Uber Might Buy Me a Mercedes Benz: An Empirical Investigation of the Sharing Economy and Durable Goods Purchase

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Abstract
In this work, we examine how the introduction of sharing-economy platforms (e.g., Uber, Lift, and Airbnb) affects durable goods purchase. On the one hand, the introduction of such platforms may cannibalize durable goods sales, as they may change the decision making calculus of consumers by offering a lower cost "rental" option in lieu of purchase. On the other hand, such platforms may stimulate purchase, as the presence of these platforms may enhance the value of ownership (because participants may seek to capture the rents associated with participating in the sharing economy). In this study, we resolve this tension using a unique dataset of new vehicle registrations in China. In particular, we exploit a natural experiment, the variation in timing of Uber entry across different locations, to estimate the effect on vehicle purchase. Findings suggest that Uber entry is associated with a considerable increase (8%) in new vehicle ownership, suggesting that consumers are actively changing their stock of held resources in order to capture excess rents offered by these platforms. Further, results indicate that the effect of Uber entry varies considerably across gender, age, and vehicle types.

Keywords*: Sharing economy, platform, ridesharing, car sales, durable goods purchase, difference in difference*

1. Introduction

Although the rise of the sharing economy has dramatically changed the way consumers and producers of services interact, researchers are scrambling to determine how these platform-enabled businesses (e.g., Uber, Lift, and Airbnb), are affecting behavior at the macro (societal), mezzo (firm), and micro (individual) levels. A host of recent research attests to such changes, including increased access to goods (Einav et al. 2016), strategic changes in the behavior of incumbent firms (Seamans and Zhu 2013, Wallsten 2015, Zervas et al. 2017), increased efficiency and asset utilization (Cramer and Krueger 2016), improved public safety (Greenwood and Wattal 2017, Park et al. 2016), and changing access to job opportunities (Burtch et al. 2017). Yet, within this expanding corpus of work, scholars have predominantly focused on the demand side of the economy, i.e. those who leverage or utilize such services, instead of the supply side, i.e., those who provide such services (with the notable exceptions of Burtch et al. (2017), Hall and Krueger (2016), and Edelman et al. (2017)). And scholars have hitherto not considered how consumers may be updating their behavior to capture the rents offered by this economy (even within these exceptions), which is projected to grow to $335B by 2025.[1] While these platforms often label themselves as intermediaries for individuals to share existing or at rest resources, opponents argue that individuals who participate on such platforms may be making capital investments to capture rents as professional workers (thereby circumventing regulations that govern traditional businesses (Malhotra and Van Alstyne 2014)). In this study, we investigate such possibilities by asking: how does the introduction of ridesharing platforms affect new vehicle ownership?

Interestingly, the *a priori* relationship between the presence of sharing platforms and durable goods purchase is far from clear. Unlike traditional markets for durable goods, a key feature of sharing platforms is that both the demand side and supply side are consumers of the durable good (e.g. a home in the case of Airbnb or an automobile in the case of Uber), meaning that both may be actively changing their stocks of the good to capture the benefits of the platform. In the case of the demand side, consumers who utilize these platforms may jettison ownership as alternative ways to consume the product, i.e. "rental" or "temporary ownership," become available. In the case of the supply side, this means that suppliers of the good may have

an incentive to acquire more of the product in order to capture the excess rents.

The implication of this unique consumption behavior forms the basis for our question. On the one hand, the presence of sharing platforms may lead to a *cannibalization effect* on product sales (Jiang and Tian 2016). If ridesharing platforms provide efficient and low-cost transportation options (Cramer and Krueger 2016, Greenwood and Wattal 2017), and freedom from the financial, social, and emotional burdens of ownership (Eckhardt and Bardhi 2015, Schaefers et al. 2016), these platforms may reduce a consumers' propensity to own a car (Abhishek et al. 2016, Fraiberger and Sundararajan 2015, Horton and Zeckhauser 2016, Jiang and Tian 2016); the reason being that the individual no longer needs to subsidize the entire cost of vehicle ownership when cheaper transportation options are available. Indeed, the popular press has identified several such cases of people opting out of vehicle ownership as a result of ridesharing availability (Manjoo 2014). On the other hand, the presence of sharing platforms may lead to a *value enhancement effect* on ownership (Abhishek et al. 2016, Jiang and Tian 2016). As such platforms imbue owners the possibility of sharing excess capacity with others to earn additional income (Burtch et al. 2017), as well as access flexible working schedules (Hall and Krueger 2016), consumers may adjust their valuation of the ownership, and make additional durable goods purchases while defraying the cost through the platform (Abhishek et al. 2016, Fraiberger and Sundararajan 2015, Horton and Zeckhauser 2016, Jiang and Tian 2016).

To resolve this tension, we exploit a natural experiment, the variation in the timing of Uber's entry into different cities in China between 2010 and 2015.[2] We estimate the effect of Uber entry on vehicle purchases using a unique dataset on new car registrations. Importantly, because the platform entered different cities at different times, i.e. implementation is both geographically and temporally dispersed, we are able to leverage a difference-in-difference estimation technique, as well as an extensive set of robustness and falsification checks (Autor 2003, Burtch et al. 2017, Greenwood and Wattal 2017), including alternative model specifications, the inclusion of additional controls, and a variety of placebo tests.

Results indicate several notable findings. First, estimates suggest that Uber entry is strongly correlated

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[2] Uber was introduced in China in August 2013 and started to offer peer-to-peer ridesharing service, e.g., People's Uber, in August 2014.

with an increase in new car ownership, indicating that the prospect of value enhancement (i.e., flexible work schedule or earning additional income) through sharing excess capacity dominates any cannibalization. The magnitude of the estimated effect is also pronounced. Results indicate that the entry of Uber's ridesharing service into cities in China, on average, yields an 8% increase in new car ownership. These results are robust to a host of alternative specifications (e.g., additional control variables) and estimators (e.g., OLS and pseudo Poisson maximum likelihood), with no pre-entry trend detected (indicating that the parallel trend assumption required for the difference-in-difference model is not violated (Angrist and Pischke 2008)).

To better identify the underlying mechanism of the effect, we extend these analyses by considering conditions under which the effect of platform entry may vary. Specifically, we decompose the effect by vehicle type, gender, and age. Results consistently indicate that platform entry leads to a significant increase in new vehicle purchases, but the effect varies across vehicle types and buyers. First, results indicate a stronger effect of platform entry on smaller cars than larger cars. This corroborates value enhancement as the key mechanism, suggesting that the presence of sharing platforms incentivizes individuals to make investment in vehicles that are more likely to be utilized by suppliers on these platforms, viz. vehicles that are lighter and have smaller engines (and hence superior fuel economy). Second, we examine the heterogeneous effect by gender, finding that women are less affected than men. This corroborates recent surveys which suggest that female drivers are disproportionally less likely to become Uber drivers (Hall and Krueger 2016). However, women are still (positively) affected in a non-trivial manner, underscoring recent claims that the gig-economy helps to ameliorate traditional gender biases (Galluzzo 2016, Wright 2017). Third, we find a differential impact of platform entry on different age groups, with middle-aged adults being less affected than buyers in other age groups. Theoretically, given that younger and older people have higher income volatility (Clark and Summers 1981, Gundersen and Ziliak 2008, Jaimovich and Siu 2009, Ríos-Rull 1996), this supports the notion that such platforms are incentivizing labor market participation on the part of traditionally marginalized groups.

Several contributions stem from this work. First, we contribute to the emerging stream of literature on sharing economy by empirically examining whether and how the introduction of sharing platforms affects

durable goods purchase. While researchers have begun to broach this subject analytically (Abhishek et al. 2016, Jiang and Tian 2016), this work is the first to provide empirical evidence of an impact of sharing platforms on ownership in the context of ridesharing platforms. Our findings also underscore a key feature of the sharing platforms. That is, as both the demand side and supply side consist of consumers of the durable goods, the introduction of sharing platforms may affect the consumers on both sides. While the consumers on the demand side may utilize these platforms as an alternative way to consume the product temporarily without owning one, this work concludes that the consumers on the supply side are actively changing their stock of the product to take advantage of the opportunity to capture excess rents offered by these platforms. In this same vein, our work extends extant research on the societal impacts of the sharing economy (Burch et al. 2017, Cohen et al. 2016, Greenwood and Wattal 2017, Li et al. 2016, Park et al. 2016). Contrary to claims made by platforms managers that individuals working on such platforms are simply exploiting existing assets, our findings suggest that a considerable group of suppliers make active capital investments.

Second, this study provides implications for manufacturers. Despite consistent claims in the popular press that platforms may cannibalize sales (Newberg 2015), our results suggest that such concerns are unfounded. Though the introduction of these sharing platforms may reduce a consumers' propensity to own a car (Abhishek et al. 2016, Fraiberger and Sundararajan 2015, Horton and Zeckhauser 2016, Jiang and Tian 2016) as cheaper transportation options are available, our empirical examination suggests that vehicle purchases do rise. In particular, results provide evidence that manufacturers may benefit from the emergence of the sharing economy, as sharing platforms provide a venue to enhance the value of product ownership, which in turn increase product sales. This is notably true for manufacturers whose products are more likely to be utilized by platform participants, such as smaller vehicles with superior fuel economy. Thus, for manufacturers, there are significant incentives to support the entry of sharing platforms, and to target consumers who are more likely to participate in the supply side of sharing economy.

Finally, notable policy implications extend from this study. With respect to the current policy debate on the legality of sharing platforms (Malhotra and Van Alstyne 2014), our finding suggests that the sharing economy may create positive externality to local markets in the form of durable goods purchase. Coupled

with evidence of other benefits of the sharing economy, e.g. higher efficiency and productivity (Cramer and Krueger 2016), improved public safety (Greenwood and Wattal 2017, Park et al. 2016), improved traffic condition (Li et al. 2016), and higher consumer surplus (Cohen et al. 2016), the externality is non-trivial. It is worth noting, however, that our findings undercut the claims made by platforms that individuals working on them are simply exploiting already existing resources. It is clear that some platform workers are making capital investments to exploit these platforms (in the form of accelerated vehicle purchase), suggesting some form of nascent professionalism on the part of platform workers who are actively changing their stock of resources in order to capture excess rents offered by these platforms. In other words, consumers are making purchases in order to participate in the sharing economy instead of simply sharing existing, spare resources. This buoys recent claims made by critics of ridesharing services, who allege that platform participants are engaging in regulatory arbitrage to bypass the bureaucratic red tape surrounding transportation services (Baker 2014, Malhotra and Van Alstyne 2014).

2. Related Literature
2.1 Sharing Economy
The sharing economy has received considerable attention from researchers since its emergence (Abhishek et al. 2016, Edelman et al. 2017, Horton and Zeckhauser 2016, Malhotra and Van Alstyne 2014, Sundararajan 2013, Sundararajan 2014, Zervas et al. 2017). And, as a number of technology-enabled sharing platforms (e.g., Uber, Lyft, TaskRabbit, Airbnb) have proliferated, so has interest in these platforms that facilitate access-based utilization of durable goods (Sundararajan 2013). Broadly speaking, prior literature on the sharing economy can, generally, be grouped into three distinct but related streams: platform design (Einav et al. 2016, Fradkin 2015, Fradkin et al. 2015, Zervas et al. 2015), the impacts on incumbent industries (Abhishek et al. 2016, Benjaafar et al. 2015, Seamans and Zhu 2013, Wallsten 2015, Zervas et al. 2017), and the societal impacts of these platforms (Burtch et al. 2017, Cheng et al. 2016, Edelman et al. 2017, Ge et al. 2016, Greenwood and Wattal 2017, Horton and Zeckhauser 2016, Li et al. 2016, Park et al. 2016, Rhue and Clark 2016). We describe each of these briefly below.

Focus within the body of literature that has examined the design of sharing platforms, to date, has primarily been on the means by which the utility derived by customers might be increased (Einav et al. 2016,

Fradkin et al. 2015, Zervas et al. 2015). Einav et al. (2016), for example, discuss various aspects of platform design, including algorithm design to facilitate search and matching between buyers and sellers, pricing mechanisms, and design of reputation systems to gauge trust. Other studies have examined more specific aspects of platform design. For example, Fradkin (2015) finds that the current ranking algorithm utilized by Airbnb results in substantial search frictions for Airbnb users, and proposes several alternative algorithms to reduce such frictions. Edelman et al. (2017) and Cui et al. (2016) find that the current matching mechanism of Airbnb results in racial discrimination (a finding echoed by Rhue and Clark (2016) in the context of Kickstarter). Zervas et al. (2015) document that in the case of Airbnb, nearly 95% of the properties listed on Airbnb have an average rating of 4.5 stars or higher. Fradkin et al. (2017) also use the context of Airbnb to study how the design of reputation systems affects the ability to solicit unbiased ratings and reviews, and potential ways to reduce review bias. A small number of studies analyze Uber's platform design and find that the surge pricing mechanism utilized by Uber significantly increased labor supply (Chen and Sheldon 2015) and generated substantial consumer surplus (Cohen et al. 2016).

When considering the impact that digital platforms have on incumbent industries and their business models, results are equally rich. Within this stream of literature, two broad questions have been examined. First, how does the emergence of platforms influence market dynamics (Abhishek et al. 2016, Horton and Zeckhauser 2016)? Second, are there direct implications for competitors (Seamans and Zhu 2013, Wallsten 2015, Zervas et al. 2017)? Zervas et al. (2017), for example, investigate the effect of Airbnb on the hotel industry, and find that an increase in the supply of Airbnb listings is associated with a significant decrease in hotel revenue. Alternatively, Wallsten (2015) studies the effect of Uber entry on the taxi industry, and finds that an increase in the popularity of Uber is associated with a decline in the number of complaints about taxi services. In each case, results underscore the potential disruptive capability of sharing economy platforms. Interestingly, however, findings continually highlight the fact that it is lower quality vendors that are displaced by the entrance of these platforms, indicating a significant price sensitivity on the part of participants in these marketplaces (Greenwood and Wattal 2017, Zervas et al. 2017).

The third stream, which is most closely related to this research, examines societal impacts of the

sharing economy. Strikingly, despite significant concerns about the negative impacts of the sharing economy expressed by both academics (Horton 2015, Malhotra and Van Alstyne 2014) and the popular press (Baker 2014), recent research has expressed a more balanced view of the sharing economy, with numerous benefits and drawbacks emerging. For example, Greenwood and Wattal (2017) find that Uber entry is associated with a significant decline in drunk driving related fatality rate in California (a finding echoed by others at the national level (Martin-Buck 2017)). Further, scholars have highlighted the ability of platforms to ease traffic congestion (Li et al. 2016), minimize sexual assault (Dills and Mulholland 2016, Park et al. 2016), and generate non-trivial levels of consumer welfare in a dramatically more efficient fashion (Cohen et al. 2016, Cramer and Krueger 2016). Alarmingly, negative repercussions have also been observed, such as those in the form of gender and racial discrimination (Cui et al. 2016, Edelman et al. 2017, Ge et al. 2016, Rhue and Clark 2016). And, while research has yet to address whether or not such prejudices are stronger or weaker than the ones observed in the traditional economy, their presence is nonetheless troubling.

Interestingly, while this wide stream of research considering both consumption behavior and competitive dynamics has extensively documented the effects wrought on the demand side of the sharing economy, i.e. users of such services, research considering the supply side is considerably more sparse (with notable exceptions (Burtch et al. 2017, Chen and Sheldon 2015, Hall and Krueger 2016)). And within the body of work that does examine the supply side of the economy, focus has primarily been on the characteristics of likely participants (Hall and Krueger 2016), or how temporal micro-dynamics influence the marginal decision to participate more or participate less (Chen and Sheldon 2015), rather than what steps a decision maker might take in order to gain access to the sharing economy. This is particularly problematic, because, as discussed previously, it is not clear whether the suppliers are purely utilizing existing owners of durable goods in order to participate in those platforms, or making new investments in durable goods to exploit these platforms as professional workers (Malhotra and Van Alstyne 2014).

2.2 Sharing Platforms and Durable Goods Purchase
We examine how the presence of sharing platforms may affect durable goods purchase. While several analytical papers have provided theoretical predictions of either a positive or negative impact of sharing platforms on durable goods purchase (Abhishek et al. 2016, Benjaafar et al. 2015, Fraiberger and Sundararajan

2015, Horton and Zeckhauser 2016, Jiang and Tian 2016), empirical study remains conspicuously absent.

As discussed, the *a priori* relationship between the presence of sharing platforms and durable goods purchase is far from clear. Unlike traditional markets, sharing platforms are unique in that both the demand side and supply side are consumers of the durable good (Fraiberger and Sundararajan 2015, Horton and Zeckhauser 2016). The demand side consists of consumers who utilize these platforms as an alternative way to consume the product, i.e. "rental" (Abhishek et al. 2016, Fraiberger and Sundararajan 2015, Horton and Zeckhauser 2016) or "access-based consumption" (Bardhi and Eckhardt 2012). The supply side consists of owners of the product who may participate in the sharing economy in order to capture the excess rents offered by these platforms. This key feature of sharing platforms offers two opposing effects of sharing platforms on durable goods purchase. On the one hand, the presence of sharing platforms may lead to a *cannibalization effect* on product sales (Jiang and Tian 2016). On the other hand, the presence of sharing platforms may lead to a *value enhancement effect* (Abhishek et al. 2016, Jiang and Tian 2016), which may alter consumers' incentives to invest in assets and induce new ownership (Fraiberger and Sundararajan 2015).

2.3 Cannibalization Effect
Cannibalization effects occur when consumers who would buy a product no longer make such a purchase after an alternative product or service is introduced (Copulsky 1976, Mason and Milne 1994); in this case, the introduction of the sharing platform. For example, illegal sharing of information goods may cannibalize legal sales (Danaher et al. 2014, Oberholzer-Gee and Strumpf 2007); the introduction of online channels may cannibalize the sales of offline channels (Brynjolfsson and Smith 2000, Seamans and Zhu 2013); the secondary market of durable goods may cannibalize the sales of new products (Coase 1972, Ghose et al. 2006); and the availability of a rental option may cannibalize the sales of the purchase option (Knox and Eliashberg 2009). Sharing platforms, similarly, provide a low-cost "rental" option to consume the products, which may reduce the probability of purchase and ownership (Abhishek et al. 2016, Fraiberger and Sundararajan 2015, Horton and Zeckhauser 2016, Jiang and Tian 2016).

Why might such an effect manifest? Received research indicates that ridesharing platforms, e.g., Uber and Lyft, provide efficient and low-cost transportation options for consumers (Belk 2014, Cramer and

Krueger 2016, Greenwood and Wattal 2017).[3] As a result, it is plausible that some consumers may no longer need to subsidize the entire cost of vehicle ownership for at least three reasons.

First, there may be significant economic and social cost savings from exclusively leveraging ridesharing services. In addition to jettisoning the continual costs of upkeep, insurance, and parking/storage (Bardhi and Eckhardt 2012, Belk 2014, Sundararajan 2013), ridesharing services may be more attractive for consumers who do not want to curtail their social activities as a result of needing to drive, i.e. drinking (Rayle et al. 2014). Second, in addition to lower cost, such access-based sharing also provides a more transient mode of consumption than ownership, and offers convenience, flexibility and adaptability (Bardhi and Eckhardt 2012). Third, this access-based consumption allows consumers to experiment with different products and increase the diversity of goods consumed (Bardhi and Eckhardt 2012, Horton and Zeckhauser 2016, Sundararajan 2013), and may be perceived as a trendy and environmentally friendly consumption alternative to ownership (Belk 2014, Bardhi and Eckhardt 2012, Li et al 2016). Indeed, the popular press has identified several cases of people opting out of vehicle ownership as a result of ridesharing availability (Manjoo 2014). A recent survey by the Shared-Use Mobility Center also find that people who use more shared transportation modes, including bike sharing, car sharing (e.g., Zipcar), and ridesharing (e.g., Uber), are less likely to own a car and spend less on transportation (Murphy 2016).

2.4 Value Enhancement Effect

The presence of sharing platforms may also lead to a *value enhancement effect* on product ownership; wherein a change in the market may significantly increase the value of ownership to consumers (Abhishek et al. 2016, Jiang and Tian 2016). In the case of the sharing economy, such an effect may alter consumers' incentives to invest in assets and induce new ownership (Fraiberger and Sundararajan 2015), thereby leading to additional durable goods purchase (Chevalier and Goolsbee 2009). Prior literature has shown, for example, that the presence of a secondary market may enhance the value of ownership, as forward-looking consumers may adjust their valuation of the product by taking into account the product's resale value in the secondary market (Chevalier and Goolsbee 2009, Ghose et al. 2005, Miller 1974, Rust 1986). Sharing platforms could have a

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[3] Uber often markets itself as a low-cost alternative to car ownership—https://newsroom.uber.com/economics-of-car-ownership/

similar effect on ownership, because consumers may adjust their valuation of the product by taking into account the rents that can be captured on the platform. In turn, such a valuation may increase the probability of purchase and ownership (Abhishek et al. 2016, Fraiberger and Sundararajan 2015, Horton and Zeckhauser 2016, Jiang and Tian 2016). To the extent that ridesharing platforms imbue owners with the possibility of sharing excess capacity with others, all the while earning additional income (Hall and Krueger 2016) on a flexible schedule (Chen and Sheldon 2015, Hall and Krueger 2016), such an effect is not implausible.

Empirical evidence lends support for such a possibility. For example, a recent survey by Hall and Krueger (2016) found that eight percent of the Uber drivers in the United States were unemployed before they started working through Uber, and 20 percent of the Uber drivers have Uber as the only source of income. Moreover, most of the drivers who participated in contingent work through Uber choose the platform as an additional source of income (91 percent), for flexible work schedule (87 percent), and as a way to maintain stable income (74 percent). Burtch et al. (2017), who find that the entry of Uber has a negative effect on local entrepreneurship by providing stable employment opportunity for the un- and under-employed individuals, corroborate such conclusions. If this is the case, then it follows that participants may be willing to make capital investments in durable goods in order to access such markets.[4]

It should be noted, that these theoretical mechanisms are not mutually exclusive, inasmuch as some consumers may have their purchases cannibalized and others may seek value enhancement. However, taken together, the two opposing theoretical arguments introduce a natural tension. On the one hand, the presence of sharing platforms, as an alternative consumption mode to ownership, may decrease the sales of durable goods (i.e., the cannibalization effect). On the other hand, the presence of sharing platforms may enhance the value of ownership of durable goods, thereby increasing the sales of durable goods. In absence of a compelling *a priori* expectation, we empirically resolve this tension to identify the dominant effect.

3. Context and Data

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[4] The flexible work schedule provided by such platforms only serves to underscore such an effect, by smoothing pay variability during job transitions (Farrell and Greig 2016, Hall and Krueger 2016) and allowing workers to redeploy resources (Burtch et al. 2017), e.g., time, and experiment with other potential opportunities (Greve 2007, Kerr et al. 2014, Shah and Tripsas 2007). The surge pricing algorithm of the Uber platform, in particular, serves as a mechanism to incentivize drivers' supply of rides on the platform, resulting in drivers dynamically adjusting their hours and number of trips depending on the changes in price (Chen and Sheldon 2015).

3.1 Context

To examine the effect of the presence of sharing platforms on durable goods purchase, we leverage the entry

of Uber, an app-based ridesharing service, into different markets in China between 2010 and 2015. Founded

in 2009 in San Francisco, California, Uber is currently the largest ridesharing service in the world by both

valuation and footprint ($66 billion and servicing more than 545 cities / 66 countries as of 2016). Further, the

platform offers a wide selection of transportation options, ranging from ultra-premium luxury services such

as Uber Lux and Uber Chopper, to discount services such as Uber X.

In this study, we focus on the Chinese counterpart of the discount Uber X service, People's Uber.[5]

Until its purchase by DiDi Chuxing in August of 2016, People's Uber was Uber's low-cost ridesharing service

in China. To use this service, passengers can submit a trip request through Uber's smartphone application,

and Uber then assign drivers who use their own car to fulfill the request. People's Uber was first introduced

in Beijing in August 2014. By December 2015, People's Uber had entered 21 cities in China. Data on the

entry of People's Uber are collected from Uber's official blog and social media accounts (e.g., Weibo).

We focus on People's Uber for several reasons. First, as seen in Table 1, the introduction of People's

Uber in China is geographically and temporally dispersed, thereby allowing us to estimate the effect of Uber

entry using a difference-in-difference design. Intuitively, with such a design, we can leverage cities where

People's Uber has not yet entered, i.e. cities that have not yet received the treatment, as a control group.

Second, when Uber enters a new city, the company makes announcements on its official blog and social

media platforms. This creates the added benefit of cleanly identifying the entry timing for each city. It also

stands in stark contrast to other ride-hailing platforms, such as DiDi Chuxing[6] (formerly Didi Kuaidi from

the merger of Didi Dache and Kuaidi Dache), which do not consistently make formal announcements when

entering into new regions. Third, Uber was the first platform to offer peer-to-peer ridesharing services in

China, and was the dominant ridesharing service in China during the sample period. Again, this separates it

from other domestic platforms that mainly offered taxi-hailing services (viz. DiDi Chuxing), or black car

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[5] Consistent with prior studies (e.g., Burtch et al. 2017, Greenwood and Wattal 2017), we focus on discount services, as opposed to premium services (e.g. Uber Black), due to their significantly larger networks of drivers and significantly smaller barriers to entry. While People's Uber allows drivers to utilize a broader range of low-cost vehicles, other premium services require drivers to have a higher-cost vehicle.
[6] http://www.xiaojukeji.com/

services (e.g., DiDi Zhuanche as offered by DiDi Chuxing, ShenZhouZhuanChe, and Yidao Yongche),
during the sample.

Before discussing our data, we make note of three potential complicating factors. First, among the 21
cities that launched People's Uber by December 2015, three cities (Wuhan, Tianjin, and Nanjing) first
launched the service unofficially, during which the service was only available in limited areas of these cities
(Table 1). We discuss our empirical strategy for resolving this concern below. Second, in our sample, seven
cities imposed ownership lotteries or auctions to limit the number of passenger cars that can be registered
each year. Shanghai is the first city that implemented such a license auction, the policy becoming effective in
1994. After Shanghai, Beijing adopted a lottery mechanism by setting an annual quota of vehicle registrations.
Five other cities then followed (viz. Guiyang, Guangzhou, Tianjin, Hangzhou, and Shenzhen). Because these
annual quotas may influence the number of vehicles purchased annually, we exclude these cities from our
analysis. Finally, near the end of our sample (May 2015), DiDi Chuxing introduced a competing peer-to-peer
ridesharing service, DiDi Kuaiche. While People's Uber remained the dominant discount ridesharing service
(recall that DiDi focused exclusively on taxi's and black car services prior to this time), this is nevertheless
worth noting. Further, although the appearance of an additional ridesharing service does not undermine our
theoretical arguments regarding the effect of the sharing economy, since DiDi is subject to the same
theoretical mechanisms as People's Uber, we do control for the potential effect of the introduction of DiDi
Kuaiche on car purchase. To do so, we collected the Baidu Index of DiDi Chuxing (on which we elaborate
below) for each city during each month and included this as a control variable.

3.2 Data
To estimate the effect of the introduction of People's Uber on new car ownership, we construct a unique
longitudinal data set that contains new car registrations in China from 2010 to 2015. These data give us
information on the number of new passenger cars registered for personal use in each prefecture-level city[7] by
month. Passenger cars refer to "motor vehicles with at least four wheels, used for the transport of passengers,

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[7] In China, provinces (the first-level divisions) are divided into prefecture-level cities (the second-level divisions), which are similar to
the function of counties in the United States.

and comprising no more than eight seats in addition to the driver's seat."[8] Our data only include domestic

passenger cars which are manufactured in China (regardless of where the headquarters of the manufacturer

are located). For example, BMW sedans or SUVs manufactured in China (e.g., the 3 series or X3) are

considered as domestic passenger cars. Imported cars, i.e. cars manufactured outside China, are not included.

Empirically, this is of little concern. In the six-year observation window of our study, newly imported

passenger cars constituted less than 7.5% of the passenger car market, and were mostly comprised of super

high-end luxury vehicles (e.g., Aston Martin, Bentley, Ferrari, Porsche, etc.)(Wagner et al. 2009). In total, our

sample includes monthly data on 14 cities over 6 years.

3.3 Variable Definitions

*3.3.1 Dependent Variable - T*he dependent variable, *ln(NumCars)$_{it}$*, is the natural log of the number of new

vehicles registered in city i during month t.[9] The log transformation allows us to interpret the effect as a

percentage change, and resolves the issue of right-skewed distribution.

*3.3.2 Independent Variables –*Because People's Uber rolls out in a two-phased manner, i.e. the unofficial and

then official launch, we leverage two dichotomous indicators as our primary independent variables of interest

in our initial analysis. The first variable, *UnofficialUberEntry$_{it}$*, indicates the soft entry of the People's Uber

ridesharing service in city i in month t. The second variable, *OfficialUberEntry$_{it}$*, indicates the official entry of

the People's Uber ridesharing service in city i in month t. It should be noted that *UnofficialUberEntry* always

occurs before *OfficialUberEntry* (if at all). A list of treated cities and entry is available in Table 1.[10]

3.3.3 Controls - In addition to the above variables, we include a robust set of controls. First, we obtain annual

city-level demographic data from China City Statistical Yearbook, which has been widely used in prior

research (Glaeser et al. 2017, Jia 2014, Wu et al. 2016, Zheng and Kahn 2017). In particular, we include three

types of demographic data, i.e., economic characteristics, mobile and internet access, and transportation and

traffic related characteristics. For economic characteristics, we include the log-transformed population, the

number of employed workers, the number of registered unemployed citizens, average wage of employed

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[8] http://oica.net/wp-content/uploads/stats-definition1.pdf.
[9] A value of one is added before the log transformation.
[10] We manually collected the entry dates from multiple sources, including Uber Newsroom, the official Weibo accounts of each local Uber office, and news articles. Weibo is a Chinese microblog platform similar to Twitter.

workers, and gross region production (GRP) per capita. Second, because Uber is a mobile-based platform that requires mobile and internet access, we include the log-transformed number of mobile subscribers and internet subscribers. Third, we control for factors related to other transportation methods and traffic conditions, including the log transformed highway passenger traffic, the number of buses, bus passenger volume, the number of taxis, road area per capita, and metro length.

Next, we collected data from the Baidu Index, which is a search volume index provided by Baidu, the largest Chinese search engine. Conceptually, this is the Chinese equivalent of the US-based Google Trends data that has been widely used in prior literature as a proxy for demand (Ghose 2009, Li et al. 2016, Wallsten 2015). We include these controls to measure the search volume of search terms related to other domestic ride-hailing platforms in China.[11] The Baidu Index data were collected for each search term in each prefecture-level city for each month. It well reflects the popularity of a platform after it entered into each local market. Specifically, *ln(Didi)* is the natural log of the Baidu index of search terms related to Didi Chuxing (including Didi Chuxing, Didi Dache, Kuaidi Dache, and Didi Kuaiche); *ln(Shenzhou)* is the natural log of the Baidu index of the search term ShenZhouZhuanChe; and *ln(Yidao)* is the natural log of the Baidu index of the search term YiDao YongChe. [12] Finally, we apply time and location fixed effects. Summary statistics of the variables are in Table 2 and a correlation matrix of the variables is presented in Appendix A.

4. Empirical Strategy and Results
4.1 Difference-in-Difference Model
To estimate the effect of Uber entry on durable goods purchase in the form of new car ownership, we use a difference-in-difference (DID) model (Angrist and Pischke 2008, Bertrand et al. 2004). The DID model offers us several advantages, chief among them being that we can exploit the fact that People's Uber entered different locations at different times. In doing so, we leverage cities where Uber has not yet entered as the control group for cities where Uber is currently active. In particular, we estimate the DID model using only cities that eventually introduced Uber, thereby dramatically increasing the homogeneity of the sample on both observables and unobservables (Bapna et al. 2015). Our unit of analysis is the city-month. We model

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11 All search terms are in Chinese.
12 Note that Lyft, Sidecar, and other dominant US competitors to Uber did not operate in China during the period of the sample.

ln(NumCars)$_{it}$, the log-transformed number of new domestic passenger cars registered for personal use in city *i* during month *t*, using the following specification:

$$ln(NumCars)_{it} = a_i + \gamma_t + \beta_1 UnofficialUberEntry_{it} + \beta_2 OfficialUberEntry_{it} + \varepsilon_{it}, \tag{1}$$

where a_i represents city fixed effect, and γ_t represents month fixed effect. As noted earlier, *UnofficialUberEntry*$_{it}$ is a dummy indicating whether Uber had unofficially launched People's Uber in city *i* by month *t* (1 for yes, and 0 for no). *OfficialUberEntry*$_{it}$ is an dummy indicating whether Uber had officially announced the launch of People's Uber in city *i* by month *t* (1 for yes, and 0 for no). The specification above allows us to estimate the effect of unofficial entry (as captured by β_1) and the effect of official entry (as captured by β_2) separately. To reduce concerns about heteroscedasticity and within-city serial correlation, we use robust standard errors clustered at the city level. Results are reported in Column 1 of Table 3.

As can be seen, the official introduction of People's Uber into is significantly positively correlated with the number of new car registrations ($\beta_2 = 0.217$, $p < 0.05$), suggesting that the introduction of People's Uber is associated with a 22% increase in the number of new car registrations. This indicates that the prospect of value enhancement effect (i.e., flexible work schedule or earning additional income) through sharing excess capacity encourages consumers to own a car. With limited observations that experienced unofficial entry of People's Uber, we find no significant effect of unofficial Uber entry on the number of new car registrations ($\beta_1 = 0.180$, $p > 0.1$).

One potential issue with Equation (1) is that it does not account for idiosyncratic time trends in each city. To address this issue, we next include city-specific linear time trends by estimating the following equation:

$$ln(NumCars)_{it} = a_i + \gamma_t + \beta_1 UnofficialUberEntry_{it} + \beta_2 OfficialUberEntry_{it} + \theta_i t + \varepsilon_{it}, \tag{2}$$

where θ_i captures the linear time trend of city *i*. That is, we allow each city to have a different time trend, even after controlling for time invariant city specific heterogeneity (through city fixed effects) as well as general trends that are city invariant (through month fixed effects). Results are in Column 2 of Table 3.

The coefficient from Column 2 of Table 3 suggests that the entry of People's Uber is associated with

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an average of 8% increase in car sales in China, which is smaller than the effect estimated based Equation (1). Again, the positive coefficient provides evidence of the value enhancement effect that increases consumers propensity to own a car. Furthermore, anecdotal accounts lend credence the size of such an estimate. Almost 100,000 drivers and 800,000 passengers registered for the service within the first month of its availability in Hefei, a city with a population of seven million.[13]

4.2 Relative Time Model

One critical assumption required for the validity of the DID estimation is that the pre-treatment trends of the dependent variable, i.e. changes in the rate of new vehicle registration, must be parallel prior to treatment, conditional upon controls (Angrist and Pischke 2008, Bertrand et al. 2004). Practically, this means that there should be no difference in pre-treatment trends in the dependent variable across cities prior to Uber entry once controls are applied. To validate this assumption, we execute the leads and lags model proposed by Autor (2003), i.e. a relative time model, which has been widely used in extant literature (Bapna et al. 2015, Burtch et al. 2017, Carnahan 2013). The model is implemented by including a series of dummy variables that indicate the relative temporal distance (in three-month intervals) between an observation period, t, and the timing of Uber entry in city i, in addition to the absolute month fixed effects. Conceptually, the relative time model allows us not only to test whether there is a difference in pre-treatment trends, but also to estimate treatment effects that may vary offer time. We specify the relative time model as follows:

$$ln(NumCars)_{it} = a_i + \gamma_t + \sum_j \tau_j PreUberEntry_{it}(j) + \delta UnofficialUberEntry_{it}$$
$$+ \varphi OfficialUberMonth_{it} + \sum_k \omega_k PostUberEntry_{it}(k) + \varepsilon_{it}$$

(3)

Again, $ln(NumCars)_{it}$ is the log-transformed number of domestic passenger cars registered for personal use in city i during month t, a_i represents city fixed effects, and γ_t represents month fixed effects. $PreUberEntry_{it}(j)$ is an indicator equal to one if the temporal distance between Uber's initial entry into city i and month t is j (in three-month intervals to increase interpretability (Burtch et al. 2017)). Results are identical using month dummies and are available upon request. For cities that experienced unofficial Uber entry, the initial Uber

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entry time refers to the unofficial Uber entry time. For other cities, the initial Uber entry time refers to the official Uber entry. Recall, *UnofficialUberEntry*$_{it}$ is an indicator of whether Uber has unofficially launched People's Uber in city i by month t. *OfficialUberMonth*$_{it}$ is an indicator of whether Uber officially entered city i in month t. *PostUberEntry*$_{it}$*(k)* is an indicator that equals one if the temporal distance between Uber's official entry into city i and month t is k (again, in three-month intervals). Consistent with prior work, we use the period prior to the first Uber entry as the baseline by normalizing the coefficient of *PreUberEntry*$_{it}$*(-1)* to zero (Burtch et al. 2017).[14] To account for potential correlation in the standard errors within city, we cluster standard errors at the city level (Cameron and Miller 2015).

Intuitively, this model allows us to test whether there is a difference in pre-treatment trend across cities. Specifically, the vector of coefficients τ_j captures the differences in those pre-treatment trends. If there is a selection bias based on pre-treatment car sales, the estimates of the τ_j parameters will be significantly different from zero. Moreover, the relative time model is more flexible than the DID, because it estimates both the short term and long term effects of Uber entry semi-parametrically. That is, the dummy variables for the temporal distance relative to Uber entry impose no parametric assumption on the time trend of the effect and allow for nonlinear effects on car purchases. Specifically, the vector of coefficients w_k captures the potential difference in post-treatment trends. In contrast, the DID model is more restrictive because it imposes the assumption that the effect is constant over time.

Results are reported in Table 4. Column 1 reports results without controlling for city-specific time trend. As we can see, if city-specific time trend is not controlled, there exists a statistically significant difference in pre-treatment trends. In particular, the coefficients of the pre-treatment dummies seem to suggest an increasing time trend in the number of new car registrations even in the absence of Uber. To further control for city-specific time trend, we estimate the following model:

$$
\begin{aligned}
ln(NumCars)_{it} = a_i + \gamma_t + &\sum_j \tau_j PreUberEntry_{it}(j) + \delta UnofficialUberEntry_{it} \\
&+ \varphi OfficialUberMonth_{it} + \sum_k \omega_k PostUberEntry_{it}(k) + \theta_i t + \varepsilon_{it}
\end{aligned}
\tag{4}
$$

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14 Choosing a different baseline yields identical conclusions.

Similar to Equation (2), θ_i in Equation (4) captures the city-specific time trend of city i. Results are presented in Column 2 of Table 4. We first note that none of the pre-treatment relative time dummies (i.e., *PreUberEntry$_{it}$(j)*) is significant, suggesting that, once we control for city-specific trend, we fail to find a statistically significant difference in pre-treatment trends across cities that introduced Uber. Second, we see a significant increase (10%) in the number of car registrations immediately after the official entry of People's Uber, and the positive effect is even stronger over time, from 10% in the first three months (*PostUberEntry$_{it}$(1)*) to 28% after six months (*PostUberEntry$_{it}$(3)*).[15] Again, these results provide compelling evidence that the presence of Uber has increased consumers' propensity to purchase new cars in order to capture the excess rents offered by these platforms (i.e., value enhancement).

5. Robustness Checks
Next, we consider an extensive set of tests to establish the robustness of the main result. This includes the inclusion of additional controls, the consideration of alternative estimators, placebo models, random shuffle tests, inference based on a wild cluster bootstrap procedure, and additional tests using cities with vehicle purchase restrictions.

5.1 Additional Controls
While the relative time model controls for time invariant heterogeneity across cities (through city fixed effects), city invariant changes over time (through month fixed effects), and unobserved city-specific linear time trends, it is possible that there may be other time-varying, city-specific factors that are correlated with the entry decisions made by Uber managers. To alleviate this concern, we next replicate our baseline analyses using a series of additional controls. Importantly, in each of these analyses we include city level linear trends in order to obviate the parallel trend concerns observed in Column 1 of Table 4. To control for additional demographic factors that may be correlated with Uber entry, we leverage data from Chinese City Statistical Yearbook. As population and economic factors may affect Uber entry, we include the log-transformed population (to control for the size of the car market), the number of employed workers and registered

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[15] It is worth noting that the lack of significance for the coefficients of *PostUberEntry(4)* and *PostUberEntry(5)* may be due to the insufficient number of observations (i.e. such a test is underpowered). In our sample, only six observations have *PostUberEntry(4)* equal one, and only three observations have *PostUberEntry(5)* equal to one. Therefore, we cautiously interpret the lack of significance as a result of lack of power.

unemployed citizens (to control for employment status), average wage of employed workers, and gross region production (GRP) per capita (to control for local economic growth). Second, access to mobile and internet technologies may also affect Uber entry, because Uber is a smartphone-based service that requires mobile and internet access. We thus include the log-transformed number of mobile subscribers and internet subscribers. Third, we control for factors related to other transportation methods and traffic conditions that may affect Uber's entry decision. Specifically, we included the log transformed highway passenger traffic, number of buses, bus passenger volume, number of taxis, road area per capita, and metro length.

Finally, it is reasonable to be concerned with the presence of other ride-hailing services. Insofar as it is difficult to identify the entry timing of other services into each city, we collect data on the search volumes of these services from Baidu, the largest search engine in China, to control for the popularity of these services. Specifically, we included the log of the Baidu index of search terms related to DiDi Chuxing, the search term ShenZhouZhuanChe, and the search term YiDao YongChe.

We then re-estimate the relative time model by including different combinations of demographic variables and Baidu indexes as controls. Results are in Table 5. After including additional controls, results remain consistent. That is, we find a statistically significant effect of the post-entry dummies, suggesting that vehicle purchases rise significantly post Uber entry, with little evidence of a pre-treatment effect. Again, we see that a significant increase in the number of car registrations immediately after the official entry of People's Uber, and the estimated effect manifests over time, from 10% in the first three months ($PostUberEntry_{it}(1)$) to 28% after six months ($PostUberEntry_{it}(3)$). The fact that the estimates remain similar after the inclusion of different control variables suggest the robustness of the main results.

5.2 Count Model

In our main analysis, we have thus far used the log-transformed number of cars as the dependent variable. As the count variable, i.e. the number of cars registered, is highly skewed, the log transformation allows us to make the distribution closer to a normal distribution so as to estimate the linear regression models using an ordinary least squares (OLS) estimator. Moreover, using the log number of cars as the dependent variable allows us to estimate the coefficient of an independent variable as a percentage change in the number of cars

when the independent variable increases by one unit.

In light of concerns raised by O'Hara and Kotze (2010), Silva and Tenreyro (2006), Silva and Tenreyro (2011), we next replicate the estimations using a traditional count estimator, as opposed to a log-transformed dependent variable with an OLS. We re-estimate the relative time model using the number of cars (*NumCars*) as the dependent variable, both with and without control variables, using a Poisson pseudo-maximum likelihood (PPML) estimator (Simcoe 2007). Such an approach has been widely used in recently studies with count data (Burtch et al. 2017, Burtch et al. 2014, Greenwood and Wattal 2017), and offers several benefits over other estimators such as a fixed effect negative binomial (Allison and Christakis 2006). The use of PPML also allows us to estimate clustered standard errors, even in the presence of over-dispersion (Wooldridge 1997). Results with and without controls are presented in Columns 1 and 2 of Table 6, respectively, and remain consistent. Again, we do not find evidence of significant differences in pre-treatment trends, which support the parallel trends assumption. Further, we see that the entry of People's Uber significantly increases the number of new cars registered. Economically, estimates suggest that the number of new cars registrations increases by 14%-22% after Uber entry.

5.3 Placebo Test Using Data on Prior Years

While results thus far have shown remarkable consistency, it remains plausible that the results are an artifact of an idiosyncrasy associated with cities in China that receive the Uber treatment, as opposed a result of Uber entry. We therefore next execute a placebo test using data on prior years, wherein we attempt to calculate the probability of such an effect manifesting purely by chance. Further, such a test allows us not only to check whether the observed effect is driven by chance, but provides a reasonable check against outliers.

To run the placebo test, we re-estimated the DID and relative time models by regressing the log-transformed number of cars purchases of month *t* on placebo treatments, i.e., the treatment variables of month *t+24* (i.e. 2 years later). Ideally, the placebo treatments should have no effect on the outcome variable. The results of the DID model are presented in Table 7, and the results of the relative time model are presented in Table 8. As we can see, using both DID and relative time models, the estimated coefficients of the placebo treatment variables are statistically insignificant, suggesting that the observed real effect of Uber entry) is unlikely to be driven by random chance or outliers.

5.4 Random (Shuffled) Treatment Test

In addition to the placebo test using data on prior years, we further implemented a random treatment test to examine the possibility of false significance due to serial correlation in the dependent variable or spurious effects (Bertrand et al. 2004). Similar to Greenwood and Agarwal (2016) and Burtch et al. (2017), we first randomly shuffle the treatment variables (i.e. *UnofficialUberEntry* and *OfficialUberEntry*), then run the DID model in Equation (2) using the shuffled treatment variables and store the estimates. This procedure is replicated 1,000 times with different set of random treatments.

(a) Distribution of pseudo effects of
random unofficial Uber entry

(b) Distribution of pseudo effects of
random official Uber entry
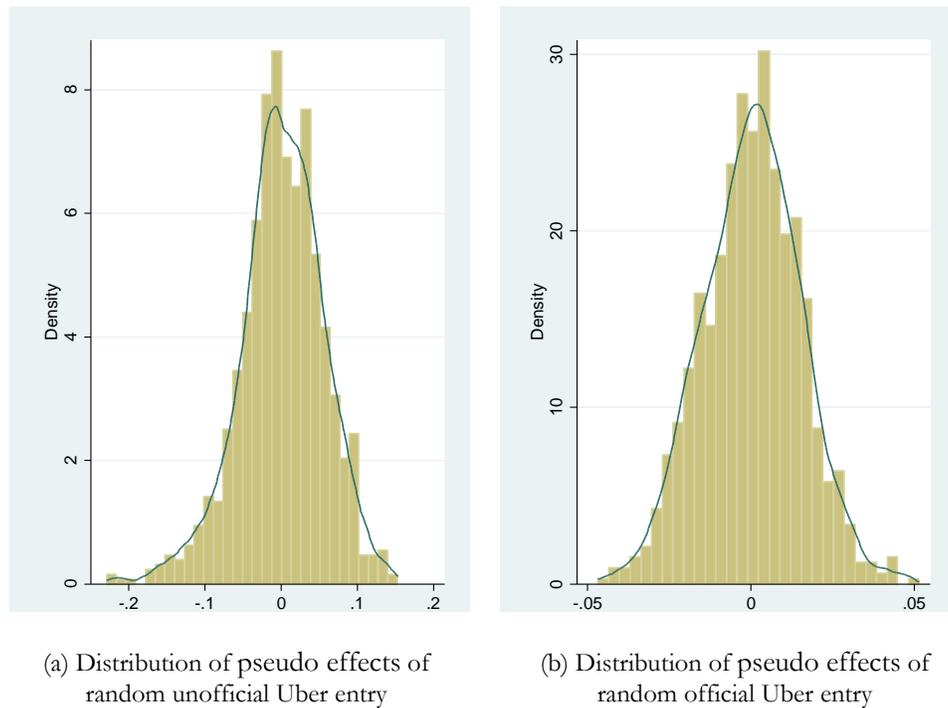
Figure 1 Distribution of Coefficients based on Random Treatments

In Figure 1, we present the distributions of the DID estimates of both *UnofficialUberEntry* and *OfficialUberEntry* based on the 1,000 replications. First, as expected, the distributions of the pseudo effects are centered around zero. Second, a t-test fails to reject the null hypothesis that the average estimated effect based on random treatments is statistically different from zero (t-test for the pseudo effects of *UnofficialUberEntry*: p-value=0.701; t-test for the pseudo effects *OfficialUberEntry*: p-value=0.536). Third, comparing the distribution of the pseudo effects of random official Uber entry, we see that the real DID estimate of official Uber entry is at the right tail of the distribution (higher than the 99% percentile),

suggesting that the probability of obtaining a similar estimate by random chance is extremely low (p-value<0.001).

5.5 Wild Cluster Bootstrap Based Inference

In our main analyses, we cluster robust standard errors by city to allow for serial correlation and ensure correct inference. However, when there are only a small number of clusters, the cluster robust standard error may generate p-values that are too small, which may result in over-rejection (Bertrand et al. 2004). As suggested by Cameron et al. (2008) and Cameron and Miller (2015), a common correction for this problem is to use a wild cluster bootstrap procedure, which allows for serial correlation when there are only a few clusters. In Appendix B, we report the p-values obtained from the wild cluster bootstrap procedure. Specifically, Table B1 presents the wild cluster bootstrap based p-values of the DID model in addition to the coefficients and cluster robust standard errors that are reported in Table 3, and Table B2 presents the wild cluster bootstrap based p-values of the relative time model in addition to the coefficients and cluster robust standard errors that are reported in Table 4. As can be seen, the results remain consistent, suggesting that our inference is not affected by the issue of few clusters.

5.6 Additional Test Using Cities with Vehicle Purchase Quota Restriction

As noted earlier, seven cities that imposed quotas on the number of passenger car registrations issued per year during our sample period are excluded from our main analysis. Among the seven cities, six cities[16] introduced the restriction policy prior to the entry of People's Uber. To the extent that such restrictions should preclude a concomitant rise in vehicle purchases, the existence of such quotas offers us a powerful falsification test (in as much as a significant increase in vehicle purchases post Uber entry would call our results into question).

We therefore repeat our analysis using these six cities, with the inclusion of $PostPurchaseRestriction_{it}$, i.e., a dummy indicating whether a vehicle purchase restriction policy is effective in city i by month t. Ideally, after we control for the implementation of vehicle purchase restriction, the entry of People's Uber should have no impact on the number of car registrations in these cities. The results of the DID model are presented in Table 9. Column 1 presents the results of the simple DID model (similar to Equation 2) without controls.

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[16] The only exception is Shenzhen, where People's Uber was introduced prior to the implementation of the vehicle purchase restriction.

Column 2 includes additional controls. Because the introduction of vehicle purchase restriction may change the time trend within each city, we allow each city to have different time trends before and after the implementation of vehicle purchase restriction in Columns 3 and 4. Consistent with our expectation, results indicate across each of the specifications that Uber does not have a significant impact on the number of cars registered in the presence of an annual purchase quota. In addition, we find a significant decline in vehicle registrations following the implementation of annual purchase quota.

6. Empirical Extensions

Our analyses thus far have consistently shown a positive average effect of official Uber entry on new vehicle registrations across multiple model specifications and estimations, and results are robust to a stringent series of falsification checks. We next consider heterogeneity in the observed effect, to better identify the underlying mechanism driving the realized change.

6.1 Effect by Vehicle Type

Thus far, results consistently suggest that a value enhancement effect is dominating any cannibalization effect. However, some vehicles may not be as cost efficient as others when being used by drivers to garner rents. Intuitively, if the value enhancement effect is valid, we expect the effect may be stronger for smaller cars than larger cars. To the degree that larger and heavier vehicles, which often have larger engine displacement, possess significantly worse fuel economy, it stands to reason that rent-seeking individuals on the platform would be more likely to avoid these vehicles. Moreover, to the extent that individuals driving on the platform are often capital constrained (Burtch et al. 2017), and inasmuch as extent literature suggests that vehicles with larger engines are more likely to be purchased by wealthier individuals (Wagner et al. 2009), it follows that potential drivers would be more likely to purchase smaller, more fuel-efficient vehicles.

We therefore decompose our data along two dimensions: engine size and vehicle size. Regarding engine size, we decompose car registrations by engine displacement into four displacement tiers: 1-1.5 L, 1.5-2 L, 2-2.5 L, and above 2.5 L. If our proposed mechanism, i.e. a value enhancement effect, is correct, a stronger effect should manifest with vehicles with smaller engines. Results are reported in Table 10 (without controls) and Table 11 (with controls). Consistent with our expectation, results suggest that the effect of Uber entry is positive and significant for vehicles with engine displacement below 2 L, but not significant for those

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above 2 L. Intuitively, this corroborates our intuition that vehicles with larger engines may not be cost efficient means to garner rents from the platform.

To proxy our effect based on vehicle size, we replicate the relative time model by running separate regressions for vans and non-van vehicles, as vans represent significantly larger and heavier vehicles than standard passenger vehicles (and with inferior fuel economy as a result). Results are reported in Table 12. Columns 1 and 2 present results without controls, and Columns 3 and 4 present results with controls. As we can see in Columns 1 and 3, the effect of Uber entry on van sales is only significant during the month of official entry, and disappears once controls are included. Meanwhile, for non-vans (Columns 2 and 4), estimates indicate that the effect of Uber entry is consistently positive and significant, in both the presence and absence of controls. In sum, these results support the explanation that the presence of Uber as a sharing platform enhances the value of owning a car by offering owners the option to share excess capacity with others to earn additional income, which incentivizes individuals to actively invest in personal cars that are more likely to be operated in order to participate in the Uber platform as drivers.

6.2 Effect by Gender

Earlier results suggest that Uber entry positively affected the number of new car registrations, which indicates that the presence of Uber generated a value enhancement effect on car ownership. It is interesting to consider, however, whether or not men and women vary in the degree to which they exploit the platform to accumulate rents. On the one hand, it is possible that the introduction of Uber may have a stronger effect on women than men, as prior literature has documented that women are more likely than men to engage in an alternative work arrangements (including temporary help agency workers, on-call workers, contract workers, and independent contractors/freelancers, as defined by the Bureau of Labor Statistics[17]) (Katz and Krueger 2016). Alternative employment arrangements are different from permanent employment arrangements and perceived as a better employment option for individuals with family responsibilities (Albert and Bradley 1997). In particular, women prefer job opportunities, on the margin, with flexible schedule, variety, and freedom (Ellingson et al. 1998), and they may be forced to work around family demands (Loscocco 1997) or

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[17] https://www.bls.gov/news.release/conemp.nr0.htm

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to balance the demands between work and family roles (Frone et al. 1992). Working on the platform is one such alternative work arrangements, and provides benefits such as flexible work schedules and the potential to work shorter hours (Chen and Sheldon 2015, Hall and Krueger 2016).

On the other hand, it is possible that the introduction of Uber may have a weaker effect on women than men, as women driving a vehicle professionally is often seen as a violation of workplace gender roles (Eagly 2013). One recent survey, for example, indicates that only 9% of the drivers for ridesharing services are women.[18] Another survey based on Uber drivers in the U.S. also finds that the percentage of female Uber drivers (14%) is much lower than the percentage of female workers in the workforce (47%) (Hall and Krueger 2016). As pointed out by Nick Allen, the confounder and former CEO of SideCar, "this economic opportunity has excluded women–not purposefully, but women have self-selected out of it," with one of the main reasons being "the perception of safety or lack thereof."[19] As women may be less likely to drive professionally, it may be that women's car purchase decisions are less likely to be affected by Uber entry.

To estimate the differential impact of Uber entry on women versus men, we decompose our data by gender, and replicate the relative time model by running separate regressions for each gender. The results are reported in Table 14. Columns 1 and 2 present results without controls, and Columns 3 and 4 present results with controls. As we can see in Columns 1 and 3, the effect of Uber entry on the number of cars purchased by female are positive significant, both with and without including additional control variables. Meanwhile, the results in Columns 2 and 4 suggest that the effect of Uber entry on the number of cars purchased by male are also consistently positive and significant. However, when comparing Column 1 with Column 2, and Column 3 with Column 4, we find that the magnitude of the effect is larger for male than female.[20] Two key takeaways stem from these results. First, they bolster our claim that ridesharing services are driving the effect. To the degree that men, overwhelmingly, make up the population of drivers, the finding that women make up the lion's share of additional purchases would significantly undercut the credibility of our results. Second,

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[18] http://yuqing.people.com.cn/n/2015/1020/c212888-27719709.html

[19] http://www.forbes.com/sites/ellenhuet/2015/04/09/female-uber-lyft-drivers/#2751ef625298

[20] Results from a Wald Chi-square test after a seemingly unrelated regression indicates that differences in the coefficients across gender are statistically significant.

these results are encouraging as a signal of decreasing levels of gender inequality in a traditionally male-dominated field. Inasmuch as women are still impacted in a non-trivial manner, our findings underscore recent claims in the popular press that the gig-economy helps to ameliorate traditional gender biases (Galluzzo 2016, Wright 2017), even if it does not resolve them entirely.

6.3 Effect by Age

Next, we investigate the heterogeneous effect of Uber entry, based on buyer age. If the introduction of Uber positively affects a consumer's propensity of purchasing cars by providing them a means to earn additional income or access flexible employment, we would expect that the effect is stronger for certain age groups that are in higher need of such employment opportunities. Prior literature on income volatility has documented a U-shaped relationship between income volatility and age; suggesting higher income volatility among young adults and elderly compared to middle-aged adults (Clark and Summers 1981, Gundersen and Ziliak 2008, Jaimovich and Siu 2009, Ríos-Rull 1996). The reasons for such a relationship are multifaceted, including an unstable attachment to the labor force (Topel and Ward 1992), a higher propensity to engage in hourly (as opposed to salaried) work, and more frequent transitions between jobs (Gundersen and Ziliak 2008). Conversely, middle-aged adults tend to have more stable employment.

We therefore re-estimate the relative time model on five different age groups: individuals aged 18-24 years, 25-34, 35-44, 45-54, and above 55 years of age. Results are reported in Table 14 (without controls) and Table 15 (with controls). Consistent with our expectation, results suggest a U-shaped pattern between the estimated effects and age. That is, middle-aged adults (35-44, as shown in Column 3) are less likely to affected by the introduction of Uber that young adults and people above the age of 45, which support the idea that the presence of Uber has a higher impact on those who are more likely to participate in the Uber platform as drivers in order to capture the employment benefits provided by the platform. Interestingly, much like the effect for women, this finding also suggests that the gig-economy is opening up labor opportunities for traditionally marginalized workers, i.e. younger and older workers who are less likely to be capturing stable rents from the market (Clark and Summers 1981, Gundersen and Ziliak 2008, Jaimovich and Siu 2009, Ríos-Rull 1996). To the degree that these results suggest the gig-economy may be incentivizing additional labor force participation, our results corroborate the findings of Burtch et al. (2017), who argue that the gig-

economy often offers viable employment for the un- and under-employed.

7. Discussion and Conclusion

Internet-based sharing platforms have emerged as a means of product sharing among owners with excess capacity and consumers with temporary needs. In this study, we examine the effect of sharing platforms on durable goods purchase. As such platforms imbue the owners of such goods both with the possibility of extracting rents from their durable goods, as well as the possibility of consuming goods at a discounted rate, the effect on purchase is far from clear. On the one hand, the presence of sharing platforms may cannibalize durable goods purchase by providing consumers an alternative way of temporarily consuming the product. Rather than owning the product, the consumers can now consume the product through the sharing platform, thus reducing their likelihood of purchasing the product. On the other hand, these platforms may enhance the value of ownership by providing owners of durable goods a way to share excess capacity and an employment alternative, thus increasing their likelihood of purchasing the product to capture the benefits provided by these platforms. Using the context of Uber's entry in China, we utilize a different-in-difference approach to analyze how People's Uber, a peer-to-peer ridesharing service, affects new vehicle purchase. Results suggest that the entry of People's Uber has a significant and positive effect on new car purchases. Intuitively, this indicates that consumers are strategically changing their stock of held resources in order to capture the excess rents offered by these platforms. Interestingly, results further suggest that while there is a stronger effect for male owners than female owners, the effect on female purchasing is non-trivial. Moreover, findings indicate that there are stronger effects for those below 35 or above 55, as compared with people who are middle-aged. Each of these empirical extensions suggest that gig-economy platforms like Uber are facilitating labor market participation among traditionally marginalized groups.

Our study makes the following contributions. First, it contributes to the emerging literature on sharing economy by empirically examining how the presence of sharing platforms affects durable goods market. While recent theoretical work has provided competing views on how sharing platforms may affect the ownership of durable goods (Abhishek et al. 2016, Jiang and Tian 2016), this paper is the first to provide empirical evidence of the impact on ownership. This finding provides evidence of a value enhancement effect on product ownership, suggesting that individuals on the supply side may actively change their stock of the

product to participate on such platforms instead of using existing, at rest resources. Our work also contributes to the stream of literature on the societal impacts of the sharing economy by showing that the introduction of these sharing platforms creates positive externality to local markets by stimulating durable goods purchase and offering employment opportunity to traditionally marginalized groups.

Second, this study provides implications for both manufacturers and sharing platforms. Despite concerns that sharing platforms might cannibalize sales, our results suggest the opposite effect dominates, and sales rise significantly. Interestingly, results suggest manufacturers may benefit from the presence of such platforms, in that these platforms may enhance to value of product ownership and stimulate purchases. In particular, results regarding the heterogeneous effects across vehicle types suggest that manufacturers who offer products that are more likely to be utilized (e.g., smaller cars with higher fuel economy) are more likely to benefit from the emergence of these platforms. Manufacturers, therefore, should embrace the opportunities offered by the emergence of the sharing economy by targeting consumers who are more likely to participate in the sharing economy as suppliers, and advertising vehicle models that are more likely to be utilized by them. Sharing platforms may also consider collaborating with manufacturers to further incentivize consumers to purchase durable goods in order to participate on these platforms.

Finally, our study yields notable policy implications. Despite the current policy debate regarding the legality issue of sharing platforms such as Uber and Airbnb and concerns about the potential negative impacts of these platforms, our finding suggests that the sharing economy may create positive societal benefits to local markets by stimulating durable goods purchase and providing opportunities for owners of durable goods to earn additional income and have flexible work schedule. And, while our study considers only a portion of the effect the sharing economy may have on overall public welfare, considering these benefits in the presence of other observed benefits, e.g. higher efficiency, productivity, and public safety, as suggested by prior research, indicates that these effects may be substantial. As a result, policy makers may take into account these positive effects on sharing platforms when designing policies and regulations.

It should also be noted, however, that such active investments on the part of platform participants in order to capture rents does fly in the face of claims made by platforms that individuals are simply exploiting

at rest resources. And this creates certain regulatory challenges, e.g., how to ensure that the market is competitive for both incumbent, as well as emergent, players. While some innovations of the ridesharing market could easily be enacted by industry incumbents (e.g. digital solicitation and billing), it is worth noting that these markets are still heavily regulated compared to their upstart counterparts. As a result, policy makers should certainly consider reducing the degree of regulation to which taxi and licensed livery services (e.g. caps on medallions, onerous insurance minimums, etc.), in order to ensure fair market competition.

Our study is subject to several limitations. First, our empirical analysis focuses on People's Uber, a ridesharing service introduced by Uber in China, due to data availability. While China is a large market with a population of 1.37 billion and 17 million new car registrations in 2015, further to ensure that these results generalize to other markets will be critical. Relatedly, because sharing economy is a recent phenomenon, we are unable to examine the long-term impact on durable goods purchase. It is worth noting that, while People's Uber remained the dominant peer-to-peer ridesharing service during our sample period, DiDi Chuxing introduced a competing peer-to-peer ridesharing service, DiDi Kuaiche, near the end of our sample. While DiDi is subject to the same theoretical mechanisms as People's Uber, we do attempt to control for the potential effect of the introduction of DiDi Kuaiche on car purchase by using search volume data to proxy for the popularity of DiDi. Nevertheless, future work may examine the effect of DiDi to ensure the robustness of the results when data are available. Second, while Uber is a dominant ridesharing platform in the ridesharing market, future research may focus on a different type of sharing platforms (such as Airbnb) to examine whether and how the presence of those platforms may affect the durable goods market (such as the housing market). Third, our identification of the effect of Uber entry is based on the variation in timing of Uber entry across different locations using archival data. Although we have included city fixed effects (to control for city-invariant heterogeneity), time fixed effects (to control for seasonality), city-specific time trends (to control for city-specific sales trends), and an extensive set of controls, future research may consider other potential factors that may affect car sales to increase the efficiency of estimation. Fourth, the coefficient estimates we obtained from the empirical analysis capture the net effect of the opposing mechanisms, and the sign of the coefficient estimates helps us identify the dominant mechanism. Nevertheless, it remains possible

that the introduction of sharing platforms may cannibalize the sales of durable goods under some conditions.

Finally, it is important to note that this research addresses only part of the overall effect of the entry of

ridesharing services. It would be inappropriate, therefore, to draw any conclusions about the totality of the

public welfare loss or gain which emerges from these platforms. Instead, we hope that the sharing economy

continues to evolve as an interesting area, and future work can build on our work to develop an integrated,

comprehensive understanding of both the demand side and supply side of the sharing economy.

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Table 1 Cities in China Experiencing Uber Entry by 2015

City	Unofficial Entry Month	Official Entry Month	Vehicle Purchase Quota Restriction
Shanghai		October, 2014	Yes
Shenzhen		October, 2014	Yes
Guangzhou		October, 2014	Yes
Beijing		August, 2014	Yes
Hangzhou		October, 2014	Yes
Chengdu		October, 2014	
Wuhan	October, 2014	March, 2015	
Chongqing		November, 2014	
Tianjin	December, 2014	March, 2015	Yes
Qingdao		May, 2015	
Foshan		June, 2015	
Changsha		June, 2015	
Suzhou		June, 2015	
Nanjing	June, 2015	October, 2015	
Xi'an		July, 2015	
Ningbo		August, 2015	
Xiamen		September, 2015	
Dalian		September, 2015	
Jinan		September, 2015	
Yantai		September, 2015	
Guiyang		December, 2015	Yes

Table 2 Summary Statistics

Variable	Mean	Std. Dev.	Min	Max
UnofficialUberEntry	0.00893	0.0941	0	1
OfficialUberEntry	0.0972	0.296	0	1
ln(NumCars)	9.433	0.535	7.739	10.98
ln(Didi)	4.679	4.907	0	11.82
ln(Yidao)	5.003	3.562	0	10.04
ln(Shenzhou)	1.483	3.323	0	10.05
ln(Population)	6.533	0.595	5.182	8.125
ln(Employed)	5.029	0.505	4.034	6.862
ln(Registered Unemployed)	11.01	0.506	9.843	11.94
ln(Average Wage)	10.807	0.290	10.009	11.772
ln(GRP Per Capita)	11.21	0.396	10.04	12.20
ln(Mobile Subscribers)	7.008	0.406	5.874	7.860
ln(Internet Subscribers)	5.247	0.540	4.182	7.337
ln(Highway Passenger Traffic)	10.10	0.831	8.110	12.02
ln(Buses)	8.523	0.416	7.414	9.400
ln(Bus Passenger Volume)	11.39	0.522	10.18	12.40
ln(Taxis)	8.918	0.622	7.670	9.925
ln(Road Area Per Capita)	2.728	0.345	1.917	3.252
ln(Metro Length)	1.975	2.012	0	5.313

Note: The sample contains data from 2010 to 2015. We use 14 cities that experienced People's Uber entry and do not have car purchase restrictions. There are 1008 observations at city-month level.

Table 3 Difference-in-Difference Model

	(1)	(2)
Dependent Variable	ln(NumCars)	ln(NumCars)
Unofficial Uber Entry	0.180	0.000
	(0.143)	(0.0837)
Official Uber Entry	0.217**	0.0820*
	(0.0788)	(0.0429)
Observations	1,008	1,008
R-squared	0.798	0.850
Month FE	Yes	Yes
City FE	Yes	Yes
City trend	No	Linear

Note: Robust standard errors clustered at city level in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 4 Relative Time Model

	(1)	(2)
Dependent Variable	ln(NumCars)	ln(NumCars)
Pre-Uber-Entry (-8)	-0.310*	-0.0361
	(0.161)	(0.145)
Pre-Uber-Entry (-7)	-0.289*	-0.0418
	(0.153)	(0.143)
Pre-Uber-Entry (-6)	-0.299*	-0.0809
	(0.145)	(0.134)
Pre-Uber-Entry (-5)	-0.290**	-0.109
	(0.125)	(0.0913)
Pre-Uber-Entry (-4)	-0.194*	-0.0543
	(0.0904)	(0.0721)
Pre-Uber-Entry (-3)	-0.102	-0.00844
	(0.0923)	(0.0763)
Pre-Uber-Entry (-2)	0.0150	0.0657
	(0.0651)	(0.0576)
	Baseline:	
	Pre-Uber-Entry (-1) omitted	
Unofficial Uber Entry	0.176	0.0408
	(0.133)	(0.0890)
Official Uber Month	0.150**	0.100**
	(0.0685)	(0.0453)
Post-Uber-Entry (1)	0.182*	0.105**
	(0.0851)	(0.0474)
Post-Uber-Entry (2)	0.347***	0.202**
	(0.115)	(0.0874)
Post-Uber-Entry (3)	0.516***	0.283*
	(0.166)	(0.148)
Post-Uber-Entry (4)	0.373***	0.178
	(0.0966)	(0.243)
Post-Uber-Entry (5)	0.410**	0.215
	(0.154)	(0.232)
Observations	1,008	1,008
R-squared	0.815	0.855
Month FE	Yes	Yes
City FE	Yes	Yes
City trend	No	Linear

Note: Robust standard errors clustered at city level in parentheses. Relative time dummies for more 2 years prior to Uber entry are estimated but not reported. *** p<0.01, ** p<0.05, * p<0.1

Table 5 Relative Time Model: Including Control Variables

Dependent Variable	(1) ln(NumCars)	(1) ln(NumCars)	(1) ln(NumCars)
Pre-Uber-Entry (-8)	-0.0206	0.0300	0.0282
	(0.162)	(0.107)	(0.112)
Pre-Uber-Entry (-7)	-0.0340	0.0114	0.00183
	(0.165)	(0.113)	(0.125)
Pre-Uber-Entry (-6)	-0.0809	-0.0418	-0.0573
	(0.149)	(0.114)	(0.118)
Pre-Uber-Entry (-5)	-0.108	-0.0817	-0.0936
	(0.106)	(0.0765)	(0.0814)
Pre-Uber-Entry (-4)	-0.0532	-0.0361	-0.0454
	(0.0814)	(0.0575)	(0.0609)
Pre-Uber-Entry (-3)	-0.00815	0.00459	-0.000525
	(0.0788)	(0.0744)	(0.0726)
Pre-Uber-Entry (-2)	0.0671	0.0727	0.0721
	(0.0594)	(0.0593)	(0.0590)
	Baseline: Pre-Uber-Entry (-1) omitted		
Unofficial Uber Entry	0.0480	0.0283	0.0301
	(0.0828)	(0.0844)	(0.0810)
Official Uber Month	0.0983*	0.105**	0.102**
	(0.0477)	(0.0440)	(0.0461)
Post-Uber-Entry (1)	0.0965*	0.120**	0.110**
	(0.0539)	(0.0402)	(0.0437)
Post-Uber-Entry (2)	0.185*	0.224***	0.206***
	(0.0904)	(0.0409)	(0.0504)
Post-Uber-Entry (3)	0.263	0.316***	0.294***
	(0.149)	(0.0754)	(0.0814)
Post-Uber-Entry (4)	0.140	0.254*	0.215*
	(0.238)	(0.128)	(0.118)
Post-Uber-Entry (5)	0.174	0.289**	0.252*
	(0.235)	(0.119)	(0.128)
ln(Didi)	0.0471		0.0502
	(0.0327)		(0.0286)
ln(Yidao)	0.0126		0.0178
	(0.00991)		(0.0118)
ln(Shenzhou)	0.00641		0.0405
	(0.0718)		(0.0650)
ln(Population)		1.260	1.216
		(2.099)	(2.075)
ln(Employed)		0.0922**	0.0927**
		(0.0316)	(0.0422)
ln(Registered Unemployed)		-0.0750	-0.0599
		(0.0760)	(0.0829)
ln(Average Wage)		0.0211	0.0288
		(0.0361)	(0.0342)
ln(GRP Per Capita)		-0.00886	0.0123
		(0.0697)	(0.0715)
ln(Mobile Subscribers)		0.184	0.229
		(0.271)	(0.317)
ln(Internet Subscribers)		0.00123	1.81e-05
		(0.0236)	(0.0213)
ln(Highway Passenger Traffic)		-0.0480	-0.0418
		(0.0494)	(0.0519)
ln(Buses)		0.380*	0.443**
		(0.197)	(0.198)
ln(Bus Passenger Volume)		0.0307	0.0306
		(0.142)	(0.142)
ln(Taxis)		-0.149	-0.180
		(0.119)	(0.126)
ln(Road Area Per Capita)		-0.186	-0.178
		(0.112)	(0.113)
ln(Metro Length)		-0.00400	-0.00159
		(0.0139)	(0.0138)
Observations	1,008	1,008	1,008
R-squared	0.857	0.860	0.861
Month FE	Yes	Yes	Yes
City FE	Yes	Yes	Yes
City trend	Linear	Linear	Linear

Note: Robust standard errors clustered at city level in parentheses. Relative time dummies for more 2 years prior to Uber entry estimated but not reported. *** p<0.01, ** p<0.05, * p<0.1

Table 6 Relative Time Model: Poisson Pseudo-Maximum Likelihood Estimation

Dependent Variable	(1) NumCars	(2) NumCars
Pre-Uber-Entry (-8)	-0.0220	0.0621
	(0.151)	(0.115)
Pre-Uber-Entry (-7)	-0.0351	0.0289
	(0.145)	(0.116)
Pre-Uber-Entry (-6)	-0.0654	-0.0298
	(0.136)	(0.108)
Pre-Uber-Entry (-5)	-0.0773	-0.0512
	(0.0947)	(0.0755)
Pre-Uber-Entry (-4)	-0.0500	-0.0304
	(0.0781)	(0.0574)
Pre-Uber-Entry (-3)	-0.00816	0.00971
	(0.0737)	(0.0604)
Pre-Uber-Entry (-2)	0.0692	0.0785
	(0.0567)	(0.0542)
	Baseline:	
	Pre Uber Entry (-1) omitted	
Unofficial Uber Entry	0.0190	0.0307
	(0.0917)	(0.0791)
Official Uber Month	0.0434	0.0520
	(0.0421)	(0.0460)
Post-Uber-Entry (1)	0.0559	0.0679
	(0.0377)	(0.0511)
Post-Uber-Entry (2)	0.147**	0.153***
	(0.0715)	(0.0490)
Post-Uber-Entry (3)	0.221*	0.224***
	(0.121)	(0.0733)
Post-Uber-Entry (4)	0.145	0.163*
	(0.198)	(0.0925)
Post-Uber-Entry (5)	0.141	0.162
	(0.197)	(0.110)
Controls	No	Yes
Observations	1,008	1,008
Month FE	Yes	Yes
City FE	Yes	Yes
City trend	Linear	Linear

Note: Robust standard errors clustered at city level in parentheses.
Relative time dummies for more 2 years prior to Uber entry not
reported. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 7 Difference-in-Difference Model: Placebo Treatments Using Data on Prior Years

Dependent Variable	(1) ln(NumCars)	(2) ln(NumCars)
Unofficial Uber Entry	0.0196	0.0296
	(0.0522)	(0.0451)
Official Uber Entry	0.0340	0.0395
	(0.0353)	(0.0358)
Controls	No	Yes
Observations	672	672
R-squared	0.791	0.800
Month FE	Yes	Yes
City FE	Yes	Yes
City trend	Linear	Linear

Note: Robust standard errors clustered at city level in parentheses.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 8 Relative Time Model: Placebo Treatments Using Data on Prior Years

Dependent Variable	(1) ln(NumCars)	(2) ln(NumCars)
Pre-Uber-Entry (-8)	-0.0445	-0.0456
	(0.107)	(0.131)
Pre-Uber-Entry (-7)	-0.0425	-0.0247
	(0.103)	(0.122)
Pre-Uber-Entry (-6)	0.0231	0.0378
	(0.0954)	(0.112)
Pre-Uber-Entry (-5)	-0.0501	-0.0393
	(0.0807)	(0.0923)
Pre-Uber-Entry (-4)	-0.0327	-0.0315
	(0.0786)	(0.0875)
Pre-Uber-Entry (-3)	-0.0289	-0.0227
	(0.0655)	(0.0700)
Pre-Uber-Entry (-2)	0.0242	0.0266
	(0.0493)	(0.0538)
	Baseline:	
	Pre-Uber-Entry (-1) omitted	
Unofficial Uber Entry	0.0303	0.0357
	(0.0559)	(0.0504)
Official Uber Month	0.0845	0.0963
	(0.0551)	(0.0554)
Post-Uber-Entry (1)	0.0328	0.0263
	(0.0439)	(0.0505)
Post-Uber-Entry (2)	0.0618	0.0502
	(0.0915)	(0.108)
Post-Uber-Entry (3)	0.115	0.0729
	(0.113)	(0.146)
Post-Uber-Entry (4)	0.0983	0.0688
	(0.234)	(0.245)
Post-Uber-Entry (5)	0.0976	0.0716
	(0.243)	(0.270)
Controls	No	Yes
Observations	672	672
R-squared	0.796	0.804
Month FE	Yes	Yes
City FE	Yes	Yes
City trend	Linear	Linear

Note: Robust standard errors clustered at city level in parentheses. Relative time dummies for more 2 years prior to Uber entry not reported.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 9 Difference-in-Difference Model: Using Six Cities with Vehicle Purchase Quota Restriction

Dependent Variable	(1) ln(NumCars)	(2) ln(NumCars)	(3) ln(NumCars)	(4) ln(NumCars)
Post Purchase Restriction	-0.750***	-0.840***	-0.553***	-0.776*
	(0.0895)	(0.114)	(0.124)	(0.327)
Unofficial Uber Entry	-0.151	-0.190	-0.229	-0.190
	(0.104)	(0.140)	(0.126)	(0.144)
Official Uber Entry	0.0341	-0.0639	-0.0989	-0.103
	(0.154)	(0.159)	(0.163)	(0.156)
Controls	No	Yes	No	Yes
Observations	432	432	432	432
R-squared	0.599	0.652	0.633	0.677
Month FE	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
City trend	Linear (Assume same trend before and after restriction)	Linear (Assume same trend before and after restriction)	Linear (Assume different trends before and after restriction)	Linear (Assume different trends before and after restriction)

Note: Robust standard errors clustered at city level in parentheses.

Six cities where vehicle purchase quota restriction was introduced before the entry of People's Uber are included in the analysis. Shenzhen, where vehicle purchase quota restriction was introduced after the entry of People's Uber, is excluded.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 10 Relative Time Model: By Engine Displacement (Without Controls)

Dependent Variable	(1) ln(NumCars): Displacement: 1-1.5 L	(2) ln(NumCars): Displacement: 1.5-2 L	(3) ln(NumCars): Displacement: 2-2.5 L	(4) ln(NumCars): Displacement: Above 2.5L
Pre-Uber-Entry (-8)	-0.178	-0.0144	0.140	0.0732
	(0.113)	(0.162)	(0.138)	(0.132)
Pre-Uber-Entry (-7)	-0.182	-0.0222	0.130	0.0477
	(0.124)	(0.158)	(0.133)	(0.117)
Pre-Uber-Entry (-6)	-0.200	-0.0710	0.0824	0.0185
	(0.124)	(0.145)	(0.123)	(0.114)
Pre-Uber-Entry (-5)	-0.198**	-0.111	0.0401	-0.0302
	(0.0814)	(0.0975)	(0.0863)	(0.0770)
Pre-Uber-Entry (-4)	-0.123*	-0.0630	0.0690	-0.0240
	(0.0599)	(0.0789)	(0.0703)	(0.0595)
Pre-Uber-Entry (-3)	-0.0602	-0.00515	0.0829	0.0110
	(0.0657)	(0.0829)	(0.0754)	(0.0691)
Pre-Uber-Entry (-2)	0.0288	0.0717	0.122*	0.0534
	(0.0562)	(0.0610)	(0.0571)	(0.0579)
Baseline: Pre-Uber-Entry (-1) omitted				
Unofficial Uber Entry	0.0942	0.0420	-0.00750	-0.0290
	(0.0599)	(0.117)	(0.0854)	(0.104)
Official Uber Month	0.128**	0.110**	0.0613	0.00579
	(0.0531)	(0.0465)	(0.0421)	(0.0627)
Post-Uber-Entry (1)	0.158**	0.136**	0.00950	-0.0421
	(0.0541)	(0.0461)	(0.0449)	(0.0781)
Post-Uber-Entry (2)	0.292***	0.239**	0.0377	-0.0766
	(0.0964)	(0.0878)	(0.0880)	(0.109)
Post-Uber-Entry (3)	0.347**	0.323**	0.0456	-0.0253
	(0.157)	(0.149)	(0.130)	(0.143)
Post-Uber-Entry (4)	0.288	0.256	-0.117	-0.168
	(0.234)	(0.239)	(0.228)	(0.183)
Post-Uber-Entry (5)	0.381	0.261	-0.144	-0.162
	(0.221)	(0.246)	(0.220)	(0.205)
Controls	No	No	No	No
Observations	1,008	1,008	1,008	1,008
R-squared	0.860	0.840	0.869	0.755
Month FE	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
City trend	Linear	Linear	Linear	Linear

Table 11 Relative Time Model: By Engine Displacement (With Controls)

Dependent Variable	(1) ln(NumCars): Displacement: 1-1.5 L	(2) ln(NumCars): Displacement: 1.5-2 L	(3) ln(NumCars): Displacement: 2-2.5 L	(4) ln(NumCars): Displacement: Above 2.5 L
Pre-Uber-Entry (-8)	-0.0104	0.0390	0.148	0.0720
	(0.122)	(0.137)	(0.103)	(0.130)
Pre-Uber-Entry (-7)	-0.0643	0.0176	0.126	0.0270
	(0.137)	(0.143)	(0.112)	(0.112)
Pre-Uber-Entry (-6)	-0.127	-0.0442	0.0654	-0.0112
	(0.130)	(0.135)	(0.106)	(0.112)
Pre-Uber-Entry (-5)	-0.146	-0.0911	0.0212	-0.0615
	(0.0961)	(0.0923)	(0.0754)	(0.0751)
Pre-Uber-Entry (-4)	-0.0865	-0.0491	0.0514	-0.0544
	(0.0722)	(0.0704)	(0.0619)	(0.0667)
Pre-Uber-Entry (-3)	-0.0288	0.00506	0.0726	-0.0116
	(0.0650)	(0.0852)	(0.0710)	(0.0757)
Pre-Uber-Entry (-2)	0.0495	0.0792	0.121*	0.0386
	(0.0592)	(0.0655)	(0.0575)	(0.0595)
Baseline: Pre-Uber-Entry (-1) omitted				
Unofficial Uber Entry	0.0617	0.0349	-0.0172	-0.0316
	(0.0489)	(0.109)	(0.0819)	(0.0847)
Official Uber Month	0.112**	0.111**	0.0676	0.0279
	(0.0475)	(0.0510)	(0.0436)	(0.0648)
Post-Uber-Entry (1)	0.132**	0.140**	0.0205	-0.00133
	(0.0440)	(0.0474)	(0.0460)	(0.0813)
Post-Uber-Entry (2)	0.244***	0.239***	0.0520	-0.00909
	(0.0725)	(0.0541)	(0.0624)	(0.0979)
Post-Uber-Entry (3)	0.289**	0.327***	0.0695	0.0683
	(0.120)	(0.0798)	(0.0785)	(0.129)
Post-Uber-Entry (4)	0.231	0.282**	-0.0753	-0.0100
	(0.175)	(0.104)	(0.116)	(0.150)
Post-Uber-Entry (5)	0.326	0.282**	-0.102	0.0100
	(0.191)	(0.120)	(0.140)	(0.170)
Controls	Yes	Yes	Yes	Yes
Observations	1,008	1,008	1,008	1,008
R-squared	0.870	0.845	0.874	0.765
Month FE	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
City trend	Linear	Linear	Linear	Linear

Table 12 Relative Time Model: By Car Type

Dependent Variable	(1) ln(NumCars): Van	(2) ln(NumCars): Non-Van Cars	(3) ln(NumCars): Vans	(4) ln(NumCars): Non-Van Cars
Pre-Uber-Entry (-8)	-0.115	-0.0345	-0.0110	0.0278
	(0.118)	(0.148)	(0.116)	(0.117)
Pre-Uber-Entry (-7)	-0.0851	-0.0412	-0.00285	0.000746
	(0.122)	(0.145)	(0.129)	(0.128)
Pre-Uber-Entry (-6)	-0.0656	-0.0819	-0.00373	-0.0595
	(0.118)	(0.136)	(0.120)	(0.121)
Pre-Uber-Entry (-5)	-0.0620	-0.112	-0.0156	-0.0973
	(0.105)	(0.0927)	(0.0970)	(0.0836)
Pre-Uber-Entry (-4)	-0.0284	-0.0566	0.00608	-0.0483
	(0.0846)	(0.0738)	(0.0800)	(0.0629)
Pre-Uber-Entry (-3)	-0.00331	-0.00999	0.0199	-0.00243
	(0.0824)	(0.0770)	(0.0900)	(0.0728)
Pre-Uber-Entry (-2)	0.0539	0.0649	0.0699	0.0711
	(0.0697)	(0.0577)	(0.0756)	(0.0586)
		Baseline:		
		Pre Uber Entry (-1) omitted		
Unofficial Uber Entry	0.0205	0.0425	0.0151	0.0309
	(0.0791)	(0.0893)	(0.0848)	(0.0807)
Official Uber Month	0.105**	0.101**	0.0874*	0.103**
	(0.0432)	(0.0458)	(0.0478)	(0.0467)
Post-Uber-Entry (1)	0.0492	0.110**	0.0135	0.115**
	(0.0507)	(0.0482)	(0.0605)	(0.0438)
Post-Uber-Entry (2)	0.0785	0.211**	0.0176	0.216***
	(0.0502)	(0.0896)	(0.0609)	(0.0507)
Post-Uber-Entry (3)	0.116	0.294*	0.0361	0.306***
	(0.0929)	(0.151)	(0.0778)	(0.0824)
Post-Uber-Entry (4)	0.0989	0.185	-0.0223	0.225*
	(0.0841)	(0.249)	(0.108)	(0.119)
Post-Uber-Entry (5)	0.0328	0.225	-0.105	0.267*
	(0.114)	(0.237)	(0.125)	(0.129)
Controls	No	No	Yes	Yes
Observations	1,008	1,008	1,008	1,008
R-squared	0.802	0.855	0.810	0.861
Month FE	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
City trend	Linear	Linear	Linear	Linear

Note: Robust standard errors clustered at city level in parentheses. Relative time dummies for more 2 years prior to Uber entry not reported.

*** p<0.01, ** p<0.05, * p<0.1

Table 13 Relative Time Model: By Gender

Dependent Variable	(1) ln(NumCars): Female	(2) ln(NumCars): Male	(3) ln(NumCars): Female	(4) ln(NumCars): Male
Pre-Uber-Entry (-8)	0.00952	-0.0686	0.120	-0.0205
	(0.198)	(0.140)	(0.129)	(0.131)
Pre-Uber-Entry (-7)	-0.0190	-0.0739	0.0526	-0.0399
	(0.164)	(0.149)	(0.129)	(0.146)
Pre-Uber-Entry (-6)	-0.0521	-0.130	-0.0145	-0.110
	(0.154)	(0.146)	(0.126)	(0.138)
Pre-Uber-Entry (-5)	-0.0732	-0.150	-0.0561	-0.132
	(0.102)	(0.0960)	(0.0837)	(0.0926)
Pre-Uber-Entry (-4)	-0.0124	-0.0848	-0.00751	-0.0700
	(0.0683)	(0.0849)	(0.0554)	(0.0745)
Pre-Uber-Entry (-3)	0.00153	-0.0163	0.0104	-0.00678
	(0.0719)	(0.0899)	(0.0711)	(0.0864)
Pre-Uber-Entry (-2)	0.0468	0.0737	0.0526	0.0823
	(0.0573)	(0.0614)	(0.0597)	(0.0645)
	Baseline:			
	Pre-Uber-Entry (-1) omitted			
Unofficial Uber Entry	0.0541	0.0101	0.0255	0.00300
	(0.0880)	(0.109)	(0.0773)	(0.0988)
Official Uber Month	0.0962*	0.114**	0.0964*	0.111**
	(0.0454)	(0.0462)	(0.0448)	(0.0485)
Post-Uber-Entry (1)	0.0897	0.127**	0.0990*	0.120***
	(0.0531)	(0.0460)	(0.0490)	(0.0384)
Post-Uber-Entry (2)	0.170*	0.236**	0.184***	0.219***
	(0.0937)	(0.0920)	(0.0559)	(0.0544)
Post-Uber-Entry (3)	0.219	0.318*	0.248***	0.292***
	(0.138)	(0.161)	(0.0720)	(0.0885)
Post-Uber-Entry (4)	0.0754	0.241	0.151	0.217
	(0.236)	(0.263)	(0.102)	(0.138)
Post-Uber-Entry (5)	0.0840	0.282	0.168	0.250
	(0.234)	(0.260)	(0.116)	(0.151)
Controls	No	No	Yes	Yes
Observations	1,008	1,008	1,008	1,008
R-squared	0.872	0.831	0.880	0.836
Month FE	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
City trend	Linear	Linear	Linear	Linear

Note: Robust standard errors clustered at city level in parentheses. Relative time dummies for more 2 years prior to Uber entry not reported. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 14 Relative Time Model: By Age (Without Controls)

Dependent Variable	(1) ln(NumCars): Age: 18-24	(2) ln(NumCars): Age: 25-34	(3) ln(NumCars): Age: 35-44	(4) ln(NumCars): Age: 45-54	(5) ln(NumCars): Age: Above 55
Pre-Uber-Entry (-8)	-0.0143	0.0276	0.132	-0.130	-0.111
	(0.159)	(0.122)	(0.137)	(0.115)	(0.177)
Pre-Uber-Entry (-7)	-0.0482	-0.00848	0.141	-0.0914	-0.123
	(0.132)	(0.124)	(0.129)	(0.122)	(0.176)
Pre-Uber-Entry (-6)	-0.0286	-0.00763	0.164	-0.0961	-0.164
	(0.128)	(0.111)	(0.128)	(0.134)	(0.192)
Pre-Uber-Entry (-5)	-0.109	-0.127	0.0253	-0.130	-0.144
	(0.0773)	(0.0781)	(0.0878)	(0.0960)	(0.147)
Pre-Uber-Entry (-4)	-0.0952	-0.0662	0.0670	-0.0412	-0.0596
	(0.0649)	(0.0624)	(0.0655)	(0.0794)	(0.112)
Pre-Uber-Entry (-3)	-0.0408	-0.0341	0.0717	0.00889	-0.0212
	(0.0803)	(0.0657)	(0.0737)	(0.0829)	(0.108)
Pre-Uber-Entry (-2)	0.00485	0.0262	0.0401	0.0616	0.0563
	(0.0619)	(0.0622)	(0.0681)	(0.0628)	(0.0772)
			Baseline: Pre-Uber-Entry (-1) omitted		
Unofficial Uber Entry	0.0141	0.0347	0.0365	0.0171	-0.0239
	(0.136)	(0.0874)	(0.0901)	(0.0871)	(0.0945)
Official Uber Month	0.107**	0.125**	0.0771	0.111*	0.111*
	(0.0455)	(0.0429)	(0.0490)	(0.0541)	(0.0612)
Post-Uber-Entry (1)	0.109*	0.141***	0.0627	0.112**	0.123**
	(0.0615)	(0.0454)	(0.0548)	(0.0492)	(0.0566)
Post-Uber-Entry (2)	0.199**	0.259***	0.130	0.212**	0.261*
	(0.0824)	(0.0840)	(0.0967)	(0.0980)	(0.127)
Post-Uber-Entry (3)	0.335***	0.360**	0.164	0.251	0.286
	(0.105)	(0.152)	(0.151)	(0.149)	(0.176)
Post-Uber-Entry (4)	0.242	0.300	0.0231	0.149	0.229
	(0.172)	(0.252)	(0.264)	(0.253)	(0.247)
Post-Uber-Entry (5)	0.255	0.350	0.0362	0.187	0.251
	(0.176)	(0.239)	(0.254)	(0.256)	(0.272)
Controls	No	No	No	No	No
Observations	1,008	1,008	1,008	1,008	1,008
R-squared	0.965	0.978	0.973	0.971	0.913
Month FE	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes
City trend	Linear	Linear	Linear	Linear	Linear

Note: Robust standard errors clustered at city level in parentheses. Relative time dummies for more 2 years prior to Uber entry not reported. *** p<0.01, ** p<0.05, * p<0.1

Table 15 Relative Time Model: By Age (With Controls)

Dependent Variable	(1) ln(NumCars): Age: 18-24	(2) ln(NumCars): Age: 25-34	(3) ln(NumCars): Age: 35-44	(4) ln(NumCars): Age: 45-54	(5) ln(NumCars): Age: Above 55
Pre-Uber-Entry (-8)	-0.131	0.0632	0.194*	-0.0473	0.111
	(0.138)	(0.118)	(0.101)	(0.0757)	(0.180)
Pre-Uber-Entry (-7)	-0.156	0.0109	0.167	-0.0484	0.0186
	(0.129)	(0.129)	(0.107)	(0.1000)	(0.193)
Pre-Uber-Entry (-6)	-0.114	-0.00205	0.162	-0.0917	-0.104
	(0.122)	(0.112)	(0.120)	(0.117)	(0.215)
Pre-Uber-Entry (-5)	-0.167*	-0.119	0.0123	-0.142	-0.127
	(0.0834)	(0.0852)	(0.0806)	(0.0918)	(0.186)
Pre-Uber-Entry (-4)	-0.140*	-0.0610	0.0493	-0.0575	-0.0603
	(0.0753)	(0.0628)	(0.0559)	(0.0643)	(0.146)
Pre-Uber-Entry (-3)	-0.0816	-0.0307	0.0666	0.00622	0.000877
	(0.0945)	(0.0653)	(0.0703)	(0.0690)	(0.0987)
Pre-Uber-Entry (-2)	-0.0161	0.0312	0.0399	0.0637	0.0753
	(0.0624)	(0.0592)	(0.0624)	(0.0572)	(0.0733)
			Baseline: Pre-Uber-Entry (-1) omitted		
Unofficial Uber Entry	0.0152	0.0330	0.00932	-0.0136	-0.0558
	(0.121)	(0.0784)	(0.0875)	(0.0821)	(0.0665)
Official Uber Month	0.130**	0.126***	0.0840*	0.108**	0.0927*
	(0.0596)	(0.0413)	(0.0462)	(0.0497)	(0.0494)
Post-Uber-Entry (1)	0.148*	0.142***	0.0790	0.112**	0.0866*
	(0.0777)	(0.0407)	(0.0482)	(0.0413)	(0.0457)
Post-Uber-Entry (2)	0.268**	0.253***	0.151**	0.212***	0.187*
	(0.0985)	(0.0426)	(0.0580)	(0.0622)	(0.102)
Post-Uber-Entry (3)	0.404***	0.352***	0.199**	0.263**	0.227
	(0.118)	(0.0685)	(0.0754)	(0.0948)	(0.152)
Post-Uber-Entry (4)	0.337**	0.300**	0.109	0.185	0.138
	(0.135)	(0.115)	(0.126)	(0.152)	(0.219)
Post-Uber-Entry (5)	0.345*	0.341***	0.131	0.241	0.174
	(0.168)	(0.112)	(0.120)	(0.166)	(0.264)
Controls	Yes	Yes	Yes	Yes	Yes
Observations	1,008	1,008	1,008	1,008	1,008
R-squared	0.966	0.979	0.974	0.973	0.926
Month FE	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes
City trend	Linear	Linear	Linear	Linear	Linear

Note: Robust standard errors clustered at city level in parentheses. Relative time dummies for more 2 years prior to Uber entry not reported. *** p<0.01, ** p<0.05, * p<0.1

Appendix A. Correlation Among Variables

Table A1 Correlation Among Variables

Variable	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
1 UnofficialUberEntry																		
2 OfficialUberEntry	-0.031																	
3 ln(NumCars)	0.088	0.338																
4 ln(Didi)	0.114	0.390	0.415															
5 ln(Yidao)	0.102	0.368	0.472	0.810														
6 ln(Shenzhou)	0.131	0.705	0.284	0.522	0.483													
7 ln(Population)	0.012	0.131	0.518	0.031	0.043	0.022												
8 ln(Employed)	0.063	0.297	0.646	0.391	0.402	0.230	0.711											
9 ln(Registered Unemployzed)	0.057	0.128	0.361	0.078	0.094	0.034	0.753	0.605										
10 ln(Average Wage)	0.089	0.237	0.391	0.453	0.656	0.348	0.152	0.369	0.260									
11 ln(Highway Passenger Traffic)	-0.074	-0.199	0.391	-0.212	-0.162	-0.427	0.640	0.420	0.286	-0.092								
12 ln(GRP Per Capita)	0.087	0.099	0.032	0.483	0.466	0.211	-0.555	-0.239	-0.459	0.306	-0.409							
13 ln(Mobile Subscribers)	0.047	0.257	0.788	0.349	0.418	0.184	0.704	0.684	0.498	0.346	0.535	-0.161						
14 ln(Internet Subscribers)	0.082	0.266	0.544	0.315	0.393	0.247	0.556	0.644	0.415	0.455	0.295	-0.106	0.660					
15 ln(Buses)	0.102	0.220	0.588	0.269	0.323	0.172	0.532	0.649	0.589	0.312	0.224	-0.275	0.707	0.585				
16 ln(Bus Passenger Volume)	0.067	0.137	0.378	0.141	0.193	0.065	0.520	0.612	0.690	0.226	0.167	-0.442	0.484	0.431	0.859			
17 ln(Taxis)	0.101	0.133	0.406	0.121	0.162	0.079	0.565	0.552	0.725	0.237	0.127	-0.401	0.502	0.402	0.878	0.930		
18 ln(Road Area Per Capita)	0.065	-0.011	-0.094	0.123	0.127	0.087	-0.400	-0.222	-0.218	0.125	-0.415	0.485	-0.339	-0.234	-0.323	-0.192	-0.158	
19 ln(Metro Length)	0.132	0.249	0.443	0.302	0.345	0.246	0.420	0.515	0.445	0.348	0.221	0.026	0.522	0.450	0.669	0.537	0.556	-0.264

Appendix B. Wild Cluster Bootstrap Based Inference

Table B1 Difference-in-Difference Model based on Wild Cluster Bootstrap

Dependent Variable	(1) ln(NumCars)		(2) ln(NumCars)	
	Coefficient (Clustered SE)	p-value based on wild cluster bootstrap	Coefficient (Clustered SE)	p-value based on wild cluster bootstrap
Unofficial Uber Entry	0.180 (0.143)	0.680	0.000 (0.0837)	1.000
Official Uber Entry	0.217** (0.0788)	0.0050	0.0820* (0.0429)	0.0470**
Observations	1,008		1,008	
R-squared	0.798		0.850	
Month FE	Yes		Yes	
City FE	Yes		Yes	
City trend	No		Linear	

Note: Robust standard errors clustered at city level in parentheses. The Stata command boottest is used to perform wild cluster bootstrap. *** p<0.01, ** p<0.05, * p<0.1

Table B2 Relative Time Model based on Wild Cluster Bootstrap

Dependent Variable	(1) ln(NumCars)		(2) ln(NumCars)	
	Coefficient (Clustered SE)	p-value based on wild cluster bootstrap	Coefficient (Clustered SE)	p-value based on wild cluster bootstrap
Pre-Uber-Entry (-8)	-0.310* (0.161)	0.100	-0.0361 (0.145)	0.800
Pre-Uber-Entry (-7)	-0.289* (0.153)	0.102	-0.0418 (0.143)	0.798
Pre-Uber-Entry (-6)	-0.299* (0.145)	0.0850*	-0.0809 (0.134)	0.568
Pre-Uber-Entry (-5)	-0.290** (0.125)	0.0590*	-0.109 (0.0913)	0.260
Pre-Uber-Entry (-4)	-0.194* (0.0904)	0.0810*	-0.0543 (0.0721)	0.492
Pre-Uber-Entry (-3)	-0.102 (0.0923)	0.319	-0.00844 (0.0763)	0.908
Pre-Uber-Entry (-2)	0.0150 (0.0651)	0.856	0.0657 (0.0576)	0.294
		Baseline: Pre-Uber-Entry (-1) omitted		
Unofficial Uber Entry	0.176 (0.133)	0.625	0.0408 (0.0890)	0.694
Official Uber Month	0.150** (0.0685)	0.0440**	0.100** (0.0453)	0.049**
Post-Uber-Entry (1)	0.182* (0.0851)	0.0770*	0.105** (0.0474)	0.025**
Post-Uber-Entry (2)	0.347*** (0.115)	0.00400***	0.202** (0.0874)	0.085*
Post-Uber-Entry (3)	0.516*** (0.166)	0.0250**	0.283* (0.148)	0.149
Post-Uber-Entry (4)	0.373*** (0.0966)	0.0260**	0.178 (0.243)	0.550
Post-Uber-Entry (5)	0.410** (0.154)	0.0530*	0.215 (0.232)	0.519
Observations	1,008		1,008	
R-squared	0.815		0.855	
Month FE	Yes		Yes	
City FE	Yes		Yes	
City trend	No		Linear	

Note: Robust standard errors clustered at city level in parentheses. Relative time dummies for more 2 years prior to Uber entry are estimated but not reported. The Stata command boottest is used to perform wild cluster bootstrap. *** $p<0.01$, ** $p<0.05$, * $p<0.1$