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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. 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 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 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. Using data from Canada, Redelmeier et al. (2003), found a negative short term effect of traffic citation on the likelihood of being involved which vanished after three months. These authors'

[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 et al. 1984), motorcycle helmet laws (Jones and Bayer 2007), the drinking age (Asch and Levy 1990), and vehicle safety inspections (Merrell 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).

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 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).

Results of existing 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.[5]

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

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

property tax revenues beyond Massachusetts legal limits established by Proposition 2 ½ in 1980. 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.

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

When using instrumental variables to identify causal relationships, validity of the instrument is always a concern. While it is unlikely that fiscal distress has a direct effect on traffic accidents, there are reasons for some concern. For example, Ruhm (2000) shows a positive correlation between recessions and several positive health outcomes. We address potential endogeneity issues through both a variety of control variables and the use of alternative instruments. We find our key results to be robust to a variety of model and instrument specifications.

When the fiscal situation is tight, fines exacted on drivers from out of town make an especially appealing source of additional revenue because they do not vote in local elections. Because municipalities are small in Massachusetts, many drivers are out of town drivers. The median town area is only 20.5 square miles and over 78 percent of all accidents involve an out of town driver. This makes it possible to test the sensitivity of our instrument to alternative specifications while controlling for the direct effect of fiscal conditions on accidents via the override indicator. This alternative instrument is the number of stopped out of town drivers in cities experiencing fiscal distress.[6] We find that in towns with fiscal distress police officers issue more tickets to out of town drivers.[7] Since it seems reasonable to assume that the interaction of out of town drivers stopped and local fiscal distress does not directly affect accidents, the interaction represents a suitable 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 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.

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

[6] 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.

[7] 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).

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, which allows additional revenue to be raised for the following fiscal year. An override referendum can be proposed and placed on an electoral ballot by a majority vote of the town board of selectmen (aldermen). 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 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). A passed referendum will raise the property tax revenue raised in the subsequent fiscal year, which begins July 1st and runs through June 31st of the following calendar year.

Evidence suggests that while limits on personal property taxation have curtailed spending (Cutler et al. 1999; Bradbury et al.), they have also made Massachusetts local governments more dependent on other local sources of revenues.[8] 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.[9]

[8] "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)

[9] (*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)

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.[10] 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.[11] The remainder is allocated to the state treasury and 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.

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

[11] "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.)

Officers have the discretion to issue a warning, which carries neither a fine nor points for the driver's record.[12, 13] Makowsky and Stratmann (2009) show in a cross-section that failure to pass an override referendum increases officer strictness.[14] 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.

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,[15] the driver's age, place of residence, gender, whether the stop was a night, and the type of infraction, including miles per hour over the speed limit when it was a speeding related offense.[16]

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[12] 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)

[13] 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.

[14] Makowsky and Stratmann (2008) show this in a cross section for a two month period across 350 municipalities. 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.

[15] 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.

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

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 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 filings.[17]

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

[17] 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.

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 percent. On average, police officers issued 82 tickets per municipality, per month, with zero tickets recorded in less than one percent of the sample.

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 $1,3 million, while towns where all referenda passed received an average of $1 million.

We use two measures for traffic law enforcement: the sum of tickets issued by local officers that are related to traffic safety and the sum of tickets issued per mile of local (non-highway) road.[18] 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

[18] 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. Local roads are all roads within the municipal boundaries excluding highways, which are the jurisdiction of Massachusetts state troopers.

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

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 mile of local road. The vector $\textbf{StoppedDrivers}_{it}$ includes the number of stopped drivers from out of town, and their characteristics, that is, the number 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, and the number of drivers stopped at night.[19] $\textbf{MunicipalityX}_{it}$ is a vector of municipal characteristics. This vector includes local police expenditures, Chapter 90 highway and road funding from the state, the property value per capita, road safety related expenditures (not including spending on police and fire departments) per capita, and population. These variables vary by fiscal year. The vector also includes the number of unemployment filings and the number of registered vehicles

[19] 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.

per capita, which vary by calendar year.[20] 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).[21] We cluster standard errors by municipality in all specifications.

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 1st to June 30th. For example, the fiscal year 2002 runs from July 1st 2001 until June 30th 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 fiscal

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

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

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, whether it failed or passed, and the dollar amount requested in the referenda. 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 at 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

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

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 ($\$\text{OverridePass}_{it}$).[22] We code the override indicator variable to equal one for each of the 12 months of the fiscal year to which referenda applies and zero otherwise. We include the OverridePass_{it} variable in addition to the OverrideFail_{it}

[22] 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.

variable because excluding the former would lump towns with no override referendum together with towns that had a successful referendum. We include $\$OverridePass_{it}$ and $\$OverrideFail_{it}$, 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. 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.[23] 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.

The most serious concern in our model is that fiscal distress is correlated with ε_{it} in (1) and thus that fiscal distress is correlated with driver behavior. For example, Ruhm (2000) finds that many population health outcomes improve during a recession. While he does not examine the relationship between government budgets and driver behavior, he does identify a negative relationship between the unemployment rate and the number of traffic accident related fatalities. Economic intuition would suggest that unemployed individuals would be driving less (absent a job to drive to) and might be more careful when they drove, being less able to "afford" the cost of a traffic accident. The correlation between budgetary shortfalls and unemployment is of course imperfect, but enough to warrant concern, particularly when fiscal distress reflects a distinctly local economic event (such as the closing of a major employer). We address this potential source of endogeneity two ways. First, we control for the number of unemployment filings in the

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

municipality in all specifications. Second, we run an alternative specification that uses the interaction of the Override vector and the number of out of town drivers stopped by local officers ($Outtown_{it}$* $\mathbf{Override_{it}}$**)** as our instrument**.** The interaction between out of town drivers stopped the override vector indicates the confluence of fiscal distress and the opportunity to export taxes to individuals who do not vote in local elections. We hypothesize that this combination of incentive and opportunity is exogenous to driver behavior and will identify an increase in tickets. Therefore, our alternative specification for the first stage is

(3) $$Tickets_{it} = \beta_0 + \boldsymbol{\beta_1 Override_{it}} + \boldsymbol{\beta_2 Override_{it}} * OuttownDrivers_{it} + \beta_3 StoppedDrivers_{it} + \boldsymbol{\beta_4 MunicipalityX}_{it} + Municipality_i + Month_t + \mu_{it}$$

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.
As such, we can control for the correlation of fiscal distress and traffic accidents in the second stage, while still identifying the relationship between tickets and accidents using the increase in tickets written to out of town drivers motivated by revenue needs. 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 (Makowsky and Stratmann 2009). Being from out of town, these drivers are less

connected to local economic conditions, and their driving behavior is less likely to correlate to local economic conditions.

In our data set, the bulk of drivers on a given stretch of road in Massachusetts 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.[24] 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, 2003 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.[25] Table A2 shows that between January 2002 and January 2003 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 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.

To test for the sensitivity of results to the measurement of the dependent variable, in a second set of specifications we measure the dependent variable as “crashes per miles of local road” where local roads constitute all roads within municipal boundaries excluding highways, which are the jurisdiction of state troopers. In these specifications,

[24] 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.
[25] According to the Massachusetts Department of Highway Traffic Engineering, the accident records computer system used prior to 2002 did not contain the residence of drivers.

all appropriate independent variables, including tickets, are measured per local mile.[26] As an additional robustness check, we ran the basic first stage specifications with a "placebo instrument." Tickets written by local officers are hypothesized to increase with local fiscal distress, while tickets written within municipal boundaries by state troopers should not

IV. Results

Table 2 shows the effect of our measures for fiscal distress on the number of tickets issued. The dependent variable in the first two columns is the number of tickets, and the dependent variable in the next two columns is tickets per mile. We employ two different instrument specifications of our first stage. Column 1 includes only the override dummies and the override dollar amount variables. Column 2 includes both the original set of override variables and the additional interaction variables with out of town drivers stopped. Columns 3 and 4 reflect columns 1 and 2, but with tickets and the appropriate control variables measured per mile. All regressions have month and town fixed effects, and we cluster standard errors at the town level.

The coefficients on the override related variables in Table 2, column 1 largely support the primary hypothesis behind our identification strategy. The number of tickets written is increasing with a failed referendum and decreasing with a passed referendum. Further, the number of tickets is increasing with the dollar amounts of both passed and failed referendums. The point estimate on the passage of an override referendum shows that passage leads to a drop in the number of tickets written by 14.6 tickets, but that with

[26] Tables 2 through 4 indentify when the dependent variable is measured per mile which independent variables mirror this scaling. Assessed property values are measured per capita (as opposed to per local mile) in all specifications due to the direct connection to personal tax burdens.

each \$100,000 increase in the amount asked for in the referendum, another 1.8 tickets are written. This implies that a passed override referendum leads to fewer tickets when the amount asked for was below \$555,000 and that the number of tickets issued increases for higher amounts.[27] In our data sample, 38 percent of observations with passed referendum were for requests in excess of \$555,000, and thus resulted in additional tickets being issued.

While the positive coefficient on OverrideFail_{it}, $\$\text{OverrideFail}_{it}$, and $\$\text{OverridePass}_{it}$ 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 $\$\text{OverridePass}_{it}$, however, is consistent with our hypothesis. The results in column 1 suggest that passage of overrides requesting large dollar amounts, indicating significant fiscal distress, lead to more tickets issued. Smaller passed overrides, however, alleviate the fiscal pressure to pursue alternative revenues, and in turn correspond to fewer tickets.

Table 2, column 2 reports the coefficients from the alternative instrument specification and includes the Outtown_{it} $\mathbf{* \ Override_{it}}$ interaction variables. Here we find that many of the override variables fail to be statistically significant, with three exceptions. The $\text{Outtown}_{it} * \text{OverrideFail}_{it}$, and $\text{Outtown}_{it} * \$\text{OverridePass}_{it}$ coefficients are statistically significant and positive with substantial magnitudes. Perhaps surprisingly, the OverrideFail_{it} coefficient is negative, with a point estimate of -10.8. One possible interpretation is that this is indicative of an increase in tickets to out of town drivers coupled with a decrease in tickets to locals. Columns 3 and 4 reflect the specifications

[27] 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).

from columns 1 and 2, but measured by miles of local road, with similar results, though \$OverrideFail$_{it}$ is not statistically significant in column 3.

Table 2, columns 1 and 2 show that the Kleibergen-Paap F statistics are 11.27 and 11.89 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.[28] For example, a ten percent bias is associated with a Kleibergen-Paap F statistic of 10 (five percent with an F statistic of 16.85) (see Stock and Yogo 2005). The excluded instruments using per mile measured variables (columns 3 and 4), however, have Kleinbergen-Paap F-statistics of 7.46 and 5.04, suggesting that the per mile measurement specifications are based on weaker instruments.

The Anderson-Rubin Wald test offers a more robust test of the potential weakness of instruments, especially for models, such as ours, which use several excluded variables to identify a single endogenous variable (Stock et al. 2002). The Anderson-Rubin null hypothesis that the excluded variables coefficients equal zero can be rejected at the 2 and 7 percent level in the primary IV specifications and at the 9 and 11 percent level in the per mile measured IV. First stage results using state trooper issued tickets as a "placebo instrument failed to identify increases in state trooper tickets. The first stage specification using **Override$_{it}$** and the alternative specification using Outtown$_{it}$*** Override$_{it}$** resulted in Kleibergen-Paap F statistics of 2.0 and 0.25. This lends further credence to our hypothesis that tickets specifically issued by officers employed by local government are connected to local fiscal distress and the efforts to raise revenues.

[28] See Stock and Yogo 2001 for a discussion of this issue http://ksghome.harvard.edu/~jstock/pdf/rfa_6.pdf.

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 5 percent level (Table 3, column 1).[29] These findings are likely due to the omitted variable bias indicated previously: in dangerous towns, more tickets are issued 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 4.1 fewer car crashes. In our data set the mean number of accidents and tickets are 37 and 83 respectively, with standard deviations of 60 for accidents and 132 for tickets. Thus, the 0.041 point estimate implies that a one standard deviation increase in tickets leads to 5 fewer accidents, 8 percent of the standard deviation in accidents.[30]

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 1. The bottom panel of the table includes results for overidentifying restrictions

[29] Without month effects the point estimate is positive and statistically significant as well.
[30] As noted earlier, in just under 12% of our observations have $OverridePass_{it}$ or $Override\ Fail_{it}$ equal one. Only 24% subset of municipalities have either $OverridePass_{it}$ or $Override\ Fail_{it}$ equal one at any point within the sampled time frame. The test for the dependence of our results on sample selection, we ran the Table 3, columns 1 and 2 specifications on the subsample of municipalities that experienced an override pass or failure at any time period in the full sample. The resulting coefficient on tickets went to zero without fixed effects (col. 1), and remained almost identical with fixed effects (col. 2). The negative effect of tickets on accidents does not appear to be a subsample phenomenon.

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 triples, and suggests that 100 extra tickets lead to 12.3 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 26 percent of the standard deviation of accidents. We employ our alternative instrument specification, that is the Outtown_{it}*** $\textbf{Override}_{\textbf{it}}$** interaction variables, in our IV analysis in Table 3, column 4. The results are very similar to those found in column 3. The coefficient on tickets is negative and statistically significant at the 1 percent level. The magnitude is greater, suggesting that 100 extra tickets would lead to 16.1 fewer accidents.

Table 3, columns 5 through 8 present model the same specifications as in columns 1 through 4, but with variables measured per mile of local road when appropriate. The coefficients on tickets per mile 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 triples in magnitude when instrumental variables are added (columns 7 and 8). The results in columns 7 suggest that for every 100 tickets written per mile, there are 13.3 fewer accidents per mile. This estimate implies that a one standard deviation increase in tickets per mile reduces accidents per mile by 0.176, or 43 percent of a standard deviation in accidents per mile.

Unlike the model that does not measure accidents per mile, the 2SLS specification with Outtown_{it}*** $\textbf{Override}_{\textbf{it}}$** interaction variables (column 8) shows a slightly lower magnitude, with 100 extra tickets correlating to 8.6 fewer accidents, but with a similar standard error. The coefficient on tickets per local mile in column 8 is only significant at the 15 percent level.

Tables 4 and 5 include the analysis of injuries and fatalities. Injuries and fatalities are measure of the severity of accidents. Similar to the results with accidents, the OLS estimate without municipality fixed effects shows a positive coefficient on tickets with injuries(Table 4, column 1), but the sign reverses and becomes negative and statistically significant when adding municipality fixed effects (Table 4, column 2). The two IV estimates show a negative effect of tickets on the number of injuries that is statistically significant at the 15 percent (Table 4, columns 3) and 5 percent level (column 4). 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. The IV specification using the Outtown_{it} interaction variables as the excluded instrumental variables (column 4) indicates that 100 additional tickets lead to 6 fewer injuries associated with traffic accidents. This implies that a one standard deviation in tickets reduces injuries by 27 percent of a standard deviation in injuries. Columns 5 and 6 in Table 4 represent the per mile measured specifications for injuries. Neither specification results in statistically significant coefficients on tickets per mile.

In Table 5, columns 1 and 2, tickets are not statistically significant in either specification for fatalities. Because only a relatively small fraction of the sample has zeroes for accidents and injuries, we estimated the accident and injury models using OLS.

Because the fraction of observations with zero fatalities (90 percent) is large we also estimated the regression using a Poisson model. Poisson estimation with month and municipal fixed effects (column 3) yields a negative coefficient on tickets with a relatively large standard error that is not statistically significant. The IV estimates for fatalities are sensitive to the instrument specification. Both IV estimates of the tickets coefficient are negative (columns 4 and 5), but only the result that is based on the first stage where we use the Outtown_{it} interaction variables as the excluded instrument is statistically significant. Columns 6 and 7 in Table 5 represent the per mile measured specifications for fatalities. Neither specification results in statistically significant coefficients on tickets per mile.

The coefficients on unemployment filings in our IV analysis of automobile accidents are positive with accidents and negative in accidents per mile, but are not statistically significant with either. Conversely, the coefficient on unemployment is negative and statistically significant with both injuries and injuries per mile, in strong concurrence with the findings of Ruhm (2000).

The model results show that tickets are an effective means for reducing accidents and injuries, while the effect of ticket issuance on fatalities is inconclusive. 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 greater element of 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.

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 its 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.

We have not discussed specific mechanisms for tickets to reduce accidents in this paper beyond the basic rational choice story that traffic fines increase the price of unsafe driving, leading to less consumption of unsafe driving and fewer accidents. While there is no doubt a variety of stories that can be told to add nuance to this standard economic logic, some relate more directly to our data and their specific social setting. An increase in ticket writing behavior during a particularly tight fiscal year might quickly give a town's officers a reputation for strictness. First or second hand stories of speed traps or towns "looking to raise money" can spread quickly. When local news affiliates in Massachusetts air stories of the upcoming referendum vote in a neighboring town, drivers may take note to drive more conservatively within its jurisdiction. Before reputations are made, however, additional tickets may improve traffic safety by changing the subjective probability of future tickets, and thus the behavior, of their recipients. While tickets are given to a very small proportion of drivers, they are more likely to be given to the most reckless drivers. Changing their behavior would have a disproportionate effect on traffic safety.

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Figure 1

Average number of crashes and tickets across municipalities by month

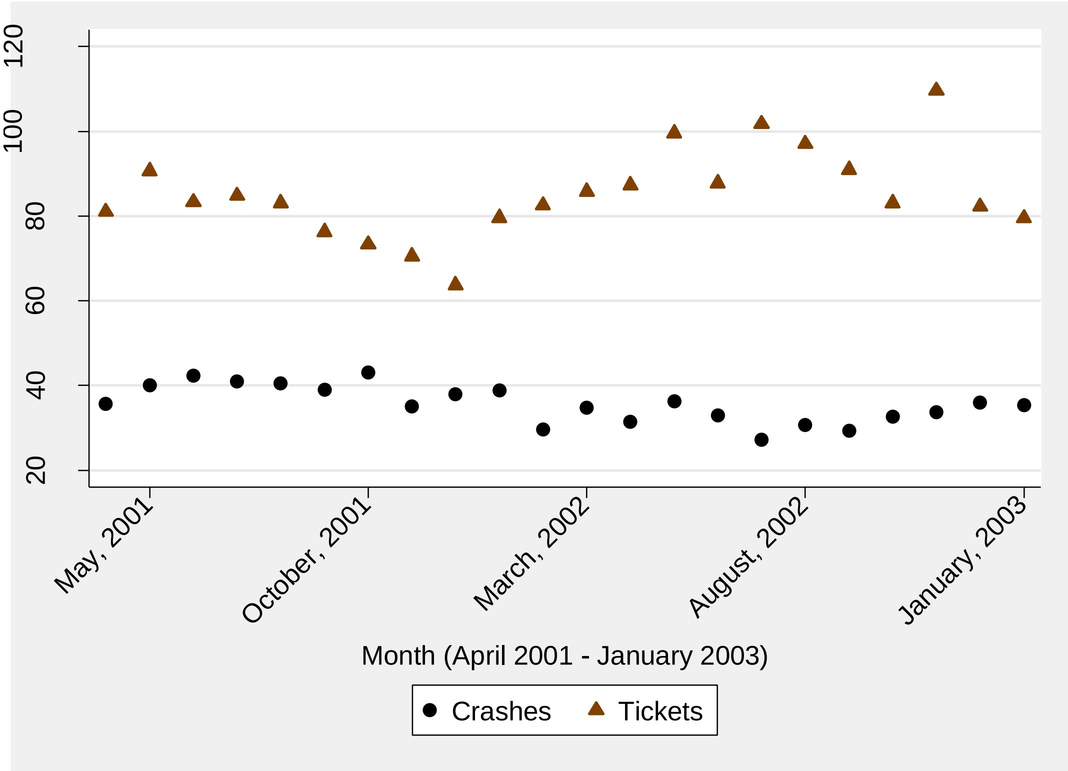

Table 1
Summary Statistics

Variable	Mean	Std. Dev.	Min	Max
Accidents	36.89	60.25	0	674
Tickets	82.63	129.01	0	1556
Injuries	15.82	27.33	0	315
Fatalities	0.12	0.39	0	6
Out of Town Drivers Stopped	111.48	135.76	0	1678
Other Public Safety expenditures ($1000)	424.09	833.30	1.21	11628.06
Total registered vehicles (1000)	15.91	16.37	0.44	125.59
Average Mph over the speed limit	17.21	2.83	7.50	50.00
Number of minority drivers stopped (100)	0.32	0.86	0	12.71
Drivers stopped at night (100)	0.72	1.03	0	12.32
Average age of stopped drivers	32.28	4.06	0	69.50
Number of female stopped drivers (100)	0.48	0.75	0	7.95
Chapter 90 Highway funding ($1000)	288.81	253.63	18.78	2171.61
Unemployment Filings (100)	4.92	7.18	0.03	55.09
Property Value ($10,000) per capita	11.40	15.58	1.93	281.89
Police expenditures ($1000)	2952.37	4308.56	0.46	41275.40
Population (1000)	17.99	22.05	0.35	175.71
Total mileage of local roads	74.89	50.25	3.50	388.38
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)
	Tickets		Tickets per Mile	
	OLS	OLS	OLS	OLS
Override Pass	-14.614***	-6.001	-0.078*	0.034
	(3.639)	(5.703)	(0.047)	(0.068)
Override Fail	0.937	-10.800*	0.084	-0.134
	(4.710)	(5.967)	(0.073)	(0.085)
Override Pass Dollars	1.803***	0.179	0.008***	-0.007
	(0.303)	(0.217)	(0.002)	(0.006)
Override Fail Dollars	0.068*	0.038	-0.001	0.007
	(0.039)	(0.073)	(0.001)	(0.005)
Out of Town Drivers Stopped * Override Pass		-0.005		-0.061
		(0.070)		(0.041)
Out of Town Drivers Stopped * Override Fail		0.125***		0.163**
		(0.047)		(0.082)
Out of Town Drivers Stopped * Override Pass Dollars		0.004***		0.009**
		(0.001)		(0.004)
Out of Town Drivers Stopped * Override Fail Dollars		0.001		-0.013
		(0.002)		(0.010)
Out of Town Drivers Stopped†	0.316***	0.307***	0.494***	0.490***
	(0.092)	(0.093)	(0.043)	(0.043)
Other Public Safety expenditures†	0.020*	0.018	-0.007	-0.007
	(0.011)	(0.011)	(0.013)	(0.013)
Registered vehicles†	-0.277	-0.268	-2.715	-2.716
	(1.338)	(1.328)	(1.998)	(2.023)
Avg. Mph over the speed limit	-0.458*	-0.433*	-0.007***	-0.007***
	(0.264)	(0.252)	(0.002)	(0.002)
Minority drivers stopped†	41.509***	42.003***	17.780	17.803
	(11.808)	(11.915)	(13.004)	(13.055)
Drivers stopped at night†	-18.309	-18.192	-12.097***	-12.066***
	(12.797)	(12.880)	(3.229)	(3.292)
Average age of stopped drivers	-0.053	-0.061	-0.001	-0.001
	(0.123)	(0.121)	(0.001)	(0.001)
Number of female stopped drivers†	45.729***	45.779***	21.921**	21.951**
	(16.366)	(16.311)	(9.417)	(9.263)
Ch. 90 Highway funding†	-1.659***	-1.622***	-0.191	-0.188
	(0.463)	(0.459)	(0.333)	(0.318)
Unemployment claims†	10.631***	10.712***	4.361**	3.960**
	(2.828)	(2.824)	(1.842)	(1.772)
Property Value per capita	0.402	0.469	0.006	0.006*
	(0.336)	(0.362)	(0.004)	(0.004)
Police expenditures†	-0.003	-0.003	0.017**	0.017**
	(0.009)	(0.009)	(0.007)	(0.007)
Population†	-26.197*	-28.449*	-18.638*	-18.798*
	(14.742)	(15.179)	(10.935)	(10.774)
Constant	910.284***	941.167***	4.185	4.223*
	(323.426)	(329.248)	(2.540)	(2.487)
Town and month fixed effect?	Yes	Yes	Yes	Yes
Kleibergen-Paap F-Stat	11.27	11.89	7.46	5.04
R-squared	0.94	0.94	0.93	0.93

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. All variables denoted with a †are measured per mile of local road in columns 3 and 4.

Table 3
Effects of Traffic Enforcement on Accidents

	(1)	(2)	(3)	(4)	(5)	(6)	(7	(8)
	Crashes				Crashes per mile of local road			
	OLS	OLS	IV	IV	OLS	OLS	IV	IV
Tickets†	0.071**	-0.041***	-0.123***	-0.161***	0.009	-0.042***	-0.133**	-0.086
	(0.036)	(0.015)	(0.031)	(0.057)	(0.027)	(0.010)	(0.053)	(0.058)
Out of town drivers	0.050	0.026	0.052**	0.064**	0.001	0.072***	0.117***	0.094***
stopped†	(0.057)	(0.016)	(0.024)	(0.027)	(0.032)	(0.025)	(0.035)	(0.035)
Other public safety	0.003	0.011**	0.012**	0.013***	-0.001	0.003	0.003	0.003
expenditures†	(0.003)	(0.005)	(0.005)	(0.005)	(0.003)	(0.006)	(0.006)	(0.006)
Registered vehicles†	-2.033***	0.145	0.113	0.102	-0.367	-0.315	-0.567	-0.439
	(0.749)	(1.193)	(1.181)	(1.198)	(0.519)	(0.570)	(0.610)	(0.627)
Avg. Mph over the	0.049	-0.036	-0.068	-0.085	-0.003	-0.001	-0.002*	-0.002
speed limit	(0.291)	(0.084)	(0.085)	(0.089)	(0.003)	(0.001)	(0.001)	(0.001)
Minority drivers	-32.665***	-2.466	0.923	2.509	3.991	0.187	1.816	0.970
stopped†	(9.102)	(2.236)	(2.734)	(3.200)	(4.610)	(1.493)	(2.113)	(1.603)
Drivers stopped at	5.157	4.030*	2.464	1.779	-1.939	1.061	-0.058	0.512
night†	(4.257)	(2.145)	(2.883)	(3.091)	(2.248)	(2.339)	(2.299)	(2.400)
Average age of stopped	0.190	0.023	0.019	0.017	0.001	0.000	0.000	0.000
drivers	(0.136)	(0.029)	(0.031)	(0.033)	(0.001)	(0.000)	(0.000)	(0.000)
Number of female	0.959	-0.824	3.017	4.755	-0.782	-9.513**	-7.484**	-8.516**
stopped drivers†	(7.440)	(2.181)	(2.636)	(4.141)	(4.580)	(4.067)	(3.551)	(4.178)
Ch. 90 Highway	0.043	0.401**	0.247	0.183	0.092***	0.136	0.119*	0.129*
funding†	(0.029)	(0.157)	(0.183)	(0.190)	(0.024)	(0.087)	(0.066)	(0.074)
Unemployment claims†	4.628***	0.622	1.412	1.822	0.754	-1.240*	-0.862	-1.064
	(0.832)	(1.017)	(1.084)	(1.398)	(0.728)	(0.739)	(0.724)	(0.830)
Property Value per	0.078**	-0.017	0.018	0.026	-0.002**	-0.000	0.000	0.000
capita	(0.039)	(0.086)	(0.071)	(0.073)	(0.001)	(0.002)	(0.002)	(0.002)
Police expenditures†	-0.001	0.001	0.000	0.000	0.001	-0.000	0.001	0.000
	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Population†	2.292***	9.694	7.932	6.903	1.295***	19.048***	17.337***	18.238***
	(0.843)	(10.372)	(9.989)	(9.885)	(0.460)	(6.298)	(6.151)	(6.095)
Override Pass				-0.048				0.010
				(1.166)				(0.012)
Override Fail				1.228				-0.011
				(1.696)				(0.031)
Override Pass Dollars				0.064				-0.000
				(0.134)				(0.001)
Override Fail Dollars				-0.007				-0.000
				(0.017)				(0.000)
Constant	-14.387*	-268.715			-0.164*	-3.994***		
	(8.662)	(196.306)			(0.098)	(1.311)		
Kleibergen-Paap F-Stat			11.27	11.89			7.46	5.04
Overidentifying Restrictions (p value)			0.88	0.67			0.76	0.48
Anderson-Rubin (p value)			0.02	0.07			0.09	0.11
Month fixed effect?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Town fixed effect?	No	Yes	Yes	Yes	No	Yes	Yes	Yes
R-squared	0.81	0.95			0.64	0.87		

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 are, respectively, columns 1,2,3, and 4 from Table 2. All variables denoted with a †are measured per mile of local road in columns 5 through 8.

Table 4
Effect of Traffic Enforcement on Injuries

	(1) Injuries	(2) Injuries	(3) Injuries	(4) Injuries	(5) Injuries per mile	(6) Injuries per mile
	OLS	OLS	IV	IV	IV	IV
Tickets†	0.013	-0.017**	-0.033	-0.061**	-0.012	0.027
	(0.022)	(0.008)	(0.022)	(0.031)	(0.033)	(0.032)
Out of town drivers stopped†	0.032	0.016	0.021	0.030*	0.021	0.002
	(0.032)	(0.010)	(0.013)	(0.016)	(0.019)	(0.015)
Other public safety exp†	-0.001	0.005**	0.005**	0.006***	0.003	0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Registered vehicles†	-0.988**	0.030	0.023	0.015	0.195	0.298
	(0.399)	(0.499)	(0.483)	(0.485)	(0.376)	(0.428)
Avg. Mph over the speed limit	0.101	0.015	0.009	-0.004	-0.001	-0.000
	(0.137)	(0.047)	(0.047)	(0.050)	(0.001)	(0.001)
Minority drivers stopped†	-5.650	-3.599*	-2.915	-1.759	2.025	1.349
	(4.780)	(1.939)	(1.986)	(2.200)	(3.531)	(3.848)
Drivers stopped at night†	2.287	2.532**	2.216	1.712	2.574***	3.044***
	(2.265)	(1.162)	(1.363)	(1.612)	(0.986)	(1.153)
Average age of stopped drivers	0.092	0.006	0.005	0.004	0.001*	0.001**
	(0.060)	(0.016)	(0.016)	(0.017)	(0.000)	(0.000)
Number of female stopped drivers†	-3.944	-0.836	-0.062	1.202	-5.600**	-6.453**
	(2.969)	(1.593)	(1.869)	(2.239)	(2.464)	(2.969)
Ch. 90 Highway funding†	0.013	0.307***	0.276***	0.230**	0.093*	0.099
	(0.014)	(0.083)	(0.096)	(0.097)	(0.052)	(0.061)
Unemployment claims†	2.299***	-0.842*	-0.683	-0.389	-1.069**	-1.245**
	(0.532)	(0.461)	(0.489)	(0.611)	(0.455)	(0.524)
Property Value per capita	0.034	0.018	0.025	0.036	0.001	0.001
	(0.024)	(0.051)	(0.048)	(0.046)	(0.001)	(0.001)
Police expenditures†	-0.002	0.000	0.000	0.000	-0.002	-0.002**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Population†	1.309***	2.006	1.651	0.907	4.894	5.572
	(0.495)	(4.105)	(4.021)	(3.968)	(3.435)	(3.504)
Override Pass				-0.349		0.000
				(0.546)		(0.008)
Override Fail				0.257		0.004
				(1.010)		(0.011)
Override Pass Dollars				0.050		-0.000
				(0.052)		(0.000)
Override Fail Dollars				-0.015**		-0.000**
				(0.008)		(0.000)
Constant	-4.720	-111.545			-0.012	0.027
	(3.625)	(79.453)			(0.033)	(0.032)
Kleibergen-Paap F-Stat			11.27	11.89	7.46	5.04
Overidentifying Restrictions (p value)			0.88	0.67	0.76	0.48
Anderson-Rubin (p value)			0.02	0.07	0.09	0.11
Month fixed effect?	Yes	Yes		Yes		Yes
Town fixed effect?	No	Yes		Yes		Yes
R-squared	0.73	0.91				

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, 5, and 6 are, respectively, columns 1,2,3, and 4 from Table 2. All variables denoted with a †are measured per mile of local road in columns 5 and 6.

Table 5
Effect of Traffic Enforcement on Fatalities

	(1) Fatalities	(2) Fatalities	(3) Fatalities	(4) Fatalities	(5) Fatalities	(6) Fatalities per mile	(7) Fatalities per mile
	OLS	OLS	Poisson	IV	IV	IV	IV
Tickets†	-0.000	-0.000	-0.001	-0.001	-0.002**	0.001	0.001
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Out of town drivers stopped†	-0.000	0.000*	0.002	0.001*	0.001**	-0.000	-0.000
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
Other public safety exp†	-0.000	0.000***	0.000	0.000***	0.000***	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Registered vehicles†	0.007**	-0.028*	-0.063	-0.028*	-0.029*	-0.018	-0.016
	(0.003)	(0.016)	(0.049)	(0.016)	(0.016)	(0.018)	(0.018)
Avg. Mph over the speed limit	0.001	-0.003*	-0.036	-0.003*	-0.004**	-0.000**	-0.000**
	(0.002)	(0.002)	(0.027)	(0.002)	(0.002)	(0.000)	(0.000)
Minority drivers stopped†	0.027	0.036	0.138	0.056	0.114*	-0.006	-0.017
	(0.019)	(0.032)	(0.170)	(0.047)	(0.062)	(0.039)	(0.036)
Drivers stopped at night†	-0.010	-0.047	-0.206*	-0.056	-0.083*	0.016	0.023
	(0.023)	(0.029)	(0.105)	(0.036)	(0.049)	(0.029)	(0.031)
Average age of stopped drivers	-0.001	-0.001	-0.029	-0.001	-0.001	-0.000	-0.000
	(0.001)	(0.001)	(0.018)	(0.001)	(0.001)	(0.000)	(0.000)
Number of female stopped drivers†	0.031	-0.009	-0.034	0.014	0.079	-0.116	-0.128
	(0.046)	(0.055)	(0.208)	(0.068)	(0.087)	(0.086)	(0.092)
Ch. 90 Highway funding†	0.000***	0.001	0.002	0.000	-0.002	0.000	0.000
	(0.000)	(0.001)	(0.007)	(0.002)	(0.003)	(0.001)	(0.001)
Unemployment claims†	0.007*	0.005	0.019	0.010	0.025*	0.001	-0.001
	(0.004)	(0.008)	(0.025)	(0.010)	(0.014)	(0.010)	(0.010)
Property Value per capita	-0.001	-0.001	-0.073	-0.001	-0.001	-0.000	-0.000
	(0.000)	(0.002)	(0.049)	(0.002)	(0.001)	(0.000)	(0.000)
Police expenditures†	0.000	0.000*	0.000	0.000**	0.000***	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Population†	-0.008**	-0.003	0.044	-0.014	-0.049	0.044	0.056
	(0.003)	(0.058)	(0.291)	(0.058)	(0.065)	(0.058)	(0.056)
Override Pass					-0.023		0.000
					(0.021)		(0.000)
Override Fail					-0.010		0.000
					(0.029)		(0.001)
Override Pass Dollars					0.002		-0.000
					(0.002)		(0.000)
Override Fail Dollars					0.002***		0.000***
					(0.000)		(0.000)
Constant	0.041	0.118				0.001	0.001
	(0.055)	(1.197)				(0.002)	(0.001)
Kleibergen-Paap F-Stat				11.27	11.89	7.46	5.04
Overidentifying Restrictions (p value)				0.88	0.67	0.76	0.48
Anderson-Rubin (p value)				0.02	0.07	0.09	0.11
Month fixed effect?	Yes	Yes	Yes		Yes		Yes
Town fixed effect?	No	Yes	Yes		Yes		Yes
R-squared	0.11	0.18					
N (municipalities coded)	7,038	7,038	4759	7,038	7,038	7,038 (338)	7,038 (338)
		(338)	(220)	(338)	(338)		

The first stage for columns 4, 5, 6, and 7 are, respectively, columns 1,2,3, and 4 from Table 2. All variables denoted with a †are measured per mile of local road in columns 5 and 6.

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