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New York City Drunk Driving After Uber
Jessica Lynn Peck
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ABSTRACT

This study investigates the effect of the introduction of Uber in New York City in May 2011 on drunk-driving. A difference-in-differences estimation of this effect implies a 25-35% decrease in the alcohol-related collision rate for the affected New York City boroughs, or about 40 collisions per month. With differentiated treatment effects for each effected county, the difference-in-differences effect is higher for Manhattan, average for the Bronx and Brooklyn, and lower for Queens. A synthetic control analysis shows pronounced effects over time in the Bronx and Brooklyn, and a permutation test confirms the effect is not commonly reproducible using untreated counties.

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Despite fifty years of new traffic policies and research, intoxicated driving is still a leading cause of death and injury in the United States. Intoxicated crashes accounted for 31% of road fatalities in the United States in 2014.[1] Most policy efforts to combat drunk driving have been punitive, impeding access to alcohol or increasing drunk driving's cost for violators. For example, state laws raised and then standardized the drinking age at 21 in the mid 1980s across U.S. states. "Dry counties" and "blue laws" restricted the sale of alcohol based on place or time, though research in health economics has demonstrated that banning the sale of alcohol in a county does not necessarily suppress drunk driving.[2] Alcohol price increases through excise taxes increased the cost of drinking, decreasing alcohol consumption and by extension, drunk driving.[3] State laws sought to define the line between sober and intoxicated driving by imposing differential penalties at higher blood alcohol content (BAC) levels, and zero BAC laws for minors.[4] Some state laws instituted immediate license suspension, or differential license suspension for those caught driving under the influence of alcohol or drugs.[5] Lawmakers failed to mandate ignition locks following research on the efficacy of physically restricting intoxicated

[1] In 2014, the FARS dataset contains 32,675 road fatalities nationwide, and 9,967 road fatalities from alcohol-related collisions. Department of Transportation (US), National Highway Traffic Safety Administration (NHTSA). *Traffic Safety Facts 2014 data: alcohol-impaired driving.* Washington, DC: NHTSA; 2015.

[2] Lovenheim and Slemrod (2010) examine the drinking-age law evasion cost of heterogeneous state drinking age laws. Dee and Evans (2001) and Carpenter and Dobkin (2011) give an overview of the effect of increasing the drinking age nationwide. Baughman et al (2001) show that allowing the sale of beer and wine may reduce the drunk driving toll in a county, though the sale of hard liquor increases drunk driving.

[3] Grossman and Saffer (1987) are the first of many authors to exploit the variation in various alcohol prices, especially through excise taxes, to measure price elasticities for youth road fatalities. Increases in alcohol prices cause decreases in alcohol consumption and binge drinking. Decreases in alcohol consumption lead to fewer adverse consequences of drinking, including drunk driving fatalities.

[4] Dee (2001) indicates that setting a 0.08 BAC as the limit between intoxicated and sober driving was effective in reducing road fatalities from drunk driving. Carpenter (2004) shows that zero tolerance (zero BAC) limits for minors produce no robust drunk driving effects for either gender.

[5] Benson and Ramussen (1999) examine several policies that potentially deter drunk-driving infractions. Evans, Neville and Graham (1991) show that an increase in putative punishment severity (rather than increase in probability of detection) produces no measurable decrease in drunk driving.

drivers' car access, though vehicle use is the resolvable variable after intoxication is certain.[6] No laws have sought to make the lawful vehicle-use alternative to drunk driving more attractive.

A recent increase in the ease and availability of alternative rides for intoxicated passengers partially explains the steep decrease in alcohol-related collisions in New York City since 2011. I examine the specific case of Uber's car service launch in New York City in May 2011, a unique example of a sudden increase in cab availability for intoxicated passengers.[7] This study draws on a dataset of all New York State alcohol-related collisions maintained by the New York State Department of Motor Vehicles from 1989 through 2013. My inference is based on the variation in Uber access across New York State counties over time and the careful choice of New York State counties that provide an appropriate control group for New York City's drunk-driving behavior.

My econometric analyses show that each of the New York City boroughs that experienced significant Uber service coverage (Manhattan, the Bronx, Brooklyn, and Queens) experienced a 25-35% decrease in its alcohol-related collision rate using difference-in-differences estimation and standard errors clustered at the county level. My most conservative estimate of this intent-to-treat (ITT) effect implies a decrease of approximately 43 crashes per month across the New York City boroughs with Uber coverage, based on pre-period collision rates and population.

[6] Coben and Larken (1999) estimate that interlock devices reduce DUI recidivism between 15% and 65% during the period of observation in their study.

[7] Uber is a service that matches cab drivers and passengers with a smart-phone app and a location-based matching algorithm. Passengers pay through the smartphone app with previously submitted credit card details, and are emailed a receipt after the ride. Drivers are summoned to the passengers GPS location unless the passenger specifies a street address. Compared to street-hail taxis or livery taxis, Uber is particularly innovative tool for an intoxicated or disoriented passenger: they need not carry or acquire sufficient cash for a taxi ride, and the ride can be summoned to a precise GPS location. A general overview of Uber's purpose circa 2011 (near the time of Uber's introduction in NYC) can be found in Wired (http://www.wired.com/2011/04/app-stars-uber/).

The main challenge to inference rests on the choice of which control group constitutes a valid counterfactual to New York City before and after the introduction of Uber. The determinants of drunk driving are not well understood, but are generally unobservable in this dataset (i.e.: probability of getting caught, cost of punishment, cost of alcohol, availability of alcohol, family or peer attitudes towards alcohol, and age of the driver) or are observable but not (or nearly not) time-varying (i.e.: number of bars or liquor stores in the county). Instead I choose control counties based on geographic location, population density, and similarity of a county's pre-Uber drunk driving rate to New York City's drunk driving rate.

Supporting the robustness of these difference-in-differences estimates, I run a placebo test by choosing a random intervention date in the pre-period. While some effects are significant at the 5% level, small changes in the specification of the estimating equation lead to lack of significance and in some cases a change in the sign of the effect. The standard difference-in-differences estimates embody an average effect over all the New York City boroughs that received Uber access. To examine the county-by-county effects, I also estimate separate treatment effects for each treated borough. I find that Uber's entrance produced larger effects for Manhattan, middling effects for Brooklyn and the Bronx, and smaller effects for Queens. This runs counter to the expected effects if the average ITT difference-in-differences estimates were evenly distributed over the population of each affected borough. This distribution suggests that Uber's launch did not simply fill in neighborhoods underserved by existing public transit.

I produce evidence of the dynamic effects on each affected borough both in in a differences-in-differences model and through synthetic control analysis. The post-Uber yearly treatment effects in a differences-in-differences framework show an effect that grows over time, which fits a consumer-learning or fleet-expansion story. Separate synthetic control estimates for

each borough similarly show a large period of decrease in drunk driving rates in the year after Uber's launch date in New York City, but those effects taper off by the end of the sample period in 2013. To test the robustness of the synthetic control estimates, I produce a permutation test (Abadie et al 2010) to show that the affected New York City boroughs experienced an extreme drop in their alcohol-related collision rate, compared to all synthetic control estimates that could be produced with any other New York State county.

While many economists have used taxi datasets to examine labor supply and industrial organization topics, this paper contributes to the drunk-driving policy literature in health economics as one of the first to examine Uber's effect on drunk driving in the cities where it operates.[8] The discussion of this effect in the context of New York City is advantageous because cab access is not novel, only heterogeneously distributed (Grynbaum 2011, Wellington 2014). This study borrows from the literature on transit choice, which typically weighs public transportation options (train, bus, subway) against driving as a joint decision with housing choice; Uber constitutes an additional transit option that is particularly suited to circumstances when an individual requires transportation while intoxicated.

1. Mechanism

Uber's New York City launch in May 2011 marked the introduction of a new transit mode that has a different collection of characteristics than the other available modes. In the context of McFadden's discrete choice models for transit demand, Uber represents a reduction in potential

[8] See Camerer et al. (1997), Chou (2002), Farber (2015), and Farber (2005) for taxi drivers' intertemporal substitution of labor hours. Frechette, Lizzeri and Saltz (2015) model the taxi market as perfectly competitive with regulation and search frictions, with drivers who behave as independent profit -maximizers with prices set by the regulator. Greenwood and Wattel (2015) examine the effect of the Uber rollout across localities in California on motor vehicle homicide.

waiting times for customers, and a potential reduction in the cost of rides over medallion taxis.[9] The Uber transit mode is also characteristically easier to use than cabs, bus, and subway for trips whose purpose is alcohol consumption or whose timeframe is late at night. McFadden characterizes urban travel demand as a derived demand stemming from the destination and purpose of the trip, implying that consumers who value Uber's transit characteristics higher will substitute to Uber rides from other transit modes after the introduction of the service in order to fulfill their trip needs (McFadden 1974, p. 304). This study does not examine the long-run decisions of the consumer with regard to travel demand and transit mode (residential location or automobile ownership) that might result from the Uber launch in New York City. Instead, this study will show the short-run effect on drunk driving outcomes given Uber access.

A few mechanics of the Uber service allow for varying wait times and prices relative to medallion taxis. The market regulator continues to cap the number of medallion street-hail taxis in New York City, but does not restrict the number of Uber drivers in the city, allowing for an increase in drivers to meet short-term demand spikes. Medallion taxi drivers must adhere to posted prices set by the regulator and may not adjust prices based on an observed increase in the quantity of cab rides demanded. In contrast, Uber ride prices may fluctuate from day to day or hour to hour. Analysis of medallion taxi trip data has verified the large dip in taxi supply at the 5 o'clock PM taxi shift change. Medallion taxi drivers transfer their cabs to the next driver at the same time that potential riders begin to leave work. Uber has taken steps to recruit off-duty drivers at peak cab demand times using the pricing mechanism.[10]

The health economics literature has not addressed a specific determinant of drunk driving: the cost of obtaining safe travel between locations, conditional on intoxication. If the regulator

[9] See McFadden (1975), Chapter 3 for a framework for consumer choice behavior applied to travel demand.
[10] See Cramer and Kruger (2016) on surge pricing and the efficiency of Uber in the context of the taxi market.

fixes the taxi fare, wait time for a customer takes on the features of a price mechanism. As street-hail taxis are one of the few markets in the developed world where suppliers and consumers must physically search to find a match and complete a transaction, customers must wait longer for a ride at high customer-demand times rather than paying higher prices (Flechette, Lizzeri, and Saltz, 2016). A smart phone app-hail system may thus increase the information that both driver and passenger have about the location of potential transaction partners. This increase of information decreases search frictions in terms of reduced wait time and effort required to secure a ride for customers.

In addition to the cost measured in wait time, Uber may also address the increased difficulty of cognitive functions after drinking. Navigation, uncertainty, and the necessity of completing arithmetic may generate higher costs when intoxicated. A rider may summon an Uber ride to her physical location using GPS rather than navigating an unknown neighborhood to hail a cab or communicating on the phone with a cab dispatcher using a street address. Updates on the GPS location of her driver may be a valuable assurance that a cab has actually been dispatch, unlike the experience with phone dispatched or street hail taxis. The rider also need not retain enough cash for a ride home through Uber or calculate tip. In this context, the May 2011 Uber launch marks the beginning of a period of differentially-lower ride costs for intoxicated consumers in terms of price, wait time, and ease-of-use.

2. Data

Data on all alcohol-related crashes in New York State are available from the New York State Department of Motor Vehicles (NYS-DMV) and Department of Transportation (NYS-

DOT).[11] For my analysis, I use data from collisions that occurred from January 2007 through July 2013. This period includes Uber's entrance into the New York market in May 2011, allows for a substantial pre-intervention period, and omits two potentially confounding entrances in the New York City transportation market that could influence the alcohol-related collision rate. The first entrance occurred in August 2013 as New York City introduced a new form of taxi medallion to serve only the "outer boroughs" of Brooklyn, Queens, the Bronx, Staten Island, and northern Manhattan. These "boro" taxis were painted green rather than the typical yellow medallion taxis, and banned from picking up in lower Manhattan, where most street-hail yellow taxi rides originate. The second entrance occurred in July 2013 as Uber launched coverage in the Hamptons (Suffolk County) for summer weekends only with a massive publicity stunt offering helicopter rides from New York City to the Hamptons. Omitting Suffolk County from the analysis does not significantly change the results, supporting the suspicion that this initial launch of coverage was slow to provide significant coverage and did not do so in July 2013.

A collision's inclusion in the NYS-DMV alcohol-related subsample implies a police officer determined that alcohol was one of the causes of the collision. Each record contains detailed information about the logistics of the collision, including the jurisdiction, but no personal information about the drivers (or passengers, pedestrians, and cyclists) involved. I transform the collision-level data into monthly alcohol-related collision counts by county. County-level aggregates and monthly frequency is preferred since the Uber launch in this study occurs at the county level (rather than state or neighborhood) in May 2011. The full estimation sample contains 4,526 observations of monthly data in 62 New York State counties, five of which are New York City boroughs, from January 2007 through July 2013.

[11] The data set obtained from NYS-DMV for this study spans January 1989 through December 2013. For my analysis, I trim the data to a historically relevant period around the entrance of Uber in New York City.

In this study I use alcohol-related collision counts to measure the effect of Uber's introduction on drunk driving. Other studies have used alcohol-related automobile fatalities from the publicly available FARS (Fatal Accident Reporting System) dataset, which includes information on alcohol involvement.[12] The set of all fatal, alcohol-related collisions is a subset of all alcohol-related collisions in New York State. Using the larger set of alcohol-related collisions provides enough observations to estimate effects using county aggregates at monthly frequency.

While the NYS-DMV data contains information about the location of the collision, it does not contain information about the home addresses of people involved in the collisions (nor does FARS). In this study, I make inferences linking the car-owning population of a county and the number of collisions in that county. It may be that the driver in a collision is not a resident of the county in which they collided. I do not make this distinction for statistical purposes: random measurement error in the dependent variable does bias the estimates of the treatment variable. This study's result may have implications for state and local government, however, so I emphasize that the outcome variable is alcohol-related collisions in a county scaled by the number of resident vehicles in that county. I do not imply that local governments' choice to let Uber operate directly reduces the health outcomes of taxpayers residing in that locality; rather I observe the frequency of collisions occurring in counties where Uber does and does not operate. This assumed connection between drivers and county populations is made similarly or left unaddressed in most of the drunk driving literature.

To gauge the potential for collisions, I use information on yearly counts of registered vehicles by county, provided by the NYS-DMV. These counts are broken into subgroups by vehicle type and I use only the standard automobile registrations (rather than taxis, heavy trucks,

[12] For examples of studies that use FARS to examine policy changes see Dee (2001), Benson, Ramussen (1999), Grossman (1991), or Lovenheim and Slemrod (2010). Also note some authors in this area produced their own fatal collisions data by state for within-state county-level comparative case studies, as in Baughman (2001).

farm equipment, or motorcycles). Using registration counts to form county alcohol-related collision rates (as alcohol-related collisions per 100,000 registered vehicles) produces an outcome variable that is less driven by the large differences in population between counties in New York. This vehicle-based measure is also more pertinent to the public health of drivers than the public health of all residents, who may travel only by subway or train.

While unsuited to my dynamic measure of drunk driving, resident population can be used to form a county population density measure to indicate highly urban counties. The New York State Department of Labor publishes population statistics and land area in square miles at the county level, which I use to calculate a monthly state population and monthly state population density by interpolation.

Finally, the popular press has reported on the entrance of non-medallion taxi companies like Uber in major cities in the United States and Uber produces press releases before most expansions into new territory on its website.[13] From these articles, blog posts, and press-releases describing the transit markets in New York City, I have constructed a timeline to determine when Uber entered the New York City market, and when similar services launched that might potentially confound the estimation of an Uber effect on drunk driving.

I use alcohol-related collisions per 100,000 standard vehicles rather than per 100,000 residents in this study to correctly frame the public health question, because relatively few NYC residents are car owners and drivers. As an example, in 2010 the Bronx, Brooklyn, Queens, and Manhattan had a combined 7,623,628 residents, but only 3,052,853 standard vehicle driver's licenses and 1,518,763 standard car registrations (see Appendix 1 for summary statistics by county). I also use this measure because this it varies less among New York State counties than the per-capita measure. To see the wide variation in alcohol-related collisions in levels, Figure 1

[13] Chokkotu and Crook (2014)

presents a shaded map of New York State. Similar to the variation in Figure 1, variation in per capita collisions would reflect only the great differences between NYC and non-NYC counties' populations. Figure 2 shows the car-registration based collision rate, with many New York State counties displaying a similar 2010 level to New York City.

3. Difference-in-Differences Estimation

In order to estimate the effect of Uber on drunk driving, I designate certain New York State counties as treated by the intervention and I designate other New York State counties as a control group. Based on newspaper articles and press releases, this study considers the Bronx, Brooklyn, Queens and Manhattan as treated, but not Staten Island (Richmond County). In the earliest data period that is available, Staten Island's Uber pickup count was an order of magnitude smaller the other New York City boroughs. Table 1 contains summary statistics from the Uber dataset that highlight this disparity in pickups between Staten Island and the rest of the New York City boroughs. Corroborating the impression that Uber operated at much lower volume on Staten Island than elsewhere in New York City, Uber launched a publicized expansion campaign in the summer of 2015, including driver incentives to attempt an increase in coverage on Staten Island.[14]

Figure 3 plots treated versus untreated New York State counties over time. The treated series is the average alcohol-related collision rate for treated New York City counties, omitting Staten Island as it does not receive sufficient Uber coverage. The untreated series is the average alcohol-related collision rate of all other New York State counties. The introduction of Uber in May 2011 is indicated with a black vertical line. While the noisiness of the data makes it difficult

[14] For news coverage of Uber's new incentives in July 2015, see (http://www.ny1.com/nyc/staten-island/news/2015/07/28/uber-expands-staten-island-service-using-driver-incentives.html)

to visually distinguish small trends, there is a divergence in these two series in the post-May-2011 period.

Figure 4 plots each of the treated counties' alcohol-related collision rates individually over time. Each county shows a large drop in its alcohol-related collision rate after the introduction of Uber, but it is also apparent that the four counties' alcohol-related collision rates converge over time. This may reflect the proportion of the population with a lower reserve price than the Uber price, or it may reflect some stable proportion of the population that eschews public transit when intoxicated regardless of price or mode characteristics.

Identification of the estimated intent-to-treat effect relies on variation in Uber access before and after the Uber launch in May 2011, between counties where Uber service was substantial and the counties where it was not. This analysis uses only New York State counties as potential control counties for the New York City boroughs. New York State introduced a state-wide law to target repeat offenders of drunk driving in September 2012. This law made it more difficult for violators with multiple drunk driving offenses to relicense after their driver's licenses were revoked.[15] Because the new law came into effect in all New York State counties simultaneously, I use non-NYC New York State counties, rather than Connecticut or New Jersey counties, as part of a control group to help identify the effect of the Uber launch.

The difference-in-differences estimates are produced using four different control group specifications based on different criteria exogenous to the intervention. For the first control group, I use all possible control counties to minimize information loss. This specification includes all 58 New York State counties that are not part of the treatment group. Not all counties in the control group may be good counterfactuals for New York City counties, however, and this

[15] See this summary of the changes to New York State Vehicle and Traffic Laws from the Office of the Governor of New York: https://www.governor.ny.gov/news/governor-cuomo-announces-regulations-protect-new-yorkers-dangerous-drivers.

specification may inflate the treatment effect based on fundamental differences between counties that vary over time rather than the effect of the intervention. Motivated by this concern, I form three additional control groups for comparison. For the second control group, I select the ten New York State counties that are geographically close to New York City. Counties that are physically close together may share common population characteristics and common trends in drunk driving behavior. These counties are within the feasible commuting radius of New York City, and many share common behaviors as members of the same metropolitan area. Third, I select the ten most densely populated New York State counties using 2006 census data. Urban centers may differ from rural areas in terms of road conditions, traffic levels, and behavioral norms of alcohol consumption. New York City's counties (boroughs) are the most densely populated in the state, and this density-based specification is motivated by the differences between the state's urban communities and rural communities that may be poor counterfactuals for each other despite their similar laws and climate. Fourth, I select counties based on average alcohol-related collision rates between 2009 and 2010. The four treated New York City boroughs are evenly spaced throughout the distribution of drunk driving rates for this period. To create the control group in this specification, I select eight total counties: the counties with the closest value above and below each treated county's alcohol-related collision rate value.[16]

Using these four control group specifications, I estimate the following difference-in-differences model, where the unit of analysis is county-months.

$$CR_{tc} = \beta_0 + \beta_1 X_c + \beta_2 T_t + \beta_3 (Post_t \times Treated_c) + \epsilon_{tc} \quad (1)$$

CR is an alcohol-related collision rate constructed using county-level registered vehicle counts within a given month, *X* is a matrix of county fixed effects that control for time-invariant

[16] I did not pursue a cohort of cities approach as in Card (1997). Gathering monthly-frequency data for each city would require a separate Freedom of Information Act request that would have taken prohibitively long.

differences between counties, *T* is a matrix of month-year fixed effects that control for common variation over time across all counties (e.g.: a harsh winter, a state-wide recession, or a state-wide change in alcohol control laws), and the variable of interest is the product of a treated county indicator (*Treated*) and a time indicator equal to one after the Uber launch in May 2011 (*Post*).

4. Difference-in-Differences Results

In Table 2 I present results from difference-in-differences estimation. Each column contains an estimate of the effect of Uber on the alcohol-related collision rate, clustered standard errors in parentheses, and wild-bootstrap county-clustered standard errors in brackets.[17] These results are presented with and without county-specific time trends for the four different specifications of control group. The magnitude of the estimate is consistent across specifications, as is the size of the standard errors. Scaled by the mean of the dependent variable, the effect represents an average decrease in the alcohol-related collision rate of 17 to 35 percent for the treated counties taken as a group. The estimates from specifications two through four restrict the control group to more comparable counties, and show a 25 to 35 percent decrease in the alcohol-related collision rate.

To check the robustness of these results, Table 3 presents a placebo test using an intervention date of June 2009, the midpoint of the pre-intervention time-series, instead of the observed intervention date of May 2011. The new pre-intervention period is from July 2007 to April 2009.

[17] Cameron, Gelbach, and Miller (2008) introduce wild bootstrap standard errors to address within-cluster dependence, which becomes particularly important with data that has few clusters. One iteration of the wild bootstrap procedure reforms the dependent variable by multiplying each cluster of residuals by 1 or -1 and adding the resulting residual values to the fitted value. While 62 New York State counties are examined in this study, in some specifications eight control counties are compared to four treatment counties. My cluster total of twelve approaches the "ten or less" threshold where Cameron, Gelbach, and Miller show incorrect standard errors from other methods. I use Graham, Arai, and Hagströmer's R package, "multiwaycov" which can be accessed here: http://sites.google.com/site/npgraham1/research/code

The estimated effects of the placebo intervention differ strongly from the actual intervention results in Table 2, producing effects with much smaller magnitudes, most of which are positively signed. None of the effects are significant at the 1 percent level though some are significant at the 5% level, and the inclusion of county-specific time trends causes changes in sign, magnitude, and significance. This difference in the estimated effects suggests that the estimated treatment effects in Table 1 are not easily reproducible by chance.

To help put the estimated effects of the Uber launch from Table 2 in context, Table 4 scales the treatment effect by the number of registered vehicles and the average monthly crash count in each treated county in 2010, the last calendar year before the Uber launch. This table shows that if the average effect were apportioned across the four treated counties by their population of vehicles, it would imply a crash count decrease in Queens of 16 - 22 crashes per month, a 33 to 45 percent decrease from Queens' 2010 average monthly alcohol-related crash count. On the lower end of the spectrum, these average effects imply 5 – 7 fewer crashes per month in Manhattan, a 16 to 22 percent decrease based on Manhattan's 2010 average alcohol-related crash count. The transformed estimates in Table 4 put the results in a more relatable format, but it is unrealistic to expect the introduction of a new transit mode to produce uniformly distributed effects over the population of vehicles in a city.

To examine heterogeneity in the Uber effect on drunk driving in each NYC borough, Table 5 presents the same OLS difference-in-differences estimation, but with separate treatment effects for each treated county using the following estimating equation.

$$CR_{tc} = \beta_0 + \beta_1 X_c + \beta_2 T_t + \beta_3(Post_t \times Bronx_c) + \beta_4(Post_t \times Brooklyn_c) + \beta_5(Post_t \times Queens_c) + \beta_6(Post_t \times Manhattan_c) + \epsilon_{tc} \quad (2)$$

Allowing for separate treatment effects for each treated county, I find that the magnitude of the effect for Manhattan is much larger than the average effects in Table 2. Instead of collision-rate

effect between -2.5 and -3.3 as in Table 2, Manhattan's separate treatment effect ranges between -3.5 and -4.3 when county-specific time trends are included. The separate treatment effect for Queens is much smaller than the NYC average treatment effect, with a change in collision rate ranging between -1.9 and -2.7 when county-specific time trends are included. Both Brooklyn and the Bronx show treatment effects close to the NYC average effect. Rather than filling in areas with less access to public-transit, this range of effects suggests the Uber launch served as an addition mode to access the established urban center.

Just as I estimated separate treatment effects for each NYC borough to highlight the heterogeneity of treatment effects across space, I estimate separate treatment effects over time. Table 6 presents difference-in-differences estimates for treatment effects that may vary each year after the intervention.[18] This method shows smaller effects immediately following the Uber launch and larger effects in the later years of the sample period. Compared to the average alcohol-related crash rate effect of -2.5 from Table 2 using the population-dense control group, the separate time effect estimation shows a -1.48 change in the crash rate for the first year after the Uber launch, a -3.9 change in the crash rate in year two, and a -3.27 change in the crash rate in year three. This tendency for a small effect in year one is robust across control group specifications and may be explained by low salience immediately after the Uber launch, low consumer trust for non-medallion taxi services, or the dynamics of Uber's fleet size through driver acquisition.

5. Synthetic Control Analysis

[18] I have borrowed the methodology from Wolfers' (2006) study on unilateral divorce laws. His post-intervention time series is longer than mine, so instead of bins of 2 years, I use a dummy variable for each post-treatment single year available in this study: 2011, 2012, and 2013.

The analysis above establishes that there was a large, negative effect of Uber's NYC launch on alcohol-related collisions. As a further robustness check and also to explore the dynamics of county-by-county effects, I now employ a synthetic control analysis and examine each treated borough separately.[19]

Synthetic control methods may improve upon standard difference-in-differences analysis in identifying a control group that more closely resembles the treatment group in the pre-treatment period and is therefore a more plausible counterfactual. The time series plot of automobile collisions is noisier than the time series typically used in synthetic control analyses, making it difficult to achieve a reasonable fit between the treated group and the synthetic control in the pre-treatment period.[20] To help select control units based on signal rather than noise, I run the synthetic control optimization process on a 3-month moving average of the original series.

For each of the four treated boroughs in New York City, all of the 58 untreated counties serve as members of the donor pool. Each synthetic comparison county I produce is the strictly positively weighted sum of some combination of counties from the donor pool. The non-negative, mean-squared-error minimizing county weights that produce each synthetic control county series are provided in Appendix 2. I use only the pre-intervention alcohol-related collision rate values to construct the synthetic control weights.

Figure 5 shows the synthetic control gap plots for each treated county, with a vertical line to mark the May 2011 entry of Uber in New York City. The largest effect over time appears to be in Brooklyn (Kings County), though all of the treated boroughs show significant divergence from

[19] Abadie, Diamond, and Hainmueller's (2010) synthetic controls method selects a weighted average of several potential control units based on pre-treatment variables for use in difference-in-differences estimation. This method is deployed in this study using Abadie, Diamond, and Hainmueller's R package "Synth," available here: https://cran.r-project.org/web/packages/Synth/Synth.pdf

[20] Cigarette sales per capita in Abadie et al (2010) and GDP per capita in Abadie and Gardeazabal (2003) are significantly smoother.

their synthetic control, suggesting an increase in Uber fleets and the NYC population's learning curve.

As a robustness check, I implement the synthetic control method using Richmond County (Staten Island), a similar but untreated county, to produce a placebo synthetic control gap plot. Staten Island is a borough of New York City, but access to Uber was minimal even years after the Uber launch in New York City (see Table 1 for a comparison of pre-intervention Uber pickups by county). In Figure 6, the gap plot does not show the same decrease as the counties in Figure 5. This lack of effect in Figure 5 is further evidence that the Uber intervention in New York City is not easily replicated by chance. It is possible that the lack of effect in the Staten Island plot, however, is a product of a poorly-fit synthetic control unit since the pre-period gap plot varies widely from the zero line.

For a more rigorous robustness check of the synthetic control results, I follow Abadie et al. (2010) and perform a permutation test. Plotting a synthetic control gap plot for each of the 62 New York State counties in the same figure, allows me to visually check whether the New York City treatment counties constitute extreme events in the distribution of all New York State county gap plots with a May 2011 intervention date. If each treated county plot is not more extreme than 95 – 99% of the untreated county plots in the distribution of such plots, it might be an indication that the synthetic control outcome could be obtained by chance.

Figure 7 shows all 62 synthetic control gap plots, one for each New York State county. The treated county plots (for Manhattan, Brooklyn, the Bronx, and Queens) are shown in red and the untreated county plots shown in blue. All synthetic control gap plots are constructed from 3-month moving averages of the original series to reduce the probability of optimizing on noise rather than central tendency. I present all 62 plots in Figure 7 for completeness but my analysis is

based on Figure 8 and 9 both of which omit poorly constructed plots with high mean squared prediction errors (MSPE) in the pre-period. If a synthetic county series fits the original county series poorly in the pre-period, it is likely that the post-intervention period effects are mostly noise. I seek to compare the treated county synthetic control gap plots only to a distribution of similarly well-constructed control county gap plots. In this spirit, I omit control county gap plots with very large MSPEs in two waves based on cutoffs that are multiples of my treatment county MSPEs.

Figure 8 omits any county with a MSPE greater than 20 times Queens' MSPE. Figure 9 omits any county with a MSPE greater than 5 times Queens' MSPE for a stricter level of comparability. Figure 8 contains 38 plots and Figure 9 contains 20 plots out of the original 62. These sample sizes would suggest that each treated county plot should be more extreme than all but two or one plot in the distribution, respectively. All four of New York City's treated borough treatment effects were extreme outcomes in the distribution of comparable synthetic control plots with a May 2011 intervention date.

6. Conclusion

Uber's launch in New York City produced quasi-experimental public health data in New York City. Leveraging this intervention, I estimated the effect of Uber's entrance on drunk driving using alcohol-related collisions and designating unaffected New York State counties as counterfactuals. Using difference-in-differences estimation I find that the introduction of Uber decreased the alcohol-related collision rate by 25 to 35 percent in affected counties. Using differentiated treatment effects in a difference-in-differences framework, and later using synthetic control methodology, I decompose this effect for each of four New York City counties

that experienced significant Uber access. I find that Manhattan, where public transit and cabs were already plentiful, has the largest effect. To determine if this effect was sustained, I decompose the treatment effect over time and find that the effect of the intervention increased over time before tapering off. Synthetic control analysis shows a significant dynamic decrease in drunk driving in all four treated boroughs, and a permutation test on the synthetic control results suggests these results were not random. Placebo and robustness tests did not show significant deviations from this narrative.

While non-medallion cabs companies including Uber continue to spread to other cities, this study may provide some insight for municipalities' transit decisions. It is vital to recall, however, that alcohol-related collision reductions of this magnitude are not necessarily generalizable to other cities, as New York City enjoyed many forms of public transit for a century before the Uber launch and boasts a large population that does not own cars, which might encourage higher adoption rates of new transit options. Two studies have been published since I began this project that examine drunk driving reductions from Uber access in areas not traditionally known for public transit.[21] They show a smaller negative vehicular homicide effect of Uber access in California and a puzzling increase in alcohol-related traffic fatalities in metropolitan counties with Uber access across the US, respectively.

City government's provision of transportation services for intoxicated consumers is an important public health concern and allowing non-medallion cab services like Uber seems to help with that goal. After the Uber launch, each of New York City's boroughs experienced a different magnitude of effect and different dynamic effects over time, however, demonstrating that the experience of different neighborhoods within a metropolitan area may vary widely from

[21] See Greenwood and Wattel (2015) for an analysis using California, and Brazil and Kirk (2016) for an analysis using US metropolitan areas.

the average effect for the entire area. The introduction of a service like Uber is not necessarily homogenously distributed in a city, even years after its launch, and some neighborhoods, like Staten Island, may not have access at all.

Without microdata on Uber's pickups on Staten Island, generated from another author's freedom of information request, Uber's 2011 New York City launch would seem like evidence that Staten Island was "treated" with a significant increase in cab service. In service of reducing the cost of the alternative to drunk driving for city residents, city governments should examine the microdata of transit access in their city, since this study shows that services like Uber will not necessarily target neighborhoods underserved by transit and complete access for most of the city's population could take many years to become reliable.

Table 1: New York City Uber Pickups By Borough, 2014

	April 2014		April - September 2014		
	Ride Count	Average Rides Per Day	Ride Count	Average Rides Per Month	Average Rides Per Day
Manhattan	453,547	15,118.2	3,443,456	573,909.3	18,920.1
Brooklyn	61,686	2,056.2	593,594	98,932.3	3,261.5
Queens	32,881	1,096.0	342,225	57,037.5	1,880.4
Bronx	3,023	100.8	31,584	5,264.0	173.5
Staten Island	121	4.0	1,034	172.3	5.7

Note: this dataset was provided via a freedom of information request to the New York City Taxi and Limousine Commission from fivethirtyeight.com, available at https://github.com/fivethirtyeight/uber-tlc-foil-response

Table 2: Difference-in-Differences OLS Estimates
Dependent Variable: Alcohol-Related Collisions per 100,000 Registered Vehicles by County

	(1)		(2)	
	full data set		matching on geographic proximity	
treatment	-2.42	-3.30	-2.72	-3.32
	(0.70)	(0.52)	(0.76)	(0.51)
	[0.68]	[0.50]	[0.71]	[0.47]
County Fixed Effects	Y	Y	Y	Y
County Time Trends		Y		Y
Dep. Var. Mean	14.08	14.08	9.47	9.47
%Δ	-17.2%	-23.5%	-28.7%	-35.1%
p value	< 0.001	< 0.001	< 0.001	< 0.001
N	4526	4526	1022	1022
Adj. R Sq.	0.27	0.28	0.56	0.57
	(3)		(4)	
	matching on pre-period population density		matching on pre-period dependent variable	
treatment	-3.08	-2.50	-2.77	-2.81
	(0.75)	(0.46)	(0.89)	(0.78)
	[0.66]	[0.42]	[0.81]	[0.72]
County Fixed Effects	Y	Y	Y	Y
County Time Trends		Y		Y
Dep. Var. Mean	9.79	9.79	11.17	11.17
%Δ	-31.5%	-25.5%	-24.8%	-25.1%
p value	< 0.001	< 0.001	< 0.001	< 0.001
N	1022	1022	876	876
Adj. R Sq.	0.69	0.71	0.33	0.33

Note: OLS estimates of ITT effect between treated counties (New York, Kings, Queens, Bronx) and control counties that differ in each specification. Specification (1) includes all New York counties. Specification (2) includes Richmond, Nassau, Suffolk, Westchester, Rockland, Dutchess, Orange, Putnam, Ulster and Sullivan counties. Specification (3) includes Richmond, Nassau, Suffolk, Rockland, Westchester, Monroe, Erie, Schenectady, Onondaga, and Albany counties. Specification (4) includes Nassau, Rockland, Schenectady, Niagara, Onondaga, Yates, Delaware, and Broome counties. County-level clustered standard errors are reported in parentheses. County-level clustered wild-bootstrap standard errors using Rademacher weights are reported in brackets. All specifications include month-year fixed effects.

Table 3: Placebo Difference-in-Differences OLS Estimates
Dependent Variable: Alcohol-Related Collisions per 100,000 Registered Vehicles by County

	(1)		(2)	
	full data set		matching on geographic proximity	
treatment	0.76	-0.52	0.82	0.21
	(0.38)	(0.71)	(0.60)	(0.88)
	[0.38]	[0.66]	[0.55]	[0.88]
County Fixed Effects	Y	Y	Y	Y
County Time Trends		Y		Y
Dep. Var. Mean	14.70	14.70	10.20	10.20
%Δ	5.2%	-3.5%	8.0%	2.1%
p value	0.05	0.44	0.02	0.788175
N	2852	2852	644	644
Adj. R Sq.	0.27	0.44	0.58	0.59
	(3)		(4)	
	matching on pre-period population density		matching on pre-period dependent variable	
treatment	0.25	1.15	0.52	0.30
	(0.31)	(0.64)	(0.42)	(1.25)
	[0.29]	[0.56]	[0.39]	[1.10]
County Fixed Effects	Y	Y	Y	Y
County Time Trends		Y		Y
Dep. Var. Mean	10.43	10.43	11.94	11.94
%Δ	2.4%	11.0%	4.4%	2.5%
p value	0.39	0.04	0.18	0.79
N	644	644	552	552
Adj. R Sq.	0.69	0.69	0.30	0.29

Note: For a placebo treatment date of June 2009, OLS estimates of the ITT between treated counties (New York, Kings, Queens, Bronx) and controls that differ in each specification. Specification (1) includes all New York counties. Specification (2) includes Richmond, Nassau, Suffolk, Westchester, Rockland, Dutchess, Orange, Putnam, Ulster and Sullivan counties. Specification (3) includes Richmond, Nassau, Suffolk, Rockland, Westchester, Monroe, Erie, Schenectady, Onondaga, and Albany counties. Specification (4) includes Nassau, Rockland, Schenectady, Niagara, Onondaga, Yates, Delaware, and Broome counties. Heteroskedastic-consistent standard errors are reported in parentheses. Wild-bootstrap standard errors clustered at the county level using a Rademacher distribution for weighting are reported in brackets. All specifications include state and month-year fixed effects.

Table 4: Difference-in-Differences Estimates
Scaled by Pre-Period Level of Registered Vehicles by Treated County

	1 (All)		2 (Proximity)		3 (Pop Density)		4 (Matched Rate)	
Estimate	-2.42	-3.30	-2.72	-3.32	-3.08	-2.50	-2.77	-2.81
Dep Var Mean	13.64	13.64	9.09	9.09	9.57	9.57	10.85	10.85
County Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
County Time Trends		Y		Y		Y		Y
Percentage Change	-17.2%	-23.5%	-28.7%	-35.1%	-31.5%	-25.5%	-24.8%	-25.1%
Manhattan								
2010 Registered Vehicles	220,959	220,959	220,959	220,959	220,959	220,959	220,959	220,959
2010 Avg Monthly Crash Count	33.2	33.2	33.2	33.2	33.2	33.2	33.2	33.2
Implied Crash Decrease Per Month	-5.3	-7.3	-6.0	-7.3	-6.8	-5.5	-6.1	-6.2
Borough-Specific Percentage Change	-16%	-22%	-18%	-22%	-21%	-17%	-18%	-19%
Brooklyn								
2010 Registered Vehicles	403,125	403,125	403,125	403,125	403,125	403,125	403,125	403,125
2010 Avg Monthly Crash Count	53.7	53.7	53.7	53.7	53.7	53.7	53.7	53.7
Implied Crash Decrease Per Month	-9.8	-13.3	-11.0	-13.4	-12.4	-10.1	-11.2	-11.3
Borough-Specific Percentage Change	-18%	-25%	-20%	-25%	-23%	-19%	-21%	-21%
Queens								
2010 Registered Vehicles	667,093	667,093	667,093	667,093	667,093	667,093	667,093	667,093
2010 Avg Monthly Crash Count	49.2	49.2	49.2	49.2	49.2	49.2	49.2	49.2
Implied Crash Decrease Per Month	-16.1	-22.0	-18.1	-22.1	-20.5	-16.7	-18.5	-18.7
Borough-Specific Percentage Change	-33%	-45%	-37%	-45%	-42%	-34%	-38%	-38%
Bronx								
2010 Registered Vehicles	227,585	227,585	227,585	227,585	227,585	227,585	227,585	227,585
2010 Avg Monthly Crash Count	28.8	28.8	28.8	28.8	28.8	28.8	28.8	28.8
Implied Crash Decrease Per Month	-5.5	-7.5	-6.2	-7.6	-7.0	-5.7	-6.3	-6.4
Borough-Specific Percentage Change	-19%	-26%	-22%	-26%	-24%	-20%	-22%	-22%

Note: The top five rows of this table contain the estimated difference-in-differences effects, dependent variable mean, specification indicators, and percentage change levels already presented in Table 2. For intuition and clarity, these figures are converted to "crashes per month" and "borough-specific percentage change" to place the difference-in-difference estimates in context. Each named borough section below the header scales the average intent to treat effect by a particular borough's pre-intervention registered vehicle population and average monthly crash count from 2010. If the average effect represented in Table 2's difference-in-differences estimation were directly applied to a particular borough, the implied decrease in crashes per month and the implied percentage change would be the quantities presented. The columns indicate the four sets of control counties compared to the treated NYC boroughs. Specification (1) includes all New York counties. Specification (2) includes Richmond, Nassau, Suffolk, Westchester, Rockland, Dutchess, Orange, Putnam, Ulster and Sullivan counties. Specification (3) includes Richmond, Nassau, Suffolk, Rockland, Westchester, Monroe, Erie, Schenectady, Onondaga, and Albany counties. Specification (4) includes Nassau, Rockland, Schenectady, Niagara, Onondaga, Yates, Delaware, and Broome counties.

Table 5: Difference-in-Differences OLS Estimates With Differentiated Treatment Variables
Dependent Variable: Alcohol-Related Collisions per 100,000 Registered Vehicles

	1		2		3		4	
	All Counties		Matched on Proximity		Matched on Population Density		Matched on Pre-Period Dependent Variable	
Manhattan x Treated	-3.67	-4.28	-3.97	-4.30	-4.34	-3.48	-4.02	-3.79
	(0.25)	(0.42)	(0.32)	(0.39)	(0.29)	(0.32)	(0.55)	(0.71)
	[0.24]	[0.40]	[0.29]	[0.36]	[0.26]	[0.30]	[0.50]	[0.67]
Queens x Treated	-0.32	-2.68	-0.62	-2.70	-0.99	-1.87	-0.67	-2.19
	(0.25)	(0.42)	(0.32)	(0.39)	(0.29)	(0.32)	(0.55)	(0.71)
	[0.24]	[0.40]	[0.29]	[0.36]	[0.26]	[0.30]	[0.50]	[0.67]
Brooklyn x Treated	-2.60	-3.05	-2.89	-3.07	-3.26	-2.25	-2.94	-2.56
	(0.25)	(0.42)	(0.32)	(0.39)	(0.29)	(0.32)	(0.55)	(0.71)
	[0.24]	[0.40]	[0.29]	[0.36]	[0.26]	[0.30]	[0.50]	[0.67]
Bronx x Treated	-3.09	-3.20	-3.39	-3.21	-3.76	-2.39	-3.44	-2.70
	(0.25)	(0.42)	(0.32)	(0.39)	(0.29)	(0.32)	(0.55)	(0.71)
	[0.24]	[0.40]	[0.29]	[0.36]	[0.26]	[0.30]	[0.50]	[0.67]
County Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
County Trends		Y		Y		Y		Y
Depvar Mean	14.08	14.08	9.47	9.47	9.79	9.79	11.17	11.17
Observations	4526	4526	1022	1022	1022	1022	876	876
Adj. R-Sq	0.27	0.28	0.56	0.57	0.70	0.71	0.33	0.33

Note: OLS estimates of placebo ITT effect for July 2010 between treated counties (New York, Kings, Queens, Bronx) and controls that differ in each specification. Specification (1) includes all New York counties. Specification (2) includes Richmond, Nassau, Suffolk, Westchester, Rockland, Dutchess, Orange, Putnam, Ulster and Sullivan counties. Specification (3) includes Richmond, Nassau, Suffolk, Rockland, Westchester, Monroe, Erie, Schenectady, Onondaga, and Albany counties. Specification (4) includes Nassau, Rockland, Schenectady, Niagara, Onondaga, Yates, Delaware, and Broome counties. Cluster-robust standard errors clustered at the county level are reported in parentheses. Wild-bootstrap standard errors clustered at the county level using Rademacher weights are reported in brackets. All specifications include state and month-year fixed effects.

Table 6: Dynamic Treatment Estimates by Post-Intervention Year
Difference-in-Differences OLS Estimates of Dynamic Treatment Effects For Year 1-3
Dependent Variable: Alcohol-Related Collision Rate

	(1)		(2)	
	all counties		matching on geographic proximity	
treatment x Year 1	-1.04	-2.10**	-1.17	-2.08**
	(0.65)	(0.51)	(0.66)	(0.47)
treatment x Year 2	-3.50**	-4.96**	-3.78**	-5.03**
	(0.80)	(0.73)	(0.76)	(0.68)
treatment x Year 3	-2.15**	-3.99**	-2.67**	-4.24**
	(0.80)	(0.76)	(0.82)	(0.73)
County Time Trends		Y		Y
Dep. Var. Mean	14.08	14.08	9.47	9.47
N	4526	4526	1022	1022
Adj. R Sq.	0.27	0.28	0.56	0.58

	(3)		(4)	
	matching on pre-period population density		matching on pre-period crash rate	
treatment x Year 1	-1.53**	-1.48**	-0.76	-1.23
	(0.60)	(0.42)	(1.00)	(0.82)
treatment x Year 2	-3.96**	-3.90**	-4.30**	-4.96**
	(0.79)	(0.59)	(1.01)	(0.82)
treatment x Year 3	-3.36**	-3.27**	-2.45**	-3.28**
	(0.83)	(0.68)	(0.98)	(0.89)
County Time Trends		Y		Y
Dep. Var. Mean	9.79	9.79	11.17	11.17
N	1022	1022	876	876
Adj. R Sq.	0.70	0.71	0.33	0.34

Note: OLS estimates of ITT effect between treated counties (New York, Kings, Queens, Bronx) and control counties that differ in each specification. Specification (1) includes all New York counties. Specification (2) includes Richmond, Nassau, Suffolk, Westchester, Rockland, Dutchess, Orange, Putnam, Ulster and Sullivan counties. Specification (3) includes Richmond, Nassau, Suffolk, Rockland, Westchester, Monroe, Erie, Schenectady, Onondaga, and Albany counties. Specification (4) includes Nassau, Rockland, Schenectady, Niagara, Onondaga, Yates, Delaware, and Broome counties. County-level clustered standard errors are reported in parentheses. County-level clustered wild-bootstrap standard errors using Rademacher weights are reported in parentheses. One asterisk indicates a p-value no greater than 0.05 and two asterisks indicates a p-value no greater than 0.01. All specifications include month-year fixed effects.

Figure 1: Chloropleth Map of New York State Alcohol-Related Collisions

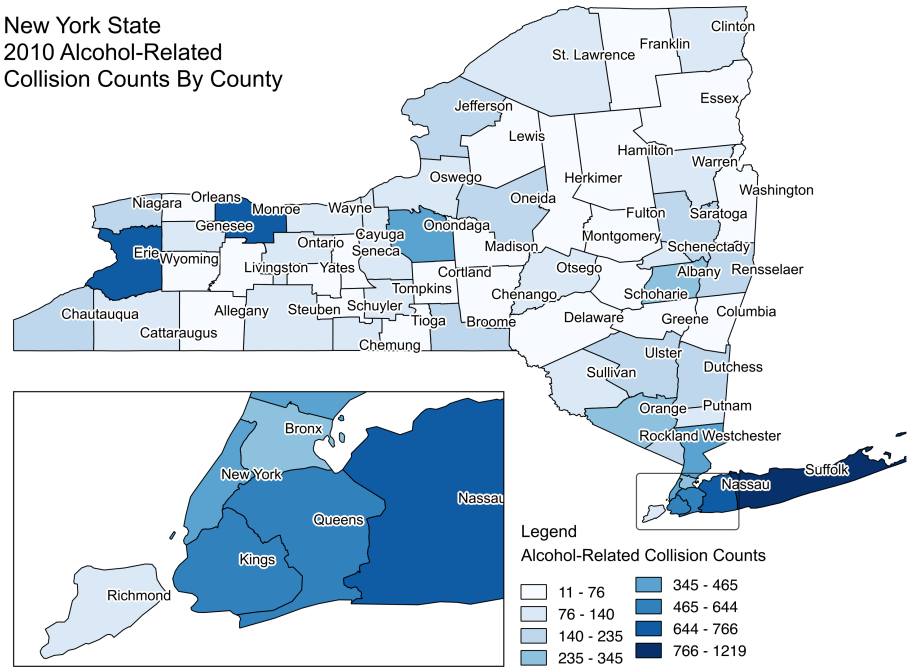


Figure 2: Chloropleth Map of New York State Alcohol-Related Collisions Rate

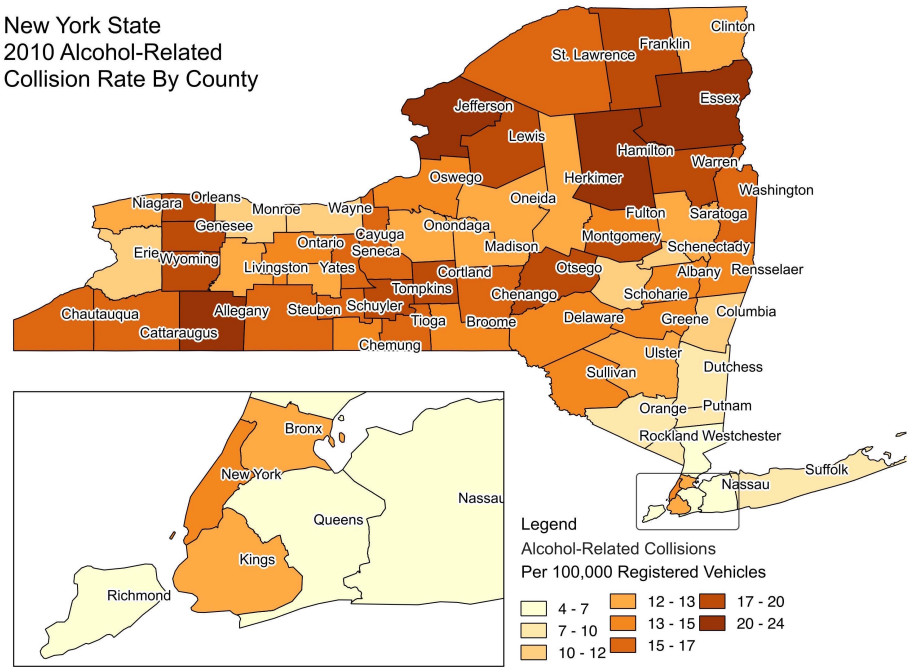


Figure 3: Average Alcohol-Related Collision Rates, Treated Counties Versus All Other Counties

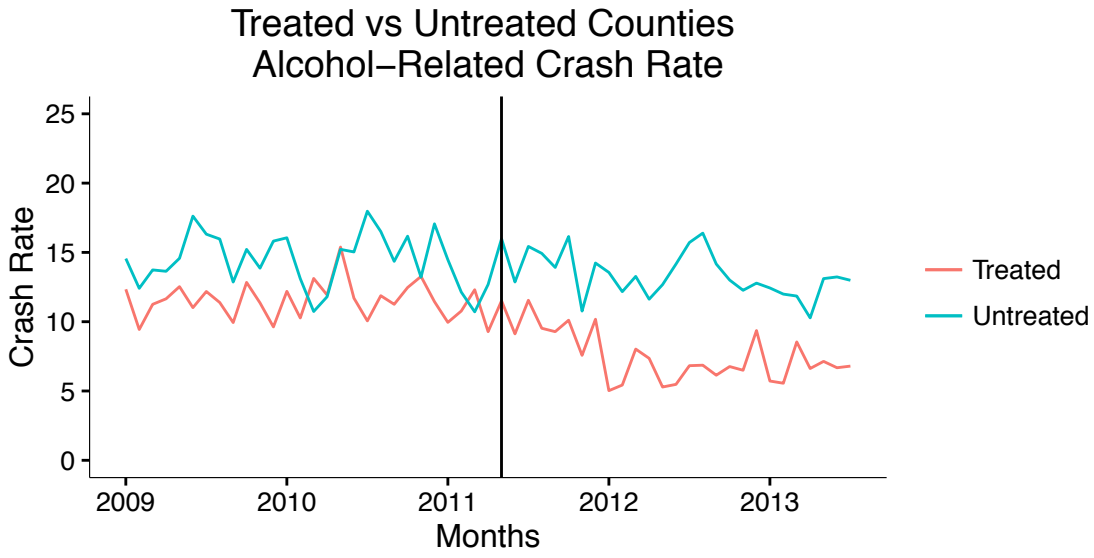


Figure 4: Alcohol-Related Collision Rates, Treated Counties Only

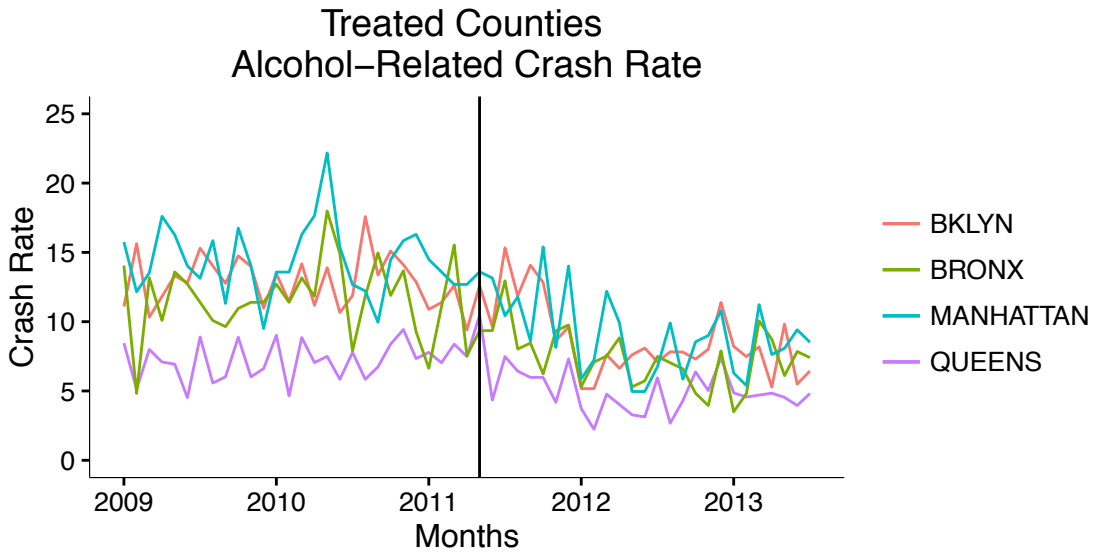


Figure 5: Synthetic Control Plots for Four Treated NY Counties

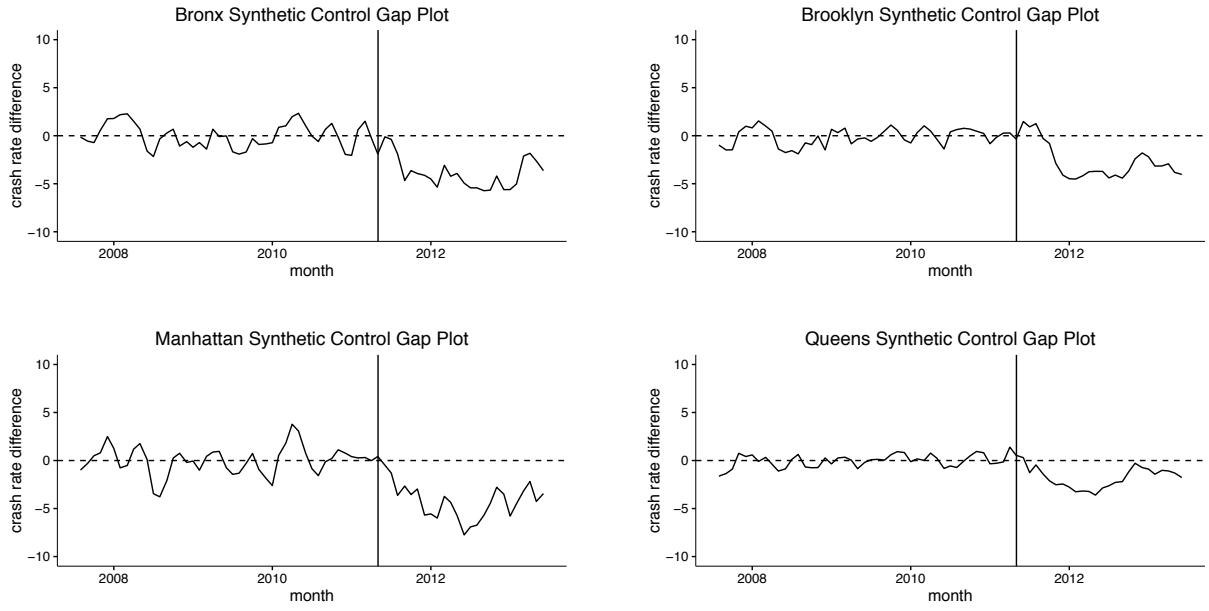


Figure 6: Placebo Synthetic Control Plots for Richmond County (Borough of Staten Island)

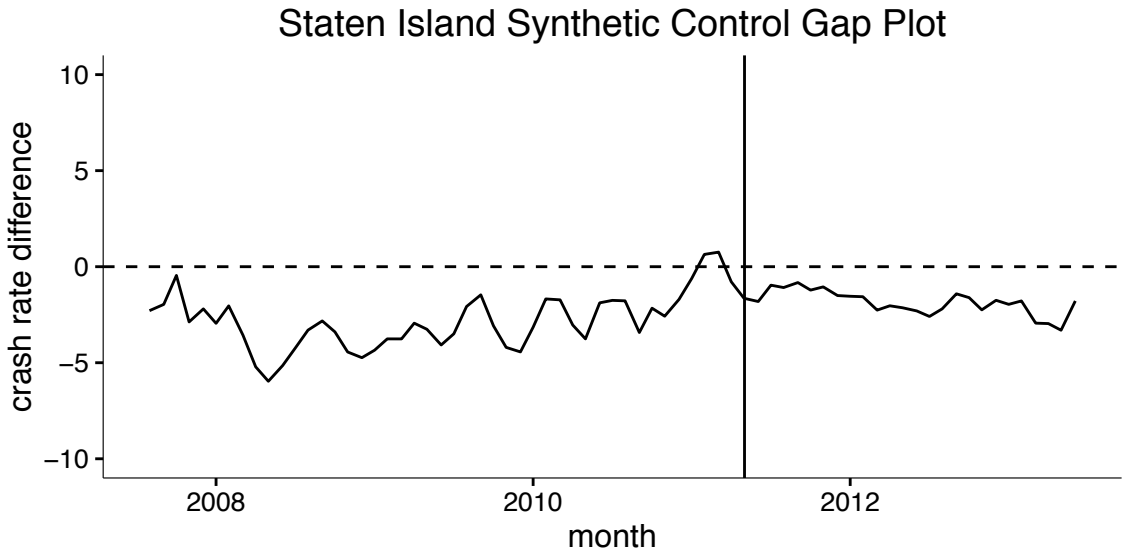


Figure 7: Comparison Plot of Treated County Synthetic Control Gap Series to All Donor County Synthetic Control Gap Series (3-Month Moving Average)

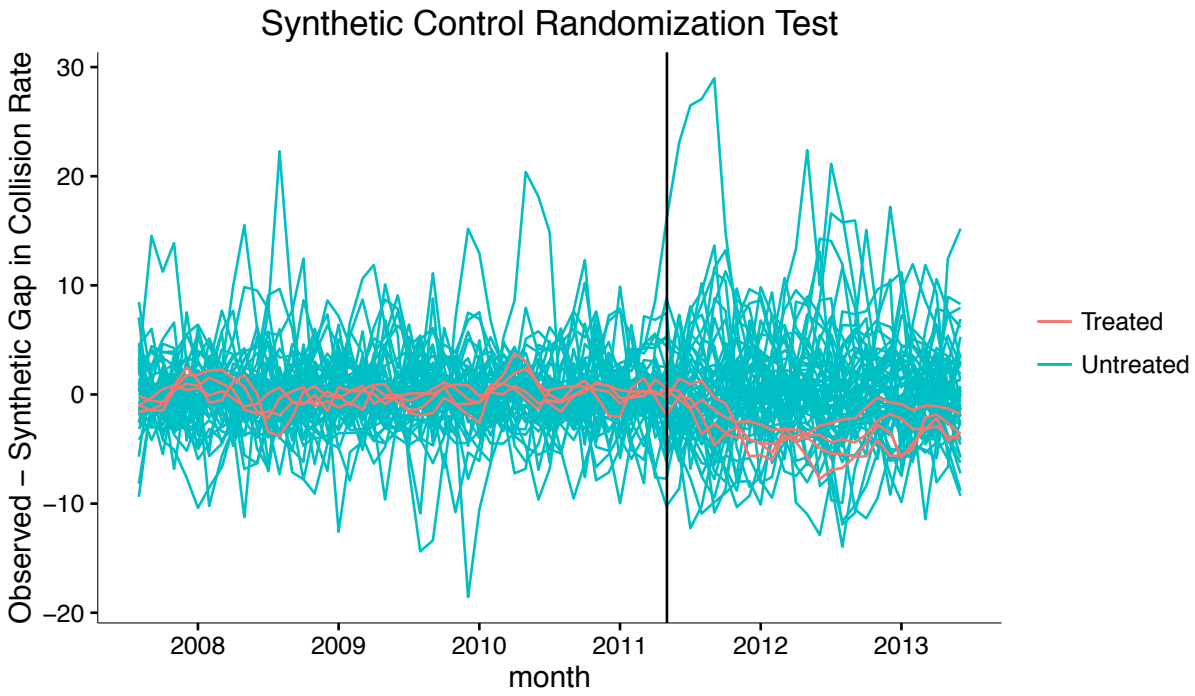


Figure 8: Comparison Plot of Treated County Synthetic Control Gap to Donor County Synthetic Control Gap Series Meeting the Criterion: < 20 Times the Treated County MSPE (3-Month Moving Average)

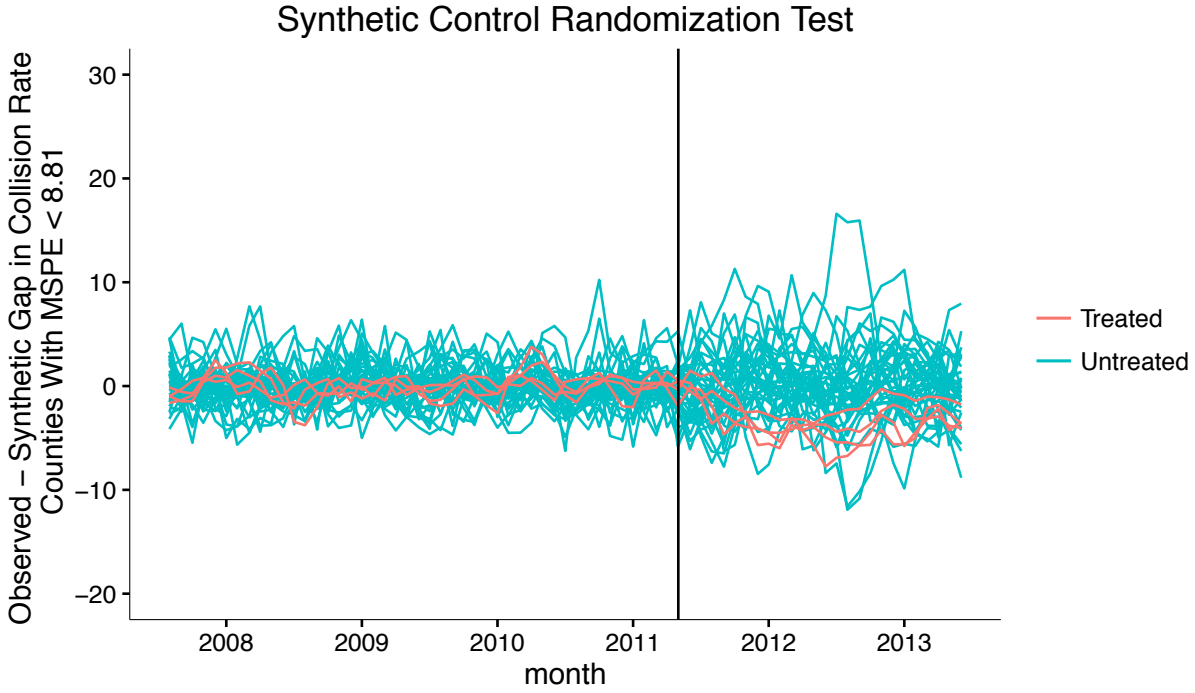
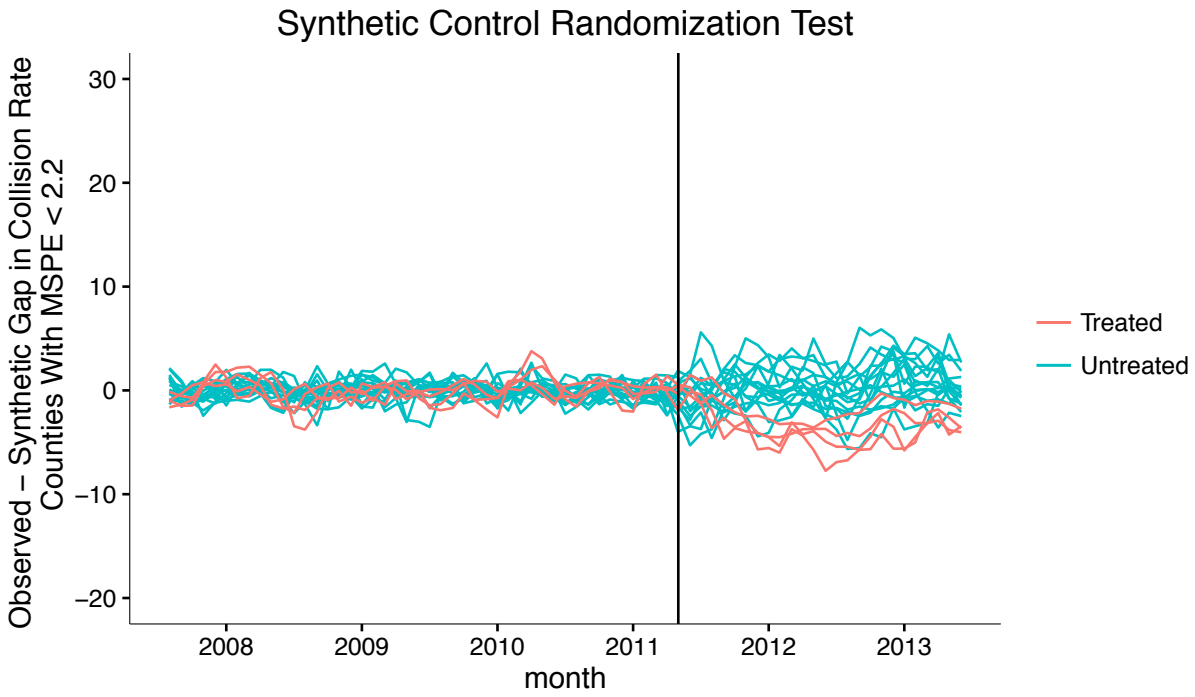


Figure 9: Comparison Plot of Treated County Synthetic Control Gap to Donor County Synthetic Control Gap Series Meeting the Criterion: < 5 Times the Smallest Treated County MSPE (3-Month Moving Average)



Appendix 1: 2010 Summary Statistics By County For Pre-Intervention Comparisons

County	Crash Count	Population	Licensed Drivers	Registered Vehicles	Crash Rate (Registration)	Crash Rate (License)	Crash Rate (Population)	2006 Population Density	2010 Population Density
NEW YORK	398	1,589,999	715,332	220,959	15.0	4.6	2.1	70,179.4	69,250.8
KINGS	644	2,510,446	871,173	403,125	13.3	6.2	2.1	35,532.7	35,553.7
BRONX	345	1,387,754	422,752	227,585	12.6	6.8	2.1	32,395.2	33,018.2
QUEENS	590	2,235,430	1,043,597	667,094	7.4	4.7	2.2	20,645.1	20,463.5
RICHMOND	122	469,370	293,746	246,539	4.1	3.5	2.2	8,163.3	8,026.2
NASSAU	757	1,340,685	988,094	903,543	7.0	6.4	4.7	4,624.0	4,676.4
WESTCHESTER	465	950,320	639,488	597,964	6.5	6.1	4.1	2,193.4	2,195.6
ROCKLAND	205	312,262	204,759	193,015	8.8	8.3	5.5	1,693.1	1,792.3
SUFFOLK	1219	1,494,273	1,084,187	1,067,068	9.5	9.4	6.8	1,611.2	1,638.1
MONROE	694	744,732	511,386	469,361	12.3	11.3	7.8	1,108.5	1,129.6
ERIE	766	919,000	644,398	556,969	11.5	9.9	6.9	882.4	880.1
SCHENECTADY	140	154,774	114,962	105,223	11.1	10.1	7.5	729.9	751.0
ONONDAGA	452	467,178	321,041	281,335	13.4	11.7	8.1	585.4	598.7
ALBANY	312	304,198	199,754	180,161	14.4	13.0	8.5	568.5	581.1
ORANGE	275	373,295	249,810	243,618	9.4	9.2	6.1	461.1	457.3
PUTNAM	99	99,775	76,377	79,140	10.4	10.8	8.3	435.0	431.4
NIAGARA	210	216,355	158,924	137,334	12.7	11.0	8.1	413.3	413.7
DUTCHESS	230	297,631	210,883	209,272	9.2	9.1	6.4	368.2	371.3
BROOME	220	200,388	140,757	126,471	14.5	13.0	9.1	277.7	283.5
SARATOGA	235	219,930	168,428	155,134	12.6	11.6	8.9	265.4	270.9
RENSSELAER	163	159,350	110,614	99,912	13.6	12.3	8.5	237.5	243.7
CHEMUNG	95	88,935	61,341	54,861	14.4	12.9	8.9	217.2	217.9
TOMPKINS	119	101,728	62,338	51,875	19.1	15.9	9.7	210.9	213.7
ONEIDA	220	234,756	160,751	137,102	13.4	11.4	7.8	192.9	193.6
ONTARIO	119	108,140	79,267	70,059	14.1	12.5	9.2	161.9	167.8
ULSTER	204	182,437	133,645	128,763	13.2	12.7	9.3	162.2	162.0
WAYNE	93	93,671	69,549	62,367	12.4	11.1	8.3	153.7	155.0
OSWEGO	123	122,132	85,548	72,500	14.1	12.0	8.4	129.1	128.1
CHAUTAUQUA	158	134,837	93,784	78,326	16.8	14.1	9.8	127.5	127.0
MONTGOMERY	76	50,213	35,607	31,766	19.9	17.8	12.6	121.3	124.0
GENESEE	88	60,016	44,290	37,897	19.3	16.6	12.2	119.1	121.5
CAYUGA	93	79,874	55,031	46,069	16.8	14.1	9.7	117.2	115.2
MADISON	67	73,319	50,495	41,474	13.5	11.1	7.6	107.0	111.8
FULTON	65	55,446	39,967	35,077	15.4	13.5	9.8	111.7	111.7
ORLEANS	58	42,853	29,562	25,421	19.0	16.4	11.3	110.4	109.5
SENECA	40	35,266	24,085	20,592	16.2	13.8	9.5	106.9	108.5
LIVINGSTON	61	65,246	45,118	39,563	12.8	11.3	7.8	101.5	103.2
COLUMBIA	58	62,978	47,740	45,701	10.6	10.1	7.7	99.0	99.1
CORTLAND	66	49,299	32,279	27,445	20.0	17.0	11.2	97.0	98.7
TIOGA	67	51,056	38,387	33,471	16.7	14.6	10.9	98.9	98.4
JEFFERSON	166	116,541	72,581	63,558	21.8	19.1	11.9	89.8	91.6
SULLIVAN	88	77,439	55,353	52,585	13.9	13.2	9.5	79.0	79.9
CLINTON	81	82,075	56,498	50,632	13.3	11.9	8.2	79.1	79.0
GREENE	65	49,140	37,578	36,383	14.9	14.4	11.0	76.9	75.9
WASHINGTON	75	63,258	45,344	39,343	15.9	13.8	9.9	75.9	75.7
WARREN	98	65,679	52,419	46,499	17.6	15.6	12.4	76.0	75.6
YATES	22	25,356	16,903	14,177	12.9	10.8	7.2	73.1	75.0
STEUBEN	129	98,973	71,976	61,444	17.5	14.9	10.9	70.5	71.1
WYOMING	57	42,098	29,700	24,279	19.6	16.0	11.3	71.9	71.0
OTSEGO	80	62,211	43,770	37,879	17.6	15.2	10.7	62.4	62.0
CATTARAUGUS	88	80,223	56,647	46,149	15.9	12.9	9.1	62.3	61.2
CHENANGO	62	50,364	38,256	33,133	15.6	13.5	10.3	57.9	56.3
SCHUYLER	25	18,334	14,020	12,189	17.1	14.9	11.4	59.1	55.8
SCHOHARIE	33	32,679	24,080	22,238	12.4	11.4	8.4	51.8	52.5
ALLEGANY	75	48,971	32,612	26,489	23.6	19.2	12.8	48.8	47.5
HERKIMER	57	64,454	45,493	37,413	12.7	10.4	7.4	44.9	45.7
ST. LAWRENCE	118	111,929	74,696	60,706	16.2	13.2	8.8	41.4	41.7
DELAWARE	56	47,879	36,603	32,653	14.3	12.8	9.7	32.5	33.1
FRANKLIN	68	51,596	34,462	28,895	19.6	16.4	11.0	31.2	31.6
ESSEX	66	39,335	28,503	24,108	22.8	19.3	14.0	21.5	21.9
LEWIS	36	27,066	19,965	15,537	19.3	15.0	11.1	20.9	21.2
HAMILTON	11	4,832	4,737	3,963	23.1	19.4	19.0	3.0	2.8

Note: Crash counts and rates are for alcohol-related collisions only. Reported crash rates are alcohol-related collisions per 100,000 persons of the indicated group (i.e.: license-holders). License holders and registered vehicles comprise standard automobile licenses and registration only (no heavy trucks, motorcycles, mopeds, farm equipment, or other vehicles).

Appendix 2: Synthetic Control Weights

Figure 2.1: Composition of Synthetic Control Counties From Section 5

Synthetic Control Weights					
Bronx	Brooklyn	Manhattan	Queens	Staten Island	Donor County Name
0	0.224	0	0	0	ALBANY
0	0	0	0	0	ALLEGANY
0	0.025	0.029	0	0	BROOME
0.04	0.004	0.123	0	0	CATTARAUGUS
0	0	0	0	0	CAYUGA
0	0	0	0	0	CHAUTAUQUA
0	0	0	0	0	CHEMUNG
0	0	0	0	0	CHENANGO
0	0	0	0	0	CLINTON
0	0	0	0.015	0	COLUMBIA
0	0	0	0	0	CORTLAND
0	0	0	0	0	DELAWARE
0	0	0	0	0	DUTCHESS
0	0	0.048	0	0	ERIE
0	0	0	0	0	ESSEX
0	0	0	0	0	FRANKLIN
0	0	0	0	0	FULTON
0	0	0	0	0	GENESEE
0	0	0.018	0	0	GREENE
0	0	0	0	0	HAMILTON
0	0	0	0	0	HERKIMER
0	0.065	0	0	0	JEFFERSON
0	0	0	0	0	LEWIS
0	0	0	0	0	LIVINGSTON
0	0	0	0	0	MADISON
0	0	0.039	0.045	0	MONROE
0.038	0.088	0	0	0	MONTGOMERY
0	0.147	0	0.096	0.182	NASSAU
0	0	0	0	0	NIAGARA
0	0	0	0	0	ONEIDA
0.171	0.13	0.224	0.161	0	ONONDAGA
0	0	0	0	0	ONTARIO
0.174	0	0	0	0	ORANGE
0	0	0	0	0	ORLEANS
0	0	0	0	0	OSWEGO
0.141	0.054	0	0	0	OTSEGO
0	0	0	0	0	PUTNAM
0	0.01	0.224	0	0	RENSSELAER
0.245	0.11	0	0.582	0	RICHMOND
0	0.07	0	0	0	ROCKLAND
0	0	0	0	0	SARATOGA
0.051	0	0.105	0	0	SCHENECTADY
0	0	0	0	0	SCHOHARIE
0	0	0	0	0	SCHUYLER
0	0	0	0	0	SENECA
0.019	0	0	0	0	STEUBEN
0	0	0	0	0	STLAWRENCE
0	0	0	0.068	0	SUFFOLK
0	0	0	0	0	SULLIVAN
0	0	0	0	0	TIOGA
0	0	0	0.023	0	TOMPKINS
0	0	0	0	0	ULSTER
0.035	0.076	0.191	0	0	WARREN
0	0	0	0	0	WASHINGTON
0	0	0	0	0	WAYNE
0	0	0	0	0.818	WESTCHESTER
0.087	0	0	0.009	0	WYOMING
0	0	0	0	0	YATES

Note: weights from synthetic control process producd a synthetic observational unit for each of the four treated counties above and one untreated county (Staten Island) for placebo test purposes. For the Staten Island synthetic control unit, the following counties were omitted from the potential donor pool before mean square prediction error minimization: Richmond county (Staten Island), Kings county (Brooklyn), New York county (Manhattan), Queens county, and Bronx county.

Appendix 3: Main Results Using Crash Counts as Dependent Variable

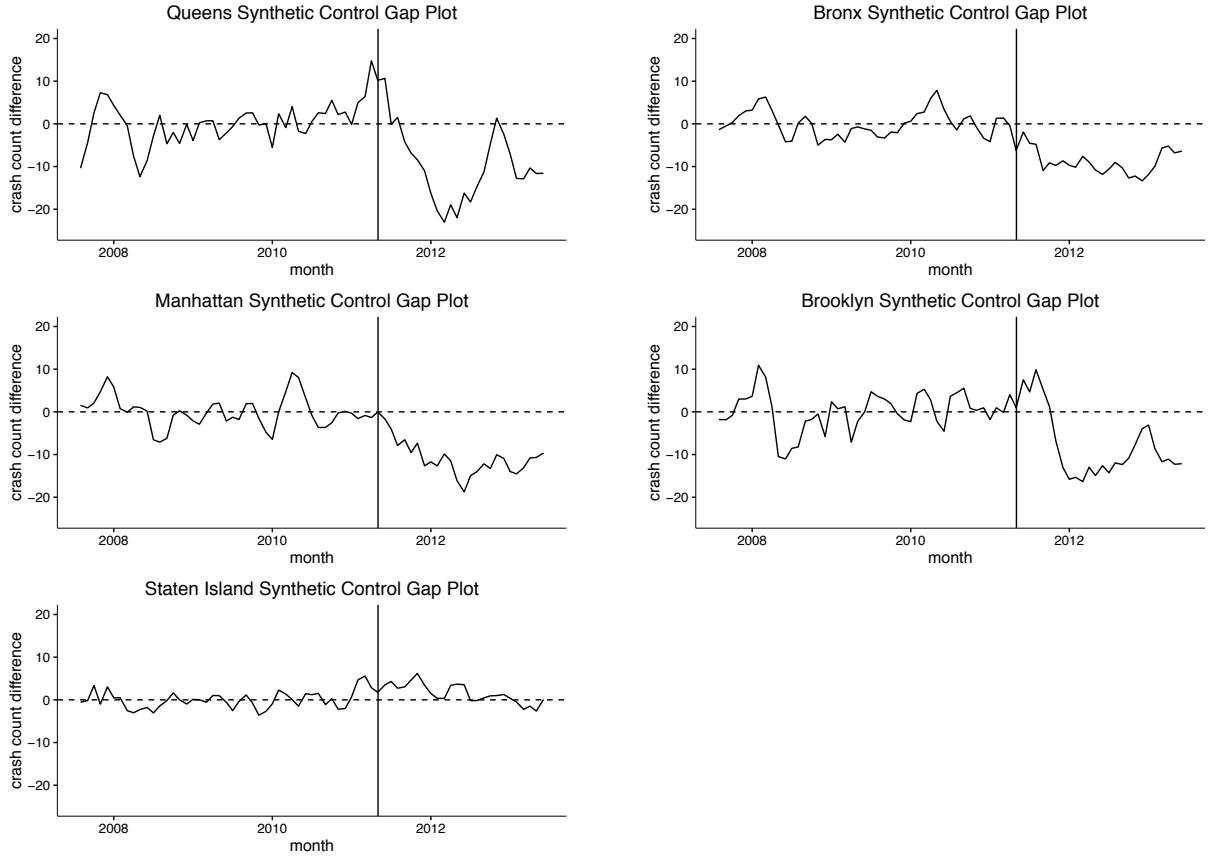
Table 3.1: Difference-in-Differences OLS Estimates
Dependent Variable: Alcohol-Related Collision Count

	(1)		(2)	
	full data set		matching on geographic proximity	
treatment	-10.80	-10.82	-8.01	-10.75
	(1.027)	(1.987)	(2.269)	(2.837)
	[1.009]	[1.889]	[2.108]	[2.554]
County Fixed Effects	Y	Y	Y	Y
Population of Vehicles	Y	Y	Y	Y
County Time Trends		Y		Y
Dep. Var. Mean	15.24	15.24	30.68	30.68
%Δ	-17.2%	-23.5%	-28.7%	-35.1%
p value	< 0.001	< 0.001	< 0.001	< 0.001
N	4526	4526	1022	1022
Adj. R Sq.	0.93	0.93	0.90	0.91

	(3)		(4)	
	matching on pre-period population density		matching on pre-period crash rate	
treatment	-7.93	-8.41	-9.98	-10.54
	(2.417)	(2.778)	(1.569)	(1.974)
	[2.196]	[2.450]	[1.411]	[1.769]
County Fixed Effects	Y	Y	Y	Y
Population of Vehicles	Y	Y	Y	Y
County Time Trends		Y		Y
Dep. Var. Mean	38.91	38.91	25.35	25.35
%Δ	-31.5%	-25.5%	-24.8%	-25.1%
p value	< 0.001	< 0.001	< 0.001	< 0.001
N	1022	1022	876	876
Adj. R Sq.	0.88	0.89	0.88	0.89

Note: OLS estimates of ITT effect between treated counties (New York, Kings, Queens, Bronx) and control counties that differ in each specification. Specification (1) includes all New York counties. Specification (2) includes Richmond, Nassau, Suffolk, Westchester, Rockland, Dutchess, Orange, Putnam, Ulster and Sullivan counties. Specification (3) includes Richmond, Nassau, Suffolk, Rockland, Westchester, Monroe, Erie, Schenectady, Onondaga, and Albany counties. Specification (4) includes Nassau, Rockland, Schenectady, Niagara, Onondaga, Yates, Delaware, and Broome counties. County-level clustered standard errors are reported in parentheses. County-level clustered wild-bootstrap standard errors using Rademacher weights are reported in brackets. All specifications include month-year fixed effects.

Figure 3.1: Synthetic - Observed Synthetic Control Gap Plots, Alcohol-Related Crash Counts (3 Month Moving Average)



Note: synthetic control gap plots represent the difference between the observed NYC counties and their synthetic control counterparts. These plots use a 3-month moving average of alcohol-related crash counts by county at monthly frequency using the same methodology as the synthetic control plots of the alcohol-related collision rates presented earlier in the paper. As a reminder, Staten Island did not receive sufficient Uber coverage at this time, so its flat gap plot is a robustness check.

Table 3.2
Weights For Synthetic Control Counties Using Alcohol-Related Collision Counts

Bronx		Brooklyn		Manhattan		Queens	
Orange	0.26	Albany	0.424	Warren	0.265	Onondaga	0.681
Nassau	0.17	Nassau	0.208	Onondaga	0.18	Suffolk	0.142
Richmond	0.156	Suffolk	0.203	Monroe	0.172	Monroe	0.124
Onondaga	0.154	Onondaga	0.117	Richmond	0.111	Richmond	0.053
Rockland	0.1	Monroe	0.048	Westchester	0.103		
Otsego	0.084			Albany	0.068		
Wyoming	0.074			Erie	0.052		
Montgomery	0.001			Nassau	0.048		

Note: these are the county weights in the W matrix for each of the synthetic control counties using the alcohol-related collision county series. This synthetic controls optimization process uses alcohol-related crash counts and vehicle registrations as predictor series, as well as all of the individual monthly observations of alcohol-related crash counts as special predictors.

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