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

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Traffic accidents are one of the leading causes of injury and death in the United States. 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 the number of 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 numbers of tickets written reduce the number of motor vehicle accidents and accident-related injuries. The findings show that failure to control for endogeneity results in a significant underestimation of the positive effect of law enforcement on traffic safety.

1. 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 ninth 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 15 chance of being involved in a traffic accident during a given year (National Highway Traffic Safety Administration 2001). A wide range of social scientists, including economists, have long studied the efficacy of policies, such as speed limits and mandatory seat belt use, that are intended to improve

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traffic safety.[1] The effect 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, and Evans 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.[2] This model predicts that ticketing leads to fewer motor vehicle accidents.[3] However, to date there is little evidence on whether and by how much enforcement reduces the number of accidents. Only a handful of studies address this issue. Using data from Canada, Redelmeier, Tibshirani, and Evans (2003) found a negative short-term effect of traffic citation on the likelihood of being involved in an accident, which vanished after 3 months. These authors' simultaneous determination of accidents and citations may have led them to underestimate 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 the number of accidents, it 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 squares (OLS) 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.[4]

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-month period between 2001 and 2003. We control for omitted variables with month and municipality fixed effects and

[1] These policies include mandatory seat belt use (Loeb 1995), air bags (Kneuper and Yandle 1994; Levitt and Porter 2001), the speed limit (Forester, McNown, and Singell 1984), motorcycle helmet laws (Jones and Bayer 2007), the drinking age (Asch and Levy 1990), and vehicle safety inspections (Merrell, Poitras, and Sutter 1999).

[2] 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).

[3] In a survey conducted by Williams, Paek, and Lund (1995) that asked drivers how different factors motivated them to practice safe driving habits, 61 percent of respondents said concern that they may receive a traffic fine motivated them "a lot," ranking only behind potential accidents (82 percent) and potential increase in insurance premiums (63 percent).

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

address the concern of time-varying omitted variables with instrumental variable (IV) estimation. Our instrument is the financial health of a town, measured by whether a town asks voters to approve a referendum to override the limit on property taxes (hereafter, an override referendum). An override referendum allows towns to collect property tax revenues beyond Massachusetts's legal limits established by Proposition $2\frac{1}{2}$ in 1980. By putting an override referendum in front of voters, the town board indicates that the town is in fiscal distress and that it would like to raise additional revenue.

When towns are in 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 is 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 effect 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) finds a positive correlation between recessions and several positive health outcomes. We address potential endogeneity issues through 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 from 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 out-of-town drivers who are stopped in towns experiencing fiscal distress.[5] We find that in towns in fiscal distress, police officers issue more tickets to out-of-town drivers.[6] Since it seems reasonable to assume that the interaction of out-of-town drivers stopped and local fiscal distress does not directly affect the number of accidents, the interaction represents a suitable instrument.

[5] Massachusetts provides an invaluable setting for our study not just because of the Proposition $2\frac{1}{2}$ property tax legal institution but also because of its 351 municipalities dividing what is, at 10,000 square miles, the sixth smallest state in the United States into 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) where he or she is actually driving "in town." In our 2 years of traffic stops (including stops that result in tickets and those that do not), 63 percent of all drivers stopped are not local residents. A survey by Progressive Auto Insurance of 11,000 policyholders found that 77 percent of accidents happened more than 2 miles from their customer's home (Progressive.com 2002), which in Massachusetts would place the bulk of drivers involved in accidents outside of their home municipality.

[6] These results are consistent with other work showing that towns in financial decline use traffic tickets as a revenue-generation tool (Helland and Tabarrok 2002; Makowsky and Stratmann 2009).

We demonstrate that the OLS estimator reveals a positive correlation between tickets and crashes. Ordinary least squares regression including town and month fixed effects finds a negative correlation between numbers of tickets and crashes. Adding IV estimation, using the Proposition $2\frac{1}{2}$–related instruments, we document that issuing tickets reduces the number of car crashes and that the magnitude of this effect is nearly 3 times larger than in the OLS estimation. Further, we document that increased enforcement reduces the number of injuries associated with traffic accidents. Results for fatalities show a negative correlation with enforcement but are less conclusive.

2. Background on Institutions and Officers' Strictness

In 1980, Massachusetts voters passed referendum Proposition $2\frac{1}{2}$, 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 1 year to the next. If a town government wishes to raise funds from property taxes beyond the levy limit prescribed by Proposition $2\frac{1}{2}$, 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 any time 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 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 2010). A passed referendum will increase the property tax revenue raised in the subsequent fiscal year, which begins July 1 and runs through June 31 of the following calendar year.

Evidence suggests that while limits on personal property taxation have curtailed spending (Cutler, Elmendorf, and Zechhauser 1999; Bradbury, Mayer, and Case 2001), they have also made Massachusetts local governments more dependent on other local sources of revenue.[7] Galles and Sexton (1998), for example, suggest that increases in nontax revenue may have returned spending to pre–Proposition $2\frac{1}{2}$ levels. Non-property-tax revenues include receipts from the motor vehicle excise, charges for services, departmental revenue (for example, libraries), licenses and permits, and fines. Traffic citations fall under the category of fines.[8]

[7] "Since the passage of Proposition $2\frac{1}{2}$ 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 $2\frac{1}{2}$, 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" (Massachusetts Department of Revenue 2006).

[8] Massachusetts Department of Revenue, Cherry Sheet Manual (http://www.mass.gov/dor/docs/dls/cherry/cherrysheetmanual.pdf).

There are limitations, however, placed on revenue generated from fees, licenses, and permits. Municipalities are allowed to recover only 100 percent of the cost of providing fee-based services.[9] 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.[10] 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 $2\frac{1}{2}$ override referendum to raise additional revenue. The referendum is subject to a majority-rule vote open to all local voters. The failure of an override referendum reduces the amount of 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.[11,12] Makowsky and Stratmann (2009) show in a cross-sectional analysis that failure to pass an override referendum increases the strictness of officers.[13] 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.

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

[10] "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" (Mass. Gen. Laws ch. 280, sec. 2).

[11] Officers' use of discretion under Mass. Gen. Law ch. 90C, sec. 3, was recently challenged by the Newton (Mass.) Police Association. The Massachusetts State Court of Appeals ruled against the appeal, protecting the capacity of officers to issue warnings (*Newton Police Ass'n vs. Police Chief of Newton,* 63 Mass. App. Ct. 697 [June 9, 2005]).

[12] During the time period studied in this paper (2001–3), the Massachusetts police did not keep explicit records of warnings. We exclude records of stops in which a fine of $0 was recorded as warnings, counting only stops in which the driver was issued a fine as a ticket.

[13] Makowsky and Stratmann (2009) show this in a cross-sectional analysis of a 2-month period across 350 municipalities. These findings are consistent with the results of Garrett and Wagner (2009), who find that officers in North Carolina issue more tickets in the year after a decline in county revenue.

3. 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 driver who was stopped received a ticket or a warning,[14] the driver's place of residence, gender, whether the stop was at night, and the type of infraction, including miles per hour over the speed limit when it was a speeding-related offense.[15]

The Massachusetts Highway Department and Highway Safety Division collects accident data in its Crash Data System (CDS). The 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 and traffic stop data consist of reports from each 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 no) injuries or fatalities. For each municipality, we aggregated to the month the numbers of accidents and 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, such as unemployment filings, are based on the annual year.[16]

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 no accidents during a month in a few towns to 674 in Worcester in 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, and Fatalities equals zero for 90 percent. On average, police officers issued 82 tickets per

[14] Warnings are not explicitly labeled in the data set, and we categorize all observations with a fine of $0 as warnings. These observations account for 46 percent of observations, which is similar to the 48 percent warning rate observed in a subset of the data wherein warnings are explicitly labeled.

[15] Traffic stop data were collected by the Massachusetts state legislature and provided to us by Bill Dedman of the Boston Globe and MSNBC.com.

[16] We could have collapsed 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 for the dependent variables, and thus the month unit makes ordinary least squares regression a defensible estimation method.

Table 1
Summary Statistics

Variable	Mean	SD	Min	Max
Accidents	36.93	60.29	0	674
Tickets	82.68	129.01	0	1,556
Injuries	15.83	27.35	0	315
Fatalities	.12	.39	0	6
Out-of-town drivers stopped	111.52	135.86	0	1,678
Other public safety expenditures ($1,000s)	424.53	833.94	1.21	11,628.06
Registered vehicles (1,000s)	15.92	16.38	.44	125.59
Average miles per hour over the speed limit	17.21	2.83	7.50	50.00
Minority drivers stopped (100s)	.32	.86	0	12.71
Drivers stopped at night (100s)	.72	1.03	0	12.32
Female drivers stopped (100s)	.48	.75	0	7.95
Chapter 90 highway funding ($1,000s)	288.91	253.83	18.78	2,171.61
Unemployment filings (100s)	4.92	7.18	.03	55.09
Property value ($10,000s) per capita	11.40	15.59	1.93	281.89
Police expenditures ($1,000s)	2,739.05	4,210.57	.99	41,275.40
Population (1,000s)	18.01	22.06	.35	175.71
Mileage of local roads	74.90	50.29	3.50	388.38
Override pass	.09	.29	0	1
Override failure	.03	.16	0	1
Failed referenda amount ($100,000s)	.32	5.70	0	176.70
Passed referenda amount ($100,000s)	.96	5.80	0	117.62

Note. Monetary values are in 2003 Consumer Price Index–adjusted dollars. $N = 7{,}026$.

municipality, per month, with zero tickets recorded in less than 1 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 337 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 in which all referenda failed asked for an average of $1.3 million, while towns in which 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 (nonhighway) road.[17] Parking tickets, for example, are not included in this data set. Table A1 shows the types of violations that resulted in a ticket. The most commonly issued tickets are for speeding, composing 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, from April 2001 to January 2003. The figure shows no strong pattern suggesting that issuing

[17] We focus on local officers because of our instrumental variable strategy: local officers have an incentive to react to a budgetary shortfall in 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.

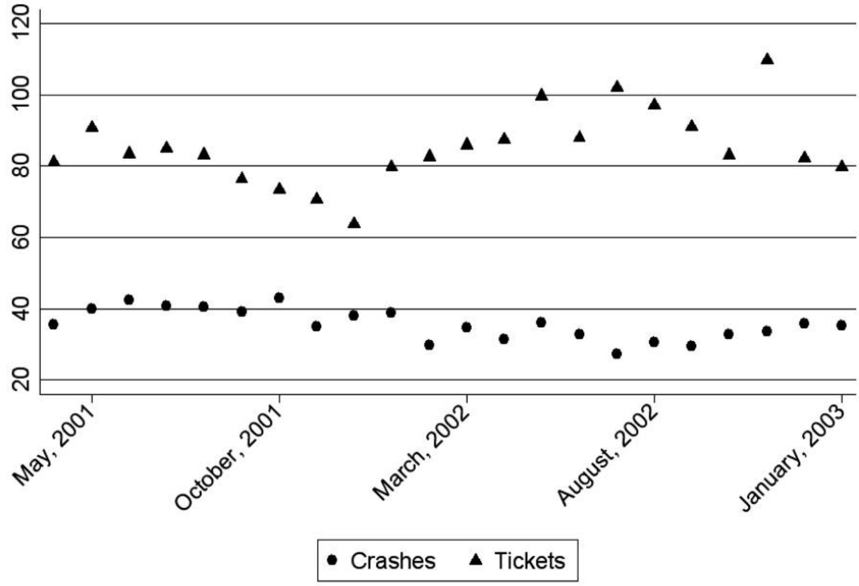

Figure 1. Average number of crashes and tickets across municipalities by month

traffic tickets reduces 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

$$\text{Accidents}_{it} = \beta_0 + \beta_1\text{Tickets}_{it} + \beta_2\text{StoppedDrivers}_{it} + \beta_3\text{Municipality}X_{it} + \text{Municipality}_i + \text{Month}_t + \varepsilon_{it}. \tag{1}$$

The accidents and tickets variables 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 $\text{StoppedDrivers}_{it}$ includes the number of stopped drivers from out of town and their characteristics, that is, the number of stopped drivers who are minority drivers and female drivers, 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.[18] $\text{Municipality}X_{it}$ is a

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

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 per capita, which vary by calendar year.[19] Because our unit of observation is the month, we attribute the data that come by fiscal year to the months associated with 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$_t$).[20] 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 3 calendar years. A fiscal year in Massachusetts runs from July 1 to June 30. For example, the fiscal year 2002 runs from July 1, 2001, until June 30, 2002. We have 3 months of data from fiscal year 2001 (April 1, 2001, to June 31, 2001), 12 months from fiscal year 2002 (July 1, 2001, to June 31, 2002), and 6 months from fiscal year 2003 (July 1, 2002, to January 31, 2003). With OLS regression, Tickets is likely to be correlated with the error term, ε_{it}, resulting in biased estimates. The reason for the endogeneity is omitted variable bias: in towns in which drivers drive recklessly, many tickets are issued and many crashes occur. Thus, OLS regressions will underestimate the true effect of issuing tickets on the number of accidents. The inclusion of municipal fixed effects alleviates some of the omitted variable problem because it accounts for town-specific factors that simultaneously affect the numbers of 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 an instrument that indicates whether a town is in fiscal distress. Traffic tickets are one source of revenue, 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. 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. While referenda are held at almost all times of the year, they always apply to the following fiscal year.

[19] Municipal data, including records of override referendum votes and their outcomes, are from Massachusetts Department of Revenue (2010).

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

Our first-stage regression is

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

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 variable for whether an override referendum passed (OverridePass_{it}) during the fiscal year, whether the referendum failed (OverrideFail_{it}), and separate measures for the total dollar amounts requested when an override referendum failed ($\$\text{OverrideFail}_{it}$) and when it passed ($\$\text{OverridePass}_{it}$).[21] We code Override to equal one for each of the 12 months of the fiscal year to which the referendum applies and zero otherwise. We include OverridePass_{it} in addition to OverrideFail_{it} 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 government 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 an 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 OverrideFail 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 OverridePass 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 than 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 referendum passes or fails, requests for larger dollar amounts indicate greater fiscal distress, which suggests that the estimated coefficient on $OverridePass and $OverrideFail will be positive.

In the second-stage regression, the first stage controls for town-specific characteristics via municipal 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 the number of

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

traffic accidents via avenues other than tickets. Towns in which 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 regressions for spending on road maintenance and other public safety spending.[22] 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 equation (1) and thus that fiscal distress is correlated with drivers' 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 drivers' behavior, he does identify a negative relationship between the unemployment rate and the number of traffic-accident-related fatalities. Economic intuition suggests that unemployed individuals would drive less (absent a job to drive to) and might be more careful when they drive, being less able to afford the cost of a traffic accident. The correlation between budgetary shortfalls and unemployment is of course imperfect but is 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 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 ($\text{Outtown}_{it} \times \text{Override}_{it}$) as our instrument. The interaction of out-of-town drivers stopped and Override 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 drivers' behavior and will identify an increase in tickets. Therefore, our alternative specification for the first stage is

$$\begin{aligned}\text{Tickets}_{it} = {} & \beta_0 + \beta_1\text{Override}_{it} + \beta_2\text{Override}_{it} \times \text{Outtown}_{it} \\ & + \beta_3\text{StoppedDrivers}_{it} + \beta_4\text{Municipality}X_{it} \qquad (3) \\ & + \text{Municipality}_i + \text{Month}_t + \mu_{it},\end{aligned}$$

where we interact the Override vector with Outtown, the number of drivers who are stopped and whose license plates and drivers' licenses indicate that they are from out of town. Some of these drivers 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

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

increase 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 those conditions.

In our data set, the bulk of drivers on a given stretch of road in Massachusetts are from out of town, representing 66 percent of drivers who are stopped and 69 percent of drivers who are issued tickets.[23] There is also some direct evidence that out-of-town drivers are involved in the majority of car crashes. For a subperiod 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; that information is not available for the first 9 months of our data set.[24] Table A2 shows that between January 2002 and January 2003, the number of accidents involving an out-of-town driver is roughly proportional to the number of tickets issued to out-of-town drivers and that out-of-town drivers are involved in the vast majority (78 percent) of accidents. Thus, to increase strictness on out-of-town drivers is to increase 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 are defined as all roads within municipal boundaries excluding highways, which are the jurisdiction of state troopers. In these specifications, all appropriate independent variables, including tickets, are measured per local mile.[25] As an additional robustness check, we run the basic first-stage specifications with a placebo instrument. Numbers of tickets written by local officers are hypothesized to increase with local fiscal distress, while numbers of tickets written within municipal boundaries by state troopers should not.

4. Results

Table 2 shows the effect of our measures for fiscal distress on the number of tickets issued. 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 adds the interaction variables with out-of-town

[23] 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 percent 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 plates.

[24] According to the Massachusetts Department of Transportation's Highway Traffic Engineering Section, the computer system containing accident records used prior to 2002 did not contain residence information for drivers (Richard Conard, Highway Traffic Engineering Section, e-mail to the authors, October 9, 2008).

[25] Tables 2–4 identify when the dependent variable is measured per mile, the independent variables of which mirror this scaling. Assessed property values are measured per capita (as opposed to per local mile) in all specifications because of the direct connection to personal tax burdens.

Table 2

Override Referenda and Traffic Tickets: Ordinary Least Squares Estimates

	Tickets				Tickets per Mile			
	(1)		(2)		(3)		(4)	
OverridePass	−14.301**	(3.805)	−5.078	(5.685)	−.064	(.047)	.044	(.066)
OverrideFail	.601	(4.514)	−10.639+	(5.803)	.098	(.071)	−.119	(.081)
$OverridePass	1.830**	(.314)	.134	(.226)	.008**	(.001)	−.006	(.005)
$OverrideFail	.093**	(.036)	.066	(.068)	−.000	(.001)	.006	(.005)
Outtown[a] × OverridePass			−.006	(.070)			−.059	(.041)
Outtown[a] × OverrideFail			.121*	(.049)			.152*	(.075)
Outtown[a] × $OverridePass			.004**	(.001)			.008*	(.003)
Outtown[a] × $OverrideFail			.001	(.001)			−.010	(.009)
Outtown[a]	.316**	(.092)	.306**	(.092)	.487**	(.046)	.482**	(.046)
Other public safety expenditures[a]	.021**	(.007)	.020**	(.007)	−.005	(.013)	−.005	(.012)
Registered vehicles[a]	22.708**	(8.587)	23.169**	(8.640)	5.610	(4.569)	5.371	(4.414)
Average miles per hour over the speed limit	−.490+	(.256)	−.464+	(.243)	−.007**	(.002)	−.007**	(.002)
Minority drivers stopped[a]	38.608**	(11.213)	39.012**	(11.320)	18.949	(13.297)	19.040	(13.354)
Drivers stopped at night[a]	−15.913	(11.913)	−15.648	(11.959)	−11.563**	(3.566)	−11.612**	(3.633)
Female stopped drivers[a]	45.099**	(15.807)	45.095**	(15.757)	22.451*	(9.393)	22.643*	(9.380)
Chapter 90 highway funding[a]	−1.318**	(.414)	−1.273**	(.406)	−.181	(.331)	−.177	(.317)
Unemployment claims[a]	7.958*	(3.106)	7.968*	(3.119)	5.022**	(1.581)	4.659**	(1.515)
Property value per capita	.340	(.290)	.402	(.315)	.007+	(.004)	.008*	(.004)
Police expenditures[a]	.022	(.025)	.022	(.025)	.029*	(.012)	.029*	(.012)
Population[a]	−34.701*	(15.584)	−37.208*	(16.108)	−19.744+	(11.264)	−19.814+	(11.081)
Constant	581.535+	(325.332)	606.171+	(330.256)	2.474	(2.700)	2.544	(2.634)
Kleibergen-Paap *F*-statistic	14.24		14.41		10.81		5.8	
R^2	.94		.94		.93		.93	

Note. Robust standard errors, clustered by municipality, are in parentheses. Town and month fixed effects include 337 municipalities and 21 individually coded months and are included in all regressions. $N = 7{,}026$.

[a] Measured per mile of local road in columns 3 and 4.

+ Significant at the 10% level.

* Significant at the 5% level.

** Significant at the 1% level.

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 column 1 largely support the primary hypothesis behind our identification strategy. The number of tickets written increases with a failed referendum and decreases with a passed referendum. Further, the number of tickets increases with the dollar amounts requested for both passed and failed referenda. The point estimate on the passage of an override referendum indicates that passage leads to a decrease in the number of tickets issued by 14.3 tickets but that with each $100,000 increase in the amount requested in the referendum, another 1.8 tickets are issued. This implies that passing an override referendum leads to fewer tickets when the amount requested is below $794,000 and that the number of tickets issued increases for higher amounts.[26] In our data sample, 30.1 percent of observations with a passed referendum were for requests in excess of $794,000 and thus resulted in additional tickets being issued.

The positive coefficients on OverrideFail_{it}, $\$\text{OverrideFail}_{it}$, and $\$\text{OverridePass}_{it}$ are in line with our predictions, but 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 passing overrides requesting large dollar amounts, indicating significant fiscal distress, leads to more tickets issued. Passing smaller overrides, however, alleviates the fiscal pressure to pursue alternative revenues and in turn corresponds to fewer tickets.

Table 2, column 2, reports the coefficients from the alternative instrument specification and includes the interaction $\text{Outtown}_{it} \times \text{Override}_{it}$. Here we find that many of the override variables fail to be statistically significant, with three exceptions. The $\text{Outtown}_{it} \times \text{OverrideFail}_{it}$ and $\text{Outtown}_{it} \times \text{OverridePass}_{it}$ coefficients are statistically significant and positive with substantial magnitudes. Perhaps surprisingly, the OverrideFail_{it} coefficient is negative, with a point estimate of −10.64. One possible interpretation is that this is indicative of an increase in tickets issued to out-of-town drivers coupled with a decrease in tickets issued to locals. The results in columns 3 and 4 are similar, although $\$\text{OverrideFail}_{it}$ is not statistically significant in column 3.

The Kleibergen-Paap *F*-statistics in columns 1 and 2 are 14.24 and 14.41 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 in only a small bias of two-stage least squares (2SLS) estimates (see Stock and Yogo [2005] for a discussion of this issue). For

[26] We have 654 observations for which the override referendum passed. For these observations, the mean dollar amount requested in the referendum was $1,031,709 (SD = $1,631,979). For failed override referenda, we have 178 observations with a mean of $1,275,545 (SD = $3,366,799).

example, a 10 percent bias is associated with a Kleibergen-Paap *F*-statistic of 10 (5 percent with an *F*-statistic of 16.85; see Stock and Yogo 2005). The excluded instruments using per-mile measured variables, however, have Kleibergen-Paap *F*-statistics of 10.81 and 5.8, which suggests 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, that use several excluded variables to identify a single endogenous variable (Stock, Wright, and Yogo 2002). The Anderson-Rubin null hypothesis that the excluded variables' coefficients equal zero can be rejected at the 2 and 6 percent level in the primary IV specifications and at the 5 and 16 percent level in the per-mile measured IV specifications (Table 3). 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 $\text{Outtown}_{it} \times \text{Override}_{it}$ result in Kleibergen-Paap *F*-statistics of 2.0 and .25. This lends further credence to our hypothesis that tickets issued by officers employed by local government are connected to local fiscal distress and the efforts to raise revenues.

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 (column 1).[27] 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. The coefficient on Tickets is negative and statistically significant, which indicates that OLS regression without town fixed effects underestimates the effectiveness of traffic law enforcement on accidents. The point estimate in column 2 implies that issuing 100 extra tickets leads to 4.0 fewer car crashes. In our data set, the mean numbers of accidents and tickets are 37 and 86, respectively, with standard deviations of 60 for accidents and 129 for tickets. Thus, the .04-point estimate implies that a 1-standard-deviation increase in tickets leads to 6.6 fewer accidents, 11 percent of the standard deviation in accidents.[28]

The fixed effects specification does not control for the possibility that dangerous behavior ebbs and flows in a municipality and that law enforcement responds accordingly. If changes in dangerous behavior in a town lead to more

[27] Without month effects, the point estimate is positive and statistically significant as well.

[28] In just under 12 percent of our observations is OverridePass_{it} or OverrideFail_{it} equal to one. Only 24 percent of a subset of municipalities have either OverridePass_{it} or OverrideFail_{it} equal to one at any point in the sampled time frame. To test for the dependence of our results on sample selection, we ran the Table 3 ordinary least squares (OLS) crash 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 town fixed effects and remained almost identical with town fixed effects. The negative effect of tickets on accidents does not appear to be a subsample phenomenon.

Table 3
Effects of Traffic Enforcement on Accidents

	Crashes				Crashes per Mile of Local Road			
	OLS		IV		OLS		IV	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Tickets[a]	.071^{+} (.037)	−.040** (.015)	−.127** (.034)	−.162** (.055)	.009 (.027)	−.042** (.010)	−.143** (.055)	−.075 (.061)
Outtown[a]	.034 (.047)	.026 (.016)	.054* (.025)	.065* (.026)	.001 (.032)	.074** (.026)	.123** (.036)	.090* (.037)
Other public safety expenditures[a]	.005 (.004)	.010* (.005)	.012* (.005)	.012** (.005)	−.001 (.003)	.003 (.006)	.002 (.006)	.002 (.006)
Registered vehicles[a]	−2.486* (.990)	−3.579 (4.908)	−1.840 (4.850)	−1.055 (5.299)	−.487 (.598)	−6.190** (1.454)	−5.654** (1.538)	−6.041** (1.498)
Average miles per hour over the speed limit	−.037 (.300)	−.026 (.078)	−.062 (.080)	−.079 (.082)	−.003 (.003)	−.001 (.001)	−.002 (.001)	−.001 (.001)
Minority drivers stopped[a]	−28.575** (6.312)	−2.240 (2.330)	1.176 (3.025)	2.528 (3.193)	3.802 (4.474)	.134 (1.552)	2.056 (2.271)	.766 (1.562)
Drivers stopped at night[a]	5.882 (4.299)	3.671 (2.261)	2.160 (3.055)	1.615 (3.119)	−2.054 (2.329)	.501 (2.298)	−.691 (2.232)	.107 (2.387)
Female stopped drivers[a]	2.315 (7.428)	−.732 (2.184)	3.355 (2.611)	4.923 (4.002)	−.665 (4.471)	−9.271* (4.064)	−6.972* (3.495)	−8.491* (4.261)
Chapter 90 highway funding[a]	.053* (.026)	.350* (.145)	.209 (.157)	.163 (.153)	.094** (.025)	.139 (.089)	.120^{+} (.064)	.134^{+} (.076)
Unemployment claims[a]	4.617** (.782)	.883 (1.007)	1.512 (1.053)	1.793 (1.351)	.788 (.720)	−.920 (.712)	−.434 (.716)	−.777 (.832)
Property value per capita	.135* (.065)	−.019 (.082)	.012 (.070)	.017 (.072)	−.001* (.001)	−.001 (.002)	.000 (.002)	−.000 (.002)
Police expenditures[a]	−.006 (.004)	−.000 (.004)	.002 (.004)	.003 (.004)	.001 (.002)	.002 (.005)	.005 (.006)	.003 (.005)
Population[a]	3.253** (1.157)	11.082 (11.029)	8.538 (10.400)	7.291 (10.440)	1.345* (.521)	19.322** (5.991)	17.321** (5.709)	18.649** (5.758)
OverridePass				−.083 (1.151)				.013 (.012)
OverrideFail								−.004 (.030)
$OverridePass				.060 (.138)				−.001 (.001)
$OverrideFail				−.009 (.018)				−.000 (.000)
Constant	−13.311* (6.457)	−212.289 (181.638)			−.153* (.077)	−3.081* (1.454)		
Kleibergen-Paap F-statistic			14.24	14.41			10.81	5.8
Overidentifying restrictions (p-value)			.82	.70			.91	.51
Anderson-Rubin (p-value)			.02	.06			.05	.16
Town fixed effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes
R^2	.80	.951			.637	.874		

Note. Robust standard errors, clustered by municipality, are in parentheses. Town and month fixed effects include 337 municipalities and 21 individually coded months; all regressions include month fixed effects. The first stage for columns 3, 4, 7, and 8 are, respectively, columns 1, 2, 3, and 4 from Table 2. OLS = ordinary least squares; IV = instrumental variables. N = 7,026.

[a] Measured per mile of local road in columns 5–8.
$^{+}$ Significant at the 10% level.
* Significant at the 5% level.
** Significant at the 1% level.

tickets and more accidents, then the coefficient in column 2 is biased upward. Column 3 of Table 3 addresses this concern by using the instruments and first stage presented in Table 2, column 1. The results for overidentifying restrictions tests and the Kleibergen-Paap and Anderson-Rubin results provide 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 using instrumental variables. Relative to OLS estimates, the coefficient on Tickets triples and suggests that issuing 100 additional tickets leads to 12.7 fewer car crashes. The results from this 2SLS model imply that a 1-standard-deviation increase in tickets leads to a reduction in accidents of 27 percent of the standard deviation of accidents. We employ our alternative instrument specification, the $\text{Outtown}_{it} \times \text{Override}_{it}$ 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, which suggests that issuing 100 additional tickets would result in 16.2 fewer accidents.

Columns 5–8 present the same specifications as in columns 1–4, but with variables measured per mile of local road when appropriate. The coefficients on tickets issued per mile exhibit signs and magnitudes that correspond to the results in columns 1–4. 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 IV are added (columns 7 and 8). The results in column 7 suggest that for every 100 tickets written per mile, there are 14.3 fewer accidents per mile. This estimate implies that a 1-standard-deviation increase in tickets per mile reduces accidents per mile by .184, or 45 percent of a standard deviation in accidents per mile. Unlike the model that does not measure accidents per mile, the 2SLS specification with $\text{Outtown}_{it} \times \text{Override}_{it}$ (column 8) shows a slightly lower magnitude, with issuing 100 extra tickets correlating to 7.5 fewer accidents per mile, and is not statistically significant ($p = .22$).

Tables 4 and 5 present analyses of injuries and fatalities. Injuries and fatalities are a measure of the severity of accidents. Similar to the results for 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 ($p = .11$) when adding municipality fixed effects (column 2). The two IV estimates show a negative effect of tickets on the number of injuries that is statistically significant at the 5 percent level. The coefficient on tickets in the injury regression triples from the OLS specification in column 2 to the IV specification in column 3. The IV specification using the Outtown_{it} interaction variables as the excluded instrumental variables (column 4) indicates that issuing 100 additional tickets results in 6.7 fewer injuries associated with traffic accidents. This implies that a 1-standard-deviation increase in tickets issued reduces injuries

Table 4

Effect of Traffic Enforcement on Injuries

	Injuries								Injuries per Mile			
	OLS				IV				IV			
	(1)		(2)		(3)		(4)		(5)		(6)	
Tickets[a]	.012	(.021)	−.013	(.008)	−.044*	(.022)	−.067*	(.030)	−.023	(.036)	.060	(.046)
Outtown[a]	.019	(.024)	.016	(.010)	.026*	(.013)	.033*	(.015)	.030	(.019)	−.011	(.019)
Other public safety expenditures[a]	.000	(.002)	.004^{+}	(.002)	.004*	(.002)	.005*	(.002)	.002	(.003)	.002	(.003)
Registered vehicles[a]	−1.238*	(.499)	−6.445**	(1.837)	−5.827**	(1.941)	−5.328*	(2.137)	−6.969**	(1.129)	−7.471**	(1.393)
Average miles per hour over the speed limit	.045	(.140)	.039	(.046)	.026	(.048)	.014	(.051)	−.000	(.001)	.000	(.001)
Minority drivers stopped[a]	−2.881	(2.900)	−3.330	(2.049)	−2.116	(2.018)	−1.226	(2.118)	2.065	(3.372)	.505	(4.149)
Drivers stopped at night[a]	3.045	(2.191)	2.050^{+}	(1.188)	1.513	(1.468)	1.150	(1.623)	1.745*	(.866)	2.712*	(1.153)
Female stopped drivers[a]	−3.133	(2.955)	−.735	(1.528)	.717	(1.932)	1.742	(2.343)	−5.192*	(2.417)	−7.067*	(3.430)
Chapter 90 highway funding[a]	.020^{+}	(.012)	.216**	(.077)	.166*	(.080)	.135^{+}	(.080)	.093*	(.045)	.106	(.068)
Unemployment claims[a]	2.216**	(.401)	−.422	(.477)	−.198	(.487)	−.018	(.605)	−.668	(.469)	−1.095^{+}	(.573)
Property value per capita	.064^{+}	(.035)	.015	(.045)	.026	(.042)	.034	(.042)	.001	(.001)	.000	(.001)
Police expenditures[a]	−.005*	(.002)	−.001	(.001)	−.000	(.002)	.000	(.002)	−.001	(.003)	−.003	(.003)
Population[a]	1.839**	(.648)	4.377	(4.374)	3.473	(4.239)	2.646	(4.219)	5.166^{+}	(3.460)	6.733^{+}	(3.460)
OverridePass							−.467	(.525)	.001	(.008)	.001	(.008)
OverrideFail							.556	(.973)	.005	(.012)	.005	(.012)
$OverridePass							.044	(.054)	−.001	(.000)	−.001	(.000)
$OverrideFail							−.022**	(.008)	−.000**	(.000)	−.000**	(.000)
Constant	−7.747*	(3.131)	−23.411	(76.544)								
Kleibergen-Paap *F*-statistic					14.24		14.41		10.81		5.8	
Overidentifying restrictions (*p*-value)					.75		.76		.51		.34	
Anderson-Rubin (*p*-value)					<.01		.08		<.01		<.01	
Town fixed effects	No		Yes		Yes		Yes		Yes		Yes	
R^2	.757		.915									

Note. Robust standard errors, clustered by municipality, are in parentheses. Town and month fixed effects include 337 municipalities and 21 individually coded months; all regressions include month fixed effects. The first stage for columns 3, 4, 5, and 6 are, respectively, columns 1, 2, 3, and 4 from Table 2. OLS = ordinary least squares; IV = instrumental variables. N = 7,026.

[a] Measured per mile of local road in columns 5 and 6.

$^{+}$ Significant at the 10% level.

* Significant at the 5% level.

** Significant at the 1% level.

Table 5

Effect of Traffic Enforcement on Fatalities

	Fatalities					Fatalities per Mile	
	OLS		Poisson	IV		IV	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Tickets[a]	−.000 (.000)	−.000 (.000)	−.002* (.001)	−.001 (.001)	−.002* (.001)	.001 (.002)	.002 (.002)
Outtown[a]	−.000 (.000)	.000^{+} (.000)	.002** (.001)	.001 (.000)	.001* (.000)	−.000 (.001)	−.001 (.001)
Other public safety expenditures[a]	−.000 (.000)	.000** (.000)	.000 (.000)	.000** (.000)	.000** (.000)	.000 (.000)	.000 (.000)
Registered vehicles[a]	.007^{+} (.004)	−.001 (.046)	.084** (.022)	.006 (.050)	.041 (.059)	−.010 (.027)	−.013 (.032)
Average miles per hour over the speed limit	.001 (.002)	−.003 (.002)	−.002 (.019)	−.003^{+} (.002)	−.004* (.002)	−.000* (.000)	−.000^{+} (.000)
Minority drivers stopped[a]	.026 (.019)	.047 (.036)	.138* (.057)	.061 (.043)	.119* (.058)	−.011 (.044)	−.023 (.042)
Drivers stopped at night[a]	−.006 (.022)	−.055 (.036)	−.081 (.088)	−.061 (.037)	−.085^{+} (.047)	.021 (.029)	.028 (.035)
Female stopped drivers[a]	.032 (.047)	−.009 (.056)	−.191 (.173)	.008 (.064)	.077 (.082)	−.120 (.089)	−.132 (.107)
Chapter 90 highway funding[a]	.000** (.000)	.001 (.001)	.001^{+} (.001)	.001 (.002)	−.001 (.002)	.000 (.001)	.000 (.001)
Unemployment claims	.008* (.004)	.006 (.007)	.039** (.011)	.008 (.008)	.020* (.011)	.001 (.011)	−.002 (.011)
Property value per capita	−.001 (.000)	−.001 (.002)	−.041** (.010)	−.001 (.002)	−.001 (.001)	−.000 (.000)	−.000 (.000)
Police expenditures[a]	.000 (.000)	.000* (.000)	−.000* (.000)	.000 (.000)	.000 (.000)	−.000 (.000)	−.000 (.000)
Population[a]	−.007* (.003)	−.009 (.062)	.056** (.016)	−.019 (.064)	−.071 (.072)	.042 (.061)	.054 (.060)
OverridePass					−.026 (.022)		.000 (.000)
OverrideFail					−.011 (.030)		.000 (.001)
$OverridePass					−.011 (.030)		−.000 (.000)
$OverrideFail					.002** (.000)		.000** (.000)
Constant	−.026 (.039)	−.292 (1.313)	−2.756** (.424)				
Kleibergen-Paap *F*-statistic				14.24	14.41	10.81	5.80
Overidentifying restrictions (*p*-value)				.76	.16	.72	.18
Anderson-Rubin (*p*-value)				<.01	<.01	<.01	<.01
Town fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes
R^2	.113	.178	.012				

Note. Robust standard errors, clustered by municipality, are in parentheses. Town and month fixed effects include 337 municipalities and 21 individually coded months; all regressions include month fixed effects. The first stage for columns 4, 5, 6, and 7 are, respectively, columns 1, 2, 3, and 4 in Table 2. OLS = ordinary least squares; IV = instrumental variables. N = 7,026.

[a] Measured per mile of local road in columns 6 and 7.

$^{+}$ Significant at the 10% level.

* Significant at the 5% level.

** Significant at the 1% level.

by 32 percent of a standard deviation in injuries. Neither per-mile measured specification results in statistically significant coefficients on tickets per mile.

In Table 5, Tickets is not statistically significant in either OLS specification for fatalities. Because only a relatively small fraction of the sample has zeroes for accidents and injuries, we estimate the accident and injury models using OLS regression. Because the fraction of observations with no fatalities (90 percent) is large, we also estimate the regression using a Poisson model. Poisson estimation with month and municipal fixed effects (column 3) yields a negative coefficient on Tickets that is statistically significant at the 5 percent level. 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. Neither of the per-mile specifications for fatalities results in statistically significant coefficients on tickets per mile.

The coefficients on unemployment filings in our IV analysis of automobile accidents are positive for accidents and negative for accidents per mile but are not statistically significant with either. Conversely, the coefficient on unemployment claims is negative and statistically significant for injuries per mile (Table 4, column 6), in strong concurrence with the findings of Ruhm (2000).

The model's results show that tickets are an effective means of 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 a 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.

We estimate a number of alternative specifications to check the robustness of our findings.[29] First, we address the possibility that government policies may

[29] In our data set, 5.8 percent of our dependent variables in the crash regressions (Table 3) and 15.7 of our dependent variables in our injuries regressions (Table 4) have a value of zero. Given that the dependent variable includes zeros, an alternative estimation method is the Tobit model. When estimating the model using fixed effects but without instrumental variables, we find that our estimates are similar to those when we estimate the model with OLS regressions. For example, in Table 3, column 3, the OLS (with town fixed effects) estimates on tickets is −.040, while the corresponding marginal effect for the Tobit estimate is −.037. Further, in Table 4, with town fixed effects the OLS estimate on tickets is −.013, while Tobit estimate's marginal effect is −.011. In addition, 90.3 percent of our dependent variables in our fatalities regression have a value of zero. Table 5, column 3, presents the fixed effects estimates when we estimate the fatalities regression with a Poisson model. When estimating the Tobit models with instrumental variables, we find that our results are similar to using two-stage least squares regressions. For example, in Table 3, column 3, the instrumental variables estimate on tickets is −.127, while the corresponding Tobit estimate is −.119. The instrumental variables estimate in Table 4, column 3, is −.044, while it is −.037 using Tobit regression. We prefer our OLS estimates to the Tobit and Poisson estimates because of the incidental parameters problem associated with panel Tobit and Poisson models. The maximum-likelihood estimator in nonlinear panel data models with fixed effects is biased and inconsistent when the length of the panel is small and fixed (Greene 2003; Baltagi 2005).

operate with a lag by lagging our override instruments by 1 quarter. Using the lag makes sense for the override variable, but using a lag for the instruments that use stopped out-of-town drivers does not, since it is not clear why out-of-town drivers stopped in the previous quarter should lead to an increase in tickets in the following quarter. Thus, for our first stage, we focus on introducing lags for specifications 1 and 3 of Table 2. Table A3 shows that in this first stage, the point estimates on the lags have the same sign and have magnitude and levels of statistical significance similar to the corresponding estimates in Table 2.[30] Table A4 shows our second-stage results for crashes, injuries, and fatalities. The point estimates have the same signs and magnitudes and levels of significance similar to those in our specifications without lagged instruments. This finding demonstrates the robustness of our results when specifying the first stage such that fiscal conditions in the previous period affect the issuing of tickets in the current period.

As a second robustness test, we use an alternative measure of budgetary conditions in the first stage. Instead of override-related variables, we use the change in the current total revenues from the previous fiscal year and 2-year lagged total revenues. Table A5 presents the first stage from this specification, showing that an increase in revenues leads to a statistically significant drop in tickets issued. Lagged revenues have a positive sign, and the point estimates are statistically significant in both specifications. Table A6 shows the second stage and reveals that issuing more tickets leads to a statistically significant decrease in crashes. The negative effect of the number of tickets on injuries is not statistically significant ($p < .13$), and there is no statistically significant effect of number of tickets on fatalities. Qualitatively, these results are the same as our previous results. However, the estimates in Table A6 are more than 3 times larger than those in our specification that uses the override referenda as an instrument.[31] If municipal events lead to changes in revenues and at the same time to changes in the number of crashes, revenues may not be a proper instrument. Although the specification based on revenues, as an alternative measure of fiscal conditions, leads to similar qualitative results as those based on override referenda, our preferred specifications are those based on the defeat and approval of override referenda.

5. Conclusion

This paper shows that issuing traffic fines reduces the numbers of automobile accidents and related injuries. We address the endogeneity problem that remains

[30] However, we observe a steady, uninterrupted decline in instrument strength the longer the lag (longer lag specifications are not reported). The drop in instrument strength is not surprising—older budgetary conditions as measured by whether a town had an override referendum in the previous year might be less salient than the fact that the town asked voters to vote on an override referendum in the current year.

[31] Because we obtained revenue data from 1999 and 2000, we did not lose any observations when lagging revenues by 1 year.

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 out-of-town drivers who are stopped by the police. Using panel data, we find that more tickets are issued when a town has asked for an override referendum and that ticket issuance increases the more out-of-town drivers are stopped, which lends 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 accident-related injuries than OLS estimation would indicate. The results from this 2SLS model imply that a 1-standard-deviation increase in the number of tickets issued leads to a reduction in accidents by a third of the standard deviation of accidents.

We have not discussed specific mechanisms by which tickets reduce accidents 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 that a variety of stories 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 secondhand stories of speed traps or towns seeking to raise money can spread quickly. When local news affiliates in Massachusetts air stories of an upcoming referendum vote in a neighboring town, drivers may take note to drive more conservatively within its jurisdiction. Before reputations are made, however, issuing additional tickets may improve traffic safety by changing the subjective probability of future tickets, and thus the behavior, of their recipients. While tickets are issued 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.

Appendix A

Table A1

Breakdown of 20 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	.85
Department of Public Works state highway regulations	5,061	.82
Keep right/no view	4,452	.72
No child restraint	4,341	.70
Improper passing	3,785	.61
Fail to yield to pedestrian	3,073	.50
Impeding operation	2,900	.47

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

Table A2

Out-of-Town Drivers on the Road from January 1, 2002, to January 31, 2003

	Out-of-Town Drivers		Local Drivers		Drivers' Hometown Unidentified		
	N	%	*N*	%	*N*	%	Total
Received a ticket	250,413	67	123,640	33	0		374,053
Involved in a crash	111,287[a]	78	30,548	21.5	250	.5	142,085

[a] Crashes involving at least one out-of-town driver are considered out-of-town-driver crashes.

Table A3

First Stage: Results Based on (1 Quarter) Lagged Override Referenda

	Tickets (1)		Tickets per Mile (2)	
OverridePass	−8.790*	(3.89)	−.055	(.056)
OverrideFail	3.812	(4.067)	.188+	(.104)
$OverridePass	1.486**	(.3412)	.006**	(.002)
$OverrideFail	.018	(.032)	−.001+	(.001)
Kleibergen-Paap *F*-statistic	6.52		7.12	
R^2	.59		.69	

Note. Both regressions include town and month fixed effects and the control variables from Table 2. $N =$ 6,962.
+ Significant at the 10% level.
* Significant at the 5% level.
** Significant at the 1% level.

Table A4

Instrumental Variables Estimates: Results Based on (1 Quarter) Lagged Override Referenda

	Crashes (1)	Crashes per Mile (2)	Injuries (3)	Injuries per Mile (4)	Fatalities (5)	Fatalities per Mile (6)
Tickets	−.064*	−.138	−.049**	−.077+	−.001**	−.183
	(.029)	(.084)	(.013)	(.043)	(.0005)	(.141)
Overidentifying restrictions (*p*-value)	.55	.96	.67	.48	.78	.62
Anderson-Rubin (*p*-value)	.04	.02	<.01	<.01	.10	.19

Note. When the dependent variable is measured per mile, tickets are also measured per mile. Robust standard errors, clustered by municipality, are in parentheses. All regressions include town and month fixed effects and the control variables in Table 3. $N = 6{,}962$.
+ Significant at the 10% level.
* Significant at the 5% level.
** Significant at the 1% level.

Table A5

First Stage: Alternative Instrument Is Change in Local Revenues

	Tickets (1)		Tickets per Mile (2)	
Local revenues ($t-2$)	63.92E−06**	(1.13E−06)	1.10E−08**	(7.05E−09)
Δ Local Revenues	−2.09E−06*	(8 .21E−07)	−1.65E−08**	(5.60E−09)
Kleibergen-Paap *F*-statistic	10.42		7.21	
R^2	.83		.44	

Note. Δ Local Revenues = Local Revenues (t) − Local Revenues ($t-1$), where t is the fiscal year. Both regressions include town and month fixed effects and the control variables in Table 2. $N = 6{,}057$
* Significant at the 5% level.
** Significant at the 1% level.

Table A6

Instrumental Variables Estimates: Alternative Instrument Is Change in Local Revenues

	Crashes (1)	Crashes per Mile (2)	Injuries (3)	Injuries per Mile (4)	Fatalities (5)	Fatalities per Mile (6)
Tickets	−.213*	−.248+	−.061	−.065	.000	.000
	(.104)	(.015)	(.040)	(.062)	(.001)	(.00)
Overidentifying restrictions (p-value)	.47	.10	.42	.22	.65	.23
Anderson-Rubin (p-value)	<.01	.01	.09	.19	.86	

Note. When the dependent variable is measured per mile, Tickets is also measured per mile. Robust standard errors, clustered by municipality, are in parentheses. All regressions include town and month fixed effects and the control variables in Table 3. $N = 6{,}057$.
+ Significant at the 10% level.
* Significant at the 5% level.

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