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

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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 in 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 framework of a natural experiment. 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, and several robustness checks. The effects of improving insurance pricing on past claims are not significant.

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

JEL Classification: C33, C35, D81, D82, G22, R41.

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Highlights

- We study the effects of insurance incentives on road safety in China.
- We use the difference-in-differences methodology.
- We provide evidence of a causal effect of moral hazard on accident frequency.
- The main effect is obtained from insurance pricing based on traffic violation.
- The effect of improving pricing on past claims are not significant.

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. Improvements 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 enhance road safety (Vukina and Nestić, 2015). Concerning incentives, monetary mechanisms such as fines and non-monetary mechanisms such as point-record drivers' licenses have proven to be effective[1]. An 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 in China has the features of a natural experiment, which allows us to examine drivers' reactions 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 with a

[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).

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 claim or traffic violation 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 resulting change in driving behavior can be attributed to moral hazard when the population remains unchanged in each group (Chiappori, 2000).

Ghali, 2005; Rowel et al., forthcoming; Zavadil, 2015) 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, namely the studies by 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 gives us an opportunity to use the methodology of difference-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 that of Abay (forthcoming), 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 utilized extensively 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. We find that the incentive effects of the enforcement of an experience rating scheme based on traffic violations in repeated insurance contracts have 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, and several robustness checks. We also find that the effects of improving the experienced-rated pricing 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 estimation reform effects on road safety 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 and value of the vehicle. 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. Another explanation is that the pricing was based on reported past claims and not on total past accidents.

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 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[3].

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.

[3] The growth rates for the total population, the ratio of males in the total population, and the number of civil vehicles during our study period (from 2009 to 2012) are almost equal across the treatment city and contrrol city.

	Treatment City			Control City		
	2009	2012	Growth Rate	2009	2012	Growth Rate
Total Population	9,950,100	10,547,400	6%	6,874,700	7,261,800	6%
Ratio of males in the total population	53%	53%	0	49%	49%	0
Number of civil vehicles	1, 419, 005	2, 213, 975	56%	761, 158	1, 197, 638	57%

Table 1 presents the BMC schedules for the treatment city and the control city respectively after the first-stage reform.

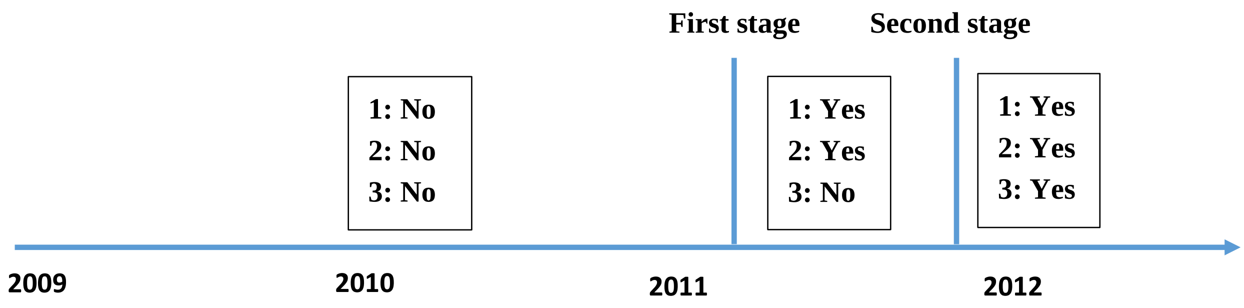

Figure 1 – Two-stage reform in the treatment city

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 bonus malus factor has switched to a bigger range; at 2, whether there is enforcement of insurers' application of the new bonus malus factor 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.

This first-stage reform was introduced to improve the effectiveness of the BMC. 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 for the treatment city (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 even 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. We should mention here that many of these accidents are small: 33% of claims are lower than 1,000 yuan and 63% are lower than 2,000 yuan while the average value of a vehicle is 120,000 yuan. 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.

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.

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

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 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 for last year 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

This table presents the new insurance pricing based on traffic violations. 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, drivers who have a traffic violation for drunk driving in year *t* – 1, will have a MC equal to 1.3 in year *t*. If, in addition, these drivers drove without a licence, their MC will be equal to the maximum 1.5 instead of 1.6.

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%

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 next 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 quasi-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 does. 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 informed 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 whole sample). The call center manages more than half of the company's total individual policies. The data span the years 2009 to 2012 and the annual contracts are concluded on everyday basis from 2009/01/01-2012/12/31. 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 until 2012) and four (from 2009 until 2012) consecutive years,[4] which corresponds to about 24 percent of the whole sample. This study sample includes data on 43,500 vehicles that stay from 2010 until 2012 and 20,545 vehicles that stay from 2009 until 2012. We have a total of 212,680 observations after excluding missing values.

Each observation is a one-year vehicle insurance policy.[5] Our sample contains detailed policy underwriting information and at-fault claims records. The underwriting data are based on

[4] 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.

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

vehicle characteristics[6] 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 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 reported claim frequency. Three variables—Once, Twice, and Number— are employed to act as proxies for accident frequency. Table 4 shows the summary statistics for the outcome variables, the DID variables, and the control variables[7]: 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[8]: 83.9% of the policies in the sample are from the treatment city. Table 4 shows that the policies

[6] Unlike in many countries, vehicle insurers in China do not use drivers' information, such as age, gender, and years of driving experience, for insurance pricing.

[7] The table below reports the summary statistics of the outcome variables in the whole sample of policies obtained from the insurer compared with their counterparts in our study sample in Table 4. In Table 4, we are limited to vehicles that stay in the sample for three or four consecutive years. 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	883,207
Twice	0.146	0.353	0	0	1	883,207
Number	0.591	0.957	0	0	25	883,207

[8] 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.

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

Variable	Definition
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 insured vehicle is in the reform city, and 0 otherwise
$After_1$	A dummy variable that equals 1 when the end of the insurance period of the vehicle is after the first-stage reform, and 0 otherwise
$After_2$	A dummy variable that equals 1 when the end of the insurance period of the vehicle is after the second-stage reform, and 0 otherwise
$Reform_1$	Interaction of the two variables, Treat and $After_1$
$Reform_2$	Interaction of the two variables, Treat and $After_2$
Vehicle's characteristics	
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)
Load	The cargo capacity of the vehicle (in tons)
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

This table reports the statistics of the variables used in this study. The total number of observations is 212,680.

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 characteristics					
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
Load	0.027	0.176	0	0	4.99
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

If we look at the variables regarding the vehicle characteristics in Table 4, we see that the average age of the vehicle is 3.859 years. The average value of the vehicle is 120,460 yuan and the average actual premium is 2,960 yuan. The average load is 0.027 tons (because the cargo capacity of most regular automobiles is nil). 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).

The summary statistics of the control variables for the treatment group and the control group during the pre-treatment period (Before Mar 1, 2011) are presented in Table 5. We can see that the treatment and the control group differ along several of the observable dimensions. This implies that we must use an appropriate methodology to verify that our results are not confounded by these differences.

Table 5 - Summary statistics of variables for vehicle characteristics by city group during the pre-treatment period

This table reports the statistics of the control variables in the two cities during the pre-treatment period. The number of observations in the treatment city and control city are 23,590 and 4,087 respectively.

Control variables	**Treatment City**					**Control City**				
	Mean	**Sd**	**Min**	**Median**	**Max**	**Mean**	**Sd**	**Min**	**Median**	**Max**
Age	2.836	1.897	0.003	2.074	17.260	2.498	1.692	0.003	2.019	16.603
Value	125.460	93.555	8.120	100.375	1750.000	97.870	67.272	20.000	85.000	1400.000
Premium	3.361	1.482	0.259	3.201	26.742	2.276	1.074	0.380	2.247	29.184
Load	0.012	0.115	0.000	0.000	4.000	0.082	0.301	0.000	0.000	1.995
Import	0.033	0.180	0.000	0.000	1.000	0.019	0.137	0.000	0.000	1.000
Type1	0.015	0.121	0.000	0.000	1.000	0.080	0.271	0.000	0.000	1.000
Type2	0.000	0.000	0.000	0.000	0.000	0.000	0.016	0.000	0.000	1.000
Type3	0.878	0.327	0.000	1.000	1.000	0.780	0.415	0.000	1.000	1.000
Type4	0.104	0.306	0.000	0.000	1.000	0.139	0.346	0.000	0.000	1.000
Type5	0.996	0.066	0.000	1.000	1.000	0.999	0.038	0.000	1.000	1.000

3.2 Methodology

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 pilot 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 potential bias that comes from the effects other than the reform, which 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 frequency 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 on this important issue).[9] Equation (1) shows our basic regression approach.

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

where w 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

[9] In fact, we force the populations of vehicle owners to be the same in each city before and after the reforms in the robustness analysis (Table 13) and the results are not affected.

the vehicle characteristics, including the age, age squared, vehicle value, and actual premium, all included in the vector X_{it}. α is the vector of the 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 control variables. 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 (Table 15).

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. The 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 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 treatment group and control group

Figure 2 depicts the averages of daily Once, Twice, and Number by city groups and treatment periods based on the end date of each policy. To facilitate visual comparison, every date point plotted in Figure 2 represents a 30-day moving average of Once, Twice, and Number surrounding a specific date. Over time, we observe a downward trend for accident frequency (measured by Once in Figure 2-1, Twice in Figure 2-2 and Number in Figure 2-3) for the treatment group and the control group. During our study period, the government continually strengthened the road safety regulations nationwide,[10] which justifies control for the time fixed effects in the model. We see that the three accident frequency variables moved in roughly the same pattern before the first-stage reform if we exclude a seasonality effect in the control city. The time fixed effect dummies control for this effect before and after the two reforms. After the reform, the three variables of both the treatment group and the control group declined continuously. However, the disparity between the treatment group and the control group seems to expand after the second-stage reform. The observed enlarged disparity seems to be related to a much greater decrease in the claim frequency of the treatment group compared with the control group. This is consistent with our expectations that the new insurance incentives introduced in the treatment group would reduce accident frequency accordingly.

[10] 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.

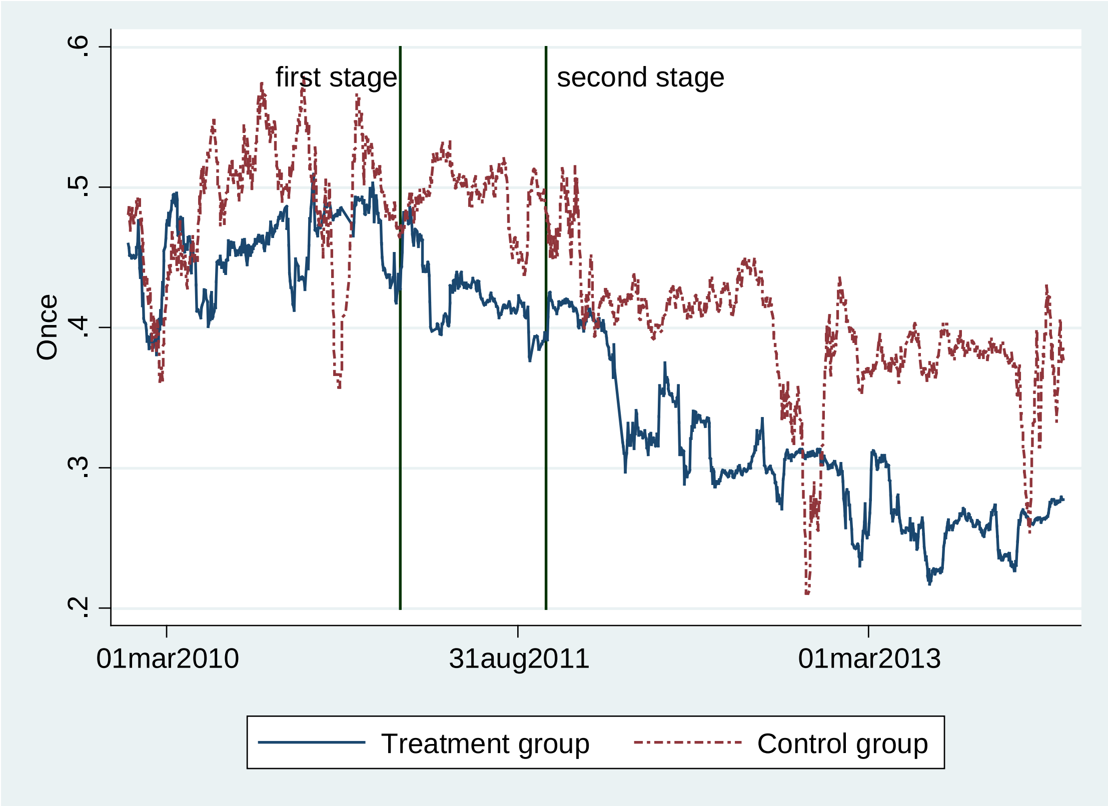

Figure 2-1 Time series of Once by city group and treatment periods

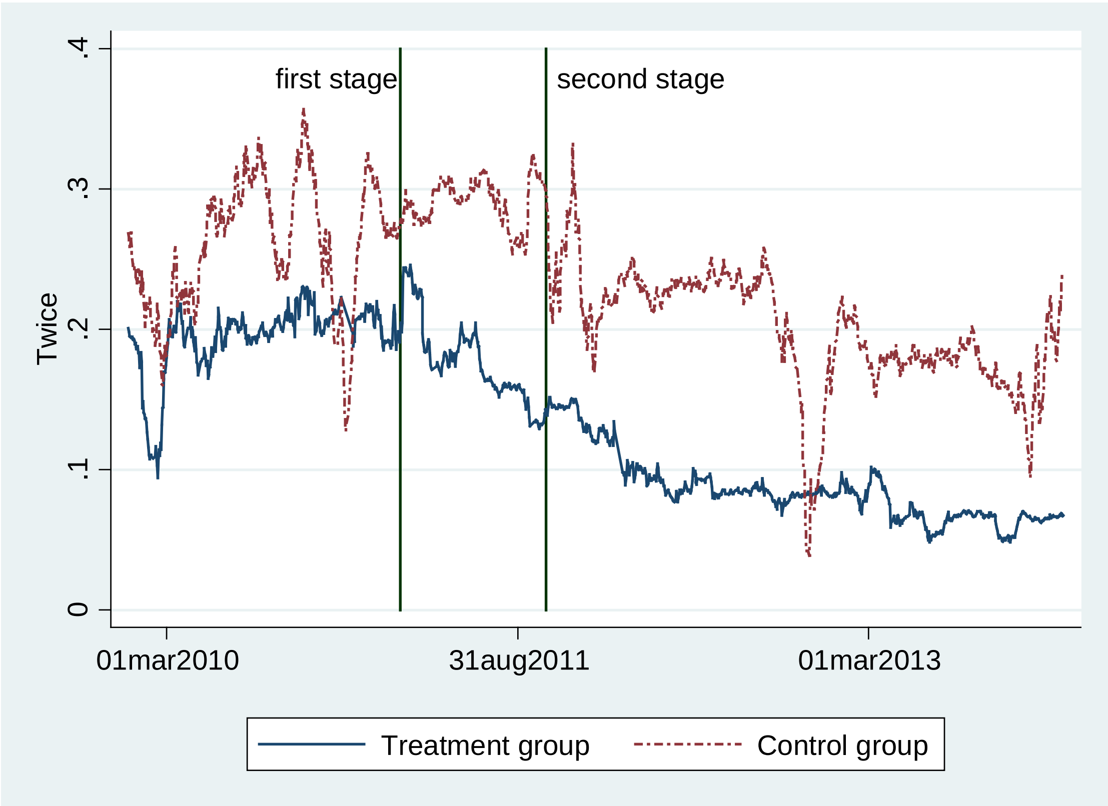

Figure 2-2 Time series of Twice by city group and treatment periods

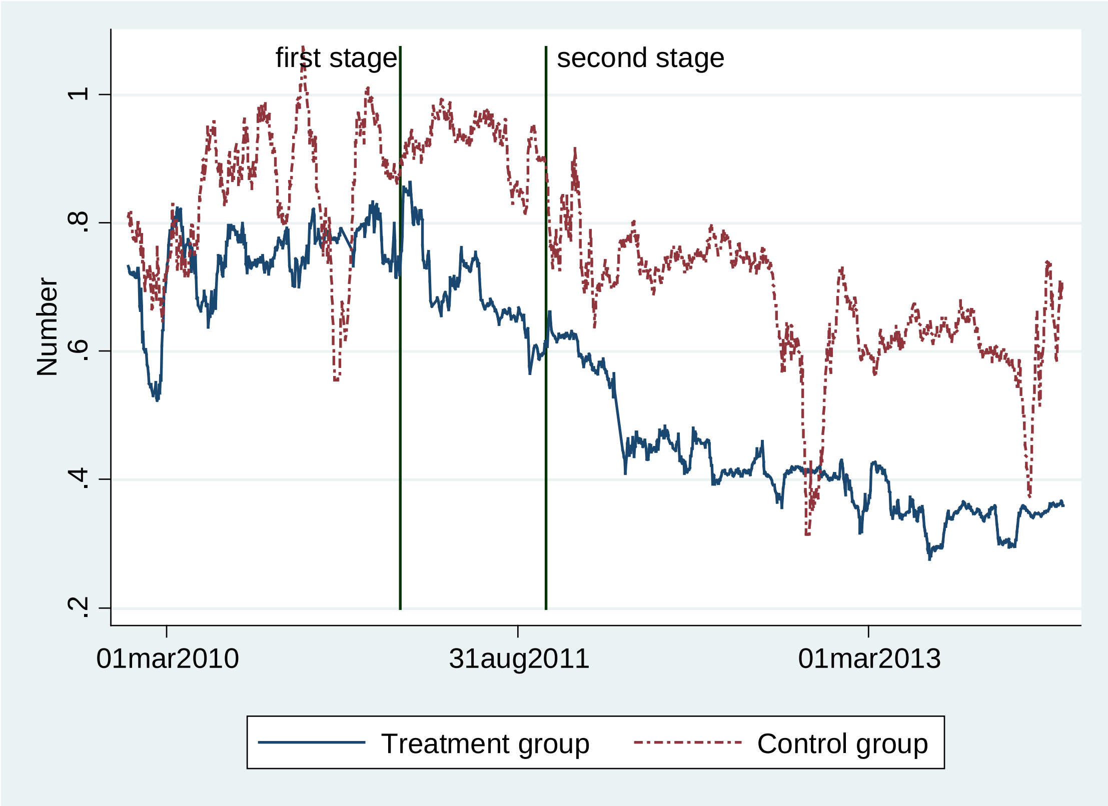

Figure 2-3 Time series of Number by city group and treatment periods

Figure 2 - Time series of outcome variables by city group and treatment periods

This figure depicts the time series for the three outcome variables by city group and treatment periods. We calculate the daily averages of the outcome variables based on the end date of each policy during the study period then plot the 30-day moving averages to facilitate the visual comparison. The moving average of the outcome variable at date d is $Accident(d) = 1/31\sum_{i=-15}^{15} Accident(d+i)$, where $Accident$ is measured by Once, Twice, and Number, and i is a count number.

4.2 Multivariate results

The effects of the reform are 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 controls 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 with the exception of model (3) for twice without control variables (only at 10%).

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 the new pricing on past claims are not significant. Regarding vehicle controls, results show that the age and value of the vehicle, and the insurance premium paid negatively affect the accident frequency; 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.

From Figure 2 we see a decreasing time trend both for the treatment group and the control group at the macro level. By assuming that the day fixed effects are equal across the study period, we now use a linear time trend, λt, to replace the day fixed effects, η_d in Equation 1. The results are reported in Table 7. After comparing Table 6 and Table 7, we find that the results in Table 7 (linear time trend) confirm the results in Table 6 (day fixed effects). The results in Table 7 indicate bigger magnitude and statistical significance for the second-stage reform. Figure 3 depicts the averages of the fitted values (according to the end date of each insurance policy) of Once, Twice, and Number based on regressions in model (1), (3), and (5) in Table 7. We observe significant and consistent effects of the second-stage reform. For the first-stage reform, the effects work only for Twice (without control variables) and Number (wrong sign), as observed in Table 7.

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. *** indicates 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	212,680	212,680	212,680	212,680	212,680	212,680
Number of Vehicles	64,045	64,045	64,045	64,045	64,045	64,045

Table 7 - Effects of insurance incentives on accident frequency (with linear time trend)

This table reports the results of the effects of the two-stage reform with the linear time trend variable. The OLS fixed effects model is employed for all specifications based on the Equation 1 with the day fixed effects, η_d, replaced by a linear time trend, λt. 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. *** indicates p<0.001, ** p<0.01, and * p<0.05.

	(1)Once	(2)Once	(3)Twice	(4)Twice	(5)Number	(6)Number
Reform$_1$	-0.001	0.009	-0.010**	-0.003	-0.002	0.020*
	(-0.32)	(1.92)	(-2.93)	(-0.76)	(-0.27)	(2.31)
Reform$_2$	-0.047***	-0.053***	-0.036***	-0.043***	-0.118***	-0.136***
	(-12.49)	(-13.96)	(-12.85)	(-15.29)	(-16.60)	(-19.03)
Vehicle Controls	No	Yes	No	Yes	No	Yes
Linear Time Trend	Yes	Yes	Yes	Yes	Yes	Yes
Vehicle Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Within-R^2	0.038	0.043	0.032	0.04	0.049	0.058
Observations	212,680	212,680	212,680	212,680	212,680	212,680
Number of Vehicles	64,045	64,045	64,045	64,045	64,045	64,045

Figure 3-1 Averages of fitted values of Once by city group and treatment periods

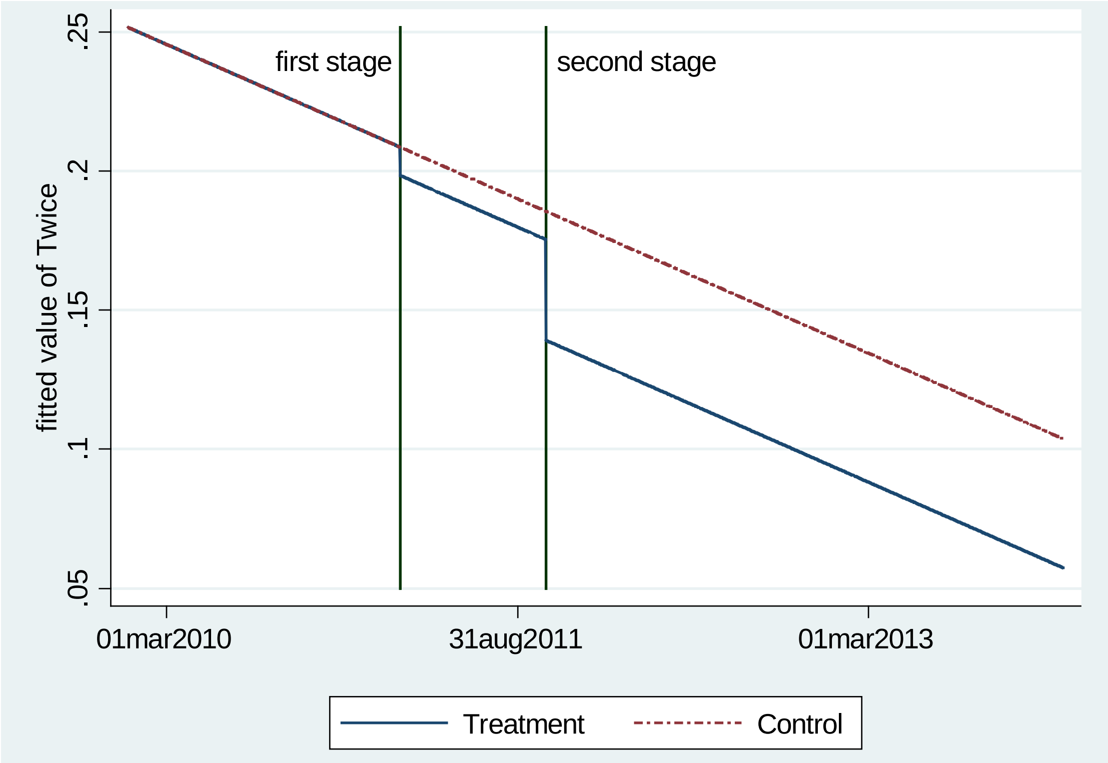

Figure 3-2 Averages of fitted values of Twice by city group and treatment periods

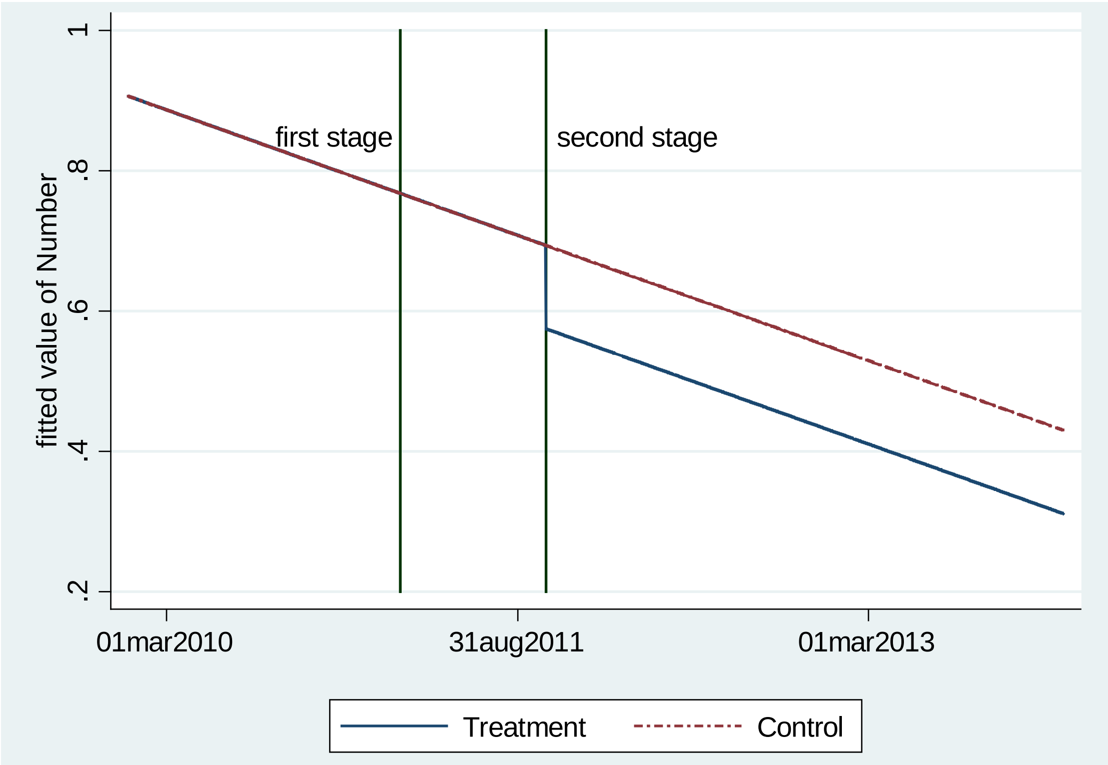

Figure 3-3 Averages of fitted values of Number by city group and treatment periods

Figure 3 – Averages of fitted values of outcome variables by city group and treatment periods

The figure depicts the averages of the fitted values for the three outcome variables by city group and treatment periods based on model (1), (3), and (5) respectively in Table 7. The averages are calculated on the end date of each insurance policy.

4.4 Robustness checks

Value of vehicle

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 question whether 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,[11] 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 regressions in Table 6 for these two groups. The results for the low value group are reported in Table 8.1 and those of the high value group in Table 8.2. 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 (the three models without control variables).

After comparing the magnitude and the statistical significance for the second-stage reform in tables 8.1 and 8.2, we find that the low value group seems more responsive than the high value group. To obtain more evidence we include, in an additional regression, a dummy variable that equals 1 when the value of the vehicle is higher than or equal to the median and an interaction of the dummy and Reform2 variable to rerun the OLS regressions. The results (available from authors) show that the null hypothesis of no statistical difference can be rejected at the 0.1% level for Twice and Number. We therefore conclude that the low value group (less wealthy

[11] 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.

people) respond more to the second-stage reform than the wealthy people do, which is partly consistent with Bar-Ilan and Sacerdote's (2004) results.

Domestic vehicles

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 9. The results are fairly consistent.

Table 8.1 - Robustness check: Effects of insurance incentives on accident frequency of low value group

Based on the median 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 8.1 and 8.2 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. *** indicates p<0.001, ** p<0.01, and * 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	98,960	98,960	98,960	98,960	98,960	98,960
Number of Vehicles	29,834	29,834	29,834	29,834	29,834	29,834

Table 8.2 - 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	113,720	113,720	113,720	113,720	113,720	113,720
Number of Vehicles	34,211	34,211	34,211	34,211	34,211	34,211

Table 9 - 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. *** indicates $p<0.001$.

	(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	205,055	205,055	205,055	205,055	205,055	205,055
Number of Vehicles	61,713	61,713	61,713	61,713	61,713	61,713

Age of vehicle

We use the median 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 10.1 and for the high age group in Table 10.2. 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 4,600 yuan, compared with 4,100 yuan for the older ones. Therefore, obtaining a high multiplicative malus factor is more costly for newer vehicles.

Regular automobile

Table 5 shows that most of the vehicles are regular automobiles (Type 3) 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 11. Once again, we see (with few exceptions and without control variables) the consistently significant effects of the second-stage reform on accident frequency measured by Once, Twice and Number.

Less than three claims

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 12. The previous conclusion is confirmed once again by the subsample analysis, meaning that the incentive effects of the reform are also significant for low-risk owners.

Moral hazard

Up to now, we have controlled for the vehicle type in order to limit the adverse selection effect. However, bad drivers can leave the market following a large increase in their insurance premium, particularly in the treatment city. We do not have data on drivers but we know the owners of the vehicles and those who face large insurance premiums may sell their car to new owners who believe they are better drivers and can reduce the premium of the vehicle over time. These changes in ownership may partly explain the results in Table 6 and reduce the pure moral hazard effect.

During our study period 382 vehicles change owners. We delete them to run the robustness check shown in Table 13. The results in Table 6 are confirmed again with the sample of the same vehicle owners before and after the reform. This result reinforces the interpretation that the reform effects are mainly related to a reduction in moral hazard because the two populations of owners (not only of vehicles) are identical before and after the reform in Table 13.

Other events: Effects of the subway

The occurrence of some unobservable events before and after the reform in the treatment city may influence the outcome variables, which can hinder the objectivity of the evaluation 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-second-stage-reform years of our study. One exception may be several subway lines that were put into use in the treatment city; drivers may choose to take the subway instead of drive, which will surely reduce the accident frequency. Line 2 opened on Dec 28, 2010 (during the pre-reform period) and Line 3 and 5 on Jun 28, 2011 (after the first-stage reform and before the second-stage reform). We run a similar regression as in Table 6 to see whether the pre-reform accident frequency of the treatment city was affected by Line 2 and the results are reported in Table 14. We do not find any significant effects. In addition, considering that the effects of the first-stage reform are insignificant and that Lines 3 and 5 were introduced during the post-first-stage-reform period, we believe that the effects of the new subway lines on road safety are negligible. We therefore conclude that the pricing reform based on traffic violations is the only reason that the accident frequency of the treatment city deviates from the common trend of the two cities.

Non-linear models

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 15 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 10.1 - Robustness check: Effects of insurance incentives on accident frequency of low age group

According to the median 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 tables 10.1 and 10.2 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. *** indicates p<0.001 and * 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	153,998	153,998	153,998	153,998	153,998	153,998
Number of Vehicles	46,304	46,304	46,304	46,304	46,304	46,304

Table 10.2 - 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	58,682	58,682	58,682	58,682	58,682	58,682
Number of Vehicles	17,741	17,741	17,741	17,741	17,741	17,741

Table 11 - 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. *** indicates p<0.001 and * 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	180,946	180,946	180,946	180,946	180,946	180,946
Number of Automobiles	54,436	54,436	54,436	54,436	54,436	54,436

Table 12 - 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. *** indicates p<0.001, ** p<0.01, and * 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	189,856	189,856	189,856	189,856	189,856	189,856
Number of Vehicles	57,386	57,386	57,386	57,386	57,386	57,386

Table 13 - Effects of insurance incentives on accident frequency: sample of same vehicle owner

This table reports the results of the effects of the two-stage reform with 382 vehicles that change owners during the study period, removed from the study sample. 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. *** indicates p<0.001.

	(1)Once	(2)Once	(3)Twice	(4)Twice	(5)Number	(6)Number
Reform$_1$	0.005	0.014	-0.012	-0.006	-0.022	-0.003
	(0.44)	(1.20)	(-1.17)	(-0.60)	(-0.89)	(-0.11)
Reform$_2$	-0.039***	-0.055***	-0.011	-0.031***	-0.057***	-0.108***
	(-5.18)	(-7.30)	(-1.61)	(-4.70)	(-3.62)	(-6.80)
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	0.053	0.062	0.071
Observations	211,392	211,392	211,392	211,392	211,392	211,392
Number of Vehicles	63,663	63,663	63,663	63,663	63,663	63,663

Table 14 - Effects of subway line 2 on accident frequency during the pre-reform period

This table reports the results for the effects of subway line 2 on accident frequency during the pre-reform period (After$_s$ equals 1 when the vehicle was insured after the subway line 2 opened on Dec 28, 2010, otherwise it equals 0; Subway is the interaction of the variables Treat and After$_s$). The OLS fixed effects model (Equation 1) is employed for all specifications with the effects of the first-stage and second-stage reform removed and the effects of Line 2 included. 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
Subway	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	27,677	27,677	27,677	27,677	27,677	27,677
Number of Vehicles	25,119	25,119	25,119	25,119	25,119	25,119

Table 15 - 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); and 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 all the non-linear model specifications. *** indicates $p<0.001$ and * $p<0.05$.

	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	-47,342.021	-47,100.961	-24,161.479	-23,991.701	-92,122.783	-91,840.113	-91,776.1	-91,568.6
Observations	134,630	134,630	72,549	72,549	149,526	149,526	149,526	149,526
Number of Vehicles	40,103	40,103	21,565	21,565	44,771	44,771	44,771	44,771
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 in 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. We find strong behavioral effects arising from the reform. The results show that the addition of an experience-rated premium based on traffic violations reduces the accident frequency significantly (more than 15% for the total number of accidents). This conclusion is robust to the inclusion of vehicle controls, alternative definitions of claim frequency, and several robustness checks. We also find that the effects of improving the experience-rated premium 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 (Panel 1a, Table 1), whereas the second-stage reform is a complete punishment when they accumulate traffic violations. The different 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 has been complete and accurate since the second-stage reform. Insurers are required by law to use this public information only, even if the insured chooses to change insurance companies. There is no possibility of underreporting past traffic violations under the 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.

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