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Does ridesharing affect external-cause mortality? Evidence from U.S. Vital Statistics Data☆

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HIGHLIGHTS

- Ridesharing entry increases external-cause deaths (ages 18–45) by about 10%.
- Increased deaths are driven primarily by alcohol and drug intoxication.
- Effects are concentrated on weekends and in areas with weaker transit access.
- Uses a DiD imputation estimator robust to heterogeneous treatment timing.

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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 issue, we use restricted-access data from the National Vital Statistics System to study how ridesharing 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 of our findings by presenting event studies, placebo analyses, sensitivity and heterogeneity analyses, and a variety of robustness checks, including difference-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.

1. Introduction

Ridesharing services such as Uber and Lyft 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 taxi markets (Chang, 2017; Berger et al., 2018; Brodeur and Nield, 2018), 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., 2023; Anderson and Davis, 2026), and, because it offers safe transportation to and from social gatherings, alcohol consumption (Zhou, 2020; Teltser et al., 2021). Moreover, 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

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of evidence on its impacts on transportation patterns (particularly intoxicated driving and access to hospitals), substance use, and exposure to violent crime, ridesharing may have meaningful effects on deaths driven by these underlying factors.

In this paper, we estimate the reduced-form effect of the presence of ridesharing on deaths from external causes including deaths involving violent crime (i.e., homicide), suicide, motor vehicle accidents, alcohol, and drug overdoses among individuals in the U.S. aged 18 to 45. We focus on ages 18 to 45 because those over 45 are less likely to have a smartphone and to use ridesharing services during our sample period (2008–2019).[1] 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 measure deaths across these categories, we use restricted access mortality data from the Centers for Disease Control and Prevention's National Vital Statistics System (NVSS).

To identify the effect of ridesharing on mortality, we use spatial and temporal variation in UberX (Uber's mainstream taxi-like service) entry dates across U.S. Core Based Statistical Areas (CBSAs) from late 2012 through 2019. We employ a difference-in-differences imputation approach (Borusyak et al., 2024) that accounts for any heterogeneous treatment timing bias (Goodman-Bacon, 2021; Baker et al., 2022). Throughout, our treatment variable is a binary indicator of ridesharing availability because we are particularly interested in (and well-situated to estimate) the reduced-form effect of an exogenous shock to transportation availability. In contrast, measuring usage intensity across place and time requires proprietary ridership data and reflects endogenous equilibrium outcomes jointly determined by supply, demand, and prices.[3] Our main estimates 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. Central counties comprise more than 90% of the average CBSA's population.

Our findings suggest that the entry of ridesharing is associated with 2.01 additional external-cause deaths per 100,000 individuals aged 18 to 45 per quarter. Primarily, the increase in mortality is driven by 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 the time 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. We also find 0.48 additional suicide deaths per 100,000 population (roughly 10%). The increase in deaths from suicide aligns with expectations, given the documented relationship between drug and alcohol use and suicide attempts (e.g., Conner et al., 2014; Esang and Ahmed, 2018). While we find a 9% decrease in homicides and a 4% decrease in motor vehicle accident fatalities, neither estimate is robust nor statistically significant at conventional levels.

We support a causal interpretation of our findings by presenting a series of event studies, sensitivity and heterogeneity analyses, and a variety of robustness checks. As part of those robustness checks, we show that our results are similar when using the Callaway and Sant'Anna (2021) and Wooldridge (2021) estimators.[4] Importantly, our event study analyses show no evidence of differential trends between treated and not-yet-treated areas in the periods leading up to UberX entry. The event studies also show growth in treatment effects over time after UberX entry. Such a pattern is consistent with existing work that documents increasing ridesharing availability and usage following initial entry. For example, Hall and Krueger (2018) show that there are relatively few driver-partners in the first 6 to 18 months followed by rapid growth. The pattern also lines up well with public interest in UberX over time as measured by Google Trends data (see Fig. 1 of Hall et al., 2018).

Further supporting a causal claim, we find that 79% of the increase in mortality occurs on weekends. According to Uber, "[d]emand for rides is always at its peak on the weekends—Friday and Saturday nights, 5 p.m.–5 a.m."[5] Our analyses also reveal that effects are strongest in areas with weaker existing public transit options, suggesting that expanded transportation availability induced by ridesharing is the key driver of our findings. Finally, we do not find comparable effects when we examine "internal" causes of death such as stroke, heart disease, or cancer, suggesting that our main findings are not driven by broader changes in health.

Examining group-specific mortality, we find the overall effects are largest among those aged 31 to 45, though substance-use deaths increase by similar percentages across the 18–30 and 31–45 age groups. Heterogeneity by race and gender is more nuanced. Consistent with Teltser et al. (2021), who find null results on ridesharing's effect on alcohol consumption among Black respondents, the increase in alcohol-related mortality is concentrated among white individuals. However, the increase in overall external-cause mortality is, if anything, larger among women and racial and ethnic minorities, driven by homicide, suicide, and drug-related deaths. This suggests that increased alcohol consumption documented in earlier work captures only part of the broader impacts of ridesharing on health outcomes.

Our work makes several contributions to the existing literature, significantly extending our understanding of the social benefits and costs of ridesharing. To date, many researchers have focused on Uber's positive impacts on consumer surplus (Cohen et al., 2016; Christensen et al., 2025), business formation (Barrios et al., 2020), public transport (Hall et al., 2018), impaired driving (e.g., Dills and Mulholland, 2018; Anderson and Davis, 2026), labor market flexibility (Chen et al., 2019; Angrist et al., 2021; Chen et al., 2026), transportation for under-served groups (Gentry, 2026), and transportation to hospitals (Moskatel and Slusky, 2019). We provide new relatively comprehensive evidence that these benefits are accompanied by unintended consequences that entail significant public health costs. While previous research has documented that ridesharing entry leads to an increase in alcohol consumption (e.g., Teltser et al., 2021; Zhou, 2020), our paper goes beyond behavioral changes to document effects on mortality. This is important because behavioral changes (e.g., increased socialization, substance use) do not always result in changes in health or mortality. Moreover, by leveraging restricted-access administrative mortality data, we are able to examine deaths across a wide range of (multiple) causes of death, and do so with more geographic, temporal, and demographic granularity than is often possible with public-use datasets.

[1] According to a 2018 Gallup Poll (close to the end of our sample period), young adults are much more likely to use ride-sharing services; 45% of adults aged 18 to 29 said they use ride-sharing services, whereas this dropped to 36% among those 30 to 49 years old, 23% among those aged 50 to 64 and 13% among those aged 65 or older. See https://news.gallup.com/poll/237965/snapshot-uses-ride-sharing-services.aspx.

[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.

[3] If ridesharing usage post-entry is near-zero, delayed, or gradually increasing, our estimated effects will tend to be conservative relative to the full change in mortality caused by going from zero ridesharing to the steady-state level of ridesharing availability/usage.

[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.

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

Second, our findings help to explain the mixed evidence on ridesharing's overall effects on socially important outcomes such as crime and traffic fatalities (e.g., Dills and Mulholland, 2018; Burtch et al., 2018; Barrios et al., 2023; Anderson and Davis, 2026). By documenting that the mortality increase is driven primarily by alcohol-related poisonings and drug overdoses rather than motor vehicle accidents, we show that any "safe ride home" effect is at least partly undermined by an increase in risky substance use facilitated by expanded transportation access. While the overall effect is driven by substance-use-related deaths, we also contribute to the literature examining the causal determinants of violent crime and deaths of despair more broadly (Cook and Ludwig, 2006; Stevenson and Wolfers, 2006; Classen and Dunn, 2012; Evans and Moore, 2012; Edwards et al., 2018). In particular, we show how changes in transportation may impact homicide and suicide rates, either directly by allowing people to remove themselves from situations where they may become victims of a crime, or through increased use of substances that may induce violent behavior or diminish mental health.

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. We demonstrate that a supply-side transportation shock can act as a catalyst for greater substance-use-related deaths, a finding of critical relevance to those concerned with the net social impact of ridesharing.

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 evidence to alert policymakers of an important unintended consequence. Given the massive costs of young-adult mortality, our work carries clear policy relevance. 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 deaths. Instead, we rely on observable characteristics in the vital statistics data along with the existing literature to provide insight into the potential causal pathways.

In Section 2, we provide further background and motivation for our research question. In Section 3, we summarize our mortality and UberX entry 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.

2. Background and motivation

Ridesharing has become a common mode of transportation in the United States since the launch of UberX in New York City in late 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 UberX 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. Notably, across the same time period, deaths relating to alcohol consumption have also increased. 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] 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.

Multiple studies support Uber's road safety assertions (e.g., Greenwood and Wattal, 2017; Martin-Buck, 2017; Peck, 2017; Dills and Mulholland, 2018; Anderson and Davis, 2026).[9] On the other hand, some studies find a null or slightly positive effect on the number of traffic fatalities (e.g., Barrios et al., 2023). One potential explanation for the somewhat mixed findings is that safe transportation options may affect drinking behavior among both riders and non-riders, such that the average trip may be safer but the aggregate impact on traffic fatalities is not unambiguously negative. 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 aspect of their model is that utility is increasing in the number of other people who are also socializing (i.e., a sort of network externality). Jackson and Owens provide support for their theoretical predictions by studying District of Columbia Metro expansions from 1999 to 2003, where they find arrests for alcohol-related crimes increased by at least 5.4% in neighborhoods with one or more bars within 100 meters of Metro stations that expanded late night service. Their findings suggest that, to the extent that safe transportation and alcohol are complementary, demand for alcohol will increase when ridesharing becomes available. Complementing Jackson and Owens' work, Teltser et al. (2021) find a 5.4% increase in total drinks per month following UberX entry. The effects are driven by areas with weaker existing transit connectivity. They also show that much of the additional consumption occurs away from home by examining data on earnings and employment at bars and restaurants, which are generally proportional to the amount of consumption at those establishments.

The "night out" framework suggests that ridesharing's net effect on mortality is unlikely to be captured by any single cause of death, and instead depends on how expanded transportation access interacts with each category we examine – i.e., the leading causes of death among young adults (Carpenter and Dobkin, 2009). That is, ridesharing may reduce homicide by allowing people to remove themselves from situations of heightened risk, but it may also increase the pool of potential victims and perpetrators by encouraging more people to be out late at night. Ridesharing alters transportation patterns in ways that could reduce traffic fatalities, consistent with a substitution away from drunk driving, but could also leave overall accident exposure unchanged if it entails more trips. Services like Uber and Lyft may induce people to engage in more social activities, which could improve mental health and reduce suicide, but at the same time those activities frequently involve substance use which can diminish mental health and increase suicide risk. Finally, expanded access to safe late-night transportation may itself directly increase instances of alcohol poisoning, drug overdoses, and accidents related to impaired decision-making and reduced physical coordination (e.g., falls, burns, and drownings). Given the theoretical ambiguity in the net effects of ridesharing on the various external causes

[6] See https://newsroom.uber.com/wp-content/uploads/2015/01/Uber MADD-Report.pdf.

[7] See https://injuryfacts.nsc.org/motor-vehicle/historical-fatality-trends/deaths-and-rates/.

[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] A separate literature examines the mechanisms underlying individual trip quality and safety on ridesharing platforms more generally (e.g., Athey et al., 2026).

of death, we aim to shed light on these relationships with our empirical analysis. In the next section, we explain our data and 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, localities submit standardized death certificates to their states' vital statistics offices, which then forward them to the NCHS. The NCHS edits and assembles these records for national publication using the Mortality Medical Data System.[10] The NVSS data therefore include rich information on the universe of reported deaths of residents in the United States, including causes of death classified using International Classification of Diseases tenth-edition (ICD-10) codes and the county where the death occurred. Studying 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.[11]

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 to be extremely high (Harris et al., 2021). Within the NVSS mortality data, we focus on multiple cause of death, county-level death records for the years 2008 to 2019. Further, our main estimates focus on deaths among those aged 18 to 45 because younger adults adopted smartphones earlier than older age groups and are more frequent users of ridesharing services. Deaths among this group in the NVSS data, particularly unexpected or sudden deaths, are likely to have an accurate cause of death recorded. This is because the National Association of Medical Examiners' Forensic Autopsy Performance Standards indicate that autopsies will be performed in cases where the death is caused by criminal violence, is associated with police action, or is due to apparent intoxication with alcohol, drugs, or poison.[12] Following the approach of Carpenter and Dobkin (2009), when there is more than one listed cause of death, we assign the death in the following priority ordering: homicide, suicide, motor vehicle accident, alcohol, drugs.

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 definitively 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 more information on the NVSS data and a complete breakdown of the ICD-10 codes associated with each category of death in the appendix.

We obtain data on UberX entry dates from Teltser et al. (2021), who expand on the 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. We observe UberX entry dates for 329 CBSAs in our NVSS data. As we mention in the introduction, our estimates focus on mortality in designated "central" counties within CBSAs (these tend to be the more densely populated counties and the ones geographically closer to the core of the CBSA or division thereof). We focus on central counties as delineated by the Census Bureau, rather than choosing our own population or density threshold, because that delineation is an independently determined measure of urbanization and commuting integration that preserves the geographic contiguity of counties within each CBSA.

We present a map of all counties that ever experienced UberX entry in Fig. 1 and the share of counties in our sample that have UberX available in Fig. 2. Further, we provide maps showing entry by county for each year as an appendix item (see Fig. D1). We also provide a complete updated list of entry dates by CBSA/division as an appendix item (Tables D1–D7). Our data include 937 total CBSAs and divisions, of which 329 experienced UberX entry during 2008-19. Our preferred sample, comprising only central counties in CBSAs that UberX ever entered between 2008 and 2019, therefore consists of 639 unique counties in 329 ever-treated Core-Based Statistical Areas (CBSAs) and divisions, totaling 30,672 county-quarter observations.[13] 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. This reflects that UberX rollout announcements, like those compiled by Hall et al. (2018) and Teltser et al. (2021), are made at the city/CBSA level rather than for individual counties or commuting zones. We similarly exclude the small number of later, broader rollout announcements (e.g., the whole state of North Dakota in 2019), as these expansions need not (and seem unlikely to) entail any meaningful change in ride availability.

Two further comments are noteworthy when discussing UberX entry data. First, we do not control for the presence of Uber's main competition, Lyft, in any of our analyses. As a practical matter, it is more challenging to determine accurate Lyft entry dates. The main reason for ignoring Lyft entry timing, however, is that we are interested in the effect of ridesharing, rather than any particular service, on mortality. Accounting for UberX entry accomplishes our goal because Lyft's entrance into cities generally trailed Uber's entry. Moreover, Lyft's market share is less than that of Uber; in 2019, the final year of our sample period, Uber's market share was roughly 70% while Lyft controlled 30% of the market.[14] Notably, omitting Lyft entry would lead to attenuation bias in the places where Lyft entered before UberX, which would make finding significant effects less likely. Second, of the 329 CBSAs that UberX enters between 2008 and 2019, it exits 22 CBSAs, or 6.6%, at some point (often due to regulatory barriers). Of the 22 CBSAs that UberX ever exits, UberX re-enters all of them (and does so within one

[10] 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.

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

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

[13] As alluded to above, some larger more densely-populated Core-Based Statistical Areas are split into subdivisions of counties, and some of these divisions have distinct UberX entry dates. Note also that, in our main specifications, we restrict to ever-treated CBSAs. In the Borusyak et al. (2024), Callaway and Sant'Anna (2021), and Wooldridge (2021) estimators, this entails dropping the time periods of and following the final observed treatment date, which occurred in the second quarter of 2019. Thus, when using these estimators, the sample includes 2008 Q1 through 2019 Q1 (i.e., 45 year-quarters).

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

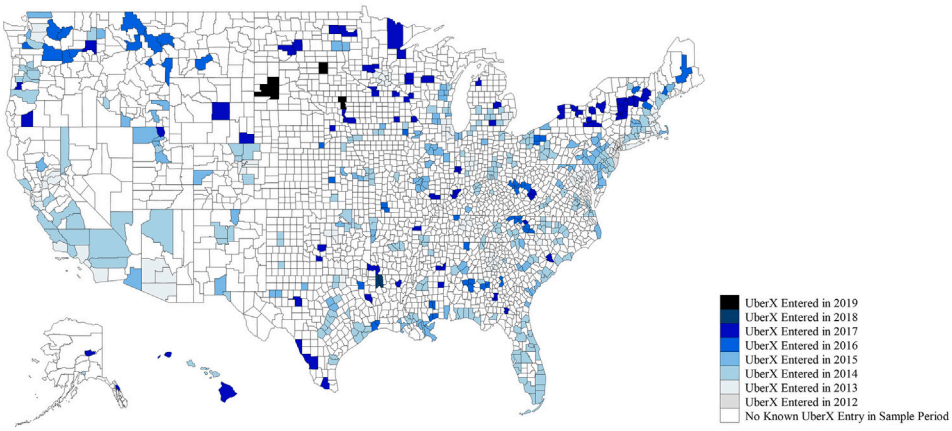


Fig. 1. Entry of UberX over Time.

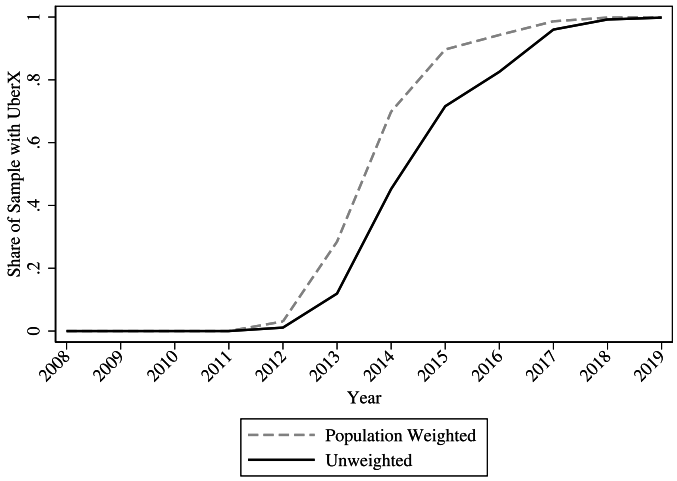


Fig. 2. Share of counties in our sample with UberX over Time.

Table 1
Summary Statistics by CBSA-Quarter.

		Pre-UberX Entry		Post-UberX Entry	
		Mean	SD	Mean	SD
Mortality 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 (100,000 s)	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	13.31	6.33	11.68	5.87
	% Unemployed	7.25	2.51	4.40	1.58
	% Bachelors or higher	25.02	9.11	32.57	10.82
	% White	82.81	14.48	79.9	14.99
	% Men	49.70	1.64	49.66	1.49
Number of CBSAs		329	329	329	329
Number of Counties		639	639	639	639

Note: Ever treated, central counties, National Vital Statistics System (NVSS) and Surveillance, Epidemiology, and End Results (SEER) data. Mortality rates and population by quarter are for ages 18–45. Mortality rates are denominated by the county population aged 18–45. Income, poverty rate, unemployment, and bachelor's or higher, are all at the county level and not specific to certain ages or demographics.

year in 12 of the 22). Because UberX exiting an area would attenuate any treatment effect, we make the conservative decision to consider an area treated by UberX from the date it first enters the area, and to ignore any subsequent exit and re-entry events.

Our main outcomes of interest are expressed as quarterly deaths per 100,000 persons in an area for various subgroups. For county-level population estimates, we use county-level single-year age-group data from the Surveillance, Epidemiology, and End Results Program (SEER).[15] These data allow researchers to pull yearly total population estimates, as well as population by demographic group, such as gender, age, and race at the county level. Our other economic and demographic control variables come from the SEER time-varying county characteristics file. The information 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, suicide, motor vehicle accidents, alcohol, drugs. With this ordering, there is no "double counting" of deaths, in the sense that if someone dies by homicide while also intoxicated by alcohol, the death is counted only as a homicide in our data.[16]

3.2. Estimation

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

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

In Eq. (1), y_{jt} refers to quarterly deaths per 100,000 persons in geographic area j in time period t for the selected cause of death, y. We

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

[16] 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.

Table 2
Estimates of UberX's impact on mortality.

	Outcome: Deaths per 100k Population in the Noted Category						
	Age 18–45 (1)	All Counties (2)	CSDiD (3)	TWFE (4)	Wooldridge (5)	Age 18–45 (6)	Age 18–45 (7)
External Cause	2.008***	2.988***	2.061***	1.171***	2.242***	2.012***	1.949***
	(0.392)	(1.058)	(0.649)	(0.279)	(0.410)	(0.378)	(0.419)
	[20.61]	[20.99]	[20.61]	[20.61]	[20.61]	[20.50]	[20.50]
Homicide	−0.187	0.007	−0.168	−0.038	−0.294*	−0.092	−0.082
	(0.141)	(0.148)	(0.162)	(0.060)	(0.155)	(0.144)	(0.141)
	[2.06]	[1.77]	[2.06]	[2.06]	[2.06]	[2.04]	[2.04]
Suicide	0.481**	0.330	0.759***	0.290***	0.511***	0.504**	0.509**
	(0.196)	(0.473)	(0.276)	(0.089)	(0.150)	(0.199)	(0.205)
	[4.26]	[4.51]	[4.26]	[4.26]	[4.26]	[4.23]	[4.23]
MVA	−0.133	0.006	−0.211	0.204**	−0.244	−0.311	−0.193
	(0.259)	(0.784)	(0.285)	(0.102)	(0.330)	(0.264)	(0.245)
	[3.33]	[4.61]	[3.33]	[3.33]	[3.33]	[3.38]	[3.38]
Alcohol	0.422***	0.396***	0.540***	0.044	0.400**	0.462***	0.454***
	(0.113)	(0.100)	(0.154)	(0.051)	(0.201)	(0.114)	(0.111)
	[1.10]	[0.95]	[1.10]	[1.10]	[1.10]	[1.07]	[1.07]
Drugs	1.724***	2.239***	1.331***	0.519***	1.776***	1.939***	1.811***
	(0.458)	(0.446)	(0.476)	(0.180)	(0.582)	(0.465)	(0.448)
	[6.15]	[5.45]	[6.15]	[6.15]	[6.15]	[6.01]	[6.01]
CBSAs	329	329	329	329	329	325	325
Counties	639	1057	639	639	639	616	616
Observations	28,755	47,565	28,755	30,672	28,755	27,720	27,720
Population & Unemployment						Yes	Yes
Income (per capita)						Yes	Yes
Demographic Controls							Yes

Estimates are based on data from the National Vital Statistics System (NVSS) with the sample restricted to central counties that UberX ever entered, except where indicated. Each estimate in this table is produced by a separate regression using the Borusyak et al. (2024) difference-in-differences imputation estimator unless indicated otherwise. Coefficients should be interpreted as the effect of UberX entry on the mortality rate per 100,000 population. Death counts are denominated by the county population belonging to the applicable demographic group analyzed in that specification. Standard errors are robust to clustering at the CBSA/division level. Numbers in square brackets are the respective category means. Demographic controls include average population age, poverty rate, percent with a bachelor's degree, fraction white, and fraction male. *** p<0.01, ** p<0.05, * p<0.1.

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, θ_j, time period fixed effects, ϕ_t, and an idiosyncratic error term, ε_{jt}. We cluster standard errors at the CBSA level. In the eleven CBSAs that consist of subdivisions of counties, we treat the divisions as unique clusters.[17] In some specifications we include county-level demographics, which comprise 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:

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

The key difference from Eq. (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 the 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 ridesharing 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 and estimating the impact on mortality over a relevant time horizon.

A potential complication with using Eqs. (1) and (2) to estimate effects 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 because it may take time for consumers to begin using UberX in an area. Consequently, in our preferred event study designs and regression estimates, we use a difference-in-differences imputation estimator (Borusyak et al., 2024). 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 bias when estimating impacts 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 CBSA 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,

[17] These CBSAs are Boston, Chicago, Dallas, Detroit, Los Angeles, Miami, New York City, Philadelphia, San Francisco, Seattle, and Washington, D.C.

and drugs.[18] With this approach, there is no "double counting" of deaths.[19] Note also that each coefficient throughout our tables of estimates is produced by a separate regression using the Borusyak et al. (2024) difference-in-differences imputation estimator unless otherwise indicated.

The estimates in column (1) of Table 2 show that the advent of ridesharing in a CBSA 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), the effect amounts to a 9.74% 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 the specification. Using our estimates, a back of the envelope calculation would suggest that the advent of ridesharing is associated with 6473 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 8631.

When we examine the specific causes of these deaths we see that most of the overall effect consists of 0.42 more alcohol-related deaths and 1.72 additional drug-related deaths per 100,000 population per quarter. 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. Our estimate is 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.

While our 38% estimate may appear large at first, it is worth pointing out that alcohol deaths are quite rare while excessive drinking is quite common, such that even a small percentage increase in excessive drinking can result in a large percentage increase in deaths. To provide additional context, Teltser et al. (2021) report an estimated 5% increase in self-reported binge drinking instances among respondents aged 21 to 64 following UberX entry, and 9.5% among those aged 21 to 34.[20] Using their conservative 5% estimate relative to their reported baseline individual-level mean of 0.78 binge drinking instances in the past month, along with our sample's average baseline county population aged 18 to 45 (roughly 126,000), we calculate 14,742 additional binge drinking instances per county-quarter. Scaling our ridesharing-related alcohol death estimate of 0.422 by 1.26 (to transform deaths per 100,000 into deaths per 126,000), we calculate 0.532 additional alcohol deaths per county-quarter. Taken together, these calculations imply 1 additional alcohol death for every 27,710 additional instances of binge drinking.[21]

Similarly, the 1.72 per 100,000 increase in drug-related deaths per quarter represents a 28% increase compared to the category mean for those aged 18 to 45. This estimate is in line with recent work on how changes in transportation access can affect drug-related deaths. For example, Huh and Reif (2021), using a regression discontinuity design, find a 29% increase in drug-related deaths when young people gain a driver's license. Our estimate of 1.72 drug-related deaths per 100,000 individuals aged 18 to 45 per quarter implies that ridesharing is associated with roughly 5500 additional drug deaths per year.[22] While substantial, that would represent only 6% of all overdose deaths in the U.S. in 2020 and would explain only about 10% of the change in annual overdose deaths between 2010 and 2020.[23]

Our estimates also show that there is a statistically significant increase of 0.48 in suicide deaths per 100,000 population. This is perhaps unsurprising considering that drug and alcohol use is 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) report that alcohol intoxication is involved in approximately 22% of deaths by suicide and that illicit drugs are present in almost 40% of suicide deaths.[24] In contrast, while the point estimates imply a 9% decline in homicide deaths and a 4% decline in MVA (motor vehicle accident) deaths, the effects are not statistically significantly different from zero. As discussed in Section 2, these modest and relatively imprecisely estimated effects are consistent with the theoretical ambiguity of ridesharing's impact on homicide and MVA risk: ridesharing can simultaneously reduce risk in both categories (e.g., by removing people from dangerous late-night waits for transportation, or by substituting for drunk driving) and increase it (e.g., by drawing more people into risky late-night environments, or by generating more total trips). This MVA estimate is consistent with the somewhat mixed traffic-safety literature, where Anderson and Davis (2026) find a 5.2% decline in traffic fatalities using proprietary Uber ridership data, Martin-Buck (2017) finds a 10% reduction in fatal alcohol-related traffic incidents, Greenwood and Wattal (2017) report a 3.6% decline in motor vehicle fatalities, Dills and Mulholland (2018) estimate a 1.6% quarterly reduction in fatal traffic incidents, and Barreto et al. (2021) document a 10–17% reduction across Brazilian cities, while Barrios et al. (2023) find a 3% *increase* in the number of fatal traffic accidents across U.S. cities.

These findings contribute to a large literature showing that the consumption of alcohol and illicit substances can have harmful social effects, particularly among younger adults. For example, 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. Further examples include studies that find greater alcohol consumption is related to 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).

Naturally, our main estimates are the product of several sample selection, specification, and estimation choices. One of those choices is to focus on deaths in central counties. The estimates in column (2) expand our sample to include outlying counties within a CBSA. While the

[18] As a reminder, Appendix Table B1 provides the ICD codes for deaths in each category.

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

[20] The CDC defines binge drinking instances as those where an individual consumed X or more drinks on one occasion, where X is four for women and five for men (see https://www.cdc.gov/drink-less-be-your-best/facts-about-excessive-drinking/index.html).

[21] Note that, while this calculation appears reasonable, it is far from perfect. First, it involves comparing baseline means, counts, and estimates drawn from administrative and survey data, and from different age groups and geographies. Second, it may not fully reflect the "first stage" of additional alcohol consumption, especially at the margin of death due to alcohol poisoning (i.e., having a blood alcohol content over 0.3% per 100 ml; see https://my.clevelandclinic.org/health/diagnostics/22689-blood-alcohol-content-bac). For example, even if the number of binge drinking instances did not change, ridesharing could induce each drinking occasion to involve a larger number of drinks on average, putting existing binge drinkers closer to the margin of death.

[22] The back of the envelope calculation here is based on 639 counties with 6.88 deaths per 100,000 population per year (i.e., 1.72 per quarter), scaled for average county population across pre-UberX periods. If we use average population across the post-UberX periods, the number of drug related deaths is 7386 (168,000 average county population aged 18 to 45, 6.88 deaths per 100,000 population per year, and 639 counties).

[23] 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.

[24] 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%.

number of counties grows substantially, the population in outlying counties amounts to only 10% of the average CBSA population. Therefore, adding these counties does not substantially change the pattern relative to our main estimates. Given the small proportion of deaths that occur in non-central counties and the fact that we cannot be sure of UberX availability in those areas, we focus on deaths in central counties in later heterogeneity, sensitivity, robustness, and falsification exercises.

In columns (3), (4), and (5) of Table 2, we present estimates using the Callaway and Sant'Anna (2021) estimator, the traditional two-way fixed effects (TWFE) estimator, and the Wooldridge estimator (Wooldridge, 2021). For the Callaway and Sant'Anna and Wooldridge estimators, we see similar patterns relative to our main estimates with some small deviations. Notably, the effect on deaths in motor vehicle accidents (MVA) using those estimators is larger (roughly a 6 to 7% decline) but the effect remains not statistically different from zero. Further, the decline in homicide deaths increases to 14.3% and becomes statistically significant at the 10% level when using the Wooldridge estimator. We observe significant attenuation, however, in the TWFE estimates. In 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. Subsequent estimates in the paper use the Borusyak et al. imputation-based estimator, except when indicated.

In columns (6) and (7) 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.[25] Those estimates are very similar, easing concerns that time-varying factors (such as simultaneous economic growth) could confound our estimates in a meaningful way. Given the similarity between those estimates and the potential for time-varying covariates to bias treatment effect estimates (Caetano et al., 2022), all further estimates in the paper use the specification without those covariates as our main specification.

4.1. Event studies

Because we use a staggered timing design, 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 Fig. 3 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 entry.[26] 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 Fig. 3, we find no evidence of pre-trends that would undermine the parallel trends assumption and thus bias our results.

The event studies also reveal that the effects of UberX on our outcomes of interest increase over time. This pattern is consistent with how the number of driver-partners grows over time within a city (Hall and Krueger, 2018). 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. As we discussed earlier, the pattern also lines up well with interest in Uber over time as measured by Google Trends data (Hall et al., 2018, see their Fig. 1).

Note that event studies using the new difference-in-differences estimators must be interpreted with caution (Roth, 2024). For example, using the Borusyak et al. (2024) 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 in Appendix C; there we see no discontinuous jump in suicides around the treatment date.

In addition to these event studies, in Appendix A.1 we test whether UberX's entry timing can be predicted by pre-2012 trends in our mortality outcomes or area-level economic and demographic characteristics. The presence of any such predictive relationships could undermine a causal interpretation of our findings. In Panel A of Appendix Table A1, we regress the year-quarter of UberX entry on pre-2012 yearly changes in each of our six mortality outcomes (external-cause, homicide, suicide, MVA, alcohol, and drug mortality rates). We find no statistically significant relationship for any of them. In Panel B, we conduct a similar exercise using pre-2012 changes in real per-capita income, the unemployment rate, and the population aged 18 to 45. While two of the three relationships are marginally statistically significant, both imply economically negligible effects on entry timing: a $1000 increase in real per-capita income growth is associated with UberX entering roughly 12 days later, and a 1 percentage point increase in unemployment growth is associated with entry occurring roughly 15 days earlier. Reassuringly, our main estimates in Table 2 are also robust to directly controlling for these area characteristics. Taken together, these results indicate that UberX's rollout was not meaningfully predicted by prior mortality trends or the broader economic environment, further supporting a causal interpretation of our findings.

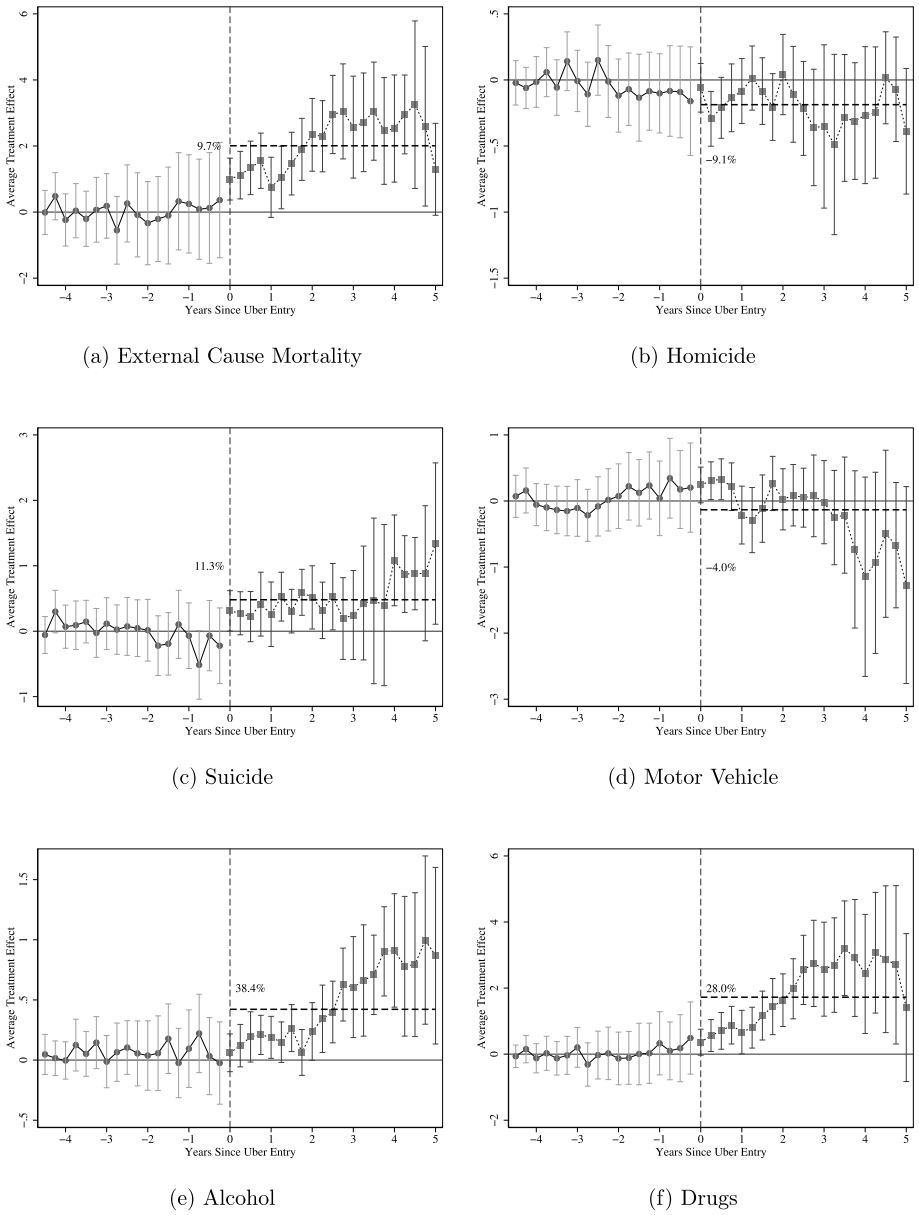
4.2. Mechanisms

In Table 3, we consider the extent to which ridesharing is the most likely explanation for our findings. In columns (1) and (2), we separately examine deaths on the weekend versus on weekdays. If ridesharing 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.[27] We define the weekend to be from Friday through Monday. We include Monday to ensure we count Sunday evening/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 that 79% of the total increase occurs on the weekend (i.e., the weekend estimate of 0.335, divided by the corresponding estimate of 0.422 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%). 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 weekdays (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. Information on the timing of death would be useful to confirm this pattern of results – that is, showing that demand for ridesharing services in evening/early morning hours, where individuals

[25] We examine outcomes stratified by race and gender as a separate appendix item.

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

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



(a) External Cause Mortality (b) Homicide

(c) Suicide (d) Motor Vehicle

(e) Alcohol (f) Drugs

Fig. 3. Event studies for main Outcomes. *Notes*: Estimates are based on data from the National Vital Statistics System (NVSS) with the sample restricted to central counties that UberX ever enters. Estimation uses the difference-in-differences imputation approach (Borusyak et al., 2024). Coefficients should be interpreted as the effect of UberX entry on the mortality rate per 100,000 population. Death counts are denominated by the county population belonging to the applicable demographic group analyzed in that specification. The dashed line reflects our main difference-in-differences estimate, and the figure also includes the implied percent effect (i.e., the coefficient divided by the category mean). Vertical bands around each estimate depict 95% confidence intervals based on standard errors that are robust to clustering at the CBSA/division level.

are traveling to and from drinking establishments, coincides with mortality. Time of death, however, is unavailable in the restricted-use death certificates.

In columns (1) and (2) of Table 3, note that the 1.598 increase in external cause deaths is relative to a baseline of 12.37 deaths per 100,000 on weekends, representing a 12.91% increase. In contrast, the 0.419 increase relative to a weekday baseline of 8.22 corresponds to only a 5.09% increase. The pattern highlights that external cause deaths are disproportionately rising on weekends versus weekdays. For MVA deaths, the estimates imply a 21.7% decline on weekdays, which is significant at the 10% level. However, the effect is a 6% increase on the weekend, albeit not statistically distinguishable from zero. For homicide, we see a 15.3% decline on the weekend with a negligible effect on weekdays.

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 ridesharing facilitating transportation to places where people may engage in risky substance use. Note that for MVA and homicide the effects are relatively small. However, for homicide, we see a 20.6% decline in at home deaths, significant at the 10% level.

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 affect the impact

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**
	(0.367)	(0.213)	(0.275)	(0.303)	(0.489)	(0.549)
	[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
	(0.117)	(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**
	(0.111)	(0.194)	(0.151)	(0.300)	(0.233)	(0.158)
	[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)	(0.172)
	[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
	(0.320)	(0.197)	(0.281)	(0.170)	(0.570)	(0.431)
	[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
Observations	28,755	28,755	28,755	28,755	11,700	17,055

Notes: Estimates are based on data from the National Vital Statistics System (NVSS) with the sample restricted to central counties that UberX ever entered. Each estimate in this table is produced by a separate regression using the Borusyak et al. (2024) difference-in-differences imputation estimator. Coefficients should be interpreted as the effect of UberX entry on the quarterly number of deaths per 100,000 population. Death counts are denominated by the county population belonging to the applicable demographic group analyzed in that specification. These estimates are unweighted, and standard errors are robust to clustering at the 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.

of ridesharing, in columns (5) and (6) of Table 3 we present estimates in which 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 stratifying the sample by TCI scores show that areas with weaker transit options experience larger changes in external cause mortality and deaths that are substance use-related. For example, in areas with below median TCI scores, the 0.584 coefficient represents a 52% increase in alcohol-related deaths. Some examples of areas with below median TCI scores include Baton Rouge, LA, Greensboro, NC, and Worcester, MA. The corresponding increase for above median areas is just 0.022 (2%) and is statistically insignificantly different from zero. Areas with above median scores include Louisville, KY, Dayton, OH, and Syracuse, NY.[28] We see a similar pattern for drug-related mortality by TCI score, with a statistically significant 35% increase in areas with below median TCI scores and a statistically insignificant 12% increase in areas with above median scores. We find a 15% increase in suicide deaths, significant at the 1% level, in areas with weaker transit. In contrast, the effect is about 9% and significant only at the 10% level in those areas with better transit. The pattern of estimates reinforces the idea that ridesharing has a greater impact in areas with relatively weaker public transit options. Importantly, this highlights that for some other change to confound our findings, it would also have to predominantly affect those areas where transit options are relatively weaker.

Additionally, for MVA and homicide the effects are relatively small and/or statistically insignificant. One exception is that we see a 10.7% increase in MVA deaths in areas with better transit, significant at the 10% level, which suggests that even in areas with good existing transit options ridesharing does not seem to improve safety among young adults.

Notably, TCI scores are strongly correlated with population – large CBSAs have better transit connectivity scores than smaller ones.[29] Our estimates by TCI scores are nearly identical to what we see when we divide our sample into above and below median population groups.[30] That also means that our main estimates are driven by effects in smaller areas. 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 appears to widen the existing differences.

As an additional exercise, in Appendix Table A2, we examine changes in internal cause deaths (stroke, cancer, etc.) for those aged 18 to 45 after UberX enters. While we mostly do not find any effect of UberX on mortality across the various internal-cause categories, we do find a statistically significant decrease in deaths from heart disease and related conditions both overall and on weekend days, as well as a decline in

[28] New York City, Boston, and San Francisco earn the highest scores while hundreds of small areas receive a TCI score of 0.

[29] In our data, the correlation between TCI score and population is $\rho = 0.37$ and the estimate from a simple regression reveals that each 100,000-person increase in area population is associated with an approximate 5% increase in TCI score.

[30] Given the similarity, we do not show these results in the paper but they are available upon request. We provide estimates where we weight by population in Table 5.

Table 4
Heterogeneity of effects by age Group.

	Outcome: Deaths per 100k Population in the Noted Category				
	Age <18 (1)	Age 18–30 (2)	Age 31–45 (3)	Age 46–64 (4)	Age 65+ (5)
External Cause	−0.031	0.546	3.486***	1.892	19.528
	(0.337)	(0.440)	(0.548)	(2.067)	(12.529)
	[3.83]	[17.77]	[23.74]	[52.95]	[205.12]
Homicide	−0.027	−0.287	−0.088	−0.007	0.041
	(0.059)	(0.213)	(0.146)	(0.073)	(0.157)
	[0.60]	[2.47]	[1.68]	[0.89]	[0.36]
Suicide	−0.290	−0.372	1.359***	−0.929***	−0.515
	(0.222)	(0.322)	(0.394)	(0.203)	(0.422)
	[0.63]	[3.95]	[4.62]	[5.02]	[4.18]
MVA	0.140	−0.094	−0.215	0.234	0.431
	(0.121)	(0.299)	(0.345)	(0.311)	(0.476)
	[0.91]	[3.95]	[2.83]	[2.78]	[3.92]
Alcohol	−0.032	0.197*	0.648***	0.103	0.089
	(0.024)	(0.114)	(0.150)	(0.187)	(0.082)
	[0.02]	[0.71]	[1.49]	[1.60]	[0.41]
Drugs	0.014	1.166**	2.327***	2.979	12.782
	(0.021)	(0.522)	(0.530)	(2.396)	(12.077)
	[0.14]	[4.07]	[8.26]	[30.93]	[138.07]
CBSAs	329	329	329	329	329
Counties	639	639	639	639	639
Observations	28,755	28,755	28,755	28,755	28,755

Notes: Estimates are based on data from the National Vital Statistics System (NVSS) with the sample restricted to central counties that UberX has ever entered. Each estimate in this table is produced by a separate regression using the Borusyak et al. (2024) difference-in-differences imputation estimator. Coefficients should be interpreted as the effect of UberX entry on the quarterly number of deaths per 100,000 population. Death counts are denominated by the county population belonging to the applicable demographic group analyzed in that specification. These estimates are unweighted, and standard errors are robust to clustering at the CBSA/division level. Numbers in square brackets are the respective category means. *** p<0.01, ** p<0.05, * p<0.1.

internal cause deaths on weekdays. These significant effects may reflect ridesharing being used as an attractive substitute for ambulance rides when significant medical events occur (Moskatel and Slusky, 2019).

4.3. Heterogeneity by demographics

In Table 4, we examine outcomes separately for those aged under 18, 18 to 30, 31 to 45, 46 to 64, and 65 and older. We see this as a valuable exercise because Uber should primarily impact young adults. There are significant differences in the baseline mean number of deaths across these groups. For example, there are roughly twice as many deaths from both alcohol and drugs per 100,000 population in the 31 to 45 age group compared to the 18 to 30 age group. Consistent with those baseline patterns, we see smaller increases in the number of deaths in the younger age group after the advent of Uber, although the percent changes are similar. For example, the increase in deaths involving alcohol for the 18 to 30 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 entry of UberX. Among the 31 to 45 age 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, 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 30 s and 40 s than any other age group.[31]

Continuing to focus on those aged 18 to 30 versus those aged 31 to 45, our estimates show that the percent changes in overall external cause deaths differ noticeably across these two age groups. Aside from the changes in deaths involving alcohol and drugs, another explanation appears to be different patterns in the rates of suicide. For example, suicide increases by 29.4% in the 31 to 45 group after the advent of Uber, with the coefficient significant at the 1% level, but declines by 9.4% in the 18 to 30 group, with the estimate not statistically distinguishable from zero.

In Table 4, we also provide estimates for those under 18, those aged 46 to 64, and those over 65. We generally find no statistically significant effect on external cause deaths or alcohol and drug related deaths for any of these groups, though in some cases the magnitudes are economically meaningful while the estimates are statistically imprecise (e.g., a 9.5% increase in external cause deaths among those aged 65+, driven primarily by a 9.3% increase in drug overdose deaths). This pattern is generally consistent with expectations given our earlier discussion about UberX and smartphone usage across demographic groups during our sample period.[32] That said, we do see a statistically and economically significant 18.5% decline in suicide deaths among those 46 to 64 years old. It is possible that individuals in this older age group use UberX to socialize (with or without substance use), such that the mental health benefits of socialization (leading to fewer suicides) dominate any potential negative mental health consequences of substance use. Also, among those aged 46 to 64, we find similar level effects on drug overdoses and overall external-cause mortality to those for the 18 to 45 age group. That said, it is important to note that the baseline per-capita death rates are also considerably larger among the older age group (2.5× or more, relative to those aged 18 to 45). Thus, the effects are much smaller in implied percentage terms and more statistically imprecise. A similar pattern exists (though to an even larger extent, 10× or more) when comparing those aged 65+ to those between 18 and 45.

We also examine heterogeneity in the mortality effects by race and gender, and present the estimates in Appendix Table A3. We find larger level increases in substance use deaths among men than women, though when compared to baseline mortality means, the implied percentage changes are larger for women. Women experience a larger overall increase in external-cause deaths, driven by statistically significant increases in both homicide and suicide. By race, we only find a statistically and economically significant increase in alcohol-related deaths among white individuals, which is consistent with null results on alcohol consumption among Black respondents in Teltser et al. (2021). That said, our pooled estimate of alcohol mortality for racial and ethnic minorities is not sufficiently precise to statistically rule out potentially large effects in either direction. In contrast, suicide deaths increase more among racial and ethnic minorities, and drug overdose deaths increase significantly and similarly for both groups.

4.4. Sensitivity and robustness

In this section we examine how our estimates are affected by various estimation choices. For example, in column (1) of Table 5, we provide estimates that are weighted by population. The pattern of findings is similar to our main estimates but the coefficients are smaller in magnitude. As we show earlier, our estimates are mainly driven by UberX's effect on those aged 18 to 45 in areas with weaker transit connectivity. When we weight our estimates, assigning greater weight to CBSAs with larger populations, that also means we are assigning more weight to areas with better transit connectivity (see Section 4.2 above for additional details). Thus, weighting by population attenuates our findings. Note however that the population-weighted estimates reveal a negative and statistically significant effect of UberX on homicide deaths, suggesting

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

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

Table 5
Additional sensitivity and Robustness.

	Outcome: Deaths per 100k Population Age 18–45 in the Noted Category			
	Main Estimates, Pop.-Weighted (1)	Overlapping Estimates (2)	County of Residence (3)	Including "Never-Treated" CBSAs (4)
External Cause	1.257***	2.008***	1.809***	1.340***
	(0.376)	(0.392)	(0.533)	(0.321)
	[19.59]	[20.61]	[20.78]	[22.03]
Homicide	−0.309**	−0.187	−0.258**	−0.085
	(0.132)	(0.141)	(0.128)	(0.065)
	[2.71]	[2.06]	[2.11]	[1.95]
Suicide	0.187	0.476**	0.234	−0.235***
	(0.174)	(0.196)	(0.156)	(0.072)
	[3.63]	[4.26]	[4.27]	[4.68]
MVA	0.010	−0.130	−0.329	0.088
	(0.229)	(0.259)	(0.213)	(0.086)
	[2.76]	[3.34]	[3.36]	[4.24]
Alcohol	0.396***	0.396***	0.413**	0.261***
	(0.103)	(0.100)	(0.163)	(0.047)
	[1.11]	[1.41]	[1.11]	[1.04]
Drugs	1.127***	2.030***	1.903***	0.992***
	(0.418)	(0.483)	(0.420)	(0.243)
	[5.79]	[7.28]	[6.24]	[6.09]
CBSAs	329	329	329	937
Counties	639	639	639	1277
Observations	28,755	28,755	28,755	61,296

Notes: Estimates are based on data from the National Vital Statistics System (NVSS) with the sample restricted to central counties that UberX ever entered. Each estimate in this table is produced by a separate regression using the Borusyak et al. (2024) difference-in-differences imputation estimator. Coefficients should be interpreted as the effect of UberX entry on the quarterly number of deaths per 100,000 population. Death counts are denominated by the county population belonging to the applicable demographic group analyzed in that specification. The estimates are population-weighted in column (1) only. Standard errors are robust to clustering at the CBSA/division level. Numbers in square brackets are the respective category means. Overlapping estimates refer 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. "Including Never-Treated CBSA" refers to including central counties in CBSAs with no known UberX entry during our sample period. *** p<0.01, ** p<0.05, * p<0.1.

that UberX may improve safety in some ways in more populated areas (i.e., those where individuals perhaps previously relied mostly on public transit).

In column (2) of Table 5, 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 by 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 estimates are similar and show clear evidence of increases in mortality overall, consisting mostly of deaths relating to suicide, drugs, and alcohol.

Note that our main estimates focus on deaths occurring within a county. The estimates in column (3) of Table 5 assign deaths based on the county of residence. Naturally, the county of death and the county of residence are often the same. However, if we found substantially different results using county of residence, it would suggest that ridesharing reallocates some deaths across neighboring counties. That said, we find similar effects to our main estimates, suggesting minimal if any geographic reallocation of deaths. We discuss the estimates in column (4) of Table 5 in the next subsection.

Finally, in Appendix Figs. A4 and A5, we present placebo distributions and "leave-one-out" analyses for each of our main outcomes of interest as additional checks on robustness and inference. In the placebo distributions, we present coefficient estimates from a placebo exercise where we randomly reassign the actual UberX entry dates across CBSAs in our sample while preserving the true distribution of entry dates, and then re-estimate the effects of these random entry assignments 500 times. For reference, we include in the figure the estimate from our main specification as reported in Table 2. For drug and alcohol deaths – the outcomes where we observe the largest/strongest effects – the true entry date corresponds to an estimate in the far right tail of the distribution created by using random entry dates. In the leave-one-out analyses, we plot the density of the coefficient estimates from regression exercises where we sequentially remove one CBSA from our sample and then return it to the sample before removing another. Apart from a few outliers, the leave-one-out estimates are clustered tightly around the main estimates.

4.5. Further concerns about identification

There are two potential concerns about identification that we cannot fully rule out with our data, but which we argue are unlikely to represent substantial threats. The first is the degree to which fentanyl diffusion is correlated with the timing of UberX entry across the United States. One key challenge is that we want to avoid simply excluding fentanyl-related overdose deaths, as ridesharing-induced substance use (that leads to overdose) likely includes fentanyl and other fentanyl-laced drugs. Examining law enforcement seizures of fentanyl is also likely problematic, as ridesharing may both facilitate fentanyl distribution and help the distributors avoid detection. Instead, we rely on our overall pattern of findings to argue against the notion that the diffusion of fentanyl confounds our estimated effects of the rollout of UberX.

First, mortality effects are concentrated in areas with worse existing transit connectivity, suggesting that expanded transportation availability is driving our results. Second, we find no evidence of differential pre-trends in the drug overdose death event studies, which suggests that our estimates are not driven by differential diffusion of fentanyl into treated areas relative to not-yet-treated areas. Moreover, the event studies reveal that drug and alcohol deaths increase very similarly over time after UberX entry, which also suggests that the effects are driven by ridesharing-induced substance use rather than by confounding fentanyl diffusion. Third, in Table 5, column (4), when we include areas with no known UberX entry during our sample period in our analyses (which should also have experienced the entry and growth of fentanyl), we find slightly attenuated but still statistically and economically significant effects on all external-cause deaths, alcohol deaths, and drug deaths. Finally, earlier estimates show that the increased deaths occur primarily on weekends, and are concentrated among younger adults. Overall, it is quite unlikely that fentanyl diffusion and take-up would follow these exact patterns or overlap extremely closely with the UberX rollout, suggesting that ridesharing is indeed the key driver of our findings. Instead, the main concern is likely one of external validity. That is, we suspect that the drug overdose effect would not have been as large (in levels) if ridesharing had instead arrived in a world with little to no fentanyl availability.

The second concern pertains to our use of a binary indicator for UberX entry.[33] We view using an indicator for entry as most appropriate because we are interested in the effect of an exogenous transportation

[33] In a working paper version of their article, Anderson and Davis (2026) argue that UberX entry explains little of the variation in usage. The argument was based on R^2 comparisons across specifications, which is problematic as R^2 can be very sensitive to the composition of treated versus untreated areas, the number of pre- and post-treatment periods included in the estimation sample, and more.

availability shock (i.e., the first arrival of meaningful ridesharing access) in an area. In contrast, using ridesharing usage intensity as a treatment variable measures an equilibrium outcome jointly determined by supply, demand, and prevailing prices. Differences in usage intensity across place and time are much less likely to be plausibly exogenous to individuals' behavior and their resulting mortality than a binary indicator of the initial entry of UberX. To the extent that ridesharing usage was nonexistent, delayed, or gradual following initial entry, our estimated effects will tend to be attenuated (i.e., conservative) relative to the increase from no ridesharing to the full steady-state level of ridesharing availability and usage. Despite this, we find an economically meaningful and statistically significant increase in external-cause mortality which grows over time following UberX entry and is driven primarily by drug and alcohol deaths.

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 the arrival of ridesharing in an area 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 the impact of ridesharing on mortality manifests primarily via increases in alcohol-related and drug-related deaths. Our estimates suggest that the advent of ridesharing is associated with somewhere between 6,400 and 8,600 additional deaths per year, depending on assumptions. These estimates are very similar in magnitude to the alcohol-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, it is not surprising that we find the largest impacts on younger adults, and that the effects are concentrated around weekends. Heterogeneity by race and gender is more mixed. Alcohol-related deaths increase primarily among white individuals, mirroring the null effect on alcohol consumption among Black respondents found in Teltser et al. (2021), but women and racial and ethnic minorities experience larger increases in overall external-cause mortality driven by homicide, suicide, and drug-related deaths.

Overall, the evidence is consistent with ridesharing primarily increasing mortality by increasing instances of "socializing" that can lead to riskier decision-making while under the influence of narcotics or alcohol. 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. For example, media reports suggest ridesharing services may also facilitate drug distribution.[34] In any case, we provide suggestive evidence on mechanisms using observable characteristics in our vital statistics data, while also highlighting findings from the existing literature on the effects of ridesharing.

To provide additional context, Ruhm (2019) argues that the availability of illicit opioids has played a significant role in the increasing pace of overdose deaths. Studying the impact of local economic conditions and changes in drug availability on county-level substance-related mortality rates between 1999 and 2015, Ruhm 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 were 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 entry of ridesharing 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 the consumption of alcohol and illicit substances as complementary goods. This is a novel and complementary explanation for the ongoing "fatal drug epidemic."

More broadly, examining how ridesharing affects mortality helps us better understand its safety impacts and unintended consequences, and alerts policymakers who can devote resources to mitigating the harmful consequences. Given our findings that the mortality effects are concentrated on weekends and in areas with weaker pre-existing transit options, targeted public health intervention (e.g., expanded weekend and late-night harm-reduction resources, or public health messaging timed to periods of peak ridesharing demand) could help alleviate the unintended mortality consequences of ridesharing, regardless of the underlying mechanism. If future research finds that supply-side mechanisms (e.g., drug distribution) are also present, that could warrant additional intervention such as platform regulation or additional law enforcement efforts.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Notably, that argument and the corresponding analysis do not appear in the peer-reviewed published version of the paper.

[34] 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.

Appendix A. Additional estimates

A.1. Predicting UberX entry

In Table A1, we regress the timing of UberX entry on various changes in area level characteristics in the years leading up to UberX entry (2008 to 2011). The goal of this exercise is to eliminate the potential for UberX's entry to be correlated with other changes that might explain increases in mortality.

To begin, in Panel A of Table A1 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 of Table A1, 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 $1000 in average real per-capita annual income growth prior to 2012 was associated with entry 0.13 quarters later, which is roughly 12 days. Further, this estimate is only marginally significant at the 10% level. Additionally, 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.

Table A1
Predicting year & quarter of UberX Entry.

	Panel A					
	External-Cause Mortality Rate (1)	Homicide Mortality Rate (2)	Suicide Mortality Rate (3)	MVA Mortality Rate (4)	Alcohol Mortality Rate (5)	Drug Mortality Rate (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]
Observations	12,141	12,141	12,141	12,141	12,141	12,141
	Panel B					
	Unemployment Rate (1)	Real Per-capita Income (2)	Population 18–45 (3)			
Variable Coefficient	−0.165**	0.130*	6.194			
	(0.064)	(0.067)	(17.77)			
	[0.10]	[0.002]	[132.31]			
Observations	12,122	11,704	12,141			
CBSAs	329	329	329	329	329	329
Counties	639	639	639	639	639	639

Notes: Regressions estimated using Ordinary Least Squares. Panel A uses National Vital Statistics System (NVSS) data. Panel B outcomes come from the Surveillance, Epidemiology, and End Results (SEER) data. 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 variables in the regression, using first-differences. Coefficients should be interpreted as the effect of the growth rate in the value of the column variable prior to 2012 on the future UberX quarter-year of entry. The unemployment rate is in units ranging from 0 to 100, e.g., a value of 5.0 indicates a 5% unemployment rate. Real per-capita income is in 2007 dollars in 1000 s. The population explanatory variable is calculated in percentage changes, implying the interpretation that a 1 percentage point increase in the pre-2012 population growth rate is associated with later entry of 0.062 quarters. These estimates are unweighted, and include year-quarter fixed effects, with standard errors robust to clustering at the CBSA/division level. Numbers in square brackets are the respective means of the explanatory variables in level first-differences (including population). *** p<0.01, ** p<0.05, * p<0.1.

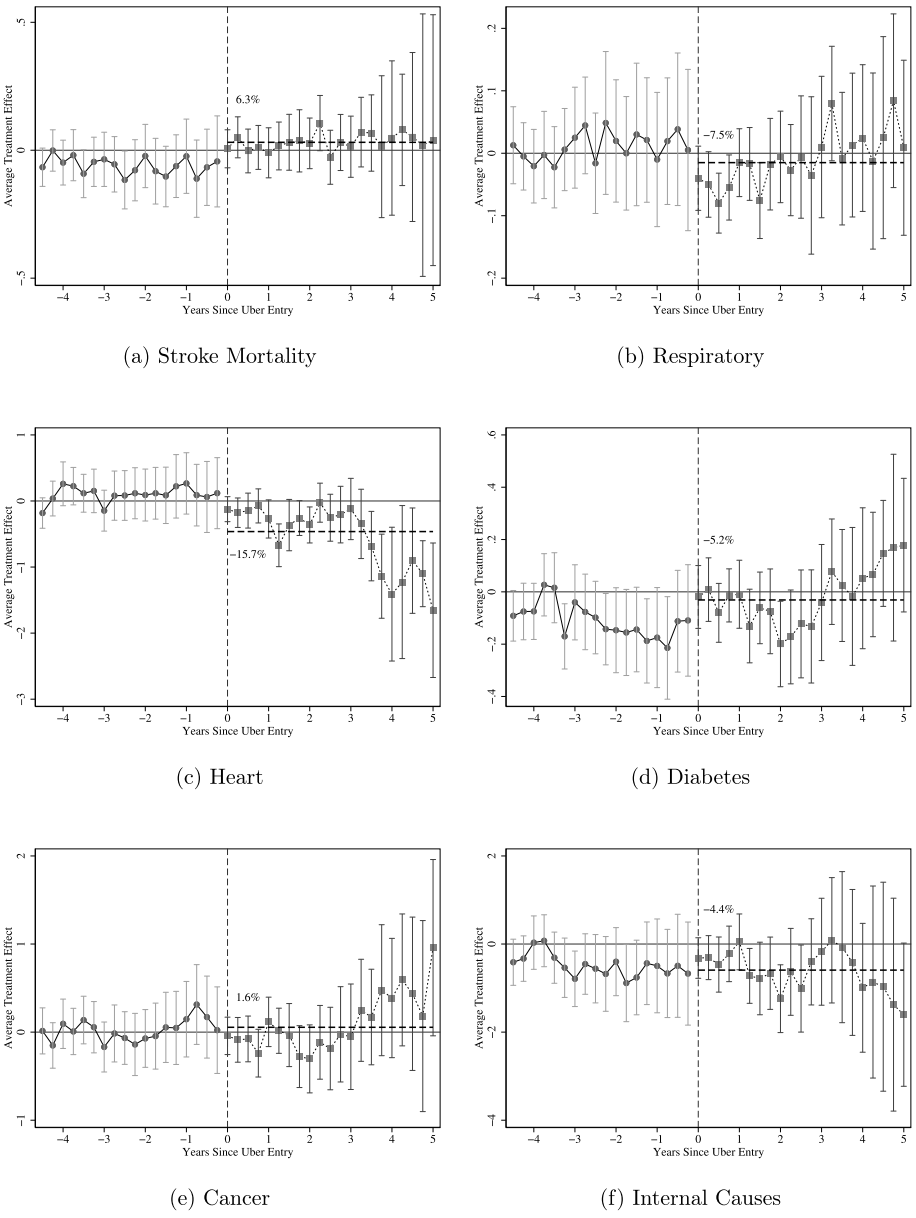
A.2. Internal-cause deaths

Table A2 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 A2 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 that 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 Fig. A1, we provide event study analyses for these outcomes.

Table A2
Estimates for internal-cause deaths.

	Outcome: Deaths per 100k Population in the Noted Category		
	Age 18–45 (1)	Weekend 18–45 (2)	Weekday 18–45 (3)
Stroke	0.031	0.030	0.001
	(0.050)	(0.037)	(0.028)
	[0.49]	[0.28]	[0.22]
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]	[1.22]
Diabetes	−0.031	−0.066	0.039
	(0.065)	(0.054)	(0.033)
	[0.60]	[0.34]	[0.26]
Cancer	0.055	0.114	−0.059
	(0.193)	(0.130)	(0.106)
	[3.40]	[1.94]	[1.46]
Internal Causes	−0.592	−0.178	−0.417**
	(0.450)	(0.394)	(0.208)
	[13.50]	[7.75]	[5.74]
CBSAs	329	329	329
Counties	639	639	639
Observations	28,755	28,755	28,755

Notes: Estimates are based on data from the National Vital Statistics System (NVSS) with the sample restricted to central counties that UberX ever entered. Each estimate in this table is produced by a separate regression using the Borusyak et al. (2024) difference-in-differences imputation estimator. Coefficients should be interpreted as the effect of UberX entry on the quarterly number of deaths per 100,000 population. Death counts are denominated by the county population belonging to the applicable demographic group analyzed in that specification. Internal-cause deaths exclude those that contain any external cause of death from Table B1. These estimates are unweighted, and standard errors are robust to clustering at the CBSA/division level. *** p<0.01, ** p<0.05, * p<0.1.



(a) Stroke Mortality (b) Respiratory

(c) Heart (d) Diabetes

(e) Cancer (f) Internal Causes

Fig. A1. Event studies for Internal-Cause Deaths. *Notes*: Estimates are based on data from the National Vital Statistics System (NVSS) with the sample restricted to central counties that UberX ever entered, except where indicated. Estimation uses the difference-in-differences imputation approach (Borusyak et al., 2024). Coefficients should be interpreted as the effect of UberX entry on the mortality rate per 100,000 population. Death counts are denominated by the county population belonging to the applicable demographic group analyzed in that specification. The dashed line reflects our main differences-in-differences estimate, and the figure also includes the implied percent effect (i.e., the coefficient divided by the category mean). Vertical bands around each estimate depict 95% confidence intervals, based on standard errors that are robust to clustering at the CBSA/division level.

A.3. Heterogeneity by race and gender

We examine outcomes separately for men, women, white individuals, and racial and ethnic minorities in Table A3. In Teltser et al. (2021), the authors find larger impacts on alcohol consumption among men and white individuals. If UberX-induced alcohol consumption were the sole driver of our mortality results, we might expect a similarly concentrated pattern here. Table A3 shows a more nuanced picture, which we discuss by gender and then by race below.

When looking at men only and women only in columns (1) and (2), we find larger level effects on alcohol and drug deaths for men, though the implied percentages (when comparing the coefficients to baseline mortality means) are larger for women. We also find that overall external-cause deaths increase more among women than men, alongside statistically and economically significant increases in homicides and suicides among women. In contrast, we see fewer homicides among men (-0.54 per 100,000, or -16%, significant at the 10% level).

Turning to race, in columns (3) and (4), we find an increase in alcohol-related deaths only among white individuals, consistent with Teltser et al. (2021)'s null result on alcohol consumption among Black respondents.[35] Drug overdose deaths, however, increase significantly for both white individuals and racial and ethnic minorities, and suicide deaths increase considerably more among racial and ethnic minorities.

Taken together, these gender and race patterns indicate that channels beyond alcohol consumption alone (including homicide, suicide, and drug overdose) contribute to the broader mortality impact of ridesharing, and that these channels do not map neatly onto the same demographic groups as the drinking-specific effects documented in our earlier work.

Table A3
Heterogeneity of effects by race and Gender.

	Outcome: Deaths per 100k Population Age 18–45 in the Noted Category			
	Men (1)	Women (2)	White (3)	Racial and Ethnic Minorities (4)
External Cause	1.850**	2.380***	2.315***	3.969**
	(0.887)	(0.772)	(0.541)	(1.669)
	[29.98]	[11.19]	[21.40]	[18.96]
Homicide	−0.543*	0.180***	−0.037	−0.241
	(0.294)	(0.064)	(0.108)	(0.489)
	[3.38]	[0.76]	[1.11]	[4.49]
Suicide	0.605	0.347**	0.436*	1.011***
	(0.450)	(0.144)	(0.258)	(0.382)
	[6.68]	[1.80]	[4.73]	[2.90]
MVA	−0.376	0.177	−0.133	−0.598
	(0.501)	(0.169)	(0.286)	(0.628)
	[4.90]	[1.76]	[3.49]	[3.29]
Alcohol	0.517***	0.335***	0.503***	0.020
	(0.180)	(0.073)	(0.108)	(0.453)
	[1.69]	[0.50]	[1.21]	[0.80]
Drugs	2.106***	1.390***	2.102***	2.145***
	(0.562)	(0.431)	(0.475)	(0.495)
	[8.17]	[4.11]	[6.93]	[3.87]
CBSAs	329	329	329	329
Counties	639	639	639	639
Observations	28,755	28,755	28,755	28,755

Notes: Estimates are based on data from National Vital Statistics System (NVSS) with the sample restricted to central counties that UberX ever entered. Each estimate in this table is produced by a separate regression using the Borusyak et al. (2024) difference-in-differences imputation estimator. Coefficients should be interpreted as the effect of UberX entry on the quarterly number of deaths per 100,000 population. Death counts are denominated by the county population belonging to the applicable demographic group analyzed in that specification. These estimates are unweighted, and standard errors are robust to clustering at the CBSA/division level. Numbers in square brackets are the respective category means. *** p<0.01, ** p<0.05, * p<0.1.

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

A.4. Estimates excluding drugs and alcohol deaths

Table A4 presents estimates for external causes where we exclude any deaths that involve alcohol or drugs in any way. We present the corresponding estimates including alcohol and drugs for ease of comparison.

Table A4
Estimates stratified by alcohol or drug involvement.

	Outcome: Deaths per 100k Population in the Noted Category	
	Involving Alcohol or Drugs Age 18–45 (1)	Not Involving Alcohol or Drugs Age 18–45 (2)
External Cause	2.153***	−0.145
	(0.446)	(0.329)
	[7.92]	[12.69]
Homicide	−0.093	−0.095
	(0.065)	(0.149)
	[0.04]	[2.02]
Suicide	0.142**	0.339
	(0.058)	(0.208)
	[0.44]	[3.83]
MVA	−0.043	−0.091
	(0.076)	(0.208)
	[0.20]	[3.13]
CBSAs	329	329
Counties	639	639
Observations	28,755	28,755

Notes: Estimates are based on data from National Vital Statistics System (NVSS) with the sample restricted to central counties that UberX has ever entered. Each estimate in this table is produced by a separate regression using the Borusyak et al. (2024) difference-in-differences imputation estimator. Deaths are assigned to Homicide, Suicide, and Motor Vehicle in the same manner as 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. Coefficients should be interpreted as the effect of UberX entry on the quarterly number of deaths per 100,000 population. Death counts are denominated by the county population belonging to the applicable demographic group analyzed in that specification. These estimates are unweighted, and standard errors are robust to clustering at the CBSA/division level.*** p<0.01, ** p<0.05, * p<0.1.

A.5. Additional event studies

Fig. A2 shows event studies that use the same specification as in the main text of the paper but where we limit the sample to CBSAs with below-median TCI scores. Fig. A3 shows these event studies for only those areas with above-median TCI scores. Neither set of figures shows evidence of problematic pre-trends. 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 Fig. 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 Fig. A2 to four years of post UberX entry coefficients.

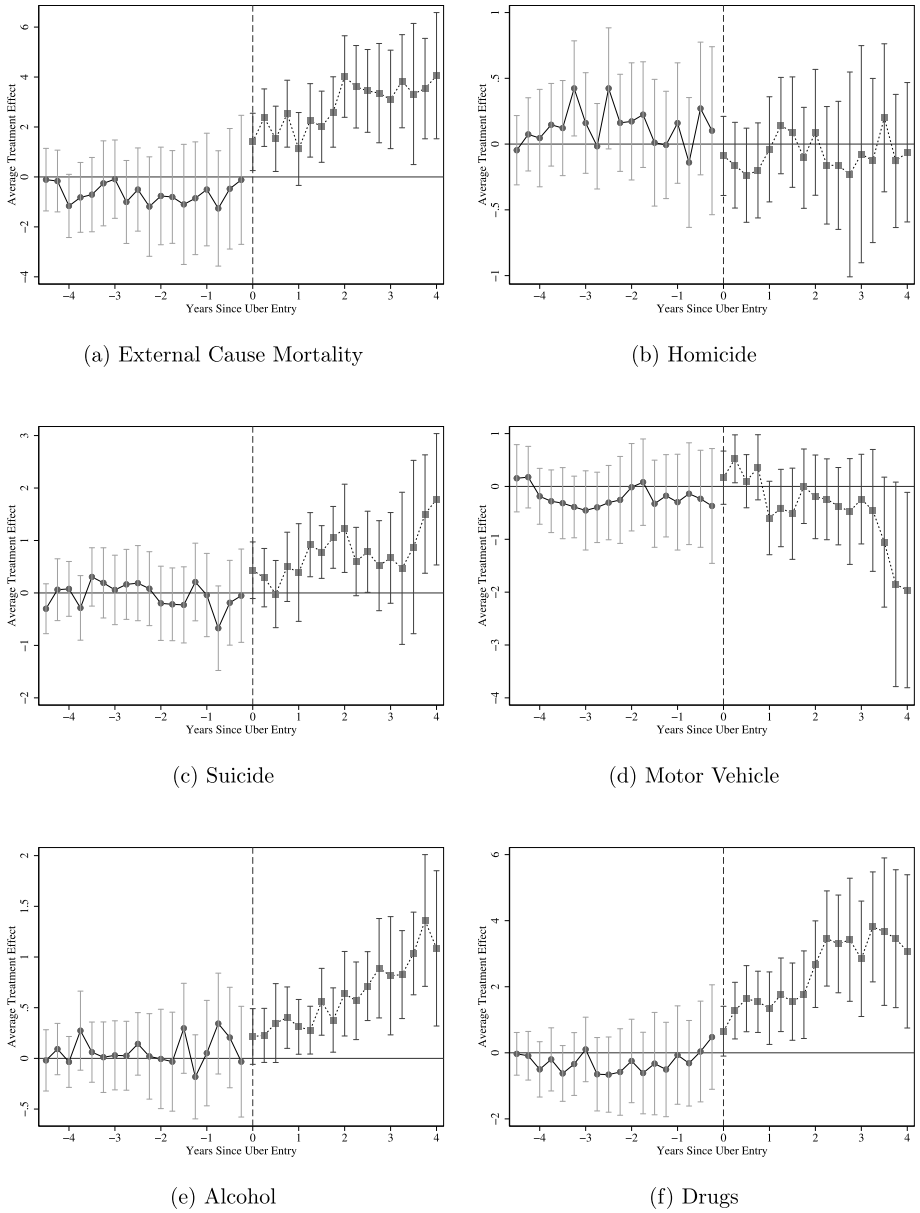
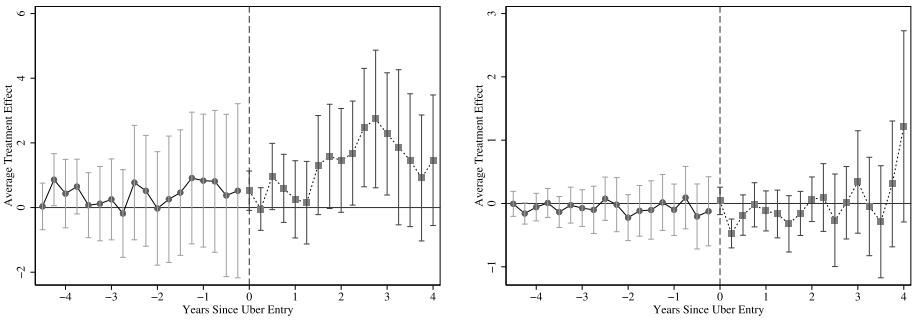
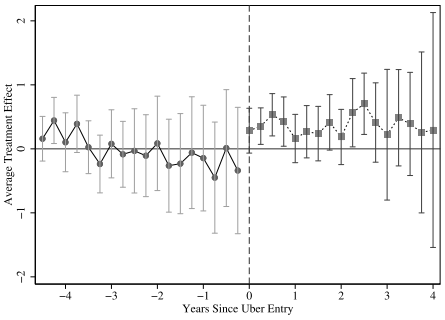


Fig. A2. Event studies for main outcomes in below median TCI score Areas. *Notes*: Estimates are based on data from the National Vital Statistics System (NVSS) with the sample restricted to central counties that UberX ever entered. Estimation uses the difference-in-differences imputation approach (Borusyak et al., 2024). Coefficients should be interpreted as the effect of UberX entry on the mortality rate per 100,000 population. Death counts are denominated by the county population belonging to the applicable demographic group analyzed in that specification. Vertical bands around each estimate depict 95% confidence intervals based on standard errors that are robust to clustering at the CBSA/Division level.

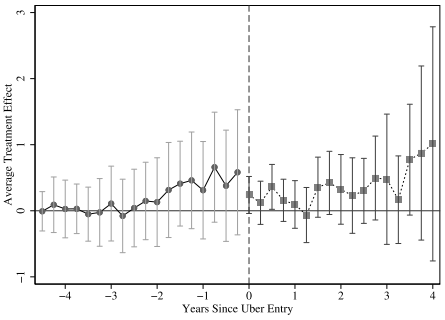


(a) External Cause Mortality

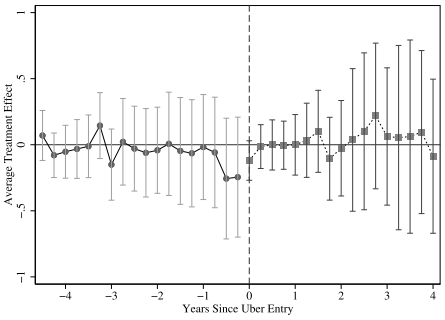
(b) Homicide



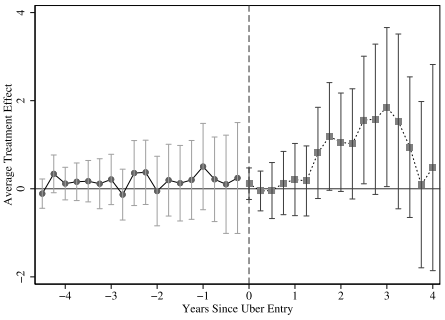
(c) Suicide



(d) Motor Vehicle



(e) Alcohol



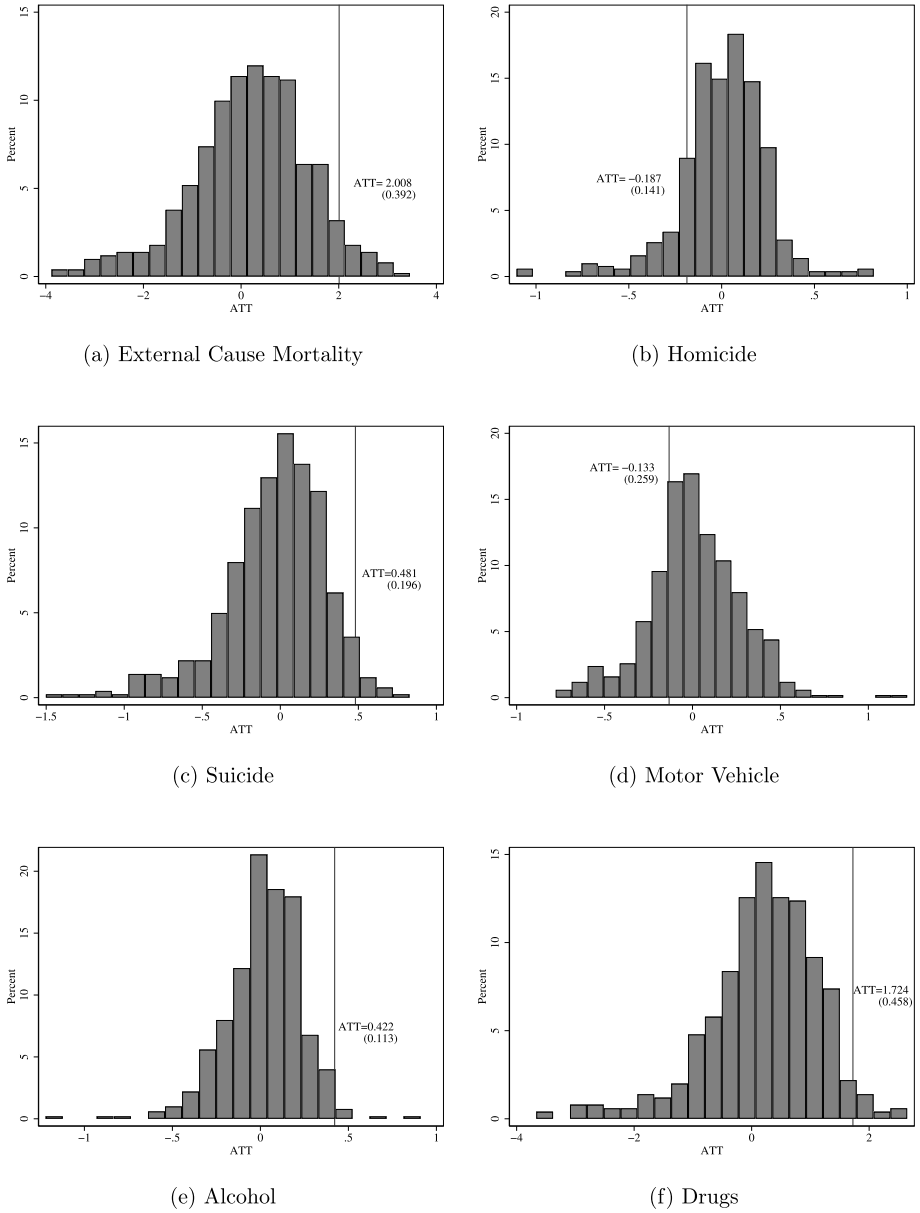
(f) Drugs

Fig. A3. Event studies for main outcomes in above median TCI score Areas. *Notes*: Estimates are based on data from the National Vital Statistics System (NVSS) with the sample restricted to central counties that UberX ever entered. Estimation uses the difference-in-differences imputation approach (Borusyak et al., 2024). Coefficients should be interpreted as the effect of UberX entry on the mortality rate per 100,000 population. Death counts are denominated by the county population belonging to the applicable demographic group analyzed in that specification. Vertical bands around each estimate plot 95% confidence intervals based on standard errors that are robust to clustering at the CBSA/division level.

A.6. Placebo distributions and leave one out analyses

In Fig. A4, we present coefficient estimates from a placebo exercise where we randomly reassign the actual UberX entry dates across CBSAs in our sample while preserving the true distribution of entry dates, and then re-estimate the effects of these random entry assignments 500 times. For reference, we include in the figure the estimate from our main specification as reported in Table 2. For drug and alcohol deaths – the outcomes where we observe the largest/strongest drivers of external-cause mortality – the true entry date corresponds to an estimate in the far right tail of the distribution created by using random entry dates.

Fig. A5 presents "Leave One Out" analyses for each of our main outcomes of interest. In each figure, we plot the density of the coefficient estimates from regression exercises where we sequentially remove one CBSA from our sample and then return it to the sample before removing another. We use the same specification, sample selection, standard error calculations, and point estimator as in our main difference-in-differences estimates in Table 2. We provide the treatment effect from our main estimates for comparison. Apart from a few outliers, the effects, regardless of which CBSA is removed from the sample, are clustered relatively tightly around the main effect.

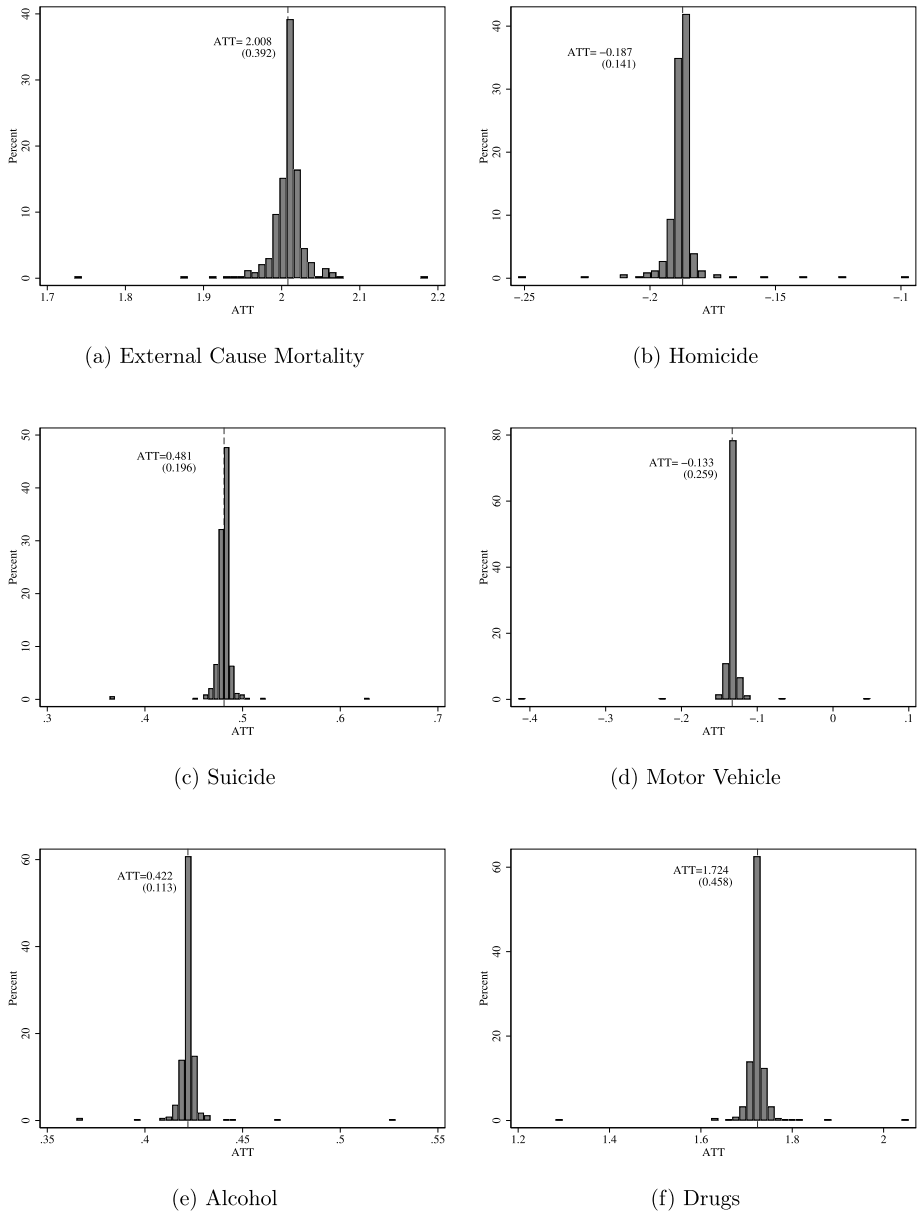


(a) External Cause Mortality (b) Homicide

(c) Suicide (d) Motor Vehicle

(e) Alcohol (f) Drugs

Fig. A4. Placebo Analyses. *Notes*: Plots show distributions of our main estimates (i.e., Table 2, column (1)) from 500 iterations using random UberX entry dates, drawn from the set of actual entry dates. ATT = average treatment effect on the treated.



(a) External Cause Mortality (b) Homicide

(c) Suicide (d) Motor Vehicle

(e) Alcohol (f) Drugs

Fig. A5. Leave-One-Out Analyses. *Notes*: The figures show "Leave One Out Analyses" for each of our main outcomes of interest. In each figure, we plot the probability density of the coefficient estimates from hundreds of regressions where we sequentially remove one CBSA from our sample and then return it to the sample before removing another. We also provide the treatment effect (ATT, treatment on the treated) from our main estimates for comparison.

Appendix B. ICD codes by category

In Table B1, we explain the ICD codes that form our homicide, suicide, MVA, alcohol, and drug cause of death categories. We focus on these categories of death 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, 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

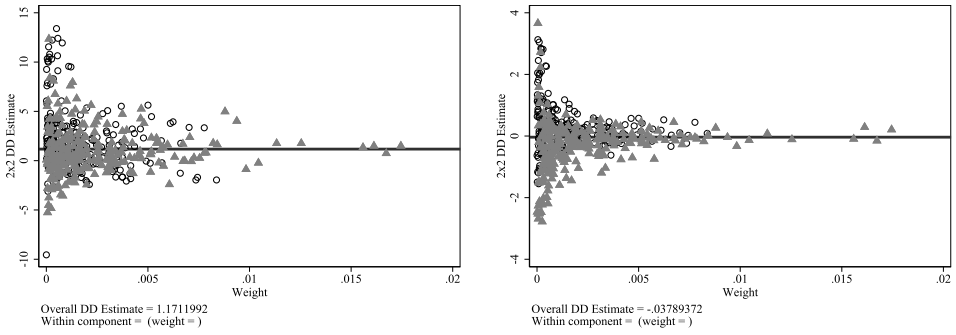
Note: The table gives ICD-10 codes that comprise our main outcomes of interest. ICD-10 block descriptions are as follows: 'F00–F99' Mental and behavioral disorders, 'K00–K93' Diseases of the digestive system, 'R00–R99' Symptoms, signs and abnormal clinical and laboratory findings, 'S00–T88' Injury, poisoning 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

Fig. 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. (2024) 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 subcategories of external causes except suicide, the aggregate late versus early ATT (average treatment on the treated) coefficient is smaller in magnitude than that for the earlier versus later treated comparisons, suggesting downward bias due to heterogeneous treatment effects.

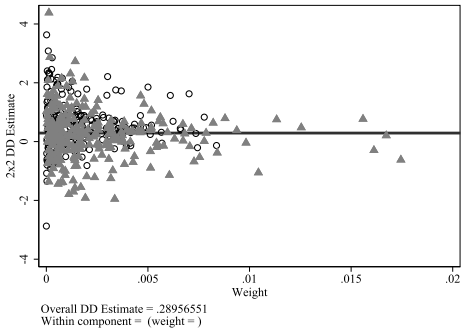
In Table C1, we show how a TWFE approach leads to a downward bias. In that table, the estimates in columns (1), (3), and (5) use the TWFE difference-in-differences estimator without correcting for heterogeneous treatment effect bias. Column (1) shows the difference-in-differences estimate from early 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 weights 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., 2024) for comparison purposes.

Further, event study coefficients calculated using the newer difference-in-differences methods are not all relative to the period $t = -1$. In particular, when using the Borusyak et al. (2024) imputation approach, the pre-period coefficients are relative to the earliest pre-treatment period while the post period coefficients are relative 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 Figs. C2 and C3. They show no evidence of any issue.

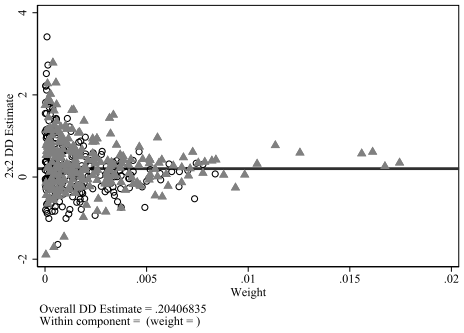


(a) External Cause Mortality

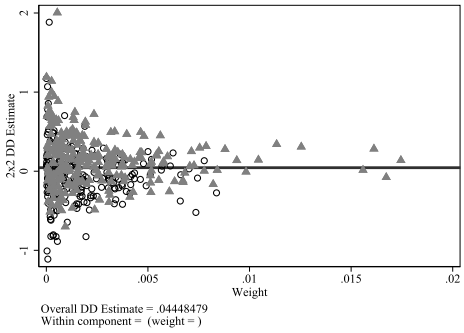
(b) Homicide



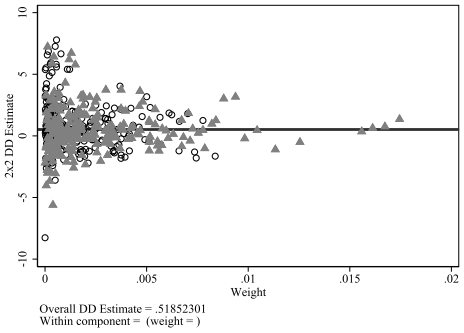
(c) Suicide



(d) Motor Vehicle



(e) Alcohol



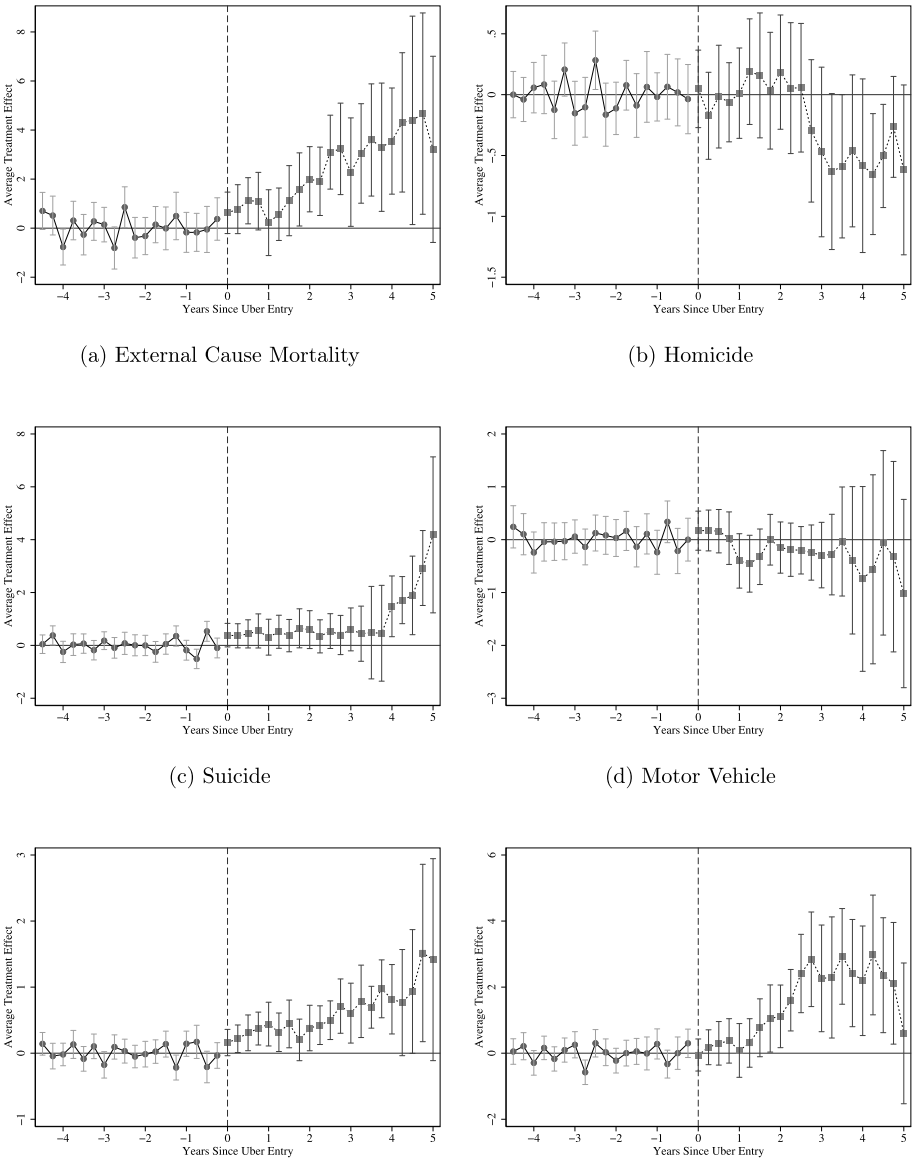
(f) Drugs

Fig. C1. Goodman-Bacon Decomposition. *Notes*: Goodman-Bacon decompositions for our main outcomes of interest. The shaded triangles represent the "early vs. late" treated comparisons, and the unshaded circles represent the problematic "late vs. early" treated comparisons.

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 (1)	Weight Assigned (2)	DID Coefficient (3)	Weight Assigned (4)	TWFE (5)	DID Imputation (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
Observations					30,672	28,755

Notes: Estimates are based on data from National Vital Statistics System (NVSS) with the sample restricted to central counties that UberX ever enters. Coefficients should be interpreted as the effect of UberX entry on the quarterly number of deaths per 100,000 population. Death counts are denominated by the county population belonging to the applicable demographic group analyzed in that specification. These estimates are unweighted, and standard errors are robust to clustering at the 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.

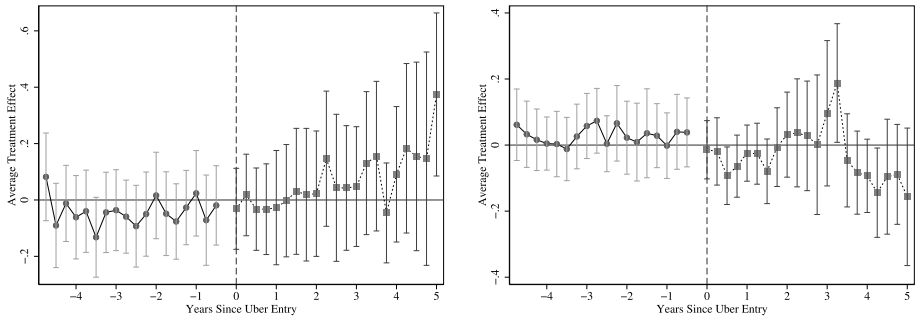


(a) External Cause Mortality (b) Homicide

(c) Suicide (d) Motor Vehicle

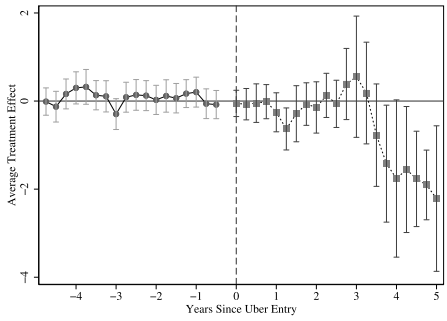
(e) Alcohol (f) Drugs

Fig. C2. Event studies for main outcomes - Callaway and Sant'Anna (2021) "CSDiD" with "long2" option to deal with asymmetric coefficient calculations (Roth, 2024).

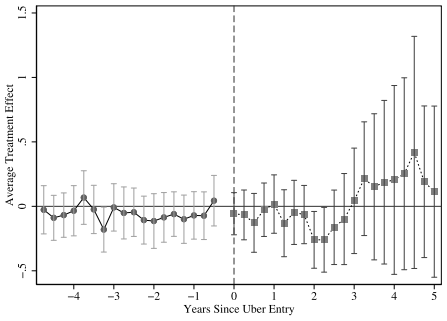


(a) Stroke Mortality

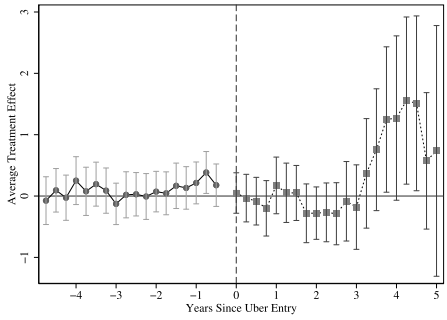
(b) Respiratory



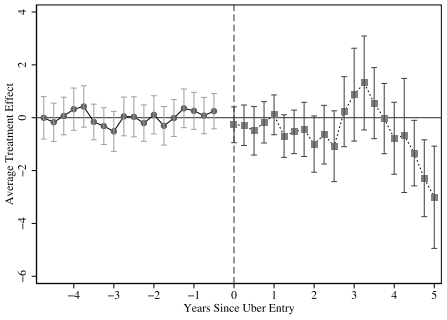
(c) Heart



(d) Diabetes



(e) Cancer



(f) Internal Causes

Fig. C3. Event studies for Internal-Cause deaths - Callaway and Sant'Anna (2021) "CSDiD" with "long2" option to deal with asymmetric coefficient calculations (Roth, 2024).

Appendix D. UberX entry dates and maps

In this section we provide a complete list of all UberX entry dates (Tables D1–D7). 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 (Fig. D1). We also provide a map showing non-central counties versus central counties (Fig. D2).

Table D1
UberX 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)
20,524*	Dutchess County-Putnam County, NY	08/24/2012	40,900	Sacramento–Roseville–Arden-Arcade, CA	09/30/2013
35,614*	New York-Jersey City-White Plains, NY-NJ	08/24/2012	19,740	Denver-Aurora-Lakewood, CO	10/04/2013
36,084*	Oakland-Hayward-Berkeley, CA	01/18/2013	46,060	Tucson, AZ	10/10/2013
41,884*	San Francisco-Redwood City-South San Francisco, CA	01/18/2013	36,420	Oklahoma City, OK	10/30/2013
42,034*	San Rafael, CA	01/18/2013	42,200	Santa Maria-Santa Barbara, CA	10/31/2013
11,244*	Anaheim-Santa Ana-Irvine, CA	03/14/2013	19,804*	Detroit-Dearborn-Livonia, MI	10/31/2013
31,084*	Los Angeles-Long Beach-Glendale, CA	03/14/2013	47,664*	Warren-Troy-Farmington Hills, MI	10/31/2013
42,644*	Seattle-Bellevue-Everett, WA	04/11/2013	19,124*	Dallas-Plano-Irving, TX	11/05/2013
16,974*	Chicago-Naperville-Arlington Heights, IL	04/22/2013	12,580	Baltimore-Columbia-Towson, MD	11/06/2013
20,994*	Elgin, IL	04/22/2013	45,940	Trenton, NJ	11/13/2013
29,404*	Lake County-Kenosha County, IL-WI	04/22/2013	35,084*	Newark, NJ-PA	11/13/2013
41,740	San Diego-Carlsbad, CA	05/09/2013	34,980	Nashville-Davidson–Murfreesboro–Franklin, TN	12/10/2013
14,454*	Boston, MA	06/04/2013	41,500	Salinas, CA	02/04/2014
15,764*	Cambridge-Newton-Framingham, MA	06/04/2013	42,100	Santa Cruz-Watsonville, CA	02/04/2014
40,484*	Rockingham County-Strafford County, NH	06/04/2013	23,420	Fresno, CA	02/05/2014
12,060	Atlanta-Sandy Springs-Roswell, GA	06/26/2013	26,420	Houston-The Woodlands-Sugar Land, TX	02/21/2014
35,004*	Nassau County-Suffolk County, NY	07/01/2013	18,140	Columbus, OH	02/25/2014
41,940	San Jose-Sunnyvale-Santa Clara, CA	07/24/2013	31,540	Madison, WI	03/06/2014
43,524*	Silver Spring-Frederick-Rockville, MD	08/08/2013	38,300	Pittsburgh, PA	03/13/2014
47,894*	Washington-Arlington-Alexandria, DC-VA-MD-WV	08/08/2013	17,140	Cincinnati, OH-KY-IN	03/27/2014
33,460	Minneapolis-St. Paul-Bloomington, MN-WI	09/04/2013	46,140	Tulsa, OK	03/27/2014
26,900	Indianapolis-Carmel-Anderson, IN	09/05/2013	33,340	Milwaukee-Waukesha-West Allis, WI	03/28/2014
38,060	Phoenix-Mesa-Scottsdale, AZ	09/05/2013	41,700	San Antonio-New Braunfels, TX	03/28/2014
39,300	Providence-Warwick, RI-MA	09/12/2013	14,860	Bridgeport-Stamford-Norwalk, CT	04/01/2014
16,740	Charlotte-Concord-Gastonia, NC-SC	09/27/2013	47,220	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
UberX 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)
15,804*	Camden, NJ	04/01/2014	33,124*	Miami-Miami Beach-Kendall, FL	06/04/2014
33,874*	Montgomery County-Bucks County-Chester County, PA	04/01/2014	46,520	Urban Honolulu, HI	06/12/2014
33,700	Modesto, CA	04/02/2014	12,540	Bakersfield, CA	06/13/2014
40,140	Riverside-San Bernardino-Ontario, CA	04/03/2014	30,460	Lexington-Fayette, KY	06/13/2014
17,460	Cleveland-Elyria, OH	04/08/2014	45,780	Toledo, OH	06/13/2014
45,104*	Tacoma-Lakewood, WA	04/08/2014	20,500	Durham-Chapel Hill, NC	06/26/2014
45,300	Tampa-St. Petersburg-Clearwater, FL	04/11/2014	21,340	El Paso, TX	06/26/2014
11,460	Ann Arbor, MI	04/22/2014	22,180	Fayetteville, NC	06/26/2014
31,140	Louisville/Jefferson County, KY-IN	04/24/2014	24,660	Greensboro-High Point, NC	06/26/2014
32,820	Memphis, TN-MS-AR	04/24/2014	31,180	Lubbock, TX	06/26/2014
35,300	New Haven-Milford, CT	04/24/2014	48,900	Wilmington, NC	06/26/2014
39,580	Raleigh, NC	04/26/2014	49,180	Winston-Salem, NC	06/26/2014
10,740	Albuquerque, NM	04/30/2014	12,100	Atlantic City-Hammonton, NJ	06/27/2014
47,260	Virginia Beach-Norfolk-Newport News, VA-NC	05/01/2014	36,140	Ocean City, NJ	06/27/2014
17,820	Colorado Springs, CO	05/02/2014	16,700	Charleston-North Charleston, SC	07/10/2014
18,580	Corpus Christi, TX	05/02/2014	17,900	Columbia, SC	07/10/2014
27,260	Jacksonville, FL	05/05/2014	24,860	Greenville-Anderson-Mauldin, SC	07/10/2014
36,540	Omaha-Council Bluffs, NE-IA	05/05/2014	34,820	Myrtle Beach-Conway-North Myrtle Beach, SC-NC	07/10/2014
23,104*	Fort Worth-Arlington, TX	05/05/2014	38,900	Portland-Vancouver-Hillsboro, OR-WA	07/10/2014
44,060	Spokane-Spokane Valley, WA	05/08/2014	12,940	Baton Rouge, LA	07/11/2014
28,140	Kansas City, MO-KS	05/09/2014	11,100	Amarillo, TX	07/16/2014
42,220	Santa Rosa, CA	05/12/2014	37,100	Oxnard-Thousand Oaks-Ventura, CA	07/17/2014
41,620	Salt Lake City, UT	05/27/2014	42,020	San Luis Obispo-Paso Robles-Arroyo Grande, CA	07/17/2014
12,420	Austin-Round Rock, TX	06/04/2014	25,540	Hartford-West Hartford-East Hartford, CT	07/22/2014
36,740	Orlando-Kissimmee-Sanford, FL	06/04/2014	21,660	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
UberX 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)
41,420	Salem, OR	07/23/2014	30,700	Lincoln, NE	08/28/2014
22,420	Flint, MI	07/24/2014	37,060	Oxford, MS	08/28/2014
24,340	Grand Rapids-Wyoming, MI	07/24/2014	43,780	South Bend-Mishawaka, IN-MI	08/28/2014
28,020	Kalamazoo-Portage, MI	07/24/2014	45,220	Tallahassee, FL	08/28/2014
29,620	Lansing-East Lansing, MI	07/24/2014	46,220	Tuscaloosa, AL	08/28/2014
24,540	Greeley, CO	08/01/2014	47,380	Waco, TX	08/28/2014
40,060	Richmond, VA	08/06/2014	48,620	Wichita, KS	08/28/2014
22,744*	Fort Lauderdale-Pompano Beach-Deerfield Beach, FL	08/07/2014	19,780	Des Moines-West Des Moines, IA	09/12/2014
48,424*	West Palm Beach-Boca Raton-Delray Beach, FL	08/07/2014	11,260	Anchorage, AK	09/18/2014
11,700	Asheville, NC	08/21/2014	22,380	Flagstaff, AZ	09/18/2014
15,940	Canton-Massillon, OH	08/22/2014	14,260	Boise City, ID	10/02/2014
14,500	Boulder, CO	08/27/2014	38,860	Portland-South Portland, ME	10/02/2014
22,660	Fort Collins, CO	08/27/2014	49,340	Worcester, MA-CT	10/06/2014
10,420	Akron, OH	08/28/2014	15,540	Burlington-South Burlington, VT	10/09/2014
12,020	Athens-Clarke County, GA	08/28/2014	17,860	Columbia, MO	10/09/2014
12,220	Auburn-Opelika, AL	08/28/2014	24,580	Green Bay, WI	10/16/2014
13,980	Blacksburg-Christiansburg-Radford, VA	08/28/2014	31,700	Manchester-Nashua, NH	10/17/2014
14,020	Bloomington, IN	08/28/2014	16,180	Carson City, NV	10/24/2014
16,820	Charlottesville, VA	08/28/2014	29,820	Las Vegas-Henderson-Paradise, NV	10/24/2014
17,780	College Station-Bryan, TX	08/28/2014	39,900	Reno, NV	10/24/2014
19,380	Dayton, OH	08/28/2014	37,964*	Philadelphia, PA	10/24/2014
22,220	Fayetteville-Springdale-Rogers, AR-MO	08/28/2014	30,780	Little Rock-North Little Rock-Conway, AR	11/06/2014
23,540	Gainesville, FL	08/28/2014	40,220	Roanoke, VA	11/06/2014
28,940	Knoxville, TN	08/28/2014	16,860	Chattanooga, TN-GA	11/13/2014
29,200	Lafayette-West Lafayette, IN	08/28/2014	42,140	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
UberX entry dates 4 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)
47,300	Visalia-Porterville, CA	12/01/2014	40,420	Rockford, IL	02/15/2015
15,980	Cape Coral-Fort Myers, FL	12/04/2014	29,740	Las Cruces, NM	02/18/2015
16,300	Cedar Rapids, IA	12/04/2014	25,940	Hilton Head Island-Bluffton-Beaufort, SC	03/27/2015
18,880	Crestview-Fort Walton Beach-Destin, FL	12/04/2014	29,540	Lancaster, PA	03/27/2015
19,660	Deltona-Daytona Beach-Ormond Beach, FL	12/04/2014	39,740	Reading, PA	03/27/2015
28,580	Key West, FL	12/04/2014	42,340	Savannah, GA	03/27/2015
29,460	Lakeland-Winter Haven, FL	12/04/2014	49,620	York-Hanover, PA	03/27/2015
34,940	Naples-Immokalee-Marco Island, FL	12/04/2014	12,260	Augusta-Richmond County, GA-SC	04/06/2015
35,840	North Port-Sarasota-Bradenton, FL	12/04/2014	21,500	Erie, PA	04/10/2015
36,100	Ocala, FL	12/04/2014	35,380	New Orleans-Metairie, LA	04/16/2015
37,340	Palm Bay-Melbourne-Titusville, FL	12/04/2014	29,940	Lawrence, KS	04/23/2015
37,460	Panama City, FL	12/04/2014	31,740	Manhattan, KS	04/23/2015
37,860	Pensacola-Ferry Pass-Brent, FL	12/04/2014	45,820	Topeka, KS	04/23/2015
38,940	Port St. Lucie, FL	12/04/2014	44,140	Springfield, MA	04/24/2015
27,140	Jackson, MS	12/11/2014	41,540	Salisbury, MD-DE	04/27/2015
27,980	Kahului-Wailuku-Lahaina, HI	12/18/2014	23,060	Fort Wayne, IN	05/07/2015
44,100	Springfield, IL	01/09/2015	23,844*	Gary, IN	05/07/2015
25,420	Harrisburg-Carlisle, PA	01/29/2015	22,020	Fargo, ND-MN	05/12/2015
10,900	Allentown-Bethlehem-Easton, PA-NJ	01/30/2015	39,540	Racine, WI	05/21/2015
29,180	Lafayette, LA	01/30/2015	12,700	Barnstable Town, MA	05/22/2015
42,540	Scranton–Wilkes-Barre–Hazleton, PA	02/06/2015	28,620	Kill Devil Hills, NC	05/22/2015
44,300	State College, PA	02/06/2015	12,300	Augusta-Waterville, ME	05/25/2015
44,660	Stillwater, OK	02/12/2015	17,660	Coeur d'Alene, ID	06/04/2015
14,010	Bloomington, IL	02/15/2015	26,820	Idaho Falls, ID	06/04/2015
16,580	Champaign-Urbana, IL	02/15/2015	38,540	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
UberX entry dates 5 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)
46,300	Twin Falls, ID	06/04/2015	37,900	Peoria, IL	11/24/2015
33,660	Mobile, AL	06/11/2015	22,540	Fond du Lac, WI	11/25/2015
48,864*	Wilmington, DE-MD-NJ	06/11/2015	27,500	Janesville-Beloit, WI	11/25/2015
49,740	Yuma, AZ	06/12/2015	36,780	Oshkosh-Neenah, WI	11/25/2015
28,660	Killeen-Temple, TX	07/02/2015	36,260	Ogden-Clearfield, UT	12/18/2015
43,900	Spartanburg, SC	07/16/2015	45,340	Taos, NM	12/22/2015
19,340	Davenport-Moline-Rock Island, IA-IL	07/21/2015	13,820	Birmingham-Hoover, AL	12/28/2015
20,100	Dover, DE	07/31/2015	24,260	Grand Island, NE	02/01/2016
11,180	Ames, IA	08/03/2015	13,140	Beaumont-Port Arthur, TX	02/03/2016
10,180	Abilene, TX	08/12/2015	33,860	Montgomery, AL	02/04/2016
33,260	Midland, TX	08/12/2015	17,300	Clarksville, TN-KY	03/03/2016
36,220	Odessa, TX	08/12/2015	26,620	Huntsville, AL	03/04/2016
14,540	Bowling Green, KY	08/27/2015	30,340	Lewiston-Auburn, ME	03/21/2016
22,900	Fort Smith, AR-OK	09/01/2015	12,620	Bangor, ME	03/23/2016
39,340	Provo-Orem, UT	09/03/2015	12,740	Barre, VT	03/26/2016
11,540	Appleton, WI	09/10/2015	26,980	Iowa City, IA	04/28/2016
41,180	St. Louis, MO-IL	09/18/2015	31,420	Macon-Bibb County, GA	05/10/2016
14,740	Bremerton-Silverdale, WA	09/30/2015	47,580	Warner Robins, GA	05/10/2016
24,300	Grand Junction, CO	10/06/2015	41,460	Salina, KS	05/21/2016
17,020	Chico, CA	10/08/2015	18,180	Concord, NH	06/01/2016
19,060	Cumberland, MD-WV	10/20/2015	49,660	Youngstown-Warren-Boardman, OH-PA	06/23/2016
25,180	Hagerstown-Martinsburg, MD-WV	10/20/2015	25,060	Gulfport-Biloxi-Pascagoula, MS	07/01/2016
25,500	Harrisonburg, VA	10/23/2015	25,620	Hattiesburg, MS	07/01/2016
15,260	Brunswick, GA	11/06/2015	36,500	Olympia-Tumwater, WA	07/15/2016
13,380	Bellingham, WA	11/11/2015	16,620	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
UberX entry dates 6 of 7.

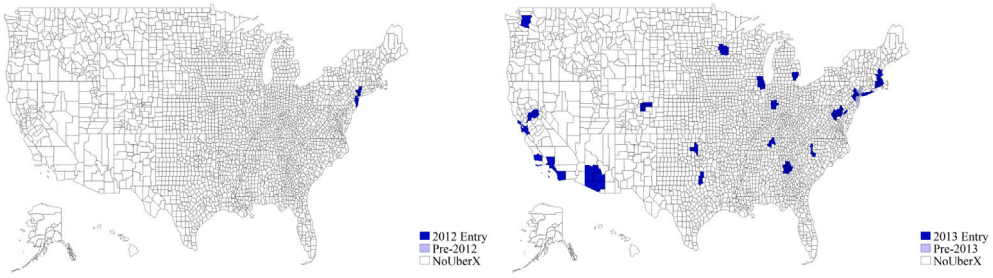
CBSA Area/ Division Code (1)	CBSA Area/Division Title (2)	Date UberX Entered (3)	CBSA Area/ Division Code (1)	CBSA Area/Division Title (2)	Date UberX Entered (3)
26,580	Huntington-Ashland, WV-KY-OH	07/19/2016	16,060	Carbondale-Marion, IL	03/02/2017
28,060	Kalispell, MT	08/01/2016	20,740	Eau Claire, WI	03/02/2017
33,540	Missoula, MT	08/01/2016	24,220	Grand Forks, ND-MN	03/02/2017
24,500	Great Falls, MT	08/02/2016	29,100	La Crosse-Onalaska, WI-MN	03/02/2017
15,580	Butte-Silver Bow, MT	08/03/2016	31,860	Mankato-North Mankato, MN	03/02/2017
25,740	Helena, MT	08/03/2016	40,340	Rochester, MN	03/02/2017
14,580	Bozeman, MT	08/04/2016	40,980	Saginaw, MI	03/02/2017
13,740	Billings, MT	08/05/2016	41,060	St. Cloud, MN	03/02/2017
27,740	Johnson City, TN	08/19/2016	48,140	Wausau, WI	03/02/2017
28,700	Kingsport-Bristol-Bristol, TN-VA	08/19/2016	16,220	Casper, WY	03/03/2017
48,300	Wenatchee, WA	08/19/2016	16,940	Cheyenne, WY	03/03/2017
17,980	Columbus, GA-AL	09/20/2016	28,180	Kapaa, HI	03/10/2017
46,340	Tyler, TX	09/22/2016	17,200	Claremont-Lebanon, NH-VT	03/22/2017
44,180	Springfield, MO	11/17/2016	30,020	Lawton, OK	03/22/2017
25,860	Hickory-Lenoir-Morganton, NC	12/15/2016	20,220	Dubuque, IA	04/01/2017
28,420	Kennewick-Richland, WA	12/15/2016	43,580	Sioux City, IA-NE-SD	04/01/2017
49,420	Yakima, WA	12/16/2016	47,940	Waterloo-Cedar Falls, IA	04/01/2017
14,380	Boone, NC	01/13/2017	20,260	Duluth, MN-WI	05/01/2017
40,860	Rutland, VT	01/20/2017	10,500	Albany, GA	05/17/2017
21,780	Evansville, IN-KY	01/25/2017	46,660	Valdosta, GA	05/17/2017
30,860	Logan, UT-ID	02/01/2017	27,940	Juneau, AK	06/19/2017
45,460	Terre Haute, IN	02/28/2017	21,820	Fairbanks, AK	06/21/2017
25,900	Hilo, HI	03/01/2017	32,580	McAllen-Edinburg-Mission, TX	06/27/2017
27,100	Jackson, MI	03/01/2017	10,580	Albany-Schenectady-Troy, NY	06/29/2017
13,900	Bismarck, ND	03/02/2017	13,780	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.

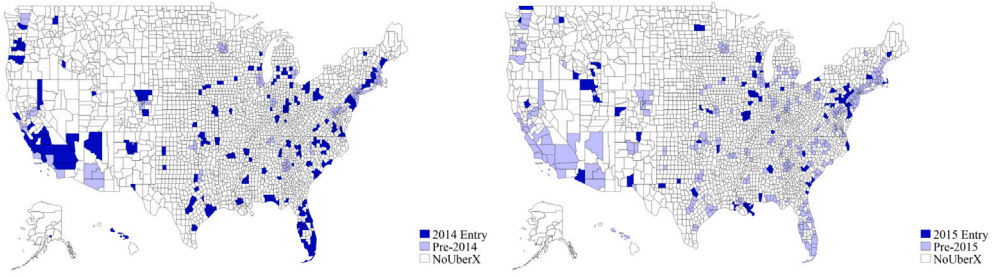
Table D7
UberX entry dates 7 of 7.

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)
15,380	Buffalo-Cheektowaga-Niagara Falls, NY	06/29/2017	43,340	Shreveport-Bossier City, LA	02/15/2018
21,300	Elmira, NY	06/29/2017	10,100	Aberdeen, SD	06/20/2019
24,020	Glens Falls, NY	06/29/2017	39,660	Rapid City, SD	06/20/2019
27,060	Ithaca, NY	06/29/2017	43,620	Sioux Falls, SD	06/20/2019
40,380	Rochester, NY	06/29/2017			
45,060	Syracuse, NY	06/29/2017			
46,540	Utica-Rome, NY	06/29/2017			
29,700	Laredo, TX	07/12/2017			
20,580	Eagle Pass, TX	07/13/2017			
27,860	Jonesboro, AR	08/01/2017			
41,660	San Angelo, TX	08/04/2017			
18,060	Columbus, MS	08/18/2017			
32,940	Meridian, MS	08/18/2017			
44,260	Starkville, MS	08/18/2017			
48,660	Wichita Falls, TX	08/22/2017			
18,700	Corvallis, OR	09/20/2017			
33,740	Monroe, LA	09/20/2017			
35,740	Norfolk, NE	09/23/2017			
45,500	Texarkana, TX-AR	09/26/2017			
34,860	Nacogdoches, TX	09/27/2017			
39,420	Pullman, WA	09/29/2017			
45,900	Traverse City, MI	10/17/2017			
32,780	Medford, OR	12/01/2017			
22,500	Florence, SC	12/08/2017			
13,220	Beckley, WV	12/24/2017			

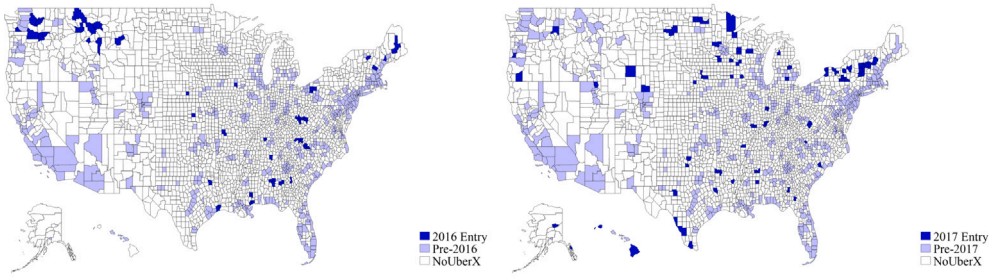
Notes: Wherever UberX entry is specified at the Core Based Statistical Area (CBSA) Division level, these cases are denoted by a star in column (1). Otherwise CBSA codes and titles refer to Core Based Statistical Areas.



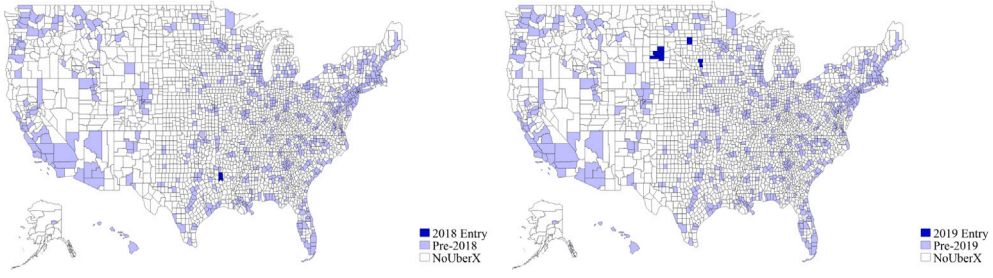
(a) 2012 UberX Entry (b) 2013 UberX Entry



(c) 2014 UberX Entry (d) 2015 UberX Entry

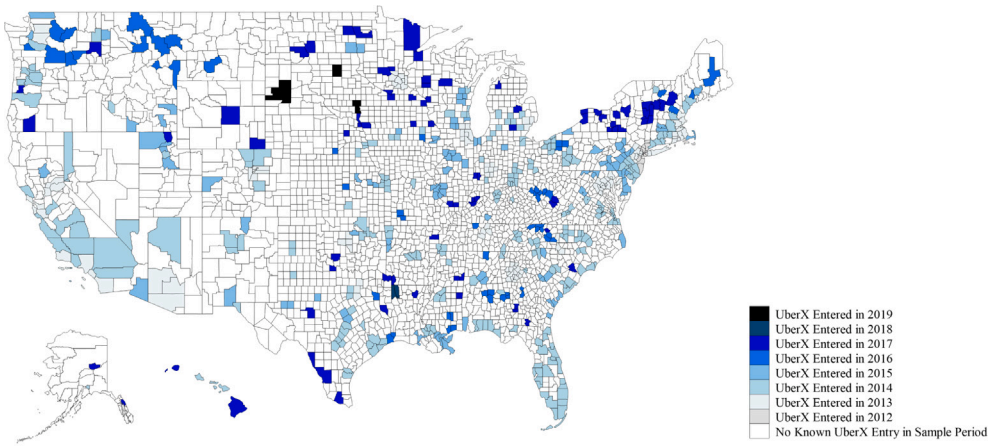


(e) 2016 UberX Entry (f) 2017 UberX Entry

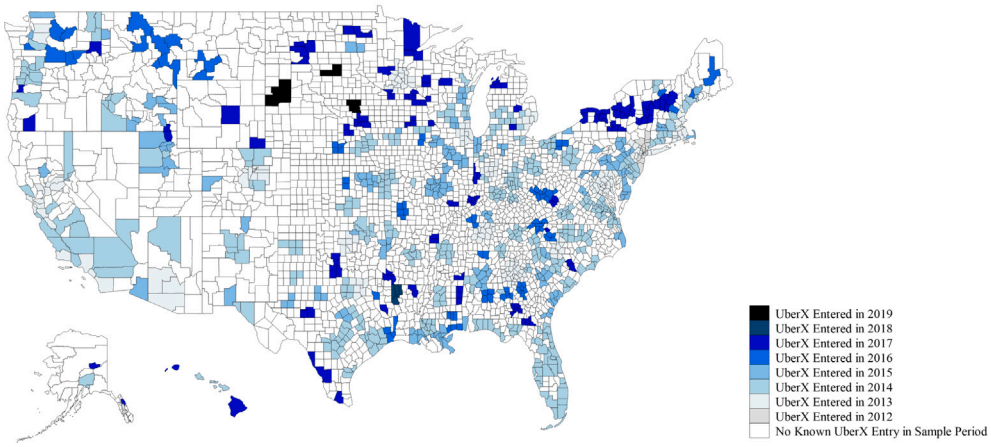


(g) 2018 UberX Entry (h) 2019 UberX Entry

Fig. D1. Entry of UberX by county over Time.



(a) UberX Entry Over Time - Central Counties Only



(b) UberX Entry Over Time - All CBSA Counties

Fig. D2. Entry of UberX by year as indicated - central counties versus all Counties.

Appendix E. Additional information on NVSS mortality data

As we mention in the text, the NVSS Mortality Data is considered to be of high quality. 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 include assigning the 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 Statistics, 2003). Extensive technical guidance is available from the Vital Statistics Cooperative Program (VSCP) and state vital statistics organizations and their personnel.[36]

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 causes 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

[36] 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 the 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 policymakers.

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 the 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 cases where the death is caused by criminal violence, associated with police action, or where the death is by apparent intoxication with alcohol, drugs, or a poison.[37] 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 fall within the categories of death that are most frequently autopsied and for which the cause of death was ascertained from an autopsy, providing further confidence in the accuracy of our data.

Data availability

The authors do not have permission to share data.

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