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ORIGINAL ARTICLE

Warranty regulation and consumer demand: evidence from China's automobile market

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Abstract Although government regulations on product warranties are common, few studies have empirically examined their effects on product demand. Using a natural experiment, we examine the effect of a newly introduced government warranty regulation in China's automobile market on product demand. Exploiting the exogenous variation in warranty coverage caused by the regulation, we apply a difference-in-differences analysis and find that the regulation increases the sales of the affected vehicles. Moreover, we find that (1) the demand effects of the regulation decrease as vehicle quality increases, and that (2) these effects are weaker for luxury brand vehicles and stronger for non-luxury vehicles.

Keywords Warranty · Warranty regulation · Automobile market · Natural experiment

JEL Classification L52 · L92 · L98

1 Introduction

Durable goods are often sold with warranties, and government regulations on product warranties are prevalent, as governments and consumer protection agencies aim to protect consumers from product failures with such regulations (Brickey 1978). For example, California law (California Civil Code 900) requires contractors to pro-

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vide at least a one-year warranty for their new home construction and remodeling work. In China, most consumer products are covered by government-mandated repair, replacement and return warranties (the so-called "3R warranties" in China). However, although warranty regulations are common, little empirical research has examined their effects on consumer demand.

Popular wisdom suggests that government warranty regulations should increase consumer demand because they usually result in better warranty coverage for products. Previous studies also show that warranties play an important role in consumers' judgments of product quality (U.S. Department of Commerce 1992) and potentially positively affect consumer demand (Shimp and Bearden 1982). However, market evidence suggests that consumers may *not* always value better warranty coverage to the degree that the government or firms have expected. For example, in March 2015, General Motors (GM) cut the powertrain warranty coverage on its Chevrolet and GMC vehicles in the North American market from 5 years/100,000 miles to 5 years/60,000 miles. Regarding the main reason for this decision to reduce the warranty coverage, Steve Hill, GM North America Vice President, noted that "warranty coverage does not rank high as a reason to purchase a vehicle among buyers of non-luxury brands".[1]

Meanwhile, the effects of warranty regulations on consumer demand, if any, may be heterogeneous. First, such effects may vary across products, as market evidence also suggests that consumers may value warranty coverage differently for different brands. For example, as cited in Guajardo et al. (2012), the market research firm CNW Marketing Research conducted a survey among potential vehicle buyers in 2006, which showed that 54.5 % of potential buyers of Hyundai brand vehicles considered warranty length "extremely or very important", while only 28.4 % of the potential buyers of Toyota held the same opinion of warranties. Second, the demand effects of warranty regulations may vary across consumers, as different consumers may have different risk attitudes; therefore, consumers may have different valuations of product warranties (Kubo 1986; Matthews and Moore 1987; Padmanabhan and Rao 1993). For example, higher-income consumers have generally been shown to buy shorter warranties because of their lower level of risk aversion (Chu and Chintagunta 2011).

In this paper, motivated by the anecdotal evidence discussed above, we aimed to empirically investigate the effect of government warranty regulations on product demand. Specifically, we explore three questions: first, whether such regulation, which engenders better warranty coverage for products, leads to increased consumer demand; second, how the demand effects of warranty regulation vary across products with different levels of quality; and, third, how the demand effects of warranty regulation vary between luxury (e.g., BMW and Lexus) and non-luxury vehicles (e.g., Toyota and Ford).[2]

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[1] See the news article "GM slices warranties, free service on Chevy, GMC" in *USA Today*. http://www.usatoday.com/story/money/cars/2015/03/12/gm-chevrolet-gmc-warranty-service-cut-reduced/70210660/.

[2] We examine whether high-quality vehicles (i.e., vehicles with lower Initial Quality Survey (IQS) scores) tend to have higher prices in our dataset. To examine this, we run the following hedonic price regression: $\ln(p_{jt}) = \omega + \sigma IQS_{jt} + \varphi x_{jt} + \mu_{jt}$, where p_{jt} is the manufacturer-suggested retail price (MSRP) of vehicle j at time t; ω is the constant; IQS_{jt} is the IQS score for vehicle j, which we use as a proxy for vehicle j's quality; x_{jt} is the vector containing other observed vehicle characteristics for vehicle j (as listed

We examine the effects of government warranty regulations on consumer demand in the context of China's automobile market. We take advantage of a natural experiment arising from the Chinese government's introduction of a warranty regulation on all new passenger vehicles that took effect on October 1, 2013. The regulation requires all new passenger vehicles sold in China on and after October 1, 2013 to be covered by a basic warranty with a minimum coverage of 3 years/60,000 km, whichever occurs first. China's implementation of the warranty regulation in the automobile market offers a unique opportunity for our research purposes, as it provides exogenous changes in the warranty coverage of some vehicles, but not others, and thus allows us to establish causality between the warranty regulation and product demand.

Our data include monthly new vehicle sales and vehicle characteristics at the vintage-model level (e.g., 2011 generation Toyota Camry) in China from January 2012 to September 2014. Utilizing the panel nature of the data, we then adopt the difference-in-differences (DD) approach by exploiting the fact that the regulation affected only vehicle models with unqualified warranties (i.e., less than 3 years/60,000 km) prior to the regulation.

Three main results emerge from our analysis. First, the vehicle warranty regulation in question has indeed boosted the demand for the affected vehicles. Quantitatively, such demand effects range from 5 to 6 % for different econometric specifications. This finding is consistent with the insurance role of warranties for consumers (Heal 1977), as the warranty upgrade mandated by the regulation provides consumers with further insurance against product failures and thus increases demand. Second, the demand effect of the regulation decreases as vehicle quality increases. That is, conditional on vehicles' luxury status (i.e., a proxy for consumer income),[3] the demand effects are stronger for lower-quality vehicles and weaker for higher-quality vehicles. This finding further reveals that the effects of the regulation on vehicle demand arise through the insurance channel, as the mandatory warranties provide more assurance and, in turn, economic value for consumers who purchase lower quality vehicles. Third, the demand effect of the regulation is stronger for non-luxury vehicles but weaker for luxury vehicles. This finding is consistent with the fact that, on average, consumers of luxury vehicles have higher incomes and thus tend to have lower levels of risk aversion. Therefore, they have lower valuations of the warranty upgrade introduced by the regulation.

Our research contributes to the small yet nascent literature on product warranties by being, to the best of our knowledge, among the first to empirically investigate the causal effects of a warranty regulation on consumer demand. We also explore the regulation's heterogeneous effects on vehicles of different levels of quality and on vehicles in different product segments (i.e., luxury vs. non-luxury vehicles). Moreover, based

Footnote 2 continued
in Table 1). u_{jt} is the error term that captures the unobserved vehicle characteristics that may affect vehicle prices. The estimation result gives the point estimate of σ 0.012, with standard error 0.694, suggesting that there is no systematic difference in prices across vehicles with different levels of quality. In other word, our analysis of heterogeneous effects of the warranty regulation among luxury and non-luxury vehicles is *not* confounded with the effects of the warranty regulation on vehicles with different quality levels.

[3] Because a vehicle's luxury status does not change over time, vehicle vintage-model fixed effects in our regressions control for the demand effects of vehicles' luxury status.

on a natural experiment, our approach to empirical analysis stands in stark contrast to an alternative approach that merely explores differences in sales among products with different warranty coverage. With this alternative approach, examining the causal effects of product warranties would be difficult because unobserved product heterogeneity may be correlated with products' warranty coverage. Incorporating product fixed effects is a way to address this issue, but it may produce little, if any, variation in the warranty coverage measure, as product warranty coverage usually changes very infrequently.

The rest of the paper proceeds as follows: Section 2 reviews the related literature. Section 3 illustrates the industry background and data. Section 4 introduces our empirical method. Section 5 presents our empirical results, and Sect. 6 concludes.

2 Related literature

Our paper is related to two streams of literature. The first stream concerns the empirical analysis of product warranties (see Lazar 2014 for a comprehensive review). Menezes and Currim (1992) use a sales response model to explore the explanatory power of several product attributes for warranty length. Chu and Chintagunta (2009) quantify the economic value of warranties in the U.S. computer server market and find that product warranties benefit manufacturers, channel intermediaries and consumers. Choi and Ishii (2010) investigate the extent to which warranties affect consumer choice and find that consumers may *not* always value longer warranty durations. Guajardo et al. (2012) investigate the impact of service attributes on consumer demand for automobiles in the U.S. and find that after-sale service quality also affects consumers' valuations of product warranties.

Our paper contributes to this stream of literature in several ways. First, we investigate the demand effects of a government-mandated warranty regulation, which has not been examined so far. Second, we examine the heterogeneity in the demand effects of the warranty regulation among vehicles of different quality levels and among vehicles in different product segments (i.e., luxury and non-luxury vehicles), which has received little attention in the literature. Third, our research context (i.e., a government-mandated warranty regulation) provides exogenous changes in the warranty coverage of some vehicles, but not others, and thus naturally solves the problem that the warranty coverage may be endogenous.

Our paper is also related to the literature on the economic roles of product warranties (see Emons 1989 for a comprehensive review). Although theoretical works on the economic roles of product warranties are extensive, very few empirical works have examined the roles of product warranties. On notable exception is Chu and Chintagunta (2011), who empirically test the different economic roles of warranties in U.S. automobile and computer server markets.

We add to this stream of literature by examining whether and how the insurance function of product warranties plays a role in driving the demand effects of the warranty regulation in question. The insurance theory of warranties suggests that warranties protect consumers from future product failures and function as a risk-sharing mechanism between consumers and manufacturers (e.g., Heal 1977). Therefore, we rely on two major predictions of the insurance theory of warranties to test this theory in our context. First, consumers' valuations and, in turn, the demand effects of product warranties should decrease as product quality increases because higher-quality products have lower probabilities of product failure. Second, the demand effects of product warranties should also decrease as the product's level of luxury increases, as consumers of luxury (non-luxury) products tend to have higher (lower) incomes and thus tend to have lower (higher) levels of risk aversion. Guided by these two predictions, we examine the heterogeneous demand effects of the warranty regulation among vehicles with different levels of quality and between luxury and non-luxury vehicle brands.

3 Industry background and data description

3.1 Industry background

In China, the General Administration of Quality Supervision, Inspection and Quarantine (AQSIQ) is responsible for issuing and enforcing warranty regulations for most consumer products. For example, AQSIQ requires that manufacturers of most consumer electronic products offer a minimum 2-year basic warranty and that most home appliances offer a minimum 1-year warranty on the entire unit and a 3-year warranty on its major parts.

Surprisingly, China's automobile market has not been covered by a warranty regulation until very recently, although it has been the largest market in the world based on both production and sales since 2008 and has more than 50 competing manufacturers (Li et al. 2015).

In response to this lack of regulation, AQSIQ introduced a vehicle warranty regulation that took effect on October 1, 2013, under which all new vehicles sold in China on and after October 1, 2013 were required to be covered by a minimum 3-year/60,000-km basic warranty; in addition, vehicle models with unqualified basic warranties (i.e., less than 3 years or 60,000 km) should at least upgrade their warranties to the minimum standard. The regulation drew considerable media attention[4] and affected many vehicle manufacturers. To contextualize the regulation's impact, 14 manufacturers had to upgrade the basic warranties on their vehicles under the regulation, and they collectively sold 6.24 million vehicles from October 2013 to September 2014, one year after the regulation was introduced. By contrast, the total vehicle sales during the same period were 16.96 million. Therefore, approximately 37 % of the vehicle sales were affected by this regulation.

Finally, the regulation has been strictly enforced. For example, in 2014, a nationwide survey of vehicle dealers conducted by AQSIQ showed that more than 80 % of the vehicle dealers explicitly informed customers about the new warranty regulation and that more than 90 % of the dealers performed after-sales services based on the new regulation.[5]

[4] For example, all major web portals in China (e.g., sina.com.cn, sohu.com) established special columns on this issue.

[5] See the article on AQSIQ website: http://zlgls.aqsiq.gov.cn/cpzl/201412/t20141231_429384.htm (in Chinese).

3.2 Data description

We obtained our data on new vehicle sales, vehicles' manufacturer-suggested retail prices (MSRPs)[6] and other characteristics from R. L. Polk & Company, the world's leading market research firm that specializes in the automobile market. Our data set contains information on the monthly sales and specifications of all vehicle models sold in China at the vintage-model level (e.g., the 2012 generation Toyota Camry) from January 2012 to September 2014, with 69,464 observations and 2185 distinct vintage-model combinations.[7]

Our product quality measure for a particular vehicle model is based on J.D. Power's Initial Quality Study (IQS) scores, which track the number of owner-reported problems per 100 vehicles in the first 90 days of new vehicle ownership. The problems are defined as "problems that have caused a complete breakdown or malfunction, or where controls of features may work as designed, but are difficult to use or understand". We obtain J.D. Power IQS scores for the years 2012, 2013 and 2014 for each vehicle model in China. For example, in 2012, the best vehicle model in the SUV segment in China was the Toyota RAV 4, with 59 problems per 100 vehicles, while in 2014, the best SUV model was the Subaru Forrester, with 56 problems per 100 vehicles. We take the negative value of the IQS scores and divide it by 100 to serve as our vehicle quality measure, ensuring that it measures the number of problems per vehicle and that a higher value in our quality measure denotes higher product quality.

We are able to obtain yearly basic warranty information (i.e., years and length) for all of our vehicle models in the data set from SINA Auto.[8] For example, we have the warranty information for the 2012 vintage Toyota Camry for the years 2012, 2013, and 2014. We then identify vehicle models in the treatment group as those with basic warranties of less than 3 years/60,000 km prior to the regulation (i.e., October 2013) and assign those vehicle models that already offered qualified warranties prior to the regulation to the control group. Table 1 presents the separate summary statistics for the two groups' vehicle sales and characteristics.

Comparing the statistics across groups, we find that, on average, vehicles in the treatment and control groups are similar in terms of most vehicle characteristics, except that vehicles in the treatment group have lower MSRPs and less powerful engines than those in the control group.

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6 MSRPs are set by manufacturers and are generally constant across cities and within a given vintage year. The MSRPs of vehicles in China include two types of taxes: a consumption tax, which ranges from 1 % for small-engine vehicles to 40 % for large-engine vehicles, and a value-added tax of 17 %.

7 In fact, our data set provides vehicle sales and characteristics at the *trim* level (e.g., 2011 vintage BMW 528Li vs. 2011 vintage BMW 535Li sedans) for each vintage-model vehicle (e.g., 2011 vintage BMW 5 series sedan). Therefore, in our analysis, we actually use the more detailed *vintage-trim*-level data. However, to follow industry conventions, we still refer to our data as being at the *vintage-model* level, even though they constitute a much more detailed data set.

8 See http://auto.sina.com.cn.

Table 1 Summary statistics by groups

	Treatment group				Control group			
	Mean	SD	Minimum	Maximum	Mean	SD	Minimum	Maximum
Warranty duration (in years)	2.76	0.34	2	4	3.89	0.61	3	7
Warranty length (in ’0,000 km)	6.62	1.82	3	10	8.22	1.79	6	10
Monthly sales	577.76	1786.94	1	11890	615.76	2057.65	1	70593
Vehicle quality ($-1\times$ number of problems per vehicle)	−1.69	0.44	−2.77	−0.04	−1.74	0.58	−2.93	−0.05
Fuel cost (RMB per kilometer, 2008 level)	0.59	0.12	0.35	1.40	0.64	0.17	0.31	1.55
Vehicle MSRP (in ’0,000 RMB, 2008 level)	17.79	31.32	2.79	530	23.65	39.44	3.20	388
Weight (kg)	1574.46	673.25	1096	2950	1497.58	880.35	1178	3450
Length (mm)	4446.40	469.44	3173	5256	4530.38	454.38	3278	6934
Height (mm)	1566.84	174.1363	1407	1980	1535.27	146.54	1309	1945
Width (mm)	1749.51	204.86	1534	2149	1750.50	277.47	1599	2216
Wheelbase (mm)	2649.58	204.83	1790	3430	2688.20	207.86	1867	3900
Horse power (kw)	140.11	61.48	58	659	175.01	91.66	48	700

The number of observation is 31,392 for the treatment group and 38,072 for the control group. There are 943 vehicle models at the vintage-model level in the treatment group, and 1242 vehicle models at the vintage-model level in the control group. Vehicle quality is the negative of J.D. Power’s Initial Quality Study (IQS) score divided by 100, which measures the negative of number of problems per vehicle. Vehicle MSRPs are vehicle prices set by manufacturers and are constant within a vintage-model combination
SD standard deviation, *MSRP* manufacturer suggested retail price

4 Empirical method

The goal of our empirical analysis is to estimate the average effect of the vehicle warranty regulation on the sales of vehicles that are subject to this regulation (i.e., the treatment group). The key to obtaining the treatment effect's magnitude is to construct the counterfactual outcome, that is, the sales of the vehicles that are not subject to the regulation. If the government had randomly chosen the vehicle models that were subject to the warranty requirement, one could just compare the sales of the vehicle models affected with those unaffected by the regulation to determine the treatment effects. Without randomized regulation assignment, we adopt the DD method by taking advantage of the panel nature of our data. In particular, using the vehicle models unaffected by the warranty regulation as the control group, the DD method compares the differences in sales in the treatment group before and after the regulation with the same differences in the control group. By focusing on changes instead of levels, the DD method controls for time-invariant vehicle characteristics that could correlate with the manufacturers' decisions to provide warranties and affect vehicle sales. By comparing changes in the two groups, we can also control for time trends in vehicle sales, which are the same across the two groups.

Table 2 provides a descriptive analysis of the effect of the warranty regulation on log monthly new vehicle sales. Specifically, the first row of Table 2 shows the average log monthly new vehicle sales during the pre-regulation period (i.e., January 2012 to September 2013) for vehicles in the treatment and control groups; the second row shows the average log monthly sales during the post-regulation period (i.e., October 2013 to September 2014) for vehicles in the treatment and control groups. The third row of Table 2 shows the within-group difference between pre- and post-regulation periods for treatment and control groups. Finally, the last row shows the DD results using the within-group differences. Because we compare log vehicle sales, this difference indicates that the warranty regulation has increased vehicle sales by approximately 4.8 %, with a standard deviation of 2.5 percentage points.

Table 2 suggests that the warranty regulation in question has had a positive effect on vehicle sales in the treatment group. However, other confounding factors may also affect vehicle sales. Therefore, in the subsequent analysis, we conduct a formal

Table 2 Changes in log monthly vehicle sales before and after the regulation

	Vehicles by groups	
	Treatment group	Control group
Pre-regulation average log monthly sales	3.701	3.588
	(0.021)	(0.017)
Post-regulation average log monthly sales	3.793	3.633
	(0.017)	(0.014)
Change in average log monthly sales	0.092	0.044
	(0.023)	(0.021)
Difference-in-difference	0.048 (0.025)	

Pre-regulation period is from January 2012 to September 2013, and post-regulation period is from October 2013 to September 2014. Standard errors are in the parenthesis econometric analysis controlling for other potential confounding factors to accurately quantify the effects of the regulation on consumer demand.

The starting point of our empirical strategy involves specifying the DD method by using the following two-way fixed effect linear regression model:

$$\ln(q_{jt}) = c + \alpha d_{jt} + \beta x_{jt} + \delta_j + \eta_t + \varepsilon_{jt} \tag{1}$$

where j denotes the vehicle (i.e., a vintage-model combination) and t denotes the time (i.e., a month). q_{jt} is the sales of vehicle j at time t. d_{jt} is a dummy variable, which equals 1 if vehicle j is affected by the warranty regulation in question at time t and 0 otherwise. x_{jt} is a vector of the observed vehicle characteristics. δ_j is a set of vehicle vintage-model fixed effects that control for time-invariant demand factors at the vehicle vintage-model level; η_t is a full set of time fixed effects that control for demand variations across different periods; c is the constant term; and ε_{jt} is the idiosyncratic error term.

As an industry practice, vehicle manufacturers usually keep vehicles' observed characteristics (e.g., vehicles' powertrains and dimensions) and unobserved characteristics (e.g., vehicles' safety equipment) constant between two consecutive vintages for a vehicle model (Klier and Linn 2010). For example, the vehicle characteristic (observed and unobserved) of the 2006 generation Toyota Camry remained the same until the next generation Camry was introduced into the market in 2012.[9] Therefore, the vintage-model fixed effects δ_j in our empirical model absorb most of the observed (e.g., engine power, length, width, height, and weight) and unobserved vehicle characteristics (e.g., safety equipment and handling performance).

One concern related to our price variable is that, in our data, we only have vehicles' MSRPs, but *not* the actual transaction prices that are negotiated between vehicle dealers and customers. Vehicles' MSRPs are set by manufacturers and are constant over time within a given model-vintage combination. Therefore, the effect of vehicles' MSRPs on consumer demand is absorbed into vintage-model fixed effects δ_j in our empirical model. Indeed, actual transaction prices may increase for vehicles whose warranties were upgraded under the regulation, which is the profit-maximizing response by vehicle manufacturers to the increased costs associated with the required warranty upgrade or to the added economic value that the upgraded warranty has brought up. In this case, using MSRPs in the place of actual transaction prices would *underestimate* the impacts of the warranty regulation on sales, as the increases in actual transaction prices would reduce sales. Therefore, our estimated impacts of the warranty regulation on sales are actually the *lower* bounds of the actual effects.

Moreover, in our data, vehicles' warranty coverage is also very stable within a given vintage model, and nearly all the warranty coverage changes in our data are

[9] Nevertheless, manufacturers can introduce certain upgrades within a vintage-model combination, such as powertrain upgrades or an exterior "facelift", and if such upgrades occur, our data set treats the upgraded model as a new vintage.

attributable to the introduction of the regulation in question.[10] Therefore, the vintage-model fixed effects δ_j are able to capture the effects of warranty characteristics (i.e., the years and the kilometers that the warranty covers) on consumer demand, and the treatment dummy d_{jt} should capture the effects of the warranty regulation on vehicle sales. Finally, in x_{jt}, the time-variant vehicle characteristics within a vintage-model combination, we include vehicles' fuel costs per kilometer and quality scores because the former fluctuates monthly according to gasoline prices and the latter changes annually with each vehicle vintage model.

In our empirical setup, the OLS estimate $\hat{\alpha}$ can be used to estimate the average treatment effect on the treated group. However, the unbiasedness and consistency of $\hat{\alpha}$ still rely on two additional assumptions. First, the time trends in vehicle sales, η_t, should be the same between the treatment and control groups. Given that vehicles in the treatment and control groups exhibit nontrivial differences for certain characteristics (e.g., vehicle MSRPs and engine power), this assumption may not hold. Because we have vehicle sales data in multiple periods before the regulation was introduced, we can test whether the pre-regulation time trends are the same for the vehicles in both groups (Ashenfelter 1978; Heckman and Hotz 1989; Meyer 1995). If they are the same, we can reasonably assume that the time trends would have remained the same after the regulation was introduced. Many previous studies have used this testing strategy of parallel pretreatment time trends between control and treatment groups (e.g., Eissa and Liebman 1996; Galiani et al. 2005). This strategy specifically involves estimating a modified version of Equation (1), in which we add an interaction term between a time trend and a treatment group indicator with only pre-regulation data. The results presented in the next section show that we cannot reject the hypothesis that pre-regulation time trends are the same for the two groups.

As a further step to control for possible different time trends in sales across different vehicle models, we also estimate a random trend model as an alternative specification:

$$\ln(q_{jt}) = c + \alpha d_{jt} + \beta x_{jt} + \delta_j + \eta_t + \mu_j t + \varepsilon_{jt} \tag{2}$$

where the coefficient on time, μ_j, is different across vehicle models j. The random trend model can accommodate more flexible time trend patterns in vehicle sales, as it controls for any systematic differences in the time trends across vehicle models. To estimate Eq. (2), we take the first difference and obtain the following transformed equation:

$$\Delta\ln(q_{jt}) = \alpha\Delta d_{jt} + \beta\Delta x_{jt} + \Delta\eta_t + \mu_j + \Delta\varepsilon_{jt} \tag{3}$$

where the vintage-model fixed effects δ_j in Eq. (2) are differenced out. Because the first differencing produces a new set of vintage-model fixed effects μ_j in Equation (3), we adopt a fixed-effect estimation method.

The second assumption for the consistent identification of the DD model is that the introduction of the warranty regulation is mean independent of the error term ε_{jt} in

[10] Only 47 of 2185 vintage-model combinations in our data changed their warranty coverage, which were not required by the regulation.

Equation (1). This assumption could be violated if the regulation were introduced in response to factors for vehicle sales that are *specific* to the treatment group. However, such a violation would be unlikely to arise because the regulation was introduced to set warranty standards for the *entire* automobile market in China, not merely vehicles in the treatment group.

With the aforementioned assumptions, our empirical model can be consistently estimated using OLS. However, Bertrand et al. (2004) show that conventional OLS standard errors in DD estimations may lead to biased standard errors (i.e., it may seriously understate the standard deviations of the treatment effects) if the error term ε_{jt} exhibits serial or spatial correlation. To address this issue, we follow Bertrand et al. (2004) in using the block bootstrap method to construct standard errors. Specifically, we draw, with replacements, the entire time series of observations for all the vehicles (i.e., vintage-model combinations) from the same parent brand (e.g., Toyota and Ford). This method considers both the serial and the spatial correlations across different vehicle models from the same parent brand.

5 Estimation results

In this section, we first present the test results for the parallel pre-regulation sales trends between the treatment and control groups. Then, we present the estimation results for the baseline specification [i.e., Eq. (1)], followed by results from alternative specifications [e.g., Eqs. (2), (3)]. We also provide estimation results from additional robustness checks. Finally, we provide the estimation results of the regulation's heterogeneous effects across vehicles with different levels of quality, as well as between luxury and non-luxury vehicles.

5.1 Testing for parallel pre-regulation trends

In Fig. 1, we present the average monthly vehicle sales in logarithm for the 33 months during our sample period after we remove seasonality from the data. The solid line (the dashed line) depicts the monthly sales average over vehicles in the control (treatment) group, and the vertical line denotes the month (i.e., October 2013) when the regulation was introduced. We can see that the trends for the two groups are stable over time and that average sales have been slightly higher in the treatment group than in the control group since the regulation was introduced.

To statistically test the validity of the assumption of parallel pre-regulation trends, we compare the sales patterns from January 2012 to September 2013—before the warranty regulation was implemented—of the group of vehicle models that were later affected by the regulation ($d_j = 1$) and the group of vehicle models that were not ($d_j = 0$). This comparison can reveal whether systematic differences in sales trends existed between the two groups of vehicles before the regulation was implemented. Specifically, we estimate the following *pre-regulation* equation:

$$\ln(q_{jt}) = c + \delta_j + \phi(t \cdot d_j) + \beta x_{jt} + \eta_t + \varepsilon_{jt} \tag{4}$$

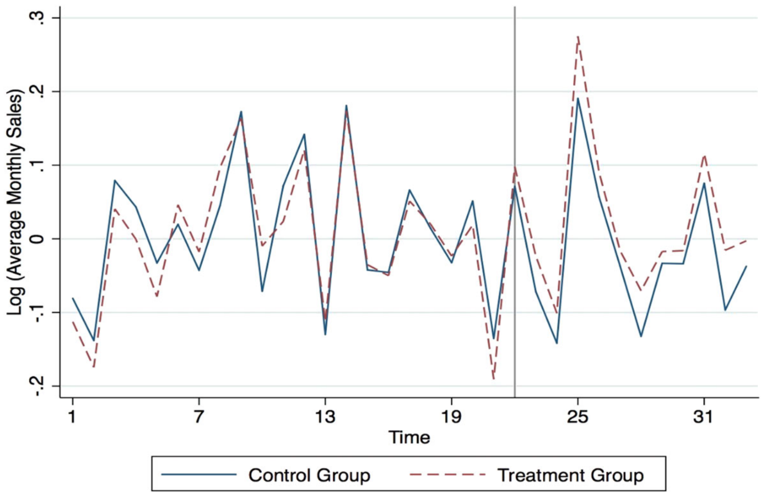

Fig. 1 Average monthly new vehicle sales in logarithm without Seasonality Jan. 2012-Sept. 2014. This figure depicts the average monthly sales of new vehicles after removing seasonality at the vintage-model level in the control and treatment group, respectively

In Eq. (4), the vehicle model-vintage fixed effect δ_j controls for any systematic difference in sales *levels* among different vehicle models.[11] ϕ controls for any systematic difference in sales *trends* between the two groups. A significant estimate of ϕ would indicate a systematic difference in sales trends between the two groups of vehicles, thus raising concerns about the validity of the DD approach.

In Table 3, we report the estimation results from Eq. (4). The coefficient estimates of the trend difference ϕ in all specifications are close to zero and statistically non-significant at all conventional levels. In other words, it appears as though the pre-regulation trends are the same between the two groups. Therefore, we are confident that the post-regulation trends likely would have also been the same if the regulation had not been introduced.

5.2 Difference-in-differences results

Table 4 presents the parameter estimates from several specifications of our DD analysis. As discussed before, the vehicle vintage-model fixed effects absorb most of the vehicle characteristics in our data, including vehicles' MSRPs, engine power, length, height, width and wheelbase, as these characteristics do not change within a given vintage-model combination. Therefore, the only variables that are left in x_{jt}, the time-variant vehicle characteristics within a vintage-model combination, are the vehicles' fuel costs per kilometer and quality scores.

[11] Because we use only *pre-regulation* data and there is no within-vehicle change in the treatment status during the *pre-regulation* period, the model-vintage fixed effect δ_j absorbs the effects of the treatment indicator.

Table 3 Testing parallel time trend using pre-regulation data

	Dependent variable: log vehicle sales	
	(1)	(2)
Trend difference (ϕ)	0.011	−0.004
	(0.084)	(0.162)
Vehicle quality score	–	0.384**
		(0.173)
Fuel cost (RMB per km)	–	−3.271***
		(0.293)
Constant	5.843***	4.938***
	(0.022)	(0.030)
Vehicle vintage-model fixed effects	Yes	Yes
Time fixed effects	Yes	Yes
R^2	0.937	0.942
Number of observations	47,552	47,552

This table reports the estimation results from Eq. (4). The data are from January 2012 to September 2013. Standard errors are in the parenthesis, which are from block bootstrap where a block is a vehicle brand (e.g., Toyota and Ford)

***, **, and * denote statistical significance at 1, 5, and 10 % level, respectively

Table 4 Estimation results from the difference-in-differences analysis

	Dependent variable: log vehicle sales		
	(1) Difference-in-differences	(2) Random trends	(3) Propensity score matching
Treatment	0.060***	0.058***	0.050***
	(0.015)	(0.014)	(0.019)
Vehicle quality score	0.358***	0.294***	0.339**
	(0.142)	(0.116)	(0.150)
Fuel cost (RMB per km)	−3.822***	−4.055***	−3.945***
	(0.913)	(0.927)	(0.881)
Constant	3.947***	4.527***	4.296***
	(0.542)	(0.607)	(0.599)
Vintage-model fixed effects	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes
R^2	0.956	0.961	0.963
Number of observations	69,464	69,464	69,464

These are estimation results from the DD analysis. The standard errors are in parenthesis are from block bootstrap where a block is defined as a parent brand (e.g., Toyota and Ford). Column (1) presents the results from our baseline DD specification as in Eq. (1); column (2) presents the results from the random trend specification as in Eqs. (2) and (3); column (3) presents the results from the specification that combines difference-in-differences and propensity score matching

***, **, and * denote statistical significance at 1, 5, and 10 % level, respectively

Column (1) in Table 4 presents the parameter estimates from our baseline specification [i.e., Equation (1)]. The coefficient α on d_j is estimated to be 0.060 with standard error 0.015, which is statistically significant at the 1 % level. Because the demand functions we estimate are all of semi-logarithmic form and our explanatory variable of interest d_{jt} is a dummy variable, $\exp[\hat{\alpha} - \widehat{var}(\hat{\alpha})] - 1$ should give an unbiased and consistent estimate of the effect of the regulation in terms of the percent change in sales, as suggested by Kennedy (1981). By this transformation, the estimates in Column (1) suggest that the warranty regulation increases the sales of affected vehicles by approximately 6.2 percent.

In Column (2), we estimate a random trend model as described in Eq. (2), where vintage-model specific time trends are included in addition to vintage-model fixed effects, and we estimate this model using first differencing as described in Eq. (3). The estimation results resemble the results presented in Column (1): the coefficient α on d_j is estimated to be 0.058 with standard error 0.014, which is statistically significant at the 1 % level. By the transformation described above, such estimates suggest that the sales of vehicles affected by the warranty regulation increased by approximately 5.9 % after the regulation was introduced.

In Column (3), we combine DD analysis with propensity score matching. This specification weights the control group observations so that the control group matches the treatment group in observed vehicle characteristics. For this specification, we first estimate a logit model in which the dependent variable is the treatment dummy and the explanatory variables are observed vehicle characteristics.[12] Then, we then use the estimates from the logit model to predict the probability that any given vehicle model will fall into the treatment group ($\hat{p}$). We subsequently generate weight $\hat{p}/(1 - \hat{p})$ for observations in the control group. With these generated weights, we apply a weighted regression for Eq. (1), and the estimation result is similar to those from the models in Column (1) and (2): the coefficient α on d_j is estimated to be 0.050 with standard error 0.019, which is also statistically significant at the 1 % level. After the transformation, these estimates suggest that the warranty regulation increases the demand for the affected vehicles by approximately 5.1 %.

In all three specifications, the coefficient estimates of other explanatory variables are statistically significant, and they all have intuitive signs. For example, vehicles' fuel costs have a negative impact on sales, while vehicles' quality scores have a positive impact.

5.3 Robustness checks

In this section, we conduct several robustness checks to ensure that our main results are still valid under alternative assumptions.[13] One concern regarding the analysis above is that the public may have known about the release of the warranty regulation before it became effective in October 2013. Therefore, some consumers may have chosen to wait for the upgraded warranty coverage by postponing their vehicle purchase.

[12] The explanatory variables for the log regression are vehicles' MSRPs, weight, length, width, height, wheelbase, fuel cost per kilometer, quality score, and engine horsepower as well as their quadratic terms. The pseudo-R^2 of the logit regression is 0.359.

[13] We thank an anonymous reviewer for suggesting the robustness checks in this section.

We conduct two robustness checks to examine whether such postponing purchase behaviors affect our main empirical results in Table 4.

First, we drop the data from September, October and November 2013—the time window around the implementation of the regulation—to remove the impacts of such postponing effects around the time of the regulation implementation. We estimate our baseline DD model without the data from these three months, and the estimation result is presented in Table 5, Column (1). The coefficient α on d_j is estimated to be 0.052 with standard error 0.020, which is statistically significant at the 5 % level. By the transformation described above, these estimates suggest that the regulation increases consumer demand for affected vehicles by approximately 5.3 % without the observations from this three-month window, thus suggesting that the potential purchase-postponing behaviors around the time when the regulation was implemented do not affect our main results in Table 4.

Second, we examine the impacts of possible purchase-postponing behaviors using the data from the *pre-regulation* periods. We conduct the same DD analysis as in Eq. (1), but we use *pre-regulation* data and assume that the regulation had been implemented in January 2013, which was the time at which the draft of the warranty regulation in question was first released to the public. The result in Table 5, Column (2) shows that the coefficient of the treatment indicator in this specification is close to zero (0.012) and is statistically insignificant, suggesting that the information release before the regulation was actually implemented did not drive consumers to postpone their vehicle purchases.

Another concern regarding our main results in Table 4 is that, although we control for vehicle vintage-model fixed effects δ_j, time fixed effects η_t, and a set of time-variant, model-specific vehicle characteristics x_{jt} in our DD specifications, some other *time-variant* factors that also affect vehicle sales could exist. For example, the vehicle brands that the regulation requires increase their warranty coverage may also use advertising campaigns to increase public awareness of such upgrades. Meanwhile, the number of vehicle dealer shops for a particular brand may also affect the demand effects of the warranty regulation in question because consumers usually bring the vehicles to the dealer shops for the repair work that is covered by the warranty. Therefore, the quality of warranty-related services and, in turn, consumers' valuations of vehicle warranty coverage may be positively associated with the number of dealer shops that each vehicle brand operates.

To account for the time-variant effects discussed above, we collect information on the vehicle manufacturers' advertising expenditures and the number of dealer shops on a yearly basis from the Emerging Markets Information Service (EMIS) dataset, which contains annual data on annual advertising expenditures (in billion RMB, deflated to 2012 level) and the number of dealer shops in each year during our sample period for the *20 largest* vehicle brands (e.g., Ford and Toyota) in China. These 20 brands account for more than 85 % of the market share during our sample period. We add these two additional control variables to our baseline DD analysis, and we present the estimation result in Table 5, Column (3). As expected, the estimation result shows that both vehicle manufacturers' annual advertising expenditures and the number of dealer shops that a vehicle brand operates are positively associated with vehicle sales. More importantly, the result in Column (3) shows that, after controlling

Table 5 Robustness checks

	Dependent variable: log vehicle sales			
	(1) Without data from September, October, and November 2013	(2) Pre-regulation sample, assuming regulation implemented in January 2013	(3) With additional explanatory variables	(4) With brand-time fixed effects
Treatment	0.052***	0.012	0.046**	0.041**
	(0.020)	(0.149)	(0.023)	(0.019)
Vehicle quality score	0.324**	0.389**	0.297*	0.315*
	(0.145)	(0.168)	(0.161)	(0.174)
Fuel cost (RMB per km)	−3.791***	−4.188***	−3.506***	−3.922***
	(0.826)	(0.915)	(0.791)	(0.871)
Log annual advertisement spending (in billion RMB, 2010)	–	–	0.342**	–
			(0.169)	
Log number of dealer shops	–	–	1.628*	–
			(0.959)	
Constant	4.050***	4.335***	3.671***	4.276***
	(0.637)	(0.703)	(0.524)	(0.803)
Vintage-model fixed effects	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	No
Brand-time fixed effects	No	No	No	Yes
R^2	0.945	0.968	0.970	0.973
Number of observations	62,051	47,552	60,273	69,464

These are estimation results from the baseline DD specification as in Eq. (1). The standard errors are in parenthesis are from block bootstrap where a block is defined as a parent brand (e.g., Toyota and Ford). Column (1) presents the estimation result without using data from September, October and November 2013; column (2) presents the estimation result from using only pre-regulation sample and assuming that the warranty regulation had been implemented in January 2013; column (3) presents the estimation result from the specification with additional control variables (i.e., log annual advertisement spending at the parent-brand level, and log number of dealer shops of a vehicle brand; column (4) presents the estimation result from the specification with brand-time fixed effects

***, **, and * denote statistical significance at 1, 5, and 10 % level, respectively

advertising expenditures and dealer shop numbers, the demand effect of the warranty regulation in question is still positive (0.046) and statistically significant at the 5 % level.

Finally, because of the lack of the data, some *time-variant unobserved* heterogeneity for which we cannot directly control may still exist. For example, as a significant proportion of consumers purchase vehicles through financing, the interest rate of auto loans that a vehicle brand offers should also affect consumer demand for vehicles. However, we do not have the data on the interest rates offered by each vehicle brand. Therefore, to control for such *time-variant*, *brand*-level unobserved heterogeneity, we include brand-time fixed effects in our baseline DD specification. Column (4) in Table 5 shows the estimation result from this specification, and it shows that, after

Table 6 Heterogeneous effects by vehicle quality

	Dependent variable: log vehicle sales		
	(1) Difference-in differences	(2) Random trends	(3) Propensity score matching
Treatment	0.068**	0.072***	0.061***
	(0.033)	(0.029)	(0.025)
Treatment × vehicle quality score	−0.032**	−0.048**	−0.036**
	(0.015)	(0.022)	(0.017)
Vehicle quality score	0.235**	0.276**	0.305**
	(0.107)	(0.130)	(0.145)
If during the post-regulation period × vehicle quality score	0.074	0.015	0.104
	(0.532)	(0.924)	(0.572)
If belong to the treatment group × vehicle quality score	0.132	0.089	0.104
	(1.428)	(0.756)	(1.024)
Fuel cost (RMB per kilometer)	−3.962***	−4.496***	−4.287***
	(1.213)	(1.352)	(1.564)
Constant	4.930***	4.561***	4.793***
	(0.619)	(0.724)	(0.715)
Vintage-model fixed effects	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes
R^2	0.980	0.985	0.986
Number of observations	69,464	69,464	69,464

These are estimation results from the DD analysis. The standard errors are in parenthesis are from block bootstrap where a block is defined as a parent brand (e.g., Toyota and Ford). Column (1) presents the result from our baseline DD specification as in Eq. (1); column (2) presents the result from the random trend specification as in Eqs. (2) and (3); column (3) presents the result from the specification that combines difference-in-differences and propensity score matching
***, **, and * denote statistical significance at 1, 5, and 10 % level, respectively

using brand-time fixed effects to control for such *time-variant*, *brand*-level unobserved heterogeneity, the warranty regulation in question still increases consumer demand for the affected vehicles by approximately 4.1 %.

5.4 Heterogeneous effects

If warranty regulations indeed affect vehicle sales, as our previous analysis suggests, the effects of the warranty regulation should vary across vehicles with different quality levels because one of the major functions of warranties is to provide insurance against product failures. Therefore, we examine whether the regulation has heterogeneous effects on vehicles with different levels of quality.

Table 7 Heterogeneous effects: luxury vs. non-luxury vehicles

	Dependent variable: log vehicle sales		
	(1) Difference-in differences	(2) Random trends	(3) Propensity score matching
Treatment	0.079**	0.068***	0.063**
	(0.040)	(0.021)	(0.030)
Treatment × Luxury	−0.047**	−0.040**	−0.034**
	(0.023)	(0.018)	(0.016)
Vehicle quality score	0.265**	0.294**	0.315**
	(0.124)	(0.150)	(0.161)
If during post-regulation period × Luxury	−0.032	−0.016	0.024
	(0.034)	(0.054)	(0.069)
Fuel cost (RMB per kilometer)	−4.075***	−4.611***	−4.287***
	(1.367)	(1.730)	(1.564)
Constant	5.372***	4.861 ***	5.041***
	(0.799)	(0.824)	(0.920)
Vintage-model fixed effects	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes
R^2	0.973	0.978	0.981
Number of observations	69,464	69,464	69,464

These are estimation results from the DD analysis. The standard errors are in parenthesis are from block bootstrap where a block is defined as a parent brand (e.g., Toyota and Ford). Column (1) presents the estimation result from our baseline DD specification as in Eq. (1); column (2) presents the estimation result from the random trend specification as in Eqs. (2) and (3); column (3) presents the estimation result from the specification that combines difference-in-differences and propensity score matching
***, **, and * denote statistical significance at 1, 5, and 10 % level, respectively

For this purpose, we estimate a DD specification in which we add an interaction term between the treatment dummy and the vehicle quality score. We use 2012 vehicle quality scores as the quality measure in the interaction term.[14] The estimation results are presented in Table 6. The coefficient estimates for the interaction term are negative (−0.032, −0.048, and −0.036 in three specifications, respectively) and statistically significant at the 5 % level, thus suggesting that the effect of the warranty regulation on vehicle demand decreases as vehicle quality increases.

On the other hand, the effects of the warranty regulation should also vary across different consumers, as consumers tend to have different levels of risk aversion.[15] In particular, consumers' incomes should play a significant role in determining consumers' risk aversion levels because consumers with higher (lower) incomes are more (less) capable of bearing risks and thus will have lower (higher) valuations of the extended warranty coverage. For this purpose, we examine the differential impacts of

[14] We also estimate specifications with vehicle quality scores in 2013 or 2014 as the quality measure in the interaction term and obtain very similar results.

[15] We thank an anonymous reviewer for suggesting this analysis.

the regulation between luxury (e.g., BMW and Lexus) and non-luxury vehicle brands (e.g., Volkswagen and Toyota). The underlying intuition is that consumers who purchase luxury (non-luxury) brand vehicles tend to have higher (lower) incomes and lower (higher) levels of risk aversion and thus tend to have lower (higher) valuations of the warranty upgrade induced by the regulation in question.

To examine such heterogeneous effects, we estimate a DD specification in which we add an interaction term between the treatment dummy and the indicator of luxury vehicle brands. The estimation result is presented in Table 7, which indicates that the demand effects of the regulation on luxury vehicle brands are approximately 3.5 to 4.8 % smaller compared with those on non-luxury vehicles across different specifications; these estimates are all statistically significant at 5 % level. Therefore, the results in Table 7 suggest that, consistent with the risk aversion argument above, the effect of the warranty regulation on consumer demand is indeed stronger for non-luxury vehicles and weaker for luxury vehicles.

6 Conclusion

Many countries impose warranty regulations on various products to protect consumers against product failures. Such regulations usually provide consumers with better warranty coverage. By exploiting a natural experiment, we examine the demand effects of a government-imposed warranty regulation in China's automobile market. We find that this warranty regulation indeed increases the sales of affected vehicles and that the demand effects of the regulation are stronger for lower-quality or non-luxury vehicles and are weaker for higher-quality or luxury vehicles. This paper is a first attempt to investigate the demand effects of a warranty regulation and the heterogeneity of such effects related to vehicle quality and the vehicle segment (i.e., non-luxury vs. luxury vehicles). Meanwhile, our findings also provide evidence that supports the insurance role of product warranties.

As with any other research, this study is not without limitations. For example, prior research shows that risk preferences are different among individuals from different countries or from different societal and cultural backgrounds. For example, Douglas and Wildavsky (1982) show that individuals in societies in which an individualistic market orientation predominates (e.g., the U.S.) tend to be less risk averse, while individuals from hierarchical and bureaucratic societies (e.g., China) are more risk averse. Furthermore, such differences in the levels of risk aversion between China and Western countries, such as the U.S., may be especially salient in the case of vehicle warranties, as owning a vehicle is less common in China. Therefore, the results that we obtain in this paper may not be generalizable to other countries, and the research on similar issues in other countries or cultural environments is necessary.

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