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Empirical assessment of the impact of highway design exceptions on the frequency and severity of vehicle accidents

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ABSTRACT

Compliance to standardized highway design criteria is considered essential to ensure roadway safety. However, for a variety of reasons, situations arise where exceptions to standard-design criteria are requested and accepted after review. This research explores the impact that such design exceptions have on the frequency and severity of highway accidents in Indiana. Data on accidents at carefully selected roadway sites with and without design exceptions are used to estimate appropriate statistical models of the frequency and severity of accidents at these sites using recent statistical advances with mixing distributions. The results of the modeling process show that presence of approved design exceptions has not had a statistically significant effect on the average frequency or severity of accidents – suggesting that current procedures for granting design exceptions have been sufficiently rigorous to avoid adverse safety impacts. However, the findings do suggest that the process that determines the frequency of accidents does vary between roadway sites with design exceptions and those without.

1. Introduction

Design exceptions, which are granted to allow highways to be constructed or reconstructed without meeting all current highway design standards, have been a focus of concern for many years because the impact of such exceptions, in terms of their effect on road safety, is not fully understood. Common reasons for considering design exceptions include: impact to the natural environment; social or right-of-way impacts; preservation of historic or cultural resources; sensitivity to context or accommodating community values; and construction or right-of-way costs (Federal Highway Administration, 1997; American Association of State Highway and Transportation Officials, 2004). Because of the potential for serious safety consequences and tort liability, the process for granting design exceptions is very closely monitored by state and federal highway agencies, although practices and standards for granting design exceptions can vary significantly from state to state (National Cooperative Research Program, 2003).

Over the years, there have been numerous research efforts that have attempted to evaluate the safety impacts of design exceptions. For example, Agent et al. (2002) studied the effect of design exceptions on crash rates in the state of Kentucky. They found that the most common design exception was for a design speed lower than the posted speed limit, followed by a lower than standard sight distance, curve radius or shoulder width. With an average of about 39 design exceptions per year in Kentucky, they concluded (based on observations of crash rates) that design exceptions did not result in projects with high crash rates relative to average statewide rates. Unfortunately, when working with crash rates (as opposed to studying the frequency and severity of individual accidents), the amount of data available (which is limited because of the small number of design exceptions granted per year and the highly detailed roadway and accident information required) has made it difficult for previous studies to develop statistically defensible models to assess the safety impacts of design exceptions in a multivariate framework.

Given the scarcity of design-exception data and associated accident data, some have attempted to infer the effects of design exceptions from statistical models that have been estimated on a general cross section of all roadway segments in an effort to uncover the impact of specific design features (shoulder width, median presence, etc.) on the frequency of accidents and the severity of accidents in terms of resulting injuries. Common statistical approaches to determine the relationship between roadway characteristics and accident frequencies include: Poisson and negative binomial models (Jones et al., 1991; Shankar et al., 1995; Hadi et al., 1995; Poch and Mannering, 1996; Milton and Mannering, 1998; Abdel-Aty and Radwan, 2000; Lord, 2006; Wang and Abdel-Aty, 2008; Lord and Park, 2008); zero-inflated negative binomial mod-

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els (Shankar et al., 1997; Carson and Mannering, 2001; Lee and Mannering, 2002); negative binomial with random effects models (Shankar et al., 1998); Conway–Maxwell–Poisson generalized linear models (Lord et al., 2008); negative binomial with random parameters (Anastasopoulos and Mannering, 2009) and dual-state negative binomial Markov switching models (Malyshkina et al., 2009; Malyshkina and Mannering, in press). For the severity of accidents, quantifying the effects of roadway characteristics on vehicle-occupant injuries have been undertaken using a wide variety of models including multinomial logit models, dual-state multinomial logit models, nested logit models, mixed logit models and ordered probit models (O'Donnell and Connor, 1996; Shankar and Mannering, 1996; Shankar et al., 1996; Duncan et al., 1998; Chang and Mannering, 1999; Carson and Mannering, 2001; Khattak, 2001; Khattak et al., 2002; Kockelman and Kweon, 2002; Lee and Mannering, 2002; Abdel-Aty, 2003; Kweon and Kockelman, 2003; Ulfarsson and Mannering, 2004; Yamamoto and Shankar, 2004; Khorashadi et al., 2005; Lee and Abdel-Aty, 2005; Islam and Mannering, 2006; Eluru and Bhat, 2007; Savolainen and Mannering, 2007; Milton et al., 2008; Eluru et al., 2008; Malyshkina and Mannering, 2009).

However, attempting to infer the impact of design exceptions from general roadway-segment data is problematic because roadway segments that are granted design exceptions are likely to be a non-random sample of the roadway-segment population (segments may have common special features that make them more likely to require a design exception). If this is the case, roadway segments prone to design exceptions will share unobserved effects and the relationship of their characteristics to the frequency and severity of accidents may be significantly different than the relationship on the non-design-exception roadway-segment sample. One way of resolving this problem is to gather a sample of sufficient size that includes roadway segments with design exceptions and similar roadway segments without design exceptions (not a random sample of roadway segments without design exceptions), and to use random parameter models to account for possible unobserved heterogeneity. The intent of this study is to use such a sample and modeling approach to assess the effect of design exceptions on the frequency and severity of accidents.

2. Empirical setting

The Indiana Department of Transportation currently has a hierarchy of three levels of highway design criteria. Level One includes those highway design elements which have been judged to be the most critical indicators of highway safety and serviceability. There are 14 Level-One design criteria with minimum standards being met for: design speed; lane widths; shoulder widths; bridge width; bridge structural capacity; horizontal curvature; superelevation transition lengths, stopping-sight distance on horizontal and vertical curves; maximum grade; superelevation rate; minimum vertical clearance; accessibility for the handicapped; and bridge rail safety. Level-Two design criteria are judged to be important to safety and serviceability but are not considered as critical as Level One. Factors in Level-Two criteria include: roadside safety elements; the obstruction-free zone; median and side slopes; access control; acceleration lane length; deceleration lane length; shoulder cross slope; auxiliary lane and shoulder widths; minimum grade for drainage; minimum level-of-service criteria; parking lane width; two-way left-turn width; and critical length of grade. Finally, Level-Three design criteria include all other design criterion not listed in Levels One and Two. This research focuses on the impact of design exceptions within the most important Level-One category, which includes the most critical indicators of highway safety and serviceability.

In Indiana, Level-One design exceptions can be granted for both roadways on the National Highway System (which includes the Interstate Highway System and other roads that are deemed important to the nation's economy, defense and mobility) and those that are not. For all design exceptions, the request must include: a project description, a description of the design feature that does not meet the Department's criteria, detailed construction costs, project design details, a cost-effective analysis, a description of the possible effect the design exception may have on other design elements, a description of remedial actions (to mitigate the possible effects of design exceptions), and a discussion of any other factors that may affect the final decision. In addition to these, the request must include a safety analysis, part of which is based on an assessment of crash experience on the facility for the previous 3-year period. A comparison of the crash rate on the highway under consideration with the statewide crash rate and other comparisons may be provided to justify the request along with other safety-related analyses as appropriate.[1]

For this study, we consider Level-One design exceptions granted between 1998 and 2003. Over this period, our data consist of roadway segments (defined with common geometric conditions) with 35 segments having design exceptions at bridges and 13 segments having design exceptions along the roadway. Of the 35 bridge segments with design exceptions, 6 had exceptions relating to vertical clearance, bridge width or bridge-rail safety. The other "bridge" segments had design exceptions relating cross slopes, stopping-sight distances, superelevation, horizontal/vertical alignment, design speed and shoulder width. Thus the great majority of design exceptions at bridges were not so much features related directly to the bridges (such as vertical clearance and bridge-rail design), but to design compromises relating to traditional roadway geometrics at bridges (which were deemed appropriate given the complexities involved in specific bridge projects). Of the 13 roadways that had design exceptions along the segment, stopping-sight distance, horizontal obstruction clearance, vertical alignment and shoulder width were among the exceptions granted.

With this available design-exception sample, we then collected a control sample (similar segments without design exceptions). Unfortunately, unlike controlled studies in other fields, the multitude of physical roadway characteristics, the potential of drivers' behavioral responses (seat belt usage, speeds, etc.), variations in weather conditions, and so on, make the application of traditional sample-size determination techniques, such as those described in Chow et al. (2007) and countless other sources, nearly impossible. Thus, given the design-exception sample, we developed a control sample that is roughly twice the size – expecting this to be more than sufficient to uncover statistically significant differences.[2] For the control sample (roadways without design exceptions), 69 control bridges and 26 control roadways were carefully chosen so that their characteristics were as similar as possible to those of the design-exception sites (geographic location, road characteristics, traffic conditions, roadway functional classification, and so on). In the case of the control roadway segments, these were chosen to be just a few miles away from their design-exception segment counterparts and control bridge segments were chosen as close as possible (nearest bridge of similar characteristics). With the design exception and control samples, there are combined total 143 sites.[3]

[1] A complete description of Indiana's design-exception policy is available on line at: http://www.in.gov/dot/div/contracts/standards/memos/0605-pc.pdf.

[2] As will be shown in the forthcoming statistical analysis, this sample was of sufficient size to make appropriate statistical inferences.

[3] It is important to note that all bridges are geographically localized sites (they are points on the map). As a result, a procedure was developed for determining the effective length of influence (before and after the localized site), and accidents that occurred on this portion of the roadway were considered. This influence length was

Data on individual vehicle accidents were gathered from Indiana State accident records for the period 2003–2007 inclusive (5 years). The accident data included information on weather, pavement conditions, traffic conditions, number and severity of injuries, contributing factors by each vehicle, type and model of each vehicle, posted speed limit, driver's age and gender, safety belt usage, and so on. The data base created from these data included 127 variables for each accident. In all, data on the 5889 accidents that occurred from 2003 to 2007 on the roadway-segment sample were available for our analysis. Of these accidents, 3429 accidents occurred near bridges, 1192 accidents occurred in the proximity of design-exception bridges and 2237 accidents occurred in the proximity of control bridges. Among 2460 accidents occurred on roadway segments, 739 accidents occurred on design-exception segments and 1721 accidents occurred on control segments. Of the 5889 accidents, 26.39% were single-vehicle accidents, 54.54% were two-vehicle accidents involving two passenger vehicles (car, minivan, sport-utility vehicle or pick-up truck), 7.79% were two-vehicle accidents involving a passenger vehicle and a heavy truck, and 11.28% were accidents involving more than two vehicles.

In terms of injury severities, 77.91% of the 5889 accidents were no-injury (property damage only), 21.68% were possible, evident or disabling injury, and 0.41% were fatalities. Among 1931 accidents that occurred on design-exception sites, 75.71% were no-injury, 23.82% were injury, and 0.47% were fatalities. Among 3958 accidents occurred on non-design-exception (control) sites, 78.97% were no-injury, 20.64% were injury, and 0.39% were fatalities. In terms of accident frequencies, for all 143 roadway segments, the average 5-year accident frequency was 41.13 with a standard deviation of 101.23. Note that for 48 segments with design exceptions, the average 5-year accident frequency was 40.19 with a standard deviation of 90.93. For 95 segments without design exceptions, the average 5-year accident frequency was 41.60 with a standard deviation of 106.51.

Detailed information on roadway segments including segment length, locality of the road (rural/urban), number of lanes, median surface type, median width (in feet), interior shoulder presence and width, outside shoulder presence and width, number of bridges, number of horizontal curves, number of ramps, horizontal curve lengths and radii were determined by using the Google Earth software. Average annual daily traffic volumes were obtained from the Indiana Department of Transportation. Road class (interstate, US route, state route, county road, street), rumble strips, median type, road surface type, speed limit value, road type (one-lane, two-lane, multi-lane, one-way, two-way, undivided, divided, alley, private drive) are taken from the available data on individual accident records. Annual accident frequencies on roadway segments were found by matching locations of segments and individual accidents for each of the 5 years considered (2003–2007 inclusive).

determined to be 1.1 miles (0.55 miles before and 0.55 miles after of the bridge for both directions of traffic) using a maximum likelihood estimation as described in Malyshkina and Mannering (2009). To determine the range of influence of bridges on accidents, a multinomial logit model for the severity of all accidents is estimated considering all accidents within 2 miles of the bridge (along the same highway). A distance variable d_n is defined as the distance between the nth accident and the bridge, and D is defined as the distance of influence along the roadway. A variable is then included in the model where d_n is used only if $d_n < D$, otherwise D is used. Systematically using increasing values of D, the severity model that produced the highest log-likelihood was used to determine D. The idea is that at some distance away from the bridge the effect of distance from the bridge diminishes. We consider this distance to be extent of influence. Thus the total roadway-segment length could be as high as 1.1 miles if the geometric conditions are constant throughout – the 1.1-mile distance would be divided into shorter segment lengths if geometric conditions change (e.g., number of lanes, etc.).

3. Methodological approach

For accident injury severity, three possible discrete outcomes are considered: fatal, injury (possible injury, evident injury, disabling injury) and no-injury (property damage only). To model accident severity, the standard multinomial logit model is used with N available data observations and I possible discrete outcomes giving the probability $P_n^{(i)}$ of the ith outcome in the nth observation as (assuming a generalized extreme value distribution for disturbance terms as shown in McFadden, 1981)[4]

$$P_n^{(i)} = \frac{EXP(\boldsymbol{\beta}_i'\mathbf{X}_{in})}{\sum_{j=1}^{I} EXP(\boldsymbol{\beta}_j'\mathbf{X}_{jn})} \tag{1}$$

where $\mathbf{X}_{in}$ is the vector of explanatory variables influencing injury severity for the nth observation and $\boldsymbol{\beta}_i$ is the vector of model parameters to be estimated ($\boldsymbol{\beta}_i'$ is the transpose of $\boldsymbol{\beta}_i$). Note that the numerator and denominator of the fraction in Eq. (1) can be multiplied by an arbitrary number without any change of the probabilities. As a result, if the vector of explanatory variables does not depend on discrete outcomes (if $\mathbf{X}_{in} = \mathbf{X}_n$), then without any loss of generality one of vectors of model parameters can be set equal to zero. We choose the no-injury vector $\boldsymbol{\beta}_I$ to be zero in this case. This model is estimable by standard maximum likelihood methods (see Washington et al., 2003).

With regard to the magnitude of the influence of specific explanatory variables on the discrete outcome probabilities, elasticities $E_{X_{jn,k}}^{P_n^{(i)}}$ are computed from the partial derivatives of the outcome probabilities for the nth observation as (see Washington et al., 2003) as

$$E_{X_{jn,k}}^{P_n^{(i)}} = \frac{\partial P_n^{(i)}}{\partial X_{jn,k}} \cdot \frac{X_{jn,k}}{P_n^{(i)}} \tag{2}$$

where $P_n^{(i)}$ is the probability of outcome i given by Eq. (1), $X_{jn,k}$ is the kth component of the vector of explanatory variables $\mathbf{X}_{jn}$ that enters the formula for the probability of outcome j, and K is the length of this vector. If $j = i$, then the elasticity given by Eq. (2) is a direct elasticity, otherwise, if $j \neq i$, then the elasticity is a cross elasticity. The direct elasticity of the outcome probability $P_n^{(i)}$ with respect to variable $X_{in,k}$ measures the percent change in $P_n^{(i)}$ that results from an infinitesimal percentage change in $X_{in,k}$. The cross elasticity of $P_n^{(i)}$ with respect to variable $X_{jn,k}$ (note now with $j \neq i$) measures the percent change in $P_n^{(i)}$ that results from an infinitesimal percentage change in $X_{jn,k}$. It is customary to report averaged elasticities, which are the elasticities averaged over all observations. Thus, the averaged direct elasticity is given by (see Washington et al., 2003)

$$\bar{E}_{i;X_k}^{(i)} = \langle E_{X_{in,k}}^{P_n^{(i)}} \rangle_n = \langle [1 - P_n^{(i)}] \times \beta_{i,k} X_{n,k} \rangle_n \tag{3}$$

[4] Past research indicates that the most widely used statistical models to study injury severities have been the multinomial logit model (with nested and mixed logit extensions) and the ordered probit model. However, there are at least two potential problems with applying ordered probability models to accident severity outcomes (see Savolainen and Mannering, 2007). The first relates to the fact that non-injury accidents are likely to be under-reported in accident data because they are less likely to be reported to authorities. The presence of under-reporting in an ordered probability model can result in biased and inconsistent model parameter estimates. In contrast, the parameter estimates of the standard multinomial logit model remain consistent in the presence of such under-reporting, except for the constant terms (Washington et al., 2003). The second problem with ordered probability models is related to undesirable linear restrictions that such models place on influences of the explanatory variables (Eluru et al., 2008; Washington et al., 2003). As a result of the ordered probit limitations, the multinomial logit approach is used herein.

and the averaged cross elasticity is given by

$$\bar{E}^{(j)}_{i;X_k} = \langle E^{P^{(j)}_n}_{X_{in,k}} \rangle_n = -\langle P^{(i)}_n \times \beta_{i,k} X_{n,k} \rangle_n \tag{4}$$

where brackets $\langle \cdots \rangle_n$ indicate averaging over all observations $n = 1, 2, 3, \ldots, N$.

The elasticity formulas given above are applicable only when explanatory variable $X_{jn,k}$ used in the outcome probability model is continuous. In the case when $X_{jn,k}$ takes on discrete values, the elasticities given by Eq. (2) cannot be calculated, and they are replaced by pseudo-elasticities (for example, see Washington et al., 2003). The later are given by the following equation, which is the discrete counterpart of Eq. (2):

$$E^{P^{(i)}_n}_{X_{jn,k}} = \frac{\Delta P^{(i)}_n}{\Delta X_{jn,k}} \cdot \frac{X_{jn,k}}{P^{(i)}_n} \tag{5}$$

where $\Delta P^{(i)}_n$ denotes the resulting discrete change in the probability of outcome i due to discrete change $\Delta X_{jn,k}$ in variable $X_{jn,k}$.

In addition to the simple multinomial logit model, we also consider mixed multinomial logit models of accident severity to account for possible variations across observations. In a mixed multinomial logit model, the probability of the ith outcome in the nth observation is (see McFadden and Train, 2000; Milton et al., 2008)

$$\tilde{P}^{(i)}_n = \int P^{(i)}_n q(\boldsymbol{\beta}_i|\phi_i)\, d\boldsymbol{\beta}_i \tag{6}$$

The right-hand-side of Eq. (6) is a mixture of the standard multinomial probabilities $P^{(i)}_n$, given by Eq. (1). Probability distribution $q(\boldsymbol{\beta}_i|\boldsymbol{\varphi}_i)$ is the distribution of the multinomial logit parameters $\boldsymbol{\beta}_i$, given fixed parameters $\boldsymbol{\varphi}_i$. This model is estimable by standard simulated likelihood methods (see McFadden and Train, 2000).

With regard to accident frequency, the most commonly used statistical models for count data are the Poisson and negative binomial models. The Poisson model is a special case of the more general negative binomial model (a negative binomial model reduces to a Poisson model when the over-dispersion parameter is zero). As a result, without loss of generality, we consider only the negative binomial model in this study.

With λ_{tn} being the mean accident rate (Poisson parameter) on roadway segment n during time period t (with $\lambda_{tn} = EXP(\boldsymbol{\beta}'\mathbf{X}_{tn}) + \varepsilon_{tni}$ and $EXP(\varepsilon_i)$ being a Gamma-distributed error term with mean 1 and variance α^2) the standard negative binomial model of the probability of accident frequency A_{tn}, which is the number of accidents occurred on road segment n during some time period t, is given as (Washington et al., 2003)

$$P^{(A_{tn})}_{tn} = \frac{\Gamma(A_{tn} + 1/\alpha)}{\Gamma(1/\alpha) A_{tn}!} \left(\frac{1}{1+\alpha\lambda_{tn}}\right)^{1/\alpha} \left(\frac{\alpha\lambda_{tn}}{1+\alpha\lambda_{tn}}\right)^{A_{tn}} \tag{7}$$

where $\mathbf{X}_{tn}$ is the vector of explanatory variables during time period t for the roadway segment n, Γ is the gamma-function. The vector $\boldsymbol{\beta}$ and the over-dispersion parameter α are unknown estimable parameters of the negative binomial model. This model is estimable by standard maximum likelihood estimation (see Washington et al., 2003).

With regard to the magnitude of the influence of specific explanatory variables on the expected accident frequency, instead of the elasticities used for the severity analysis we use marginal effects which are easier to interpret for count-data models. The marginal effect is computed as (see Washington et al., 2003)

$$\frac{\partial E(A_{tn}|\mathbf{X}_{tn})}{\partial X_{tn,k}} = \frac{\partial \lambda_{tn}}{\partial X_{tn,k}} = \frac{\partial}{\partial X_{tn,k}}[\exp(\boldsymbol{\beta}'\mathbf{X}_{tn})] = \lambda_{tn}\boldsymbol{\beta} \tag{8}$$

where $X_{tn,k}$ is the kth component of the vector of explanatory variables $\mathbf{X}_{tn}$. The marginal effect gives the effect that a one unit change in the explanatory variable $X_{tn,k}$ has on the mean accident frequency λ_{tn}. As was the case with elasticities, because each observation generates its own marginal effect, the average across all observation will be reported in the forthcoming empirical analysis.

The possibility of a mixed (random parameters) negative binomial model, which is defined as in the mixed multinomial logit model, is also considered. To allow for such random parameters in the negative binomial model, individual parameters can be written as $\beta_{tn} = \beta + \xi_{tn}$ where ξ_{tn} is a randomly distributed term (for example, a normally distributed term with mean zero and variance σ^2). With this, the mean accident frequency becomes $\lambda_{tn}|\xi_{tn} = EXP(\boldsymbol{\beta}'\mathbf{X}_{tn} + \varepsilon_{tn})$ with the corresponding probabilities now $P^{(A_{tn}|\xi_{tn})}_{tn}$ (see Eq. (7)). This random parameters negative binomial model can be estimated by standard maximum likelihood simulation procedures with the log-likelihood function (see Greene, 2007; Anastasopoulos and Mannering, 2009):

$$LL = \sum_{\forall tn} \ln \int_{\xi_{tn}} g(\xi_{tn}) P^{(A_{tn}|\xi_{tn})}_{tn}\, d\xi_{tn} \tag{9}$$

where $g(\cdot)$ is the probability density function of the ξ_{tn}.

Finally, to test whether a statistically significant difference between models estimated separately for design-exception and non-design-exception segments exits, a likelihood ratio test is applied for both accident severity and frequency. The test statistic is (Washington et al., 2003)

$$X^2 = -2[LL(\boldsymbol{\beta}_{all}) - LL(\boldsymbol{\beta}_{DE}) - LL(\boldsymbol{\beta}_{NDE})] \tag{10}$$

where $LL(\boldsymbol{\beta}_{all})$ is the model's log-likelihood at convergence of the model estimated on all data, $LL(\boldsymbol{\beta}_{DE})$ is the log-likelihood at convergence of the model estimated with only sites with design exceptions, and $LL(\boldsymbol{\beta}_{NDE})$ is the log-likelihood at convergence of the model estimated on sites without design exceptions. If the number of observations is sufficiently large, the test statistic X^2 is χ^2-distributed with degrees of freedom equal to the summation of parameters estimated in the design-exception and non-design-exception models minus the number of parameters estimated in the total-data model (Gourieroux and Monfort, 1996).

In the case when the data sample size is small, the asymptotic χ^2 distribution is likely to be a poor approximation for the test statistic X^2. To resolve this problem, Monte Carlo simulations can be undertaken to find the true distribution of the test statistic X^2. This is done by first generating a large number of artificial data sets under the null hypothesis that the model is the same for segments with and without design exceptions. Then the test statistic values X^2, given by Eq. (10), for each of the simulated data sets are computed, and these values are used to find the true probability distribution of X^2. This distribution is then used for determining the p-value that corresponds to the X^2 calculated for the actual observed data. The p-value is then used for the inference. This Monte Carlo simulations-based approach to the likelihood ratio test is universal, it works for any number of observations (Cowen, 1998).

4. Estimation results

The estimation results for the mixed multinomial logit accident severity model are given in Table 1. The findings in this table show that the severity model has a very good overall fit (McFadden ρ^2 statistic above 0.5) and that the parameter estimates are of plausible sign, magnitude and average elasticity. We find that two variables produce statistically significant random parameters (in the mixed-logit formulation). The indicator variable for having two vehicles involved in the crash was found to be normally distributed in the injury-crash outcome with a mean −1.85 and standard deviation of 2.65. This means that for 75.7% of the observations having two vehicles involved in the crash reduced the probability of the

Table 1
Estimation results for the mixed multinomial logit model of accident severities.

Variable	Parameter (t-ratio)		Averaged elasticities[a]					
	Fatal	Injury	$\bar{E}_1^{(1)}$	$\bar{E}_1^{(2)}$	$\bar{E}_1^{(3)}$	$\bar{E}_2^{(1)}$	$\bar{E}_2^{(2)}$	$\bar{E}_2^{(3)}$
Fixed parameters								
Constant	−6.09 (−10.4)	−4.59 (−7.26)	–	–	–	–	–	–
Two vehicle indicator (1 if two vehicles are involved, 0 otherwise)	−2.41 (−3.63)	–	−1.45	0.0012	0.0030	–	–	–
Snow-slush indicator (1 if roadway surface was covered by snow or slush, 0 otherwise)	−0.843 (−2.41)	−0.843 (−2.41)	−0.0460	0.0001	0.0002	0.0038	−0.0255	0.0038
Help indicator (1 if help arrived in 10 min or less after the crash, 0 otherwise)	0.609 (3.69)	0.609 (3.69)	0.358	−0.0008	−0.0014	−0.0488	0.1576	−0.0488
Number of occupants in the vehicle at fault	−0.328 (−2.54)	−0.328 (−2.54)	−0.479	0.0010	0.0016	0.0574	−0.2301	0.0574
The largest number of occupants in any single vehicle involved in the crash	0.303 (2.70)	0.303 (2.70)	0.526	−0.0013	−0.0040	−0.0666	0.243	−0.0666
Age of the vehicle at fault (in years)	0.139 (3.38)	–	0.972	−0.0033	−0.0055	–	–	–
Age of the oldest vehicle involved in the accident (in years)	–	0.0417 (2.96)	–	–	–	−0.0457	0.160	−0.0457
Non-intersection indicator (1 if the accident did not occur at an intersection, 0 otherwise)	–	−0.409 (−2.43)	–	–	–	0.0285	−0.129	0.0285
Indiana vehicle license/fault indicator (1 if the at-fault vehicle was licensed in Indiana, 0 otherwise)	–	0.390 (2.11)	–	–	–	−0.0390	0.145	−0.0390
Urban indicator (1 if the accident occurred in an urban location, 0 otherwise)	–	−0.686 (−2.78)	–	–	–	0.0533	−0.210	0.0533
Driver/cause indicator (1 if the primary cause of accident is driver-related, 0 otherwise)	–	2.27 (8.28)	–	–	–	−0.255	0.814	−0.255
Posted speed limit (if the same for all vehicles involved)	–	0.0239 (2.66)	–	–	–	−0.129	0.511	−0.129
Signal/fault indicator (1 if the traffic control device for the vehicle at fault is a signal, 0 otherwise)	–	0.724 (2.90)	–	–	–	−0.0146	0.0353	−0.0146
Female-fault indicator (1 if gender of the driver at fault is female, 0 otherwise)	–	0.308 (2.06)	–	–	–	−0.0155	0.0543	−0.0155
Middle age indicator (1 if the age of the oldest driver is between 30 and 39 years old, 0 otherwise)	–	0.577 (3.12)	–	–	–	−0.0162	0.0501	−0.0162
Design exception and segment-type parameters								
Design-exception indicator (1 if the road segment had a design exception, 0 otherwise)	0.460 (0.752)	−0.0974 (−0.622)	0.147	−0.0004	−0.0007	0.0038	−0.0149	0.0038
Bridge-segment indicator (1 if the road segment is a bridge segment, 0 otherwise)	−0.244 (−0.395)	0.175 (1.11)	−0.141	0.0003	0.0005	−0.0119	0.0443	−0.0119
Random parameters								
Two vehicle indicator (1 if two vehicles are involved, 0 otherwise)	–	−1.85 (−3.67)	–	–	–	0.0223	−0.0185	0.0223
Interstate indicator (1 if the roadway segment is an interstate highway, 0 otherwise)	–	−2.26 (−2.59)	–	–	–	−0.0141	0.104	−0.0141
Standard deviations of parameter distributions								
Two vehicle indicator (1 if two vehicles are involved, 0 otherwise)	–	2.65 (3.86), normal	–	–	–	–	–	–
Interstate indicator (1 if the roadway segment is an interstate highway, 0 otherwise)	–	6.03 (3.74), uniform	–	–	–	–	–	–
Model statistics								
Log-likelihood at zero				−4027.51				
Log-likelihood at convergence				−1828.38				
Number of parameters				21				
Number of observations				3666				
McFadden ρ^2				0.546				

[a] Refer to Eqs. (3)–(6), where subscript and superscript outcomes “1”, “2”, “3” correspond to “fatal”, “injury”, “no-injury”. The subscript represents the outcome that is being considered for X. The superscript represents the outcome that is being considered for the change in probability. If subscript and superscript values are the same, the elasticity is a direct elasticity estimating the effect of a change in variable X in the subscript outcome has on the probability of that same outcome. If subscript and superscript values differ, they are cross elasticities estimating the effect of a change in variable X in the subscript outcome has on the probability of the superscript outcome.

injury outcome and for 24.3% of the observations having two vehicles involved increased the probability of an injury outcome. Also, the parameter for the interstate-highway indicator variable is uniformly distributed with a mean of −2.26 and a standard deviation of 6.03.

Some other interesting results included the age of the at-fault vehicle (where elasticity values show that a 1% increase in at-fault vehicle age increases the probability of a fatal injury by 0.972%) and the age of the oldest vehicle involved in the accident (which also increased the probability of an injury). These two variables may be capturing improvements in safety technologies on newer vehicles.

The presence of snow and slush was found to reduce the probability of fatality and injury. This finding is consistent with a number of previous studies. For example, Shankar et al. (1996) found that accidents occurring on snow-covered pavements in Washington State significantly reduced the probability of evident injury, disabling injury and fatality. This weather-related finding may be due to a number of factors, including the possibility that people may be driving more cautiously during adverse conditions. In fact, there are a number of studies that provide empirical evidence showing the degree to which drivers slow their speed during adverse weather conditions (see, for example, Liang et al., 1998).

Accidents that did not occur at an intersection and those that occurred in urban areas were less likely to result in an injury (by an average of 12.9% and 21% respectively as indicated by the average elasticities). Finally, accidents involving female drivers who were at fault, having the at-fault vehicle under signal control, having higher posted speed limits, and having driver-related causes indicated as the primary cause of the accident all resulted in a higher likelihood of an injury accident.

Turning to the effect of design exceptions on the severity of accidents, note that in Table 1 the design-exception parameter is statistically insignificant, suggesting that design exceptions do not have a statistically significant impact on the severity of accident injuries. To provide further evidence, a likelihood ratio test, described in the previous section, was conducted to determine whether there is a statistically significant difference between mixed multinomial logit models estimated for severity of the accidents that occurred on design-exception sites and non-design-exception (control) sites. Because the accident severity data sample is large (5889 accidents), we rely on this χ^2 approximation when doing likelihood ratio test for the accident severity model. The test statistic X^2 value, given by Eq. (10), was 27.21 with 21 degrees of freedom. The corresponding p-value based on the χ^2 distribution, is 0.164 (the critical χ^2 value at the 90% confidence level is 29.62). Therefore, the hypothesis that design-exception and non-design-exception model parameters were statistically the same cannot be rejected, and it can be concluded that design exceptions have not had a statistically significant effect on the parameter estimates in the accident severity model.[5]

With regard to accident frequency, we attempted the estimation of a random parameters negative binomial model. Trying various distributions, all estimated parameters were determined to be fixed (standard deviations of parameter estimates across the population were not significantly different from zero implying that the parameters were fixed across observations). Thus, a standard negative binomial model is estimated on 5-year accident frequencies, and 122 of the 143 road segments had complete data for use in the accident-frequency model estimation. For these 122 road segments, the average 5-year accident frequency was 34.84 with a standard deviation of 65.51.

[5] Likelihood ratio tests were also conducted to determine if there were differences in accident severities between those roadway segments near bridges and those segments that are not near bridges. To test whether bridge and non-bridge segments were statistically different, we estimated a model on all data and then compared to the separately estimated bridge and non-bridge models. The test statistic is $X^2 = -2[LL(\boldsymbol{\beta}_{all}) - LL(\boldsymbol{\beta}_{bridge}) - LL(\boldsymbol{\beta}_{non\text{-}bridge})]$ where $LL(\boldsymbol{\beta}_{all})$ is the model's log-likelihood at convergence of the model estimated on all data, $LL(\boldsymbol{\beta}_{bridge})$ is the log-likelihood at convergence of the model estimated with only bridge-segment data, and $LL(\boldsymbol{\beta}_{non\text{-}bridge})$ is the log-likelihood at convergence of the model estimated on non-bridge segment data. This statistic is χ^2 distributed with degrees of freedom equal to the summation of parameters estimated in the bridge and non-bridge models minus the number of parameters estimated in the total-data model. The X^2 value of this test was 26.59 with 21 degrees of freedom. The corresponding p-value is 0.185 (the critical χ^2 value at the 90% confidence level is 29.62). Therefore, we cannot reject the hypothesis that the parameter estimates in bridge and non-bridge segments are the same, and, as a result, these segments are considered together.

The negative binomial estimation results are given in Table 2 along with the marginal effects as previously discussed. The results show that the parameter estimates are of plausible sign and magnitude and the overall statistical fit is quite good (McFadden ρ^2 statistic above 0.75).

Table 2 shows that the design-exception parameter is statistically insignificant again suggesting that design exceptions do not have a statistically significant impact on the mean frequency of accidents.[6]

Turning to the specific model results shown in Table 2, we find that urban roads have a significantly higher number of accidents and that the higher the degree of curvature (defined as 18,000 divided by π times the radius of the curve in feet), the lower the accident risk. This second finding seems counterintuitive (sharper curves result in fewer accidents) but is consistent with the findings of a number of previous studies (for example, Shankar et al., 1997 and Anastasopoulos et al., 2008) and could be reflecting the fact that drivers may be responding to sharp curves by driving more cautiously and/or that such curves are on lower design-speed segments with inherently lower accident risk. Other results in Table 2 show that: increases in average annual daily traffic per lane increase accident frequencies (the marginal effect shows that for every 1000 vehicle increase in AADT per lane the 5-year accident frequency goes up by 2.04 accidents); longer road-segment lengths increase accident frequencies (this is an exposure variable because it is related to the number of miles driven on the roadway segment); and for interstates the higher the number of ramps the greater the number of accidents (with marginal effects indicating that each ramp increases the 5-year accident rate by 6.52 accidents).

The asphalt surface indicator was found to result in fewer accidents. This is likely capturing unobserved information relating to pavement friction and condition (as measured by the International Roughness Index, rutting measurements, and so on) because other studies with detailed pavement-condition information have found the type of roadway surface (concrete or asphalt) to be statistically insignificant (see Anastasopoulos et al., 2008 and Anastasopoulos and Mannering, 2009). Also, for multilane highways, the presence of an interior shoulder was found to decrease accident frequency, presumably as a result of providing more suitable recovery space. However, the estimation findings also show that multilane highways with median widths that were less than 30 ft have lower accident frequencies. We speculate that this finding may be capturing unobserved characteristics associated with the highway segments that had medians less than 30 ft (which was about 57% of the sample) or 30 ft or more. Such unobserved characteristics could include the geographic locations of segments, traffic characteristics (in terms of truck/passenger car sizes and mix), and driver behavior (actual driving speeds, following distances, lane changing frequencies, etc.). Some or all of these may be varying systematically between roadway segments with lower and higher median widths.

We also conducted likelihood ratio tests as was done for the mixed-logit severity analysis. The test statistic X^2, given by Eq. (10) was 23.00 with 10 degrees of freedom. The corresponding p-value based on the χ^2 distribution, is 0.0107 (the critical χ^2 value at the 90% confidence level is 15.99). However, because we have only a limited number of accident-frequency observations (equal to 122), the parameter estimates of the separate frequency models (for design-exception and non-design-exception segments) are not necessarily statistically reliable (high standard errors) and the asymptotic χ^2 distribution is likely to be a poor approximation for the test statistic X^2. To arrive at more defensible results for

[6] The bridge-segment indicator variable was also statistically insignificant suggesting no difference between bridge and non-bridge segments.

Table 2
Estimation results for the standard negative binomial model of 5-year accident frequencies.

Variable	Parameter (t-ratio)	Averaged marginal effect
Constant	3.12 (7.23)	–
Urban indicator (1 if the accident occurred in an urban location, 0 otherwise)	1.80 (4.43)	71.9
Degree of curvature of the sharpest horizontal curve on the road segment	−0.0562 (−2.08)	−2.24
Average annual daily traffic per lane in thousands	0.0509 (2.28)	2.04
Natural logarithm roadway segment length in miles	0.937 (2.83)	37.5
Total number of ramps on road segment if interstate	0.163 (2.00)	6.52
Asphalt surface indicator (1 if roadway surface is asphalt, 0 otherwise)	−1.08 (−3.13)	−43.4
Multilane highway interior shoulder indicator (1 if road segment is a divided highway with an interior shoulder, 0 otherwise)	−1.25 (−3.10)	−50.1
Multilane highway median-width indicator (1 if median width is less than 30 feet, 0 otherwise)	−0.905 (−2.55)	−36.2
Design exception and segment-type parameters		
Design-exception indicator (1 if the road segment had a design exception, 0 otherwise)	0.0601 (0.204)	–
Bridge-segment indicator (1 if the road segment is a bridge segment, 0 otherwise)	−0.155 (−0.414)	–
Dispersion parameter		
Over-dispersion parameter (α)	1.37 (7.94)	–
Model statistics		
Log-likelihood at convergence	−1963.29	
Log-likelihood at zero	−472.77	
Number of parameters	12	
Number of observations	122	
McFadden ρ^2	0.759	

the likelihood ratio test, Monte Carlo simulations are conducted to determine the true distribution of the test statistic X^2, as previously described. These results are shown in Fig. 1. In this figure the histogram shows the true distribution of X^2 that was obtained from the Monte Carlo simulations (10^5 artificial data sets were used) and the solid curve shows the approximate asymptotic χ^2 distribution of X^2. The vertical dashed line in this figure shows the X^2 value computed for the observed accident-frequency data. The true p-value, calculated by using the simulations-based distribution of X^2 (this p-value is equal to the area of the histogram part located to the right of the dashed line), is 0.0311, which is about three times larger than the approximate χ^2-based value 0.0107. However, both these values are below 5%. Therefore, the hypothesis that design-exception and non-design-exception sites had the same parameter estimates is rejected, and it can be concluded that design-exception and non-design-exception segments have statistically different parameter estimates. This is an extremely important finding. The fact that the indicator variable for design exceptions was found to be statistically insignificant suggests that the difference between design-exception and non-design-exception segments in terms of higher accident frequencies is not significant. However, the likelihood ratio test results suggest that the process (estimated parameters) generating the accident frequencies of the design-exception and non-design-exception segments are significantly different. This has important implications in that potential changes in explanatory variables **X** could produce significantly different accidents frequencies between design-exception and non-design-exception segments. While more data would be needed to completely uncover these effects, this finding indicates that caution needs to be exercised even when granting design exceptions that appear to have been acceptable based on historical data.

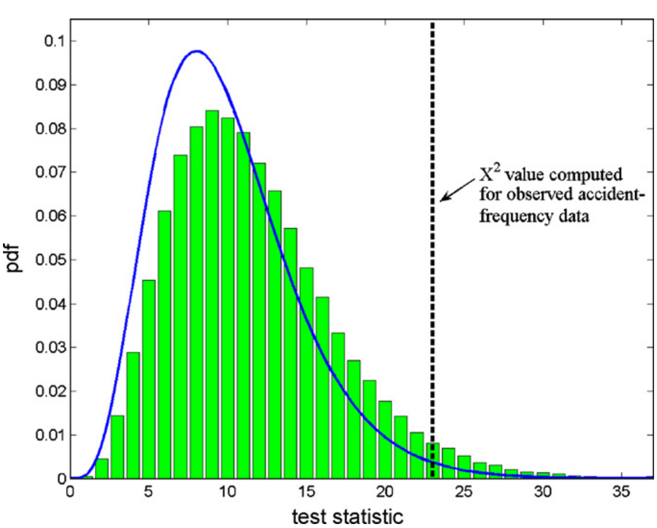

Fig. 1. The X^2 test statistic distribution for accident frequency (histogram shows the true distribution of X^2 from the Monte Carlo simulations and the solid curve shows the approximate asymptotic χ^2 distribution of X^2).

5. Summary and conclusions

Overall, our results suggest that the current process used to grant design exceptions has been sufficiently strict to avoid adverse safety consequences resulting from design exceptions – although the finding that different processes may be generating the frequencies of accidents in design-exception and non-design-exception segments is cause for concern with regard to the future granting of design exceptions.[7]

Our specific findings provide some insight into areas where caution should be exercised when granting Level-One design exceptions. With regard to the severity of accidents, while most of the factors that affected severity were driver characteristics, we did find that urban-area accidents have a lower likelihood of injury

[7] To gain further insight into this potentially important finding, additional data (ideally from other states and geographic regions) would be needed to explore the underlying differences in factors generating accident occurrences at sites with and without design exceptions.

and that the posted speed limit is critical (higher speed limits result in a significantly higher probability of an injury accident). Thus, urban/rural location and design exceptions on highways with higher speed limits need to be given careful scrutiny.

With regard to the frequency of accidents, we find that horizontal curvature is critical and thus special attention needs to be paid to design exceptions relating to horizontal curves. For multilane highways, the presence of interior shoulders was found to significantly reduce the frequency of accidents so this should be considered carefully when granting design exceptions. Also, higher accident frequencies were found in urban areas suggesting that special attention should be given to design exceptions that could compromise safety in these areas (as expected, urban areas have higher accident frequencies but lower severities). Finally, the asphalt surface indicator was found to result in fewer accidents. As stated previously, this is likely capturing unobserved information relating to pavement friction and condition (as measured by the International Roughness Index, rutting measurements, and so on), and suggests that friction and pavement conditions have to be watched closely when design exceptions are granted.

In terms of a process in the form of a decision support system for guiding future Level-One design exceptions, the statistical findings of this research effort suggest that using previous design exceptions as precedents would be a good starting point. While the current study indicates that the design exceptions granted over the 1998–2003 timeframe have not adversely affected overall safety, the number of available design exceptions is too small to make broad statements with regard to policy. Thus, a case-by-case comparison with previously granted design exceptions is the only course of action that can be recommended.

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