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Do ridesharing services increase alcohol consumption?☆

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ABSTRACT

Recent studies suggest ridesharing services, such as Uber and Lyft, may reduce instances of intoxicated driving. However, such services may reduce the costs, and thus increase the frequency and intensity, of drinking activity. To examine whether ridesharing affects alcohol consumption, we leverage spatial and temporal variation in the presence of Uber's taxi-like service, UberX, across the United States. Using self-reported measures of alcohol consumption in the past 30 days among individuals aged 21 to 64, we find that UberX is associated with a 3.6% increase in number of drinks per drinking day, a 2.7% increase in drinking days, a 5.4% increase in total drinks, a 4.3% increase in the maximum number of drinks in a single occasion, and a 1.3% increase in those who report drinking any alcohol. For certain groups, such as males, individuals aged 21–34, and students, UberX is associated with even larger increases in drinking. For example, among those aged 21–34, total drinks increase by 7.4% and binge drinking instances increase by 9.5%. We also find that the marginal impact of Uber on drinking is larger in areas that have weaker public transit. Using administrative employment data, we find that some of the additional alcohol consumption is occurring at bars. Specifically, we estimate that UberX is associated with a 3.5% increase in employment and a 3.7% increase in total earnings among workers at NAICS-designated "drinking places".

1. Introduction

Ridesharing services such as Uber and Lyft reduce transportation costs by allowing individuals to seamlessly transmit or receive ride requests, share location and contact information, and exchange payment using only their smartphones. By allowing riders and drivers to rate each other, these services also incorporate reputation as a mechanism to maintain service quality. For these reasons, many individuals consider ridesharing a more convenient, better quality, and lower cost option than traditional taxi services.

In addition to the private benefits, Uber and Lyft highlight that their services can reduce alcohol-related social harms, including instances of drunk driving. For example, a 2015 report by Uber notes that "[d]runk driving is

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a scourge on our society" and that it "wrecks lives, shatters families and puts communities and innocent bystanders at risk."[1] In the same report, Uber finds that entry into Seattle coincided with a 10% decrease in driving under the influence (DUI) arrests, and that peak Uber use is correlated with bar closing times in Miami, Pittsburgh, and Chicago. Some studies support Uber's claims (Greenwood and Wattal, 2017; Martin-Buck, 2017; Peck, 2017; Dills and Mulholland, 2018), but the literature is not unanimous (Brazil and Kirk, 2016; Barrios et al., 2020a).

Notably, in their examination of the relationship between Uber, drunk driving, and crime, Dills and Mulholland (2018) suggest that "[ridesharing's] ease of use might also increase alcohol consumption and other risky behavior." Jackson and Owens (2011) make a similar argument in their study of D.C. Metro expansions from 1999 to 2003, where they find a 5.4% increase in alcohol-related arrests around bars near stations that experienced expanded late night service. Their findings suggest that, to the extent that safe transportation and alcohol are complementary, demand for alcohol will increase when ridesharing becomes available. A ridesharing "Peltzman effect" may also cause additional alcohol consumption, where individuals compensate for the reduction in risks associated with intoxicated driving by drinking more (Peltzman, 1975).

To examine whether ridesharing is associated with an increase in the frequency and intensity of alcohol consumption, we estimate the impact of UberX presence on survey respondents' reported alcohol consumption in the past 30 days using 2009 to 2017 Behavioral Risk Factor Surveillance System Selected Metropolitan/Micropolitan Area Risk Trends (BRFSS SMART) data. UberX is the company's taxi-like service, which is more prevalent and affordable than their luxury services such as Uber Black.[2] We employ a differences-in-differences empirical strategy that leverages spatial and temporal variation in the presence of UberX across U.S. Metropolitan and Micropolitan Statistical Areas (MMSAs), where we define our treatment variable as the proportion of the year that UberX is present in a respondent's MMSA.[3]

In our preferred specification, we use a differences-in-differences design with two-way fixed effects, individual-level controls, and area-level controls. Focusing on those aged 21 to 64, we find that UberX is associated with a 3.6% increase in the average number of drinks per drinking day, a 2.7% increase in drinking days, and a 5.4% increase in total drinks. A 5.4% increase in the total number of drinks per month corresponds to more than 580,000 additional drinks per MMSA-month.[4] We also find a 4.3% increase in the maximum number of drinks in a single drinking occasion and a 0.8 percentage point (1.33%) increase in the number of people who report any alcohol consumption over the previous 30 days. Finally, we find that UberX is associated with a 5.1% increase in binge drinking instances, though this is not statistically significant at conventional levels ($p = 0.17$).[5]

Looking at particular groups of respondents, our estimates suggest that Uber's impact is larger among individuals aged 21–34, including a 7.4% increase in total drinks, a 9.5% increase in binge drinking instances, and a 1.5 percentage point (2.3%) increase in the number who report any alcohol consumption in the past 30 days. Further, we find that UberX does not appear to increase drinking among those aged 65 or older. The pattern of findings by age supports a causal interpretation of our central differences-in-differences estimates for two reasons. First, our summary statistics show that younger individuals drink more, implying that their alcohol consumption may be more constrained by the absence of safe transit options. Second, accessing ridesharing requires a smartphone, and younger individuals have adopted and adapted to smartphone technology more quickly.[6]

Next, we examine how Uber's impact relates to the quality of existing transit options. In particular, Hall et al. (2018) show that Uber substitutes for public transportation in areas with stronger transit systems, suggesting that we might expect Uber to have a smaller impact on drinking in such areas. To test this idea, we group areas into quartiles according to their AllTransit Transit Connectivity Index (TCI) values and then estimate the effects of UberX on alcohol consumption by TCI quartile.[7] Indeed, we find that the effects on drinking are larger in areas where transit is weaker, which reinforces the idea that Uber is facilitating away-from-home alcohol consumption.

To examine whether any of the additional alcohol consumption occurs in bars when UberX is present, we use administrative data on employment and earnings from the Quarterly Census of Employment and Wages (QCEW) for NAICS-designated drinking places (NAICS 7224–10).[8] Focusing on earnings and employment at drinking places is useful because greater spending drives increases in labor demand and earnings via proportional tips. Our estimates imply that Uber is associated with a 3.5% increase in per-capita employment and a 3.7% increase in per-capita total employee earnings at drinking places. To the extent that individuals are less likely to engage in heavy drinking at restaurants, even if safe and reliable transportation is

[1] See https://newsroom.uber.com/wp-content/uploads/2015/01/UberMADD-Report.pdf.

[2] Note that we use the terms Uber and UberX interchangeably throughout the remainder of the paper.

[3] We must use fraction of year treated because, unlike in the regular BRFSS with state identifiers, information on the month of a respondent's interview is absent from BRFSS SMART after 2012.

[4] The baseline average number of drinks per month for those aged 21 to 64 is 12.94 (see Table 1). Our estimates suggest that UberX increases total drinks per month by 5.4%. The average population in 2017 among metros in our sample is 1.39 million. Last, approximately 60% of the U.S. population is aged 21 to 64. Thus, we can calculate 5.4% more drinks per month $\times$ 12.94 drinks on average $\times$ 1.39 million population $\times$ 60% between ages 21–64 = 582,765.

[5] The Centers for Disease Control defines binge drinking as drinking five or more drinks in a single occasion for men or four or more drinks in a single occasion for women. See https://www.cdc.gov/alcohol/data-stats.htm.

[6] For example, survey data from 2015 suggest only 27% of those aged 65+ owned a smartphone. Among those aged 18–29, 85% owned a smartphone. See www.pewresearch.org/internet/2015/04/01/chapter-one-a-portrait-of-smartphone-ownership/ for more.

[7] See https://www.alltransit.cnt.org/.

[8] NAICS is the common acronym for the North American Industry Classification System.

available, we estimate employment and earnings effects at full-service restaurants (NAICS 7225-11) as a quasi-placebo analysis.[9] As expected, we find a smaller 1.2% increase in employment and no statistically significant effect on total employee earnings at full-service restaurants. In contrast, if our main findings were driven by unobserved shocks to disposable income such that general demand for leisure and entertainment increases, we would expect to see comparable increases in earnings and employment at both bars and restaurants.

Formally, our BRFSS and QCEW estimates can be interpreted as causal as long as any omitted idiosyncratic shocks are not correlated with both Uber's presence and our measures of drinking activity. We examine the validity of our identifying assumption and the robustness of our findings in several distinct ways. First, while our main estimation sample consists only of ever-treated areas, we also show that including never-treated areas yields almost identical estimates. Second, using an event study approach, we find a lack of differential pre-trends in the outcomes of interest leading up to UberX entry. Third, we show that trends in drinking in the pre-UberX years (2009 to 2012) do not predict eventual UberX entry timing. Fourth, we show that our central estimates change very little when controlling for time-varying location-level demographics and location-specific linear time trends.

Finally, we examine whether Uber affects self-reported health outcomes among BRFSS respondents, including general health, mental health, and smoking. While we find little to no evidence that UberX affects these outcomes, there is perhaps some suggestive evidence of negative mental health effects. That said, we note that all of our health outcome estimates should be interpreted with caution, as increased alcohol consumption is not the only mechanism through which UberX may affect health. For example, Moskatel and Slusky (2019) find that UberX is a substitute for ambulance rides, illustrating that ridesharing might improve access to health services. For these reasons, we present our analyses of the self-reported health effects of Uber in Appendix B.

Overall, our paper makes three key contributions. First, we provide direct evidence on the relationship between ridesharing and alcohol consumption, which complements concurrent work by Zhou (2020) on the same topic.[10] Second, to the extent that ridesharing may induce additional alcohol consumption among both riders and non-riders, we show in Section 2 that our findings may help explain the mixed evidence on the relationship between ridesharing and drunk driving as highlighted by Barrios et al. (2020a). Third, our work documents the existence of a new quasi-experimental setting that researchers may be able to leverage to study the individual and social impacts of increased alcohol consumption.

The remainder of the paper proceeds as follows. In Section 2, we expand on the relationships between ridesharing, alcohol consumption, and social harms. In Section 3, we describe our data. In Section 4, we develop our approach to estimation and identification. In Section 5, we present our main findings, robustness checks, and heterogeneity analyses. In Section 6, we conclude.

2. Ridesharing, alcohol consumption, and harm

Several recent studies examine the effects of ridesharing on a range of outcomes. For example, Cohen et al. (2016) examine how Uber creates consumer surplus and Chen et al. (2019) estimate the value of flexible work for drivers. Other work examines Uber's impact on local economic conditions, including entrepreneurial activity (Burtch et al., 2018; Barrios et al., 2020b) and public transit use (Hall et al., 2018). Much of the literature, however, focuses on the effect of Uber on motor vehicle accidents, fatalities, and arrests relating to intoxicated driving (Brazil and Kirk, 2016; Greenwood and Wattal, 2017; Martin-Buck, 2017; Peck, 2017; Dills and Mulholland, 2018; Barrios et al., 2020a). That literature mostly paints ridesharing as an attractive alternative to driving while inebriated.

For example, using National Highway Traffic Safety Administration (NHTSA) data, Martin-Buck (2017) finds that ridesharing reduces fatal alcohol-related traffic incidents by at least 10%. Using data from the Federal Bureau of Investigation Uniform Crime Reporting program, he also finds reductions in arrests for driving under the influence (DUI), especially in cities where public transit is utilized less. Using data from the California Highway Patrol, Greenwood and Wattal (2017) find that UberX reduces motor vehicle fatalities by 3.6%. Peck (2017) focuses on New York City boroughs and finds that Uber is associated with up to a 35% decrease in the rate of alcohol-related collisions. Dills and Mulholland (2018) find that ridesharing is associated with up to a 1.6% decline in fatal traffic incidents for each additional quarter Uber is available. They also find some evidence of a reduction in DUI arrests.

However, the literature is not unanimous on the relationship between ridesharing and traffic safety. In an earlier study featuring fewer cities and years of NHTSA data, Brazil and Kirk (2016) find that Uber's arrival was not associated with any change in aggregate traffic fatalities, drunk-driving fatalities, or traffic fatalities during weekends and holidays. Barrios et al. (2020a) also examine the effect of ridesharing on overall traffic incidents. Modeling the accident rate as a function of vehicle miles traveled and driver quality, they find a surprising 0.5% to 1.5% *increase* in alcohol-related accidents and fatalities following the introduction of ridesharing. Barrios et al. explain that a change in the classification of alcohol-related incidents in 2008 is driving the difference in findings between their work and those of earlier papers using the same data.

One potential explanation for an increase in alcohol-related accidents is that, due to the network effects inherent to social drinking, any expansion in safe transportation options may induce additional drinking among

[9] Major employers in the full-service restaurant category include places like Olive Garden and Applebee's.

[10] Indeed, our papers obtain similar estimates of the effect of UberX on self-reported drinking using the BRFSS, though we document a significant increase in drinking days while Zhou (2020) does not. Note that we include an additional year of UberX introductions and data, while additionally including a richer set of robustness tests and heterogeneity analyses, an examination of potential health effects, and evidence of employment and earnings effects at drinking places.

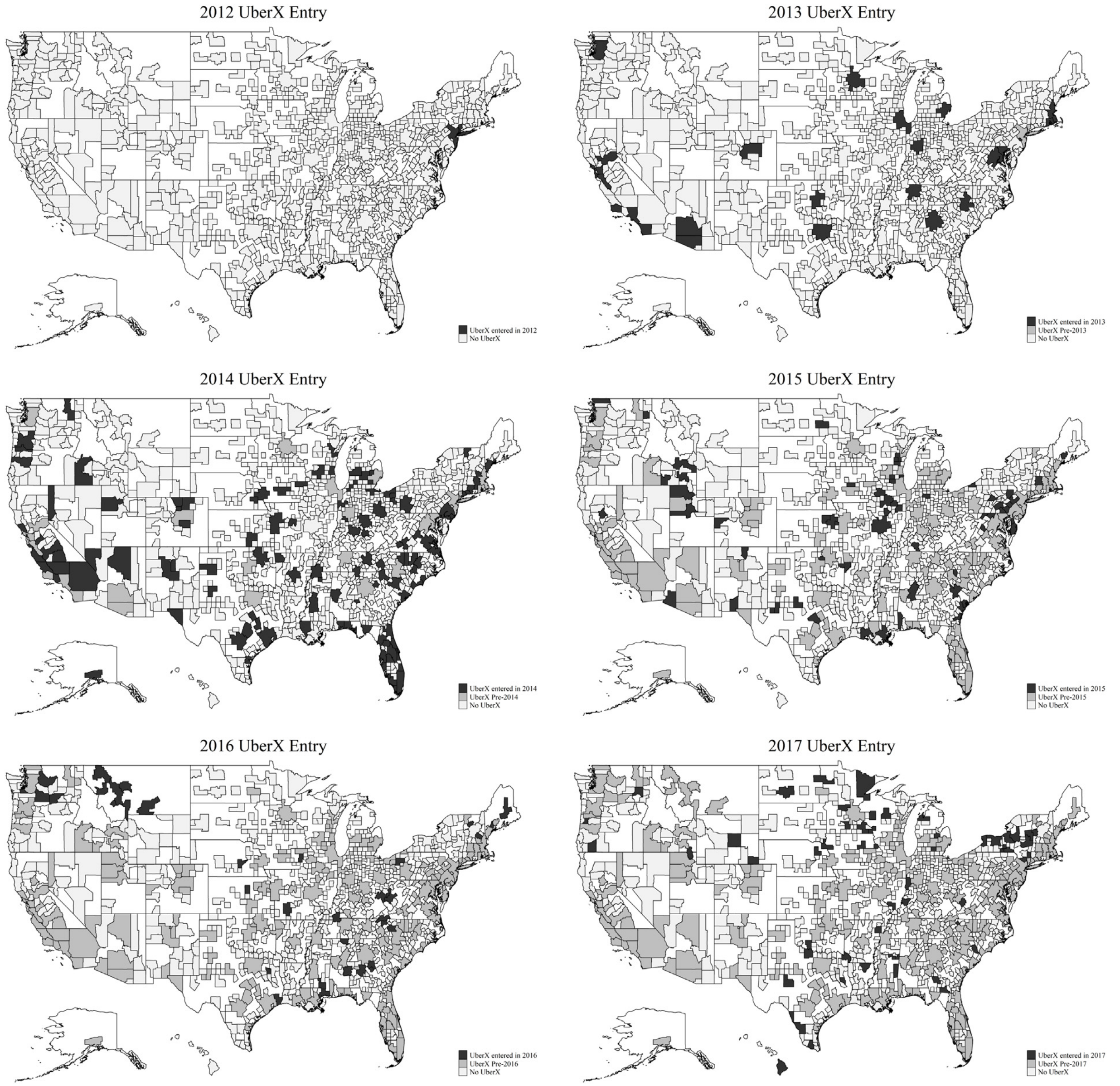


Fig. 1. Uber Geographic Expansion by Year. Notes: Each map presents the continental United States along with Hawaii and Alaska (not to scale). We also plot the outline of all states and each CBSA area. Each sub-figure presents information for a particular year, we color areas where Uber entered that area in that year dark gray, areas that do not have Uber light gray, and areas that already have Uber in a medium gray. Note that, even though there are a handful of CBSAs where there is variation in Uber entry timing across CBSA divisions within a CBSA, we do not plot CBSA divisions separately on the map. Such variation would be difficult to see on a map at this scale. Instead, we plot only the first Uber entry within a CBSA for each CBSA. For completeness, we provide a table of all Uber entry dates by MMSA (how BRFSS refers to CBSAs and CBSA divisions) in Tables C1–C5.

both riders and non-riders. Jackson and Owens (2011) formalize such an argument by developing a model of drinking and transportation as inputs into a “night out.” All else equal, they show that an expansion in transportation options will tend to increase the quantity of nights out, and thus increase alcohol consumption, for both transit riders and non-riders. The intuition behind non-riders going out more is that the utility from a night out is increasing in the number of other people who are also socializing.

In Appendix A, we adapt Jackson and Owens’ model to the context of ridesharing to formally establish the same result. Riders substituting away from driving reduces instances of drunk driving, while an increase in nights out among (driving) non-riders increases instances of drunk driving. We then extend Jackson and Owens’ model to show that expanded transportation options could increase the quantity of alcohol consumed on a night out. The model’s predictions may help to explain why some studies, such as Brazil and Kirk (2016) and Barrios et al. (2020a), find non-negative effects of ridesharing on alcohol-related accidents and fatalities. As an additional consideration not included in the model, there exists the possibility that consuming alcohol could undermine one's own earlier plan to use safe transportation options. For example, some individuals may drive to consume alcohol away from their home and plan on using a ridesharing service to return home. After becoming inebriated, however, they may ultimately decide to drive home instead. Without ridesharing, such individuals may have simply stayed home.

Beyond explaining why the net effect of ridesharing on drunk driving may be theoretically ambiguous, understanding how ridesharing affects drinking activity helps us better understand the broader potential for unintended consequences associated with the introduction of ridesharing. In particular, a large literature shows that alcohol consumption can have harmful effects, particularly among younger adults. Early examples include studies that find increased risky sexual activity and child abuse (Chesson et al., 2000; Markowitz and Grossman, 2000; Rees et al., 2001; Sen, 2002; Rashad and Kaestner, 2004). Carpenter (2004) and Carpenter (2005b) examine how a change in alcohol consumption, driven by age-targeted "Zero Tolerance" drunk driving laws, affected suicide rates and risky sexual behavior among U.S. youths.[11] Carpenter and Dobkin (2009) use a regression discontinuity design to show that there are large and immediate increases in drinking at age 21 and "a discrete 9% increase in the mortality rate at age 21." Further, 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 an increase in the rate at which young men experience self-injury, assault, and alcohol poisoning. Similarly, Marcus and Siedler (2015) find that a ban on late-night alcohol sales in Germany reduced "alcohol-related hospitalizations among adolescents and young adults by about 7%." Finally, Fertig and Watson (2009), Barreca and Page (2015), and Nilsson (2017) examine how increased access to alcohol can have lasting negative effects on children via prenatal exposure.

3. Data

We obtain UberX entry and exit dates through mid-2017 from Hall et al. (2018), who explain that "[e]ntry and exit were determined based on newspaper articles as well as Uber's press releases, blog posts, and social media posts."[12] We implement the same approach to determine UberX entry and exit dates through the end of 2017. In Fig. 1, we map UberX's expansion across U.S. metro areas from 2012 to 2017. In Appendix Tables C1–C7, we present UberX entry and exit dates (when appropriate) for all statistical areas that appear in our BRFSS SMART and QCEW data. We follow Hall and coauthors in that we do not account for the entry of Lyft, Uber's main ridesharing competitor. While Lyft's share of the market has grown over time, it was below 20% until 2017.[13] Moreover, Lyft entry typically lagged Uber entry. For example, Uber launched in New York City in August 2012 and operated in 144 MMSAs by the end of 2014, whereas Lyft launched in New York City in late July 2014 and operated in only 65 MMSAs by the end of 2014. Though we are not aware of any cases where Lyft was the first ridesharing entrant, failing to account for such cases would work against us finding an effect of ridesharing on alcohol consumption.

We then combine information on the presence of UberX with data on self-reported alcohol consumption from BRFSS SMART spanning 2009 to 2017. In addition, we indirectly measure drinking at alcohol-serving establishments using data on employment and workers' earnings at bars and restaurants from the Quarterly Census of Employment and Wages (QCEW). In the following subsection, we describe and summarize the BRFSS SMART and QCEW data.

Finally, motivated by Hall and coauthors' study on the relationship between ridesharing and public transit use, we supplement our UberX entry and exit data with a metro-level measure of transit use and quality. Of particular relevance to our work, Hall et al.'s findings suggest that Uber is a substitute for existing transit in cities with better public transit options. Thus, Uber's impact on alcohol consumption may be attenuated in such cities. To examine this question further in Section 5, we incorporate AllTransit Transit Connectivity Index (TCI) values as our measure of transit use and quality.[14] These values are available for all 225 MMSAs that have UberX by the end of 2017, and are derived from underlying Census block group index values.[15] We present a map of metro areas and associated TCI values in Fig. C3.

3.1. BRFSS and QCEW data

The Centers for Disease Control and Prevention explains that the "Behavioral Risk Factor Surveillance System (BRFSS) is the nation's premier system of health surveys that collect state data about U.S. residents regarding their health-related risk behaviors and events, chronic health conditions, and use of preventive services." BRFSS collects survey responses for 400,000 individuals aged 18 and older across all 50 states, plus the District of Columbia and U.S. territories, every year.[16] The BRFSS Selected

[11] Carpenter (2005a, 2007) uses the same identifying variation to examine alcohol's effect on nuisance crimes such as vandalism, public drunkenness, and disorderly conduct, and finds that the adoption of the stricter laws reduced the fraction of alcohol-related crime arrests attributable to 18 to 20-year-olds by between 3% and 5%.

[12] The replication files for Hall et al. (2018) are available here: http://individual.utoronto.ca/jhall/. Last accessed October 20th, 2019.

[13] See https://qz.com/1563536/how-lyft-stacks-up-against-uber/.

[14] We downloaded the latest AllTransit TCI values from https://alltransit.cnt.org/rankings/ on February 21, 2020. Unfortunately, historical TCI values (e.g., pre-UberX) are not available for download.

[15] That is, the block group with the best access to transit is assigned an index value of 100 and any block group with no transit access is assigned 0. Index values for a census block are based upon the fraction of land area covered by a series of one-eighth mile buffers, such that Census blocks with more area covered by the first one-eighth mile buffer around a transit route will have higher TCI values. In their rankings, AllTransit also considers the frequency of transit service, the percent of people using transit, and assigns weights to each eighth-mile buffer using location and demographic information. See https://alltransit.cnt.org/methods/AllTransit-Methods.pdf for more on AllTransit's TCI methodology.

[16] See https://www.cdc.gov/brfss/factsheets/pdf/DBS_BRFSS-SMART-BRFSS_12.pdf.

Metropolitan/Micropolitan Area Risk Trends (SMART) files are particularly useful for our study because they provide more granular geographic identifiers (MMSA-level) than the regular BRFSS files (state-level). The MMSA designation "refers to metropolitan statistical areas, micropolitan statistical areas, and metropolitan divisions."[17] Data for a particular MMSA-year are available in BRFSS SMART when more than 500 interviews are collected in that MMSA, which does not occur every year for every MMSA.[18] In Section 5, we show that our central findings are robust to further restricting our estimation sample to MMSAs with data sufficiency for the full 9-year sample period. Notably, unlike in the regular BRFSS, the Centers for Disease Control (CDC) does not report timing information (neither file nor interview month) in BRFSS SMART after 2012.[19] Thus, we cannot observe whether any particular respondent was affected by UberX in years where UberX is present for only part of the year. To account for this in our BRFSS analyses, we define our treatment variable as the fraction of days UberX is present in a year.

In Table 1, we report summary statistics for BRFSS SMART respondents aged 21 to 64 in areas where UberX eventually becomes available in the sample period. Due to challenges presented by data suppression and treatment status ambiguity, we exclude seemingly-untreated MMSAs from our main analyses (32 out of 257 total, see Table C5).[20] That said, in Section 5 we show that including these 32 MMSAs has virtually no impact on our central estimates. Restricting to individuals aged 21 to 64 allows us focus on the group of legal drinking age adults where (a) social drinking activity is most likely to be constrained by a lack of safe transportation alternatives and (b) smartphone technology adoption is highest.[21] In Section 5, we present the analogous analyses for those aged 18–20 as well as those aged 65+.[22]

The key variables of interest include, over the past 30 days, whether the individual has drank any alcohol, how many days the respondent has consumed alcohol, the average number of drinks consumed on a drinking day, total number of drinks (= drinking days $\times$ avg. drinks per drinking day), maximum number of drinks consumed on a single occasion, and instances of binge drinking (defined by CDC BRFSS as $\geq$4 drinks in a single occasion for women, $\geq$5 for men). Our summary statistics include measures of alcohol consumption along with demographic information on income, race, age, gender, marital status, student status, and employment status. In addition to sample mean and standard deviation, we present the 25th, 50th, 75th, and 95th percentile values for each non-binary outcome and control variable to provide greater insight on the distributions of these measures. In our main estimation sample, due to concerns about data reliability, we treat a small number of responses where individuals report unrealistically extreme daily alcohol consumption as missing responses. This includes respondents who report drinking more than 20 drinks on each drinking-day, engaging in more than 30 instances of binge drinking in the past 30 days, or reporting greater than 20 drinks in a single drinking occasion.[23] In Section 5, we show that restoring these as valid responses does not meaningfully affect our central estimates.

Because treatment varies by MMSA, we also incorporate MMSA-level demographic information as controls in our analyses. We provide the relevant summary information in Table 1, including MMSA-level median age, household income, % white, % male, and % aged 20–44 for the metro area, which we obtain from the American Community Survey (ACS).

In Table 1, we also present per-capita employment, per-capita earnings, and average weekly earnings data (per 1000 population) from the Quarterly Census of Employment and Wages (QCEW) at the county-quarter level for employees in two industries that serve alcohol: "drinking places" and "full-service restaurants" (North American Industry Classification System, or NAICS, codes 7224-10 and 7225-11). The QCEW covers "more than 95% of U.S. jobs... at the county, MSA, state and national levels by industry."[24] The NAICS website explains that NAICS 7224-10 "comprises establishments known as bars, taverns, nightclubs, or drinking places primarily engaged in preparing and serving alcoholic beverages for immediate consumption. These establishments may also provide lim-

[17] A micropolitan or metropolitan statistical area is a group of one or more counties that has at least one urban cluster of 10,000–50,000 (micropolitan) or 50,000+ (metropolitan) residents along with "adjacent territory that has a high degree of social and economic integration with the core as measured by commuting ties." A metropolitan division refers to "a smaller group of counties within a metropolitan statistical area of 2.5 million or more inhabitants." MMSAs are a BRFSS term for CBSAs (Core-Based Statistical Areas) and CBSA divisions. See more on CBSAs and MMSAs at https://www.census.gov/topics/housing/housing-patterns/about/core-based-statistical-areas.html and https://www.cdc.gov/brfss/smart/smart_faq.htm.

[18] While BRFSS is a large survey, many of the respondents live outside of a MMSA. Thus, achieving a total of 500 interviews in any given MMSA is not as common as one might expect. We present a map of MMSA areas and BRFSS data availability in Fig. C1.

[19] Also, the CDC only reports file month in 2012, which can be different from the actual interview month.

[20] Due to data suppression, most of the seemingly-untreated MMSAs do not contribute to identification. That is, of the 32 MMSAs in our BRFSS SMART data to which we cannot assign a treatment date, 20 of them only contain one year of survey responses. An additional 4 contain only two years of survey responses. Five more contain three or four years of survey responses. Moreover, in 28 of these 29 MMSAs, the responses are all from 2012 or earlier (i.e., pre-UberX). The lone exception is Berlin, NH-VT, which has responses in 2012, 2014, and 2016. The remaining 3 MMSAs are Minot ND, North Platte NE, and Scottsbluff NE, which have responses for 8–9 years. However, we are unfortunately unable to determine with full confidence that these areas are actually never-treated.

[21] Smartphone ownership is low among those over 65 years of age during our sample period. For example, in the last year of our sample period (2017), only 42% of Americans aged 65 or older owned a smartphone device (https://www.pewresearch.org/fact-tank/2017/01/12/evolution-of-technology/). Moreover, ownership rates remain low (53%) among those aged 65 and older as late as 2019 (https://www.pewresearch.org/internet/fact-sheet/mobile/).

[22] Summary statistics for respondents of all ages are reported in Appendix Tables B1 and B2.

[23] In our sample, 1648 respondents (>99th percentile) report more than 20 drinks on each drinking-day, 104 respondents (>99th percentile) report more than 30 binge drinking instances, and 3237 respondents (>99th percentile) report having a maximum number of drinks in a single occasion greater than 20.

[24] See https://www.bls.gov/cew/.

Table 1
BRFSS, QCEW, and ACS summary statistics for estimation sample.

	Mean	SD	P25	Median	P75	P95
BRFSS						
Any alcohol?	0.59	0.49				
Num. drinking days	4.90	7.53	0	2	6	25
Avg. drinks per drinking day	1.44	2.01	0	1	2	5
Total drinks	12.94	29.86	0	2	13	60
Max drinks single occasion	2.13	2.95	0	1	3	8
Binge drinking instances	0.77	2.88	0	0	0	4
Age	42.60	12.64	32	42	53	63
Male	0.49	0.50				
White	0.69	0.46				
Married	0.56	0.50				
College degree	0.35	0.48				
Employed	0.69	0.46				
Student	0.04	0.19				
HH income ≥35k	0.66	0.48				
Respondents				1,552,600		
MMSA-level controls from ACS and AllTransit						
MMSA population	1,342,345	1,917,640	279,417	686,989	1,683,178	4,478,410
HH income ($)	55,127	11,204	47,351	53,048	60,146	76,864
Median age	37.1	3.5	35.3	37.1	39.1	42.6
% Male	49.1	0.8	48.6	49.0	49.6	50.4
% Aged 20–44	33.9	2.9	32.0	33.7	35.7	38.4
% White	77.4	12.9	69.6	79.9	87.3	94.5
Transit Connectivity Index	2.1	2.6	0.7	1.4	2.4	7.7
QCEW – drinking places – NAICS 7224-10						
Num. employees per 1000 pop.	1.30	1.00	0.66	1.06	1.63	3.39
Qtrly. earnings per 1000 pop. ($)	5387	5388	2577	4078	6641	13,503
Avg. weekly wage ($)	308.41	78.72	256	303	350	432
County-quarter obs.				19,816		
QCEW – full-service restaurants – NAICS 7225-11						
Num. employees per 1000 pop.	16.92	7.37	12.95	16.48	19.62	27.55
Qtrly. earnings per 1000 pop. ($)	80,858	53,197	54,238	73,887	95,263	144,829
Avg. weekly wage ($)	354.94	69.84	305	349	397	474
County population	270,771	548,914	42,291	111,821	280,157	949,050
County-quarter obs.				31,437		

Notes: Our summary statistics refer to BRFSS, QCEW, and ACS data from 2009–2017 only for those MMSAs where UberX eventually becomes available. For BRFSS data, we weight using BRFSS-provided survey weights, restrict the sample to respondents aged 21–64 (as in the main estimation sample). Note that there are missing data for some BRFSS MMSA-years whenever there are an insufficient number of responses to permit public disclosure. For QCEW data, which are county-quarter level aggregates, we weight observations using county population. There are also missing county-time observations in our QCEW data because of data disclosure requirements.

ited food services."[25] The same website explains that NAICS 7225-11 "comprises establishments primarily engaged in providing food services to patrons who order and are served while seated (i.e., waiter/waitress service) and pay after eating. These establishments may provide this type of food service to patrons in combination with selling alcoholic beverages, providing carryout services, or presenting live nontheatrical entertainment." Chain restaurants, including Applebee's, Olive Garden, and Red Lobster, comprise the largest employers in this category.[26]

As with the BRFSS analyses, we exclude counties in never-treated MMSAs from our main analyses. Note that our QCEW samples include more metro areas than the BRFSS SMART data. In particular, there are 306 ever-treated MMSAs in the drinking-place sample and 316 in the full-service restaurant sample. In Section 5, we show that restricting the QCEW estimation samples to only the MMSAs present in the BRFSS SMART data yields very similar point estimates, but with larger standard errors. Note also that, as with the BRFSS SMART, our QCEW panels are unbalanced because of data restrictions. The QCEW suppresses data to protect "the identity, or identifiable information, of cooperating employers."[27] Because of the relative size and scope of the full-service restaurant industry, there are fewer suppressed observations relative to our sample of drinking places. Note that data suppression only biases our central estimates if it is systematically correlated with both Uber and our measures of employment and earnings. In Section 5, we use two methods to show that data suppression does not appear to pose any issues. First, we re-estimate our central specifications using only counties designated as "central" within their respective metro areas. Second, we restrict our sample to only the counties where data sufficiency is met for all 36 quarters across our 9-year sample period.

25 See https://www.naics.com/naics-code-description/?code=722410.
26 See https://www.naics.com/naics-code-description/?code=722511.

27 See https://www.bls.gov/cew/questions-and-answers.htm for more on this issue. In Fig. C2 we present a map of QCEW data availability for drinking places and restaurants by county for the sample period.

Table 2
BRFSS, QCEW, and ACS means – early vs. late intros.

	Early intros (2012–2014)			Late intros (2015+)		
	Full sample Mean	Pre-UberX Mean	Post-UberX Mean	Full sample Mean	Pre-UberX Mean	Post-UberX Mean
BRFSS						
Any alcohol?	0.60	0.60	0.60	0.56	0.56	0.57
Num. drinking days	4.93	4.91	4.95	4.61	4.58	4.73
Avg. drinks per drinking day	1.44	1.44	1.44	1.41	1.41	1.42
Total drinks	12.94	12.73	13.15	12.99	12.87	13.40
Max drinks single occasion	2.13	2.12	2.13	2.09	2.08	2.11
Binge drinking instances	0.77	0.74	0.80	0.82	0.80	0.89
Age	42.56	42.49	42.62	42.97	42.96	43.01
Male	0.49	0.49	0.49	0.49	0.49	0.49
White	0.68	0.71	0.66	0.78	0.79	0.75
Married	0.55	0.58	0.53	0.57	0.58	0.53
College degree	0.36	0.38	0.34	0.30	0.30	0.29
Employed	0.69	0.68	0.70	0.68	0.68	0.69
Student	0.04	0.04	0.04	0.04	0.03	0.04
HH income $\geq$35k	0.66	0.66	0.65	0.64	0.64	0.66
Respondents	1,251,968	672,300	579,668	300,632	245,426	55,206
MMSA-level controls from ACS and AllTransit						
MMSA population	1,833,807	1,602,014	2,138,797	381,265	350,345	535,866
HH income ($)	57,482	54,160	61,853	50,521	49,462	55,818
Median age	37.0	36.8	37.2	37.4	37.3	37.5
% Male	49.1	49.1	49.1	49.3	49.3	49.3
% Aged 20–44	34.5	34.6	34.3	32.8	32.8	33.1
% White	74.4	75.0	73.8	83.1	83.5	81.0
Transit Connectivity Index	2.9	–	–	1.1	–	–
QCEW – drinking places – NAICS 7224-10						
Num. employees per 1000 pop.	1.25	1.24	1.27	1.56	1.56	1.55
Qtrly. earnings per 1000 pop. ($)	5436	4858	6065	5126	4942	5770
Avg. weekly wage ($)	318.84	291.05	349.06	253.09	242.79	289.15
County-quarter obs.	12,512	6913	5599	7304	5806	1498
QCEW – full-service restaurants – NAICS 7225-11						
Num. employees per 1000 pop.	17.09	16.32	17.93	16.07	15.84	16.92
Qtrly. earnings per 1000 pop. ($)	84,150	73,771	95,590	64,625	61,642	75,560
Avg. weekly wage ($)	365.89	338.71	395.84	300.95	292.22	332.92
County population	356,379	331,504	388,320	124,559	120,741	140,769
County-quarter obs.	19,905	11,169	8736	11,532	9320	2212

Notes: Our summary statistics refer to BRFSS, QCEW, and ACS data from 2009–2017 only for those MMSAs where UberX eventually becomes available. For BRFSS data, we weight using BRFSS-provided survey weights and report summary statistics only for respondents aged 21–64 (that is, our main estimation sample). Note that there are missing data for BRFSS SMART MMSA-years whenever there are an insufficient number of responses to permit public disclosure. For QCEW data, which are county-quarter level aggregates, we weight area characteristics using county population. There are missing county-time observations in our QCEW data also because of data disclosure requirements.

In Table 2, we present the same set of summary statistics stratified by early (2012–2014) versus late (2015 or later) UberX entry. Note that there are pronounced differences over time in the types of MMSAs that Uber enters. For example, UberX entered MMSAs with larger populations, higher incomes, and higher bar and restaurant earnings earlier. These summary statistics suggest that Uber's decision to enter an area may be endogenous to alcohol consumption, or at least area-level characteristics associated with alcohol consumption. However, in Appendix Tables B3 and B4, we show that there is no clear pattern of evidence to suggest that treatment is systematically correlated with observable area- and individual-level demographic characteristics. Moreover, in Section 4, we discuss how we deal with such concerns by controlling for individual and area-level demographic controls, time fixed effects, MMSA fixed effects, and MMSA-specific trends. We also show that trends in alcohol consumption from 2009 to 2012 do not predict the timing of an area's eventual UberX entry.

4. Estimation

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

$$Y_{(i)jt} = \alpha \text{Uber}_{jt} + X_{(i)jt}\beta + \delta_j + \gamma_t + \epsilon_{(i)jt}. \quad (1)$$

$Y_{(i)jt}$ refers to the consumption measure of interest for individual i in geographic area j (MMSA for BRFSS, county for QCEW) in time period t.[28] The first set of outcomes of interest are individual-level BRFSS measures of alcohol consumption, including an indicator for any alcohol consumption at all, number of drinking days in the past 30 days, average drinks per drinking day in the past 30 days, total drinks in the past 30 days, maximum drinks in a single

[28] Note, because the QCEW data are county-level aggregates, we drop the i subscript in those specifications.

occasion in the past 30 days, and instances of binge drinking in the past 30 days. Note that, with the exception of the any-consumption indicator, our outcomes of interest are reported as counts in the BRFSS data. Moreover, the data consists of many zeros because more than 40 percent of respondents report no drinking activity in the past 30 days. Thus, for all count outcomes, we estimate Eq. (1) using a Poisson model to obtain estimates of the effect of UberX in terms of percentage changes. We estimate the effect of UberX on the any-consumption indicator via OLS using a linear probability model. To estimate the effects of UberX on employment and worker earnings at alcohol-serving establishments, we estimate Eq. (1) via OLS where Y_{jt} includes logged county-level measures of employment per 1000 population, total earnings per 1000 population, and average weekly earnings from the QCEW.

For the yearly BRFSS data, our treatment variable, Uber_{jt}, indicates the fraction of days in year t UberX is available in MMSA j. When using the QCEW data, the Uber_{jt} term indicates the fraction of days in quarter t that UberX is available in county j. Depending on specification, we also include controls for aggregate-level demographics and location-specific linear time trends, captured by the term $X_{(i)jt}$. All specifications include geographic unit fixed effects, δ_j, time period fixed effects, γ_t, and an idiosyncratic error term, $\epsilon_{(i)jt}$. In our BRFSS analyses, we are also able to include individual-level controls for respondents' age group, gender, race, marital status, education level, work status, and household income level. Within such a setup, as long as there are not omitted idiosyncratic shocks correlated with both Uber's presence and alcohol consumption, α represents the causal effect of Uber's introduction on the outcome of interest, $Y_{(i)jt}$. In all our analyses, we report standard errors that are robust to clustering at the MMSA level. In our BRFSS analyses, we weight observations by their BRFSS-provided survey weights. In our QCEW analyses, we weight observations by the corresponding county population.

4.1. Credibility of differences-in-differences design

To lend credibility to our empirical strategy, we first test for the presence of differential pre-trends in the outcomes of interest between treated and yet-to-be-treated jurisdictions, which would threaten our ability to interpret our differences-in-differences estimates as causal parameters. To this end, we estimate a time-disaggregated version of the differences-in-differences approach specified in Eq. (1) following Jacobson et al. (1993) and Goodman-Bacon and Cunningham (2019). The specification is as follows:

$$Y_{(i)jt} = \sum_{k=-l}^{m} \alpha_k 1(t - T_j = k) + X_{(i)jt}\beta + \delta_j + \gamma_t + \epsilon_{(i)jt}. \qquad (2)$$

With the exception of the any-drinking indicator, where we use OLS estimation, we estimate all BRFSS specifications using a Poisson model. We estimate all QCEW specifications via OLS using logged outcome variables. Thus, the key difference from Eq. (1) is that we replace the treatment variable for Uber's presence in an area with a set of indicators $1(t - T_j = k)$.[29] This indicator term equals 1 when the observation's time period t is k periods relative to location j's UberX entry period T_j, which is defined as the first quarter (QCEW) or year (BRFSS) where UberX was present for at least 50% of the period.[30] For the event studies only, we exclude MMSAs where Uber exited at any time following their initial entry, as well as those where UberX entered after June 30, 2017 (or after the fourth quarter of 2017 for the QCEW outcomes).[31] The α_k terms associated with these indicators capture the effect of Uber's presence on the outcome of interest, $Y_{(i)jt}$, in each time period leading up to and following Uber entry. We assign period $k = m$ to observations where $t \geq m$, and assign $k = -l$ to observations where $t \leq -l$. All specifications include MMSA-level controls, individual controls (BRFSS only), time fixed effects, and location fixed effects. Statistical inference relies on MMSA-level cluster-robust standard errors. We specify the baseline period to be $k = -1$.

In Fig. 2, we present event study plots for our BRFSS alcohol consumption measures. Across all six BRFSS outcomes, there is almost no evidence of any differential trends in the outcomes of interest leading up to UberX introduction. The lone exception is perhaps maximum number of drinks in a single occasion, where there seems to be a slight positive pre-trend leading up to UberX entry. These findings substantially mitigate the concern that increasing alcohol consumption is driving the timing of UberX entry. Providing our first evidence that UberX is associated with increases in alcohol consumption, these plots also demonstrate discrete increases in drinking in the first year where UberX is present for at least 50% of the year. While these increases seem to persist over time for average drinks per drinking day, max drinks on one occasion, total drinks, and binge drinking, and (to a lesser, statistically insignificant, extent) increases in drinking days, the increase in the proportion of individuals reporting that they drank anything at all in the past 30 days does not appear to persist past the first year.

We present the corresponding event study plots for our QCEW outcomes of interest in Fig. 3. In all specifications, we find little to no evidence of differential pre-trends based on timing of UberX entry. The pattern of estimates suggest that UberX is associated with statistically significant increases in total earnings and employment among employees of NAICS-designated drinking places, though they show no effect on employees' average weekly earnings. The increase in total earnings, therefore, appears to be driven by an increase in drinking-place employment rather

[29] Note that this indicator term is not interacted with a treatment dummy. This is because our main estimation sample includes only MMSAs that ever experience UberX entry. Note also that the key parameters of interest, α_k, are still identified by virtue of collapsing observations where $t > m$ into period $k = m$ and those where $t < -l$ into period $k = -l$ (Sun and Abraham, 2020).

[30] In the event study framework, we cannot use a continuous treatment variable to reflect partial-period treatment as we do in our standard differences-in-differences approach. Thus, we deal with partial-period treatment by imposing a cutoff of ≥50% treated to determine a location's period of UberX entry T_j.

[31] See tables in Appendix C to see the MMSAs that fall into these two categories.

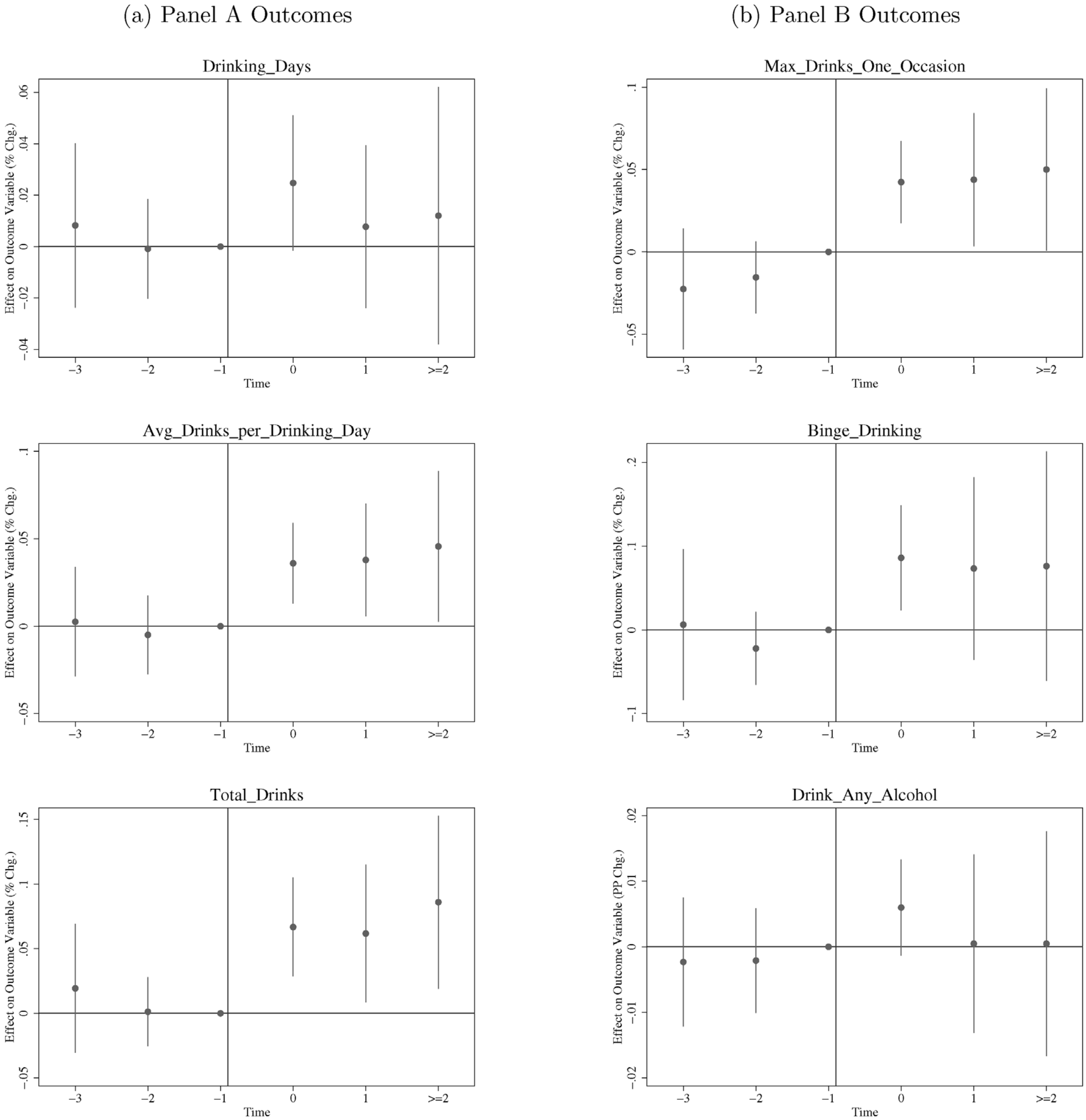


Fig. 2. Event studies – BRFSS data. Notes: Our estimation sample consists of BRFSS SMART respondents aged 21 to 64 in ever-treated MMSAs. We estimate all specifications using a Poisson model, except in the case of the "Any Alcohol?" indicator, where we estimate a linear probability model via OLS. In all specifications, we weight observations using BRFSS-provided survey weights, and include MMSA fixed effects, year fixed effects, and individual and MMSA-level covariates. We include an indicator for $t \leq -4$ in all specifications but do not plot it here. We plot 95% confidence intervals with each point estimate. Standard errors are robust to clustering at the MMSA level.

than by an increase in earnings among existing employees. As hypothesized, the event studies illustrate that the effects on employees of NAICS-designated full-service restaurants are considerably smaller or nonexistent. In particular, we see only a slight increase in employment, no effect on total earnings, and a mild negative effect on employees' average weekly earnings.

In Figs. B1 and B2 in Appendix B, we present the analogous event study figures obtained when we also include location-specific linear time trends in the set of controls. There, we also find little to no evidence of differential pre-trends across both sets of event studies. In the BRFSS event studies we do find that (1) increases in self-reported drinking are larger across the board, and (2) the increase in drinking days persists past the first year. There appear to be no key differences across the two sets of QCEW event studies.

We further examine the relationship between UberX entry timing and drinking activity by estimating whether drinking from 2009 to 2012 predicts an MMSA's even-

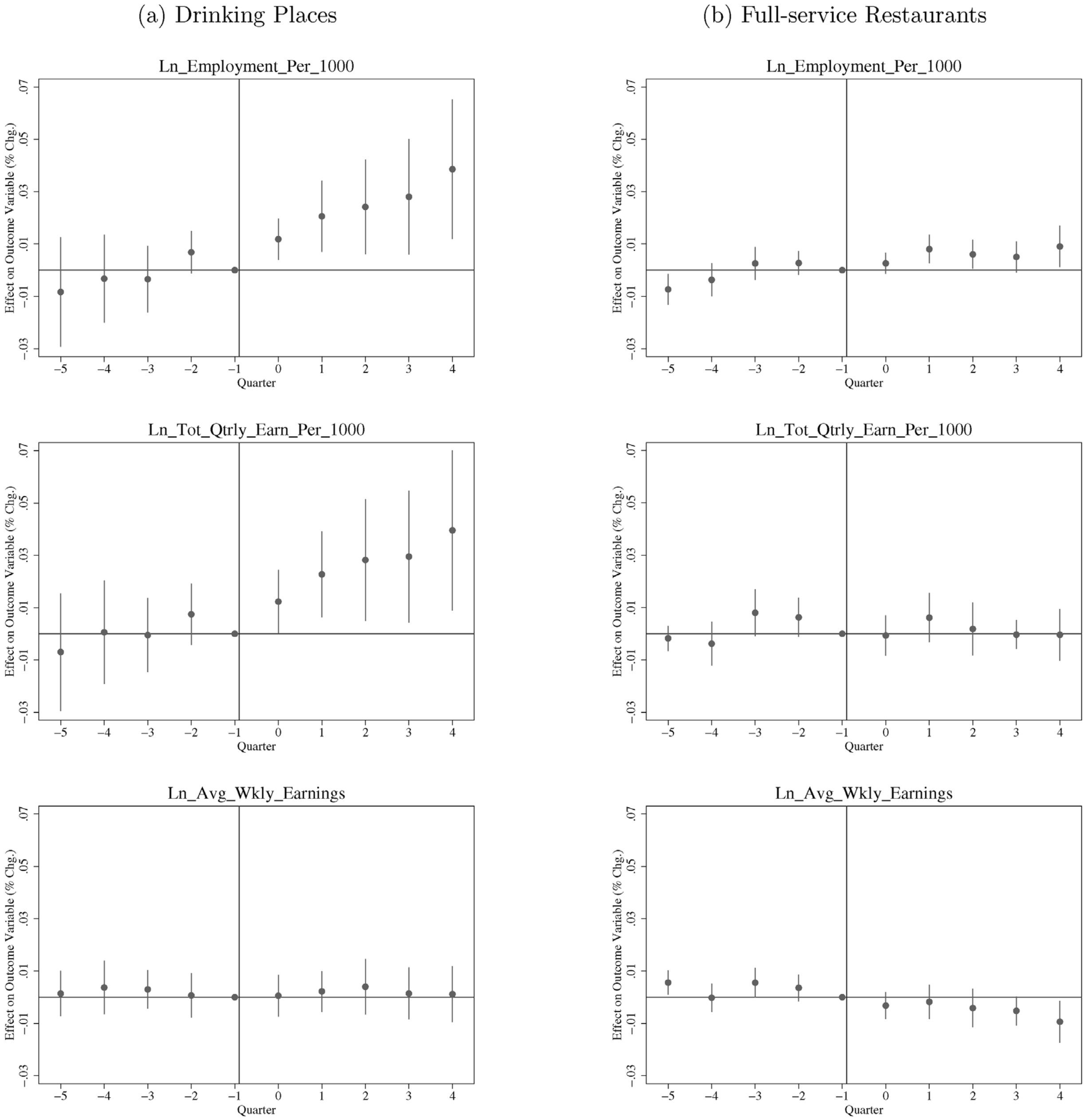


Fig. 3. Event studies – QCEW data. Notes: Our estimation sample consists of all ever-treated MMSAs in the QCEW data. Dependent variables are in logs and observations are at the county-quarter-year level. We estimate all specifications using OLS, weighting observations using county population. We include county fixed effects, quarter-year fixed effects, and county-level covariates in all specifications. We include indicators for $t \leq -6$ and $t \geq 5$ in all specifications but do not plot them here. Standard errors are robust to clustering at the MMSA level. We plot 95% confidence intervals with each point estimate.

tual UberX entry date. In these analyses, we regress the month-year of UberX entry on each of our BRFSS and QCEW outcomes of interest, using both area-based average levels and percentage changes over time. We control for the same set of MMSA-level covariates and (averaged) individual-level covariates (BRFSS only), as well as time period fixed effects. Note that we cannot use location fixed effects, nor location-specific trends, because UberX entry date is time-invariant within each MMSA. We present these estimates in Panel A of Table 3, along with the means of each BRFSS right-hand side variable in brackets, where we find that only a higher number of reported drinking days predicts an earlier UberX entry. Specifically, the −1.365 estimate in column (2) suggests that for each additional drinking day per month, Uber entered that area 1.365 months (or approximately 5.5 weeks) earlier.

Importantly, however, our main approach estimates percentage changes in alcohol consumption while accounting for differences in average consumption levels across areas by controlling for area-specific fixed effects. Thus, the

Table 3
Do pre-UberX outcomes predict UberX entry timing?

Panel A: BRFSS variables (RHS)	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: UberX entry month-year					
	Drinking days	Avg. drinks per D-day	Total drinks	Max drinks one occasion	Binge drinking instances	Any alcohol?
BRFSS var. coefficient (avg. levels, 2009–12)	−1.365**	−0.934	−0.260	−0.348	−0.767	−8.552
	(0.647)	(2.617)	(0.194)	(1.685)	(2.286)	(8.110)
Mean of RHS var.	[4.92]	[1.41]	[12.97]	[2.11]	[0.76]	[0.58]
BRFSS var. coefficient (% changes, 2009–12)	−0.104	−1.820	−0.752	−3.252	−0.959	−5.075
	(1.497)	(1.989)	(1.081)	(2.070)	(0.804)	(3.414)
Mean of RHS var.	[0.043]	[0.041]	[0.086]	[0.040]	[0.134]	[0.015]
Panel B: QCEW variables (RHS)	Drinking places			Full-Service Restaurants		
	Employment per 1000	Total earnings per 1000	Avg. weekly earnings	Employment per 1000	Total earnings per 1000	Avg. weekly earnings
QCEW var. coefficient (levels, 2009–12)	0.679	−0.0000003	−0.0322**	−0.358***	−0.00007***	−0.0234
	(0.675)	(0.000197)	(0.0136)	(0.0746)	(0.00001)	(0.0178)
Mean of RHS var.	[1.61]	[5106]	[247.6]	[17.09]	[66,732]	[292.1]
QCEW var. coefficient (% changes, 2009–12)	−0.400	0.0238	0.944	−2.888	−2.603	1.314
	(1.214)	(1.044)	(1.397)	(1.912)	(1.691)	(1.797)
Mean of RHS var.	[0.008]	[0.021]	[0.012]	[0.008]	[0.019]	[0.010]

Notes: These estimates are based on our main estimation sample, further restricted to only pre-UberX years (2009–2012). Each coefficient is from a separate OLS regression of initial UberX entry month-year on the drinking measure of interest, controlling for the usual covariates and time fixed effects. Note that we cannot use MMSA fixed effects, nor MMSA-specific trends, because a MMSA's initial UberX entry date does not vary across time. For interpretation, a negative coefficient indicates an association between earlier UberX entry and the noted independent variable. For example, the −1.365 estimate in column (1) suggests that an area with one additional drinking day per month (relative to the average baseline) experienced UberX entry 1.365 months earlier (≈ 5.5 weeks). Standard errors are robust to clustering at MMSA level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

more relevant relationship to test is the one between entry timing and pre-UberX percentage changes in self-reported consumption for each year from 2009 through 2012. We present these estimates, along with the mean percentage change for each variable, in the second row of Panel A. For example, we find that a 1 percentage point higher pre-Uber drinking-day growth rate is statistically insignificantly associated with a 0.1 month (or 3 day) earlier UberX entry date. Indeed, across the board, we find no significant relationship between percentage changes in drinking from 2009 to 2012 and eventual entry date of UberX.

For completeness, we also examine the analogous relationships for our set of QCEW outcomes (in Panel B of Table 3). When using levels, we find that higher average weekly earnings among drinking place employees, higher employment at full-service restaurants, and higher total earnings among full-service restaurant employees appear to predict earlier UberX entry. As with our BRFSS analyses, however, we find no evidence that differential percentage changes in employment or earnings predict eventual UberX entry timing.

5. Main estimates

In this section, we present several sets of results. To start, we present our main estimates focusing on the impact of Uber's presence on drinking activity using self-reported alcohol consumption data from BRFSS. We then show that our main estimates are robust to alternative specifications, estimation methods, and sample restrictions. To provide a richer understanding of the effect of Uber on drinking, we also explore heterogeneity across age, gender, race, student status, and TCI values. Next, we support our main analyses by estimating Uber's impact on employment and workers' earnings in establishments that serve alcohol, namely bars and restaurants, using county-level QCEW data. Finally, we show that our main QCEW estimates are robust to alternative specifications, weighting choices, and sample restrictions.

5.1. BRFSS alcohol consumption estimates

In Table 4, we present estimates from three differences-in-differences specifications for each alcohol-related BRFSS outcome of interest. The first specification includes only MMSA and year fixed effects. The second specification adds MMSA-level demographic controls from the ACS and individual-level controls from the BRFSS, and is our preferred specification since we find no evidence of pre-trends (see Section 4.1). Our third specification controls for MMSA-specific linear time trends. Our BRFSS outcomes of interest include UberX's impact on number of drinking days, average drinks per drinking day, total drinks, maximum drinks in a single occasion, instances of binge drinking, and an indicator for any alcohol consumed in the past 30 days. We use a Poisson model to estimate the effects of UberX as percentage changes, except when estimating Uber's effect on the "Any Alcohol?" indicator variable where we estimate a linear probability model via OLS. In all specifications, we weight by the BRFSS-provided survey weights and report standard errors robust to clustering at the MMSA-level.

From our preferred specification, which controls for both individual and area-level characteristics, we find that

Table 4
BRFSS alcohol consumption estimates.

Panel A: Mean of Dep. Var. in Brackets	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Drinking days [4.90]			Avg. drinks per drinking day [1.45]			Total drinks [12.98]		
UberX	0.030**	0.027**	0.028*	0.040**	0.036**	0.033*	0.060**	0.054**	0.063**
	(0.015)	(0.012)	(0.015)	(0.017)	(0.016)	(0.019)	(0.027)	(0.026)	(0.031)
Observations	1,429,274	1,429,274	1,429,274	1,416,137	1,416,137	1,416,137	1,414,582	1,414,582	1,414,582
Panel B: Mean of Dep. Var. in Brackets	Max drinks single occasion [2.14]			Binge drinking instances [0.78]			Any alcohol? (LPM) [0.60]		
UberX	0.049***	0.043***	0.040**	0.059*	0.051	0.079*	0.008	0.008**	0.004
	(0.015)	(0.014)	(0.018)	(0.036)	(0.037)	(0.045)	(0.005)	(0.004)	(0.004)
Observations	1,399,029	1,399,029	1,399,029	1,417,596	1,417,596	1,417,596	1,432,334	1,432,334	1,432,334
MMSA FE &Year FE	Y	**Y**	Y	Y	**Y**	Y	Y	**Y**	Y
MMSA &indiv. controls		**Y**	Y		**Y**	Y		**Y**	Y
MMSA-specific Trends			Y			Y			Y
N of MMSAs	225	**225**	225	225	**225**	225	225	**225**	225

Notes: Our estimation sample consists of individual respondents aged 21 to 64 in ever-treated MMSAs in BRFSS SMART data. Because we estimate linear probability models for “Any Alcohol?”, a coefficient of 0.008 should be interpreted as a 0.8 percentage point increase when UberX is present. We estimate the effect on the remaining outcomes (all count variables) using Poisson models, meaning those coefficients approximate percentage changes. For example, the estimate of 0.030 in Panel A column 1 reflects about a 3.0% increase in the number of drinking days. In all specifications, we weight observations using BRFSS-provided survey weights. We include MMSA fixed effects, year fixed effects, individual and MMSA-level covariates, and MMSA-specific linear trends as indicated. Standard errors are robust to clustering at the MMSA level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Uber’s presence is associated with a 2.7% increase in drinking days, a 3.6% increase in the average number of drinks per drinking day, and a 5.4% increase in total drinks. Each estimate is statistically different from zero at the 5% level. As we explain in the introduction, a 5.4% increase in the total number of drinks per month corresponds to more than 580,000 additional drinks per MMSA-month. In addition, we find that UberX is associated with a 4.3% increase in the maximum number of drinks in a single drinking occasion (significant at the 1% level), a 5.1% increase in binge drinking (not statistically significant at conventional levels, $p = 0.17$), and a 0.8 percentage point (1.33%) increase in the number of people who report any alcohol consumption over the previous 30 days (significant at the 5% level). These estimates are representative of the results we obtain from all three specifications for each outcome, though they may be conservative in some cases. For example, we find that including MMSA-specific linear trends magnifies the effect on total drinks to 6.3% and the effect on binge drinking to 7.9% (now significant at the 10% level).

In the following table, Table 5, we present several robustness checks alongside our preferred estimates from Table 4 in Panel A. In particular, we present estimates from our preferred specification when we use OLS with levels (Panel B) and logged dependent variables (Panel C). We also present the estimates we obtain when we include all BRFSS MMSAs regardless of treatment status (Panel D) and restrict our analysis to only MMSAs that are present in the BRFSS SMART every year from 2009 through 2017 (Panel E). In each specification, we consistently find a generally statistically significant positive relationship between UberX and alcohol consumption. Finally, we also present estimates where we restore to our sample responses reflecting unrealistically high (as explained in Section 3) levels of average drinks per drinking day, total drinks, max drinks on one occasion, and binge drinking instances (Panel F). Here, we still find a positive relationship between UberX and drinking. That said, including the outliers slightly attenuates the percentage change estimates for average drinks, total drinks, and maximum drinks. In a separate set of robustness analyses, presented in Table B6, we omit several major metro areas one-by-one to show that none of them are driving our central estimates.

In additional analyses presented in Appendix B, we examine how UberX’s presence affects some placebo outcomes from the BRFSS, namely consumption of fruit, juice, vegetables, and non-work-related exercise activities. Because there is no reason to suspect Uber’s presence directly affects such outcomes, the estimates from this exercise can help us test whether our findings are driven by a general increase in food/drink consumption or available leisure time. We present these estimates in Table B5. Columns 1 to 3 are Poisson model estimates of UberX’s impact on total fruit, juice, and vegetable consumption in the past 30 days, and should be interpreted as percentage changes. Note that these survey questions are only asked every other year. Column 4 reports OLS estimates from a linear probability model where the outcome is an indicator for having completed any exercise in the past 30 days. We present these results across several relevant samples, including our main estimation sample, males

Table 5
BRFSS estimates – alternative specifications.

	(1) Drinking days	(2) Avg. drinks	(3) Total drinks	(4) Max drinks one occasion	(5) Binge drinking instances	(6) Any alcohol? (LPM)
Panel A: Main estimates (Poisson)						
UberX	0.027**	0.036**	0.054**	0.043***	0.051	0.008**
	(0.012)	(0.016)	(0.026)	(0.014)	(0.037)	(0.004)
Mean of DV	4.90	1.45	12.98	2.14	0.78	0.60
Observations	1,429,274	1,416,137	1,414,582	1,399,029	1,417,596	1,432,334
N of MMSAs	225	225	225	225	225	225
Panel B: OLS						
UberX	0.139**	0.054**	0.738**	0.098***	0.039	–
	(0.055)	(0.024)	(0.342)	(0.030)	(0.028)	–
Mean of DV	4.90	1.45	12.98	2.14	0.78	–
Observations	1,429,274	1,416,137	1,414,582	1,399,029	1,417,596	–
N of MMSAs	225	225	225	225	225	–
Panel C: OLS with logged dependent variable – ln(1+Y)						
UberX	0.023***	0.015**	0.037***	0.023***	0.014***	–
	(0.008)	(0.006)	(0.013)	(0.007)	(0.005)	–
Observations	1,429,274	1,416,137	1,414,582	1,399,029	1,417,596	–
N of MMSAs	225	225	225	225	225	–
Panel D: Including untreated MMSAs						
UberX	0.026**	0.036**	0.054**	0.043***	0.050	0.008**
	(0.012)	(0.016)	(0.026)	(0.014)	(0.036)	(0.004)
Mean of DV	4.84	1.45	12.98	2.14	0.78	0.60
Observations	1,455,819	1,442,493	1,440,907	1,425,112	1,443,952	1,458,945
N of MMSAs	257	257	257	257	257	257
Panel E: Including only MMSAs with zero missing years of data						
UberX	0.028**	0.039**	0.059**	0.047***	0.056	0.007*
	(0.013)	(0.019)	(0.030)	(0.016)	(0.043)	(0.004)
Mean of DV	4.90	1.45	12.98	2.15	0.78	0.60
Observations	1,192,615	1,181,409	1,180,198	1,167,013	1,182,642	1,194,973
N of MMSAs	96	96	96	96	96	96
Panel F: Including observations with "Unreasonable" reported alcohol consumption						
UberX	–	0.026	0.040*	0.032**	0.051	–
	–	(0.016)	(0.023)	(0.014)	(0.037)	–
Mean of DV	–	1.49	13.62	2.23	0.79	–
Observations	–	1,417,372	1,415,813	1,401,844	1,417,681	–
N of MMSAs	–	225	225	225	225	–

Notes: DV = dependent variable. Our estimation sample consists of individual respondents aged 21 to 64 in ever-treated MMSAs in the BRFSS SMART data unless noted otherwise. Because we estimate linear probability models for "Any Alcohol?", the coefficient of 0.008 in Panel A should be interpreted as a 0.8 percentage point increase when UberX enters. We estimate the effect on the remaining outcomes (all count variables) using Poisson models, meaning that those coefficients approximate percentage changes. For example, the estimate of 0.027 in Panel A column 1 reflects a roughly 2.7% increase in the number of drinking days. In all specifications, we include MMSA fixed effects, year fixed effects, individual and MMSA-level covariates, and weight observations using BRFSS-provided survey weights. Standard errors are robust to clustering at the MMSA level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

only, students only, young adults aged 21–34, individuals aged 18–20, and those aged 65+. We find effects that are generally small, sometimes negative, and statistically indistinguishable from zero in every case but one.[32]

Note that we also explore whether UberX has any short-run impacts on self-reported health among BRFSS respondents. In particular, we look at outcomes that may be associated with additional drinking, including measures of general health, mental health, and smoking, and present the results in Table B8 in Appendix B. We find generally small and statistically insignificant estimates of the relationship between UberX and self-reported health. For some groups, we find small increases in depressive disorders; in others we find small increases in poor mental health days. But overall, we do not find convincing evidence that Uber affects this set of health outcomes. In addition, we note that our estimates must be interpreted with caution. While they may accurately reflect the net effects of UberX on self-reported health, we cannot isolate the effects on health occurring via the channel of increased alcohol consumption. In particular, Uber may affect health outcomes in other ways, such as improving access to health care providers and emergency rooms (Moskatel and Slusky, 2019). Moreover, by allowing people to safely attend social events, Uber may also have a competing *positive* effect on mental health outcomes.

5.2. BRFSS alcohol consumption heterogeneity

In this subsection, we aim to provide a richer understanding of the effect of Uber on drinking by exploring heterogeneity across age, gender, race, student status, and

[32] The lone exception is a 1.7 percentage point reduction in having completed any exercise in the past 30 days among respondents age 65+.

Table 6
BRFSS estimates – heterogeneity by age.

	(1) Drinking days	(2) Avg. drinks	(3) Total drinks	(4) Max drinks one occasion	(5) Binge drinking instances	(6) Any alcohol? (LPM)
Panel A: Ages 21–34						
UberX	0.044**	0.055*	0.074*	0.047*	0.095**	0.015*
	(0.018)	(0.033)	(0.038)	(0.025)	(0.040)	(0.008)
Mean of DV	4.80	1.83	14.97	2.83	1.08	0.64
Observations	281,765	277,812	277,633	272,661	278,739	282,077
N of MMSAs	225	225	225	225	225	225
Panel B: Ages 18–20 (excluded from main estimates)						
UberX	0.017	−0.015	−0.028	0.021	−0.117	−0.016
	(0.098)	(0.088)	(0.113)	(0.111)	(0.136)	(0.026)
Mean of DV	1.95	1.19	8.06	1.66	0.68	0.35
Observations	42,248	41,601	41,566	41,142	41,904	42,308
N of MMSAs	221	221	221	221	218	224
Panel C: Ages 65+ (excluded from main estimates)						
UberX	0.030	−0.020	−0.031	0.006	−0.050	−0.003
	(0.024)	(0.042)	(0.057)	(0.027)	(0.096)	(0.009)
Mean of DV	5.11	0.72	9.11	0.96	0.22	0.45
Observations	680,488	675,551	674,469	671,680	676,644	682,247
N of MMSAs	225	225	225	225	225	225
Panel D: All ages (18+)						
UberX	0.028**	0.031*	0.043	0.041***	0.040	0.005
	(0.013)	(0.018)	(0.028)	(0.016)	(0.032)	(0.004)
Mean of DV	4.77	1.30	12.03	1.90	0.68	0.57
Observations	2,152,024	2,133,303	2,130,631	2,111,865	2,136,181	2,156,891
N of MMSAs	225	225	225	225	225	225

Notes: DV = dependent variable. Our estimation sample consists of individual respondents aged 21 to 64 in ever-treated MMSAs in the BRFSS SMART data unless noted otherwise. Because we estimate linear probability models for “Any Alcohol?”, the coefficient 0.015 in Panel A should be interpreted as a 1.5 percentage point increase when UberX enters. We estimate the effect on the remaining outcomes (all count variables) using Poisson models, meaning that those coefficients approximate percentage changes. For example, the estimate of 0.044 in Panel A column 1 reflects a roughly 4.4% increase in the number of drinking days. In all specifications, we include MMSA fixed effects, year fixed effects, individual and MMSA-level covariates, and weight observations using BRFSS-provided survey weights. Standard errors are robust to clustering at the MMSA level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

TCI value. To start, Table 6 reports estimates by various age groups. First, we restrict our estimation sample to young drinking-age adults aged 21 to 34. In this group, the positive relationship between Uber and alcohol consumption appears to be larger. In particular, UberX is associated with a 4.4% increase in drinking days, 5.5% increase in average drinks, 7.4% increase in total drinks, 4.7% increase in maximum drinks on one occasion, and a 1.5 percentage point (2.3%) increase in whether an individual has consumed any alcohol. We also find a 9.5% increase in binge drinking, which is nearly double our main estimate and is statistically significant at the 5% level. Note, however, that visual inspection suggests the age 21–34 estimates are generally not statistically distinguishable from our main set of estimates at conventional levels.

Because we restrict our main sample to ages 21 to 64, we also report estimates among respondents aged 18–20 (Table 6, Panel B), 65+ (Panel C), and then all respondents regardless of age (Panel D). We do not find any evidence that UberX affects drinking activity among BRFSS respondents aged 18 to 20. While this finding may be somewhat surprising, it would make sense if drinking activity among underage adults occurs in settings where ridesharing is less relevant. In any case, the estimates are quite imprecise likely due to sample size limitations. We also find that UberX does not lead to increased drinking activity among those aged 65 or older. Beyond providing greater insight on how ridesharing affects the elderly, we view this finding as providing additional support for a causal interpretation of our central estimates. First, because the elderly have lower baseline levels of alcohol consumption, their drinking activity may be less constrained by the absence of safe transportation alternatives. Second, survey data from the Pew Research Center suggests that younger individuals have more quickly and widely adopted smartphone technology than older individuals.[33]

Beyond age, we also explore heterogeneity by looking at student status, gender, and race. In Table 7, we find that across the board the relationship between UberX and drinking appears to be much larger among students. In particular, we find a 9.8% increase in drinking days, 13.1% increase in average drinks, and 20.2% increase in total drinks. While not statistically significant at conventional levels, we also find estimates suggesting a 7.1% increase in maximum drinks on one occasion, 14% increase in binge drinking, and 2.7% increase in whether an individual has consumed any alcohol. In Table 7, we also show that both baseline alcohol consumption and the effects of UberX on drinking in percentage terms are generally larger for males than females (though, based on visual inspection, do not

[33] For example, in 2015, survey data suggests only 27% of those aged 65+ owned a smartphone. Among those aged 18–29, 85% owned a smartphone (see www.pewresearch.org/internet/2015/04/01/chapter-one-a-portrait-of-smartphone-ownership/).

Table 7
BRFSS estimates – heterogeneity by race, gender, and student status.

	(1) Drinking days	(2) Avg. drinks	(3) Total drinks	(4) Max drinks one occasion	(5) Binge drinking instances	(6) Any alcohol? (LPM)
Panel A: Students only						
UberX	0.098*	0.131*	0.202*	0.071	0.140	0.027
	(0.051)	(0.078)	(0.118)	(0.056)	(0.087)	(0.023)
Mean of DV	3.20	1.43	10.47	2.16	0.79	0.50
Observations	54,518	53,936	53,899	53,339	54,057	54,584
N of MMSAs	223	223	223	223	213	225
Panel B: Males only						
UberX	0.024	0.050**	0.077***	0.056***	0.050	0.011*
	(0.017)	(0.023)	(0.026)	(0.018)	(0.039)	(0.006)
Mean of DV	6.06	1.86	18.35	2.86	1.13	0.66
Observations	602,766	595,222	594,494	584,601	596,437	604,280
N of MMSAs	225	225	225	225	225	225
Panel C: Females only						
UberX	0.034**	0.015	0.012	0.022	0.065	0.005
	(0.014)	(0.017)	(0.037)	(0.018)	(0.048)	(0.006)
Mean of DV	3.76	1.04	7.84	1.46	0.44	0.56
Observations	826,508	820,915	820,088	814,428	821,159	828,054
N of MMSAs	225	225	225	225	225	225
Panel D: White only						
UberX	0.034**	0.034**	0.043*	0.047***	0.047	0.012**
	(0.015)	(0.016)	(0.025)	(0.014)	(0.032)	(0.005)
Mean of DV	5.52	1.53	14.39	2.34	0.85	0.63
Observations	1,111,412	1,103,163	1,101,979	1,090,823	1,103,969	1,113,759
N of MMSAs	225	225	225	225	225	225
Panel E: Black only						
UberX	−0.071	−0.016	0.047	0.018	−0.138	−0.020
	(0.072)	(0.065)	(0.123)	(0.069)	(0.199)	(0.017)
Mean of DV	3.54	1.19	9.58	1.59	0.61	0.52
Observations	156,380	154,006	153,820	151,759	154,077	156,725
N of MMSAs	215	215	215	215	206	216
Panel F: Other race only						
UberX	0.075	0.124*	0.149*	0.060	0.200	0.016
	(0.056)	(0.072)	(0.083)	(0.047)	(0.129)	(0.013)
Mean of DV	3.38	1.27	9.68	1.75	0.63	0.50
Observations	132,082	130,255	130,144	128,358	130,629	132,294
N of MMSAs	225	225	225	225	224	225

Notes: DV = dependent variable. Our estimation sample consists of respondents aged 21 to 64 in ever-treated MMSAs in the BRFSS SMART data (unless noted otherwise). Because we estimate linear probability models for "Any Alcohol?", the coefficient 0.027 in Panel A should be interpreted as a 2.7 percentage point increase when UberX enters. We estimate the effect on the remaining outcomes (all count variables) using Poisson models, meaning those coefficients approximate percentage changes. For example, the estimate of 0.034 in column 1 of Panel A reflects roughly a 9.8% increase in the number of drinking days. In all specifications, we include MMSA fixed effects, year fixed effects, individual and MMSA-level covariates, and weight observations using BRFSS-provided survey weights. Standard errors are robust to clustering at the MMSA level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

appear to be statistically distinguishable from one another at conventional levels). For example, among males we find a 5% increase in the average number of drinks per drinking day and a 7.7% increase in total drinks. Among females, these estimates are 1.5% and 1.2%, respectively, and are not statistically different from zero at conventional levels. In fact, while the effects among females are generally positive, only the estimated effect on number of drinking days is statistically significant. Looking at the effects by race, we see in Table 7 that the estimated effects of UberX on drinking among white respondents are consistently positive and statistically significant. Among black respondents, in general, the coefficients are smaller, less precise, and statistically indistinguishable from zero. We find the largest effect sizes among respondents who report a race other than white or black, though, as with the black-only subsample (likely due to much smaller sample sizes), the estimates here are also quite imprecise.

Finally, motivated by Hall et al. (2018) who find that Uber is a substitute for public transit in cities with better public transit options, we estimate the effects of UberX on drinking activity by quartiles of MMSAs determined by their AllTransit Transit Connectivity Index (TCI) values (see Fig. C3 for a map of TCI values by quartile). We would expect that the marginal impact of Uber on drinking is lower in places where individuals already have better access to public transit. To test this, we interact our treatment variable with TCI quartile indicators, and plot the coefficients in Fig. 4. Indeed, across all of our measures of alcohol consumption, we find that UberX appears to have a larger effect on individuals in MMSAs with lower (worse) TCI scores.

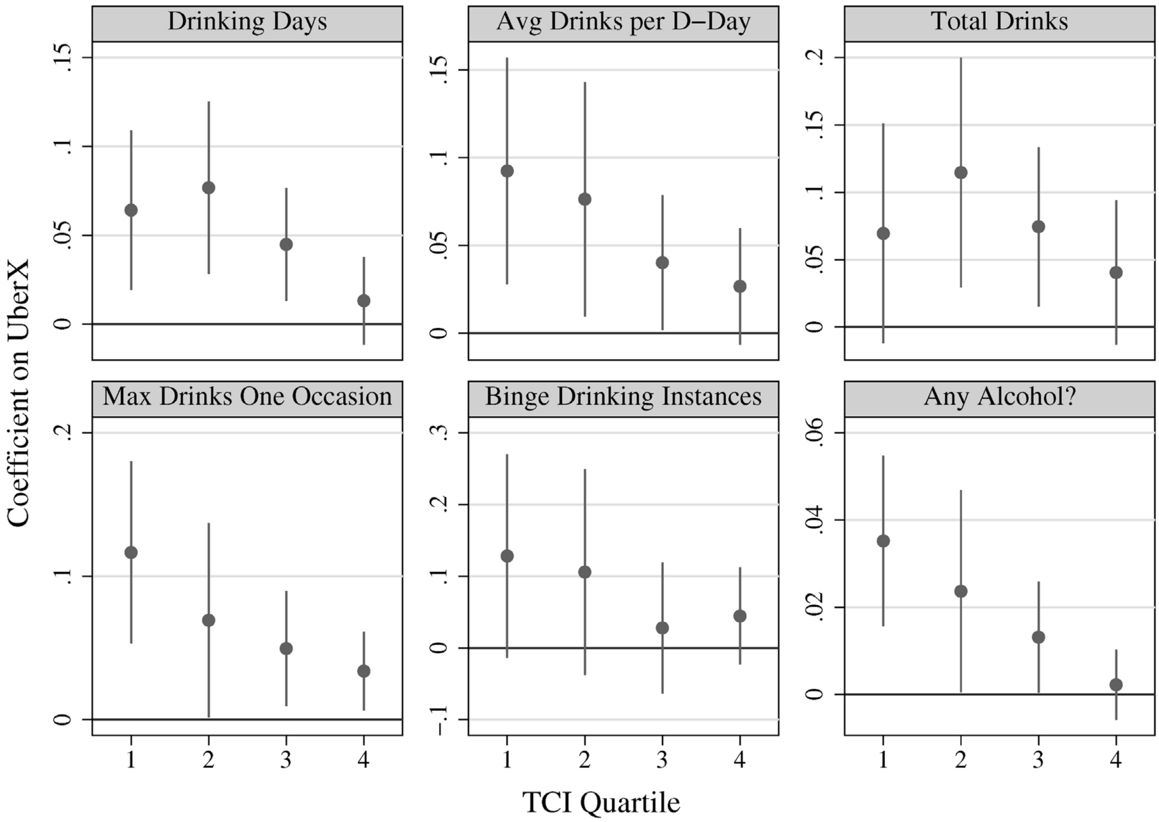


Fig. 4. BRFSS estimates by AllTransit TCI quartile. Notes: Our estimation sample consists of BRFSS SMART respondents aged 21 to 64 in ever-treated MMSAs. We estimate all specifications using a Poisson model, except in the case of the "Any Alcohol?" indicator, where we estimate a linear probability model via OLS. In all specifications, we interact our treatment variable with TCI quartile indicators (see Fig. C3 for TCI scores by area), weight observations using BRFSS-provided survey weights, and include MMSA fixed effects, year fixed effects, and individual and MMSA-level covariates. We plot 95% confidence intervals with each point estimate. Standard errors are robust to clustering at the MMSA level.

5.3. QCEW earnings and employment estimates

Because Uber makes it less costly to consume alcohol away from one's home, at least some of the additional alcohol consumption may be occurring in bars. To examine this relationship, we estimate the effects of UberX on employment and workers' earnings in industries that serve alcohol, namely bars and restaurants, using QCEW data which are provided at the county-quarter-year level. Our outcomes of interest are log employment per 1000 population, log total worker earnings per 1000 population, and log average weekly earnings per worker. We estimate these specifications via OLS with various sets of controls, weighted by county population, and present our results in Table 8. As with our main table of BRFSS estimates, we present three specifications for each outcome of interest: (1) location and time fixed effects only, (2) fixed effects plus county-level demographic controls, and (3) fixed effects, county-level ACS controls, and county-specific linear trends. In our preferred specification (2), we find that UberX is associated with a 3.5% increase in employment and a 3.7% increase in earnings among workers at NAICS-designated drinking places (i.e., bars). We find no effect on average weekly earnings per employee. Taken together, both of these findings are consistent with an increase in demand for alcohol consumption at bars. Moreover, they suggest that bars are hiring additional employees to meet the increased demand rather than relying on existing employees to handle the additional workload.

As a quasi-placebo test, we then examine UberX's impact on earnings and employment at full-service restaurants. We call this a quasi-placebo because, to the extent that individuals are less likely to engage in heavy drinking at restaurants (even when transportation is not a constraint), we should expect smaller (or no) increases in earnings and employment at such establishments. As predicted, we find a smaller 1.2% increase in employment, no effect on total earnings, and a slight negative effect on average weekly earnings among restaurant workers. If our main BRFSS estimates were driven by unobserved shocks affecting leisure and consumption in general, then we would expect to see comparable increases in earnings and employment at both bars and restaurants. Instead, we only find substantial increases in earnings and employment at bars, supporting a causal interpretation of our estimated effects of UberX on drinking activity.

Next, we present several robustness checks in Table 9. Panel A includes our preferred QCEW estimates for ease of comparison. Panel B presents the results from a triple-difference estimation approach, where the main coefficient

Table 8
QCEW earnings and employment at drinking places & full-service restaurants.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Log employment per 1000			Log total earnings per 1000			Log avg weekly earnings		
Panel A: Drinking places									
UberX	0.043***	0.035***	0.025***	0.046***	0.037***	0.024***	0.003	0.003	−0.001
	(0.011)	(0.010)	(0.007)	(0.012)	(0.012)	(0.008)	(0.004)	(0.004)	(0.003)
County-quarter obs.	19,799	19,799	19,799	19,799	19,799	19,799	19,799	19,799	19,799
N of MMSAs	306	306	306	306	306	306	306	306	306
Panel B: Full-service restaurants									
UberX	0.013**	0.012**	0.006*	0.008	0.006	−0.000	−0.005*	−0.006**	−0.006**
	(0.005)	(0.005)	(0.003)	(0.005)	(0.005)	(0.004)	(0.003)	(0.003)	(0.003)
County-quarter obs.	31,434	31,434	31,434	31,434	31,434	31,434	31,434	31,434	31,434
N of MMSAs	316	316	316	316	316	316	316	316	316
County FE &qtr-year FE	Y	**Y**	Y	Y	**Y**	Y	Y	**Y**	Y
County controls		**Y**	Y		**Y**	Y		**Y**	Y
County-specific trends			Y			Y			Y

Notes: Estimation sample consists of all ever-treated MMSAs in QCEW data. Dependent variables are in logs, observations are at the county-quarter level. We estimate all specifications using OLS, and weight observations using county population. We include county fixed effects, quarter-year fixed effects, county-level covariates, and county-specific linear trends as indicated. Standard errors are robust to clustering at the MMSA level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 9
QCEW estimates – alternative specifications.

	(1)	(2)	(3)	(4)	(5)	(6)
	Drinking places			Full-service restaurants		
	Log employment per 1000	Log tot. earnings per 1000	Log avg wkly earnings	Log employment per 1000	Log tot. earnings per 1000	Log avg wkly earnings
Panel A: Main estimates						
UberX	0.035***	0.037***	0.003	0.012**	0.006	−0.006**
	(0.010)	(0.012)	(0.004)	(0.005)	(0.005)	(0.003)
County-quarter obs.	19,799	19,799	19,799	31,434	31,434	31,434
N of MMSAs	306	306	306	316	316	316
Panel B: Triple difference (drinking places vs. full-service restaurants)						
UberX	0.030***	0.038***	0.009**	–	–	–
	(0.011)	(0.011)	(0.004)	–	–	–
County-Quarter-NAICS Obs.	38,662	38,662	38,662	–	–	–
N of MMSAs	305	305	305	–	–	–
Panel C: Unweighted regressions						
UberX	0.031**	0.025	−0.006	0.001	−0.006	−0.007*
	(0.014)	(0.017)	(0.006)	(0.006)	(0.007)	(0.004)
County-quarter obs.	19,799	19,799	19,799	31,434	31,434	31,434
N of MMSAs	306	306	306	316	316	316
Panel D: Including only "Central" counties						
UberX	0.035***	0.038***	0.003	0.012**	0.006	−0.005**
	(0.011)	(0.012)	(0.004)	(0.005)	(0.005)	(0.003)
County-quarter obs.	16,295	16,295	16,295	21,051	21,051	21,051
N of MMSAs	306	306	306	316	316	316
Panel E: Including only BRFSS MMSAs						
UberX	0.028	0.030	0.002	−0.004	−0.010	−0.006
	(0.019)	(0.020)	(0.005)	(0.009)	(0.010)	(0.004)
County-quarter obs.	16,654	16,654	16,654	26,277	26,277	26,277
N of MMSAs	219	219	219	224	224	224
Panel F: Including untreated counties						
UberX	0.035***	0.040***	0.005	0.024***	0.022***	−0.002
	(0.011)	(0.013)	(0.004)	(0.007)	(0.006)	(0.002)
County-quarter obs.	27,600	27,600	27,600	52,378	52,378	52,378
N of MMSAs	666	666	666	880	880	880
Panel G: Including only counties with zero missing quarters of data						
UberX	0.030***	0.032***	0.003	0.011**	0.005	−0.006**
	(0.011)	(0.012)	(0.004)	(0.005)	(0.005)	(0.003)
County-quarter obs.	14,292	14,292	14,292	24,552	24,552	24,552
N of MMSAs	234	234	234	315	315	315

Notes: Estimation sample consists of all ever-treated MMSAs in QCEW data. Dependent variables are in logs, observations are at the county-quarter level. We estimate all specifications using OLS. Unless noted otherwise, we include county fixed effects, quarter-year fixed effects, county-level covariates, and weight observations using county population. Standard errors are robust to clustering at the MMSA level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

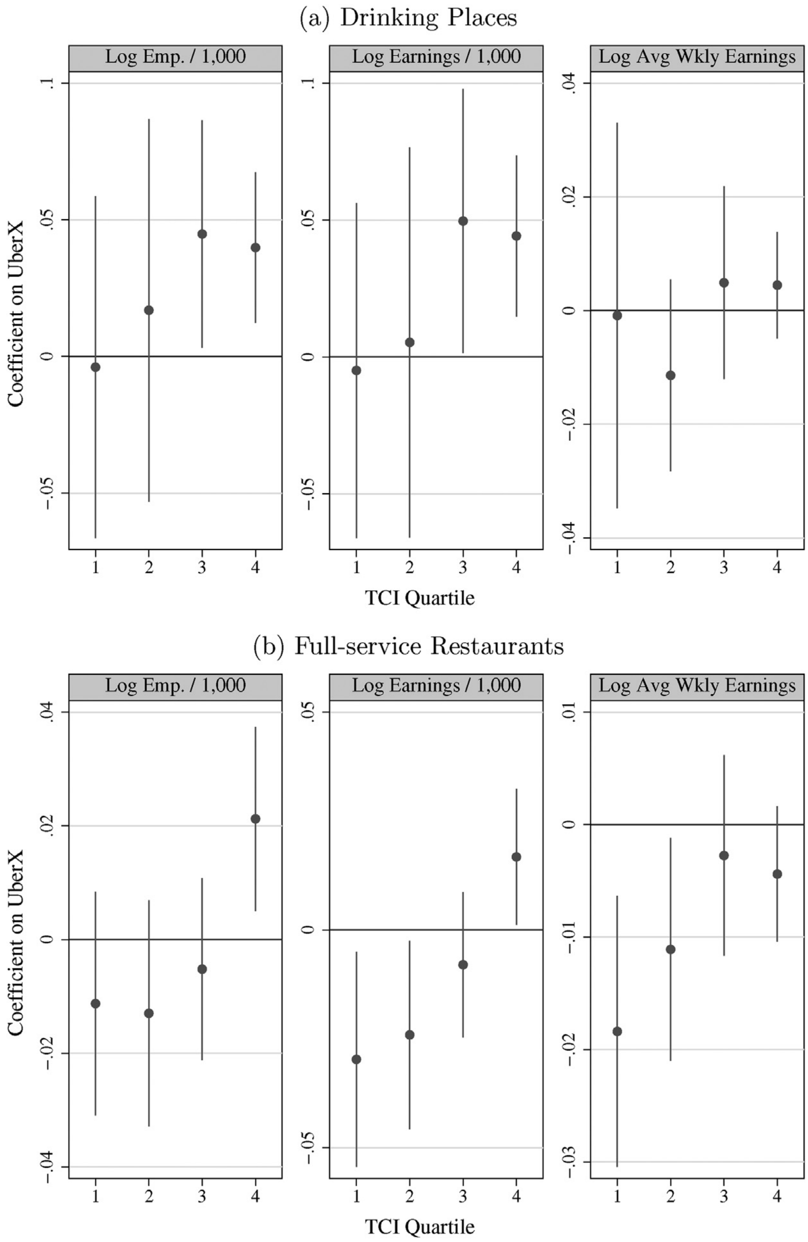


Fig. 5. QCEW estimates by AllTransit TCI quartile. Notes: Our estimation sample consists of all ever-treated MMSAs in the QCEW data. Dependent variables are in logs and observations are at the county-quarter-year level. We estimate all specifications using OLS with UberX by TCI quartile interactions, and weighting observations by county population. We include county fixed effects, quarter-year fixed effects, and county-level covariates in all specifications. Standard errors are robust to clustering at the MMSA level. We plot 95% confidence intervals with each point estimate.

of interest is the interaction of quarter-year, county, and establishment type. Here, we also include the appropriate two-way interactions (quarter by county, quarter by establishment type, and county by establishment type). The results are quite similar, suggesting that Uber is associated with a 3% increase in employment, 3.8% increase in total earnings, and 0.9% increase in average weekly earnings per employee. In Panels C through G, we show that our results are remarkably similar whether we (C) do not weight by county population, (D) include only counties designated as "central" to their MMSA, (E) include only the MMSAs present in the BRFSS sample, (F) include untreated counties, and (G) exclude counties that are missing data for any quarter-year from 2009–17. In a separate set of robustness analyses, presented in Table B7, we omit several major metro areas one-by-one to show that none of them are driving our estimates.

We want to emphasize that examining these QCEW outcomes only captures one part of the alcohol consumption market. This paper is ultimately concerned with drinking activity overall, regardless of where it occurs. That is, people may use UberX for transportation to parties, weddings, and various other social events involving alcohol. Because of this, it is important to note that in theory ridesharing could increase drinking activity without any effect on bar and restaurant employment and earnings or vice versa. For example, ridesharing may enable individuals to consume more alcohol at home before traveling to a bar, meaning they may consume less alcohol at the bar than if they had to rely on other transportation options. In the opposite direction, if ridesharing makes it less costly to visit bars where alcohol is relatively more expensive per unit, it could increase earnings (tips) for bar employees while reducing or having no net impact on the total amount of alcohol consumption. Moreover, the effect of ridesharing on going out to bars may vary by availability and quality of local bars, alcohol prices, local culture, and more.

Recall that, in our BRFSS analyses, we find Uber's impact on alcohol consumption is smaller in areas where existing transit connectivity is higher. For completeness, in Fig. 5, we present coefficients from QCEW regressions where we interact TCI quartile with our UberX treatment variable. While the estimates are imprecise and generally statistically indistinguishable from one another, they reveal an interesting pattern. Namely, they suggest that UberX has a somewhat *larger* effect on employment and earnings at bars and restaurants in higher-TCI areas. Combined with our BRFSS analyses by TCI quartile, this suggests that Uber mostly shifts existing drinking activity toward bars and restaurants in high-TCI areas. Meanwhile, Uber seems to induce more drinking overall in low-TCI areas, which appears to occur mostly in non-bar and non-restaurant venues.

6. Conclusion

One of the obvious potential benefits of ridesharing is its capacity to reduce drunk driving by reducing the cost of obtaining safe transportation. However, to the extent that ridesharing reduces the costs associated with drinking away from one's home, it could increase both the quantity and frequency of alcohol consumption in social settings. To examine whether ridesharing affects alcohol consumption, we use BRFSS data from 2009 to 2017 and a differences-in-differences empirical strategy that relies on variation in the timing of UberX's introduction across U.S. metro areas. We unambiguously find that the introduction of Uber is associated with increases in the frequency and quantity of self-reported alcohol consumption.

To put our estimates in context, Anderson et al. (2013) find that medical marijuana laws reduce total drinks consumed by about 10.6% among those aged 20–29. This is slightly larger than our estimated 7.4% increase in total drinks consumed by those aged 21 to 34 when UberX becomes available. Additionally, Ruhm et al. (2012) estimate the price elasticity of demand for alcohol to be roughly −0.3. Thus, to increase total drinks consumed by 5.4%, as we find in Table 4, alcohol prices would need to fall by 18%.

Inspection of the pre-trends from event study estimates, the inclusion of relevant controls, and the results of various robustness tests suggests that our central estimates can be interpreted as causal parameters. We also present evidence to support our proposed mechanism using several additional sets of analyses. First, we find that the estimated effect of Uber on alcohol consumption varies by the level of transit connectivity in a city. In particular, Uber appears to have a larger positive effect on drinking in areas where existing transit connectivity is relatively weak. Second, Uber appears to have larger positive effects among young adults and students, whose drinking activity is more likely to be constrained by the absence of ridesharing and who have more quickly and widely adopted smartphone technology. Third, using QCEW data, we show that Uber is associated with much larger increases in employment and earnings at NAICS-designated drinking places relative to full-service restaurants. These findings offer additional evidence that ridesharing increases alcohol consumption, and suggest that some of the additional drinking is indeed occurring in bars.

Our findings complement the existing literature on Uber's effects on a range of socially important outcomes, such as overall consumer surplus, the value of flexible work, local economic conditions, public transit use, drunk driving, and crime. We hope that our work spurs further research on a wide range of potential benefits and costs associated with ridesharing, particularly via its effects on alcohol consumption.

Appendix A. Transportation and alcohol consumption

Jackson and Owens (2011) provide a model of the demand for drinking in social settings (a "night out", denoted N_i). Individual *i*'s utility, U_i, from such social activity is

$$U_i = f_i(g_i(N_i, \theta_{i'}), Y_i) \tag{A.1}$$

where $\theta_{i'}$ is the quantity of nights out for other people ($\theta_{i'} = \Sigma^{I}_{i \neq i'} N_{i'}$), and $g_i(\cdot)$ is a function that is strictly increasing in both arguments. The functional form of *g* assumes that a night out is "better" if more people are also "out" (and therefore ignores issues of overcrowding). *Y* is a numeraire good ($p_Y = 1$).

Individual *i*'s budget constraint is $B_i = Y_i + N_i \cdot C_{iN}$, where C_{iN} is the price of a night out for *i*. Within such a setup, individual *i* maximizes their utility wherever the marginal utility from one unit of *Y* (MU_{iY}) equals $\frac{MU_{iN}}{I} C_{iN}$. All else being equal, the consumer will choose relatively less of the numeraire good as the price of a night out, C_{iN}, decreases, and *vice versa*.

The price of a night out consists of the cost of drinking, determined by a fixed quantity of drinks consumed in a night out (*D*) times their exogenously given price p_D, plus transportation costs. For Jackson and Owens, individual *i* can drive or take the D.C. metro. To adapt their model to our setting, assume individual *i* can either drive or use a rideshare service and that the price of driving is p_{ic} and the price of ridesharing is p_{ir}. These prices are determined by subjective assessments of convenience costs, ownership and maintenance costs, time and accessibility issues, relative safety concerns, and so on. The total price of a night out is

$$C_{iN} = D \cdot p_D + T_{ic} \cdot p_{ci} + T_{ir} \cdot p_{ri} \tag{A.2}$$

where $T_{ic} = 1$ if and only if $p_{ic} < p_{ir}$, and is 0 otherwise, and $T_{ir} = 1$ if $p_{ic} > p_{ir}$, and 0 otherwise.[34] For individual i, $p_{ic} < p_{ir}$ before the advent of ridesharing, because $p_{ir} = \infty$. The advent of ridesharing leads to a reduction in C_N for i or for some or all i' whenever the individual's cost of ridesharing is lower than the cost of driving.[35] In turn, i or at least some i' choose more nights out (N) in equilibrium.

Within the model, more nights out increases total alcohol consumption only by increasing the *frequency* of consumption. It is, however, relatively simple to extend the model to allow the intensity of consumption to also be affected by the advent of ridesharing. For example, we could have $D = D_c$ when $p_{ic} < p_{ir} + (D_r - D_c) \cdot p_D$ and $D = D_r$ otherwise, where $D_r > D_c$. In such a set up, we are assuming that the quantity of drinks D tends to increase if a safe ride is available (for simplicity, we continue to assume that D does not also vary across individuals, nor does it increase the utility from a "night out"). In such a version of the model, individual i's cost of a night out changes in two ways after the advent of ridesharing, restricting the number who may find ridesharing attractive. Therefore, $T_{ic} = 1$ if and only if $p_{ic} + D_c \cdot p_D < p_{ir} + D_r \cdot p_D$, and is 0 otherwise, and $T_{ir} = 1$ when $T_{ic} = 0$, and 0 otherwise.

Given $g(\cdot)$ is increasing in both arguments, however, it is worth noting that the advent of ridesharing leads to more nights out (N) for i and each i', even if most people are non-ridesharers. The net equilibrium effect on intoxicated driving is, therefore, theoretically ambiguous. On one hand, ridesharing induces some of those who would otherwise drive to switch to a ridesharing service. This reduces intoxicated driving at the margin. On the other hand, ridesharing increases the frequency of nights out because the utility of a night out is increasing in the number of other people. Some of these individuals may find it subjectively optimal to choose to drive, thereby increasing intoxicated driving at the margin. Notably, the model ignores any behavioral effect of alcohol consumption on choices (such as choosing to drive, instead of taking an Uber, after consuming a large quantity of alcohol). This conceptual framework may help explain why Brazil and Kirk (2016) and Barrios et al. (2020a) find non-negative effects of Uber on measures of intoxicated driving.

Finally, to the extent that ridesharing increases the frequency of nights out and the intensity of alcohol consumption on such occasions, demand for drinks away from one's home will increase. Given that at least some of this additional drinking likely occurs at bars, we would expect this demand shock to also lead to an increase in employment at bars and/or earnings among bar employees (which are largely driven by proportional tips). We examine this proposition further in our analyses presented in Sections 4 and 5.

[34] For brevity, we ignore the case where $p_{ic} = p_{ir}$.

[35] Otherwise, $T_r = 0$ for i and for all i', which implies that ridesharing cannot exist.

Appendix B. Additional figures and tables

In Figs. 2 and 3 in the body of the paper, we present sets of BRFSS and QCEW event studies without MMSA-specific linear trends. For completeness, we present event studies where the specification includes MMSA-specific linear trends in Figs. B1 and B2. There remains no evidence of problematic pretrends in the outcomes of interest prior to Uber entry. It is worth noting that in many cases including linear trends increases the size of UberX's effect on our BRFSS measures of drinking activity. We then present BRFSS summary statistics for all ages in Table B1 and, in Table B2, stratified by early versus late UberX entry. In the body of the paper, summary statistics refer only to our main estimation sample (those aged 21 to 64). Next, Table B5 presents placebo analyses for our BRFSS outcomes of interest, examining whether UberX is associated with other forms of food and drink consumption (fruit, vegetables, juice) or leisure activity (exercise). As we describe in the main text, we find no evidence to suggest that our findings are driven by an increase in food/drink consumption or the availability of leisure time.

In Table B6, we present BRFSS drinking activity estimates where we sequentially eliminate cities that may have an outsized impact on our estimates. Specifically, we separately (and then collectively) exclude several highly-populated U.S. cities, including NYC, San Francisco, Los Angeles, Chicago, Dallas-Fort Worth, Houston, and Washington, D.C. These are areas that UberX entered early in the sample period and also have a large population. The estimates from this exercise are very similar to our main estimates illustrating that our findings are not driven by what occurs in one or a handful of large metro areas. In Table B7, we complete the same exercise for employment and earnings for workers at bars and restaurants. Reassuringly, we find little impact of removing any one (or all) of these seven cities on our point estimates. We do, however, see the estimates become slightly attenuated and lose some precision when we eliminate all seven cities from the estimation sample.

In Tables B3 and B4 we present balance tests that estimate whether Uber entry is associated with our ACS area level covariates and then our individual-level BRFSS controls, first with and then without including other controls in the estimating equations. We find a statistically significant association between our treatment variable and observable control variables in only a couple of instances. Because we also include these controls in our preferred specification, our balance tests suggest there are no concerns that might undermine our identification strategy.

Finally, we examine whether additional drinking induced by ridesharing has any subsequent effects on self-reported measures of health from BRFSS. In particular, we use an indicator for whether one rates their general health as very good or excellent (rating of 1 or 2 on a scale from 1 to 5), whether one has ever been diagnosed with a depressive disorder, whether one is a smoker, whether one has smoked in the past 3 months, number of days with poor mental health in the past 30 days, number of days with poor physical health in the past 30 days, and number of days disrupted by poor health in the past 30 days. For all

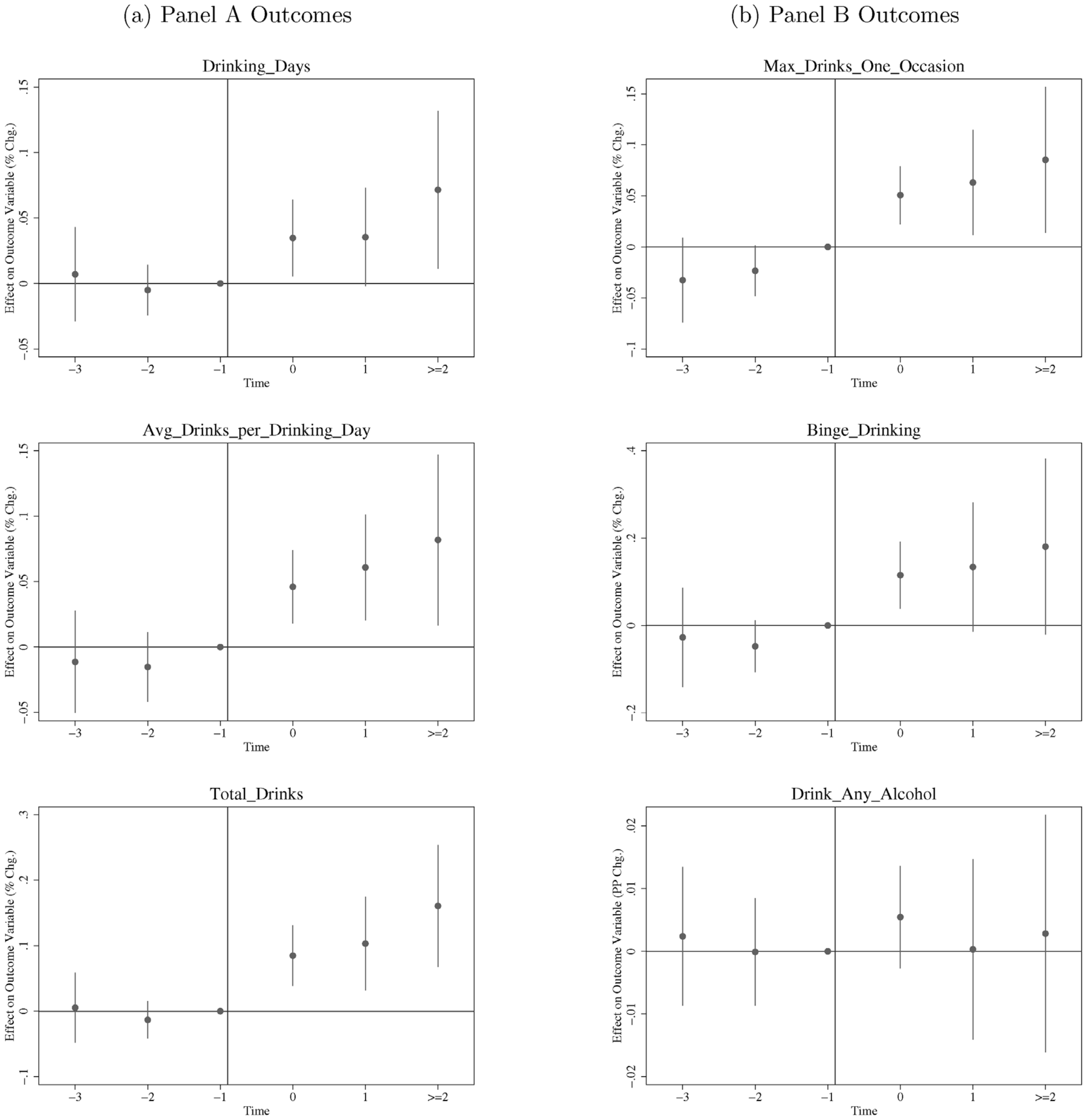


Fig. B1. BRFSS event studies – with MMSA-specific trends. Notes: Our estimation sample consists of BRFSS SMART respondents aged 21 to 64 in ever-treated MMSAs in the BRFSS SMART data. We estimate all specifications using a Poisson model, except in the case of the "Any Alcohol?" indicator, where we estimate a linear probability model via OLS. In all specifications, we weight observations using the BRFSS-provided survey weights, and include MMSA fixed effects, year fixed effects, MMSA-specific linear trends, and individual and MMSA-level covariates. We include an indicator for $t \leq -4$ in all specifications but do not plot it here. Standard errors are robust to clustering at the MMSA level. Bars around point estimates represent 95% confidence intervals.

the regressions, we use our preferred specification (MMSA fixed effects, year fixed effects, individual and area-level controls). We estimate the effects on the first four outcomes using a linear probability model and OLS, and the last three outcomes (all counts of days) using a Poisson model.

In Table B8, we present estimates using our main estimation sample, as well as only students, only males, and only young adults aged 21 to 34 (i.e., the groups that experience the largest increases in alcohol consumption). Despite finding large increases in alcohol consumption, and perhaps because subsequent health effects might take more time to manifest, we find generally small and insignificant estimates of the relationship between UberX and self-reported health. One exception is that we find a 0.7 percentage point (4.1%) increase in whether one has been diagnosed with a depressive disorder. Focusing on students only, we find a 3 percentage point (20%) increase. For young adults aged 21 to 34, we find a statistically insignificant

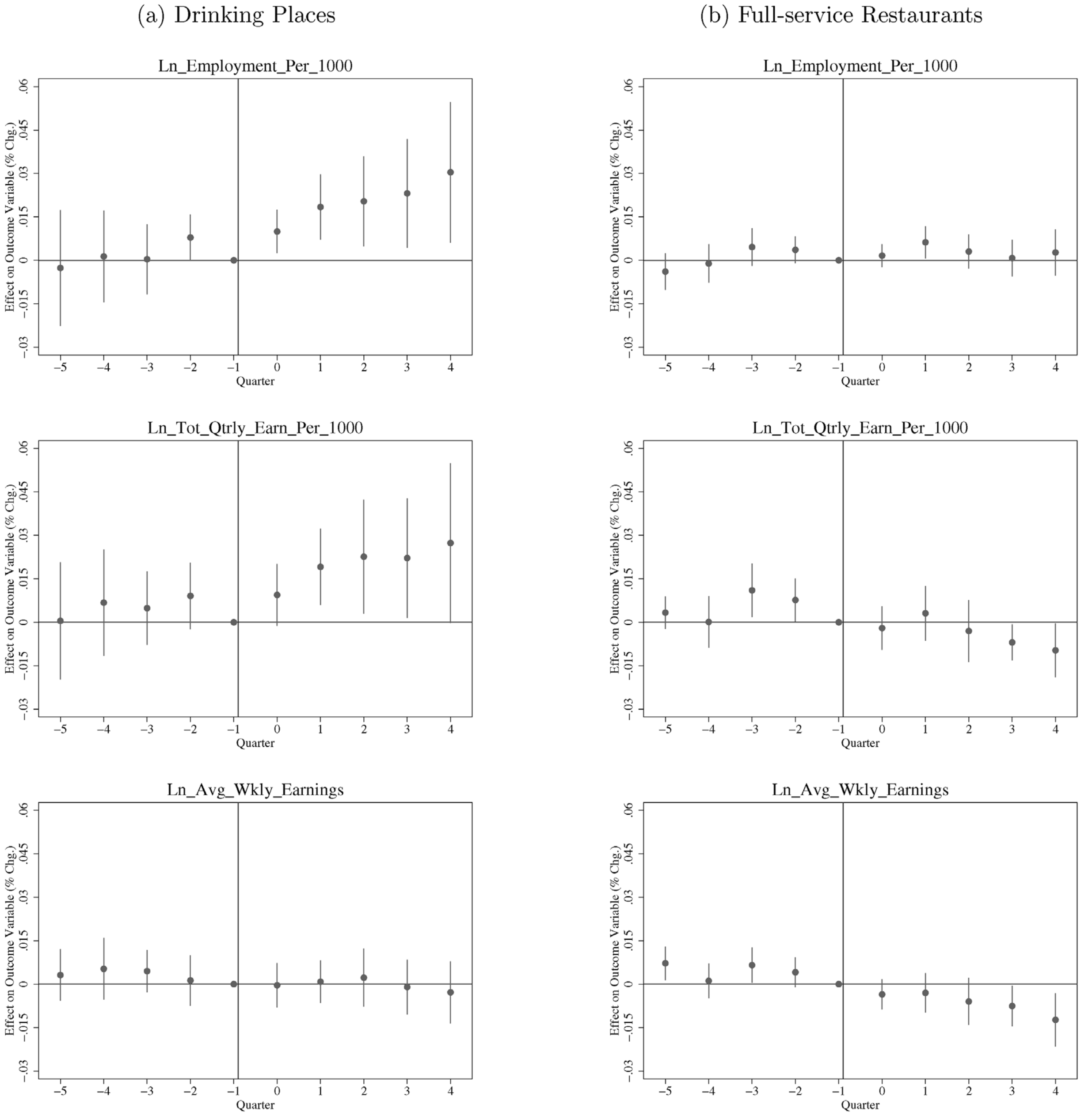


Fig. B2. QCEW event studies – with county-specific trends. Notes: Our estimation sample consists of all ever-treated MMSAs in the QCEW data. Dependent variables are in logs, observations are at the county-quarter-year level. We estimate all specifications using OLS, and weight observations using county population. We include county fixed effects, quarter-year fixed effects, county-specific linear trends, and county-level covariates in all specifications. We include indicators for $t \leq -6$ and $t \geq 5$ in all specifications but do not plot them here. Standard errors are robust to clustering at the MMSA level. Bars around point estimates represent 95% confidence intervals.

1 percentage point (6.3%) increase. Along with this, we also find some evidence of increased poor mental health days. Though this estimate is only 0.7% and indistinguishable from zero for the main sample, we find a statistically significant 4.7% increase among males, as well as statistically insignificant increases of 6.1% among students and 4% among young adults.

That said, these estimates must be interpreted with caution. While they may accurately reflect the net effects of UberX on self-reported health, we cannot isolate the effects on health occurring via the channel of increased alcohol consumption. That is, Uber may affect health outcomes in other ways, such as improving access to health care providers and emergency rooms (Moskatel and Slusky, 2019). Moreover, by allowing people to safely attend social events, Uber may also have a competing *positive* effect on mental health outcomes.

Table B1
BRFSS summary statistics – all ages.

	Mean	SD	P25	Median	P75	P95
Any alcohol?	0.56	0.50				
Num. drinking days	4.77	7.74	0	1	5	25
Avg. drinks per drinking day	1.30	1.94	0	1	2	5
Total drinks	12.03	28.66	0	1	12	60
Max drinks single occasion	1.90	2.82	0	1	3	8
Binge drinking instances	0.68	2.71	0	0	0	4
Age	46.41	17.57	32	45	60	78
Male	0.48	0.50				
White	0.71	0.45				
Married	0.52	0.50				
College degree	0.32	0.47				
Employed	0.58	0.49				
Student	0.06	0.23				
HH income $\geq$35k	0.63	0.48				
Respondents			2,259,414			

BRFSS data from 2009–2017 for MMSAs where UberX eventually becomes available. We weight observations using BRFSS-provided survey weights and include respondents of all ages. Note that there are missing data for some MMSA-years because of lack of a sufficient number of responses to permit public disclosure.

Table B2
BRFSS means – all ages – early vs. late intros.

	Early intros (2012–2014)			Late intros (2015+)		
	Full sample	Pre-UberX	Post-UberX	Full sample	Pre-UberX	Post-UberX
Any alcohol?	0.56	0.56	0.56	0.52	0.51	0.52
Num. drinking days	4.81	4.79	4.83	4.42	4.39	4.52
Avg. drinks per D-day	1.31	1.31	1.30	1.26	1.26	1.26
Total drinks	12.06	11.95	12.16	11.81	11.70	12.16
Max drinks one occasion	1.91	1.92	1.90	1.84	1.84	1.84
Binge drinking instances	0.67	0.66	0.69	0.71	0.70	0.75
Age	46.33	46.14	46.51	47.18	47.10	47.44
Male	0.48	0.49	0.48	0.48	0.48	0.48
Pct. white	0.70	0.72	0.68	0.79	0.80	0.76
Married	0.52	0.54	0.50	0.54	0.55	0.50
College degree	0.33	0.34	0.31	0.27	0.27	0.26
Employed	0.58	0.58	0.59	0.57	0.57	0.57
Student	0.06	0.06	0.06	0.05	0.05	0.05
HH income $\geq$ 35k	0.63	0.63	0.64	0.61	0.61	0.63
Respondents	1,812,345	956,429	855,916	447,069	362,832	84,237

BRFSS data from 2009–2017 for MMSAs where UberX eventually becomes available. We weight observations using BRFSS-provided survey weights and include respondents of all ages. Note that there are missing data for some MMSA-years because of lack of a sufficient number of responses to permit public disclosure.

Table B3
Balance tests – ACS area level.

	Log(Population)	Log(Income)	Median Age	Male %	Age 20–44%	White %
Panel A: Only MMSA FE, year FE on RHS						
UberX	−0.001	0.003	0.070	−0.065**	−0.100	−0.012
	(0.004)	(0.003)	(0.069)	(0.027)	(0.075)	(0.165)
Observations	1,552,600	1,552,600	1,552,600	1,552,600	1,552,600	1,552,600
N of MMSAs	225	225	225	225	225	225
Panel B: MMSA FE, year FE, and all other unrelated controls on RHS						
UberX	−0.003	0.004	0.005	−0.049**	−0.015	0.003
	(0.004)	(0.004)	(0.054)	(0.020)	(0.058)	(0.158)
Observations	1,540,939	1,540,939	1,552,600	1,540,939	1,552,600	1,540,939
N of MMSAs	225	225	225	225	225	225

Notes: Estimation sample consists of respondents aged 21 to 64 in ever-treated MMSAs in BRFSS SMART data. Each column presents the regression of a covariate on our treatment variable plus the indicated set of controls. Standard errors are robust to clustering at the MMSA level. $^{*}p < 0.10$, $^{**}p < 0.05$. $^{***}p < 0.01$.

Table B4
Balance tests – BRFSS individual level.

	Age	Male	White	Married	College	Employed	Student	Income >35k
Panel A: Only MMSA FE, year FE on RHS								
UberX	−0.055	0.004	0.007	0.012**	0.011*	0.018**	0.010	0.004
	(0.208)	(0.004)	(0.007)	(0.005)	(0.006)	(0.007)	(0.007)	(0.004)
Observations	1,540,939	1,552,600	1,552,600	1,552,600	1,552,600	1,552,600	1,552,600	1,552,600
N of MMSAs	225	225	225	225	225	225	225	225
Panel B: MMSA FE, year FE, and all other unrelated controls on RHS								
UberX	−0.012	0.003	0.007	0.011**	0.005	0.011**	0.008	−0.000
	(0.191)	(0.004)	(0.006)	(0.005)	(0.005)	(0.006)	(0.006)	(0.004)
Observations	1,540,939	1,540,939	1,540,939	1,540,939	1,540,939	1,540,939	1,540,939	1,540,939
N of MMSAs	225	225	225	225	225	225	225	225

Notes: Estimation sample consists of respondents aged 21 to 64 in ever-treated MMSAs in BRFSS SMART data. Each column presents the regression of a covariate on our treatment variable plus the indicated set of controls. Standard errors are robust to clustering at the MMSA level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B5
UberX entry and placebo outcomes.

	(1) Fruit (Total, 30 days)	(2) Juice (Total, 30 days)	(3) Vegetables (Total, 30 days)	(4) Exercise (Any, 30 days)
Panel A: Main sample (ages 21–64)				
UberX	0.015	0.022	0.023	0.000
	(0.013)	(0.020)	(0.015)	(0.005)
Mean of DV	30.9	12.8	27.6	0.77
Observations	773,631	768,249	767,754	1,451,235
N of MMSAs	203	203	203	225
Panel B: Males				
UberX	−0.009	−0.001	0.019	0.002
	(0.026)	(0.027)	(0.030)	(0.005)
Mean of DV	26.9	13.9	25.1	0.78
Observations	325,644	323,679	322,760	612,423
N of MMSAs	203	203	203	225
Panel C: Students				
UberX	0.008	−0.014	0.090	−0.001
	(0.078)	(0.135)	(0.081)	(0.016)
Mean of DV	30.2	13.0	27.0	0.86
Observations	18,748	18,632	18,587	34,429
N of MMSAs	203	203	203	225
Panel D: Ages 21–34				
UberX	0.037	−0.019	0.032	0.002
	(0.027)	(0.035)	(0.024)	(0.010)
Mean of Dep. Var.	29.8	14.3	27.0	0.82
Observations	159,073	158,017	157,158	295,785
N of MMSAs	203	203	203	225
Panel E: Ages 18–20				
UberX	0.078	−0.049	0.039	0.009
	(0.058)	(0.117)	(0.053)	(0.016)
Mean of Dep. Var.	28.9	15.0	24.4	0.83
Observations	27,667	27,363	27,187	54,041
N of MMSAs	203	203	203	225
Panel F: Ages 65+				
UberX	0.009	0.031	0.010	−0.017**
	(0.022)	(0.030)	(0.019)	(0.008)
Mean of Dep. Var.	30.9	12.8	27.6	0.77
Observations	359,609	357,190	355,427	691,622
N of MMSAs	203	203	203	225

Notes: Estimation sample consists of respondents in ever-treated MMSAs in BRFSS SMART data. Columns 1 to 3 present Poisson estimates of UberX's impact on the noted outcome. Note that these survey questions are only asked every other year. Column 4 reports OLS estimates from a linear probability model where the outcome variable is an indicator for having completed any exercise (outside of activities at the respondent's job) in the past 30 days. In the first panel of the table, we present estimates for our main sample. We then present analyses of the same outcomes for various relevant sub-samples. In all specifications, we include MMSA fixed effects, year fixed effects, individual and MMSA-level covariates, and weight observations using the BRFSS-provided survey weights. Standard errors are robust to clustering at the MMSA level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B6
BRFSS estimates – omitting largest cities.

	(1) Drinking days	(2) Avg. drinks per D-day	(3) Total drinks	(4) Max drinks one occasion	(5) Binge drinking instances	(6) Any Alcohol? (LPM)
Panel A: Main estimates (Poisson)						
UberX	0.027**	0.036**	0.054**	0.043***	0.051	0.008**
	(0.012)	(0.016)	(0.026)	(0.014)	(0.037)	(0.004)
Panel B: Omit New York City MSA						
UberX	0.026*	0.028	0.047	0.042**	0.080*	0.010**
	(0.014)	(0.018)	(0.029)	(0.018)	(0.043)	(0.004)
Panel C: Omit San Francisco MSA						
UberX	0.027**	0.034**	0.056**	0.044***	0.055	0.007*
	(0.012)	(0.017)	(0.027)	(0.014)	(0.038)	(0.004)
Panel D: Omit Los Angeles MSA						
UberX	0.021*	0.035**	0.047*	0.037***	0.034	0.006*
	(0.011)	(0.017)	(0.027)	(0.013)	(0.033)	(0.004)
Panel E: Omit Chicago MSA						
UberX	0.024**	0.036**	0.049*	0.040***	0.040	0.008**
	(0.012)	(0.017)	(0.027)	(0.014)	(0.036)	(0.004)
Panel F: Omit Dallas-Fort Worth MSA						
UberX	0.025**	0.035**	0.052*	0.043***	0.055	0.007*
	(0.012)	(0.017)	(0.027)	(0.014)	(0.038)	(0.004)
Panel G: Omit Houston MSA						
UberX	0.030**	0.045***	0.071***	0.051***	0.072*	0.008*
	(0.012)	(0.015)	(0.023)	(0.013)	(0.038)	(0.004)
Panel H: Omit Washington, DC MSA						
UberX	0.029**	0.038**	0.059**	0.044***	0.050	0.008**
	(0.012)	(0.016)	(0.026)	(0.014)	(0.037)	(0.004)
Panel I: Omit All of the Above MSAs						
UberX	0.024*	0.037*	0.059**	0.042**	0.094**	0.009*
	(0.014)	(0.019)	(0.024)	(0.017)	(0.038)	(0.005)

Notes: Our estimation sample consists of individual respondents aged 21 to 64 in ever-treated MMSAs in the BRFSS SMART data unless noted otherwise. Because we estimate linear probability models for "Any Alcohol?", the coefficient 0.008 in Panel A should be interpreted as a 0.8 percentage point increase when UberX enters. We estimate the effect on the remaining outcomes (all count variables) using Poisson models, meaning that those coefficients approximate percentage changes. For example, the estimate of 0.027 in Panel A column 1 reflects a roughly 2.7% increase in the number of drinking days. In all specifications, we include MMSA fixed effects, year fixed effects, individual and MMSA-level covariates, and weight observations using BRFSS-provided survey weights. Standard errors are robust to clustering at the MMSA level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B7
QCEW estimates – omitting largest cities.

	(1)	(2)	(3)	(4)	(5)	(6)
	Drinking places			Full-service restaurants		
	Log employment per 1000	Log tot. earnings per 1000	Log avg wkly earnings	Log employment per 1000	Log tot. earnings per 1000	Log avg wkly earnings
Panel A: Main estimates						
UberX	0.035***	0.037***	0.003	0.012**	0.006	−0.006**
	(0.010)	(0.012)	(0.004)	(0.005)	(0.005)	(0.003)
Panel B: Omit New York City MSA						
UberX	0.027**	0.029**	0.002	0.008**	0.002	−0.006*
	(0.011)	(0.013)	(0.005)	(0.004)	(0.003)	(0.003)
Panel C: Omit San Francisco MSA						
UberX	0.036***	0.038***	0.002	0.011**	0.006	−0.005*
	(0.011)	(0.012)	(0.004)	(0.005)	(0.005)	(0.003)
Panel D: Omit Los Angeles MSA						
UberX	0.036***	0.039***	0.003	0.010*	0.006	−0.004*
	(0.011)	(0.012)	(0.004)	(0.006)	(0.005)	(0.002)
Panel E: Omit Chicago MSA						
UberX	0.036***	0.038***	0.001	0.013***	0.007	−0.006**
	(0.010)	(0.012)	(0.004)	(0.005)	(0.005)	(0.003)
Panel F: Omit Dallas-Fort Worth MSA						
UberX	0.035***	0.039***	0.004	0.012**	0.007	−0.005*
	(0.010)	(0.012)	(0.004)	(0.005)	(0.005)	(0.003)
Panel G: Omit Houston MSA						
UberX	0.034***	0.034***	0.001	0.013**	0.007	−0.007**
	(0.011)	(0.013)	(0.004)	(0.005)	(0.005)	(0.003)
Panel H: Omit Washington, DC MSA						
UberX	0.035***	0.037***	0.002	0.012**	0.007	−0.005**
	(0.011)	(0.012)	(0.004)	(0.005)	(0.005)	(0.003)
Panel I: Omit All of the Above MSAs						
UberX	0.026*	0.024	−0.002	0.008***	0.005	−0.003
	(0.014)	(0.016)	(0.005)	(0.003)	(0.003)	(0.002)

Notes: Estimation sample consists of all treated MMSAs in the QCEW data. Dependent variables are in logs, observations are at the county-quarter level. We estimate all specifications using OLS. Unless noted otherwise, we include county fixed effects, quarter-year fixed effects, county-level covariates, and weight observations using county population. Standard errors are robust to clustering at the MMSA level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B8
UberX entry and health outcomes.

	(1) Very good gen. health rating	(2) Depressive disorder	(3) Smoker	(4) Smoked in past 3 months	(5) Poor mental health days	(6) Poor physical health days	(7) Days disrupted by poor health
Panel A: Main estimation sample (ages 21–64)							
UberX	−0.002	0.007*	−0.001	−0.003	0.007	−0.003	0.014
	(0.005)	(0.004)	(0.003)	(0.004)	(0.018)	(0.022)	(0.021)
Mean of DV	0.55	0.17	0.18	0.20	3.87	3.43	2.32
Observations	1,496,621	1,180,437	1,460,296	1,123,732	1,480,740	1,480,114	1,488,386
N of MMSAs	225	212	225	212	225	225	225
Panel B: Students (any age)							
UberX	−0.034	0.030***	−0.001	0.001	0.061	0.009	−0.019
	(0.022)	(0.011)	(0.009)	(0.010)	(0.082)	(0.154)	(0.083)
Mean of DV	0.65	0.15	0.10	0.11	4.20	2.13	1.53
Observations	57,106	49,129	55,653	47,353	56,546	56,529	56,850
N of MMSAs	225	212	225	212	225	224	222
Panel C: Males							
UberX	−0.008	0.004	0.005	−0.000	0.047**	0.012	0.015
	(0.008)	(0.004)	(0.004)	(0.006)	(0.024)	(0.028)	(0.035)
Mean of DV	0.55	0.13	0.21	0.23	3.28	3.11	2.10
Observations	633,421	510,609	616,696	486,534	626,957	626,925	630,284
N of MMSAs	225	212	225	212	225	225	225
Panel D: Ages 21–34							
UberX	−0.002	0.010	−0.002	−0.008	0.040	−0.005	0.045
	(0.008)	(0.006)	(0.007)	(0.007)	(0.034)	(0.049)	(0.047)
Mean of DV	0.60	0.16	0.20	0.23	4.10	2.39	1.70
Observations	297,065	249,519	288,265	238,520	294,378	294,348	296,008
N of MMSAs	225	212	225	212	225	225	225

Notes: Estimation sample consists of respondents in ever-treated MMSAs in BRFSS SMART data. We present OLS estimates (linear probability models) in columns 1–4. The outcome in column 1 is a binary variable equal to one if an individual reports being in very good or excellent general health (i.e., a rating of 1 or 2 on a scale of 1 to 5). The outcomes in columns 2–4 are also binary, so the coefficients in columns 1–4 reflect percentage point changes. Columns 5–7 present coefficients from Poisson models, and thus approximate percentage changes. Because of changes in the survey questions, columns 2 and 4 lack data from years 2009–2010. In all specifications, we include MMSA fixed effects, year fixed effects, individual and MMSA-level covariates, and weight observations using the BRFSS-provided survey weights. Standard errors are robust to clustering at the MMSA level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Appendix C. UberX rollout and data availability maps for BRFSS and QCEW

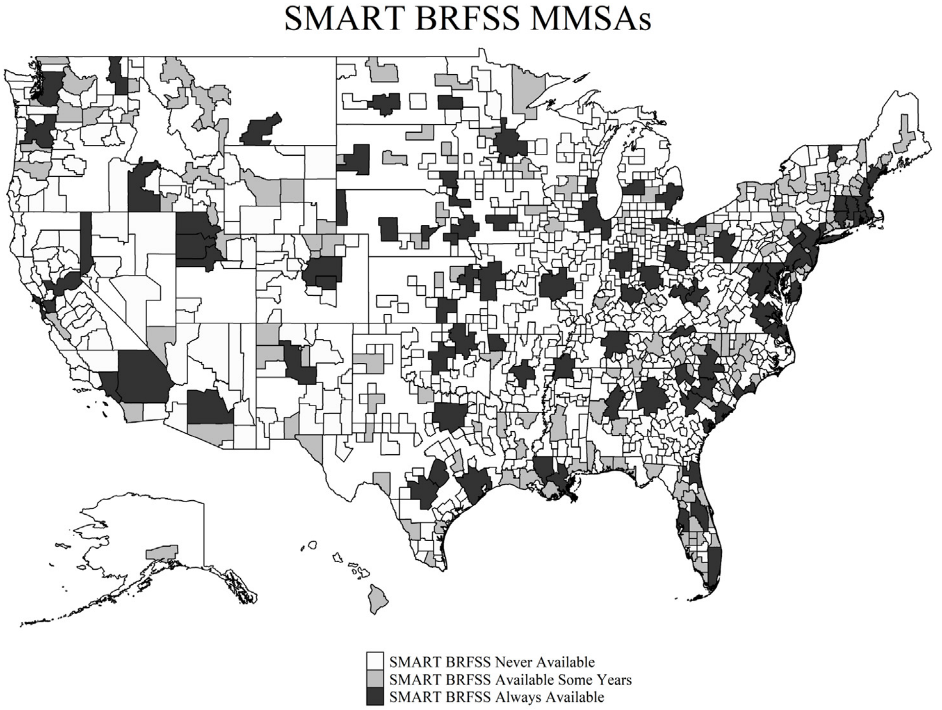
In Fig. 1, in the main body of the paper, we present a series of maps that illustrate Uber's geographic rollout by year from 2012 to 2017. Each map presents the continental United States plus Hawaii and Alaska (not to scale). In every year, we plot the outline of all metro areas. Areas that Uber entered in a given year are dark gray and areas that do not have Uber are light gray. Last, we color areas that Uber entered in a prior year a medium gray. In this appendix section, we also provide maps for BRFSS and QCEW data availability, overall Uber entry by the end of 2017, and TCI values by MMSA.

We present BRFSS SMART data availability across the sample period in Fig. C1. Recall that BRFSS SMART data are only available for a metro area when there are more than 500 survey responses for that area. Those areas where BRFSS SMART data are available for every year from 2009 to 2017 are colored dark gray. Areas where BRFSS SMART data are sometimes available are colored lighter gray. Areas outside a defined metro or where BRFSS SMART data are never available are colored white. Note that there are a handful of metro areas where there is variation in Uber entry timing across divisions within the metro area. We do not, however, attempt to plot those MMSAs that correspond to such CBSA divisions (MMSA is the term BRFSS uses to collectively refer to CBSAs and CBSA divisions). Variation at the sub-CBSA level would be difficult to see on a map at this scale. Instead, we plot only the date of first Uber entry within the metro area.

In the second map in Fig. C1, we present overall Uber availability by the end of 2017 for ease of comparison. For completeness, we provide Uber entry dates by BRFSS MMSA from 2012 to 2017 in Tables C1–C5. Because Uber sometimes began offering rides in an area but then had to cease operating for legal or other reasons, we also provide the date Uber stopped providing service. If they subsequently resumed service, we provide the date of that resumption. We add Uber entry dates for areas that Uber enters that appear in our QCEW data but not in BRFSS data in Tables C6 through C7.

In Fig. C2 we present QCEW data availability for drinking places and full service restaurants by county. Note that QCEW data disclosure is incomplete in some year-quarters to preserve establishment/employer anonymity. Counties where QCEW data is available in every year-quarter throughout the sample period are colored dark gray. Counties where QCEW data is sometimes available are colored gray. Counties where QCEW data are never available are colored white.

In Fig. C3 we provide a map of Transit Connectivity Index (TCI) values. In the map, those areas shaded dark gray are in the top quartile of TCI values. Those metros in the next quartile are a lighter gray, and so on.



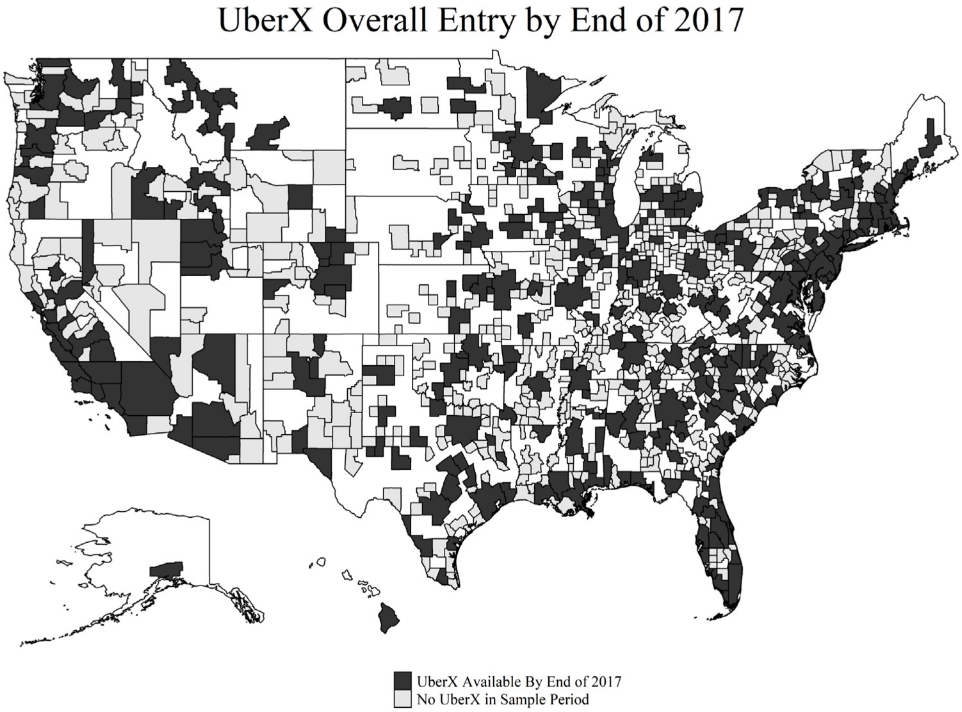
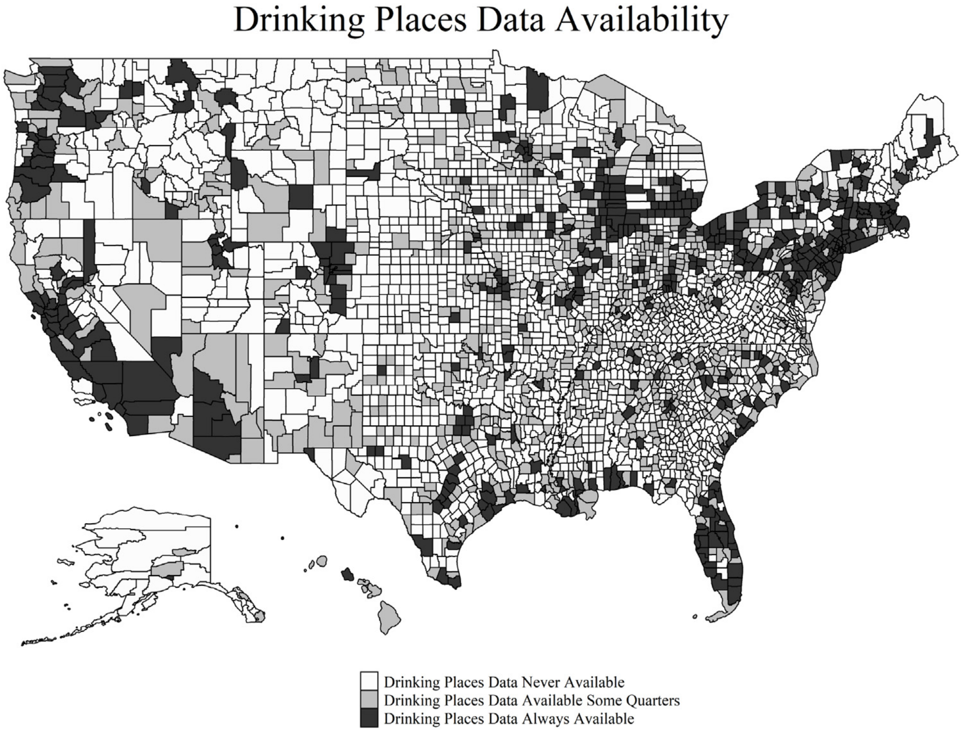


Fig. C1. BRFSS SMART and Uber MMSAs. Notes: Maps present the continental United States along with Hawaii and Alaska (not to scale). We also plot the outline of all states and CBSAs. In the first map, those areas where BRFSS SMART data are available throughout the 2009–2017 sample period are colored dark gray. BRFSS SMART Data are only available for an MMSA when there are more than 500 survey responses for that area. Areas where BRFSS SMART data are sometimes available are colored a lighter gray. Areas where BRFSS SMART data are never available are colored white. In the second map, we present Uber availability by the end of 2017 for ease of comparison. Note that we do not attempt to plot CBSA divisions, which are sub-components of CBSAs. There are a handful of CBSAs where there is variation in Uber entry timing across divisions within a metro. Because such variation would be difficult to see on a map at this scale, we plot only the date of first Uber entry within the metro area for each area. For completeness, we provide a table of all Uber entry dates by MMSA area (MMSAs are how BRFSS collectively refers to CBSAs and CBSA divisions) in Tables C1–C5.



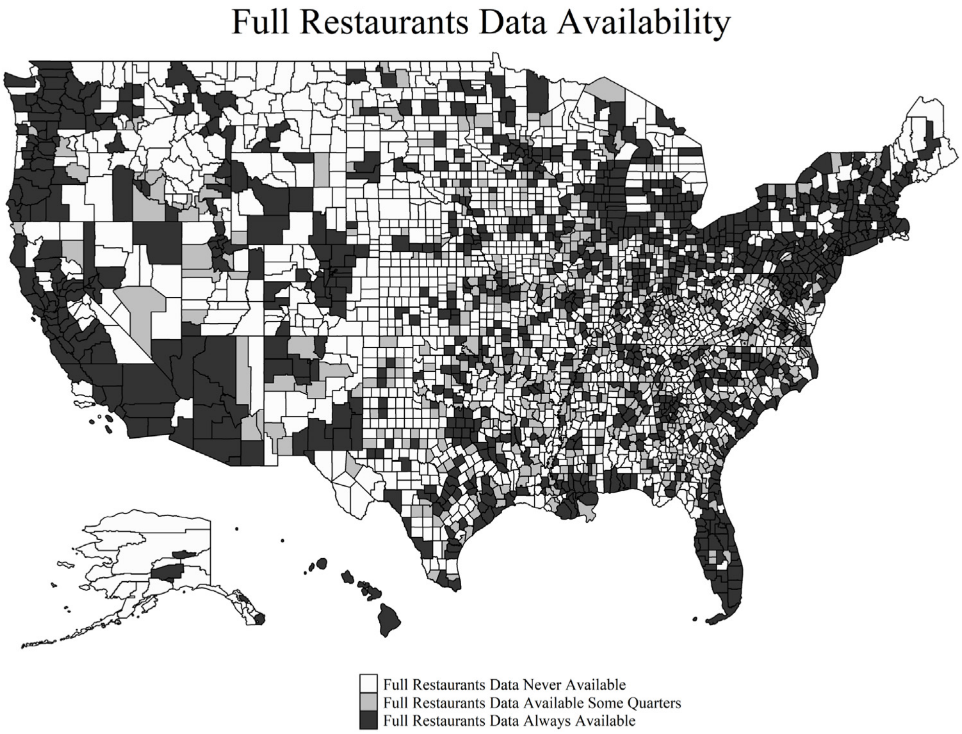


Fig. C2. QCEW county data on drinking places and restaurants. Notes: Maps present the continental United States along with Hawaii and Alaska (not to scale). We also plot the outline of all U.S. states and counties. QCEW data disclosure is incomplete in some year-quarters to preserve establishment/employer anonymity. Counties where QCEW data is always available throughout the sample period are colored dark gray. Counties where QCEW data is sometimes available are colored lighter gray. Counties where QCEW data are never available are colored white.

Table C1
BRFSS UberX entry and exit dates, part 1.

MMSA	MMSA name	1st UberX entry	1st UberX exit	2nd UberX entry
35644	New York-White Plains-Wayne, NY-NJ	24-Aug-12		
41860	Oakland-Fremont-Hayward, CA	18-Jan-13		
31080	Los Angeles-Long Beach-Glendale, CA	14-Mar-13		
42644	Seattle-Bellevue-Everett, WA	11-Apr-13		
16980	Chicago-Naperville-Joliet, IL-IN-WI	22-Apr-13		
41740	San Diego-Carlsbad-San Marcos, CA	9-May-13		
14484	Boston-Quincy, MA	4-Jun-13		
15764	Peabody, MA	4-Jun-13		
40484	Rockingham County-Strafford County, NH	4-Jun-13		
12060	Atlanta-Sandy Springs-Marietta, GA	26-Jun-13		
35004	Nassau-Suffolk, NY	1-Jul-13	5-Jun-15	29-Jun-17
41940	San Jose-Sunnyvale-Santa Clara, CA	24-Jul-13		
13644	Bethesda-Frederick-Gaithersburg, MD	8-Aug-13		
47894	Washington-Arlington-Alexandria, DC-VA-MD-WV	8-Aug-13		
33460	Minneapolis-St. Paul-Bloomington, MN-WI	4-Sep-13		
26900	Indianapolis-Carmel, IN	5-Sep-13		
38060	Phoenix-Mesa-Scottsdale, AZ	5-Sep-13		
39300	Providence-New Bedford-Fall River, RI-MA	12-Sep-13		
16740	Charlotte-Gastonia-Concord, NC-SC	27-Sep-13		
40900	Sacramento-Arden-Arcade-Roseville, CA	30-Sep-13		
19740	Denver-Aurora, CO	4-Oct-13		
46060	Tucson, AZ	10-Oct-13		
36420	Oklahoma City, OK	30-Oct-13		
19820	Detroit-Livonia-Dearborn, MI	31-Oct-13		
19124	Dallas-Plano-Irving, TX	5-Nov-13		
12580	Baltimore-Towson, MD	6-Nov-13		
35084	Newark-Union, NJ-PA	13-Nov-13		
45940	Trenton-Ewing, NJ	13-Nov-13		
34980	Nashville-Davidson-Murfreesboro-Franklin, TN	10-Dec-13		
26420	Houston-Sugar Land-Baytown, TX	21-Feb-14	1-Feb-16	29-May-17
18140	Columbus, OH	25-Feb-14		
31540	Madison, WI	6-Mar-14		
38300	Pittsburgh, PA	13-Mar-14		
17140	Cincinnati-Middletown, OH-KY-IN	27-Mar-14		
46140	Tulsa, OK	27-Mar-14		
33340	Milwaukee-Waukesha-West Allis, WI	28-Mar-14		
41700	San Antonio, TX	28-Mar-14	1-Apr-15	13-Oct-15
14860	Bridgeport-Stamford-Norwalk, CT	1-Apr-14		
15804	Camden, NJ	1-Apr-14		
47220	Vineland-Bridgeton, NJ	1-Apr-14		
40140	Riverside-San Bernardino-Ontario, CA	3-Apr-14		
17460	Cleveland-Elyria-Mentor, OH	8-Apr-14		
45104	Tacoma, WA	8-Apr-14		
45300	Tampa-St. Petersburg-Clearwater, FL	11-Apr-14		
31140	Louisville/Jefferson County, KY-IN	24-Apr-14		
32820	Memphis, TN-MS-AR	24-Apr-14		
35300	New Haven-Milford, CT	24-Apr-14		
39580	Raleigh-Cary, NC	26-Apr-14		
10740	Albuquerque, NM	30-Apr-14		
47260	Virginia Beach-Norfolk-Newport News, VA-NC	1-May-14		
17820	Colorado Springs, CO	2-May-14		
18580	Corpus Christi, TX	2-May-14	13-Mar-16	27-Jun-19
23104	Fort Worth-Arlington, TX	5-May-14		
27260	Jacksonville, FL	5-May-14		

Table C2
BRFSS UberX entry and exit dates, part 2.

MMSA	MMSA name	1st UberX entry	1st UberX exit	2nd UberX entry
36540	Omaha-Council Bluffs, NE-IA	5-May-14		
44060	Spokane, WA	8-May-14		
28140	Kansas City, MO-KS	9-May-14	5-May-15	22-May-15
41620	Salt Lake City, UT	27-May-14		
12420	Austin-Round Rock, TX	4-Jun-14	9-May-16	29-May-17
33100	Miami-Fort Lauderdale-Pompano Beach, FL	4-Jun-14		
36740	Orlando-Kissimmee, FL	4-Jun-14		
46520	Honolulu, HI	12-Jun-14		
30460	Lexington-Fayette, KY	13-Jun-14		
45780	Toledo, OH	13-Jun-14		
20500	Durham, NC	26-Jun-14		
21340	El Paso, TX	26-Jun-14		
22180	Fayetteville, NC	26-Jun-14		
24660	Greensboro-High Point, NC	26-Jun-14		
31180	Lubbock, TX	26-Jun-14		
48900	Wilmington, NC	26-Jun-14		
49180	Winston-Salem, NC	26-Jun-14		
12100	Atlantic City-Hammonton, NJ	27-Jun-14		
36140	Ocean City, NJ	27-Jun-14		
16700	Charleston-North Charleston-Summerville, SC	10-Jul-14		
17900	Columbia, SC	10-Jul-14		
24860	Greenville-Mauldin-Easley, SC	10-Jul-14		
34820	Myrtle Beach-North Myrtle Beach-Conway, SC	10-Jul-14		
38900	Portland-Vancouver-Beaverton, OR-WA	10-Jul-14	21-Dec-14	24-Apr-15
12940	Baton Rouge, LA	11-Jul-14		
11100	Amarillo, TX	16-Jul-14		
25540	Hartford-West Hartford-East Hartford, CT	22-Jul-14		
21660	Eugene-Springfield, OR	23-Jul-14	5-Apr-15	1-Sep-18
41420	Salem, OR	23-Jul-14		
24340	Grand Rapids-Wyoming, MI	24-Jul-14		
29620	Lansing-East Lansing, MI	24-Jul-14		
24540	Greeley, CO	1-Aug-14		
40060	Richmond, VA	6-Aug-14		
11700	Asheville, NC	21-Aug-14		
15940	Canton-Massillon, OH	22-Aug-14		
14500	Boulder, CO	27-Aug-14		
22660	Fort Collins-Loveland, CO	27-Aug-14		
10420	Akron, OH	28-Aug-14		
17780	College Station-Bryan, TX	28-Aug-14		
19380	Dayton, OH	28-Aug-14		
22220	Fayetteville-Springdale-Rogers, AR-MO	28-Aug-14		
23540	Gainesville, FL	28-Aug-14		
28940	Knoxville, TN	28-Aug-14		
30700	Lincoln, NE	28-Aug-14		
43780	South Bend-Mishawaka, IN-MI	28-Aug-14		
45220	Tallahassee, FL	28-Aug-14		
46220	Tuscaloosa, AL	28-Aug-14	8-Oct-14	18-Aug-16
48620	Wichita, KS	28-Aug-14	5-May-15	22-May-15
19780	Des Moines-West Des Moines, IA	12-Sep-14		
11260	Anchorage, AK	18-Sep-14	6-Mar-15	16-Jun-17
14260	Boise City-Nampa, ID	2-Oct-14		
38860	Portland-South Portland-Biddeford, ME	2-Oct-14		
49340	Worcester, MA	6-Oct-14		
15540	Burlington-South Burlington, VT	9-Oct-14		
31700	Manchester-Nashua, NH	17-Oct-14		
29820	Las Vegas-Paradise, NV	24-Oct-14	25-Nov-14	15-Sep-15
37964	Philadelphia, PA	24-Oct-14		

Table C3
BRFSS UberX entry and exit dates, part 3.

MMSA	MMSA name	1st UberX entry	1st UberX exit	2nd UberX entry
39900	Reno-Sparks, NV	24-Oct-14	25-Nov-14	15-Sep-15
30780	Little Rock-North Little Rock-Conway, AR	6-Nov-14		
40220	Roanoke, VA	6-Nov-14		
16860	Chattanooga, TN-GA	13-Nov-14		
42140	Santa Fe, NM	19-Nov-14		
15980	Cape Coral-Fort Myers, FL	4-Dec-14		
16300	Cedar Rapids, IA	4-Dec-14		
18880	Crestview-Fort Walton Beach-Destin, FL	4-Dec-14		
19660	Deltona-Daytona Beach-Ormond Beach, FL	4-Dec-14		
28580	Key West, FL	4-Dec-14	31-Jul-15	1-Jul-17
29460	Lakeland-Winter Haven, FL	4-Dec-14		
34940	Naples-Marco Island, FL	4-Dec-14		
35840	North Port-Bradenton-Sarasota, FL	4-Dec-14		
36100	Ocala, FL	4-Dec-14		
37340	Palm Bay-Melbourne-Titusville, FL	4-Dec-14		
37460	Panama City-Lynn Haven-Panama City Beach, FL	4-Dec-14	1-Mar-15	10-Mar-17
37860	Pensacola-Ferry Pass-Brent, FL	4-Dec-14		
38940	Port St. Lucie, FL	4-Dec-14		
27140	Jackson, MS	11-Dec-14		
27980	Kahului-Wailuku, HI	18-Dec-14		
25420	Harrisburg-Carlisle, PA	29-Jan-15		
10900	Allentown-Bethlehem-Easton, PA-NJ	30-Jan-15		
29180	Lafayette, LA	30-Jan-15		
42540	Scranton-Wilkes-Barre, PA	6-Feb-15		
29740	Las Cruces, NM	18-Feb-15		
25940	Hilton Head Island-Beaufort, SC	27-Mar-15		
12260	Augusta-Richmond County, GA-SC	6-Apr-15		
35380	New Orleans-Metairie-Kenner, LA	16-Apr-15		
29940	Lawrence, KS	23-Apr-15	5-May-15	22-May-15
31740	Manhattan, KS	23-Apr-15	5-May-15	22-May-15
45820	Topeka, KS	23-Apr-15	5-May-15	22-May-15
44140	Springfield, MA	24-Apr-15		
41540	Seaford, DE	27-Apr-15		
23060	Fort Wayne, IN	7-May-15		
22020	Fargo, ND-MN	12-May-15		
12700	Barnstable Town, MA	22-May-15		
12300	Augusta-Waterville, ME	25-May-15		
17660	Coeur d'Alene, ID	4-Jun-15		
26820	Idaho Falls, ID	4-Jun-15		
46300	Twin Falls, ID	4-Jun-15		
33660	Mobile, AL	11-Jun-15		
48864	Wilmington, DE-MD-NJ	11-Jun-15		
43900	Spartanburg, SC	16-Jul-15		
19340	Davenport-Moline-Rock Island, IA-IL	21-Jul-15		
20100	Dover, DE	31-Jul-15		
33260	Midland, TX	12-Aug-15	1-Feb-16	2-Jun-16
22900	Fort Smith, AR-OK	1-Sep-15		
39340	Provo-Orem, UT	3-Sep-15		
41180	St. Louis, MO-IL	18-Sep-15		
14740	Bremerton-Silverdale, WA	30-Sep-15		
19060	Cumberland, MD-WV	20-Oct-15		
25180	Hagerstown-Martinsburg, MD-WV	20-Oct-15		
13380	Bellingham, WA	11-Nov-15		
36260	Ogden-Clearfield, UT	18-Dec-15		
13820	Birmingham-Hoover, AL	28-Dec-15		
24260	Grand Island, NE	1-Feb-16		
33860	Montgomery, AL	4-Feb-16		

Table C4
BRFSS UberX entry and exit dates, part 4.

MMSA	MMSA name	1st UberX entry	1st UberX exit	2nd UberX entry
26620	Huntsville, AL	4-Mar-16		
30340	Lewiston-Auburn, ME	21-Mar-16		
12620	Bangor, ME	23-Mar-16		
12740	Barre, VT	26-Mar-16		
41460	Salina, KS	21-May-16		
18180	Concord, NH	1-Jun-16		
49660	Youngstown-Warren-Boardman, OH-PA	23-Jun-16		
25060	Gulfport-Biloxi, MS	1-Jul-16		
36500	Olympia, WA	15-Jul-16		
16620	Charleston, WV	19-Jul-16		
26580	Huntington-Ashland, WV-KY-OH	19-Jul-16		
28060	Kalispell, MT	1-Aug-16		
33540	Missoula, MT	1-Aug-16		
24500	Great Falls, MT	2-Aug-16		
15580	Butte-Silver Bow, MT	3-Aug-16		
25740	Helena, MT	3-Aug-16		
14580	Bozeman, MT	4-Aug-16		
13740	Billings, MT	5-Aug-16		
28700	Kingsport-Bristol-Bristol, TN-VA	19-Aug-16		
48300	Wenatchee, WA	19-Aug-16		
46340	Tyler, TX	22-Sep-16		
25860	Hickory-Lenoir-Morganton, NC	15-Dec-16		
28420	Kennewick-Pasco-Richland, WA	15-Dec-16		
49420	Yakima, WA	16-Dec-16		
40860	Rutland, VT	20-Jan-17		
21780	Evansville, IN-KY	25-Jan-17		
30860	Logan, UT-ID	1-Feb-17		
25900	Hilo, HI	1-Mar-17		
13900	Bismarck, ND	2-Mar-17		
24220	Grand Forks, ND-MN	2-Mar-17		
40340	Rochester, MN	2-Mar-17		
41060	St. Cloud, MN	2-Mar-17		
16220	Casper, WY	3-Mar-17		
16940	Cheyenne, WY	3-Mar-17		
28180	Kapaa, HI	10-Mar-17		
17200	Lebanon, NH-VT	22-Mar-17		
43580	Sioux City, IA-NE-SD	1-Apr-17		
20260	Duluth, MN-WI	1-May-17		
21820	Fairbanks, AK	21-Jun-17		
32580	McAllen-Edinburg-Mission, TX	27-Jun-17		
10580	Albany-Schenectady-Troy, NY	29-Jun-17		
13780	Binghamton, NY	29-Jun-17		
15380	Buffalo-Niagara Falls, NY	29-Jun-17		
24020	Glens Falls, NY	29-Jun-17		
40380	Rochester, NY	29-Jun-17		
45060	Syracuse, NY	29-Jun-17		
46540	Utica-Rome, NY	29-Jun-17		
29700	Laredo, TX	12-Jul-17		
48660	Wichita Falls, TX	22-Aug-17		
33740	Monroe, LA	20-Sep-17		
35740	Norfolk, NE	23-Sep-17		
22500	Florence, SC	8-Dec-17		
13220	Beckley, WV	24-Dec-17		
43340	Shreveport-Bossier City, LA	15-Feb-18		
10100	Aberdeen, SD	20-Jun-19		
39660	Rapid City, SD	20-Jun-19		
43620	Sioux Falls, SD	20-Jun-19		

Table C5
BRFSS never-UberX or treatment unclear.

MMSA	MMSA name
10780	Alexandria, LA
11580	Arcadia, FL
13620	Berlin, NH-VT
15100	Brookings, SD
18100	Columbus, NE
19620	Del Rio, TX
22140	Farmington, NM
23700	Gallup, NM
25580	Hastings, NE
25720	Heber, UT
26140	Homosassa Springs, FL
26380	Houma-Bayou Cane-Thibodaux, LA
28260	Kearney, NE
28300	Keene, NH
29060	Laconia, NH
29340	Lake Charles, LA
29380	Lake City, FL
30300	Lewiston, ID-WA
31300	Lumberton, NC
33500	Minot, ND
35820	North Platte, NE
35980	Norwich-New London, CT
36700	Orangeburg, SC
38180	Pierre, SD
40180	Riverton, WY
42380	Sayre, PA
42420	Scottsbluff, NE
42700	Sebring, FL
43940	Spearfish, SD
45860	Torrington, CT
47980	Watertown, SD
48100	Wauchula, FL

Table C6
QCEW additional UberX entry and exit dates, part 1.

MMSA	MMSA name	1st UberX entry	1st UberX exit	2nd UberX entry
41500	Salinas, CA	4-Feb-14		
42100	Santa Cruz-Watsonville, CA	4-Feb-14		
23420	Fresno, CA	5-Feb-14		
33700	Modesto, CA	2-Apr-14		
11460	Ann Arbor, MI	22-Apr-14		
42220	Santa Rosa-Petaluma, CA	12-May-14		
12540	Bakersfield-Delano, CA	13-Jun-14		
37100	Oxnard-Thousand Oaks-Ventura, CA	17-Jul-14		
42020	San Luis Obispo-Paso Robles, CA	17-Jul-14		
22420	Flint, MI	24-Jul-14		
28020	Kalamazoo-Portage, MI	24-Jul-14		
12020	Athens-Clarke County, GA	28-Aug-14		
12220	Auburn-Opelika, AL	28-Aug-14	15-Jan-15	16-Aug-16
13980	Blacksburg-Christiansburg-Radford, VA	28-Aug-14		
14020	Bloomington, IN	28-Aug-14		
16820	Charlottesville, VA	28-Aug-14		
37060	Oxford, MS	28-Aug-14	23-Oct-14	1-Jul-16
47380	Waco, TX	28-Aug-14		
22380	Flagstaff, AZ	18-Sep-14		
17860	Columbia, MO	9-Oct-14		
24580	Green Bay, WI	16-Oct-14		
16180	Carson City, NV	24-Oct-14	25-Nov-14	15-Sep-15
47300	Visalia-Porterville, CA	1-Dec-14		
44100	Springfield, IL	9-Jan-15		
44300	State College, PA	6-Feb-15		
44660	Stillwater, OK	12-Feb-15		
14060	Bloomington-Normal, IL	15-Feb-15		
16580	Champaign-Urbana, IL	15-Feb-15		

Table C6 (*Continued*)

MMSA	MMSA name	1st UberX entry	1st UberX exit	2nd UberX entry
40420	Rockford, IL	15-Feb-15		
29540	Lancaster, PA	27-Mar-15		
39740	Reading, PA	27-Mar-15		
42340	Savannah, GA	27-Mar-15		
49620	York-Hanover, PA	27-Mar-15		
21500	Erie, PA	10-Apr-15		
39540	Racine, WI	21-May-15		
28620	Kill Devil Hills, NC	22-May-15		
38540	Pocatello, ID	4-Jun-15		
49740	Yuma, AZ	12-Jun-15		
28660	Killeen-Temple-Fort Hood, TX	2-Jul-15		
11180	Ames, IA	3-Aug-15		
10180	Abilene, TX	12-Aug-15		
36220	Odessa, TX	12-Aug-15		
14540	Bowling Green, KY	27-Aug-15		
11540	Appleton, WI	10-Sep-15		
24300	Grand Junction, CO	6-Oct-15		
17020	Chico, CA	8-Oct-15		
25500	Harrisonburg, VA	23-Oct-15		
15260	Brunswick, GA	6-Nov-15		
37900	Peoria, IL	24-Nov-15		
22540	Fond du Lac, WI	25-Nov-15		
27500	Janesville, WI	25-Nov-15		
36780	Oshkosh-Neenah, WI	25-Nov-15		
45340	Taos, NM	22-Dec-15		
13140	Beaumont-Port Arthur, TX	3-Feb-16		
17300	Clarksville, TN-KY	3-Mar-16		
26980	Iowa City, IA	28-Apr-16		

Table C7
QCEW additional UberX entry and exit dates, part 2.

MMSA	MMSA name	1st UberX entry	1st UberX exit	2nd UberX entry
31420	Macon, GA	10-May-16		
47580	Warner Robins, GA	10-May-16		
25620	Hattiesburg, MS	1-Jul-16		
27740	Johnson City, TN	19-Aug-16		
17980	Columbus, GA-AL	20-Sep-16		
44180	Springfield, MO	17-Nov-16		
14380	Boone, NC	13-Jan-17		
45460	Terre Haute, IN	28-Feb-17		
27100	Jackson, MI	1-Mar-17		
16060	Carbondale, IL	2-Mar-17		
20740	Eau Claire, WI	2-Mar-17		
29100	La Crosse, WI-MN	2-Mar-17		
31860	Mankato-North Mankato, MN	2-Mar-17		
40980	Saginaw-Saginaw Township North, MI	2-Mar-17		
48140	Wausau, WI	2-Mar-17		
30020	Lawton, OK	22-Mar-17		
20220	Dubuque, IA	1-Apr-17		
47940	Waterloo-Cedar Falls, IA	1-Apr-17		
10500	Albany, GA	17-May-17		
46660	Valdosta, GA	17-May-17		
27940	Juneau, AK	19-Jun-17		
21300	Elmira, NY	29-Jun-17		
27060	Ithaca, NY	29-Jun-17		
39100	Poughkeepsie-Newburgh-Middletown, NY	29-Jun-17		
20580	Eagle Pass, TX	13-Jul-17		
27860	Jonesboro, AR	1-Aug-17		
41660	San Angelo, TX	4-Aug-17		
18060	Columbus, MS	18-Aug-17		
32940	Meridian, MS	18-Aug-17		
44260	Starkville, MS	18-Aug-17		
18700	Corvallis, OR	20-Sep-17		
45500	Texarkana, TX-Texarkana, AR	26-Sep-17		
34860	Nacogdoches, TX	27-Sep-17		
39420	Pullman, WA	29-Sep-17		
45900	Traverse City, MI	17-Oct-17		
32780	Medford, OR	1-Dec-17		

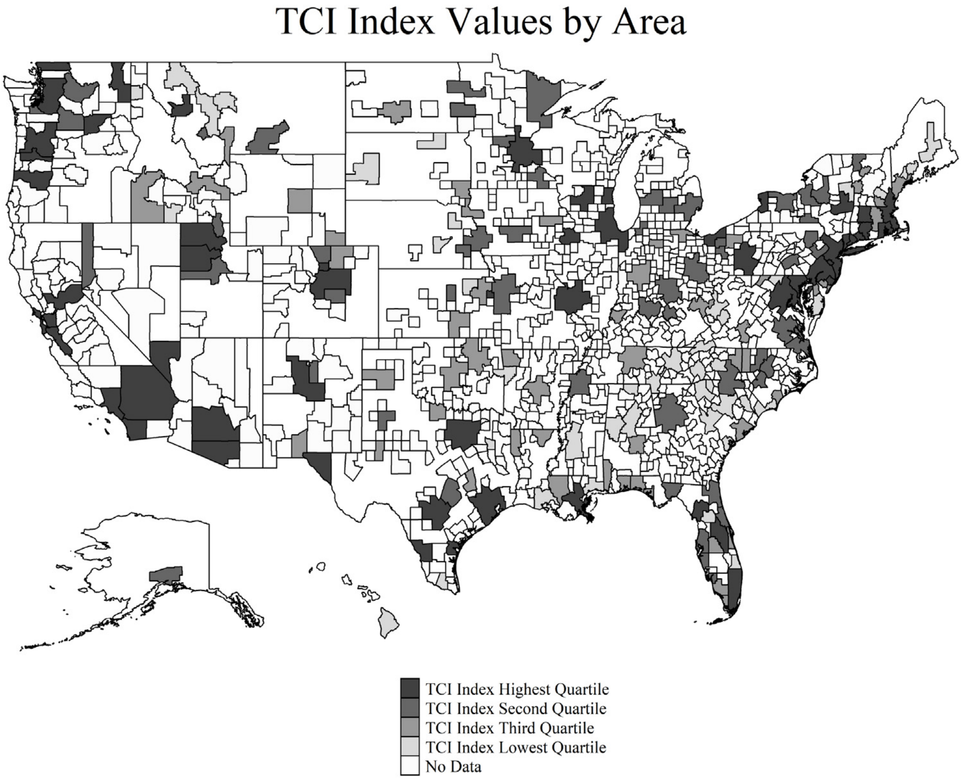


Fig. C3. Transit Connectivity Index values map. Notes: Map presents the continental United States along with Hawaii and Alaska (not to scale). We also plot the outline of all U.S. states and CBSAs. We then plot CBSAs in the highest quartile of TCI connectivity scores dark gray, and each subsequent quartile of the TCI scores a progressively lighter shade of gray. We have TCI Index Scores for all 225 ever-treated MMSAs. Areas for which we do not have TCI data are colored white.

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