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9/11, Act II: A Fine-Grained Analysis of Regional Variations in Traffic Fatalities in the Aftermath of the Terrorist Attacks

Wolfgang Gaissmaier and Gerd Gigerenzer
Harding Center for Risk Literacy, Max Planck Institute for Human Development, Berlin, Germany

Abstract

Terrorists can strike twice—first, by directly killing people, and second, through dangerous behaviors induced by fear in people's minds. Previous research identified a substantial increase in U.S. traffic fatalities subsequent to the September 11 terrorist attacks, which were accounted for as due to a substitution of driving for flying, induced by fear of dread risks. Here, we show that this increase in fatalities varied widely by region, a fact that was best explained by regional variations in increased driving. Two factors, in turn, explained these variations in increased driving. The weaker factor was proximity to New York City, where stress reactions to the attacks were previously shown to be greatest. The stronger factor was driving opportunity, which was operationalized both as number of highway miles and as number of car registrations per inhabitant. Thus, terrorists' second strike exploited both fear of dread risks and, paradoxically, an environmental structure conducive to generating increased driving, which ultimately increased fatalities.

Keywords

decision making, risk perception, risk taking, terrorism

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Terrorists can strike twice—first, by directly killing people, and second, through dangerous behaviors induced by fear in people's minds. In the aftermath of the terrorist attacks on September 11 (9/11), 2001, from October 2001 to September 2002, there were approximately 1,600 more traffic fatalities in the United States than would normally be expected—an increase about 6 times the number of people (256) who died in the aircrafts on September 11 (Gigerenzer, 2006). To explain the increase in traffic fatalities, Gigerenzer (2004) proposed the *dread hypothesis*:

> People tend to fear dread risks, that is, low-probability, high-consequence events, such as the terrorist attack on September 11, 2001. If [a] Americans avoided the dread risk of flying after the attack and [b] instead drove some of the unflown miles, one [c] would expect an increase in traffic fatalities. (p. 286)

This three-part hypothesis was based on a suggestion by Myers (2001) and first tested for the 3 months after 9/11 by Gigerenzer (2004), who found support for each part.

Su, Tran, Wirtz, Langteau, and Rothman (2009) divided the United States into three regions and found that fatalities increased solely in the Northeast, in proximity to New York City (NYC), but not in the rest of the country. Here, we report more fine-grained analyses based on data from the 50 individual states plus Washington, D.C. Figure 1a depicts the absolute monthly increase in fatalities per million inhabitants in each state for the 3 months after 9/11. Although traffic fatalities indeed increased in the Northeast, the map clearly shows that substantial increases also occurred in many states more distant from NYC, particularly in the Midwest.

We propose that a combined analysis of psychological reactions to terrorism and the structure of the environment can explain these substantial variations. To describe the relation between mind and environment, Simon (1990) coined the metaphor of "a scissors whose two blades are the structure of task environments and the computational capabilities of the actor" (p. 7). In the case under consideration, fear induced by terrorist attacks is one blade; the other is an environmental structure that allows fear to become manifest in dangerous behavior such as driving instead of flying. Specifically, we consider driving opportunity to be the second blade.

We hypothesized that both fear and good driving opportunity are relevant for explaining where driving actually increased in

Corresponding Author:
Wolfgang Gaissmaier, Max Planck Institute for Human Development, Harding Center for Risk Literacy, Lentzeallee 94, 14195 Berlin, Germany
E-mail: gaissmaier@mpib-berlin.mpg.de

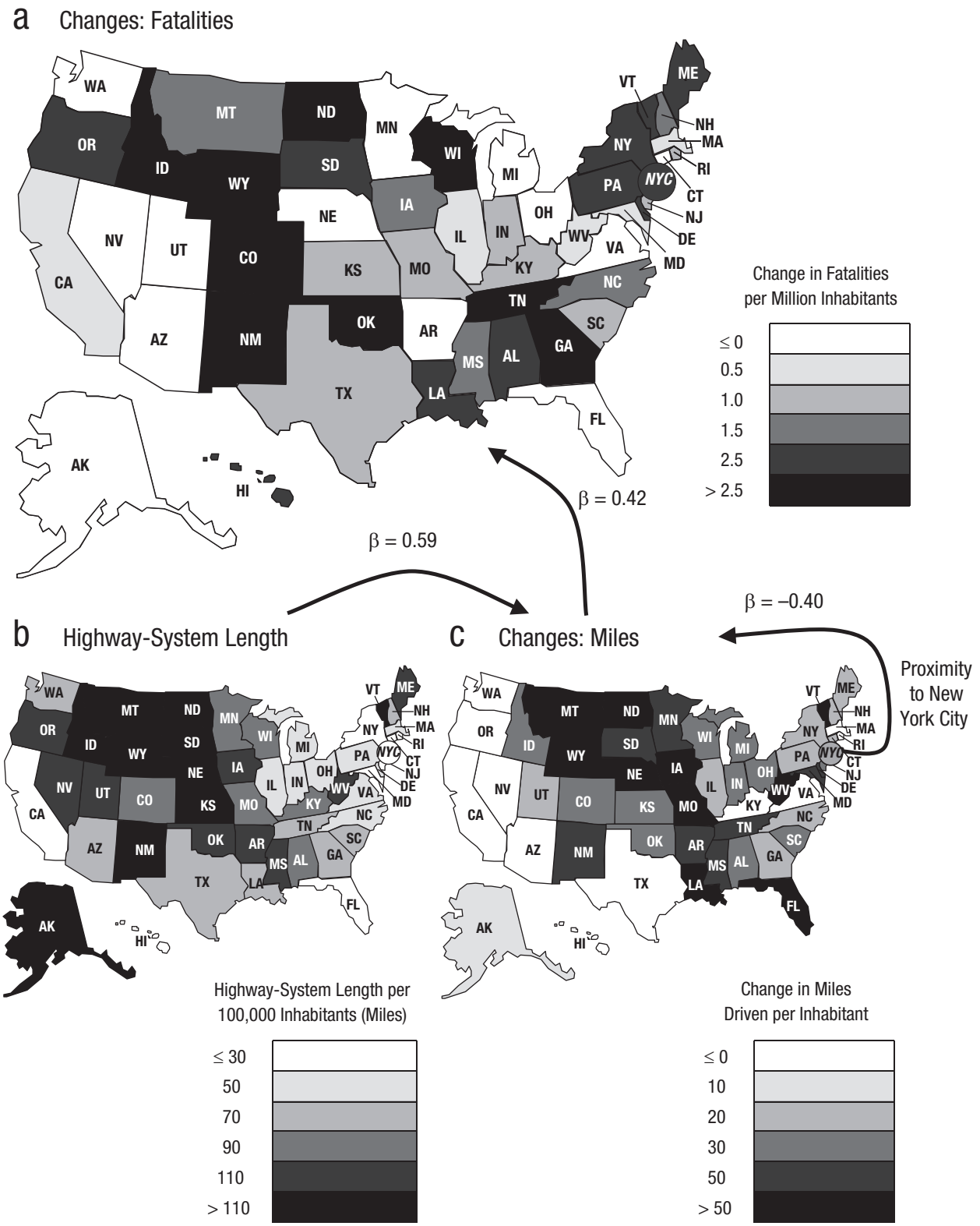

Fig. 1. U.S. maps showing, by state, (a) the change in driving fatalities in the 3 months subsequent to the September 11 terrorist attacks, (b) highway-system length, and (c) the change in the number of miles driven in the 3 months subsequent to the attacks. Also shown are results of regression analyses in which highway-system length and proximity to New York City were predictors of changes in miles driven, which, in turn, predicted changes in driving fatalities.

the aftermath of 9/11. We expected that increased driving, in turn, would explain increased traffic fatalities in those areas. As a prerequisite, we tested whether driving actually increased, which Su et al. (2009) contested.

Method

Overview

We analyzed data for the 3 months following the 9/11 attacks (i.e., October to December 2001) and compared them with data for the same period in the 5 previous years (i.e., 1996 to 2000). To rule out the possibility that there was anything unusual about the year 2001 in general, we also investigated the pre-September months in 2001 (i.e., January to August). Except where we indicate otherwise, the data were based on official sources (U.S. Census Bureau, 2002, 2011, n.d.; U.S. Department of Transportation, Federal Highway Administration, 2004, n.d.; U.S. Department of Transportation, National Highway Traffic Safety Administration, n.d.). The 50 individual states plus Washington, D.C., were the units of analysis.

Measures

Changes in miles driven and traffic fatalities. We computed changes (from the previous year) in the numbers of traffic fatalities and miles driven for each of the 50 states plus Washington, D.C., for the years 1996 to 2001. Unlike in previous research, we analyzed absolute changes per inhabitant (miles driven) and absolute changes per million inhabitants (traffic fatalities) rather than relative percentage changes. Absolute changes are more robust than relative changes when the number of observations is small, as it was for small states, particularly regarding fatalities. Furthermore, absolute changes are symmetrical for increases and decreases (e.g., the absolute difference for an increase from 5 to 10 is identical to the absolute difference for a decrease from 10 to 5, whereas the relative changes in this case are +100% and −50%, respectively). For instance, to determine a state's increase in fatalities for the post-September months of 2001 (October–December), we calculated the following for each month: fatalities 2001/millions of inhabitants 2001 – fatalities 2000/millions of inhabitants 2000; we then averaged the results for these 3 months. As a control, we recalculated each analysis using relative instead of absolute changes; all results remained virtually identical.

Fear. We could not measure post-9/11 fear directly, but in agreement with Su et al. (2009), we find it plausible that fear was higher for people who lived closer to NYC, where the major attacks happened. Supporting this assumption, a survey conducted a few days after the attacks showed that people closest to NYC had the highest rate of substantial stress reactions and that the rate of such reactions decreased with distance from NYC (Schuster et al., 2001). To measure proximity to NYC for each state, we used an online mapping and distance tool (http://www.acscdg.com/), entering NYC as the starting point and the name of the state as the end point. Because only NYC was a well-defined point on the map, the tool standardized the choice of one point within each state; these points were approximately in the middle of the states.

Driving opportunity. Driving opportunity in each state for the year 2001 was calculated as the length of the state's nationally significant highways—defined by inclusion in the National Highway System (NHS) in 2001—divided by the number of the state's inhabitants in 2001. We chose inclusion in the NHS as our criterion because the NHS represents only 4% of the total U.S. roads but 40% of overall traffic and 90% of tourist traffic, and because 90% of the U.S. population lives within 8 km of an NHS road (Slater, 1996). We used length per capita rather than absolute length because absolute length naturally increases with the number of inhabitants and thus is a stronger reflection of state population than of driving opportunity. This variable was skewed; hence, we log-transformed the values. To check the results for robustness, we also operationalized driving opportunity as the number of car registrations per inhabitant.

Results

Miles driven

Across states, the average monthly increase in miles driven per inhabitant in the post-September months of 2001 was 27.2 miles, which was substantially larger than the average increase of 9.9 miles observed in the previous 5 years in those same months, paired-sample $t(50) = 3.90$, 95% confidence interval (CI) on the difference: [8.4 miles, 26.2 miles], mean difference = 17.3 miles. In 36 of the 51 cases, the change was larger than the average change in the previous 5 years (sign test: $Z = 2.80$, $p = .005$). For the pre-September months, the average monthly changes in miles driven in 2001 did not differ from the average changes in the 5 previous years or were even lower, paired-sample $t(50) = -1.85$, 95% CI on the difference: [−17.0 miles, 0.7 miles], mean difference = −8.2 miles.

Next, we analyzed regional variations in changes in miles driven. Miles driven increased only slightly as a function of proximity to NYC, if at all, $r(49) = -.19$, 95% CI: [−.44, .09]. However, miles driven increased substantially with driving opportunity, whether operationalized as highway miles, $r(49) = .45$, 95% CI: [.20, .65], or as car registrations per inhabitant, $r(49) = .42$, 95% CI: [.17, .62]. The two driving-opportunity variables were strongly correlated with each other, $r(49) = .65$, 95% CI: [.46 .78], and car registrations per inhabitant did not explain variance beyond that explained by highway miles per inhabitant, so we focus here on the latter measure. It is encouraging that the two variables yielded basically identical results.

As distance to NYC and highway miles per inhabitant were correlated with each other, $r(49) = .35$, we included both

variables in a multiple regression. The overall model resulted in an R^2 value of .35, $F(2, 48) = 12.66$, $p = .00004$. The increase in miles driven was greater the closer a state was to NYC ($\beta = -0.40$, $p = .002$) and the more highway miles the state had per inhabitant ($\beta = 0.59$, $p = .00002$; Figs. 1b and 1c). In an exploratory regression model, we separated highway miles per inhabitant into rural and urban miles; rural miles predicted increased driving ($\beta = 0.59$, $p = .00003$), but urban miles did not ($\beta = 0.08$, $p = .517$).

Traffic fatalities

Across states, the average monthly increase in traffic fatalities per million inhabitants in the post-September months of 2001 was 1.11; in contrast, there was a decrease of −0.24 fatalities on average in those months in the 5 previous years, paired-sample $t(50) = 4.64$, 95% CI on the difference: [0.76 fatalities, 1.93 fatalities], mean difference = 1.35 fatalities. In 39 of the 51 cases, the change in 2001 was larger than the average change in the previous 5 years (sign test: $Z = 3.64$, $p = .0003$). For the pre-September months, the average monthly changes in fatalities per million inhabitants in 2001 did not differ from the average changes in the 5 previous years or were even lower, paired-sample $t(50) = -1.32$, 95% CI on the difference: [−0.81 fatalities, 0.17 fatalities], mean difference = −0.32 fatalities.

Finally, we analyzed by-state variations in changes in fatalities after 9/11 (Fig. 1a). Fatalities increased with changes in miles driven, $r(49) = .42$, 95% CI: [.16, .62]. They did not, however, increase substantially with proximity to NYC, $r(49) = -.11$, 95% CI: [−.38, .17], or highway miles per inhabitant, $r(49) = .19$, 95% CI: [−.09, .44], and when we controlled for changes in miles driven, these already small correlations approached zero, $r(48) = -.03$ and $r(48) = .01$, respectively.

General Discussion

Both miles driven and traffic fatalities increased substantially in the aftermath of 9/11 compared with the last 3 months in the previous 5 years, and did so in many more states than expected by chance. Moreover, these variables did not increase in the pre-September months in 2001, which lends support to the conclusion that the increases after 9/11 were related to the attacks.

This finding is inconsistent with Su et al.'s (2009) conclusion that miles driven did not increase after 9/11. Their conclusion partly stemmed from a one-way analysis of variance based on nine data points (3 years × 3 months). Although the effect size they obtained was substantial ($\eta_p^2 = .27$; erroneously reported as .37), the analysis did not have sufficient power to detect such an effect given an α of .05 (observed power = .23; Faul, Erdfelder, Lang, & Buchner, 2007). Additionally, Su et al. argued that the post-9/11 increase in miles driven was not pronounced in comparison with historical trends. The literature outside of psychology (e.g., Blalock, Kadiyali, & Simon, 2009; Sivak & Flannagan, 2004), however, backs the finding of increased miles driven reported here and by Gigerenzer (2004, 2006). Taking historical trends into consideration, Blalock et al. even argued that the overall increase in miles driven that they reported might underestimate the 9/11 effect, as their analyses included miles driven in commercial vehicles, and such miles were not expected to increase. Thus, it seems likely that the number of miles driven did increase.

Increases in miles driven, however, were not observed uniformly across the country. To explain these regional variations, we analyzed post-9/11 fear (operationalized as proximity to NYC) together with a relevant aspect of environmental structure (driving opportunity, operationalized as highway miles and car registrations per inhabitant). For a complete picture of increased driving following the terrorist attacks, it was not sufficient to consider only post-9/11 fear. Rather, fear and—additionally and to an even greater extent—driving opportunity explained regional variations in increased driving. Increased driving, in turn, was the best predictor of increased fatalities and where they occurred, a finding consistent with results reported by Blalock et al. (2009) and Sivak and Flannagan (2004).

Because the data reported here are correlational by nature, however, caution is warranted in interpreting the observed relationships causally. Even if the 9/11 attacks were an underlying cause of increased driving, fear of dread risk is not the only possible reason for the increase. Blalock, Kadiyali, and Simon (2007) showed that driving could also have increased because new security measures made flying more inconvenient. Furthermore, the assumed substitution of driving for flying is not directly observable. Consistent with such a substitution, however, is the finding that post-9/11 out-of-state traffic fatalities increased more than home-state traffic fatalities, which suggests that the fatalities occurred on longer-distance trips potentially suited to replace air travel (Blalock et al., 2009; but see Sivak & Flannagan, 2004). It also fits the substitution hypothesis that rural but not urban highway-system miles predicted where miles driven increased, and that driving particularly increased on rural interstate highways (Gigerenzer, 2004).

Finally, there is convergent evidence for the importance of driving opportunity for predicting the substitution: After the terrorist attacks in Madrid on March 11, 2004, Spaniards reacted similarly to Americans, experiencing dread-risk fear and reducing their use of trains. But unlike Americans, they did not increase their driving, so that traffic fatalities did not increase (López-Rousseau, 2005). López-Rousseau speculated that this might have happened because "there is less of a car culture in Spain than in the United States" (p. 427). Data for a measure of "car culture" that predicted variations in increased driving in our study—the number of car registrations per inhabitant—support this speculation. The number of car registrations per 1,000 inhabitants was as high as 808 in the United States in 2001, but only 586 in Spain in 2004

(Eurostat, 2011a, 2011b).[1] We believe that the assumed causal model of how and where the 9/11 attacks indirectly led to additional fatalities is at least plausible in light of the evidence, taken together.

Nevertheless, it has been reported that the increase in traffic fatalities was larger than would be expected if it were based purely on increased driving (Blalock et al., 2009). Su et al. (2009) presented evidence that a decrease in driving quality as a consequence of stress could account for this gap. Similar effects of attack-induced stress on traffic fatalities have been demonstrated in Israel (Stecklov & Goldstein, 2004). Although our results do not provide evidence for such a mechanism, such as a direct link between proximity to NYC and increased traffic fatalities, they also do not preclude it. Detailed analyses of driving quality are beyond the scope of this report, but we checked one straightforward yet powerful indicator of driving quality: If driving quality decreased after 9/11, there should have been more fatalities per mile driven. Indeed, traffic fatalities per mile driven increased, on average, by 4.2% in the post-September months in 2001, but decreased by −1.3% and −5.4% in the same months in 1999 and 2000, respectively. Moreover, the data are consistent with Su et al.'s assertion of particularly increased stress in the Northeast, as the post-9/11 increase in traffic fatalities per mile driven was 10.7% in that region.

Ten years after 9/11, the news focused again on the physical impact of terrorism, paying little heed to terrorists' second strike, the psychological Act II of 9/11. A closer look at the indirect damage in the form of increased traffic fatalities, however, shows that it is important to fight the effects of terrorism in people's minds. Fear caused by terrorism can initiate potentially dangerous behaviors, such as driving more miles or driving less carefully. Understanding fear, however, is not enough. Rather, in order to foresee where secondary effects of terrorism will strike fatally, one also needs to consider environmental structures that allow fear to become manifest in dangerous behaviors, such as driving instead of flying.

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Declaration of Conflicting Interests

The authors declared that they had no conflicts of interest with respect to their authorship or the publication of this article.

Note

1. We did not compare highway-system miles between the United States and Spain because the NHS (United States) and the statistical information service of the European Commission, Eurostat (Spain), define highway ("motorway," in the case of Eurostat) differently.

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