Restricting Large Firms to Protect Small Businesses: Evidence from Retail Operating-Day Restrictions

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ABSTRACT

Governments often try to protect small businesses by restricting larger rivals rather than subsidizing them directly. Whether such policies achieve their objectives depends on how much displaced demand is actually transferred to the intended beneficiaries, a question that requires tracing substitution toward other days, locations, or unregulated sellers, yet direct evidence on these margins remains limited. We study South Korea's policy requiring large retail chains to close two days per month, which provides predetermined variation in closure schedules across districts and weeks. Using high-frequency card-transaction data, we estimate that only 18 percent of displaced spending is redirected to small, independent retailers; larger shares are recouped by restricted chains on other shopping days or diverted to other unrestricted retail formats, many of them operated by large firms. Consistent with these limited transfers, we find no evidence that the policy improves the long-run survival of independent retailers. Mapping the observed substitution patterns into a discrete-choice framework identified by policy-induced variation in travel time to open stores, we estimate that the consumer welfare losses from restricted access are 2.7 times larger than the gains accruing to independent retailers. These findings suggest that restricting large firms can impose meaningful consumer costs while providing little durable support to the small businesses such policies are intended to protect.

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I. Introduction

Governments often try to protect small businesses not by subsidizing them directly, but by restricting larger rivals. In retail and other markets, regulators restrict when, where, or how large firms may operate. The common premise is that constraining large firms will redirect demand toward smaller competitors. Whether this premise holds, however, depends on how consumers substitute when access to preferred options is restricted. Some displaced spending may be redirected to the small businesses policymakers aim to protect, but it may also shift to other times, locations, or alternative sellers outside the regulation's scope. The effectiveness of protective regulations therefore hinges on substitution along margins that policymakers do not directly control, and these same margins determine welfare consequences for both producers and consumers. Despite the prevalence of such policies, direct evidence on the full set of substitution responses and their welfare implications remains limited.

Retail markets offer a particularly well-suited setting to study these forces. The expansion of large chains has brought consumer benefits while also raising concerns about the viability of independent stores, prompting governments to respond with entry regulations, zoning rules, and restrictions on operating days (Atkin, Faber and Gonzalez-Navarro, 2018; Bossler and Oberfichtner, 2017; Bronnenberg and Ellickson, 2015; Jia, 2008; Sadun, 2015). In this paper, we study the trade-off between protection and consumer cost in South Korea, where policymakers sought to protect small independent retailers by requiring large retail chains to close on two designated days per month. Since 2012, regulated chains have typically closed on the second and fourth Sunday, although some districts instead designate the second and fourth Wednesday.

Our empirical strategy exploits the fact that these closure calendars are predetermined and differ across districts. We compare spending on the same day of the week in closure weeks versus otherwise similar non-closure weeks within a district, which allows us to measure how consumers reallocate expenditure across days and retailer types when large chains are closed. Cross-district differences in Sunday-versus-Wednesday designations additionally allow us to measure spatial substitution. A key identifying assumption is that mandated closures affect spending only within the week in which they occur. We provide direct support for this assumption using months with five weeks, which mechanically generate consecutive non-closure weeks; we find no significant differences in total expenditure between non-closure weeks that follow a closure week and those that follow another non-closure week. We further show that retail prices remain stable on and around closure days using barcode-level price indices, confirming that expenditure changes reflect shifts in consumer behavior rather than price responses. We implement this design using high-frequency card-transaction data from a large payment processor covering the Seoul Metropolitan Area in 2019. The data record daily expenditures and transaction counts by store location, retail format, and consumers' place of residence, allowing us to trace substitution across time, seller type, and geography.

We find that the mandatory closures redirect only a limited share of spending toward the policy's intended beneficiaries. On closure days, only 18 percent of sales displaced from restricted chains are diverted to independent retailers. By comparison, 20 percent is captured by other unrestricted retail formats, such as chain convenience stores and department stores, and 25 percent is recouped by the same chains on other shopping days. Spatial substitution to open branches in districts with different closure schedules is limited on average, though it is concentrated where an open outlet remains nearby. Overall, more spending is reallocated to unintended outlets and back to restricted chains than to the small retailers the policy was designed to help.

To assess how the policy redistributes profits across retailer types, we map sales changes into profits using operating margins from the 2019 Census of the Wholesale and Retail Industry. Independent retailers capture about a third of the gross Sunday profit losses experienced by restricted chains, but this translates into an average monthly gain of only USD 41.7 per independent retailer. Moreover, these modest transfers do not appear to provide durable protection. Using panel data on establishment counts from the Korean Establishment Census, we find no evidence that the policy increases the number or improves the long-run survival of independent retailers, whose numbers continue to decline over time.

We then assess the consumer side of the trade-off. Policymakers and courts have framed operating-day restrictions as pro-consumer, arguing that they preserve retail diversity and restrain market power (Constitutional Court of Korea, 2018; National Assembly of the Republic of Korea, 2013; Supreme Court of Korea, 2015), but this rationale rests on statutory intent rather than direct evidence of consumer welfare effects. We provide such evidence by exploiting district-specific designations of Sunday or Wednesday closure days, which generate plausibly exogenous variation in travel time to the nearest open large-chain store. Embedding the observed substitution patterns into a discrete-choice model in which travel time enters as a generalized price, we estimate a compensating variation of approximately USD 0.78-0.88 per affected transaction, implying that the consumer welfare losses from restricted access are about 2.7 times as large as the profit gains to independent retailers. Taken together with the absence of large transfers or long-run survival effects, these findings suggest that restricting large firms can impose meaningful consumer costs while providing little durable support to the intended beneficiaries.

This paper contributes to the literature on the effects of protective regulation in retail markets (Haltiwanger, Jarmin and Krizan, 2010; Hausman and Leibtag, 2007; Jia, 2008; Sadun, 2015). Existing work focuses primarily on entry and zoning regulations, which operate on the extensive margin by determining whether and where large retailers may locate. These policies have yielded mixed results, in some cases reducing overall retail provision without improving small-retailer viability (Maican and Orth, 2018; Schivardi and Viviano, 2011). We study a distinct yet common policy instrument—restrictions on when existing retailers can operate—that has received far less attention. Unlike entry regulations, which permanently alter market structure and confound consumer responses with supply-side adjustments, operating-day restrictions periodically remove access to existing retailers while holding market structure fixed. This creates a recurring counterfactual that allows us to directly observe the complete set of consumer substitution margins across retailers, time, and space that jointly determine the welfare consequences of protective regulation. We provide a comprehensive accounting of these margins and show that the majority of displaced spending is reallocated to channels the policy was not designed to support.

We also contribute to the literature on store-hour regulation, which has largely examined the effects of deregulating shopping hours on employment, prices, and profits (Bossler and Oberfichtner, 2017; Paul, 2015; Reddy, 2012; Tanguay, Vallge and Lanoie, 1995; Wenzel, 2011). Since deregulation episodes typically apply uniformly across retailer types, they cannot identify how consumers reallocate spending across competing formats. We analyze a policy that moves in the opposite direction and is selectively targeted: mandated closures that apply only to large chains while leaving other formats to operate. This asymmetric design identifies cross-format substitution patterns, which we show are central to evaluating the policy's distributional objectives.

Lastly, we contribute to a growing literature that uses second-choice or diversion information to discipline demand estimation. Berry, Levinsohn and Pakes (2004) incorporate survey-based second-choice responses as micro moments in a random-coefficients logit model; Leard (2019) use second-choice data to study substitution between new and used vehicles; and Conlon, Mortimer and Sarkis (2023) show how first- and second-choice probabilities can nonparametrically identify preferences and diversion ratios. In a similar spirit, we exploit policy-induced diversion—measured from card-transaction data around restricted days—as an aggregate analogue of second-choice information. Unlike survey-based second choices, which may be subject to hypothetical bias and often capture a single substitution margin, the operating-day restriction generates revealed-preference reallocation across the full set of margins—across retailer types, days, and districts—simultaneously. Embedding these policy-induced diversion moments in a discrete-choice framework identified by exogenous variation in travel time to open stores, we translate the observed substitution into transparent, money-metric measures of consumer welfare. This approach may be applicable to a range of settings in which regulations or shocks periodically restrict access to a subset of options, generating revealed-preference variation in consumer choices.

The remainder of the paper is organized as follows. Section II describes the evolution of Korea's grocery retail sector and the institutional details of the mandatory shutdown policy. Section III introduces the data sources we use to study consumer behavior, retailer profits, and the prevalence of independent retailers. Section IV outlines our empirical strategy and presents the main reduced-form results on substitution patterns, heterogeneity by market structure, profit impacts, and the long-run evolution of independent retailers. Section V develops a discrete-choice framework that maps policy-induced diversion into money-metric measures of consumer welfare. Section VI combines producer and consumer margins to assess the overall welfare consequences of the policy, considers whether distributional objectives justify the efficiency costs, and discusses the external validity of our findings. Section VII concludes.

II. Korea's Retail Industry and the Mandatory Shutdown Policy

The grocery retail industry in Korea went through a significant transformation beginning in the 1990s with the emergence of large retail chains due to retail globalization. This challenged independent retailers and traditional markets (similar to farmers' markets in the United States) that previously dominated Korea's grocery retail industry. Large retail chains, offering lower prices, wider product variety, and superior customer service through extensive organizational structures, rapidly became dominant. This shift is evident in the changing number of stores by retailer type. Between 1995 and 2011, a year before the policy was implemented, the number of independent retailers nationwide was cut in half, declining from 151,694 to 79,879, while large retail chains more than doubled, growing from 666 to 1,609 (Establishment Census), where "large retail chains" here refers broadly to chain-affiliated modern grocery retailers, including both large-format and smaller conglomerate-owned outlets that will later be distinguished by regulatory size thresholds.

In April 2012, the Korean government implemented a mandatory biweekly shutdown for large retail chains under the *Distribution Industry Development Act*. The policy was intended to support small, independent retailers and to curb the growing dominance of large retail chains. Policymakers were concerned that continued expansion of large retail chains would crowd out independent retailers, reduce retail variety, and ultimately increase grocery prices. Both the potential loss of retail diversity and the possibly higher consumer prices were seen as threats to consumer welfare, motivating government intervention.

In legislative debates and subsequent court challenges, the shutdown policy was repeatedly justified as a pro-consumer measure. Lawmakers and courts argued that limiting the operating days of large chains would preserve a diverse retail ecosystem, restrain market power, and prevent grocery prices from rising to monopoly levels (Constitutional Court of Korea, 2018; National Assembly of the Republic of Korea, 2013; Supreme Court of Korea, 2015). However, this rationale is grounded largely in statutory purpose clauses and judicial reasoning rather than in empirical evidence of actual consumer welfare gains.

The policy also reflected political pressure from small-business advocacy groups, including the Korea Federation of Small and Medium-Sized Enterprises and the Network for Revitalizing Small Merchants, which attributed declining sales and market share to large retail chains. Consistent with this emphasis, the Act's purpose emphasizes the need for "balanced development of the distribution industry" (Article 1) as a means of protecting small and independent retailers.

Public opinion initially aligned with the government's justification: according to a newspaper-reported survey in 2013, approximately 46% of consumers thought the biweekly closure was appropriate, and 40% believed it should be further strengthened.[1] However, the welfare cost associated with losing access to large corporate retailers on closure days soon became more salient. In more recent surveys, about three-quarters of respondents reported preferring to relax or eliminate the closure requirement in favor of greater shopping convenience.[2]

The policy mandates large retail chains to close twice a month, with the shutdown days varying by district. The policy applies to any retailer with a store format exceeding 3,000 square meters (hereafter referred to as "large restricted retailers") and conglomerate-owned stores between 165 and 3,000 square meters (hereafter referred to as "small restricted retailers").

Although independent retailers are the intended beneficiaries of the policy, other store formats, such as chain convenience stores, department stores, agricultural cooperative grocers (e.g., Nonghyup Hanaro Mart), and general merchandise stores are not subject to the periodic closure restrictions.[3]

The determination of shutdown days occurs at the district level, with the default closure days for restricted retailers set as the second and fourth Sundays. However, local district governments have the authority to modify these days through ordinances. Consequently, some districts designate the second and fourth Wednesdays as closure days, while others adopt a mixed policy, such as closing on the second Wednesday and the fourth Sunday.

To document these schedules, we hand-collected information on closure days for each district by verifying whether a local government ordinance had been enacted to implement modifications. For districts without publicly available online information, we supplemented our data using news reports (BNT News, 2023). In 2020, we also hand-collected the exact locations (longitude and latitude) of large and small restricted retailers, along with their closure days, using APIs for Naver Map and Mart Monster. Because store entry and exit may have changed since

[1] "86% of respondents believe the mandatory closure for large retail chains should be maintained or strengthened," Hankyoreh, April 21, 2013, https://www.hani.co.kr/arti/economy/economy_general/583804.html.

[2] "Allow large retailers to open on Sundays – 76% of consumers support easing the restrictions," Maeil Business Newspaper, January 21, 2024, https://www.mk.co.kr/news/economy/10925723.

[3] Stores generating more than 55% of their sales from agricultural products can request an exemption, subject to local government evaluation. However, only 14 restricted retailers in the Seoul Metropolitan Area have had the policy waived. Agricultural cooperative grocers, operated by the Nonghyup Agribusiness Group, a large corporation in Korea, were exempted from these regulations because over 51% of their sales come from agricultural and fishery products. Furthermore, smaller stores run by local agricultural cooperatives were never subject to the regulation due to their size.

2019, we complemented our data with the Status of Large-Scale Stores dataset from the Korean Ministry of the Interior and Safety, which provides information on the licensing and approval of large stores. These store-level data also allowed us to verify district-level closure schedules.

Figure 1 illustrates the variation in closure schedules across districts. This variation allows consumers to travel to other districts to access open large retail chains when closures occur in their own district. The cost of traveling to another district with a different closure day varies depending on the distance.

By the end of 2019, Korea had 406 large restricted retailer store units and 1,215 small restricted retailer store units subject to the policy. Figure A1 shows the geographic distribution of these stores across districts in the Seoul Metropolitan Area, with an average of three large and eleven small restricted retailers per district.

An important aspect of the policy is that it cannot be circumvented via restricted retailers' online sales because online portals are forbidden from delivering groceries on the days when physical stores are closed. Consumers can still place orders and schedule deliveries for other days.

III. Data

A. *Consumer Transaction Data*

Our primary data source is a proprietary dataset of bank card expenditures obtained from the largest credit and debit card payments processor in Korea, which held a 24.2 percent market share in 2019 (Credit Finance Association of Korea). The data report expenditure amounts and transaction counts by date, retailer category, store location, and consumers' residential location. The sample covers 2019 and spans the Seoul Metropolitan Area—nearly half of the national population—comprising 971 neighborhoods (77 districts) across Seoul, Incheon, and Gyeonggi-do.

Retailers are classified into eight categories: large restricted retailers; small restricted retailers; independent retailers; chain convenience stores (e.g., Seven Eleven); department stores; agricultural cooperative grocers (e.g., Nonghyup); exempted large retail chains; and general merchandise stores. Retailer classification and store location are based on the merchant name and merchant category code recorded by the card payment processor. Cardholder residence location is obtained from the address reported at the time of card registration.

In our main specification, we aggregate transactions to the daily level by district and retailer type. We use districts as the unit of analysis because they contain, on average, three large retailers, making them a natural catchment area for consumer purchasing decisions.

To capture shopping behavior around Sundays, we define weeks as running from Thursday to Wednesday. We focus on transactions occurring in weeks that include the second (restricted) and third (unrestricted) Sundays of each month. Other weeks are excluded to limit confounding influences from beginning- or end-of-month effects, such as monthly pay cycles or end-of-month promotions. Under the closure policy, weeks containing the second Sunday constitute restricted weeks, while weeks containing the third Sunday serve as unrestricted weeks, as illustrated in Figure 2. We exclude months in which major national holidays fall within the second or third week, as retail operating schedules and consumer spending patterns are atypical during periods such as Lunar New Year and Chuseok. Finally, we code zero expenditure at restricted retailers on mandated closure days, which would otherwise appear as missing observations.

Although comprehensive, the dataset has several limitations. First, it captures only credit and debit card transactions and therefore excludes payments made in cash, gift certificates, vouchers, or other methods. According to the Bank of Korea, in 2019, credit cards accounted for 53.8 percent of total expenditures, debit cards for 15.3 percent, cash for 17.4 percent, bank transfers for 8.0 percent, and mobile payments for 3.8 percent; thus, bank card transactions represent nearly 70 percent of total spending. Moreover, because virtually all retailers in Korea accept bank cards, and payment method choice is largely determined by consumer type rather than by retailer type, our estimates by retailer category are unlikely to be systematically biased.

A second caveat is that the data originate from a single payment processor rather than from the universe of processors, and may therefore not be fully representative of the population. If cardholder characteristics differ systematically across processors, this could bias our estimates. This concern is mitigated by the fact that the data come from a payment processor—analogous to Visa or Mastercard—that intermediates transactions across multiple issuing banks rather than from a single bank with a potentially more selected customer base.

Table 1 reports summary statistics for district-by-day transaction data, measured in millions of Korean won (KRW), for the Sunday-closure districts that constitute our main analysis sample.[4] The first two columns present the mean and standard deviation for restricted weeks, the next two columns report the corresponding moments for unrestricted weeks, and the final column shows mean differences along with statistical significance from two-sample t-tests. Average daily district-level card expenditure is highest among other unrestricted retailers, followed by independent retailers, and lowest among restricted retailers, reflecting differences in retailer density: on average, a district contains approximately 13 restricted retailers, 174 independent retailers, and 369 other unrestricted retailers. During restricted weeks, average daily expenditure at restricted retailers is KRW 70 million, compared with KRW 82 million during unrestricted weeks, confirming that the closure policy substantially reduces sales at restricted retailers. For both independent retailers and other unrestricted retailers, average daily expenditure is slightly higher during restricted weeks than unrestricted weeks (KRW 107 million versus 104 million for independent retailers, and KRW 177 million versus 172 million for other unrestricted retailers). These patterns provide descriptive evidence of substitution across retailer types during restricted weeks, visible directly in the raw data.

Figure 3 plots unconditional means and standard deviations of differences in card expenditure between restricted and unrestricted weeks by day of the week. The figure illustrates both temporal and cross-retailer substitution patterns. Across all panels, expenditures at the beginning of the week (Thursday) are nearly identical in restricted and unrestricted weeks, supporting the comparability of the two week types. Panel A shows a sharp decline in expenditure at restricted retailers on restricted Sundays, consistent with mandatory store closures. The figure also reveals temporal substitution, with higher expenditures during restricted weeks on Mondays and small increases on Saturdays, Tuesdays, and Wednesdays. Panel B shows preliminary evidence of across-retailer substitution: during restricted weeks, Sunday expenditures rise at unrestricted retailers. Panel C shows that, although expenditure levels are lower, residents of Sunday-closure districts increase spending at restricted retailers located in Wednesday-closure districts on restricted Sundays.

[4]The 2019 purchasing power parity (PPP) exchange rate for household consumption expenditure from the International Comparison Program (ICP) is KRW 971 per USD.

B. Other Datasets

Profit Margins- For the purpose of projecting changes in sales onto profits, we calculate profit margins by retailer type using data from the 2019 Census of the Wholesale and Retail Industry conducted by Statistics Korea (KOSTAT). This annual survey gathers information on approximately 200,000 establishments nationwide in industries such as wholesale and retail trade, accommodation, and food services, representing 10 percent of all establishments in these sectors. Key variables include revenue, operating costs, province-level location, organizational structure, operating months, and the number of scheduled closure days per month (e.g., none, 1, 2-3, 4-6, 6-7, or 8+ days). We restrict the sample to establishments operating continuously throughout the year within the Seoul Metropolitan Area.

Profit margin is defined as the percentage of operating profit relative to total revenue. Total revenue encompasses income from business activities, including the sales of goods or services, while operating costs include selling, administrative, and other expenses itemized in cost statements.[5] Operating profit is calculated by subtracting operating costs from revenue.[6]

With these data, we compute operating profit margins for each establishment by retailer type. Median margins are 2.4 percent for large restricted retailers, 2.6 percent for small restricted retailers, 4.6 percent for independent retailers, 1.1 percent for chain convenience stores, and 10.7 percent for department stores. These magnitudes are comparable to profit margins reported in other countries—including the United States, European economies, and Japan—based on data from Bloomberg, Morningstar, Capital IQ, and Compustat (Damodaran, 2019).[7]

For the profit analysis, we construct representative margins for broader retailer categories by pooling establishment-level margins across the relevant types and taking the median.

[5]Operating costs include the cost of goods sold; labor costs (wages, retirement contributions, and employee benefits); rent; taxes and duties (excluding value-added, corporate, and income taxes); depreciation; bad-debt expenses; research and development expenditures; and other operating expenses, such as supplies and communication costs.

[6]The Census does not provide detailed industry codes to distinguish retailer types, except for department stores and chain convenience stores. To address this limitation, we classify retailers using additional variables such as store area, business organization type, type of business, and the number of scheduled closure days per month. Based on this information, we define large restricted retailers as incorporated businesses with store areas exceeding 3,000 square meters and 2-3 scheduled closure days per month. Similarly, we identify small restricted retailers using the same criteria but with store areas between 165 and 3,000 square meters. Independent retailers are individually operated businesses with store areas smaller than 3,000 square meters.

[7]For comparison, the 2019 pre-tax lease- and R&D-adjusted operating profit margin for the retail sector in the United States is 3.9 percent overall and 1.9 percent for grocery and food retailers. In Europe, these margins are 3.1 percent and 2.6 percent, respectively, while in Japan, they are 3.5 percent and 4.4 percent.

Specifically, the profit margin for restricted retailers is defined as the median across all establishments classified as large or small restricted retailers, yielding 2.4 percent. Similarly, the margin for other unrestricted retailers (the unintended beneficiaries) is defined as the median across all establishments classified as chain convenience stores and department stores, yielding a representative margin of 3.2 percent.

Price Data-To interpret expenditure-based estimates as changes in consumer behavior, we assess below whether prices remain stable around the closure days of large retail chains. For this purpose, we use the 2019 Agri-Food Consumer Panel Survey administered by Korea's Rural Development Administration, which records all daily retail purchases of approximately 1,000 households. The data include receipt-level information on products, prices, quantities, store names, retailer types, and transportation modes, with items identified by detailed product descriptors analogous to barcodes.

We harmonize retailer classifications with our card expenditure data using store names and infer store districts via map APIs, as district identifiers are not directly reported. Household residence districts are proxied by the modal district of stores accessed on foot. This allows us to analyze barcode-level prices across retailer types and districts.

We impose the same sample restrictions as in the card expenditure analysis: we focus on purchases in the second and third weeks of each month, restrict the sample to households residing in Sunday-closure districts, and exclude months with national holidays.

Independent Retailer Establishment Data- We use the Establishment Census from Statistics Korea (KOSTAT) to study the long-run effects of the mandatory shutdown policy on the prevalence of independent grocers. This annual census, available since 1994, provides comprehensive coverage of establishments nationwide and reports information on location, industry classification, and organizational structure.

Using these data, we construct an annual district-level panel of establishment counts by retailer type. Our analysis covers the period 1995–2019.[8] Figure 4 plots the log of the unconditional

[8]We exclude 1994 due to major changes in administrative boundaries that prevent consistent district-level tracking, and we omit 2020 and 2021 because of disruptions associated with the COVID-19 pandemic. We further exclude three districts that experienced substantial boundary changes during the sample period. Retailer types are defined using

mean number of establishments per district by retailer type. It is clear that the number of independent retailers declines steadily over time, whereas the number of chain convenience stores and both large and small restricted retailers increases. Importantly, there is no visible break or change in trend for independent retailers following the introduction of the mandatory shutdown policy in 2012.

IV. Empirical Strategy and Results

A. *Effects on Consumer Behavior*

We exploit the biweekly closure schedule of restricted retailers by comparing daily card expenditures of consumers residing in Sunday-closure districts across restricted and unrestricted weeks, with expenditures aggregated by district of residence. Under the assumptions tested below, this variation allows us to identify the causal effect of mandatory shutdowns of large retail chains on consumer behavior. We study four adjustment margins—across-retailer substitution to both intended and unintended beneficiaries, temporal substitution, and across-district substitution—and estimate the following difference-in-differences model:

$$Y_{jt} = \sum_{k=Fri}^{Wed} \alpha_k (RWeek(t) \times I_{d(t)=k}) + \sum_{k=Fri}^{Wed} \beta_k I_{d(t)=k} + \gamma RWeek(t) + \delta_j + \xi_{m(t)} + \varepsilon_{jt} \tag{1}$$

The dependent variable, Y_{jt}, represents the log total expenditure by residents of district j on date t, defined as $log(expenditure_{jt} + 1)$ to accommodate zero values on closure days. $d(t)$ indicates the day of the week (i.e., Monday), $m(t)$ the calendar month (i.e., April); $I_{d(t)=k}$ is an indicator equal to one if day of the week equals k, and $RWeek(t)$ equals one for the second week of the month (the closure week). The main regressor is the interaction between $RWeek(t)$ and $I_{d(t)=k}$, with Thursday as the excluded category. District fixed effects, δ_j, control for time-invariant district characteristics, and month fixed effects $\xi_{m(t)}$ absorb any common seasonal variation. Standard errors are clustered at the district-week level to allow for serial correlations within

industry codes and organizational forms. Restricted retailers are establishments classified under the five-digit industry code for large supermarkets and organized as corporate entities. Independent retailers are defined as food and beverage retailers operating as sole proprietorships. Department stores and chain convenience stores are identified based on industry codes alone.

district-week cells. All regressions are weighted by district population to ensure population representativeness.

When total expenditure at restricted retailers is the dependent variable, the coefficient α_{Sun} captures the direct treatment effect of the policy, measuring the reduction in expenditure at restricted retailers on closure days. The coefficients α_k for $k \neq Sun$ capture within–restricted-retailer spillovers across days of the week, reflecting temporal substitution of shopping activity to non-closure days.

When total expenditure at unrestricted retailers is used as the dependent variable, α_{Sun} measures across-retailer substitution, capturing the extent to which consumers reallocate spending from restricted to unrestricted retailers on closure days.

Finally, when we study across-district substitution, j continues to index districts of residence, and the dependent variable is constructed from those residents' total expenditures at restricted retailers in districts with Wednesday closures. The indicator $RWeek(t)$ remains defined with respect to the Sunday-closure calendar of the home district. In this specification, the coefficient α_{Sun} captures across-district substitution, reflecting shopping activity diverted to other districts where retailers remain open.

1. Validity of the Identifying Assumptions

Our empirical strategy exploits within-month variation between restricted and unrestricted weeks, implicitly assuming that grocery purchasing decisions are independent across weeks. Under this assumption, expenditure patterns in unrestricted weeks provide a valid counterfactual for restricted weeks. A potential concern is that consumers may strongly prefer shopping at restricted retailers on Sundays and therefore postpone purchases until the following week, rather than reallocating spending across retailer types or days within the same week. If such intertemporal substitution were prevalent, the use of unrestricted weeks following restricted weeks in the estimation would result in biased estimates.

We view this concern as unlikely in this context. Survey evidence from the Seoul Institute indicates that 93 percent of households in Seoul follow a weekly or more frequent grocery shopping cycle: 8.8 percent shop daily, 46.7 percent shop two to three times per week, and 37.6 percent shop once per week, while only 6.9 percent shop fortnightly or less (SeoulInfographics, 2019). This distribution suggests limited scope for deferring grocery purchases by a full week.

We provide direct empirical evidence supporting the independence of expenditures across weeks by exploiting calendar variation in months with five weeks. Specifically, we compare unrestricted weeks that immediately follow a restricted week (i.e., the first week after a fourth week) to unrestricted weeks that follow another unrestricted week (i.e., the first week after a fifth week). We examine total expenditure across all retailer types over the Thursday–Wednesday shopping cycle. If consumers systematically delayed purchases by one week, total expenditure would be higher in unrestricted weeks following restricted weeks. Instead, Table A1 shows that the estimated effect of an unrestricted week following a restricted week is small and statistically insignificant, indicating no meaningful spillovers across weeks.

A second identifying assumption is that prices do not respond differentially to the closure policy on or around the restricted day. This condition is required for interpreting expenditure changes as reflecting shifts in consumer behavior rather than retailer pricing responses. If, for example, large retailers adjusted prices on non-closure days or independent retailers raised prices on restricted Sundays, expenditure changes could reflect price variation rather than changes in quantities or shopping patterns.

To assess price stability, we estimate an extension of equation (1) at the barcode level:

(2)

$$\log P_{bjt} = \sum_{k=Fri}^{Wed} \alpha_k \left(RWeek(t) \times I_{d(t)=k} \right) + \sum_{k=Fri}^{Wed} \beta_k I_{d(t)=k} + \gamma RWeek(t) + \delta_j + \xi_{m(t)} + \nu_{c(b)} + \varepsilon_{jt},$$

where P_{bjt} denotes the price of barcode b in district j on date t. Relative to equation (1), this specification includes detailed product-category fixed effects ($\nu_{c(b)}$), allowing prices to be identified from within-category variation across barcodes.

Table A2 shows no evidence of systematic price changes on or around the closure day across retailer types. This finding supports the maintained assumption of price stability and validates our interpretation of expenditure responses as reflecting changes in consumer shopping behavior.

2. *Main Results*

Table 2 presents our main results, reporting the estimated α_k coefficients from equation (1). Columns (1)–(3) focus on temporal substitution within restricted retailers, pooling all restricted retailers in column (1) and disaggregating them into large and small restricted retailers in columns (2) and (3), respectively.

The estimated coefficient on $RWeek \times$ Sunday in column (1) is large and negative, reflecting the mechanical effect of the mandatory shutdown. After exponentiating and subtracting one, the estimate of -18.52 implies an approximately 100 percent reduction in district-level card expenditure at restricted retailers on restricted Sundays, consistent with full compliance with the closure policy.

In contrast, the positive and statistically significant coefficients on other days of the week indicate substantial within-week temporal substitution. For example, the coefficient on $RWeek \times$ Monday in column (1) is 0.13, implying that expenditure at restricted retailers is 14.2 percent higher on Mondays following a closure than on Mondays in unrestricted weeks. Expenditure also rises on Saturdays—the day preceding the closure—by 5.7 percent.

To summarize the magnitude of temporal substitution, we sum the estimated percentage increases in expenditure on days with statistically significant responses—Saturday, Monday, Tuesday, and Wednesday—providing a measure of how much of the lost Sunday sales is reallocated to other days within the same week.

Columns (4) and (5) examine across-retailer substitution in response to the mandatory closure policy. Column (4) shows sales for the policy's intended beneficiaries—independent retailers—while column (5) provides results for other unrestricted retailers, which include chain convenience stores, department stores, agricultural cooperatives, exempted large retail chains, and general merchandise stores.

In column (4), the coefficient on $RWeek \times$ Sunday is 0.18, implying a 19.2 percent increase in expenditures at independent retailers on closure days. This result indicates that the mandatory shutdown policy redirects a meaningful share of consumer spending toward independent retailers. Column (5) shows that substitution is not limited to independent retailers: expenditure at other unrestricted retailers also increases on closure days, by 13.6 percent, indicating broader reallocation across retail formats.

Column (6) analyzes across-district spatial substitution, exploiting variation in closure days across districts. Specifically, it captures spending by consumers residing in Sunday-closure districts who travel to Wednesday-closure districts to shop at open large retail chains. The estimated coefficient of 0.31 on the closure day corresponds to a 36.9 percent increase in expenditure at restricted retailers in Wednesday-closure districts. While sizable in percentage terms, this effect is economically limited, as mean expenditure for this margin is small relative to the others, since only a small fraction of consumers engage in cross-district shopping.

Finally, column (7) presents results from a pooled specification that directly compares restricted and unrestricted retailers, abstracting from the split sample estimation in columns (1)–(6). The estimates in column (7) are consistent in sign and magnitude with the preceding results, reinforcing the overall pattern of temporal, across-retailer, and across-district substitution documented above.

We implement a placebo test by estimating equation (1) for non-grocery retailers, which were neither subject to the mandatory shutdown policy nor plausibly affected by grocery-specific substitution patterns. This category includes specialty stores (e.g., pet supply and sporting goods), concession stands, clothing and fashion retailers, and fabric and bedding stores. Because these retailers were unaffected by the policy and operate outside the grocery sector, we expect no systematic change in expenditure on closure days.

Table A3 confirms this prediction. The coefficient on the interaction between the restricted-week indicator and the Sunday indicator is small and statistically insignificant. The absence of any detectable effect in this placebo sample indicates that the substitution patterns documented in columns (1)–(7) of Table 2 are not driven by common time trends, seasonality, or unobserved shocks affecting retail spending more broadly, but instead reflect behavioral responses to the mandatory shutdown policy.

Table 3 provides a quantification of the magnitude of the expenditure reallocation induced by the mandatory shutdown policy across the different substitution margins in absolute terms, as well as a share of the reduction in sales during Sunday closures. We begin by estimating the reduction in sales at restricted retailers. During unrestricted weeks, the average daily expenditure at restricted retailers on Sundays is approximately KRW 105,979 thousand, which provides a benchmark for the magnitude of displaced spending.

Across-retailer substitution toward independent retailers accounts for a substantial share of this displacement. We apply the estimated $RWeek \times$ Sunday coefficient to baseline expenditure at independent retailers, defined as the mean expenditure on Thursdays in unrestricted weeks. This implies that approximately KRW 18,538 thousand in spending is redirected to independent retailers on closure days, corresponding to 17.5 percent of the reduction in sales at restricted retailers.[9] Substitution toward other unrestricted retailers is of comparable magnitude: we estimate that KRW 21,637 thousand, or 20.4 percent of displaced spending, is reallocated to these outlets.

Temporal substitution within restricted retailers accounts for an additional share of the reallocation. Aggregating the estimated percentage increases in expenditure on Saturday, Monday, Tuesday, and Wednesday and applying them to the baseline restricted-retailer expenditure yields a total of KRW 26,058 thousand. This corresponds to 24.6 percent of the reduction in Sunday sales at restricted retailers.

Finally, across-district substitution plays a quantitatively minor role. We estimate that approximately KRW 1,055 thousand—about 1.0 percent of displaced sales—is diverted to restricted retailers in neighboring districts with different closure days.

Taken together, these estimates show that the majority of displaced expenditure is reallocated through within-week and across-retailer substitution, with cross-district shopping accounting for only a small fraction of the response.

Our estimates are consistent with findings from other studies. Prior work using consumer survey data finds that approximately 17–19 percent of sales at large retail chains are redirected to independent retailers (Choi and Jeong, 2016), while firm-level evidence shows comparable increases in independent-retailer sales in areas surrounding restricted stores (Yang and Kim, 2019). The magnitudes we estimate fall squarely within this range.

An additional question is whether the mandatory shutdown policy affected online grocery shopping. To examine this margin, we classify online grocery transactions into three categories. The first consists of online stores operated by restricted retailers (e.g., Costco.com). The second includes unrestricted online retailers that specialize in grocery delivery services. The third com-

[9] An alternative approach uses a linear specification with expenditure levels as the dependent variable to compute diversion ratios directly. The implied magnitudes are similar. We report results from the log-linear specification, which better accommodates the skewness of the expenditure distribution.

prises other unrestricted online platforms on which consumers may purchase grocery-related items; for example, a purchase of a condiment on a general marketplace such as eBay is classified as grocery-related based on the transaction classification algorithm used by the card payment processor.

Table A4 reports results from specifications analogous to those used in the main analysis. Online transactions in the card data are timestamped by order time. Since deliveries by restricted retailers are prohibited on closure days, the large negative coefficient for restricted retailers' own online platforms is mechanical. We find no statistically or economically meaningful spillovers from the closure policy to online unrestricted platforms. This absence of substitution suggests that, in the Korean context, online and brick-and-mortar grocery retail operate as largely segmented markets.

B. Heterogeneity by Independent Retailer Market Share

We next examine whether the effects of the mandatory shutdown policy vary with local retail market structure. Where independent retailers are more competitive, they are more likely to serve as substitutes for restricted retailers, implying greater across-retailer substitution.

We measure market structure using the ratio of average daily card expenditure at independent retailers to that at restricted retailers within each district. Districts above the median are classified as having a strong independent retail presence, while those at or below the median are classified as restricted-retailer dominated. We estimate equation (1) separately for these groups.

Table 4 shows substantial heterogeneity in the share of displaced restricted-retailer sales captured by different substitution channels across market structures. In districts with a strong independent retail presence, 19.6 percent of displaced expenditure is redirected to independent retailers, compared with 15.8 percent in restricted-retailer-dominated districts. A similar pattern holds for other unrestricted retailers (25.1 versus 16.9 percent). These differences in shares largely reflect differences in the reduced restricted retailer sales across districts (KRW 89,319 thousand versus 122,639 thousand). These results indicate that the policy's ability to support independent retailers, in terms of the share of reduced sales at restricted retailers that they capture, depends critically on local market structure and is likely to weaken as their market share continues to decline.

C. Effects on Retailer Profits

To assess the impact of the mandatory shutdown policy on retailer profits, we translate the estimated expenditure responses into profit changes using median profit margins for each retailer type.

Table 5 summarizes the implied profit effects. Restricted retailers experience an average profit loss of KRW 2,498 thousand per district. Of this loss, 34.1 percent (KRW 851 thousand) is reallocated to independent retailers—the policy's intended beneficiaries—while 28.2 percent accrues to other unrestricted retailers. An additional 24.6 percent (KRW 614 thousand) is recovered by restricted retailers through temporal substitution, and 1.0 percent reflects across-district substitution. Notably, profit gains for independent and other unrestricted retailers exceed their corresponding expenditure gains, reflecting the higher profit margins of these retailers relative to restricted retailers.

To gauge the economic significance of these gains at the firm level, we compute the implied increase in monthly profits for independent retailers. Dividing the district-level profit gain among independent retailers by the average number of such retailers per district yields an average gain of KRW 4,904 per independent retailer. After adjusting for the market share of the card payment processor and accounting for two closure days per month, this translates into an average monthly profit increase of approximately USD 41.7 per independent retailer (PPP-adjusted).

D. Independent Retailer Survival

Although the per-retailer profit gains are modest, the mandatory shutdown policy could still be welfare relevant if it affected the prevalence of independent retailers. To examine this possibility, we study entry and exit dynamics using an event-study design.

Because the policy was implemented nationwide, we define treatment status based on exposure to the policy before the policy was enacted. The treatment group consists of districts that hosted both large and small restricted retailers in each year from 2007 to 2011, a pre-policy period chosen to exclude the early expansion phase of large retail chains. The control group includes districts that never hosted large restricted retailers during the same period and were therefore less likely to be affected by the policy. This yields 21 treated districts and 71 control districts.

A key concern with this strategy is that districts with many restricted retailers may differ systematically from others in ways that affect independent retailer dynamics. To address differential pre-trends, we follow Wolfers (2006). Specifically, for each district, we estimate a linear trend using only pre-treatment periods, with the number of independent retailers as the outcome. We then use the extrapolated residuals as the dependent variable in the event study. The specification includes district and year fixed effects and controls for population and the presence of chain convenience stores, department stores, and large and small restricted retailers. This residualization removes district-specific pre-trends and time-invariant heterogeneity. We also adopt the normalization proposed by Miller (2023), which constrains the average of pre-treatment coefficients to zero rather than normalizing a single pre-period. This approach mitigates concerns about over-interpreting individual pre-treatment estimates.

Results are presented in Figure 5. The post-policy estimates are positive but small and statistically indistinguishable from zero, implying no detectable effect of the mandatory shutdown policy on the number of independent retailers. The average post-policy coefficient corresponds to a 2.2% change relative to the mean pre-policy level, indicating a quantitatively modest effect.

These results are robust to alternative definitions of treatment and control groups based on different thresholds for the presence of restricted retailers, as well as to alternative approaches for addressing pre-trends, including the method proposed by Goodman-Bacon (2021). Consistent with these findings, the event-study results align closely with the aggregate trends shown in Figure 4, which display no discernible change in independent retailer trajectories before or after the policy.

Overall, we find that while the policy reallocates some consumer spending toward independent retailers, it does not appear to affect their entry or survival over the longer horizon. This pattern suggests that the expenditure shifts resulting from this policy are insufficient to alter the structural forces shaping independent retailer viability.

E. Robustness Checks

1. Transaction Counts

To assess whether the expenditure responses documented in the main results reflect changes in shopping frequency rather than solely changes in spending intensity, we re-estimate our baseline specification using transaction counts as the outcome. Interpreting each card transaction as a store visit, this exercise provides a complementary measure of consumer diversion and intertemporal substitution.

We estimate the same specification as in equation (1), replacing expenditures with the log of district-level transaction counts. Table A5 reports the results by day of the week and retailer type. As expected, transactions at restricted retailers fall by 100 percent on restricted Sundays, reflecting complete closure (column (1)). On closure days, transactions increase by 17.4 percent at independent retailers and by 11.6 percent at other unrestricted retailers (columns (4) and (5)), mirroring the expenditure-based diversion patterns. Within restricted retailers, transaction counts rise on non-closure days—by 5.1 percent on Saturdays and Tuesdays and by 9.4 percent on Mondays—consistent with intertemporal shifting of shopping activity. We also observe a 23.4 percent increase in transactions at restricted retailers in Wednesday-closure districts made by consumers from Sunday-closure districts, indicating cross-district substitution (column (6)).

Comparing transaction and expenditure responses clarifies how consumers adjust their behavior. On closure days, expenditures at independent retailers increase by 19.2 percent, while transaction counts rise by 17.4 percent, implying a modest increase in spending per visit. Switchers spend slightly more per transaction at independent retailers (KRW 15,422) than regular Sunday shoppers (KRW 15,337), but substantially less than their typical spending at large retail chains in the absence of the policy (KRW 32,771). This pattern is consistent with diverted consumers spending less at independent retailers, rather than shifting their full shopping baskets there.

2. *Cross-District Comparison Using Wednesday-Closure Districts as Controls*

In this subsection, rather than using non-closure weeks as the counterfactual, we use districts with Wednesday closures as the comparison group for districts with Sunday closures. This across-district design provides a complementary robustness check but is not our preferred specification, as it requires the absence of across-district spillovers. Our main results indicate that such spillovers, while economically small, are statistically detectable, violating this identifying assumption. We therefore interpret the estimates in this subsection as suggestive rather than definitive.

Table A6 reports summary statistics for daily district-level expenditures during restricted weeks, by district closure type. We exclude Wednesdays so that Wednesday-closure districts do not face a closure day in the estimation sample, while Sunday-closure districts do. As expected, expenditures at restricted retailers are lower in Sunday-closure districts. At the same time, average expenditures at unrestricted retailers are generally higher in Sunday-closure districts for both intended and unintended beneficiaries. This suggests that Sunday-closure districts tend to have higher baseline economic activity and scale.

We estimate the following specification:

$$Y_{jt} = \sum_{k=Fri}^{Tue} \alpha_k \big(SunClosure(j) \times I_{d(t)=k}\big) + \sum_{k=Fri}^{Tue} \beta_k I_{d(t)=k} + \delta_j + \xi_{m(t)} + \varepsilon_{jt}, \tag{3}$$

where $SunClosure(j)$ equals one for districts in which restricted retailers close every other Sunday and zero for districts with Wednesday closures. We exclude districts with mixed schedules and restrict attention to restricted weeks. Thursday is the omitted category, and Wednesday is excluded. Relative to equation (1), this specification does not capture across-district substitution and instead isolates across-retailer diversion and within-week temporal reallocation.

Table A7 reports the α_k estimates. We find clear evidence of temporal substitution within restricted retailers, with expenditures shifting away from Sundays toward other days, particularly adjacent days. At independent retailers, expenditures rise on restricted Sundays, consistent with the main results, but decline on Saturdays, with modest offsetting increases on Mondays. We find no statistically significant spillovers to other unrestricted retailers. However, as suggested by the summary statistics, baseline differences between Sunday- and Wednesday-closure districts raise concerns about the validity of this comparison.

To address this concern, we also compare Sunday-closure districts during restricted weeks to Wednesday-closure districts during unrestricted weeks. The results, reported in Table A8, closely mirror the main findings: expenditures shift intertemporally within restricted retailers and divert toward independent retailers. In contrast to the main specification, however, we do not detect statistically significant spillovers to other unrestricted retailers.

3. *A Triple-Difference Design Across Districts, Restricted Weeks, and Days*

As a final robustness check, we estimate a triple-difference specification that combines three sources of variation: cross-district differences in the designated closure day, within-district differences between restricted and unrestricted weeks, and day-of-week variation. As in the preceding across-district comparison, this design is not our preferred specification because it requires the absence of across-district spillovers. Our main analysis documents that such spillovers, while quantitatively small, are statistically detectable. We therefore interpret the triple-difference estimates as corroborative rather than as our primary source of identification.

The results are reported in Table A9. Because identification relies on variation in closure timing across districts, this specification is well suited for examining across-retailer substitution and within-week temporal reallocation, but—by construction—does not isolate across-district substitution. Consistent with the main results, we find robust evidence of diversion toward independent retailers on restricted Sundays, with expenditures increasing by 13.9 percent, alongside smaller but statistically significant increases of 6.2 percent at other unrestricted retailers. These patterns confirm that the mandatory shutdown generates spillovers not only toward the intended beneficiaries—independent retailers—but also toward other unrestricted formats.

We also observe clear temporal substitution within restricted retailers, with expenditures shifting toward adjacent non-closure days, as expected. Expenditures increase by 9.4 percent on Saturdays and by 19.7 percent on Mondays, confirming our baseline estimates. Overall, even under this alternative identification strategy, the triple-difference results reproduce the core findings of the paper: consumers reallocate expenditures intertemporally and divert spending toward both intended and unintended beneficiaries of the policy.

V. Consumer Welfare

A. *Theoretical Framework*

The preceding section documented substantial reallocation of grocery shopping in response to mandatory Sunday closures, both across retailer types and across space. In this section, we use these diversion patterns to quantify the consumer welfare costs of the policy. The central identifying insight is that, on closure days, observed spatial diversion toward more distant large-format retailers primarily reflects an increase in travel-related hassle costs, rather than changes in prices, product availability, or store attributes. This setting therefore provides revealed-preference variation that links expenditure diversion to consumers' sensitivity to travel time and, in turn, into money-metric measures of welfare. Our discrete-choice framework uses this information to aggregate policy-induced diversion shares—across space, retailers, and days—into overall consumer welfare losses. We begin by outlining the model's key primitives, and then describe the likelihood function and estimation procedure.

In our theoretical framework, each consumer allocates her grocery budget across an infinitesimal continuum of expenditure units. Relative to modeling store visits, this approach avoids treating small, incidental purchases at smaller retailers (e.g., buying a few drinks at a convenience store) as equivalent to large grocery trips at big-box supermarkets (Ellickson, Grieco and Khvastunov, 2020). For each expenditure unit i, a consumer residing in district j makes a discrete choice of shopping mode c under policy environment $s \in \{U,R\}$, where U denotes an unrestricted Sunday and R denotes a restricted Sunday. The indirect utility from allocating expenditure unit i to mode c (e.g., Sunday shopping at restricted retailers) is given by:

$$u^{s}_{ijc} = V^{s}_{jc} + \varepsilon_{ijc}, \qquad s \in \{U,R\}. \tag{4}$$

where V^{s}_{jc} denotes the deterministic component of utility in policy environment s, and ε_{ijc} is an i.i.d. Type-I extreme value shock capturing unobserved tastes for shopping mode c. We assume that the distribution of these unobserved tastes is invariant across policy environments. In particular, for each expenditure unit we define the Sunday–restricted-retailer option (Sun) as allocating that unit to a restricted retailer on Sunday. Conditional on choosing Sun, the consumer patronizes the nearest open restricted branch in environment s. Thus, the closure policy does not introduce a new alternative; it affects the attractiveness of Sun only through the travel-time attribute $TravelTime^{s}_{j}$.

We limit our analysis to consumers residing in the Sunday-closure districts who, in the absence of operating-day regulations, would have shopped at restricted retailers on an unrestricted Sunday (Sun^{U}). These consumers are directly affected by the policy and therefore most informative for identifying substitution patterns. By concentrating on this directly affected group, we can simplify the choice set—and therefore the relevant set of deterministic utilities V^{s}_{jc}—based on the substitution patterns revealed in the empirical analysis in Section IV.2. Specifically, the deterministic utility V_{jc}^{s} varies by shopping mode as follows:

$$(5)\qquad V_{jc}^{s} = \begin{cases} \theta_{\text{Sun}} + \kappa TravelTime_{j}^{s} & \text{if } c = \text{Sunday Shopping at Restricted Retailer (Sun)}, \\ \theta_{jc} & \text{if } c \in \left\{ \begin{array}{l} \text{Temporal Substitution (Tem)} \\ \text{Independent Retailers (Ind)} \\ \text{Unintended Beneficiaries (Uni)} \end{array} \right\}, \\ 0 & \text{if } c = \text{Outside Option (Out)}. \end{cases}$$

A consumer who allocates a unit of expenditure to a restricted retailer on Sunday receives deterministic utility θ_{Sun} but incurs a travel cost. We treat travel time, $TravelTime_{j}^{s}$, as a price analogue, converting travel costs into monetary units and evaluating welfare accordingly. The parameter κ captures the marginal (dis)utility of travel time. To measure $TravelTime_{j}^{s}$, we calculate the great-circle distance (km) from the population-weighted centroid of district j to the nearest open restricted retailer under environment s, using neighborhood-level population data. This distance is divided by the average weekend driving speed of each province—24.2 km/h for Seoul, 45.8 km/h for Gyeonggi, and 41.4 km/h for Incheon—to obtain travel time.[10] The average (median) travel time to the nearest open restricted retailer increases from 2.6 (2.2) minutes on unrestricted Sundays to 17.6 (17.0) minutes on restricted Sundays.

Figure A2 illustrates how cross-district diversion varies with the implied increase in travel time to the nearest open large-format chain. Across Sunday-closure districts, those experiencing larger increases in travel time exhibit a lower fraction of counterfactual expenditure (expenditure at restricted retailers on unrestricted Sundays) redirected to restricted retailers in Wednesday-closure districts on restricted Sundays. This negative association motivates our treatment of travel time as a generalized price in the discrete-choice model and highlights that the small aggregate share of cross-district substitution masks substantial heterogeneity across districts:

[10] Average weekend speeds are drawn from Seoul Metropolitan Government, TOPIS Center (2019) for Seoul and Incheon Metropolitan City (2021) for Incheon. For Gyeonggi, we calculate a length-weighted space-mean speed across three non-expressway surface road classes—nationally designated local routes (gukjido), provincial roads (jibangdo), and district roads (sigundo)—using each class's 2019 average speed and reported network length (Gyeonggi-do Provincial Government, 2020).

most diversion comes from locations where travel-time increases are modest, while diversion tends to be negligible once the travel-time ratio $TravelTime_j^R/TravelTime_j^U$ becomes large.

We assume θ_{Sun} is constant across districts. Intuitively, consumers value a Walmart outlet in district A and a Walmart outlet in district B equally; the only distinction is the travel time from their residence. A consumer residing in district j typically patronizes the nearest Costco under the unrestricted environment ($s = U$; travel time $TravelTime_j^U$). Under the mandatory-closure environment ($s = R$), if that outlet is closed on Sunday, the consumer instead travels farther to the nearest open Costco—often in a neighboring district whose closure day differs—implying travel time $TravelTime_j^R$.[11] The deterministic utilities governing the other substitution options are allowed to vary across districts but are held fixed across policy environments.[12] We normalize the deterministic utility of allocating one unit of expenditure to the outside option to zero.

After recovering the deterministic utility parameters and the travel-time coefficient κ, we compute the compensating variation (CV) for an expenditure unit of a representative consumer residing in district j using the standard log-sum expression. In our specification, all deterministic utility components (including travel-time terms) depend on the district of residence j but not on the expenditure-unit index i. Therefore, for a representative consumer residing in a given district j, the implied CV is common across expenditure units, i.e., $CV_{ij} = CV_j$ for all i, and we denote it by CV_j:

$$
\begin{aligned}
CV_j = \frac{1}{\kappa}\Big\{ &\log\Big(\exp(\theta_{\text{Sun}} + \kappa\, TravelTime_j^R) + \sum_{k\neq \text{Sun}} \exp \theta_{jk}\Big) \\
&- \log\Big(\exp(\theta_{\text{Sun}} + \kappa\, TravelTime_j^U) + \sum_{k\neq \text{Sun}} \exp \theta_{jk}\Big)\Big\}
\end{aligned} \tag{6}
$$

To express CV in monetary terms, we monetize travel time rather than prices: we scale the log-sum expression by $1/\kappa$ (the travel-time coefficient) and multiply by an empirical estimate of the value of time. Following Kim, Lee and Yun (2017), we set the weekend value of time to KRW 9,044 per hour ($\simeq$ USD 9.31 per hour using the PPP exchange rate).

[11] Prior work documents within-franchise similarity across locations (e.g., Walmart outlets in different districts). We extend this idea to restricted retailers as a category, assuming a comparable utility across districts. See DellaVigna and Gentzkow (2019); Hitsch, Hortacsu and Lin (2021); Hwang, Bronnenberg and Thomadsen (2010).

[12] Allowing the utilities of substitution options to differ by district mitigates concerns about unrealistic independence-of-irrelevant-alternatives (IIA) substitution. District-level heterogeneity induces distinct substitution patterns across residential areas, yielding a more credible model.

Using the monetized CV_j for each district, we compute two summary measures of consumer welfare loss. The first is the CV per affected consumer per closure:

$$\text{CV per affected consumer per closure} = \frac{\sum_j CV_j \cdot N_j}{\sum_j N_j} \tag{7}$$

where N_j denotes the number of affected consumers in district j, proxied by the number of transactions at restricted retailers on the unrestricted Sunday among residents of district j, assuming one transaction per consumer. This expression yields the average CV per affected consumer, weighting each district by its number of affected consumers.

The second measure is the total monthly CV for Sunday-closure districts in the Seoul Metropolitan Area:

$$\text{Total Monthly CV} = \frac{1}{0.242} \cdot 2 \cdot \sum_j CV_j \cdot N_j \tag{8}$$

where district-level CVs are aggregated using the number of affected consumers as weights, multiplied by two to reflect two closure Sundays per month, and grossed up by the inverse of the card payment processor's market share (24.2%).

B. Estimation

We next derive the relevant conditional probabilities from the discrete-choice model in order to construct the likelihood function. Specifically, we consider (i) the probability that an expenditure unit is still spent at a restricted retailer on a restricted Sunday—despite the greater travel time—conditional on that unit having been spent at a restricted retailer on an unrestricted Sunday, and (ii) the probability that an expenditure unit is diverted to any substitute channel under the same condition. These two probabilities are given in equations (9) and (10), respectively.[13]

$$(9)\quad \Pr_j(\text{Sun}^R \,|\, \text{Sun}^U)$$

$$= \Pr_j\big(\text{Select Sun with shutdown} \;\big|\; \text{Select Sun without the shutdown}\big)$$

$$= \exp\big[\kappa\,(\mathit{TravelTime}_j^R - \mathit{TravelTime}_j^U)\big] \cdot \frac{\exp\big(\theta_{\text{Sun}} + \kappa\,\mathit{TravelTime}_j^U\big) + \sum_{k\neq\text{Sun}} \exp\theta_{jk}}{\exp\big(\theta_{\text{Sun}} + \kappa\,\mathit{TravelTime}_j^R\big) + \sum_{k\neq\text{Sun}} \exp\theta_{jk}}$$

$$(10)\quad \Pr_j(c \,|\, \text{Sun}^U)$$

$$= \Pr_j\big(\text{Select } c \text{ with shutdown} \;\big|\; \text{Select Sun without the shutdown}\big)$$

$$= \exp\theta_{jc}\,\frac{1 - \exp\big(\kappa\,\mathit{TravelTime}_j^R \,-\, \kappa\,\mathit{TravelTime}_j^U\big)}{\exp\big(\theta_{Sun} + \kappa\,\mathit{TravelTime}_j^R\big) + \sum_{k\neq\text{Sun}} \exp\theta_{jk}}$$

$$\text{for } c \in \{\text{Tem, Ind, Uni, Out}\}$$

The log likelihood function to maximize is:

$$(11)\qquad \ell(\Theta) = \sum_{j=1}^{J}\Big[\mathit{Amt}_{j,\text{Sun}} \ln \Pr_j(\text{Sun}^R \mid \text{Sun}^{\text{U}}) + \sum_{c\neq\text{Sun}} \mathit{Amt}_{j,c} \ln \Pr_j(c \mid \text{Sun}^{\text{U}})\Big].$$

For $c \neq \text{Sun}$, we define $\mathit{Amt}_{j,c}$ as the change in spending for shopping mode c between the unrestricted and restricted Sundays. We measure $\mathit{Amt}_{j,\text{Sun}}$ as the increase in bank card expenditure at restricted retailers located in districts where they remain open on Sunday (i.e., Wednesday-closure districts) on the restricted Sunday. We define $\mathit{Amt}_{j,\text{Out}}$ residually as the portion of spending that would have occurred at restricted retailers on Sundays in the absence of the regulation but is not captured by observed increases in any other shopping mode. Specifically, it equals the counterfactual Sunday expenditure at restricted retailers minus the sum of spending changes across all other modes. The counterfactual expenditure is proxied by expenditure at restricted retailers in Sunday-closure districts on unrestricted Sundays.

Prior to maximizing the log-likelihood in equation (11), we recover closed-form expressions

[13] See Appendix A.A5 for derivation details.

for the district-by-mode deterministic utility terms θ_{jc} for $c \in \{\text{Tem}, \text{Ind}, \text{Uni}\}$ in equation (5). Dividing equation (10) by $\Pr_j(\text{Out} \mid \text{Sun}^U)$ eliminates the common diversion term; taking logs yields closed-form estimates $\hat{\theta}_{jc}$.[14] We substitute these district-specific terms into equation (11). With $\{\theta_{jc}\}_{c\in\{\text{Tem},\text{Ind},\text{Uni}\}}$ fixed, only two parameters remain to be estimated: θ_{Sun} and κ.

We then estimate θ_{Sun}, which captures the utility of Sunday shopping at restricted retailers, and κ, the marginal disutility of travel time, by maximizing the weighted log-likelihood in equation (11) using the Broyden–Fletcher–Goldfarb–Shanno (BFGS) algorithm. To compute standard errors, we implement a district-level bootstrap with 1,000 resamples with replacement.

Table 6 reports the parameter estimates. We estimate the model using Sunday-closure districts in the Seoul Metropolitan Area and report results for three nested samples (Baseline / Extended I / Extended II; $N = 42, 46, 50$). The baseline sample uses districts requiring no regularization; extended samples apply small, data-scaled floors to ensure non-negativity and preserve accounting identities; Appendix A.A5 provides implementation details and district lists.[15] All square brackets report 95% bootstrap confidence intervals, constructed from 1,000 replications using the 2.5$^{\text{th}}$ and 97.5$^{\text{th}}$ percentiles.

Rows 1–2 of Table 6 report estimates of κ and θ_{Sun}. The estimated disutility parameters are stable across the 50-district specifications ($|\kappa| \simeq 41$–49; $\theta_{\text{Sun}} \simeq 6.9$–$8.3$). Rows 3–5 report the median deterministic utilities for each shopping mode (Temporal: temporal substitution; Independent: substitution toward independent retailers; Unintended: substitution toward unintended beneficiaries).

Row 6 reports the CV per affected consumer per closure from equation (7). Under our preferred specification (Extended II with $\rho = 0.5\%$), the CV per affected consumer per Sunday closure is approximately USD 0.85; this estimate is stable across regularization choices and

[14] See Appendix A.A5 for derivation details.

[15] We start from 54 Sunday-closure districts and drop four island/low-traffic jurisdictions. In a handful of remaining districts, week-to-week volatility can yield negative estimated diversion amounts and/or a negative residual outside option. We impose a floor equal to ρ times unrestricted-Sunday sales at large retailers, with $\rho \in \{1\%, 0.5\%, 0.1\%\}$, and adjust the diversion amounts, the residual outside amount, and, when needed, the counterfactual expenditure (expenditure at restricted retailers in Sunday-closure districts on unrestricted Sundays) accordingly. In the Extended II sample, a regularization step is required for a small number of districts in which the implied outside option is negative (4 out of 50 districts). The procedure minimally increases the counterfactual total expenditure for restricted retailers on the unrestricted Sunday to restore non-negativity and preserve the accounting identity, which mechanically scales down all implied diversion shares in those locations. Appendix A.A5 reports the magnitude of these adjustments. Reassuringly, the key welfare estimates are highly comparable across the Baseline, Extended I, and Extended II samples (Table 6).

sample definitions, ranging from USD 0.78 to 0.88. With two closure Sundays per month, a habitual large-chain shopper bears a monthly welfare loss of roughly USD 1.6–1.8. Row 7 reports the total monthly CV from equation (8), estimated at approximately USD 0.98 million under our preferred specification.

Relative to mean spending of USD 33.66 per transaction at restricted retailers, the implied per-transaction welfare loss corresponds to roughly 2.5–2.6 percent of a typical basket. These magnitudes indicate that the per-transaction cost to consumers is modest, but aggregate to a sizeable monthly and annual loss at the market level.

VI. Discussion

To assess the overall welfare consequences of the mandatory Sunday closure, we aggregate producer profits and consumer welfare, as summarized in Table 7. All figures are expressed on a monthly basis for the Sunday-closure districts of the Seoul Metropolitan Area and reported in PPP-adjusted USD. Producer-side totals are constructed by scaling per-district profit estimates (column (3) of Table 5) by two closures per month and the 50 districts included in the welfare analysis, adjusting for the card processor's market share and converting from KRW to PPP-adjusted USD. Consumer welfare losses correspond to the monthly counterpart of our compensating-variation estimates.

The central result is that the policy's welfare impact is overwhelmingly driven by consumer losses. Aggregate consumer welfare declines by approximately USD 0.98 million per month. On the producer side, independent retailers—the policy's intended beneficiaries—gain about USD 362 thousand per month, less than half of the net monthly losses borne by restricted retailers (approximately USD 791 thousand after accounting for temporal and spatial substitution). A substantial share of the remaining reallocation accrues to other unrestricted formats—about USD 299 thousand per month, concentrated in department stores and convenience stores that are themselves largely owned by conglomerates and are outside the policy's intended scope. Taking all producer groups together, total producer surplus declines by approximately USD -130 thousand per month (A + B).

Put differently, for every USD 1 in profits gained by independent retailers, consumers lose about USD 2.71, and restricted retailers suffer net losses of approximately USD 2.18. These ratios make clear that the policy inefficiently redistributes revenue across retail formats while imposing substantial welfare costs on consumers, resulting in an unambiguously negative net welfare effect.

A natural question is whether the equity characteristics of the beneficiaries might justify these efficiency costs. In Section A.A3, using nationally representative household expenditure data, we show that self-employed solo retailers—a proxy for independent retailers—are drawn from households with expenditure levels roughly 8 to 20 percent below those of comparable groups (all other households, solo self-employed in non-retail sectors, and wage workers in retail). The fact that the policy's beneficiaries are disproportionately lower-resource households provides some equity-based rationale for the transfers. However, two things limit this argument. First, the monthly profit gain per independent retailer is only USD 41.7—too small and thus unlikely to change household economic circumstances. Second, the policy does not improve independent retailers' long-run survival, implying that whatever equity benefits arise are transitory rather than structural. Even viewed through a distributional lens, the magnitude of consumer welfare losses relative to producer gains underscores the high efficiency cost of achieving these distributional objectives through mandatory closures.

Although our estimates are specific to the Seoul Metropolitan Area in 2019, the central findings likely extend beyond this setting. The result that most displaced spending is recaptured by restricted retailers or diverted to other unrestricted conglomerate-owned formats reflects a structural feature of modern retail markets: large chains can absorb lost sales across multiple formats they own and locations they operate, leaving only a small share for independent retailers. This suggests that operating-day restrictions are a blunt instrument for redirecting spending toward small retailers in any market where large chains operate multiple branches or formats across regions.

VII. Conclusion

This paper studies South Korea's biweekly operating-day restrictions on large retail chains using granular card transaction data and quasi-experimental variation in closure timing across districts and weeks. By combining reduced-form evidence on substitution patterns with a structural discrete-choice framework, we provide a comprehensive assessment of how mandatory closures reallocate spending across retailers, time, and space, and how these reallocations translate into consumer welfare losses.

We document four primary margins of adjustment: diversion toward independent retailers, spillovers to other unrestricted formats, intertemporal substitution within restricted chains, and limited cross-district substitution driven by differential closure schedules. While independent retailers—the intended beneficiaries—capture a nontrivial share of displaced spending, a comparable share accrues to other unrestricted formats, and a substantial fraction is recouped by restricted retailers through timing adjustments. Event-study evidence shows no detectable long-run effects on the survival of independent grocers. Overall, the policy primarily reshuffles expenditures across formats rather than inducing persistent changes in market structure.

To quantify the consumer cost of these reallocations, we exploit spatial diversion on closure days—where substitution toward more distant stores reflects increased travel-related hassle—to identify consumers' sensitivity to distance. Embedding this variation in a discrete-choice model yields precise estimates of compensating variation per transaction. Aggregated to the district-month level, consumer welfare losses substantially exceed the profit gains of independent retailers. As a result, the net welfare effect of the policy is negative and driven overwhelmingly by consumer costs, while producer gains and losses largely offset across formats.

These findings have direct implications for ongoing policy debates in Korea. Recent legislative proposals would expand trading-day restrictions to additional retail formats and public holidays, and similar regulatory attention is increasingly directed at large online marketplaces. Our results suggest that extending channel-blocking regulations is likely to generate meaningful consumer welfare losses, while only weakly targeting benefits toward small, independent retailers. Policymakers weighing such interventions should therefore carefully consider substitution patterns and consumer costs alongside distributional objectives, and compare mandatory closures to alternative, more targeted instruments for supporting small retailers.

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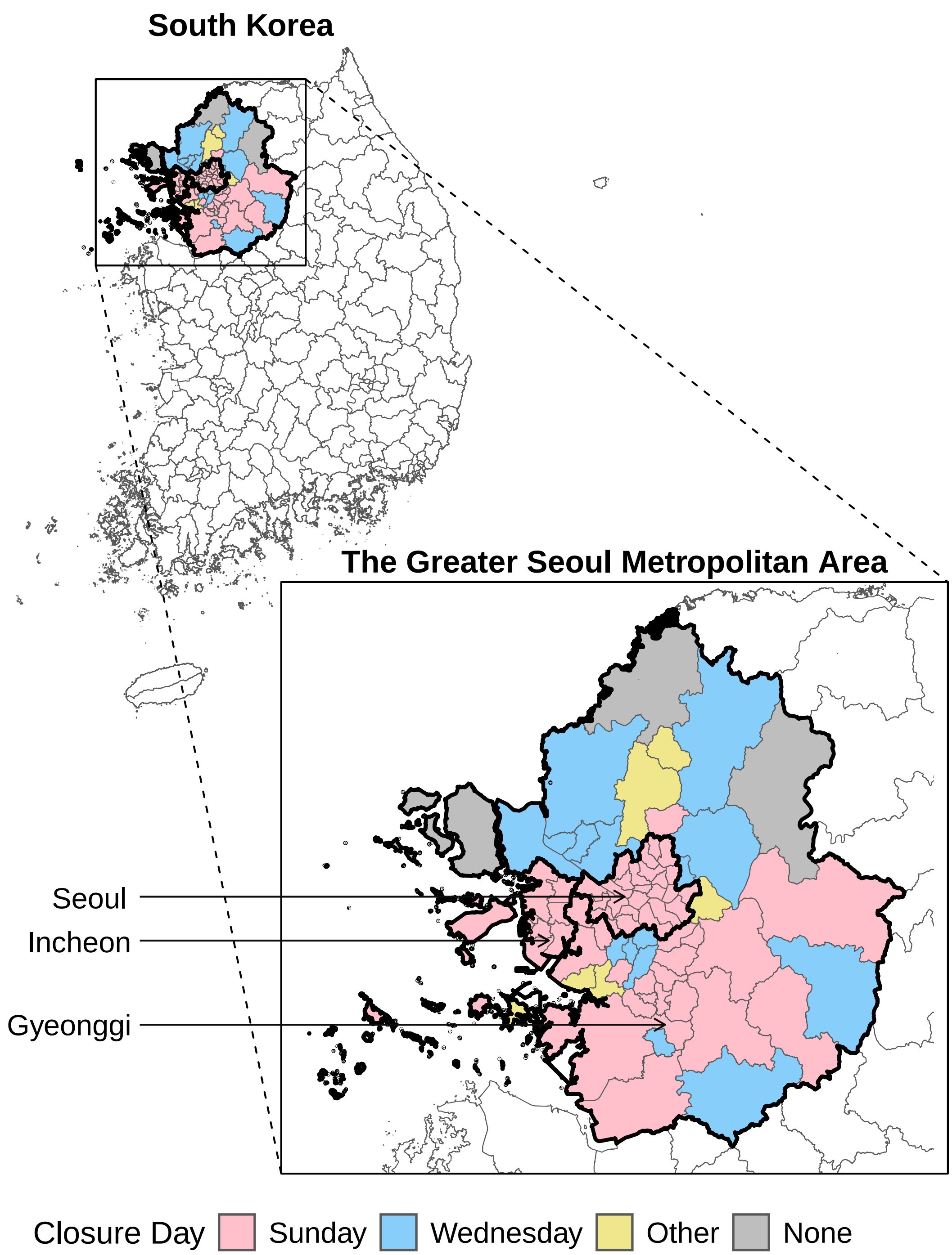
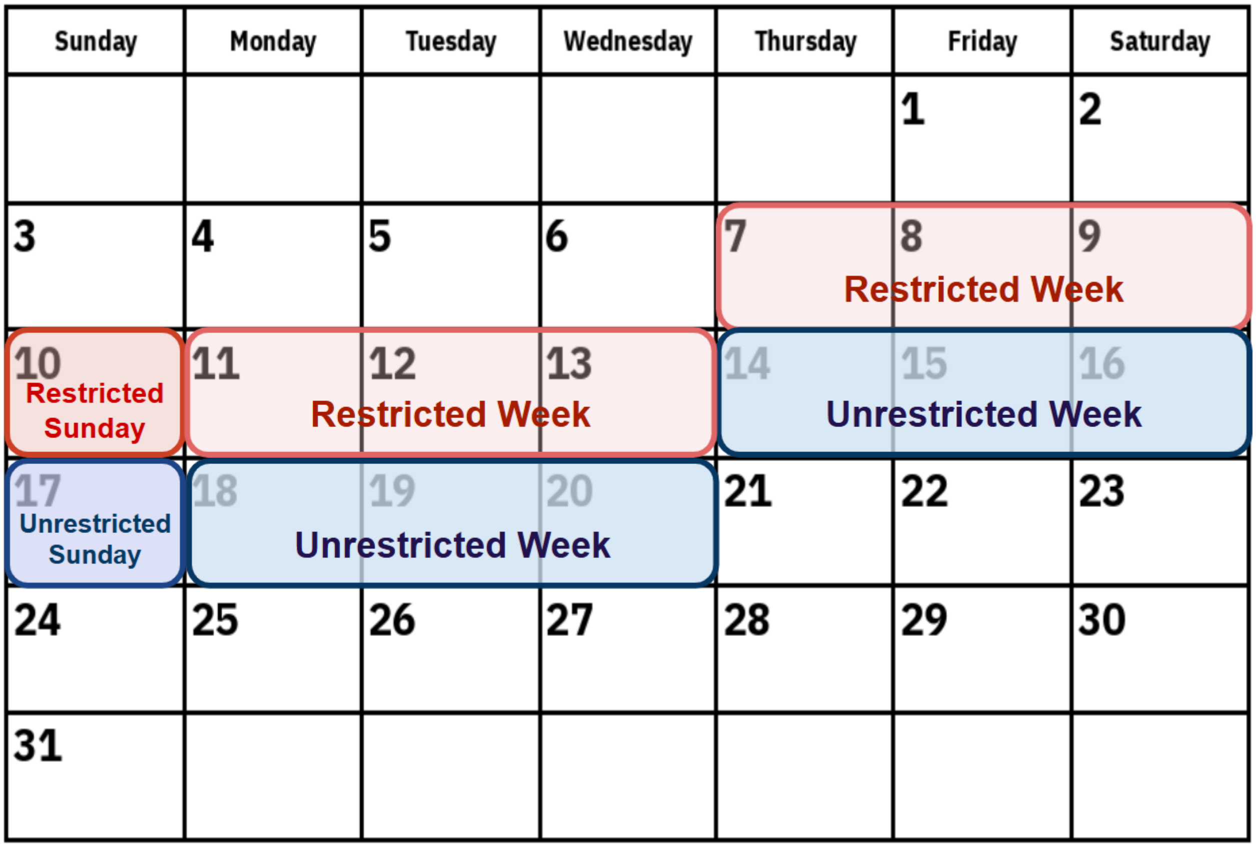
Figures

Figure 1. Geographic Variation in Mandatory Shutdown Schedules



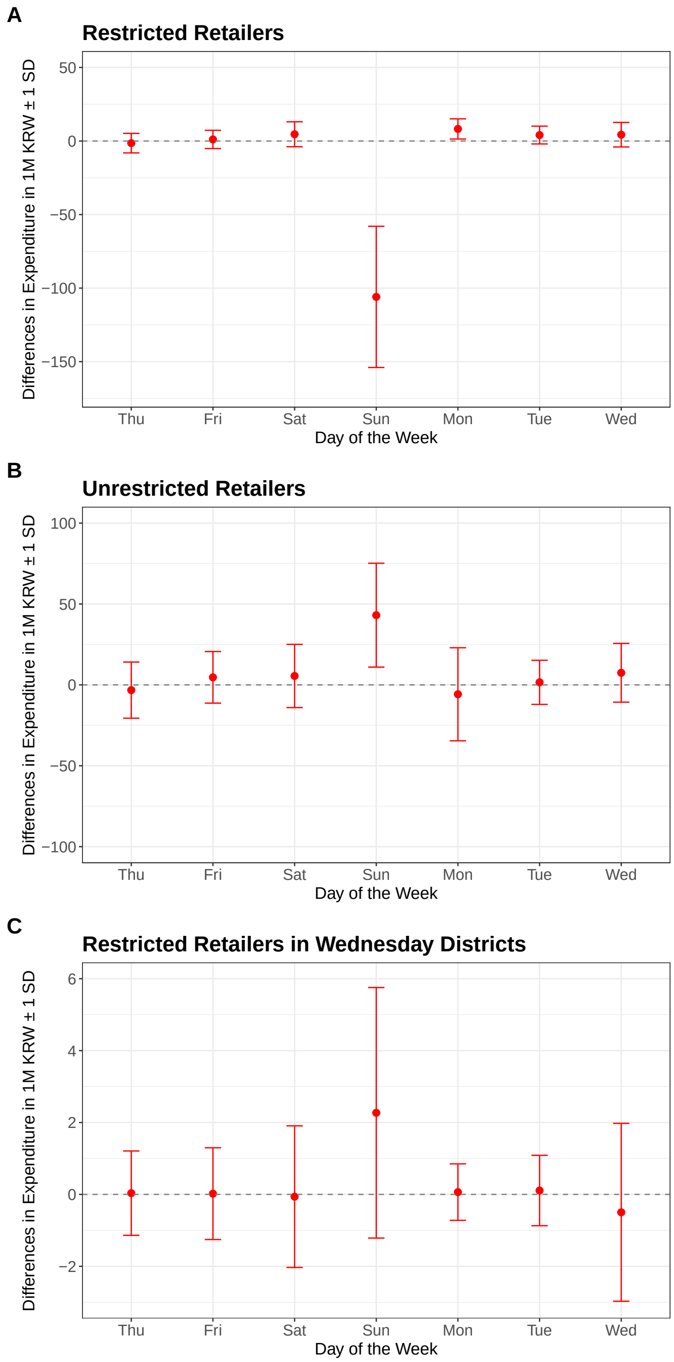
Note: This figure shows the variation in the mandatory closure schedule for large retail chains across districts of the Seoul Metropolitan Area, South Korea. Since 2012, the policy has required two closures per month, with the specific days determined by local district ordinances. Most districts adopt the second and fourth Sundays (pink), while others designate the second and fourth Wednesdays (blue) or a mixed schedule (yellow). Districts labeled "None" (gray) have no designated closure days, either because no regulated retailers are present or the population is small. Data on closure days were hand-collected from local government ordinances, supplemented with news reports (BNT News, 2023).

Figure 2. Week Definition for Empirical Strategy



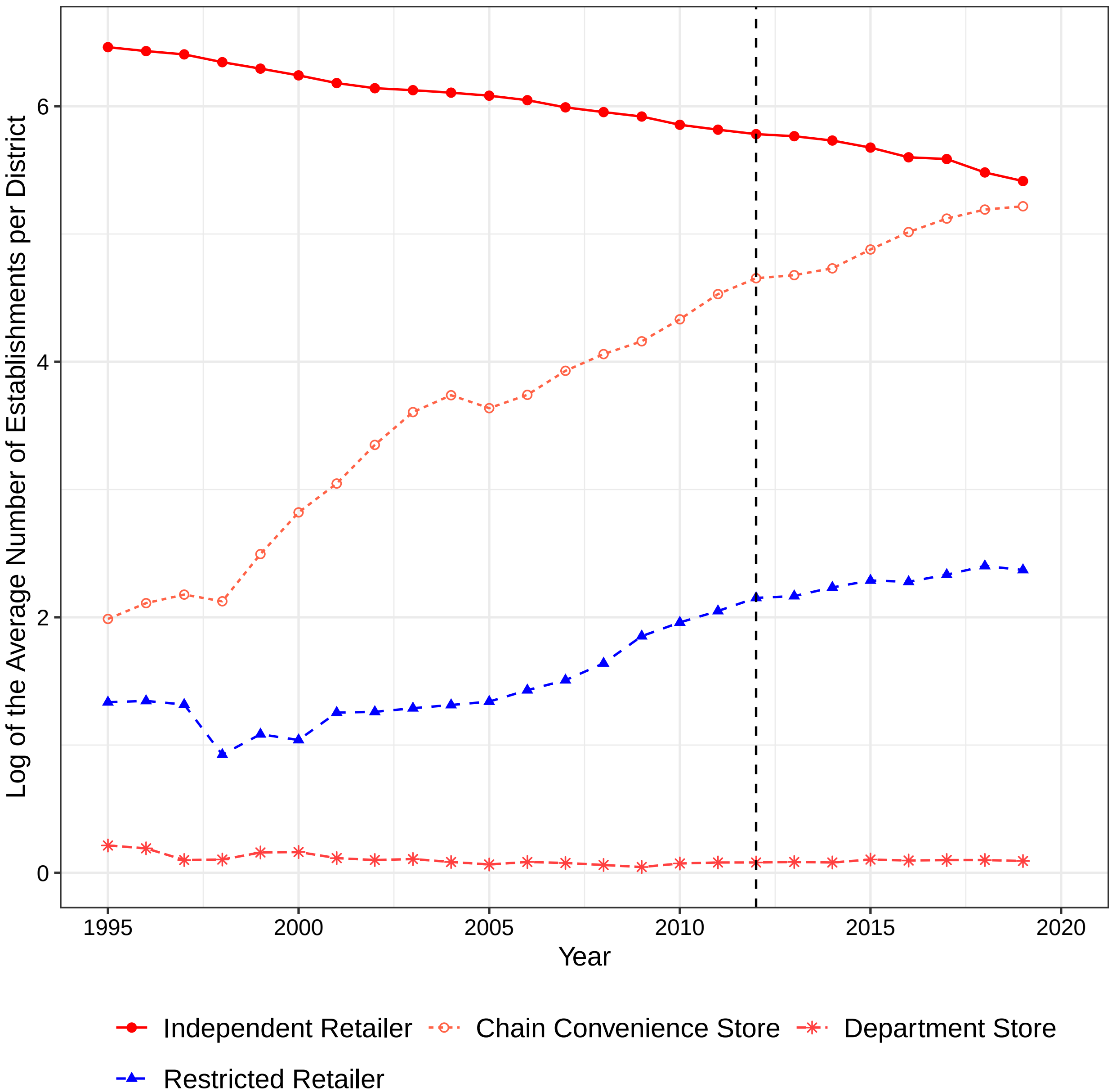
Note: This figure illustrates the definition of the Restricted (Treatment) and Unrestricted (Control) weeks used in the Difference-in-Differences (DD) analysis. The weeks are defined to start on Thursday to capture the full shopping cycle leading up to and following the Sunday closure. The second week of the month is used as the Restricted Week (containing the closure day), and the immediately following third week is used as the Unrestricted Week (no closure day), to minimize confounding effects from beginning/end-of-month pay cycles and discounts.

Figure 3. Unconditional Mean Differences in Daily District Total Card Expenditure Between Restricted and Unrestricted Weeks by Day of the Week



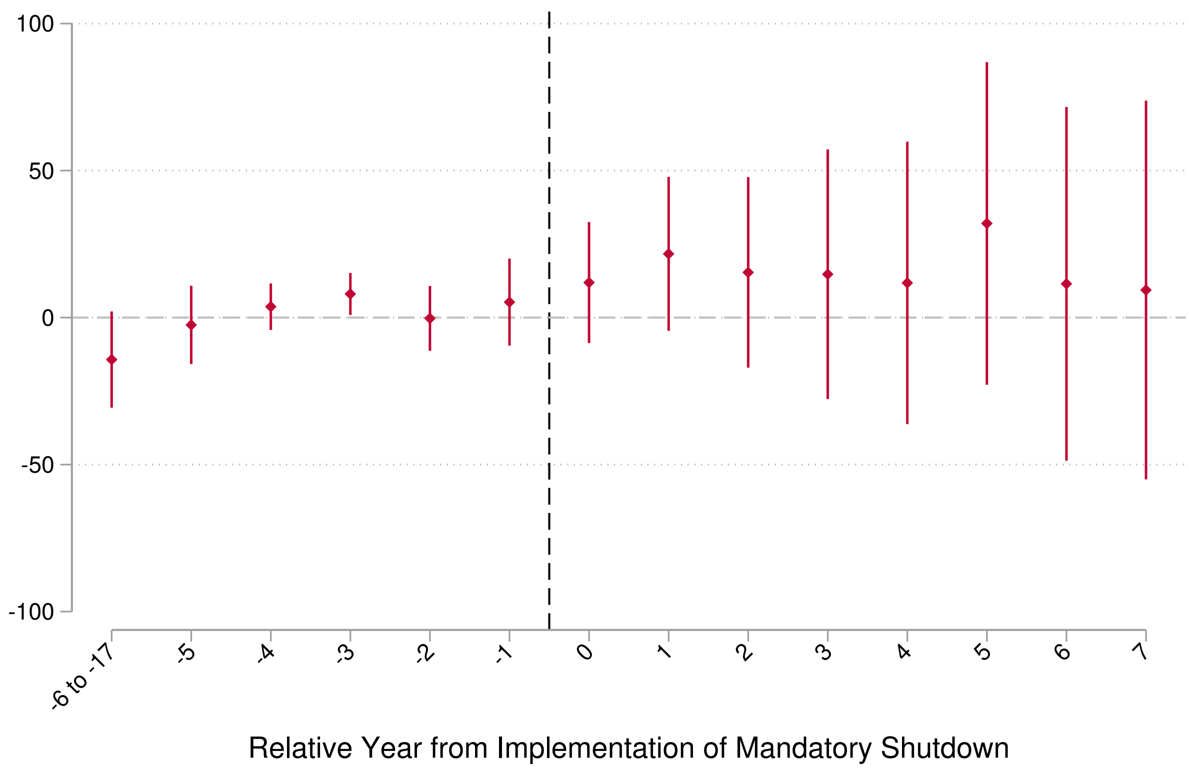
Note: This figure plots the unconditional mean difference in daily district-level card expenditure (Restricted Week minus Unrestricted Week) for the 54 Sunday-closure districts in 2019, showing raw substitution patterns. Panel A (Restricted Retailers) illustrates the expected sales drop on Sunday and temporal substitution to other days (e.g., higher sales on Saturday and Monday). Panel B (Unrestricted Retailers) shows evidence of across-retailer substitution on the restricted Sunday. Panel C (Restricted Retailers in Wednesday Closure Districts) shows raw evidence of across-district substitution on the restricted Sunday.

Figure 4. Trends in the Average Number of Establishments in Each District by Retailer Type



Note: This figure plots the time trend of the log of the unconditional average number of establishments per district by retailer type, using annual district-level panel data from the Korean Establishment Census for the period 1995 to 2019. It illustrates the long-term decline in independent retailers and the increase in large/small restricted retailers and chain convenience stores, spanning the introduction of the mandatory shutdown policy in 2012. The absence of a noticeable jump or change in the trend of independent retailers following 2012 suggests the policy did not immediately improve their prevalence.

Figure 5. Long-Run Effect on the Number of Independent Retailers



Note: This event-study figure presents the long-term dynamic effect of the mandatory Sunday shutdown policy on the prevalence of independent retailers (the policy's intended beneficiaries). The treatment group consists of high-exposure districts (those identified as having a persistent presence of restricted retailers) where the policy's effect is expected to be greatest. The control group consists of low-exposure districts (those identified as having no restricted retailers). The dependent variable is the residualized number of independent retailer establishments per district, where residuals are obtained by regressing the raw counts on a linear pre-trend using only pre-policy data. This residualization accounts for pre-existing, long-term trends in independent retailer decline. The estimation uses annual, district-level data from the Korean Establishment Census for the period 1995–2019. Relative year 0 marks the first year of policy implementation (2012). The figure plots the coefficients from the event-study regression for leads and lags, with lags before 2006 aggregated into a single coefficient. The insignificant and near-zero coefficients in the post-policy years indicate that the mandatory biweekly closure policy did not reverse the secular decline in independent retailer establishment counts, even in the districts most exposed to large chain competition.

Tables

Table 1— Summary Statistics of Daily District Expenditure for Restricted and Unrestricted Week by Retailer Type

	Daily District Expenditure (1M KRW)				
	Restricted Week		Unrestricted Week		
Retailer Type	Mean	SD	Mean	SD	Mean Diff.
Restricted Retailers	70	49	82	43	-12.16^{***}
Large Restricted	46	35	56	32	-9.17^{***}
Small Restricted	24	18	27	16	-3^{***}
Independent Retailers	107	55	104	52	3.16^{**}
Other Unrestricted Retailers	177	95	172	89	4.59^{*}

Note: This table reports mean and standard deviation of daily district-level card expenditure (in millions of KRW) for restricted and unrestricted weeks in 2019. The sample is drawn from the 54 Sunday-closure districts of the Seoul Metropolitan Area. Restricted week refers to the second week of the month (treatment), and unrestricted week refers to the third week of the month (control). The data excludes months with national holidays. Restricted retailers are large retail chains subject to the mandatory biweekly Sunday closure. The mean of "Restricted Retailers" is calculated over all district–date cells, with zero expenditure assigned to store formats not present in a given district. By contrast, the reported means for large and small restricted retailers are computed conditional on the presence of each format. Consequently, the aggregate mean is not expected to equal the sum of the component means. The same applies to "Other Unrestricted Retailers." The stars in the last column indicate statistical significance from a two-sample t-test: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table 2— Effects of the Mandatory Shutdown Policy on Retail Expenditures

	Dep var: Log Daily District Expenditure (KRW)						
	Temporal			Across Retailer		Across District	
	All Restricted Retailers	Large Restricted Retailers	Small Restricted Retailers	Independent Retailers	Other Unrestricted Retailers	Restricted Retailers in Wed District	Total
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
RWeek×Friday	0.04	0.05*	0.01	0.02	0.04	0.04	0.03
	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)	(0.06)	(0.02)
RWeek×Saturday	0.06**	0.07***	0.02	−0.01	0.05**	0.03	0.02
	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.07)	(0.02)
RWeek×Sunday	−18.52***	−18.18***	−17.16***	0.18***	0.13***	0.31***	0.15***
	(0.06)	(0.07)	(0.09)	(0.03)	(0.03)	(0.08)	(0.02)
RWeek×Monday	0.13***	0.15***	0.11***	−0.01	−0.02	0.07	−0.01
	(0.02)	(0.03)	(0.03)	(0.02)	(0.06)	(0.09)	(0.04)
RWeek×Tuesday	0.08***	0.09***	0.05*	0.01	0.02	0.06	0.02
	(0.03)	(0.03)	(0.03)	(0.01)	(0.03)	(0.09)	(0.02)
RWeek×Wednesday	0.09**	0.09**	0.07*	0.03	0.05	−5.77*	0.04
	(0.04)	(0.04)	(0.04)	(0.02)	(0.03)	(2.96)	(0.03)
RRetailer × *RWeek*×Friday							0.01
							(0.01)
RRetailer × *RWeek*×Saturday							0.03*
							(0.02)
RRetailer × *RWeek*×Sunday							−18.67***
							(0.06)
RRetailer × *RWeek*×Monday							0.14***
							(0.02)
RRetailer × *RWeek*×Tuesday							0.06***
							(0.02)
RRetailer × *RWeek*×Wednesday							0.04**
							(0.02)
District, Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep var of *UWeek* (1M KRW)	82.21	55.53	26.68	103.62	172.33	3.52	119.39
Mean N. stores per district	13.20	2.78	10.43	173.54	369.35	10.69	185.37
N. observation	5,292	5,292	5,292	5,292	5,292	5,292	15,876
Adjusted R^2	1.00	1.00	1.00	0.98	0.97	0.73	0.99

Note: This table reports the core Difference-in-Differences (DD) estimates of the impact of the mandatory Sunday closure policy on daily district-level bank card expenditure. The dependent variable is the log daily district expenditure for the retailer type indicated. The key coefficients measure the differential effect of the closure week on day of the week k compared to the excluded day (Thursday). $RWeek \times [Mon - Wed, Sat]$ coefficients capture temporal substitution within the restricted week (columns 1-3). $RWeek \times Sunday$ coefficients capture across-retailer substitution (columns 4-5). Column (6) captures across-district substitution by using expenditure at restricted retailers in Wednesday-closure districts as the dependent variable. All regressions include district fixed effects and month fixed effects. Standard errors (in parentheses) are clustered at the district-by-week level, and results are weighted by district population. *p<0.1; **p<0.05; ***p<0.01

Table 3— Quantifying Expenditure Substitution

	Baseline district expenditure (KRW, '000s) (1)	Estimated effect (%) (2)	Estimated effect (KRW, '000s) (3)	As a share of reduced sales at *RRetailer* (4)
Substitution towards unrestricted retailers				
Independent retailers (intended)	96,324	+19.2	18,538	17.5%
Other (unintended)	159,283	+13.6	21,637	20.4%
Substitution within restricted retailers				
Temporal	70,050	+37.2	26,058	24.6%
Across-district	2,855	+36.9	1,055	1.0%

Note: This table evaluates the magnitude of substitution patterns in terms of the lost sales at restricted retailers on a closure day. Column (1) reports baseline expenditure levels in thousand KRW, which is defined as the mean expenditure during Thursdays of the unrestricted weeks at the relevant retailer type. Column (2) shows percentage changes on closure Sundays, column (3) translates these into expenditure effects, and column (4) expresses them as a share of lost sales at restricted retailers. Lost sales are computed as average restricted retailer expenditure on unrestricted Sundays (KRW 105,979 thousand). "Independent retailers" capture substitution to intended beneficiaries, while "Other" include unintended beneficiaries such as department stores, convenience stores, agricultural cooperatives, exempted large chains, and general merchandise stores. "Temporal" substitution refers to within-restricted retailer shifts to other days of the week, and "Across-district" substitution refers to consumers shopping at restricted retailers in Wednesday closure districts on restricted Sundays.

Table 4— Quantifying Expenditure Substitution by Market Structure

	Baseline district expenditure (KRW, '000s) (1)	Estimated effect (%) (2)	Estimated effect (KRW, '000s) (3)	As a share of reduced sales at *RRetailer* (4)
*Panel A: Districts with **above** median independent retailer market share*				
Substitution towards unrestricted retailers				
Independent retailers (intended)	101,117	+17.3	17,543	19.6%
Other (unintended)	167,908	+13.4	22,444	25.1%
Substitution within restricted retailers				
Temporal	58,266	+39	22,752	25.5%
Across-district	2,916	+45.8	1,335	1.5%
*Panel B: Districts with **below** median independent retailer market share*				
Substitution towards unrestricted retailers				
Independent retailers (intended)	91,531	+21.1	19,335	15.8%
Other (unintended)	150,658	+13.8	20,785	16.9%
Substitution within restricted retailers				
Temporal	81,835	+35.4	28,981	23.6%
Across-district	3,939	+51.1	2,014	1.6%

Note: This table evaluates the magnitude of substitution patterns in terms of the lost sales at restricted retailers on a closure day as in Table 3, but split by district market structure. Market structure is classified using the ratio of daily average card sales at independent retailers to those at restricted retailers within a district during unrestricted weeks. Column (1) reports baseline expenditure levels in unrestricted weeks (in thousand KRW). Column (2) shows percentage changes on closure Sundays, column (3) translates these into expenditure effects, and column (4) expresses them as a share of lost sales at restricted retailers. Lost sales are computed as average restricted retailer expenditure on unrestricted Sundays, which amounts to KRW 89,319 thousand for Panel A and KRW 122,639 thousand for Panel B. "Independent retailers" capture substitution to intended beneficiaries, while "Other" include unintended beneficiaries such as department stores, convenience stores, agricultural cooperatives, exempted large chains, and general merchandise stores. "Temporal" substitution refers to within-restricted retailer shifts to other days of the week, and "Across-district" substitution refers to consumers shopping at restricted retailers in Wednesday closure districts.

Table 5— Quantifying Profit Substitution

	Estimated effect on sales (KRW, ’000s) (1)	Estimated profit margins (%) (2)	Estimated effect on profits (KRW, ’000s) (3)	As a share of reduced profits at *RRetailer* (4)
Substitution towards unrestricted retailers				
Independent retailers (intended)	18,538	4.6	851	34.1%
Other (unintended)	21,637	3.2	703	28.2%
Substitution within restricted retailers				
Temporal	26,058	2.4	614	24.6%
Across-district	1,055	2.4	25	1.0%

Note: This table translates the estimated daily changes in district-level expenditure from the DD analysis (Table 2) into corresponding changes in retailer profits (thousand KRW). The conversion uses median operating profit margins estimated from the 2019 Census of the Wholesale and Retail Industry. Column (1) reproduces estimates from column (3) of Table 3. Column (2) reports the profit margins used to convert expenditure changes into profit changes shown in column (3). Column (4) is calculated by dividing column (3) by the estimated reduction in profits at restricted retailers (*RRetailer*). This reduction equals the average expenditure at restricted retailers on unrestricted Sundays multiplied by their median profit margin, amounting to KRW 2,498 thousand. “Independent retailers” capture substitution toward intended beneficiaries, while “Other” include unintended beneficiaries such as department stores, convenience stores, agricultural cooperatives, exempted large chains, and general merchandise stores. “Temporal” substitution refers to within-district shifts to other days of the week, whereas “Across-district” substitution refers to consumers shopping in Wednesday-closure districts.

Table 6— Parameter Estimates and Implied Compensating Variation

	Baseline	**Extended I** Floor for Negative Diversion Shares			**Extended II** Floor for Negative Outside-option Shares		
		1%	0.5%	0.1%	1%	0.5%	0.1%
κ (in travel time)	38.93 [33.63, 49.18]	37.29 [31.15, 48.29]	37.44 [31.15, 48.83]	37.56 [31.77, 49.30]	41.44 [33.80, 52.89]	43.44 [34.48, 56.13]	49.40 [36.01, 66.34]
θ_{Sun}	5.97 [4.89, 7.81]	6.10 [4.96, 8.09]	6.12 [4.99, 8.24]	6.13 [5.00, 8.33]	6.93 [5.51, 8.94]	7.28 [5.63, 9.44]	8.33 [5.69, 11.05]
θ_{Temporal}	−0.54	−0.54	−0.54	−0.54	−0.42	−0.42	−0.42
$\theta_{\text{Independent}}$	−0.49	−0.50	−0.50	−0.50	−0.47	−0.47	−0.47
$\theta_{\text{Unintended}}$	−0.48	−0.49	−0.49	−0.49	−0.41	−0.41	−0.41
CV per Affected Consumer per Closure (USD)	0.78 [0.71, 0.85]	0.83 [0.74, 0.92]	0.83 [0.74, 0.91]	0.83 [0.74, 0.91]	0.84 [0.74, 0.93]	0.85 [0.75, 0.94]	0.88 [0.77, 0.97]
Total Monthly *CV* (USD, '000s)	772 [648, 907]	892 [744, 1048]	892 [758, 1030]	893 [752, 1041]	972 [817, 1137]	983 [819, 1163]	1,015 [830, 1194]
N. Districts	42	46	46	46	50	50	50

Note: This table reports parameter estimates and implied compensating variation (CV) from the discrete-choice model estimated on Sunday-closure districts in the Seoul Metropolitan Area. The model is estimated on three nested samples depending on the regularization required to ensure non-negativity of the expenditure amounts entering the likelihood. The Baseline sample includes only districts requiring no regularization. Extended I additionally incorporates districts with negative diversion amounts by replacing them with a small floor scaled to unrestricted Sunday sales at restricted retailers. For Extended II, we further include districts where the implied outside option remains negative after the Extended I adjustment, by imposing an analogous floor on the outside option and minimally increasing the counterfactual expenditure to preserve the accounting identity. Rows 1–2 report estimates of κ (the travel-time coefficient) and θ_{Sun} (the deterministic utility of Sunday shopping at restricted retailers). The coefficient κ is negative; for presentation we report absolute values. Rows 3–5 report the within-sample median of the district-specific deterministic utilities for each substitution channel, which are common across floor choices within a given sample. Row 6 reports the CV per affected consumer per closure Sunday, defined as the affected consumer-weighted average of district-level CVs. Row 7 reports the total monthly CV, which aggregates the district-level CV across all sample districts, scaled by two to reflect two closure Sundays per month, and adjusts for the card company's market share (24.2%). Square brackets report 95% confidence intervals from 1,000 bootstrap replications. All monetary values are expressed in PPP-adjusted USD.

Table 7— Effect on Monthly Total Welfare

	Estimate (USD)
Independent retailers (intended beneficiaries)	362,156
Other (unintended beneficiaries)	299,251
Changes in total profits at unrestricted retailers (A)	661,407
On the closure day	−1,062,863
Temporal substitution	261,338
Across-district substitution	10,576
Changes in total profits at restricted retailers (B)	−790,949
Changes in total consumer welfare (C)	−982,988
Overall change in welfare (A + B + C)	−1,112,530

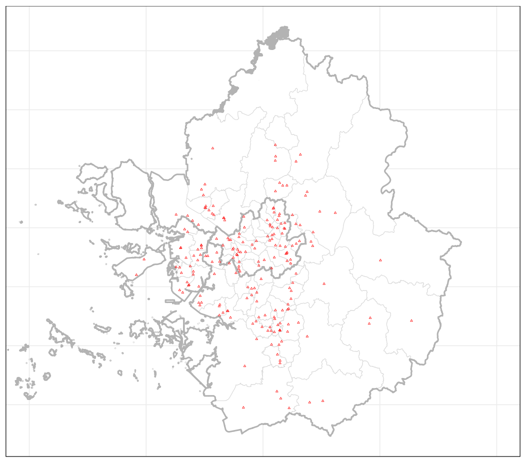
Note: This table aggregates monthly producer and consumer welfare effects of the mandatory Sunday shutdown in 50 Sunday-closure districts of the Seoul Metropolitan Area. Producer effects are based on expenditure-to-profit conversions using the 2019 Census of the Wholesale and Retail Industry profit margins. Consumer welfare losses are based on compensating variation estimates from the discrete-choice model. All values are expressed in PPP-adjusted USD. Net welfare change is the sum of producer and consumer effects.

Online Appendix

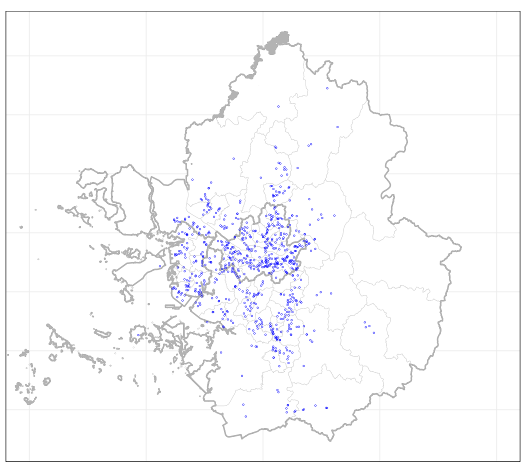
A1. *Additional Figures*

Figure A1. Geographic Distribution of Restricted Retailers

(a) Large Restricted Retailers

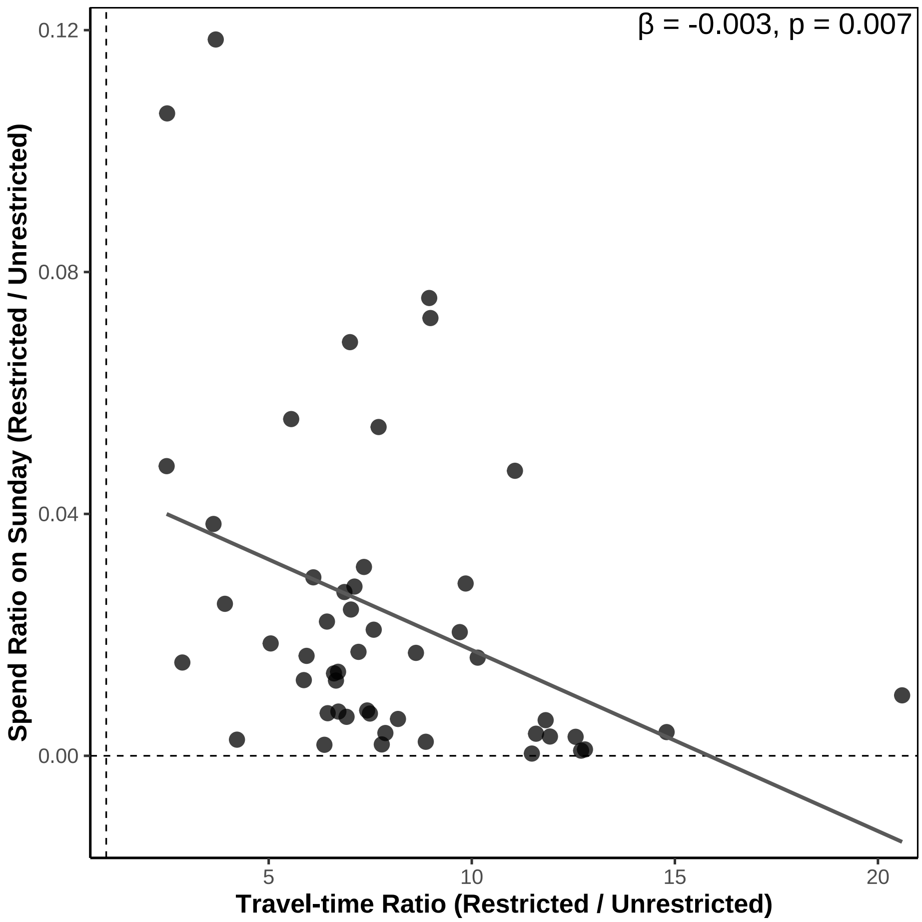


(b) Small Restricted Retailers



Note: This figure maps the geographic distribution of large and small restricted retailer store units across districts in the Seoul Metropolitan Area. Restricted retailers are those subject to the mandatory biweekly closure policy, defined as stores exceeding 3,000 m² (large) and conglomerate-owned stores between 165 m² and 3,000 m² (small). The distribution illustrates that, on average, a district contains approximately three large and ten small restricted retailers. Store counts and locations were hand-collected using Naver Map and Mart Monster (2020) and complemented with licensing data from the Korean Ministry of the Interior and Safety.

Figure A2. Cross-district Diversion and Travel-time Ratios



Note: Each dot represents a Sunday-closure district. The horizontal axis plots the ratio of travel time to the nearest open large restricted retailer on restricted vs. unrestricted Sundays. The vertical axis reports the share of baseline Sunday expenditure at restricted retailers in Sunday-closure districts that is diverted to restricted retailers in Wednesday-closure districts on restricted Sundays. The downward-sloping fitted line (β = –0.003, p = 0.007) indicates that cross-district substitution declines as the travel-time ratio increases.

A2. Additional Tables

Table A1— No Across Week Spillovers

	Dep var: Log Weekly District Expenditure (KRW)
	(1)
UWeek following *RWeek* = 1	0.017
	(0.027)
District FE	Yes
Mean dep var (1M KRW)	2,703
N. observation	378
Adjusted R^2	0.983

Note: This table presents the results of a test for the independence of consumer purchasing behavior across weeks. The dependent variable is the log weekly district expenditure (Thursday to Wednesday) across all retailer types. The key independent variable is an indicator for an unrestricted week (*UWeek*) that immediately follows a restricted week (*RWeek*) in five-week months. The reference category is an unrestricted week following another unrestricted week. A statistically significant positive coefficient would suggest consumers postpone their shopping by a full week. The estimation includes district fixed effects. Standard errors (in parentheses) are clustered at the district-by-week level, and results are weighted by district population. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table A2— Effects of the Mandatory Shutdown Policy on Retail Prices

	Dep var: Log Barcode Price (KRW)		
	Restricted Retailers	Independent Retailers	Other Unrestricted Retailers
	(1)	(2)	(3)
RWeek×Friday	−0.07	0.00	0.06*
	(0.05)	(0.02)	(0.03)
RWeek×Saturday	−0.05	0.00	0.02
	(0.04)	(0.02)	(0.03)
RWeek×Sunday		0.01	0.03
		(0.02)	(0.06)
RWeek×Monday	−0.00	0.02	0.08
	(0.03)	(0.01)	(0.05)
RWeek×Tuesday	−0.01	0.02	0.01
	(0.03)	(0.03)	(0.04)
RWeek×Wednesday	−0.02	0.05*	0.01
	(0.04)	(0.03)	(0.06)
District, Month FE	Yes	Yes	Yes
Detailed item category FE	Yes	Yes	Yes
Mean dep var of *UWeek* (KRW)	5,088	3,643	4,848
N. observation	30,521	42,546	12,254
Adjusted R^2	0.54	0.55	0.62

Note: This table presents estimates from a regression model (equation (2)) examining whether prices remain stable on and around the closure day. The dependent variable is the log price of each barcode. The estimation includes district and month fixed effects, as well as detailed item category fixed effects, so identification comes from within-category variation across barcodes. The statistically insignificant coefficients for the interaction terms support the assumption that observed expenditure changes mainly reflect changes in sales volume rather than price changes. The data are household-level purchases from the 2019 Agri-Food Consumer Panel Survey. Standard errors (presented in parentheses) are clustered at the district-by-week level. *p<0.1; **p<0.05; ***p<0.01

Table A3— Placebo Test

	Dep var: Log Daily District Expenditure (KRW)			
	Specialty Store	Concession Stand	Clothing/ Fashion	Fabric/ Bedding
	(1)	(2)	(3)	(4)
RWeek×Friday	0.06	0.04	0.06	0.20
	(0.05)	(0.36)	(0.04)	(0.17)
RWeek×Saturday	0.02	0.79^{***}	0.08	-0.22^{**}
	(0.04)	(0.25)	(0.06)	(0.09)
RWeek×Sunday	0.14	0.07	0.11	0.47
	(0.09)	(0.17)	(0.10)	(0.52)
RWeek×Monday	−0.06	0.31^{**}	0.02	−0.29
	(0.05)	(0.12)	(0.06)	(0.23)
RWeek×Tuesday	−0.01	0.33^{***}	0.02	−0.23
	(0.06)	(0.06)	(0.05)	(0.25)
RWeek×Wednesday	−0.02	0.35^{**}	0.03	0.03
	(0.07)	(0.15)	(0.07)	(0.29)
District, Month FE	Yes	Yes	Yes	Yes
Mean dep var of *UWeek* (1M KRW)	28.07	7.429	8.293	1.57
N. observation	5,194	4,410	5,292	5,194
Adjusted R^2	0.90	0.71	0.75	0.37

Note: This table reports the DD estimates of the impact of the mandatory Sunday closure policy on daily district-level bank card expenditure at less relevant retail categories. The dependent variable is the log daily district expenditure for the retailer type indicated. Specialty stores focus on specific product categories, including pet supply and golf equipment shops. Concession stands refer to small retail spaces located within institutions such as schools, hospitals, or workplaces, including informal or temporary vendors such as souvenir shops, food trucks, and pop-up stalls at public events. Clothing/Fashion stores sell apparel and fashion accessories. Textiles/Bedding stores specialize in fabrics, curtains, bedding sets, and other home textile goods. All regressions include district fixed effects and month fixed effects. Standard errors (in parentheses) are clustered at the district-by-week level, and results are weighted by district population. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table A4— Effects of the Mandatory Shutdown Policy on Online Retail Expenditures

	Dep var: Log Daily District Total Card Expenditure (KRW)		
	Grocery Online Expenditures at		
	Restricted Retailers	Unrestricted: Online Exclusive Retailers	Unrestricted: Other Retailers
	(1)	(2)	(3)
RWeek×Friday	0.04	0.06	0.05
	(0.23)	(0.22)	(0.15)
RWeek×Saturday	−0.66*	0.23	0.02
	(0.36)	(0.20)	(0.30)
RWeek×Sunday	−0.93**	0.46	−0.05
	(0.32)	(0.34)	(0.18)
RWeek×Monday	−0.03	0.08	0.05
	(0.26)	(0.20)	(0.13)
RWeek×Tuesday	−0.31	−0.05	−0.21
	(0.42)	(0.26)	(0.18)
RWeek×Wednesday	−0.20	0.38	−0.03
	(0.31)	(0.33)	(0.13)
District, Month FE	Yes	Yes	Yes
Mean dep var of *UWeek* (1M KRW)	0.68	1.45	22.03
N. observation	5,292	5,292	5,292
Adjusted R^2	0.53	0.63	0.99

Note: This table reports the DD estimates of the impact of the mandatory Sunday closure policy on daily district-level bank card expenditure at online grocery stores. The dependent variable is the log daily district expenditure for the retailer type indicated. The first column includes online stores operated by restricted retailers (e.g., Costco.com). Second are the unrestricted online stores that exclusively provide grocery delivery services (an example in the United States is Amazon Fresh). Third are other unrestricted platforms where consumers can purchase grocery-related items. For example, a consumer may buy a condiment from eBay, a general retailer, and this purchase is classified as grocery-related based on an internal algorithm used by the card payment processing company. Note that the p-value for the hypothesis test assessing spillovers from restricted weeks to unrestricted weeks using online expenditure as the dependent variable is 0.189. Therefore, using a DD approach is suitable for analyzing online card expenditure as well. All regressions include district fixed effects and month fixed effects. Standard errors (in parentheses) are clustered at the district-by-week level, and results are weighted by district population. *p<0.1; **p<0.05; ***p<0.01

Table A5— Effects of the Mandatory Shutdown Policy on Retail Transactions

	Dep var: Log Daily District Number of Transactions						
	Temporal			Across Retailer		Across District	
	All Restricted Retailers	Large Restricted Retailers	Small Restricted Retailers	Independent Retailers	Other Unrestricted Retailers	Restricted Retailers in Wed District	Total
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
RWeek×Friday	0.03	0.04	0.02	0.03	0.03	0.03	0.04
	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.05)	(0.02)
RWeek×Saturday	0.05***	0.07***	0.03**	0.01	0.04**	0.01	0.02
	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)	(0.05)	(0.02)
RWeek×Sunday	−8.17***	−7.59***	−7.26***	0.16***	0.11***	0.21***	−0.95***
	(0.06)	(0.07)	(0.08)	(0.03)	(0.03)	(0.06)	(0.04)
RWeek×Monday	0.09***	0.11***	0.08**	0.00	0.03	0.03	−0.01
	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.06)	(0.03)
RWeek×Tuesday	0.05**	0.07**	0.03	0.01	0.03	0.02	0.01
	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.06)	(0.02)
RWeek×Wednesday	0.07*	0.07*	0.06	0.04	0.05*	−1.48	0.03
	(0.03)	(0.04)	(0.03)	(0.02)	(0.02)	(0.93)	(0.03)
RRetailer × *RWeek*×Friday							−0.00
							(0.01)
RRetailer × *RWeek*×Saturday							0.03*
							(0.01)
RRetailer × *RWeek*×Sunday							−7.22***
							(0.06)
RRetailer × *RWeek*×Monday							0.10***
							(0.02)
RRetailer × *RWeek*×Tuesday							0.04**
							(0.02)
RRetailer × *RWeek*×Wednesday							0.04*
							(0.02)
District, Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep var of *UWeek*	3,105	1,601	1,504	7,158	16,114	109	4,663
Mean N. stores per district	13.20	2.78	10.43	173.54	369.35	10.69	112.70
N. observation	5,292	5,292	5,292	5,292	5,292	5,292	79,380
Adjusted R^2	1.00	0.99	0.99	0.99	0.99	0.79	0.08

Note: This table replicates Table 2 but with the dependent variable as the log daily district-level card transactions counts. Transactions are interpreted as store visits. All regressions include district fixed effects and month fixed effects. Standard errors (in parentheses) are clustered at the district-by-week level, and results are weighted by district population. *p<0.1; **p<0.05; ***p<0.01

Table A6— Summary Statistics of Daily District Expenditure for Sunday and Wednesday Closure Districts by Retailer Type

	Daily District Expenditure (1M KRW)				
	Sunday Closure		Wednesday Closure		
Retailer Type	Mean	SD	Mean	SD	Mean Diff.
Restricted Retailers	76	62	80	65	-3.39*
Large Restricted	56	46	55	45	0.24
Small Restricted	26	25	28	29	-1.57*
Independent Retailers	103	75	76	48	26.31***
Other Unrestricted Retailers	174	152	119	82	54.63***

Note: This table reports mean and standard deviations for daily district expenditure for Sunday and Wednesday closure districts by retailer type. The sample consists of expenditures made during second (restricted) weeks in Sunday and Wednesday closure districts. Expenditures on Wednesdays are dropped, and months with national holidays are excluded. Restricted retailers are those mandated to close on Sundays or Wednesdays (depending on the district) during restricted weeks. The mean of "Restricted Retailers" is calculated over all district–date cells, with zero expenditure assigned to store formats not present in a given district. By contrast, the reported means for large and small restricted retailers are computed conditional on the presence of each format. Consequently, the aggregate mean is not expected to equal the sum of the component means. The same applies to "Other Unrestricted Retailers." The stars in the last column indicate statistical significance from a two-sample t-test: *p<0.1; **p<0.05; ***p<0.01

Table A7— Effects of the Mandatory Shutdown Policy on Retail Expenditures Comparing Sunday and Wednesday Closure Districts

	Dep var: Log Daily District Expenditure (KRW)				
	Temporal			Across Retailer	
	All Restricted Retailers	Large Restricted Retailers	Small Restricted Retailers	Independent Retailers	Other Unrestricted Retailers
	(1)	(2)	(3)	(4)	(5)
SunClosure×Friday	0.10**	0.12**	0.08**	−0.01	0.01
	(0.03)	(0.03)	(0.03)	(0.01)	(0.01)
SunClosure×Saturday	0.14**	0.15**	0.07	−0.07*	−0.01
	(0.05)	(0.05)	(0.05)	(0.03)	(0.07)
SunClosure×Sunday	−18.59***	−18.29***	−17.22***	0.08**	0.04
	(0.10)	(0.13)	(0.11)	(0.03)	(0.08)
SunClosure×Monday	0.24***	0.24***	0.23***	0.03***	−0.03
	(0.04)	(0.04)	(0.03)	(0.01)	(0.04)
SunClosure×Tuesday	0.13**	0.13**	0.13**	0.01	0.00
	(0.04)	(0.04)	(0.05)	(0.00)	(0.01)
District, Month FE	Yes	Yes	Yes	Yes	Yes
Mean dep var of *WedClosure* (1M KRW)	78.952	54.709	27.662	76.456	119.943
Mean N. stores per district	12.629	3.016	10.147	161.316	348.678
N. observation	2,940	2,688	2,856	2,940	2,940
Adjusted R^2	1.00	1.00	1.00	0.98	0.95

Note: This table presents DD estimates comparing card expenditure between districts where restricted retailers close on Sunday (*SunClosure*) and districts where they close on Wednesday (*WedClosure*), using data from the second week of the month. The dependent variable is the log daily district expenditure for the indicated retailer type. The key interaction term, *SunClosure* × Day of the Week, captures the differential effect of a Sunday closure compared to a Wednesday closure. District and month fixed effects are included. Standard errors (presented in parentheses) are clustered at the district-by-week level. The results are weighted based on district population. *p<0.1; **p<0.05; ***p<0.01

Table A8— Effects of the Mandatory Shutdown Policy on Retail Expenditures Comparing Restricted Weeks of Sunday Closure Districts and Unrestricted Weeks of Wednesday Closure Districts

	Dep var: Log Daily District Expenditure (KRW)				
	Temporal			Across Retailer	
	All Restricted Retailers	Large Restricted Retailers	Small Restricted Retailers	Independent Retailers	Other Unrestricted Retailers
	(1)	(2)	(3)	(4)	(5)
SunClosure×Friday	0.08*	0.09**	0.06	0.01	0.04
	(0.04)	(0.04)	(0.04)	(0.02)	(0.03)
SunClosure×Saturday	0.10*	0.12**	0.02	−0.07**	0.03
	(0.05)	(0.05)	(0.05)	(0.03)	(0.07)
SunClosure×Sunday	−18.54***	−18.22***	−17.24***	0.13***	0.11
	(0.09)	(0.12)	(0.10)	(0.04)	(0.08)
SunClosure×Monday	0.18***	0.18***	0.19***	0.02	−0.05
	(0.04)	(0.05)	(0.04)	(0.02)	(0.05)
SunClosure×Tuesday	0.10*	0.10*	0.09*	0.04**	0.02
	(0.05)	(0.06)	(0.04)	(0.01)	(0.03)
District, Month FE	Yes	Yes	Yes	Yes	Yes
Mean dep var of *WedClosure* (1M KRW)	75.844	52.166	26.939	76.127	118.393
Mean N. stores per district	12.629	3.016	10.147	161.316	348.678
N. observation	2,940	2,688	2,856	2,940	2,940
Adjusted R^2	1.00	1.00	1.00	0.98	0.95

Note: This table compares substitution patterns between Sunday closure districts during restricted weeks and Wednesday closure districts during unrestricted weeks. The dependent variable is log daily district-level expenditure. Results show temporal substitution at restricted retailers and across-retailer substitution to independent retailers. District and month fixed effects are included. Standard errors (presented in parentheses) are clustered at the district-by-week level. The results are weighted based on district population. *p<0.1; **p<0.05; ***p<0.01

Table A9— Effects of the Mandatory Shutdown Policy on Retail Expenditures Using Triple Differences

	Dep var: Log Daily District Expenditure (KRW)				
	Temporal			Across Retailer	
	All Restricted Retailers	Large Restricted Retailers	Small Restricted Retailers	Independent Retailers	Other Unrestricted Retailers
	(1)	(2)	(3)	(4)	(5)
SunClosure × *RWeek* ×Friday	0.06*	0.07*	0.03	0.00	0.01
	(0.03)	(0.03)	(0.03)	(0.01)	(0.01)
SunClosure × *RWeek* ×Saturday	0.09**	0.09*	0.07**	−0.00	0.01
	(0.04)	(0.05)	(0.03)	(0.01)	(0.01)
SunClosure × *RWeek* ×Sunday	−18.50***	−18.21***	−17.09***	0.13***	0.06***
	(0.10)	(0.12)	(0.10)	(0.01)	(0.01)
SunClosure × *RWeek* ×Monday	0.18***	0.20***	0.15***	0.00	0.02
	(0.04)	(0.05)	(0.04)	(0.01)	(0.03)
SunClosure × *RWeek* ×Tuesday	0.11*	0.12*	0.08	−0.02**	−0.00
	(0.05)	(0.06)	(0.05)	(0.01)	(0.01)
District, Month FE	Yes	Yes	Yes	Yes	Yes
Mean dep var of *WedClosure* (1M KRW)	86.566	63.797	29.068	103.419	176.092
Mean N. stores per district	12.629	3.016	10.147	161.316	348.678
N. observation	5,880	5,376	5,712	5,880	5,880
Adjusted R^2	1.00	1.00	1.00	0.98	0.95

Note: This table reports results from a triple-differences specification exploiting variation across closure-day districts (Sunday vs. Wednesday), restricted vs. unrestricted weeks, and days of the week. The dependent variable is log daily district-level expenditure. District and month fixed effects are included. Standard errors (presented in parentheses) are clustered at the district-by-week level. The results are weighted based on district population. *p<0.1; **p<0.05; ***p<0.01

A3. Distribution of Expenditure

To assess the distributional effects of the mandatory supermarket closure policy, we compare household expenditure levels between business owners of independent retailers and other households. This analysis helps us evaluate the equity implications of the policy. If independent retailers are more likely to come from lower-income households, then each additional dollar of profit gains may imply a greater welfare gain, due to their potentially higher marginal utility of income for these households.

We use data from the 2019 Household Income and Expenditure Survey provided by KOSIS. This nationally representative survey offers detailed information on household-level income and expenditure and serves as the primary data source for constructing the weights needed for the official Consumer Price Index in Korea. The dataset includes categorical information on monthly household income and continuous measures of household expenditure. It also provides individual-level characteristics for up to nine household members, including class of worker (e.g., wage-employed, self-employed) and industry code, allowing us to identify individuals who are self-employed in the retail sector. For self-employed individuals, the survey additionally indicates whether they have employees.

We focus on monthly household expenditure as our outcome variable, as it serves as a reliable proxy for income and is measured continuously, making it more suitable for analysis. The data do not include geographic information such as province or district, so we are unable to restrict the analysis to households in the Seoul Metropolitan Area.

We define households of independent retailers as those with at least one member who is self-employed without employees in the retail sector. Out of 8,631 surveyed households, 20.8% (1,796 households) include at least one self-employed member. While this is a household-level measure and therefore not directly comparable to individual-level statistics, it is of similar magnitude to national figures: the 2019 Population and Housing Census reports that approximately 25% of the working population was self-employed. In our data, 443 households have a self-employed member in retail, and 375 of these are solo self-employed, operating without employees.

To construct appropriate comparison groups for these solo self-employed retailers, we use three approaches. The first is to compare them to all other households. The second is to compare solo self-employed households in retail to those in other sectors. The last is to compare them to other classes of workers in the retail sector, such as wage workers or self-employed households with employees.

We estimate a log-linear model in which the dependent variable is the log of monthly household expenditure in KRW. The key independent variable is a binary indicator equal to 1 if the household is a solo self-employed retailer and 0 if it belongs to the relevant comparison group. The baseline estimates are presented in columns (1), (4), and (7) of Table A10, corresponding to the three different comparison strategies discussed earlier. In subsequent columns, we add control variables that may influence household expenditure levels. These include the number of household members, the presence of a household member aged 65 or older, whether the household spans multiple generations (e.g., single-generation, two-generation, or three-or-more-generation households), and whether the household resides in an urban area. In the final specification for each comparison group, we additionally apply sample weights provided in the survey to account for the survey's sampling design.

Across all specifications — except for column (1), which does not include control variables — we find that households with solo self-employed members in the retail sector have significantly lower expenditure levels, by approximately 7.7% to 16.5%, compared to the relevant comparison groups. This suggests that independent retailers are more likely to come from households with lower levels of economic resources, as proxied by expenditure.

While the mandatory closure policy shows only a limited redirection of sales from large retail chains to independent retailers (17.5%), and was not effective in promoting the entry or long-term survival of independent retailers, nor did it generate large monthly profit gains (estimated at just 41.7 USD), the policy's beneficiaries might still warrant policy attention. Given that many independent retailers come from lower-expenditure households, even modest income gains from increased local demand may have disproportionately positive welfare effects. This highlights the potential equity rationale for supporting small independent retailers.

Table A10— Differential Household Expenditure: Self-Employed in Retail

	Dep var: Log Monthly Household Expenditure (KRW)								
	Control group:								
	All other HH			Solo SE HH			Retail HH		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Solo SE in Retail HH	0.12***	−0.12***	−0.17***	−0.08**	−0.09***	−0.13***	−0.17***	−0.14***	−0.18***
	(0.04)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)
N. HH members		0.29***	0.28***		0.22***	0.23***		0.24***	0.26***
		(0.01)	(0.01)		(0.02)	(0.02)		(0.02)	(0.02)
HH has family member aged 65+		−0.62***	−0.61***		−0.45***	−0.43***		−0.60***	−0.63***
		(0.02)	(0.02)		(0.05)	(0.05)		(0.07)	(0.08)
Generations: Two		−0.08***	−0.07***		−0.01	−0.06		−0.08	−0.10*
		(0.02)	(0.02)		(0.04)	(0.04)		(0.05)	(0.05)
Generations: Three+		−0.35***	−0.31***		−0.22**	−0.23***		−0.35***	−0.35***
		(0.05)	(0.04)		(0.09)	(0.09)		(0.10)	(0.10)
Urban		−0.09***	−0.08***		−0.09**	−0.07**		−0.04	−0.01
		(0.02)	(0.02)		(0.03)	(0.03)		(0.05)	(0.04)
Constant	14.69***	14.14***	14.20***	14.90***	14.29***	14.30***	14.99***	14.29***	14.29***
	(0.01)	(0.02)	(0.02)	(0.02)	(0.04)	(0.04)	(0.02)	(0.04)	(0.04)
Sample Weights	No	No	Yes	No	No	Yes	No	No	Yes
N. observation	8,631	8,631	8,631	1,536	1,536	1,536	1,114	1,114	1,114
Adjusted R^2	0.00	0.46	0.45	0.00	0.33	0.35	0.01	0.37	0.36

Note: This table reports estimates from log-linear regressions of monthly household expenditure (in KRW) on an indicator for whether a household includes a solo self-employed individual in the retail sector, which serves as a proxy for ownership of an independent retail business. The dependent variable is the log of total monthly household expenditure. "Solo self-employed retailers" are households with at least one member who is self-employed without employees in the retail sector, as identified in the 2019 Household Income and Expenditure Survey (KOSIS). Columns (1), (4), and (7) compare these households respectively to (i) all other households, (ii) solo self-employed households in non-retail sectors, and (iii) other types of workers in the retail sector (e.g., wage workers, self-employed with employees). Subsequent columns sequentially add household-level controls, including the number of household members, presence of an elderly member (age 65+), household generation structure, and urban residence. Final specifications additionally apply sampling weights provided in the survey. Standard errors are shown in parentheses. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

A4. Substitution Patterns by Detailed Retailer Type

Table A11— Effects of the Mandatory Shutdown Policy on Retail Expenditures by Detailed Retailer Type

	Dep var: Log Daily District Expenditure (KRW)							
	Large Restricted	Small Restricted	Independent Retailers	Convenience Chain Stores	Department Stores	Agricultural Co-op	Exempted Large Chains	General Merchandise Stores
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RWeek×Friday	0.05*	0.01	0.02	0.03	0.04	0.01	−0.03	0.07
	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.39)	(0.05)
RWeek×Saturday	0.07***	0.02	−0.01	0.05	0.05**	0.01	−0.07	0.05
	(0.02)	(0.02)	(0.01)	(0.03)	(0.02)	(0.02)	(0.24)	(0.04)
RWeek×Sunday	−18.18***	−17.16***	0.18***	0.12***	0.12***	0.21***	0.88*	0.15
	(0.07)	(0.09)	(0.03)	(0.04)	(0.03)	(0.04)	(0.43)	(0.09)
RWeek×Monday	0.15***	0.11***	−0.01	0.05	−0.28	−0.03	0.10	−0.01
	(0.03)	(0.03)	(0.02)	(0.05)	(0.19)	(0.04)	(0.36)	(0.05)
RWeek×Tuesday	0.09***	0.05*	0.01	0.02	0.03	0.00	0.19	−0.02
	(0.03)	(0.03)	(0.01)	(0.04)	(0.04)	(0.03)	(0.28)	(0.05)
RWeek×Wednesday	0.09**	0.07*	0.03	0.04	0.08	−0.02	−0.08	0.02
	(0.04)	(0.04)	(0.02)	(0.03)	(0.05)	(0.04)	(0.47)	(0.04)
District, Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep var of *UWeek* (1M KRW)	55.531	26.679	103.622	88.521	63.743	14.366	0.22	5.483
Mean N. stores per district	2.80	10.40	173.50	327.60	1.70	6.70	0.10	33.60
N. observation	5,292	5,292	5,292	5,292	5,292	5,292	5,292	5,292
Adjusted R^2	1.00	1.00	0.98	0.99	0.88	0.93	0.64	0.77

Note: This table reports the core DD estimates of the impact of the mandatory Sunday closure policy on daily district-level bank card expenditure by each detailed category.. The dependent variable is the log daily district expenditure for the retailer type indicated. The key coefficients measure the differential effect of the closure week on day of the week k compared to the excluded day (Thursday). $RWeek \times [Mon - Wed, Sat]$ coefficients capture temporal substitution within the restricted week (columns 1-2). $RWeek \times Sunday$ coefficients capture across-retailer substitution (columns 3-8). All regressions include district fixed effects and month fixed effects. Standard errors (in parentheses) are clustered at the district-by-week level, and results are weighted by district population. *p<0.1; **p<0.05; ***p<0.01

A5. Estimation of Consumer Welfare Changes

In this section, we provide a formal derivation and estimation of the consumer welfare losses reported in section V. We derive the relevant conditional probabilities implied by the multinomial logit model shown in equation (5), construct the likelihood function, and describe the compensating variation (CV) calculations used to measure aggregate consumer welfare changes.

Derivation of Conditional Probabilities

Let's derive conditional probabilities shown in equations (9) and (10).

Let Sun indicate the option to choose restricted retailers on Sundays. The conditional probability that a representative consumer in district j continues to shop at restricted retailers on a restricted Sunday — given that the same consumer would have done so on an unrestricted Sunday — is as follows:

$$
\begin{aligned}
\text{(A1)} \qquad & \Pr_j(\text{Sun}^R \,|\, \text{Sun}^U) \\
&= \Pr_j\big(\text{Select Sun with the shutdown} \;\big|\; \text{Select Sun without the shutdown}\big) \\
&= \frac{\Pr_j(\{\text{Select Sun with the shutdown}\} \cap \{\text{Select Sun without the shutdown}\})}{\Pr_j(\{\text{Select Sun without the shutdown}\})}
\end{aligned}
$$

Because any consumer who shops at a restricted retailer when it is closed would have chosen that same shopping mode in the absence of the restriction, the numerator can be simplified. So, $\Pr_j(\text{Sun}^R \,|\, \text{Sun}^U)$ can be rewritten as:

$$
\text{(A2)} \qquad \Pr_j(\text{Sun}^R \,|\, \text{Sun}^U) = \frac{\Pr_j(\{\text{Select Sun with the shutdown}\})}{\Pr_j(\{\text{Select Sun without the shutdown}\})}
$$

Under the multinomial logit error structure with type-1 extreme-value errors ε and deterministic utilities θ, the probability of choosing restricted retailers on a restricted Sunday (the numerator of equation (A2)) is:

(A3) $\Pr_j(\text{Select Sun with the shutdown})$

$$= \Pr_j \begin{pmatrix} \theta_{\text{Sun}} + \kappa \cdot TravelTime_j^R + \varepsilon_{j,\text{Sun}} > \theta_{j,\text{Tem}} + \varepsilon_{j,\text{Tem}}, \\ \theta_{\text{Sun}} + \kappa \cdot TravelTime_j^R + \varepsilon_{j,\text{Sun}} > \theta_{j,\text{Ind}} + \varepsilon_{j,\text{Ind}}, \\ \theta_{\text{Sun}} + \kappa \cdot TravelTime_j^R + \varepsilon_{j,\text{Sun}} > \theta_{j,\text{Uni}} + \varepsilon_{j,\text{Uni}}, \\ \theta_{\text{Sun}} + \kappa \cdot TravelTime_j^R + \varepsilon_{j,\text{Sun}} > \varepsilon_{j,\text{Out}} \end{pmatrix}$$

$$= \frac{exp(\theta_{\text{Sun}} + \kappa \cdot TravelTime_j^R)}{exp(\theta_{\text{Sun}} + \kappa \cdot TravelTime_j^R) + exp(\theta_{j,\text{Tem}}) + exp(\theta_{j,\text{Ind}}) + exp(\theta_{j,\text{Uni}}) + 1}$$

where **Tem** indicates temporal substitution, **Ind** represents substitution towards independent retailers, **Uni** means shopping at unintended-beneficiary retailers, and **Out** represents the outside option. The deterministic utilities θ's for $\{\text{Tem, Ind, Uni, Out}\}$ are indexed by j to capture district-specific heterogeneity in substitution patterns.

Similarly, the denominator of equation (A2) can be reformulated as follows:

(A4) $\Pr_j(\text{Select Sun without the shutdown})$

$$= \frac{exp(\theta_{\text{Sun}} + \kappa \cdot TravelTime_j^U)}{exp(\theta_{\text{Sun}} + \kappa \cdot TravelTime_j^U) + exp(\theta_{j,\text{Tem}}) + exp(\theta_{j,\text{Ind}}) + exp(\theta_{j,\text{Uni}}) + 1}$$

Dividing equation (A3) by equation (A4), $\Pr_j(\text{Sun}^R \,|\, \text{Sun}^U)$ in equation(A2) can be reformulated as follows:

(A5) $\Pr_j(\text{Sun}^R \,|\, \text{Sun}^U)$

$$= \frac{exp(\theta_{\text{Sun}} + \kappa \cdot TravelTime_j^R)}{exp(\theta_{\text{Sun}} + \kappa \cdot TravelTime_j^R) + exp(\theta_{j,\text{Tem}}) + exp(\theta_{j,\text{Ind}}) + exp(\theta_{j,\text{Uni}}) + 1} \Bigg/ \frac{exp(\theta_{\text{Sun}} + \kappa \cdot TravelTime_j^U)}{exp(\theta_{\text{Sun}} + \kappa \cdot TravelTime_j^U) + exp(\theta_{j,\text{Tem}}) + exp(\theta_{j,\text{Ind}}) + exp(\theta_{j,\text{Uni}}) + 1}$$

$$= \exp\left[\kappa\,(TravelTime_j^R - TravelTime_j^U)\right] \cdot \frac{\exp\left(\theta_{\text{Sun}} + \kappa\, TravelTime_j^U\right) + \sum_{k \neq \text{Sun}} \exp\theta_{jk}}{\exp\left(\theta_{\text{Sun}} + \kappa\, TravelTime_j^R\right) + \sum_{k \neq \text{Sun}} \exp\theta_{jk}}$$

The last line of the above equations represents the last line of equation (9).

Similarly, the conditional probability that a representative consumer who diverted from restricted retailers due to the shutdown to an alternative shopping mode $c \in \{\text{Tem, Ind, Uni, Out}\}$ is:

$$\text{(A6)} \qquad \Pr_j(c \,|\, \text{Sun}^U) = \frac{\Pr_j(\{\text{Select } c \text{ with the shutdown}\} \cap \{\text{Select Sun without the shutdown}\})}{\Pr_j(\{\text{Select Sun without the shutdown}\})}$$

The denominator is the same as equation (A4).

In our counterfactual comparison we hold the taste shocks ε_{ijc} fixed across environments (U,R), and the shutdown affects only the deterministic utility of the Sunday-restricted-retailer option *Sun* through travel time, while V_{jc} for $c \neq Sun$ is unchanged. Since the shutdown weakly lowers the utility of *Sun*, no consumer can switch away from any $c \neq Sun$ when moving from U to R. Hence the set of types choosing *Tem* under U is a subset of those choosing *Tem* under R, implying

$$\Pr_j(Tem^R \cap Sun^U) \;=\; \Pr_j(Tem^R) - \Pr_j(Tem^U),$$

which justifies the first equality in (A7).

Using the temporal substitution channel (Tem) as an example, the numerator of the equation (A6) can be written as:

$$
\begin{aligned}
\text{(A7)}\qquad & \Pr_j(\{\text{Select Tem with the shutdown}\} \cap \{\text{Select Sun without the shutdown}\}) \\
&= \Pr_j(\{\text{Select Tem when } TravelTime_j = TravelTime_j^R\}) - \\
&\quad \Pr_j(\{\text{Select Tem when } TravelTime_j = TravelTime_j^U\}) \\
&= \frac{exp(\theta_{j,\text{Tem}})}{exp(\theta_{\text{Sun}} + \kappa \cdot TravelTime_j^R) + exp(\theta_{j,\text{Tem}}) + exp(\theta_{j,\text{Ind}}) + exp(\theta_{j,\text{Uni}}) + 1} - \\
&\quad \frac{exp(\theta_{j,\text{Tem}})}{exp(\theta_{\text{Sun}} + \kappa \cdot TravelTime_j^U) + exp(\theta_{j,\text{Tem}}) + exp(\theta_{j,\text{Ind}}) + exp(\theta_{j,\text{Uni}}) + 1}
\end{aligned}
$$

Here, for the ease of notation, we define:

$$
\begin{aligned}
\text{(A8)}\qquad & S^U := exp(\theta_{\text{Sun}} + \kappa \cdot TravelTime_j^U) + exp(\theta_{j,\text{Tem}}) + exp(\theta_{j,\text{Ind}}) + exp(\theta_{j,\text{Uni}}) + 1 \\
& S^R := exp(\theta_{\text{Sun}} + \kappa \cdot TravelTime_j^R) + exp(\theta_{j,\text{Tem}}) + exp(\theta_{j,\text{Ind}}) + exp(\theta_{j,\text{Uni}}) + 1
\end{aligned}
$$

Then equation (A7) can be simplified as follows:

$$
\begin{aligned}
\text{(A9)}\qquad & \frac{exp(\theta_{j,\text{Tem}})}{S^R} - \frac{exp(\theta_{j,\text{Tem}})}{S^U} \\
&= exp(\theta_{j,\text{Tem}}) \frac{exp(\theta_{\text{Sun}} + \kappa \cdot TravelTime_j^U) - exp(\theta_{\text{Sun}} + \kappa \cdot TravelTime_j^R)}{S^R \cdot S^U}
\end{aligned}
$$

To derive equation (A6), let's divide the simplified numerator, equation (A9), by the denominator, equation (A4):

(A10)

$$exp(\theta_{j,\text{Tem}})\frac{exp(\theta_{\text{Sun}})[exp(\kappa\cdot TravelTime_j^U)-exp(\kappa\cdot TravelTime_j^R)]}{S^R\cdot S^U}\Big/\frac{exp(\theta_{\text{Sun}}+\kappa\cdot TravelTime_j^U)}{S^U}$$
$$= exp(\theta_{j,\text{Tem}})\cdot\frac{(1-\exp\big(\kappa TravelTime_j^R\ -\ \kappa TravelTime_j^U\big))}{S^R}$$
$$= \exp(\theta_{j,\text{Tem}})\cdot\frac{(1-\exp\big(\kappa TravelTime_j^R\ -\ \kappa TravelTime_j^U\big))}{\exp\big(\theta_{\text{Sun}}+\kappa TravelTime_j^R\big)+\sum_{k\neq\text{Sun}}\exp\theta_{jk}}$$

where $\sum_{k\neq\text{Sun}}\exp\theta_{jk}$ includes $\theta_{j,\text{Out}}=1$. Similarly, the conditional probabilities for the other choice options {Ind, Uni, Out} can be formulated as follows:

(A11) $$\Pr_j(\text{Ind}\,|\,\text{Sun}^U)=exp(\theta_{j,\text{Ind}})\cdot\frac{(1-exp(\kappa\cdot TravelTime_j^R-\kappa\cdot TravelTime_j^U))}{\exp\big(\theta_{\text{Sun}}+\kappa TravelTime_j^R\big)+\sum_{k\neq\text{Sun}}\exp\theta_{jk}}$$

(A12) $$\Pr_j(\text{Uni}\,|\,\text{Sun}^U)=exp(\theta_{j,\text{Uni}})\cdot\frac{(1-exp(\kappa\cdot TravelTime_j^R-\kappa\cdot TravelTime_j^U))}{\exp\big(\theta_{\text{Sun}}+\kappa TravelTime_j^R\big)+\sum_{k\neq\text{Sun}}\exp\theta_{jk}}$$

(A13) $$\Pr_j(\text{Out}\,|\,\text{Sun}^U)=\frac{(1-exp(\kappa\cdot TravelTime_j^R-\kappa\cdot TravelTime_j^U))}{\exp\big(\theta_{\text{Sun}}+\kappa TravelTime_j^R\big)+\sum_{k\neq\text{Sun}}\exp\theta_{jk}}$$

Now we can recover θ_{jc}'s dividing equations (A10), (A11), and (A12) by equation (A13).

(A14) $$\frac{\Pr_j(\text{Tem}\mid\text{Sun}^U)}{\Pr_j(\text{Out}\mid\text{Sun}^U)}=\exp(\theta_{j,\text{Tem}}),$$
$$\frac{\Pr_j(\text{Ind}\mid\text{Sun}^U)}{\Pr_j(\text{Out}\mid\text{Sun}^U)}=\exp(\theta_{j,\text{Ind}}),$$
$$\frac{\Pr_j(\text{Uni}\mid\text{Sun}^U)}{\Pr_j(\text{Out}\mid\text{Sun}^U)}=\exp(\theta_{j,\text{Uni}}).$$

$$\text{(A15)} \qquad \theta_{j,\text{Tem}} = \ln\Pr_j(\text{Tem} \mid \text{Sun}^U) - \ln\Pr_j(\text{Out} \mid \text{Sun}^U),$$
$$\theta_{j,\text{Ind}} = \ln\Pr_j(\text{Ind} \mid \text{Sun}^U) - \ln\Pr_j(\text{Out} \mid \text{Sun}^U),$$
$$\theta_{j,\text{Uni}} = \ln\Pr_j(\text{Uni} \mid \text{Sun}^U) - \ln\Pr_j(\text{Out} \mid \text{Sun}^U).$$

Likelihood Function

Our estimation is based on the amount of expenditure rather than the number of visits. This approach aligns with our theoretical framework, in which each consumer allocates an infinitesimal continuum of expenditure units across available shopping modes. Accordingly, each expenditure amount can be interpreted as a probabilistic choice share, reflecting the proportion of total expenditure directed to a particular shopping mode. This specification allows us to capture substitution patterns in spending behavior (not just visit frequency) and to weight the likelihood function by the magnitude of observed expenditures across substitution channels.

Let $Amt_{j,\text{Sun}}$ denote the change in card expenditure on shopping mode Sun for consumers residing in district j between unrestricted and restricted weeks. Because consumers have to travel to non-Sunday-closure districts to continue using shopping mode Sun during restricted weeks, $Amt_{j,\text{Sun}}$ is defined as the expenditure that remains in the Sun mode via across-district substitution under the mandatory shutdown.

For $c \in \{\text{Tem, Ind, Uni}\}$, $Amt_{j,c}$ denotes the amount of expenditure diverted to shopping mode c when restricted retailers are closed on restricted Sundays. These are defined as in the empirical analysis (section IV).

$Amt_{j,\text{Tem}}$ captures the intertemporal substitution within restricted retailers located in Sunday-closure districts. It is constructed as the sum of the differences in expenditure at these retailers between restricted and unrestricted weeks, aggregated over Saturday, Monday, Tuesday, and Wednesday. Similarly, $Amt_{j,\text{Ind}}$ captures substitution from restricted retailers to independent retailers within Sunday-closure districts. It is constructed as the difference in expenditure by district-j residents at independent retailers in these districts on the restricted Sunday relative to the unrestricted Sunday. $Amt_{j,\text{Uni}}$ is defined in the same way as $Amt_{j,\text{Ind}}$, with expenditures measured at unintended-beneficiary retailers instead of independent retailers.

$Amt_{j,\text{Out}}$ is defined residually as the portion of expenditure that would have occurred at restricted retailers on Sundays in Sunday-closure districts but is not reallocated to other shopping modes during the restriction. The counterfactual level of this expenditure is proxied by spending at restricted retailers on unrestricted Sundays in Sunday-closure districts and is denoted by Amt_j^{CF}. Formally,

$$Amt_{j,\text{Out}} := Amt_j^{CF} - Amt_{j,\text{Sun}} - Amt_{j,\text{Tem}} - Amt_{j,\text{Ind}} - Amt_{j,\text{Uni}}$$

We now define the likelihood function and the corresponding log-likelihood function as follows:

$$\text{(A16)} \qquad \mathscr{L}(\Theta) = \prod_{j=1}^{J} \left[\Pr_j\big(\text{Sun}^{\text{R}} \mid \text{Sun}^{\text{U}}\big)^{Amt_{j,\text{Sun}}} \times \prod_{c \neq \text{Sun}} \Pr_j\big(c \mid \text{Sun}^{\text{U}}\big)^{Amt_{j,c}} \right]$$

$$\text{(A17)} \qquad \ell(\Theta) = \sum_{j=1}^{J} \left[Amt_{j,\text{Sun}} \cdot \ln \Pr_j(\text{Sun}^{\text{R}} \mid \text{Sun}^{\text{U}}) + \sum_{c \neq \text{Sun}} Amt_{j,c} \cdot \ln \Pr_j(c \mid \text{Sun}^{\text{U}}) \right]$$

Sample construction and non-negativity regularization

We begin with the 54 Seoul Metropolitan districts subject to Sunday closures and exclude four island/low-traffic jurisdictions (Ongjin-gun [Incheon], Yangpyeong-gun [Gyeonggi], Jung-gu [Incheon], and Dong-gu [Incheon]).[16] The resulting working sample contains 50 districts.

The expenditure amounts $\{Amt_{j,c}\}$ enter the likelihood as weights and map into conditional choice shares; hence they should be nonnegative. A small number of districts nonetheless exhibit $Amt_{j,c} < 0$ for some diversion channel $c \in \{\text{Sun}, \text{Tem}, \text{Ind}, \text{Uni}\}$ and/or a negative residual outside option due to sampling noise or week-to-week volatility. We therefore apply a small, data-scaled floor and report results for three nested samples, $S0 \subset S1 \subset S2$.

[16]Across the seven analysis months (January, March, April, May, July, November, and December), total visits on third Sundays never exceed 2,500 in these jurisdictions; the next-lowest district records roughly 5,000.

(i) S0 (Baseline; $N = 42$): Districts requiring no regularization. This group includes districts that require no regularization—that is, all channel-specific amounts and the implied outside option are nonnegative. Among the 50 districts in the working sample, 42 satisfy this criterion.

(ii) S1 (Extended I; $N = 46$): Districts with negative estimated substitution into alternative shopping modes. In addition to the baseline sample, this group includes districts with $Amt_{j,c} < 0$ for some $c \in \{\text{Sun}, \text{Tem}, \text{Ind}, \text{Uni}\}$. We replace each negative $Amt_{j,c}$ with a small, data-scaled floor $\rho_{\text{div}} Amt_j^{CF}$, where $\rho_{\text{div}} \in \{1\%, 0.5\%, 0.1\%\}$, and recompute the outside option residually to satisfy the accounting identity. In all S1 districts the implied outside option remains strictly positive after this adjustment, so Amt_j^{CF} is left unchanged.

(iii) S2 (Extended II; $N = 50$): Districts with a negative outside option. This group extends S1 by including districts in which the implied outside option remains below the floor. For these districts, we set the value of the outside option to the floor $\rho_{out} Amt_j^{CF}$ and adjust the counterfactual upward by the corresponding shortfall to preserve the accounting identity (i.e., $Amt_j^{CF} \leftarrow Amt_j^{CF} + (\rho_{out} Amt_j^{CF} - Amt'_{j,\text{Out}})$, where $Amt'_{j,\text{Out}}$ is the implied outside option after flooring). We consider $\rho_{out} \in \{1\%,\ 0.5\%,\ 0.1\%\}$. For example, under the 1% adjustment, a counterfactual of KRW 1,000,000 becomes KRW 1,010,000. In Extended II, we fix ρ_{div} from Extended I at 0.1% and vary $\rho_{out} \in \{1\%, 0.5\%, 0.1\%\}$.

The S2 regularization is required only for a small subset of districts with a negative implied outside option (4 out of 50 districts; see Table A12). In these locations, we minimally increase the counterfactual total Amt_j^{CF} to restore non-negativity while preserving the accounting identity, which mechanically shrinks all implied diversion shares $Amt_{j,c}/Amt_j^{CF}$ in the affected districts. In our data, the proportional increase $\Delta Amt_j^{CF,\text{adjusted}}/Amt_j^{CF,\text{unadjusted}}$ has a median of 7.63% (mean 13.38%). Reassuringly, the key welfare estimates are very similar across S0, S1, and S2 (Table 6), indicating that our welfare results are not sensitive to this regularization choice.

Table A12 lists the affected districts.

Recovering Parameters

The following estimation procedures are applied to each analysis sample (S0, S1, and S2); results are presented in section V.

Table A12— Districts excluded or regularized in welfare estimation

Category	Districts
Excluded (island/low-traffic)	Ongjin-gun (Incheon); Yangpyeong-gun (Gyeonggi); Jung-gu (Incheon); Dong-gu (Incheon).
S1 only: negative diversion amounts	Uijeongbu-si (Gyeonggi); Icheon-si (Gyeonggi); Dobong-gu (Seoul); Yeonsu-gu (Incheon).
S2 only: negative outside option	Bucheon-si (Gyeonggi); Siheung-si (Gyeonggi); Giheung-gu (Yongin-si, Gyeonggi); Gangnam-gu (Seoul).

Building on equation (A6), the estimated counterpart of the conditional probability for shopping mode c can be expressed as

$$
\begin{aligned}
\text{(A18)} \qquad & \widehat{\Pr}_j(c \mid \text{Sun}^U) \\
&= \frac{\widehat{\Pr}_j(\{\text{Select } c \text{ with the shutdown}\} \cap \{\text{Select Sun without the shutdown}\})}{\widehat{\Pr}_j(\{\text{Select Sun without the shutdown}\})} \\
&= \frac{Amt_{j,c}}{\mathscr{T}_j} \Big/ \frac{Amt_j^{CF}}{\mathscr{T}_j} \\
&= \frac{Amt_{j,c}}{Amt_j^{CF}}
\end{aligned}
$$

where $\mathscr{T}_j$ denotes the unobserved *total market size*—the aggregate expenditure across all alternatives in the consideration set when visiting restricted retailers on Sunday is included. Although $\mathscr{T}_j$ is not observed, it cancels out between the numerator and the denominator, as shown in the last line of equation (A18). Therefore, the estimated conditional probability $\widehat{\Pr}_j(c \mid \text{Sun}^U)$ depends only on observable expenditure changes $Amt_{j,c}$ and the counterfactual Amt_j^{CF}.

Using the logit structure, we can recover the district-specific deterministic utility parameters, θ's for $c \in \{\text{Tem, Ind, Uni}\}$ as follows:

$$
\text{(A19)} \qquad \begin{aligned} \frac{\Pr_j(c \mid \text{Sun}^U)}{\Pr_j(\text{Out} \mid \text{Sun}^U)} &= e^{\theta_{jc}} \\ \Rightarrow \quad \widehat{\theta}_{j,c} &= \ln \frac{\widehat{p}_j(c \mid \text{Sun}^U)}{\widehat{p}_j(\text{Out} \mid \text{Sun}^U)} \\ &= \ln\left(\frac{Amt_{j,c}}{Amt_j^{CF}} \Big/ \frac{Amt_{j,\text{Out}}}{Amt_j^{CF}} \right) \\ &= \ln \frac{Amt_{j,c}}{Amt_{j,\text{Out}}} \end{aligned}
$$

The remaining parameters—θ_{Sun} (deterministic utility of shopping at restricted retailers on a Sunday) and κ (the travel-time coefficient)—are assumed to be homogeneous across districts. With the normalization of $\widehat{\theta}_{j,\text{Out}} = 0$, substituting the recovered district-specific $\widehat{\theta}_{j,c}$ values for $c \in \{\text{Tem}, \text{Ind}, \text{Uni}\}$ into equation (A17) yields the log likelihood:

(A20)

$$
\begin{aligned}
&\ell(\theta_{\text{Sun}}, \kappa; \widehat{\Theta}_-) \\
&= \sum_{j=1}^{J} \Big[Amt_{j,\text{Sun}} \ln \Pr_j(\text{Sun}^R \mid \text{Sun}^{\text{U}}; \widehat{\Theta}_-) + \sum_{c \neq \text{Sun}} Amt_{j,c} \ln \Pr_j(c \mid \text{Sun}^{\text{U}}; \widehat{\Theta}_-) \Big] \\
&= \sum_{j=1}^{J} \Bigg[Amt_{j,\text{Sun}} \ln \Big[\exp\big[\kappa\,(TravelTime_j^R - TravelTime_j^U)\big] \cdot \frac{\exp\big(\theta_{\text{Sun}} + \kappa\, TravelTime_j^U\big) + \sum_{c \neq \text{Sun}} \exp \widehat{\theta_{jc}}}{\exp\big(\theta_{\text{Sun}} + \kappa\, TravelTime_j^R\big) + \sum_{c \neq \text{Sun}} \exp \widehat{\theta_{jc}}} \Big] \\
&\quad + \sum_{c \neq \text{Sun}} Amt_{j,c} \ln \Big[exp(\widehat{\theta_{jc}}) \cdot \frac{(1 - exp(\kappa \cdot TravelTime_j^R - \kappa \cdot TravelTime_j^U))}{\exp\big(\theta_{\text{Sun}} + \kappa\, TravelTime_j^R\big) + \sum_{c \neq \text{Sun}} \exp \widehat{\theta_{jc}}} \Big] \Bigg]
\end{aligned}
$$

where $\Pr_j(c \mid \text{Sun}^U; \widehat{\Theta}_-)$ denotes the model probability evaluated at the plug-in parameters $\widehat{\Theta}_- = \{\widehat{\theta}_{j,\text{Tem}}, \widehat{\theta}_{j,\text{Ind}}, \widehat{\theta}_{j,\text{Uni}}\}$ recovered in the previous step.

We estimate θ_{Sun} and κ by maximizing the log-likelihood using a Broyden–Fletcher–Goldfarb–Shanno (BFGS) algorithm. To compute standard errors, we apply a bootstrap that resamples districts 1,000 times with replacement.