Driving Fatalities After 9/11: A Hidden Cost of Terrorism[*]

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Abstract

We show that the public's response to terrorist threats can have unintended consequences that rival the attacks themselves in severity. Driving fatalities increased significantly after the September 11, 2001 terrorist attacks, events that prompted many travelers to substitute road transportation for safer air transportation. After controlling for time trends, weather, road conditions, and other factors, we find that travelers' response to 9/11 resulted in 344 driving deaths per month in late 2001. Moreover, while the effect of 9/11 weakened over time, a total of about 2,170 driving deaths may be attributable to the attacks.

Keywords: security, terrorism, road and air transportation
JEL codes: H56, L92, L93

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1. Introduction

Recent bombings of rail transportation in London and Madrid, ongoing attacks on public buses in Israel, and similar threats in the New York City subway and elsewhere all influence individuals' decisions about if, when, and how to travel. Policy makers must understand the nature, magnitude, and consequences of the public's response to these incidents in order to implement security measures that minimize economic and welfare losses. As a first step in developing this understanding, we examine the impact of the September 11, 2001 terrorist attacks on driving fatalities.

Recent press reports, confirmed by comparisons of fatality counts, indicate that driving deaths increased after September 11, 2001 (e.g., *Wall Street Journal*, March 23, 2004; Gigerenzer, 2004; Sivak and Flannagan 2005). These reports attribute the increase in fatalities to the terrorist attacks, arguing that a fear of flying prompted many travelers to substitute driving for flying: "The crashes of four airplanes and the massive loss of life in the September 11, 2001, terrorist attacks traumatized people throughout the nation. Tragically, in the last three months of that year, fear of flying revved up car use and caused a second toll of lives on U.S. roads." (*Science News*, 2004). In addition, the inconvenience of tighter airport security after 9/11 may have further encouraged substitution to road travel. Because road transportation is less safe than air transportation, even with the very small possibility of a hijacking this substitution increases the likelihood of death or serious injury during travel.[1] The resulting fatalities are a hidden cost of terrorism that is more prolonged than the casualties of the attacks themselves.

However, the increase in driving fatalities after 9/11, by itself, does not demonstrate a causal relationship. A variety of other factors could explain part or all of a shift from air to road travel as well as the rise in driving fatalities. For example, changes in relative prices could affect substitution and weather, traffic conditions, or highway maintenance could affect fatality rates. Without dismissing these alternative explanations, we cannot know what harm, if any, was caused by the public's reaction to 9/11 and subsequent changes to airport security. Our goal is to identify the existence and extent of such harm so that policy makers can minimize it when developing responses to the terrorist threats in London, Madrid, and elsewhere.

We examine U.S. driving fatalities from 1994 to 2003 to assess the impact of 9/11. Because 9/11 affected travelers' decision to fly or drive, we focus on the deaths of non-commercial drivers. Our identification strategy uses commercial driving fatalities as a control group to exclude the possibility that other factors, such as weather or road conditions, might have increased non-commercial driving fatalities. The identifying assumption, which we test and confirm below, is that commercial driving fatalities may respond to economic, road, and weather conditions but are uninfluenced by the fear and security inconvenience stemming from 9/11. We also examine whether the 9/11 effect has diminished over time as tighter security measures decreased the fear of flying and as travelers became accustomed to new airport security procedures. Finally, consistent with our hypothesis, we confirm that road traffic increased after 9/11.

[1] For example, in a recent discussion of whether infants on airplanes should be placed in child seats (*New York Times*, August 4, 2004; also see Windle and Dresner 1991), the Federal Aviation Administration was concerned about whether overall risk to infants would increase if families substituted unsafe driving rather than paying full-fare for children under the age of two. Hahn (1996) uses the same logic to argue against draconian security measures in airports that make travelers switch from air travel to surface transportation.

Our findings reveal an additional 344 driving fatalities per month for the period from October to December 2001. Further analysis suggests that the 9/11 effect weakened over time and that a cumulative 2,170 lives may have been lost because of the reaction to the attacks.

This paper joins a small but growing empirical literature measuring the impact of terrorism on welfare and the economy. For example, Krueger and Maleckova 2002 attempts to understand the genesis of terrorism through an examination of welfare and economic conditions such as education and poverty. Others like Abadie and Gardeazabel 2002 and Eckstein and Tsiddon 2003 measure the impact of terrorist conflict on macroeconomic outcomes. Becker and Rubinstein 2004 assesses the effect of terrorism on consumer choices by examining Israeli commuters' decisions to ride public buses after suicide bombings, as well as the drop in U.S. air traffic after 9/11. Our work adds to Becker and Rubinstein by measuring the cost in lives of consumer choices that are influenced by fears of terrorism or by the inconvenience of counter-terrorism safety measures. Our findings support the argument in Mueller 2004 that the greatest cost of terrorism may be the public's response to the attacks rather than the attacks themselves.

The rest of the paper is organized as follows. In section 2, we discuss why we should expect 9/11 to increase driving fatalities. In section 3, we provide details of the data. Section 4 discusses our empirical approach and results, and section 5 concludes.

2. Why Substitute Driving for Flying?

The 9/11 attacks might cause travelers to substitute driving for flying for two main reasons. First, the attacks likely increased travelers' fear of flying. This fear may cause travelers to substitute driving for flying if they place too much weight on the possibility of another terrorist attack, as suggested by prospect theory (Tversky and Kahneman, 1979). Consistent with this overweighting of low probability events, travelers' behavior may also reflect what Sunstein (2003) calls "probability neglect," a condition in which large variations in the probability of an event occurring have no bearing on people's choices. His results show that "when strong emotions are involved, people tend to focus on the badness of outcome, rather than on the probability that the outcome will occur." Similarly, Slovic (1987, 2000) documents the distortion of probability assessment when the risks are unfamiliar and beyond control, termed "dread risk."[2] Taking a different approach, Rubinstein and Becker (2004) persuasively argue and empirically support the idea that consumers' utility from travel (and other activities) may be influenced by the fear of terrorist attacks, regardless of whether an attack actually occurs.

Second, the changes in airport security following 9/11 reduced the convenience of air travel, which likely caused travelers to substitute driving for flying. Air travelers experienced many changes in airport security procedures in the months after the terrorist attacks. For example, airlines instructed passengers to arrive at airports as much as two hours before departure. After passing through security checkpoints, where many flyers were asked to remove their shoes, passengers were randomly selected for additional screening, including hand searching of their carry-on bags, in the boarding area. Beginning in late 2002, the Transportation Safety Administration (TSA) introduced baggage screening in many airports, with all airports screening

[2] An interesting contrast is provided by Fischoff *et. al.* (2003) who report results of a November 2001 survey showing systematic underassessment of the risk of being involved in a terrorist attack. They attribute this underassessment to "motivational biases (e.g., the desire to feel more secure) or cognitive ones (e.g. not realizing how much easier it is to see one's own precautionary measures, compared to others)." Kunreuther and Michel-Kerjan (2004) explain the corporate demand for terrorism risk insurance in part due to underassessment of risk.

all checked baggage by the end of 2002. Survey data indicate that added security made flying less convenient; 63 percent of travelers said that airport security "is becoming more of a hassle" (USA Today, 2003). Reflecting this inconvenience, the introduction of baggage screening reduced air passenger volume by five to eight percent (Blalock, Kadiyali, and Simon, 2005).

The above arguments suggest that the effect of 9/11 should weaken over time. If the effect is based on fear of flying, the absence of additional terrorist attacks on U.S. flights might reduce consumers' risk perception. If the 9/11 effect is based on the inconvenience of the new security measures, it is possible that either consumers will grow accustomed to this hassle or that the procedures will become more efficient and convenient. Several surveys conducted since 9/11 find that passengers are willing to accept some inconvenience and or higher prices in exchange for better security (Travelocity, 2002; University of Nebraska at Omaha, 2003). Moreover, these surveys indicate that the security measures implemented since 9/11 have increased passengers' confidence in the safety of air travel, which may result in greater demand.

3. Data

We obtained data on driving fatalities from the Fatality Analysis Reporting System (FARS), which details all fatal motor vehicle crashes in the United States. From FARS we construct monthly road fatalities counts from 1994 to 2003. We exclude September 2001from the sample because of its unusual circumstances.

We first extract only the fatalities from non-commercial vehicles. Our goal is to include deaths of individuals traveling in what might be substitutes for air travel: cars, minivans, intercity buses, motorhomes, etc. We exclude deaths in dual-usage vehicles such as light-weight pickup trucks that could be commercial or non-commercial in use, and thus likely undercount non-commercial fatalities.[3] As we explain below, we also need a count of commercial vehicle fatalities. For this measure, we include fatalities occurring in medium- and heavy-weight trucks and again exclude fatalities in dual-usage vehicles.

We use additional data to control for other economic factors that might influence travel decisions and driving fatalities: the unemployment rate, GDP, gasoline prices, plane ticket prices, total air traffic, and rail freight prices. Finally, we use data on vehicle miles traveled (VMT) obtained from the Bureau of Transportation Statistics. Table 1 has summary statistics, and figure 1 plots airline passenger volume, non-commercial driving fatalities, and commercial driving fatalities over time.

---Insert table 1, figure 1 here---

Figure 1 clearly shows a steep drop in air traffic immediately following 9/11. Non-commercial road fatalities fall slightly after 9/11. However, this decline reflects a seasonal trend as fatalities always fall at this time of the year. We also see an overall upward trend in airline passenger volume and a downward trend in non-commercial driving deaths. These plots do not indicate that Americans substituted road travel for air travel after 9/11 and highlight the need for more careful analysis to separate the 9/11 effect from other predictors of driving fatalities.

[3] We were conservative in determining which vehicle categories to include. We excluded some vehicles that FARS categorizes as passenger vehicles (large utilities, compact pick-up trucks (Gross Vehicle Weight<4,500 lbs), standard pick-up trucks (4,500<=Gross Vehicle Weight<10,000), etc. When we include these additional passenger-vehicle categories, the qualitative nature of our results remain. However, as expected, the overall effect of 9/11 increases. Therefore, our estimate should be seen as a conservative one.

4. Empirical Approach and Results

4.1: The existence of a 9/11 effect

A variety of observed and unobserved factors might affect driving fatalities. Our goal is to examine whether there is a 9/11 effect and to econometrically isolate it from these other factors. Our empirical approach initially includes controls for many observable factors and year and month fixed effects to remove annual and seasonal trends. Below we use commercial fatalities to control for unobservable factors common to both commercial and non-commercial fatalities. Our baseline estimating equation is:

$$\begin{aligned} NonCommericalFatalities_{ym} = {} & \beta_0 Post\_9/11 + \beta_1 Gasolineprice_{ym} + \beta_2 Airfare_{ym} \\ & + \beta_3 Railfreightprice_{ym} + \beta_4 Unemployment_{ym} + \beta_5 GDP_{yq} \\ & + Year_y + Month_m + Intercept \end{aligned} \quad (1)$$

The subscript *ym* indicates a year-month pair, e.g., July 2002 and the subscript *yq* indicates a quarter-year pair. The key variable of interest is *Post_9/11*, which indicates the months from October 2001 to December 2003 to account for any 9/11 effect. The price of regular unleaded gasoline controls for the cost of driving and the price of airline tickets controls for the cost of substitute air travel. Rail freight prices represent the cost of a substitute form of goods transport, a control that is needed when we later examine commercial fatalities. GDP and unemployment rate control for macroeconomic conditions. We include year fixed effects, *Year*, and month fixed effects, *Month*, to account for trends and seasonality in driving fatalities. The estimation results are in the first column of table 2.

---Insert table 2 here---

Higher airfares lead to more driving fatalities (likely through more road travel) and higher gasoline prices have the reverse effect. Deaths appear to decline when the unemployment rate is high, although there is no significant relationship between deaths and GDP. All of the year indicators are significant and illustrate a downward time trend in fatalities, likely the result of improving automobile safety standards and a decline in drunk driving. The monthly indicators are also highly significant, showing increases in fatalities during the peak summer travel months and the winter holiday period. The most salient result is the positive and statistically significant coefficient on the post-9/11 indicator, which shows an additional 344 non-commercial driving fatalities per month after September 2001. We thus find evidence for a 9/11 effect over and above price differences and economy-wide influences.

Although our results suggest a 9/11 effect, we cannot yet dismiss unobservable factors, such as road conditions and weather, as the true cause of the fatalities. We thus introduce commercial fatalities as a control group to rule out these alternative explanations. Our identifying assumption is that commercial fatalities are little affected by fear or inconvenience stemming from the 9/11 attacks. To verify this assumption, we estimate equation 1 substituting commercial deaths for non-commercial deaths as the left-hand-side variable. Column 2 of table 2 shows the results, which confirm that commercial fatalities do not increase after 9/11 and are generally less sensitive to all of the factors. Commercial fatalities decline annually, although none of the year fixed effects is statistically significant, and they are also less seasonal than are non-commercial fatalities. Overall, our findings suggest that demand for commercial transportation is less volatile and less discretionary than demand for non-commercial transportation. Columns 3 and 4 repeat the estimation of columns 1 and 2, but taking the log of fatalities as the dependent variable. The

logarithmic transformation expresses the coefficients as approximate percentages and thus allows for a normalized comparison between non-commercial and commercial deaths. The results show an approximately 13.5 percent rise in non-commercial fatalities and a statistically insignificant five percent decline in commercial fatalities.

We next consider several alternative specifications as robustness tests. Column 1 of table 3 estimates equation 1, but adds commercial fatalities as a control variable.

---Insert table 3 here---

Our goal here is to condition non-commercial deaths on unobserved road and weather conditions that are captured by commercial deaths. Not surprisingly, non-commercial and commercial fatalities are positively correlated. The main effect of 9/11 on non-commercial fatalities remains almost unchanged. Column 2 adopts an alternative approach in which the ratio of non-commercial to commercial fatalities is the left-hand-side variable. The positive effect of 9/11 provides additional evidence that non-commercial deaths increased more than did commercial deaths.

Finally, we estimate a pooled model with two observations, commercial and non-commercial deaths, for each month-year and a dummy variable indicating the non-commercial observations. We then estimate the effect of 9/11 on the pooled sample with an interaction term between the 9/11 dummy and the non-commercial dummy. Because the time trends for commercial and non-commercial fatalities may differ, we also interact the year fixed effects by the non-commercial dummy. Column 3 of table 3 shows the results. In an average month, there are 2,116 more non-commercial than commercial deaths. The effect of 9/11 on commercial fatalities is insignificant, while the marginal effect on non-commercial fatalities, shown in the interaction term, is a highly significant 320.5 additional deaths. As expected from our earlier results, non-commercial deaths increased by a significantly greater amount after 9/11.

4.2: The Longer-Term Impact: Did the 9/11 Effect Weaken?

Because our specification includes a dummy variable indicating months after 9/11 as well as year (and month) fixed effects, it is difficult to separate an overall time trend from the 9/11 effect. In particular, the coefficient on the post-9/11 dummy is identified only by the last three months of 2001. This creates two problems in interpreting the results.

First, we underestimate the 9/11 effect because the 2001 fixed effect, which differences out the mean per-month fatalities for the entire year, includes the three months after the attacks. Therefore, we underestimate the 9/11 effect because part of the negative effect is reflected in the 2001 mean. Likewise, we overestimate the 2001 fixed effect because it captures some of the 9/11 effect.

Second, because the post-9/11 indicator is perfectly collinear with the 2002 and 2003 indicators during those years, we cannot disentangle the long-term impact of 9/11 from the annual variation in fatalities captured by the 2002 and 2003 dummy variables.[4] Therefore, our results identify the 9/11 effect in just the last three months of 2001 to be about 1,033 non-commercial driving fatalities, three times the per-month estimate of 344.298. However, it seems unlikely that

[4] A simple demonstration clarifies the point. If we redefine of the 9/11 dummy to take a value of one only for the last three months of 2001 and then re-estimate equation 1, we obtain identical results, except that the 2002 and 2003 fixed effects each shift up by exactly 344.298, the coefficient on the 9/11 dummy.

travelers immediately reverted back to pre-9/11 behavior in January 2002. If travelers continued to substitute driving for flying in 2002 and beyond, then we would substantially underestimate the true cumulative effect of the 9/11 attacks.

Although we are unable to separate the 2001 effect from the post-9/11 effect, we can disentangle the longer-term impact of 9/11 from the annual variation in fatalities captured by the 2002 and 2003 fixed effects. In doing so, we can better answer two questions: (1) did the 9/11 effect weaken over time and (2) what was the cumulative effect of 9/11 on driving deaths?

One might expect the 9/11 effect to weaken as travelers grew more confident in new security measures, the hassle of those security measures lessened, and as the fear of new attacks diminished. We use two approaches to test for weakening over time. First, we introduce a linear post-9/11 trend. Second, we allow a more flexible inverse function that allows the effect to decay over time. Table 4 displays the results.

---Insert table 4 here---

The post-9/11 trend variable takes the value of 1 in October 2001, 2 in November 2001, and so on. Column 1 estimates the effect of the post-9/11 trend on non-commercial deaths, which reveals a decline of about 11 deaths per month and approaches statistical significance. Column 2 introduces an inverse function, which takes the value 1/2 for October 2001, 1/3 for November 2001, and so on, which allows the effect to decline over time. Although the effect is positive (meaning the effect declines), it is not significant. Column 3 adds an inverse squared variable, taking values 1/4 in October 2001, 1/9 in November 2001, etc. The advantage of including the inverse squared term is that it offers a flexible functional form in which the decay in the first few months after 9/11 could differ from later months. Both inverse terms are highly significant suggesting that the more flexible functional form fits the data well.

The results in Column 3 provide clear evidence that the 9/11 effect weakened over time and allows us to assess the cumulative impact of 9/11 on driving deaths. Adding the predictions of all 9/11 variables (post 9/11 dummy, inverse and inverse squared) together indicates that 239 deaths occurred in October 2001 and 313 in November 2001, after which the effect gradually declined to zero in June 2003. Summing the predicted deaths from October 2001 to May 2003 yields an estimate of nearly 2,170 total fatalities.

4.3: Mechanisms for the 9/11 Effect

We have suggested that the increase in post-9/11 driving fatalities resulted from travelers substituting driving for flying. To test the substitution hypothesis, we first compare the increase in non-commercial fatalities occurring in the driver's home state with the increase occurring in other states. We assume the state that issued the driver's license to be his or her home state. If travelers substituted interstate driving for flying after 9/11, then we would expect fatalities to increase more outside of the driver's home state. Table 5 displays the results.

---Insert table 5 here---

Column 1 shows that fatalities occurring in the driver's home state increased by about 15 percent after 9/11, whereas column 2 reveals the out-of-state increase to be 22 percent. Although a pooled model with both fatality types and an interaction term does not demonstrate the difference to be statistically significant, the higher out-of-state point estimate is consistent with a substitution from air to road travel for long interstate trips.

To further examine the substitution hypothesis, we examine the impact of 9/11 on air traffic, miles driven, and fatalities per mile driven. Table 6 displays the results

---Insert table 6 here---

Column 1 confirms the widely documented decline in air travel by showing that passenger volume fell approximately 22 percent after 9/11. Column 2 indicates that vehicle miles traveled (VMT) increased by about 3.8 percent following 9/11. This increase supports the substitution hypothesis, but it is less than the 13.5 percent increase in fatalities reported in Table 2.[5] The difference between the fatality increase and the VMT increase suggests that deaths per VMT rose after 9/11. The results of column 3 confirm this rise and demonstrate that factors beyond VMT likely contributed to the increase in fatalities following 9/11.

Several factors could explain why fatalities per VMT have risen. In particular, the composition of drivers may have changed after 9/11 or drivers may have adopted more aggressive driving habits. We briefly consider each of these explanations. Table 7 examines driver demographics. Columns 1 and 2 show no change in either the average age or the gender composition of drivers in fatal accidents. Columns 3 and 4 reveal no change in the background of drivers as reflected by past accidents or drunk driving convictions.

---Insert table 7 here---

Table 8 examines whether risky driving behavior increased following 9/11. Column 1 shows that alcohol-related fatalities increased by about two percentage points, or by about ten percent (from a mean of 22 percent of all fatalities). Similarly, column 2 shows that the proportion of fatal accidents in which the vehicle's speed exceeded the posted speed limit increased by 3.2 percentage points, or by about 4.7 percent (from a mean of 68 percent). These results indicate that risky driving increased after 9/11. Moreover, the absence of changes in driver characteristics suggests that the increase is not attributable to a self-selection of risky drivers substituting driving for flying post-9/11

---Insert table 8 here---

Taken together, our results indicate that an increase in driving, as well as an increase in the fatality rate per VMT, contributed to the rise in fatalities after 9/11. The 9/11 effect appears to explain the increase in VMT, whereas an increase in risky driving behavior contributed to the higher fatality rates per VMT. However, we do not suggest that 9/11 caused the change in driver behavior and it is beyond the scope of this paper to assess all of the factors that increased fatalities per VMT. Nonetheless, making the most conservative assumption, that the 9/11 effect consists only of the 3.8 percent increase in VMT, then our results indicate that about 97 additional people per month died in automobile accidents in the months following 9/11.[6]

To summarize, we observe four key outcomes after 9/11: (1) non-commercial driving fatalities increased substantially; (2) commercial driving fatalities did not increase; (3) the increase in non-commercial fatalities weakened over time, dissipating by May 2003; and (4) VMT increased by nearly four percent. Taken together, these results are strong evidence that

[5] We believe that the increase in fatalities is a more reliable statistic because the FARS data are a full enumeration of accidents. In contrast, vehicle miles traveled estimates are based on a sample of traffic-counting locations nationwide (USDOT, 2005).

[6] We calculate the fraction of fatalities attributable to the increase in VMT, and then multiply this by the number of additional deaths per month post 9/11: (0.038/0.135)*344.298=96.913.

substitution of road travel for air travel was a prime mechanism for the rise in non-commercial driving fatalities.

5. Conclusion

We have measured how many lives were lost as travelers responded to the events of 9/11 by switching from air transportation to road transportation. After controlling for observable and unobservable factors that influence fatalities, we estimate that about 2,170 lives were lost because of travelers' response to 9/11. These deaths are a hidden cost of the 9/11 attacks.

Although we are unable to identify precisely reasons for the 9/11 effect, our evidence is consistent with arguments that fear of flying and/or the increased inconvenience of stricter airport security led many travelers to driver rather than fly. Our results show that the public's response to terrorist threats can have unintended consequences that rival the attacks themselves in their severity. Policy makers must consider this response when determining appropriate anti-terrorism policies.

Our findings emphasize the importance of ensuring confidence in the security of air and rail networks while minimizing inconvenience that shifts travelers to more dangerous modes of transportation, such as automobiles and motorcycles. Evidence that post-9/11 airport security measures have reduced demand for air travel (Blalock, Kadiyali, and Simon, 2005) highlights the challenge policy makers face in achieving this balance.

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Table 1: Summary Statistics

Variable	Mean (s.d.)	
Non-Commercial fatalities per month	2583.81	(242.34)
Commercial fatalities per month	421.79	(48.65)
Gasoline price ($ per gallon, regular unleaded)	1.29	(0.19)
Air fare (Dec. 1989 = 100)	182.81	(32.65)
Rail freight price (Dec. 1989 = 100)	114.50	(3.29)
Gross domestic product (billions current $)	9004.24	1295.71
Unemployment rate (%)	5.12	(0.76)
Air passengers per month (1000's of enplanements)	46943.10	(5450.33)
Vehicle miles traveled (billions)	220.00	(20.67)

Table 2: Effect of 9/11 on Driving Deaths

	(1)	(2)	(3)	(4)
	Non-Commercial Deaths	Commercial Deaths	Ln(Non-Commercial Deaths)	Ln(Commercial Deaths)
Post_9/11	344.298	-18.655	0.135	-0.050
	(68.820)**	(30.240)	(0.028)**	(0.072)
Gas	-171.651	31.792	-0.072	0.078
	(106.430)	(42.385)	(0.042)+	(0.102)
Air fare	6.644	-0.300	0.003	-0.001
	(2.472)**	(0.737)	(0.001)**	(0.002)
Rail freight price	-41.802	4.998	-0.015	0.016
	(28.338)	(10.996)	(0.011)	(0.028)
GDP (millions $)	355.642	72.227	0.141	0.164
	(195.460)+	(76.907)	(0.076)+	(0.187)
Unemployment	-137.279	-2.237	-0.058	-0.006
	(52.981)*	(15.719)	(0.022)*	(0.038)
1995	-163.933	-37.313	-0.067	-0.089
	(79.996)*	(28.057)	(0.031)*	(0.068)
1996	-408.987	-46.980	-0.166	-0.109
	(160.805)*	(62.721)	(0.063)*	(0.152)
1997	-678.398	-64.925	-0.276	-0.154
	(255.622)**	(96.858)	(0.100)**	(0.234)
1998	-920.470	-97.532	-0.376	-0.236
	(344.536)**	(123.692)	(0.134)**	(0.300)
1999	-1,206.455	-136.588	-0.492	-0.329
	(446.007)**	(167.845)	(0.174)**	(0.407)
2000	-1,527.858	-192.988	-0.624	-0.464
	(560.832)**	(208.134)	(0.219)**	(0.506)
2001	-1,655.310	-223.411	-0.679	-0.546
	(632.421)*	(233.633)	(0.248)**	(0.568)
2002	-1,733.682	-257.802	-0.708	-0.631
	(698.897)*	(260.633)	(0.273)*	(0.634)
2003	-1,864.296	-310.996	-0.766	-0.767
	(781.139)*	(295.441)	(0.306)*	(0.724)
February	-204.131	-41.725	-0.092	-0.111
	(32.919)**	(12.852)**	(0.015)**	(0.033)**
March	99.743	-2.330	0.043	-0.004
	(35.112)**	(12.582)	(0.015)**	(0.032)
April	99.035	-19.153	0.043	-0.047
	(46.892)*	(16.994)	(0.020)*	(0.043)
May	278.995	1.846	0.115	0.005
	(48.743)**	(14.516)	(0.020)**	(0.037)
June	316.179	46.641	0.129	0.112
	(42.408)**	(13.180)**	(0.018)**	(0.031)**

July	426.684	40.518	0.168	0.097
	(58.051)**	(18.932)*	(0.024)**	(0.045)*
August	468.797	66.468	0.182	0.152
	(61.770)**	(18.843)**	(0.025)**	(0.044)**
September	252.130	39.890	0.103	0.097
	(56.876)**	(17.642)*	(0.023)**	(0.042)*
October	358.349	60.190	0.139	0.139
	(75.791)**	(25.594)*	(0.030)**	(0.061)*
November	261.928	6.275	0.104	0.019
	(75.099)**	(24.958)	(0.030)**	(0.060)
December	322.335	15.995	0.126	0.041
	(74.394)**	(24.498)	(0.030)**	(0.059)
Constant	4,591.571	-652.028	8.435	3.011
	(3,023.803)	(1,062.598)	(1.179)**	(2.753)
Observations	119	119	119	119
R-squared	0.92	0.74	0.92	0.74

Robust standard errors in parentheses
+ significant at 10%; * significant at 5%; ** significant at 1%

We exclude September 2001. Month indicators are relative to the omitted month of January. Year indicators are relative to the omitted year of 1994.

Table 3: Alternative Models of the Effect of 9/11 on Driving Deaths

	(1)	(2)	(3)
	Non-Commercial Deaths	Ratio Commercial/Non-Commercial Deaths	Deaths
Post_9/11	359.591	1.135	2.592
	(69.580)**	(0.461)*	(53.768)
Non-Commercial Deaths			2,116.333
			(78.422)**
Post_9/11*Non-Commercial Deaths			320.458
			(83.516)**
Commercial Deaths	0.820		
	(0.273)**		
Gas	-197.713	-0.917	-69.929
	(109.122)+	(0.635)	(58.641)
Air fare	6.890	0.019	3.172
	(2.242)**	(0.010)+	(1.406)*
Rail freight price	-45.899	-0.203	-18.402
	(26.498)+	(0.160)	(16.337)
GDP (millions $)	296.433	-0.087	213.935
	(192.230)	(1.165)	(106.963)*
Unemployment	-135.445	-0.282	-69.758
	(51.364)**	(0.245)	(28.413)*
1995	-133.345	0.145	-145.956
	(79.855)+	(0.442)	(62.538)*
1996	-370.474	-0.330	-278.358
	(159.769)*	(0.954)	(101.053)**
1997	-625.175	-0.731	-409.245
	(257.976)*	(1.503)	(142.968)**
1998	-840.517	-0.829	-511.668
	(353.978)*	(2.008)	(182.433)**
1999	-1,094.485	-0.977	-673.063
	(453.034)*	(2.645)	(233.768)**
2000	-1,369.654	-0.914	-872.173
	(568.623)*	(3.295)	(302.131)**
2001	-1,472.166	-0.725	-912.632
	(643.184)*	(3.724)	(333.519)**
2002	-1,522.345	-0.424	-885.930
	(714.208)*	(4.175)	(367.294)*
2003	-1,609.353	0.060	-938.750

	(796.063)*	(4.728)	(413.922)*
1995*Non-Commercial Deaths			90.667
			(106.706)
1996*Non-Commercial Deaths			100.750
			(103.813)
1997*Non-Commercial Deaths			75.167
			(101.787)
1998*Non-Commercial Deaths			5.333
			(99.035)
1999*Non-Commercial Deaths			3.083
			(100.761)
2000*Non-Commercial Deaths			23.500
			(99.004)
2001*Non-Commercial Deaths			-53.458
			(108.186)
2002*Non-Commercial Deaths			-219.625
			(121.497)+
2003*Non-Commercial Deaths			-297.792
			(130.975)*
February	-169.926	0.095	-122.928
	(33.116)**	(0.204)	(17.964)**
March	101.654	0.266	48.707
	(33.651)**	(0.188)	(19.554)*
April	114.736	0.555	39.941
	(45.685)*	(0.265)*	(25.750)
May	277.481	0.679	140.420
	(48.171)**	(0.232)**	(25.735)**
June	277.945	0.091	181.410
	(47.392)**	(0.211)	(21.663)**
July	393.469	0.423	233.601
	(64.833)**	(0.311)	(29.136)**
August	414.308	0.176	267.633
	(68.217)**	(0.283)	(32.484)**
September	219.430	0.031	146.010
	(61.718)**	(0.289)	(28.559)**
October	309.007	-0.017	209.270
	(83.905)**	(0.415)	(38.577)**
November	256.784	0.509	134.101
	(79.407)**	(0.414)	(37.615)**
December	309.223	0.522	169.165
	(76.440)**	(0.394)	(38.597)**
Constant	5,126.080	29.354	911.605
	(3,033.151)+	(17.584)+	(1,602.877)
Observations	119	119	238
R-squared	0.92	0.49	0.99

Robust standard errors in parentheses
+ significant at 10%; * significant at 5%; ** significant at 1%

We exclude September 2001. Month indicators are relative to the omitted month of January. Year indicators are relative to the omitted year of 1994.

Table 4: Duration of 9/11 Effect

	(1)	(2)	(3)
	Non-Commercial Deaths	Non-Commercial Deaths	Non-Commercial Deaths
Post_9/11	290.227	205.427	-101.431
	(82.866)**	(188.457)	(216.782)
Post_9/11 trend	-11.172		
	(7.659)		
Inverse		320.369	2,366.240
		(403.922)	(907.059)*

Inverse squared			-3,369.845
			(1,246.762)**
Gas	-143.859	-157.559	-55.100
	(104.879)	(104.851)	(114.061)
Air fare	5.612	5.990	4.852
	(2.676)*	(2.579)*	(2.726)+
Rail freight price	-25.294	-41.326	-31.106
	(31.081)	(28.468)	(28.773)
GDP (millions $)	432.773	341.870	283.205
	(175.023)*	(195.231)+	(199.916)
Unemployment	-101.562	-122.210	-95.122
	(58.203)+	(53.946)*	(55.088)+
1995	-161.215	-146.794	-106.811
	(76.664)*	(81.647)+	(86.425)
1996	-414.157	-374.343	-293.210
	(151.614)**	(164.121)*	(174.870)+
1997	-706.796	-625.214	-500.576
	(232.764)**	(258.789)*	(273.114)+
1998	-987.075	-854.490	-690.701
	(303.670)**	(346.314)*	(362.980)+
1999	-1,290.528	-1,124.476	-919.144
	(396.813)**	(449.481)*	(471.056)+
2000	-1,644.242	-1,418.048	-1,185.597
	(500.215)**	(568.022)*	(591.893)*
2001	-1,817.762	-1,536.226	-1,291.210
	(560.353)**	(639.231)*	(661.530)+
2002	-1,862.304	-1,529.426	-1,191.414
	(633.631)**	(733.838)*	(769.174)
2003	-1,947.096	-1,636.342	-1,225.588
	(728.409)**	(820.097)*	(863.768)
February	-202.024	-202.683	-200.497
	(33.682)**	(32.540)**	(31.899)**
March	101.487	101.797	102.278
	(34.199)**	(34.790)**	(32.585)**
April	91.008	102.493	106.469
	(43.890)*	(46.545)*	(44.898)*
May	274.353	284.032	289.535
	(49.315)**	(49.165)**	(49.409)**
June	311.665	321.903	328.556
	(41.791)**	(42.633)**	(42.060)**
July	416.531	434.835	451.448
	(55.360)**	(58.477)**	(59.379)**
August	462.245	477.952	496.703
	(59.318)**	(61.315)**	(61.331)**
September	248.937	262.419	281.762
	(55.899)**	(58.062)**	(60.416)**
October	350.719	368.717	409.441
	(69.878)**	(75.664)**	(79.264)**
November	256.692	277.502	307.436
	(69.649)**	(76.513)**	(78.039)**
December	321.906	341.479	376.964
	(73.947)**	(76.477)**	(81.191)**
Constant	2,095.540	4,609.779	3,742.734
	(3,248.512)	(3,030.652)	(2,964.214)
Observations	119	119	119
R-squared	0.92	0.92	0.92

Robust standard errors in parentheses
+ significant at 10%; * significant at 5%; ** significant at 1%

We exclude September 2001. Month indicators are relative to the omitted month of January. Year indicators are relative to the omitted year of 1994.

Table 5: In-State and Out-of-State Driving Deaths

	(1)	(2)
	ln(Same State Deaths)	ln(Different State Deaths)
Post_9/11	0.148	0.219
	(0.034)**	(0.082)**
Gas	-0.059	-0.180
	(0.049)	(0.113)
Air fare (/100)	0.252	0.292
	(0.114)*	(0.245)
Rail freight price	-0.014	-0.013
	(0.013)	(0.021)
GDP (millions $)	0.161	0.169
	(0.090)+	(0.183)
Unemployment	-0.058	-0.087
	(0.027)*	(0.042)*
1995	-0.075	-0.073
	(0.035)*	(0.085)
1996	-0.176	-0.193
	(0.073)*	(0.176)
1997	-0.296	-0.295
	(0.113)**	(0.275)
1998	-0.402	-0.442
	(0.151)**	(0.343)
1999	-0.520	-0.584
	(0.196)**	(0.455)
2000	-0.655	-0.758
	(0.248)**	(0.586)
2001	-0.721	-0.791
	(0.279)*	(0.656)
2002	-0.759	-0.907
	(0.311)*	(0.726)
2003	-0.831	-0.946
	(0.348)*	(0.802)
February	-0.095	-0.045
	(0.017)**	(0.037)
March	0.032	0.177
	(0.018)+	(0.042)**
April	0.042	0.161
	(0.023)+	(0.047)**
May	0.103	0.262
	(0.022)**	(0.040)**
June	0.109	0.371
	(0.022)**	(0.045)**
July	0.124	0.509
	(0.027)**	(0.062)**
August	0.142	0.500
	(0.028)**	(0.058)**
September	0.081	0.242
	(0.027)**	(0.059)**
October	0.122	0.208
	(0.037)**	(0.077)**
November	0.098	0.124
	(0.036)**	(0.078)
December	0.111	0.205
	(0.034)**	(0.071)**
Constant	8.023	5.886
	(1.372)**	(2.210)**
Observations	119	119
R-squared	0.89	0.85

Robust standard errors in parentheses
+ significant at 10%; * significant at 5%; ** significant at 1%

Accidents that occur outside of the state in which the driver is licensed are different state deaths. We exclude September 2001. Month indicators are relative to the omitted month of January. Year indicators are relative to the omitted year of 1994.

Table 6: Effect of 9/11 on Vehicle Miles Traveled

	(1)	(2)	(3)
	ln(Air Passengers)	ln(Vehicle Miles Traveled)	ln(Fatalities Per Mile)
Post_9/11	-0.219	0.038	0.096
	(0.033)**	(0.014)**	(0.029)**
Gas	0.020	-0.014	-0.058
	(0.032)	(0.015)	(0.042)
Air fare (/100)	-0.069	0.022	0.255
	(0.075)	(0.036)	(0.095)**
Rail freight price	-0.005	-0.004	-0.011
	(0.009)	(0.004)	(0.012)
GDP (millions $)	0.003	0.099	0.042
	(0.063)	(0.028)**	(0.079)
Unemployment	0.008	-0.013	-0.045
	(0.017)	(0.008)	(0.021)*
1995	0.036	-0.013	-0.054
	(0.025)	(0.011)	(0.032)+
1996	0.044	-0.035	-0.131
	(0.055)	(0.024)	(0.065)*
1997	0.137	-0.059	-0.217
	(0.085)	(0.038)	(0.103)*
1998	0.166	-0.080	-0.296
	(0.110)	(0.049)	(0.139)*
1999	0.209	-0.113	-0.379
	(0.143)	(0.064)+	(0.180)*
2000	0.274	-0.147	-0.477
	(0.182)	(0.085)+	(0.227)*
2001	0.297	-0.160	-0.519
	(0.202)	(0.097)	(0.256)*
2002	0.422	-0.187	-0.521
	(0.230)+	(0.109)+	(0.283)+
2003	0.476	-0.207	-0.558
	(0.251)+	(0.125)	(0.318)+
February	-0.007	-0.038	-0.054
	(0.012)	(0.006)**	(0.013)**
March	0.195	0.109	-0.066
	(0.010)**	(0.007)**	(0.014)**
April	0.143	0.098	-0.054
	(0.013)**	(0.007)**	(0.018)**
May	0.167	0.156	-0.041
	(0.012)**	(0.007)**	(0.020)*
June	0.215	0.153	-0.025
	(0.012)**	(0.007)**	(0.016)
July	0.255	0.177	-0.009
	(0.017)**	(0.009)**	(0.023)
August	0.254	0.179	0.003
	(0.017)**	(0.009)**	(0.024)
September	0.073	0.093	0.010
	(0.020)**	(0.010)**	(0.022)
October	0.174	0.116	0.023
	(0.024)**	(0.012)**	(0.030)
November	0.126	0.049	0.055
	(0.025)**	(0.012)**	(0.030)+
December	0.152	0.056	0.070
	(0.029)**	(0.012)**	(0.029)*
Constant	17.940	11.867	-3.433

	(0.869)**	(0.479)**	(1.292)**
Observations	119	119	119
R-squared	0.97	0.99	0.90

Robust standard errors in parentheses
+ significant at 10%; * significant at 5%; ** significant at 1%

We exclude September 2001. Month indicators are relative to the omitted month of January. Year indicators are relative to the omitted year of 1994.

Table 7: Driving Death Demographics

	(1)	(2)	(3)	(4)
	Driver Age	Driver Gender (1=female)	Previous Accidents Per Driver	Previous DWI Incidents Per Driver
Post_9/11	-0.897	-0.017	-0.007	0.004
	(0.637)	(0.012)	(0.011)	(0.004)
Gas	-1.233	0.012	-0.024	-0.001
	(1.163)	(0.020)	(0.016)	(0.006)
Air fare (/100)	-0.812	-0.053	0.008	0.007
	(2.059)	(0.030)+	(0.026)	(0.013)
Rail freight price	-0.116	0.002	0.000	0.001
	(0.186)	(0.004)	(0.003)	(0.002)
GDP (millions $)	-0.366	-0.000	0.004	0.017
	(1.345)	(0.027)	(0.029)	(0.011)
Unemployment	0.361	0.008	-0.011	0.002
	(0.377)	(0.007)	(0.007)	(0.003)
1995	0.662	0.008	-0.011	-0.007
	(0.593)	(0.012)	(0.011)	(0.004)+
1996	0.775	0.019	-0.013	-0.016
	(1.308)	(0.024)	(0.024)	(0.009)+
1997	2.182	0.035	-0.031	-0.027
	(2.013)	(0.038)	(0.038)	(0.015)+
1998	2.417	0.038	-0.046	-0.036
	(2.606)	(0.050)	(0.050)	(0.020)+
1999	2.495	0.044	-0.054	-0.043
	(3.367)	(0.066)	(0.065)	(0.026)+
2000	3.530	0.050	-0.058	-0.054
	(4.459)	(0.084)	(0.081)	(0.033)
2001	4.030	0.048	-0.047	-0.064
	(4.980)	(0.095)	(0.092)	(0.037)+
2002	4.712	0.041	-0.034	-0.080
	(5.476)	(0.106)	(0.104)	(0.042)+
2003	5.271	0.040	-0.042	-0.098
	(6.266)	(0.122)	(0.116)	(0.048)*
February	-0.039	0.002	0.001	0.002
	(0.295)	(0.005)	(0.005)	(0.002)
March	-0.708	-0.002	0.003	0.004
	(0.420)+	(0.008)	(0.006)	(0.002)
April	-0.582	0.002	0.005	-0.003
	(0.290)*	(0.007)	(0.007)	(0.003)
May	-0.583	0.008	0.008	-0.002
	(0.352)	(0.007)	(0.006)	(0.003)
June	-1.591	0.009	0.012	-0.003
	(0.324)**	(0.007)	(0.007)	(0.003)
July	-1.775	0.013	0.006	-0.006
	(0.385)**	(0.008)+	(0.009)	(0.004)+
August	-1.370	0.008	0.006	-0.003
	(0.406)**	(0.008)	(0.009)	(0.003)
September	0.126	0.003	0.001	-0.004
	(0.469)	(0.009)	(0.009)	(0.004)
October	0.249	0.002	0.003	-0.009
	(0.621)	(0.011)	(0.012)	(0.005)+

November	0.181	0.001	-0.001	-0.008
	(0.565)	(0.011)	(0.013)	(0.005)+
December	0.347	0.005	-0.007	-0.010
	(0.543)	(0.011)	(0.012)	(0.005)*
Constant	51.451	1.177	0.236	-0.225
	(21.283)*	(0.462)*	(0.374)	(0.190)
Observations	119	119	119	119
R-squared	0.65	0.37	0.64	0.46

We exclude September 2001. Month indicators are relative to the omitted month of January. Year indicators are relative to the omitted year of 1994.

Table 8: Risky Behavior Associated with Driving Deaths

	(1)	(2)
	Alcohol Involved	Driver Speeding
Post_9/11	0.020	0.032
	(0.012)+	(0.012)**
Gas	-0.014	0.002
	(0.014)	(0.014)
Air fare (/100)	0.087	-0.008
	(0.029)**	(0.032)
Rail freight price	0.002	-0.004
	(0.004)	(0.004)
GDP (millions $)	0.037	0.017
	(0.029)	(0.035)
Unemployment	-0.000	-0.008
	(0.007)	(0.007)
1995	-0.031	0.000
	(0.011)**	(0.000)
1996	-0.058	0.037
	(0.025)*	(0.104)
1997	-0.099	0.024
	(0.038)*	(0.089)
1998	-0.131	0.015
	(0.051)*	(0.077)
1999	-0.159	0.006
	(0.066)*	(0.060)
2000	-0.187	0.003
	(0.083)*	(0.044)
2001	-0.215	0.009
	(0.094)*	(0.031)
2002	-0.244	0.000
	(0.107)*	(0.016)
2003	-0.276	0.000
	(0.121)*	(0.000)
February	0.005	0.002
	(0.005)	(0.004)
March	0.018	0.007
	(0.005)**	(0.006)
April	0.013	0.011
	(0.006)*	(0.006)+
May	0.007	0.012
	(0.006)	(0.007)
June	0.000	0.013
	(0.005)	(0.008)+
July	-0.005	0.007
	(0.007)	(0.009)
August	-0.006	0.007
	(0.007)	(0.008)
September	-0.001	0.014
	(0.008)	(0.009)

October	-0.011	0.001
	(0.011)	(0.012)
November	-0.012	0.005
	(0.011)	(0.014)
December	-0.030	-0.007
	(0.010)**	(0.013)
Constant	-0.376	1.016
	(0.492)	(0.464)*
Observations	119	95
R-squared	0.71	0.32

Robust standard errors in parentheses
+ significant at 10%; * significant at 5%; ** significant at 1%

We exclude September 2001. Month indicators are relative to the omitted month of January. Year indicators are relative to the omitted year of 1994.

Figure 1: Time Trends of Driving Fatalities and Air Passenger Volume