Safety Effects of Differential Speed Limits on Rural Interstate Highways

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FOREWORD

The Surface Transportation and Uniform Relocation Assistance Act, (STURAA) enacted on April 2, 1987, permitted individual States to raise rural interstate speed limits from the previously mandated national speed limit of 89 kilometers per hour (km/h) (55 miles per hour (mi/h)) to 105 km/h (65 mi/h) on rural interstate highways. Of those that changed their speed limits, some States raised the limits for passenger cars but not trucks while other States raised the limits for both passenger cars and trucks. The former category, with different speed limits for cars and trucks, is known as differential speed limits (DSL). The latter category, which mandates the same speed limits for cars and trucks, is known as uniform speed limits (USL). The 1995 repeal of the national maximum speed limit gave States additional flexibility in setting their limits, such that by 2002 several States had experimented with both DSL and USL.

This report compares the safety effects of USL for all vehicles as opposed to DSL for cars and heavy trucks. Detailed crash data, speed monitoring data, and traffic volumes were sought for rural interstate highways in 17 States for the period 1991 to 2000. The information and results of the study will be of particular interest to State traffic managers in making decisions about the application of USL or DSL in their highway systems.

Michael Trentacoste, Director
Office of Safety Research
and Development

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No consistent safety effects of DSL as opposed to USL were observed within the scope of the study. The mean speed, 85th percentile speed, median speed, and crash rates tended to increase over the 10-year period, regardless of whether a DSL or USL limit was employed. When all sites within a State were included in the analysis, temporal differences in these variables were often not significant. Further examination suggests that while these data do not show a distinction between DSL and USL safety impacts, the relationship between crashes and traffic volume cannot be generalized but instead varies by site within a single State. Because application of the modified empirical Bayes methodology suggested that crash risk increased for all four policy groups, a mathematical model that predicts sharp changes in crash rates based only on ADT does not appear valid at the statewide level.</td></tr>
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SI* (MODERN METRIC) CONVERSION FACTORS				
APPROXIMATE CONVERSIONS TO SI UNITS				
Symbol	**When You Know**	**Multiply By**	**To Find**	**Symbol**
		LENGTH		
in	inches	25.4	millimeters	mm
ft	feet	0.305	meters	m
yd	yards	0.914	meters	m
mi	miles	1.61	kilometers	km
		AREA		
in^2	square inches	645.2	square millimeters	mm^2
ft^2	square feet	0.093	square meters	m^2
yd^2	square yard	0.836	square meters	m^2
ac	acres	0.405	hectares	ha
mi^2	square miles	2.59	square kilometers	km^2
		VOLUME		
fl oz	fluid ounces	29.57	milliliters	mL
gal	gallons	3.785	liters	L
ft^3	cubic feet	0.028	cubic meters	m^3
yd^3	cubic yards	0.765	cubic meters	m^3
		NOTE: volumes greater than 1000 L shall be shown in m^3		
		MASS		
oz	ounces	28.35	grams	g
lb	pounds	0.454	kilograms	kg
T	short tons (2000 lb)	0.907	megagrams (or "metric ton")	Mg (or "t")
		TEMPERATURE (exact degrees)		
^oF	Fahrenheit	5 (F-32)/9 or (F-32)/1.8	Celsius	^oC
		ILLUMINATION		
fc	foot-candles	10.76	lux	lx
fl	foot-Lamberts	3.426	candela/m^2	cd/m^2
		FORCE and PRESSURE or STRESS		
lbf	poundforce	4.45	newtons	N
lbf/in^2	poundforce per square inch	6.89	kilopascals	kPa
APPROXIMATE CONVERSIONS FROM SI UNITS				
Symbol	**When You Know**	**Multiply By**	**To Find**	**Symbol**
		LENGTH		
mm	millimeters	0.039	inches	in
m	meters	3.28	feet	ft
m	meters	1.09	yards	yd
km	kilometers	0.621	miles	mi
		AREA		
mm^2	square millimeters	0.0016	square inches	in^2
m^2	square meters	10.764	square feet	ft^2
m^2	square meters	1.195	square yards	yd^2
ha	hectares	2.47	acres	ac
km^2	square kilometers	0.386	square miles	mi^2
		VOLUME		
mL	milliliters	0.034	fluid ounces	fl oz
L	liters	0.264	gallons	gal
m^3	cubic meters	35.314	cubic feet	ft^3
m^3	cubic meters	1.307	cubic yards	yd^3
		MASS		
g	grams	0.035	ounces	oz
kg	kilograms	2.202	pounds	lb
Mg (or "t")	megagrams (or "metric ton")	1.103	short tons (2000 lb)	T
		TEMPERATURE (exact degrees)		
^oC	Celsius	1.8C+32	Fahrenheit	^oF
		ILLUMINATION		
lx	lux	0.0929	foot-candles	fc
cd/m^2	candela/m^2	0.2919	foot-Lamberts	fl
		FORCE and PRESSURE or STRESS		
N	newtons	0.225	poundforce	lbf
kPa	kilopascals	0.145	poundforce per square inch	lbf/in^2

*SI is the symbol for the International System of Units. Appropriate rounding should be made to comply with Section 4 of ASTM E380.
(Revised March 2003)

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ABSTRACT

To compare the safety effects of a uniform speed limit (USL) for all vehicles as opposed to a differential speed limit (DSL) for cars and heavy trucks, detailed crash data, speed monitoring data, and traffic volumes were sought for rural interstate highways in 17 States for the period 1991 to 2000. Data from nine of those States were used such that they could be divided into four policy groups based on the type of speed limit employed during the period. These were maintenance of a uniform limit only, maintenance of a differential limit only, a change from a uniform to a differential limit, and a change from a differential to a uniform limit. Conventional statistical tests (analysis of variance, Tukey's test, and Dunnett's test) were used to study speed and crash rate changes in the four policy groups. A modified empirical Bayes formation was used to evaluate crash frequency changes without presuming a constant relationship between crashes and traffic volume.

No consistent safety effects of DSL as opposed to USL were observed within the scope of the study. The mean speed, 85th percentile speed, median speed, and crash rates tended to increase over the 10-year period, regardless of whether a DSL or USL limit was employed. When all sites within a State were included in the analysis, temporal differences in these variables were often not significant. Further examination suggests that while these data do not show a distinction between DSL and USL safety impacts, the relationship between crashes and traffic volume cannot be generalized but instead varies by site within a single State. Because application of the modified empirical Bayes methodology suggested that crash risk increased for all four policy groups, a mathematical model that predicts sharp changes in crash rates based only on ADT does not appear valid at the statewide level.

Any study that relies on historical data will be subject to the limitations of incomplete data sets, and to that extent, additional data collection may shed insights not available from an examination of 1990s data alone. Because the investigators believe that accurate mathematical models may require extensive calibration data, a future effort may be more productive if resources are focused on a small group of States over a period of several years, so that speed variance information and crash information may be obtained by individual roadway segment.

INTRODUCTION

The Surface Transportation and Uniform Relocation Assistance Act (STURAA), enacted on April 2, 1987, permitted individual States to raise rural interstate speed limits from the previously mandated national speed limit of 89 kilometers per hour (km/h) (55 miles per hour (mi/h)) to 105 km/h (65 mi/h) on rural interstate highways. Of those that changed their speed limits, some States raised the limits for passenger cars but not trucks while other States raised the limits for both passenger cars and trucks. The former category, with different speed limits for cars and trucks, is known as differential speed limits (DSL). The latter category, which mandates the same speed limits for cars and trucks, is known as uniform speed limits (USL). The 1995 repeal of the national maximum speed limit gave States additional flexibility in setting their limits, such that by 2002, several States had experimented with both DSL and USL. Figure 1 shows the types of speed limits throughout the 1990s for rural interstate highways used in this study.

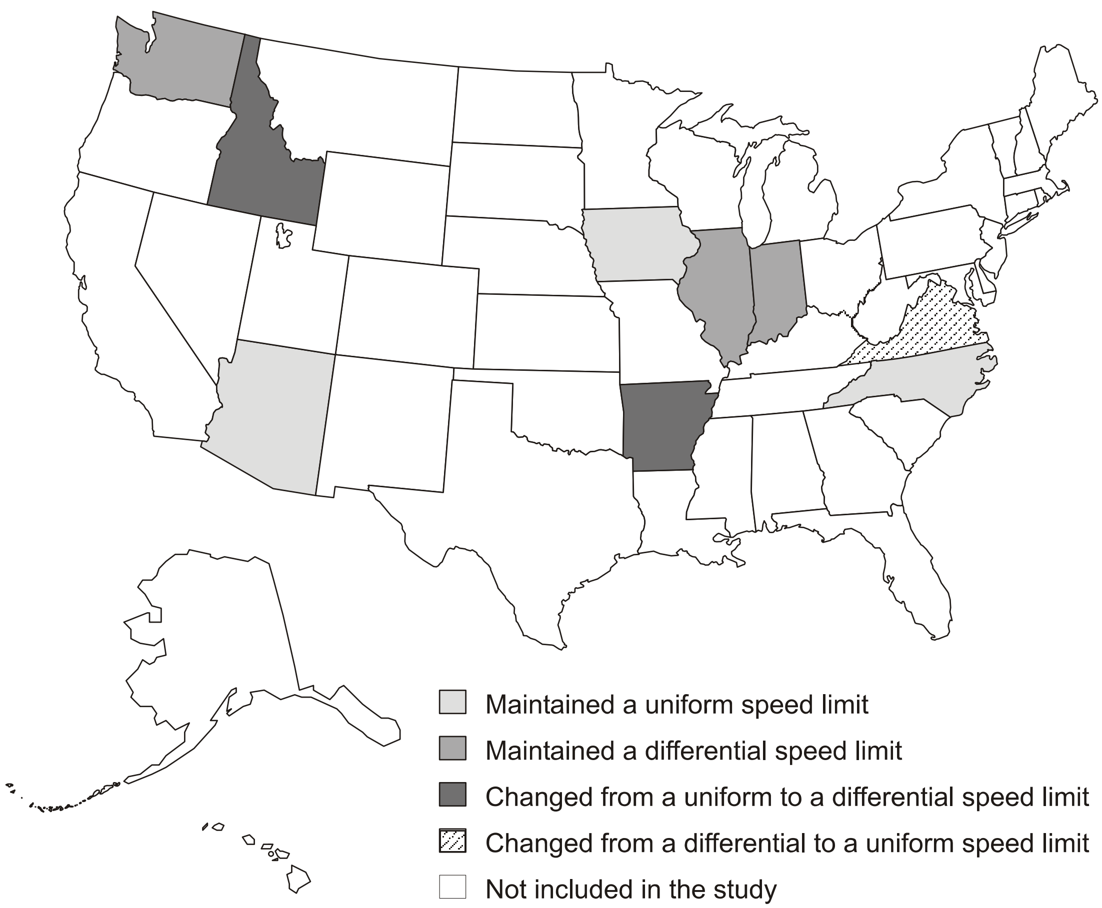


Figure 1. Map. Speed limits throughout the 1990s on rural interstate highways.

Proponents of DSL note that heavy trucks require a larger stopping distance and stopping time than passenger cars. Thus, a lower speed for trucks would reduce the risk of crashes that would otherwise be associated with these larger vehicles traveling at high speeds. Proponents of USL contend that different limits could increase speed variance, resulting in more conflicts between trucks and other types of vehicles, especially rear-end and sideswipe collisions. Advocates of a uniform limit also suggest that the higher driver position in a truck provides a greater sight distance than would be the case for a passenger car, giving truck drivers more time to stop.

Studies conducted during the early 1990s to compare the safety impacts of USL and DSL were constrained because of the limited data available at that time. Most such studies compared effects at different physical sites, such as Interstate 64 in the western portion of Virginia and the adjacent section of Interstate 64 in the eastern portion of West Virginia, where, in 1990 the former had a uniform limit and the latter had a differential limit. With more than a decade having elapsed since the passage of STURAA, however, the Federal Highway Administration (FHWA) requested that a longitudinal study be conducted, focusing especially on States that had changed their limits from USL to DSL or vice versa.

PURPOSE AND SCOPE

The purpose of this study is to compare the safety impacts of DSL and USL on rural interstate highways. Thus, the study's objectives are twofold:

- To compare the effect of DSL and USL on crashes.
- To compare the effect of DSL and USL on vehicle speeds.

The scope of this study is limited to crash and speed data available in the U.S. on rural interstate highways between the period 1991and 2000.

LITERATURE REVIEW

One of FHWA's justifications for a longitudinal study was the lack of consistent findings from previous research. Previous authors had focused on three main indicators of safety: speed, speed variance, and the crashes themselves. Some studies found no difference between DSL and USL, and some studies found one or the other to be a better policy choice. Because it was thought that a new methodological approach might help resolve these inconsistencies, literature suggesting the empirical Bayes approach was also reviewed.

Impact of DSL on Mean Speed

In 1990, Freedman and Williams analyzed speed data collected at 54 sites in 11 Northeastern States to determine the effect of DSL on mean and 85th percentile speeds.(1) Six States had retained a uniform limit of 89 km/h (55 mi/h), three had raised speed limits for all vehicles to a uniform value of 105 km/h (65 mi/h), and two States employed a differential limit for cars and trucks of 105/89 km/h (65/55 mi/h), respectively. The results showed that for *passenger cars*, the mean speed and 85th percentile speed for the two DSL States were not significantly different from the States with a uniform limit of 105 km/h (65 mi/h). Further, the mean and 85th percentile *truck* speeds in DSL states were close to those of the USL States. Similar results were obtained when comparing the percentage of vehicles complying with the speed limit. Harkey and Mera also found there to be no significant difference between passenger car and truck mean speeds when comparing DSL and USL.(2) Garber and Gadiraju, however, did find a significant difference between truck mean speeds under DSL and USL, as well as an increase in passenger cars' mean speed when the speed limits for those vehicles were raised.(3)

Impact of DSL on Speed Variance

Garber and Gadiraju also found that speed variance for all types of vehicles were significantly greater at DSL sites than at non-DSL sites.[3] The implication of increased variance is that of increased interactions between vehicles and, thus, a potential in some types of crashes. While researchers found differences in truck speed variance in 10 of 13 comparisons between a USL site and DSL site, Harkey and Mera found no significant differences were between car speed variances at the DSL and USL sites.[2] Furthermore, they found no difference between the speed distributions for both cars and trucks for the 105/97 km/h (65/60 mi/h) and 105/105 km/h (65/65 mi/h) speed limits.

Impact of DSL on Crashes

Harkey and Mera also looked at crash results from 26 sites in 11 States, where the sites were grouped into pairs.[2] Each pair was comprised of a USL site with a speed of 105 km/h (65 mi/h) or 89 km/h (55 mi/h) and a DSL site with a speed of 105/89 km/h (65/55 mi/h) or 105/97 km/h (65/60 mi/h). The study investigated the percentage of different collision types for the total number of crashes for each of four types of speed limits. Three types of collisions were taken into consideration: rear end, sideswipe, and all other crashes. Table 1 shows that a higher proportion of car-into-truck and truck-into-car crashes occurred in USL States than in DSL States, with the exception of rear-end crashes where more car-into-truck collisions happened in the DSL group.

Table 1. Accident proportions by speed limit, collision type, and vehicle involvement.

Speed Limit	Rear-End		Sideswipe		Other	
	Car-into-Truck	Truck-into-Car	Car-into-Truck	Truck-into-Car	Car-into-Truck	Truck-into-Car
USL: 105 km/h (65 mi/h) and 89 km/h (55 mi/h)	10.91	10.78	22.12	21.07	2.57	2.01
DSL: 105/89 km/h (65/55 mi/h) and 105/97 km/h (65/60 mi/h)	13.70	6.86	21.52	14.96	2.07	0.99

In contrast, Garber and Gadiraju, who had compared sites in three DSL States (California, Michigan, and Virginia) against two USL States (Maryland and West Virginia) found no statistically significant differences between crash rates when stratifying by collision type and crash severity.[3] Council, Duncan, and Khattack in 1998 found that for rear-end collisions between cars and trucks, a high speed differential increases the severity of the crash.[4] A simulation study by Garber reported a potential for an increase in crash rates for facilities using DSL, especially in the case of high vehicle volumes and truck percentages.[5] Further, a 1991 study found no evidence indicating that the increase of the speed limit to 105 km/h (65 mi/h) for trucks at the test sites resulted in a significant increase in fatal, injury and overall accident rates.[6] In that study, comparisons of crash rates in the adjacent States of Virginia (DSL) and West Virginia (USL) showed relatively more rear-end crashes in Virginia, suggesting that DSL might have a negative impact on safety.

Lending credence to the use of speed variance as a surrogate for crashes, Garber and Gadiraju found that crash rates increased with increasing speed variance for all classes of roads.(7) A 1974 study by Hall and Dickinson showed that speed differences contributed to crashes, primarily rear-end and lane-change collisions.(8) The existence of a posted DSL, however, was not found to be related to the occurrence of truck crashes. The study also noted that lower rates of truck crashes could be expected with higher speed limits and hence the study recommended an increase of truck speed limits to 105 km/h (65 mi/h) for highways carrying a higher truck percentage. Finally, an evaluation conducted by the Idaho Department of Transportation found that a change from USL to DSL did not increase crashes.(9)

Modified Empirical Bayes Methodology

Because of the discrepancies in findings in the literature with respect to the safety impacts of DSL versus USL, the investigators considered a new conceptual approach that had been refined during the past decade. The empirical Bayes method, developed by Ezra Hauer and modified by the investigators because of data issues specific to this analysis, was applied because recent literature suggested it could delineate between random variation and variation that resulted from some policy change (such as DSL to USL) more clearly than is the case with conventional methods.(10) Specifically, four advantages of the empirical Bayes approach have been cited in the literature:

- *It employs the correct mathematical distribution for crashes*. Many conventional statistical tests are predicated on the assumption of normality; however, previous studies have indicated that the error or residuals (difference between predicted values and actual values) structure of the negative binomial distribution is a better description of the variation of crash frequency between sites. (See references 10, 11, 12, and 13.)

- *It does not assume that other conditions remain constant*. The estimated number of crashes at a particular site usually does not remain constant from one year to the next because of variation in traffic volumes, traffic flow characteristics, weather conditions, driver attitudes, and a host of other factors beyond the control of the researcher. While conventional methods strive to control for these factors by either judiciously selecting sites that are common in most characteristics except that being studied (such as USL versus DSL) or using a control group, the empirical Bayes technique provides for the use of trend data. It has been shown that this method can be used to evaluate safety impacts even when yearly data are not available. Although yearly trend information is lost, researchers still benefit from the use of reference groups to pinpoint which effects are significant.(14)

- *It does not assume a proportional relationship between crashes and average daily traffic (ADT)*. Instead, the number of crashes, or crash frequency, is used to reflect highway safety, given that empirical studies have suggested that a percentage change in ADT will not necessarily have the same percentage change in crashes, even if all other factors were constant.

- Crash estimation models are used to account for the fact that the observed crash frequency is just a point observation from some underlying distribution. In sum, the actual number of crashes observed may not be an unbiased estimator of the expected number of crashes. Thus, in lieu of this point estimate, a crash estimation model (CEM) is employed to statistically predict the best estimator of crash frequency. The generalized linear modeling (GLM) technique has been suggested as a way of determining parameters for the crash estimation model, although recently the generalized estimating equation (GEE) has been proposed as an alternative. (See references 10, 11, 14, 15, 16, and 17.)

METHODOLOGY

Four general steps comprised the methods used to compare the safety impacts of DSL and USL. First, crash and speed data were synthesized from 17 States that had been recommended by FHWA, were recommended by other researchers, or were thought to have changed their speed limits at least once during the 1990s from USL to DSL or vice versa. Second, conventional statistical approaches, such as the analysis of variance (ANOVA) were used to analyze speed monitoring data from these States. Third, comparable approaches were used to evaluate crash data from these States. Last, the empirical Bayes procedure was applied to these crash data.

Data Synthesis

As shown in appendix A, crash, speed, and volume data were solicited from multiple States by phone and e-mail. Raw data formats varied widely. Some data were only available in hardcopy format and were manually entered. Moreover, the detail of electronic data sets also varied widely. For example, some States provided detailed speed data in 8.0 km/h (5 mi/h) bins, whereas other States only provided a mean speed. During this data synthesis, data records that appeared likely to contain errors were removed. As an illustration, consider the 24-hour ADTs available from one State. The available data were the number of axles per vehicle, the vehicle speed, and the distance between the axles. There were a few records that showed both a speed of less than 8.0 km/h (5 mi/h), and a total axle-distance less than 1.2 meters (m) (4 feet); thus, these were removed from the data. In some cases, State data were not used because of concerns about the data quality. For example, after data had been partially synthesized for one State, practitioners from that State told investigators that the location system for the speed limits was so imprecise that the data were not reliable; thus, that State was omitted from the analysis. In other cases, local knowledge suggested that a facility should be excluded. For example, an experienced Virginia Transportation Research Council (VTRC) technician noted that although Interstate 66 had been designated as a “rural” section, the high volumes on that segment caused it to function as an urban section. In a few cases, the data were of good quality but the time required for reformatting the data for the purposes of this analysis was prohibitive. However, repeated telephone calls were helpful for clarifying the meaning of the data elements within the individual data sets.

Table 2 shows that the States that provided data used in this study may be divided into four *policy groups* based on their speed limits during the 1990s:

1. States that maintained a uniform limit for cars and trucks.
2. States that maintained a differential limit for cars and trucks.
3. States that changed from a uniform to a differential limit.
4. States that changed from a differential to a uniform limit.

Table 2. Overview of data availability for rural interstates from the various States.

Rural Interstate Speed Limits: 1991–2000		Crash Data	Speed Data
Policy Group 1: Maintained USL			
Arizona[a]	121 km/h (75 mi/h)	Y	N
Iowa	105 km/h (65 mi/h)	N	Y
North Carolina	105 km/h (65 mi/h) before 1996 113 km/h (70 mi/h) after 1996	Y	N
Policy Group 2: Maintained DSL (passenger cars/trucks)			
Illinois	113/105 km/h (70/65 mi/h)	N	Y
Indiana	105/97 km/h (65/60 mi/h)	N	Y
Washington[b]	105/97 km/h (65/60 mi/h)	Y	N
Policy Group 3: Changed from USL to DSL (passenger cars/trucks)			
Arkansas	From: 105 km/h (65 mi/h) To: 113/105 km/h (70/65 mi/h) 1996	Y	N
Idaho	From: 105 km/h (65 mi/h) To: 121 km/h (75 mi/h) 1996 To: 121/105 km/h (75/65 mi/h) 1998	Y	Y
Policy Group 4: Changed from DSL to USL (passenger cars/trucks			
Virginia	From: 105/89 km/h (65/55 mi/h) To: 105 km/h (65 mi/h), 1994	Y	Y

[a] Prior to December 1995, Arizona's uniform limit was 105 km/h (65 mi/h). The State raised the limit by route to 121 km/h (75 mi/h) between December 1995 and the summer of 1996.
[b] The Washington State limits shown refer to nine sections on I-90, which comprise the entire Washington data set used for this study. During inclement weather, speed limits on those sections drop to 40.2 km/h (25 mi/h).

As shown in table 3, the number of sites with speed data could vary from year to year.

Table 3. Available speed data.

Policy Group	State		Speed Data Availability				Number of Sites	Years of Data
		Mean Speed	Speed Variance	85% Speed	Median Speed	Non-compliance		
1	Iowa	x	N/A	N/A	N/A	N/A	1 to 27	1991–2000
2	Illinois	x	x	x	x	x	4	1993, 1994, 1997–1999
	Indiana	x	N/A	x	x	N/A	4, 3	1991, 2000
3	Idaho	x	N/A	x	N/A	N/A	24 to 38	1991–1999
4	Virginia	x	x	x	x	x	3 to 7	1991,1993,1995,2000, 2001

Note: "x" indicates that the correspondent data are available.

Table 4 shows the categories of crash data obtained from the different States used in this study.

Table 4. Available crash data for all sites.

Policy Group	State	Crash Data Availability						Number of Sites	Years of Data
		Crash Rate for all Vehicles			Crash Rates for Trucks				
		Total	Fatal	Rear-End	Total	Fatal	Rear-End		
1	North Carolina	x	x	x	x	x	x	26	1991–1995
									1997–2000
	Arizona	x	x	x	x	x	x	278	1991–2000
3	Idaho	x	x	x	x	x	x	29	1991–2000
	Arkansas	x	x	x	x	N/A	N/A	10	1991–1995
									1997–1999
4	Virginia	x	x	x	x	x	N/A	267	1991–1993
									1995–1999

Note: “x” indicates that the correspondent data are available.

Analysis of Speed Data

To the extent possible with available data, annual changes in five speed variables (mean speed, speed variance, 85th percentile speed, median speed, and noncompliance rates) were compared within individual States. The Analysis of Variance (ANOVA) was used to determine if the change between the USL and DSL periods was significant. For those States that never changed their policy, the data were categorized into two virtual groups, 1990–1995 and 1996–2000, to determine whether significant changes occurred over time even without a policy shift between USL and DSL.

ANOVA was also used to look at speed changes on a yearly basis within every State. When a significant difference at the 5 percent confidence level was detected for these yearly changes, then, as shown in figure 2, three additional statistical tests were employed. Levene’s test served as a screening procedure to determine if the groups had equal variances. If so, then Tukey’s test was used to determine whether the differences were significant, while Dunnett’s test was used for samples with unequal variances, since the former assumes equal variances and the latter does not.[18,19,20] The Levene, Tukey, and Dunnett tests were performed with the SPSS software following ANOVA test; hence, they are referred to as the “post hoc analysis” in figure 2.

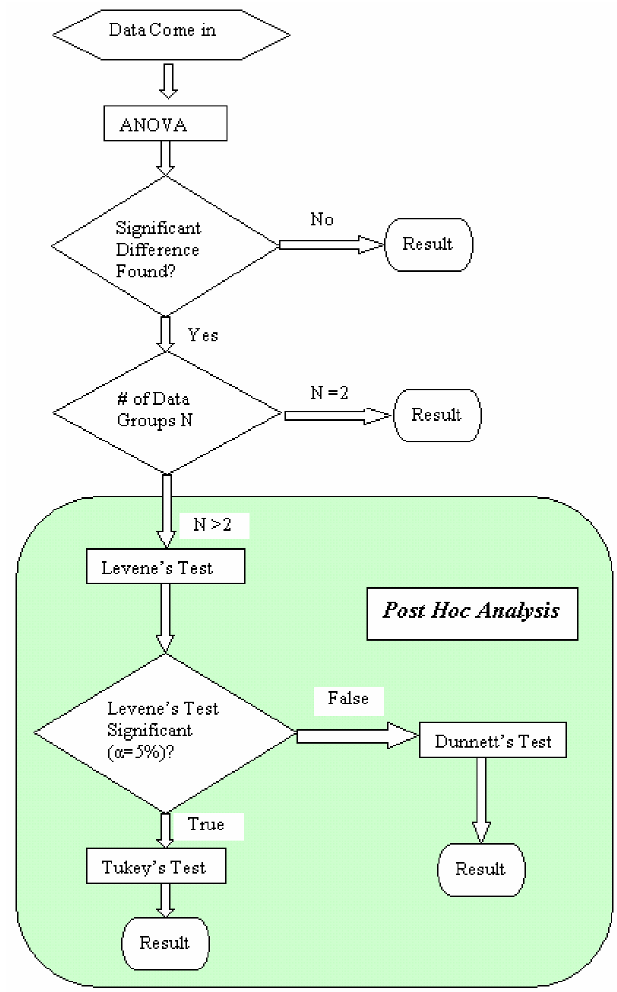


Figure 2. Chart. Data analysis process flowchart.

Analysis of Crash Data Using Conventional Statistical Approaches

The same procedure employed for evaluating the speed data was employed for evaluating the crash data, using the ANOVA, Levene, Tukey, and Dunnett tests. Six types of crash rates were studied: total, fatal only, rear-end only, total truck-involved, fatal truck involved, and truck-involved rear-end. The crash rate was computed as shown in figure 3, where the annual crash frequency was simply the annual number of crashes; ADT was the average daily traffic.(21)

$$Crash\ rate = \frac{(100{,}000{,}000)(Annual\ Crash\ Frequency)}{(365)(ADT)(Section\ Length)}$$

Figure 3. Equation. Crash rate.

Analysis of Crash Data Using the Empirical Bayes Technique

A fundamental reason for applying the empirical Bayes technique is that a traditional before/after test, which simply compares the number of crashes on a facility before and after some type of treatment, may not necessarily have been the result of the treatment. Figure 3 illustrates how the use of a reference population can assist in making this determination, provided the reference group is judiciously selected.[10] To understand the impact of assumptions, it is helpful to trace how the available crash data can be mapped to figure 4.

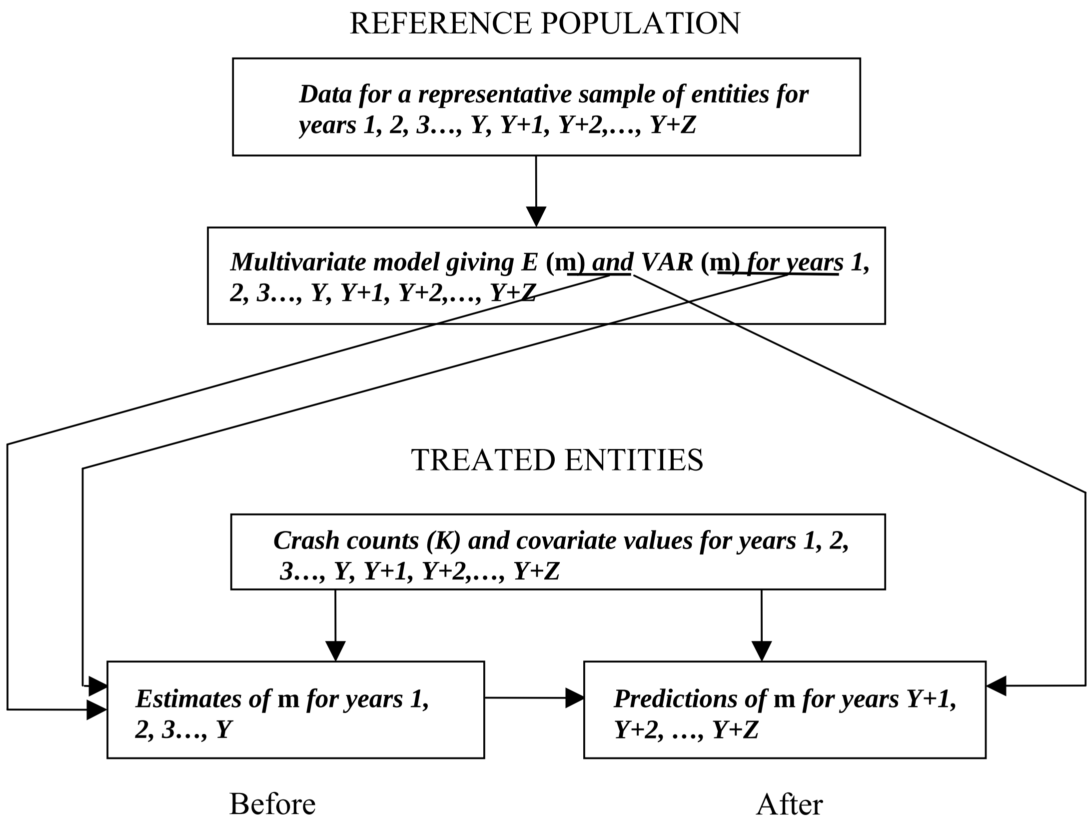


Figure 4. Chart. Fundamental steps of the empirical Bayes approach.

Conceptual Overview

Consider a State such as Arkansas that changed from a uniform limit to a differential limit in the mid 1990s. If the shift to DSL is viewed as a treatment, then figure 4 shows that years 1 to Y refer to the before-treatment portion and years Y+1 to Y+Z refer to the after-treatment portion, such that the before period has a duration of Y years and the after period has a duration of Z years. For each roadway segment within the State, there is sequence of crash counts K for year 1 to year Y; thus, the number of crashes at a particular site i during a year y may be designated as $K_{i,y}$. To evaluate how this treatment affected safety, there must be a prediction as to what the expected crash frequency m would have been in the after period had there had been no such treatment and then compare this would-have-been value to the actual number of crashes that occurred during the after period.

To determine this would-have-been value, a single multivariate crash estimation model (CEM) was developed to estimate the mean $E(m_{i,y})$ and the variance $VAR(m_{i,y})$ of the expected crash frequency m for each year y and at each site i during the before and after periods. For the treated entity with covariate values available, the multivariate model was applied to calculate the $E(m_{i,y})$ and $VAR(m_{i,y})$ for the before and after years. The expected crash frequency m was then calculated from the $E(m_{i,y})$ from the before year values. Finally, these frequencies (m) of the before years then serve as a basis to obtain the predictions of the frequencies (m) of the after years of treated entities, with the use of the multivariate model. These steps are illustrated in the subsections that follow.

Development of the Crash Estimation Model

The CEM predicts the mean of the expected frequency of crashes $E(m_{i,y})$ and is especially relevant for predicting would-have-been crashes. The ideal CEM will account for all sources of variation other than the treatment being studied, which in this case are the DSL and USL policies. Thus, the ideal CEM would account for effects such as *operational changes* (e.g., volume growth, enforcement modifications, and the installation of safety service patrols), *geometric changes* (e.g., work zones, median barriers, or new interchange construction), and *driver changes* pertaining to behavior, licensing, and vehicle maintenance. Unfortunately, the data for these other sources of variation either were not available, or at best were very limited. The research team, therefore, had to develop the crash estimation models based on the available data. For example, in Virginia, the before data were from 1991–1994 and the after data were from 1995–1999. Unfortunately, there is no perfect technique for selecting the data from which to build the crash estimation model for the after period. Given this, there are two options for acquiring these data:

- *Use data from the same time period but from a different State*. In Virginia's case, the researchers could select crash data for the period from 1995 to 1999 from a different State with comparable geometric and volume characteristics. The advantage is that if there is some temporal trend that applies nationwide, the reference group will capture that trend. The disadvantage is that the chosen State would need characteristics comparable to those of Virginia.

- *Use data from the before period but for the same State*. The advantages and disadvantages are the reverse of the previous option. In this case, the researchers are guaranteed of having the right geometric characteristics, but may miss temporal trends that are present in the after years, but not present in the before years.

The investigators chose the latter course of action, deeming the disparity between States as being greater than the disparity between time periods and using as an example a comparison of two crash models from the before periods for Virginia and Washington State from the same time period of 1991–1993. Both States maintained a differential speed limit at the time.

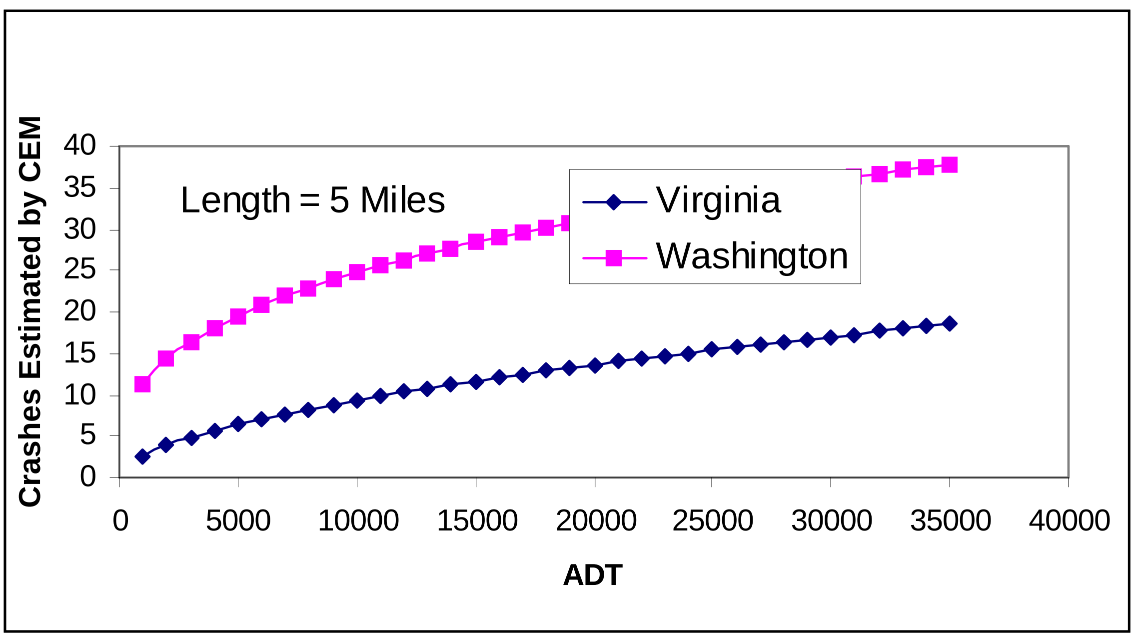
$$E(m) = 0.022(Length)^{0.622}\,(ADT)^{0.548}$$

Figure 5. Equation. Crash models for Virginia.

$$E(m) = 0.531(Length)^{0.440}\,(ADT)^{0.340}$$

Figure 6. Equation. Crash models for Washington.

Visual inspection of the plots of the crash estimation models, shown in figure 7 below for a fixed length of 8 km (5 mi) and a range of ADTs, confirms that a different number of crashes at a given site with a given volume could be predicted. For example, if an 8-km (5-mi) site registered an ADT of 10,000, then the CEM for Virginia would have predicted approximately 9 crashes, whereas the CEM for Washington would have predicted almost 25 crashes.



5 mi = 8 km

Figure 7. Chart. Comparison of crash estimation models for Virginia and Washington State based on 1991–1993 data.

Generally, the variation between States in terms of the crash estimation models was found to be relatively large, even among States with similar speed limit policies. Thus, for this specific application, it appeared that capture of nontreatment sources of variation was best accomplished by using CEMs developed within the same State, which is a significant departure from the empirical Bayes formulation as given in the literature. For that reason, the nomenclature *modified empirical Bayes* will be used for the remainder of this report to acknowledge that the selection of comparison sites herein deviates from that original methodology.

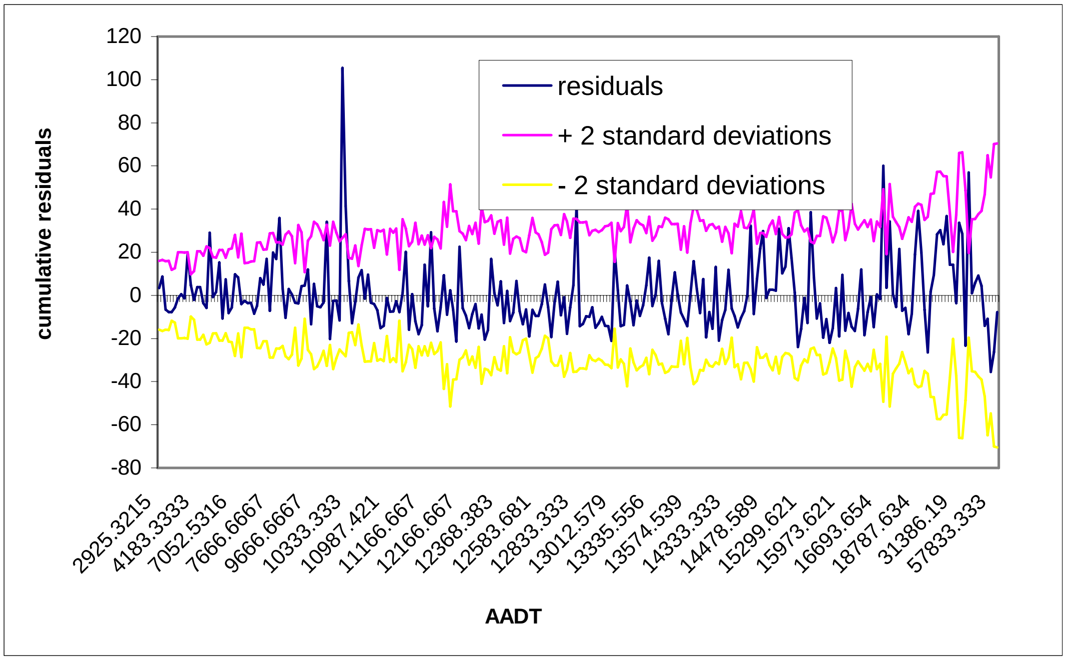
The crash estimation model took the form shown in figure 8, where the expected mean value of crashes for a roadway segment during a given year was the function of the length and ADT. As recommended in the literature, the maximum likelihood technique was used to estimate the parameters since the crash distribution was thought to follow a negative binomial distribution.(17) Note parameters β_1 and β_2 do not imply a proportionate effect of ADT and length unless equal to unity, since others have suggested that such an effect cannot be assumed.(22,23)

$$E(m) = \alpha(Length)^{\beta_1}(ADT)^{\beta_2}$$

Figure 8. Equation. Expected mean value of crashes.

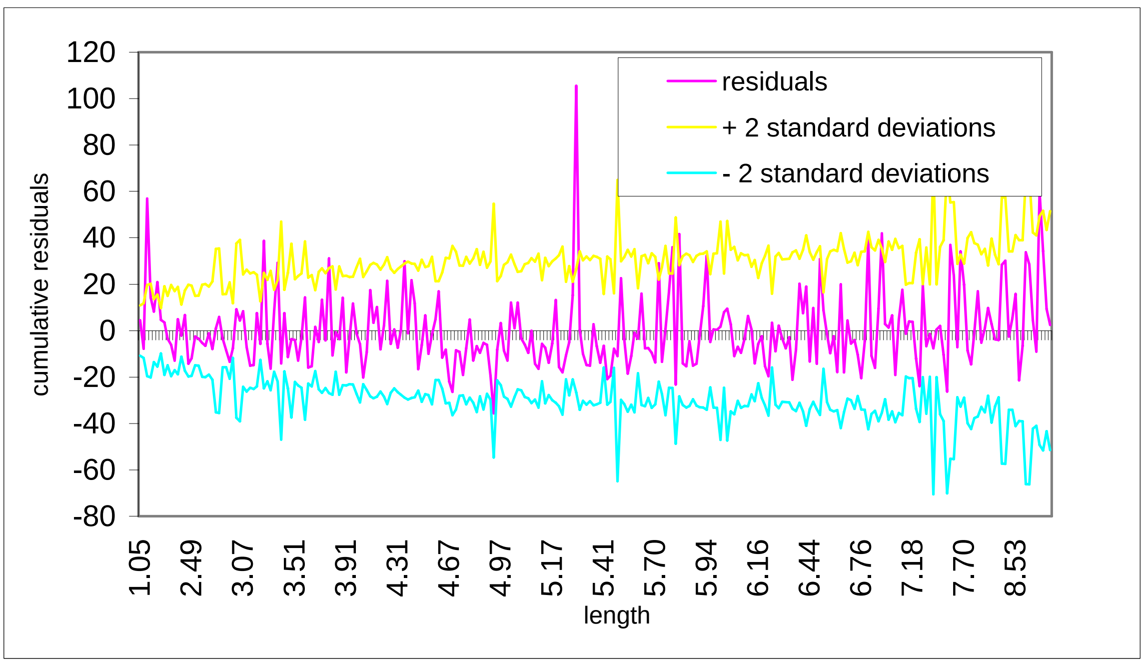
Estimation of the parameters was done with the GENSTAT software package (release 4.2.1). The procedure also gives the variance, VAR($m_{i,y}$) of expected crash frequency for each section, which enables researchers to determine an aggregation parameter, shown as *k*, which describes the underlying crash distribution as *k* approaches infinity, when the distribution moves from negative binomial to Poisson. Theoretical explanations of *k* and techniques for estimating *k* are given in the literature. (See references 12, 13, 17, and 24.) Software for estimating *k* used for this effort was developed by Persaud and Lord.(17)

It should be noted that the equation for this effort shown in figure 8 was chosen after investigators considered a variety of formulations and determined that this formulation could be applied most consistently for each State's data set. To ascertain whether the model was appropriate, the method of cumulative residuals may be used.(17) (The cumulative residual is the difference between actual crash counts and model estimated crash counts. If the cumulative residuals oscillate around zero within the range of the two plots of two standard deviations, a good quality of fit is reflected.)(17,25) Figures 9 and 10, for example, show the cumulative residual plots with respect to section length and annual ADT (AADT) for Virginia. Although there existed several sites where the cumulative residuals exceed the range of two standard deviations, the overall figures show a good quality of fit.



Note: top line is +2 standard deviations, middle line is residuals, bottom line is −2 deviations.

Figure 9. Chart. Plot of goodness of fit for the crash estimation model versus ADT.



Note: top line is +2 standard deviations, middle line is residuals, bottom line is −2 deviations.

Figure 10. Chart. Plot of goodness of fit for the crash estimation model versus length.

An alternative crash estimation model that was explored in the course of this work entailed the use of yearly trend analysis and is given in figure 10, where a_y indicates the yearly trend. This expression was ultimately not used because reliable estimates could not be obtained for the after years for the States that changed from USL to DSL or from DSL to USL. That is, for a State such as Virginia, which changed from DSL to USL in 1994, calibration of this model would indeed yield a coefficients for the before period (a_{1991}, a_{1992}, a_{1993}, and a_{1994}); however, data were not available to estimate the appropriate coefficients for the after years, e.g., a_{1997}, a_{1998}, and a_{1999}. In short, the formulation shown in figure 8 proved advantageous over the formulation below because it required less data.

$$\mathrm{E}(m) = \alpha_y (Length)^{\beta_1} (ADT)^{\beta_2}$$

Figure 11. Equation. Alternative crash estimation model.

In terms of specific software, general estimating equation (GEE) and generalized linear modeling (GLM) can accommodate both expressions, but the investigators used GEE for all States because it considers the possibility of temporal correlation between the years of the analysis.

In sum, several considerations governed the crash estimation model formulation used in this study. First, using total crashes for Virginia as a case study, four different formulations were considered (the equation in figure 8 using GEE and figure 8 using GLM, figure 11 using GEE and using GLM), and it was found with the Virginia case that the equation in figure 7 using the GEE approach was most suitable. As shown in tables 5 and 6, the parameters for these formulations were quite similar, but GEE was chosen over GLM because it accounts for temporal correlation; and the equation in figure 8 was chosen over the one in figure 11 because it meshed better with the data available for the study. Second, the method of cumulative residuals showed that, for total crashes in Virginia, both variables were indeed appropriate. The Genstat software used to implement the GEE method also showed that the *t*-statistics for length and ADT exceeded the critical values; thus both variables are significant. Third, the equation in figure 8 using the GEE software was applied for the remaining States and crash types.

Tables 5 and 6 illustrate the parameters for the equations in figures 8 and 11 with GLM and GEE respectively. A fifth formulation based on 3-year averages was also considered in the course of this study as shown in the rightmost columns of tables 5 and 6; however, this model ultimately was not used because ADT variation by year is not included therein.

Table 5. Five potential models for total number of crashes for Virginia rural interstate highways.

Number of Sites: 267										
Years of Data: 1991, 1992, 1993										
Parameters	**Figure 10**		**Figure 10**		**Figure 7**		**Figure 7**		**Not Shown in the Report**	
	GEE with trend		**GLM with trend**		**GEE without trend**		**GLM without trend**		**GLM without trend**	
	Yearly data		Yearly data		Yearly data		Yearly data		3-year averaged data	
	$E(m)= \alpha_y (Length)^{\beta_1} (ADT)^{\beta_2}$		$E(m)= \alpha_y (Length)^{\beta_1} (ADT)^{\beta_2}$		$E(m)= \alpha (Length)^{\beta_1} (ADT)^{\beta_2}$		$E(m)= \alpha (Length)^{\beta_1} (ADT)^{\beta_2}$		$ave\_E(m)= \alpha (Length)^{\beta_1} (ave\_ADT)^{\beta_2}$	
	Estimate	Standard error	Estimate	Standard error	Estimate	Standard error	Estimate	Standard error	Estimate	Standard error
$LN(\alpha_1)$	−3.774	0.5271	−3.774	0.41	–	–	–	–	–	–
$LN(\alpha_2)$	−3.848	0.5277	−3.848	0.415	–	–	–	–	–	–
$LN(\alpha_3)$	−3.732	0.528	−3.732	0.416	–	–	–	–	–	–
$LN(\alpha)$	−3.78467	0.5276	−3.78467	0.413667	−3.828	0.5274	−3.829	0.41	−3.791	0.592
β_1	0.631	0.1167	0.6309	0.0526	0.632	0.1163	0.6323	0.0528	0.6117	0.0737
β_2	0.545	0.0588	0.5447	0.042	0.549	0.0585	0.5492	0.0418	0.5481	0.0607
K	5.62		5.62		5.61		5.61		5.56	

Table 6. Five models for total number of crashes on Arizona rural interstates.

Number of Sites: 556										
Years of Data: 1991–2000										
Para-meters	**Figure 11**		**Figure 11**		**Figure 8**		**Figure 8**		**Not Shown in the Report**	
	GEE with trend		GLM with trend		GEE without trend		GLM without trend		GLM without trend	
	yearly data (5560pts)		yearly data (5560pts)		yearly data (5560pts)		yearly data (5560pts)		10-year data (556pts)	
	E(m)= α_y $(\text{Length})^{\beta_1}$ $(\text{ADT})^{\beta_2}$		E(m)= α_y $(\text{Length})^{\beta_1}$ $(\text{ADT})^{\beta_2}$		E(m)= α $(\text{Length})^{\beta_1}$ $(\text{ADT})^{\beta_2}$		E(m)= α $(\text{Length})^{\beta_1}$ $(\text{ADT})^{\beta_2}$		E(m)= α $(\text{Length})^{\beta_1}$ $(\text{ADT})^{\beta_2}$	
	Estimate	Standard error	Estimate	Standard error	Estimate	Standard error	Estimate	Standard error	Estimate	Standard error
LN(α_1)	−1.963	0.6397	−1.963	0.238	–	–	–	–	–	–
LN(α_2)	−1.933	0.6373	−1.934	0.238	–	–	–	–	–	–
LN(α_3)	−1.908	0.6381	−1.908	0.238	–	–	–	–	–	–
LN(α_4)	−1.819	0.6381	−1.819	0.24	–	–	–	–	–	–
LN(α_5)	−1.853	0.6434	−1.853	0.241	–	–	–	–	–	–
LN(α_6)	−1.808	0.6511	−1.809	0.243	–	–	–	–	–	–
LN(α_7)	−1.687	0.6506	−1.687	0.243	–	–	–	–	–	–
LN(α_8)	−1.621	0.6493	−1.621	0.244	–	–	–	–	–	–
LN(α_9)	−1.649	0.6523	−1.649	0.246	–	–	–	–	–	–
LN(α_{10})	−1.657	0.6572	−1.657	0.249	–	–	–	–	–	–
LN(α)	−1.7898	0.64571	−1.79	0.242	−2.328	0.6196	−2.33	0.233	−1.68	0.831
β_1	1.114	0.0609	1.1144	0.0242	1.133	0.0606	1.1325	0.0242	1.151	0.0826
β_2	0.24	0.0668	0.2403	0.024	0.297	0.0639	0.2974	0.0234	0.2242	0.0842
k	0.73		0.73		0.73		0.73		0.73	

The equation shaded in tables 5 and 6 was ultimately used in this study. The standard error are lower than those shown for GLM because GEE accounts for the possibility of correlation between years.

Critical Assumptions of the Crash Estimation Model

It is important to recognize two critical characteristics of the crash frequency distribution that affect the form of the crash estimation model.

- For any particular site *i*, such as an 8-km (5-mi) segment of rural interstate highway, the annual number of crashes for year *y* will follow the Poisson rather than the normal distribution. Thus, in the equations that follow, the crash frequency variable $K_{i,y}$ obeys the Poisson distribution.

- For any particular year y, such as 1995, the crash frequency variable $K_{i,y}$ for sites i follows the negative binomial distribution rather than the Poisson distribution.

Although recent studies have indicated that the negative binomial distribution is a better descriptor for crash frequencies between sites, the investigators confirmed this by comparing the actual crash frequencies for Arizona, Idaho, North Carolina, and Virginia with theoretical frequencies using the Poisson and negative binomial distributions. Appendix C shows that the negative binomial distribution is a valid descriptor of these data sets.

Application of the Crash Estimation Model with the Before Data

After CEM was developed, the expected crash frequency $m_{1,}$ $m_{2,...}$ m_y for the treated segments for the after years were determined. To accomplish this, the following steps were undertaken in sequence:

- CEM was applied to the before years data to obtain $E(m_{i,y})$, as shown in figure 8. (Recall that $E(m_{i,y})$ is the mean of the estimated crash frequency of site i during year y.)

- The ratio $C_{i,y}$ was then computed for each of these before years from $y = 1, 2, \ldots .Y$. Thus $C_{i,y}$ is the ratio of the current $E(m_{i,y})$ to the first year $E(m_{i,1})$.

$$C_{i,y} = \frac{E(m_{i,y})}{E(m_{i,1})}$$

Figure 12. Equation. CEM for before years.

Note that in the application of figure 12, the literature points out it is not essential that the first year of the before period be the denominator.(10) Appendix D confirms this view by illustrating that the use of another year rather than the first year for Virginia would not affect the results.

- Recall that k was determined from the calibration process for the crash estimation model and essentially reflects the type of crash distribution reflected in the model. (On the other hand, recall that $K_{i,y}$ represents the actual crashes observed at site i in year y.) The expected crash frequency $m_{i,y}$ and its variance $VAR(m_{i,y})$ for years of $y = 1, 2, \ldots .Y$ are then calculated from the next four equations.(10)

$$m_{i,1} = \frac{k + \sum_{y=1}^{Y} K_{i,y}}{\frac{k}{E(m_{i,1})} + \sum_{y=1}^{Y} C_{i,y}}$$

Figure 13. Equation. Expected crash frequency m for period 1.

$$VAR(m_{i,1}) = \frac{k + \sum_{y=1}^{Y} K_{i,y}}{(\frac{k}{E(m_{i,1})} + \sum_{y=1}^{Y} C_{i,y})^2} = \frac{m_{i,1}}{\frac{k}{E(m_{i,1})} + \sum_{y=1}^{Y} C_{i,y}}$$

Figure 14. Equation. Variance of expected crash frequency m for period 1.

$$m_{i,y} = C_{i,y}\ m_{i,1}$$

Figure 15. Equation. Expected crash frequency m for period y.

$$\text{VAR}(m_{i,y}) = (C_{i,y})^2\text{VAR}(m_{i,1})$$

Figure 16. Equation. Variance of expected crash frequency m for period y.

Application of the Crash Estimation Model with the After Data

With the expected crash frequency $m_1, m_2, \ldots m_y$ estimated for before period for each treated entity from figure 15, investigators can compute the "would have been crash frequency" $m_{Y+1}, m_{Y+2, \ldots} m_{Y+Z}$ for the after period as if there had been no such treatment using the following sequential steps.

- The crash estimation model from figure 8 is applied to the after years data in order to obtain $\text{E}(m_{i,y})$ for from $y = Y + 1$ to $y = Y + Z$.
- The ratios $C_{i,y}$ are be computed using the equation in figure 12 for the years from $y = Y + 1$ to $y = Y + Z$.
- The values for $m_{i,y}$ and $\text{VAR}(m_{i,y})$ are computed using equations 6 and 7 for the years from $y = Y + 1$ to $y = Y + Z$. These $m_{i,y}$ are the crashes that would have occurred had no treatment been made.

Thus, this process is very similar to application of the crash estimation model for the before years, with the exception of figures 13 and 14, used to compute $m_{i,1}$ and its variance $\text{VAR}(m_{i,1})$ do not need to be repeated since the quantities $m_{i,1}$ and $\text{VAR}(m_{i,1})$ have already been determined. (That is, $m_{i,1}$ and $\text{VAR}(m_{i,1})$ refer to year 1 as designated in the subscript $m_{i,1}$.)

Quantifying the Safety Impact of the Speed Limit Change

The effect of the treatment, which in this case is a change in the speed limit for Arkansas from uniform to differential, is quantified by comparing the would-have-been crashes (shown above as $m_{i,y}$ for y ranging from $Y + 1$ to $Y + Z$) to the actual crashes in the after period (shown above as $K_{i,y}$ with y ranging from $Y + 1$ to $Y + Z$).* The would-have-been crashes at each site i during the after period are denoted as π_i while the actual crashes at each site during the after period are denoted as λ_i. Figures 17 and 18 are used to sum the crashes from the individual sites as π and λ.

$$\pi = \sum_{\mathrm{i}} \pi_{\mathrm{i}}$$

Figure 17. Equation. Would-have-been crashes, had there been no speed limit change.

$$\lambda = \sum_{i} \lambda_{\mathrm{i}}$$

Figure 18. Equation. Actual crashes, given that the speed limit did change.

Then, the literature suggests two alternative formulations for assessing the safety impact, based on a comparison of π and λ.[10]

- *Reduction in the expected number of crashes*. Figure 19 computes δ as the difference between would-have-been and actual crashes, such that a positive value of δ indicates that the treatment had a desirable effect of reducing the number of crashes. Figures 20 and 21 show the computation of the variance and the resultant confidence intervals for δ.

$$\delta = \pi - \lambda$$

Figure 19. Equation. The difference between would-have-been and actual crashes.

$$\mathrm{Var}(\delta) = \mathrm{Var}(\pi) + \mathrm{Var}(\lambda) = \Sigma \mathrm{Var}(\pi_i) + \Sigma \mathrm{Var}(\lambda_i)$$

Figure 20. Equation. Variance for δ.

* The investigators acknowledge that, in theory, the $K_{i,y}$ are themselves random observations that could be replaced by a crash estimation model that smoothes the annual data. The investigators felt that using CEM would add additional confusion when interpreting the results; thus the actual $K_{i,y}$ were used as indicators of the actual crashes in the after period.

$$\text{empirical confidence bounds for } \delta = \delta \pm 2[\mathrm{Var}(\delta)]^{0.5}$$

Figure 21. Equation. Confidence intervals for δ.

- *Ratio of actual to would-have-been crashes.* Figure 22 computes this ratio, also known as the index of effectiveness (θ), such that a value of θ less than 1.0 indicates that the speed limit change improved safety. The unbiased estimator, however, is not the equation in figure 22 but rather the one in figure 23 as shown below.(10) Figures 24 and 25 show the variance and confidence bounds, respectively.

$$\theta = \lambda/\pi \text{(“actual crashes” divided by “would have been” crashes)}$$

Figure 22. Equation. Reduction in the expected number of crashes.

$$\theta = (\lambda/\pi)/\{1+\mathrm{Var}(\pi)/\pi^2\}$$

Figure 23. Equation. Ratio of actual to would-have-been crashes.

$$\mathrm{Var}(\theta)=\theta^2\{[\mathrm{var}(\lambda)/\lambda^2]+[\mathrm{var}(\pi)/\pi^2]\}/[1+\mathrm{var}(\pi)/\pi^2]^2$$

Figure 24. Equation. Variance of ratio of actual to would-have-been crashes.

$$\theta \text{ is } \theta \pm [2\mathrm{Var}(\theta)]^{0.5}$$

Figure 25. Equation. Confidence intervals for θ.

The confidence bounds, shown in figures 21 and 25, are used to determine whether the values for δ and θ show a statistically significant safety impact. If the confidence bounds for δ and θ contain 0 and 1 respectively, then the safety impact computed by figures 20 and 24 are not significant; thus, it cannot be said that the treatment had a measurable effect.

RESULTS, DISCUSSION, AND LIMITATIONS

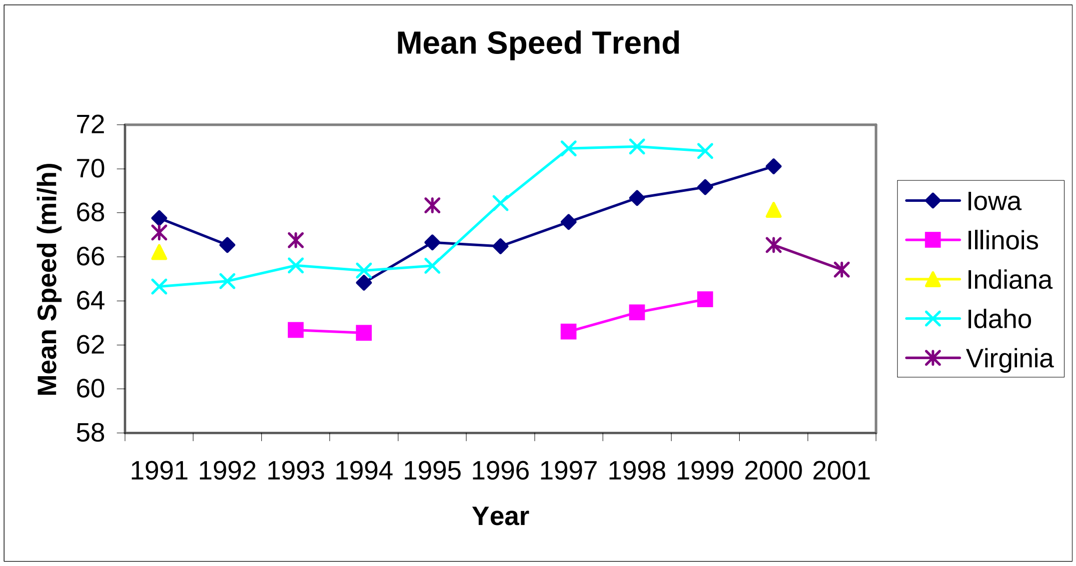
The results of the analysis are presented across three key areas: vehicle speeds as collected from speed monitoring data and evaluated using conventional statistical approaches, crash rates that were evaluated using conventional statistical approaches, and crashes as evaluated with the empirical Bayes technique.

Vehicle Speeds

The five types of speed data (mean speeds, speed variance, 85th percentile speeds, median speeds, and noncompliance rates) were analyzed for all States where such speed monitoring data were readily available. Furthermore, because Idaho provided a large number of speed monitoring sites and was a State that had changed its type of speed limit (from USL to DSL), speed data from that State were studied in detail.

Mean Speeds: An Example of How the Data May Be Assessed

Figure 26 illustrates the trends in mean speeds among five States from which speed data were analyzed. Two observations that arise from examination of figure 26 are, that with the exception of Virginia, speeds appear to be increasing over time (whether or not the differences are practically or statistically significant) and, unfortunately, data cannot always be obtained for all time periods.



1 mi/h = 1.6 km/h

Figure 26. Chart. Mean speed for all vehicles.

Tables 7 and 8 illustrate the two types of analyses that were conducted for all speed and crash data: a before-after analysis to determine whether the speeds from the before period were significantly different from the after period at the 95 percent confidence level, and a year-by-year analysis, to determine whether individual years showed a significant difference. For this report, *p* values of 0.05 or lower were considered significant and are designated by an asterisk (*).

Table 7. Before/after mean speed comparisons from the ANOVA test.

Policy Group	State	Before-After *p*
Group 1 (uniform limit)	Iowa	0.000*(+)
Group 2 (differential limit)	Illinois	0.626(+)
	Indiana	0.537(+)
Group 3 (uniform to differential)	Idaho [a]	0.000* (+) (uniform to uniform) 0.790(−) (uniform to differential)
Group 4 (differential to uniform)	Virginia	0.318(−)

*Designates a significant difference at the 0.05 level.
[a] Throughout this report, when before/after comparisons were done for the State of Idaho, the *first* statistic reflects a before group with a uniform speed limit of 105 km/h (65 mi/h) for all vehicles and an after group with a uniform limit of 121 km/h (75 mi/h) for all vehicles. The *second* statistic reflects a before group with the same uniform limit of 121 km/h (75 mi/h) and an after group with a differential speed limit of 121/105 km/h (75/65 mi/h) for cars and trucks, respectively.

Table 7 shows two sets of results for Idaho since its speed limits were changed twice: first, a raising of its uniform limits and second, a lowering of truck speeds only. In table 8, the Levene test showed the variances to be significantly different; Dunnett's test was used to compare annual mean speeds because it does not require comparison groups to have similar variances.

Table 8. Annual mean speed comparisons.

State	*p* values for tests of significant differences in the variances (based on Levene's test)	Years where the means are significantly different at the 0.05 level (based on Dunnett's Test)
Iowa	.000*	1995<2000, 1996<2000, 1997<2000
Idaho	.000*	1991<1996, 1992<1996, 1993<1996, 1994<1996, 1995<1996, 1996<1997, 1996<1998, 1996<1999
Virginia	.004*	1995>2001

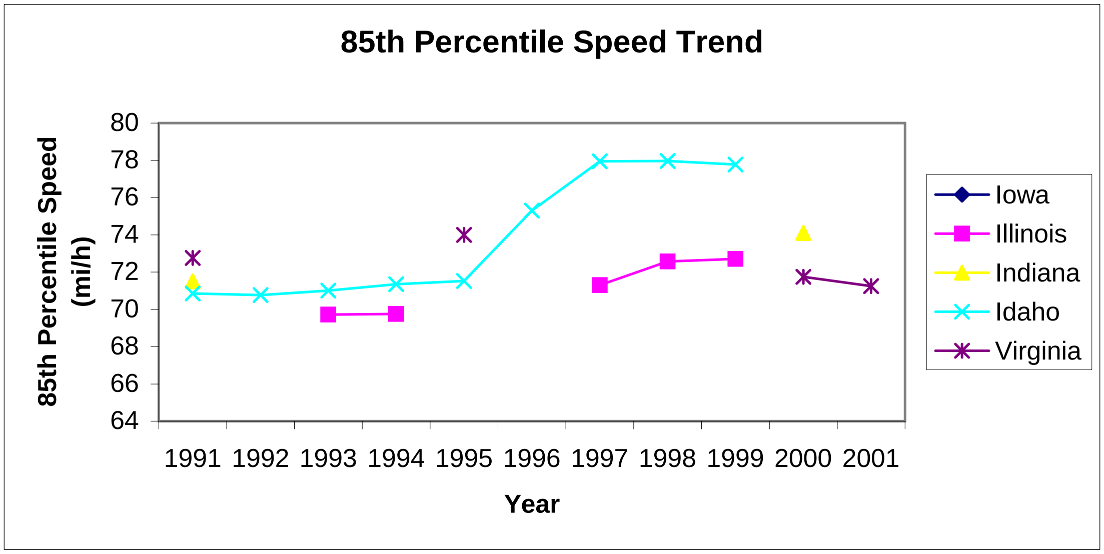
*Denotes significance at the .05 confidence level.

Examination of the statistics in table 7 shows that the mean speed of Iowa, a State that maintained a uniform limit, increased by 3.6 km/h (2.2 mi/h), which was significant. The mean speed change for Idaho, which maintained a uniform speed limit but increased that limit by 16 km/h (10mi/h) for all vehicles, was also significant with an increase of 9.2 km/h (5.7 mi/h). In Illinois and Indiana, which maintained DSL, their mean speeds increased by 1.2 km/h (0.8 mi/h) and 3 km/h (1.9 mi/h), respectively, which were not significant. Also, Idaho's second change, which was a shift from USL to DSL, resulted in a slight decrease in the mean speed of 0.18 km/h (0.11 mi/h), but this decrease was not significant. The mean speed in Virginia, which changed from DSL to USL by increasing the speed limit for trucks by 16 km/h (10 mi/h), decreased by 1.15 km/h (0.71 mi/h), which was insignificant. In examining the results of the year-to-year analysis shown in table 8, the mean speed in Iowa for 1999, after the change from USL to DSL, was significantly higher than that for 1996 before the change, and that for 2001 was significantly lower than for 1995.

Appendix E discusses, in detail, whether the sample size used in testing for statistical significance should be the number of speed monitoring sites or the number of vehicles. As explained in that appendix, the investigators chose to use the number of sites for speed monitoring, although this decision had a pragmatic rather than a theoretical justification.

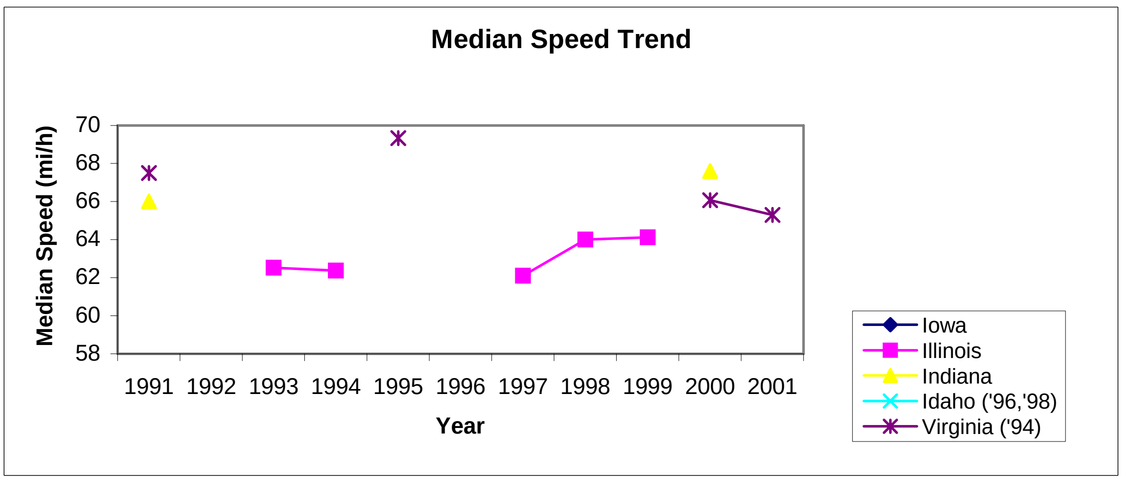
Graphical Overview of Changes in Speed Variance, 85th Percentile Speed, Median Speed, and Noncompliance Rates

Figures 26–29 give a pictorial understanding of how speeds changed from 1991 to 2000 for the States where data were available. These figures facilitated the observations of three general phenomena. First, there appear to be some correlations among the speed variables where data are available. For example, the mean and median speeds for Virginia are similar to one another, and the same can be said for Indiana. Likewise, the mean speeds and 85th percentile speeds for Idaho show similar trends. Although this is not surprising, it should not be taken for granted, since differences can sometimes be observed in the 85th percentile speed even though no differences are observed in mean speeds.[26] Second, there is not a clear difference in behavior between uniform and differential States. For example, Iowa (always a uniform limit) and Illinois (always a differential limit) show similar peaks in 1999, followed by a decreasing trend with respect to speed variance. Similarly, most States tended to show an overall increase in speeds. Virginia (the one State that went from differential to uniform) did show a decrease in mean speeds. However, table 6 shows that this difference was not significant. (Furthermore, table 8 shows that decreases in Virginia for the 85th percentile and median speeds were also not significant). Last, figure 29 suggests, but does not prove, that there may be a correlation between speed variance and noncompliance rates. The small amount of data available do not justify firm conclusions, but do suggest a relationship worthy of further study.



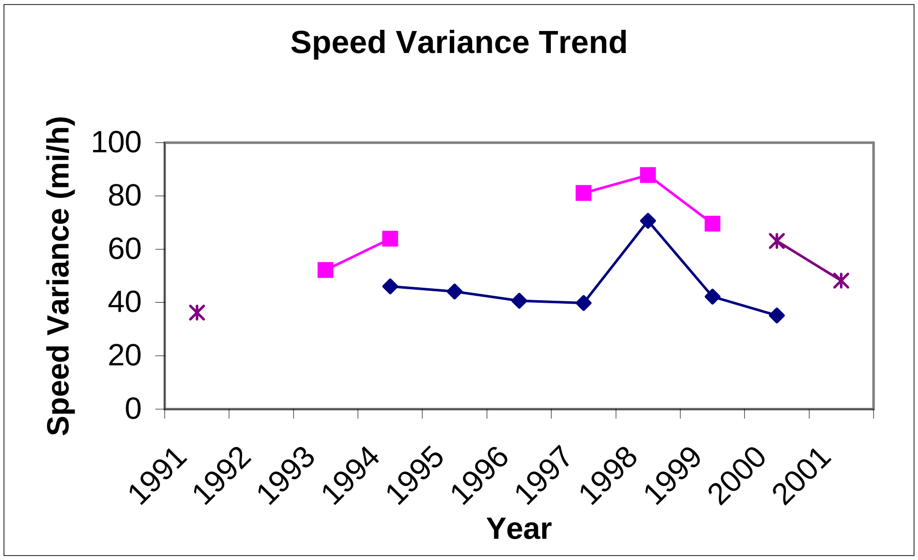
1 mi/h = 1.6 km/h

Figure 27. Chart. 85th Percentile speeds and median speeds.



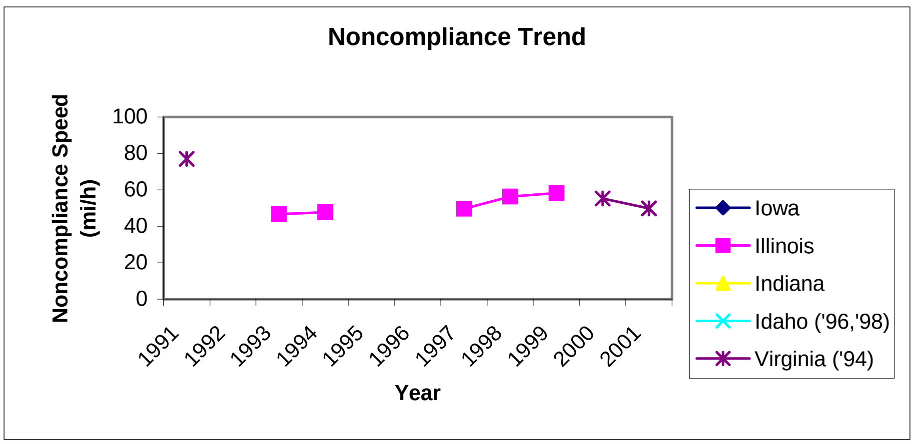
1 mi/h = 1.6 km/h

Figure 28. Chart. Median speed trends.



1 mi/h = 1.6 km/h

Figure 29. Chart. Speed variance rates.



1 mi/h = 1.6 km/h

Figure 30. Chart. Noncompliance rates.

Statistical Results of Changes in Speed Variance, 85th Percentile Speed, Median Speed, and Noncompliance Rates

Table 9 highlights the results of the statistical analysis and suggests that while increasing speeds were observed in most cases, these were not usually significant. (Idaho and Iowa are exceptions, which definitely do show a significant increase in speeds; it should be noted, however, that Iowa had a small number of sites.) The fact that no State showed a significant decrease in speeds and that States from all four policy groups showed either a significant or insignificant increase lends weight to the idea that changes in speed as a result of a differential or uniform speed limit were not supported by this study.

Table 9. Longitudinal comparison of speed variables within the States.

Policy Group	State	Variable	*p* value	Effect from Before to After Period	Significant at the 5% confidence level
1: USL	Iowa	Mean Speed	0.000	Increase	Y
		Speed Variance	0.878	Increase	N
		85th-Percentile Speed			
		Median Speed			
		Noncompliance			
2: DSL	Illinois	Mean Speed	0.626	Increase	N
		Speed Variance	0.250	Increase	N
		85th-Percentile Speed	0.171	Increase	N
		Median Speed	0.535	Increase	N
		Noncompliance	0.350	Increase	N
	Indiana	Mean Speed	0.537	Increase	N
		Speed Variance			
		85th-Percentile Speed	0.338	Increase	N
		Median Speed	0.608	Increase	N
		Noncompliance			
3: USL to DSL	Idaho[a]	Mean Speed	0.000, 0.790	Increase, Decrease	Y, N
		Speed Variance			
		85th-Percentile Speed	0.000, 0.563	Increase, Decrease	Y, N
		Median Speed			
		Noncompliance			
4: DSL to USL	Virginia	Mean Speed	0.318	Decrease	N
		Speed Variance	0.136	Increase[b]	N
		85th-Percentile Speed	0.356	Decrease	N
		Median Speed	0.209	Decrease	N
		Noncompliance	0.000	Decrease	Y

[a]As stated previously, when before/after comparisons were made for the State of Idaho, the first statistic reflects a before group with the uniform speed limit of 105 km/h (65 mi/h) for all vehicles and an after group with a uniform limit of 121 km/h (75 mi/h) for all vehicles. The second statistic reflects a before group with the same uniform limit of 121 km/h (75 mi/h) and an after group with a differential speed limit of 121/105 km/h (75/65 mi/h) for cars and trucks, respectively.
[b]As shown in figure 28, Virginia speed variance calculations are based on only 3 years of data.

Noncompliance rate should be considered as an example of the influence of other factors. In Virginia, these data were only available for 3 years: 1991, 2000, and 2001, with the percentage of drivers exceeding the speed limit being 77 percent, 55.2 percent, and 50 percent, respectively.

Differences between the before DSL period (1991) and the after DSL period (2000 and 2001) were significant, whereas differences between 2000 and 2001 were not significant. Statistically, a strong case can be made that this State's results suggest that elimination of DSL was correlated with a lowering of the noncompliance rate in this one particular State. Given the sparse data available from Virginia, which are more than that available from other States, however, this inference is tenuous, given the small data points shown in figure 29.

Comparison of Six Interstate Highway Segments in Idaho

It could be argued that the five different States shown in table 9 may have masked impacts of speed limit changes because of differences among them. Fortunately, one State—Idaho—had six sites where speed data were available; some of these sites had DSL and some had USL. Thus, the changes in the 85th percentile speed and mean speeds were compared at these sites. As shown in table 10, a shift from a uniform limit to a differential limit occurred at the three rural sites meant that researchers could look closely at how speeds at those sites changed after 1998.

Table 10. Idaho speed limits.

Site (Interstate and Milepost)	Speed Limit Change (mi/h)	Speed Limit Change (km/h)	Year of Change	Type of Area
I-84 MP 14.9	65/65–75/75–75/65	104/104–120/120–121/105	1996, 1998	Rural
I-84 MP 19.1	65/65–75/75–75/65	104/104–120/120–121/105	1996, 1998	Rural
I-90 MP 35.59	65/65–75/75–75/65	104/104–120/120–121/105	1996, 1998	Rural
I-84 MP 51	55/55–65/65	105/105–120/120	1996	Urban
I-90 MP 61.6	65/65–70/70	105/105–112/112	1996	Urban
I-90 MP 86.2	65/65–70/70	105/105–112/112	1996	Urban

Statistical tests of these data show that, while significant differences were observed in the Idaho speeds when comparing yearly data, none of the differences were significant when comparing DSL to USL, as shaded in tables 10 and 11. The finding is that, although some factors clearly caused Idaho interstate mean speeds and 85th percentile speeds to change, a decrease in the truck speed limit (changing that limit from USL to DSL) for the three rural segments clearly did not affect these speeds. For example, for the first site shown in table 11 (Interstate 84 at milepost 14.9), it is clear that the first speed limit change, which raised the USL by 16.1 km/h (10 mi/h), significantly increased the mean and 85th percentile speeds ($p = 0.000$). Yet the second speed limit change for that site, which is shown in table 10, a change to DSL, did not significantly change the mean or 85th percentile speeds as shaded in table 11. Shaded results indicate that there was no significant change in the mean or 85th percentile speeds.

Table 11. ANOVA results of mean speed and 85th percentile speed in Idaho.

Interstate	Number of Data Points	Variable	Before-After ANOVA
I-84 MP 14.9	3–14	Mean Speed	0.000(+)* 0.099(−)
		85th Percentile Speed	0.000(+)* 0.084(−)
I-84 MP 19.1	8–12	Mean Speed	0.000(+)* 0.710(−)
		85th Percentile Speed	0.000(+)* 0.241(−)
I-90 MP 35.59	8–12	Mean Speed	0.000(+)* 0.935(−)
		85th Percentile Speed	0.000(+)* 0.937(−)
I-84 MP 51	6–12	Mean Speed	0.000(+)*
		85th Percentile Speed	0.000(+)*
I-90 MP 61.6	3–12	Mean Speed	0.000(+)*
		85th Percentile Speed	0.000(+)*
I-90 MP 86.2	6–12	Mean Speed	0.000(+)*
		85th Percentile Speed	0.000(+)*

*The asterisk indicates that there was a significant increase in the mean and 85th percentile speeds.

Given that this is a regulatory change and assuming the statistical test is accurate, three implications are possible: the lowering of the speed limit was ignored by trucks; trucks were a relatively small percentage of the ADT such that the behavior of trucks did not influence speeds in an observable way; or increases in car speeds offset decreases in truck speeds. (Table 11 could be viewed as evidence that a shift to DSL can decrease speeds, since all changes in speed limits lead to statistically significant increases except the change to DSL. However, the only way to make this determination would be to evaluate changes at non-DSL sites during 1998.)

Discussion of Speed Impacts

Several inferences become apparent in examining these speed data:

- Significant increases in mean speed for Idaho and Iowa were observed over the study period. The other States tended to show insignificant increases. This trend suggests that the mean speed tended to increase over the 1990s regardless of whether the speed limit was uniform or differential. The 85th percentile speeds and median speeds showed a comparable upward trend, although fewer data were available for those two variables.

- Idaho's first speed limit change, from a uniform limit of 105 km/h (65 mi/h) to a uniform limit of 121 km/h (75 mi/h), resulted in a significant increase in both the mean speed and the 85th percentile speed.

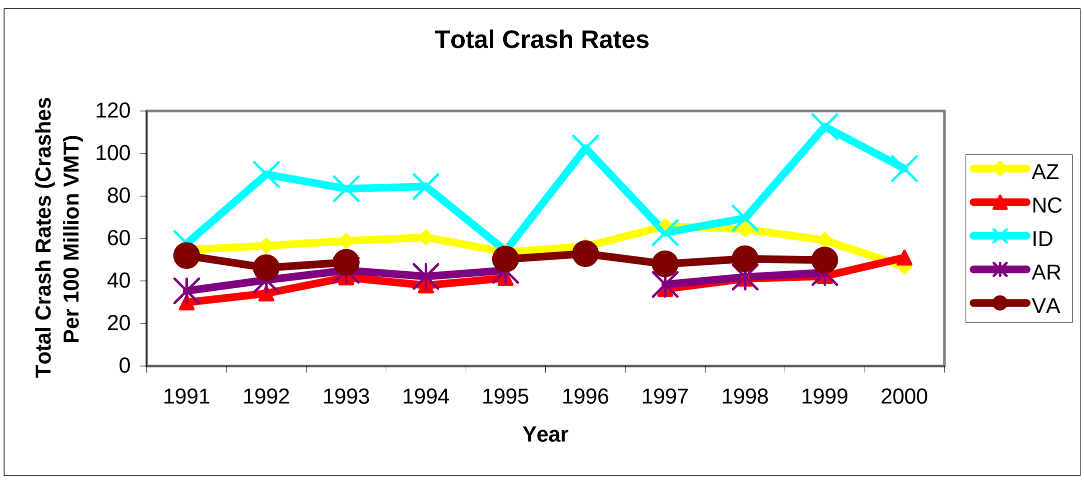
- Idaho's second speed limit change, from a uniform limit of 121 km/h (75 mi/h) to a differential limit of 121/105 km/h (75/65 mi/h) did not significantly change either the mean speed or the 85th percentile speed, although these did decrease slightly.

- In Illinois, where no speed limit changes were implemented, the noncompliance rate increased steadily. Given the increasing mean speed of Illinois, this could be due to drivers desiring to travel at a higher speed.

Crash Rates (Analyzed by Conventional Methods)

Table 4 showed that crash rate data could be obtained from five States (Arizona, Arkansas, Idaho, North Carolina, and Virginia). Six types of crash rates (total crash rate, fatal crash rate, rear-end crash rate, total truck-involved crash rate, truck-involved fatal crash rate, and truck-involved rear-end crash rate) were evaluated. As was done with the speed data, both a before-after comparison and a year-pair comparison were performed. (Again, for those States that never changed their policy, the data were categorized into two virtual groups, 1990–1995 and 1996–2000.)

Figure 31 graphically compares the total crash rate in the five States for all sites, while table 12 indicates which changes were significant. Of the two States that maintained USL, one (North Carolina) showed a significant increase in the total crash rate, whereas only Arizona showed an insignificant increase. None of the other States, all of which had either changed from DSL to USL or from USL to DSL, showed a significant change in the total crash rate.



1 mi/h = 1.6 km/h

Figure 31. Chart. Total crash rates,

Note that speed limits changed in Arkansas (1996), Idaho (1996, 1998), and Virginia (1994)

Table 12 shows the before versus after period change in the six types of crash rates of the five States, with the *p* values from the ANOVA test given in parentheses. In examining table 12, there were 27 cases where both an ADT and a crash rate were available. There was a significant difference between the before and after period in 3 of those 27 cases.

Table 12. Statistical Tests for Significance in Crash Rates.

Policy	State	Type of Crash Rate	Difference	Significance (*p*)
Group 1: Maintained Uniform	Arizona	Total	+	N (0.583)
		Fatal	+	N (0.140)
		Rear-end	+	N (0.052)
		Total truck-involved	+	N (0.949)
		Truck-involved fatal	+	N (0.134)
		Truck-involved rear-end	+	N (0.406)
	North Carolina**	Total	+	Y (0.007)
		Fatal	+	N (0.100)
		Rear-end	+	Y (0.035)
		Total truck-involved	+	N (0.504)
		Truck-involved fatal	−	N (0.525)
		Truck-involved rear-end	+	N (0.366)
Group 3: Changed from Uniform to Differential	Arkansas	Total	−	N (0.935)
		Fatal	+	N (0.495)
		Rear-end	+	N (0.258)
		Total truck-involved	+	N (0.250)
		Truck-involved fatal		
		Truck-involved rear-end		
	Idaho	Total	−, +	N, N (0.539,0.153)
		Fatal	−, +	N, N (0.336,0.192)
		Rear-end	−, +	N, N (0.539, 0.327)
		Total truck-involved	−, +	N, N (0.473,0.139)
		Truck-involved fatal*	−, 0	N, N (0.656,1.000)
		Truck-involved rear-end	−, +	N, N (0.820,0.370)
Group 4: Changed from Differential to Uniform	Virginia	Total	+	N (0.425)
		Fatal	−	N (0.270)
		Rear-end	+	N (0.119)
		Total truck-involved	+	Y (0.000)
		Truck-involved fatal	+	N (0.665)
		Truck-involved rear-end		

*Note: The number of truck-involved fatal crashes was 0 in Idaho, which is why "1.000" is shown in that cell.
**North Carolina maintained a uniform limit but also raised this limit for both passenger cars and trucks.

Table 12 also shows that, as was the case with the speed analysis, there is no consistent trend in crash rates matching the change in speed limits. For example, Virginia and Arkansas, two States that were diametrically opposed in terms of their policies (Arkansas changed to DSL and Virginia changed from DSL), both showed statistically insignificant increases in rear-end crashes with almost identical *p*-values.

On the basis of the limited data available, there is potential evidence that the change to USL (as in Virginia) resulted in an increase in the number of rear-end crashes since the number of these crashes increased in Virginia, albeit insignificantly. This viewpoint, however, is tempered by two observations: first, such an increase was insignificant; and second, there was no corresponding decrease in the States that shifted from USL to DSL. In fact, the States that made such a shift (Idaho and Arkansas) saw an increase (albeit insignificant) in the number of rear-end crashes. Virginia observed a reduction in the fatal crash rates after the change from DSL to USL, and Idaho observed an increase in the fatal crash rate after changing from USL to DSL, although both of these changes were insignificant.

The most striking feature of table 12 is that, despite all four combinations of speed limit policies (maintaining USL, maintaining DSL, changing from USL to DSL, or changing from DSL to USL), not a single State saw a significant decrease in any of the various crash rate categories. Crash rates either did not change significantly or significantly increase.

Because crash data were available for five interstate segments in Virginia, these crash data were scrutinized to see if patterns could be gleaned from an examination of the sites grouped by interstate. A snapshot of these data is shown in figure 32, which illustrates that no single segment dominated the statewide average for total truck-involved crash rate. Figure 31 does, however, show a slight increase over time for the crash rates on the interstate segments.

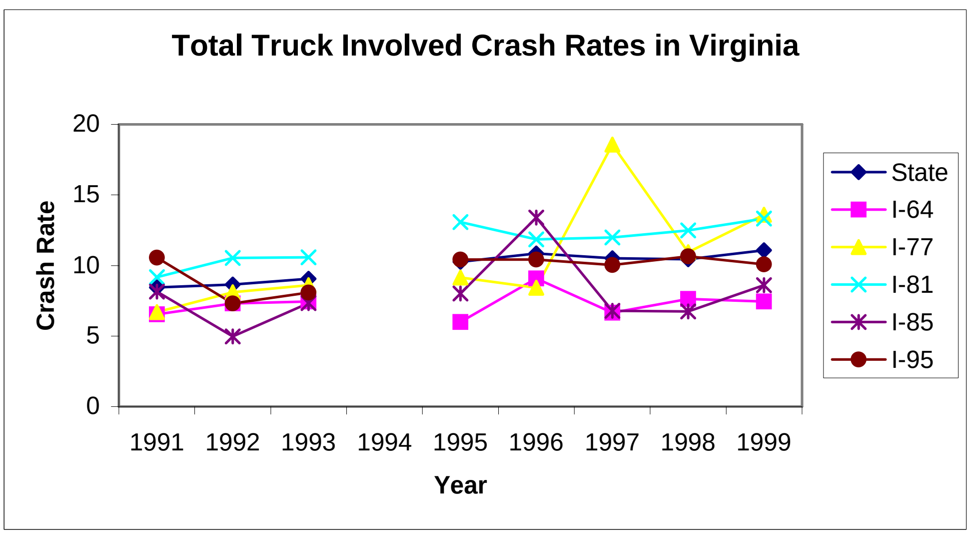


Figure 32: Chart. Total truck-involved crash rates in Virginia interstate highways.

Crashes (Analyzed by the Modified Empirical Bayes Method)

Because the empirical Bayes approach is best suited for a before/after scenario where one State undergoes some type of treatment—such as a change in the speed limits—the results are presented first for the policy group 3 and 4 States, where States changed from either a differential limit to a uniform limit or from a uniform to a differential limit.

Virginia Crashes (DSL to USL))

For the 266 sites that comprised the Virginia study data, the length of the sites ranged from 1.7 km to 22.9 km (1.05 mi to 14.25 mi) in length, and substantial variation in ADT was observed, as shown in table 13. The minimum, maximum, and total crashes per segment are shown for each crash type and reveal substantial variation, as well.

Table 13. Virginia data for the before and after periods.

Year	**ADT**	**Total Crash**	**Fatal Crash**	**Rear-End Crash**	**Total Crash with Truck**	**Fatal Crash with Truck**
	Min-Max-Average	Min-max-total	Min-max-total	Min-max-total	Min-max-total	Min-max-total
1991	3,067-55,050-12,719	0-55-2,888	0-2-59	0-23-554	0-11-528	0-1-10
1992	2,634-145,000-14,968	0-43-2,839	0-2-51	0-22-524	0-21-592	0-2-17
1993	3,075-62,558-14,638	1-53-3,322	0-3-58	0-35-580	0-17-648	0-1-20
1995	3,422-63,204-15,500	0-82-3,608	0-3-77	0-50-727	0-17-772	0-1-17
1996	3,669-65,860-16,135	1-73-3,964	0-3-57	0-47-732	0-17-867	0-2-17
1997	4,216-74,644-16,940	0-58-3,735	0-4-69	0-34-713	0-18-846	0-3-19
1999	3,600-61,132-17,693	1-76-4,070	0-2-53	0-56-798	0-17-948	0-2-26

Calibration of the parameters for the crash estimation model of the form shown in figure 8 as $E(m) = \alpha(Length)^{\beta_1}(ADT)^{\beta_2}$ are shown in table 14 for the Virginia data. The fact that β_2 is less than 1.0 for all crash types means that the model presumes that ADT does not have an equal proportional effect on crashes. Instead, a certain percentage increase in ADT will result in a small percentage increase in the number of crashes. In short, an increase in ADT should yield a lower crash *rate* according to the model. Similarly, the β_1 values below 1.0 mean that an increase in section length, according to the model, will increase crashes by a smaller percentage. (For all States, the β_1 exponent usually was also almost always less than 1.0, meaning that as the section length increases, the number of crashes forecasted by the crash estimation model will correspond to a lower crash rate.)

Table 14. Crash estimation model parameters for Virginia data.

Parameter	Total	Fatal	Rear-End	Truck Total	Truck Fatal
k	5.9	5.58	1.71	3.99	15.0
α	0.02243	0.00119	0.000325	0.000316	0.000017
β_1	0.622	0.842	0.536	0.819	1.226
β_2	0.548	0.398	0.829	0.788	0.639

The application of equations in figures 17–20 is reflected in table 15. The application of figure 18 results in the actual crash data for each year being shown in the "λ" column with the cumulative values of λ shown in the "Cumu λ" column. Similarly, the application of the equation in figure 17 shows the predicted would-have-been crash data for each year during the after period listed in the fourth column ("π"), with the cumulative values of these would-have-been crashes shown in the fifth column ("Cumu π"). The variance of π was calculated for each year during the after period and listed in the sixth column ("VAR(π)"), with the cumulative values of VAR(π) listed in the next column ("Cumu VAR(π)"). The equation in figure 19 means that the evaluation was conducted using the difference between the would-have-been after crashes and the actual after crashes, shown as "Excess δ" and tabulated by year in the "Cumu δ" column. The evaluation was also investigated using figure 22—by obtaining the ratio of the actual after crashes to the would-have-been after crashes for each year ("θ") and the total values of ("Cumu θ"). Variances for both the difference δ and the ratio θ are also calculated and listed to the right of table 15 as an application of figures 20 and 25.

Table 15. Total crashes for Virginia.

YEAR	λ	Cumu λ	π	Cumu π	VAR(π)	Cumu VAR(π)	*Excess* δ	Cumu δ	Ratio θ	Cumu θ	Var(δ)	Var(θ)	Cumu Var(δ)	Cumu Var(θ)
1995	3,608	3,608	3,223	3,223	986	986	-385	-385	1.119	1.119	4,594	0.000466	4,594	0.000466
1996	3,964	7,572	3,296	6,519	1,031	2,017	-668	-1,053	1.202	1.161	4,995	0.000502	9,589	0.000242
1997	3,735	11,307	3,383	9,903	1,088	3,105	-352	-1,404	1.104	1.142	4,823	0.000442	14,412	0.000156
1999	4,070	15,377	3,463	13,366	1,142	4,247	-607	-2,011	1.175	1.150	5,212	0.000471	19,624	0.000117
Ave	3,844		3,341		4,247		-503		1.150					

Table 15 shows that the ratio θ for total crashes in Virginia was larger than 1.0. Furthermore, when the empirical confidence bounds are selected in accordance with the equation in figure 25, it is clear that the upper and lower bounds for θ are very close to 1.15 and certainly do not include 1.0 within that range. Thus, according to the empirical Bayes technique, because the ratio of the actual after crashes (λ) to the would-have-been after crashes (π) is greater than 1.0, then the treatment (a change from a differential limit to a uniform limit) resulted in an increase in the number of crashes. In fact, for all Virginia crash types, θ is greater than 1.0. If the analysis is restricted to Virginia alone and used only the empirical Bayes method, then the interpretation would be that the change to a uniform speed limit increased the number of crashes. However, as is explained in the following sections, data from other States do not support this interpretation.

Table 16 shows a comparable analysis except that it is restricted to fatal crashes in Virginia. Statistically, application of the equation in figure 25 shows that the cumulative value of θ is 1.06 for total fatal crashes. Close examination of table 16 shows inconsistent performance from year to year. Although θ is greater than 1.0 for 1995 and 1997, it is less than 1.0 for 1996 and 1999.

Table 16. Virginia total fatal crashes.

YEAR	λ	Cumu λ	π	Cumu π	VAR(π)	Cumu VAR(π)	Excess δ	Cumu δ	Ratio θ	Cumu θ	Var(δ)	Var(θ)	Cumu Var(δ)	Cumu Var(θ)
1995	77	77	58.65	58.65	2.28	2.28	-18.36	-18.36	1.31	1.31	79.28	0.02	79.28	0.02347
1996	57	134	59.57	118.22	2.36	4.64	2.57	-15.79	0.96	1.13	59.36	0.02	138.64	0.01000
1997	69	203	60.82	179.04	2.46	7.10	-8.18	-23.97	1.13	1.13	71.46	0.02	210.10	0.00661
1999	53	256	61.82	240.85	2.53	9.62	8.82	-15.15	0.86	1.06	55.53	0.01	265.62	0.00460
Ave	64		60.21		9.62		-3.79		1.06					

Arkansas Crashes (USL to DSL)

Arkansas is of special interest because it is the reverse of Virginia in that the State changed from USL to DSL. The Arkansas data shown in table 17 are from 10 interstate sections in Arkansas, each 16 km (10 mi) in length.

Table 17. Crash data for Arkansas.

Year	ADT	Total Crash	Fatal Crash	Rear-End Crash	Total Crash with Truck
	Min-max-average	Min-max-total	Min-max-total	Min-max-total	Min-max-total
1991	18,000-26,220-21,406	14-36-275	0-2-5	3-12-74	5-14-107
1992	18,968-22,860-21,956	15-65-326	0-2-6	1-25-85	5-23-118
1993	16,250-28,500-23,313	25-54-380	0-5-11	4-23-88	9-21-153
1994	15,780-31,000-24,600	25-53-374	0-1-4	5-24-113	8-24-143
1995	15,000-28,504-24,133	15-57-387	0-2-5	4-23-99	5-32-150

Table 17. Crash data for Arkansas—continued.

Year	ADT	Total Crash	Fatal Crash	Rear-End Crash	Total Crash with Truck
	Min-max-average	Min-max-total	Min-max-total	Min-max-total	Min-max-total
1997	26,288-33,746-29,711	26-61-414	0-2-6	7-22-113	8-30-194
1998	26,000-39,339-30,182	31-59-459	0-5-14	6-21-119	14-28-197
1999	27,000-35,312-29,675	26-74-476	0-3-10	3-44-173	11-32-195

As shown in table 18, the β_1 parameter was assumed to be 1.0 since all sections were of the same length and the β_2 parameter was found to be less than 1.0 for all crash types except that of rear-end crashes. The 1.774 coefficient from table 18 means that, according to the model, a certain increase in ADT would increase crashes by a larger proportion. In short, at a given site, increasing the ADT will increase the crash rate.

Table 18. Crash estimation model parameters for Arkansas data.

Parameter	Total	Fatal	Rear-End	Truck Total
k	19.03	25.0	14.86	18.2
α	0.00267	0.0598	0.000000016	0.0239
β_1	1.0	1.0	1.0	1.0
β_2	0.714	0.00355	1.774	0.401

Except for rear-end crashes, all crash types for Arkansas showed that θ was significantly greater than 1.0, meaning that the shift (from USL to DSL) increased the number of crashes. Rear-end crashes were the only crash type that did not follow this trend because such crash types showed θ less than 1.0 in table 19; moreover, as shown in table 18, only rear-end crashes had a β_2 ADT coefficient that was greater than 1.0. Table 20 shows the application of the empirical Bayes approach for the total number of crashes on Arkansas rural interstate highways. Another important finding, which supports the statistical analysis, is that although both Virginia and Arizona showed an increase in the fatal crashes after the changes in the speed policies, the crashes in Virginia increased by only 6 percent. (See table 21 that shows Arkansas had a 60-percent increase when the State changed from DSL to USL. There is some variation in the Arkansas results, however, with θ being greater than 1.0 for 2 of the 3 years.)

Table 19. Arkansas rear-end crashes.

YEAR	λ	Cumu λ	π	Cumu π	VAR(π)	Cumu VAR(π)	Excess δ	Cumu δ	Ratio θ	Cumu θ	Var(δ)	Var(θ)	Cumu Var(δ)	Cumu Var(θ)
1997	113	113	143.31	143	34.34	34.34	30.31	30.31	0.787	0.787	147.34	0.00650	147	0.00650
1998	119	232	148.01	291	37.48	71.82	29.01	59.33	0.803	0.796	156.48	0.00649	304	0.00326
1999	173	405	141.66	433	33.52	105.34	-31.34	27.98	1.219	0.935	206.52	0.01104	510	0.00265
Ave	135		144.33		105.34		9.33		0.936					

Table 20. Total crashes for Arkansas.

YEAR	λ	Cumu λ	π	Cumu π	VAR(π)	Cumu VAR(π)	Excess δ	Cumu δ	Ratio θ	Cumu θ	Var(δ)	Var(θ)	Cumu Var(δ)	Cumu Var(θ)
1997	414	414	419.24	419	91.34	91.34	5.24	5.24	0.987	0.987	505.34	0.00286	505	0.00286
1998	459	873	425.14	844	94.16	185.49	-33.86	-28.62	1.079	1.034	553.16	0.00314	1058	0.00150
1999	476	1,349	417.70	1262	90.64	276.13	-58.30	-86.92	1.139	1.069	566.64	0.00340	1625	0.00104
Ave	450		420.70		276.13		-28.97		1.068					

Finally, the total number of fatal crashes for Arkansas was only 30 during the after period. Certainly, this small sample size contributes to the width of the corresponding confidence interval shown in table 25. Yet, the exceptionally low value of calibration parameter β_2 shown in table 24 may be a result of the fact that, for the specific case of Arkansas fatal crashes, the low calibration parameter of β_2 for Arkansas was not significant, as shown in table 26. Overall, the effect of that low value is to make the effect of increasing ADT almost negligible in terms of increasing crash risk. Arguably, this is a reasonable impact for fatal truck crashes, and the extraordinarily low value of that parameter relative to that of the other crash types makes one hesitant to draw conclusions from that set of truck-involved fatal crashes for Arkansas. (For Arkansas only, all β_1 values were set to 1.0 because lengths for all Arkansas sites were identical.)

Table 21. Fatal crashes for Arkansas.

YEAR	λ	Cumu λ	π	Cumu π	VAR(π)	Cumu VAR(π)	Excess δ	Cumu δ	Ratio θ	Cumu θ	Var(δ)	Var(θ)	Cumu Var(δ)	Cumu Var(θ)
1997	6	6	6.204	6.204	0.137	0.137	0.204	0.204	0.964	0.964	6.137	0.157	6.137	0.157
1998	14	20	6.205	12.409	0.137	0.274	-7.795	-7.591	2.248	1.609	14.137	0.376	20.274	0.134
1999	10	30	6.204	18.613	0.137	0.411	-3.796	-11.387	1.606	1.700	10.137	0.265	30.411	0.089
Ave	10		6.204		0.411		-3.7957		1.606					

Idaho Crashes (USL to DSL)

Similar to Arkansas, Idaho changed its speed limit from uniform to differential. Table 22 shows that 1996 was chosen as the before period and 1999–2000 were chosen as the after period, such that 32 sections from Interstates 15, 84, 86, and 90 comprised the study group. Section lengths ranged from 0.16 km to 139 km (0.1 mi to 86.1 mi).

Table 22. Crash data for Idaho.

Year	ADT	Total Crash	Rear-End Crash	Total Crash with Truck	Rear-End with Truck
	Min-max-average	Min-max-total	Min-max-total	Min-max-total	Min-max-total
1997	2,800-36,782-11,006	0-15-152	0-10-33	0-2-8	0-1-2
1999	3,050-40,522-11,881	0-44-237	0-42-70	0-2-21	0-2-6
2000	3,100- 41,686-12,131	0-23-184	0-18-55	0-4-21	0-2-5

With β_1 and β_2 parameters of 0.81 and 0.75 respectively for total crashes, Idaho data suggest that the total number of crashes was approximately 29 percent higher than would have occurred, had the uniform limit been retained. The variation of θ, shown in the far right column of table 23, suggests that these results are statistically significant. In fact, θ values were greater than 1.0 and significant for all types of Idaho crashes. There were cases where the variance of θ was not always so low. For example, for Idaho truck rear-end crashes, the variance of θ was computed as approximately 0.5. In that instance, however, θ had been found to be about 2.35, meaning that the impact of a speed limit change on the number of Idaho truck rear-end crashes was still significant according to the empirical Bayes method.

Table 23. Total crashes for Idaho.

YEAR	λ	Cumu λ	π	Cumu π	VAR(π)	Cumu VAR(π)	*Excess* δ	Cumu δ	Ratio θ	Cumu θ	Var(δ)	Var(θ)	Cumu Var(δ)	Cumu Var(θ)
1999	237	237	161.6	161.6	93.76	93.76	-75.41	-75.41	1.461	1.461	330.8	0.0166	330.8	0.0166
2000	184	421	162.9	324.5	95.55	189.31	-21.06	-96.47	1.125	1.295	279.6	0.0114	610.3	0.00697
Ave	210.5		162.3		189.3		-48.23		1.293					

Crashes from the States of Arizona, Missouri, North Carolina, and Washington

Table 24 shows the values of θ that were obtained when evaluating the effect of maintaining the same speed limit in four other States. For comparison purposes, the values from Arkansas, Idaho, and Virginia are shown as well. In most cases, table 24 shows that θ was greater than 1.0. On examining these θ values for individual States, it seems that, although the ratio for total crashes in Virginia (changed from DSL to USL) is higher than that for Arkansas (changed from USL to DSL), it is lower than that for Idaho, which, like Arkansas, also changed from USL to DSL. An important factor in the case of Virginia is that the total fatal crash ratio is 1.06, whereas that for Arkansas was 1.61. All ratios for Idaho were much higher than those for Virginia. This suggests that, although there was an overall trend for an increase in crashes, the percentage of increases, particularly for fatal crashes, tended to be higher in States that changed from USL to DSL.

On the other hand, the data from table 24 are not consistent. For total crashes, θ remained approximately 1.0 for the one State that maintained DSL (Washington), whereas it was greater than 1.0 for States that maintained USL, Arizona and North Carolina. At first glance, the results might be interpreted to mean that maintaining DSL caused no change in crashes while maintaining USL caused an increase in the number of crashes. Examination of the fatal crashes shows inconsistency in those States that maintained USLs where fatal crash data were available; in these cases, θ was greater than 1.0 for Arizona, but less than 1.0 for North Carolina.

Table 25 presents confidence intervals for the expected percentage crash increase for each type of crash and for each State according to the empirical Bayes method. For example, consider the category of "total crash with truck involved." Table 24 shows θ values of 1.31 and 1.25 for Arkansas and Virginia, respectively, suggesting that there was a 31 percent increase in Arkansas and a 25 percent increase in Virginia as a result of the changes in the speed limit policies. However, these percentage increases are not perfect, given the variability that can occur in crashes. Thus, table 25 is used to suggest that the confidence interval for Arkansas is between 18.9 percent and 42.8 percent, and between 20.0 percent and 29.8 percent for Virginia. Clearly, larger sample sizes tended to lead to smaller confidence intervals.

Generally, the standard interpretation of confidence intervals is to declare the change insignificant if the confidence interval includes zero. Using the Virginia example, it can be inferred that according to this application of the empirical Bayes methodology, the change from DSL to USL in Virginia did not affect truck-involved fatal crashes (since that confidence interval runs from –12.5 percent to +38.4 percent and thus includes zero), but that it did cause total crashes to significantly increase (since the corresponding confidence interval is 12.9 percent to 17.2 percent and thus does *not* include zero.)

Interestingly, confidence intervals associated with the use of the empirical Bayes method in table 25 showed either increases in crashes or no change in crashes for all States and for all categories of crashes regardless of the speed policy change, with only one exception—fatal crashes in North Carolina, a State which maintained USL.

Table 24. Impact of speed limit changes according to the empirical Bayes formulation.

Policy Group	State	Crash type	Ratio θ	β_2	Inference of Modified Empirical Bayes Method *Only**
Group 4: Changed from DSL to USL	Virginia	Total crash	1.15	0.548	According to Virginia data, the actual number of crashes during the after period was higher than would have been expected without the change to USL.
		Total crash with truck involved	1.25	0.788	
		Rear-end crash	1.16	0.829	
		Fatal crash	1.06	0.398	
		Fatal crash with truck involved	1.13	0.639	
		Average	1.15		
Group 3: Changed from USL to DSL	Arkansas	Total crash	1.07	0.714	According to Arkansas data, the actual number of crashes during the after period was higher than would have been expected without the change to DSL except for rear-end crashes.
		Total crash with truck involved	1.31	0.401	
		Rear-end crash	0.93	1.774	
		Fatal crash	1.61	0.00355	
		Average	1.23		
	Idaho	Total crash	1.29	0.745	According to Idaho data, the actual number of crashes during the after period was higher than would have been expected without the change to DSL.
		Total crash with truck involved	2.46	0.717	
		Rear-end crash	1.62	1.717	
		Rear-end crash with truck involved	2.36	1.698	
		Average	1.93		

Table 24. Impact of speed limit changes according to the empirical Bayes formulation—continued.

Policy Group	State	Crash type	Ratio θ	β_2	Inference of Modified Empirical Bayes Method *Only**
Group 1: Maintained USL	Arizona	Total crash	1.26	0.127	According to Arizona data, the actual number of crashes during the after period was higher than would have been expected even though USL was maintained.
		Total crash with truck involved	1.16	0.208	
		Rear-end crash	1.2	0.757	
		Rear-end crash with truck involved	1.07	0.537	
		Fatal crash	1.33	0.120	
		Fatal crash with truck involved	1.63	0.180	
		Average	1.28		
	North Carolina	Total crash	1.26	0.827	According to North Carolina data, the actual number of total crashes during the after period was higher than would have been expected even though USL was maintained, except for some specific crash types where the opposite was observed to increase total crashes and decrease fatal crashes.
		Total crash with truck involved	0.91	1.704	
		Rear-end crash	1.002	1.619	
		Rear-end crash with truck involved	0.97	1.834	
		Fatal crash	0.74	2.878	
		Average	0.98		
Group 2: Maintained DSL	Washington	Total crash	0.99	0.340	According to Washington data, the decision to maintain DSL had no effect on crashes.

*The inferences shown here are findings that would be drawn if each State were examined in isolation and the modified empirical Bayes method as applied were the only analysis technique available. As discussed later in this paper, a consideration of all States together using all available methods leads to different conclusions than those shown in table 24.

**North Carolina maintained its uniform limit but also raised this limit for both passenger cars and trucks.

Table 25. Crash increases and confidence intervals according to the empirical Bayes formulation.

Policy Group	State	Crash Type	Lower Bound Increase	Upper Bound Increase	Statistical Interpretation
Group 4: Changed from DSL to USL	Virginia	Total crash	12.9%	17.2%	Increase
		Total crash with truck involved	20.0%	29.8%	Increase
		Rear-end crash	11.2%	21.1%	Increase
		Fatal crash	−7.3%	19.9%	*No change*
		Fatal crash with truck involved	−12.5%	38.4%	*No change*
Group 3: Changed from USL to DSL	Arkansas	Total crash	0.4%	13.4%	Increase
		Total crash with truck involved	18.9%	42.8%	Increase
		Rear-end crash	−16.7%	3.9%	*No change*
		Fatal crash	1.5%	121.3%	Increase
	Idaho	Total crash	13.2%	46.7%	Increase
		Total crash with truck involved	68.6%	224.9%	Increase
		Rear-end crash	30.6%	94.8%	Increase
		Rear-end crash with truck involved	−7.4%	281.1%	*No change*

Table 25. Crash increases and confidence intervals according to the empirical Bayes formulation—continued.

Policy Group	State	Crash Type	Lower Bound Increase	Upper Bound Increase	Statistical Interpretation
Group 1: Maintained USL	Arizona	Total crash	24.2%	28.6%	Increase
		Total crash with truck involved	12.1%	20.7%	Increase
		Rear-end crash	14.8%	25.3%	Increase
		Rear-end crash with truck involved	−1.5%	14.7%	*No change*
		Fatal crash	20.6%	45.2%	Increase
		Fatal crash with truck involved	30.0%	97.4%	Increase
	North Carolina	Total crash	19.9%	31.9%	Increase
		Total crash with truck involved	−19.7%	1.5%	*No change*
		Rear-end crash	−12.6%	13.1%	*No change*
		Rear-end crash with truck involved	−23.4%	18.6%	*No change*
		Fatal crash	−50.1%	−2.1%	*Decrease*
Group 2: Maintained DSL	Washington	Total crash	−6.6%	5.0%	*No change*

Table 26. *T*-Statistics for the empirical Bayes crash estimation models (before data).

Policy Group	State	Crash Type	*T*-Statistic for β_1 (length)	*T*-Statistic for β_2 (ADT)	Degrees of Freedom	Critical *T*-Statistic	Were the Values Significant?
Group 4: Changed from DSL to USL	Virginia	Total crash	10.69	11.93	795	1.96	Yes, for all crash types.
		Total crash with truck involved	9.45	12.45			
		Rear-end crash	4.07	7.91			
		Fatal crash	3.63	2.54			
		Fatal crash with truck involved	2.86	2.54			

Table 26. *T*-Statistics for the empirical Bayes crash estimation models (before data)—continued.

Policy Group	State	Crash Type	*T*-Statistic for β_1 (length)	*T*-Statistic for β_2 (ADT)	Degrees of Freedom	Critical *T*-statistic	Were the Values Significant?
Group 3: Changed from USL to DSL	Arkansas	Total crash	Lengths were same length	2.29	48	2.01	Yes for total crash and rear-end crash. No for total crash with truck involved and fatal crash.
		Total crash with truck involved		1.02			
		Rear-end crash		3.25			
		Fatal crash		0.00			
	Idaho	Total crash	4.77	3.96	29	2.05	Yes, except β_2 was not significant for total crash with truck involved.
		Total crash with truck involved	2.97	1.66			
		Rear-end crash	2.95	6.05			
		Rear-end crash with truck involved	2.59	2.81			
Group 1: Maintained USL	Arizona	Total crash	25.32	2.97	1,665	1.96	Yes, except β_2 was not significant for fatal crash and fatal crash with truck involved.
		Total crash with truck involved	19.46	3.65			
		Rear-end crash	13.72	9.93			
		Rear-end crash with truck involved	11.74	5.20			
		Fatal crash	8.93	0.93			
		Fatal crash with truck involved	4.4	0.57			
	North Carolina	Total crash	5.27	9.86	122	1.98	β_2 was always significant, but β_1 was only significant for total crash and rear-end crash.
		Total crash with truck involved	0.43	8.27			
		Rear-end crash	2.25	7.25			
		Rear-end crash with truck involved	0.21	4.91			
		Fatal crash	0.75	4.22			
Group 2: Maintained DSL	Washington	Total crash	1.49	1.17	87	1.99	Neither β_1 nor β_2 was significant.

*Shaded values were not significant at the 95 percent confidence level

Finally, table 26 shows the *T*-statistics associated with the β_1 and β_2 values used in the crash estimation model. An interpretation of table 26 is that, for all States and crash types, either section length, section ADT, or both were significant determinants of the number of crashes with three exceptions: total crashes in Washington, total crashes with truck involved in Arkansas, and fatal crashes in Arkansas. For the State of Washington, the corresponding confidence interval from table 25 had shown no change in crashes; in that sense, the results from table 26 are not surprising. For the case of Arkansas fatal crashes, the results from table 26 show that the crash estimation model for that particular case was probably spurious. In that sense, the decision from table 24 (that, because of the abnormally low value for β_2 observed for Arkansans fatal crashes in table 24, one should be cautious about inferences for this particular case) is confirmed by the corresponding *T*-statistic in table 26.

Relating Speed and Crash Changes

The logical question is whether any relationship exists between crashes or crash rate changes and changes in speed. Accordingly, researchers may compare the *speed* results shown in table 9 to the *crash* results shown in table 24, and/or the *crash rate* results shown in table 12. The States that intersect both speed-related data in table 8 and either of the crash tables (tables 12 or 24) are Idaho and Virginia. From those two States, investigators can glean three sets of changes in speed policy: Idaho increasing its USL, Idaho changing from USL to DSL, and Virginia changing from DSL to USL. Yet even these results are inconclusive:

- When Idaho raised its USL, there was an increase in both the mean speed and the 85th percentile speed (other speed data were not available), as expected. Yet there was no significant change in the crash rate.

- When Idaho changed USL to DSL, there was no significant change in its speed or its crash rate. There was, on the other hand, an increase in the number of crashes as reflected in tables 24 and 25. (Except for the category of truck-involved rear-end crashes, the upper and lower confidence bounds for all Idaho crash rate categories were positive. To the extent that having the 95 percent confidence interval not include zero indicates statistical significance, therefore, one would say that the Idaho crash rate increases for all categories except rear-end with truck involved were significant. For the category of rear-end with truck involved, research could deduce no significant change because the confidence interval includes zero. The lower confidence interval is less than zero, and the upper confidence interval is greater than zero.)

- When Virginia changed DSL to USL, there were no significant changes in any speed measures except for one—the rate of noncompliance significantly dropped (generally viewed as a positive development). There was a significant increase in two of five crash rate categories (rear-end and total truck involved) and an increase in three of the five categories for the number of crashes via the empirical Bayes method (the other two categories showed no change in crash rates).

Looking at only these three bullets, a logical inference based on these changes in speed policy would find no relationship between changes in speed and safety changes. Idaho's first change saw an increase in speeds but no adverse safety impacts, whereas Idaho's second change and Virginia's change each saw no change in most measures of speed and adverse safety impacts.

The only exception was Virginia's decreased rate of noncompliance—accompanied by an increase in the number of crashes. Certainly, it is plausible that the indicators of speed used in this study are simply not useful measures of crash risk.

Acceptance of this outcome—that there is no relationship between changes in speed indicators and crash changes—however, requires two logical steps. First, there must be the tacit acceptance that crashes (as measured by the empirical Bayes method) are a better indicator of safety than crash rates (thereby simplifying the second and third bullets), but that crash rates are a better measure of safety than nothing (thereby simplifying the first bullet). Second, tables 24 and 25 must be accepted as true even though they generally tended to show increasing crash risk, regardless of the policy speed limit change. In sum, this study offers some tendency for speed indicators and crash indicators to not be correlated. However, because these inferences are based on observations from only two States, such results prevent investigators from using this study to make that conclusion.

STUDY LIMITATIONS

Caveats About the Use of the Empirical Bayes Method

Several *data limitations* may have influenced the values shown in tables 24 and 25. These data limitations apply to the investigators' use of the modified empirical Bayes method and not necessarily to the application of the empirical Bayes method in other situations.

First, table 24 shows the corresponding exponent β_2 for each crash type and State, where β_2 reflects the impact of a change in ADT according to the crash estimation model developed for that State and crash type. A plausible explanation for this is that, in most States, ADT increases over time, so with a β_2 less than 1.0 the would-have-been crashes predicted by the crash estimation model will correspond to a lower crash *rate*. As β_2 approaches zero, large increases in ADT tend to only increase crashes by a disproportionately small amount, with the effect being that the would-have-been crash rate is so low that it is virtually impossible for any policy change to show a θ less than 1.0, which would be an improvement in safety. In sum, a low value of β_2 far from unity renders the model very insensitive to changes in ADT from the before scenario (in the sense that a low value of β_2 means that large additional ADT should only increase crashes slightly). Thus, the interpretation that Arkansas fatal crashes increased as a result of the change from USL to DSL should be tempered by the very low β_2 value of 0.00355 shown in table 24, which would mean that almost any change would probably result in a θ value greater than 1.0. Alternatively, one can cite that the insignificance of the crash estimation model, based on table 26, for the specific case of Arkansas fatal crashes, is also a reason not to rely solely on inferences from Arkansas fatal crashes.

Second, comparison groups are imperfect. Ideally, the comparison (control) group would have been selected from the same State at the same time as the studied group. For example, in Virginia, if after the statewide differential speed limit of the early 1990s was repealed, one section of Interstate 81 could have been kept at DSL and another section of the highway could have been changed to DSL. Unfortunately, not only were comparison groups composed of different roadways, but they also drew from different States altogether. Thus, since the actual Virginia reference group was an extrapolation of the temporal trend that occurred during the before period from 1991 to 1993, later aberrations in this trend may not have been identified. In other words, if some significant change occurred in one year that increased the crashes but had nothing to do with speed limits policy change, then unfortunately this change would not be captured in the comparison group and therefore would not be reflected in the models. In short, a problem would arise if the relationship between ADT and crashes had drastically changed from the before to the after period. When this relationship is based on multiple years of data and multiple sites, one can have some confidence in the model, but temporal changes in the after year are possible that might alter this relationship.

Third, although speed-monitoring data were available to clarify general speed trends throughout a State, specific speeds often were not available on every section of interstate that was a segment in the crash analysis. (In other words, for a given 322-km (200-mi) stretch of interstate, annual data indicating overall speed trends might be available from two speed-monitoring sites. While analyzing crashes on those interstates, however, if the interstates were divided into 20 sections for analysis, it would be best to have 20 speed-monitoring sites to include actual speeds in the crash estimation models.)

Next, the crash estimation models used identical treatment and comparison sites, which, as discussed earlier in the paper, was done to reduce error in the prediction of crashes by CEM. In traditional before/after studies, such a decision could possibly subject the study to what Hauer describes as "regression-to-mean (RTM) bias" or "selection bias."[(10)] As explained by Hauer, RTM arises when "there is a link between the decision to treat an entity and its accident history."[(10)] If the basis of site selection was those sites with the largest number of crashes in a single year, then it is possible that subsequent crash reductions would be erroneously attributed to the treatment when in fact such reductions were truly the result of random variation that would have transpired even without the treatment.[†] Two characteristics of this study likely eliminate the possibility of regression to the mean bias. First, the sites studied were generally not chosen by persons with an interest in testing the effects of USL and DSL; rather, the sites were those that had available data. Second, the sites did not show behavior expected in a study where regression to the mean occurs. Instead of seeing dramatic crash reductions, States generally saw crash increases. For the rate-based method, the increases were often not significant and for the modified empirical Bayes approach, the increases were sometimes significant. Had the bias been present, researchers would have expected to see crash *reductions* in lieu of increases.

Finally, the crash estimation model used only two variables, AADT and section length. This could have been the result of too few variables, in case other factors, such as the number of interchanges per mile, could have influenced crash rates. Other variables besides AADT and section length also may have been relevant. To mitigate the impact of this last problem, goodness of fit tests were conducted for the crash estimation model for total crashes for Virginia, as shown in figures 9 and 10. Based on the Virginia results, this model formulation was applied for other

[†] In practice, RTM occurs when an agency identifies, based on a short period of data, the most unsafe locations and then makes subsequent engineering modifications to those locations. Since crash data have an element of probability, choosing the worst locations based on a short period of data may mean that investigators simply have identified sites that, at random, happened to exhibit a large number of crashes in a given year and would probably exhibit a lower number of crashes the following year, *even if no change were made*. Investigators seek to avoid this bias by selecting sites that have good and bad crash records and studying those accordingly with a new treatment.

States. However, additional data elements, such as the number of interchanges per mile, might have helped reduce the amount of noise used throughout this analysis.

General Caveats

There are six additional limitations that, unlike the concept of the comparison reference groups cited above, apply to all three sets of results: speed, crashes, and crash rates. As was the case with the limitations described for the application of the modified empirical Bayes method, these caveats arise because of the limited data available. Firstly, the sample size varied by State. For example, there may be less certainty for the North Carolina crash results (based on 26 sites) being representative of North Carolina than that of the Arizona crash results (based on 278 sites) being representative of Arizona. For the empirical Bayes method, Washington, which maintained a differential speed limit, comprised the only State in policy group 2 with nine sites. Secondly, the selected sites may be an unbiased sample; however, the investigators cannot control site selection; their randomness is a function of how individual States set up their individual speed monitoring programs. Thirdly, the durations used in this study are relatively short. Certainly 3 years is normal for a before/after study, but some of these States, notably Idaho, only had a speed limit in effect for 1 or 2 years. Fourthly, the rural interstates were analyzed at an annual level of detail, without stratification by time period or season. Fortunately, since congestion is usually lower for rural interstates, this annual approach should not have been as significant a problem as it would have been for urban interstates. Next, the speed data shown are based on all vehicles, not just trucks. Except for speed variance, researchers ideally want to know how truck speeds, not just all vehicle speeds, were affected by differential versus uniform speed limit policy. Similarly, when looking at truck-involved crashes, investigators would want to be able to delineate between crash types that are directly affected by DSL versus USL policy (e.g., car-into-truck collisions) and those that are indirectly affected by such a policy (e.g., truck-into-truck collisions).[27]

Finally, the sample size used in the statistical tests associated with the speed analysis was defined as the number of speed monitoring sites. Although the investigators decided that this was appropriate to determine whether speed changes were meaningful, an argument can be made that the sample size should have been estimated as the number of vehicles at all of the sites combined. Acceptance of this latter view would give significance to many of the statistical tests shown in table 11 as insignificant, although there still would be no clear pattern as to the effect of DSL versus USL for speed. (A minor item is the need to make some assumptions regarding the underlying vehicle distribution since individual vehicle speeds were not available; appendix E discusses this issue further.) An extension of this argument arises in the computation of confidence intervals for the 85th percentile speed; hence, theoretical issues associated with determining significant differences in 85th percentile speed are discussed in appendix F but are not believed to influence the outcome of this study.[(28)]

CONCLUSIONS

The original purpose of this study was to compare the safety impacts of DSL and USL, with safety impacts being assessed through crashes and speeds, and to that extent, findings may be presented across both those areas. An unforeseen outcome of this study, however, was to identify considerations in the application of the empirical Bayes methodology, both with respect to the formulation of the crash estimation model and the types of data necessary to conduct the analysis.

Safety Impacts of DSL Versus USL

1. *Speed characteristics were generally unaffected by a DSL versus USL policy*. Except for Virginia, the mean, 85th-percentile, and median speeds tended to increase over the 1990s regardless of whether the State maintained USL, maintained DSL, or changed from one to the other. In some cases, the difference was significant; in other cases, it was not.

2. *Crash rates*, *when compared using conventional statistical methods, did not show an obvious relationship to the type of speed limit chosen*. When States were stratified into four policy groups (USL, DSL, shift from USL to DSL, and vice versa), the changes in crash rates and crash rate types did not all correspond to one group.

3. *Actual number of crashes, when compared using the modified empirical Bayes approach indicated that the percentage of increase of the fatal actual crashes over that for the would-have-been fatal crashes was higher for the States that changed from USL to DSL than for Virginia, which changed from DSL to USL*. On the other hand, the same empirical Bayes approach showed that the percentage increase of the total actual crashes over the would-have-been total crashes was higher for the States that maintained USL than for the State that maintained DSL.

4. *Measurable variation within speeds and crash rates by year and by State may confound any statistical tests employed*. The performance of Illinois annual speed variances, as shown in figure 17, is indicative of the noise associated with random variation, where the annual speed variance has an insignificant but observable upwards and downwards trend despite the fact that Illinois made no policy changes to its speed limits.

Methodological Findings

1. In this study, most of the crash estimation models developed by the investigators for the modified empirical Bayes approach were very sensitive to changes in ADT. In most cases for this study, as time passed, the actual number of crashes for the after period was larger than the predicted would-have-been crashes. The investigators believe this to be the result of the less than unity exponent associated with the equation in figure 8.

2. Even within a single State, different trends are observed on different rural interstates. Examination of figure 19, for example, suggests that the between-interstate variation adds quite a bit of noise to any attempt to study statewide temporal trends. The reality may be that DSL and USL have different safety impacts, as might be implied by an imagined extrapolation of the shaded Idaho data in table 11 (beyond 1998) or the Virginia mean speed downward trend in figure 26 (beyond 2000 to 2001). This particular study, however, could not identify such a difference. It may be the case, therefore, that a detailed crash-by-crash evaluation at a few interstate segments may be able to reveal a more precise analysis, especially after a few more years of data have been collected at States such as Idaho that have recently made changes form USL to DSL.

3. The proportionality of crashes to ADT has been questioned but not resolved. The crash estimation models developed for this study showed a nonlinear effect of crashes to ADT, such that it was often the case that a large increase in ADT would, according to the crash estimation models, yield a small increase in the number of crashes. For that reason, investigators can state that a proportional relationship between crashes and ADT, which is presumed in the use of crash rates, is *questionable*. Yet, the same approach yielded contradictory results on a State-by-State basis. Some CEMs predicted, for example, that one policy adversely affected safety, whereas others predicted that the opposite policy adversely affected safety. For this reason, the highly nonproportionate effect of crashes to ADT may be overstated by the CEMs developed for this study—but these results cannot prove or disprove that assessment. Thus, because of the contradictory nature of these CEMs, the proportionality of crashes to ADT is *not resolved*.

4. To assist decisionmakers under conditions of sparse data availability, it may be productive to analyze safety impacts through both the traditional method of crash rates and the newer method of the empirical Bayes (or modified empirical Bayes) approach. If the aggregate results from the two methods are the same, then results can be presented using the method that is more familiar. If the aggregate results from the two methods are contradictory, then researchers have an indication that there may be a flaw in the analysis. For example, consider the States analyzed in this study:

    - Application of the modified empirical Bayes method showed no consistent trend among the States. Regardless of speed limit policy change, table 24 suggests a statistically significant increase in crashes for each State. The aggregate result of the modified empirical Bayes method, therefore, is that, because the State-by-State results are inconsistent, *the modified empirical Bayes method does not show that changes in speed limit policy have a statistically significant impact on crashes*.

    - Application of the traditional crash rate method, shown in table 12, also indicates that most State-by-State results were statistically insignificant. In the few cases where a statistically significant rate was found, results were not consistent among States in the same group. For example, one State that maintained USL saw a significant increase, whereas the other two States that maintained USL did not see a significant increase. The aggregate results of the traditional crash rate method may be stated thus: "Because most States saw insignificant changes in the crash rate and because there was no discernible pattern in the States that did have a significant change in the crash rate*, the traditional crash rate method does not show that the changes in speed limit policy have a statistically significant impact on crash rates*."

Because the two bulleted methods show the same conclusions, the investigator may have greater confidence that the analysis is correct than would be the case had the two bulleted methods yielded contradictory conclusions.

APPENDIX A. EXAMPLES OF DATA COLLECTION LETTERS AND PROCESSING

Appendix A shows the initial data request letter.

Dear Mr. Baldwin,

As I mentioned on the phone, the Virginia Transportation Research Council is working with the Federal Highway Administration to evaluate the potential safety impacts of differential speed limits for cars and trucks on interstate facilities. I would like to request your assistance with obtaining crash and speed data (on interstate highways) that can help us with this study. While we already have some limited data for the sites shown at the bottom of this letter, I'd like to obtain some additional data pertaining to these and other sites.

I realize that obtaining data can be time consuming so I am certainly willing to do whatever is possible to make it easier for you to fulfill our request. If at all possible, we would like to obtain the following crash and speed data in an electronic format. The crash data and the speed data may come from the same sites or they may come from different sites, whichever is easier (but all should be on interstate highways, with the speed limit shown. At sites with differential speed limits please list the speed limits for passenger cars and trucks separately).

For each year from 1991 to 2000, we would like to obtain the following speed data elements at each site:

1. Average speed.
2. Individual vehicle speeds or speed bins (e.g., **x** vehicles between 51-55 mph, **y** vehicles 56-60 mph, etc.).
3. Individual truck speeds or average truck speeds (if available).
4. Individual car speeds and average car speeds (if available).
5. Critical geometric data such as:
    a. The number of lanes
    b. The number of interchanges (or the number of interchanges per mile)

The **speed data sites** that interest us are these plus any additional sites you recommend.

Route 29	Northbound	Milepost 29
Route 35	Northbound	Milepost 14

For each year from 1991 to 2000, I would also like to obtain the following crash data elements:

1. Total number of crashes
    a. All crashes that do not involve trucks.
    b. All crashes that do involve trucks.
    c. All fatal crashes (regardless of truck involvement).
    d. All fatal crashes (of truck involvement).
    e. Total number of rear-end crashes that do involve trucks.
    f. Total number of rear-end crashes that do not involve trucks.

The crash sites should be the same from year to year, and can either be the sites shown above or be different from the speed sites. Each crash site should be a homogeneous section that can show some crashes (e.g., whether a crash site is 1 mi long or 10 mi long, it should be (a) big enough to obtain some crashes annually, yet (b) small enough such that speeds and geometric characteristics for the site are homogeneous). We would like, if possible, up to **10 crash sites altogether.**

If possible, we would like the data to be in the following format. But we are also happy to have any format of data you sent to us.

Route	Year	Beginning Mile Post	Ending Mile Post	AADT	Total Crash	Fatal Crash	Rear -End	Truck Total	Truck Fatal	Truck RE	Mean Speed	85th Speed

Finally, I would like to confirm that you have had since 1991 a uniform limit, that is, the same speed limit for cars and trucks.

Again, I sincerely appreciate your assistance. I would also be delighted to provide you with additional information about the purpose of this study, as well as any findings that result.

APPENDIX B. EXAMPLE OF A CLARIFYING DATA REQUEST LETTER

Appendix B shows an example of a follow up letter providing clarification.

Dear Mr. Wyatt,

Thank you very much for the data you sent to us! Once again we have another question. The accident data we got from you are from 1991 to 2000 as shown below

I77, From Future I-74 to Va. State Line
I77,From Yadkin Co. to Future I-74
I85,Randolph
I85,Vance
I40,Johnston
I40,Duplin
I40, Pender
I-95, Halifax
I-95,Nash
I-95,Wilson
I-85,Granville

[The names above refer to counties.]

I have four quick questions:

1. Can we get the ADT by year, from 1991 to 2000, for the accident sites shown above?

2. Do you have any crash data for:
   (a) Rear-end crashes involving trucks, and
   (b) fatal crashes involving trucks?

3. When tabulating crashes, some States include only crashes on the mainline, and others include ramp crashes as well (regardless of causality). For North Carolina, which should we assume? If it is the case that ramp crashes are included, then do you know roughly how the number of ramp crashes compares to the number of mainline crashes?

4. Is the speed limit for cars and trucks 65 on North Carolina interstates, and has the limit changed since 1990?

APPENDIX C. CONFIRMATION OF THE NEGATIVE BINOMIAL DISTRIBUTION TO CRASH DATA

As stated in the literature, the assumption in the application of the empirical Bayes formulation as done herein is that for a particular site *i*, the distribution of the number of crashes $K_{i,y}$ over the years *y* obeys the Poisson distribution. Further, for a particular year *y*, the distribution of the number of crashes $K_{i,y}$ between different *i* sites follows the negative binomial distribution. Based on these two assumptions, the expected number of crashes of a group m_{i1} are Gamma distributed.[10,11,12, 13] Figure 33 illustrates these concepts, where $K_{i,y}$ is the actual crash count for site *i* and year *y* and $m_{i,y}$ is the expected crash counts for site *i* and year *y.*

Site	**1991**		**1992**		**1993**		**1994**		**1995**		**1996**	
	K_{11}	m_{11}	K_{12}	m_{12}	K_{13}	m_{13}	K_{14}	m_{14}	K_{15}	m_{15}	K_{16}	m_{16}
2	K_{21}	m_{21}	K_{22}	m_{22}	K_{23}	m_{23}	K_{24}	m_{24}	K_{25}	m_{25}	K_{26}	m_{26}
3	K_{31}	m_{31}										
4	K_{41}	m_{41}										
5	K_{51}	m_{51}										
6	K_{61}	m_{61}										

K_{1y} : Poisson

K_{i1}: Negative Binomial

m_{i1}: Gamma

Figure 33. Chart. Relationship between the Poisson and negative binomial distributions for crash frequencies.

The Poisson and negative binomial distributions were tested with the data sets for selected states as described herein.

Verification of the Poisson Distribution

Using data from Virginia and Arizona, two techniques were used to verify that the Poisson distribution is appropriate. Firstly, theoretical versus actual frequencies were compared graphically. Secondly, the chi-square test was used to determine whether a statistically significant difference existed between the actual and theoretical distributions for the K_{iy} over time.

Figure 34 compares the actual crash frequency distribution and the Poisson distribution using one site on Interstate 85 in Virginia between milepost 19.52 and milepost 24.73, looking at the annual number of crashes between 1991 and 1999.

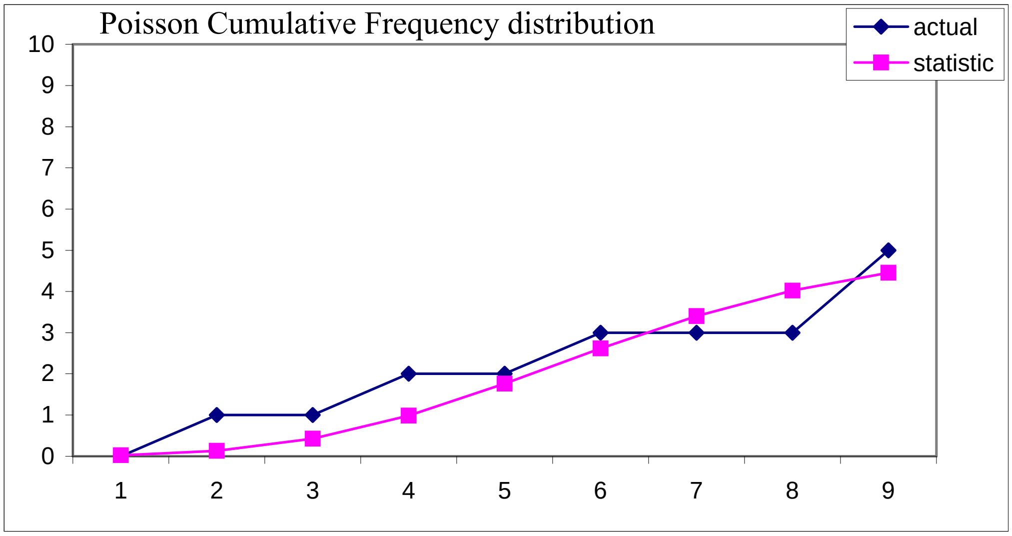


Figure 34. Chart. Comparison of Poisson distribution and actual crash distribution.

Table 27 shows that the calculated χ^2 value is less than the critical (tabulated) χ^2 value, which means that the assumed distribution is accepted. Theoretically, the computed chi-square value (which represents error in, or divergence from, the Poisson distribution) is less than the tabulated chi-square value; therefore, the hypothesis that the distributions are different cannot be proven at the 5 percent confidence level.

Table 27. Poisson validation description and results using the total crashes at four test sites.

State	Test Sites	Sample Size	Data	$\chi^2_{calculated}$	$\chi^2_{sta,\ 0.05}$	$\chi^2_{sta,\ 0.01}$
Arizona	I-8 mp 42.06 to 54.96	10	1991–2000	9.530	14.07	18.47
Arizona	I-10 mp 19.79 to 26.65	10	1991–2001	6.738	15.51	20.08
Virginia	I-85 mp 19.52 to 24.73	5	1995–1999	8.764	11.07	15.09
Virginia	I-81 mp 206.04 to 213.48	6	1995–2000	6.468	7.82	11.33

Verification of the Negative Binomial Distribution

A similar procedure was used to test the validity of the negative binomial distribution, except that crash rates as defined in figure 3 rather than the total number of crashes, was used to as the variable of interest. Table 28 highlights the result of the chi-square test and visual inspection of figure 35 suggests that the negative binomial distribution is appropriate for these data. (Crash rates rather than the number of crashes was used because of variation in the section lengths.)

Table 28. Negative binomial validation description and results.

State	Year	Sample Size	Test Site	Crash Type	χ^2_{cal}	$\chi^2_{sta, 0.05}$	$\chi^2_{sta, 0.01}$	Result at 5% Level	Result at 1% Level
VA	1991	91	85n,95n,81n	total	42.326	44.8	60.1	Yes	yes
VA	1992	90	85n,95n,81n	total	14.516	19.68	24.75	Yes	yes
VA	1993	91	85n,95n,81n	total	10.730	20.08	14.07	Yes	yes
VA	1995	117	85, 95, 81n	total	21.386	22.37	27.72	Yes	yes
VA	1996	116	85, 95,81n	total	18.153	18.31	23.91	Yes	yes
VA	1997	116	85, 95,81n	total	14.135	19.68	24.76	Yes	yes
VA	1999	84	85n, 85s, 81n	total	29.852	27.59	33.44	no(close)*	yes
NC	1993	26	40,95,77,85	total	4.005	6	9.22	Yes	yes
NC	1999	25	40,95,77	total	7.385	11.07	15.09	Yes	yes
ID	1992	32	84,86,90,15	total	28.327	38.89	45.67	Yes	yes
ID	1994	32	84,86,90,16	total	28.305	37.66	44.34	Yes	yes
ID	1999	32	84,86,90,16	total	36.322	42.57	49.61	Yes	yes
ID	2000	32	84,86,90,16	total	26.209	32.68	38.96	Yes	yes
AZ	1991	277	8,10,15,17,19,40	total	24.151	21.03	26.25	no(close)*	yes
AZ	1993	277	8,10,15,17,19,40	total	20.392	23.37	27.71	Yes	yes
AZ	1994	278	8,10,15,17,19,40	total	17.697	26.3	32.03	Yes	yes
AZ	1995	277	8,10,15,17,19,40	total	21.232	22.37	27.71	Yes	yes
AZ	1996	278	8,10,15,17,19,40	total	19.977	22.37	27.71	Yes	yes
AZ	1998	279	8,10,15,17,19,41	total	21.164	22.37	27.71	Yes	yes
AZ	1999	280	8,10,15,17,19,42	total	20.149	23.69	29.17	yes	yes
AZ	2000	281	8,10,15,17,19,43	total	24.187	27.59	33.44	yes	yes

*The significance level of a chi-square test is actually a proof that a theoretical distribution does not fit the data. Thus, if a calculated chi-square value is sufficiently large such that it exceeds the 5 percent chi-square value, then it can be said that "researchers are 95 percent certain that the two distributions are different." In the two rows with asterisks, there is a 95 percent certainty that the two distributions are different but not 99 percent certain. In all other cases, it cannot be proved at the 95 percent level that the theoretical and actual distributions are different; therefore, it is presumed they are the same.

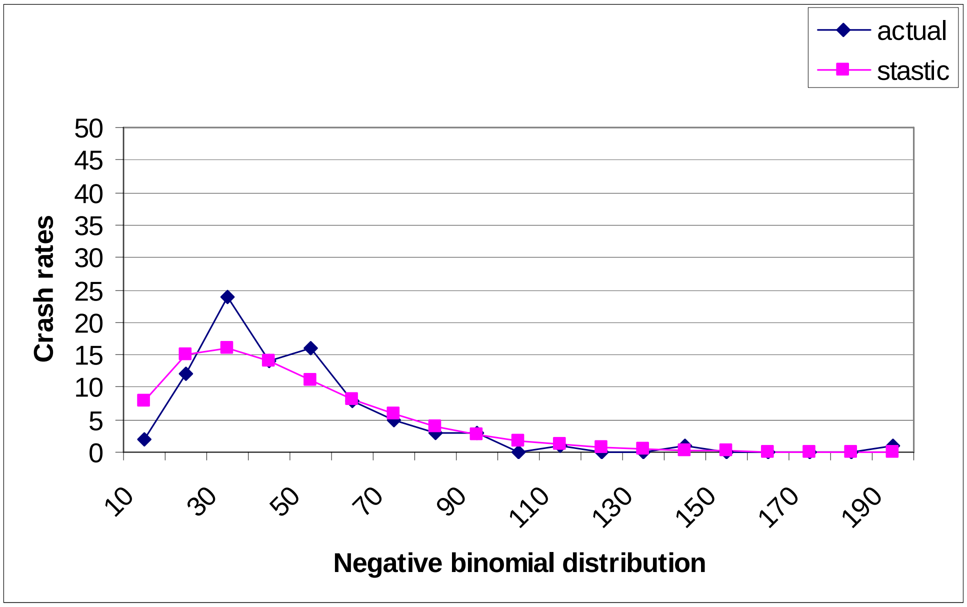


Figure 35. Chart. comparison of negative binomial distribution and actual crash distribution (probability density function).

APPENDIX D: EFFECT OF CHANGING THE BASE YEAR IN $C_{I,Y}$

Figure 12 showed that the ratio of the expected crash frequency for a given year *y* to the expected crash frequency during the "base" year is given as the expression $C_{i,y}$. The question may arise as to whether the use of a different base year would significantly influence the results. Both a mathematical derivation and a data-driven experiment suggest that the selection of the base year will not influence the analysis.

In the derivation that follows, the ratio using the first year as base in the denominator as $C_{i,y}^{1}$, while the ratio with the third year as base in the denominator is $C_{i,y}^{3}$. Thus, the equation in figure 12 may be rewritten for each case as:

$$\frac{m_{i,y}}{m_{i,1}} = \frac{E(m_{i,y})}{E(m_{i,1})} = C_{i,y}^{1}$$

Figure 36. Equation. Crash frequency for year 1 as base year.

$$\frac{m_{i,y}}{m_{i,3}} = \frac{E(m_{i,y})}{E(m_{i,3})} = C_{i,y}^{3}$$

Figure 37. Equation. Crash frequency for year 3 as base year.

Using the First Year as the Base Year

If the first year is used as a base, the expected value of the first year crash count is estimated first as following:

$$m_{i,1} = \frac{k + \sum_{y=1}^{Y} K_{i,y}}{\frac{k}{E(m_{i,1})} + \sum_{y=1}^{Y} C_{i,y}^{1}}$$

Figure 38. Equation. Expected value of crash count for year 1.

$$VAR(m_{i,1}) = \frac{k + \sum_{y=1}^{Y} K_{i,y}}{\left(\frac{k}{E(m_{i,1})} + \sum_{y=1}^{Y} C_{i,y}^{1}\right)^2} = \frac{m_{i,1}}{\frac{k}{E(m_{i,1})} + \sum_{y=1}^{Y} C_{i,y}^{1}}$$

Figure 39. Equation. Variance of expected value of crash count for year 1.

The estimation of the expected values of crash counts of the other years was then calculated by multiplying the first year expected estimation of its changing ratio using the following expressions.

$$m_{i,y} = C^{1}_{i,y} m_{i,1} = \frac{E(m_{i,y})}{E(m_{i,1})} m_{i,1}$$

Figure 40. Equation. Estimation of estimated values of crash counts for year 1.

$$VAR\ (m_{i,y}) = (C^{1}_{i,y})^{2} m_{i,1} = [\frac{E(m_{i,y})}{E(m_{i,1})}]^{2} m_{i,1}$$

Figure 41. Equation. Variance of estimation of estimated values of crash counts for year 1.

For example, applying these equations for the third year expected value yields the following equation in figure 42.

$$\mathrm{m_{i,3}} = \mathrm{C_{i,3}}\ \mathrm{m_{i,1}} = \frac{E(m_{i,3})}{E(m_{i,1})} \mathrm{m_{i,1}}$$

$$= \frac{E(m_{i,3})}{E(m_{i,1})} \frac{k + \sum_{y=1}^{Y} K_{i,y}}{\frac{k}{E(m_{i,1})} + \sum_{y=1}^{Y} C^{1}_{i,y}} = \frac{E(m_{i,3})}{E(m_{i,1})} \frac{k + \sum_{y=1}^{Y} K_{i,y}}{\frac{k}{E(m_{i,1})} + \sum_{y=1}^{Y} \frac{E(m_{i,y})}{E(m_{i,1})}}$$

$$= E(m_{i,3}) \frac{k + \sum_{y=1}^{Y} K_{i,y}}{E(m_{i,1})[\frac{k}{E(m_{i,1})} + \sum_{y=1}^{Y} \frac{E(m_{i,y})}{E(m_{i,1})}]}$$

$$= E(m_{i,3}) \frac{k + \sum_{y=1}^{Y} K_{i,y}}{k + \sum_{y=1}^{Y} E(m_{i,y})}$$

$$= \frac{k + \sum_{y=1}^{Y} K_{i,y}}{\frac{k}{E(m_{i,3})} + \sum_{y=1}^{Y} \frac{E(m_{i,y})}{E(m_{i,3})}} = \frac{k + \sum_{y=1}^{Y} K_{i,y}}{\frac{k}{E(m_{i,3})} + \sum_{y=1}^{Y} C^{3}_{i,y}}$$

Continues on next page.

$$VAR(m_{i,3}) = (C_{i,3}^{1})^2 VAR(m_{i,1}) = [\frac{E(m_{i,3})}{E(m_{i,1})}]^2 VAR(m_{i,1})$$

$$= [\frac{E(m_{i,3})}{E(m_{i,1})}]^2 \frac{k + \sum_{y=1}^{Y} K_{i,y}}{[\frac{k}{E(m_{i,1})} + \sum_{y=1}^{Y} C_{i,y}^{1}]^2} = [\frac{E(m_{i,3})}{E(m_{i,1})}]^2 \frac{k + \sum_{y=1}^{Y} K_{i,y}}{[\frac{k}{E(m_{i,1})} + \sum_{y=1}^{Y} \frac{E(m_{i,y})}{E(m_{i,1})}]^2}$$

$$= [E(m_{i,3})]^2 \frac{k + \sum_{y=1}^{Y} K_{i,y}}{[E(m_{i,1})]^2 [\frac{k}{E(m_{i,1})} + \sum_{y=1}^{Y} \frac{E(m_{i,y})}{E(m_{i,1})}]^2}$$

$$= [E(m_{i,3})]^2 \frac{k + \sum_{y=1}^{Y} K_{i,y}}{[k + \sum_{y=1}^{Y} E(m_{i,y})]^2}$$

$$= \frac{k + \sum_{y=1}^{Y} K_{i,y}}{[\frac{k}{E(m_{i,3})} + \sum_{y=1}^{Y} \frac{E(m_{i,y})}{E(m_{i,3})}]^2}$$

$$= \frac{k + \sum_{y=1}^{Y} K_{i,y}}{[\frac{k}{E(m_{i,3})} + \sum_{y=1}^{Y} C_{i,y}^{3}]^2}$$

Figure 42. Equation. Expected value of crash count for year 3.

Using the Third Year as the Base Year

If the third year is used as a base in the denominator, then the expected value of the third year crash count was estimated first as following:

$$m_{i,3} = \frac{k + \sum_{y=1}^{Y} K_{i,y}}{\frac{k}{E(m_{i,3})} + \sum_{y=1}^{Y} C_{i,y}^{3}}$$

Figure 43. Equation. Expected value of crash count, year 3 as base year.

$$VAR(m_{i,3}) = \frac{k + \sum_{y=1}^{Y} K_{i,y}}{\left(\frac{k}{E(m_{i,1})} + \sum_{y=1}^{Y} C_{i,y}^{3}\right)^2} = \frac{m_{i,1}}{\frac{k}{E(m_{i,1})} + \sum_{y=1}^{Y} C_{i,y}^{3}}$$

Figure 44. Equation. Variance of expected value of crash count, year 3 as base year.

Comparing the two results, it is evident that the expected crash result using the third year as a base year is the same as that which would be calculated using the first year as a base year and then multiplying this by the $C_{i,y}$ ratio for third year.

As an empirical example, a 9.8-km (6.13-mi) section of Interstate 64 East in Virginia was selected, and the crash estimation model was established. The results obtained from using the 1991 as the first year in the denominator of $C_{i,y}$ is shown in table 29, and the results from using the 1993 as the denominator of $C_{i,y}$ are shown in table 30. The results are identical.

Table 29. Estimation of expected crashes using 1991 data as a base in the $C_{i,y}$ ratio.

Year	$K_{i,y}$	$E_{i,y}$	$C^{1}_{i,y}$	$m_{i,y}$	$VAR(m_{i,y})$
1991	7	11.454	1	7.4	2.137
1992	4	10.372	0.906	6.701	1.753
1993	9	12.21	1.066	7.888	2.429
1994	—	—	—	—	—
1995	13	11.35	0.991	7.332	2.099
1996	15	11.35	0.991	7.332	2.099
1997	6	12.428	1.085	8.029	2.516
1998	10	12.94	1.13	8.36	2.728
1999	10	13.435	1.173	8.68	2.941

Table 30. Estimation of expected crashes using 1993 data as a base in the $C_{i,y}$ ratio.

Year	$K_{i,y}$	$E_{i,y}$	$C^3_{i,y}$	$m_{i,y}$	$VAR(m_{i,y})$
1991	7	11.454	0.938	7.4	2.137
1992	4	10.372	0.849	6.701	1.753
1993	9	12.21	1	7.888	2.429
1994	—	—	—	—	—
1995	13	11.35	0.93	7.332	2.099
1996	15	11.35	0.93	7.332	2.099
1997	6	12.428	1.018	8.029	2.516
1998	10	12.94	1.06	8.36	2.728
1999	10	13.435	1.1	8.68	2.941

However, as is the case with any data set, it is always possible that a single year could be an outlier. Thus, it should be clarified that appendix D only tests the effect of changing the $E(m_{i,1})$ shown in the denominator from year 1 to another year. It does not test for the effect of removing 1991 from the data set entirely.

APPENDIX E. DETERMINING THE SAMPLE SIZE

Although the speeds of individual vehicles are not available, mean speeds from speed monitoring stations in Idaho are shown in table 31 below. For example, in 1991, vehicle speeds were monitored at 24 sites in Idaho, where each site measured an unknown number of vehicles. The mean speeds from these 24 sites were 104 km (64.66 mi/h).

Table 31. Summary Idaho data from speed sampling sites.

Year	Number of Sites	Mean Speed from the Sites
1991	24	64.66
1992	36	64.89
1993	36	65.61
1994	27	65.37
1995	36	65.59
1996	37	68.45
1997	36	70.92
1998	32	71.01
1999	38	70.81

Test for Significant Differences Using the Sample Size as the Number of Sites

Visually, figure 1 generally shows an upward trend in speeds. Statistically, however, there is not always a significant different in *mean* speeds of the individual site means. For example, compare the two shaded rows that contrast 1991 and 1995. With $N_x = 24$, $N_y = 36$, and $U_x - U_y = 65.59 - 64.66$, there is no statistically significant difference as measured by the equation in figure 45. The logical conclusion is that there is no statistically significant difference in the mean speed of speed sampling site means.

$$Is \; \left|U_x - U_y\right| > 1.96\sqrt{\frac{S_x^2}{n_x} + \frac{S_y^2}{n_y}} \; ?$$

Figure 45. Equation. Statistically significant difference in mean speeds.

Test for Significant Differences Using the Sample Size as the Number of Vehicles

While the last sentence in the above paragraph is correct, it is not very meaningful in practice. The problem is that the sites shown in figure 45 (e.g., the 24 sites that comprise the initial 1991 year) are not individual speeds. Rather, they are means of individual vehicles. That is, the 24 sites from 1991 do not represent 24 vehicles. Rather, they represent far more vehicles. Unfortunately, there are two pieces of information that prevent researchers from performing an exact statistical test on the means of individual speeds:

- The number of vehicles represented by each site is unknown.
- The variation (or even the distribution) of those vehicles at each site is unknown.

Simulating the Speed Variances

To estimate the types of individual speed distributions that might give rise to those shown in figure 45, the speeds of individual vehicles were simulated for two hypothetical sites. Each site initially had 1,365 speeds generated within a predefined range. Because normality could not be guaranteed at the site, the individual vehicle speeds were generated in the form of a nonnormal distribution. Figure 46 shows a histogram for the individual speeds at site X and site Y, with each bin being 1.6 km/h (1 mi/h) in width.

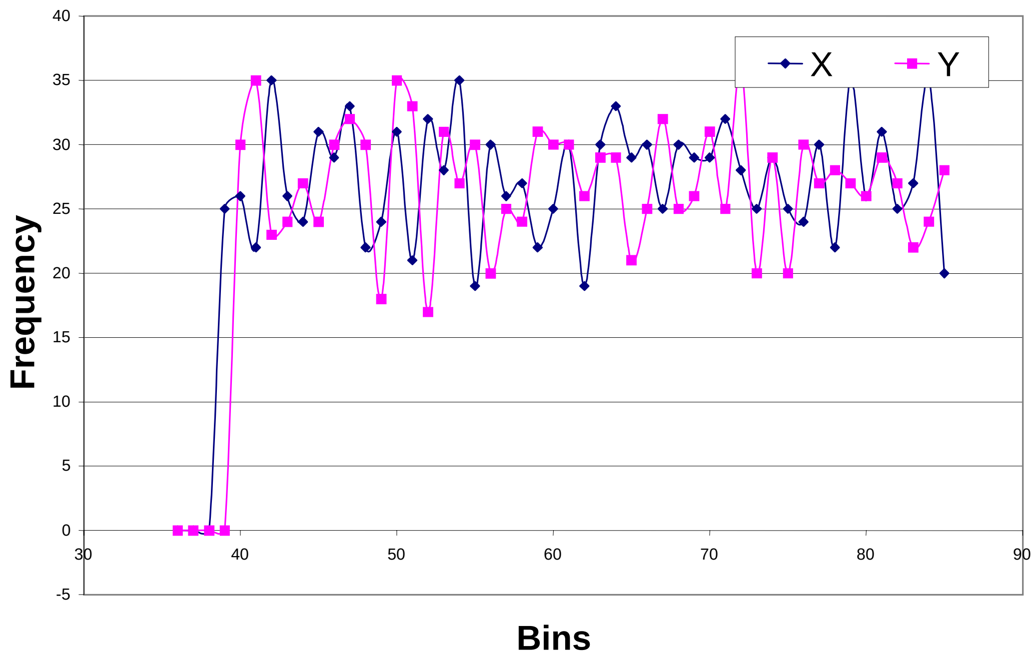


Figure 46. Chart. Histogram based on random numbers.

This simulation showed standard deviations of 14.61 when 25 speeds were used and standard deviation of 14.31 when 1,365 speeds were used. Thus, for a normal distribution, the standard deviation would likely be lower.

Determining the Difference in Mean Speeds Needed to Show Significant Difference Based on the Number of Vehicles

Returning to figure 3 and using the larger standard deviation of 14.61, researchers may ask at what point a difference in mean speeds leads to a significant difference. Table 32 summarizes these results. With just 100 vehicles, for example, a difference of 3.94 mi/h is required to show a significant difference, whereas with 10,000 vehicles, the difference need be only 0.62 km/h (0.39) mi/hour.

Table 32. Sample sizes required to achieve significant differences.

n	Difference (km/h)	Difference (mi/h)
100	6.34	3.94
200	4.49	2.79
300	3.66	2.27
1000	2.01	1.25
2000	1.42	0.88
3000	1.16	0.72
5000	0.90	0.56
10000	0.63	0.39
20000	0.20	0.28

Researchers can compare these data to the number of vehicles available for each State, summarized as follows:

Idaho: Between 5,874 and 19,255 vehicles per site.
Illinois: No ADT data.
Indiana: Between 3,978 and 23,000 vehicles per site.
Iowa: No ADT data.
Virginia: Between 10,312 and 20,071 vehicles per site.

Thus, if researchers were to base the sample size on the number of vehicles, the results for Indiana and Virginia would change from an insignificant difference to a significant difference, as noted in the body of the report.

Discussion

This analysis considers only statistical significance, which is a different issue from practical significance. In fact, table 32 may be carried to its illogical extreme in the sense that a sample size of 100,000 would mean that a mean speed difference of about one tenth of one mi/h indicated a statistically significant difference. Yet, intuitively, researchers would argue that such a difference is not meaningful because the difference is so tiny that it cannot be readily observed. As researchers extend table 32 to a larger number of vehicles, there comes a point at which the test of statistical significance is not a useful indicator for what is being measured: whether a change in speeds has significant meaning. The question becomes, then, at what point does that occur? One way to address this issue is to realize that by using the number of sites as the sample size *n*, the researchers prevent themselves from reaching this point. In short, using the number of sites gives a practical way of ensuring that statistically significant differences have a practical meaning. Another way of summarizing this issue is to state that by using the number of sites as *n*, researchers raise the bar for the test of significant differences.

APPENDIX F: THEORETICAL CONSIDERATIONS IN THE COMPUTATION OF CONFIDENCE INTERVALS FOR THE 85TH PERCENTILE SPEED

It was informally suggested to the investigators that a question arises as to how to determine the confidence intervals for the 85th percentile speed.[(28)] The team considered this question and developed the following derivation. To determine confidence intervals associated with a mean speed, for example, investigators generally use the formula shown in figure 47:

$$\bar{x} \pm \frac{zs}{\sqrt{n}}$$

Figure 47. Equation. Formula to determine confidence intervals associated with mean speed.

Thus, for a sample of *n* = 200 vehicles, a standard deviation of 4.82 km/h (3 mi/h), a mean speed of 80.5 km/h (50 mi/h), the 95 percent confidence interval becomes:

$$50 \pm \frac{(1.96)(3)}{\sqrt{200}} = 49.58 \text{ to } 50.42 \text{ mi/h}$$

Figure 48. Equation. Example of formula in figure 47.

Suppose for an instant that speeds are represented perfectly by the normal distribution, such that the 85th percentile speed is 85.4 km/h (53.11 mi/h). If the investigator applies this same equation to compute a 95 percent confidence interval for that 85th percentile speed, then the investigator computes the confidence interval as being:

$$53.11 \pm \frac{(1.96)(3)}{\sqrt{200}} = 52.69 \text{ to } 53.53 \text{ mi/h}$$

Figure 49. Equation. Confidence interval for 85th percentile speed.

If the investigator can assume that the central limit theorem still applies to the question of determining an 85th percentile speed, then figure 49 should still be applicable. However, it may be the case that the investigator cannot presume that the central limit theorem will hold at relatively small *n*; for example, it may be the case that at the upper tail of the normal distribution, the odds of observing a vehicle are relatively small. One way to explore the implications of this is to use the binomial distribution, where the investigator says that there are two groups of speeds: group 1 (the vehicles traveling below the 85th percentile speed) and group 2 (the vehicles traveling above the 85th percentile speed). Thus, assuming an *observed* 85th percentile speed of 85.4 km/h (53.11 mi/h), a vehicle has an 85 percent probability of being in group 1 and a 15 percent probability of being in group 2. The investigator can establish 95 percent confidence bounds for the binomial distribution using figure 50.

$$p \pm z\sqrt{\frac{p(1-p)}{n}} = 0.85 \pm 1.96\sqrt{\frac{0.85(1-0.85)}{200}} = 0.8005 \text{ to } 0.8995$$

Figure 50. Equation. Binomial distribution.

The interpretation of figure 50 is that while investigators may guess that about 0.8500 of the vehicles fall into group 1, they are 95 percent confident that at least 0.8005 of the vehicles fall into this group and that no more than 0.8995 of the vehicles fall into this group. The investigators can then map each of these proportions shown in figure 50 back to the normal distribution. For example, the researcher finds that the value on the normal distribution curve corresponding to a cumulative frequency of 0.8005 of the vehicles (assuming a mean of 80.5 km/h (50 mi/h) and a standard deviation of 4.8 km/h (3 mi/h)) is 84.5 km/h (52.53 mi/h). Similarly, the investigator finds that the value on the normal distribution curve corresponding to a cumulative frequency of 0.8995 of the vehicles is 86.6 km/h (53.84 mi/h). Thus, the 95 percent confidence bounds are 84.5 km/h (52.53 mi/h) to 86.6 km/h (53.84 mi/h) and shaded in table 33 for a sample size of 200.

It is apparent that the bounds from the binomial assumption (84.5 km/h (52.53 mi/h) to 86.6 km/h (53.84 mi/h)) are wider than the range based on the normal assumption in figure 49, presuming a sample size of $n = 200$. Table 33 shows that as n increases, the range given by the normal assumption and the binomial assumption become very similar.

Table 33. 95 Percent confidence intervals for the 85th percentile speed.*

	***n* = 30**		***n* = 200**		***n* = 2000**	
	Lower Bound	**Upper Bound**	**Lower Bound**	**Upper Bound**	**Lower Bound**	**Upper Bound**
Normal Assumption	52.04	54.18	52.69	53.53	52.98	53.24
Binomial Assumption	51.77	56.03	52.53	53.84	52.91	53.32

***Assumes a mean speed of 80.5 km/h (50 mi/h) and a standard deviation of 4.8 km/h (3 mi/h).

Examination of table 33 shows that if the "binomial assumption" based on figure 52 is correct, yet the investigator uses the normal assumption as was done in this study, then the error the investigator may make is to assume significant differences when, in fact, such differences are not significant. Fortunately, this study, which used the normal assumption, tended to *not* find significant differences in the 85th percentile speed, as shown in tables 9 and 11, except in the cases of Idaho where the p value was an extremely low 0.000. Thus, it appears that despite some theoretical imperfections in consideration of the 85th percentile speed, the use of the analysis of variance to detect significant differences was practically appropriate.

APPENDIX G: EXAMINATION OF THE EFFECTS OF ADT ON TOTAL CRASH RATES

Table 14 showed that while the removal of sites with extremely high and low ADT affects the results of the statistical tests, the relationship between ADT and crashes is still unclear. Researchers may speculate that various types of relationships exist. For example, an investigator could argue that a high ADT/low speed situation results in more vehicle interaction, which would therefore increase the likelihood of a property damage only crash, but lessen the likelihood of a serious crash.

Histograms of ADT Versus Total Crash Rate

The data employed in these histograms were the total crash rates and related ADTs from two States: Arizona and Virginia. The highest 5 percent and lowest 5 percent of ADTs were removed from the data set, because they were regarded as extreme conditions. Figures 51–53 relate ADT to crash rates for Arizona (where no speed limit change was instituted), the Virginia data when DSL was in place, and the Virginia data where USL was in place.

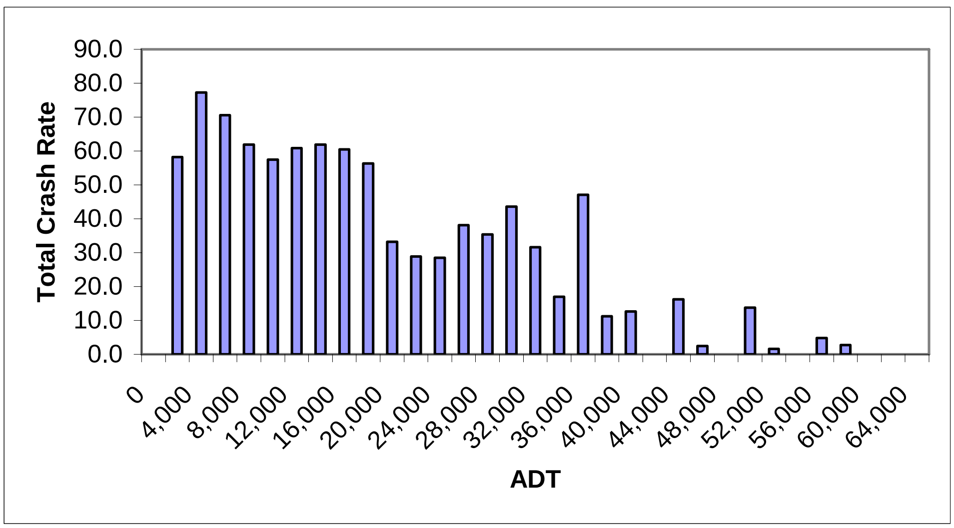


Figure 51. Chart. Arizona total crash rate versus ADT.

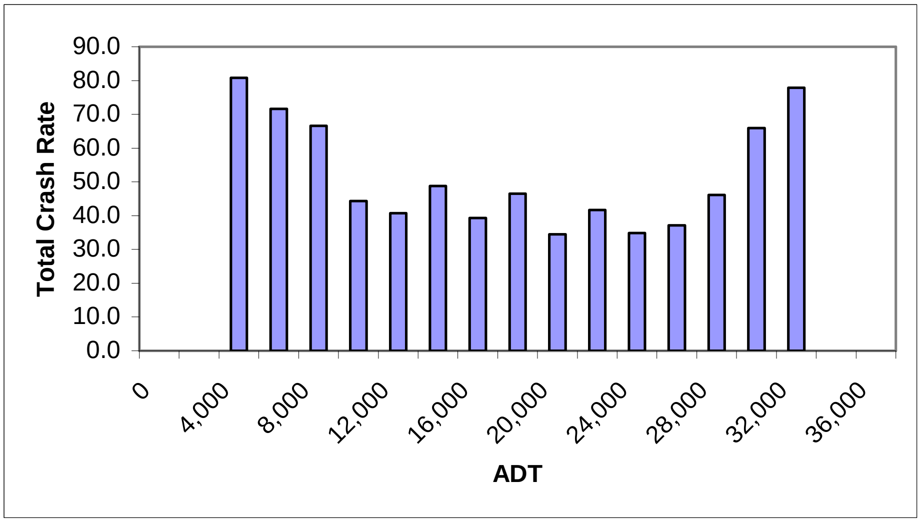


Figure 52. Chart. Virginia total crash rate versus ADT (DSL in place).

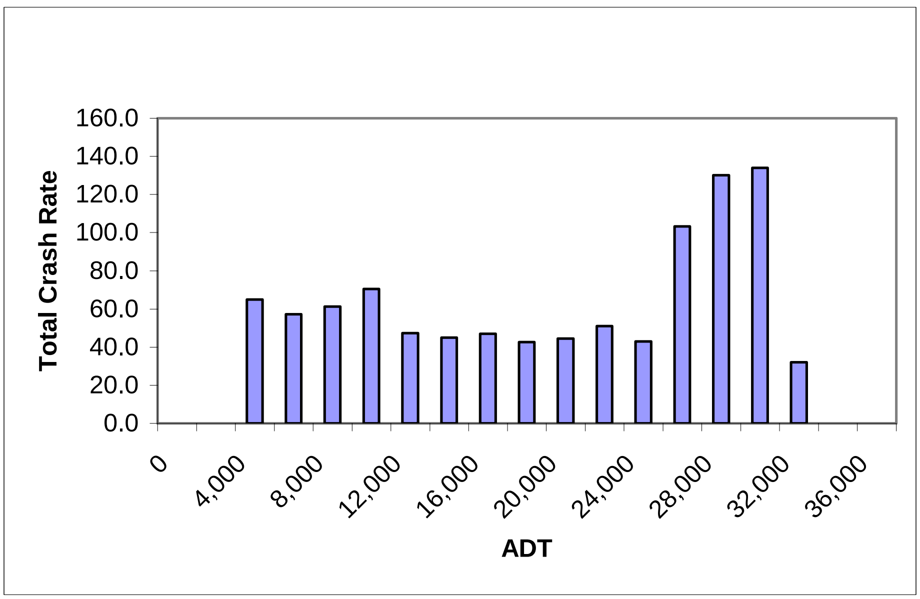


Figure 53. Chart. Virginia total crash rate versus ADT (USL in place).

Figure 53 suggests that, in Arizona, as the ADT increases, the total crash rate decreases, which would in fact support a β_2 ADT exponent less than 1.0. However, figures 52 and 53 do not show a comparable trend.

Two-Way Analysis of Variance

The effect of two independent variables (ADT and speed limit type) was assessed on the total crash rate for both Arizona and Virginia, as illustrated in table 34 and the ANOVA results in tables 35 and 36 for Arizona and Virginia, respectively. In table 35, no significant difference was found between the before and after period, noting the *p* values of 0.137 and 0.129 for Arizona and Virginia. However, ADT did have a significant influence on total crash rates in both States. Meanwhile, the interaction of these two variables was significant in Arizona but not in Virginia.

Table 34. ANOVA variable definitions.

Variable	State	Level	Definition	Note
Speed Limits	Virginia	1	1991–1993	Year
		2	1995–1999	Year
	Arizona	1	1991–1995	Year
		2	1996–1999	Year
ADT	Virginia	1	0–14,999	ADT Value
		2	15,000–27,499	ADT Value
		3	27,500–39,999	ADT Value
	Arizona	1	0–14,999	ADT Value
		2	15,000–27,499	ADT Value
		3	27,500–39,999	ADT Value
		4	40,000–52,499	ADT Value
		5	52,500–65,000	ADT Value

Table 35. ANOVA Arizona results.

Source	Type III Sum of Squares	df	Mean Square	F	Significance
Corrected Model	692,674.348[a]	9	76,963.816	22.487	0.000
Intercept	365,693.055	1	365,693.055	106.848	0.000
Speed Limit Type (DSL or USL)	7,570.345	1	7,570.345	2.212	0.137
ADT	480,476.984	4	120,199.246	35.096	0.000
Speed Limit Type (ADT)	36,564.203	4	9,141.051	2.671	0.031
Error	8,529,027.298	2,492	3,422.563	—	—
Total	18,507,548.500	2,502	—	—	—
Corrected Total	9,221,701.646	2,501	—	—	—

Table 36. ANOVA Virginia results.

Source	Type III Sum of Squares	df	Mean Square	F	Significance
Corrected Model	59,074.774[a]	5	11,814.955	9.438	0.000
Intercept	1,034,578.071	1	1,034,578.071	826.446	0.000
Speed Limit Type (DSL or USL)	2,891.319	1	2,891.319	2.310	0.129
ADT	45,023.284	2	22,511.642	17.983	0.000
Speed Limit Type (ADT)	746.468	2	373.234	0.298	0.742
Error	2,121,868.836	1,695	1,251.840	—	—
Total	6,480,839.204	1,701	—	—	—
Corrected Total	2,180,943.611	1,700	—	—	—

APPENDIX H. EXAMPLE APPLICATION OF THE CRASH ESTIMATION MODEL TO THE AFTER DATA

Once the crash estimation model of the form shown in figure 8 has been developed using the before data, the next step is to apply the equations in figures 17–25 to determine whether the policy change has had a net reduction in the expected number of crashes. Appendix H illustrates this procedure, using a single site as an example.

In 1994, Virginia changed speed limits on rural interstate highways from a differential to a uniform limit. For this illustration, suppose that the investigator has only one site in the group, such that the before data are represented by the years 1991, 1992, and 1993. Because of data limitations, the only after data that are available are 1995, 1996, and 1997.

Thus, the investigator begins with the crash estimation model, shown as figure 54, which from just one site has been calibrated as shown below, using the Virginia data from 1991–1993. Table 37 shows the before and after crash and volume data for this site, which measures 7.16 mi in length.

$$E(m)= 0.02242775 * (Length)^{0.62225} (ADT)^{0.5480}$$

Figure 54. Equation. Crash estimation model.

Table 37. Before and after crash data for a single site.

Before Data			After Data		
Year	**ADT**	**Total Crashes**	**Year**	**ADT**	**Total Crashes**
1991	4000	8	1995	4500	8
1992	4250	11	1996	4650	7
1993	4300	7	1997	4800	10
Sum		26	1999	4825.56	5
			Sum		30

Estimation of Expected Crash Frequency $M_1, M_2, \ldots M_y$ for the Before Period

The crash estimation model shown above is used with the data in table 37 to calculate the $E(m_{1,y})$, that is, the *mean* of the estimated crash frequency of site *i* for each year, as shown in figures 55–57. (Normally, *i* will range from 1 to the number of sites (e.g., for Virginia, with 266 sites, there would be equations with *i* = 1, *i* = 2, … *i* = 266. In this example with only one site, however, *i* will always be 1.)

$$E(m_{1,1991}) = 0.02242775 * (7.16)^{0.62225} (4000)^{0.5480} = 7.191$$

Figure 55. Equation. Mean of the estimate for 1991.

$$E(m_{1,1992}) = 0.02242775 * (7.16)^{0.62225} (4250)^{0.5480} = 7.434$$

Figure 56. Equation. Mean of the estimate for 1992.

$$E(m_{1,1993}) = 0.02242775 * (7.16)^{0.62225} (4300)^{0.5480} = 7.481$$

Figure 57. Equation. Mean of the estimate for 1993.

Next, the ratios $C_{1,y}$ which are the ratios of $E(m_{1,y})$ to $E(m_{1,1})$ for each before year y, are calculated using the form of figure 58 and applied in figures 59–61.

$$C_{i,y} = \frac{E(m_{i,y})}{E(m_{i,1})}$$

Figure 58. Equation. Calculation for ratio before year *y*.

$$C_{1,1991} = E(m_{1,1991}) / E(m_{1,1991}) = 1$$

Figure 59. Equation. Ratio before year 1991.

$$C_{1,1992} = E(m_{1,1992}) / E(m_{1,1991}) = 1.033782$$

Figure 60. Equation. Ratio before year 1992.

$$C_{1,1993} = E(m_{1,1993}) / E(m_{1,1991}) = 1.040429$$

Figure 61. Equation. Ratio before year 1993.

The next step is to calculate the expected crash counts $m_{i,y}$ on this site for each before year with their variance VAR($m_{i,y}$) using the equations in the figures below.

$$m_{i,1} = \frac{k + \sum_{y=1}^{Y} K_{i,y}}{\frac{k}{E(m_{i,1})} + \sum_{y=1}^{Y} C_{i,y}}$$

Figure 62. Equation. Expected crash counts.

$$VAR(m_{i,1}) = \frac{k + \sum_{y=1}^{Y} K_{i,y}}{\left(\frac{k}{E(m_{i,1})} + \sum_{y=1}^{Y} C_{i,y}\right)^2} = \frac{m_{i,1}}{\frac{k}{E(m_{i,1})} + \sum_{y=1}^{Y} C_{i,y}}$$

Figure 63. Equation. Variance of the expected crash counts for year 1.

$$m_{i,y} = C_{i,y}\ m_{i,1}$$

Figure 64. Equation. Expected crash counts.

$$\mathrm{VAR}(m_{i,y}) = C_{i,y}^{\ 2}\ \mathrm{VAR}(m_{i,1})$$

Figure 65. Equation. Variance of expected crash counts.

The application of these four expressions is shown in the equations in figures 66–71.

$$m_{1,1991}=(5.9+26)/(5.9/7.191+3.074211)= 8.190599$$

Figure 66. Equation. Application for 1991.

$$VAR(m_{1,1991})=8.190599/(5.9/7.191+3.074211)= 2.103007$$

Figure 67. Equation. Application for variance 1991.

$$m_{1,1992}= C_{1,1992}\ m_{1,1991}=1.033782*8.190599=8.467292$$

Figure 68. Equation. Application for 1992.

$$\mathrm{VAR}(m_{1,1992})= C_{1,1992}^{\ 2} * \mathrm{VAR}(m_{1,1991})= 1.033782\text{^}2*2.103007=2.247493$$

Figure 69. Equation. Application for variance 1992.

$$m_{1,1993}= C_{1,1993}\ m_{1,1991}=1.040429*8.190599=8.521739$$

Figure 70. Equation. Application for 1993.

$$\mathrm{VAR}(m_{1,1993})= C_{1,1993}^{\ 2} * \mathrm{VAR}(m_{1,1991})= 1.040429\text{^}2*2.103007=2.27649$$

Figure 71. Equation. Application for variance for 1993.

These results are summarized in table 38.

Table 38. Estimation results for the before years.

Year	ADT	Total Crashes	$E_{(m1,y)}$	$C_{1,y}$	$m_{1,y}$	VAR(m1,y)
1991	4000.000	8	7.191	1.0	8.190599	2.103007
1992	4250.000	11	7.434	1.033782	8.467292	2.247493
1993	4300.000	7	7.481	1.040429	8.521739	2.27649
Sum		26		3.074211		

Prediction of $M_{y+1}, M_{y+2},...M_{y+Z}$ for the After Period

The next step is to use the crash estimation model from figure 56 to compute the mean of the expected would-have-been crash frequency for the after period years. That is, even though Virginia changed from a differential to a uniform limit in 1994, there question remains of "what would the mean of the expected crash frequency have been had Virginia not changed its speed limit policy." Computation of these $E(_{m1,y})$ values for the after period, as shown in figure 72 and 73, answers this question.

$$E(m_{1,1995}) = 0.02242775 * (7.16)^{0.62225}\ (4500)^{0.5480} = 7.670$$

Figure 72. Equation. Computation of $E(m_{1,1995})$.

$$E(m_{1,1996}) = 0.02242775 * (7.16)^{0.62225}\ (4650)^{0.5480} = 7.809$$

Figure 73. Equation. Computation of $E(m_{1,1996})$.

This process is repeated for the 1997 and 1999 years.

One then computes the ratio $C_{i,y}$ using figure 60, as illustrated in figures 74 and 75.

$$C_{1,1995} = E(m_{1,1995}) / E(m_{1,1991}) = 1.066677$$

Figure 74. Equation. Computation of $C_{1,1995}$.

$$C_{1,1996} = E(m_{1,1996}) / E(m_{1,1991}) = 1.086018$$

Figure 75. Equation. Computation of $C_{1,1996}$.

Finally, figures 76 and 77 allow one to predict the would-have-been expected crash frequencies for the after years. Application of these methods is shown in figures 78–81.

$$m_{i,y} = C_{i,y}\ m_{i,1}$$

Figure 76. Expected crash counts, year y.

$$\mathrm{VAR}(m_{i,y}) = C_{i,y}{}^{2}\ \mathrm{VAR}(m_{i,1})$$

Figure 77. Variance of expected crash counts, year y.

$$m_{1,1995} = C_{1,1995}\ m_{1,1991} = 1.066677*8.190599 = 8.73672$$

Figure 78. Equation. Expected crash counts, year 1995.

$$\mathrm{VAR}(m_{1,1995}) = C_{1,1995}{}^{2} * \mathrm{VAR}(m_{1,1991}) = 1.066677^{2}*2.103007 = 2.392799$$

Figure 79. Variance of expected crash counts, year 1995.

$$m_{1,1996} = C_{1,1996}\ m_{1,1991} = 1.086018*8.190599 = 8.895134$$

Figure 80. Expected crash counts, year 1996.

$$\mathrm{VAR}(m_{1,1996}) = C_{1,1996}{}^{2} * \mathrm{VAR}(m_{1,1991}) = 1.086018^{2}*2.103007 = 2.480358$$

Figure 81. Variance of expected crash counts, year 1996.

These results are summarized in table 39.

Table 39. Prediction results for the after years.

Year	ADT	[a]$K_{i,y}$	[b]$E(m_{1,y})$	[c]$C_{1,y}$	[d]$m_{1,y}$	[e]$\mathrm{VAR}(m_{1,y})$	$m_{1,y} - K_{i,y}$	$K_{i,y}/m_{1,y}$
1995	4500	8	7.670	1.066677	8.73672	2.392799	0.73672	0.915675
1996	4650	7	7.809	1.086018	8.895134	2.480358	1.895134	0.786947
1997	4800	10	7.946	1.105079	9.051255	2.568189	–0.94874	1.104819
1999	4825.56	5	7.970	1.1083	9.077637	2.583182	4.077637	0.550804
Sum		30	31.39535	4.366072	35.76075	10.02453	5.760747	3.358246
Average		7.5	7.849	1.091518	8.940187	2.506132	1.440187	0.839561

[a]$K_{i,y}$: The actual after crashes for year *y*.
[b]$E(m_{1,y})$: The mean of the expected would-have-been after crashes for year *y*.
[c]$C_{1,y}$: The changing ratio for the would-have-been after crashes.
[d]$m_{1,y}$: The expected would-have-been after crashes for year *y*.
[e]$\mathrm{VAR}(m_{1,y})$: The variance of the expected would-have-been after crashes for year *y*.

Evaluation of Safety Effects of Changing the Speed Limit for This Particular Site

The effect of the treatment (that is, changing from a differential speed limit to a uniform speed limit) is determined by comparing the actual after crashes $K_{i,y}$ with the predicted after crashes $m_{i,y}$ for each year of the after period. The cumulative differences, shaded in table 40, are computed by applying the equations in figures 82–84.

$$\pi_i = \sum m_{1,y} = 8.73672 + 8.895134 + 9.051255 + 9.077637 = 35.76075$$

Figure 82. Equation. Total would-have-been crashes for a particular site.

$$\lambda_i = \sum K_{1,y} = 8 + 7 + 10 + 5 = 30$$

Figure 83. Equation. Total actual crashes for a particular site.

$$\pi_i - \lambda_i = 35.76075 - 30 = 5.76$$

Figure 84. Equation. Safety impact for a particular site.

Table 40. Evaluation of the treatment for the example site.

Year	$K_{i,y}$	Cumulative $K_{1,y}$	$m_{1,y}$	Cumulative $m_{1,y}$	VAR($m_{1,y}$)	*Excess* $m_{1,y} - K_{i,y}$	Cumulative Excess	$K_{i,y} / m_{1,y}$
1995	8	8	8.73672	8.73672	*2.392799*	0.73672	0.73672	0.915675
1996	7	15	8.895134	17.63185	*2.480358*	1.895134	2.631854	0.786947
1997	10	25	9.051255	26.68311	*2.568189*	−0.94874	1.683114	1.104819
1999	5	30	9.077637	35.76075	*2.583182*	4.077637	5.760751	0.550804
Average	7.5		8.940187		2.506132	1.440187		0.839561

An interpretation of table 40 is that, over the 4-year after period, the actual number of crashes at this site was 5.76 less than the predicted number of crashes that would have resulted had there been no change in the speed limit. Alternatively, the investigator could use the equation in figure 85 to indicate that the speed limit change decreased crashes by approximately 16 percent at the site, since the ratio of the actual crashes to “would-have-been crashes had no change occurred” is 0.84.

$$\lambda_i / \pi_i = 30/35.76075 = 0.84 = 84\%$$

Figure 85. Equation. Ratio of actual to would-have-been crashes.

The investigator can graphically portray these cumulative differences as shown in figure 86.

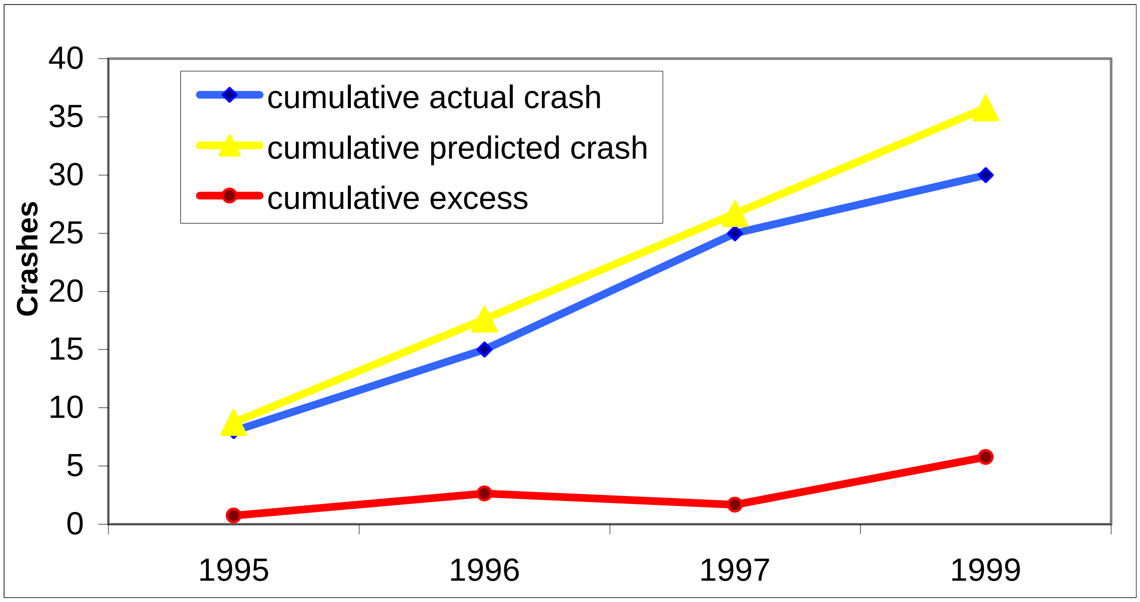


Figure 86. Chart. Cumulative differences, by year, at the example site.

Evaluation of Safety Effects of Changing the Speed Limit for All Sites

The example shown above has been applied to only one particular site, but realistically a researcher would apply these concepts to all sites (e.g., in Virginia, 266 sites). Thus, using similar computations from all 266 sites, the investigator compares (a) the actual after crashes for the after period 1995 through 1999 that resulted under the uniform speed limit imposed in 1994, and (b) the would-have-been crashes for the after period 1995 through 1999 that would have resulted had the speed limit not been changed in 1994. Two basic performance measures, the reduction in the expected number of crashes (δ) and the index of effectiveness (θ), are computed and tested for statistical significance.

The equations in figures 87–88 compute, respectively, the would-have-been crashes (those that would have occurred had no changes taken place) and the actual crashes that did occur. Since the equation in figure 89 shows that the actual number of crashes (λ) is larger than these would-have-been crashes (π), the negative value of δ suggests that the speed limit change had an adverse impact on safety, and it would have been better not to make the change. This statement, however, needs to be tested for statistical significance.

$$\pi = \sum_i \pi_i = 13{,}365.91$$

Figure 87. Total would-have-been crashes.

$$\lambda = \sum_i \lambda_i = 15{,}377$$

Figure 88. Total actual crashes.

$$\delta = \pi - \lambda = 13{,}365.91 – 15{,}377 = -2011.09$$

Figure 89. Safety impact.

To determine statistical significance, the starting point is the variance as calculated in figure 92. The term $\Sigma Var(\pi_i)$ is obtained by summing the values of VAR($m_{i,y}$) for each site and each year. Thus, the italicized values shown in the sixth column of table 40 (e.g., 2.39, 2.48, 2.57, and 2.58) would be computed for each site and for each year, and with 266 sites and 4 years of data, the 1,064 values of VAR($m_{i,y}$) are summed to equal 4,246.913. Because of statistical properties appropriate to the Poisson distribution, the summation of the λ_i values are equivalent to the variance of these values, which is 15,377 as tabulated in figure 89 above. These two items are used in figure 90 below to obtain Var(δ), which intuitively may be described as the variation associated with the difference between would-have-been crashes and actual crashes.

$$Var(\delta) = Var(\pi) + Var(\lambda) = \Sigma Var(\pi_i) + \Sigma Var(\lambda_i) = 4{,}246.91 + 15{,}377 = 19{,}623.91$$

Figure 90. Variance of the difference between would-have-been crashes and actual crashes.

The standard deviation is thus the square root of this variance in figure 91, such that:

$$\sigma(\delta) = \{Var(\delta)\}^{0.5} = 140.0854$$

Figure 91. Standard deviation of the difference between would-have-been crashes and actual crashes.

Empirical confidence bounds are thus $\delta \pm 2\sigma(\delta)$ or –2011 ± 2(140).

Computation of the index of effectiveness is accomplished via the equation below as:

$$\theta = (\lambda/\pi)/\{1+Var(\pi)/\pi^2\}$$
$$\theta = (15{,}377/13{,}365.91)/(1 + 4{,}246.913/13{,}365.91^2)$$
$$\theta = 1.150437\text{, in other words, about a 15 percent increase}$$

Figure 92. Equation. Computation of the index of effectiveness.

The variance of θ is given below as:

$$Var(\theta) = \theta^2\{[var(\lambda)/\lambda^2]+[var(\pi)/\pi^2]\}/[1+ var(\pi)/\pi^2]^2$$
$$Var(\theta) = 1.150437^2 * \{15{,}377/15{,}377^2+4{,}246.913/13{,}365.91^2\}/[1+4{,}246.913/13{,}365.91^2]^2$$
$$Var(\theta) = 0.000118$$

Figure 93. Equation. Variance of θ.

The empirical confidence bounds are shown below:

$$\theta \pm 2\text{Var}(\theta)^{0.5}$$
$$1.15 \pm 2(0.000118)^{0.5} \text{ in absolute units, or}$$
$$15\% \pm 2(0.0118\ \%)^{0.5} \text{ if expressed as a percentage, or}$$
$$15\% \pm 2(1.08\%), \text{ or}$$
$$12.9\% \text{ to } 17.2\%$$

Figure 94. Equation. Empirical confidence bounds.

Thus, the change from DSL to USL in Virginia increased the number of crashes by about 15 percent, with the full response being that this 15 percent is not a perfect estimator but instead should be given as a range, such that the increase was between approximately 12.9 percent and 17.2 percent according to this application of the empirical Bayes method. Assuming all the other factors remained constant except for the change of speed limit from differential to uniform, therefore, the value of θ being greater than 1.0 means that the change had an adverse impact on safety, as reflected in the number of crashes on rural interstates in Virginia.

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