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Dollars for Lives: The Effect of Highway Capital Investments on Traffic Fatalities

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16. Abstract There is no research that links capital investments on highways with highway fatalities. Our research project aimed to fill that gap. We used state-level data from the 48 contiguous states of the U.S. from 1968 through 2010 to estimate the effects of highway fatalities on capital expenditures and highway capital stock, which we defined as the total street and highway capital value that has accumulated up to a given point in time. We estimated these effects by controlling for a set of control variables, together with state and year dummy variables, and state-specific linear time trends. We found that capital expenditures and capital stock had significant and negative effects on highway fatalities. The results of our research emphasize that while state and local governments are currently fiscally strained, it is important for them to continue investments in roadways to enhance traffic safety and, more significantly, to save lives.			
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Abstract

There is no research that links highway capital investments with highway fatalities. Our research project aimed to fill that gap. We used state-level data from the 48 contiguous states of the U.S. from 1968 through 2010 to estimate the effects of highway fatalities on capital expenditures and highway capital stock, which we defined as the total street and highway capital value that has accumulated up to a given point in time. We estimated these effects by controlling for a set of control variables, together with state and year dummy variables, and state-specific linear time trends. We found that capital expenditures and capital stock had significant and negative effects on highway fatalities. The results of our research emphasize that while state and local governments are currently fiscally strained, it is important for them to continue investments in roadways to enhance traffic safety and, more significantly, to save lives.

Chapter 1 Introduction

Highway fatalities in the United States have declined for decades. According to the

Federal Highway Administration's (FHWA) annual Highway Statistics reports, the total number

of highway deaths in the nation fell from 53,816 in 1970 to 32,885 to 2010.[1] Despite this falling

trend in highway fatalities, motor vehicle traffic crashes remain a leading cause of death in the

country. The most recent available reports (Heron, 2013; Subramanian, 2012) indicated that that

motor vehicle crashes ranked eleventh as a cause of death in 2010 and fifth in terms of years of

life lost (the number of remaining years that a person would have expectedly lived had (s)he not

died) in 2009. Highway fatalities seem to have recently climbed back up. Relative to 2010,

highway fatalities increased in 2012 by 2.1% to 33,561, equivalent to an average daily death toll

of nearly 92. The 3.3% increase in 2012 from a death toll of 32,479 in 2011 represented the first

increase in highway fatalities since 2005 (National Highway Traffic Safety Administration,

2013).

Substantial research has focused on factors that could enhance traffic safety. As reviewed

in the proceeding section, some of these factors are state-determined, including minimum legal

drinking age, maximum speed limit, seat belt use, and state highway expenditures on law

enforcement. However, no study has ever been conducted to explore the effects of investments in

highway capital on traffic fatalities. This study is designed to fill this gap in the literature. To

examine the effects of capital expenditures on highway fatalities, we utilized state-level data for

all 48 contiguous states in the U.S. between 1968 and 2010. To preview our results, we found

strong evidence that investments in highway capital reduced highway fatalities. Specifically,

both the highway capital stock (to be defined formally later) and current capital expenditures had

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[1] As in Highway Statistics, the term "highway" used in this study includes state-administered highways, other arterials and collectors, and local roads.

significant negative effects on highway fatalities. We also found that the effect of capital expenditures was dependent on the existing level of the capital stock. States that had higher levels of highway capital stock had smaller marginal effects from capital expenditures.

This paper begins with some background on traffic fatalities as well as a theoretical explanation of how investments in highway capital can affect fatalities. Chapter 3 presents our estimation methods, including a description of our robustness tests and a presentation of the data we used in this study. Chapter 4 discusses the results of our main specification and of the robustness tests. Chapter 5 summarizes and concludes our study with an implication for public policy.

Chapter 2 Background

There is a large body of literature on the determinants of highway fatalities in the United States. While some state-level studies investigate highway fatality-related factors that are not state-mandated such as gasoline prices (Grabowski & Morrisey, 2004), unemployment (Leigh &Waldon, 1991), and precipitation (Eisenberg, 2004), we now focus our discussions on studies examining major state-mandated determinants, namely, speed limit, safety belt use, and minimum legal drinking age. Increases in speed limits have been found to have negative safety consequences on traffic fatalities (Baum, Wells, & Lund, 1990; Farmer, Retting, & Lund, 1999; Patterson, Frith, Povey, & Keall, 2002). Studies have also found that mandatory seatbelt laws, especially with primary enforcement, decrease traffic fatalities (Cohen & Einav, 2003; Farmer & Williams, 2005; Houseton & Richardson, 2006; Wagenaar, Maybee, & Sullivan, 1988). While Asch and Levy (1987) found no evidence on the influence of the state minimum drinking age on traffic fatalities, more recent studies found that higher minimum legal drinking ages reduce highway deaths (Fell et al., 2008; McCartt, Hellinga, & Kirley, 2010; Voas, Tippetts, & Fell, 2003).

In addition to those state-mandated factors, a couple of studies provide some evidence on the effects of non-capital highway expenditures on traffic fatalities. Specifically, studies by Koushki, Yaseen, and Hulsey (1995) and Zlatoper (1991) found evidence that spending on highway law enforcement and safety was negatively related with traffic fatality rates. This study seeks to add to the traffic safety literature by examining the effects on highway fatalities of investments in highway capital in the form of highway capital expenditures and highway capital stock.

How might investments in highway capital affect highway fatalities? Any effect of highway capital investments on fatalities could come potentially from additions to, and improvements in, (or lack thereof) various components of highway capital that may be correlated with highway fatalities, such as lane width, shoulder width, shoulder surface, fixed roadside objects (guardrails, light or overhead poles), and road surface quality. For example, in an analysis of 8,050 km of two-lane highways from seven states, Zegeer and Council (1995) found that lane widening could reduce traffic accidents by up to 40 percent, and shoulder widening could reduce related accidents by as much as 49 percent. Noland and Oh (2004) also found a negative association between outside shoulder width and traffic fatalities. Holdridge, Shankar, and Ulfarsson (2005) found that fixed roadside objects increased the propensity of fatal traffic injuries.

The conditions of a road may matter as poor road conditions could cause problems with steering, breaking, maneuvering, and other responses that could lead to the loss of vehicle control (Al-Masaeid, 1997; Anastasopoulos, Tarko, & Mannering, 2008; Burns, 1981). Better roads do not however necessarily lead to lower fatalities. Regular drivers usually pay more attention to roads in poorer conditions. In addition, speed may be lower on roads in poor conditions. This possibility is supported by a study in Canada. Transport Canada (1995) found that 97% of road accidents took place on roads described as "good." In a study using the Highway Statistics series, Noland (2003) also found little effect of infrastructure improvements on highway fatalities. Ultimately, the effects of highway capital investments on fatalities are an empirical question that this study aims to answer.

Chapter 3 Methods and Data

3.1 Methods

  We followed the literature on the determinants of traffic safety to model the effects of highway capital expenditures on highway fatalities. We defined the total persons fatally injured (*y*) in a given state (*s*) and year (*t*) as a function of logged highway capital expenditures per capita (*Flow*), logged highway capital stock per capita (*Stock*), and defined a set of controls for economic conditions (*Economy*), driver characteristics (*Driver*), government regulations (*Government*), and locational factors (*Location*). The variables contained in these characteristics are summarized in table 1. Amodel of the equation is represented as

$$y_{st} = f \begin{pmatrix} Flow_{st-1}, Stock_{st-2}, Economy_{st}, Driver_{st}, Government_{st}, \\ Location_{st}, State_{st}, Year_{st}, Trend_{st}, Error_{st} \end{pmatrix} \qquad (3.1)$$

where *State, Year, Trend,* and *Error* represent a set of state dummy variables or fixed effects, a set of year dummy variables, a set of state-specific linear trends, and an idiosyncratic error term, respectively.

  A key concern is the choice of an appropriate estimation method. Fatalities can be considered rare events (or count data) and are most likely to follow a Poisson or negative binomial distribution. We followed previous studies on traffic fatalities, such as Morrisey Grabowski, Dee, and Campbell (2006), Noland (2003), Noland and Oh (2004), and Ossiander and Cummings (2002), and estimated the conditional maximum likelihood approach for negative binomial models, where the log of state total annual population was included as an offset variable with its coefficient being constrained to 1. Because a negative binomial regression is more appropriate to correct for possible overdispersion, it was chosen over a Poisson regression.

We included state dummies to control for state unobserved factors that do not change (or change very little) over time, such as weather conditions (e.g., snowfall) that may affect both fatalities and capital expenditures. Year dummies control for common factors that affect highway fatalities across all the states in a year (e.g., safer vehicle models) that may bias our estimates of the effect of highway capital investments on highway fatalities. State-specific linear time trends control for observed underlying factors that follow a linear trend and are correlated with the error term. Our estimates of the key coefficients are based on the correlation between deviations from the state-specific trend in highway capital investments and deviations in highway fatalities.

State and year fixed effects and state-specific linear time trends did not control for variables associated with highway capital investments that changed over time nonlinearly within a state. As a result, we also controlled for a set of time-variant characteristics that measured economic conditions, driver characteristics, government regulations, and locational factors. Economic conditions were controlled for with a set of variables consisting of the unemployment rate, the log of per capita gross state product, the log of per capita personal income, and the log of the annual average retail price of a gallon of gas. Driver characteristics included the log of licensed drivers per capita, the log of registered vehicles per capita, and the share of trucks.[2] In terms of government regulations, we controlled for the presence and types (i.e., primary or secondary enforcement) of seatbelt laws, the maximum speed limits on rural and urban interstates, the enactment of child safety restraint laws, and the legal minimum age to purchase beer. Locational factors consisted of the log of total lane miles, the log of vehicle miles of travel

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[2] We did not include a variable representing minimum legal driving age because we did not find a data source that tracked its state-by-state changes over the period of 1980–2010. However, it is highly unlikely that our estimates suffered from bias as a result of this omitted variable for two reasons. First, all states established laws on the minimum driving age as early as 1954. We therefore believe that this minimum driving age had little within-state variation during our sample period, and thus should be captured mostly by state fixed effects we included in the estimations. Second, we already controlled for licensed drivers per capita which should capture, at least partially, any changes in the minimum driving age.

(VMT) per million miles, the log of population density, and the indices of annual precipitation and temperature.

The list of control variables might leave out unobserved variables that are systematically correlated with both variables of interest (*Flow* and *Stock* that will discussed in the proceeding paragraphs) and the dependent variable, but that are not captured by linear state-specific trends and state and year fixed effects. Such omitted variables, if in existence, would bias our estimates. However, this scenario is unlikely.

Capital investments include both capital stock (*Stock*) and capital expenditures (*Flow*) made by both state and local governments. Capital stock represents the condition of highways before new investments in highways are made. Capital expenditures per capita include both capital outlay per capita (*Outlay*) and maintenance per capita (*Maintenance*). According to the Federal Highway Administration (2013), capital outlay refers to expenditures on highway improvements, additions, and betterments. More specifically, it includes the costs of acquiring right-of-way, the construction of roads and structures (e.g., bridges, viaducts, tunnels, overpasses), and traffic service facilities including projects to enhance safety. Maintenance activities are intended to preserve highways as close as possible to the original condition by offsetting the negative effects from age, weather, use, damage, and design and construction faults. We lagged capital expenditures one year for two reasons. First, the lagged term eliminated the potential problem of reverse causality between traffic fatalities and capital expenditures. Moreover, unlike enforcement interventions, capital expenditures on highway infrastructure might not have immediate effects.[3]

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[3] An extreme example is when all capital expenditures are spent by the end of a fiscal year.

While capital expenditures is a flow variable representing spending over the course of a year, highway capital stock is a stock variable. It represents the total street and highway capital value that has accumulated over time and is measured at a given point in time (Young & Musgrave, 1980). We do not, however, have a direct measure of the value of the capital stock. Following previous studies (Boarnet, 1997; Wang, Duncombe, & Yinger, 2011), we derived a measure of physical highway capital stock using annual highway capital expenditures. More specifically, the capital stock measure for a given year was computed as the cumulative depreciated state and local capital outlay on highways, represented in the following equation:

$$\ln Stock_t = \ln\left(\sum_{i=0}^{10}\left((Outlay + Maintenance)_{t-(T-i)} \times (1 - d)^{T-i}\right)\right) \tag{3.2}$$

where $T = 10$ and d is a depreciation rate.

With $T = 10$, *Stock* is measured as the depreciated state and local spending on highway capital and maintenance in the current year and nine immediately preceding years. As in Wang, Duncombe, and Yinger (2011), we assumed d to be 2.02% based on the depreciation rate calculated by the Bureau of Economic Analysis (2004) for state and local government highways.

When estimating (1), we lagged *Stock* by two years because, as briefly noted above, capital stock should represent the condition of highways before states make new highway investments.[4] We hypothesized that more investments in the past, or larger capital stock, had negative effects on traffic fatalities. By holding constant the capital stock in this way, the one

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[4] We also tried a three-year lagged measure of the capital stock alongside the two-year lagged measure. However, if included, this three-year lagged measure was not significant while the coefficients on *Flow* and the two-year lagged *Stock* were still robust.

year lagged *Flow* variable then captured the marginal effect of the new investments in highways on traffic fatalities.

We also hypothesized that this marginal effect on fatalities of new highway investments was smaller for states that had already made substantial highway investments in the past to improve traffic safety. To investigate this hypothesis, we interacted *Flow* with a dummy variable indicating a state's annual capital stock levels relative to its peers. This dummy variable, (D_{50}), was equal to one if a state's *Stock* was above the median value of *Stock* for all states within a year. Our hypothesis is supported when the coefficient of this interaction term is positive. This would suggest that the combined effect of capital expenditures from both *Flow* and the interaction is smaller for high capital stock states.

3.2 Robustness Checks

To check how sensitive our main findings were to different measures and specifications, we subjected the findings to a battery of robustness tests. The first robustness check used bootstrapped standard errors for hypothesis testing. Bootstrapping, which was formally introduced by Efron (1979), is a form of resampling the original dataset to produce a series of datasets from which the standard error of an estimate is derived. Bootstrapped standard errors are recommended in the case of time-series data where the standard errors may be serially correlated (Bertrand, Duflo, & Mullainathan, 2004). We conducted this test to ensure that our hypothesis testing was robust to different estimates of standard errors.

As discussed previously, we did not have a direct measure of capital stock. As a result, we estimated it using the cumulative depreciated state and local capital outlay on highways, as represented by equation 3.2. To ensure that our results were robust to alternative measures of the

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capital stock, we conducted a series of estimations with the depreciation rates, d, of 1 or 3% (instead of 2.02%), and with $T = 9$ or $T = 11$ (instead of $T = 10$).

Finally, we estimated a pair of ordinary least squares regressions (OLS) to test the robustness of our results to different regression specifications. Following Grabowski and Morrisey (2004; 2006) and Houston and Richardson (2008), both of these analyses used the log of persons fatally injured as the dependent variable. The first OLS regression included the same control variables and the same measures of *Flow* and *Stock* as the main results. The second OLS model reports the results with fixed effects from a model with random intercepts for states and random coefficients for *Flow* and *Stock*, using the independent structure of the covariance matrix for the random effects. This has the effect of estimating a regression where each state has its own intercept and its own slope, but these intercepts and slopes are drawn randomly from a normal distribution (Hsiao, 2003).

3.3 Data

The main data source for this study is the Federal Highway Administration's (2013) Highway Statistics Annual Series (HS). HS provided us with data on motor vehicle highway fatalities, our dependent variable. The fatality data are originally reported from the National Highway Traffic Safety Administration. The fatalities counted are those in crashes involving a motor vehicle traveling on a traffic way and resulting in the death of at least one person within 30 days of the crash (National Highway Safety Administration, 2012). We also obtained data from HS on capital outlay and maintenance expenditures, vehicle miles traveled in millions, lane mileage, the share of motor vehicles that are trucks, and total of vehicle registrations. As discussed earlier, we used capital outlay and maintenance expenditures to develop a measure of a state's highway capital stock. While capital stock was derived from data from 1968 to 2010, all

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of the remaining variables were from 1980 to 2010. We converted all monetary values to 2010 dollars based on government price indexes published by the Bureau of Economic Analysis (2014a).

We obtained information on safety belt legislation and speed limits from the Insurance Institute for Highway Safety (2014a, b). Safety belt legislation takes the form of dummy variables for primary and secondary enforcement. Primary enforcement allows officers to pull over and ticket drivers solely for not wearing a seatbelt. With secondary enforcement, officers are only allowed to do so when there is a separate infraction. The National Highway Traffic Safety Administration (2011) provided data on child passenger protection laws. We retrieved data from the Bureau of Economic Analysis (2014b) on state population, gross state product, and personal income. The Bureau of Labor Statistics (2014) provided us with data on unemployment. We used data on population and land area to create a measure of population density. We obtained data on gas prices from the Energy Information Administration (2014). Weather data in the form of average annual precipitation and temperature were collected from the National Climatic Data Center (2014).

Table 1 presents descriptive statistics for the variables used in this study. The average state in our dataset had 880 fatalities. This statistic ranges from a minimum of 63 to a maximum of 5,504. The capital stock per capita of the average state was estimated to be worth $2,086 with a standard deviation of $1,219. The average state in the dataset spent $534 per person in capital expenditures. There is considerable variation in this measure. Capital expenditures ranged from $157 to over $3,250 per person.

The average state in our dataset saw drivers travel 49.3 trillion miles on 169,821 miles of lanes. Licensed drivers made up 69% of residents. There were 0.80 registered vehicles per

person, and 36.9% of all registered vehicles were trucks. We found that only a minority of states had primary enforcement of seat belt laws, though most states had some form of enforcement. Speed limits for rural and urban roads in our dataset ranged from 55 to a maximum of 75 miles per hour. Almost all states had some form of a child restraint law. The average legal minimum age to purchase beer was 20.7 years old. The price of a gallon of gas averaged $1.97. The average state received 3.08 inches of precipitation in a month and had an average monthly temperature of 52.47°. Average gross state product per capita in the dataset was $42,765, and the

average personal income per capita was $36,388. Unemployment was relatively low over the course of 1980 to 2010 at an average of 5.93%, and was as low as 2.3% and as high as 17.4%. Average population density in the data was 169 persons per square mile, with a standard deviation of 227.

Chapter 4 Results

4.1. Main results

Table A.2 presents our main set of regression results. While the results for our non-capital-related variables are also presented in this table, we focus on the capital-related variables of primary interest for reasons of parsimony. We report our negative binomial results in the form of incidence rate ratios for ease of interpretation. Column 1 shows that the *Flow* variable is negative and significant at the 0.05 level with the incidence rate ratio less than 1, suggesting that an increase in capital expenditures per capita was associated with a decrease in the number of persons fatally injured in a state. Specifically, we found that a one-*unit* increase in logged capital expenditures per capita was associated with a 2.5% decrease in the total persons fatally injured in a state. In percentage terms, a state's fatalities would decline by 2.5% as a result of an increase of 172% in per capita capital expenditures.[5] A 1% increase in capital expenditures is correlated with a reduction in fatalities of about 0.015% (=2.5/172). The effect of capital stock is also negative and significant though somewhat larger than the effect of capital expenditures. We find that a one-unit increase in logged capital stock per capita was associated with a 5.9% decrease in the total persons fatally injured in a state. In percentage terms, a 1% increase in capital stock per capita was correlated with a reduction of 0.034% in fatalities. These results provide strong evidence that capital investments in the form of capital expenditures or in the capital stock could reduce the number of fatalities in a state.

In column 2 of table A.2, we included D_{50} and its interaction with *Flow*, both of which were significant at the 0.01 level. The estimates in this column provide evidence for the

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[5] Suppose a state has the mean per capita capital spending of $534 (or 6.28 in natural logarithm). It then increases the expenditures by $1,450, or one more unit of logged expenditures, representing 172% [=((1,450 – 534) / 534) × 100].

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hypothesis that the marginal effect of new investments in highways was smaller for high capital stock states, such as states that have already invested substantially in highways in the past. Specifically, while the effect of capital stock on fatalities was almost the same as in column 1, we found (using similar calculations as derived above) that a 1% increase in capital expenditures per capita made by a state in a lower capital stock group is associated with a 0.03% decrease in highway fatalities. However, the marginal effect of new capital expenditures on traffic fatalities among states in the higher capital stock group was indistinguishable from 0. Consequently, we failed to reject the null hypothesis for a post-regression statistical test on the combined effect of *Flow* and (*Flow* × *D*$_{50}$). Although new investments in highways were found not to produce immediate effects on traffic fatalities for these high capital stock states, investments in highways seemed to prove helpful in lowering fatalities in the long run, represented by a highly significant coefficient of the dummy variable of *D*$_{50}$.

4.2 Robustness Checks

Table A.3 presents the results of our robustness checks described earlier. The results were marked in their consistency. Capital expenditures had a negative and significant impact on highway fatalities in all models. Capital stock was negative and statistically significant in almost all of the columns. The interaction term was positive and significant in each column as well, suggestive that the effects on fatalities of capital expenditures were larger for states in the lower capital stock group and smaller for those in the higher capital stock group.

In column 1 of this table A.3, we conducted hypothesis tests using bootstrapped standard errors instead of Huber-White robust standard errors. As in column 2 of table A.2, the four key variables were all significant, though *Stock* was only significant at the 0.10 level. In columns 2-5, we used different measures of the capital stock measures for our estimations. In columns 2 and 3,

we estimated capital stock using depreciation rates of one and three percent, respectively. Instead of 10 years (T=10), we used nine and eleven years to compute the capital stock in columns 4 and 5, respectively. All of these changes appeared to have little impact on the estimated effects of our capital investment variables on traffic fatalities.

Columns 6 and 7 present results from ordinary least squares (as opposed to negative binomial) regressions of capital investments in highways. Column 6 shows larger effects of capital investments on fatalities than those obtained in negative binomial regressions. This column suggests that a 1% increase in highway capital expenditures among states in the lower capital stock group was associated with a 0.056% decrease in fatalities. Capital stock was marginally significant at the 0.10 level, but also appeared to have a similar negative effect. Finally, as in our negative binomial regressions, this specification showed that the capital expenditures had little effect on fatalities in states that had relatively high levels of capital stock.

Column 7 of table A.3 reports results on fixed effects from a model with random intercepts for states and random coefficients for *Flow* and *Stock*, using the independent structure of the covariance matrix for the random effects. In this specification, a 1% increase in capital expenditures for states in the lower capital stock group was associated with a 0.032% decrease in highway fatalities, holding all else equal. The effect was substantially similar in size to that found in column 2 of Table 2. Increasing in magnitude from column 6 to 7 was the estimate of the effect of capital stock. A 1% increase in capital stock was, on average, associated with a 0.084% decrease in highway fatalities. As in all our regressions, the interaction term was highly significant, suggesting a smaller effect from capital expenditures for states with high levels of capital stock.

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Chapter 5 Conclusion

This is the first study to examine the relationship between investments in highway capital and traffic fatalities using nationwide state-level panel data. We found empirical evidence that highway capital expenditures and highway capital stock had significant and negative effects on traffic fatalities. Also, the effect of highway capital expenditures was conditional on a state's capital stock level, measured by past highway investments. These findings were robust to a battery of robustness checks.

Our findings suggest that if state and local governments are interested in reducing traffic fatalities, they must continue to make investments in the road system in both capital and maintenance. Indeed, past expenditures on capital and maintenance (captured in our capital stock measure) matter as much, if not more, in reducing fatalities than present capital expenditures. In Texas, the state with the highest number of traffic fatalities (2,998) in 2010, and in the lower capital stock group, the main estimation results suggest that a 10% increase in per capita real capital expenditures in 2009 (or a $37 increase from $372 to $409.2) would save the lives of approximately nine people the following year. Capital stock is expected to have an effect of similar size on fatalities as the results of the capital expenditures suggest.

We also found that the effect of capital expenditures was moderated by prior investments in the highway capital stock of a state. Capital expenditures had a larger effect on fatalities in states with relatively low levels of capital stock. This result suggests that states with low levels of capital stock can "catch up" to their peers through immediate investments in highway capital. On the other hand, new capital expenditures appeared to have very small immediate effects on fatalities in states with a relatively high level of capital stock. However, for these states, current

investments in highway capital still potentially end up saving human lives through the effect of the capital stock.

The findings of this study provide important policy implications given recent reductions in state revenues for transportation. The American Recovery and Reinvestment, or federal stimulus, provided $27.5 billion for repair and construction of highways and bridges (Hossain, Cox, McGrath, & Weitberg, 2012). However, most of these dollars ended in 2012. According to estimates by the Congressional Budget Office (2012), when fully enacted in 2040, new federal fuel-economy standards will reduce gas tax revenues—a major source of states' highway revenues—by 21%. States faced with declines in gas tax revenues have already cut back spending drastically on roads, including maintenance and capital outlay (Reed & Rall, 2011). If this trend continues, it may undermine traffic safety. Investments in the road system in the form of capital expenditures and capital stock are not only essential to the economy of a state, they are essential to saving lives.

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Appendix

Table A.1 Summary statistics

Variables	Mean	Standard Deviation	Minimum	Maximum
Dependent Variables				
Total persons fatally injured	880	861	63	5,504
Independent Variables				
Total highway expenditures per capita (1979–2009)	534	289	157	3,250
Capital stock per capita (*T=10*) (1978–2008)	2,086	1,219	455	12,203
Driver Characteristics				
Vehicle miles travelled (in millions)	49,327	50,832	3,717	329,267
Total lane miles	169,821	105,752	11,257	670,335
Licensed drivers per capita	0.69	0.05	0.51	0.91
Ratio of trucks to total registered vehicles	36.89	11.01	6.29	67.83
Total registered vehicles per capita	0.80	0.13	0.33	1.26
Government Regulations				
Dummy variable for primary enforcement of seat belt laws	0.22	0.41	0	1
Dummy variable for secondary enforcement of seat belt laws	0.48	0.50	0	1
Maximum speed limit in rural areas	64.13	6.81	55	75
Maximum speed limit in urban areas	60.24	6.25	55	75
Dummy variable for child restraint law	0.89	0.32	0	1
Legal minimum age to purchase beer	20.68	0.86	18	21
Locational Factors				
Price of a gallon of gas	1.97	0.74	0.87	3.98
Annual average precipitation index	3.08	1.25	0.45	6.72
Annual average temperature index	52.47	7.60	36.53	72.58
Economic Conditions				
Gross state product per capita (in thousands)	42.76	8.90	24.63	78.55
Income per capita (in thousands)	36.39	6.93	20.96	61.33
Unemployment rate	5.93	2.12	2.30	17.40
Population density	169	227	4.64	1,130

Notes: There are 1,488 observations for 48 contiguous states during 1980–2010. All monetary values are in 2010 dollars. For parsimony, some variables used in robustness tests, namely, capital stock measures using different discount rates or different *T*'s, are not presented in this table.

Table A.2 Negative binomial regression results

Dependent Variable: Total Persons Fatally Injured

Variable	(1)	(2)
Log of one year-lagged total highway expenditures per capita (*Flow*)	0.975 (-2.47)**	0.949 (-3.96)***
Log of two-year lagged capital stock per capita (*Stock*)	0.941 (-2.49)**	0.944 (-2.09)**
Dummy variable (=1 if annual *Stock* is above median and 0 otherwise) (D_{50})		0.715 (-2.93)***
Flow $\times$ D_{50}		1.056 (2.92)***
Log of VMT (in millions)	1.124 (2.32)**	1.116 (2.16)**
Log of lane mileage	0.850 (-3.13)***	0.840 (-3.30)***
Truck share	1.002 (2.09)**	1.002 (2.17)**
Log of registered vehicles per capita	0.998 (-0.07)	0.997 (-0.11)
Log of licensed drivers per capita	1.041 (0.82)	1.046 (0.90)
Maximum speed on rural interstates	1.008 (5.70)***	1.008 (5.80)***
Maximum speed on urban interstates	1.001 (1.27)	1.001 (1.31)
Primary enforcement of seatbelt laws	0.954 (-4.49)***	0.956 (-4.26)***
Secondary enforcement of seatbelt laws	0.979 (-2.75)***	0.979 (-2.65)***
Dummy variable for child restraint law	0.993 (-0.56)	0.990 (-0.76)
Minimum age to purchase beer	0.992 (-2.00)**	0.992 (-1.96)*
Monthly average precipitation index	0.980 (-5.32)***	0.980 (-5.23)***
Monthly average temperature index	1.008 (3.24)***	1.008 (3.27)***
Log of average price of a gallon of gas	0.939	0.954

	(-1.19)	(-0.87)
Log of gross state product per capita	1.118	1.141
	(1.45)	(1.71)*
Log of income per capita (in thousands)	1.753	1.694
	(4.55)***	(4.21)***
Unemployment rate	0.980	0.980
	(-8.43)***	(-8.49)***
Log of population density	2.196	2.195
	(6.52)***	(6.47)***

Notes: The coefficients are reported in terms of incidence rate ratios. All regressions control for state and year fixed effects and state-specific linear time trends. *T*-statistics are in parentheses. Hypothesis testing is done with Huber-White robust standard errors adjusted for clustering by state. There are 1,488 observations. *p<0.10, ** p<0.05, *** p<0.01.

Table A.3 Results of robustness tests

Key Variables	Bootstrapped standard errors	$d = 1$ percent	$d = 3$ percent	$T = 9$	$T = 11$	OLS	Random coefficients
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log of one-year lagged total highway expenditures per capita (*Flow*)	0.949 (-3.64)***	0.949 (-4.00)***	0.949 (-3.97)***	0.952 (-3.75)***	0.946 (-4.21)***	-0.056 (-3.46)***	-0.032 (-1.96)*
Log of two-year lagged capital stock per capita (*Stock*)	0.944 (-1.94)*	0.947 (-1.97)**	0.945 (-2.08)**	0.942 (-2.28)**	0.954 (-1.65)*	-0.050 (-1.64)	-0.084 (-3.02)***
Dummy variable (=1 if annual *Stock* is above average and 0 otherwise) (D_{50})	0.715 (-2.89)***	0.712 (-2.97)***	0.710 (-3.00)***	0.727 (-2.81)***	0.695 (-3.20)***	-0.31 (-2.51)**	-0.36 (-2.87)***
Flow $\times$ D_{50}	1.056 (2.86)***	1.056 (2.95)***	1.057 (2.98)***	1.053 (2.77)***	1.061 (3.17)***	0.050 (2.55)**	0.049 (2.90)***

Notes: Column 1 uses bootstrapped standard errors. Columns (2) and (3) employ depreciation rates (*d*) of 1 and 3 percent, respectively, to compute the stock measure using equation (2). Columns (4) and (5) employ $T=9$ and $T=11$, respectively, to compute for the stock measure using equation (2). The dependent variable for columns (6) and (7) is logged fatalities. Column (6) is estimated with OLS; column (7) reports the results on fixed effects from a model with random intercepts for states and random coefficients for *Flow* and *Stock*, using the independent structure of the covariance matrix for the random effects. All regressions, except for column (7), control for state and year fixed effects and state-specific linear time trends. *T*-statistics are in parentheses. Hypothesis testing is done with bootstrapped standard errors in column (1), robust standard errors adjusted for clustering by state in columns (2)–(6), and robust standard errors in column (7). The number of observations and unreported control variables are the same as Table 2.
* $p<0.10$, ** $p<0.05$, *** $p<0.01$.