# Do Vehicle Recalls Reduce the Number of Accidents? The Case of the U.S. Car Market[§]

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The number of automobile recalls in the U.S. has sharply increased in the last 2 decades, and the number of units involved are often counted in the millions. In 2010 alone, over 20 million vehicles were recalled in the United States, and the massive recalls of full model lines by Toyota have brought this issue to the front pages around the country and the world. However, there is no quantitative evidence of the effect of recalls on safety. Without that evidence, the government and insurance companies have been reluctant to request and use more detailed recall information to increase correction rates, and regulators have not studied the possible link between the growing number of recalls and the risk of life for consumers. In this paper we empirically quantify the effect of vehicle recalls on safety using repeated cross-sections on accidents of individual drivers and aggregate vehicle recall data, to construct synthetic panel data on individual drivers of a particular vehicle model. We estimate the effect of recalls on the number of accidents and find that a 10 percent increase in the recall rate of a particular model reduces the accidents of that model by between 0.78 percent and 1.6 percent when using the full sample of accidents in our data. We also find that recalls classified as "hazardous" are more effective in reducing accidents and the recall effect is especially strong when we restrict attention to accidents that lead to personal injuries and only include vehicles more likely to be at fault for the accident, but much less so for accidents that only lead to property damage. We also find that vehicle models with recalls with higher correction rates have on average fewer accidents in the years following a recall, which indicates the importance of the role of drivers' behavior regarding recalls on safety. Our findings suggest that policymakers should consider, for example, policies to allow insurance companies to take into account recall correction behavior when pricing auto insurance, which could be made possible through regulatory changes by the U.S. government, and should revisit the complex trade-offs between pre- and post-market regulation in this important industry.

**Keywords:** Vehicle Recalls, Car accidents, Synthetic Panel Data, Correction Rates, Auto Insurance.

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## INTRODUCTION

The recall system for motor vehicles in the U.S. was first introduced in 1966 to solve potential safety problems and to remove unsafe and dangerous vehicles from the roads.[1] Between 1966 and 2009, the National Highway Traffic Safety Administration (NHTSA) and manufacturers have issued more than 15,063 recalls, and the campaigns have been more active in recent years, especially since 1990. More than 19 million vehicles were recalled in 2003, accounting for about 8 percent of U.S. registered vehicles, compared to 5 percent in 1993 (Power & Lundegaard, 2004). According to the NHTSA, the total number of vehicles recalled increased by 61 percent during 2004 to an all-time high of over 30 million vehicles. Since then the number of recalls has somewhat declined, but during 2009 over 16.4 million vehicles were recalled, and more than 20 million were recalled during 2010.[2] Furthermore, the recent massive recalls by Toyota, which have included full product lines and over 6 million vehicles (in the U.S. alone), have brought the issue to the front pages around the globe, and have prompted some to suggest the need for the government to better regulate the vehicles that manufacturers put on the road. Little is known, however, about the effect of vehicle recalls on the number of accidents, and there has been no quantitative analysis of this link. This link is one of the keys in the policy arena because if the research we present here finds that recalls have an effect on safety, it means that the number of unsafe cars on the road has an effect on the safety of drivers and should be a concern to policymakers.

The arguments regarding the likely effects of vehicle recall on safety problems mainly have come from the manufacturers and the regulatory agents. The NHTSA states that its combined efforts (including recall campaigns, traditional safety regulations, education on safety driving, increase in road safety investment) result in a decrease in accidental harm, but quantitative evidence is hard to come by. Experts who are in favor of solutions by automotive engineering insist that one of the main determinants of the safety problem is the drivers' behavior. Evans (2002) divides the main determinants of safety into two categories: driver behavior and engineering, including recalls. He argues that even though people pay a great deal of attention to the drivers' behavior, more focus still goes to crash-test ratings, product-liability trials, and product recalls. He says that this is misplaced and overemphasized. He also insists that misplaced focus on vehicle factors has encouraged American drivers to regard safety as something out of their control, and the focus on safety equipment, such as airbags, misleads U.S. drivers into the belief that safety can be achieved without action on their part (see Peltzman, 1975, for the seminal work on this argument, and Hause, 2006, for a recent theoretical discussion.). He suggests that policies that can change drivers' behavior or improve road environments are more desirable for safety because recalls do not save any lives. However, he acknowledges that quantitative evidence supporting this latter conclusion has not been developed.[3] Similarly, McDonald (2006) argues that there is over-recall, implying that recalls have little effect on safety, but he doesargue in favor of some of the regulatory policy suggestions we discuss to increase correction rates. However, no empirical analysis of any kind is presented in the book to defend any of the arguments put forward. Our study provides an empirically based framework where it is possible to quantify the effects of recalls on safety.[4]

There are a number of reasons why there are relatively few studies of the effect of recalls on safety. First, recalls are specific to particular vehicle models, which means that each recall is issued for the stock of particular vehicle models that are in use on the road at a particular point in time. When the government or manufacturers find a defect that might cause serious accidents for the particular year models, they have to decide whether or not a recall has to be issued and, if they do, how many units should be included. Recalls are often heterogeneous because each defective unit that might cause accidents has a different risk level. Therefore, the decision on whether to recall a vehicle, the scope, and the range of the defective parts are quite different according to the seriousness of the defects of the models. In addition to these problems, some recall data, such as recall costs, are not available to researchers. And there is no direct link between recall data, accident data and vehicle information, so researchers need to construct this link themselves.

We use a methodology that groups individual drivers by types to produce synthetic panel data to analyze the effect of recalls on the number of accidents. Meanwhile we control for the inherent unobserved heterogeneity of drivers, which is likely to matter a lot in explaining who gets into an accident, which could confound the results in a purely cross-sectional framework.[5] The results show that recalls reduce the number of accidents. Specifically, recalls of a particular model reduce accidents of that model by between 7.8 percent and 16 percent. Which means that a 10 percent increase on the recall rate of a particular model will reduce the accidents of that model by 0.8 percent to 1.6 percent. Recalls classified as “hazardous” are more effective, and the recall effect is especially strong on the number of accidents linked to injuries, a result robust to restricting attention to the vehicles likely to be responsible for the accident. Our findings should not be understood as suggesting that more recalls (on top of the hundreds of recalls of millions of cars already issued every year) would be beneficial, in fact, as we discuss further below, we hope our findings foster a discussion of the trade-offs of the pre- and post-market safety and quality screening in the automobile industry, given that the effectiveness of recalls in terms of the numbers of accidents (and also severity) suggest that additional ex-ante safety controls could be beneficial to society.

We also find that vehicle models with recalls with higher correction rates have, other things equal, fewer accidents in the 3 years after the recall, indicating the importance of the role of drivers' behavior, regarding recalls, on safety. The latter suggests that society as a whole, individual drivers, and insurance companies could benefit from an initiative to take into account correction rates of outstanding and past recalls of the drivers' vehicles when pricing auto insurance.

Official communications between the Insurance Institute for Highway Safety (IIHS) and the NHTSA suggest that it is realistic to believe that this use of the recall information will materialize in the near future. Insurance companies, through the IIHS, have already started to pressure the government to release more information about recalls, such that it is possible to have the Vehicle Identification Numbers (VIN) of the vehicles recalled. This would allow a better monitoring of who fixes their cars and open the door for insurance companies to take it into account in their pricing strategies. In a letter written in November of 2001, the Senior Vice President of the IIHS petitioned the NHTSA to require manufacturers to include the VIN numbers of the vehicles recalled in their recall announcements. This would reverse a decision of the NHTSA of 1986, which accepted a petition by manufacturers to drop this requirement, which had been in place since 1974. As of the completion of our research this petition is still pending, but if granted, our results suggest that the likely increase in correction rates would have a welfare improving effect by reducing accidents.[6]

There are two additional important ways in which public policy can play an important role on these issues. First, a number of legislators have been trying to change certain policies to foster correction rates and information flows to consumers regarding recalls. In early 2005, the California State Senator Debra Bowen (D-Redondo Beach, who is now California's Secretary of State), introduced a bill (SB 114) that would require the Department of Motor Vehicles, when it provides a notice of registration renewal for a vehicle that is subject to a recall, to also notify the registered owner that the vehicle is the subject of a manufacturer's safety-related recall and provide the vehicle recall information. While the bill passed the committee stage and the Senate, it was never implemented. The state senator's office told us that the American car manufacturers were against the measure and considered it quite unfortunate that a simple regulation likely to have an important effect on taking faulty vehicles off the road is not implemented mainly due to the the lack of evidence on its advantages.[7] Notice that the latter is the key, given the cost to manufacturers of a sharp increase in correction rates, with little evidence that this will have any meaningful effect on the safety of drivers and passengers.[8]

The other important, and probably more far reaching, issue in which public policy can play a role comes from the fact that for years (if not decades) manufacturers have apparently felt relatively little pressure to minimize the problems of cars before they are put on the road, since the direct and indirect costs to society of the increasing number of recalls (and the accidents linked to them) seem to be small compared with the likely investments (and loss of revenue due to delays in introducing new models in an ever more competitive, and increasingly complex, industry) needed to reduce defects to a level that would assure a small number of recalls and prevent accidents. The latter is naturally related to the issue of optimal pre-market testing in a given industry and the likelihood of observing errors in the screening process by the regulator, which has considerable tradition in the regulatory literature.[9]

The idea is that the optimal pre-market testing results from minimizing the total cost to society of implementing this testing. The total cost is a function, on the one hand, of the costs to the manufacturers (private costs) to test for vehicle defects, and the costs associated with delaying the introduction of better and safer vehicles, which are an increasing function of the pre-market testing. On the other hand, the total cost is also a function of the expected cost (social cost) for society of allowing unsafe cars to be marketed, which could also include the additional private costs to manufacturers of fixing faulty vehicles (induced by post-market regulation), and be influenced by possible behavioral responses to pre-market regulation that would put very safe cars on the road, leading drivers to be less careful on the roads (Peltzman, 1975). Overall this second set of costs is likely to decrease in pre-market testing, but in general it is more difficult to estimate than the costs from performing pre-market tests (especially the social cost component, which is unlikely to be internalized by profit-maximizing manufacturers except through very tough post-market regulation. The latter way of making producers internalize the externality might be inefficient under uncertainty, as discussed by Kolstad, Ulen, & Johnson, 1990). In fact, our research can be understood as trying to link the possible effectiveness of the current post-market regulation of the automobile market, to the slope of the declining cost function to society associated with pre-market testing. The reason for this link is that if recalls are effective in reducing accidents it is likely that the current pre-market testing is at a lower level than socially optimal, due the fact that it is believed (without much empirical evidence, or due to lack thereof) that some of the costs associated with putting unsafe cars on the road (the social part of these costs) might not be declining in the pre-testing efforts after a certain relatively low level of pre-testing.

The connection between pre-market testing and market outcomes has produced important recent contributions (Carpenter & Ting, 2005; 2007; Carpenter, Grimmer, & Lomazoff, 2009). The latter literature connects pre-market screening with likely errors in terms of the quality of products put in the market, modeling the process as a game of incomplete information between manufacturers and the regulator, which fits well with the structure of the automobile industry. One interesting result of the last set of authors indicates that under plausible assumptions a reliable quality screening process (even if it leads to few actual improvements in safety or quality) could lead to more confidence in the product by consumers and therefore higher sales. These results suggest that even car manufacturers could benefit from a careful revamping of the pre-market screening of vehicles, even if they believe the effects on the average safety and quality of cars put on the road is unlikely to be affected in a significant way. Another even more relevant result of this literature is that if the pre-market experimentation with the new products is short (as is the case with new vehicles given the market pressure to put new models on the road) the approval of unreliable products introduced by larger firms increases, which again seems to be in line with the events we have seen in the automobile industry in the last years.

Clearly, given the incredibly high number of recalls during the 1990s and the first decade of the new centuryin the last years, it is becoming imperative for policymakers to consider whether a deeper reform of the automobile industry's quality control process is necessary. The very pressure to produce more and newer cars (as discussed by Johnson, 2010, and McDuffie & Fujimoto, 2010, and implicitly acknowledged by Toyota's CEO in front of Congress when he said "I have personally placed the highest priority on improving quality over quantity, and I have shared that direction with our stakeholders"), coupled with an apparently lenient system to control what manufacturers put on the roads, has lead to situations in which measures are only implemented when loss of life or property has already occurred. This pre- vs. post-market screening of safety and quality of vehicles system must be revisited, and our work tries to contribute in the direction of showing that the problems for which cars are recalled are serious. Removing recalled cars from the road has real effects, which suggests that not having them on the road on the first place would be a much better outcome for drivers and society as a whole. If the post-market actions reduce the cost to society proxied by the decline in the number of accidents that lead to injuries and property damage, it means additional pre-market testing could have prevented some accidents. From the point of view of manufacturers it is likely too costly to have a system of quality control so effective that a very small number of defective cars are put on the road. However, what seems clear is that the current system in which over 15 million vehicles are recalled every year is consistent with a policy in which the cost of testing the performance of vehicles is in part passed on to consumers instead of quality control engineers, and maybe leading to less trust in the products put on the road resulting in lower likelihood of buying more products.[10]

The remainder of the paper is structured as follows. The second section provides an overview of the recall system in the United States and a short review of the related literature. The third section describes the data. The fourth section discusses the econometric models we estimate. The fifth section presents the empirical results, and the sixth section offers concluding remarks and discusses the policy implications of our results.

## BACKGROUND ON THE RECALL SYSTEM AND THE RELATED LITERATURE

### Definitions and the Recall Process

The National Traffic and Motor Vehicle Safety Act gives the NHTSA the authority to require manufacturers to recall vehicles with safety-related defects that could cause loss of vehicle control such as steering, braking, fire, or repeated stalling. The NHTSA sets a Federal Motor Vehicle Safety Standard to regulate vehicles so that manufacturers have to comply with the standard in order to sell their vehicles in the market. Even if they comply with the standard, if serious accidents occur or are expected because of potential defects, then a recall is required.

Recalls may be either voluntary or mandatory. The government gives manufacturers the opportunity to announce recalls voluntarily. If the manufacturers do not agree with the government's recall decision, they can resolve the disputes in the courts. While this does not seem to happen very often in some cases, it has delayed the recall process considerably. Tobin (1982) already discussed the negotiations that occurr between manufacturers and the governments regarding recall details, given the costs attached to having to fix potentially millions of cars. Regarding the owners of the vehicles, the NHTSA releases information on how to complain when owners think that their vehicles have any safety-related problem. When they are injured in accidents, and think that their injuries are due to those problems, they are asked to file a defect report directly to the NHTSA. Alternatively, they may report the problems to the manufacturers. After recalls are issued, they have to take their vehicles to the places assigned by the manufacturers to be repaired.

The whole recall process is lengthy.[11] For example, a Firestone tire recall that affected some Ford vehicles took more than 7 years from the first report until they reached a final decision. There are many steps before a final decision to recall vehicles is made. Manufacturers may begin their initial recall process once they find some safety-related defects, even if there have been no complaint reports from their customers. This decision can be made based on their own investigative activities, given that they regularly monitor the quality of the vehicles. However, it is vital for either manufacturers or the regulatory agents to receive the complaint reports from the owners to begin the recall process. After a recall issuance, it also takes time to finish all corrective procedures because it depends upon the vehicle owners. The NHTSA requires manufacturers to report the correction rates quarterly. On average, more than 40 percent of owners have not taken any corrective action (on vehicle recalls) by the first 18 months.

The first step of the process, which is taken by the regulatory agency, is known as "screening." Once all the information from the reports is entered into a database, technical staff at the Office of Defects Investigation (ODI) of the NHTSA look at the complaints and check if there is any trend in accidents. If the trend shows an increasing risk of accidents, then the NHTSA starts an investigation.

The next step is called “petition analysis.” However, this does not always follow screening because any person can submit a petition to the NHTSA. For example, an owner who had a serious injury from an accident can write a petition to the NHTSA, when he or she believes that there may have been some defects in the vehicle that may have played a role in causing the accident. The role of the NHTSA is then to decide whether the petition is accepted or denied. Once it is accepted, more detailed investigation is conducted. In the final stage before recall issuance, the ODI sends a notice to manufacturers to give them an opportunity to argue against it or provide new evidence. Manufacturers can issue a recall at any of the stages, which makes the recall voluntary. If the manufacturers do not recall, then the NHTSA investigates further. If the govern ment experts find the safety standard has been violated, the government agency contacts the manufacturer who starts the recall process. In rare occasions the manufacturers fight the decision of the government and bring the issue to court.

Once recalls are announced, the manufacturers send notice letters to their customers and also announce the recall through the media so that the vehicles can be brought in and the defects fixed. The Recall Management Division, part of the NHTSA, monitors the post-recall process.

**Recall Trends**

Recall data with detailed information on all individual recalls since 1966 were obtained from the National Center for Statistics and Analysis (NCSA), an office of the NHTSA. Although we will impose some restrictions in the empirical analysis due to variables not available in the last few years of data, we include all vehicle recalls up to 2009 in the discussion below to analyze the overall trends. The data include the following information on recalls of particular vehicles: model year, beginning and ending dates of manufacturing, potential number of units affected, potential number of units defective, recall initiator, the number of units corrected, hazard category (only up to 2001), the summaries of defects, the possible consequences, and the correction required to eliminate the defects. The number of units affected might be different from the number of units defective, since the latter is the number of units that actually have the problem, after carefully investigating the defects. The NHTSA assigns four different hazard ratings to each recall: A, B, C, and D from the highest to the lowest hazardous recalls. Even though the category B only includes around 5 percent of recalls, here we define the "hazard recalls" as the ones that receive high hazard ratings: A or B, given that many recalls in the latter category are quite serious and are clearly more serious than those included in the other two categories.

Figure 1 shows that the number of recalls has increased significantly over time. In 1966, 58 recalls were issued. In 2000 the number of recalls issued had increased to 631, and by 2008 they were close to 800. The increase is especially sharp since the early-mid 1990s, and the increase in the number of hazard recalls (only shown up to 2001 given data availability) is proportional to the number of all recalls. Since each recall involves a different number of units, we can plot the average number of units per recall over time. Figure 2 shows the annual average units per recall. From the 1960s and up to the late 1970s, the average number of units involved in a recall almost never reached 50,000, and after a couple of episodes of major recalls that trend returned. However, the substantial change occurred in the 1990s when the average number of units rose considerably, and while there has been a slight decline since then, the average number has stayed quite high. Additionally, the average number of units with high hazard ratings (shown up to 2001) clearly increased in the 1990s.

Figure 3 shows the proportion of all total vehicles recalled initiated by domestic manufacturers, which up to the mid 1990s had fluctuated considerably every year moving from highs above 90 percent to as low as 50 percent. The end of the fluctuations has coincided with a sharp decline in the proportion of units recalled initiated by domestic manufacturers. This proportion reached a low point just above 40 percent during the first decade of the new century. In part this reflects the growing importance of foreign manufacturers in the U.S. car market, but also their push towards massive production, which some suggest might come with a lowering of their quality (Johnson, 2010; McDuffie & Fujimoto, 2010), reaching a pinnacle with the ongoing recalls by Toyota. It is interesting to emphasize that a larger proportion of the recalls issued by foreign manufacturers are considered hazardous compared with domestic manufacturers. It is also important to emphasize that in Figure 3 we are not accounting for very small manufacturers that recall a very small number of vehicles The recalls represented in the figure account for almost all the vehicles actually recalled.

Figure 4 shows the evolution of the proportion of recalled vehicles with the process initiated by the government and not the manufacturers. This proportion has fluctuated considerably over time, and since early 1990s, has consistently reached 60 percent of the vehicles recalled. Recalls can be quite costly for manufacturers and in some cases they seem reluctant to initiate the recall as they balance the cost and benefits of delaying action. Interestingly, an analysis of the proportion of recalls irrespective of number of vehicles involved (figure not shown), the proportion of mandatory recalls is never above 40 percent, and usually fluctuates between 20 percent and 30 percent, which indicates that mandatory recalls are usually also those with large number of units involved.

Tables 1 to 3 review this trends from 1988 to 2001, which is the period of our econometric analysis, given the data limitations in the later periods. Table 1 shows that manufacturers issued 3,886 recalls (74.3 percent of the total), while the ODI of the NHTSA initiated 1,032 recalls (19.73 percent) and the Office of Vehicle Safety Compliance (OVSC) initiated the rest. 54.96 percent of the recalls received the highest hazard rating, while 4.62 percent and 40.16 percent of the recalls were assigned the second and third hazard levels, respectively. Correction rates are defined as the ratio of the number of units repaired to the number of units recalled during the first 6 quarters after the recall. We have not been able obtain data on quarterly (or monthly) correction rates because the manufacturers do not disclose that information. The manufacturers report data on the number of units repaired during the first 6 quarters to the NHTSA. The table shows that the average correction rate is 55 percent. This means that more than half of the units have been fixed within the first 18 months after the recall.

One underlying concern with the trends we have shown is that the increase in both recall issuance and units per recall may result from the increase in the market size. Column 7 of Table 2 shows that the total number of vehicles sold has followed a different pattern. The level of 15.7 million vehicles sold in 1988 was not reached again until 1999 with 16.9 million, after a low of 12.6 million in 1991. In 2000 17.6 million were sold, and 16.6 million were sold in 2001.[12] Sales during the first decade of this century fluctuated at around those figures, and the last few years have seen a considerable decline in vehicles sold at the same time that recall issuance has not shown any sign of declining.

In line with what we saw in the figures, column 3 of Table 2 shows that the number of recalls from foreign vehicle manufacturers represents approximately one quarter of all recalls. This ratio is quite constant over time. The 4th column of Table 2 shows who initiates the recalls. The proportion of recalls that are voluntary has fluctuated over time, but seems to be on the rise since the late 1990s, accounting for over three quarters of the recalls.

Table 3 shows more details about recalls in 1988 to 2001. The total number of recalls from 1988 to 2001 was 5,233. The average value of the potential number of units affected was 94,237. The number of units that are issued to each recall range from 1 to 32 million. The NHTSA assigns four different hazard ratings according to the degree of possible risk. The value is 4 (which corresponds to category A) if a recall receives the most hazardous rating. The lowest hazardous rating here is 1 (which corresponds to category D). The average value was 3.143.

One last issue to be mentioned about the recalls is that although a recall is usually issued on a particular year-model, some recalls have been issued for two or more year-models. We solve this by grouping data on sales and correction rates for all year-models. In the cases that a given recall is issued to different vehicle models because they are produced from the same production line, then the number of units we use is divided by its share.

**Previous Literature**

Many researchers (e.g., Ashenfelter & Greenstone, 2004) have studied the effects of direct safety regulations, such as mandated speed limits, but the economics profession has not provided any quantitative evidence on the effects of these recalls on safety. Hoffer (1975) and Crafton, Hoffer, and Reilly (1981) focus on the effects of recalls on consumers' demand for the vehicles, and Nichols and Foumier (1999) and more recently Rhee and Haunschild (2003) study the effects on manufacturers' reputations. Jarrell and Peltzman (1985) analyze the stock market's response to the news of product defects. Using medical and automobile recall data, they show that the capital market internalizes the indirect costs of recalls and these costs are large enough to dominate direct recall costs.[13] However, these results were later somewhat discredited by the work of Hoffer, Pruitt, and Reilly (1988) showing that some changes in the construction of the recall events to more appropriately identify the effects lead to an important weakening (in most cases leading to statistical insignificance) of the original results. In any case, these costs can be a considerable deterrence to producing risky products, and give the manufacturers incentives to make safer vehicles. Rupp (2004) using more recent data, and focusing on the attributes of the recalls, finds results more in line with the significant results, and so do Chen, Ganesan, and Liu (2009) with data from other consumer products. Hartman (1987) assesses whether the resale market for cars discounts the information on recalls, and finds fairly substantial effects in terms of lower prices for cars that have been subject to important recalls.

Hoffer, Pruitt, and Reilly (1994) show that recalls of domestic new model vehicles with severe safety defects generate the largest corrective rates. They define the rates as the ratio of the number of the units repaired to the number of the units issued for the vehicle model. They argue that it is much easier for the owners of domestic vehicles to access the designated repair shops than the owners of foreign vehicles because the domestic manufacturers have much more well-organized dealerships across the country. Therefore, the time cost is lower for the owners of domestic vehicles. Accordingly, domestic vehicles' correction rates are higher. Hoffer, Pruit, and Reilly did not study whether these corrective rates directly affected accidents.

Rupp and Taylor (2002) investigate the initiation of recalls. They find evidence that the government initiates less hazardous recalls that affect larger number of units, while the manufacturers issue inexpensive recalls. They also analyze the determinants of recall corrective actions. Again, their study does not deal directly with the safety issue.

Huble and Arndt (1996) use car crash data to analyze how this information can be used to support the different positions in a safety dispute. They use a particular vehicle model (a GM truck) to see the effects of changing the fuel tank location on safety by comparing it with other similar types of trucks. They find that the conclusions might be quite different even if the manufacturer and the government use the same data source. This paper does not directly deal with recalls, but compares the damage to the vehicles and the loss of life before and after a model's design change. Of course, a model's design change can be a response to defects in order to reduce the number of accidents.

Bates et al. (2007) study the trends of vehicle recalls in the United Kingdom, describing some common features to the U.S. car market, but do not relate their study to any measure of safety.

## THE DATA

We use accident data from the General Estimation System (GES), which contains a nationally representative sample of all vehicle accidents that have happened based on police reports.[14] Recall data is available from the NHTSA, and vehicles' sales data and information about design changes has been gathered from Ward's automotive yearbook.

The GES designates 60 areas that represent geographic and demographic regions. Every week data collectors visit around 400 police stations within these areas. They randomly select about 50,000 Police Accident Reports (PAR) each year. The system started its operation in 1988, and data files through 2001 were, until recently, freely available online. We use the data files from 1998 to 2001 in the econometric analysis. More recent data does not have the same level of detail and a number of important variables are missing, like the hazard level of the recall.[15]

These data files consist of three distinct data sets: the accident file, the vehicle/driver file, and the personal file. The accident file contains information describing environmental conditions and roadway characteristics at the time of the crash, as well as information about the severity of the injuries for the passengers involved. The vehicle/driver file contains information describing the vehicles involved in the accidents and their drivers. It includes information such as make and model of the car and model year of the car. The personal file contains general information describing all persons involved in the crash: drivers, passengers, and pedestrians. It includes information such as age, sex, and injury severity.

The second source of information is obtained from Ward's annual automotive yearbook. These data contain all U.S. new vehicle sales by line. These sales data will be combined with recall data to produce the recall variables. The third source of information contains data on recalls. In order to match the first two data sets, we will use recall data starting in 1988. For the empirical study, we will use data on the potential number of units affected, the dummy variables on the manufacturers, calendar year of recall, and hazard category code for each recall.

The variables used in the empirical analysis are defined in Tables A1 and A2, in the Appendix, and explained below.

### Measuring Accidents by Driver Type

Most literature related to safety issues uses either fatality rates or accident rates as measures of accidental harm. However, those rates do not fit well with our objectives. The variable we choose should reflect common characteristics that are specific to type of driver. Furthermore, data should be identified by the vehicle models because recalls do not have a geographic dimension.

We define the types of drivers by age, gender, whether the driver was in the striking vehicle, and by the vehicle model. The latter has the effect of reducing the number of individuals by group but it captures many unobservable characteristics about the drivers, following the conjecture that the car that someone drives is an indicator of income, wealth, and in some cases attitudes towards risk. We use the number of accidents for a particular driver type as our dependent variable, which when we restrict attention to accidents linked to personal injuries, can be interpreted as a measure of accidental harm. Since each type contains the same kind of drivers and these drivers drive the same vehicle models, the numbers of accidents are different across types and over time. These differences between the members of a type reflect the frequency of the accidents and they are unique to each type. Therefore, these numbers measure the relative personal and property harm linked to accidents.[16] We construct annual data on the number of accidents in which a particular type has been involved. From the GES data set, we aggregate data, which report individual accidents by model, to produce the yearly number of accidents of a particular vehicle model.

We define the dependent variable $ln\_Acc\_Type_{it}$ as the natural logarithm of the number of accidents in which a particular driver type was involved in a given year. With this definition of the dependent variable, we face one potentially serious problem. The number of vehicles in use is not constant over time because the vehicles that had severe accidents will be removed from the road, but we do not observe the number of vehicles that have been dropped from the road over time. Therefore, if we simply use the number of vehicles in use on the road regardless of the vehicle year-model, we have to know the number of vehicles that have been discarded during a particular year. One solution is to restrict the number of vehicle-year models. It is reasonable to restrict the vehicles up to the 5-year-old ones because the number of vehicles in use does not change much during the first 5 years. Ross and Wenzel (2001) argue that 97 percent of 5-year-old vehicles are still in use on the road. Therefore, it is reasonable to restrict the data up to 5-year-old or newer vehicle models. Another reason for this restriction is that one of the main determinants of accidents may come from drivers' negligence in maintenance as the vehicles are getting older. For example, if a vehicle is a 20-year-old model, then the probability that the original defects of the vehicle cause an accident is very low. Also, around 90 percent of the recalls are issued within 3 years after the introduction of the new-year model. Therefore, our conclusions are unlikely to be affected by restricting our attention to newer vehicle data.

Following the recommendations from two anonymous referees we have decided to drop some vehicle models because of their likely use. In particular, we have eliminated the model Crown Victoria from our data sample because of its heavy public use, given that as many as 25 percent of the accidents that involved this model were identified as police cars, and the accidents they are likely to be involved in are clearly not likely to be related to defects in the vehicles. The removal of this model has actually helped our estimation results in terms of reducing the standard errors of our estimates. Other related concerns raised by the referees include the use of certain cars as rentals. Unfortunately, the accident data does not allow us to identify rental cars, and while we agree that drivers of rentals cars behave differently than drivers of individually owned vehicles, we are still interested in observing what the effects of recalls on these rental cars are, after controlling for the characteristics of drivers and the unobserved heterogeneity captured by the composite types we construct. Interestingly, we have found information by the Department of Transportation that indicates many time rental fleets and even taxi-fleets have been found to have correction rates below those of privately owned vehicles.[17] This finding indicates that if we find sizable effects of recalls on the number of accidents, these effects could be biased downwards given that we would be including rentals, which are less likely to be corrected and are also driven by drivers less likely to know about the defects of the vehicle. Therefore, this group of vehicles are more likely to lead to results that show small effects of recalls on accidents over time.[18]

Other issues with this definition of the dependent variable are connected with the representativeness of the data collected on accidents. Some of the worries include the fact that the accidents will for sure be reported if they led to injuries and are more likely to be reported if the cars are newer and rentals. Some of these issues we will be able to control for, others will be harder for us to deal with, but we will try to provide convincing sensitivity analysis of our results to make the case for the robustness of our main findings regarding the effects of recalls on the number of accidents.

When estimating the effect of correction rates on the number of accidents after a recall, the dependent variable is calculated as the natural logarithm of the average number of accidents of a particular vehicle year model in the 3 years after the particular recall.[19] In this case the level of observation is the recall of a particular vehicle year model. When a recall includes more than one year model, or more than one model of the same manufacturer, we aggregate across years and models.

**Explanatory Variables**

The main independent variables of interest are the ones related to recalls. Each recall has a different number of units, with some recalls containing a small number of vehicles while others include a substantially larger number. To account for this we construct a weighted recall variable, which is achieved by calculating the ratio of the number of vehicles affected by a particular recall to the number of the vehicles in use on the road. To avoid possible endogeneity problems, the denominator will be the sum of the vehicles sold in the 4 years prior to the year of that particular recall. We define

$$Rec\_Rate_{it}^{v} = \frac{\sum_{n=1}^{N}[R_{int}^{v}]}{\sum_{j=t-1}^{t-4}[S_{ijt}^{v}]}, \tag{1}$$

where $R_{int}^{v}$ is the number of the units of a recall $n$, issued at time $t$, for the vehicle model $v$. The vehicle model includes only year models no older than 5 years (from $t$ to $t-4$). $S_{ijt}^{v}$ is the number of units of the $j$ year-vehicle model that are on the road as of time $t-1$. All drivers in group $i$ drive the same vehicle model $v$. This variable represents how many vehicles are at risk among all vehicles on the road as of the year before the recall, and also how many vehicles are to be fixed due to recall issuance.[20] In the econometric specification, the coefficient on this variable can be directly interpreted as the effect of recalls on the number of accidents in percentage terms, given the nature of the dependent variable. The effect should be understood to affect only the units recalled.

Notice that issuance itself is different from correction. However, it sends a signal to the drivers whose vehicles are potentially dangerous. The signal may change the drivers' behavior: They may drive less frequently or drive more carefully until their vehicles are fixed. Therefore, recall announcements themselves may affect the number of accidents by either changing drivers' behavior or fixing the defects. In the empirical work, we will use data on correction rates to make the argument that although we do not know the effect that changes in behavior have on accidents, we can show that correction rates do matter, suggesting that while we cannot dismiss the behavioral change from drivers as an explanation of any changes in the number of accidents, it is not the whole story, otherwise correction rates would have no effects on accidents.

When we use the whole sample of accidents, the indicator *Strike* takes the value of 1 if the vehicle struck other vehicles or objects. In the specifications that include all accidents, the variable *Strike* is used to distinguish the role of the drivers' faults and the vehicle's defects. In all the specifications, and following comments from anonymous referees, we have also used this variable to restrict attention only to accidents linked to striking vehicles, given that struck vehicles' involvement in the accidents is unlikely to be linked to any defects, as long as this variable is a good indicator of which vehicle is at fault in the accident.

Given that age is also an important control, we divide individuals by their age into 4 groups. The first age group is for drivers under 26. The second age group is between 26 and 35, the third age group consists of those aged 36 to 49, and the fourth for those 50 and older.

Another important explanatory variable reflects whether the manufacturers have redesigned a vehicle model in the period of analysis. This is an important variable because one natural reaction by manufacturers to a large number of accidents tied to defects is to fix the problems in a new model. However, design changes can also introduce an array of new problems, especially if car makers feel the pressure to launch new models to keep up with market rivals. This market pressure to put new models up for sale might be going on in the current environment, leading, for example, to serious defects in cars built by Toyota shortly after it became the world's largest producer and launched a number of new year models. This scenario is supported by the recent theoretical work of Carpenter, Grimmer, and Lomazoff (2009) who find that short experimentation before the launching of a product can lead to lower quality products reaching the market when the producers of those products are large companies. We build upon the definition used in Berry, Levinshon, and Pakes (1995) and consider that a new model was introduced if either horsepower changed by 10 percent or more, or one of the other indicators (width, length, and wheelbase) had a substantial change, considering the usual changes in that type of indicator. A related explanatory variable is a measure of the vintage of the cars on the road of a particular model. We calculate this variable as the percentage of cars of a particular model that belong to the two latest year models.[21]

An additional logical control is the total number of cars on the road of a particular vehicle model. We use the sum of all the cars of a particular model sold in the 4 years preceding the year of observation. This helps control for the level effect correlated with the probability of being in an accident. Given that the sales effect could be nonlinear, we also include sales squared as an independent variable. The latter turns out to be quite important, given that its omission leads to a biasing upwards (in absolute value) of the recall coefficient, given that it is a ratio and therefore captures some nonlinearities of relationships we study.

In all specifications we add year dummies to capture the changes in accidents as the number of total vehicles on the road evolves in the 1988 to 2001 period. We also include binary indicators for the different vehicle models. These variables are used to group drivers into the different types. Therefore, these indicators are constant over time, but changing across the types. Table A3, in the Appendix, provides the list of vehicle models included in the analysis.

When estimating the effect of correction rates on the number of accidents after a recall, the key independent variable is the correction rate of the particular recall analyzed. This measures the proportion of vehicles that have been fixed, out of the original pool of defective vehicles. These rates are reported by the manufacturers. Other independent variables in the analysis include the average sales of the particular year model, the size of the recalls and its square, the number of recalls after the particular recall of analysis, whether the recall was considered hazardous, and the manufacturer of the vehicle recalled.

**Summary Statistics**

There are roughly 250 vehicle models in the U.S. automobile market and the number of models varies slightly over time. In our analysis we choose the vehicle models according to the following selection criteria: First, while 97 percent of light vehicles are still on the road, only 93 percent of trucks are on the road within 5 years. Therefore, we concentrate on light vehicles and one SUV.

Another justification for this is that trucks are mostly commercial. However, we decided to consider the inclusion of SUVs in the sample because recently we have seen more frequent and notorious recalls for SUVs, even though SUVs are in the truck category, most SUVs are not used for commercial purposes. However, many SUVs were problematic since recall data was reported by model lines that have separate accident data, like the Ford Explorer and the Ford Bronco, or the Plymouth and Chrysler Voyager, preventing us from tracking the variables of interest as with other models. Second, we add more new vehicles that have recently appeared in the market if their market shares are substantially increasing over time. Third, we exclude models with a market share under 1 percent of the market of current year models to avoid small sample problems in our empirical strategy, which in some cases affected vehicles that loss considerable market share by the mid 1990s like the Camaro. Fourth, during the time period of interest, some firms have merged and other firms stopped production of a particular model. We have excluded those models at this time. As mentioned earlier we have eliminated one popular car, the Crown Victoria, because it is widely used as a police car. Overall, we include 19 vehicle models whose unit sales are consistent over time. These vehicle models have been popular and have large market shares over time.

In Table 4 we provide summary statistics for the full sample (74,806 observations) of individual-level accidents of our 14 years of repeated cross-sections. Fifty percent of individuals involved in accidents are males, around 43 percent are aged over 35, and the most popular vehicle models in our sample are Escort, Accord, Cavalier, Taurus, and Civic. This is very much in line with the market shares in the U.S. car market in the period between the late 1980s and the turn of the century.

Table 5 shows summary statistics once we have grouped individuals by *composite types*. There are almost 4,000 observations in this data set, which comes from following the 304 types (which include 19 different vehicle models, four age categories, two genders, and whether the vehicle was the striking one) for 14 years. The average composite type got into just over 18 accidents on average during a year. The maximum number of accidents among all groups is 141. Regarding the recall variables, we see that 14 percent of the vehicles on the road have been recalled. The maximum value is larger than 1 because one recall may include more than one defective part so more than one recall can affect a particular model in a given year, which appears here as recalling a larger number of vehicles. Of the vehicles for which recalls were issued, 55 percent received hazard ratings (A or B). In our empirical work, and in order to compare our empirical strategy to the seminal work on synthetic panels, we also divide our sample only by the 19 vehicle models we analyze in the paper. It will be clear later that the qualitative results do not change much, but do illuminate the trade-offs between minimizing the possible measurement error problem involved in analysis of repeated cross-sections and computing efficient parameter estimates of the variables of interest, the efficiency of which is a function of the homogeneity of the composite-types.

Table 6 provides summary statistics for the variables used in the correction rate estimation. The average number of accidents for the recalls analyzed is over 200, and the correction rate for the recalls in the sample is 69.13 percent, the average units sold in the 3 years after the recall are 687,000, the average size of the recalls included in the sample is 241,000, and the number of recalls after the current recall, of the same year model in the 3 years after the original recall, is 2.13.

## THE ECONOMETRIC MODEL

### Number of Accidents and Recalls

In order to analyze the effects of recalls on safety, it would be ideal to have panel data to control for the individual specific heterogeneity that results in potentially different outcomes when faced with a given situation on the road. It is also necessary to control for vehicle characteristics, manufacturers decisions regarding new model introduction, and characteristics of the drivers. The problem is that there are no panel data on accidents of individual drivers, so it is essentially impossible to observe an individual driver's behavior and his or her response to recalls over time. Only repeated cross-sectional data on accidents is available, which does not allow us to control for individual specific driving abilities. Without those controls our models can say relatively little about the effects of recalls on safety over time.

To circumvent this problem we propose to produce synthetic panel data using the repeated cross-sections independently collected each year, following the work of Deaton (1985) and Verbeek and Nijman (1992).[22] For this, we use the concept of a "type" or "group." This notion starts with the fact that, corresponding to individual drivers' behavior, there will be a group version of such behavior, if we group drivers by some characteristics, and the type of car they drive. If we group drivers whose characteristics are similar into a type, we can then track the drivers' behavior over time through these types. Within a group, we have drivers whose driving characteristics are similar, and we can consider this type as if it were an individual. Browning, Deaton, and Irish (1985) provide an empirical application using British data from the 1970s, where they divide groups by aggregating age cohorts and by whether the head of the household was a manual worker. Attanasio and Weber (1993) and Blundell, Browning, and Meghir (1994) use a very similar specification, also using British expenditure data, and divide the groups by age.

One of the major differences among these drivers is that they drive different vehicle models. Since we investigate the effect of recalls of particular vehicle models on accidents, the type must also contain information on vehicle characteristics. If we again divide these drivers by vehicle models, then we can control for drivers' and vehicles' fixed effects. We also believe that dividing by vehicle models controls for additional unobserved characteristics of drivers correlated with the type of car they drive, such as income, wealth, or even attitude towards risk. Since a type contains a particular vehicle model and a driver's type at the same time, we call this a "composite type."[23]

This netting-out effect enables us to statistically distinguish characteristics that are related to the defects from all others. Therefore, our level of observation is a group of individual drivers who have the same personal characteristics and drive the same vehicle model at the time of the accident. Now each composite type appears repeatedly over time. If we have enough composite types, successive cross-sections of accident data will generate successive random samples from the composite type population.

Notice, as we have mentioned in the previous sections, that one disadvantage of using the synthetic panel strategy is that we cannot use the sample weights provided in the GES data set to make the data we use truly representative of the U.S. population, given that our level of observation is the composite type, and the relevant information is the number of accidents that composite types is involved in each year, preventing us from assigning the weights linked to each accident.

*The Measurement Error Problem*

Given that each composite type has its own characteristics, like an individual driver, we need to use summary measures that represent these characteristics, not individual measures because they should show common characteristics of individuals within a type. Composite type means are the statistic we choose in this setting. However, if we use sample means of the type, then we face a measurement error problem. The unobserved effects are no longer constant over time since composite type population means are different from composite type sample means. These errors are added to the unobserved type effects so that the effects change over time. Deaton (1985) showed that it is possible to use the synthetic panel data model solely from cross-sectional data if large numbers of observations are available in each period, or if the estimators are corrected for the error in variables problem.

Verbeek and Nijman (1992) reached the same conclusion, and they investigate the conditions that make this approach valid. Their conclusion is that the larger the number of observations per type, the less severe the measurement error problem will be. The latter set of authors and Collado (1997) pointed out that there is a trade-off between the number of observations per type and the number of types, given a sample size. Collado argues that the cross-sectional sizes of the most widely used data sets are relatively small, and therefore the problem comes from the fact that we are trying to make as many types as we can, but with a relatively small number of total observations, the measurement error becomes serious. In general in order to make the measurement error less serious the number of observations per cohort should be large enough, but not so large that the variance of the parameters of interest is too large.

Measurement error, however, is unlikely to be an issue in our preferred specifications with finer types, because we are not averaging any characteristics within the groups. The dependent variable is not an average but a cell count (the number of accidents in the cell) and the independent variables are in all cases either the variables we use to divide the types, or in the case of the recall rate, the variable is the same number for all the vehicles of a particular model in a given year.

*Econometric Specification of the Number of Accidents Estimation*

Consider the following equation to represent the relationship between accidental harm of a particular individual and the possible causes involved in the accident,

$$y_{dt} = f_d(x_{dt}, \xi^v_{dt}, \zeta^v_{dt}, c_d, \tau^v_d) \tag{2}$$

where $y_{dt}$ measures accidental harm that a driver $d$ incurred at time $t$. $d = 1,..., D$. $D$ is the total number of drivers who had the accidents at time $t$. $x_{dt}$ are the driver $d$ 's characteristics that affect the accidents in which he or she is involved. They include the observed factors that are used to group drivers. $\xi^v_{dt}$ are the vehicle characteristics that are not related to the defects, where $v$ is an indicator of the vehicle he or she drives. $\zeta^v_{dt}$ are the vehicle characteristics that are related to the defects. $c_d$ is the driver's unobserved factors. $\tau^v_d$ is the vehicle's unobserved factors.

A convenient functional form to express the relationship is

$$y_{dt} = x_{dt}\gamma_1 + \xi^v_{dt}\gamma_2 + \zeta^v_{dt}\gamma_3 + c_d + \tau^v_d + \varepsilon_{dt} \qquad t = 1,..., T \qquad d = 1,..., D \tag{3}$$

where $\varepsilon_{dt}$ is the unobserved random disturbance. The aggregating process changes the latter equation to

$$\tilde{y}_{ct} = x_{ct}\beta_1 + \tilde{\xi}_{ct}\beta_2 + \tilde{\zeta}_{ct}\beta_3 + \tilde{c}_c + \tilde{\varepsilon}_{ct} \qquad t = 1,...,T \qquad c = 1,...,C \tag{4}$$

where $c$ denotes a composite type. $\tilde{y}_{ct} = (1/c_n)\sum_{d=c_1}^{c_n} y_{dt}$ . $x_{ct} = (1/c_n)\sum_{d=c_1}^{c_n} x_{dt}$ . $\tilde{\xi}_{ct} = (1/c_n)\sum_{d=c_1}^{c_n} \tilde{\xi}_{dt}$. $\tilde{\zeta}_{ct} = (1/c_n)\sum_{d=c_1}^{c_n} \tilde{\zeta}_{dt}$. $\tilde{c}_c = (1/c_n)\sum_{d=c_1}^{c_n} \tilde{c}_d$. $n$ is the number of observations in the composite type $c$. The variables become the types' mean values and represent group characteristics that affect the dependent variable. Since the variables in $\xi$ are the dummy variables indicating vehicle models, the unobserved vehicle fixed effect is absorbed into these variables. Rewriting the last equation

$$\tilde{y}_{ct} = x_{ct}\beta_1 + \sum_{v=1}^{V-1} d_v \beta_{2v} + \tilde{\zeta}_{ct}\beta_3 + \tilde{c}_c + \tilde{\varepsilon}_{ct} \qquad t = 1,...,T \qquad c = 1,...,C \tag{5}$$

where $d_v$ is the vector of vehicle dummies.

This grouping controls for driver and vehicle characteristics that are not related to the defects. The difference in accidental harm over time and across types may come from other observable factors including the changes in recall variables and other sources of unobserved heterogeneity between the types. By making the composite type, some unobserved components are controlled by the panel data strategy. However, the error term, $\tilde{\varepsilon}_{ct}$, now contains a type-specific trend in the process of aggregation. If there exists a cohort-specific trend, then an additional source of heterogeneity arises. Therefore we need a separate term to account for this. We can then write

$$\tilde{y}_{ct} = x_{ct}\beta_1 + \sum_{v=1}^{V-1} d_v \beta_{2v} + \tilde{\zeta}_{ct}\beta_3 + \tilde{c}_c + T\ g + \varepsilon_{ct} \qquad t = 1,...,T \qquad c = 1,...,C \tag{6}$$

and we will define $\tilde{\varepsilon}_{it} = T\ g + \varepsilon_{ct}$. In the panel setting, $t = 1988,...,2001$ and $c = 1,.....,304$, where $t$ is a year and $c$ is a composite type. To express the equation in a simple matrix notation, we redefine the equation as

$$y_{it} = X_{it}\beta + c_i + \varepsilon_{it} \qquad t = 1,...,T \qquad i = 1,..., N \tag{7}$$

where $y_{it} = \tilde{y}_{ct}$, $X_{it} = (x_{ct} \vdots \sum_{v=1}^{V} d_v \vdots \tilde{\zeta}_{ct})$, $\beta = [\beta_1 \vdots \beta_2 \vdots \beta_3]'$, and $c_i = \tilde{c}_c$. Now $i$ represents a composite type. $X_{it}$ contains observable variables that change across $t$ but not $i$, variables that change across $i$ but not $t$, and variables that change across $i$ and $t$. We call $c_i$ the unobserved composite type effect.

We can modify this model to account for a type-specific trend:

$$y_{it} = X_{it}\beta + z_{it}a_i + c_i + \varepsilon_{it} \qquad t = 1,\ldots,T \qquad i = 1,\ldots,N \tag{8}$$

where $z_{it} = (1,T)$ , $z_{it}$ is $1\times 2$, $a_i$ is $2\times 1$, $y_{it}$ is $1\times K$ , $X_{it}$ is $1\times K$ , and $\beta$ is $K\times 1$. The latter is the equation we use for estimation of the traditional synthetic panel data model. For the model, the strict exogeneity assumption of the idiosyncratic error term with respect to the regressors is imposed. After constructing this basic panel data structure, the same inference procedure as in traditional panel data models can be used.[24]

Finally, once we construct our composite types in a finer fashion, the measurement error issues disappear, and we are left with a traditional panel data estimation of equation (8), where the level of observation is the composite type, and where the maintained assumption is that the unobserved heterogeneity components, which we are accounting for, are group specific and it is meaningful to track the number of accidents of these types over time.

**Correction Rates Model**

When estimating the effect of correction rates on the number of accidents after a recall, we estimate the following equation

$$y_i = X_i\beta + v_{it} \qquad t = 1,\ldots,T \qquad i = 1,\ldots,N \tag{9}$$

where $E(v_{it}\,|x) = 0$ . Here $y_i$ is the natural logarithm of the average number of accidents of the year model recalled, in the 3 years after the recall, and the explanatory variables $X_i$ are the ones explained in the data section, including our main variable of interest in this estimation, that is, the correction rate of the recall for that particular year model, as reported by the manufacturer 18 months after the initial recall. If more than one recall was issued in a given year, we take the one with the highest hazard rate or the one that recalls the largest number of units, if the hazard levels are the same.

## EMPIRICAL RESULTS

### Do recalls reduce the number of accidents?

Tables 7A to 7D report the random effects estimates of the different specifications of the panel data models applied to repeated cross-sections of accidents and using vehicle models recall data.[25] In all of these tables we focus only on the 19 vehicle models we analyze, and we report the effect on the number of accidents of all recalls and also hazardous recalls, restricting in some cases attention to accidents of striking vehicles, and also selecting the sample by whether the accidents lead to injuries or not. Notice here that we are only controlling for the vehicle model effect, not for any unobserved heterogeneity linked to drivers.

The first two columns of all the tables group accidents only by the type of vehicle model involved in the accident, and do not include covariates that need averaging within groups. This specification essentially uses a panel of the vehicle models we have chosen to analyze and studies the connection between the accidents affecting those vehicles and the recalls issued on those them. Columns 3 and 4 of the tables use standard synthetic panel specifications (Deaton, 1985), and while grouping accidents only by vehicle model, they do include covariates that are the result of averaging characteristics of the drivers within each group. These latter parameter estimates are potentially subject to measurement error. However, the large number of observations in each of the groups, ameliorates this problem considerably. As shown in Table 8, the number of observations in most of the groups is in the several hundreds. Here each group represents a vehicle model.

Table 7A shows the results of considering all accidents from the vehicle models analyzed, which means we are not separating accidents that lead to injury from those that do not, and not separating accidents of striking or struck vehicles. The result suggests that recalls reduce accidents by between 10 percent and 16 percent, depending on whether we look at hazard recalls and include average characteristics by groups of drivers, but the effects are not statistically significant. The fit of the model is, however, quite good, especially when accounting for the average characteristics of the drivers of those models.

Table 7B restricts attention to accidents linked to striking vehicles, given that it could be argued that including in the analysis vehicles that were struck could be rather noisy and not very meaningful, given that the police seemed to have identified them as playing a fairly passive role in the accident. The latter, however, is unclear. A vehicle could be the struck one and be the one that provoked the accident. The classification by the police can potentially be a noisy measure of the actual events of the accident, much noisier than other measures which are easier to observe (by looking at the cars, the drivers, or from the drivers' licences) and do not depend on the accounts of the accident by the drivers (and passengers) of the vehicles involved. In any case we also present all of our results for this subsample of accidents. The point estimate of the recall coefficients are similar (the rest of the coefficients are almost all significant and vary relatively little from the previous specification), but now the results are significant once we control for the average characteristics of drivers and the effects of recalls on accidents varies between just below 16 percent to just below 20 percent, with the fit of the model still at very high levels. The effect is still predicted to be quite large, but we have to remember that as in Table 7A, we are still not accounting for the unobserved characteristics of drivers.

Table 7C further restricts the sample by only including accidents linked to personal injuries (and also only striking vehicles). The idea here (following an anonymous referee's comment) is that this could allow us to circumvent selection concerns regarding the sample of accidents gathered by the police, given that accidents in which injuries occurred will almost always be reported to the police, while accidents in which only property damage is involved are more likely to be kept off the books. One disadvantage of this restriction is that if a recall campaign had the effect of reducing the severity of accidents (as show in Bae & Benítez-Silva, in press), then this restriction could potentially bias upwards (making the coefficient more negative) the effect of recalls on accidents, given that even if the number of accidents for that group of drivers stays the same, as long as the severity declines enough, the estimate would show an effect of recalls. As conjectured, the coefficients are now much larger (more negative) and statistically significant, indicating a large effect of recalls on the number of accidents. Given our discussion, these results should be taken with caution and understood as indicating that recalls likely reduce severity of accidents as well.

Table 7D presents the results of restricting attention to accidents linked to striking vehicles that did not lead to personal injuries, but only property damage. Here the results are somewhat weaker, with a smaller effect of recalls on accidents but still significant once we control for average characteristics. As we will see later, this result becomes even weaker when we control for additional sources of unobserved heterogeneity, suggesting that less serious accidents are unlikely to be the result of car defects.

All these results are suggestive, but this coarse grouping by vehicle model, however, results in the inefficiencies discussed in the literature, due to the prevalent heterogeneity within each group. So we would ideally further control for other sources of heterogeneity, which could be biasing our estimates, and we do so by defining in a finer fashion the composite groups in our specifications.

In Tables 9 and 10 we show our main results, where we improve upon the specifications just discussed, in that the groups are now defined much more finely. A type is now defined not only by vehicle model, but also by gender, age group, and by whether the vehicle was the striking one in the accident in the specifications that include both striking and struck vehicles. Overall we have more than 300 groups. This fine grouping allows us to estimate the parameters of interest in a considerably more efficient way and avoid completely the measurement error problem that could be biasing our coefficients of interest, since the problematic covariates of interest are in this case indicators of whether the accident belongs to a particular age group, gender group, or striking group, and are not averaged within groups.

Table 9 reports the estimates by random effects of the panel data model in equation (8) for the whole sample of types, and all recalls, and shows four specifications, depending of whether we include accidents by all vehicles involved or only striking ones, and whether we include all accidents or only those that lead to injuries, or no injuries. Again, the main finding is that recalls are effective in reducing the number of accidents in all specifications. The coefficients in the different specifications of our main explanatory variable of interest, $Rec\_Rate$, is negative and statistically significant at the 5 percent level or better, and depending on the particular sub-sample it varies from 7.8 percent when concentrating on striking vehicles in all types of accidents, to over 16 percent when we further restrict our attention to only injury related accidents. These results should be understood to mean that, for example, an increase of 10 percent in the recall rate of a particular model, will decrease the number of accidents by between 0.78 percent and 1.6 percent. Interestingly, in the last column of the table, in the specification that restricts attention to accidents that did not lead to any injury, the effect is much smaller and not statistically significant, indicating that non-serious accidents do not seem to vary with recall rates. Of course, these results are the product of a given timing of the recall with respect to the accidents or events that lead to them, and the average type of recall (more or less hazardous) that is observed in the data used for estimation.

The variable that indicates whether a model's design change was introduced in a given period, *Design Change*, has a positive and significant effect, indicating that although design changes are likely to fix problems with previous versions of the car, new features and engineering changes lead to a higher number of accidents, other things equal, and after controlling for time trends and vehicle indicators. Notice that this effect is fairly stable across the specifications shown, suggesting that a design change does not seem to affect the likelihood of entering the sample of accidents given that even when concentrating on injury-related accidents the effect of design changes on the number of accidents does not differ much. The positive coefficient suggests that the many pressures that car makers have to launch new products every year could be resulting in larger and larger number of problems with the vehicles. This effect seems to be offsetting the possible improvement in engineering that comes with the introduction of a new design.[26]

The stability of the *Design Change* indicator contrasts with the sizable changes in the *Vintage* coefficient across specifications. The very large and positive effect when analyzing all vehicles and all injury levels is reduced considerably when concentrating only on striking vehicles, and is further reduced dramatically when concentrating on striking vehicles linked to accidents that lead to injuries. Such reduction in the coefficient suggests that newer vehicles are likely to appear in larger numbers in the accident data collected by the government (police reports) and drivers seem to be more likely to report relatively minor accidents in order to be able to get covered by their insurance companies. This can be considered a problem linked to selection (which we cannot directly control for, by construction of the data, which is a cross-section of accidents only), which can reveal itself as a type of misspecification. This suggests we should be careful interpreting this parameter estimate, especially when selectivity concerns are present given that it likely captures the effect of *Vintage* on the number of accidents as well as the fact that newer vehicles are more likely to enter the sample accidents we use. Later in the section we will provide the results of omitting this variable (and the *Design Change* variable) from the estimation. However, that strategy can lead to possibly serious omitted variable biases, given that newer vehicles are also more likely to be recalled (the correlation coefficient is around 0.24 in the larger sample), so the omission of the *Vintage* indicator will bias the recall coefficient towards zero.

Not surprisingly, the number of vehicles on the road of a particular model has a strong and significant positive effect on the number of accidents, with the model predicting that an increase in 100,000 cars on the road increases the number of accidents by around 50 percent through the linear term; however, the quadratic negative term (also significant) reduces this effect considerably. The effects are similar across the specifications we show in the table. We also include in the estimation binary indicators of the different vehicle models driven by our composite types. Since the Dodge Caravan was omitted from the specification, the coefficient for any particular model is the relative difference in accident rates for that model and the Caravan. We have also experimented with omitting other vehicle models, such as the Ford Mustang, and others that could be used more intensively in rental fleets (like Escorts, Cavaliers, or Intrepids), but the results (available from the authors upon request) were essentially unchanged. Regarding these indicators, notice that even after controlling for the size of the market, more popular vehicle models like Escort, Accord, Mustang, or Civic are predicted to be positively related to the number of accidents on the road.

The rest of the coefficients show a fairly consistent pattern, except for the effect of gender (male indicator in this case), which turns negative in the specifications that only analyze injury-related accidents, suggesting that once we control for unobserved characteristics women are more likely to be involved in these type of accidents. Notice that in the first specification the *Strike* indicator is positive and significant, likely capturing the fact that in a number of accidents only one car (the striking car) was involved. The age dummies have the expected signs and are all positive and significant given that the omitted category is drivers 50 and over. Notice that the overall fit of the model is quite good, explaining almost 60 percent of the variation in the number of accidents, and doing an especially good job in explaining the variation between the different types of drivers.

We have performed the Breusch-Pagan Lagrange multiplier test for the presence of an unobserved effect, and we can soundly reject the OLS model in favor of the random effects structure. Also the Hausman test cannot reject the random effects assumption versus the fixed effects characterization of the model. This is not surprising since the very nature of our data, a cross-section of accidents randomly drawn from the population of accidents on the road each year, fits perfectly with the assumptions of the random effects model. Also, it is difficult to argue that the unobserved heterogeneity component that affects our composite-type driver has much to do with the covariates related to the vehicle model they are driving. Therefore, in all the tables we report the results of a modified random effects model that contains a time-trend. In fact, this modified characterization (which includes time dummies and vehicle dummies) makes the random effects and the fixed effects essentially identical, explaining the results of the Hausman test mentioned above. In fact, for the recall variable the results are identical, given that the recall variable drops from a "between" estimation since all types that share a vehicle model face the same recall rates over time.[27]

Table 10 concentrates only on hazardous recalls, given that it could be argued that recalls of fairly minor defects (such as a defect in the windshield) are unlikely to have any effect on the accidents on the road. The results do not change much from the ones reported above, except that the recall effects are now larger across the board. For the full sample they go up to 14.85 percent, and they go up to around 19.5 percent if we restrict attention to accidents of striking vehicles that resulted in injuries.

Table 11 first replicates the second column of results from Table 10, which includes accidents of striking vehicles only that resulted in all types of injuries, and then separates the results by whether the cars involved are produced by a domestic or foreign manufacturer. There are no major differences between these two categories of cars, suggesting that once we control for unobserved heterogeneity the recall effect is not a function of the producers of the vehicles. There are some differences, however, in some of the other coefficients like a non-significant coefficient of the gender dummy in the case of foreign vehicles, and the much stronger age effects for their manufacturers, while the *Vintage* effect (the proportion of vehicles produced in the last 2 years as a fraction of the total produced in the last 5 years) is only significant for domestic vehicles as a predictor of the number of accidents.[28] This difference in the *Vintage* coefficient is likely linked to the market shares for different manufacturers, which leads to higher (lower) proportion of newer vehicles on the road after years of high (low) sales, leading to more accidents (an effect that the sales measure cannot capture). This effect is more clearly identified for domestic manufacturers given the higher variability in their market shares in the period of analysis, with sharp declines in the market shares of GM and Ford, but steady and relatively modest increases by the Japanese manufacturers (see Train & Winston, 2007, and *http://wardsauto.com/keydata/historical/UsaSa28summary/* at Ward's Automotive's website).

In Table A.4 we also show results using hazard recalls but drop the *Vintage* and *Design Change* variables to take into account possible concerns regarding their role in the selection process that leads to the sample of accidents in the GES system, given that newer vehicles and newer models are more likely to appear as reporting accidents than older vehicles. However, as discussed earlier, omitting these variables from the specification opens the door to possible omitted variable biases, given the fairly strong correlation of these variables with the recall rates, since most of the recalls happen in the first years after the launching of a new model. We can see that the recall coefficient is now a bit smaller in some of the specifications, going as low as to 8.3 percent, but not by much once we restrict attention to injury-related accidents, which provide the estimate more likely to be free of possible biases due to the possible selection into the sample of accidents. All of this can be interpreted as suggesting that newer vehicles are more likely to be recalled, but also have more accidents, so when we remove them from the estimation a classic omitted variable bias arises reducing the coefficient on the recall variable. The rest of the coefficients do not change much, and the overall pattern of the results regarding the effects of recalls on the number of accidents is very much in line with the evidence we have shown in the previous tables.[29]

**Are Higher Correction Rates of Recalls Linked to Fewer Accidents?**

The main results presented in the previous subsection leave one question open. Are recalls effective because people fix their cars or because they change their behavior after they know about the problem, even if they do not take the car to be fixed? The debate on how behavior adjustments by individuals can affect safety outcomes goes back to Peltzman (1975), and a large number of articles with mixed empirical results, including Crandall and Graham (1984), and more recently, using Canadian data, Sen (2001).[30] It is unlikely that the reduction in accident rates caused by recalls is all due to behavioral adjustment, given that information is sent to drivers from manufacturers only about the need to fix their cars, not about how dangerous the defect could be. It is clear, however, from the numbers on correction rates that not everyone takes their cars to the shop to be fixed. Manufacturers report an average correction rate of about 68.85 percent for the recalls of the vehicle models analyzed in this paper. The number would not be very different if we were to take all recalls issued in the last few years.[31]

The estimation of equation (9) presented in Table 12, provides evidence of the importance of correction rates in the number of accidents after a recall. We use the sample of recalls linked to the 19 vehicle models used in the previous section (therefore we exclude recalls of year models older than 5 years), and after restricting attention only to one recall if multiple recalls are issued on a given year, and after having to aggregate across many models or year models due to the fact that many vehicles share components that are recalled, we have a sample of a just fewer than 100 observations. Even with this relatively small number of observations our results are very clear. The higher the correction rates of a recall, the lower the number of accidents of that year model in the 3 years following the recall. Other variables that are significant are the average total sales of that vehicle model in the 3 years of analysis (and its square term) and the size of the recall, both correlated with a higher number of accidents. Regarding the indicators of the different manufacturers, Chrysler cars subject to recalls are predicted to have the least number of accidents. The fit of this simple model is very good, with an $R^2$ close to 90 percent.

## CONCLUSIONS

In this paper we have investigated the effects of vehicle recalls on safety, measured by the number of accidents. A synthetic panel data model approach was adopted and estimated, linking government data on accidents with industry data on recalls, sales, introduction of new models, and other market indicators. Our results show that recalls reduce the number of accidents. Recalls of a particular model reduce accidents of that model by around 10 percent (the results vary from around 8 percent to as much as 19 percent depending on the specification), which means that an increase on the recall rate of a particular model by 10 percent will reduce the accidents of that model by 1 percent. We find that hazard recalls are more effective and if we restrict attention to injury-related accidents, the effects gets stronger but could be considered a likely upper bound on the effects. We also find that higher correction rates of recall defects are predicted to decrease the number of accidents.

This is the first study to quantify the effects of recalls on the number of accidents, supporting the intuition that recalls, if meaningful, should have some effect on safety, but not in line with the conjectures of some industry insiders who believe that recalls are an example of government over-regulation. We hope this research encourages further research regarding the benefit-cost analysis of recalls and the role of policy in making sure drivers fix their cars; What is even more important is our hope that it fosters the discussions of regulations that can make manufacturers more aware of the social costs of putting a large number of potentially faulty cars on the roads, balanced against the private costs associated with making vehicles safer.

We also believe that our results provide support for a more important role by insurance companies and the government in fostering drivers' education regarding recalls.[32] The lack of quantitative evidence linking recalls with increases in safety has limited the amount of support for any measure that would make possible an industry-wide role in using recall information. We find that recalls reduce accidents and correction rates do matter. Therefore, insurance companies should consider taking into account the correction history of particular drivers and cars when pricing their insurance, and maybe even make coverage conditional on fixing major recalls. If discounts are given to drivers that have fixed their cars, we are likely to see a decline in accidents and insurance costs, with the resulting welfare improving effects for society, derived from the reduction in the monetary costs and the costs of loss of life due to accidental harm. Also, whether drivers have fixed their cars can be a good indicator of overall maintenance effort in their vehicles, likely to be correlated with the likelihood of being in an accident.

Finally, by showing empirically and quantitatively that recalls are effective, we also hope to make policymakers, and the public at large, aware of the fact that maybe some of those recalls, and therefore many accidents, could have been prevented. Therefore, the balance between pre-market vs. post-market regulation should be revisited for the case of automobiles production and commercialization in the United States, given that our finding could be understood as suggesting that the presence of post-market costs associated with preventable accidents are indirect evidence of the declining cost to society associated with additional pre-market testing. We believe that manufacturers currently feel little pressure to minimize the problems of cars before they are put on the road, since the direct and indirect costs of the increasing number of recalls seem to be small (given that many people never take their recalled cars to be fixed, and many believe and defend that recalls have little or no effect on safety) compared with the likely investments (and loss of revenue due to delays in introducing new models in an ever more competitive, and increasingly complex, industry) needed to reduce defects to a level that would assure a smaller number of recalls and prevent accidents.

We strongly believe that our results, and the results of future research on this topic, are likely to have an influence in the car and insurance industry. As we discussed in the introduction, the trend seems to be towards asking for a wider release of information regarding recalls, which will result in higher correction rates, and in fewer accidents, especially if the insurance industry is able to use information on recalls on their pricing strategies. Additionally, our findings suggest that the best possible outcome is to prevent as many faulty automobiles from reaching the streets as possible, which requires a change in the approach to quality control of the auto industry and the trade-off between pre- and post-market testing. Interestingly, recent research suggest that increasing the information to consumers about the quality and safety of products within a regulated framework could lead to a higher demand for vehicles, providing some justification for car companies to be interested in revamping the pre-market regulations affecting the industry.

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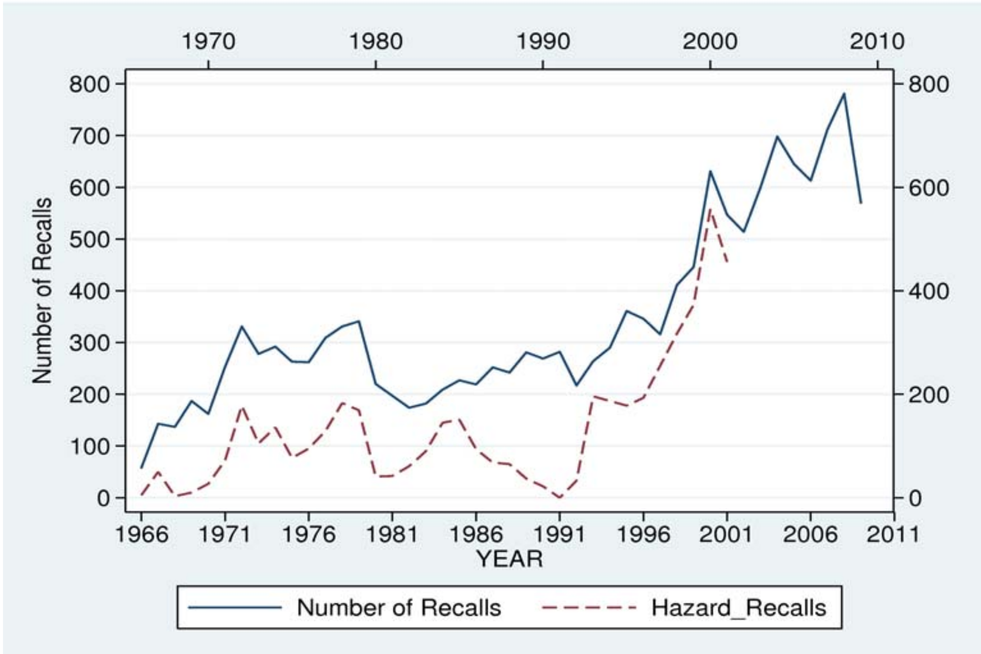


**Figure 1.** Number of Recalls Issued

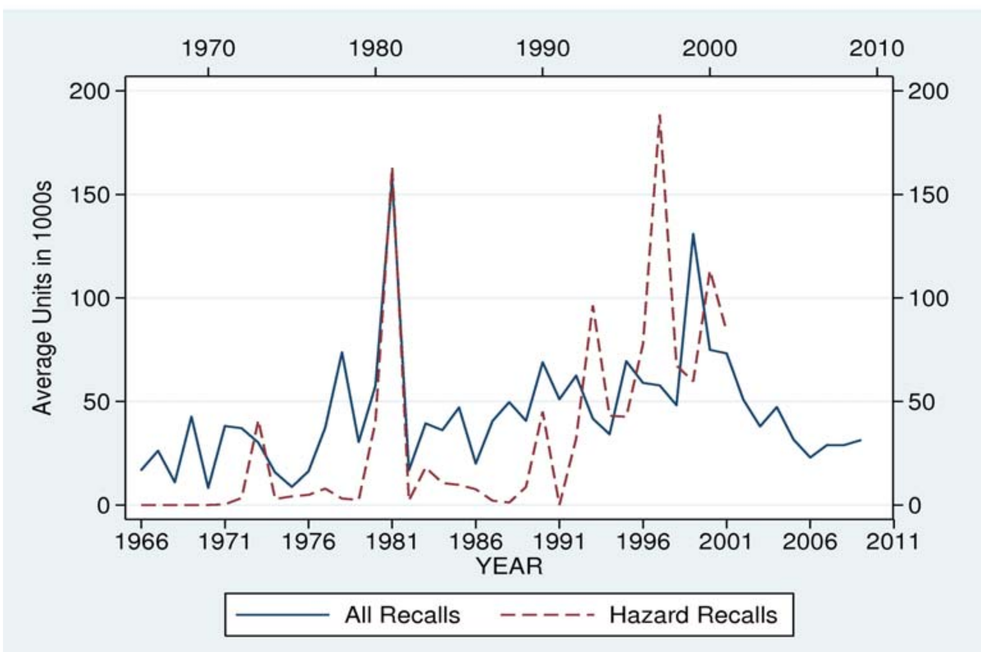


**Figure 2.** Average Units Per Recall

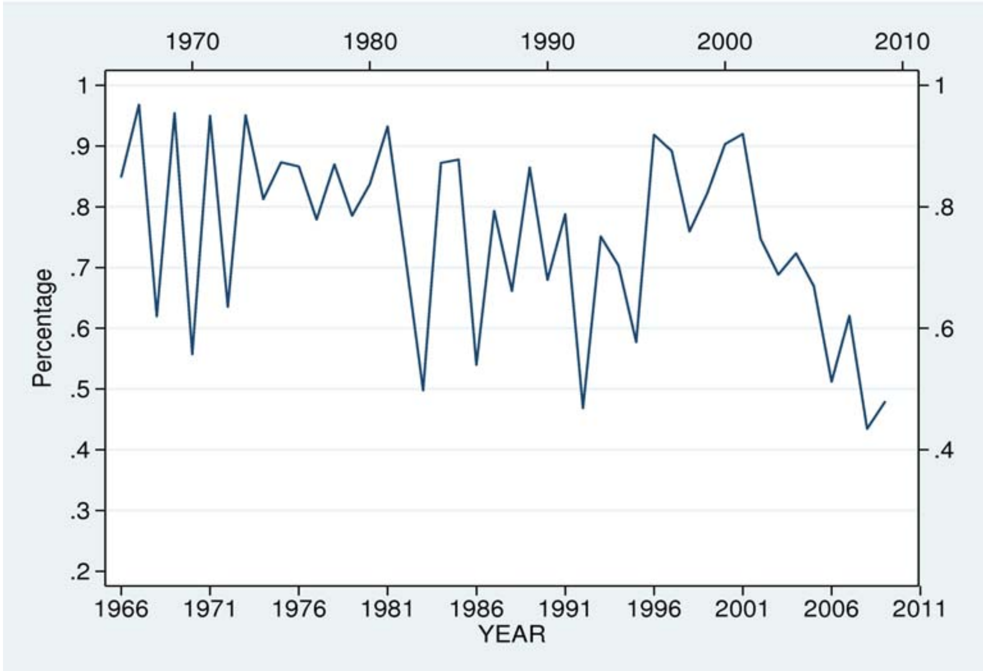


**Figure 3.** Percentage of Domestic Recalls

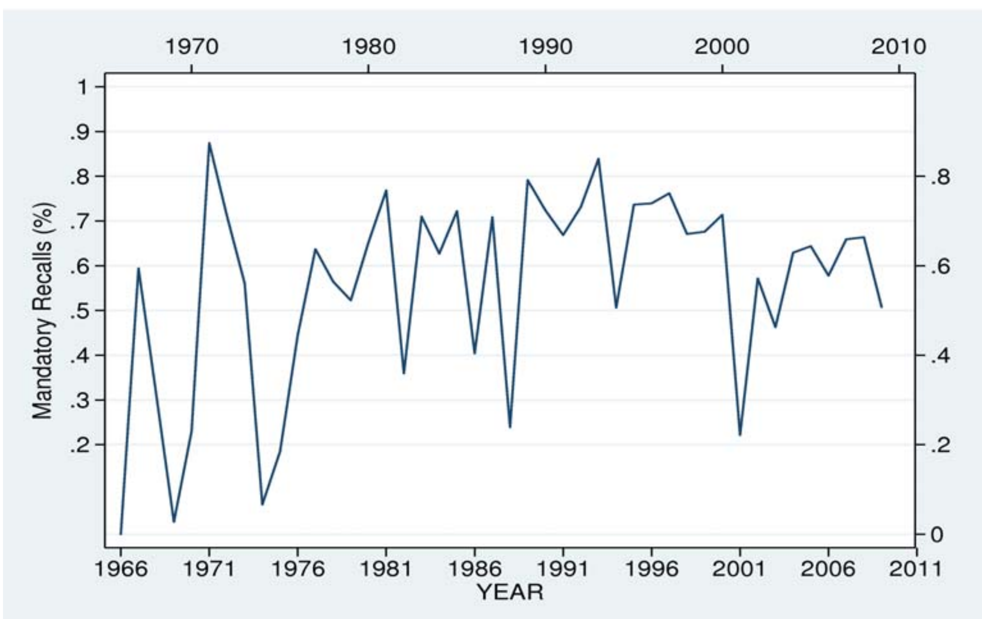


**Figure 4.** Proportion of Vehicles Recalled

**Table 1.** Summary Statistics of Recalls: 1988 to 2001

| Variable | Category | Frequency | Percent | Variable | Category | Frequency | Percent |
|---|---|---|---|---|---|---|---|
| **Initiation** | MFR | 3,886 | 74.30 | **Hazard** | A | 2,870 | 54.96 |
| | ODI | 1,032 | 19.73 | | B | 241 | 4.62 |
| | OVSC | 312 | 5.79 | | C | 2,097 | 40.16 |
| | | | | | D | 14 | 0.27 |
| | Total | 5,230 | 100.00 | | Total | 5,222 | 100.00 |
| **Domestic** | No | 1,208 | 23.09 | **Type of recall** | Vehicle | 4,485 | 85.71 |
| | Yes | 4,023 | 76.91 | | Other† | 748 | 14.29 |
| | Total | 5,231 | 100.00 | | Total | 5,233 | 100.00 |

† Includes tires and other equipment.

**Table 2.** Recalls by Year: 1988 to 2001

| Year | (1) Issuance | (2) High hazard | (3) Foreign | (4) Voluntary | (5) Vehicle† | (6) Units per recall | (7) Sales ‡ |
|---|---|---|---|---|---|---|---|
| 1988 | 237 | 27.43 | 20.25 | 81.86 | 83.12 | 9.7 | 15.7 |
| 1989 | 283 | 13.12 | 20.14 | 74.56 | 83.04 | 27.4 | 14.9 |
| 1990 | 270 | 8.15 | 20.37 | 67.78 | 77.41 | 68.1 | 14.1 |
| 1991 | 288 | 0.00 | 19.44 | 62.15 | 78.82 | 56.7 | 12.6 |
| 1992 | 221 | 14.93 | 22.17 | 67.87 | 85.52 | 59.6 | 13.1 |
| 1993 | 284 | 69.26 | 15.49 | 64.44 | 84.86 | 69.3 | 14.2 |
| 1994 | 302 | 61.92 | 23.84 | 75.50 | 86.09 | 52.2 | 15.3 |
| 1995 | 345 | 51.59 | 29.45 | 70.72 | 82.61 | 141.7 | 15.1 |
| 1996 | 331 | 58.66 | 25.98 | 72.26 | 86.10 | 75.4 | 15.4 |
| 1997 | 425 | 60.57 | 22.82 | 66.35 | 87.53 | 166.4 | 15.5 |
| 1998 | 475 | 66.88 | 28.21 | 78.11 | 89.47 | 76.6 | 15.5 |
| 1999 | 531 | 70.24 | 24.86 | 76.08 | 90.40 | 130.0 | 16.9 |
| 2000 | 663 | 84.29 | 25.49 | 81.00 | 87.48 | 112.0 | 17.6 |
| 2001 | 578 | 79.03 | 18.69 | 83.56 | 86.51 | 96.3 | 16.6 |

Source: NHTSA
∗ Columns (2) to (5) are in percentages.
Units per recall is the average number of vehicles per recall issued.
† The recalls for tires and other equipment are excluded.
‡ Total vehicle sales in the U.S., in millions of units for the model year.

**Table 3.** Characteristics of Recalls: 1988 to 2001

| Variable | Obs. | Mean | Std. dev. | Min | Max |
|---|---|---|---|---|---|
| Potential number of units affected † | 5,217 | 94,237 | 625,516 | 1 | 32 |
| Potential number of units defective † | 5,005 | 93,456 | 30,458 | 1 | 32 |
| Domestic manufacturer | 5,231 | 0.77 | .42 | 0 | 1 |
| No. of units involved † | 4,802 | 93,994 | 652,414 | 1 | 32 |
| No. of units inspected with defect † | 4,972 | 2,138 | 17,289 | 1 | 0.4 |
| No. of units corrected † | 4,917 | 41,814 | 192,508 | 0 | 6 |
| Ratio of units corrected to units issued | 4,802 | 0.55 | .31 | 0 | 7.5 |
| No. of unreachable units † | 4,911 | 8,477 | 400,198 | 0 | 28 |
| Hazard category code | 5,222 | 3.14 | .97 | 1 | 4 |

Source: NHTSA.
† Units of maximum values are in millions.

**Table 4.** Summary Statistics for Individual-Level Observations ( *n* = 74,806 )

| Variable | Mean | Std. dev. | Min | Max |
|---|---|---|---|---|
| Male | .5008 | .5000 | 0 | 1 |
| Strike | .5868 | .4924 | 0 | 1 |
| Age 16 to 25 | .3224 | .4674 | 0 | 1 |
| Age 26 to 35 | .2439 | .4295 | 0 | 1 |
| Age 36 to 49 | .2393 | .4266 | 0 | 1 |
| Age 50+ | .1944 | .3957 | 0 | 1 |
| | | | | |
| Mustang | .0552 | .2283 | 0 | 1 |
| Escort | .0992 | .2990 | 0 | 1 |
| Accord | .1043 | .3057 | 0 | 1 |
| Century | .0215 | .1451 | 0 | 1 |
| Cavalier | .0905 | .2868 | 0 | 1 |
| Marquis | .0177 | .1319 | 0 | 1 |
| Cougar | .0163 | .1268 | 0 | 1 |
| Civic | .0819 | .2742 | 0 | 1 |
| Corolla | .0591 | .2359 | 0 | 1 |
| Cherokee | .0584 | .2345 | 0 | 1 |
| Sentra | .0553 | .2287 | 0 | 1 |
| Taurus | .0839 | .2773 | 0 | 1 |
| Sable | .0211 | .1437 | 0 | 1 |
| GrandAM | .0596 | .2367 | 0 | 1 |
| Camry | .0732 | .2604 | 0 | 1 |
| Altima | .0273 | .1630 | 0 | 1 |
| Intrepid | .0147 | .1202 | 0 | 1 |
| LeSabre | .0243 | .1539 | 0 | 1 |
| Caravan | .0365 | .1875 | 0 | 1 |

**Table 5.** Summary Statistics for All Accidents of Composite Types: 1988 to 2001.
( $c$ = 3,952 )

| Variable | Mean | Std. dev. | Min | Max |
|---|---|---|---|---|
| **Dependent variable:** | | | | |
| Ln_Acc_Type | 2.5348 | 1.0136 | -.6931 | 4.9488 |
| Acc_Type | 18.7869 | 15.5972 | .5 | 141 |
| Ln_Acc_Type_NI | 2.1823 | 1.0391 | -.6931 | 4.6347 |
| Acc_Type_NI | 13.4728 | 11.3405 | .5 | 103 |
| Ln_Acc_Type_I | 1.1856 | 1.0638 | -.6931 | 3.9512 |
| Acc_Type_I | 5.3363 | 5.3229 | .5 | 52 |
| **Independent variables:** | | | | |
| Male | .5 | .5000 | 0 | 1 |
| Strike | .5 | .5000 | 0 | 1 |
| Age 16 to 25 | .25 | .4331 | 0 | 1 |
| Age 26 to 35 | .25 | .4331 | 0 | 1 |
| Age 36 to 49 | .25 | .4331 | 0 | 1 |
| Age 50+ | .25 | .4331 | 0 | 1 |
| Rec_Rate | .1400 | .2568 | 0 | 1.616 |
| Rec_Rate_H | .0740 | .1894 | 0 | 1.616 |
| Sales | 8.1146 | 3.7835 | 1.2669 | 16.6016 |
| Sales square | 80.1586 | 69.9183 | 1.6051 | 275.6132 |
| Design change | .4049 | .4909 | 0 | 1 |
| Vintage | .4111 | .0805 | .1925 | .9866 |
| Mustang | .0567 | .2313 | 0 | 1 |
| Escort | .0567 | .2313 | 0 | 1 |
| Accord | .0567 | .2313 | 0 | 1 |
| Century | .0567 | .2313 | 0 | 1 |
| Cavalier | .0567 | .2313 | 0 | 1 |
| Marquis | .0567 | .2313 | 0 | 1 |
| Cougar | .0567 | .2313 | 0 | 1 |
| Civic | .0567 | .2313 | 0 | 1 |
| Corolla | .0567 | .2313 | 0 | 1 |
| Cherokee | .0536 | .2253 | 0 | 1 |
| Sentra | .0567 | .2313 | 0 | 1 |
| Taurus | .0567 | .2313 | 0 | 1 |
| Sable | .0567 | .2313 | 0 | 1 |
| GrandAM | .0567 | .2313 | 0 | 1 |
| Camry | .0567 | .2313 | 0 | 1 |
| Intrepid | .0324 | .1771 | 0 | 1 |
| Altima | .0202 | .1408 | 0 | 1 |
| LeSabre | .0567 | .2313 | 0 | 1 |
| Caravan | .0405 | .1971 | 0 | 1 |

**Table 6.** Summary Statistics for Correction Rate Estimation

| Variable | Obs. | Mean | Std. dev. | Min | Max |
|---|---|---|---|---|---|
| **Dependent variable:** | | | | | |
| Ln_Ave_Acct | 87 | 5.07 | .7433 | 3.34 | 6.80 |
| Ave_Accidents | 87 | 210.35 | 171.46 | 28.25 | 901.75 |
| **Independent variables:** | | | | | |
| Corrate | 87 | 69.13 | 20.7120 | 8.37 | 100 |
| Ave_Sales | 87 | 687.11 | 559.1277 | 98.09 | 2850.51 |
| Ave_Sales_Square | 87 | 781156.2 | 1445979 | 9426.273 | 8125385 |
| Size_Recall | 87 | 2.41 | 3.68 | .0024 | 22.16 |
| Size_Square | 87 | 1.92 | 6.1088 | 0 | 49.12 |
| Recalls_After | 87 | 2.13 | 2.59 | 0 | 17 |
| Hazard | 87 | .54 | .5013 | 0 | 1 |
| Ford | 87 | .2529 | .4372 | 0 | 1 |
| GM | 87 | .2759 | .4495 | 0 | 1 |
| Chrysler | 87 | .2414 | .4304 | 0 | 1 |
| Toyota | 87 | .1034 | .3063 | 0 | 1 |
| Honda | 87 | .0920 | .2906 | 0 | 1 |
| Nissan | 87 | .0345 | .1835 | 0 | 1 |
| Year_1988 | 87 | .0690 | .2549 | 0 | 1 |
| Year_1989 | 87 | .0230 | .1507 | 0 | 1 |
| Year_1990 | 87 | .0230 | .1507 | 0 | 1 |
| Year_1991 | 87 | .0805 | .2736 | 0 | 1 |
| Year_1992 | 87 | .0920 | .2906 | 0 | 1 |
| Year_1993 | 87 | .0805 | .2736 | 0 | 1 |
| Year_1994 | 87 | .0690 | .2549 | 0 | 1 |
| Year_1995 | 87 | .0920 | .2906 | 0 | 1 |
| Year_1996 | 87 | .1609 | .3696 | 0 | 1 |
| Year_1997 | 87 | .1724 | .3799 | 0 | 1 |
| Year_1998 | 87 | .1379 | .3468 | 0 | 1 |

*Notes:* If many recalls have been issued in a particular year, only the most hazardous or the largest recall is included, if two or more have the same hazard level. The recalls between 1999 and 2001 are not included in this sample because the accident data sets after 2001 are not available. Some recalls with very low correction rates (fewer than 1 percent) are dropped.

**Table 7.A.** Synthetic Panel Data Model—Grouping by the 19 Vehicle Models with All Injury Levels Including No Injury, and Striking and Struck Vehicles

| | Without average | | | | With average | | | |
|---|---|---|---|---|---|---|---|---|
| | All recalls | | Hazard recalls | | All recalls | | Hazard recalls | |
| Variables | | | | | | | | |
| Rec_Rate | -.1053 | (.0713) | - | | -.1163 | (.0712) | - | |
| Rec_Rate_H | - | | -.1676 | (.1020) | - | | -.1495 | (.1022) |
| Strike | - | | - | | -.9995 | (.4069)** | -.9579 | (.4082)** |
| Male | - | | - | | -.8387 | (.3876)** | -.8305 | (.3878)** |
| Age 16 to 25 | - | | - | | 1.6933 | (.2940)*** | 1.6965 | (.2925)*** |
| Age 26 to 35 | - | | - | | .0273 | (.4393) | .0389 | (.4390) |
| Age 36 to 49 | - | | - | | .3189 | (.4239) | .2926 | (.4237) |
| Design change | .1380 | (.0355)*** | .1341 | (.0357)*** | .1410 | (.0359)*** | .1369 | (.0360)*** |
| Vintage | 1.1131 | (.2822)*** | 1.1392 | (.2863)*** | .7942 | (.2749)*** | .8128 | (.2796)*** |
| Sales | .4661 | (.0361)*** | .4597 | (.0363)*** | .4090 | (.0326)*** | .4047 | (.0331)*** |
| Sales square | -.0159 | (.0018)*** | -.0156 | (.0018)*** | -.0141 | (.0016)*** | -.0139 | (.0017)*** |
| Constant | 2.8033 | (.2368)*** | 2.8240 | (.2344)*** | 3.5952 | (.3961)*** | 3.5835 | (.3953)*** |
| Num. of obs. | 247 | | 247 | | 247 | | 247 | |
| $R^2$ | 0.7612 | | 0.7603 | | 0.8545 | | .8542 | |

*Notes:* Standard errors are in parentheses. Estimation also includes a battery of year dummies (not shown).

**Table 7.B.** Synthetic Panel Data Model—Grouping by the 19 Vehicle Models with All Injury Levels including "No Injury," and Striking Vehicles Only

| | Without average | | | | With average | | | |
|---|---|---|---|---|---|---|---|---|
| | All recalls | | Hazard recalls | | All recalls | | Hazard recalls | |
| Variables | | | | | | | | |
| Rec_Rate | -.1018 | (.0709) | - | | -.1573 | (.0750)** | - | |
| Rec_Rate_H | - | | -.1469 | (.1018) | - | | -.1983 | (.1052)* |
| Male | - | | - | | -1.0074 | (.3118)*** | -1.0485 | (.3117)*** |
| Age 16 to 25 | - | | - | | 1.4239 | (.2547)*** | 1.2909 | (.2647)*** |
| Age 26 to 35 | - | | - | | .0628 | (.3363) | .0647 | (.3369) |
| Age 36 to 49 | - | | - | | .3967 | (.3679) | .4124 | (.3664) |
| Design Change | .1608 | (.0353)*** | .1571 | (.0356)*** | .1497 | (.0374)*** | .1476 | (.0369)*** |
| Vintage | .9084 | (.2836)*** | .9210 | (.2883)*** | .5988 | (.2826)** | .6440 | (.2867)** |
| Sales | .4901 | (.0366)*** | .4848 | (.0369)*** | .4159 | (.0338)*** | .4210 | (.0349)*** |
| Sales square | -.0172 | (.0018)*** | -.0169 | (.0018)*** | -.0145 | (.0017)*** | -.0146 | (.0017)*** |
| Constant | 2.1020 | (.2447)*** | 2.1225 | (.2421)*** | 2.5606 | (.3077)*** | 2.5661 | (.3161)*** |
| Num. of obs. | 247 | | 247 | | 247 | | 247 | |
| $R^2$ | 0.7282 | | 0.7268 | | 0.8347 | | 0.8260 | |

*Notes:* Standard errors are in parentheses. Estimation also include a battery of year dummies (not shown).

**Table 7.C.** Synthetic Panel Data Model—Grouping by the 19 Vehicle Models with Injury Related Accidents Only, and Striking Vehicles Only

| | Without average | | | | With average | | | |
|---|---|---|---|---|---|---|---|---|
| | All recalls | | Hazard recalls | | All recalls | | Hazard recalls | |
| Variables | | | | | | | | |
| Rec_Rate | -.2425 | (.1044)** | - | | -.2801 | (.0960)*** | - | |
| Rec_Rate_H | - | | -.3746 | (.1577)** | - | | -.3611 | (.1387)*** |
| Male | - | | - | | .4868 | (.2061)** | .4888 | (.2071)** |
| Age 16 to 25 | - | | - | | 1.7586 | (.2349)*** | 1.7547 | (.2351)*** |
| Age 26 to 35 | - | | - | | 1.2402 | (.2582)*** | 1.2026 | (.2600)*** |
| Age 36 to 49 | - | | - | | .9450 | (.2599)*** | .9371 | (.2618)*** |
| Design Change | .2186 | (.0520)*** | .2105 | (.0521)*** | .1677 | (.0471)*** | .1596 | (.0473)*** |
| Vintage | .3295 | (.4118) | .4019 | (.4170) | .3607 | (.3671) | .3992 | (.3722) |
| Sales | .5069 | (.0524)*** | .4961 | (.0528)*** | .4248 | (.0441)*** | .4178 | (.0448)*** |
| Sales square | -.0180 | (.0026)*** | -.0174 | (.0026)*** | -.0154 | (.0022)*** | -.0150 | (.0022)*** |
| Constant | .9748 | (.3425)*** | .9950 | (.3398)** | .2940 | (.2994) | .3121 | (.2998) |
| Num. of obs. | 247 | | 247 | | 247 | | 247 | |
| $R^2$ | .6463 | | .6440 | | .7955 | | 0.7921 | |

*Notes:* Standard errors are in parentheses. Estimation also include a battery of year dummies (not shown)

**Table 7.D.** Synthetic Panel Data Model—Grouping by the 19 Vehicle Models with No Injury Related Accidents Only, and Striking Vehicles Only

| | Without average | | | | With average | | | |
|---|---|---|---|---|---|---|---|---|
| | All recalls | | Hazard recalls | | All recalls | | Hazard recalls | |
| Variables | | | | | | | | |
| Rec_Rate | -.1186 | (.0802) | - | | -.1769 | (.0857)** | - | |
| Rec_Rate_H | - | | -.1990 | (.1151)* | - | | -.2498 | (.1205)** |
| Male | - | | - | | -.8231 | (.3482)** | -.8758 | (.3491)** |
| Age 16 to 25 | - | | - | | 1.3719 | (.2704)*** | 1.2652 | (.2802)*** |
| Age 26 to 35 | - | | - | | .0318 | (.3754) | .0347 | (.3764) |
| Age 36 to 49 | - | | - | | .3923 | (.4119) | .4002 | (.4108) |
| Design change | .1470 | (.0340)*** | .1422 | (.0402)*** | .1375 | (.0427)*** | .1340 | (.0422)*** |
| Vintage | 1.0982 | (.3176)*** | 1.1246 | (.3220)*** | .8221 | (.3160)*** | .8741 | (.3208)*** |
| Sales | .4944 | (.0406)*** | .4848 | (.0407)*** | .4171 | (.0369)*** | .4180 | (.0382)*** |
| Sales square | -.0173 | (.0020)*** | -.0169 | (.0020)*** | -.0146 | (.0019)*** | -.0145 | (.0019)*** |
| Constant | 1.6619 | (.2663)*** | 1.6999 | (.2619)*** | 2.0585 | (.3320)*** | 2.0825 | (.3406)*** |
| Num. of obs. | 247 | | 247 | | 247 | | 247 | |
| $R^2$ | .7232 | | .7221 | | .8109 | | .8051 | |

*Notes:* Standard errors are in parentheses. Estimation also include a battery of year dummies (not shown).

**Table 8.** Number of Accidents in Each Group in Each Year

| Group no. | 1988 | 1989 | 1990 | 1991 | 1992 | 1993 | 1994 | 1995 | 1996 | 1997 | 1998 | 1999 | 2000 | 2001 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | 293 | 320 | 274 | 214 | 215 | 221 | 200 | 246 | 260 | 310 | 320 | 437 | 429 | 387 |
| 2 | 618 | 589 | 510 | 390 | 438 | 479 | 492 | 474 | 501 | 502 | 458 | 725 | 634 | 613 |
| 3 | 335 | 345 | 446 | 459 | 535 | 615 | 561 | 523 | 530 | 573 | 542 | 799 | 760 | 781 |
| 4 | 190 | 163 | 118 | 88 | 84 | 77 | 73 | 96 | 93 | 74 | 82 | 149 | 148 | 175 |
| 5 | 491 | 450 | 413 | 416 | 426 | 467 | 428 | 429 | 348 | 444 | 421 | 681 | 720 | 633 |
| 6 | 145 | 80 | 76 | 65 | 68 | 65 | 71 | 87 | 86 | 86 | 70 | 147 | 147 | 132 |
| 7 | 183 | 129 | 147 | 80 | 91 | 113 | 90 | 97 | 70 | 92 | 52 | 37 | 23 | 18 |
| 8 | 264 | 273 | 295 | 285 | 313 | 399 | 382 | 421 | 374 | 467 | 466 | 742 | 708 | 738 |
| 9 | 218 | 167 | 216 | 212 | 250 | 295 | 261 | 291 | 327 | 371 | 314 | 513 | 488 | 501 |
| 10 | 34 | 35 | 21 | 11 | 141 | 213 | 299 | 394 | 321 | 367 | 389 | 750 | 749 | 644 |
| 11 | 295 | 301 | 347 | 344 | 272 | 333 | 331 | 302 | 314 | 329 | 215 | 276 | 248 | 233 |
| 12 | 193 | 255 | 325 | 262 | 316 | 507 | 476 | 443 | 412 | 488 | 484 | 717 | 705 | 695 |
| 13 | 51 | 76 | 93 | 76 | 94 | 113 | 92 | 117 | 101 | 108 | 125 | 195 | 175 | 163 |
| 14 | 165 | 216 | 264 | 240 | 220 | 352 | 355 | 303 | 362 | 342 | 264 | 507 | 469 | 396 |
| 15 | 154 | 152 | 206 | 216 | 254 | 375 | 394 | 410 | 394 | 376 | 440 | 734 | 723 | 645 |
| 16 | - | - | - | - | - | - | 71 | 151 | 182 | 252 | 212 | 258 | 379 | 377 |
| 17 | - | - | - | - | - | - | - | - | - | 168 | 185 | 181 | 235 | 250 |
| 18 | 121 | 106 | 109 | 106 | 120 | 129 | 142 | 104 | 118 | 116 | 108 | 424 | 175 | 181 |
| 19 | - | - | - | - | 144 | 195 | 232 | 190 | 250 | 235 | 236 | 420 | 422 | 401 |
| Totals | 3,750 | 3,657 | 3,860 | 3,464 | 3,981 | 4,948 | 4,950 | 5,078 | 5,043 | 5,700 | 5,383 | 8,692 | 8,337 | 7,963 |

*Note:* Total number of observations is 74,806, which is equal to the number of observations in Table 4.

**Table 9.** Estimation of the Effect of Recalls on Accidents: All Recalls

| | Striking + struck all injury levels | | Striking only all injury levels | | Striking only injury only | | Striking only no injury only | |
|---|---|---|---|---|---|---|---|---|
| Rec_Rate | -.0999 | (.0331)*** | -.0788 | (.0448)* | -.1639 | (.0571)*** | -.0523 | (.0511) |
| Strike | .3009 | (.0474)*** | - | | - | | - | |
| Male | .1046 | (.0474)** | .1487 | (.0694)** | -.1883 | (.0741)** | .2917 | (.0686)*** |
| Age 16 to 25 | .2350 | (.0670)*** | .3958 | (.0981)*** | .3750 | (.1048)*** | .4327 | (.0970)*** |
| Age 26 to 35 | .1450 | (.0670)** | .1985 | (.0981)** | .2202 | (.1048)** | .2234 | (.0970)** |
| Age 36 to 49 | .2074 | (.0670)*** | .2115 | (.0981)** | .1905 | (.1048)* | .2211 | (.0970)** |
| Design change | .1289 | (.0165)*** | .1664 | (.0223)*** | .1487 | (.0284)*** | .1410 | (.0255)*** |
| Vintage | 1.0928 | (.1365)*** | .9641 | (.1849)*** | .4367 | (.2354)* | .9655 | (.2109)*** |
| Sales | .4980 | (.0182)*** | .5332 | (.0247)*** | .4174 | (.0314)*** | .5324 | (.0281)*** |
| Sales_Square | -.0172 | (.0009)*** | -.0189 | (.0012)*** | -.0142 | (.0015)*** | -.0188 | (.0013)*** |
| Mustang | 1.1827 | (.1503)*** | 1.4818 | (.2191)*** | 1.4305 | (.2364)*** | 1.4053 | (.2184)*** |
| Escort | .7378 | (.1478)*** | .8937 | (.2160)*** | 1.1269 | (.2317)*** | .7949 | (.2143)*** |
| Accord | .8092 | (.1478)*** | .9204 | (.2160)*** | 1.0205 | (.2316)*** | .9067 | (.2143)*** |
| Century | .3949 | (.1508)*** | .4946 | (.2197)** | .3797 | (.2373) | .4053 | (.2192)* |
| Cavalier | .6818 | (.1471)*** | .8134 | (.2151)*** | 1.0537 | (.2302)*** | .7213 | (.2131)*** |
| Marquis | .3448 | (.1527)** | .5001 | (.2222)** | .4097 | (.2409)* | .4602 | (.2224)** |
| Cougar | .9195 | (.1592)*** | 1.0892 | (.2303)*** | .9139 | (.2530)*** | 1.0224 | (.2329)*** |
| Civic | .7866 | (.1471)*** | .9449 | (.2151)*** | 1.1320 | (.2303)*** | .8585 | (.2131)*** |
| Corolla | .7224 | (.1475)*** | .8625 | (.2156)*** | 1.0483 | (.2311)*** | .7482 | (.2138)*** |
| Cherokee | .1734 | (.1482) | .2760 | (.2165) | .4625 | (.2325)** | .2783 | (.2150) |
| Sentra | 1.0731 | (.1491)*** | 1.2012 | (.2176)*** | 1.2835 | (.2341)*** | 1.1022 | (.2165)*** |

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| Taurus | .5087 | (.1483)*** | .6163 | (.2166)*** | .7053 | (.2326)*** | .5954 | (.2152)*** |
| Sable | .7953 | (.1529)*** | .9384 | (.2224)*** | .8441 | (.2412)*** | .8351 | (.2227)*** |
| GrandAM | .7159 | (.1475)*** | .8304 | (.2157)*** | .8594 | (.2311)*** | .8044 | (.2139)*** |
| Camry | .4981 | (.1474)*** | .6083 | (.2155)*** | .7667 | (.2309)*** | .5531 | (.2137)*** |
| Altima | 1.0891 | (.1517)*** | 1.1960 | (.2227)*** | 1.1868 | (.2418)*** | 1.1242 | (.2231)*** |
| Intrepid | .9555 | (.1564)*** | 1.0261 | (.2268)*** | .7221 | (.2479)*** | 1.0860 | (.2284)*** |
| LeSabre | .2887 | (.1499)* | .3796 | (.2186)* | .2699 | (.2356) | .3451 | (.2178) |
| Constant | -1.2989 | (.1801)*** | -1.3236 | (.2498)*** | -1.6285 | (.2964)*** | -1.7787 | (.2688)*** |
| Num. of obs. | 3952 | | 1976 | | 1976 | | 1976 | |
| # of groups | 304 | | 152 | | 152 | | 152 | |
| $R^2$:within | 0.4467 | | 0.4763 | | 0.3426 | | 0.3908 | |
| $R^2$:between | 0.7402 | | 0.7479 | | 0.7270 | | 0.7605 | |
| $R^2$:overall | 0.6345 | | 0.6535 | | 0.5734 | | 0.6237 | |

*Notes:* Standard errors are in parentheses. The vehicle model Dodge Caravan is the omitted model. Estimation also includes a battery of highly significant year dummies (not shown). Breusch-Pagan Lagrange Multiplier (OLS vs. RE):

$\chi^2(1)$= 3918.28, P-value = 0.000 for column (1)

$\chi^2(1)$= 2189.26, P-value = 0.000 for column (2)

$\chi^2(1)$= 1300.88, P-value = 0.000 for column (3)

$\chi^2(1)$= 1422.49, P-value = 0.000 for column (4)

**Table 10.** Estimation of the Effect of Recalls on Accidents: Hazard Recalls

| | Striking + struck all injury levels | | Striking only all injury levels | | Striking only injury only | | Striking only no injury only | |
|---|---|---|---|---|---|---|---|---|
| Rec_Rate_H | -.1485 | (.0471)*** | -.1140 | (.0638)* | -.1953 | (.0813)** | -.0578 | (.0728) |
| Strike | .3009 | (.0474)*** | - | | - | | - | |
| Male | .1046 | (.0474)** | .1487 | (.0694)** | -.1883 | (.0741)** | .2917 | (.0686)*** |
| Age 16 to 25 | .2350 | .0670)*** | .3958 | (.0981)*** | .3750 | (.1048)*** | .4327 | (.0970)*** |
| Age 26 to 35 | .1450 | (.0670)** | .1985 | (.0981)** | .2201 | (.1048)** | .2234 | (.0970)** |
| Age 36 to 49 | .2074 | (.0670)*** | .2115 | (.0981)** | .1905 | (.1048)* | .2211 | (.0970)** |
| Design change | .1260 | .0165)*** | .1641 | (.0224)*** | .1446 | (.0285)*** | .1398 | (.0255)*** |
| Vintage | 1.1270 | .1384)*** | .9890 | (.1874)*** | .4609 | (.2388)* | .9702 | (.2138)*** |
| Sales | .4951 | .0184)*** | .5312 | (.0249)*** | .4166 | (.0317)*** | .5325 | (.0284)*** |
| Sales_Square | -.0170 | (.0009)*** | -.0188 | (.0012)*** | -.0141 | (.0015)*** | -.0188 | (.0014)*** |
| Mustang | 1.1935 | (.1501)*** | 1.4908 | (.2189)*** | 1.4540 | (.2360)*** | .4133 | (.2181)*** |
| Escort | .7406 | (.1477)*** | .8965 | (.2159)*** | 1.1399 | (.2315)*** | .7998 | (.2142)*** |
| Accord | .8101 | (.1478)*** | .9218 | (.2159)*** | 1.0320 | (.2316)*** | .9114 | (.2142)*** |
| Century | .3886 | (.1509)*** | .4902 | (.2198)** | .3775 | (.2375) | .4054 | (.2194)* |

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| Cavalier | .6786 | (.1471)*** | .8114 | (.2151)*** | 1.0562 | (.2303)*** | .7228 | (.2132)*** |
| Marquis | .3534 | (.1525)** | .5074 | (.2219)** | .4314 | (.2406)* | .4678 | (.2221)** |
| Cougar | .9282 | (.1590)*** | 1.0966 | (.2301)*** | .9361 | (.2527)*** | 1.0307 | (.2326)*** |
| Civic | .7793 | .1472)*** | .9397 | (.2152)*** | 1.1274 | (.2304)*** | .8578 | (.2133)*** |
| Corolla | .7235 | (.1475)*** | .8640 | (.2156)*** | 1.0588 | (.2310)*** | .7523 | (.2138)*** |
| Cherokee | .1724 | (.1482) | .2757 | .2165) | .4692 | (.2325)** | .2812 | (.2150) |
| Sentra | 1.0733 | (.1491)*** | .2017 | (.2176)*** | 1.2893 | (.2341)*** | 1.1046 | (.2165)*** |
| Taurus | .5023 | (.1484)*** | .6118 | (.2167)*** | .7029 | (.2328)*** | .5954 | (.2153)*** |
| Sable | .7943 | (.1529)*** | .9366 | (.2224)*** | .8491 | (.2413)*** | .8377 | (.2227)*** |
| GrandAM | .7175 | (.1475)*** | .8322 | (.2156)*** | .8705 | (.2310)*** | .8088 | (.2138)*** |
| Camry | .4925 | (.1475)*** | .6145 | (.2156)*** | .7671 | .2311)*** | .5541 | (.2138)*** |
| Altima | 1.0999 | (.1529)*** | 1.2050 | (.2224)*** | 1.2120 | (.2413)*** | 1.1330 | (.2227)*** |
| Intrepid | .9553 | (.1564)*** | 1.0263 | (.2268)*** | .7278 | (.2479)*** | 1.0884 | (.2284)*** |
| LeSabre | .2959 | (.1497)* | .3854 | (.2184)* | .2905 | (.2352) | .3525 | (.2174) |
| Constant | -1.3024 | (.1799)*** | 1.3274 | (.2496)*** | -1.6503 | (.2963)*** | -1.7872 | (.2686)*** |
| Num. of obs. | 3952 | | 1976 | | 1976 | | 1976 | |
| Num. of groups | 304 | | 152 | | 152 | | 152 | |
| $R^2$ :within | 0.4468 | | 0.4763 | | 0.3417 | | 0.3906 | |

| | | | | |
|---|---|---|---|---|
| $R^2$ :between | 0.7402 | 0.7479 | 0.7270 | 0.7605 |
| $R^2$ :overall | 0.6345 | 0.6535 | 0.5730 | 0.6236 |

*Notes:* Standard errors are in parentheses. The Dodge Caravan is the omitted model. Estimation also includes a battery of highly significant year dummies (not shown).

Breusch-Pagan Lagrange Multiplier (OLS vs. RE):

$\chi^2$(1)= 3919.48, P-value = 0.000 for column (1)

$\chi^2$(1)= 2189.42, P-value = 0.000 for column (2)

$\chi^2$(1)= 1298.10, P-value = 0.000 for column (3)

$\chi^2$(1)= 1421.99, P-value = 0.000 for column (4)

**Table 11.** Estimation of the Effect of Recalls on Accidents Domestic vs. Foreign Recalls

| | Striking vehicles only, and all injury levels | | | | | |
|---|---|---|---|---|---|---|
| | All recalls | | Domestic recalls | | Foreign recalls | |
| Rec_Rate | -.0788 | (.0448)* | -.1248 | (.0607)** | -.1006 | (.0593)* |
| Male | .1487 | (.0694)** | .2219 | (.0909)** | -.0089 | (.0634) |
| Age 16 to 25 | .3958 | (.0981)*** | .1629 | (.1286) | .8986 | (.0896)*** |
| Age 26 to 35 | .1985 | (.0981)** | .0064 | (.1286) | .6137 | (.0896)*** |
| Age 36 to 49 | .2115 | (.0981)** | .1092 | (.1286) | .4322 | (.0896)*** |
| Design Change | .1664 | (.0223)*** | .1380 | (.0299)*** | .1145 | (.0313)*** |
| Vintage | .9641 | (.1849)*** | 2.0111 | (.2638)*** | -.2324 | (.2416) |
| Sales | .5332 | (.0247)*** | .6710 | (.0332)*** | .3248 | (.0316)*** |
| Sales_Square | -.0189 | (.0012)*** | -.0234 | (.0016)*** | -.0124 | (.0016)*** |
| Mustang | 1.4818 | (.2191)*** | 1.8191 | (.2402)*** | - | |
| Escort | .8937 | (.2160)*** | .8733 | (.2348)*** | - | |
| Accord | .9204 | (.2160)*** | - | | - | |
| Century | .4946 | (.2197)** | .8701 | (.2415)*** | - | |
| Cavalier | .8134 | (.2151)*** | .8165 | (.2332)*** | - | |
| Marquis | .5001 | (.2222)** | .9402 | (.2455)*** | - | |
| Cougar | 1.0892 | (.2303)*** | 1.7298 | (.2595)*** | - | |
| Civic | .9449 | (.2151)*** | - | | -.1760 | (.1125) |
| Corolla | .8625 | (.2156)*** | - | | -.3494 | (.1150)*** |
| Cherokee | .2760 | (.2165) | .3497 | (.2356) | - | |
| Sentra | 1.2012 | (.2176)*** | - | | -.2496 | (.1197)** |

| | | | | | | |
|---|---|---|---|---|---|---|
| Taurus | .6163 | (.2166)*** | .5171 | (.2362)** | - | |
| Sable | .9384 | (.2224)*** | 1.3733 | (.2458)*** | - | |
| GrandAM | .8304 | (.2157)*** | .9404 | (.2341)*** | - | |
| Camry | .6083 | (.2155)*** | - | | -.3557 | (.1092)*** |
| Altima | 1.1960 | (.2227)*** | - | | -.3601 | (.1298)*** |
| Intrepid | 1.0261 | (.2268)*** | 1.4116 | (.2488)*** | - | |
| LeSabre | .3796 | (.2186)* | .6909 | (.2393)*** | - | |
| Constant | -1.3236 | (.2498)*** | -2.6122 | (.3199)*** | 1.5567 | (.2397)*** |
| Num of Obs | | 1976 | | 1352 | | 624 |
| Num of Groups | | 152 | | 104 | | 48 |
| $R^2$ :within | | 0.4763 | | 0.4798 | | 0.6299 |
| $R^2$ :between | | 0.7479 | | 0.7362 | | 0.8390 |
| $R^2$ :overall | | 0.6535 | | 0.6433 | | 0.7226 |

Notes: Standard errors are in parentheses. The vehicle models Dodge Caravan and Honda Accord are the omitted models. Estimation also include a battery of highly significant year dummies (not shown).

Breusch-Pagan Lagrange Multiplier (OLS vs. RE):

$\chi^2(1)$= 2189.26, P-value = 0.000 for column (1)

$\chi^2(1)$= 1541.16, P-value = 0.000 for column (2)

$\chi^2(1)$= 266.55, P-value = 0.000 for column (3)

**Table 12. The Effect of Correcting Defects**

| Variables | Without hazard | | With hazard | |
|---|---|---|---|---|
| Correction rate | -.0048 | (.0019)** | -.0046 | (.0018)** |
| Ave_Sales | .0024 | (.0003)*** | .0023 | (.0003)*** |
| Ave_Sales_Square | -.0000005 | (.0000001)*** | -.00000049 | (.0000001)*** |
| Size_Recall | .0212 | (.0265) | .0218 | (.0261) |
| Size_Square | -.0205 | (.0118)* | -.0217 | (.0118)* |
| Recalls_After | -.0431 | (.0248)* | -.0443 | (.0234)* |
| Hazard | - | | -.0995 | (.0711) |
| Ford | -.2998 | (.1075)*** | -.2832 | (.1209)** |
| GM | -.5308 | (.1154)*** | -.5211 | (.1234)*** |
| Chrysler | -.6196 | (.0890)*** | -.6076 | (.0984)*** |
| Toyota | -.5701 | (.0901)*** | -.5813 | (.1030)*** |
| Honda | -.4087 | (.1034)*** | -.4014 | (.1206)*** |
| Year_1988 | -.3365 | (.1034)*** | -.3711 | (.0983)*** |
| Year_1989 | -.7306 | (.1465)*** | -.7488 | (.1397)*** |
| Year_1990 | -.3844 | (.1309)*** | -.4029 | (.1025)*** |
| Year_1991 | -.1944 | (.0943)** | -.2685 | (.1045)** |
| Year_1992 | -.2843 | (.1151)** | -.2978 | (.1210)** |
| Year_1993 | -.1662 | (.1616) | -.1515 | (.1616) |
| Year_1994 | -.2662 | (.2250) | -.2749 | (.2176) |
| Year_1995 | -.5608 | (.1175)*** | -.5863 | (.1239)*** |
| Year_1996 | -.2957 | (.0997)*** | -.2864 | (.0988)*** |
| Constant | 4.9222 | (.1407)*** | 4.9855 | (.1554)*** |
| $R^2$ | | 0.8797 | | 0.8831 |
| Num of Obs. | | 87 | | 87 |

*Notes:* Standard errors are in parentheses. Nissan is the omitted car maker.

**Table A1**. Variable Definitions

| Variable | Description |
|---|---|
| **Dependent variable:** | |
| Acc_Type | Number of accidents of a composite type |
| Ln_Acc_Type | The natural logarithm of *Acc_Type* |
| Acc_Type_NI | Number of accidents of a composite type with no injury |
| Ln_Acc_Type_NI | The natural logarithm of *Acc_Type_NI* |
| Acc_Type_I | Number of accidents of a composite type with injury |
| Ln_Acc_Type_I | The natural logarithm of *Acc_Type_I* |
| **Driver characteristics** | |
| Male | 1 if driver is male |
| Strike | 1 if the vehicle strikes other vehicles or objects |
| Age 16 to 25 | 1 if driver's age is fewer than 26 |
| Age 26 to 35 | 1 if driver's age is between 26 and 35 |
| Age 26 to 49 | 1 if driver's age is between 36 and 49 |
| Age 50+ | 1 if driver's age is 50 or higher |
| **Vehicle characteristics** | |
| Design Change | 1 if there is any substantial design change within the last 5 years |
| Vintage | The ratio of current and last year's vehicle stocks to total 5 year stock |
| Sales | The number of vehicles sold in the previous 4 years in 100,000s of units |
| Sales_Square | The square of the sales |
| Mustang | 1 if the vehicle model is Ford Mustang |
| ...............† | Other vehicle models |
| **Recall characteristics** | |
| Rec_Rate | The ratio of units recalled to units sold in the previous 4 years |
| Rec_Rate_H | The ratio of high hazard units recalled to units sold in the previous 4 years |
| **Other characteristics** | |
| Year_1988 to 2001 | Year Dummies for the 1988 to 2001 period |

† Other vehicle dummies. A total of 19 vehicle models are included in our estimation sample.

**Table A2**. Variable Definition for Correction Rate Estimation

| Variable | Description |
|---|---|
| **Dependent variable:** | |
| Ln_Ave_Acct † | The natural logarithm of the average number of accidents within 3 years |
| **Independent variables:** | |
| Correction Rate | Correction rates of a recall $i$, 18 months after issuance |
| Ave_Sales † | Vehicles' Sales (Units: 1,000 vehicles) |
| Ave_Sales_Square | The square of the sales |
| Size_Recall | The size of a recall (in vehicle Units: 100,000) |
| Size_Square | The square of the size of the recall |
| Recalls_After | The number of recalls after the recall $i$ within 3 years |
| Hazard | Whether the recall was category A or B (Hazardous) |
| Ford | Manufacturer dummy |
| .....‡ | Other manufacturer dummies |
| Year_1988 to 2001 | Year dummies for the 1988 to 2001 period |

† When a recall covers many vehicle models we sum them up.

‡ Other manufacturers dummies.

**Table A3.** Vehicle Models

| Variable | Vehicle models | Variable | Vehicle models |
|---|---|---|---|
| Mustang | Ford Mustang | Accord | Honda Accord |
| Escort | Ford Escort | Civic | Honda Civic |
| Century | Buick Century | Corolla | Toyota Corolla |
| Cavalier | Chevrolet Cavalier | Sentra | Nissan Sentra |
| Altima | Nissan Altima | Camry | Toyota Camry |
| Marquis | Mercury Marquis | Intrepid | Dodge Intrepid |
| Cougar | Mercury Cougar | LeSabre | Buick LeSabre |
| Cherokee | Jeep Cherokee | Caravan | Dodge Caravan |
| Taurus | Ford Taurus | | |
| Sable | Mercury Sable | | |
| GrandAM | Pontiac Grand AM | | |

**Table A4.** Estimation of the Effect of Recalls on Accidents: Hazard Recalls Specification without *Design Change* and *Vintage* Variables

| | Striking + struck all injury levels | | Striking only all injury levels | | Striking only injury only | | Striking only no injury only | |
|---|---|---|---|---|---|---|---|---|
| Rec_Rate_H | -.0830 | (.0465)* | -.0668 | (.0632) | -.1865 | (.0796)** | -.0074 | (.0716) |
| Strike | .3010 | (.0474)*** | - | | - | | - | |
| Male | .1046 | (.0474)** | .1487 | (.0694)** | -.1883 | (.0741)** | .2917 | (.0686)*** |
| Age 16 to 25 | .2351 | (.0670)*** | .3958 | (.0981)*** | .3750 | (.1048)*** | .4327 | (.0970)*** |
| Age 26 to 35 | .1450 | (.0670)** | .1984 | (.0981)** | .2201 | (.1048)** | .2233 | (.0970)** |
| Age 36 to 49 | .2073 | (.0670) *** | .2114 | (.0981)** | .1905 | (.1048)* | .2211 | (.0970)** |
| Design change | - | | - | | - | | - | |
| Vintage | - | | - | | - | | - | |
| Sales | .4443 | (.0176)*** | .4853 | (.0240)*** | .3935 | (.0302)*** | .4880 | (.0272)*** |
| Sales_Square | -.0151 | (.0009)*** | -.0171 | (.0012)*** | -.0133 | (.0015)*** | -.0171 | (.0013)*** |
| Constant | -.4143 | (.1525)*** | -.4932 | (.2140)** | -1.1918 | (.2455)*** | -.9906 | (.2243)*** |
| Num. of obs. | | 3952 | | 1976 | | 1976 | | 1976 |
| # of groups | | 304 | | 152 | | 152 | | 152 |
| $R^2$ :within | | 0.4300 | | 0.4551 | | 0.3317 | | 0.3755 |
| $R^2$ :between | | 0.7402 | | 0.7479 | | 0.7270 | | 0.7605 |
| $R^2$ :overall | | 0.6282 | | 0.6459 | | 0.5689 | | 0.6179 |

*Notes:* Standard errors are in parentheses. The Dodge Caravan is the omitted model. Estimation also includes a battery of highly significant year and vehicle dummies (not shown).

[1] The NHTSA estimated the total economic cost of motor vehicle crashes in the year 2000 as being more than $230.6 billion in terms of the present value of lifetime costs (NHTSA, 2002). Various regulations, such as the seat belt regulation and speed limits, have been put in place to reduce these costs. Viscusi, Vernon, and Harrington (2000, Ch. 22) provide a detailed discussion of the many regulations, other than recalls, affecting the automobile industry. The number of vehicle accidents in the United States has been fairly stable in the last decade going from a high of a bit more than 6.7 million in 1996 to just below 6 million in 2008. In that year 37,261 people were killed in the 5,811,000 police-reported motor vehicle traffic crashes. More than 2.35 million people were injured and 4,146,000 crashes involved property damage only (NHTSA, *Traffic Safety Facts*, 2010). Most accidents were likely caused by drivers' mistakes or misbehavior. However, it cannot be underestimated that vehicle defects may play a role in causing accidents.

[2] Peters (2005) discusses the phenomenal increase in the number of recalls in the last few years and blames the increased complexity of the cars and also the changes in the regulatory environment that came to be at the beginning of this decade with the passing of the Transportation Recall Enhancement, Accountability, and Documentation Act, which requires more communication between manufacturers and the government regarding potential safety issues. Another possible explanation is connected with the fact that nowadays more vehicle models use the same parts, and the effect of one defect on the safety of many vehicle models is more common than ever.

[3] Evans is the president of Science Serving Society, International Traffic Medicine Association (ITMA). He was affiliated with GM for over 30 years and is the author of “Traffic Safety and the Driver" and more recently, “Traffic Safety." He said, “My best guess is zero. I believe that fixes due to vehicle recalls have saved zero, or close to zero lives. However, driving to have them attended to imposes the risk of death associated with additional travel—so, arguably, recalls increase traffic deaths and certainly increase fuel use, etc. It seems to me that there ought to be much stronger evidence on the proposers of such policies to show evidence that some good flows from them” (email correspondence with the authors, April 11, 2003).

[4] Mr. McDonald actually contacted us when he was about to complete his book when he became aware of an early working paper of ours that had some of the ideas and discussions we present here. He cited our working paper in his book, but did not present any discussion of our findings or conclusions. The concerns about the fact that the definition of a defect could be too broad and could lead to costly recalls that have no consequences for safety is not new, and is already discussed in some detail in Tobin (1982) and a number of references therein.

[5] The need to use this methodology to control for the unobserved heterogeneity, likely to explain a great deal of the variation in the number of accidents of different vehicle models, prevents us from using the sample weights in the accidents data (which is cross-sectional in nature, resulting from police reports collected in a given calendar year) that we use in the paper. The reason for this is that those weights only control for the stratified sampling by location of the accident, but cannot be used to make accidents by particular vehicle models or particular drivers, representative at the national level.

[6] The letter we refer to was sent by Stephen L. Oesch, Senior Vice President, IIHS to President Jeffrey W. Runge, M.D., Administrator, NHTSA. It can be downloaded from the Internet following the link to the 2001 petition at *http://www.iihs.org/laws/petitions/* and states “The Insurance Institute for Highway Safety hereby petitions the National Highway Traffic Safety Administration (NHTSA) to amend the Defect and Noncompliance Reports (49 CFR 573) to require that vehicle manufacturers supply NHTSA with the vehicle identification number (VIN) for each defective or noncompliant vehicle.”

[7] The office of the then California state senator sent a press release on August 30, 2005, made available to us by e-mail, which quotes the senator. She stated, “Car makers foot the bill for the recall repairs, so obviously it's best for their bottom line if people don't find out about them and don't bring their cars in to get repaired. That's why GM opposed the bill.” She then continues, “To argue that giving people more information would somehow reduce the odds they'll get their car repaired is just laughable. The bill would have increased the likelihood that people will find out about a recall and get their car repaired free of charge, which is precisely why consumer groups supported the measure and why GM worked so hard to kill it.”

[8] Interestingly, these policy suggestions are in line with early recommendations by researchers (Tobin, 1982) regarding the possibility of having states help prevent vehicles from passing motor vehicle inspections if they

are not repaired, or prevent the transferring of ownership of vehicles not yet repaired.

[9] See Grabowski and Vernon (1983, and the discussion of that work in Viscusi, Vernon, and Harrington (1992, Chapter 23), in particular Figure 23.2., in the context of regulating pharmaceutical products. More recently, Kolstad, Ulen, and Johnson (1990) present a theoretical discussion about ex-ante vs. ex-post safety regulation, and Marino (1997) presents a model of recalls with imperfect monitoring that shows that recalls can be considered a blend of ex-ante and ex-post safety regulation.

[10] As of late 2010 a bill is being considered in Congress that would allow the NHTSA to speed up the process of mandatory recalls, which can now take months. In addition, they are considering requiring car manufacturers to install devices that would record what happened during an accident, similar to what is in place in airplanes. See "Highway safety agency wants more auto recall power," AP Press, May 6, 2010.

[11] Information for this process can be obtained from the NHTSA. Details are described in its homepage, http://www.nhtsa.gov/.

[12] Ward's Automotive Yearbook from 1983 to 2002. These are model year numbers, starting from September in the previous year.

[13] Direct costs are the ones that are used to correct the defects: repair costs, advertising costs, and so on. Indirect costs are the ones that are incurred through the financial and the goods markets due to recalls.

[14] This general representativeness provides some external validity for the results we present and the policy suggestions linked to the results. Notice, however, as discussed in the introduction, that we use the raw data in our estimates, and do not use the regional weights provided to make the data representative of the US, due to the fact that the pseudo-panel construction prevents us from assigning a weight to each accident, since now the level of observation is a composite-type in a given year.

[15] Bae and Benítez-Silva (in press) use more recent accident data in their study of the link between recalls and the severity of accidents, with results very much in line with those presented in this paper. Many recalls take some time to be issued, and therefore using more recent data can face serious right-censoring problems.

[16] In fact, each accident has different accidental harm measured by severity, and we also have a measure of whether personal injury occurred. Therefore, it does not strictly reflect the true value of the harm for the type. But if we use a weighted value using information on severity for the composite type, then the estimation is likely to be an error-ridden one. Bae and Benítez-Silva (2010) present a "severity model," using only cross-sectional data. The results of that research show that recalls reduce the severity of injury to drivers.

[17] Recently, a California jury awarded $15 million in a wrongful death lawsuit involving the death of two people while driving a vehicle rented from Enterprise Rent-A-Car Company, which was subject to a recall and was not fixed. The company knew all too well it was supposed to be taken to the dealer, yet still rented the vehicle to people. See the news section corresponding to June 24, 2010 in *http://www.pensacolapersonalinjuryattorneys.com/*.

[18] See the discussion of reforms to Section 30120 of Title 49 of the United States Code, to make sure rental fleets, along with schools buses, and taxi fleets comply with the repairs linked to recalls, see *http://testimony.ost.dot.gov/final/secveh.htm*.

[19] If the recall happened in the first 6 months of a particular year, the 3 years used include the year in which the recall was issued. Otherwise, include the 3 years following the recall.

[20] To exemplify how this key variable is computed, consider a recall by the Ford Motor company issued in 1993 for its Mustang 1993 model year (NHTSA recall ID #: 93V159000). The number of vehicles affected was 4,100. From 1989 to 1992, 468,679 Mustangs were sold. Therefore, the recall value is $\frac{4,100}{468,679} = .0087$. If there were another recall in the same year with 5,000 cars, then it would be $\frac{4,100 + 5,000}{468,679} = .019$.

[21] Following the recommendations from an anonymous referee we provide in the results section a sensitivity analysis in our specifications in which we remove two variables: *Design Change* and *Vintage*. We did this due to the variables' possible endogeneity linked to the fact that they are likely good explanatory variables in the selection process of the observed accidents in the data, given that newer vehicles when involved in even small accidents are much more likely to report it to authorities. However, omitting them from the specification opens the door to possible omitted variable biases if these variables are likely to be correlated with the recall rates. Even if we did not remove these variables, by restricting attention to striking vehicles

and restricting attention to accidents that lead to injuries (as we do in many of our specifications), this effect is likely ameliorated as also conjectured by the aforementioned referee.

[22] Moffitt (1993), Collado (1997), Girma (2000), McKenzie (2004), and Verbeek and Vella (2005) focus on the estimation and identification of dynamic models using a time series of repeated cross sections. The demands on the data by those types of models are considerably higher than in the linear pseudo-panel data model that we estimate.

[23] If we stop to consider the way we arrange the data from one of these cross sections, the empirical strategy presented in this section can be understood as trying to describe the evolution of the empirical distributions (densities) composed of the number of accidents in the different cells, where a cell is defined by a combination of driver and vehicle characteristics. This distribution(s) can in turn be understood as the reduced form derived from a structural model in which drivers make decisions regarding which vehicles to drive (buy), how to drive them, and which accidents to report when they have such a choice. Manufacturers make decisions regarding which vehicles to commercialize and how much pre-market testing to perform on their cars responding to the incentive structure set up by the government regarding recall initiation and report. The agents face a stochastic environment regarding road conditions, the behavior of other drivers, and tastes for certain vehicle types. Our econometric specifications can be thought of as trying to estimate the set of parameters that can better describe this reduced form distribution, and its evolution, and therefore should not carry a causal interpretation.

[24] We could use count data techniques to estimate our models. However, given that there are almost no zeros in our data set, the easier interpretation of the coefficients using standard moment condition methods, the appropriateness for inference of regression methods, and the fact that none of our main results change in any significant way, we have decided to report the results using standard panel data regression techniques.

[25] In all the tables that follow the level of significance of the coefficients presented is indicated by stars, with ***, **, and *, representing significance at the 1 percent, 5 percent, and 10 percent level, respectively.

[26] An alternative explanation is that newer cars are more likely to be driven by riskier drivers or be used as rental cars as suggested by an anonymous referee. Unfortunately, there is no easy way to separate the contribution of these different types of explanations to our result. We can, however, suggest that our explanations, while plausible, should be taken with caution.

[27] It is interesting, however, as suggested by an anonymous referee, to explore the sources of variation in our panel data, following the interesting work in agricultural economics by Mundlak, Larson, and Butzer (1999) and Mundlak, Butzer, and Larson (2008). Borrowing from their characterization, we can think of our random effects estimates as akin to the estimation of a production function of accidents (trying to describe the empirical distribution of accidents by their characteristics and over time), accounting for time variation and also variation across our composite types, where we estimate the effects of observed inputs (the controls of the characteristics of vehicles and drivers) and unobserved technology (the unobserved characteristics of drivers). In this context, we can actually estimate a panel data between composite-type estimator, where we only control for the year dummies and compare the results with the main models. In those estimates, available from the authors upon request, the key recall variable changes signs and turns positive and because we do not control for vehicle models, the recall rate captures the fact that the differences in accidents across composite types can be explained by the fact that models with more recalls also have more accidents, even if within a particular model, and over time, higher recalls predict declining accidents. In addition, drivers' characteristics do not change almost at all from the random effects estimator, but the indicators of *Design Change* and the proportion of newer vehicles in the total vehicles on the road in the last 5 years (*Vintage*) also change signs. The between estimation also turns negative, so when comparing the variation in accidents across composite types, those linked to models that change designs and have a newer vehicle fleet happen in a smaller proportion. These results illuminate the variation in our synthetic panel data and provide some interesting support for our overall empirical strategy to identify the effects of recalls on accidents over time and across vehicle models, controlling for drivers' characteristics.

[28] Other specifications that separate domestic and foreign vehicles give similar results, and only when we focus in a small category of accidents do we observe any difference between the two types of vehicles, but even then they are not significant due to the large standard errors of those small sample specifications.

[29] The fairly strong relationship between the recall variable and the *Vintage* measure, and the link of the latter with the likelihood of an accident being reported to the police, reminds us of the tension between the consequences of multicollinearity vs. omitted variable biases in linear models. Our results regarding the parameter estimates of the recall and *Vintage* variable can be understood in light of the classical concept of

concentration ellipsoid as presented in Malinvaud (1966), where due to their covariance structure, the separate effect of two independent variables on the dependent variable can only be precisely identified up to some combination of the two, in the direction of the large characteristic roots. For example, if from Table 10 we re-estimate the four models presented in the columns but omit the recall rate measure, we obtain a table (not shown) akin to Table A.4 but with the *Design Change* and *Vintage* variable included. The coefficients on the vintage indicator are then reduced by a large fraction of the value of the omitted coefficient, suggesting the vintage variable erroneously captures the negative effect that recalls have on accidents, in the same way that the recall variable erroneously captures the positive effect of the *Vintage* (and *Design Change*) variable in Table A.4. As a fraction of the coefficients estimated, the changes in the recall variable due to the omission of the selection related variables (*Design Change* and *Vintage*), presented in Table A.4, are smaller than the changes in the *Vintage* variable due to the omission of the recall variable, because of the smaller standard deviation of the *Vintage* variable. While collinearity is clearly present in our preferred specification, it does not seem very severe given the significance of our parameter estimates, suggesting that the biases present in the simpler models is likely to be costlier than the effects of collinearity on the parameters of interest.

[30] Hause (2006) provides an up–to-date theoretical treatment of offsetting behavior geared towards empirical testing of this phenomenon.

[31] Low correction rates are also quite common in the recalls of many other products, as discussed, for example in Felcher (2003).

[32] As the investigation regarding the major recall of Ford Explorers linked to tire problems showed, the government was not collecting the appropriate information from manufacturers, and therefore the use of information by insurance companies was essentially impossible.