Fiscal Incentives in Law Enforcement

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Abstract

In recent years numerous observers have raised concerns about "policing for profit," or the deployment of law enforcement resources to raise revenue rather than to provide public safety. However, identifying the causal effects of fiscal incentives on law enforcement behavior has remained elusive. In a regression discontinuity design implemented on traffic citation and accident data from Saskatchewan, Canada between 1995 and 2016, a fiscal rule reducing by 75% the share of traffic fine revenue captured by the province in towns above 500 in 1996 population is associated with increased rates of accidents, accident-involved vehicles, accident costs, and accident-related injuries in towns just above this threshold, relative to towns just below the threshold. Further, cited drivers in towns just below this threshold are given fewer days to pay their fines and are less likely to pay their fines on time, leading to higher risks of late fees and license suspensions. These findings suggest that fiscal incentives can indeed distort the allocation of law enforcement effort, with distributional consequences for both public safety and economic well-being.

1 Introduction

In recent years numerous observers have raised concerns about "policing for profit," or the deployment of law enforcement resources to raise funds for cash-strapped jurisdictions (Carpenter et al 2015; United States Department of Justice 2015, Makowsky 2019). Fiscal incentives may distort law enforcement priorities, leading to allocations of law enforcement effort that are not fully responsive to public safety needs. Fiscally-motivated law enforcement effort may also have significant economic consequences for those targeted by this effort.

However, identifying the causal effect of fiscal incentives on law enforcement behavior has remained elusive. First, the endogenous reactions of potential offenders to increased revenue-seeking by law enforcers may confound estimates of the effects of fiscal incentives on law enforcement behavior. Jurisdictions with greater fiscal incentives to generate revenue may direct officers to increase enforcement effort, for example by issuing more traffic citations (Makowsky and Stratmann 2009, 2011). However, drivers may respond to an increased probability of receiving a citation by driving more safely. In equilibrium, fiscally-induced increases in unobservable law enforcement effort devoted to issuing citations may not result in increases in observable citation rates, and may even result in decreases in citation rates if drivers are sufficiently responsive to increases in enforcement effort. More generally, fiscally-induced increases in unobservable enforcement effort may have only indeterminate effects on observable enforcement actions. However, revenue-induced increases in law enforcement effort may have other observable implications (e.g. reductions in the frequency of offending).

Second, the conditions giving rise to spatial and temporal variation in fiscal incentives across jurisdictions are not randomly assigned. The same factors that cause some jurisdictions to experience greater fiscal stress, or to enact rules providing for greater revenue extraction from enforcement actions, may also directly affect law enforcement policy and/or offender behavior. Existing empirical work has yet to successfully address the potential endogeneity of the spatial and temporal variation in fiscal incentives.

This article addresses both of these issues in the existing literature by exploiting a fiscal rule reducing by 75% the share of traffic fine revenue captured by the Canadian province of Saskatchewan in towns above 500 in 1996 population. Using yearly and fully aggregated traffic citation and accident data between 1995 and 2016 for towns policed under the province's contract with the Royal Canadian Mounted Police, I find that this fiscal rule is associated with increased rates of accidents, accident-involved vehicles, accident costs, and accident-related injuries in towns just above the population threshold, relative to towns just below the threshold. Towns just above the population threshold experience on average an estimated additional 3 accidents per year, relative to a baseline rate of 4 accidents per year in towns just below the threshold, or a 75% increase in the accident rate. The per capita rates of vehicles involved in accidents, accident costs, and accident-involved injuries are likewise estimated to increase by approximately 80%, 115% and 250%, respectively, just above the population threshold.

There are no discontinuities in traffic citation rates at the population threshold, suggesting that fiscal incentives induced greater enforcement effort in towns just below the population threshold without increasing observable enforcement actions, a finding consistent with endogenous driver response to increased enforcement. There are also no discontinuities in the accident data at the population threshold during the period prior to the introduction of this fiscal rule, in the areas "near" these jurisdictions, within which the province receives 100% of fine revenue throughout the period of interest, or at any of multiple placebo thresholds constructed on either side of the actual population threshold. There are also no discontinuities in the density of towns near the threshold, or in pretreatment town-level census covariates, suggesting that neither sorting nor compound treatments can account for the findings. The findings are substantively large, and are robust to alternative specifications, including local randomization inference. They indicate that fiscal incentives can indeed distort the allocation of law enforcement effort, with distributional consequences for public safety.

Further, cited drivers in towns just below the population threshold, wherein the province receives a larger share of citation revenue, are given approximately 14 fewer days to pay their fines, relative to cited drivers in towns just above this threshold. The share of fines that are not paid on time also doubles in towns just below the population threshold, relative to towns just above the threshold, increasing from 3% to 6%, leading to higher risks of late fees and license suspensions. These findings suggest that revenue-induced enforcement effort can have significant negative impacts on the economic well-being of those targeted by this effort.

The article proceeds as follows. Section 2 reviews the existing literature on fiscal incentives in law enforcement, and discusses the empirical challenges to estimating causal impacts from those incentives. Section 3 describes the empirical setting of traffic safety enforcement in Saskatchewan, Canada, introduces the regression discontinuity design used to estimate causal effects from a rule allocating citation revenue, and reports the results of tests for both sorting and compound treatments at the population threshold specified by the rule. Section 4 describes the traffic and accident data used to estimate the effects of the fiscal rule, and reports descriptive patterns in the data using both plots and summary statistics. Section 5 reports regression discontinuity estimates of the effects of the fiscal rule on the frequency of both citations and accidents, for the periods prior to and after the rule's implementation. Section 6 reports results from a number of additional tests, finding no effects on accidents at any of multiple placebo thresholds constructed on either side of the actual population threshold, or in the areas "near" towns, within which the province receives 100% of fine revenue throughout the period of interest, but finding that results are robust to the use of local randomization inference. Section 7 reports regression discontinuity estimates of the effects of the fiscal rule on the number of days given to cited drivers to pay their citations, and on the proportion of fines that are not paid on time.

The article concludes with a discussion of the generalizability of the findings, given the empirical context of contract policing in small towns. Most research on policing in the United States has focused on its largest cities (Weisburst 2018). However, 70% of the approximately 36,000 town and municipal governments in the United States have populations of fewer than 2,500 residents (U.S. Census Bureau, 2012 Census of Governments), and many of these small towns contract with larger jurisdictions for policing services. The findings reported here suggest that these smaller jurisdictions are not immune to the distorting effects and distributional consequences of fiscal incentives in law enforcement.

2 Fiscal Incentives in Law Enforcement

2.1 Prior Literature

Providing financial incentives to law enforcement actors used to be a common practice in the United States (Parrillo 2013). In many jurisdictions, police officers were rewarded financially for making arrests, and prosecutors were rewarded for indictments or convictions. These financial incentives for the enforcement of criminal laws were largely eliminated by the late nineteenth century, however, amid concerns about the over-enforcement of those crimes with the greatest financial rewards for enforcers (Ibid.). The potentially distorting effects of financial incentives on the enforcement of the criminal law is also emphasized by Landes and Posner (1975), who suggest that profit-induced private enforcement of the criminal law will generally lead to over-enforcement of those crimes with the greatest financial returns.

In recent years renewed attention has been directed at financial incentives for enforcement of the criminal law. For example, federal and state statutes enabling the seizure and forfeiture of assets suspected of a connection to a crime have been widely criticized for improperly motivating the over-enforcement of laws proscribing crimes with seizable assets, particularly in the case of defendants with the least economic means to contest asset seizures (Carpenter et al 2015). The Department of Justice's recent report on Ferguson, Missouri likewise criticized the city's reliance on traffic citations as a source of revenue, suggesting that the city had inappropriately over-enforced traffic safety laws as a means to generate revenue, particularly from its most vulnerable residents, contributing to distrust between the city's police force and those residents (United States Department of Justice 2015).

If law enforcement agencies can keep the proceeds from seized and forfeited assets, and/or face political pressure from elected officials to generate citation revenue, they may over-enforce laws proscribing revenue-generating crimes. This selective over-enforcement may result in suboptimal outcomes, for example by diverting law enforcement resources from the enforcement of laws proscribing violent crime, and/or by generating negative economic externalities for those who are the targets of revenue extraction (Goldstein et al 2018, Makowsky 2019).

Empirical social scientists have pursued a number of analyses designed to identify the causal effects of fiscal incentives on law enforcement behavior. Some prior studies have looked at the effect of de facto or de jure variation in civil asset forfeiture revenue sharing rules on observable law enforcement actions. Baicker and Jacobson (2007), for example, find that increases in the de facto share of counties' civil asset forfeiture revenues kept by local law enforcement agencies are associated with increases in opiate and cocaine drug arrests between 1991 and 1999. Kelly and Kole (2016) find positive effects of local law enforcement agency seizures on drug arrest rates between 2000 and 2007.[1] Makowsky et al (2019) find that county-level deficits have a larger positive effect on drug arrest rates for nonwhite defendants in states wherein local governments may retain revenue from asset forfeitures.

Others have looked at the effect of municipalities' fiscal conditions on traffic citation behavior. Using a two-month sample of speeding citations from 350 Massachusetts municipalities in 2001, Makowsky and Stratmann (2009) find that the likelihood of receiving a fine rather than a warning, and the dollar amount of a fine conditional on receiving a fine, are higher in municipalities with a failed referendum vote on a property tax increase in the prior fiscal year, and in those with lower property tax bases. Using North Carolina counties observed over a panel of 14 years, Garrett and Wagner (2009) find that annual decreases in county revenue per capita are followed by annual increases in traffic tickets per capita. Using a set of 300 Massachusetts municipalities observed over a 21 month period between 2001 and 2003, Makowsky and Stratmann (2011) find that a failed referendum vote on a property tax increase increases ticketing in the following fiscal year, and that increased ticketing is associated with decreased accident rates. Goldstein et al (2018) estimate the effect of a city's reliance on fines and fees as a proportion of own-source revenue on violent and property crime arrest rates, finding that lower proportions of revenue sourced from fines and fees are associated with higher arrest rates for violent and property crime.

2.2 Endogenous Offender Response

Identifying the causal impacts of fiscal incentives on law enforcement behavior is, however, a challenging exercise. First, offenders may respond to fiscally-motivated increased enforcement effort in ways that confound the estimation of causal effects. Existing studies have generally used observable law enforcement actions (e.g., arrest and citation rates) as the outcome of interest. However, law enforcement agents and potential offenders may condition their actions on each other's behavior. If potential offenders perceive an increased probability of detection, perhaps as a result of fiscally-induced increased law enforcement effort, they may be less likely to offend. If law enforcement effort itself is not directly observable, as is typically the case, financial incentives that induce greater enforcement effort, resulting in an increased probability of detection of offenders, may result in more, the same, or fewer observable enforcement actions, depending on the elasticities of offenders' responses (Mungan 2018a, 2018b).

Imagine, for example, that an enforcement agency seeks to increase the revenue realized from issuing speeding tickets by increasing the effort it devotes to detecting speeding. The probability of being sanctioned for speeding will increase. This will make speeding a more expensive activity for those who derive utility from it. The marginal speeder, who only weakly prefers speeding to not speeding, will change his behavior and comply with the speed limit, which will reduce the number of speeders on the road. Because the expected number of tickets equals the probability of detection times the number of speeders, an increase in the probability of detection increases [decreases] the expected number of tickets if the number of speeders is inelastic [elastic] with respect to the probability of detection. The number of tickets issued may also be completely unresponsive to enforcement effort, if the number of speeders is unit-elastic with respect to the probability of detection. In short, the relationship between law enforcement effort and the number of tickets issued depends on the elasticities of offenders' responses to that effort.

In other words, the number of tickets issued by a revenue-maximizing enforcement agency need not be related to the effort its agents exert in any meaningful way. In particular, a revenue-maximizing enforcement agency tasked with allocating its resources across two identical districts, one from which it receives higher returns per issued ticket, may issue more, the same number of, or even fewer tickets in the district in which it receives higher returns per ticket, relative to the second district. This logic suggests that using the number of tickets issued (or the frequency of observable enforcement actions more generally) as a measure of the effect of fiscal incentives on enforcement effort is problematic. Other proxies, such as accident rates (or the frequency of offending more generally), may be better measures of the effect of fiscal incentives on enforcement effort.

Moreover, the revenue generated by tickets is not only a function of the number of tickets issued, but is rather the product of the number of tickets issued and the average fine per ticket. In many settings, offenders can choose how much to deviate from the legal option, and sanctions are increasing in the magnitude of this deviation. Speeding falls squarely under this setting: an offender chooses not only whether to speed above the limit, but also how much to speed above the limit, and speeding fines are generally increasing in detected speed. If increased enforcement effort deters the marginal speeders, who only drive slightly above the speed limit, the remaining population of speeders will have higher average speed, relative to the population of speeders at lower enforcement levels. Increasing the probability of detection of these high speeders may increase an agency's fine revenue, even though the total population of speeders and the overall average speed in the jurisdiction have decreased.

This means that there exist plausible configurations of driver preferences under which an enforcement agency will find it profitable to allocate greater enforcement effort to a high-revenue district, and, despite this, we will not necessarily observe a higher rate of citations in this district, relative to a low-revenue/low-enforcement district. However, we will see decreased average driver speed in the high-revenue/high-enforcement district, relative to the low-revenue/low-enforcement district. To the extent that the frequency and severity of accidents are increasing in average driver speed, we would also expect to see an increased frequency and severity of accidents in the low-revenue/low-enforcement district, relative to the high-revenue/high-enforcement district.

2.3 Endogenous Fiscal Incentives

A second challenge to estimating the causal effects of fiscal incentives in law enforcement is created by the fact that variation in fiscal incentives across jurisdictions is likely not randomly assigned. The lack of random assignment implies that spatial and temporal variation in fiscal incentives may not be causal of variation in enforcement or offending behavior. For example, it may be the case that in jurisdictions where (unobservable to the researcher) drug use is increasing, voters become more supportive of asset forfeiture rules that reward law enforcement agencies for drug enforcement. In that case, drug arrests could increase because the frequency of drug offending is increasing, not because enforcement agencies are responding to fiscal incentives. Likewise, jurisdiction-specific fiscal stress may not be causal of observed increases in ticketing. Fiscal stress may directly affect driver behavior, leading to increased offending; increases in ticketing could result from increases in offending directly caused by financial stress, not from agencies' response to fiscal incentives.

While existing studies have contributed to our knowledge about the empirical contexts within which revenue-induced policing may occur, no study has yet convincingly identified a causal effect of fiscal incentives on law enforcement behavior. Moreover, given that policing agencies may face pressure from their principals not only to generate revenues, but also to increase safety (i.e., reduce crime), it is not ex ante clear that fiscal incentives will dominate safety incentives.

This article addresses these shortcomings in the existing literature. First, the impacts of fiscal incentives are estimated on both offender and law enforcement behavior. Second, a regression discontinuity design is implemented to isolate the causal impacts of fiscal incentives on offender behavior.

3 Empirical Setting and Design

In 1997, a statute enacted in Saskatchewan, Canada changed the rules governing the distribution of revenue from traffic citations issued by the Royal Canadian Mounted Police under their policing contract with the province.[2] Prior to the passage of this statute, towns between 500 and 1500 in 1991 population were policed under the province's RCMP contract, but the province delegated to these towns responsibility for both the costs and the direction of RCMP traffic enforcement, as well as 100% of the revenue from RCMP-issued citations, within their municipal boundaries.[3] The province was responsible for the costs and the direction of RCMP traffic enforcement in municipalities with less than 500 in population, and in all areas outside of municipal boundaries, and retained 100% of the revenue from RCMP citations issued in these areas.

In The Police Act Amendments 1997, effective January 1, 1999, the province was made responsible for the costs and direction of RCMP traffic safety enforcement in towns between 500 and 1500 in 1991 population, in addition to towns below 500 in population and all rural areas outside of towns. However, the province allowed towns with populations of at least 500 in the 1996 census to retain 75% of the revenue from citations issued within their municipal boundaries under this new regime, even though these citations would now be issued by RCMP officers directed by the province, rather than by the towns (The Police Regulations 1998). The province continued to retain 100% of citation revenue in municipalities with less than 500 in population, and in all areas outside of municipal boundaries. This allocation of citation revenue remained in effect through 2016.

The 1997 Amendments to The Police Act thus created two distinct revenue opportunities for the province under its RCMP policing contract. After January 1, 1999, the province employed, paid for, and supervised all RCMP officers enforcing traffic safety under the provisions of the province's Traffic Safety Act.[4] But while the province received 100% of the fine revenue from traffic citations written by these RCMP officers within the boundaries of towns with less than 500 in 1996 population (and also outside of the boundaries of all municipalities), it received only 25% of the fine revenue from traffic citations written by these officers within the boundaries of towns with 500 or greater in 1996 population.

The 1997 Amendments to The Police Act applied to 119 towns, in 39 of which the province received 100% of citation revenue between 1999 and 2016, and in 80 of which the province received only 25% of citation revenue. High-revenue and low-revenue towns are distributed throughout the province. During this period most RCMP detachments were responsible for policing areas within which the province received different proportions of revenue from traffic citations issued by RCMP officers.[5]

The citation revenue regime enacted in the 1997 Police Act Amendments is well-known to provincial leaders, who have been repeatedly lobbied by those municipalities not receiving citation revenue to be included in its revenue-sharing provisions.[6] To date the province has resisted extending revenue-sharing to other municipalities, citing fiscal concerns. The citation revenue-sharing arrangement also appears to be known to the province's RCMP patrol officers. During an interview conducted in June 2018, for example, when asked about the province's practice of requiring RCMP patrol officers to record whether a traffic offense occurred inside or outside a municipality's boundaries, the patrol officer replied, "That's how they know where the money goes."

Clearly there were financial incentives for provincial leaders to want to see relatively more citations per capita issued in the high-revenue towns below the population threshold, relative to the low-revenue towns above the population threshold. These incentives may have been transmitted to the province's agent, the Saskatchewan division of the RCMP. Even assuming that these incentives were communicated to RCMP agents, and that those agents attempted to respond to those incentives, however, we would not necessarily expect to see a higher rate of citations in the high-revenue towns, relative to the low-revenue towns, due to the deterrent effect of increased enforcement. Yet we would expect greater enforcement effort in the high-revenue towns below the population threshold to have decreased average driver speed, relative to the low-revenue towns above the population threshold. To the extent that the frequency of accidents is increasing in average driver speed, we would expect to see an increased frequency of accidents in the low-revenue towns, relative to the high-revenue towns.

The institutional variation in the treatment of citation revenue introduced by the 1997 Amendments to Saskatchewan's Police Act is essentially cross-sectional, and by construction correlated with population size. Conventional cross-sectional estimation strategies will fail to identify any causal effect of this fiscal institution on ticket and accident outcomes, given the likely associations between population size and unobserved characteristics that may influence driving behavior. For example, because of their larger numbers of drivers, larger towns may be more likely to fund advertising campaigns promoting safe driving behaviors, or to fund roadway safety improvements. Instead of attempting to control for cross sectional variation, we can leverage the discontinuity induced by the 1997 Amendments between towns just below the threshold of 500 in 1996 population, and those at or just above this threshold.

Eggers et al (2018) identify two potential threats to causal inference that may arise when population thresholds are used in regression discontinuity (RD) designs. The first is that of compound treatments at the threshold, or multiple policies using the same population cutoff. The second is that of manipulative sorting at the cutoff by jurisdictions with incentives to report population totals above or below the cutoff.

A virtue of the Saskatchewan institutional context is that there do not appear to be compound treatments at the threshold of 500 in 1996 population. The threshold originated in a Canada-wide RCMP requirement that municipalities of less than 500 in population must be policed under a PPSA, while municipalities at or above the threshold of 500 in population may sign MPSAs with the RCMP. The province adopted this threshold in its 1990 Police Act to require that municipalities above 500 in population sign the equivalent of MPSAs with the province. It later rescinded this rule in the 1997 Amendments to the Police Act, keeping the threshold of 500 in 1996 population only for the purposes of allocating citation revenue. The province does not use the population threshold of 500 in 1996 population to define rules for other policies. Moreover, as reported in Table 1 in the Supplementary Materials, populations of Saskatchewan administrative units generally span the threshold of 500 in 1996 population. Towns, the administrative unit that is the focus of the design used here, range between 89 and 4679 in 1996 population. In order to explicitly address the possibility of compound treatments, however, the discontinuity models are replicated during the period prior to the introduction of the fiscal rule on January 1, 1999.

There also does not appear to have been manipulative sorting at the threshold of 500 in 1996 population. In theory, we might be concerned that towns would have had incentives to manipulate the 1996 population data so as to be above the cutoff of 500, and/or that provincial officials would have had incentives to manipulate the census data so as to produce more towns below the cutoff (McCrary 2008). We can test for sorting at the threshold using the density manipulation test in Cattaneo, Jansson, and Ma (2017). Figure 1 reports the results of this test; there is no discontinuous jump in the density of 1996 town populations near the cutoff of 500.[7]

Because sorting could still have occurred in both directions and be unobservable in the density manipulation test, we can also explore the behavior of town-level pretreatment covariates near the population cutoff. In the absence of sorting and/or compound treatments, towns just below and just above this cutoff should have been relatively similar to each other in observed pretreatment covariates. We can look for continuity in a variety of town-level covariates sourced from the 1996 Canadian census, including: median income,

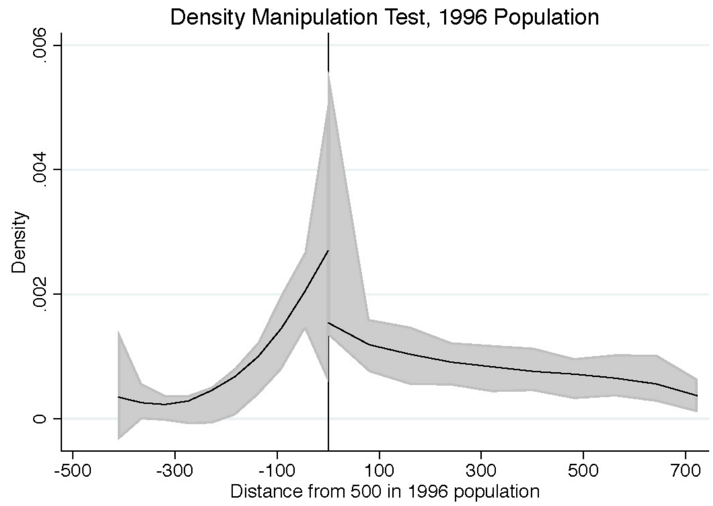

Figure 1: Local quadratic density estimation with local cubic bias correction; triangular kernel; MSE-optimal bandwidth selection with jackknife standard errors; 95 % confidence intervals; sample of 119 towns used in regression discontinuity models.

percent married, percent nonwhite ("visible minorities"), percent without a high school degree, and percent of the male population who drive themselves to work.[8] We can also check for smoothness near the cutoff in average populations over the period of interest, where town-level population is linearly interpolated from the Canadian censuses of 1996, 2001, 2006, 2011, and 2016, then averaged over the period January 1, 1999 through December 31, 2016.

Table 1 reports descriptive statistics for these measures, for towns below and above the population threshold. There appear to be few average differences in pretreatment covariates across the two categories of towns. However, as one would expect, towns below the population threshold have on average fewer residents between 1999 and 2016, relative to towns above the threshold.

Equation 1 estimates discontinuities in these covariates near the population cutoff:

$$Y_i = \alpha + T_i\tau + X_i\beta_- + T_iX_i\beta_+ + \varepsilon_i \tag{1}$$

where Y_i represents a town-level covariate; T_i indicates whether a town receives fine revenue ($T_i = 1$) or does not receive fine revenue ($T_i = 0$); X_i represents the distance of each town's

Table 1: Summary Statistics, Census Covariates

	Mean	SD	Min	Max	N
Towns Below Population Threshold					
Median Income 1996	$15,982.85	2857.20	$12,154	$22,329	34
Percent Married 1996	0.58	0.04	0.48	0.66	39
Percent Visible Minorities 1996	0.009	0.02	0	0.14	39
Percent No HS Degree 1996	0.17	0.07	0.06	0.35	38
Percent Male Drivers 1996	0.75	0.14	0.33	1	38
Average Population 1999-2016	361.84	128.17	83.87	636.20	39
Towns Above Population Threshold					
Median Income 1996	$16,484.24	3220.80	$11,531	$26,002	80
Percent Married 1996	0.57	0.05	0.43	0.71	80
Percent Visible Minorities 1996	0.009	0.02	0	0.09	80
Percent No HS Degree 1996	0.20	0.09	0.02	0.41	80
Percent Male Drivers 1996	0.76	0.12	0.50	1	80
Average Population 1999-2016	843.20	304.18	388.68	1906.04	80

1996 population from the population cutoff of 500 in 1996 population, and contains only units $X_i \in [-h, h]$, where -h and h denote the MSE-optimal bandwidths to the left and right of the population cutoff, respectively; τ is estimated using local linear regression with a triangular kernel; and ε_i is estimated using heteroskedasticity-robust nearest neighbor variance estimation with a minimum of three neighbors; models that implement local quadratic bias correction of the local linear point estimates, and local quadratic bias correction with robust variance estimation, are also reported (Calonico et al 2017, 2018a).[9] Equation 1 is estimated using the set of 119 towns subject to the citation revenue rules enacted in the 1997 Amendments to The Police Act. Table 2 reports these estimates. There are no discontinuities at the population cutoff in either 1996 pretreatment covariates or average population over the period of interest.

A final potential concern is the possibility of displacement or substitution effects (Marceau 1997). Drivers may respond to greater enforcement effort by driving less in the towns that receive more enforcement effort, driving more in the towns that receive less enforcement effort, and/or opting to use mass transit more frequently in the towns with more enforcement effort. The result might be a relatively lower frequency of accidents in the latter towns simply because of a relatively lower volume of drivers, not because these drivers were deterred from speeding.

Table 2: Population Discontinuity Regressions, Pretreatment/Population Covariates

	1996 Median Income	1996 Percent Married	1996 Percent Nonwhite	1996 Pct No HS Deg	1996 Pct Male Drive Work	1999-2016 Average Pop
Conventional	-121.88	-0.02	0.00	0.04	-0.08	-2.73
	(2793.45)	(0.02)	(0.01)	(0.04)	(0.09)	(41.42)
Bias-corrected	1792.30	-0.02	0.01	0.05	-0.09	-3.41
	(2793.45)	(0.02)	(0.01)	(0.04)	(0.09)	(41.42)
Robust bias-corrected	1792.30	-0.02	0.01	0.05	-0.09	-3.41
	(3610.74)	(0.02)	(0.01)	(0.06)	(0.12)	(54.89)
Point/Bias Bandwidths	75/106	181/309	141/276	140/215	110/156	154/239
Towns −/Towns +	19/14	30/28	28/21	28/21	25/17	30/23
N	114	119	119	118	118	119

Estimates of ($T_i = 1$), or a town is above cutoff of 500 in 1996 population, and province receives only 25% of citation revenue. Local linear point estimators using a triangular kernel; bias-corrected models use a quadratic bias estimator. Optimal MSE bandwidth selection. Heteroskedasticity-robust nearest neighbor standard errors (minimum of three neighbors). * p<.10, ** p<.05, *** p<.01.

However, Saskatchewan's geography renders such spillover effects highly implausible. First, there is essentially no mass transit in the province of Saskatchewan. Second, as can be seen in Figures 1 and 2 in the Supplementary Materials, each of the towns wherein the province receives 100% of ticket revenue is surrounded by considerable open space. The province also receives 100% of the revenue in this open space, so RCMP patrol effort should be more or less constant in and near these towns, undermining the possibility of displacement effects. Finally, the towns below the population cutoff are generally not near the towns above the population cutoff. The average distance between a town below the cutoff and all towns above the cutoff ranges from 201 to 383 miles. The shortest distance between a town below the cutoff and all towns above the cutoff ranges from 9 to 64 miles. These distances again undermine the plausibility of displacement effects.

4 Stop and Accident Data

The effects of the fiscal incentives created by the 1997 Amendments to The Police Act are estimated using data on traffic stops and accidents in Saskatchewan between April 1995 and December 2016.

Traffic stop data were obtained from the Saskatchewan Ministry of Justice in the form of all citations issued under the provisions of the Traffic Safety Act. Each citation records a driver identifier as well as the date on which and the jurisdiction within which the citation was issued. Citations were aggregated by driver, day, and jurisdiction to create records of traffic stops (a single stop could result in multiple citations). The citation data also record the fine amount per citation, the date on which a fine is due, and any additional fees assessed per citation because of late payment. These latter data are used in a later section.

Accident data were obtained from Saskatchewan Government Insurance (SGI), the government insurance agency. These data include detailed information on each accident reported for payment.[10] Contained in these data are the date on which and the jurisdiction within which each accident occurred, the number of vehicles involved in the accident, the cost of the accident (as paid out by SGI),[11] and the numbers of individuals injured in each accident. The stop and accident data are available from April 1, 1995 through December 31, 2016.[12]

In the small towns that are near the population threshold of interest, stops and accidents are not frequent events; accident-related injuries sufficiently severe to be reported to SGI are particularly infrequent. To reduce the frequency of zeros, data are aggregated to the town/year. Stops, accidents, numbers of accident-involved vehicles, accident costs, and accident-related injuries are aggregated by town/year, and then divided by town/year population x 100.[13]

Table 3 reports summary statistics for these measures of stops and accidents for the 119 towns in the RD sample between January 1, 1999 and December 31, 2016, at the level of the town/year. There appear to be on average fewer stops per capita in the low-revenue towns above the population threshold, and also more accidents, more vehicles involved in accidents, higher accident costs, and more accident-related injuries per capita, relative to the high-revenue towns below the population threshold. These differences are consistent with relatively greater enforcement effort being directed to the high-revenue towns below the population threshold, relative to the low-revenue towns above the threshold.

Figure 2 plots the stop and accident rates reported in Table 3 as distributions over the distance of each town's 1996 population from the citation revenue threshold of 500. Observations are binned using the default mimicking-variance evenly spaced method provided

Table 3: Summary Statistics, Stop and Accident Data by Town/Year

	Mean	**SD**	**Min**	**Max**	**N**
Stops and Accidents In Towns Below Population Threshold, 1999-2016					
Stops/100 Population	5.12	14.82	0	2227.5	702
Accidents/100 Population	0.78	0.68	0	5.12	702
Accident-Involved Vehicles/100 Population	1.27	1.09	0	7.66	702
Accident Costs/100 Population	$4,102	5,800	0	$75,701	702
Accident-Related Injuries/100 Population	0.11	0.34	0	3.75	702
Stops and Accidents in Towns Above Population Threshold, 1999-2016					
Stops/100 Population	3.83	3.79	0	32.11	1,440
Accidents/100 Population	1.03	0.60	0	3.58	1,440
Accident-Involved Vehicles/100 Population	1.79	1.06	0	6.20	1,440
Accident Costs/100 Population	$5,126	3,328	0	$21,688	1,440
Accident-Related Injuries/100 Population	0.14	0.21	0	1.84	1,440

Entries reported for 39 towns below and 80 towns above population cutoff observed over 18 years (January 1, 1999 – December 31, 2016).

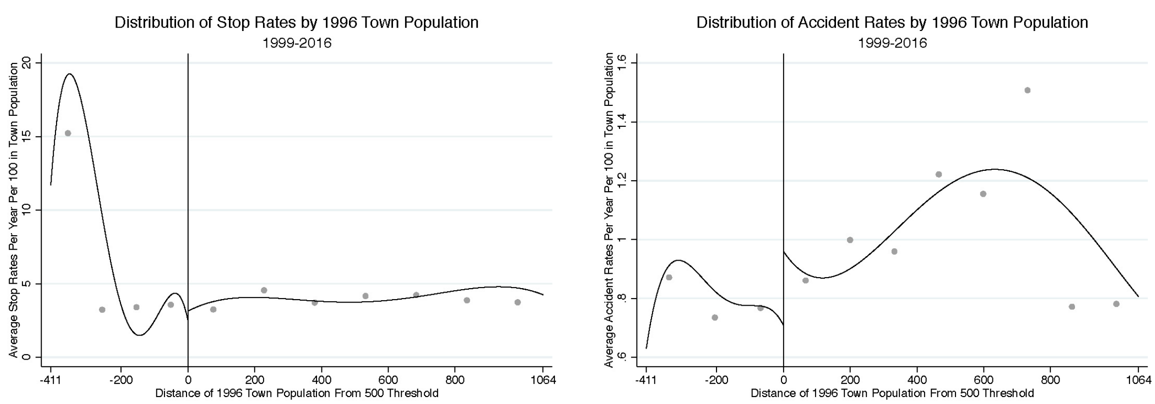

Figure 2: Plots report the distributions of average annual stop and accident rates per 100 in town population, between 1999 and 2016, by the distance of each town's 1996 population from the 500 threshold. Data are binned by rdplot (Calonico et al 2017) using the default mimicking-variance evenly spaced method; a polynomial of degree 4 approximates the population conditional means on either side of the revenue threshold.

by Calonico et al (2017); a polynomial of degree 4 approximates the population conditional means on either side of the revenue threshold. There appears to be an increase of approximately .5 accidents per year per 100 in town population just above the population cutoff of 500, relative to a baseline rate of approximately .7 accidents per year per 100 in town population. Put differently, for the towns close to the cutoff of 500 in population, there is a discontinuous increase of 2-3 accidents per year just above the revenue cutoff, relative to a baseline rate of 3-4 accidents per year just below the revenue cutoff. There does not appear to be any discontinuous increase in stop rates at the population cutoff.

These overall comparisons, however, do not take into account either observed or unobserved confounders other than town/year population. In order to address the possible influence of these confounders on stops and accidents, we can formally leverage the discontinuity at the 1996 population threshold, both with and without the inclusion of pretreatment covariates.

5 Stop and Accident Analysis

To detect the presence of discontinuities in stops and accidents at the 1996 population threshold, Equation 2 is estimated using the town/year stop and accident data summarized in Table 3:

$$Y_{iy} = \alpha + T_i\tau + X_i\beta_- + T_iX_i\beta_+ + \mathbf{Z}_i\gamma + \boldsymbol{\lambda}_y + \varepsilon_{iy}. \tag{2}$$

Calonico et al (2019) suggest that the inclusion of covariates may improve the efficiency of RD point estimates, provided that the covariates themselves behave smoothly near the RD cutoff. Given the smoothness of the behavior near the cutoff of the town-level covariates explored in Table 2, models are estimated both without and with the vector of pretreatment covariates included in Table 2 ($\mathbf{Z}_i$, resulting in the 5 towns with 1996 population below 200 being dropped from the sample) and year fixed effects ($\boldsymbol{\lambda}_y$). Standard errors are clustered on towns.

Before examining the main results, we can test the identifying assumption by looking for discontinuities in stop and accident outcomes during the period prior to the implementation of the citation revenue regime enacted in the 1997 Amendments to the Police Act. This citation revenue regime came into effect on January 1, 1999. The stop and accident data are available from April 1, 1995 through December 31, 1998. During this pretreatment period, we would not expect to see discontinuities in the stop and accident data at the threshold of 500 in 1996 population.

Table 4 reports RD estimates for the pretreatment period of April 1, 1995 - December 31, 1998, including pretreatment covariates. There are no discontinuities in any of the stop or accident measures per 100 persons per town/year during the pretreatment period. Table 3 in the Supplementary Materials reports estimates using logged stop and accident rates; there continue to be no discontinuities at the population threshold. These estimates indicate the general absence of preexisting discontinuities at the threshold of 500 in 1996 population, prior to the introduction of the revenue regime that went into effect on January 1, 1999.

Table 4: Population Discontinuity Regressions, 1995-1998
Stops and Accidents Per 100 in Population Per Town Per Year

	Stops/ 100 Pop/ Town/ Year	Accidents/ 100 Pop/ Town/ Year	Vehicles/ 100 Pop/ Town/ Year	Acc. Cost/ 100 Pop/ Town/ Year	Injuries/ 100 Pop/ Town/ Year
Conventional	-1.95	0.00	-0.01	-727.71	-0.05
	(4.41)	(0.16)	(0.28)	(639.15)	(0.04)
Bias-corrected	-3.74	0.02	0.04	-858.42	-0.08*
	(4.41)	(0.16)	(0.28)	(639.15)	(0.04)
Robust	-3.74	0.02	0.04	-858.42	-0.08
	(6.10)	(0.22)	(0.38)	(949.55)	(0.06)
N	456	456	456	456	456
Point/Bias Bandwidths	71/98	66/98	61/95	69/104	67/121
Towns −/Towns +	14/14	13/14	12/14	14/14	13/14
Year FE	Yes	Yes	Yes	Yes	Yes
Pretreatment Covariates	Yes	Yes	Yes	Yes	Yes

Estimates of ($T_i = 1$), or a town is above cutoff of 500 in 1996 population, and province receives only 25% of citation revenue. Local linear point estimators using a triangular kernel; bias-corrected models use a quadratic bias estimator. Optimal MSE bandwidth selection. Robust standard errors clustered on town. * p<.10, ** p<.05, *** p<.01.

Table 5 reports estimates of Equation 2 for stop and accident measures per 100 in population, by town/year, for the posttreatment period of January 1, 1999 - December 31, 2016. The top panel reports estimates without covariates; the bottom panel reports estimates with covariates. Both with and without the inclusion of covariates, the point estimates for stop rates are generally negative, indicating fewer stops in the low-revenue towns just above the population threshold. However, none of these estimates are significant at conventional thresholds. By contrast, the point estimates for accidents, accident-involved vehicles, accident costs, and accident-involved injuries, all per 100 in population per town/year, are all positive and significant at conventional thresholds, indicating riskier driving behavior in the low-revenue towns just above the population threshold. Consistent with the finding that covariates behave smoothly near the cutoff, estimates are largely unchanged across all models after the inclusion of covariates.[14] Table 4 in the Supplementary Materials reports estimates for logged stop and accident rates, with similar results.

Table 5 also reports baseline averages for each outcome variable, computed within the bandwidths below the population threshold estimated for each model. These baseline averages allow for computation of the magnitudes of effect estimates. Calonico et al (2018) recommend evaluating effect significance using robust bias-corrected p-values, but evaluating effect magnitudes using conventional point estimates. Using the models with included covariates, a high-revenue town within the estimated bandwidth just below the population cutoff saw on average 0.78 accidents per 100 persons per year; the estimated effect of moving just above the cutoff into the low-revenue towns is to increase accidents by 0.59 accidents per 100 persons per year, or by approximately 76%. A high-revenue town within the estimated bandwidth just below the cutoff saw on average 1.3 vehicles involved in accidents per 100 persons per town/year; the estimated effect of moving just above the cutoff into the low-revenue towns is to increase this number by 1.08 accident-involved vehicles per 100 persons per town/year, or an increase of 83%. A high-revenue town within the estimated bandwidth just below the cutoff saw on average \$3,385 in accident costs per 100 persons per town/year; the estimated effect of moving just above the cutoff into the low-revenue towns is to increase accident costs by \$3,950 per 100 persons per town/year, or by about 117%. Finally, a high-revenue town within the estimated bandwidth just below the cutoff saw on average 0.06 accident-related injuries per 100 persons per year; the estimated effect of moving just above the cutoff into the low-revenue towns is to increase accident-related injuries by 0.15 per 100 persons per town/year, or by 250%.

Table 5: Population Discontinuity Regressions, 1999-2016
Stops and Accidents Per 100 in Population Per Town Per Year

	Stops/ 100 Pop/ Town/ Year	Accidents/ 100 Pop/ Town/ Year	Vehicles/ 100 Pop/ Town/ Year	Acc. Cost/ 100 Pop/ Town/ Year	Injuries/ 100 Pop/ Town/ Year
Conventional	-0.93	0.48***	0.87***	3193.41***	0.12*
	(1.68)	(0.14)	(0.23)	(883.71)	(0.06)
Bias-corrected	-0.87	0.52***	0.95***	3290.96***	0.14**
	(1.68)	(0.14)	(0.23)	(883.71)	(0.06)
Robust bias-corrected	-0.87	0.52***	0.95***	3290.96***	0.14*
	(2.09)	(0.16)	(0.26)	(1035.56)	(0.08)
N	2142	2142	2142	2142	2142
Towns	119	119	119	119	119
Years	18	18	18	18	18
Point/Bias Bandwidths	140/217	82/149	80/150	84/146	107/160
Towns –/Towns +	25/21	18/14	18/14	19/14	22/17
Baseline Rate –	3.40	0.72	1.19	$3,403	0.09
Year FE	No	No	No	No	No
Pretreatment Covariates	No	No	No	No	No
Conventional	0.02	0.59***	1.08***	3950.03***	0.15**
	(2.58)	(0.13)	(0.18)	(866.42)	(0.06)
Bias-corrected	-0.28	0.73***	1.34***	4774.62***	0.19***
	(2.58)	(0.13)	(0.18)	(866.42)	(0.06)
Robust bias-corrected	-0.28	0.73***	1.34***	4774.62***	0.19***
	(2.90)	(0.18)	(0.28)	(989.58)	(0.06)
N	2052	2052	2052	2052	2052
Towns	114	114	114	114	114
Years	18	18	18	18	18
Point/Bias Bandwidths	46/81	43/83	42/82	58/97	58/91
Towns –/Towns +	9/13	9/12	9/12	11/14	11/14
Baseline Rate –	4.01	0.78	1.30	$3,385	0.06
Year FE	Yes	Yes	Yes	Yes	Yes
Pretreatment Covariates	Yes	Yes	Yes	Yes	Yes

Estimates of ($T_i = 1$), or a town is above cutoff of 500 in 1996 population, and province receives only 25% of citation revenue. Baseline average rates computed within estimated bandwidths below cutoff for each model. Local linear point estimators using a triangular kernel; bias-corrected models use a quadratic bias estimator. Optimal MSE bandwidth selection. Robust standard errors clustered on town. * p<.10, ** p<.05, *** p<.01.

While these estimated effects are small in absolute terms, reflecting the empirical context of small Saskatchewan towns, they are strikingly large in relative terms, suggesting the potentially large effects of fiscal incentives on both law enforcement and offender behavior.

Figure 3 reports annual RD estimates between 1995 and 2001 of the effect of moving above the population threshold into the low revenue towns, with 90% confidence intervals. The first four plots report coefficient estimates for accident-related outcomes per 100 in town population per year; the last plot reports estimates for traffic stops per 100 in town population per year. Each RD coefficient is estimated using annual averages of the accident and stop rates for each of the 119 towns in the RD sample, including all pretreatment covariates. While the estimates are relatively underpowered, we can see that, across all four accident measures, the effect of the population threshold appears only after the introduction of the citation revenue regime on January 1, 1999. However, we see no post-1999 effect emerge for traffic stop rates.

One concern may be that the models reported in Tables 5 and **??** do not fully address the possible cluster dependence in the error term (Bertrand, Duflo and Mullainathan, 2004). To address this concern we can collapse the data to the town level. Fully aggregating to the town level also addresses any remaining concerns about zeros in the annual data; after aggregating, with the exception of 2 out of the 119 towns that report no accident-related injuries over the sample period, there are no remaining zeros in the stop and accident data. Estimates of Equation 1 using the fully collapsed stop and accident data, both unlogged and logged, are reported in Tables 5 and 6 in the Supplementary Materials. Estimates are qualitatively similar to those estimated using annual data.

Traffic safety enforcement in all of the towns in the estimation sample is provided by the RCMP under a province-wide contract. The RCMP is employed by the province of Saskatchewan under this contract; its commanding officers report to provincial officials in the Ministry of Justice. The province receives very different shares of the citation revenue generated by RCMP traffic enforcement in these towns, depending on where that enforcement happens. These institutional rules suggest that provincial leaders would have had fiscally-motivated incentives to direct RCMP traffic safety enforcement effort to towns within which the province received a larger share of citation revenue, resulting in increased

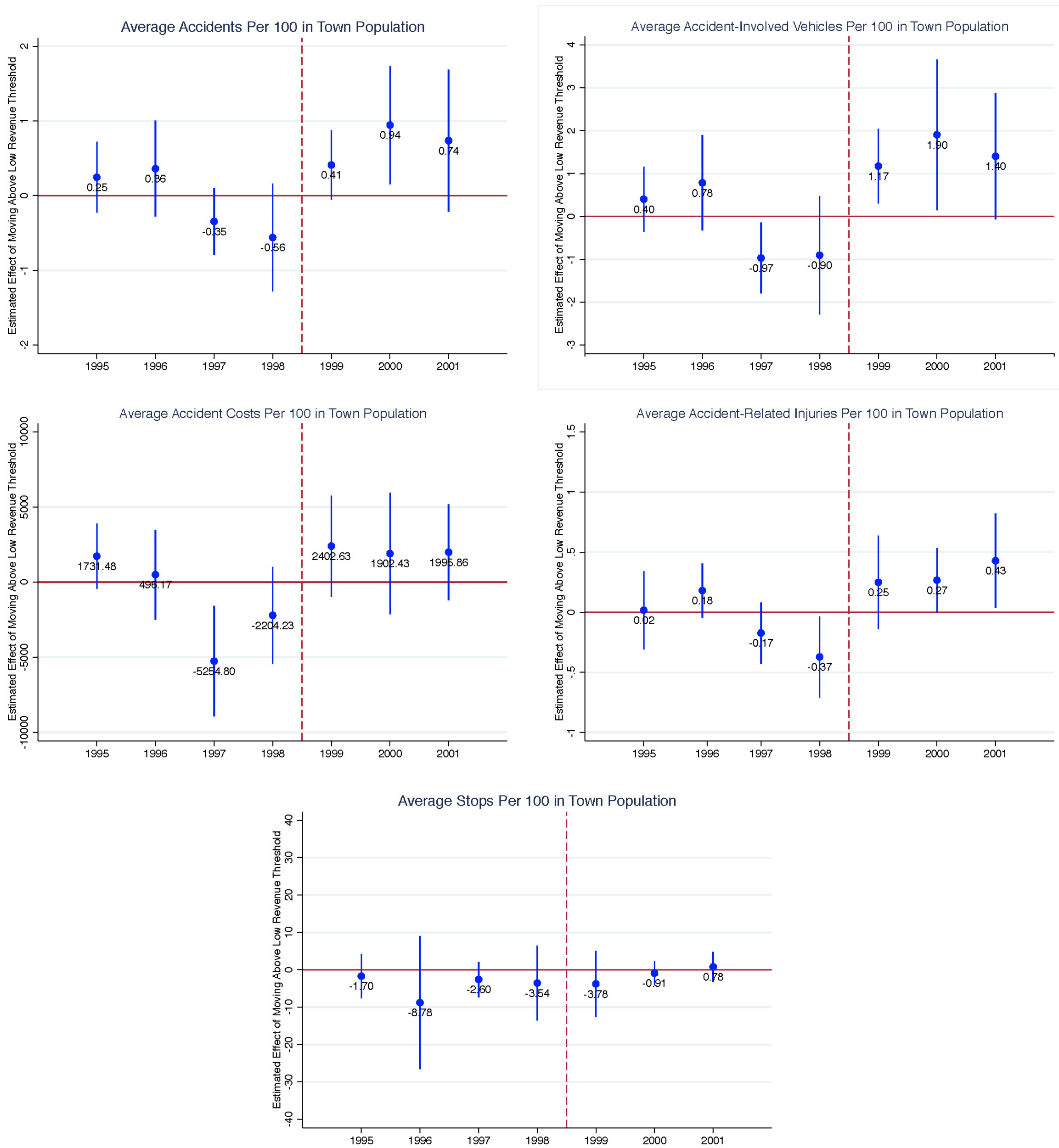

Figure 3: Each plot reports annual RD estimates between 1995 and 2001 of the effect of moving above the population threshold into the low revenue towns, with 90% confidence intervals. The first four plots report coefficient estimates for accident-related outcomes per 100 in town population per year; the last plot reports estimates for traffic stops per 100 in town population per year. Each RD coefficient is estimated using annual averages of the accident and stop rates for each of the 119 towns in the sample, including all pretreatment covariates.

roadway safety in these towns. Using multiple measures, the findings indicate that in fact, the frequency of accidents per capita is higher in towns just above the population threshold distinguishing towns within which the province receives only 25% of citation revenue, from towns within which the province receives 100% of citation revenue. We also see that the frequency of traffic stops appears to be lower just over the threshold in the low-revenue towns, although these estimates are not distinguishable from zero. Although we do not directly observe either RCMP traffic safety enforcement effort (e.g., the volume of RCMP patrol cars on the roads) or driver behavior, these findings are consistent with greater RCMP enforcement effort being directed to the towns within which the province receives a larger share of RCMP citation revenue, with consequent safer driving behaviors, fewer accidents, and (perhaps to the chagrin of the province) reduced opportunities for officers to issue citations for unsafe driving.

6 Robustness

We can challenge the robustness of the stop and accident findings in a number of ways. First, we can create a series of placebo population thresholds on either side of the actual threshold of 500 in 1996 population. Each placebo threshold creates a new grouping of "treated" and "control" towns within the 119 towns in the RD sample. Figure 4 reports RD estimates at each of these placebo thresholds, using the yearly accident data and including all pretreatment covariates, with 90% and 95% confidence intervals. The placebo thresholds are both close to the actual threshold of 500 in 1996 population (e.g. 490 and 510) and further from the actual threshold (e.g. 600). There are significant increases in accident outcomes only at the actual threshold of interest, not at any of the placebo thresholds, for each of the outcome measures.[15]

We can also test the robustness of the results by analyzing citations and accidents that are reported to have occurred "near" a town in our estimation sample. RCMP officers are required to report whether a citation or accident occurred "at" or "near" a municipality. The "at" flag indicates that the accident or stop occurred within a town's municipal boundaries. Stops that occurred "at" a town are subject to the citation revenue rule described above.

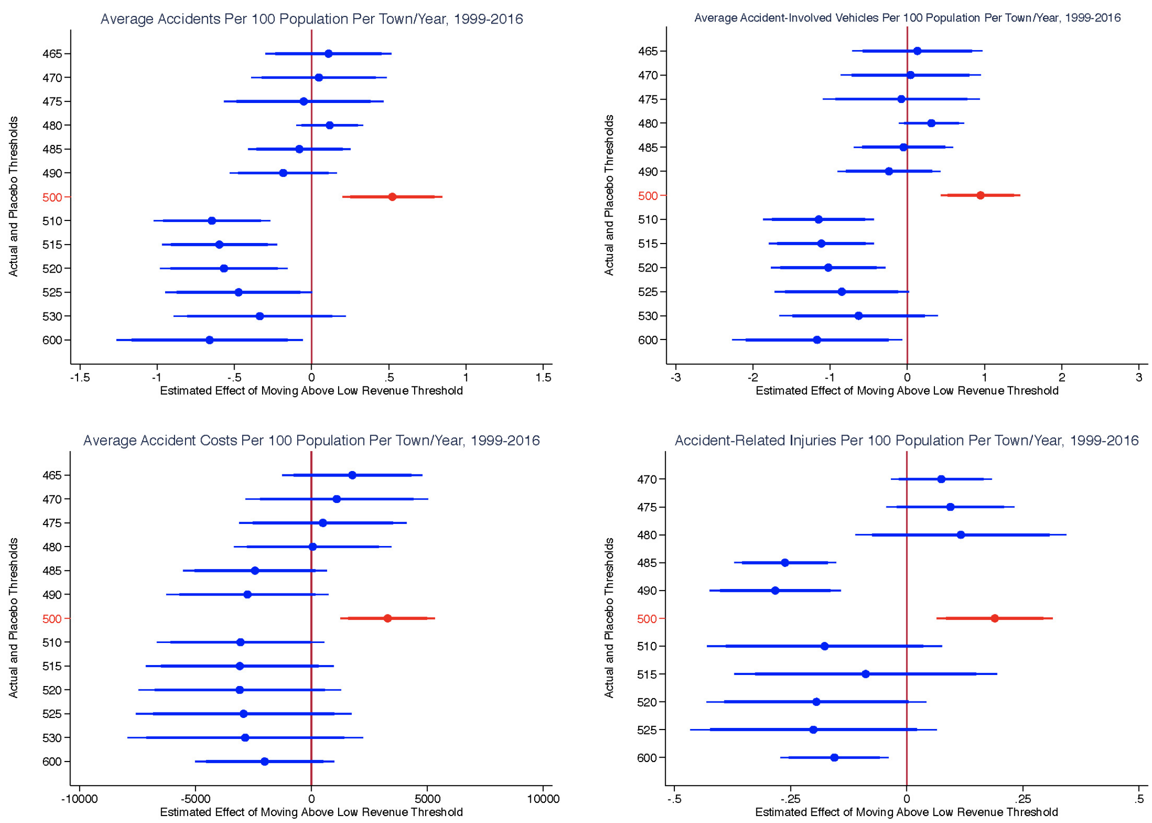

Figure 4: Each plot reports RD estimates at a series of placebo population thresholds on either side of the actual threshold of 500 in 1996 population, using the yearly accident data and including all pretreatment covariates, with 90% and 95% confidence intervals.

The "near" flag indicates that the accident or stop occurred outside a town's municipal boundaries, but was sufficiently close to the town that, according to RCMP lore, the town's grain elevator could be seen by the responding officer.[16] All revenue from citations issued "near" a town is captured by the province, irrespective of the citation revenue rule governing citations issued "at" the town. We should therefore see no discontinuities in accidents at the population threshold in these "near" areas.[17]

Table 7 in the Supplementary Materials reports summary statistics for stops and accidents "near" the towns in the estimation sample, by 100 in town population per town/year.[18] Notably, there appear to be on average *fewer* accidents, accident-involved vehicles and injuries, and *lower* accident costs per 100 persons per town/year, in areas just outside the low-revenue towns above the population threshold, relative to areas just outside the high-revenue towns below the population threshold. These differences are the inverse of those observed inside municipal boundaries, within which citation revenue distribution is governed by the 1997 Amendments to the Police Act, suggesting that the differences we see in the within-municipality accident data are not being driven by unobserved factors spanning areas within and without municipal boundaries.

Table 6 reports stop and accident estimates for locations "near" the towns in the RD design, using per capita measures aggregated to the town/year between 1999 and 2016. As expected, there are no discontinuities at the population threshold in stops or accidents "near" the towns in our RD design, to which the revenue distribution regime does not apply.

Finally, we can deploy local randomization inference at the population threshold as an alternative estimation strategy (Cattaneo et al 2015). This strategy requires identifying the largest window around the population cutoff within which all five pretreatment covariates analyzed in Table 2 demonstrate covariate balance in differences in means tests with p-values of at least 0.15, using 1,000 simulations of the distribution of these means, and then conducting differences in means tests on our outcome variables within this window, again using 1,000 simulations of the distributions of these means.

Table 7 reports the results of these tests using the fully collapsed accident data.[19] All five pretreatment covariates demonstrate balance with p-values of at least 0.15 within a window of 34 in 1996 population on either side of the population cutoff of 500; this window

Table 6: Population Discontinuity Regressions, 1999-2016
Stops and Accidents "Near" Towns Per 100 in Population Per Town Per Year

	Stops/ 100 Pop/ Town/ Year	Accidents/ 100 Pop/ Town/ Year	Vehicles/ 100 Pop/ Town/ Year	Acc. Cost/ 100 Pop/ Town/ Year	Injuries/ 100 Pop/ Town/ Year
Conventional	-1.10	0.37	0.52	1680.85	0.08
	(2.96)	(1.77)	(1.96)	(12709.27)	(0.37)
Bias-corrected	-1.61	0.32	0.47	1328.93	0.11
	(2.96)	(1.77)	(1.96)	(12709.27)	(0.37)
Robust bias-corrected	-1.61	0.32	0.47	1328.93	0.11
	(3.44)	(2.15)	(2.37)	(15235.21)	(0.43)
N	2142	2142	2142	2142	2142
Towns	119	119	119	119	119
Years	18	18	18	18	18
Point/Bias Bandwidths	87/137	99/148	99/148	99/146	88/133
Towns –/Towns +	22/15	25/16	25/16	25/16	22/15
Year FE	No	No	No	No	No
Pretreatment Covariates	No	No	No	No	No
Conventional	-2.77	-0.17	-0.03	-493.62	0.12
	(2.54)	(1.52)	(1.72)	(11042.46)	(0.31)
Bias-corrected	-4.14	-0.10	0.14	3699.88	0.21
	(2.54)	(1.52)	(1.72)	(11042.46)	(0.31)
Robust bias-corrected	-4.14	-0.10	0.14	3699.88	0.21
	(2.92)	(1.75)	(1.96)	(12618.86)	(0.37)
N	2052	2052	2052	2052	2052
Towns	114	114	114	114	114
Years	18	18	18	18	18
Point/Bias Bandwidths	43/80	97/150	92/144	83/128	88/134
Towns –/Towns +	12/12	24/16	23/16	22/14	22/15
Year FE	Yes	Yes	Yes	Yes	Yes
Pretreatment Covariates	Yes	Yes	Yes	Yes	Yes

Estimates of ($T_i = 1$), or a town is above cutoff of 500 in 1996 population, and province receives only 25% of citation revenue. Local linear point estimators using a triangular kernel; bias-corrected models use a quadratic bias estimator. Optimal MSE bandwidth selection. Robust standard errors clustered on town. * p<.10, ** p<.05, *** p<.01.

Table 7: Randomization Inference, 1999-2016
Accidents Per 100 in Population Per Town

	Avg. Accidents/ 100 Pop/ Town	Avg. Vehicles/ 100 Pop/ Town	Avg. Acc. Cost/ 100 Pop/ Town	Avg. Injuries/ 100 Pop/ Town
Difference in Means	6.65***	11.98***	45,699.13***	2.01***
	(0.005)	(0.002)	(0.000)	(0.008)
Covariate Balance Window	34	34	34	34
Towns-/Towns+	12/10	12/10	12/10	12/10
Baseline Rate	14.14	23.42	$65,783	1.14
N	119	119	119	119

Local randomization inference, triangular kernel weighting, 1000 simulations, p-values reported in parentheses. Baseline average rates computed within estimated bandwidths below cutoff for each model.

contains 12 towns to the left of the cutoff and 10 towns to the right of the cutoff. Within this window, we see p-values for differences in means tests on accident variables that are consistently less than .01. We also see estimated effect magnitudes that are somewhat smaller than those estimated in the RD design, but still substantively quite large, with increases in accident outcomes above the low revenue population threshold of 47% for per capita accident rates, 51% for per capita accident-involved vehicle rates, 69% for per capita accident cost rates, and 176% for per capita injury rates.

7 The Economic Consequences of Fiscal Incentives in Law Enforcement

The results reported here suggest that the citation revenue regime enacted in the 1997 Amendments to Saskatchewan's Police Act in fact incentivized provincial leaders to direct RCMP enforcement effort to towns wherein the province received a larger share of citation revenue, relative to towns wherein the province received a smaller share of citation revenue. The findings suggest that RCMP officers devoted more effort to enforcing traffic safety in these towns, causing drivers to slow down, get into fewer accidents, and (perhaps partially confounding the province's strategy) provide fewer opportunities for RCMP officers to make traffic stops. Given that enforcement agencies have limited resources to allocate across different towns, an increase in the resources devoted to some towns naturally implies a reduction in the resources devoted to other towns, resulting in less safe conditions in those towns. This type of asymmetric enforcement incentive has the potential of distorting enforcer behavior away from the optimal course of action to one where above optimal resources are devoted to some towns and below optimal resources are devoted to others.

The hypothesized causal mechanism rests on the assumption that RCMP commanding officers, as incentivized by their provincial principals, cared about revenue extraction in their detachments. Devoting more patrol officer effort to making traffic stops in the high-revenue towns in their detachments would have been one obvious revenue-extraction strategy. There may also have been opportunities for officers to extract greater revenue per stop in the high-revenue towns. For example, conditional on having observed some form of unsafe driving behavior warranting a traffic stop, RCMP officers in the high-revenue towns presumably would have had incentives to issue a larger number of citations per stop and/or a larger fine per citation. However, detecting these efforts to extract revenue on the intensive margin (i.e., by extracting more revenue per stop) is complicated by the same deterrence mechanisms complicating our ability to detect efforts to extract revenue on the extensive margin (i.e., by making more stops). Enforcement-induced safer driving behaviors in the high-revenue towns would presumably have reduced opportunities for RCMP officers to issue a greater number of citations per stop and/or higher fines per stop (e.g., for driving at a higher speed). The net effect in the high revenue towns of greater incentives but reduced opportunities to extract revenue on the intensive margin is indeterminate.

However, there is one feature of traffic citations in Saskatchewan that may have enabled greater revenue extraction in the high-revenue towns without confounding deterrence effects. RCMP officers in Saskatchewan have the discretion to vary the amount of time that a driver has to pay their fine. Per Saskatchewan's Traffic Safety Act, a driver is to appear in court or settle their fine no earlier than one month and no later than six months after the infraction. Officers thus have the discretion to vary payment windows from one to six months from the date of an offense. RCMP officers may have used their discretion over these payment windows to extract revenue more quickly in the high-revenue towns. As the payment windows are not connected to the nature of the offenses committed by drivers, we should be able to detect shorter payment windows in the high-revenue towns even conditional on drivers committing fewer and/or less serious offenses in these towns.

Table 8 in the Supplementary Materials reports summary statistics for the average number of days a cited driver has to pay his or her fine, conditional on being stopped and cited, for both "at" and "near" locations, by town. Across both "at" and "near" locations, for towns both above and below the population threshold, drivers receive on average between 51–53 days to pay their fines; there are few differences across locations. However, these averages may mask differences near the population threshold.

Table 8 reports the RD estimates for the average number of days a cited driver has to pay his or her fine, conditional on being stopped and cited, for both "at" and "near" locations, using the fully aggregated town-level data and including all pretreatment covariates. In the estimated bandwidths below the population threshold in both "at" and "near" locations, as in the overall sample, drivers are given on average about 51–53 days to pay their fines. In the "near" locations there is no jump in the length of payment windows above the population threshold. However, in the 'at" locations there is a discontinuous jump in the average length of payment windows of approximately 14 days, a 28% increase over the length of the baseline window. Put differently, cited drivers just below the population threshold identifying the high-revenue towns were given approximately 14 fewer days, or 22% fewer days, to pay back their fines over our period of study, relative to drivers just over this threshold in the low-revenue towns.

We can also explore the economic consequences of these shorter payment windows in the high-revenue towns. The citation data from Saskatchewan's Department of Justice report not only the dollar amount of assessed fines per citation, but also identify those citations that were processed for late payment. This process is unforgiving. Once a payment due date has been missed, a late fee is assessed after 16 days. After 28 days the courts are informed that the payment has not been received. At 56 days a first notification of delinquency is sent. At 77 days a second delinquency notification is sent. At 91 days Saskatchewan Government Insurance is informed, resulting in the driver's license being suspended and the delinquent payment being sent to a collections agency. If drivers were being given less time to pay their fines in the high-revenue towns, this may have had the consequence of

Table 8: Average Days to Pay Fine, By Town
"At" and "Near", 1999-2016

	"At" Locations	"Near" Locations
Conventional	11.18***	-0.50
	(3.57)	(1.95)
Bias-corrected	14.20***	-0.94
	(3.57)	(1.95)
Robust bias-corrected	14.20***	-0.94
	(4.30)	(2.42)
N	119	119
Point/Bias Bandwidths	101/181	179/256
Towns-/Towns+	25/16	30/28
Baseline Rate –	50.71	53.01
Pretreatment Covariates	Yes	Yes

Estimates of ($T_i = 1$), or a town is above cutoff of 500 in 1996 population, and province receives only 25% of citation revenue. Baseline average rates computed within estimated bandwidths below cutoff for each model. Local linear point estimators using a triangular kernel; bias-corrected models use a quadratic bias estimator. Optimal MSE bandwidth selection. Heteroskedasticity-robust nearest neighbor standard errors (minimum of three neighbors). * p<.10, ** p<.05, *** p<.01.

leading to increased late payment of fines, and thus to an increased risk of exposure to late fees, license suspension, and further actions by a collections agency.[20]

Table 8 in the Supplementary Materials also reports summary statistics for the average dollar amount of late fines as a percentage of assessed fines, by town, for both "at" and "near" locations.[21] In both "at" and "near" locations there appear to be few differences in late fines as a percentage of assessed fines across towns below and above the population threshold.[22] However, these overall comparisons do not take into account observed or unobserved confounders.

Table 9 reports the RD estimates for the average amount of late fines as a percentage of assessed fines, by town, for both "at" and "near" locations, including all pretreatment covariates. Drivers cited within the municipal boundaries of towns in the estimated bandwidth just below the population threshold experience late fines that are on average 6 percent of their assessed fines. There is a discontinuous drop of approximately 3 percentage points in late fines as a percentage of assessed fines within the municipal boundaries of towns just above the population threshold, a relatively large effect. This finding is consistent with the

Table 9: Average Late Fines as Percent of Assessed Fines, By Town "At" and "Near", 1999-2016

	"At" Locations	"Near" Locations
Conventional	-0.03**	0.02
	(0.01)	(0.03)
Bias-corrected	-0.03**	0.01
	(0.01)	(0.03)
Robust bias-corrected	-0.03*	0.01
	(0.02)	(0.03)
N	119	119
Point/Bias Bandwidths	200/312	190/260
Towns −/Towns +	32/32	31/31
Baseline Rate −	0.06	0.15
Pretreatment Covariates	Yes	Yes

Estimates of ($T_i = 1$), or a town is above cutoff of 500 in 1996 population, and province receives only 25% of citation revenue. Baseline average rates computed within estimated bandwidths below cutoff for each model. Local linear point estimators using a triangular kernel; bias-corrected models use a quadratic bias estimator. Optimal MSE bandwidth selection. Heteroskedasticity-robust nearest neighbor standard errors (minimum of three neighbors). * p<.10, ** p<.05, *** p<.01.

finding that drivers just above the population threshold are being given approximately 28% more days to pay their fines, and is suggestive of the negative economic consequences of the use of law enforcement resources to extract revenue in the towns just below the population threshold. By contrast, drivers cited outside of but "near" the municipal boundaries of towns in the estimated bandwidth below the population threshold experience no change in late fines as a percentage of assessed fines at the population threshold.

8 Discussion

The citation revenue regime enacted in the 1997 Amendments to Saskatchewan's Police Act plausibly incentivized provincial leaders to direct RCMP enforcement effort to towns wherein the province received a larger share of citation revenue. Yet we do not observe that the RCMP in fact made more traffic stops in these towns after the law's enactment, relative to towns wherein the province received a smaller share of citation revenue. We might then naively conclude that the province did not pursue the financial opportunities posed by the law, and/or that the RCMP successfully resisted any fiscally-motivated pressure from provincial leaders.

However, the effects of fiscal incentives on traffic stops and other observable enforcement actions may be indeterminate in many contexts. Instead, researchers should arguably direct their attention to the effects of fiscal incentives on the frequency of offending. In the empirical context explored here, the citation revenue regime enacted in the 1997 Amendments to Saskatchewan's Police Act appears to have increased the frequency of accidents in the low-revenue towns just above the relevant population threshold, wherein the province had fewer revenue-induced incentives to enforce traffic safety; this effect is robust to the level of aggregation in the accident data.

We observe no discontinuities in the accident data at the population threshold during the period prior to the 1997 Amendments; at any of multiple placebo thresholds constructed on either side of the actual population threshold; or at the population threshold in the areas "near" the affected towns, within which the province received 100% of fine revenue throughout our period of interest. We also observe no discontinuities in the density of towns near the population threshold, or in pretreatment town-level census covariates. The findings are substantively large, and are robust to alternative specifications, including local randomization inference.

Moreover, we observe that the province's revenue extraction efforts appear to have contributed to negative economic consequences for cited drivers in the high-revenue towns. Drivers in high-revenue towns just below the population threshold are given approximately 22% fewer days to pay their fines, relative to drivers in low-revenue towns just above the population threshold. Drivers in high-revenue towns just below the population threshold also experience late fines as a percentage of original fines that are approximately 100% higher than the late fines as a percentage of original fines incurred by drivers in low-revenue towns just above the population threshold.

One might wonder whether the apparent greater enforcement effort devoted to the high-revenue towns was worth it to the province financially, given that the RCMP does not appear in fact to have made more traffic stops in these towns. However, the province extracted on average $700 per 100 persons per year through traffic citations in the high-revenue towns below the population threshold, over the sample period. After taking into account the 75% revenue discount, the province extracted on average only $171.50 per 100 persons per year through traffic citations in the low-revenue towns above the population threshold, over the same period.

Finally, one might have concerns about the external validity of the findings. As reported in Table 1, the 119 towns in the sample range in average population between 1999-2016 from 84 to 1906 persons. A legitimate concern is whether estimates of the effects of fiscal incentives on law enforcement in towns this small tell us anything about the effects of fiscal incentives on law enforcement more generally. It is the case, however, that of the 35,789 municipal and town governments in the United States in 2012, fully 49.5% had populations of less than 1,000 persons, and 69.6% had populations of less than 2,499 persons (U.S. Census Bureau, 2012 Census of Governments). Even if the generalizability of the findings were bounded to the empirical context of local governments of this size, these small town governments make up the lion's share of all local governments in the U.S..

Moreover, many of these smaller jurisdictions in the United States contract with larger jurisdictions for policing services. St. Louis County, for example, home to the municipality of Ferguson, Missouri, funds a county police force that generates additional revenue by contracting with more than 60 jurisdictions within the county for a variety of policing services.[23] Across the country, jurisdictions ranging from small towns[24] to university campuses[25] to private firms[26] contract with larger state and local law enforcement agencies for public safety services.

The findings reported here suggest that law enforcement agencies, including in the many small towns that make up the majority of municipal and town governments in the United States, are not immune to the fiscal incentives faced by their principals. To the extent that deploying law enforcement agencies to pursue revenue extraction undermines the pursuit of public safety goals that do not raise revenue, and/or contributes to inequitable economic consequences in the communities targeted for revenue extraction, these findings should raise concerns about the negative impact of fiscal incentives on public safety.

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Notes

[1]In a working paper, Kantor et al (2017) find that passage of the Comprehensive Crime Control Act (CCCA) of 1984, enacting federal asset forfeiture revenue sharing provisions, increased drug arrests in states where forfeiture revenue sharing was initially less generous than the CCCA's provisions.

[2]Traffic safety in the province of Saskatchewan is regulated by the provisions of the province's Traffic Safety Act. The TSA's provisions are enforced by one of three kinds of policing: 1) the province's Provincial Police Service Agreement (PPSA) with the Royal Canadian Mounted Police (RCMP); 2) a Municipality Police Service Agreement (MPSA) signed by a municipality and the RCMP; or 3) a municipal police service established and funded by a municipality.

[3]PPSAs are signed for periods of 20 years in duration. Under the Saskatchewan PPSA in effect from April 1, 1992 through March 31, 2012, the RCMP was contracted to police municipalities with populations less than 1500 in the 1991 census, and all areas outside of municipal boundaries. Towns with populations above 500 but less than 1500 in the 1991 census were initially required by the province either to establish and fund their own municipal police services, or to enter into agreements with the province through which the province would delegate to the town both financial and administrative responsibilities for the town's share of the province's PPSA costs (The Police Act 1990, effective January 1, 1992).

[4]All RCMP PPSAs are signed by the federal representative for the RCMP and the provincial or territorial minister who will be responsible for the PPSA's execution. In Saskatchewan, the Minister of Justice enters into the PPSA on behalf of the province and is responsible for its implementation. The RCMP's federal Contract Management Committee has stipulated that, with respect to all PPSAs, administrative authority over the nature and level of policing under a PPSA rests with the provinces: "The RCMP, as the PT [Provincial or Territorial] Police Service, delivers services according to the strategic policing direction of the contract jurisdiction...the RCMP assists the PTs with the administration of justice by implementing the PT policing priorities, goals and objectives as set out by the respective PT Minister" (2014 RCMP Provincial and Territorial Companion Document). Further, under all PPSAs, the RCMP provincial commanding officer "acts under the direction of the PT Minister": "The CO is required to engage and consult with the PT Minister by implementing the objectives, priorities and goals established by the PT Minister, and ensuring the deployment of personnel and Equipment reflects these priorities to the extent possible." The provincial minister participates in the selection of the CO and in the appointment of Detachment Commanders, and may request at any time that either the CO or a Detachment Commander be replaced. Finally, a PPSA may be exited by a province at any time (2014 RCMP Provincial and Territorial Companion Document).

[5]Figures 1 and 2 in the Supplementary Materials display maps of the province's RCMP detachment districts, along with the locations and revenue status of the 119 towns to which the 1997 Police Act Amendments applied.

[6]See, for example, the 2015 position statement by the Saskatchewan Urban Municipalities Association: https://suma.org/img/uploads/issues/fine_revenue_distribution_policy.pdf.

[7]Table 2 in the Supplementary Materials reports the point estimate, p value, and bandwidths from the density manipulation test.

[8]Some measures were not available for the towns below 200 in 1996 population.

[9]All RD models are estimated using the updated version of rdrobust in Stata 15.1 (Calonico et al., 2017).

[10]Insurance premia in Saskatchewan are determined exclusively by type of car and age of vehicle. Driving behavior is not a factor in determining an individual's insurance premium. Moreover, all accident costs, including damages, are covered by SGI. These institutional features reduce the likelihood of selective under-reporting of accidents.

[11]These costs include vehicle costs as well as medical costs.

[12]The stop and accident data were shared under a data use agreement with the Royal Canadian Mounted Police; I thank the RCMP for allowing me access to these data.

[13]After aggregating, 95.7% of the stop rate observations, 94.4% of the accident rate, vehicle-involved accident rate, and accident cost rate observations, and 39.4% of the accident-related injury rate observations are nonzero.

[14]As detailed in Calonico et al (2019), the formula for the estimation of MSE-optimal bandwidths when covariates are included is not the same as the formula for estimation without covariates. Bandwidths can thus differ across models estimated with and without covariates.

[15]Lack of variation in accident-related injuries prevents estimation of effects at all placebo thresholds for the injury data.

[16]Interview with RCMP Inspector David Rudderham, February 12, 2018.

[17]The province receives 100% of the fine revenue from all citations issued outside of all towns more generally, including in the "rural municipalities" that are the functional equivalent of U.S. counties, so there are no evident strategic incentives for officers to record locations as "near" towns rather than "at" rural municipalities.

[18]Because we lack information on the precise locations of stops and accidents occurring "near" towns, we also lack information on the numbers of residents of these areas. Stop and accident counts "near" towns are divided by town populations, on the theory that towns with more residents inside municipal boundaries will also have more residents just outside municipal boundaries.

[19]Local randomization inference was conducted using the rdlocrand package in Stata 15.1 (Cattaneo, Titiunik and Vazquez-Bare, 2016).

[20]The province retains 100% of the revenue from late fees in all locations, irrespective of the citation revenue regime in effect in a given location.

[21]Citations that are dismissed, withdrawn, or acquitted are coded as imposing assessed fines of zero dollars, and are therefore not included in this measure.

[22]Late fines comprise a significantly higher percentage of assessed fines in "near" locations, relative to "at" locations; this difference may be due to a higher percentage of citations in "near" locations being issued on highways to out-of-town or out-of-province drivers who may be less likely to pay fines on time.

[23] https://www.stlouiscountypolice.com/Who-We-Are/Inside-SLCPD/Police-Contract-Services.

[24] https://www.springfieldnewssun.com/news/local-govt--politics/new-carlisle-asking-residents-renew-450-000-ye

[25] https://www.opb.org/news/article/oregon-state-police-osu-contract-end.

[26] https://www.vice.com/en_us/article/d3akm7/how-facebook-bought-a-police-force.

Fiscal Incentives in Law Enforcement

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January 1, 2020

SUPPLEMENTARY MATERIALS

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Table 1: Saskatchewan Administrative Units, 1996 Census

Category of Place	**Pop Mean**	**Pop Min**	**Pop Max**	**N**
Cities	33788	2839	193647	16
Towns	987	89	4679	148
Villages	159	5	1147	317
Resort Villages	65	4	463	41
Northern Municipalities	516	40	1966	24
Rural Municipalities	662	154	7152	298
Indian Reserves	407	2	1552	96
NSAD	1405	1405	1405	1

Table 2: RD Manipulation Test

	Town Density
Robust bias-corrected	0.35
	(0.73)
Bandwidths	218/241
N-/N+	34/33
Pretreatment covariates	No
Averaged population	No
N	119

Local quadratic density estimation with local cubic bias correction; p-value in parentheses; triangular kernel; MSE-optimal bandwidth selection with jackknife standard errors; sample of 119 towns used in regression discontinuity models.

Figure 1: Saskatchewan, CA, Subsection. RCMP detachment boundaries in black; administrative boundaries in red. Green: RCMP Detachments; Black: Towns within which province receives 100% of fine revenue; Blue: Towns within which province receives 25% of fine revenue

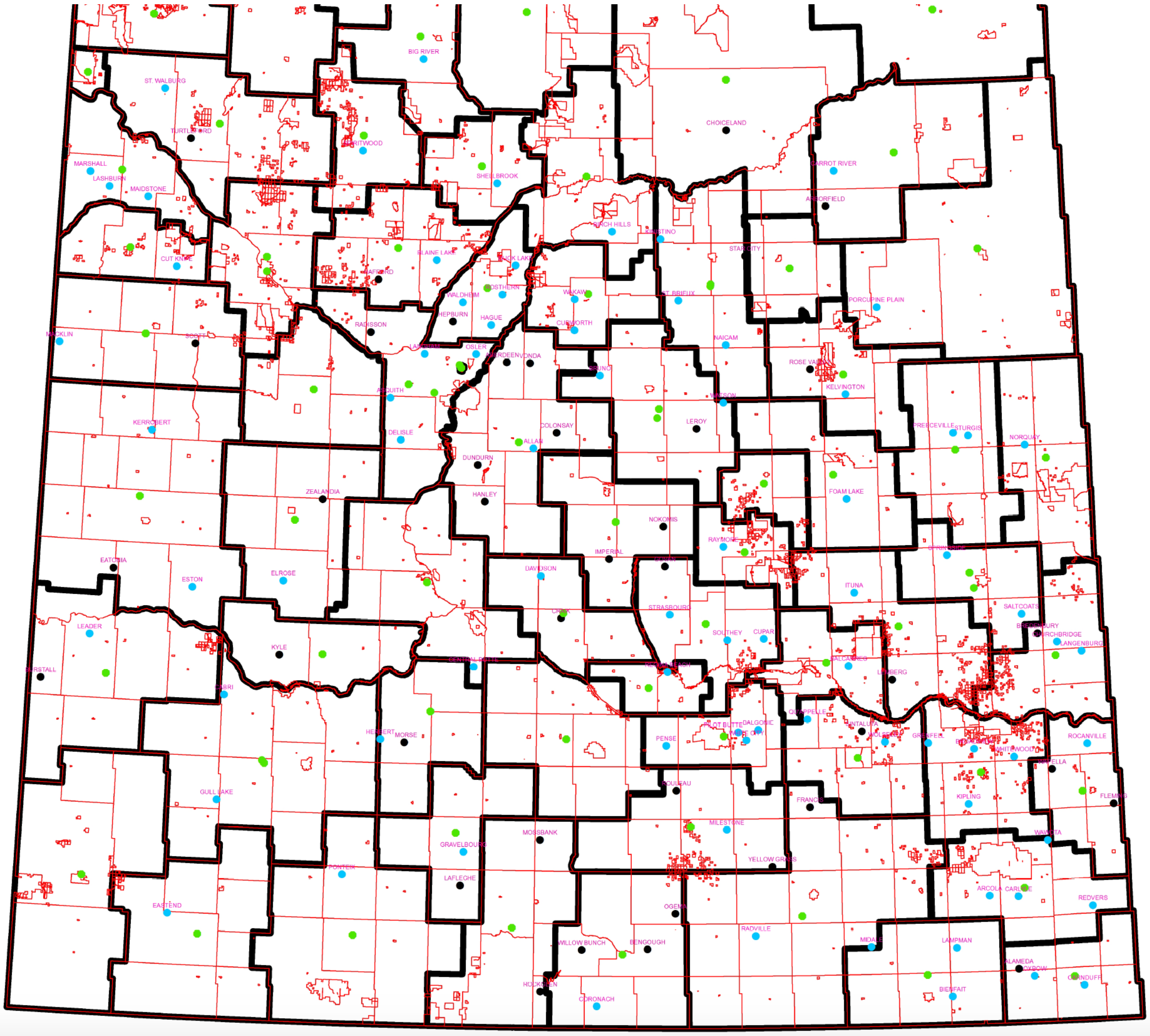

Figure 2: Saskatchewan, CA

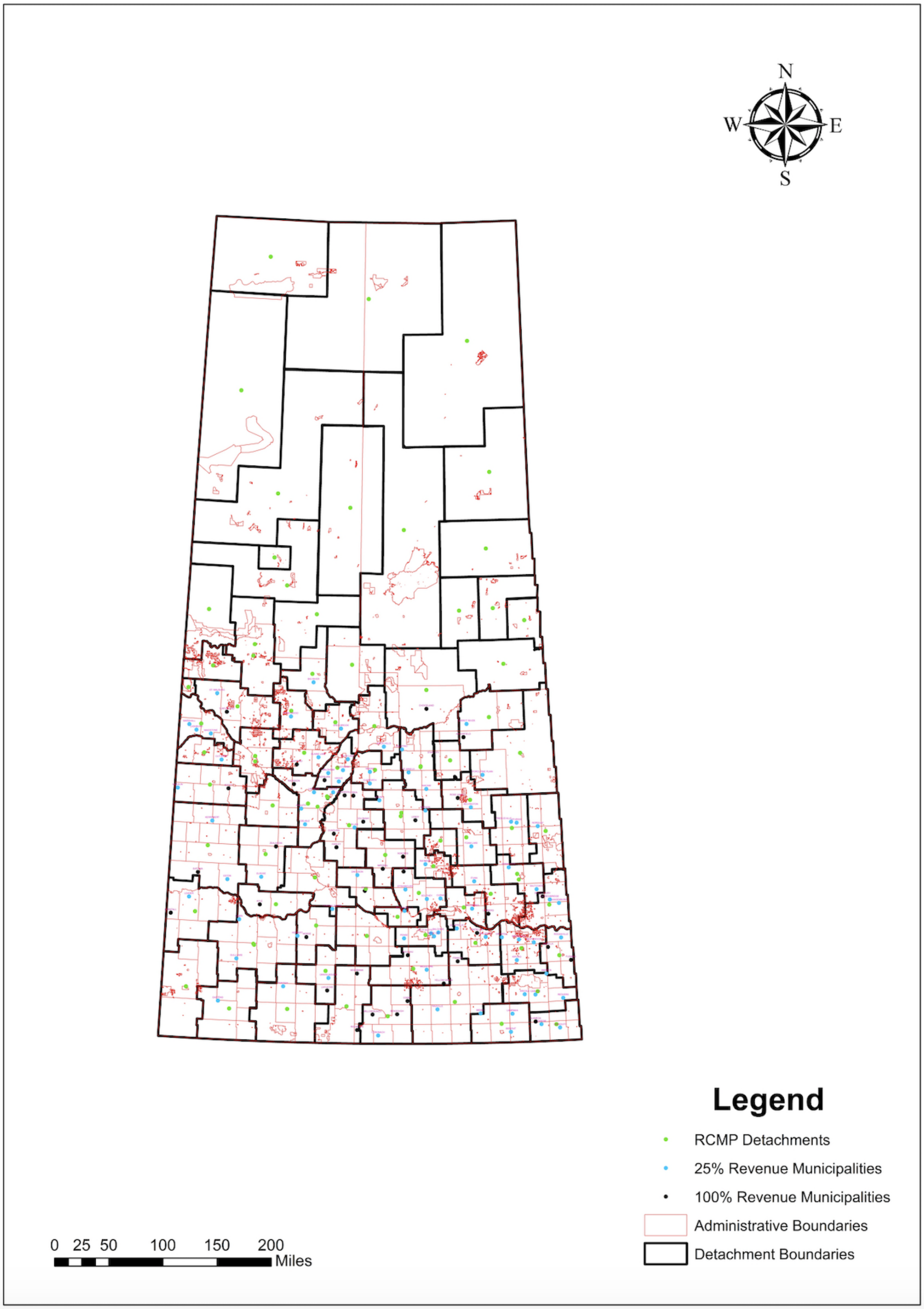

Table 3: Population Discontinuity Regressions, 1995-1998
Logged Stops and Accidents Per 100 in Population Per Town Per Year

	Log Stops/ 100 Pop/ Town/ Year	Log Accidents/ 100 Pop/ Town/ Year	Log Vehicles/ 100 Pop/ Town/ Year	Log Acc. Cost/ 100 Pop/ Town/ Year	Log Injuries/ 100 Pop/ Town/ Year
Conventional	-1.14	-0.24	-0.20	-0.78	-0.26
	(1.15)	(0.41)	(0.46)	(1.10)	(0.25)
Bias-corrected	-1.34	-0.20	-0.15	-0.65	-0.49**
	(1.15)	(0.41)	(0.46)	(1.10)	(0.25)
Robust bias-corrected	-1.34	-0.20	-0.15	-0.65	-0.49
	(1.46)	(0.54)	(0.60)	(1.46)	(0.41)
N	456	456	456	456	456
Point/Bias Bandwidths	62/88	69/97	67/94	69/98	67/113
Towns −/Towns +	12/14	14/14	13/14	14/14	13/14
Year FE	Yes	Yes	Yes	Yes	Yes
Pretreatment Covariates	Yes	Yes	Yes	Yes	Yes

Estimates of ($T_i = 1$), or a town is above cutoff of 500 in 1996 population, and province receives only 25% of citation revenue. Local linear point estimators using a triangular kernel; bias-corrected models use a quadratic bias estimator. Optimal MSE bandwidth selection. Robust standard errors clustered on town. * p<.10, ** p<.05, *** p<.01.

Table 4: Population Discontinuity Regressions, 1999-2016
Logged Stops and Accidents Per 100 in Population Per Town Per Year

	Log Stops/ 100 Pop/ Town/ Year	Log Accidents/ 100 Pop/ Town/ Year	Log Vehicles/ 100 Pop/ Town/ Year	Log Acc. Cost/ 100 Pop/ Town/ Year	Log Injuries/ 100 Pop/ Town/ Year
Conventional	-0.13	0.88***	0.97***	1.84***	0.93***
	(0.51)	(0.19)	(0.20)	(0.36)	(0.34)
Bias-corrected	-0.25	0.96***	1.06***	2.03***	1.08***
	(0.51)	(0.19)	(0.20)	(0.36)	(0.34)
Robust bias-corrected	-0.25	0.96***	1.06***	2.03***	1.08***
	(0.61)	(0.24)	(0.25)	(0.46)	(0.39)
N	2142	2142	2142	2142	2142
Towns	119	119	119	119	119
Years	18	18	18	18	18
Point/Bias Bandwidths	136/221	75/138	74/138	73/134	121/212
Towns –/Towns +	25/21	16/14	16/14	15/14	24/20
Baseline Rate –	3.40	0.74	1.22	$3,435	0.09
Year FE	No	No	No	No	No
Pretreatment Covariates	No	No	No	No	No
Conventional	-0.60	1.08***	1.20***	2.42***	1.07***
	(0.70)	(0.17)	(0.17)	(0.32)	(0.33)
Bias-corrected	-0.79	1.31***	1.44***	2.89***	1.35***
	(0.70)	(0.17)	(0.17)	(0.32)	(0.33)
Robust bias-corrected	-0.79	1.31***	1.44***	2.89***	1.35***
	(0.86)	(0.24)	(0.24)	(0.44)	(0.40)
N	2052	2052	2052	2052	2052
Towns	114	114	114	114	114
Years	18	18	18	18	18
Point/Bias Bandwidths	45/79	44/78	44/78	47/78	61/96
Towns –/Towns +	9/13	9/12	9/13	9/13	12/14
Baseline Rate –	4.01	0.78	1.30	$3,628	0.07
Year FE	Yes	Yes	Yes	Yes	Yes
Pretreatment Covariates	Yes	Yes	Yes	Yes	Yes

Estimates of ($T_i = 1$), or a town is above cutoff of 500 in 1996 population, and province receives only 25% of citation revenue. Baseline average rates computed within estimated bandwidths below cutoff for each model. Local linear point estimators using a triangular kernel; bias-corrected models use a quadratic bias estimator. Optimal MSE bandwidth selection. Robust standard errors clustered on town. * p<.10, ** p<.05, *** p<.01.

Table 5: Population Discontinuity Regressions, 1999-2016
Average Stops and Accidents Per 100 in Population Per Town

	Avg. Stops/ 100 Pop/ Town	Avg. Accidents/ 100 Pop/ Town	Avg. Vehicles/ 100 Pop/ Town	Avg. Acc. Cost/ 100 Pop/ Town	Avg. Injuries/ 100 Pop/ Town
Conventional	-21.01	5.65**	11.21***	43527.51***	2.05*
	(30.83)	(2.28)	(3.81)	(13896.28)	(1.12)
Bias-corrected	-23.57	7.38***	14.02***	53372.89***	2.58**
	(30.83)	(2.28)	(3.81)	(13896.28)	(1.12)
Robust bias-corrected	-23.57	7.38***	14.02***	53372.89***	2.58*
	(40.06)	(2.75)	(4.56)	(17037.69)	(1.47)
N	119	119	119	119	119
Point/Bias Bandwidths	195/262	129/199	121/192	128/198	132/183
Towns −/Towns +	31/32	27/20	27/20	27/20	28/21
Baseline Rate −	58.23	13.76	22.73	$67,637	1.65
Pretreatment Covariates	No	No	No	No	No
Conventional	-13.19	5.75**	11.70***	43909.72***	1.73*
	(46.12)	(2.73)	(4.44)	(15603.69)	(1.04)
Bias-corrected	-2.94	8.00***	15.55***	54581.62***	2.14**
	(46.12)	(2.73)	(4.44)	(15603.69)	(1.04)
Robust bias-corrected	-2.94	8.00**	15.55***	54581.62***	2.14
	(57.61)	(3.54)	(5.74)	(20736.14)	(1.33)
N	114	114	114	114	114
Point/Bias Bandwidths	94/134	91/126	87/121	98/140	106/139
Towns −/Towns +	23/16	23/15	22/15	25/16	25/17
Baseline Rate −	62.78	13.24	21.71	$65,806	1.55
Pretreatment Covariates	Yes	Yes	Yes	Yes	Yes

Estimates of ($T_i = 1$), or a town is above cutoff of 500 in 1996 population, and province receives only 25% of citation revenue. All estimates per 100 in town population. Baseline average rates computed within estimated bandwidths below cutoff for each model. Local linear point estimators using a triangular kernel; bias-corrected models use a quadratic bias estimator. Optimal MSE bandwidth selection. Heteroskedasticity-robust nearest neighbor standard errors (minimum of three neighbors). * p<.10, ** p<.05, *** p<.01.

Table 6: Population Discontinuity Regressions, 1999-2016
Logged Average Stops and Accidents Per 100 in Population Per Town

	Log Avg. Stops/ 100 Pop/ Town	Log Avg. Accidents/ 100 Pop/ Town	Log Avg. Vehicles/ 100 Pop/ Town	Log Avg. Acc. Cost/ 100 Pop/ Town	Log Avg. Injuries/ 100 Pop/ Town
Conventional	-0.16	0.32**	0.37***	0.51***	0.68*
	(0.47)	(0.14)	(0.13)	(0.16)	(0.37)
Bias-corrected	-0.23	0.43***	0.48***	0.64***	0.80**
	(0.47)	(0.14)	(0.13)	(0.16)	(0.37)
Robust bias-corrected	-0.23	0.43***	0.48***	0.64***	0.80*
	(0.60)	(0.16)	(0.16)	(0.19)	(0.45)
N	119	119	119	119	119
Point/Bias Bandwidths	187/257	143/235	128/200	148/251	182/282
Towns –/Towns +	31/30	28/21	27/20	29/21	30/28
Pretreatment Covariates	No	No	No	No	No
Conventional	-0.11	0.34**	0.41**	0.57***	0.58
	(0.67)	(0.17)	(0.17)	(0.20)	(0.41)
Bias-corrected	-0.02	0.46***	0.53***	0.70***	0.57
	(0.67)	(0.17)	(0.17)	(0.20)	(0.41)
Robust bias-corrected	-0.02	0.46**	0.53**	0.70**	0.57
	(0.86)	(0.23)	(0.23)	(0.27)	(0.52)
N	114	114	114	114	114
Point/Bias Bandwidths	76/115	96/136	91/132	97/140	104/147
Towns –/Towns +	20/14	24/16	23/15	24/16	25/16
Pretreatment Covariates	Yes	Yes	Yes	Yes	Yes

Estimates of ($T_i = 1$), or a town is above cutoff of 500 in 1996 population, and province receives only 25% of citation revenue. All estimates per 100 in town population. Baseline average rates computed within estimated bandwidths below cutoff for each model. Local linear point estimators using a triangular kernel; bias-corrected models use a quadratic bias estimator. Optimal MSE bandwidth selection. Heteroskedasticity-robust nearest neighbor standard errors (minimum of three neighbors). * p<.10, ** p<.05, *** p<.01.

Table 7: Summary Statistics, Stop and Accident Data
"Near" Towns by Town/Year

	Mean	SD	Min	Max	N
Stops and Accidents "Near" Towns Below Population Threshold, 1999-2016					
Stops/100 Town Population	7.78	12.68	0	121.02	702
Accidents/100 Town Population	5.98	4.57	0.54	37.09	702
Accident-Involved Vehicles/100 Town Population	6.64	5.01	0.65	38.33	702
Accident Costs/100 Town Population	$40,276	31,909	$1,632	$189,821	702
Injuries/100 Town Population	1.24	1.64	0	19.36	702
Stops and Accidents "Near" Towns Above Population Threshold, 1999-2016					
Stops/100 Town Population	4.64	7.01	0	66.47	1,440
Accidents/100 Town Population	4.36	2.99	0.29	25.07	1,440
Accident-Involved Vehicles/100 Town Population	4.88	3.31	0.29	27.31	1,440
Accident Costs/100 Town Population	$28,414	19,627	$1,458	$178,457	1,440
Injuries/100 Town Population	0.90	0.85	0	7.08	1,440

Entries reported for stops and accidents "near" 39 towns below and 80 towns above population cutoff observed over 18 years (January 1, 1999 – December 31, 2016).

Table 8: Summary Statistics, Payment Windows and Late Fines
"At" and "Near" Towns, 1999-2016

	Mean	**SD**	**Min**	**Max**	**N**
"At" Towns Below Population Threshold, 1999-2016					
Days to Pay Fine	51.42	6.52	37.41	63.34	39
Late Fines as Percent of Assessed Fines	0.06	0.04	0	0.17	39
"At" Towns Above Population Threshold, 1999-2016					
Days to Pay Fine	52.64	6.37	32.36	64.8	80
Late Fines as Percent of Assessed Fines	0.07	0.03	0.02	0.18	80
"Near" Towns Below Population Threshold, 1999-2016					
Days to Pay Fine	53.25	3.49	45.02	59.03	39
Late Fines as Percent of Assessed Fines	0.16	0.06	0.06	0.29	39
"Near" Towns Above Population Threshold, 1999-2016					
Days to Pay Fine	53.34	3.84	44.53	64.25	80
Late Fines as Percent of Assessed Fines	0.16	0.05	0.04	0.38	80

Late fines as a percentage of assessed fines are normalized by stop for those stops per town for which non-missing fine data exist.