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SHOW ME THE WAY TO GO HOME: AN EMPIRICAL INVESTIGATION OF RIDE-SHARING AND ALCOHOL RELATED MOTOR VEHICLE FATALITIES[1]

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In this work, we investigate how the entry of ride-sharing services influences the rate of alcohol related motor vehicle fatalities. While significant debate has surrounded ride-sharing, limited empirical work has been devoted to uncovering the societal benefits of such services (or the mechanisms which drive these benefits). Using a ***difference in difference*** *approach to exploit a natural experiment, the entry of Uber Black and Uber X into California markets between 2009 and 2014, we find a significant drop in the rate of fatalities after the introduction of Uber X. Further, results suggest that not all services have the same effect, insofar as the effect of the Uber Black car service is intermittent and manifests only in selective locations (i.e., large cities). These results underscore the importance of coupling increased availability with cost savings in order to exploit the public welfare gains offered by the sharing economy. Practical and theoretical implications are discussed.*

Keywords: Uber, sharing economy, ride-sharing, drunk driving, vehicular fatalities, difference in difference, natural experiment, platforms

Introduction

The introduction of ride-sharing platforms such as Uber and Lyft has sparked a host of policy debates over the last half decade. Detractors of such platforms argue not only that the entry of these firms puts the public at significant risk through their limited liability corporate structure,[2] but that patrons are equally at risk,[3] and these firms upset the delicate balance of service providers.[4] Alternatively, both scholars and policy makers have argued that such services resolve market failures by providing customers with a much needed option that circumnavigates the bureaucratic processes of licensed livery (Rempel 2014). However, limited empirical evidence exists to establish the social benefits (or lack thereof) of these platforms. To the extent that Uber, the market leader in ride-sharing by market valuation (MacMillan and Demos 2015) and penetration (DePillis 2013), has entered more than 58 countries and 300 cities worldwide as of 2015 (and many are debating legislation regarding these platforms), a robust estimate of any social impact could provide much needed empirical evidence to ground policy debates.

One social benefit consistently associated with ride-sharing, and presently being debated in the media, is the potential for

[1]Ravi Bapna was the accepting senior editor for this paper. Nigel Melville served as the associate editor.

[2]http://www.nytimes.com/2014/10/19/upshot/when-uber-lyft-and-airbnb-meet-the-real-world.html?abt=0002&abg=0.

[3]http://www.sfexaminer.com/sanfrancisco/uber-driver-suspected-of-attacking-passenger-in-sf-raises-safety-concerns/Content?oid=2907619.

[4]http://www.nytimes.com/2014/09/30/business/uniteds-deal-with-uber-raises-concerns.html.

reducing the instances of drunk driving (Badger 2014). As existing regulatory structures for traditional vehicle for hire services, viz. taxicabs, are designed to retard the number of licensed vehicles on the road in order to manufacture excess demand (Sternberg 1996),[5] the absence of a sufficient number of taxis may result in citizens operating motor vehicles under the influence of alcohol (Grove 2013, Jackson and Owens 2011). Inasmuch as these welfare losses are often borne by taxpayers, through the cost of prosecuting and incarcerating individuals convicted of DUI, the effective management of the number of, and type of, vehicle for hire services poses a significant challenge for policy makers.

Preliminary analysis conducted by ride-sharing firms and several industry analysts suggests that the introduction of ride-sharing services has a negative effect on DUI arrests.[6] However, these studies have been questioned on several grounds, including involvement of ride-sharing firms in the data analysis, methodological rigor (i.e., single city estimations), and the presence of confounding factors such as changes in a city's population, bar scene, and tougher enforcement.

Moreover, the mechanisms by which such services influence the rate of intoxicated driving are not well understood. On one hand, it is possible that the decrease is simply the result of availability of vehicles for hire. Insofar as it is often difficult to hire a taxi, based on time, location, or even the race of the patron (Meeks 2010), it is plausible that the presence of the platform mitigates these market inefficiencies by soliciting the driver electronically. As electronic solicitation should be significantly easier (Davis 1989; DeLone and McLean 1992), and be accompanied by reduced search costs (Parker and Van Alstyne 2005), an excess of utility should be generated for the consumer. On the other hand, it is equally plausible that the consumer's choice to drive under the influence is impacted by the cost of hiring a taxi as well as the availability of drivers; that is, the cost of searching for and hiring a car is prohibitive (Clarke and Cornish 1985; Cornish and Clarke 2014). If this is the case, the decrease in intoxicated driving after the introduction of ride-sharing may be a result of reduced cost as well as vehicle availability. These broad questions—what is the impact of ride-sharing introduction on alcohol related motor vehicle fatalities and by what mechanism is such change affected—form the core of the research investigated in this paper.

Empirically, we exploit a natural experiment to investigate the effect: the introduction of the ride-sharing service Uber into cities in the State of California between 2009 and 2014. Leveraging this econometric setup offers us two advantages. First, to the extent that the entrance of Uber is staggered temporally and geographically, we execute a *difference in difference* estimation to establish the effect, thereby simulating a laboratory experiment with data from a natural experiment. Second, Uber offers multiple services with varying price points (note that these services also enter at varying times and in varying orders). On one hand, Uber Black, a town car service, offers transportation with a significant markup over taxicabs (~20% to ~30% price premium). On the other, the Uber X service is a personalized driving service which offers significant *discounts* (~20% to ~30% price reductions from taxis). To the degree that each of these services may identify a different mechanism (availability versus availability and price point), we are able to cleanly isolate the dominant mechanisms. We test these using hand collected data from the California Highway Patrol's (CHP) safety and crash dataset and a custom webscraper which indicates when each service entered a geographic area in California.

Results indicate four notable findings. First, while the entry of Uber X strongly and negatively affects the number of motor vehicle fatalities, limited evidence exists to support previous claims that this occurs with the Uber Black car service as well (indicating that prior claims about the efficacy of ride-sharing, specifically Uber, may have been overstated; Badger 2014)). Second, results indicate that the time for such effects to manifest is nontrivial (upwards of 9 to 15 months). Third, results suggest no effect of entry when surge pricing is likely in effect, thereby underscoring the importance of cost considerations. Fourth, results indicate no negative effect of entry on the rate of non-alcohol related motor vehicle fatalities (suggesting that the potential spike in automobiles on the road is not negatively affecting other drivers). These results are robust to a variety of estimations (e.g., OLS, quasi-maximum likelihood count models) and operationalizations, with no heterogeneous pretreatment trend detected, indicating that the primary assumption of the difference in difference model is not violated (Angrist and Pischke 2008; Bertrand et al. 2004).[7] Econometrically, results indicate that the entrance of Uber X results in a 3.6% to 5.6% decrease in the rate of

[5]Media accounts suggest that the cost of a medallion in New York City, for example, was in excess of $1,000,000 as of 2013. An in-depth discussion of the history of taxi medallions and their effect on scarcity can be found at http://blog.priceonomics.com/post/47636506327/the-tyranny-of-the-taxi-medallions.

[6]http://blog.uber.com/duiratesdecline.

[7]Note also that diagnostics of the estimations suggest that the residuals do not suffer the serial correlation problems which often plague difference in difference estimations (Bertrand et al. 2004).

motor vehicle fatalities per quarter in the state of California. With more than 1,000 deaths occurring in California due to alcohol related car crashes every year (California DMV 2014), this represents a substantial opportunity to improve public welfare and save lives.

Theoretically, these results add interesting nuance to extant understanding of the sharing economy. To the extent that researchers have proposed the sharing economy as a viable alternative to established firms in many markets—for example, AirBnB for hotels (Edelman and Luca 2014) and the crowdfunding of nascent ventures (Burtch et al. 2013)—our results highlight the importance of cost considerations in resolving such market failures. While it is plausible that increased access to services, regardless of cost, would allow consumers to price point differentiate based on their own needs, a preference of consumers toward established providers as costs increase is suggested. Further, to the degree that our results underscore the beneficial effects of ride-sharing services, inasmuch as considerable public welfare losses in the form of motor vehicle fatalities are avoided, this work informs the ongoing policy debate. Finally, this work contributes to the small, but growing, stream of literature discussing both the societal impacts of electronic platforms (Burtch et al. 2013; Chan and Ghose 2014; Greenwood and Agarwal 2016; Seamans and Zhu 2013) as well as the need to conceptualize IT services as a core aspect of the IS field (Alter 2010). To the degree that platforms have been found to both enhance and diminish public welfare, our work contributes by drawing a richer picture of the public welfare implications of platform introduction and how these services are driving commerce.

Related Literature

To investigate the potential mechanisms underlying the hypothesized relationship between the introduction of ride-sharing services and alcohol related motor vehicle fatalities, we invoke three literatures: existing research in technology adoption, platforms, and criminology literature regarding rational choice theory.[8]

Platform Theory

Extant work in platforms has a rich tradition in information systems and economics spanning more than two decades (Bakos and Bailey 1997; Brynjolfsson et al. 2003; Brynjolfsson and Smith 2000; Malone et al. 1987; Parker and Van Alstyne 2005; Rochet and Tirole 2003). To date, two perspectives have been taken. In the first, scholars have argued that the creation of platforms that promote commerce can reduce market inefficiencies by facilitating the buyer–seller match (Bakos and Bailey 1997). As a result, the implementation of the platform reduces the cost of transactions by increasing the likelihood that an individual who is leveraging the platform finds an acceptable trading partner. In the other, platforms have been argued to increase information transparency in markets by reducing information asymmetries (Brynjolfsson et al. 2003). In this work, researchers have argued that the platform facilitates frictionless commerce by protecting the buyer and seller from opportunism on the part of the other party through increased price transparency (Williamson 1981). While the perspectives taken by each of these streams is different, the end result is the same; by increasing the amount of publicly available knowledge regarding prices and products, platforms are able to expedite the exchange of goods and services while creating a surplus of welfare for both buyer and seller (Parker and Van Alstyne 2005).

While early manifestations of such work were either analytically driven to advance platform theory (Birkland and Lawrence 2009), or focused on more traditional examples of Internet platforms such as eBay or Amazon.com (Brynjolfsson et al. 2003; Chevalier and Goolsbee 2003; Forman et al. 2008), a recent burgeoning literature on the societal impact of platforms has emerged. Interestingly, a bevy of topics have been investigated, ranging from dating (Bapna et al. 2012), to the disruption of established media vendors (Seamans and Zhu 2013), to the spread of HIV (Chan and Ghose 2014; Greenwood and Agarwal 2016), to crowdfunding (Burtch et al. 2013), and even to the spread of hate crimes (Chan et al. 2016). In each, as was the case for commerce-driven platforms, two mechanisms have been suggested to drive the effect: self-selection into using the platform and decreased search costs (Brynjolfsson and Smith 2000). It is within this budding literature on the societal impact of platforms where we position this work. To the degree that regulating America's roadways has received significant attention from scholars (Feng et al. 2013; West 2004), due both to its economic importance and the externalities that it generates (Parry et al. 2007), it is an ideal context to further the scope of this literature.

Rational Choice Theory and Drunk Driving

Next, we reference prior work that sheds light on how intoxicated individuals make decisions. Although intoxication will clearly bias an individual's perception of risks (Assaad and

[8]We thank the anonymous reviewer for pointing us toward the technology adoption literature for our theorizing.

Exum 2002; Exum 2002), extant research suggests that even inebriated decision makers take action only after comparing viable alternatives (Jackson and Owens 2011; Turrisi and Jaccard 1992). Based on Rational Choice Theory (Clarke and Cornish 1985; Cornish and Clarke 2014), this research argues that individuals commit crimes out of a set of trade-offs from which they benefit, as opposed to individual-level psychoses or a natural predilection to engage in criminal enterprise. More simply, rational choice theory suggests that offenders respond selectively to particular situations based on the probability of being apprehended, the benefit they will reap from the crime, and the opportunity cost of selecting one option over another (Clarke and Cornish 1985). In the context of drunk driving, the theory would suggest that intoxicated individuals respond selectively to particular situations based on the probability of being apprehended, the cost of varying alternatives (e.g., court costs, cost of the taxi, social stigma, jail sentences), and the payoff of achieving the objective (i.e., arriving at the intended destination) (Jackson and Owens 2011; Ross 1982; Thurman et al. 1993; Turrisi and Jaccard 1992). Moreover, significant anecdotal and empirical evidence exists to support such findings. Anecdotal evidence suggests, for example, that DUIs are linked to a lack of available low cost public transport options, suggesting that individuals evaluate the cost of drunk driving versus the cost of available alternatives.[9] Academic research further supports this idea. Jackson and Owens (2011), for example, found that DUIs decreased by 40% in the Washington metropolitan area when late night public transportation services were expanded by the DC Transit Authority.

Hypothesis Development

Impact of Premium Ride-Sharing Services

Why might the introduction of a premium ride-sharing service influence the rate of alcohol related motor vehicle fatalities? Reviewed research offers two perspectives as to why an effect may accrue. The first, as discussed above, relates to extant platforms theory. The second is rooted in the extensive IS literature on technology adoption (Davis 1989; DeLone and McLean 1992).

To the extent that it is often difficult to hire a cab (Meeks 2010), platform theory would suggest that the search costs associated with finding transportation may decrease significantly when a ride-sharing app is used. Insofar as the app mitigates information asymmetries by granting the patron access to information like the type of vehicle and the time it will take the driver to get to the user's location, the patron should garner significant utility; the reason being that the patron need no longer rely on stochastic discovery of a cab by standing on the side of the road. Moreover, as ride-sharing services have been consistently characterized as "taxi[s] without the hassle" (Solinsky 2014), existing literature in technology adoption would also suggest that ride-sharing apps will be adopted and utilized. To the degree that the hiring of a ride-sharing car requires only opening the app and setting the pickup location (which is automatically determined by the phone's GPS), it is self-evident that the app is significantly easier to use and more useful (Davis 1989, DeLone and McLean 1992, 2003). The fact that the patron is automatically updated with the current location of the driver and cash is unnecessary[10] only underscores this point.

As a result, it is plausible that consumers would be willing to pay a premium for such a service by trading off the costs of searching for a cab/ease of solicitation with the certainty of knowing both when and if the car will arrive. Put another way, because the process of discovering a traditional cab is not costless, the search for a cab is characterized by considerable uncertainty, and ride-sharing apps significantly increase the ease with which a car can be summoned, it is plausible that risk averse users will value the certainty of knowing when the car will arrive more than the time spent searching for a cab. As a result, users may be willing to pay a premium for the service. Following this logic through to completion, this would suggest that a decrease in the rate of drunk driving could conceivably be tied to a service like Uber Black, which charges users a price premium over taxis, but mitigates the vast majority of uncertainty.[11] We therefore propose the following:

H1: Implementation of a premium ride-sharing service will be associated with a negative and significant effect on the rate of alcohol related motor vehicle fatalities.

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[9]http://www.mutineermagazine.com/blog/2008/10/the-dui-and-the-failure-of-los-angeles-public-transportation/.

[10]Payment using ride-sharing apps is automatically integrated via credit card, PayPal, or Google Wallet/Apple Pay, etc.

[11]It should be noted that our empirical investigation cannot control for the sequence of steps the user takes before using a ride-sharing service like Uber, for example, if the user installs the app before or after becoming inebriated. However, as the purpose of this research is to quantify the effect of entry on the alcohol related motor vehicle fatality rate, the sequence of steps is outside the scope of this investigation.

Impact of Discount Ride-Sharing Services

The proposition put forth in H1 relies on two assumptions. First, users are willing to pay for taxis in the first place. Second, the utility the user garners from the platform's ease of use and reduced search cost is sufficient to bridge the gap in price between the price point of a taxi service and the price point of the premium service. Given that received research suggests that the price of cabs is often a component in a person's decision to drive under the influence (Jackson and Owens 2011; Nagin and Paternoster 1993; Thurman et al. 1993; Turrisi and Jaccard 1992), these assumptions are questionable. It is therefore possible that premium services will not decrease the drunk driving rate, notably if the platform's decreased search costs/ease of use does not generate excess utility.

While platform theory would suggest that intoxicated driving is the result of individuals being unable to hire a cab (i.e., availability), rational choice theory would suggest that individuals may be able to find drivers, but are electing to drive themselves based on the price point those taxis offer (i.e., cost or a mix of availability and cost). More simply, because of the cost of hiring a taxi, and the perceived cost and probability of being apprehended by the police, individuals are making the decision to drive themselves under the influence. As a result, services such as Uber X, which offer a significant price reduction over traditional taxi cabs (~20% to 30% depending on location) may have a greater negative effect on the drunk driving rate because they both increase the accessibility/ease of use of transportation (much like a premium service) and decrease the gap between the costs of being discovered driving under the influence and the cost of hiring the driver.

Before proposing our hypothesis we make one cautionary note. As mentioned previously, alcohol consumption has been tied inexorably to bias in the perception of risks (Assaad and Exum 2002; Exum 2002). However, this does not imply that, conditional on consuming alcohol, individuals are purely irrational (Jackson and Owens 2011). Recall that, in the focal context, the individual may only be comparing the options of being taken home by a premium car service, a discount car service, or driving themselves. As a result, the comparisons are relatively simplistic and do not require a complex analysis of tradeoffs. Further, as discussed by Paternoster (1989, p. 10): "although rule breaking [i.e., drunk driving] is presumed to be a product of informed choice, the rational choice model does not presume perfect or even optimally accurate informed choice." We would conclude, therefore, that while an individual under the influence of alcohol may not make decisions which appear rational to a sober person, the decision is "substantively rational" to the individual at the time the decision is made (Assaad and Exum 2002; Goldfarb et al. 2009). Therefore, we propose the following:

H2: Implementation of a discount ride-sharing service will be associated with a negative and significant effect on the rate of alcohol related motor vehicle fatalities.

Before moving to our empirical analysis, we note that these two hypotheses (H1 and H2) are not mutually exclusive. To the degree that some individuals may be motivated by costs, and others are willing to pay the premium cost associated with a black car service, it is plausible that both services have an effect. However, the goal of this investigation is to determine the dominant mechanism by which ride-sharing services influence the rate of alcohol related motor vehicle fatalities.

Methodology

Context

As discussed above, we investigate the effect of ride-sharing using Uber, an app-based service operating in more than 58 countries and 300 cities across the globe as of August 2015. Founded in March of 2009 in San Francisco, California, the service offers a platform for owner-operator drivers to find local fares electronically and provide them with transportation to their intended destination. As of December 2014, the firm was valued at over $40 billion with $10 billion in projected 2015 revenues.[12] Originally designed as a black car service, where users would pay a premium to be taken to their destination by a fleet of high end vehicles (e.g., Lincoln Town Cars, Cadillacs), the service now offers a host of transportation options, including car seat services for families, SUV services, and even helicopter services for super luxury passengers that will take them from New York City to the Hamptons. Most pertinent to our research, however, is that the firm introduced the lower price Uber X option in 2012, where drivers could use their personal vehicles to transport patrons at a discounted price.

Figure 1 contains a screen shot of the Uber app. As can be seen, the app provides an estimated time it will take the patron to be picked up, as well as a sliding bar that allows the user to choose which service she wishes to use. Once the vehicle has been requested, the fare is linked to the user's credit card or PayPal account (which is stored in the app) and, after the transaction is complete, the user's account is electronically billed. The app also allows for ratings of both passengers and drivers through a traditional one to five star rating system. It

[12]http://www.businessinsider.com/uber-revenue-projection-in-2015-2014-11.

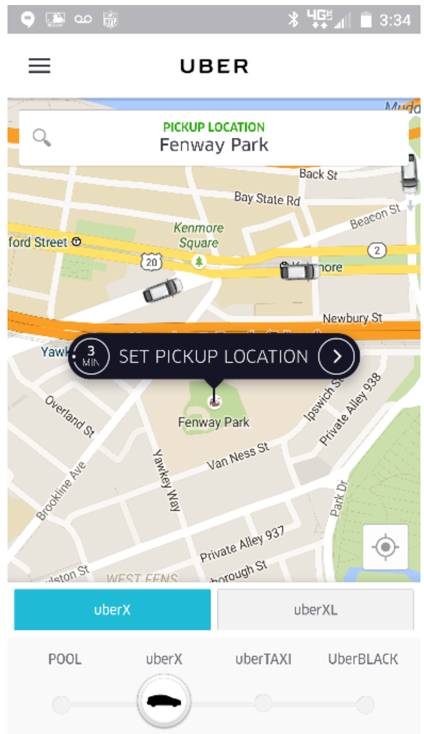


Figure 1. Screen Shot of Uber App

should be noted that the user does not have the option of installing the app for one service (e.g., Uber X) but not the other (e.g., Uber Black). All locally implemented services are available when the app is installed and it is costless to switch between them.

Important for our research question, the two dominant services used, Uber Black (the traditional black car service) and Uber X (the discount service), offer significantly different price points. As discussed previously, Uber Black charges a premium over traditional taxi cab services (~20% to 30%) while Uber X offers a price reduction (~20% to 30% lower than taxis). The services were also rolled out in different cities at different times, and in varying orders (Table 2). Because both of the services offer the platform advantages of increased availability and increased ease of use, but different price points, this setup, as well as the staggered rollout, allows us to determine if either or both services will have an effect.

Data

To empirically estimate the effect of Uber entry on the motor vehicle fatality rate, we create a unique dataset from several sources within the California Highway Patrol's Statewide Integrated Traffic Report System (SWITRS). These data are then combined with entry data which is retrieved directly from the Uber website. This rich dataset gives us information not only on the number of crashes that occurred within each township in the state of California, but the blood alcohol content of the driver (if alcohol was involved), the number of parties involved, weather, speed, and other environmental factors. Although California is a single state, the fact that it is the most populated state in the nation and has had Uber service the longest, makes it ideal for testing our research question. When combined, this dataset comprises 12,420 observations spanning 23 quarters (January 2009–September of 2014) over 540 townships in the state of California.[13] Summary statistics can be found in Table 1 and reveal several interesting pieces of information. First, we see that Uber Black has treated roughly 10% of the sample while Uber X has treated only 7%. Although Uber X has been implemented in more locations, this is ultimately unsurprising given how much longer Uber Black has been available. Second, we note

[13]Townships refer to judicial townships such as incorporated cities and towns within counties in the State of California. No townships in the State of California straddle county lines. These data were collected in November 2014.

Table 1. Summary Statistics and Correlations (N =12,420)

		Mean	Std. Dev.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(1)	ln(Num Deaths)	0.202	0.444								
(2)	Uber X	0.069	0.254	-0.041							
(3)	Uber Black	0.101	0.301	-0.007	0.506						
(4)	ln(Population)	13.636	1.725	0.080	0.241	0.393					
(5)	ln(Median)	10.927	0.230	-0.025	0.008	-0.008	0.322				
(6)	ln(Poverty)	2.808	0.297	0.054	0.098	0.148	0.017	-0.869			
(7)	ln(Elderly)	11.541	1.618	0.072	0.248	0.408	0.994	0.346	-0.026		
(8)	ln(Police)	7.033	1.675	0.080	0.259	0.429	0.978	0.214	0.092	0.976	
(9)	ln(College)	12.304	1.888	0.065	0.230	0.387	0.982	0.458	-0.131	0.987	0.949

that Uber is more likely to enter locations with large populations and college educated populations. Strikingly, the median income of the local area appears not to be a significant correlate, but the number of elderly and law enforcement officials is. This, however, could simply be an artifact of population.

Variable Definitions

Dependent Variable

The dependent variable, *ln(NumDeaths)*, is the natural log of one plus the number of people who were killed in a motor vehicle accident in town *j*[14] during quarter *t* where at least one of the involved parties was under the influence of alcohol (i.e., a blood alcohol content $\geq$ 0.08%).[15] Logging the variable permits us to interpret the effect as a percentage change and resolves a normality concern.[16]

Independent Variables

Our primary independent variables of interest are two dichotomous treatment indicators, *Uber X* and *Uber Black*, which indicate the entry of the Uber Black car service and Uber X service into the county where city *j* is located at time *t*[17] (i.e., the treatment is applied at the county level). A full listing of the counties which receive Uber is available in Table 2. As discussed previously, *Uber Black* is a premium car service which can be hired through the app at a price premium. Further, *Uber X* is a discount service where the driver brings the user to his/her requested location using a personal vehicle. Information regarding Uber entry is retrieved by hand from the Uber website.[18] These variables are coded as 1 during the first *full* quarter the city receives treatment. Finally, to complete the difference in difference estimation, we include time (quarter) and city fixed effects (i.e., a single dummy for each township in California and a single dummy for each quarter).

Empirical Estimation

As mentioned above, we use a difference in difference estimation to establish the effect of Uber entry on the rate of alcohol related motor vehicle fatalities. The primary benefit of such a model is that we can mimic an experimental design using observational data because the treatments (i.e., *Uber X* and *Uber Black*) are applied in different locations at different times (i.e., are geographically and temporally dispersed) as indicated in Table 2. Conceptually, what the difference in difference estimation allows us to do is compare how the trajectory in the number of fatalities changes after the treatment is applied (as compared with control locations). Because our data contain information on both treated and untreated locations, before and after treatment, the net effect

[14]Note that results are consistent when estimated at the week and month level. We use quarters, as opposed to these time periods, to increase the interpretability of the later estimations, viz. the relative time model.

[15]We use the number of deaths, as opposed to the number of crashes or traffic stops, because there is a significant delay in the aggregation of data that does not involve significant injury. At the time of data collection, non-injury collision data were available only through October 2013 (thereby dramatically limiting the variability in the entry of Uber services and the duration of treatment).

[16]Robustness checks with an untransformed DV are performed as well.

[17]Attempts to acquire data on the number of drivers working for Uber in each location were made but denied by the firm.

[18]http://blog.uber.com.

Table 2. Listing of Uber Black and Uber X Treated Counties (Month/Year)

County	Uber Black	Uber X
Riverside		5/2014
San Bernardino		5/2014
Kern		7/2014
Fresno		2/2014
Los Angeles	3/2012	9/2013
Stanislaus		4/2014
Orange	4/2014	9/2013
Palm Springs		9/2013
Sacramento	1/2013	11/2013
San Diego	2/2012	5/2013
San Francisco	6/2010	7/2012
San Luis Obispo		7/2014
Santa Barbara	10/2013	4/2014
Ventura		7/2014

of the treatment is quantified as the difference in the change in the dependent variable across these locations (i.e., the difference in the differences post treatment).

Unsurprisingly, difference in difference estimations have become a popular way to infer causal relationships in economics and social sciences (Bertrand et al. 2004) because *ex ante* differences between the units of observation (i.e., towns) can be controlled for through the use of fixed effects. This allows us to avoid the "endogeneity problems that typically arise when making comparisons between heterogeneous individuals" (Bertrand et al. 2004, p. 250). While these models offer enormous benefits, they are not without their drawbacks. First, there can be serial correlation in the residuals that yield inconsistent standard errors (Bertrand et al. 2004). Second, the model assumes a homogeneous pretreatment trend between treated and control observations (Angrist and Pischke 2008). We deal with each of these concerns in robustness checks below. We estimate the effect using the following equation:

$$y_{it} = M'\theta_1 + H'\eta_1 + R'\gamma_1 + \varepsilon \qquad (1)$$

where y_{jt} represents the log of the number of drivers killed in alcohol related crashes, M is the vector of Uber treatments, H is the vector of time fixed effects, and R is the vector of town fixed effects. ε indicates the error term. $\{\theta, \eta, \gamma\}$ represent the terms to be estimated. To reduce heteroscedasticity concerns, we leverage robust standard errors clustered at the county level. The results are shown in Table 3.

Before discussing the results, we first remediate several well-known concerns with the difference in difference estimation (Angrist and Pischke 2008; Bertrand et al. 2004). Chief among them is the assumption that there is no difference in the pretreatment trend across observations that is not resolved by the location fixed effects. To the extent that randomly distributed factors across the state of California may result in pretreatment heterogeneity, such as nonrandom selection into different counties (i.e., endogenous entry), we replicate our estimations using the relative time model discussed in Greenwood and Agarwal (2016). This is done by creating a second series of time dummies, in addition to the chronological time dummies, which indicate the relative chronological distance between time t and the time Uber is implemented in city j. Intuitively, what this model allows us to do is measure the effect of treatment over time (both before and after the treatment is applied). Econometrically, the primary benefit of this model is that it can determine if a pretreatment trend exists (i.e., a significant difference between treated and untreated counties before treatment) in order to determine if the untreated counties are an acceptable control group. If such a trend exists, it would violate one of the primary assumptions of the model (Angrist and Pischke 2008). We therefore model y_{jt} using the following specification:

$$y_{jt} = \rho'[s_2 * \varphi] + H'\eta_2 + R'\gamma_2 + \varepsilon \qquad (2)$$

As before, y_{jt} represents the log of the number of people killed in alcohol related crashes, H is the vector of time fixed effects, and R is the vector of town fixed effects. ε indicates

Table 3. Time Series OLS Estimations of Uber Entry on Alcohol Related Driving Fatalities

Dependent Variable	(1) ln(Num Deaths)	(2) ln(Num Deaths)	(3) ln(Num Deaths)
Uber X	-0.0369**		-0.0362**
	(0.0180)		(0.0179)
Uber Black		-0.0142	-0.00156
		(0.0153)	(0.0151)
Constant	0.250***	0.250***	0.250***
	(0.0123)	(0.0123)	(0.0123)
Time Fixed Effects	Yes	Yes	Yes
City Fixed Effects	Yes	Yes	Yes
N	12,420	12,420	12,420
R-squared	0.035	0.035	0.035

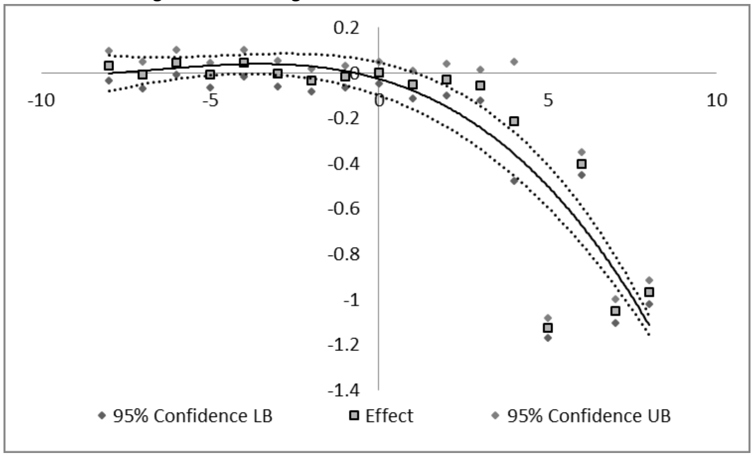
Robust standard errors in parentheses (Clustered on County)
***p < 0.01, **p < 0.05, *p < 0.1

Table 4. Relative Time Model of Uber Entry on Alcohol Related Motor Vehicle Deaths

Dependent Variable	(1) ln(Num Deaths)	(2) ln(Num Deaths)
Model	**Uber X**	**Uber Black**
Rel Time $_{(t-4)}$	0.0435	-0.0269
	(0.0280)	(0.0346)
Rel Time $_{(t-3)}$	-0.00199	0.0141
	(0.0270)	(0.0360)
Rel Time $_{(t-2)}$	-0.0314	-0.0112
	(0.0274)	(0.0361)
Rel Time $_{(t-1)}$	-0.0159	0.00498
	(0.0272)	(0.0361)
Rel Time $_{(t0)}$	Omitted Base Case	
Rel Time$_{(t+1)}$	-0.0494*	-0.0155
	(0.0292)	(0.0346)
Rel Time$_{(t+2)}$	-0.0301	0.0315
	(0.0312)	(0.0414)
Rel Time$_{(t+3)}$	-0.0539*	-0.0205
	(0.0314)	(0.0372)
Rel Time$_{(t+4)}$	-0.214***	-0.0353
	(0.0705)	(0.0402)
Rel Time$_{(t+5)}$	-1.124***	-0.0277
	(0.300)	(0.0390)
Constant	0.216***	0.251***
	(0.0185)	(0.0158)
Time Fixed Effects	Yes	Yes
City Fixed Effects	Yes	Yes
N	12,420	12,420
R-squared	0.041	0.041

Robust standard errors in parentheses (Clustered on county)
***p < 0.01, **p < 0.05, *p < 0.1

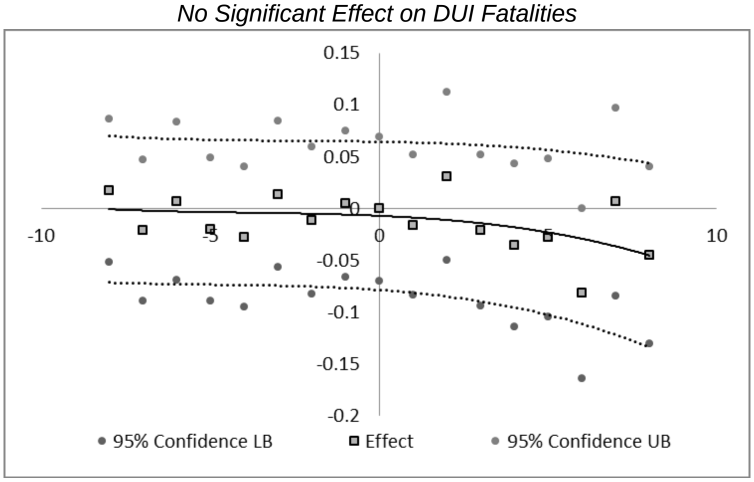
Negative and Significant Effect on DUI Fatalities



Y-Axis – Logged Fatality Rate / X- Axis – Time (Quarters)
Solid line is trend of Uber X relative time coefficients (third degree polynomial); dotted lines are trend of 95% confidence intervals (third degree polynomial).

Figure 2. Effect of Uber X

No Significant Effect on DUI Fatalities



Y-Axis – Logged Fatality Rate / X-Axis – Time (Quarters)
Solid line is trend of Uber Black relative time coefficients (third degree polynomial); dotted lines are trend of 95% confidence intervals (third degree polynomial)

Figure 3. Effect of Uber Black

the error term. s_2 is a dichotomous variable which indicates whether or not Uber will ever affect city *j* during the study and the vector $\{\rho\}$ contains the relative time parameters to be estimated (i.e., the chronological distance between time *t* and the time the Uber service will be implemented in city *j*). Standard errors are robust and clustered at the county level. The results are shown in Table 4. Graphical representations are presented in Figures 2 and 3.

Results

With respect to our independent variables of interest, *Uber X* and *Uber Black*, the results are intriguing. While results suggest that introducing *Uber X* (Columns 1 and 3 of Table 3) into a city has a significant dampening effect on the number of alcohol related driving fatalities, the introduction of *Uber Black* (Columns 2 and 3) does not.[19] All else equal, this suggests several key pieces of information. First, it suggests that previous within-city investigations of the effect of Uber entry may have been overstated (e.g., Badger 2014). Second, it suggests that a coupling of cost, availability, and ease of use is the driving force behind the decrease in DUI related deaths, indicating that patrons are unwilling to pay a price premium for the *Uber Black* service, even in the short term. Econometrically, these results suggest an average decrease in alcohol related fatalities of 3.6% in locations treated by *Uber X* in the state of California.

The results from the relative time model (Table 4) further underscore these findings. We first note that none of the pretreatment time dummies (i.e., Rel Time$_{(t-x)}$) are significant, thereby allowing us to validate the assumptions of the difference in difference model (Angrist and Pischke 2008; Bertrand et al. 2004).[20] The absence of significance suggests that there is no significant heterogeneity, pretreatment, across cities that receive the Uber treatment, and those that do not, which has not been accounted for. Second, we see that while an effect manifests almost immediately for *Uber X*, it does not become stable until roughly nine months after treatment. This further underscores the absence of an effect for *Uber Black*, even in the long term. Finally, the fact that the stable effect takes a significant period of time to manifest casts further doubt on prior investigations that claim an effect appears in weeks or even days.

[19]The insignificant effect for *Uber Black* persists if all observations where *Uber X* has been implemented are excluded. We thank the anonymous reviewer for this suggestion.

[20]Note that the other relative time dummies (those greater than four quarters pretreatment and five quarters post treatment) are included in the model and omitted in the interest of space. Full results are available upon request.

Figures 2 and 3 corroborate these findings. In both figures, polynomial trend lines have been superimposed on the estimates and we see no significant pretreatment trend, indicating no unaccounted for heterogeneity between the treated and untreated locations. Further, in Figure 3 (*Uber Black*), we see no significant post treatment change, thereby underscoring the lack of significant effect for the premium service. Finally, in Figure 2 (*Uber X*), we see a minimal initial trend which bends sharply down roughly 9 to 12 months after implementation. Taken in sum, the results indicate a significant effect for *Uber X*, and the absence of an effect for *Uber Black*.

Robustness Checks

Selection Model

While our preliminary results indicate the absence of a significant pretreatment trend, the assumption that Uber entry into varying locations is purely exogenous remains questionable. To further test this assumption, we include a robust set of controls which may influence the decision by Uber executives to enter local markets. Specifically, to account for population level factors (e.g., age, education, population, wealth) that might influence the entry of Uber into a local area, we combine the existing dataset with information from the U.S. Department of Health and Human Services' Area Resource File and the Federal Bureau of Investigation's Law Enforcement Officers Killed and Assaulted dataset.

The resulting dataset contains three additional sets of controls. First, because the population in locales may influence entry, we include the log of the local population (to control for the size of the market), median income (to control for the wealth of the market), and number of college graduates (to control for the market of likely users). Second, to control for the portion of the extant population unlikely to leverage the Uber service, we include the log of the population living in poverty, who have limited disposable income and are less likely to use cutting edge IT (DiMaggio et al. 2004), and those over the age of 65 (i.e., the elderly), who are also likely to suffer from digital inequalities (Warschauer 2004). Third, as the expansion of Uber has been contentious legally, we include the log of the number of individuals within the county working in law enforcement. We then replicate the estimation of equations 1 and 2 with these controls included. The results are available in Tables 5 and 6.

Before considering the effect of *Uber Black* and *Uber X* in these estimations we first consider the effects from our control variables. Interestingly, we see that a change in the other controls does not significantly influence the number of motor vehicle fatalities involving alcohol during the period of inves-

Table 5. OLS Estimations of Uber Entry on Alcohol Related Driving Fatalities including Controls

Dependent Variable	(1) ln(Num Deaths)	(2) ln(Num Deaths)	(3) ln(Num Deaths)
Uber X	-0.0321**		-0.0324**
	(0.0141)		(0.0153)
Uber Black		-0.0105	0.000716
		(0.0125)	(0.0136)
ln(Population)	-75.04	-27.13	-76.68
	(664.4)	(664.8)	(665.1)
ln(Median)	0.0163	0.0351	0.0160
	(0.145)	(0.145)	(0.146)
ln(Poverty)	-0.108	-0.111	-0.108
	(0.0707)	(0.0709)	(0.0709)
ln(Elderly)	0.162	0.166	0.163
	(0.171)	(0.174)	(0.174)
ln(Police)	0.000451	0.000353	0.000559
	(0.0350)	(0.0351)	(0.0351)
ln(College)	74.68	26.71	76.31
	(664.5)	(664.9)	(665.2)
Constant	103.0	39.66	105.1
	(883.8)	(884.4)	(884.8)
Time Fixed Effects	Yes	Yes	Yes
City Fixed Effects	Yes	Yes	Yes
N	12,420	12,420	12,420
R-squared	0.036	0.035	0.036

Robust standard errors in parentheses (Clustered on county)
***p < 0.01, **p < 0.05, *p < 0.1

tigation. This further underscores the fact that the fixed effects for the local municipalities are effectively controlling for across-city heterogeneity in the estimations. Recall that, as there are time fixed effects in the estimations as well, these variables should be interpreted as changes in the independent variable. Moreover, results from the primary variables of interest remain consistent insofar as we see a negative and significant effect of *Uber X* and no significant effect of *Uber Black.*

Count Model

Although our initial regressions have shown consistency across several specifications, other potentially confounding problems remain. The first is that the distribution of the dependent variable is not strictly Gaussian, despite being logged. To the extent that this violates one of the basic assumptions of the Gauss-Markov theorem, because the distribution of the error term will not be Gaussian, it may lead to inconsistent estimations of the results. To remedy this concern, we reestimate our results using a non-transformed dependent variable to increase our confidence in the baseline estimations.

Empirically, we perform these regressions using two different estimators. The first is a traditional OLS. The second is a Poisson quasi-maximum likelihood estimator (QMLE) (Simcoe 2007), which has been used extensively in recent work (Azoulay et al. 2010; Greenwood and Gopal 2015). We use the QMLE, in lieu of other options like the Poisson or negative binomial estimators, for several reasons. First, it allows for the creation of robust standard errors when the distribution of the dependent variable is not negative binomial or Poisson (Azoulay et al. 2010). Second, because the QMLE is not constrained by the same assumptions as the negative binomial or Poisson estimators (i.e., that the conditional variance of y given x is equal to the conditional mean), the assumptions of the model are not violated if the distribution of the dependent variable is not negative binomial or Poisson. A full description of the estimator, as well as its derivation, can be found in Wooldridge (1997). As before, we replicate the estimation of both equation 1 and 2 using the non-transformed DV. The results are shown in Tables 7 and 8.

Table 6. Relative Time Model of Uber Entry on Alcohol Related Motor Vehicle Deaths

Dependent Variable	(1) ln(Num Deaths)	(2) ln(Num Deaths)
Model	**Uber X**	**Uber Black**
Rel Time $_{(t-4)}$	0.0428	-0.0296
	(0.0280)	(0.0348)
Rel Time $_{(t-3)}$	-0.00251	0.0116
	(0.0270)	(0.0361)
Rel Time $_{(t-2)}$	-0.0316	-0.0138
	(0.0274)	(0.0362)
Rel Time $_{(t-1)}$	-0.0160	0.00491
	(0.0272)	(0.0361)
Rel Time $_{(t0)}$	Omitted Base Case	
Rel Time$_{(t+1)}$	-0.0487*	-0.0154
	(0.0292)	(0.0346)
Rel Time$_{(t+2)}$	-0.0291	0.0318
	(0.0312)	(0.0414)
Rel Time$_{(t+3)}$	-0.0530*	-0.0200
	(0.0314)	(0.0373)
Rel Time$_{(t+4)}$	-0.212***	-0.0346
	(0.0705)	(0.0402)
Rel Time$_{(t+5)}$	-1.114***	-0.0270
	(0.301)	(0.0390)
ln(Population)	-242.4	-34.69
	(665.4)	(321.4)
ln(Median)	0.00978	0.0495
	(0.148)	(0.145)
ln(Poverty)	-0.104	-0.0939
	(0.0713)	(0.0658)
ln(Elderly)	0.122	0.128
	(0.173)	(0.190)
ln(Police)	-0.00972	-0.00628
	(0.0351)	(0.0306)
ln(College)	242.2	34.27
	(665.5)	(321.6)
Constant	324.4	49.95
	(885.1)	(425.9)
Time Fixed Effects	Yes	Yes
City Fixed Effects	Yes	Yes
N	12,420	12,420
R-squared	0.042	0.041

Robust standard errors in parentheses (Clustered on county)
***p < 0.01, **p < 0.05, *p < 0.1

Table 7. Count Model Estimates of Uber Entry on Alcohol Related Motor Vehicle Deaths

Dependent Variable	(1) Num Deaths	(2) Num Deaths	(3) Num Deaths	(4) Num Deaths	(5) Num Deaths	(6) Num Deaths
Estimator	OLS	OLS	OLS	QMLE	QMLE	QMLE
Uber X	-0.142*		-0.126**	-0.0345		-0.00921
	(0.0726)		(0.0534)	(0.0902)		(0.0950)
Uber Black		-0.0931	-0.0493		-0.0576	-0.0556
		(0.0839)	(0.0766)		(0.0623)	(0.0656)
Constant	18.36	0.546***	0.546***			
	(11.46)	(0.0350)	(0.0350)			
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
City Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	12,420	12,420	12,420	9,200	9,200	9,200
R-squared	0.030	0.029	0.030			
χ-squared				325.89	326.56	326.55

Robust standard errors in parentheses (Clustered on County)
***p < 0.01, **p < 0.05, *p < 0.1

The results in Table 7 add interesting nuance to the previous estimations. While the effect of *Uber Black* remains insignificant using both estimators, the effect of *Uber X* is significant only using the OLS. However, when considering the results from Table 8, the reason behind the insignificant result becomes clear. While the log relative time model (Tables 4 and 6) and the OLS count model (Table 8, Column 2) both suggest the effect becomes consistently significant after nine months, the QMLE suggests that the effect takes significantly longer to manifest (five quarters). All else equal, this suggests that the delay in the time for the effect to manifest (i.e., the initially insignificant effect) is masking the later significant effect. Furthermore, both models show an intermittent effect for *Uber Black* (Columns 2 and 4), although the rarity with which the effect appears makes any conclusion being drawn from the estimations dubious.

Introduction of Other Ride-Sharing Services

The next concern we address is the fact that other ride-sharing services, which were emerging contemporaneously to Uber, may be biasing the estimations. Inasmuch as it is difficult to tell if these omitted factors (which would not have been resolved by the town fixed effects because their presence is heterogeneous over time) are actually driving the observed effect, an omitted variable bias may exist. We therefore gather data on the implementation of Uber's four major competitors: Lyft, Sidecar, Flywheel (previously Cabulous), and Curb (previously Taxi Magic),[21] exclude all observations when one of these services is operating, and replicate our estimations. We elect to exclude the observations, as opposed to controlling for them with dummies, for two reasons. First, due to the similarity in the implementation patterns between the services, the inclusion of controls creates significant multicollinearity problems. Second, as the ride-sharing market continues to witness new competitors entering, the model would *still* be improperly specified unless every competitor's exact implementation schedule could be determined. Results are in Table 9 and remain consistent with our earlier findings. Entry of the *Uber X* service is correlated with a significant decrease in the rate of fatalities and *Uber Black* is not.[22]

Coarsened Exact Match

Our next concern is that while the controls and fixed effects account for much of the unobserved heterogeneity between treated and untreated groups, insofar as the controls in Tables 5 and 6 yield no significant effect on the dependent variable, it is plausible that the untreated cities are not a representative counterfactual for treated cities.[23] To resolve this we execute a coarsened exact matching (CEM) procedure to limit the *ex ante* differences between the treatment and control samples (Blackwell et al. 2009, Iacus et al. 2012). Principally, the CEM allows us to match explicitly on observable characteristics and simultaneously limit the differences between the

[21] It is worth noting that many taxi firms have recently developed their own hailing apps. However, we were unable to identify any instances where one of these apps entered a market before one of the Uber competitors. The same is true of more recent emerging competitors like Hailo.

[22] We thank the anonymous reviewer for suggesting this test.

[23] Recall that the level of the observation is the city but the treatment is applied at the county level.

Table 8. Count Based Relative Time Model of Uber Entry on Alcohol Related Motor Vehicle Deaths

Dependent Variable	(1) Num Deaths	(2) Num Deaths	(3) Num Deaths	(4) Num Deaths
Model	Uber X	Uber Black	Uber X	Uber Black
Estimator	OLS	OLS	QMLE	QMLE
Rel Time $_{(t-4)}$	0.158**	-0.0874	0.0438	-0.203
	(0.0715)	(0.0742)	(0.142)	(0.147)
Rel Time $_{(t-3)}$	0.0108	0.0387	-0.160	0.0134
	(0.0690)	(0.0693)	(0.158)	(0.124)
Rel Time $_{(t-2)}$	-0.0435	-0.00880	-0.228	-0.0683
	(0.0698)	(0.0706)	(0.145)	(0.135)
Rel Time $_{(t-1)}$	-0.0481	-0.00129	-0.211*	-0.0437
	(0.0696)	(0.0814)	(0.126)	(0.154)
Rel Time $_{(t0)}$	Omitted Category			
Rel Time $_{(t+1)}$	-0.118	-0.0401	-0.393**	-0.147
	(0.0745)	(0.0933)	(0.175)	(0.186)
Rel Time $_{(t+2)}$	-0.124	0.108	-0.266	0.124
	(0.0796)	(0.0910)	(0.220)	(0.148)
Rel Time $_{(t+3)}$	-0.155*	-0.122	-0.450	-0.168
	(0.0800)	(0.141)	(0.351)	(0.226)
Rel Time $_{(t+4)}$	-0.660***	-0.225*	-0.580	-0.354*
	(0.180)	(0.137)	(0.572)	(0.194)
Rel Time $_{(t+5)}$	-2.723***	-0.125	-14.84***	-0.115
	(0.767)	(0.119)	(1.023)	(0.185)
Rel Time $_{(t+6)}$	-1.650**	-0.287**	-0.761***	-0.467***
	(0.768)	(0.114)	(0.146)	(0.168)
Rel Time $_{(t+7)}$	-2.580***	-0.0928	-14.26***	-0.00810
	(0.768)	(0.149)	(1.027)	(0.225)
Rel Time $_{(t+8)}$	-2.433***	-0.242	-11.96***	-0.477
	(0.768)	(0.195)	(1.118)	(0.337)
	0.414***	0.541***		
Constant	(0.0473)	(0.0372)		
Time Fixed Effects	Yes	Yes	Yes	Yes
City Fixed Effects	Yes	Yes	Yes	Yes
Observations	12,420	12,420	9,200	9,200
R-squared	0.037	0.036		
χ-squared			353.04	350.28

Robust standard errors in parentheses (Clustered on County)
***p < 0.01, **p < 0.05, *p < 0.1

Table 9. Time Series OLS Estimations of Uber Entry on Alcohol Related Driving Fatalities Observations with Other Ride-Sharing Services Omitted

Dependent Variable	(1) ln(Num Deaths)	(2) ln(Num Deaths)	(3) ln(Num Deaths)
Uber X	-0.0578***		-0.0588***
	(0.0174)		(0.0161)
Uber Black		-0.000293	0.0115
		(0.0106)	(0.0132)
Constant	0.214***	0.211***	0.214***
	(0.0126)	(0.0131)	(0.0124)
Time Fixed Effects	Yes	Yes	Yes
City Fixed Effects	Yes	Yes	Yes
N	7,476	7,476	7,476
R-squared	0.031	0.030	0.031

Robust standard errors in parentheses (Clustered on county)
***p < 0.01, **p < 0.05, *p < 0.1

Table 10. Coarsened Exact Match OLS of Uber Entry on Alcohol Related Motor Vehicle Deaths

Dependent Variable	(1) ln(Num Deaths)	(2) ln(Num Deaths)	(3) ln(Num Deaths)
Uber X	-0.0559**		-0.0566**
	(0.0236)		(0.0234)
Uber Black		-0.0542	-0.0567
		(0.0550)	(0.0547)
Constant	0.186***	0.216***	0.217***
	(0.0194)	(0.0355)	(0.0354)
Time Fixed Effects	Yes	Yes	Yes
City Fixed Effects	Yes	Yes	Yes
Observations	2,037	2,037	2,037
R-squared	0.056	0.054	0.057

Robust standard errors in parentheses (Clustered on county)
***p < 0.01, **p < 0.05, *p < 0.1

two groups from both a multivariate and univariate perspective. To the extent that this increases the homogeneity between the two samples, it increases the strength of the causal claims from change in the treatment (Overby and Forman 2015), that is, Uber entry. To execute this procedure we match on three different criteria: the population of the city as determined by the SWITRS dataset, per capita income of the city, and current period.[24] We then replicate the analysis from Table 3. The results (Table 10) indicate a strong and significant effect of *Uber X* entry, and an insignificant effect of *Uber Black* entry. Moreover, we note that the size of the *Uber X* coefficient is significantly larger in this far more constrained model (more than 1.5 times the size).

Data Generating Process

As mentioned previously, we eschew the use of alcohol related crashes as the dependent variable for this study because of the significant time delay in incorporating non-injury data into the SWITRS dataset. However, to the extent that initial under-reporting or delayed reporting may occur, we must ensure that the data generating process for fatal crashes is not biased as well. Put another way, insofar as there *may* be a delay in the acquisition of fatality data, we must ensure that any potential delay is not correlated with the

[24]The inclusion of additional matching variables reduced the size of the sample, and therefore the power of the estimations, to a point where robust conclusions could not be drawn from the data.

Table 11. Time Constrained Estimate of Uber Entry on Alcohol Related Motor Vehicle Deaths Final Year of Dataset Omitted From Estimation

Dependent Variable	(1) ln(Num Deaths)	(2) ln(Num Deaths)	(3) ln(Num Deaths)
Uber X	-0.120***		-0.118***
	(0.0225)		(0.0228)
Uber Black		-0.00660	-0.00306
		(0.0121)	(0.00923)
Constant	0.250***	0.250***	0.250***
	(0.0110)	(0.0112)	(0.0111)
Time Fixed Effects	Yes	Yes	Yes
City Fixed Effects	Yes	Yes	Yes
Observations	10,260	10,260	10,260
R-squared	0.009	0.009	0.009

Robust standard errors in parentheses (Clustered on county)
***p < 0.01, **p < 0.05, *p < 0.1

independent variables of interest, viz. Uber entry. We therefore remove the final year (four quarters) of data from our analysis and replicate our estimations. The results, shown in Table 11, remain consistent with previous estimations. Strikingly, as with the CEM model, we note that the effect of *Uber X* is significantly larger in this more constrained estimate.

Diagnosis of Standard Errors

The final set of robustness tests we run relate to an examination of the standard errors. As discussed by Bertrand et al. (2004), apart from heterogeneous pretreatment trends, one of the most significant problems with difference in difference estimations is serial correlation within the residuals.

Random Treatment Model

The first diagnostic test we run is a random implementation model to determine the probability of the observed effect occurring purely by chance. Pragmatically, this test allows us to do two things. First, placebo tests can cleanly identify if correlation within the county-quarter is unaccounted for (Bertrand et al. 2004; Donald and Lang 2007). Second, to the extent that significant changes in the motor vehicle fatality rate may be occurring in untreated locations, or the effect of the Uber treatment is substantially driven by a single location, this model provides a check against outliers.

To execute this model, we take two approaches. In the first we randomly apply the *Uber X* treatment to 862 city-quarters (1,249 for *Uber Black*). We then regress the log of the number of alcohol related motor vehicle fatalities upon this "pseudo" treatment and store the coefficient. This analysis is then replicated 1,000 times and the draw of the actual treatment is compared against the mean and standard deviation of the pseudo-treatments. In the second approach, we apply the pseudo treatment only to cities that eventually receive the *Uber* treatment. The results are shown in Table 12. As can be seen, the probability of a similar coefficient occurring purely by chance is exceptionally likely for *Uber Black* (which is unsurprising given the insignificant coefficient in the majority of the estimated models). However, in both random treatments (both purely random and random within treated cities), the probability of a similarly sized coefficient appearing purely by chance for *Uber X* is exceptionally low ($p < 0.001$). Finally, in all models the estimated placebo coefficient is insignificantly different from zero, suggesting correlation within the county-quarter has been accounted for.

Direct Tests

In addition to the placebo test, Bertrand et al. (2004) suggest two additional tests. The first is to block bootstrap the standard errors, as opposed to clustering them, in the manner discussed by Efron and Tibshirani (1994). As with the placebo test, the block bootstrap provides a reliable check to ensure that the standard errors are well behaved. The results are shown in Table 13 and remain consistent.

The second suggested test is a direct examination of the autocorrelation coefficients of the residuals. Intuitively, what this test allows us to do is determine, first hand, if there is a signi-

Table 12. Output of Random Implementation Model

Sample	Random Implementation		Random Implementation in Treated	
	Uber X	Uber Black	Uber X	Uber Black
μ of Random β	0.00215	-0.00027	-0.00041	-0.00039
σ Random β	0.01060	0.00897	0.01028	0.00856
Estimated β	-0.0362	-0.00156	-0.0362	-0.00156
Replications	1000	1000	1000	1000
Z-Score	-3.619029	-0.144076	-3.481857	-0.137099
P-Value	p<0.001	0.44272	p<0.001	0.44548

Table 13. Block Bootstrapped Standard Errors of Uber Entry on Alcohol Related Motor Vehicle Deaths

Dependent Variable	(1) ln(Num Deaths)	(2) ln(Num Deaths)	(3) ln(Num Deaths)
Uber X	-0.03691**		-0.03621**
	(0.0139)		(0.0151)
Uber Black		-0.1417	-0.00156
		(0.0122)	-0.0133
Constant	0.5805***	0.5805***	0.5805***
	(0.06230)	(0.06230)	(0.06230)
Time Fixed Effects	Yes	Yes	Yes
City Fixed Effects	Yes	Yes	Yes
N	12,420	12,420	12,420
R-squared	0.011	0.011	0.011

Robust standard errors in parentheses (Clustered on county)
***p < 0.01, **p < 0.05, *p < 0.1

Table 14. Examination of the Auto-Correlation Coefficients of Residuals

Dependent Variable	(1) $Residual_{jt}$	(2) $Residual_{jt}$
$Residual_{jt-1}$	-0.00967	-0.0101
	(0.00921)	(0.00940)
$Residual_{jt-2}$		-0.0123
		(0.00939)
Constant	5.89e-11	0
	(0.00262)	(0.00268)
Observations	11,880	11,340
R-squared	0.000	0.000

Robust standard errors in parentheses (Clustered on county)
***p < 0.01, **p < 0.05, *p < 0.1

ficant correlation between the residual y_{jt} and y_{jt+1}. To perform this test, we replicate our regressions using the fully specified model and extract the residuals. We then regress the residual from y_{jt} on y_{jt-1} and then again on y_{jt-1} and y_{jt-2}. The results are shown in Table 14. Both the first and second order residuals are insignificant.[25]

Empirical Extensions

While our empirical estimations thus far suggest that the coupling of availability, ease of use, and cost considerations are of the utmost importance when consumers avoid operating under the influence,it is worth considering the boundary conditions of this effect (i.e., when the strength of the effect is intensified or attenuated) To explore these conditions we consider two potential moderators to demand: days of the year when demand is likely to spike, thereby causing Uber's surge pricing to be put into effect, and the size of the local population, which should correlate with the steady state demand in the local market. Further, we examine the effect of Uber entry on *non-alcohol related* driving fatalities.

Times of Likely Surge Pricing

The first empirical extension we investigate is whether or not the effect of Uber still manifests during spikes in demand. To the extent that spikes in demand will cause Uber's surge pricing[26] to be put into effect, thereby raising the price of hiring either an Uber X or Uber Black, this is an important extension to conduct because of the dependence of our results on low cost options. If, for example, the effect of Uber intensified or stayed constant during periods of higher demand, this would suggest that the lack of supply of taxis is the dominant mechanism by which the drop in alcohol related motor vehicle fatalities occurs. Alternatively, if the effect shrinks during spikes in demand, when cost concomitantly rises due to the surge pricing, but quality, ease of use, and availability remain constant, this would suggest that cost is indeed the driving mechanism because Ubers, of either type, are no longer being hired.

To estimate the effect of Uber entry during these times, we recalculate the dependent variable as the number of alcohol related motor vehicle deaths during weekends (i.e., when drinking is more prevalent) and major U.S. holidays which involve drinking,[27] thereby resulting in a likely increased load on ride-sharing services. We then reestimate equation 1. The results in Table 15 indicate no significant effect of Uber entry on the number of fatal accidents during these times. Taken in sum, this underscores the importance of cost, coupled with availability, as the driving factor in influencing the alcohol related motor vehicle fatality rate.

Population

Our next empirical extension relates to the size of the local population. To the extent that population will affect the steady state demand, and by extension the supply of Ubers in the local area, it is reasonable to assume that markets will exist in a steady state equilibrium. While this would suggest that there would be no difference in the per capita effect of Uber, by city population size, the opposite may also be true. For example, the effect in larger cities may be smaller because larger cities often have more established alternative transportation options, viz. public transportation. Alternatively, it is also possible that the effect would be larger in large cities because smaller townships have too small a population to garner significant attention from Uber drivers. As an *a priori* expectation of the effect is absent, and an understanding of how different locations are affected differently paints a richer picture of how the sharing economy influences public welfare, we allow our empirical analysis to guide us.

To investigate in which cities Uber has a stronger or weaker effect, we trichotomize the population data from the SWITRS dataset into three groups: small cities (which serves as the base case), medium-sized cities (those with populations greater than 50,000 people and less than 250,000 people), and large cities (those with populations greater than 250,000 people). We then interact these new variables with the Uber treatment and replicate our estimations.[28] The results are shown in Table 16. Strikingly, these findings suggest several interesting differences. First, we see that as the population of

[25]In unreported tests we also examine bootstrapped standard errors with 10,000 replications as well as AR(1) and AR(2) models. Results remain consistent and are available upon request.

We thank the senior editor for suggesting these additional diagnostics for the standard errors.

[26]A full explanation of surge pricing from Uber can be found at https://help.uber.com/.

[27]The full list of holidays includes the Fourth of July, Memorial Day, Labor Day, Cinco de Mayo, Thanksgiving, the day before Thanksgiving, Christmas, Christmas Eve, Halloween, Easter, New Year's Eve, and Superbowl Sunday. The source of these data is http://content.time.com/time/specials/packages/article/0,28804,1986906_1986905_1986891,00.html.

[28]Note that the base effect (i.e., the noninteracted term) of the newly created variables will not be estimated because the city fixed effect perfectly predicts the base effect.

Table 15. Estimations of Uber Entry on Alcohol Related Deaths on High Demand Days High Demand Days Defined as Weekends and Drinking Holidays

Dependent Variable	(1) ln(Num Deaths)	(2) ln(Num Deaths)	(3) ln(Num Deaths)
Uber X	-0.00240		-0.00628
	(0.0110)		(0.0120)
Uber Black		0.00640	0.00859
		(0.00893)	(0.00973)
Constant	0.0922***	0.0922***	0.0922***
	(0.00892)	(0.00892)	(0.00892)
Time Fixed Effects	Yes	Yes	Yes
City Fixed Effects	Yes	Yes	Yes
N	12,420	12,420	12,420
R-squared	0.011	0.011	0.011

Robust standard errors in parentheses (Clustered on county)
***p < 0.01, **p < 0.05, *p < 0.1

Table 16. OLS Estimations of Uber Entry Interacted with Population Medium City Indicates Population 50,000 – 250,000; Large City Indicates Population ≥ 250,000

Dependent Variable	(1) ln(Num Deaths)	(2) ln(Num Deaths)	(3) ln(Num Deaths)
Uber X	0.00745		0.00404
	(0.0166)		(0.0174)
Uber X * Medium City	-0.164***		-0.166***
	(0.0534)		(0.0552)
Uber X * Large City	-0.523***		-0.426***
	(0.111)		(0.115)
Uber Black		0.0128	0.00709
		(0.0145)	(0.0151)
Uber Black * Medium City		-0.0745*	0.00401
		(0.0427)	(0.0412)
Uber Black * Large City		-0.411***	-0.196*
		(0.0953)	(0.104)
Constant	0.250***	0.250***	0.250***
	(0.0123)	(0.0123)	(0.0123)
Time Fixed Effects	Yes	Yes	Yes
City Fixed Effects	Yes	Yes	Yes
N	12,420	12,420	12,420
R-squared	0.044	0.039	0.045

Robust standard errors in parentheses (Clustered on county)
***p < 0.01, **p < 0.05, *p < 0.1

Table 17. OLS Estimations of Uber Entry on Log of All Driving Fatalities

Dependent Variable	(1) ln(All Deaths)	(2) ln(All Deaths)	(3) ln(All Deaths)
Uber X	-0.0397		-0.0351
	(0.0256)		(0.0267)
Uber Black		-0.0223	-0.0101
		(0.0195)	(0.0182)
Constant	0.444***	0.444***	0.444***
	(0.0159)	(0.0162)	(0.0162)
Time Fixed Effects	Yes	Yes	Yes
City Fixed Effects	Yes	Yes	Yes
N	12,420	12,420	12,420
R-squared	0.061	0.061	0.061

Robust standard errors in parentheses (Clustered on county)
***p < 0.01, **p < 0.05, *p < 0.1

local cities increases, there is a concomitant rise in the effect of Uber entry. Moreover, we see that a significant effect also manifests for *Uber Black* (although the size of the effect declines precipitously in the presence of *Uber X* (Column 3)). Taken in sum, these results suggest a significantly stronger negative effect on the alcohol related fatality rate in larger cities than in smaller cities.

Overall Fatalities

While our examination has provided compelling evidence both for the effect of ride-sharing services on the alcohol related motor vehicle fatality rate, as well as the boundary conditions of such an effect, it is plausible that the introduction of Uber into local markets has undesirable, unintended, consequences as well. For example, Uber entering a market may result in an increased number of vehicles on the road at any given time. To the extent that congestion is a major cause of accidents, it is possible that the Uber service is decreasing the number of alcohol related fatalities, but *increasing* the overall number of fatalities. We therefore recalculate our dependent variable as the log (+1) of all motor vehicle fatalities[29] and replicate our estimations. The results are shown in Table 17 and indicate no significant correlation.

[29]The results are consistent when operationalizing the DV as exclusively sober deaths. We thank the anonymous reviewer and the AE for the suggesting these tests.

Discussion and Conclusion

In this work, we investigated the effect of ride-sharing services on the rate of alcohol related motor vehicle fatalities. While intuition would suggest that the number of alcohol related crashes should decrease after alternate transportation options enter a market, we argued that the willingness to pay for such services and the necessary conditions for an effect to manifest are still unknown. On one hand, it is plausible that an effect would manifest purely as a result of the increased availability of driving services. On the other hand, it is equally plausible that cost and availability are both factors preventing individuals from hiring cabs. To the extent that rational choice theory (Clarke and Cornish 1985, Cornish and Clarke 2014) suggests that most decisions to engage in illegal activity are a function of the reward, potential penalty, and the probability of being apprehended by law enforcement, it is possible that these deaths are a result of "reasoned" choice on the part of consumers. Results suggest that the entry of lower priced options, viz. Uber X, has a significant effect on the number of fatalities, indicating that price, conditional upon sufficient availability of the service, is the main barrier to reducing DUIs in many jurisdictions. This finding is corroborated by the lack of effect when surge pricing is likely in effect (i.e., during weekends and drinking holidays). Furthermore, results suggest a significantly stronger effect in large cities and no effect on the overall (i.e., sober) fatality rate.

Econometrically, findings indicate that the entrance of Uber X results in a 3.6% to 5.6% decrease in the rate of motor vehicle fatalities per quarter in the state of California. With more than 13,000 deaths occurring nationally each year due to alcohol related crashes, at a cost of $37 billion,[30] results indicate that a complete implementation of Uber X would save roughly 500 lives annually and create a public welfare net of over $1.3 billion to American taxpayers. Moreover, with costs to the individual (e.g. court costs, insurance rate increases, loss of income) totaling between $5,000 and $12,000 for the first DUI offence,[31] significant welfare accrues to the individual by leveraging these services.

Theoretically, these results have many implications for the sharing economy. To the degree that vendors such as AirBnB, Uber, and Lyft have been proposed as solutions to many market failures, our work provides cautionary evidence that consumers may continue to use established vendors when prices increase. As a result, while lower priced hotels and car services may be usurped by these emerging business models, minimal evidence exists to suggest that premium vendors will be displaced (as evidenced by the absence of a stable and consistent effect for the premium Uber Black service).

These findings also have direct implications for policy makers and regulators by informing the ongoing debate regarding the legality of ride-sharing services. Although the results of this investigation cannot speak to public welfare losses which may result from improper vehicle handling or safety on the part of consumers (although our results do not indicate an effect on sober deaths), they provide important insights into the potential benefits of the sharing economy and inform licensed livery services of the necessary steps which need to be taken to compete with these nascent ventures. For policy makers, by allowing ride-sharing services to operate, a nontrivial effect (i.e., decreased mortality) is realized by constituents. For the managers and regulators of the taxi industry, two notable implications exist as well. First, these results underscore the punitive effects of barriers to entry. If limited pools of medallions, onerous insurance and licensing procedures, and other forms of regulation are making it impossible for existing livery services to compete, then there are serious implications which need to be balanced against these regulations. Second, these results highlight what cab companies need to do in order to compete with ride-sharing firms: integrate the hailing process into ubiquitous mobile technology and decrease price.

Furthermore, results indicate significant potential benefit for restaurateurs, event planners, and nightlife managers (i.e., individuals whose livelihood often depends on the sale of alcohol). In particular, this work suggests the potential benefits of partnering with ride-sharing firms. To the extent that vendors can be held culpable for overserving patrons, and to the degree that return business is vital for these firms, integration of digital ride hailing during the dining or event experience offers significant benefit for all parties. In particular, the vendor is able to eschew a significant liability. Moreover, as chauffeured service is often seen as a sign of prestige, there may be additional social externalities which accrue to both patron and vendor.

Finally, this work contributes to the small, but growing, literature in information systems about the societal impacts of information sharing (Bapna et al. 2012; Burtch et al. 2013; Chan and Ghose 2014; Greenwood and Agarwal 2016). To the degree that platforms have been found to both enhance (Burtch et al. 2013) and diminish (Chan and Ghose 2014; Greenwood and Agarwal 2016) public welfare, our work contributes by drawing a richer picture of the public welfare implications of platform introduction. Moreover, it serves as an open call to extend this research into other aspects of the sharing economy, such as education market places, government to citizen platforms, and innovation markets.

It is important to note that this work is subject to several limitations which offer rich opportunities for future research. First, we conduct our analysis only in the state of California due to data availability. While California is a large and economically diverse state, which offers the ability to study Uber over a protracted period of time, this is simply a limitation and further research will be necessary to ensure the robustness of the results. Second, although results indicate an absence of unaccounted for heterogeneity before the implementation, it is important to note that the results of this work are not based on a randomized trial. As a result, further work is necessary to ensure that there are not confounding factors that also influence the findings. Third, to the degree that limited information is available about the drivers of vehicles involved in the crashes, we are unable to uncover which populations and subpopulations are influenced to the greatest degree based on race, gender, age, or socio-economic status. Given the paucity of data available about such factors, we leave them as topics for future research. Finally, although the positive externalities resulting from the introduction of Uber are significant, this work does not attempt to quantify the negative externalities which may emerge from the introduction of ride-sharing platforms (e.g., fair wages, patron safety through either inadequate liability coverage or poor driver

[30]http://www.rita.dot.gov/bts/sites/rita.dot.gov.bts/files/publications/by_the_numbers/drunk_driving/index.html.

[31]http://dui.drivinglaws.org/resources/how-much-does-a-first-offense-dui-cost.htm.

screening,[32] the facilitation of escort services[33]). In light of this limitation, it would be inappropriate to make any inference about the overall public welfare effect of Uber (or ride-sharing services in general) from this work.

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[32]http://www.forbes.com/sites/ellenhuet/2014/06/03/uber-driver-with-felony-conviction-charged-with-battery-for-allegedly-hitting-passenger/.

[33]http://www.chicagobusiness.com/article/20150225/OPINION/150229886/uber-should-focus-disruption-on-technology-not-effects-of-lousy-pr.

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