Do Traffic Tickets Reduce Motor Vehicle Accidents?
Evidence from a Natural Experiment

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Abstract

This paper analyzes the effect of traffic tickets on motor vehicle accidents. OLS estimates may be upward-biased because police officers tend to focus on areas where and periods when there is heavy traffic and thus higher rates of accidents. This paper exploits the dramatic increase in tickets during the Click-it-or-Ticket campaign to identify the causal impact of tickets on accidents using data from Massachusetts. I find that tickets significantly reduce accidents and non-fatal injuries. However, there is limited evidence that tickets lead to fewer fatalities. I provide suggestive evidence that tickets have a larger impact at night and on female drivers.

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I. Introduction

Reducing motor vehicle accidents is a key concern for health policy makers. Motor vehicle accidents cause more than 40,000 deaths and several million injuries each year, and are also the leading cause of death among children in the United States (Center for Disease Control 2009). While a large body of literature examines the impact of regulations and technological innovations, such as seat belts, airbags, and child safety seats (Braver et al., 1997; Carpenter and Stehr 2008; Levitt 2008), there has been considerably less work on the effect of traffic law enforcement. However, addressing the question is complicated by the issue of reverse causality – more police officers are stationed at areas and during periods with higher rates of traffic accidents, in which case OLS estimates of accidents on tickets may be upward biased. Figure 1, which depicts a scatter point graph of the daily average number of motor vehicle accidents against tickets using data from Massachusetts, reinforces this concern – at first glance, there is no discernible relationship between the two variables.

Further, while the ostensible goal of traffic tickets is to deter reckless driving, recent literature demonstrates that traffic tickets are often used as a tool to generate revenue for local municipality budgets (Makowsky and Stratmann 2008; Garrett and Wagner 2009).[1] There is also compelling evidence that police officers are influenced by personal preferences when giving out tickets (Anbarci and Lee 2008; Antonovics and Knight 2009).[2] It is thus unclear whether tickets would fulfill their intended purpose of improving road safety.

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[1] Makowsky and Stratmann (2008) show that police officers in towns which are more budget-strapped are more likely to issue a ticket than a warning. Garrett and Wagner (2009) find that significantly more tickets are issued in counties the year following a decline in revenue.

[2] Anbarci and Lee (2008) find that minority officers, particularly African-Americans, are harsher on all motorists but even harsher on minority motorists. Antonivics and Knight (2009) find evidence for preference-based discrimination

The limited literature addressing the impact of traffic law enforcement on road safety has produced mixed results. Carr, Schnelle and Kirchner (1980) use the Nashville traffic police strike in 1978 as a natural experiment and report no significant deterioration in road safety during the strike. Conversely, Beenstock et al. (1999) use panel data on road sections in Israel and find some evidence that large-scale enforcement reduces road accidents. Babcock et al. (2008) show a negative correlation between police and accidents in Kansas. Makowsky and Stratmann (2011) use the financial health of town as an instrument for the number of tickets issued to demonstrate that more tickets lead to fewer accidents. However, most of the existing studies rely on monthly data of the number of policemen as the measure of traffic law enforcement, which may not be accurate because police have other duties besides patrolling traffic. Further, monthly data may obscure the sensitivity of the behavior of drivers to traffic tickets.

This paper exploits exogenous variation in the number of tickets issued to identify the causal impact of traffic tickets on motor vehicle accidents using daily municipality-level data from Massachusetts. In the fall of 2002, Massachusetts participated for the first time in the Click-it-or-Ticket (CIOT) program, a federal program that was initiated and funded by the National Highway Traffic Safety Administration (NHTSA). The program is carried out throughout the federal year through six "mobilizations" of one to two week periods, during which designated police officers specifically and aggressively focus on traffic law enforcement. Although the program focuses on seat belt use, Massachusetts had a secondary seat belt law until 2007, which meant that while drivers and passengers were required to wear seat belts, police cannot pull them over solely for failing to wear a seat belt. The motorist would have to be committing another

among police officers. Their results demonstrate that officers are more likely to search if officer race and driver race differ.

traffic offense, such as speeding, in order for the police to have sufficient grounds for pulling a motorist over. Thus, I argue that the main impact of the CIOT campaign is to increase the degree of traffic law enforcement overall, and serves as a natural experiment to examine the impact of tickets on motor vehicle accidents.

There were two state-wide mobilizations that took place in Massachusetts during November and December of 2002. The November mobilization was held the two weeks surrounding Thanksgiving and the December mobilization was carried out the week before Christmas. However, these periods were presumably chosen because there are higher traffic volumes (and therefore potentially higher rates of accidents) near holiday periods. To address the concern of endogeneity, I control for the periods that would presumably have been chosen for mobilization in 2001 had the campaign taken place as comparison. After controlling for these periods in 2001, time and municipality fixed effects, as well as a host of other variables, the increase in tickets during the actual mobilization periods in 2002 is then arguably exogenous to accidents. Both results from reduced form regressions and using the mobilizations as an instrument for tickets suggest that there is a negative and significant relationship between tickets and accidents. The estimated accident elasticity with respect to tickets is approximately -0.28 – a 1 percent increase in tickets issued leads to a 0.28 percent decline in motor vehicle accidents. The elasticity of non-fatal injuries with respect to tickets is smaller at -0.17; there is no discernible impact on fatalities. Further, I show the reduction in accidents is higher in municipalities that issued more tickets during the mobilizations, which provides evidence tickets *per se* are reducing accidents rather than other concurrent factors. The main results are robust to a number of different specification checks, which I discuss in Section V. Finally, I explore when tickets are more effective and who tickets affect most in Section VI, which could help inform policy makers on how to allocate enforcement in order to achieve the highest impact. I provide suggestive evidence that tickets have a larger impact at night, and a larger effect on female drivers. However, tickets do not appear to differentially affect age groups.

The rest of the paper is organized as follows. Section II describes the CIOT program and using data. Section III presents the identification strategy. Section IV reports and discusses the main results. Section V offers several robustness checks. Section VI explores heterogeneous effects of tickets, and Section VII concludes.

II. Background and Data

II.A. The Click-it-or-Ticket (CIOT) Program

The CIOT program was first conceived in North Carolina in 1993, and by 2001 the program had spread to a number of other states in the region. (The program is still in existence today.) The initial program combined 3,000 enforcement checkpoints and paid advertising to build public awareness about seat belt use. In 2002, eighteen states participated in a national CIOT campaign pilot program, including Massachusetts (Tison et al. 2006).

In Massachusetts, the CIOT campaign is organized as following. Local police agencies are given grants according to their population size, and police officers apply for overtime to work during the campaign mobilizations. During the mobilizations, police officers focus exclusively on traffic law enforcement and did not have to respond to any other calls. Further, they are expected to give out a certain number of tickets per shift, although they are not penalized if they do not attain the number. Since the campaign focuses on safety belt use, officers target drivers who were not using seat belts. However, Massachusetts had a secondary seat belt law until 2007, meaning drivers and passengers were required to wear seat belts but police cannot pull them over solely for failing to wear a seat belt. Officers must have another reason to pull over a car, such as speeding, and then may issue a $25 citation to the driver and passengers who are not wearing seat belts. Therefore, it is not just tickets issued for seat belt violations that rose in number during the mobilization periods, but tickets of all types of offenses (as described in more detail in Section II.C). In addition to increased traffic law enforcement, Massachusetts engaged in a state-wide media campaign to publicize the CIOT program via radio and television advertisements. The CIOT program, combined with the fact that Massachusetts still had a secondary seatbelt law in 2002, provides the unique setting to examine the impact of traffic citations on motor vehicle accidents.

II.B. Data Sources and Description

The data used in this paper are drawn from two main sources. The tickets data include all traffic tickets issued in the state from April 1, 2001, through January 31, 2003, covering all 350 municipalities. The database has detailed information on the offense type, ticket amount, location and time, demographic data about the driver, as well as the model and make of the car. The particular beginning and end dates of the sample period are exogenous to this study – the tickets data are kindly shared by Bill Dedman and Francie Latour, who first collected the extensive data from the Massachusetts Registry of Motor Vehicles for a series of Boston Globe articles examining ticketing behavior and racial profiling in Massachusetts. The authors were not aware of the CIOT mobilizations.

The motor vehicle accident data are from the Massachusetts Highway Safety Division and includes all accidents that were reported to the Registry of Motor Vehicles for the same period. Each crash includes information on date, time, location, number of vehicles involved, and crash severity. I also obtain additional demographic data from the Highway Safety Division regarding the age and gender of the drivers involved in each accident. The demographic data are available only from 2002 onwards. I then link the two datasets by municipality and date and combine them to create a panel dataset describing the number of tickets issued and accidents for each day in the sample period, for each municipality. The unit of observation is thus at the date/municipality level.

In addition to the tickets and accidents data, I collect daily weather information and gas data, which could potentially affect both traffic volume and accidents. The weather data includes daily precipitation, snowfall, snow depth on the ground, and mean temperatures for each municipality based on the closest weather station. There are altogether 57 weather stations which collected daily weather data for the relevant data period. I link the weather of each municipality to the closest weather station. Gas data consists of retail prices for regular unleaded gas for each municipality for each day in the sample period.

Finally, I include year-varying municipality level data that are intended to capture characteristics that could be correlated with both traffic volume and accidents, such as the municipality unemployment rate, population, number of registered motor vehicles per capita, luxury cars per capita, trucks per capita, motorcycles per capita, and average car age.

II.C. Summary Statistics

Panel A of Table 1 reports the overall summary statistics. On average, there are 4.8 tickets issued and 0.9 motor vehicle accidents per 100 miles of public road length. There are 0.381 nonfatal injuries per 100 road miles and there are close to zero fatalities due to motor vehicle crashes. The measures of tickets and accidents are adjusted per length of public road in each municipality to account for the different sizes (and also populations) of municipalities. Figure 2 depicts the number of tickets and accidents by week for the sample period. There is a large and distinct spike in tickets for both the highlighted areas in 2002 relative to the same periods in 2001, and relative to all other periods.

Panel B of Table 1 presents the summary statistics of the main dependent variables by different groups – time of day, gender, and age group. Unsurprisingly, there are more tickets and accidents in the day because of higher traffic volumes. Males are involved in more accidents than females, which could reflect both that there are fewer female drivers and gender differences in driving behavior. There are also more tickets and accidents among the 25-64 age group.[3]

III. Identification Strategy

The identification strategy of this paper uses the sharp increase of tickets during the CIOT mobilizations to examine the impact on accidents. It also rests on the assumption that the increase in tickets is exogenous, an assumption that I argue in this section is reasonable controlling for other factors. The first CIOT mobilization occurred from November 18, 2002 until December 1, 2002, and Thanksgiving Day was on November 28. The concern is that these periods were chosen because there are high traffic volumes surrounding these holidays, in which case any changes in tickets or accidents could simply be an artifact of increased traffic volume. To address this issue, I control for the comparable periods in 2001. More specifically, if the CIOT campaign took place in 2001, the mobilization period would presumably have been chosen in the same way – the week of Thanksgiving and the week before. Since Thanksgiving was on November 22 in 2001, I define the comparable period to be November 12-25, 2001. The second mobilization in 2002 occurred from December 16-22. I similarly define the comparison period in 2001 to be December 17- December 23. The comparison periods are defined to begin and end on

[3] Adjusted for population, however, the accident rate of the 15-24 age group is twice as large as that of the 25-64 age group.

the same day of week, in order to prevent any bias from having unequal number of weekdays and weekends. For lack of a better term, I call the following weeks *Treatment Periods*: November 12-25, 2001, December 17-23, 2001, November 18-December 1, 2002, and December 16-22, 2002. *Treatment Periods* x *Year2002* is then the equivalent of the actual CIOT mobilizations. The control periods are all other days in the sample period.

The identification strategy would also be violated if other factors not controlled for systematically correlated with the mobilization periods. For example, the mobilizations in 2002 could be systematically correlated with inclement weather or shocks to gas prices, which could affect traffic volume and in turn both the number of tickets and crashes. I therefore control for weather conditions and retail unleaded gas prices as well.

Another issue to consider is that using the CIOT mobilizations to capture the overall impact of tickets may not be appropriate if only tickets for seat belt violations for given out. However, as discussed earlier, Massachusetts did not have a primary seat belt law until 2007, which means that before 2007, law enforcement officers may issue a ticket for not wearing a seat belt only when there is another citable traffic infraction. Figure 3 presents the overall ten most frequent traffic violations over the control and mobilization periods. In the control sample, speeding tickets are the most common, followed by seat belt violations and failure to stop. During the CIOT mobilization periods, seat belt violations surpass speeding tickets as the most common type of ticket, but there is also a uniform rise of almost all types of offenses. The types of offenses that tickets are issued for are also the same for eight of the ten most frequent offenses in the control periods. It therefore seems reasonable to interpret the mobilization periods led to overall stricter traffic law enforcement as measured by tickets.

IV. Results

IV.A. OLS Results

According to the basic model in Becker's seminal paper (1968) on crime and punishment, an individual makes the decision of whether to commit a crime or not by weighing the expected costs against the expected benefits. In the context of this paper, the degree of traffic law enforcement increases dramatically during the CIOT mobilizations, which can be translated into a higher probability of getting caught and thus a higher expected cost of committing a traffic violation. We should therefore expect to see fewer traffic violations and potentially fewer accidents, i.e., we would expect β_1 to carry a negative sign if we model the relationship between accidents and tickets as such:

$$Accidents_{it} = \alpha_i + \delta_t + \beta_1 Tickets_{it} + X_{it}\beta_2 + \varepsilon_{it} \qquad (1)$$

where α_i and δ_t are municipality and calendar fixed effects, which include day-of-week, month-by-year, and public holidays (and the contiguous weekend if the holiday falls on a Friday or Monday) fixed effects. X_{it} represents a vector of controls that may influence both traffic volume and accidents, which include the amount of snowfall, snow depth on ground, rainfall, temperature, retail gas prices, population, unemployment rate, registered cars per 100 miles, trucks per 100 miles, trailers per 100 miles, and motorcycles per 100 miles. The weather conditions and gas prices variables vary at the day-municipality level; the rest vary at the calendar year-municipality or fiscal year-municipality level.

However, OLS estimates of equation (1) would lead to biased estimates if there is a correlation between ε_{it} and $Tickets_{it}$. This is probable because police officers tend to focus on areas where and periods when accidents are prone to happen. If so, estimates of β_1 will be upward-biased. Table 2 reports the OLS estimates from regressing accidents on tickets directly.

The results in Table 2 confirm what we have already seen in Figure 1 – the coefficients on tickets are close to zero and in fact slightly positive, suggesting that the estimates are indeed upward-biased.

IV.B. First Stage Results

The identification strategy proposed in this paper is to use the CIOT periods as a single instrument for tickets. To be valid instrument, the CIOT periods should be able to demonstrate strong explanatory power of tickets (Bound et al., 1995). The first stage regression representing the relationship between tickets issued in town *i* on date *t* and the CIOT periods is modeled as follows:

$$Tickets_{it} = \alpha_i + \delta_t + \beta_2 Treatment\_Periods_t + \beta_3 CIOT_t + X_{it}\beta_4 + \varepsilon_{it} \quad (2)$$

where α_i and δ_t are again municipality and calendar fixed effects. $Treatment\_Periods_t$ is a dummy representing the treatment weeks defined in Section III. $CIOT_t$ indicates when the CIOT mobilizations took place in 2002, and is also equivalent to $Treatment\_Periods_t *$ $Year2002$. X_{it} is the same vector of controls as before. β_3 thus captures the rise in tickets during the CIOT mobilizations in 2002.

Column 1 of Table 3 shows that the CIOT mobilizations increased on average the number of tickets issued per 100 miles by 1.9 per day/municipality. Given the mean of 4.8, the change represents approximately a 40% increase. The F-statistic is approximately 100. The first-stage relationship between the CIOT periods and tickets is clearly strong.

IV.C. Reduced Form Evidence

Table 3 (Columns 2-4) reports the results from estimating the reduced-form relationship between the number of accidents and the CIOT mobilization program:

$$Accidents_{it} = \alpha_i + \delta_t + \beta_2 Treatment\_Periods_t + \beta_3 CIOT_t + X_{it}\beta_4 + \varepsilon_{it} \quad (2)$$

Using accidents as the dependent variable (Columns 2 – 4), the results suggest that the reduced form impact of the campaign on motor vehicle accidents was around -0.10, which would translate into a 11 percent reduction. It appears that the number of accidents fell significantly during the mobilization periods, relative to both the comparable periods in 2001 and the control periods. Injuries went down during the CIOT periods as well, but the reduction in reduced form is not statistically different from zero at conventional levels. The estimate on fatalities is very imprecisely estimated. This is perhaps unsurprising given the low incidence of fatalities overall.

IV.D. IV Results

The main results of the paper, which are the estimates from the IV regressions, are documented in Table 4. The coefficient on tickets in Column 1 implies that a unit increase in tickets decreases accidents by 0.05. Since there are on average 4.8 tickets issued and 0.88 accidents in a municipality, this implies the elasticity of accidents with respect to tickets is approximately -0.28 – a 1 percent increase in tickets leads to a 0.28 percent decrease in motor vehicle accidents.[4] The estimates shown in Table 4 are those produced when using the full set of controls, but the estimates are stable across different specifications.

Next, I examine whether tickets have any impacts on nonfatal injuries and fatalities caused by motor vehicle accidents. Column 2 documents the result from using the number of nonfatal injuries as the dependent variable. For injuries, the IV estimate is significant at the 10 percent level. If we take the estimate at face value, the elasticity of injuries with respect to tickets would be -0.17. Column 3 presents the coefficient on tickets for fatalities. Neither the reduced form nor IV estimates support that tickets reduce fatalities.

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[4] All results using the level numbers of tickets and accidents (instead of rates per 100 miles) yield similar results and can be obtained by request.

IV.E. Mechanism: Tickets versus Information

There are two main mechanisms that could lead to the observed result of fewer accidents during the CIOT mobilizations. The first is a deterrence effect, both for drivers who receive a ticket and those who observe other drivers receiving a ticket. For example, giving a ticket to a driver who was going 80 mph in a 55 mph road could deter the recipient from further speeding, and thus be less likely to be involved in an accident. There could also be a visual deterrence effect, for example, drivers observe another car being pulled over for speeding and therefore slow down and drive more carefully.

Second, it is possible that tickets *per se* are not driving the reduction in accidents. As noted in Section IIA, there was a concurrent state-wide media campaign during the CIOT periods. [5] While the media campaign would only be credible if there were an actual increase in ticketing, the effect of tickets found in Section IV would be overstated if the media campaign was the main mechanism through which accidents were reduced.

I argue that the main mechanism was indeed through tickets and not through the spread of information through the media campaign (or other related channels, such as through the social networks of ticketed drivers). If the spread of information about the CIOT mobilizations was the main mechanism, the reduction in accidents should not vary systematically by how many tickets were written in a particular town. In other words, if ticketing mattered, then it should have had a larger effect in the towns that issued more tickets. In Panel A of Table 5, I include an interaction term of *CIOT* and *Active Ticketing*, a binary variable which I define to be 1 if the average number of tickets issued daily during the CIOT periods exceed the average number of tickets in

[5] It is to my understanding from discussions with the Massachusetts Highway Safety Division that the main component of the CIOT program, particularly in its inception year, was the increase in traffic law enforcement.

the control periods. This seems to be a reasonable proxy for active traffic enforcement during the mobilizations. By construction, the coefficient on the interaction term *CIOT*Active Ticketing* for *Tickets* is much larger than and statistically different from the coefficient on *CIOT*. Of more interest are the coefficients on the interaction term of *CIOT*Active Ticketing* for *Accidents* and *Injuries* (Columns 2 and 3) – they are both statistically different from zero and from the *CIOT* term. In municipalities where the main exposure to the CIOT program is through the media campaign and not through increased ticketing, we do not observe a significant decrease in accidents or injuries, whereas the opposite is true for municipalities that actively ticketed. This helps rule out the hypothesis that the media campaign is driving the results. IV regressions (Table 5 Panel B) using only the participating municipalities yield similar but even more precisely estimated coefficients to using the entire sample.

IV.F. Comparison of Results to Existing Literature

How do these results compare with other mechanisms that aim to improve road safety? Carpenter and Stehr (2008) look at the effect of mandatory seatbelt laws, and find that they significantly reduced traffic fatalities and serious injuries resulting from fatal crashes by 8 and 9 percent, respectively. Levitt (2006) finds that child safety seats are no better than seat belts at reducing fatalities among children aged 2-6. Dee et al. (2005) demonstrate that graduated driver licensing restrictions for teens reduced traffic fatalities among 15–17-year-olds by at least 5.6%. However, these interventions focus more on traffic *regulations* rather than traffic law *enforcement*. By contrast, there has been little direct investigation of the effect on road safety of traffic law enforcement. This paper contributes to the understanding of how traffic law enforcement, as measured by tickets, affects road safety by offering a novel identification strategy.

To my knowledge, Makowsky and Stratmann (2010) is the only other study that examines the impact of traffic tickets on road safety. They use the financial health of town - whether a town asks voters to approve a property tax override referendum – as the instrument for tickets. By putting an override referendum in front of voters, the town board indicates that the town is in fiscal distress and that they would like to raise additional revenue.[6] They find comparable results. Their IV estimates indicate that a unit increase in tickets leads to 0.12 (s.e.=0.034) fewer motor vehicle accidents, 0.044 (s.e.=0.022) injuries and 0.002 (s.e.=0.001) fatalities. Their unit of observation is at the month-municipality level and the mean of tickets, accidents, injuries are at 82.68, 36.93, and 15.83, respectively. Their results thus translate into an elasticity of accidents with respect to tickets of -0.27, which is very close to the elasticity found in this paper. They find a somewhat higher elasticity of injuries with respect to tickets of -0.23. They also present some but inconclusive evidence that tickets reduce fatalities.

V. Robustness Checks

V.A. Using Only November and December Months

As there were only 3 weeks of mobilizations in 2002, a potential concern would be there are factors in other months that are not controlled for that could confound the estimation. For example, there is a spike in tickets in May 2002 relative to May 2001 (Figure 2). The reason for this is because a number of the larger municipalities in Western MA participated in a trial CIOT mobilization the week surrounding Memorial Day Weekend (Solomon et al. 2002). However, I am unable to obtain data on which specific municipalities participated in the mobilization as the

[6] Their instrument rests on the caveat that the only impact on fiscal distress on motor vehicle accidents is through the number of tickets issued. However, one could imagine that the fiscal distress would affect accidents through many other unobservables such as unemployment (and therefore less traffic), poorer road maintenance and a host of other unobservable variables. The authors attempt to address these issues by including various controls and by using different specifications.

Department of Highway Safety did not keep records of the particulars of the trial mobilization. As a robustness check, I restrict the analysis to only the November and December months of 2001 and 2002.

The results are qualitatively similar to those using the entire sample period. Table 6 Panel A summarizes the reduced form estimates of the CIOT mobilizations on tickets and accidents using the full set of controls. The reduction in the number of injuries due to motor vehicle accidents associated with the mobilization periods is now statistically significant at the 5 percent level. Table 6 Panel B presents the IV estimates. The estimate on injuries implies that a one-unit increase in tickets decreases accidents by 0.07 injuries. As before, there is no discernible impact of tickets on fatalities. The results from using the restricted sample tell a consistent story: tickets appear to reduce the overall number of accidents and injuries, but there is no support for tickets being an effective tool in reducing fatalities.

V.B. Using Averages

Although the number of tickets varies across days and municipalities, the instrument – the CIOT mobilizations – was a state-wide program and supposedly took place across the entire state of Massachusetts, i.e., the variation in CIOT participation was by day (although there are varying degrees of participation). While I group standard errors by municipality in all regressions, one may be concerned that failing to account for intra-day correlation in errors could generate standard errors that are biased downwards. I argue that it is more important to account for intra-municipality correlation than intra-day correlation since Massachusetts has 350 municipalities and hence distinct variation in weather and traffic conditions across the state. Nonetheless, as a robustness check, I collapse the data by day into a simple time series to see if the results still hold. Panel A of Table 7 contains the reduced form estimates and Panel B the IV results. The results are consistent with those produced when using the full sample. Columns 4-8 of Table 7 Panel B give the elasticity using logarithmic forms of accidents and tickets. The estimated elasticity is around -0.35, which is also close to the elasticity (-0.28) calculated using the full sample.

VI. Heterogeneous Effects

When are tickets most effective in reducing accidents? What kind of driver do tickets affect most? From a policy perspective, it is important to understand these issues in order to efficiently allocate enforcement efforts. To my knowledge, this is the first paper to attempt to explore heterogeneous effects of traffic law enforcement.

VI.A. By Time of Day

First, I examine the impact of tickets by time of day (Table 8 Panel A). The independent variable of interest is the number of tickets issued during the day and the number of tickets issued at night. The first stage relationships between the mobilizations and tickets issued during daytime and nighttime are both strong (not shown), and the separate estimates during the day and night are statistically different at the 5 percent level. The implied elasticity for accidents with respect to tickets is close to -1 at night and is only around -0.16 during the day. This finding that tickets have a larger impact at night seems to make sense – despite 60 percent less traffic on the roads, close to half of all fatal car accidents occur at night (Elliot 2009). Driving at night could be potentially more dangerous because of poorer light conditions and more dangerous drivers (for example, drunk or fatigued drivers) on the road.

VI.B. By Gender

The Massachusetts Highway Safety Department did not start collecting demographic data of the drivers involved in motor vehicle accidents until the beginning of 2002, so unfortunately the comparable treatment periods in 2001 cannot be included in the analyses by gender and age groups. To ensure the results would not be completely discredited without using the control periods in 2001, I run the basic IV regression of accidents on tickets using only data from 2002 onwards. The coefficient on tickets is slightly smaller at -0.369 (s.e. = 0.008), but not statistically different from using the entire sample.

Since it is probably difficult for a driver to ascertain whether a fellow driver on the road being pulled over is male or female, the main independent variable of interest is the overall number of tickets (instead of the number of tickets issued to male and female drivers). Table 8 Panel B presents the coefficient on tickets using the number of male and female drivers involved in motor vehicle accidents per 100 miles. While the actual coefficients for males and females are close, the implied elasticities are very different. This is because females are involved in much fewer accidents overall (presumably because there are fewer female drivers and/or they are more careful drivers). The accident elasticity with respect to tickets for females is -0.72 and -0.24 for males, a striking three-fold difference. Given the large literature within psychology and sociology that show women tend to be more risk-adverse than men (see Croson and Gneezy (2009) for a review), it is plausible that women are more deterred by traffic law enforcement than men.

VI.C By Age Group

Finally, I investigate whether there are heterogeneous impacts of tickets by age groups in Panel C of Table 6. This is of particular interest to policy makers because young drivers under 25 tend to be at most risk on the road (Center for Disease Control 2009). Again, because it would be difficult for the driver to determine the age of another driver being pulled over, I use the overall number of tickets as the main independent variable. The results imply that the elasticities with respect to tickets are 0.76 and 0.62, for 15-24 year olds and 25-64 year olds, respectively, and these are not statistically differentiable from one another. There does not seem to be marked differences in how tickets affect the two age groups.

VII. Conclusion

This paper examines whether traffic tickets affect road safety as measured by motor vehicle accidents. A naïve OLS regression of accidents on tickets suggests that there is no impact of tickets on accidents. However, an analysis using exogenous variation in the number of tickets issued to identify the causal effect of tickets on road safety gives rise to distinctly different results – tickets in fact lead to fewer motor vehicle accidents. Further, tickets help to reduce non-fatal injuries stemming from motor vehicle accidents. In addition, the heterogeneous impact of tickets suggests that there is scope for intervention, for example, by allocating more resources towards traffic enforcement at night since tickets have a larger impact during nighttime. Also, females appear to be more deterred by traffic law enforcement than men. However, there do not appear to be differences in the impact of tickets on different age groups. Overall, the findings of this paper suggest that as unpopular as traffic tickets are among drivers, motorist behavior does respond to tickets.

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Figure 1 – Scatter Point Graph of Accidents against Tickets

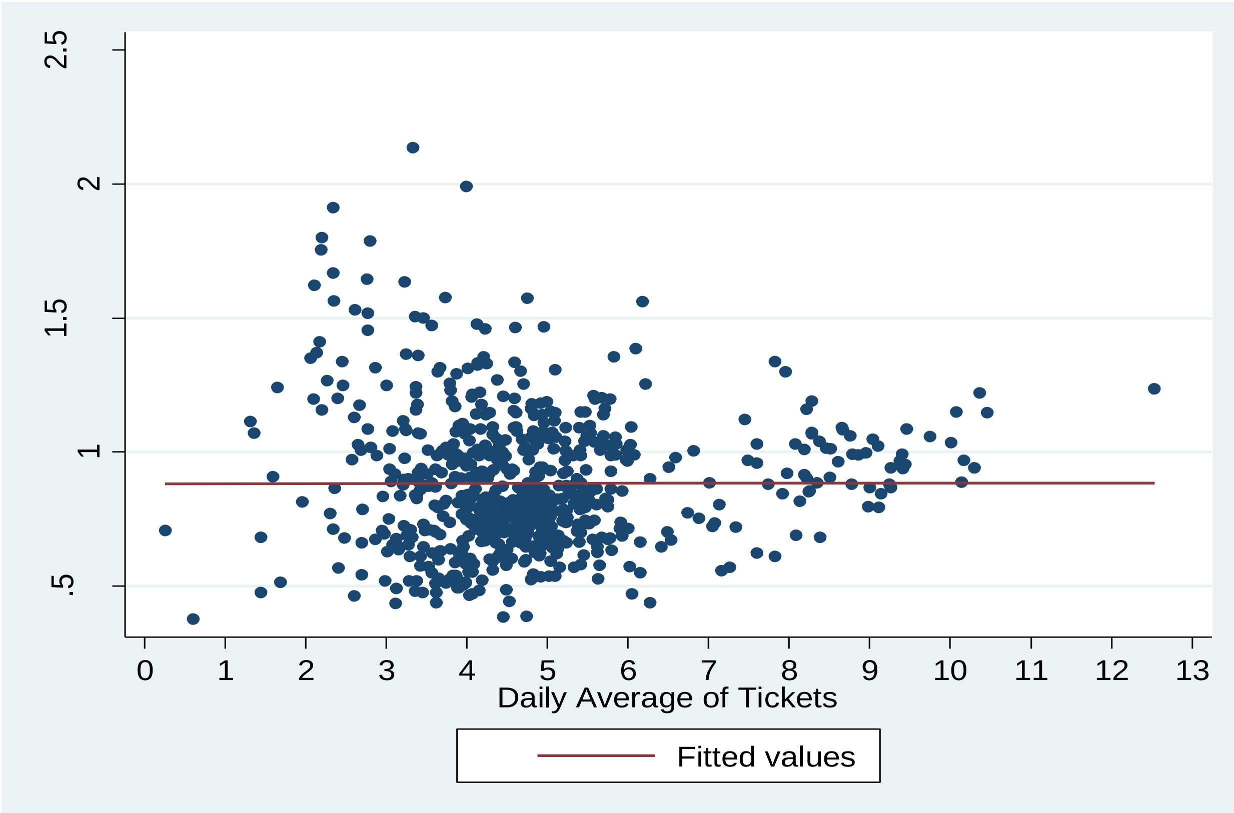

Figure 2 – Tickets and Accidents by Week, April 1, 2001 – January 31, 2003

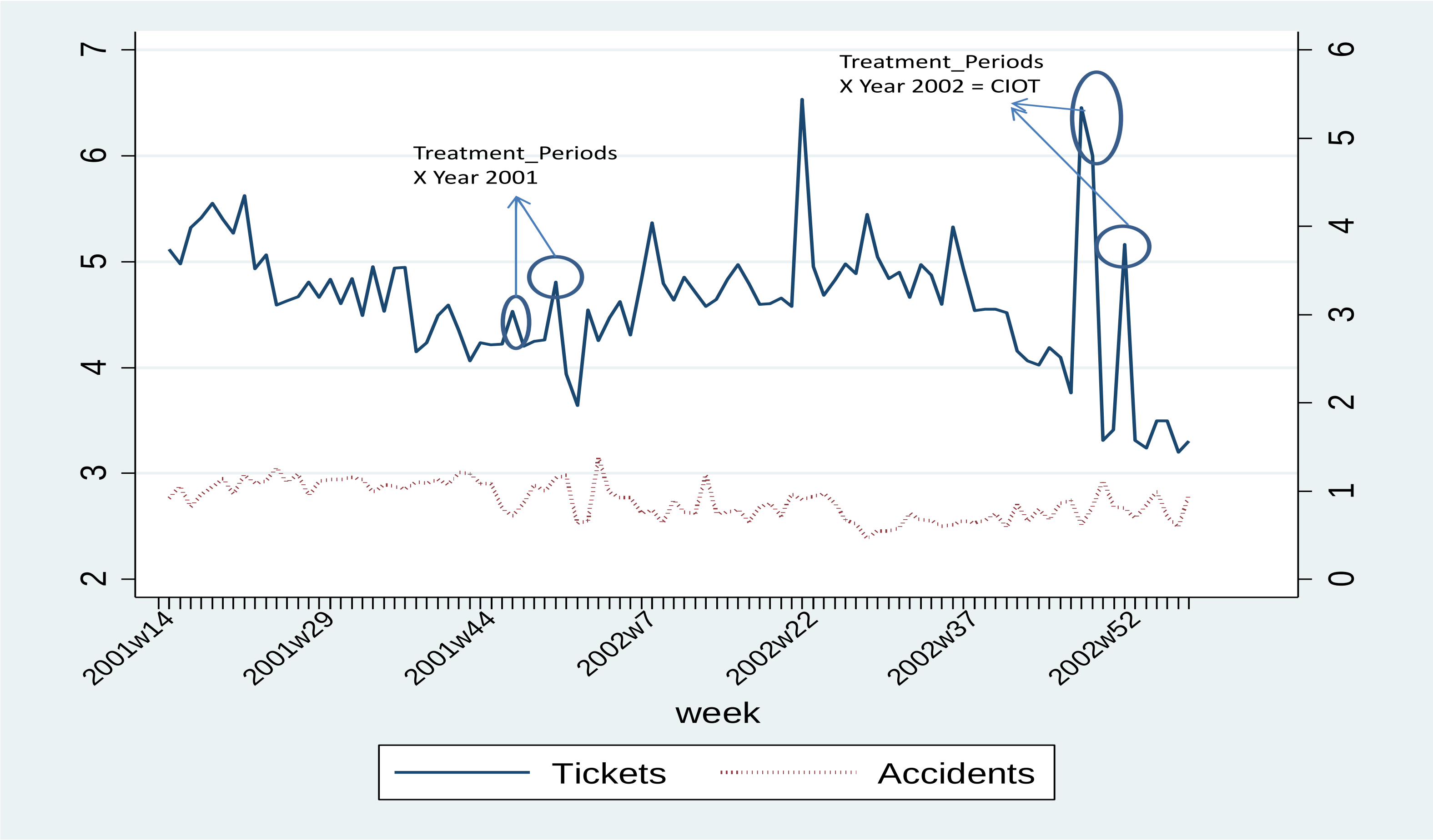

Figure 3 – Most Common Traffic Violations by Period

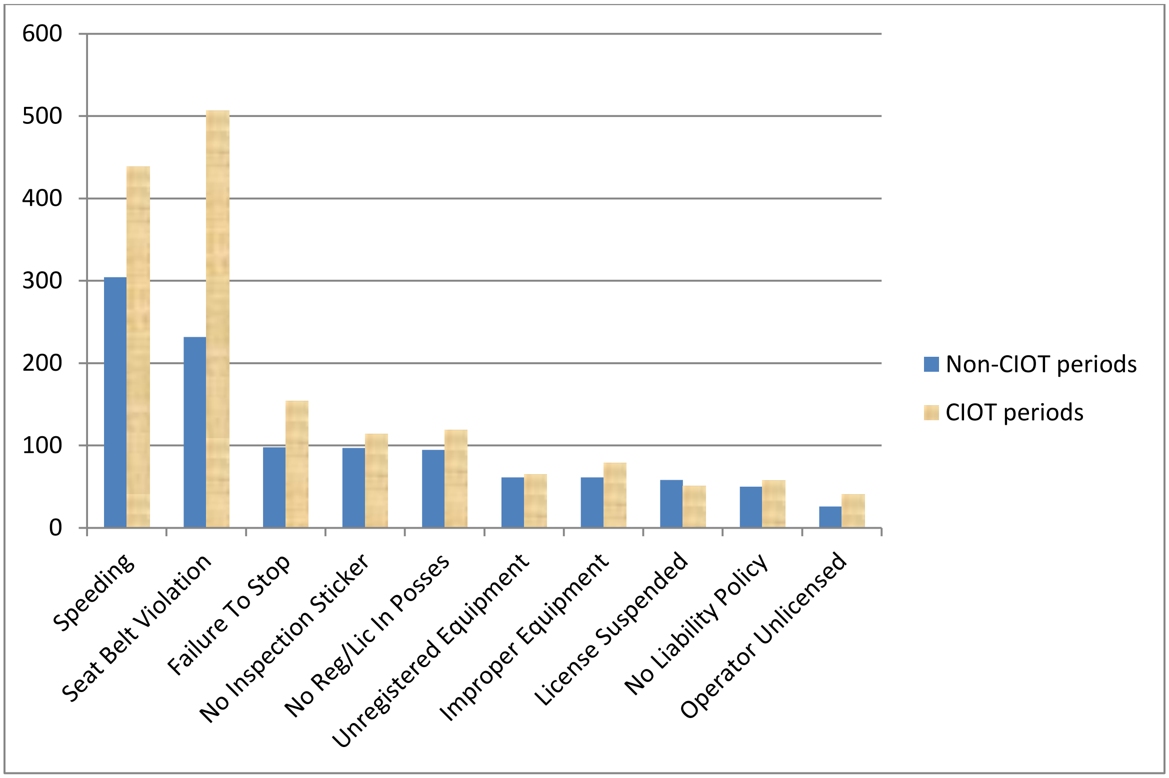

Table 1A. Summary Statistics

Variable	Mean	Std. Dev.
Tickets per 100 miles	4.78	6.88
Accidents per 100 miles	0.88	1.44
Injuries per 100 miles	0.38	1.03
Fatalities per 10,000 miles	0.30	6.11
Temperature (F)	51.01	17.45
Snowfall (inches)	0.11	0.69
Snow Depth (inches)	0.81	3.16
Rainfall (inches)	0.11	0.30
Population	18,307	37,357
Retail gas price	1.42	0.17
Unemployment rate	3.77	1.61
Registered cars per 100 miles	10,326	15,155
Trucks per 100 miles	5,126	6,158
Trailers per 100 miles	723.21	774.56
Motorcycles per 100 miles	411.47	484.61

Unit is at the day/municipality level.

The number of observations is 234,850

The sample period is from 4/1/2001 to 1/31/2003

Table 1B. Differences in Means of Selected Variables

Panel A: By Time of Day				
	Day	Night	Difference	
Tickets	2.96	1.82	1.14	***
Accidents	0.67	0.21	0.45	***
Injuries	0.28	0.10	0.18	***
Fatalities	0.15	0.15	0.00	
Panel B: By Gender				
	Female	Male	Difference	
Tickets	1.25	3.07	-1.82	***
Accidents	0.57	0.81	-0.24	***
Injuries	0.11	0.11	0.00	**
Fatalities	0.05	0.14	-0.08	***
Panel C: By Age Group				
	15-24	25-64	Difference	
Tickets	1.41	2.88	-1.469	***
Accidents	0.36	0.90	-0.535	***
Injuries	0.06	0.14	-0.079	***
Fatalities	0.04	0.12	-0.073	***

* significant at 10% ** significant at 5% *** significant at 1%

Unit is at the day/municipality level.

Panel A

Day is defined from 6 a.m. to 6 p.m; night from 6 p.m. to 6 a.m.

The sample period is from 4/1/2001 to 1/31/2003

The number of observations is 234,850

Panels B & C

The sample period is from 1/1/2002 to 1/31/2003

The number of observations is 138,204

Table 2. OLS Results: The Impact of Tickets on Motor Vehicle Accidents, Injuries, and Fatalities

	Accidents	Injuries	Fatalities
	(1)	(2)	(3)
Tickets	0.0082***	0.0042***	0.0002
	(0.0021)	(0.0013)	(0.0033)

– The sample size is 235,521. Standard errors clustered by municipality are presented in parentheses.

– Tickets, Accidents, and Injuries are measured per 100 miles of public road. Fatalities are measured per 10,000 miles of public road.

– All regressions include month-by-year, day of week, and holiday weekends dummies, a dummy for *Treatment_Periods*, and a full set of controls.

– Controls include daily municipality-specific measures of average snowfall, snow depth, rainfall, temperature, retail gas prices (plus squared terms), annual municipality-specific measures of population, unemployment rate, number of registered automobiles, trucks, motorcycles, and trailers per 100 miles.

Table 3. Reduced Form Evidence: The Impact of CIOT Mobilizations on Motor Vehicle Accidents, Injuries, and Fatalities

	Tickets	Accidents	Injuries	Fatalities
	(1)	(2)	(3)	(4)
CIOT	1.904***	-0.100***	-0.0260	0.0341
	(0.176)	(0.0238)	(0.0171)	(0.1045)

– The sample size is 235,521. Standard errors clustered by municipality are presented in

– Tickets, Accidents, and Injuries are measured per 100 public road mileage. Fatalities are measured per 10,000 public road mileage.

– All regressions include month-by-year, day of week, and holiday weekends dummies, a dummy for *Treatment_Periods*, and a full set of controls.

– Controls include daily municipality-specific measures of average snowfall, snow depth, rainfall, temperature, retail gas prices (plus squared terms), annual municipality-specific measures of population, unemployment rate, number of registered automobiles, trucks, motorcycles, and trailers per 100 miles.

Table 4. IV Results: The Impact of Tickets on Motor Vehicle Accidents, Injuries, and Fatalities

	Accidents	Injuries	Fatalities
	(1)	(2)	(3)
Tickets	-0.0519***	-0.0137*	0.0179
	(0.0125)	(0.0084)	(0.0548)

– The sample size is 235,521. Standard errors clustered by municipality are presented in parentheses.

– Tickets, Accidents, and Injuries are measured per 100 public road mileage. Fatalities are measured per 10,000 public road mileage.

– All regressions include month-by-year, day of week, and holiday weekends dummies, a dummy for Treatment_Periods, and a full set of controls.

– Controls include daily municipality-specific measures of average snowfall, snow depth, rainfall, temperature, retail gas prices (plus squared terms), annual municipality-specific measures of population, unemployment rate, number of registered automobiles, trucks, motorcycles, and trailers per 100 miles.

Table 5. Comparing Mechanisms: Tickets versus Information

Panel A. Reduced Form Evidence: The Impact of the CIOT Campaign by Active Ticketing

	Tickets	Accidents	Injuries	Fatalities
	(1)	(2)	(3)	(4)
CIOT * Active_Ticketing	4.0517***	-0.1183***	-0.0873***	0.163
	(0.3284)	(0.0359)	(0.0310)	(0.136)
CIOT	-0.6039	-0.0407	0.0159	-0.0480
	(0.1194)	(0.0325)	(0.0198)	(0.134)

Panel B. IV Results: The Impact of Tickets for only Municipalities that Actively Ticketed

	Accidents	Injuries	Fatalities
	(1)	(2)	(3)
Tickets	-0.0456***	-0.0210***	0.0033
	(0.0100)	(0.0074)	(0.0528)

* significant at 10% ** significant at 5% *** significant at 1%

– The number of observations in Panels A and B are 234,850 and 121,451 respectively. Standard errors clustered by municipality are presented in parentheses.

– Tickets, Accidents, and Injuries are measured per 100 miles of public road. Fatalities are measured per 10,000 miles of public road.

– A municipality is defined as participating if the daily average of tickets issued during the CIOT mobilizations is greater than the rest of the sample period.

– All regressions include month-by-year, day of week, and holiday weekends dummies, a dummy for Treatment_Periods, an interaction term between Active_Ticketing * Treatment_Periods, and a full set of controls.

– Controls include daily municipality-specific measures of average snowfall, snow depth, rainfall, temperature, retail gas prices (plus squared terms), annual municipality-specific measures of population, unemployment rate, number of registered automobiles, trucks, motorcycles, and trailers per 100 miles.

Table 6. Robustness Check: Using November and December Months only

Panel A: Reduced Form Evidence

	Tickets	Accidents	Injuries	Fatalities
	(1)	(2)	(3)	(4)
CIOT	1.777***	-0.129***	-0.0423**	-0.0032
	(0.171)	(0.0242)	(0.0182)	(0.1003)

Panel B: IV Results

	Accidents	Injuries	Fatalities
	(1)	(2)	(3)
Tickets	-0.0729***	-0.0238**	-0.0018
	(0.0144)	(0.0108)	(0.0697)

* significant at 10% ** significant at 5% *** significant at 1%

– The number of observations is 42,700. Standard errors clustered by municipality are presented in parentheses.

– Tickets, Accidents, and Injuries are measured per 100 miles of public road. Fatalities are measured per 10,000 miles of public road.

– All regressions include month-by-year, day of week, and holiday weekends dummies, a dummy for Treatment_Periods, and a full set of controls.

– Controls include daily municipality-specific measures of average snowfall, snow depth, rainfall, temperature, retail gas prices (plus squared terms), annual municipality-specific measures of population, unemployment rate, number of registered automobiles, trucks, motorcycles, and trailers per 100 miles.

Table 7. Robustness Check: Using Time Series Data from April 1, 2001 to January 31, 2003

Panel A: Reduced Form Evidence - The Impact of the CIOT Campaign on Tickets, Accidents, Injuries, and Fatalities

	Tickets	Accidents	Injuries	Fatalities	Tickets	Accidents	Injuries	Fatalities
	Levels				Logs			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CIOT	1.9411***	-0.1155*	-0.0297	0.0505	0.4685***	-0.1625**	-0.1301*	-0.2428
	(0.2945)	(0.0665)	(0.0290)	(0.1251)	(0.0802)	(0.0686)	(0.0750)	(0.3789)
Observations	671	671	671	671	671	671	671	479

Panel B: IV Results - The Impact of Tickets on Accidents, Injuries, and Fatalities

	Accidents	Injuries	Fatalities	Accidents	Injuries	Fatalities
	Levels			Logs		
	(1)	(2)	(3)	(4)	(5)	(6)
Tickets	-0.0595*	-0.0153	0.0261	-0.3468**	-0.2776*	-0.6322
	(0.0329)	(0.0123)	(0.0623)	(0.1507)	(0.1583)	(0.9552)
Observations	671	671	671	671	671	479

* significant at 10% ** significant at 5% *** significant at 1%

– Tickets, Accidents, and Injuries are measured per 100 miles of public road. Fatalities are measured per 10,000 miles of public road.

– All regressions include month-by-year and day of week dummies, and a dummy for Treatment_Periods. Controls include daily averages of snowfall, snow depth, rainfall, temperature, retail gas prices (plus squared terms), annual averages of population, unemployment rate, number of registered automobiles, trucks, motorcycles, and trailers per 100 miles.

Table 8. Heterogeneous Effects by Time of Day, Gender, and Age Group

Panel A - By Daytime or Nighttime

	Accidents	
Time of Day	Day	Night
Tickets by Day/Night	-0.036***	-0.117***
	(0.014)	(0.032)
% change in accidents	-5.425	-54.50
% change from 1-unit increase in tickets by day/night	33.80	54.97
Elasticity w.r.t tickets by Day/Night	**-0.161**	**-0.991**

Panel B - By Gender

	Accidents	
Gender	Female	Male
Tickets	-0.029***	-0.027***
	(0.008)	(0.010)
% change in accidents	-15.05	-5.056
% change in 1-unit increase in tickets	20.93	20.93
Accident elasticity w.r.t tickets	**-0.719**	**-0.242**

Panel C - By Age Group

	Accidents	
Age Group	15-24	25-64
Tickets	-0.016***	-0.032***
	(0.006)	(0.012)
% change in accidents	-15.99	-13.14
% change in 1-unit increase in tickets	20.93	20.93
Accident elasticity w.r.t tickets	**-0.764**	**-0.628**

* significant at 10% ** significant at 5% *** significant at 1%

– Number of observations in Panel A is 234,850. Number of observations in Panels B and C is 138,204.

– Each cell represents the IV estimate from a different regression.

– Tickets, Accidents, and Injuries are measured per 100 miles of public road. Fatalities are measured per 10,000 miles of public road.

– All regressions include month-by-year, day of week, holiday weekends dummies, and a full set of controls.

– Controls include daily municipality-specific measures of average snowfall, snow depth, rainfall, temperature, retail gas prices (plus squared terms), annual municipality-specific measures of population, unemployment rate, number of registered automobiles, trucks, motorcycles, and trailers per 100 miles.