FINAL REPORT

THE TRANSFERABILITY OF SAFETY-DRIVEN ACCESS MANAGEMENT MODELS FOR APPLICATION TO OTHER SITES

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ABSTRACT

Several research studies have produced mathematical models that predict the safety impacts of selected access management techniques. Since new models require substantial resources to construct, this study evaluated five existing models with regard to their applicability to locations other than the one for which they were designed. The predictive power of the models was assessed using three sites in Virginia. The study also considered the practical aspects of applying the models in Virginia to estimate the likelihood that necessary data are available, the number of computations required to apply the models, the simplicity of the rationale underlying the models, and the sensitivity of the models to inputs.

The applicability of the models was wide ranging. Without site-specific adjustments, the average percent error of the models ranged from 34 to a few hundred percent. With simple site-specific adjustments, the error ranged from 27 to 29 percent. Because some of the models were developed for a specific site or were intended to be used only with site-specific adjustments, these error percentages indicate only the extent to which the models are transferable with respect to estimating crashes, not the performance of the models themselves.

The wide variation in applicability was due to discrepancies in data definitions, the availability of data, the structure of the model, and the assumptions used. Recommendations were developed for using the models in practice and for understanding their limitations. Two principal conclusions were as follows: (1) existing models (with minor adjustments for some) can predict crashes as a function of access within →34 percent of the actual number, and (2) some of the models are simple enough to be used in practice.

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INTRODUCTION

Access management has been loosely described as a strategy that maximizes capacity and safety, predominantly for collector and arterial facilities.[1] In fact, however, access management comprises a host of disparate techniques. Koepke and Levinson wrote that "access management is the process that provides (or manages) access to land development while simultaneously preserving the flow of traffic on the surrounding road network in terms of safety, capacity, and speed."[2] Haas et al. defined *access management* as a function of unsignalized driveway spacing.[4] Brown et al. reported that access control is thought by some to be "all techniques intended to minimize the traffic interference associated with commercial driveways."[5]

Access management requires an explicit tradeoff between two competing goals: to maximize throughput and to maximize access. The ideal balance between access and efficiency depends on the intended function of the roadway and the perspective of the user. Since each user is different, decisions regarding access control can be controversial. Accordingly, agencies responsible for managing arterial roadways are interested in acquiring better methods to quantify the safety impacts of access management.

Gluck et al. provided a "shopping list" of 100 access management options, including using different median configurations, providing frontage roads, providing minimum distances between interchanges and adjacent driveways, and eliminating left turns.[3] Policy implications include using access codes (spacing of driveways and signals), developing zoning regulations, purchasing access rights, and establishing setbacks from interchanges and intersections. Design tactics include relocating and consolidating driveways, improving operations for signals, and eliminating U-turns.

No management strategy is painless, and officials need to be able to justify their decision to undertake a particular strategy. Most, if not all, studies have found that increasing the number of access points increases the risk of crashes; Preston et al. argued that crash history is affected by access density but not traffic volume.[6] Bowman and Rushing noted that reducing conflict points along a corridor reduces crash risk.[7] Because reducing crash risk is an important objective of access management, there is an interest in models that can estimate this reduction. One access management factor—traffic signal density and operation—merits special attention. Although many researchers maintain that increasing access density will increase the likelihood of crashes, the strength of this association must be articulated.

PROBLEM STATEMENT

To quantify the safety benefits of particular access management strategies, especially controversial strategies such as reducing signal density, administrators need to know what methods can be used, how well the methods will work in various real-world situations, and how the methods can be applied in a timely and cost-effective manner. A method that is too cumbersome to deploy or has not been validated is not likely to be useful. Hence, user-friendly techniques to evaluate access management options must be identified or developed.

PURPOSE AND SCOPE

The purpose of this research was to identify and evaluate quantitative techniques for quantifying the safety impacts of various access management strategies as a result of changing the number of signals for a corridor. The scope of the research was limited to models suggested in the literature since developing new models requires substantial resources.

This study assessed the transferability of models to locations other than those for which they were developed. Comments made about the models reviewed in this study reflect only this assessment; they do not constitute an evaluation of the performance of the models for any other factor.

METHODS

The case-study approach was used. Specifically, three tasks were performed in sequence to achieve the study objectives:

1. *Mathematical models to quantify the safety impacts, i.e., predict crashes, of access management strategies were identified through a review of the literature, and five*

models were selected that were in the public domain, predicted either crash rates or the number of crashes, and appeared feasible to apply in Virginia.

2. *Case study corridors were selected for analysis, and appropriate data were collected.* Corridors were selected on the basis of three key criteria: dramatic change in number of signals, few geometric changes, and available historic data. Appropriate data included operational, geometric, and crash profiles of the corridor.

3. *The selected models were applied to the case study corridors and compared.* To test the suitability of the models for data sets other than the ones for which they were developed, they were first applied without modification. Then, where appropriate, they were applied by fitting them to the conditions on the case study corridors. The models were compared on the basis of accuracy, sensitivity, and ease of application.

RESULTS

Identification of Methods for Quantifying Safety Impacts

The literature offers several techniques for quantifying the impacts of access management: most are some variant of regression although one technique uses a proprietary software package.

Regression Analysis

Most methods focus on the use of linear regression analysis. Regression analysis is a statistical tool that can be used to explain the relationship between an independent variable (such as the number of signals per mile) and dependent, or explanatory, variables (such as the number of crashes). That is, regression models can be used to identify which variables explain, or do not explain, the variability in the independent variable. In a study in Lee County, Florida, distance between signals and number of accidents per million vehicle miles were used to show how regression analysis illustrates the relationship between signal density and accident rates.[8] In a study of the design of signalized and unsignalized intersections, control of diverging and left-turn maneuvers, and median treatment as access management techniques, Brown et al. developed regression models (see Appendix A) to estimate the numbers of total crashes, property damage only (PDO) crashes, and severe crashes.[5]

In their attempts to quantify the impact of driveway spacing and turning volume on safety, Haas et al. used the number of evasive maneuvers observed during field studies at 22 sites as the dependent variable.[4] An *evasive maneuver* was defined as the brake lights of a through vehicle turning on as the result of the lead vehicle making a right turn (both vehicles would be in a lane that allowed through traffic and right turns into unsignalized driveways). The data were fit to a probability analysis and a linear regression analysis to estimate the percentage of right-lane-though vehicles that would be affected as a function of driveway spacing and right-turning volume. Necessary field data included median type, operating speed, number of lanes and lane type, distance of each site from the upstream and downstream signal, and detailed right-lane volume counts (number of through vehicles and number of vehicles turning into the right lane).

A potential advantage of regression models is the ease with which they can be used to estimate crash rates. A potential disadvantage is their aggregate nature in that they look at an entire corridor macroscopically rather than simulating the interactions of individual vehicles, thus, they may not be the best tool to identify the impacts of changes at a specific location. Another drawback is that some regression models use only vehicle miles traveled (VMT) rather than total vehicles entering an intersection.[3] A question unanswered by this effort is whether simulation techniques that examine the individual vehicle behaviors at each signal would address these concerns.

Software Programs

TRAF-SAFE is a commercially available software package developed to evaluate the safety impacts of various access management strategies. After meeting with the developer of this software, the authors chose not to evaluate the package as part of this research project because heavy involvement on the part of the developer would have been been required. Interested readers, however, may learn more about this model from the TRAF-SAFE Corporation at http://www.traf-safe.com.

Selection of Crash Prediction Models to Be Analyzed

Five model formulations that employ signalized or unsignalized access density were identified in the literature and selected for evaluation. The formulations for the models are presented in Appendix A, data assumptions are shown in Appendix B, and examples of how to apply the models are available from the authors and are also presented in the literature.. All models include as a dependent variable either crash rate or number of crashes. Independent variables may include average annual daily traffic (AADT), length of corridor segment, and duration of time.

The models may be summarized as follows:

- *Model 1* is a multivariate regression model that estimates absolute crashes as a function of total number of signalized and unsignalized access points, percentage of signalized access points, presence of a shoulder, and type of median. It was developed by Brown et al. in 1998 as a tool to evaluate access control on high-speed urban arterials.[5]

- *Model 2* consists of two submodels.[10] The first is a multivariate regression model that estimates absolute crashes between signals as a function of total number of signalized and unsignalized access points, number of residential driveways, percentage of signalized access points, median type, land use, whether residential parking is allowed, and proportion of PDO crashes. It was developed by Bonneson and McCoy in 1997 to determine the effect of median treatment on urban arterial safety.[9] The second submodel estimates absolute crashes at signals based on the number of vehicles entering each signalized intersection. It was developed by Persaud and Nguyen in 1998 as a disaggregate safety performance model for signalized intersections on provincial roads in Ontario.[10] Although the submodels were developed independently, they appear suitable as a crash prediction technique for corridors if used together. The submodels were combined to allow a coherent approach for estimating crashes along an entire corridor. The first submodel estimates only crashes between signals, and the second estimates only crashes at signals.

- *Models 3a, 3b, and 3c* consist of graphs and tables from a study by Gluck et al. of the impacts of access management techniques.[3] They all predict relative changes in crash rates as the dependent variable. Model 3a uses median type and total number of access points, Model 3b uses median type and number of signals, and Model 3c uses only number of signals and number of unsignalized access points. Model 3 was intended for application only in conjunction with existing crash data.

- *Model 4* is a linear regression model that estimates crash rates as a function of speed, number of access points, left-turn lane availability, spacing between driveways, and variance in driveway spacing. It was developed by White and Garber in 1995 in a study to develop guidelines for commercial driveway spacing on urban and suburban arterial roads.[11] The model was modified slightly, as shown in Appendix A, in order to use it with the available data.

- *Model 5* is a univariate regression model that estimates crash rates as a function of total number of signalized and unsignalized access points. It was developed by Preston et al. in 1998 in a study of statistical relationship between vehicular crashes and highway access.[6] There are 11 formulations of the model, each dependent on a different type of roadway facility.

Selection of Case Study Corridors and Collection of Data

Selection of Corridors

Three corridors were selected for analysis. These corridors were selected because the number of traffic signals increased substantially over a 10-year period, as noted by VDOT district engineering staff. Since the corridors have a fixed length, the increase in signal density resulted in a corresponding decrease in signal spacing—that is, the average distance between signals has become shorter over time. In addition, historical data were available for the corridors, and few geometric changes had occurred during the analysis period.

The three corridors were:

1. *Corridor I, a 5-mi (8-km) section of Route 147 (Huguenot Road) between Route 150 (Chippenham Parkway) and U.S. 60 (Midlothian Turnpike) in Richmond and Chesterfield County (see Figure 1).* Huguenot Road is a four-lane suburban arterial passing through commercial and residential land use areas. The average daily traffic (ADT) has varied between approximately 29,000 and 43,000, depending on the year and section.

2. *Corridor II, a 2.5-mi (4-km) section of Route 250 between Greenville Avenue and Sanger's Lane in Staunton and Augusta County* (see Figure 2), with ADTs between 20,000 and 27,000. The corridor is a four-lane suburban arterial.

3. *Corridor III, a 7-mi (11.2-km) section of Route 17 in York County between the Newport News City Line and Warwick and Cook Road* (see Figure 3). Route 17 functions as a four-lane divided suburban arterial; the Virginia Department of Transportation (VDOT) engineer noted that before volumes and the number of access points had increased, Route 17 could be classified as a suburban multilane highway. ADTs for Corridor III ranged from about 35,000 to 39,000.

Collection of Data

Operational, geometric, and crash profiles of the corridor were obtained from several data sources for the period 1990 through 1999. These sources included the VDOT Highway Traffic Records Information System (HTRIS); the Chesterfield County Department of Planning; the Chesterfield County Transportation Department; the City of Staunton Traffic Engineering Department; the York County Planning Office; the traffic engineering sections in VDOT's Richmond, Hampton Roads, and Staunton districts; and VDOT's Williamsburg Residency. Additional data were obtained through site visits; videotapes of the study sites; and meetings with VDOT, county, and city personnel.

Operational characteristics include traffic volumes and turning movements in each lane. Geometric characteristics include number of lanes, channelization, and signal density. Crash characteristics include number, collision type, and severity of crashes. Interpolation of data was necessary in some instances because of the historical nature of the study. By using multiple data sources, the investigators were able to synthesize a data set that was sufficiently complete for the needs of this study for the corridors. Appendix B gives examples of two types of data interpolation required. The estimation of unsignalized driveways for time periods past, as explained in Appendix B, would have been easier had access permit data been electronically searchable and understandable.

Table 1 provides an overview of the crash history and a summary of the data for each corridor:

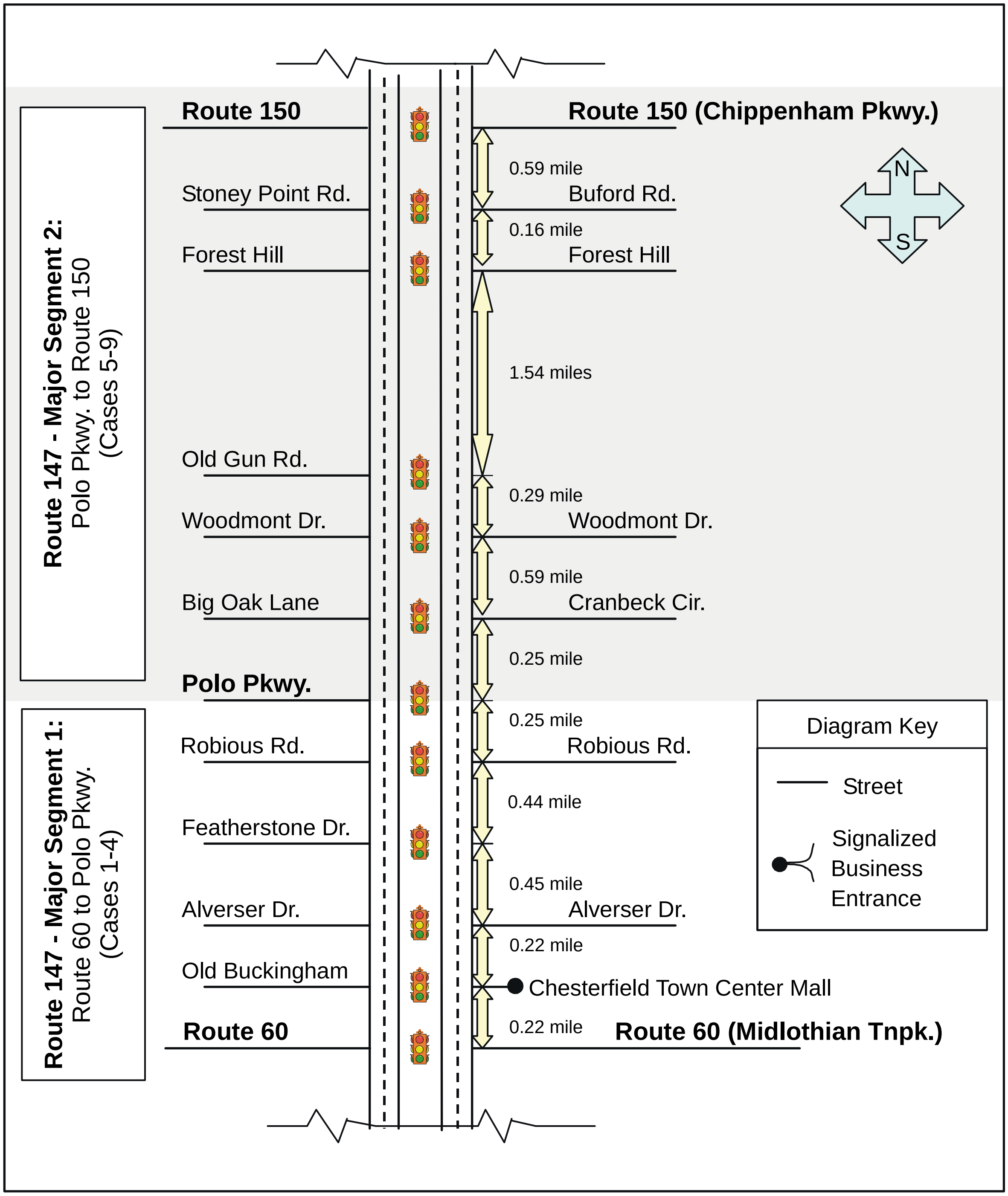

Figure 1. Diagram of Corridor I: Route 147 (Richmond, Virginia) (1 mi = 1.6 km)

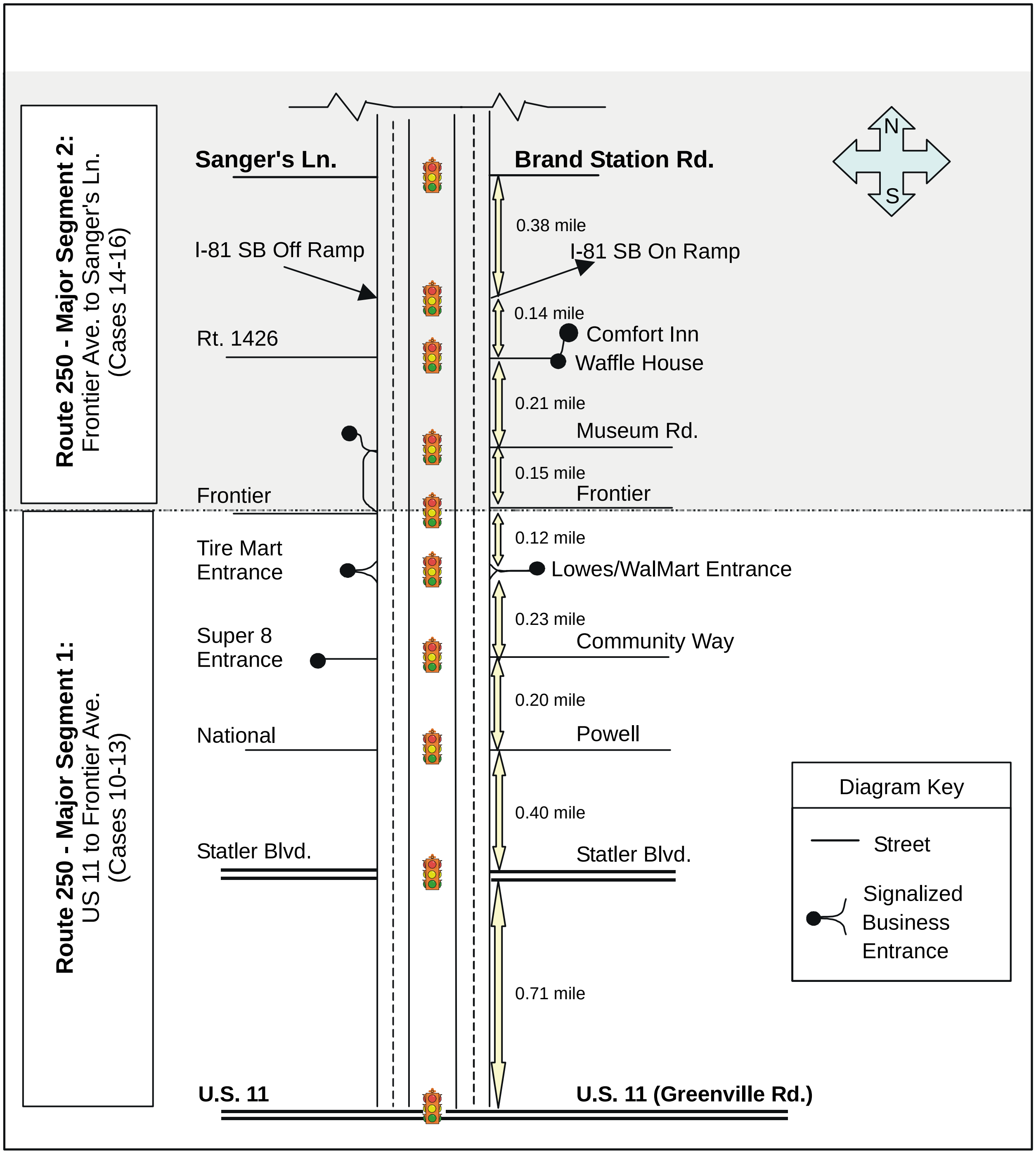

Figure 2. Diagram of Corridor II: Route 250 (Staunton, Virginia) (1 mi = 1.6 km)

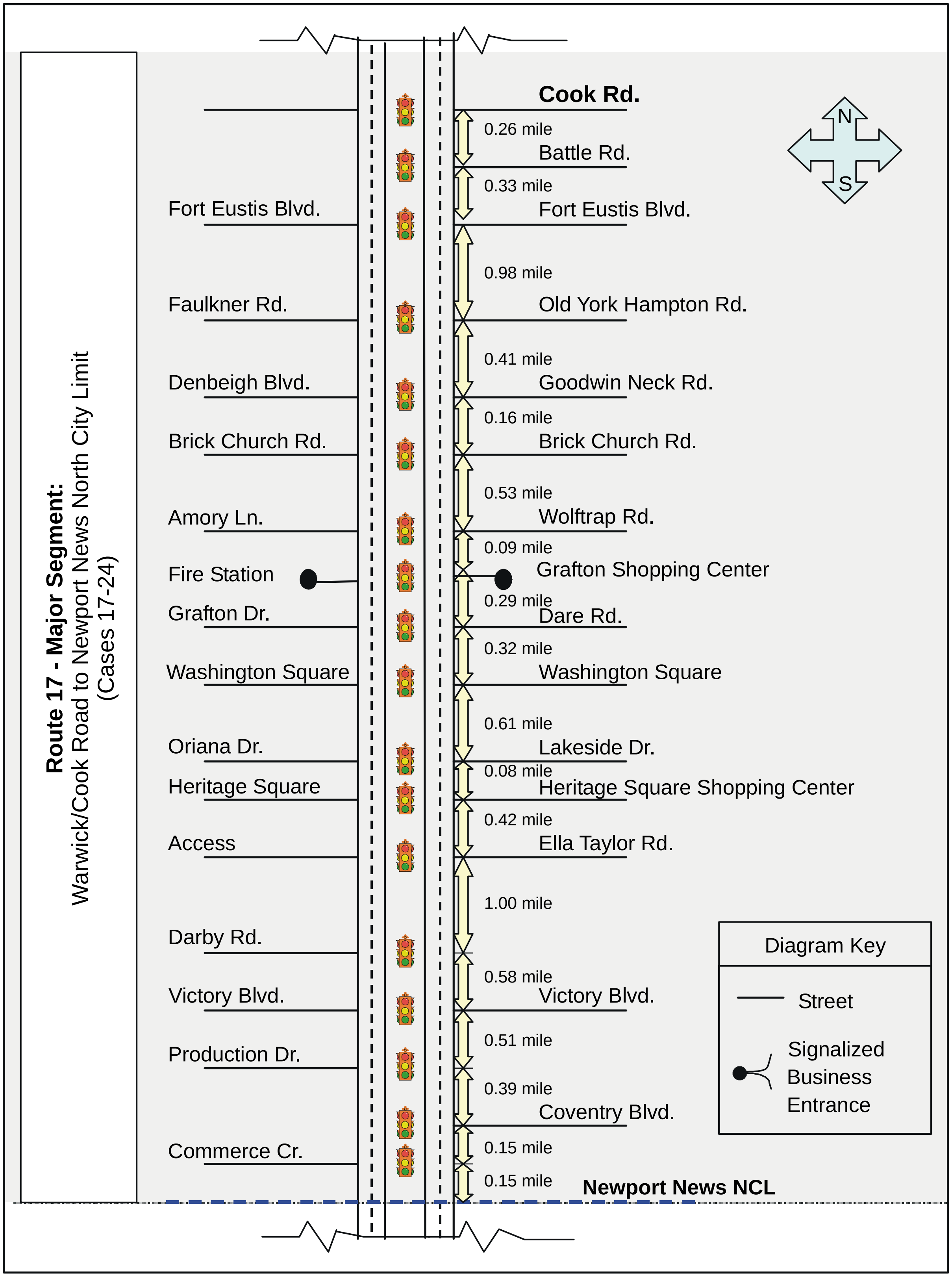

Figure 3. Diagram of Corridor III: Route 17 (York County, Virginia) (1 mi = 1.6 km)

1. Corridor I has two major segments. The southern segment—Route 60 to Polo Parkway—has two sections with a two-way left-turn lane (TWLTL) and three sections with a median treatment. The northern segment—Polo Parkway to Route 150—has one section with a TWLTL treatment, four sections with a median treatment, and one section that is undivided.

2. Corridor II has two major segments, an older portion with a TWLTL and a new section with raised curb medians.

3. Corridor III is one segment because geometric conditions were relatively constant.

As may be inferred from the table, if each segment and each time period is considered a discrete case, 24 possible cases exist for which various models could be applied.

Table 1. Data Summary for Case Study Corridors

Major Segment	Case	Time Period (Based on Signal Installation)	Duration (Yr)	No. Unsignalized Access Points	No. Signals	ADT	No. Actual Crashes	Crash Rate[a]
Corridor I: Route 147 (Richmond)								
Route 60 to Polo (1.58 mi)	1	01/1/90-04/19/91	1.3	27	1	29,411	54	245 (0)
	2	04/20/91-06/19/91	0.2	26	2	39,000	6	160 (-35)
	3	06/20/91-05/23/95	3.9	26	3	27,879	153	242 (1)
	4	05/24/95-12/31/98	3.6	24	4	35,739	185	249 (2)
Polo to Route 150 (3.42 mi)	5	01/01/90-11/5/91	1.8	38	1	33,249	79	103 (0)
	6	11/6/91-02/4/92	0.2	36	2	41,000	12	94 (-9)
	7	02/5/92-11/14/92	0.8	35	3	41,537	48	119 (16)
	8	11/15/92-08/15/96	3.8	33	4	41,069	246	128 (24)
	9	08/16/96-12/31/98	2.4	31	5	43,235	213	166 (61)
Corridor II: Route 250 (Staunton)								
U.S. 11 to Frontier Avenue (1.71 mi)	10	1/1/90-8/31/95	5.7	80	1	19,988	49	69 (0)
	11	9/1/95-8/31/96	1.0	78	2	23,000	23	160 (132)
	12	9/1/96-8/31/98	2.0	76	3	26,000	71	219 (217)
	13	9/1/98-12/31/99	1.3	74	4	27,000	42	187 (171)
Frontier to Sanger's Lane (0.89 mi)	14	1/1/90-10/31/95	5.8	4	1	23,000	72	165 (0)
	15	9/1/96-8/31/98	3.1	2	2	25,000	62	248 (50)
	16	12/1/98-12/31/99	1.1	0	3	26,000	42	458 (178)
Corridor III: Route 17 (York)								
Warwick/ Cook Road to Newport News North City Limit (7.3 mi)	17	1/1/90-7/31/92	2.6	210	10	37,792	423	164 (0)
	18	8/1/92-2/28/95	2.6	221	11	34,943	428	179 (9)
	19	3/1/95-12/31/96	1.8	226	12	38,303	325	174 (6)
	20	1/1/97-2/28/97	0.2	230	13	39,303	24	143 (-13)
	21	3/1/97-10/31/97	0.7	230	14	39,303	145	207 (27)
	22	11/1/97-2/28/98	0.3	233	15	39,303	72	210 (28)
	23	3/1/98-10/31/98	0.7	233	16	39,303	149	213 (30)
	24	11/1/98-12/31/99	1.2	235	17	39,303	233	192 (17)

1 mi = 1.6 km.
[a]The percent relative change from the base year is in parentheses.

With the possible exception of Corridor III, the crash rates are not strongly correlated with signal density or number of access points. For example, Cases 1 and 4 have comparable crash rates of 245 and 249 yet the latter has 4 times as many signals. However, from 1990 to 1998, Corridor I had relatively few access points. Gluck et al. categorized corridors by number of access points per mile, where the lowest category for urban areas was less than 20 points per mile (32 points per kilometer). The cases in Corridor I are in this lowest category, where the portion of the corridor with the highest signal density still has a relatively low overall density. Corridors II and III have a wider range of access densities.

The fact that Corridor III has more unsignalized access points than Corridors I or II reflects the fact that when the investigators sought corridors, few corridors met all three key criteria (i.e., dramatic change in number of signals, few geometric changes, and available historic data).

Application of Crash Prediction Models to Case Study Corridors and Comparison of Their Performance

As stated previously, to test the suitability of the models for data sets other than the ones for which they were developed, they were first applied without modification. Then, where appropriate, they were applied by fitting them to the conditions on the case study corridors. The models were compared on the basis of accuracy in predicting the number of crashes, sensitivity, and ease of application.

Accuracy

Without Modification

Table 2 indicates the predictions of each model for each case. As an example, for Case 1, Model 3c came the closest to predicting the actual number of crashes, i.e., 54, with a prediction of 61 crashes.

Table 3 shows the percent error for each model as applied to each case. The percent error for each case is computed as:

$$\frac{|\text{Number of actual crashes - Number of predicted crashes}|}{\text{Number of actual crashes}}$$

The average percent error (APE) is the average of these errors. If a negative number of crashes is predicted, the negative number is subtracted from (i.e., its absolute value is added to) the number of actual crashes. Although some of the periods were very short, a comparison of the errors in Table 3 with the period durations in Table 1 indicated that shorter periods were not necessarily associated with larger errors than longer periods. For example, although Cases 2 and 6, which had the shortest periods of 0.2 year, showed larger errors than the other cases for their respective segment for Models 3, 4, and 5, the key determinant of the error was the model employed, not the length of the period.

Table 2. Number of Predicted Crashes for Each Model

Major Segment	Case	No. Actual Crashes	No. Predicted Crashes Model 1	Model 2	Model 3a	Model 3b	Model 3c	Model 4	Model 5
Corridor I: Route 147 (Richmond)									
Route 60 to Polo (1.58 mi)	1	54	20	25	92	73	61	-19	103
	2	6	4	6	16	12	10	-3	18
	3	153	79	150	281	209	172	-32	311
	4	185	110	211	331	463	320	-55	366
Polo to Route 150 (3.42 mi)	5	79	59	63	252	239	207	-530	288
	6	12	11	10	42	40	34	-92	48
	7	48	39	42	133	125	107	-289	153
	8	246	213	240	638	600	500	-1447	732
	9	213	161	189	426	400	327	-1010	489
Corridor II: Route 250 (Staunton)									
U.S. 11 to Frontier Avenue (1.71 mi)	10	49	96	239	484	209	240	520	433
	11	23	21	50	98	42	48	106	88
	12	71	50	112	222	96	105	239	199
	13	42	37	96	154	132	144	165	138
Frontier to Sanger's Lane (0.89 mi)	14	72	72	110	116	120	111	-452	125
	15	62	96	88	67	142	79	-260	72
	16	42	81	40	24	52	27	-95	26
Corridor III: Route 17 (York)									
Warwick/ Cook Road to Newport News North City Limit (7.3 mi)	17	423	292	422	1344	1463	1422	1156	1574
	18	428	281	451	1290	1353	1338	1145	1496
	19	325	227	354	1026	1056	1064	925	1185
	20	24	21	34	94	95	96	86	108
	21	145	88	145	391	396	398	356	450
	22	72	44	74	192	194	195	177	222
	23	149	92	157	398	396	405	362	455
	24	233	161	282	693	688	705	636	796

1 mi = 1.6 km.

Table 4 summarizes these statistics for the application of each model to the study corridors. The best performing models at first glance are Models 1 and 2. Yet, it is not immediately clear whether differences in performance are statistically significant.

Normally, the *t* test is used to determine if the differences between values are statistically significant. Using the APE summarized in Table 4 and detailed in Table 3, the *t* test statistic is computed as:

$$T = \frac{\overline{X} - \overline{Y}}{\sqrt{\frac{(n_x - 1)Variance(X) + (n_y - 1)Variance(Y)}{n_x + n_y - 2}\left(\frac{1}{n_x} + \frac{1}{n_y}\right)}}$$

Table 3. Error Computations as Percentage of Actual Values (Rounded to Nearest Percentage)

Major Segment	Case	Model 1	Model 2	Model 3a	Model 3b	Model 3c	Model 4	Model 5
Corridor I: Route 147 (Richmond)								
Route 60 to Polo (1.58 mi)	1	63	54	70	35	12	135	91
	2	33	7	165	108	70	145	199
	3	48	2	83	37	12	121	103
	4	41	14	79	150	73	130	98
Polo to Route 150 (3.42 mi)	5	26	21	219	203	162	771	265
	6	8	14	250	232	184	864	300
	7	18	13	178	161	122	702	219
	8	13	2	159	144	103	688	198
	9	24	11	100	88	54	574	129
Corridor II: Route 250 (Staunton)								
U.S. 11 to Frontier Avenue (1.71 mi)	10	97	387	889	326	390	962	784
	11	9	116	328	84	109	360	283
	12	30	58	213	35	49	237	180
	13	12	129	267	214	243	294	228
Frontier to Sanger's Lane (0.89 mi)	14	0	53	62	66	54	727	74
	15	55	41	8	129	27	519	16
	16	94	5	42	23	35	327	37
Corridor III: Route 17 (York)								
Warwick/ Cook Road to Newport News North City Limit (7.3 mi)	17	31	0	218	246	236	173	272
	18	34	5	202	216	213	167	250
	19	30	9	216	225	227	185	264
	20	12	40	293	297	300	256	351
	21	39	0	170	173	175	145	210
	22	39	2	166	169	171	146	208
	23	39	5	167	166	172	143	205
	24	31	21	197	195	203	173	242

1 mi = 1.6 km. The developers of Model 3 noted that "Where there is an actual crash rate available for the base condition, it may be factored by this relative change to project the future crash rate with the changed conditions. In the absence of an actual crash rate, the approach in NCHRP Report 420 would result in an estimate of the relative change in crash rate, and not the crash rate itself."[14]

Table 4. Summary Statistics for Each Model

Statistic	Model 1	Model 2	Model 3a	Model 3b	Model 3c	Model 4	Model 5
Root mean square error	59	46	350	362	348	552	426
Mean absolute error	43	26	237	227	208	389	281
Average percent error (%)	34	42	198	155	141	373	217
Weighted percent error (%)	31	57	186	126	114	377	192

where n_x and n_y are the number of observations for Models 1 and 2, respectively, and X and Y are the mean values of the APE. The value of the t statistic shown here is compared to the Student's t distribution $t(\alpha/2;\, n_x + n_y\text{-}2)$, where α takes on a value of 0.05 for a 95 percent confidence interval. The t statistic for the APE when comparing Models 1 and 2 is 0.45, which is less than $t(0.025;48) = 2.021$. Thus, based on the APE, Model 1 is not significantly more accurate than Model 2 at the 95 percent confidence interval when using the t test. The t test, however, assumes a normal distribution and similar variances (although some have argued that the condition of equal variances is not critical). The ratio of the variances of the APE from Models 1 and 2 are greater than the F statistic of $F(0.05;23,23) = 2.01$, meaning that the hypothesis that the variances are equal should be rejected.[15] Consequently, although the t test does not show a significant difference, it is not necessarily appropriate for these dissimilar distributions if one holds the belief that variances must be similar for the t test to be valid.

Because the variances of the two samples are significantly unequal as measured by the F statistic, a statistical test that does not assume equal variances is needed. The Mann-Whitney U test fulfills this criterion, where the U statistic is computed as

$$U = n_x n_y + \frac{n_x(n_x+1)}{2} - T_x \qquad \text{or} \qquad U = n_x n_y + \frac{n_y(n_y+1)}{2} - T_y$$

where T_x and T_y are the rank sums for samples X and Y, which, in this case, are the APE for Models 1 and 2. For cases where both the x and y samples are greater than 10, Mendenhall pointed out that a modified application is feasible, where the test statistic Z is compared to the $Z_{\alpha/2}$ statistic for a two-tailed test.[16] Thus, if Z is found to be greater than $Z_{\alpha/2}$, then the differences are statistically significant. In this case, for a 95 percent confidence interval, $Z_{\alpha/2} = 1.96$, and Z is computed as

$$Z = \left| \frac{U - (n_x n_y / 2)}{\sqrt{n_x n_y (n_x + n_y + 1)/12}} \right|$$

The results show, again, that the difference in the predictions by Models 1 and 2 is not statistically different. Although Model 2 has a higher APE than Model 1 (see Table 4), a big part of this discrepancy derives from a couple of cases where Model 2 has prediction errors of several hundred percent. When Model 1 is compared with the next best performing model (Model 3c), the difference in performance is statistically significant. Thus, Model 1 does outperform the remaining models except for Model 2. The confidence bars in Table 5 illustrate the performance rankings of the models tested. For example, there is no significant difference among Models 3a, 3b, and 3c. Although Model 3c is significantly better than Model 5, Models 3a and 3b are not. A version of the t test is available that does not presume equal variances; such a version can be used in lieu of the Mann-Whitney U test.

Table 5. Accuracy of Models Without Modification[a]

Accuracy	Model 1	Model 2	Model 3c	Model 3b	Model 3a	Model 5	Model 4
Without modification	Highest -- Lowest						

[a]A horizontal arrow indicates no significant difference.

Figure 4 shows the ratio of the number of crashes estimated by each model divided by the number of crashes for the 24 cases. Ideally, a model should have a ratio of 1.0. Models 1 and 2 were the closest to this value, ranging between 0.2 and 2.0 (except for a few cases where the spike of Model 2 exceeded the upper range). Some of the models were similar in their response to particular sites. Model 4 is not shown because of the computation of negative values.

In summary, without modification, Models 1 and 2 outperformed the other models in terms of predicting the number of crashes.

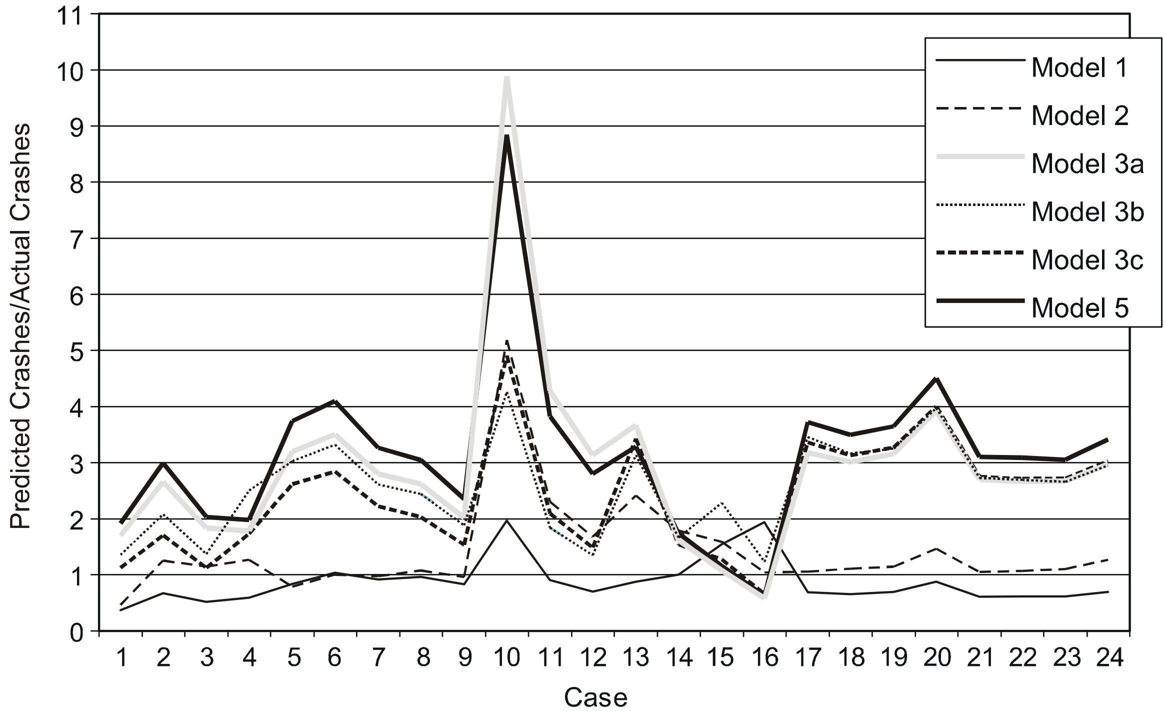

Figure 4. Ratio of Number of Crashes Estimated by Each Model Divided by Number of Crashes for the 24 Cases. Model 4 is not shown because of the computation of negative values.

With Modification

One way to increase the accuracy of a model is to fit it to a particular site and then use the revised model for future predictions. To test the feasibility of adjusting the models for a specific site, Models 3 and 5 were selected for further investigation. Unlike Model 1, they did not yield tolerable error rates without site-specific adjustment, and unlike Model 2, it was easier to make adjustments to the model because Models 3 and 5 have simpler forms that require only one equation for each corridor computation. Using Case 1 as a baseline, the coefficients for unsignalized streets and signalized streets were modified such that Model 5 predicted the number of crashes perfectly for the site. For Model 5, the coefficient for the number of access points was multiplied by a number between 0.00235 and 0.325 to replicate base year conditions. For Models 3a, 3b, and 3c, as suggested by Gluck et al.,[3] the ratio of future computed accident rates to base year computed accident rates, as computed by the models, was used along with actual base year accident rates. Then, the revised model was applied to the remaining 23 cases. In other words, the test was whether “fitting” a model to a base year case could substantially improve its performance. The results were that Model 5 had an APE of 85, which was better than the APE of 217 without modification shown in Table 4. If the analyst had been lucky enough to select a different case for the calibration case—say Case 5—then the APE would have dropped to 37. The practical application of this exercise is significant: a transportation engineer can take a model from elsewhere, modify it slightly to fit base year conditions at a particular site, and then apply the model to future forecasts. The APE does not include the base year computation, which, of course, has a zero percent error.

Realistically, however, an analyst would not have this hindsight: he or she would apply the models and then calibrate them based on the present-day conditions shown in Sites 1 and 5, respectively. Hence, making a universal change to the models based on only one site can improve performance substantially, but it will not guarantee that the model will perform acceptably, as shown by the potential error rate of 85 percent.

A more labor-intensive alternative would be to adjust the model based on each corridor segment. For example, when applying Model 5 to the first segment, the model could be adjusted based on Site 1 and then used for Sites 2, 3, and 4. Then, when applying the model to the second segment, it could be adjusted based on Site 5 and then applied to Sites 6, 7, 8, and 9. In essence, this approach means using present-day conditions at each site to fit the model perfectly to the base year. For Model 5, this initially was done by multiplying the number of access points by an adjustment factor to reproduce the number of crashes for a base year, which dropped the APE to 27. Gluck et al.[3] suggested a different approach, whereby the model is left intact but the expected change in crash rates is multiplied by the current crash rate; this yielded APEs of 27 to 29 for Model 3 and 27 for Model 5, as shown in Table 6.

Although this approach is probably too labor intensive for all corridors in an agency’s jurisdiction, it is practical for key corridors where increases in access are the subject of contentious debate. Thus, in practice, an analyst could apply Model 3 or 5 to a particular corridor, adjust the model weights or application to replicate base year conditions, and then use the revised model for future what-if scenarios. Had such an approach been used for these data, the scenarios would have had an APE between 27 and 29, which is tolerable considering the variability of crash data. This site-specific adjustment is the procedure specified by Gluck et al.[3]

The results shown in Table 6 used the first period for each segment as the base year for fitting the models. To test the sensitivity of Table 6 results to the base year, Model 5 was reapplied using the second period for each segment as the base year to predict crashes during the remaining periods for the segment. The procedure was then repeated using the third and fourth periods as the base period; the APEs were 29, 39, and 30, respectively. Although selection of the base period does affect the APEs, these Model 5 results suggest that the range is still between 27 and 39 percent. The test using the fourth period as the base year excluded the second segment of Corridor II (Cases 14-16) since the segment had only three periods.

Table 6. Average Percent Error for Models 3 and 5, With and Without Modification

Modification Used	Model 3a	Model 3b	Model 3c	Model 5
None[a]	198	155	141	217
Fit the model to each site	27	29	27	27

[a]Model 3 was intended to be applied as shown in the bottom row (i.e., with the model being fit to each site).

Sensitivity

Another way to evaluate a model is to examine its sensitivity, i.e., how it responds to changes in data inputs. All five models use ADT and some measure of signalized and/or unsignalized access density, but some explicitly consider other variables such as median type, land use, presence of residential driveways, and presence of left-turn lanes. Changes to these variables can have a significant or virtually no impact on the number of predicted crashes, depending on the model.

Sensitivity to ADT

An increase in ADT for Models 1 and 3 had a proportional impact on the number of crashes; e.g., an increase of 50 percent in ADT will increase the number of predicted crashes by 50 percent. This was not the case for Models 2 and 4. The impact is nonlinear for Model 2. The exponents shown in Model 2 render the between-signal submodel less sensitive to ADT for corridors with a median or TWLTL treatment: a 50 percent increase in ADT causes a 45 percent increase in predicted crashes. For an undivided segment with a residential or industrial land use, a 50 percent increase in ADT causes a 119 percent increase in predicted crashes. For the signal submodel, the relationship between an increase in ADT and the number of crashes is also nonlinear: a 50 percent increase in ADT can increase the number of predicted crashes between 52 and 54 percent if the values for the number of entering vehicles at a signal are realistic.

Models 1 and 2 predict the number of crashes for a segment, whereas Models 3 and 4 predict the crash rate where the rate is defined as the number of crashes divided by 100 million VMT. Consequently, to apply Models 3 and 4, one must multiply the crash rate by the VMT where VMT is the product of ADT, the number of days for the study period, and the length of the segment. This distinction becomes important because in the case of Model 4, where ADT is first used to determine crash rates and then to determine VMT, a small increase in ADT can cause a surprisingly large change in the number of crashes without increasing the crash rate significantly. For example, depending on the conditions, a 50 percent increase in ADT will result in a 60 percent increase in the predicted number of crashes and a 7 percent increase in the predicted crash rate.

Sensitivity to Median Type

Each model treats median type differently. With Model 1, changing a section from undivided lanes to a TWLTL or median treatment will consistently reduce predicted crashes by

53 and 45 percent, respectively. This is evident from the exponent in the model structure. For Model 2, these reductions are 2 and 26 percent, respectively, for a particular segment. Unlike Model 1, these reductions can be determined only on a case-by-case basis. The lookup tables summarizing Models 3a and 3b indicate that on average, a TWLTL and non-traversable median should reduce predicted crash rates by 20 and 40 percent, respectively. Yet, the developers of Model 3 point out that median impacts cannot be considered in isolation from access: when the number of access points is large (e.g., more than 60/mi, or 96/km), a TWLTL reduces the predicted crash rate by only 4 percent and a median reduces it by 43 percent. Model 4 does not explicitly consider the median type but does include a potentially correlated variable, the left-turn lane availability, which can reduce a predicted crash rate by as much as 514.5 crashes per 100 million VMT.

Because of its structure, one component of Model 2 can have surprising results. For the first study section that was undivided, business land use prevailed, meaning that the variables I_{ri} and I_{bo} were 0 and 1, respectively. Had these models been applied to a corridor with identical geometric characteristics but different land uses such that the variables were reversed, however, Model 2 would have predicted different crash reductions that varied with each case. In fact, with a substantially lower ADT on the segment (e.g., 10,000) and only a residential land use, the model predicted lower crash rates for an undivided segment than for a segment with a raised curb median or TWLTL, although boundary conditions may play a role.

Sensitivity to Signalized and Unsignalized Access

The number of access points for a corridor is included in each model. It is, thus, appropriate to study the sensitivity of the models to changes in the number of access points by testing with a specific example of how changes in access affect the model predictions. Table 7 illustrates how the number of predicted crashes would change based on doubling or tripling the signal density and unsignalized density, assuming all other variables are held constant, for a particular base case. Model 3a required interpolation as values for TWLTL segments with a very low signal density are not given.

Table 7. Percentage Increase in Number of Crashes or Crash Rates Because of Increase in Access Density Using Case 1 as Base Case

Action	Model 1	Model 2	Model 3a	Model 3b	Model 3c	Model 5
Double unsignalized access points	24	3	48	0	13	38
Triple unsignalized access points	64	6	83	0	27	67
Double number of signals	19	58	4	0	0	3
Triple number of signals	39	117	8	0	0	7

Case 1 was used as the base case since most models performed best for this case. However, two factors affecting the sensitivity of the models to the changes in access density need clarification: model structure and the specific case section. Model 3b, of course, is not affected by changes in the number of unsignalized access points. A graphic for Model 3c (which shows the influence of the number of signals and unsignalized access points on crash rates) and the table for Model 3b do indicate that signal density influences crash rates; however, in both cases, signal density is categorized, e.g., less than 2.0 signals per mile (3.2 per kilometer), 2.1 to 4.0 signals per mile (3.4 to 6.4 per kilometer), 4.1 to 6.0 signals per mile (6.6 to 9.6 per kilometer), and more than 6.0 signals per mile (9.6 per kilometer). For this section of Case 1, the signal density was so low that a doubling and tripling of signals did not always affect the number of crashes predicted. In lieu of Table 7, examination of the graphic associated with Model 3c suggests that if one moves from the "less than 2.0 signals per mile" to the category "2.1–4.0 signals per mile," predicted crash rates increase by approximately 64 percent assuming the unsignalized access density shown for Case 1.

The percentages for Model 4 were negative because a negative number of crashes was forecast for the base case. Figure 5, which graphs the response of Model 4 to changes in access, is more illustrative. Figure 5 indicates only signals (counted as two access points for Model 1 or one access point for Model 4); the remainder of the accesses are unsignalized access points. Using conditions similar to those for Case 1 as a baseline, Figure 5 shows that although the number of access points increases the number of predicted crashes, the impact of an increase at the number of accesses is more dramatic when there are fewer access points. Figure 5 shows the opposite effect for Model 1. For Model 1, an increase in the number of access points initially has a small effect on the number of predicted crashes; however, as the number of access points increases, each additional access point has a greater effect on the number of predicted crashes. In this particular case, the proportion of left-turn lanes was kept constant for Model 1.

These two models perform differently since they were developed with different types of roadways. Further, the curves in Figure 5 extend beyond some of the boundary conditions for Model 4. Thus, Figure 5 illustrates two contrasting views about the impact of an additional access point on the predicted number of crashes. Clearly, for each model, the effect of a single additional signal is not uniform but rather can be great or little depending on the number of signals already in place in the corridor.

Ease of Application

A key factor that determines the usefulness of a model is the ease with which data can be obtained and the results interpreted.

Data Collection

The models are similar in that they require the number of unsignalized access points (except Model 3b), roadway segment length, ADT, and (except for Model 3c) median treatment type or the presence of left-turning lanes, all of which can be obtained relatively easily. Models

3a, 3b, and 3c require the least amount of data, and Model 2 requires additional land use and residential driveway data. Model 4 requires another variable: the spacing of individual driveway segments, which may require considerable effort to obtain. This requirement in itself is not a reason to reject Model 4, however, as roadway data become more easily available (e.g., in an automated GIS format). Thus, with the exception of the driveway spacing variable required for Model 4 and the land use data and residential data required for Model 2, the data requirements of the models are similar.

Computation

Once the data have been collected, there is a considerable variation in the ease with which each model can be applied. Models 3a, 3b, and 3c are easier to apply since they require that values be looked up in a graph or table. Models 1 and 4 require slightly greater effort but are still practical since one equation of several independent variables can be applied. Model 2 is the most labor intensive since applying a single equation to a corridor segment is not possible. Instead, an equation is applied to each segment within the corridor that is between two signals, and then the second component of Model 2 is applied to the signals themselves. The result is that for cases where there are five signals, 10 equations are needed for Model 2 and only 1 equation is required with Model 1 or 4. This in itself is not a reason to discount Model 2, for the equations can be placed in a spreadsheet, but it does mean that Model 2 requires substantially more effort to implement.

Boundary conditions also place limitations on the transferability of a model. One reason Model 3a may not have performed quite so well is that for the TWLTL sections on Corridor I, total access density was so low that it was outside the range of Model 3a in some instances. Another reason is that data were used from several states but excluded Virginia.[3] However, as shown previously, simple site-specific modifications, as suggested by the developers of Model 3, render these models usable.

Interpretation

The ease in which the models can be interpreted varies considerably. The tables in Model 3 are the most straightforward, since the impacts of changing access density or median type are readily evident. Model 1, which consists of a single equation, can also be readily understood and explained. Changing a segment from median to TWLTL produces values that are quickly understood. Similarly, the use of only one equation in Model 4 renders it understandable. The only exception is the use of ADT in the computation of crash rate; as stated earlier, the use of ADT in a crash rate combined with the use of ADT to determine VMT can have surprising effects. Additionally, the variable “average difference in driveway spacing” is harder to follow. Finally, the use of a different equation for each median type in Model 2 can make visualizing the impacts of each type of treatment harder, although this can be overcome through the use of sensitivity analyses offered by the developers of the model.[9]

Conveyance of Results

The focus of each model is different. For example, Model 2 accounts for the number of residential driveways whereas the other models do not; Model 3c does not account for median treatments, but Models 3a and 3b do. Differences in definitions are also appropriate to remember. For example, a signal at a four-way intersection is usually counted as two access points, but this was not the case for Models 3b and 3c, which used the number of signals per mile. In addition, a signal counts as one access point when "signalized access density" is computed.[3] Not all models differentiate between signalized and unsignalized access points. Finally, the signals at the endpoints were not included in the segments within the signal density computations, and some might argue that this should have been done (although it would have increased the number of predicted crashes, which already tended to be too high, for these corridors). Appendix B illustrates that with Model 5, this decision affects the number of predicted crashes for the corridors by an average of 3 percent. In hindsight, computations would have been cleaner had corridors been selected such that the boundary was set some distance away from the last signal rather than at the signal itself.

Finally, model performance can become a relevant issue if citizens or officials want to challenge the results a model portrays. Models 1 and 2 are the most accurate in terms of predicting the actual number of crashes without using a modification step. The advantage, therefore, of using either model is that one can state the model is being applied "as it was originally developed"—without modification.

Should data needs or poor performance at other sites necessitate that other models be considered, accuracies of between 27 and 29 percent can be obtained, with a modification step at each site, using Models 3 and 5. Although some modelers may argue that this compromises the integrity of the model, such compromises are still reflected in these relatively low error rates.

Summary

Some of the models can estimate crashes reasonably well as a function of access. Models 3a, 3b, 3c, and 5, when applied according to the approach recommended by Gluck et al. where one uses a site-specific adjustment, estimated crashes on average within 27 percent and 29 percent of the correct value. Model 1, when unmodified, gave average percent errors of 34 percent.

Some of the models are practical enough such that they can be applied without extensive data collection. Although Model 2 requires substantial effort, Models 1, 3, 5, and a slightly modified version of Model 4 use data elements that are reasonable to obtain. Models 3 and 5 require the least data and are the easiest to apply. It is the authors' subjective opinion that the data elements required for Models 1, 3, and 5 are easier for a layperson to understand then a few of the data elements for Models 2 and 4, although that view has not been verified by a sample of laypersons. Further, this last issue is relatively minor and could be overcome with additional explanation or public education efforts as to the meaning of the additional data elements in Model 2 and Model 4.

DISCUSSION

It is probably not possible to select a model that has been developed elsewhere, apply it with the given parameters, and expect it to produce results that replicate crash history at another location. There are several reasons for this:

- *Assumptions under which data are collected and categorized may vary*. Although not an issue with these particular corridors, it is possible that the land use classification (e.g., whether industrial and commercial should be in the same category) or even whether the data element should be used in a model can vary among states. Another data issue is whether the value for TWLTL should be restricted to 0 or 1 (denoting whether a two-way left turn lane is present or not) or whether the TWLTL can be a decimal number between 0 and 1 (e.g., 0.5 for a corridor that is TWLTL for half its length). Had the value of TWLTL been changed to this second representation, Model 1 would not have performed as well as Model 2 for Corridor I (although the differences would not have been significant). Interpretation of the variables can be quite difficult.

- *Models are calibrated to specific data sets*. Not all models are based on the same amount of data, e.g., Model 3 was based on data from several states. Still, it appears that each model was built based on a particular set of data. For example, Model 5 indicates a lower crash rate for four-lane urban arterial roadways without left-turn lanes than for those with left-turn lanes. Yet, as suggested by Model 4, left-turn lanes should reduce crash rates if all other characteristics are equal. Since Model 5 was calibrated based on the experience of one state, it is possible that sites without left-turn lanes in the state might have benefited from other characteristics (such as coordinated signal timing) that might have reduced crash risk.

- *The base data sets may reflect different degrees of driver risk*. It is possible, for example, for two cities to have the same number of signals per mile yet have differences in crash rates because of other factors, such as the progression of the signals, the behavior of the driving population, and the amount of law enforcement, none of which is reflected in the models tested.

- *There is inherent variability in crash rates from year to year*. This natural variation will prevent a model from perfectly forecasting the number of crashes.

- *Error checking is required*. It is essential to avoid data entry errors in order to apply the models correctly. Experience has demonstrated that it is very easy to make mistakes when there are many inputs that propagate throughout the individual submodels.

Although Models 1 and 2 performed the best for the particular corridors employed, other factors should be considered when choosing a model: transparency, feasibility, and the ability to fit the model to the data collected. Fortunately, as was demonstrated with Models 3 and 5, one minor site-specific modification increased the accuracy of the models such that they could be used.

However, it is not correct to assume that the predictive capabilities of the models are reflected solely by their ability to predict the number of crashes. First, the number of crashes is not the only measure of safety at an intersection: Helman pointed out that operational effects such as speed variance and observed numbers of conflicts can also serve as indicators of safety (along with the warning that one should be skeptical of statistical correlations without an understanding of the underlying operations).[17] Second, crashes themselves are rare events. Thus, although Models 1 and 2 clearly outperformed the other models for this particular segment, it is also appropriate to consider other measures of model utility in addition to the ability to predict the number of crashes. Third, model structures themselves are different: a model that uses only a few variables and is based on a large data set, for example, would seem more easily transferred than a model fit to many independent variables and based on a small data set. However, the testing of models with data sets different than those with which they were constructed can highlight the transferability of the models, as was done with three corridors in this effort.

Of persons who have reviewed this work, two opposing schools of thought have emerged with respect to the evaluation of the models. One school argues that one should apply *only* a model without a site-specific adjustment (e.g., Model 1) in order to use the predicted nonlinear response, such as that shown in Figure 5, correctly. An opposing school is that these models are really intended to be used *only* with site-specific adjustment (e.g., Model 3). For both paradigms, however, the impact of the errors is reflected in the test cases presented here, where test data that are different from model training data are used to truly evaluate the model on a new set of conditions. The results show that for both viewpoints, the error rates are still between 27 percent and 34 percent, even though the application of the model does not meet a measure of rigor expressed by one of the schools of thought.

CONCLUSIONS

1. *Without site-specific modification, the accuracy, in terms of being able to predict the actual number of crashes, is quite variable for the different models.* Error rates ranged from 34 percent for Model 1 to a few hundred percent for Model 4. Possible reasons for this discrepancy between the model's predictions and reality include different paradigms inherent in the model, variance in how data elements are defined from one location to another, and application of the model for conditions that are beyond the range for which the model was initially calibrated. Model 1 had the lowest average percent error rate whereas Model 2 had the lowest mean absolute error. Table B-3 in Appendix B illustrates how this can occur.

2. *With a simple site-specific modification, the accuracy of the models can be increased substantially.* Site-specific adjustment reduced error rates from approximately 200 percent to between 27 percent and 29 percent for the models tested. As suggested in by Gluck et al. and

in the section that follows, these modifications are not as labor intensive as recalibrating the models.

3. *The data collection needs vary for the models.* To illustrate, one may consider the input required for one particular variable: the number of unsignalized access points. Model 1 requires the number of unsignalized streets. Model 2 requires the number of unsignalized streets plus the number of unsignalized residential driveways. Model 4 requires the individual spacing between each unsignalized street (but elimination of this variable seemed acceptable).

4. *The computations required for each model vary from the simple to the complex.* Models 3a, 3b, and 3c can be applied by simply using a lookup table or graphic. Model 1 requires more calculations but can be applied for an entire corridor. Model 2 requires a series of sub-calculations first for the portions of the corridor between each signal and then for the signals themselves, such that Model 2 at a particular site required 10 separate equations compared to a single equation for Model 1.

5. *The paradigms reflected in the models are different, and may not apply to another location.* For example, the Model 1 suggests that a TWLTL treatment will decrease crash risk slightly more than does a median treatment. Models 2 and 3 suggest that a median treatment has a far greater crash reduction impact than the TWLTL treatment. Probably the TWLTL treatments for Model 1 were a surrogate for other factors that reduce crash risk, such as reduced speeds. Yet, all of the models generally suggest that an increase in signal density or unsignalized driveways will lead to an increase in crash risk. The differences are in the extent to which these spacings influence crash risk. Figure 5 indicates that, as the number of access points increases, one model presumes a gradual increase in crash rates whereas another model presumes a sharper increase.

6. *Corridor I implies no significant change occurs as a result of slightly increasing access when access is already low.* The corridor in question went from a very low number of access points per mile to a moderate number of access points per mile, and the number of crashes increased only slightly and non-uniformly, as shown in Table 2. By extension, there exists a need to be able to evaluate microscopically the safety impacts of small changes in access, given that state DOTs often face access management decisions, such as the granting or denying of direct access by a business, on a case-by-case basis. This particular corridor follows the same line of thought suggested in Model 1.

7. *Judgment or consistency is required to interpret the application of the models.* For example, should a four-way unsignalized intersection be considered as one or two access points? Although each model in Appendix A has a strict definition that can be applied, it can be argued that the interpretation should be made on a case-by-case basis. As an illustration, consider the northbound direction only: if the turning movements associated with the westbound access point will affect northbound traffic, then certainly such an access has more conflict points than if it only affects southbound traffic. In hindsight, therefore, although application of these models is a good first cut, there may not necessarily be a uniform interpretation for all situations. In that event, consistency is a good remedy: if the number of

conflict points were used as a weighting factor in determining the number of access points for one corridor, then either repeat the same procedure in other areas or do not use it all. Although the latter tack was used in this research, either approach, if applied consistently over time, should give a clearer indication of whether model predictions are accurate.

RECOMMENDATIONS

The study results indicate that using existing traffic data, one can estimate crashes within 27 to 34 percent of the actual number. To do this in practice, the following five steps are recommended. These steps are not intended to replace a research effort that builds a model from primary data, but they can enable practitioners to apply models developed elsewhere.

1. *Select a practical model*. As an illustration, Model 5 meets this test: it relies on data elements that can be collected relatively easily, it uses one basic equation per segment, the meanings of the variables are explained sufficiently, and it does not have nonlinearities that restrict its application.

   For a four-lane urban arterial roadway with left turning lanes, Model 5 is given as:

   $$\text{Accident rate} = \exp(0.12) \bullet (\text{access density})^{(0.49)}$$

2. *If necessary, fit the model to a particular study site*. Overall, Model 1 performed adequately without modification for the two corridors selected. However, accuracy can be improved by fitting Model 5 (or another model being used) to a specific site, using a method given by Gluck et al.[3] as illustrated in Step 3.

   For example, a four-lane urban arterial roadway with left-turning lanes may have these base data:

   - 1.58 mi in length (2.5 km)
   - 27 unsignalized access points + 1 signal at a four-way intersection = 29 access points
   - 29,411 ADT
   - 54 crashes observed over a 1.3-year period.

   The actual crash rate is computed as

   $$\frac{(54\ crashes)(1{,}000{,}000)}{(29{,}411\ vehicles/day)(1.3\ years)(365\ days/year)(1.58\ miles)}$$

   = 2.45 crashes/million VMT = 1.53 crashes/million VkmT.

Model 5 computes the crash rate as

$= \exp(0.12) \bullet (29 \text{ access points}/1.58 \text{ miles})^{(0.49)} = 4.69$ crashes/million VMT

= 2.93 crashes/million VkmT.

Because this error is so large, for future use, the model should be fit to the specific site.

3. *Apply the model consistently.* Considerable judgment must be exercised, and for some situations, such as when a model indicates "use 0 or 1 to indicate TWLTL availability," it was not always clear what the proper interpretation of the variable should be for a particular real-world situation. The approach used herein was to at least be consistent in how the model is applied: thus, if values of 0 or 1 were strictly used for a particular variable on a corridor, then the same convention should be used on other corridors.

   For example, suppose the safety impact of doubling the number of access points from 29 to 58 is to be assessed. Then, because the model should be fit to the specific site, compare the change in crashes, as in:

$$\frac{\exp(0.12)\ \ (29 \text{ access points}/1.58 \text{ miles})^{(0.49)}}{\exp(0.12)\ \ (58 \text{ access points}/1.58 \text{ miles})^{(0.49)}}(54 \textit{ crashes originally}) = 76 \textit{ crashes now}$$

   For computational purposes, an alternative approach would be to multiply the number of access points by 0.21 and then apply the model directly, as in

$$\text{Accident rate} = \exp(0.12) \bullet (\underline{0.21} \bullet \text{access density})^{(0.49)}$$

   since the factor of 0.21 enables the model to predict crashes perfectly for the original base data. (This alternative has some value in that it can simplify the computations slightly, but in some cases, it may increase the APE.)

4. *Acknowledge the error rates based on prior experiences with the model.* Model 1 had an APE, without site-specific modification, of approximately 34. Models 3 and 5, with site-specific calibration, yield APEs of 27 to 29. Thus, up front, these methods cannot forecast crashes perfectly (nor should they, since crashes have inherent variability).

   For example, it should be indicated that:

   "We estimate that, on average, doubling the access density will increase crashes from 54 to 76 assuming the same time period and the same VMT. However, because of a historical average percent error of about 27 percent when using this method, the range of estimated crashes is presented as

76 → 27 percent, or *between 55 and 97 crashes for that same time period*."

5. *Realize the limitations of this approach*. These methods can be applied macroscopically for a corridor to yield a quick estimate of the impacts of increasing the number of signals on crashes. They cannot replace a more detailed study that would investigate specific considerations such as signal progression, grade, turning movements, volume distribution by lane, crash severity, and crash type (e.g., angle crashes may be much more severe than rear end crashes).

   For example, one should note that:

   "Although this illustration explains how increasing access density will increase crashes, it does not consider the benefits that may result from signalizing some of the unsignalized access points if such changes will reduce the number of angle crashes in favor of increased rear end crashes."

SUGGESTIONS FOR FURTHER RESEARCH

1. *Continue to assess the utility of access management models for other corridors*. This study presents a reproducible method for evaluating these types of models. VDOT staff who use these models and find different results from those presented herein should indicate such results to the VDOT Access Management Committee. The committee is a good forum for determining whether the average percent errors (currently thought to be 27 to 34 percent) are acceptable. The results of VTRC's expected future effort to devise a simple automated approach for applying a few of these models should also be presented to this committee.

2. *If technically feasible, devise a software subroutine to archive the date and extent of permanent changes to roadway access.* VDOT has an information system that stores the date an access permit was granted and the date the permit was closed, but it is cumbersome to separate permits that result in an access change (e.g., construction of a driveway for a business) and those that reflect temporary work (such as laying underground cable); furthermore, the date of the change is not always clear. If possible, a software subroutine that indicates the history of access changes to key arterial roadways would assist a VDOT access management program with evaluating how changes in access affect safety and operations.

3. *Investigate the use of simulation models to increase the accuracy of a model.* Operational parameters that can be observed by simulation or direct field study, such as queue length, speed variance, expected number of lane changes, volume-to-capacity ratio, average percent of stopped vehicles, and traffic conflicts (e.g., near misses, activation of brake lights) may be surrogates for safety.[4,18] The fact that some researchers have noted a sharp nonlinear increase in crash risk when a certain threshold of access is exceeded suggests that simulation models may be an appropriate means to determine this breakpoint.[19]

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