Attempt To Define Relationship Between Forces To Crash-Test Vehicles and Occupant Injury in Similar Real-World Crashes

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Roadside safety devices are designed to protect vehicle occupants from injuries. Because new designs cannot be tested with human occupants, the safety of new designs has traditionally been measured in crash tests, with criteria for success being structural adequacy of the device, vehicle trajectory after collision, and occupant risk. The relationship between occupant risk as measured in crash tests and the ultimate measure of occupant risk—driver injury—is explored. Vehicles from the 1973 through 1986 North Carolina crash files were matched with similar crash-test vehicles on the basis of feature struck; make, model, and year; and Traffic Accident Data Project impact location and severity. Contingency table analysis and logistic regression modeling were used to explore the potential relationship between crash-test measures and injury. Results indicated the lack of a strong relationship between driver injury and peak 50-msec longitudinal and lateral forces to the vehicle or momentum change. With respect to the newer proposed "flail space" measures of occupant risk, the limited amount of data available made conclusions virtually impossible to draw. Because of the continuing need for a strong link between crash-test measures and injury, recommendations for modification of the methodology, the data files, the test matrix, and the measures themselves are provided.

Roadside safety features protect occupants of errant vehicle from serious injury (e.g., guardrail shielding steep sideslopes or bridge piers and bridge rail shielding waterways) or enhance the driving or living environment (e.g., signs, luminaire supports, utility poles). This hardware is designed to "either gently stop the vehicle, or readily break away, without subjecting occupants to major injury producing forces" (*1*).

In the best of all hardware-development environments, alternative designs for each safety device would be tested in the real world, with changes in injury patterns to vehicle occupants being the major criteria of success. However, because of cost reasons related to available sample sizes, limitations in police-reported accident data bases related to details on the specific design of a roadside feature, and moral and legal reasons requiring that designs be tested before use, such a research and development scenario is not possible.

Instead, research conducted by FHWA and the states related to roadside safety device performance has involved crash-testing of vehicles striking a given device. In such testing, which is designed to represent practical worst-case scenarios, evaluation criteria used include measures of (a) structural adequacy of the device (e.g., whether the vehicle penetrated the guardrail), (b) vehicle trajectory after collision (e.g., whether the vehicle rebounds into lanes of traffic), and (c) occupant risk.

Perhaps the most important criterion, and the one with which the most problems arise in measurement, is occupant risk. Ultimate occupant risk is measured by occupant injury distributions. However, because human subjects cannot be put in crash-test vehicles, the criterion traditionally used is a measure of *g*-forces (accelerations) acting on the vehicle or a crash dummy, some measure of change in speed of the vehicle upon striking an obstacle (delta-*v*), or some measure of momentum change. Detailed crash-test procedures, measurements, and acceptance guidelines in terms of "preferred" and "acceptable" vehicle accelerations (e.g., the peak 50-msec average acceleration measured near the vehicle center of mass, or the maximum allowable momentum change) have been established for many years (*2–4*).

Unfortunately, although the test conditions, the measurement conditions, and the acceptance guidelines are specified in great detail, the same publications that document the guidelines note that "These criteria are not valid, however, for use in predicting occupant injury in real or hypothetical accidents" (*4*). Although logical attempts to link these vehicle accelerations with occupant injury have been made [e.g., the TTI "severity index" (*5*)], they are not suitable for "direct assessment of human injury" (*6*). Clearly, at that time, the linkage between crash-test and occupant injuries was tenuous at best.

Supporting criticisms of these measures in an earlier work (*7*), Michie (*8*) hypothesized that, although these measures might indicate in some overall manner the degree of occupant risk, they are inconsistent, inadequate, and overly conservative. Michie further noted that momentum change would be expected to be the best indicator, average vehicle accelerations in crash-cushion impacts second best, and maximum 50-msec *g*-forces the least adequate.

Michie then attempted to define a better linkage between the crash test and occupant injury—the "flail-space" model. Here, the occupant is considered to be a free body that travels across the compartment space after the vehicle has struck an object and begun to decelerate. The occupant, continuing to travel at the original impact speed, then contacts a part of the vehicle interior (e.g., windshield, dashboard), which has

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decelerated to a lower speed. The difference between the occupant speed and the vehicle interior speed (referred to here as delta-*v*, or change in velocity for the occupant) is the first critical measure in the flail space model and is referred to as longitudinal or lateral "occupant risk." Following the impact, the vehicle and occupant continue to experience deceleration as a unit, and the second critical measure of acceleration to the occupant is considered to be the highest average vehicle acceleration over any subsequent 10-msec interval, referred to as longitudinal or lateral "occupant ridedown."

A pass-fail judgment of the barrier is then based on comparison of these two measures with critical levels of accelerations. These critical levels were based by Michie on what are considered to be acceptable impact velocities and *g*-forces from earlier studies related to dummy head impacts into windshields and car interiors (*9*), on limited accident analysis related to reconstruction of forces in side impacts (*10*), and on a variety of past attempts to determine the peak survival acceleration forces in other laboratory experiments (*11,12*).

Although Michie's hypotheses concerning deficiencies in the traditional measures and his flail-space measure of occupant risk are based primarily on vehicle dynamics, virtually no accident-based verification of his criticisms of the three original models or of the efficacy of his newer model has been conducted. Thus, while the relationship between forces to crash-test vehicles and ultimate occupant injury in real-world crashes is both logical and based in the physics of crashes, the specific nature of the relationship remains somewhat undefined. Such definition is important for several reasons.

- The measure used must reflect ultimate occupant injury, because decisions concerning which designs to approve for use are based on the measure used. Criteria not reflecting injury would lead to erroneous acceptance/rejection decisions.
- Because initial hardware design and subsequent design changes are based on differences in the measured crash test criteria (i.e., a design resulting in a lower measured criterion is considered more desirable than one resulting in a higher measured criteron), the lack of a relationship with occupant injury would mean that meaningless (or erroneous) design changes would be made—costs would be incurred without additional benefit.
- If differences in crash-test measures can be shown to reflect differences in occupant injury, they could be used with accident data to define better severity indexes for roadside features. Such severity indexes are used in economic analyses that help guide the design of the roadside—the use of safety features to shield roadside hazards. If the crash test measures are sensitive to changes in ultimate injury, the indexes based on accident analyses could at least be fine tuned or modified for specific feature types based on the crash test results. For example, although accident data might provide a general severity index for guardrail on the shoulder, the crash test results might allow modification of the general index to define specific indexes for different types of guardrail (e.g., W-beam versus thrie beam versus cable). Such refinements may never be possible using accident data alone because of sample size problems.

The analyses reported in this paper attempted to quantify this relationship better by linking mass accident data to the crash-test information. We attempted to examine Michie's hypothesized criticisms and, to the extent possible, his flail-space solution. Because of data and methodology problems, we were not very successful. However, because of the continuing importance of defining such a relationship, the details of the methodology used have been included for the benefit of other researchers considering such an approach. In addition, suggestions of future analytical efforts and methods that might prove to be more successful were provided.

METHODOLOGY

To study the relationship of crash-test measures to injury, a sample is needed of vehicles in crashes that have both the crash-test measure and injury specified. Such a sample does not exist because we cannot place human drivers in crash-test vehicles and cannot measure true acceleration forces in real-world crashes. Because the methodological goal was to use injury data from the real world, a sample had to be constructed in which real-world crashes that include injury data were supplemented (appended) with measured acceleration forces from similar situations in crash tests. To ensure a close match, as many vehicle, crash type, and severity variables as possible were matched and then controlled for other possible injury-related factors in the modeling.

On the basis of this goal, the unit of analysis in this study is a vehicle that was involved in a crash in North Carolina between 1973 and 1986. The sample of vehicles in the analysis file had to closely match those vehicles in crash tests. The match is based on (a) vehicle factors (i.e., make, model, and year), (b) crash specifics (i.e., type of object struck and location of impact on vehicle), and (c) crash severity (as measured by the Traffic Accident Data (TAD) project vehicle deformation scale). The crash-test data are then appended to the matching-vehicle record.

Data

The crash-test data used in this effort were extracted from recently computerized summary sheets found in FHWA's Roadside Safety Library. Information on crash tests conducted since the early 1970s included (a) the type of test and type of feature tested; (b) make, model, year, and weight of the test vehicle; (c) test specifics including impact angle, impact speed, and location of impact on the feature and on the test vehicle; and (d) results of the test. This final subset includes information on vehicle damage measured by the TAD and Vehicle Deformation Index (VDI) scales, the behavior of the vehicle, and information related to 50 msec average longitudinal and lateral accelerations, delta-*v* and momentum change, and occupant "flail space" measures as found in a work by Michie (*8*).

The complete file received contained some information on almost 1,000 cases, but the number of usable cases was much smaller because of incomplete summary sheets. Using the requirement that the crash-test case had to have usable information on the feature struck, car make and model, TAD level, and at least one of the pertinent crash forces (i.e., either lateral or longitudinal acceleration or momentum change), 223 usable crash tests were available for analysis—36 for guardrails, 20 for median barriers, 57 for bridge rails, and 8 for sign supports. The remainder were for features that could not be linked with the North Carolina crash file either because a limited number of matches were found (e.g., guardrail-end treatments and crash cushions) or because the objects crash tested were not similar to the objects in the real-world crashes (e.g., utility poles and mailboxes, which are considered "breakaway" in the crash tests). A number of cases that required editing were found. Although case-by-case quality control checks were not possible, repeated use of the variables in the matching and analysis efforts resulted in corrections of problems when found and lead one to believe that the data used in the final analysis files are relatively accurate.

The nature of crash testing is such that the tests do not include a random sample of the entire range of vehicles or crash situations that are found in real-world crashes. Indeed, as prescribed under NCHRP 230 and earlier guidelines, the tests are designed to represent practical worst-case situations—the upper end of the crash distributions. Thus, the testing protocols result in numerous crash tests involving the same vehicle striking the same longitudinal barrier at very similar speeds and angles, and resulting in the same TAD rating. In these cases, accelerations, changes in momentum, and measurements of delta v were averaged for all crash tests in which the vehicle, the barrier struck, and the TAD were the same. (There may have been differences in the impact angle and impact speed, but because neither of these variables is obtainable from the real-world crash data, they were not used to differentiate one crash test from another.) The final number of independent crash-test records used in matching, after similar cases were averaged for the hardware classes ultimately analyzed, is given in Table 1. A total of 75 independent data points resulted.

The real-world crash information used in this study was extracted from annual files of North Carolina crash data, files that are considered relatively accurate because of statewide uniformity of report forms and enhancements through repeated use in research efforts. The annual statewide files contain records of 150,000–200,000 accidents and 300,000–370,000 vehicles per year. Because the crash tests studied included vehicles with model years in the early 1970s, annual accident data tapes from 1973 to 1986 were used (with the exception of 1979 where the data tape was unusable), which provided a sample of approximately 4,000,000 vehicles against which to match the crash-test vehicles. Files from later years were not used because of inaccuracies in reported occupant restraint use after passage of a mandatory belt law in 1987.

File Linkage

The key variables on which the two sets of data were to be matched included the specific make and model of the vehicle involved, the type of feature struck in the crash test, the TAD-related location and severity of vehicle deformation, and the estimated original speed before accident as a surrogate for impact speed. A primary goal was to maximize the sample size, a goal that also defined the nature of the matching parameters.

With respect to make, model, and year, both exact matches between the two files and matches with "clones" of the crash-test vehicle were allowed. Clones are vehicles of very similar design (e.g., a 1974 Oldsmobile 98 could be matched with a 1971–1976 Oldsmobile 98 and a 1971–1976 Buick Electra). Clones were defined in discussions with representatives from automobile manufacturers and researchers at the National Highway Traffic Safety Administration who have done similar work using clones, a review of published material from the Insurance Institute for Highway Safety, and a detailed review of wheelbase and gross vehicle weight information found in the *Branham Automobile Reference Book* (*13*).

With respect to type of feature struck, vehicles in the North Carolina crash file were retained as case vehicle if

1. They were involved in single-vehicle accidents in which the "first harmful" and "most harmful event" was either ran-off-road or collision with a fixed object. This eliminated cases in which driver injury might have resulted from crashes with other vehicles instead of hardware.
2. The "object struck" code matched the crash-test object by indicating a "guardrail face," "median barrier face," "bridge rail," or "sign support."

TABLE 1 Sample Sizes of Usable Crash Test Cases, Cases After Averaging Similar Tests, and Matched Vehicle Records in Final Analysis File

Feature	Original Crash Tests	No. Crash Tests After Averaging	Matched Vehicle Records
Guardrail	36	23	53
Median Barrier	20	16	5
Bridge Rail	57	28	122
Sign Support	11	8	52
Total	223	75	232

For the third linkage variable, impact severity, the measure used was the TAD vehicle deformation scale (*14*), which is provided by both the investigating police officer and the crash-test engineer. Preliminary research has shown that the deformation rating can be made reliably by police officers and is superior to the officers' estimate of speed in terms of control for crash severity (*15,16*). When this information was missing or obviously erroneous in the crash-test file, a second deformation scale, the VDI was used to edit erroneous TADs and to generate missing ones. Because early file-matching runs indicated that matches with "exact" TADs led to very small samples, a decision was made to accept a match if the location of the impact was in the same area and if the TAD severity rating was within plus or minus one level of the crash-test severity value. In the final analysis file, approximately 50 percent of the vehicles had exact TAD matches.

The 75 crash-test data points referred to earlier were then matched with the 1973–1986 North Carolina crash file. The expansion of both the vehicle make and model year to clones and the TADs to plus or minus one severity level resulted in a file of 232 vehicle records suitable for analysis. These restrictions resulted in the final sample sizes used in the modeling as shown in Table 1.

Analysis Methodology

The basic methodology used to analyze the matched data involved the development and analysis of a series of contingency tables (cross-tabulations) to define potential relationships between injury and crash-test measures, and the development of a series of predictive models based on logistic regression. The outcome variable in these models was some measure of driver injury, and the independent variables included the various crash-test measures and vehicle and crash descriptors. An attempt was made to develop such a model for each feature type tested—guardrails, median barriers, bridge rails, and sign supports.

[A preliminary modeling effort examined the question of whether TAD—one of the primary matching variables—was related to crash forces in the crash tests. By using general linear regression models, various measures of crash-test forces (longitudinal acceleration, lateral acceleration, and resultant acceleration) were predicted as a function of TAD severity (coded 1 to 7), car size (grouped into small, median, and large car groups), impact speed, and impact angle. Unlike later modeling in which the unit of analysis is a vehicle in a real-world crash, the unit of analysis here is a vehicle in a crash test. The results indicated that when TAD, impact speed, and impact angle were included in the same model as predictors of crash forces in guardrail impacts, TAD was not a strong predictor. When impact speed and angle were excluded from the model, TAD was a significant predictor, meaning that it was acting as a proxy for impact angle and speed. TAD was a strong predictor for median barriers (along with car size) and for bridge rails (along with impact speed). The results of this modeling did not dissuade us from using TAD as a matching variable but did point out the need to attempt to include some measure of vehicle speed in the subsequent injury models.]

RESULTS FOR TRADITIONAL MEASURES OF VEHICLE FORCES

The contingency table analysis pointed out a very serious limitation of this investigation—the extent of data available for analysis, in terms of both total sample size of matched vehicles where complete crash-test data existed (as shown in Table 1) and in the range of variation in many of the relevant variables. For example, for the already limited sample of 53 matched guardrail-related vehicles, examination of the resulting distribution of R-force values indicated that, although the values of R-force (resultant force) ranged from 3.47 to 19.02 *g*, 33 of the 53 (62 percent) had the same value of 5.98. This was a result of the fact that some of these matches were groups of similar vehicles in the North Carolina crash file that matched the same crash-test vehicle (or groups of similar crash-test vehicles for which data were averaged). Further analysis indicates that the 52 matches (vehicles in the real world) were the result of only 17 crash tests (of the total of 36) and only 12 distinct data points (after averaging similar tests). The resulting problem, of course, is that the sample available for analysis is small, in terms of number of cases, and has only limited variability, in terms of crash forces assigned to vehicles. Similar problems were noted with the median barrier sample. Bridge rails were less problematic in that a larger, more variable sample was available for analysis. For all longitudinal barriers combined, while nearly 179 total observations (vehicles in crashes) were available, more than half were concentrated at just a few specific R-force values.

Given these limitations, the first phase of the contingency table analysis involved examination of the distributions of driver injury corresponding to specific R-force values when all matched vehicles that had struck guardrails, median barriers, and bridge rails were combined, and when 10 or more accident observations were found for the same R-force level. (Note that this and all later combinations of the data continued to retain the integrity of the original match, in that each record represented a vehicle that had already been matched to a specific crash-test object, and was assigned the crash-test forces for that object. Thus, a vehicle striking a guardrail in the real world was not assigned force values from median barrier crash tests. Only entire matched records were combined for analysis.) As in all subsequent analyses, driver injury is defined by the KABCO scale, with K indicating "killed," and A, B, and C denoting progressively less severe injury.

The analyses consistently indicated a wide range of driver injuries corresponding to specific values of R-force. There was no apparent trend of more severe injuries corresponding to higher R-force values. Neither of these findings indicated a strong relationship between injury and R-force.

Momentum change values (ΔMV) were assigned to 52 crashes into sign supports; none were available for luminaire crashes. The distribution of these values ranged from 1,834 to 10,511 N/sec (412 to 2,362 lbf/sec). Although the spread of values for the matched vehicles was somewhat better than for the *g*-force measures, again, 25 of the 52 cases were matched to only two different crash-test momentum change values. All of the 11 crash-test cases with usable data available for analysis were matched, but they generated only eight different data points.

A cross-tabulation of driver injury by the highest frequency momentum changes was generated and examined. Although no strong relationship between injury and ΔMV was indicated, there was perhaps a hint of injury severity increasing with increasing values of ΔMV, at least at the higher injury categories.

In the comparisons thus far, other variables such as car size have not been controlled for. To further explore relationships between *g*-forces and driver injury, the final analyses involved fitting a series of logistic regression models to the data. The logistic regression model fits a linear function of the independent variables to the quantities

$$\text{lot} \frac{\text{proportion injured at specified level}}{\text{proportion with lesser or no injury}}$$

by the method of maximum likelihood. Logistic models are particularly useful when both continuous and categorical dependent variables are being studied, and where some of the groups may have a zero value.

For each of these models, driver injury was characterized as a dichotomous (grouped) variable ranging from serious (K or A) injury versus lesser injury, to any injury (K, A, B, or C) versus no injury. In models involving crashes into any of the longitudinal barriers, either R-force or a variable labeled "test" were included as independent variables, where "test" was defined as follows:

Test = 1 if lateral acceleration, longitudinal acceleration, and R-force are all in the preferred range (i.e., lateral $\leq 3\,g$, longitudinal $\leq 5\,g$, R-force $\leq 6\,g$).

= 2 if not all in preferred range but all in acceptable range (i.e., lateral $\leq 5\,g$, longitudinal $\leq 10\,g$, R-force $\leq 12\,g$).

= 3 if at least one *g*-force beyond acceptable range.

The definitions of "preferred" and "acceptable" range were taken from Transportation Research Circular 191. Other independent variables included car size [large = 1, where vehicle weight is greater than 1816 kg (4,000 lb); medium = 2, where vehicle weight is between 908 and 1816 kg (2,000 and 4,000 lb); small = 3, where vehicle weight is less than 908 kg (2,000 lb)], seat belt use (yes = 0, no = 1), and the police estimate of original speed before accident. (The police estimate of "impact speed" is uncoded in many cases and is of questionable accuracy.) For vehicles crashing into sign supports, ΔMV was the force-related independent variable. Preliminary analyses indicated that "driver age," usually an important variable in injury prediction, was not significantly related to injury in this data set. Thus, it was not included in the models.)

Two sets of analyses of longitudinal barriers were carried out, the first set with no restrictions on estimated vehicle speed. Because an early reviewer noted that TAD alone may not be an entirely adequate matching variable (given that the same TAD may result from two different changes in velocity if the initial speeds differ), an attempt was made to provide some control for the speed of the crash-involved vehicle. Unfortunately, there is no adequate measure of "impact speed" provided in a police investigation. Instead, the only "surrogate" available—the police estimate of "original speed prior to accident—had to be used." Since all crash-test vehicles in the final matching file had crash-test impact speeds of approximately 89–105 km/hr (55–65 mph), the analysis file was restricted to include only those vehicles with estimated original speeds greater than 64.4 km/hr (40 mph). (Further restriction would have reduced the sample to an unusable size.) Approximately 81 percent of these vehicles had estimated original speeds between 72 and 113 km/hr (45 and 70 mph).

The results of these speed-restricted analyses are given in Table 2. Here, the column labeled "Injury Level" defines one of the two contrast injury groups used in the model. Thus, in the first model, the proportion of drivers with K or A injury was compared with the proportion with less severe injuries (i.e., B, C, or no injury). As indicated in the column referring to feature type, in most models all three types (guardrails, median barriers, and bridge rails) were combined.

The R-force values are significant predictors of injury in four of the nine final models developed. More specifically, R-force values are significant predictors for contrasts involving any injury versus no injury for all barriers combined (Models 3 and 4) but are not significant predictors for contrasts involving more serious injury, where one might have expected to see the greatest effect. The categorical measure of force ("test") is a significant predictor for contrasts of fatal plus serious plus moderate injuries (K, A, B) versus other injuries (Models 5 and 7). Vehicle speed, even within this restricted subsample, appears to be a fairly consistent predictor of injury. It may also be noted that the estimated car size effect, although statistically significant in Models 3 and 4, is contrary to intuition in that higher injury rates are associated with large cars relative to smaller cars. This is most likely because the large cars are tested at higher impact angles than are the small cars under existing testing procedures.

In logistic regression, there is no measure of "fit" of the model comparable to R^2 in general linear regression. Instead, the model is used to classify each case into one of the two injury categories (e.g., injury versus no injury), and the results of the predicted classification are then compared with the true injury/no injury class for each case. Here, for example, such verification of Model 4 (chosen since both R-force and speed are significant predictors of total injury at the $p < .08$ level) indicates that the predictive capability is low. The model correctly classifies 77 percent of the no-injury cases, but it correctly classifies only 45 percent of the injury cases.

Additional models involving vehicles crashing into sign supports were developed, but detailed results are not presented here. Briefly, the variable related to momentum change was a statistically significant predictor of moderate or more severe injury, but the strength of the model was quite low.

In summary, the analysis of the relationship between the traditional crash-test measures—longitudinal and lateral 50-msec *g*-forces and momentum change—and driver injury indicated some relationship in only four of nine final models examined. The predictive strengths of the models were quite low. This lack of relationship could have been partially a function of available sample sizes and of the use of the "expanded TAD" matches.

TABLE 2 Logistic Regression Results for Models Involving Longitudinal Barriers

		Parameter Estimate (Standard Error)				
Feature[a] Type	Injury Level	R-Force	Test	Belts	Car Size	Speed
G,MB,BR	K or A	.06 (.07)	--	-.44 (.75)	.07 (.31)	.06* (.02)
G,MB,BR	K,A,B	.08 (.05)	--	.40 (.59)	.04 (.22)	.04* (.02)
G,MB,BR	K,A,B,C	.12* (.06)	--	.56 (.55)	-.50* (.20)	.03 (.02)
G,MB,BR	K,A,B,C	.12* (.06)	--	--	-.43* (.18)	.04 (.02)
G,MB,BR	K,A,B	--	.61* (.29)	--	-.30 (.26)	.04* (.02)
G,MB,BR	K,A,B	--	.52 (.29)	--	--	.04* (.02)
B,MB,BR	K,A,B	--	.62* (.29)	--	--	--
MB,BR	K,A,B,C	.12 (.07)	--	--	-.41 (.35)	.02 (.03)
MB,BR	K,A,B,C	.09 (.05)	--	--	--	--

[a] G = Guardrail, MB = Median Barrier, BR = Bridge Rail

* Significant at $p<.05$

RESULTS FOR OCCUPANT RISK AND RIDEDOWN MEASURES

The lack of a strong relationship between the traditional crash-test measures and injury was hypothesized earlier by Michie (*8*) and others. As a result, Michie developed alternative "flail space" measures to be used in crash tests—occupant "risk" and "ridedown."

Unfortunately, even though these measures have been used since 1981, the computerized data set available is very limited. Indeed, there were only 39 usable guardrail tests, 34 median barrier tests, 3 bridge-rail tests, and no matchable tests for sign supports. After similar tests were combined, there were even fewer unique measures, ranging from 2 for bridge rails to 24 for median barriers.

Even though limited, the longitudinal barrier tests were linked with crashes in the North Carolina crash file, using only those cases in which the most harmful event was related to strike a fixed object, and then restricting those by estimated initial speed. With the speed restriction in place, the linkage resulted in 34 matched vehicles striking guardrails, 24 striking median barriers, and 4 striking bridge rails.

It is obvious that these samples of available vehicles were quite small. Again, as with the earlier *g*-force data, these samples were also restricted in terms of the variability of the recorded risk and ridedown measures. For example, using the data restricted by both harm and speed, of the 34 cases in which a measure of longitudinal risk was available for crashes with guardrails, 21 (62 percent) had the same value (22.2). Of the 35 guardrail cases in which there was a measure of lateral risk, 21 (60 percent) had the same value (14.4). Although the measures of longitudinal and lateral ridedown were less clustered, there were only 21 guardrail cases in which any data existed in the matched analysis file when restricting only on harmful event, and only 15 when further restricting on speed.

Attempts were made to analyze these data through the development of contingency tables of each flail-space measure versus injury and the development of a limited number of models, but the results were relatively meaningless. This was not unexpected. Because of the small sample sizes and the lack of variability in the data, a relatively strong relationship between flail space and injury would have had to exist to be apparent. In addition, one other factor of note operated against finding such a relationship in these limited data—the use of driver injury as the outcome variable.

Driver injury was the outcome variable of choice throughout all the analyses, primarily because it is a constant measure in that a driver is always present and because roadside hardware must be designed to protect the occupant most likely to be in the vehicle—the driver. However, flail space is based on an occupant compartment distance through which the occupant can travel before striking part of the interior during the deceleration. Not only does the distance (and thus the impact speed differential) for the driver and the right front passenger differ—0.305 m (1 ft) forward for the driver (to the steering wheel) versus 0.610 m (2 ft) for the passenger (to the dashboard)—but the nature of which occupant measure is critical in a crash test depends on the impact point on the vehicle. For left front or side impacts (e.g., into median barriers), the driver measure would be the most important. For right front or side impacts (e.g., into shoulder guardrails), the passenger measure would be critical. Thus, flail-space measures on the crash tests are based on what is considered the most critical occupant. (This is somewhat problematic in a philosophical sense in that there appears to be a need to define some measure of risk for the driver in right-side impacts, particularly given that a driver is always present, but a passenger only sometimes present.) In the data analyzed here, further division into driver and passenger injury or right- versus left-side impacts for matching purposes would even more severely restrict the available insufficient sample size.

In short, because of the small sample size and the need to further restrict to specific occupants, no conclusions can be drawn concerning the relationship between flail-space measures and injury. Clearly, many more data are needed before the ability of these measures to correctly predict subsequent driver injury can be assessed appropriately.

DISCUSSION OF RESULTS

There are strong reasons for attempting to better examine and define the relationship between crash-test measures and occupant injury. These reasons are related both to the need to be certain that the measures being used are resulting in the correct acceptance or rejection decisions and design modifications for roadside features and the need to expand the use of these relatively expensive crash-test data to other uses, such as the development and refinement of severity indexes. This attempt to use existing computerized crash-test data and North Carolina accident files to examine occupant risk measures and driver injury was not successful in defining such a relationship.

To some extent, these analyses appear to support the crash-test community's earlier reservations about use of the traditional measures. Although the lack of an indicated relationship could have resulted partially from the small sample sizes and the use of the expanded TAD match (which would lead to more variability in the injury/measure match), it is also likely to have resulted from the hypothesized underlying weak relationship. Unfortunately, these efforts to determine whether the proposed improved measures—the flail-space measures—are indeed superior in terms of predicting true occupant risk were not successful. Part of the problem stemmed from the methodology and part from the nature of the files used.

With respect to the files, the major problems include the following:

- In FHWA's computerized Roadside Safety Library, there appear to be significant amounts of missing data, particularly data related to the flail-space measures.
- By design, the nature of the test matrix severely limits the variability in the impacts tested, in terms of both vehicles and speeds, and this greatly restricts matches with vehicles in accidents and the range of measures (both traditional and flail-space) that can be analyzed.
- In the North Carolina accident file and those from other states, there is no good measure of impact velocity or impact angle, and the KABCO injury scale does not adequately subdivide serious injuries.

With respect to the flail-space measure itself

- There is a need for a method to combine the risk and ridedown measures to predict occupant risk. In this work, an attempt was made to analyze each of the four flail-space measures (lateral and longitudinal risk and lateral and longitudinal ridedown) separately. In contrast, for peak vehicle accelerations, a combination of lateral and longitudinal *g*s were combined to form a resultant force vector for study. If the flail-space measures more accurately capture the results of the complex movements of an occupant within an impacting vehicle, perhaps some combination would be even better predictors than the individual measures.
- From the point of view of testing for (and thus protecting) the vehicle occupant most often exposed to impact—the driver—and a parallel need to increase the available sample size of cases that can be studied in these correlational efforts, there is a need to define a flail-space measure for the driver in off-side impacts.

With respect to the methodology explored—the linking of crash-test and real world data—the major problem was in the limitation on the preciseness of the real-world and crash-test match. The lack of impact speed estimates in the North Carolina files and the lack of variability in vehicles and crash speeds in the crash-test file led to the inability to match on speed or angle and the need to use an expanded rather than an exact TAD severity match. Clearly, both these problems erode the analytical ability to detect a relatively weak relationship and should be corrected in future research. Additional crash-test cases, which include angles, speeds, and vehicles not now in the computerized file, are needed.

In addition, the use of vehicle crush as defined by TAD may not be an entirely sufficient match variable for analysis of occupant ridedown. Under the flail-space assumption that the occupant and interior decelerate as a unit after occupant impact with the interior, deceleration peaks to the vehicle frame (e.g., the frame snagging on a guardrail post) could be transmitted to the interior and could result in injury but not in changes in vehicle crush. In these cases, TAD would not capture the necessary information, and the resulting true correlation would be lessened.

In terms of what should be considered by both the crash-test community and other researchers attempting similar efforts, the following is offered:

1. Maximize the use of the existing Roadside Safety Library through a case-by-case review of post-1980 test documenta-

tion such that available flail-space and other key measures are placed in the computerized file. Concentrate this effort to capture impact situations not now captured (i.e., angle, speed, and vehicle size gaps in the analysis file).

2. Attempt to identify additional crash-test conditions from non-FHWA or new FHWA-NCHRP tests not now in the library, concentrating on combinations that increase the variability of the test conditions.

3. Consider future (research, rather than compliance) funding for tests that increase the variability in the crash-test data by varying the vehicles and the speed and impact angles tested. For research, the vehicles tested within the weight classes should constitute as large a share of vehicles in the existing population as possible. This could be determined by examination of vehicle registration files supplemented by knowledge of possible clones.

4. Examine the possible use of other accident files, such as the National Accident Sampling System or the Longitudinal Barrier Special Study, which include information on estimated impact speed and angle and enhanced injury data, or the possibility of new large-scale crash reconstruction efforts targeted to vehicles used in crash testing. (Sample size will be a problem in the existing files if the clone requirement is retained.)

5. Consider alternative methodology involving "pseudo" measures of injury such as Head Injury Criteria and that of the Texas Transportation Institute through comparison of flail-space or traditional measures with data from anthropomorphic dummies. Problems with such dummy-related measures exist (e.g., correlation with injury and variability due to out-of-position dummies) and must be considered carefully in such research.

In summary, this has been a limited effort at attempting to define relationships between crash-test measures and occupant severity. Although it was not successful, there remains a clear need to determine whether the measures currently used in the design and testing of roadside features indeed maximize protection for occupants of the vehicles while minimizing cost.

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