CIRRELT

Centre interuniversitaire de recherche
sur les réseaux d'entreprise, la logistique et le transport

Interuniversity Research Centre
on Enterprise Networks, Logistics and Transportation

Effects of Insurance Incentives on Road Safety: Evidence from a Natural Experiment in China

Georges Dionne
Ying Liu

March 2017

CIRRELT-2017-16

Bureaux de Montréal :
Université de Montréal
Pavillon André-Aisenstadt
C.P. 6128, succursale Centre-ville
Montréal (Québec)
Canada H3C 3J7
Téléphone : 514 343-7575
Télécopie : 514 343-7121

Bureaux de Québec :
Université Laval
Pavillon Palasis-Prince
2325, de la Terrasse, bureau 2642
Québec (Québec)
Canada G1V 0A6
Téléphone : 418 656-2073
Télécopie : 418 656-2624

www.cirrelt.ca

Effects of Insurance Incentives on Road Safety: Evidence from a Natural Experiment in China

Georges Dionne[1,*], Ying Liu[2]

[1] Interuniversity Research Centre on Enterprise Networks, Logistics and Transportation (CIRRELT) and Department of Finance, HEC Montréal, 3000 Côte-Sainte-Catherine, Montréal, Canada H3T 2A7

[2] School of Economics, Shandong University, No 27, Shanda South Road, Jinan, China, 250100

Abstract. We investigate the incentive effects of insurance experience rating on road safety by evaluating the claim frequency following a regulatory reform introduced in a pilot city of China. Our contribution to the growing literature on moral hazard is to offer a neat identification of a causal effect of experience rating on road safety by employing the differences-in-differences methodology in the framework of a natural experiment. The pre-treatment placebo test corroborates the assumption that the pilot city and the control city share the same pre-reform time trends in claims. We find that basing insurance pricing on traffic violations reduces claim frequency significantly. These results are robust to the inclusion of vehicle controls, alternative definitions of claim frequency, two placebo experiment tests, and several robustness checks. The effects of basing pricing on past claims are not significant.

Keywords. Insurance incentives, experience rating, road safety, natural experiment, China, traffic violation, past claim, moral hazard.

Results and views expressed in this publication are the sole responsibility of the authors and do not necessarily reflect those of CIRRELT.

Les résultats et opinions contenus dans cette publication ne reflètent pas nécessairement la position du CIRRELT et n'engagent pas sa responsabilité.

---

* Corresponding author: Georges.Dionne@cirrelt.ca

Dépôt légal – Bibliothèque et Archives nationales du Québec
Bibliothèque et Archives Canada, 2017

© Dionne, Liu and CIRRELT, 2017

1. Introduction

Traffic accidents cause serious injuries, disabilities and fatalities all over the world. It is therefore worthwhile to study what policy interventions can improve road safety, and how effective these policies are. Improvement in automobile safety equipment and highway design, strict enforcement of traffic laws and attempts to stimulate safe driving behavior via monetary or non-monetary incentives are regarded as three important channels to improve road safety (Vukina and Nestić, 2015). From the perspective of incentives, monetary mechanisms such as fines and non-monetary mechanisms such as point-record drivers' licenses have proven to be effective[1]. Experienced-rated premium based on past claims and traffic violations in multi-period insurance contracts is another form of monetary incentive, which can be justified by the potential presence of asymmetric information between insured and insurer regarding individual risks (Dionne et al., 2013b). However, the causal effect of asymmetric information on automobile accidents is far from confirmed, because appropriate data isolating the causality of the incentive effects are rare.

The main goal of this study is to fill this gap in the literature by reporting the results of a natural experiment on insurance incentives for road safety. The introduction of experience rating in a pilot city of China has the features of a natural experiment, which allows us to examine the reactions of drivers to the introduction of exogenous incentives for safe driving. We contribute to this expanding literature by investigating the impact of insurance incentives on road safety

[1] On fines, see Bar-Ilan and Sacerdote (2004) and Makowsky and Stratmann (2011). On point-record drivers' licenses, see Abay (forthcoming); Bourgeon and Picard (2007); Castillo-Manzano and Castro-Nuño (2012); De Paola et al. (2013) and Dionne et al. (2011).

with a different methodology. We also provide evidence of the presence of moral hazard in the vehicle insurance market studied.

Asymmetric information goes in two directions in insurance contracting: adverse selection and moral hazard, both of which indicate a positive correlation between accident probability and the generosity of the coverage chosen by the insured. Adverse selection means that high-risk insured choose more coverage than do low-risk insured, whereas moral hazard means that more coverage reduces the incentives for safe driving and therefore causes more accidents. The literature that tries to disentangle these two information problems dates back to Arrow (1963). In the presence of moral hazard, past claims or traffic violations pricing may help reduce future accidents (Abbring et al., 2003; Bourgeon and Picard, 2007). For adverse selection, risk classification seems more efficient (Crocker and Snow, 1985, 1986; Hoy, 1982). It is then important to empirically distinguish moral hazard from adverse selection because it can give insight into the optimal policy scheme that can reduce inefficiencies associated with asymmetric information (Weisburd, 2015).

The evidence of moral hazard in the automobile insurance market is mixed. Using cross-sectional data, Chiappori and Salanié (2000) and Dionne et al. (2001) find no evidence of asymmetric information. Chiappori and Salanié (2000; 2013) suggest that either dynamic panel data or a natural experiment[2] should be exploited to disentangle adverse selection and moral hazard. Although panel data were employed, some studies (Abbring et al., 2003; Dionne and

[2] Natural experiments where the population is randomly split into groups are valuable but scarce. If identical populations face different incentives schemes for exogenous reasons, this can be regarded as a quasi-natural experiment to test for moral hazard. In this paper, the experiment is mainly a quasi-natural experiment; the resulted change in driving behavior can be attributed to moral hazard because the population remains unchanged (Chiappori, 2000).

Ghali, 2005; Zavadil, 2015; Rowel et al., forthcoming) did not find any evidence of moral hazard while other scholars did (Dionne et al., 2011; Dionne et al., 2013a; Israel, 2007; Wang et al., 2008; Weisburd, 2015). To our knowledge, few studies utilize natural experiments related to an exogenous regulatory change because appropriate data available before and after natural experiments are scarce. There are a few exceptions, however: Dionne and Ghali (2005), Dionne et al. (2011), Lee (2013), and Li et al. (2007). Nonetheless, these studies do not meet all the criteria for strong conclusions regarding causal effects. The major problem is that these studies do not have access to a satisfactory control group, which is necessary to identify other changes that may have affected insurance incentives for road safety during the experiment.

Overcoming these limitations, we consider the introduction of an experience rating mechanism in a pilot city in China as a natural experiment, which provides us with an opportunity to use the methodology of differences-in-differences (henceforth, DID). The experiment compares the effect of the reform in the pilot city with the experience of another city unaffected by the reform to investigate the effect of insurance incentives on road safety. The paper most closely related to our contribution is Abay's (forthcoming) study, which examines the introduction of a demerit-point scheme in Denmark as a natural experiment to investigate the differential behavioral responses of the drivers in the treatment and control groups using DID. Yet because of data limitations, the research design of this study endogenously separates drivers into treatment and control groups based on their driving behavior after a common reform for the two groups, which is not the best practice for conducting a DID study.

Ashenfelter (1978) imported the DID methodology from the natural sciences to economic research. Since then, this methodology has been extensively utilized to evaluate the effectiveness of policy interventions in the economic literature. Compared with the wide applications of DID in education and health economics, public economics and other fields of economics (Bauernschuster and Schlotterm, 2015; Imbens and Wooldridge, 2009), our study is the first to analyze the impact of insurance experience rating on safe driving using an appropriate DID design. To ensure that the treatment and control groups followed the same trends in the absence of the treatment, we conducted a pre-reform placebo experiment. The results show that accidents followed the same time trend in both cities during the pre-reform years. We find that the incentive effects of the enforcement of an experience rating scheme based on traffic violations in repeated insurance contracts show a strongly significant impact on accident frequency. We conducted a series of robustness checks to confirm the validity of our empirical findings. Our results are robust to the inclusion of various available controls, alternative definitions of accident frequency, two placebo experiments, and several robustness checks. We also find that the effects of the experienced-rated premium based on past claims are not significant.

The remainder of the paper is organized as follows. Section 2 discusses the institutional background of the research setting and introduces the regulatory reform in vehicle insurance pricing. Section 3 presents the data, summary statistics, and methodology. Section 4 reviews the main empirical results, the results for the two placebo experiments and the robustness analyses. Section 5 concludes the paper.

2. Institutional background and regulatory reform

2.1 Institutional background

All vehicle insurers in China offer almost the same contract options to the market under strictly regulated pricing rules. The vehicle insurance market consists of two parts: 1) compulsory third-party liability insurance and 2) commercial insurance. This article investigates the commercial insurance part. The four main lines of commercial insurance are vehicle damage and loss insurance, third-party liability insurance, theft insurance, and driver and passenger liability insurance. As in many other countries, insurers in China use both a priori pricing and a posteriori pricing. A priori pricing is based on observable variables, whereas a posteriori pricing is based on a bonus-malus coefficient (henceforth, BMC). In a priori pricing insurers compute the base premium at the start of the contract given observables such as the age of the vehicle, the value of the vehicle, etc. The base premium should be identical across insured with the same characteristics. When contracts are renewed, premiums are revised using the BMC adjusted on past claims, which is supposed to work as an incentive scheme for safe driving.

Although the experience-rated premium has existed in the Chinese vehicle insurance market for a long time, its efficiency is questionable. Given the fierce competition among vehicle insurers and the lack of an obligation to share claims information in a common platform, insurers are not committed to enforcing the BMC because insured can easily escape the cost of their bad record by switching to another insurer without any punishment. Dionne and Ghali (2005) assessed the impact of introducing experience rating on road safety in Tunisia, and found that the effect was not statistically significant. One possible explanation for this was that the new experience rating scheme was not put into best practice because there was no sharing of information on past experience of insured between insurers, and therefore no commitment to use the potentially public information.

2.2 Regulatory reform

To enhance incentives and fairness in vehicle insurance, a pricing reform was implemented in the vehicle insurance market of Shenzhen, a city located in the province of Guangdong, China. This regulatory reform established a new vehicle insurance pricing mechanism based on past claims and traffic violations of the insured. The pricing mechanism in other markets in Guangdong remained unchanged. We consider the reform city as the treatment group. The city of Foshan, also located in Guangdong, near the pilot city, acts as the control group.

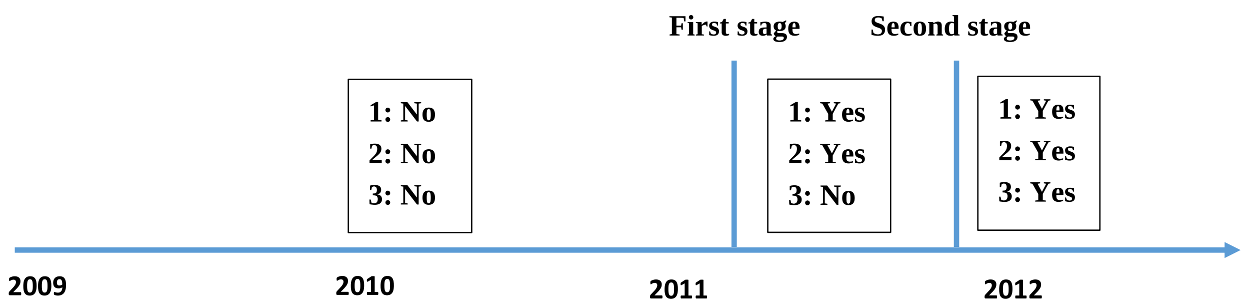

Figure 1 – Two-stage reform in the treatment city
This figure depicts the two-stage reform introduced in the treatment city. The two vertical lines indicate the start of the two reforms respectively. In the three boxes, yes or no at 1 indicates whether the BMC has switched to a bigger range; at 2, whether there is enforcement of insurers' application of the new BMC using public information on past claims; and at 3 whether the premium is also based on traffic violations with the same enforcement of insurers' use of information on violations.

Figure 1 illustrates the two-stage reform introduced in the treatment city. Stage 1 started on March 1, 2011 and premiums continued to be revised according to past claims. However, the

BMC was steeper in the treatment city than in the control city, where the BMC was not affected. Table 1 presents the BMC schedules for the treatment city and the control city respectively after the first-stage reform. Previously, the multiplicative coefficient of the base premium ranged from 0.7 to 1.3 (Panel 1b). After this first-stage reform it changed to between 0.5 and 2.0 (Panel 1a). In both cities, each new insured starts at a BMC equal to one. In the treatment city, insured who filed more than three claims during the last year get higher BMCs than in the control city, but the difference is not very important although it is more penalizing than in the control city. The higher penalizing structure is more for bad drivers who accumulate more than four claims during a given year. An insured has to cumulate more than ten accidents in the previous year to get the maximum BMC of 2.0 in the treatment city, whereas the maximum BMC in the control city is 1.3 for five claims or more: 33% of claims are lower than one thousand yuan and 63% are lower than two thousand yuan while the average value of a vehicle is 120 thousand yuan. We should mention here that many of these accidents are small. The new BMC in the treatment city is also more beneficial for insured who have no claims in previous years, but the differences between the two cities are not very large.

Table 1 – Detailed BMC based on past claims after the first-stage reform

Panel 1a – Detailed BMC in the treatment city

Level	Type of Past Claims	BMC
0	Buying commercial insurance for the first time	1.0
1	No claims for last three consecutive years	0.50
2	No claims for last two consecutive years	0.55
3	No claims for last year	0.60
4	One claim for last year	0.7
5	Two claims for last year	1.0
6	Three claims for last year	1.1
7	Four claims for last year	1.3

8	Five claims for last year	1.5
9	6-10 claims for last year	1.8
10	More than 10 claims for last year	2.0

Panel 1b – Detailed BMC in the control city

Level	Type of Past Claims	BMC
0	Buying commercial insurance for the first time	1.0
1	No claims for last three consecutive years	0.7
2	No claims for last two consecutive years	0.8
3	No claims for last year	0.9
4	Fewer than three claims for last year	1.0
5	Three claims for last year	1.1
6	Four claims for last year	1.2
7	Five claims for last year	1.3

Moreover, in the new reform, insurers in the pilot city are required by law to share the claims records of the insured through a new Vehicle Insurance Information Exchange Platform, and to use this information for insurance pricing according to the new BMC formula. This first-stage reform was introduced to improve the effectiveness of the BMC. The BMC now follows the insured in the treatment city even if the insured switches to another insurer, as in France (Dionne et al., 2013a). The new experience rating based on past claims had been enforced for all vehicle insurers in the treatment city. This may help to improve road safety, but the relative numbers in Table 1 still may not introduce the appropriate incentives because they do not differ very significantly. Specifically, the coefficients for 2 past claims and 3 past claims are the same for the treatment city and the control city, and these two levels of BMC represent a large number of insureds.

Table 2 – Detailed MC based on traffic violations in the treatment city

Level	Type of Traffic Violations	Penalty Coefficient
1	Driving on the wrong side or backwards three or more times	10%
2	Failure to observe traffic lights three or more times	10%
3	Exceeding the speed limit by more than 50% three or more times	10%
4	Driving without a license or with a revoked license	30%
5	Fleeing traffic accidents	30%
6	Drinking before driving	10%
7	Drunk driving	30%

The variation of the insurance premium in year t uses the information on traffic violations from the year $t-1$ only, along with the information on past accidents in previous years. In other words, a traffic violation is used only once and the cumulative malus coefficient for traffic violations has a maximum of 1.5. For example, if a driver has a traffic violation for drunk driving in year $t-1$, his BMC will be equal to 1.3 in year t. If in addition he drove without a licence, his BMC will be equal to the maximum 1.5 instead of 1.6.

Stage two of the reform started on Oct 15, 2011. Since that date, the pricing depends not only on past claims but also on past traffic violations of the insured. The additional multiplicative malus coefficient (MC) to the basic premium ranges from 1.0 to 1.5 depending on the seriousness of cumulative traffic violations during the previous year. Table 2 presents the coefficients related to different traffic violations. The system has no cumulative memory over time in the sense that only traffic violations committed in the previous year matter. There are 7 levels of malus coefficient for different traffic violations that insured commit. The total malus coefficient is the sum of the penalty coefficients accumulated over the previous year plus one. The total malus coefficient reaches its maximum at 1.5. During the first year of application, the individual's cumulative traffic violations were taken from the date of the second-stage reform to the start of the current insurance contract. In the subsequent insurance periods, the traffic violations over the past 365 days are used for the next year insurance pricing.

This is the first time in the Chinese vehicle insurance market that the insurance premium is legally adjusted according to the record of insureds' traffic violations. Vehicle insurers in the treatment city must use available information on past infractions to price insurance, as in the new BMC scheme. In the next sections, our analysis will be based on the experiment that the two-step new experience rating system has been put into practice in the treatment city, whereas the control city did not experience any change in the vehicle insurance pricing system during the same time period. At this point, when comparing the two-stage reform, it seems that the second-stage reform provides more incentives for road safety than the first stage. It is important to mention that the reform did not suddenly appear. From Nov 4 to Nov 25, 2010, the Insurance Association in the treatment city had consulted the public regarding the forthcoming pricing reform. Considering that the insurance premium accounts for only a small part of insureds' disposable income, we believe that the possibility of self-selection for the residence city according to the pricing reform is small.

3. Data and methodology

3.1 Data

The data include the underwriting information and at-fault claims information for the two cities of Shenzhen and Foshan obtained from one of the three largest property and liability insurers in China, whose written premiums accounted for about 19% of the total vehicle insurance market in China in 2014.

We obtained the complete set of individual vehicle policies and at-fault claims data from the company's call center. The call center manages more than half of the company's total individual policies. The data span the years 2009 to 2012. During this four-year-period, insured could join or leave the insurer freely. To address potential sample selection and attrition issues, we keep only the vehicles that stay with this insurer for three (from 2010 till 2012) and four (from 2009 till 2012) consecutive years,[3] which corresponds to about 24 percent of the whole sample. Our sample includes data on 43,500 vehicles that stay from 2010 till 2012 and 20,545 vehicles that stay from 2009 till 2012. We have a total of 212,680 observations after excluding missing values.

Each observation is a one-year vehicle insurance policy[4]. Our sample contains detailed policy underwriting information and at-fault claims records. The underwriting data are based on vehicle characteristics[5] such as cargo capacity (load), age, value, actual premium, and type of vehicle. The claims data record the claim frequency during each one-year insurance period, which represent the accident history of the insured. The claims are all based on accidents for which the insured is fully or partially responsible. Therefore, our estimation will not be biased by the claims for which the third party’s insurer is fully responsible. For the bonus malus management, the insurers treat fully and partially responsible claims in the same manner.

The definitions of all available variables for this study are presented in Table 3. Past accidents are measured by claim frequency. Three variables—Once, Twice, and Number— are employed to act as proxies for accident frequency. Table 4 shows the summary statistics for each

[3] Only four vehicles in our sample (2 in the 2010-2012 subsample and 2 in the 2009-2012 subsample) switched between the treatment and the control city within the same insurer. We delete them to avoid the possible endogeneity caused by self-selection.

[4] The insurance period of each policy is either 364 or 365 days.

[5] Unlike vehicle insurers in many countries, vehicle insurers in China do not collect drivers’ information on a regular basis, such as age, gender, and years of driving experience.

variable[6]: the mean, standard deviation, minimum value, median value, and maximum value. Note that the frequencies of at least one (Once) and at least two (Twice) accidents during the insurance period are 0.363 and 0.135 respectively, indicating the very high accident frequency in this country, similar to many Asian countries. The total average number of claims during the insurance period is 0.555[7]. 83.9% of the policies in the sample are from the treatment city. Table 4 shows that the policies after the reform constitute the majority; policies after the first-stage reform and the second-stage reform account for 87.0% and 68.7% of the total respectively. This is due to the fact that we have much fewer observations in 2009.

Table 3 – Definitions of variables

Variables	**Definitions**
Outcome variables	
Once	A dummy variable that equals 1 when the insured has filed at least one claim during the insurance period, and 0 otherwise
Twice	A dummy variable that equals 1 when the insured has filed at least two claims during the insurance period, and 0 otherwise
Number	The number of claims during the insurance period
DID variables	
Treat	A dummy variable that equals 1 when the vehicle is insured in the reform city, and 0 otherwise
$After_1$	A dummy variable that equals 1 when the vehicle is insured after the first-stage reform, and 0 otherwise
$After_2$	A dummy variable that equals 1 when the vehicle is insured after the second-stage reform, and 0 otherwise

[6] The table below reports the summary statistics of the outcome variables in the whole population compared with their counterparts in our study sample in Table 4. It shows that the characteristics of the study sample we retain for estimation are almost identical to those of the whole sample.

Outcome Variables	**Mean**	**Sd**	**Min**	**Median**	**Max**	N
Once	0.370	0.483	0	0	1	883207
Twice	0.146	0.353	0	0	1	883207
Number	0.591	0.957	0	0	25	883207

[7] The loss ratios of the study company are better than the averages of the whole Chinese vehicle insurance industry. From 2009 until 2012 the average loss ratios of the China insurance industry as a whole were 55.7%, 45.8%, 49.96% and 56.1% respectively, compared with 38.3%, 38.2%, 38.2% and 38.6% for this company.

$Reform_1$	Interaction of the two variables, Treat and $After_1$
$Reform_2$	Interaction of the two variables, Treat and $After_2$
Vehicle's characteristics	
Load	The cargo capacity of the vehicle (in tons)
Age	The age of the vehicle (in years)
Age^2	Age squared of the vehicle
Value	The value of the vehicle (in thousands of yuan)
Premium	The actual premium during the insurance period (in thousands of yuan)
Import	A dummy variable that equals 1 when the vehicle is imported, and 0 otherwise
Type1	A dummy variable that equals 1 when the vehicle is a truck (2 tons or less), and 0 otherwise
Type2	A dummy variable that equals 1 when the vehicle is a truck (2-5 tons), and 0 otherwise
Type3	A dummy variable that equals 1 when the vehicle is a regular automobile (6 passengers or less), and 0 otherwise
Type4	A dummy variable that equals 1 when the vehicle is a minibus (7-10 passengers), and 0 otherwise
Type5	A dummy variable that equals 1 when the vehicle is a minibus (11-20 passengers), and 0 otherwise

Table 4 – The Basic statistics of the variables

Variables	Mean	Sd	Min	Median	Max
Outcome variables					
Once	0.363	0.481	0	0	1
Twice	0.135	0.342	0	0	1
Number	0.555	0.887	0	0	12
DID variables					
Treat	0.839	0.368	0	1	1
$After_1$	0.870	0.336	0	1	1
$After_2$	0.687	0.464	0	1	1
$Reform_1$	0.728	0.445	0	1	1
$Reform_2$	0.580	0.494	0	1	1
Vehicle's characteristics					
Load	0.027	0.176	0	0	4.99
Age	3.859	2.303	0	3.085	20.553
Age^2	20.195	25.308	0	9.517	422.443
Value	120.457	99.681	8.12	94.32	3000
Premium	2.958	1.555	0.143	2.779	49.677

Import	0.036	0.186	0	0	1
Type1	0.029	0.166	0	0	1
Type2	0.000	0.008	0	0	1
Type3	0.851	0.356	0	1	1
Type4	0.118	0.323	0	0	1
Type5	0.002	0.049	0	0	1

Note: Number of observations: 212,680.

If we look at the variables regarding the vehicle's characteristics in Table 4, we see that the average load of the vehicle is 0.027 tons (because the cargo capacity of most regular automobiles is nil) and the average age is 3.859 years. The average value of the vehicle is 120.46 thousand yuan and the average actual premium is 2.96 thousand yuan. Only 3.6% of the vehicles are imported from other countries; the rest are Chinese domestic vehicles. Variables Type1 to Type5 describe the type of the vehicle; 85.1% are regular automobiles (with 6 or fewer passengers).

3.2 Method

To examine the impact of the pricing reform on safe driving, we can calculate the difference in the accident frequency before and after the reform in the reform city. However, some other factors, both observable and unobservable, may influence road safety over time. The existence of the control group can isolate some common economic shocks. Given that the reform city is a pilot city, it can be regarded as the treatment group; the other city in the same province is the control group. By comparing the difference in the treatment group and the difference in the control group before and after the reform, DID eliminates the bias that comes from the effects other than the reform, that could affect the treatment group. The DID, which measures the differential effect of the reform across the two groups, is highly suitable for establishing causal relationships in the setting of a natural experiment. We expect to observe a lower accident rate in the treatment group compared with the control group after the introduction of experience rating. Moreover, owing to the rather short period of analysis, one can assume that the populations of drivers in the two cities are fairly similar during the four years, and conclude that any causal relationship is more attributable to moral hazard than to adverse selection (See Chiappori, 2000, and Chiappori and Salanié, 2013, for a longer discussion of this important issue). Equation (1) shows our basic regression approach.

$$Accident_{it} = \varpi + \sum_{s=1}^{2} \beta_s Reform_{sit} + X_{it}\alpha + u_i + \eta_d + \varepsilon_{it} \tag{1}$$

where ϖ is a constant term. $Accident_{it}$ is measured by claim frequency (Once, Twice and Number). Subscripts *i* and *t* denote the insurance contract of vehicle *i* and year *t* (from 2009 to 2012) respectively, $s=1$ and $s=2$ denote the first-stage reform and the second-stage reform respectively. u_i is the vehicle fixed effect and η_d is the day fixed effect. We further control for the vehicle characteristics, including the age, age squared, value, and actual premium, all included in the vector X_{it}. α is the vector of parameters. Age squared in the regression should capture a possible non-linear effect for the age of the vehicle. Equation (1) is estimated with and without vehicle controls. The DID methodology is not well developed for non-linear models such as the Poisson model (Blundell and Costa Dias, 2008). Consequently, our main results are presented using the linear model in (1). We will revisit this issue in the robustness section of the article.

The DID methodology addresses the concerns of omitted variables that might affect both the treatment group and the control group in the same way. The main explanatory variables of interest are Reform_1 and Reform_2, the interaction of the two reform period indicator variables After_1 and After_2 with the treatment indicator variable Treat, which evaluate the differential effects of the two-stage reform across the treatment group and control group. The variable Treat captures the difference in claims behavior of the treatment group and control group during the whole study period. Variables After_1 and After_2 capture, respectively, the differences before and after the first-stage reform and the second-stage reform in both cities. Because we employ the fixed effects model in all model specifications we must eliminate possible multicollinearity in the parameter estimation of the time-invariant variables. Therefore, the treatment indicator variable Treat is not estimated separately. We also do not include the two reform period indicator variables, After_1 and After_2, in model specifications because these two variables would be collinear with the day fixed effects. The inclusion of vehicle fixed effects guarantees the control of the vehicle-level heterogeneity. The day fixed effect accounts for the common aggregate shocks.

4. The impact of the reform on road safety

4.1 Before-after analysis for both groups together

In Table 5, we perform a simple “pre” and “post” test by taking the time-averages before and after the reform for the two groups. We present the results for the first-stage reform and the second-stage reform. We note that Once, Twice, and Number declined after both the first-stage reform and the second-stage reform. For example, the probability of whether to claim at least once declined by 13% after the first-stage reform. Compared with the pre-reform mean of 48%, this is a substantial decline. These results do not separate the two groups and cannot be used for an evaluation of the reform' effects. Given that the two reform periods overlap we cannot really associate the variations with a particular reform without an appropriate methodology that separates the effect of each reform.

Table 5 – "Pre" and "post" claim average before and after the reform

	First-stage reform			Second-stage reform		
	Before	**After**	**Difference**	**Before**	**After**	**Difference**
Once	0.476	0.346	-0.130***	0.455	0.321	-0.133***
	(0.003)	(0.001)	(0.003)	(0.002)	(0.001)	(0.002)
Twice	0.216	0.123	-0.093***	0.202	0.105	-0.097***
	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)
Number	0.790	0.520	-0.270***	0.756	0.463	-0.293***
	(0.006)	(0.002)	(0.006)	(0.004)	(0.002)	(0.004)

Note: Standard errors are reported in parentheses; *** for p<0.001.

4.2 Before-after analysis for treatment group and control group

In Figure 2, 3, and 4, we plot the time series of Once, Twice, and Number separately for both the treatment group and the control group. Basically, we observe a downward trend for accident frequency (measured by Once, Twice and Number) for the treatment group and the control group. During our study period, the government continually strengthened the road safety regulations nationwide,[8] which justifies the control for the time fixed effects in the model. We see that the accident frequency variables, namely, Once, Twice, and Number, moved in roughly the same pattern before the first-stage reform. After the reform, the three frequency variables of both the treatment group and the control group declined considerably. However, the disparity between the treatment group and the control group seems to expand after the second reform. The observed enlarged disparity seems to be related to a much greater decrease in the claim

[8] For instance, the revised regulations for applying for and using driving licenses came into force on April 1, 2010. One of the most important revisions is to increase the demerit points for serious traffic violations. On May 1, 2011, China began imposing criminal punishments on people found guilty of drunk driving.

frequency of the treatment group than that of the control group. This is consistent with our expectations that the new insurance incentives introduced in the treatment group would reduce accident frequency accordingly. The figures indicate a seasonality effect in the eleventh month of each year in the control city. The time fixed effect variables will control for this effect.

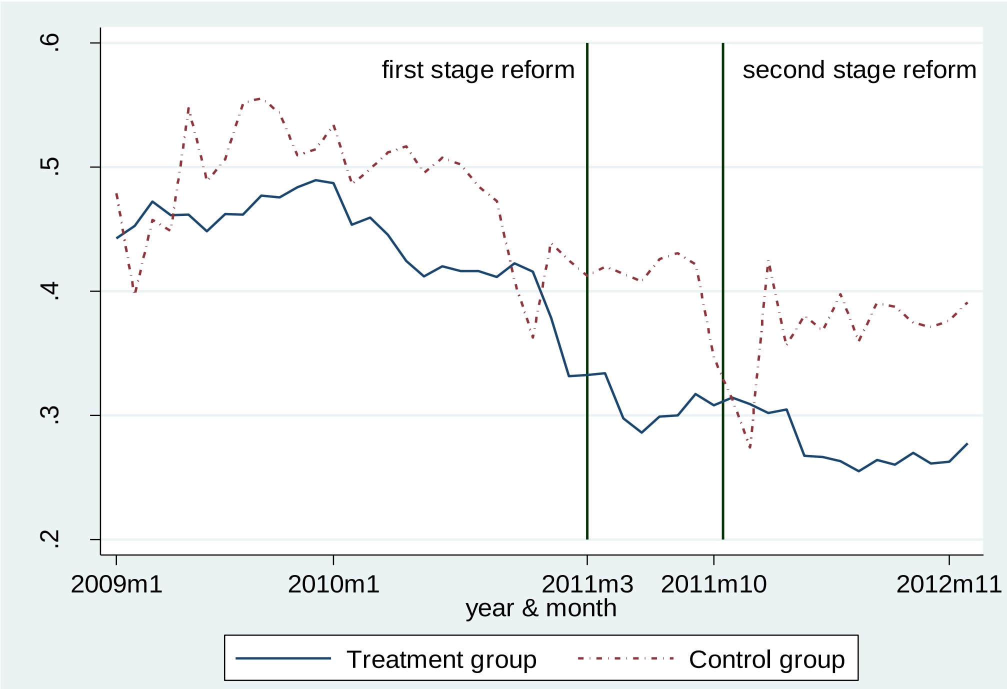

Figure 2 – Accident frequency: Probability of claiming at least one accident

Note: We plot the time series for the variable Once, the probability of claiming at least one accident during an insurance period for both the treatment group and the control group.

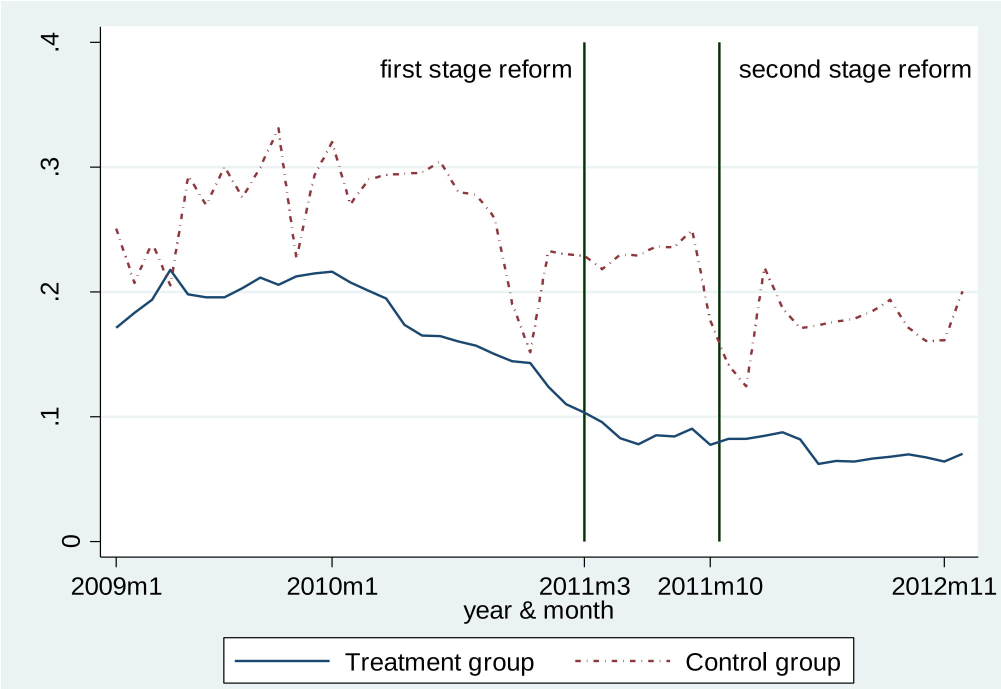

Figure 3 – Accident frequency: Probability of claiming at least two accidents

Note: We plot the time series for the variable Twice, the probability of claiming at least two accidents during an insurance period, for both the treatment group and the control group.

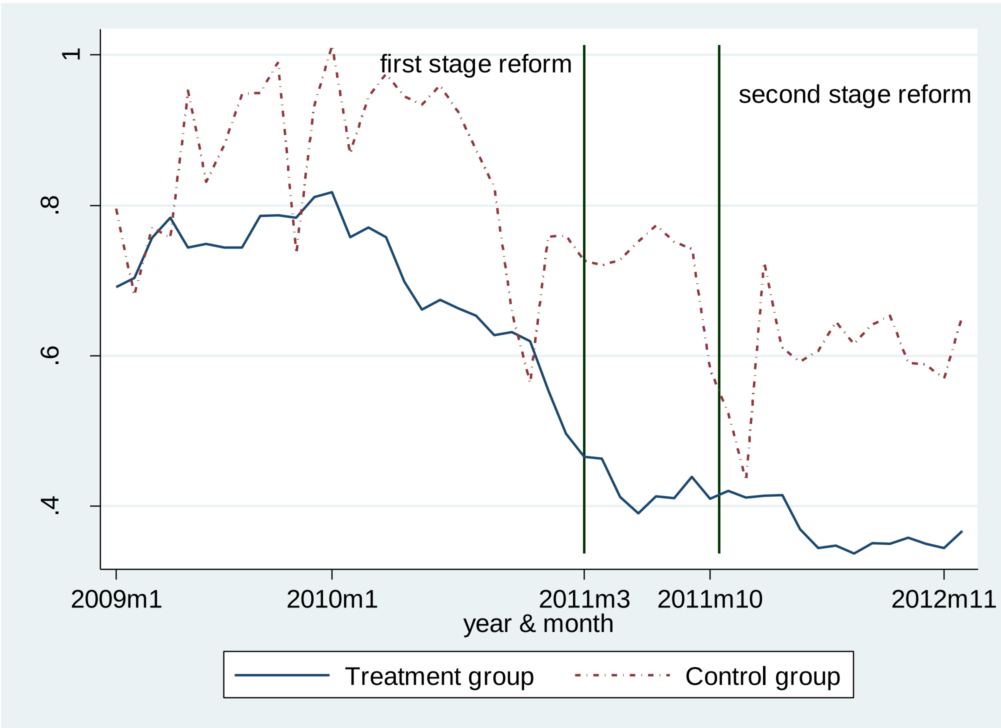

Figure 4 – Accident frequency: Number of claims

Note: We plot the time series for the variable Number, the number of claims during an insurance period, for both the treatment group and the control group.

4.3 Multivariate results

Compared with the model in the previous section, the effects of the reform are better captured by the DID results presented in Table 6. Models (1) and (2) report the results for Once; models (3) and (4) for Twice; and models (5) and (6) for Number. In models (1), (3), and (5) we report the basic regression results without the inclusion of vehicle controls. We further add vehicle characteristics in models (2), (4), and (6). For accident frequencies we see that the coefficients of Reform1 are not significant at the conventional level, while the coefficients of Reform2 are consistently highly significant at the 0.1% level for every model except model (3). Specifically, the coefficient on Reform2 for Number is -0.109 in Model (6). Given that the pre-second-stage-reform mean of the number of claims in the treatment city is 0.721, the implementation of the second-stage reform reduced the number of claims in the treatment city by 15.1%. We conclude that the new insurance pricing based on traffic violations introduced by the second-stage reform have reduced the accident frequency significantly, whereas the effects of basing the pricing on past claims are weak and limited. Regarding vehicle controls, results show that the age and value of the vehicle, and the insurance premium paid negatively affect the accident frequency, and the effects are significant at the 0.1% level in each case. When we look at the coefficient for Age and Age^2, we see a significant U-shaped influence.

4.4 Placebo analyses: validity of the natural experiment

One key assumption for the utilization of DID is that there are no unobserved variables that change over time and affect the differential effects on accident outcomes of the treatment city as compared with the control city. An indirect way to test the plausibility of this assumption is a placebo experiment in the years preceding the actual reforms to see whether there are deviations for the treatment city from the common tread in the pre-treatment years. More precisely, we split the pre-treatment years into two time periods: one is from Jan 2009 until Oct 2010 and the other is from Nov 2010 until Feb 2011. The reason we use Nov 2010 as a dividing point is because this is when the new pricing mechanism was introduced to the public. However, it started in Mar 2011. According to the pre-treatment placebo experiment estimates reported in Table 7, we can see that the coefficients of the placebo reform are far from any conventional significance level, which corroborates the assumption of the DID methodology, namely that the treatment city and the control city exhibit identical pre-treatment trends in accident frequencies.

The occurrence of some unobservable events after the reform in the treatment city may influence the outcome variables, which can violate the objectivity of the evaluation of the effectiveness of the reform because of the omission of key control variables in the model (Meyer, 1995). To the best of our knowledge, no other event may have caused the differential accident frequencies of the two groups during the post-treatment years of our study. After integrating this observation with the pre-reform placebo estimations, we conclude that the pricing reform is the only reason for the treatment city to deviate from the common trend of the two cities.

To further investigate the validity of the DID methodology, we conduct another placebo experiment considering the following scenario: we randomly assigned 84%[9] of the vehicles

[9] The reason why we draw 84% from the control group is that the mean of the treatment dummy variable, Treat, is 0.84. We keep the panel structure as before, that is, the unbalanced panel of the combination of

from the control city to the placebo treatment group, and the rest of the vehicles in the control city remain the control group. The estimations are reported in Table 8. As expected, we do not find any significant effects of the placebo estimations.

These placebo experiments are meant to show that the decreasing accident frequency is the effect of the second-stage reform and not the effect of some artefact of other factors. This is confirmed by tables 7 and 8: we do not find any significant effects for both the pre-reform placebo experiment and the placebo treatment group experiment.

the 3-year balanced panel (2010-2012) and the 4-year balanced panel (2009-2012).

Table 6 - Effects of insurance incentives on accident frequency

This table reports the results of the effects of the two-stage reform. The OLS fixed effects model (Equation 1) is employed for all specifications. Once, Twice, and Number are three proxies for accident frequency, which stand for the probability of making at least one claim, the probability of making at least two claims, and the number of claims during the insurance period respectively. Robust standard errors are employed and *t* statistics are reported in parentheses. *** for p<0.001.

	(1)Once	(2)Once	(3)Twice	(4)Twice	(5)Number	(6)Number
Reform$_1$	0.007	0.015	-0.012	-0.006	-0.02	-0.001
	(0.57)	(1.31)	(-1.14)	(-0.57)	(-0.82)	(-0.05)
Reform$_2$	-0.039***	-0.056***	-0.011	-0.032***	-0.058***	-0.109***
	(-5.27)	(-7.38)	(-1.70)	(-4.81)	(-3.72)	(-6.91)
Age		-0.118***		-0.092***		-0.271***
		(-10.20)		(-10.99)		(-12.04)
Age^2		0.005***		0.004***		0.012***
		(21.72)		(28.20)		(30.27)
Value		-0.001***		-0.000***		-0.002***
		(-10.74)		(-4.93)		(-8.21)
Premium		-0.025***		-0.031***		-0.077***
		(-11.47)		(-18.07)		(-15.60)
Day Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Vehicle Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Within-R^2	0.049	0.054	0.045	0.052	0.062	0.071
Observations	212680	212680	212680	212680	212680	212680
Number of Vehicles	64045	64045	64045	64045	64045	64045

Table 7 - Pre-reform placebo test: split the pre-treatment years into two time periods

This table reports the results for the pre-reform placebo experiment, in which we split the pre-treatment years into two time periods ($After_p$ equals one when the vehicle is insured after the placebo reform, otherwise it equals 0; $Reform_p$ is the interaction of the variables Treat and $After_p$). The OLS fixed effects model (Equation 1) is employed for all specifications. Once, Twice, and Number are three proxies for accident frequency, which stand for the probability of making at least one claim, the probability of making at least two claims, and the number of claims during the insurance period respectively. Robust standard errors are employed and *t* statistics are reported in parentheses.

	(1)Once	(2)Once	(3)Twice	(4)Twice	(5)Number	(6)Number
Reform$_p$	0.031	0.034	0.001	0	0.015	0.016
	(0.82)	(0.90)	(0.03)	(0.01)	(0.21)	(0.23)
Vehicle Controls	No	Yes	No	Yes	No	Yes
Day Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Vehicle Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Within-R^2	0.048	0.058	0.041	0.048	0.051	0.067
Observations	27677	27677	27677	27677	27677	27677
Number of Vehicles	25119	25119	25119	25119	25119	25119

Table 8 - Placebo test: A random draw from the control group as the treatment group

This table reports the results for the placebo treatment group experiment, in which we randomly assigned 84% of the vehicles from the control group to the placebo treatment group, and the remainder are the control group ($Treat_p$ equals one when the vehicle is insured in the placebo treatment group, otherwise it equals 0; $Reform_{p1}$ and $Reform_{p2}$ are the interactions of $Treat_p$ and $After_1$ and $After_2$ respectively). The OLS fixed effects model (Equation 1) is employed in all specifications. Once, Twice, and Number are three proxies for accident frequency, which stand for the probability of making at least one claim, the probability of making at least two claims, and the number of claims during the insurance period respectively. Robust standard errors are employed and *t* statistics are reported in parentheses.

	(1)Once	**(2)Once**	**(3)Twice**	**(4)Twice**	**(5)Number**	**(6)Number**
$Reform_{p1}$	-0.017	-0.014	0.031	0.034	0.064	0.071
	(-0.66)	(-0.54)	(1.27)	(1.37)	(1.14)	(1.27)
$Reform_{p2}$	0.005	0.002	-0.012	-0.014	-0.045	-0.051
	(0.29)	(0.09)	(-0.80)	(-0.93)	(-1.30)	(-1.48)
Vehicle Controls	No	Yes	No	Yes	No	Yes
Day Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Vehicle Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Within-R^2	0.084	0.097	0.08	0.086	0.089	0.099
Observations	34301	34301	34301	34301	34301	34301
Number of Vehicles	10589	10589	10589	10589	10589	10589

4.5 Robustness checks

The capacity of monetary instruments to deter traffic violations is expected to vary depending on the wealth and income of the vehicle owners (Bar-Ilan and Sacerdote, 2004; Polinsky, 2006; Polinsky and Shavell, 1991). We may then wonder if the impact of the reform differs depending on the levels of wealth and income of the insured. Given that we do not have access to data on wealth and income, we consider the value of the vehicle as a proxy variable for the wealth of the insured. Using the median value of the vehicles in the first year at the insurance company,[10] we split the sample into two groups, namely, low value group and high value group, to investigate the differential impact of the reform between these two groups. We repeat the

[10] In our study sample, the first year for the 3-year data is 2010, and for the 4-year data it is 2009. Using this criterion enables us to keep the same panel structure as before.

regressions in Table 6 for these two groups. The results for the low value group are reported in Table 9.1 and those of the high value group in Table 9.2. We notice that the claim frequency measured by Once, Twice and Number of the two groups is consistently negative at the 0.1% or 1% significance level with one exception for the low value group and two exceptions for the high value group. We hence obtain mixed results for the two groups. This is not totally consistent with Bar-Ilan and Sacerdote's (2004) results. In contrast, the second-stage reform has reduced the accident frequency significantly in the two groups with few exceptions.

Because there are expensive imported vehicles (3.6% of the vehicles are imported) in our data, we drop them and keep only the domestic ones to see whether the results are robust. The results for domestic vehicles only are shown in Table 10. We notice that the results are fairly consistent.

In these two subsections, we examined whether the effectiveness of the introduction of experience rating based both on past claims and on traffic violations varies with vehicle characteristics such as the value and origin of the vehicle. We find that our main results are robust to these two different sample variations. We observe that the owners of low and high value vehicles respond almost identically to the second-stage reform. Future research is needed before concluding that the experience rating may be imposed differentially based on individuals' socio-economic status including income and wealth.

Table 9.1 - Robustness Check: Effects of insurance incentives on accident frequency of low value group

According to the median of the value of vehicles in the first year, we split the sample into two sub-samples: 1) the low value sample, which is lower than the median; and 2) the high value sample, which is equal to or higher than the median. The results for the low value group and high value group are reported in tables 9.1 and 9.2 respectively. The OLS fixed effects model (Equation 1) is employed for all specifications. Once, Twice, and Number are three proxies for accident frequency, which stand for the probability of making at least one claim, the probability of making at least two claims, and the number of claims during the insurance period respectively. Robust standard errors are employed and *t* statistics are reported in parentheses. *** for p<0.001, ** for p<0.01, and * for p<0.05.

	(1)Once	(2)Once	(3)Twice	(4)Twice	(5)Number	(6)Number
Reform$_1$	0.028	0.041*	-0.008	0.002	0.008	0.039
	(1.75)	(2.51)	(-0.62)	(0.16)	(0.27)	(1.23)
Reform$_2$	-0.047***	-0.064***	-0.007	-0.035***	-0.067**	-0.133***
	(-4.55)	(-6.13)	(-0.82)	(-3.98)	(-3.22)	(-6.32)
Vehicle Controls	No	Yes	No	Yes	No	Yes
Day Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Vehicle Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Within-R^2	0.06	0.067	0.055	0.067	0.073	0.087
Observations	98960	98960	98960	98960	98960	98960
Number of Vehicles	29834	29834	29834	29834	29834	29834

Table 9.2 - Robustness Check: Effects of insurance incentives on accident frequency of high value group

	(1)Once	(2)Once	(3)Twice	(4)Twice	(5)Number	(6)Number
Reform$_1$	-0.019	-0.006	-0.016	-0.008	-0.052	-0.026
	(-1.05)	(-0.36)	(-0.95)	(-0.46)	(-1.33)	(-0.67)
Reform$_2$	-0.030**	-0.045***	-0.013	-0.032**	-0.041	-0.086***
	(-2.72)	(-3.99)	(-1.32)	(-3.12)	(-1.69)	(-3.50)
Vehicle Controls	No	Yes	No	Yes	No	Yes
Day Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Vehicle Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Within-R^2	0.057	0.063	0.056	0.064	0.072	0.081
Observations	113720	113720	113720	113720	113720	113720
Number of Vehicles	34211	34211	34211	34211	34211	34211

Table 10 - Robustness Check: Effects of insurance incentives on accident frequency of domestic vehicles

We drop imported vehicles and keep only domestic ones. The OLS fixed effects model (Equation 1) is employed in all specifications. Once, Twice, and Number are three proxies for accident frequency, which stand for the probability of making at least one claim, the probability of making at least two claims, and the number of claims during the insurance period respectively. Robust standard errors are employed and *t* statistics are reported in parentheses. *** for p<0.001.

	(1)Once	(2)Once	(3)Twice	(4)Twice	(5)Number	(6)Number
Reform$_1$	0.004	0.015	-0.014	-0.007	-0.027	-0.003
	(0.33)	(1.27)	(-1.33)	(-0.61)	(-1.11)	(-0.13)
Reform$_2$	-0.039***	-0.056***	-0.009	-0.033***	-0.054***	-0.112***
	(-5.11)	(-7.39)	(-1.43)	(-4.92)	(-3.43)	(-6.99)
Vehicle Controls	No	Yes	No	Yes	No	Yes
Day Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Vehicle Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Within-R^2	0.05	0.056	0.046	0.054	0.063	0.074
Observations	205055	205055	205055	205055	205055	205055
Number of Vehicles	61713	61713	61713	61713	61713	61713

Further, we use the median of the age of the vehicle in the first-year portfolio of the insurance company to split the sample into two groups: 1) low age group (below 3 years) and 2) high age group, to test whether the impact of the reform differs between these two groups. We rerun the regressions in Table 6. The results for the low age group are reported in Table 11.1 and for the high age group in Table 11.2. The estimations in both groups confirm the results in Table 6, but the low age group seems more responsive than the high age group. This may be explained by the difference in the basic premiums of the a priori pricing of the vehicles: the average basic premium (without the multiplicative BMC and MC) for vehicles less than three years old is 4.6 thousand yuan, compared with 4.1 thousand yuan for the older ones. Therefore, obtaining a high multiplicative malus factor is more costly for newer vehicles.

Table 4 shows that 85.1% of the vehicles are regular automobiles with 6 or fewer passengers. We keep these automobiles and delete the other types of vehicles to see whether the results change fundamentally. The results for regular automobiles are reported in Table 12. Once again, we see (with few exceptions) the consistently significant effects of the second-stage reform on accident frequency measured by Once, Twice and Number.

In our study sample, 89.6% of the vehicles filed fewer than 3 claims (0 claim, one claim or two claims) during the insurance period for either three consecutive years, from 2010 until 2012, or four consecutive years from 2009 until 2012. We keep these vehicles to run the robustness check, and the results are reported in Table 13. The previous conclusion is confirmed once again by the subsample analysis. Therefore, the incentive effects do matter for lower risk drivers.

Finally we could have used non-linear models to estimate the different accident frequency models. As Blundell and Costa-Dias (2008) contend, extending the standard DID methodology to non-linear models needs adjustment in many circumstances if one wants to keep all the properties of the methodology. In our case, the results are fairly consistent. Table 14 presents our main results with fixed effects non-linear models (Logit, Poisson, and Negative Binomial). Again, the main results of our study presented in Table 6 are robust to the methodology used in these estimations.

Table 11.1 - Robustness Check: Effects of insurance incentives on accident frequency of low age group

According to the median of the age of vehicle in the first year, we split the sample into two sub-samples:1) low age sample, which is smaller than the median; and 2) high age sample, which is equal to or higher than the median. The results for the low age group and high age group are reported in Table 11.1 and 11.2 respectively. The OLS fixed effects model (Equation 1) is employed for all specifications. Once, Twice, and Number are three proxies for accident frequency, which stand for the probability of making at least one claim, the probability of making at least two claims, and the number of claims during the insurance period respectively. Robust standard errors are employed and *t* statistics are reported in parentheses. *** for p<0.001 and * for p<0.05.

	(1)Once	**(2)Once**	**(3)Twice**	**(4)Twice**	**(5)Number**	**(6)Number**
$Reform_1$	0.002	0.012	-0.024*	-0.016	-0.047	-0.023
	(0.18)	(0.87)	(-1.97)	(-1.29)	(-1.66)	(-0.80)
$Reform_2$	-0.050***	-0.067***	-0.015	-0.039***	-0.075***	-0.135***
	(-5.75)	(-7.56)	(-1.86)	(-4.93)	(-4.03)	(-7.10)
Vehicle Controls	No	Yes	No	Yes	No	Yes
Day Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Vehicle Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Within-R^2	0.058	0.062	0.055	0.063	0.074	0.083
Observations	153998	153998	153998	153998	153998	153998
Number of Vehicles	46304	46304	46304	46304	46304	46304

Table 11.2 - Robustness Check: Effects of insurance incentives on accident frequency of high age group

	(1)Once	**(2)Once**	**(3)Twice**	**(4)Twice**	**(5)Number**	**(6)Number**
$Reform_1$	0.017	0.033	0.028	0.040*	0.06	0.093*
	(0.70)	(1.35)	(1.48)	(2.09)	(1.30)	(2.02)
$Reform_2$	-0.019	-0.033*	-0.012	-0.026*	-0.035	-0.068*
	(-1.32)	(-2.24)	(-1.09)	(-2.24)	(-1.26)	(-2.39)
Vehicle Controls	No	Yes	No	Yes	No	Yes
Day Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Vehicle Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Within-R^2	0.059	0.064	0.057	0.061	0.067	0.074
Observations	58682	58682	58682	58682	58682	58682
Number of Vehicles	17741	17741	17741	17741	17741	17741

Table 12 - Robustness Check: Effects of insurance incentives on accident frequency of regular automobiles

This table reports the results for estimating the effects of the reform when the sample is limited to regular automobiles (with 6 or fewer passengers) only. The OLS fixed effects model (Equation 1) is employed for all specifications. Once, Twice, and Number are three proxies for accident frequency, which stand for the probability of making at least one claim, the probability of making at least two claims, and the number of claims during the insurance period respectively. Robust standard errors are employed and *t* statistics are reported in parentheses. *** for p<0.001 and * for p<0.05.

	(1)Once	(2)Once	(3)Twice	(4)Twice	(5)Number	(6)Number
Reform$_1$	0.001	0.011	-0.018	-0.011	-0.034	-0.013
	(0.05)	(0.80)	(-1.42)	(-0.86)	(-1.19)	(-0.44)
Reform$_2$	-0.033***	-0.049***	0.001	-0.019*	-0.032	-0.083***
	(-3.98)	(-5.78)	(0.12)	(-2.50)	(-1.74)	(-4.45)
Automobile Controls	No	Yes	No	Yes	No	Yes
Day Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Automobile Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Within-R^2	0.052	0.057	0.048	0.056	0.066	0.076
Observations	180946	180946	180946	180946	180946	180946
Number of Automobiles	54436	54436	54436	54436	54436	54436

Table 13 - Robustness Check: Effects of insurance incentives on accident frequency of less than 3 claims subsample

This table reports the results for the fewer than 3 claims subsample. The OLS fixed effects model (Equation 1) is employed for all specifications. Once, Twice, and Number are three proxies for accident frequency, which stand for the probability of making at least one claim, the probability of making at least two claims, and the number of claims during the insurance period respectively. Robust standard errors are employed and *t* statistics are reported in parentheses. *** for p<0.001, ** for p<0.01, and * for p<0.05.

	(1)Once	(2)Once	(3)Twice	(4)Twice	(5)Number	(6)Number
Reform$_1$	0.01	0.018	0.002	0.006	0.012	0.024
	(0.80)	(1.38)	(0.18)	(0.55)	(0.61)	(1.18)
Reform$_2$	-0.026**	-0.046***	-0.002	-0.018**	-0.028*	-0.064***
	(-3.18)	(-5.53)	(-0.35)	(-2.82)	(-2.26)	(-5.05)
Vehicle Controls	No	Yes	No	Yes	No	Yes
Day Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Vehicle Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Within-R^2	0.041	0.045	0.031	0.036	0.046	0.053
Observations	189856	189856	189856	189856	189856	189856
Number of Vehicles	57386	57386	57386	57386	57386	57386

Table 14 - Effects of insurance incentives on accident frequency with non-linear models

This table reports the results for the effects of the two-stage reform using non-linear models. The logit fixed effects model is employed in models (1) to (4); Poisson fixed effects model for models (5) and (6); and the negative binomial fixed effects model for models (7) and (8). Once, Twice, and Number are three proxies for accident frequency, which stand for the probability of making at least one claim, the probability of making at least two claims, and the number of claims during the insurance period respectively.. Models (1) and (2) report the results for Once; models (3) and (4) for Twice; models (5) and (6) for Number (Poisson Model); models (7) and (8) for Number (negative binomial model). To reduce the numbers of dummies, we use the year-month fixed effects instead of day fixed effects for the non-linear model specifications for the logit, Poisson, and negative binomial models. *** for p<0.001 and * for p<0.05.

Dependent Variable	**Frequency**							
	(1)Once	**(2)Once**	**(3)Twice**	**(4)Twice**	**(5)Number**	**(6)Number**	**(7)Number**	**(8)Number**
$Reform_1$	0.039	0.081	-0.148*	-0.101	-0.046	-0.012	-0.051	-0.034
	(0.67)	(1.38)	(-2.24)	(-1.52)	(-1.71)	(-0.45)	(-1.78)	(-1.20)
$Reform_2$	-0.167***	-0.200***	-0.366***	-0.391***	-0.232***	-0.232***	-0.228***	-0.231***
	(-4.80)	(-5.69)	(-8.79)	(-9.31)	(-12.97)	(-12.88)	(-12.60)	(-12.75)
Age		-0.619***		-0.801***		-0.435***		-0.315***
		(-9.81)		(-8.24)		(-7.95)		(-12.90)
Age^2		0.018***		0.026***		0.012***		0.010***
		(11.85)		(10.92)		(11.00)		(10.41)
Value		-0.007***		-0.008***		-0.006***		-0.003***
		(-14.15)		(-10.69)		(-12.40)		(-15.57)
Premium		-0.105***		-0.126***		-0.037***		-0.020***
		(-11.43)		(-11.02)		(-5.86)		(-3.85)
Year-Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Vehicle Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Log Likelihood/Within-R^2	-47342.021	-47100.961	-24161.479	-23991.701	-92122.783	-91840.113	-91776.1	-91568.6
Observations	134630	134630	72549	72549	149526	149526	149526	149526
Number of Vehicles	40103	40103	21565	21565	44771	44771	44771	44771
Model Specification	Logit-FE	Logit-FE	Logit-FE	Logit-FE	Poisson-FE	Poisson-FE	NBR-FE	NBR-FE

5. Conclusion

This paper provides evidence of a causal effect of moral hazard on accident frequency in China. To establish causality, we exploit a vehicle insurance pricing reform introduced in a pilot city of China as a natural experiment. We obtained data from an insurer that is present in both the treatment city and the control city. Prior to the reform, the pricing mechanism was the same in both cities. The placebo pre-treatment estimations show that the two cities shared identical time trends in accident outcomes in the pre-reform years. As a result, the DID methodology is applicable, and we find strong behavioral effects from the reform. The results show that the addition of an experience-rated premium based on traffic violations reduces the accident frequency significantly. This conclusion is robust to the inclusion of vehicle controls, alternative definitions of claim frequency, two placebo experiments and several robustness checks. We also find that the effects of introducing experience-rated premiums based on past claims are not significant.

An open question is why the effects of the traffic violation reform are stronger than those of past claims reform. The change in the pricing formula based uniquely on past claims may not have been sufficiently large to change claims behavior even if the first-stage reform forced insurers to commit to using past claims when applying the new pricing policy. Specifically, for less risky insured, namely those who file fewer than two claims per insurance period, the first-stage reform is still more a reward than a punishment (Table 1a), whereas the second-stage reform is a complete punishment when they accumulate traffic violations. The relative results for the two reforms may be due to the possibility that a punishment stimulates safe driving better than a reward does, although, in theory, both rewards and punishments can act as incentives. Of course, the relative values are important to set the optimal incentive scheme for road safety, and the second-stage reform parameters appear to be more penalizing.

Moreover, because the insurer observes only the claims and not all accidents, insured may have chosen to underreport some past (minor) accidents in order to avoid an increase in their premium (Cohen 2005; Robinson and Zheng, 2010). In fact, insured have a greater incentive to underreport past claims after the first-stage reform than before because of the new commitment rule and the steeper BMC. Consequently, the insignificant net effect may be explained by a trade-off between additional incentives for road safety along with additional incentives for underreporting accidents. Because the observed distribution of claims is a truncation of the true accident distribution, the observed effects of basing the pricing on past claims may be biased (Chiappori, 2000, Dionne et al. 2013a).

Further, the traffic violation information of insured, collected and kept in the Bureau of Traffic Control and shared by all vehicle insurers in the treatment city, is now complete and accurate. Insurers are restricted by law to use this public information even if the insured chooses to change insurance companies. There is no possibility of underreporting past traffic violations under second-stage reform. It is obviously impossible to compare the probably underestimated effects of basing insurance pricing on past claims and the actual effects of experience-rated premiums based on past traffic violations when we do not have access to complete accident information.

Acknowledgements

This research was done partly while Ying Liu was visiting the Canada Research Chair in Risk Management. A previous version was presented at the 2016 Annual Meeting of the American Risk and Insurance Association, where the authors received helpful comments. The authors wish to thank Qiang Chen, Jinyan Hu, Pierre-Carl Michaud, Steven Ongena, Jorn-steffen Pischke, and Yanyan Ren for useful comments and encouragement, along with the employees from the firm that provided the data analyzed in this study, for their generous collaboration.

References

Abay, K.A., forthcoming. How Effective Are Non-Monetary Instruments for Safe Driving? Panel Data Evidence on the Effect of the Demerit Point System in Denmark. Scandinavian Journal of Economics.

Abbring, J., Chiappori, P.A., Pinquet, J., 2003. Moral Hazard and Dynamic Insurance Data. Journal of the European Economic Association. 1, 767–820.

Arrow, K., 1963. Uncertainty and the Welfare Economics of Medical Care. American Economic Review. 53, 941-973.

Ashenfelter, O., 1978. Estimating the Effect of Training Programs on Earnings. Review of Economics and Statistics. 60, 47-57.

Bar-Ilan, A., Sacerdote, B., 2004. The Response of Criminals and Noncriminals to Fines. Journal of Law and Economics. 47, 1-17.

Bauernschuster, S., Schlotter, M., 2015. Public Child Care and Mothers' Labor Supply-Evidence from Two Quasi-experiments. Journal of Public Economics. 123, 1-16.

Blundell, R., Costa Dias, M., 2008. Alternative Approaches to Evaluation in Empirical Microeconomics. Mimeo, University College London. 124 p.

Bourgeon, J.M., Picard, P., 2007. Point-Record Driver’s License and Road Safety: An Economic Approach. Journal of Public Economics. 91, 235-258.

Castillo-Manzano, J.I., Castro-Nuño, M., 2012. Driving Licenses based on Points Systems: Efficient Road Safety Strategy or Latest Fashion in Global Transport Policy? A Worldwide Meta-analysis. Transport Policy. 21. 191-201.

Chiappori, P.A., 2000. Econometric Models of Insurance under Asymmetric Information, in: Dionne, G. (Ed.), Handbook of Insurance. Boston: Kluwer Academic Publishers, pp. 365-394.

Chiappori, P.A., Salanié, B., 2000. Testing for Asymmetric Information in Insurance Markets. Journal of Political Economy. 108, 56-78.

Chiappori, P.A., Salanié, B., 2013. Asymmetric Information in Insurance Markets: Predictions and Tests, in: Dionne, G. (Ed.), Handbook of Insurance. New York: Springer, pp. 397-422.

Cohen, A., 2005. Asymmetric Information and Learning in the Automobile Insurance Market. Review of Economics and Statistics. 87, 197-207.

Crocker, K. Snow, A., 1986. The Efficiency Effect of Categorical Discrimination in the Insurance Industry. Journal of Political Economy. 94, 321-344.

Crocker, K. Snow, A., 1985. The Efficiency of Competitive Equilibria in Insurance Markets with Asymmetric Information. Journal of Public Economics. 26, 207-219.

De Paola, M., Scoppa, V., Falcone, M., 2013. The Deterrent Effects of the Penalty Points System for Driving Offences: A Regression Discontinuity Approach. Empirical Economics. 45, 965-985.

Dionne, G., Ghali, O., 2005. The (1992) Bonus-Malus System in Tunisia: An Empirical Evaluation. Journal of Risk and Insurance. 72, 609-633.

Dionne, G., Gouriéroux, C., Vanasse, C., 2001. Testing for Evidence of Adverse Selection in the Automobile Insurance Market: A Comment. Journal of Political Economy. 109, 444-453.

Dionne, G., Michaud, P.C., Dahchour, M., 2013a. Separating Moral Hazard from Adverse Selection and Learning in Automobile Insurance: Longitudinal Evidence from France. Journal of the European Economic Association. 11, 897-917.

Dionne, G., Michaud, P.C., Pinquet, J., 2013b. A Review of Recent Theoretical and Empirical Analyses of Asymmetric Information in Road Safety and Automobile Insurance. Research in Transportation Economics. 43, 85-97.

Dionne, G., Pinquet, J., Maurice, M., Vanasse, C., 2011. Incentive Mechanisms for Safe Driving: A Comparative Analysis with Dynamic Data. Review of Economics and Statistics. 93, 218-227.

Hoy, M., 1982. Categorizing Risk in the Insurance Industry. Quarterly Journal of Economics. 97, 321-336.

Imbens, G., Wooldridge, J.M., 2009. Recent Developments in the Econometrics of Program Evaluation. Journal of Economic Literature. 47, 5-86.

Israel, M., 2007. Do We Drive More Safely when Accidents are More Expensive? Identifying Moral Hazard from Experience Rating Schemes. Working paper. Wharton School, University of Pennsylvania.

Lee, Y.W., 2013. Testing for the Presence of Moral Hazard Using the Regulatory Reform in the Car Insurance Market: Case of Korea. Japanese Economic Review. 64, 414-429.

Li, C.S., Liu, C.C. Yeh, J.H., 2007. The Incentive Effects of Increasing Per-claim Deductible Contracts in Automobile Insurance. Journal of Risk and Insurance. 74, 441-459.

Makowsky, M.D., Stratmann, T., 2011. More Tickets, Fewer Accidents: How Cash-strapped Towns Make for Safer Roads. Journal of Law and Economics. 54, 863-888.

Meyer, B.D., 1995. Natural and Quasi-experiments in Economics. Journal of Business & Economic Statistics. 13, 151-161.

Polinsky, A.M., 2006. The Optimal Use of Fines and Imprisonment When Wealth is Unobservable. Journal of Public Economics. 90, 823-835.

Polinsky, A.M., Shavell, S., 1991. A Note on Optimal Fines When Wealth Varies among Individuals. American Economic Review. 81, 618-621.

Robinson, C., Zheng, B., 2010. Moral hazard, Insurance Claims, and Repeated Insurance Contracts. Canadian Journal of Economics. 43, 967-993.

Rowel, D., Nghiem, S., Connelly, L.B., forthcoming. Two Tests for Ex Ante Moral Hazard in a Market for Automobile Insurance. Journal of Risk and Insurance. DOI: 10.1111/jori.12161

Vukina, T., and D.Nestić, 2015, Do People Drive Safer When Accidents Are More Expensive: Testing for Moral Hazard in Experience Rating Schemes, Transportation Research Part A: Policy and Practice, 71: 46-58.

Wang, J.L., C.F. Chung, and L.Y. Tzeng, 2008, An Empirical Analysis of the Effects of Increasing Deductibles on Moral Hazard, Journal of Risk and Insurance, 75(3): 551-566.

Weisburd, S., 2015, Identifying Moral Hazard in Car Insurance Contracts, Review of Economics and Statistics, 97(2): 301-313.

Zavadil, T., 2015, Do the Better Insured Cause More Damage? Testing for Asymmetric Information in Car Insurance, Journal of Risk and Insurance, 82(4): 865-88