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Dear Colleagues:

During the upcoming workshop, I will present results from two ongoing projects that explore the relationship between health shocks and financial stress. The first looks at shocks arising from sudden trauma (car crashes) and is described in the attached paper, a version of which I presented at Columbia's faculty workshop in 2012. The second project studies shocks arising from cancer diagnoses. This project is at an early stage. Although a paper is not ready yet (I hope to circulate a very rough draft in the coming days), I will present preliminary results at the workshop.

The two projects can be summarized by the following abstract:

> We document the endogeneity of health shocks using novel administrative data on two of the most important shocks experienced by consumers: trauma and cancer diagnoses. We link data on both types of shocks to bankruptcy filing data. Although it is well known that households exhibit substantial heterogeneity in financial fragility, we show that household financial fragility is positively correlated with the probability that households experience health shocks. This endogeneity potentially generates an upward bias in previous studies examining the effects of heath shocks on household financial outcomes, such as bankruptcy. We show that this bias can be mitigated by running within-shock regressions. Assuming shock severity is a proxy for household financial fragility prior to the shock, we subset on households who suffered comparable shocks at different points in time. Within a given period, household who experienced a shock can be viewed as a treatment group and households who experienced the same shock after that period can be viewed as a control group. Applying this framework, we find that the effect of cancer diagnoses on bankruptcy filing rates is substantially smaller than prior estimates and varies substantially by type of trauma and cancer. These findings highlight the difficulty in measuring the relationship

between shocks and household outcomes: The most distressed households appear to experience the most severe shocks. These findings also raise questions about the extent to which U.S. households are insured (formally and informally) against important health shocks

For my co-authors and me, this is an ideal time to get your feedback on our research. Both projects are evolving and represent part of a broader agenda that I look forward to discussing.

I look forward to seeing you.

Best regards,

Ed

CHICAGO

COASE-SANDOR INSTITUTE FOR LAW AND ECONOMICS WORKING PAPER NO. 655
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Health and Financial Fragility: Evidence from Car Crashes and Consumer Bankruptcy

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HEALTH AND FINANCIAL FRAGILITY: EVIDENCE FROM CAR CRASHES AND CONSUMER BANKRUPTCY*

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October 30, 2013

This paper assesses the importance of adverse health shocks as triggers of bankruptcy filings. We view car crashes as a proxy for health shocks and draw on a large sample of police crash reports linked to hospital admission records and bankruptcy case files. We report two findings: (i) there is a strong positive correlation between an individual's pre-shock financial condition and his or her likelihood of suffering a health shock, an example of behavioral consistency; and (ii) after accounting for this simultaneity, we are unable to identify a causal effect of health shocks on bankruptcy filing rates. These findings emphasize the importance of risk heterogeneity in determining financial fragility, raise questions about prior studies of "medical bankruptcy," and point to important challenges in identifying the triggers of consumer bankruptcy. *JEL* Codes: D12, D14, K35.

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1 Introduction

A large literature assesses household financial fragility by examining responses to unexpected shocks such as job loss, marital divorce, health problems, and natural disasters. If households are not fully insured through financial markets or self-insurance, these shocks can generate financial distress, as measured by consumer defaults or bankruptcy filings. The sensitivity of households to shocks of various magnitudes is relevant both to academic studies of risk-sharing as well as to policy debates about the design of public insurance programs and the administration of debt relief laws.

Health problems in particular are an important source of individual financial risk and are commonly thought to be an important driver of consumer bankruptcy filings. President Obama and members of Congress cited the phenomenon of "medical bankruptcy"—bankruptcy filings triggered by health care costs—in speeches advocating the recent health care reform legislation (Jacoby and Holman 2010). Members of Congress have also proposed legislation that would make the Bankruptcy Code more generous for consumers with significant health-related debts (an example is the "Medical Bankruptcy Fairness Act of 2009"). These public policy arguments find support in several well-known studies, including Himmelstein et al. (2005, 2010), which examined bankruptcy files and found substantial medical bills (around $5,000) in over half of the cases.

Although these studies have played an important role in academic and policy debates, they do not show whether adverse health conditions *cause* bankruptcy filings in the sense that households would not have filed for bankruptcy if these conditions had not occurred (or were better insured against). An alternative hypothesis is that long-term background characteristics, such as personal financial management or underlying risk preferences, are the fundamental drivers of financial distress and bankruptcy filing rates. Background personal characteristics could simultaneously elevate the probability of developing health conditions and experiencing financial default. This potentially complex relationship between personal health and financial management presents a fundamental challenge to properly identifying the causal impact of health shocks on financial distress.

Our paper makes two contributions. First, we confirm empirically that common variables affect both financial and bodily health. We show this by studying car crashes, which we view as a type of shock that exposes households to health problems. We draw on a unique panel dataset that links (i) police reports from all car crashes in Utah during 1992 through 2005 to (ii) hospital admission records and (iii) bankruptcy case files that allow us to measure bankruptcy filing rates before and after crashes. Using these data, we find that background driver characteristics are important determinants of both the probability of experiencing a crash and the probability of filing for bankruptcy. In particular, drivers who sustained severe accidents had substantially higher bankruptcy filing rates—both *before* and *after* the crashes—relative to drivers who experienced only minor accidents. This difference persists even when we focus only on drivers who were judged by the police to be "not at fault" for their accidents.

These findings strongly indicate that unobservable driver characteristics elevate both the risk of a severe accident and the probability of a bankruptcy filing—a type of behavioral consistency—and suggest a possible bias in prior studies that fail to account for the joint determination of household shocks and financial distress.

We address this bias using a new difference-in-difference (DD) strategy that exploits the panel construction of our data. We construct two groups of drivers: a treatment group that experienced crashes in period t, and an observationally similar control group that experienced equally severe crashes in period $t+n$. We compare the treatment and control groups during the $n-1$ periods (quarters or years) before and after period t. During this period, only drivers in the treatment group experienced a crash. We compare the bankruptcy filing rates of treatment and control group drivers before and after period t to estimate the causal effect of this type of health shock on bankruptcy filing rates. This empirical strategy deals with secular time trends in bankruptcy filing rates, which are a strong feature of our data. More importantly, by conditioning on crash severity, this strategy allows for the possibility that treatment and control group drivers share a common (unobservable) characteristic that jointly determines bankruptcy filing rates and the propensity to experience a crash. By conditioning on this unobservable heterogeneity across drivers, we can test the causal effect of a health shock among drivers with similar background financial characteristics.[1]

Our empirical strategy assumes that treatment and control group drivers are comparable during the years immediately before and after the treatment date. It also assumes that the timing of a crash, conditional on its severity, is independent of the bankruptcy filing rate. We present evidence consistent with both assumptions.

The second contribution of our paper is that, after accounting for the joint determination of accident risk and financial risk, we find no evidence that the health shocks investigated here are a cause of bankruptcy filings. Among drivers with an average hospital charge around $13,000 (measured in year 2012 dollars), we observe no increase in the probability of a bankruptcy filing, relative to the control group, through the first three years following the crash. This finding persists when we subset on drivers who are less likely to be fully insured (at-fault drivers, uninsured drivers, and drivers who incurred health care costs substantially in excess of state-mandated insurance levels) and drivers who are plausibly more financially fragile (drivers between ages 35 and 45, those living in lower-income zip codes, and those driving with children, who may also have been injured).

These findings indicate that one of the most commonly observed health shocks—automobile crashes—is not an important driver of bankruptcy filings. One interpretation is that the typical driver (at least in Utah) is fully insured against the shocks arising from automobile crashes measured at different magnitudes, at least with respect to filing for bankruptcy. This interpretation is interesting given several facts about insurance markets. First, although not-at-fault drivers can recover some of the costs of crashes—which include bodily harm, property damage, and wage loss—by drawing on the legally mandated personal liability insurance of at-fault drivers, not all at-fault drivers cary sufficient insurance. In 2007, eight percent of Utah drivers lacked any auto insurance (Insurance Research Council 2009). Second, the at-fault drivers must draw on other forms of insurance to cover both their own injuries as well as the injuries of the not at-fault-driver. As noted above, at-fault drivers may not hold liability insurance to cover the costs of the other driver. Additionally, a substantial fraction of at-fault drivers do not carry health insurance to cover all medical expenses arising from

[1] A similar empirical strategy is employed by Hilger (2013), who uses variation in the time of treatment, conditional upon treatment, as the principal source of variation.

an automobile crash. In 2009, seventeen percent of adult Utahns (aged 19 to 64) had no health insurance (Kaiser Family Foundation, State Health Facts 2010). Although Utah requires drivers to carry a minimum of $3,000 in "personal injury protection" insurance to cover medical costs from crashes, our findings persist when we focus on severe crashes in which the average medical charge was about $16,000. It is also likely that a substantial fraction of at-fault drivers do not carry personal automobile collision insurance sufficient to cover the costs of their own property damage. Despite the incomplete nature of formal insurance coverage—particularly among at-fault drivers—we conclude that a combination of formal and informal insurance mechanisms are sufficient to prevent a consumer bankruptcy filing after shocks of the magnitudes investigated in our paper.

Related Literature. Our paper intersects three literatures. The first focuses on the determinants of consumer bankruptcy. A number of papers have emphasized the financial fragility of households and the link between adverse events and bankruptcy filing rates. In an important line of studies, Himmelstein, et al. (2010, 2005) survey bankruptcy filers and elicit information about medical expenditures prior to the bankruptcy filings, reporting in their most recent work that illness or medical bills contributed to at least sixty-two percent of the bankruptcies. Similar findings—that medical expenditures are an important driver of bankruptcy filings—are reported by Miller (2011), Lindblad et al. (2011), Robertson et al. (2010), Gross and Notowidigdo (2009), Jacoby et al. (2001), and Domowitz and Sartain (1999).

Other studies are more skeptical about a causal link between idiosyncratic shocks and bankruptcy filing rates. Fay et al. (2002) find no correlation between self-reported health problems and the probability of a bankruptcy filing, after controlling for debt levels. Dranove and Millenson (2006) argue that the high prevalence of self-reported medical debts does not necessarily imply that medical debts were a causal contributor to most bankruptcy filings. Hollingworth et al. (2007) compare pre- and post-operation bankruptcy filing rates after brain or spinal cord surgery and find a temporary increase in filing rates during the first two years after the operation, but no permanent increase (filing rates return to their pre-operation level after five years). Hankins et al. (2011) find no correlation between positive shocks (winning the lottery) and bankruptcy filing rates. Positive shocks tend to delay bankruptcy filings, but do not produce a permanent reduction in filing rates.

Our paper also contributes to the literature on behavioral consistency, including Barksy et al. (1997), and Cronquist et al. (2012). These papers have found a positive relation between an individual's risk preferences in different settings (e.g., volatile stock investments, risky entrepreneurship, and consumption of alcohol and tobacco). Moskowitz and Vissing-Jørgensen (2002) suggest that entrepreneurs may be more risk tolerant than others, while Pollmann (2011) finds a correlation between risk preferences and occupational sorting.

Finally, our paper is related to the literature testing whether households are insured against idiosyncratic shocks. Beginning with Cochrane (1991) and Townsend (1994), this literature has frequently rejected full insurance for certain types of shocks, such as long illness and involuntary job loss. Many of these papers ignore potential heterogeneity in risk preference across individuals. Two exceptions are Dynarski and Gruber (1997) and Fuchs-Schündeln and Schündelen (2005). In work similar to ours, Schulhofer-Wohl (2011) finds that risk-tolerant individuals pick jobs with higher earnings risk and, that after accounting for this risk heterogeneity, the effect of income shocks on consumption is economically and statistically small. To be sure, much of this literature focuses primarily on whether household consumption varies in response to household exposure to shocks, whereas we focus on bankruptcy. It is possible for shocks to affect household consumption without affecting bankruptcy filing rates (a consumer, for example, can default on credit card debt without filing for bankruptcy).

This paper is organized as follows. Section 2 describes our data, hypotheses, and empirical approach. Section 3 presents our main results using our difference-in-difference identification approach. Section 4 concludes.

2 Empirical Approach

2.1 Background

Automotive crashes are a frequent cause of injury in the United States, harming about 2.8 percent of licensed drivers during 2008.[2] Crashes yield substantial costs: they are

[2]NHTSA reports 5.8 million police-reported crashes and 208.3 million licensed drivers during 2008. See NHTSA, Traffic Safety Facts 2008.

the leading leading cause of death for Americans under 35 years of age and represent one of the biggest sources of idiosyncratic risk for drivers. Using data from 2000, Parry et al. (2007) estimate that injury-producing car crashes generated $433 billion in costs, including quality-adjusted life years, property damage, travel delay, medical expenditures, wage loss, and other losses. These costs average $13,766 per crash, about one-third of median household income during 2000. However, some losses itemized by Parry et al. (2007) do not represent immediate reductions in income or out-of-pocket expenditures and therefore may not alter an individual's bankruptcy probability. Focusing only on losses that affect a driver's cash flow, the average cost per crash is about $6,656. Among crashes that generate serious bodily injury, but not death, the average cost rises to about $40,176, almost equal to median household income.

Medical expenditures form a substantial fraction of the costs generated by automobile crashes. Among crashes that cause bodily injury, medical expenditures account for eighty-four percent of the average cost of the most serious crashes and about one third of the cost of minor-injury crashes.[3] These stylized facts suggest that automotive crashes can be viewed as an important shock to health status and financial well-being.[4]

A potential confound arises from tort (accident) law. In Utah, and elsewhere, an at-fault driver is generally obligated to compensate injuries suffered by the not at-fault driver. Additionally, Utah law requires all drivers to carry liability insurance, which will be used to compensate not at-fault drivers. Although about eight percent of Utah drivers fail to purchase liability insurance (Insurance Research Council 2009), the typical not at-fault driver can expect to be at least partially compensated in the event of a crash, though there are limits to both typical liability insurance payments as well as the categories of damages that are covered. Therefore, it would not be surprising if we found no significant correlation between crash severity and bankruptcy filing rates among not at-fault drivers. We have different expectations, however, regarding at-fault drivers. These drivers bear the costs of a crash and must rely on privately purchased market insurance, public insurance programs, and self-

[3] A "minor-injury crash" is defined by Parry et al. (2007) as one that reduces quality-adjusted life years by less than $4,500, using data from calendar year 2000.

[4] See also Doyle (2005).

insurance to cover their costs. In the analysis below, we will distinguish between drivers generally and at-fault drivers.

Even if we observe an effect of crash severity on bankruptcy filing rates, we cannot assume that the effect is due entirely to medical costs. Automobile crashes result in a variety of financial costs, including property damage and work loss. Although we explore the effects of these other losses in our analysis below, we are ultimately unable to isolate their importance relative to medical costs. For example, we hypothesize that the importance of property damage will vary with the age of the car. Holding bodily injury constant, damage to a relatively new car (manufactured within five years of the crash) is likely to be more costly to the driver than damage to an older car. Newer cars tend to have greater resale value and higher repair costs than older ones. Drivers also often purchase new cars with loans. Therefore, drivers with new cars may be more heavily indebted—and more financially unstable—than those with older cars.

2.2 Data

We use data on a comprehensive universe of vehicle crashes in Utah during 1992–2005. The data are drawn from the Crash Outcome Data Evaluation System (CODES) maintained by the University of Utah. CODES is a federally-funded project that links police-recorded crash data with hospital admissions records. To date, twenty-six states have developed CODES databases (U.S. D.O.T. 2010). Utah data were chosen because they cover a longer period than data maintained by most of the other states (Utah was one of six states selected in 1992 to pilot the CODES program) and because the University of Utah was interested in linking these data to bankruptcy filings.

The Utah database includes information about vehicle make and model; date, time, and location of the crash; speed of cars prior to impact; area of car that was damaged; police assessment of fault and bodily injury severity; driver age, gender, and zip code; and a variety of other crash characteristics. If a driver subsequently visited a hospital, CODES records the hospital treatment, expenditure, and duration of stay; whether the driver received emergency room treatment only or was admitted to the hospital; whether the driver carried medical insurance; and other medical information.

We link CODES data to Utah bankruptcy filings during the period January 1992 through May 2010. Our bankruptcy data are obtained from the PACER website for the Bankruptcy Court for the District of Utah and include every consumer filing during the sample period (including both Chapter 7 and 13 cases). We match crashes to bankruptcy filings based on the driver's last name, zip code, and last four digits of his or her social security number.[5] Through this linkage, we create a panel dataset that tracks the bankruptcy behavior of drivers during the years before and after a crash.

The CODES data provide a comprehensive universe of accidents in Utah during this period. A crash is excluded form our analysis under the following conditions:

1. The crash involves a driver who had another crash within the preceding three years. This exclusion alleviates the potential confounding effect of multiple crashes during the treatment period.

2. The driver died during the crash. Our data do not allow us to track bankruptcy filings by the deceased driver's family.

3. The driver's residence was outside Utah. Because these drivers will typically file for bankruptcy in their states of residence, their filings will not be included in our data.

4. The date of the crash is missing.

5. The crash represents the seventh or higher crash of a driver. We analyze the effect of the first six crashes experienced by a driver. A very small fraction of drivers have more than six crashes during our study period.

In addition, we subset on drivers who were between ages 25 and 55 at the time of their crash. Drivers younger than 25 are less likely to be financially independent, and drivers over age 55 tend to have relatively low bankruptcy filing rates.

In much of the analysis below, we will compare all crashes to those in which the driver was deemed "at-fault" for the crash. We say that a driver was "at-fault" if (i) the driver was

[5] An overview of Utah's linking procedure and citations to the relevant literature are available at `http://www.utcodes.org/probabilisticLinkage/index.html`.

involved in a two-car crash and (ii) police records indicate that the driver was at-fault. Not at-fault drivers are identified similarly. We exclude from the fault analysis crashes in which the driver's fault is less clear, including one-car crashes and multi-car crashes.

Table I presents basic information from our linked dataset: total number of annual crashes and yearly bankruptcy filing rate. Among these drivers, our data include roughly 382,000 crashes over a fourteen-year period, 1992–2005. The bankruptcy filing rate doubles over this period among drivers who suffered crashes, rising from one percent to over two percent. This pattern mirrors trends among Utahns generally, as seen in comparison with the final column of Table I, which presents annual bankruptcy filing rates among Utahns aged eighteen or older.

Because we study crashes in Utah, we cannot be confident that our analysis below would yield similar estimates in other states. Utah had one of the highest bankruptcy filing rates in the United States during our period of study (See Lown (2008)). Indeed, it had the nation's highest filing rate during the early 2000s. On the other hand, scholars such as Lown (2008) have argued that the determinants of bankruptcy filing rates in Utah are similar to the determinants elsewhere in the United States. Medical debt, in particular, is thought to be a major contributor to bankruptcy in Utah as in other states.

2.3 The Key Identification Challenge: Behavioral Consistency in Driving and Financial Decisions

Perhaps the most straightforward empirical strategy would exploit differences in crash severity among observationally similar drivers. We could, for example, define a treatment group as drivers who suffered high-severity crashes ("high-severity drivers") and a control group as comparable drivers with low-severity crashes ("low-severity drivers"). We would then test whether the probability of a bankruptcy filing increased among high-severity drivers, relative to low-severity drivers, immediately after the crash. This strategy would be similar to the approach taken in prior studies, including Himmelstein et al. (2010), Fay et al. (2002), and Domowitz and Sartain (1999).

Table II suggests what we would find if we pursued this strategy. This table computes the average bankruptcy filing rate during the three years following a crash among (i) drivers who were admitted to an emergency room after the crash ("EDAdmit") and (ii) drivers who did not seek medical care after a crash ("Not EDAdmit"). We observe a substantial difference between the two groups: Without distinguishing at-fault from not at-fault drivers, the filing rate for the EDAdmit group is over two percentage points larger—forty-five percent larger—than the rate among drivers who did not seek medical care. Restricting to at-fault drivers, the difference is comparably large (a thirty-two percent difference).

The comparison in Table II, however, could overstate the causal effect of health shocks on bankruptcy if unobservable driver characteristics are correlated with our measure of crash severity and with the probability of a bankruptcy filing.[6] Financially unstable drivers may be more prone to suffer severe accidents, perhaps due to unobservable characteristics that increase the risk of both financial and health shocks. Financially fragile individuals may also be risky drivers.

Table III provides evidence consistent with the hypothesis that crash severity and financial conditions are jointly determined. We compute annual bankruptcy filings rate for EDAdmit and Not EDAdmit drivers during the three years before and after their crashes. We observe a substantial gap in pre-crash filing rates: bankruptcy filing rates among EDAdmit drivers are thirty to fifty percent higher than Not EDAdmit rates in every *pre-crash* year, regardless of whether the driver was judged to be at-fault for the crash. This is strong evidence that crash severity is correlated with the background financial characteristics of drivers, implying that a household's exposure to this kind of shock (car crash) is endogenous to the household's underlying characteristics.

[6]Table II could also *understate* the effects of health shocks because, in our data, severity is measured using data gathered at an emergency room. Financially unstable drivers may be less likely to seek medical care after an accident, perhaps because they are less likely to carry health insurance. If so, our measure of crash severity (EDAdmit in Table II) will tend to exclude drivers who are the most financially unstable. We assess this bias in Table XV of the Appendix, which presents the insurance status of EDAdmit drivers with different injury severity levels. Even if uninsured drivers generally prefer to avoid emergency room care, their insurance status is less likely to deter them from seeking care after highly severe injuries. Thus, if Table II is biased, we should observe that uninsured drivers represent a higher proportion of high-severity injuries than of low-severity injuries. Table XV assess this claim using two different metrics of injury severity. One, in Panel A, measures severity using the total expenditure reported by the hospital. Panel A divides this expenditure into quintiles. Panel B measures severity using reports filed by police at the scene of the crash. Together these panels offer little evidence of a systematic tendency of uninsured drivers to avoid emergency room treatment. Panel A shows that uninsured drivers are most represented in the lowest and highest quintiles. Panel B is similar, with higher proportions of uninsured drivers at the lowest and highest severity classifications.

This finding points to *behavioral consistency*: In our data, drivers exhibit heterogeneous background risk levels which generate a correlation between their financial condition and driving outcomes. Additional evidence of behavioral consistency can be seen in Table IV, which identifies several categories of risky driving behavior: reckless or at-fault driving (as determined by traffic police), driving the wrong way down a road (Wrong Way), driving under the influence of drugs or alcohol (DUI), driving under the influence of drugs (Drugs), speeding, and driving without a seat belt. For each category, we compute the bankruptcy filing rate during the three years prior to the crash for drivers who engaged in the risky driving behavior and for drivers who did not. In all but one category (Wrong Way), we observe higher pre-crash bankruptcy rates among drivers who engaged in risky driving. For some categories, the difference is substantial. Reckless drivers, for example, exhibit pre-crash bankruptcy rates (7.43%) over seventy-five percent larger than drivers who were not cited for reckless driving (4.24%). Similarly, drivers who used drugs prior to a crash had a pre-crash filing rate (9.15%) over one hundred percent higher than those who did not (4.25%).

These differences persist in regression models predicting the probability of a bankruptcy filing prior to a crash. They also persist when we compare bankruptcy filing rates more than three years prior to the crash. For example, for each crash year, we identified drivers who experienced crashes with at least one of the risk factors listed in Table IV ("risky crashes") and drivers whose crashes exhibited none of those factors ("non-risky crashes"). In unreported tables, we compute the average annual bankruptcy filing rate for the two groups during the three to six years, six to nine years, and nine to twelve years prior to the crash. During each of these lookback periods, we find that drivers in risky crashes exhibit a higher filing rate than those in non-risky crashes, though the difference diminishes as we look farther into the past. During the three to six years prior to the crash, we observe an eighteen percent difference in fling rates, but during the nine to twelve years before the crash we observe a ten percent difference.

2.4 Empirical Strategy

The existence of behavioral consistency suggests that a driver's crash risk depends on his or her pre-existing financial condition. This presents a bias in tests that seek to discover the causal role of car accidents. To the extent that financially stressed households have higher crash risk, the bankruptcy filing rate of drivers who experience severe crashes will be higher than the rate of drivers who experience minor (or no) crashes purely for selection reasons unrelated to the crash itself.

To address this bias observed in our data, we propose a novel difference-in-difference (DD) strategy that exploits differences in the *timing* of crashes experienced by observationally similar drivers. Some drivers experience crashes earlier in our data than others. Assuming the precise timing of a crash is uncorrelated with the driver's pre-crash financial condition, we can treat drivers with later crashes as a control group for drivers with earlier crashes. In particular, we match drivers who suffered a crash in year t (treatment group) to drivers who suffered a crash of comparable severity in year $t{+}n$ (control group). For each driver in the control group, we create a "placebo" crash date, equal to their actual crash date minus n years. We then compare the bankruptcy behavior of treatment and control group around year t. Specifically, we study the n years before and after year t. During this period, drivers in the treatment group experienced a crash, but drivers in the control group did not. More formally, our estimator computes the increase in bankruptcy filing rates during the period $[t{-}n, t{+}n)$ among treatment group drivers relative to control group drivers. Figure I provides an illustration of our basic identification strategy.

This difference-in-difference strategy allows us to estimate whether high severity crashes—which generate substantial medical expenditures among drivers who are highly unlikely to refuse medical care—cause an increase in the bankruptcy filing rate of drivers who suffered such crashes in period t, relative to drivers who suffered comparable high severity crashes later in time $t + n$.

By conditioning on the treatment variable, our empirical strategy allows for the possibility that exposure to car accident risk, in particular to high-severity car accident risk, is correlated with household financial characteristics. Instead, our identifying assumption is that, conditional on having a crash, a driver's pre-crash financial condition is uncorrelated with the timing of his or her crash. Put differently, during period t, the same financial characteristics are shared by drivers who suffered crashes in period t and by drivers who suffered similar crashes in period $t + n$. This assumption is more plausible when n is small. In our estimates below, we let n equal 1 year or 3 years. We also confirm that the pre-t characteristics of treatment and control drivers are comparable regardless of whether we let n equal 1 year or 3 years.

The principal advantage of our approach is that it addresses the potential endogeneity of crash severity and underlying financial status. Financially unstable drivers may have a relatively high probability of suffering severe crashes and a relatively low probability of seeking medical attention. Although we cannot observe a driver's pre-crash financial characteristics, drivers who suffer high severity (or low severity) crashes may share similar characteristics. As long as these characteristics are time-invariant over at least short periods (say, x years), drivers who suffer crashes in year t likely share the same financial characteristics as those who suffer comparable crashes in year $t + x$.

This empirical strategy requires additional restrictions to our sample data. Although we have information on all crashes during 1992–2005, complete data on hospital-related variables is available beginning 1996. We therefore include in the Treatment Group all crashes that occurred during 1999–2002 (we stop at 2002 because we need to verify that drivers with crashes during 2002 had no additional crashes during the subsequent three years). The Control Group includes crashes that occurred during 2002–2005. [7] This allows us to observe three years of bankruptcy behavior before and after each crash.[8]

2.5 Comparability of Treatment and Control Groups

Table V presents summary statistics for drivers who suffered crashes in year t (the treatment group) and for those who suffered crashes in year t+3 (the control group). Statistics that vary with time (e.g., driver and car age, whether a bankruptcy was filed in the preceding three years) are measured at time t for all drivers. Crash-specific variables (e.g., fault, EDAdmit, and insurance status) are measured at the time of the crash—t for the treatment

[7] Crashes are sometimes included twice: once as a treatment crash, and again as a control crash. When this occurs, the bankruptcy filing probability is computed over different time intervals for the two observations: it is computed relative to the actual treatment date when the crash enters the Treatment Group; it is computed relative to the placebo treatment date when the crash enters the Control Group

[8] We similarly construct a sample in which $n = 1$. The Treatment Group for that sample consists of crashes during 1997–2004, and the Control Group consists of crashes during 1998–2005.

group and $t + 3$ for the controls. As noted above, our strategy assumes that these crash-specific variables are correlated with time-invariant, unobservable background characteristics of the drivers. This intuition is supported by Table III, which showed a strong correlation between crash severity and pre-crash bankruptcy filing rates, suggesting that unobservable characteristics are an important driver of both crashes and bankruptcies.

Table V shows that treatment and control drivers are closely comparable across a broad range of observable characteristics, including driver age and gender and most crash-related variables. There are, however, potentially important differences in car age ("New Car") and between the two groups' pre-crash bankruptcy filing rates (listed under *Bankruptcy Data*). The difference in car age is an artifact of our identification strategy: Among drivers in the control group, we take car age at the actual crash date, $t+3$, and subtracting 3 to impute car age at "treatment" date t. This assumes that the driver owned the same car during the past three years. If a driver's car age at $t + 3$ is less than or equal to 3, we cannot impute car age at t. We could code car age as missing at t (reflecting the lack of information about car age at that date) or treat the driver as having a new car at t (thereby taking advantage of the information that the driver purchased a new car at $t + 3$). Because our analysis is the same regardless of the approach, Table V treats a placebo driver as having a new car at t if the driver had a new car at $t + 3$.

The differences in pre-crash bankruptcy filing rates likely reflect immigration patterns. When we compute the bankruptcy rate during the years preceding t, we are implicitly assuming that the drivers lived in Utah during these years and, therefore, that any bankruptcy filing would have been filed in a Utah court. If a driver did not live in Utah during this period and filed for bankruptcy in a non-Utah court, information about this filing is not included in our database. This bias is more important for drivers in the Control Group because we are measuring the bankruptcy filing rate during the three years prior to their placebo crash, which is equivalent to measuring the rate during the six years prior to their actual crash.

The immigration hypothesis is supported by Table VI, which compares drivers who suffered crashes in year t (treatment group) to those who suffered crashes in $t+1$. Here, there is a much smaller gap in time between the actual crash dates of treatment and control drivers.

In any event, we do not believe that immigration-induced differences present a potential confound in our empirical analysis below. As reported below, our results are largely the same whether we use a control group that suffered crashes in $t+1$ or in $t+3$.

2.6 Econometric Specification

We estimate a panel probit specification of the following form:

$$\begin{aligned}\Pr(B_{it}=1) = \Phi(\alpha + \beta \cdot \mathrm{Crash}_i + \mu_{-3} \cdot \mathrm{Year}_{-3} + \mu_{-2} \cdot \mathrm{Year}_{-2} + \\ \mu_1 \cdot \mathrm{Year}_1 + \mu_2 \cdot \mathrm{Year}_2 + \mu_3 \cdot \mathrm{Year}_3 + \\ \delta_{-3} \cdot \mathrm{Crash}_i \cdot \mathrm{Year}_{-3} + \delta_{-2} \cdot \mathrm{Crash}_i \cdot \mathrm{Year}_{-2} + \\ \delta_1 \cdot \mathrm{Crash}_i \cdot \mathrm{Year}_1 + \delta_2 \cdot \mathrm{Crash}_i \cdot \mathrm{Year}_2 + \delta_3 \cdot \mathrm{Crash}_i \cdot \mathrm{Year}_3 + \\ \gamma \cdot X_{it})\end{aligned}$$

The dependent variable ($B_{it}=1$) is a dummy equal to one if driver i files for bankruptcy during year t. We estimate this probability for each year $t \in$ {-3, -2, -1, 1, 2, 3} preceding and following the date of an actual crash (for the treatment group) or a placebo crash (for the control group). If a driver files for bankruptcy in year t, the driver drops out of our analysis until he or she is legally eligible to file for bankruptcy again (the law at this time prevented a driver from filing for bankruptcy during the eight years after receiving a bankruptcy discharge).[9] Crash_i is a dummy variable that takes the value 1 if driver i is a member of the treatment group. The variables Year_t are time dummies that identify each year $t \in$ {-3, -2, -1, 1, 2, 3} preceding and following the actual or placebo crash. The excluded category is $\mathrm{Year}_{\text{-}1}$, the twelve months immediately preceding the actual or placebo crash. Thus, each year dummy Year_t measures the difference between (i) the average bankruptcy probability in year t and the (ii) the average in year $t=-1$. We define this difference as the "change in bankruptcy filing rates in year t." We interact these Year_t time dummies with the treatment indicator

[9] We verified whether the driver obtained a discharge after filing for bankruptcy. If the driver did not receive a discharge (perhaps because the case was dismissed prior to discharge), the driver remained in our analysis because he or she was still eligible for bankruptcy relief. Our results, however, are not sensitive to how we treat this legal issue. We obtain similar results whether drivers remain in our sample or drop out after receiving a discharge.

Crash$_i$. Each interaction is a difference-in-difference estimator, measuring the difference between treatment and control drivers with respect to the change in bankruptcy filing rates in year t. The coefficients of interest are $\{\delta_1, \delta_2, \delta_3\}$, which measure the difference-in-difference estimators during the years immediately following the (actual or placebo) crash. Coefficients for the other interactions $\{\delta_{\text{-}3},\ \delta_{\text{-}2}\}$ identify time-varying pre-crash differences between the treatment and control group drivers.

Finally, X_{it} is a vector of driver, car, and crash characteristics, including driver age and gender, car age, and whether the driver suffered crashes or filed for bankruptcy prior to the actual or placebo crash. Standard errors are clustered by driver.

This specification is analogous to a discrete-time hazard model, similar to the models estimated in Grogger and Bronars (2001) and DeCicca, et al. (2002). It is well-known that nonlinear models like the one proposed here are vulnerable to important biases. Greene (2004) catalogues some of the problems. We have verified that the results reported below are qualitatively the same when we apply a linear probability model and a conditional (fixed effects) logit model.

3 Results

3.1 Univariate Comparisons

Figure II plots bankruptcy filing rates for treatment and control drivers during the three years before and after the crash date, which is defined as the actual crash date for treatment group drivers and the placebo crash date for control group drivers. The placebo crash date here is defined as the date three years before the actual crash date of the control group drivers. Looking across all crashes, Plot (a) shows no meaningful difference between treatment and control filing rates before or after the crash date. This is unsurprising because the vast majority crashes caused minor injuries to property (fender benders). The subsequent plots, therefore, subset on crashes with relatively high severity levels and on drivers who may be more financially fragile.

Plot (b) subsets on crashes that were immediately followed by emergency room visits (EDAdmit Crashes). The pattern here is largely the same, with no apparent effect of crashes on the relative filing rate of treatment group drivers. Plot (b), however, may be biased against observing an effect of a crash. This is true for two reasons. First, drivers can choose whether to visit an emergency room, especially when their injuries are minor. If financially unstable drivers are less likely to seek emergency room care, the patterns in Plot (b) will be biased against finding an effect. The treatment group will be weighted toward drivers who are financially stable and unlikely to file for bankruptcy in response to a shock. Additionally, many emergency room treatments are for minor injuries, such as cuts and bruises, which are unlikely to generate sufficiently large medical bills that they could cause financial instability and bankruptcy.

Plot (c) addresses this issue by subsetting on crashes with two characteristics: (i) the driver subsequently visited an emergency room and (ii) the driver incurred hospital charges that ranked among the top twenty-five percent of all charges in our dataset (High Charge Crashes). The mean charge for these drivers is $12,971 (in 2012 dollars), the minimum is $1,663, and the maximum $709,875.[10] We think it is plausible to assume that virtually all drivers who suffer injuries of such magnitude will visit an emergency room. The magnitude may also be large enough to trigger a bankruptcy filing. Among these drivers, Plot (c) in Figure 2 shows largely parallel filing rates of treatment and control drivers, before and after the treatment date, again suggesting no effect of crashes on filing rates.

Because some drivers in Plot (c) had charges as low as $1,663, Plot (d) subsets on High Charge crashes in which the driver incurred at least $5,000 in hospital charges. This number was chosen because Utah law requires all drivers to carry Personal Injury Protection insurance equal to $3,000. We selected a number substantially larger than that minimum in order to isolate injuries that could destabilize financially fragile households. Here we see the difference between treatment and controls widen slightly during the year before the crash and during the second year after. But the post-crash increase in the treatment groups filing rate (relative to the controls) declines in the third year, suggesting that the post-crash variation may be attributable to random variation.

Finally, Plots (e) and (f) subset on EDAdmit and High Charge crashes among uninsured drivers, who are likely more financially fragile than the average driver. There are two

[10] Results are the same when we subset on the top ten percent (with a mean charge of $21,333), but the sample size is substantially smaller.

limitations to this analysis. First, we observe health insurance status only for drivers who visit an emergency room after a crash. Second, the sample sizes here are relatively small. Among High Charge crashes, for example, we observe only about 550 crashes and 13 bankruptcy filings on average per year involving uninsured drivers. In both plots, we observe pre- and post-crash increases of treatment group drivers, relative to controls. The post-crash increases appear in the second year, but largely disappear in the third.

Figure III presents the same plots, but focuses on the four quarters before and after the crash date. In these plots, the placebo date for control group drivers is the date one year before their actual crash date. Plots (a) and (b) show no increase in the treatment group filing rate, relative to controls, in the full sample or among EDAdmit crashes. Among High Charge crashes—Plots (c) and (d)—we observe pre- and post-crash increases in the relative filing rate of the treatment group, with the post-crash increase rising temporarily in the third quarter. Among uninsured drivers, there is no apparent increase in treatment group filing rates among EDAdmit crashes in Panel (e). Panel (f) may show a post-crash relative increase among High Charge crashes, but the difference between treatment and controls varies substantially by quarter. Figures IV and V present the same plots, but subset on at-fault drivers. Again, there is no apparent increase in the filing rate of treatment group drivers, relative to controls, after the crash date. Instead, the post-crash difference tends to narrow in most plots.

Together, these figures suggest that crashes may not have a sizable effect on bankruptcy filing rates, regardless of crash severity. The patterns for uninsured drivers, however, are largely inconclusive due to small sample sizes.

3.2 Baseline Estimates

Tables VII and VIII implement our empirical specification for the one-year and three-year splits, respectively. Each table reports marginal effects from a panel probit model.[11] The marginal effects can be compared to the bankruptcy filing rate during the period immediately

[11]Marginal effects are obtained from Stata's dprobit routine. Because we are estimating DD effects, we do not make the adjustments recommended by Ai and Norton (2003). See Kremer and Snyder (2010) and Puhani (2008).

preceding the crash, as reported at the bottom of the tables ("Ref. Bankruptcy Probability"). Standard errors are clustered by driver.

We view Tables VII and VIII as estimates of short-term and longer-run impacts of crashes. In each table, Columns (1) and (2) estimate the effects of crashes, regardless of severity, on the probability of a bankruptcy filing. We include a minimal set of controls in Column (1): time dummies, the crash dummy, interactions between the time and crash dummies, and county and calendar year fixed effects. Column (2) adds driver-specific controls, including car age, driver age and gender, and the prior bankruptcy and crash history of the driver (the coefficients for these controls are reported in the Appendix, Tables XVI and XVII). The coefficients of interest are the interactions "Year t After Crash $\times$ Crash" in Table VII and "Quarter t After Crash $\times$ Crash" in Table VIII. These coefficients measure the change in bankruptcy filing rate among treatment group drivers, relative to controls, during period t relative to the period immediately preceding the crash. In Column (1) of Table VII, for example, the coefficient for "Year 1 After Crash $\times$ Crash" equals 0.000025. This indicates that the difference between treatment and controls was larger during the first quarter following the crash than during the quarter immediately before (Year -1 is the omitted category).[12] The increase, however, is very small relative to the filing rate among treatment group drivers during the year prior to the crash. This rate is given by the row "Ref. Bankruptcy Probability" and is equal to .016 in Column (1). Thus, the .000025 percentage point relative increase during the first year following the crash represents a 0.16% increase relative to the pre-crash filing rate among treatment group drivers. The effect is also insignificant.

Across Columns (1) and (2) of Tables VII and VIII, we observe no significant increase in the relative bankruptcy filing rate of treatment group drivers during the post-crash period. Many coefficients are negative and significant. These non-results are unsurprising, as noted above, because most crashes are fender-benders and therefore unlikely to impact bankruptcy filing rates. On the other hand, Columns (1) and (2) indicate that treatment and control group drivers exhibited different bankruptcy filing rates prior to the treatment date. Several

[12] We also find comparable results when we let the number of years between the control and treatment groups n equal 5.

of the "Quarter t Before Crash $\times$ Crash" coefficients are negative and significant (or nearly so). This raises the possibility that unobservable differences between the two groups may be confounding our estimates.

The remaining columns in Tables VII and VIII subset on drivers who suffered sufficiently serious injuries that an effect on bankruptcy filing rates is plausible. Columns (3) and (4) subset on EDAdmit drivers; Columns (6) and (7) subset on High Charge drivers. For each group, the first column includes minimal controls; the second column adds driver-specific controls. Although an effect on bankruptcy is plausible, the results here are similar to those reported in the previous columns. We observe no statistically significant, positive impact of crashes on treatment group bankruptcy rates, relative to controls, during the first three years (Table VII) or first four quarters (Table VIII) following the crash. Even when the coefficient is positive, the coefficient is small relative to the pre-crash filing rate, reported at the bottom of the table ("Ref. Bankruptcy Probability"). Additionally, we observe no evidence of pre-trends in either Table VII or VIII.

Finally Columns (5) and (8) subset on EDAdmit and High Charge drivers who did not have health insurance (private or public) when they received treatment. For these drivers, high hospital charges could be financially destabilizing. Yet the results in Tables VII and VIII suggest otherwise. We continue to observe no post-crash increase in the relative filing rate of treatment group drivers. We do observe a pre-trend in in Table VIII. Treatment group drivers had a substantially higher bankruptcy filing rate, relative to the control group, during the year before the crash. We caution, however, that the sample size here is small.

Tables IX and X rerun these regressions, but subset on drivers who were at-fault. Recall that these drivers have access to less insurance than not at-fault drivers (who can bring suit against at-fault drivers). The results are largely the same, showing no persistent or sizable difference between treatment and control drivers during the quarters or years following a crash (Appendix Tables XVIII and XIX report estimates for the remaining controls).[13] Here

[13]The foregoing tables assume that drivers have identical propensities to file for bankruptcy, conditional upon crash severity and other observables. In unreported regressions, we relax this assumption by employing a fixed-effect version of our empirical specification. Due to the incidental parameters problem, discussed in Greene (2004), we estimate a conditional logit model instead of a fixed effects probit. We obtain largely the same results, although our estimates are less stable due to small sample sizes.

(and below), we omit regressions that subset on uninsured drivers with High Charge crashes due to small sample sizes.

Across all specifications, then, we observe no statistically significant or persistent relationship between crashes and post-crash bankruptcy filing rates. The magnitudes of the coefficients are small and often negative. These results are inconsistent with the hypothesis that households are financially fragile and that unexpected shocks can induce bankruptcy filings.

3.3 Extensions

Although we find no effect of severe crashes on bankruptcy filing rates, our analysis thus far may conceal important heterogeneity across drivers. Our data, for example, do not include information about income, debt burdens, and other driver characteristics correlated with financial fragility. Perhaps we would find an effect of crashes on bankruptcy filing rates if we identified drivers who were particularly financially fragile. We attempt to do this in the remaining tables.

Tables XI and XII rerun our regressions on subsets of at-fault drivers who may be more or less fragile than the average driver. Throughout these tables we subset on drivers who experienced crashes that resulted in emergency room visits (EDAdmit drivers).[14] Column (1) in each table subsets on drivers whose car was relatively new at the time of the crash (purchased within the prior three years). Because many new cars are purchased with loans, these drivers may have higher debt levels and therefore be more financially fragile. Additionally, crashes tend to cause more expensive property damage for drivers with newer cars. This means, of course, that the coefficients in Column (1) will reflect the impact of both health trauma as well as expensive property damage. Column (2) uses the same sample as Column (1), but subsets further on drivers did not carry health insurance. Across both tables, we observe no significant increase in the relative filing rate of treatment group drivers. Although the coefficients of interest are often positive, particularly in Column (1), they are highly statistically insignificant.

[14]We obtain similar results when we run our analysis on the full sample of drivers.

Columns (3) and (4) subset on drivers between ages 35 and 45 at the treatment date (i.e., the crash date for treatment group drivers and placebo date for controls). Individuals in this age range tend to have the highest bankruptcy filing rates, due to indebtedness (for houses and cars) and family expenses. Across both columns in all tables, we observe no effect of crashes on the relative bankruptcy filing rate of treatment group drivers.

Columns (5) and (6) subset on drivers who lived in zip codes with relatively low mean household income (defined as zip codes in the bottom 25% of the income distribution across Utah zip codes). Here too we observe no effect of crashes on bankruptcy filing rates.

Finally, Tables XIII and XIV rerun our regressions on the subset of at-fault drivers who experienced a crash while traveling with an underage child. We hypothesize that a driver's financial fragility may be higher when a child is a passenger. The driver is likely to be a parent of the passenger and parents tend to be financially fragile due to the expenses of childrearing. Additionally, the crash may injure the child as well as the driver. If the driver is financially responsible for the child, the child's injuries will increase the magnitude of the "shock" caused by the crash. Across all specifications, however, we observe no post-crash increase in the relative bankruptcy filing rate of treatment group drivers. Note that some specifications subset on drivers with new cars. We observe no effect of crashes among these drivers.[15]

Together, these results suggest that severe automobile crashes generally do not destabilize drivers.

4 Conclusion

We find evidence that automobile crashes are endogenous to the driver's financial condition. Severe crashes and crashes exhibiting risk factors (such as driving under the influence) are more likely to involve drivers with a relatively high pre-existing propensity to file for bankruptcy. We interpret this as suggestive evidence that adverse shocks, such as car accidents, are not exogenous shocks. Instead, we appear to observe a form of behavior consistency: there is a positive correlation between a household's probability of experiencing

[15] We obtain similar results when we run our analysis on the full sample of drivers.

a health shock and the household's pre-shock financial condition. Failure to account for this correlation results in an upward bias in causal estimates of the impact of adverse health shocks on bankruptcy filing rates.

We address this endogeneity by developing a difference-in-difference strategy that attempts to isolate unobservable background characteristics driving both accident and financial risk. We compare drivers who suffered comparable crashes at different points in time. Assuming that unobservable background characteristics are persistent, we allow for the possibility that drivers who experience more severe accidents may differ in important ways, so long as the *timing* of their crash, conditional on having a crash, is unrelated to household characteristics. We view the difference in crash timing as a treatment effect separating a treatment group (who suffered a crash in year t) from a control group (who suffered no crash in year t, but did suffer one in year $t+3$). We emphasize that our empirical approach can be broadly applied in a variety of contexts in which selection into treatment is a concern, but in which the precise timing of the treatment is more plausibly exogenous.

Applying this strategy, we find no causal effect of car accidents on bankruptcy filing rates, either economically or statistically. All of the variation in bankruptcy filing rates across individuals is explained by cross-sectional heterogeneous *ex ante* exposure to risk; none of the variation in bankruptcy filing rates is explained by exposure to the health shock. This result holds true even for individuals in our sample who face high levels of uninsured medical bills, although our estimates are imprecise due to small sample sizes. Our interpretation is that the households in our sample are insured in the sense of being able to avoid bankruptcy filings for the shocks investigated in our paper.

Our findings are qualified by several limitations of our research design. First, car crashes may not be informative about genuinely large financial shocks. Among drivers who visited the emergency room, the mean charge was about \$10,000. While this is a substantial amount relative to both the typical household shock and median household income, more severe shocks could elevate bankruptcy filing rates. Additionally, our data are drawn from a single geographic area (Utah) with distinctive socioeconomic characteristics. We are also studying a particular, extreme response to health shocks—bankruptcy—but households may respond to these shocks in other ways, such as by reducing consumption or defaulting on debts without filing for bankruptcy. We are examining these three limitations in follow-on work studying cancer patients.

A more important limitation is that we cannot rule out reverse causation. Persistent financial distress may be a cause of risk-taking behavior, such as risky driving. To the extent that distressed households tend to be judgment-proof, they do not fully internalize the costs of their driving behavior.

Finally, we cannot rule out the importance of strategic behavior. Although we find that persistent household characteristics are more important than adverse events as determinants of consumer bankruptcy filings,[16] it remains unclear how households determine the optimal timing of filings. It is possible that households strategically time their filings to obtain the largest possible benefit, as in Fay et al. (2002).

With these limitations in mind, we believe that our findings cast doubt on a wide range of studies arguing that shocks are driver of household distress. We are unaware of any study that addresses the potential endogeneity of shocks and household financial condition, which generates an upward bias in prior estimates. Our empirical strategy provides a new, useful way to address this endogeneity.

[16]This may help explain the phenomenon, reported by Porter and Thorne (2006), that a significant proportion of bankruptcy filers continue to suffer financial instability after obtaining a bankruptcy discharge.

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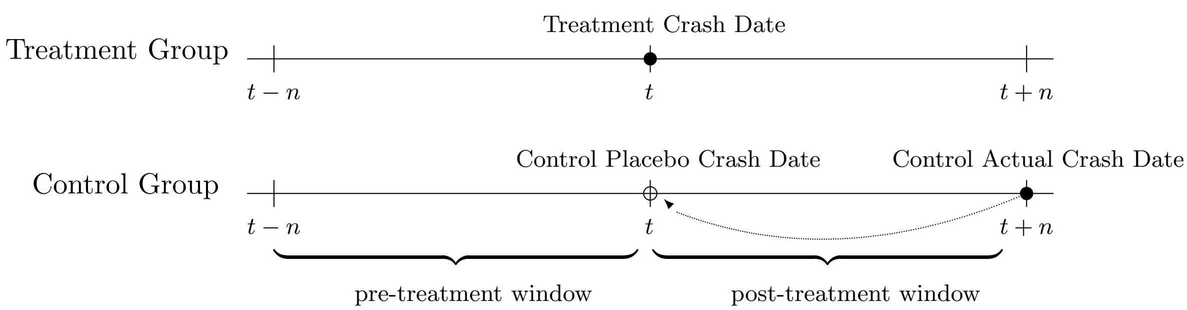
FIGURE I

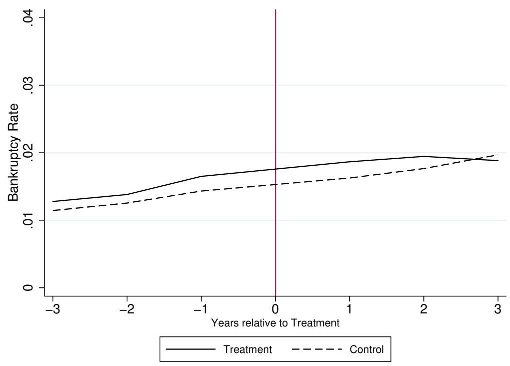
Illustration of Difference-in-Difference Identification Strategy



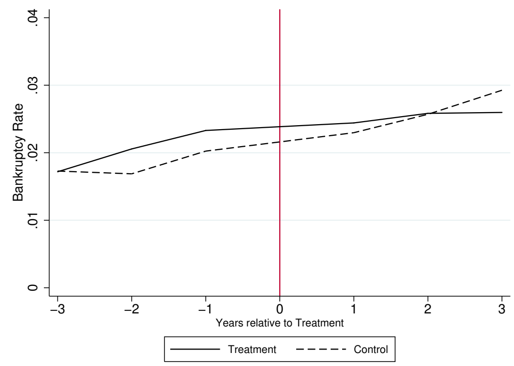
Notes. The difference-in-difference effect is computed as the change in bankruptcy rates between $[t, t+n)$ (the "post-treatment window") and $[t-n, t)$ (the "pre-treatment window") among drivers in the Treatment Group relative to the change in bankruptcy between $[t, t+n)$ and $[t-n, t)$ among drivers in the Control Group. Both the Treatment and Control Groups are selected from a population experiencing a crash. For the Treatment Group, date t is the actual date of a car crash. For the Control Group, date t is n periods prior to the actual crash date, thereby excluding the date of the actual crash from the sample interval.

FIGURE II

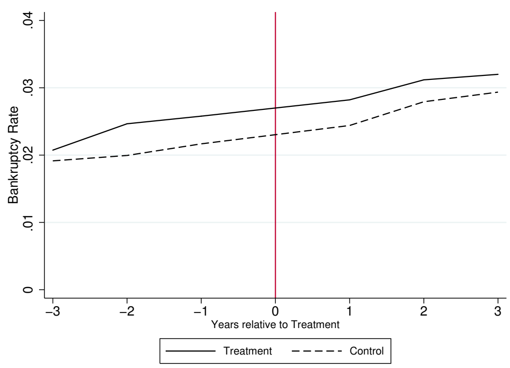
Annual Hazard of Bankruptcy Filing, Three Year Split



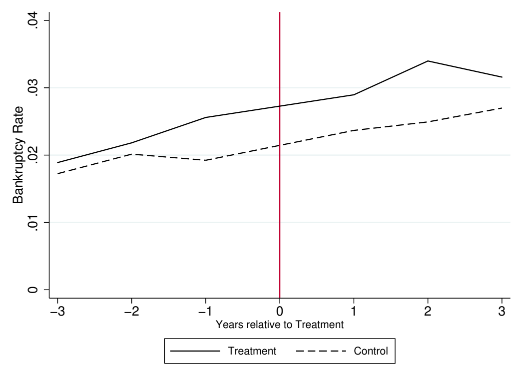
(a) All Crashes



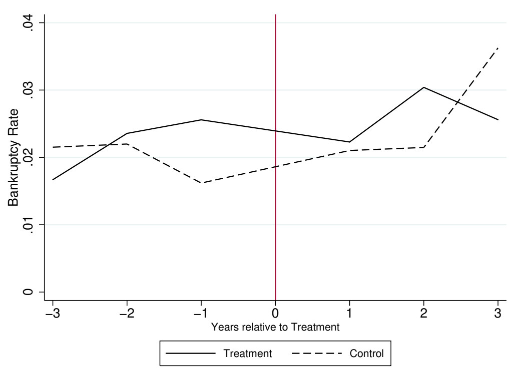
(b) EDAdmit Crashes



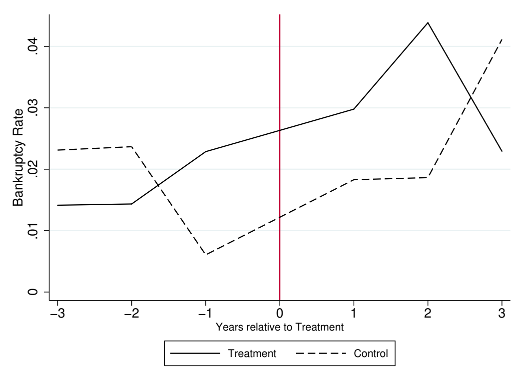
(c) High Charge Crashes



(d) High Charge Crashes over $5K



(e) EDAdmit Crashes, Uninsured

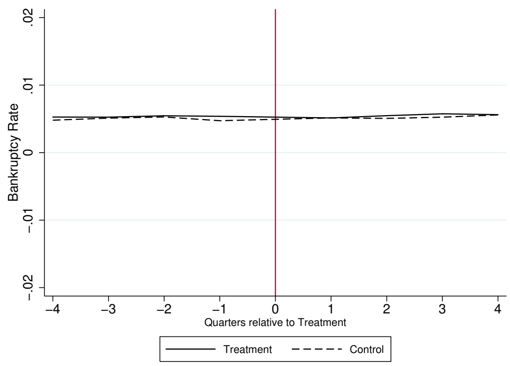


(f) High Charge Crashes, Uninsured

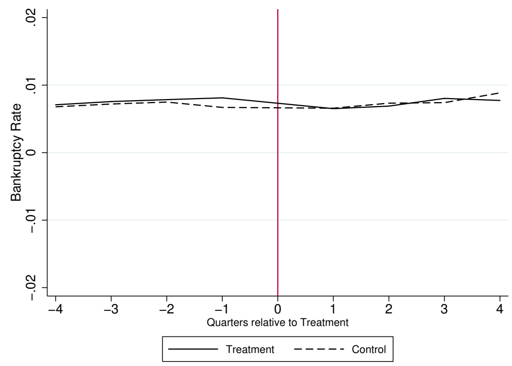
Notes. These plots show the difference in bankruptcy filing rates between treatment and control group crashes. Drivers in the treatment group suffered a crash at time 0; drivers in the control group suffered a crash three years after this date. Plot (a) shows results for all drivers, Plot (b) subsets (for both treatment and controls) on drivers who visited the hospital after their crash. Plot (c) subsets on crashes with charges in the top 25 percent of all charges in our dataset. Plot (d) subsets on crashes in which the driver's hospital charges totaled at least $5,000. Plot (e) subsets on crashes involving drivers who visited the emergency room and carried no health insurance. Plot (f) subsets on crashes involving drivers who visited the emergency room, carried no health insurance, and incurred charges in the top 25 percent of charges in our dataset.

FIGURE III

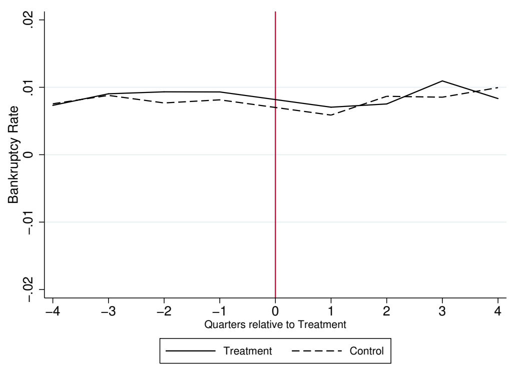
Quarterly Hazard of Bankruptcy Filing, One Year Split



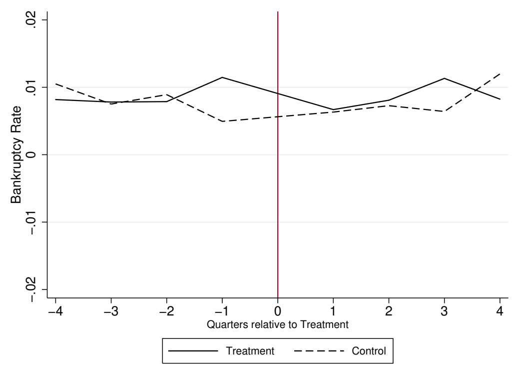
(a) All Crashes



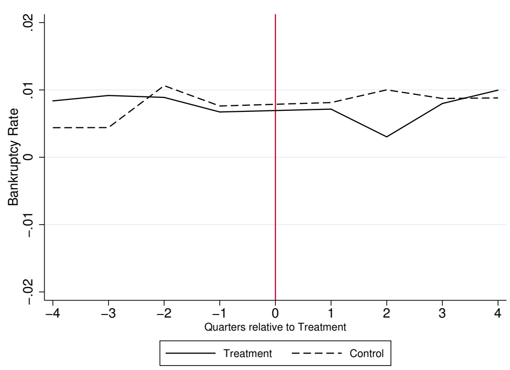
(b) EDAdmit Crashes



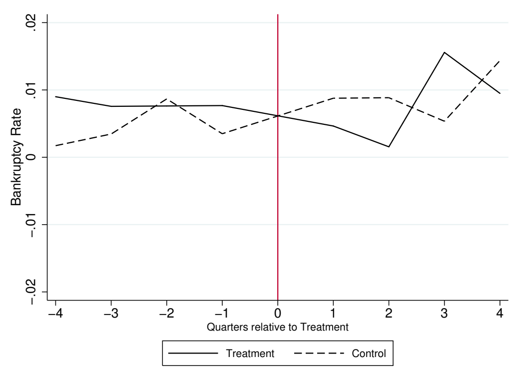
(c) High Charge Crashes



(d) High Charge Crashes over $5K



(e) EDAdmit Crashes, Uninsured



(f) High Charge Crashes, Uninsured

Notes. These plots show the difference in bankruptcy filing rates between treatment and control group crashes. Drivers in the treatment group suffered a crash at time 0; drivers in the control group suffered a crash a year after this date. Plot (a) shows results for all drivers, Plot (b) subsets (for both treatment and controls) on drivers who visited the hospital after their crash. Plot (c) subsets on crashes with charges in the top 25 percent of all charges in our dataset. Plot (d) subsets on crashes in which the driver's hospital charges totaled at least $5,000. Plot (e) subsets on crashes involving drivers who visited the emergency room and carried no health insurance. Plot (f) subsets on crashes involving drivers who visited the emergency room, carried no health insurance, and incurred charges in the top 25 percent of charges in our dataset.

TABLE I

Average Bankruptcy Filing Rate by Year by Group.

	All		At Fault		Utah Average, Age 18+
	N	Rate (%)	N	Rate (%)	Rate (%)
1992	33303	1.01	11639	1.07	0.91
1993	35109	.86	12710	.95	0.73
1994	34034	.78	12024	.85	0.67
1995	31120	.82	10570	.94	0.72
1996	31920	1.02	10927	1.11	0.91
1997	30072	1.33	10460	1.34	1.16
1998	28136	1.55	9730	1.6	1.33
1999	25751	1.59	8754	1.72	1.31
2000	24508	1.69	8345	1.77	1.39
2001	23168	2.05	7714	2.12	1.73
2002	21929	2.31	7407	2.38	1.93
2003	20786	2.27	6901	2.32	1.87
2004	21668	2.1	7228	2.11	1.70
2005	20060	2.11	6694	2.19	1.70

Notes. Count (N) reports the number of crashes during the relevant year for all drivers and for at-fault drivers. Bankruptcy rate (*Rate*(%)) indicates the bankruptcy filing rate among drivers who experienced a crash during the relevant year. The rate is reported for all drivers and for at-fault drivers. Counts and Bankruptcy Rates are calculated using the subsample of drivers who were between ages 25 and 55 during the study period. Fault status is determined by police-assessed crash report.

TABLE II

Bankruptcy Filing Rate by Medical Status During Three Years Following Crash.

	All	At Fault
Not EDAdmit	.053	.063
EDAdmit	.077	.083

Notes. This table reports bankruptcy rates during the three years following a crash among drivers aged 25 to 55. Bankruptcy rates are reported by crash severity (Not EDAdmit, EDAdmit, Both) and by Fault (All, At Fault). EDAdmit refers to drivers admitted to an emergency room following a crash.

TABLE III

Annual Bankruptcy Filing Rate, Before and After Crash.

Years Relative to Crash	-3	-2	-1	1	2	3
Panel A: Not at Fault						
- Not EDAdmit	0.013	0.014	0.016	0.019	0.019	0.019
- EDAdmit	0.017	0.021	0.024	0.026	0.026	0.026
Panel B: At Fault						
- Not EDAdmit	0.014	0.014	0.017	0.020	0.020	0.0193
- EDAdmit	0.019	0.020	0.026	0.028	0.023	0.027

Notes. This table reports the annual bankruptcy filing rate, during the three years before and after a crash, by injury severity and fault. EDAdmit refers to crashes in which the driver was admitted to an emergency room. Fault status is determined by the police-assessed crash report. Standard errors are given in parentheses. All drivers are aged 25–55.

TABLE IV

PRIOR THREE YEAR BANKRUPTCY RATE BY DANGEROUS DRIVING STATUS.

	No (%)	**Yes** (%)
Violations		
- Reckless Driving	4.24	7.43
- Wrong Way	4.25	4.20
At Fault		
- At Fault	4.09	4.55
DUI		
- DUI	4.22	5.39
- Drugs	4.25	9.15
Speeding		
- Speeding 5 Above	4.24	5.22
- Speeding 10 Above	4.24	5.11
Seatbelt		
- No Seatbelt	4.13	4.89

Notes. This table reports the probability of a bankruptcy filing during the three years *prior* to the crash. Bankruptcy rates are calculated separately for crashes with different proxies for risky driving. The proves are derived from police assessments taken shortly after the crash.

TABLE V

SUMMARY STATISTICS, 3-YEAR SPLIT

ALL CRASHES

	Control Group			Treatment Group			Total		
	mean	sd	count	mean	sd	count	mean	sd	count
Crash Data									
- New Car	0.66	0.47	211912	0.48	0.50	243241	0.56	0.50	455153
- New Car Missing	0.0048	0.069	211912	0.0057	0.075	243241	0.0053	0.072	455153
- Male	0.57	0.50	211912	0.58	0.49	243241	0.57	0.49	455153
- Driver Age	37.6	8.54	211912	37.5	8.59	243241	37.5	8.56	455153
- Speeding	0.012	0.11	211912	0.013	0.11	243241	0.012	0.11	455153
- Speeding Missing	0.50	0.50	211912	0.50	0.50	243241	0.50	0.50	455153
- DUI	0.020	0.14	211912	0.024	0.15	243241	0.022	0.15	455153
- Fault	0.34	0.47	211912	0.34	0.47	243241	0.34	0.47	455153
- Fault Missing	0.35	0.48	211912	0.35	0.48	243241	0.35	0.48	455153
- Kids in Car	0.15	0.36	211912	0.14	0.35	243241	0.15	0.35	455153
- Kids Injured	0.030	0.17	211912	0.031	0.17	243241	0.030	0.17	455153
- High Damage	0.046	0.21	211912	0.046	0.21	243241	0.046	0.21	455153
- Damage Missing	0.47	0.50	211912	0.36	0.48	243241	0.42	0.49	455153
- Crash in prior 3 years	0.14	0.34	211912	0.15	0.36	243241	0.15	0.35	455153
- Two Crashes Prior to 3 Years	0.022	0.15	211912	0.026	0.16	243241	0.024	0.15	455153
CODES Data									
- EDAdmit	0.11	0.32	211912	0.12	0.32	243241	0.12	0.32	455153
- Admitted	0.067	0.25	24238	0.068	0.25	28402	0.067	0.25	52640
- Married	0.59	0.49	10272	0.55	0.50	11874	0.57	0.49	22146
- Uninsured	0.048	0.21	24238	0.076	0.26	28402	0.063	0.24	52640
- High Charge (> 8612)	0.034	0.18	211912	0.025	0.16	243241	0.029	0.17	455153
Bankruptcy Data									
- Bankruptcy in Prior Years	0.038	0.19	211912	0.042	0.20	243241	0.040	0.20	455153
- 1+ Prior Bankruptcy	0.068	0.25	211912	0.075	0.26	243241	0.071	0.26	455153
- 2+ Prior Bankruptcies	0.011	0.11	211912	0.013	0.11	243241	0.012	0.11	455153

Notes. Treatment crashes report crash, hospital, and bankruptcy statistics around the time of the actual crash for the Treatment Group. Placebo Crashes report crash and hospital information around the time of the real crash, but bankruptcy statistics around the time of an imputed placebo crash date that is three years prior to the actual crash. The full construction of Placebo and Treatment Groups is described in section 2.4. Crash data are taken from Utah police crash records. CODES data reflect hospital information that has been merged with crash information. Bankruptcy data are taken from the PACER website.

TABLE VI

SUMMARY STATISTICS, 1-YEAR SPLIT

ALL CRASHES

	Control Group			Treatment Group			Total		
	mean	sd	count	mean	sd	count	mean	sd	count
Crash Data									
- New Car	0.54	0.50	335426	0.47	0.50	350836	0.50	0.50	686262
- New Car Missing	0.0054	0.074	335426	0.0058	0.076	350836	0.0056	0.075	686262
- Male	0.58	0.49	335426	0.58	0.49	350836	0.58	0.49	686262
- Driver Age	37.5	8.62	335426	37.5	8.64	350836	37.5	8.63	686262
- Speeding	0.012	0.11	335426	0.013	0.11	350836	0.013	0.11	686262
- Speeding Missing	0.49	0.50	335426	0.49	0.50	350836	0.49	0.50	686262
- DUI	0.021	0.14	335426	0.022	0.15	350836	0.022	0.15	686262
- Fault	0.34	0.47	335426	0.34	0.48	350836	0.34	0.47	686262
- Fault Missing	0.35	0.48	335426	0.35	0.48	350836	0.35	0.48	686262
- Kids in Car	0.15	0.35	335426	0.14	0.35	350836	0.15	0.35	686262
- Kids Injured	0.030	0.17	335426	0.030	0.17	350836	0.030	0.17	686262
- High Damage	0.045	0.21	335426	0.046	0.21	350836	0.046	0.21	686262
- Damage Missing	0.42	0.49	335426	0.38	0.49	350836	0.40	0.49	686262
- Crash in prior 1 years	0.060	0.24	335426	0.066	0.25	350836	0.063	0.24	686262
- Two Crashes Prior to 3 Years	0.0037	0.060	335426	0.0041	0.064	350836	0.0039	0.062	686262
CODES Data									
- EDAdmit	0.11	0.32	335426	0.11	0.32	350836	0.11	0.32	686262
- Admitted	0.064	0.24	38563	0.066	0.25	40243	0.065	0.25	78806
- Married	0.56	0.50	16248	0.55	0.50	16812	0.56	0.50	33060
- Uninsured	0.059	0.24	38563	0.068	0.25	40243	0.064	0.24	78806
- High Charge (> 8612)	0.030	0.17	335426	0.028	0.16	350836	0.029	0.17	686262
Bankruptcy Data									
- Bankruptcy in Prior Years	0.020	0.14	335426	0.021	0.14	350836	0.021	0.14	686262
- 1+ Prior Bankruptcy	0.087	0.28	335426	0.090	0.29	350836	0.088	0.28	686262
- 2+ Prior Bankruptcies	0.016	0.13	335426	0.017	0.13	350836	0.016	0.13	686262

Notes. Treatment crashes report crash, hospital, and bankruptcy statistics around the time of the actual crash for the Treatment Group. Placebo Crashes report crash and hospital information around the time of the real crash, but bankruptcy statistics around the time of an imputed placebo crash date that is one year prior to the actual crash. The full construction of Placebo and Treatment Groups is described in section 2.4. Crash data are taken from Utah police crash records. CODES data reflect hospital information that has been merged with crash information. Bankruptcy data are taken from the PACER website.

TABLE VII

3 Year Sample, All Crashes

	(1)-All	(2)-All	(3)-EDAdmit	(4)-EDAdmit	(5)-EDAdmit, Uninsured	(6)-High Charge	(7)-High Charge	(8)-High Charge, Uninsured
Crash	0.0022**	0.0015**	0.0031*	0.0023	0.0093	0.0045	0.0042	0.026*
	(6.00)	(4.15)	(2.39)	(1.84)	(1.72)	(1.61)	(1.50)	(2.03)
Year 3 Before Crash × Crash	-0.00047	-0.00037	-0.0030	-0.0029	-0.013*	-0.0023	-0.0023	-0.022**
	(-0.89)	(-0.71)	(-1.79)	(-1.74)	(-2.46)	(-0.61)	(-0.63)	(-4.38)
Year 2 Before Crash × Crash	-0.00070	-0.00062	0.0012	0.0013	-0.0074	0.00088	0.00080	-0.021**
	(-1.34)	(-1.21)	(0.62)	(0.66)	(-1.17)	(0.22)	(0.20)	(-4.36)
Year 1 After Crash × Crash	0.000025	0.000026	-0.0016	-0.0016	-0.0076	-0.00061	-0.00060	-0.013
	(0.05)	(0.05)	(-0.94)	(-0.93)	(-1.18)	(-0.16)	(-0.16)	(-1.35)
Year 2 After Crash × Crash	-0.00059	-0.00062	-0.0028	-0.0027	-0.0022	-0.0013	-0.0013	-0.0067
	(-1.18)	(-1.28)	(-1.70)	(-1.70)	(-0.28)	(-0.37)	(-0.36)	(-0.50)
Year 3 After Crash × Crash	-0.0027**	-0.0027**	-0.0051**	-0.0051**	-0.014**	-0.0019	-0.0018	-0.021**
	(-5.99)	(-6.14)	(-3.48)	(-3.52)	(-3.11)	(-0.52)	(-0.49)	(-4.98)
County	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Current Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	No	Yes	No	Yes	No	No	Yes	Yes
N. of cases	2634443	2634443	300293	300293	18511	75170	75170	4374
Ref. Bankruptcy Probability	0.016	0.016	0.023	0.023	0.026	0.026	0.026	0.023
F-Test for Future Periods	1.2e-09	4.7e-10	0.013	0.012	0.076	0.96	0.97	0.038

Notes. This table reports estimates of a probit specification using data on all crashes during the period 1999-2002. The unit of observation is a driver-year. The dependent variable is equal to one in a particular year if a driver files for bankruptcy during that year. All drivers in the sample have a "crash date." For a driver in the treatment group, the crash date is an actual crash date, occurring during 1999-2002. For a driver in the control group, the crash date is a "placebo crash" date, defined as the date three years prior to the driver's actual crash date (which occurred during 2002-2005). Column (1) includes only county and calendar year dummies, a crash dummy that identifies drivers in the treatment group, time dummies indicating the years before and after the crash date, and interactions between the crash and time dummies. The excluded category is the year prior to the crash date. Column (2) adds additional controls. Columns (3) and (4) subset on drivers in EDAdmit crashes. Columns (6) and (7) subset on drivers in High Charge crashes. Columns (5) and (8) subset on uninsured drivers in EDAdmit and High Charge crashes, respectively. Coefficients reported are marginal effects from a probit regression; t-statistics are in parentheses; standard errors are clustered at the driver ID level. * $p < 0.05$, ** $p < 0.01$.

TABLE VIII

1 Year Sample Panel, All Crashes.

	(1)-All	(2)-All	(3)-EDAdmit	(4)-EDAdmit	(5)-EDAdmit, Uninsured	(6)-High Charge	(7)-High Charge	(8)-High Charge, Uninsured
Crash	0.00065**	0.00055**	0.0014*	0.0013*	-0.0011	0.0011	0.0010	0.0050
	(3.74)	(3.27)	(2.31)	(2.17)	(-0.46)	(0.89)	(0.84)	(0.94)
Quarter 4 Before Crash × Crash	-0.00017	-0.00017	-0.0010	-0.0010	0.0071	-0.0011	-0.0011	0.0068
	(-0.73)	(-0.74)	(-1.36)	(-1.37)	(1.15)	(-0.68)	(-0.69)	(0.47)
Quarter 3 Before Crash × Crash	-0.00048*	-0.00046*	-0.00093	-0.00093	0.0082	-0.00083	-0.00081	-0.00037
	(-2.14)	(-2.09)	(-1.24)	(-1.25)	(1.27)	(-0.52)	(-0.51)	(-0.05)
Quarter 2 Before Crash × Crash	-0.00046*	-0.00045*	-0.00100	-0.00099	-0.00039	0.00055	0.00053	-0.0041
	(-2.03)	(-2.05)	(-1.35)	(-1.35)	(-0.13)	(0.30)	(0.29)	(-1.21)
Quarter 1 After Crash × Crash	-0.00062**	-0.00060**	-0.0014	-0.0014	-0.000028	0.00013	0.000090	-0.0057*
	(-2.78)	(-2.78)	(-1.94)	(-1.92)	(-0.01)	(0.07)	(0.05)	(-2.51)
Quarter 2 After Crash × Crash	-0.00026	-0.00025	-0.0016*	-0.0016*	-0.0049**	-0.0020	-0.0020	-0.0072**
	(-1.11)	(-1.11)	(-2.37)	(-2.39)	(-3.01)	(-1.44)	(-1.46)	(-4.97)
Quarter 3 After Crash × Crash	-0.00017	-0.00016	-0.00073	-0.00070	0.00025	0.0011	0.0011	0.0023
	(-0.74)	(-0.71)	(-0.94)	(-0.92)	(0.07)	(0.59)	(0.59)	(0.26)
Quarter 4 After Crash × Crash	-0.00058**	-0.00057**	-0.0021**	-0.0020**	0.0022	-0.0022	-0.0022	-0.0050
	(-2.66)	(-2.66)	(-3.22)	(-3.23)	(0.54)	(-1.63)	(-1.59)	(-1.96)
County	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Current Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	No	Yes	No	Yes	No	No	Yes	Yes
N. of cases	5388419	5388419	614015	614015	37856	154484	154484	8622
Ref. Bankruptcy Probability	0.0054	0.0054	0.0081	0.0081	0.0067	0.0093	0.0093	0.0077
F-Test for Future Periods	0.029	0.029	0.046	0.044	0.13	0.17	0.17	0.10

Notes. This table reports estimates of a probit specification using data on all crashes during the period 1997-2004. The unit of observation is a driver-quarter. The dependent variable is equal to one in a particular quarter if a driver files for bankruptcy during that quarter. All drivers in the sample have a "crash date." For a driver in the treatment group, the crash date is an actual crash date, occurring during 1998-2005. For a driver in the control group, the crash date is a "placebo crash" date, defined as the date three years prior to the driver's actual crash date (which occurred during 2002-2005). Column (1) includes only county and calendar year dummies, a crash dummy that identifies drivers in the treatment group, time dummies indicating the quarters before and after the crash date, and interactions between the crash and time dummies. The excluded category is the quarter prior to the crash date. Column (2) adds additional controls. Columns (3) and (4) subset on drivers in EDAdmit crashes. Columns (6) and (7) subset on drivers in High Charge crashes. Columns (5) and (8) subset on uninsured drivers in EDAdmit and High Charge crashes, respectively. Coefficients reported are marginal effects from a probit regression; t-statistics are in parentheses; standard errors are clustered at the driver ID level. * $p < 0.05$, ** $p < 0.01$.

TABLE IX

3 Year Sample Panel, At-Fault Drivers.

	(1)-All	(2)-All	(3)-EDAdmit	(4)-EDAdmit	(5)-EDAdmit, Uninsured	(6)-High Charge	(7)-High Charge
Crash	0.0028**	0.0020**	0.0066*	0.0056*	0.020	0.0084	0.0083
	(4.22)	(3.18)	(2.57)	(2.19)	(1.76)	(1.54)	(1.53)
Year 3 Before Crash × Crash	-0.00052	-0.00045	-0.0037	-0.0035	-0.017	-0.0084	-0.0084
	(-0.55)	(-0.49)	(-1.09)	(-1.06)	(-1.86)	(-1.44)	(-1.49)
Year 2 Before Crash × Crash	-0.00061	-0.00056	0.00023	0.00017	-0.0099	0.0016	0.00075
	(-0.65)	(-0.61)	(0.06)	(0.05)	(-0.81)	(0.20)	(0.10)
Year 1 After Crash × Crash	-0.00045	-0.00041	-0.0039	-0.0038	-0.018*	-0.000024	-0.00045
	(-0.50)	(-0.47)	(-1.23)	(-1.24)	(-2.41)	(-0.00)	(-0.06)
Year 2 After Crash × Crash	-0.00082	-0.00086	-0.0065*	-0.0065*	-0.013	-0.0093	-0.0093
	(-0.94)	(-1.01)	(-2.21)	(-2.27)	(-1.26)	(-1.79)	(-1.83)
Year 3 After Crash × Crash	-0.0030**	-0.0030**	-0.0079**	-0.0079**	-0.018*	-0.0094	-0.0095
	(-3.86)	(-3.90)	(-2.97)	(-3.06)	(-2.52)	(-1.85)	(-1.92)
County	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Current Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	No	Yes	No	Yes	No	No	Yes
N. of cases	891765	891765	73622	73622	4699	19789	19789
Ref. Bankruptcy Probability	0.017	0.017	0.026	0.026	0.028	0.026	0.026
F-Test for Future Periods	0.0012	0.00096	0.064	0.054	0.36	0.19	0.19

Notes. This table reports estimates of a probit specification using data on crashes involving at-fault drivers during the period 1999-2002. The unit of observation is a driver-year. The dependent variable is equal to one in a particular year if a driver files for bankruptcy during that year. All drivers in the sample have a "crash date." For a driver in the treatment group, the crash date is an actual crash date, occurring during 1999-2002. For a driver in the control group, the crash date is a "placebo crash" date, defined as the date three years prior to the driver's actual crash date (which occurred during 2002-2005). Column (1) includes only county and calendar year dummies, a crash dummy that identifies drivers in the treatment group, time dummies indicating the years before and after the crash date, and interactions between the crash and time dummies. The excluded category is the year prior to the crash date. Column (2) adds additional controls. Columns (3) and (4) subset on drivers in EDAdmit crashes. Columns (6) and (7) subset on drivers in High Charge crashes. Columns (5) and (8) subset on uninsured drivers in EDAdmit and High Charge crashes, respectively. Coefficients reported are marginal effects from a probit regression; t-statistics are in parentheses; standard errors are clustered at the driver ID level. * $p < 0.05$, ** $p < 0.01$.

TABLE X

YEAR SAMPLE PANEL, AT-FAULT DRIVERS

	(1)-All	(2)-All	(3)-EDAdmit	(4)-EDAdmit	(5)-EDAdmit, Uninsured	(6)-High Charge	(7)-High Charge
Crash	0.00045	0.00035	0.0011	0.0010	-0.0016	-0.0028	-0.0027
	(1.46)	(1.18)	(0.92)	(0.89)	(-0.33)	(-1.13)	(-1.10)
Quarter 4 Before Crash × Crash	0.00020	0.00019	0.00028	0.00023	0.025	0.0016	0.0015
	(0.44)	(0.44)	(0.15)	(0.13)	(0.92)	(0.39)	(0.36)
Quarter 3 Before Crash × Crash	-0.000022	0.0000026	0.00029	0.00025	0.0095	0.0024	0.0022
	(-0.05)	(0.01)	(0.17)	(0.15)	(0.75)	(0.57)	(0.55)
Quarter 2 Before Crash × Crash	0.00014	0.00014	0.0033	0.0031	0.030	0.014	0.014
	(0.32)	(0.32)	(1.40)	(1.38)	(0.98)	(1.84)	(1.81)
Quarter 1 After Crash × Crash	-0.000096	-0.000081	0.00026	0.00022	-0.0028	0.012	0.011
	(-0.22)	(-0.19)	(0.15)	(0.13)	(-0.55)	(1.59)	(1.57)
Quarter 2 After Crash × Crash	0.00018	0.00020	-0.0013	-0.0013	-0.0059	-0.0010	-0.0013
	(0.41)	(0.45)	(-0.88)	(-0.92)	(-1.82)	(-0.31)	(-0.40)
Quarter 3 After Crash × Crash	0.00037	0.00038	0.00062	0.00057	0.0061	0.0047	0.0047
	(0.83)	(0.86)	(0.35)	(0.32)	(0.58)	(0.99)	(0.99)
Quarter 4 After Crash × Crash	-0.00019	-0.00018	-0.0022	-0.0023	0.0029	0.0052	0.0049
	(-0.47)	(-0.44)	(-1.78)	(-1.83)	(0.34)	(0.99)	(0.96)
County	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Current Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	No	Yes	No	Yes	No	No	Yes
N. of cases	1846367	1846367	152159	152159	9217	40405	40405
Ref. Bankruptcy Probability	0.0056	0.0056	0.0090	0.0090	0.0098	0.0079	0.0079
F-Test for Future Periods	0.69	0.68	0.29	0.27	0.40	0.11	0.10

Notes. This table reports estimates of a probit specification using data on crashes involving at-fault drivers during the period 1997-2004. The unit of observation is a driver-quarter. The dependent variable is equal to one in a particular quarter if a driver files for bankruptcy during that quarter. All drivers in the sample have a "crash date." For a driver in the treatment group, the crash date is an actual crash date, occurring during 1998-2005. For a driver in the control group, the crash date is a "placebo crash" date, defined as the date three years prior to the driver's actual crash date (which occurred during 2002-2005). Column (1) includes only county and calendar year dummies, a crash dummy that identifies drivers in the treatment group, time dummies indicating the quarters before and after the crash date, and interactions between the crash and time dummies. The excluded category is the quarter prior to the crash date. Column (2) adds additional controls. Columns (3) and (4) subset on drivers in EDAdmit crashes. Columns (6) and (7) subset on drivers in High Charge crashes. Columns (5) and (8) subset on uninsured drivers in EDAdmit and High Charge crashes, respectively. Coefficients reported are marginal effects from a probit regression; t-statistics are in parentheses; standard errors are clustered at the driver ID level. * $p < 0.05$, ** $p < 0.01$.

TABLE XI

3 Year Sample Panel, At Fault Drivers

	(1)-New Car	(2)-New Car, Uninsured	(3)-Age 35-45	(4)-Age 35-45 Uninsured	(5) Low Income
Crash	-0.00099	0.019	0.0019**	0.019	0.025
	(-0.30)	(1.33)	(3.11)	(1.94)	(1.52)
Year 3 Before Crash × Crash	0.0029	-0.015*	-0.00078	-0.015	-0.023
	(0.58)	(-2.23)	(-0.93)	(-1.80)	(-1.10)
Year 2 Before Crash × Crash	0.0027	-0.013	-0.00100	-0.020**	0.024
	(0.51)	(-1.86)	(-1.21)	(-3.11)	(0.87)
Year 1 After Crash × Crash	0.0042	-0.0092	-0.00014	-0.015	-0.0023
	(0.78)	(-1.16)	(-0.17)	(-1.90)	(-0.10)
Year 2 After Crash × Crash	0.0042	-0.011	-0.0013	-0.015	-0.036*
	(0.70)	(-1.60)	(-1.74)	(-1.82)	(-1.98)
Year 3 After Crash × Crash	0.0014	-0.011	-0.0033**	-0.018**	-0.027
	(0.24)	(-1.96)	(-4.75)	(-2.80)	(-1.34)
Current Year	Yes	Yes	Yes	Yes	Yes
County	Yes	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes	Yes
N. of cases	33277	1503	968148	6152	7146
Ref. Bankruptcy Probability	0.015	0.032	0.017	0.034	0.10
F-Test for Future Periods	0.81	0.74	0.000014	0.33	0.23

Marginal effects; t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$

TABLE XII

1 YEAR SAMPLE PANEL, AT-FAULT DRIVERS.

	(1)-New Car	(2)-New Car, Uninsured	(3)-Age 35-45	(4)-Age 35-45 Uninsured	(5) Low Income
Crash	0.0013	0.00021	0.00069*	0.0040	-0.00056
	(0.80)	(0.36)	(2.42)	(0.94)	(-0.08)
Quarter 4 Before Crash × Crash	-0.00075	0.84**	-0.00014	-0.0031	0.0034
	(-0.34)	(28.31)	(-0.36)	(-0.77)	(0.30)
Quarter 3 Before Crash × Crash	-0.0020	0.79**	-0.00081*	-0.0018	0.0040
	(-1.11)	(22.06)	(-2.35)	(-0.36)	(0.36)
Quarter 2 Before Crash × Crash	0.0014	0.88**	-0.00083*	-0.0018	0.022
	(0.52)	(35.83)	(-2.40)	(-0.37)	(1.34)
Quarter 1 After Crash × Crash	0.0020	-0.00028	-0.00043	-0.0026	-0.00063
	(0.66)	(-0.48)	(-1.18)	(-0.57)	(-0.06)
Quarter 2 After Crash × Crash	0.0015	0.00042	-0.00033	-0.0080**	0.00060
	(0.53)	(0.23)	(-0.86)	(-5.94)	(0.06)
Quarter 3 After Crash × Crash	-0.00081	-0.00042	0.00023	-0.0022	0.024
	(-0.38)	(-1.11)	(0.55)	(-0.49)	(1.46)
Quarter 4 After Crash × Crash	-0.0021	0.0000018	-0.00064	-0.0019	-0.0015
	(-1.27)	(0.00)	(-1.79)	(-0.43)	(-0.16)
Current Year	Yes	Yes	Yes	Yes	Yes
County	Yes	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes	Yes
N. of cases	58720	2328	1936854	12045	16542
Ref. Bankruptcy Probability	0.0051	0.017	0.0059	0.017	0.027
F-Test for Future Periods	0.41	0.94	0.16	0.27	0.26

Marginal effects; t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$

TABLE XIII
3 Year Sample Panel, At Fault Drivers with Underage Passengers

	(1)-All	(2)-EdAdmit	(3)-High Charge	(4)-New Car	(5)-Age 35-45	(6)-Low Income	(7)-Uninsured
Crash	0.0024	0.012	0.0099	0.00048	0.0063	-0.020	0.28**
	(1.40)	(1.91)	(0.77)	(0.23)	(1.15)	(-0.46)	(4.11)
Year 3 Before Crash × Crash	-0.0013	-0.0058	-0.0012	-0.00044	-0.0054	0.040	0.0044*
	(-0.54)	(-0.72)	(-0.06)	(-0.15)	(-0.82)	(0.57)	(2.39)
Year 2 Before Crash × Crash	-0.0019	-0.0041	-0.0017	0.0019	-0.0048	0.090	-0.019*
	(-0.80)	(-0.49)	(-0.10)	(0.57)	(-0.76)	(1.03)	(-2.42)
Year 1 After Crash × Crash	-0.0015	-0.012*	-0.0082	0.0054	-0.0069	-0.040	-0.022**
	(-0.66)	(-2.24)	(-0.64)	(1.42)	(-1.16)	(-0.98)	(-2.67)
Year 2 After Crash × Crash	0.00041	-0.0077	0.017	0.012*	-0.0096	0.017	-0.018*
	(0.17)	(-1.07)	(0.52)	(2.39)	(-1.91)	(0.27)	(-2.39)
Year 3 After Crash × Crash	-0.00058	-0.0045	0.0051	0.0081	0.00011	0.064	-0.017*
	(-0.25)	(-0.58)	(0.25)	(1.69)	(0.02)	(0.77)	(-2.52)
Current Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N. of cases	125456	11998	2708	65113	17053	1258	477
Ref. Bankruptcy Probability	0.017	0.026	0.032	0.016	0.040	0.064	0.016

Marginal effects; t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$

TABLE XIV

1 Year Sample Panel, At Fault Drivers with Underage Passengers

	(1)-All	(2)-EdAdmit	(3)-High Charge	(4)-New Car	(5)-Age 35-45	(6)-Low Income	(7)-Uninsured
Crash	0.00096	0.0044	0.0023	0.00034	0.0033	-0.0075	0.0049
	(1.13)	(1.49)	(0.68)	(0.30)	(1.63)	(-0.55)	(0.86)
Quarter 4 Before Crash × Crash	-0.0010	-0.0049**	-0.0025**	0.00032	-0.0042**	-0.017*	0.91**
	(-1.02)	(-2.62)	(-2.62)	(0.20)	(-3.04)	(-2.17)	(14.71)
Quarter 3 Before Crash × Crash	0.0013	0.0020	0.0012	0.0021	-0.0032	0.040	-0.00053
	(0.93)	(0.40)	(0.22)	(1.00)	(-1.79)	(0.94)	(-0.30)
Quarter 2 Before Crash × Crash	-0.0010	-0.0016	-0.0011	-0.0010	-0.0023	0.044	0.00032
	(-1.06)	(-0.51)	(-0.44)	(-0.82)	(-1.10)	(0.93)	(0.09)
Quarter 1 After Crash × Crash	-0.00100	0.0016	0.86**	-0.000041	-0.000076	0.026	
	(-0.98)	(0.28)	(49.85)	(-0.03)	(-0.02)	(0.63)	
Quarter 2 After Crash × Crash	-0.00062	-0.0043*	-0.0022*	0.0020	-0.0033	0.0073	
	(-0.59)	(-2.21)	(-2.07)	(0.99)	(-1.85)	(0.32)	
Quarter 3 After Crash × Crash	-0.00058	-0.0022	-0.0018	0.0014	-0.0022	0.019	-0.00046
	(-0.54)	(-0.76)	(-1.21)	(0.71)	(-1.03)	(0.62)	(-0.27)
Quarter 4 After Crash × Crash	0.00020	-0.0033	-0.00016	0.0048	-0.0041**	0.012	
	(0.16)	(-1.32)	(-0.04)	(1.60)	(-2.98)	(0.43)	
Current Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N. of cases	257191	24105	5498	115311	35526	2875	494
Ref. Bankruptcy Probability	0.0056	0.0090	0.0053	0.0046	0.012	0.021	0.021

Marginal effects; t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$

APPENDIX

TABLE XV

Proportion of Drivers Who are Uninsured by Charge Quintile and Injury Status

Panel A: Charge Quintiles	1	2	3	4	5
Uninsured	0.0696	0.0608	0.0589	0.0492	0.0652
Panel B: Injury Status	No Injury	Possible Injury	Bruises/ Abrasions	Broken Bones/ Bleeding	Fatal
Uninsured	0.0632	0.0493	0.0642	0.0803	0.0608

TABLE XVI
3 Year Sample Panel, All Crashes

	(1)-All	(2)-All	(3)-EDAdmit	(4)-EDAdmit	(5)-EDAdmit, Uninsured	(6)-High Charge	(7)-High Charge	High Charge Uninsured
Year 3 Before Crash (d)	-0.0032**	-0.0033**	-0.0033*	-0.0033**	0.0067	-0.0029	-0.0031	0.045
	(-8.97)	(-9.42)	(-2.55)	(-2.59)	(0.86)	(-1.13)	(-1.22)	(1.43)
Year 2 Before Crash (d)	-0.0019**	-0.0020**	-0.0036**	-0.0036**	0.0073	-0.0017	-0.0018	0.043
	(-5.22)	(-5.52)	(-2.86)	(-2.90)	(0.92)	(-0.66)	(-0.69)	(1.40)
Year 1 After Crash (d)	0.0020**	0.0021**	0.0029*	0.0029*	0.0060	0.0030	0.0032	0.028
	(4.89)	(5.12)	(1.98)	(2.04)	(0.76)	(1.04)	(1.12)	(1.06)
Year 2 After Crash (d)	0.0035**	0.0037**	0.0057**	0.0059**	0.0069	0.0068*	0.0074*	0.029
	(8.06)	(8.64)	(3.70)	(3.87)	(0.85)	(2.21)	(2.39)	(1.07)
Year 3 After Crash (d)	0.0056**	0.0059**	0.0093**	0.0097**	0.024*	0.0084**	0.0093**	0.074
	(12.23)	(13.01)	(5.68)	(5.90)	(2.36)	(2.65)	(2.87)	(1.82)
Crash (d)	0.0022**	0.0015**	0.0031*	0.0023	0.0093	0.0045	0.0042	0.026*
	(6.00)	(4.15)	(2.39)	(1.84)	(1.72)	(1.61)	(1.50)	(2.03)
Year 3 Before Crash × Crash (d)	-0.00047	-0.00037	-0.0030	-0.0029	-0.013*	-0.0023	-0.0023	-0.022**
	(-0.89)	(-0.71)	(-1.79)	(-1.74)	(-2.46)	(-0.61)	(-0.63)	(-4.38)
Year 2 Before Crash × Crash (d)	-0.00070	-0.00062	0.0012	0.0013	-0.0074	0.00088	0.00080	-0.021**
	(-1.34)	(-1.21)	(0.62)	(0.66)	(-1.17)	(0.22)	(0.20)	(-4.36)
Year 1 After Crash × Crash (d)	0.000025	0.000026	-0.0016	-0.0016	-0.0076	-0.00061	-0.00060	-0.013
	(0.05)	(0.05)	(-0.94)	(-0.93)	(-1.18)	(-0.16)	(-0.16)	(-1.35)
Year 2 After Crash × Crash (d)	-0.00059	-0.00062	-0.0028	-0.0027	-0.0022	-0.0013	-0.0013	-0.0067
	(-1.18)	(-1.28)	(-1.70)	(-1.70)	(-0.28)	(-0.37)	(-0.36)	(-0.50)
Year 3 After Crash × Crash (d)	-0.0027**	-0.0027**	-0.0051**	-0.0051**	-0.014**	-0.0019	-0.0018	-0.021**
	(-5.99)	(-6.14)	(-3.48)	(-3.52)	(-3.11)	(-0.52)	(-0.49)	(-4.98)
New Car (d)		-0.0027**		-0.0022**			-0.000087	-0.0038
		(-17.55)		(-4.08)			(-0.07)	(-0.86)
New Car Missing (d)		0.0013		0.0022			0.0067	0.017
		(1.25)		(0.65)			(0.84)	(0.45)
Driver Age		0.00095**		0.0018**			0.0016**	0.0061*
		(11.87)		(6.25)			(2.63)	(2.52)
Driver Age^2		-0.000016**		-0.000027**			-0.000023**	-0.000080**
		(-16.03)		(-7.39)			(-3.09)	(-2.59)
Male (d)		0.0015**		0.0021**			0.0010	-0.013*
		(10.02)		(4.02)			(0.94)	(-2.46)
Crash in prior 3 years (d)		0.0055**		0.0072**			0.0069**	0.0062
		(23.22)		(8.76)			(4.02)	(0.94)
Two Prior Crashes (d)		0.010**		0.013**			0.0061	0.012
		(16.30)		(6.23)			(1.65)	(0.73)
County	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Current Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N. of cases	2634443	2634443	300293	300293	18511	75170	75170	4374
Ref. Bankruptcy Probability	0.016	0.016	0.023	0.023	0.026	0.026	0.026	0.023
F-Test for Future Periods	1.2e-09	4.7e-10	0.013	0.012	0.076	0.96	0.97	0.038

Marginal effects; t statistics in parentheses
(d) for discrete change of dummy variable from 0 to 1
* $p < 0.05$, ** $p < 0.01$

TABLE XVII
1 Year Sample Panel, All Crashes

	(1)-All	(2)-All	(3)-EDAdmit	(4)-EDAdmit	(5)-EDAdmit, Uninsured	(6)-High Charge	(7)-High Charge	High Charge Uninsured
Quarter 4 Before Crash (d)	0.000070	0.000018	0.000059	0.000016	-0.0032	-0.00069	-0.00069	-0.0029
	(0.39)	(0.10)	(0.09)	(0.03)	(-1.70)	(-0.58)	(-0.59)	(-0.58)
Quarter 3 Before Crash (d)	0.00039*	0.00033	0.00047	0.00044	-0.0032	0.00062	0.00060	0.00021
	(2.10)	(1.85)	(0.73)	(0.70)	(-1.68)	(0.48)	(0.47)	(0.03)
Quarter 2 Before Crash (d)	0.00057**	0.00053**	0.00082	0.00078	0.0026	-0.00047	-0.00047	0.0086
	(3.02)	(2.89)	(1.25)	(1.21)	(0.92)	(-0.39)	(-0.39)	(0.86)
Quarter 1 After Crash (d)	0.00045*	0.00037*	-0.000046	-0.00011	0.00045	-0.0022*	-0.0021*	0.0097
	(2.38)	(2.06)	(-0.07)	(-0.18)	(0.18)	(-1.99)	(-1.97)	(0.92)
Quarter 2 After Crash (d)	0.00037*	0.00033	0.00067	0.00062	0.0020	0.00062	0.00066	0.0086
	(1.99)	(1.81)	(1.02)	(0.96)	(0.72)	(0.48)	(0.51)	(0.86)
Quarter 3 After Crash (d)	0.00056**	0.00053**	0.00074	0.00070	0.0010	0.00042	0.00045	0.0036
	(2.96)	(2.84)	(1.13)	(1.08)	(0.39)	(0.33)	(0.35)	(0.46)
Quarter 4 After Crash (d)	0.00091**	0.00090**	0.0022**	0.0021**	0.0011	0.0017	0.0017	0.016
	(4.64)	(4.68)	(3.01)	(3.00)	(0.42)	(1.20)	(1.22)	(1.21)
Crash (d)	0.00065**	0.00055**	0.0014*	0.0013*	-0.0011	0.0011	0.0010	0.0050
	(3.74)	(3.27)	(2.31)	(2.17)	(-0.46)	(0.89)	(0.84)	(0.94)
Quarter 4 Before Crash × Crash (d)	-0.00017	-0.00017	-0.0010	-0.0010	0.0071	-0.0011	-0.0011	0.0068
	(-0.73)	(-0.74)	(-1.36)	(-1.37)	(1.15)	(-0.68)	(-0.69)	(0.47)
Quarter 3 Before Crash × Crash (d)	-0.00048*	-0.00046*	-0.00093	-0.00093	0.0082	-0.00083	-0.00081	-0.00037
	(-2.14)	(-2.09)	(-1.24)	(-1.25)	(1.27)	(-0.52)	(-0.51)	(-0.05)
Quarter 2 Before Crash × Crash (d)	-0.00046*	-0.00045*	-0.00100	-0.00099	-0.00039	0.00055	0.00053	-0.0041
	(-2.03)	(-2.05)	(-1.35)	(-1.35)	(-0.13)	(0.30)	(0.29)	(-1.21)
Quarter 1 After Crash × Crash (d)	-0.00062**	-0.00060**	-0.0014	-0.0014	-0.000028	0.00013	0.000090	-0.0057*
	(-2.78)	(-2.78)	(-1.94)	(-1.92)	(-0.01)	(0.07)	(0.05)	(-2.51)
Quarter 2 After Crash × Crash (d)	-0.00026	-0.00025	-0.0016*	-0.0016*	-0.0049**	-0.0020	-0.0020	-0.0072**
	(-1.11)	(-1.11)	(-2.37)	(-2.39)	(-3.01)	(-1.44)	(-1.46)	(-4.97)
Quarter 3 After Crash × Crash (d)	-0.00017	-0.00016	-0.00073	-0.00070	0.00025	0.0011	0.0011	0.0023
	(-0.74)	(-0.71)	(-0.94)	(-0.92)	(0.07)	(0.59)	(0.59)	(0.26)
Quarter 4 After Crash × Crash (d)	-0.00058**	-0.00057**	-0.0021**	-0.0020**	0.0022	-0.0022	-0.0022	-0.0050
	(-2.66)	(-2.66)	(-3.22)	(-3.23)	(0.54)	(-1.63)	(-1.59)	(-1.96)
New Car (d)		-0.00095**		-0.00086**			-0.00018	0.00066
		(-15.93)		(-4.07)			(-0.40)	(0.35)
New Car Missing (d)		0.00035		0.0019			0.0059	0.021
		(0.87)		(1.31)			(1.57)	(0.97)
Driver Age		0.00048**		0.00070**			0.00080**	0.0017
		(13.87)		(5.83)			(3.14)	(1.60)
Driver Age^2		-0.0000075**		-0.000010**			-0.000011**	-0.000023
		(-16.77)		(-6.60)			(-3.49)	(-1.68)
Male (d)		0.00056**		0.00063**			0.00010	-0.0024
		(9.40)		(3.02)			(0.24)	(-1.27)
Crash in prior 1 years (d)		0.0019**		0.0017**			0.0024*	-0.0022
		(13.33)		(3.70)			(2.48)	(-0.85)
Two Prior Crashes (d)		0.0038**		0.0051*			0.0053	0.011
		(5.94)		(2.41)			(1.33)	(0.61)
County	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Current Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N. of cases	5388419	5388419	614015	614015	37856	154484	154484	8622
Ref. Bankruptcy Probability	0.0054	0.0054	0.0081	0.0081	0.0067	0.0093	0.0093	0.0077
F-Test for Future Periods	0.029	0.029	0.046	0.044	0.13	0.17	0.17	0.10

Marginal effects; t statistics in parentheses

(d) for discrete change of dummy variable from 0 to 1

* $p < 0.05$, ** $p < 0.01$

TABLE XVIII
3 Year Sample Panel, At-Fault Drivers

	(1)-All	(2)-All	(3)-EDAdmit	(4)-EDAdmit	(5)-EDAdmit, Uninsured	(6)-High Charge	(7)-High Charge
Year 3 Before Crash	-0.0029**	-0.0030**	-0.0031	-0.0028	0.011	-0.00015	-0.000027
	(-4.52)	(-4.65)	(-1.15)	(-1.05)	(0.57)	(-0.03)	(-0.00)
Year 2 Before Crash	-0.0020**	-0.0020**	-0.0051	-0.0048	0.010	0.00022	0.00049
	(-2.96)	(-3.05)	(-1.93)	(-1.86)	(0.55)	(0.04)	(0.09)
Year 1 After Crash	0.0033**	0.0033**	0.0067*	0.0066*	0.031	0.0037	0.0041
	(4.29)	(4.40)	(2.06)	(2.06)	(1.28)	(0.62)	(0.68)
Year 2 After Crash	0.0037**	0.0039**	0.0050	0.0051	0.017	0.012	0.013
	(4.81)	(5.11)	(1.59)	(1.63)	(0.80)	(1.79)	(1.87)
Year 3 After Crash	0.0062**	0.0065**	0.012**	0.012**	0.046	0.018*	0.019*
	(7.54)	(7.94)	(3.44)	(3.46)	(1.61)	(2.39)	(2.51)
Crash	0.0028**	0.0020**	0.0066*	0.0056*	0.020	0.0084	0.0083
	(4.22)	(3.18)	(2.57)	(2.19)	(1.76)	(1.54)	(1.53)
Year 3 Before Crash × Crash	-0.00052	-0.00045	-0.0037	-0.0035	-0.017	-0.0084	-0.0084
	(-0.55)	(-0.49)	(-1.09)	(-1.06)	(-1.86)	(-1.44)	(-1.49)
Year 2 Before Crash × Crash	-0.00061	-0.00056	0.00023	0.00017	-0.0099	0.0016	0.00075
	(-0.65)	(-0.61)	(0.06)	(0.05)	(-0.81)	(0.20)	(0.10)
Year 1 After Crash × Crash	-0.00045	-0.00041	-0.0039	-0.0038	-0.018*	-0.000024	-0.00045
	(-0.50)	(-0.47)	(-1.23)	(-1.24)	(-2.41)	(-0.00)	(-0.06)
Year 2 After Crash × Crash	-0.00082	-0.00086	-0.0065*	-0.0065*	-0.013	-0.0093	-0.0093
	(-0.94)	(-1.01)	(-2.21)	(-2.27)	(-1.26)	(-1.79)	(-1.83)
Year 3 After Crash × Crash	-0.0030**	-0.0030**	-0.0079**	-0.0079**	-0.018*	-0.0094	-0.0095
	(-3.86)	(-3.90)	(-2.97)	(-3.06)	(-2.52)	(-1.85)	(-1.92)
New Car		-0.0024**		-0.0026*			0.00073
		(-8.69)		(-2.33)			(0.33)
New Car Missing		0.0057**		0.014			0.079*
		(2.75)		(1.56)			(1.99)
Driver Age		0.0011**		0.0029**			0.0031**
		(8.05)		(5.10)			(2.71)
Driver Age^2		-0.000019**		-0.000041**			-0.000043**
		(-10.18)		(-5.53)			(-2.92)
Male		0.0018**		0.0011			0.0015
		(7.04)		(1.00)			(0.70)
Crash in prior 3 years		0.0062**		0.0074**			0.0050
		(14.91)		(4.47)			(1.59)
Two Prior Crashes		0.012**		0.017**			0.0076
		(11.09)		(3.97)			(1.14)
County	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Current Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N. of cases	891765	891765	73622	73622	4699	19789	19789
Ref. Bankruptcy Probability	0.017	0.017	0.026	0.026	0.028	0.026	0.026
F-Test for Future Periods	0.0012	0.00096	0.064	0.054	0.36	0.19	0.19

Marginal effects; t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$

TABLE XIX
1 Year Sample Panel, At-Fault Drivers

	(1)-All	(2)-All	(3)-EDAdmit	(4)-EDAdmit	(5)-EDAdmit, Uninsured	(6)-High Charge	(7)-High Charge
Quarter 4 Before Crash	-0.000051	-0.000083	-0.0024*	-0.0023*	-0.0091**	-0.0029	-0.0028
	(-0.16)	(-0.27)	(-2.27)	(-2.20)	(-3.50)	(-1.53)	(-1.48)
Quarter 3 Before Crash	0.000016	-0.000024	-0.00064	-0.00056	-0.0041	-0.00057	-0.00049
	(0.05)	(-0.08)	(-0.55)	(-0.48)	(-1.11)	(-0.26)	(-0.22)
Quarter 2 Before Crash	0.00019	0.00017	-0.0024*	-0.0023*	-0.0091**	-0.0038*	-0.0037*
	(0.60)	(0.55)	(-2.25)	(-2.21)	(-3.56)	(-2.14)	(-2.09)
Quarter 1 After Crash	0.00023	0.00016	-0.00094	-0.00096	0.00030	-0.0045**	-0.0044**
	(0.70)	(0.52)	(-0.81)	(-0.84)	(0.06)	(-2.66)	(-2.63)
Quarter 2 After Crash	0.00031	0.00027	0.00057	0.00052	0.0019	-0.00073	-0.00062
	(0.96)	(0.84)	(0.44)	(0.42)	(0.36)	(-0.33)	(-0.28)
Quarter 3 After Crash	0.00044	0.00040	-0.00044	-0.00050	-0.0023	0.00036	0.00033
	(1.34)	(1.25)	(-0.36)	(-0.42)	(-0.56)	(0.15)	(0.14)
Quarter 4 After Crash	0.00074*	0.00072*	0.0023	0.0022	-0.0020	-0.0025	-0.0024
	(2.19)	(2.17)	(1.59)	(1.55)	(-0.48)	(-1.25)	(-1.21)
Crash	0.00045	0.00035	0.0011	0.0010	-0.0016	-0.0028	-0.0027
	(1.46)	(1.18)	(0.92)	(0.89)	(-0.33)	(-1.13)	(-1.10)
Quarter 4 Before Crash × Crash	0.00020	0.00019	0.00028	0.00023	0.025	0.0016	0.0015
	(0.44)	(0.44)	(0.15)	(0.13)	(0.92)	(0.39)	(0.36)
Quarter 3 Before Crash × Crash	-0.000022	0.0000026	0.00029	0.00025	0.0095	0.0024	0.0022
	(-0.05)	(0.01)	(0.17)	(0.15)	(0.75)	(0.57)	(0.55)
Quarter 2 Before Crash × Crash	0.00014	0.00014	0.0033	0.0031	0.030	0.014	0.014
	(0.32)	(0.32)	(1.40)	(1.38)	(0.98)	(1.84)	(1.81)
Quarter 1 After Crash × Crash	-0.000096	-0.000081	0.00026	0.00022	-0.0028	0.012	0.011
	(-0.22)	(-0.19)	(0.15)	(0.13)	(-0.55)	(1.59)	(1.57)
Quarter 2 After Crash × Crash	0.00018	0.00020	-0.0013	-0.0013	-0.0059	-0.0010	-0.0013
	(0.41)	(0.45)	(-0.88)	(-0.92)	(-1.82)	(-0.31)	(-0.40)
Quarter 3 After Crash × Crash	0.00037	0.00038	0.00062	0.00057	0.0061	0.0047	0.0047
	(0.83)	(0.86)	(0.35)	(0.32)	(0.58)	(0.99)	(0.99)
Quarter 4 After Crash × Crash	-0.00019	-0.00018	-0.0022	-0.0023	0.0029	0.0052	0.0049
	(-0.47)	(-0.44)	(-1.78)	(-1.83)	(0.34)	(0.99)	(0.96)
New Car		-0.00084**		-0.00098*			0.000011
		(-7.87)		(-2.23)			(0.01)
New Car Missing		0.0012		0.0032			0.016
		(1.52)		(0.99)			(1.31)
Driver Age		0.00054**		0.0013**			0.0014**
		(8.90)		(5.44)			(2.68)
Driver Age^2		-0.0000080**		-0.000017**			-0.000017**
		(-10.22)		(-5.47)			(-2.69)
Male		0.00073**		0.00088*			0.0014
		(6.94)		(2.00)			(1.62)
Crash in prior 1 years		0.0021**		0.0011			0.0022
		(8.70)		(1.28)			(1.20)
Two Prior Crashes		0.0040**		0.0018			0.0091
		(3.71)		(0.56)			(1.17)
County	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Current Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N. of cases	1846367	1846367	152159	152159	9217	40405	40405
Ref. Bankruptcy Probability	0.0056	0.0056	0.0090	0.0090	0.0098	0.0079	0.0079
F-Test for Future Periods	0.69	0.68	0.29	0.27	0.40	0.11	0.10

Marginal effects; t statistics in parentheses
* $p < 0.05$, ** $p < 0.01$

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