

## **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 327 driving deaths per month in late 2001. Moreover, while the effect of 9/11 weakened over time, as many as 2,300 driving deaths may be attributable to the attacks.

**Keywords:** security, terrorism

**JEL code:** H56

## 1. Introduction

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. Specifically, we try to identify the "9/11 effect" on fatalities: the increase in fatalities attributable to travelers substituting driving for flying following the 9/11 attacks.

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 2004). 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, this substitution increases the likelihood of death or serious injury during travel.<sup>1</sup>

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<sup>1</sup> For example, in a 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 airfare 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.

The resulting fatalities are a hidden cost of terrorism that might be greater 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, state regulations 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 a 9/11 effect.

We examine U.S. driving fatalities from 1994 to 2003 to assess the impact of 9/11. Because 9/11 affected travelers' decisions 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 as tighter security measures and the passage of time since the attacks decreased the fear of flying, and as travelers became accustomed to new airport security procedures. In addition, to provide evidence that the substitution of driving for flying underlies the 9/11 effect, we assess whether out-of-state fatalities increased more than in-state fatalities, as well as whether fatalities increased more in the Northeast corridor, where driving provides a closer substitute to flying. Finally, consistent with our hypothesis, we confirm that driving increased after 9/11.

Our findings reveal an additional 327 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, cumulatively, 2,300 lives may have been lost because of the reaction to the attacks. Moreover, we find a greater increase in out-of-state driving deaths than in-state driving deaths, and a much larger increase in driving deaths in the Northeast corridor than elsewhere in the US.

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 2004, measure the impact of terrorist conflict on macroeconomic outcomes. Zussman and Zussman 2006 examines whether counter-terrorism policy is effective in reversing the negative macroeconomic impact of terrorism using Israeli stock market data. 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 US 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. In particular, 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."<sup>2</sup> Taking a different approach, Rubinstein and Becker 2004 persuasively argues and empirically supports 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 many 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 all checked baggage by the end of 2002. Survey data indicate that added security made

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<sup>2</sup> An interesting contrast is provided by Fischhoff *et al* 2003 which reports results of a November 2001 survey showing systematic underassessment of the risk of being involved in a terrorist attack. It attributes 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 that the corporate demand for terrorism risk insurance is in part due to underassessment of risk.

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 six to nine 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 and implementation of tighter security 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 fatality counts from 1994 to 2003. We exclude September 2001 from the sample because of its unusual circumstances.

We first count only the fatalities in non-commercial vehicles. Here, our goal is to include only those fatalities occurring in vehicles that might be substitutes for passenger air travel: cars, minivans, intercity buses, motorhomes, etc. Fatalities occurring in these vehicles should be most influenced by the 9/11 effect, because drivers and passengers of these vehicles are the most likely to substitute driving for flying. As we explain below, we also want to create a control

group of fatalities that should not be influenced by the 9/11 events. To do so, we count commercial vehicle fatalities. For this measure, we include fatalities occurring in medium- and heavy-weight trucks. These vehicles are the most likely to be used for transporting non-human cargo—finished goods, materials, etc.—and therefore are the least likely to substitute for passenger air travel. As a result, fatalities in these vehicles should be least affected by an increase in fear and inconvenience of flying, and therefore least influenced by the 9/11 events. Note that our classification of deaths of individual drivers and passengers in non-commercial vehicles and deaths in commercial vehicles is based on differences in the usage of these types of vehicles, not on the identities of drivers. In both cases, we exclude deaths in dual-usage vehicles such as light-weight pickup trucks that could be used for the transportation of non-human cargo and therefore may not be a substitute for air travel. By excluding these vehicles from both groups, we likely undercount both commercial and non-commercial fatalities.<sup>3</sup>

We use additional data to control for other economic factors that might influence travel decisions and driving fatalities. Gasoline prices control for the cost of driving, while airfares and rail freight prices control for the prices of substitutes for non-commercial and commercial vehicle driving, respectively.<sup>4</sup> The unemployment rate and personal income control for general economic conditions which research has shown to influence motor vehicle fatality rates (Dee and Sela, 2003; Cohen and Dehejia, 2004).<sup>5</sup> In addition, we use data on air passenger volume and freight and mail volume obtained from the Department of Transportation's T-100 Domestic Market Dataset, and vehicle miles traveled (VMT) obtained from the Federal Highway

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<sup>3</sup> 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, standard pick-up trucks, etc.). When we include these additional passenger-vehicle categories, our results remain largely unchanged.

<sup>4</sup> We obtained data on these prices from the Bureau of Labor Statistics.

<sup>5</sup> We obtained unemployment rate data from the Bureau of Labor Statistics and income data from the Bureau of Economic Analysis.

Administration. Table 1 has summary statistics, and figure 1 plots non-commercial driving fatalities (on the left axis) and commercial driving fatalities (on the right axis). Figure 2 shows air passenger volume and figure 3 shows VMT over time.

---Insert table 1, figures 1, 2, and 3 here---

Figure 1 shows that non-commercial road fatalities fell slightly after 9/11. However, this decline reflects a seasonal trend as fatalities always fall at this time of the year. The close movement between non-commercial and commercial fatalities provides some evidence that the latter is a good control group for the former. Figure 2 clearly shows the steep drop in air passenger volume following 9/11 and figure 3 reveals strong seasonal variation in VMT. However, 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.

#### 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—an increase in driving fatalities due to travelers substituting driving for flying—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 such as congestion, weather, and road conditions, which are common to both commercial and non-commercial fatalities. Our baseline estimating equation is:

$$\begin{aligned} NonCommercialFatalities_{ym} = & \beta_0 Post9/11 + \beta_1 Gasolineprice_{ym} + \beta_2 Airfare_{ym} \\ & + \beta_3 Railfreightprice_{ym} + \beta_4 Unemployment_{ym} \\ & + \beta_5 Personalincome_{yq} + Year_y + Month_m + Intercept + \varepsilon_{ym} \end{aligned} \quad (1)$$

The subscript *ym* indicates a year-month pair, e.g., July 2002. The key variable of interest is *Post9/11*, 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. The national unemployment rate and personal income 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 OLS estimation results are reported in the first column of table 2.

---Insert table 2 here---

Deaths appear to increase with air fares, likely reflecting substitution of driving for air travel. Deaths are also higher when the unemployment rate is low and when income levels are high. This is consistent with other studies finding that driving fatalities are pro-cyclical, and may indicate that people drive more during good economic times, and/or that people drive less safely during good economic times (Dee and Sela, 2003; Cohen and Dehejia, 2004). In particular, studies have found that drunk-driving related fatalities increase with good economic conditions, perhaps because people drink more outside of the home when income and employment are high (Saffer, 1997; Gerdtham & Ruhm 2002). All of the year indicators are statistically 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 327 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 congestion, 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 influenced by congestion, road conditions, weather, and other unobservable factors influencing driving safety, but 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 also show substantial seasonality and a general decline over time, although none of the year fixed effects is statistically significant. 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 use the log of fatalities as the dependent variable. Because coefficients are interpreted as semi-elasticities, the semilog equation allows for a normalized comparison between non-commercial and commercial deaths. The results show an approximately 12.8 percent rise in non-commercial fatalities and a statistically insignificant seven percent decline in commercial fatalities.

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

---Insert table 3 here---

Again, our goal here is to use non-commercial deaths to control for unobserved road and weather conditions. Not surprisingly, non-commercial and commercial fatalities are positively correlated. Moreover, the main effect of 9/11 on non-commercial fatalities (0.138) increases in magnitude. Column 2 adopts an alternative approach in which the ratio of non-commercial to commercial fatalities is the left-hand-side variable. The 9/11 coefficient (1.23) indicates that the ratio of non-commercial to commercial fatalities increased by about 1.23 following 9/11 (starting from a pre-9/11 ratio of 6.08). These results provide additional evidence that non-commercial deaths increased more than did commercial deaths.

Next, we estimate a pooled model with two observations, one indicating commercial deaths and one indicating 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. The results in column 3 indicate that the effect of 9/11 on commercial fatalities is insignificant, while the marginal effect on non-commercial fatalities is statistically significant. The interaction term indicates that the effect of 9/11 on non-commercial fatalities is 5.6 percentage points greater than for commercial fatalities. However, because the time trends for commercial and non-commercial fatalities, as well as the effects of the other variables, may differ for commercial and non-commercial fatalities, we also estimate a fully-interacted model, which replicates the split sample results reported in table 1. Column 4 of table 3 shows the results. The coefficient on the non-commercial indicator (4.634) shows that there are more than 5.6 times more non-commercial than commercial deaths in an average month. The effect of 9/11 on commercial fatalities is insignificant, while the marginal effect on non-commercial fatalities, shown in the interaction

term (with a coefficient of 0.201), indicates that the effect of 9/11 on non-commercial fatalities is about 20 percentage points greater than for commercial fatalities.<sup>6</sup>

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

Because our specification includes a dummy variable indicating all 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 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.<sup>7</sup> Therefore, our results identify the 9/11 effect in just the last three months of 2001 to be about 982 non-commercial driving fatalities, three times the per-month estimate of 327.469. However, it seems unlikely that 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 cumulative effect of the 9/11 attacks.

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<sup>6</sup> One might be concerned that changes in state regulations or other regional conditions may have influenced fatalities. For example, stricter drunk driving laws may have reduced fatalities, while higher speed limits may have increased fatalities. However, because the above results use national data, the net effect on US fatalities of any unobserved state-level changes is likely to be small and should be captured by the year fixed effects. Nonetheless, to verify that state-level changes are not biasing our results, we disaggregate our data to create a non-commercial fatality count for each state in each month. We then estimate the effect of 9/11 on these fatality counts and include state-year fixed effects to control for unobserved changes in state conditions. The effect of 9/11 increases and suggests that any bias from unobserved state-specific factors is negative.

<sup>7</sup> A simple demonstration clarifies the point. If we redefine 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 327.469, the coefficient on the 9/11 dummy.

Although we are unable to separate the effect of the 2001 indicator from the post-9/11 indicator, 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 trend to decay over time. Table 4 displays the results.

---Insert table 4 here---

Column 1 includes the post-9/11 trend variable, which takes the value of 1 in October 2001, 2 in November 2001, and so on. The results reveal a statistically insignificant downward trend in fatalities after 9/11. 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 term, 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 indicate that the 9/11 effect weakened over time and allow 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 221 deaths

occurred in October 2001 and 294 in November 2001, after which the effect gradually declined to zero in October 2003. Summing the predicted deaths from October 2001 to September 2003 yields an estimate of about 2,302 total fatalities.

#### 4.3: Mechanisms for the 9/11 Effect

We have shown that non-commercial driving fatalities increased following 9/11 and we argue that this 9/11 effect 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 14 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 (not shown) does not demonstrate the difference to be statistically significant, the substantially higher effect for out-of-state fatalities is consistent with drivers substituting driving for flying for interstate trips.

Similarly, we should expect more substitution of driving for flying, and hence a greater increase in driving fatalities, in areas where driving provides a closer substitute for flying. The Northeast corridor of the United States, stretching from Washington, D.C., to Massachusetts contains many large cities connected by extensive highways, and is arguably the region where driving provides the closest substitute to flying. In addition, because all of the 9/11 attacks occurred on the East Coast, the fear of additional attacks may have been greatest there (Fischhoff

*et al* 2003). Therefore, we compare the increase in noncommercial fatalities in the Northeast corridor with that for the rest of the country.

Column 3 displays the results for the Northeast corridor where fatalities increased by 33 percent following 9/11. In contrast, column 4 shows that fatalities across the rest of the country increased by only 10 percent.

To further examine the substitution hypothesis, we examine the impact of 9/11 on air traffic, miles driven, and fatalities per mile driven. Here, we also control for total pounds of freight and mail carried on domestic flights in order to control for transportation-related economic activity. 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 17 percent after 9/11. Column 2 indicates that vehicle miles traveled (VMT) increased by about 4.8% following 9/11. Although this increase supports the substitution hypothesis, it is much less than the 12.8 percent increase in noncommercial fatalities reported in Table 2. However, the VMT data includes all vehicles; it is not disaggregated by vehicle type. In particular the VMT data include miles driven in commercial vehicles, which we would not expect to increase in response to the 9/11 effect. To better compare the fatality and VMT measures, we aggregate the fatalities in all types of vehicles: commercial and noncommercial fatalities, as well fatalities in difficult-to-categorize “dual-usage” vehicles. In column 3 we estimate the impact of 9/11 on this aggregated measure of fatalities. The results reveal that aggregated fatalities increased by 8.5 percent following 9/11, an effect much closer to the 4.8 percent increase in VMT. In column 4 we assess whether fatalities per mile rose following 9/11 (we divide the total fatalities for all vehicles by VMT). The results indicate that

fatalities per mile did not increase significantly following 9/11. Together these results provide strong support that the 9/11 effect was caused by travelers substituting driving for flying.

Although the effect in column 4 is statistically insignificant, the above results do provide some suggestion that fatalities per mile may have increased after 9/11. It is beyond the scope of this paper to assess all of the factors that may have increased fatalities per VMT.<sup>8</sup> Nonetheless, making the most conservative assumption, that the 9/11 effect consists only of the 4.8 percent increase in VMT, then our results indicate that about 185 additional people per month died in automobile accidents in the months following 9/11.<sup>9</sup>

To summarize, we observe the following 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 October 2003; (4) out-of-state fatalities increased more than in-state fatalities; (5) fatalities in the Northeast corridor increased more than fatalities elsewhere; and (6) VMT increased by about 4.8 percent. Taken together, these results provide strong evidence for the 9/11 effect, indicating that substitution of road travel for air travel was a prime mechanism for the rise in non-commercial driving fatalities following the 9/11 attacks.

## **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 as many as 2,300 lives were lost because of travelers' response to 9/11. These deaths are a hidden cost of the 9/11 attacks.

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<sup>8</sup> For one possible explanation, see Steklov and Goldstein 2004 which shows that terrorist attacks in Israel produce a 2-3 day slump in non-fatal road accident rates, followed by a 35% increase in fatal accident rates beginning three days after the attack. The paper speculates that this pattern could partly be attributable to attack-induced stress.

<sup>9</sup> We calculate the fraction of total fatalities attributable to the increase in VMT, and then multiply this by the number of additional non-commercial fatalities per month post 9/11:  $(0.048/0.085)*327.469=181.177$ .

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 drive 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 Deaths                            | 2583.82     | (242.34)  |
| Commercial Deaths                                | 421.79      | (48.65)   |
| Gasoline Price (\$ Per Gallon, Regular Unleaded) | 1.28        | (0.19)    |
| Airfare Index (Dec. 1989 = 100)                  | 182.44      | (32.54)   |
| Rail Freight Price Index (Dec. 1989 = 100)       | 114.47      | (3.29)    |
| Unemployment Rate (%)                            | 5.13        | (0.76)    |
| Personal Income (Billions of dollars)            | 7575.36     | (1146.16) |
| Air Passengers (1000's)                          | 47071.88    | (5286.87) |
| Vehicle Miles Traveled (Billions)                | 219.80      | (20.76)   |
| Air Freight and Mail (Millions of Pounds)        | 788.58      | (502.30)  |

All values are monthly.

**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       | 327.469<br>(69.373)**     | -27.128<br>(28.537)   | 0.128<br>(0.028)**        | -0.073<br>(0.070)     |
| Gas             | -142.136<br>(103.454)     | 35.799<br>(43.091)    | -0.060<br>(0.041)         | 0.086<br>(0.104)      |
| Airfare         | 6.204<br>(2.443)*         | -0.408<br>(0.754)     | 0.003<br>(0.001)**        | -0.001<br>(0.002)     |
| Rail Freight    | -37.471<br>(29.149)       | 7.644<br>(10.489)     | -0.013<br>(0.011)         | 0.024<br>(0.027)      |
| Unemployment    | -106.460<br>(59.608)+     | -3.716<br>(18.711)    | -0.045<br>(0.024)+        | -0.014<br>(0.046)     |
| Personal Income | 0.425<br>(0.213)*         | 0.011<br>(0.080)      | 0.000<br>(0.000)*         | -0.000<br>(0.000)     |
| 1995            | -161.144<br>(69.735)*     | -16.817<br>(23.897)   | -0.066<br>(0.027)*        | -0.029<br>(0.059)     |
| 1996            | -402.690<br>(140.689)**   | 1.569<br>(52.553)     | -0.164<br>(0.057)**       | 0.031<br>(0.131)      |
| 1997            | -651.508<br>(215.953)**   | 13.069<br>(78.530)    | -0.266<br>(0.086)**       | 0.069<br>(0.196)      |
| 1998            | -940.329<br>(310.842)**   | 3.202<br>(110.728)    | -0.385<br>(0.124)**       | 0.059<br>(0.277)      |
| 1999            | -1,191.050<br>(383.250)** | -1.148<br>(140.939)   | -0.487<br>(0.152)**       | 0.063<br>(0.352)      |
| 2000            | -1,578.286<br>(516.318)** | -26.637<br>(186.934)  | -0.647<br>(0.206)**       | 0.025<br>(0.471)      |
| 2001            | -1,740.240<br>(603.030)** | -37.570<br>(217.184)  | -0.715<br>(0.239)**       | 0.004<br>(0.546)      |
| 2002            | -1,784.889<br>(650.372)** | -46.605<br>(234.308)  | -0.732<br>(0.257)**       | -0.011<br>(0.589)     |
| 2003            | -1,873.332<br>(715.892)*  | -71.964<br>(258.964)  | -0.773<br>(0.283)**       | -0.072<br>(0.658)     |
| February        | -220.943<br>(36.172)**    | -42.204<br>(13.432)** | -0.099<br>(0.016)**       | -0.110<br>(0.035)**   |
| March           | 67.163<br>(37.325)+       | -3.792<br>(13.274)    | 0.030<br>(0.016)+         | -0.004<br>(0.034)     |
| April           | 94.522<br>(46.665)*       | -12.678<br>(16.287)   | 0.042<br>(0.020)*         | -0.028<br>(0.041)     |
| May             | 263.457<br>(54.744)**     | 7.728<br>(16.359)     | 0.108<br>(0.022)**        | 0.024<br>(0.042)      |
| June            | 286.936<br>(48.876)**     | 52.074<br>(17.059)**  | 0.117<br>(0.020)**        | 0.132<br>(0.042)**    |
| July            | 425.314<br>(53.343)**     | 53.400<br>(19.633)**  | 0.167<br>(0.022)**        | 0.135<br>(0.048)**    |
| August          | 457.714<br>(63.613)**     | 78.930<br>(21.179)**  | 0.178<br>(0.025)**        | 0.190<br>(0.051)**    |
| September       | 229.303<br>(62.183)**     | 52.285<br>(21.245)*   | 0.093<br>(0.025)**        | 0.135<br>(0.054)*     |
| October         | 367.951<br>(66.586)**     | 80.888<br>(22.550)**  | 0.143<br>(0.027)**        | 0.198<br>(0.057)**    |
| November        | 258.585<br>(70.307)**     | 26.661<br>(24.835)    | 0.103<br>(0.028)**        | 0.079<br>(0.062)      |

|              |                          |                         |                    |                  |
|--------------|--------------------------|-------------------------|--------------------|------------------|
| December     | 306.584<br>(79.193)**    | 36.169<br>(27.020)      | 0.119<br>(0.032)** | 0.101<br>(0.068) |
| Constant     | 3,988.440<br>(3,146.216) | -490.516<br>(1,078.289) | 8.191<br>(1.227)** | 3.557<br>(2.810) |
| Observations | 119                      | 119                     | 119                | 119              |
| R-squared    | 0.92                     | 0.74                    | 0.92               | 0.74             |

We exclude September 2001. Month indicators are relative to the omitted month of January. Year indicators are relative to the omitted year of 1994. + significant at 10%, \* significant at 5%, \*\* significant at 1%.

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

|                                | (1)                       | (2)                                     | (3)                | (4)                |
|--------------------------------|---------------------------|-----------------------------------------|--------------------|--------------------|
|                                | Ln(Non-Commercial Deaths) | Non-Commercial Deaths/Commercial Deaths | Ln(Deaths)         | Ln(Deaths)         |
| Post-9/11                      | 0.138<br>(0.029)**        | 1.235<br>(0.460)**                      | -0.000<br>(0.040)  | -0.073<br>(0.070)  |
| Ln(Commercial Deaths)          | 0.140<br>(0.043)**        |                                         |                    |                    |
| Non-Commercial                 |                           |                                         | 1.802<br>(0.009)** | 4.634<br>(0.000)   |
| Non-Commercial*Post-9/11       |                           |                                         | 0.056<br>(0.018)** | 0.201<br>(0.073)** |
| Gas                            | -0.072<br>(0.041)+        | -0.887<br>(0.624)                       | 0.013<br>(0.058)   | 0.086<br>(0.105)   |
| Airfare                        | 0.003<br>(0.001)**        | 0.019<br>(0.009)*                       | 0.001<br>(0.001)   | -0.001<br>(0.002)  |
| Rail Freight                   | -0.016<br>(0.011)         | -0.238<br>(0.161)                       | 0.005<br>(0.015)   | 0.024<br>(0.027)   |
| Unemployment                   | -0.043<br>(0.023)+        | -0.144<br>(0.280)                       | -0.030<br>(0.028)  | -0.014<br>(0.046)  |
| Personal Income                | 0.000<br>(0.000)*         | 0.001<br>(0.001)                        | 0.000<br>(0.000)   | -0.000<br>(0.000)  |
| Non-Commercial*Gas             |                           |                                         |                    | -0.146<br>(0.101)  |
| Non-Commercial*Airfare         |                           |                                         |                    | 0.003<br>(0.002)*  |
| Non-Commercial*Rail Freight    |                           |                                         |                    | -0.036<br>(0.026)  |
| Non-Commercial*Unemployment    |                           |                                         |                    | -0.031<br>(0.045)  |
| Non-Commercial*Personal income |                           |                                         |                    | 0.000<br>(0.000)   |
| 1995                           | -0.062<br>(0.026)*        | -0.231<br>(0.364)                       | -0.048<br>(0.034)  | -0.029<br>(0.060)  |
| 1996                           | -0.168<br>(0.054)**       | -1.223<br>(0.781)                       | -0.066<br>(0.075)  | 0.031<br>(0.132)   |
| 1997                           | -0.276<br>(0.083)**       | -2.106<br>(1.196)+                      | -0.099<br>(0.111)  | 0.069<br>(0.197)   |
| 1998                           | -0.394<br>(0.122)**       | -2.801<br>(1.746)                       | -0.163<br>(0.154)  | 0.059<br>(0.278)   |
| 1999                           | -0.496<br>(0.148)**       | -3.477<br>(2.172)                       | -0.212<br>(0.197)  | 0.063<br>(0.353)   |
| 2000                           | -0.650<br>(0.197)**       | -4.234<br>(2.857)                       | -0.311<br>(0.268)  | 0.025<br>(0.472)   |
| 2001                           | -0.716<br>(0.228)**       | -4.536<br>(3.297)                       | -0.355<br>(0.312)  | 0.004<br>(0.548)   |
| 2002                           | -0.730<br>(0.246)**       | -4.593<br>(3.578)                       | -0.371<br>(0.335)  | -0.011<br>(0.590)  |
| 2003                           | -0.763<br>(0.269)**       | -4.482<br>(3.974)                       | -0.422<br>(0.374)  | -0.072<br>(0.659)  |
| Non-Commercial*1995            |                           |                                         |                    | -0.037<br>(0.058)  |

|                          |                     |                   |                     |                     |
|--------------------------|---------------------|-------------------|---------------------|---------------------|
| Non-Commercial*1996      |                     |                   |                     | -0.195<br>(0.124)   |
| Non-Commercial*1997      |                     |                   |                     | -0.336<br>(0.190)+  |
| Non-Commercial*1998      |                     |                   |                     | -0.445<br>(0.278)   |
| Non-Commercial*1999      |                     |                   |                     | -0.550<br>(0.345)   |
| Non-Commercial*2000      |                     |                   |                     | -0.672<br>(0.453)   |
| Non-Commercial*2001      |                     |                   |                     | -0.719<br>(0.521)   |
| Non-Commercial*2002      |                     |                   |                     | -0.720<br>(0.566)   |
| Non-Commercial*2003      |                     |                   |                     | -0.701<br>(0.627)   |
| February                 | -0.084<br>(0.015)** | 0.043<br>(0.215)  | -0.104<br>(0.019)** | -0.110<br>(0.035)** |
| March                    | 0.030<br>(0.015)+   | 0.177<br>(0.206)  | 0.013<br>(0.019)    | -0.004<br>(0.034)   |
| April                    | 0.045<br>(0.019)*   | 0.417<br>(0.254)  | 0.007<br>(0.023)    | -0.028<br>(0.041)   |
| May                      | 0.105<br>(0.022)**  | 0.513<br>(0.258)* | 0.066<br>(0.025)**  | 0.024<br>(0.042)    |
| June                     | 0.098<br>(0.022)**  | -0.117<br>(0.266) | 0.124<br>(0.024)**  | 0.132<br>(0.042)**  |
| July                     | 0.148<br>(0.024)**  | 0.175<br>(0.300)  | 0.151<br>(0.027)**  | 0.135<br>(0.049)**  |
| August                   | 0.151<br>(0.027)**  | -0.099<br>(0.299) | 0.184<br>(0.031)**  | 0.190<br>(0.052)**  |
| September                | 0.075<br>(0.026)**  | -0.285<br>(0.332) | 0.114<br>(0.030)**  | 0.135<br>(0.054)*   |
| October                  | 0.115<br>(0.029)**  | -0.373<br>(0.351) | 0.171<br>(0.032)**  | 0.198<br>(0.057)**  |
| November                 | 0.092<br>(0.029)**  | 0.113<br>(0.398)  | 0.091<br>(0.034)**  | 0.079<br>(0.062)    |
| December                 | 0.105<br>(0.032)**  | 0.085<br>(0.435)  | 0.110<br>(0.038)**  | 0.101<br>(0.069)    |
| Non-Commercial*February  |                     |                   |                     | 0.011<br>(0.036)    |
| Non-Commercial*March     |                     |                   |                     | 0.034<br>(0.034)    |
| Non-Commercial*April     |                     |                   |                     | 0.070<br>(0.042)+   |
| Non-Commercial*May       |                     |                   |                     | 0.085<br>(0.042)*   |
| Non-Commercial*June      |                     |                   |                     | -0.015<br>(0.043)   |
| Non-Commercial*July      |                     |                   |                     | 0.033<br>(0.049)    |
| Non-Commercial*August    |                     |                   |                     | -0.012<br>(0.048)   |
| Non-Commercial*September |                     |                   |                     | -0.042<br>(0.053)   |

|                         |                    |                    |                    |                   |
|-------------------------|--------------------|--------------------|--------------------|-------------------|
| Non-Commercial*October  |                    |                    |                    | -0.055<br>(0.056) |
| Non-Commercial*November |                    |                    |                    | 0.024<br>(0.064)  |
| Non-Commercial*December |                    |                    |                    | 0.018<br>(0.069)  |
| Constant                | 7.693<br>(1.219)** | 24.144<br>(17.449) | 4.973<br>(1.562)** | 3.557<br>(2.816)  |
| Observations            | 119                | 119                | 238                | 238               |
| R-squared               | 0.93               | 0.50               | 1.00               | 1.00              |

We exclude September 2001. Month indicators are relative to the omitted month of January. Year indicators are relative to the omitted year of 1994. + significant at 10%, \* significant at 5%, \*\* significant at 1%.

**Table 4: Duration of 9/11 Effect**

|                                 | (2)                      | (3)                       | (4)                        |
|---------------------------------|--------------------------|---------------------------|----------------------------|
|                                 | Non-Commercial Deaths    | Non-Commercial Deaths     | Non-Commercial Deaths      |
| Post-9/11                       | 296.855<br>(87.111)**    | 254.935<br>(176.870)      | -78.655<br>(247.557)       |
| Post-9/11 trend                 | -4.709<br>(9.333)        |                           |                            |
| Inverse Post-9/11 trend         |                          | 164.992<br>(374.933)      | 2,159.111<br>(1,080.288)*  |
| Inverse-Squared Post-9/11 Trend |                          |                           | -3,119.874<br>(1,447.432)* |
| Gas                             | -130.195<br>(108.948)    | -135.998<br>(102.633)     | -45.111<br>(113.102)       |
| Air Fare                        | 5.706<br>(2.639)*        | 5.872<br>(2.541)*         | 4.654<br>(2.775)+          |
| Rail Freight                    | -27.920<br>(34.096)      | -36.840<br>(29.404)       | -24.827<br>(30.001)        |
| Unemployment                    | -98.211<br>(61.019)      | -101.381<br>(59.614)+     | -90.329<br>(58.768)        |
| Personal Income                 | 0.370<br>(0.255)         | 0.397<br>(0.218)+         | 0.193<br>(0.258)           |
| 1995                            | -134.959<br>(91.905)     | -147.048<br>(73.970)*     | -69.982<br>(91.574)        |
| 1996                            | -345.510<br>(190.237)+   | -372.345<br>(150.367)*    | -204.060<br>(190.375)      |
| 1997                            | -570.820<br>(283.177)*   | -605.276<br>(228.143)**   | -349.751<br>(286.906)      |
| 1998                            | -839.933<br>(395.025)*   | -877.963<br>(323.827)**   | -517.624<br>(405.287)      |
| 1999                            | -1,060.540<br>(494.611)* | -1,113.789<br>(401.966)** | -667.213<br>(504.434)      |
| 2000                            | -1,412.347<br>(654.111)* | -1,473.549<br>(544.238)** | -904.208<br>(675.510)      |
| 2001                            | -1,563.920<br>(741.375)* | -1,622.960<br>(632.493)*  | -986.657<br>(776.743)      |
| 2002                            | -1,568.212<br>(833.459)+ | -1,619.473<br>(718.519)*  | -843.391<br>(897.636)      |
| 2003                            | -1,609.523<br>(944.354)+ | -1,691.404<br>(790.572)*  | -813.532<br>(990.080)      |
| February                        | -217.941<br>(37.206)**   | -219.078<br>(36.102)**    | -208.652<br>(35.984)**     |
| March                           | 71.330<br>(38.816)+      | 70.247<br>(37.677)+       | 85.797<br>(38.536)*        |
| April                           | 99.918<br>(48.221)*      | 98.368<br>(47.037)*       | 116.912<br>(48.168)*       |
| May                             | 271.319<br>(58.178)**    | 268.776<br>(55.947)**     | 293.948<br>(59.232)**      |
| June                            | 296.502<br>(54.243)**    | 293.505<br>(50.071)**     | 326.334<br>(55.433)**      |
| July                            | 437.276<br>(60.295)**    | 433.055<br>(54.661)**     | 474.425<br>(62.220)**      |
| August                          | 472.235<br>(71.389)**    | 466.581<br>(64.334)**     | 514.580<br>(70.818)**      |

|              |                          |                          |                          |
|--------------|--------------------------|--------------------------|--------------------------|
| September    | 247.050<br>(76.019)**    | 239.607<br>(65.063)**    | 294.535<br>(77.228)**    |
| October      | 388.926<br>(82.075)**    | 378.108<br>(68.383)**    | 450.539<br>(83.290)**    |
| November     | 282.269<br>(88.272)**    | 272.300<br>(74.475)**    | 342.589<br>(88.258)**    |
| December     | 334.030<br>(99.552)**    | 323.002<br>(85.017)**    | 405.662<br>(102.634)**   |
| Constant     | 3,234.398<br>(3,396.078) | 4,085.719<br>(3,149.810) | 3,889.108<br>(3,160.037) |
| Observations | 119                      | 119                      | 119                      |
| R-squared    | 0.92                     | 0.92                     | 0.92                     |

We exclude September 2001. Month indicators are relative to the omitted month of January. Year indicators are relative to the omitted year of 1994. + significant at 10%, \* significant at 5%, \*\* significant at 1%.

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

|                 | (1)                 | (2)                     | (3)                                           | (4)                                   |
|-----------------|---------------------|-------------------------|-----------------------------------------------|---------------------------------------|
|                 | Ln(In-State Deaths) | Ln(Out-of-State Deaths) | Ln(Non-Commercial Deaths): Northeast Corridor | Ln(Non-Commercial Deaths): Rest of US |
| Post-9/11       | 0.140<br>(0.034)**  | 0.221<br>(0.075)**      | 0.333<br>(0.097)**                            | 0.104<br>(0.029)**                    |
| Gas             | -0.046<br>(0.048)   | -0.163<br>(0.109)       | -0.063<br>(0.128)                             | -0.060<br>(0.043)                     |
| Air Fare        | 0.002<br>(0.001)*   | 0.003<br>(0.002)        | 0.004<br>(0.003)                              | 0.002<br>(0.001)*                     |
| Rail Freight    | -0.012<br>(0.014)   | -0.014<br>(0.020)       | -0.044<br>(0.031)                             | -0.010<br>(0.012)                     |
| Unemployment    | -0.045<br>(0.030)   | -0.057<br>(0.048)       | -0.075<br>(0.058)                             | -0.041<br>(0.024)+                    |
| Personal Income | 0.000<br>(0.000)+   | 0.000<br>(0.000)        | 0.000<br>(0.000)                              | 0.000<br>(0.000)*                     |
| 1995            | -0.073<br>(0.030)*  | -0.110<br>(0.082)       | -0.093<br>(0.098)                             | -0.064<br>(0.028)*                    |
| 1996            | -0.171<br>(0.065)** | -0.280<br>(0.182)       | -0.289<br>(0.214)                             | -0.152<br>(0.057)**                   |
| 1997            | -0.280<br>(0.096)** | -0.421<br>(0.275)       | -0.346<br>(0.297)                             | -0.261<br>(0.087)                     |
| 1998            | -0.404<br>(0.140)** | -0.652<br>(0.379)+      | -0.482<br>(0.412)                             | -0.380<br>(0.126)**                   |
| 1999            | -0.506<br>(0.172)** | -0.829<br>(0.481)+      | -0.613<br>(0.519)                             | -0.480<br>(0.153)**                   |
| 2000            | -0.668<br>(0.230)** | -1.119<br>(0.655)+      | -0.789<br>(0.723)                             | -0.641<br>(0.206)**                   |
| 2001            | -0.748<br>(0.270)** | -1.219<br>(0.748)       | -0.792<br>(0.824)                             | -0.718<br>(0.240)**                   |
| 2002            | -0.769<br>(0.292)** | -1.354<br>(0.813)+      | -0.883<br>(0.890)                             | -0.726<br>(0.259)**                   |
| 2003            | -0.821<br>(0.318)*  | -1.410<br>(0.882)       | -0.908<br>(0.979)                             | -0.770<br>(0.284)**                   |
| February        | -0.103<br>(0.018)** | -0.058<br>(0.037)       | -0.110<br>(0.051)*                            | -0.097<br>(0.016)**                   |
| March           | 0.017<br>(0.018)    | 0.152<br>(0.040)**      | -0.013<br>(0.052)                             | 0.035<br>(0.017)*                     |
| April           | 0.041<br>(0.023)+   | 0.144<br>(0.045)**      | 0.023<br>(0.054)                              | 0.044<br>(0.019)*                     |
| May             | 0.097<br>(0.023)**  | 0.237<br>(0.043)**      | 0.145<br>(0.058)*                             | 0.104<br>(0.022)**                    |
| June            | 0.097<br>(0.023)**  | 0.336<br>(0.049)**      | 0.148<br>(0.061)*                             | 0.114<br>(0.020)**                    |
| July            | 0.124<br>(0.024)**  | 0.483<br>(0.062)**      | 0.174<br>(0.063)**                            | 0.166<br>(0.021)**                    |
| August          | 0.138<br>(0.028)**  | 0.467<br>(0.055)**      | 0.211<br>(0.066)**                            | 0.174<br>(0.026)**                    |
| September       | 0.072<br>(0.028)*   | 0.199<br>(0.070)**      | 0.093<br>(0.073)                              | 0.093<br>(0.025)**                    |
| October         | 0.128<br>(0.031)**  | 0.177<br>(0.077)*       | 0.183<br>(0.076)*                             | 0.138<br>(0.027)**                    |

|              |                    |                   |                    |                    |
|--------------|--------------------|-------------------|--------------------|--------------------|
| November     | 0.098<br>(0.032)** | 0.082<br>(0.082)  | 0.160<br>(0.087)+  | 0.096<br>(0.028)** |
| December     | 0.105<br>(0.036)** | 0.153<br>(0.081)+ | 0.163<br>(0.099)   | 0.113<br>(0.031)** |
| Constant     | 7.767<br>(1.422)** | 5.058<br>(2.362)* | 9.496<br>(3.599)** | 7.665<br>(1.289)** |
| Observations | 119                | 119               | 119                | 119                |
| R-squared    | 0.89               | 0.85              | 0.90               | 0.91               |

Accidents that occur outside of the state in which the driver is licensed are out-of-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. + significant at 10%, \* significant at 5%, \*\* significant at 1%.

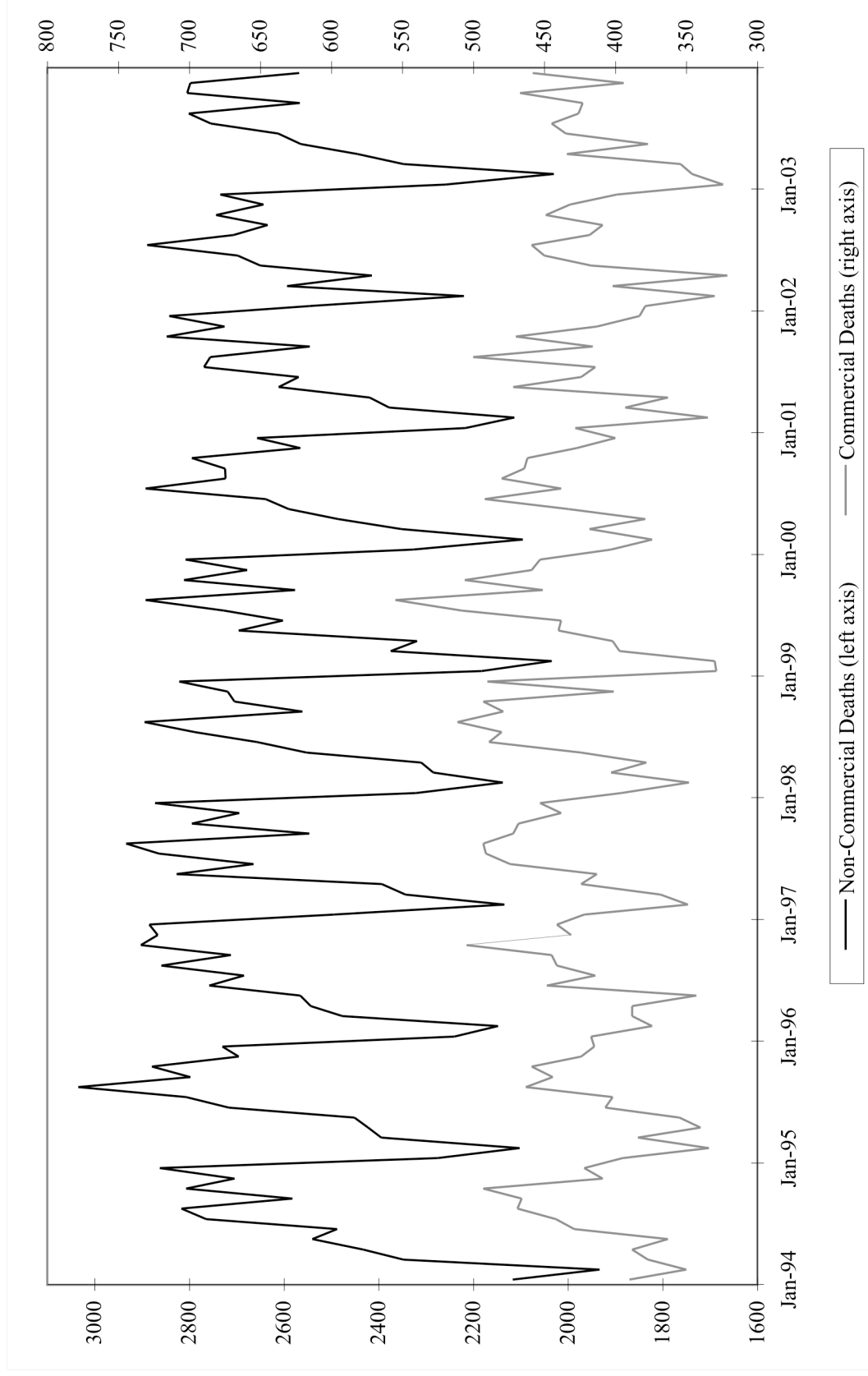
**Table 6: Effect of 9/11 on Air Passengers and Vehicle Miles Traveled**

|                      | (1)                 | (2)                 | (3)                       | (4)                 |
|----------------------|---------------------|---------------------|---------------------------|---------------------|
|                      | ln(Air Passengers)  | ln(VMT)             | Ln(Deaths in All Crashes) | Deaths Per Mile     |
| Post-9/11            | -0.172<br>(0.030)** | 0.048<br>(0.016)**  | 0.085<br>(0.034)*         | 0.037<br>(0.035)    |
| Gas                  | -0.023<br>(0.029)   | -0.023<br>(0.017)   | -0.027<br>(0.044)         | -0.004<br>(0.041)   |
| Air Fare             | -0.000<br>(0.001)   | 0.000<br>(0.000)    | 0.001<br>(0.001)          | 0.001<br>(0.001)    |
| Rail Freight         | -0.011<br>(0.007)   | -0.004<br>(0.004)   | -0.011<br>(0.013)         | -0.007<br>(0.014)   |
| Unemployment         | 0.004<br>(0.014)    | -0.010<br>(0.008)   | -0.031<br>(0.020)         | -0.021<br>(0.020)   |
| Personal Income      | 0.000<br>(0.000)    | 0.000<br>(0.000)**  | 0.000<br>(0.000)          | -0.000<br>(0.000)   |
| ln(Freight and Mail) | 0.085<br>(0.029)**  | 0.031<br>(0.011)**  | -0.032<br>(0.025)         | -0.063<br>(0.024)** |
| 1995                 | -0.003<br>(0.027)   | -0.015<br>(0.011)   | -0.018<br>(0.031)         | -0.003<br>(0.033)   |
| 1996                 | -0.042<br>(0.062)   | -0.038<br>(0.024)   | -0.051<br>(0.067)         | -0.013<br>(0.071)   |
| 1997                 | 0.003<br>(0.092)    | -0.061<br>(0.038)   | -0.086<br>(0.101)         | -0.025<br>(0.107)   |
| 1998                 | -0.021<br>(0.128)   | -0.097<br>(0.054)+  | -0.130<br>(0.141)         | -0.033<br>(0.147)   |
| 1999                 | -0.026<br>(0.159)   | -0.122<br>(0.067)+  | -0.165<br>(0.180)         | -0.043<br>(0.188)   |
| 2000                 | -0.023<br>(0.221)   | -0.173<br>(0.089)+  | -0.214<br>(0.239)         | -0.041<br>(0.250)   |
| 2001                 | -0.082<br>(0.257)   | -0.212<br>(0.105)*  | -0.208<br>(0.279)         | 0.004<br>(0.295)    |
| 2002                 | -0.008<br>(0.284)   | -0.238<br>(0.115)*  | -0.196<br>(0.305)         | 0.042<br>(0.322)    |
| 2003                 | -0.022<br>(0.314)   | -0.261<br>(0.129)*  | -0.170<br>(0.339)         | 0.091<br>(0.358)    |
| February             | -0.008<br>(0.011)   | -0.042<br>(0.006)** | -0.103<br>(0.015)**       | -0.061<br>(0.013)** |
| March                | 0.182<br>(0.011)**  | 0.098<br>(0.007)**  | 0.037<br>(0.015)*         | -0.061<br>(0.014)** |
| April                | 0.133<br>(0.013)**  | 0.097<br>(0.006)**  | 0.058<br>(0.018)**        | -0.039<br>(0.017)*  |
| May                  | 0.154<br>(0.015)**  | 0.151<br>(0.007)**  | 0.147<br>(0.021)**        | -0.004<br>(0.021)   |
| June                 | 0.201<br>(0.017)**  | 0.146<br>(0.008)**  | 0.178<br>(0.019)**        | 0.031<br>(0.020)    |
| July                 | 0.240<br>(0.019)**  | 0.178<br>(0.008)**  | 0.227<br>(0.020)**        | 0.048<br>(0.020)*   |
| August               | 0.232<br>(0.021)**  | 0.175<br>(0.009)**  | 0.253<br>(0.025)**        | 0.078<br>(0.026)**  |
| September            | 0.051<br>(0.025)*   | 0.088<br>(0.010)**  | 0.168<br>(0.024)**        | 0.080<br>(0.024)**  |
| October              | 0.130<br>(0.027)**  | 0.113<br>(0.012)**  | 0.209<br>(0.030)**        | 0.096<br>(0.032)**  |

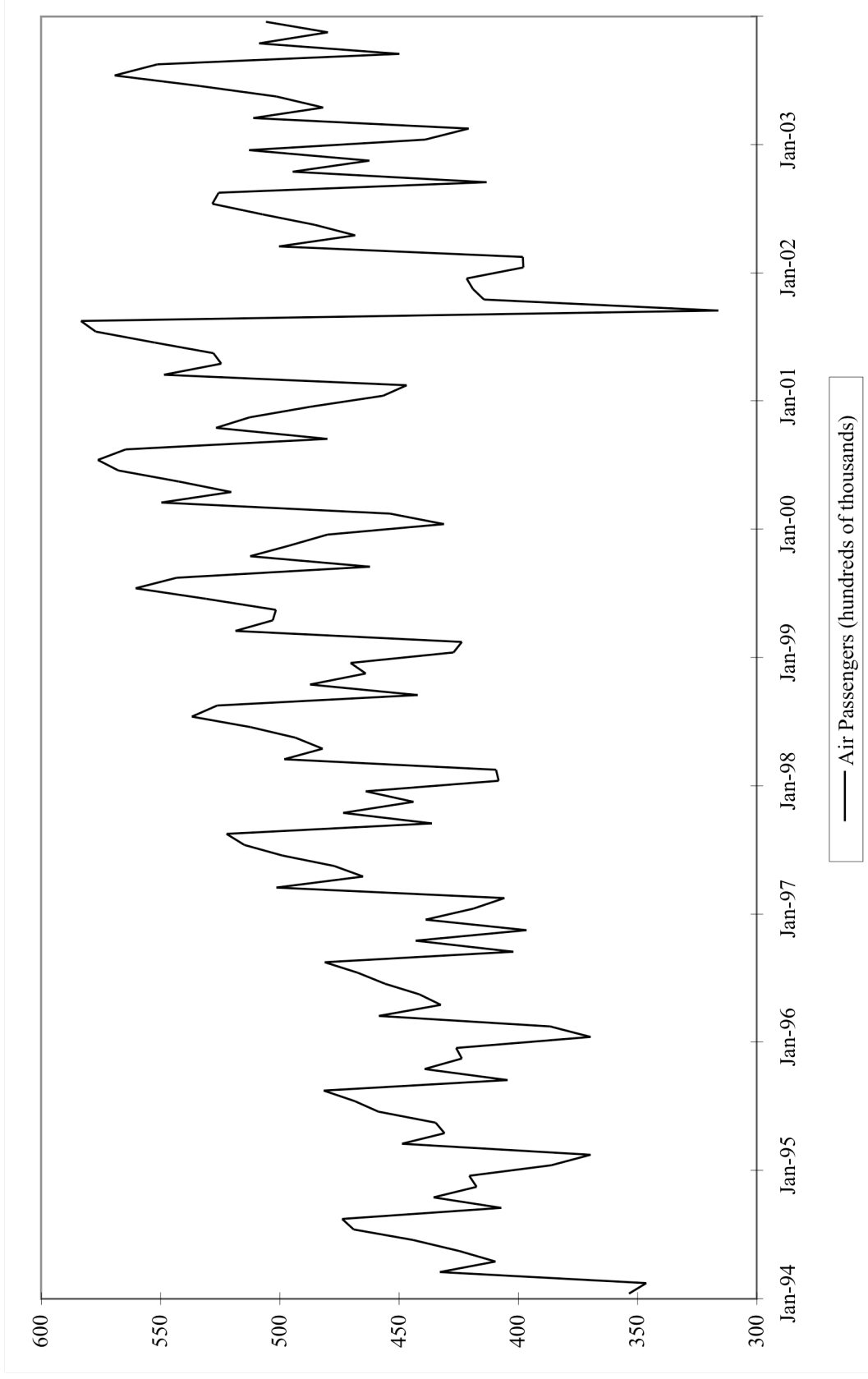
|              |                     |                     |                    |                    |
|--------------|---------------------|---------------------|--------------------|--------------------|
| November     | 0.083<br>(0.031)**  | 0.044<br>(0.012)**  | 0.138<br>(0.032)** | 0.094<br>(0.033)** |
| December     | 0.098<br>(0.032)**  | 0.044<br>(0.014)**  | 0.152<br>(0.037)** | 0.108<br>(0.037)** |
| Constant     | 16.270<br>(0.994)** | 11.244<br>(0.460)** | 9.824<br>(1.290)** | -1.420<br>(1.359)  |
| Observations | 119                 | 119                 | 119                | 119                |
| R-squared    | 0.97                | 0.99                | 0.94               | 0.90               |

We exclude September 2001. Month indicators are relative to the omitted month of January. Year indicators are relative to the omitted year of 1994. + significant at 10%, \* significant at 5%, \*\* significant at 1%.

**Figure 1: Commercial and Non-Commercial Driving Deaths by Month**



**Figure 2: Air Passenger Volume by Month**



**Figure 3: Vehicle Miles Traveled (VMT) by Month**

