# *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 the new law was effective in deterring motorists from speeding. Moreover, the effect was most pronounced for males compared to females, for young compared to old, and especially so for drivers with high demerit points from past tickets compared to those with few or no tickets. In sum, the change in behaviour was most apparent for those drivers who are intended targets for the legislation.

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

*Résumé.*

JEL classification: K42, K49

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## 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 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

1 The legislation has been unsuccessfully challenged in court.

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 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. Comparing the results 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 difference in the age effect for females. Furthermore, we find that the deterrence effect increases sharply with the balance of demerit points from prior offences, but this result is most pronounced for male drivers.

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 the policy 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. More importantly, although the effect is noticed for the average drivers, who are mainly not speeding, we observe a substantial response from the drivers who get tickets—those drivers who are appropriate targets for the legislation.

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 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 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.

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).

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.

| | 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

## 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

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

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 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

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.

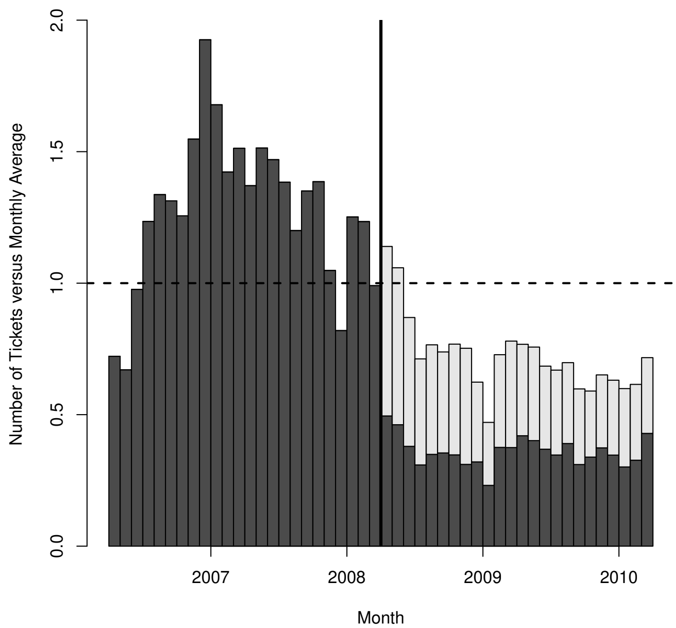


FIGURE 1 Monthly frequency of 5- and 10-point violations
Monthly frequency of 5-point violations are shown before the policy change and 5- or 10-point violations are shown after, divided by the average number of 5- and 10-point violations for each calendar month. The dashed line at 1.0 indicates the 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.

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

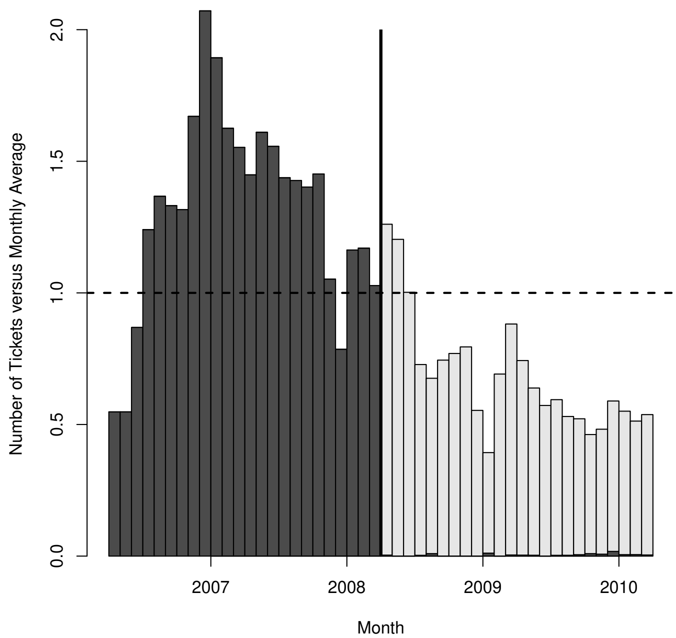


FIGURE 2 Monthly frequency of 7- and 14-point violations
Monthly frequency of 7-point violations are shown before the policy change and 7- or 14-point violations are shown after, divided by the average number of 7- and 14-point violations for each calendar month. The dashed line at 1.0 indicates the 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.

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 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

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.

| | 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.

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 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. For simplicity, we focus on the two-dimensional comparison of risk preferences by gender. We later use the conclusions reached from this analysis to provide testable predictions for the empirical analysis of the population of drivers who differ by gender, age, and their historical records of traffic offences.

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 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, $\boldsymbol{agecat}$ is a set of age category dummies, $\boldsymbol{ptsgrp}$ is a set of demerit point balance categories, $\boldsymbol{calendar}$ is a set of month and weekday indicator variables, and $\varepsilon_{it}$ 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

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

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.

## 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. This dual approach presents the effect of deterrence in terms of the both the percentage-point decrease in the probability of getting a ticket, from the linear model, and the approximately proportional change in probability, from 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). 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 simplicity, this treatment effect, in the AME and MER, equals[10]

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

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.

10 Ai and Norton (2003) 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. As a result, there is no reason to believe the coefficients $\boldsymbol{\beta}_{D\cdot A}$ should match in statistical significance between the linear and logistic regression models.

For the MER, we specified a 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 or female driver with some previous violations three months after the introduction of the policy. We use this definition of a representative driver to illustrate the effect of the policy on drivers who tend to get tickets.

The theoretical model of Section 4 suggests that the effects differ by gender. We thus fit our regression models on separate samples by gender. The results of this analysis are displayed in Table 3. 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. The lower panel for each gender shows the estimates for the model with policy and age group interactions. The benchmark age category represents new drivers, aged 14 to 16, who are the drivers without much driving history. In these regressions, the coefficient on the policy dummy is insignificant for this benchmark group. We also see a distinct pattern: the effect is similar between the ages of 16 and 24, and it declines throughout the entire life cycle, 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. In the model with age interactions for female drivers, none of the coefficients are significant at the elevated 1% level. These findings suggest that the results are driven almost entirely by males under the age of 65.

It is important to note that the estimate of the effect of the law in these regressions can be interpreted as an average treatment effect; this treatment effect includes the effect on 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

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 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 3
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.

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. The regression specification is

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

for $k = 1, 2, 3, 4, 5, 7,$ and 9 or more points. This specification excludes the age-policy interaction terms and we report only the coefficient $\beta_{D,j}$ for the policy effect in Table 4.

This strategy allows 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 4. 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 after the policy change. Note that the coefficient sizes for some of the higher ticket point categories on Table 4 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.

| | Logistic Regression | | | | | Linear Probability Model | | |
|---|---|---|---|---|---|---|---|---|
| | Marginal Effects | | Estimate | Standard Error | Sig. | Estimate | Standard Error | Sig. |
| | AME | MER | | | | | | |
| **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 4
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.

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

| | 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 5
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.

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 5.2. 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 in Table 5.

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.

### *5.4. Regression results for drivers with different demerit-point balances*

Next, we ran regressions with indicator variables for the drivers' current demerit-point balances, along with an interaction with the policy indicator. The results are depicted in Figure 3 with the male drivers' response shown with black and charcoal lines and that for females with the lighter grey lines. For males and females, the solid lines show the policy effect from a model without age-policy interactions. The dashed-and-dotted lines show the policy effect on the drivers aged 20 to 24 from a model with age-policy interactions. For the four sets of estimates, 95% confidence bands are shown in dashed lines, without age-policy interactions, and dotted lines, with age-policy interactions.

It is clear from the darker lines that the effect is strong for male drivers and this effect gets stronger for those who currently have a high balance of demerit points. In contrast, a smaller negative policy effect is measured for females, however, the the upper 95% confidence band remains close to zero. Furthermore, the age effect is more pronounced for males, with a notable increase in effect across the demerit-point balance levels. In contrast, there exists a barely perceptible difference in the age effect for females.

## 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

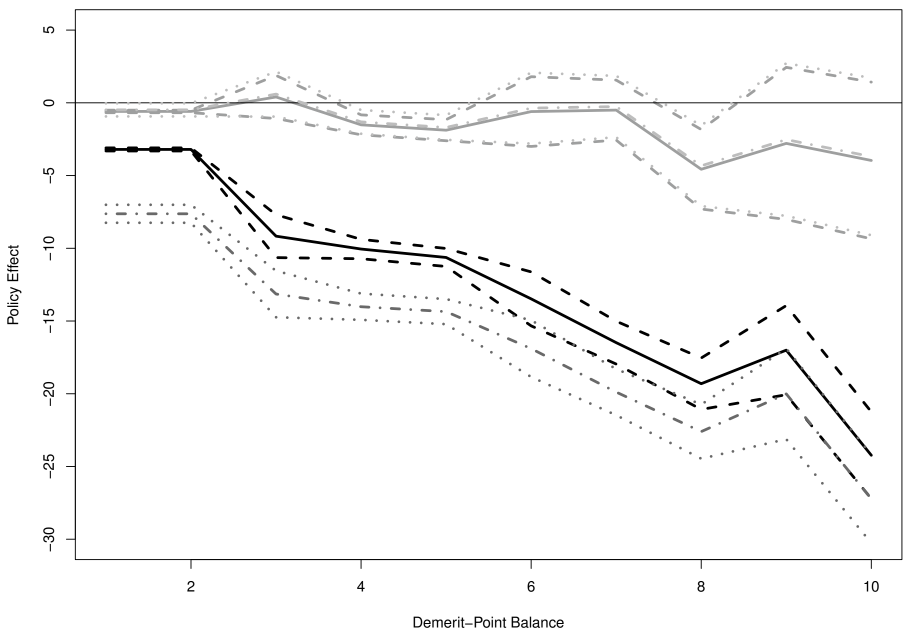


FIGURE 3 Policy change and demerit-point group interactions
The policy effects for male drivers are shown in black and charcoal and those for females are shown in grey and light grey. The darker solid lines show the overall policy effect without an age interaction, with 95% confidence intervals shown with dashed lines. The dashed-and-dotted lines in lighter shades show the policy effect for drivers aged 20 to 24 in grey for female drivers and charcoal for male drivers, with the dotted lines representing the 95% confidence interval. Estimates were obtained with the linear probability model and heteroskedasticity-robust standard errors were calculated and, in the case of the model with the age group policy interaction, using a quadratic form on the covariance matrix to account for the covariance of the 20-24 age group policy effect and the effect for the benchmark age group. Drivers with ten demerit points or more are all contained in category 10.

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 6.

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

| | Logistic Regression | | | | | Linear Probability Model | | |
|---|---|---|---|---|---|---|---|---|
| | Marginal Effects | | Estimate | Standard Error | Sig. | Estimate | Standard Error | Sig. |
| | AME | MER | | | | | | |
| **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 6
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.

zeros given their magnitude and small standard errors.[12] If there were a

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).

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 4, 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 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 7. The variable called "Policy Indicator" equals 1 for the entire period after the law came into effect. In each of the

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 7
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.

subsequent 12 months, the effect of the policy is measured as the sum of 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 4. For male drivers, the probability of obtaining any ticket increased by 0.00450 and 0.00245 percentage points in the first two

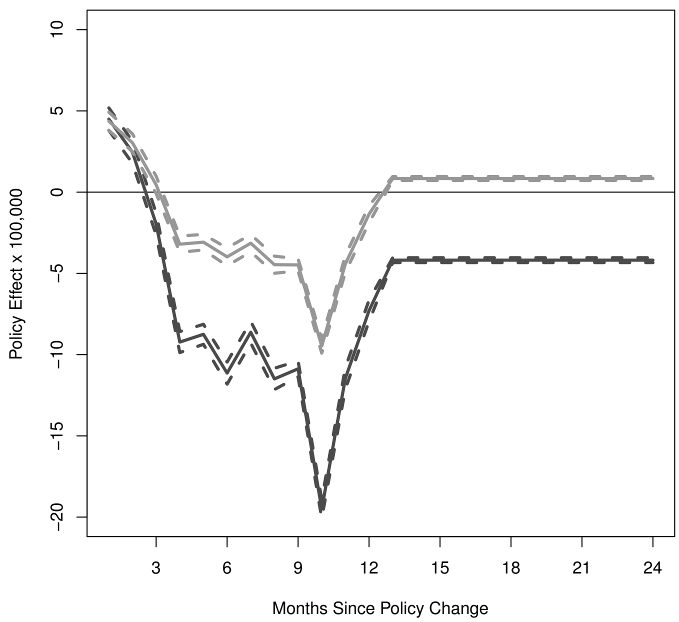


FIGURE 4 Monthly pattern of the policy effects
The points along the path reflect the net effect measured by the coefficients for the "Policy Indicator" and the corresponding "Month" indicator, shown in Table 7, multiplied by 100,000. The black lines represent the effects for males and the grey lines those for females. The dashed lines are 95% confidence bands.

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 3. 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.

We also estimated a model with driver-specific fixed effects to account for the possibility that the tendency to get tickets is not independent between drivers within the same category. To facilitate the calculation, we aggregated the data across the time dimension rather than across drivers to achieve the same degree of data compression as in the regressions reported above. We augmented these estimates with cluster-robust standard error estimates by clustering on the individual drivers. For our purposes, this model has severe limitations. First, the fixed effects annihilate the age and gender categories, which we found were strongly related to driving behaviour and worth analyzing in a model. Second, and more importantly, the remaining variation in our dataset comprises only the demerit-point balance indicators and the policy indicator. Although still of interest, including demerit-point balances is problematic because this variable is a moving average of a variable

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

closely related to the dependent variable: the number of demerit points for a ticket instead of the indicator for a ticket. Still, we found a negative policy effect that was decreasing in the current demerit-point balance; however, the estimates were orders of magnitude larger than those recorded in the models without fixed effects. This finding is explained by the bias introduced by the direct relationship between the demerit-point balance and the dependent variable, which we confirmed with simulation evidence.

Another issue is the relative rarity of the events (the driver-days where the dependent variable is equal to 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 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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| | Logistic Regression | | | | | Linear Probability Model | | |
|---|---|---|---|---|---|---|---|---|
| | Marginal Effects | | Estimate | Standard Error | Sig. | Estimate | Standard Error | Sig. |
| | AME | MER | | | | | | |
| **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 A1
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.

## Appendix A1: Empirical Results: Pooled Regressions

We first conduct the regression using the full sample of both male and female drivers shown in Table A1. 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 A1 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

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

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 $\beta_D$ 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 $\beta_D$ in the logistic regression model.

| | 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 A2
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.

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 A2. The overall policy effect of approximately -3.8 per 100,000 driver days (see table A1) 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 A1. These results are consistent with our theoretical model in Section 4, since younger drivers engage in more risky driving behaviour.

## Appendix A2: Empirical Results: Fixed Effects Regressions

Our dataset includes a small set of explanatory variables, all of which are categorical. Also, the dataset is very large: it comprises several billion driver days. On the other hand, the dataset is very sparse, which lends itself to methods of aggregation and econometric models that can be computed with frequency-weighted observations. Drivers rarely get tickets, so that most observations represent zero tickets, resulting in many thousands of equivalent observations with many similar drivers receiving no ticket on a given day. Furthermore, we observe many instances in which several drivers get tickets with the same point value on a particular day, all of whom are in a single category, e.g. all males, aged 20-24, with two demerit points on their driving record. This drives the aggregation method that we followed in the main text, retaining the weekday and month categories and aggregating over the number of drivers. In this appendix, we investigate a model with fixed effects and cluster-robust standard errors, both calculated over the individual drivers.

Estimating fixed effects for the individual drivers accounts for all the variation explained by age, sex and other unobserved heterogeneity between the drivers. The demerit point balance is the only remaining variable that changes over time for a particular driver. This categorical variable is defined as the sum of all points on tickets that a driver has received within the last two years. It is designed to reflect the demerit points that remain on a particular driver's record, which is taken into account when this balance reaches thresholds to warrant suspension or revocation of the driver's license. We calculate this balance including the two-year period before the sample start date of 01 April 2006, to ensure that the measurement is consistent across drivers and throughout the sample. Note, however, that this variable is closely related to lagged values of the dependent variable, whether a ticket occurred, so there is no reason to expect that the estimates are unbiased. The main reason to conduct this sensitivity analysis is to gauge the effect of calculating standard errors clustered on individual drivers with driver-specific fixed effects.

### ***All Drivers (with both high and low past demerit points)***

We estimated the fixed effects model with indicators for each demerit point category and the interactions of these demerit point indicators with the period after the policy change. The coefficients on the indicators for demerit point categories were largely insignificant for all samples considered in Table **??**, along with the coefficients for the interaction terms. The point estimates of these effects, however, are similar for male and female drivers, and the coefficients are largely increasing in the number of demerit points. We show only the interaction between the policy effect and demerit points in Figure A1. We plotted the sum of the coefficients for these interactions and the policy indicator to capture the net effect of the policy for each group of drivers. We also plotted 95% confidence intervals for each sum of coefficients, for which the standard errors are calculated using the standard formula to take into account the variance of both policy coefficients and the covariance of the two. The standard errors of the individual coefficients were calculated with the cluster-robust variance estimator, in which the clusters were taken to be the individual drivers.

Not much of an effect is apparent, whether for male or female drivers, with six or fewer demerit points, and the effect is only slightly stronger for drivers with between seven and ten demerit points. For drivers with more than ten demerit points, however, the relationship is much more pronounced. Drivers with ten to twenty demerit points are expected to get two fewer tickets per thousand driver days under the excessive speeding legislation. For drivers with more than thirty demerit points, this number is as high as four tickets per thousand days—slightly more than one ticket per year—after the policy change. Note that these values are much larger in magnitude than those in the text—we multiplied the coefficients by 100,000 driver days—compared to this analysis, the coefficients are on the order of ten or one hundred times as large. Although the results support the notion that drivers with high point balances were more strongly influenced by the policy, one must be cautions in interpreting these values in light of the bias induced by the fact that the demerit points are essentially a moving average of a variable closely related to the dependent variable.

### *High-Point Drivers*

We then studied the sample that is restricted to drivers with a history of getting tickets. We calculated balances of demerit points over rolling windows of two-year periods in the sample before the four-year window around the policy change. This way, we calculated the number of demerit points on each driver's record over the past two years, for all days from 01 April 2002 to 31 March 2006, which is the four-year period before the sample. Using this approach, we ensured that none of the tickets that were counted in those balances were included in the dataset for the regression model. We defined the sample of high-point drivers to include all drivers with six or more points during this pre-sample period.

Figure A2 depicts the net effect of the policy indicator and the interaction of the policy indicator with the points group indicator for drivers with high point balances. First of all, the effects are very similar for males and females, aside from a slight difference among drivers with nine demerit points. Perhaps more striking is how narrow the confidence intervals are for drivers with a past record of demerit points. The policy effect is not very strong for drivers with three points or less but this relationship declines steadily for drivers with more demerit points. As with the full sample of drivers, the relationship takes a sharp downturn for drivers with more than ten points. The level of the effect is similar to that measure on the sample of drivers with all point levels: from

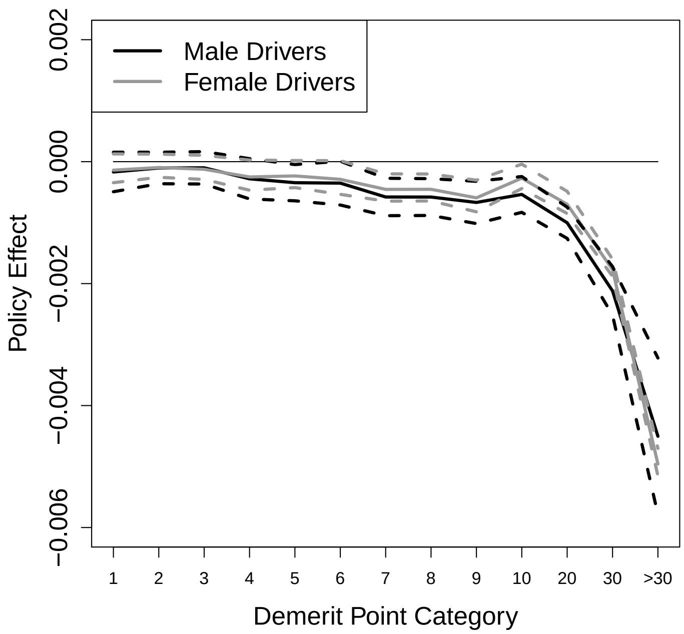


FIGURE A1 Policy change and demerit points group interactions (all drivers). Solid lines represent the policy effect for drivers for each demerit-point category, calculated as the sum of the policy indicator and the interaction of the indicators for the policy change and the demerit point category. Male drivers are represented by black lines and female drivers with grey. Dashed lines represent 95% confidence intervals, calculated using cluster-robust standard errors, clustered on individual drivers. All estimates were calculated by fitting a fixed effects regression model, with intercept coefficients for each driver.

a decrease of one ticket every three years to one ticket per year. Again, these numbers are much larger in magnitude than those we measured in the models without fixed effects and should be interpreted with caution.

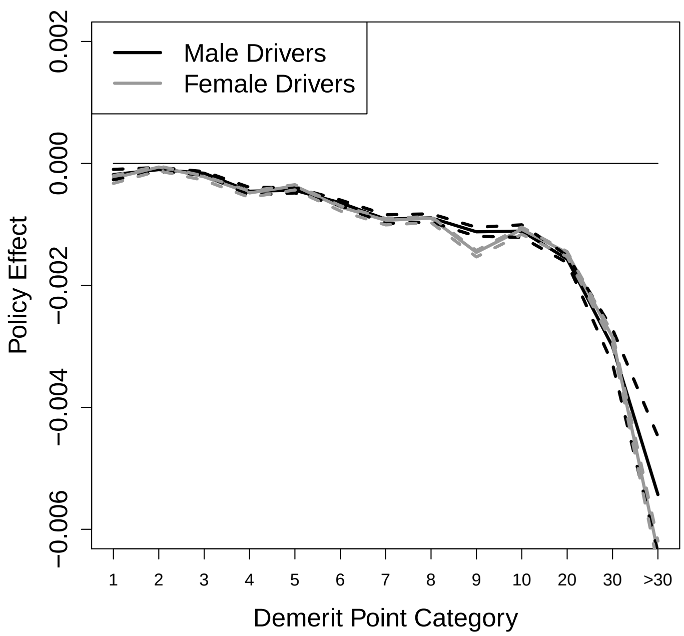


FIGURE A2 Policy change and demerit points group interactions (drivers with high demerit points). Solid lines represent the policy effect for drivers for each demerit-point category, calculated as the sum of the policy indicator and the interaction of the indicators for the policy change and the demerit point category. Male drivers are represented by black lines and female drivers with grey. Dashed lines represent 95% confidence intervals, calculated using cluster-robust standard errors, clustered on individual drivers. All estimates were calculated by fitting a fixed effects regression model, with intercept coefficients for each driver.