Ridesharing and External-Cause Mortality*

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Abstract

Existing research suggests ridesharing affects transportation access, substance use, and exposure to crime. Ridesharing may therefore also have meaningful effects on mortality related to these underlying factors. To address this question, we use restricted-access data from the National Vital Statistics System to study how UberX, Uber's taxi-like service, affects mortality from external causes. Our identification strategy relies on spatial and temporal variation in UberX entry across U.S. counties. Among those aged 18 to 45, we find that UberX entry into an area is associated with 2.01 additional deaths per quarter per 100,000 population (roughly a 10% increase). We find that these deaths are primarily related to alcohol and drug use. We support a causal interpretation for our findings by presenting event studies, placebo analyses, sensitivity and heterogeneity analyses, and a variety of robustness checks, including differences-in-differences estimators that are robust to heterogeneous treatment effects. Our work contributes to the literature by addressing both the benefits and costs of ridesharing, and by informing stakeholders who are interested in the cumulative impact of ridesharing on public health and well-being.

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1 Introduction

Ridesharing services have changed transportation access and usage by allowing people to use their smartphones to easily request trips, share their location and contact details, and make payments. The convenience of ridesharing has had significant impacts on public transit usage and congestion (Hall et al., 2018; Tarduno, 2021; Agrawal and Zhao, 2023), traffic fatalities and drunk driving (Brazil and Kirk, 2016; Greenwood and Wattal, 2017; Dills and Mulholland, 2018; Barrios et al., 2022; Anderson and Davis, 2023), and, because ridesharing offers safe transportation to and from social gatherings, alcohol consumption (Zhou, 2020; Teltser et al., 2021). Further, media reports suggest that ridesharing services are often used to distribute illicit drugs.[1] On the other hand, ridesharing offers an attractive transportation alternative to ambulances (Moskatel and Slusky, 2019) and can reduce exposure to violent crime by decreasing the time people spend waiting for transportation in public places at night (Dills and Mulholland, 2018). Considering the body of evidence on ridesharing's impacts on transportation patterns (particularly intoxicated driving and access to hospitals), alcohol and illicit drug use, and exposure to violent crime, ridesharing could have meaningful effects on deaths associated with these underlying factors.

In this paper, we seek to estimate the reduced-form effect of ridesharing on deaths from external causes including deaths involving violent crime (i.e., homicide), suicide, motor vehicle accidents, alcohol, and drug overdoses among Americans aged 18 to 45. We focus on those age 18 to 45 because older individuals are less likely to have a smartphone or to use ridesharing services during our sample period (2008-2019). We focus on deaths involving homicide, suicide, motor vehicle accidents (MVA), alcohol, and drugs because they are plausibly affected by the advent of ridesharing services and because they are the leading causes of death among young adults (Carpenter and Dobkin, 2009).[2]

To identify the effect of ridesharing on mortality, we use spatial and temporal variation in UberX entry dates across U.S. Core Based Statistical Areas (CBSAs) from late 2012 through 2019. UberX is Uber's mainstream taxi-like service. Our estimates rely on restricted access mortality data from the Center for Disease Control's National Vital Statistics System (NVSS) and a difference-in-differences imputation approach (Borusyak et al., 2021) that accounts for any heterogeneous

[1] For example, see https://www.vice.com/en/article/qkjjwb/how-uber-is-changing-drug-dealing. Uber also offers package delivery services and drivers appear to be concerned about the contents of those packages https://www.nbcnews.com/tech/tech-news/uber-drivers-drug-mules-connect-rcna38464.

[2] In our data section, we explain that the list of causes of death here is in order of "priority." We follow Carpenter and Dobkin by assigning deaths involving more than one underlying cause only to the highest priority category. In Table B1, we explain the ICD codes that form our homicide, suicide, MVA, alcohol, and drug cause of death categories.

treatment timing bias (Goodman-Bacon, 2021; Baker et al., 2022). We focus on mortality in "central" CBSA counties because outlying counties are quite far outside of the core cities and suburbs of CBSAs, and thus likely have considerably less access to UberX. Further, central counties comprise more than 90% of the average CBSA's population.

Our findings suggest that the entry of UberX is associated with 2.01 additional external-cause deaths per 100,000 population per quarter. Primarily, the increase in mortality relates to deaths involving alcohol (0.42 per 100,000 population) and drug use (1.72 per 100,000), such as acute alcohol poisoning and overdoses. These estimates represent an increase of 38% in alcohol-related deaths and a 28% increase in drug-related deaths. For context, Huh and Reif (2021) show that there is a 29% increase in drug-related deaths around when young people gain a driver's license. Further, Carpenter and Dobkin (2009) report that turning 21 (i.e., the legal drinking age in the U.S.) was associated with a 9% increase in mortality and at least a 35% increase in deaths involving alcohol. Our analysis also shows 0.48 additional suicide deaths per 100,000 population. The effect represents roughly a 10% increase compared to the pre-UberX mean. It also aligns with our expectations, given the documented relationship between drug and alcohol use and suicide attempts (e.g., Conner et al., 2014; Esang and Ahmed, 2018). We find no comparable effects on homicide or motor vehicle accident deaths.

We support a causal interpretation for our findings by presenting a series of event studies, sensitivity and heterogeneity analyses, and a variety of robustness checks. Notably, we find that 75% of the increase in mortality occurs on the weekend. According to Uber, "[d]emand for rides is always at its peak on the weekends: Friday and Saturday nights: 5pm-5am."[3] If the increasing number of deaths were uniformly distributed throughout the week, we might not suspect UberX to be the reason for those deaths. As part of our robustness checks, we show that our results are similar when using the Callaway and Sant'Anna (2021) and Wooldridge (2021) estimators.[4]

Examining group-specific mortality by race, gender, age, and location, we find the effects of UberX are largest among those aged 31 to 45, white men, and residents of areas with weaker public transport options prior to UberX entry. Given Teltser et al. (2021) find that the effects of UberX entry on alcohol consumption are greatest for white men and in areas with weaker public transit

[3] See https://www.uber.com/en-AU/blog/making-the-most-of-your-time-on-the-road/.

[4] On the other hand, we do find evidence of heterogeneous treatment timing bias (Baker et al., 2022). For that reason, the traditional two-way fixed effects (TWFE) approach shows significantly attenuated estimates.

options, the patterns in our findings strongly support the idea that increased substance use related to the advent of UberX has led to increased deaths.[5]

Our work makes three key contributions to the existing literature. First, while many researchers have examined UberX's positive impacts on business formation, public transport, impaired driving, labor market flexibility, and hospital transport services (Hall et al., 2018; Dills and Mulholland, 2018; Chen et al., 2019; Moskatel and Slusky, 2019; Barrios et al., 2020; Anderson and Davis, 2023), we document how UberX's entry into an area can have significant public health impacts. Understanding how ridesharing affects mortality, beyond changes relating to motor vehicle accidents, helps us better understand the broader potential for unintended consequences associated with the introduction of ridesharing. Second, by providing evidence of increases in risky alcohol and illicit substance use, our findings may help to explain the mixed evidence on Uber's overall effects on socially important outcomes such as crime, traffic fatalities, and use of public transport (Hall et al., 2018; Dills and Mulholland, 2018; Barrios et al., 2022). Finally, in contrast to work focused on macroeconomic factors and the drug environment (Ruhm, 2019; Case and Deaton, 2020), we offer a novel and complementary explanation for part of the increase in drug overdose deaths in recent years.

In summary, our research addresses how ridesharing affects public safety and health, provides insights into the economic and societal implications of ridesharing, particularly its effect on the well-being of young adults, and offers data-driven evidence to alert policymakers of an important unintended consequence of ridesharing. Policymakers may use this information to devote additional resources to offset increases in substance use, for example, or perhaps to combat the resulting harms. A limitation of our paper is that our data cannot provide direct evidence on the underlying mechanisms driving changes in external-cause deaths. Instead, we rely on observable characteristics in our vital statistics data as well as the existing literature to provide insight on the potential causal pathways.

In Section 2, we situate our work relative to existing studies on how ridesharing has affected economic outcomes. In Section 3, we summarize our mortality data and describe our approach to estimation. We present our main findings in Section 4, along with event studies, sensitivity and heterogeneity analyses, and a variety of robustness checks. We offer concluding remarks in Section 5.

[5]For example, Teltser et al. find a 7.7% increase in total drinks for men compared to 1.2% for women after Uber became available.

2 Background and Existing Literature

Ridesharing has become a common mode of transportation in the United States since the launch of UberX in New York City in 2012. Because ridesharing services are so convenient to use, they are widely considered to be superior to traditional taxi services. Besides the private benefits experienced by individuals, ridesharing companies, such as Uber and Lyft, regularly highlight that their services can help prevent social harm. As one example, in a 2015 report, Uber points out that after they started operating in Seattle, arrests for driving while intoxicated (DWI) dropped by 10%. In the same report, they also emphasize that the highest demand for Uber matches the times when bars close in Miami, Pittsburgh, and Chicago.[6] On the other hand, according to the National Safety Council, 46,980 people died in motor vehicle accidents in 2021, representing a 33.1% increase relative to the 35,303 deaths in 2011, just before the advent of ridesharing.[7] Prior to 2011, rates of traffic accident deaths had declined consistently for five decades.

Multiple studies, such as Greenwood and Wattal (2017), Martin-Buck (2017), Peck (2017), and Dills and Mulholland (2018), support Uber's safety assertions. Notably, Martin-Buck (2017) reports a 10% reduction in fatal alcohol-related traffic incidents and a decrease in driving under the influence (DUI) arrests based on National Highway Traffic Safety Administration (NHTSA) data. Greenwood and Wattal (2017) observe a 3.6% decline in motor vehicle fatalities using California Highway Patrol data following Uber's introduction. Additionally, Dills and Mulholland (2018) estimate a quarterly 1.6% reduction in fatal traffic incidents post-Uber entry, aligning with a decrease in DUI arrests similar to Martin-Buck's findings.

On the other hand, Barrios et al. (2022) report a 0.5% to 1.5% *increase* in alcohol-related accidents and fatalities following the advent of ridesharing. The authors suggest that the reason why their results differ from previous studies is that the way alcohol-related incidents are categorized changed in 2008. Perhaps the highest-quality evidence for Uber's impact on traffic fatalities comes from Anderson and Davis (2023), who use proprietary data from Uber that measures monthly Uber activity at the Census tract level, and find a consistent 5.4% decline in traffic-related fatalities. The mixed findings in this literature are of concern mainly because the economic costs associated with traffic fatalities are staggering, encompassing millions of dollars in medical expenses, property damage, and lost productivity. Understanding whether ridesharing contributes to traffic incidents and fatalities is an important step towards improving road safety.

[6] See https://newsroom.uber.com/wp-content/uploads/2015/01/UberMADD-Report.pdf.
[7] See https://injuryfacts.nsc.org/motor-vehicle/historical-fatality-trends/deaths-and-rates/.

One potential explanation for the mixed findings in this literature is that safe transportation options may affect drinking behavior among both riders and non-riders. Jackson and Owens (2011) formalize this type of reasoning by developing a model where alcohol consumption and transportation are inputs into a "night out." Their model predicts that an expansion in transportation options will tend to increase the quantity of nights out (and therefore alcohol consumption) for both transit riders and non-riders. A key assumption in their model is that utility is increasing in the number of other people who are also socializing.

Jackson and Owens provide support for their model by studying D.C. Metro expansions from 1999 to 2003, where they find a 5.4% increase in alcohol-related arrests around bars near stations that experienced expanded late night service. Jackson and Owens' findings suggest that, to the extent that safe transportation and alcohol are complementary, demand for alcohol will increase when ridesharing becomes available. Therefore, it is not surprising that Teltser et al. (2021) find a 5.4% increase in total drinks per month after UberX entry and show that the additional consumption is occurring away from home by using data on earnings for bar and restaurant workers.

Other work has examined the effects of ridesharing on a range of outcomes not directly related to driving. For example, Cohen et al. (2016) attempt to measure Uber-related consumer surplus and Chen et al. (2019) estimate how flexible schedules are valued by Uber drivers. Research shows that Uber also impacts local economic conditions, including entrepreneurial activity (Burtch et al., 2018; Barrios et al., 2020) and use of public transport (Hall et al., 2018). We contribute to this growing literature on the effect of ridesharing on our society by showing that new forms of transportation can lead to greater mortality, particularly substance-related mortality.

In that sense, our work also contributes to a large literature showing that the consumption of alcohol and illicit substances can have harmful social effects, particularly among younger adults. Examples include studies that find increases in risky sexual activity and child abuse (Chesson et al., 2000; Markowitz and Grossman, 2000; Rees et al., 2001; Sen, 2002; Rashad and Kaestner, 2004). Relying on the minimum drinking age of 21 as a cut-off in a regression discontinuity design, Carpenter and Dobkin (2017) find that "inpatient hospital admissions and emergency department (ED) visits increase by 8.4 and 71.3 per 10,000 person-years, respectively, at age 21," and that they are driven by self-injury, assault, and alcohol poisoning among young men. Showing direct mortality effects, Carpenter and Dobkin (2009) find large and immediate increases in drinking at age 21 and "a discrete 9% increase in the mortality rate at age 21."

Notably, deaths relating to alcohol consumption continue to increase in the U.S. For example, Spencer et al. (2022) show that there were 49,061 deaths involving alcohol in 2020, compared to 25,692 in 2010 and 19,643 in 2000.[8] However, drug-related deaths have increased by an even greater proportion. In 2020, for example, the CDC reported 91,799 drug overdose deaths, with 69.4% of those deaths occurring among men and 48.4% occurring among those aged 25 to 44 years old (Hedegaard et al., 2021). In 2010, there were 38,329 overdose deaths, and only 17,415 in 2000.

Ruhm (2019) argues that the availability of illicit opioids has played a significant role in the increasing pace of overdose deaths. Ruhm studies the impact of local economic conditions and changes in drug availability on county-level substance-related mortality rates between 1999 and 2015. He finds that economic conditions could explain no more than 10% of the increase in drug and opioid-related deaths. In contrast, he finds that men and younger adults are relatively more affected when the primary driver of the fatal drug epidemic transitioned from prescription to illicit opioids. Ruhm's findings suggest that improving local economies may not significantly reduce overdose mortality, but interventions that address drug availability may have greater potential. Our work shows that the advent of UberX in an area appears to add "fuel to the fire," with significant increases in alcohol and drug-related mortality. Moreover, our findings suggest that factors that alter alcohol consumption patterns could also be driving illicit substance abuse, including consuming alcohol and illicit substances as complementary goods. This is a novel and complementary explanation for the ongoing "fatal drug epidemic" (Ruhm, 2019).[9]

However, we are interested in more than just deaths from drugs and alcohol. In particular, we follow Carpenter and Dobkin (2009) by examining deaths relating to homicide, suicide, motor vehicle accidents (MVA), alcohol, and drugs among young adults (our main estimates focus on those aged 18 to 45). These categories comprise the leading causes of death among young adults (Carpenter and Dobkin, 2009). As we mention in the introductory section, we focus on young adults because they adopted smartphones earlier than older age groups and are more frequent users of ridesharing services.[10] Moreover, these mortality categories are the ones plausibly affected by the advent of ridesharing services. For example, these services provide transportation to allow people

[8]They consider ICD-10 codes E244, F10, G312, G621, G721, I426, K292, K70, K852, K860, R780, X45, X65, and Y15. They exclude unintentional injuries, homicides, other causes of death from conditions either indirectly or partially related to alcohol use, and deaths of newborns that are associated with maternal alcohol use.

[9]As an appendix item, we show that there is no relationship between UberX entry timing and greater increases in overdose deaths in the years prior to entry.

[10]According to a 2018 Gallup Poll, young adults are much more likely to use ride-sharing services. For example, 45% of adults aged 18 to 29 say they use ride-sharing services, whereas this drops to 36% among those 30 to 49 years old, 23% among those aged 50 to 64 and 13% among those aged 65 or older. https://news.gallup.com/poll/237965/snapshot-uses-ride-sharing-services.aspx.

to remove themselves from situations of heightened risk, potentially reducing violent crime such as homicides. On the other hand, ridesharing services could encourage more people to be out late at night, potentially increasing the pool of crime victims and perpetrators. Ridesharing also alters transportation patterns, potentially reducing traffic fatalities. At the same time, ridesharing can allow people to engage in more social activities, perhaps improving mental health outcomes (i.e., ridesharing could reduce instances of suicide). However, social activities frequently involve alcohol and/or illicit substance use, potentially diminishing mental health and increasing alcohol and drug overdoses (or accidents relating to impaired decision making and reduced physical abilities including drowning, burns, falls, etc.).

By investigating the impact of ridesharing on homicide and suicide, we contribute to a larger literature examining the causal determinants of violent crime and deaths of despair more broadly. For example, Edwards et al. (2018) find that changes in state laws prolonging delays for firearm purchases reduce suicide rates by 2 to 5%, with no effect on homicides. Cook and Ludwig (2006) use county- and state-level panels and document a positive elasticity between gun prevalence and suicide. Stevenson and Wolfers (2006) leverage state-level variation in the passage of unilateral divorce laws and finds that they lead to an 8 to 16% decline suicide rates among women, a 30% drop in domestic violence, and a 10% reduction in the number of women murdered by their partners. Classen and Dunn (2012) analyze unemployment duration as a driver of suicide, and find that suicide risk increases after 15-26 weeks. As another example, Evans and Moore (2012) find that mortality increases during times of financial liquidity. Our work adds to this literature by considering how changes in transportation, either directly by allowing people to remove themselves from situations where they may become a victim of a crime, or through increased use of substances that may induce violent behavior or diminish mental health, may impact homicide and suicide rates.

In summary, our work contributes to the literature on ridesharing and causes of deaths among young adults by providing novel evidence on how ridesharing affects the main categories of mortality among young adults in the U.S. Given the devastating costs of mortality from drugs, alcohol use, and motor vehicle accidents among young adults, our work is of critical policy relevance. In the next section, we explain our data and our approach to estimation.

3 Data and Estimation

3.1 Data

For our outcomes of interest, we use restricted access mortality data from the National Vital Statistics System (NVSS). The National Vital Statistics System (NVSS) serves as the national repository of vital records in the U.S., and is managed by the National Center for Health Statistics (NCHS) under the Centers for Disease Control and Prevention (CDC). The legal authority for registering vital events, including births, deaths, marriages, divorces, and fetal deaths, resides with 50 states, 2 cities (Washington, D.C., and New York City), and 5 territories (Puerto Rico, the Virgin Islands, Guam, American Samoa, and the Commonwealth of the Northern Mariana Islands). Each year these localities submit standardized death certificates to their state's vital statistics office which are then forwarded to the NCHS. The NCHS edits and assembles these records for national publication using the Mortality Medical Data System.[11] The NVSS data therefore includes rich information on the universe of reported deaths of residents in the United States, including causes of death classified using International Classification of Disease tenth-edition (ICD-10) codes and the county where the death occurred. The restricted use data, which includes county of occurrence and residence and does not suppress county-month counts less than 10, requires approval from the NVSS.[12] Within the NVSS data, we focus on multiple cause of death, county-level death records for the years 2008 to 2019.

The NVSS mortality records are one of the most widely used and longest-spanning datasets for epidemiological research, and the quality of vital statistics in the U.S. is considered extremely high (Harris et al., 2021). This is in part due to the standardization of death certificates and the uniformity of coding procedures for both state localities and staff at NCHS. As a practical matter, medical examiners, coroners, attending physicians, or funeral directors complete death certificate records, which includes assigning cause and manner of death. Detailed guidance on coding practices and cause of death certification is made available through the CDC via the Medical Examiners' and Coroners' Handbook on Death Registration and Fetal Death Reporting (National Center for Health

[11] For a more detailed description of the NVSS, see the 2009 report by the National Research Council (NRC). https://nap.nationalacademies.org/catalog/12714/vital-statistics-summary-of-a-workshop.

[12] For information on accessing NVSS mortality records, see https://www.cdc.gov/nchs/data_access/vitalstatsonline.htm.

Statistics, 2003). Extensive technical guidance is available from the Vital Statistics Cooperative Program (VSCP) and state vital statistics organizations and their personnel.[13]

A number of points are worth mentioning that are relevant to our analysis. First, the mortality records published by NCHS differentiate between underlying and multiple-cause of death. The underlying cause of death is "the disease or injury which initiated the train of morbid events leading directly to death, or the circumstances of the accident or violence which produced the fatal injury" (see p.31 of WHO, 2004). The underlying cause is assigned by NCHS using an automated system known as the Automated Classification of Medical Entities (ACME) according to "the sequence of conditions on the certificate, provisions of the ICD, and associated selection rules and modifications" (see p.62 of Kochanek et al., 2019). While the underlying cause is a useful and well-accepted measure of mortality, it necessarily omits information on multiple causes, or causes that were involved in the causal sequence of events leading to death or otherwise contributed to death. As such, NCHS also provides multiple causes of death designed to capture other causes listed on the death certificate that do not qualify as the direct cause but nonetheless contain useful etiological information found in the death certificate (Remund et al., 2018; Kochanek et al., 2019).

Second, autopsies are often employed to obtain further information on cause of death and as such can potentially impact the accuracy of cause of death coding. State laws vary substantially with respect to the types of deaths requiring an autopsy, but autopsies are generally indicated in situations where the death is unexplained or unexpected, the result of violence, the result of an overdose, or where foul play is suspected. For example, the National Association of Medical Examiners Forensic Autopsy Performance Standards indicate that autopsies will be performed in deaths where the death is caused by criminal violence, associated with police action, or where the death is by apparent intoxication by alcohol, drugs, or a poison.[14] To illustrate, in 2007, over 97% of assault-related deaths in the NVSS records received an autopsy; 79% of accidental poisonings were autopsied; and 80.8% of events of undetermined intent were autopsied (Hoyert, 2011). Autopsies are not generally necessary in situations that lack foul play, where adequate medical history is available, or where the death is known to be naturally occurring. Our outcomes of interest tend to

[13]This includes, but is not limited to, the Vital Statistics Training Program, which is a one week set of courses offered to staff of vital registration offices, as well as promulgation of revisions to the U.S. Standard Certificates and Reports, whereby recommendations of best practices are developed in cooperation with a consortium of public and private researchers and policy makers.

[14]See https://www.thename.org/assets/docs/2016%20NAME%20Forensic%20Autopsy%20Standards%209-25-2020.pdf.

fall within the categories of death that are most frequently autopsied and for which cause of death was ascertained from an autopsy, providing further confidence in the accuracy of our data.

Despite the high quality of NVSS records, the involvement of opioids in causes of death tends to be understated due to the specific class or type of drug being unspecified. Ruhm (2017) estimates that as many as 20%-25% of fatal overdoses involving opioids are not classified as involving opioids. We therefore choose to follow the practice of prior studies (Goodman-Bacon and Sandoe, 2017; Venkataramani and Chatterjee, 2019) by examining the broad class of drug overdose deaths without differentiating between specific classes of substances. Our results examining drug poisonings cannot speak definitely to the involvement of opioids, but we interpret these results as suggestive evidence of their likely role, noting evidence from Ruhm (2017) who estimates that over 60% of drug overdose deaths are likely attributable to opioids. We provide a complete breakdown of the ICD-10 codes associated with each category of deaths as an appendix item.

We obtain data on UberX entry dates from Teltser et al. (2021), who expand on UberX entry dates provided by Hall et al. (2018). Hall et al. determine UberX entry and exit dates through mid-2017 "based on newspaper articles as well as Uber's press releases, blog posts, and social media posts." Teltser et al. supplement Hall et al.'s data through 2019 using the same approach. They have 225 CBSAs in their BRFSS data. Our NVSS data contains more counties in more CBSAs. We manually gather UberX entry dates for the additional counties in the 104 CBSAs in our data, again using newspaper articles, press releases, and social media posts to determine the date UberX began operation in the area. As we mention in the introductory section, our estimates focus on mortality in designated central counties (these are more densely populated counties within a CBSA or division) within CBSAs.

We present a map of all counties that ever experienced UberX entry in Figure 1. We provide maps showing entry by county for each year as an appendix item (see Figure D1). We also provide a complete updated list of entry dates by county as an appendix item. Our data includes 937 total CBSAs and divisions, of which 329 experience UberX entry at some point between 2008 and 2019. Our preferred sample, comprising only central counties in CBSAs that UberX ever enters between 2008 and 2019, therefore consists of 639 unique counties in 329 ever-treated Core-based Statistical Areas/divisions (CBSAs), totaling 15,792 county-quarter observations.[15] We restrict our sample to counties that are in a CBSA because accurately gathering UberX entry dates for non-CBSA counties (i.e., low population density, rural) would be infeasible.

[15] Core-based Statistical Divisions are the larger, more densely-populated CBSAs.

Two comments are noteworthy when discussing UberX entry data. First, we do not control for Ubers main competition, Lyft, in our analyses. Lyft's entrance in cities has historically trailed Uber's entry and Lyft's market share is less than that of Uber. In 2019, Uber's market share was roughly 70% of total market sales while Lyft controlled 30% of the market.[16] Notably, our omission of Lyft entry should lead to an attenuation bias, which would make finding a significant effect less likely. Second, of the 329 CBSAs that UberX enters from 2008-2019, they exit 22 CBSAs, or 6.6%, of these at some point (often due to regulatory barriers). Of the 22 CBSAs that Uber ever exits, Uber re-launches in that market within one year in 12 of those CBSAs. As UberX exiting an area would attenuate any treatment effect, we make the conservative assumption to consider an area as treated by UberX from the date it enters the area, and ignore any subsequent exit and re-entry decisions.

Our main outcomes of interest are expressed as deaths per 100,000 persons in an area for various subgroups. For county-level population estimates, we use county-level single-year age groups data from the Surveillance, Epidemiology, and End Results Program (SEER).[17] These data allow researchers to pull yearly total population estimates, as well as population by demographic groups such as by gender, age, and race at the county level. The data used to construct the SEER data comes from the U.S. Census Bureau.

We provide summary statistics for our outcome variables and county-level covariates in Table 1. The data refer to ever treated, central counties and are reported at the county level. We remind the reader that our approach follows Carpenter and Dobkin (2009) by assigning external cause deaths to priority categories as follows - homicide, motor vehicle accidents, suicide, alcohol, drugs. With this ordering, there is no "double counting" of deaths, in the sense that if someone dies from homicide while also intoxicated from alcohol, the death is counted only as a homicide in our data.[18]

3.2 Estimation

To estimate the effect of UberX on mortality, we use a difference-in-differences approach, exploiting variation in UberX entry across time and place. Our estimating equation is as follows;

$$y_{jt} = \alpha + \beta \cdot \text{Uber}_{jt} + X_{jt}\Pi + \theta_j + \phi_t + \varepsilon_{jt}. \tag{1}$$

[16] See https://www.statista.com/statistics/910704/market-share-of-rideshare-companies-united-states/.

[17] For further information or to access the SEER data, see https://seer.cancer.gov/popdata/download.html.

[18] In Table 5, we present estimates where we allow deaths to appear in more than one category if there is more than one cause of death listed on the death certificate.

In equation (1), y_{jt} refers to deaths per 100,000 persons in geographic area j in time period t for the selected cause of death, y. We capture UberX availability using an indicator, Uber_{jt}, that equals one if UberX is present in county j for any part of quarter t and then stays equal to one for all subsequent time periods, ignoring any UberX exit and re-entry in any subsequent periods. All specifications include geographic unit fixed effects, δ_j, time period fixed effects, γ_t, and an idiosyncratic error term, ε_{jt}. We cluster standard errors at the CBSA level, except in cases where CBSAs consist of smaller CBSA Divisions, in which case we cluster at the CBSA division level. In some specifications, we include county-level demographics captured by the X_{jt} term.

In our setup, if there are no unaccounted for idiosyncratic shocks that are correlated with both UberX entry and changes in mortality, then β represents the impact of UberX entry on mortality y. If our identifying assumption is valid, the trend in mortality in counties that UberX enters would be parallel to the trend in mortality in counties that UberX has not yet entered, absent UberX's entry. That assumption is untestable, but we can examine trends prior to UberX entry using an event study approach. Following Jacobson et al. (1993), Goodman-Bacon and Cunningham (2019), and Teltser et al. (2021) our specification is as follows:

$$y_{jt} = \sum_{k=-s}^{S} \beta_k \cdot 1(t - T_j = k) + X_{jt}\Pi + \theta_j + \phi_t + \varepsilon_{jt}. \tag{2}$$

The key difference from Equation (1) is that we replace the treatment variable for UberX's presence in an area with a set of indicators $1(t - T_j = k)$, where T_j is the time that UberX launches in county j, t is calendar time, and k is event time, or the number of periods relative to UberX market launch in county j. We examine $s = 18$ quarters (4.5 years) before UberX entry to ensure a fully balanced sample of cities when estimating our event study leads. We examine five years ($S = 20$) of post-UberX entry data to allow us to report on how UberX impacts mortality over the medium to long-run. However, because our data end in 2019, we cannot observe 20 quarters of data for CBSAs where UberX entered after 2014. We believe, however, that our choice strikes a reasonable balance between minimizing compositional change in our sample while also estimating the impact of UberX on mortality over a relevant time horizon.

A potential complication with using equations (1) and (2) to estimate the effect of UberX on mortality is the possibility of heterogeneous treatment effect bias (Baker et al., 2022). This phenomenon can occur in settings with staggered treatment timing. In our setting, our estimates may be biased if the treatment effect on mortality is not constant over time. This is a particular

concern for us because it may take time for consumers to begin using Uber in an area. In these situations, equation (1) is misspecified because it imposes a constant, linear treatment effect. This misspecification can bias β in either direction and result in erroneous conclusions regarding the presence of pre-trends in an event study design. Consequently, in our preferred event study designs and regression estimates, we use a difference-in-differences imputation estimator (Borusyak et al., 2021). Borusyak et al.'s estimator imputes a counterfactual without imposing any restrictions on treatment-effect heterogeneity, and is robust to heterogeneous treatment effect bias. As part of our robustness checks, we show that our estimates are not appreciably different when using the Callaway and Sant'Anna (2021) or Wooldridge (2021) estimators. On the other hand, we document significant evidence of heterogeneous treatment effects, which leads to a bias when estimating UberX's impact on mortality using the traditional two-way fixed effects (TWFE) design.

4 Findings

We present our main estimates in Table 2. Recall that our estimates reflect quarterly deaths per 100,000 persons in ever treated, "central" counties and that our approach follows Carpenter and Dobkin (2009) by assigning external cause deaths to priority categories as follows - homicide, suicide, motor vehicle accidents (MVA), alcohol, drugs. With this approach, there is no "double counting" of deaths.[19]

The estimates in column (1) show that the advent of UberX in an area is associated with 2.01 additional external cause deaths per 100,000 population aged 18 to 45 per quarter. Compared to the category mean (provided in brackets), that represents a 10% increase in mortality. To put these estimates in context, Carpenter and Dobkin (2009), using a regression discontinuity design, found that external cause deaths increase by between 8% and 11% around age 21, depending on specificaiton. Using our estimates, a back of the envelope calculation would suggest that the advent of ridesharing is associated with 6,473 additional deaths per year in the U.S. (consisting of 2.01 deaths per 100,000 per quarter, times the pre-UberX entry average population of 126,000 in ever-treated counties, times 639 treated counties). If we use the post-entry population (168,000), the number of annual deaths would be as high as 8,631.

When we examine the specific cause of these deaths we see that most of the overall effect consists of 0.42 more alcohol-related deaths per 100,000 population and 1.72 per 100,000 additional

[19] In Table 5, we allow deaths to "overlap."

drug-related deaths. These estimates are each statistically significant at the 5% level or greater. Looking first at alcohol-related deaths, the 0.42 increase represents 38% of alcohol-related deaths among those aged 18 to 45 relative to the category mean. Again, however, our estimates are in line with Carpenter and Dobkin (2009) who report that alcohol-related deaths increase by 35% or more at the drinking age discontinuity (i.e., 21 years old). Moreover, Carpenter and Dobkin show that relatively small increases in alcohol consumption can have significant impacts on mortality; their estimates imply that a 10% increase in the number of days that alcohol is consumed leads to a 4.3% increase in mortality among young adults.

Similarly, the 1.72 per 100,000 increase in drug-related deaths represents a 28% increase compared to the category mean for those aged 18 to 45. A back of the envelope calculation would therefore suggest that ridesharing leads to almost 6,000 additional drug deaths per year. While substantial, that would represent only 6.5% of all overdose deaths in the U.S. in 2020 and would explain only about 12% of the change in annual overdose deaths between 2010 and 2020.[20] Our estimates are also in line with recent estimates of how changes in transportation can affect drug-related deaths. For example, Huh and Reif (2021), using a regression discontinuity design, find a 29% increase in drug-related deaths around when young people gain a driver's license.

Our estimates also show that there is a statistically significant 0.48 increase in suicide deaths per 100,000 population. This is to be expected given that drug and alcohol use is highly correlated with suicide attempts. For example, acute use of alcohol is associated with roughly a five- to ten-fold increase in suicide risk (Conner et al., 2014). Moreover, Esang and Ahmed (2018) reports that alcohol intoxication is involved in approximately 22% of deaths by suicide. Esang and Ahmed also report that illicit drugs were present in almost 40% of suicide deaths.[21] In contrast, we see a small and statistically insignificant decline in Homicide and MVA (Motor Vehicle Accident) deaths. As a reminder, Appendix Table B1 provides the ICD codes for deaths in each category.

Naturally, our main estimates are the product of several sample selection, specification, and estimation choices. One of those choices is our focus on younger adults. For that reason, in column (2) of Table 2 we show that UberX had no similar impact on those aged 46 to 64. For that older group, the only statistically significant effect is a decline in suicide deaths. In contrast, we report an increase in suicide deaths among the younger group. Our findings are in line with expectations

[20] In 2020, the CDC reported 91,799 drug overdose deaths (Hedegaard et al., 2021). In 2010, there were a total of 38,329 overdose deaths.

[21] The leading category is opiates at 20%, next is marijuana at 10.2%. Cocaine is involved in 4.6% of suicide deaths, while amphetamines are involved in 3.4%.

given our earlier discussion about Uber and smartphone usage across demographic groups during our sample period.[22] Another choice is to focus on deaths in central counties. The estimates in column (3) expand our sample to include central and outlying counties within a CBSA. While the number of counties grows substantially, the population in outlying counties amounts to only 10% of the average CBSA population. Therefore, adding these counties doesn't change the pattern relative to our main estimates. Given the estimates in columns (2) and (3) of Table 2, we focus on deaths among those age 18 to 45 in central counties in later heterogeneity, sensitivity, robustness, and falsification exercises.

In columns (4), (5), and (6) of Table 2, we present estimates using the Callaway and Sant'Anna (2021) estimator, the traditional TWFE estimator, and the Wooldridge estimator (Wooldridge, 2021). For the Callaway and Sant'anna and Woolridge estimators, we see similar patterns relative to our main estimates. However, we observe significant attenuation in the TWFE estimates. As an appendix item, we show that treatment timing bias is a significant issue in our sample (Goodman-Bacon, 2021), meaning that an alternative difference-in-differences estimator is appropriate. For all subsequent estimates in the paper we use the Borusyak et al. imputation-based estimator, given the similarity between the imputation estimates and the Callaway and Sant'anna and Wooldridge approaches.

In columns (7) and (8) of Table 2 we repeat the estimates from column (1), but add county level covariates, including population, area level unemployment, and real income, along with age, gender, and racial demographic information as indicated. Those estimates show broadly similar patterns, easing concerns that time-varying covariates could have a significant impact on our estimates (Caetano et al., 2022). Given the similarity between those estimates and the potential for time-varying covariates to bias treatment effect estimates, all further estimates in the paper consider the specification without those covariates as our main specification.

4.1 Event Studies

Because we use a staggered timing design, and as we explain in Section 3, we further support our identifying assumption using event studies that can tell us whether there are pre-trends that would undermine identification in our setting. In Figure 2 we present the Borusyak et al. difference-in-differences imputation-based event studies for our main outcomes. We use 18 quarters, or 4.5 years, in the pre-entry period to ensure that each area is observed for at least 18 quarters prior to UberX

[22] Again, see https://news.gallup.com/poll/237965/snapshot-uses-ride-sharing-services.aspx.

entry.[23] We use five years of data in our post-entry period to allow us to estimate the impact of UberX entry over the medium to long term. We note, however, that the composition of cities in the treated group experiences attrition as we estimate more distant event study lags. Reviewing Figure 2, we do not see significant evidence of pre-trends that would bias our findings.

The event studies provide evidence that the effect of UberX on our outcomes of interest occurs relatively soon after UberX entry, but also increases over time. This pattern is consistent with how the number of Uber driver partners grows over time within a city (Hall and Krueger, 2018).[24] For instance, Hall and Krueger use proprietary data from Uber to show that driver partners grow at a rate of just over 4% per month after UberX entry. The pattern of growth is exponential, with the number of driver partners being relatively low in the initial 6 to 18 months in almost all cities, before rapidly increasing. Hall and Krueger demonstrate that only Miami and Las Vegas deviate noticeably from the typical driver partner growth trajectory. The pattern also lines up well with interest in Uber over time as measured by Google Trends data (Hall et al., 2018, see their Figure 1).

4.2 Mechanisms

In Table 3, we consider whether the advent of ridesharing explains our findings. In columns (1) and (2), we separately examine deaths on the weekend versus on weekdays. If UberX is contributing to increased mortality, we would expect to see changes in deaths concentrated on the weekend as that is when Uber claims they are busiest.[25] We define the weekend to be from Friday through Monday. We include Monday to ensure we count "Sunday night" as part of the weekend. We find a disproportionate fraction of the overall change in deaths happens on weekend days. For example, if the additional alcohol-related deaths were distributed evenly throughout the week, we would expect 4/7ths (57.1%) of them to occur on a weekend day (i.e., Friday through Monday). Instead, we see 79.4% of the total increase occurs on the weekend (i.e., the weekend estimate of .335, divided by

[23] Uber entered CBSA Divisions Duchess County-Putnam County, NY (20524) and New York-Jersey City-White Plains, NY-NJ (35614) in late August, 2012.

[24] As Roth (2024) notes, event studies using the new difference-in-differences estimators must be interpreted with caution. For example, using the Borusyak et al. (2021) approach, the coefficient for the time periods before the date of UberX entry are calculated relative to the first time period while the post-period is relative to the average of the pre-periods. This creates the potential for a discontinuity at the treatment date as we see when we plot the event study for suicide deaths. One solution to this issue is to use the Callaway and Sant'Anna (2021) approach while selecting the 'long2' package option (in *STATA*) to force the coefficients to be calculated symmetrically on either side of the treatment date. We present those event studies as an Appendix item; there we see no discontinuous jump in suicides around the treatment date.

[25] According to Uber, "Demand for rides is always at its peak on the weekends: Friday and Saturday nights: 5pm-5am." https://www.uber.com/en-AU/blog/making-the-most-of-your-time-on-the-road/.

the corresponding estimate of .422 for the full week from Table 2). In column (2), when looking at weekday deaths after the advent of UberX in an area, we see a statistically insignificant effect of 0.091 (21.6%). If the increase in deaths were distributed uniformly across days of the week, we should expect to see 3/7ths (42.9%) of the additional deaths occurring on week days (i.e., Tuesday, Wednesday, Thursday). We see a similar pattern, where the majority of the deaths are occurring over the weekend period, for external cause deaths overall, along with suicide and drug-related deaths.

In columns (3) and (4), we show that there are larger increases in deaths "away from home," particularly for alcohol- and drug-related deaths. For example, the increase in away from home drug deaths is 31.1% relative to the category mean versus a 24% increase for deaths that occur at home. For alcohol deaths, the increase in away from home deaths is 46.9% compared to a 34.9% increase in at home deaths. We view these estimates as consistent with Uber facilitating transportation to places where people may engage in risky substance use.

We might expect that UberX has the potential to have a greater impact on mortality in areas with less robust existing public transit options. To illustrate that existing transit connectivity options determine the impact of ridesharing, in columns (5) and (6) of Table 3 we present estimates where we separate the sample into counties in CBSAs with below median and above median "All Transit" scores. These All Transit (Transit Connectivity Index or "TCI") scores are generated by the Center for Neighborhood Technology (CNT) whose goal is to provide a "robust, one of a kind database consisting of stop, route and frequency information for 902 transit agencies in regions with populations greater than 100,000 as well as a large number of smaller regions and agencies." The CNT effectively summarizes the overall transit quality and connectivity of each metro using these TCI scores.

Our estimates when dividing the sample by TCI scores show that areas with weaker transit options experience larger changes in external cause mortality and the various subcategories of interest. For example, in areas with below median TCI scores, the 0.584 coefficient represents a 52% increase in alcohol-related deaths. The corresponding increase for above median areas is just 0.022 (2%) and is statistically insignificantly different from zero. These estimates reinforce the idea that ridesharing has a greater impact in areas with relatively weaker public transit options. If some other change was driving our main findings, it would be surprising if it also affected primarily those counties where transit options are relatively weaker. Further, it is worth noting that smaller areas tend to have higher baseline mortality per 100,000 persons. Put differently, these areas already

fared poorly on these measures of population health and well-being, and the advent of ridesharing widens the existing differences.

Finally, as an appendix item, we examine changes in internal cause deaths (stroke, cancer, etc.) for those aged 18 to 45 after UberX enters. See Table A3 for those estimates. There, the only statistically significant effect is a 16% decline in deaths from heart disease and related conditions. This may be a spurious effect or may reflect ridesharing being used as a substitute for ambulance rides when significant medical events occur (Moskatel and Slusky, 2019). Also as an appendix item, we examine whether changes in our outcomes of interest or area level characteristics (income, unemployment, etc.) can predict UberX entry timing. Any such relationship would undermine a causal interpretation for our findings. There, however, we see no evidence of this potential issue.

4.3 Heterogeneity by Subgroup

We examine outcomes separately for men versus women and for white persons versus all other races in Table 4. In Teltser et al. (2021), the authors found larger impacts on alcohol consumption for men and white individuals. If the mortality effects we find are caused by UberX's impact on alcohol consumption and subsequent complementary substance use, then we should see larger effects among those subgroups. We again focus only on those age 18 to 45. When looking at men only and women only in columns (1) and (2), we find larger effects on alcohol and drug deaths for men, as anticipated. However, we uncover evidence of some heterogeneous effects, with external cause deaths among women increasing more than men, with statistically significant increases in homicides and suicides among women. In contrast, we see fewer homicides among men (-0.54 per 100,000, significant at the 10% level).

We also find some interesting differences in death rates between white people and other races. In particular, we see smaller changes in suicide deaths and only find an increase in alcohol-related deaths among white individuals. This again echoes Teltser et al. (2021) who found limited impacts of ridesharing on alcohol consumption when focusing on black respondents in their sample.[26] Across specifications, we see consistent large estimates for changes in overdose deaths by gender and race.

Finally, in columns (5) and (6) of Table 4, we examine outcomes separately for those aged 18 to 30 versus those 31 to 45. There are some significant differences in the baseline means across these two groups, with roughly twice as many deaths from both alcohol and drugs per 100,000 population

[26]Specifically, in Panel E of their Table 7, they report statistically insignificant effects on their alcohol consumption measures for black respondents. Moreover, the sign of the effect is inconsistent across outcomes of interest (i.e., average drinks per drinking occasion is negative versus a positive effect on drinks in a single occasion).

in the older age group. Consistent with the existing baseline patterns, we see smaller increases in deaths in the younger age group after the advent of Uber. The increase in deaths involving alcohol for the younger age group is 0.197 per 100,000 population, significant at the 10% level, from a mean of 0.71, implying a 27.7% increase after the advent of Uber. Among the older group, the post-entry change is 0.648, significant at the 5% level, from a mean of 1.49, implying a 43.5% increase. For drugs, the corresponding percent changes are 28.6% for the younger group and 28.2% for the 31 to 45 group. Therefore, we see that ridesharing leads to a relatively similar percentage increase in drug and alcohol related deaths across the two age groups. Significant increases in overdose deaths among those who are over 30 lines up well with various CDC reports showing overdoses are more common among people in their 30s and 40s than any other age group.[27]

Notably, our estimates show that the percent changes in overall external cause deaths differs noticeably across age groups. Aside from the effect of deaths involving alcohol and drugs across age groups, another explanation appears to be different patterns in rates of suicide. For example, suicide increases 29.4% in the older group after the advent of Uber, with the coefficient significant at the 1% level, but declines by 9.4% in the younger group, with the estimate not statistically different from zero.

4.4 Further Sensitivity and Robustness

In column (1) of Table 5, we provide estimates that focus only on those aged under 18. We find no statistically significant changes in mortality among that group. If our findings are explained by changes that affect the population more generally, we would expect to also see changes in mortality among groups other than the demographic who is most likely to own a smartphone and be an Uber user in our sample period.

In column (2), we allow for overlapping death categories. As we explain earlier, our main estimates follow Carpenter and Dobkin (2009) and assign external cause deaths to priority categories as follows - homicide, suicide, motor vehicle accidents, alcohol, drugs. With this ordering, there is no "double counting" of deaths, in the sense that if someone dies from homicide while also intoxicated from alcohol, the death is counted only as a homicide in our data. Continuing with the homicide plus alcohol example, in the data used to estimate the coefficients in column (2), the death would now appear in our data as both a homicide and a death involving alcohol. Reassuringly, our

[27] For example, see https://www.cdc.gov/nchs/products/databriefs/db428.htm.

estimates are little different and show clear evidence of increases in mortality overall, consisting mostly of deaths relating to suicide, drugs, and alcohol.

Finally, our main estimates focus on deaths occurring within a county. The estimates in column (3) assign deaths based on county of residence. Naturally, county of death and county of residence are often the same. Unsurprisingly, we see similar effects relative to our main estimates.

5 Conclusion

We consider whether the advent of ridesharing has led to an increase in mortality in the United States. Using restricted access data from the CDC's National Vital Statistics System (NVSS) and spatial and temporal variation in UberX entry for identification, we find that UberX is associated with significant increases in external cause deaths. While we consider how ridesharing could affect mortality relating to crime (homicide), suicide, and motor-vehicle accidents, our estimates consistently show that Uber's impact on mortality manifests primarily via increases in alcohol-related and drug-related deaths. Our estimates suggest that the advent of UberX is associated with somewhere between 6,400 and 8,600 additional deaths per year, depending on assumptions. These estimates are similar in magnitude to the alochol-related mortality effects associated with turning 21 (Carpenter and Dobkin, 2009) and the drug-related mortality effects of gaining one's driver's license (Huh and Reif, 2021). Given the demographic profile of smartphone users during our sample period and the findings of Teltser et al. (2021), it is not surprising that we find the largest impacts on younger individuals, white people, men, and that they are concentrated on weekend days.

Overall, the evidence highlights that our findings are consistent with ridesharing primarily increasing mortality via two channels: (1) Uber increases instances of "socializing" that can lead to riskier decision making while under the influence of narcotics or alcohol and (2) Uber may facilitate drug distribution. That said, the main limitation of our paper is that analyzing the exact underlying mechanisms driving these additional external-cause deaths falls outside our scope. Instead, we provide suggestive evidence using observable characteristics in our vital statistics data, while also highlighting findings from the existing literature on the effects of ridesharing. Examining how ridesharing affects substance use and related mortality helps us to understand the unintended consequences associated with the introduction of ridesharing, and alerts policymakers who can devote resources to mitigating the harmful consequences.

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Tables and Figures

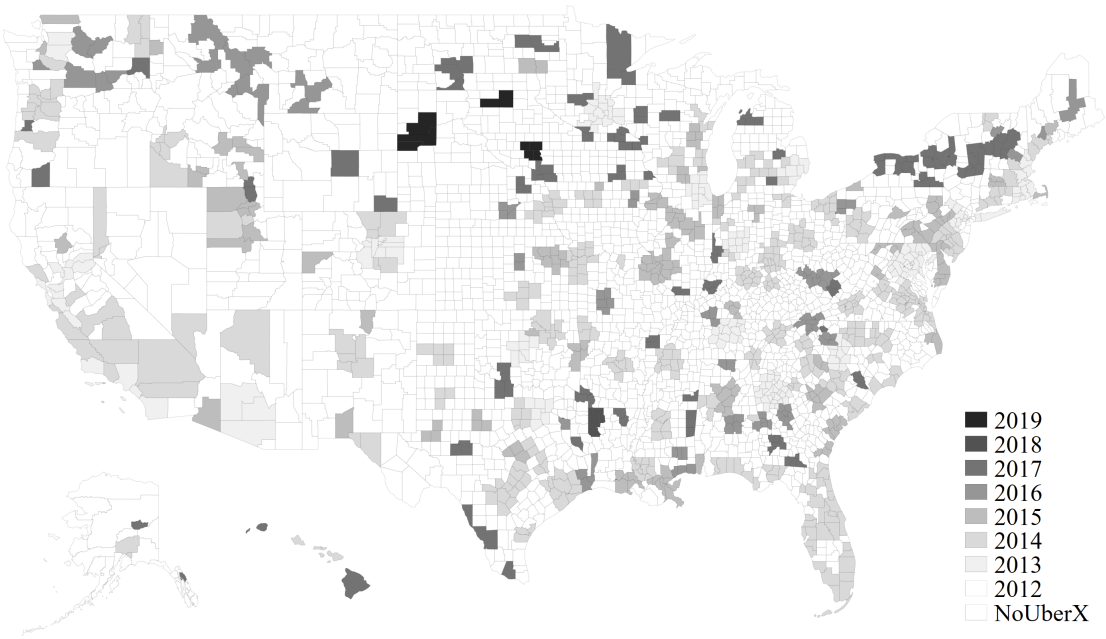
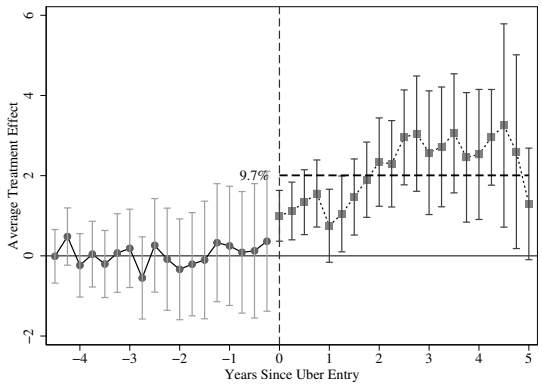
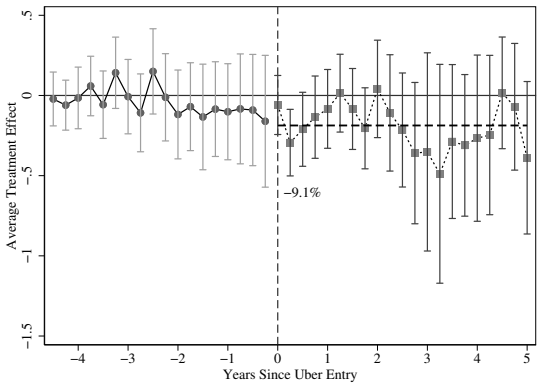


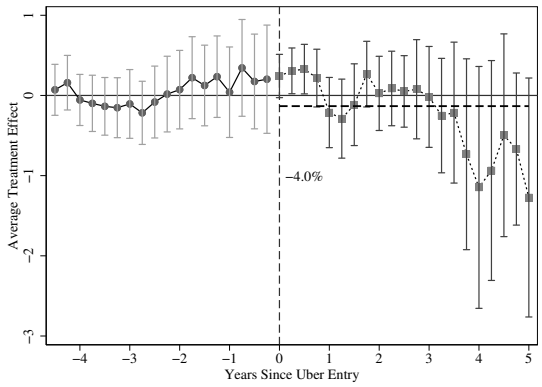
Figure 1: Entry of UberX By County Over Time



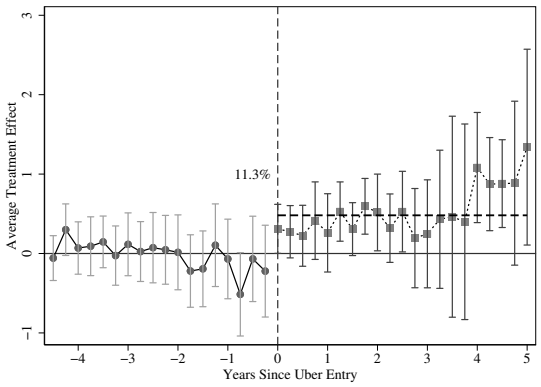
(a) External Cause Mortality



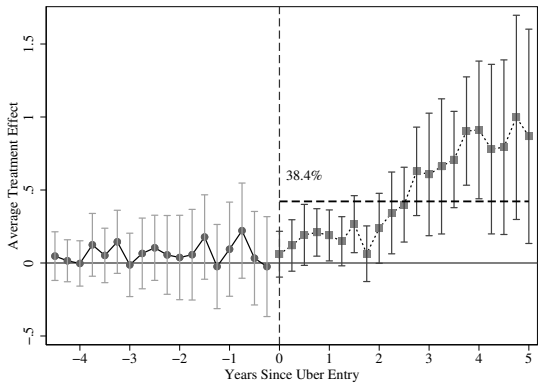
(b) Homicide



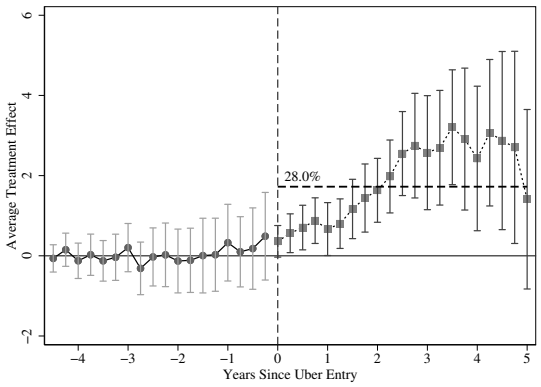
(c) Motor Vehicle



(d) Suicide



(e) Alcohol



(f) Drugs

Figure 2: Event Studies for Main Outcomes - Difference-in-differences Imputation (Borusyak et al., 2021). Dashed line refers to our difference-in-differences estimate as a percent value (i.e., the coefficient divided by the category mean).

Table 1: Summary Statistics

		Pre-Uber Entry		Post-Uber Entry	
		Mean	SD	Mean	SD
Outcomes (per 100k population)					
Internal Cause		14.03	10.49	12.77	9.08
External Cause		18.92	10.86	22.91	13.59
	Homicide	1.92	3.10	2.25	3.62
	Suicide	4.06	3.60	4.54	3.89
	MVA	3.48	3.88	3.13	3.39
	Alcohol	0.94	1.74	1.31	1.95
	Drugs	4.71	4.59	8.10	7.60
	Other	3.81	3.97	3.58	3.74
Covariates					
	Population (100,000s)	1.26	2.29	1.68	2.96
	Age	37.39	2.73	38.46	2.74
	Per Capita Income	41,139	10,305	51,174	15,314
	Poverty Rate	0.13	0.06	0.12	0.06
	% Unemployed	7.25	2.51	4.4	1.58
	% Bachelors or higher	25.02	9.11	32.57	10.82
	% White	0.83	0.14	0.8	0.15
	% Men	0.5	0.02	0.5	0.01
Total # of CBSAs		329	329	329	329
Total # of Counties		639	639	639	639

Note: Ever treated, central counties. Mortality rates and population are for ages 18-45. Income, poverty rate, unemployment, bachelors or higher, are all at the county level and not specific to certain ages or demographics.

Table 2: Estimates of UberX's Impact on Mortality

	Outcome: Deaths per 100k Population in the Noted Category							
	Age 18-45	Age 46-64	All Counties	CSDiD	TWFE	Wooldridge	Age 18-45	Age 18-45
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
External Cause	2.008***	1.892	2.988***	2.061***	1.171***	2.242***	2.012***	1.141***
	(0.392)	(2.067)	(1.058)	(0.649)	(0.279)	(0.413)	(0.383)	(0.399)
	[20.61]	[52.95]	[21.28]	[20.61]	[20.61]	[20.61]	[20.61]	[20.61]
Homicide	-0.187	-0.007	0.007	-0.168	-0.038	-0.294*	-0.092	-0.285*
	(0.141)	(0.073)	(0.148)	(0.162)	(0.060)	(0.155)	(0.144)	(0.154)
	[2.06]	[0.89]	[1.80]	[2.06]	[2.06]	[2.06]	[2.06]	[2.06]
Suicide	0.481**	-0.929***	0.33	0.759***	0.290***	0.511***	0.504*	0.387*
	(0.196)	(0.203)	(0.473)	(0.276)	(0.089)	(0.150)	(0.199)	(0.204)
	[4.26]	[5.02]	[4.56]	[4.26]	[4.26]	[4.26]	[4.26]	[4.26]
MVA	-0.133	0.234	0.006	-0.211	0.204**	-0.244	-0.311	-0.37
	(0.259)	(0.311)	(0.784)	(0.285)	(0.102)	(0.33)	(0.264)	(0.267)
	[3.33]	[2.78]	[4.62]	[3.33]	[3.33]	[3.33]	[3.33]	[3.33]
Alcohol	0.422***	0.103	0.396***	0.540***	0.044	0.400***	0.462***	0.381***
	(0.113)	(0.187)	(0.100)	(0.154)	(0.051)	(0.073)	(0.114)	(0.113)
	[1.10]	[1.60]	[0.97]	[1.10]	[1.10]	[1.10]	[1.10]	[1.10]
Drugs	1.724***	2.979	2.239***	1.331***	0.519***	1.776***	1.939***	1.581***
	(0.458)	(2.396)	(0.446)	(0.476)	(0.180)	(0.584)	(0.469)	(0.444)
	[6.15]	[30.93]	[5.63]	[6.15]	[6.15]	[6.15]	[6.15]	[6.15]
CBSAs	329	329	329	329	329	329	329	329
Counties	639	639	1,057	639	639	639	639	639
Population & Unemployment							Yes	Yes
Income (per capita)							Yes	Yes
Demographic Controls								Yes

Notes: Estimates based on data from NVSS with the sample restricted to central counties that UberX ever enters. Estimates come from "DiD Imputation" (Borusyak et al., 2021). Death counts are denominated by the number of county residents belonging to the applicable demographic group analyzed in that specification. Estimates should be interpreted as the effect of UberX entry on the mortality rate per 100,000 residents. These estimates are unweighted, and standard errors are clustered at CBSA or CBSA Division level. Numbers in square brackets are the respective category means. Unless otherwise stated, we rely on the "DiD Imputation" estimator (Borusyak et al., 2021), age 18-45, and central ever-treated counties. Demographic controls include average population age, poverty rate, percent bachelor's degree, fraction white, and fraction men. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Mechanisms

	Outcome: Deaths per 100k Population Age 18-45 in the Noted Category					
	Weekend	Weekday	Away from Home	At Home	Below Median All-Transit Scores	Above Median All-Transit Scores
	(1)	(2)	(3)	(4)	(5)	(6)
External Cause	1.598***	0.419**	0.881***	0.556*	2.719***	1.167**
	(0.368)	(0.214)	(0.277)	(0.309)	(0.489)	(0.553)
	[12.37]	[8.22]	[10.57]	[6.60]	[23.18]	[18.85]
Homicide	-0.193*	0.006	-0.016	-0.070*	-0.066	-0.087
	(0.118)	(0.050)	(0.105)	(0.039)	(0.166)	(0.158)
	[1.26]	[0.79]	[1.45]	[0.34]	[2.06]	[2.05]
Suicide	0.323***	0.168	0.282*	0.041	0.737***	0.361**
	(0.112)	(0.194)	(0.152)	(0.300)	(0.233)	(0.159)
	[2.44]	[1.82]	[1.73]	[2.20]	[4.89]	[3.83]
MVA	0.125	-0.258*	-0.147	-0.014	-0.350	0.293*
	(0.200)	(0.147)	(0.231)	(0.010)	(0.316)	(0.171)
	[2.14]	[1.19]	[2.77]	[0.02]	[4.19]	[2.75]
Alcohol	0.335***	0.091	0.244***	0.164**	0.584***	0.022
	(0.057)	(0.073)	(0.060)	(0.073)	(0.136)	(0.150)
	[0.71]	[0.39]	[0.52]	[0.47]	[1.12]	[1.08]
Drugs	1.000***	0.720***	0.732***	0.649***	2.308***	0.696
	(0.322)	(0.198)	(0.282)	(0.176)	(0.570)	(0.435)
	[3.64]	[2.50]	[2.36]	[2.70]	[6.69]	[5.77]
CBSAs	329	329	329	329	162	167
Counties	639	639	639	639	260	379

Notes: Estimates based on data from NVSS with the sample restricted to central counties that UberX ever enters. Estimates use difference-in-differences imputation estimator (Borusyak et al., 2021). Estimates should be interpreted as the effect of UberX entry on the mortality rate per 100,000 residents. Death counts are per 100,000 county residents belonging to the applicable demographic group analyzed in that specification. These estimates are unweighted, and standard errors are clustered at CBSA or CBSA Division level. Numbers in square brackets are the respective category means. In Columns 5 and 6, CBSAs are separated into below and above-median All-Transit Scores based on All-Transit data obtained from https://alltransit.cnt.org/rankings/. The median CBSA-level All-Transit Score is 1.4. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Heterogeneity of Effects by Demographic Characteristics

	Outcome: Deaths per 100k Population Age 18-45 in the Noted Category					
	Men	Women	White	Other Race	Age 18-30	Age 31-45
	(1)	(2)	(3)	(4)	(5)	(6)
External Cause	1.850**	2.380***	2.315***	3.969**	0.546	3.486***
	(0.889)	(0.773)	(0.546)	(1.669)	(0.433)	(0.551)
	[29.98]	[11.19]	[21.40]	[18.96]	[17.77]	[23.74]
Homicide	-0.543*	0.180***	-0.037	-0.241	-0.287	-0.088
	(0.295)	(0.064)	(0.108)	(0.489)	(0.214)	(0.146)
	[3.38]	[0.76]	[1.11]	[4.49]	[2.47]	[1.68]
Suicide	0.605	0.347**	0.436*	1.011***	-0.372	1.359***
	(0.451)	(0.144)	(0.259)	(0.382)	(0.323)	(0.394)
	[6.68]	[1.80]	[4.73]	[2.90]	[3.95]	[4.62]
MVA	-0.376	0.177	-0.133	-0.598	-0.094	-0.215
	(0.501)	(0.169)	(0.286)	(0.628)	(0.300)	(0.346)
	[4.90]	[1.76]	[3.49]	[3.29]	[3.95]	[2.83]
Alcohol	0.516***	0.335***	0.503***	0.020	0.197*	0.648***
	(0.180)	(0.073)	(0.108)	(0.452)	(0.114)	(0.151)
	[1.69]	[0.50]	[1.21]	[0.80]	[0.71]	[1.49]
Drugs	2.106***	1.390***	2.102***	2.145***	1.166**	2.327***
	(0.569)	(0.431)	(0.480)	(0.496)	(0.527)	(0.532)
	[8.17]	[4.11]	[6.93]	[3.87]	[4.07]	[8.26]
CBSAs	329	329	329	329	329	329
Counties	639	639	639	639	639	639

Notes: Estimates based on data from NVSS with the sample restricted to central counties that UberX ever enters. Estimates use difference-in-differences imputation estimator (Borusyak et al., 2021). Estimates should be interpreted as the effect of UberX entry on the mortality rate per 100,000 residents. Death counts are per 100,000 county residents belonging to the applicable demographic group analyzed in that specification. These estimates are unweighted, and standard errors are clustered at CBSA or CBSA Division level. Numbers in square brackets are the respective category means. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Additional Sensitivity and Robustness

	Outcome: Deaths per 100k Population Age 18-45 in the Noted Category		
	Age <18	Overlapping Estimates	County of Residence
	(1)	(2)	(3)
External Cause	-0.031	2.008***	1.809***
	(0.337)	(0.395)	(0.533)
	[3.83]	[20.61]	[20.78]
Homicide	-0.027	-0.187	-0.258**
	(0.059)	(0.142)	(0.128)
	[0.60]	[2.06]	[2.11]
Suicide	-0.290	0.476**	0.234
	(0.222)	(0.196)	(0.156)
	[0.63]	[4.26]	[4.27]
MVA	0.140	-0.130	-0.329
	(0.121)	(0.259)	(0.213)
	[0.91]	[3.34]	[3.36]
Alcohol	-0.032	0.396***	0.413**
	(0.024)	(0.100)	(0.163)
	[0.24]	[1.41]	[1.11]
Drugs	0.014	2.030***	1.903***
	(0.021)	(0.488)	(0.420)
	[0.14]	[7.28]	[6.24]
CBSAs	329	329	329
Counties	639	639	639

Notes: Estimates based on data from NVSS with the sample restricted to central counties that UberX ever enters. The estimates are unweighted, and standard errors are clustered at CBSA or CBSA Division level. Numbers in square brackets are the respective category means. Overlapping estimates refers to counting any deaths listing the given cause, rather than using the Carpenter and Dobkin (2009) priority ranking approach to assign each death to a single category (see text for more information). County of residence means counting deaths based on where the individual resides rather than where they died. *** p<0.01, ** p<0.05, * p<0.1.

Appendix A - Additional Estimates

A.1 Estimates Excluding Drugs and Alcohol Deaths

Table A1 presents estimates for external causes where we exclude any death that involves alcohol or drugs in any way. We present the corresponding estimates including alcohol and drugs for ease of comparison.

Table A1: Estimates Stratified by Alcohol or Drug Involvement

	Outcome: Deaths per 100k Population in the Noted Category	
	Involving Alcohol or Drugs	Not Involving Alcohol or Drugs
	Age 18-45	Age 18-45
	(1)	(2)
External Cause	2.146***	-0.138
	(0.505)	(0.407)
	[7.24]	[13.37]
Homicide	-0.093	-0.095
	(0.065)	(0.145)
	[0.04]	[2.02]
Suicide	0.142**	0.339
	(0.058)	(0.209)
	[0.44]	[3.83]
MVA	-0.043	-0.091
	(0.076)	(0.208)
	[0.20]	[3.13]
CBSAs	329	329
Counties	639	639

Notes: Estimates based on data from NVSS with the sample restricted to central counties that UberX ever enters. Estimates use (Borusyak et al., 2021) difference-in-differences imputation estimator. Deaths are assigned to Homicide, Suicide, and Motor Vehicle in the same manner used in our main estimates. Deaths in these categories are then stratified according to whether any alcohol or drug causes are also listed on the death certificate. Estimates should be interpreted as the effect of UberX entry on the mortality rate per 100,000 residents. Death counts are denominated by the number of county residents belonging to the applicable demographic group analyzed in that specification. These estimates are unweighted, and standard errors are clustered at CBSA or CBSA Division level.*** p<0.01, ** p<0.05, * p<0.1.

A.2 Predicting Uber Entry

In Table A2, we regress the timing of Uber entry on various changes in area level of characteristics in the years leading up to Uber entry (2008 to 2011). The goal of this exercise is to eliminate the potential for Uber's entry to be correlated with other changes that might explain increases in mortality.

To begin, in Panel A of Table A2 we show how UberX entry is related to changes in our main outcomes of interest in the pre-entry period. If the timing of UberX entry across counties was correlated with differences in trends in external cause mortality, that would undermine our identification strategy. However, the estimates in Panel A are small in magnitude and statistically insignificant.

In Panel B, we examine how UberX is related to the overall economic environment. Broadly speaking, we find that UberX entry timing is only mildly related to these outcomes. For example, UberX entered later, on average, in areas with higher growth in real per-capita income. Our estimate implies that an additional $1,000 of average real per-capita annual income growth prior to 2012 was associated with a 0.013 quarter later entry, which is a little more than one day. Further, this estimate is only marginally significant at the 10% level. Further, our estimates suggest that a 1 percentage point increase in the rate of growth in unemployment predicts UberX entering a county roughly 0.165 quarters earlier, or about 15 days. While these effects may seem negligible, we also show that our estimates are robust to controlling for income, unemployment, and population in Table 2 in the text of the paper.

One further potential threat to identification would be if UberX was more likely to enter those areas where fentanyl was already a growing problem. Put differently, if the timing of UberX entry was correlated with the size of changes in fentanyl-related deaths, we could expect to see more drug deaths in the years after UberX entry. In a sense, this would be a reverse causation story, with more drug deaths predicting earlier Uber entry. However, the estimate in column (1) of Panel B in Table A2, suggests there was no relationship between UberX entry and pre-entry growth in fentanyl deaths.

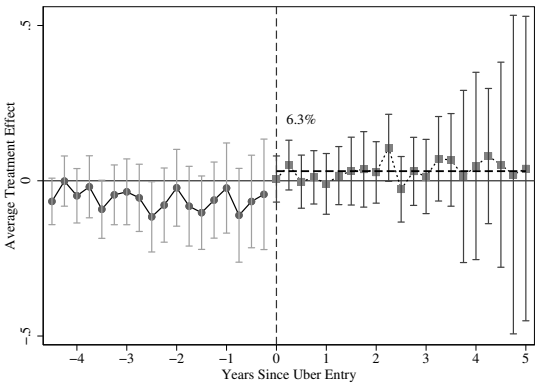
Table A2: Predicting Year & Quarter of UberX Entry

	Panel A					
	External-Cause Mortality Rate	Homicide Mortality Rate	Suicide Mortality Rate	MVA Mortality Rate	Alcohol Mortality Rate	Drug Mortality Rate
	(1)	(2)	(3)	(4)	(5)	(6)
Variable Coefficient	-0.001	0.001	0.003	-0.003	0.004	-0.003
	(0.001)	(0.004)	(0.003)	(0.002)	(0.007)	(0.003)
	[-0.016]	[0.009]	[0.013]	[-0.024]	[0.0014]	[0.012]
	Panel B					
	Fentanyl Mortality Rate	Unemploy-ment Rate	Real Per-capita Income	Population 18-45		
	(1)	(2)	(3)	(4)		
Variable Coefficient	-0.010	-0.165**	0.013*	6.194		
	(0.010)	(0.069)	(0.007)	(19.076)		
	[-0.0007]	[0.10]	[0.023]	[132.31]		
CBSAs	329	329	329	329	329	329
Counties	639	639	639	639	639	639

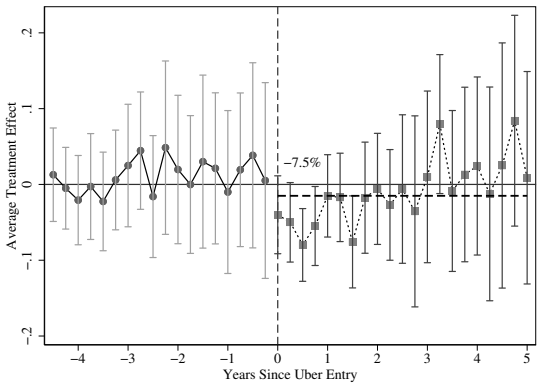
Notes: Regressions estimated using Ordinary Least Squares. The unit of observation is the county-year-quarter level from 2008-2011. The dependent variable is the year and quarter of UberX entry. Column headings refer to the explanatory variable in the regression, using its first-difference in the regression. Estimates should be interpreted as the effect of the growth rate (in levels) in the value of the column variable prior to 2012 on UberX entry timing in quarters. For population 18-45 we convert our estimates to percent changes for ease of interpretation. Unemployment rate is in units, e.g. a value of 5.0 indicates a 5% unemployment rate. Real per-capita income is in 2007 dollars in 1,000s. Population is in actual units and our estimate is expressed as percentage point changes in the growth rate with mean percent growth rate in brackets. These estimates are unweighted, include year-quarter fixed effects, with standard errors clustered at CBSA or CBSA Division level. Numbers in square brackets are the respective category means. *** p<0.01, ** p<0.05, * p<0.1.

A.3 Falsification Tests

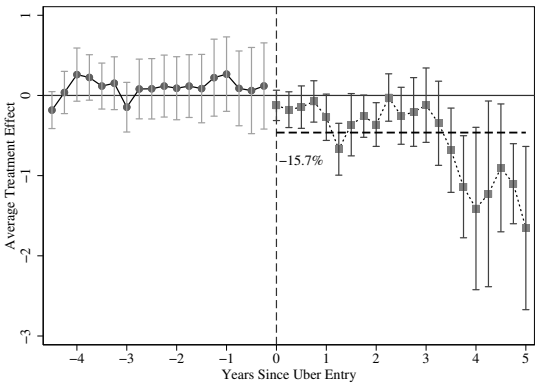
Table A3 examines mortality outcomes that we would not expect to be affected by changes in transportation accessibility, including deaths from cancer and diabetes. In order to minimize the degree of overlap between these categories of mortality and external-cause deaths, we exclude deaths from Table A3 that contain any external-cause ICD code. We see a statistically significant decline in deaths relating to cardiovascular events, such as heart attacks, after UberX enters an area. None of the other causes of death show a significant change and we suspect the effect we see for heart deaths is not causal. For example, when we look at weekend versus weekday deaths, the coefficients are similar (-0.239 and -0.226). In contrast, deaths from external causes plausibly related to UberX were concentrated heavily on weekend days, when UberX is busiest. One other plausible explanation for the observed decline in heart-related deaths is the increased availability of transportation (i.e., UberX) could facilitate faster access to medical care (Moskatel and Slusky, 2019). For the other outcomes, they are either too rare (e.g., stroke) or would not benefit from a marginal improvement in the time it takes to receive care (e.g., cancer). For completeness, in Figure A1, we provide event study analyses for these placebo outcomes.



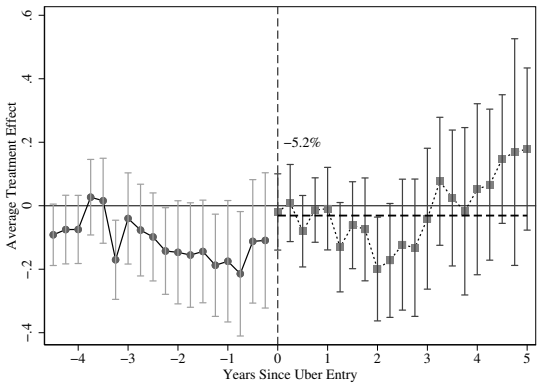
(a) Stroke Mortality



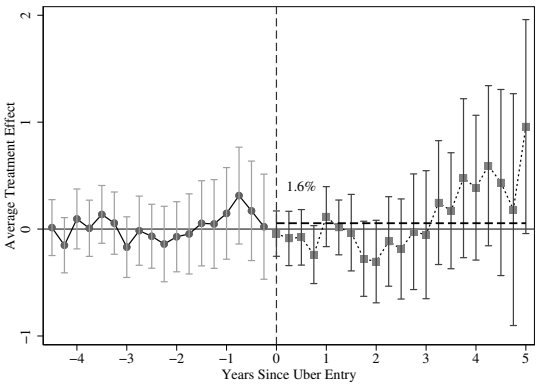
(b) Respiratory



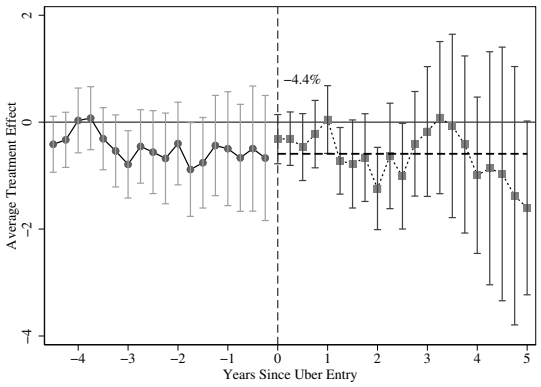
(c) Heart



(d) Diabetes



(e) Cancer



(f) Internal Causes

Figure A1: Event Studies for Falsification Outcomes - Difference-in-differences Imputation (Borusyak et al., 2021).

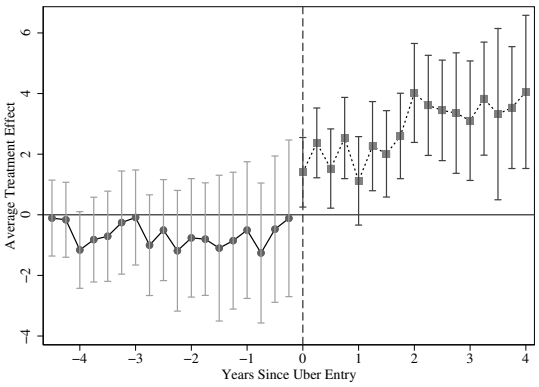
Table A3: Estimates For Falsification Outcomes

	Outcome: Deaths per 100k Population in the Noted Category		
	Age 18-45	Weekend 18-45	Weekday 18-45
	(1)	(2)	(3)
Stroke	0.031	0.030	0.001
	(0.050)	(0.037)	(0.028)
	[0.49]	[0.28]	[0.216]
Respiratory	-0.015	-0.026	0.010
	(0.031)	(0.026)	(0.012)
	[0.20]	[0.12]	[0.08]
Heart	-0.463***	-0.239***	-0.226
	(0.141)	(0.088)	(0.163)
	[2.94]	[1.72]	[1.46]
Diabetes	-0.031	-0.066	0.039
	(0.602)	(0.054)	(0.033)
	[0.60]	[0.34]	[0.26]
Cancer	0.055	0.114	-0.059
	(0.194)	(0.130)	(0.106)
	[3.40]	[1.94]	[1.46]
Internal Causes	-0.592	-0.178	-0.417**
	(0.451)	(0.394)	(0.208)
	[13.50]	[7.75]	[5.74]
CBSAs	329	329	329
Counties	639	639	639

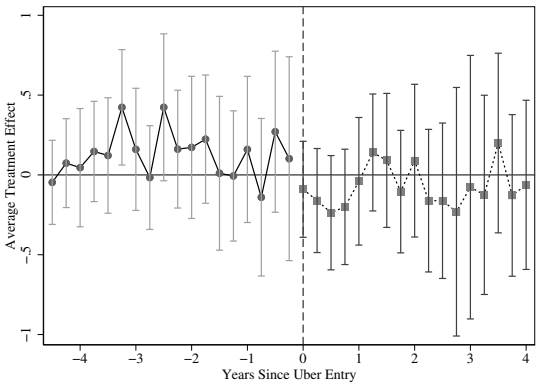
Notes: Estimates based on 2008-2019 data from NVSS with the sample restricted to central counties that UberX ever enters. Estimates use (Borusyak et al., 2021) difference-in-differences imputation estimator. Estimates should be interpreted as the effect of UberX entry on the mortality rate per 100,000 residents. Death counts are denominated by the number of county residents belonging to the applicable demographic group analyzed in that specification. Falsification outcomes exclude deaths that contain any external cause of death from Table B1. These estimates are unweighted, and standard errors are clustered at CBSA or CBSA Division level.*** p<0.01, ** p<0.05, * p<0.1.

A.4 Additional Event Studies

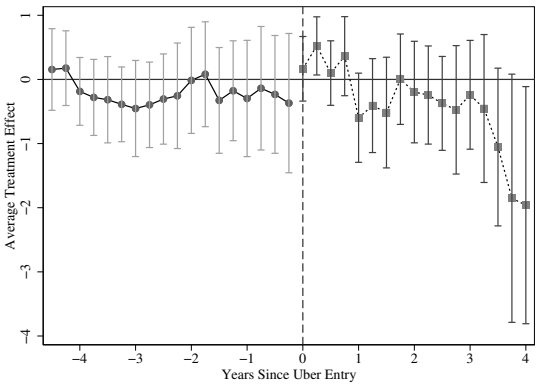
Figure A2 shows event studies that are the same specification as in the main text of the paper but where we limit the sample to CBSAs with below-median TCI scores. Figure A3 shows these event studies for only those areas with above-median TCI scores. Neither set of figures shows evidence of problematic pretrends. However, the figures reinforce the idea that ridesharing has bigger effects in CBSAs where lack of transportation was a significant constraint prior to UberX entry. Note that we plot only four years of post-period coefficients in Figure A3 because we run out of "yet-to-be-treated" CBSAs towards the end of the sample period when limiting to those areas with above median TCI scores. For consistency, we also limit Figure A2 to four years of post UberX entry coefficients.



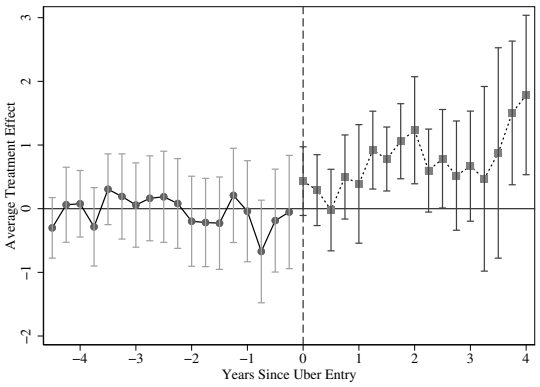
(a) External Cause Mortality



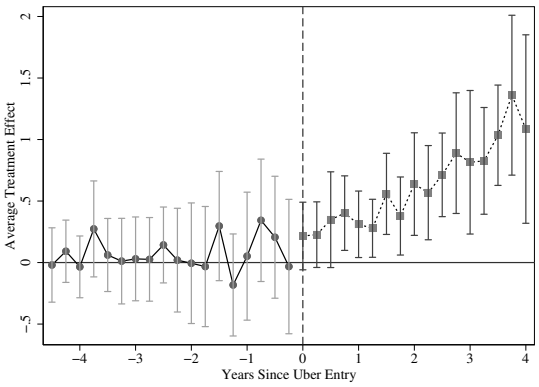
(b) Homicide



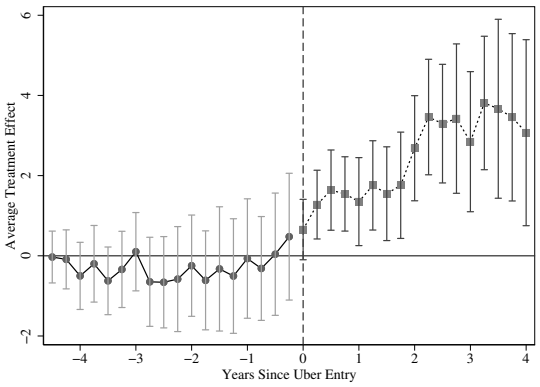
(c) Motor Vehicle



(d) Suicide

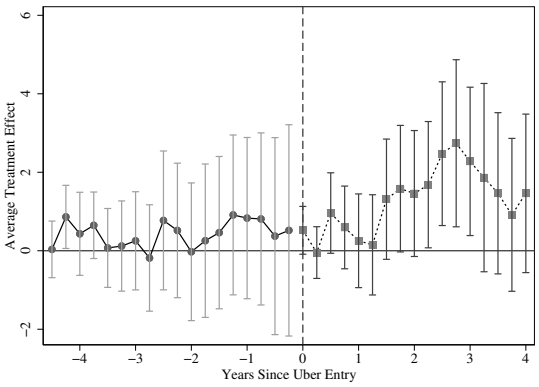


(e) Alcohol

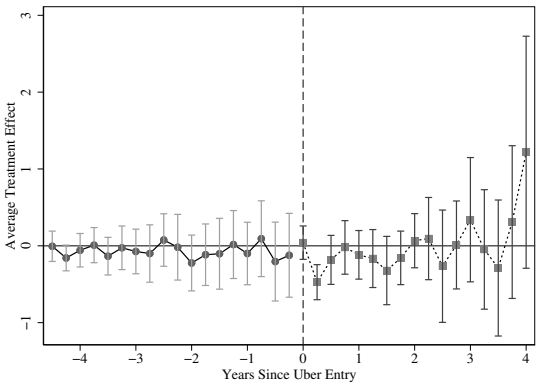


(f) Drugs

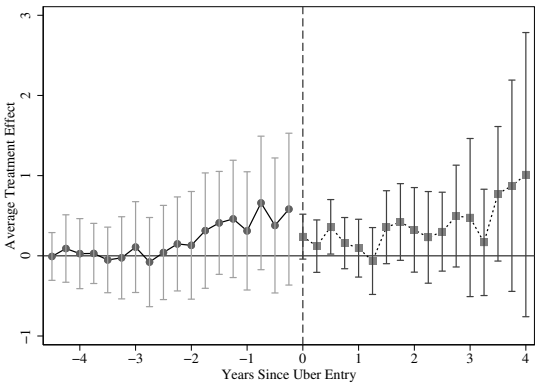
Figure A2: Event Studies for Main Outcomes in Below Median TCI Score Areas - Difference-in-differences Imputation (Borusyak et al., 2021).



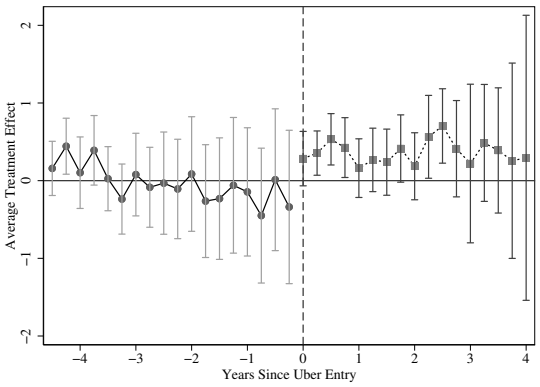
(a) External Cause Mortality



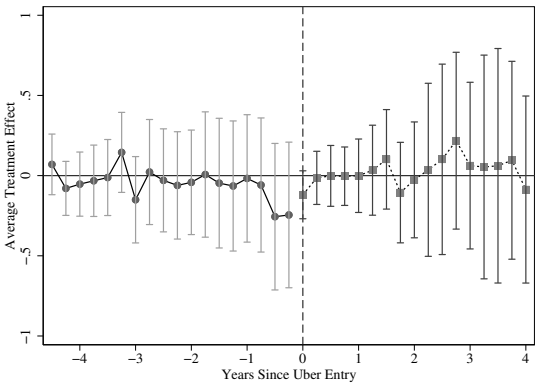
(b) Homicide



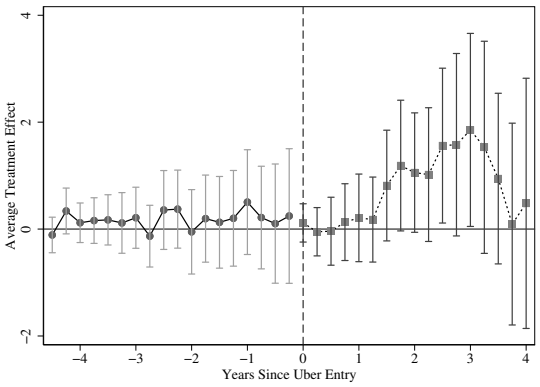
(c) Motor Vehicle



(d) Suicide



(e) Alcohol



(f) Drugs

Figure A3: Event Studies for Main Outcomes in Above Median TCI Score Areas - Difference-in-differences Imputation (Borusyak et al., 2021).

Appendix B ICD Codes by Category

In Table B1, we explain the ICD codes that form our homicide, MVA, suicide, alcohol, and drug cause of death categories. We focus on these categories of deaths because they are the leading causes of death among young people. Moreover, as we explain in detail in the text, these mortality categories are plausibly affected by the advent of ridesharing services.

We again remind the reader that we follow Carpenter and Dobkin (2009) by examining deaths relating to homicide, suicide, MVA, alcohol, and drugs. These are listed in order of decreasing "priority." In cases where the cause of death includes more than one of these categories, we assign the death to the higher priority category.

Table B1: Cause of Death Categories and ICD-10 Codes

Category	ICD-10 Codes
Homicide	X85, X86, X87, X88, X89, X9, Y0
Suicides	X6, X7, X80, X81, X82, X83, X84, X870
Motor Vehicle Accidents (MVA)	V01, V1, V2, V3, V4, V5, V6, V7, V8
Acute Alcohol-Induced	X45, X65, Y15, Y91, T51, X46, Y90, R780, K852, Z721, T518, T519
Drugs/Overdoses	F11, F12, F13, F14, F15, F16, F17, F18, F19, F55,
	T40, T41, T43, F55, X40, X42
	excluding {F116, F126, F136, F146, F156, F166, F176, F171, F172, F186, F196}
Other External Causes	The following codes excluding those previously specified above:
	V, W, X, Y, Z, K70, K860

Note: Table gives ICD-10 codes that comprise our main outcomes of interest. ICD-10 block descriptions are as follows: 'F00-F99' Mental and behavioral disorders, 'K00-K93' Diseases of the digestive system, 'R00-R99' Symptoms, signs and abnormal clinical and laboratory findings, 'S00-T88' Injury, poisoning and certain other consequences of external causes, 'V00-V99' Transport Accidents, 'W00-X85' Other External Causes of Accidental Injury, 'X71-X83' Intentional Self-Harm, 'X92-Y09' Assault, 'Y21-Y33' Events of Undetermined Intent, 'Y35-Y38' Legal Intervention, Operations of War, Military Operations, and Terrorism, 'Y62-Y84' Complications of Medical and Surgical Care, 'Y90-Y99' Supplementary Factors Related to Causes of Morbidity Classified Elsewhere, 'Z00-Z99' Factors influencing health status and contact with health services, not elsewhere classified.

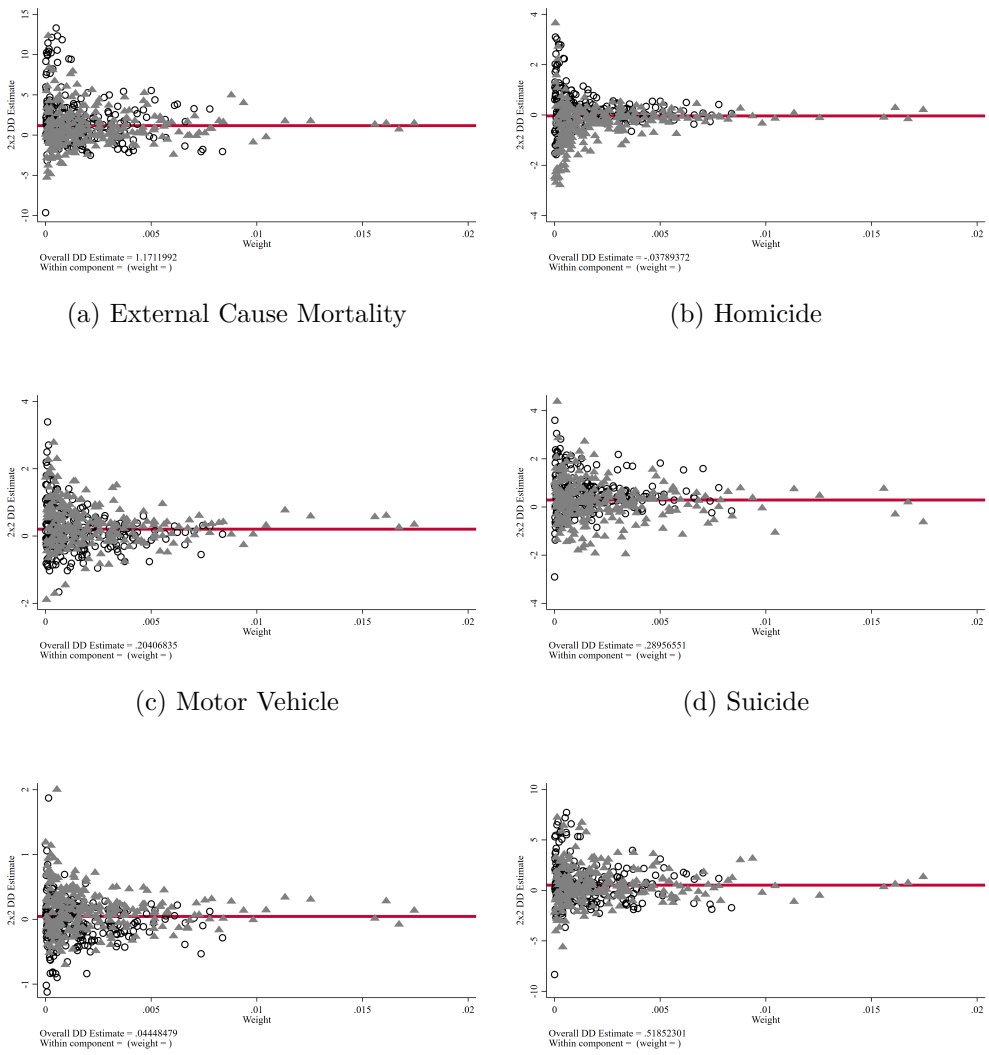
Appendix C - Heterogeneous Treatment Timing Bias

Figure C1 examines the severity of the treatment timing bias with UberX entry (using the Bacon Decomposition command *bacondecomp* in STATA; Goodman-Bacon et al., 2022). The figure shows the 2×2 difference-in-differences estimates for each year-quarter and county in our sample: treated versus not yet treated and later treated versus earlier treated. These individual estimates form the traditional TWFE (two-way fixed effects) treatment effect. The figure shows considerable evidence of what Borusyak et al. (2021) refer to as "forbidden" comparisons (i.e., comparing outcomes for later versus earlier treated groups). The Bacon Decomposition procedure indicates that the aggregate weight assigned to all early versus later treated comparisons is 62.9%, and the weight assigned to later versus earlier treated comparisons is 37.1% (i.e., the "forbidden" comparison). In all sub-categories of external causes except suicide, the aggregate late versus early ATT (average treatment on the treated) coefficient is smaller in magnitude than the earlier versus later treated comparisons, suggesting downward bias due to heterogeneous treatment effects.

In Table C1, we show how a TWFE approach leads to a downward bias. In that table, the estimates in columns (1), (3), and (5) use the TWFE difference-in-differences estimator without correcting for heterogeneous treatment effect bias. Column (1) shows the difference-in-differences estimate from early treated versus later treated comparison groups, and column (3) shows the estimate from later treated versus early treated comparison groups, (i.e., the "forbidden" comparison). Columns (2) and (4) show the respective aggregate weight assigned to each comparison type, from the *bacondecomp* command. Column (5) shows the aggregate difference-in-differences TWFE estimate, which is a weighted average of the estimates in columns (1) and (3). Estimates in column (6) are from a difference-in-differences imputation estimator (Borusyak et al., 2021) for comparison purposes.

Further, event study coefficients calculated using the newer difference-in-difference methods are not all relative to period $t = -1$. In particular, when using the Borusyak et al. (2021) imputation approach, the pre-period coefficients compare to the earliest pre-treatment period while the post period coefficients compare to the average of the pre-treatment periods. This creates the potential for a discontinuity at the treatment date. To check for this issue in our setting we follow the recommendation of Roth (2024) to report Callaway and Sant'Anna (2021) event studies while

using the "long2" package option, which forces symmetric calculation of the treatment effects. We report those in Figure C2. They show no evidence of an issue.



(a) External Cause Mortality

(b) Homicide

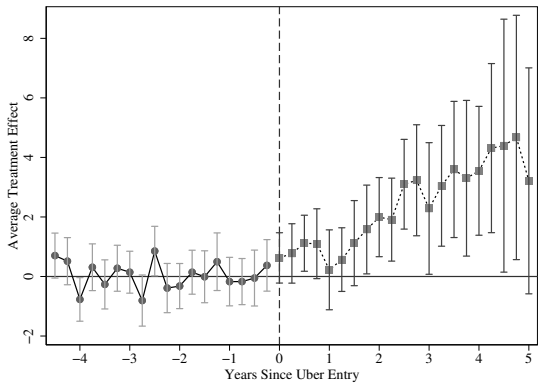
(c) Motor Vehicle

(d) Suicide

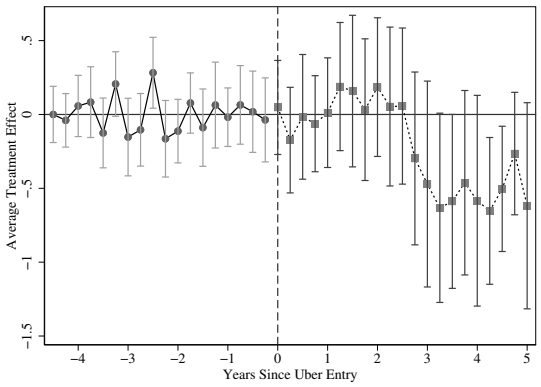
(e) Alcohol

(f) Drugs

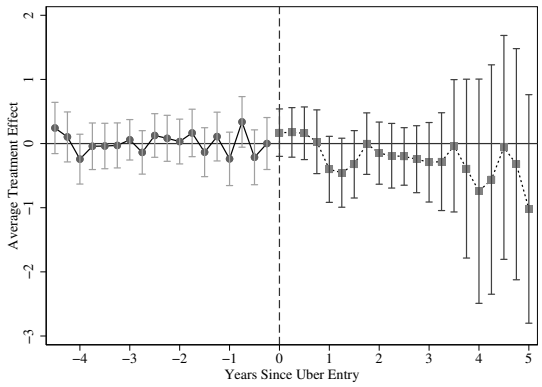
Figure C1: Goodman-Bacon Decomposition. The shaded triangles represent the "early vs. late" treated comparisons, and the unshaded circles represent the problematic "late vs. early" treated comparisons.



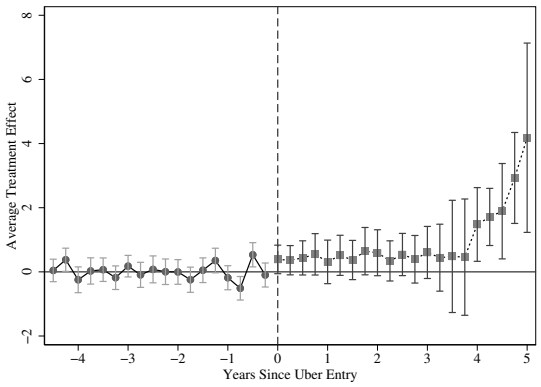
(a) External Cause Mortality



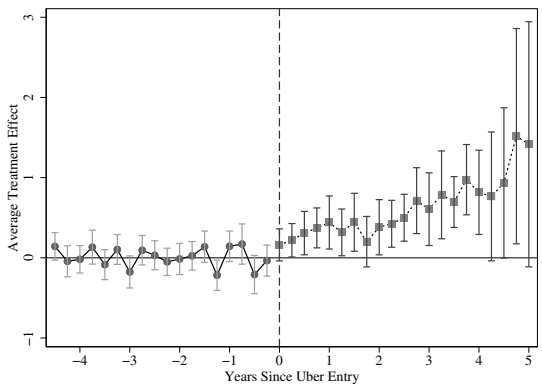
(b) Homicide



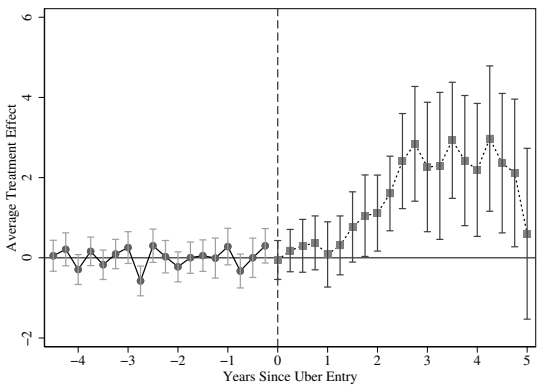
(c) Motor Vehicle



(d) Suicide

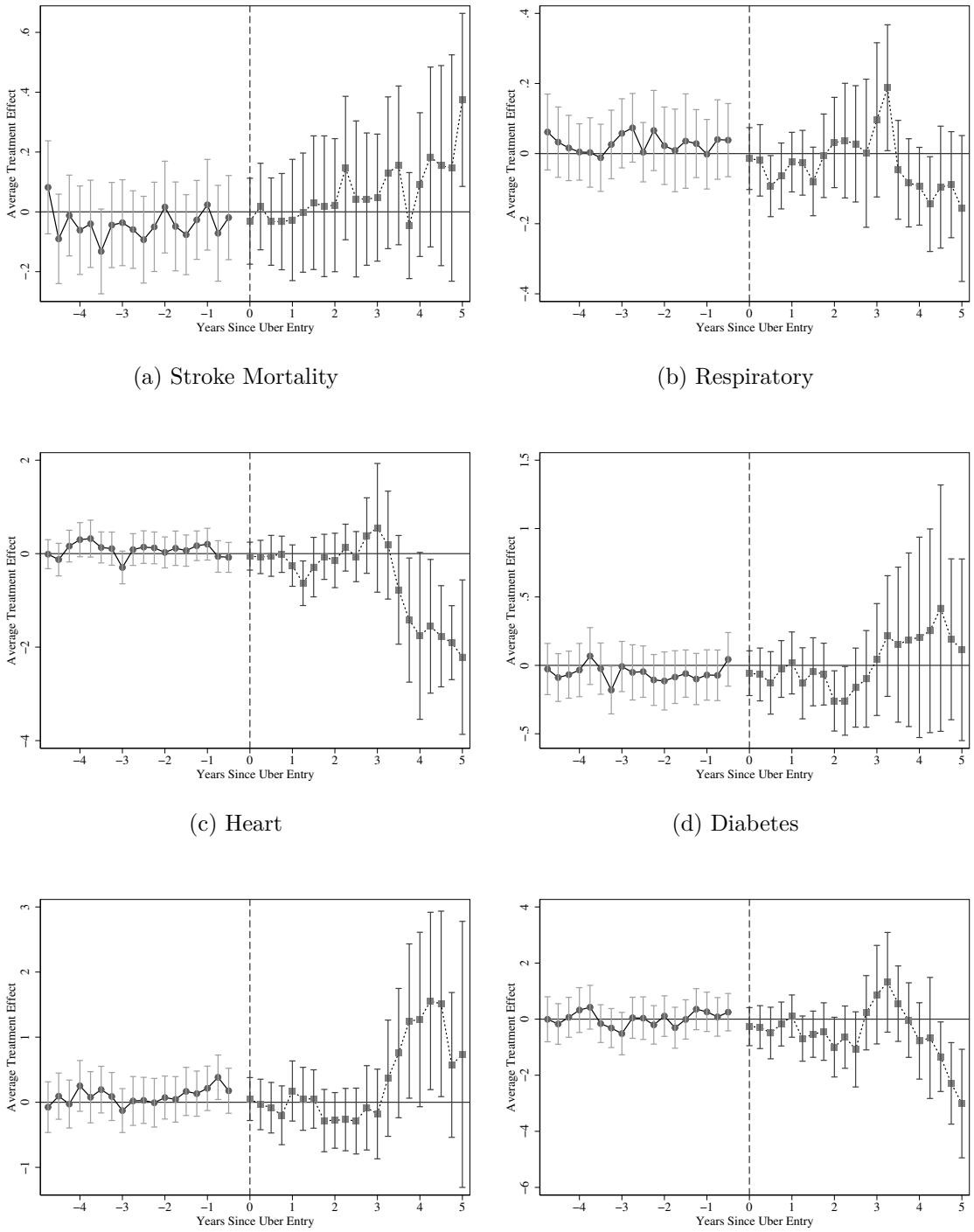


(e) Alcohol



(f) Drugs

Figure C2: Event Studies for Main Outcomes - Callaway and Sant'Anna (2021) "CSDiD" with "long2" option to deal with asymmetric coefficient calculations (Roth, 2024)



(a) Stroke Mortality

(b) Respiratory

(c) Heart

(d) Diabetes

(e) Cancer

(f) Internal Causes

Figure C3: Event Studies for Falsification Outcomes - Callaway and Sant'Anna (2021) "CSDiD" with "long2" option to deal with asymmetric coefficient calculations (Roth, 2024)

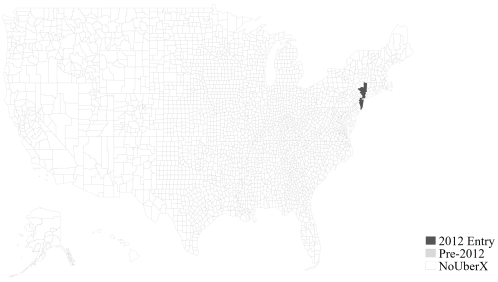
Table C1: Heterogeneous Treatment Effect Bias

	Outcome: Deaths per 100k Population Age 18-45 in the Noted Category					
	Early Treated vs Late Treated as Controls		Late Treated vs Early Treated as Controls			
	DID Coefficient	Weight Assigned	DID Coefficient	Weight Assigned	TWFE	DID Imputation
	(1)	(2)	(3)	(4)	(5)	(6)
External Cause	1.083	0.629	1.285	0.371	1.171***	2.008***
					(0.279)	(0.395)
Homicide	-0.141	0.629	0.134	0.371	-0.038	-0.187
					(0.060)	(0.142)
Suicide	0.133	0.629	0.550	0.371	0.290***	0.481**
					(0.089)	(0.196)
MVA	0.276	0.629	0.075	0.371	0.204**	-0.133
					(0.102)	(0.259)
Alcohol	0.111	0.629	-0.074	0.371	0.044	0.422***
					(0.051)	(0.113)
Drugs	0.551	0.629	0.455	0.371	0.519***	1.724***
					(0.180)	(0.461)
CBSAs	329	329	329	329	329	329
Counties	639	639	639	639	639	639

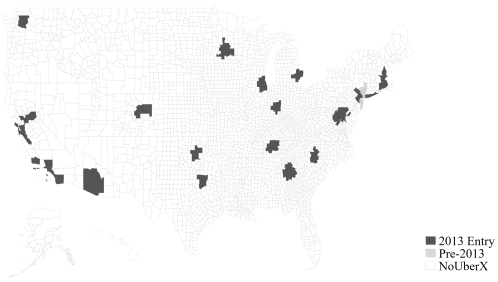
Notes: Estimates based on data from NVSS with the sample restricted to central counties that UberX ever enters. Estimates should be interpreted as the effect of UberX entry on the mortality rate per 100,000 residents. Death counts are per 100,000 county residents belonging to the applicable demographic group analyzed in that specification. These estimates are unweighted, and standard errors are clustered at CBSA or CBSA Division level. The *bacondecomp* command only provides standard errors for the aggregate DiD estimate, which are provided in column (5) with statistical significance denoted, unlike columns (1) and (3). *** p<0.01, ** p<0.05, * p<0.1.

Appendix D - Uber Entry Dates and Maps

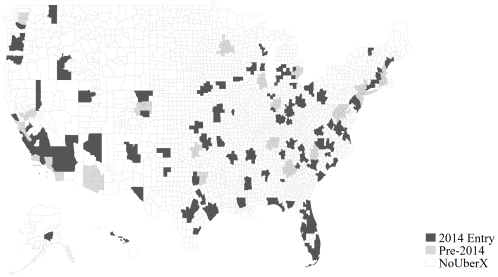
In this section we provide a complete list of all UberX entry dates. As with any hand-collected data, we cannot be certain they are 100% accurate. We also offer a series of maps showing cumulative UberX entry dates across U.S. counties by year.



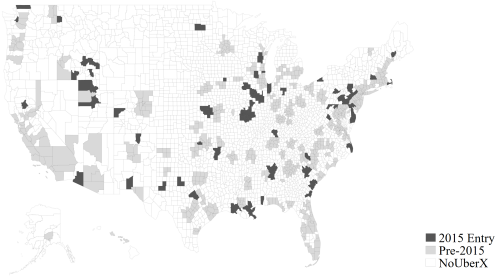
(a) 2012 UberX Entry



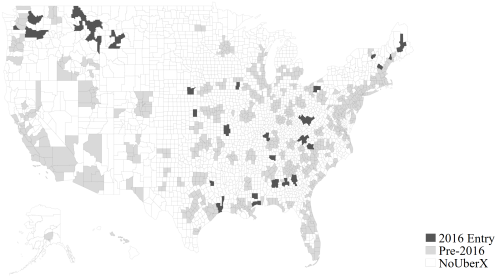
(b) 2013 UberX Entry



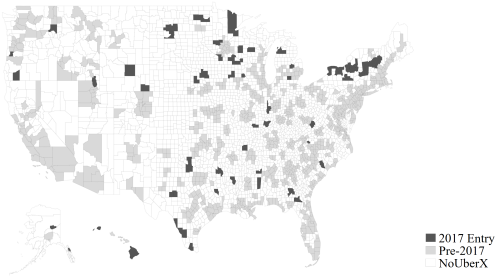
(c) 2014 UberX Entry



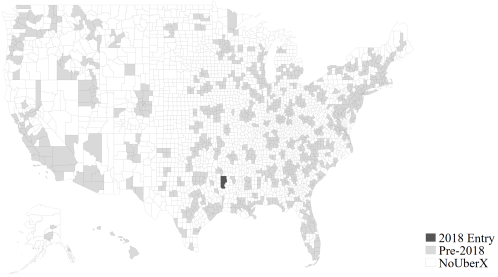
(d) 2015 UberX Entry



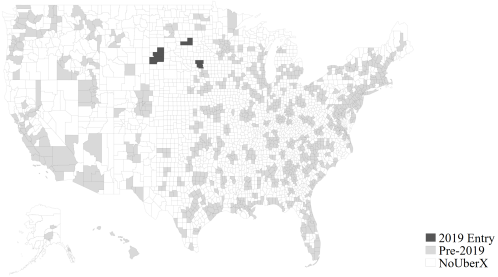
(e) 2016 UberX Entry



(f) 2017 UberX Entry



(g) 2018 UberX Entry



(h) 2019 UberX Entry

Figure D1: Entry of UberX By County Over Time

Table D1: Uber Entry Dates 1 of 7

CBSA Area/Division Code (1)	CBSA Area/Division Title (2)	Date UberX Entered (3)	CBSA Area/Division Code (1)	CBSA Area/Division Title (2)	Date UberX Entered (3)
20524*	Dutchess County-Putnam County, NY	08/24/2012	40900	Sacramento–Roseville–Arden-Arcade, CA	09/30/2013
35614*	New York-Jersey City-White Plains, NY-NJ	08/24/2012	19740	Denver-Aurora-Lakewood, CO	10/04/2013
36084*	Oakland-Hayward-Berkeley, CA	01/18/2013	46060	Tucson, AZ	10/10/2013
41884*	San Francisco-Redwood City-South San Francisco, CA	01/18/2013	36420	Oklahoma City, OK	10/30/2013
42034*	San Rafael, CA	01/18/2013	42200	Santa Maria-Santa Barbara, CA	10/31/2013
11244*	Anaheim-Santa Ana-Irvine, CA	03/14/2013	19804*	Detroit-Dearborn-Livonia, MI	10/31/2013
31084*	Los Angeles-Long Beach-Glendale, CA	03/14/2013	47664*	Warren-Troy-Farmington Hills, MI	10/31/2013
42644*	Seattle-Bellevue-Everett, WA	04/11/2013	19124*	Dallas-Plano-Irving, TX	11/05/2013
16974*	Chicago-Naperville-Arlington Heights, IL	04/22/2013	12580	Baltimore-Columbia-Towson, MD	11/06/2013
20994*	Elgin, IL	04/22/2013	45940	Trenton, NJ	11/13/2013
29404*	Lake County-Kenosha County, IL-WI	04/22/2013	35084*	Newark, NJ-PA	11/13/2013
41740	San Diego-Carlsbad, CA	05/09/2013	34980	Nashville-Davidson–Murfreesboro–Franklin, TN	12/10/2013
14454*	Boston, MA	06/04/2013	41500	Salinas, CA	02/04/2014
15764*	Cambridge-Newton-Framingham, MA	06/04/2013	42100	Santa Cruz-Watsonville, CA	02/04/2014
40484*	Rockingham County-Strafford County, NH	06/04/2013	23420	Fresno, CA	02/05/2014
12060	Atlanta-Sandy Springs-Roswell, GA	06/26/2013	26420	Houston-The Woodlands-Sugar Land, TX	02/21/2014
35004*	Nassau County-Suffolk County, NY	07/01/2013	18140	Columbus, OH	02/25/2014
41940	San Jose-Sunnyvale-Santa Clara, CA	07/24/2013	31540	Madison, WI	03/06/2014
43524*	Silver Spring-Frederick-Rockville, MD	08/08/2013	38300	Pittsburgh, PA	03/13/2014
47894*	Washington-Arlington-Alexandria, DC-VA-MD-WV	08/08/2013	17140	Cincinnati, OH-KY-IN	03/27/2014
33460	Minneapolis-St. Paul-Bloomington, MN-WI	09/04/2013	46140	Tulsa, OK	03/27/2014
26900	Indianapolis-Carmel-Anderson, IN	09/05/2013	33340	Milwaukee-Waukesha-West Allis, WI	03/28/2014
38060	Phoenix-Mesa-Scottsdale, AZ	09/05/2013	41700	San Antonio-New Braunfels, TX	03/28/2014
39300	Providence-Warwick, RI-MA	09/12/2013	14860	Bridgeport-Stamford-Norwalk, CT	04/01/2014
16740	Charlotte-Concord-Gastonia, NC-SC	09/27/2013	47220	Vineland-Bridgeton, NJ	04/01/2014

Notes: Wherever UberX entry is specified at the Core Based Statistical Area (CBSA) Division level, these cases are denoted by a star in column (1). Otherwise CBSA codes and titles refer to Core Based Statistical Areas.

Table D2: Uber Entry Dates 2 of 7

CBSA Area/Division Code (1)	CBSA Area/Division Title (2)	Date UberX Entered (3)	CBSA Area/Division Code (1)	CBSA Area/Division Title (2)	Date UberX Entered (3)
15804*	Camden, NJ	04/01/2014	33124*	Miami-Miami Beach-Kendall, FL	06/04/2014
33874*	Montgomery County-Bucks County-Chester County, PA	04/01/2014	46520	Urban Honolulu, HI	06/12/2014
33700	Modesto, CA	04/02/2014	12540	Bakersfield, CA	06/13/2014
40140	Riverside-San Bernardino-Ontario, CA	04/03/2014	30460	Lexington-Fayette, KY	06/13/2014
17460	Cleveland-Elyria, OH	04/08/2014	45780	Toledo, OH	06/13/2014
45104*	Tacoma-Lakewood, WA	04/08/2014	20500	Durham-Chapel Hill, NC	06/26/2014
45300	Tampa-St. Petersburg-Clearwater, FL	04/11/2014	21340	El Paso, TX	06/26/2014
11460	Ann Arbor, MI	04/22/2014	22180	Fayetteville, NC	06/26/2014
31140	Louisville/Jefferson County, KY-IN	04/24/2014	24660	Greensboro-High Point, NC	06/26/2014
32820	Memphis, TN-MS-AR	04/24/2014	31180	Lubbock, TX	06/26/2014
35300	New Haven-Milford, CT	04/24/2014	48900	Wilmington, NC	06/26/2014
39580	Raleigh, NC	04/26/2014	49180	Winston-Salem, NC	06/26/2014
10740	Albuquerque, NM	04/30/2014	12100	Atlantic City-Hammonton, NJ	06/27/2014
47260	Virginia Beach-Norfolk-Newport News, VA-NC	05/01/2014	36140	Ocean City, NJ	06/27/2014
17820	Colorado Springs, CO	05/02/2014	16700	Charleston-North Charleston, SC	07/10/2014
18580	Corpus Christi, TX	05/02/2014	17900	Columbia, SC	07/10/2014
27260	Jacksonville, FL	05/05/2014	24860	Greenville-Anderson-Mauldin, SC	07/10/2014
36540	Omaha-Council Bluffs, NE-IA	05/05/2014	34820	Myrtle Beach-Conway-North Myrtle Beach, SC-NC	07/10/2014
23104*	Fort Worth-Arlington, TX	05/05/2014	38900	Portland-Vancouver-Hillsboro, OR-WA	07/10/2014
44060	Spokane-Spokane Valley, WA	05/08/2014	12940	Baton Rouge, LA	07/11/2014
28140	Kansas City, MO-KS	05/09/2014	11100	Amarillo, TX	07/16/2014
42220	Santa Rosa, CA	05/12/2014	37100	Oxnard-Thousand Oaks-Ventura, CA	07/17/2014
41620	Salt Lake City, UT	05/27/2014	42020	San Luis Obispo-Paso Robles-Arroyo Grande, CA	07/17/2014
12420	Austin-Round Rock, TX	06/04/2014	25540	Hartford-West Hartford-East Hartford, CT	07/22/2014
36740	Orlando-Kissimmee-Sanford, FL	06/04/2014	21660	Eugene, OR	07/23/2014

Notes: Wherever UberX entry is specified at the Core Based Statistical Area (CBSA) Division level, these cases are denoted by a star in column (1). Otherwise CBSA codes and titles refer to Core Based Statistical Areas.

Table D3: Uber Entry Dates 3 of 7

CBSA Area/Division Code (1)	CBSA Area/Division Title (2)	Date UberX Entered (3)	CBSA Area/Division Code (1)	CBSA Area/Division Title (2)	Date UberX Entered (3)
41420	Salem, OR	07/23/2014	30700	Lincoln, NE	08/28/2014
22420	Flint, MI	07/24/2014	37060	Oxford, MS	08/28/2014
24340	Grand Rapids-Wyoming, MI	07/24/2014	43780	South Bend-Mishawaka, IN-MI	08/28/2014
28020	Kalamazoo-Portage, MI	07/24/2014	45220	Tallahassee, FL	08/28/2014
29620	Lansing-East Lansing, MI	07/24/2014	46220	Tuscaloosa, AL	08/28/2014
24540	Greeley, CO	08/01/2014	47380	Waco, TX	08/28/2014
40060	Richmond, VA	08/06/2014	48620	Wichita, KS	08/28/2014
22744*	Fort Lauderdale-Pompano Beach-Deerfield Beach, FL	08/07/2014	19780	Des Moines-West Des Moines, IA	09/12/2014
48424*	West Palm Beach-Boca Raton-Delray Beach, FL	08/07/2014	11260	Anchorage, AK	09/18/2014
11700	Asheville, NC	08/21/2014	22380	Flagstaff, AZ	09/18/2014
15940	Canton-Massillon, OH	08/22/2014	14260	Boise City, ID	10/02/2014
14500	Boulder, CO	08/27/2014	38860	Portland-South Portland, ME	10/02/2014
22660	Fort Collins, CO	08/27/2014	49340	Worcester, MA-CT	10/06/2014
10420	Akron, OH	08/28/2014	15540	Burlington-South Burlington, VT	10/09/2014
12020	Athens-Clarke County, GA	08/28/2014	17860	Columbia, MO	10/09/2014
12220	Auburn-Opelika, AL	08/28/2014	24580	Green Bay, WI	10/16/2014
13980	Blacksburg-Christiansburg-Radford, VA	08/28/2014	31700	Manchester-Nashua, NH	10/17/2014
14020	Bloomington, IN	08/28/2014	16180	Carson City, NV	10/24/2014
16820	Charlottesville, VA	08/28/2014	29820	Las Vegas-Henderson-Paradise, NV	10/24/2014
17780	College Station-Bryan, TX	08/28/2014	39900	Reno, NV	10/24/2014
19380	Dayton, OH	08/28/2014	37964*	Philadelphia, PA	10/24/2014
22220	Fayetteville-Springdale-Rogers, AR-MO	08/28/2014	30780	Little Rock-North Little Rock-Conway, AR	11/06/2014
23540	Gainesville, FL	08/28/2014	40220	Roanoke, VA	11/06/2014
28940	Knoxville, TN	08/28/2014	16860	Chattanooga, TN-GA	11/13/2014
29200	Lafayette-West Lafayette, IN	08/28/2014	42140	Santa Fe, NM	11/19/2014

Notes: Wherever UberX entry is specified at the Core Based Statistical Area (CBSA) Division level, these cases are denoted by a star in column (1). Otherwise CBSA codes and titles refer to Core Based Statistical Areas.

Table D4: Uber Entry Dates 4 of 7

CBSA Area/Division Code	CBSA Area/Division Title	Date UberX Entered	CBSA Area/Division Code	CBSA Area/Division Title	Date UberX Entered
(1)	(2)	(3)	(1)	(2)	(3)
47300	Visalia-Porterville, CA	12/01/2014	40420	Rockford, IL	02/15/2015
15980	Cape Coral-Fort Myers, FL	12/04/2014	29740	Las Cruces, NM	02/18/2015
16300	Cedar Rapids, IA	12/04/2014	25940	Hilton Head Island-Bluffton-Beaufort, SC	03/27/2015
18880	Crestview-Fort Walton Beach-Destin, FL	12/04/2014	29540	Lancaster, PA	03/27/2015
19660	Deltona-Daytona Beach-Ormond Beach, FL	12/04/2014	39740	Reading, PA	03/27/2015
28580	Key West, FL	12/04/2014	42340	Savannah, GA	03/27/2015
29460	Lakeland-Winter Haven, FL	12/04/2014	49620	York-Hanover, PA	03/27/2015
34940	Naples-Immokalee-Marco Island, FL	12/04/2014	12260	Augusta-Richmond County, GA-SC	04/06/2015
35840	North Port-Sarasota-Bradenton, FL	12/04/2014	21500	Erie, PA	04/10/2015
36100	Ocala, FL	12/04/2014	35380	New Orleans-Metairie, LA	04/16/2015
37340	Palm Bay-Melbourne-Titusville, FL	12/04/2014	29940	Lawrence, KS	04/23/2015
37460	Panama City, FL	12/04/2014	31740	Manhattan, KS	04/23/2015
37860	Pensacola-Ferry Pass-Brent, FL	12/04/2014	45820	Topeka, KS	04/23/2015
38940	Port St. Lucie, FL	12/04/2014	44140	Springfield, MA	04/24/2015
27140	Jackson, MS	12/11/2014	41540	Salisbury, MD-DE	04/27/2015
27980	Kahului-Wailuku-Lahaina, HI	12/18/2014	23060	Fort Wayne, IN	05/07/2015
44100	Springfield, IL	01/09/2015	23844*	Gary, IN	05/07/2015
25420	Harrisburg-Carlisle, PA	01/29/2015	22020	Fargo, ND-MN	05/12/2015
10900	Allentown-Bethlehem-Easton, PA-NJ	01/30/2015	39540	Racine, WI	05/21/2015
29180	Lafayette, LA	01/30/2015	12700	Barnstable Town, MA	05/22/2015
42540	Scranton–Wilkes-Barre–Hazleton, PA	02/06/2015	28620	Kill Devil Hills, NC	05/22/2015
44300	State College, PA	02/06/2015	12300	Augusta-Waterville, ME	05/25/2015
44660	Stillwater, OK	02/12/2015	17660	Coeur d'Alene, ID	06/04/2015
14010	Bloomington, IL	02/15/2015	26820	Idaho Falls, ID	06/04/2015
16580	Champaign-Urbana, IL	02/15/2015	38540	Pocatello, ID	06/04/2015

Notes: Wherever UberX entry is specified at the Core Based Statistical Area (CBSA) Division level, these cases are denoted by a star in column (1). Otherwise CBSA codes and titles refer to Core Based Statistical Areas.

Table D5: Uber Entry Dates 5 of 7

CBSA Area/Division Code	CBSA Area/Division Title	Date UberX Entered	CBSA Area/Division Code	CBSA Area/Division Title	Date UberX Entered
(1)	(2)	(3)	(1)	(2)	(3)
46300	Twin Falls, ID	06/04/2015	37900	Peoria, IL	11/24/2015
33660	Mobile, AL	06/11/2015	22540	Fond du Lac, WI	11/25/2015
48864*	Wilmington, DE-MD-NJ	06/11/2015	27500	Janesville-Beloit, WI	11/25/2015
49740	Yuma, AZ	06/12/2015	36780	Oshkosh-Neenah, WI	11/25/2015
28660	Killeen-Temple, TX	07/02/2015	36260	Ogden-Clearfield, UT	12/18/2015
43900	Spartanburg, SC	07/16/2015	45340	Taos, NM	12/22/2015
19340	Davenport-Moline-Rock Island, IA-IL	07/21/2015	13820	Birmingham-Hoover, AL	12/28/2015
20100	Dover, DE	07/31/2015	24260	Grand Island, NE	02/01/2016
11180	Ames, IA	08/03/2015	13140	Beaumont-Port Arthur, TX	02/03/2016
10180	Abilene, TX	08/12/2015	33860	Montgomery, AL	02/04/2016
33260	Midland, TX	08/12/2015	17300	Clarksville, TN-KY	03/03/2016
36220	Odessa, TX	08/12/2015	26620	Huntsville, AL	03/04/2016
14540	Bowling Green, KY	08/27/2015	30340	Lewiston-Auburn, ME	03/21/2016
22900	Fort Smith, AR-OK	09/01/2015	12620	Bangor, ME	03/23/2016
39340	Provo-Orem, UT	09/03/2015	12740	Barre, VT	03/26/2016
11540	Appleton, WI	09/10/2015	26980	Iowa City, IA	04/28/2016
41180	St. Louis, MO-IL	09/18/2015	31420	Macon-Bibb County, GA	05/10/2016
14740	Bremerton-Silverdale, WA	09/30/2015	47580	Warner Robins, GA	05/10/2016
24300	Grand Junction, CO	10/06/2015	41460	Salina, KS	05/21/2016
17020	Chico, CA	10/08/2015	18180	Concord, NH	06/01/2016
19060	Cumberland, MD-WV	10/20/2015	49660	Youngstown-Warren-Boardman, OH-PA	06/23/2016
25180	Hagerstown-Martinsburg, MD-WV	10/20/2015	25060	Gulfport-Biloxi-Pascagoula, MS	07/01/2016
25500	Harrisonburg, VA	10/23/2015	25620	Hattiesburg, MS	07/01/2016
15260	Brunswick, GA	11/06/2015	36500	Olympia-Tumwater, WA	07/15/2016
13380	Bellingham, WA	11/11/2015	16620	Charleston, WV	07/19/2016

Notes: Wherever UberX entry is specified at the Core Based Statistical Area (CBSA) Division level, these cases are denoted by a star in column (1). Otherwise CBSA codes and titles refer to Core Based Statistical Areas.

Table D6: Uber Entry Dates 6 of 7

CBSA Area/Division Code	CBSA Area/Division Title	Date UberX Entered	CBSA Area/Division Code	CBSA Area/Division Title	Date UberX Entered
(1)	(2)	(3)	(1)	(2)	(3)
26580	Huntington-Ashland, WV-KY-OH	07/19/2016	16060	Carbondale-Marion, IL	03/02/2017
28060	Kalispell, MT	08/01/2016	20740	Eau Claire, WI	03/02/2017
33540	Missoula, MT	08/01/2016	24220	Grand Forks, ND-MN	03/02/2017
24500	Great Falls, MT	08/02/2016	29100	La Crosse-Onalaska, WI-MN	03/02/2017
15580	Butte-Silver Bow, MT	08/03/2016	31860	Mankato-North Mankato, MN	03/02/2017
25740	Helena, MT	08/03/2016	40340	Rochester, MN	03/02/2017
14580	Bozeman, MT	08/04/2016	40980	Saginaw, MI	03/02/2017
13740	Billings, MT	08/05/2016	41060	St. Cloud, MN	03/02/2017
27740	Johnson City, TN	08/19/2016	48140	Wausau, WI	03/02/2017
28700	Kingsport-Bristol-Bristol, TN-VA	08/19/2016	16220	Casper, WY	03/03/2017
48300	Wenatchee, WA	08/19/2016	16940	Cheyenne, WY	03/03/2017
17980	Columbus, GA-AL	09/20/2016	28180	Kapaa, HI	03/10/2017
46340	Tyler, TX	09/22/2016	17200	Claremont-Lebanon, NH-VT	03/22/2017
44180	Springfield, MO	11/17/2016	30020	Lawton, OK	03/22/2017
25860	Hickory-Lenoir-Morganton, NC	12/15/2016	20220	Dubuque, IA	04/01/2017
28420	Kennewick-Richland, WA	12/15/2016	43580	Sioux City, IA-NE-SD	04/01/2017
49420	Yakima, WA	12/16/2016	47940	Waterloo-Cedar Falls, IA	04/01/2017
14380	Boone, NC	01/13/2017	20260	Duluth, MN-WI	05/01/2017
40860	Rutland, VT	01/20/2017	10500	Albany, GA	05/17/2017
21780	Evansville, IN-KY	01/25/2017	46660	Valdosta, GA	05/17/2017
30860	Logan, UT-ID	02/01/2017	27940	Juneau, AK	06/19/2017
45460	Terre Haute, IN	02/28/2017	21820	Fairbanks, AK	06/21/2017
25900	Hilo, HI	03/01/2017	32580	McAllen-Edinburg-Mission, TX	06/27/2017
27100	Jackson, MI	03/01/2017	10580	Albany-Schenectady-Troy, NY	06/29/2017
13900	Bismarck, ND	03/02/2017	13780	Binghamton, NY	06/29/2017

Notes: Wherever UberX entry is specified at the Core Based Statistical Area (CBSA) Division level, these cases are denoted by a star in column (1). Otherwise CBSA codes and titles refer to Core Based Statistical Areas.

Table D7: Uber Entry Dates 7 of 7

CBSA Area/Division Code (1)	CBSA Area/Division Title (2)	Date UberX Entered (3)	CBSA Area/Division Code (1)	CBSA Area/Division Title (2)	Date UberX Entered (3)
15380	Buffalo-Cheektowaga-Niagara Falls, NY	06/29/2017	43340	Shreveport-Bossier City, LA	02/15/2018
21300	Elmira, NY	06/29/2017	10100	Aberdeen, SD	06/20/2019
24020	Glens Falls, NY	06/29/2017	39660	Rapid City, SD	06/20/2019
27060	Ithaca, NY	06/29/2017	43620	Sioux Falls, SD	06/20/2019
40380	Rochester, NY	06/29/2017			
45060	Syracuse, NY	06/29/2017			
46540	Utica-Rome, NY	06/29/2017			
29700	Laredo, TX	07/12/2017			
20580	Eagle Pass, TX	07/13/2017			
27860	Jonesboro, AR	08/01/2017			
41660	San Angelo, TX	08/04/2017			
18060	Columbus, MS	08/18/2017			
32940	Meridian, MS	08/18/2017			
44260	Starkville, MS	08/18/2017			
48660	Wichita Falls, TX	08/22/2017			
18700	Corvallis, OR	09/20/2017			
33740	Monroe, LA	09/20/2017			
35740	Norfolk, NE	09/23/2017			
45500	Texarkana, TX-AR	09/26/2017			
34860	Nacogdoches, TX	09/27/2017			
39420	Pullman, WA	09/29/2017			
45900	Traverse City, MI	10/17/2017			
32780	Medford, OR	12/01/2017			
22500	Florence, SC	12/08/2017			
13220	Beckley, WV	12/24/2017			

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