Theory of the Firm in the Platform Economy

by

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Dedication

To my family and my wife

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Abstract

Platforms are often considered as a new organizational form, yet the theoretical connection between platforms and the theories of the (traditional) firms is unclear. A unique feature of platforms is their strong reliance on complementors. While this enables the platform to save the cost of acquiring resources, it also deprives it of its control rights over complementors. Weak control rights over complementors enable complementors to operate on multiple platforms. This makes complementors' engagement interdependent and creates potential spillover effects across platforms and businesses. In three inter-related studies, the dissertation explores how platforms' strategic choices influence complementor engagement across platforms and businesses. Study I investigates how platform access restrictions affect complementor engagement with the focal platform and competing platforms. The study argues that restricting a complementor's access on a platform may prevent a complementor from achieving scope economies that arise from sharing resources across platforms, thereby incentivizing it to abandon both restricted and unrestricted platforms. Study II examines how a platform firm's diversification into a new business influences complementor engagement with the existing business. The study argues that platform diversification provides complementors the opportunities to share and reallocate resources between the existing and new businesses, thereby diverting complementors away from both the diversifying and competing platform firms in the existing business. Study III explores how a platform acquisition influences complementor engagement with the target platform. The study argues that complementors on the target platform incur adjustment costs after the acquisition, which may escalate with the number of complementors on the acquiring platform and force complementors on the target platform to leave the merged platform. I test my predictions in the context of rideshare and food delivery businesses. The dissertation highlights the importance of accounting for interdependencies across complementor activities when platforms make strategic choices.

Chapter 1
Introduction

Since the turn of the century, platforms have become the nexus of technological and economic exchanges in many industries (Davis & Sinha, 2021; Gawer & Cusumano, 2002; Lamberson & Page, 2018; Seamans & Zhu, 2014).[1] Despite the heightened interest in platforms, the extent to which theories of the (traditional) firm apply to platforms or not is unclear. This dissertation aims to extend the understanding of the platform economy by incorporating various strands of literature on the theory of the firm.[2]

Compared with traditional firms, platforms are unique in their strong reliance on complementors, or providers of complementary products/services built on a platform (Brandenburger & Nalebuff, 2011; Gawer & Cusumano, 2002). Relying on complementors enables the platform to save the cost of acquiring complementors' resources and harness network effects, thereby creating a competitive advantage. For example, Airbnb, the largest accommodation provider, does not own any real estate, and Uber, the largest rideshare company, does not own a single vehicle (Goodwin, 2015).

At the same time, reliance on complementors deprives the platform of its control rights over complementors and ownership rights over complementors' resources (Jacobides et al.,

[1] I define platforms as products, services, or technologies that act as intermediaries and facilitate transactions between complementors and users (Gawer & Cusumano, 2002). Organizations that operate and provide platforms to users and complementors are referred to as platform firms (Boudreau & Hagiu, 2009; Eisenmann et al., 2009).
[2] In the dissertation, I mainly focus on transaction platforms, where transactions mainly occur offline and complementors' resources are mostly constrained. I discuss the application of my theory to other types of platforms (e.g., innovation platforms) in Chapter 5.

2018; Kretschmer et al., 2022). This unique feature creates challenges in applying traditional theories of the firms to platforms. In particular, the resource-based view (RBV) considers resources as the source of competitive advantage and argues that firms need to *own* and *control* resources to achieve superior performance. For example, Barney (1991: 101) defined resources as "assets … *controlled* by a firm that enables the firm to conceive of and implement strategies." Similarly, Wernerfelt (1984: 172) defined resources as "(tangible and intangible) assets which are *tied semi-permanently* to the firm." Lastly, Amit & Schoemaker (1993: 35) defined resources as "stocks of available factors that are *owned or controlled* by the firm." Ownership and control rights over resources enable firms to reconfigure resources, which empowers firms to accumulate firm-specific resources over time and build competitive advantages (Dierickx & Cool, 1989; Peteraf, 1993).[3]

While prior studies have shown that firms can still create competitive advantages without possessing full control over resources through alliances with other firms (Ahuja et al., 2009; Gulati & Singh, 1998; Lavie, 2006), the number of firms involved in alliances has been limited. That is, the size of alliances is bounded by firms' finite resources to manage alliance relationships (e.g., managers' cognitive constraints). In addition, such relationships tend to be long-term and are often reinforced by formal contracts among a selective group of firms to prevent the loss of valuable resources to alliance partners (Gulati, 1995; Kale et al., 2000).

In contrast, the competitive advantage of platforms stems from attracting a large pool of complementors to generate network effects (Gawer & Cusumano, 2002; Rochet & Tirole, 2003). To save the cost of acquiring complementor resources, platforms often forego ownership of these

[3] In a similar vein, transaction cost economics argue that, because relationship-specific investments are exposed to partners' opportunistic behaviors and contractual hazards (Coase, 1937; Williamson, 1975), firms should internalize or build long-term relationships with partners to mitigate risks and reduce uncertainty (Demsetz, 1974).

resources (Boudreau, 2010). This causes platforms to lose ownership-based control rights (e.g., authority) over complementor resources. Moreover, to achieve diversity in complement offerings, platforms grant complementors a high level of autonomy in how complements are produced or designed and avoid using long-term contracts that could bind complementors to specific quantity, quality, or price requirements (Kretschmer et al., 2022). Consequently, a platform is restricted from exercising contract-based control rights over complementor resources. Ironically, the efforts to harness network effects force platforms to form relationships with countless complementors and deprive the platforms of their ownership- or contract-based control rights over complementors.

Figure 1 exhibits different governance forms with varying levels of control over resources. On the left side, hierarchy possesses the strongest level of control over resources through ownership, long-term employment, and fiat (Williamson, 1975, 1985). On the right side, market with spot contracts exhibits the weakest level of control over resources. While both alliances and platforms are located in the middle of the hierarchy-market continuum, platforms involve a much larger number of inter-firm relationships (with complementors) that cannot be managed by repeated, long-term formal contracts. This weakens platforms' control over (complementors') resources, thereby pushing platforms towards a governance form that is closer to the market side.

Insert Figure 1 about here

The weak control rights over complementors prevent platforms from forbidding complementors from operating on multiple platforms. In turn, complementors are often "shared" across different platforms or businesses. For instance, game developers can affiliate with multiple platforms (Android and iOS platforms in the smartphone O/S business) or with multiple businesses (PC game and video game businesses). Similarly, a driver can work on multiple platforms (Uber and Lyft in the rideshare business) or on multiple businesses (rideshare and food delivery businesses). This makes complementors' engagement interdependent and creates potential spillover effects across platforms and businesses.

Notwithstanding the critical role of complementors in value creation, the platform literature has extensively focused on a small number of platforms, viewing them as the "backbones" of the platform economy. However, thousands of firms are complementors that act as the driving force in the platform economy. Moreover, heterogeneous complementors exhibit a large variation in their characteristics, such as their resource allocation decisions across platforms and businesses. Without fully understanding how complementors engage across multiple platforms and businesses, implications for platforms' strategic choices can be misguided. My dissertation aims to fill in this gap by examining *how platforms' strategic choices – governance policies (Study I), diversification (Study II), and acquisition (Study III) – influence complementor engagement across platforms and businesses*. My theory is empirically examined in the context of the rideshare and food delivery businesses. Figure 2 summarizes the empirical overview of the three studies.

Insert Figure 2 about here

Study I explores how platform access restriction – a typical governance policy by platforms – affects complementor engagement with the focal platform and competing platforms. Existing studies suggest that platform access restriction may cause restricted complementors to switch to competing platforms, which will increase complement engagement on competing platforms. The study re-examines this prediction by accounting for the impact of cross-platform scope economies on complementor responses to platform access restriction. Specifically, I focus on the scope economies that arise from a complementor sharing resources across different platforms. Restricting the complementor's access on a platform may prevent the complementor from achieving such scope economies, thereby incentivizing it to reduce engagement in both restricted and (unrestricted) competing platforms.

To test my argument, I analyze over one billion trip records provided by rideshare services and taxicabs in New York City (NYC). Using a regulatory change as a quasi-natural experiment, the study compares driver engagement on the Lyft and Uber platforms before and after Lyft restricted drivers' access on its app in NYC. I find that Lyft's access restriction reduced trip volumes not only on the Lyft platform but also on the Uber platform. In addition, both Lyft's and Uber's trip volumes decreased not only during the restricted low-demand periods (e.g., non-rush hours) but also during the unrestricted high-demand periods (e.g., rush hours). These results are robust with additional analyses that account for unobservable time-variant factors using three counterfactuals: Lyft trips after Uber's access restriction in NYC, taxi trips in NYC, and rideshare trips in Chicago, respectively.

Study II examines how a platform's diversification into a new business influences complementor engagement with the existing business. The diversification literature claims that diversification into a new business requires (non-platform) firms to share or reallocate (owned) resources from existing businesses. Due to the lack of control rights over complementors, a diversifying platform faces a unique challenge in allocating complementors' resources between businesses. Diversification by a platform offers its complementors opportunities to share their resources (e.g., driving skills and idle time) across businesses. However, utilizing these resources often requires other complementary resources that are subject to either (1) scale constraints (e.g., drivers' time) or (2) exclusive use (e.g., drivers' vehicles) to be reallocated to the new business. Therefore, the existing business could experience a reduction in the level of complementor engagement after the diversification. In addition, the potential synergies from the diversifying platform may divert complementors from competing platforms. When the multi-homing costs from sharing resources between the diversifying and competing platforms outweigh the synergetic benefit across platforms, complementors may be incentivized to switch to the diversifying platform. Therefore, a platform's diversification can cause a reduction in the level of complementor engagement on the competing platform.

Using Uber's diversification into Uber Eats as an exogenous localized shock for rideshare drivers, I examine how Uber's and Lyft's rideshare trip volumes in Manhattan were impacted by the launch of Uber Eats in March 2016. The study employs a continuous difference-in-differences (DID) model, where treatments are the proportion of restaurants that joined Uber Eats in each geographic zone in Manhattan. I find that the proportion of restaurants that joined Uber Eats in a geographic zone was associated with a reduction in trip volumes for both Uber and Lyft in the same zone. However, these negative impacts were weakened during rush hours, when the opportunity costs of resource reallocation to Uber Eats were higher for the rideshare drivers.

Study III explores how a merger between two platforms affects complementor engagement with the merged platform and competing platforms. The acquisition increases the number of users on the merged platform and provides complementors with larger network effects. However, not all complementors may benefit from the enlarged user base. I argue that complementors on the target platform incur adjustment costs after the acquisition, which leads to a competitive disadvantage in attracting users relative to complementors on the acquiring platform. The competitive disadvantage may escalate with the number of complementors on the acquiring platform and force complementors on the target platform to leave the merged platform.

I empirically test these arguments using Uber Eats' acquisition of Postmates in the U.S. food delivery industry. I find that Postmates restaurants were more likely to leave Uber Eats and switch to competing platforms (i.e., DoorDash or Grubhub) in cities where Uber Eats had a relatively larger number of restaurants before the acquisition. This effect was weaker for Postmates restaurants that also operated on Uber Eats before the acquisition but stronger for Postmates restaurants that also operated on competing platforms before the acquisition.

Chapter 2

Platform Governance in the Presence of Within-Complementor Interdependencies

(with Yue Maggie Zhou, Sendil Ethiraj)

2.1 Introduction

A key characteristic of platforms is their reliance on complementors to create value (Gawer & Cusumano, 2002). Unlike traditional hierarchical organizations, platforms lack authority and fiat (Jacobides et al., 2018; Kretschmer et al., 2022), which creates unique challenges for platforms to govern complementor activities. One important instrument of platform governance is access control (Boudreau, 2010; Parker & Van Alstyne, 2018). By relaxing or tightening access restrictions, platforms can moderate complementor activities within and across platforms.

Existing studies suggest two effects of platform access restriction. On the one hand, it reduces competition among complementors and encourages them to improve the *quality* of their products or services (or complements) (Casadesus-Masanell & Hałaburda, 2014; Chu & Wu, 2023). On the other hand, it reduces the *quantity* of complements on the restricted platform due to the exit of restricted complementors (Boudreau, 2010; Eisenmann et al., 2009). Some of these restricted complementors will switch to competing platforms, causing a loss of competitive advantage for the restricted platform. For example, a video game platform can use licensing policies to restrict the number of game developers. Nintendo used such policies during the late 1980s to restrict the number of developers that could publish game titles on its platform (Brandenburger, 1995). Although such restriction enabled Nintendo to retain a group of high-quality game developers, it also forced other developers to switch to Sega, a competing platform that did not enforce strict access restrictions (Schilling, 2003).

However, the impact of access restriction on complement *quantity* may not be so straightforward once we account for *cross-platform economies of scope*. As complementors make large investments in their resources, some of them need to operate on multiple platforms to spread their fixed investments across a larger scale. Such calculations may influence their decision to engage with different platforms. For example, since the early 2000s, video game platforms began to provide software development toolkits (SDKs) that lowered development costs for individual developers (Ozalp et al., 2018). This induced a large pool of new developers to enter the industry.[4] The intensified competition forced developers to differentiate with complementor-specific investments, such as investments in content, art design, and music composition (Reimer, 2005). Recouping these large investments necessitated larger markets – "larger perhaps than any one platform can provide" (Corts & Lederman, 2009: 125).[5] Consequently, game developers were incentivized to spread their development costs by operating on multiple platforms (i.e., multi-homing) (Cennamo et al., 2018; Hagiu, 2009).

Multi-homing is common not only in innovation platforms such as video game consoles and smartphone operating systems but also in transaction platforms such as e-commerce and rideshare services.[6] A complementor needs to obtain resources (e.g., game content or vehicles) and develop capabilities (e.g., video game development skills or driving skills) to operate on a

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[4] For instance, hundreds of PC game developers signed up to develop video games for Microsoft Xbox 360 after Microsoft released its SDKs, DirectX and XNA Game Studio (Srinvasan & Venkatraman, 2018).
[5] Video game development costs skyrocketed from each technological generation to the next. The average development costs were below $1 million in the fourth generation (early 1990s) but jumped to $15-30 million in the seventh generation (late 2000s). As of 2020, a typical blockbuster video game requires $60-80 million to develop (Coughlan, 2004; Veresockaya, 2020).
[6] For example, fifty-two percent of video games are published on multiple game console platforms (Rietveld & Eggers, 2018). Sixty-four percent of the smartphone app developers publish their apps on both Apple's iOS and Google's Android platforms (Kapoor & Agarwal, 2017).

platform (Baldwin & Woodard, 2009; Kapoor & Agarwal, 2017). Some of these resources and capabilities are platform-independent but complementor-specific (e.g., specialized programming skills or luxury cars with customized items). When a complementor cannot recoup the costs of acquiring complementor-specific resources or capabilities from a single platform due to insufficient platform-specific demand, it can multi-home to take advantage of market-wide (rather than platform-wide) network effects to amortize such costs (Corts & Lederman, 2009), thereby creating *within-complementor interdependencies* (or cross-platform economies of scope) across platforms.

Against this background, we re-examine complementors' responses to platform access restriction by accounting for interdependencies across their activities. We argue that when complementors share resources across activities between platforms, restricting access on a platform reduces the market size for complementors and deprives them of potential scope economies. Losing such scope economies may force them to withdraw from both restricted and unrestricted platforms, thereby creating a potential cross-platform spillover effect.

We empirically test the predictions using trip-level taxi and rideshare data in New York City (NYC), where the two dominant rideshare platforms, Uber and Lyft, compete to attract drivers and users. The data includes about 0.6 billion trip records provided by rideshare services and Yellow Taxi for years 2018 and 2019, with information on pick-up/drop-off times and locations for every trip. About 45 percent of rideshare drivers in NYC work for at least two rideshare platforms (Parrott & Reich, 2018). In 2019, following city regulations, Lyft and Uber restricted driver access to their platforms in different geographic zones and at different times, which allows us to exploit heterogeneity in platform accessibility.

The results confirm the cross-platform spillover effect of restricting platform access. Lyft's access restriction reduced the number of trips not only on Lyft but also on Uber. These results are robust to additional analyses that account for unobservable time-variant factors using three counterfactuals: Lyft trips after Uber's access restriction in NYC, taxi trips in NYC, and rideshare trips in Chicago, respectively. We also find evidence in support of the mechanism: The exit of multi-homing drivers in NYC following Lyft's access restriction. Lastly, we find both Uber and Lyft experienced a decline in their trip volumes not only during restricted low-demand periods (e.g., non-rush hours) but also during unrestricted high-demand periods (e.g., rush hours). These results highlight the importance of accounting for interdependencies across complementor activities when designing platform governance policies.

2.2 Related literature

Orchestrating a large pool of complementors is crucial for the success of a platform (Gawer & Cusumano, 2002). Unlike hierarchical organizations, platforms lack direct control over complementors and cannot use levers such as employment contracts or hierarchical authority to dictate complementor activities (Jacobides et al., 2018; Kretschmer et al., 2022). Instead, platforms rely on a different set of levers such as standards, certifications, reputation systems, and information control (Li & Zhu, 2021; Miller & Toh, 2022; Rietveld et al., 2021). One prominent governance tool is platform access restriction, which regulates the level of access to the platform (Eisenmann et al., 2009; Parker & Van Alstyne, 2018). Access restriction can vary along multiple dimensions, such as access costs (e.g., licensing fees), pricing rules (e.g., subscription renewals), or regulation rules (e.g., restriction on interactions with users or other complementors) (Gawer & Cusumano, 2014; Shapiro & Varian, 1999).

An existing strand of literature has examined how access restriction influences complementor activities within the restricted platform and revealed two countervailing effects. On the one hand, access restriction enables the platform to selectively retain high-quality complementors and reduces competition among remaining complementors, which encourages them to make platform-specific investments and improve complement *quality* (Casadesus-Masanell & Hałaburda, 2014; Chu & Wu, 2023). On the other hand, access restriction reduces complement *quantity* on the restricted platform as restricted complementors leave (Boudreau, 2010; Eisenmann et al., 2009).

In addition, a limited number of studies on cross-platform effects, mostly theoretical or based on case studies, suggest that some of those restricted complementors may switch to a competing platform, thereby creating a substitutive spillover effect across platforms and a loss of competitive advantage for the restricted platform (Schilling, 2003; West, 2003). Anecdotally, Sony failed to maintain its initially dominant position after VCR producers who were disallowed on Sony's Betamax platform joined JVC's VHS platform (Cusumano et al., 1992). Symbian, once the most prevalent smartphone platform until 2010, lost its dominance after its access restriction policy led major handset manufacturers to join the Android platform (West & Wood, 2014).

Taken together, existing studies on the within- and cross-platform effects of access restriction suggest that, while access restriction enables a platform to improve complement quality, it could also reduce complement quantity on the restricted platform and increase complement quantity on competing platforms. This substitutive spillover effect across platforms in quantity is often derived from the assumption that activities performed by the same complementor are independent of each other. In reality, many complementors share resources between activities across different platforms (Corts & Lederman, 2009), which may result in *within-complementor interdependencies (or cross-platform economies of scope)* that influence complementors' participation on multiple platforms. Consequently, we examine *how platform access restriction influences the behavior of complementors across platforms*.

2.3 Theoretical development

Complementors provide their products or services on a platform by leveraging both platform-specific resources and complementor-specific resources. Platform-specific resources refer to platform infrastructure that a platform provides to complementors, such as distribution channels, development tool kits, and matching algorithms between users and complementors (Baldwin & Clark, 2000; Gawer & Cusumano, 2014). Complementor-specific resources refer to a complementor's resources that can be combined with platform-specific resources to produce complements (Baldwin & Woodard, 2009; Cennamo et al., 2018). Examples of complementor-specific resources include developers' programming skills in writing software for multiple operating systems or game consoles, reputation (e.g., game franchises, restaurants' reviews and ratings), and rideshare drivers' driving skills and vehicles.

A complementor can leverage these two types of resources as 'stepping stones' to create economies of scope in its activities. For example, a complementor can share complementor-specific resources between activities across different platforms (Corts & Lederman, 2009).[7] Complementor-specific resources enable a complementor to differentiate its complements from those offered by others, but acquiring these resources can be costly (Burtch et al., 2018; Tae et al., 2020). Protecting complementor-specific resources also incurs considerable costs. In the

[7] A complementor can also share platform-specific resources between activities across different segments within a platform. We discuss this in detail in Section 2.5.3.

smartphone app market, app developers often adopt both formal (e.g., copyrights, trademarks) and informal (e.g., design complexity, rapid innovation) strategies to protect their resources (Miric et al., 2019). Resources are often accumulated over time and require significant investments (Dierickx & Cool, 1989). For instance, in the video game industry, a large portion of development costs is related to content, art design, music composition, and licensing fees (Reimer, 2005). In the rideshare industry, 80 percent of drivers in NYC purchased a vehicle to join rideshare platforms. A typical driver pays a one-time fixed cost of more than $20,000 for vehicle purchase or leasing, licensing, and registration (Parrott & Reich, 2018). The driver also needs to continuously invest in driving skills as well as vehicle maintenance and upgrades to maintain a high rating on the platforms.

When a complementor cannot recover the fixed costs from a single platform, multi-homing becomes an imperative (Corts & Lederman, 2009; Hagiu, 2009). Platforms often have different characteristics that attract distinct groups of users (Cennamo & Santalo, 2013; Rietveld & Eggers, 2018). For example, in the video game industry, Sony PlayStation focuses on sophisticated graphics and attracts users who favor action and sports games. In contrast, Nintendo Wii mostly targets casual gamers based on its simple interfaces. In the rideshare industry, users adopt different platforms based on their personal preferences, third-party promotion programs (e.g., credit cards, firms partnered with platforms), and references (e.g., from family and friends, from another app). To the extent that different platforms have non-overlapping user bases, there is the potential for complementors to amortize their costs through multi-homing and benefit from market-wide network effects (Corts & Lederman, 2009). Such within-complementor interdependencies can create a spillover effect in the choice of activities across platforms.

Access restriction on one platform may hinder a multi-homing complementor's ability to share resources and amortize development costs across multiple platforms, thereby reducing its profit on the remaining (unrestricted) platforms. In turn, some multi-homing complementors will withdraw activities from both the restricted and unrestricted platforms, resulting in a complementary reduction in complementor activities across multiple platforms. If a significant number of complementors multi-home to achieve cross-platform economies of scope, the reduction in complementor activities (from the exit of multi-homing complementors) might outweigh the increase in complementor activities (from the inflow of restricted single-homing complementors).[8] Therefore, we predict that *in the presence of within-complementor interdependencies (or cross-platform economies of scope), restricting complementor access on one platform reduces complementor activities on competing platforms.*

2.4 Empirical design

The empirical context of the study is the rideshare market in New York City, the second-largest in the U.S. (Akhtar & Kiersz, 2019). Rideshare services in NYC have grown significantly since the entry of Uber in 2011 and the entry of Lyft in 2014. As of 2019, rideshare services in NYC provided about 0.7 million daily trips, compared to 0.2 million daily trips provided by taxicabs. Uber provided the largest share (70%) of rideshare trips, followed by Lyft (22%), Via (5%), and Juno (3%).

This is an appropriate setting for our study for several reasons. First, the success of rideshare platforms depends on aggregating a large pool of drivers. Rideshare platforms are

[8] In addition to incumbent complementors, our theory also applies to new complementors. Access restriction on one platform will discourage some (marginal) new complementors who need businesses from multiple platforms from entering all platforms. Empirically, we only focus on incumbent complementors as my sample period was after a regulatory prohibition of new entry.

known for their aggressive expansion strategy to recruit drivers to provide real-time matching between customers and drivers (Garud et al., 2022; Paik et al., 2019). Second, the rideshare market in NYC is characterized by strong within-complementor interdependencies. According to Parrott and Reich (2018), about 45 percent of drivers work for at least two rideshare platforms (i.e., multi-home) to amortize the cost of acquiring or leasing vehicles. Third, NYC provides trip-level data for both rideshare and taxi businesses, which allows us to use taxi trips as a control group to account for unobserved location- or time-specific factors that might confound our results. Lastly, and most importantly, a regulatory change in NYC provides a quasi-natural experiment for our study. Starting in 2018, NYC imposed several regulations on rideshare companies to ease road congestion. In particular, on February 1st, 2019, the city council passed Local Law 150 of 2018 that penalized rideshare platforms for running too many empty vehicles on the streets. This led Lyft and Uber to implement new access restriction policies. Starting June 27th, 2019, Lyft blocked drivers from accessing its app in low-demand periods or locations (Lyft, 2019). Drivers had to either drive to a busier location or wait until demand picked up. Uber followed suit on September 17th, 2019 (Uber, 2019). Because Lyft and Uber restricted access to their platforms during different time periods, we can exploit heterogeneity in platform access restriction across different platforms.

The context has a few additional features that enable us to tease apart multiple effects of platform access restriction across platforms: (1) the complementary spillover effect from the exit of multi-homing complementors dependent on scope economies, (2) the substitutive spillover effect from the shift of single-homing complementors to unrestricted platforms, and (3) the entry-deterrent effect of new complementors.

First, access restriction policies in NYC were more binding on marginal complementors than on productive complementors due to the exceptions made for productive drivers. For instance, Lyft drivers who maintained an acceptance rate above 90 percent and completed 100 trips in 30 days were exempted from the restriction (Lyft, 2019). Uber drivers needed to complete at least 425 trips in the prior month and have at least a 4.8-star rating to be exempted from the restriction (Uber, 2019). This setting, together with the information on service quality before and after the access restriction, allows us to focus on (marginal) multi-homing complementors who need businesses from multiple platforms to break-even.

Second, Uber started restricting access three months after Lyft, when the majority of (marginal) multi-homing drivers would have already exited the industry. Because Uber's restriction threshold was stricter than that of Lyft, Uber drivers who did not satisfy Uber's access criteria would switch to Lyft after Uber's access restriction. Consequently, Uber's access restriction should have a larger substitutive spillover effect from the shift of single-homing complementors since a large quantity of the complementary spillover effect would have been eliminated with Lyft's access restriction.

Finally, the loss of scope economies following access restriction on one platform can discourage some (marginal) new complementors from entering both platforms. Empirically, following Local Law 147 of 2018, Uber and Lyft stopped accepting new drivers on April 1st and April 19th, 2019, respectively (Rubinstein, 2019). This enables us to study the activities of incumbent complementors without worrying about complications due to new entry.

2.4.1 Data and sample

We obtain anonymized trip-level data from NYC Taxi and Limousine Commission (TLC), the agency responsible for licensing and regulating the city's taxis and rideshare vehicles. All taxi fleets and rideshare services are required to submit their trip records to TLC. The data includes about 1.3 billion trip records provided by rideshare services and Yellow Taxi from 2015 to 2019, with information on pick-up/drop-off times and locations for every trip. Information on locations is given as one of 263 taxi zones in NYC.[9]

We estimate the change in trip volumes by Lyft and Uber drivers after Lyft restricted access to its app on June 27th, 2019. We choose the sample period to be from four weeks before June 27th, 2019 to four weeks after (May 30th–July 24th, 2019). We exclude weekends as demand and supply for transportation could differ between weekdays and weekends. We aggregate the trip-level data to the hour-day-zone level. Our final sample contains 252,480 observations across 960 day-hours and 263 zones.

2.4.2 Variables

The main dependent variables, U_{iht} and L_{iht}, are trip volumes reported on Uber and Lyft, respectively, in zone *i*, during the h^{th} hour of day *t*. Because the trip volumes exhibit a large variation across different zones and during different periods, we log-transform the dependent variables to reduce value dispersion.

The independent variable, A_t, is a binary variable that equals one for dates after Lyft restricted access to its app (June 27th, 2019), and zero otherwise.

The control variables (X_{iht}) include (log) trip volumes reported on Lyft or Uber in zone *i* during the h^{th} hour of day *t* from the previous year (2018),[10] as well as taxi's (log) trip volumes in zone *i* during the h^{th} hour of day *t* from the current year (2019).

[9] Detailed maps for the 263 taxi zones can be accessed from Trip Record Data (https://www1.nyc.gov/site/tlc/about/tlc-trip-record-data.page).
[10] That is, we control for the trip number in the same zone, same hour, same day-of-the-week, and same week of the year in 2018. To compare the same day-of-the-week, date *t* in 2019 is matched to date *(t+1)* in 2018. For example, to estimate Lyft trip volume on June 28th, 2019 (Friday), we control for its trip volume on June 29th, 2018 (Friday).

Table 1 provides summary statistics at the hour-day-zone level. On average, Uber provided the highest number of trips (69) per hour and zone, followed by taxis (36) and Lyft (22). Supplementary statistics show that during rush (non-rush) hours, Uber provided 94 (61) trips per hour and zone, taxis provided 47 (32) trips, and Lyft provided 28 (20) trips.

Insert Table 1 about here

2.4.3 Specifications

First, we estimate the cross-platform effect of Lyft's access restriction on Uber driver activities using the following specification:

$$U_{iht} = \beta_0 + \beta_1 A_t + X_{iht}\boldsymbol{B} + \alpha_i + \delta_h + \gamma_t + \varepsilon_{iht} \quad (1),$$

where U_{iht}, A_t, and X_{iht} are as explained earlier. α_i, δ_h, and γ_t are zone, hour, and day-of-the-week fixed effects, respectively. We cluster robust standard errors at the zone level to account for correlation among trip volumes within the same zone.

Next, as a supplementary analysis, we estimate the within-platform effect of Lyft's access restriction on Lyft driver activities using the following specification:

$$L_{iht} = \beta_0 + \beta_1 A_t + X_{iht}\boldsymbol{B} + \alpha_i + \delta_h + \gamma_t + \varepsilon_{iht} \quad (2),$$

where L_{iht} is trip volumes on Lyft.

2.5 Results

2.5.1 Cross-platform spillover effect

Table 2 estimates the change in trip volumes on Uber and Lyft after Lyft's access restriction. We first examine the average effect on Uber trip volumes in Columns 1 and 2. The coefficient in Column 2 shows that, overall, the trips provided by Uber drivers decreased by 4.26

percent ($p < .001$) after Lyft's access restriction.[11] Based on Uber's trip volume during the month right before the access restriction (13,949,257), we infer that Lyft's access restriction reduced Uber's trip volume by about 594,238. We estimate the average fare per trip to be $18.36.[12] As Uber typically takes a 20 percent cut from the fare, these numbers imply that Lyft's access restriction resulted in a loss of about $2.2 million in revenue per month.

Next, we investigate the average effect on Lyft trip volumes in Columns 3 and 4. The coefficient in Column 4 shows that, overall, trips provided by Lyft drivers decreased by 4.71 percent ($p < .001$), similar to the percentage drop in Uber trips. That is, Lyft's access restriction reduced Lyft's trip volumes by about 224,635 trips and caused a loss in revenue of about $0.9 million per month.[13] Although Uber had a much higher total trip volume than Lyft (likely due to lower demand on Lyft), the numbers of drivers on each platform (inferred by the number of unique vehicles registered) were not as asymmetric: Uber had 76,681 active drivers, whereas Lyft had 52,185 active drivers as of June 2019. This suggests that: (1) Uber drivers each delivered more trips than Lyft (188 trips for Uber drivers vs. 88 trips for Lyft drivers in June 2019), and (2) the number of drivers withdrawn from Uber might not be very different than those from Lyft. In fact, supplementary analysis shows that more drivers withdrew from Lyft (3,629) than from Uber (2,989) during July and August 2019, two months after Lyft's access restriction.

Insert Table 2 about here
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[11] To estimate the percentage changes in trip volumes, we exponentiate each coefficient, subtract one, and multiply by 100.

[12] From the database, we calculate the average trip distance (2.89 miles) and duration (18 minutes) of Uber trips during June 2019. In NYC, Uber charges $1.62 per mile and $0.74 per minute. We estimate the average trip fare to be about $18.36. For simplicity, we do not account for the minimum fare per trip ($8) or congestion fee charged to trips in Manhattan ($2.75), which cause our estimation to be slightly lower than the estimated average fare amount ($22) by news media (Lekach, 2019). Therefore, the actual economic loss could be larger.

[13] The average trip distance and duration were 3.08 miles and 19 minutes for Lyft trips during June 2019.

2.5.2 Unobservable time-variant factors and counterfactuals

Our main specification controls for the trip volume on the platform of interest in the prior year (to account for platform-specific seasonal fluctuations) and the volume of taxi trips during the same sample period (to account for unobserved common shocks in a geographic area or time period that might influence trip volumes across all private transportation services). However, there still might be unobservable time-variant factors that the control variables do not fully capture. The paper's empirical goal is to estimate the effect of a focal platform's (e.g., Lyft's) access restriction on other platforms (e.g., Uber) that share multi-homing drivers with the focal platform. Therefore, an ideal counterfactual for the test would be an otherwise identical firm (a platform or a private transportation provider) that (1) did not share multi-homing drivers with the restricted platform, or (2) did not experience an access restriction, or both. Not surprisingly, such ideal counterfactuals do not exist in my setting. Instead, we use three counterfactuals that are close to the ideal counterfactual, together with discussions of the unobservable time-variant factors that they each intend to address.

2.5.2.1 Counterfactual 1: A platform (e.g., Lyft) that did not share multi-homing drivers with the platform that restricted its access (e.g., Uber) in NYC

There could be unobservable time-variant factors *specific to rideshare platforms in NYC* that might cause the results. For example, rideshare drivers in NYC might expect the unfavorable regulations toward rideshare would continue or even worsen in the near future and decide to exit the industry. That is, the reduction in Uber trips might be caused not by the exit of multi-homing drivers hurt by Lyft's restriction but by a "chilling effect" experienced by all rideshare drivers.

To account for such factors, we leverage a subsequent access restriction by Uber, the rival rideshare platform. More specifically, we use as a counterfactual Lyft trips after Uber's access restriction on September 17th, 2019, three months after Lyft's access restriction. We expect the "chilling effect" to stay or even strengthen after both Lyft and Uber restricted their access. That is, there would be a decrease in Lyft trips after Uber's access restriction if the "chilling effect" dominated. In contrast, our theory would accommodate an increase in Lyft trips because most of the multi-homing drivers who depended on the cross-platform scope economies would have left the industry after Lyft's access restriction, and the remaining Uber drivers were likely to be single-homing. For instance, after Uber's restriction, Uber drivers that did not satisfy Uber's access criteria (e.g., those who had a rating lower than 4.8 stars or completed fewer than 425 trips per month required by Uber) would switch to Lyft.

To test these predictions, we replicate the models in Table 2 but instead estimate the effect of Uber's access restriction on Uber and Lyft trip numbers, respectively. We change the sample period to be from four weeks before Uber's policy change to four weeks after the policy change (August 20th–October 14th, 2019). Results are presented in Table 3. Column 2 indicates that Uber's trip volumes reduced by 2.83 percent ($p < .001$) after its access restriction. In contrast, Column 4 shows that Lyft trip volumes increased by 5.92 percent ($p < .001$) after Uber's access restriction. Therefore, we can infer that the main result is driven by the cross-platform spillover effect and not by time-variant factors common to all rideshare platforms in NYC, such as the "chilling effect."

Insert Table 3 about here

2.5.2.2 Counterfactual 2: Taxis in NYC, which did not share multi-homing drivers with Lyft

There could be unobservable time-variant factors *specific to private transportation services (i.e., taxis and rideshare services) in NYC* that might also cause our results. For example, passengers may be less likely to use private transportation services during summer (Bloomberg & Yassky, 2014). Although we have controlled for seasonal fluctuations in the main specification, we employ a difference-in-differences (DID) model to compare Uber and taxi trips before and after Lyft's access restriction using the following specification:

$$Y_{siht} = \beta_0 + \beta_1 A_t U_s + \beta_2 X_{siht} + \alpha_i + \delta_h + \gamma_t + \tau_t + \varepsilon_{siht} \quad (3),$$

where A_t is as defined earlier. The dependent variable, Y_{siht}, is the (log) number of trips on private transportation service *s* (Uber or taxi) in zone *i* during the h^{th} hour of day *t*. U_s is a binary variable that equals one for trips provided by Uber, and zero otherwise (trips provided by taxis). The control variable (X_{siht}) includes private transportation service *s*'s (Uber or taxi) (log) trip number in zone *i* during the h^{th} hour of day *t* from the previous year (2018).[14] All regressions include zone (α_i), hour (δ_h), day-of-the-week (γ_t), and week (τ_t) fixed effects.

Results are presented in Table 4. Column 1 shows that, compared to taxi trips, Uber trips decreased by 8.57 percent ($p < .001$) after Lyft's access restriction, supporting a spillover effect unique to platforms that share multi-homing drivers with Lyft.

One potential weakness of this approach is that there could be a potential violation of the Stable Unit Treatment Value Assumption (SUTVA) (Rubin, 1980), where the outcome in the control group is affected by the treatment. In our context, demand for taxi trips might have

[14] Similar to the main specification, we control for the trip volume in the same zone, same hour, same day-of-the-week, and same week of the year in 2018.

increased after rideshare trips decreased. Figure 3 shows that the taxi trip numbers decreased, rather than increased, after Lyft's access restriction (June 2019), potentially due to seasonal fluctuations in demand.

Insert Figure 3 about here

To mitigate the bias from the potential violation of SUTVA, we run a subsample analysis using taxi trips from outer boroughs. Prior studies show that Uber and taxis are not fully substitutable in NYC (Fischer-Baum & Carl, 2015), especially in boroughs outside Manhattan (i.e., The Bronx, Brooklyn, Queens, and Staten Island), which have been under-served by taxis. Column 2 in Table 4 shows that, in outer boroughs, Uber trips decreased by 10.69 percent ($p <$ .001) compared to taxi trips after Lyft's access restriction, similar to the result from the full sample.

Insert Table 4 about here

2.5.2.3 Counterfactual 3: Rideshare platforms in Chicago that were not subject to access restriction in NYC

There could be unobservable time-variant factors *specific to rideshare platforms nationwide* that might also cause our results. For example, economic cycles and changes in user preference for rideshare services might influence multiple U.S. cities, including NYC. The rideshare business in Chicago shares several similar characteristics (e.g., active platforms, platforms' market shares, etc.) with NYC. As the sixth-largest rideshare market in the U.S. (Akhtar & Kiersz, 2019), rideshare services in Chicago provided about 0.3 million daily trips in

2019 (compared to 0.7 million in NYC). Similar to NYC, Uber enjoyed the largest market share (72%) in Chicago, followed by Lyft (27%) and Via (1%) (Bellon, 2019a).[15]

Starting from November 2018, the City of Chicago has provided anonymized data on rideshare trips, with information on pick-up/drop-off times and locations for every trip. Information on locations is given as one of 77 Community Areas in Chicago.[16] One weakness of the Chicago dataset is that, unlike the NYC dataset, it does not provide rideshare trip volumes by each platform, which prevents us from separating Uber trips from Lyft and Via trips. Therefore, we can only perform a limited number of counterfactual analyses.

First, we collect some comparative statistics using datasets in NYC and Chicago. Figure 4 shows that the rideshare trip volume decreased significantly in NYC compared to Chicago since June 2019, when Lyft's access restriction started. Therefore, it is unlikely that the reduction in NYC rideshare trip volume was caused by trends at the national level.

Insert Figure 4 about here

To further validate that our results are driven by the exit of multi-homing drivers, we compare the number of multi-homing drivers in NYC and Chicago. Whereas the Chicago dataset provides the monthly number of multi-homing drivers, the NYC dataset does not. Therefore, we first check supplementary NYC statistics on the monthly number of total unique vehicles (reported for each platform) and unique drivers (reported across all platforms). Under the assumption that one driver operates one vehicle, we can roughly infer the number of multi-

[15] Another advantage of using Chicago rideshare trips as a counterfactual is that SUTVA is not violated. Because network effects are often localized within each geographic market (E. Lee et al., 2006; Zhu et al., 2021), rideshare trips in Chicago are not affected by Uber trips in NYC.

[16] The map of Community Areas can be viewed on Chicago Data Portal (https://data.cityofchicago.org/Facilities-Geographic-Boundaries/Boundaries-Community-Areas-current-/cauq-8yn6, last accessed on August 15th, 2022).

homing drivers by calculating the difference between the total number of unique vehicles reported on all platforms (which includes duplicate vehicle count) and the number of unique drivers (which excludes duplicate driver count). Figure 5 shows that, after Lyft's access restriction, the number of multi-homing drivers decreased significantly in NYC, whereas it remained constant in Chicago.[17] Again, this comparison suggests that our results are not driven by macro trends in the rideshare business. In addition, we can coarsely infer that the decline in multi-homing drivers is driving my results for the cross-platform spillover effect in NYC.

Insert Figure 5 about here

To provide more precise evidence, we employ a DID estimation to compare Uber trips in NYC and rideshare trips in Chicago before and after Lyft's access restriction. Not being able to separate Uber and Lyft trips in Chicago could be problematic if there existed certain shocks that affected only Uber but no other rideshare platforms in Chicago. For example, news media reported that Uber's shared trips (i.e., UberPool) decreased significantly in Chicago after Uber increased the price for shared trips in 2019 (Bellon, 2019a).[18] Consistent with these reports, during our sample period, shared trips (-22.31%) decreased significantly more than solo trips (1.06%) in Chicago, whereas there was no such difference between shared and solo trips in NYC. To account for this idiosyncratic change in Chicago, we exclude shared rideshare trips from both the Chicago and NYC datasets, which account for 19.3 percent of all trips in Chicago

[17] We calculate the multi-homing ratio in Chicago using the number of active drivers (drivers that completed at least one trip in the given month). Our result hold when I use the number of total drivers, total vehicles, or active vehicles.
[18] Uber drivers cannot block shared trip requests (UberPool) though they can decline those requests. However, doing so would hurt drivers' acceptance rate. Accordingly, we can view the drop in shared trips as a decline in demand due to increased price (rather than a change on the supply side). In addition, Uber states that UberPool serves low-income neighborhoods that cannot afford solo rides. Therefore, users of UberPool are more likely to switch to public transportation rather than Uber solo rides when the price of UberPool increases.

and 11.04 percent of all trips in NYC, respectively, during the sample period. We use the following specification:

$$\log\left(Y_{ciht}\right) = \beta_0 + \beta_1 A_t N_c + \beta_2 X_{ciht} + \alpha_i + \delta_h + \gamma_t + \tau_t + \varepsilon_{ciht} \qquad (4),$$

where A_t is as defined earlier. The dependent variable, Y_{ciht}, is the (log) trip volume in City *c* (Uber solo trips in NYC or rideshare solo trips in Chicago) in zone *i* during the h^{th} hour of day *t*. N_c is a binary variable that equals one for geographic zones located within NYC, and zero otherwise (for geographic zones located in Chicago). The control variable (X_{ciht}) includes (log) taxi trip number in City *c*, zone *i* during the h^{th} hour of day *t*. All regressions include zone (α_i), hour (δ_h), day-of-the-week (γ_t), and week (τ_t) fixed effects.

Column 3 in Table 4 indicates that, compared to rideshare solo trips in Chicago, Uber solo trips in NYC decreased by 6.6 percent ($p < .001$) after Lyft's access restriction. We also run the same model using the full sample (both solo and shared trips). The results are qualitatively similar.

2.5.3 Restricted vs. unrestricted segments and within-platform spillover effect

Next, we investigate potential heterogeneity in such an effect across different market segments (i.e., restricted vs. unrestricted time periods) on Uber, as well as a possibility that restricting access in one segment would encourage complementors to switch to unrestricted segments within the same platform (i.e., Lyft) (within-platform spillover effect).

First, we identify which segments were most likely to be subject to access restriction. Both Uber and Lyft stated that their access restrictions would apply to low-demand periods or areas but did not specify when and where the restriction would apply. Lyft stated on its website that: "the number of drivers who can be on the road at any given time will be determined by passenger demand" (Lyft, 2019). Uber stated on its website that: "[Drivers] trying to drive in an area where there isn't enough rider demand at that time will not be able to go online." (Uber, 2019). News reporting on the policy change was also unclear about when and where the access restriction policy would apply (Bellon, 2019b).

Our reading of the company announcements, news articles, and industry reports suggests that which time periods and areas would be subject to an access restriction was determined endogenously based on actual traffic data rather than exogenously *ex-ante*. Consequently, to identify restricted time periods, we draw heat maps using the hourly trip volumes of Uber and Lyft, respectively, in NYC during 2018 (Figure 6). The heat maps show that both Lyft and Uber provided high trip numbers during rush hours (7–10 am and 5–8 pm), suggesting that non-rush hours had a higher probability of being subject to access restriction. Therefore, we assume non-rush hours (rush hours) to be restricted (unrestricted) segments. Our categorization of rush hours aligns with the high-demand periods specified in existing studies of the rideshare market (NYC TLC and Department of Transportation 2019).

Insert Figure 6 about here

It is possible that the time periods with low trip volumes on the heat maps capture a relative shortage of driver supply rather than low demand (which would subject the periods to access restriction). To further validate the distinction of restricted vs. unrestricted segments, we compare my heat maps with heat maps based on surge pricing in prior studies (Cohen et al. 2016). Surge pricing is the pricing algorithm that increases the prices of rides during periods of excessive demand relative to driver supply. If the time periods with low trip volumes on my heat maps were high-demand periods with few drivers, those periods should be subject to surge pricing. A comparison between our heat maps and heat maps based on surge pricing (Cohen et al., 2016) shows that the time periods with low trip volumes on our heat maps (i.e., non-rush hours) did not experience surge pricing, but the time periods with high trip numbers on our heat maps (i.e., rush hours) did. This confirms that the time periods with low trip volumes (i.e., non-rush hours) on our heat maps capture low-demand periods that were prone to access restriction.

Following our identification of the segments that were most likely to be subject to access restrictions (i.e., non-rush hours), Table 5 first investigates potential heterogeneity in the cross-platform spillover effect across restricted vs. unrestricted segments. Columns 1 and 2 indicate that following Lyft's access restriction, Uber trips declined not only during non-rush hours but also during rush hours, implying that drivers withdrew from both time periods. Specifically, Uber trips decreased by 2.73 percent ($p < .001$) and 8.37 percent ($p < .001$) during non-rush hours and rush hours, respectively.

Insert Table 5 about here

Next, we investigate the changes in Lyft trip volumes across different time periods after Lyft's access restriction. Columns 3 and 4 in Table 5 show that Lyft trip volumes decreased during both non-rush hours (2.68%, $p < .001$) and rush hours (10.69%, $p < .001$). These findings contradict expectations based on prior studies that restricting access in one segment would encourage complementors to switch to unrestricted segments within the same platform (Kretschmer & Claussen, 2016).

We believe the contrasting results of a complementary within-platform spillover effect of access restriction may be attributed to within-platform economies of scope. A complementor has to incur costs to acquire platform-specific resources and comply with platform-specific rules. The costs of tailoring complements to platform-specific interfaces and architectures are often immense and recurring (Anderson et al., 2014; Cennamo et al., 2018). In the smartphone app market, in addition to routine maintenance and security updates, app developers have to adjust their apps and fix errors when new versions of smartphone operating systems are released (Kapoor & Agarwal, 2017; Temizkan et al., 2012).

When a complementor cannot recover these costs from a single segment, it needs to operate in multiple segments within the platform (e.g., time periods, generations, or geographic areas) to fully amortize these costs and realize economies of scope. For example, a video game developer needs to invest significant efforts to learn console-specific technologies, such as technological interfaces and SDKs (Ozalp et al., 2018). Game developers can amortize these costs by redesigning game titles from old to new generation consoles. Similarly, in the rideshare market, Uber drivers need to maintain a minimum of 4.6 rating to avoid suspension from the platform and exploit real-time matching algorithms. Moreover, only drivers with a 4.85 rating or higher are allowed to provide premium services such as Uber Black and Lyft Lux. Maintaining a high rating is costly, and the cost may only be justified if rideshare drivers can leverage their ratings across different time segments. This creates within-platform interdependencies for each complementor and may cause a complementary spillover effect across segments on the same platform: Losing access to one segment could hamper within-platform economies of scope and force a complementor to withdraw from both restricted and unrestricted segments. In our context, non-rush hour transactions provided a strong incentive for drivers to operate during rush hours. If Lyft drivers enjoyed scope economies from operating across rush and non-rush hours, restricting access during non-rush hours would have incentivized drivers to exit from both time periods.

2.5.4 Robustness checks and alternative explanations

We run a battery of supplementary tests to check the robustness of the results. First, we re-estimate our models using different sample periods, ranging from one week to eight weeks before and after Lyft's access restriction policy. The results reveal that the effect of Lyft's restriction policy was similar across different time periods.

Second, we estimate the effect of Lyft's access restriction on other rideshare platforms in NYC (i.e., Juno and Via). The results show that trips provided by Juno and Via both decreased after Lyft's access restriction, suggesting that the cross-platform spillover effect was not Uber-specific but rather universal across all rideshare platforms that shared drivers.

Third, some geographic zones in NYC were residential areas and may not exhibit high demand during rush hours. We re-estimate the models using the subsample of trips in Manhattan, the central business district with high traffic demand during rush hours. The results are consistent with the main findings.

Fourth, we re-estimate our models using trips made during weekends, when high-demand time periods were late-night periods (Sat 9 pm–Sun 3 am, Figure 2). Our results hold.

Finally, the categorization of rush vs. non-rush hours might not fully capture time periods that were subject to access restriction. To show that Lyft's access restriction had a spillover effect on Uber across most hours, we calculate Uber's average trip numbers for each hour-day of the week pair (ATHD) for the four weeks before and after Lyft's access restriction.[19] Then, we plot the differences between the two sets of ATHDs as a heat map (Figure 7). The heat map confirms that Uber trip numbers decreased during most day-hours. Next, we estimate the impact

[19] We drop late-night periods (1–5 am) when few users and drivers were active and trip numbers were negligible (less than 5% of the total trip number).

of Lyft's access restriction on Uber by hour. According to data on Lyft trips in 2019 until its access restriction, 7–8 pm was the busiest hour and, therefore, least likely to be subject to access restriction. Thus, we set 7–8 pm as the baseline. Results confirm that Uber trip volumes decreased during most of the hours relative to 7–8 pm.

Insert Figure 7 about here

2.5.5 Cross-platform spillover effect on complement quality

Our main analysis focuses on the effect of access restriction on complement *quantity*. As a supplementary analysis, we examine the impact of access restriction on complement *quality*. The literature has established that access restriction enables a platform to retain high-quality complementors (Casadesus-Masanell & Hałaburda, 2014; Chu & Wu, 2023). To the extent that interdependencies within multi-homing complementors cause them to withdraw from both the restricted and competing platforms after an access restriction, we expect a quality improvement on all these platforms among remaining complementors due to the exit of marginal multi-homing complementors.

We estimate the impact of Lyft's access restriction on Lyft's and Uber's trip duration (controlling for distance and the total trip number of rideshare services and taxis during the hour), the only (indirect) quality measure that can be obtained from the data.[20] Prior studies have used driver detours to measure Uber and taxi drivers' fraud and treated longer trip duration as lower service quality (Balafoutas et al., 2013; M. Liu et al., 2021; T. Liu et al., 2019). Our results (Table 6) show that the trip duration of Lyft and Uber decreased by 2.41 percent and 2.96

[20] Supplementary statistics show that on average, trip durations were the longest for Lyft (19.1 minutes), followed by taxis (18.6 minutes) and Uber (18.3 minutes). For the average trip distance, taxi trips had the longest distance (3.3 miles), followed by Lyft (3.08 miles) and Uber (2.89 miles).

percent, respectively, after Lyft's access restriction ($p < .001$).[21] This result, despite being inconclusive due to data limitations, confirms our expectation of a quality improvement due to the exit of marginal multi-homing drivers from both the restricted and competing platforms.[22]

Insert Table 6 about here

2.6 Discussion and conclusion

The main objective of this study was to examine the cross-platform spillover effects of platform access restriction on complementor activities in the presence of within-complementor interdependencies. Using trip-level data of rideshare services in New York City, we investigate changes in Lyft's and Uber's trip volumes after Lyft restricted drivers' access to its app during low-demand periods. We find that Lyft's access restriction reduced trip volumes not only on Lyft but also on Uber. In addition, both Lyft's and Uber's trip volumes decreased not only during restricted low-demand periods (e.g., non-rush hours) but also during unrestricted high-demand periods (e.g., rush hours). Our findings support the argument that restricting platform access could weaken potential scope economies from resource sharing and motivate complementors to withdraw from both restricted and unrestricted platforms or segments. With these findings, we provide the first empirical evidence for within-complementor interdependencies.

[21] One caveat of our measure is that trip duration may be endogenous to the number of vehicles on the road. Even though we control for trip distance and the total trip number of rideshare services and taxis during the hour to partly account for road congestion, we do not have data on public transportation or privately owned vehicles to account for the impact of reduced congestion. To mitigate the bias from reduced road congestion, we employ a DID estimation using taxi trips as a counterfactual. Taxi trips were also subject to the reduced congestion after Lyft access restriction, which enables us to control for the time trend in road congestion. Our results hold.

[22] The literature has also pointed to potential quality advantages of multi-homing complementors compared to single-homing complementors. For example, they are likely to possess more resources, higher technological capability, and a larger user base (e.g., Cennamo et al., 2018), which enable them to offer better quality. Consequently, the exit of multi-homing complementors may downgrade complement quality on all platforms they withdraw from. Our results do not support this alternative prediction in our specific context.

Although we focus on the interdependencies that arise from resource sharing across platforms in our context, the concept of within-complementor interdependencies can be extended to other settings where complementors share resources across market segments or businesses. For example, an Uber driver can leverage its vehicle and operate as an Uber Eats driver, creating interdependencies between activities across the rideshare and food delivery businesses.

Our study contributes to the platform literature in several ways. First, we point out two sources of within-firm interdependencies based on different types of resources that complementors can utilize. Even though complementors leverage both platform-specific resources and complementor-specific resources to generate complements (Baldwin & Woodard, 2009; Giustiziero et al., 2023), prior studies have mainly focused on scope economies from sharing platform-specific resources, neglecting scope economies that could arise from sharing complementor-specific resources across platforms. Our study fills in this gap.

Second, this paper speaks to the multi-homing literature. Several studies claim that multi-homing by complementors prevents a large platform from dominating the market and reduces its likelihood of "winner-take-all" (Eisenmann et al., 2009). They suggest that a large platform should prohibit or penalize multi-homing through means such as exclusive contracting, vertical integration, or price discrimination (Armstrong & Wright, 2007; Li & Zhu, 2021). On the other hand, it has been discovered empirically that dominant platforms might lose a competitive advantage when enforcing such policies as multi-homing can provide access to high-quality complements that have been developed for new platforms (Lee 2013, Schilling 2003). The paper reconciles these studies by suggesting that the prohibition or penalty against multi-homing can potentially discourage complementors from participating on *all* platforms due to the loss of economies of scope across platforms. Therefore, the net impact of prohibiting multi-homing will hinge in part on the presence of within-complementor interdependencies. When such interdependencies are high, platforms may need to allow multi-homing by complementors in order to achieve market-wide (in addition to platform-wide) network effects (Corts & Lederman, 2009).

The study has a few limitations that offer opportunities for future research. First, our prediction assumes that a significant portion of complementors multi-home to amortize costs across platforms. Therefore, our theory is not likely to hold in industries where most complementors single-home due to high multi-homing costs (Cennamo et al., 2018; Eisenmann, 2007) or exclusive contracts required by platforms (Armstrong & Wright, 2007; Lee, 2013).

In addition, the lack of complementor identifiers prevents us from connecting trips to drivers or vehicles. As a result, we cannot identify individual drivers who operated on single vs. multiple platforms or during rush vs. non-rush hours. Future studies can investigate contexts where within-complementor interdependencies may be directly observed.

Chapter 3

Resource Exclusivity, Oscillation, and Cannibalization following Platform Diversification

(with Yue Maggie Zhou, Christine Choi)

3.1 Introduction

During the last decade, multiple platform firms have broadened their scope by diversifying into new businesses. For instance, Amazon, originally an e-commerce platform, has diversified into e-reader, digital streaming, and smart home device businesses. Uber, which started as a rideshare platform, has entered food delivery, grocery delivery, and freight businesses.

Despite the prevalence of platform diversification across businesses, the platform literature has mostly been limited to platform activities within a single business. A few exceptional studies on platform diversification have focused on potential economies of scope arising from sharing platform-specific resources (e.g., platform technologies) across businesses (Eisenmann et al., 2011; Wormald et al., 2021); the role of complementor-specific resources (e.g., rideshare drivers' vehicles, driving skills, and time) has mostly been neglected. Given that platform firms rely on complementor-specific resources (henceforth *complementor resources*) to create value (Gawer & Cusumano, 2002), resource allocation by complementors may play a critical role in differentiating diversification by platform firms from that by traditional firms (Ahuja & Novelli, 2017: 379; Kretschmer et al., 2022: 419).

Against this background, we investigate how complementors reallocate their resources following their platform firm's diversification. We argue that diversification by a platform firm enables its complementors to share some complementor resources across businesses. For example, the launch of Uber Eats allows Uber drivers to share their driving skills and idle time (for rest and recovery, as well as waiting for the next transaction) for both rideshare and food delivery businesses. Such sharing increases resource utilization and generates synergistic benefits for the complementors. At the same time, it also creates opportunities for the complementors to reallocate their other (complementary) resources between businesses and may cause a cannibalization effect on the platform firm's existing business. For example, allowing Uber drivers to share driving skills and idle time between the rideshare and food delivery businesses enables the drivers to reallocate their vehicles and some driving hours from rideshare to food delivery, thereby potentially reducing their engagement with the rideshare business (within-platform-firm spillover effect).

Although cannibalization of existing business caused by resource reallocation can also happen in non-platform firms (Roberts & McEvily, 2005; Schoar, 2002; Wu, 2013), the sharing-enabled resource reallocation may be more fluid and salient for platform firms due to their difficulty in governing exclusive-use resources.[23] Resources are reallocated (rather than shared) not only when they are subject to scale constraints (Levinthal & Wu, 2010), but also when they are slack but exclusive in use. For example, even when an Uber driver has the extra capacity to transport multiple riders in the same car or when an Airbnb host has enough space in her house

[23] We conceptualize resource reallocation broadly as resource oscillation between segments (resources constantly being reallocated back and forth without being permanently withdrawn from either segment) which is similar to but broader than resource redeployment defined in the existing literature (Helfat & Eisenhardt 2004: 1222), that "[t]he analysis of inter-temporal economies of scope pertains to redeployment of resources between businesses over time in reaction to permanent changes in technologies and market demand. The analysis does not apply to a repeated pattern of resource transfer between ongoing businesses, such as that which occurs in some seasonal businesses."

for multiple groups of guests to stay at the same time, they may be prohibited from doing so *within* a single transaction.[24]

While exclusive use of resources by a particular party can be easily governed through authority within a non-platform firm or exclusive contracts in a supplier relationship, it is much harder to govern within a platform firm. Reliance on complementors for value creation deprives the platform firm of its authority- or contract-based control rights over complementors (Jacobides et al., 2018; Kretschmer et al., 2022). As a result, exclusivity is usually enforced at the transactional rather than organizational level. Compared to employees or suppliers, the lack of organizational exclusivity gives complementors more flexibility both to share and to reallocate (or oscillate) their resources across businesses *between* transactions. Consequently, complementors can offer their products and services across multiple platforms or business segments. However, the consumption of exclusive-use complementor resources for a specific transaction prevents complementors from simultaneously performing other transactions that need the same resources. For example, while an Uber driver can work for both rideshare and food delivery businesses, he is prohibited from carrying a rider and delivering food at the same time. Ironically, the sharing-enabled reallocation of resources by complementors to maximize utilization may also cannibalize a platform firm's existing business.

The opportunity to maximize resource utilization across business segments may also divert complementors away from competing platform firms. The lack of organizational exclusivity enables complementors to share resources across different platform firms. However, operating on multiple platform firms entails multi-homing costs (Cennamo et al., 2018). To

[24] In contrast, it is rare for an employee to work for multiple companies or multiple divisions within the same company even when he has a lot of downtime (slack): He must resign from one to work for another. Even for a part-time employee working for multiple firms, he is constrained to working for a certain number of days or hours for each particular employer, despite the slack time he may have during each of his jobs.

avoid multi-homing costs across different platform firms, complementors wishing to work in the new business may choose to share and allocate their resources between businesses within the same platform firm, i.e., the diversifying platform firm.[25] For instance, a Lyft driver attempting to work on Uber Eats may switch to Uber to avoid the cost of coordinating schedules across Uber Eats and Lyft.[26] Complementors switching to the diversifying platform firm can cause a cannibalization effect on competing platform firms (cross-platform-firm spillover effect).

Finally, we explore how demand conditions in the existing business will influence complementors' decision to reallocate their resources both within and across platform firms. Opportunity costs for reallocating resources are higher in a high-demand submarket compared to a low-demand submarket within the same business segment (Anand & Singh, 1997; Wu, 2013).[27] Accordingly, complementors will be less likely to reallocate their resources away from a high-demand submarket in the existing business to the new business. As a result, both within- and cross-platform-firm spillover effects will be weaker for high-demand submarkets in the existing business.

We empirically test my predictions in the rideshare business in New York City (NYC). In March 2016, Uber diversified into the food delivery business through the launch of Uber Eats in Manhattan. The launch of Uber Eats (and restaurants' decisions to join Uber Eats) created an exogenous shock for rideshare drivers, which enables us to employ a continuous difference-in-differences (DID) model. Using as treatment the proportion of restaurants that joined Uber Eats in each geographic zone in Manhattan, we compare Uber's and Lyft's rideshare trip volumes

[25] While complementors may also face multi-homing costs for operating on different platforms within the same platform firm, these platforms share similar platform architectures, which significantly lowers the within-platform-firm multi-homing costs (Srinvasan & Venkatraman, 2018).
[26] For a driver working on Uber and Uber Eats, the Uber app connects the driver to both rideshare and food delivery orders and minimize the burden of coordinating schedules.
[27] We use submarkets to refer to individual markets in different time periods, geographic areas, or product segments (Ethiraj & Zhou, 2019).

before and after the launch of Uber Eats (i.e., between October 2015 and October 2016) to identify both within- and cross-platform-firm spillover effects. We further exploit heterogeneity in the two spillover effects against demand differences throughout the day (e.g., rush vs. non-rush hours). To supplement the quantitative data, we collect qualitative information through semi-structured interviews with rideshare/food delivery drivers and restaurant owners who joined Uber Eats when the service was launched in Manhattan.

We find that an increase in the proportion of restaurants joining Uber Eats in a geographic zone was associated with a reduction in rideshare trip volumes for both Uber and Lyft in the same zone. Specifically, a one percent increase in local restaurants joining Uber Eats was associated with 2.1 percent fewer Uber trips and 6.8 percent fewer Lyft trips. However, these negative impacts were weaker during rush hours, a high-demand submarket for the rideshare business. To rule out alternative demand- and supply-side explanations, we conduct additional analyses, including DID models that examine the changes in Uber's and Lyft's rideshare trip volumes following the launch of DoorDash, an independent food delivery platform. These analyses confirm that our results were mainly driven by drivers reallocating resources to the food delivery business to improve resource utilization.

3.2 Theoretical development

Diversification, a central concern in the field of strategy, has received little attention from platform scholars (Ahuja & Novelli, 2017: 379; Kretschmer et al., 2022: 419).[28] Studies on platforms have mostly examined platform activities within a single segment or platform firms'

[28] For instance, prior studies have pointed out that "companies such as Uber are increasingly fashioning themselves as platforms, considering expansion into many markets. What are the boundaries to such scope expansion is currently not very clear." (Ahuja & Novelli, 2017: 379) and "we know relatively little about … to what degree these diversification strategies of platforms are similar to or different from those of traditional firms." (Kretschmer et al., 2022: 419).

vertical scope expansion into complementors' businesses (Gawer & Henderson, 2007; Li & Agarwal, 2017). A limited number of studies on platform diversification attribute diversification to potential economies of scope arising from sharing platform firms' resources across businesses (Eisenmann et al., 2011; Wormald et al., 2021). For instance, Eisenmann et al. (2011) show that, through the bundling of different platform functionalities (i.e., platform envelopment), a platform firm can leverage its user base to enter adjacent businesses. However, platform firms need to leverage both platform-specific and complementor-specific resources to create value. Thus, the impact of platform diversification may be misspecified without fully understanding how complementors allocate their resources across businesses or platforms.

3.2.1 Resource scalability, exclusivity, and divisibility

A hallmark of the literature on diversification is the notion of economies of scope as the main rationale (Montgomery & Wernerfelt, 1988; Panzar & Willig, 1981). Economies of scope can arise from either resource sharing or resource reallocation between businesses (Helfat & Eisenhardt, 2004) depending on the characteristics of the resources, such as scalability (Levinthal & Wu, 2010): Non-scale free resources can no longer be shared and must be reallocated once the scale constraint is met.

While the literature has mainly attributed resource reallocation to lack of scalability, we argue that it may not be the only driver; another can be exclusivity in use (Klein et al., 1978; Marvel, 1982). While certain resources (e.g., equipment) are exclusive in use by their nature, the mode of governance over them to ensure exclusive access by a specific party varies across organizational forms, ranging from vertical integration or ownership (Mahoney, 1992;

Williamson, 1985) to exclusive contracts (Klein et al., 1978; Somaya et al., 2011).[29] For instance, in the soft-drink industry, large concentrate producers (CPs) normally write exclusive contracts with their bottlers to ensure exclusive access to the bottlers' assets and services, even when the bottlers have excess capacity to serve other CPs during slack time (Zhou & Wan, 2017). The bottlers must terminate their contracts with their existing CPs to start using their resources for a different CP. Even within the same firm, due to hierarchical and administrative constraints, it is rare for an employee with a lot of downtime to work for multiple divisions.

The conceptualization of exclusivity in use is different from physical indivisibility that the literature has proposed as a driver for diversification (Penrose, 1959; Teece, 1980). Physically indivisible resources can be both of non-exclusive use (and therefore shared, e.g., reputation, knowledge) and exclusive use (and therefore reallocated, e.g., equipment). Similarly, divisible resources can be both of non-exclusive use (therefore shared, e.g., employee's idle time) and exclusive use (and therefore reallocated, e.g., employee's working time). Table 7 gives some examples of resource sharing and reallocation depending on scalability, divisibility, and exclusivity.

Insert Table 7 about here

Table 7 suggests a new theoretical possibility: sharing-enabled resource reallocation. As resources often work in bundles, exploiting one particular resource may require other complementary resources (Miller & Toh, 2022; Roberts & McEvily, 2005). For instance, exploiting new technological know-how often requires managerial time to organize training, manufacturing plants for trial runs, and channels to distribute products assembled using the new

[29] Exclusive contracts include vertical contracts in supplier relationship (Klein et al., 1978; Mahoney, 1992; Zhou & Wan, 2017) and horizontal licensing agreements (Arora & Fosfuri, 2003; Somaya et al., 2011).

technology (Teece, 1998). Similarly, sharing brand names across product markets requires reallocation of managerial attention (Miller & Yang, 2016). Such complementarity in resource usage suggests that even when sharing some scale-free resources (e.g., technological know-how or brand name) across businesses, a firm (or a complementor) may have to reallocate other non-scale free or exclusive-use resources from one business to another.

Sometimes, it is the sharing of some resources that creates the opportunity for the reallocation of other complementary resources. In the earlier example, it is the sharing of technological know-how between businesses that makes each business aware of the opportunity and enables the reallocation. In the platform context, Uber's diversification into the food delivery business enables drivers to share their driving skills and idle time between the rideshare and food delivery businesses. At the same time, such sharing makes it easier for drivers to reallocate their vehicles and driving time to the food delivery business.

3.2.2 Governance of resource exclusivity by platform firms

Traditional (non-platform) firms can use authority or long-term contracts to govern exclusivity of resources to their best use at the organization level (Klein et al., 1978; Mahoney, 1992; Williamson, 1985). For example, the firm can specify a primary segment/division for an engineer and allow the engineer to solve problems for another segment/division only upon approval from the primary segment/division managers based on the expected workload.

However, governance for exclusivity at the organizational level is much harder for platform firms due to their heavy reliance on complementors. The success of platform firms critically depends on attracting a large pool of complementors to provide heterogenous products or services (i.e., complements) to users (Gawer & Cusumano, 2002). The heavy reliance on complementors deprives the platform firm of its ownership- or contract-based control rights over complementors. Consequently, exclusivity is typically enforced only at the transactional rather than organizational level.[30] The lack of organizational exclusivity allows complementors a broader span of control in both resource sharing and allocation. For instance, unlike most employees, complementors can provide products and services on multiple businesses or platforms. As a result, sharing-enabled resource reallocation can be more fluid for platform firms.

3.2.3 Within-platform-firm spillover effect

A platform firm's expansion into a new business creates an opportunity for complementors in the existing business to share some complementor resources across businesses to obtain economies of scope. For example, complementors can amortize the costs of acquiring complementor resources across multiple businesses.

At the same time, sharing some resources also creates opportunities for complementors to reallocate some other complementary resources, potentially hurting the platform firm's existing business. The lack of authority- or contract-based control rights over complementor resources makes it challenging for a platform firm to regulate the sharing of complementor resources to maximize the joint benefit for the platform firm and complementors, or to prevent the reallocation of complementor resources to minimize cannibalization of the platform firm's existing business. In the presence of transactional exclusivity, if a significant number of complementors reallocate their resources to the new business, the existing business could experience a reduction in the level of complementor activities.

[30] While platform firms may be able to secure exclusivity on complementor resources by producing complements themselves (i.e., first-party complements) or by contracting exclusively with a handful of complementors (Hagiu & Spulber, 2013; Lee, 2013), excessive enforcement of such exclusivity may drive a majority of complementors to exit as they avoid being 'locked-in' by a single platform (Schilling, 2003; Shapiro & Varian, 1999).

Hypothesis 1 (H1). *A platform firm's diversification into a new business reduces complementor activities in the existing business (within-platform-firm spillover effect).*

3.2.4 Cross-platform-firm spillover effect

When a platform firm diversifies, the opportunity to maximize resource utilization through real-time sharing and reallocation of complementor resources between multiple businesses may encourage complementors on competing platform firms to participate in the new business. However, allocating resources across platform firms is not without cost. Each platform firm has a unique platform architecture and requires complementors to follow platform-firm-specific rules, such as technology standards, interfaces, and policies (Baldwin & Woodard, 2009; Kapoor & Agarwal, 2017). To operate on multiple platform firms, complementors need to tailor their products or services to heterogenous platform-firm-specific interfaces and architectures, thereby incurring high multi-homing costs (Cennamo et al., 2018).

In contrast, platforms within the same platform firm share similar platform architectures, which significantly reduces the need (and therefore multi-homing costs) for complementors to adjust their products or services across different platforms (Ozalp et al., 2018; Srinvasan & Venkatraman, 2018). Consequently, multi-homing costs between different platform firms (e.g., Lyft and Uber Eats) will be larger than multi-homing costs between multiple platforms within the same platform firm (e.g., Uber and Uber Eats). When multi-homing costs across platform firms are sufficiently larger than multi-homing costs within a platform firm to outweigh the benefit from sharing-enabled resource reallocation across platform firms, complementors may be incentivized to switch to the diversified platform firm. For instance, the Uber app connects drivers to both rideshare and food delivery orders, potentially minimizing Uber drivers' idle time. In contrast, when working on the Uber Eats platform, Lyft drivers have to log off the Lyft app to avoid getting rideshare requests from Lyft (and having to reject them). Coordination costs arising from joint scheduling and adjustments are known to limit firms' activity scope (Zhou, 2011). Therefore, the workload of coordinating schedules across Uber Eats and Lyft will encourage drivers to switch from working on Lyft and Uber Eats to working on Uber and Uber Eats. If a significant number of complementors withdraw their resources from the competing platform firm, the latter will experience a reduction in complementor activities.[31]

Hypothesis 2 (H2). *A platform firm's diversification into a new business reduces complementor activities on competing platform firms in the existing business (cross-platform-firm spillover effect).*

3.2.5 Demand conditions of the existing business

Resource allocation depends on not only the expected return from the target use but also the opportunity costs related to alternative uses (Levinthal & Wu, 2010). Accordingly, when a platform firm diversifies, complementors in its existing business will compare the expected return from continuing to operate in the existing business and the expected return from reallocating their resources to the new business.

Prior studies suggest that demand conditions in different businesses can affect opportunity costs for firm resources, thereby influencing allocation decisions (Chandler, 1969; Giarratana & Santaló, 2020). Specifically, firms face a higher opportunity cost for withdrawing resources from a high-demand submarket than from a low-demand submarket (Anand & Singh,

[31] A natural question to ask is whether the cross-platform-firm spillover effect in H2 will offset the within-platform-firm spillover effect in H1 and leave the existing business of the diversifying platform firm unaffected. We believe this is unlikely because porting costs between different platform firms are typically higher than porting costs between platforms within the same platform firm (Ozalp et al., 2018; Srinvasan & Venkatraman, 2018). Therefore, more complementors in the diversifying platform firm will switch to the new business (H1) than complementors from the competing platform firm switching to the existing business of the diversifying platform firm (H2), making H1 hold despite the offsetting effect of H2.

1997; Wu, 2013). Similarly, for both the diversifying and competing platform firms, complementors in the existing business will be less likely to withdraw resources from high-demand submarkets than from low-demand submarkets. Therefore, both the within- and cross-platform spillover effects will be weaker in high-demand submarkets of the existing business.

Hypothesis 3 (H3). *Both within- and cross-platform spillover effects are weaker in high-demand submarkets than in low-demand submarkets of the existing business.*

3.3 Empirical design

The empirical context of the study is the rideshare market in New York City (NYC). In March 2016, Uber launched Uber Eats, a food delivery platform, in several U.S. cities, including NYC (Uber, 2016). In NYC, Uber Eats was only available in Manhattan at first but expanded to most areas by December 2016 (Covert & Fickenscher, 2016). By October 2016, about four percent of the restaurants located in Manhattan (495 out of 10,880) joined Uber Eats.

This is an appropriate setting for our study for several reasons. First, Uber's entry into the food delivery business (and restaurants' decisions to join Uber Eats) created an exogenous and localized shock to rideshare drivers in NYC. Our reading of news articles suggests that the launch of Uber Eats was not determined by the availability of Uber drivers within each geographic zone: Uber Eats focused on recruiting popular restaurants rather than drivers to attract users (Uber, 2016). Even on the delivery side, Uber Eats focused on recruiting bike couriers rather than vehicle drivers. Due to parking difficulty in Manhattan, the majority of the food delivery couriers in NYC used bikes rather than cars (Novellino, 2016). As detailed in Table 8, our interviews with Uber drivers in Manhattan confirmed that Uber Eats did not provide any incentives for Uber drivers to join Uber Eats. In addition, restaurant owners who joined Uber

Eats before October 2016 attributed their decision to the additional business opportunity from Uber Eats rather than the availability of Uber drivers near their restaurants.

Insert Table 8 about here

Figure 8 visualizes the percentages of restaurants that joined Uber Eats for each geographic zone in Manhattan by October 2016, which exhibited a large variation across the 69 zones. Consequently, the launch of Uber Eats could have both exogenous and heterogeneous impacts on rideshare drivers depending on their active zones, providing an opportunity to identify zone-specific treatment effects.

Insert Figure 8 about here

Second, NYC has also become the leading food delivery market (Renub Research, 2020), with multiple food delivery platforms fiercely competing to attract restaurants and customers. The sequential entry of major food delivery platforms allows us to compare the effect of entry by a diversified platform firm.

Lastly, NYC Taxi and Limousine Commission (TLC) provides anonymized trip-level data. Detailed trip records allow us not only to measure rideshare trip volume at a granulate level but also to identify heterogeneous effects of Uber's diversification across submarkets with different demands in the rideshare business (e.g., rush hours vs. non-rush hours). (i.e., Uber Eats) with that by specialized platform firms (i.e., DoorDash).

3.3.1 Data and sample

We draw on data from multiple sources. First, we collect anonymized trip-level data from NYC TLC for rideshare services and Yellow Taxi from 2015 to 2016 (and 2019 for additional mechanism tests), with information on pick-up time and location.[32] Second, we obtain from ReferenceUSA information on all restaurants operating in NYC during 2015 and 2016. Third, we acquire historical information scraped from all major food delivery platforms (i.e., DoorDash, Grubhub, Postmates, and Uber Eats) to identify restaurants that joined each of these platforms by October 2016, the earliest month when such data was available for all major food delivery platforms. Finally, we obtain population demographics information for each zone from the American Community Survey.

To minimize unobservable location- and time-specific variations in the rideshare business that could confound the results, we choose zone-hour-day as the level of analysis. We compare trip volumes of Uber and Lyft in the same zone, hour-of-the-day, day-of-the-week, and week-of-the-year between October 2015 and October 2016 (i.e., before and after the launch of Uber Eats in March 2016).[33] We exclude weekends to account for differences in demand and supply for transportation services between weekdays and weekends. We also exclude three island zones in Manhattan where no rideshare trips nor restaurants were observed during the sample period.[34] Our final sample contains over 61,000 observations across 960 day-hours and 66 geographic zones in Manhattan.

To supplement the quantitative data, we collect qualitative information through semi-structured interviews with rideshare drivers and restaurant owners who joined Uber Eats around March 2016. These interviews confirmed that: (1) the launch of Uber Eats served as an exogenous shock to rideshare drivers, (2) there existed significant multi-homing costs from

[32] Locations are given as one of 263 taxi zones in NYC. 69 zones are located in Manhattan.
[33] To compare the same day-of-the-week, date t in 2016 is matched to date ($t+2$) in 2015. For example, to estimate Uber trip volume on October 3rd, 2016 (Monday), we control for its trip volume on October 5th, 2015 (Monday). Accordingly, we compare rideshare trips during October 1st–28th, 2016 to Oct 3rd – 30th, 2015.
[34] These are Zone 103 (Liberty Island), Zone 104 (Ellis Island), and Zone 105 (Governor's Island).

operating on multiple platform firms (i.e., coordinating schedules between platform firms), (3) the food delivery business by itself was not more profitable than the rideshare business, and (4) drivers constantly reallocated their resources (i.e., vehicles and driving time) between rideshare and food delivery businesses. This qualitative information is provided in detail in Table 8.

3.3.2 Variables

The main dependent variable is the number of trips, Y_{ijht}, that was reported on platform *i* (Uber or Lyft), in zone *j*, during the h^{th} hour of day *t*. The variable is log-transformed and the estimated coefficients reflect percentage changes in the dependent variable.

The main independent variable, UE_j, represents the proportion of restaurants in zone *j* that joined Uber Eats by October 2016. We use proportions rather than numbers because the total number of restaurants in each zone differed significantly. The results hold when we use the number of restaurants that joined Uber Eats instead.

The second independent variable, R_h, is a binary variable that equals one for rush hours (7–10 am and 5–8 pm), and zero otherwise. To confirm our categorization, we construct heat maps using the hourly trip volume of Uber and Lyft in NYC during 2015 (Figure 9). The heat maps suggest that the categorization corresponds to high-volume submarkets in 2015.

Insert Figure 9 about here

The control variable (X_{jt}) includes the total number of restaurants in zone *j* during year *t*, which accounts for geographic and time variations in restaurant demand.

Summary statistics (Table 9) at the zone-hour-day level reveal substantial heterogeneity in the proportion of restaurants that operated on Uber Eats during our sample period. On average, four percent of restaurants joined Uber Eats in Manhattan, but the proportion varied from zero to 12 percent across different zones.

Insert Table 9 about here

Among the 66 zones in Manhattan, 54 had at least one restaurant that joined Uber Eats (the treatment group), whereas 12 zones did not have any restaurants on Uber Eats (the control group). Figure 10 compares the monthly Uber trip volumes between the treated and control groups, normalized by the trip volumes during March 2016 (when Uber Eats was launched). It shows that, after the launch of Uber Eats, Uber trips in the treated group exhibited a slower growth compared to Uber trips in the control group.

Insert Figure 10 about here

3.3.3 Specification

We employ a continuous DID estimation with treatments being the proportion of restaurants in each zone that joined Uber Eats. We estimate the effect of Uber's diversifying entry into food delivery on rideshare trip volumes using the following specification:

$$Y_{ijht} = \beta_0 + \beta_1 Post_t UE_j + \beta_2 X_{jt} + \alpha_j + \delta_h + \gamma_t + \tau_t + \varepsilon_{ijht} \quad (4),$$

where Y_{ijht}, UE_j, and X_{jt} are as defined earlier. $Post_t$ is a binary variable that equals one for dates after the launch of Uber Eats (March 22nd, 2016), and zero otherwise. All regressions include zone (α_j), hour-of-the-day (δ_h), day-of-the-week (γ_t), and week-of-the-year (τ_t) fixed effects. Standard errors are clustered at the zone level.

To test H1, we use (log) Uber trip volume as the dependent variable. To test H2, we use (log) Lyft trip volume as the dependent variable. To test H3, we employed a triple differences model, shown in equation (5):

$$Y_{ijht} = \beta_0 + \beta_1 Post_t UE_j + \beta_2 Post_t UE_j R_h + \beta_3 X_{jht} + \alpha_j + \delta_h + \gamma_t + \tau_t + \varepsilon_{ijht} \quad (5),$$

where control variables X_{jht} include other two-way interaction terms (i.e., $Post_t R_h$, $UE_j R_h$) as well as the total number of restaurants in each zone.

3.4 Results

3.4.1 Within-platform-firm spillover effect

First, we compare changes in Uber trip volumes in zones where at least one restaurant joined Uber Eats and changes in zones where no restaurant joined Uber Eats. The coefficient in Column 1, Table 10 suggests that, for every one percent of restaurants in a specific zone that joined Uber Eats, Uber rideshare trips in the same zone were 2.12 percent (p = .001) fewer compared to zones where no restaurant joined Uber Eats. Our result highlights the hidden cost of Uber's diversification. Based on Uber's Manhattan trip volume during 2016 (41,973,334 trips) and the average proportion of restaurants that joined Uber Eats in 2016 (4%), we can infer that, on a yearly basis, the launch of Uber Eats reduced Uber's potential trip volume in Manhattan by about 3,359,339 trips. We estimate the average fare per trip to be $13.[35] Because Uber typically collected 20 percent of the fare, these numbers imply that the launch of Uber Eats resulted in a loss of about $0.9 million in revenue per year just from the rideshare business in Manhattan.

[35] While we do not have information on average fare for rideshare trips, trips provided by rideshare platforms and taxis in NYC exhibited similar average trip distances/durations. From the database, we calculate the average trip distance (2.97 miles) and duration (15 minutes) of taxi trips during 2016. In NYC, Uber charged $1.75 per mile and $0.35 per minute with a base fare of $2.55 during 2016 (Furfaro, 2016). Assuming average trip distances/durations of Uber were similar to those of taxis, we estimated the average fare to be about $13.

Insert Table 10 about here

3.4.2 Cross-platform-firm spillover effect

Next, we compare changes in Lyft trip volumes in zones where at least one restaurant joined Uber Eats and changes in zones where no restaurant joined Uber Eats. The coefficient in Column 3, Table 10 suggests that, for every one percent of restaurants in a specific zone that joined Uber Eats, Lyft rideshare trips in the same zone were 6.43 percent (p = .002) fewer than in zones where no restaurant joined Uber Eats. Our result shows that Lyft partially paid the cost of Uber's diversification. We estimate that the launch of Uber Eats reduced Lyft's potential trip volume in Manhattan by about 1,337,594 trips and caused a loss in revenue by about $0.3 million per year just from the rideshare business in Manhattan.

3.4.3 Demand conditions in the existing business segment

Lastly, we investigate the heterogeneous effects across different demand conditions. Column 2 in Table 10 compares the effects of Uber Eats on Uber trip volumes during rush and non-rush hours. It shows that, while the launch of Uber Eats had a negative effect on Uber trip volumes during both rush and non-rush hours, the effect was weaker during rush hours (-0.7%, p < .001) than non-rush hours (-2.57%, p < .001). Column 4 compares the effects of Uber Eats on Lyft trip volumes during rush and non-rush hours. It shows that the decline in Lyft trip volumes was weaker during rush hours (-4.84%, p = .021) than non-rush hours (-6.92%, p = .001).

3.4.4 Alternative explanations and mechanisms

The DID models enable us to control for unobservable time-variant factors that affected both the treated and control groups. However, there might be unobservable time-variant factors that influenced only the treated but not the control group, thereby generating differences between the two groups that were similar to those predicted by our theory. Therefore, we conducted a few analyses to rule out alternative explanations and mechanisms.

3.4.4.1 Unobservable time-variant factors from the demand side

The launch of Uber Eats might have incentivized diners who used to go to restaurants to order food delivery instead. That is, the relative reduction in rideshare trips in the treated group might be caused by some users switching to food delivery from visiting restaurants in rideshare vehicles. While our interview with rideshare drivers confirmed that drivers did not experience significant changes in trips that ended near restaurants, we conduct the following analyses to account for this possibility.

First, if users in the treated group took private transportation services (i.e., taxis and rideshare services) to visit restaurants before but later switched to Uber Eats, we would see a similar reduction in the growth rates of taxi trips for the treated group after the launch of Uber Eats. We replicate the models in Table 10 to estimate the effect of the launch of Uber Eats on taxi trip volumes. The coefficient in Column 1, Table 11 shows that the launch of Uber Eats did not have a statistically significant impact on taxi trip volume.

Insert Table 11 about here

Second, it is possible that users who had used Uber and Lyft to visit restaurants had a higher likelihood of switching to Uber Eats compared to those who had used taxis. For instance, the younger population is more familiar with smartphones and may be more likely to use Uber than taxis to visit restaurants. If our results were caused by younger users switching to Uber Eats, zones with younger populations would exhibit a larger reduction in rideshare trips compared to zones with older populations. To test this possibility, we employ a triple differences model by including Age_j in equation (6):

$$Y_{ijht} = \beta_0 + \beta_1 Post_t UE_j + \beta_2 Post_t UE_j Age_j + \beta_3 X_{jht} + \alpha_j + \delta_h + \gamma_t + \tau_t + \varepsilon_{ijht} \qquad (6),$$

where Y_{ijht}, $Post_t$, UE_i, and X_{jht} are as defined earlier. Age_j is a binary variable that equals one if the median age in a given zone was above the median age for the entire Manhattan (39) in 2015, and zero otherwise.[36] Results in Table 12 suggest that the effect of the launch of Uber Eats did not vary significantly with population age. We also re-estimate equation (3) where Age_j represents the median population age in zone *j* during 2015. Our results are similar.

Insert Table 12 about here

Finally, if our results were mainly driven by users switching from going to restaurants via rideshare services or taxis to ordering through delivery platforms, we should see a similar drop in rideshare trips with the launch of other food delivery platforms. To test this, we re-estimate the changes in rideshare trips following the launch of DoorDash in April 2015, initially in Brooklyn only (Bromwich, 2015). We employ a DID model that compares the trip volumes of Uber and Lyft between zones in Brooklyn and zones in other boroughs (i.e., The Bronx, Manhattan, Queens, and Staten Island) before and after the launch of DoorDash (i.e., between March 2015 and May 2015, before it was launched outside Brooklyn). Results in Table 13 show that the launch of DoorDash did not have a significant impact on either Uber or Lyft trip volumes.

Insert Table 13 about here
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[36] Zones in Manhattan exhibited a significant variation in median population age. In 2015, the average median age was 39, the minimum median age was 30, and the maximum median age was 55.

3.4.4.2 Resource utilization across businesses vs. profits in the new business

We argue that the reduction in rideshare trip volume was caused by incumbent rideshare drivers reallocating their resources to the food delivery business to maximize utilization. An alternative explanation is drivers reallocating resources to seek higher profits in the food delivery business. We believe this is unlikely in my context: The average hourly income (net of expenses) in NYC was $14.25 for rideshare drivers and $12.21 for food delivery couriers (Figueroa et al., 2021; Parrott & Reich, 2018). Our interviews with rideshare drivers indeed confirm that they only switched to food delivery when the rideshare business was low.

If rideshare drivers reallocated their resources to food delivery because of higher profits in the new business instead of potential improvement in resource utilization, we should also observe a decline in rideshare trip volume following the launch of a non-Uber Eats food delivery platform. The last such platform launched in NYC was by DoorDash in March 2015. As discussed earlier, results in Table 5 show that the launch of DoorDash did not have a significant impact on either Uber or Lyft trip volumes. Therefore, Table 5 not only helps to rule out the alternative explanation of shifts in substitutive demand between rideshare and food delivery, but also invalidates the possibility that rideshare drivers shift to food delivery because of its higher profits.

3.4.4.3 Changes made by incumbent vs. new complementors

Our theoretical arguments have been focused on resource reallocation by incumbent complementors. However, a platform firm's diversification may also affect potential new complementors. On the one hand, a larger number of potential complementors may join the diversifying platform firm to maximize resource utilization between the two businesses. In our context, this would increase the volume of rideshare trips for Uber, making the results for H1 more conservative. On the other hand, opportunity in a new business may attract potential complementors away from the existing business and incentivize them to enter the new business instead, providing an alternative explanation for our results. We undertook a few analyses to address this alternative mechanism.

First, if Uber Eats were attracting potential new rideshare drivers, I should expect the same to happen with the launch of other food delivery platforms, such as DoorDash. Our results in Table 5 show that the launch of DoorDash did not have a significant impact on either Uber or Lyft trip volumes. Therefore, it is unlikely that the main results were driven by the reduction in new rideshare drivers shifting to Uber Eats.

Second, to further separate the effects of incumbent vs. new drivers, we take advantage of a subsequent NYC regulation (Local Law 147 of 2018) that paused the issuance of new licenses for rideshare vehicles. Following the city regulation, Uber stopped accepting new drivers in April 2019 (Rubinstein, 2019). Therefore, any changes in the rideshare volumes after April 2019 were caused by incumbent drivers. If our results were mainly driven by new drivers joining Uber Eats instead of Uber, after April 2019, we should not observe significant differences in Uber trip volumes between zones where the proportion of restaurants that joined Uber Eats increased and zones where the proportion remained the same. On the other hand, if incumbent Uber drivers had been reallocating resources to Uber Eats, we should still observe a relative reduction in Uber trip volumes in zones where the proportion of restaurants that joined Uber Eats increased compared to other zones even after April 2019.

To test this, we replicate the DID models in Table 2 but instead compared Uber trip volumes between May and December 2019, when no new drivers could join Uber (Table 14). In Column 1, the treatment is the proportion of restaurants that newly joined Uber Eats in a given zone after May 2019. In Column 2, we use a binary treatment effect where the treatment equals one if a given zone had at least one restaurant that newly joined Uber Eats after May 2019, and zero otherwise. These are weaker treatments compared to the original launch of Uber Eats in 2016. Still, our results hold.

Insert Table 14 about here

3.4.5 Robustness checks

We run a battery of supplementary tests to check the robustness of the results. First, continuous DID models assume a linear relationship between the treatment level and the dependent variable, which might not be the case. To estimate the average effect on all zones where at least one restaurant joined Uber Eats, we re-estimate the DID models using a binary treatment effect (i.e., whether a given zone had at least one restaurant that joined Uber Eats). The results are similar.

Second, our approach may be subject to a potential violation of SUTVA (Rubin, 1980). In our context, rideshare drivers who worked in zones where no restaurants joined Uber Eats (control group) might move to zones where at least one restaurant joined Uber Eats (treatment group) to work on Uber Eats, causing rideshare trip volumes in the control group to decrease. This would attenuate the result and make it more difficult to find support for our hypothesis. Thus, the point estimates could be interpreted as the lower bound of the true effect size.

Additional robustness checks include DID models using different treatment windows and analyses using trips during weekends. Our results hold.

3.5 Discussion and conclusion

The main objective of this study is to explore how diversification by a platform firm affects its existing business and that of its competitors. Using datasets on the rideshare and food delivery businesses in NYC, we find that Uber's expansion into the food delivery business reduced rideshare trip volumes for both Uber and Lyft. In addition, these negative effects were weaker during rush hours compared to non-rush hours.

Our study contributes to several strands of literature. First, it extends the platform literature by connecting it to the diversification literature. Research on platform scope has mostly focused on platform firms' vertical scope decisions, such as decisions to enter complementors' businesses (Gawer & Henderson, 2007; Li & Agarwal, 2017). We complement these studies by highlighting the impact of horizontal diversification for both the diversifying and competing platform firms through complementors' resource reallocation.

Second, we address concerns in the resource-based view (RBV) literature that "strategy scholars have spent little time considering how RBV's precepts apply to platform (firms)" (Eisenmann et al., 2011: 1282). Prior studies have mainly explored benefits that platform firms gain from forfeiting control rights over complementor resources, such as access to diverse knowledge from a large pool of complementors. In contrast, our findings suggest that the lack of control rights over complementor resources may impair platform firms' ability to forbid complementors from reallocating resources across business segments and platforms, thereby causing negative within-platform-firm and cross-platform-firm spillover effects. Through these analyses, we answer the call for a stronger connection between platforms and mainstream theories in strategy (Gawer, 2021: 13; Rietveld & Schilling, 2021: 1547).

The paper is subject to a few caveats that create opportunities for future studies. First, a unique feature of our empirical context is that the majority of complementors in the new business (e.g., bike couriers in the food delivery business) are less likely to switch to the existing business (rideshare business) due to the low fungibility of their resources (e.g., bikes cannot be used for rideshare trips). This enables us to study complementors in the existing business without the complication of a reverse flow of complementor resources from the new business to the existing business. Future studies may examine how complementor engagement changes direction with the level of resource fungibility across businesses or platforms.

Relatedly, we lack empirical data to study the spillover effect of platform diversification on competitors in the new business. The launch of Uber Eats can influence not only competing platform firms in the rideshare business (e.g., Lyft) but also those in the food delivery business (e.g., DoorDash, Grubhub, or Postmates). Intuitively, there could be two countervailing effects. On the one hand, a diversifying platform firm may divert complementors (i.e., food delivery couriers) away from competing platform firms in the new business, thereby weakening their market power. On the other hand, complementors from its existing business may start working for competitors in the new business after accumulating enough experience in the new business, thereby strengthening competitors' market power. Future studies can explore such dynamics when data become available.

Chapter 4

Adjustment Costs and Complementor Engagement Following Platform Acquisition

4.1 Introduction

Platforms have often used mergers and acquisitions (M&As) as a main growth strategy to enlarge their network sizes and market share. For instance, Airbnb has enlarged its size by acquiring other home rental platforms such as Urbandoor and Vamo. Uber Eats has fueled its growth by acquiring rival food delivery platforms such as Postmates and Careem.[37]

With the increasing prevalence of M&As in platform businesses, a small number of studies have started to investigate platform M&As (Farronato et al., 2023; Miric et al., 2021; Parker et al., 2021), mainly focusing on the synergies from redeploying platform resources (e.g., Uber Eats' routing technology) and stronger network effects from an enlarged user base.[38] With rare exceptions (e.g., Farronato et al., 2023), the role of complementors has mostly been neglected.[39] Given that platforms rely on complementors to create value (Gawer & Cusumano, 2002), it becomes imperative to explore how platforms' reliance on complementors may shape the consequences of platform M&As differently compared to non-platform M&As (Kretschmer et al., 2022: 419; Rietveld & Schilling, 2021: 1549).

[37] I define platforms as organizations that act as intermediaries and facilitate transactions between firms and/or individuals (Cusumano et al., 2019). Complementors are independent providers of products or services that are built on a platform (Brandenburger & Nalebuff, 2011; Kapoor & Lee, 2013).
[38] The paper focuses on the *horizontal* mergers or acquisitions between platforms. It does not investigate platforms' *vertical* acquisitions of complementors (e.g., Eisenmann et al., 2011; Li & Agarwal, 2017).
[39] Farronato et al. (2023) examine how users on the *acquiring* platform were influenced by a platform acquisition. In contrast, I focus on complementors on the *target* platform.

Non-platform M&As are known to create synergies for the merged firm by redeploying underutilized resources or divesting redundant resources (Anand & Singh, 1997; Capron et al., 1998; Graebner, 2004; Kaul & Wu, 2016).[40] Such synergies enhance the competitive advantage of the merged firm at an indirect cost of competing firms (Chatterjee, 1986; Clougherty & Duso, 2011). The effect of M&As on the merged and competing firms is claimed to be even stronger for platforms. In addition to the synergies, stronger network effects from the enlarged user base on the merged platform may attract more complementors, which could benefit the merged platform and harm competing platforms (Miric et al., 2021; Parker et al., 2021).

One underlying assumption behind these arguments is that the merged firms have strong control over resources in the acquiring and target firms.[41] In contrast, a unique feature of platforms is their weak control rights over complementors and their resources (Chen et al. 2022, Jacobides et al. 2018, Kretschmer et al. 2022).[42] Absent control rights over complementors' resources, the merged platform cannot forbid complementors on the target platform from exiting the platform. For instance, after acquiring Postmates, Uber Eats cannot forbid restaurants on Postmates from leaving Uber Eats or joining other food delivery platforms, such as DoorDash or Grubhub. Without accounting for complementors' choice of engagement with platforms, our understanding of platform acquisitions may be incomplete. Against this background, I examine how complementors on the target platform engage across platforms following a platform acquisition.

[40] While separate strands of literature have offered alternative theoretical explanations for M&As, such as market power (e.g., Eckbo, 1983; Kim & Singal, 1993) information signaling (e.g., Clougherty & Duso, 2009; Song & Walkling, 2000), I build upon the resource-based view and study acquisitions as a means to access resources in the target firm.

[41] The human capital literature argues that firms may not have full control over human capital (e.g., CEOs, employees, etc.). I discuss in detail in Section 4.2. on how complementors are different from human capital.

[42] Similar to non-platform firms, platforms have strong control rights over *platform* resources, such as Uber's routing technologies. Given that the core competitive advantage of platforms (i.e., network effects) arise from complementors, I focus on complementors and complementors' resources throughout the paper.

A platform acquisition increases the number of users on the merged platform and offers complementors stronger network effects (Farronato et al., 2023; Miric et al., 2021). However, I argue that not all complementors benefit equally from the enlarged user base. In particular, complementors on the target platform (*target complementors*) will incur adjustment costs following the acquisition.[43] The adjustment costs include the direct costs of redeploying resources from the target to the merged platform (Cennamo et al., 2018; Sakhartov & Folta, 2014) as well as the indirect costs of losing platform-specific investments made on the target platform (Argyres et al., 2023; Zhu & Liu, 2018). In tandem, the direct and indirect adjustment costs result in a competitive disadvantage for target complementors relative to complementors on the acquiring platform (*acquirer complementors*). For instance, restaurants on Postmates had to tailor their menus and adjust order management systems after Postmates was acquired by Uber Eats. Moreover, Postmates restaurants couldn't transfer customer ratings and reviews on Postmates to Uber Eats, which put them in a disadvantaged position to attract users. The competitive disadvantage may escalate with the number of acquirer complementors and incentivize target complementors to not join the merged platform.

Next, I explore how experiences of operating on multiple platforms (i.e., multi-homing) will moderate target complementors' choice to join the merged platform. On the one hand, target complementors that had also operated on the *acquiring* platform before the acquisition will face lower adjustment costs (Cennamo et al., 2018) and less marginal increase in competition when joining the merged platform. Therefore, they will have a weaker incentive to not join the merged platform. On the other hand, target complementors that had also operated on *competing* platforms (that rival both the acquiring and target platforms) will face lower adjustment costs in

[43] Adjustment costs are also referred to as "repositioning costs" (Menon & Yao, 2017; Seamans & Zhu, 2017) or "redeployment costs" (Helfat & Eisenhardt, 2004; Sakhartov & Folta, 2014).

joining the *competing* platforms, which provide a viable outside option (Toh & Agarwal, 2023; R. Wang & Miller, 2020). Consequently, they will have a stronger incentive to not join the merged platform and switch to the competing platforms.

I empirically test my predictions in the U.S. food delivery industry. On December 1st, 2020, Uber Eats completed the acquisition of Postmates for $2.65 billion (Uber Newsroom, 2020). Restaurants on Postmates had to decide whether to join the merged Uber Eats platform or leave by June 2021, when the two platforms were fully integrated. Using a continuous difference-in-differences (DID) model, I investigate Postmates restaurants' engagement on Uber Eats and competing platforms (i.e., DoorDash and Grubhub) before and after the acquisition. In particular, I compare Postmates restaurants' engagement on Uber Eats in cities where Uber Eats had a larger number of restaurants before the acquisition to that in cities where Uber Eats had a smaller number of restaurants. I find that Postmates restaurants were less likely to join the merged Uber Eats platform (and more likely to switch to competing platforms such as DoorDash and Grubhub) when Uber Eats had a larger number of restaurants prior to the acquisition. In addition, this effect was weaker for Postmates restaurants that had also operated on Uber Eats but stronger for those that had also operated on DoorDash or Grubhub before the acquisition.

4.2 Theoretical Development

4.2.1. Acquisitions and control rights over resources

Acquisitions are often used as the main growth strategy for firms. In particular, the resource perspective claims that acquisitions enable firms to create synergies by gaining access to resources that are difficult to gain via internal development (Anand & Singh, 1997; Capron et al., 1998; Graebner, 2004; Kaul & Wu, 2016), e.g., due to time compression diseconomies

(Dierickx & Cool, 1989). At the same time, studies have shown that most acquisitions fail to create value. One of the main reasons for such failures is the difficulty of post-merger integration (PMI) (Graebner et al., 2017), which causes disruption in routine (Graebner, 2004; Puranam et al., 2006) and firm scope (Kaul, 2012; Tandon et al., 2023) in the target firm after acquisitions. Consequently, the success of acquisitions often depends on the effective uses of target firms' resources: redeploy strategic resources to their most efficient uses and divest redundant resources.

Synergies that arise from the effective use of resources may enhance the competitive advantage of the merged firm, which indirectly harms competing firms that do not benefit from the synergies (Chatterjee, 1986; Clougherty & Duso, 2011). One underlying assumption behind these arguments is that firms have strong control rights over resources in the target firm. Without tackling this assumption, recent studies on platforms have pointed out that the positive effect of acquisitions on the merged firms (as well as the negative effect on competing firms) could be stronger and more salient for platform acquisitions. Similar to non-platform firms, platforms may realize synergies by redeploying platform resources, such as platform infrastructure (Giustiziero et al., 2023).[44] In addition to the synergies from platform resources, platforms may generate stronger network effects from the enlarged user base on the merged platform, thereby attracting more complementors (Miric et al., 2021; Rochet & Tirole, 2003). In sum, the existing literature suggests that a platform acquisition may benefit the merged platform and damage competing platforms (Jacobides & Lianos, 2021; Parker et al., 2021).

I re-examine this argument in a context where firms might not have strong control over resources: platform acquisitions in which platforms have weak control rights over

[44] For example, in my empirical context, Uber CEO Dara Khosrowshahi claimed that the company would gain "$200 million in cost-saving synergies … (redeploying) Uber's routing technology." (Patnaik & Bellon, 2020)

complementors and their resources.[45] A key feature of platforms is their reliance on a large number of complementors (Gawer & Cusumano, 2002). While this enables the platform to harness network effects, it also deprives it of its control rights over complementors and ownership rights over complementors' resources (Jacobides et al., 2018; Kretschmer et al., 2022). Absent strong control rights over complementors, the merged platform cannot force target complementors to join the merged platform after a platform acquisition.

While retaining human capital in the target platform (e.g., CEOs, employees, inventors) has been pointed out as a critical issue in non-platform acquisitions (Graebner et al., 2017; Hambrick & Cannella Jr, 1993; Meyer-Doyle et al., 2019; Paruchuri et al., 2006), movement of human capital across firms is limited compared to the movement of complementors across platforms for several reasons. First, (non-platform) firms incentivize or require employees to make firm-specific investments (Wang et al., 2009) and possess complementary assets (Seo & Somaya, 2022) that limit the potential value that could be created outside the firm. Second, firms are often protected by non-compete agreements and patent enforcement, which create mobility frictions and limit the transfer of human capital to other firms (Starr et al., 2019). Lastly, formal contracts with firms forbid employees from working at multiple firms at the same time.[46]

In contrast, compared to non-platform firms, platforms cannot force complementors to join, stay, or exit platforms (Jacobides et al., 2018; Kretschmer et al., 2022). Moreover,

[45] A small number of studies have investigated the impact of acquisitions when firms do not have full control over resources (Tandon et al., 2023) or users (Rogan, 2014; Rogan & Greve, 2015). The number of firms involved in such relationships tended to be limited and therefore within the capacity of individual firms to avoid potential hazards. My study builds upon this stream of literature by examining a context where the number of firms involved (i.e., complementors) is too large that it is impossible for firms (i.e., platforms) to manage each individual relationships.

[46] Complementors may be similar to short-term contract workers, who are also allowed to work on multiple firms at the same time. However, the lack of formal contracts that specify working hours and the quantity/quality of services causes the movement by complementors to be more salient, which could strengthen network effects and result in "winner-take-all" outcomes (Rochet & Tirole, 2003).

complementors can operate on multiple platforms at the same time (Cennamo et al. 2018, Chen et al. 2022), which may cause the movement of complementors across platforms (and the redeployment of resources across platforms) to be more fluid and at a much larger scale. Because platforms have to rely on network effects to attract users, the massive movement of complementors can be costlier for platforms than for traditional firms. Consequently, “there remains much to be understood about how platform mergers might influence market structure” (Rietveld & Schilling, 2021: 1549), and we need to examine “to what degree the merger and acquisition strategies of platforms are similar to or different from those of traditional firms” (Kretschmer et al., 2022: 419). In particular, given the unique feature of platforms (i.e., reliance on complementors), understanding individual complementors’ incentives to engage with the merged platform becomes critical in fully examining the impact of a platform acquisition. Therefore, I explore the following question: *How does a platform acquisition influence the target platform’s complementor engagement on the merged platform?*

4.2.1 Platform acquisition, competition among complementors, and adjustment costs

A platform acquisition could have two countervailing effects on complementors. On the one hand, a larger user base on the merged platform enables complementors to enjoy stronger network effects (Farronato et al., 2023; Miric et al., 2021). The benefits may increase complementors’ profit and incentivize them to increase their engagement on the merged platform. On the other hand, the increased number of complementors on the merged platform intensifies competition within the platform and lowers their profits (Boudreau, 2012; Cennamo & Santaló, 2019). This may cause complementors to decrease their engagement on the merged platform.

Whether the benefit from a larger user base or the costs from intensified competition dominate may depend on the adjustment costs that complementors encounter upon joining the merged platform. Firms incur adjustment costs when redeploying resources to a new environment (Helfat & Eisenhardt, 2004; Sakhartov & Folta, 2014), which could lead to a competitive disadvantage (Argyres et al., 2019; Penrose, 1959). After a platform acquisition, in contrast to acquirer complementors whose products or services are already tailored to the merged platform, target complementors need to adjust their resources to the platform-specific architectures of the merged platform (Anderson et al., 2014; Argyres et al., 2023). Adjusting resources to a different platform often downgrades the quality of products or services (Cennamo et al., 2018), which may cause target complementors to lose their existing users.

In addition to the direct costs of redeploying resources to the merged platform, target complementors also face indirect adjustment costs: the loss of platform-specific investment complementors have made in the target platform (e.g., customer ratings and reviews for restaurants on food delivery platforms). Platform-specific investments provide competitive advantages in competing against other complementors and attracting users (Zhu & Liu, 2018). Compared to acquirer complementors that could maintain their investments, target complementors face higher opportunity costs in joining the merged platform as they have to forego their investments on the target platform (Argyres et al., 2023). Consequently, target complementors may fail to attract new users even if the user base on the platform becomes larger. In tandem, the direct and indirect adjustment costs cause target complementors to suffer from a competitive disadvantage in attracting users relative to acquirer complementors.

The competitive disadvantage arising from the adjustment costs indicates that, even if the merged platform provides an enlarged user base as a result of the acquisition, target complementors will be in a disadvantaged position to attract those users.[47] When the number of acquirer complementors on the merged platform is larger, target complementors will have fewer opportunities to attract new users. Moreover, a larger number of acquirer complementors may even divert target complementors' existing users (from the target platform) to acquirer complementors. Therefore, the competitive disadvantage that target complementors encounter will be stronger when they have to compete against a larger number of acquirer complementors. This could incentivize target complementors not to join the merged platform.

Hypothesis 1 (H1). *Complementors on the target platform are less likely to operate on the merged platform after a platform acquisition when the relative number of complementors on the acquiring platform is larger.*

4.2.2 Multi-homing complementors

Because platforms cannot forbid complementors from joining other platforms, a complementor often operates on multiple platforms (i.e., multi-homing) (Cennamo et al. 2018, Chen et al. 2022). For instance, a target complementor may have multi-homed with either the acquiring or competing platforms prior to a platform acquisition.

While multi-homing enables a complementor to gain access to a broader user base (Corts & Lederman, 2009), it also incurs multi-homing costs (Cennamo et al. 2018, Chen et al. 2022). For instance, restaurants receive food delivery orders from tablets that are provided by food delivery platforms. Restaurant employees have to manually enter the orders from those tablets into their order management systems. The workload of scheduling and providing food delivery services to multiple platforms complicates operations, causing a cognitive burden on employees

[47] Theoretically, I follow prior studies in assuming that the number of users and complementors on a platform are positively correlated, which has been the premise of (indirect) network effects (Rochet & Tirole, 2003). Empirically, I control for the number of users on each platform.

and coordination costs for restaurants (Zhou, 2011).[48] Therefore, multi-homing may be justified only when a complementor can obtain enough demand across platforms. The multi-homing experiences with different platforms might influence the adjustment costs that target complementors have to encounter, which may cause them to respond differently after a platform acquisition.

4.2.2.1 Multi-homing with the acquiring platform

When a target complementor has operated on the acquiring platform before a platform acquisition, the complementor has already adjusted some of its resources (e.g., restaurants' order management systems, kitchen facility) to the architecture of the acquiring platform. Consequently, the cost of adjusting the remaining resources that were used on the target platform to the merged platform would be relatively low (Cennamo et al., 2018), which would alleviate the threat of losing existing users. In addition, the prior experience on the acquiring platform indicates that the target complementor has made at least partial investments on the acquiring platform, which mitigates its disadvantages in attracting new users.[49] Lastly, the target complementor has already been competing against acquirer complementors before the acquisition, which alleviates competitive pressures from joining the merged platform. Therefore, compared to complementors that only operated on the target platform, complementors that operated on both the target and acquiring platforms face lower adjustment costs, which incentivize them to be more likely to join the merged platform after a platform acquisition.

[48] Several restaurant owners complained that they had to hire a separate employee just to manage orders from food delivery platforms (Houck, 2017).

[49] The prior experience on the acquiring platform does not fully compensate the competitive disadvantage, as a multi-homing target complementor still has some resources allocated and invested on the target platform (that need to be redeployed or foregone upon joining the merged platform).

Hypothesis 2 (H2). *The effect in H1 is weaker for complementors on the target platform that operated on the acquiring platform before the acquisition.*

4.2.2.2 Multi-homing with competing platforms

In a similar vein, when a target complementor has operated on the competing platform before a platform acquisition, its products or services are already tailored to the architecture of the competing platform. The lower adjustment costs in switching to competing platforms provide a viable outside option for the complementor (Toh & Agarwal, 2023; Wang & Miller, 2020).

If a complementor expects to earn a lower profit after joining the merged platform, it might not choose to operate on both the merged and competing platforms, as the decreased demand on the merged platform might not compensate for the multi-homing costs. Facing a competitive disadvantage relative to complementors on the acquiring platform, complementors on the target platform that also operated on the competing platform may find switching to the competing platform attractive. Therefore, compared to complementors that only operated on the target platform, complementors that operated on both the target and competing platforms face lower adjustment costs in switching to the competing platform, which incentivizes them to be more likely to leave the merged platform after a platform acquisition.

Hypothesis 3 (H3). *The effect in H1 is stronger for complementors on the target platform that operated on competing platforms before the acquisition.*

4.3 Empirical Design

The empirical context is the U.S. food delivery industry. The industry has experienced explosive growth after the outbreak of COVID-19. The U.S. market size reached $55 billion in 2020, with four food delivery platforms (DoorDash, Grubhub, Uber Eats, and Postmates) dominating the industry (Beyrouthy, 2023). Summary statistics based on my data show that (1) the food delivery industry represented about 12% of the total restaurant sales and (2) DoorDash provided the largest share (58%) of total sales, followed by Uber Eats (20%), Grubhub (14%), and Postmates (7%). The remaining food delivery platforms had less than 2% of the market share.[50] The statistics are similar to the market share provided by Bloomberg, validating the representativeness of my data (Bloomberg Second Measure, 2023). The average amount per order during 2021-2022 was $25, which was similar across different platforms (Sullivan, 2022).

I investigate how Uber Eats' acquisition of Postmates in December 2020 influenced restaurants' engagement across the merged platform (i.e., Uber Eats) and competing platforms (i.e., DoorDash and Grubhub). On December 1st, 2020, Uber Eats completed the acquisition of Postmates for $2.65 billion (Uber Newsroom, 2020).[51] Restaurants that operated on Postmates had about six months to transfer to Uber Eats until June 2021, when the Postmates app for restaurants was integrated with the Uber Eats app for restaurants.[52]

This is an appropriate context to test my theory for several reasons. First, the success of food delivery platforms depends on aggregating a large number of restaurants (i.e., complementors). To satisfy the skyrocketing demand for food delivery services, platforms excessively concentrated on attracting popular restaurants. This incentivized restaurants to actively join, switch, or exit from multiple platforms, enabling me to directly observe complementor engagement across different platforms.

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[50] These platforms were Beyond Menu, delivery.com, DoorstepDelivery.com, EatStreet, Favor Delivery, Foodler, Groupon, Munchery, SkipTheDishes, Waitr (changed to ASAP in 2022).

[51] The announcement of the acquisition was first made on July 6th, 2020 (Siddiqui & Telford, 2020).

[52] While the Postmates app for users still operate as a separate app, the app shows the same restaurants as users will see on the Uber Eats app.

Second, the local nature of the food delivery services allows me to treat each geographic area (i.e., city) as a separate market. The market shares of food delivery platforms vary significantly across different cities. Figure 11 reveals that there existed substantial heterogeneity in the pre-merger market share of Uber Eats (in terms of restaurant numbers). On average, 39% of restaurants operated on Uber Eats in November 2020, but the proportion varied from 19% to 58%. This enables me to employ a continuous DID model to examine the heterogeneous (post-merger) impacts of the acquisition on restaurants depending on the (pre-merger) market shares of food delivery platforms.

Insert Figure 11 here

Third, due to the COVID-19 pandemic, starting from March 2020, most states and cities in the U.S. implemented commission fee cap regulations for major food delivery platforms that limited the commission fees on restaurants to 15%. This allows me to mitigate heterogeneous platform-specific factors that would influence restaurants' decisions to operate on a platform.

4.3.1 Data and Sample

I draw on data from multiple sources. First, I obtain anonymized proprietary data on card transaction records from Facteus, a provider of financial data for business analytics. The data contains about 10 billion transactions from 34 million debit and credit cards from 2019 to 2022 and represents about 7-10% of U.S. total spending during the period.[53] Each transaction contains information on transaction time, amount, merchant, food delivery platforms, merchant category

[53] The data consists of 28 million debit cards and 6 million credit cards. The sample roughly follows the overall U.S. spending patterns and has been used by the Federal Reserve Bank of Chicago and several research teams to track U.S. consumer spending and to build economic indices (e.g., Brave et al., 2021; Chetty et al., 2020; Karger & Rajan, 2020).

codes, unique card identifiers, cardholders' dates of birth, and zip codes.[54] To identify transactions that occurred on restaurants and food delivery platforms, I focus on transactions with the merchant category codes 5812 ("Eating places and restaurants") and 5814 ("Fast food restaurants") and transactions that have food delivery platforms on intermediaries. About 1.8 billion transactions were in these categories.

While the data enables me to observe restaurants' online and offline transactions, it is not without some potential selection bias. First, restaurants with small business sizes might not be shown in my data due to few transaction records. Second, the cardholders in my sample might have unobservable preferences toward certain food delivery platforms that incentivized them to use one platform over others. This would cause me to observe restaurants' engagement with only one platform, even if the restaurants were listed on multiple platforms.

To address these issues, I acquire restaurant listing data from YipitData, a data analytic company. The data contains historical monthly information of all U.S. restaurants that were listed on at least one of four major food delivery platforms (i.e., DoorDash, Grubhub, Uber Eats, and Postmates) from 2019 to 2022. The data contains information on restaurant name, address (street, city, state, zip code), and food delivery platforms that the restaurants were listed on. Because the data is scrapped online, restaurant names could differ across different months and platforms. To construct a unique list of restaurants, I matched restaurants across four food delivery platforms over 36 months.

Lastly, I obtain population demographics information from the American Community Survey and the number of Coronavirus cases in the U.S. from the New York Times.

[54] To protect privacy and avoid individual users being identified from the record, Facteus has injected random noise that follows a uniform distribution into transaction time (mean: 15 minutes), amount (mean: 3% of the actual amount), and users' year of birth (mean: 2 years). Because my sample is aggregated at the month level, the added perturbations would not affect my result.

The sample period is from June 2020 to May 2021, six months before and after the merger between Uber Eats and Postmates. Started by California on March 19th, 2020, the majority of the U.S. states and cities enforced stay-at-home orders and other related restrictions (e.g., mask mandates) to stunt the spread of the coronavirus. By mid-April, 42 states had implemented statewide stay-at-home orders, and three states had partial orders.[55] These restrictions were maintained until June 2020 (except one state, New Mexico, which maintained the order until August 28, 2020). Therefore, the sample period does not involve any policy changes related to the pandemic, which could influence demand for food delivery services.

The data consists of 568,284 restaurants in 237 major U.S. cities. During my sample period, about 55% of restaurants operated on more than one food delivery platform (i.e., multi-homing). Among these restaurants, I focus on 349,156 restaurants that operated on Postmates (i.e., incumbent complementors on the target platform) for at least one month between June 2020 and November 2020, six months before Uber's acquisition of Postmates. About 24% of the restaurants also operated on Uber Eats, and 22% of the restaurants also operated on competing platforms, such as DoorDash or Grubhub. My final sample contains 8,379,744 observations across 237 U.S. cities and 12 months.

4.3.2 Variables

The main dependent variable, Y_{ict}, is a binary variable that equals one if restaurant *i* on Postmates operated on Uber Eats in city c in month *t*, and zero otherwise.

The main independent variable, $Treat_c$, is the relative size of Uber Eats compared to Postmates in terms of restaurant numbers in city c in November 2020, the month before Uber's

[55] I run a robustness test excluding the eight states that did not had full stay-at-home orders and my results hold.

acquisition of Postmates. That is, the treatment variable reflects the relative increase in competition among restaurants if a restaurant on Postmates decides to join Uber Eats.

$$Treat_c = \frac{The\ number\ of\ restaurants\ that\ were\ only\ on\ Uber\ Eats\ in\ city\ c}{The\ number\ of\ restaurants\ that\ were\ on\ Postmates\ in\ city\ c}$$

To ease the interpretation of the coefficient, I also construct a binary treatment variable that equals one for cities where Uber Eats had an above-average increase (28%) in the number of restaurants after the acquisition and zero otherwise.

Figure 12 compares the monthly number of restaurants on Postmates that were in the treated and control groups. They show that before the acquisition (grey line), restaurants in both groups exhibited similar monthly trends. However, after the acquisition, restaurants in the treated group were less likely to join Uber Eats compared to restaurants in the control group.

Insert Figure 12 here

The second independent variable, MH_UE_i, is a binary variable that equals one if restaurant *i* on Postmates in city *c* also operated on Uber Eats in November 2020. The third independent variable, MH_DDGH_i, is a binary variable that equals one if restaurant *i* on Postmates in city *c* also operated on DoorDash or Grubhub in November 2020.

Summary statistics (Table 15) at the restaurant-platform-city-month level reveal substantial heterogeneity in the relative number of restaurants that were on Uber Eats before the acquisition. On average, the increase in the number of restaurants was 30%, but the proportion varied from 11% to 86%.

Insert Table 15 about here

4.3.3 Specification

I employ a continuous DID estimation with the treatments being the relative number of restaurants on Uber Eats in each city before the acquisition of Postmates. That is, I examine the effect of the number of Uber Eats restaurants on the decisions of Postmates restaurants to join the merged platform or switch to competing platforms, such as DoorDash and Grubhub. The unit of analysis is at the restaurant-platform-city-month level. To test Hypothesis 1, I employ the following equation:

$$Y_{ict} = \beta_0 + \beta_1 Post_t Treat_c + \beta_2 X_{ict} + \alpha_i + \gamma_t + \varepsilon_{ict} \qquad (7),$$

where Y_{ict} and $Treat_c$ are as defined earlier. $Post_t$ is a binary variable that equals one for months after Uber's acquisition of Postmates (December 2020), and zero otherwise.

To test Hypotheses 2 and 3, I add MH_UE_i, MH_DDGH_i to the main independent variable in equation (7). Equation (8) is a triple-differences model where X_{ict} includes the number of COVID-19 cases in city *c* in month *t* and other two-way interaction terms between the independent variables.

$$\begin{aligned} Y_{ict} = {} & \beta_0 + \beta_1 Post_t Treat_c + \beta_2 Post_t Treat_c MH_{UE_i} + \beta_3 Post_t Treat_c MH_{DDGH_i} \\ & + X_{ict} B + \alpha_i + \gamma_t + \varepsilon_{ict} \qquad (8), \end{aligned}$$

All regressions include restaurant (α_i) and year-month (γ_t) fixed effects. Because the treatment is given at the city level, robust standard errors are clustered at the city level.

4.4 Results

4.4.1 Main effect

I first examine the impact of Uber Eats' acquisition of Postmates on Postmates restaurants' decision to join Uber Eats. Column 1, Table 16 shows that, for every 10% increase in the relative number of Uber Eats restaurants, Postmates restaurants were 2.6% less likely to remain on Uber Eats after the acquisition. For instance, in cities where the number of restaurants increased by 11%, Postmates restaurants were 17% more likely to join Uber Eats after the acquisition. However, in cities where the number of restaurants increased by 86%, Postmates restaurants were 2% less likely to join Uber Eats after the acquisition. Columns 2–4, Table 16 show that the effect was weaker for Postmates restaurants that operated on Uber Eats but stronger for Postmates restaurants that operated on competing platforms (i.e., DoorDash or Grubhub) prior to the acquisition.

Insert Table 16 about here

I next investigate the impact of Uber Eats' acquisition of Postmates on Postmates restaurants' decision to switch competing platforms, such as DoorDash or Grubhub. Column 1, Table 17 shows that, for every 10% increase in the relative number of Uber Eats restaurants, Postmates restaurants were 2.2% more likely to switch to competing platforms. For instance, in cities where the number of restaurants increased by 11%, Postmates restaurants were 6% less likely to switch to competing platforms after the acquisition. However, in cities where the number of restaurants increased by 86%, Postmates restaurants were 11% more likely to switch to competing platforms after the acquisition. Columns 2–4 show that the effect was weaker for Postmates restaurants that operated on Uber Eats but stronger for Postmates restaurants that operated on competing platforms (i.e., DoorDash or Grubhub) prior to the acquisition.

Insert Table 17 about here

To specify the competing platforms that Postmates restaurants switched to, I separate the dependent variable into *Operation on DoorDash* and O*peration on Grubhub*. Columns 2 and 4 in Table 18 indicate that multi-homing on any competing platforms per se does not increase the probability of switching to a particular platform. For instance, only Postmates restaurants that operated on Grubhub were more likely to switch to Grubhub and not Postmates restaurants that operated on DoorDash. This indicates that the lower adjustment costs arising from platform-specific investments were driving restaurants' decisions to switch to competing platforms.

Insert Table 18 about here

4.4.2 Mechanism tests

4.4.2.1 Parallel trend assumption

A core assumption in the DID model is the parallel trends assumption. In my context, the tendency for Postmates restaurants to join Uber Eats should not differ between the treated and control groups in the absence of Uber Eats' acquisition of Postmates. Figure 12 shows that the number of Postmates restaurants that joined Uber Eats increased in a similar trend for the treated and control groups.

To further confirm the parallel trends assumption, I re-estimate my models using placebo thresholds before the actual treatment (i.e., Uber Eats' acquisition of Postmates in December 2020). If my results were confounded by a potential pre-trend that affected the treated group but not the control group, the placebo thresholds should also show significant coefficients. I limited the sample to six months before Uber Eats acquired Postmates (June–November 2020) and replaced the *Post* variable with two placebo time variables (August 2020, October 2020). The results are shown in Table 19. Coefficients in Columns 1–4 are insignificant, which addresses the concern that there might be a pre-trend that artificially could have driven my results.

Insert Table 19 about here

4.4.2.2 Enlarged user base vs. competition between complementors

Next, I examine how complementors on the *acquiring* platform (i.e., restaurants on Uber Eats) responded to the acquisition. My theory predicts that *target* complementors suffer from a competitive disadvantage and are less likely to enjoy the enlarged user base on the merged platform. This indicates that *acquirer* complementors should benefit from the increased number of users on the merged platform as they possess a competitive advantage over target complementors. Therefore, the larger the number of target complementors prior to the acquisition, the more likely the complementors on the acquiring platform will join the merged platform.[56]

I use the same models in Equations 1 and 2 but changed the sample to 151,347 restaurants that were on Uber Eats before the acquisition. The treatment is the pre-merger market share of Postmates in terms of the number of restaurants. The treatment variable ranges from 33% to 69%. Table 20 shows that the larger the pre-market share of Postmates before the acquisition, restaurants on Uber Eats were more likely to remain on Uber Eats and less likely to switch to competing platforms (i.e., DoorDash or Grubhub).

Insert Table 20 about here
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[56] I assume that the number of users and complementors on a platform are positively correlated.

4.4.2.3 Restaurant age as a proxy for adjustment costs

My theory predicts that target complementors will not join the merged platform due to adjustment costs, which include the loss of platform-specific investments that were made on the target platform. If so, complementors from the target platform will be more incentivized not to join the merged platform when the investments made on the target platform are larger (Argyres et al., 2015, 2019). Compared to new complementors, older complementors face higher opportunity costs of joining the merged platform as they would have made relatively more investments on the target platform (Argyres et al., 2023; Cennamo, 2018; Zhu & Liu, 2018)

While I cannot directly observe platform-specific investments (e.g., customer reviews and ratings), I proxy them by using restaurant age. The assumption is that the longer the period a restaurant had operated on Postmates, the larger the Postmates-specific investments the restaurant had made on Postmates (i.e., efforts to maintain high ratings and good customer reviews). I added an interaction term, $RestaurantAge_i$, to the main independent variable in Equation 1. Table 21 shows that the tendency for Postmates restaurants not to join Uber Eats after the acquisition was stronger for Postmates restaurants that operated longer on Postmates before the acquisition.

Insert Table 21 about here

4.4.3 Robustness checks

I implement several supplementary tests to check the robustness of my results. First, I re-estimate my models using different sample periods, ranging from 3 months to 5 months before and after Uber's acquisition of Postmates. The results show that the effect of Uber's acquisition of Postmates on restaurants was similar across different time windows.

Second, my models assume competition among restaurants at the city level. However, in some large cities, restaurants might not compete against all restaurants in the city, depending on their locations. I re-estimate the models using a zipcode-level treatment variable. That is, I assume that restaurants only compete against other restaurants in the same zip code. One potential issue with this approach is that the results may be subject to a potential violation of SUTVA (Rubin, 1980). In my context, restaurants might be competing against not only restaurants in the same zip code but also against restaurants in adjacent areas with different zip codes. Consequently, the actual treatment intensity might be higher than my measure. This would attenuate my result, making it more difficult to find support for my predictions. Thus, the results using the zip code level treatment should be interpreted as the lower bound of the true effect size.

Third, some restaurants in my sample are chain restaurants with multiple branches, and the decisions to join or exit food delivery platforms could have been made by the headquarters of the parent companies, not by individual restaurant owners. I believe this factor is unlikely to confound my results for two reasons. First, 80% of all restaurants listed on food delivery platforms were independent restaurants and thus made their own decisions. Second, even within chain restaurants, franchised restaurants could make their own decisions regarding the operations on food delivery platforms (Sullivan, 2022). Only chain restaurants that were owned and directly managed by the parent companies did not make these decisions. Given that, I re-examine the models after removing chain restaurants from my sample. My results hold when only independent restaurants were used as the sample.

Fourth, eight states did not impose stay-at-home orders during the sample period, which could have negatively influenced demand for food delivery services.[57] If cities in these eight states had a high correlation with the treatment group, my results might be biased. Because the cities in these eight states in my sample are mostly located in rural areas with small food delivery market sizes and 95% of the U.S. population was effectively under the lockdown restrictions, it is unlikely this could have biased my results. Given that, I re-estimate the models after excluding cities that were in these states from my sample. My results hold.

Fifth, the main regression models use a continuous treatment to test the effect of changes in treatment intensity rather than the effect of the treatment *per se*. However, recent studies have pointed out the problems with a continuous treatment variable even when there are only two time periods (De Chaisemartin & d'Haultfoeuille, 2020). I re-estimate the models using a binary treatment variable. My results hold when the binary treatment variable is used.

Sixth, I employed triple-differences models to test Hypotheses 2 and 3. However, recent studies suggest that the interaction terms in linear fixed effect models could capture cross-unit variations rather than within-unit variations (that two-way fixed effects intend to capture) (Shaver, 2019). Following the suggestions from Shaver (2019), I re-estimate the models using subsample analyses and compared the coefficients across different subsamples. My results hold.

4.5 Discussion and Conclusion

The study explores the impact of a platform acquisition on complementors on the target platform and the role of adjustment costs in shifting complementor engagement across platforms.

[57] These states are Arkansas (AR), Iowa (IA), Nebraska (NE), North Dakota (ND), Oklahoma (OK), South Dakota (SD), Utah (UT), and Wyoming (WY). Specifically, five states (Arkansas, Iowa, Nebraska, North Dakota, South Dakota) did not implement stay-at-home orders while three states (Oklahoma, Utah, Wyoming) had partial enforcements due to the resistance of several major cities.

I argue that the larger the number of complementors on the acquiring platform, the larger the competitive disadvantages that complementors on the target platform face, which will incentivize them to abandon the merged platform. Using data on U.S. restaurants and their transaction records, I find that Postmates restaurants in cities where Uber Eats had a high market share before the acquisition were less likely to operate on Uber Eats (i.e., merged platform) and more likely to operate on DoorDash or Grubhub (i.e., competing platforms). However, the tendency of Postmates restaurants to switch from the merged to competing platforms was weaker for restaurants that also operated on Uber Eats and stronger for restaurants that also operated on DoorDash or Grubhub prior to the acquisition.

The main contribution of the study is to bridge the two streams of research on platforms and mergers and acquisitions. First, I extend the platform literature that mainly focuses on the effect of platform acquisitions on consumer welfare (Chandra & Collard-Wexler, 2009; Jeziorski, 2014). I argue that the benefits of platform acquisitions (i.e., redeployment of platform resources and network effects from the enlarged user base) might partially be offset by the loss of target complementors.[58] In doing so, the study highlights the cross-platform spillover effect of a platform acquisition that arises from complementors' engagement across platforms.

Second, I build upon the M&A literature and re-examine the consequences of acquisitions when firms lack strong control over target firms' resources. I show that in situations where resources are difficult to control, acquisitions may actually benefit competing firms at the cost of the acquiring firm, especially in markets where the acquiring firm has a larger market share before the acquisition (i.e., a larger number of acquirer complementors). Accordingly, the

[58] Farronato et al. (2023) show that the benefit of acquisitions from network effects can be offset by platform differentiation. I complement the study by highlighting the adjustment costs that target complementors incur, even when platforms are not differentiated and provide similar products or services to users.

study aims to forge a stronger bridge between research on platforms and corporate strategy (Gawer, 2021: 13; Rietveld & Schilling, 2021: 1547).

In addition, the study complements the M&A literature by connecting it with the literature on adjustment costs. While the concept of adjustment costs has been well-recognized in various topics, such as diversification (Helfat & Eisenhardt, 2004), competitive dynamics (Seamans & Zhu, 2017), and innovation shock (Argyres et al., 2019), less attention has been paid in the M&A literature. However, studies on post-merger integration have pointed out that the disruption on target firms caused by the acquisition (e.g., Graebner et al., 2017, Puranam et al., 2006) could potentially incur adjustment costs. I complement this stream of research and show that the adjustment costs could be critical in assessing the impact of acquisitions, especially in contexts when acquiring firms lack control or ownership over target firms' resources.

The study is subject to a few limitations that offer opportunities for future research. First, I study the effect of one platform acquisition (i.e., Uber's acquisition of Postmates). Analyzing millions of complementors within a single platform acquisition allowed me to eliminate unobserved heterogeneity across platforms and extract detailed data on complementor activities. However, the M&A literature suggests that the consequences of M&As could depend on several firm-level factors. In particular, a large number of studies show that the M&A outcomes could vary depending on the integration level between the acquiring and target firm (Graebner et al., 2017; Puranam et al., 2006). In my empirical setting, the Postmates app for restaurants was fully integrated with the Uber Eats app, forcing restaurants on Postmates to either mitigate to Uber Eats or leave the platform. Because the core motivation behind platform acquisitions is to enlarge their size and harness network effects, most target platforms were integrated after being acquired (Farronato et al., 2023; Li & Netessine, 2020).

Nevertheless, some platforms might want to maintain the target platform to preserve users and complementors on the target platform, even at the cost of weaker network effects. One such case would be cross-border acquisitions. When acquiring platforms in different countries, target platforms often continued to operate as a separate entity for several reasons, such as overcoming the liability of foreignness (Zaheer, 1995) and cultural distance (Siegel et al., 2013). For instance, Just Eat Takeaway, a European food delivery platform, acquired Grubhub in June 2020, but Grubhub continued to operate as a separate entity in the U.S. As such, the post-merger integration level may impose boundary conditions for my findings and warrant future studies of platform acquisitions with different levels of integration or in the international context.

Second, my theory assumes that complementors mainly earn profit by operating on platforms and do not consider outside options (e.g., offline sales) (Wang & Miller, 2020). Empirically, my sample period was during the pandemic, when most restaurants had to rely on food delivery platforms due to the stay-at-home orders. Future studies may investigate the impact of platform acquisitions in settings where complementors have reliable offline sales and higher bargaining power against platforms.

Third, I empirically focus on restaurants as complementors on food delivery platforms. However, the food delivery industry is a three-sided market where food delivery platforms facilitate interactions between users and two types of complementors: restaurants and food delivery couriers. My interviews with restaurant owners and food delivery couriers show that restaurant owners did not make their decisions based on the availability of food delivery couriers. Rather, food delivery couriers were more likely to join platforms with a large number of restaurants. Therefore, it is unlikely that the size of food delivery couriers in each geographic market might have influenced restaurants' decisions to join or leave food delivery platforms.

Nonetheless, future studies might explore how different types of complementors influence platform competition differently in multi-sided markets (Seamans & Zhu, 2014).

Fourth, my findings are based on the food delivery industry. Prior studies on (non-platform) M&As have used various contexts, including the information technology industry (Graebner, 2004), the manufacturing industry (Capron et al., 1998), and the pharmaceutical industry (Paruchuri et al., 2006). Given that an increasing number of industries are adopting platform business models, future studies may examine acquisitions by platforms across different industries.

Lastly, I examine how complementors respond to one strategic choice by platforms (i.e., acquisition). Prior studies have shown that acquisition is but one of many boundary choices that a firm could make (Tong & Li, 2011; Villalonga & McGahan, 2005). Future studies may investigate how complementors respond to different choices by platforms (e.g., alliances, divestitures).

Chapter 5
Conclusion

The broad objective of the dissertation is to investigate whether the theories of the firm (i.e., resource-based view) apply to the platform economy or not. By contracting platforms with traditional, hierarchical organizations, I examine how the unique feature of platforms (i.e., weak control rights over complementors) can result in different consequences of strategic choices (i.e., governance, diversification, and acquisition). In particular, I show that platforms' weak control rights over complementors enable complementors to operate on different platforms, thereby creating interdependencies across multiple platforms and businesses.

My dissertation aims to contribute to several strands of literature. First, and most importantly, I contribute to the resource-based view (RBV) of the firm by enriching the concept of resource ownership and typology. While the role of resources has been a central concern in the theory of the firm, "strategy scholars have spent little time considering how RBV's precepts apply to platforms" (Eisenmann et al., 2011: 1282), and the connection between resources and the scope of platform firms has puzzled strategy scholars (Jacobides et al., 2018; Kretschmer et al., 2022). For instance, Jacobides et al. (2018: 2270) stress that: "RBV mostly concern themselves with owned resources. … If firms gain from others participating in an ecosystem, but cannot fully control them, what does that imply for how they attain advantage?" Prior platform studies have argued that, by forfeiting ownership of particular resources, a platform can retain flexible access to diverse knowledge, thereby accelerating product innovation and harnessing network effects (Baldwin & von Hippel, 2011; Boudreau, 2010; Parker & Van Alstyne, 2018). In contrast, I suggest a potential limit to leveraging (unowned) complementor resources: the loss of complementors to competitors that could cannibalize their business. In doing so, I answer the call for stronger connections between platforms and mainstream theories (Kretschmer et al., 2022: 419; Rietveld & Schilling, 2021: 1547).

Second, the dissertation extends the research on within-firm interdependencies by connecting it with the research on platforms. Recent studies on platform governance show that a change in governance policies can significantly influence the activity scope of complementors (e.g., product portfolios) (Koo & Eesley, 2021; Rietveld et al., 2021; Tae et al., 2020). However, these studies "do not consider multi-homing of complementors" (Tae et al., 2020: 324) and are "conducted within the boundary of one platform" (Koo & Eesley, 2021: 961), thereby neglecting potential interdependencies that could arise from (multi-homing) complementor activities across platforms. I show that these interdependencies may bring unintended consequences of platform access restriction. Studies on within-firm interdependencies suggest that a reduction in firm scope (e.g., divestiture) can potentially destroy synergies and hurt performance (Feldman, 2014; Natividad & Rawley, 2016). By highlighting interdependencies across complementor activities, I address an important question in the platform literature, that is, "how do multi-homing (complementors) shape the co-evolution of different platforms?" (Koo & Eesley, 2021: 961).

Finally, my dissertation offers implications for platforms and industry regulators. For managers, I highlight the hidden costs of platform firms' strategic choices: the loss of complementors that arise from the interdependencies across platforms. Prior literature argues that platform policies need to consider "interactions that do not happen at firm's boundaries" (Boudreau & Hagiu, 2009: 187). My dissertation supports this suggestion and cautions platforms against making strategic choices without fully understanding complementors' interdependencies across platforms or businesses, as a small change in a complex system composed of numerous interdependent activities can cause unintended ripple effects (Ethiraj & Levinthal, 2004). In particular, I argue that managers should be aware of the strategic decisions (i.e., governance policy, diversification, and acquisition) not only by their own (platform) firm but also by competing (platform) firms, as they may divert complementors away from their own platforms.

In addition, while antitrust authorities have started to focus attention on platforms, both policymakers and academia have yet to conclude the best way to regulate platforms (Greenwood et al., 2017; Jacobides & Lianos, 2021; Katz, 2019; Parker et al., 2021). In July 2022, the U.S. House of Representatives published a report on the Investigation of Competition in Digital Markets. The European Commission proposed the Digital Markets Act, which was enforced in November 2022. Both focus on preventing platforms from abusing their power on complementors. I argue that antitrust regulations should not overlook potential interdependencies across platforms: Regulating dominant platforms might also damage smaller platforms by forcing (multi-homing) complementors to abandon both platforms. As such, misguided platform strategies might excessively constrain complementor activities and threaten the overall health of the platform ecosystem (Iansiti & Levien, 2004).

The dissertation is not without limitations that set boundary conditions for my predictions. My theory and empirical contexts mainly focus on transaction platforms, where the platforms' main role is to match complementors and users (Cusumano et al., 2019). These platforms include rideshare platforms (e.g., Uber, Lyft), food delivery platforms (e.g., Uber Eats, DoorDash), home rental platforms (e.g., Airbnb), and freelancing platforms (e.g., Upwork, Fiverr). In transaction platforms, interactions between complementors and users mainly occur offline, which create geographic constraints and limit the strength of network effects. In addition, complementors' resources tend to be physical (e.g., rideshare drivers' vehicles, restaurant owners' kitchen facilities) and are mostly capacity constrained. Therefore, once constraints are met, complementors' resources are subject to allocation decisions across platforms and businesses.

My theory may not apply to innovation platforms, where platforms offer technological building blocks that complementors can use to develop new complementary products or services (Gawer, 2014). These platforms include operating systems platforms (e.g., Microsoft's Windows, Google's Android, and Apple's iOS) and video game console platforms (e.g., Sony's PlayStation, Microsoft's Xbox, and Nintendo's Wii). In innovation platforms, interactions between complementors and users mainly occur online without geographical constraints. Consequently, network effects occur globally (rather than locally). In addition, complementors' resources tend to be scale-free (e.g., software contents, intellectual property) (Levinthal & Wu, 2010), and complementors often share (rather than reallocate) their resources across different platforms. Therefore, platforms' strategic choices may cause expansion effects (rather than cannibalization effects) across platforms and businesses.

Figures

Figure 1. Various governance modes and control over resources

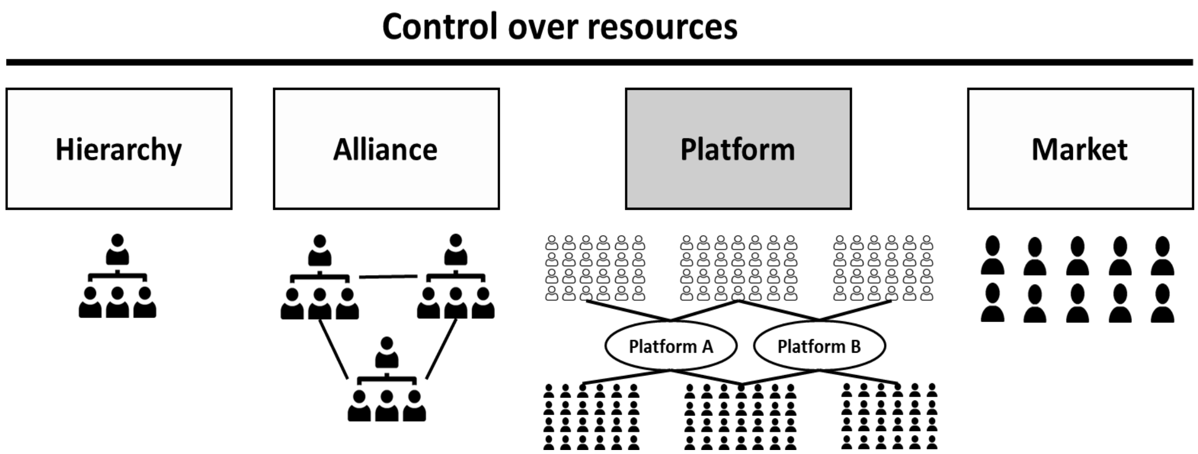


Figure 2. Empirical overview of three studies

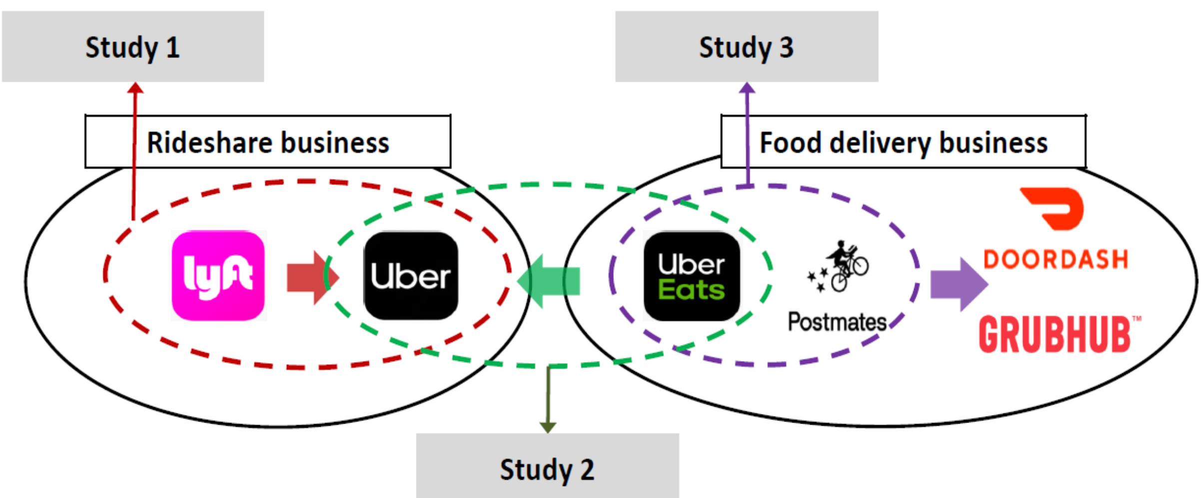
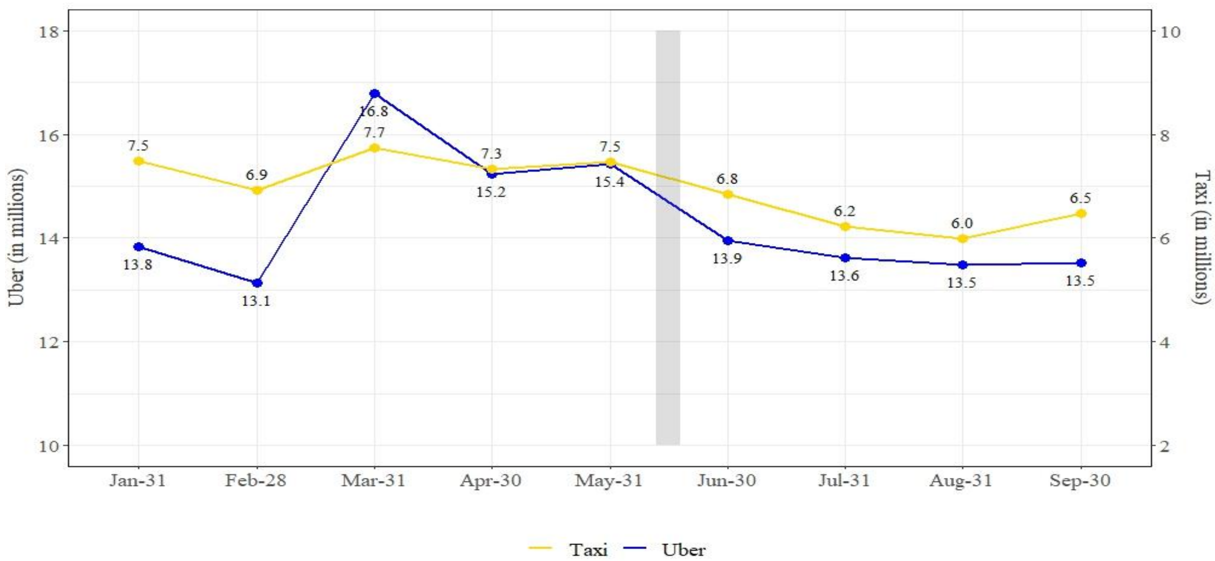


Figure 3. Monthly trip volume of Uber (left) and taxi (right) during 2019

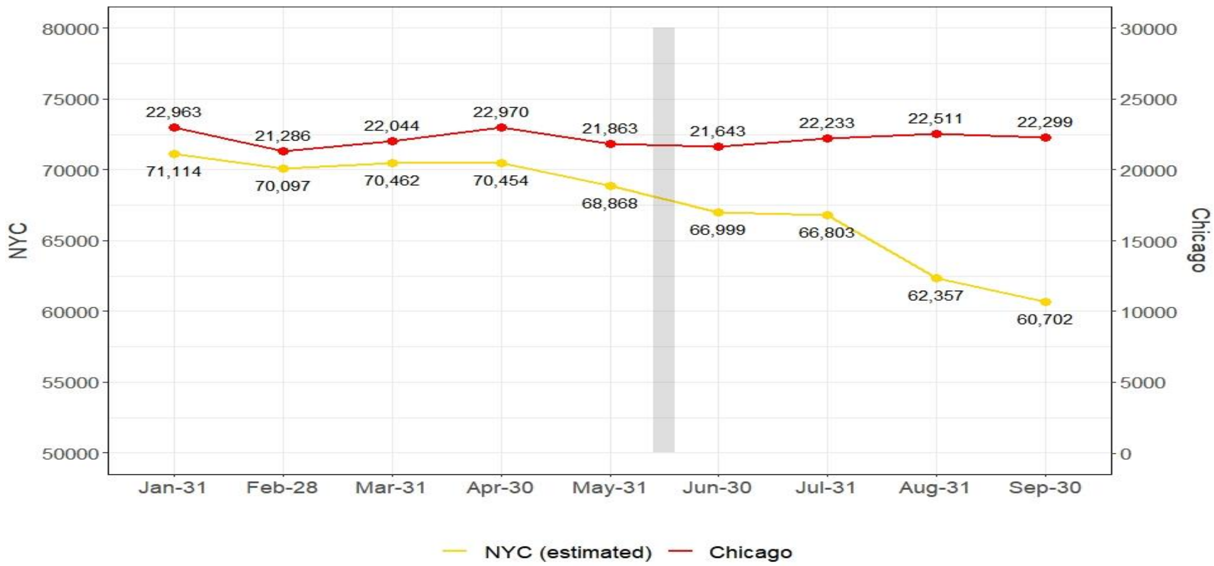


The grey line indicates the date for Lyft's access restriction (June 27th, 2019).

Figure 4. Monthly rideshare trip volume of NYC (left) and Chicago (right) during 2019

The grey line indicates the date for Lyft's access restriction (June 27th, 2019).

Figure 5. Monthly number of multi-homing drivers in 2019



The grey line indicates the date for Lyft's access restriction (June 27th, 2019).

Figure 6. Heat map of trip number by hour of the week for Uber (left) and Lyft (right)

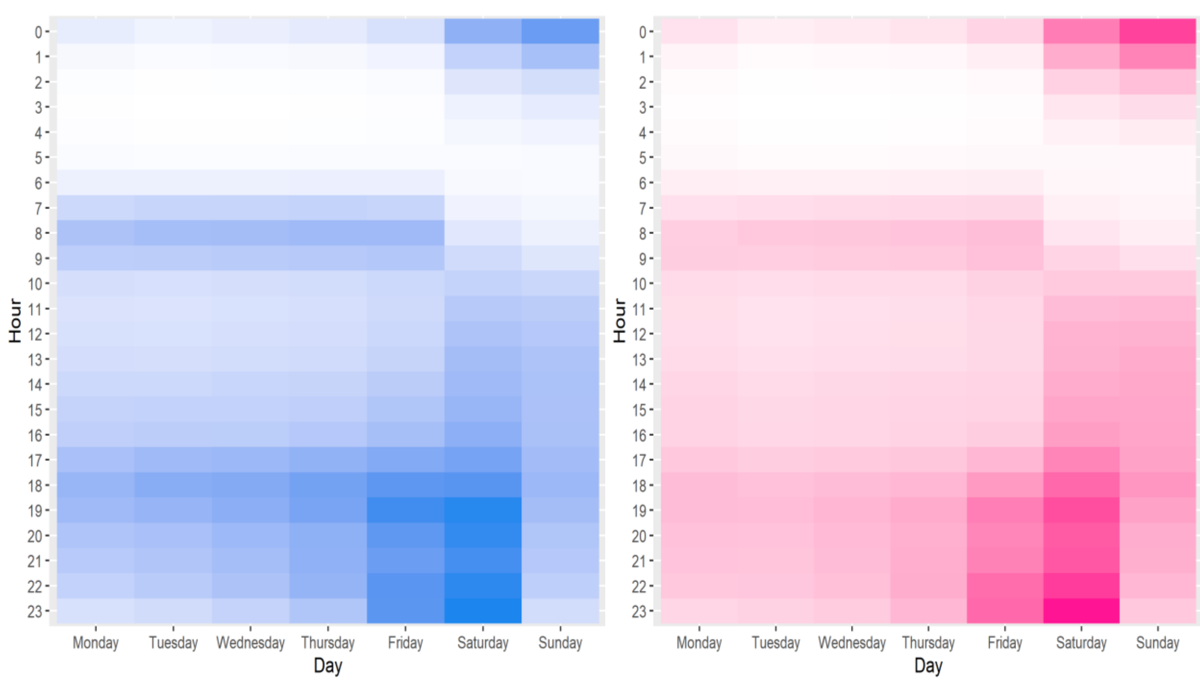
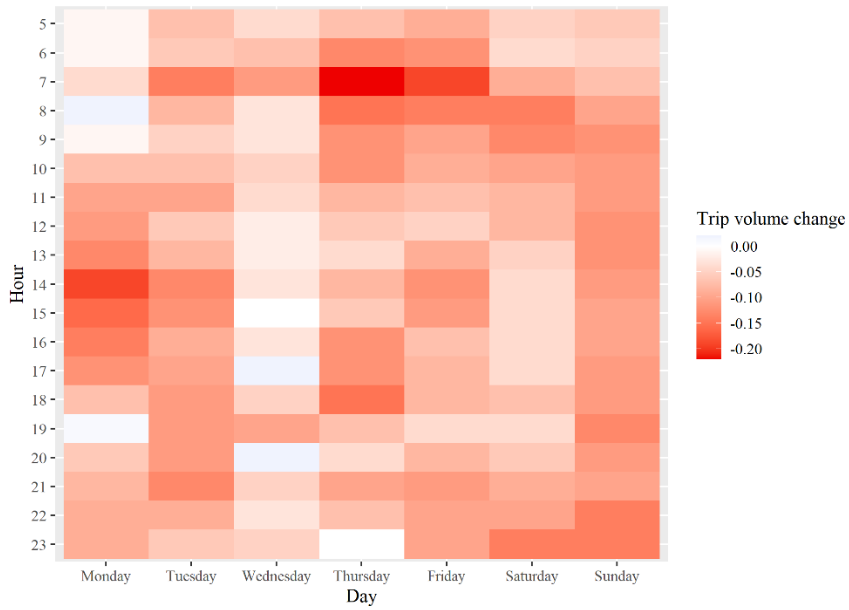


Figure 7. Differences in Uber trip volume before and after Lyft's access restriction (May 30th–July 24th, 2019)



Red color (blue color) is used for day-hour segments that experienced a decrease (increase) in Uber trip volume after Lyft's access restriction (June 27th, 2019).

Figure 8. Percentages of restaurants that joined Uber Eats within each zone in Manhattan

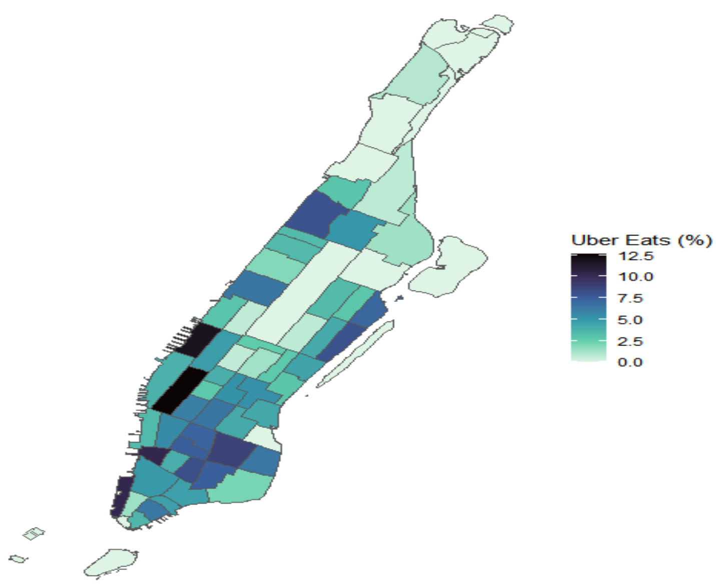


Figure 9. Heat maps of trip volume during 2015 by the hour of the week for Uber (left) and Lyft (right)

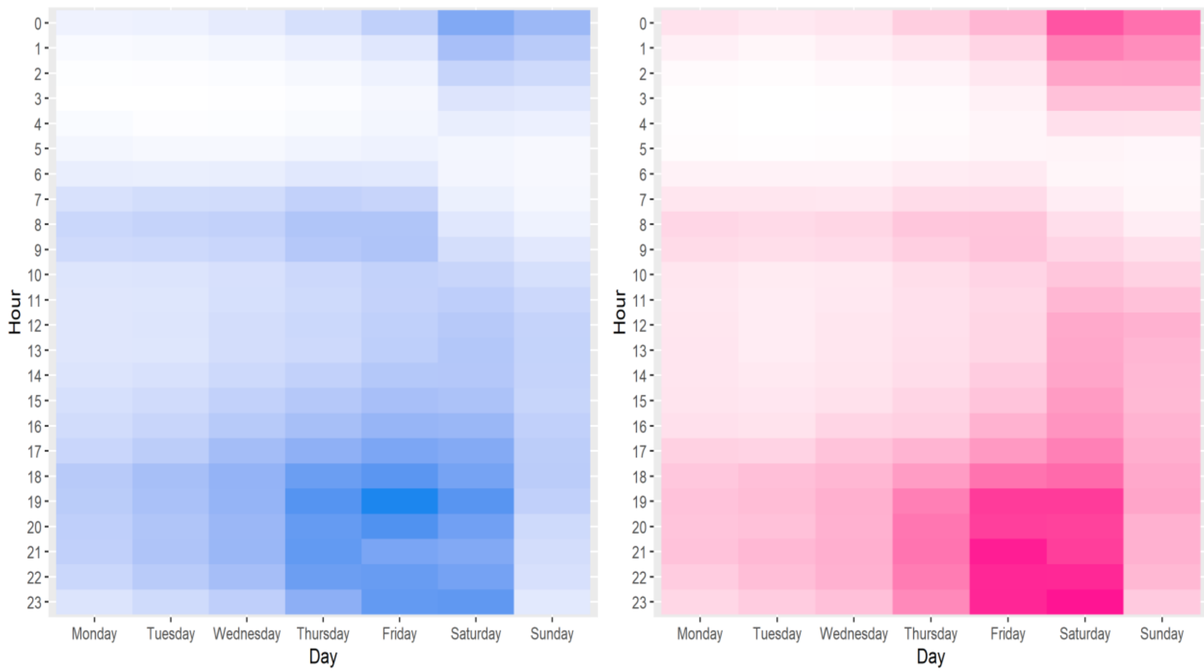
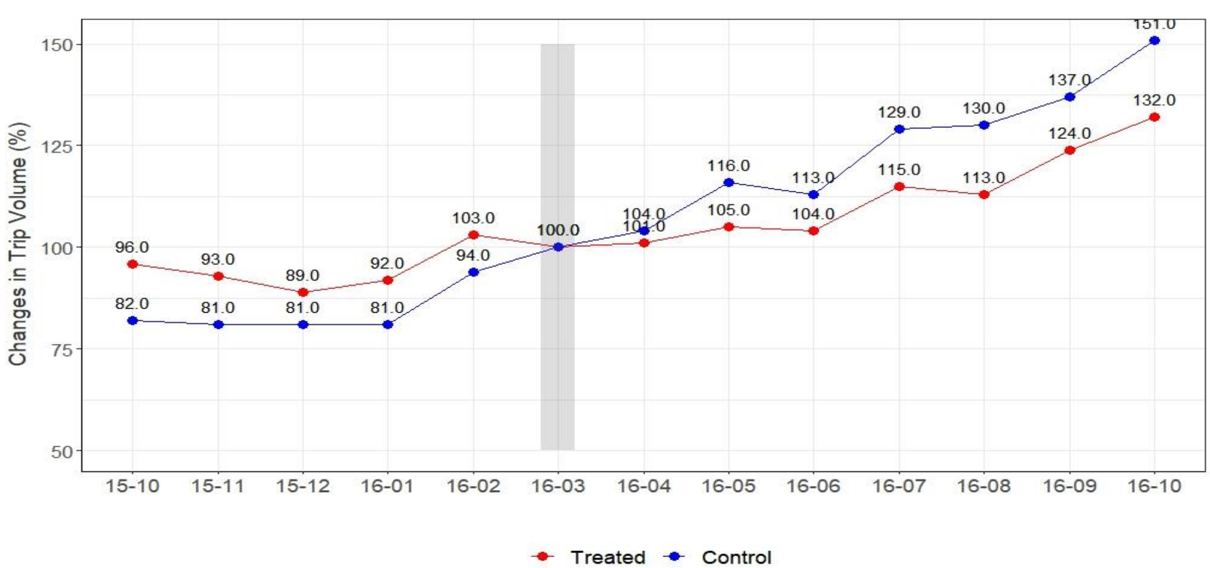


Figure 10. Monthly Uber trip volumes for the treated group (red) and control group (blue)



The grey line indicates the date for Uber Eats' launch in Manhattan (March 22nd, 2016).

Figure 11. Percentages of Uber Eats' market share in November 2020 (the month before Uber's acquisition of Postmates)

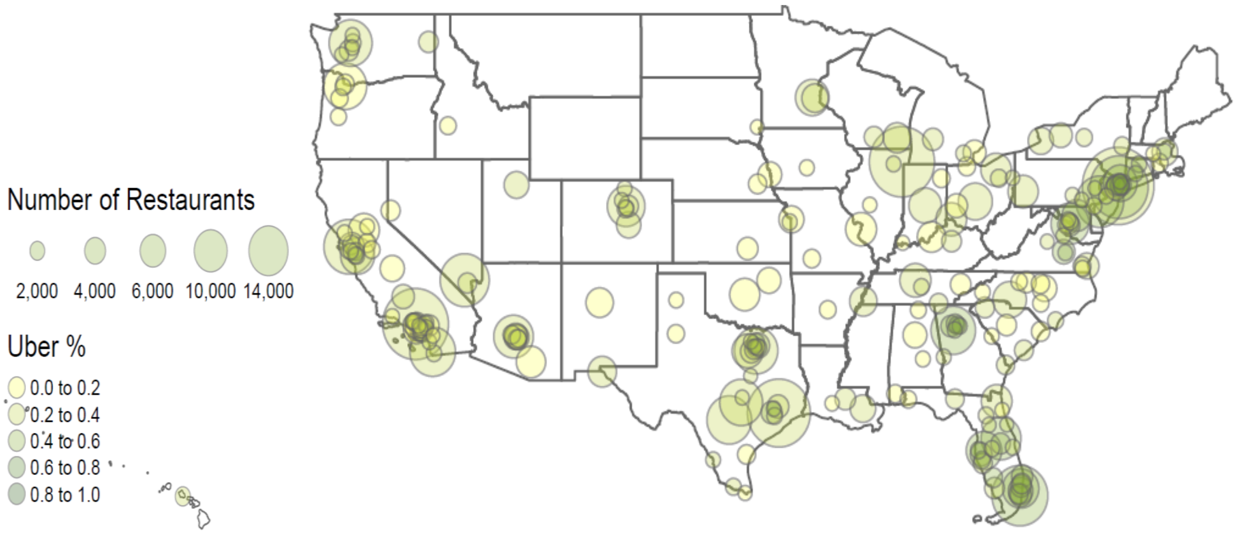
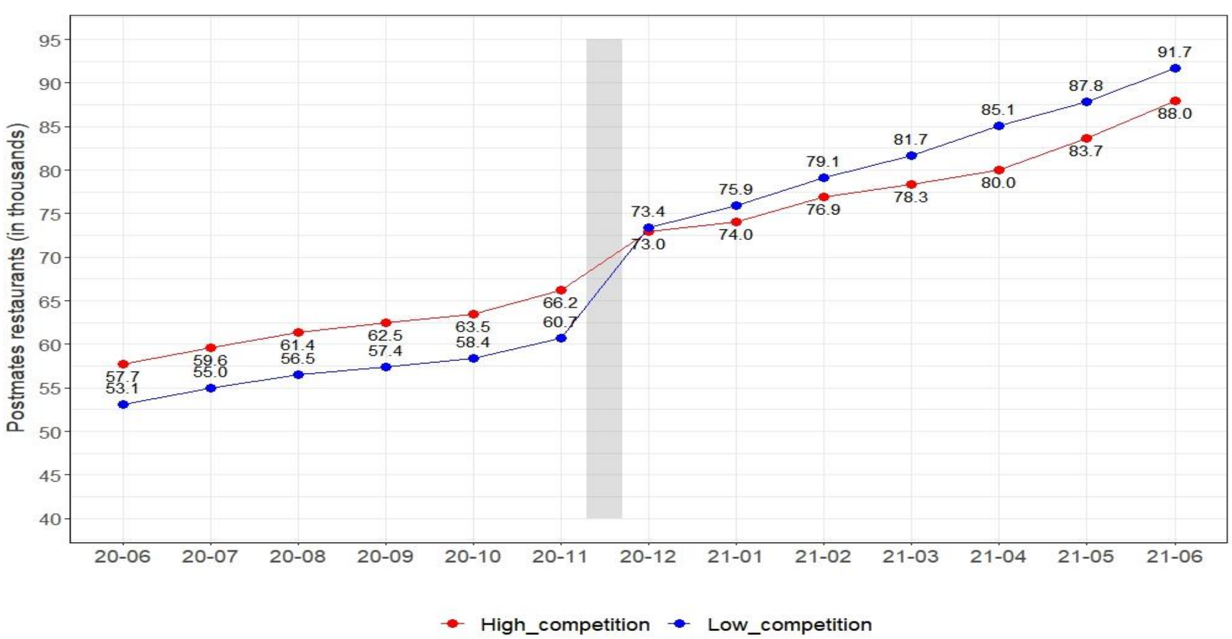


Figure 12. Number of restaurants (in thousands) from Postmates that operated on Uber Eats for the treated group (red) and the control group (blue)



The grey line indicates the month for Uber's acquisition of Postmates (December 2020).

Tables

Table 1. Summary statistics

	Variable	Mean	SD	Min	Max	(1)	(2)	(3)	(4)	(5)	(6)
(1)	Lyft's Access Restriction	0.50	0.50	0	1	1.00					
(2)	Lyft Trip Number	22.10	28.45	0	732	-0.03	1.00				
(3)	Uber Trip Number	69.35	78.33	0	1330	-0.02	0.89	1.00			
(4)	Taxi Trip Number	35.69	100.38	0	1072	-0.02	0.53	0.66	1.00		
(5)	Lyft Trip Number in 2018	16.70	22.02	0	398	-0.01	0.91	0.82	0.51	1.00	
(6)	Uber Trip Number in 2018	65.73	75.79	0	1240	0.02	0.85	0.93	0.64	0.85	1.00

Table 2. The effect of Lyft's access restriction on the trip volumes of Uber and Lyft

	(log) Uber trip #		**(log) Lyft trip #**	
	(1)	**(2)**	**(3)**	**(4)**
Lyft's Access Restriction	-0.024 (0.005) [0.000]	-0.043 (0.005) [0.000]	-0.055 (0.006) [0.000]	-0.048 (0.005) [0.000]
(log) Uber Trip # in 2018	-	0.340 (0.025) [0.000]	-	-
(log) Lyft Trip # in 2018	-	-	-	0.212 (0.012) [0.000]
(log) Taxi Trip #	-	0.068 (0.008) [0.000]	-	0.078 (0.008) [0.000]
Zone fixed effects	Yes	Yes	Yes	Yes
Hour fixed effects	Yes	Yes	Yes	Yes
Day-of-the-week fixed effects	Yes	Yes	Yes	Yes
Observations	252,480	252,480	252,480	252,480
Adjusted R^2	0.912	0.925	0.857	0.867

Robust standard errors clustered at the zone level are included in parentheses. *p*-values are included in square brackets.

Table 3. The effect of Uber's access restriction on the trip volumes of Uber and Lyft

	(log) Uber Trip #		(log) Lyft Trip #	
	(1)	**(2)**	**(3)**	**(4)**
Uber's Access Restriction	-0.039 (0.006) [0.000]	-0.029 (0.004) [0.000]	0.059 (0.007) [0.000]	0.058 (0.006) [0.000]
(log) Uber Trip # in 2018	-	0.392 (0.026) [0.000]	-	-
(log) Lyft Trip # in 2018	-	-	-	0.233 (0.014) [0.000]
(log) Taxi Trip #	-	0.070 (0.009) [0.000]	-	0.077 (0.009) [0.000]
Zone fixed effects	Yes	Yes	Yes	Yes
Hour fixed effects	Yes	Yes	Yes	Yes
Day-of-the-week fixed effects	Yes	Yes	Yes	Yes
Observations	252,480	252,480	252,480	252,480
Adjusted R^2	0.911	0.927	0.853	0.865

Robust standard errors clustered at the zone level are included in parentheses. *p*-values are included in square brackets.

Table 4. DID estimation of the effect of Lyft's access restriction on Uber trip volumes

(log) Trip #	**NYC taxi trips as a counterfactual**		**Chicago rideshare trips as a counterfactual**
	(1) Full sample	**(2)** Outer boroughs subsample	**(3)**
Lyft's Access Restriction × Uber (1: Uber, 0: Taxi)	-0.090 (0.012) [0.000]	-0.113 (0.015) [0.000]	-
Lyft's Access Restriction × NYC (1: NYC, 0: Chicago)	-	-	-0.068 (0.009) [0.000]
(log) Trip # in 2018	0.311 (0.015) [0.000]	0.237 (0.015) [0.000]	-
(log) Taxi Trip #	-	-	0.089 (0.009) [0.000]
Zone fixed effects	Yes	Yes	Yes
Hour fixed effects	Yes	Yes	Yes
Day-of-the-week fixed effects	Yes	Yes	Yes
Week fixed effects	Yes	Yes	Yes
Observations	504,960	372,480	326,400
Adjusted R^2	0.926	0.907	0.910

Robust standard errors clustered at the zone level are included in parentheses. *p*-values are included in square brackets.

Table 5. The effect of Lyft's access restriction in restricted and unrestricted segments

	(log) Uber Trip #		**(log) Lyft Trip #**	
	(1) Restricted segments	**(2)** Unrestricted segments	**(3)** Restricted segments	**(4)** Unrestricted segments
Lyft's Access Restriction	-0.028 (0.005) [0.000]	-0.087 (0.005) [0.000]	-0.027 (0.005) [0.000]	-0.113 (0.008) [0.000]
(log) Uber Trip # in 2018	0.315 (0.025) [0.000]	0.358 (0.036) [0.000]	-	-
(log) Lyft Trip # in 2018	-	-	0.199 (0.012) [0.000]	0.209 (0.020) [0.000]
(log) Taxi Trip #	0.070 (0.009) [0.000]	0.055 (0.005) [0.000]	0.081 (0.010) [0.000]	0.067 (0.006) [0.000]
Zone fixed effects	Yes	Yes	Yes	Yes
Hour fixed effects	Yes	Yes	Yes	Yes
Day-of-the-week fixed effects	Yes	Yes	Yes	Yes
Observations	189,360	63,120	189,360	63,120
Adjusted R^2	0.919	0.941	0.859	0.892

Robust standard errors clustered at the zone level are included in parentheses. *p*-values are included in square brackets.

Table 6. The effect of Lyft's access restriction on the trip duration of Lyft and Uber

(log) Trip Duration (in minutes)	**(1)** Lyft trip duration	**(2)** Uber trip duration
Lyft's Access Restriction	-0.024 (0.001) [0.000]	-0.030 (0.001) [0.000]
Trip Distance	0.157 (0.002) [0.000]	0.159 (0.003) [0.000]
(log) Total Trip #	0.101 (0.004) [0.000]	0.082 (0.004) [0.000]
Zone fixed effects	Yes	Yes
Hour fixed effects	Yes	Yes
Day-of-the-week fixed effects	Yes	Yes
Observations	228,338	240,014
Adjusted R^2	0.710	0.746

Robust standard errors clustered at the zone level are included in parentheses. *p*-values are included in square brackets.

Table 7. Resource typology based on transactional exclusivity and divisibility

	Exclusive use	**Non-exclusive use**
Indivisible	**Reallocate (non-scale free)** E.g., equipment, rideshare drivers' vehicle	**Share (scale-free)** E.g., reputation, knowledge, rideshare platforms' matching algorithms, rideshare drivers' driving skills
Divisible	**Reallocate (non-scale free)** E.g., employees' and rideshare drivers' working time	**Share (non-scale free)** E.g., employees' and rideshare drivers' idle time

Table 8. Quotes from interviews with restaurant owners and rideshare drivers

Interviewee	Quotes	Predictions confirmed from the interviews
Owner of an Asian restaurant in Manhattan	"An Uber salesman came to the restaurant and asked me to join. There were no promotions or rewards. … I had no idea how many Uber drivers were out there. I just wanted to get more money and thought Uber Eats could be a good opportunity."	Restaurants' decisions to join Uber Eats were not determined by the availability of Uber drivers near their restaurants.
Owner of a café in Manhattan	"I just joined UE out of curiosity, in the hope of getting more money. … I didn't consider how many drivers were out there."	
Rideshare drivers (a total of 8 drivers)	"Acceptance rate is important for us, as high rates give us trips with a better fare. … (to maintain the acceptance rate) We need to turn off one app while serving a trip from another app. Though I've been working for years now, I sometimes still forget to do so."	Operating across Uber Eats and Lyft entailed scheduling burdens for drivers (high multi-homing costs).
	"When Uber Eats started, I did get a pop-up in the Uber app asking us to join, but no promotion or rewards was given for it, so I did not join it."	Uber did not provide incentives for joining Uber Eats to drivers when Uber Eats was launched.
	"I once considered being a food delivery driver, but did not. It doesn't give me enough money. I'll say you can earn as much as $2,500 per week from Uber, but about $2,000 per week from Uber Eats … I only work (on food delivery platforms) when the rideshare business is slow." "I only work on food delivery apps when the rideshare business is slow. Uber is always my priority. … In Manhattan, especially during rush hours, food delivery is not good as it is easy to get tickets. I would do delivery maybe after 8, 9 pm." "I focus on Uber and Lyft during rush hours and food delivery during other times. Say, I would do Uber from 5-10 am, do food delivery from 10 am – 2 pm, and go back to Uber from 5-10 pm."	Food delivery business did not provide higher profit opportunities compared to rideshare business. Drivers constantly reallocated their working hours and vehicles between rideshare and food delivery businesses.

Table 9. Summary statistics

	Variable	Mean	SD	Min	Max	(1)	(2)	(3)	(4)	(5)	(6)
(1)	Uber trip #	74.45	82.73	0	2,044	1.00					
(2)	Lyft trip #	8.00	8.86	0	239	0.82	1.00				
(3)	Rush Hour	0.25	0.43	0	1	0.16	0.12	1.00			
(4)	Uber Eats Restaurants (%)	0.04	0.03	0	0.12	0.23	0.24	-0.01	1.00		
(5)	Total restaurant #	169.96	127.15	0	677	0.50	0.43	-0.01	0.27	1.00	
(6)	Median age	38.13	3.94	30.39	52.14	0.05	-0.08	0.00	-0.29	-0.03	1.00

Table 10. The effect of Uber Eats on Uber's and Lyft's trip volumes

	(log) Uber Trip #		(log) Lyft Trip #	
	(1)	(2)	(3)	(4)
Post × UE restaurants (%)	-2.119 (0.633) [0.001]	-2.566 (0.684) [0.000]	-6.429 (1.984) [0.002]	-6.918 (1.940) [0.001]
Post × UE restaurants (%) × Rush hours	-	1.867 (0.490) [0.000]	-	2.080 (0.880) [0.021]
Post × Rush hours	-	0.207 (0.028) [0.000]	-	0.332 (0.053) [0.000]
UE restaurants (%) × Rush hours	-	-0.459 (0.971) [0.638]	-	-0.407 (1.505) [0.788]
(log) Total restaurant #	-0.058 (0.014) [0.000]	-0.061 (0.014) [0.000]	-0.163 (0.089) [0.072]	-0.168 (0.089) [0.064]
Zone FEs	Yes	Yes	Yes	Yes
Week FEs	Yes	Yes	Yes	Yes
Day-of-the-week FEs	Yes	Yes	Yes	Yes
Hour FEs	Yes	Yes	Yes	Yes
Observations	61,702	61,702	61,702	61,702
Adjusted R^2	0.831	0.833	0.636	0.639

Robust standard errors clustered at the zone level are included in parentheses. *p*-values are included in square brackets.

Table 11. The effect of Uber Eats on taxi trip volumes

(log) Taxi Trip #	(1)	(2)
Post × UE restaurants (%)	-0.248 (0.647) [0.703]	-0.576 (0.674) [0.396]
Post × UE restaurants (%) × Rush hours	-	1.406 (0.446) [0.002]
Post × Rush hours	-	0.234 (0.027) [0.000]
UE restaurants (%) × Rush hours	-	-0.968(1.350) [0.476]
(log) Total restaurant #	0.030 (0.019) [0.110]	0.026 (0.019) [0.162]
Zone FEs	Yes	Yes
Week FEs	Yes	Yes
Day-of-the-week FEs	Yes	Yes
Hour FEs	Yes	Yes
Observations Adjusted R^2	61,702 0.906	61,702 0.907

Robust standard errors clustered at the zone level are included in parentheses. *p*-values are included in square brackets.

Table 12. The effect of Uber Eats on Uber and Lyft trip volumes across zones with different median population ages

	(1) (log) Uber Trip #	(2) (log) Lyft Trip #
Post × UE restaurants (%)	-1.481 (1.404) [0.295]	-6.762 (1.671) [0.000]
Post × UE restaurants (%) × Age	-0.986 (1.605) [0.541]	-0.806 (3.310) [0.808]
Post × Age	0.056 (0.066) [0.396]	0.245 (0.198) [0.221]
(log) Total restaurant #	-0.050 (0.015) [0.001]	-0.127 (0.083) [0.128]
Zone FEs	Yes	Yes
Week FEs	Yes	Yes
Day-of-the-week FEs	Yes	Yes
Hour FEs	Yes	Yes
Observations Adjusted R^2	61,702 0.831	61,702 0.637

Robust standard errors clustered at the zone level are included in parentheses. *p*-values are included in square brackets.

Table 13. The effect of DoorDash on Uber and Lyft trip volumes

	(log) Uber Trip #		(log) Lyft Trip #	
	(1)	(2)	(3)	(4)
Post × Brooklyn	-0.005 (0.021) [0.807]	-0.001 (0.021) [0.948]	0.146 (0.119) [0.223]	0.143 (0.122) [0.240]
Post × Brooklyn × Rush hours	-	-0.015 (0.016) [0.362]	-	0.009 (0.027) [0.732]
Brooklyn × Rush hours	-	-0.021 (0.029) [0.475]	-	-0.003 (0.005) [0.577]
Brooklyn × Rush hours	-	-0.035 (0.010) [0.001]	-	-0.009 (0.016) [0.577]
Zone FEs	Yes	Yes	Yes	Yes
Week FEs	Yes	Yes	Yes	Yes
Day-of-the-week FEs	Yes	Yes	Yes	Yes
Hour FEs	Yes	Yes	Yes	Yes
Observations	189,419	189,419	189,419	189,419
Adjusted R^2	0.760	0.760	0.359	0.359

Robust standard errors clustered at the zone level are included in parentheses. *p*-values are included in square brackets.

Table 14. The effect of Uber Eats on Uber and Lyft trip volumes (DID estimations that compare Uber trip volumes between May and December 2019)

: *UE restaurants (%)* is the proportion of restaurants that newly joined Uber Eats in a given zone after May 2019. *UE restaurants (1/0)* is a binary variable that equals one if a given zone had at least one restaurant that newly joined Uber Eats after May 2019, and zero otherwise.

(log) Uber Trip #	(1)	(2)
Post × UE restaurants (%)	-14.158 (2.089) [0.000]	-
Post × UE restaurants (1/0)	-	-0.147 (0.012) [0.000]
Zone FEs	Yes	Yes
Week FEs	Yes	Yes
Day-of-the-week FEs	Yes	Yes
Hour FEs	Yes	Yes
Observations	241,931	241,931
Adjusted R^2	0.889	0.889

Robust standard errors clustered at the zone level are included in parentheses. *p*-values are included in square brackets.

Table 15. Summary statistics

	Variable	Mean	SD	Min	Max	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(1)	Operate on Uber Eats	0.43	0.49	0.00	1.00	1.00							
(2)	Operate on competing platforms	0.62	0.48	0.00	1.00	0.32	1.00						
(3)	Treat	0.30	0.12	0.11	0.86	0.06	0.00	1.00					
(4)	Post	0.54	0.50	0.00	1.00	0.14	-0.01	0.00	1.00				
(5)	Multi-homing between Postmates and Uber Eats	0.40	0.49	0.00	1.00	0.76	0.28	0.09	0.00	1.00			
(6)	Multi-homing between Postmates and competing platforms	0.65	0.48	0.00	1.00	0.28	0.69	-0.02	0.00	0.32	1.00		
(7)	Independent restaurants	0.73	0.44	0.00	1.00	-0.19	-0.04	0.07	0.00	-0.25	-0.03	1.00	
(8)	Covid cases (%)	0.00	0.01	0.00	0.09	0.06	-0.01	0.02	0.41	0.00	0.01	0.00	1.00

Table 16. The number of Postmates restaurants that operated on Uber Eats

	(1)	(2)	(3)	(4)
Post × Treat	-0.257 (0.024) [0.000]	-0.338 (0.035) [0.000]	-0.183 (0.023) [0.000]	-0.223 (0.028) [0.000]
Post × Treat × MH on Uber Eats	-	0.312 (0.038) [0.000]	-	0.339 (0.041) [0.000]
Post × Treat × MH on DoorDash or Grubhub	-	-	-0.111 (0.021) [0.000]	-0.168 (0.026) [0.000]
Controls	Yes	Yes	Yes	Yes
Year-month FEs	Yes	Yes	Yes	Yes
Restaurant FEs	Yes	Yes	Yes	Yes
Observations	2,797,353	2,797,353	2,797,353	2,797,353
Adjusted R^2	0.813	0.813	0.817	0.820

Robust standard errors clustered at the city level are included in parentheses. *p*-values are included in square brackets.

Table 17. The number of Postmates restaurants that operated on competing platforms

	(1)	(2)	(3)	(4)
Post × Treat	0.217 (0.034) [0.000]	0.200 (0.054) [0.000]	0.309 (0.052) [0.000]	0.243 (0.059) [0.000]
Post × Treat × MH on Uber Eats	-	-0.250 (0.044) [0.000]	-	-0.261 (0.048) [0.000]
Post × Treat × MH on DoorDash or Grubhub	-	-	0.019 (0.053) [0.719]	0.090 (0.057) [0.118]
Controls	Yes	Yes	Yes	Yes
Year-month FEs	Yes	Yes	Yes	Yes
Restaurant FEs	Yes	Yes	Yes	Yes
Observations	2,797,353	2,797,353	2,797,353	2,797,353
Adjusted R^2	0.776	0.777	0.777	0.778

Robust standard errors clustered at the city level are included in parentheses. *p*-values are included in square brackets.

Table 18. The number of Postmates restaurants that operated on DoorDash or Grubhub

	DoorDash		**Grubhub**	
	(1)	(2)	(3)	(4)
Post × Treat	0.052 (0.014) [0.000]	-0.003 (0.013) [0.837]	0.262 (0.045) [0.000]	0.240 (0.061) [0.000]
Post × Treat × MH on DoorDash	-	0.069 (0.024) [0.004]	-	-0.204 (0.036) [0.000]
Post × Treat × MH on Grubhub	-	0.050 (0.025) [0.043]	-	0.172 (0.061) [0.005]
Controls	Yes	Yes	Yes	Yes
Year-month FEs	Yes	Yes	Yes	Yes
Restaurant FEs	Yes	Yes	Yes	Yes
Observations	2,797,353	2,797,353	2,797,353	2,797,353
Adjusted R^2	0.837	0.837	0.776	0.780

Robust standard errors clustered at the city level are included in parentheses. *p*-values are included in square brackets.

Table 19. Placebo tests using fake Post periods

	Placebo month: August 2020		Placebo month: October 2020	
	(1) Uber Eats	(2) Competing platforms	(3) Uber Eats	(4) Competing platforms
Post × Treat	0.006 (0.007) [0.402]	0.032 (0.019) [0.099]	0.006 (0.005) [0.259]	0.038 (0.022) [0.079]
Controls	Yes	Yes	Yes	Yes
Year-month FEs	Yes	Yes	Yes	Yes
Restaurant FEs	Yes	Yes	Yes	Yes
Observations	1,291,086	1,291,086	1,291,086	1,291,086
Adjusted R^2	0.942	0.849	0.942	0.849

Robust standard errors clustered at the city level are included in parentheses. *p*-values are included in square brackets.

Table 20. The effect of the acquisition on Uber Eats restaurants

	Uber Eats		**Competing platforms**	
	(1)	(2)	(3)	(4)
Post × Treat	0.196 (0.043) [0.000]	0.229 (0.092) [0.013]	-0.173 (0.035) [0.000]	-0.254 (0.054) [0.000]
Post × Treat × MH on Postmates	-	0.095 (0.053) [0.074]	-	0.055 (0.042) [0.199]
Post × Treat × MH on DoorDash or Grubhub	-	-0.315 (0.077) [0.000]	-	0.085 (0.049) [0.085]
Controls	Yes	Yes	Yes	Yes
Year-month FEs	Yes	Yes	Yes	Yes
Restaurant FEs	Yes	Yes	Yes	Yes
Observations	1,967,511	1,967,511	1,967,511	1,967,511
Adjusted R^2	0.393	0.406	0.848	0.848

Robust standard errors clustered at the city level are included in parentheses. *p*-values are included in square brackets.

Table 21. The moderating effect of restaurant age on the effect of acquisition on Postmates restaurants

: *Restaurant Age* indicates restaurants' operation period on Postmates by November 2020 (months)

	Uber Eats		**Competing platforms**	
	(1)	(2)	(3)	(4)
Post × Treat	-0.276 (0.022) [0.000]	-0.126 (0.033) [0.000]	0.196 (0.029) [0.000]	0.306 (0.056) [0.000]
Post × Treat × Restaurant Age	-	-0.019 (0.004) [0.000]	-	-0.015 (0.005) [0.003]
Controls	Yes	Yes	Yes	Yes
Year-month FEs	Yes	Yes	Yes	Yes
Restaurant FEs	Yes	Yes	Yes	Yes
Observations	2,797,353	2,797,353	2,797,353	2,797,353
Adjusted R^2	0.816	0.816	0.780	0.784

Robust standard errors clustered at the city level are included in parentheses. *p*-values are included in square brackets.

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