Penalties for Speeding and their Effect on Moving Violations: Evidence from Quebec Drivers

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Abstract.
In 2008, the province of Quebec drastically increased penalties for speeding well above the speed limit by doubling fines and instituting on-the-spot licence suspension. Using administrative driving and licensing records in Quebec from 2006 to 2010, we examine whether the new law discouraged unlawful driving behaviour by investigating the frequency with which motorists received traffic citations. We find that males became less likely to get traffic tickets after the law came into effect, with the largest effects on those aged 16 to 24. The effect on the behaviour of female drivers, in contrast, was much smaller than that for males.

Keywords: driving behaviour, law enforcement, risk aversion, speeding.

Résumé.

JEL classification: K42, K49

1. Introduction

In 2018, 1,743 individuals died in Canada following a car accident (Transport Canada, 2018). Such accidents are the leading cause of death for individuals

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aged between 15 and 44 (Statistics Canada, 2020). Many OECD countries have recently introduced harsher punishments to deter the types of behaviour that increase the likelihood of such tragedies. For example, penalties for speeding well above the speed limit now typically include some combination of substantially increased fines, immediate vehicle seizure, and licence suspension. These laws are typically referred to as excessive speeding laws or stunt-driving laws.

Quebec followed this trend and introduced excessive speeding penalties in 2008. Its provisions are triggered when driving well above the speed limit. For example, driving at a speed of 100km/h in a 60km/h zone would be considered excessive speeding. These harsher punishments received widespread media coverage both before and after the change in legislation, and there was a sustained campaign by the provincial government to help ensure drivers were aware of the law. The Quebec government has since declared the legislation to be successful, with the number of excessive speeding tickets decreasing over time.[1] Furthermore, the number of accidents resulting in bodily harm decreased from 36,816 in 2006 to 32,371 in 2010, and the number of fatal accidents decreased from 666 to 441 over the same period (Gendreau, M. and Pichette, F. and Tardiff, F., 2011). Even though these findings are encouraging, they don't provide conclusive evidence that the harsher penalties truly caused changes in behaviour.

Since Becker (1968), economists have theorized that harsher punishments alter the incentives of individuals and thus ultimately their behaviour. Helland and Tabarrok (2007) provide evidence for this mechanism studying the impact of California's three-strike legislation on the recidivism rates of felons. However, the effectiveness of deterrence is unclear in the context of driving. Indeed, Bourgeon and Picard (2007) hypothesize that some drivers may be impossible to deter either because they do not care about the penalties or because they are not aware they are speeding.

A broad literature has investigated the role of deterrence on driving and particularly alcohol consumption (e.g. Hansen, 2015), but less attention has been devoted to speeding. Some empirical research has determined the impact of very influential policies like the introduction of a demerit point system. For example, Benedittini and Nicita (2009) show a reduction in road fatalities through deterrence and incapacitation following the introduction of such a system. More generally, Castillo-Manzano and Castro-Nuño (2012) provide a meta-study demonstrating the broad positive impact of such a policy in a variety of countries. In Quebec, Dionne et al. (2011) focus on the threat of the loss of licence on the behaviour of a driver close to the demerit point threshold of suspension. They find a reduction in the probability of violation for drivers with a large number of demerit points and conclude the system is successful

1 The legislation has been unsuccessfully challenged in court.

in deterring the worst offenders. Finally, closest to this paper, Meirambayeva et al. (2014) show the introduction of a street-racing law in Ontario decreased the number of accidents by conducting an intervention analysis with an ARIMA model of the monthly number of accidents in Ontario. Using monthly, aggregated data, they find an intervention effect for young males but not for females or mature males.

In this paper, we investigate the effect of Quebec's excessive speeding legislation on the frequency and types of violations incurred by Quebec drivers using an event-study design. Such violations are a proxy for driving behaviour, so this study will glean insight into the effect of increasing penalties on dangerous driving. It is important to note that we are looking at all violations which result in demerit points, and not just those that are affected by the change in the law. In contrast to Meirambayeva et al. (2014), our focus on traffic violations studies an event further up the chain of causation that happens more frequently. Although these events are still rare, we can measure gender and age differences more precisely, using a large dataset of individual drivers at the daily frequency.

We analyze driving records obtained from administrative data sets of the Government of Quebec comprising the universe of violations from 2006 to 2010 and records on drivers' licences over the same period. The use of large administrative datasets is necessary because only a small fraction of all drivers are impacted by the policy change; yet, these drivers are particularly important because they are generally responsible for accidents causing bodily harm and property damage. We then present a simple theoretical model to examine the predictions of economic theory on the effect of the law on drivers. The model predicts the possibility of heterogeneous effects by age and gender, which guides our empirical specification to test these predictions. We examine the heterogeneous effects of the excessive speeding law on both the extensive margin (getting a ticket) and the intensive margin (getting a more severe ticket) across gender and age.

We find that the daily probability of receiving a ticket (extensive margin) decreases after the implementation of the law. When we examine the results by age group, we find that the effects vary substantially, with young drivers between the ages of 16 and 24 being the most affected by the law, while there is little effect for drivers over the age of 45. Repeating the analysis by gender, we see that both males and females change their behaviour, but the magnitude of the effect on males is about eight times that of females. Examining the breakdown by age categories, we see that the effect gradually declines for males until age 55, while there appears to be no age effect for females.

We then investigate the effect on the intensive margin. For males, the probability of getting tickets worth only one demerit point actually increases following the new policy, while tickets for all other point values decrease. This result suggests that male drivers are still exceeding the speed limit but are driving more slowly than before the introduction of the legislation. A similar pattern exists for female drivers for one and two point violations. We conclude that Quebec's 2008 excessive speeding law has had substantial spillover effects on both the extensive and intensive margins of driving behaviour. In other words, not only has it reduced the number of drivers driving well above the speed limit, it has also led to a decrease in the propensity to commit other moving violations.

This paper contributes to the literature in several ways. It is the first examination using administrative data into the effect of an excessive speeding law on driving behaviour as proxied by violations by gender and age group. Such analysis is important because most countries currently use demerit point systems. The question now is not whether these systems work but whether and how they can be adjusted to increase road safety. Moreover, this paper is to our knowledge the first one to empirically investigate the impact of such laws on both the intensive and extensive margins of speeding. Finally, by studying the impact of deterrence by gender and age, this paper fills a gap acknowledged by Freeman (1999) on the role of gender in studies surrounding criminality.

The rest of this article is organized as follows. Section 2 covers the details of Quebec's excessive speeding law and the relevant institutional background. The data and summary statistics are presented in Section 3. We construct a simple theoretical model investigating the effects of the law that forms the basis of our empirical specification in Section 4. In Section 5, we conduct the empirical analysis. Robustness checks and placebo regressions are conducted in Section 6. We conclude with a policy discussion in Section 7.

2. Institutional background

Vehicular conveyance in the province of Quebec is primarily overseen by a public organization known as the Société de l'assurance automobile du Québec, commonly abbreviated as SAAQ. This organization was legislated into existence in 1978 and has several mandates. First, it has a public monopoly on the portion of insurance that covers bodily injury. Second, it is responsible for enforcing two key pieces of legislation relating to driving: the Highway Safety Code and the Automobile Insurance Act.[2] Finally, it manages the driving records of Quebec drivers, including the demerit point system, and the organization promotes road safety through awareness campaigns.

The demerit point system generally operates along the following lines. If a driver is caught committing a violation, the police officer gives the person a ticket according to the violation in question. All violations include a fine

2 A current list of offences that result in demerit points under the Highway Safety Code can be found at the following web address: `https://saaq.gouv.qc.ca/en/drivers-licences/demerit-points/offences-and-demerit-points/` (Accessed May 29, 2020).

	First offence	Second offence	Third offence	Subsequent offences
Licence suspension	7 days	30 days	60 days if all three offences were committed in a zone of 60km/h or less, otherwise 30 days	60 days if this offence and at least two others were committed in a zone of 60km/h or less, otherwise 30 days
Vehicle seizure	none	30 days if both offences committed in a zone of 60km/h or less	30 days if this offence and at least one other were committed in a zone of 60km/h or less	30 days if this offence and at least one other were committed in a zone of 60km/h or less
Fines	doubled	doubled	doubled	doubled
Demerit Points	doubled	doubled	doubled	tripled

TABLE 1
Penalties for Excessive Speeding

and a number of demerit points. Drivers can either admit guilt by paying the ticket or challenge the sanction in court. The violation is recorded in the driver's file when the guilty plea is received or when the judge convicts the driver. The points are added to the driver's file when the violation is recorded and remain there for a period of 24 months. If drivers accumulate points beyond a particular threshold, they lose their licence for a period of time after which they can reapply for one.[3] They will only receive a new licence if they successfully complete the theoretical and practical driving examinations.

Quebec's excessive speeding law came into force on April 1, 2008 and changed the demerit point system managed by the SAAQ.[4] This change was advertised by the SAAQ both before and after the law came into effect. Excessive speeding is defined by the law as exceeding the speed limit by 40

3 The threshold depends on the driver's age and type of driver's licence (e.g., learner's permit) and the term of the licence suspension increases every time drivers lose their licence.

4 On September 30, 2007, the Ontario government introduced legislation against street racing. If drivers decided to go to Quebec to engage in street racing to avoid this law, these tickets would not be in this database, because these drivers would not have a Quebec driver's licence.

km/h in a zone of 60 km/h or less, by 50 km/h in a zone between 60 to 90 km/h, and by 60 km/h in a zone where the speed limit is equal to or greater than 100 km/h. The law worked in tandem with the then currently legislated speeding violations, increasing fines and demerit point penalties and imposing licence suspensions and vehicle seizures. Although offences involving demerit points remain on a person's driving record for two years, excessive speeding convictions remain on a person's driving record for 10 years. Table 1 details the penalties for violating the excessive speeding law. Note that the licence suspension and vehicle seizure occur immediately after being pulled over regardless of the driver's innocence or guilt, while the fines and demerit points are only entered into the record once the individual admits guilt or is later found guilty in a court of law.

3. Data

We use records of traffic violations and drivers licences obtained from SAAQ administrative data to generate a dataset containing the universe of driver-days from April 1, 2006 to March 31, 2010 for the province of Quebec.[5] Our dataset contains information on the age, gender, and details concerning traffic violations of the offender. In all, we have approximately 9.7 billion driver-day observations over the sample period. This very large sample will afford us the opportunity to examine detailed subgroups and give us the statistical power to detect effects that are small in absolute magnitude.

We begin with a graphical analysis of some select demerit point values. Here, we examine monthly ticket frequencies for given point values before and after the policy change. Unfortunately, the dataset does not distinguish directly between single and multiple violations for a single police stop. For example, a driver with two 3-point violations is recorded the same as a driver with a single 6-point violation—all we observe is that both drivers gained 6 demerit points on a given day. In some cases, however, we can deduce from the demerit point values that multiple violations had occurred. Fortunately, multiple violation stops are likely very rare in our sample.[6]

Since the demerit point values of some violations doubled after the excessive speeding law came into effect, we will compare stops associated with a certain number of points before the policy change with those associated with the

5 The dataset on driver's licences allows us to include observations that do not receive any tickets during the sample period.

6 For example, before the excessive speeding law, there were no violations worth 6 points, but the sample shows 517 stops resulting in 6 demerit points compared to 43,006 stops resulting in 5 demerit points. As another example, a single 7-point violation was present before the policy change, but none after; the number of 7-point tickets before the policy change was 8,366, and it decreased to only 24 after the policy change. There are no violations in the Highway Safety Code worth 8 or 11 demerit points at any time in our sample period, and our data shows no stops with demerit point totals of these values.

same number of points and double the number of points after the policy change. For example, a driver speeding 46km/h to 49km/h over the speed limit before the policy change would receive a 5-point ticket, but the same violation would be worth 10 points after the policy change if it qualifies for excessive speeding. Because excessive speeding doubles the point values of some speeding violations, we will need to compare the frequency of 5-point stops before the policy change to 5- or 10-point stops afterwards (as not all 5-point speeding violations may qualify as excessive speeding). Due to the aforementioned possibility of stops with multiple violations, the number of tickets with 5- or 10-point values will contain combinations of violations which will be counted in the post period that were not counted in the pre period, and so the effect of the law will be underestimated in this case, but the overall effect should be minimal.

If drivers do not adjust their behaviour, there should be about as many drivers with 5 points before the policy as there were drivers with 5 or 10 points after the policy. If drivers slow down, the number of 5-point or 10-point violations will decrease.

Taking into consideration the seasonality of speeding, we see in Figure 1 an important reduction in the number of 5- or 10-point tickets in the summer of 2008 compared to the preceding summer. Overall, there is a general downward trend in the number of tickets after the policy change compared to before the policy change, and the 5- and 10-point tickets after the change are approximately evenly split.

With 7- or 14-point stops, in Figure 2, we see a different picture: nearly all 7-point violations are worth 14 points after the policy change, while only a few 7-point violations remain. Since there is no violation worth 7 points after the policy change, all of the 7-point stops after the policy change are due to being pulled over for multiple violations totalling 7 points. Once again, we see a downward trend in the number of total violations.

Table 2 reports the number of tickets by point value, for male and female drivers, before and after the change in penalties. In the 1- and 2-point categories, the number of tickets increases for both males and females on a per driver-day basis, and generally decreases in the higher-point categories. Recall that several types of violations earn a higher number of points after the policy change; for example, the 14-point tickets are all formerly 7-point tickets.

To put these numbers in a broader context, the vast majority of the sample are non-events. Before the excessive speeding law came into effect, the average driver had a probability of 0.04% to receive a ticket on any particular day. This probability decreased by approximately 3.6% after the policy change. If we look at the demerit points per driver per day, they decreased after the policy change for males by 6% and by 1% for females. This result is particularly interesting, because excessive speeding penalties doubled the value of many

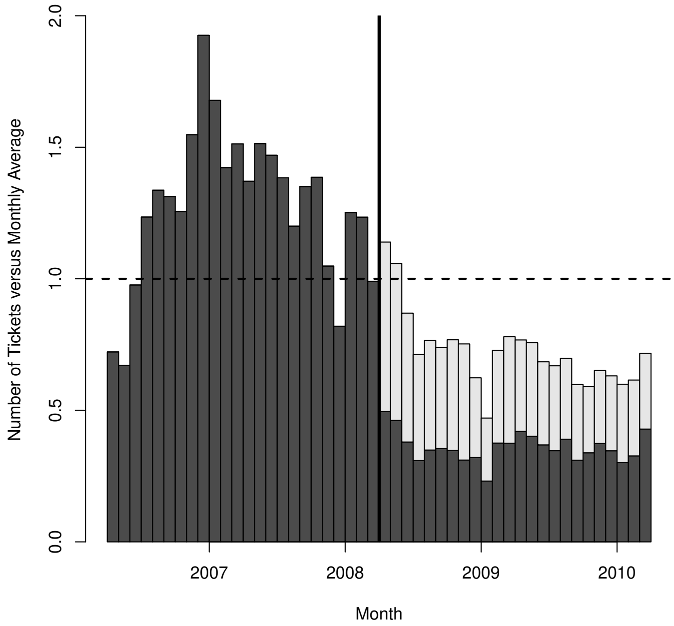

FIGURE 1 Monthly frequency of 5- and 10-point violations

Notes: Authors' calculations. Monthly frequency of 5-point violations before the policy change and 5- or 10-point violations after the policy change, divided by the average number of 5- and 10-point violations for each calendar month. The dashed line at 1.0 indicates the point at which the number of tickets is equal to the average for each month for tickets of the same point values over the entire sample. Dark grey areas correspond to 5 point-stops and light grey areas to 10-point stops.

speeding violations previously worth 5, 7, and 9 points.[7] In the absence of a change in behaviour, the number of demerit points per driver per day would have mechanically increased.

Overall, females represent half of drivers yet only 20% of all traffic tickets. The last two columns of Table 2 report the gender ratio by point value. Females claim one third of the tickets for 1 or 2 points but only a quarter of 3-point

7 Some 3-point speeding tickets are subject to the excessive speeding law, but the circumstances are quite particular: the suspect needs to be exceeding the speed limit in a zone with a posted limit of 60km/h or less by 40 to 45 km/h.

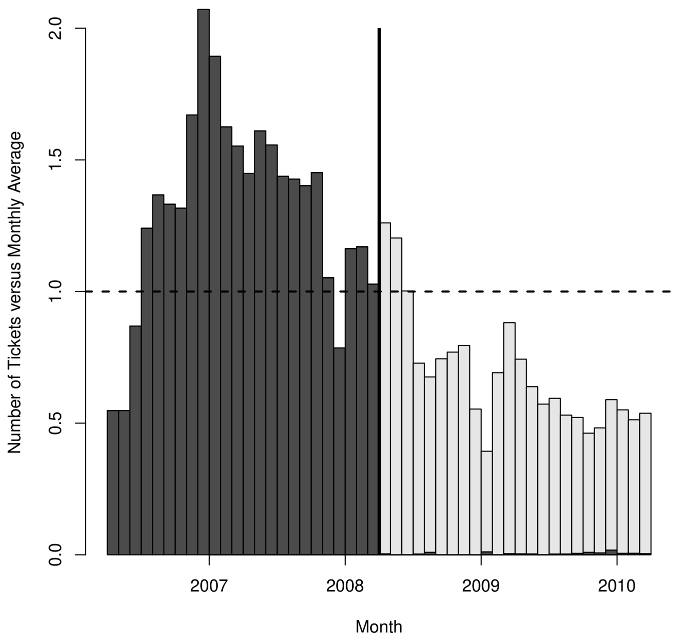

FIGURE 2 Monthly frequency of 7- and 14-point violations

Notes: Authors' calculations. Monthly frequency of 7-point violations before the policy change and 7- or 14-point violations after the policy change, divided by the average number of 7- and 14-point violations for each calendar month. The dashed line at 1.0 indicates the point at which the number of tickets is equal to the average for each month for tickets of the same point values over the entire sample. Dark grey areas correspond to 7 point-stops and light grey areas to 14-point stops.

tickets. Males account for the majority of tickets with higher point values, with the gender ratio approaching 100% male for the most severe cases of excessive speeding. It might be the case that males drive more often than females; however, if that were the only cause of the difference, one would expect the gender ratio to be constant across the different point values. The extreme gender ratio in the upper tail for excessive speeding offences suggests that males engage in risky driving behaviour more often than females.

Economists Croson and Gneezy (2009) document a large literature analyzing gender differences in preferences from many perspectives, including financial decisions, as in Charness and Gneezy (2012). This topic has a long

	Male Drivers		Female Drivers		Gender Ratio (Percent Males)	
Points	Pre	Post	Pre	Post	Pre	Post
1	101,298	122,899	45,382	61,778	69%	67%
2	533,167	572,194	249,669	283,108	68%	67%
3	701,053	627,807	247,991	239,554	74%	72%
4	15,567	15,278	2,216	2,470	88%	86%
5	43,006	12,368	8,172	2,272	84%	84%
6	496	12,000	21	3,296	96%	78%
7	7,688	18	648	6	92%	75%
9	7,382	5,791	2,587	2,431	74%	70%
10	0	12,747	0	2,137	-	86%
12	127	0	1	0	99%	-
14	0	4,145	0	302	-	93%
15	17	0	1	0	94%	-
18	3	560	0	23	100%	96%
24	0	98	0	4	-	96%
30	0	17	0	0	-	100%
36	0	4	0	0	-	100%
Total	1,409,804	1,385,926	556,688	597,381	72	70

TABLE 2
Frequency of tickets by point value
The headings “Pre” and “Post” columns refer to offences that occurred before and after the policy change. The gender ratio is measured as the percentage of the number of offences committed by males divided by the total number of offences committed by all drivers.

history in the psychological literature: Byrnes et al. (1999) reviewed over 150 papers on gender differences in risk perception. In their words, the literature “clearly” indicated that “male participants are more likely to take risks than female participants” (p. 377). Exploring factors aside from risk aversion, Powell and Ansic (1997) report that the gender difference in risk-taking is irrespective of familiarity and framing, costs or ambiguity. Harris et al. (2006) consider not only the incidence and severity of negative outcomes but also the enjoyment expected from engaging in risky activities. This partially mediated the perceived lower propensity of females toward risky choices in decisions involving gambling, recreation, and health. We explore these differences in perception to explain the differences in both the tendency for speeding and the reaction when the cost of speeding increases.

Aside from gender differences, there also exists the potential for differences by age, which is also observed in our dataset. In fact, this is also one of the main findings in Byrnes et al. (1999): there were significant shifts in the size of the gender gap between successive age levels. We explore this further with a more precise empirical specification.

4. Model

In this section, we present a simple theoretical model of driving behaviour, which guides our empirical specification. We use this to appeal to economic theory to determine whether to expect differences in age and gender as a result of a policy that increases the risk of driving, and if so, whether there would be a pattern in these differences. We later use the conclusions reached from this analysis to provide testable predictions for the empirical analysis.

Consider the utility maximization problem for the representative agent

$$u_j(s) = g(s) - r_j(s)$$

where $g(s)$ is the utility of driving at speed s and $r_j(s)$ is the disutility from the risk of driving at speed s, and j indexes males and females $\{m, f\}$; therefore, we assume the representative male and the representative female have different risk preferences (and therefore utility functions). Assume $g(s)$ is concave increasing ($g'(s) > 0, g''(s) < 0$) and $r_j(s)$ is convex increasing ($r_j'(s) > 0, r_j''(s) > 0$). Let $g(s)$ and $r_j(s)$ be continuous in the positive orthant. Impose the regularity conditions $g(s) \geq 0 \forall s$ and $r_j(s) \geq 0 \forall s$. Let there exist values of s such that $g(s) > r_j(s) > 0$; this guarantees the existence of a non-trivial equilibrium. Taking the first order condition of the objective function, the ideal speed s^* is chosen such that $g'(s^*) = r_j'(s^*)$, and this is a global maximum because $u_j''(s) = g''(s) - r_j''(s) < 0$. Plotting each curve separately on a graph, the point s^* maximizes the vertical distance between the concave and convex curves, and this occurs at the point where the slopes are equal. Let $r_m(s) < r_f(s) \forall s$; that is, the perceived risk of driving at any given speed is higher for females than it is for males, following Harris et al. (2006) and consistent with Croson and Gneezy (2009). Graphically, the risk function for the representative females will be more convex than it is for the representative male. Examining the first order conditions, we see that, on average, males will drive faster than females ($s_m^* > s_f^*$) since $u_m'(s) = g'(s) - r_m'(s) > g'(s) - r_f'(s) = u_f'(s)$.

Proposition 1. *Let $u_j(s) = g(s) - r_j(s)$ represent consumers' utility, where $g(s)$ is the utility of driving at speed s and $r_j(s)$ is the disutility from the risk of driving at speed s, both of which are continuous in the positive orthant. Assume $g(s)$ is concave increasing ($g'(s) > 0, g''(s) < 0$) and $r_j(s)$ is convex increasing ($r_j'(s) > 0, r_j''(s) > 0$), where j indexes males and females: $j \in \{m, f\}$. Suppose the risk profile increases for both males and females such that driving at speed s produces a risk of $r_j(s + \epsilon)$. Then, the decrease in driving speed for males will be greater than the decrease in driving speed for* *females.*

Proof: Let the new equilibrium point be labelled s_j^{**}. It is immediate that $s_j^* > s_j^{**}$ for both $j = \{m, f\}$ by the convexity of $r_j(s)$. By the concavity of $g(s)$ and because $r_m(s) < r_f(s) \forall s, (s_m^* - s_m^{**}) - (s_f^* - s_f^{**}) > 0$. ■

Informally, the female objective function for the representative female will reach its new equilibrium speed sooner because both $g(s)$ and $r_j(s)$ are steeper when moving from the old equilibrium to the new equilibrium.

This theoretical model predicts that people who have more acute perception of the risk of a certain behaviour are less likely to be affected by additional disincentives for that behaviour. If the penalties for speeding increase, females are less likely to be affected because they perceived higher risk without the added penalties. The model can analogously be applied to age: younger people tend to be more risk-seeking (e.g. Gong and Yang, 2012), so we also predict that our empirical results will show that the effect of the law on risk taking behaviour in driving will decrease with age.

This leads us to the specify the following empirical model. We analyze the effect of the excessive speeding law on traffic tickets by means of an event study. The main regression specification is

$$\Pr\{y_{it} = 1\} = F(\beta_{0,j} + \beta_{D,j} d_t + \boldsymbol{\beta}'_{D\cdot A,j} d_t \boldsymbol{agecat}_{it} + \boldsymbol{\beta}'_{A,j} \boldsymbol{agecat}_{it} + \boldsymbol{\beta}'_{P,j} \boldsymbol{ptsgrp}_{it} + \boldsymbol{\beta}'_{C,j} \boldsymbol{calendar}_{it} + \varepsilon_{it})$$

where d_t is a dummy variable equal to 1 after the policy change and 0 before, ***agecat*** is a set of age category dummies, ***ptsgrp*** is a set of demerit point balance categories, ***calendar*** is a set of month and weekday indicator variables, and ε_{it} is the usual error term.[8] The dependent variable y_{it} is equal to 1 if individual i of gender j received a ticket on day t and 0 otherwise. The age category controls are 16 to 19, 20 to 24, 25 to 34, 35 to 44, 45 to 54, 55 to 64, and 65 and over. The demerit point balance is the sum of demerit points on a driver's record over the last two years. This variable is divided into categories of 1 to 3, 4 to 6, 7 to 9, and 10 and over.

Our coefficients of interest are the scalars $\beta_{D,j}$ and the vectors $\boldsymbol{\beta}_{D\cdot A,j}$, for $j \in \{m, f\}$. We include the vectors $\boldsymbol{\beta}_{D\cdot A,j}$ in some specifications and estimate separately by gender because the theoretical model above predicted the possibility of heterogeneous effects due to differential attitudes towards risk: females and those of higher ages are likely to be more sensitive to changes in perceived risk.

8 Note that the bolded items represent vectors rather than scalars.

	Logistic Regression					Linear Probability Model		
	Marginal Effects		Estimate	Standard	Sig.	Estimate	Standard	Sig.
	AME	MER		Error			Error	
Full sample Drivers (9,675,245,494 observations)								
Model without age-policy interaction:								
Policy	-3.7849	-17.8748	-0.0926	0.0010	**	-3.8656	0.0411	**
Model with age-policy interaction:								
Policy	-0.6709	-2.9587	-0.0435	0.0370		-1.1761	0.5700	
Age 16-19 * policy	-4.9094	-13.7384	-0.0684	0.0373		-6.2697	0.6707	**
Age 20-24 * policy	-5.3116	-14.2921	-0.0822	0.0371		-6.7723	0.6059	**
Age 25-34 * policy	-3.9081	-12.1981	-0.0834	0.0370		-5.1489	0.5805	**
Age 35-44 * policy	-1.7999	-6.0114	-0.0430	0.0371		-2.6807	0.5780	**
Age 45-54 * policy	-1.1679	-4.2241	-0.0337	0.0371		-1.9497	0.5759	*
Age 55-64 * policy	-0.6156	-2.4087	-0.0225	0.0371		-1.2160	0.5762	
Age 65+ * policy	0.7289	3.1682	0.0385	0.0372		0.3767	0.5752	

TABLE 3
Pooled Regressions for all offences, male and female drivers
For each regression, the dependent variable is an indicator that a driver has committed any offence on a particular day. All regressions contain age category and demerit point category controls, as well as monthly and weekday indicator variables. The baseline age category comprises drivers under the age of 16. The heading "Sig." is an abbreviation for statistical significance, with the symbol * denoting statistical significance at the 0.1% level and ** the 0.001% level. Marginal effects, as well as linear probability model coefficients and standard errors, are multiplied by 100,000. The linear probability model uses heteroskedasticity-robust standard errors.

5. Empirical Results

5.1. Regression results for any moving violation

For the regressions in this study, we estimate both the linear probability model and the logistic model. Due to the very large sample sizes being employed, we elect to consider only statistical significance at the 0.1% and the 0.001% levels. Where there is statistical significance at our elevated thresholds, the estimates are nearly always similar. For expositional simplicity, we will focus our interpretation on the marginal effects of the linear probability model.[9]

Since the logistic regression model allows the predicted changes in probability to depend on the values of explanatory variables, we show both average marginal effects (AME) and marginal effects for a representative driver (MER). The AME estimates were calculated by taking differences of

9 For readability, we multiply the estimated coefficients and standard errors by 100,000 for all tables of regression results for the linear probability model.

pairs of predicted probabilities (both with and without the corresponding coefficient) and taking the average over the entire sample. For the MER, we specified a male representative driver aged 20 to 24, with 6 to 10 demerit points on their record, on a Monday in July. This combination represents a typical male driver with some previous violations two months after the introduction of the policy.

We first conduct the regression using the full sample of both male and female drivers shown in Table 3. The imposition of the policy increases the daily probability of receiving a ticket by 0.00387 percentage points, which is very precisely estimated (t-statistic $= -94.054$). Once we add the age and policy interactions, the coefficient on the policy dummy is no longer significant, and heterogeneous effects by age group emerge. The significance of the age and policy interactions depends on the model specification. Most of the effect is concentrated in the 16 to 19 and 20 to 24 age groups. The magnitude declines steadily for older age groups. The effect loses its statistical significance at the 0.1% level once we reach the 55 to 64 age group. The average marginal effects are slightly lower than the coefficients from the linear probability model, although qualitatively similar. In contrast, the marginal effects are three to four times as high for representative drivers who tend to get tickets.

Although running the pooled regressions in Table 3 is inconsistent with our theoretical model and our preliminary data analysis, we present these results to highlight three issues. First, the pooled regressions measure the overall policy effect, which can be compared to other policy analyses that do not consider gender differences. Second, the misspecification induced by pooling serves as a robustness check for the interactions by age. Indeed, in the logistic model, the age-policy effect is specified as a proportional change in probability and therefore not precisely estimated. However, in the linear probability model, when it is specified as a constant effect independent of the other covariates, it is precisely estimated. Finally, while the proportional changes measured by the logistic model already account for differences by age group, these differences can only be accommodated by introducing interaction terms in the linear model.

This key difference between the two models is worth explaining in detail, as it also clarifies the calculation of the marginal effects. For brevity, let A_{it} denote the indicator for a particular age group among the categorical variables $\boldsymbol{agecat}_{it}$ and let $\boldsymbol{\beta}_j$ and $\boldsymbol{X}$ denote the remaining coefficients and explanatory variables with $\boldsymbol{X}_{it} = [\mathbf{1}, \boldsymbol{ptsgrp}_{it}, \boldsymbol{calendar}_{it}]$. The marginal effects, AME and MER, were calculated as the treatment effect following Puhani (2012): the cross difference of the observed outcome minus the cross difference of the potential non-treatment outcome. It corresponds to the incremental effect of the interaction term coefficients. In our notation, with j subscripts suppressed for the pooled regression, this treatment effect, in the AME and MER, equals

$$F(\beta_D + \beta_A + \beta_{D \cdot A} + \boldsymbol{\beta}' \boldsymbol{X}_{it}) - F(\beta_D + \beta_A + \boldsymbol{\beta}' \boldsymbol{X}_{it}).$$

	Logistic Regression					Linear Probability Model		
	Marginal Effects		Estimate	Standard Error	Sig.	Estimate	Standard Error	Sig.
	AME	MER						
Drivers in Age Group 16-19 (252,903,500 observations)								
Policy Indicator	-6.6974	-13.0280	-0.0853	0.0045	**	-6.5742	0.3539	**
Drivers in Age Group 20-24 (679,154,662 observations)								
Policy Indicator	-8.5951	-23.2737	-0.1199	0.0029	**	-8.4513	0.2059	**
Drivers in Age Group 25-34 (1,721,109,220 observations)								
Policy Indicator	-6.6050	-20.6684	-0.1268	0.0021	**	-6.5547	0.1102	**
Drivers in Age Group 35-44 (1,957,261,955 observations)								
Policy Indicator	-3.9187	-12.7882	-0.0877	0.0021	**	-3.9221	0.0956	**
Drivers in Age Group 45-54 (2,171,413,198 observations)								
Policy Indicator	-3.0705	-11.3413	-0.0837	0.0022	**	-3.0670	0.0822	**
Drivers in Age Group 55-64 (1,611,824,607 observations)								
Policy Indicator	-2.2195	-9.7673	-0.0775	0.0030	**	-2.2167	0.0843	**
Drivers in Age Group 65-199 (1,262,602,202 observations)								
Policy Indicator	-0.4329	-2.5930	-0.0232	0.0041	**	-0.4337	0.0768	**

TABLE 4
Regressions for all offences, by age group
For each regression, the dependent variable is an indicator that a driver has committed any offence on a particular day. All regressions contain age category and demerit point category controls, as well as monthly and weekday indicator variables. The baseline age category comprises drivers under the age of 16. The heading "Sig." is an abbreviation for statistical significance, with the symbol * denoting statistical significance at the 0.1% level and ** the 0.001% level. Marginal effects, as well as linear probability model coefficients and standard errors, are multiplied by 100,000. The linear probability model uses heteroskedasticity-robust standard errors.

This expression is different from the cross difference of the observed outcome in the nonlinear model:

$$F(\beta_D + \beta_A + \beta_{D\cdot A} + \boldsymbol{\beta}'\boldsymbol{X}_{it}) - F(\beta_D + \boldsymbol{\beta}'\boldsymbol{X}_{it}) - F(\beta_A + \boldsymbol{\beta}'\boldsymbol{X}_{it}) + F(\boldsymbol{\beta}'\boldsymbol{X}_{it}).$$

To obtain the treatment effect, we subtract the cross difference of the potential non-treatment outcome:

$$F(\beta_D + \beta_A + \boldsymbol{\beta}'\boldsymbol{X}_{it}) - F(\beta_D + \boldsymbol{\beta}'\boldsymbol{X}_{it}) - F(\beta_A + \boldsymbol{\beta}'\boldsymbol{X}_{it}) + F(\boldsymbol{\beta}'\boldsymbol{X}_{it}),$$

which is the cross difference in the nonlinear model without an interaction term; it is nonzero in general, but is zero in the linear model. This point is raised by Ai and Norton (2003), who caution that the interaction effect in a logistic model is not correctly characterized by the sign, magnitude, or statistical significance of the coefficient on the interaction term. Intuitively, in the logistic regression model, the policy effect β_D already has a different proportional effect on the predicted probability, proportional to the $\boldsymbol{\beta}_A$ coefficients, even when the interaction term $\beta_{D \cdot A}$ is zero. For drivers in the respective age groups, the coefficients in $\boldsymbol{\beta}_{D \cdot A}$ measure the effect of the policy *in excess of* the proportional effect of β_D in the logistic regression model. In sum, there is no reason to believe the coefficients $\boldsymbol{\beta}_{D \cdot A}$ should match in statistical significance between the models.

To provide further evidence of the concordance between the models, we conduct separate regressions by age group. These estimates are presented in Table 4. The overall policy effect of approximately -3.8 per 100,000 driver days (see table 3) closely matches the coefficients for the drivers aged 35 to 44. In absolute terms, the coefficients are highest for the 20 to 24 age group and then decrease for older age groups, thus matching the pattern in Table 3. These results are consistent with our theoretical model in Section 4, since younger drivers engage in more risky driving behaviour.

The theoretical model of Section 4 also suggests that the effects differ by gender. We thus rerun the model separating the sample by gender. The results of this analysis are displayed in Table 5. In the sample using only males, the policy increases the daily probability of receiving a ticket by 0.00597 percentage points, which is approximately 55% higher than in the pooled sample. Once we add the policy and age group interactions, the coefficient on the policy dummy again becomes insignificant. Again, we see a very distinct pattern: the effect is similar between the ages of 16 and 24, and it declines throughout the entire lifecycle, being statistically insignificant at the 0.1% level for the age 65 and over age group. The age-policy AME values from the logistic regression are qualitatively similar to the coefficients from the linear probability model for male drivers, for whom those coefficients are significant. The MER values are two or three times as large, indicating a more pronounced response from drivers who tend to get tickets.

The effect is much smaller for females: it is 13.4% of the size coefficient for male drivers. Once the age interactions are added, none of the coefficients are significant at the elevated 1% level. These findings suggest that the pooled results are driven almost entirely by males under the age of 65. The difference between the significance of the age-policy interactions for males and females reinforces the notion that the pooled regressions in Table 3 are misspecified: the age-policy coefficients were biased toward zero when pooling the sample, since the coefficients on the female age-policy interactions are not significant. More precisely, this also suggests that the age-specific policy effect is more than proportional to the age effect for the male drivers aged 20 to 34 but the

	Logistic Regression					Linear Probability Model		
	Marginal Effects		Estimate	Standard Error	Sig.	Estimate	Standard Error	Sig.
	AME	MER						
Male Drivers (5,335,033,221 observations)								
Model without age-policy interaction:								
Policy	-5.8346	-23.5011	-0.1113	0.0012	**	-5.9663	0.0628	**
Model with age-policy interaction:								
Policy	-0.3718	-1.4247	-0.0195	0.0386		-1.0915	0.7342	
Age 16-19 * policy	-10.6130	-24.0600	-0.1107	0.0389		-11.1587	0.9191	**
Age 20-24 * policy	-10.8708	-23.8645	-0.1300	0.0387	*	-11.9225	0.8017	**
Age 25-34 * policy	-7.6030	-19.9233	-0.1301	0.0387	*	-8.6158	0.7536	**
Age 35-44 * policy	-4.5014	-12.8637	-0.0891	0.0387		-5.0295	0.7484	**
Age 45-54 * policy	-3.1065	-9.5411	-0.0713	0.0387		-3.5740	0.7450	**
Age 55-64 * policy	-2.0814	-6.9077	-0.0594	0.0387		-2.5200	0.7455	*
Age 65+ * policy	0.0269	0.1009	0.0011	0.0389		-0.2808	0.7427	
Female Drivers (4,340,212,273 observations)								
Model without age-policy interaction:								
Policy	-0.7812	-4.2791	-0.0294	0.0019	**	-0.8000	0.0495	**
Model with age-policy interaction:								
Policy	-0.3697	-1.8779	-0.0760	0.1304		-0.7470	0.6348	
Age 16-19 * policy	2.5923	9.5218	0.0625	0.1307		0.7804	0.7413	
Age 20-24 * policy	1.7554	6.0629	0.0415	0.1305		-0.0442	0.6765	
Age 25-34 * policy	0.6728	2.4781	0.0200	0.1304		-0.9585	0.6483	
Age 35-44 * policy	1.6309	6.1424	0.0508	0.1304		0.0531	0.6458	
Age 45-54 * policy	1.0967	4.4729	0.0450	0.1304		-0.1831	0.6424	
Age 55-64 * policy	1.0472	4.6017	0.0587	0.1305		0.1339	0.6424	
Age 65+ * policy	1.6217	7.6916	0.1335	0.1306		0.9727	0.6416	

TABLE 5
Regressions for all offences
For each regression, the dependent variable is an indicator that a driver has committed any offence on a particular day. All regressions contain age category and demerit point category controls, as well as monthly and weekday indicator variables. The baseline age category comprises drivers under the age of 16. The heading "Sig." is an abbreviation for statistical significance, with the symbol * denoting statistical significance at the 0.1% level and ** the 0.001% level. Marginal effects, as well as linear probability model coefficients and standard errors, are multiplied by 100,000. The linear probability model uses heteroskedasticity-robust standard errors.

policy effect is roughly proportional to the age effect for drivers in other age groups.[10]

10 Again, we refer the reader to Ai and Norton (2003) for an explanation of the differences in significance in the age-policy interactions between the logistic regression model and the linear probability model.

It is important to note that the estimate of the effect of the law in the main regression can be interpreted as an average treatment effect; this treatment effect includes drivers who rarely sufficiently exceed the speed limit or otherwise break the law to be penalized with traffic tickets. Assuming these more careful drivers are not affected by the law at all and that they make up a large segment of the population, the effect of the law on the relevant subpopulation that is affected by the law may be well underestimated.[11] The MER values support this notion: these marginal effects are two to four times as large as the average marginal effects, and this suggests that the effect is, in fact, larger for drivers who tend to get tickets.

5.2. Regression results by point total

In this section, we examine the effects of Quebec's excessive speeding law by point total. We repeat the policy dummy specification in Section 5.1 but run a regression for each particular ticket point value: 1, 2, 3, 4, 5, 7, and 9 or more points. For each of these regressions, the dependent variable is equal to 1 if the driver earns a ticket of that point value on that day, and is equal to 0 otherwise. This strategy will allow us to investigate the changes in the intensive margin of demerit points given to drivers after the policy change. Individuals may substitute driving well above the speed limit with driving at lower speeds but still above the speed limit. As before, the demerit points lost after the policy change take into account the doubling of the penalty due to the excessive speeding law. For example, the 5-point category therefore includes tickets worth 5 points before the policy change and 5 or 10 points after the policy change. These effects might be slightly underestimated (that is, they may have a slight downward bias) since some ticket combinations yielding 10 points after the policy change would be captured by these regressions. However, as previously argued, these sorts of incidents are likely very rare.

We see the results of these regressions by ticket point value in Table 6. For males, we see a very minor increase in the number of tickets worth 1 point after the policy change. This increase in 1-point tickets is dwarfed by the decrease in the tickets in all of the other point categories and is alone cancelled out by the decrease in 2-point tickets. For females, a similar pattern is found in that 1- and 2-point tickets increase slightly, but this increase is more than cancelled out by the decrease in 3-point tickets. There is a decrease in 4-point tickets, but it is not precisely estimated. All ticket values of 5 or more points decrease

11 Whether to interpret these estimates as average treatment effects is a question that has not yet been broached in the literature. We briefly consider this issue here. Since the entire population is being treated by the policy change, one can argue that the average treatment effect (ATE) equals the average treatment effect on the treated (ATT). However, one may claim that since the law was only meant to catch people who routinely speed in the first place, this subpopulation of habitual speeders make up the treatment group and thus the average effect on them would be the ATT, while the ATE refers to the average effect on the whole population.

	Logistic Regression					Linear Probability Model		
	Marginal Effects		Estimate	Standard	Sig.	Estimate	Standard	Sig.
	AME	MER		Error			Error	
Male Drivers (5,335,033,221 observations)								
All point values	-5.8346	-23.5011	-0.1113	0.0012	**	-5.9663	0.0628	**
1 point	0.3993	1.1872	0.0953	0.0043	**	0.3930	0.0177	**
2 points	-0.3960	-1.3014	-0.0191	0.0019	**	-0.4315	0.0394	**
3 points	-4.7086	-21.2669	-0.1872	0.0017	**	-4.7786	0.0436	**
4 points	-0.0725	-0.5024	-0.1252	0.0114	**	-0.0804	0.0066	**
5 points	-0.8123	-6.5090	-0.6470	0.0080	**	-0.8189	0.0100	**
7 points	-0.1607	-1.4815	-0.7392	0.0193	**	-0.1625	0.0042	**
9 or more points	-0.0657	-0.2363	-0.2501	0.0170	**	-0.0675	0.0045	**
Female Drivers (4,340,212,273 observations)								
All point values	-0.7812	-4.2791	-0.0294	0.0019	**	-0.8000	0.0495	**
1 point	0.5197	2.3386	0.2124	0.0062	**	0.5174	0.0150	**
2 points	0.3712	1.7956	0.0303	0.0028	**	0.3613	0.0336	**
3 points	-1.4226	-8.8404	-0.1256	0.0029	**	-1.4289	0.0323	**
4 points	-0.0011	-0.0093	-0.0098	0.0293		-0.0010	0.0032	
5 points	-0.2126	-3.1046	-0.7494	0.0187	**	-0.2105	0.0053	**
7 points	-0.0195	-0.5213	-0.9113	0.0695	**	-0.0191	0.0015	**
9 or more points	-0.0180	-0.0516	-0.1541	0.0282	**	-0.0180	0.0033	**

TABLE 6
Regressions by ticket-point value
The dependent variable in each regression is equal to one if a driver receives a ticket with a particular point value (that of the first column for a particular row) on that day, and is otherwise equal to zero. The categories of tickets with 3, 5 and 7 points includes tickets with 6, 10 and 14 points after the policy change, respectively, and the category with 9 or more points includes tickets with all corresponding doubled values after the policy change. All regressions contain age category and demerit point category controls, as well as monthly and weekday indicator variables. The baseline age category comprises drivers under the age of 16. The heading "Sig." is an abbreviation for statistical significance, with the symbol * denoting statistical significance at the 0.1% level and ** the 0.001% level. Marginal effects, as well as linear probability model coefficients and standard errors, are multiplied by 100,000. The linear probability model uses heteroskedasticity-robust standard errors.

after the policy change. Note that the coefficient sizes for some of the higher ticket point categories on Table 6 are quite small. Since high ticket values are rare, any decrease in their probability will have a smaller coefficient, because it represents a change from one small number to another small one.

The AME values from the logistic regression are very similar to the coefficients from the linear probability model. The MER, however, for drivers who tend to get tickets, show an effect that is four or more times as large as that from the average across the sample. The MER values for females show reductions that are roughly in line with the AME for males, which indicates that the subset of females who tend to get tickets show a change in behaviour similar to that averaged across all males, including those who rarely get tickets.

These patterns suggest that many drivers have decreased their maximum speed after the policy change. It appears likely that many people who used to speed well above the limit have decreased their speed such that they are still exceeding the limit, but not as much as before. Since the extensive margin of tickets has decreased, many who used to speed at moderate speeds over the limit no longer exceed the speed limit.

5.3. Regression results for drivers with high point balances

It may be of interest to know how drivers who typically drive less carefully (and thus accumulate more demerit points) may have seen their point balances shift on average after the implementation of the policy. We examine the subsample of drivers who at one point in the pre-period had a point balance of between 6 and 10 demerit points using the regression specification of Section 5.1. Therefore, two categories of drivers are excluded: those whose point balance never reaches 6 (most of the sample), and those who received serious tickets and therefore whose point balance is never in this range. For example, a person who received a singular ticket for excessive speeding worth 12 demerit points will not be a part of this sample because their point balance will remain at 12 as long as the ticket is on their record, and the balance will drop down to 0 when the ticket's demerit points expire: at no point was this driver's demerit point balance between 6 and 10. We need to exclude these drivers to avoid issues associated with the drivers' licence revocation. Indeed, a revocation would necessarily lead to a reduction in the number of violations in the post-policy period, because the individual is not allowed to drive. The results of this exercise by gender are on Table 7 below.

For both males and females, the effect of the policy both in general and by ticket point value shows much larger effects in the negative direction. For example, the effect of the policy on males for 3-point tickets is five times larger in the high point group compared to the overall sample. Also, the MER for males highlight the most pronounced response to the change in excessive speeding laws; in this subsample, however, the representative drivers differ only in that they *currently* have 6 to 10 demerit points and are driving on days in which drivers usually get tickets. Even the female drivers in this group show a fairly large response, although, again, the MER figure for females is roughly in line with the AME that is averaged across all males in this subsample. Overall, the frequency of tickets decreases by a relatively large margin for this group of drivers after the policy.

	Logistic Regression					Linear Probability Model		
	Marginal Effects		Estimate	Standard Error	Sig.	Estimate	Standard Error	Sig.
	AME	MER						
Male Drivers (921,131,812 observations)								
All point values	-38.3085	-57.3556	-0.3732	0.0021	**	-38.0770	0.2114	**
1 point	-0.5567	-0.6172	-0.0735	0.0076	**	-0.5454	0.0572	**
2 points	-7.7110	-9.4813	-0.2111	0.0035	**	-7.7125	0.1261	**
3 points	-24.6472	-39.8692	-0.4677	0.0029	**	-24.5075	0.1520	**
4 points	-0.9036	-2.2192	-0.8975	0.0228	**	-0.8445	0.0205	**
5 points	-3.3687	-8.0148	-1.0016	0.0124	**	-3.3206	0.0393	**
7 points	-0.7491	-1.6777	-1.1495	0.0291	**	-0.7270	0.0173	**
9 or more points	-0.3658	-0.4571	-0.7647	0.0319	**	-0.3543	0.0145	**
Female Drivers (249,294,627 observations)								
All point values	-26.2094	-42.9183	-0.4252	0.0052	**	-26.0411	0.3154	**
1 point	-0.1042	-0.1669	-0.0239	0.0193		-0.0916	0.0830	
2 points	-5.9275	-8.6399	-0.2441	0.0082	**	-5.9044	0.1970	**
3 points	-17.7920	-29.9523	-0.5749	0.0075	**	-17.6976	0.2250	**
4 points	-0.2546	-0.5826	-1.2986	0.1060	**	-0.2424	0.0181	**
5 points	-1.6624	-5.2147	-1.3612	0.0425	**	-1.6387	0.0469	**
7 points	-0.2080	-0.7392	-1.6962	0.1444	**	-0.2020	0.0151	**
9 or more points	-0.2632	-0.2503	-1.1624	0.0942	**	-0.2568	0.0202	**

TABLE 7
Regressions for high-point drivers by ticket-point value
The dependent variable in each regression is equal to one if a driver receives a ticket with a particular point value (that of the first column for a particular row) on that day, and is otherwise equal to zero. The categories of tickets with 3, 5 and 7 points includes tickets with 6, 10 and 14 points after the policy change, respectively, and the category with 9 or more points includes tickets with all corresponding doubled values after the policy change. All regressions contain age category and demerit point category controls, as well as monthly and weekday indicator variables. The baseline age category comprises drivers under the age of 16. The heading "Sig." is an abbreviation for statistical significance, with the symbol * denoting statistical significance at the 0.1% level and ** the 0.001% level. Marginal effects, as well as linear probability model coefficients and standard errors, are multiplied by 100,000. The linear probability model uses heteroskedasticity-robust standard errors.

6. Concerns of Validity

6.1. Alternative explanations for the downturn in tickets

To examine the possibility that these results are driven by a secular trend, we repeat the regression specified in Section 5.1 by splitting the pre-period in half. Because of the very heterogeneous effects by gender, we perform two sets of placebo checks using the regression specification from Section 5.1, one for males and one for females. The results of this analysis are displayed in Table 8.

	Logistic Regression					Linear Probability Model		
	Marginal Effects		Estimate	Standard	Sig.	Estimate	Standard	Sig.
	AME	MER		Error			Error	
Male Drivers (2,618,869,407 observations)								
Model without age-policy interaction:								
Policy	-0.1306	-0.5478	-0.0024	0.0017		-0.2109	0.0905	
Model with age-policy interaction:								
Policy	-1.0812	-4.1848	-0.0572	0.0540		-1.8092	1.0215	
Age 16-19 * policy	-1.1446	-2.6473	-0.0106	0.0545		-2.9360	1.3097	
Age 20-24 * policy	2.0266	4.5628	0.0204	0.0542		-0.1000	1.1226	
Age 25-34 * policy	3.2514	8.7684	0.0457	0.0542		1.3441	1.0507	
Age 35-44 * policy	2.8733	8.4706	0.0496	0.0542		1.2368	1.0420	
Age 45-54 * policy	3.4577	10.9720	0.0698	0.0542		1.9795	1.0375	
Age 55-64 * policy	3.5248	12.0052	0.0879	0.0543		2.3344	1.0386	
Age 65+ * policy	3.3942	12.9623	0.1316	0.0545		2.7337	1.0342	
Female Drivers (2,109,880,955 observations)								
Model without age-policy interaction:								
Policy	-0.1543	-0.8795	-0.0059	0.0027		-0.1803	0.0706	
Model with age-policy interaction:								
Policy	0.8415	4.3695	0.1696	0.1874		0.6983	0.9249	
Age 16-19 * policy	-6.8789	-26.4519	-0.1940	0.1879		-1.1349	1.0789	
Age 20-24 * policy	-6.4219	-23.3417	-0.1686	0.1875		-0.0914	0.9821	
Age 25-34 * policy	-5.7121	-22.0027	-0.1848	0.1875		-1.0372	0.9438	
Age 35-44 * policy	-5.4912	-21.6223	-0.1970	0.1875		-1.4878	0.9396	
Age 45-54 * policy	-3.7063	-15.7414	-0.1681	0.1875		-0.8437	0.9355	
Age 55-64 * policy	-2.4244	-11.0054	-0.1496	0.1876		-0.6454	0.9358	
Age 65+ * policy	-1.0624	-5.1866	-0.1028	0.1878		-0.3173	0.9345	

TABLE 8
Placebo regressions for all offences
For each regression, the dependent variable is an indicator that a driver has committed any offence on a particular day. All regressions contain age category and demerit point category controls, as well as monthly and weekday indicator variables. The baseline age category comprises drivers under the age of 16. The heading "Sig." is an abbreviation for statistical significance, with the symbol * denoting statistical significance at the 0.1% level and ** the 0.001% level. Marginal effects, as well as linear probability model coefficients and standard errors, are multiplied by 100,000. The linear probability model uses heteroskedasticity-robust standard errors.

The regression results show no statistical evidence of pre-trends, and none of the coefficients of interest are precisely estimated. Moreover, the magnitude of the coefficients in both placebo regressions for the model without age-policy interactions are very similar and are far smaller than their counterparts in the real regressions; we argue that this is evidence of precisely estimated zeros given their magnitude and small standard errors.[12] If there were a secular trend in the pre-period driving the results of the main regressions, the male coefficient would have a substantially larger magnitude than the female coefficient, but this is not the case. In the set of regressions containing the age category dummies interacted with the policy variable, none of the interactions are statistically significant, and there is no pattern among the coefficients either. This result contrasts with the coefficients on the age interactions for males in the main regression in which we observe a clear pattern: the effect is similar from ages 16 to 25, and then slowly declines with age. Overall, we do not find any convincing evidence that the effects found in the real main regression are an artifact of something other than the excessive speeding laws.

An alternative explanation is the idea that police leniency may have changed as a result of the law; we examine this possibility here. The introduction of additional penalties for excessive speeding may motivate police officers to note tickets as lesser speeding violations. For example, excessive speeding in zones with a limit of 60km/h could be marked down to a 3-point ticket, while excessive speeding in zones with higher limits could be reduced to a 5-point ticket. Three arguments speak against this possibility. First, police officers could behave in this fashion to avoid appearing in court in case the driver contests the charges. In Quebec, however, police officers are not required to appear in traffic court.[13] Second, a police officer aiming to be lenient would reduce the speed on a ticket to a level where the excessive speeding provisions would not take effect. However, according to Table 6, the incidence of tickets for men decreases for every category above 1 point, while for women it decreases for every category above 2 points. In other words, the categories to which the tickets would be marked down (3-point or 5-point tickets) still saw decreases.[14] Finally, the overall number of tickets per driver-day still decreased (the extensive margin), and leniency against the provisions of the excessive speeding law would only affect the intensive margin of demerit points. We conclude it is very unlikely that a change in police leniency could be driving the results.

An increase in police vigilance during the implementation of the policy could also have affected the magnitude of the results. Indeed, it would have increased the number of tickets written in that period, which would result in

12 Even with the very large sample employed in this analysis, unless the effect is exactly zero in the population, a non-zero standard error and coefficient estimate will be still be produced. For a discussion on precise zeros, see Penney (2013).

13 See the following website (in French) for details: `https://educaloi.qc.ca/capsules/la-contestation-dune-contravention/` (Accessed July 18, 2020).

14 Note that there are no speeding violations worth 4 points under the Quebec highway safety code, and that 4-point tickets are much less common than 3-point or 5-point tickets; see Table 2.

	Logistic Regression					Linear Probability Model		
	Marginal Effects		Estimate	Standard Error	Sig.	Estimate	Standard Error	Sig.
	AME	MER						
Male Drivers (5,335,033,221 observations)								
Policy Indicator	-4.0366	-16.4792	-0.0762	0.0015	**	-4.1859	0.0763	**
Month 1	9.9449	38.5317	0.1483	0.0047	**	8.6823	0.2761	**
Month 2	7.2862	27.2675	0.1110	0.0046	**	6.6386	0.2726	**
Month 3	2.2160	8.3591	0.0380	0.0048	**	2.2264	0.2683	**
Month 4	-4.7201	-17.3888	-0.0965	0.0049	**	-5.0416	0.2534	**
Month 5	-4.1329	-17.4499	-0.0969	0.0052	**	-4.5641	0.2379	**
Month 6	-6.4410	-20.9716	-0.1206	0.0047	**	-6.9509	0.2708	**
Month 7	-4.2653	-14.4849	-0.0782	0.0046	**	-4.4353	0.2648	**
Month 8	-6.3291	-22.5706	-0.1320	0.0049	**	-7.3088	0.2584	**
Month 9	-4.9332	-35.9259	-0.2503	0.0071	**	-6.6876	0.1737	**
Month 10	-10.5940	-44.5275	-0.3699	0.0057	**	-15.3145	0.2167	**
Month 11	-6.2712	-23.1921	-0.1366	0.0051	**	-7.2667	0.2609	**
Month 12	-2.8571	-10.5662	-0.0551	0.0047	**	-3.1070	0.2560	**
Female Drivers (4,340,212,273 observations)								
Policy Indicator	0.8179	4.6888	0.0310	0.0022	**	0.8391	0.0611	**
Month 1	3.7539	19.1217	0.1063	0.0070	**	3.5263	0.2238	**
Month 2	2.1374	10.6644	0.0632	0.0069	**	2.2000	0.2191	**
Month 3	-0.4495	-2.3531	-0.0157	0.0074		-0.3857	0.2112	
Month 4	-3.4773	-18.6622	-0.1527	0.0078	**	-4.0417	0.1945	**
Month 5	-3.2337	-19.8371	-0.1654	0.0083	**	-3.9171	0.1824	**
Month 6	-4.5281	-19.8371	-0.1654	0.0071	**	-4.8207	0.2167	**
Month 7	-3.8277	-17.3447	-0.1390	0.0071	**	-3.9811	0.2116	**
Month 8	-4.5030	-21.4857	-0.1842	0.0074	**	-5.3036	0.2072	**
Month 9	-2.9968	-32.3390	-0.3584	0.0117	**	-5.3165	0.1302	**
Month 10	-6.0362	-37.1693	-0.5268	0.0095	**	-10.3117	0.1611	**
Month 11	-4.3594	-22.6167	-0.1978	0.0080	**	-5.2484	0.2036	**
Month 12	-2.1026	-10.5533	-0.0772	0.0072	**	-2.1935	0.2059	**

TABLE 9
Regressions with indicators for month since policy change
For each regression, the dependent variable is an indicator that a driver has committed any offence on a particular day. All regressions contain age category and demerit point category controls, as well as monthly and weekday indicator variables. The baseline age category comprises drivers under the age of 16. The heading "Sig." is an abbreviation for statistical significance, with the symbol * denoting statistical significance at the 0.1% level and ** the 0.001% level. Marginal effects, as well as linear probability model coefficients and standard errors, are multiplied by 100,000. The linear probability model uses heteroskedasticity-robust standard errors.

an underestimation of the effect. We investigated this possibility by including separate dummy variables for the first 12 months after the change in laws. These results are shown in Table 9. The variable called "Policy Indicator" equals 1 for the entire period after the law came into effect. In each of the subsequent 12 months, the effect of the policy is measured as the sum of

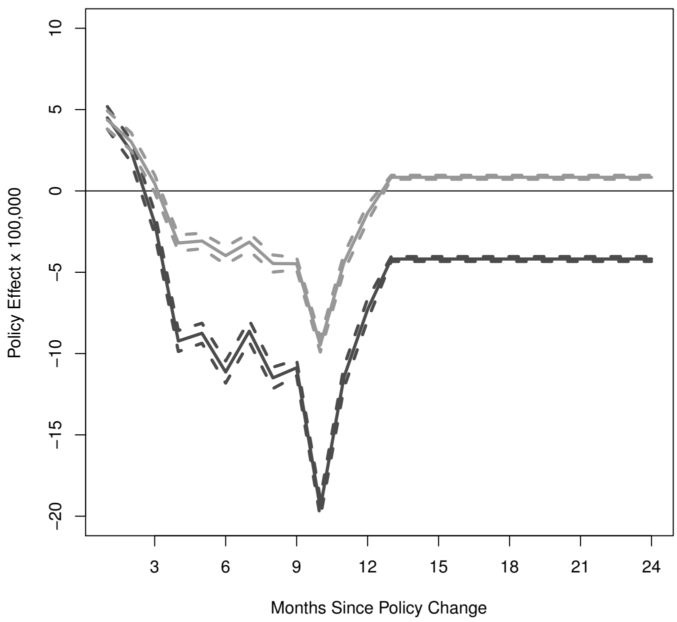

FIGURE 3 Monthly pattern of the policy effects

The points along the path reflect the net effect measured by the coefficients for the "Policy Indicator" and the corresponding "Month" indicator, shown in Table 9, multiplied by 100,000. The black lines represent the effects for males and the grey lines those for females. The dashed lines are 95% confidence bands.

the "Policy Indicator" coefficient and the coefficient for the numbered month. These monthly policy effects are plotted over the two-year period after the policy change in Figure 3. For male drivers, the probability of obtaining any ticket increased by 0.00450 and 0.00245 percentage points in the first two months of the policy but decreased by the third month. The program had maximum effectiveness over the last half of 2008, with a decrease of 0.00729 percentage points in the twelfth month, not far from the overall policy effect of 0.00597 in Table 5. For female drivers, the pattern was similar, except that the magnitude of the decline was smaller and the effect in the second year was a small increase in magnitude. Together, this suggests some combination of an increase in police vigilance and a learning curve over the course of a year.

During about the last third of the sample period after the policy change, the province of Quebec introduced a photo radar pilot. It started issuing tickets for speeding on August 19, 2009. We do not believe this had a substantial effect on driver behaviour for several reasons. First, the number of photo radar machines was very small: there were 15 in the entire province of Quebec, which had a population of approximately 8 million people at the time.[15] Second, during the pilot, the photo radar machines were placed in plain sight, and warning signs were placed ahead of them to clearly alert drivers of their location.

One last consideration is that, in April 2008, the Quebec government also introduced new legislation banning the use of handheld mobile devices while driving. This new violation associated with 3 demerit points could have increased the number of violations in the dataset. In 2008, there were 18,254 violations for this charge and 48,835 in 2009 (Tardif, 2010, table 1.3). Despite the introduction of these new violations, we still observe a decrease in the total number of violations, suggesting our results could be underestimating the real impact of the excessive speeding laws.

6.2. Statistical properties

For our empirical analysis, we estimated regressions using both the linear probability model and logistic regression; both models have limitations and we address those here.

Concerns may be raised about the mathematical properties of estimates derived from the use of linear probability models. For example, Horrace and Oaxaca (2006) claim that predicted probabilities outside of the $[0, 1]$ interval indicate bias and inconsistency of the linear probability model regression estimates. For all regressions conducted in our paper, no predicted probabilities fall outside of this interval, assuaging this concern. Furthermore, the absence of negative predictions is not a product of chance: because the explanatory variables in our regressions are all categorical variables, the predictions are essentially proportions, rather than linear predictions from continuous variables. This helps to mitigate the usual criticism of the linear probability model.

Another issue is the relative rarity of the events (the driver-days where the dependent variable is equal 1 rather than 0). King and Zeng (2001) show that rare events cause estimated probabilities to be biased downwards for logit estimation (in the case where ones are rare relative to zeros). The level of the rare events bias is a function of the frequency of events relative to the total sample size: for example, a sample size of 1,000 with 2 events (0.2% of the sample) may suffer from rare events bias, but a sample size of 100,000 with 200 events (also 0.2%) may not. To examine whether rare events bias

15 Of these 15, 6 were for speeding, 6 were for red light violations, and 3 were mobile (Bisson, 2020).

potentially exists in our analysis, we conduct a simulation as follows. We set up a simulation using an effect size that is similar to the regression involving females but uses a much smaller sample size: if rare events bias appears absent, it should not be a concern of note in the real regression which has a sample 100 times larger. The simulation has 1,000 repetitions. For each, we generate a dataset with 43,390,582 observations where 0.00369% of observations in the pre-period have an event, and 0.004449% in the post-period. The effect size of interest is the difference between these two numbers which is 0.000759%. The results are as follows. We find no statistical evidence of rare events bias: the mean effect size of the simulations is also 0.000759% and the estimates are tightly distributed, with the 25th percentile being equal to 0.000620%, and the 75th percentile to 0.000889%. Moreover, the statistical power is healthy, with 30.1% of the samples producing statistically significant results for the effect size at the 0.001% level; this is despite the simulation using a sample size only one one-hundredth that of the sample used in the analysis. We conclude that it is unlikely that rare events bias is influencing the results in a statistically meaningful fashion.

7. Policy Discussion

This paper studies the impact of an increase in penalties for speeding on the number of tickets issued. It is important to study this question to improve the demerit point system and thus promote roadway safety. Our results provide the following policy implications.

First, we find clear evidence of deterrence. Increasing the number of points associated with certain violations decreased the likelihood of tickets issued for these violations. Drivers therefore respond to an increase in the severity of penalties.

Second, young males seem to react particularly strongly to such changes in penalties. Our model explains this result by using differences in risk aversion across gender. However, the model does not distinguish between different types of punishments which usually combine a fine and a number of points: the fine represents a short-term punishment, while the points increase the likelihood of losing one's licence in the long-term. Our results could show that young males are particularly responsive to the threat of losing their licence. A policymaker who wants to target young male offenders could choose to use demerit points since it seems a particularly salient punishment for this group. This result opens the door to a literature on the role of gender in deterrence. Males and females may be deterred differently by different types of punishments. To our knowledge, this literature is still nascent.

Finally, we document a significant spillover effect from the introduction of these new penalties. Even though excessive speeding events targeted by the law are relatively rare, the law appears to have influenced many drivers. Indeed, if the only reactions were from people usually speeding well above the speed limit, we would likely not find statistically detectable effects in the overall sample, given the low share of tickets for excessive speeding (see Table 2). Overall, the legislation decreased the number of violations and caused people who habitually drive above the limit to speed less. This spillover effect is somewhat puzzling. Perhaps the excessive speeding law made people more aware of speeding penalties in general. Since drivers do not always pay close attention to their speed, the threat of more severe penalties may have increased their awareness for their speed in more mundane circumstances.

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