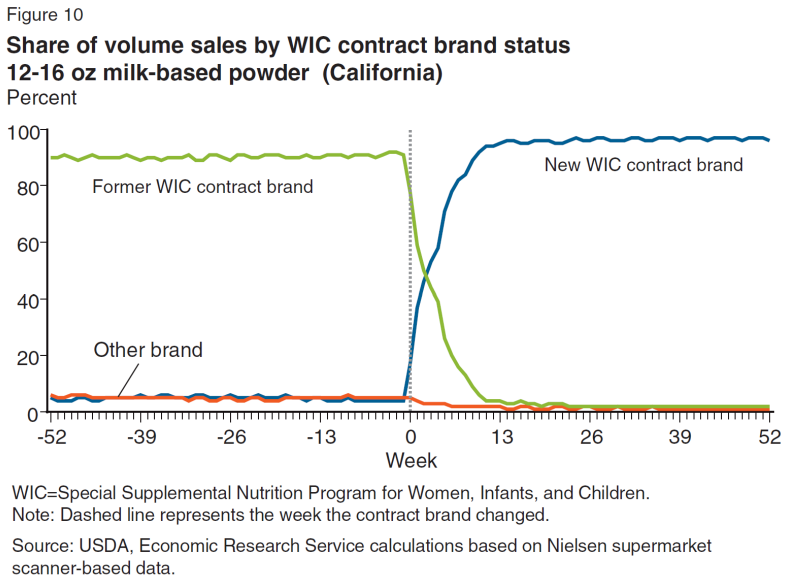
Winning a WIC contract affects a manufacturer's demand beyond the purchases made directly through the program. Following Oliveira et al. (2011), we refer to this increase in demand outside the WIC program as a *demand spillover*. Oliveira et al. distinguish this spillover from the direct effect of the contract, whereby WIC participants switch to the newly designated brand. Using retail scanner data, they show that when the identity of the WIC supplier changes, the new contract manufacturer gains substantial market share, with part of this increase reflecting sales outside the WIC program as reported in Figure 1. It shows that the market share of the WIC brand in the regular-size category rises sharply following a contract change, while the former WIC supplier's share falls. Although much of this shift reflects purchases by WIC participants, the demand spillover implies that households purchasing formula outside the program also shift toward the contract manufacturer.

[14] https://www.ers.usda.gov/amber-waves/2011/september/infant-formula-market/

[15] https://www.cbpp.org/research/food-assistance/wics-competitive-bidding-process-for-infant-formula-is-highly-cost

[16] In contrast, other WIC food items are not procured through such exclusive contracts through auctions.

Figure 1: Event Study by USDA



Source: Oliveira et al. (2011)

2.3 Retailers

Once WIC-participating households receive vouchers or EBT cards from the state agency, they may redeem them only at WIC-authorized retailers. To ensure adequate access for WIC participants, each state WIC agency imposes minimum inventory requirements on authorized retailers, mandating that they carry a minimum quantity of the contract manufacturer's infant formula products. This minimum inventory policy creates strong incentives for offline retailers to allocate scarce shelf space toward the contract brand, as these stores anticipate higher demand from WIC-eligible households. As a result, the increased shelf presence of WIC products may also raise their visibility and salience among non-WIC households.

As of 2025, WIC does not permit fully online transactions in most states. Even in states that allow online ordering, such programs remain largely experimental, and households are typically required to pick up their orders in person at local retail stores. Online shopping does not expose consumers to the physical shelf environment faced in brick-and-mortar stores. Online retailers could nevertheless display WIC-related information or promote particular products digitally. In a descriptive audit conducted in 2026, we find little evidence that the top-displayed infant formula brand systematically follows the state's WIC contract manufacturer; we report this exercise in the Online Appendix. However, a descriptive audit of Walmart, Target, and Amazon using VPN

access from multiple states finds little evidence that the ordering of infant formula search results systematically varies with the state's WIC contract manufacturer. We report this result in the online appendix.

As explained above, when a WIC participant redeems a voucher or an EBT card, the state reimburses the retailer at the prevailing retail price. The state subsequently collects a negotiated rebate from the manufacturer, resulting in a net price to the government that is substantially lower than the shelf price. Because WIC participants face zero marginal cost at the point of sale, this reimbursement structure could, in principle, give retailers incentives to raise the prices of contract brands.

To mitigate such incentives, state agencies impose price regulations for WIC-authorized vendors, known as Maximum Allowable Reimbursement (MAR) rules. These rules limit the reimbursement rate to levels that are not excessively higher than prices paid by non-WIC customers. The MAR is determined relative to a vendor's peer group, with retailers grouped based on characteristics such as size and location. A retailer whose infant formula prices consistently exceed those of comparable peers may face sanctions or disqualification from the WIC program. Hence, retailers do not have full control of retail prices.

2.4 Hospital-provided formula samples

Despite pediatricians' strong recommendations in favor of breastfeeding, hospitals and physicians routinely stock infant formula to ensure that newborns can be fed when medical complications arise or when breastfeeding is insufficient.

When hospitals procure infant formula, they typically do so through Group Purchasing Organizations (GPOs), which negotiate large-scale contracts with manufacturers. Although hospitals are not federally required to stock their state's WIC contract brand, many choose to do so to maintain "continuity of care" for WIC-supported households after hospital discharge. In some cases, this linkage is explicit. For example, the Wisconsin WIC agency requires physicians to recommend the WIC contract brand.

Historically, infant formula manufacturers also distributed free "discharge bags" containing formula samples to new parents at hospitals. While this practice has declined sharply in recent years due to state-level regulations and changes in hospital policies, such samples may have been perceived by parents as implicit endorsements by medical professionals. As of today, company-sponsored infant formula marketing bags have been banned in all hospitals—either through regulation or

universal voluntary compliance—in Delaware, Massachusetts, Maryland, and Rhode Island.[17]

3 Data and Descriptive Statistics

For our empirical analysis, we combine several unique data sets. In particular, we combine household panel data on online and offline purchases with federal poverty guidelines, WIC auction outcomes, and hospital data on infant formula provision and assistance in making WIC appointments.

We use households' online and offline purchases from 2018 to 2025 from Numerator. Numerator is a market research firm that has recruited over two million U.S. households. This firm compiles data on the purchasing habits of its panelists, obtaining half through snapshots of physical receipts, and half through linked loyalty accounts and email scraping. The data include purchases made through both e-commerce and traditional retail channels for home use or consumption, and cover various types of retail stores as well as online purchases (e.g., grocery, mass retailers, club stores, convenience stores, liquor stores, drug stores). The data also include demographic information, including race, income, education, and household size.[18]

To identify households that do not participate in the WIC program in the Numerator data, we observe whether a household uses WIC benefits in its transactions and classify a household as a WIC participant if it uses WIC benefits at least once during the sample period. We then define our main non-WIC sample as households that both never use WIC benefits in any observed transaction and have household income above the WIC income-eligibility threshold. An advantage of the Numerator data is that WIC participation is measured from observed transactions rather than self-reported participation or inferred solely from reported income, as is typical in other widely used household purchase datasets. Combining these two criteria allows us to focus on households that are not directly treated by the WIC program.

To examine purchases by households that do not participate in WIC in states where a new manufacturer replaces the incumbent supplier, we combine information on WIC auction winners with contract start and end dates from 2018 to 2025, collected from state WIC agencies. During our sample period, there are 97 auctions, including 27 cases in which the contract winner changes.

[17] Historically, hospital discharge packs have been sponsored by national infant formula manufacturers rather than private-label producers. Consequently, if hospital exposure were the primary mechanism, it would mainly influence competition among national brands rather than between national and private-label products."

[18] One concern of the period of the data we use is that it includes the period when Abbott's infant formula products were recalled in February 2022. The recall induced a national-level shortage of infant formula products. In the online appendix, we show the results of the empirical analysis excluding the recall period and find that the results are quite robust.

Finally, we obtain hospital formula provision practices from the Centers for Disease Control and Prevention (CDC)'s national survey of Maternity Practices in Infant Nutrition and Care (mPINC). The survey includes de-identified hospital and zip-code level data on all hospitals in U.S. states and territories that provide maternity care services for the years 2018, 2020, and 2022. We exclude 2024, which only includes geographic data at the hospital and state level.

Table 1: Non-WIC Household Characteristics

	N	mean	std	min	25%	50%	75%	max
Trips	15,156	7.1	7.6	2.0	2.0	4.0	8.0	104.0
Brands Purchased	15,156	1.5	0.7	1.0	1.0	1.0	2.0	4.0
Total Price Paid	15,156	321.5	410.3	1.3	92.0	170.7	379.8	13,424.3
Total Price Paid WIC	9,011	214.8	337.7	0.0	50.0	97.6	229.0	8,858.4
Oz	15,156	214.1	278.3	0.0	55.6	111.2	253.0	4,356.1
Oz WIC	9,011	128.6	203.1	0.0	25.0	57.6	136.8	3,398.0
Cans	15,156	10.1	12.7	2.0	3.0	6.0	12.0	334.0
Duration (days)	15,156	287.1	292.0	30.0	98.0	197.0	337.0	2,624.0

Note: Table shows the number of trips taken by each household, as well as cans of formula, ounces of formula, ounces of formula purchased from the WIC supplier, the price paid, and the duration from the first purchase to the last purchase. We restrict the sample to households that are observed for more than 30 days during the data period.
Source: Numerator Data 2018-2025.

Summary Statistics We first examine Numerator panelists who do not participate in WIC, based on the conditions above. Table 1 shows that these households make, on average, 7 trips over 9 months. These households purchase, on average, 10 cans of formula containing 214 ounces of powder and spend $321 on formula over the period in which they are observed. The average number of manufacturers purchased is 1.5.

Table 2 reports manufacturer shares separately for non-WIC households, distinguishing between regular-size products (eligible for WIC redemption) and bulk-size products (ineligible for redemption). Among households who do not participate in WIC, the market structure closely mirrors that of the overall market, with Abbott and Mead Johnson together accounting for roughly 85% of purchases in the regular-size segment and about 60% in the bulk segment. Nestlé plays a relatively minor role, and store brands are largely absent in the regular-size category but account for a substantial share in the bulk segment.

Importantly, the regular-size segment represents a non-trivial share of total purchases even among non-WIC households, both in terms of the number of cans (about 30%) and total quantity purchased (about 20%). This indicates that a substantial portion of ineligible households' demand

occurs in the segment directly affected by WIC contracts. As a result, these households are exposed to the retail environments shaped by the program, despite not receiving any subsidies. Together, these patterns highlight that the regular-size segment, where WIC operates, is both economically meaningful and highly concentrated among national brands, making it a natural setting to study demand spillovers among consumers who are not directly treated by the program.

Table 2: Manufacturer Shares (non-WIC Households)

	Regular		Bulk	
Manufacturer	Cans	Share	Cans	Share
Abbott Laboratories	22,979	0.44	32,906	0.30
Mead Johnson	22,347	0.43	33,785	0.31
Nestle	3,254	0.06	3,402	0.03
Store	3,468	0.07	40,194	0.36

Note: Table shows the number and share of cans that Homescan households purchase from each manufacturer in regular and bulk-size containers. Regular size refers to 12-16 oz and bulk size refers to containers larger than 16 oz.
Source: Numerator Data 2018-2025

Table 3 provides further insight into which households purchase regular-size products by comparing households that purchase only regular-size formula, only bulk-size formula, or both. Households that purchase only regular-size products tend to have lower income and lower levels of education compared to those that purchase only bulk products, and are more likely to be Black.[19] In contrast, households purchasing bulk products tend to be higher income, more educated, and purchase larger quantities per transaction. These differences show that the regular-size segment is disproportionately composed of lower-income and less-educated households, even within our non-WIC sample. Thus, the spillover effects we document are concentrated among households with systematically different socioeconomic characteristics from those purchasing only bulk-size products, highlighting the potential distributional consequences of the WIC-induced demand spillover.

Now, we provide evidence that a manufacturer that wins the WIC auction becomes more attractive even to households that do not participate in the WIC program. We examine states in which a new manufacturer replaces the incumbent WIC manufacturer during 2018-2025.[20] During our sample period, there were 97 auctions, among which 27 of those experience a new winner. Figure 2a shows manufacturers' share of non-WIC households' in-store purchases of regular-size cans that are

[19]The median household income in US is $83,730 in 2024.

[20]States with more than one such contract change are split up into 2 "states", each with a single contract change, with the date of the split as the midway point between the two contract changes.

Table 3: Household Characteristics by Purchasing Method

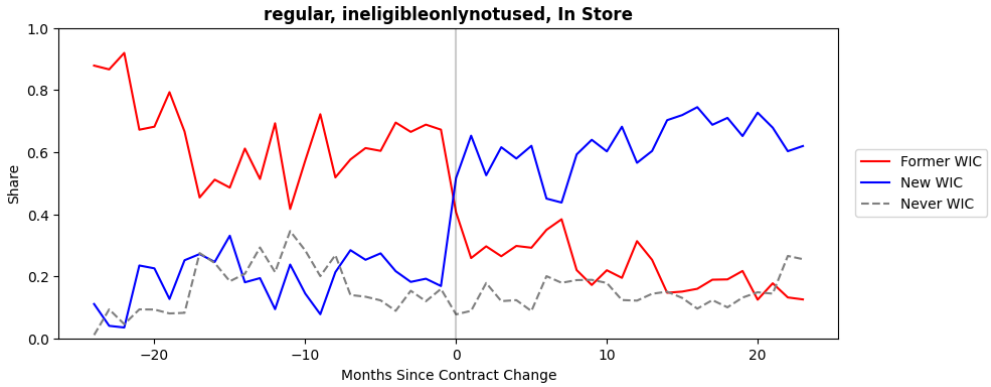
	Only regular	**Both**	**Only bulk**
Income (median)	85,000	85,000	95,000
Household size	3.2132	3.3090	3.2908
College or more	0.7567	0.8109	0.8417
Black	0.0866	0.0729	0.0489
Hispanic	0.0597	0.0798	0.0908
Asian	0.0687	0.0652	0.1146
White	0.7313	0.7486	0.7167
N transactions	9.0030	12.8309	11.1325
Total oz	204.0354	397.8738	446.2901
Oz per transaction	21.3278	30.7426	40.3261
Cans	16.0682	18.6679	15.6220
Duration (days)	229.2552	380.3699	265.3600
N households	337	3,252	2,717

Note: The table reports household characteristics by infant formula package-size purchasing behavior. Households are classified as purchasing only regular-size formula, only bulk-size formula, or both sizes during the sample period. Income is reported in dollars. Education and race variables are indicator means. Total ounces purchased and ounces per transaction are calculated from all infant formula purchases observed in the data. We restrict the sample to households that make more than 5 purchases.

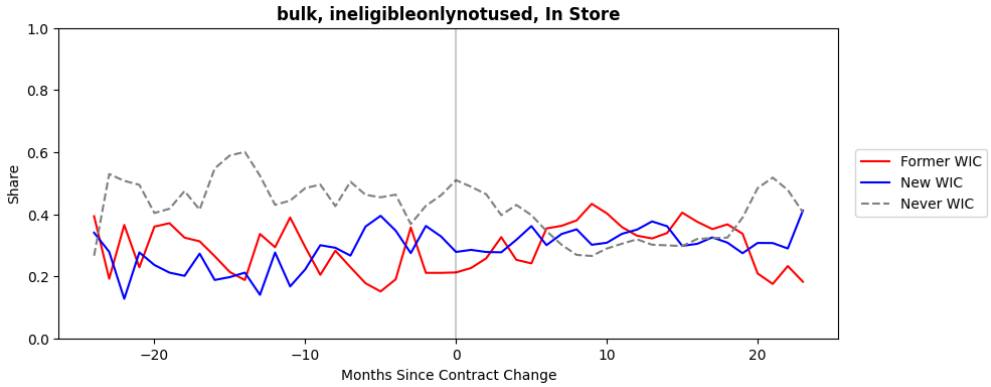
Source: Numerator Data 2018-2025, USDA auction data 2018-2025.

Figure 2: Share of Non-WIC Household Purchases by Manufacturer

(a) Regular Size



(b) Bulk Size



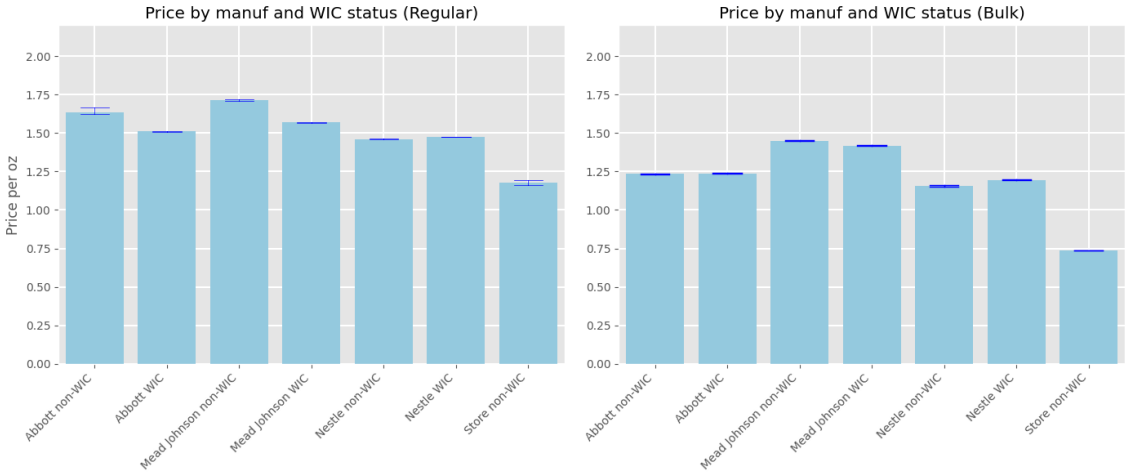
Notes: The figure reports market shares of infant formula purchases among non-WIC households by manufacturer separately for regular-size and bulk-size products. Source: Numerator, USDA auction data 2018-2025.

eligible for redemption in the program. Prior to the contract change, the incumbent WIC supplier generally holds a substantially larger share than the future WIC supplier. Around the contract change, the future WIC supplier's share increases sharply while the former WIC supplier's share declines. The third manufacturer's share shows considerably less systematic movement around the contract change. In contrast, Figure 2b shows that the contract changes have little impact on manufacturers' share of non-WIC households' purchases of bulk-size cans that are ineligible for redemption in the program.

One might wonder if some of the share changes are driven by changes in prices. Figure 3 displays the price paid by Numerator panelists by manufacturer, size, and whether the manufacturer was the WIC supplier. Abbott and Mead Johnson set the highest prices, with Nestle priced lower. Store

brands charge much lower prices than the three premium brands. The Abbott and Mead Johnson products are priced lower and Nestle products are priced slightly higher when the manufacturer wins the WIC contract. This pattern could reflect several forces. On the demand side, increased demand from WIC participants may put upward pressure on prices when Nestlé wins the contract, while retail price caps may constrain price increases for higher-priced premium brands. On the cost side, winning a WIC contract may also affect manufacturers differently through changes in scale and distribution costs, particularly if transportation costs vary with the geographic configuration of production and demand. We therefore do not interpret these descriptive price differences as identifying a particular mechanism. Prices for bulk products are similar regardless of whether a manufacturer holds the WIC contract.

Figure 3: Price by Manufacturer and WIC Status



Note: Panels show price per oz for each manufacturer, for periods where the manufacturer is or is not the WIC supplier. The left panel shows prices for 12-16 oz containers (regular size) and the right panel shows prices for containers above 16 oz (bulk size). 95% confidence intervals are shown.

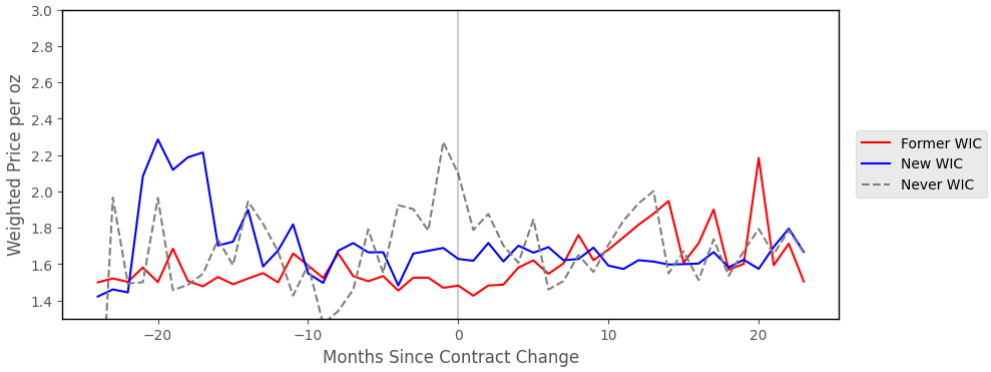
Source: Numerator, auction data provided by USDA, 2018-2025

Figure 4 shows the price trend around the timing of the contract change for the regular-size products. The graph shows that when a manufacturer becomes the new WIC supplier, its prices do not change much, while the incumbent supplier's prices increase a bit after their contract ends, suggesting that the huge market share shift in Figure 2 may not be largely driven by price changes.

Online Transactions One of the features of the Numerator data is that it contains both online and offline transactions. We present several key summary statistics by channel.

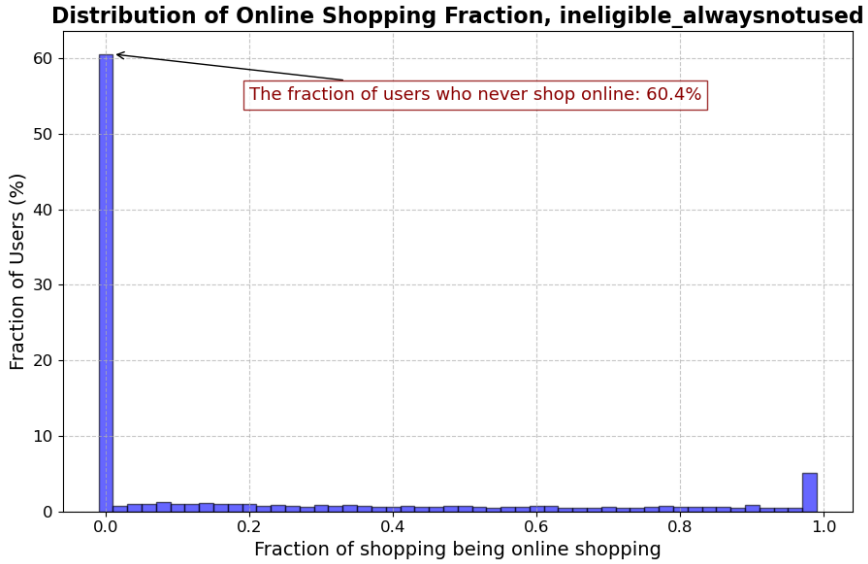
First, Figure 5 displays a histogram that illustrates the proportion of online purchases made

Figure 4: Price of Non-participating Household Purchases by Manufacturer



Note: Figure shows price per ounce for 12-16 oz containers (regular-size) for the incumbent WIC supplier, the new WIC supplier, and the third bidding manufacturer who is neither the incumbent nor the new WIC manufacturer. Source: Numerator, auction data, 2018-2025.

Figure 5: Fraction of Purchases Made Online



Note: Figure shows a histogram of the fraction of purchases made online by each non-WIC household. We keep the sample to the households with more than 5 trips to create the figure.

by each household that is not participating in WIC. Notably, approximately 60% of households have never bought infant formula from online retailers. Additionally, around 10% of households exclusively obtain their infant formula through online channels.

Table 4 compares the demographics of non-WIC households by formula-purchasing channel. Exclusively online shoppers have lower average income and similar education levels compared with exclusively in-store shoppers, with smaller shares of Black, Hispanic, and Asian households. Households using both channels have the highest average income and college share. Table 5 compares the

purchase characteristics of non-WIC households who shop exclusively in-store, exclusively online, and both in-store and online. Online shoppers make fewer purchases over a shorter period of time but spend more on each purchase, and are more likely to purchase bulk-size containers.

Table 4: Comparison of in-store and online purchasers' demographics

Purchase Type	Income	College	Black	Hispanic	Asian
In-store	102,007	0.813	0.068	0.083	0.106
Online	97,861	0.817	0.064	0.053	0.070
Both	103,797	0.828	0.061	0.096	0.057

Note: The table compares the demographics of non-WIC households who shop exclusively in-store, exclusively online, and both in-store and online.
Source: Numerator data 2018-2025.

Table 5: Comparison of in-store and online purchasers' purchases

Purchase Type	Purchases	Total Spent	Duration (days)	Bulk
In-store	9.35	428.26	305.23	0.76
Online	9.87	520.17	229.17	0.80
Both	14.15	681.63	350.67	0.80

Note: The table compares the purchases of non-WIC households who shop exclusively in-store, exclusively online, and both in-store and online.
Source: Numerator data 2018-2025.

4 Mechanism

Our identification proceeds in three steps. First, we use changes in the identity of the WIC contract manufacturer across states and over time to identify the demand spillover among non-WIC households. Second, we compare responses across in-store and online purchases, and across regular- and bulk-size products, to distinguish mechanisms specific to the physical retail environment from broader changes in brand demand. Third, we exploit differences between WIC-authorized and unauthorized retailers, together with variation in hospital formula practices, to distinguish retail visibility from WIC labeling and hospital exposure. We discuss the identifying assumptions underlying each comparison below and report a series of robustness checks in the Online Appendix.

Retail Channel One potential mechanism behind the demand spillover is the retail environment associated with the WIC program. When a manufacturer becomes the WIC supplier, retailers

may respond in several ways, including allocating greater shelf prominence to the contract brand, displaying WIC-related logos or labels, adjusting inventory allocation, or otherwise increasing the product's visibility in stores. Households that do not participate in WIC may interpret these retail cues as signals of product quality, popularity, or institutional endorsement. Figure 6 presents several examples of WIC-related logos displayed on shelves in grocery stores. Consumers can easily observe these visual cues, which may influence purchase decisions even among households that do not directly receive WIC benefits.[21]

One challenge in studying retail mechanisms is that there are no readily available large-scale data on shelf-space allocation, product placement, displays, or inventory management across retailers and states over time. As a result, directly observing how retailers adjust in-store merchandising in response to WIC contract changes is difficult. Accordingly, our empirical analysis focuses on identifying the broader role of physical retail exposure and in-store visibility mechanisms rather than separately estimating the effect of any individual retail input. Because shelf allocation is one of the most salient and economically important components of the in-store retail environment, we occasionally refer to these broader retail visibility mechanisms as "shelf-space effects" for expositional convenience, while recognizing that the data do not separately identify individual merchandising inputs.[22]

Our approach to investigating the potential mechanism behind demand spillover hinges on the fact that WIC typically does not permit online ordering and transactions. Although the USDA is considering proposals to eliminate barriers to online ordering and internet-based transactions in WIC, as of 2025, only a few states and supermarkets have begun to experiment with online ordering and widespread implementation of this policy across all states is expected to take some time. Consequently, online shoppers are not exposed to the physical shelf environment faced by in-store shoppers, although they may still encounter digital WIC-related information or product promotion. Comparing responses to WIC contract changes across online and in-store purchases therefore allows us to assess whether responses are stronger in the physical retail environment, rather than treating online shopping as a completely unexposed benchmark. This comparison relies on the assumption that differential responses across channels are not driven by systematic

[21] For instance, Luffarelli et al. (2019), Cian et al. (2014) study the effects of logo design on consumer behavior and perception about the brands.

[22] Throughout the paper, we occasionally use the terms shelf space, retail visibility, retail exposure, and retail environment interchangeably. Our empirical design identifies the broader retail channel through which WIC contracts affect consumer demand, including product prominence, shelf allocation, inventory requirements, and other in-store merchandising practices. Because we do not directly observe these individual inputs, the analysis does not separately identify the contribution of each component.

Figure 6: WIC Logos in Grocery Stores





Note: *Source:* Panel A: Business Insider (2020), "Foods like peanut butter, cheese, and eggs might have a 'WIC' label in your grocery store." Panel B: PHFE WIC (2025), "Need WIC Shopping Help? Send Someone You Trust."

differences in the composition of online and in-store shoppers or by state-specific online promotion of WIC brands. We provide complementary evidence on these concerns in Appendix D.4 and the second by examining online product displays across states in Appendix F.[23]

To quantify the impacts of WIC status on market shares by channel, we consider event study models to illustrate how WIC contract changes affect market shares in in-store markets and online markets. In particular, we estimate the following regression for in-store market shares and online market shares.

$$share_{i,s,t} = \sum_{j=-m}^{m} \gamma_{i,j} D_{i,s,t-j} + \alpha_i + \mu_s + \delta_t + \epsilon_{i,s,t}, \tag{1}$$

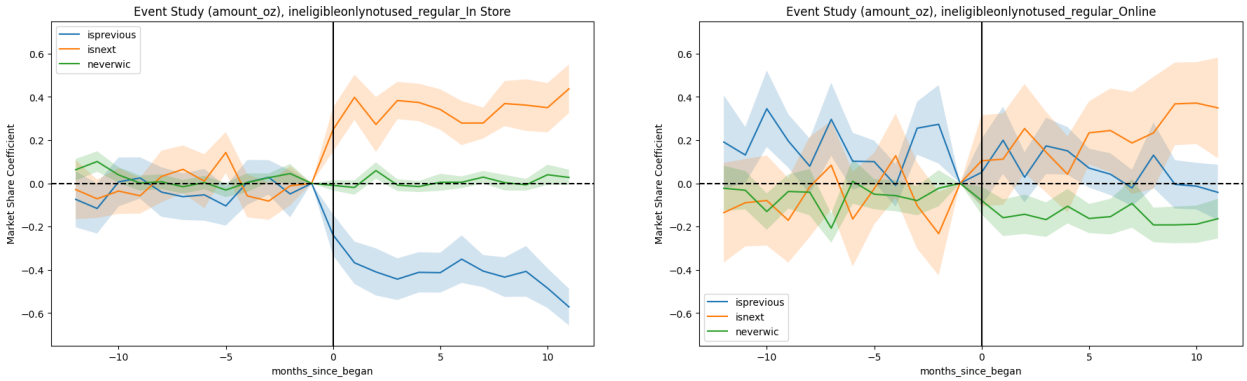
where $share_{i,s,t}$ is the market share of manufacturer type i in state s at month t. There are three types of manufacturers ($i \in \{\text{previous, next, never}\}$). If $i =$ previous, then the manufacturer holds the WIC contract before the contract change, if $i =$ next, the manufacturer holds the new WIC contract, and if $i =$ never, then the manufacturer does not hold the WIC contract. Next, $D_{i,s,t-j}$ is a dummy variable that indicates for event month j. That is, the event ("a contract change") occurs j months before this observation's calendar time. The coefficients after the event has occurred ($\gamma_{i,j}$ for $j \geq 0$) capture the dynamic effects of the treatment. The standard errors are clustered at the state level.[24]

Figures 7 report the estimated coefficients, where for panel (a), we use the in-store market

[23]We also examined advertising spending and found that it is mostly national, with little local variation. Local advertising is therefore unlikely to explain the demand shifts around state-level WIC contract changes.

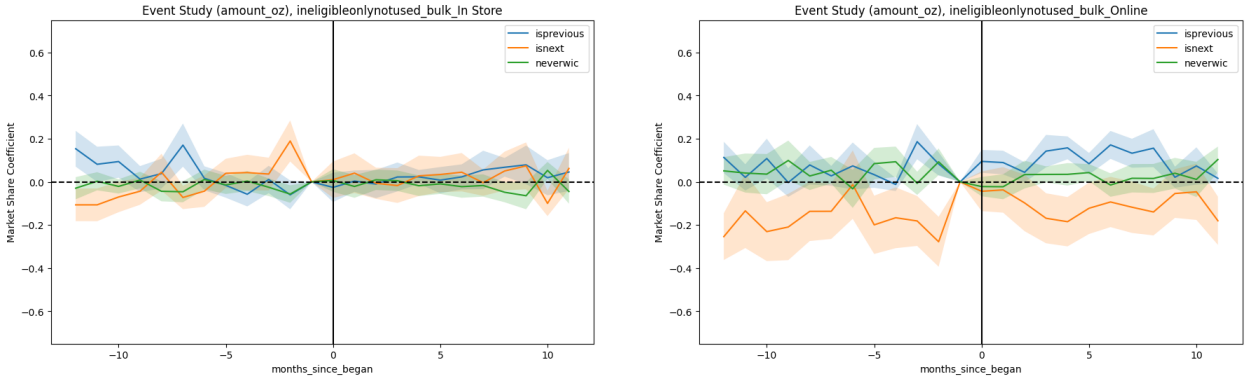
[24]The results are robust to the inclusion of the manufacturer-state fixed effects, the manufacturer-time fixed effects.

Figure 7: Event Study Results: In-store vs. Online, Regular Size



Note: Panel (a) shows the event study coefficients for the in-store market shares, while Panel (b) shows online market shares. Source: Numerator, auction data from USDA, 2018-2025.

Figure 8: Event Study Results: In-store vs. Online, Bulk Size



Note: Panel (a) shows the event study coefficients for the in-store market shares, while Panel (b) shows online market shares. Source: Numerator, auction data from USDA, 2018-2025.

shares, while for panel (b), we use the online market shares. We find that the in-store market share of the previous contract holder decreases by 50%, while the market share of the new contract holder increases by 50% when the contract changes. The market share of the manufacturer that does not have a WIC contract before and after the contract change remains the same. By contrast, the online estimates are substantially noisier and smaller than the in-store estimates, particularly in the months immediately following the contract change. However, the online panel does not provide a clean null: the new supplier's share increases in some later post-contract months, and the pre-contract estimates are also imprecise. We therefore interpret the comparison as evidence that the response is stronger and more immediate in physical stores, while allowing for delayed spillovers that may operate across both channels.

Since bulk-size products are not eligible for WIC, consumers who consider only bulk-size products are not exposed to WIC logos in retail stores or online. Indeed, Figure 8 shows that following WIC contract, there is no discernible change in bulk purchasing patterns either in retail stores or online. Figure 8 reports the estimated coefficients of equation (1) where the market shares are calculated within the bulk size category. As in Figure 7, panel (a) uses the in-store market shares while panel (b) uses the online market shares. Both figures illustrate that market shares do not respond to the contract change in both categories.

Because equation 1 pools contract changes that occur in different states at different times, two-way fixed-effects estimates can be biased when treatment effects vary across cohorts or over time. We therefore re-estimate the event study using the Callaway and Sant'Anna (2021) estimator, which compares treated states only with states whose contracts have not (yet) changed. The post-contract estimates are similar to those in Figure 7, although some differences emerge in the pre-contract period. Overall, the post-contract pattern is robust to using an estimator that accounts for staggered treatment timing. Detailed results are reported in the Online Appendix.

WIC Authorization and Retail Spillovers We next examine whether the retail effects of WIC contracts are confined to retailers that participate directly in the WIC program or propagate more broadly across the retail market. This distinction is important for both policy and management because it determines whether the spillover operates primarily through retail features directly tied to WIC participation or extends to retail environments outside the program. The comparison also provides suggestive evidence about the role of WIC-specific retail mechanisms, including WIC labeling.

To examine these mechanisms, we exploit the fact that only WIC-authorized retailers are permitted to display WIC logos, whereas unauthorized retailers are not. Households that shop exclusively at WIC-authorized retailers are therefore exposed to WIC-specific retail features, including WIC logos and minimum stocking requirements, as well as any accompanying changes in retail visibility. In contrast, households that shop exclusively at unauthorized retailers make their observed formula purchases at locations where WIC logos are absent and WIC-specific stocking requirements do not apply, although they may still experience changes in retail visibility. Unauthorized retailers may adjust shelf allocation or product prominence in response to broader changes in demand for the WIC brand following a contract change. Consequently, comparing these two shopping environments allows us to assess whether demand spillovers are confined to retailers directly participating in WIC

Table 6: WIC Authorized Stores

Year	#Stores	#WIC-Authorized	#WIC-Unauthorized	Fraction WIC-Authorized
Panel (A): All Stores				
2018	54,153	19,622	34,531	0.36
2019	54,313	20,283	34,030	0.37
2020	51,403	17,517	33,886	0.34
2021	55,469	19,903	35,566	0.36
2022	65,574	25,698	39,876	0.40
2023	64,384	25,111	39,273	0.39
2024	62,855	24,270	38,585	0.38
2025	35,477	13,893	21,584	0.39
Panel (B): Numerator Sample Stores				
2018	18,384	8,629	9,755	0.47
2019	19,958	9,183	10,775	0.46
2020	19,391	8,930	10,461	0.46
2021	20,140	9,716	10,424	0.48
2022	22,946	11,390	11,556	0.50
2023	22,847	10,837	12,010	0.47
2024	22,575	10,464	12,111	0.46
2025	15,720	7,540	8,180	0.48

Note: The table reports the annual number of retailers observed in the purchase data and the number that are authorized to accept WIC benefits. WIC authorization status is obtained from state WIC agency retailer lists. The final column reports the fraction of retailers that are WIC-authorized in each year.
Source: USDA and Numerator data 2018–2025.

or propagate more broadly across the retail market. Differences between the two environments may also reflect additional mechanisms specific to authorized retailers, including WIC labeling.

We identify WIC-authorized retailers using administrative data from the USDA, which directly records store-level authorization status.[25] Table 6 reports the number of WIC-authorized and unauthorized retailers both in the full USDA retailer universe and in the subset of stores observed in the Numerator data. In the full sample of retailers, roughly 35–40% of stores are WIC-authorized throughout the sample period. In the Numerator sample, the fraction is somewhat higher, around 45–50%, reflecting the fact that larger grocery retailers are more likely to both participate in WIC and appear in the transaction data.

We first examine where non-WIC households shop. A substantial share (35%) of non-WIC

[25]We geocode the addresses in the USDA list of WIC-authorized vendors and match each store in the Numerator data to the nearest listed vendor. A purchase is coded as made at a WIC-authorized store if that vendor is within 1 km, shares a word in its business name with the Numerator retailer, and is authorized in the fiscal year of the purchase.

households shop exclusively at WIC-unauthorized retailers, while a smaller group shops exclusively at WIC-authorized retailers (17%). Hence, there is substantial variation in the extent to which non-WIC households are exposed to WIC-related retail environments.

We also compare purchasing behavior between households who shop exclusively at WIC-authorized retailers and those who shop exclusively at non-WIC retailers. We find that households shopping exclusively at WIC-authorized retailers are more likely to purchase WIC-brand products. Although these distributions alone are not sufficient to establish causal effects, the patterns suggest that differences between WIC-authorized and unauthorized retailers—such as the presence of WIC logos or differences in retail visibility—may influence household purchase behavior.

To examine these mechanisms, we exploit the fact that only WIC-authorized retailers are permitted to display WIC logos, whereas unauthorized retailers are not. Households that shop exclusively at WIC-authorized retailers are therefore exposed to WIC-specific retail features, including WIC logos and minimum stocking requirements, as well as any accompanying changes in retail visibility. In contrast, households that shop exclusively at unauthorized retailers make their observed formula purchases at locations where WIC logos are absent and WIC-specific stocking requirements do not apply, although they may still experience changes in retail visibility. Unauthorized retailers may adjust shelf allocation or product prominence in response to broader changes in demand for the WIC brand following a contract change. Consequently, comparing these two shopping environments allows us to assess whether demand spillovers are confined to retailers directly participating in WIC or propagate more broadly across the retail market. Differences between the two environments may also reflect additional mechanisms specific to authorized retailers, including WIC labeling.

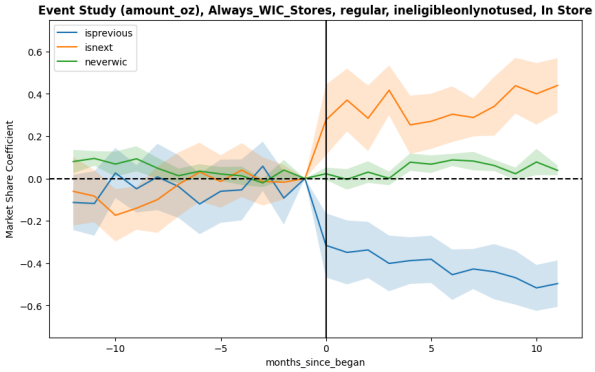
We next examine whether purchases shift toward the new WIC supplier among households shopping at authorized and unauthorized retailers. As in Section 3, we examine states in which a new manufacturer replaces the incumbent WIC manufacturer during 2018–2025. Figure 9a shows regular-size purchases among non-WIC households who exclusively purchase formula at WIC-authorized retailers, where WIC logos may be displayed and the contract brand may receive greater retail prominence. Prior to the contract change, the incumbent WIC supplier enjoys a high share of around 60%, while the future WIC supplier holds a share of around 20%. After the new WIC supplier takes up the contract, its share quickly increases to around 60%, while the former WIC supplier's share drops to around 20%.

Figure 9b shows a shift toward the new WIC supplier following the contract change among households shopping exclusively at non-WIC retailers. However, several pre-contract estimates for

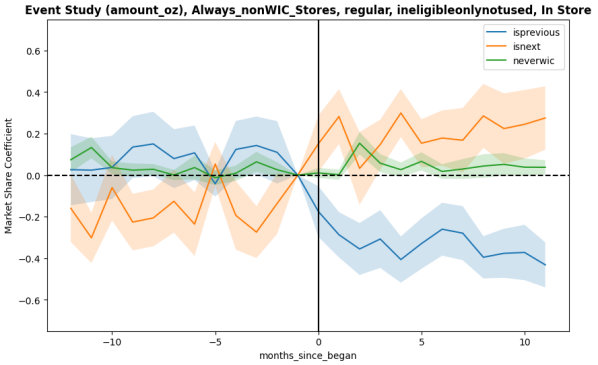
the new supplier are below zero, suggesting that some adjustment may have occurred before the contract began. This pattern warrants caution in attributing the entire post-contract shift to the contract change. The results are consistent with spillovers beyond WIC-authorized retailers, but do not establish which mechanisms drive them.[26]

Figure 9: Share of ineligible household purchases by manufacturer and retailer type

(a) Exclusive shopping in WIC retailers



(b) Exclusive shopping in non-WIC retailers



Notes: The figure reports the event study coefficients of regular-size infant formula purchases by manufacturer across retailer types and WIC authorization status.
Source: USDA and Numerator data 2018–2025.

Another limitation of the retail-channel analysis is that we do not directly observe shelf-space allocation or the complete set of products available at each retailer. Some retailers may not carry all infant formula brands because of capacity constraints or local assortment decisions, implying that observed demand shifts may partly reflect changes in product availability rather than retail prominence alone. To address this concern, we conduct robustness checks restricting the analysis to large retailers that are more likely to carry the full set of major national brands. The results remain qualitatively similar, suggesting that incomplete assortment is unlikely to fully explain the estimated retail exposure effects. We report the results of the robustness check in the online appendix.

Hospital Channel Previous literature has proposed that hospitals could drive increased sales of the WIC supplier's products among ineligible households. The Government Accountability Office found that free formula samples given to new mothers upon discharge ("discharge packs") affected

[26] Although some pre-contract estimates differ from zero, the overall pattern remains clear: market share shifts from the former WIC supplier toward the new supplier around the contract change. The pre-contract differences qualify the causal interpretation, but do not alter this descriptive finding.

Figure 10: WIC Sample Bag in Hospitals





Note: Panel (a) shows an example of a hospital formula sample for new mothers. Panel (b) shows an image of an advertisement for Abbott's brand.
Source: Similac, "MySimilac Rewards 2026: Formula Coupons Free Samples," Abbott Laboratories. Image reproduced from the official Similac website and accessed in June 2026.

both WIC and non-WIC households' formula consumption (GAO, 2006). Because half of infants will use the WIC contract brand, hospitals may provide this brand to new mothers so that they do not need to switch brands after leaving the hospital. However, hospitals may provide the WIC contract brand to all mothers, regardless of whether they are eligible for the program (Oliveira et al., 2011), to simplify or reduce the cost of the hospital's formula procurement process. Figure 10 provides examples of industry-sponsored hospital samples and advertising that emphasize the brand's provision in hospitals.

We use the Maternity Practices in Infant Nutrition and Care (mPINC) survey[27] to measure whether hospitals give mothers free infant formula, including formula discharge packs, excluding items prescribed as part of medical care.

Identifying the hospital channel empirically is substantially more challenging than identifying the retail channel. First, it is extremely difficult to observe where each baby was born. We therefore proxy hospital exposure using the hospital geographically closest to each household. We match each household to the geographically closest hospital participating in the mPINC survey based on ZIP-code centroid coordinates, restricting matches to hospitals within the same state. For each purchase, we assign hospital practices using the maximum observed value ("ever Yes")

[27] About every two years, the Center of Disease Control invites all US hospitals with maternity services to participate. The response rate is high; in 2024, 2,070 of 2,657 eligible hospitals participated (78%). See `https://www.cdc.gov/breastfeeding-data/mpinc/national-report.html`

across the available mPINC survey waves, rather than selecting a purchase-year-specific wave. Hospitals not observed in a relevant survey wave are retained, with missing responses ignored when calculating the maximum across available waves. Because some households may give birth at hospitals farther from their residence, this matching procedure may introduce measurement error in hospital exposure variables, potentially attenuating the estimated effects. Second, it is also challenging to observe which infant formula brand each hospital provides to new mothers. Although the mPINC data contain information on whether hospitals distribute formula samples or assist with WIC appointments, they do not identify the manufacturer associated with those practices. As a result, our analysis cannot directly measure household-level exposure to a specific WIC contract brand through hospitals.

We next leverage variation in the hospitals closest to non-participants and whether those hospitals provide formula samples or refer patients to WIC. Table 8 compares the demographic characteristics of non-WIC households who differ in whether their nearest hospital provides infant formula samples or helps make appointments to obtain WIC benefits. Households whose nearest hospital provides formula samples have less income, are less educated, and are more likely to be white. Household income is positively associated with whether the nearest hospital helps make WIC appointments.

Figure 11 shows that the probability that a purchase involves a WIC product is broadly similar among households regardless of whether their nearest hospital provides formula samples or helps mothers make WIC appointments. Figure B.2 in the appendix shows that this pattern also holds within household income terciles.

Table 7: The number of hospitals by hospital characteristics

Hospital Type	Number of Hospitals	Share
None	785	0.311
Baby friendly	160	0.063
Formula sample	580	0.230
WIC appointments	486	0.192
Baby friendly, Formula sample	17	0.007
Baby friendly, WIC appointments	171	0.068
WIC appointments, Formula sample	313	0.124
Baby-friendly + WIC + sample	13	0.005

Note: The table reports the number of hospitals with their status.
Source: mPINC data 2018-2022.

The spillover effect is more likely driven by the retailer channel rather than the hospital channel.

Table 8: Comparison of purchasers' demographics by hospital characteristics

Hospital Type	Fraction of Households	Income	College	Black	Hispanic	Asian	White
None	0.597	102,154	0.82	0.07	0.10	0.08	0.71
Baby friendly	0.034	105,613	0.86	0.07	0.10	0.08	0.72
Formula sample	0.119	95,584	0.80	0.06	0.07	0.06	0.79
WIC appointments	0.135	103,905	0.83	0.07	0.09	0.09	0.71
Baby friendly, Formula sample	0.005	111,590	0.85	0.11	0.05	0.03	0.78
Baby friendly, WIC appointments	0.039	96,620	0.80	0.10	0.08	0.08	0.70
WIC appointments, Formula sample	0.068	99,367	0.82	0.05	0.06	0.06	0.80
Baby-friendly + WIC + sample	0.002	106,479	0.77	0.08	0.03	0.04	0.70

Note: The table compares the demographics of non-WIC households by whether their nearest hospital provides infant formula samples or helps make appointments to obtain WIC benefits.
Source: Numerator data 2018-2025, mPINC data 2018-2022.

Figure 11 shows that the probability of purchasing the WIC brand is similar among households regardless of whether their nearest hospital provides formula samples or helps mothers make WIC appointments. Figure B.2 in the appendix shows that this pattern holds within household income terciles. Figure 12 shows that the probability of purchasing the WIC brand differs more between in-store and online purchasers than between purchasers who live near hospitals that do and do not provide formula samples. Figure B.3 in the appendix shows that this pattern holds within household income terciles.

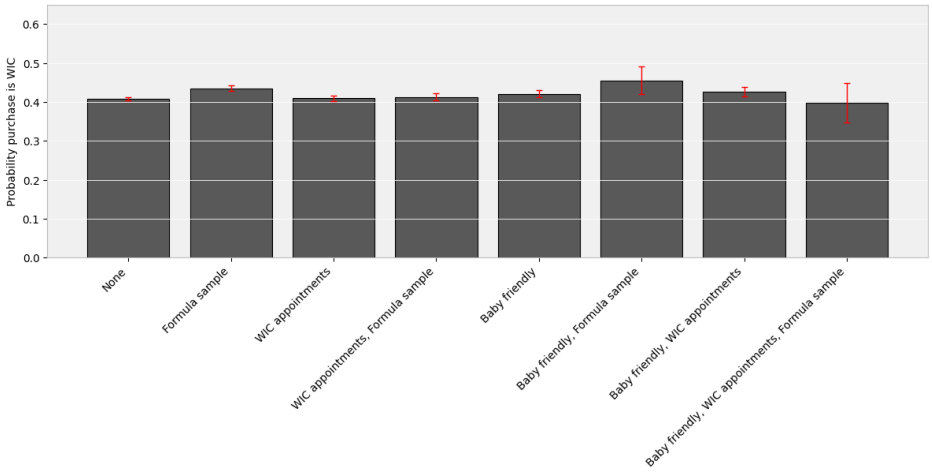
5 Demand Analysis

5.1 Discrete Choice Model of Infant Formula

In this section, we estimate a unified discrete choice model to quantify the relative importance of the mechanisms behind the WIC demand spillover. The model includes both regular-size and bulk-size infant formula products, allowing us to distinguish between the segment directly affected by the WIC program and the broader infant formula market within a single framework.

This distinction is central to our analysis. WIC benefits apply only to regular-size products, and the program directly changes their retail environment through voucher eligibility, minimum stocking requirements, and associated changes in product visibility. Consequently, the estimated effects for regular-size products capture the primary mechanism through which WIC influences consumer demand. Bulk-size products, in contrast, are not eligible for WIC redemption. Any estimated effects for these products therefore reflect spillovers beyond the directly treated segment, such as broader changes in brand perceptions or retail responses that extend across product sizes.

Figure 11: Probability of WIC purchase by hospital characteristics



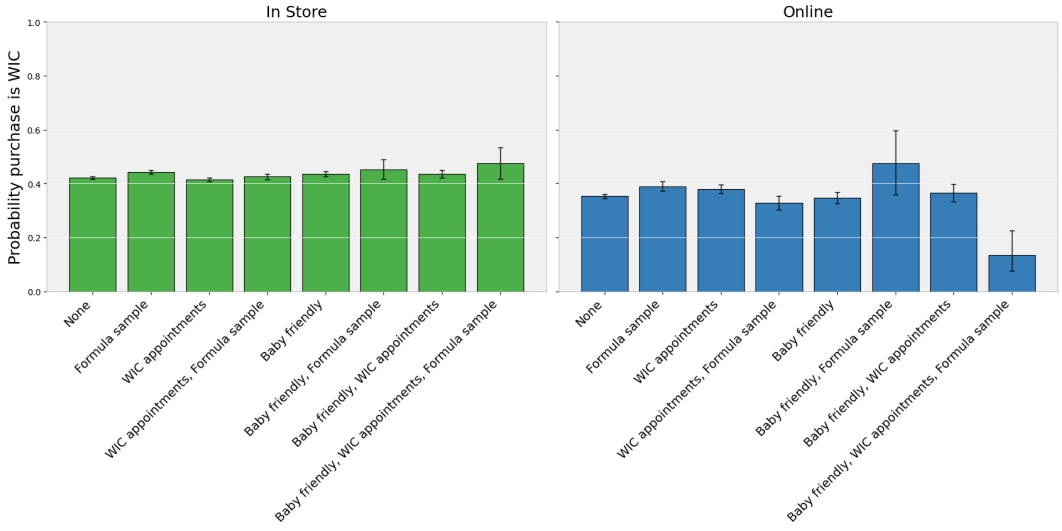
Note: Figure compares the fraction of non-WIC households that purchase the WIC brand among those whose nearest hospital neither provides formula samples nor helps make WIC appointments, only provides formula samples, only makes WIC appointments, or both. Purchases are limited to regular-size purchases that are the households' first observed purchase of infant formula. 95% confidence intervals are shown.
Source: Numerator data 2018-2025.

For the discrete choice analysis, we focus on each household's first observed infant formula purchase. This restriction avoids using subsequent observed purchases by the same household and thereby limits the direct influence of repeated purchasing, inventory accumulation, and state dependence within our sample.[28] Because the first purchase we observe need not be the household's first-ever formula purchase, however, we interpret the estimates as determinants of the first observed brand choice, rather than the household's initial brand choice. As a robustness check, the Online Appendix re-estimates the model using all purchase transactions and obtains qualitatively similar results.

We model consumer choice over the combined set of regular-size products (J_R) and bulk-size products (J_B) within a unified discrete choice framework. This specification allows WIC exposure to affect the utility of regular-size products directly while also permitting spillovers to bulk-size products through changes in brand preferences or the retail environment. The utility that household i derives from product j is given by

[28] As shown in Section 3, the average number of brands a household purchases is 1.5.

Figure 12: Probability of WIC purchase by hospital characteristics and purchase method



Note: Figure compares the fraction of non-WIC households that purchase the WIC brand among those whose nearest hospital neither provides formula samples nor helps make WIC appointments, only provides formula samples, only makes WIC appointments, or both. The left panel shows the purchases made in store. The right panel shows the purchases made online. Purchases are limited to regular-sized purchases that are the households' first observed purchase of infant formula. 95% confidence intervals are shown.
Source: Numerator data 2018-2025.

$$
\begin{aligned}
u_{ijst} = \delta_{jst} &+ \beta_p^i price_{jst} + \gamma^i WICbrand_j \\
&+ \gamma^W WICbrand_j \times WICstore_{st} + \gamma^{NW} WICbrand_j \times nonWICstore_{st} \\
&+ \gamma^A WICbrand_j \times Abbott_j + \gamma^N WICbrand_j \times Nestle_j \\
&+ \eta WICbulk_j + \eta^W WICbulk_j \times WICstore_{st} + \eta^{NW} WICbulk_j \times nonWICstore_{st} \\
&+ WICbrand_j \times H'_{is}\gamma^H + \varepsilon_{ijst}.
\end{aligned} \tag{2}
$$

where $j \in J_R \cup J_B$, and the utility of purchasing regular sized store brand online is normalized to 0.

The term δ_{jst} captures mean utility and includes brand-size-specific intercepts (e.g., Abbott regular and Mead Johnson bulk), allowing baseline preferences to vary across both manufacturers and package sizes. The coefficient β_p^i captures household-specific price sensitivity.

The indicator $WICbrand_j$ equals one if product j is produced by the current WIC contract manufacturer, regardless of package size. The other WIC interaction terms allow the utility premium associated with the WIC contract brand to vary by retailer type, where the indicator ($WICstore_{st}$)

equals one for purchases at WIC-authorized retailers, while ($nonWICstore_{st}$) equals one for purchases at unauthorized retailers.[29] Since the baseline is for an online purchase, γ^i captures all spillover effects that are common to the online and retail channels. This includes word-of-mouth peer effects, hospital formula feeding upon birth, advice or formula samples provided at pediatrician offices before or after birth, and manufacturer samples provided directly to households. The coefficients (γ^W) and (γ^{NW}) capture the additional utility premium associated with the WIC contract brand at each retailer type, relative to online purchases. The unauthorized-store premium is consistent with retail visibility mechanisms that operate without WIC logos. The difference ($\gamma^W - \gamma^{NW}$) may reflect an additional effect of WIC labeling, but may also capture differences in retail visibility and household sorting across retailer types. Interpreting this difference as a logo effect requires comparable non-labeling influences across authorized and unauthorized stores.

A key assumption for interpreting the retailer interaction coefficients (γ^W) and (γ^{NW}) as incremental effects of the physical retail environment is that other mechanisms responding to WIC contract changes do not differentially affect in-store and online shoppers. Permanent differences between households that shop online and in stores do not by themselves generate these interaction effects, because identification comes from changes in the identity of the WIC contract brand across states and over time. A potential concern, however, is that some non-retail mechanisms may themselves respond to contract changes differently across household types. For example, households that shop primarily in stores may be more likely to interact with WIC participants and therefore receive more information about the newly designated WIC brand through peers. We address this concern by controlling flexibly for household characteristics, including income, education, and race and ethnicity, and allowing responsiveness to the WIC contract brand to vary with these characteristics. The estimated retailer premiums remain similar with these controls; see Appendix **??**. Nevertheless, these controls cannot rule out all unobserved differences in exposure across shopping channels, so we interpret the retailer interactions as incremental retail-environment premiums rather than as pure causal effects of shelf space.

The other WIC interaction terms allow the utility premium associated with the WIC contract brand to vary by package size and hospital exposure.[30] The indicator $WICbulk_j$ equals one if

[29] In principle, variation in WIC authorization status and contract assignments within retailers over time could help isolate changes in retail exposure while holding fixed store-specific characteristics. However, our demand specification does not include retailer fixed effects because the large number of retailers and products would introduce an infeasibly large number of parameters in the structural estimation. As a result, the model combines both cross-sectional and time-series variation across retail environments.

[30] Price endogeneity is unlikely to drive our main results. Prices are posted retail prices observed at the transaction level, not household-specific negotiated prices or market-level averages. Our baseline model also uses households' first

product j is both produced by the current WIC contract manufacturer and sold in the bulk-size category. The interaction terms with retailer type allow the effects of WIC designation to differ between WIC-authorized and unauthorized retailers. Finally, H_{is} is a vector of hospital-related exposure variables, including whether the household's nearest hospital provides infant formula samples, has Baby-Friendly status, or assists with WIC appointments.

The idiosyncratic utility component ε_{ijst} is assumed to follow a Type I extreme value distribution, yielding the standard mixed logit choice probabilities. To capture unobserved heterogeneity, we allow random coefficients on price and $WICbrand_j$, permitting households to differ in both price sensitivity and preferences for products manufactured by the current WIC supplier.[31]

We model households' product choices conditional on purchasing infant formula and therefore do not include non-purchase as an outside option. The model is intended to capture substitution across formula products, which is the primary margin affected by changes in the WIC supplier and the associated retail environment. Thus, the specification does not assume that households cannot substitute away from infant formula; rather, the extensive margin of whether to purchase formula lies outside the model. Accordingly, the demand elasticities and counterfactual substitution patterns below should be interpreted as conditional on a formula purchase.[32]

5.2 Estimation Results

Table 9 reports the parameter estimates from the demand model. We first note that the price coefficient is negative and precisely estimated, indicating substantial price sensitivity. The mean own price elasticity is -1.9. The estimated standard deviation of the random price coefficient is also positive and statistically significant, implying considerable heterogeneity in households' price responsiveness.

Turning to the WIC-related variables, we find strong evidence that becoming the WIC contract manufacturer substantially increases product utility among non-WIC households. The baseline utility premium associated with the WIC contract brand is positive and statistically significant, suggesting that the demand spillover operates through mechanisms that impact both online and in

observed infant formula purchase, before retailers are likely to have information about household-specific formula preferences. In addition, the key WIC effects are identified from state-level contract changes in the identity of the WIC supplier, while prices change little around these transitions relative to the large market-share shifts we observe. Finally, prices enter the model as controls; we do not conduct price counterfactuals or welfare exercises that rely on treating prices as fully exogenous.

[31] We estimated a version of the discrete choice model with a correlation between these two random coefficients. We found the correlation is not statistically significant.

[32] In Online Appendix, we show the evidence that the total purchase amount is not associated with a contract change.

Table 9: Discrete Choice Model Estimates: Both sizes

	(1)	(2)
(Intercept):Abbott_bulk	0.362***	0.364***
	(0.056)	(0.056)
(Intercept):Abbott_regular	0.422***	0.421***
	(0.059)	(0.059)
(Intercept):MJ_bulk	1.005***	1.004***
	(0.055)	(0.055)
(Intercept):MJ_regular	0.627***	0.628***
	(0.059)	(0.059)
(Intercept):Nestle_bulk	−1.337***	−1.331***
	(0.067)	(0.067)
(Intercept):Nestle_regular	−1.111***	−1.118***
	(0.076)	(0.076)
(Intercept):Store_bulk	0.204***	0.206***
	(0.054)	(0.054)
price	−1.533***	−1.529***
	(0.021)	(0.021)
WICbrand	0.419***	0.266***
	(0.066)	(0.077)
WICbrand:WICstore	0.375***	0.618***
	(0.055)	(0.071)
WICbrand:nonWICstore	0.220***	0.324***
	(0.053)	(0.069)
WIC_bulk	−0.514***	−0.221***
	(0.033)	(0.081)
WIC_bulk:WICstore		−0.497***
		(0.083)
WIC_bulk:nonWICstore		−0.196**
		(0.081)
WICbrand:baby_fre_hosp	0.059	0.058
	(0.042)	(0.042)
WICbrand:hospital_sample	0.080**	0.079**
	(0.031)	(0.031)
WICbrand:WIC_appointments	−0.003	−0.003
	(0.029)	(0.029)
WICbrand:Nestle	1.034***	1.036***
	(0.084)	(0.084)
WICbrand:Abbott	−0.098	−0.096
	(0.062)	(0.062)
sd.price	0.253***	0.253***
	(0.014)	(0.014)
sd.WICbrand	0.029	0.026
	(0.089)	(0.089)
Included Stores	All	All
Observations	22,065	22,065
R^2	0.115	0.115
Log Likelihood	−35,528.890	−35,496.510

Notes: The table reports parameter estimates from the regular and bulk-size infant formula discrete choice model under alternative fixed-effect specifications. Across columns, we vary the set of controls for product and retailer heterogeneity while maintaining the same underlying utility specification.
***:$p < 0.01$, **:$p < 0.05$, and *:$p < 0.10$.

store shoppers, and may include word-of-mouth peer effects, hospital formula feeding upon birth, advice or formula samples provided at pediatrician offices before or after birth, and manufacturer samples provided directly to households. The interaction coefficient with non-WIC authorized retailers is 0.324 and larger than the baseline WIC brand premium, suggesting that retail visibility is an even stronger strong driver of the demand spillover than mechanisms that affect both online and in store shoppers. Moreover, the WIC brand premium is considerably larger at WIC-authorized retailers than at non-WIC retailers: the interaction coefficient with WIC-authorized stores is 0.618, compared with 0.324 at non-WIC retailers.This pattern is consistent with the WIC-induced demand effect extending across physical retailers, with an additional premium at WIC-authorized stores. The latter may reflect WIC-specific mechanisms such as labeling and stocking requirements, but may also capture differences in retail prominence or household sorting across retailer types. We will investigate the economic magnitude of these coefficients in the next subsection.

We next examine whether these effects extend to bulk-size products, which are not eligible for WIC redemption. The coefficient on $WICbulk_j$ is negative, indicating that the WIC premium is substantially attenuated for bulk products. The implied utility premium for WIC bulk products equals the sum of the coefficients on $WICbrand_j$ and $WICbulk_j$, which is approximately $0.266 - 0.221 = 0.045$ in column (2).[33] Thus, while the WIC contract generates a small positive spillover to bulk products, the effect is much smaller than for regular-size products. This finding suggests that the demand spillover is concentrated in the product category directly affected by the WIC program.

Finally, we find limited evidence that hospital discharge-pack provision accounts for a substantial portion of the demand spillover. The interaction with hospital-provided formula samples is positive and statistically significant but economically small relative to the retailer interactions. The other hospital-related interactions are also small.[34] By contrast, the retail interaction terms are considerably larger, consistent with an important role for the retail environment in generating the spillover. Likewise, while households exhibit substantial heterogeneity in price sensitivity, we find little evidence of meaningful heterogeneity in preferences for WIC contract brands, as the estimated standard deviation of the random coefficient on $WICbrand_j$ is small and statistically insignificant.

A natural concern is that these hospital estimates are attenuated by measurement error, since

[33]If the WIC-winning brand's bulk products enjoyed no demand spillover from the subsidized regular product — i.e., were treated as an ordinary non-WIC brand — the probability of purchasing a bulk infant-formula product would be 2.2% (1.38 pp) lower."

[34]The pseudo R^2 with and without those hospital variables are essentially the same, implying the hospital variables do not explain consumer behavior.

we observe whether a hospital provides formula but not which brand it provides. To address this concern, Appendix B.2 considers a broader outcome that does not require observing the brand included in the discharge pack. Specifically, we examine whether households whose nearest hospital provides formula samples are more likely to purchase a national brand rather than a store brand.[35] We find little evidence of such a relationship, suggesting that discharge-pack provision has limited effects on subsequent national-brand demand on this margin.

We conduct two additional robustness checks. Appendix D.6 adds state-by-manufacturer fixed effects to absorb persistent regional manufacturer preferences. The retailer interaction coefficients remain close to the baseline estimates, and the authorized–unauthorized difference remains statistically significant. Appendix D.7 restricts estimation to switching states within twelve- and six-month windows around contract changes. The retailer interaction coefficients remain positive, but the authorized–unauthorized difference is less precisely estimated and is not statistically significant in either window.

Price elasticities and a pricing puzzle. Our demand estimates also have pricing implications. Table 10 reports own-price elasticities, conditional on purchasing formula, under the estimated model and under a model in which all WIC effects are set to zero; Appendix Table E.15 reports the full matrix of cross-price elasticities and the implied Bertrand margins. For regular-size formula, the WIC spillover makes demand for the contract brand less price elastic: its own-price elasticity is -1.84, compared with -2.08 without WIC effects. For bulk formula, where the negative bulk interactions largely offset the WIC-brand premium, the contract brand's elasticity changes little and becomes slightly larger in magnitude (-1.65 versus -1.63). Other national brands and the store brand become slightly more elastic in both sizes. Because each manufacturer prices its regular and bulk products jointly, static Bertrand pricing implies that the contract brand's price–cost margin (Lerner index) should be 0.10–0.12 higher than without WIC effects in both sizes (median across markets), and its price 6–7% higher. In the data, however, the price of the new contract brand falls by about 5% after a contract change. Several features of the market could reconcile the two: rebates that rise with the wholesale price, WIC price limits on authorized vendors, national wholesale pricing against state-level contracts, dynamic incentives arising from brand loyalty formed in infancy, and retailers rather than manufacturers setting shelf prices. We leave this puzzle for future work.

[35] `https://perrigopediatrics.com/pages/faqs`

Table 10: Own-price elasticities of demand with and without WIC effects

	Regular		Bulk	
	With WIC	Without WIC	With WIC	Without WIC
WIC contract brand	−1.842	−2.077	−1.652	−1.630
	(0.023)	(0.025)	(0.021)	(0.021)
Other national brands	−2.338	−2.291	−1.761	−1.687
	(0.027)	(0.026)	(0.022)	(0.021)
Store brand	−1.989	−1.971	−1.364	−1.310
	(0.007)	(0.007)	(0.017)	(0.017)

Notes: Percentage change in market share when a product's own price rises by 1%, averaged over the products in each group with weights equal to their predicted shares, from the main demand model (Table 9, column 2), pooled over $N = 22{,}065$ choice situations (300 simulation draws, common random numbers). *With WIC* uses the estimated model; *Without WIC* sets all WIC coefficients, including the standard deviation of the WIC-brand coefficient, to zero and holds prices and all other parameters fixed. The WIC contract brand is the brand holding the state's WIC contract at the time of purchase; other national brands are the remaining Abbott, Mead Johnson, and Nestlé products. Each entry is the sum over products in the group and over choice situations of $p\,\partial P/\partial p$, divided by the corresponding sum of predicted choice probabilities. Elasticities are conditional on purchasing formula (no outside option). Standard errors in parentheses are from 500 parametric bootstrap draws of the demand parameters. Elasticities by brand, including cross-price elasticities, are in Appendix Table E.15.

5.3 Accounting Exercise

While the parameter estimates from the demand model provide evidence on the direction and statistical significance of the WIC-related mechanisms, they are less informative about the quantitative importance of these mechanisms in generating the large market share shifts observed in the data. To assess the aggregate implications of the estimated model, we run a series of simulations around the timing of WIC contract changes. These simulations allow us to quantify how much of the observed market share reallocation can be attributed to WIC-induced demand spillovers and to the underlying retail exposure mechanisms.

Figure 13 presents a simulation based on the estimated demand model in which the WIC effect is entirely removed. Using the estimated parameters from the demand model, we simulate market shares around the timing of WIC contract changes after setting all WIC-related utility components to zero, while holding prices and other product characteristics fixed.

The figure compares the simulated market share paths for the previous WIC supplier, the new WIC supplier, and the never-WIC manufacturer. In the observed data, market shares shift sharply after the contract change, with the new WIC supplier gaining substantial share and the previous supplier experiencing a corresponding decline. In contrast, once the WIC effect is removed, these market share reallocations become much smaller.

The simulation indicates that the large market share movements observed following WIC contract changes are primarily driven by the WIC-induced demand spillover rather than by baseline brand preferences or price differences alone. Absent the WIC effect, the model predicts relatively stable market shares around the contract transition.

Figure 13b removes the component of the offline contract-brand premium shared across authorized and unauthorized retailers, while retaining the additional authorized-store premium. Relative to the baseline, the simulated market-share response to contract changes is attenuated. Figure 13c additionally removes the remaining authorized-store premium, leaving the baseline contract-brand premium and hospital-related terms unchanged. The remaining response illustrates the model's allowance for spillovers that do not depend on retailer type.

This decomposition is consistent with a retail effect that operates broadly across physical retailers and an additional WIC-specific retail effect at authorized stores, potentially including WIC labeling. That interpretation requires the non-labeling component of the offline premium to be the same across retailer types. We therefore interpret the simulations as quantifying the implications of the estimated retailer premiums, rather than separately identifying the causal effects of shelf space and WIC logos.

Finally, Figure 13c presents a stronger counterfactual exercise in which the effects of both WIC authorized and unauthorized stores to be 0. Under this scenario, market shares become even more stable around the contract transition. Relative to panel (b), the additional removal of the WIC effect leads to only modest incremental changes in predicted market shares.

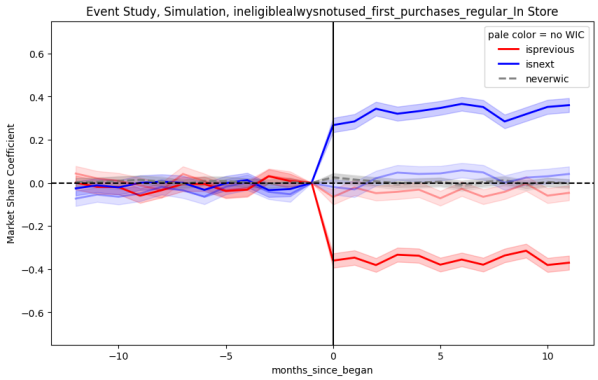
The simulations indicate that retailer-specific premiums account for an important share of the observed market-share reallocation, but a substantial response remains even after these premiums are removed. We therefore interpret the results as showing that the physical retail environment makes a meaningful contribution to the spillover, rather than as establishing that it is the dominant mechanism. The remaining response is consistent with spillover components that are common across shopping channels, although these components need not correspond exclusively to non-retail mechanisms because preferences formed through prior retail exposure may also carry across channels.

Discussion One limitation of our analysis is that we condition on the retailer visited by the household and therefore treat store choice as exogenous to product choice. If households with stronger preferences for WIC brands systematically sort into particular retail environments, part

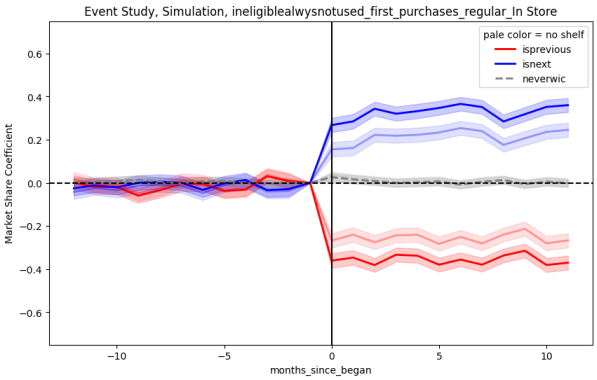
of the estimated retailer premiums could reflect endogenous store selection rather than differences in the retail environment alone. Our baseline demand specification uses both cross-sectional and time-series variation across retail environments, so the retailer interaction coefficients should not be interpreted as being identified solely from within-retailer or within-store-type contract transitions.

Several features of the empirical setting help assess this concern. First, the estimated effects are concentrated in the regular-size segment directly affected by the WIC program, while the bulk-size segment exhibits substantially weaker responses. Second, the results remain qualitatively similar after controlling flexibly for household demographics and after adding grouped controls for retailer heterogeneity. Third, the reduced-form event studies provide complementary evidence based directly on contract transitions. Nevertheless, because the baseline demand model does not fully absorb household sorting or retailer-specific unobserved heterogeneity, we interpret the retailer interaction coefficients as conditional retailer premiums rather than as pure causal effects of the physical retail environment.

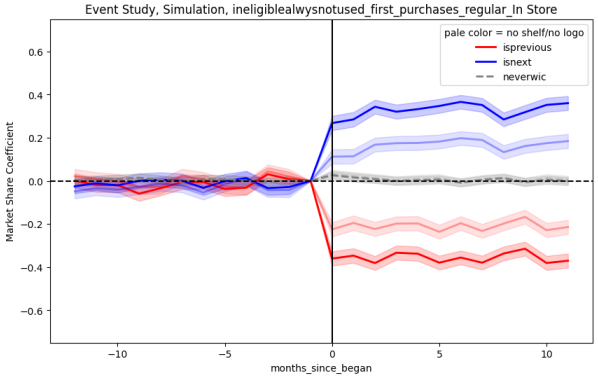
Figure 13: Event Study Results under Counterfactual Scenarios



(a) No WIC



(b) Removing the Incremental WIC-Store Effect



(c) Removing the Incremental both WIC- and non-WIC Store Effect

Notes: The figure reports simulated market shares under alternative counterfactual scenarios based on the estimated discrete choice model. The baseline scenario reproduces observed market shares, while counterfactual scenarios remove or alter specific mechanisms associated with the WIC program. Market shares are computed using regular-size infant formula purchases.
Source: Numerator data, 2018–2025.

5.4 Robustness Checks

We conduct a series of robustness checks to assess the sensitivity of our findings to alternative sample definitions, measurement issues, and identification assumptions. The detailed results are reported in the Online Appendix. Across all specifications, the qualitative conclusions remain unchanged: retail exposure effects remain substantially larger than hospital-related effects, supporting the interpretation that the retail environment is the primary mechanism behind the observed spillovers.

Abbott Recall One concern is that the nationwide Abbott infant formula recall beginning in February 2022 may have altered purchasing patterns independently of the WIC program. To address this concern, we re-estimate the demand models after excluding the recall period. The results remain qualitatively similar, suggesting that the estimated spillover effects are not driven by temporary disruptions associated with the recall.

All Purchase Records Our baseline specification focuses on households' first infant formula purchase in order to minimize the influence of state dependence, habit formation, and inventory accumulation. As a robustness check, we re-estimate the models using all purchase records. The results remain broadly consistent with the baseline estimates.

Store Capacity Because we do not directly observe shelf-space allocation or complete product assortments, an alternative explanation of our results is that the estimated effects reflect differences in product availability rather than retail prominence. To address this concern, we restrict the analysis to large retailers that are more likely to carry the full set of major national brands. The estimated spillover effects remain similar in magnitude and significance.

Selection on Observables Households that shop online, shop at WIC-authorized retailers, or are exposed to different hospital environments differ systematically in observable characteristics such as income, education, and race. We therefore augment the demand model with detailed demographic controls and interaction terms. The estimated retail exposure effects remain economically and statistically significant after controlling for these characteristics.

Store Heterogeneity WIC-authorized and unauthorized retailers may differ in ways that are unrelated to WIC status. To control flexibly for store heterogeneity, we follow the approach of Bonhomme et al. (2022) and approximate high-dimensional store fixed effects by clustering retailers

based on sales and WIC authorization status. The results remain robust to the inclusion of these cluster-based controls for store heterogeneity.

Store Choice One concern is that households may systematically sort into different retail environments. For example, households with a stronger preference for WIC brands may be more likely to shop at WIC-authorized retailers, causing the estimated retail effects to partly reflect consumer selection rather than differences in the retail environment. To address this concern, we allow the effects of shopping at WIC-authorized and unauthorized retailers to vary flexibly across latent household types. Specifically, following Bonhomme et al. (2022), we cluster households based on their purchase patterns to approximate unobserved household heterogeneity, and interact these household-cluster fixed effects with the WIC-store and non-WIC-store indicators. This specification absorbs persistent differences in households' shopping behavior while preserving identification from changes in the retail environment. The results remain qualitatively unchanged.

6 Conclusion

The mechanisms underlying the WIC program's spillover effects on ineligible households have remained a puzzle since the early 2000s. In this paper, we combine data on online and offline purchases, hospital provision of infant formula samples, and the timing of WIC contract changes to isolate the roles of retail and hospital environments in shaping ineligible households' purchasing decisions. We show that the WIC-induced demand shift extends beyond retailers directly participating in the program, with substantial spillovers even among households shopping at unauthorized retailers. This pattern indicates that direct exposure to WIC labeling at the point of purchase is not necessary for the broader retail spillover, while leaving open the possibility that labeling contributes to the additional effect observed at authorized retailers. Separately, we find little evidence that hospital provision of formula discharge packs generates substantial subsequent demand for national formula brands.

More broadly, our findings highlight the importance of understanding how public procurement programs interact with retail environments and firm incentives in concentrated consumer goods markets. While our analysis documents substantial spillovers operating through physical retail exposure, fully characterizing the equilibrium interaction between manufacturers, retailers, and consumers remains an important direction for future research. In particular, a richer equilibrium framework could model how manufacturers optimally bid for WIC contracts, how retailers ad-

just product prominence and inventory allocation in response to expected WIC demand, and how consumers update consideration sets across shopping environments. Such a framework could help quantify the long-run implications of procurement policy for competition, retail organization, and market concentration.

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Appendix

A More Descriptive Evidence on the Retail Channel

A.1 Share Regression

To quantify the magnitude of the share changes beyond what would be expected from price changes, Table A.1 examines the shares of non-participating households' purchases of formula in ounces that are captured by each manufacturer in each state and month, controlling for prices as follows.

$$Share_{jmt} = \beta_0 + \beta_W \times WIC_{jmt} + \sum_{b \in \{\text{MJ,Nestle}\}} \beta_b \times WIC_{jmt} \times b + \beta_P \times price_{jmt} + \eta_{b(j)} + \gamma_t + \varepsilon_{jmt}. \quad (3)$$

Here, WIC_{jmt} is an indicator for the WIC status, b is an indicator for each brand (Abbott is excluded), $price_{jmt}$ is the unit price. The model also controls for brand and time fixed effects.

Column 1 focuses on regular-size products and shows that a manufacturer's share increases by 34 percentage points after becoming the WIC supplier. Column 2 allows this effect to differ across manufacturers. The estimated increase is 32 percentage points for Abbott, 38 percentage points for Mead Johnson, and 32 percentage points for Nestlé. These results suggest that price differences do not fully account for the large market-share shifts associated with WIC contract status.

One may be concerned that the gain in market share is driven not by consumers' preferences for the WIC supplier's products but by changes in choice sets induced by stores carrying a manufacturer's products only when they are the WIC supplier. Columns 3 and 4 address this concern by limiting the sample to the ten largest retailers by observed formula purchase-record count, which are more likely to carry a broad set of major formula brands. We find a similar spillover effect, suggesting that the result is not driven solely by differences in assortment across small and large retailers.

The spillover effect is smaller but still present for bulk-size products. Column 6 implies increases of approximately 6 percentage points for Abbott and Mead Johnson and 5 percentage points for Nestlé. Thus, the estimated bulk-size effects are relatively similar across manufacturers and substantially smaller than those for regular-size products. Although changes in choice set are less of a concern here because bulk products are not eligible for WIC vouchers, for completeness, in columns 7 and 8, we similarly restrict the analysis to the ten largest retailers, which are more likely to carry

Table A.1: Market share regression

	Dependent variable: Manufacturer share							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
WIC	0.344***	0.317***	0.316***	0.289***	0.058***	0.058***	0.056***	0.056***
	(0.006)	(0.012)	(0.007)	(0.013)	(0.006)	(0.010)	(0.006)	(0.010)
WIC*MJ		0.062***		0.058***		0.001		0.001
		(0.020)		(0.021)		(0.016)		(0.016)
WIC*Nestle		0.005		0.021		-0.008		-0.004
		(0.023)		(0.025)		(0.021)		(0.021)
MJ	0.025***	-0.004	0.045***	0.018	-0.013**	-0.013	-0.008	-0.008
	(0.007)	(0.012)	(0.007)	(0.013)	(0.006)	(0.010)	(0.006)	(0.010)
Nestle	-0.161***	-0.168***	-0.141***	-0.153***	-0.212***	-0.210***	-0.212***	-0.211***
	(0.010)	(0.014)	(0.011)	(0.016)	(0.008)	(0.010)	(0.008)	(0.011)
Store	-0.111***	-0.126***	-0.087***	-0.102***	0.032***	0.033***	0.040***	0.040***
	(0.010)	(0.011)	(0.010)	(0.012)	(0.008)	(0.009)	(0.008)	(0.009)
Price	-0.013**	-0.013**	-0.016***	-0.016**	0.007	0.008	0.002	0.002
	(0.006)	(0.006)	(0.006)	(0.006)	(0.009)	(0.009)	(0.010)	(0.010)
Included sizes	Regular	Regular	Regular	Regular	Bulk	Bulk	Bulk	Bulk
Included stores	All	All	Big	Big	All	All	Big	Big
Observations	5623	5623	5198	5198	8757	8757	8419	8419
Adjusted R^2	0.627	0.627	0.595	0.595	0.357	0.357	0.368	0.368
Note:								*p<0.1; **p<0.05; ***p<0.01

Note: The dependent variable is a brand's share in a particular state and month. Additional regressors include state, year, and month dummies. Abbott is the excluded category. Regular size refers to 12-16 oz and bulk size refers to containers larger than 16 oz. Big stores are the ten largest retailers by observed formula purchase-record count.

a broad set of formula brands. The results remain similar, suggesting that broad differences in retailer assortment are unlikely to explain the bulk-size results.

A.2 Online Regression

We further examine the differences in shopping patterns between offline and online transactions. First, we run the following regression to see (i) whether consumers are less likely to purchase WIC brand products online compared to offline, and (ii) the extent to which incorporating consumer fixed effects alters the coefficient related to the online transactions. The first objective seeks to validate the event study results previously discussed, confirming the observed trend. The second objective aims to understand how consumer behavior, po tentially influenced by knowledge of WIC brands acquired in physical stores, varies in online settings.

$$WIC_{it} = \beta_0 + \beta_1 Online_{it} + \beta_2 Bulk_{it} + X_i'\gamma + \varepsilon_{it}. \tag{4}$$

In the model, WIC_{it} is a dummy variable for whether a household purchases WIC brand's product or not, $Online_{it}$ is a dummy variable for whether the transaction is online or not, $bulk_{it}$ is a dummy variable for whether a household buys a bulk-size product, and X_i is the vector of household characteristics.

Table A.2: Online WIC Purchase

	(1)	(2)	(3)	(4)	(5)
Online	-0.0770***	-0.0764***	-0.0341***	-0.0340***	-0.0045*
	(0.0035)	(0.0035)	(0.0028)	(0.0028)	(0.0024)
Unit price		-0.0014***		-0.0002***	-0.0007***
		(0.0001)		(0.0001)	(0.0001)
Bulk					-0.0666***
					(0.0023)
Manufacturer	No	No	No	No	Yes
User	No	No	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes
R-squared	0.0038	0.0054	0.0012	0.0013	0.0117
No. observations	126246	126246	126246	126246	126246

Note: The dependent variable is whether a transaction includes WIC brand infant formula.
Standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.2 presents the results of our analysis, where each column incorporates a different set of fixed effects, either manufacturer fixed effects, household fixed effects, or both. The data reveals a couple of notable trends: Firstly, the coefficient on $Online_{it}$ is consistently negative and statistically significant across all specifications. This strongly supports the observation that consumers are less likely to purchase WIC brand products during online shopping.

Secondly, when household fixed effects are included in the model, the absolute value of the online coefficients becomes smaller. This implies that consumers who are possibly aware of which brands are supported by WIC, based on their offline shopping experiences, may still prefer WIC brand products in online shopping. This trend indicates that consumers' brand preferences, potentially developed in physical stores, could influence their online purchasing decisions.

B More Descriptive Evidence on Hospital Channel

B.1 State-level Variation in Discharge Packs Regulation

To identify whether hospital infant formula samples are driving the WIC spillover effect, we look into various sources. First, we use variation in whether hospitals provide formula samples to new mothers upon discharge. We first compare the probability of purchasing the WIC brand in states that have begun and have not begun eliminating the practice of providing infant formula samples to new mothers. Sadacharan et al. (2011) identify the 10 states with the best and worst records in eliminating infant formula samples as of 2007. In the 10 best record states, 18% to 43% of hospitals had eliminated formula samples, while in the 10 worst record states, 0%-1% of hospitals had eliminated formula samples. Since nearly all hospitals in the worst record states provide formula samples, if hospital-provided formula samples cause non-WIC households to purchase the WIC brand, we would expect non-WIC households in these states to purchase the WIC brand at a higher rate than non-WIC households in the states that have eliminated hospital-provided formula samples.

Table B.3 compares the demographic characteristics of non-WIC households who differ in whether their state provides a discharge pack with infant formula samples. Households living in states that provide formula samples have less income, are more likely to be black, and are less likely to be Asian. Table B.4 compares the purchase characteristics of these non-WIC households. Households living in states that provide formula samples are less likely to purchase bulk-size containers.

Table B.3: Comparison of purchaser's demographics by state's hospital characteristics

State Type	Fraction of Households	Income	College	Black	Hispanic	Asian	White
Top 10 states with discharge packs	0.19	98,021	0.79	0.11	0.14	0.06	0.65
Top 10 states w/o discharge packs	0.16	101,800	0.82	0.05	0.17	0.16	0.57
Other	0.66	95,368	0.79	0.09	0.07	0.05	0.75

Note: The table compares the demographics of non-WIC households by whether they live in one of the top 10 states that have eliminated formula samples in 2007 (WA, MN, CA, VT, MA, WI, NM, NH, OR, RI) or one of the top 10 states that continue to provide formula samples in 2007 (AR, DC, MD, MS, NJ, OK, SD, WV, IA, TX). Source: Numerator 2018-2025.

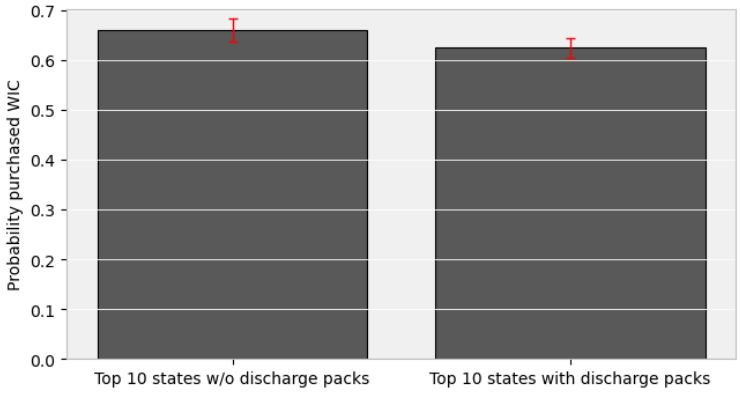
We find that households in states that provide discharge packs are not much more likely to purchase the WIC contract brand than the households in states that have begun eliminating formula samples. Figure B.1 compares the fraction of non-WIC households that purchase the WIC brand in 10 states that began eliminating infant formula hospital discharge packs and 10 states that have

Table B.4: Comparison of purchases by purchaser's state's hospital characteristics

State Type	Cans Purchased	Total Spent	Duration (days)	Bulk
Top 10 states with discharge packs	9.55	311.23	270.54	0.73
Top 10 states w/o discharge packs	10.84	348.98	283.98	0.78
Other	10.13	324.82	276.50	0.74

Note: The table compares the purchases of non-WIC households by whether they live in one of the top 10 states that have eliminated formula samples in 2007 (WA, MN, CA, VT, MA, WI, NM, NH, OR, RI) or one of the top 10 states that continue to provide formula samples in 2007 (AR, DC, MD, MS, NJ, OK, SD, WV, IA, TX).
Source: Numerator 2018-2025.

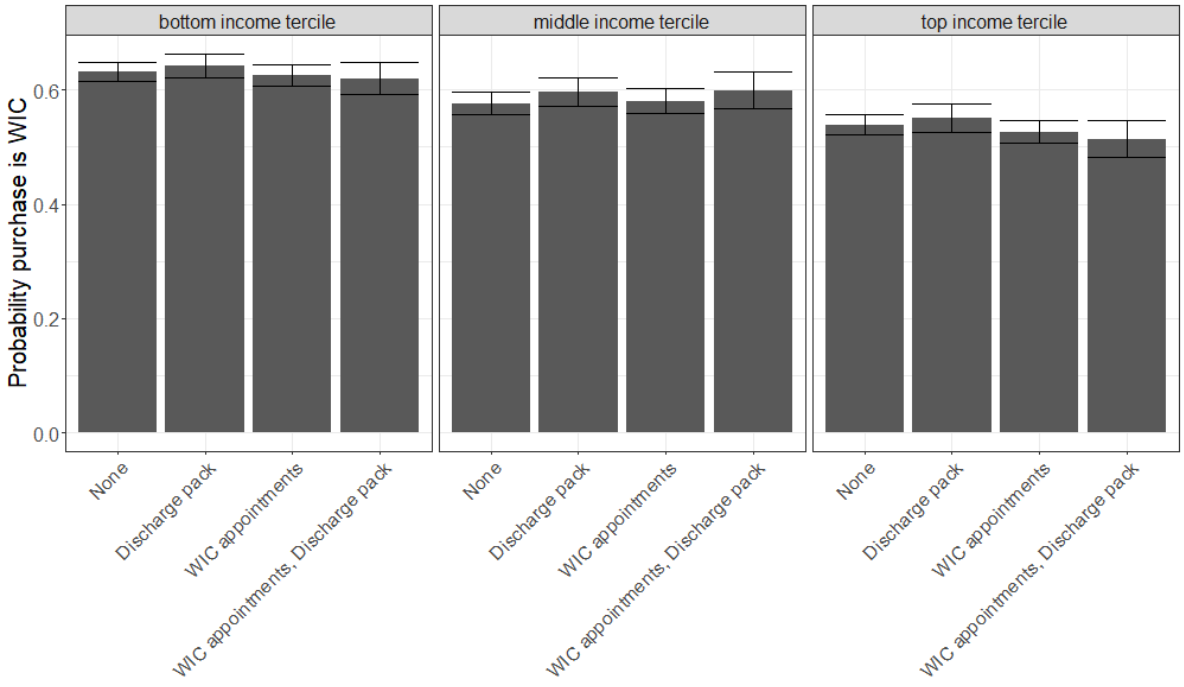
Figure B.1: Probability of WIC purchase by state hospital characteristics



Note: Figure compares the fraction of non-WIC households that purchase the WIC brand in 10 states that began eliminating infant formula hospital discharge packs and 10 states that have not eliminated them. Purchases are limited to regular-sized purchases that are the household's first observed purchase of infant formula. 95% confidence intervals are shown.
Source: Numerator 2018-2025.

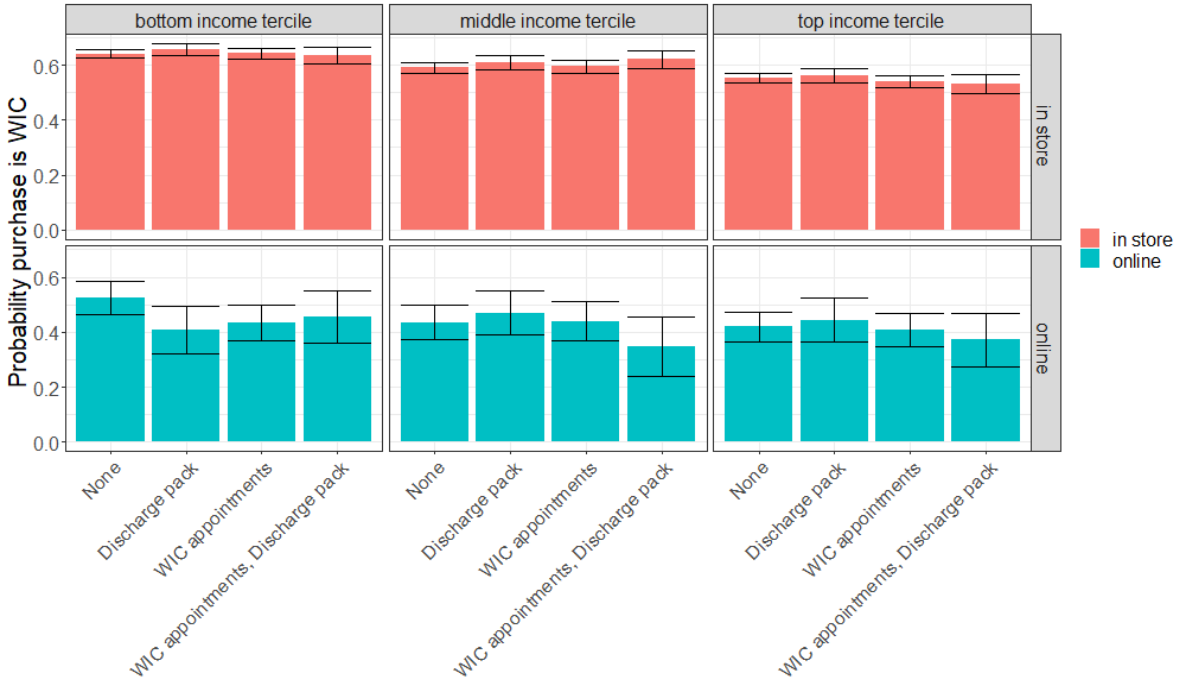
not eliminated them.

Figure B.2: Probability of WIC purchase by hospital characteristics and income



Note: Figure compares the fraction of non-WIC households that purchase the WIC brand among those whose nearest hospital neither provides discharge packs containing formula nor helps make WIC appointments, only provides discharge packs containing formula, only makes WIC appointments, or both. The left, middle, and right panels show purchases made by households in the bottom, middle, and top income tercile. Purchases are limited to regular-sized purchases that are the households' first observed purchase of infant formula. 95% confidence intervals are shown.
Source: Numerator data 2018-2025.

Figure B.3: Probability of WIC purchase by hospital characteristics, purchase method, and income



Note: Figure compares the fraction of non-WIC households that purchase the WIC brand among those whose nearest hospital neither provides discharge packs containing formula nor helps make WIC appointments, only provides discharge packs containing formula, only makes WIC appointments, or both. The top three panels show purchases made in store and the bottom three panels show purchases made online. The left, middle, and right panels show purchases made by households in the bottom, middle, and top income tercile. Purchases are limited to regular-sized purchases that are the households' first observed purchase of infant formula. 95% confidence intervals are shown.
Source: Numerator data 2018-2025.

B.2 Hospical Effects on Purchasing Any Premium Brands

Our discrete-choice specification allows the utility of the WIC-contracted brand to vary with characteristics of the household's hospital — whether the hospital distributes infant-formula discharge packs, helps schedule WIC appointments, or holds a Baby-Friendly designation — and the estimated hospital effects are modest. A natural concern is that these estimates are attenuated by measurement error. We observe whether a hospital distributes formula, but not which brand it distributes. Because the influence of a discharge pack is spread across whichever brands a hospital happens to stock, a brand-by-brand specification can understate the hospital's total effect on formula choice.

We assess this with a specification that does not require knowing the brand a hospital dispenses. Hospitals stock nationally branded formula and do not distribute private-label ("store") brands. If hospital exposure meaningfully steers households toward nationally branded formula, it should raise the probability that a household's first observed purchase is a branded (non-store) product, whatever the specific brand. We therefore estimate a linear probability model of NonStore_i on a hospital characteristic H_i (or the full set of three), demographics X_i, and state and first-purchase-year fixed effects, on non-WIC (income-ineligible) households, one observation per household, with standard errors clustered by state.

Table B.5 reports the estimates. None of the three hospital characteristics predicts branded-brand purchasing. Entered individually (columns 1–3), jointly (column 4), or jointly with demographic controls (column 5), the discharge-pack, WIC-appointment, and Baby-Friendly coefficients are small and insignificant; in the full specification they are $+0.003$, -0.006, and -0.002, and each 95% confidence interval lies within roughly two percentage points of zero against a baseline non-store share of 0.79.

These precisely estimated zeros indicate that the limited role of the hospital channel is not merely an artifact of our not observing which brand a hospital dispenses: even on the coarse branded-versus-store margin, where the brand-identity problem is absent, hospital characteristics do not move formula choice among non-WIC households. One source of attenuation remains — the hospital measures are proxies, assigned from the household's linked hospital rather than a verified record that a given mother received a discharge pack, so measurement error in treatment (as distinct from brand) could still bias these estimates toward zero.

Table B.5: Hospital characteristics and the probability of buying a non-store (branded) product at first observed purchase

	(1)	(2)	(3)	(4)	(5)
Discharge kit	0.0024			0.0022	0.0028
	(0.0062)			(0.0062)	(0.0062)
WIC appointments		−0.0048		−0.0048	−0.0063
		(0.0064)		(0.0065)	(0.0065)
Baby-friendly hospital			−0.0009	0.0005	−0.0015
			(0.0086)	(0.0088)	(0.0084)
Income ($100k)					−0.0224***
					(0.0060)
Household size					−0.0021
					(0.0025)
College or more					−0.0105*
					(0.0054)
Black					0.0754***
					(0.0073)
Hispanic					0.0305**
					(0.0141)
Asian					0.0400**
					(0.0162)
Demographics	No	No	No	No	Yes
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	21,824	21,824	21,824	21,824	21,824
R^2	0.008	0.008	0.008	0.008	0.013

Notes: Linear probability models on non-WIC (income-ineligible) households, one observation per household. The dependent variable equals one if the household's first observed purchase is a non-store (branded: Abbott, Mead Johnson, or Nestlé) product rather than a private-label store brand. *Discharge kit*, *WIC appointments*, and *Baby-friendly hospital* are indicators for the household's linked hospital (mPINC items $G5_A1$ and $E7_A2$, and Baby-Friendly USA designation, respectively). All columns include state and first-purchase-year fixed effects. Standard errors clustered by state in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

C Additional Event Study

C.1 Total Formula Purchases around Contract Changes

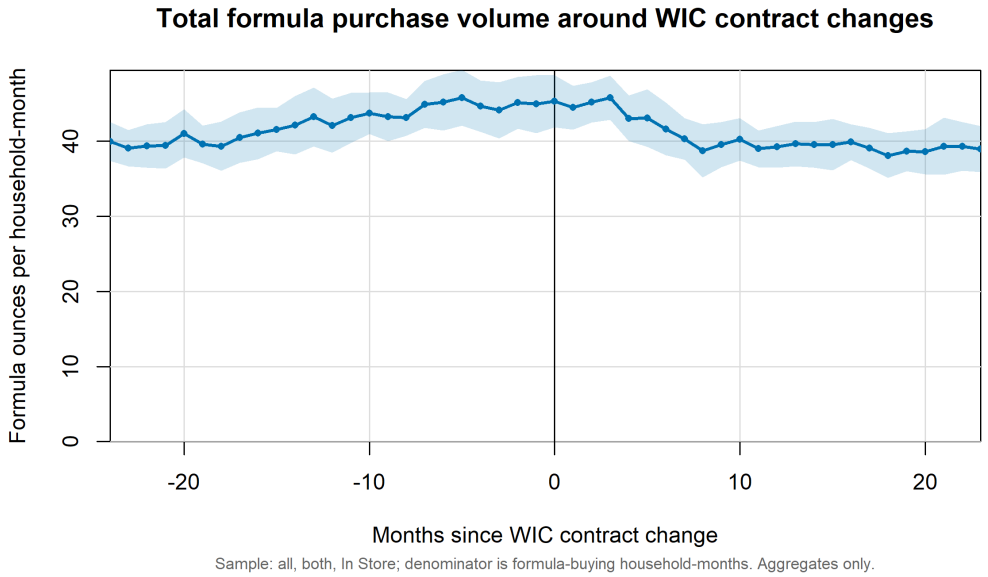


Figure C.4: Total formula purchase volume around WIC contract changes

Notes: The figure plots total infant-formula purchase volume around WIC contract changes, measured as ounces per formula-buying household-month. Event time is measured in months relative to the contract change date. The sample includes all in-store formula purchases of known regular or bulk size in states with genuine WIC contract changes after January 1, 2018, within a two-year window before and after each change. The solid line reports the pooled household-month weighted mean in each event month. The shaded region shows approximate 95% confidence intervals based on variation across state-event cells. The vertical line marks the contract-change month.

Figure C.4 examines whether WIC contract changes are associated with changes in overall formula purchase volume. The figure plots total formula ounces purchased per formula-buying household-month, pooling across manufacturers and across regular and bulk package sizes. The series is relatively stable around the event date. Thus, the manufacturer-level reallocation documented in the main event-study figures does not appear to reflect a large increase in total formula quantity purchased among observed formula-buying households.

C.2 Robustness of the Event Study to Staggered Contract Timing

The event-study regressions in Section 3 pool 27 WIC contract changes that occur in different states at different times. A recent econometric literature shows that in such staggered-adoption designs, two-way fixed-effects (TWFE) estimators recover a weighted average of treatment effects in which

some weights can be negative: states treated early implicitly serve as controls for states treated later, so if treatment effects vary across cohorts or evolve with time since treatment, the TWFE event-study path can be biased. This appendix assesses whether our results are affected, using the estimator of Callaway and Sant'Anna (2021), which is robust to this problem by construction.

We construct a state–manufacturer monthly panel and estimate separate effects for incoming and outgoing WIC suppliers. Treatment is defined by each state's first genuine contract-manufacturer change during the sample, beginning in the calendar month of the transition for changes effective on or before the fifteenth, and in the following month otherwise. The outcomes are the market shares of the incoming and outgoing manufacturers, with these identities held fixed before and after the transition. The Callaway–Sant'Anna estimator compares treated units with never-treated and not-yet-treated state–manufacturer units, excluding outgoing suppliers from the incoming-supplier model and incoming suppliers from the outgoing-supplier model. Eligible controls include manufacturers in states without an in-sample change and uninvolved manufacturers in switching states. Unlike the baseline's event-specific construction, the CS analysis retains only the first change in each state and censors observations from the assigned month of a second change onward.

Figure C.5 plots the CS event-study path for the new and former WIC suppliers alongside the baseline TWFE path. The two estimators are similar. Hence, we conclude that the demand spillover documented in Section 3 is not an artifact of staggered treatment timing.

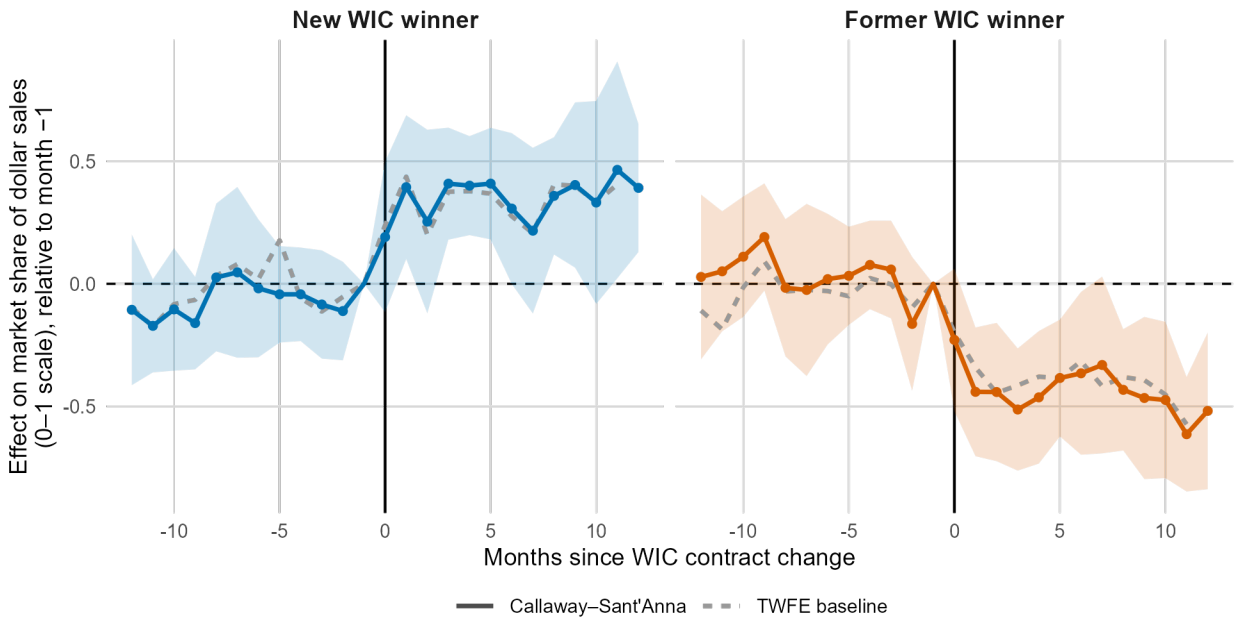


Figure C.5: Callaway–Sant'Anna and TWFE event-study estimates of the effect of WIC contract changes on non-WIC households' market shares

Notes: The solid lines report Callaway–Sant'Anna (2021) dynamic average treatment effects on the market share of the new (left panel) and former (right panel) WIC supplier among non-WIC households, by months since the contract change; shaded regions are simultaneous 95% confidence bands based on a multiplier bootstrap clustered by state. The grey dashed lines reproduce the baseline TWFE event-study path from Section 4. Shares are of regular-size, in-store dollar sales, on a 0–1 scale, normalized to zero in the month before the change. *Source:* Numerator data and USDA contract data, 2018–2025.

D Demand Model Robustness Checks

This appendix presents additional robustness checks for the empirical analysis. To conserve space, we report only the robustness results for the discrete choice model, which constitutes the main quantitative analysis of the paper. Across all robustness exercises, the qualitative conclusions remain unchanged: retail exposure effects remain substantially larger than hospital-related effects, and the results continue to support the importance of in-store retail mechanisms in generating the observed spillovers.

D.1 Excluding Abbott Recall Periods

First, we re-estimate the model after excluding the period affected by the Abbott infant formula recall beginning in February 2022. The recall generated substantial disruptions in supply and purchasing patterns nationwide, raising the concern that the estimated WIC effects could partly

reflect temporary shortages or substitution patterns induced by the recall rather than the WIC program itself. The results remain qualitatively similar after excluding the recall period.

Table D.6: Regular size - Abbott recall period excluded - All stores

	(1)	(2)	(3)	(4)
(Intercept):Abbott_regular	0.308***	0.428***	0.313***	0.435***
	(0.066)	(0.077)	(0.067)	(0.077)
(Intercept):MJ_regular	0.399***	0.426***	0.401***	0.429***
	(0.068)	(0.078)	(0.068)	(0.078)
(Intercept):Nestle_regular	−1.050***	−1.461***	−1.048***	−1.460***
	(0.092)	(0.104)	(0.093)	(0.104)
price	−0.856***	−0.859***	−0.349***	−0.344***
	(0.042)	(0.040)	(0.126)	(0.123)
price:WICstore			−0.651***	−0.661***
			(0.139)	(0.136)
price:nonWICstore			−0.486***	−0.494***
			(0.133)	(0.130)
WICproduct	0.570***	0.586***	0.666***	0.688***
	(0.113)	(0.134)	(0.118)	(0.138)
WICproduct:WICstore	0.546***	0.493***	0.425***	0.364***
	(0.116)	(0.113)	(0.120)	(0.117)
WICproduct:nonWICstore	0.324***	0.301***	0.241**	0.214*
	(0.112)	(0.109)	(0.117)	(0.114)
WICproduct:baby_fre_hosp	0.038	0.013	0.034	0.010
	(0.087)	(0.085)	(0.088)	(0.086)
WICproduct:hospital_sample	0.051	0.030	0.047	0.025
	(0.062)	(0.061)	(0.063)	(0.062)
WICproduct:WIC_appointments	−0.025	−0.037	−0.028	−0.041
	(0.059)	(0.058)	(0.059)	(0.058)
WICproduct:Nestle		1.135***		1.138***
		(0.159)		(0.160)
WICproduct:Abbott		−0.190		−0.192
		(0.156)		(0.157)
sd.price	0.261***	0.257***	0.305***	0.301***
	(0.025)	(0.024)	(0.030)	(0.030)
sd.WICproduct	0.454***	0.282	0.463***	0.292*
	(0.160)	(0.174)	(0.162)	(0.175)
Included Stores	All	All	All	All
Observations	6,074	6,074	6,074	6,074
R^2	0.226	0.232	0.229	0.235
Log Likelihood	−4,973.926	−4,936.709	−4,952.152	−4,914.440

Table D.7: Both sizes - Abbott recall period excluded - All stores

	(5)	(6)	(7)	(8)
(Intercept):Abbott_bulk	0.527***	0.581***	0.529***	0.584***
	(0.057)	(0.060)	(0.057)	(0.060)
(Intercept):Abbott_regular	0.276***	0.374***	0.278***	0.376***
	(0.061)	(0.063)	(0.061)	(0.063)
(Intercept):MJ_bulk	0.990***	0.982***	0.993***	0.984***
	(0.057)	(0.059)	(0.057)	(0.059)
(Intercept):MJ_regular	0.417***	0.442***	0.417***	0.441***
	(0.061)	(0.064)	(0.061)	(0.064)
(Intercept):Nestle_bulk	−1.128***	−1.350***	−1.121***	−1.343***
	(0.073)	(0.075)	(0.073)	(0.075)
(Intercept):Nestle_regular	−0.987***	−1.387***	−0.981***	−1.381***
	(0.079)	(0.090)	(0.079)	(0.090)
(Intercept):Store_bulk	0.370***	0.357***	0.376***	0.362***
	(0.058)	(0.058)	(0.058)	(0.058)
price	−1.166***	−1.180***	−0.843***	−0.862***
	(0.021)	(0.021)	(0.057)	(0.057)
price:WICstore			−0.242***	−0.232***
			(0.063)	(0.063)
price:nonWICstore			−0.419***	−0.415***
			(0.059)	(0.059)
WICproduct	0.358***	0.354***	0.268***	0.269***
	(0.075)	(0.082)	(0.077)	(0.084)
WICproduct:WICstore	0.635***	0.605***	0.709***	0.675***
	(0.075)	(0.075)	(0.078)	(0.078)
WICproduct:nonWICstore	0.346***	0.332***	0.464***	0.446***
	(0.073)	(0.073)	(0.076)	(0.076)
WIC_bulk	−0.355***	−0.316***	−0.287***	−0.250***
	(0.085)	(0.085)	(0.086)	(0.086)
WIC_bulk_WICstore	−0.426***	−0.428***	−0.480***	−0.480***
	(0.087)	(0.087)	(0.089)	(0.089)
WIC_bulk_nonWICstore	−0.179**	−0.181**	−0.271***	−0.271***
	(0.085)	(0.085)	(0.086)	(0.086)
WICproduct:baby_fre_hosp	0.058	0.043	0.057	0.042
	(0.045)	(0.045)	(0.045)	(0.045)
WICproduct:hospital_sample	0.100***	0.089***	0.100***	0.089***
	(0.033)	(0.033)	(0.033)	(0.033)
WICproduct:WIC_appointments	−0.018	−0.025	−0.018	−0.024
	(0.031)	(0.031)	(0.031)	(0.031)
WICproduct:Nestle		1.136***		1.135***
		(0.095)		(0.095)
WICproduct:Abbott		−0.132**		−0.134**
		(0.066)		(0.066)
sd.price	0.170***	0.171***	0.180***	0.180***
	(0.013)	(0.013)	(0.014)	(0.014)
sd.WICproduct	0.080	−0.012	0.078	−0.015
	(0.093)	(0.094)	(0.093)	(0.094)
Included Stores	All	All	All	All
Observations	19,089	19,089	19,089	19,089
R^2	0.103	0.106	0.103	0.106
Log Likelihood	−30,761.600	−30,669.150	−30,745.840	−30,653.440

D.2 All Purchase Records

Second, we estimate the model using all purchase observations rather than restricting the analysis to households' first infant formula purchase. Our baseline specification focuses on first purchases in order to minimize the role of dynamic considerations such as state dependence, habit formation, or inventory accumulation. Including all purchases provides a robustness check for whether the estimated spillover effects persist once repeat purchasing behavior is incorporated into the analysis. The results remain broadly consistent with the baseline specification.

Table D.8: Both sizes - All Purchase Records - All stores

	(1)	(2)	(3)	(4)
(Intercept):Abbott_bulk	0.658***	0.675***	0.654***	0.672***
	(0.031)	(0.032)	(0.031)	(0.032)
(Intercept):Abbott_regular	0.198***	0.257***	0.192***	0.253***
	(0.033)	(0.035)	(0.033)	(0.035)
(Intercept):MJ_bulk	1.147***	1.165***	1.142***	1.160***
	(0.031)	(0.032)	(0.031)	(0.032)
(Intercept):MJ_regular	0.464***	0.521***	0.456***	0.512***
	(0.033)	(0.034)	(0.033)	(0.034)
(Intercept):Nestle_bulk	−1.015***	−1.215***	−1.016***	−1.217***
	(0.039)	(0.040)	(0.039)	(0.040)
(Intercept):Nestle_regular	−0.828***	−1.188***	−0.831***	−1.192***
	(0.043)	(0.048)	(0.043)	(0.048)
(Intercept):Store_bulk	0.574***	0.563***	0.572***	0.561***
	(0.031)	(0.031)	(0.031)	(0.031)
price	−1.122***	−1.135***	−1.039***	−1.061***
	(0.011)	(0.011)	(0.025)	(0.025)
price:WICstore			0.033	0.046
			(0.029)	(0.029)
price:nonWICstore			−0.172***	−0.164***
			(0.027)	(0.027)
WICproduct	0.110***	0.043	0.086**	0.024
	(0.035)	(0.039)	(0.036)	(0.039)
WICproduct:WICstore	0.837***	0.803***	0.837***	0.799***
	(0.035)	(0.035)	(0.036)	(0.036)
WICproduct:nonWICstore	0.440***	0.421***	0.491***	0.470***
	(0.034)	(0.034)	(0.035)	(0.035)
WIC_bulk	−0.180***	−0.138***	−0.167***	−0.127***
	(0.038)	(0.038)	(0.038)	(0.038)
WIC_bulk_WICstore	−0.596***	−0.593***	−0.589***	−0.583***
	(0.040)	(0.040)	(0.040)	(0.040)
WIC_bulk_nonWICstore	−0.188***	−0.186***	−0.222***	−0.219***
	(0.038)	(0.038)	(0.038)	(0.038)
WICproduct:baby_fre_hosp	0.113***	0.097***	0.114***	0.098***
	(0.023)	(0.023)	(0.023)	(0.023)
WICproduct:hospital_sample	−0.013	−0.018	−0.013	−0.019
	(0.017)	(0.017)	(0.017)	(0.017)
WICproduct:WIC_appointments	0.029*	0.020	0.030*	0.020
	(0.016)	(0.016)	(0.016)	(0.016)
WICproduct:Nestle		1.228***		1.228***
		(0.052)		(0.052)
WICproduct:Abbott		0.005		0.001
		(0.034)		(0.034)
sd.price	0.218***	0.218***	0.236***	0.236***
	(0.008)	(0.008)	(0.009)	(0.009)
sd.WICproduct	0.155***	0.069	0.155***	0.070
	(0.049)	(0.049)	(0.049)	(0.049)
Included Stores	All	All	All	All
Observations	70,505	70,505	70,505	70,505
R^2	0.103	0.106	0.103	0.106
Log Likelihood	−111,486.9	−111,180.6	−111,445.3	−111,138.1

D.3 Capacity Contraints

Third, we re-estimate the model using only large retailers. One limitation of the retail-channel analysis is that we do not directly observe shelf-space allocation or the complete set of products available at each retailer. Smaller retailers may not carry all infant formula brands because of capacity constraints or local assortment decisions, implying that observed demand shifts could partly reflect differences in product availability rather than retail prominence alone. Restricting the analysis to large retailers mitigates this concern because these retailers are more likely to carry the full set of major national brands and therefore provide a more complete observed choice set. The estimated retail exposure effects remain qualitatively similar in this restricted sample.

Table D.9: Both sizes - Abbott recall period included - Big stores

	(1)	(2)	(3)	(4)
(Intercept):Abbott_bulk	0.896***	0.209**	0.896***	−0.079
	(0.084)	(0.082)	(0.084)	(0.081)
(Intercept):Abbott_regular	2.520***	0.994***	2.519***	1.439***
	(0.088)	(0.086)	(0.088)	(0.087)
(Intercept):MJ_bulk	2.978***	1.469***	2.976***	1.823***
	(0.085)	(0.082)	(0.085)	(0.084)
(Intercept):MJ_regular	2.922***	1.327***	2.920***	2.078***
	(0.088)	(0.087)	(0.088)	(0.090)
(Intercept):Nestle_bulk	0.210**	−1.154***	0.211**	−1.344***
	(0.099)	(0.095)	(0.099)	(0.096)
(Intercept):Nestle_regular	1.036***	−0.763***	1.037***	−0.585***
	(0.098)	(0.103)	(0.098)	(0.101)
(Intercept):Store_bulk	1.214***	0.364***	1.214***	−0.062
	(0.087)	(0.079)	(0.087)	(0.080)
price	−4.623***	−2.486***	−4.470***	−3.409***
	(0.087)	(0.046)	(0.218)	(0.169)
price:WICstore			−0.055	−0.098
			(0.224)	(0.178)
price:nonWICstore			−0.221	−1.359***
			(0.216)	(0.173)
WICproduct	0.400**	0.375**	0.383**	−0.004
	(0.157)	(0.150)	(0.157)	(0.207)
WICproduct:WICstore	0.547***	0.531***	0.555***	0.553***
	(0.159)	(0.142)	(0.159)	(0.200)
WICproduct:nonWICstore	0.310**	0.282**	0.337**	0.639***
	(0.156)	(0.139)	(0.156)	(0.196)
WIC_bulk	−0.172	−0.229	−0.155	0.162
	(0.179)	(0.162)	(0.179)	(0.174)
WIC_bulk_WICstore	−0.552***	−0.536***	−0.558***	−0.552***
	(0.185)	(0.168)	(0.185)	(0.180)
WIC_bulk_nonWICstore	−0.317*	−0.245	−0.344*	−0.650***
	(0.181)	(0.164)	(0.181)	(0.176)
WICproduct:baby_fre_hosp	0.119	0.088	0.119	0.094
	(0.083)	(0.075)	(0.083)	(0.118)
WICproduct:hospital_sample	0.111*	0.035	0.112*	0.080
	(0.060)	(0.055)	(0.060)	(0.085)
WICproduct:WIC_appointments	0.028	0.001	0.028	−0.038
	(0.055)	(0.050)	(0.055)	(0.078)
WICproduct:Nestle		1.139***		1.415***
		(0.121)		(0.170)
WICproduct:Abbott		−0.151		−0.156
		(0.105)		(0.128)
sd.price	2.908***	−0.770***	2.912***	1.728***
	(0.078)	(0.025)	(0.078)	(0.040)
sd.WICproduct	−0.050	−0.193	−0.051	−1.890***
	(0.143)	(0.144)	(0.143)	(0.166)
Included Stores	Big	Big	Big	Big
Observations	8,682	8,682	8,682	8,682
R^2	0.205	0.167	0.205	0.172
Log Likelihood	−12,884.330	−13,494.790	−12,883.180	−13,413.230

D.4 Selection on Observables

Finally, we examine whether the results are sensitive to potential selection concerns arising from differences in household characteristics across shopping environments. As documented in the descriptive statistics, households that shop online, shop at WIC-authorized retailers, or are exposed to different hospital environments differ systematically in demographics such as income, education, and race. To assess whether the estimated spillover effects are driven by such compositional differences, we re-estimate the model including household demographic controls and interactions. The estimated coefficients remain qualitatively similar after controlling for these household characteristics, suggesting that selection on observable demographics is unlikely to fully explain the estimated retail exposure effects.

Table D.10: Both sizes - Abbott recall period included - All stores - Household Characteristics

	Coef.	S.E.
(Intercept):Abbott_bulk	−0.050	(0.173)
(Intercept):Abbott_regular	0.432***	(0.060)
(Intercept):MJ_bulk	0.593***	(0.173)
(Intercept):MJ_regular	0.635***	(0.059)
(Intercept):Nestle_bulk	−1.731***	(0.177)
(Intercept):Nestle_regular	−1.112***	(0.077)
(Intercept):Store_bulk	−0.205	(0.172)
price	−1.192***	(0.149)
price:WICstore	−0.285***	(0.061)
price:nonWICstore	−0.437***	(0.058)
price:College	0.120	(0.076)
price:High_School	0.125*	(0.076)
price:Advanced	0.271***	(0.080)
price:Mid_income	−0.204*	(0.120)
price:High_income	−0.054	(0.125)
price:Hispanic	−0.195***	(0.064)
price:Asian	0.278***	(0.042)
price:Black	0.193***	(0.043)
price:Other	0.595***	(0.050)
WICproduct	0.287	(0.176)
WICproduct:WICstore	0.678***	(0.074)
WICproduct:nonWICstore	0.426***	(0.073)
WICproduct:hospital_sample	0.083***	(0.032)
WICproduct:WIC_appointments	−0.007	(0.029)
WICproduct:baby_fre_hosp	0.056	(0.043)
WICproduct:College	0.019	(0.121)
WICproduct:High_School	0.089	(0.125)
WICproduct:Advanced	−0.103	(0.125)
WICproduct:Mid_income	−0.135	(0.107)
WICproduct:High_income	−0.241**	(0.111)
WICproduct:Hispanic	0.212***	(0.052)
WICproduct:Asian	0.055	(0.057)
WICproduct:Black	0.232***	(0.054)
WICproduct:Other	0.092	(0.078)
WICproduct:Nestle	1.038***	(0.085)
WICproduct:Abbott	−0.088	(0.063)
WIC_bulk	−0.153*	(0.082)
WIC_bulk_WICstore	−0.561***	(0.085)
WIC_bulk_nonWICstore	−0.290***	(0.083)
College:isbulk	0.357***	(0.124)
High_School:isbulk	0.285**	(0.128)
Advanced:isbulk	0.383***	(0.129)
Mid_income:isbulk	0.043	(0.112)
High_income:isbulk	0.259**	(0.118)
Hispanic:isbulk	−0.018	(0.057)
Asian:isbulk	0.317***	(0.063)
Black:isbulk	−0.236***	(0.056)
Other:isbulk	0.053	(0.082)
sd.price	0.309***	(0.016)
sd.WICproduct	−0.010	(0.089)
Included Stores	All	
Observations	21,811	
R^2	0.121	
Log Likelihood	−34,872.440	

First, the estimated WIC spillover effects remain economically large after controlling for detailed household characteristics. The retailer interaction terms remain positive and statistically significant, with a larger estimated premium at WIC-authorized retailers than at unauthorized retailers. This pattern indicates that the spillover effect is strongest in WIC-authorized retailers, supporting the interpretation that retail exposure and in-store visibility are central mechanisms behind the spillovers. The effects remain positive even in non-WIC stores, suggesting that cross-store shopping behavior also contributes to spillovers.

Second, the interactions with household demographics reveal meaningful heterogeneity in responsiveness to WIC exposure. The spillover effects tend to be stronger among Hispanic and Black households, while the interactions for high-income households are negative. This pattern is consistent with the idea that lower-income or demographically closer households may be more exposed to WIC-related retail environments or more responsive to retail visibility associated with WIC products. By contrast, education interactions are generally small and statistically insignificant, suggesting that the spillover is not primarily driven by differences in educational attainment.

Third, the hospital-related coefficients are generally smaller than the retailer interaction terms. The interaction with hospital formula provision is positive and statistically significant, while the Baby-Friendly and WIC-appointment interactions are small and statistically insignificant. Overall, the substantially larger retailer interaction coefficients are consistent with an important role for the retail environment in generating the observed spillovers.

Finally, the price coefficients imply substantial heterogeneity in price sensitivity across demographic groups. Lower education groups and Hispanic households tend to be more price sensitive, while Asian and Black households exhibit lower price sensitivity in several specifications. Importantly, however, the inclusion of these rich household-level interactions does not materially alter the estimated WIC spillover effects, suggesting that the baseline findings are not driven by omitted household composition differences.

D.5 Store Heterogeneity

The final robustness check addresses the concern that WIC-authorized and WIC-unauthorized retailers may differ along dimensions other than their authorization status. For example, authorized retailers may systematically differ in size, customer composition, product assortment, or other unobserved characteristics that could be correlated with consumer demand. If so, the estimated WIC effects may partly reflect cross-store differences rather than differences in retail exposure

induced by the WIC program.

To address this concern, we introduce flexible controls for retailer heterogeneity. Intuitively, this approach compares changes in demand within the same retailer as WIC contract status changes, thereby controlling for time-invariant store characteristics. An empirical challenge, however, is that estimating a discrete choice model with retailer fixed effects requires a very large number of fixed effects. Following the idea of Bonhomme et al. (2022), we approximate the high-dimensional fixed-effect structure by clustering retailers using k-means based on retailer sales and WIC authorization status. We then include cluster-by-store-type-by-product fixed effects in the discrete choice model.

Table D.11 reports the results. The estimated retailer premiums remain positive and statistically significant with the grouped store controls. However, the authorized-store premium declines and the gap across retailer types narrows, indicating that the precise decomposition is sensitive to store heterogeneity. Overall, the results continue to support an important role for retail exposure in generating the observed spillovers.

Table D.11: Both sizes - Store Type FE

	(5)	(6)
Main variables		
price	-1.502^{***} (0.021)	-1.516^{***} (0.021)
WICproduct	0.399^{***} (0.101)	0.393^{***} (0.106)
WICproduct:WICstore	0.446^{***} (0.108)	0.388^{***} (0.108)
WICproduct:nonWICstore	0.275^{***} (0.106)	0.246^{**} (0.106)
WICproduct:hospital_sample	0.094^{***} (0.031)	0.083^{***} (0.031)
WICproduct:WIC_appointments	0.009 (0.029)	0.002 (0.029)
WICproduct:baby_fre_hosp	0.078^{*} (0.042)	0.063 (0.042)
WIC_bulk	-0.309^{**} (0.124)	-0.283^{**} (0.124)
WIC_bulk_WICstore	-0.308^{**} (0.135)	-0.297^{**} (0.135)
WIC_bulk_nonWICstore	-0.238^{*} (0.131)	-0.223^{*} (0.131)
WICproduct:Nestle		1.026^{***} (0.084)
WICproduct:Abbott		-0.095 (0.062)
sd.price	0.253^{***} (0.015)	0.253^{***} (0.015)
sd.WICproduct	0.112 (0.089)	0.027 (0.090)
Observations	22,065	22,065
Sales Cluster × Store Type × Product FE	Yes	Yes
Log Likelihood	−35,266.670	−35,171.870

Store Choice. One concern is that households may systematically sort into different retail environments. For example, households with stronger preferences for WIC brands may be more likely to shop at WIC-authorized retailers, causing the estimated retailer premiums to partly reflect con-

sumer selection rather than differences in the retail environment. To address this concern, we group households based on their online shopping intensity and allow baseline product preferences to vary flexibly across these household groups by including consumer-cluster-by-product fixed effects. Table D.12 reports the results. The WIC-authorized-store premium remains large and statistically significant, while the unauthorized-store premium is smaller but remains positive and statistically significant in the richer specification. Overall, these results suggest that persistent differences in product preferences across household shopping types do not account for the estimated retailer premiums.

D.6 Demand Estimates with State-by-Manufacturer Fixed Effects

Persistent regional preferences for formula manufacturers could confound the relationship between WIC contract status and household demand. To assess this concern, we augment the mixed-logit model with state-by-manufacturer fixed effects. These intercepts allow households in each state to have different underlying preferences for Abbott, Mead Johnson, and Nestlé relative to store brands. Each state–manufacturer intercept applies to both regular and bulk products, while the model retains the baseline brand-size intercepts. Thus, the specification absorbs persistent regional manufacturer preferences but does not allow the regional adjustment to differ by package size. We retain the original price and WIC interactions and the random coefficients on price and the WIC-brand indicator. Both specifications use the full estimation sample of 22,065 income-ineligible households never observed using WIC benefits, rather than restricting the sample to switching states or an event window.[36]

Table D.13 compares the baseline and augmented specifications using 1,000 Halton draws and standard errors clustered by state. The retailer-related coefficients are very similar across the two columns. For regular-size products, the additional WIC-brand coefficient at authorized retailers changes from 0.705 to 0.692, while the corresponding coefficient at unauthorized retailers changes from 0.419 to 0.417. Their difference changes from 0.286 to 0.275 and remains statistically significant at the 1% level. The bulk-size interactions also remain close to their baseline values; the net authorized–unauthorized difference for bulk products remains close to zero and statistically insignificant.

[36]The regional effects are normalized relative to Alaska and store brands. Nestlé is never chosen in Connecticut, Delaware, or DC in this estimation sample, so the corresponding intercepts are evaluated at their limiting values of minus infinity. This assigns zero choice probability to those alternatives while retaining all households and observed choices. The remaining 147 normalized regional effects are estimated as finite parameters.

Table D.12: Both Sizes – All Purchases – Consumer Cluster Fixed Effects

	(1)	(2)
Price	-0.925^{***}	-0.658^{***}
	(0.015)	(0.031)
Price × WIC store		-0.162^{***}
		(0.037)
Price × non-WIC store		-0.427^{***}
		(0.035)
WIC product	0.258^{***}	0.226^{***}
	(0.051)	(0.052)
WIC product × WIC store	0.509^{***}	0.517^{***}
	(0.050)	(0.050)
WIC product × non-WIC store	0.060	0.136^{***}
	(0.049)	(0.049)
WIC product × hospital sample	-0.069^{***}	-0.069^{***}
	(0.022)	(0.022)
WIC product × WIC appointments	0.031	0.032
	(0.020)	(0.020)
WIC product × Baby-Friendly hospital	0.111^{***}	0.109^{***}
	(0.030)	(0.030)
WIC bulk	-0.180^{***}	-0.157^{***}
	(0.052)	(0.052)
WIC bulk × WIC store	-0.545^{***}	-0.545^{***}
	(0.057)	(0.057)
WIC bulk × non-WIC store	−0.078	-0.134^{**}
	(0.056)	(0.056)
WIC product × Nestlé	1.352^{***}	1.350^{***}
	(0.071)	(0.071)
WIC product × Abbott	0.044	0.036
	(0.042)	(0.042)
SD price	0.275^{***}	0.302^{***}
	(0.013)	(0.014)
SD WIC product	0.028	0.026
	(0.062)	(0.062)
Consumer cluster × product FE	Yes	Yes
Retailer-specific price coefficients	No	Yes
Observations	45,046	45,046
Log Likelihood	$-69,392.710$	$-69,329.720$

Notes: The table reports estimates from the discrete choice model including consumer-cluster-by-product fixed effects. Households are grouped according to their online shopping intensity. Consumer-cluster-by-product fixed-effect estimates are omitted for brevity. Column (2) additionally allows price sensitivity to differ between WIC-authorized and unauthorized retailers. Standard errors are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

The baseline WIC-brand coefficient is essentially unchanged, from 0.256 to 0.257, but its standard error increases from 0.094 to 0.134, leaving it significant at the 10% level. This coefficient describes the online reference channel at the reference values of the hospital and manufacturer interactions, rather than an average WIC-brand premium across households. The formula-sample interaction is smaller in the augmented specification, declining from 0.104 to 0.052, and is also significant at the 10% level.

Overall, the stability of the retailer coefficients suggests that persistent state-specific manufacturer preferences do not account for the estimated retailer-related WIC-brand premiums. This exercise does not absorb time-varying regional preferences or household sorting across retail channels. It therefore supports the robustness of the demand estimates without separately establishing that the retailer coefficients capture only shelf availability or WIC labeling.

D.7 Demand Estimates around WIC Contract Changes

As a complementary robustness exercise, we re-estimate the demand model using purchases in states that change WIC contract manufacturers, restricting attention to observations close to those changes. This focuses the estimation on states and periods surrounding a change in contract status. We retain the baseline mixed-logit specification with regular and bulk alternatives and apply the event-window restriction to the original first-purchase observations of income-ineligible households never observed using WIC benefits. We do not select a new first purchase within each window or add state-by-manufacturer fixed effects in this exercise.

We first use a twelve-month window on either side of each contract change. Eligible events must have complete calendar coverage, no second contract change within the window, and observed purchases on both sides. We then narrow the window to six months using the events admitted to the twelve-month exercise, again requiring purchases on both sides. The resulting samples contain 3,260 households in 17 states and 1,717 households in 15 states, respectively.[37]

Table D.14 compares these estimates with the full-sample baseline. The additional regular-size WIC-brand coefficients at authorized and unauthorized retailers remain positive. In the twelve-month window, these coefficients are 0.585 and 0.497, respectively, significant at the 1% and 10% levels. In the six-month window, they are 1.122 and 0.898, significant at the 1% and 5% levels. However, the baseline WIC-brand coefficient changes from 0.256 in the full sample to -0.010 and

[37] South Dakota and Vermont lack purchases on both sides within the six-month window. Windows include the starting date and exclude the ending date, and households are never duplicated across events. Observations whose saved contract-winner assignments conflict with the contract calendar are excluded rather than relabeled.

Table D.13: Demand estimates with state-by-manufacturer fixed effects

	Baseline	State × manufacturer FE
Price	-2.511^{***}	-2.566^{***}
	(0.095)	(0.102)
WIC brand	0.256^{***}	0.257^{*}
	(0.094)	(0.134)
WIC brand × authorized	0.705^{***}	0.692^{***}
	(0.070)	(0.071)
WIC brand × unauthorized	0.419^{***}	0.417^{***}
	(0.074)	(0.075)
WIC bulk	−0.171	−0.175
	(0.114)	(0.115)
WIC bulk × authorized	-0.576^{***}	-0.574^{***}
	(0.094)	(0.095)
WIC bulk × unauthorized	-0.295^{**}	-0.296^{**}
	(0.112)	(0.113)
WIC brand × formula sample	0.104^{***}	0.052^{*}
	(0.032)	(0.028)
WIC brand × WIC appointments	−0.008	−0.020
	(0.041)	(0.035)
WIC brand × Baby-Friendly	0.040	0.016
	(0.055)	(0.059)
WIC brand × Nestlé	1.164^{***}	0.717^{*}
	(0.175)	(0.366)
WIC brand × Abbott	−0.157	−0.055
	(0.119)	(0.157)
SD price	2.138	2.183
	(0.063)	(0.062)
SD WIC brand	0.003	0.003
	(0.006)	(0.004)
Authorized minus unauthorized (regular)	0.286^{***}	0.275^{***}
	(0.069)	(0.067)
Authorized minus unauthorized (bulk)	0.005	−0.003
	(0.080)	(0.084)
Brand-size intercepts	Yes	Yes
State × manufacturer fixed effects	No	Yes
Households	22,065	22,065
States/DC	51	51
Halton draws	1,000	1,000

Notes: Both columns estimate the mixed-logit model with regular and bulk alternatives using the same 22,065 income-ineligible households never observed using WIC benefits, with one first-purchase observation per household. The second column adds state-by-manufacturer intercepts shared across regular and bulk sizes. Store brand and Alaska provide the manufacturer and state normalizations. Price and the WIC-brand coefficient are independently normally distributed; SD denotes their estimated standard deviations. WIC brand indicates the state's contract manufacturer, and WIC bulk is its interaction with the bulk-size indicator. Authorized and unauthorized refer to retailer WIC authorization, with online purchases as the reference channel. The reported WIC-brand coefficient is conditional on the reference values of the hospital and manufacturer interactions. The regular-size contrast is the difference between the two retailer interaction coefficients; the bulk-size contrast also includes their respective bulk interactions. State-clustered standard errors appear in parentheses. Stars use two-sided $t(50)$ tests: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$. No stars are assigned to standard-deviation parameters. Three Nestlé intercepts (Connecticut, Delaware, and DC) are evaluated at their zero-choice-probability limits; all households and observed choices are retained. Brand-size intercepts and regional fixed-effect coefficients are omitted from the table.

−0.501 in the two event windows; neither restricted- sample estimate is statistically significant. These coefficients describe the online reference channel at the reference values of the hospital and manufacturer interactions, rather than average premiums across households.

The regular-size authorized–unauthorized difference is 0.088 in the twelve-month window and 0.224 in the six-month window, compared with 0.286 in the full sample. Both restricted-sample differences are statistically insignificant, with standard errors of 0.170 and 0.145. Thus, the event-window estimates continue to show positive retailer-related increments relative to the online reference channel, but provide less precise evidence on the additional premium at authorized retailers. They do not establish that this additional premium is zero. The bulk interactions remain negative, although their precision also varies across windows.

This exercise changes the estimation population as well as the time period, so differences from the baseline should not be attributed solely to proximity to a contract change. Moreover, restricting observations to switching states does not make all demand parameters depend exclusively on within-state contract changes. Together with the full-sample fixed-effects exercise in Appendix D.6, these results support the persistence of retailer-related WIC-brand premiums while indicating greater uncertainty about their allocation across channels in the smaller event-window samples. Neither exercise alone separates shelf availability from labeling or other retailer characteristics.

E Implied Bertrand Margins

This appendix describes how we compute the elasticities and the implied Bertrand margins in Table 10 and Appendix Table E.15. All calculations use the main demand model (Table 9, column 2). We do not estimate a supply model. Instead, we ask what margins and prices the estimated demand system would imply if manufacturers set prices as in a static Bertrand–Nash game.

Demand and elasticities. Let $P_{ij}(\theta)$ denote the probability that the household in choice situation i chooses product j. There are eight products: Abbott, Mead Johnson, Nestlé, and store-brand formula, each in regular and bulk sizes. The probability is simulated as $P_{ij} = R^{-1} \sum_r P_{ijr}$, where P_{ijr} is the logit probability evaluated at draw r of the random coefficients on price and on the WIC brand. We use $R = 300$ draws, the same draws (common random numbers) in every calculation, and the same seed throughout. A product that is not in a situation's choice set receives zero

Table D.14: Demand estimates around WIC contract changes

	Full sample	±12 months	±6 months
Price	-2.511^{***}	-4.771^{***}	-5.259^{***}
	(0.095)	(0.624)	(0.808)
WIC brand	0.256^{***}	-0.010	-0.501
	(0.094)	(0.311)	(0.299)
WIC brand × authorized	0.705^{***}	0.585^{***}	1.122^{***}
	(0.070)	(0.183)	(0.250)
WIC brand × unauthorized	0.419^{***}	0.497^{*}	0.898^{**}
	(0.074)	(0.284)	(0.321)
WIC bulk	-0.171	-0.178	-0.166
	(0.114)	(0.430)	(0.511)
WIC bulk × authorized	-0.576^{***}	-0.492^{*}	-0.834^{**}
	(0.094)	(0.281)	(0.320)
WIC bulk × unauthorized	-0.295^{**}	-0.298	-0.703^{*}
	(0.112)	(0.401)	(0.373)
WIC brand × formula sample	0.104^{***}	0.079	0.138
	(0.032)	(0.063)	(0.164)
WIC brand × WIC appointments	-0.008	-0.080	-0.003
	(0.041)	(0.077)	(0.065)
WIC brand × Baby-Friendly	0.040	0.087	0.163
	(0.055)	(0.179)	(0.298)
WIC brand × Nestlé	1.164^{***}	1.035^{**}	1.154^{**}
	(0.175)	(0.427)	(0.395)
WIC brand × Abbott	-0.157	-0.001	0.076
	(0.119)	(0.398)	(0.381)
SD price	2.138	3.013	3.216
	(0.063)	(0.290)	(0.232)
SD WIC brand	0.003	0.004	0.001
	(0.006)	(0.002)	(0.002)
Authorized minus unauthorized (regular)	0.286^{***}	0.088	0.224
	(0.069)	(0.170)	(0.145)
Brand-size intercepts	Yes	Yes	Yes
Households	22,065	3,260	1,717
States/DC	51	17	15
Halton draws	1,000	1,000	1,000

Notes: All columns use the mixed-logit specification with regular and bulk alternatives and income-ineligible households never observed using WIC benefits. The first column uses the full sample. The remaining columns restrict the original first purchases to switching states within twelve or six calendar months of a WIC contract-manufacturer change. Events must have complete calendar coverage, no other switch within the window, and purchases before and after the switch. The six-month column draws from the events admitted to the twelve-month exercise; South Dakota and Vermont lack purchases on both sides within six months and are excluded. First purchases are selected before applying event windows, and households are not duplicated across events. Observations with inconsistent contract-winner assignments are excluded. No state-by-manufacturer fixed effects are added in this table. WIC brand indicates the state's contract manufacturer; WIC bulk interacts this indicator with bulk size. Authorized and unauthorized refer to retailer WIC authorization, with online purchases as the reference channel. The WIC-brand coefficient is conditional on reference hospital and manufacturer values. Price and the WIC-brand coefficient are independently normally distributed; SD denotes their standard deviations. Brand-size intercepts are included but omitted. Parentheses report state-clustered standard errors. Stars use two-sided $t(G-1)$ tests, with G equal to the number of states/DC in each column: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$. No stars are assigned to standard-deviation parameters.

probability. The derivative of choice probabilities with respect to prices is

$$\frac{\partial P_{ij}}{\partial p_{ik}} \;=\; \frac{1}{R}\sum_{r} \beta^{p}_{r}\, P_{ijr}\left(\mathbf{1}\{j=k\} - P_{ikr}\right), \tag{5}$$

where β^{p}_{r} is the price coefficient at draw r. The elasticity of product j's share with respect to product k's price aggregates over all $N = 22{,}065$ choice situations:

$$\eta_{jk} \;=\; \frac{\sum_i p_{ik}\, \partial P_{ij}/\partial p_{ik}}{\sum_i P_{ij}}. \tag{6}$$

Because demand is conditional on purchasing formula, there is no outside option, and $\sum_j \partial P_{ij}/\partial p_{ik} = 0$.

Markets, firms, and prices. A market m is a state $\times$ contract-brand cell. There are 70 such markets, and we report results for the 62 markets with at least 30 choice situations. Four firms set prices: Abbott, Mead Johnson, Nestlé, and the store brand, which we treat as a single firm. Each firm sets the prices of its regular and bulk products. Let Ω be the 8×8 ownership matrix, with $\Omega_{jk} = 1$ if products j and k belong to the same firm.

Observed prices vary across choice situations within a market, for example across retailers and months. To keep the pricing problem tractable, we summarize each product's price in market m by a price index x_{mj}, the mean observed price over the situations in which j is available. We then write the price in situation i as $p_{ij} = \rho_{ij}\, x_{mj}$, where $\rho_{ij} = p_{ij}/x_{mj}$ is held fixed. We assume that marginal costs scale in the same way, $c_{ij} = \rho_{ij}\, c_{mj}$. Under this assumption the Lerner index $(p_{ij} - c_{ij})/p_{ij} = (x_{mj} - c_{mj})/x_{mj}$ is common to all situations in a market.

First-order conditions. Let n_m be the number of choice situations in market m. Firm f's profit per situation is

$$\pi_{mf} \;=\; \sum_{j \in f} (x_{mj} - c_{mj})\, \tilde{s}_{mj}, \qquad \tilde{s}_{mj} \;=\; \frac{1}{n_m}\sum_{i \in m} \rho_{ij}\, P_{ij}, \tag{7}$$

where $\tilde{s}_{mj}$ is product j's share weighted by the relative price ρ_{ij}. Since $\partial p_{ik}/\partial x_{mk} = \rho_{ik}$,

$$\frac{\partial \tilde{s}_{mj}}{\partial x_{mk}} \;=\; \frac{1}{n_m}\sum_{i \in m} \rho_{ij}\, \rho_{ik}\, \frac{\partial P_{ij}}{\partial p_{ik}}. \tag{8}$$

The first-order conditions of the multi-product Bertrand–Nash game are

$$\tilde{s}_{mj} + \sum_{k} \Omega_{jk} \left(x_{mk} - c_{mk} \right) \frac{\partial \tilde{s}_{mk}}{\partial x_{mj}} \;=\; 0 \quad \text{for all } j. \tag{9}$$

Stacking them gives

$$x_m - c_m \;=\; \Delta_m^{-1} \, \tilde{s}_m, \qquad \Delta_{m,jk} \;=\; -\,\Omega_{jk} \, \frac{\partial \tilde{s}_{mk}}{\partial x_{mj}}. \tag{10}$$

Products not available in a market are dropped from that market's system. Evaluating these conditions at the observed price indices x_m under the full demand model recovers the implied marginal costs c_m and Lerner indices $L_{mj} = (x_{mj} - c_{mj})/x_{mj}$.

Margins without WIC effects. To isolate the role of WIC, we recompute the Lerner indices at the same observed prices under demand systems in which the WIC coefficients are set to zero. As in Figure 13, we add the WIC terms back cumulatively in four steps:

1. no WIC effect, with all WIC coefficients, including the standard deviation of the WIC-brand coefficient, set to zero;
2. the baseline WIC-brand effect;
3. the WIC retail-environment terms;
4. the hospital terms, which gives the full model.

Panel B of Appendix Table E.15 reports, for each step, the change in the Lerner index relative to the no-WIC demand system, ΔL_{mj}. We report the median over market $\times$ product cells, separately for the contract brand and for the other brands, by package size.

Implied equilibrium prices. The changes in margins are computed at observed prices. To translate them into prices, we hold the marginal costs c_m implied by the full model fixed and set all WIC coefficients to zero. We then solve for the new equilibrium price indices x_m^0 by damped fixed-point iteration on the first-order conditions:

$$x_m^{(t+1)} \;=\; (1-\lambda)\, x_m^{(t)} + \lambda \left[c_m + \Delta_m\!\left(x_m^{(t)}\right)^{-1} \tilde{s}_m\!\left(x_m^{(t)}\right) \right], \tag{11}$$

with $\lambda = 0.5$, starting from observed prices and holding the relative prices ρ_{ij} fixed. Iteration stops when the largest relative gap between $x_m^{(t)}$ and the bracketed term falls below 10^{-7} in every market.

In a few market $\times$ product cells (9 under the full model), the implied Lerner index lies outside $(0, 1)$, which implies a negative marginal cost. As a robustness check, we restrict the sample to the 55 markets in which every implied marginal cost lies between zero and price; the implied price changes are essentially unchanged. In this check the set of markets is redefined in each bootstrap draw.

Caveats. Three caveats apply to these calculations. First, because demand is conditional on buying formula, the model has no outside option. Aggregate formula demand is therefore perfectly inelastic in the model, which overstates the level of market power. For this reason we report changes in margins and prices rather than their levels. Second, the calculation treats manufacturers as setting retail prices directly. It abstracts from retailers' pricing, from wholesale and rebate contracts, and from dynamic considerations. These are exactly the features that may explain why the predicted price increase does not appear in the data. Third, prices and costs within a market are assumed to scale with a common index, so the calculation does not capture differences in margins across retailers.

Table E.15: Price elasticities and implied Bertrand pricing incentives

Panel A. Own- and cross-price elasticities (row: share of; column: price of)

	Abb. R	MJ R	Nes. R	Store R	Abb. B	MJ B	Nes. B	Store B
Abbott, regular	−1.933	0.318	0.047	0.037	0.402	0.517	0.049	0.322
	(0.025)	(0.007)	(0.002)	(0.002)	(0.008)	(0.009)	(0.002)	(0.005)
Mead Johnson, regular	0.307	−2.045	0.047	0.041	0.349	0.563	0.049	0.308
	(0.007)	(0.025)	(0.002)	(0.002)	(0.007)	(0.010)	(0.002)	(0.005)
Nestlé, regular	0.333	0.343	−2.131	0.040	0.360	0.509	0.123	0.296
	(0.008)	(0.009)	(0.028)	(0.002)	(0.008)	(0.011)	(0.008)	(0.006)
Store brand, regular	0.325	0.381	0.054	−1.989	0.336	0.487	0.048	0.294
	(0.006)	(0.007)	(0.002)	(0.007)	(0.006)	(0.008)	(0.002)	(0.005)
Abbott, bulk	0.427	0.384	0.056	0.040	−1.551	0.541	0.055	0.329
	(0.008)	(0.008)	(0.002)	(0.002)	(0.020)	(0.010)	(0.002)	(0.006)
Mead Johnson, bulk	0.374	0.418	0.053	0.039	0.367	−1.814	0.052	0.313
	(0.007)	(0.008)	(0.002)	(0.002)	(0.007)	(0.022)	(0.002)	(0.005)
Nestlé, bulk	0.371	0.386	0.128	0.039	0.380	0.537	−1.938	0.311
	(0.008)	(0.009)	(0.008)	(0.002)	(0.008)	(0.010)	(0.025)	(0.006)
Store brand, bulk	0.393	0.389	0.051	0.040	0.379	0.531	0.049	−1.364
	(0.007)	(0.008)	(0.002)	(0.002)	(0.008)	(0.009)	(0.002)	(0.017)

Panel B. Implied Bertrand pricing incentives relative to demand without WIC effects

	Contract brand		Other brands	
	Regular	Bulk	Regular	Bulk
Δ Lerner index $(p-c)/p$ at observed prices:				
Baseline WIC	0.012	0.013	−0.004	−0.005
	[−0.014, 0.041]	[−0.018, 0.050]	[−0.012, 0.001]	[−0.016, 0.001]
+ WIC retail	0.091	0.104	−0.019	−0.027
	[0.072, 0.108]	[0.081, 0.122]	[−0.021, −0.015]	[−0.033, −0.022]
+ Hospital (full)	0.102	0.116	−0.021	−0.031
	[0.089, 0.116]	[0.102, 0.131]	[−0.023, −0.018]	[−0.035, −0.026]
Implied price, full vs. no-WIC equilibrium (%):				
Markets with $n \geq 30$ (62)	6.25	6.97	−1.33	−1.66
	[5.50, 7.49]	[5.90, 8.39]	[−1.55, −1.07]	[−1.90, −1.28]
Markets with valid costs (55)	6.22	6.90	−1.33	−1.66
	[5.36, 7.42]	[5.86, 8.29]	[−1.62, −1.08]	[−1.93, −1.29]

Notes: All quantities are computed from the main demand model (Table 9, column 2; mixed logit with random coefficients on price and the WIC brand), $N = 22{,}065$ choice situations, 300 simulation draws with common random numbers. Demand is conditional on purchasing formula (no outside option). Panel A: cell (j, k) is the percentage change in product j's share when product k's price rises by 1%, pooled over choice situations; standard errors in parentheses. Panel B: markets are state × contract-brand cells; manufacturers set prices for their regular and bulk products (store brands treated as one firm). Marginal costs are backed out from multi-product Bertrand first-order conditions at observed prices; within a market, prices and costs in each choice situation are proportional to a market price index (the mean observed price), so the Lerner index is common within a market. Δ Lerner is the change in the implied margin relative to demand with all WIC coefficients set to zero. The implied price change holds full-model marginal costs fixed, sets the WIC coefficients to zero, and re-solves for equilibrium prices; it reports (observed − no-WIC)/no-WIC in percent. Panel B entries are medians over market × product cells in the 62 markets with at least 30 choice situations; "valid costs" restricts to the 55 markets in which every implied marginal cost lies between zero and price (the set is redefined in each bootstrap draw). Standard errors and 95% intervals (in brackets) are from 500 parametric bootstrap draws of the demand parameters.

F Online Product Displays Across States

Our comparison between online and in-store purchases assumes that online retailers do not systematically promote the WIC contract brand based on the customer's location. This assumption would be questionable if online retailers used the customer's state to place the locally designated WIC brand in a more prominent position. In that case, online shoppers could also be exposed to a state-specific retail environment, making the online channel a less useful comparison for the physical retail channel.

To examine this possibility, we conducted a descriptive audit of infant formula search results at Walmart, Target, and Amazon. Before each search, we cleared the browser history, cookies, cached files, autofill information, and site permissions, and then opened a private browsing session. We used a virtual private network to access each retailer's website from different states and searched for "infant formula." The states differed in the identity of the WIC contract manufacturer at the time of the audit.

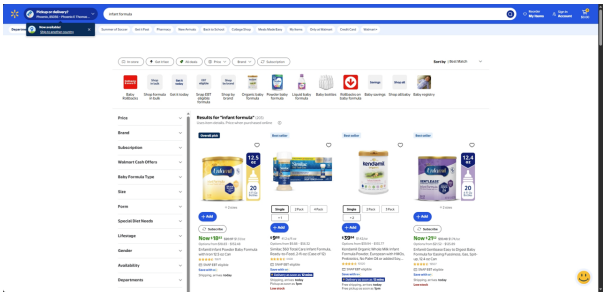
Table F.16: Most prominent infant formula displayed in online search results

State	Retailer	WIC Contract Brand	Top Displayed Brand	Match
Arizona	Walmart	Similac	Enfamil	No
Florida	Walmart	Similac	Enfamil	No
New Mexico	Walmart	Similac	Enfamil	No
Virginia	Walmart	Similac	Enfamil	No
Washington	Walmart	Similac	Enfamil	No
California	Walmart	Enfamil	Enfamil	Yes
Colorado	Walmart	Enfamil	Enfamil	Yes
Georgia	Walmart	Enfamil	Enfamil	Yes
New York	Walmart	Enfamil	Enfamil	Yes
Texas	Walmart	Enfamil	Enfamil	Yes
Virginia	Target	Similac	Bobbie	No
California	Target	Enfamil	Bobbie	No
New York	Target	Enfamil	Bobbie	No
Texas	Target	Enfamil	Bobbie	No
Virginia	Amazon	Similac	Similac	Yes
California	Amazon	Enfamil	Similac	No
New York	Amazon	Enfamil	Similac	No
Texas	Amazon	Enfamil	Similac	Yes

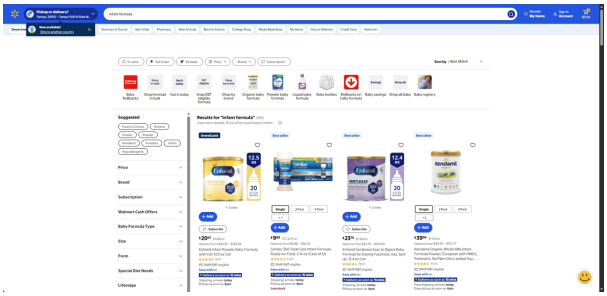
Notes: Searches were conducted on July 17, 2026 after clearing browser history, cookies, cache, autofill information, and other stored browser data. Each search was performed in a private browsing session using a VPN to access the retailer from different states. The table reports the manufacturer of the first prominently displayed infant formula product returned by the search query "infant formula." Overall, the identity of the most prominent product appears largely invariant to the customer's location and does not systematically follow the state's WIC contract manufacturer.

Table F.16 summarizes the results. For each state and retailer, the table reports the state's WIC contract brand and the brand occupying the most prominent position in the search results. The most prominent displayed brand does not systematically correspond to the state's WIC contract brand. Instead, the identity of the top-displayed product tends to remain stable within a retailer across states with different WIC contract manufacturers.

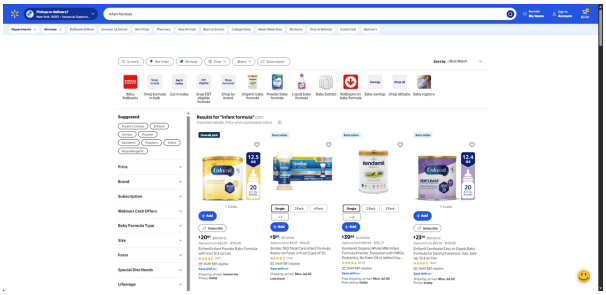
Figures F.6, F.7, F.8 provide representative examples of the search results underlying Table F.16. The figure compares Walmart search results from Virginia, where Similac was the WIC contract brand, and California, where Enfamil was the WIC contract brand. Despite the difference in WIC contract assignments, the products shown in the most prominent positions are nearly identical across the two locations. In particular, Walmart does not appear to move the state-specific WIC contract brand to the top of the search results.



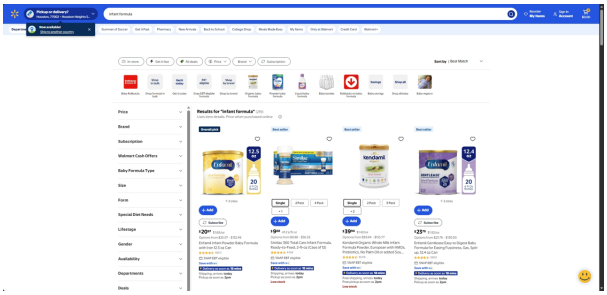
(a) Arizona, WIC = Similac



(b) Florida, WIC = Similac

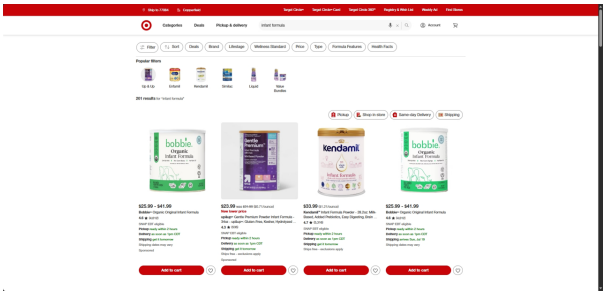


(c) New York, WIC = Enfamil

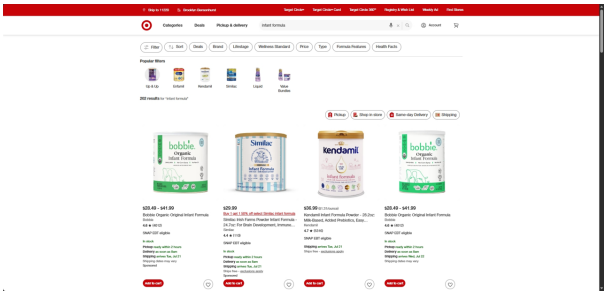


(d) Texas, WIC = Enfamil

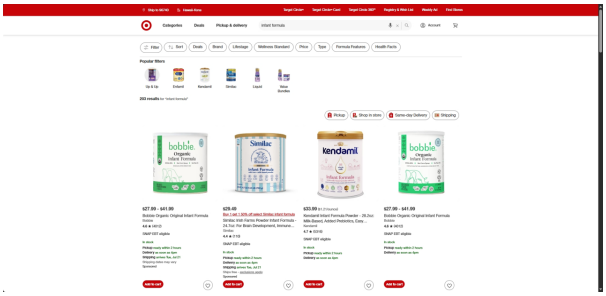
Figure F.6: Representative online search results across states - Walmart



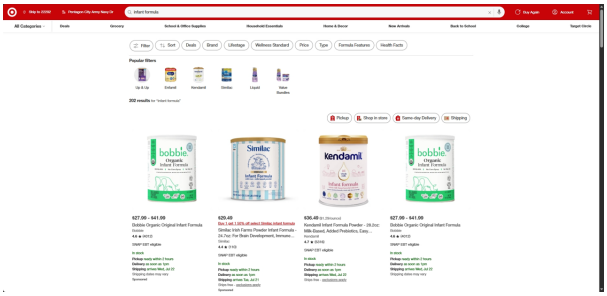
(a) California, WIC = Enfamil



(b) New York, WIC = Enfamil

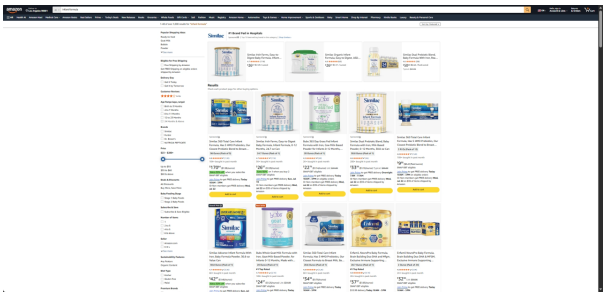


(c) Texas, WIC = Enfamil

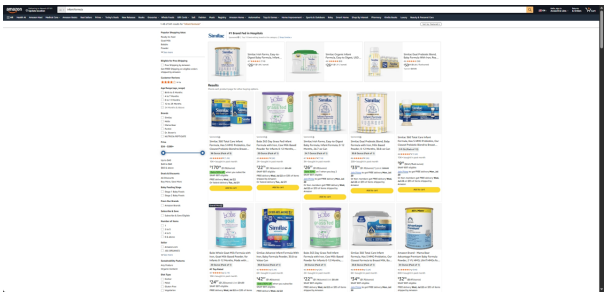


(d) Virginia, WIC = Similac

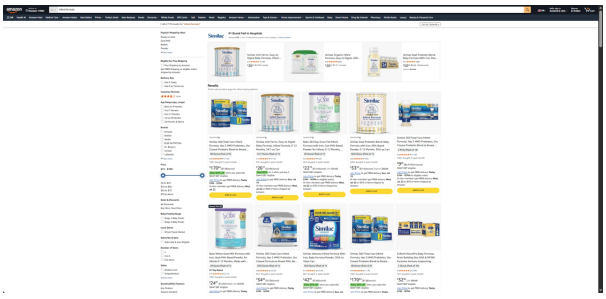
Figure F.7: Representative online search results across states - Target



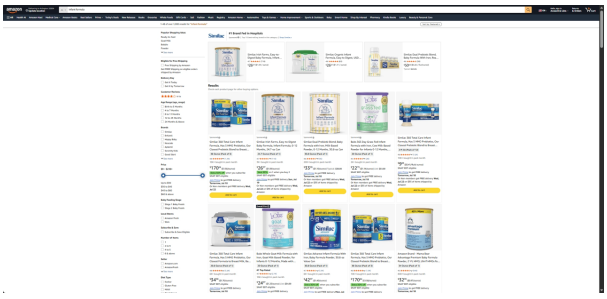
(a) California, WIC = Enfamil



(b) New York, WIC = Enfamil



(c) Texas, WIC = Enfamil



(d) Virginia, WIC = Similac

Figure F.8: Representative online search results across states - Amazon

G Price Construction and Choice Sets

The Numerator data report the transaction price of the product purchased by each household, but they do not report the prices of products that the household did not purchase. To construct prices for these non-chosen alternatives and define the set of products available to each household, we supplement the Numerator data with transaction data from Nielsen whenever available.

For the 2016–2023 period, we combine transactions from Numerator and Nielsen and restrict the sample to products with a price per ounce greater than \$0.30. Beginning in 2024, Nielsen transaction data are not available to us, so we construct prices and choice sets using Numerator transactions alone. We otherwise apply the same procedure in both periods.

We define a product as a manufacturer $\times$ size-category combination. Because the relevant geographic market differs between physical and online shopping, we define markets separately by purchase channel. For in-store purchases, a market is a year $\times$ month $\times$ ZIP-3 combination, where ZIP-3 denotes the first three digits of the ZIP code and captures a relatively local geographic area. For online purchases, for which the relevant geographic market is broader, we define a market as a year $\times$ month $\times$ state combination.

For the product actually purchased by a household, we use the household's observed transaction price per ounce. For each non-chosen alternative, we impute its price using the average transaction price per ounce of that product among all observed transactions in the same market. Thus, for an in-store choice, the price of a non-chosen product is its average price per ounce in the same year, month, and ZIP-3. For an online choice, it is the corresponding average in the same year, month, and state. For 2016–2023, these market-level averages are calculated using the combined Numerator and Nielsen transactions; from 2024 onward, they are calculated using Numerator transactions only.

We also use observed transactions to construct market-specific choice sets. A product is included in the choice set for a given market only if we observe at least one transaction for that product in that market. If a product is not observed in a particular year-month-ZIP-3 market for in-store purchases, or year-month-state market for online purchases, it is excluded from the choice set of every household making a purchase in that market. This procedure therefore defines the empirical choice set as the set of products observed to transact in the corresponding market.