# Platform governance in the presence of within-complementor interdependencies: Evidence from the rideshare industry

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**Abstract**

Existing studies suggest that platform access restrictions may cause restricted complementors to switch to competing platforms, which will increase complement quantity on competing platforms. We re-examine this prediction by accounting for the impact of cross-platform synergy on complementor responses to platform access restriction. We argue that restricting a complementor's access on a platform may prevent it from achieving synergy from multi-homing, thereby incentivizing it to abandon both the restricted and (unrestricted) competing platforms. Using rideshare data in New York City, we compare the numbers of trips made by Lyft and Uber drivers, respectively, before and after Lyft restricted drivers' access on its platform. We find that Lyft's access restriction reduced trip numbers not only on the Lyft platform but also on the Uber platform. In addition, both Lyft's and Uber's trip numbers 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 highlight the importance of accounting for interdependencies across complementor activities when designing platform access restriction policies. [<=250 words]

**Keywords:** platform governance, interdependencies, multi-homing, complementor, rideshare

**[Preliminary draft: Please do not circulate without authors' permission]**

## 1. Introduction

Over the last decade, platforms have become a ubiquitous mode for organizing technological and economic exchanges in many industries, attracting attention from academic scholars, policymakers, and business practitioners alike (Adner et al. 2019, Gambardella and Von Hippel 2019, Kapoor and Agarwal 2017, Miric and Jeppesen 2020, Rietveld and Schilling 2021, Seamans and Zhu 2014). A key characteristic of platforms is their reliance on complementors to create value (Baldwin 2020, Gawer and Cusumano 2002).[1] Unlike traditional hierarchical organizations, platforms lack the authority and direct control over complementors (Hagiu and Wright 2015, Jacobides et al. 2018), which creates unique challenges for platforms to govern complementor activities. One important instrument of platform governance is access control (Boudreau 2010, Parker and Van Alstyne 2018). By relaxing or tightening access restrictions, platforms can moderate the quantity and quality of complementor activities within and across platforms (Eisenmann et al. 2009, Halaburda et al. 2018).

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 (Boudreau 2012, Cennamo and Santaló 2019). On the other hand, it reduces the *quantity* of complements on the restricted platform due to the exit of restricted complementors (Cusumano et al. 1992, 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 (Hagiu 2014). Nintendo used such policies during the late 1980s to restrict the number of developers that could publish game titles on its platform (Brandenburger 1995, Casadesus-Masanell and Hałaburda 2014). While 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 restriction (Schilling 2003).

[1]We define complementors as providers of complementary products or services built on a platform, such as video game developers or rideshare drivers (Gawer and Cusumano 2014).

However, we argue in this paper that the impact of access restriction on complement quantity may not be so straightforward once we account for cross-platform synergy. As complementors make large investments in complementor-specific resources to differentiate their products and services, 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 console platforms began to provide software development toolkits (SDKs) that lowered the development costs for individual developers (Hagiu 2006, Ozalp et al. 2018). This attracted a large pool of new developers.[2] The intensified competition forced developers to differentiate with complementor-specific investments, such as investments in content, art design, and music composition (Pachter et al. 2014, Reimer 2005). Recouping these large investments necessitated larger markets – "larger perhaps than any one platform can provide" (Corts and Lederman 2009, p. 125).[3] Consequently, game developers were incentivized to spread their development costs by operating on multiple platforms, or multi-homing (Cennamo et al. 2018, Hagiu 2009).

Multi-homing is common not only in technological platforms such as video game consoles and smartphone operating systems but also in transaction platforms such as e-commerce and rideshare services.[4] A complementor needs to obtain resources and develop capabilities (e.g., video game or software app development skills, vehicles and driving skills in the rideshare market) to operate on a platform (Baldwin and Woodard 2009, Kapoor and Agarwal 2017). Some of these resources and capabilities are platform-independent but complementor-specific (e.g., sophisticated programming skills or luxury cars). When a complementor cannot recover the costs of acquiring complementor-specific resources or capabilities from

[2]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 and Venkatraman 2018, Tran 2001).
[3]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 2001, Graft 2010, Veresockaya 2020).
[4]46 percent of video games are published on multiple game console platforms (Cennamo and Santaló 2019). 64 percent of the smartphone app developers publish their apps on both Apple's iOS and Google's Android platforms (Bresnahan et al. 2015). 70 percent of rideshare drivers drive for at least two rideshare platforms (Campbell 2019).

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 and Lederman 2009), thereby creating within-complementor interdependencies (or synergy) 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 risks shrinking potential markets for complementors and depriving them of the potential synergy. Losing such synergy may force them to withdraw from both the restricted and unrestricted platforms, thereby creating a potential cross-platform spillover effect.

We empirically test our 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. Our 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.[5] About 45 percent of rideshare drivers in NYC work for at least two rideshare platforms (Parrott and 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. In addition, we can separate the effect of Lyft's access restriction on Lyft drivers (within-platform effect) and Uber drivers (cross-platform effect), respectively, before Uber also restricted access to its platform (when most of the cross-platform complementary effect had been eliminated with Lyft's earlier access restriction but the within-platform effect should still remain).

Our results confirm the cross-platform spillover effect of restricting platform access. Lyft's access restriction reduced the number of trips not only on the Lyft platform but also on the Uber platform. These results are robust with additional analyses that account for unobservable time-variant factors using three

[5]Information on locations is given as one of 263 taxi zones in NYC, which are specified by NYC Taxi and Limousine Commission.

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 numbers 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 access restriction policies.

## 2. Related literature

Orchestrating a large pool of complementors is crucial for the success of a platform (Gawer and Cusumano 2002). Unlike hierarchical organizations, platforms lack direct control over complementors and cannot use levers such as salary, employment contracts, or hierarchical authority to dictate complementor activities (Hagiu and Wright 2015, Jacobides et al. 2018). Instead, platforms rely on a different set of levers such as standards, certifications, reputation systems, and information control (Claussen et al. 2013, Hagiu and Wright 2019a, Li and Zhu 2021, Rietveld et al. 2019, 2021). One prominent governance tool is platform access restriction, which regulates the level of access to the platform (Boudreau 2010, Parker and 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 and Cusumano 2014, Hagiu 2014, Shapiro and 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 retain committed complementors (Casadesus-Masanell and Hałaburda 2014, Chu and Wu 2021) and reduces competition among remaining complementors, which encourages them to make platform-specific investments and improve complement *quality* (Boudreau 2012, Zhang et al. 2022). On the other hand, access restriction may reduce the *quantity* of complements 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 (Eisenmann et al. 2009, 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 led major handset manufacturers to join the Android platform (West and Wood 2013).

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 and Lederman 2009), which may result in within-complementor interdependencies that influence complementors' participation on multiple platforms. Consequently, we examine how platform access restriction influences the behavior of complementors across platforms.

## 3. Theoretical development

Complementors provide their products or services (or complements) 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 (Baldwin and Clark 2000, Gawer and Cusumano 2014). Examples of platform-specific resources include distribution channels, development tool kits, and algorithms such as those matching drivers and users of rideshare services. Complementor-specific resources refer to a complementor's resources that can be combined with platform-specific resources to produce complements (Boudreau 2010, 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), and rideshare drivers' driving skills and vehicles.

A complementor can leverage these two types of resources as 'stepping stones' to create synergy in its activities. For example, a complementor can create synergy by sharing complementor-specific resources between activities across different platforms (Corts and Lederman 2009).[6] Complementor-specific resources enable a complementor to differentiate its complements from those offered by others (Boudreau 2012), but acquiring these resources can be costly for a complementor (Burtch et al. 2018, Tae et al. 2020).[7] Resources are often accumulated over time (Dierickx and Cool 1989) and require significant investments (Gawer and Henderson 2007, Zhu and Liu 2018).[8] For instance, in the video game industry, a large portion of development costs is related to content, art design, music composition, and licensing fees (Pachter et al. 2014, Reimer 2005). In the rideshare industry, 80 percent of the 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 and 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 (Armstrong 2006, Hagiu 2009). Platforms often have different characteristics that attract distinct groups of users (Cennamo and Santalo 2013, Rietveld and Eggers 2018). For example, in the video game industry, Sony PlayStation focuses on sophisticated graphics and attracts users that favor action and sports games. In contrast, Nintendo Wii mostly targets casual gamers based on its simple interfaces (Megerian 2007). In the rideshare industry, users adopt different platforms based on their personal

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[6]A complementor can also create synergy by sharing platform-specific resources between activities across different segments within a platform. We discuss this in detail in Section 5.3.

[7]Because complementors in platform-based industries lack resources compared to firms in other industries, their exit rates tend to be higher. For example, the smartphone app market is plagued by high mortality rates of apps, ranging from 41 to 69 percent depending on the operating system (Koetsier 2013). In the NYC rideshare market, about 25 percent of new drivers leave the industry within their first year (Parrott and Reich 2018).

[8]Protecting complementor-specific resources also incur considerable costs (Miric and Jeppesen 2020). In the smartphone app market, app developers often adopt both formal (e.g., copyrights and trademarks) and informal (e.g., design complexity, rapid innovation) strategies to protect their resources (Miric et al. 2019).

preferences, third-party promotion programs (e.g., credit cards, firms partnered with platforms, etc.), and references (e.g., from family and friends, from another app, etc.). 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 (Armstrong 2006, Corts and 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. Therefore, we expect that *in the presence of within-complementor interdependencies, restricting complementor access on one platform reduces complementor activities on competing platforms.*

**4. Empirical design**

The empirical context of our study is the rideshare market in New York City, the second-largest in the U.S. (Akhtar and Kiersz 2019). Due to high levels of traffic congestion, NYC has the lowest rate of private car ownership in the U.S. (City of New York 2016). At the same time, the restrictive medallion system constrained the capacity of traditional taxis and created an opportunity for rideshare services. 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 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). 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).[9] Because Lyft and Uber restricted access to their platforms during different time periods, we are able to exploit heterogeneity in platform access restriction across different platforms. For example, because Lyft started restricting access to its platform three months before Uber, we can separate the effect of Lyft's policy on Lyft (within-platform effect) and its effect on Uber (cross-platform effect) before Uber also restricted access to its platform, which should only have a within-platform effect since most of the cross-platform effect would have been eliminated with Lyft's access restriction.

### 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

[9]Exceptions were made for qualified drivers. Lyft drivers who maintained an acceptance rate above 90 percent and completed 100 trips in 30 days were exempted from the restriction (Lyft 2019), whereas Uber drivers needed to have completed at least 425 trips in the prior month and had at least a 4.8-star rating to be exempted from the restriction (Uber 2019).

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.[10]

We estimate the change in trip numbers by Lyft and Uber drivers after Lyft restricted access to its app on June 27th, 2019. We choose our sample period to be from four weeks before June 27th, 2019 to four weeks after (May 30th–July 24th, 2019). Because demand and supply for transportation could differ between weekdays and weekends, we follow prior studies in the transportation sector (e.g., Forbes and Lederman 2010, Prince and Simon 2009) to exclude weekends. We aggregate the trip-level data to the hour-day-zone level. Our final sample contains 242,556 observations across 960 day-hours (40 days×24 hours per day) and 263 zones.

**4.2. Variables**

Our main dependent variable is the number of trips, $Y_{ijht}$, that was reported on platform $i$ (Lyft or Uber), in zone $j$, during the $h^{th}$ hour of day $t$. Because the trip numbers exhibit a large variation across different zones and during different periods, we log-transform the dependent variable to reduce value dispersion.

Our 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.

Our control variables ($X_{ijht}$) include platform $i$'s (Uber or Lyft) (log) trip number in zone $j$ during the $h^{th}$ hour of day $t$ from the previous year (2018),[11] as well as taxi's (log) trip number in zone $j$ during the $h^{th}$ hour of day $t$ from the current year (2019).

Table 1 provides summary statistics at the hour-day-zone level. On average, Uber provided the highest number of trips (72) per hour and zone, followed by taxi (37) and Lyft (23). Supplementary statistics

[10] Detailed maps for the 263 taxi zones specified by TLC are shown in Appendix Figure A1.
[11] 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 number on June 28th, 2019 (Friday), we control for its trip number on June 29th, 2018 (Friday).

show that during rush (non-rush) hours, Uber provided 97 (64) trips per hour and zone, taxi provided 49 (33) trips, and Lyft provided 29 (21) trips.

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Insert Table 1 about here
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### 4.3. Specifications

We estimate the effect of Lyft's access restriction on driver activities using the following specification:

$$\log(Y_{ijht}) = \beta_0 + \beta_1 A_t + X_{ijht}B + \alpha_j + \delta_h + \gamma_t + \varepsilon_{iht}, \quad (1)$$

where $Y_{ijht}$, $A_t$, and $X_{ijht}$ are as explained earlier, and $\alpha_j$, $\delta_h$, and $\gamma_t$ 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 numbers within the same zone. Our theory predicts $\beta_1 < 0$.

## 5. Results

### 5.1. Cross-platform spillover effect

Table 2 estimates the change in trip numbers on Lyft and Uber after Lyft's access restriction. We first investigate the average effect on Lyft trip numbers in Columns 1 and 2. The coefficient in Column 2 shows that, overall, trips provided by Lyft drivers decreased by 5.06 percent ($p < .001$).[12] Based on Lyft's trip number during the month right before the access restriction (4,769,324), we can infer that Lyft's access restriction reduced Lyft's trip numbers by about 241,328. We estimate the average fare per trip to be \$21.94.[13] As Lyft typically takes a 20 percent cut from the fare (Lamberti 2020), these numbers imply that

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[12]To estimate the percentage changes in trip numbers, we exponentiate each coefficient, subtract one, and multiply 100.

[13]From our database, we first calculate the average trip distance (3.08 miles) and duration (19 minutes) of Lyft trips during June 2019. In NYC, Uber and Lyft charge a base fare of \$2.55, and additionally \$1.62 per mile and \$0.74 per minute (INSHUR 2021). We estimate the average trip fare to be about \$21.94 (=2.55+1.62×3.08+0.74×19). For simplicity, we do not account for the minimum fare per trip (\$8), which cause our estimation to be slightly lower than the estimated average fare amount (\$22–25.91) by news media (INSHUR 2021, Lekach 2019). Therefore, the actual economic loss could be larger.

Lyft's access restriction resulted in a loss of about \$1.1 million (=\$21.94×247,527×0.2) in revenue per month.

Next, we examine the average effect on Uber trip numbers in Columns 3 and 4. The coefficient in Column 4 shows that, overall, the trips provided by Uber drivers decreased by 4.68 percent ($p < .001$) – similar to the percentage drop in Lyft trips – after Lyft's access restriction, suggesting a strong complementary effect across platforms. That is, Lyft's access restriction reduced Uber's trip numbers by about 652,825 trips and caused a loss in revenue by about \$2.7 million per month.[14] These results support our prediction.

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Insert Table 2 about here
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**5.2. Unobservable time-variant factors and counterfactuals**

Our main specification controls for the trip number on the platform of interest in the prior year (to account for platform-specific seasonal fluctuations) and the number 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 numbers across all private transportation services). However, there still might be unobservable time-variant factors that our control variables do not fully capture. Our 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 our 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 our setting. Instead, we use three counterfactuals that are close (but not identical) to the ideal counterfactual together with discussions of the unobservable time-variant factors that

[14]The average trip distance and duration was 2.89 miles and 18 minutes for Uber trips during June 2019.

they each intend to address.

### 5.2.1. Counterfactual 1: A platform (e.g., Lyft) that does not share multi-homing drivers with the platform that restricts its access (e.g., Uber) in NYC

There could be unobservable time-variant factors *specific to rideshare platforms in NYC* that might cause our results. For example, rideshare drivers in NYC might expect that 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 17$^{th}$, 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 no increase 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 synergy would have left the industry after Lyft's access restriction, and the remaining Uber drivers were likely to be single-homing. 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 the Uber platform, as explained in footnote 9) 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 20$^{th}$–October 14$^{th}$, 2019). Results are presented in Table 3. Columns 1 and 2 indicate that Uber's trip numbers reduced by 2.63 percent ($p < .001$) after its access restriction. In contrast, Columns 3 and 4 show that Lyft trip numbers increased by 6.36 percent ($p < .001$) after Uber's access restriction. Therefore, we can infer that our 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."

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Insert Table 3 about here
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#### 5.2.2. Counterfactual 2: Taxis in NYC, which do 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, our sample period (Bloomberg and Yassky 2014). While we have controlled for seasonal fluctuations in our 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:

$$\log (Y_{ijht}) = \beta_0 + \beta_1 A_t U_i + \beta_2 X_{ijht} + \alpha_j + \gamma_t + \delta_h + \tau_t + \varepsilon_{iht}, \tag{2}$$

where $A_t$ is as defined earlier. Our dependent variable, $Y_{ijht}$, is the (log) number of trips on private transportation service *i* (Uber or taxi) in zone *j* during the $h^{th}$ hour of day *t*. $U_i$ is a binary variable that equals 1 for trips provided by Uber, and 0 otherwise (trips provided by taxis). Our control variable ($X_{ijht}$) includes private transportation service *i*'s (Uber or taxi) (log) trip number in zone *j* during the $h^{th}$ hour of day *t* from the previous year (2018).[15] All regressions include zone ($\alpha_j$), day-of-the-week ($\gamma_t$), hour ($\delta_h$), and week ($\tau_t$) fixed effects. Our theory predicts $\beta_1 < 0$.

Results are presented in Table 4. Column 1 shows that, compared to taxi trips, Uber trips decreased by 9.08 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 in the Stable Unit

[15]Similar to our main specification, 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.

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 increased after rideshare trips decreased. Upon a closer examination of the raw trip numbers of taxicabs and Uber, we find that the taxi trip numbers decreased, rather than increased, after Lyft's access restriction (June 2019), potentially due to seasonal fluctuations in demand (see Appendix Figure A2). To mitigate the bias from the potential violation of SUTVA, we run a subsample analysis using taxi trips from outer boroughs. Several studies show that Uber and taxis are not fully substitutable in NYC (Barclays 2020, Fischer-Baum and Carl 2015), especially in boroughs outside Manhattan (i.e., The Bronx, Brooklyn, Queens, and Staten Island), which have been traditionally under-served by taxis. Column 2 in Table 4 shows that, in outer boroughs, Uber trips decreased by 11.49 percent ($p < .001$) compared to taxi trips after Lyft's access restriction, similar to the result from the full sample.

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Insert Table 4 about here
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#### 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, market share) with NYC. As the sixth-largest rideshare market in the U.S. (Akhtar and 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). A detailed comparison between the rideshare businesses in Chicago and NYC is shown in the Appendix (Tables A1 and A2).[16]

[16]Another advantage of using Chicago rideshare trips as a counterfactual is that SUTVA is not violated. Because

Starting from November 2018, the City of Chicago provides 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.[17] One weakness of the Chicago dataset is that, unlike the NYC dataset, it does not provide rideshare trip numbers by each platform, which prevents us from separating Uber trips from Lyft and Via trips. Therefore, we are only able to perform a limited number of counterfactual analyses.

We first collect some comparative statistics using datasets in NYC and Chicago. The rideshare trip volume decreased significantly in NYC compared to Chicago since June 2019, when Lyft's access restriction started (see Appendix Figure A3). Therefore, it is unlikely that the reduction in NYC rideshare trip volume was caused by trends at the national level.

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. While 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 only across all platforms). Under the assumption that one driver operates one vehicle, we can roughly infer the number of multi-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 1 shows that, after Lyft's access restriction, the number of multi-homing drivers decreased significantly in NYC while it remained constant in Chicago.[18] 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 our results for the cross-platform spillover effect in NYC.

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network effects are often localized within each geographic market (Lee et al. 2006, Zhu et al. 2021), rideshare trips in Chicago are not affected by Uber trips in NYC.

[17]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 March 21st, 2022).

[18]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 we use the number of total drivers, total vehicles, or active vehicles.

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Insert Figure 1 about here
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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 (due to data limitations) 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).[19] Consistent with these reports, we discover that, 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 and 11.04 percent of all trips in NYC, respectively, during our sample period. We use the following specification:

$$\log(Y_{ijht}) = \beta_0 + \beta_1 A_t C_i + \beta_2 X_{ijht} + \alpha_j + \gamma_t + \delta_h + \tau_t + \varepsilon_{iht}, \qquad (3)$$

where $A_t$ is as defined earlier. Our dependent variable, $Y_{ijht}$, is the (log) trip number in City *i* (Uber solo trips in NYC or rideshare solo trips in Chicago) in zone *j* during the $h^{th}$ hour of day *t*. $C_i$ is a binary variable that equals 1 for geographic zones located within NYC, and 0 otherwise (for geographic zones located in Chicago). Our control variable $(X_{ijht})$ includes (log) taxi trip number in zone *j* during the $h^{th}$ hour of day *t*. All regressions include zone $(\alpha_j)$, day-of-the-week $(\gamma_t)$, hour $(\delta_h)$, and week $(\tau_t)$ fixed effects. Our theory predicts $\beta_1 < 0$.

[19]Uber drivers cannot block shared trip requests (UberPool) though they can decline those requests. However, doing so would hurt drivers' acceptance rate (Helling 2021). 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.

Column 3 in Table 4 indicates that, compared to rideshare solo trips in Chicago, Uber solo trips in NYC decreased by 7.09 percent ($p < .001$) after Lyft's access restriction. We also run the same model using the full sample (both solo and shared trips), and the results are qualitatively similar.

**5.3. Restricted vs. unrestricted segments and within-platform spillover effect**

We have so far established the complementary cross-platform spillover effect of Lyft's access restriction on Uber trips. 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) (Kretschmer and Claussen 2016, West 2003).

We first identify which segments are the 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, Flamm 2019).

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 numbers of Uber and Lyft, respectively, in NYC during 2018 (Figure 2). 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 (Bialik et al. 2015, NYC TLC and Department of Transportation 2019).

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Insert Figure 2 about here
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It is possible that the time periods with low trip numbers on our heat maps capture a relative shortage of driver supply rather than low demand (which would subject the periods to access restriction). To further validate our distinction of restricted vs. unrestricted segments, we compared our 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 numbers on our 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 numbers 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 numbers (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 are the 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 3.26 percent ($p < .001$) and 8.52 percent ($p < .001$) during non-rush hours and rush hours, respectively.

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Insert Table 5 about here
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Next, we investigate the changes in Lyft trip numbers across different time periods after Lyft's access restriction. Columns 3 and 4 in Table 5 show that Lyft trip numbers decreased during both non-rush hours (by about 3.02%, $p < .001$) and rush hours (by about 11.04%, $p < .001$). These results 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 and Claussen 2016, West 2003).

We believe the contrasting results of a complementary within-platform spillover effect of access restriction may be attributed to within-platform interdependencies. A complementor has to incur costs to acquire platform-specific resources and to comply with platform-specific rules.[20] 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 (Baldwin and Clark 2000). For example, a video game developer needs to invest significant efforts to learn console-specific technologies, such as technological interfaces and SDKs (Anderson et al. 2014, 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 Uber Lux (Helling 2020). Maintaining a high rating is costly, and the cost may only be justified if Uber drivers can leverage their ratings across different time segments (Benson et al. 2020). 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 synergy 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 synergy from operating across rush and non-rush hours, restricting access during non-rush hours would have incentivized Lyft drivers to exit from both time periods. Results in Columns 3 and 4 support such a

[20]The costs of tailoring complements to platform-specific interfaces and architectures are often immense and recurring (Anderson et al. 2014, Cennamo et al. 2018). For example, 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 and Agarwal 2017, Temizkan et al. 2012).

conjecture.

### 5.4. Robustness checks and alternative explanations

We run a battery of supplementary tests to check the robustness of our results. These additional tests are presented in the Appendix. 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 (Appendix Tables A3– A6). The results reveal that the effect of Lyft's restriction policy was similar across different time periods. We conduct additional analyses using individual week dummies after the policy change. Coefficients on the week dummies reflect the relative sizes of trip numbers in each week compared to the average trip numbers in the excluded period, that is, four weeks before the policy change. The result shows that no specific individual week strongly drove the overall result (Appendix Table A7).

Second, we estimate the effect of Lyft's access restriction on other rideshare platforms in NYC: Juno and Via (Appendix Table A8). 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 zones in NYC were residential areas and may not exhibit high demand during rush hours. We re-estimate our models using the subsample of trips in Manhattan, the central business district with high traffic demand during rush hours. The results (Appendix Table A9) are consistent with our main findings. Fourth, we re-estimate our models using trips made during weekends (Appendix Table A10), when high-demand time periods were late-night periods (Sat 9 pm–Sun 3 am, Figure 2). Our results hold.

Finally, our 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 first 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.[21] We then plot the differences between the two

[21]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). Our result holds when we include late-night periods.

sets of ATHDs as a heat map (Appendix, Figure A4). The heat map confirms that Uber trip numbers decreased during most day-hours. Next, we estimate the impact 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, though setting 1–5 am as the baseline generates similar results. Results (Appendix, Table A11) confirm that Uber trip numbers decreased during most of the hours relative to 7–8 pm.

### 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 reduces competition and encourages complementors to improve quality (Boudreau 2012, Casadesus-Masanell and Hałaburda 2014, Chu and Wu 2021). 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 lessened competition.

We estimated the impact of Lyft's access restriction on both 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 our data.[22] 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, Liu et al. 2019, 2021). Our results (in Appendix Table A12) show that the trip duration of Lyft and Uber decreased by 2.41 percent and 2.96 percent, respectively, after Lyft's access restriction ($p < .001$).[23] This result, despite being inconclusive due to data limitation, confirms our expectation of a

[22]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 (3.2 miles) and Uber (3 miles).
[23]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

quality improvement due to lessened competition after multi-homing complementors withdrew from both the restricted and competing platforms.[24]

## 6. Discussion and conclusion

The main objective of this study is 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 numbers after Lyft restricted drivers' access to its app during low-demand periods. We find that Lyft's access restriction reduced trip numbers not only on the Lyft platform but also on the Uber platform. In addition, both Lyft's and Uber's trip numbers 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 synergy 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.

Our study contributes to the platform literature in several ways. First, it extends the research on platform governance by connecting it with the research on within-firm interdependencies. 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 and Eesley 2021, Rietveld et al. 2021, Tae et al. 2020). However, these studies "do not consider multi-homing of complementors" (Tae et al. 2020, p. 324) and are "conducted within the boundary of one platform" (Koo and Eesley 2021, p. 961), thereby neglecting potential interdependencies that could arise from (multi-homing) complementor activities across platforms.

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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 generally hold.

[24]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 (Cennamo et al. 2018), higher technological capability (Wen and Zhu 2019), and a larger user base (Bresnahan et al. 2015), 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.

Our study shows that these interdependencies may bring unintended consequences of platform access restriction. Studies on within-firm interdependencies suggest not only that an expansion of a firm's horizontal scope can be constrained by within-firm interdependencies (Zhou 2011) but also that a reduction in firm scope (e.g., divestiture) can potentially destroy synergy and hurt performance (Feldman 2014, de Figueiredo et al. 2019, Natividad and Rawley 2016). By highlighting within-complementor interdependencies, this study addresses an important question in the platform literature, that is, "how do multi-homing (complementors) shape the co-evolution of different platforms?" (Koo and Eesley 2021, p. 961). Our finding suggests that multi-homing complementors may shape platform co-evolution by taking similar actions simultaneously on all the platforms they engage with.

We also point out two sources of within-firm interdependencies based on different types of resources that complementors can utilize. While the role of resources has been a central concern in the theory of the firm, "strategy scholars have spent little time considering how the resource-based view's precepts apply to platforms" (Eisenmann et al. 2011, p. 1282). Even though complementors leverage both platform-specific resources and complementor-specific resources to generate complements (Baldwin and Woodard 2009), prior studies have exclusively focused on platform-specific resources when analyzing synergy, neglecting the synergy that can arise from sharing complementor-specific resources. Our study fills in this gap.

Second, the paper connects the research on platform competition with the research on competition between non-platform firms that share common third-party relationships, such as common suppliers. For example, in the soft drink industry, when a concentrate manufacturer vertically integrates a bottling firm to pursue its own product variety strategy, it changes the latter's synergy and incentivizes them to provide lower quantity and quality of products or services to rival concentrate manufacturers that use the same bottling firm (Zhou and Wan 2017). Such "discrimination" against downstream rivals can occur despite contractual commitments that suppliers have toward their downstream rivals. In this regard, platforms are unique in their inability to secure a commitment from complementors to provide the optimal quantity by either contracts or hierarchical control. We show that in such a context, a change in governance by one platform (e.g., access restriction) can also influence complementor engagement with rival platforms by forcing multi-homing complementors to exit from both platforms. Our analysis, therefore, highlights an important common theme between the two strands of literature on platform competition and vertical relationship for non-platform firms, respectively.

Third, 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" (Corts and Lederman 2009, Eisenmann 2007, Landsman and Stremersch 2011). They suggest that a large platform should prohibit or penalize multi-homing through means such as exclusive contracting, vertical integration, or price discrimination (Armstrong and Wright 2007, Hagiu and Spulber 2013, Li and 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). Our 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 cross-platform synergy. 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 and Lederman 2009).

Finally, the paper offers implications for platform firms and industry regulators. Prior literature argues that platform policies need to consider "interactions that do not happen at firm's boundaries" (Boudreau and Hagiu 2009, p. 187). Our study supports this suggestion and cautions platform firms against implementing governance policies without fully understanding complementors' interdependencies across platforms, as a small change in a complex system composed of numerous interdependent activities can cause unintended ripple effects (Ethiraj and Levinthal 2004). In addition, while antitrust authorities have started to focus attention on platform firms, both policymakers and academia have yet to conclude the best way to regulate platform firms (Greenwood et al. 2017, Hagiu and Wright 2019b, Jacobides and Lianos 2021, Katz 2019). In October 2020, the U.S. House Judiciary Committee released a report that investigated competition in digital markets. The European Commission followed suit and proposed the Digital Markets Act in December 2020. Their focus was to prevent large platform firms from abusing their market power and ensuring fair competition. We argue that such 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 access policies or regulations might excessively constrain complementor activities and threaten the overall health of the platform ecosystem (Iansiti and Levien 2004).

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 results are 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 and Wright 2007, Lee 2013).

Second, the anonymity of our data and the lack of complementor identifiers prevent us from connecting trips to drivers or vehicles. As a result, we cannot identify individual drivers who drove on single vs. multiple platforms or during rush vs. non-rush hours. As explained in the results section, we have conducted various tests to pin down the mechanism against alternative explanations. Future studies can extend this research by examining contexts where within-complementor interdependencies may be directly observed.

Third, while we focus on the short-term impact of platform access restriction, such policies might have long-term consequences. For instance, reduced competition between complementors can incentivize remaining complementors to make platform-specific investments, thereby increasing complement quality in the long run. Unfortunately, we are unable to empirically analyze the long-run impact of access restriction due to the outbreak of COVID-19 immediately after our sample period. NYC was one of the worst affected areas in the U.S. by the global pandemic, and the rideshare business was one of the most severely damaged businesses. Future research may single out short- vs. long-term effects of platform access restriction when the external environment is more stable.

Lastly, our empirical setting does not involve the entry of new complementors. Due to NYC's regulations, Uber and Lyft stopped accepting new drivers on April 1st and April 19th, 2019, respectively (Rubinstein 2019). This setting enables us to study the activities of incumbent complementors without worrying about complications due to new entry. Future research may study platform access restriction in settings where platforms allow the entry of new complementors.

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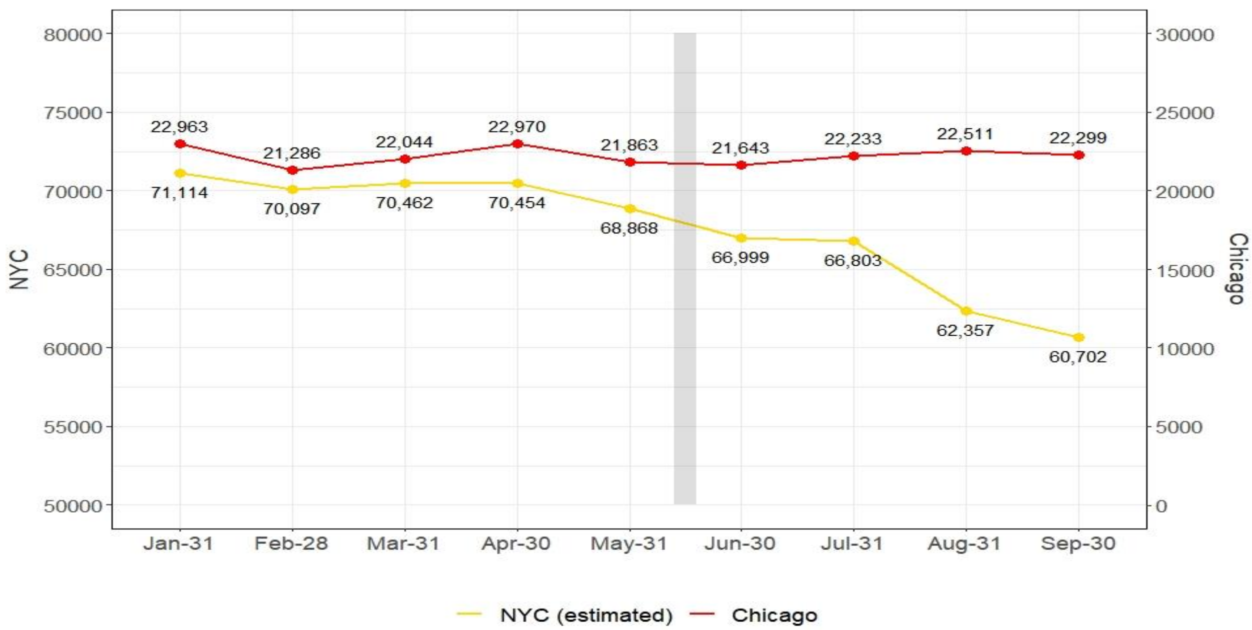
**Figure 1.** Monthly number of multi-homing drivers in 2019




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

**Figure 2.** Heat map of trip number by hour of the week for Uber (left) and Lyft (right)

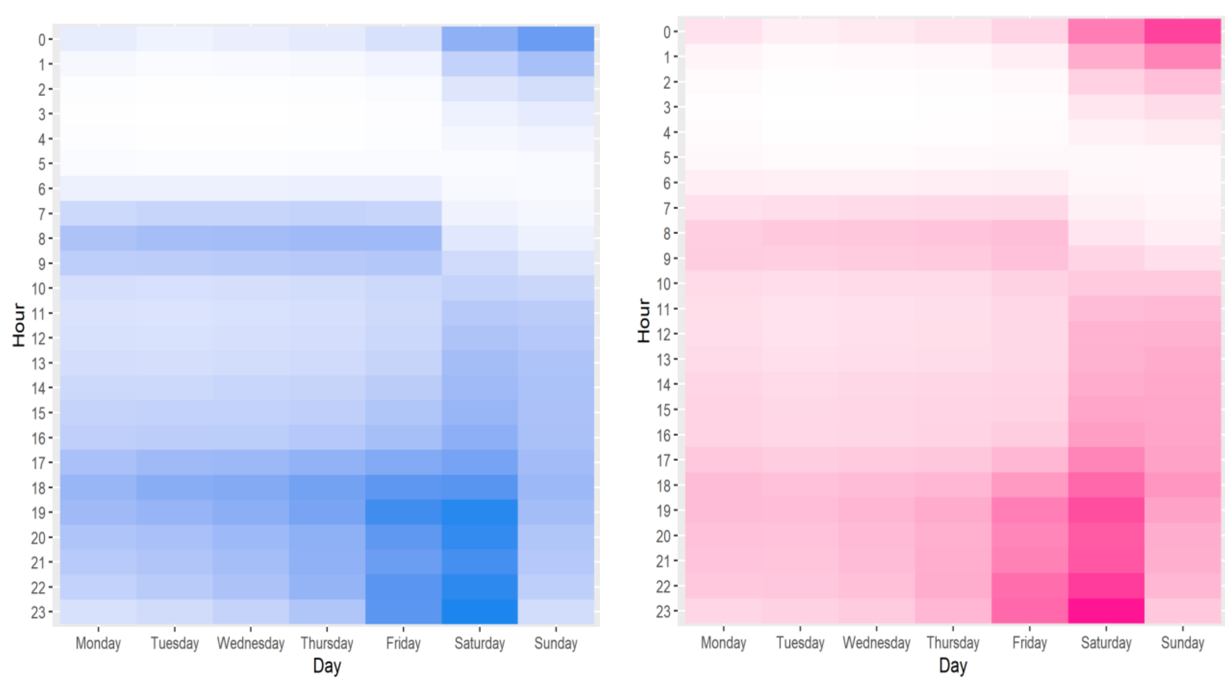


**Table 1.** Summary statistics

| | Variable | Definition | Mean | SD | Min | Max | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| (1) | Lyft's Access Restriction | A binary variable that equals 1 for dates after Lyft restricted drivers from accessing its app and 0 otherwise | 0.50 | 0.50 | 0 | 1 | 1.00 | | | | | |
| (2) | Lyft Trip # | Lyft trip number in a given zone during the given hour of the day | 23.01 | 28.67 | 0 | 732 | -0.03 | 1.00 | | | | |
| (3) | Uber Trip # | Uber trip number in a given zone during the given hour of the day | 72.18 | 78.62 | 0 | 1,330 | -0.02 | 0.88 | 1.00 | | | |
| (4) | Taxi Trip # | Taxi trip number in a given zone during the given hour of the day | 37.15 | 102.15 | 0 | 1,072 | -0.02 | 0.53 | 0.66 | 1.00 | | |
| (5) | Lyft Trip # in 2018 | Lyft trip number in 2018 in a given zone during the given hour of the day | 17.38 | 22.21 | 0 | 398 | -0.01 | 0.91 | 0.81 | 0.51 | 1.00 | |
| (6) | Uber Trip # in 2018 | Uber trip number in 2018 in a given zone during the given hour of the day | 68.40 | 76.14 | 0 | 1,240 | 0.02 | 0.84 | 0.93 | 0.64 | 0.84 | 1.00 |

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

| | **(log) Lyft trip #** | | **(log) Uber trip #** | |
|---|---|---|---|---|
| | **(1)** | **(2)** | **(3)** | **(4)** |
| Lyft's Access Restriction | -0.0586$^{***}$ (0.0055) | -0.0519$^{***}$ (0.0049) | -0.0290$^{***}$ (0.0046) | -0.0479$^{***}$ (0.0042) |
| (log) Lyft Trip # in 2018 | - | 0.202$^{***}$ (0.0119) | - | - |
| (log) Uber Trip # in 2018 | - | - | - | 0.325$^{***}$ (0.0246) |
| (log) Taxi Trip # | - | 0.0721$^{***}$ (0.0077) | - | 0.0642$^{***}$ (0.0073) |
| Zone FEs | Yes | Yes | Yes | Yes |
| Day-of-the-week FEs | Yes | Yes | Yes | Yes |
| Hour FEs | Yes | Yes | Yes | Yes |
| Observations | 242556 | 242556 | 242556 | 242556 |
| Adjusted $R^2$ | 0.819 | 0.831 | 0.889 | 0.906 |

Robust standard errors clustered at the zone level are in parentheses.
$^{*}$ $p < 0.05$; $^{**}$ $p < 0.01$; $^{***}$ $p < 0.001$

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

| | **(log) Uber Trip #** | | **(log) Lyft Trip #** | |
|---|---|---|---|---|
| | **(1)** | **(2)** | **(3)** | **(4)** |
| Uber's Access Restriction | -0.0357$^{***}$ (0.0052) | -0.0266$^{***}$ (0.0034) | 0.0633$^{***}$ (0.0065) | 0.0617$^{***}$ (0.0057) |
| (log) Uber Trip # in 2018 | - | 0.378$^{***}$ (0.0267) | - | - |
| (log) Lyft Trip # in 2018 | - | - | - | 0.221$^{***}$ (0.0138) |
| (log) Taxi Trip # | - | 0.0686$^{***}$ (0.0083) | - | 0.0709$^{***}$ (0.0091) |
| Zone FEs | Yes | Yes | Yes | Yes |
| Day-of-the-week FEs | Yes | Yes | Yes | Yes |
| Hour FEs | Yes | Yes | Yes | Yes |
| Observations | 242143 | 242143 | 242143 | 242143 |
| Adjusted $R^2$ | 0.886 | 0.908 | 0.816 | 0.829 |

Robust standard errors clustered at the zone level are in parentheses.
$^{*}$ $p < 0.05$; $^{**}$ $p < 0.01$; $^{***}$ $p < 0.001$

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

| (log) Trip # | NYC taxi trips as a counterfactual (1) Full sample | NYC taxi trips as a counterfactual (2) Outer boroughs subsample | Chicago rideshare trips as a counterfactual (3) |
|---|---|---|---|
| Lyft's Access Restriction × Uber (1: Uber, 0: Taxi) | -0.0952*** (0.0121) | -0.122*** (0.0150) | - |
| Lyft's Access Restriction × NYC (1: NYC, 0: Chicago) | - | - | -0.0735*** (0.0080) |
| (log) Trip # in 2018 | 0.298*** (0.0146) | 0.226*** (0.0149) | - |
| (log) Taxi Trip # | - | - | 0.0882*** (0.0087) |
| Zone FEs | Yes | Yes | Yes |
| Week FEs | Yes | Yes | Yes |
| Day-of-the-week FEs | Yes | Yes | Yes |
| Hour FEs | Yes | Yes | Yes |
| Observations | 485112 | 359446 | 316234 |
| Adjusted $R^2$ | 0.925 | 0.908 | 0.893 |

Robust standard errors clustered at the zone level are in parentheses.
$^{*}$ $p < 0.05$; $^{**}$ $p < 0.01$; $^{***}$ $p < 0.001$

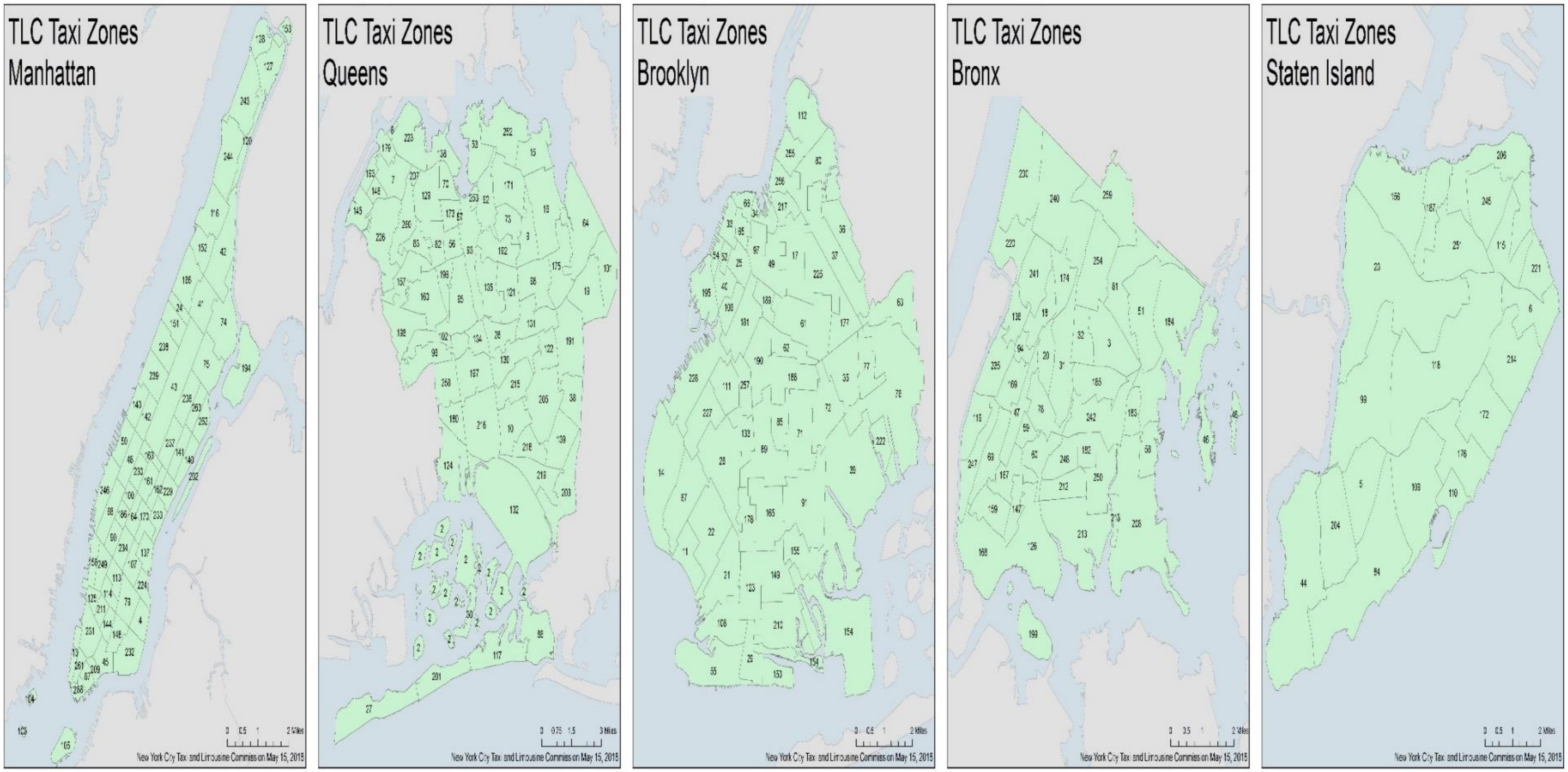
**Table 5.** The effect of Lyft's access restriction in restricted and unrestricted segments

| | (log) Uber Trip # | | (log) Lyft Trip # | |
|---|---|---|---|---|
| | (1) Restricted segments | (2) Unrestricted segments | (3) Restricted segments | (4) Unrestricted segments |
| Lyft's Access Restriction | -0.0331*** (0.0045) | -0.0890*** (0.0051) | -0.0307*** (0.0053) | -0.117*** (0.0074) |
| (log) Uber Trip # in 2018 | 0.299*** (0.0239) | 0.357*** (0.0361) | - | - |
| (log) Lyft Trip # in 2018 | - | - | 0.188*** (0.0115) | 0.210*** (0.0196) |
| (log) Taxi Trip # | 0.0670*** (0.0086) | 0.0488*** (0.0050) | 0.0744*** (0.0091) | 0.0641*** (0.0057) |
| Zone FEs | Yes | Yes | Yes | Yes |
| Day-of-the-week FEs | Yes | Yes | Yes | Yes |
| Hour FEs | Yes | Yes | Yes | Yes |
| Observations | 181450 | 61106 | 181450 | 61106 |
| Adjusted $R^2$ | 0.900 | 0.921 | 0.822 | 0.858 |

Robust standard errors clustered at the zone level are in parentheses.
$^{*}$ $p < 0.05$; $^{**}$ $p < 0.01$; $^{***}$ $p < 0.001$

## Appendix

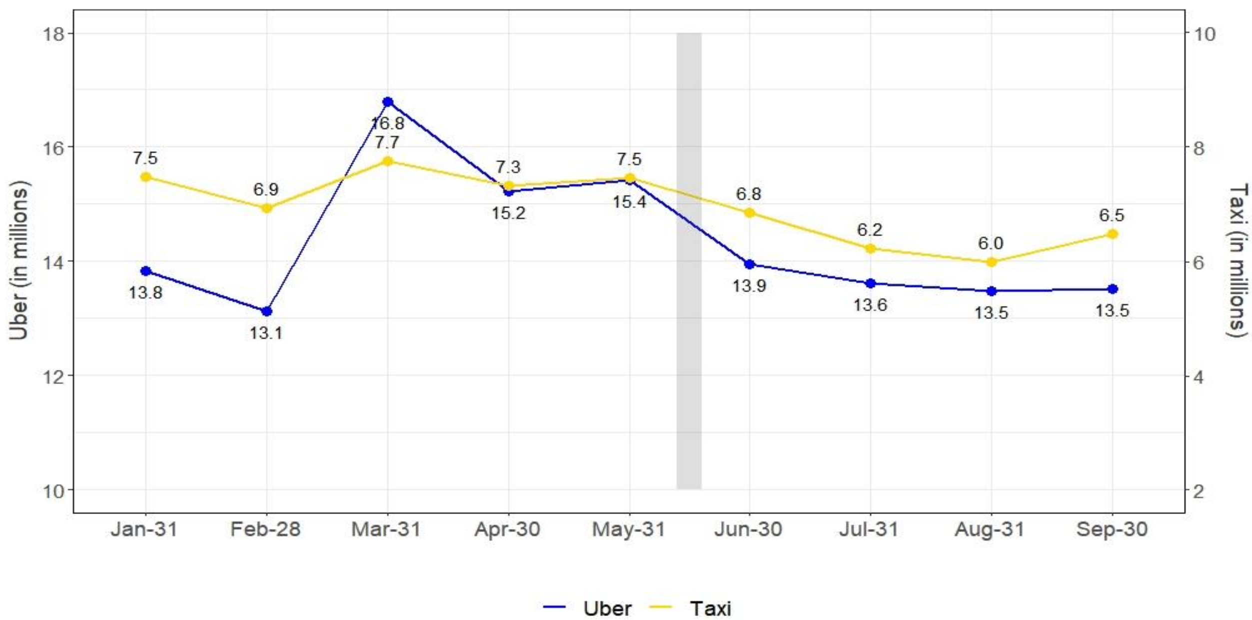
**Figure A1.** New York City taxi zone map




New York City is divided into five boroughs (Manhattan, Bronx, Staten Island, Brooklyn, and Queens), which are further segmented into 263 taxi zones.

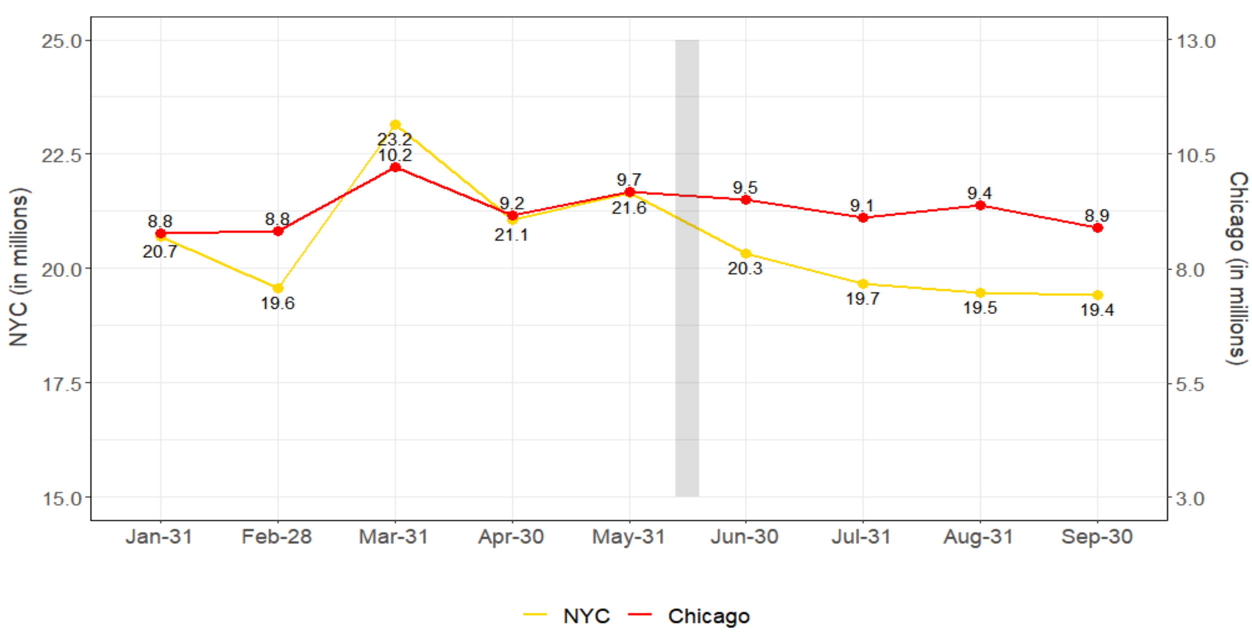
***Source:*** *TLC Trip Record Data (https://www1.nyc.gov/site/tlc/about/tlc-trip-record-data.page)*

**Figure A2.** Monthly trip volume of Uber (left) and taxi (right) during 2019



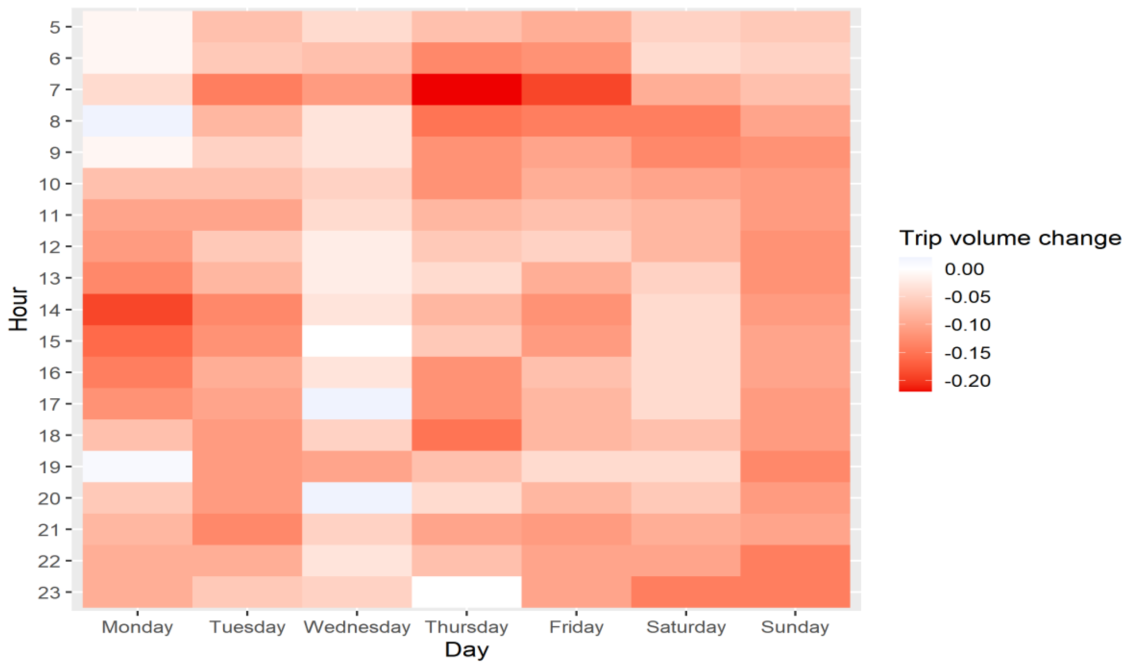

The grey line indicates the date for Lyft's access restriction (June 27th, 2019).

**Figure A3.** 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 A4.** 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).

**Table A1.** Rideshare business in New York City and Chicago

| | Platform | New York City | Chicago |
|---|---|---|---|
| **Service launch time** | Uber | 2011. 05 | 2011. 09 |
| | Lyft | 2014. 07 | 2013. 05 |
| | Via | 2014. 07 | 2015. 11 |
| | Juno | 2013. 08 | - |
| **Market share in 2019** | Uber | 70% | 72% |
| | Lyft | 22% | 27% |
| | Via | 5% | 1% |
| | Juno | 3% | - |

**Table A2.** Summary statistics of rideshare and taxi trips in New York City and Chicago

| | | New York City | Chicago |
|---|---|---|---|
| **Rideshare business** | Avg. fare amount | $21.94 | $15.15 |
| | Avg. trip duration | 18.44 minutes | 18.15 minutes |
| | Avg. trip distance | 2.91 miles | 6.06 miles |
| | Avg. monthly volume | 20,879,622 trips | 9,390,156 trips |
| **Taxi business** | Avg. fare amount | $18.30 | $18.06 |
| | Avg. trip duration | 14.18 minutes | 14.63 minutes |
| | Avg. trip distance | 3.06 miles | 3.68 miles |
| | Avg. monthly volume | 7,297,361 trips | 1,420,676 trips |

Summary statistics are calculated based on rideshare and taxi trips completed between January–June 2019 (before Lyft’s access restriction).

**Table A3.** The effect of Lyft's access restriction on Uber trip numbers with different time windows before and after Lyft's access restriction

| **(log) Uber Trip #** | **1 week** | **2 weeks** | **3 weeks** | **4 weeks** | **5 weeks** | **6 weeks** | **7 weeks** | **8 weeks** |
|---|---|---|---|---|---|---|---|---|
| Lyft's Access | -0.0288*** | -0.0407*** | -0.0413*** | -0.0479*** | -0.0457*** | -0.0731*** | -0.0833*** | -0.0918*** |
| Restriction | (0.0059) | (0.0059) | (0.0049) | (0.0042) | (0.0041) | (0.0043) | (0.0042) | (0.0043) |
| (log) Uber Trip # | 0.282*** | 0.317*** | 0.324*** | 0.325*** | 0.340*** | 0.339*** | 0.339*** | 0.341*** |
| in 2018 | (0.0242) | (0.0248) | (0.0247) | (0.0246) | (0.0245) | (0.0243) | (0.0242) | (0.0244) |
| (log) Taxi Trip # | 0.0560*** | 0.0692*** | 0.0657*** | 0.0642*** | 0.0640*** | 0.0644*** | 0.0634*** | 0.0619*** |
| | (0.0078) | (0.0075) | (0.0075) | (0.0073) | (0.0072) | (0.0073) | (0.0075) | (0.0073) |
| Zone FEs | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Day-of-the-week FEs | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Hour FEs | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 60733 | 121376 | 181989 | 242556 | 303224 | 363801 | 424321 | 484926 |
| Adjusted $R^2$ | 0.909 | 0.898 | 0.903 | 0.906 | 0.906 | 0.894 | 0.897 | 0.900 |

Robust standard errors clustered at the zone level are in parentheses.
* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$

**Table A4.** The effect of Lyft's access restriction on Uber trip numbers: DID estimations using NYC taxis as counterfactuals with different time windows before and after Lyft's access restriction

| **(log) Trip #** | **1 week** | **2 weeks** | **3 weeks** | **4 weeks** | **5 weeks** | **6 weeks** | **7 weeks** | **8 weeks** |
|---|---|---|---|---|---|---|---|---|
| Lyft's Access Restriction × Uber (1: Uber, 0: Taxi) | -0.0737*** | -0.0703*** | -0.0852*** | -0.0952*** | -0.101*** | -0.117*** | -0.124*** | -0.128*** |
| | (0.0126) | (0.0135) | (0.0126) | (0.0121) | (0.0116) | (0.0116) | (0.0114) | (0.0115) |
| (log) Trip # in 2018 | 0.279*** | 0.293*** | 0.298*** | 0.298*** | 0.304*** | 0.306*** | 0.307*** | 0.308*** |
| | (0.0149) | (0.0151) | (0.0148) | (0.0146) | (0.0146) | (0.0146) | (0.0146) | (0.0146) |
| Zone FEs | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Week FEs | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Day-of-the-week FEs | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Hour FEs | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 121466 | 242752 | 363978 | 485112 | 606448 | 727602 | 848642 | 969852 |
| Adjusted $R^2$ | 0.926 | 0.923 | 0.924 | 0.925 | 0.925 | 0.924 | 0.924 | 0.925 |

Robust standard errors clustered at the zone level are in parentheses.
* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$

**Table A5.** The effect of Lyft's access restriction on Uber trip numbers: DID estimations using NYC taxis as counterfactuals with different time windows before and after Lyft's access restriction (Outer boroughs subsample)

| (log) Trip # | 1 week | 2 weeks | 3 weeks | 4 weeks | 5 weeks | 6 weeks | 7 weeks | 8 weeks |
|---|---|---|---|---|---|---|---|---|
| Lyft's Access Restriction × Uber (1: Uber, 0: Taxi) | -0.0856*** (0.0160) | -0.0878*** (0.0161) | -0.106*** (0.0153) | -0.122*** (0.0150) | -0.129*** (0.0146) | -0.147*** (0.0146) | -0.154*** (0.0143) | -0.161*** (0.0143) |
| (log) Trip # in 2018 | 0.217*** (0.0156) | 0.224*** (0.0157) | 0.227*** (0.0153) | 0.226*** (0.0149) | 0.232*** (0.0149) | 0.234*** (0.0150) | 0.234*** (0.0150) | 0.235*** (0.0150) |
| Zone FEs | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Week FEs | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Day-of-the-week FEs | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Hour FEs | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 90004 | 179878 | 269714 | 359446 | 449362 | 539090 | 628700 | 718480 |
| Adjusted $R^2$ | 0.909 | 0.907 | 0.907 | 0.908 | 0.908 | 0.907 | 0.907 | 0.908 |

Robust standard errors clustered at the zone level are in parentheses.
* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$

**Table A6.** The effect of Lyft's access restriction on Uber trip numbers: DID estimations using Chicago rideshare trips as counterfactuals with different time windows before and after Lyft's access restriction

| (log) Trip # | 1 week | 2 weeks | 3 weeks | 4 weeks | 5 weeks | 6 weeks | 7 weeks | 8 weeks |
|---|---|---|---|---|---|---|---|---|
| Lyft's Access Restriction × NYC (1: NYC, 0: Chicago) | -0.0502*** (0.0117) | -0.0560*** (0.0116) | -0.0579*** (0.0091) | -0.0735*** (0.0080) | -0.0722*** (0.0077) | -0.0946*** (0.0079) | -0.0967*** (0.0081) | -0.101*** (0.0083) |
| (log) Taxi Trip # | 0.0796*** (0.0089) | 0.0926*** (0.0090) | 0.0899*** (0.0089) | 0.0882*** (0.0087) | 0.0899*** (0.0087) | 0.0900*** (0.0089) | 0.0893*** (0.0089) | 0.0887*** (0.0088) |
| Zone FEs | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Week FEs | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Day-of-the-week FEs | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Hour FEs | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 79161 | 158219 | 237250 | 316234 | 395297 | 474295 | 553224 | 632238 |
| Adjusted $R^2$ | 0.899 | 0.887 | 0.891 | 0.893 | 0.891 | 0.884 | 0.886 | 0.887 |

Robust standard errors clustered at the zone level are in parentheses.
* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$

**Table A7.** The effect of Lyft's access restriction on Uber trip numbers using individual week dummies

| **(log) Uber Trip #** | **(1)** Full sample | **(2)** Restricted segment | **(3)** Unrestricted segment |
|---|---|---|---|
| Week 1 Dummy | -0.0188*** (0.0056) | -0.00203 (0.0066) | -0.0624*** (0.0055) |
| Week 2 Dummy | -0.0895*** (0.0067) | -0.0453*** (0.0070) | -0.223*** (0.0081) |
| Week 3 Dummy | -0.0482*** (0.0046) | -0.0408*** (0.0049) | -0.0670*** (0.0066) |
| Week 4 Dummy | -0.0349*** (0.0042) | -0.0441*** (0.0047) | -0.00347 (0.0063) |
| (log) Uber Trip # in 2018 | 0.324*** (0.0246) | 0.299*** (0.0239) | 0.350*** (0.0360) |
| (log) Taxi Trip # | 0.0639*** (0.0073) | 0.0669*** (0.0087) | 0.0458*** (0.0049) |
| Zone fixed effect | Yes | Yes | Yes |
| Day-of-the-week FEs | Yes | Yes | Yes |
| Hour fixed effect | Yes | Yes | Yes |
| Observations | 242556 | 181450 | 61106 |
| Adjusted $R^2$ | 0.906 | 0.900 | 0.923 |

Robust standard errors clustered at the zone level are in parentheses.
$^{*}$ $p < 0.05$; $^{**}$ $p < 0.01$; $^{***}$ $p < 0.001$

**Table A8.** The effect of Lyft's access restriction on the trip numbers of Juno and Via[25]

| | **(1)** (log) Juno Trip # | **(2)** (log) Via Trip # |
|---|---|---|
| Lyft's Access Restriction | -0.0364*** (0.0070) | -0.0771*** (0.0080) |
| (log) Via Trip # in 2018 | - | 0.164*** (0.0062) |
| (log) Taxi Trip # | 0.120*** (0.0116) | 0.160*** (0.0152) |
| Zone FEs | Yes | Yes |
| Day-of-the-week FEs | Yes | Yes |
| Hour FEs | Yes | Yes |
| Observations | 242556 | 242556 |
| Adjusted $R^2$ | 0.656 | 0.819 |

Robust standard errors clustered at the zone level are in parentheses.
$^{*}$ $p < 0.05$; $^{**}$ $p < 0.01$; $^{***}$ $p < 0.001$

[25]Juno did not provide location information before 2019, which disables us to control for its trip trend in 2018.

**Table A9.** The effect of Lyft's access restriction on Uber trip numbers (Manhattan subsample)

| | **(1)**<br>Full sample | **(2)**<br>Restricted segments | **(3)**<br>Unrestricted segments |
|---|---|---|---|
| Lyft's Access Restriction | -0.0479*** | -0.0331*** | -0.0890*** |
| | (0.0042) | (0.0045) | (0.0051) |
| (log) Uber Trip # in 2018 | 0.325*** | 0.299*** | 0.357*** |
| | (0.0246) | (0.0239) | (0.0361) |
| (log) Taxi Trip # | 0.0642*** | 0.0670*** | 0.0488*** |
| | (0.0073) | (0.0086) | (0.0050) |
| Zone FEs | Yes | Yes | Yes |
| Day-of-the-week FEs | Yes | Yes | Yes |
| Hour FEs | Yes | Yes | Yes |
| Observations | 242556 | 181450 | 61106 |
| Adjusted $R^2$ | 0.906 | 0.900 | 0.921 |

Robust standard errors clustered at the zone level are in parentheses.
$^{*}$ $p < 0.05$; $^{**}$ $p < 0.01$; $^{***}$ $p < 0.001$

**Table A10.** The effect of Lyft's access restriction on Uber trip numbers during weekends

: Based on the heat map (Figure 2), we assume late-night periods (Sat 9 pm–Sun 3 am) as unrestricted time segments and other periods as restricted time segments.

| **(log) Uber Trip #** | **(1)**<br>Full sample | **(2)**<br>Restricted segment | **(3)**<br>Unrestricted segment |
|---|---|---|---|
| Lyft Access Restriction | -0.0658*** | -0.0559*** | -0.125*** |
| | (0.0040) | (0.0042) | (0.0081) |
| (log) Uber trip # in 2018 | 0.399*** | 0.384*** | 0.281*** |
| | (0.0315) | (0.0325) | (0.0477) |
| (log) Taxi Trip # | 0.0668*** | 0.0642*** | 0.0590*** |
| | (0.0077) | (0.0076) | (0.0110) |
| Zone FEs | Yes | Yes | Yes |
| Day-of-the-week FEs | Yes | Yes | Yes |
| Hour FEs | Yes | Yes | Yes |
| Observations | 97577 | 85325 | 12252 |
| Adjusted $R^2$ | 0.912 | 0.912 | 0.921 |

Robust standard errors clustered at the zone level are in parentheses.
$^{*}$ $p < 0.05$; $^{**}$ $p < 0.01$; $^{***}$ $p < 0.001$

**Table A11.** The effect of Lyft's access restriction on Uber trip numbers: Breakdown by hours (Baseline: 7 pm–8 pm)

| **(log) Uber Trips** | **(1)** | |
|---|---|---|
| Lyft's Access Restriction | 0.00622 | (0.76) |
| Lyft's Access Restriction × (12 am–1 am) | 0.00559 | (0.54) |
| Lyft's Access Restriction × (1 am–2 am) | 0.0426** | (3.13) |
| Lyft's Access Restriction × (2 am–3 am) | 0.0499*** | (3.53) |
| Lyft's Access Restriction × (3 am–4 am) | 0.0472*** | (3.57) |
| Lyft's Access Restriction × (4 am–5 am) | -0.00412 | (-0.31) |
| Lyft's Access Restriction × (5 am–6 am) | -0.0440*** | (-3.89) |
| Lyft's Access Restriction × (6 am–7 am) | -0.110*** | (-10.19) |
| Lyft's Access Restriction × (7 am–8 am) | -0.189*** | (-16.64) |
| Lyft's Access Restriction × (8 am–9 am) | -0.146*** | (-15.36) |
| Lyft's Access Restriction × (9 am–10 am) | -0.1000*** | (-11.15) |
| Lyft's Access Restriction × (10 am–11 am) | -0.110*** | (-9.71) |
| Lyft's Access Restriction × (11 am–12 pm) | -0.0868*** | (-7.54) |
| Lyft's Access Restriction × (12 pm–1 pm) | -0.0899*** | (-9.36) |
| Lyft's Access Restriction × (1 pm–2 pm) | -0.0712*** | (-7.41) |
| Lyft's Access Restriction × (2 pm–3 pm) | -0.0978*** | (-10.72) |
| Lyft's Access Restriction × (3 pm–4 pm) | -0.0857*** | (-10.63) |
| Lyft's Access Restriction × (4 pm–5 pm) | -0.0610*** | (-7.41) |
| Lyft's Access Restriction × (5 pm–6 pm) | -0.0537*** | (-7.11) |
| Lyft's Access Restriction × (6 pm–7 pm) | -0.0765*** | (-11.21) |
| Lyft's Access Restriction × (8 pm–9 pm) | -0.00815 | (-1.04) |
| Lyft's Access Restriction × (9 pm–10 pm) | -0.0289*** | (-3.38) |
| Lyft's Access Restriction × (10 pm–11 pm) | -0.0316*** | (-3.51) |
| Lyft's Access Restriction × (11 pm–12 am) | -0.0370*** | (-3.63) |
| (log) Uber Trips in 2018 | 0.324*** | (13.12) |
| (log) Taxi Trip # | 0.0641*** | (8.73) |
| Zone FEs | Yes | |
| Day-of-the-week FEs | Yes | |
| Hour FEs | Yes | |
| Observations | 242556 | |
| Adjusted $R^2$ | 0.906 | |

Robust standard errors clustered at the zone level are in parentheses.
$^{*}$ $p < 0.05$; $^{**}$ $p < 0.01$; $^{***}$ $p < 0.001$

**Table A12.** The effect of Lyft's access restriction on trip duration of Lyft and Uber

Prior studies have used driver detours to measure Uber and taxi drivers' fraud and treated longer trip duration (for the same route) as lower service quality (Balafoutas et al. 2013, Liu et al. 2019, 2021). Aligned with these studies, news media reported that Uber drivers often employ a practice known as longhauling[26] – taking an unnecessarily long route to a destination to drive up a fare (Bensinger 2018, Dorsey 2018). Thus, we use trip duration (controlling for trip distance and the total number of taxi and rideshare trips in the zone and hour) to capture service quality. We use the following equation.

$$\log(Y_{ijht}) = \beta_0 + \beta_1 A_t + X_{iht}B + \alpha_j + \delta_h + \gamma_t + \varepsilon_{iht}, \tag{1}$$

where $Y_{iht}$ is the average trip duration (in minutes) of trips reported on platform $i$ (Lyft or Uber), in zone $j$, during the $h^{th}$ hour of day $t$, $A_t$ is a binary variable that equals one for dates after Lyft restricted access to its app (June 27th, 2019), and zero otherwise, and $X_{iht}$ includes average trip distance and the total number of private transportation trips in zone $j$ during the $h^{th}$ hour of day $t$. $\alpha_j$, $\delta_h$, and $\gamma_t$ are zone, hour, and day-of-the-week fixed effects, respectively.

One caveat is that trip duration may be endogenous to the number of vehicles on the road. Even though we controlled for trip distance and the total trip number of rideshare services and taxis during the hour, 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 employed a DID estimation using taxi trip duration as a counterfactual. Taxi trips were also subject to reduced congestion after Lyft's access restriction, which enables us to control for the time trend in road congestion. Our results generally hold.

However, the DID estimation is not without problems. First, taxi drivers might not be an appropriate control group. For example, Uber drivers tend to engage less in detours than taxi drivers due to real-time monitoring and rating systems (Liu et al. 2021). Second, the demand for taxi trips could have increased after rideshare trips decreased. This could improve the service quality of taxis as taxi drivers tend to detour less when the demand is high (Liu et al. 2019). Because of these limitations, we leave it for future studies to investigate the effect of access restriction on quality using a more direct measure of service quality (e.g., driver ratings).

| **(log) Trip Duration** (in minutes) | **(1)** Lyft trip duration | **(2)** Uber trip duration |
|---|---|---|
| Lyft's Access Restriction | -0.0300*** | -0.0244*** |
| | (0.0008) | (0.0010) |
| Trip Distance | 0.159*** | 0.157*** |
| | (0.0026) | (0.0022) |
| (log) Total Trip # | 0.0817*** | 0.101*** |
| | (0.0037) | (0.0040) |
| Zone FEs | Yes | Yes |
| Day-of-the-week FEs | Yes | Yes |
| Hour FEs | Yes | Yes |
| Observations | 240013 | 228337 |
| Adjusted $R^2$ | 0.746 | 0.710 |

Robust standard errors clustered at the zone level are in parentheses.
$^{*}$ $p < 0.05$; $^{**}$ $p < 0.01$; $^{***}$ $p < 0.001$

[26] While passengers pay the fixed upfront price, drivers' pay is determined by the actual trip's mileage and time.

**Table A13.** The effect of Lyft's access restriction on Uber trip numbers

| | (1) Full sample | (2) Low-demand periods | (3) High-demand periods |
|---|---|---|---|
| Lyft's Access Restriction | -0.0582*** | -0.0438*** | -0.0972*** |
| | (0.0034) | (0.0036) | (0.0046) |
| (log) Uber Trip # in 2018 | 0.439*** | 0.379*** | 0.408*** |
| | (0.0257) | (0.0251) | (0.0335) |
| (log) Taxi trip # | 0.0736*** | 0.0728*** | 0.0609*** |
| | (0.0071) | (0.0079) | (0.0056) |
| Zone FEs | Yes | Yes | Yes |
| Day-of-the-week FEs | Yes | Yes | Yes |
| Hour FEs | Yes | Yes | Yes |
| Observations | 340134 | 266776 | 73358 |
| Adjusted $R^2$ | 0.900 | 0.899 | 0.916 |

Robust standard errors clustered at the zone level are in parentheses.
$^{*}$ $p < 0.05$; $^{**}$ $p < 0.01$; $^{***}$ $p < 0.001$

**Table A14.** The effect of Lyft's access restriction on Uber trip numbers

| (log) Uber Trip # | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Lyft's Access Restriction | -0.0290*** | -0.0476*** | -0.0307*** | -0.0479*** |
| | (0.0046) | (0.0043) | (0.0044) | (0.0042) |
| (log) Uber Trip # in 2018 | - | 0.344*** | - | 0.325*** |
| | | (0.0275) | | (0.0246) |
| (log) Taxi Trip # | - | - | 0.0871*** | 0.0642*** |
| | | | (0.0107) | (0.0073) |
| Zone FEs | Yes | Yes | Yes | Yes |
| Day-of-the-week FEs | Yes | Yes | Yes | Yes |
| Hour FEs | Yes | Yes | Yes | Yes |
| Observations | 242556 | 242556 | 242556 | 242556 |
| Adjusted $R^2$ | 0.889 | 0.904 | 0.893 | 0.906 |

Robust standard errors clustered at the zone level are in parentheses.
$^{*}$ $p < 0.05$; $^{**}$ $p < 0.01$; $^{***}$ $p < 0.001$

**Table A15.** The effect of Lyft's access restriction on Lyft trip numbers (an interaction model with rush hours)

| **(log) Lyft Trip #** | **(1)** | **(2)** | **(3)** |
|---|---|---|---|
| Lyft Access Restriction | -0.0586$^{***}$ (0.0055) | -0.0519$^{***}$ (0.0049) | -0.0300$^{***}$ (0.0053) |
| Lyft Access Restriction × Rush Hour | - | - | -0.0867$^{***}$ (0.0076) |
| (log) Lyft Trip # in 2018 | - | 0.202$^{***}$ (0.0119) | 0.202$^{***}$ (0.0120) |
| (log) Taxi Trip # | - | 0.0721$^{***}$ (0.0077) | 0.0719$^{***}$ (0.0077) |
| Zone FEs | Yes | Yes | Yes |
| Day-of-the-week FEs | Yes | Yes | Yes |
| Hour FEs | Yes | Yes | Yes |
| Observations | 242556 | 242556 | 242556 |
| Adjusted $R^2$ | 0.819 | 0.831 | 0.831 |

Robust standard errors clustered at the zone level are in parentheses.
$^{*}$ $p < 0.05$; $^{**}$ $p < 0.01$; $^{***}$ $p < 0.001$