Space and Cyberspace: The Impact of Food Delivery Platforms on Retail Real Estate

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Abstract

The rise of food delivery platforms (FDPs) presents a puzzle: as consumers switch to online ordering, do these platforms make retail spaces less or more valuable? In this paper, I first develop a spatial equilibrium model to understand how FDPs affect the retail real estate market. The model highlights two opposing forces. FDPs increase overall consumer demand for restaurant meals, which raises the need for space. At the same time, they shift part of this consumption online, which reduces the need for space. I then resolve this ambiguity using a staggered difference-in-differences strategy exploiting the geographic rollout of FDPs across the United States. The empirical results show a net positive demand shock: FDP entry increases the number of retailers by 2.1%. This growth in demand translates into a 1.0% increase in retail rents and a 0.7% increase in property values. I also find that the supply of retail floorspace is highly inelastic. Taken together, these results suggest that the value generated by the FDP-induced demand shock is captured by landlords, rather than by the retailers themselves, consistent with the classic *Ricardian Theory of Rent*.

Keywords: Retail Real Estate, Digital Economy, Food Delivery Platform

JEL Codes: R33; L81; R12; C23

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1 Introduction

A central question in urban economics is whether digital technologies act as substitutes for or complements to offline economic activity (Sinai and Waldfogel, 2004; Lieber and Syverson, 2012). The rapid rise of online food delivery platforms (FDPs), which digitize the "last-mile" service, provides a powerful setting to test this tension. Do these platforms decrease the attractiveness of brick-and-mortar locations or do they enhance the value of retail space by expanding offline market access to cyberspace? This paper addresses this question by investigating the consequences of FDPs on the demand for and value of retail space.

FDPs, such as Doordash and Uber Eats, represent a massive and recent technological shock to the urban retail landscape. Founded recently (DoorDash in 2013, Uber Eats in 2014), these platforms have experienced explosive growth, with average annual growth rates exceeding 45% between 2018 and 2024. In 2024 alone, combined sales on these two platforms surpassed $55 billion in the United States, capturing over 42 millions of users and organizing vast networks of delivery drivers[1]. While traditional e-commerce platforms like Amazon are often viewed as direct substitutes for local retailers, contributing to a "retail apocalypse" by shifting demand from local stores to remote fulfillment centers (Chung, 2023; Chava et al., 2024). FDPs, on the other hand, are intrinsically tied to local retailers, with the average delivery time being approximately 30 minutes (Pourrahmani et al., 2023). Consequently, the interaction between FDPs and local retailers is more complex, making them a critical and distinct case for studying the interaction between online and offline economies.

I develop a spatial equilibrium model to examine how FDPs, by affecting consumers' food-ordering behavior, generate effects within the retail real estate market. The model shows that FDPs create two opposing forces on restaurant space demand. First, a *market expansion effect*: by lowering access costs to restaurants, FDPs increase total demand for restaurant meals, substituting away from cooking at home (Raj and Eggers, 2022; Sullivan, 2025). Second, a *spatial reduction effect*: as they enable consumers to order online without visiting the physical store, FDPs reduce restaurants' need for large dine-in areas, with some even operating as "cloud kitchens." This initial

[1] Sales and user statistics are compiled from the annual reports and investor presentations of DoorDash and Uber Eats. Sources: DoorDash Economic Impact Report (https://doordash2024.publicfirst.co/); Uber Eats 2024 Merchant Impact Report (https://merchants.ubereats.com/ca/en/resources/community/merchant-impact/).

change in demand from the restaurant industry impacts the equilibrium retail rent, which in turn leads other retailers to adjust their space demand as they compete for limited space. Analyzing the market equilibrium before and after the FDP shock generates several testable hypotheses: (i) increase the total number of retailers; (ii) decrease the average physical size per retailer; and (iii) increase the relative prevalence of restaurants within the local retail sector. (iv) Importantly, because the market expansion and spatial reduction effects oppose each other, the model's prediction for the net effect on total retail floorspace, and the subsequent impact on retail rent, is ambiguous. This ambiguity motivates the empirical evaluation.

I identify the causal impact of FDPs by leveraging the staggered expansion of major platforms (e.g., DoorDash and Uber Eats) across U.S. cities. I employ a staggered difference-in-differences (DiD) strategy proposed by Callaway and Sant'Anna (2021), comparing outcomes in cities before and after FDP entry to outcomes in cities that have not yet been treated.[2] I carefully discusses potential selection problems of FDPs' expansion timing, including control for interactions between year fixed effects and baseline city characteristics and testing for possible confounders (Section 6.1). The empirical analysis includes two parts. First, I examine the micro-foundations of the demand shock by testing the model's hypotheses for local retailers, focusing on the number of retailers and their average establishment size. Second, I investigate how the introduction of FDPs, as a demand shock for the retail real estate market, affects market rent (price) and total occupied space (quantity). I also examine supply-side responses, such as new construction.

I combine three primary data sources containing information about major U.S. cities (CBSAs) from 2015 to 2024. First, to identify the timing of FDP expansion, I use city-level Google Trends search indices, which reflect real-time consumer awareness and platform activity (Siliverstovs and Wochner, 2018). Second, I use the Data Axle (formerly Infogroup) historical business database to construct an annual panel of retailers with industry and size information. Third, I use the CoStar Database, a leading dataprovider for commercial real estate information, to examine the retail real estate market outcomes, including asking rents, transaction prices, inventory, and new constructions. Crucially, this dataset allows me to isolate retail-specific properties from the broader

[2] The treatment variable is defined by the availability of at least one major FDP. The focus of this paper is on the introduction of online food delivery as a new consumption channel, rather than on the marginal effects of inter-platform competition. A city is therefore defined as "treated" upon the entry of the first major platform. I discuss the potential for other food delivery platforms in Appendix D.

commercial real estate market (e.g., office or industrial).

The empirical results show both the market expansion effect and the spatial reduction effect, consistent with the model's predictions. FDP entry causes a statistically significant 2.1% increase in the number of local retailers. This growth is driven primarily by the restaurants, the industry most directly affected by the platforms. Besides, I find that retailers adjust their spatial demand towards smaller physical footprints: the share of small establishments (less than 2,500 sqft) increases by 0.4 percentage points. Crucially, I find that the market expansion effect dominates the spatial reduction effect, resulting in a net positive aggregate demand shock for retail space.

This positive demand shock translates directly into higher rent and property value in the retail real estate market. Following FDP entry, retail asking rents per square foot increase by 1.0%. For an average-sized fast-food restaurant (around 4,500 sqft), this implies an annual rent increase of $900. This finding highlights that by boosting consumer demand, FDPs increase the value of the underlying physical real estate. Consistent with this rent capitalization, I find that retail property values (transaction prices per square foot) also increase by 0.7%.

To fully understand the FDP shock on the retail real estate market, I next examine whether the supply of retail space responds to the price increase. I find strong evidence that the supply of retail floorspace is highly inelastic. First, the total retail inventory remains largely unchanged, even accounting for potential conversions from other commercial uses. Second, this stability does not simply mask an absorption of vacant space, as I analyze occupancy rates and again find no significant increase. Finally, I find no significant increase in new construction. These findings strongly suggest that the economic surplus generated by the FDP innovation does not primarily accrue to retailers (tenants). Instead, consistent with a Ricardian rent framework (Ricardo, 2005; Fujita, 1989), as tenants compete for the limited, inelastically supplied space, their potential profits are bid away. The surplus generated by this technological advancement is captured by incumbent landlords in the form of higher rents and property value.[3]

These findings are robust. They are not driven by the COVID-19 pandemic, as the results hold when excluding the post-COVID period or controlling for local pandemic intensity. Placebo

[3] The surplus analysis in this paper focuses on the changes occurring on the demand and supply sides of the space market. FDPs involve other key agents, including consumers, drivers, and platforms. For example, consumers benefit from the food delivery platforms because of reduced cost and increased variety. This broader welfare analysis is beyond the scope of this paper.

tests using establishments in non-retail sectors (e.g., finance and management) show no significant effects, confirming the impact is specific to the retail environment. Finally, the results are robust to alternative dynamic DiD estimators (Sun and Abraham, 2021; Wooldridge, 2021), restricting the sample to urban areas, and excluding never-treated cities from the control group.

This paper contributes to several strands of the literature. First, it adds to the nascent literature on the economics of food delivery platforms. Existing research has primarily focused on platform operational efficiency (Chen et al., 2022; Zhang et al., 2023; Sullivan, 2025), the impact on individual restaurant performance and productivity (Raj et al., 2020; Raj and Eggers, 2022; Goolsbee et al., 2025), and labor market effects, including gig economy employment and entrepreneurship (Plotkin, 2024; Liu et al., 2024b; Chen and Liu, 2024; Shamsi, 2025). This paper provides the first comprehensive causal evidence on the spatial consequences of FDPs, analyzing how they reshape the physical retail landscape and the equilibrium outcomes in the market for retail space.

Second, this paper contributes to the growing literature examining the spatial implications of digital platforms (Forman et al., 2005; Goldmanis et al., 2010; Luca, 2016; Davis et al., 2019; Couture et al., 2021; Chava et al., 2024). Within a city, Previous studies have investigated how ride-sharing services like Uber affect transportation patterns and the redistribution of economic activity (Hall et al., 2018; Gorback, 2020), and how short-term rental platforms like Airbnb impact residential housing markets and local amenities (Garcia-López et al., 2020; Koster et al., 2021; Barron et al., 2019; Calder-Wang, 2021; Almagro and Domínguez-Iino, 2024). I extend this literature by analyzing FDPs—an economically significant platform type with strong ties to local establishments. My contribution is to examine the spatial consequences for the retail real estate market, a margin distinct from the transportation or residential markets previously studied.

Third, this paper relates to the "Consumer City" literature (Glaeser et al., 2001; Couture and Handbury, 2020). Couture (2016); Monte et al. (2017) show that the consumer's time of accessing amenities affects local retail structure. I contribute to this literature by examining a setting where consumer access cost to local amenities is lowered by an online platform. My finding of an increase in the total number of brick-and-mortar establishments is consistent with recent work by Relihan (2024). In this way, my paper also provides new evidence on the increase in non-tradable services in cities (Couture and Handbury, 2020; Brooks and Meltzer, 2025).

Lastly, the paper contributes to the literature on commercial real estate and urban economics.

(i) I contribute to the growing discussion on how technological improvements are reshaping the commercial real estate market (Rosenthal et al., 2022; Gupta et al., 2022; Duca and Ling, 2024). While some technologies act as substitutes (e.g., online-remote working reducing office demand), other work shows that physical proximity remain important even with the rise of e-commerce (Brooks and Meltzer, 2025). My paper adds to this debate by showing that platforms facilitating local delivery (FDPs) can act as a complement, enhancing the value of existing retail space and encouraging the growth of local establishments. (ii) I contribute to a more specific literature focusing distinctly on retail real estate. Few studies differentiate between retail properties and other commercial assets. This distinction is important, as retail real estate is a unique asset class characterized by its public-facing nature, reliance on consumer traffic, and concentration in dense urban areas. My analysis of retailer entry and space demand connects to foundational work on store location choice (Barwick Jia, 2008; Holmes, 2011; Pozzi, 2013) and recent studies on retail market dynamics, such as landlord strategies (Moszkowski and Stackman, 2024; Schneier, 2025) and anchor tenant effects (Liu et al., 2024a).

Within this literature, the paper closest to mine is Chung (2023), which finds that the establishment of e-commerce fulfillment centers increases nearby retail property values. My analysis differs and expands upon this work in two key ways. First, I study a distinct mechanism. FDPs are an urban phenomenon that leverages existing local retailers. In contrast, fulfillment centers are large, remotely-located logistical hubs. Second, and more importantly, I provide the retailer-level micro-foundations for why FDPs generate a positive aggregate demand shock.

The remainder of the paper is organized as follows. Section 2 presents the conceptual framework and derives the testable hypotheses. Section 3 describes the industry background and the data. Section 4 outlines the empirical strategy. Section 5 presents the main results on retail real estate outcomes and establishment dynamics. Further robustness checks are in Section 6. Section 7 concludes.

2 Conceptual Framework

I develop a model of a local neighborhood retail market to analyze the impact of Food Delivery Platforms (FDPs) on the demand for retail space.[4] The model considers how FDPs alter restaurant operations and consumer behavior, affecting the allocation of space between restaurants and other retailers via a bid-rent mechanism.

The introduction of FDPs generates two competing forces:

1. **Market Expansion Effect:** FDPs lower consumer access costs (e.g., search and travel frictions), thereby increasing the overall demand for restaurant meals.

2. **Spatial Reduction Effect:** Delivery orders require less physical space (kitchen only) compared to dine-in orders (kitchen and dining space). The shift toward delivery reduces the average space needed per order.

The model analyzes how the balance of these forces determines equilibrium rents, the number of establishments, and their size distribution, generating testable hypotheses for the empirical analysis.[5]

2.1 Model Setup

Consumers. Consumers first choose between home cooking and the restaurant sector. If choosing restaurants, they select a channel, Dine-in (DI) or FDP, and then a specific restaurant. Restaurants are assumed to be monopolistic competitors. Demand follows a nested structure: CES preferences across restaurants (elasticity $\sigma > 1$) nested within a logit choice for sector and channel (See Appendix B.1).

The attractiveness of the restaurant sector depends on the utility derived from both channels. Utility increases with food quality and ambiance (for DI) and decreases with the variety-adjusted price index and generalized access costs (T_{channel}). Consequently, the total quantity of restaurant meals demanded, Q_R, increases as access costs fall and variety (N_R) increases.

[4] The consumption for food is highly localized, even people order through FDPs, most of the orders happen within a neighborhood (Couture, 2016; Pourrahmani et al., 2023).

[5] The focus is on neighborhood/submarket outcomes.

Restaurants: Profit and Space Demand. The crucial assumption driving the spatial reduction effect is the differential space intensity of orders. A dine-in order uses kitchen (η_k) and dining space (η_d), while a delivery order uses only kitchen space (η_k).

This difference affects marginal costs, which depend on the rent r:

$$c_{\text{DI}}(r) = c + r(\eta_k + \eta_d), \qquad c_{\text{FDP}}(r) = c + r\,\eta_k.$$

Restaurants set prices with a constant markup $\mu = \sigma/(\sigma - 1)$, accounting for the platform commission δ on delivery revenue[6]:

$$p_{\text{DI}} = \mu\, c_{\text{DI}}, \qquad p_{\text{FDP}} = \frac{\mu}{1-\delta}\, c_{\text{FDP}}. \tag{1}$$

A restaurant's required space depends on its channel mix. Let χ be the share of delivery orders. The average space per order is:

$$\bar{\eta} \;=\; \eta_k + (1-\chi)\,\eta_d. \tag{2}$$

The total store size for a restaurant serving q total orders is $s_R = \bar{\eta}\,q$. Restaurants enter the market until profits, net of fixed operating costs F, are zero.

Other Retailers and the Space Market. A continuum of heterogeneous ”other retailers” also competes for space. Examples of such establishments include cloth stores, bookstores, gas stations, etc. Their aggregate demand for space, $S_O(r)$, is strictly decreasing in rent r (See Appendix B.3). The total neighborhood retail space supply, L, is assumed to be inelastic.[7]

2.2 Equilibrium

An equilibrium (r^*, N_R^*, N_O^*) is defined by (E1) free entry and optimal sizing for retailers, and (E2) space market clearing.

[6] The commission fee δ is from 0.15 to 0.25 per order. This paper take the median value of 0.20. This would imply a 25% higher menu price for delivery orders compared to dine-in orders, which is consistent with the finding in **?**. The restaurant sector is assumed to be large enough for monopolistic competition, as modeled in **?**.

[7] The equilibrium condition holds as long as the price elasticity of supply $e < 1$, which is consistent with the empirical evidence (Saiz, 2010; Baum-Snow and Han, 2024). The empirical analysis tests this assumption.

(E1) Retailers profit-maximizing and free entry The zero-profit condition determines the equilibrium number of restaurants $N_R(r)$:

$$N_R(r) \;=\; \frac{Q_R}{(\sigma-1)F}\,\bar{c}(r). \tag{3}$$

Here, $\bar{c}(r)$ represents the weighted average marginal cost across the two channels. This equation defines a fixed-point relationship, as Q_R itself depends on N_R (via variety) and r (via prices). The equilibrium orders per restaurant is consequently $q(r) = (\sigma-1)F/\bar{c}(r)$.

(E3) Space Clearing. The equilibrium rent r^* equates supply and demand:

$$L \;=\; \underbrace{N_R(r)\,s_R(r)}_{\equiv S_R(r)} + S_O(r). \tag{4}$$

The total space demanded by restaurants can be rewritten as $S_R(r) = Q_R(r)\bar{\eta}(r)$.

Existence and Uniqueness of Equilibrium. The existence of a unique equilibrium rent r^* is guaranteed because the total space demand function $H(r) = S_R(r) + S_O(r)$ is continuous and strictly decreasing in r. This downward slope occurs because higher rents increase costs, reducing both total consumption (Q_R) and the average space intensity ($\bar{\eta}$) as demand shifts toward the less space-intensive FDP channel. See Appendix B.4 for a detailed discussion.

2.3 Comparative Statics and Hypotheses

The introduction of FDPs is modeled as a technology shock that significantly lowers the consumer access cost for the delivery channel (T_{FDP}). Before FDPs, this cost was prohibitively high; after the shock, it becomes finite and accessible. (Full justifications are provided in Appendix B.5).

Model Implications: Consumer Behavior. A direct consequence of this shock is a shift in consumption patterns. By offering lower access costs and greater potential variety, FDPs encourage consumers to substitute away from home cooking towards restaurant meals, boosting total consumption ($Q_R \uparrow$), and shifting consumption towards the delivery channel ($\chi \uparrow$).

These changes activate the model's core mechanisms—Market Expansion and Spatial Reduction—leading to the following testable hypotheses regarding the retail landscape:

Hypothesis 1 (H1: Increased number of retail establishments (Market Expansion))**.** *The introduction of FDPs increases the number of restaurants ($N_R \uparrow$) and the total number of retailers ($N_{Total} \uparrow$).*

Intuition: The FDP-induced increase in total demand for restaurant meals ($Q_R \uparrow$) **creates** initial profit opportunities. Due to free entry, new restaurants enter until the zero-profit condition is restored ($N_R \uparrow$). This primary entry effect dominates any secondary displacement of other retailers due to rent changes, leading to a net increase in total establishments ($N_{Total} \uparrow$).

Hypothesis 2 (H2: Establishment Downsizing (Spatial Reduction))**.** *The introduction of FDPs decreases the average size of restaurants ($s_R \downarrow$) and the average size of all retail establishments ($\bar{s} \downarrow$).*

Intuition: The shift towards the less space-intensive delivery channel ($\chi \uparrow$) reduces the average space required per order ($\bar{\eta} \downarrow$). This effect dominates the potential increase in orders per firm (q), leading to smaller restaurants ($s_R \downarrow$).

Hypothesis 3 (H3: Equilibrium Rent (Competing Forces))**.** *The impact of FDPs on the equilibrium rent (r^*) is theoretically ambiguous.*

Intuition: The equilibrium rent is determined by the total demand for restaurant space, $S_R = Q_R\bar{\eta}$. The two core mechanisms have opposing effects. The Market Expansion Effect ($Q_R \uparrow$) increases space demand, while the Spatial Reduction Effect ($\bar{\eta} \downarrow$) decreases it. The net impact on rent depends on the relative magnitude of these two forces.

The theoretical ambiguity surrounding aggregate space demand and equilibrium rent underscores the need for further empirical investigation in this paper. Section 5 will test the existence of Hypotheses 1 and 2, and examine the direction of Hypothesis 3.

3 Industry Background and Data Sources

In this section, I provide the industry background and the data sources used in the empirical analysis. First, I provide the background on how FDPs operate and how to measure their expansion across cities in the US, introducing the geographic unit used for identification. Second, I describe the data sources used in the empirical analysis, including the panel data of retailers from Data Axle and the retail real estate data from CoStar. Third, I introduce other data sources, including census data and geographic information for the empirical analysis.

3.1 Expansion of Food Delivery Platforms

FDPs represent a transformative new consumption channel in the local retail market, operating as a unique type of online platform. A typical FDP works as follows: consumers search for restaurants, select their desired items, and pay. The platform then assigns a delivery person (hereafter referred to as a *driver*) to bring the food to the consumer. Unlike traditional food delivery, FDPs provide two key advantages: they enable consumers to access rich information (reviews, ratings, and prices) that was previously unavailable, and they significantly expand the range of restaurants capable of offering delivery services[8]. Therefore, FDPs profoundly change how consumers order food, which further reshapes how restaurants operate and their demand for physical space.

FDPs typically expand by launching city-level new marketplaces. This approach is necessary to accommodate the local regulatory environment and to build a sufficient base of active restaurants on the platform. In each city, the local team prepares for consumer launch by recruiting and onboarding restaurants and drivers. They negotiate restaurant partnerships, configure menus and delivery fees, and build their driver network through recruitment, app setup, and basic training. This prep phase often spans several weeks or months. Upon reaching a critical mass of restaurant partners, the FDP's service launches in the city. For example, DoorDash had over 1,500 local restaurants listed on its website when entering Detroit[9]. After launch, restaurants and drivers continue to join as demand grows. Figure A.2 and Figure A.3 shows how DoorDash and Uber Eats

[8] See more details at DoorDash Economic Impact Report (2024). According to the estimation of Sullivan (2025), 1.5 years after the launch of FDPs, 60% of restaurants in major US cities were on at least one platform.

[9] An example of the news coverage on launch of FDP service: WXYZ Detroit, Food delivery service DoorDash arrives in metro Detroit with 1,500 local restaurants

exhibit their "marketplace" or "find eats in your city" in practice, which are at the city level.

I construct a monthly panel dataset that records the presence of two major cross-regional FDPs, DoorDash and Uber Eats, from 2016 to 2024[10]. As long as one of the two platforms is present in a city, the city is considered as "treated". I don't differentiate the two platforms in the analysis, as they operate in a similar way and have similar impacts on the operation of restaurants. The key change is when consumers can order online. If restaurants receive orders from either platform, they are charged with a commission fee proportional to the order value, and platform-owned delivery drivers bring the food to the consumer. Although there are complicated restaurant-platform relationships and the multi-homing problem, they won't be the focus of this paper, which is to investigate the impact of FDPs on the retail space market.

To capture city-level variation of FDP presence, the geographic unit of analysis is the Core-Based Statistical Area (CBSA), defined by the US Census Bureau as "clusters and adjacent counties that have high commuting ties to that core". CBSAs represent an appropriate unit for analyzing geographically-continuous economic shocks at the city level. The analysis is restricted to CBSAs for which commercial real estate data is available. There are 917 CBSAs in the contiguous US, but I further excluded those with ambiguous names that overlap with more well-known locations (e.g., "Paris, IL"). Among all 872 CBSAs in the contiguous United States, 753 CBSAs have the commercial real estate data from CoStar and are used in the analysis.

I construct the expansion of FDPs using Google Trends data, which provides monthly search volume metrics. Its common applications include tracking the diffusion of new technologies and monitoring consumption volumes — scenarios that mirror the expansion of FDPs. Google Trends data is widely used in empirical research due to its wider coverage of users and ability to capture economic activities in near real-time. For example, Siliverstovs and Wochner (2018) argue that Google Trends provides an accurate approximation of tourism flows in Switzerland. Brodeur et al. (2021) examine the effect of COVID-19 and associated lockdowns on population well-being by measuring changes in well-being-related search terms.

In this project, the panel of FDPs expansion is built by pairing the platform name with the

[10] These two platforms are the most widely used FDPs in the United States, which together account for over 90% of the market share. Other platforms, such as Grubhub, are not included in this study because of the following reasons: (1) Having much smaller market share, other FDPs're unlikely to be the first one entering the city. (2) these platforms lack a platform-owned delivery network; consequently, their impacts on the local retail industry likely differ from those modeled in this paper. Appendix D explains this in detail.

city (e.g., "Uber Eats" and "Chicago"). A positive Google Trends search volume for a platform-specific query is interpreted as evidence that consumers in a city are aware of the food delivery service.[11] To reduce noise from sporadic searches, I define a platform's market entry as the first period during which the Google Trends index consistently registers a positive value. The inferred entry date is then cross-validated with local news reports, historical records from WebArchive.[12] [13] Examples of Google Trends over time and the inferred entry date and related news reports are shown in Figure A.1.

The platform presence pattern from Google Trends should be interpreted as a near-concurrent or slightly lagging indicator of platform entry, for two main reasons. First, before launching to consumers, FDPs typically spend time forming local networks by reaching out to existing restaurants and organizing delivery drivers. Local restaurants may also start strategic planning prior to the official launch. Second, there is a natural delay as the platform builds local presence and gets to be known by more consumers[14]. However, this delay does not severely affect the validity of the analysis, which focuses on the long-term impact of FDPs on retail real estate. Besides, if the FDPs' entry is earlier than the Google Trends index indicates, the results are still valid, as the estimated treatment effect, by comparing outcomes before versus after the treatment, would be an under-estimation of the FDPs' impact.

Figure 1 shows the platform expansion pattern. The x-axis represents the year from 2016 to 2024. The y-axis shows the share of cities treated in each year among all CBSAs in the sample. Gray areas are time periods where there were national stay-at-home orders or other similar policies.[15]

[11] Google Trends normalizes search queries with very low volumes to zero.

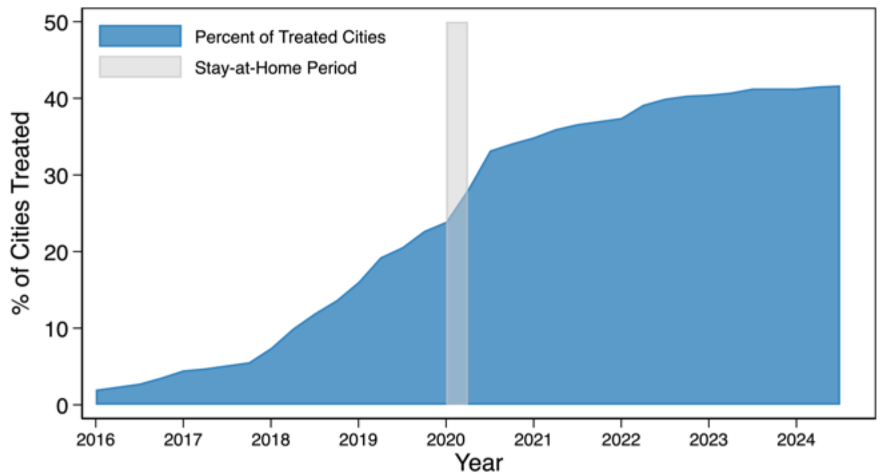
[12] For example, Google Trends index indicates that Doordash entered Detroit in February 2018, and I cross-validate its entry by checking the Eater Detroit article published on February 8, 2018, which states that "Doordash enters Metro Detroit". Report link here.

[13] An example of a WebArchive screenshot is shown in Figure A.2. However, due to the sparse temporal coverage of archived webpages, WebArchive cannot reliably determine the exact market entry date of platforms. Instead, the Google Trends index offers a more precise indication of entry timing, with WebArchive serving as a supplementary means of verification.

[14] The second bias is less likely to be a problem in this study, as FDPs usually launch with marketing campaigns and news coverage with a lot of local restaurant partners, they pay special attention to make sure that FDPs get to be known by more consumers.

[15] The policy restrictions vary by states. This figure plots the most restrictive time period in the United States. For example, Illinois lifted its state-wide stay-at-home order on May 29, 2020 *(WTTW: Illinois' Stay-at-Home Order Ends as State Moves into Next Phase of Reopening)*. While California lifted its stay-at-home order on January 25, 2021 *(CBS news: California Governor Gavin Newsom lifts virus stay-at-home orders)*.

Figure 1: Expansion of Food Delivery Platforms over Time



Notes: The plot shows the expansion of food delivery platforms over time, from 2016Q1 to 2024Q3. The x-axis is the year of first food delivery platform entry, either DoorDash or Uber Eats. The y-axis is the share of cities that have at least one food delivery platform. The gray areas are time periods where there's national stay-at-home orders or other similar policies.

As the graph indicates, FDP coverage was limited before 2016. This is consistent with the fact that during this early period, platforms like DoorDash and Uber Eats were still operating regionally and their business models were not yet fully established. Therefore, this study focuses on the primary expansion phase from 2016 to 2024.

FDPs expanded rapidly in the pre-pandemic period from 2016 to 2019, covering approximately 180 cities by the end of 2019. A significant surge in expansion occurred during the early months of the COVID-19 pandemic in 2020, coinciding with widespread lockdowns and associated surge in online ordering. The FDP expansion reached a plateau since 2022. The remaining uncovered locations are mostly micropolitan statistical areas, which are less likely to be serviced by FDPs[16]. In the appendix, Figure A.4 shows the map of platform expansion across the U.S., from 2016 to 2024.

[16] Micropolitan statistical areas are centered on an urban cluster (urban area) with a population of at least 10,000 but fewer than 50,000 people.

3.2 Establishment Data for Local Retailers

To analyze the impact of FDPs on local retailers, I construct an annual panel dataset of local establishments using the Data Axle Historical Business Database (formerly Infogroup). The database is a near-universe annual panel of roughly 35 million U.S. establishments each year. This database is widely used in academic research, especially in research on the location of urban economic activities; for example, Di Maggio et al. (2025) uses this database to identify payday lenders. Its comprehensiveness and validity are further supported by government data, such as American Community Survey (ACS) estimates, and an official report from the US Department of Housing and Urban Development details the data's quality (Ramiller et al., 2024).

Data Axle collects business names, addresses, and other details from public sources such as the Yellow Pages. The database also includes exact geocoded location, six-digit NAICS classification, establishment age, number of employees, sales volume, and corporate linkages for each establishment. One important feature of this database is that it provides a unique identifier for each establishment, allowing me to track the same establishment over time with high location accuracy.

A significant advantage of the Data Axle dataset is its focus on active establishments. The provider actively verifies the status of listed businesses, which minimizes the inclusion of obsolete records that could compromise data accuracy. Therefore, establishment counts at a local level (e.g., within a ZIP code) provide a more accurate reflection of industry health. This accuracy is crucial for my analysis. I further exclude establishments that entered and exited in the same year to avoid the data collection delay and the bias from short-lived establishments.

I construct a panel dataset of establishments spanning from 2014 to 2023 to align with the study period[17]. Using NAICS codes, I identified retail tenants and further categorized them into subgroups such as restaurants and grocery stores; see Appendix C for details[18].

[17] The 2024 data was not available at the time of analysis but is planned for inclusion in a future version of this analysis.

[18] I also used the CoStar glossary information to identify the industry classification of common retail tenants and verify their presence in the market report. CoStar Market Report on Retail Tenant Sectors

3.3 Retail Real Estate Data

To measure the effect of FDPs on the retail real estate market, I construct a quarterly panel dataset of retail real estate properties using the CoStar Database.

CoStar Group, Inc. is a leading provider of commercial real estate information and analytics. It is one of the most credible sources for both rent and sales data. Its comprehensiveness stems from its data collection methodology, which aggregates information from public records, tax assessments, and realtors. This contrasts with other providers, such as CompStak, which primarily rely on data contributed by realtors. Furthermore, CoStar employs local analyst teams who visit properties to collect data and update information regularly. This provides the most complete picture of all existing retail properties in a market, not just those that have recently transacted.

Important for comprehensive analysis, CoStar provides distinct datasets for different commercial property types as separate markets to examine the market response of retail spaces. Commercial real estate types listed on CoStar include retail, office, industrial, hospitality, and multifamily (mainly for-rent condos). This project focuses on retail properties, defined as public-facing properties whose primary use is to promote, distribute or sell products and services.[19] Some examples under the retail category include restaurants, grocery stores, and shopping centers.

Geographically, the data is organized into units CoStar calls "markets" and "submarkets". Markets are comparable to a city or Core-Based Statistical Area (CBSA), which can be further subdivided into submarkets, a geographical unit roughly comparable to the zip code area, although ZCTA is slightly finer. This geographic unit classification is consistent with the other datasets, as the "market" (CBSA) level aligns with the FDP expansion treatment variable, while the "submarket" level is consistent with the local retailer information. 752 out of 872 CBSAs are covered by the CoStar data; 25.7% of them have submarkets.[20] Figure A.5 shows an example of CoStar submarket boundaries in Nashville.

The CoStar dataset provides rich measurements of the retail real estate market outcomes, including lease and sale transactions. Besides, CoStar also includes inventory data. By keeping track of local market conditions, this feature allows me to examine the supply-side response of the retail

[19] The CoStar glossary webpage provides a detailed definition of retail properties with more examples: CoStar Glossary Link.

[20] There are 917 CBSAs in the contiguous US, but I further excluded those with names occupied by more well-known places.

real estate market.

3.4 Supplementary Data Sources

This study incorporates several supplementary datasets from census and other public sources. These datasets are used to merge and align the various data sources, and they serve key roles in the empirical strategy and subsequent robustness checks. A more detailed discussion of their application is provided in the following section of 4.

1. **Geographic Data:** Geographic boundary files are sourced from the U.S. Census Bureau's TIGER/Line Shapefiles. These include boundaries for ZIP Code Tabulation Areas (ZCTAs) and Core-Based Statistical Areas (CBSAs), which are used to define analytical units and visualize data. This study also utilizes the Census definitions for Urban Areas (UAs) and Urban Clusters (UCs). UAs represent densely developed territories with 50,000 or more people, while UCs represent areas with at least 2,500 but fewer than 50,000 people. This distinction is utilized in a later robustness check to test whether FDP operations are primarily concentrated in core urban areas and have a diminished impact in suburban parts of a market.

2. **Demographic Data:** Demographic and socioeconomic data are drawn from the U.S. Census Bureau's American Community Survey (ACS) 5-Year Estimates in the baseline year of 2015. Key variables include population density, age distribution, educational attainment, median household income, and commuting patterns. These factors could potentially affect the FDPs' expansion and the retailers' outcomes. A detailed discussion of these variables and their role in exploring potential mechanisms is provided in Appendix F.

3. **Platform Regulation Data:** I collected data on local and state-level regulations that directly impact FDP operations. These measures include municipal caps on the commission fees that FDPs can charge retailers and minimum wage laws specifically targeting delivery drivers. This dataset allows the analysis to control for the local regulatory environment, which may independently affect both FDP expansion and retailer outcomes. Data sources include local new coverages, and state-level regulations[21].

[21] Link for city-level FDP commission fee regulation: Link. Link for state-level minimum wage regulation: Link.

4. **COVID-19 Severity:** To control for the local impact of the COVID-19 pandemic, I use county-level data from the COVID-19 Data Repository maintained by the Center for Systems Science and Engineering (CSSE) at Johns Hopkins University[22].

3.5 Descriptive Analysis

Table reports descriptive statistics (means and standard deviations) for neighborhood-level outcome variables in 2015, the last year before the FDPs' expansion, by the cohort of cities' treatment timing.[23]

Table 1: Summary Statistics for Outcome Variables

	Entry 2016-2019		Entry 2020-2024		No observed entry	
	(1) mean	(2) sd	(3) mean	(4) sd	(5) mean	(6) sd
Panel A: Establishment Counts						
All industry	793	806.64	581	697.01	456	605.73
Retailer	259	260.44	190	226.30	152	199.84
Restaurant	38	42.32	26	33.05	20	28.41
Panel B: Retail Real Estate Market						
Asking rent (per sqft)	21.42	7.25	19.13	7.96	17.24	5.15
Sale price (per sqft)	208.32	79.99	185.22	81.25	180.60	60.91
Inventory (million sqft)	4.82	4.96	5.00	5.19	3.56	3.65
New construction (sqft)	43,398	80,778	45,789	81,364	24,752	61,116
Occupancy rate (%)	94.96	3.23	95.34	2.48	95.72	2.92

Notes: This table reports descriptive statistics (mean and standard deviation) for neighborhood-level outcome variables in 2015, the final pre-treatment year. The columns group observations based on the subsequent timing of FDP entry into the city (CBSA). Panel A reports establishment counts at the zip code level. Panel B reports retail real estate metrics at the submarket level. The grouping by entry cohort (e.g., 2016-2019 vs. 2020-2024) is a simplification for this presentation; the full regression analysis uses the specific quarter of FDP entry as the treatment event. Throughout the empirical analysis, cities that had FDPs before 2016 are excluded, as FDPs functioned differently before they became inter-regional platforms.

[22] The JHU COVID-19 tracker stopped updating on April 28, 2023. But the data is still available on the COVID-19 Data Repository .

[23] Throughout the empirical analysis, I excluded cities that have FDPs before 2016, as they're always-treated and the FDP effect can be different before the FDPs' national expansion. These cities include Atlanta, Austin, Boston, Chicago, Dallas, Denver, Houston, Indianapolis, Los Angeles, Minneapolis, Nashville, New York, San Francisco, San Jose, and Washington.

The table reveals systematic pre-existing differences between the cohorts, indicating that FDP expansion was not random. Cities that received FDPs early (the 2016-2019 cohort) were already larger, denser, and more expensive in 2015.

Specifically, Panel A shows that early-entry neighborhoods had significantly higher average establishment counts for all industries (793), retailers (259), and restaurants (38) compared to both later-entry (581, 190, and 26, respectively) and no-entry (456, 152, and 20, respectively) cohorts. Beyond absolute counts, the concentration of restaurants also appears to be a selection factor. In the 2015 data, restaurants constituted a larger share of both all industries (4.8%) and all retailers (14.7%) in the early-entry cohort, compared to the later-entry cohort (4.5% and 13.7%, respectively) and the no-entry cohort (4.4% and 13.2%, respectively). This suggests that FDPs strategically prioritized expansion into markets with a higher density of their core restaurant partners.

The pattern of selection is mirrored in Panel B's real estate metrics: early-entry markets had the highest average asking rents ($21.42/sqft) and sale prices ($208.32/sqft) in 2015. Other metrics of the retail real estate market are more alike. The std deviation of the total inventory and new construction of retail space are higher in all three cohorts, indicating that the quantity is more noisy. Notably, occupancy rates were high and very similar across all three groups (ranging from 95.0% to 95.7%), consistent with the fact that the US retail real estate market was in hot demand back in 2015[24].

These pre-existing differences underscore the necessity of an empirical strategy to account for time-invariant heterogeneity across markets and the strategic expansion of FDPs. Table F.6 in Appendix F provides balance test of city characteristics, such as population density and income. The next Section 4 discusses empirical strategy to address the imbalance.

4 Empirical Strategy

This paper estimates the causal effect of Food Delivery Platform (FDP) availability on local retailers and retail real estate outcomes. Identification comes from the staggered entry of major FDPs across U.S. cities between 2016 and 2024. I define a city as treated starting in the quarter when either DoorDash or Uber Eats first launched services within its boundaries. It is the time point

[24] CBRE market report: Retail occupancy inching closer to pre-recession levels.

when the consumers in the city have another channel to order food. [25]

I employ the staggered difference-in-differences (DiD) estimator proposed by Callaway and Sant'Anna (2021). This approach addresses the biases inherent in traditional two-way fixed effects (TWFE) estimators when treatment timing is staggered and effects are heterogeneous (Goodman-Bacon, 2021). Specifically, it estimates group-time average treatment effects ATT(g, t) by comparing outcomes for observations first treated in period g at time t to outcomes for observations not-yet treated at time t. Appendix E provides more details on TWFE biases.

Formally, in the event study analysis, I use the staggered DiD framework with a clean definition of control group:

$$Y_{\ell ct} = \sum_{e} \beta_e D_{\ell e} + \delta_{\ell c} + \delta_t + \gamma_t X_c + \epsilon_{\ell ct} \tag{5}$$

where $Y_{\ell ct}$ is the outcome in neighborhood ℓ at time t. At the establishment level regression, ℓ represents the zip code area and t represents the year. At the retail real estate market level regression, ℓ represents the submarket and t represents the quarter. In the analysis, I include 3 years before and 5 years after the FDP's introduction or treatment.

The indicator $D_{\ell e}$ equals to 1 if the neighborhood ℓ is e periods relative to FDP entry ($e = t - g$) from the city's FDP launch, and 0 otherwise. The coefficient of interest β_e is the weighted average treatment effect at relative period e. [26] For point estimates of the treatment effect, the staggered

[25] While continuous measures of adoption intensity (search volume) exist, the binary treatment variable is preferred as it circumvents endogeneity challenges associated with the degree of platform adoption and avoids measurement issues arising from the increasing shift towards mobile app usage over web searches for FDP services.

[26] The Callaway and Sant'Anna (2021) approach first estimate the group-time average treatment effects for each treatment cohort g (quarter of FDP entry) and time period t, defined as

$$ATT(g,t) = [EY(g)_t - EY(NT)_t] - [EY(g)_{t-1} - EY(NT)_{t-1}]\,, \text{for } t \geq g.$$

for relative periods before the group g get treated and

$$\text{ATT}(g,t) = [EY(g)_t - EY(NT)_t] - [EY(g)_{g-1} - EY(NT)_{g-1}]\,, \text{for } t \geq g.$$

$Y_t(g)$ is the potential outcome at time t under treatment starting at g, $Y_t(0)$ is the potential outcome under no treatment, and $G_g = 1$ indicates membership in cohort g

The estimated ATT coefficients of relative period e, β_e, is then calculcated as a weighted average of the relevant ATT(g, t) estimates:

$$\beta_e = \sum_{g \in \mathcal{G}: g+e \leq \mathcal{T}} w(g,e) \cdot \widehat{\text{ATT}}(g, g+e)$$

where $\mathcal{G}$ is the set of treatment cohorts, $\mathcal{T}$ is the final time period, $w(g, e)$ are weights based on the share of cohort g among all cohorts observed at event time e, and $\widehat{\text{ATT}}(g, g+e)$ are the estimated group-time effects.

DiD coefficient β is the weighted average of the treatment effect at relative period e, where the weight is the size of the relative period e.

Standard errors are clustered at the city level to account for potential within-city correlation in outcomes.

The key identifying assumption is conditional parallel trends: in the absence of FDP entry, outcomes for neighborhoods treated in period t would be similar to outcomes for neighborhoods in the control group. My baseline specification includes neighborhood fixed effects (δ_ℓ) to absorb time-invariant unobserved heterogeneity at a granular level (zip code for establishment counts, real estate submarket for market outcomes) and time fixed effects (δ_t) to control for aggregate trends and shocks. To account for potential selection bias arising from FDPs strategically entering certain types of cities, and to allow for differential trends based on initial conditions, I include interactions between time fixed effects and baseline (pre-treatment) city characteristics ($\gamma_t X_c$) as covariates in the estimation. This helps ensure that the parallel trends assumption holds conditionally and isolates the effect of FDP entry from pre-existing divergent city trajectories.

Section 6 details robustness checks, including those addressing FDP strategic expansion and model specification. A key consideration is the COVID-19 pandemic within the 2016-2024 study period, which significantly affected FDP usage and retail markets. To disentangle the FDP entry effect from pandemic influences, I confirm the main findings are robust by (1) analyzing only the pre-pandemic period (data before 2020) and (2) incorporating city-level COVID-19 death rates into the main specification (see Section 6.2).

5 Empirical Results

The conceptual framework identifies two competing forces that FDP entry exerts on the retail real estate market. The first is a *Market Expansion Effect*, whereby FDPs, by acting as a new sales channel or by increasing local amenities, may increase the total number of establishments. The second is a *Spatial Reduction Effect*, as FDPs may allow establishments (particularly restaurants) to economize on high-cost space by shifting to smaller, delivery-focused footprints.

However, the net impact of FDPs on the demand for retail real estate is therefore theoretically ambiguous and becomes an empirical question. To resolve this ambiguity, This section first examines the effect of FDP entry on the local retail sector as the foundation of demand shock, then investigates the effect on the retail real estate market.

5.1 Retail Establishments

In this subsection, I test how local retailers respond to the FDP shock, as the microfoundation of change in space demand. I focus on the number and size of retail establishments (tenants) in a neighborhood.

In Data Axle, establishments report their size in categorical bins (e.g., 0-2,500 sqft, 2,500-9,999 sqft, etc.). Therefore, I define "small" establishments as those in the lowest available category (0-2,500 sqft). As a reference, a typical fast-food restaurant is 4,500 sqft.[27] [28]

The regression specification follows Section 4, where neighborhood ℓ represents zip code areas and time t represents years.

Table 2: Effects of FDP Entry on Local Retail Establishments

	(1) ln(retailers)	(2) %(small retailers)	(3) ln(restaurants)	(4) %(small restaurants)
FDP × post	0.021*	0.438	0.036***	0.914**
	(0.013)	(0.316)	(0.012)	(0.391)
Pre-treatment mean	4.582	58.96	2.600	46.95

Notes: This table presents the effects of Food Delivery Platform (FDP) entry on local retail establishments in each neighborhood (zip code area). The dependent variables are: (1) log-transformed count of retailers, (2) share of small retailers (less than 2,500 sqft), (3) log-transformed count of restaurants, and (4) share of small restaurants. All regressions include zip code fixed effects, year fixed effects, and time-varying city-level covariates. Standard errors clustered at the city level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Data Source: Data Axle.

I first examine the broad retail sector in Columns (1) and (2) and find that both the *Market Expansion Effect* and *Spatial Reduction Effect* exist. Column (1) shows a significant 2.1% increase

[27] For example, McDonald's lists a detailed criteria for the size of a restaurant on its website: McDonald's U.S. Site Criteria.

[28] There exists a temporal misalignment between the Data Axle dataset (a July $t-1$ to June t panel for year t) and the monthly treatment timing. This poses a dilemma for classifying observations treated within that 12-month window. The study adopts a conservative methodology: an observation is coded as *treated* only if the treatment occurred before the window began (prior to July $t-1$). Note that this conservative approach can mechanically averages zero-effect (pre-treatment) and true-effect (post-treatment) periods, causing an attenuation bias that biases the coefficient toward zero. So the estimated coefficient is likely to be an underestimation of the true effect.

in the total count of retailers, providing strong evidence that FDPs boost local retail sector. Column (2) indicates a 0.4 percentage point increase in the share of small retailers overall, though this estimate is imprecise. This suggests that while the Spatial Reduction Effect is present, its impact on the aggregate retail sector is muted, likely diluted by the large share of other retailers less affected by FDPs.

I next analyze the restaurant industry (Columns (3) and (4)), which is the sector most directly treated by FDPs and the most likely source of any demand shock.[29] Here, the results are substantially stronger and more precise. FDP entry leads to a 3.6% increase in the total count of restaurants (Column 3). This finding strongly confirms that FDPs complement to restaurants, causing the number of restaurants to increase. Simultaneously, I find evidence for the *Spatial Reduction Effect*: Column (4) shows a significant 0.9 percentage point increase in the share of small restaurants. This is consistent with FDPs facilitating the entry of smaller, delivery-focused operations that require less physical space.

Event study plots for these tests are available in Appendix Figure A.6 and Figure A.7.

Taken together, these results resolve the theoretical ambiguity. Although evidence for both posited forces exists, the large and significant increase in the total count of establishments, driven by restaurants, demonstrates that the *Market Expansion Effect* dominates. Therefore, FDP entry generates a net positive demand shock for retail space. This finding provides the foundation for my main analysis, where I next investigate how this positive demand shock is capitalized into the retail real estate market.

5.2 Retail Real Estate Market Outcomes

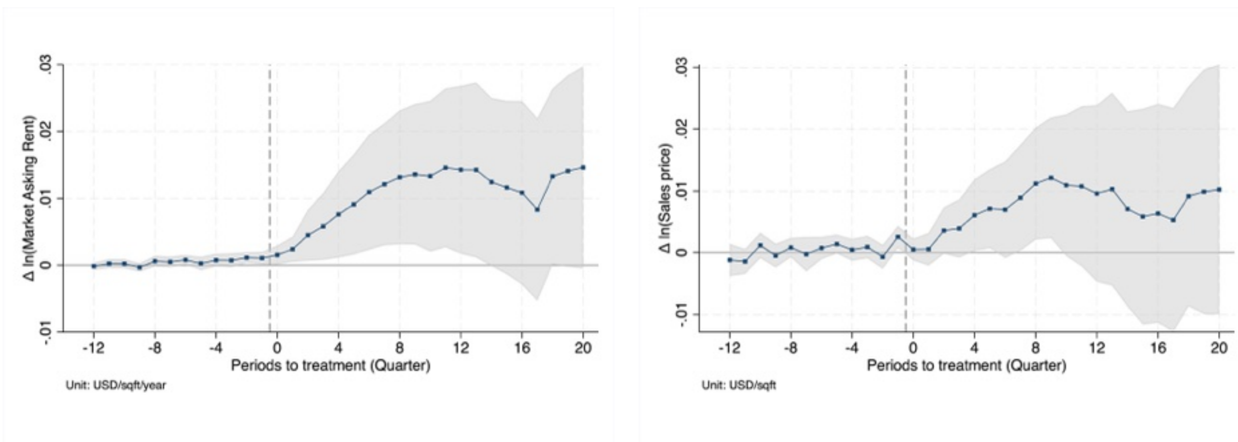
This subsection examines the average effect of food delivery platforms on other retail real estate outcomes at the neighborhood (submarket) level, given that Section 5.1 provides strong evidence that FDPs generate a positive demand shock in the local retail real estate market. The outcome variables for the retail real estate market include quarterly neighborhood-average retail rent, sale price, inventory space, new construction volume, and occupancy rate. To stay consistent with Section 5.1, all outcome variables are measured within a three-year pre-entry and five-year post-

[29] Although FDPs extended their service to the grocery delivery after COVID, I didn't find much response in terms of the number of grocery stores. See Figure A.8 for estimation results.

entry window relative to the platform's market entry.

All dependent variables in this subsection are derived from recently completed lease or sales transactions of properties within the quarter, aggregated to the neighborhood level. This implies that properties not on the market (e.g., those with existing leases) are not included in the calculation of the submarket's average rent. Similarly, properties not sold or leased within the quarter are not included in the calculation of the submarket's average sale price.

Figure 2: Effects of Food Delivery Platform on Rent and Price of Retail Spaces



(a) ln(Rent) (b) ln(Price)

Notes: The plot shows the event study estimates as specified in Section 4. The x-axis reflects event time (quarters) relative to the quarter of first food delivery platform entry (event time zero). The solid blue line is the point estimate, and the shaded area is the 95% confidence interval. Panel (a) plots the effect of platform entry on log-transformed asking rent per square footage in dollars. Panel (b) plots the effect of platform entry on log-transformed sale price per square footage in dollars. All outcome variables are measured at the neighborhood (submarket) level. Standard errors are clustered at the city level.

Figure 2 presents the dynamic effect of platform entry on the rent and transaction price of retail properties (not adjusted for inflation).[30] The x-axis represents the number of quarters since platform entry, where negative numbers represent periods prior to the treatment. To stay consistent with Section 5.1, all outcome variables are measured within a three-year pre-entry and five-year

[30] This paper focuses on *asking rent*, defined as the publicly listed rental price set by landlords. *Asking rent* is preferred because it reflects market conditions and landlord expectations, and serves as the most widely used and comparable metric across different markets. In contrast, *effective rent*—the actual amount paid by tenants—can vary significantly due to differences in payment structures, such as rent-free periods, concessions on management fees, or revenue-sharing agreements, making it less standardized and harder to compare.

post-entry window relative to the platform's market entry; this corresponds to 12 quarters before and 20 quarters after the platform's market entry. The blue line represents the coefficients of interest β_e, with the shaded area representing the 95% confidence interval. The gray dashed line represents the time of treatment.

The parallel trends assumption of the DiD approach appears to hold, as indicated by the insignificant, close-to-zero pre-treatment differences between the treatment and control groups. The pattern of no-pretrends also indicates that factors that could affect the FDPs' strategic expansion are mostly controlled by submarket fixed effects and covariates (Baker et al., 2025). [31]

After platform entry, the estimated coefficients can be interpreted as the ATT in their corresponding quarters. Figure 2 Panel (a) shows that the log-transformed asking rent per square footage in dollars increases significantly after the platform entry. The plot shows the estimated effect (ATT) exhibits a clear upward trend for the first 8 quarters (2 years) after platform entry. After quarter 8, this upward trend ceases, and the effect stabilizes, suggesting a new equilibrium is reached.

Panel (b) shows a similar dynamic for the log-transformed sale price per square footage in dollars. The estimated ATT also indicates an upward trend for the first 8 quarters (2 years) after platform entry. After quarter 8, the estimated treatment effect stabilizes over relative periods to treatment, suggesting a new equilibrium. The estimated coefficients show a wider confidence interval. The average effect over the five-year post-period is an increase of approximately 0.7%. The estimated effect on sale price is visibly noisier than that on rent, which is expected for several reasons. First, sale prices generally adjust more slowly to market changes compared to rents. Second, property sales transactions occur less frequently than lease contracts, resulting in a smaller sample for calculating neighborhood-average prices. Third, sale prices are likely to be influenced by other factors (e.g., interest rates), which introduce additional noise.

[31] The event study plot of Callaway and Sant'Anna (2021) estimates is different from the TWFE event study plot by the handling of the T-1 (pre-treatment) coefficient. A traditional TWFE model must omit one pre-treatment period (typically T-1) as the reference category to avoid perfect multicollinearity. This mechanically forces its coefficient, β_{-1}, to be zero.

Conversely, the default csdid estimator does not omit T-1 for reference. It estimates every pre-treatment coefficient β_e (for $e < 0$) as an independent test by comparing with its previous period. Thus, β_{-1} is an actual estimate by the weighted average of $[EY(g)_t - EY(NT)_t] - [EY(g)_{t-1} - EY(NT)_{t-1}]$, for $t \geq g$.. The ommitted period, using the csdid method, will be tthe earliest period (e.g., β_{-K}) because data from period $-K-1$ is unavailable for the comparison.

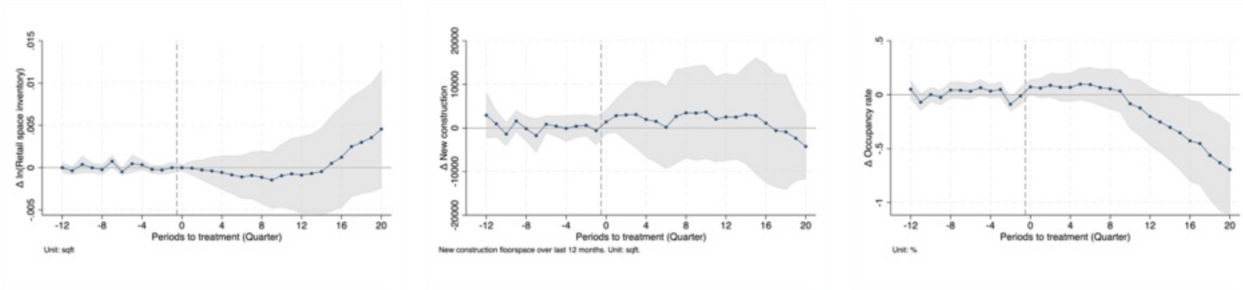
Using the CoStar data, I next examine the quantity response by analyzing three key outcomes: the total inventory of retail space, the floorspace of new construction completed in the past 12 months, and the occupancy rate.

These variables are chosen to capture different dimensions of the market's quantity adjustment. (1) Total retail floor space measures the total stock of available retail space in the submarket. It also includes the conversion from other property types to retail space. (2) Construction in the past 12 months measures the supply of new retail space. Although construction projects face significant lags from permitting to completion, the five-year post-entry window is intended to partially capture this new supply response.[32] (3) Occupancy rate (occupied space divided by total space) measures the share of leased retail space. An increase in this rate would suggest that FDP entry leads to more space being absorbed. This could occur if the positive demand shock makes it optimal for landlords to lease previously vacant or reserved units, thereby overcoming the "option value" of vacancy (Moszkowski and Stackman, 2024). Therefore, this variable is a complementary indicator to the quantity response.

Figure 3 presents the dynamic effect of platform entry on these quantity-related outcomes.

[32] I use the level of construction for this analysis, rather than a log transformation, because the variable contains a significant number of zeros (over 37% of observations). Applying a log transformation would drop these observations and potentially bias the results. Results using an alternative inverse hyperbolic sine (IHS) transformation are reported in the Appendix.

Figure 3: Effects of Food Delivery Platform on Quantity of Retail Spaces



(a) ln(Total retail floor space) (b) New construction (c) Occupancy rate (in %)

Notes: The plot shows the event study estimates as specified in Section 4. The x-axis reflects event time (quarters) relative to the quarter of first food delivery platform entry (event time zero). The solid blue line is the point estimate, and the shaded area is the 95% confidence interval. Panel (a) plots the effect of platform entry on log-transformed total retail floor space. Panel (b) plots the effect of platform entry on construction in the past 12 months. Panel (c) plots the effect of platform entry on occupancy rate (in %). All outcome variables are measured at the neighborhood (submarket) level. Standard errors are clustered at the city level.

Figure 3 presents the dynamic effect of FDP entry on these quantity-related outcomes. In all three panels, the pre-treatment coefficients are statistically insignificant and clustered around zero, which strongly supports the parallel trends assumption. After platform entry, the plot indicates that the quantity of retail space, measured by total stock and new construction, did not show any significant positive change.

Panel (a) shows that for the total inventory of retail space, the estimated effect is statistically insignificant, remaining close to zero although it becomes slightly positive after quarter 14. Panel (b) shows that the new construction effect is also close to zero, suggesting no significant change in new construction activity after platform entry. This finding is notable given the long-run nature of the analysis; the five-year post-treatment window should be sufficient to capture any new construction projects initiated by the platform's entry. Panel (c) indicates that the occupancy rate did not change significantly after platform entry. Given that the pre-treatment occupancy rate was already high (above 95%), this lack of a significant change is not surprising. Moreover, the estimated effect becomes significantly negative after the 14th quarter of treatment, suggesting that the FDP may increase retail vacancy rather than fill previously empty space.

All results, taken together, suggest that although FDP entry leads to a positive demand shock in the retail real estate market, it does not lead to a significant increase in the quantity of retail space, implying that the supply of retail space is highly inelastic to the FDP shock. [33]

Table 3: Effects of FDP Entry on Retail Real Estate Market Outcomes

	(1) ln(rent)	(2) ln(price)	(3) ln(inventory)	(4) new construction	(5) occupancy rate (%)
FDP × post	0.010**	0.007	0.000	1,687	-0.145
	(0.004)	(0.005)	(0.002)	(3,880)	(0.100)
Pre-treatment mean	2.946	5.227	14.809	39,134	95.208

Notes: This table reports the average treatment effect on the treated (ATT) of FDP entry on retail real estate market outcomes in each neighborhood (submarket). The dependent variables are: (1) ln(rent) is the log-transformed value of average annual asking rent per square foot (in USD); (2) ln(price) is the log-transformed value of average sale price per square foot (in USD); (3) ln(inventory) is the log-transformed value of total retail floor space inventory; (4) New construction is the total square footage of new construction completed in the past 12 months; (5) Occupancy Rate is the percentage of occupied retail space. All regressions include neighborhood fixed effects, year fixed effects, and time-varying city-level covariates. Standard errors clustered at the city level are in parentheses. *** $p<0.01$, ** $p<0.05$, and * $p<0.1$. Data Source: CoStar.

Table 3 reports the point estimates of the ATT, complementing the event study analysis. The results show that FDP entry leads to a 1.0% increase in retail rents, which is statistically significant at the 5% level (Column 1). To interpret this magnitude, consider a average sized fast-food restaurant of 4,500 sqft; this effect translates to an annual rent increase of approximately $900. A simple back-of-envelope calculation suggests that this 1.0% rent increase thus represents an aggregate annual transfer from tenants to landlords of approximately $1.77 billion[34], highlighting the substantial market-wide economic significance of FDP entry.

In Column (2), the point estimate for the effect on price is 0.7%. While this effect is not statistically significant at conventional levels (p=0.15), its direction is consistent with the rent finding and trend shown in event study plot in Figure 2. For illustrative purposes only, this point estimate would translate to a price increase of approximately $6,235 for the same property.

Turning to quantity responses, the results indicate that FDP entry does not lead to a significant change in the supply of retail space. The coefficient for total inventory is 0.000 and statistically

[33] Potential explanations for the inelastic supply: (1) The rent and price effect is relatively small, not enough to induce a supply response, which often involves significant fixed costs such as capital investment. (2) Retail properties are located in dense areas, so new development is difficult. (3) Unlike the possible conversion from office to residential usage, retail properties need to have good visibility and accessibility, so conversion from other property types to retail space is limited.

[34] 8.521×10^9(Total ever-treated floor space) $\times$ \$20.78 $\times$ 0.01

indistinguishable from zero (Column 3). The point estimate for new construction (Column 4) is also statistically insignificant and economically small. Finally, the effect on the occupancy rate (Column 5) is a small and insignificant -0.145 percentage points, suggesting that FDP entry does not significantly alter market tightness.

Overall, these findings suggest that FDP entry acts as a positive demand shock that is capitalized into rents, generating substantial aggregate economic value, but does not induce a significant supply-side response in the retail real estate market.

6 Robustness Checks

This section tests the robustness of the findings by the following three aspects: (1) possible selection problem from FDPs' strategic expansion, (2) effect of COVID-19 pandemic, and (3) alternative regression specifications, placebo tests on non-retail sectors, and an examination of the geographic coverage of the FDP (urban vs. suburban).

6.1 Addressing Selection Bias

As the expansion of FDPs across cities is not random, raising potential selection concerns. I solve this problem by several strategies: (i) conditional comparability on fixed effects, (ii) controlling for city characteristics that can affect the FDPs' strategic expansion, and (iii) altering control group definition by excluding never-treated cities.

6.1.1 Conditional Comparability and No Pre-Trends

My primary strategy to address this relies on my staggered difference-in-differences (DID) specification, which I validate in two ways.

First, I examine pre-existing heterogeneity using my main outcome variable, asking rent. Figure A.9 Panel (a) plots the baseline (2015) rent distribution by treatment group.[35] The raw distributions reveal significant level differences between groups. However, once absorbing the neighborhood and time fixed effects used in my main specification, the distribution of residuals for the same period overlap between groups (Figure A.9, Panel (b)). The residual distributions become highly comparable and centered at zero, indicating that my fixed effects structure effectively accounts for pre-existing heterogeneity on the retail space market.

Second, and more importantly, the parallel trends assumption is satisfied as is shown in Figure 2, which plots the dynamic treatment effects from the Callaway and Sant'Anna (2021) estimator. The coefficients for all pre-treatment periods are statistically insignificant and centered around zero, validating the parallel trends assumption. The estimated effects diverge from zero only after FDP entry, suggesting that the treatment and control groups are highly comparable.

[35] For visual clarity, I group cities into three categories: treated before 2020, treated after 2020, and never-treated. But in the regression in the main text, treatment groups are classified by quarter

6.1.2 Controlling for Selection on City Characteristics

While the last subsection shows that the parallel trends assumption is satisfied only by controlling for neighborhood and time fixed effects, the estimation may still be subject to selection bias if FDPs strategically entered cities based on observable characteristics that are also correlated with future retail rent trajectories. For example, FDPs might target cities with high-income, dense populations, which may have been on different rent growth paths irrespective of FDP entry.

To address this concern, I add covariates for several key city-level characteristics that could influence FDP entry decisions and retail real estate outcomes throughout the main analysis. I following the method in Callaway and Sant'Anna (2021); Rios-Avila et al. (2023) to apply inverse propensity score weighting (IPW) to balance pre-treatment characteristics between the treatment and control groups in the main regression. Based on the balance tests detailed in Appendix F, I incorporate the following baseline year city-level covariates into the IPW process: population density, income per capita, and median age.

The comparison between regression specification of adding covariates and only including fixed effects are presented in Appendix Figure A.10. These estimates confirm that the parallel trends assumption holds after conditioning on these key observables, as the pre-treatment coefficients remain statistically indistinguishable from zero. The post-treatment effect remains positive and significant, though its dynamic pattern is slightly attenuated compared to the specification without covariates. This suggests the model without covariates partially conflated the FDP impact with pre-existing, covariate-driven growth trajectories.

By controlling for selection on observable characteristics, the covariate-adjusted model used in the main text provides a more reliable estimate of the FDP effects.

6.1.3 Robustness to Control Group Definition

Lastly, one may still argue that the cities in the "Never Treated" group are systematically different from cities that eventually receive FDP service, making them an invalid control group. While using a never-treated cohort is a standard approach in DiD estimation (Sun and Abraham, 2021), I test the sensitivity of my findings to this specification in Table 4, Column (5). I estimate a model that excludes the never-treated group entirely and find that the treatment effect is stil alike with the main result.

Table 4: Robustness to Alternative Specification

	(1)	(2)	(3)	(4)	(5)
	Main	Different DiD Method			No never-treated
Panel A: Y = log(# retail establishments)					
Platform × post	0.021***	0.027***	0.030***	0.023***	0.005
	(0.013)	(0.010)	(0.011)	(0.008)	(0.011)
Panel B: Y = log(rent per SF)					
Platform × post	0.010**	0.015***	0.016***	0.008***	0.011***
	(0.004)	(0.004)	(0.004)	(0.003)	(0.002)
Neighborhood FE	Y	Y	Y	Y	Y
Time FE	Y	Y	Y	Y	Y

Notes: This table reports robustness checks for the impact of FDP entry using various model specifications. Column (1) is the baseline model. Columns (2)–(4) apply alternative staggered DiD methods: Column (2) uses Sun and Abraham (2021); Columns (3) and (4) apply the ETWFE proposed in Wooldridge (2021), with Column (4) allowing cohort-specific ATT. Column (5) drops the never-treated control group from the sample. The dependent variables are (Panel A) the log number of retail establishments and (Panel B) the log rent per square foot. All specifications include neighborhood and time fixed effects. Clustered standard errors at the city level are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

6.2 Addressing Effect of COVID-19 Pandemic

Admittedly, the COVID-19 pandemic is a major global event that could have influenced the FDP entry and how consumers and businesses adopt the online platform. To disentangle the FDP entry effect from pandemic influences, I confirm the main findings are robust by (1) analyzing only the pre-pandemic period (through 2019Q4) and (2) incorporating county-level COVID-19 death rates interacted with time fixed effects into the main specification.

This confluence of events could bias my main estimates. Therefore, I conduct two robustness checks to disentangle the effect of FDP entry from the pandemic's shock.

First, I test whether the FDP entry effect was present *before* the pandemic. I re-estimate my event study model using only the pre-COVID sample, dropping all observations from 2020 onward. The result is shown in Figure A.11. The pre-treatment trends are parallel, and the post-treatment coefficients show a clear positive and growing impact; in fact, the treatment effect of FDPs appears stronger when the pandemic's negative shock to the retail sector is excluded.

Second, I use the full sample but explicitly control for the pandemic's heterogeneous local impact. I add a time-varying, city-level control for the local COVID-19 death rate, which serves as a proxy for the pandemic's severity and the associated economic disruptions. [36] Note that the COVID-19 death rate can be endogenous to the FDP entry and consumer's tendency of visiting restaurants. The regression results here should only be interpreted as a robustness check for the pandemic's severity and the associated economic disruptions. Panel (b) of Figure A.11 shows the result. Table A.1 shows the regression results for these two robustness checks.

6.3 Additional Robustness Checks

Beyond selection bias and the COVID-19 pandemic, I conduct three additional robustness checks. I perform a placebo test on a non-retail sector, test alternative DiD estimators, and examine the sensitivity of the results to the geographic sample definition.

6.3.1 Placebo Test on Non-Retail Sectors

The model assumes that FDP entry impacts the *retail* sector. And the estimated effect is really the FDP effect, not the total growth of the treated city. To test this, I run a placebo test using an outcome that should not be directly affected by FDPs: the change in the number of financial offices. If my main model is valid and capturing a retail-specific shock, I should find a null effect on this placebo outcome.

Figure A.12 in the appendix shows the event study results for this test. The effect shows that FDPs do not cause the number of financial offices to increase, actually it is a slight negative effect. This successful placebo test strengthens the interpretation that my main finding is specific to the retail sector and not driven by a broader, unobserved trend affecting all commercial property types

[36] The analysis uses cumulative deaths as the primary proxy for pandemic severity. This measure is chosen over case counts or hospitalizations, as those figures are more susceptible to selection biases stemming from local testing availability and differences in residents' healthcare-seeking behaviors.

in the neighborhood.

6.3.2 Robustness to Alternative DiD Estimators

To ensure the findings are robust, I test them against several alternative estimators designed for staggered treatment adoption, which are presented in Table 4.

Column (2) uses the estimator from Sun and Abraham (2021), and Columns (3) and (4) use the extended two-way fixed effects (ETWFE) method from Wooldridge (2021). Across all these alternative specifications, the estimated FDP effect on both retail establishments (Panel A) and market rent (Panel B) remains positive and statistically significant. The consistency of these results reinforces the robustness of my main findings.

Notably, I exclude the conventional two-way fixed effects (TWFE) estimator. As shown by Goodman-Bacon (2021) and Sun and Abraham (2021), the TWFE estimator can be biased or even yield sign-reversed estimates under treatment effect heterogeneity. See Appendix E for more details.

6.3.3 Geographic Sample Definition (Urban vs. Suburban)

A final concern is that my city-level definition based on CBSA boundaries, which may include suburban or rural areas where FDPs do not actually operate. If this were the case, my main estimate could suffer from attenuation bias, as it would average the true effect in active areas with a zero effect in inactive areas.

To address this, I also tried to restrict my sample to include only neighborhoods within Census-defined Urban Areas (UAs), which is the core of each CBSA.[37] When I re-estimate the model on this urban-only sample, the results for both rent and establishment counts are quantitatively similar and remain statistically significant. This suggests my findings are not driven by this specific geographic definition, likely because the CoStar's retail real estate data already effectively excludes most economically inactive rural areas from the baseline sample. The regression results are shown in Appendix Table A.2.

[37] For the 2010 Census, an urban area will comprise a densely settled core of census tracts and/or census blocks that meet minimum population density requirements, along with adjacent territory containing non-residential urban land uses as well as territory with low population density included to link outlying densely settled territory with the densely settled core. Census Bureau's definition of Urban Areas

7 Conclusion

This paper addresses an increasingly important question in urban economics: do online platforms make physical retail space less or more valuable? I show that unlike traditional e-commerce, which shifts activity from local stores to remote warehouses, Food Delivery Platforms (FDPs) act as complements to local retailers. FDPs cause more retailers to enter the market, driving a net positive demand shock for retail space even as individual establishments become smaller. With the supply of retail floorspace being highly inelastic, this new demand is capitalized directly into rents. I find that FDP entry causes a 1.0% increase in retail rents, representing an aggregate annual transfer of approximately $1.77 billion from tenants to landlords. This finding reveals a classic Ricardian rent dynamic on the retail estate market: the economic surplus from the digital innovation is largely capitalized by landlords, the owners of the scarce factor, rather than being retained by the techonogy-adopting retailers. FDPs thus increase the value of retail properties, highlighting the powerful complementarity of space and cyberspace.

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Appendix

A Appendix Figures and Tables

Fig A.1: Examples of Google Trends Interface and Related Local News

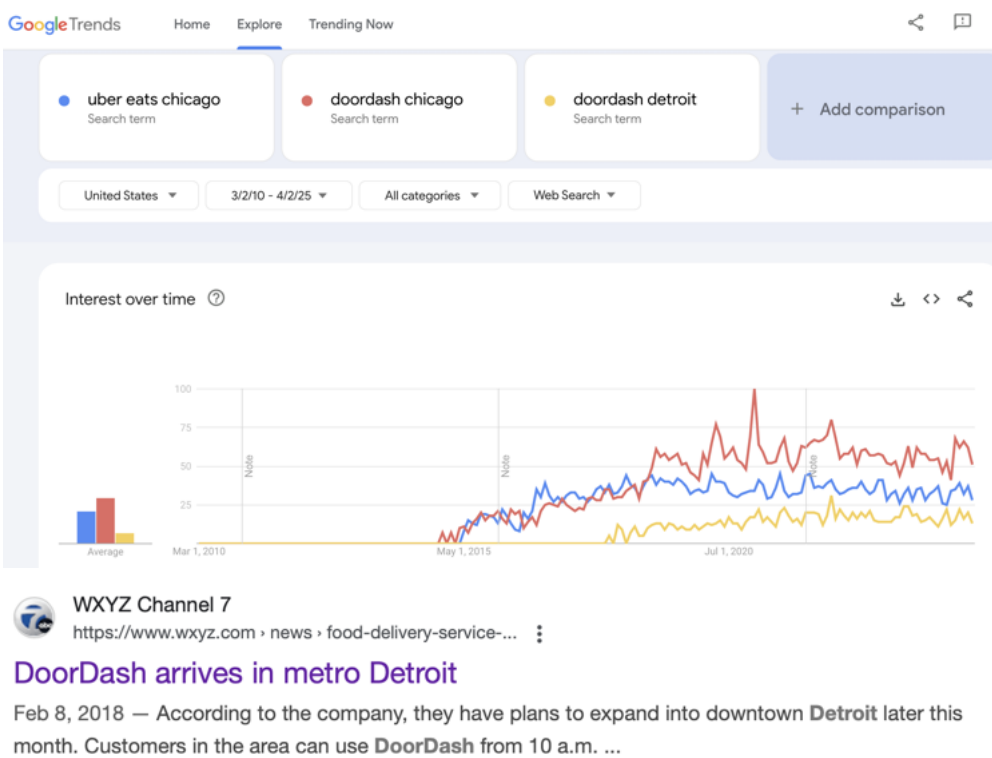


Fig A.2: Example of Door Dash WebArchive Screenshot

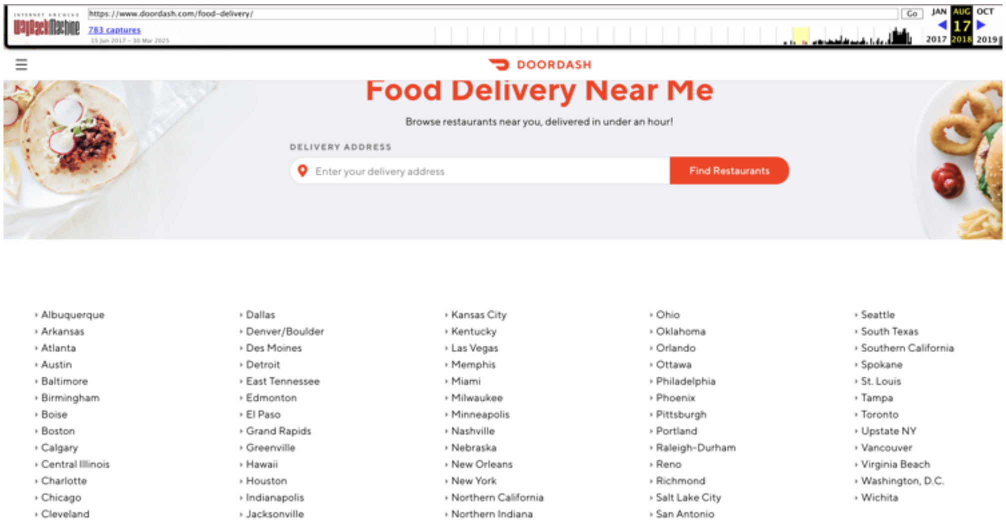
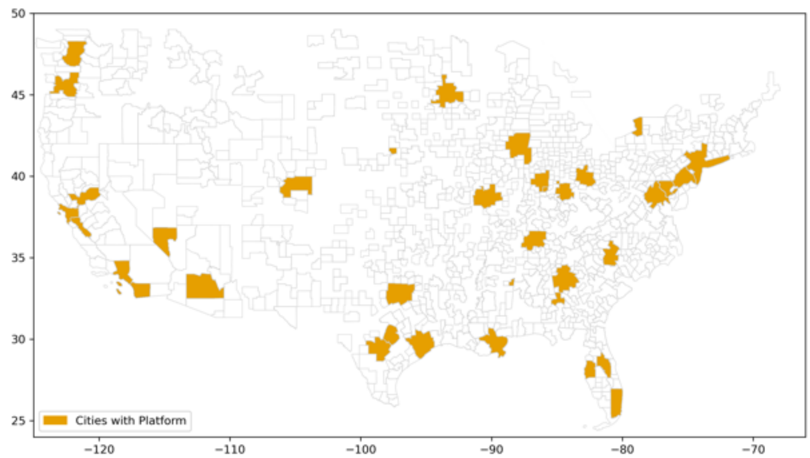
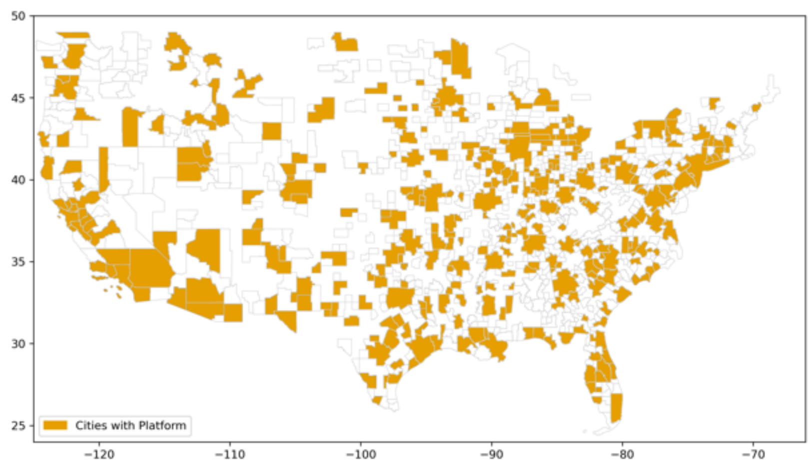


Fig A.3: Example of Cities Covered by Uber Eats

Fig A.4: Maps of Cities Covered by FDPs



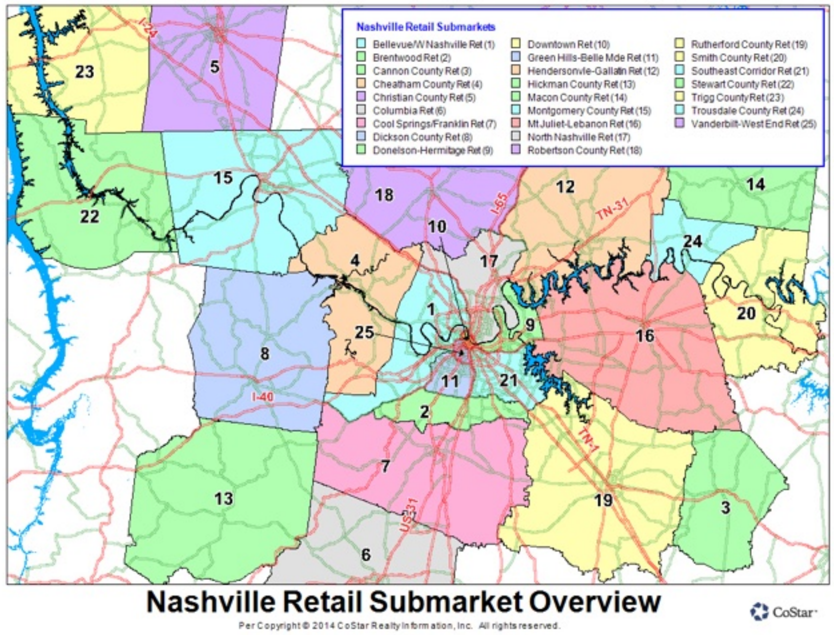
(a) Year 2016



(b) Year 2024

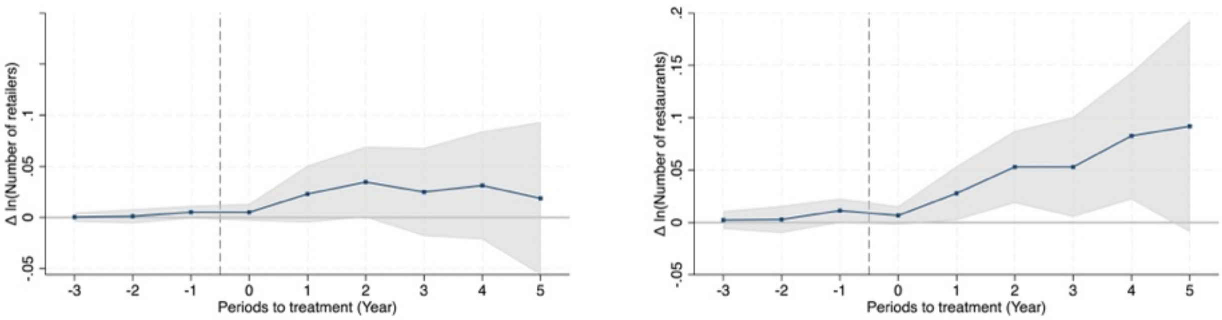
Notes: This figure shows cities covered by FDPs, in year 2016 and 2024. The yellow-shaded areas are cities where residents can order via FDPs, either DoorDash or Uber Eats.

Fig A.5: Example of CoStar Submarkets Boundaries



Notes: This figure shows the submarket boundaries in Nashville. The size of a submarket is similar to the zip code area, but it is slightly larger. Data source: CoStar.

Fig A.6: Effects of FDP Entry on Number of Retailers and Restaurants

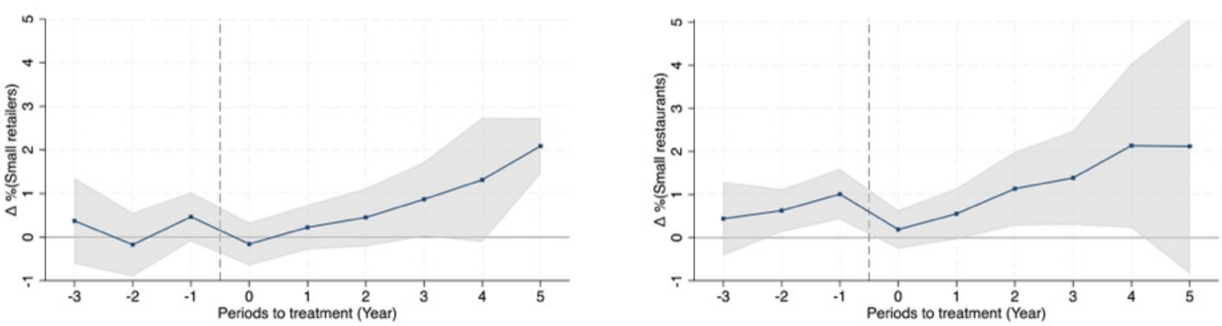


(a) ln(Number of retailers)

(b) ln(Number of restaurants)

Notes: The plot shows the dynamic effect of FDP entry on the number of retailers and restaurants in each neighborhood (zip code area). The x-axis reflects event time (quarters) relative to the quarter of first food delivery platform entry (event time zero). The solid blue line is the point estimate, and the shaded area is the 95% confidence interval. Panel (a) plots the effect of platform entry on ln(number of retailers). Panel (b) plots the effect of platform entry on ln(number of restaurants). All outcome variables are measured at the neighborhood (zip code area) level. Standard errors are clustered at the city level.

Fig A.7: Effects of FDP Entry on Spatial Size of Retail Establishments

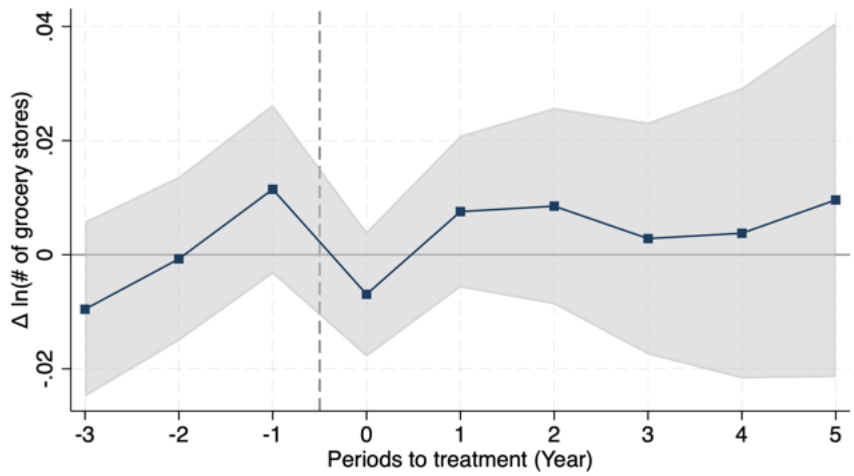


(a) Share of small retailers

(b) Share of small restaurants

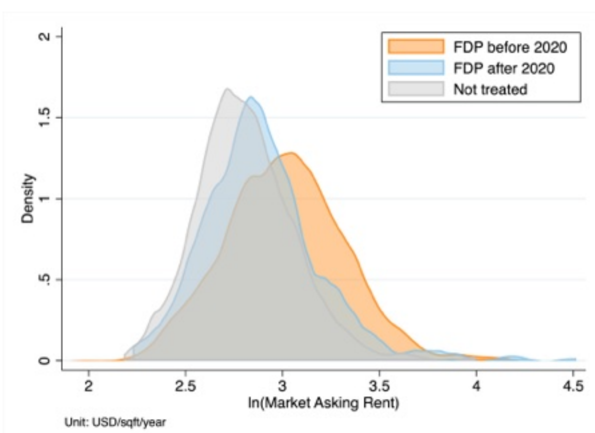
Notes: The plot shows the dynamic effect of FDP entry on the share of small retailers and small restaurants in each neighborhood (zip code area). The x-axis reflects event time (quarters) relative to the quarter of first food delivery platform entry (event time zero). The solid blue line is the point estimate, and the shaded area is the 95% confidence interval. Panel (a) plots the effect of platform entry on share of small retailers. Panel (b) plots the effect of platform entry on share of small restaurants. All outcome variables are measured at the neighborhood (zip code area) level. Standard errors are clustered at the city level.

Fig A.8: Effects of FDP Entry on Number of Grocery Stores

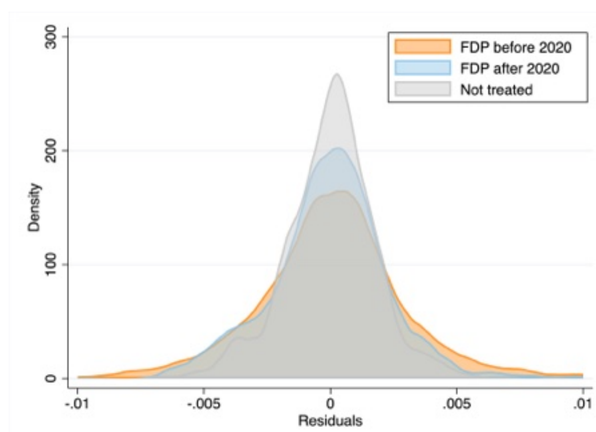


Notes: This figure shows the event study plots of the FDP effect on the number of grocery stores. The blue solid line is the preferred regression result that controls for covariates. The brown dotted line is the event study plot without covariates. The estimated coefficients indicate that FDPs do not have a significant effect on the number of grocery stores.

Fig A.9: Baseline Rent Distribution by Treatment Group



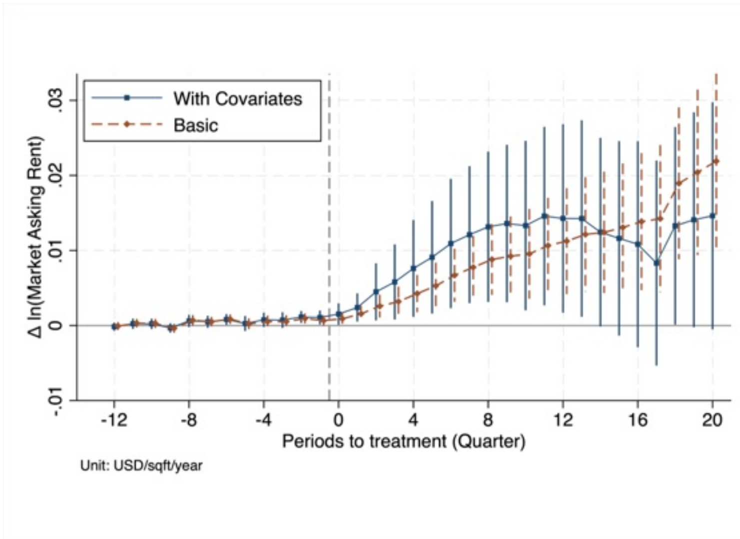
(a) Raw



(b) Control for fixed effects

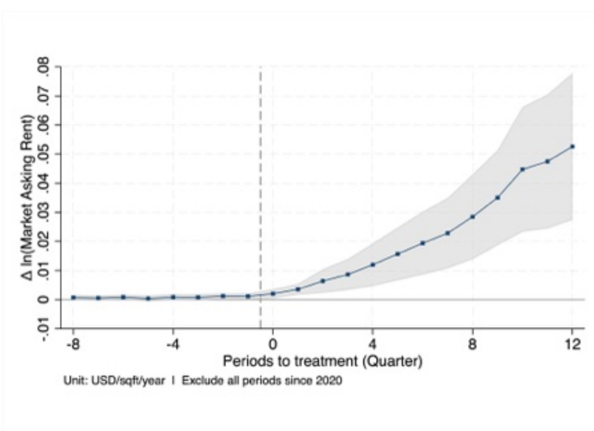
Notes: This figure plots kernel density distributions of the baseline (2015) ln(Market Asking Rent) by treatment status. Panel (a) shows the raw, unadjusted distributions. Panel (b) plots the distribution of residuals for the same baseline period, obtained after absorbing the neighborhood and time fixed effects used in the main specification. For visual clarity, treatment groups are defined as "FDP before 2020" (treated between 2015–2019), "FDP after 2020" (treated post-2020), and "Not treated".

Fig A.10: Comparison of Event Study Plots with/without Covariates

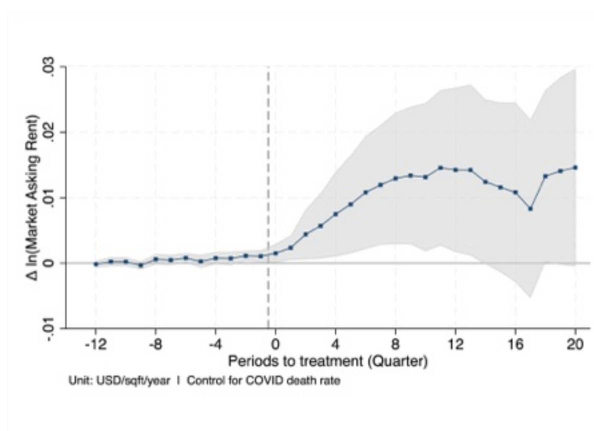


Notes: This figure compares the event study plots on the FDP effect on change of asking rent per square foot. The blue solid line is the preferred regression result that controls for covariates. The brown dotted line is the event study plot without covariates. In both cases, the parallel trend assumption is satisfied, as the pre-treatment coefficients are statistically indistinguishable from zero.

Fig A.11: COVID-19 Robustness Check



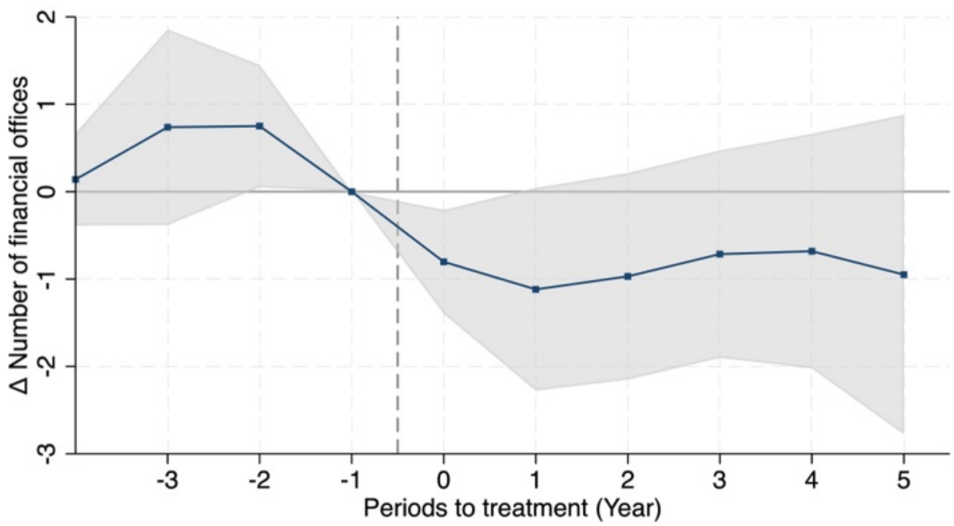
(a) Pre-pandemic periods only



(b) Control for COVID-19 severity

Notes: This figure shows the event study plots of the FDP effect on change of asking rent per square foot for the robustness check with respect to the COVID-19 pandemic. Panel (a) restricts the sample to the pre-pandemic periods only. Panel (b) adds the COVID-19 death rate to the covariates. In both cases, the estimated coefficients are very similar with the baseline result.

Fig A.12: Placebo Event Study: Effect on Number of Financial Offices



Notes: This figure shows the event study plots of the FDP effect on the change in the number of financial offices. The blue solid line is the preferred regression result that controls for covariates. The brown dotted line is the event study plot without covariates. The parallel trend assumption is satisfied, as the pre-treatment coefficients are not statistically indistinguishable from zero.

Tab A.1: Robustness to COVID-19 Pandemic

	(1) Main	(2) pre-COVID	(3) COVID severity
Panel A: Y = log(# retail establishments)			
Platform × post	0.021*	0.024	0.021*
	(0.013)	(0.015)	(0.013)
Panel B: Y = log(rent per SF)			
Platform × post	0.010**	0.017***	0.010**
	(0.004)	(0.005)	(0.004)
Neighborhood FE	Y	Y	Y
Time FE	Y	Y	Y

Notes: This table reports robustness checks for the impact of FDP entry using various model specifications. Column (1) is the baseline model. Column (2) restricts the data to the pre-COVID era (2015–2019). Column (3) add additional control for the COVID-19 severity (accumulative death rate). The estimated coefficients are very similar to the baseline model. The dependent variables are (Panel A) the log number of retail establishments and (Panel B) the log rent per square foot. All specifications include neighborhood and time fixed effects. Clustered standard errors at the city level are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Tab A.2: Robustness to Urban Area Definition

	(1)	(2)
	Main	Urban Area Only
Panel A: Y = log(# retail establishments)		
Platform × post	0.021***	0.019
	(0.013)	(0.013)
Panel B: Y = log(rent per SF)		
Platform × post	0.010**	0.011**
	(0.004)	(0.004)
Neighborhood FE	Y	Y
Time FE	Y	Y

Notes: This table reports robustness checks for the impact of FDP entry using different geographic coverage. The dependent variables are (Panel A) the log number of retail establishments and (Panel B) the log rent per square foot. Column (1) is the baseline model. All other columns have been omitted for brevity, retaining only the main specification. Column (2) restricts the sample to "urban areas", which is the core of each CBSA. All specifications include neighborhood and time fixed effects. Clustered standard errors at the city level are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

B Conceptual Model Details

This appendix provides the complete specification, derivations, and justifications omitted from the main text.

B.1 Detailed Consumer Demand Structure

In each period, local consumers with demand mass M choose between home cooking (the outside option) and the restaurant sector. The decision process is modeled as a three-level hierarchy.

Utility Specification. The indirect utilities associated with the DI and FDP channels are specified as:

$$v_{\text{DI}} = v + A - \ln(P_{\text{DI}}) - T_{\text{DI}} + \epsilon_{\text{DI}} + \epsilon \tag{6}$$

$$v_{\text{FDP}} = v - \ln(P_{\text{FDP}}) - T_{\text{FDP}} + \epsilon_{\text{FDP}} + \epsilon \tag{7}$$

where v is the intrinsic utility from food quality, $A > 0$ is the utility from ambiance (exclusive to DI), and the ϵ terms are idiosyncratic taste shocks (EVT1).

Under the assumption of symmetric restaurants setting prices p_{DI} and p_{FDP},[38] the CES price indices (Stage 3) are:

$$P_{\text{channel}} = \left(N_R \cdot p_{\text{channel}}^{1-\sigma}\right)^{\frac{1}{1-\sigma}} = N_R^{\frac{1}{1-\sigma}} p_{\text{channel}}; \text{ channel} \in \{\text{DI}, \text{FDP}\} \tag{8}$$

The Decision Hierarchy. The attractiveness of the restaurant sector is summarized by the inclusive value, Λ, which aggregates the expected utility from the available channels:

$$\Lambda = e^{v_{\text{DI}}} + e^{v_{\text{FDP}}} \tag{9}$$

1. *Sector Choice.* Consumers choose between the restaurant sector (expected utility $\ln(\Lambda)$) and

[38] Prices p_{DI} and p_{FDP} and equilibrium number of restaurants N_R are endogenous and depend on r.

home cooking (v_0 normalized to 0). The total quantity of restaurant meals demanded is:

$$Q_R = M \cdot \frac{\Lambda}{\Lambda + 1} \tag{10}$$

2. *Channel Choice.* Conditional on choosing the restaurant sector, demand is allocated based on relative utilities:

$$w_{\text{FDP}} = \frac{e^{v_{\text{FDP}}}}{\Lambda} \tag{11}$$

$$w_{\text{DI}} = 1 - w_{\text{FDP}} \tag{12}$$

In the symmetric equilibrium, the delivery quantity share χ equals w_{FDP}.

3. *Restaurant Variety Choice.* In the symmetric equilibrium, the total demand for a channel is equally split among the N_R firms:

$$q_{\text{DI}} = \frac{Q_R \cdot w_{\text{DI}}}{N_R} \tag{13}$$

$$q_{\text{FDP}} = \frac{Q_R \cdot w_{\text{FDP}}}{N_R} \tag{14}$$

B.2 Restaurant Profit and Equilibrium Derivations

The restaurant's flow profit is:

$$\pi = (p_{\text{DI}} - c_{\text{DI}})q_{\text{DI}} + \big((1-\delta)p_{\text{FDP}} - c_{\text{FDP}}\big)q_{\text{FDP}} - F. \tag{15}$$

Given the optimal prices (Lerner's Rule, Eq. (1) in the main text), the per–order profit margins are:

$$(p_{\text{DI}} - c_{\text{DI}}) = \frac{c_{\text{DI}}}{\sigma - 1}, \qquad \big((1-\delta)p_{\text{FDP}} - c_{\text{FDP}}\big) = \frac{c_{\text{FDP}}}{\sigma - 1}.$$

Derivation of the Free Entry Condition (E1). I substitute the profit margins and the equilibrium quantities ((13)–(14)) into the zero-profit condition $\pi = 0$:

$$\frac{c_{DI}}{\sigma - 1}\left(\frac{Q_R w_{DI}}{N_R}\right) + \frac{c_{FDP}}{\sigma - 1}\left(\frac{Q_R w_{FDP}}{N_R}\right) = F \tag{16}$$

Rearranging yields the equilibrium number of restaurants (Eq. (3) in the main text):

$$N_R(r) \;=\; \frac{Q_R}{(\sigma-1)F}\;\underbrace{\Big[c_{\text{DI}}(r)w_{\text{DI}} + c_{\text{FDP}}(r)w_{\text{FDP}}\Big]}_{\equiv\,\bar{c}(r)}. \tag{17}$$

Derivation of Equilibrium Quantity and Size per Restaurant. The equilibrium total orders per restaurant is $q = Q_R/N_R$. Substituting the expression for $N_R(r)$ yields:

$$q(r) = \frac{(\sigma-1)F}{\bar{c}(r)}. \tag{18}$$

A restaurant's store size is $s_R(r) = \bar{\eta}(r)\,q(r)$, using Eq. (2).

B.3 Other Retailers Setup

I introduce a mass M_O of potential "other retailers". An "other retailer" with productivity ϕ drawn from a distribution $G(\phi)$ chooses space s to maximize

$$\pi_O(\phi,s) = \phi s^{\alpha} - rs - F_O, \qquad \alpha \in (0,1),\; F_O > 0.$$

Taking the FOC, its optimal size is $s^*(\phi,r) = (\alpha\phi/r)^{1/(1-\alpha)}$. The zero–profit cutoff (E2) is:

$$\phi^*(r) = \left(\tfrac{F_O}{1-\alpha}\right)^{1-\alpha}\alpha^{-\alpha}\,r^{\alpha}. \tag{19}$$

The number of active other retailers is the mass of firms with productivity above the cutoff:

$$N_O(r) = M_O(1 - G(\phi^*(r))). \tag{20}$$

Since $\phi^*(r)$ is increasing in r, $N_O(r)$ is strictly decreasing in r.

Aggregate other–retailer space:

$$S_O(r) \equiv M_O \int_{\phi \ge \phi^*(r)} s^*(\phi,r)\,dG(\phi) \tag{21}$$

is also strictly decreasing in r.

B.4 Existence and Uniqueness of Equilibrium

Let $H(r) \equiv S_R(r) + S_O(r)$ be the total space demand. I argue that $H(r)$ is continuous and strictly decreasing in r. If $H(0) \geq L$, there exists a unique $r^* > 0$ such that $H(r^*) = L$ (the space clearing condition, Eq. (4)).

Proof Sketch. I know $S'_O(r) < 0$ from Section B.3. I analyze $S_R(r) = Q_R(r)\bar{\eta}(r)$.

1. As r increases, marginal costs c_{ch} rise, leading to higher prices p_{ch}. This generally reduces utility v_{ch} and the inclusive value Λ. Thus, $Q'_R(r) < 0$.

2. The behavior of $\bar{\eta}(r)$ depends on how the channel share w_{FDP} shifts. Since dine-in is more space-intensive ($\eta_d > 0$), c_{DI} depends more strongly on r than c_{FDP} does.

3. Consequently, p_{DI} increases relative to p_{FDP} as r rises. This relative price shift moves demand towards FDP ($w_{FDP} \uparrow$).

4. Since $\bar{\eta} = \eta_k + (1 - w_{FDP})\,\eta_d$, an increase in w_{FDP} reduces average space needs ($\bar{\eta}'(r) < 0$).

Since both Q_R and $\bar{\eta}$ decrease with r, $S'_R(r) < 0$. Thus $H'(r) < 0$, ensuring a unique equilibrium.

□

B.5 Detailed Justification of Hypotheses

H1 Justification (Consumer Welfare and Consumption). The welfare gain stems from two sources. First, the reduction in the delivery access cost (T_{FDP}) directly increases the expected utility from the restaurant sector. Second, as the number of restaurants increases (see H2), consumers gain access to greater variety, which further enhances welfare when the elasticity of substitution $\sigma > 1$.[39] This increased utility naturally leads to higher total consumption of restaurant meals ($Q_R \uparrow$) and the activation of the previously dormant FDP channel.

H2 Justification (Restaurant Entry). The FDP shock raises the overall demand for restaurant meals (Q_R). In the initial equilibrium, this demand shock would lead to positive profits for incumbent restaurants, violating the zero-profit free-entry condition (Eq. (16)). To restore the equilib-

[39] Research on urban consumption amenities shows that access to variety is a primary driver of consumer welfare, often exceeding the benefits of shorter travel times (Couture, 2016).

rium, new restaurants must enter the market until profits are competed back down to zero. This entry process results in a greater number of establishments, N_R.

H3 Justification (Restaurant Downsizing). The change in optimal restaurant size, $s_R = \bar{\eta}q$, is determined by the balance of two opposing effects. (1) A **Spatial Compression Effect (-)**: The shift in consumption towards the less space-intensive delivery channel ($w_{FDP} \uparrow$) reduces the average space required per order ($\bar{\eta} \downarrow$). (2) A **Market Expansion Effect (+)**: Higher total demand ($Q_R \uparrow$) increases the number of orders per firm, q, which pushes for a larger store size. However, $Q_R \uparrow$ also accompany with $N_R \uparrow$ that dilutes the growth of market share per restaurant. Therefore, the spatial compression effect dominates, leading to a net reduction in establishment size.

H4 Justification (Ambiguous Effect on Rent). The equilibrium rent is determined by the total demand for retail space. The ambiguity arises from the net impact on the total space demanded by restaurants, $S_R = Q_R\bar{\eta}$. The FDP shock increases total consumption ($Q_R \uparrow$) but decreases the space intensity per meal ($\bar{\eta} \downarrow$). If the market expansion effect on Q_R is stronger than the spatial compression effect on $\bar{\eta}$, total space demand S_R will rise, pushing rent up. If the compression effect dominates, S_R will fall, and rent will decrease.

H5 Justification (Total Retailer Entry). The change in the total number of retailers ($N_{Total} = N_R + N_O$) is a compositional effect. The model predicts a direct and primary increase in the number of restaurants ($N_R \uparrow$, per H2). The change in other retailers (N_O) is a secondary effect, mediated by the ambiguous rent change (H4). This hypothesis posits that the primary entry effect in the restaurant sector is of a larger magnitude than any potential displacement (or entry) of other retailers, resulting in a net increase in the total establishment count.

H6 Justification (Average Establishment Downsizing). Similar to the total number of retailers, the change in average size ($\bar{s} = (S_R + S_O)/(N_R + N_O)$) is driven by a compositional effect. There is a primary and direct downsizing of restaurants ($s_R \downarrow$, per H3). The change in the size of other retailers (s_O) is a secondary response to rent changes. The hypothesis posits that the strong downsizing effect within the growing restaurant sector will dominate any changes in the other retail sector, leading to an overall decrease in the average size per establishment.

B.6 Notation

Symbol	Meaning
Consumer Demand	
M	Local demand mass (population/daytime population).
$v_{\text{DI}}, v_{\text{FDP}}$	Indirect utilities of dine–in and platform delivery, see (6)–(7).
$P_{\text{DI}}, P_{\text{FDP}}$	Variety-adjusted CES Price Indices for dine-in and delivery.
v	Intrinsic utility from food quality (common across channels).
A	Utility from ambiance (exclusive to dine–in channel).
$T_{\text{DI}}, T_{\text{FDP}}$	Generalized access costs for dine–in and delivery channels.
ϵ	Idiosyncratic taste shocks (EVT1 distribution).
Λ	Inclusive value of the restaurant sector (endogenous): $\Lambda = e^{v_{\text{DI}}} + e^{v_{\text{FDP}}}$.
Q_R	Total quantity of restaurant meals demanded, see (10).
$w_{\text{DI}}, w_{\text{FDP}}$	Channel shares (conditional on choosing restaurant sector).
$\sigma > 1$	Elasticity of substitution across restaurants (CES).
N_R	Number of restaurants in the neighborhood.
$p_{\text{DI}}, p_{\text{FDP}}$	Consumer prices (dine–in and delivery), see (1).
Restaurants	
c	Non–space marginal cost per order.
$\eta_k, \eta_d > 0$	Space per order: kitchen and dining components.
$c_{\text{DI}}, c_{\text{FDP}}$	Channel–specific marginal costs: $c_{\text{DI}} = c + r(\eta_k + \eta_d)$; $c_{\text{FDP}} = c + r\eta_k$.
$\bar{c}(r)$	Weighted average marginal cost, see (3).
δ	Platform commission on delivery revenue (ad–valorem).
$\mu = \sigma/(\sigma - 1)$	Restaurant markup factor.
F	Fixed operating cost per restaurant.
$q_{\text{DI}}, q_{\text{FDP}}$	Per–restaurant demand via dine–in and delivery, see (13)–(14).
q	Total per–restaurant orders; $q = q_{\text{DI}} + q_{\text{FDP}}$, see (18).
$\chi \in [0, 1]$	Delivery quantity share: $\chi = q_{\text{FDP}}/q$ (equals w_{FDP} in equilibrium).
$\bar{\eta}$	Average space per order, $\bar{\eta} = \eta_k + (1 - \chi)\eta_d$, see (2).

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Symbol	Meaning
s_R	Store size (space) per restaurant, $s_R = \bar{\eta}\, q$.
π	Flow profit per restaurant, see (15).
Other Retailers	
M_O	Mass of potential other retailers.
ϕ	Productivity of an ”other retailer”; $G(\phi)$ its CDF.
$\alpha \in (0, 1)$	Curvature parameter for other retailers’ production function.
F_O	Fixed operating cost per other retailer.
$\phi^*(r)$	Zero–profit productivity cutoff for other retailers, see (19).
Market Equilibrium	
L	Fixed neighborhood retail space supply.
r (or r^*)	Equilibrium neighborhood rent per unit of space (bid rent).
N_R	Number of active restaurants, see (3).
N_O	Number of active other retailers, see (20).
N_{Total}	Total number of retailers ($N_R + N_O$).
$S_R(r)$	Aggregate retail space used by restaurants: $S_R = Q_R\bar{\eta}$.
$S_O(r)$	Aggregate space used by other retailers (strictly decreasing in r).
$H(r)$	Total space demand function: $H(r) = S_R(r) + S_O(r)$.

C Retail Real Estate Tenants by Industry

Tab C.4: Potential Tenant for Retail Real Estate by US Industry Sector

NAICS code	Definition	Retail real estate
11	Agriculture, Forestry, Fishing and Hunting	No
21	Mining, Quarrying, and Oil and Gas Extraction	No
22	Utilities	No
23	Construction	No
31-33	Manufacturing	Mixed
42	Wholesale Trade	No
44-45	Retail Trade	Yes
48-49	Transportation and Warehousing	Mixed
51	Information	Mixed
52	Finance and Insurance	No
53	Real Estate and Rental and Leasing	Mixed
54	Professional, Scientific, and Technical Services	Mixed
55	Management of Companies and Enterprises	No
56	Administrative and Support and Waste Management and Remediation Services	Mixed
61	Educational Services	Mixed
62	Health Care and Social Assistance	No
71	Arts, Entertainment, and Recreation	Mixed
72	Accommodation and Food Services	Yes
81	Other Services (except Public Administration)	Mixed
92	Public Administration	No

Note: For sectors exhibiting mixed demand for retail real estate, Table C.5 identifies specific industries at the 6-digit NAICS code level that are likely to require retail spaces, rather than facilities dedicated to manufacturing, storage, or office use.

Tab C.5: Potential Tenant for Retail Real Estate among Mixed Sectors

Sector	**Industry**	**NAICS industry title**
31		*Manufacturing*
	311811	Bread and Bakery Product Manufacturing
48-49		*Transportation and Warehousing*
	491110	Postal Service
	492110	Couriers and Express Delivery Services
	492210	Local Messengers and Local Delivery
51		*Information*
	512131	Motion Picture Theaters (except Drive-Ins)
	517312	Wireless Telecommunications Carriers (except Satellite)
52		*Finance and Insurance*
	522110	Commercial Banking
	522390	Other Activities Related to Credit Intermediation
53		*Real Estate and Rental and Leasing*
	531210	Offices of Real Estate Agents and Brokers
	532120	Truck, Utility Trailer, and RV (Recreational Vehicle) Rental and Leasing
	532210	Consumer Electronics and Appliances Rental
	532281	Formal Wear and Costume Rental
	532282	Video Tape and Disc Rental
	532283	Home Health Equipment Rental
	532284	Recreational Goods Rental
	532289	All Other Consumer Goods Rental
54		*Professional, Scientific, and Technical Services*
	541213	Tax Preparation Services
	541921	Photography Studios, Portrait
	541940	Veterinary services

continued

Potential Tenant for Retail Real Estate among Mixed Sectors (continued)

Sector	Industry	NAICS industry title
56		*Administrative and Support and Waste Management and Remediation Services*
	561439	Other Business Service Centers (including Copy Shops)
	561510	Travel Agencies
61		*Educational Services*
	611610	Fine Arts Schools
	611620	Sports and Recreation Instruction
	611691	Exam Preparation and Tutoring
	611699	All Other Miscellaneous Schools and Instruction
71		*Arts, Entertainment, and Recreation*
	713120	Amusement Arcades
	713290	Other Gambling Industries
	713940	Fitness and Recreational Sports Centers
	713950	Bowling Centers
	713990	All Other Amusement and Recreation Industries
81		*Other Services (except Public Administration)*
	811111	General Automotive Repair
	811114	Specialized Automotive Repair
	811121	Automotive Body, Paint, and Interior Repair and Maintenance
	811122	Automotive Glass Replacement Shops
	811191	Automotive Oil Change and Lubrication Shops
	811192	Car Washes
	811198	All Other Automotive Repair and Maintenance
	811210	Electronic and Precision Equipment Repair and Maintenance

continued

Potential Tenant for Retail Real Estate among Mixed Sectors (continued)

Sector	Industry	NAICS industry title
	811310	Commercial and Industrial Machinery and Equipment (except Automotive and Electronic) Repair and Maintenance
	811411	Home and Garden Equipment Repair and Maintenance
	811412	Appliance Repair and Maintenance
	811420	Reupholstery and Furniture Repair
	811430	Footwear and Leather Goods Repair
	811490	Other Personal and Household Goods Repair and Maintenance
	812111	Barber Shops
	812112	Beauty Salons
	812113	Nail Salons
	812191	Diet and Weight Reducing Centers
	812199	Other Personal Care Services
	812210	Funeral Homes and Funeral Services
	812220	Cemeteries and Crematories
	812310	Coin-Operated Laundries and Drycleaners
	812320	Drycleaning and Laundry Services (except Coin-Operated)
	812331	Linen Supply
	812332	Industrial Launderers
	812910	Pet Care (except Veterinary) Services
	812921	Photofinishing Laboratories (except One-Hour)
	812922	One-Hour Photofinishing
	812930	Parking Lots and Garages
	812990	All Other Personal Services

* Includes law firms and consumer-oriented lawyer offices.

D Major FDPs and Their Market Share

This paper focuses on DoorDash and Uber Eats, the two most widely used food delivery platforms (FDPs) in the United States, which together account for over 90% of the market share. DoorDash launched in 2013 and Uber Eats followed in 2014, though both initially operated in only a limited number of cities. The study period from 2016 to 2024 captures the majority of the expansion phase for both platforms.

Other major FDPs include Grubhub, Postmates, and Instacart. Among these, Postmates was acquired by Uber in 2020 and has since been integrated into the Uber Eats platform. Instacart, while significant, focuses primarily on grocery delivery rather than ready-to-eat meals and is therefore outside the scope of this study.

Grubhub presents a unique case and warrants further explanation as to why its entry timing is not used as a marker for FDP availability in this study. Founded in 2004 and merged with Seamless in 2013, Grubhub predates most of its competitors. However, it differs significantly from DoorDash and Uber Eats in both expansion strategy and business model.

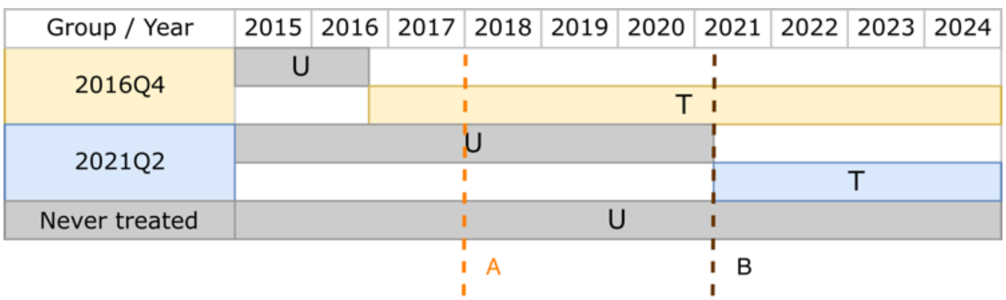
First, Grubhub's expansion relied heavily on mergers with local delivery services rather than deploying a centralized launch strategy. This approach often did not introduce delivery services to previously underserved areas, particularly suburban regions, but instead digitized existing restaurant delivery operations. Second, Grubhub's original model did not utilize a large freelance driver network. Instead, it partnered with restaurants that had their own delivery staff, serving mainly as an ordering platform. In contrast, DoorDash and Uber Eats established more vertically integrated logistics operations, fulfilling nearly all orders through their own driver networks.

These structural differences have had long-term implications. Grubhub's model has proven less scalable and less competitive, resulting in a significant loss of market share over time. This further justifies the exclusion of Grubhub as a primary focus in the current study.

E Bias of TWFE Estimator in the Dynamic Heterogeneous Treatment Effect Setting

The traditional Two-Way Fixed Effects (TWFE) model is inappropriate for staggered DiD settings with heterogeneous treatment effects because it relies on an implicit assumption that treatment effects are homogeneous across groups and time. When this assumption fails, the TWFE estimator wrongly apply early-treated observations in the control group, inducing the "forbidden comparisons" that cause bias (Goodman-Bacon, 2021; Sun and Abraham, 2021).

Fig E.13: Staggered Treatment Timing



This figure illustrates the problem:

- **Setup:** There are three groups: an early-treated group (2016Q4, yellow), a late-treated group (2021Q2, blue), and a never-treated group (gray).
- **At Time A:** The 2016Q4 group is treated, while the 2021Q2 and Never-treated groups serve as clean controls. A simple DiD comparison here is valid.
- **At Time B:** The 2021Q2 group (blue) becomes newly treated. A valid DiD estimator would use *only* the Never-treated group (gray) as its control.
- **Forbidden comparisons:** The TWFE model, by design, pools all observations. When estimating the effect for the 2021Q2 group (blue) at Time B, it implicitly uses *both* the Never-treated group (gray) and the *already-treated* 2016Q4 group (yellow) as controls.

This is problematic because the 2016Q4 group is not a valid control; it has been treated since 2017. If its treatment effect is dynamic (i.e., changing over time), comparing the newly-treated blue group to the already-treated yellow group does not isolate the treatment effect for the blue group. The resulting TWFE coefficient is a weighted average of valid DiDs (blue vs. gray) and invalid, “forbidden” DiDs (blue vs. yellow).[40]

This figure thus reflects that the idea of control groups must vary over time: The 2021Q2 group is a valid control for the 2016Q4 group (pre-2021), but only the Never-treated group is a valid control for the 2021Q2 group.

The estimator proposed by Callaway and Sant’Anna (2021) resolves this. It explicitly estimates group-time average treatment effects ($ATT(g,t)$) by comparing each treated group (g) *only* to a clean, well-defined control group (units not yet treated or never treated). This avoids the contamination from forbidden comparisons that plagues the TWFE model.

[40] A research note from Mingze Gao provides helpful insights on this issue: Difference-in-Differences Estimation.

F Factors that Influence FDP Expansion and Balance Tests

Drawing from the urban and industrial organization literature, I identify several key city-level factors that may influence an FDP's decision to enter a market. These factors could independently affect retail real estate trends, making it crucial to control for them. My chosen covariates are as follows:

- **Urban Form and Density:** The operational efficiency and potential profitability of an FDP are heavily influenced by the size and density of a market. I measure this using the natural log of the total population (`census_total_pop_ln`), population density (`census_pop_density`), and the percentage of commuters who drive (`census_comm_drive`), reflecting the accessibility of the market as well as the potential for platform-supported delivery.
- **Local Economic Conditions:** A city's economic health affects both consumer disposable income and the vitality of the restaurant sector. I use the natural log of total GDP (`ln_gdp`) and the natural log of per-capita income (`census_percapita_inc_ln`).
- **Demographics:** The speed of technology adoption can vary across different demographic groups. I include the percentage of the population with at least a high school diploma (`census_hs_grad_plus_percent`) and the median age (`census_median_age`).
- **Regulatory Environment:** Local regulations can impact the operating costs of FDP services. I include an indicator for whether the state-level minimum wage is above the federal level (`min_wage_state`) and an indicator for the presence of a fee cap (`fee_cap`).[41]

Table F.6 presents the mean and standard deviation for each covariate across the three groups, alongside the **standardized mean difference** (also known as the normalized difference) between the adoption cohorts and the never-treated group. The standardized mean difference is a scale-invariant measure of the difference between two group means, expressed in units of pooled standard deviation. I follow the common heuristic (e.g., Imbens and Wooldridge, 2009) of flagging any standardized difference with an absolute value greater than 0.3 as a potential sign of meaningful imbalance that warrants inclusion in my main DiD model. I opt for standardized mean differences

[41] While the fee cap is a post-treatment measure for some cities, it serves as the most direct available proxy for a city's general regulatory posture towards FDPs. Its inclusion does not materially alter my findings.

over t-tests because my objective is to assess the *magnitude* of the imbalance, not its *statistical significance*. In large samples, such as those common in panel data, t-tests are misleading. They have high statistical power and may flag trivial, economically insignificant differences as statistically significant (i.e., $p < 0.05$). Conversely, in small samples, t-tests may lack the power to detect meaningful differences. The standardized mean difference provides a scale-invariant metric of magnitude that is independent of sample size, making it a more appropriate diagnostic for judging the potential for selection bias (Caliendo and Kopeinig, 2008; Imbens and Wooldridge, 2009).

Tab F.6: Covariate Balance Test

	Group 1: < 2020		**Group 2:** ≥ 2020		**Group 0: Not Observed**	
Variable	Mean	(SD)	Mean	(SD)	Mean	(SD)
`census_total_pop_ln`	12.78	(1.24)	11.83	(0.97)	11.01	(0.74)
`ln_gdp`	16.62	(1.37)	15.52	(1.02)	14.77	(0.78)
`census_pop_density`	90.68	(72.13)	61.48	(80.99)	32.99	(28.83)
`census_comm_drive`	89.88	(4.04)	90.95	(4.03)	90.79	(4.20)
`census_hs_grad_plus`	87.21	(6.12)	86.79	(5.22)	85.25	(6.79)
`census_median_age`	37.14	(3.98)	38.24	(4.62)	38.91	(5.31)
`census_percapita_inc_ln`	10.17	(0.18)	10.11	(0.18)	10.05	(0.19)
`min_wage_state`	0.41	(0.49)	0.39	(0.49)	0.39	(0.49)
`fee_cap`	0.15	(0.36)	0.06	(0.24)	0.07	(0.26)
	Std. Mean Diff. (1 vs 0)			**Std. Mean Diff. (2 vs 0)**		
`census_total_pop_ln`	1.95			1.02		
`ln_gdp`	1.88			0.89		
`census_pop_density`	1.29			0.61		
`census_comm_drive`	-0.22			0.04		
`census_hs_grad_plus`	0.30			0.24		
`census_median_age`	-0.36			-0.13		
`census_percapita_inc_ln`	0.63			0.32		
`min_wage_state`	0.05			0.01		
`fee_cap`	0.27			-0.04		

The results show significant pre-treatment differences. The presence of this systematic sorting is critical. It suggests that FDPs did not enter cities randomly; they first entered larger, wealthier, and denser markets. This finding underscores the importance of controlling for these observable differences to avoid biasing my estimate of the FDP entry effect.

As I established, my heuristic for imbalance is a standardized mean difference with an absolute value greater than 0.3. Table F.6 shows that five variables meet this criterion: `census_total_pop_ln`

(1.95), `ln_gdp` (1.88), `census_pop_density` (1.29), `census_percapita_inc_ln` (0.63), and `census_median_age` (-0.36).

To control for this imbalance while avoiding collinearity between closely related variables, I select the following three covariates for the IPW process and also ensure that these covariates satisfy the common support requirement.

1. **`census_pop_density`**: I select this variable to control for market size and density. While `census_total_pop_ln` also shows high imbalance, it measures a similar concept, and population density is a more precise measure of the urban form relevant to FDP operations.

2. **`census_percapita_inc_ln`**: I select this variable to control for local economic conditions. I choose it over the highly imbalanced `ln_gdp` because per-capita income is a more direct proxy for consumer disposable income than total city GDP.

3. **`census_median_age`**: This variable's imbalance (SMD = -0.36) exceeds the threshold, and I include it to control for demographic differences.