More Tickets, Fewer Accidents: How Cash-Strapped Towns Make for Safer Roads

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Abstract

Traffic accidents are one of the leading causes of injury and death in the U.S. The role of traffic law enforcement in the reduction of accidents has been studied by relatively few papers and with mixed results that may be due to a simultaneity problem. Traffic law enforcement may reduce accidents, but police are also likely to be stricter in accident-prone areas. We use municipal budgetary shortfalls as an instrumental variable to identify the effect of traffic citations on traffic safety and show that budgetary shortfalls lead to more frequent issuance of tickets to drivers. Using a panel of municipalities in Massachusetts, we show that increases in the number of tickets written reduce motor vehicle accidents and accident related injuries, and that tickets issued to younger drivers have a larger effect in reducing accidents. The findings show that failure to control for endogeneity results in a significant underestimation of the positive impact of law enforcement on traffic safety.

Keywords: traffic accidents, safety, law enforcement, simultaneity

JEL Codes: K32. K42, H71, C33

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I. Introduction

Traffic accidents remain one of the leading causes of injury and death in the United States. Placed in a public health context, motor vehicle accidents are the 9th leading cause of death in the United States, with a mortality rate of 15.3 per 100,000 population (Heron 2007; Miniño, Heron et al. 2007). The average driver has a one in fifteen chance of being involved in a traffic accident during a given year.[1] A wide range of social scientists, including economists, has long studied the efficacy of policies, such as speed limits and mandatory seat belt use, intended to improve traffic safety.[2] The impact of these policies, however, is contingent on the enforcement of their associated laws and relatively few papers examine the effect of law enforcement on automobile accidents (McCarthy 1999; Redelmeier, Tibshirani et al. 2003).

Rational choice predicts that as officers issue more tickets to drivers operating in violation of the law, drivers respond to the increasing cost of breaking the law by driving more safely.[3] This model predicts that ticketing leads to fewer motor vehicle accidents.[4] However, to date there is little evidence on whether and by how much enforcement reduces accidents. Only a handful of studies address this issue.[5] Results of existing

[1] National Highway Traffic Safety Administration, Traffic Safety Facts Report, 2001.
[2] These policies include mandatory seat belt use (Loeb 1995), air bags (Kneuper and Yandle 1994; Levitt and Porter 2001), the speed limit (Forester, McNown et al. 1984), motorcycle helmet laws (Jones and Bayer 2007), the drinking age (Asch and Levy 1990), and vehicle safety inspections (Merrell, Poitras et al. 1999).
[3] Rational choice models predict that the levels of enforcement and punishment for traffic violations are based on the degree of infringement by the offending party, the marginal returns to local safety, and the costs of enforcement (Becker 1968; Lee 1985; Polinsky and Shavell 1992; Ehrlich 1996).
[4] In a survey conducted by Williams et al.(1995) asking drivers how different factors motivated them to practice safe driving habits, 61 percent of respondents said concern that they may receive a traffic fines motivated them "a lot," ranking only behind potential accidents (82 percent) and potential increase in insurance premiums (63 percent).
[5] Using data from Canada, Redelmeier et al. (1995), found a negative short term effect of traffic citation on the likelihood of being involved which vanished after three months. These authors' simultaneous determination of accidents and citations may have led them to underestimate of the effect (p. 2181). McCarthy (1999) found that traffic arrests are negatively correlated with fatal accidents. More recent

studies on the effects of law enforcement have been called into question because they have not satisfactorily addressed the issue that enforcement and traffic accidents are simultaneously determined (Elvik 2002; Blais and Dupont 2005). For example, while stricter enforcement may reduce accidents, stricter enforcement may also be a response to higher accidents rates. That is, officers issue more fines in cities and in pockets of time identified with unsafe driving behavior and higher accident rates. In this case, ordinary least square estimation will not identify a causal effect of enforcement on accidents. Not controlling for the simultaneous determination of enforcement and accidents leads to a biased estimate, and thus an underestimation of the effect of enforcement.[6]

To examine the effect of traffic law enforcement on accidents we use a panel of municipality level monthly traffic accident and traffic stop data in over 300 towns in Massachusetts. We study a 21 months period, between 2001 and 2003. We control for omitted variables with month and municipality fixed effects and address the concern of time varying omitted variables with instrumental variable estimation. Our instrument is the financial health of a town, measured by whether a town asks voters to approve a property tax override referendum. An override referendum allows towns to collect more revenues than stipulated by Proposition 2 ½. By putting an override referendum in front of voters, the town board indicates that the town is in financial distress and that they would like to raise additional revenue.

When towns are in a financial distress, government officials have an incentive to seek extra revenues not only through an increase in property taxes, but also by increasing

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innovations in enforcement, such as red light and speed cameras, as well as driver intoxication checkpoints have also been studied (for a review of this literature, see Blais and Dupont 2005).

[6] Similar simultaneity issues arise in the study of police and deterrence of crimes (Levitt 1997; Levitt 2002).

fines. One potential source of fines are traffic tickets. We document that when towns seek extra revenues through override referenda, police officers in that town issue more traffic fines and that our instrument has a statistically significant impact on traffic tickets.

Because municipalities are small in Massachusetts, many drivers are out of town drivers. The median town area is only 20.5 square miles, over 78 percent of all accidents involve an out of town driver. When the fiscal situation is tight, municipal governments can turn to out of town drivers for increasing town revenues by increasing the probability of a fine for stopped out of town drivers. This motivates the use of an alternative instrument which allows us to examine whether the results are sensitive to the chosen instrumental variables. This alternative instrument is the number of stopped out of town drivers in cities with financial distress.[7] We find that in town with financial distress police officers are more likely to issue a ticket than a warning to out of town drivers. This behavior increases the number of tickets.[8] Since these decisions to issue additional tickets is uncorrelated with crashes, the number of stopped out of town drivers in towns in financial distress is a suitable alternative instrument.

We demonstrate that the OLS estimator reveals a positive correlation between tickets and crashes. OLS estimation including town and month fixed effects shows a negative correlation between tickets and crashes. Adding instrumental variable

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[7] Massachusetts provides for a invaluable setting for our study not just because of the Proposition 2 ½ budget institution, but also because of its 351 municipalities dividing what is, at 10,000 square miles, the 6th smallest state in the United States, into local towns with a median area of only 20.5 square miles. As a result, there is a relatively small radius from a driver's home (approximately 2.5 miles for a resident of the median town) that he or she is actually driving "in town." In our two years of traffic stops (including stops that result in tickets and those that do not), 63% of all stopped drivers are not local residents. A recent survey by Progressive Auto Insurance of 11,000 policyholders found that 77% of accidents happened more than two miles from their customer's home (Insurance.com 2007), which in Massachusetts would place the bulk of accident participants outside of their home municipality.

[8] These results are consistent with other recent work showing that towns in financial decline use traffic tickets as a revenue generation tool (Helland and Tabarrok 2002; Makowsky and Stratmann 2008).

estimation, using the Proposition 2 ½ related instruments, we document that tickets reduce car crashes and that the magnitude of this effect is nearly three times larger than in the OLS estimation. Further, we document that more enforcement reduces injuries and associated with traffic accidents. Results regarding fatalities show a negative correlation with enforcement, but are less conclusive.

We further explore how ticketing reduces traffic accidents, namely whether tickets given to a population commonly associated with riskier behavior have a larger effect on reducing accidents than tickets issued to less risky populations. We divide the sample by age and find that the traffic ticket effect is larger for those under the age of thirty.

II. Background on Institutions and Officer Strictness

In 1980 Massachusetts voters passed referendum Proposition 2 ½, which placed limits on both the maximum amount of revenue generated through property taxation by Massachusetts municipalities and the amount by which any municipality may increase this revenue from one year to the next. If a town government wishes to raise funds from property taxes beyond the levy limit prescribed by Proposition 2 ½, it has the option to pass an "override" referendum. An override referendum can be proposed and placed on an electoral ballot by a majority vote of the town board of selectmen (aldermen). The override question must be presented in dollar terms and specify the purpose of the additional funds. Passage of the override requires a majority vote of approval by the electorate (Massachusetts Department of Revenue 2001).

Evidence suggests that while limits on personal property taxation have curtailed spending (Cutler, Elmendorf et al. 1999; Bradbury, Mayer et al.), they have also made Massachusetts local governments more dependent on other local sources of revenues.[9] Galles and Sexton (1998), for example, suggest that increases in non-tax revenue may have returned spending to pre-Proposition 2 ½ levels. Non-property tax revenues include receipts from the motor vehicle excise, charges for services, departmental revenue (e.g. libraries), licenses and permits, and fines. Traffic citations fall under the category of fines.[10]

There are limitations, however, placed on revenue generated from fees, licenses, and permits. Municipalities are allowed only to recover one hundred percent of the cost of providing fee-based services.[11] In contrast, no statute or regulation limits revenue accrued from fines. Municipalities retain 50 percent of the revenues collected from traffic fines issued in their jurisdictions.[12] The remainder is allocated to the state treasury and

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[9] "Since the passage of Proposition 2 ½ in 1980, municipal budgeting has been revenue driven…Therefore, at the start of the annual budget process, a community should review its four major sources of revenue – tax levy, state aid, local estimated receipts, and available funds…However, because of the constraints of Proposition 2 ½, recent fluctuations in state aid, and the depletion of local reserves, communities have become more aware of local receipts as a source of needed funds." – (Division of Legal accessed January 23, 2006)

[10] (*Massachusetts Department of Revenue, Division of Local Services official Budget Control Worksheet for Local Receipts* http://www.dls.state.ma.us/publ/misc/umas.pdf, accessed January 23, 2006)

[11] Some municipalities choose to recover only direct costs, while others include "indirect" costs as well, such as administrative and debt management costs.

[12] "Fines imposed under the provisions of chapters eighty-nine and ninety, including fines, penalties and assessments imposed under the provisions of chapter ninety C for the violation of the provisions of chapters eighty-nine and ninety, fines assessed by a hearing officer of a city or town as defined in sections twenty A and twenty A 1/2 of chapter ninety and forfeitures imposed under the provisions of section one hundred and forty-one of chapter one hundred and forty, shall be paid over to the treasury of the city or town wherein the offense was committed; provided, however, that only fifty per cent of the amount of fines, penalties and assessments collected for violations of section seventeen of chapter ninety or of a special speed regulation lawfully made under the authority of section eighteen of said chapter ninety shall be paid over to the treasury of the city or town wherein the offense was committed and the remaining fifty per cent shall be paid over to the state treasurer and credited to the Highway Fund. "(Massachusetts State Law. Part IV, Title II, Chapter 280, Section 2.)

the Highway Fund. Because towns can keep half of the revenues from traffic tickets, this revenue can serve as a substitute for property tax revenue.

When a municipal government faces a budgetary shortfall, that is, revenue expectations fall short of desired spending, it has the option of calling for a Proposition 2 ½ override referendum. A referendum's wording includes the total amount of additional local property tax revenue government officials will collect and the manner in which the additional revenue will be spent. The referendum is subject to a majority-rule vote open to all local voters. The failure of an override referendum reduces funds that would have otherwise been available for the designated fiscal year. Failure of a referendum is likely to make local officials more eager to pursue alternative sources of revenue.

Officers have the discretion to issue a warning, which carries neither a fine nor points for the driver's record. [13, 14] Makowsky and Stratmann (2009) show in a cross-section that failure to pass an override referendum increases officer strictness.[15] In these towns officers issue fewer warnings and more tickets to out of town drivers. Officers can exercise this discretion, because in Massachusetts it is up to an officer's judgment whether to issue a fine or a warning for traffic infractions. For example, when a police officer stops a driver for driving in excess of the speed limit, the officer is not obligated to issue the driver a citation and a fine.

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[13] Officers' use of discretion under Massachusetts General Law Part I Chapter 90C Section 3 was recently challenged by the Newton (MA) Police Association. Their appeal was ruled against by the Massachusetts State Court of Appeals, protecting the capacity of officers to issue warnings, *NEWTON POLICE ASSOCIATION vs. POLICE CHIEF OF NEWTON* (Massachusetts State Court of Appeals, 6/9/2005)

[14] During the time period studied in this paper (2001 to 2003) the Massachusetts police did not keep explicit records of warnings, Rather, we exclude records of stops in which a fine of zero dollars was recorded as warnings, counting only stops where the driver was issued a fine as tickets.

[15] Makowsky and Stratmann (2008) show this in a cross section for a two month period. These findings are consistent with the results by (Garrett and Wagner 2008) who find that officers in North Carolina issue more tickets in the year after a decline in county revenue.

III. Data and Empirical Methods

The Massachusetts legislature required the collection of data from traffic stops between April 1, 2001 and January 31, 2003. These data include information on every traffic stop in Massachusetts during this time. Data include whether a stopped driver received a ticket or a warning[16], the driver's age, place of residence, gender, and the type of infraction, including miles per hour over the speed limit when it was a speeding related offense.[17]

The Massachusetts Highway Department and Highway Safety Division collects accident data in its Crash Data System (CDS). CDS data include all reported accidents involving property damage in excess of $1,000 to any vehicle or other property, a fatality or injury. Reports are submitted to the Registry of Motor Vehicles (RMV) by police, agencies, and drivers who are involved in accidents that qualify. Accidents include collisions with objects, pedestrians, and other vehicles. We obtained accident data from the CDS from April 1, 2001 to January 31, 2003, the time span for which traffic ticket data are available.

The daily accident data and daily traffic stop data consist of reports from each individual traffic accident and stop. A recorded accident event always involves at least one automobile. While an event always represents a single "crash," an event may account for multiple (or zero) injuries or fatalities. For each municipality we aggregated to the month the accidents and number of traffic tickets, so that our unit of observation is the

[16] In this data set warnings are not explicitly labeled and we categorize all observations with a fine of zero dollars as warnings. These observations account for 46 percent of observations, similar to 48 percent warning rate observed in a subset of the data wherein warnings are explicitly labeled.

[17] Traffic stop data was collected by Massachusetts State legislature, and provided to us by Bill Dedman of the Boston Globe and MSNBC.com.

total number of tickets and accidents per month in each municipality. The rationale for the aggregation to the month level is that some of our control variables are based on the fiscal year, such as which fiscal year is affected by the passage or failure of the override referendum, while other controls are based on the annual year, such as unemployment.[18]

Our measure for traffic law enforcement is the sum of tickets issued by local officers that are related to traffic safety.[19] Parking tickets, for example, are not included in this data set. Table A1 in the appendix shows the types of violations that resulted in a ticket. The most commonly issued tickets are for speeding, comprising 39 percent of all tickets. The next most common are tickets issued for seat belt violations (13 percent) and failure to stop (12 percent).

Figure 1 plots the mean number of crashes and tickets over time. The first month in the figure is April 2001 and the last month is January 2003. The figure shows no strong pattern suggesting that traffic tickets reduce car accidents. In November 2002 the number of tickets reaches its maximum. This maximum is not due to any one town issuing many tickets in that month, but instead reflects a uniform increase in ticketing in all towns.

To control for other factors that affect car accidents, besides tickets, we estimate the regression

$$(1) \quad \text{Accidents}_{it} = \beta_0 + \beta_1 \text{Tickets}_{it} + \beta_2 \mathbf{StoppedDrivers}_{it} + \beta_3 \mathbf{MunicipalityX}_{it} + \text{Municipality}_i + \text{Month}_t + \varepsilon_{it}$$

[18] We also could have collapse the data to quarters or weeks. However, we have two incomplete quarters, and using a month as a unit of observation allows us to have complete data for each observation. Further, when using months instead of weeks, we have fewer zeros in the dependent variables, and thus the month unit makes ordinary least squares a defensible estimation method.

[19] We focus on local officers because of our instrumental variable strategy: local officers have an incentive to react to a budgetary shortfall of the municipality because they are employed by the town, while state troopers are employed by the state.

The accidents and tickets variables in equation (1) measure how many traffic crashes and tickets were recorded in municipality *i* during month *t*. Depending on the specification, accidents is either the number of crashes or the number of crashes per capita. The vector $\mathbf{StoppedDrivers}_{it}$ includes the number of stopped drivers from out of town, and their characteristics, that is, the fraction of stopped drivers that are minority drivers, female drivers, the average age of a stopped driver, and the average speed that was recorded on the ticket or warning issued when the driver was stopped for driving in excess of the limit.[20] $\mathbf{MunicipalityX}_{it}$ is a vector of municipal characteristics. This vector includes population, Chapter 90 highway and road funding from the state, the property value per capita, and road safety related expenditures (not including spending on police and fire departments) per capita. These variables vary by fiscal year. The vector also includes the number of unemployment filings, and the number of registered vehicles per capita, which are measured for each calendar year.[21] Because our unit of observation is the month, we attribute the data that come by fiscal year to the months associated to that fiscal year and proceed similarly for calendar year data. To account for other sources of heterogeneity across municipalities that are constant over time, we include fixed effects for each municipality (Municipality_i) and month (Month_t).[22] We cluster standard errors by municipality in all specifications.

[20] Data on average monthly characteristics of all drivers in a town are not available. Instead, we use the data for those drivers who were stopped in the municipality by the local police. These are the data included in the StoppedDrivers variable. To the extent that characteristics of drivers who have accidents are likely to be more similar to those of stopped drivers than to all drivers, characteristics of stopped drivers may be a better measure than the unavailable measure of average driver characteristics.

[21] Municipal data, including records of override referendum votes and their outcomes, are from the Massachusetts Department of Revenue.

[22] There is no perfect collinearity between the month indicators and the annual municipality based variables because the latter vary by municipality.

The availability of detailed data on traffic tickets dictates the time period of our analysis. Our data span over 3 fiscal years and three calendar years. A fiscal year in Massachusetts runs from July 1^{st} to June 30^{th}. For example, the fiscal year 2002 runs from July 1^{st} 2001 until June 30^{th} 2002. We have 3 months of data from fiscal year 2001 (4/1/2001 to 6/31/2001), 12 months from fiscal year 2002 (7/1/2001 to 6/31/2002), and 6 months from fiscal year 2003 (7/1/2002 to 1/31/2003). With OLS, the tickets variable is likely to be correlated with the error term, ε_{it}, resulting in biased estimates. The reason for the endogeneity is an omitted variable bias: in towns where drivers drive recklessly, many tickets are issued and many crashes occur. Thus OLS will underestimate the true effect of tickets on accidents. The inclusion of municipal fixed effects alleviates some of the omitted variable problem because it accounts for town specific factors that simultaneously affect tickets and crashes. However, fixed effects cannot control for time-varying omitted variables that are specific to the municipality. An example of such a variable is a local event, which may be associated with both more traffic tickets and accidents. To address this issue we use as an instrument whether a town is in financial distress. Traffic tickets are one source of revenues and city officials have an incentive to seek more funds through traffic fines when the fiscal situation is bleak (Makowsky and Stratmann 2009).

Our measure of fiscal distress is whether a town puts an override referendum in front of voters and whether it failed or passed. A referendum can be held at anytime during the year. In our data referenda occur at a higher frequency in the spring, but they are held almost all times of the year. The wording of the referendum has to be specific in that it says how much money is requested and for what purpose the money will be used. Further, the referendum always applies to the following fiscal year.

Our first stage regression is

(2) $\text{Tickets}_{it} = \beta_0 + \boldsymbol{\beta_1 \text{Override}_{it}} + \boldsymbol{\beta_2 \text{StoppedDrivers}}_{it} + \boldsymbol{\beta_3 \text{MunicipalityX}}_{it} + \text{Municipality}_i + \text{Month}_t + \mu_{it}$

With this equation we test whether towns that are in a financial crunch are more likely to issue tickets to increase local revenues. The **Override** vector includes an indicator variables for whether a override referendum passed (OverridePass_{it}) during the fiscal year, whether the referendum failed (OverrideFail_{it}), as well as separate measures for the total dollar amounts requested when an override referendum failed ($\$\text{OverrideFail}_{it}$) and when it passed ($\$\text{OverridePass}_{it}$).[23] We include the OverridePass_{it} variable in addition to the OverrideFail_{it} variable because excluding the former would lump towns with no override referendum together with towns that had a successful referendum. We include $\$\text{OverridePass}_{it}$ and $\$\text{OverrideFail}_{it}$, because the dollar amount of the increase in property tax revenue requested by the local offers a measure of the magnitude of fiscal distress.

A strong indicator that a town is fiscally healthy, with regard to revenues, is the absence of an override referendum vote. When local officials call for an override referendum, they are indicating an anticipated revenue shortfall. When a called override referendum fails, town officials may try to collect revenues via alternative means, such as traffic tickets. If a town tries to collect extra traffic ticket revenues when an override referendum fails, the estimated coefficient on failed override referenda will be positive.

[23] In a handful of instances, multiple referenda were called within town during the same fiscal year, with some passing and others failing. Due to the ambiguity of this outcome, these observations were dropped from the analysis.

Even if the override vote passes, however, there is reason to believe that towns will issue more tickets, and the estimated coefficient on passed override referenda will be positive as well. While likely better off than if the override vote had failed, towns that pass an override are less fiscally sound relative to towns whose revenues were sufficient to begin with and did not need to call for an override referendum. Further, towns that pass an override referendum have raised only the additional revenue to support exactly what was enumerated in the request for additional tax revenue, leaving no slack for underestimated and unanticipated expenses. Town officials may believe that larger requests are less likely to pass and therefore may ask for only a fraction of their desired amounts in the referendum. Regardless of whether an override passes or fails, larger dollar amount requests indicate greater fiscal distress, suggesting that the estimated coefficient on the dollar amounts of passed and failed override referenda will be positive.

In the second stage regression, the first stage controls for town specific characteristics via town fixed effects. Therefore, the effect of override referenda is identified by changes in whether a town asked for tax increases through referenda. Failure to approve an override referendum may affect traffic accidents via other avenues than tickets. Towns where a referendum fails may shift funds from street maintenance or other projects related to public safety to other areas. Worse street maintenance, pedestrian pathways, or public signage, could lead to an increase in accidents. To address this issue we control in our regression for spending on road maintenance and other public safety spending.[24] To the extent that these variables do not control for all road maintenance activities, there will be a bias against a finding that more enforcement reduces traffic accidents.

[24] Our "other public safety" spending measure does not include spending on police and fire departments.

IV. Results

Table 1 gives descriptive statistics for the data. In our data set there are on average 37 car accidents per month per town, ranging from zero accidents during a month in a few towns to 674 in Worcester in the month of October 2001. All recorded accidents involve at least one automobile. For 409 observations, or less than 6 percent of the observations the traffic accident variable has a value of zero. The injury variable equals zero for 16 percent of the sample used, fatalities equals zero for 90%. The mean number of tickets is 82, and for less than one percent of the sample zero tickets were recorded in a given month.

The override failure variable takes the value of one for 2.4 percent of the sample, and includes 18 of the 338 towns included in the sample. The override pass variable takes the value of one for 9 percent of the sample, representing 69 of all towns analyzed. Towns where all referenda failed asked for an average total of $1,327,761, while towns where all referenda passed received an average of $1,031,709.

Table 2 shows the effect of our measures for fiscal distress on the number of tickets issued. Column 1 contains the dummy failed overrides, passed overrides, the dollar amount requested in all passed referenda, and the dollar amount in failed referenda. The second column mirrors the first with respect to the excluded instruments, but adds variables controlling for relevant town and driver characteristics. Column 2 is first stage for the regression results in Table 3, column 3. We include the first column of Table 2 to show that the effect of fiscal distress on ticket giving is robust to whether or not time-varying municipality and driver characteristics are included in the specification. All regressions have month and town fixed effects, and we cluster standard errors at the town level.

In Table 2, column 2 shows that the coefficient on the dollar amount associated with failed referenda is positive and significant, while the coefficient on override failure indicator is positive, but not statistically significant. This shows that the larger the amount asked for and rejected by voters, the larger the number of tickets issued. The point estimate on the passage of an override referendum shows that passage leads to a drop in the number of tickets written by 15.7 tickets, but that with each $100,000 increase in the amount asked for in the referendum, another 2.1 tickets are written. This implies that a passed override referendum leads to fewer tickets when the amount asked for was below $750,000 and that the number of tickets issued increases for higher amounts. [25] In our data sample, 30 percent of passed referendum were for requests in excess of $750,000, and thus resulted in additional tickets being issued.

While the positive coefficient on $\$OverridePass_{it}$ is in line with our predictions, the negative sign on $OverridePass_{it}$ is different than expected. The total effect of a passed override with a large price tag, when considering coefficients on both $OverridePass_{it}$ and $\$OverridePass_{it}$, however, is consistent with our hypothesis. The results in column 2 suggest that passage of overrides requesting large dollar amounts correspond to significant fiscal distress leading to more tickets issued. Smaller passed overrides, however, appear to alleviate the fiscal pressure to pursue alternative revenues, and in turn correspond to fewer tickets.

[25] We have 661observations where the override referendum passed. For these observations the mean (standard deviation) dollar amount asked for in the referendum was $ 1,020,783 (1,626,738). For failed override referenda we have 171 observations with a mean (standard deviation) of $1,327,761 (3,425,252).

Table 2, column 2 shows that the Kleibergen-Paap F statistic is 14.06 for the excluded instruments. This indicates that the outcome of the override referenda and their associated dollar amounts are strong instruments and that the use of these instruments results only in a small bias of two stage lest squares.[26] For example, a five percent bias is associated with a Kleibergen-Paap F statistic of 16.85 (ten percent with an F statistic of 10) (see Stock and Yogo 2005). The Anderson-Rubin Wald test offers a more robust test of the potential weakness of instruments, especially for models, such as ours, which use a large number of excluded variables to identify a single endogenous variable (Stock, Wright et al. 2002). The Anderson-Rubin null hypothesis that the excluded variables coefficients equal zero can be rejected at the less than 1 percent level in all IV specifications used in this paper.

Table 3 reports the results from examining the determinants of automobile accidents. When estimating the regression without town fixed effects, but including month effects, the point estimate is positive, and statistically significant at the 10 percent level (Table 3, column 1).[27] These findings are likely due to the omitted variable bias indicated previously: in dangerous towns, more tickets are issues and more automobile accidents occur. Column 2 controls for such town specific and time invariant factors via town fixed effects as well as month effects. Now the coefficient on tickets is negative and statistically significant, indicating that OLS without town fixed effects underestimates the effectiveness of traffic law enforcement on accidents. The point estimate in column 2 implies that 100 extra tickets lead to 5.5 fewer car crashes. In our data set the mean number of accidents and tickets are 37 and 83 respectively, with standard deviations of

[26] See Stock and Yogo 2001 for a discussion of this issue http://ksghome.harvard.edu/~jstock/pdf/rfa_6.pdf.
[27] Without month effects the point estimate is positive and statistically significant as well.

with a standard deviations of 60 for accidents and 132 for tickets. Thus, the 0.055 point estimate implies that a one standard deviation increase in tickets leads to 7 fewer accidents, almost 12 percent of the standard deviation in accidents.

The fixed effects specification does not control for the possibility that dangerous behavior ebbs and flows within a municipality, and that law enforcement responds accordingly. If changes in dangerous behavior within a town lead to more tickets and more accidents, then the coefficient in column 2 is biased upward. Columns 3 of Table 3 addresses this concern by using the instruments and first stage presented in Table 2, column 2. The bottom panel of the table includes results for overidentifying restrictions tests, as well as Kleibergen-Paap and Anderson-Rubin results, as evidence for the validity of our instruments. The overidentifying restrictions test does not reject the null-hypothesis that the instruments are valid.

The results in column 3 show that, as predicted, the magnitude of the coefficient on tickets increases when addressing the endogeneity concerns via instrumental variables. Relative to OLS, the coefficient on tickets more than doubles, and suggests that 100 extra tickets lead to 14.3 fewer car crashes. The results from this 2SLS model imply that a one standard deviation increase in tickets leads to a reduction of accidents by a third of the standard deviation of accidents.

Table 3, columns 5 through 7 present model the same specifications as in columns 1 through 4, but with crashes per capita as the dependent variable. The coefficients on tickets issued exhibits signs and magnitudes that correspond to the results in columns 1 through 4. Similar to the previous analysis, the observed coefficient on tickets is positive in the OLS specification (column 5), becomes negative when adding municipality fixed effects (column 6), and doubles in magnitude when instrumental variables are added (columns 7). The results in columns 7 suggest that for every 1,000 tickets written, there are 2.12 fewer accidents per 1,000 population. This estimate implies that a one standard deviation increase in tickets reduces accidents per 1000 capita by 0.28, or 23 percent of a standard deviation in per capita accidents.

Among our control variables, our measure of traffic density, measured as the number of registered vehicles per capita has a consistently positive and statistically significant effect on crashes in the town fixed effects regressions (Table3, columns 2 and 3): more density leads to more crashes. In these specifications the point estimates on out of town drivers are positive and statistically significant, indicating that towns with more out of town drivers have more crashes on their roads. Each additional out of town driver is associated with a between 0.04 and 0.06 increase in the number of crashes.

To examine how our second stage estimates are sensitive to the selection of instruments we introduce an alternative set of instruments. This set is motivated by politicians' incentives to tax non voters instead of voters. Politicians have an incentive to export taxes because when taxes are levied on local voters, those voters may be less likely to vote for them. But this response is not possible for non-voters. Applying this logic to traffic tickets, politicians would prefer to raise traffic ticket revenue from out of town drivers instead of local drivers. We will therefore test whether police officers in towns that are in financial distress issue more tickets to out of town drivers than to local drivers. In these town, stopped out of town drivers who were previously let off with a warning will instead receive a ticket when a town is in fiscal distress, implying that the threshold of issuing a ticket to out of town drives is lower in towns in financial distress.

We will test this hypothesis by including interaction terms between override referenda and out of town drivers in our regression. These interaction terms serve as our additional instruments.

This additional strictness towards out of town drivers can have an impact on traffic safety because out of town drivers represent a large fraction of driver volume. This is because in Massachusetts, where municipalities average only 20.5 square miles, the majority of drivers on roads within a municipal police department's jurisdiction are from out of town. One indicator of such is that the majority of drivers stopped by police officers are from out of town. For the time period that we examine in this paper, out of town drivers represent sixty-six percent of drivers stopped, and sixty-nine percent of drivers issued tickets.[28]

There is also some direct evidence out of town drivers are involved in the majority of car crashes. For a sub-period of our data set, from January 1, 2002 through January 31, 2002 we have data on whether a driver who is involved in an accident is from out of town. Earlier data, that is data for the first nine months of our data set, are not available.[29] Table A2 shows that between January 2002 and January 2004 the number of accidents by out of town drivers is roughly proportional to the number of tickets to out of town drivers, and that out of town drivers are involved in the vast majority of accidents. Seventy-eight percent of all accidents had at least one driver involved who was from out

[28] The prevalence of out of town drivers is also increased by our exclusion of Boston, whose population density far exceeds the rest of the state. Only 13% of drivers stopped in our sample are from out of state. Out of state drivers represent a slightly different set of circumstances because they can, potentially, be identified by an officer prior to a stop by their out of state license plate.

[29] According to Richard Conard from the Massachusetts Department of Highway Traffic Engineering prior to 2002 and older accident records computer system was used which did not contain the residence of drivers.

of town. Thus, increasing strictness on out of town drivers is in fact an increase in strictness on drivers who are primarily involved in crashes.

Therefore, our alternative specification for the first stage is

(3) $\text{Tickets}_{it} = \beta_0 + \boldsymbol{\beta_1 \text{Override}_{it}} + \boldsymbol{\beta_2 \text{Override}_{it}} * \text{StoppedDrivers}_{it} + \beta_3 \text{StoppedDrivers}_{it} + \boldsymbol{\beta_4 \text{MunicipalityX}}_{it} + \text{Municipality}_i + \text{Month}_t + \mu_{it}$

where we now interact the **Override** vector with the number of stopped out of town drivers. Stopped out of town drivers are drivers whose license plate and drivers' license indicates that they are from out of town. It is a count of the number of stopped out of town drivers. Some of these drivers have received a ticket, and others a warning.

The results from this specification are shown in column 3 and 4 of Table 2. Both columns differ in that column 3 excludes control variables that appear in the second stage (with the exception of the "out of town drivers stopped" variable) while column 4 does not. Column 4 is the first stage for regressions shown in later tables. The results in both columns lend support to the tax exportation hypothesis. The coefficients on the interaction effects between out of town drivers and the override failure indicator and between out of town drivers and the override pass dollar amount are positive and statistically significant. The introduction of these interaction effects makes the point estimates on the levels of override failure and passage statistically insignificant, suggesting that much of the extra ticketing associated with override referendum is due to ticketing of out of town drivers. The findings show that out of town drivers' likelihood of receiving a ticket increases when they drive through towns that had put an override referendum to voters. [30] Relative to column 2 in Table 2 the Kleinbergen-Paap F-statistic increases to 18.7, suggesting that our instruments are strong.

The second stages for this alternative set of instruments are in Table 3, columns 4 and 8. The point estimates in the regressions explaining for total number of crashes and per capita crashes are very similar to our first set of instruments. For example, the previous point estimate on tickets in column 3 was 0.143, and the point estimate on tickets in column 4 which uses this alternative set of instruments is 0.145. Further, in both specifications the test for overidentifying restrictions indicates that our instruments are valid.

Table 4 analyses the effects of tickets on injuries (column 1 to 4) and on fatalities (columns 5 to 8). Injuries and fatalities are measure of the severity of accidents. As for accidents, the OLS estimate without municipality fixed effects shows a positive coefficient of tickets (Table 4, column 1), but the sign reverses and becomes negative and statistically significant when adding municipality fixed effects (Table 4, column 2). For fatalities, the sign on tickets also changes from positive to negative when adding town fixed effects, and the estimates are not statistically significant in both cases. The 2SLS estimates, however, show a negative and statistically significant effect on tickets for the number of injuries and fatalities.[31] The coefficient on tickets in the injury regression doubles from the OLS specification in column 2 to the instrumental variable specification in column 3 in Table 4. It indicates that 100 additional tickets lead to 6 fewer injuries associated with traffic accidents. This implies that a one standard deviation in tickets

[30] Support for our hypothesis that the finding is driven by increasing strictness to stopped out of town drivers, rather than by stopping more out of town drivers comes of a regression of the number of stopped out of town drivers on measures of fiscal distress. In these regressions we do not find evidence that more out of town drivers are stopped when a town is financially strapped.

[31] These 2SLS estimates as well as those in the remaining tables have Table 2, column 4 as the first stage. The estimates in Table 4 and subsequent tables are very similar when the first stage is Table 2, column 2.

reduces injuries by 27 percent of a standard deviation in injuries. For the injury per capita variable as the dependent variable, Table 4, column 4 shows that the point estimate on tickets is not statistically significant, although it has the anticipated negative sign. Columns 5 to 8 of Table 4 shows the same set of regressions for fatalities. OLS shows a negative effect of tickets on fatalities, and 2SLS also shows that tickets reduce fatalities. The latter point estimate is statistically significant at the ten percent level. The coefficient on fatalities per capita, however, is not statistically significant, with a positive coefficient close to zero.

To this point, the results show that tickets are an effective means for reducing accidents and injuries, while the effects of ticket issuance on fatalities is less conclusive. This is likely, in part, because car accidents may result in injuries, but whether they result in fatalities as opposed to a serious injury has some randomness to it, and may require a larger sample over a longer period of time to find statistically significant effects. Fatalities may also be more dependent on driver specific factors, such as whether the driver was wearing a seat belt, for which we are unable to control.

Next, we examine the effect of tickets on a subset of drivers, namely young drivers. These drivers are disproportionately represented in traffic accidents for reasons of both inexperience and still developing higher-order cognitive skills (Deery 1999). Younger drivers on average have lower incomes and in turn greater sensitivity to potential fines and associated increases in insurance premiums. Given the greater sensitivity of young drivers to costs of receiving a citation, and given that they are disproportionately represented in accidents we will test whether tickets issued to drivers under the age of thirty will reduce the number of accidents by a greater amount than tickets written to all drivers.

Table 5 presents the first stage estimates using the number of tickets issued to drivers under thirty years of age. The point estimates are smaller in magnitude and the significance levels are a little lower than those using the full sample. However, the basic findings of Table 2 remain: stopped out of town drivers are more likely to receive a ticket when they drive through towns experiencing a budget crunch. The Kleinbergen-Paap F-statistics are, depending on the specification, 14.5 and 12.1, which suggests the instrument remains strong and the bias from using instrumental variables will still be relatively small.

Tables 6 (crashes) and 7 (injuries and fatalities) report model specifications that correspond to Tables 3 and 4, but test the impact of only tickets issued to drivers under the age of 30. The signs of the results are similar to analysis of the full sample, but with larger coefficient magnitudes. The coefficient on tickets in Table 6, column 3 implies that a one standard deviation increase in tickets to drivers under the age of 30 (mean 42, standard deviation. 63) reduces accidents by 31.8 which is 53 percent of a standard deviation in accidents. The results in Table 7, column 3 imply that a one standard deviation increase in tickets under the age of 30 reduces injuries by 12.5, or 46 percent of a standard deviation in injuries. This effect of ticketing to drivers under 30 is three times larger than the effect found in the sample of all drivers. We also find that tickets decrease crashes per capita. The coefficient on tickets in Table 6, columns 8, corresponds to five fewer accidents per thousand population for every 1000 tickets written. This estimate implies that a one standard deviation increase in tickets to drivers under 30 reduces accidents per 1,000 capita by 0.315 or 41 percent relative to the mean.

V. Conclusion

This paper shows that traffic fines reduce the number of car accidents and related injuries. We address the endogeneity problem that remains after using town and time effects by estimating the fixed effects model with instrumental variables. Our instrument is whether a town asked for more money through an override referendum and it's interaction with stopped out of town drivers. Using panel data, we find that more tickets are issued when a town has asked for an override referendum, and that tickets issuance increase the more out of town drivers that are stopped, lending support to the tax exporting hypothesis while controlling for town fixed effects. Using these estimates, we find that tickets are a far more effective reducer of car accidents and automobile accident related injuries than ordinary least square estimation would indicate. The results from this 2SLS model imply that a one standard deviation increase in tickets leads to a reduction of accidents by a third of the standard deviation of accidents.

Arguably, the traffic policy debate with the highest public profile has been the speed limit. The push for a fifty-five mile per hour speed limit on all U.S. highways centered on the proposition that reducing the average speed of American drivers would prevent accidents and save lives (Jondrow, Marianne et al. 1983; Moore, Dolinis et al. 1995). Lave (1985), however, offered a different theory, proposing that it is the variance of driver speed, and not the mean, that is the leading determinant of traffic safety. Keeler (Keeler 1994) has since found additional evidence supporting the Lave's theory of driver speed variance. The variance of any distribution is sensitive to outliers (Mosteller and Tukey 1977). In the context of driver speed, outliers are by definition breaking the law,[32] often exceeding the speed limit in great excess. If variance is the key determinant of traffic accidents, then the legislated speed limit diminishes in importance, and the level of enforcement rises in importance. By punishing drivers exceeding the speed limit, traffic tickets can reduce the variance of driver speed, thereby reducing traffic accidents.

[32] While we are mainly speaking to outliers on the upper (higher speed) tail of the distribution, outliers in the lower tail are often violating minimum speed limits, and adding the variance of driver speed as well.

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Figure 1

Average number of crashes and tickets across municipalities by month

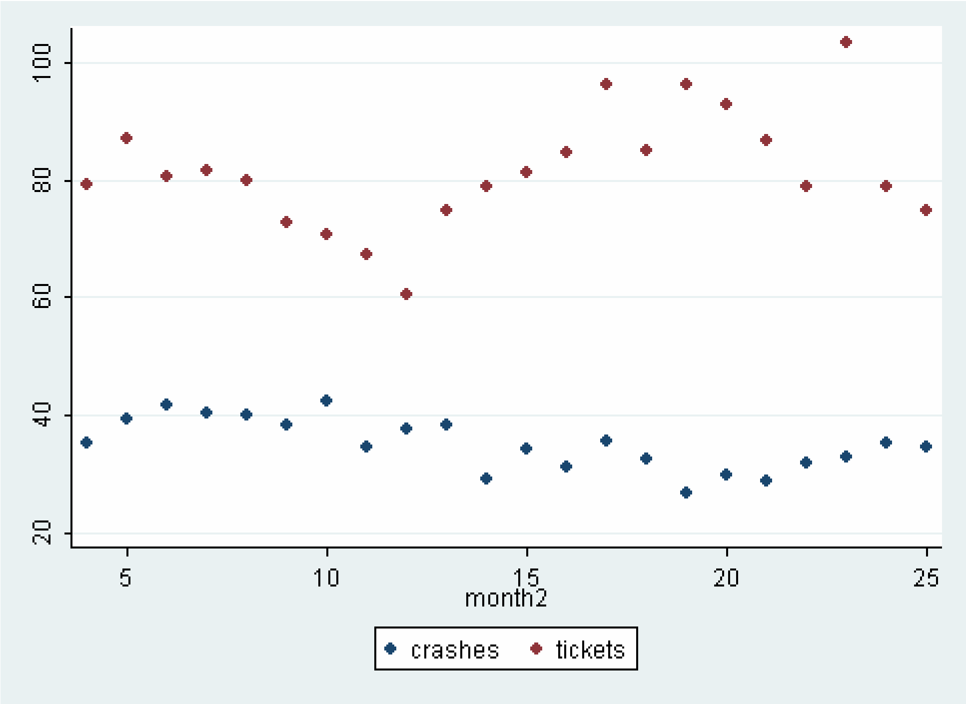

Table 1
Summary Statistics

Variable	Mean	Std. Dev.	Min	Max
Accidents	36.89	60.25	0	674
Accidents per 1000 capita	1.77	1.22	0	10.10
Tickets	82.63	129.01	0	1940
Tickets to drivers under 30	41.71	62.94	0	947
Injuries	15.82	27.33	0	315
Injuries per 1000 capita	0.77	0.72	0	10.30
Fatalities	0.12	0.39	0	6
Fatalities per 1000 capita	.008	.049	0	1.33
Out of Town Drivers Stopped	111.48	135.76	0	1678
Other Public Safety expenditures per capita	28.16	29.74	0.14	310.70
Average Mph over the speed limit	17.21	2.83	7.50	50.00
Number of minority drivers stopped	31.49	85.54	0	1271
Total registered vehicles per capita	1.01	0.18	0.49	2.15
Chapter 90 Highway funding per mile	3236.21	1005.93	1894.95	11459.58
Unemployment	491.20	717.93	3	5509
Property Value ($10,000) per capita	11.40	15.58	1.93	281.89
Average age of stopped drivers	32.28	4.06	0	69.50
Population (1000s)	17.99	22.05	0.35	175.71
Number of female stopped drivers	47.74	74.92	0	795
Override Pass	0.09	0.29	0	1
Override Fail	0.02	0.15	0	1
Failed Referenda Dollars ($100,000)	0.32	5.70	0	176.70
Passed Referenda Dollars ($100,000)	0.96	5.80	0	117.62

N= 7,038. All dollars are in 2003 CPI adjusted dollars.

Table 2
Override Referenda and Traffic Tickets

	(1)	(2)	(3)	(4)
	OLS	OLS	OLS	OLS
Override Pass	-20.294**	-15.718**	-7.558	-7.540
	(6.122)	(3.793)	(5.766)	(5.695)
Override Fail	-6.266	1.719	-9.509+	-9.094
	(7.928)	(4.455)	(5.670)	(5.793)
Override Pass Dollars	2.643**	2.091**	0.223	0.327
	(0.532)	(0.339)	(0.207)	(0.216)
Override Fail Dollars	0.067	0.091*	0.015	0.052
	(0.048)	(0.038)	(0.080)	(0.067)
Out of Town Drivers Stopped * Override Pass			0.010	0.008
			(0.071)	(0.069)
Out of Town Drivers Stopped * Override Fail			0.093+	0.114*
			(0.054)	(0.047)
Out of Town Drivers Stopped * Override Pass Dollars			0.004**	0.005**
			(0.001)	(0.001)
Out of Town Drivers Stopped * Override Fail Dollars			0.001	0.001
			(0.002)	(0.001)
Out of Town Drivers Stopped		0.247**	0.525**	0.237**
		(0.071)	(0.065)	(0.072)
Other Public Safety expenditures per capita		0.158		0.132
		(0.117)		(0.117)
Average Mph over the speed limit		-0.486+		-0.456+
		(0.269)		(0.255)
Number of minority drivers stopped		0.310**		0.316**
		(0.112)		(0.113)
Total registered vehicles per capita		-23.589**		-24.366**
		(8.208)		(8.237)
Average age of stopped drivers		0.070		0.061
		(0.121)		(0.119)
Number of female stopped drivers		0.459*		0.460*
		(0.178)		(0.178)
Chapter 90 Highway funding per mile		-0.046		-0.044
		(0.027)+		(0.027)
Unemployment		0.127		0.127**
		(0.029)**		(0.030)
Property Value per capita		0.529		0.571
		(0.397)		(0.435)
Population		-26.774+		-29.179+
		(15.566)		(16.072)
Constant	81.210**	586.509+	-17.150	624.510*
	(3.078)	(310.827)	(11.074)	(317.318)
Town and month fixed effect?	Yes	Yes	Yes	Yes
Kleibergen-Paap F-Stat		14.02		18.66
N	7080	7038	7080	7038
R-squared	0.85	0.93	0.92	0.93

Robust standard errors, clustered by municipality, in parentheses. + significant at 10%; * significant at 5%; ** significant at 1%. Town and month fixed effect include 338 municipalities and 21 individually coded months.

Table 3
Effects of Traffic Enforcement on Accidents

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Crashes				Crashes per 1000 capita			
	OLS	OLS	IV	IV	OLS	OLS	IV	IV
Tickets	0.074+	-0.055**	-0.143**	-0.145**	0.001	-0.001*	-0.002*	-0.002**
	(0.040)	(0.017)	(0.024)	(0.015)	(0.001)	(2.9E-4)	(0.001)	(4.4E-4)
Out of Town Drivers Stopped	0.045	0.039**	0.061**	0.062**	0.004**	0.002**	0.002**	0.002**
	(0.053)	(0.012)	(0.016)	(0.015)	(0.001)	(0.001)	(0.001)	(0.001)
Other Public Safety expenditures per capita	0.035	0.062	0.072	0.073	0.001	-0.001	-0.001	-0.001
	(0.041)	(0.063)	(0.058)	(0.058)	(0.002)	(0.004)	(0.003)	(0.003)
Average Mph over the speed limit	-0.034	-0.039	-0.076	-0.077	-0.004	0.006	0.005	0.005
	(0.299)	(0.084)	(0.087)	(0.086)	(0.013)	(0.008)	(0.008)	(0.008)
Number of minority drivers stopped	-0.281**	0.013	0.040	0.040	-0.004**	-6.6E-5	4.0E-4	3.0E-4
	(0.096)	(0.029)	(0.027)	(0.026)	(0.001)	(4.4E-4)	(0.001)	(0.001)
Total registered vehicles per capita	14.689	11.842**	9.609**	9.545**	0.857*	0.491+	0.453	0.461+
	(10.521)	(3.327)	(3.270)	(3.265)	(0.371)	(0.285)	(0.277)	(0.279)
Average age of stopped drivers	0.221+	-0.007	0.001	0.001	0.013+	0.005	0.005	0.005
	(0.117)	(0.031)	(0.034)	(0.034)	(0.008)	(0.004)	(0.004)	(0.004)
Number of female stopped drivers	0.026	0.002	0.044	0.046	-0.004*	-0.002+	-0.001	-0.001
	(0.086)	(0.023)	(0.029)	(0.030)	(0.002)	(0.001)	(0.001)	(0.001)
Chapter 90 Highway funding per mile	0.002	0.018+	0.014+	0.013+	1.2E-4	3.7E-4	2.9E-4	3.0E-4
	(0.003)	(0.010)	(0.008)	(0.008)	(8.6E-5)	(3.8E-4)	(3.4E-4)	(3.5E-4)
Unemployment	0.041**	0.004	0.014	0.015	0.001**	1.2E-6	1.8E-4	1.5E-4
	(0.010)	(0.012)	(0.012)	(0.012)	(1.5E-4)	(1.4E-4)	(1.8E-4)	(1.6E-4)
Property Value per capita	-0.051	0.040	0.086	0.088	-0.005	2.7E-4	0.001	0.001
	(0.068)	(0.112)	(0.101)	(0.101)	(0.004)	(0.006)	(0.006)	(0.006)
Population	1.307**	7.808	5.934	5.880	-0.008	0.120	0.088	0.095
	(0.492)	(10.056)	(9.496)	(9.472)	(0.006)	(0.163)	(0.156)	(0.154)
Constant	-34.133*	-180.238			-0.305	-2.581		
	(13.947)	(185.797)			(0.624)	(3.002)		
Kleibergen-Paap F-Stat			14.02	18.66			14.02	18.66
Overidentifying Restrictions (p value)			0.75	0.86			0.79	0.92
Anderson-Rubin			p<0.001	p<0.001			p<0.001	p<0.001
Month fixed effect?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Town fixed effect?	No	Yes	Yes	Yes	No	Yes	Yes	Yes
R-squared	0.80	0.95			0.14	0.62		

N = 7,038. Robust standard errors, clustered by municipality, in parentheses. + significant at 10%; * significant at 5%; ** significant at 1%. Town and month fixed effect include 338 municipalities and 21 individually coded months. The first stage for columns 3 and 7 is column 2 in Table 2, and the first stage for columns 4 and 8 is Table 2, column 4.

Table 4
Effect of Traffic Enforcement on Injuries and Fatalities

	(1) Injuries OLS	(2) Injuries OLS	(3) Injuries IV	(4) Injuries per 1000 capita IV	(5) Fatalities OLS	(6) Fatalities OLS	(7) Fatalities IV	(8) Fatalities per 1000 capita IV
Tickets	0.017	-0.027**	-0.056**	-1.4E-4	-2.8E-4	-3.6E-4	-0.001+	1.5E-5
	(0.022)	(0.009)	(0.017)	(3.6E-4)	(2.3E-4)	(2.9E-4)	(0.001)	(2.8E-5)
Out of Town Drivers Stopped	0.040	0.026**	0.033**	0.001**	1.9E-4	2.4E-4	3.3E-4	1.0E-5
	(0.026)	(0.007)	(0.009)	(4.2E-4)	(1.6E-4)	(2.4E-4)	(2.9E-4)	(1.6E-5)
Other Public Safety expenditures per capita	0.004	0.026	0.029	-2.7E-4	-2.2E-4	0.001	0.001	-9.1E-5
	(0.016)	(0.031)	(0.029)	(0.002)	(1.8E-4)	(0.001)	(0.001)	(1.2E-4)
Average Mph over the speed limit	0.043	0.014	0.002	-0.002	2.1E-4	-0.003+	-0.004*	-0.001**
	(0.127)	(0.047)	(0.048)	(0.004)	(0.002)	(0.002)	(0.002)	(2.7E-4)
Number of minority drivers stopped	-0.036	-0.012	-0.003	-3.7E-4	6.3E-5	3.9E-4	0.001+	-7.63E-6
	(0.055)	(0.024)	(0.021)	(3E-4)	(1.9E-4)	(3.0E-4)	(3.6E-4)	(1.4E-5)
Total registered vehicles per capita	3.967	9.933**	9.186**	0.625**	0.141**	-0.071	-0.085	0.020
	(5.164)	(2.419)	(2.382)	(0.236)	(0.039)	(0.109)	(0.106)	(0.014)
Average age of stopped drivers	0.099*	-0.014	-0.011	0.004	-0.001	-0.001	-0.001	-2.6E-4
	(0.045)	(0.018)	(0.018)	(0.003)	(0.001)	(0.001)	(0.001)	(1.8E-4)
Number of female stopped drivers	-0.052	-0.002	0.012	-0.001*	6.6E-5	-1.8E-4	-7.5E-6	-2.9E-5
	(0.033)	(0.017)	(0.019)	(0.001)	(4.5E-4)	(0.001)	(0.001)	(2.7e-5)
Chapter 90 Highway funding per mile	-0.001	0.009+	0.008	1.2E-4	-4.3E-5**	2.9E-5	-9.9E-6	-5.4E-7
	(0.002)	(0.005)	(0.005)	(1.2E-4)	(1.1E-5)	(6.3E-4)	(6.8E-5)	(4.5E-6)
Unemployment	0.020**	-0.010*	-0.007	-6.4E-5	3.4E-5	8.2E-5	1.3E-4+	-4.0E-6
	(0.006)	(0.005)	(0.005)	(7.7E-5)	(3.7E-5)	(7.3E-5)	(9.9E-5)	(4.1E-6)
Property Value per capita	-0.024	0.046	0.062	0.004	-0.001*	-0.001	-0.001	-0.001
	(0.030)	(0.051)	(0.047)	(0.003)	(3.6E-4)	(0.002)	(0.002)	(4.4E-4)
Population	0.442+	1.004	0.377	-0.066	0.006**	-0.024	-0.036	0.001
	(0.266)	(4.380)	(4.181)	(0.075)	(0.001)	(0.059)	(0.058)	(0.002)
Constant	-4.816	-39.499			0.030	0.574		
	(5.573)	(79.023)			(0.072)	(1.134)		
Kleibergen-Paap F-Stat			14.02	14.02			14.02	14.02
Overidentifying Restrictions (p value)			0.75	0.41			0.77	0.72
Anderson-Rubin Wald			p<0.001	p<0.001			p<0.001	p<0.001
Month fixed effect?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Town fixed effect?	No	Yes	Yes	Yes	No	Yes	Yes	Yes
R-squared	0.73	0.91			0.11	0.17		

N = 7,038. Robust standard errors, clustered by municipality, in parentheses. + significant at 10%; * significant at 5%; ** significant at 1%. Town and month fixed effect include 338 municipalities and 21 individually coded months. The first stage for columns 3, 4, 7, and 8 is Table 2, column 2. Results are very similar to these results when the first stage is Table 2, column 4.

Table 5
Override Referenda and Traffic Tickets to Drivers Under 30

	(1) OLS	(2) OLS	(3) OLS	(4) OLS
Override Pass	-7.348**	-5.098**	-1.301	-1.146
	(2.243)	(1.288)	(2.303)	(2.120)
Override Fail	-4.947	-1.756	-8.068**	-9.038**
	(3.913)	(2.473)	(3.020)	(3.434)
Override Pass Dollars	0.832**	0.600**	0.181+	0.218*
	(0.191)	(0.086)	(0.095)	(0.102)
Override Fail Dollars	0.035	0.049*	0.035+	0.062**
	(0.027)	(0.022)	(0.019)	(0.023)
Out of Town Drivers Stopped * Override Pass			-0.025	-0.023
			(0.025)	(0.021)
Out of Town Drivers Stopped * Override Fail			0.060**	0.078**
			(0.023)	(0.021)
Out of Town Drivers Stopped * Override Pass Dollars			0.001*	0.001**
			(0.000)	(0.000)
Out of Town Drivers Stopped * Override Fail Dollars		0.098**	0.231**	0.097**
		(0.034)	(0.030)	(0.034)
Out of Town Drivers Stopped		0.058		0.049
		(0.052)		(0.052)
Other Public Safety expenditures per capita		-0.180		-0.177
		(0.126)		(0.122)
Average Mph over the speed limit		0.216**		0.217**
		(0.046)		(0.046)
Number of minority drivers stopped		-6.939+		-7.139+
		(3.796)		(3.756)
Total registered vehicles per capita		-0.576**		-0.579**
		(0.086)		(0.085)
Average age of stopped drivers		0.180*		0.180*
		(0.087)		(0.087)
Number of female stopped drivers		-0.015		-0.015
		(0.011)		(0.011)
Chapter 90 Highway funding per mile		0.057**		0.057**
		(0.017)		(0.017)
Unemployment		0.204		0.216
		(0.173)		(0.183)
Property Value per capita		-10.617		-11.133
		(6.672)		(6.813)
Population	39.570**	243.072+	-3.367	251.353+
	(1.402)	(132.021)	(5.456)	(133.954)
Constant	-7.348**	-5.098**	-1.301	-1.146
	(2.243)	(1.288)	(2.303)	(2.120)
Town and month fixed effect?	Yes	Yes	Yes	Yes
Kleibergen-Paap F-Stat		14.49		12.11
N	7080	7038	7080	7038
R-squared	0.86	0.93	0.91	0.93

Robust standard errors, clustered by municipality, in parentheses. + significant at 10%; * significant at 5%; ** significant at 1%. Town and month fixed effect include 338 municipalities and 21 individually coded months.

Table 6

Effect for Traffic Enforcement on Accidents for Drivers under 30

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Crashes				Crashes per 1000capita			
	OLS	OLS	IV	IV	OLS	OLS	IV	IV
<30 Tickets	0.128	-0.084+	-0.504**	-0.470**	0.003**	-0.001	-0.007*	-0.005+
	(0.089)	(0.051)	(0.090)	(0.080)	(0.001)	(0.001)	(0.003)	(0.004)
Out of Town Drivers Stopped	0.055	0.034**	0.075**	0.072**	0.004**	0.002**	0.003**	0.002**
	(0.052)	(0.013)	(0.022)	(0.021)	(0.001)	(0.001)	(0.001)	(0.001)
Other Public Safety expenditures per capita	0.035	0.059	0.079	0.078	0.001	-0.001	-0.001	-0.001
	(0.040)	(0.064)	(0.057)	(0.057)	(0.002)	(0.004)	(0.003)	(0.003)
Average Mph over the speed limit	-0.038	-0.029	-0.096	-0.091	-0.004	0.006	0.005	0.005
	(0.305)	(0.085)	(0.102)	(0.099)	(0.013)	(0.008)	(0.008)	(0.008)
Number of minority drivers stopped	-0.279**	0.014	0.104*	0.097*	-0.004**	-1.3E-4	0.001	0.001
	(0.097)	(0.034)	(0.044)	(0.042)	(0.001)	(0.001)	(0.001)	(0.001)
Total registered vehicles per capita	14.654	12.610**	9.447**	9.705**	0.855*	0.503+	0.453	0.471+
	(10.794)	(3.392)	(3.506)	(3.470)	(0.371)	(0.286)	(0.278)	(0.283)
Average age of stopped drivers	0.362**	-0.060+	-0.300**	-0.280**	0.015*	0.004	4.1E-4	0.002
	(0.123)	(0.033)	(0.069)	(0.063)	(0.008)	(0.004)	(0.004)	(0.005)
Number of female stopped drivers	0.030	-0.008	0.070	0.063	-0.004**	-0.002*	-0.001	-0.001
	(0.074)	(0.021)	(0.044)	(0.041)	(0.002)	(0.001)	(0.001)	(0.001)
Chapter 90 Highway funding per mile	0.002	0.019+	0.012	0.013+	1.2E-4	3.9E-4	2.8E-4	3.2E-4
	(0.003)	(0.011)	(0.008)	(0.008)	(8.7E-5)	(3.9E-4)	(3.3E-4)	(3.5E-4)
Unemployment	0.040**	0.002	0.025+	0.023	0.001**	-4.1E-5	3.2E-4	1.9E-4
	(0.009)	(0.012)	(0.015)	(0.015)	(1.4E-4)	(1.4E-4)	(2.5E-4)	(2.4E-4)
Property Value per capita	-0.049	0.028	0.108	0.102	-0.006	7.5E-5	0.001	0.001
	(0.069)	(0.115)	(0.106)	(0.104)	(0.004)	(0.006)	(0.006)	(0.006)
Population	1.313**	8.215	4.371	4.685	-0.007	0.128	0.068	0.089
	(0.491)	(10.288)	(9.800)	(9.792)	(0.006)	(0.165)	(0.159)	(0.157)
Constant	-40.997**	-189.598			-0.400	-2.766		
	(14.051)	(190.717)			(0.619)	(3.044)		
Kleibergen-Paap F-Stat			14.49	12.11			14.49	12.11
Overidentifying Restrictions (p value)			0.96	0.35			0.80	0.92
Anderson-Rubin Wald			p<0.001	p<0.001			p<0.001	p<0.001
Month fixed effect?	Yes	Yes	Yes		Yes	Yes	Yes	Yes
Town fixed effect?	No	Yes	Yes		No	Yes	Yes	Yes
R-squared	0.80	0.95			0.15	0.62		

N = 7,038. Robust standard errors, clustered by municipality, in parentheses. + significant at 10%; * significant at 5%; ** significant at 1%. Town and month fixed effect include 338 municipalities and 21 individually coded months. The first stage for columns 3 and 7 is column 2 in Table 5, and the first stage for columns 4 and 8 is Table 5, column 4.

Table 7
Effect for Traffic Enforcement on Injuries and Fatalities for Drivers under 30

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Injuries	Injuries	Injuries	Injuries per 1000 capita	Fatalities	Fatalities	Fatalities	Fatalities per 1000 capita
	OLS	OLS	IV	IV	OLS	OLS	IV	IV
< 30 Tickets	0.013	-0.030	-0.199**	-4.2E-4	-3.4E-4	-4.7E-4	-0.002	-4.2E-4
	(0.042)	(0.025)	(0.065)	(0.001)	(3.9E-4)	(0.001)	(0.003)	(0.001)
Out of Town Drivers Stopped	0.043	0.022**	0.039**	0.001**	1.4E-4	2.0E-4	3.9E-4	0.001**
	(0.026)	(0.008)	(0.012)	(4.3E-4)	(1.6E-4)	(2.3E-4)	(3.6E-4)	(4.0E-4)
Other Public Safety expenditures per capita	0.003	0.024	0.032	-2.6E-4	-2.2E-4	0.001	0.001	-2.6E-4
	(0.016)	(0.032)	(0.029)	(0.002)	(1.8E-4)	(0.001)	(0.001)	(0.002)
Average Mph over the speed limit	0.043	0.021	-0.006	-0.002	2.3E-4	-0.003+	-0.004+	-0.002
	(0.128)	(0.047)	(0.055)	(0.004)	(0.002)	(0.002)	(0.002)	(0.003)
Number of minority drivers stopped	-0.035	-0.013	0.023	-3.2E-4	4.6E-5	3.7E-4	0.001	-3.2E-4
	(0.055)	(0.027)	(0.031)	(4.1E-4)	(2.0E-4)	(3.1E-4)	(0.001)	(4.1E-4)
Total registered vehicles per capita	3.977	10.395**	9.128**	0.630**	0.141**	-0.065	-0.080	0.625
	(5.207)	(2.491)	(2.470)	(0.236)	(0.039)	(0.109)	(0.107)	(0.236)
Average age of stopped drivers	0.119+	-0.034+	-0.130**	0.003	-0.002+	-0.001	-0.002	0.003
	(0.061)	(0.019)	(0.044)	(0.003)	(0.001)	(0.001)	(0.002)	(0.003)
Number of female stopped drivers	-0.044	-0.009	0.022	-0.001*	-1.7E-5	-2.6E-4	1.0E-4	-0.001
	(0.029)	(0.016)	(0.025)	(0.001)	(4.4E-4)	(0.001)	(0.001)	(0.001)
Chapter 90 Highway funding per mile	-0.001	0.010+	0.007	1.2E-4	-4.4E-5**	3.8E-5	5.1E-6	1.1E-4
	(0.001)	(0.006)	(0.004)	(1.2E-4)	(1.1E-5)	(6.5E-5)	(7.2E-5)	(1.2E-4)
Unemployment	0.021**	-0.012*	-0.003	-5.8E-5	3.0E-5	6.5E-5	1.7E-4	-5.8E-5
	(0.007)	(0.005)	(0.007)	(9.6E-5)	(3.7E-5)	(7.0E-5)	(1.5E-4)	(9.6E-5)
Property Value per capita	-0.023	0.038	0.070	0.004	-0.001*	-0.001	-0.001	0.004
	(0.031)	(0.053)	(0.048)	(0.003)	(3.7E-4)	(0.002)	(0.002)	(0.003)
Population	0.432	1.306	-0.233	-0.067	0.007**	-0.021	-0.039	0.067
	(0.274)	(4.454)	(4.349)	(0.075)	(0.001)	(0.059)	(0.063)	(0.075)
Constant	-6.100	-46.577			-0.053	0.495		
	(5.805)	(80.903)			(0.069)	(1.131)		
Kleibergen-Paap F-Stat			14.49	14.49			14.49	14.49
Overidentifying Restrictions (p value)			0.70	0.40			0.78	0.40
Anderson-Rubin Wald			p<0.001	p<0.001			p<0.001	p<0.001
Month fixed effect?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Town fixed effect?	No	Yes	Yes		No	Yes	Yes	Yes
R-squared	0.72	0.91			0.11	0.17		

N = 7,038. Robust standard errors, clustered by municipality, in parentheses. + significant at 10%; * significant at 5%; ** significant at 1%. Town and month fixed effect include 338 municipalities and 21 individually coded months. The first stage for columns 3, 4, 7, and 8 is Table 5, column 2. Results are very similar to these results when the first stage is Table 5, column 4.

Appendix

Table A1
Breakdown of Twenty Most Common Violations*

Ticket Description	Frequency	Percent of Total
Speeding	238,234	38.48%
Seat Belt Violation	82,622	13.35%
Failure To Stop	72,178	11.66%
No Inspection Sticker	53,923	8.71%
Unregistered / Improper Equipment	23,945	3.87%
No Registration or License	19,676	3.18%
Improper Equipment	12,733	2.06%
Lane Violation	10,274	1.66%
Minor Traffic	10,112	1.63%
Fail To Use Safety	10,103	1.63%
Illegal Operation	9,305	1.50%
Street Highway Violation	9,277	1.50%
Right of Way Intersection	7,339	1.19%
Display Number Plate	5,241	0.85%
DPW State Highway Regulations	5,061	0.82%
Keep Right / No View	4,452	0.72%
No Child Restraint	4,341	0.70%
Improper Passing	3,785	0.61%
Fail to Yield to Pedestrian	3,073	0.50%
Impeding Operation	2,900	0.47%

* These violations account for 95% of the 619,104 traffic tickets issued by local officers from April 1, 2001 until January 31,2003.

Table A2. Out of Town Drivers on the Road for the January1, 2002 through January 31, 2003

	Out of Town Drivers (Percent)	Local Drivers	Drivers' Hometown Unidentified in Data Set	Total
Tickets (Recipient)	250,413 (67%)	123,640 (33%)	0	374,053
Crashes (Participants)	111,287 (78%)†	30,548 (21.5%)	250 (0.5%)	142,085

†Crashes that involve 1 or more out of town drivers are counted as "Out of Town Driver" Crashes