Can behavioral interventions be too salient? Evidence from traffic safety messages*

Jonathan D. Hall† Joshua Madsen‡

July 24, 2020

Abstract

Behavioral interventions are a popular tool for encouraging socially desirable behavior and are expressly designed to seize people's attention. However, little consideration has been given to the costs of seizing attention. We estimate these costs in the context of an increasingly common highway traffic safety campaign that displays roadside fatality counts on highway dynamic message signs (DMSs). We exploit detailed data on DMS and crash locations, DMS log files, and a unique setting in Texas where fatality messages are shown only during one week each month. We find that this behavioral intervention significantly increases the number of traffic crashes. The increase in crashes is immediate, dissipates over longer distances, and increases with the displayed fatality count. Furthermore, drivers do not habituate to these messages, even after five years, and the effects do not persist beyond the treated weeks. Crashes increase statewide during treated weeks, inconsistent with any benefits. Our results show that behavioral interventions, designed to be salient, can crowd out more important considerations, causing interventions to backfire with costly consequences.

*We are grateful for helpful feedback from Hunt Allcott, Jennifer Campos, Brad Greenwood, Stephan Heblich, Sarah Jacobson, Melissa Kearney, Kory Kroft, Leslie Martin, Rob McMillan, Peter Morrow, Karthik Muralidharan, Devin Pope, Myles Shaver, Joel Waldfogel, and seminar audiences at the University of California–Irvine, University at Buffalo, University of Toronto, University of Minnesota, and the annual meetings of the Urban Economics Association and the International Transport Economics Association. We thank Brian Stanford of TxDOT and Carlton Allen of Houston TranStar for help acquiring data and institutional details. We gratefully acknowledge that this research was supported by the Social Sciences and Humanities Research Council of Canada, and thank Marc-Antoine Schmidt and Marie Killian for excellent research support.

†Department of Economics and Munk School of Global Affairs and Public Policy, University of Toronto, 150 St. George Street, Toronto, ON M5S 3G7, Canada. jonathan.hall@utoronto.ca

‡Carlson School of Management, University of Minnesota, 321 19th Avenue South Minneapolis, MN 55455, USA. jmmadsen@umn.edu

1 Introduction

There is growing interest among academics and policy makers in using behavioral interventions as a low-cost and easy-to-implement way of encouraging socially desirable behaviors. Reflecting this interest, such interventions are used by over 200 governments and institutions to address a wide variety of issues, including voter turnout, charitable giving, retirement savings, water conservation, energy conservation, hand-washing, caloric intake, diarrhea, and risky sexual behavior (Byrne et al., 2018).[1] Many of these interventions are expressly designed to "seize people's attention" at a time when they can make the desired action (OECD, 2019, p. 29); however, little consideration has been given to individuals' cognitive constraints, and that seizing one's attention may crowd out other, more important, considerations. This paper shows, in a high-stakes context, that crowding out can occur and cause even a simple intervention to backfire with costly individual and social consequences.

Our context is a seemingly innocuous behavioral campaign with the stated objective of reducing traffic crashes, the leading cause of death of 5- to 45-year olds in the U.S. and worldwide (CDC 2018, WHO 2018). This campaign displays the year-to-date count of roadside fatalities on already available dynamic message signs (DMSs) (e.g., "1669 deaths this year on Texas roads," see Figure 1). These fatality messages are expressly designed to be salient, with official statements expressing the "hope" that these "in your face" safety messages will "motivate motorists to exercise caution behind the wheel" and that a "sobering new message. . . will [hopefully] help save lives."[2] Because of its low cost and ease of implementation, this campaign has spread to at least 27 states since 2012 and affected at least 90 million drivers.[3]

[1] For a list of governments using behavioral interventions see Afif et al. (2018) and https://www.oecd.org/gov/regulatory-policy/behavioural-insights.htm. Academic research examples include Duflo and Saez (2003), Frey and Meier (2004), Allcott (2011), Kremer et al. (2011), Costa and Kahn (2013), Beshears et al. (2015), and Allcott and Kessler (2019).

[2] Cunningham, Kailey, and Suzanne Stratford. 2015. "ODOT to display amount of traffic deaths on digital boards along Ohio Highways." Fox 8 Cleveland, July 1, 2015; CBS DFW. 2012. "TxDOT Signs To Regularly Display Traffic Death Numbers." CBSDFW.COM, August 21, 2012.

[3] See Appendix Table A.1 for a list of states that have shown a fatality message. Data on the number of drivers per state comes from U.S. Department of Transportation (2019b). Fatality messages have also been used in at least one other country (South Korea).

This campaign is widely believed to be effective. For instance, in Illinois, the decision to start showing fatality messages was unanimously supported by the Department of Transportation, State Police, and Department of Public Health.[4] Drivers also believe that fatality statistics make safety messages more effective (Boyle et al., 2014). Belief in the effectiveness of these messages is likely an additional factor in their rapid spread. We find, in sharp contrast to these expectations, that this campaign is *increasing* the number of traffic crashes.

One key challenge when measuring the effect of fatality messages is that they are frequently displayed during safer times when the DMS is not being used for more pressing concerns (e.g., travel times, crash alerts), biasing any naïve analysis towards finding a lower frequency of crashes when fatality messages are displayed.

The state of Texas provides a unique setting to overcome this challenge. Unlike in most states, the Texas Department of Transportation (TxDOT) displays the current fatality count only one week each month: the week prior to the monthly meeting of the Texas Transportation Commission. Since August 2012, TxDOT broadcasts the fatality message on as many DMSs as possible during this week. While other messages can pre-empt the fatality message, traffic engineers are instructed that along corridors with a large number of DMSs "the fatality message should be displayed on a few [DMSs]."[5] We confirm that fatality messages concentrate in the designated weeks and use this assignment to treatment to estimate the effect of fatality messages on traffic crashes.

We estimate the effect of showing fatality messages, relative to the status quo usage of DMSs, by comparing how the number of crashes downstream of a DMS (i.e., road "segments") differs the week prior to a meeting of the Texas Transportation Commission ("board meeting") relative to the same segment the rest of the month. We conduct our analysis at the segment-hour level, and include an extensive fixed effect structure to control for inherent variation across different segments over time and throughout each day. As such, our estimates compare, for example, the number of crashes within 10 km downstream of a DMS from 2 to 3PM on Thursday, July

[4] Brandel, Jennifer. 2013. "What's The Deal with Illinois' Traffic Death Highway Signs?" WBEZ 91.5 Chicago, April 9, 2013.

[5] See Figure A.1 for the instructions sent to traffic engineers.

18, 2013 (which occurs during the week prior to a board meeting), to the number of crashes on the same segment from 2 to 3PM on the other three Thursdays in July 2013. We find that during treated weeks there are on average 1.8% *more* crashes over the first kilometer after DMSs, diminishing to 0.8% more crashes over 8–10 km after DMSs.

We conduct two placebo tests to address the possibility that the weeks prior to TxDOT board meetings are inherently more dangerous than other weeks within the same month. First, we estimate the change in crashes occurring *upstream* of DMSs. As a segment can be upstream of one DMS and downstream of another, we restrict this test to those DMSs where the nearest upstream DMS is more than 10 km away. Second, we estimate a placebo effect using data from the pre-treatment period. Both tests produce statistically and economically insignificant placebo effects.

Our main results are difference-in-differences estimates that exploit both within-month variation in when fatality messages are instructed to be displayed and differences between the pre-treatment (January 2010–July 2012) and treatment (August 2012–December 2017) periods. We find that during treated weeks there are 1.36% more crashes over the 10 km after DMSs. Further, it is likely that the true effect of displaying fatality messages is even larger since fatality messages are regularly turned on 1–3 days early. Redefining treatment as beginning one day earlier increases the estimated treatment effect to 2.17%. These results suggest that, inconsistent with the policy's stated objective, increasing awareness of the risks of driving via fatality messages increases the number of traffic crashes.

The magnitude of the effect is large given the simplicity of the intervention. The above estimates measure the effect of the *assignment* to show a fatality message. Due to imperfect compliance, we use instrumental variables to estimate the effect of *displaying* a fatality message.[6] We find that displaying a fatality message increases the number of crashes over the 10 km downstream by 4.5–7.9%. Based on prior research, this is comparable to raising the speed limit by 3–9 miles per hour (van Benthem, 2015) or reducing the number of highway troopers by 12–24% (DeAngelo and Hansen, 2014). Our back-of-the-envelope calculations suggest that fatality

[6] Because we only have log files for a subsample of DMSs, we use two sample 2SLS.

messages cause an additional 2,600 crashes and 16 fatalities per year in Texas alone, with a total social cost of $377 million per year.

Our proposed explanation for this surprising finding is that fatality messages add to drivers' cognitive loads, crowding out their capacity to drive safely. We provide several pieces of evidence that support this hypothesis. First, we find that fatality messages are more harmful when they report a larger number of fatalities (i.e., a plausibly more shocking and distracting number). In a related test, we find that as the year progresses, and the number of displayed fatalities increases, the effect worsens, with the largest number of additional crashes occurring in January (when the fatality number is the highest).[7] We also find that the increase in crashes concentrates in areas where drivers' cognitive loads are already high, as proxied for by annual vehicle kilometers traveled, downstream lane kilometers, downstream centerline kilometers, or the presence of multiple DMSs. Finally, we find that fatality messages increase the number of multi-vehicle crashes, but not single-vehicle crashes, consistent with increased cognitive loads causing drivers to make small mistakes such as drifting out of their lane, rather than large errors such as driving off the road.

In contrast, when cognitive loads are low or the message is less distracting, fatality messages plausibly help or have no effect. We find that showing a fatality message helps when fatality counts are below the 25$^{\text{th}}$ percentile or when our measures of complexity are more than a standard deviation below their means.

It is possible that fatality messages distract drivers in the moment, but then lead them to drive more safely either elsewhere or later in the month. We provide evidence suggesting that this is not the case. First, drivers do not drive more safely the days immediately following campaign weeks. Second, drivers are not getting used to the messages. Fatality messages are associated with an increase in crashes every year, except one, between 2013 and 2017. Finally, drivers do not drive more safely elsewhere during treated weeks. We estimate that during treated weeks there are 2.0% more crashes statewide, primarily driven by increased highway crashes.

We rule out several alternative explanations, including the possibility that reading a message (rather than the content of the message) causes the crash and the possibility

[7] The fatality count resets in February so that it is not trivially low in January.

that the reported number of fatalities is less than drivers expect, so that drivers respond rationally by driving more recklessly.

This paper makes four contributions. First, we show that behavioral interventions can be too salient, causing them to backfire. In doing so, we build on research that documents other policies that backfired, including policies designed to reduce discrimination in labor markets that increase discrimination (Behaghel et al., 2015, Agan and Starr, 2018), and expanded rent controls in San Francisco in 1994 that ultimately lead to higher rents (Diamond et al., 2019). Perhaps most closely related, Byrne et al. (2018) find that behavioral interventions which compare an individual's and peer's usage of electricity can result in some individuals *increasing* their consumption. We build on this work by documenting a new mechanism by which behavioral interventions can backfire: they can increase individuals' cognitive load, crowding out more important considerations.

This result matters for three reasons. First, it helps inform the design of other behavioral interventions by showing it is important to: (1) consider individuals' cognitive loads when interventions will occur, (2) be careful interventions are not too salient, and (3) measure the effect of the intervention, ideally building evaluation into the intervention's design. Second, it warns that the growing practice of sending reminders can be taken too far and may crowd out other desirable activities.[8] Third, it shows that because individuals face cognitive constraints, a full accounting of the welfare effects of an intervention should consider whether adding to participants' cognitive loads has spillover effects outside of the targeted domain.[9]

Second, we also contribute by providing evidence that individuals do not always habituate to behavioral interventions nor do their effects necessarily persist after treatment stops.[10] A large literature measures whether the effects of behavioral interventions persist after treatment stops. While there are some notable exceptions,

[8] For examples of successful reminder campaigns, see Karlan et al. (2016), Calzolari and Nardotto (2017), and York et al. (2018).

[9] For examples of excellent papers evaluating the welfare effects of behavioral interventions see Bernheim et al. (2015), Allcott and Kessler (2019), Butera et al. (2019), and Farhi and Gabaix (Forthcoming).

[10] For a summary of research on the design and effectiveness of behavioral interventions, see Rogers and Frey (2015).

such as Allcott and Rogers (2014) and Bernedo et al. (2014), this literature typically finds little persistence (Brandon et al., 2017). We find no evidence that fatality messages affect behavior outside the designated weeks. A much smaller literature tests whether individuals habituate to behavioral interventions, as budget considerations make it difficult to test long-run effects. One exception is Allcott and Rogers (2014), who using the Opower "home energy report" find that individuals appear to largely habituate after only four reports. In contrast, we find that drivers do not habituate to fatality messages, with the treatment effect remaining virtually unchanged five years after the initial implementation.

Third, we contribute to a large literature spanning economics, accounting, finance, and psychology on how to best disclose risks as well as how individuals respond to these disclosures.[11] Although risk disclosures are common in many markets, many tend to be *generic* rather than *specific* (e.g., "driving is dangerous" vs. "sharp turn ahead").[12] There is concern that generic risk disclosures may be ineffective at reducing risk taking due to their lack of specificity, but there is limited empirical evidence on their effectiveness. Our setting allows us to measure the effectiveness of a generic risk disclosure. We find that generic yet plausibly shocking risk disclosures can affect individual behavior, albeit not necessarily as intended by policy makers.

Finally, we contribute to the large literature on traffic safety.[13] We show, contrary to drivers' and policy makers' expectations, that increasing awareness of the risk of driving via fatality messages causes additional traffic crashes. This suggests that an easy way to improve road safety is to stop displaying these messages on DMSs. As discussed earlier, the magnitude is comparable to other potential policy changes, such as reducing speed limits and hiring more police. Existing research on DMS safety

[11] For examples, see Forsythe et al. (1999), Jin and Leslie (2003), SEC (2005), Longo (2005), FDA (2012).

[12] Examples of generic risk disclosures include: "past performance provides no guarantee of future results" (financial markets), "I am aware of the risks associated or related to indoor rock climbing" (sports), and "the Department of State alerts US citizens to the continued threat of terrorist attacks throughout Europe" (travel). Many mandatory disclosures in public firms' 10-K filings are also generic.

[13] See, for instance, Levitt and Porter (2001), Abouk and Adams (2013), Anderson and Auffhammer (2014), Kapoor and Magesan (2014), Hansen (2015), Francesconi and James (2019), Ang et al. (2020), and Gallagher and Fisher (forthcoming).

messages by transportation engineers finds evidence that messages about speeding, fog, or slippery roads are effective at reducing drivers' speeds (Rämä and Kulmala, 2000, Al-Ghamdi, 2007, Chaurand et al., 2015). In contrast, we find that fatality messages reduce driver safety.

2 Data, Summary Statistics, and Research Design

2.1 Data

We collect data on traffic crashes, DMS locations and messages shown, the meeting schedule of the TxDOT board, weather, the Texas road network, and US federal holidays. Table 1 summarizes the full period, January 2010–July 2012 "pre-treatment" period (i.e., the time period before TxDOT began displaying fatality messages), and August 2012–December 2017 "treatment" period. We have data on 886 DMSs. The treatment sample covers 65 calendar months and 40,070,893 hourly observations, whereas the pre-treatment sample covers 31 calendar months and 20,047,441 hourly observations.

Our data on traffic crashes comes from the TxDOT Crash Records Information System (CRIS) and includes all reported crashes occurring on Texas roads.[14] This dataset includes the GPS coordinates and the number of fatalities for each crash.[15]

We collect DMS location data from the TxDOT website and lists provided by TxDOT of all DMSs in 2013 and 2015. We combine this location data, and validate and correct it using Google Maps.[16] Figure 2 plots the locations of these DMSs within the entire state and Figure 3 plots those in the Houston area. These maps show that DMSs are located primarily within urban areas, and, within urban areas

[14] By law, crashes must be reported if the apparent damage exceeds $1,000 or if the crash resulted in an injury or death (http://www.txdot.gov/driver/laws/crash-reports.html).

[15] For 23% of the observations, GPS coordinates are recorded at the site of the crash, while in remainder TxDOT geocodes it from the reported address.

[16] We correct 18% of the DMS locations. We update the direction of travel for 26 DMSs. We drop 174 DMSs which are portable, test DMSs, or smaller than standard. These smaller DMSs are often just able to display a few characters and are used for showing travel times or tolls. The largest DMS we drop for being too small can show two lines of 12 characters, while standard DMSs can show three lines of 15 or 18 characters. Finally, we drop four DMSs located on local roads rather than highways.

are spaced fairly evenly apart, with a median driving distance of 5.3 km between consecutive DMSs. Our main results assume all DMSs that exist during our sample, exist for the entire sample.[17]

We gather data on the messages displayed on DMSs from two sources. First, we obtain log files for the DMSs located in the Houston area for the years 2012–2013, and second, we collect hourly DMS message content directly from the TxDOT website for all Texas DMSs for 2016–2017.

We collect the meeting schedule of the Texas Transportation Commission from the TxDOT website. These board meetings are typically held the last Thursday of each month, except in November and December when they are held earlier to avoid conflicting with Thanksgiving and Christmas.

We obtain hourly weather data from the U.S. National Oceanic and Atmospheric Administration's Integrated Surface Database (ISD). Figures 2 and 3 also show the locations of the weather stations we use. The median distance between a DMS and the nearest weather station is 14 km.[18]

2.2 Variable Definitions

Our primary outcome variable is the hourly number of crashes on a given road segment. Road segments begin at DMS locations and continue for x km of highway driving distance, with $x \in \{-10, -9, ..., 9, 10\}$ and where negative distances denote segments preceding the DMS (i.e., upstream) and positive distances denote segments continuing past the DMS (i.e., downstream). Driving distances are calculated using

[17] We collect information on when each DMS exists using Google Streetview. This data is limited as the mean gap between the last time a DMS is known not to exist and the first time it is known to exist is 2.9 years and the mean gap between the last time a DMS is known to exist and the first time it is known to not exist is 1.4 years. From this data we know that at least 24% of DMSs do not exist over our entire sample. Including DMSs that are not operational biases our results towards zero.

[18] As a test of the relevance of this weather data for the road segments they are matched with, we compare the weather conditions reported in the ISD to those recorded in crash reports. The probability that a crash report says it was raining increases by 46.6 percentage points (26.7% vs. 73.3%) when the associated weather station reports rain during the hour the crash occurred. Furthermore, the probability that a crash report says there were clear skies decreases by 94.4 percentage points (97.2% vs. 2.8%) when the associated weather station reports rain during the hour.

the Open Source Routing Machine and Open Street Maps data for the Texas highway network.[19] Figure 4 depicts segments of 1, 3, 5, and 10 km downstream of a sample DMS near Aledo, Texas and the crashes associated with each segment. Because we allow road segments to merge and diverge onto other highways, road segments of length $x + 1$ km typically contain more than an additional 1 km of road surface area and thus have a more than proportional increase in the number of crashes. We, therefore, scale hourly crash counts for segments of length x by the average number of crashes occurring over all segments of length x over the entire sample period to create a standardized measure of crashes that is easier to interpret. We label this variable $Crash\,(\%)_{s(x),d,h}$, where the subscripts index segment (s), segment length (x), day (d), and hour (h). See Appendix Table A.2 for detailed variable definitions.

We define treatment status using the schedule of Texas Transportation Commission board meetings. Since August 2012, TxDOT traffic engineers have been instructed to display the fatality message beginning "after morning peak" on the Monday a week prior to a board meeting and ending "before morning peak" on the following Monday. Exact times are not provided, as "morning peak" varies by highway and direction of travel. As Figure 5 shows, we observe from our sample of DMS log files that a fatality message is displayed for approximately 8% of the DMS-hours between midnight and 7 AM on the Monday a week prior to board meetings (designated first day), increasing to 12%, 18%, and 29% during the 7, 8, and 9 AM hours, respectively, and then displaying on roughly 30–42% of DMS hours during the safety campaign. Percentages less than 100% are consistent with instructions that fatality messages, "should not pre-empt needed traffic messages, incident-related messages, Emergency Operation Center messages (EOC), or Amber/Silver/Blue alerts[.]" Fatality messages also gradually begin to disappear at the end of the safety campaign, with the fatality message showing for 21%, 16%, and 12% of DMS-hours messages during the 6, 7, and 8 AM hours of the final Monday, respectively. Thus, although there is leakage into hours immediately before and after the intended display period, we find that fatality messages are concentrated during the designated week, with increased presence

[19] Our network includes all roads classified as motorways, motorway links, trunk roads, and primary roads. This is the smallest set of classifications which includes all highways, but also includes some roads that are not highways.

beginning around the 9 AM hour of the designated first day and decreased presence by the 7 AM hour of the designated last day. We define an indicator variable for the week prior to a board meeting, $Board\ meeting_{d,h}$, which equals one for all days d and hours h between 9 AM on the Monday 10 days prior to a scheduled board meeting and 7 AM the following Monday.

Figure 6 shows how the share of DMS-hours displaying the fatality message (based on the DMS message sample) has evolved over time, and plots the mean as well as 25[th] and 75[th] percentiles for both safety campaign weeks (left panel) and other weeks (right panel). The 25[th] percentile during a safety campaign is often zero, as some DMSs never display a fatality message within a month. The share of DMS-hours displaying a fatality message outside of the assigned week has also remained low. Table A.3 tabulates additional details for the 41 safety campaigns covered by our DMS message sample, and demonstrates that for most safety campaigns, 60–80% of DMSs display a fatality message at some point.[20]

To control for variation in weather conditions we define two indicators for whether the weather station closest to DMS s reported precipitation during hour h of day d. Specifically, $Trace\ precipitation_{s,d,h}$ is set equal to one if the weather station reported less than 1 millimeter of precipitation, and zero otherwise; and $Precipitation_{s,d,h}$ is set equal to one if the weather station reported 1 millimeter or more of precipitation, and zero otherwise.

Table 2 reports summary statistics for our data. As discussed earlier, due to the increasing surface area covered by segments of larger lengths, the number of crashes per hour is increasing more than proportionally in segment length, with 7.5 times more crashes within 10 km of a DMS than within 3 km.[21]

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[20] There are two months where the safety campaign occurred during the week of the board meeting. According to TxDOT, this was due to human error.

[21] We measure the length of roadway within segments, and find that the ratio of the length of roadway within x and 1 km is very similar to the ratio of the number of crashes within x and 1 km for $x \in \{3, 5, 10\}$ (see Table A.4).

2.3 Research Design

To estimate the effect of fatality messages on the number of traffic crashes, we exploit within-month variation of fatality messages while controlling for weather, holidays, and idiosyncratic segment characteristics. To control for unobservable within-month fixed segment characteristics (e.g., idiosyncratic elements of the season, time of day, and day of the week specific to each DMS highway segment), we include segment-year-month-day-of-week-hour fixed effects. Because fatality messages are only instructed to be displayed for one week each month, we can contrast, for each DMS highway segment, year-month-day-of-week-hours when the message was instructed vs. not instructed to be displayed.[22] We also include controls for precipitation (both $Trace\ precipitation$ and $Precipitation$) and holiday fixed effects. We estimate the following OLS regression using all hourly observations from Aug 1, 2012, through Dec 31, 2017:

$$\begin{aligned}\text{Crash (\%)}_{s(x),d,h} = \delta \cdot \text{Board meeting}_{d,h} + \beta_1 \cdot \text{Trace precipitation}_{s,d,h} \\ + \beta_2 \cdot \text{Precipitation}_{s,d,h} + \gamma_{s,m(d),dow(d),h} + \zeta_{holiday} + \epsilon_{s,d,h}.\end{aligned} \quad (1)$$

In regression (1) δ is our estimated treatment effect, γ is a fixed effect for each segment-year-month-day-of-week-hour, and ζ is a fixed effect for each holiday.

We also estimate the treatment effect using a difference-in-differences specification that exploits both within-month variation in when fatality messages are instructed to be displayed and differences between treatment periods. Specifically, we estimate the following regression:

$$\begin{aligned}\text{Crash (\%)}_{s(x),d,h} = {} & \delta \cdot \text{Board meeting}_{d,h} \cdot \text{Post}_d + \beta_1 \cdot \text{Board meeting}_{d,h} + \\ & + \beta_2 \cdot \text{Trace precipitation}_{s,d,h} + \beta_3 \cdot \text{Trace precipitation}_{s,d,h} \cdot \text{Post}_d \\ & + \beta_4 \cdot \text{Precipitation}_{s,d,h} + \beta_5 \cdot \text{Precipitation}_{s,d,h} \cdot \text{Post}_d \\ & + \gamma_{s,m(d),dow(d),h} + \zeta_{holiday} + \epsilon_{s,d,h},\end{aligned} \quad (2)$$

[22] We do not compare year-month-day-of-week-hours where a fatality message is displaying vs. not displaying because whether a fatality message is displaying is endogenous. We provide evidence of this endogeneity in Appendix A.2.

which is equivalent to taking the difference between δ from regression (1) for the August 2012–December 2017 sample and δ from the same regression for the January 2010–July 2012 sample. In regression (2), δ is the coefficient of interest. In our analyses we find no difference during the pre-treatment period in downstream crashes between the week prior to a board meeting and other weeks, so the primary difference between regressions (1) and (2) is that the second has larger standard errors.[23]

As a conservative approach, we cluster standard errors by geography-year-month, where geography refers to bins of size x^2 square kilometers that contain the DMS segment of length x. Thus, fewer clusters (geographic bins larger in area) are used for segments of greater length, as crashes over those longer lengths may be linked to multiple DMSs.[24]

2.4 Fatality message verification

Our research design exploits TxDOT's "intention to treat" by displaying fatality messages only over certain hours each month. The lack of DMS log files for most of the sample precludes us from using either the actual or instrumented content of the DMS for the full sample. Regressions (1) and (2) thus constitute reduced form regressions of the outcome of interest (crashes) on the source of exogenous variation (board meetings).

Before continuing with the empirical analyses, we verify an increased propensity to disclose fatality messages during the expected week using our restricted sample of DMS log files. Figures 5 and 6, discussed previously, provide initial evidence of an

[23] This is because regression (1) presumes the treated week would be exactly the same as the other weeks in the absence of treatment while regression (2) acknowledges uncertainty about whether treated weeks would be the same in the post period in the absence of treatment.

[24] To define geographic bins, we use the latitudes and longitudes that constitute the North, South, East, and West edges of the state of Texas. Using the absolute difference in these latitudes and longitudes, we assign latitudes and longitudes to create square bins of size x^2 square kilometers that cover the entire state of Texas and assign each DMS to one of these bins. We note that the full sample covers 3,184,899 crashes within 10 km of a DMS, yet because a crash can be associated with multiple DMSs there are only 849,623 distinct crashes in the full sample within 10 km of any DMS, representing 38% of the 2,220,441 total highway crashes reported statewide for the same period. This feature of the data motivates our decision to cluster standard errors by geographic bins.

increase in the display of fatality messages during the designated week. To more fully explore this relation, we estimate the following regression:

$$\begin{aligned}\text{Fatality message}_{s,d,h} = \delta \cdot \text{Board meeting}_{d,h} + \beta_1 \cdot \text{Trace precipitation}_{s,d,h} \\ + \beta_2 \cdot \text{Precipitation}_{s,d,h} + \gamma_{s,m(d),dow(d),h} + \zeta_{holiday} + \epsilon_{s,d,h}\end{aligned} \tag{3}$$

where $Fatality\ message_{s,d,h}$ is the percent of hour h on day d that the DMS at the start of segment s displayed a fatality message and the remaining variables are previously defined.[25] We tabulate coefficient estimates of equation (3) in Table 3. Consistent with the instructions provided by TxDOT to its traffic engineers, we document that the percent of hours a fatality message is displayed increases by 30.2 percentage points during the week prior to a board meeting.

Equation (3) estimates the effect of being assigned to show the fatality message relative to the status quo usage of DMSs rather than relative to the DMS being blank. This is the policy-relevant treatment effect but also means it is important to understand what messages the fatality message is replacing. We tabulate summary statistics for DMS message types during the treated period in Table 4. During non-treatment weeks, fatality messages display for an average of 1.2 minutes each hour, increasing substantially to 19.3 minutes during safety campaign weeks. This increase comes at a 10.7 minute reduction in non-safety messages, a 1.1 minute reduction in travel time messages, and a 7.5 minute decrease in blank DMSs during the hour. We find a statistically but non-economically significant 0.1 minute increase in Amber alert minutes, consistent with instructions that DMS messages are not to replace high-priority messages.

Overall, we conclude that the use of fatality messages significantly and meaningfully increases during safety campaign weeks both by using the DMS more and by reducing other types of non-essential messages. In the following section, we use the

[25] From our 2012–2013 DMS log data for Houston we can calculate $Fatality\ message_{s,d,h}$ precisely. In the 2016–2017 web scraped data we only observe the sign status at the start of each hour and assume that if a sign's status changes that the change occurred halfway between the two hours.

increased presence of fatality messages during the week preceding board meetings to estimate the effect of these messages on highway crashes.

3 Results

This section reports our main results. We begin with univariate analysis indicating an increase in crashes the week prior to a board meeting (when fatality messages are displayed) relative to other weeks. We then show that these results hold in first-difference and difference-in-differences multivariate analyses after controlling for weather, holidays, as well as segment and time-of-day/day-of-week differences. We conclude this section by showing that there is no evidence this effect has dissipated over time.

3.1 Univariate results

In Figure 7 we show that the mean number of crashes downstream of DMSs during the week prior to a board meeting is greater than the mean number of crashes in other weeks. Specifically, the circles plot the percentage difference in the average number of crashes occurring during safety campaign weeks vs. other weeks over the segments [0,1], (1–4], (4–7], and (7–10] kilometers downstream of DMSs. We find there are more crashes during safety campaign weeks, with the largest effect a 2.8% increase over the first kilometer. This effect slightly diminishes to a 1.8% increase over the (7-10] km interval.

Figure 7 provides suggestive evidence that while the estimated effect diminishes over longer distances, they do not decay to zero. We conjecture that the increase in crashes over distances farther away from DMSs is due to subsequent treatment by downstream DMSs. To map out the impact of fatality messages in the absence of subsequent treatments, the hollow squares in Figure 7 plot univariate differences in crash rates for the subset of DMSs where there are no downstream DMSs within x kilometers. We find that for DMSs with no downstream DMS within 7 or 10 kilometers, the effect over the distances (4,7] and (7,10] km become statistically insignificant, respectively.[26] These results suggest the immediate increase in crashes in response to the fatality message is short lived and concentrated after DMSs.

We next conduct two placebo tests to address the possibility that the week prior to board meetings is inherently more dangerous than other weeks. First, we examine the change in crashes upstream of DMSs. Because a segment that is upstream of one DMS may be downstream of another, we limit this test to DMSs where the nearest upstream DMS is more than 10 km away (reducing our sample by 75%). In Figure 8 Panel A we find no effect upstream for this restricted sample. All but one of the downstream estimates is above zero, with a statistically significant increase over the 8-10 kilometers downstream of DMSs. The lack of a significant upstream effect for this subsample of DMSs is consistent with fatality messages driving the increase in crashes, although we caution that we have less power to measure an effect on this non-random sample of DMSs, which mostly include DMSs on the edge of cities or in rural areas. Second, we estimate the change in crashes during the week prior to a TxDOT board meeting for the pre-treatment period. We find no downstream effect in the pre-treatment period. We also find no positive upstream effect. These results are inconsistent with driving conditions being inherently more dangerous the week prior to board meetings.

3.2 Multivariate results

We next show that these results hold when using more rigorous specifications that adjust for weather, holidays, as well as segment and time-of-day/day-of-week differences. We start with first-difference estimates from equation (1), plotted in Figure 9 using circles, for incremental distances after DMSs. Similar to the univariate results depicted in Figure 7, we find an increase in the number of crashes downstream of DMSs (significant at the 5% level over the (4-7] km distance and at the 10% level over the [0,1], (1,3], and (7,10] km distances). We estimate that fatality messages increase the number of crashes by 1.8% over the first kilometer, decreasing to a 0.8%

[26] Figure A.3 Panel A also shows a positive but insignificant effect over the first kilometer, followed by insignificant downstream effects if we limit the sample to only those DMSs with no downstream DMS within 10 km (25% of the treatment period observations).

increase over the (7,10] kilometers after DMSs. The hollow squares in Figure 9 plot coefficient estimates when the sample is restricted to segments where there are no downstream DMSs within x kilometers. Within these samples, we find the increase in crashes decays much faster, although we caution that mapping out the duration of any effect is limited due to the non-random nature of each of these sample subsets.[27]

We repeat our two placebo tests using the first-difference multivariate specification in Figure A.2. Panel A reports the results of estimating equation (1) on the restricted sample where the nearest upstream DMS is more than 10 km away. We find no evidence of additional crashes upstream during treated weeks. Panel B reports the results of estimating the first-difference regression using the pre-treatment sample. We again find that the difference in the number of crashes during the week prior to a board meeting is both economically and statistically insignificant across all distances, both upstream and downstream of DMSs.

Table 5 columns (1)–(3) report our main results: difference-in-differences estimates of equation (2) that account for the uncertainly in whether the week prior to a board meeting is inherently more dangerous. Each column in Table 5 reports results for different highway segment lengths. The first row, *Board meeting* $\times$ *post*, estimates the treatment effect of fatality messages. We find that within 5 km of a DMS there is a 1.54% increase in the number of crashes per hour (column (2)), slightly diminishing to a 1.36% increase over the 10 km after DMSs (column (3)). Both effects are statistically significant at 5% confidence levels. Within 3 km the effect is positive but not statistically significant (column (1)).[28] The second row, *Board meeting*, estimates the change in the number of crashes one week before board meetings from January 2010–August 2012. As this period pre-dates the fatality safety campaigns, we expect and find no effect (consistent with Figure A.2 panel B), with these estimates both small and statistically insignificant,

The true treatment effect is likely larger than the 1.2–1.5% documented in Table 5 columns (1)–(3). As shown in Figure 5, fatality messages regularly begin to appear

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[27] Figure A.3 Panel B maps out the decay for only those DMSs with no downstream DMS within 10 km and finds generally insignificant downstream effects.

[28] Table A.5 of the online appendix presents separate analysis of both pre-treatment and treatment periods.

1–3 days prior to the official Monday morning "start date," although we caveat that we do not have DMS log files for the full sample and thus have an imperfect measure of the amount of leakage. We define an alternative treatment measure, *Board meeting-Sunday*, where treatment begins Sunday morning at midnight rather than Monday at 9 A.M. As shown in Table 5 columns (4)–(6), our estimated treatment effects increase by 154-200% using this alternative measure. This substantial increase results from moving a day with a large treatment effect (i.e., the Sunday eleven days prior to a board meeting) from the control group to the treatment group. The magnitude of this change is large relative to the amount of leakage documented in Figure 5 (e.g., 7–8% of DMSs showing a fatality message the Sunday prior to campaign weeks). One interpretation is that Figure 5 underestimates how frequently fatality messages are displayed prior to the designated start time (leakage), since Figure 6 shows that the amount of leakage declines over time and 82% of our DMS log data is from 2016–2017.

Our estimated magnitudes are large given the intervention's simplicity. We use two-sample instrumental variables to estimate the effect of displaying a fatality message on the number of crashes downstream of a DMS. The first-stage regression is run on the subsample for which we have DMS log files, and the second-stage regression is run on the full sample. We bootstrap standard errors. The results, using both *Board meeting* and *Board meeting-Sunday*, are reported in Table 6. We find that displaying a fatality message results in a positive but insignificant increase in crashes over the first three kilometers using *Board meeting* as the instrument in the first stage (i.e., defining treatment to begin Monday at 9 A.M.). *Board meeting-Sunday*, in contrast, results in an economically and statistically significant 8.96% increase in crashes over the first three kilometers. Consistent with the pattern in Figures 7, 9, and Table 5, we find these magnitudes decrease over longer distances, with a total increase in the number of crashes of 4.5–7.9% over 10 kilometers when fatality messages are displayed. These magnitudes are comparable to increasing the speed limit by 3–9 miles per hour (van Benthem, 2015) or reducing the number of highway troopers by 12–24% (DeAngelo and Hansen, 2014).

3.3 Effect over time

We find no evidence that the effect of displaying a fatality message has dissipated over time. Figure 10 plots the coefficient estimates from a modified version of equation (1) that allows the treatment effect to vary each year. Consistent with the evidence in Figure A.2 panel B, we find an insignificant change in crashes the week prior to board meetings during 2010–2012 (generally the pre-treatment period). For all years after 2012 except 2016, the estimated coefficient is positive, and in three of those years statistically significant.

4 Mechanism

We find that displaying the year-to-date count of fatalities on DMSs increases the number of crashes. In this section, we investigate the mechanism for this increase in crashes. A large body of research documents that increased cognitive loads distract individuals, causing them to have longer response times, make more mistakes, and fail to process available information.[29] We hypothesize that by showing a potentially shocking and morbid statistic, fatality messages increase drivers' cognitive loads, distracting them, and crowding out their capacity to drive safely.[30] We provide six pieces of evidence supporting this hypothesis. We also rule out five alternative hypotheses, including the possibility that the fatality messages help in the long run. To save space, where relevant we focus in this section on the treatment effect on crashes within 10 km of a DMS.

[29] For examples, see Gilbert et al. (1988), Berggren et al. (2011), and Strayer et al. (2013).

[30] Fatality messages may add to drivers' cognitive loads more than a typical DMS message by inducing anxiety. Since Yerkes and Dodson (1908), psychologists have documented that high levels of anxiety or arousal can worsen performance on a variety of tasks by causing individuals to focus on the risk rather than the task, reducing individuals' ability to process new information, and causing people to overthink their actions, overriding faster automatic responses (Staal, 2004). In all of these cases, drivers are distracted and paying attention to the wrong things.

4.1 Shocking fatality messages distract drivers

We start with the evidence supporting our hypothesis that fatality messages distract drivers. Our first piece of evidence is that the harm done by the safety message is increasing in the reported number of deaths, suggesting that bigger fatality numbers are more distracting than smaller ones. We estimate a modified version of (2) that allows the effect of the safety campaign to vary by the quartile of reported deaths.[31] Figure 11 plots these results. When the number of reported deaths is small, displaying a fatality message *decreases* the number of crashes. However, as the number of reported deaths increases, the effect of showing a fatality message on crashes grows more harmful. The harm done when the number of deaths is in the highest quartile is nearly double the benefit when the number of deaths is in the smallest quartile.

Second, and closely related, the harm done by the safety message is increasing throughout the year. Because the number of deaths reported is mechanically climbing throughout the year,[32] this is an alternate way of showing that increases in the displayed number of deaths lead to more crashes. Figure 12 plots our difference-in-differences estimates of the treatment effect by calendar month. We find that displaying a fatality message helps in February (-3.6%, statistically significant at the 10% level), when the number of deaths displayed resets (January shows the prior

[31] Because we do not observe the displayed death count in every month, we impute the year-to-date fatality count for each month using the actual number of year-to-date fatalities. From the DMS log files we find that the reported fatality number is reported with a median lag of 22 days, and we use this lag when imputing the number of fatalities for each month.

[32] Appendix Figure A.4 displays, for all DMSs in our log file sample, both the mode year-to-date death count displayed within a given month (black diamonds), and all other death counts displayed within the month (gray circles, with sizes corresponding to their relative frequencies). The number resets in February and the prior year's fatality count is displayed in January so as not to display a trivially low number. Due to human error we note some limited within-month variation in the reported number of deaths: DMSs occasionally display the reported number of deaths from the previous month or reverse a digit (e.g., 841 deaths vs. 814 deaths).

year's total), and then its effect worsens throughout the year.[33] From October through January the effect is positive and statistically significant.

Third, as Figure 12 shows, the effect of displaying a fatality message drops 11 percentage points between January and February, when the displayed number of deaths resets. This significant change supports the hypothesis that the number displayed matters and the message distracts drivers, and is inconsistent with the variation over the year simply being due to seasonal weather or driving patterns.

Fourth, the increase in crashes is larger in areas that place high cognitive loads on drivers. We use three related measures for whether a road segment is complex and would require high cognitive loads: centerline kilometers, lane kilometers, and average daily vehicle kilometers traveled (VKT).[34] We normalize these measures to have a mean of zero and a standard deviation of one, and interact with our treatment variable. As columns (1)–(3) of Table 7 show, we find that all three measures of complexity are associated with the fatality message causing more crashes. The first row shows that a one standard deviation increase in any of our measures of complexity leads to 2.3–3.1% more crashes during treated weeks. The second row shows that for road segments of average complexity, displaying a fatality message

[33] June, July, and August are notable exceptions to this trend. A possible explanation is that these months are when children are out of school, reducing the amount of traffic, with July explained by large increases in vacation travel. Since tourists, both from out-of-state or elsewhere in Texas, are in a new area and so have higher cognitive loads, we expect fatality messages will have a larger effect on them. We have three pieces of evidence for there being more tourists in July. First, Figure A.5 shows that the share of drivers who are from out-of-state, as proxied by the statewide share of drivers in crashes from out-of-state, is higher in July than any other month. Second, according to the 2016 Travel Texas Marketing Plan, summer is the most popular season for non-resident visits to the state (https://gov.texas.gov/travel-texas). Third, Waco, the only major Texas city that provides monthly tourism totals, has the highest number of tourist visits in July for the years 2016–2019 (https://wacoheartoftexas.com/tourism-research-and-statistics/). Unfortunately, due to a lack of power, we are unable to determine whether out-of-state drivers react more strongly to treatment than in-state drivers.

[34] Lane kilometers and average daily vehicle kilometers traveled are measured from the Highway Performance Monitoring System annually and centerline kilometers are measured using Open Street Maps. All three are measured over each segment. Consider the following three examples of a 10 km segment to contrast centerline and lane kilometers: (a) a straight road with four lanes, (b) a Y-shaped road that splits halfway, with all parts having four lanes, and (c) a Y-shaped road that splits halfway where the trunk is four lanes and each branch is two lanes. Segment (a) has 10 centerline km and 40 lane km, segment (b) has 15 centerline km and 60 lane km, and (c) has 15 centerline km and 40 lane km.

causes more crashes (statistically significant when complexity is measured using centerline kilometers). The third and fourth rows show that, as expected, these measures are not associated with an increase in crashes during the week prior to a board meeting in the pre-treatment period.[35]

Fifth, and closely related, the increase in crashes is higher on segments with nearby upstream DMSs. We measure the distance (on the road network) to the nearest upstream DMS, standardize this measure to have a mean of zero and a standard deviation of one, and multiply by minus one so that the measure is increasing in proximity to an upstream DMS. As column (4) of Table 7 shows, fatality messages on DMSs with the average proximity to an upstream DMS are associated with a 1.36% increase in crashes, and that increasing the closeness of the nearest upstream DMS by one standard deviation is associated with an incremental 0.6% increase in crashes. This finding is consistent with three explanations. First, it is consistent with fatality messages having larger effects when drivers face high cognitive loads, as drivers have likely seen multiple DMS messages on these segments. Second, it is consistent with an effect due to repeated exposures, either because it means more drivers have seen the message at least once, distracting multiple drivers, or because seeing a fatality message repeatedly in quick succession increases the salience (and effect) of the message.[36] Finally, an increase in crashes on segments with nearby upstream DMSs is also consistent with treatment mattering, as at least one of these DMSs is likely to have displayed a fatality message, per TxDOT instructions that, even when there are higher priority messages, the fatality message should be displayed on a few DMSs along corridors with a large number of DMSs.[37]

Sixth, fatality messages increase multi-vehicle crashes but not single-vehicle crashes. In Table 8, we separately examine whether multi- and single-vehicle

[35] Appendix Table A.6 reports results using an indicator for whether each complexity measure is above or below the median, rather than using a continuous measure of complexity, and produces similar results.

[36] The probability that a DMS is showing a fatality message conditional on the nearest upstream DMS showing a fatality message is 61%. In comparison, during campaign weeks DMSs show fatality messages 32% of the time.

[37] During campaign weeks, it is 31% more likely that a fatality message is showing on either a DMS or its nearest upstream neighbor, than on just a given DMS.

crashes change the week prior to a board meeting. We find a 1.61% increase in crashes involving multiple vehicles, and an insignificant change in crashes involving single vehicles. As single vehicle crashes are likely a result of large mistakes (e.g., driving off the road), the increase in multi-vehicle crashes suggests that more *small* driving mistakes occur when fatality messages are shown that are plausibly related to distracted driving (e.g., drifting out of lane), rather than driving off the road. An increase in multi-vehicle crashes is also consistent with fatality messages inducing more anxiety, and so being more distracting, when driving conditions could be perceived as more dangerous (i.e., when other vehicles are nearby).[38]

4.2 Alternate hypotheses

There are at least five alternate hypotheses for how fatality messages affect traffic safety. Below we provide evidence inconsistent with each of these alternative hypotheses.

4.2.1 Fatality message helps in the long run

The first alternate hypothesis is that although fatality messages cause more crashes when displayed, they may eventually lead to safer driving. That is, the signs could be similar to a vaccine, with a little immediate harm leading to eventual large benefits. We provide four pieces of evidence that are inconsistent with this hypothesis.

First, if the messages cause individuals to drive safer after they recover from an initial shock, then the worst effects should occur at the start of each campaign week, and the days after the safety campaign should be unusually safe. To test this possibility, we estimate a modified version of equation (2) that allows the effect of the safety campaign to vary by event day i, where $i = 0$ reflects the Monday a week prior to a board meeting (i.e., the first official day of a safety campaign during the treatment period). Figure 13 plots the difference-in-differences coefficient estimates for fourteen event days (-3 through 10), with the dotted lines indicated

[38] Relatedly, a higher incidence of multi-vehicle crashes also suggests that fatality messages impose significant externalities on drivers who are not directly distracted by fatality messages, as these drivers are at risk of being in a crash due to the actions of drivers distracted by the fatality message.

the official first and last day of safety campaign weeks.[39] While the estimates do not rule out the possibility that the treatment effect is declining throughout the week, the estimated treatment effect on Friday is (slightly) higher than on the first Monday. Most importantly, the estimates do not become significantly negative once a campaign week concludes.[40]

Second, if the message "sticks" and encourages safer driving, then repeated exposure to fatality messages (even with extended breaks between viewings) should eventually lead to fewer crashes. We, however, find that the fatality messages continue to increase crashes five years after the launch of this safety campaign (see Figure 10). Furthermore, the increase in crashes that occurs during the year (i.e., from February to January) occurs consistently in each year of our sample period. Figure 14 plots estimates of the effect of the safety campaign by modified calendar quarters.[41] In the leftmost section we find either an insignificant or negative effect of fatality messages on traffic crashes during the first quarter of the year (Feb–Apr) for the years 2013–2017. The pattern is similar for the second quarter. In the third section of Figure 14 we find that all but one of the point estimates are positive during the third quarter, albeit insignificant. The notable exception is that fatality messages resulted in a 4.2% *decrease* in crashes during the first quarter they were first displayed (i.e., the third quarter of 2012). In the rightmost section we find that the point estimates are positive and statistically significant during the fourth quarter for five of the six years in our sample. Figures 10 and 14 suggest that drivers are repeatedly surprised

[39] The effects presented in Figure 13 are relative to the baseline average change in crashes over the remaining days in the month. Because treatment is supposed to end before rush hour the Monday immediately preceding a board meeting, this day is only assigned to treatment in the early morning hours.

[40] In Figure 13 we find economically large effects immediately prior to the start of a safety campaign week (e.g., a 5.9% increase in crashes on the Sunday 11 days prior to a board meeting). This finding is consistent with the evidence of leakage of the fatality message to earlier days and that including this Sunday as part of the safety campaign week significantly increases the estimated effect of fatality messages (see Figure 5 and Table 5). As discussed earlier, this increase seems large relative to the amount of leakage documented in Figure 5. Our interpretation is that Figure 5 underestimates the amount of leakage since Figure 6 shows that the amount of leakage declines over time and 82% of DMS log data is from 2016–2017.

[41] We modify the calendar quarters so that the first quarter starts in February, when the displayed death count resets.

and distracted by fatality messages, particularly messages shown in the last quarter, which contain the largest fatality numbers.

Third, if the message leads people to drive better after the initial shock, then it is possible that while the fatality message causes more crashes near the DMS, there could be a plausible reduction in crashes elsewhere. To test this possibility, we estimate whether the fatality message affects the statewide total number of crashes. We use a difference-in-differences specification similar to (2), where the outcome is the statewide total number of crashes in a given hour and we include a fixed effect for each year-month-day-of-week-hour. Given that our outcome is a statewide total number of crashes during a particular hour, we are unable to control for precipitation. Column (1) of Table 9 reports that during the week prior to a board meeting there are 1.98% more crashes statewide. Further, as columns (2) and (3) show, this effect is concentrated in the number of highway crashes (as opposed to off-highway crashes), although the difference between the two estimates is not statistically significant.[42]

Figure 15 shows that the effect of a campaign week on the statewide crash count follows the same pattern of climbing throughout the year, and that this pattern holds for both on- and off-highway crashes. Appendix Table A.7 shows that these statewide results are robust to winsorizing, quantile regression, taking the inverse hyperbolic sine of the number of crashes, and Poisson regression. Appendix Figure A.6 shows that the increase in statewide crashes is generally present each year since 2012.

Finally, if the fatality message eventually leads to safer driving, then the effect is small relative to other trends. The number of fatalities per vehicle mile traveled increased by 6.2% between 2011 and 2017 (TxDOT Texas Department of Transportation, Texas Department of Transportation).

4.2.2 Additional alternate hypotheses

We consider four other alternate hypotheses for why fatality messages cause more crashes. First, it is possible that any message is distracting and causes crashes,

[42] The statewide effect is driven almost entirely by the fact that the week prior to a board meeting was unusually safe in the pre-treatment period. This is in contrast to our main analysis using DMSs, where we found no difference between the week prior to a board meeting and other weeks in the pre-treatment period (see Figure 8 and Table 5).

reflecting a more general concern faced by traffic engineers about displaying non-essential messages on DMSs. Evidence that the treatment effect varies with the number of reported deaths, however, suggests that the increase in crashes is unlikely a result of displaying any message. Furthermore, we test whether Amber and Silver alerts, whose timing is plausibly exogenous to underlying traffic risk, affect the number of crashes. We find an insignificant increase of 0.50%, further evidence that the content of the fatality message matters.

Second, safety messages may result in more crashes by increasing the variance in drivers' speeds, as research suggests that greater variance in speeds causes crashes.[43] If only some drivers notice the messages and these drivers respond by slowing down, then the variance in speeds would increase and result in more crashes. Without data on drivers' speeds we cannot completely rule out this proposed mechanism, however, given the relatively quick decay of the treatment effect, those who respond by slowing down only do so for a few minutes.

Third, while some drivers may respond to fatality messages by driving more safely, other drivers may decide they can drive faster because some drivers are being more careful (akin to Peltzman (1975)). While we again cannot completely rule out this mechanism, we note that it is inconsistent with the relatively quick decay of the treatment effect.

Finally, it is possible that the reported number of deaths is less than people expect, and that they rationally respond by driving more recklessly. However, several of our findings are inconsistent with drivers rationally updating their beliefs about the risk involved in driving. First, the relatively quick decay of the treatment effect suggests the messages only affect behavior for a short time. Second, the treatment effect persists after five years of treatment, even though there is little new information in the messages.

[43] See Theofilatos and Yannis (2014) for a review, although there is an ongoing debate over this finding.

5 Discussion

The prior section presents evidence that fatality messages are too salient and distract drivers. Part of this evidence includes documenting heterogeneous treatment effects, with larger treatment effects when the message is plausibly more salient or when drivers' cognitive loads are higher. This same evidence suggests that there are times and places where showing fatality messages help. Specifically, fatality messages reduce the number of crashes when the number of reported fatalities is in the bottom quartile and in places where the road network complexity is at least a standard deviation below its respective mean. While these benefits do not outweigh the harm done, they show that behavioral interventions can help if the intervention is not too salient and if they are delivered when individuals' cognitive loads are low.

6 Robustness

We report several robustness tests of our difference-in-differences estimates in Appendix Table A.8. In particular, we show that clustering by segment-year-month reduces the standard error in half, that controlling for rain more flexibly does not affect our results, that not controlling at all for rain doubles our estimated treatment effect, and that not controlling for holidays increases our estimate slightly. Further, we show that the estimated treatment effects are larger when using alternate outcome measures; specifically, using an indicator variable for whether there is any crash or using the log of the number of crashes plus one.[44]

All our estimates so far have assumed that any DMS that exists during our sample, exists for the entire sample. We also test whether our results are robust to limiting the sample to those DMS-months where each DMS exists. To do so, we collect information on when each DMS exists using Google Streetview. As discussed in footnote 17, this data has large gaps, and so for each DMS-month we either know

[44] We do not use count data models (e.g., Poisson regression) because they are incompatible with our extensive fixed effect structure. These models require variation in the outcome within each fixed effect. With our fixed effect structure there are 4–5 observations per fixed effect and so for many fixed effects there is no variation in the outcome.

a DMS exists, know it does not exist, or are unsure. To deal with this uncertainty over when DMSs exist we conduct two robustness tests. First, we limit our sample to those DMS-months where we know the DMS exists, and second, we limit our sample to those DMS-months where the DMS might exist (i.e., we don't know that it doesn't exist). As expected, we find that including DMS-months where there is not an operational DMS attenuates our estimates, with the "must exist" sample leading to a higher point estimate than the "may exist" sample, which itself leads to a higher point estimate than our full sample.

We also test for an effect of the fatality message on fatal crashes. However, as only 0.58% of crashes have a fatality, we are underpowered to detect any effect. Appendix Table A.9 reports our difference-in-differences estimates. The 95% confidence intervals for 3, 5, and 10 km all stretch well past -10 to 10% and Appendix Figure A.7 shows how these estimates vary by quarter, likewise showing large confidence intervals.

We highlight three possible threats to external validity. First, we find that most of the damage is done the first few days the message is displayed (see Figure 13). This implies that in places, like Illinois, where the fatality message is displayed all the time (unless there is a more important message), the effects could be more benign; and in places, like Colorado, where fatality messages are displayed one day per week, the effects could be worse. Second, we find that fatality messages only hurt when the displayed fatality count is large (see Figure 11). Given that Texas leads the nation in traffic fatalities, no other states display such large numbers. If the negative effect of the fatality message depends on the absolute number shown, then it will not have the same negative effect in other states. However, if the negative effect depends on the number shown relative to a state's population, then our results are generalizable to other states. Third, we estimated the effect of being assigned to show a fatality message relative to the status quo usage of DMSs. States vary in how they use DMSs and this could affect the magnitude of the effect. As documented in Table 4, Texas uses its DMSs relatively intensively. If showing any message causes some level of distraction, then the effect of showing a fatality message in another state that typically leaves their signs blank could be even larger.

7 Conclusion

This paper shows that salient, generic, in-your-face safety messages delivered to individuals in the act of driving crowds out more pressing safety concerns, yielding immediate negative and socially undesirable outcomes. Our evidence suggests that even after several years, drivers do not habituate to an intervention that is delivered one week each month. Further, the negative effects of these messages appear contained to the immediate vicinity and time where delivered, inconsistent with any persistence effects.

The effect of displaying a fatality message on crashes is large relative to the simplicity of the intervention. We estimate that showing a fatality message increases the number of crashes over the next 10 km of roadway by 4.5–7.9%. Our estimates suggest that displaying these messages causes an additional 2,600 crashes per year in Texas alone.[45] Furthermore, while we are underpowered to detect an effect on fatal crashes, if we assume a similar percentage change in fatal crashes, then fatality messages cause an additional 16 fatalities per year. Using estimates from Blincoe et al. (2015), these additional crashes have a total social cost of $380 million per year. It is difficult to extrapolate these estimates to other states given varying treatment intensities. However, if we scale the effect by the number of licensed drivers in the 27 treated states, this suggests that across the United States, displaying these messages causes an additional 16,000 crashes and 98 fatalities per year, with a total social cost of $2.3 billion per year.

This evaluation of fatality messages highlights five key lessons. First, and most directly, fatality message campaigns increase the number of crashes, and so ceasing these campaigns is a low-cost way to improve traffic safety. Second, measuring the results of interventions is important, even for simple interventions, as good intentions need not imply good outcomes. Third, individuals do not necessarily habituate to behavioral interventions, increasing confidence that estimated short-run benefits in other studies may persist in the long run. Fourth, generic risk disclosure can affect individual behavior, albeit not necessarily as intended. Finally, behavioral

[45] Appendix A.3 documents how we calculate the number of additional crashes, fatal crashes, and their total social cost.

interventions can fail if they increase individuals' cognitive load to the extent that they crowd out more important considerations. Thus, given behavioral interventions are intentionally designed to be salient and seize attention, the message, delivery, and timing must be carefully designed to avoid the intervention backfiring.

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Figure 1
DMS showing safety message as part of safety campaign

Figure 2
Map of DMSs and weather stations in Texas

Notes: This figure plots the location of DMSs (triangles) and weather stations (circles) in our sample. It also shows our road network.

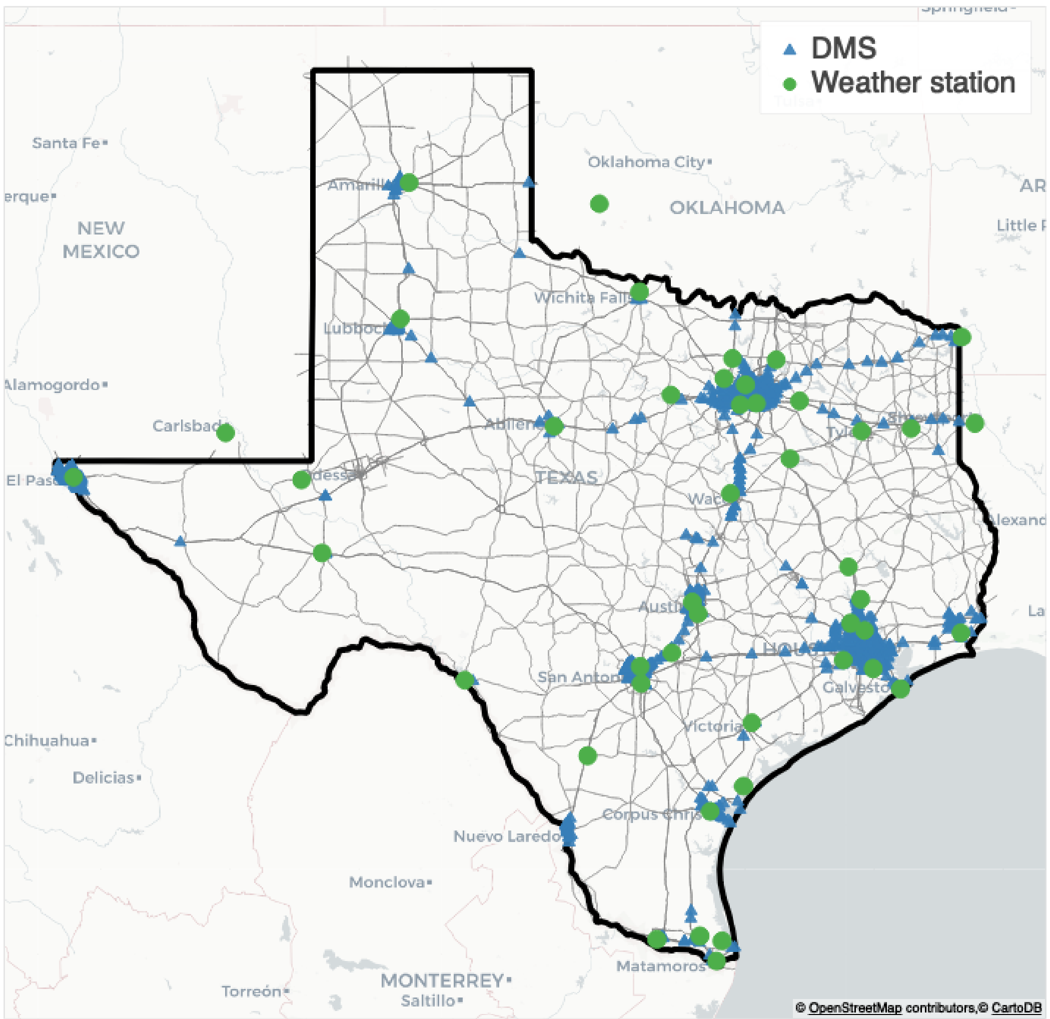


Figure 3
Map of DMSs and weather stations in Houston

Notes: This figure plots the location of DMSs (triangles, pointing in direction of travel; blue for roads traveling south or west, and red for roads traveling north or east) and weather stations (circles) within the Houston area. It also shows our road network.

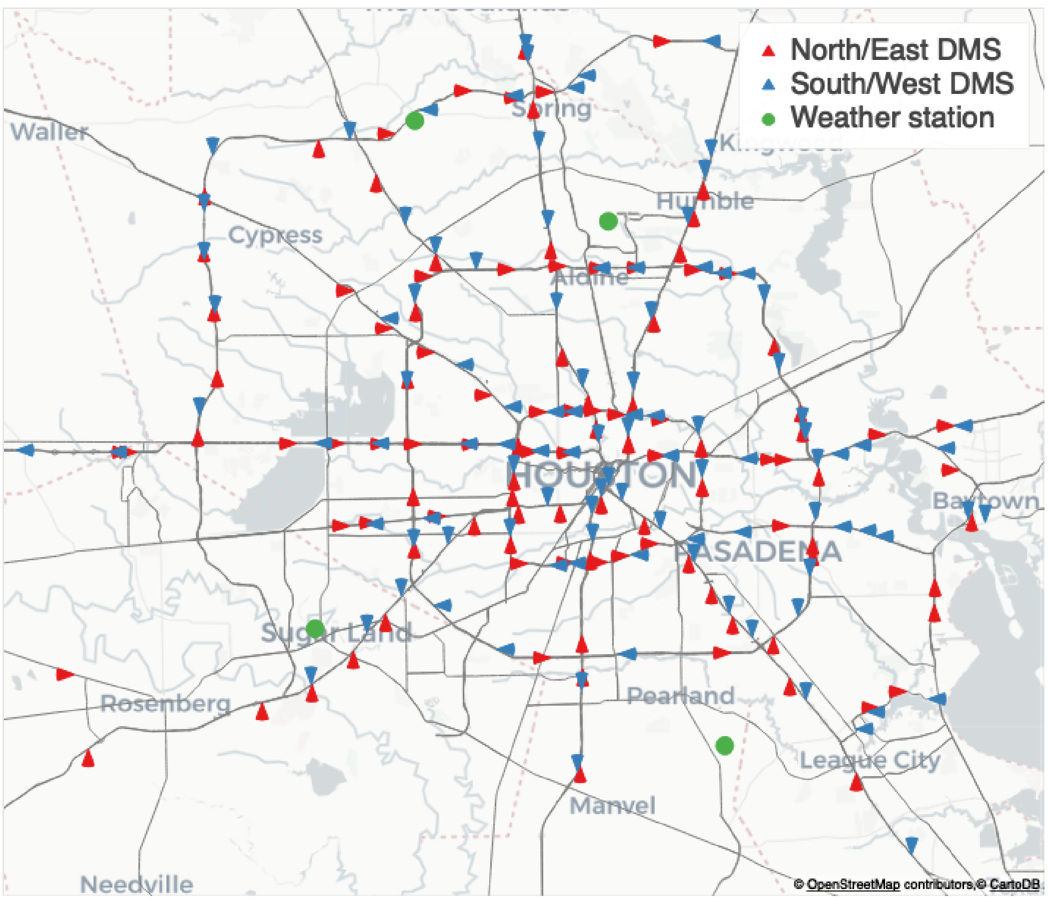


Figure 4
Example of a DMS segment

Notes: This figure depicts four road segments of lengths 1, 3, 5, and 10 kilometers and the location of all crashes (circles) occurring on these road segments associated with the DMS on I-20E near Aledo, TX.

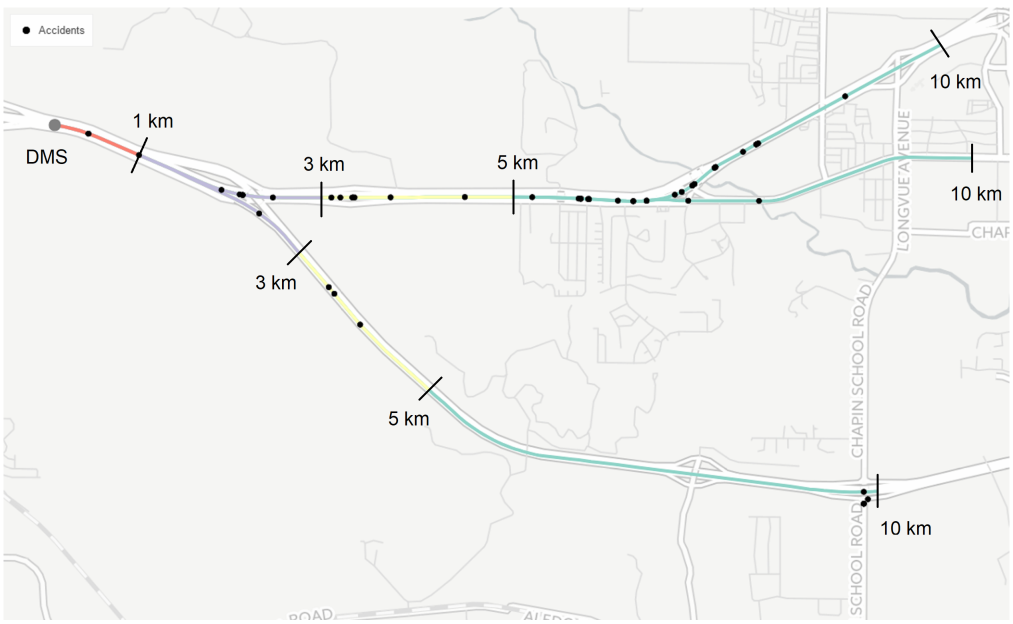


Figure 5
DMS hours displaying fatality messages around campaign weeks

Notes: This figure depicts the percentage of dynamic message signs (DMSs) displaying a fatality message each hour of the day relative to a safety campaign. Day 0 is defined as the Monday the week prior to the TxDOT monthly board meeting. We define rush hour as 7 AM to 9 AM. TxDOT traffic engineers are instructed to run the fatality message beginning after morning peak on Monday and ending before morning peak on the following Monday (see Figure A.1 for one of these official emails). The sample includes DMS log files that cover 41 safety campaigns between August 2012 and December 2017 (see Table 1 Panel B and Table A.3). Darker shaded areas represent a greater percentage of DMSs displaying a fatality message.

Days from awareness campaign

	-6	-5	-4	-3	-2	-1	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
12 AM	0%	0%	0%	1%	4%	7%	8%	38%	41%	39%	36%	35%	42%	29%	6%	4%	4%	3%	3%	3%	1%	1%
1 AM	0%	0%	0%	1%	4%	8%	8%	38%	40%	39%	36%	34%	42%	28%	6%	4%	3%	3%	3%	3%	1%	1%
2 AM	0%	0%	0%	1%	4%	8%	8%	38%	40%	39%	35%	33%	42%	28%	6%	4%	3%	3%	3%	3%	1%	1%
3 AM	0%	0%	0%	1%	4%	7%	8%	38%	40%	40%	36%	33%	43%	26%	6%	4%	3%	3%	3%	3%	1%	1%
4 AM	0%	0%	0%	1%	4%	7%	9%	40%	42%	42%	39%	34%	43%	24%	6%	4%	3%	3%	3%	3%	1%	1%
5 AM	0%	1%	1%	1%	4%	8%	9%	41%	42%	43%	39%	34%	43%	24%	6%	4%	3%	3%	3%	3%	1%	1%
6 AM	1%	1%	1%	1%	4%	8%	8%	32%	32%	32%	30%	34%	42%	21%	6%	4%	2%	2%	2%	3%	1%	1%
7 AM	1%	0%	0%	1%	4%	8%	12%	29%	30%	30%	29%	35%	42%	16%	5%	3%	2%	2%	2%	3%	1%	1%
8 AM	1%	0%	1%	2%	3%	7%	18%	30%	30%	31%	30%	30%	35%	12%	5%	3%	2%	2%	2%	2%	1%	1%
9 AM	1%	1%	1%	2%	3%	7%	29%	35%	34%	35%	34%	31%	36%	11%	5%	4%	2%	2%	2%	2%	1%	1%
10 AM	1%	1%	1%	2%	3%	7%	30%	36%	35%	35%	35%	31%	35%	9%	5%	4%	2%	2%	1%	2%	1%	1%
11 AM	0%	1%	1%	2%	3%	7%	32%	36%	34%	34%	33%	31%	34%	9%	4%	4%	2%	2%	1%	2%	1%	1%
12 PM	1%	1%	1%	3%	3%	7%	31%	36%	34%	33%	33%	32%	34%	9%	4%	4%	2%	2%	2%	2%	1%	1%
1 PM	1%	1%	1%	3%	3%	7%	31%	36%	33%	33%	33%	32%	33%	8%	4%	4%	2%	2%	2%	2%	1%	1%
2 PM	1%	1%	1%	3%	3%	7%	32%	36%	33%	33%	33%	32%	34%	8%	4%	4%	2%	2%	2%	2%	1%	1%
3 PM	1%	1%	1%	3%	3%	7%	32%	36%	32%	33%	32%	31%	34%	7%	4%	3%	2%	2%	2%	2%	1%	1%
4 PM	1%	0%	1%	3%	3%	7%	31%	34%	31%	31%	31%	32%	35%	7%	4%	3%	2%	2%	2%	2%	1%	1%
5 PM	0%	0%	1%	3%	4%	7%	28%	30%	27%	26%	27%	33%	34%	6%	3%	3%	2%	2%	2%	2%	1%	1%
6 PM	1%	0%	0%	3%	4%	7%	30%	31%	30%	27%	28%	34%	35%	6%	3%	3%	2%	2%	2%	2%	1%	1%
7 PM	1%	1%	0%	3%	4%	8%	35%	36%	36%	33%	34%	36%	37%	8%	4%	4%	3%	3%	2%	2%	1%	1%
8 PM	0%	1%	1%	4%	4%	8%	34%	36%	35%	32%	33%	37%	37%	7%	4%	4%	3%	2%	2%	2%	1%	1%
9 PM	0%	1%	1%	4%	4%	8%	34%	34%	34%	32%	32%	37%	37%	7%	4%	4%	3%	2%	2%	2%	1%	1%
10 PM	0%	0%	0%	4%	4%	8%	38%	39%	37%	34%	34%	36%	35%	6%	4%	3%	3%	2%	2%	2%	1%	1%
11 PM	0%	0%	0%	4%	4%	8%	37%	38%	37%	33%	34%	41%	36%	6%	4%	3%	3%	2%	2%	2%	1%	1%

Figure 6
Proportion of hours that DMSs display fatality messages by month

Notes: This figure plots the mean proportion of hours (black circles) and interquartile range (gray bars) that a fatality message was displayed on a DMS within a month during both safety campaign hours (left graph) and non-safety campaign hours (right graph). The data for 2012–2013 is only for the Houston area, while the data for 2016–2017 are for all of Texas (see Table 1 Panel B).

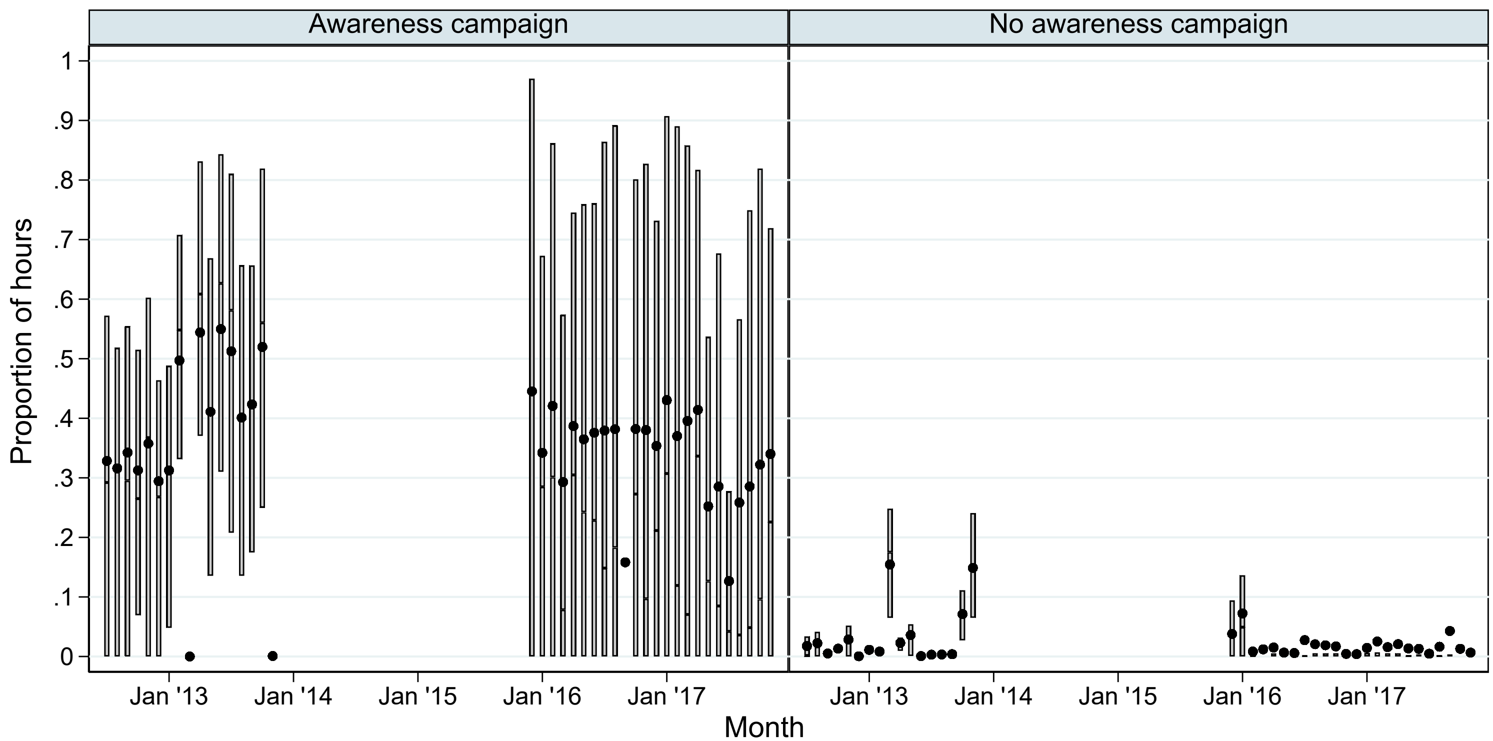


Figure 7
Effect of fatality message on crashes by distance from DMS: univariate

Notes: This figure depicts the percent change in the number of crashes on Texas highways during weeks that precede TxDOT board meetings relative to all other weeks. Highway crashes are measured over hour h of day d over the distances x (relative to DMS s) indicated on the x-axis. The circles plot the difference in the average number of crashes between Monday 9 AM–Monday 7 AM the week prior to a TxDOT board meeting (when fatality messages are instructed to be displayed) and all other hours and the associated 95% confidence intervals (bars). The hollow squares plot the difference in the average number of crashes for the sample to DMSs with no downstream DMS within x kilometers (i.e., for the distance $(1, 4]$ the closest downstream DMS is four or more kilometers away). We scale crash counts by the population average for all segments of the same distance x and multiply by 100. Standard errors are clustered by geography-year-month and are in parentheses, where geography indicates a bin of size x^2 square kilometers that contains the DMS. The sample period is August 2012–December 2017.

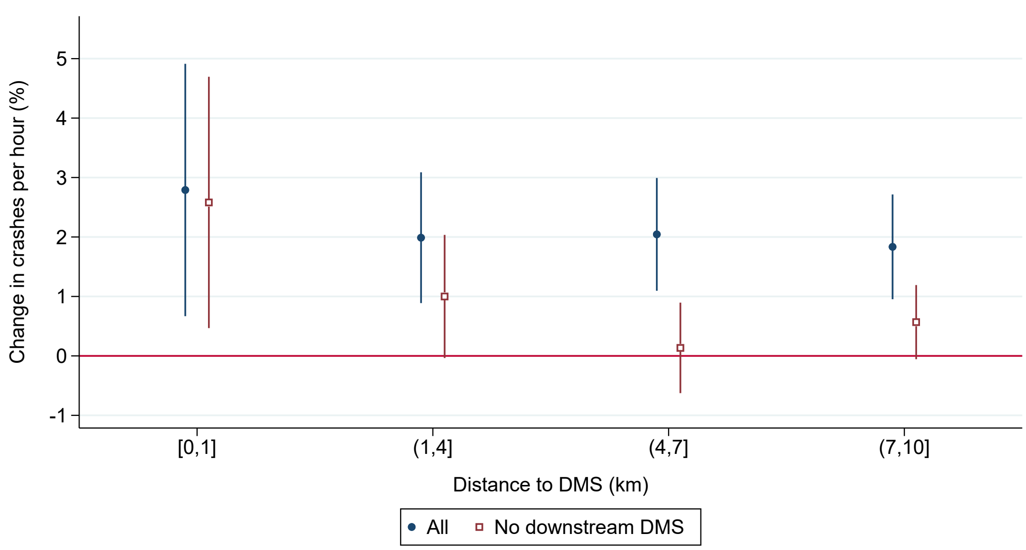
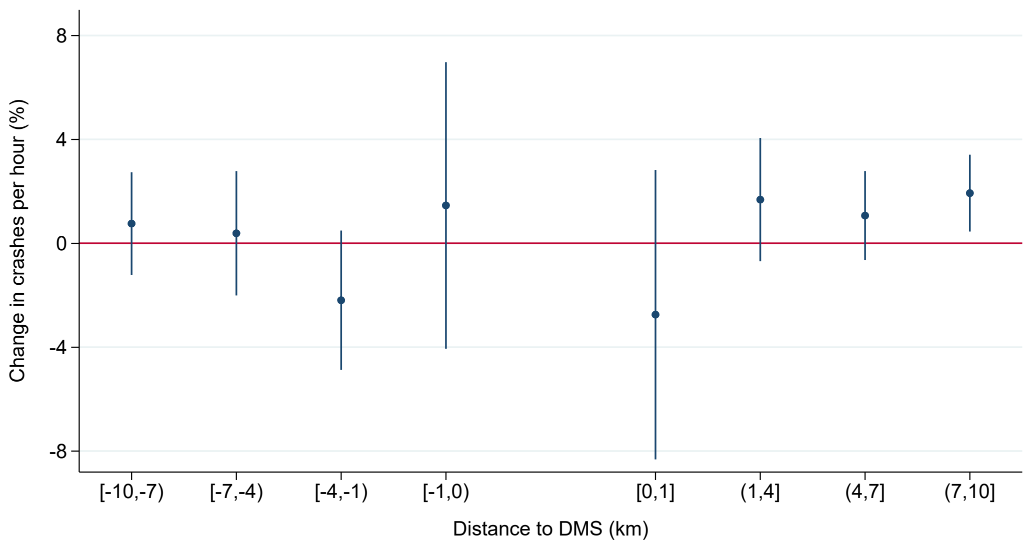


Figure 8
Univariate placebo tests

Notes: This figure depicts the percent change in the number of crashes on Texas highways during weeks that proceed TxDOT board meetings relative to all other weeks. Highway crashes are measured over hour h of day d over the distances x (relative to DMS s) indicated on the x-axis. We scale crash counts by the population average for all segments of the same distance x and multiply by 100. We plot the difference in the average number of scaled crashes between Monday 9 AM–Monday 7 AM the week prior to a TxDOT board meeting and all other hours (circles) and the associated 95% confidence intervals (bars). Standard errors are clustered by geography-year-month and are in parentheses, where geography indicates a bin of size x^2 square kilometers that contains the DMS. Panel A drops all DMSs that are preceded by a DMS within 10 km of driving distance for the sample period August 2012–December 2017, and Panel B analyzes the pre-treatment period January 2010–July 2012.

Panel A: No upstream DMS within 10 km driving distance



Panel B: Pre-treatment period

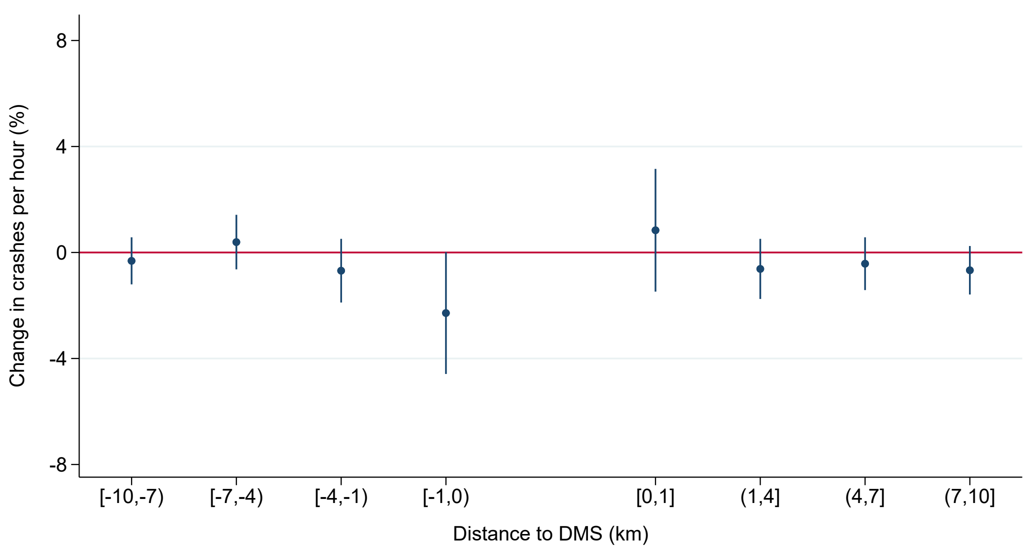


Figure 9
Effect of fatality message on crashes by distance from DMS: multivariate

Notes: This figure depicts estimates of the effect of a campaign week on number of crashes over four distances from a DMS. The dependent variable, $Crash(\%)_{s(x),d,h}$, is the number of crashes occurring on date d during hour h over the distance x (relative to DMS s) indicated on the x-axis. We scale crash counts by the population average for all segments of the same distance x and multiply by 100. The circles plot the δ coefficient on $Board\ meeting_{d,h}$, an indicator variable for whether day d and hour h fall within Monday 9 AM–Monday 7 AM the week prior to a board meeting, for the full sample. The hollow squares plot the δ coefficient from a regression which restricts the sample to DMSs with no downstream DMS within x kilometers (i.e., for the distance $(1, 4]$ the closest downstream DMS is four or more kilometers away). The bars show 95% confidence intervals. We also include but do not plot indicators for whether either trace precipitation or more than trace precipitation was measured on segment s during hour h, using data from the closest weather station, as well as segment-year-month-day-of-week-hour and holiday fixed effects. Standard errors are clustered by geography-year-month, where geography indicates a bin of size x^2 square kilometers that contains the DMS. The sample period is August 2012–December 2017.

$$\begin{aligned}\text{Crash } (\%)_{s(x),d,h} = {} & \delta \cdot \text{Board meeting}_{d,h} + \beta_1 \cdot \text{Trace precipitation}_{s,d,h} \\ & + \beta_2 \cdot \text{Precipitation}_{s,d,h} + \gamma_{s,m(d),dow(d),h} + \zeta_{holiday} + \epsilon_{s,d,h}\end{aligned}$$

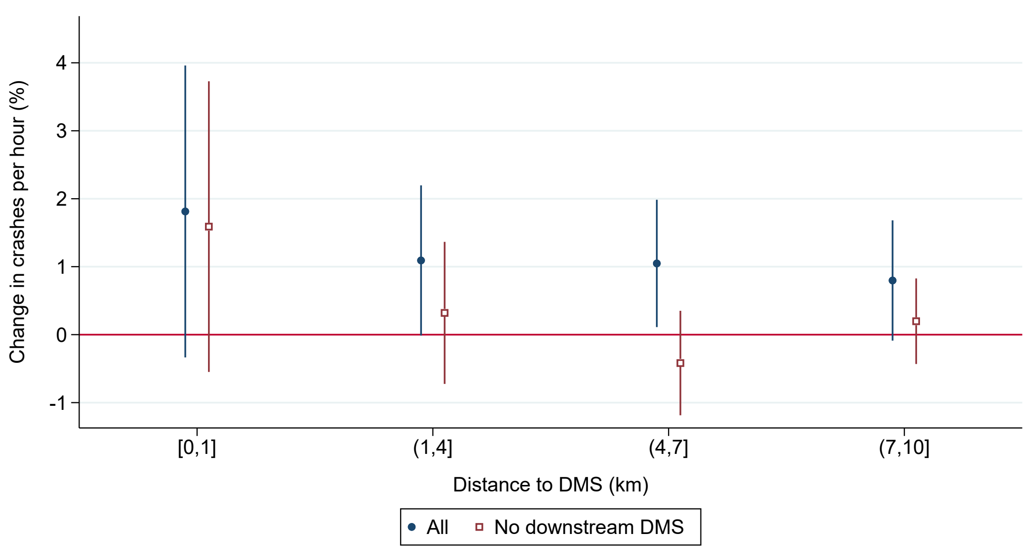


Figure 10
Effect of fatality messages on crashes by year

Notes: This figure depicts the δ_i coefficient estimates (circles) and the associated 95% confidence intervals (bars) from the regression below which allows the treatment effect to vary by year for the period January 2010–December 2017. The dependent variable, $Crash\,(\%)_{s(10\ km),d,h}$, is the scaled number of crashes occurring on day d during hour h over the 10 km downstream of DMS s; $Board\,meeting_{d,h}$ is an indicator variable for whether day d and hour h fall within Monday 9 AM–Monday 7 AM the week prior to a board meeting; and $Year_{d,i}$ is an indicator variable if day d is in year i, $i \in \{2010, 2011, \ldots, 2016, 2017\}$. We also include but do not plot indicators for whether either trace precipitation or more than trace precipitation was measured on segment s during hour h as well as segment-year-month-day-of-week-hour and holiday fixed effects. Standard errors are clustered by geography-year-month bins, where geography bins are defined as the 10^2 square kilometers containing the DMS. The dotted vertical line indicates that treatment started in August 2012.

$$\begin{aligned}\text{Crash }(\%)_{s(10\ km),d,h} = & \sum_{i\in\{2010,\ldots,2017\}} \delta_i \cdot \text{Board meeting}_{d,h} \cdot \text{year}_{d,i} \\ & + \beta_1 \cdot \text{Trace precipitation}_{s,d,h} + \beta_2 \cdot \text{Precipitation}_{s,d,h} \\ & + \gamma_{s,m(d),dow(d),h} + \zeta_{holiday} + \epsilon_{s,d,h}\end{aligned}$$

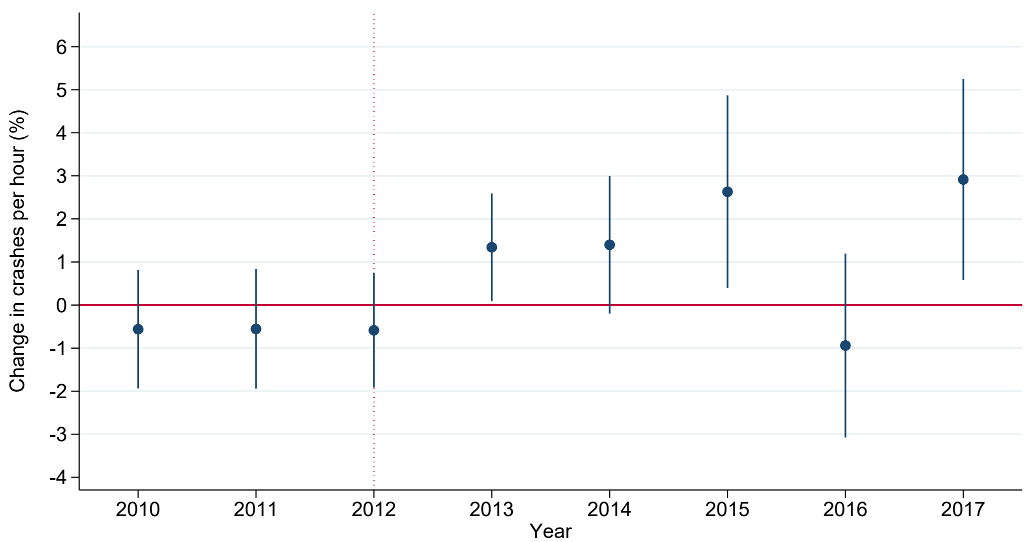


Figure 11
Effect of fatality messages on crashes by YTD death quartile

Notes: This figure depicts the δ_i coefficient estimates (circles) and the associated 95% confidence intervals (bars) from the regression below which allows the treatment effect to vary by the year-to-date (YTD) number of deaths on Texas roads. The dependent variable, $Crash\,(\%)_{s(10\ km),d,h}$, is the scaled number of crashes occurring on day d during hour h over the 10 km downstream of DMS s; $Board\,meeting_{d,h}$ is an indicator variable for whether day d and hour h fall within Monday 9 AM–Monday 7 AM the week prior to a board meeting; $YTD\,quartile_{d,i}$ is an indicator if on day d the YTD number of deaths was in quartile i; and $post_d$ is an indicator for observations after August 1, 2012. We also include but do not plot indicators for whether either trace precipitation or more than trace precipitation was measured on segment s during hour h (and their interactions with $post_d$), as well as segment-year-month-day-of-week-hour and holiday fixed effects. Standard errors are clustered by geography-year-month bins, where geography bins are defined as the 10^2 square kilometers containing the DMS. The sample period is January 2010–December 2017.

$$
\begin{aligned}
\text{Crash }(\%)_{s(10\ km),d,h} = & \sum_{i\in\{\text{Quartile }1,\ldots,\text{Quartile }4\}} \delta_i \cdot \text{Board meeting}_{d,h} \cdot \text{YTD quartile}_{d,i} \cdot \text{post}_d \\
& + \sum_{i\in\{\text{Quartile }1,\ldots,\text{Quartile }4\}} \beta_{1,i} \cdot \text{Board meeting}_{d,h} \cdot \text{YTD quartile}_{d,i} \\
& + \beta_2 \cdot \text{Trace precipitation}_{s,d,h} + \beta_3 \cdot \text{Trace precipitation}_{s,d,h} \cdot \text{post}_d \\
& + \beta_4 \cdot \text{Precipitation}_{s,d,h} + \beta_5 \cdot \text{Precipitation}_{s,d,h} \cdot \text{post}_d \\
& + \gamma_{s,m(d),dow(d),h} + \zeta_{holiday} + \epsilon_{s,d,h}
\end{aligned}
$$

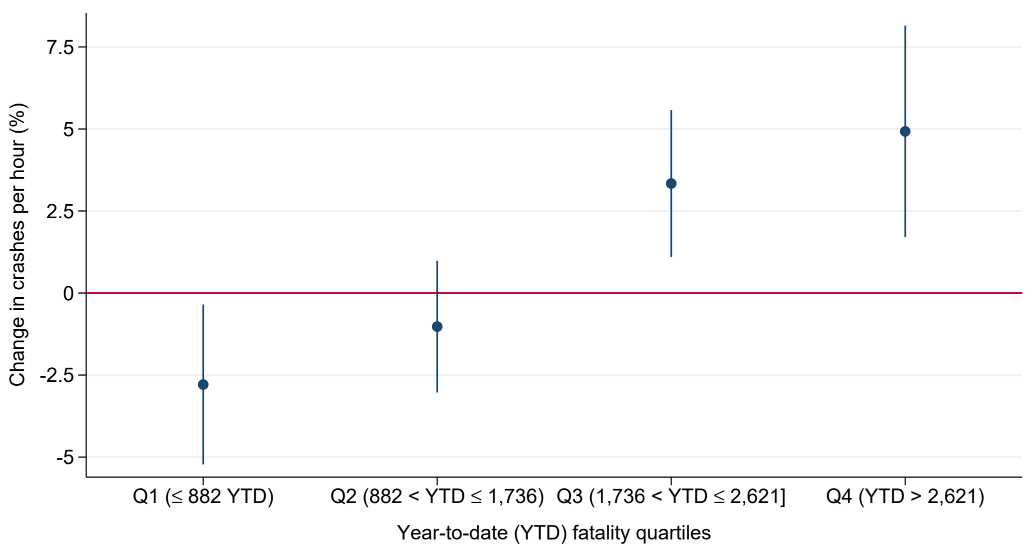


Figure 12
Effect of fatality messages on crashes by calendar month

Notes: This figure depicts the δi coefficient estimates (circles) and the associated 95% confidence intervals (bars) from the regression below which allows the treatment effect to vary by calendar month. The dependent variable, $Crash\,(\%)_{s(10\ km),d,h}$, is the scaled number of crashes occurring on day d during hour h over the 10 km downstream of DMS s; $Board\,meeting_{d,h}$ is an indicator variable for whether day d and hour h fall within Monday 9 AM–Monday 7 AM the week prior to a board meeting; $month_{d,i}$ as an indicator if day d occurs during calendar month i; and $post_d$ is an indicator for observations after August 1, 2012. We also include but do not plot indicators for whether either trace precipitation or more than trace precipitation was measured on segment s during hour h (and their interactions with $post_d$), as well as segment-year-month-day-of-week-hour and holiday fixed effects. Standard errors are clustered by geography-year-month bins, where geography bins are defined as the 10^2 square kilometers containing the DMS. The sample period is January 2010–December 2017.

$$\begin{aligned}
\text{Crash } (\%)_{s(10\ km),d,h} = & \sum_{i\in\{\text{Jan},\ldots,\text{Dec}\}} \delta i \cdot \text{Board meeting}_{d,h} \cdot \text{month}_{d,i} \cdot \text{post}_d \\
& + \sum_{i\in\{\text{Jan},\ldots,\text{Dec}\}} \beta_{1,i} \cdot \text{Board meeting}_{d,h} \cdot \text{month}_{d,i} \\
& + \beta_2 \cdot \text{Trace precipitation}_{s,d,h} + \beta_3 \cdot \text{Trace precipitation}_{s,d,h} \cdot \text{post}_d \\
& + \beta_4 \cdot \text{Precipitation}_{s,d,h} + \beta_5 \cdot \text{Precipitation}_{s,d,h} \cdot \text{post}_d \\
& + \gamma_{s,m(d),dow(d),h} + \zeta_{holiday} + \epsilon_{s,d,h}
\end{aligned}$$

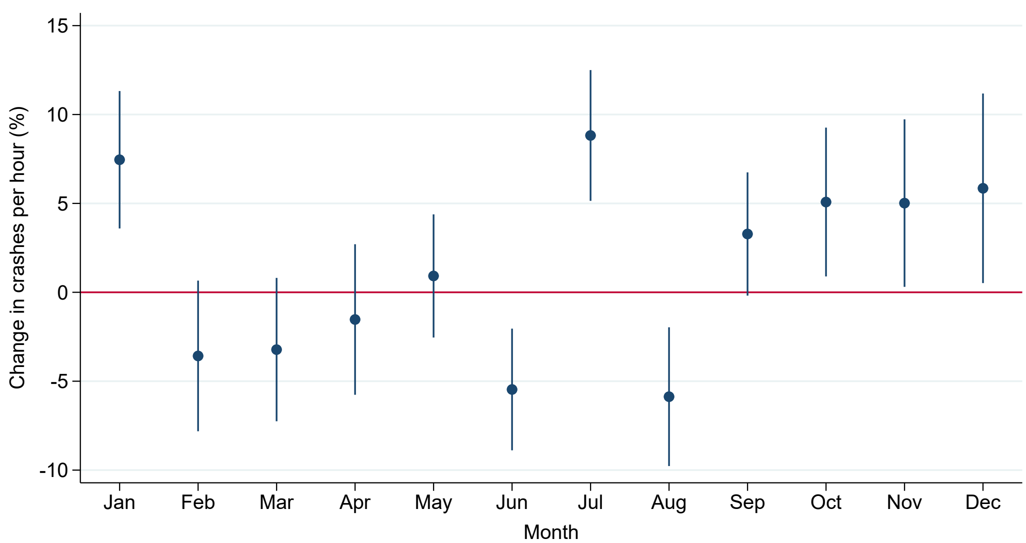


Figure 13
Effect of fatality messages on crashes by event day

Notes: This figure depicts the δi coefficient estimates (circles) and the associated 95% confidence intervals (bars) from the regression below which allows the treatment effect to vary by event day. The dependent variable, $Crash\,(\%)_{s(10\;km),d,h}$, is the scaled number of crashes occurring over the 10 kilometers after DMS s on day d during hour h; $Board\,meeting\,2\,week_d$ is an indicator variable for whether day d falls within the two week period ending on a board meeting day; $Event\,day_i$ is an indicator for each of the fourteen days during the $Board\,meeting\,2\,week_{d,h}$ window (where $i = 0$ is the Monday a week prior to a board meeting and $i \in \{-3, -2, \ldots, 9, 10\}$); and $post_d$ is an indicator for observations after August 1, 2012. We also include but do not plot indicators for whether either trace precipitation or more than trace precipitation was measured on segment s during hour h (and their interactions with $post_d$), as well as segment-year-month-day-of-week-hour and holiday fixed effects. Standard errors are clustered by geography-year-month bins, where geography bins are defined as the 10^2 square kilometers containing the DMS. The sample period is January 2010–December 2017.

$$\begin{aligned}
\text{Crash } (\%)_{s(10\;km),d,h} = & \sum_{i\in\{-3,-2,\ldots,9,10\}} \delta i \cdot \text{Board meeting 2 week}_d \cdot \text{Event day}_i \cdot \text{post}_d \\
& + \sum_{i\in\{-3,-2,\ldots,9,10\}} \beta_{1,i} \cdot \text{Board meeting 2 week}_d \cdot \text{Event day}_i \\
& + \beta_2 \cdot \text{Trace precipitation}_{s,d,h} + \beta_3 \cdot \text{Trace precipitation}_{s,d,h} \cdot \text{post}_d \\
& + \beta_4 \cdot \text{Precipitation}_{s,d,h} + \beta_5 \cdot \text{Precipitation}_{s,d,h} \cdot \text{post}_d \\
& + \gamma_{s,m(d),dow(d),h} + \zeta_{holiday} + \epsilon_{s,d,h}
\end{aligned}$$

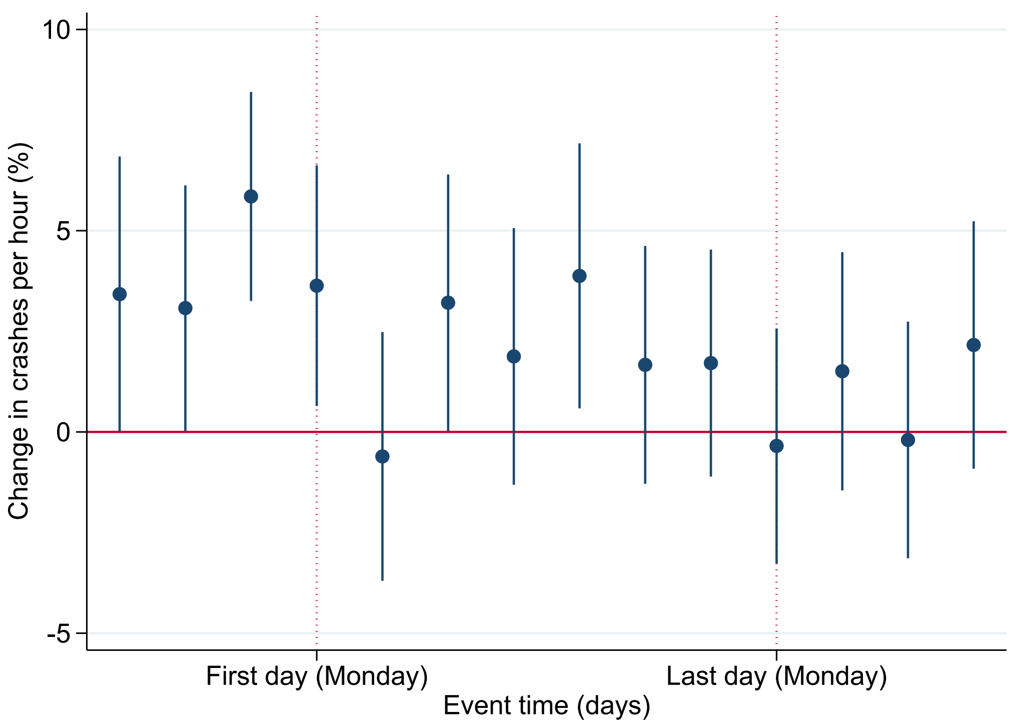


Figure 14
Effect of fatality messages on crashes by calendar quarter

Notes: This figure depicts the δ_i coefficient estimates (circles, grouped by quarter) and the associated 95% confidence intervals (bars) from the regression below which allows the treatment effect to vary by calendar quarter. The dependent variable, $Crash\,(\%)_{s(10\ km),d,h}$, is the scaled number of crashes occurring on day d during hour h over the 10 km downstream of DMS s; $Board\,meeting_{d,h}$ is an indicator variable for whether day d and hour h fall within Monday 9 AM–Monday 7 AM the week prior to a board meeting; and $Qtr_{d,i}$ as an indicator variable for whether day d occurs during the modified calendar quarter i (starting with the three-month period August through October 2012 and ending with the two-month period November through December 2017). We also include but do not plot indicators for whether either trace precipitation or more than trace precipitation was measured on segment s during hour h as well as segment-year-month-day-of-week-hour and holiday fixed effects. Standard errors are clustered by geography-year-month bins, where geography bins are defined as the 10^2 square kilometers containing the DMS. The sample period is August 2012–December 2017.

$$\begin{aligned}\text{Crash }(\%)_{s(10\ km),d,h} = & \sum_{i\in\{\text{Q3 '10},\ldots,\text{Q4 '17}\}} \delta_i \cdot \text{Board meeting}_{d,h} \cdot \text{Qtr}_{d,i} \\ & + \beta_1 \cdot \text{Trace precipitation}_{s,d,h} + \beta_2 \cdot \text{Precipitation}_{s,d,h} \\ & + \gamma_{s,m(d),dow(d),h} + \zeta_{holiday} + \epsilon_{s,d,h}\end{aligned}$$

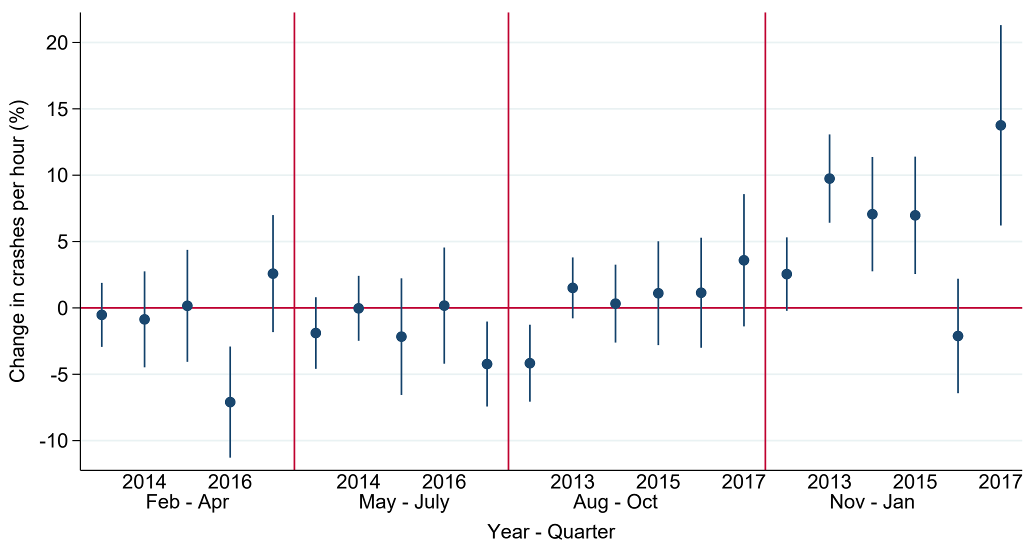


Figure 15
Effect of fatality messages on statewide crashes by calendar quarter

Notes: This figure depicts the δi coefficient estimates (circles, diamonds, and squares) and standard error bars from the regressions below which allow the effect of campaign weeks to vary by calendar quarter. Specifically, the dependent variable is the number of crashes occurring statewide (circles), statewide on the highway system (diamonds), or statewide off the highway system (squares) during hour h of day d, scaled by the population average and multiplied by 100. $Board\,meeting_{d,h}$ is an indicator variable for whether day d and hour h fall within Monday 9 AM–Monday 7 AM the week prior to a board meeting; $Qtr_{d,i}$ are indicator variables equal to one if day d occurs during calendar quarter i; and $post_d$ is an indicator for observations after August 1, 2012. We include but do not plot year-month-day-of-week-hour and holiday fixed effects. Standard errors are clustered by year-month. The sample period is January 2010–December 2017.

$$\begin{aligned}\text{Statewide crash (\%)}_{d,h} = & \sum_{i\in\{\text{Q1},\ldots,\text{Q4}\}} \delta_i \cdot \text{Board meeting}_{d,h} \cdot \text{Qtr}_{d,i} \cdot \text{post}_d \\ & + \sum_{i\in\{\text{Q1},\ldots,\text{Q4}\}} \beta_i \cdot \text{Board meeting}_{d,h} \cdot \text{Qtr}_{d,i} \\ & + \gamma_{m(d),dow(d),h} + \zeta_{holiday} + \epsilon_{d,h}\end{aligned}$$

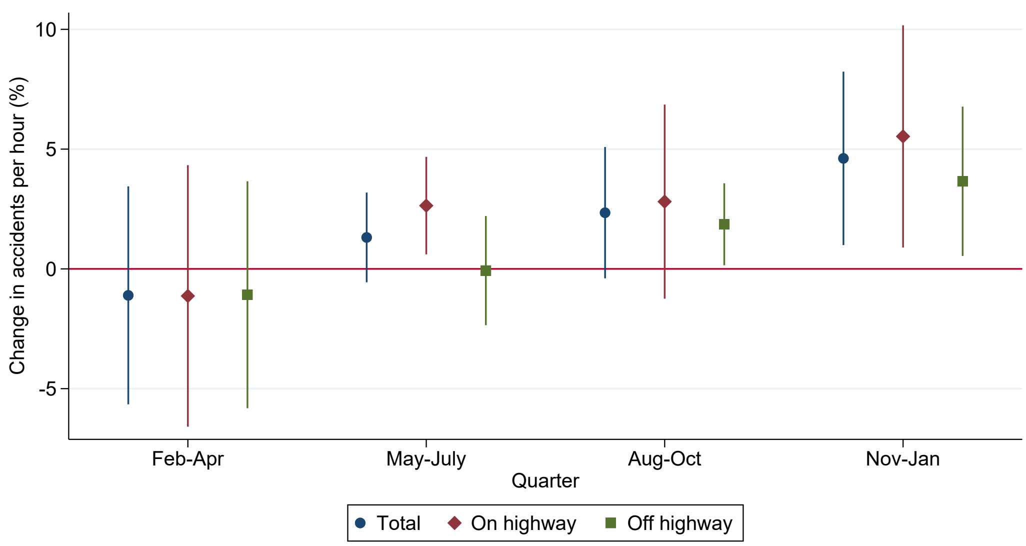


Table 1: DMS sample composition

Notes: This table presents sample summary statistics for key aspects for the entire sample of Texas DMS data. Column (1) presents info for the "full" sample from January 1, 2012 through December 31, 2017; column (2) for the "pre-treatment" sample from Jan 1, 2012 through July 31, 2012; and columns (3) and (4) for the "treatment" sample from Aug 1, 2012 through Dec 31, 2017. Column (4) presents info for the subsample for which we have DMS message log files during the treatment sample.

	Full	Pre-treatment	Treatment	
			All	DMS messages
	(1)	(2)	(3)	(4)
Texas districts	23	23	23	22
Highways	74	74	74	73
DMSs	886	886	886	830
Year-Months	96	31	65	41
DMS-Year-Months	85,056	27,466	57,590	21,438
Date-Hours	70,113	22,627	47,486	29,816
Observations	62,118,334	20,047,441	42,070,893	15,108,198

Table 2: Summary statistics

Notes: This table reports summary statistics measured at the hourly level for the entire sample period. The last three rows are statewide variables while all others are segment specific. See Appendix Table A.2 for detailed variable definitions.

	N	Mean	Median	SD
DMS-hour measures				
No precipitation	62,118,334	0.92	1	0.27
Trace precipitation	62,118,334	0.04	0	0.19
Precipitation	62,118,334	0.04	0	0.20
Crashes 3 km (10^{-3})	62,118,334	6.23	0	81.04
Crashes 5 km (10^{-3})	62,118,334	14.54	0	124.99
Crashes 10 km (10^{-3})	62,118,334	51.27	0	242.22
Fatal crashes 3 km (10^{-3})	62,118,334	0.04	0	5.93
Fatal crashes 5 km (10^{-3})	62,118,334	0.08	0	9.05
Fatal crashes 10 km (10^{-3})	62,118,334	0.30	0	17.25
Multi-vehicle crashes 10 km (10^{-3})	62,118,334	44.41	0	224.84
Single vehicle crashes 10 km (10^{-3})	62,118,334	6.86	0	84.64
Fatality message (% of hour)	16,208,397	0.08	0	0.27
Centerline km	62,118,334	42.48	33.89	31.91
Lane km	54,017,004	102.91	78	87.43
VKT (10^3)	54,017,004	1,373	925	1,308
Nearest upstream DMS km	62,048,221	13.87	6	29.08
Statewide measures (per hour)				
Crashes statewide	70,127	62.04	57	38.74
Highway crashes statewide	70,127	31.66	28	20.72
Off highway crashes statewide	70,127	30.38	28	19.16

Table 3: Effect of safety campaign on probability of showing a fatality message

Notes: This table estimates the determinants of displaying a fatality message. The dependent variable, *Fatality message*$_{s,d,h}$, is the percent of hour h on day d that the DMS at the start of segment s displayed a fatality message. We regress this variable on *Board meeting*$_{d,h}$, an indicator equal to one if the date-hour observation occurs during the week prior to a Texas Department of Transportation board meeting (where the week prior is defined as starting at 9 AM on Monday and ending at 7 AM on the following Monday). We also include as a control an indicator if trace precipitation or precipitation was measured during on segment s during hour h, using data from the closest weather station. We also include segment-year-month-day-of-week-hour and holiday fixed effects. Standard errors are clustered by segment-year-month and are in parentheses, and *, **, *** indicate 10%, 5%, and 1% two-tailed statistical significance, respectively.

$$\begin{aligned}\text{Fatality message}_{s,d,h} = \delta \cdot \text{Board meeting}_{d,h} + \beta_1 \cdot \text{Trace precipitation}_{s,d,h} \\ + \beta_2 \cdot \text{Precipitation}_{s,d,h} + \gamma_{s,m(d),dow(d),h} + \zeta_{holiday} + \epsilon_{s,d,h}\end{aligned}$$

	Fatality message
Board meeting	0.302***
	(0.002)
Trace precipitation	-0.004***
	(0.001)
Precipitation	-0.014***
	(0.001)
Observations	15,108,198
Adj R-squared	0.25
S-Y-M-D-H FE	Yes
Holiday FE	Yes

Table 4: DMS messages summary statistics

Notes: This table reports summary statistics on DMS messages over hourly (sixty minute) intervals. Columns (1)–(3) show means for the group specified by the column headings, and column (4) reports the differences during weeks prior to a scheduled board meeting ("No" and "Yes" groups). See Appendix Table A.2 for detailed variable definitions. Standard deviations are in brackets, standard errors are in parentheses, and *, **, *** indicate differences that are statistically significant at the 10%, 5%, and 1% level, respectively.

	Treatment period			
		Board meeting		
	All	No	Yes	Difference
	(1)	(2)	(3)	(4)
Fatality message minutes	5.2	1.2	19.3	18.2***
	[16.7]	[8.2]	[27.5]	(0.02)
Non-safety message minutes	36.4	38.8	28.1	-10.7***
	[28.8]	[28.2]	[29.3]	(0.02)
Travel time minutes	12.2	12.4	11.3	-1.1***
	[23.4]	[23.6]	[22.7]	(0.01)
Blank minutes	18.4	20.1	12.6	-7.5***
	[27.2]	[27.9]	[24.0]	(0.02)
Amber alert minutes	2.9	2.9	3.0	0.1***
	[12.6]	[12.5]	[12.7]	(0.01)
Observations	15,108,198	11,716,999	3,391,199	15,108,198

Table 5: Effect of fatality messages on crashes

Notes: This table reports estimates of the effect of campaign weeks on traffic crashes. The sample period is Jan 1, 2010 through Dec 31, 2017. The dependent variable is the number of crashes occurring on highway segment s of length x kilometers on date d during hour h, scaled by the population average for all segments of length x and multiplied by 100. Highway segments begin at each dynamic message sign (DMS) located on a highway and continue for x kilometers of highway driving distance, where $x \in \{3, 5, 10\}$, and are denoted in the column headers. In columns (1) through (3) we use as our primary right-hand side variable $Board\ meeting_{d,h}$, an indicator variable for whether day d and hour h fall within Monday 9 AM–Monday 7 AM the week prior to a board meeting. In columns (4) through (6) we use $Board\ meeting\text{-}Sunday$, an indicator variable where campaign weeks are defined to begin Sunday at 12 AM and end the following Monday at 7 AM. $Post_d$ indicates observations after August 1, 2012. We include but do not tabulate indicators for whether either trace precipitation or more than trace precipitation was measured on segment s during hour h, using data from the closest weather station ($Trace\ precipitation_{s,d,h}$ and $Precipitation_{s,d,h}$, respectively), and interactions between these measures and $post_d$. We also include segment-year-month-day-of-week-hour and holiday fixed effects. Standard errors are clustered by geography-year-month and are in parentheses, where geography indicates a bin of size x^2 square kilometers that contains the DMS, and *, **, *** indicate 10%, 5%, and 1% two-tailed statistical significance, respectively.

$$
\begin{aligned}
\text{Crash (\%)}_{s(x),d,h} = {} & \delta \cdot \text{Board meeting}_{d,h} \cdot \text{Post}_d + \beta_1 \cdot \text{Board meeting}_{d,h} \\
& + \beta_2 \cdot \text{Trace precipitation}_{s,d,h} + \beta_3 \cdot \text{Trace precipitation}_{s,d,h} \cdot \text{Post}_d \\
& + \beta_4 \cdot \text{Precipitation}_{s,d,h} + \beta_5 \cdot \text{Precipitation}_{s,d,h} \cdot \text{Post}_d \\
& + \gamma_{s,m(d),dow(d),h} + \zeta_{holiday} + \epsilon_{s,d,h}
\end{aligned}
$$

	Crashes per hour (%)					
	Board meeting			Board meeting–Sunday		
	3 km (1)	5 km (2)	10 km (3)	3 km (4)	5 km (5)	10 km (6)
Board meeting × post	1.19	1.54**	1.36**	2.46***	2.37***	2.17***
	(0.86)	(0.68)	(0.60)	(0.82)	(0.64)	(0.56)
Board meeting	0.35	-0.25	-0.33	-0.34	-0.59	-0.54
	(0.63)	(0.48)	(0.43)	(0.60)	(0.47)	(0.41)
Observations	62,118,334	62,118,334	62,118,334	62,118,334	62,118,334	62,118,334
Adj R-squared	0.02	0.03	0.08	0.02	0.03	0.08
Rain & interactions	Yes	Yes	Yes	Yes	Yes	Yes
S-Y-M-D-H FE	Yes	Yes	Yes	Yes	Yes	Yes
Holiday FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 6: Effect of fatality messages on crashes: instrumental variables results

Notes: This table reports two-sample instrumental variables estimates of the effect of showing a fatality message on traffic crashes. We use campaign weeks as an instrumental variable for showing a fatality message. Columns (1) through (3) show results when campaign weeks are defined to begin on Monday at 9 AM, and columns (4) through (6) show results when campaign weeks are defined to begin Sunday at 12 AM. The dependent variable is the number of crashes occurring on highway segment s of length x kilometers on date d during hour h, scaled by the population average for all segments of length x and multiplied by 100. The first stage is estimated in the treatment period only using segments for which we have message data. The second stage is run on the entire sample (including the sample used in the first stage). The first- and second-stage regressions are below, where $\widehat{Fatality\ message}$ is the predicted values from the first stage. Standard errors are calculated by bootstrapping 200 times and are clustered by geography-year-month, where geography indicates a bin of size x^2 square kilometers that contains the DMS. The entire process of normalizing the dependent variable, estimating the first stage, generating the predicted values, and estimating the second stage is redone for each bootstrap iteration. Standard errors are in parentheses; *, **, *** indicate 10%, 5%, and 1% two-tailed statistical significance, respectively.

$$\begin{aligned}
\text{Fatality message}_{s,d,h} &= \beta_1 \cdot \text{Board meeting}_{d,h} + \beta_2 \cdot \text{Trace precipitation}_{s,d,h} \\
&+ \beta_3 \cdot \text{Precipitation}_{s,d,h} + \gamma_{s,m(d),dow(d),h} + \zeta_{holiday} + \epsilon_{s,d,h} \\
\text{Crash (\%)}_{s(x),d,h} &= \delta \cdot \widehat{\text{Fatality message}}_{s,d,h} \cdot \text{Post}_d + \beta_1 \cdot \widehat{\text{Fatality message}}_{s,d,h} \\
&+ \beta_2 \cdot \text{Trace precipitation}_{s,d,h} + \beta_3 \cdot \text{Trace precipitation}_{s,d,h} \cdot \text{Post}_d \\
&+ \beta_4 \cdot \text{Precipitation}_{s,d,h} + \beta_5 \cdot \text{Precipitation}_{s,d,h} \cdot \text{Post}_d \\
&+ \gamma_{s,m(d),dow(d),h} + \zeta_{holiday} + \epsilon_{s,d,h}
\end{aligned}$$

	Crashes per hour (%)					
	Board meeting			Board meeting–Sunday		
	3 km (1)	5 km (2)	10 km (3)	3 km (4)	5 km (5)	10 km (6)
$\widehat{\text{Fatality message}}$ × post	3.92	5.08**	4.50**	8.96***	8.65***	7.93***
	(2.78)	(2.25)	(1.98)	(2.90)	(2.28)	(2.32)
$\widehat{\text{Fatality message}}$	1.16	-0.84	-1.08	-1.25	-2.16	-1.95
	(2.05)	(1.52)	(1.44)	(2.06)	(1.66)	(1.48)
Observations	62,118,334	62,118,334	62,118,334	62,118,334	62,118,334	62,118,334
Rain & interactions	Yes	Yes	Yes	Yes	Yes	Yes
Y-M-D-H FE	Yes	Yes	Yes	Yes	Yes	Yes
Holiday FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 7: Effect of fatality messages on crashes: segment characteristics

Notes: This table estimates how effect of campaign weeks on traffic crashes varies by segment characteristics. The dependent variable is the number of crashes occurring over the 10 kilometers downstream of DMS s on date d during hour h, scaled by the population average for all segments and multiplied by 100. $Board\ meeting_{d,h}$ indicates whether day d and hour h falls within Monday 9 AM–Monday 7 AM the week prior to a board meeting, $Post_d$ indicates observations after August 1, 2012, and $Measure$ is one of the following characteristics of segment s (as indicated in column header): $Centerline\ km$, $Lane\ km$, VKT, and $Upstream * (-1)$. See Table 5 for additional details and Appendix Table A.2 for detailed variable definitions. We include segment-year-month-day-of-week-hour and holiday fixed effects. Standard errors are clustered by geography-year-month and are in parentheses, where geography indicates a bin of size x^2 square kilometers that contains the DMS, and *, **, *** indicate 10%, 5%, and 1% two-tailed statistical significance, respectively.

$$
\begin{aligned}
\text{Crash (\%)}_{s(10),d,h} = {} & \delta_1 \cdot \text{Board meeting}_{d,h} \cdot \text{Measure}_s \cdot \text{Post} \\
& + \delta_2 \cdot \text{Board meeting}_{d,h} \cdot \text{Post} \\
& + \beta_1 \cdot \text{Board meeting}_{d,h} \cdot \text{Measure}_s \\
& + \beta_2 \cdot \text{Board meeting}_{d,h} \\
& + \beta_3 \cdot \text{Trace precipitation}_{s,d,h} + \beta_4 \cdot \text{Trace precipitation}_{s,d,h} \cdot \text{Post}_d \\
& + \beta_5 \cdot \text{Precipitation}_{s,d,h} + \beta_6 \cdot \text{Precipitation}_{s,d,h} \cdot \text{Post}_d \\
& + \gamma_{s,m(d),dow(d),h} + \zeta_{holiday} + \epsilon_{s,d,h}
\end{aligned}
$$

	Crashes per hour over 10 km (%)			
	Centerline km (1)	Lane km (2)	VKT (3)	DMS proximity (4)
Board meeting × measure × post	2.26***	2.80***	3.05***	0.60**
	(0.85)	(0.98)	(0.94)	(0.27)
Board meeting × post	1.36**	1.05	1.03	1.36**
	(0.60)	(0.68)	(0.69)	(0.60)
Board meeting × measure	0.25	0.38	0.13	0.05
	(0.55)	(0.71)	(0.67)	(0.20)
Board meeting	-0.33	-0.03	-0.02	-0.33
	(0.43)	(0.55)	(0.55)	(0.43)
Observations	62,118,334	54,017,004	54,017,004	62,048,221
Adj R-squared	0.08	0.08	0.08	0.08
Rain & interactions	Yes	Yes	Yes	Yes
S-Y-M-D-H FE	Yes	Yes	Yes	Yes
Holiday FE	Yes	Yes	Yes	Yes

Table 8: Effect of fatality messages by crash types

Notes: This table estimates the effect of campaign weeks on single- and multi-vehicle crashes. The dependent variable is the number of crashes occurring over the 10 kilometers downstream of DMS s on date d during hour h of a specific type, scaled by the population average for all segments of that type and multiplied by 100. See Table 5 for additional details. Standard errors are clustered by geography-year-month and are in parentheses, where geography indicates a bin of size 10^2 square kilometers that contains the DMS, and *, **, *** indicate 10%, 5%, and 1% two-tailed statistical significance, respectively.

$$\begin{aligned}\text{Crash (\%)}_{s(10),d,h} = {} & \delta \cdot \text{Board meeting}_{d,h} \cdot \text{Post}_d + \beta_1 \cdot \text{Board meeting}_{d,h} \\ & + \beta_2 \cdot \text{Trace precipitation}_{s,d,h} + \beta_3 \cdot \text{Trace precipitation}_{s,d,h} \cdot \text{Post}_d \\ & + \beta_4 \cdot \text{Precipitation}_{s,d,h} + \beta_5 \cdot \text{Precipitation}_{s,d,h} \cdot \text{Post}_d \\ & + \gamma_{s,m(d),dow(d),h} + \zeta_{holiday} + \epsilon_{s,d,h}\end{aligned}$$

	Crashes per hour over 10 km (%)	
	Multi-vehicle (1)	Single vehicle (2)
Board meeting × post	1.61***	-0.26
	(0.62)	(1.59)
Board meeting	-0.65	1.75
	(0.44)	(1.13)
Observations	62,118,334	62,118,334
Adj R-squared	0.08	0.01
Rain & interactions	Yes	Yes
S-Y-M-D-H FE	Yes	Yes
Holiday FE	Yes	Yes

Table 9: Effect of fatality messages on statewide crashes

Notes: This table estimates the effect of campaign weeks on statewide crashes. The dependent variable is the number of crashes occurring statewide (column (1)), statewide on the highway system (column (2)), or statewide off the highway system (column (3)), on date d during hour h, scaled by the population average and multiplied by 100. We include year-month-day-of-week-hour and holiday fixed effects. Standard errors are clustered by year-month and are in parentheses, and *, **, *** indicate 10%, 5%, and 1% two-tailed statistical significance, respectively.

$$\text{Statewide Crash (\%)}_{d,h} = \delta \cdot \text{Board meeting}_{d,h} \cdot \text{Post}_d + \beta_1 \cdot \text{Board meeting}_{d,h} + \gamma_{m(d),dow(d),h} + \zeta_{holiday} + \epsilon_{d,h}$$

	Total (1)	On-highway (2)	Off-highway (3)
Board meeting × post	1.98**	2.77**	1.16
	(0.96)	(1.19)	(0.95)
Board meeting	-1.61**	-2.39***	-0.79
	(0.72)	(0.89)	(0.75)
Observations	70,127	70,127	70,127
Adj R-squared	0.87	0.82	0.84
Y-M-D-H FE	Yes	Yes	Yes
Holiday FE	Yes	Yes	Yes

A Online Appendix

A.1 Additional figures and tables

Figure A.1
Safety campaigns

From: Carol Rawson (Carol.Rawson@txdot.gov)
Sent: Friday, June 14, 2013 1:02 PM
To: !DDO DIST REGION
Cc: #Traffic Engineers; !DDO ADM; Robin Frisk; Brian Burk; Penny Buller; James Moore
Subject: June Fatality Message - Dynamic Message Signs

Improving the safety of the traveling public is the number one goal of TxDOT. Our permanent dynamic message signs (DMS) provide an excellent opportunity to get our traffic safety messages out to the public.

We continue to have traffic fatalities on Texas roadways. In order to bring this critical issue to the public's attention, we are continuing an awareness campaign that displays the year to date fatalities on all Texas roadways. We will be displaying this message with an alternating safety message for one week each month. This month the safety message relates to drinking and driving. Please display the message attached and shown below beginning after the morning peak on Monday, June 17th, and ending before the morning peak on Monday, June 24th.

As always, this DMS message should not pre-empt needed traffic messages, incident-related messages, Emergency Operation Center (EOC) messages, or Amber/Silver/Blue alerts. In areas with a large number of DMS, the fatality message should be displayed on a few signs along the corridor even during peak times when travel times are being displayed.

DMS Message
Phase 1
1332 DEATHS
THIS YEAR ON
TEXAS ROADS

Phase 2
DRINK
DRIVE
GO TO JAIL

However, there are many rural areas that do not have DMS. If you have any portable changeable message signs (PCMS) available for use, you may want to display the fatality PCMS message on roadways of concern. The message for the PCMS is shown below. The safety message corresponds to the message that we have developed for our new safety campaign. This message should be displayed during the same time period as the permanent DMS message.

PCMS Message

Phase 1

1332
TRAFFIC
DEATHS

Phase 2

BE SAFE
DRIVE
SMART

Your assistance in this effort is greatly appreciated.

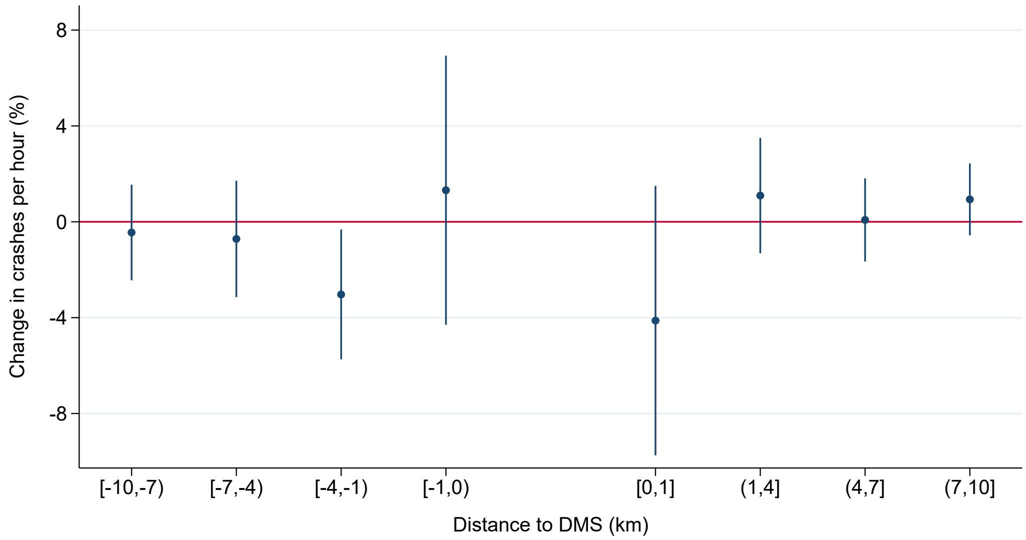
Thanks,
Carol

Figure A.2
Multivariate placebo tests

Notes: This figure depicts δ coefficient estimates (circles) and the associated 95% confidence intervals (bars) from the regression below for segments of length x. The dependent variable is the number of crashes occurring on highway segment s of length x kilometers on date d during hour h, scaled by the population average for all segments of length x and multiplied by 100. We plot the coefficient on $Board\,meeting_{d,h}$, an indicator variable for whether day d and hour h fall within Monday 9 AM–Monday 7 AM the week prior to a board meeting. See Figure 9 for additional details. Panel A drops all DMSs that are preceded by any DMS within 10 km of driving distance, and Panel B analyzes the pre-treatment period (January 2010–July 2012).

$$\begin{aligned}\text{Crash }(\%)_{s(x),d,h} = {} & \delta \cdot \text{Board meeting}_{d,h} + \beta_1 \cdot \text{Trace precipitation}_{s,d,h} \\ & + \beta_2 \cdot \text{Precipitation}_{s,d,h} + \gamma_{s,m(d),dow(d),h} + \zeta_{holiday} + \epsilon_{s,d,h}\end{aligned}$$

Panel A: No upstream DMS within 10 km driving distance, treatment period



Panel B: Pre-treatment period

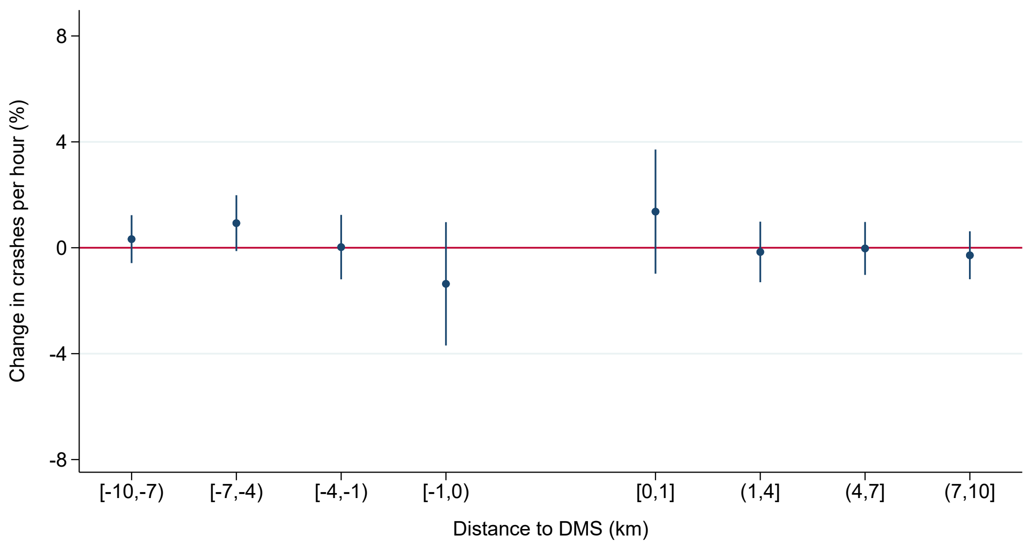
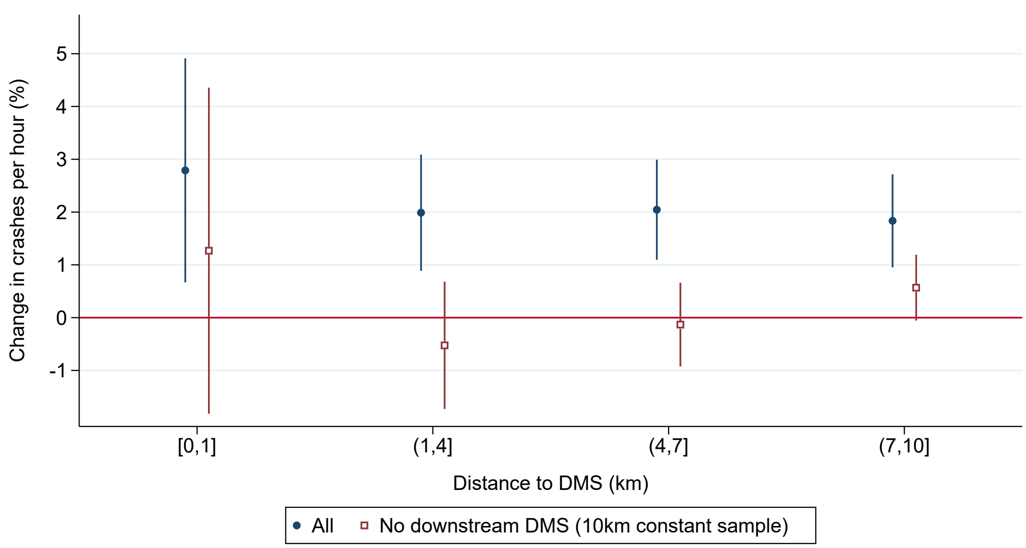


Figure A.3
Limit downstream constant sample

Notes: This figure depicts univariate (Panel A) and multivariate (Panel B) estimates of the effect of a campaign week on number of crashes over four distances from a DMS. The dependent variable, $Crash(\%)_{s(x),d,h}$, is the number of crashes occurring on date d during hour h over the distance x (relative to DMS s) indicated on the x-axis. We scale crash counts by the population average for all segments of the same distance x and multiply by 100. The circles plot the δ coefficient on $Board\,meeting_{d,h}$, an indicator variable for whether day d and hour h fall within Monday 9 AM–Monday 7 AM the week prior to a board meeting, for the full sample. The hollow squares plot the δ coefficient from a regression which restricts the sample to DMSs with no downstream DMS within 10 kilometers. The bars show 95% confidence intervals. In Panel B we also include but do not plot indicators for whether either trace precipitation or more than trace precipitation was measured on segment s during hour h, using data from the closest weather station, as well as segment-year-month-day-of-week-hour and holiday fixed effects. Standard errors are clustered by geography-year-month, where geography indicates a bin of size x^2 square kilometers that contains the DMS. The sample period is August 2012–December 2017.

Panel A: Univariate, no downstream DMS within 10 km driving distance



Panel B: Multivariate, no downstream DMS within 10 km

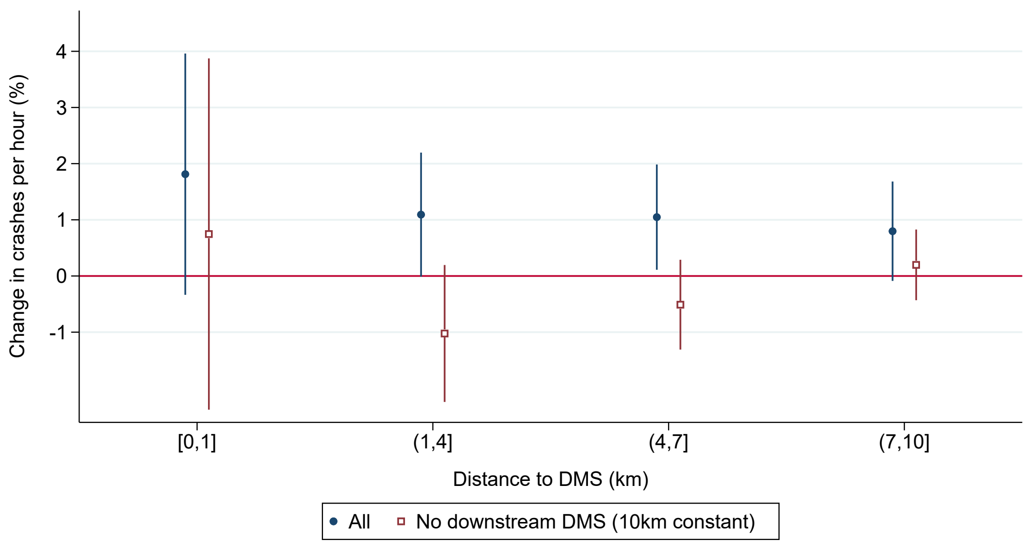


Figure A.4
Fatality counts displayed on DMSs by month

Notes: This figure plots the mode death count (black diamonds) and all other death counts (gray circles, frequency weighted) displayed each calendar month across all segment hours for the sample of DMS log files.

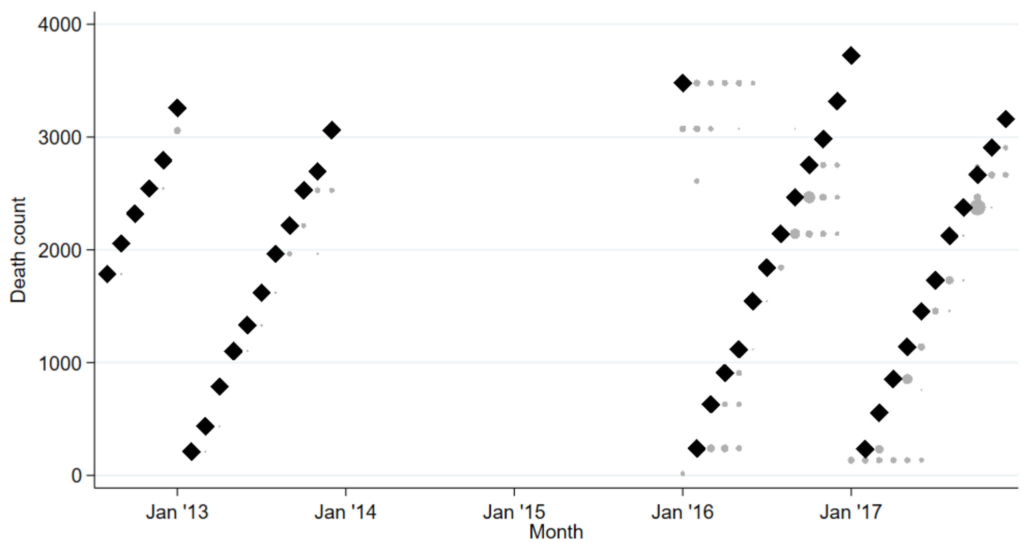


Figure A.5
Share of crashes involving an out-of-state driver relative to July

Notes: This figure shows how the share of statewide crashes that involve an out-of-state driver varies across the year. Specifically, the dependent variable is the log of the share of crashes where at least one driver's zip code is from outside of Texas. We regress this share on fixed effects for each month and year, with July as the base month and 2010 as the base year. Thus, the coefficients are the percentage difference in the share of crashes involving an out-of-state driver in the given month and July. Standard errors are clustered by month. We run the regression below and plot the β_i coefficient estimates, scaled by 100 to be a percentage change:

$$\log\left(\text{Share out-of-state}_{m,y}\right) = \sum_{i\in\{1,\ldots,6,8,\ldots,12\}} \beta_i \cdot \text{month}_{i,m} + \sum_{i\in\{2011,\ldots,2017\}} \gamma_i \cdot \text{year}_{i,y} + \epsilon_{d,h}$$

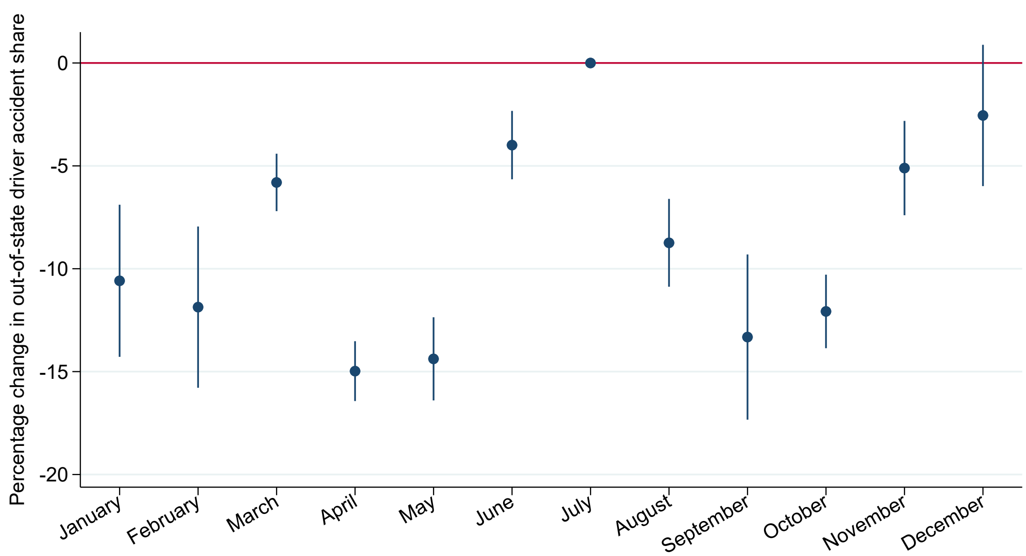


Figure A.6
Effect of fatality message by year: statewide

Notes: This figure depicts the coefficient estimates (circles, diamonds, and squares) and standard error bars from regressions allowing the effect of campaign weeks on the number of statewide crashes to vary by year for the 2010–2017. Specifically, the dependent variable is the number of crashes occurring statewide (circles), statewide on the highway system (diamonds), or statewide off the highway system (squares) scaled by the population average and multiplied by 100. $Board\ meeting_{d,h}$ is an indicator variable for whether day d and hour h fall within Monday 9 AM–Monday 7 AM the week prior to a board meeting, $year_{d,i}$ are indicator variables for years. We include year-month-day-of-week-hour and holiday fixed effects. Standard errors are clustered by month. We run the regression below and plot the δ_i coefficient estimates:

$$\text{Crash (\%)}_{d,h} = \sum_{i \in \{2010,\ldots,2017\}} \delta_i \cdot \text{Board meeting}_{d,h} \cdot \text{year}_{d,i} + \gamma_{m(d),dow(d),h} + \zeta_{holiday} + \epsilon_{d,h}$$

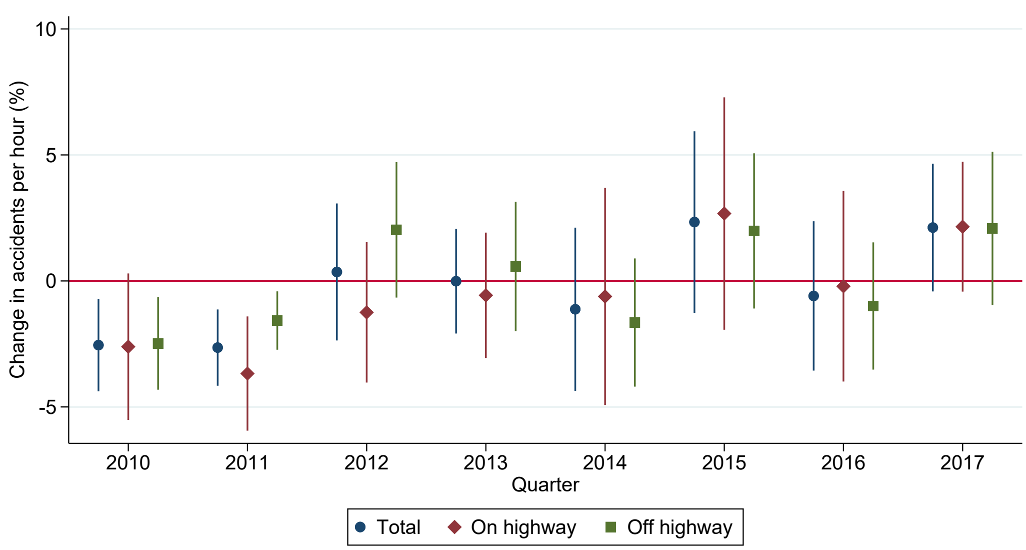


Figure A.7
Effect of fatality messages on fatal crashes by calendar quarter

Notes: This figure depicts the δ_i coefficient estimates (circles) and standard error bars from the regression below which allows the treatment effect on the number of fatal crashes to vary by calendar quarter. The dependent variable, $Fatal\ crash\ (\%)_{s(10\ km),d,h}$, is the scaled number of fatal crashes occurring over the 10 kilometers downstream of DMS s on day d during hour h; $Board\ meeting_{d,h}$ is an indicator variable for whether day d and hour h fall within Monday 9 AM–Monday 7 AM the week prior to a board meeting; $Qtr_{d,i}$ are indicator variables equal to one if day d occurs during calendar quarter i; and $post_d$ is an indicator for observations after August 1, 2012. We also include but do not plot indicators for whether either trace precipitation or more than trace precipitation was measured on segment s during hour h (and their interactions with $post_d$), as well as segment-year-month-day-of-week-hour and holiday fixed effects. Standard errors are clustered by geography-year-month bins, where geography bins are defined as the 10^2 square kilometers containing the DMS. The sample period is January 2010–December 2017.

$$
\begin{aligned}
\text{Fatal crash } (\%)_{s(10\ km),d,h} = & \sum_{i \in \{\text{Q1},\ldots,\text{Q4}\}} \delta_i \cdot \text{Board meeting}_{d,h} \cdot \text{Qtr}_{d,i} \cdot \text{post}_d \\
& + \sum_{i \in \{\text{Q1},\ldots,\text{Q4}\}} \beta_{1,i} \cdot \text{Board meeting}_{d,h} \cdot \text{Qtr}_{d,i} \\
& + \beta_2 \cdot \text{Trace precipitation}_{s,d,h} + \beta_3 \cdot \text{Trace precipitation}_{s,d,h} \cdot \text{post}_d \\
& + \beta_4 \cdot \text{Precipitation}_{s,d,h} + \beta_5 \cdot \text{Precipitation}_{s,d,h} \cdot \text{post}_d \\
& + \gamma_{s,m(d),dow(d),h} + \zeta_{holiday} + \epsilon_{s,d,h}
\end{aligned}
$$

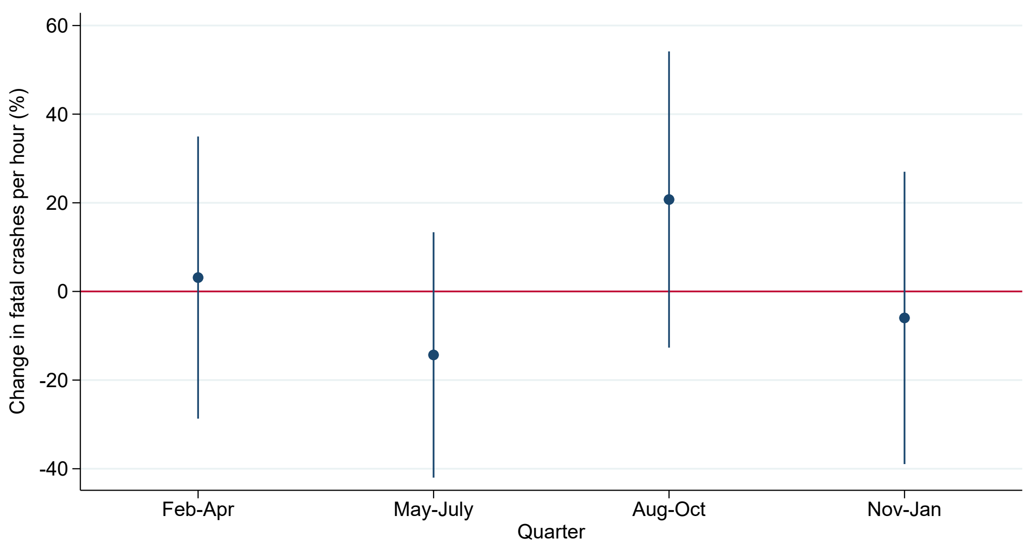


Table A.1: States showing fatality messages

State	Start date	Confirmed showing in May 2020	Licensed drivers in 2018	Source
Alaska	≤ September 2017	No	536,033	ATSSA
Arizona[†]	≤ September 2017	No	5,284,970	ATSSA
Colorado	January 2016	Yes	4,244,713	Official website
Connecticut	≤ September 2017	No	2,605,612	ATSSA
Delaware	≤ September 2017	Yes	786,504	ATSSA
Georgia	≤ January 2015	Yes	7,168,733	News article
Hawaii	March 2018	Yes	948,417	Official website
Illinois	July 2012	Yes	8,714,788	News article
Iowa	August 2013	Yes	2,260,271	News article
Michigan	July 2013	Yes	7,153,645	Official website
Minnesota	July 2013	No	3,391,057	News article
Missouri	≤ September 2017	Yes	4,272,960	ATSSA
Montana	≤ August 2019	Yes	806,204	Author's observation
Nebraska	May 2016		1,420,317	News article
Nevada	May 2013		1,983,453	Press release
New Hampshire	≤ September 2017	Yes	1,161,665	ATSSA
Ohio	July 2015	Yes	8,032,665	News article
Oklahoma	April 2016		2,504,253	Official website
Pennsylvania	≤ September 2017		8,991,370	ATSSA
South Carolina	≤ September 2017	Yes	3,846,069	ATSSA
South Dakota	August 2018	Yes	638,428	Email
Tennessee	April 2012	Yes	5,422,429	News article
Texas	August 2012	Yes	17,370,383	News article
Utah[‡]	June 2014	Yes	2,030,644	Author's observation
Vermont	April 2016	Yes	564,892	News article
Wisconsin	≤ 2014	No	4,288,171	News article
Wyoming	August 2016	Yes	419,256	News article

Notes: To confirm which states were still showing fatality messages in May 2020, we reached out to state Departments of Transportation as well as to friends and colleagues directly, and via Twitter and Facebook. Data on the number of licensed drivers in each state comes from U.S. Department of Transportation (2019b).

[†] The ATSSA reports that Arizona only showed a fatality message once.

[‡] Utah's messages have been a little different, including "61 lives lost last summer on Utah roads," the number of fatalities in the previous month, and the number of days since the last fatality.

Table A.2: Definition of variables

This table describes the calculation of variables used in the main analyses.

Variable	Definition
Crash x km $(10^{-3})_{s,d,h}$	The number of crashes occurring on the highway segment that begins with DMS s and continues for x kilometers of highway driving distance on calendar day d during hour h, multiplied by 1,000.
Crash (%) $_{s(x),d,h}$	The number of crashes occurring on the highway segment that begins with DMS s and covers x kilometers of highway driving distance on calendar day d during hour h, scaled by the population average for all segments of length x and multiplied by 100.
Multi-vehicle crash (%) $_{s(10),d,h}$	The number of crashes involving multiple vehicles on segment s of length 10 kilometers on day d during hour h, scaled by the population average for all segments and multiplied by 100.
Single vehicle crash (%) $_{s(10),d,h}$	The number of crashes involving a single vehicle on segment s of length 10 kilometers on day d during hour h, scaled by the population average for all segments and multiplied by 100.
Fatal crash $(\%)_{s(x),d,h}$	The number of fatal crashes (crashes involving a fatality) occurring on the highway segment that begins with DMS s and covers x kilometers of highway driving distance on calendar day d during hour h, scaled by the population average for all segments of length x and multiplied by 100.
Statewide crash $(\%)_{d,h}$	The number of crashes occurring statewide during hour d of day d, scaled by the population average.
Board meeting$_{d,h}$	Indicator equal to one if hour h of day d occurs during the week prior to a Texas Department of Transportation board meeting (where the week prior is defined as starting at 9 AM on the Monday 10 days prior to the board meeting and ending at 7 AM on the following Monday).
Board meeting-Sunday$_{d,h}$	Indicator equal to one if the hour h of day d occurs during the week prior to a Texas Department of Transportation board meeting (where the week prior is defined as starting at 12 AM on the Sunday 11 days prior to the board meeting and ending at 7 AM on the following Monday)
Post$_d$	Indicator equal to one if the day d occurs on or after August 1, 2012

Table A.2 (continued)

Variable	Definition
Trace precipitation$_{s,d,h}$	Indicator equal to one if the weather station closest to DMS s reported less than one millimeter of precipitation during hour h of day d.
Precipitation$_{s,d,h}$	Indicator equal to one if the weather station closest to DMS s reported more than one millimeter of precipitation during hour h of day d.
YTD quartile$_{d,i}$	Indicator equal to one if on day d the year-to-date count of crash-related fatalities falls into quartile i, with quartiles Q1–Q4 defined as <=882, 883–1736, 1737–2621, and > 2,621 fatalities. Fatality counts at the start of each awareness campaign week are derived from the actual number of fatalities reported in CRIS with a 22 day lag.
Qtr$_{d,i}$	Indicator equal to one if day d occurs during calendar quarter i, with calendar quarters Q1–Q4 defined as February—April, May—July, August—October, and November—January, respectively.
Year$_{d,i}$	Indicator equal to one if day d occurs during calendar year i, with $i \in \{2010, 2011, \ldots 2016, 2017\}$.
Board meeting 2 week$_d$	Indicator equal to one if day d occurs during the two week period ending on a board meeting of the Texas Department of Transportation (always held on a Thursday).
Event day$_{d,i}$	Indicator equal to one if day d occurs on day i during the two week period ending on a board meeting of the Texas Department of Transportation, with $i \in \{-3, -2, \ldots, 9, 10\}$ and where $i = 10$ is the date of the board meeting, and $i = 0$ is the Monday a week prior to the board meeting when fatality awareness campaigns officially being.
Fatality message$_{s,d,h}$	The percent of hour h on day d that the DMS at the start of segment s displayed a fatality message.
Fatality message minutes$_{s,d,h}$	The number of minutes during hour h of day d that DMS s displayed a fatality message.
Non-safety message minutes$_{s,d,h}$	The number of minutes during hour h of day d that DMS s displayed a non-safety related message, where safety-related messages include fatality messages.
Travel time minutes$_{s,d,h}$	The number of minutes during hour h of day d that DMS s displayed a travel time.
Blank minutes$_{s,d,h}$	The number of minutes during hour h of day d that DMS s was blank.
Amber alert minutes$_{s,d,h}$	The number of minutes during hour h of day d that DMS s displayed an amber alert.

Table A.2 (continued)

Variable	Definition
Centerline km	The length of road within a given distance downstream, regardless of how many lanes there are. Standardized to have a mean of zero and standard deviation of one.
Lane km	The centerline length times number of lanes within a given distance downstream. Standardized to have a mean of zero and standard deviation of one.
VKT	Average daily vehicle kilometers traveled over a given segment. Standardized to have a mean of zero and standard deviation of one.
Upstream DMS * (-1)	Kilometers to the nearest upstream DMS, multiplied by -1 and standardized to have a mean of zero and standard deviation of one.

Table A.3: Dates of campaign weeks in DMS message sample

This table lists the 41 safety campaign weeks in our sample of DMS log files. We tabulate the mode fatality number displayed that week (column (1)), our proxy for the official number of fatalities as of that week based on reported fatalities with a reporting lag of 22 days (the median lag based on comparison of the actual and displayed fatality counts, column (2)), the number DMSs that displayed a fatality message at least once during the week (column (3)), the count of DMSs displaying any message during the week (column (4)), the percent of DMSs displaying a fatality message during the week (column (5)), and the average percent of total possible hours the fatality message was displayed across all DMSs that week (column (6)). * indicate safety campaign weeks that occur during the week of the TxDOT board meeting, rather than the week prior.

Campaign week	Mode fatality displayed (1)	Proxy fatality cnt (2)	DMSs fatality msg (3)	DMSs online (4)	Percent DMSs msg (5)	Average percent hours (6)
20-Aug-12	1,785	1,938	142	220	65%	32%
17-Sep-12	2,058	2,225	137	220	62%	33%
15-Oct-12	2,321	2,491	146	220	66%	34%
5-Nov-12	2,545	2,691	169	220	77%	31%
3-Dec-12	2,795	2,984	158	220	72%	37%
21-Jan-13	3,258	3,447	159	220	72%	29%
18-Feb-13	211	245	170	220	77%	30%
18-Mar-13	436	471	173	220	79%	49%
15-Apr-13*	786	726	0	220	0%	0%
20-May-13	1,102	1,079	173	220	79%	51%
17-Jun-13	1,332	1,340	172	220	78%	43%
15-Jul-13	1,620	1,650	169	220	77%	54%
19-Aug-13	1,965	1,994	171	220	78%	51%
16-Sep-13	2,214	2,262	170	220	77%	40%
21-Oct-13	2,528	2,590	176	220	80%	42%
11-Nov-13	2,697	2,769	174	220	79%	53%
9-Dec-13*	3,061	3,021	1	220	0%	0%
18-Jan-16	3,479	3,594	242	434	56%	45%
15-Feb-16	240	220	494	628	79%	35%
21-Mar-16	631	595	487	646	75%	40%
18-Apr-16	908	882	428	636	67%	30%
16-May-16	1,118	1,167	470	623	75%	38%
20-Jun-16	1,545	1,551	439	632	69%	36%
18-Jul-16	1,842	1,856	455	646	70%	37%
15-Aug-16	2,142	2,128	505	656	77%	38%
19-Sep-16	2,466	2,481	440	665	66%	38%
17-Oct-16	2,752	2,756	173	658	26%	15%
7-Nov-16	2,983	2,984	480	669	72%	35%
5-Dec-16	3,318	3,331	408	672	61%	37%
16-Jan-17	3,722	3,801	487	677	72%	34%
13-Feb-17	232	218	503	686	73%	42%
20-Mar-17	555	507	477	692	69%	37%
17-Apr-17	854	829	500	702	71%	39%
15-May-17	1,139	1,116	507	700	72%	40%
19-Jun-17	1,456	1,456	454	712	64%	25%
17-Jul-17	1,730	1,736	400	718	56%	27%
21-Aug-17	2,124	2,124	408	716	57%	13%
18-Sep-17	2,376	2,409	407	715	57%	24%
16-Oct-17	2,664	2,667	445	738	60%	28%
6-Nov-17	2,906	2,904	498	738	67%	31%
4-Dec-17	3,158	3,210	465	731	64%	31%

Table A.4: Crash exposure risk by distance

Notes: This table reports estimates of the ratio of the exposure risk to crashes on segments extending x kilometers from a DMS to that on segments extending 1 kilometer from a DMS, for $x \in \{3, 5, 10\}$. The first column reports estimates based on hourly crashes. The second column reports estimates based on the lane kilometers within the segment. The third column reports estimates based on the average annual vehicle kilometers traveled over the segment. The second and third columns are based on data from U.S. Department of Transportation (2019a) for 2011–2017.

	Crashes (1)	Lane KM (2)	VKT (3)
3 km:1 km	4.22	3.82	3.67
5 km:1 km	9.85	8.57	7.95
10 km:1 km	34.72	30.16	25.99

Table A.5: Effect of fatality messages by sample period

Notes: This table estimates the effect of campaign weeks on traffic crashes. The sample period is Jan 1, 2010–July 30, 2012 in columns (1)–(3) and August 1, 2012–Dec 31, 2017 in columns (4)–(6). The dependent variable is the number of crashes occurring on highway segment s of length x kilometers on date d during hour h, scaled by the population average for all segments of length x. Highway segments begin at each dynamic message sign (DMS) located on a highway and continue for x kilometers of highway driving distance, where $x \in \{3, 5, 10\}$ as indicated in the column header. $Board\ meeting_{d,h}$ is an indicator variable for whether day d and hour h fall within Monday 9 AM–Monday 7 AM the week prior to a board meeting. We include indicators for whether either trace precipitation or more than trace precipitation was measured on segment s during hour h, using data from the closest weather station ($Trace\ precipitation_{s,d,h}$ and $Precipitation_{s,d,h}$, respectively). We also include segment-year-month-day-of-week-hour and holiday fixed effects. Standard errors are clustered by geography-year-month and are in parentheses, where geography indicates a bin of size x^2 square kilometers that contains the DMS, and *, **, *** indicate 10%, 5%, and 1% two-tailed statistical significance, respectively.

$$\text{Crash (\%)}_{s(x),d,h} = \delta \cdot \text{Board meeting}_d + \beta_1 \cdot \text{Trace precipitation}_{s,d,h} + \beta_2 \cdot \text{Precipitation}_{s,d,h} + \gamma_{s,m(d),dow(d),h} + \zeta_{holiday} + \epsilon_{s,d,h}$$

	Pre-treatment			Treatment		
	(1) 3 km	(2) 5 km	(3) 10 km	(4) 3 km	(5) 5 km	(6) 10 km
Board meeting	0.60	-0.06	-0.13	1.43**	1.21**	0.96**
	(0.63)	(0.49)	(0.43)	(0.59)	(0.48)	(0.43)
Trace precipitation	28.79***	28.09***	27.73***	26.96***	27.55***	26.77***
	(2.15)	(1.72)	(1.57)	(1.58)	(1.25)	(1.09)
Precipitation	53.26***	53.27***	52.26***	80.50***	79.90***	76.45***
	(2.04)	(1.62)	(1.67)	(1.89)	(1.65)	(1.73)
Observations	20,047,441	20,047,441	20,047,441	42,070,893	42,070,893	42,070,893
Adj R-squared	0.01	0.02	0.05	0.02	0.03	0.09
S-Y-M-D-H FE	Yes	Yes	Yes	Yes	Yes	Yes
Holiday FE	Yes	Yes	Yes	Yes	Yes	Yes

Table A.6: Effect of fatality messages on crashes: segment characteristics

Notes: This table estimates how effect of campaign weeks on traffic crashes varies by segment characteristics. The dependent variable is the number of crashes occurring over the 10 kilometers downstream of DMS s on date d during hour h, scaled by the population average for all segments and multiplied by 100. $Board\ meeting_{d,h}$ indicates whether day d and hour h falls within Monday 9 AM–Monday 7 AM the week prior to a board meeting, and $Post_d$ indicates observations after August 1, 2012. $High$ (Low) are indicators for the following characteristics of segment s (as indicated in column header): above (below) median centerline km, above (below) median lane km, above (below) median VKT, and an upstream DMS within (not within) 5 km upstream. See Appendix Table A.2 for detailed variable definitions. We include segment-year-month-day-of-week-hour and holiday fixed effects. Standard errors are clustered by geography-year-month and are in parentheses, where geography indicates a bin of size 10^2 square kilometers that contains the DMS, and *, **, *** indicate 10%, 5%, and 1% two-tailed statistical significance, respectively.

$$
\begin{aligned}
\text{Crash (\%)}_{s(10),d,h} = {} & \delta_1 \cdot \text{Board meeting}_{d,h} \cdot \text{High}_s \cdot \text{Post} \\
& + \delta_2 \cdot \text{Board meeting}_{d,h} \cdot \text{Low}_s \cdot \text{Post} \\
& + \delta_3 \cdot \text{Board meeting}_{d,h} \cdot \text{High}_s \\
& + \delta_4 \cdot \text{Board meeting}_{d,h} \cdot \text{Low}_s \\
& + \beta_2 \cdot \text{Trace precipitation}_{s,d,h} + \beta_3 \cdot \text{Trace precipitation}_{s,d,h} \cdot \text{Post}_d \\
& + \beta_4 \cdot \text{Precipitation}_{s,d,h} + \beta_5 \cdot \text{Precipitation}_{s,d,h} \cdot \text{Post}_d \\
& + \gamma_{s,m(d),dow(d),h} + \zeta_{holiday} + \epsilon_{s,d,h}
\end{aligned}
$$

	Crashes per hour over 10 km (%)			
	Centerline km (1)	Lane km (2)	VKT (3)	Upstream DMS (4)
Board meeting × high × post	2.53**	3.06***	3.28***	2.66***
	(1.05)	(0.96)	(0.96)	(1.00)
Board meeting × low × post	0.19	-0.13	-0.50	0.31
	(0.43)	(0.48)	(0.50)	(0.46)
Board meeting × high	0.01	-0.22	-0.29	-0.39
	(0.73)	(0.56)	(0.57)	(0.71)
Board meeting × low	-0.66**	-0.57	-0.42	-0.28
	(0.34)	(0.40)	(0.42)	(0.34)
Observations	62,118,334	62,118,334	62,118,334	62,118,334
Adj R-squared	0.08	0.08	0.08	0.08
Rain & interactions	Yes	Yes	Yes	Yes
S-Y-M-D-H FE	Yes	Yes	Yes	Yes
Holiday FE	Yes	Yes	Yes	Yes

Table A.7: Statewide crashes: robustness tests

Notes: This table reports robustness tests of the effect of campaign weeks on statewide crashes. Column (1) reproduces the results in Table 9. Column (2) reports results when the dependent variable is winsorized by clipping values above the 99th percentile and below the 1st percentile. Column (3) reports results from quantile regressions following Machado and Santos Silva (2019). Column (4) reports results when the dependent variable in the inverse hyperbolic sine of the count of crashes. Column (5) report results using a Poisson regression on the count of crashes. While our data is overdispersed, we use a Poisson regression due to its robustness (Wooldridge, 1999). In columns (3) and (5) standard errors are calculated by bootstrapping. Standard errors are clustered by year-month and are in parentheses, and *, **, *** indicate 10%, 5%, and 1% two-tailed statistical significance, respectively.

	(1) Main	(2) Winsorized	(3) Quantile	(4) IHS	(5) Poisson
			Panel A: Total crashes		
Board meeting × post	1.981**	1.952**	1.975**	0.0190*	0.0207**
	(0.962)	(0.936)	(0.911)	(0.00981)	(0.0101)
Board meeting	-1.605**	-1.582**	-1.551**	-0.0146*	-0.0176**
	(0.723)	(0.720)	(0.698)	(0.00804)	(0.00831)
Observations	70,127	70,127	70,127	70,127	70,127
Y-M-D-H FE	Yes	Yes	Yes	Yes	Yes
Holiday FE	Yes	Yes	Yes	Yes	Yes
			Panel B: On-highway crashes		
Board meeting × post	2.766**	2.646**	2.716**	0.024*	0.030**
	(1.190)	(1.137)	(1.099)	(0.012)	(0.013)
Board meeting	-2.386***	-2.315***	-2.285***	-0.019*	-0.027**
	(0.887)	(0.870)	(0.824)	(0.010)	(0.011)
Observations	70,127	70,127	70,127	70,127	70,127
Y-M-D-H FE	Yes	Yes	Yes	Yes	Yes
Holiday FE	Yes	Yes	Yes	Yes	Yes
			Panel C: Off-highway crashes		
Board meeting × post	1.162	1.165	1.165	0.014	0.011
	(0.951)	(0.930)	(0.920)	(0.010)	(0.011)
Board meeting	-0.791	-0.780	-0.769	-0.009	-0.008
	(0.746)	(0.744)	(0.766)	(0.008)	(0.009)
Observations	70,127	70,127	70,127	70,127	70,127
Y-M-D-H FE	Yes	Yes	Yes	Yes	Yes
Holiday FE	Yes	Yes	Yes	Yes	Yes

Table A.8: Effect of fatality messages on crashes: robustness tests

Notes: This table tabulates robustness tests of the main result in Table 5. In Panel A, column (1) reproduces the main specification estimating the effect of fatality messages on the number of crashes within 10 km of DMSs, controls for trace precipitation and more than trace precipitation as well as segment-year-month-day-of-week-hour and holiday fixed effects and clustering standard errors by geography-year-month bins. In column (2) we use the same specification, but cluster standard errors by segment-year-month (S-Y-M). In column (3) we include controls for additional levels of precipitation intensity (using precipitation cutoffs of 0, 2.5, 7.6, and 50 millimeters per hour listed on https://en.wikipedia.org/wiki/Rain), in column (4) we drop all controls for precipitation, and in column (5) we drop the holiday fixed effects. In Panel B, in columns (1)–(2) we use an indicator variable for the occurrence of a crashes as the dependent variable, and cluster standard errors by either geography-year-month or segment-year-month. In columns (3)–(4) we use the natural log of the number of hourly crashes plus one as the dependent variable, and again cluster standard errors two different ways. In column (5) we include DMS-months that we confirm exist using Google Streetview, and in column (6) we include only those DMS-months that we confirm might exist (i.e., we do not know they do not exist). *, **, *** indicate 10%, 5%, and 1% two-tailed statistical significance, respectively.

Panel A

	Crashes per hour over 10 km (%)				
	(1)	(2)	(3)	(4)	(5)
Board meeting × post	1.36**	1.36***	1.35**	2.98***	1.49**
	(0.60)	(0.29)	(0.60)	(0.61)	(0.60)
Board meeting	-0.33	-0.33	-0.37	-1.05**	0.58
	(0.43)	(0.21)	(0.43)	(0.43)	(0.43)
Observations	62,118,334	62,118,334	62,118,334	62,118,334	62,118,334
Adj R-squared	0.08	0.08	0.08	0.08	0.08
Cluster standard errors	G-Y-M	S-Y-M	G-Y-M	G-Y-M	G-Y-M
S-Y-M-D-H FE	Yes	Yes	Yes	Yes	Yes
Holiday FE	Yes	Yes	Yes	Yes	No
Baseline precipitation	Yes	Yes	No	No	Yes
Add'l precipitation	No	No	Yes	No	No

Table A.8 Robustness tests–continued

Panel B

	Crashes per hour over 10 km				Crashes per hour over 10 km (%)	
	Dummy		Log		Must exist	May exist
	(1)	(2)	(3)	(4)	(5)	(6)
Board meeting × post	0.06**	0.06***	0.04**	0.04***	1.52**	1.38**
	(0.03)	(0.01)	(0.02)	(0.01)	(0.76)	(0.64)
Board meeting	-0.02	-0.02*	-0.01	-0.01*	-0.36	-0.30
	(0.02)	(0.01)	(0.01)	(0.01)	(0.60)	(0.48)
Observations	62,118,334	62,118,334	62,118,334	62,118,334	41,877,330	54,997,588
Adj R-squared	0.08	0.08	0.08	0.08	0.09	0.08
Cluster standard errors	G-Y-M	S-Y-M	G-Y-M	S-Y-M	G-Y-M	G-Y-M
Rain & interactions	Yes	Yes	Yes	Yes	Yes	Yes
S-Y-M-D-H FE	Yes	Yes	Yes	Yes	Yes	Yes
Holiday FE	Yes	Yes	Yes	Yes	Yes	Yes

Table A.9: Effect of fatality messages on fatal crashes

Notes: This table estimates the effect of campaign weeks on fatal crashes. The dependent variable is the number of fatal crashes occurring on segment s of length x kilometers on date d during hour h, scaled by the population average for all segments of length x and multiplied by 100. See Table 5 for additional details. Standard errors are clustered by geography-year-month and are in parentheses, where geography indicates a bin of size x^2 square kilometers that contains the DMS, and *, **, *** indicate 10%, 5%, and 1% two-tailed statistical significance, respectively.

$$\begin{aligned}\text{Fatal crash (\%)}_{s(x),d,h} = &\ \delta \cdot \text{Board meeting}_{d,h} \cdot \text{Post}_d + \beta_1 \cdot \text{Board meeting}_{d,h} \\ &+ \beta_2 \cdot \text{Trace precipitation}_{s,d,h} + \beta_3 \cdot \text{Trace precipitation}_{s,d,h} \cdot \text{Post}_d \\ &+ \beta_4 \cdot \text{Precipitation}_{s,d,h} + \beta_5 \cdot \text{Precipitation}_{s,d,h} \cdot \text{Post}_d \\ &+ \gamma_{s,m(d),dow(d),h} + \zeta_{holiday} + \epsilon_{s,d,h}\end{aligned}$$

	Deaths per hour (%)		
	3 km	5 km	10 km
	(1)	(2)	(3)
Board meeting × post	9.529	4.985	-1.454
	(11.609)	(9.245)	(7.958)
Board meeting	-2.660	6.966	14.920**
	(9.122)	(7.425)	(6.483)
Observations	62,118,334	62,118,334	62,118,334
Adj R-squared	-0.00	-0.00	-0.00
Rain & interactions	Yes	Yes	Yes
S-Y-M-D-H FE	Yes	Yes	Yes
Holiday FE	Yes	Yes	Yes

A.2 Endogeneity of fatality message

This section presents evidence that the presence of a fatality message at a particular time on any given DMS is endogenous with respect to crash risk at that location and time. First, we provide evidence that when a crash occurs near where a fatality message is being displayed, district engineers often replace the fatality message with a message warning of a crash ahead. Second, we provide evidence that district engineers sometimes make these changes *prior* to the reported crash time, likely a result of measurement error in reported crash times.

First, when a crash occurs, district engineers frequently replace a fatality message with a message warning about a crash up ahead. To show this, we estimate how the probability that a DMS continues to display a fatality message changes when a crash occurs using the following regression:

$$Y_{s,d,h+1} = \delta \cdot \text{Crash}_{s,d,h} + \gamma_{s,y(d),dow(d),h} + \epsilon_{s,d,h} \,\Big|\, \text{Fatality message}_{s,d,h} = 1, \quad \text{(A.1)}$$

where $Y_{s,d,h}$ is an indicator for whether DMS s was displaying message $Y \in \{\text{Fatality message, Crash message}\}$ at the start of hour h on day d, $Crash_{s,d,h}$ is an indicator for whether there was a crash on the 10 kilometer segment starting at DMS s during hour h of day d, and $\gamma_{s,y(d),dow(d),h}$ is a DMS-year-day of week-hour fixed effect. Including this fixed effect means we are estimating how a crash changes the probability of continuing to show a fatality message holding constant the DMS, year, day of week, and hour. Doing so is important because, as Figure 5 shows, DMSs are less likely to show a fatality message during the peak traffic hours and peak traffic hours are also more likely to have crashes.[46] Panel A of Table A.10 reports the results from estimating (A.1). We find that if there is a crash during hour h, then a DMS is 2.82 percentage points less likely to display a fatality message at the end of the hour, and 2.66 percentage points more likely to warn of a crash up ahead.

Second, district engineers regularly replace a fatality message with another message *before* a crash occurs. To show this, we estimate how, conditional on a DMS

[46] Indeed, without this fixed effect we estimate that a crash occurring reduces the probability that a DMS continues to display a fatality message by 5.67 percentage points.

displaying a fatality message at the start of hour $h-1$, the probability that the DMS displays a fatality message or crash message at the start of hour h changes if there is an crash during hour h. Specifically, we estimate the following regression:

$$Y_{s,d,h} = \delta \cdot \text{Crash}_{s,d,h} + \gamma_{s,y(d),dow(d),h} + \epsilon_{s,d,h} \,\Big|\, \text{Fatality message}_{s,d,h-1} = 1. \quad \text{(A.2)}$$

Panel B of Table A.10 shows these results. We find that that the probability a DMS displays a fatality message at the start of hour h, conditional on showing one at the start of hour $h-1$, decreases by 0.48 percentage points if there is a crash during hour h. When DMSs cease displaying fatality messages, half the time the fatality message is replaced with a message warning of a crash up ahead.

The finding that engineers replace a fatality message with another message *prior* to a crash occurring is likely the result of errors in recorded crash times. There are at least two sources of error. First, police officers fill out their report when they arrive at the scene of the crash, and there is likely error in what they report as the time of the crash. Second, there is significant rounding in what times are reported. Figure A.8 plots the distribution of the minute during an hour when crashes are reported to occur. The true distribution is likely close to uniform, and so the large spikes on the hour, half hour, and at 5-minute intervals are evidence of police officers rounding reported crash times. Figure A.8 suggests that at least three percent of crashes occur during the hour prior to their reported hour.

Table A.10: Message transition probabilities

Notes: Panel A reports estimates of the probability a DMS displays a given message type at the end of an hour, conditional on displaying a fatality message at the start of the hour, and how this probability differs if there was a crash within 10 kilometers of the DMS during the hour. Panel B reports estimates of the probability of displaying a given message at the start of an hour, conditional on displaying a fatality message at the start of the previous hour, and how this probability differs if there was a crash within 10 kilometers of the DMS during the hour. Let $Y_{s,d,h}$ be an indicator for whether DMS s displays message type $Y \in \{\text{Fatality, Crash}\}$ at the start of hour h on day d. A fatality message gives the year-to-date number of fatalities on Texas roads (column (1)) and a crash message warns of a crash downstream (column (2)). Also let $Crash_{s,d,h}$ be an indicator for whether there was a crash on the 10 kilometer segment starting at DMS s during hour h of day d. We include a DMS-year-day of week-hour fixed effect. Standard errors are clustered by DMS-year-month and are in parentheses, and *, **, *** indicate 10%, 5%, and 1% two-tailed statistical significance, respectively.

$$\text{Panel A: } Y_{s,d,h+1} = \delta \cdot \text{Crash}_{s,d,h} + \gamma_{s,y(d),dow(d),h} + \epsilon_{s,d,h} \,\Big|\, \text{Fatality message}_{s,d,h} = 1$$

$$\text{Panel B: } Y_{s,d,h} = \delta \cdot \text{Crash}_{s,d,h} + \gamma_{s,y(d),dow(d),h} + \epsilon_{s,d,h} \,\Big|\, \text{Fatality message}_{s,d,h-1} = 1$$

	Fatality (1)	Crash (2)
Panel A: Current hour transition probability		
Crash within 10 km	-0.0282***	0.0266***
	(0.0012)	(0.0009)
Observations	1,281,774	1,281,774
S-Y-D-H FE	Yes	Yes
Panel B: Prior hour transition probability		
Crash within 10 km	-0.0048***	0.0025***
	(0.0010)	(0.0005)
Observations	1,281,774	1,281,774
S-Y-D-H FE	Yes	Yes

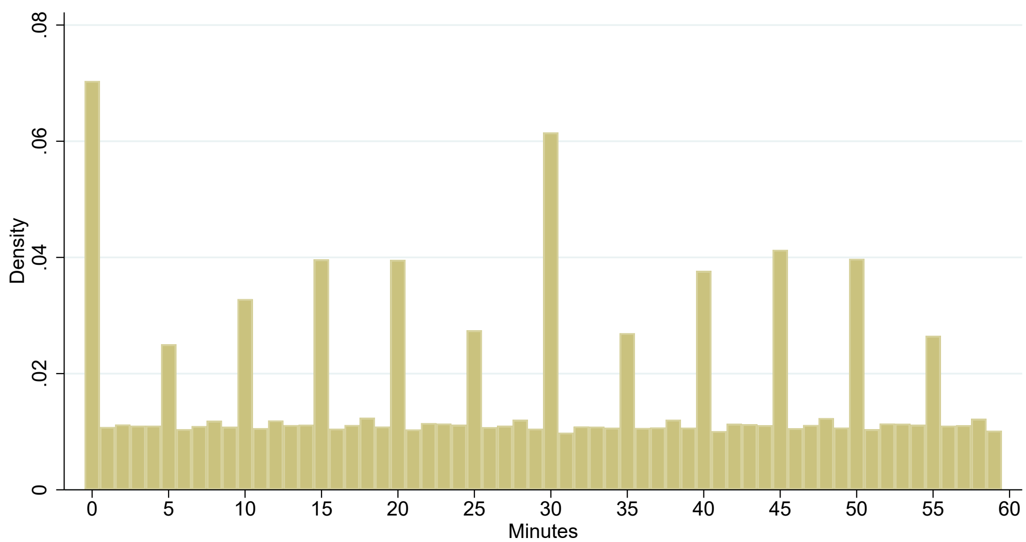


Figure A.8
Distribution of the minutes at which reported crashes occur

A.3 Number of additional crashes and their social cost

This section documents our calculations of the number of additional crashes per year due to showing a fatality message, and their total social cost.

We wish to know the additional number of crashes caused statewide as a result of these fatality messages, Δ. Let y_1 be the actual annual number of crashes, y_0 the counterfactual number of crashes in the absence of the fatality message, f the fraction of hours in the year assigned to treatment, and δ the increase in crashes due to being assigned to treatment. Then

$$\Delta = y_0 \cdot f \cdot \delta, \text{ and}$$
$$\Delta = y_1 - y_0.$$

Solving this system of equations yields

$$\Delta = y_1 \frac{f \cdot \delta}{1 + f \cdot \delta}.$$

Given our assumption about which hours are assigned to treatment, $f = 12 \cdot (24 \cdot 7 - 2)/(365 \cdot 24) = 83/365$. During the fully treated years (2013–2017) there were an average of 585,340 crashes per year, so $y_1 = 585,340$. We obtain $\delta = .0198$ from column (1) of Table 9. Thus, $\Delta = 2,624$.

To calculate the number of additional fatalities per year, we replace y_1 with the average number of fatalities per year during 2013–2017, which is 3,650. Thus, the fatality messages cause an additional 16.4 fatalities per year. Likewise, there were 3,285 fatal crashes per year during 2013–2017, and so the fatality messages cause an additional 14.7 fatal crashes.

To put a dollar value on these additional crashes and deaths, we use estimates from a National Highway Traffic Safety Administration report (Blincoe et al., 2015). This report finds that in 2010 the total social cost of police-reported crashes was 747,540 million 2010 dollars (Table 1-12) and there were 6,077,362 police-reported crashes (Table 1-3). Thus, the average total social cost of a police-reported crash is 123,004 2010 dollars. Using the CPI to adjust to 2019, this is $145,000. Thus, the estimated total cost is 377 million dollars per year.

To extrapolate these numbers to all treated states, we multiple them by the number of licensed drivers in all treated states divided by the number of licensed drivers in Texas.