# How Do Driver Assistance Technologies Affect Transportation Safety?*

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**Abstract**

Advanced Driver-Assistance Systems (ADAS), such as Automatic Emergency Braking, Blind Spot Monitoring, and Lane Keeping Assist, are now common in new vehicles. In theory, these new safety features should reduce automobile crashes. However, these technologies may be ineffective in real-world driving conditions or could encourage riskier driving behavior, offsetting some or all of the anticipated safety benefits. Using 2016 to 2020 crash data from 39 U.S. states and national-level fatal crash data, we employ a difference-in-differences approach that compares crashes among vehicles with and without driver assistance systems when exposed to an exogenous change in crash risk: precipitation. Our approach mitigates the risk of selection into vehicles with and without ADAS. We find that ADAS is associated with a 19.1% reduction in crashes. However, we find a 14.6% increase in fatal crashes, suggesting some moral hazard effects among drivers using ADAS-equipped vehicles. A regression discontinuity analysis centered on the annual transition to Daylight Savings Time provides supporting evidence. Notably, our estimates are sensitive to the composition of the non-ADAS comparison group, as older vehicles appear to experience more crashes at times of heightened risk.

***Keywords:*** Advanced Driver-Assistance Systems, Safety, Automobiles, Traffic crashes, Crashes, Fatalities

***JEL:*** I12, L62, L92, R41

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# 1 Introduction

Advanced Driver-Assistance Systems (ADAS), designed to promote safety and reduce car crashes, have become commonplace in recent years.[1] These driver assistance technologies include warning systems such as visual or audible warnings about vehicles located in one's "blind spot," intervention systems such as Automatic Emergency Braking, and driver aids such as Lane Keeping Assist. One might expect these new safety features to improve overall road safety as ADAS-equipped vehicles make up an increasing share of the U.S. vehicle stock. The National Safety Council, however, reports that traffic fatalities increased from 35,332 in 2010 to 46,980 in 2021, a 33% jump.[2]

In this paper, we examine whether ADAS can reduce collisions or help to avoid injuries and deaths. It is important to establish the causal impact of ADAS because the technology adds thousands of dollars to the price of a new vehicle (Goddard et al., 2022). While one might expect ADAS to lead to improved safety, these features may be less effective in real world driving scenarios, relative to their performance in "test track" settings. Alternatively, these technologies might cause drivers to feel safer – consciously or not – and, as a result, drive less carefully (Peltzman, 1975). At the extreme, certain autonomous interventions could lead to an increase in the number and severity of crashes if they take control away from a human driver.

To estimate the impact of ADAS, we use a difference-in-differences approach that measures changes in crash rates involving cars with and without ADAS – defined here as having at least one ADAS feature – when exposed to an exogenous change in crash risk.[3] We cannot simply compare crash rates of vehicles with and without driver assistance technology, as there are likely unobserved confounding factors that would leave any such estimates biased. For example, perhaps more cautious or more experienced drivers self-select into vehicles with

[1] In our crash data, 12% of vehicles have some type of driver assistance technology, with that number increasing consistently since the introduction of ADAS. See Figure 1.

[2] See https://injuryfacts.nsc.org/motor-vehicle/historical-fatality-trends/deaths-and-rates/, last accessed 10/1/2024.

[3] In practice, having one ADAS feature typically implies that a vehicle has several.

advanced safety systems. To avoid this source of confounding, our main estimates use precipitation as a risk shifter, focusing on total crashes and fatal crashes within 20 kilometers of a weather station. Using crash data from 39 state transportation agencies and the Fatality Analysis Reporting System (FARS), our primary outcome of interest is hourly crash counts where at least one vehicle has driver assistance technology relative to hourly crash counts where no vehicles have driver assistance technology. If the technology helps drivers avoid crashes, we should see a smaller increase in crashes involving ADAS-equipped vehicles when there is precipitation.

When using rain as a risk shifter, summary statistics suggest that ADAS is associated with a 7.7 percentage point (22.9%) decline in crashes. In a regression framework, our estimates show a 19.1% reduction in crashes involving at least one vehicle with ADAS when there is rain. On the other hand, summary statistics indicate driver assistance technologies are associated with between a 4.9 and 11.5 percentage point relative increase in instances of fatal crashes. For fatal crashes, our regression-based estimates indicate a 14.6% relative increase in crashes involving ADAS-equipped vehicles compared to the outcome mean. One potential explanation for this pattern is that ADAS helps to eliminate minor crashes but increases the relative severity of major crashes due to driver inattention. Our estimates represent the causal effect of ADAS on crashes if drivers with and without cars that have driver assistance technology respond to changes in driving conditions the same way. Implicitly, this means we assume that in the absence of driver assistance technology, the *effect* of rain on crashes would be the same for drivers of both types of vehicles.

We support a causal interpretation for our findings using a variety of robustness, sensitivity, and heterogeneity exercises. As part of those analyses, we show that the pattern in our estimates is similar when looking at the effect of very small amounts of precipitation ($< 0.2$ millimeters of rain) and when we remove accidents that occur at the rainiest times. This helps to avoid concerns that safer drivers select cars with ADAS *and* drive even more carefully – or not at all – when it is raining, biasing our estimates towards finding fewer

crashes. We also show that our estimates are similar when focusing only on accidents that occur during heavier commuting times (5am to 9am and 3pm to 7pm). We view these trips as less "discretionary" in nature, which further helps to ease concerns that the effect of ADAS on crashes is biased by differential responses to rain among drivers with and without ADAS-equipped vehicles.

Next, we show the effect of ADAS on accidents that involve injuries and property damage only. The pattern of estimates confirms that ADAS has a bigger impact on non-fatal accidents, with a large effect on crashes involving only property damage and a somewhat smaller effect on crashes involving physical injuries. That is, driver assistance technologies lead to a large decrease in property damage only crashes, a slightly smaller decrease in crashes involving injuries, but an apparent increase in fatal crashes, indicating that the effectiveness of ADAS depends on crash severity. Using our FARS data, we also show that non-vehicle occupant deaths are largely unchanged due to the technology, at least when there is precipitation, showing again that ADAS do not significantly reduce the number of serious accidents.

We additionally provide estimates where we limit the underlying sample by vehicle age. There, we see that our main estimates are somewhat inflated because we are comparing crashes among newer vehicles – most of whom are ADAS-equipped – to older vehicles that do not have ADAS. As we compare crashes among vehicles with ADAS (i.e., newer cars) to crashes among increasingly older vehicles, the effect of rain on the difference in crash risk across vehicle type – ADAS-equipped versus non-ADAS – increases. For example, if we limit the sample to crashes involving at least one vehicle that is no more than six years old at the time of the crash, the effect sizes are roughly only half as large as in our main estimates. This pattern suggests that any raw difference in crash risk involving cars with and without ADAS is at least partly related to the fact that ADAS-equipped vehicles are newer. Older vehicles may have an increasing number of mechanical issues, drivers of older vehicles may take less care precisely because the car is older, or drivers of older cars may have different overall driving patterns, particularly in terms of risky behavior. Finally, we examine effects

by type of driver assistance safety feature. These features are often bundled together and, unsurprisingly, we observe limited heterogeneity in our estimates across the various ADAS safety features.

To further support our main findings, we use a regression discontinuity analysis centered on the annual transition to Daylight Savings Time (DST). The change occurs on the second Sunday in March throughout our 2016 to 2020 sample period. Smith (2016) shows that people sleep less, experience more fatal crashes, and that the effect is *caused* by the loss of sleep because there is no similar effect when we "gain" an hour in the fall. Our DST analyses again reveals that shifts in crash risk – likely driven by the loss of sleep for some drivers – affect crash types differently. Specifically, we observe a small increase in fatal crashes, in line with Smith (2016), but the overall number of crashes declines.[4] Consistent with our estimates using rain as a risk shifter, driver assistance technology seems to help drivers in avoiding minor collisions, but the risk of severe crashes (i.e., fatal crashes) increases more for ADAS-equipped vehicles when fatal crash risk is greater.

Our work makes several contributions to the existing literature on road safety. First, we provide robust causal evidence on the effectiveness of driver assistance technology. As we explain in detail in Section 2, earlier work on driver assistance technology does not fully account for selection into vehicle type as a function of drivers' own safety concerns (Cicchino, 2019; Leslie et al., 2021). Because we focus on exogenous changes in crash risk and compare changes in crashes among drivers with and without a car that is ADAS-equipped, we are holding driver skill and safety concerns constant, to the best of our ability. Second, we offer further evidence of risk compensation and moral hazard behavior among drivers when they perceive enhanced safety (Peltzman, 1975; Horswill and Coster, 2002; Cohen and Dehejia, 2004; Winston et al., 2006). Finally, we offer policy relevant estimates of the effects of ADAS. Because we account for selection into vehicles with driver assistance technology, our

[4]This contrasts with Smith's (2016) expectations, where he argues that the effects of DST on fatal crashes are an underestimate of the total societal effects because he expects that the effect on non-fatal crashes would move in the same direction as fatal crashes.

estimates highlight that the net effect of driver technology is significantly smaller than the estimates offered in the existing economics and transportation engineering literature on this issue (Cicchino, 2017; Leslie et al., 2021; Maheshri et al., 2024). Indeed, we show that ADAS may increase the severity of some types of accidents. Given the cost of adding driver technology to cars, our findings offer a more nuanced view of the overall benefits of driver assistance technology.

In summary, our work provides evidence that driver assistance technology significantly reduces the number of car crashes, but can lead to an increase in fatal crashes. In Section 2, we situate our work within the existing literature on the causes and economic consequences of car crashes. In Section 3, we summarize our data and describe our approach to estimation. We present our main findings in Section 4, along with a series of robustness and sensitivity analyses. We offer concluding remarks in Section 5.

# 2 Background and Existing Literature

Improvements in car safety during the last 100 years have been quite slow, relative to the growth in the number of crash-related fatalities per year (National Safety Council, 2024). For many decades, it seems that manufacturers were focused on improving vehicle performance rather than passenger safety. For example, until the 1960s, cars in the United States lacked basic safety features such as seat belts.[5] Early research found that seat belts reduced the risk of death for front-seat occupants by up to 50% (Cameron, 1981; Wild et al., 1985). Peltzman (1975), however, showed that the improved safety for drivers and passengers due to seat belts might have unintended negative effects on those outside the vehicle, including pedestrians and cyclists. Peltzman's analysis suggested that drivers drive more recklessly when they wear a seat belt, increasing the risk of harm to non-vehicle occupants. Ultimately, Peltzman shows that the overall societal safety benefit from seat belts was entirely offset by riskier

[5]See https://magazine.northeast.aaa.com/daily/life/cars-trucks/auto-history/a-seat-belt-history-timeline/, last accessed 10/10/2024.

behavior on the road. A large body of work further supports the idea that drivers engage in riskier behavior when they are insulated from risk (Horswill and Coster, 2002; Cohen and Dehejia, 2004; Winston et al., 2006).

Approaches to regulation were also relatively delayed, with the National Highway Traffic Safety Administration (NHTSA) being founded as late as 1970. The NHTSA is an agency of the U.S. federal government, housed within the Department of Transportation, whose primary goal is to improve transportation safety. At the state level, many states – including Illinois and Wisconsin – did not require a driving test for licensed drivers until well into the 1950s.[6] Further, airbags, despite being invented in the 1950s, only became a standard feature in the 1990s, even though they could have helped to save many lives. For example, Høye (2010) reports that airbags reduce belted driver fatalities by about 22% overall. Although, airbags have little overall impact, and may increase fatality risk in single vehicle accidents, among drivers not wearing a seat belt (Høye, 2010). Since the 1990s anti-lock braking systems (ABS) and electronic stability control (ESC) have improved vehicle handling and crash prevention.[7] For example, Kahane and Dang (2009) report that anti-locking brakes are associated with a 6 percent decline in nonfatal crashes among passenger cars and an 8 percent decline among light trucks. However, Winston et al. (2006) show that safer drivers tend to select into vehicles with improved safety features, such as airbags and anti-lock brakes, clouding any assessment of their impact on road safety.

More recently, advances in driver assistance technologies – such as automatic emergency braking, lane departure warnings, and adaptive cruise control – have begun to shift the focus from crash mitigation – protecting passengers who are involved in a crash – to crash prevention. These developments mark the beginning of a new era in car safety, where autonomous and semi-autonomous systems aim to reduce human error, the leading cause of car crashes.

[6]See https://www.history.com/news/when-was-the-first-u-s-drivers-license-issued, last accessed 10/10/2024.

[7]See https://www.nhtsa.gov/how-vehicle-safety-has-improved-over-decades, last accessed 10/10/2024.

The literature on the effects of driver assistance technology on car crashes highlights a reduction in crash rates associated with the adoption of these technologies (Woodrooffe et al., 2013; Cicchino, 2017; Jeong and Oh, 2017; Sander and Lubbe, 2018; Cicchino, 2019; Leslie et al., 2021). Across such studies, the presence of driver assistance features, such as automatic emergency braking or lane keeping assistance, is associated with lower accident frequencies. However, these studies do not fully address the potential for selection bias or unobserved factors that may influence both the adoption of these technologies and the likelihood of a crash. For instance, drivers who purchase vehicles equipped with advanced safety features may differ systematically from those who do not, either in terms of driving habits, risk tolerance, or other characteristics that are difficult to observe but may contribute to lower crash rates. To illustrate, note that Leslie et al. explain that they compare "system-relevant and system-irrelevant (control) crash counts for equipped and unequipped vehicles." The "system-irrelevant" crashes are "rear struck" crashes meaning getting hit from behind by another vehicle. However, that empirical approach cannot account for selection into ADAS-equipped vehicles, where safer drivers choose to drive ADAS-equipped vehicles while being unable to avoid behind struck from behind by a less careful driver. Further, Cicchino (2017) focuses only on rear struck crashes – among luxury vehicles that had some optional driver assistance features as early as 2000 – and finds that vehicles with Forward Collision Warning and Automatic Emergency Breaking actually experience more rear-struck crashes. The author speculates that this is because, while the technology reduces the likelihood of being the *striking* vehicle, it increases the likelihood of then being hit from behind because of the sudden decrease in the car's velocity. If rear struck crashes are increasing, any analysis that uses being "rear struck" as its comparison group, is biased towards finding that ADAS reduces crashes.

One exception to this literature is an as-yet-unpublished article by Maheshri et al. (2024). These authors use vehicle trim level data, merging crash data from the Texas Department of Public Safety from 2010-2018 with information on ADAS availability. They use Vehicle

Identification Numbers (VINs) to link vehicles to their respective trim levels and safety features, and then compare changes in accidents within a make-model-trim as ADAS features are added. Their approach suggests ADAS are associated with reductions in accidents by nearly 60% and fatal accidents by about two-thirds. The validity of their findings, however, rests on the assumption that drivers are not selecting into these vehicles. Maheshri et al. argue that it is "unlikely that drivers systematically opt for higher trim levels because of ADAS." On the other hand, they also claim only certain drivers appear to be willing to pay for ADAS-equipped vehicles, noting that mandating the technology would not bring sufficient benefits to those who would be forced to pay for a feature they do not value. If willingness-to-pay for ADAS varies across drivers, it seems likely that drivers who sufficiently value ADAS might also systematically differ in their safety preferences and/or on-the-road behavior.

Further, as an empirical matter, Maheshri et al. (2024) compare annual accident rates across trim levels of a new vehicle to considerably older cars of the same make-model-trim. Using the authors own example in Table 1 of their paper, in 2018, the authors are comparing accidents among 2018 Acura MDX trims with ADAS to accident rates among MDX trims from as early as 2000. There are many differences between those cars, other than ADAS, aside from any differences in crash rates associated with driver characteristics. Differences in the driving habits (daily mileage, times and speed that they drive, etc.), of someone who is the owner of a *new* vehicle, relative to the habits of a driver of an 18-year old version of the "same" vehicle are potentially stark.[8] As discussed in the introductory section, our sensitivity analyses, where we restrict our underlying sample by vehicle age, show that comparing crash

[8]Maheshri et al. do include make-model-trim fixed effects, trim-calendar year fixed effects, and make-model calendar year fixed effects. These allow them to capture variation due to differences in accidents across make-model-trims (e.g., Honda CRV SEL versus Toyota RAV4 XSE regardless of year of manufacture), variation in accident rates that occur in a calendar year across trims (e.g., higher trim versus lower trim in 2018), and variation due to differences in make-model-calendar year (e.g., Honda-CRV accidents in 2017, 2018, 2019, etc.). However, the authors are missing a control for manufacture year to capture variation due to vehicle age.

rates involving cars with ADAS only to *newer* cars without ADAS finds significantly smaller differences in crash rates.

As a result, while there is substantial evidence suggesting that driver assistance technologies improve safety, the causal impact of these systems remains an open question, with the need for a variety of innovative research designs that can isolate the true effect of the technology from confounding factors. To contribute to this work, we use precipitation and the annual switch to Daylight Savings Time as sources of variation in crash risk. However, we acknowledge that, because our approach focuses on *changes* in crash risk, we are not estimating the total effect of ADAS on crashes. Instead, our approach examines changes in crashes during times of increased crash risk. Further, our approach cannot determine whether ADAS-equipped vehicles are effective at reducing at-fault accidents for drivers of cars with ADAS. Of course, a large fraction of crashes are single vehicle. However, if crashes involving ADAS-equipped vehicles are largely caused by drivers of non-ADAS vehicles, our findings may underestimate the effectiveness of driver assistance technology. In the next section, we explain our data and our approach to estimation.

# 3 Data and Estimation

## 3.1 Data

We investigate the impact of driver assistance technology on car crashes, using several data sources to construct a comprehensive dataset. Our main crash data comes from state-level records, covering 38 states and the District of Columbia (see Figure 2). Our data includes information on the severity of crashes, which we categorize into whether the most severe outcome was only property damage, an injury, or a fatality. Some states provide crash information in a readily available format, for others we collected the data by contacting state transportation agencies. In the past we could have used the State Data System to access crash data, but that is no longer available to researchers. Although our state-level

crash data spans multiple decades for some states, we focus on crashes from 2016 to 2020, as few vehicles were equipped with driver assistance technology prior to 2016. As shown in Figure 2, we have crash data for 33 states plus D.C. from 2016 through 2020, with four years of data for Utah, Maine, Massachusetts, New Jersey, and Ohio.[9]

A key feature of our crash data is that we have Vehicle Identification Numbers (VINs), enabling us to link each vehicle to the NHTSA Product Information Catalog Vehicle Listing (vPIC) (2023) to extract detailed information about the vehicles safety features, such as whether the vehicle is equipped with features that might reduce crashes, including forward collision warning, automatic emergency breaking, lane keeping assist, etc.[10] VIN numbers offer a wealth of information, including the year of manufacture – which can be derived from the 10th VIN character – as well as the make, model, and other vehicle specifications.[11] The vPIC dataset, provided by the National Highway Traffic Safety Administration (NHTSA), is accessed through an API that allows us to decode VINs and gather detailed information on vehicle specifications. The dataset is based upon manufacturer submissions to the NHTSA, making it a valuable source of vehicle-specific data, although its coverage of driver assistance technologies is incomplete, especially among luxury vehicles that had some of the earliest versions of ADAS features. Our data, however, allows us to reliably distinguish between the majority of vehicles equipped with driver assistance technology and those without, which is central to our study.

To supplement our state crash data, we also use the Fatality Analysis Reporting System (FARS, NHTSA 2022), which provides detailed records of fatal crashes nationwide. While our state-level data includes fatalities, it is not exhaustive across all states, making FARS a useful cross-check for our findings. FARS also offers information on injuries and fatalities

[9]For North Carolina, we have crash data for 2016 through 2020 but cannot geolocate any crashes in 2016 and 2017.

[10]As Cicchino (2017) shows, driver assistance features became available as an option on many luxury class vehicles prior to 2010. However, as is apparent from Figure 1, auto makers either were not required to or did not reliably report the presence of these systems to the NHTSA Product Information Catalog Vehicle Listing (vPIC) until later.

[11]Often, car characteristics are partially available in the crash data.

among non-vehicle occupants, allowing us to examine whether pedestrians and cyclists are at greater risk due to driver inattention facilitated by ADAS.

For our precipitation data, we rely on the National Oceanic and Atmospheric Administrations (NOAA) Integrated Surface Data (ISD) (US National Oceanic and Atmospheric Administration, 2023). This ensures that we accurately capture weather conditions at the time and location of each crash, allowing us to use changes in weather as a source of exogenous variation in crash risk. We only use weather stations that report data 90% of the time and that report trace rain.[12] We also incorporate a dataset of U.S. federal holidays, manually collected from the Office of Personnel Management (OPM) website.[13] This list includes standard holidays, along with notable dates such as New Years Eve, Black Friday, and Christmas Eve. Observed holidays are accounted for when the actual holiday falls on a weekend.

States differ in the geographic detail they provide about each crash's location. For many crashes, the crash report includes the GPS coordinates of the crash, while for others we have the street and cross-street of the nearest intersection, and, in many cases, data on the distance and direction the crash is from the nearest intersection. For those crashes without GPS coordinates, we find the GPS coordinates of the nearest intersection using ArcGIS, and then adjust using data, when available, on the direction and distance the crash is from the intersection. For our analyses, we assign crashes to the nearest weather station within a 20-kilometer radius. We have crash data within 20km of 400 unique weather stations in our state data and 822 weather stations in our FARS data. Our sample includes all crashes (both highway and off-highway) occurring within 20km of a valid weather station.

Using these data sources, we structure our dataset at the technology-weather station-year-month-day-hour level, allowing us to compare crash counts for vehicles with and without driver assistance technology as a function of weather/road conditions. Figure 1 provides sum-

[12]The weather stations report rain in millimeters, and some stations can report when there is rain, but less than a millimeter while others cannot.

[13]https://www.opm.gov/policy-data-oversight/snow-dismissal-procedures/federal-holidays, last accessed 10/7/2024.

mary information on the proportion of crashes in our data that involve an ADAS-equipped vehicle over time. As the figure shows, there were almost zero accidents involving vehicles that we can identify as ADAS-equipped until 2016, which is why we include only data from 2016 onward in our study. In Tables 1 and 2, we present summary statistics, including information about hourly crash counts, the breakdown between crashes and fatal crashes for ADAS-equipped and non-equipped vehicles, and weather/precipitation. We see an average of 0.045 crashes per hour involving a vehicle with ADAS versus 0.324 crashes per hour where no vehicle has ADAS. Given we have over 15 million weather station-hour observations, the mean values imply that we have data on over 700,000 accidents involving a car with ADAS from 2016 to 2020 and more than 5 million crashes among non-ADAS vehicles. Of course, that is only a fraction of all crashes as we do not have crash data from every state and in the states we have not all crashes are within a 20km radius of a weather station. To give a sense of how much data we are missing, the NHTSA reports that the number of police-involved traffic crashes was 6.1 million in 2021.[14]

[14]See https://crashstats.nhtsa.dot.gov/Api/Public/ViewPublication/813560, last accessed 11/8/2024.

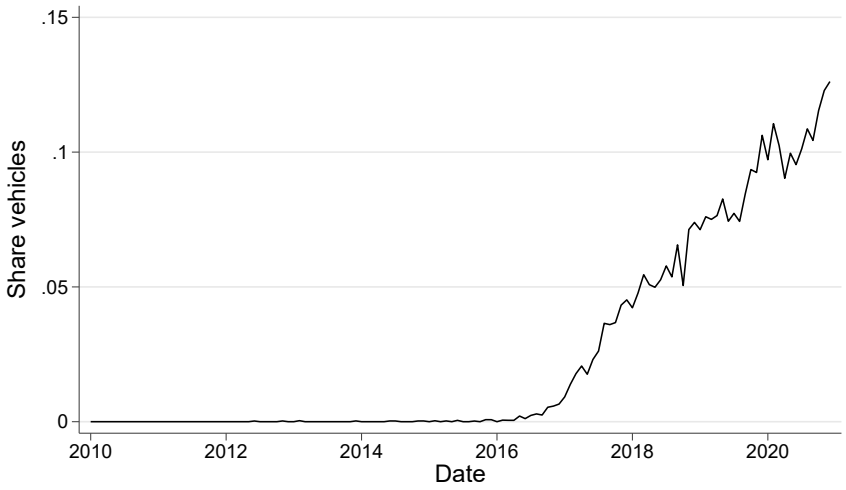



Figure 1: The Rollout of Driver Assistance Technology over Time (Share of Vehicles in Crashes that have ADAS)

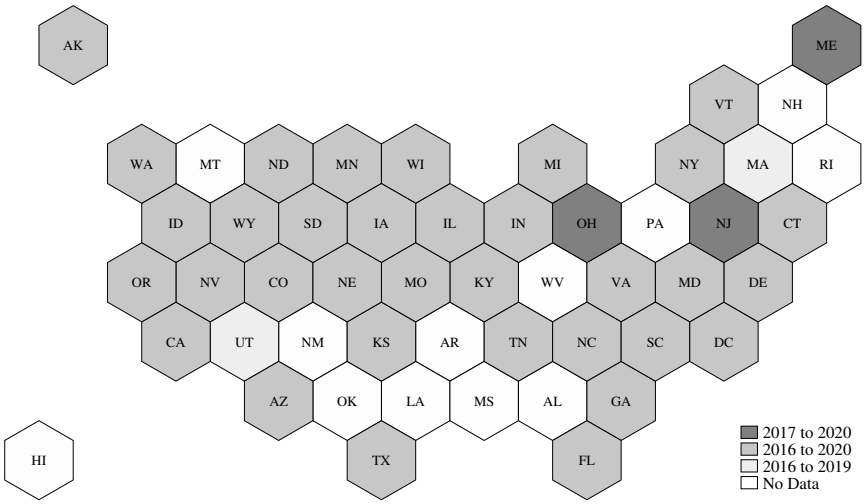



Figure 2: Data Availability by State and Year

Table 1: Summary Statistics by ADAS and Year

| | **State Data** | | **FARS Data** | |
|---|---|---|---|---|
| | Hourly Crashes | Std. Dev. | Hourly Crashes | Std. Dev. |
| All Vehicles | | | | |
| Total Crashes | 0.184614 | (0.684446) | | |
| Fatal Crashes | 0.000617 | (0.024905) | 0.000881 | (0.029748) |
| Injury Crashes | 0.044916 | (0.249823) | | |
| Property Damage Only | 0.129298 | (0.537341) | | |
| Non-vehicle Occupant Fatality | | | 0.000248 | (0.016009) |
| Non-vehicle Occupant Injury | | | 0.000015 | (0.005277) |
| Observations | 31,411,198 | | 71,253,408 | |
| Vehicles with ADAS | | | | |
| Total Crashes | 0.045367 | (0.254830) | | |
| Fatal Crashes | 0.000094 | (0.009705) | 0.000147 | (0.012109) |
| Injury Crashes | 0.010222 | (0.105896) | | |
| Property Damage Only | 0.032700 | (0.210484) | | |
| Observations | 15,705,599 | | 35,626,704 | |
| Vehicles without ADAS | | | | |
| Total Crashes | 0.323862 | (0.912806) | | |
| Fatal Crashes | 0.001141 | (0.033850) | 0.001615 | (0.040276) |
| Injury Crashes | 0.079610 | (0.333469) | | |
| Property Damage Only | 0.225896 | (0.717291) | | |
| Observations | 15,705,599 | | 35,626,704 | |
| Remove "Top 5% of Hourly Rain Observations (>3.6 mm of rain) | | | | |
| Total Crashes | 0.184269 | (0.683540) | | |
| Fatal Crashes | 0.000617 | (0.024902) | 0.000880 | (0.029738) |
| Observations | 31,237,184 | | | |
| Commuting Hours Only (5am to 9am, 3pm to 7pm) | | | | |
| Total Crashes | 0.254607 | (0.866667) | | |
| Fatal Crashes | 0.000599 | (0.024515) | 0.000862 | (0.029412) |
| Observations | 10,463,252 | | 23,737,304 | |
| Weather Conditions | | | | |
| % Hours with Rain | 11.6% | | 13.2% | |
| Average Hourly Precipitation in Millimeters | 0.090505 | (0.774389) | 0.092137 | (0.780801) |
| Total Weather Stations | 400 | | 822 | |

Note: Crash statistics come from State Level and Fatality Analysis Reporting System (FARS) Data. Precipitation data comes from the National Oceanic and Atmospheric Administration. Observations here refers to the number of hours.

Table 2: Summary Statistics by ADAS and Year

| Year | Observations | Crash Type | Mean | Std. Dev. | Min | Max |
|---|---|---|---|---|---|---|
| **Panel A: Vehicles Without ADAS** | | | | | | |
| 2016 | 2,960,432 | Total | 0.319051 | 0.943086 | 0 | 48 |
| | | Fatal | 0.001144 | 0.033910 | 0 | 2 |
| 2017 | 3,277,301 | Total | 0.358245 | 0.991153 | 0 | 41 |
| | | Fatal | 0.001176 | 0.034343 | 0 | 2 |
| 2018 | 3,271,236 | Total | 0.357557 | 0.967166 | 0 | 81 |
| | | Fatal | 0.001161 | 0.034148 | 0 | 2 |
| 2019 | 3,098,499 | Total | 0.330121 | 0.906449 | 0 | 54 |
| | | Fatal | 0.001049 | 0.032415 | 0 | 2 |
| 2020 | 3,098,131 | Total | 0.250052 | 0.718899 | 0 | 35 |
| | | Fatal | 0.001169 | 0.034337 | 0 | 2 |
| **Panel B: Vehicles With ADAS** | | | | | | |
| 2016 | 2,960,432 | Total | 0.002436 | 0.050629 | 0 | 4 |
| | | Fatal | 0.000005 | 0.002175 | 0 | 1 |
| 2017 | 3,277,301 | Total | 0.023538 | 0.169523 | 0 | 9 |
| | | Fatal | 0.000048 | 0.006921 | 0 | 1 |
| 2018 | 3,271,236 | Total | 0.052640 | 0.267187 | 0 | 12 |
| | | Fatal | 0.000091 | 0.009544 | 0 | 1 |
| 2019 | 3,098,499 | Total | 0.075075 | 0.335872 | 0 | 18 |
| | | Fatal | 0.000135 | 0.011628 | 0 | 2 |
| 2020 | 3,098,131 | Total | 0.072061 | 0.322780 | 0 | 16 |
| | | Fatal | 0.000191 | 0.013810 | 0 | 1 |

Note: Crash statistics come from State Level data. Data are at the weather station-year-month-day-hour level.

## 3.2 Estimation

To estimate the effect of driver assistance technology on car crashes, we first use a differences-in-differences approach, exploiting variation in risk across time and place caused by precipitation. To complement those estimates, we follow Smith (2016) and examine changes in crash risk due to the switch to Daylight Savings Time each March. We next explain our approach to estimation in each setting.

### 3.2.1 Using Rain as a Risk Shifter

Differences in crash risk for vehicles with and without driver assistance technologies could be related to a number of confounding factors. We therefore analyze differences in crash counts during periods when crash risk is plausibly higher for exogenous reasons. We focus on the effect of precipitation because precipitation (i.e., rain) makes driving conditions more hazardous (Eisenberg, 2004; Jung et al., 2014). We exploit this change to evaluate whether vehicles with driver assistance technologies are involved in fewer crashes when precipitation is present. Our estimating equation is as follows

$$\text{Crash}_{i,w,m,d,h} = \beta_1 \text{Rain}_{w,m,d,h} \cdot \text{Tech}_i + \beta_2 \text{Rain}_{w,m,d,h} + \beta_3 \text{Tech}_i + \gamma_{i,w,m,d,h} + \epsilon_{i,w,m,d,h},$$

where $\text{Crash}_{i,w,m,d,h}$ is the number of crashes where at least one vehicle has driver assistance technology (type $i = 1$) and crashes where no vehicles have driver assistance technology (type $i = 0$) occurring within 20km of weather station $w$ during hour $h$ of day $d$ in year-month $m$. $\text{Tech}_i$ is an indicator variable for crashes involving vehicles with driver assistance technology, and $\text{Rain}_{w,m,d,h}$ is an indicator if weather station $w$ indicates there was precipitation during hour $h$ of day $d$ in year-month $m$. The primary coefficient of interest is $\beta_1$, and is predicted to be negative if driver assistance technologies improve driver safety. To control for unobservable location-specific characteristics, we include tech by weather station by year-month by day of week by hour fixed effects, $\gamma_{i,w,m,d,h}$, where "year-month" is one term. Our fixed effects

ensure that our treatment effect estimates rely only on within-month across day variation in crashes. We cluster standard errors by weather station-year-month.

While we show that our findings are robust to a variety of estimators, our main estimates rely on OLS when using an inverse hyperbolic sine (IHS) transformation for our outcome variables to help account for the large fraction of zeroes in our hourly crash counts. The IHS transformation of a variable $y$ leads to the following generic OLS regression equation:

$$\text{IHS}(y) = \ln\left(y + \sqrt{y^2 + 1}\right) = \alpha + \beta x + \epsilon$$

The coefficients in an IHS regression framework can be interpreted similarly to log-transformed coefficients, but with some key differences. Namely, for smaller values of $y$, the IHS behaves linearly, and changes in the independent variables translate into approximately linear changes in the outcome, where the interpretation of the coefficient $\beta$ is that a one-unit increase in $x$ is associated with a $\beta$-unit change in the outcome $y$. For larger values of $y$, the IHS behaves more like a logarithmic transformation. The coefficient $\beta$ can then be interpreted as a semi-elasticity where a one unit increase in $x$ leads to an approximate $\beta$-percent change in the outcome $y$. To illustrate, consider the inverse hyperbolic sine transformation, defined as: $\text{IHS}(y) = \sinh^{-1}(y) = \ln(y + \sqrt{y^2 + 1})$. If $y$ is small, $\sinh^{-1}(y) \approx ln(y + 1) \approx y$, so a coefficient $\beta$ on $\sinh^{-1}(y)$ approximates the linear change: $\frac{\partial y}{\partial x} \approx \beta$. For large values of $y$, $\sinh^{-1}(y) \approx \ln(2|y|)$, thus $\beta$ represents a percentage change in $y$ for a unit change in $x$. For example, if $\hat{\beta} = 0.05$, then for small $y$ (i.e., close to zero), a one-unit increase in $x$ increases $y$ by approximately 0.05 units. For large $y$ (say, when $y = 100$), a one-unit increase in $x$ increases $y$ by approximately 5%.

### 3.2.2 Using Daylight Savings Time as a Risk Shifter

Prior research finds an increase in fatal crash risk during the week following the time change due to Daylight Savings Time each spring (Smith, 2016). Smith provides a rich and detailed

history of DST and the arguments for and against the annual change. To summarize, the idea is that, because the clocks jump forward one hour, people sleep less because of the change and face a greater risk of a serious crash. We can therefore use the change to DST as a source of variation to further examine whether cars with driver assistance technologies are relatively less likely to be involved in a crash during the week after the time change. We again focus on crashes per hour within 20km of weather stations. The estimating equation is as follows:

$$\text{Crash}_{w,d,h} = \beta_1 \text{Post}_d \times \text{Days}_d + \beta_2 \text{Post}_d + \beta_3 \text{Days}_d + \epsilon_{w,d,h}.$$

where $\text{Post}_d$ is an indicator variable equal to one if day $d$ falls during the week immediately following the switch away from daylight savings, and $\text{Days}_d$ is a running count variable that captures the number of days since the start of each event analysis. We estimate this regression discontinuity specification separately for accidents involving cars with and without driver assistance technology, hence the lack of an $i$ subscript. The primary coefficient of interest is $\beta_1$, and is predicted to be positive if the advent of DST increases the number of crashes. However, the increase in crashes will be smaller for ADAS-equipped vehicles if driver assistance technologies improve driver safety.

When estimating the regression discontinuity, $\text{Crash}_{w,d,h}$ – following Smith (2016) – is the residual from a regression of the (IHS-transformed) number of crashes involving vehicles of type $i$ (i.e., with or without assistance technologies), occurring within 20km of weather station $w$ on day $d$ in year-month $m$. To estimate that residual we use the following equation:

$$\text{Crash}_{i,w,m,d,h} = \kappa_1 \text{Rain}_{w,m,d,h} + \kappa_2 \text{Tech}_i + \gamma_{i,w,m,d,h} + \varepsilon_{i,w,m,d,h}.$$

When residualizing our outcome variable, to control for unobservable weather station characteristics, we also include weather station-year-month-day of week-hour fixed effects.

Again, we cluster standard errors by weather station-year-month. The next section provides our main findings.

# 4 Findings

## 4.1 Univariate Analyses

We present univariate analyses, based upon summary statistics, in Table 3. In Panel (A) we focus on all crashes, including fatal crashes. As a function of precipitation, we calculate the mean number of crashes per hour for two types of car crashes: those involving no cars with driver assistance technology and those crashes with at least one car that has driver assistance technology. We then calculate the percent change in the number of crashes when it is raining versus when it is not raining. Crashes increase substantially when it rains. However, we see a smaller percent increase for accidents involving cars with driver assistance technology. The "difference-in-differences" estimate, comprising the percentage point difference in the percent increase in crashes across crash types, indicates that driver assistance technology leads to a relative decline in crashes of about 7.7 percentage points or approximately 22.9% (7.7/33.62). The difference is statistically significant at the 1% level based on a bootstrap analysis.

In Panel (B) we provide the same calculations for fatal crashes. We see that fatal crashes decline when there is precipitation. This is a somewhat unexpected finding, perhaps driven by there being fewer serious crashes when it is raining. For example, people may reduce speed in response to adverse road conditions, or there may be more traffic "jams." In such cases, there may be more "fender bender" type crashes – partly explaining our calculations in Panel (A) – but fewer fatal crashes. However, the decline in fatal crashes occurs almost entirely in crashes where no vehicle has driver assistance technology, with 12.56% fewer fatal crashes, compared to only 1.08% fewer fatal crashes where one vehicle has driver assistance technology. If ADAS had no effect on driver behavior or crash risk, we would expect to also see a 12.56% decline in fatal crashes where at least one vehicle had ADAS. As a result, the difference-in-

differences estimate suggests that fatal crashes are greater by 11.48 percentage points due to the technology. Again, a bootstrap approach suggests the difference is statistically significant at the 1% level.

One explanation for the relative increase in fatal crashes would be that – for some reason – the technology itself may be less effective at avoiding serious crashes in rainy conditions, while still being able to help drivers avoid more minor collisions. Alternatively, it could be the case that driver assistance technology induces risk compensation behavior, where drivers of cars with ADAS do not take as much care as drivers without ADAS, regardless of weather conditions. In such a case, the technology may help avoid minor crashes (e.g., "fender benders"), but is then unable to help drivers when the driving situation is more serious, regardless of weather conditions. While we cannot hope to completely separate these two explanations, risk compensation behavior would be consistent with the effects of historical innovations that improved driver safety (Peltzman, 1975).

In Panel (C), we consider fatal crash data from the Fatality Analysis Reporting System (FARS). Our sample size grows as FARS data is at the national level and we can geolocate relatively more crashes. We see a similar pattern with fewer fatal crashes when there is rain, but a smaller decline in crashes involving ADAS-equipped vehicles. The data implies that fatal crashes are 4.9 percentage points higher for crashes where at least one vehicle has ADAS. This is a smaller change than in our state level data and is not statistically significantly different from zero, when we use a bootstrap approach. The direction of the effect, however, supports the idea that ADAS helps to avoid minor crashes but driver behavior may be leading to more serious crashes. Note that our multivariate regression-based estimates do show a statistically significant effect when using FARS data. We present those estimates in the next subsection.

Table 3: Univariate Estimates

| | No ADAS | | | ADAS | | |
|---|---|---|---|---|---|---|
| | (1) No Rain | (2) Rain | (3) Difference | (4) No Rain | (5) Rain | (6) Difference |
| Panel A: Any Crashes - State Data | | | | | | |
| # of Crashes | 0.311645 | 0.416427 | 33.62% | 0.044035 | 0.055448 | 25.92% |
| Manual Difference-in-Differences | | | | | | -7.7 p.p.*** |
| Observations | 13,880,308 | 1,825,291 | | 13,880,308 | 1,825,291 | |
| Panel B: Fatal Crashes - State Data | | | | | | |
| # of Crashes | 0.001156 | 0.001027 | -12.56% | 0.000094 | 0.000093 | -1.08% |
| Manual Difference-in-Differences | | | | | | 11.48 p.p.*** |
| Observations | 13,880,308 | 1,825,291 | | 13,880,308 | 1,825,291 | |
| Panel C: Fatal Crashes - FARS Data | | | | | | |
| # of Crashes | 0.001647 | 0.001378 | -16.33% | 0.000148 | 0.000131 | -11.49% |
| Manual Difference-in-Differences | | | | | | 4.9 p.p. |
| Observations | 31,487,029 | 4,144,928 | | 31,487,029 | 4,144,928 | |

Note: Panels A and B use our state crash data. Panel C uses Fatality Analysis Reporting System (FARS) data. Number of crashes is at the tech-weather station-year-month-day-hour level. Each crash count value represents the mean number of crashes for the indicated group. The FARS data has more observations as it is at the national level. *** p<0.01, ** p<0.05, * p<0.1.

## 4.2 Multivariate Analyses

We present regression-based estimates in Table 4. In Panel (A), our outcome is the count of crashes at the tech-weather station-year-month-day-hour level. The specifications, except where indicated, include fixed effects for tech-weather station-year-month-day of week-hour (T-W-Y-M-D-H) and holiday controls. As a reminder, we cluster our standard errors at the weather station-year-month level.

The estimates in column (1) use OLS with an inverse hyperbolic sine (IHS) transformation of the dependent variable. As we describe in Section 3, for large values of the transformed variable, we can interpret the coefficients similarly to the coefficients from a log transformation (i.e., a one unit change in the independent variable leads to a $\hat{\beta}$ percent change in the outcome variable). For smaller values of the outcome variable, as we have in our setting, we should interpret the coefficients in linear terms. To calculate percent changes, we must compare the difference-in-differences estimate from the IHS regression, the coefficient on the Rain $\times$ Technology interaction term, to the mean of the outcome variable. Therefore, the 0.03535 coefficient implies a 19.15% decline in total crashes relative to the mean number of crashes, which is 0.1846 per hour (see Table 1).[15] The effect size is similar to our univariate estimates in Table 3.

Note again that our data is structured at the tech-weather station-year-month-day-hour level. Our fixed effects therefore ensure that our estimates rely on differences in car crashes related to precipitation in a given hour of the day, on the same day of the week, within the same 20 kilometer radius of a weather station, in a year-month. To illuminate, consider crashes occurring between 4pm and 5pm on Tuesdays in May of 2017 within 20 kilometers of a particular weather station somewhere in the U.S. We learn something about the effect of ADAS only if there is variation in the number of crashes involving ADAS-equipped cars and non-ADAS cars and variation in rainfall among the ten crash counts – five for crashes

[15]The mean number of crashes in total in an hour is 0.3692, but that is split between hourly crashes for vehicles with and without ADAS.

involving vehicles with ADAS and five for those without – for that weather station between 4pm and 5pm on each of the Tuesdays in May of 2017.[16]

In our state crash data we find a similar pattern of estimates across a variety of other approaches to estimating the impact on the total number of crashes, including using OLS to estimate specifications where the dependent variable is $log(Crashes + 1)$, an indicator for crashes $I[\text{Crash} \geq 1]$, and a specification where we normalize the number of crashes to represent deviations from the mean ("Norm"). We see, however, significant variation in the coefficients, with the log transformation and the indicator approach suggesting much smaller effects on crashes. This is somewhat unsurprising given the number of zeroes in our data, ensuring that a $log(y + 1)$ approach tends to be biased towards zero. Further, using an indicator approach throws out much of the variation in our data (e.g., some weather station-hours in our data have as many as 18 crashes involving vehicles with ADAS and up to 81 crashes among vehicles without ADAS). In the final column in Table 4, we provide estimates from a Poisson approach. The Poisson Maximum Likelihood estimator cannot accommodate the same interacted fixed effects structure as our OLS approach, meaning that the estimates are not directly comparable to our main findings, but we still see a decline of close to 6% in the number of crashes involving cars with ADAS when it rains relative to cars without ADAS.[17] To elaborate briefly, the Pseudo Poisson Maximum Likelihood procedure does not converge when using the same large number of interacted fixed effect terms as in our OLS estimates. In the OLS estimates, we have weather station by year-month by day of the week by hour fixed effects for 400 weather stations for 5 years, by vehicle type. This implies $400\ stations \times 5\ years \times 12\ months \times 7\ days \times\ 24\ hours\ \times 2\ vehicle\ types = 8,064,000$ fixed effects. Using the simpler fixed effects structure, where we do not interact the terms,

[16] May 2017 contained five Tuesdays, as May 2 and May 30 were both Tuesdays.

[17] In a Poisson regression framework, the coefficients represent the logarithm of the expected count of the dependent variable, $exp^{-0.0594} \approx 0.9423$. Therefore, a coefficient of $-0.0594$ suggests that a one-unit increase in the independent variable is associated with a 5.77% decrease (1-.9423) in the expected count of the dependent variable, holding other variables constant. This is because $1 - 0.9423 = 0.0577$, or 5.77%.

means that the estimates from a Poisson Maximum Likelihood approach no longer rely *only* on within-month across day variation in crashes.

In Panel (B), as in Table 3, we focus on fatal crashes in our state data. Again, column (1) provides estimates using IHS-transformed outcomes and we see fewer fatal crashes when there is precipitation but the effect of driver assistance technology is to increase the number of crashes relative to vehicles without the technology. Relative to the outcome mean of 0.000617 fatal crashes per hour, the 0.00009 coefficient implies a 14.6% *increase* in fatal crashes. Again, the effect is caused by a decline in fatal crashes among vehicles without ADAS when there is precipitation but no similar decline for crashes where at least one vehicle is ADAS-equipped. The pattern across different estimators is very similar to Panel (A), but the OLS normalized outcome variable and the Poisson estimates lack statistical significance.

In Panel (C), we repeat the analysis of Panel (B), but using FARS data. We see a very similar pattern as in Panel (B), with fewer fatal crashes when there is rain, ADAS leading to a relative increase in fatal crashes, and the OLS normalized outcome variable and the Poisson estimates being of the same sign but again lacking statistical significance.

Because the pattern of estimates is similar across estimators, the logarithm transformation and indicator approaches bias our findings downwards, and the Poisson approach cannot accommodate our preferred fixed effects structure, we use OLS with IHS-transformed outcomes for all subsequent analyses. The next subsection examines some key threats to identification and offers evidence to suggest that our findings are not driven by differential responses to rain among those with ADAS-equipped vehicles and are similar when we focus on commuting times, where trips are less discretionary.

Table 4: Main Estimates

| | (1) IHS | (2) Log(Crashes+1) | (3) $I$[Crash$\geq$ 1] | (4) Norm | (5) PPMLE |
|---|---|---|---|---|---|
| Panel A: Any Crashes - State Data | | | | | |
| Rain × Tech | -0.03535*** | -0.02739*** | -0.02005*** | -0.11933*** | -0.05938*** |
| | (0.00059) | (0.00046) | (0.00039) | (0.00590) | (0.00879) |
| Rain | 0.03909*** | 0.03029*** | 0.02326*** | 0.23425*** | 0.19960*** |
| | (0.00064) | (0.00049) | (0.00039) | (0.00449) | (0.00315) |
| Tech | | | | | -1.9569*** |
| | | | | | (0.0112) |
| Observations | 31,404,548 | 31,404,548 | 31,404,548 | 31,404,548 | 31,324,952 |
| Panel B: Fatal Crashes - State Data | | | | | |
| Rain × Tech | 0.00009*** | 0.00007*** | 0.00010*** | 0.09170 | 0.10726 |
| | (0.00003) | (0.00002) | (0.00003) | (0.09836) | (0.08573) |
| Rain | -0.00009*** | -0.00007*** | -0.00010*** | -0.09171*** | -0.09623*** |
| | (0.00003) | (0.00002) | (0.00003) | (0.02600) | (0.02453) |
| Tech | | | | | -2.50729*** |
| | | | | | (0.03064) |
| Observations | 31,404,548 | 31,404,548 | 31,404,548 | 31,404,548 | 29,419,032 |
| Panel C: Fatal Crashes - FARS Data | | | | | |
| Rain × Tech | 0.00011*** | 0.00009*** | 0.00012*** | 0.05703 | 0.05506 |
| | (0.00002) | (0.00002) | (0.00002) | (0.05049) | (0.04734) |
| Rain | -0.00011*** | -0.00009*** | -0.00013*** | -0.07828*** | -0.10504*** |
| | (0.00002) | (0.00002) | (0.00002) | (0.01435) | (0.01409) |
| Tech | | | | | -2.40609*** |
| | | | | | (0.01647) |
| Observations | 71,253,408 | 71,253,408 | 71,253,408 | 71,253,408 | 70,661,114 |
| T-W-Y-M-D-H Fixed Effects | Yes | Yes | Yes | Yes | No |
| Holiday Fixed Effects | Yes | Yes | Yes | Yes | Yes |
| Weather Station Fixed Effects | No | No | No | No | Yes |
| Year Fixed Effects | No | No | No | No | Yes |
| Month Fixed Effects | No | No | No | No | Yes |
| Day of Week Fixed Effects | No | No | No | No | Yes |
| Hour Fixed Effects | No | No | No | No | Yes |

Note: Panels A and B use our state crash data. Panel C uses Fatality Analysis Reporting System (FARS) data. T-W-Y-M-D-H refers to tech-weather station-year-month-day of week-hour. *** p<0.01, ** p<0.05, * p<0.1. PPMLE refers to Poisson Maximum Likelihoood.

## 4.3 Robustness

In this section, we first examine whether our estimates are affected by drivers with ADAS-equipped vehicles reacting differently – in terms of the decision to drive or when to drive – to rain, which is a primary threat to identification. In Table 5, we examine how our estimates vary as a function of the amount of precipitation (i.e., trace rain versus heavy rain, where trace rain refers to rainfall of $< 0.2$ millimeters in an hour), when we exclude hours with very heavy rain (we remove hours in the top 5% of precipitation, which is anything more than 3.6 millimeters of rain), and at heavy commuting times. We use OLS with IHS-transformed outcomes for these analyses. The goal of these exercises is to help reduce concerns that our estimates are driven by different reactions to various amounts of rainfall among drivers with and without ADAS-equipped vehicles. The specifications include fixed effects for tech-weather station-year-month-day of week-hour (T-W-Y-M-D-H) and holiday controls.

In Panel (A) of Table 5, we focus on all crashes in our state data. The estimates suggest that even trace rain leads to a large increase in the number of crashes and that ADAS has a similar safety impact regardless of the amount of rain. It seems feasible that how drivers of ADAS-equipped vehicles react to rain (e.g., by driving more safely or not at all) could be driving our estimates. However, it would be unexpected if they had similarly strong reactions to almost undetectable amounts of rain. Similarly, if differential reactions to the amount of rain were driving our findings or if ADAS was particularly ineffective when it rains, we might expect to see smaller impacts when we remove the rainiest hours in our dataset.[18] Instead, in column (2) of Table 5, we see estimates that are almost identical to our main effects.

In column (3) of Panel (A) in Table 5 we show that the effects are similar when we look only at commuting hours, which we define as 5am to 9am and 3pm to 7pm. We consider trips during this time as less discretionary, therefore reducing the potential for bias relating to drivers with ADAS-equipped cars choosing not to drive due to rain.

[18]Some test condition research shows that ADAS is less effective in moderate to heavy rain. See https://newsroom.aaa.com/2021/10/rained-out-vehicle-safety-systems-struggle-to-see-in-bad-weather/, last accessed 11/26/2024.

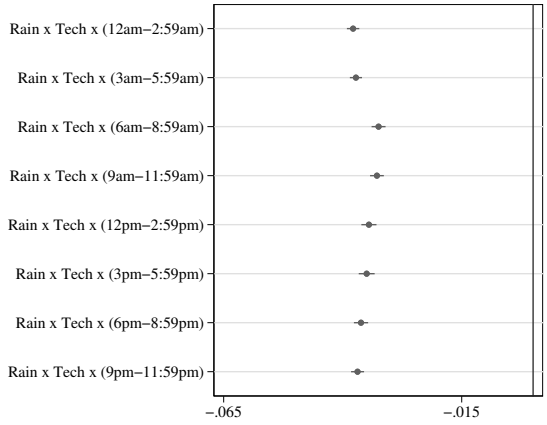
In Panels (B) and (C), we examine fatal crashes in our state and FARS data, respectively. Again, we see that fatal crashes are less common among cars without ADAS when it rains, but crashes involving ADAS-equipped vehicles do not experience a decline in fatal crashes when weather conditions deteriorate.

In Figure 3, we present the effect of rain on crashes involving cars with ADAS for a variety of times during the day. We see similar effects of rain on crashes and fatal crashes involving cars with ADAS regardless of the time of day across in both our state and FARS data. The consistent pattern across times of day again suggests that our estimates are not caused by drivers who choose ADAS-equipped vehicles altering when and where they drive due to changes in road and weather conditions.

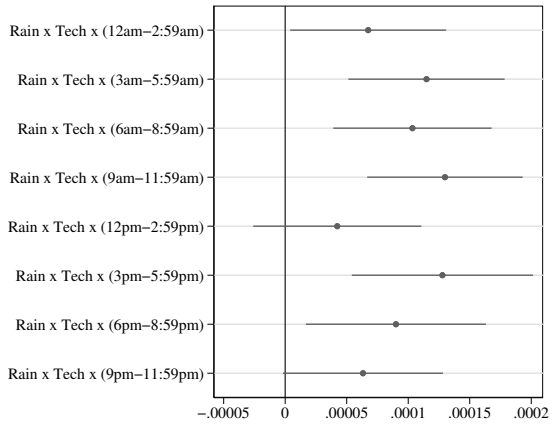
Table 5: Robustness Analyses (OLS, using IHS-Transformed Outcomes)

| | (1) Any Crash (IHS) | (2) Eliminate Very Heavy Rain | (3) Commuting Hours |
|---|---|---|---|
| Panel A: Any Crashes - State Data | | | |
| Trace Rain × Tech | -0.02968*** | | |
| | (0.00068) | | |
| Trace Rain | 0.03252*** | | |
| | (0.00070) | | |
| Heavy Rain × Tech | -0.04175*** | | |
| | (0.00078) | | |
| Heavy Rain | 0.04651*** | | |
| | (0.00082) | | |
| Rain × Tech | | -0.03518*** | -0.03924*** |
| | | (0.00060) | (0.00089) |
| Rain | | 0.03898*** | 0.04384*** |
| | | (0.00064) | (0.00091) |
| Observations | 31,404,548 | 31,237,184 | 10,463,252 |
| Panel B: Fatal Crashes - State Data | | | |
| Trace Rain × Tech | 0.00006* | | |
| | (0.00004) | | |
| Trace Rain | -0.00006* | | |
| | (0.00003) | | |
| Heavy Rain × Tech | 0.00013*** | | |
| | (0.00004) | | |
| Heavy Rain | -0.00013*** | | |
| | (0.00004) | | |
| Rain × Tech | | 0.00008*** | 0.00011** |
| | | (0.00003) | (0.00005) |
| Rain | | -0.00008*** | -0.00010** |
| | | (0.00003) | (0.00005) |
| Observations | 31,404,548 | 31,237,184 | 10,463,252 |
| Panel C: Fatal Crashes - FARS Data | | | |
| Trace Rain × Tech | 0.00009*** | | |
| | (0.00003) | | |
| Trace Rain | -0.00010*** | | |
| | (0.00003) | | |
| Heavy Rain × Tech | 0.00013*** | | |
| | (0.00003) | | |
| Heavy Rain | -0.00013*** | | |
| | (0.00003) | | |
| Rain × Tech | | 0.00011*** | 0.00013*** |
| | | (0.00002) | (0.00004) |
| Rain | | -0.00011*** | -0.00014*** |
| | | (0.00002) | (0.00003) |
| Observations | 71,253,408 | 71,252,590 | 23,737,304 |
| T-W-Y-M-D-H Fixed Effects | Yes | Yes | Yes |
| Holiday Fixed Effects | Yes | Yes | Yes |

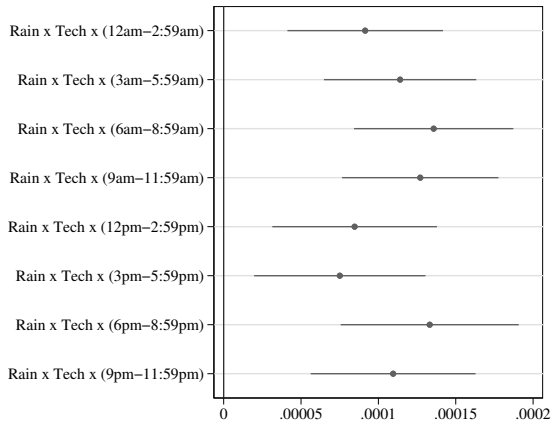
Note: Estimates based on our state and FARS (Fatality Analysis Reporting System) data as indicated. Trace rain refers to hours with rainfall of under 0.2 mm, while heavy rain refers to an amount of rain greater than 0.2 mm. All specifications use OLS with an IHS-transformed dependent variable. T-W-Y-M-D-H refers to tech-weather station-year-month-day of week-hour. *** p<0.01, ** p<0.05, * p<0.1.




(a) State Crashes by Time of Day




(b) State Fatal Crashes by Time of Day




(c) FARS Crashes by Time of Day

Figure 3: Comparison of Crashes by Time of Day

Note: Estimates based on our State and FARS (Fatality Analysis Reporting System) data as indicated. All specifications use OLS with an IHS-transformed dependent variable. Bars represent 95% confidence intervals.

A further threat to identification is that cars with ADAS involved in a crash tend to be newer than the average vehicle on the road, as the technology is only available in newer vehicles. That is, we are comparing crashes among drivers of newer vehicles with ADAS to those among drivers of older vehicles without the technology. Our identifying assumption, as a reminder, is that the effect of rain is similar across drivers who have ADAS-equipped vehicles and those who do not. Our analyses in Table 5 and Figure 3 offer some support for that assumption.

However, our main estimates implicitly consider all vehicles without ADAS as a single comparison group. It is likely that the age of a vehicle plays an important role in crash risk when rain occurs, either because of the mechanical condition of the car or the driving behavior at times of higher risk of drivers of older vehicles. For that reason, in Table 6 we present estimates for crashes involving at least one vehicle no more than three years old, six years old, and nine years old. There, we see that the treatment effect often varies by the composition of the comparison group. As we add crashes involving older vehicles to our data set, the ADAS treatment effect becomes larger in our state data although the estimates using FARS data show a more stable percent effect. The pattern in our state data indicates that some of the effect of ADAS is related to the behavior of drivers or the mechanical condition of vehicles without ADAS, further limiting the potential road safety benefits related to ADAS. The estimates in Table 6 further illustrate our concerns with the Maheshri et al. (2024) approach to studying this issue and highlights that any analysis that compares crashes among vehicles with ADAS to crashes among vehicles without ADAS requires relatively strong identifying assumptions. Note that our sample size remains similar across specifications in Table 6 because our observations are the count of crashes in a specific hour involving vehicles with and without ADAS.[19]

[19]It is not exactly the same due to dropping singletons – there are some weather-station hours where we have only a single observation for a particular day of the week-hour in a calendar month, due to missing weather station information. Our fixed effects structure excludes those from the analysis.

As an appendix item, we provide companion figures that plot the Rain $\times$ Tech interaction term for crashes involving at least one vehicle $t$ years old where $t = 1, ..., 10$. The effect size grows linearly, again indicating that the effect of rain on crashes among drivers in the comparison group (non-ADAS vehicles) varies by the age of the vehicle they are driving.

Table 6: Estimates By Vintage (OLS, using IHS-Transformed Outcomes)

| *Age of at least one vehicle in crash* | (1) ≤ 3 years | (2) ≤ 6 years | (3) ≤ 9 years |
|---|---|---|---|
| Panel A: Any Crashes - State Data | | | |
| Rain × Tech | -0.005201*** | -0.012460*** | -0.018401*** |
| | (0.000322) | (0.000396) | (0.000458) |
| Rain | 0.009207*** | 0.016440*** | 0.022367*** |
| | (0.000302) | (0.000413) | (0.000486) |
| Sample Mean | 0.072056 | 0.112810 | 0.139610 |
| Implied % Change | -7.22% | -11.05% | -13.18% |
| Observations | 28,749,214 | 29,008,112 | 29,050,964 |
| Panel B: Fatal Crashes - State Data | | | |
| Rain × Tech | 0.000008 | 0.000020 | 0.000049** |
| | (0.000015) | (0.000019) | (0.000021) |
| Rain | -0.000007 | -0.000020 | -0.000049** |
| | (0.000013) | (0.000017) | (0.000020) |
| Sample Mean | 0.000175 | 0.000279 | 0.000362 |
| Implied % Change | 4.58% | 7.17% | 13.52% |
| Observations | 28,749,214 | 29,008,112 | 29,050,964 |
| Panel C: Fatal Crashes - FARS Data | | | |
| Rain × Tech | 0.000041*** | 0.000051*** | 0.000075*** |
| | (0.000012) | (0.000015) | (0.000016) |
| Rain | -0.000033*** | -0.000044*** | -0.000067*** |
| | (0.000010) | (0.000013) | (0.000015) |
| Sample Mean | 0.000253 | 0.000390 | 0.000495 |
| Implied % Change | 16.22% | 13.06% | 15.15% |
| Observations | 67,968,148 | 68,923,888 | 69,700,782 |
| T-W-Y-M-D-H Fixed Effects | Yes | Yes | Yes |
| Holiday Fixed Effects | Yes | Yes | Yes |

Note: Estimates based on our state and FARS (Fatality Analysis Reporting System) data as indicated. All specifications use OLS with an IHS-transformed dependent variable. T-W-Y-M-D-H refers to tech-weather station-year-month-day of week-hour. *** p<0.01, ** p<0.05, * p<0.1.

## 4.4 Heterogeneity

In this section, we examine how our estimates vary across different outcomes, including injuries, property damage, and outcomes for non-vehicle occupants (NVOs). We also examine how our estimates vary by the type of assistance technology a car is equipped with.

In Table 7, Panel (A) reports the effect of rain and ADAS on injuries and property damage in our state data. The specifications include fixed effects for tech-weather station-year-month-day of week-hour (T-W-Y-M-D-H) and holiday controls. Column (1) shows a statistically significant reduction in accidents leading to injuries where at least one car had ADAS when there is rain ($\hat{\beta} = -0.00804$, $p < 0.01$, 17.9% relative to the outcome mean). Column (2) reports estimates for crashes that feature only Property Damage, again showing a statistically significant reduction in property damage from the interaction of rain and technology ($\hat{\beta} = -0.03066$, $p < 0.01$, 23.7% relative to the outcome mean). The estimates again highlight how our main findings are driven by changes in minor crashes, with ADAS having a smaller or even an opposite effect on more serious crashes.

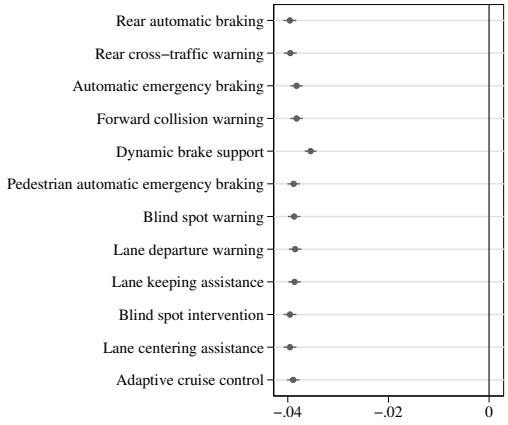
In Panel (B), we examine non-vehicle occupant deaths and injuries using FARS Data. The effect sizes are quite small and statistically insignificantly different from zero, highlighting that these are rare events, especially when further slicing the data by weather and ADAS. It is also possible that the estimates are affected by there being fewer non-vehicle occupants – pedestrians, cyclists, etc. – on the road when it is raining.

In Figure 4, we examine how outcomes vary by the type of ADAS a vehicle has. However, we see no major differences across the type of technology. This is perhaps not surprising as these assistance systems are often bundled together.

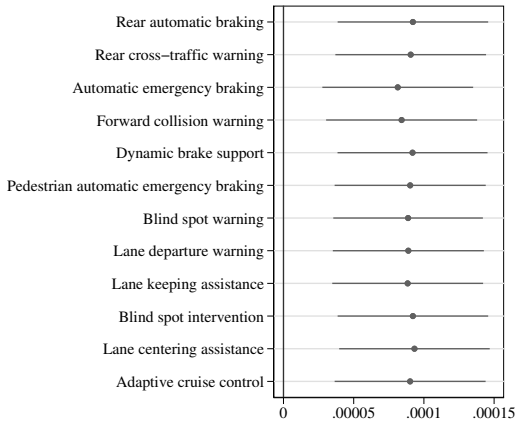
Table 7: Heterogeneity (OLS, using IHS-Transformed Outcomes)

| | (1) Injuries | (2) Property Damage |
|---|---|---|
| Panel A: State Data | | |
| Rain × Tech | -0.00804*** | -0.03066*** |
| | (0.00027) | (0.00054) |
| Rain | 0.00867*** | 0.03373*** |
| | (0.00027) | (0.00058) |
| Observations | 31,404,548 | 31,404,548 |
| | (1) Non-Vehicle Occupant Deaths | (2) Non-Vehicle Occupant Injuries |
| Panel B: FARS Data | | |
| Rain × Tech | 0.00002 | -0.00000 |
| | (0.00001) | (0.00000) |
| Rain | -0.00002 | 0.00000 |
| | (0.00001) | (0.00000) |
| Observations | 71,253,408 | 71,253,408 |
| T-W-Y-M-D-H Fixed Effects | Yes | Yes |
| Holiday Fixed Effects | Yes | Yes |

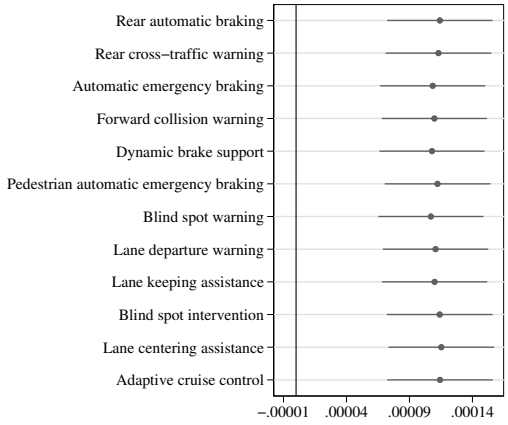
Note: Panel A uses our state crash data. Panel B uses Fatality Analysis Reporting System (FARS) data. T-W-Y-M-D-H refers to tech-weather station-year-month-day of week-hour. *** p<0.01, ** p<0.05, * p<0.1.




(a) State Crash Data - All Crashes




(b) State Crash Data - Fatal Crashes




(c) FARS Crash Data - Fatal Crashes

Figure 4: Comparison of crashes by ADAS Feature

Note: Estimates based on our State and FARS (Fatality Analysis Reporting System) crash data as indicated. All specifications use OLS with an IHS-transformed dependent variable. Bars represent 95% confidence intervals.

## 4.5 Regression Discontinuity Using Daylight Savings Time

In this section we attempt to confirm our main findings using a regression discontinuity approach centered around the annual switch to Daylight Savings Time. As Smith (2016) notes, the time change often causes people to "lose" an hour of sleep and Smith shows that a significant increase in fatal crashes occurs because of the time change.

In Table 8, we provide regression discontinuity (RD) estimates for all/total crashes, fatal crashes, injury crashes, and property damage crashes using our state data, plus we show the RD estimates from our FARS data. We estimate the effect of the time change separately for crashes involving vehicles with and without ADAS. We use the "rdrobust" package in *STATA* (Calonico et al., 2014a), which provides local polynomial RD estimators with robust bias-corrected confidence intervals and inference procedures as outlined in Calonico et al. (2014b). Following Smith (2016), we also use residualized, or "de-meaned," outcome variables, as explained in Section 3. The findings are not surprising at first glance: fatal crashes increase in both our state and FARS data. Although, the estimates lack statistical significance in our sample.

When looking at non-fatal crashes, we find total crashes generally decrease and that the implied percent effect on crashes is greatest for crashes involving vehicles with ADAS. That is, we see a drop in the number of crashes for all vehicles but the effect is is largest for crashes involving cars with driver assistance technology. For non-fatal crashes, most of the estimates are statistically significantly different from zero. The implied effects on fatal crashes run in the opposite direction, with the crash risk associated with Daylight Savings Time causing a greater percentage increase in crashes involving ADAS-equipped vehicles compared to cars without ADAS. Although, again, the observed effects are not statistically significant.

The pattern contrasts with our estimates when using rainfall as a risk shifter. Specifically, rain appears to lower the risk of fatal crashes while increasing the total number of crashes. In contrast, the transition to Daylight Savings Time (DST) seems to slightly increase fatal crash risk while reducing overall crashes. There are several potential explanations for our findings.

For example, rainy conditions might cause drivers to slow down, reducing the severity of crashes but the conditions lead to more minor "fender bender" incidents. Conversely, the DST shift may increase the risk of fatal crashes, possibly due to driver fatigue, while also making drivers more cautious or deterring them from driving altogether, resulting in fewer crashes overall.

To reaffirm this pattern, in Table 9 we report the number of crashes and fatal crashes among vehicles with and without ADAS in the week before and the week after the time change caused by DST. There, echoing our regression discontinuity estimates, we see that there are fewer overall crashes, with ADAS-equipped vehicles experiencing a bigger decline in crashes. However, we see an increase in fatal crashes in both our state and FARS data, with the increase largest when we focus on crashes involving at least one vehicle with ADAS, which again suggests the technology does help to avoid less serious crashes while increasing the risk of fatal crashes. Taken together, the evidence strongly suggests that what one might view as "times of increased risk" may not always cause an increase in crashes, but instead leads to heterogeneous impacts on the various kinds of crashes that can occur.

Table 8: Regression Discontinuity Estimates using DST As a Source of Variation

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Any Crash | | Fatal Crash | |
| | No ADAS | ADAS | No ADAS | ADAS |
| RD Estimate | -0.000257 | -0.002680*** | 0.000103 | 0.000017 |
| | (0.001470) | (0.000642) | (0.000076) | (0.000022) |
| Observations Before DST | 367,718 | 346,085 | 827,063 | 713,814 |
| Observations After DST | 369,323 | 347,753 | 832,037 | 718,573 |
| Bandwidth (each side of DST cutoff) | 204 hours | 192 hours | 461 hours | 398 hours |
| Crashes Per Hour | 0.319970 | 0.038237 | 0.001043 | 0.000069 |
| Implied % Change | -0.0803% | -7.01% | 9.88% | 24.64% |

| | Injury Crash | | Property Damage Only Crash | |
|---|---|---|---|---|
| | No ADAS | ADAS | No ADAS | ADAS |
| RD Estimate | -0.001840*** | -0.000700** | -0.000055 | -0.001380*** |
| | (0.000669) | (0.000283) | (0.001250) | (0.000531) |
| Observations Before DST | 649,383 | 493,668 | 392,883 | 389,282 |
| Observations After DST | 653,909 | 495,310 | 394,588 | 390,978 |
| Bandwidth (each side of DST cutoff) | 362 hours | 274 hours | 218 hours | 216 hours |
| Crashes Per Hour | 0.078119 | 0.008640 | 0.223985 | 0.027730 |
| Implied % Change | -2.36% | -8.10% | -0.025% | -4.98% |

| | | | FARS Fatal Crashes | |
|---|---|---|---|---|
| | | | No ADAS | ADAS |
| RD Estimate | | | 0.000074 | 0.000024 |
| | | | (0.000075) | (0.000017) |
| Observations Before DST | | | 1,270,413 | 1,746,450 |
| Observations After DST | | | 1,278,235 | 1,758,517 |
| Bandwidth (each side of DST cutoff) | | | 321 hours | 442 hours |
| Crashes Per Hour | | | 0.001471 | 0.000122 |
| Implied % Change | | | 5.1% | 19.8% |
| T-W-Y-M-D-H | Yes | Yes | Yes | Yes |
| Holiday | Yes | Yes | Yes | Yes |
| Rain | Yes | Yes | Yes | Yes |

Note: Estimates use our state crash data and an IHS-transformed dependent variable. T-W-Y-M-D-H refers to tech-weather station-year-month-day of week-hour. Observation counts refer to the "effective" observations used after the bandwidth is selected by the "rdrobust" *STATA* package Calonico et al. (2014a). *** p<0.01, ** p<0.05, * p<0.1.

Table 9: Univariate Estimates for Daylight Savings Time

| | No ADAS | | | ADAS | | |
|---|---|---|---|---|---|---|
| | (1) Week Before DST | (2) Week After DST | (3) Difference | (4) Week Before DST | (5) Week After DST | (6) Difference |
| Panel A: Any Crashes - State Data | | | | | | |
| # of Crashes | 0.328134 | 0.317742 | -3.18% | 0.040775 | 0.038390 | -5.89% |
| Manual Difference-in-Differences | | | | | | -2.71 p.p. |
| Observations | 301,402 | 302,686 | | 301,402 | 302,686 | |
| Panel B: Fatal Crashes - State Data | | | | | | |
| # of Crashes | 0.000953 | 0.001064 | 10.38% | 0.000060 | 0.000073 | 17.74% |
| Manual Difference-in-Differences | | | | | | 7.36 p.p. |
| Observations | 301,402 | 302,686 | | 301,402 | 302,686 | |
| Panel C: Fatal Crashes - FARS Data | | | | | | |
| # of Crashes | 0.001400 | 0.001500 | 3.03% | 0.000097 | 0.000117 | 21.12% |
| Manual Difference-in-Differences | | | | | | 18.09 p.p. |
| Observations | 662,849 | 666,568 | | 662,849 | 666,568 | |

Note: Panels A and B use our state crash data. Panel C uses Fatality Analysis Reporting System (FARS) data, which has more observations as it is at the national level. Number of crashes is at the tech-weather station-year-month-day-hour level. Each crash count value represents the mean number of crashes for the indicated group. [TBA: Bootstrap analysis for statistically significant differences.]

# 5 Conclusion

In 2022, traffic crashes claimed the lives of nearly 43,000 Americans.[20] In response to the growing toll of road crashes, automobile manufacturers have introduced a range of technologies aimed at making driving easier, safer, and more efficient. These innovations, known as driver assistance technologies, include systems designed to help drivers avoid collisions and maintain control of their vehicles. Examples include collision warning systems, such as blind spot warning, which alert drivers to potential hazards; collision intervention systems, like automatic emergency braking, which actively prevent or reduce the severity of crashes; and driving control assistance technologies, such as lane keeping assist, which help drivers stay within their lane on the road.

We consider whether driver assistance technologies actually work to reduce car crashes. We focus on differences in crash counts as a function of changes in crash risk to account for the idea that drivers may self-select into vehicles with driver assistance technology based on characteristics that also affect their likelihood of being in an accident. For instance, more cautious drivers might be more likely to purchase cars with advanced technology, which could bias any estimate of the technologys impact on crash risk.

Our main estimates leverage precipitation as a risk shifter to isolate the impact of driver assistance technology. As we describe earlier, rain is a natural source of exogenous variation that increases the risk of crashes regardless of driver characteristics or vehicle type (Eisenberg, 2004; Jung et al., 2014). By focusing on how crash risk changes in response to rain, we are able to estimate the differential effect of ADAS on crashes under elevated risk conditions.

We find that ADAS reduces the number of overall crashes, but increases the number of fatal crashes, highlighting that the technology has heterogeneous effects. Our findings are consistent with other examples of moral hazard due to changes in road safety (Peltzman, 1975). That is, ADAS can help to reduce crashes but drivers behave less carefully, with ADAS helping them to avoid minor crashes, while leading to a greater number of severe

[20] See https://www.nhtsa.gov/press-releases/traffic-crash-death-estimates-2022, last accessed 10/9/2024.

crashes. This pattern is also consistent with anecdotal reports of driver behavior when using Tesla's "autopilot" technology.[21]

We show that our findings are similar when removing hours with heavy rain from our sample, easing any concerns that our estimates are primarily related to ADAS simply not working in rainy conditions. To further rule out such a channel, we complement our main findings with estimates from a regression discontinuity approach around the annual change to Daylight Savings Time. There, again, we see that changes in crash risk (likely caused by losing an hour's sleep) have heterogeneous effects on the type of crash that occurs, with an increase in fatal crashes (echoing Smith, 2016), but a decline in total crashes. As in our main estimates, driver assistance technologies appear to help drivers avoid minor crashes but we see more serious crashes when crash risk is greater.

Our approach allows us to partly avoid the issue of selection bias into vehicle types. However, one limitation of our study is that we must compare crashes among newer vehicles to crashes among older vehicles, and it appears that older vehicles have more crashes during times of heightened risk. Our estimates are about half as large overall when limiting our underlying sample to only crashes among vehicles no more than 6 years old. On the other hand, if crashes involving ADAS-equipped vehicles are mostly caused by drivers of non-ADAS vehicles, our findings may underestimate the effectiveness of ADAS. Finally, our focus on changes in crash risk means we are not estimating the total effect of ADAS on crashes, but rather how well the technology performs when the risk of a crash increases for all vehicles. While we cannot measure the full impact of driver assistance technology on crashes, our approach captures an important dimension of the technology's value: its ability to reduce crashes precisely when risk is higher, which may be particularly informative for policy or consumer decisions versus the average effect across all driving conditions.

[21] See https://www.cnn.com/2024/04/08/tech/tesla-trial-wrongful-death-walter-huang/ and https://www.theguardian.com/technology/2016/jun/30/tesla-autopilot-death-self-driving-car-elon-musk, both last accessed 10/10/2024.

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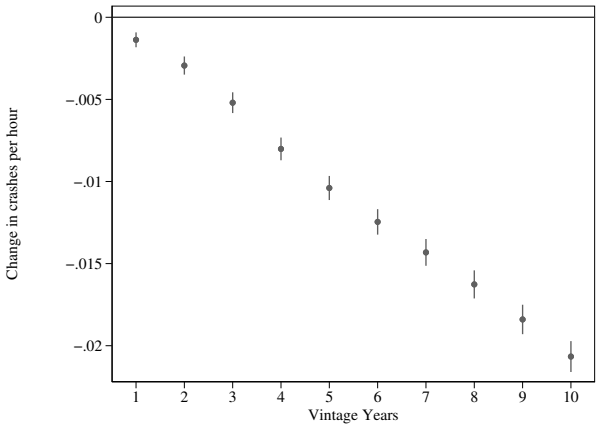
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# Appendix A - Additional Data and Estimates

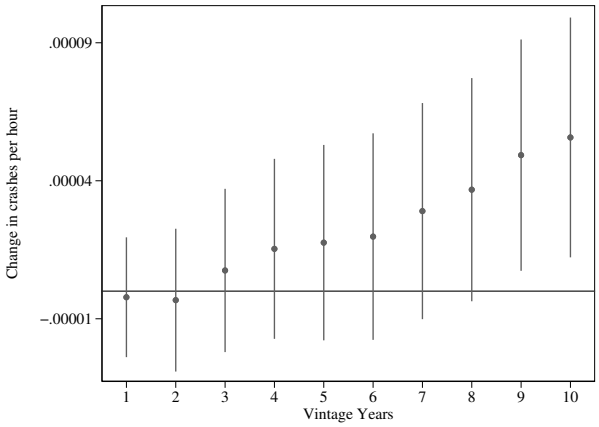
## A.1 Complete Estimates by Vintage

In Figure A1, we show how our estimates vary as we limit the sample only to crashes with at least one vehicle that is $\leq t$ years old. Each sub-figure presents the difference-in-differences estimate for Rain $\times$ Tech for $t = 1, 2, .., 10$ using our state and FARS data as indicated. We see a predictable pattern given the estimates in Section 4 of the main text, where the effect is small when we focus only on crashes among newer cars. As we allow cars up to 10 years old to be in the sample, however, the number of crashes grows disproportionately for cars without ADAS, as they tend to be older, and we are increasingly comparing crashes involving cars with ADAS to crashes among the majority of cars that do not have ADAS.

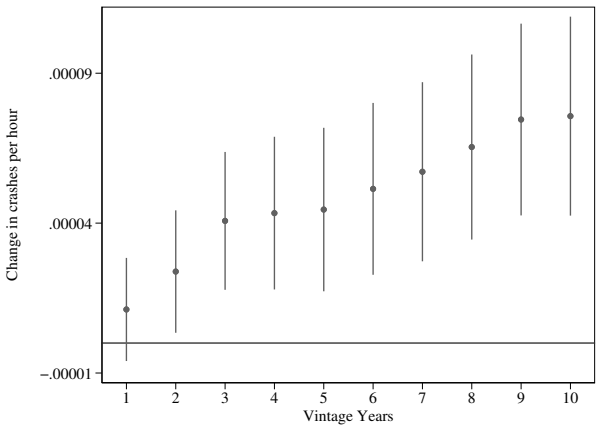
The observed pattern here is only partly driven by the change in the mean of the outcome variable. Instead, it appears that accident rates also vary substantially by vehicle age as a function of precipitation. That is, drivers of older vehicles are more likely to experience a crash when it rains relative to drivers of newer vehicles. Therefore, it is possible that ADAS is less effective than our main estimates show.




(a) State Crashes by Vintage




(b) State Fatal Crashes by Vintage




(c) FARS Fatal Crashes by Vintage

Figure A1: Comparison of Crashes by Vintage

Note: Estimates based on our State and FARS (Fatality Analysis Reporting System) crash data as indicated. All specifications use OLS with an IHS-transformed dependent variable. Bars represent 95% confidence intervals.

## A.2 Additional Summary Statistics

Table A1 provides a complete breakdown of data availability by state and year. While we use data for Arizona in our main estimates, we do not use it for the DST analyses as the state does not observe DST.

Table A1: Data Availability by State and Year - % of Crashes we can Geolocate

| State | 2016 | 2017 | 2018 | 2019 | 2020 | Years |
|---|---|---|---|---|---|---|
| Alabama | - | - | - | - | - | No Data |
| Alaska | 91.23% | 87.94% | 96.55% | 98.66% | 98.33% | 2016 to 2020 |
| Arizona | 99.66% | 99.66% | 99.83% | 99.94% | 99.90% | 2016 to 2020 |
| Arkansas | - | - | - | - | - | No Data |
| California | 55.23% | 60.12% | 63.18% | 64.13% | 66.35% | 2016 to 2020 |
| Colorado | 46.57% | 46.89% | 46.79% | 48.55% | 65.82% | 2016 to 2020 |
| Connecticut | 100% | 100% | 100% | 100% | 100% | 2016 to 2020 |
| Delaware | 100% | 100% | 100% | 100% | 100% | 2016 to 2020 |
| District of Columbia | 84.74% | 93.46% | 99.35% | 99.34% | 99.48% | 2016 to 2020 |
| Florida | 100% | 100% | 100% | 100% | 100% | 2016 to 2020 |
| Georgia | 97.14% | 97.97% | 99.18% | 99.09% | 99.19% | 2016 to 2020 |
| Hawaii | - | - | - | - | - | No Data |
| Idaho | 100% | 100% | 100% | 100% | 100% | 2016 to 2020 |
| Illinois | 90.86% | 95.10% | 99.53% | 99.56% | 99.72% | 2016 to 2020 |
| Indiana | 92.67% | 86.07% | 88.22% | 86.45% | 86.25% | 2016 to 2020 |
| Iowa | 99.61% | 99.71% | 99.80% | 99.79% | 99.67% | 2016 to 2020 |
| Kansas | 96.39% | 94.06% | 96.21% | 96.32% | 93.85% | 2016 to 2020 |
| Kentucky | 100% | 100% | 100% | 100% | 100% | 2016 to 2020 |
| Louisiana | - | - | - | - | - | No Data |
| Maine | - | 100% | 100% | 100% | 100% | 2017 to 2020 |
| Maryland | 100% | 100% | 100% | 100% | 100% | 2016 to 2020 |
| Massachusetts | 95.26% | 95.76% | 95.82% | 94.40% | - | 2016 to 2019 |
| Michigan | 96.90% | 96.37% | 97.15% | 97.02% | 96.71% | 2016 to 2020 |
| Minnesota | 93.40% | 94.87% | 96.05% | 96.62% | 97.35% | 2016 to 2020 |
| Mississippi | - | - | - | - | - | No Data |
| Missouri | 99.45% | 99.69% | 99.95% | 99.95% | 99.42% | 2016 to 2020 |
| Montana | - | - | - | - | - | No Data |
| Nebraska | 5.94% | 5.54% | 7.37% | 17.05% | 36.29% | 2016 to 2020 |
| Nevada | 96.12% | 96.17% | 57.33% | 99.57% | 99.71% | 2016 to 2020 |
| New Hampshire | - | - | - | - | - | No Data |
| New Jersey | - | 26.31% | 27.81% | 28.05% | 31.42% | 2017 to 2020 |
| New Mexico | - | - | - | - | - | No Data |
| New York | 33.98% | 34.78% | 31.52% | 32.70% | 33.14% | 2016 to 2020 |
| North Carolina | 0% | 0% | 99.85% | 57.85% | 58.39% | 2018 to 2020 |
| North Dakota | 100% | 100% | 100% | 100% | 100% | 2016 to 2020 |
| Ohio | - | 96.53% | 97.21% | 98.56% | 99.12% | 2017 to 2020 |
| Oklahoma | - | - | - | - | - | No Data |
| Oregon | 100% | 100% | 100% | 100% | 100% | 2016 to 2020 |
| Pennsylvania | - | - | - | - | - | No Data |
| Rhode Island | - | - | - | - | - | No Data |
| South Carolina | 89.18% | 90.98% | 96.54% | 97.50% | 97.95% | 2016 to 2020 |
| South Dakota | 100% | 99.99% | 99.99% | 99.96% | 100% | 2016 to 2020 |
| Tennessee | 96.47% | 97.89% | 97.18% | 99.70% | 99.73% | 2016 to 2020 |
| Texas | 91.02% | 90.07% | 88.94% | 86.93% | 89.74% | 2016 to 2020 |
| Utah | 99.99% | 100% | 93.61% | 15% | - | 2016 to 2019 |
| Vermont | 100% | 100% | 100% | 100% | 100% | 2016 to 2020 |
| Virginia | 100% | 100% | 100% | 100% | 100% | 2016 to 2020 |
| Washington | 100% | 100% | 100% | 100% | 100% | 2016 to 2020 |
| West Virginia | - | - | - | - | - | No Data |
| Wisconsin | 81.82% | 92.59% | 95.21% | 98.45% | 98.46% | 2016 to 2020 |
| Wyoming | 100% | 99.96% | 99.98% | 100% | 99.98% | 2016 to 2020 |

Data: State Level. Note that we have data from 2016 through 2020 for North Carolina, but cannot geolocate crashes in 2016 and 2017.