

## RESEARCH ARTICLE SUMMARY

## TRAFFIC SAFETY

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

Jonathan D. Hall and Joshua M. Madsen\*

**INTRODUCTION:** Policy-makers are increasingly turning to behavioral interventions such as nudges and informational campaigns to address a variety of issues. Guidebooks say that these interventions should “seize people’s attention” at a time when they can take the desired action, but little consideration has been given to the costs of seizing one’s attention and to the possibility that these interventions may crowd out other, more important, considerations. We estimated these costs in the context of a widespread, seemingly innocuous behavioral campaign with the stated objective of reducing traffic crashes. This campaign displays the year-to-date number of statewide roadside fatalities (fatality messages) on previously installed highway dynamic message signs (DMSs) and has been implemented in 28 US states.

**RATIONALE:** We estimated the impact of displaying fatality messages using data from Texas. Texas provides an ideal setting because the Texas Department of Transportation (TxDOT) decided to show fatality messages starting in

August 2012 for 1 week each month: the week before TxDOT’s monthly board meeting (campaign weeks). This allows us to measure the impact of the intervention, holding fixed the road segment, year, month, day of week, and time of day. We used data on 880 DMSs and all crashes occurring in Texas between 1 January 2010 and 31 December 2017 to investigate the effects of this safety campaign. We estimated how the intervention affects crashes near DMSs as well as statewide. As placebo tests, we estimated whether the chosen weeks inherently differ using data from before TxDOT started displaying fatality messages and data from upstream of DMSs.

**RESULTS:** Contrary to policy-makers’ expectations, we found that displaying fatality messages increases the number of traffic crashes. Campaign weeks realize a 1.52% increase in crashes within 5 km of DMSs, slightly diminishing to a 1.35% increase over the 10 km after DMSs. We used instrumental variables to recover the effect of displaying a fatality

message and document a significant 4.5% increase in the number of crashes over 10 km. The effect of displaying fatality messages is comparable to raising the speed limit by 3 to 5 miles per hour or reducing the number of highway troopers by 6 to 14%. We also found that the total number of statewide on-highway crashes is higher during campaign weeks. The social costs of these fatality messages are large: Back-of-the-envelope calculations suggest that this campaign causes an additional 2600 crashes and 16 fatalities per year in Texas alone, with a social cost of \$377 million per year.

Our proposed explanation for this surprising finding is that these “in-your-face,” “sobering,” negatively framed messages seize too much attention (i.e., are too salient), interfering with drivers’ ability to respond to changes in traffic conditions. Supporting this explanation, we found that displaying a higher fatality count (i.e., a plausibly more attention-grabbing statistic) causes more crashes than displaying a small one, that fatality messages are more harmful when displayed on more complex road segments, that fatality messages increase multi-vehicle crashes (but not single-vehicle crashes), and that the impact is largest close to DMSs and decreases over longer distances. We discuss seven alternative hypotheses, including the possibilities that treated weeks are inherently more dangerous and that fatality messages help in the long run. We provide evidence inconsistent with each alternative hypothesis.

**CONCLUSION:** Our study highlights five key lessons. First, and most directly, fatality message campaigns increase the number of crashes, so ceasing these campaigns is a low-cost way to improve traffic safety. Second, behavioral interventions can be too salient, crowding out more essential considerations and causing the intervention to backfire with costly consequences. Thus the message, delivery, and timing of behavioral interventions should be carefully designed so they are not too salient relative to individuals’ cognitive loads when the intervention occurs. Third, individuals don’t necessarily habituate to behavioral interventions, even after years of treatment. Fourth, the effects of interventions do not necessarily persist after treatment stops. Finally, it is important to measure an intervention’s effect, even for simple interventions, because good intentions do not necessarily imply good outcomes. ■

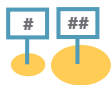
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## Displaying death counts causes 4.5% more crashes within 10 km



### Why? It distracts drivers

#### How we know it distracts drivers:



Effect is larger when the number displayed is larger...



and when road segments are more complex.



Effect is immediate and decreases over distance...



and increases multi-vehicle, but not single-vehicle, crashes.

### Key lessons for behavioral campaigns



Displaying death counts causes crashes.



Grabbing too much attention is dangerous.



People don't habituate to nudges.



Impact does not persist after treatment stops.



Evaluate new policies. Good intentions don't always lead to good outcomes.

A traffic safety campaign that leads to more crashes.

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<https://doi.org/10.1126/science.abm3427>

## RESEARCH ARTICLE

## TRAFFIC SAFETY

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

Jonathan D. Hall<sup>1,2</sup> and Joshua M. Madsen<sup>3\*</sup>

Although behavioral interventions are designed to seize attention, little consideration has been given to the costs of doing so. We estimated these costs in the context of a safety campaign that, to encourage safe driving, displays traffic fatality counts on highway dynamic message signs for 1 week each month. We found that crashes increase statewide during campaign weeks, which is inconsistent with any benefits. Furthermore, these effects do not persist beyond campaign weeks. Our results show that behavioral interventions, particularly negatively framed ones, can be too salient, crowding out more important considerations and causing interventions to backfire—with costly consequences.

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 now used by >200 governments and institutions worldwide to address a variety of issues, including voter turnout, charitable giving, retirement savings, water conservation, energy conservation, hand washing, caloric intake, diarrhea, and risky sexual behavior (1–3). Many of these interventions are expressly designed to “seize people’s attention” at a time when they can make the desired action (4), a characteristic that we refer to as salience (5–7). However, little consideration has been given to individuals’ cognitive constraints and to the possibility that seizing one’s attention may crowd out other, more important considerations (such as focusing on the task at hand).

Our context is a seemingly innocuous behavioral campaign with the stated objective of reducing traffic crashes, the leading cause of death for 5- to 45-year-olds in the United States and worldwide (8, 9). This campaign displays the year-to-date count of statewide roadside fatalities on previously installed dynamic message signs (DMSs) (e.g., “1669 deaths this year on Texas roads”; fig. S1). 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” (10, 11). Because of its low cost and ease

of implementation, this campaign has spread to at least 28 US states since 2012 and affected >100 million drivers (12).

This campaign is widely believed to be effective. For instance, in Illinois, the decision to start displaying fatality messages was unanimously supported by the Department of Transportation, the State Police, and the Department of Public Health (13). Many drivers also believe that fatality statistics make safety messages more effective (14, 15). Belief in the effectiveness of these messages is likely a factor in their rapid spread.

One key challenge when measuring the effect of fatality messages on crashes is that they are frequently displayed during safer times, when the DMS is not being used for more pressing concerns (e.g., travel times and crash alerts), biasing any naïve analysis toward finding a lower frequency of crashes when fatality messages are displayed (16).

The State of Texas provides a unique setting in which to overcome this challenge. Unlike most states, the Texas Department of Transportation (TxDOT) displays the current statewide fatality count only 1 week each month: the week before TxDOT’s monthly board meeting. Although more important messages regularly preempt 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]” (fig. S2). We confirmed that fatality messages concentrate in the designated weeks and used this assignment to treatment to estimate the effect of fatality messages on traffic crashes.

We estimated the effect of displaying fatality messages relative to the status quo usage of DMSs by comparing how the number of crashes downstream of a DMS differs the week before a TxDOT board meeting (“campaign week”) relative to the same road segment the rest of the month. We conducted our analysis at the segment-hour level and in-

cluded an extensive fixed-effect structure to control for inherent variation in crash risk 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 2:00 to 3:00 p.m. on Thursday, 18 July 2013 (which occurred during the week before a board meeting) against the number of crashes on the same road segment from 2:00 to 3:00 p.m. on the other three Thursdays in July 2013.

We conducted two tests to address the possibility that the weeks before TxDOT board meetings are inherently more dangerous than other weeks within the same month. First, we estimated the change in crashes occurring upstream of DMSs. Second, we estimated a placebo effect using data from before TxDOT began displaying fatality messages.

Our main results are difference-in-differences estimates that exploit both within-month variation in when fatality messages are instructed to be displayed (campaign weeks versus other weeks) and differences between the pretreatment (January 2010 to July 2012) and treatment (August 2012 to December 2017) periods.

## Results

We begin with univariate analyses documenting an increase in crashes the week before 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, and segment-year-month-time-of-day-day-of-week fixed effects. We conclude by estimating the impact of displaying a fatality message using instrumental variables and show that the impact of campaign weeks has not dissipated over time.

## Univariate results

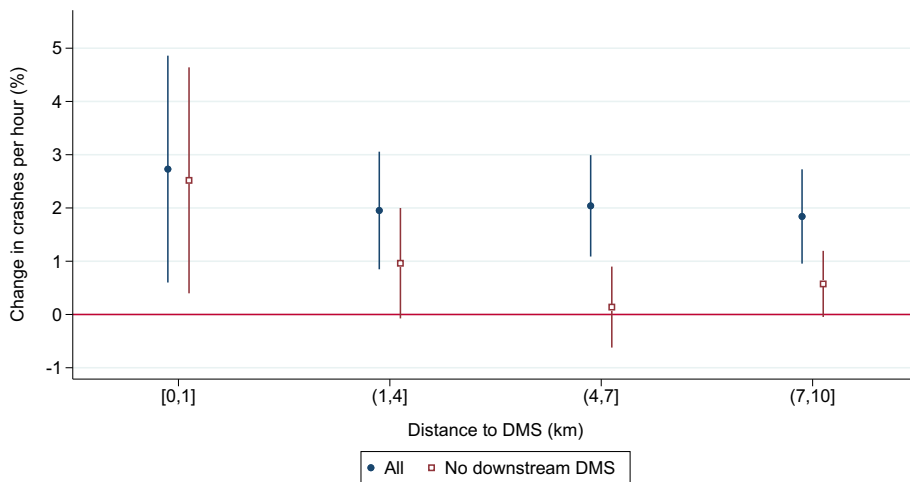
Figure 1 shows that there are more crashes downstream of DMSs during campaign weeks than in other weeks. Specifically, the circles plot the percentage difference in the average number of crashes occurring during campaign weeks versus other weeks over the segments [0,1], (1–4], (4–7], and (7–10] km downstream of DMSs. We found that there are more crashes during campaign weeks, with the largest effect being a 2.7% increase over the first kilometer ( $P = 0.012$ ). This effect diminishes to a 1.8% increase over the (7–10] km interval ( $P < 0.001$ ).

The results shown in Fig. 1 suggest that although the estimated effect diminishes over longer distances, it does not decay to 0. We conjectured that the increase in crashes over distances farther away from DMSs is caused by subsequent treatment by downstream DMSs. To map out the impact of fatality messages in

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**Fig. 1. Effect of fatality message on crashes by distance from DMS: Univariate.** Shown is the percent change in the number of crashes on Texas highways during weeks that precede TxDOT board meetings (campaign weeks) relative to all other weeks. Highway crashes are measured over hour  $h$  of day  $d$  over distance  $x$  (relative to DMS  $s$ ) and are indicated on the  $x$ -axis. The circles plot the difference in the average number of crashes during campaign weeks 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 of DMSs with no downstream DMS within  $x$  km; that is, for the distance (1,4], the closest downstream DMS is  $\geq 4$  km away. We scaled crash counts by the population average for all segments of the same distance  $x$  and multiplied by 100. Standard errors are clustered by geography-year-month, where geography indicates a bin of size  $x^2$  km<sup>2</sup> containing the DMS. The sample period is August 2012 to December 2017.

the absence of subsequent treatments, the hollow squares in Fig. 1 plot univariate differences in crash rates for the subset of DMSs in which there are no downstream DMSs within  $x$  km. We found that for DMSs with no downstream DMS within 7 or 10 km, the effect over distances (4,7] and (7,10] km becomes statistically insignificant, respectively. These results suggest that the immediate increase in crashes in response to the fatality message is short lived and concentrated after DMSs.

We next conducted two placebo tests to address the possibility that the week before board meetings is inherently more dangerous than other weeks. First, we examined the change in crashes upstream of DMSs. Because a segment upstream of one DMS may be downstream of another, we limited this test to DMSs in which the nearest upstream DMS is  $>10$  km away (reducing our sample by 75%). As shown in fig. S3A, we found no upstream effect for this restricted sample. All but one of the downstream estimates is  $>0$ , with a 1.7% increase over the (7–10] km downstream of DMSs ( $P = 0.018$ ). 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 nonrandom sample of DMSs, which mostly include DMSs on the edge of cities or in rural areas. Second, we estimated the change in crashes during the week before a TxDOT board meeting for the pretreatment

period. As shown in fig. S3B, we found neither a downstream effect nor a positive upstream effect during this period.

#### Multivariate results

We next show that these results hold when using more rigorous specifications that adjust for weather, holidays, and segment-year-month-time-of-day-day-of-week fixed effects. We start with first-difference estimates, finding that the more rigorous specification reduces the treatment effect by up to 50% (fig. S4). Multivariate versions of our two placebo tests produce similar results (fig. S5).

Table 1 reports our main results: difference-in-differences estimates that account for the uncertainty in whether the week before a board meeting is inherently more dangerous. Each column in Table 1 reports results for different highway segment lengths. The first row, “campaign week  $\times$  post,” estimates the treatment effect of fatality messages. We found that within 5 km of a DMS, there is a 1.52% increase in the number of crashes per hour ( $P = 0.025$ ), slightly diminishing to a 1.35% increase over the 10 km after DMSs ( $P = 0.025$ ). Within 3 km, the effect is positive but not statistically significant. The second row, “campaign week,” estimates the change in the number of crashes 1 week before board meetings from January 2010 to August 2012. Because this period predates the fatality safety campaigns, we expected and indeed found no effect (consistent with fig. S5B); these estimates

are both small and statistically insignificant. table S2 presents similar findings separately analyzing both pretreatment and treatment periods.

Our estimated magnitudes are large given the intervention’s simplicity and are estimates of the impact of campaign weeks on crashes. Because of imperfect compliance and competing demands, traffic engineers do not display fatality messages on all DMS hours during campaign weeks, implying that the effect of displaying a fatality message on a DMS is even larger. We used 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 was run on the subsample for which we have DMS log files, and the second-stage regression was run on the full sample. We bootstrapped standard errors. The first-stage results are presented in table S3, and the second-stage results are presented in table S4. We found that displaying a fatality message results in a positive but insignificant increase in crashes over the first 3 km. Consistent with our earlier results, we found a larger and statistically significant 5% increase in the number of crashes over 5 km and a slightly smaller increase of 4.5% over 10 km when fatality messages are displayed. These magnitudes are comparable to increasing the speed limit by 3 to 5 miles per hour (17) or reducing the number of highway troopers by 6 to 14% (18).

We found no evidence that the effect of displaying a fatality message has dissipated over time. Figure 2 plots coefficient estimates when the treatment effect is allowed to vary each year, using 2011 as the base year. We found that the treatment effect does not change from 2010 to 2012 (generally the pretreatment period). For all years after 2012 except 2016, the estimated coefficient is positive, with  $P$  ranging from 0.018 to 0.068 (19).

As described in the supplementary text, section S2, we found no evidence that the types of vehicles or drivers (by age and gender) involved in crashes differ during campaign weeks.

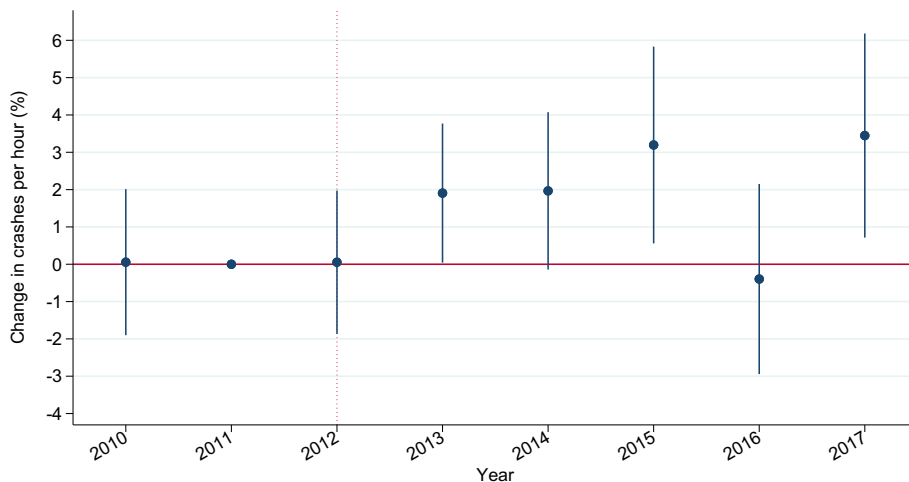
#### Mechanism

We have shown that displaying fatality messages on DMSs increases the number of crashes. In this section, we investigate the mechanism for this increase. A large body of research documents that attention and working memory are scarce resources, and that distractions create extraneous cognitive load that hampers individuals’ ability to process new information (7, 20–22). Examples include longer response times, more mistakes, and failure to process available information (23–25). Fatality messages plausibly add greater cognitive load than a typical DMS message because they are designed to be more salient than the typical message and (intentionally) communicate that



**Table 1. Effect of fatality messages on crashes.** Shown are estimates of the effect of campaign weeks on traffic crashes. The sample period is 1 January 2010 through 31 December 2017. The dependent variable is the number of crashes occurring on highway segment  $s$  of length  $x$  km 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 DMS located on a highway and continue for  $x$  km of highway driving distance, where  $x \in \{3,5,10\}$ , and are denoted in the column headers. We used as our primary right-side variable campaign week $_{d,h}$ , an indicator variable for whether day  $d$  and hour  $h$  fell within a campaign week. The variable post $_d$  indicates observations after 1 August 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 (S-Y-M-D-H) and holiday fixed effects (FE). Standard errors are clustered by geography-year-month and are shown in parentheses, where geography indicates a bin of size  $x^2$  km $^2$  containing the DMS.  $**P < 0.05$ . The equation used was as follows, where dow( $d$ ) is the day of the week associated with day  $d$ : crash(%) $_{s(x),d,h} = \delta \cdot$  campaign week $_{d,h} \cdot$  post $_d + \beta_1 \cdot$  campaign week $_{d,h} + \beta_2 \cdot$  trace precipitation $_{s,d,h} + \beta_3 \cdot$  trace precipitation $_{s,d,h} \cdot$  post $_d + \beta_4 \cdot$  precipitation $_{s,d,h} + \beta_5 \cdot$  precipitation $_{s,d,h} \cdot$  post $_d + \gamma_{s,m(d),dow(d),h} + \zeta_{\text{holiday}} + \epsilon_{s,d,h}$

	Crashes per hour (%)		
	3 km	5 km	10 km
	(1)	(2)	(3)
Campaign week × post	1.13 (0.86)	1.52 (0.68)**	1.35 (0.60)**
Campaign week	0.35 (0.63)	-0.27 (0.48)	-0.32 (0.43)
Observations	61,697,666	61,697,666	61,697,666
Adjusted R $^2$	0.02	0.03	0.08
Rain and interactions	Yes	Yes	Yes
S-Y-M-D-H FE	Yes	Yes	Yes
Holiday FE	Yes	Yes	Yes



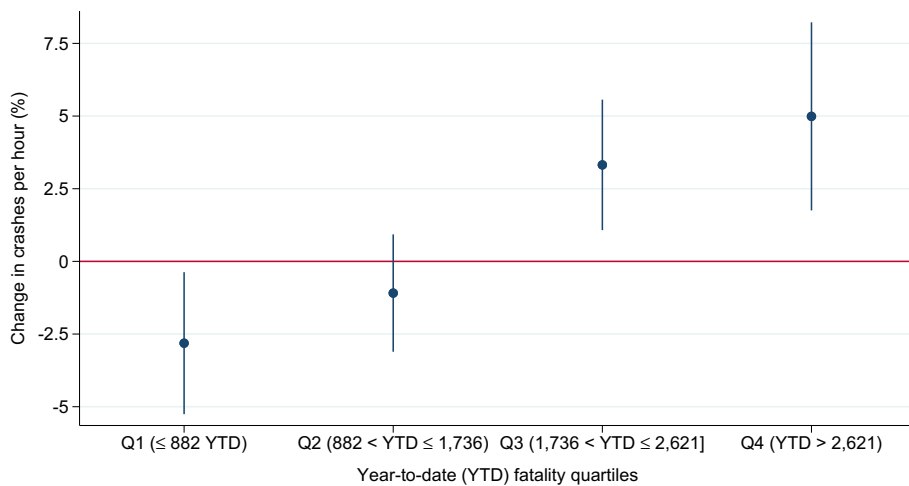
**Fig. 2. Effect of fatality messages on crashes by year.** Shown are the  $\delta_i$  coefficient estimates (circles) and the associated 95% confidence intervals (bars) from the regression below that allows the treatment effect to vary by year. Treatment effects are estimated relative to the treatment effect in 2011. The dependent variable, crash(%) $_{s(10),d,h}$ , is the scaled number of crashes occurring on day  $d$  during hour  $h$  over the 10 km downstream of DMS  $s$ ; campaign week $_{d,h}$  is an indicator variable for whether day  $d$  and hour  $h$  fell within a campaign week; and year $_{d,i}$  is an indicator variable if day  $d$  was in year  $i$ . Standard errors are clustered by geography-year-month bins, where geography bins are defined as the  $10^2$  km $^2$  containing the DMS. The dotted vertical line indicates that treatment started in August 2012. The equation used was as follows, where dow( $d$ ) is the day of the week associated with day  $d$ : crash(%) $_{s(10),d,h} = \sum_{i \in \{2010, 2012, \dots, 2017\}} \delta_i \cdot$  campaign week $_{d,h} \cdot$  year $_{d,i} + \beta_1 \cdot$  campaign week $_{d,h} + \beta_2 \cdot$  trace precipitation $_{s,d,h} + \beta_3 \cdot$  precipitation $_{s,d,h} + \gamma_{s,m(d),dow(d),h} + \zeta_{\text{holiday}} + \epsilon_{s,d,h}$

driving can be deadly (i.e., a negatively framed message). Relative to other messages, fatality messages may thus add more to drivers' cognitive loads by inducing anxiety about death. 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 on the task and causing some to overthink their actions, overriding faster automatic responses (26, 27). We thus hypothesized that fatality messages temporarily increase drivers' cognitive loads, reducing their ability to safely and quickly respond to changes in traffic conditions (e.g., stay in lane, maintain proper distance, respond to a vehicle changing lanes) and making them more likely to be involved in a crash. For conciseness, we refer to this as "distraction." We provide eight pieces of evidence supporting this hypothesis, focusing on the treatment effect on crashes occurring within 10 km of a DMS.

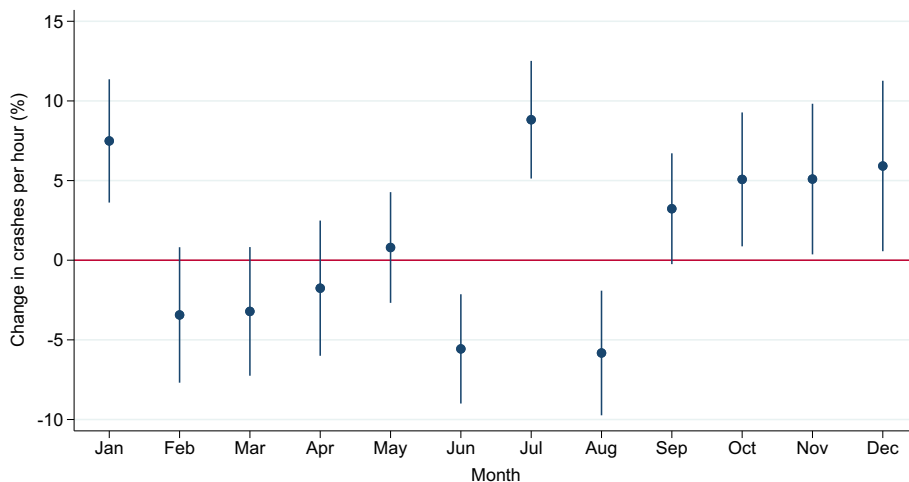
Our first piece of evidence supporting the distraction hypothesis is that the harm done by the fatality message is larger when the reported number of statewide deaths is larger, suggesting that bigger fatality statistics are more salient and distracting than smaller ones. We estimated a regression that allows the effect of campaign weeks to vary by the quartile of reported deaths. Figure 3 plots these results. When the number of reported deaths is small, displaying a fatality message decreases the number of crashes by 2.8% ( $P = 0.024$ ). However, as the number of reported deaths increases, the effect of displaying a fatality message on crashes grows more harmful, reaching a 5.0% increase ( $P = 0.003$ ).

Second, and closely related, the harm done by the fatality message increases throughout the year. Because the number of deaths reported mechanically climbs throughout the year, showing an increase in crashes throughout the year is an alternate way of showing that increases in the displayed number of deaths lead to more crashes. Figure 4 plots our difference-in-differences estimates of the treatment effect by calendar month. We found that displaying a fatality message in February, when the number of deaths displayed resets (January displays the prior year's total), reduces crashes by 3.4% ( $P = 0.113$ ); this effect then worsens throughout the year. From October through January, the effect is positive and statistically significant. In supplementary text, section S3, we discuss possible reasons why June, July, and August deviate from this pattern.

Third, as Fig. 4 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



**Fig. 3. Effect of fatality messages on crashes by YTD death quartile.** Shown are the  $\delta_i$  coefficient estimates (circles) and the associated 95% confidence intervals (bars) from the regression below that allows the treatment effect to vary by the year-to-date (YTD) number of deaths on Texas roads.  $YTD_{quartile}_{d,j}$  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 1 August 2012. Remaining variables are defined in Fig. 2 (see also table S9). Standard errors are clustered by geography-year-month bins, where geography bins are defined as the  $10^2$  km<sup>2</sup> containing the DMS. The sample period is January 2010 to December 2017. The equation used was as follows:

$$crash(\%)_{s(10),d,h} = \sum_{i \in \{quartile1, \dots, quartile4\}} \delta_i \cdot campaign_{week}_{d,h} \cdot YTD_{quartile}_{d,j} \cdot post_d + \sum_{i \in \{quartile1, \dots, quartile4\}} \beta_{1,i} \cdot campaign_{week}_{d,h} \cdot YTD_{quartile}_{d,j} + \beta_2 \cdot trace_{precipitation}_{s,d,h} + \beta_3 \cdot trace_{precipitation}_{s,d,h} \cdot post_d + \beta_4 \cdot precipitation_{s,d,h} + \beta_5 \cdot precipitation_{s,d,h} \cdot post_d + \gamma_{s,m(d),dow(d),h} + \zeta_{holiday} + \epsilon_{s,d,h}$$


**Fig. 4. Effect of fatality messages on crashes by calendar month.** Shown are the  $\delta_i$  coefficient estimates (circles) and the associated 95% confidence intervals (bars) from the regression below that allows the treatment effect to vary by calendar month:  $month_{d,j}$  as an indicator if day  $d$  occurs during calendar month  $i$ . Remaining variables are defined in Fig. 2 (see also table S9). Standard errors are clustered by geography-year-month bins, where geography bins are defined as the  $10^2$  km<sup>2</sup> containing the DMS. The sample period is January 2010 to December 2017. The equation used was as follows:

$$crash(\%)_{s(10),d,h} = \sum_{i \in \{Jan, \dots, Dec\}} \delta_i \cdot campaign_{week}_{d,h} \cdot month_{d,j} \cdot post_d + \sum_{i \in \{Jan, \dots, Dec\}} \beta_{1,i} \cdot campaign_{week}_{d,h} \cdot month_{d,j} + \beta_2 \cdot trace_{precipitation}_{s,d,h} + \beta_3 \cdot trace_{precipitation}_{s,d,h} \cdot post_d + \beta_4 \cdot precipitation_{s,d,h} + \beta_5 \cdot precipitation_{s,d,h} \cdot post_d + \gamma_{s,m(d),dow(d),h} + \zeta_{holiday} + \epsilon_{s,d,h}$$

that larger fatality numbers are more distracting 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 used 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 (VKTs) (28). We normalized these measures to have a mean of 0 and a standard deviation of 1, and interacted them with our treatment variable. As columns 1 to 3 of Table 2 show, we found that all three measures of complexity are associated with more crashes during campaign weeks. The first row shows that a 1 standard deviation increase in any of our measures of complexity is associated with 2.1 to 3.1% more crashes during treated weeks. The second row shows that for road segments of average complexity, displaying a fatality message is also associated with more crashes (statistically significant when complexity was 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 before a board meeting in the pretreatment period. Table S5 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.

Fifth, and closely related, the increase in crashes is higher on segments with nearby upstream DMSs. We measured the distance (on the road network) to the nearest upstream DMS, standardized this measure to have a mean of 0 and a standard deviation of 1, and multiplied by  $-1$  so that the measure is increasing in proximity to an upstream DMS. As column 4 of Table 2 shows, fatality messages displayed on DMSs with an average proximity to an upstream DMS are associated with a 1.35% increase in crashes ( $P = 0.024$ ), and increasing the closeness of the nearest upstream DMS by 1 standard deviation is associated with an incremental 0.6% increase in crashes ( $P = 0.026$ ).

This fifth finding is consistent with three explanations. First, it is consistent with fatality messages having larger effects when drivers face high cognitive loads, because drivers have likely seen multiple DMS messages on these segments. Second, it is consistent with an effect caused by repeated exposures either because it means that more drivers have seen the message at least once, distracting multiple drivers, or because seeing a fatality message repeatedly in quick succession increases the message's salience (and cognitive load). Finally, an increase in crashes on segments with

**Table 2. Effect of fatality messages on crashes: Segment characteristics.** Shown are estimates of how the effect of campaign weeks on traffic crashes varies by segment characteristics. “Measure” is one of the following characteristics of segment *s* (as indicated in the column header) standardized to have a mean of 0 and a standard deviation of 1: Centerline km, Lane km, VKT, and DMS proximity. See Table 1 for additional details and table S9 for detailed variable definitions. Standard errors are clustered by geography-year-month and are shown in parentheses. \*\*\**P* < 0.01; \*\**P* < 0.05. The equation used was as follows:  $\text{crash}(\%)_{s(10),d,h} = \delta_1 \cdot \text{campaign week}_{k,d,h} \cdot \text{measure}_s \cdot \text{post} + \delta_2 \cdot \text{campaign week}_{k,d,h} \cdot \text{post} + \beta_1 \cdot \text{campaign week}_{k,d,h} \cdot \text{measure}_s + \beta_2 \cdot \text{campaign week}_{k,d,h} + \beta_3 \cdot \text{trace precipitation}_{s,d,h} + \beta_4 \cdot \text{trace precipitation}_{s,d,h} \cdot \text{post}_{d,h} + \beta_5 \cdot \text{precipitation}_{s,d,h} + \beta_6 \cdot \text{precipitation}_{s,d,h} \cdot \text{post}_{d,h} + \gamma_{s,m(d),\text{down}(d),h} + \zeta_{\text{holiday}} + \epsilon_{s,d,h}$

	Crashes per hour >10 km (%)			
	Centerline km	Lane km	VKT	DMS proximity
	(1)	(2)	(3)	(4)
Campaign week × measure × post	2.05 (0.82)**	2.80 (0.98)***	3.05 (0.95)***	0.60 (0.27)**
Campaign week × post	1.61 (0.71)**	1.06 (0.69)	1.05 (0.69)	1.35 (0.60)**
Campaign week × measure	0.23 (0.53)	0.38 (0.71)	0.12 (0.67)	0.06 (0.20)
Campaign week	-0.21 (0.51)	-0.05 (0.55)	-0.04 (0.55)	-0.33 (0.43)
Observations	48,236,425	53,648,884	53,648,884	61,627,553
Adjusted <i>R</i> <sup>2</sup>	0.08	0.08	0.08	0.08
Rain and interactions	Yes	Yes	Yes	Yes
S-Y-M-D-H FE	Yes	Yes	Yes	Yes
Holiday FE	Yes	Yes	Yes	Yes

**Table 3. Effect of fatality messages by crash types.** Shown are estimates of the effect of campaign weeks on single- and multi-vehicle crashes. The dependent variable is the number of crashes occurring over the 10 km 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 1 for additional details. Standard errors are clustered by geography-year-month and are shown in parentheses. \*\**P* < 0.05. The equation used was as follows:  $\text{crash}(\%)_{s(10),d,h} = \delta \cdot \text{campaign week}_{k,d,h} \cdot \text{post}_{d,h} + \beta_1 \cdot \text{campaign week}_{k,d,h} + \beta_2 \cdot \text{trace precipitation}_{s,d,h} + \beta_3 \cdot \text{trace precipitation}_{s,d,h} \cdot \text{post}_{d,h} + \beta_4 \cdot \text{precipitation}_{s,d,h} + \beta_5 \cdot \text{precipitation}_{s,d,h} \cdot \text{post}_{d,h} + \gamma_{s,m(d),\text{down}(d),h} + \zeta_{\text{holiday}} + \epsilon_{s,d,h}$

	Crashes per hour (%)	
	Multi-vehicle	Single-vehicle
	(1)	(2)
Campaign week × post	1.60 (0.63)**	-0.26 (1.59)
Campaign week	-0.64 (0.44)	1.74 (1.13)
Observations	61,697,666	61,697,666
Adjusted <i>R</i> <sup>2</sup>	0.08	0.01
Rain and interactions	Yes	Yes
S-Y-M-D-H FE	Yes	Yes
Holiday FE	Yes	Yes

nearby upstream DMSs is also consistent with treatment mattering, because at least one of these DMSs is likely to have displayed a fatality message.

Sixth, fatality messages increase multi-vehicle crashes but not single-vehicle crashes. In Table 3, we separately examined whether multi- and single-vehicle crashes change the week before a board meeting. We found a 1.60% increase in crashes involving multiple vehicles (*P* = 0.011) and an insignificant change in crashes involving single vehicles. Because 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 displayed that are plausibly related to distracted driving (e.g., drifting out of the lane). An increase in multi-vehicle crashes is also consistent with fatality messages inducing more anxiety, and thus being more distracting, when driving conditions could be perceived as more dangerous (i.e., when other vehicles are nearby).

Seventh, the concentrated effect immediately after DMSs and decreasing effect size over longer distances previously documented is consistent with a temporary distraction effect. Prior research finds that the time to resume a task after an interruption increases with the complexity of the interruption (29). Shocking, salient fatality messages plausibly present such interruptions, and drivers are expected to eventually regain their ability to safely respond to changes in traffic conditions. At 100 km/h (62 mph, normal highway speeds), a driver will travel 5 km in 3 min. Our evidence thus suggests that the distracting effect of fatality messages lasts for more than a trivial amount of time, but that drivers do recover.

Finally, our proposed mechanism is consistent with evidence from the traffic safety literature. Most directly related, Shealy *et al.* (15) found, in a laboratory setting, that showing drivers fatality messages increased neuro-cognition, which is a proxy for attention/working memory and cognitive load. Although it is difficult to compare estimates across experiments, a rough estimate is that showing drivers a fatality message increases cognitive load by 50% (15, 30). Although there remains a debate on whether billboards cause crashes (31), recent studies using vehicle simulators have found that, depending on the content, billboards do cause people to drive worse as measured by variability in speed and lane position, reaction times, vehicle headway, and number of crashes (32–34). Furthermore, studies using vehicle simulators have found that increasing individuals’ anxiety causes them to drive worse, and that these effects can last for at least 2 km (35, 36). Thus, prior traffic safety research, combined with our seven pieces of evidence, provides strong support for a temporary distraction effect caused by fatality messages that reduces individuals’ ability to drive safely and respond to changes in traffic conditions.

**Alternative hypotheses**

In the supplementary text, section S4, we address seven alternative hypotheses, including the possibilities that treated weeks are inherently more dangerous, that fatality messages help in the long run or result in improvements away from DMSs, that displaying any message causes crashes, and that fatality messages cause some drivers to slow down, increasing the variance of vehicle speeds and thus crash risk. We provide evidence inconsistent with each of these alternative hypotheses.

**Robustness**

In table S6 we report several robustness tests of our difference-in-differences estimates. In particular, we show that clustering by segment-year-month reduces the standard error in half, clustering by just geography

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**Table 4. Effect of fatality messages on statewide crashes.** Shown are estimates of 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 shown in parentheses. \*\*\* $P < 0.01$ ; \*\* $P < 0.05$ . The equation used was as follows: statewide crashes (%) $_{d,h} = \delta \cdot \text{campaign week}_{k,d,h} \cdot \text{post}_{d,h} + \beta_1 \cdot \text{campaign week}_{d,h} + \gamma_{m(d),\text{dow}(d),h} + \zeta_{\text{holiday}} + \epsilon_{d,h}$

	Total	On-highway	Off-highway
	(1)	(2)	(3)
Campaign week × post	1.98 (0.96)**	2.77 (1.19)**	1.16 (0.95)
Campaign week	-1.61 (0.72)**	-2.39 (0.89)***	-0.79 (0.75)
Observations	70,127	70,127	70,127
Adjusted R <sup>2</sup>	0.87	0.82	0.84
Y-M-D-H FE	Yes	Yes	Yes
Holiday FE	Yes	Yes	Yes

produces slightly smaller standard errors, controlling for rain more flexibly does not affect our results, not controlling at all for rain doubles our estimated treatment effect, not controlling for holidays increases our estimate slightly, and dropping hours immediately before and after campaign weeks (i.e., hours outside of campaign weeks that sometimes display fatality messages, see fig. S6) further increases our estimate. Further, we show that the estimated treatment effect is larger when using alternative outcome measures; specifically, using an indicator variable for whether there is any crash or using the log of the number of crashes plus one. We did not use count data models (e.g., Poisson regression) because they require variation in the outcome within each fixed effect and are thus incompatible with our extensive fixed-effect structure.

All of our estimates so far have assumed that any DMS that existed during our sample existed for the entire sample. We also tested whether our results are robust to limiting the sample to the DMS months where each DMS existed. To do so, we collected information on when each DMS existed using Google Street View. For each DMS month, we either know a DMS exists, know it does not exist, or are unsure. To deal with this uncertainty over when they exist, we conducted two robustness tests. We first limited our sample to the DMS months in which we know the DMS exists, and then limited our sample to the DMS months in which the DMS might exist (i.e., we do not know that it does not exist). As expected, we found that including DMS months that lack 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.

Our main tests exploit GPS locations of both DMSs and crashes and uses an extensive fixed-effects structure. To evaluate whether a simpler approach provides similar conclusions, we evaluated the change in crashes statewide during campaign weeks. Results presented in Table 4 indicate that crashes, particularly on-highway crashes, also increase statewide during campaign weeks.

We also tested for an effect of fatality messages on several measures of crash seriousness. As shown in table S7, we found that the count of vehicles involved in crashes is 1.93% higher during campaign weeks in the treatment period ( $P = 0.002$ ). We do not have the power to detect an effect on the number of deaths, number of fatal crashes, and an estimate of the social cost of these crashes, because only 0.58% of crashes have a fatality. The 95% confidence intervals for these estimates are large, and we cannot rule out meaningful treatment effects.

**Discussion**

We present 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 displaying fatality messages does help. Specifically, these 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 1 standard deviation below its respective mean. Although these benefits do not outweigh the harm done, they show that behavioral interventions can help if they are not too salient and are delivered when individuals’ cognitive loads are low.

The effect of displaying a fatality message on crashes is large relative to the simplicity of the intervention. We estimate that displaying a fatality message increases the number of crashes over the next 10 km of roadway by 4.5%. Our estimates suggest that displaying these messages causes an additional 2600 crashes per year in Texas alone (see supplementary text S5 for details). Furthermore, although 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.* (37), these additional crashes have a total social cost of \$380 million per year. To calculate an estimate of the impact of fatality messages in the United States, we scaled our estimated treatment effect by the number of licensed drivers in the 28 treated states. Doing so suggests that across the United States, displaying these messages might cause an additional 17,000 crashes and 104 fatalities per year, with a total social cost of \$2.5 billion per year.

There are two sources of important variation across states in how fatality messages are implemented. First, US states differ in how frequently they show fatality messages. This matters because we found that most of the damage is done during the first few days that the message is displayed (fig. S7). This finding implies that in states where the fatality message is displayed all the time (unless there is a more important message), such as Illinois, the effects could be more benign, and in places where fatality messages are displayed 1 day per week, such as Colorado, the effect could be worse. Second, whereas the exact text of the message is consistent across states, the displayed fatality account varies. Texas, the second-largest state in the United States, displays larger fatality counts than most other states. This matters because we found that fatality messages only hurt when the displayed fatality count is large (Fig. 3). If the negative effect of the fatality message depends on the absolute number displayed, then it will not have the same negative effect in most other states. However, if the negative effect depends on the number displayed relative to a state’s population, then our results are more generalizable to other states. For additional discussion of this study’s external validity, see the supplementary text, section S6.

**Conclusion**

Our study shows that salient, generic, in-your-face safety messages delivered to drivers crowd out more pressing safety concerns, yielding immediate negative and socially undesirable outcomes. The treatment effect is larger when the reported number of deaths is larger and when road segments are more complex. Our evidence suggests that even after several years,

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an intervention delivered 1 week each month still increases crashes. Further, the negative effects of these messages appear to be constrained to the immediate vicinity and time where delivered.

These findings contribute to three areas of research. Existing research on DMS safety messages finds evidence that messages about speeding, fog, or slippery roads are effective at reducing drivers' speeds (38–40), but that generic safety messages have little effect (41). The traffic safety literature also finds that drivers rate negatively framed threat appraisals as more effective (42–44), but that such messages can be perceived as controlling or manipulative, potentially causing people to ignore them (45). Shealy *et al.* (15) found, in a laboratory setting, that showing drivers non-traditional safety messages, including fatality messages, increases their attention and cognitive load, which they interpreted as a good thing. By contrast, we show that this can have costly consequences. We show that, contrary to drivers' and policy-makers' expectations, using fatality messages to increase awareness of the risk of driving causes additional traffic crashes.

We also contribute to the literature on risk disclosures (46–49) and the broader literature on information disclosure (50–52). Although risk disclosures are common in many markets, many tend to be generic rather than specific (e.g., “driving is dangerous” versus “sharp turn ahead”). There is concern that generic risk disclosures may be ineffective at reducing risk-taking because of 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 found that generic, yet plausibly shocking, risk disclosures can affect individual behavior.

Finally, we contribute to the literature on behavioral interventions. An important question within this literature is whether the effects of behavioral interventions persist after treatment stops. Although there are some notable exceptions (53, 54), this literature typically finds little persistence (55). We found no evidence that fatality messages affect behavior outside of campaign weeks. Another question in this literature is whether individuals habituate to behavioral interventions, as budget considerations make it difficult to test the long-term effects (53). We found that drivers do not habituate to fatality messages, potentially because the number of displayed deaths is constantly updated, with the treatment effect remaining virtually unchanged 5 years after the initial implementation. This should increase confidence that the estimated short-term benefits in other studies may persist in the long term.

This evaluation of fatality messages highlights five key lessons. First, and most importantly, behavioral interventions can fail if they

increase individuals' cognitive loads to the extent that they crowd out more important considerations. Thus, given that behavioral interventions are intentionally designed to be salient and seize attention, the message, delivery, and timing must be carefully designed to prevent the intervention from back-firing. Second, because individuals face cognitive constraints, a full accounting of an intervention's welfare effects should consider whether adding to participants' cognitive loads has effects outside of the targeted domain. Third, our results speak to the trade-offs of using behavioral interventions versus taxes and subsidies to address externalities such as unsafe driving, pollution, and global warming (56, 57). The general lesson is that behavioral interventions targeting externalities that are largest when individuals' cognitive loads are high are likely to be less efficient than taxes or subsidies. Fourth, measuring an intervention's effect is important, even for simple interventions, because good intentions need not imply good outcomes. Finally, and most directly, fatality message campaigns increase the number of crashes, so ceasing these campaigns is a low-cost way to improve traffic safety.

## Materials and methods

### Data

We collected data on traffic crashes, DMS locations and messages displayed, TxDOT's board meeting schedule, weather, the Texas road network, and US federal holidays. Table S8 summarizes the January 2010 to July 2012 “pretreatment” period (i.e., the time period before TxDOT began displaying fatality messages) and the August 2012 to December 2017 “treatment” period. We collected data on 880 DMSs.

Our data on traffic crashes comes from the TxDOT Crash Records Information System and includes all reported crashes occurring on Texas roads. This dataset includes the GPS coordinates and other characteristics for each crash from 2010 to 2017.

We collected DMS location data from the TxDOT website and from lists provided by TxDOT of all DMSs in 2013 and 2015. We combined these location data and validated and corrected them using Google Maps. We corrected 18% of the DMS locations and updated the direction of travel for 26 DMSs. We dropped 175 DMSs that were portable, test DMSs, or smaller than standard. These smaller DMSs are often just able to display a few characters and used for displaying travel times or tolls. The largest DMSs that we dropped for being too small can display two lines of 12 characters, whereas standard DMSs can display three lines of 15 or 18 characters. We also dropped nine DMSs located on local roads rather than on highways. Fig. S8 plots the locations of DMSs within the

entire state, and fig. S9 plots those in the Houston area. These maps show that DMSs are located primarily within urban areas, and that within urban areas, DMSs are spaced fairly evenly apart, with a median driving distance of 5.3 km between consecutive DMSs.

We collected information on when each DMS exists using Google Street View. These data are limited because 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, whereas 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 these data, we know that at least 24% of DMSs did not exist over our entire sample. Our main results assume that all DMSs that exist during our sample exist for the entire sample. Including nonoperational DMSs biases our results toward 0.

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

We collected TxDOT's board meeting schedule from the TxDOT website. These 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 obtained hourly weather data from the US National Oceanic and Atmospheric Administration's Integrated Surface Database. Figures S8 and S9 also show the locations of the weather stations that we used. The median distance between a DMS and the nearest weather station is 14 km.

### Variable definitions

Our primary outcome variable is the hourly number of crashes on a given road segment. Road segments begin at DMS locations and continued for  $x$  km of highway driving distance, with  $x \in \{-10, -9, \dots, 9, 10\}$ ; negative distances denote segments preceding the DMS (i.e., upstream) and positive distances denote segments continuing past the DMS (i.e., downstream). We calculated driving distances using the Open Source Routing Machine and Open Street Maps data for the Texas highway network. Our network includes all roads classified as motorways, motorway links, trunk roads, and primary roads. This is the smallest set of classifications that includes all highways but also includes some roads that are not highways. Figure S10 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 allowed 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 scaled 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  $\text{crash}(\%)_{s(x),d,h}$  where the subscripts index segment ( $s$ ), segment length ( $x$ ), day ( $d$ ), and hour ( $h$ ). See table S9 for detailed variable definitions.

Lane kilometers and average daily VKT were measured from the Highway Performance Monitoring System annually, and centerline kilometers were measured using Open Street Maps. All three were measured over each segment.

We defined campaign weeks using the schedule of TxDOT board meetings and our DMS log files. Since August 2012, TxDOT traffic engineers have been instructed to display the fatality message beginning “after morning peak” on the Monday 1 week before a board meeting and ending “before morning peak” on the following Monday. Exact times are not provided, because “morning peak” varies by highway and direction of travel.

We determined the typical start and end times of campaign weeks using our DMS log files. Figure S6 shows that a fatality message is displayed for ~8% of the DMS hours between midnight and 7:00 am on the Monday 1 week before board meetings (designated first day), increasing to 12, 18, and 29% during the 7:00, 8:00, and 9:00 a.m. hours, respectively, and is then displayed for ~29 to 43% of DMS hours during the campaign week. Percentages <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 disappear at the end of the campaign week, with the fatality message displaying for 21, 14, and 11% of DMS hours during the 6:00, 7:00, and 8:00 a.m. hours of the final Monday, respectively. Thus, although there is leakage into hours immediately before and after the intended display period, we found that fatality messages concentrate during the designated week. On the basis of the patterns observed in fig. S6, we defined an indicator variable for the week before a board meeting,  $\text{campaign week}_{d,h}$  which equals 1 for all days ( $d$ ) and hours ( $h$ ) between 9:00 a.m. on the Monday 10 days before a scheduled board meeting and 7:00 a.m. the following Monday. In the supplementary text, section S7, we further explore how campaign weeks affect the messages displayed.

To control for variation in weather conditions, we defined two indicators for whether

the weather station closest to DMS  $s$  reported precipitation during hour  $h$  of day  $d$ . Specifically,  $\text{trace precipitation}_{s,d,h}$  was set equal to 1 if the weather station reported <1 mm of precipitation, and 0 otherwise, and  $\text{precipitation}_{s,d,h}$  was set equal to 1 if the weather station reported  $\geq 1$  mm of precipitation, and 0 otherwise.

Because we did not observe the displayed death count in every month, we imputed the year-to-date fatality count for each month using the actual number of year-to-date fatalities. From the DMS log files, we found that the reported fatality number is reported with a median lag of 22 days, and we used this lag when imputing the number of fatalities for each month.

Table S10 reports summary statistics for our data. As discussed earlier, because of the increasing surface area covered by segments of larger lengths, the number of crashes per hour increases more than proportionally in segment length, with 8.2 times more crashes within 10 km of a DMS than within 3 km. Crashes are proportional to lane kilometers and VKT (table S11).

### Research design

To estimate the effect of fatality messages on the number of traffic crashes, we exploited 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 included segment-year-month-day-of-week-hour fixed effects. Because fatality messages are only instructed to be displayed for 1 week each month, we could compare, for each DMS highway segment, year-month-day-of-week-hours when the message was instructed versus not instructed to be displayed. We also included controls for precipitation and holiday fixed effects. We estimated the following ordinary least-squares regression using all observations from 1 August 2012 through 31 December 2017 as follows:

$$\text{crash}(\%)_{s(x),d,h} = \delta \cdot \text{campaign week}_{d,h} + \beta_1 \cdot \text{trace precipitation}_{s,d,h} + \beta_2 \cdot \text{precipitation}_{s,d,h} + \gamma_{s,m(d),dow(d),h} + \zeta_{\text{holiday}} + \epsilon_{s,d,h} \quad (1)$$

In Regression 1,  $\delta$  is our estimated treatment effect,  $\gamma$  is a fixed effect for each segment-year-month-day-of-week-hour;  $\zeta$  is a fixed effect for each holiday, and  $dow(d)$  is the day of the week associated with day  $d$ .

We also estimated 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 the treatment and pretreatment periods. This approach

directly addresses the concern that campaign weeks could systematically differ from other weeks (e.g., total traffic volume or crash risk). Specifically, we estimated the following regression:

$$\text{crash}(\%)_{s(x),d,h} = \delta \cdot \text{campaign week}_{d,h} \cdot \text{post}_d + \beta_1 \cdot \text{campaign week}_{d,h} + \beta_2 \cdot \text{trace precipitation}_{s,d,h} + \beta_3 \cdot \text{trace precipitation}_{s,d,h} \cdot \text{post}_d + \beta_4 \cdot \text{precipitation}_{s,d,h} + \beta_5 \cdot \text{precipitation}_{s,d,h} \cdot \text{post}_d + \gamma_{s,m(d),dow(d),h} + \zeta_{\text{holiday}} + \epsilon_{s,d,h} \quad (2)$$

which is equivalent to taking the difference between  $\delta$  from Regression 1 for the August 2012 to December 2017 sample and  $\delta$  from the same regression for the January 2010 to July 2012 sample. In Regression 2,  $\delta$  is the coefficient of interest. In our analyses, we found no difference during the pretreatment period in downstream crashes between the week before a board meeting (“campaign weeks”) and other weeks, so the primary difference between Regressions 1 and 2 is that the second has larger standard errors. This occurs because Regression 1 presumes that campaign weeks would be exactly the same as other weeks in the absence of treatment, whereas Regression 2 acknowledges uncertainty about whether campaign weeks would be the same in the post-period in the absence of treatment.

As a conservative approach, we clustered standard errors by geography-year-month, where geography refers to bins of size  $x^2$  km<sup>2</sup> that contain a DMS segment of length  $x$ . Thus, fewer clusters (geographic bins larger in area) are used for segments of greater length, because crashes occurring over those longer lengths may be linked to multiple DMSs.

See the supplementary text, section S8, for additional discussion of our materials and methods.

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28. Consider the following three examples of a 10-km segment to contrast centerline and lane kilometers: (i) a straight road with four lanes; (ii) a Y-shaped road that splits halfway, with all parts having four lanes; and (iii) a Y-shaped road that splits halfway where the trunk is four lanes and each branch is two lanes. Segment (i) has 10 centerline km and 40 lane km, segment (ii) has 15 centerline km and 60 lane km, and (iii) has 15 centerline km and 40 lane km.
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## SUPPLEMENTARY MATERIALS

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Supplementary Text S1 to S8

Figs. S1 to S18

Tables S1 to S19

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

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### When behavioral nudges fail

Do traffic safety interventions work? Hall and Madsen present evidence from a study in Texas showing that the number of crashes actually increases by a few percentage points when motorists are confronted with displays indicating the number of road fatalities in the area (see the Perspective by Ullman and Chrysler). The authors suggest that this counterintuitive finding results from a cognitive overload experienced by drivers when confronted with multiple notices and instructions on complex stretches of road, leading to distraction. They conclude that traffic safety “nudges” need to be carefully designed and positioned to avoid backfiring. —AMS

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