Injury Epidemiology: Fourth Edition

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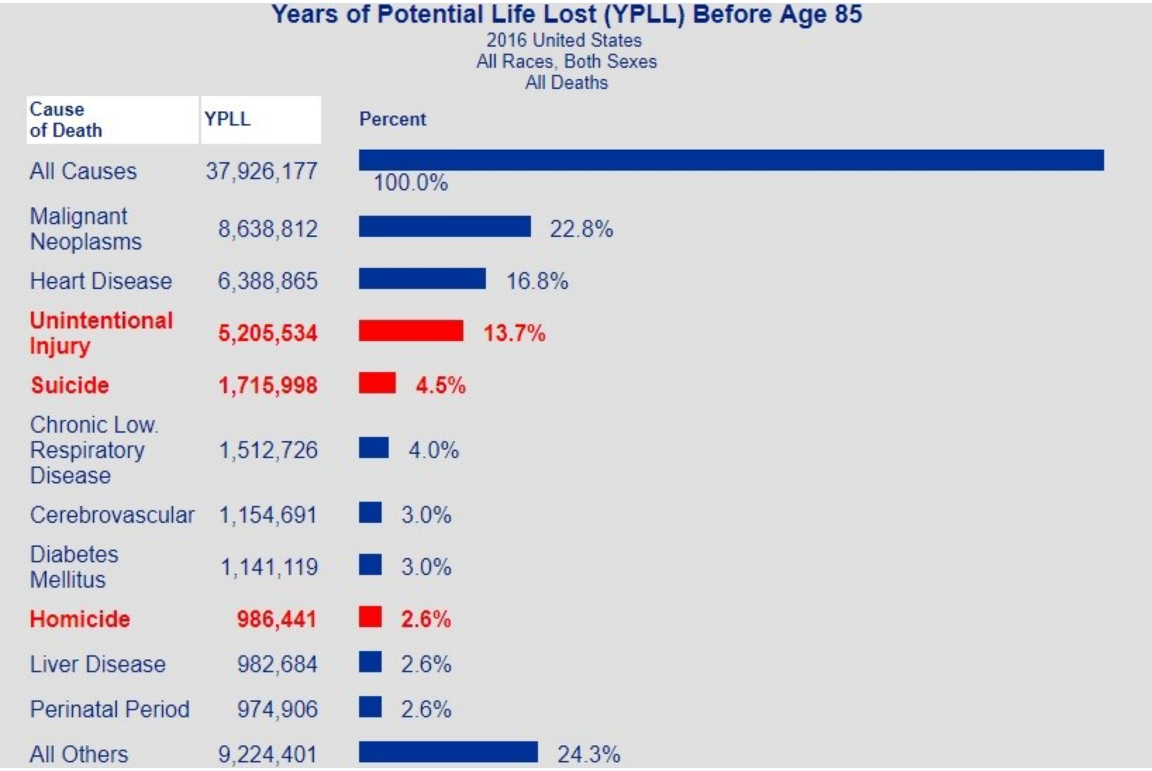
Chapter 5. INJURY STATISTICS

The simplest statistical description of injuries is the distribution by severity, obtained by counting the numbers in specific categories such as fatal, hospitalized, and ambulatory or on the severity scale of interest. For example, injury severity is sometimes described as distributed in the shape of a pyramid with deaths at the top, hospitalizations in the middle, and others at the base. More refined categories of injuries by type and severity scales, however, are not pyramid-shaped by severity. For example, there are substantially more deaths from motor vehicles than nonfatal, critical (AIS-5) injuries. Drowning and near drowning tend to result in death or a much lower number of severely brain damaged and impairment cases from oxygen deficits, or persons survive without injury.

Among the most commonly reported distributions of injury is by age and sex. While such distributions of injuries of a given type may be of use in targeting age groups for prevention, another important way of looking at age of the fatally injured is by years of potential life lost. This is calculated by multiplying the number of deaths at given age times the years of the expected life of persons of that age (from a life table) and summing the total number of years lost. (See http://www.ssa.gov/OACT/STATS/table4c6.html for a life table based on the age of deaths in the U.S.)

Figure 5-1 shows the years of potential life lost before age 85 in 2016. If homicide and suicide are included together with other injury deaths, injury is equivalent to cardiovascular disease and trails only neoplasms as takers of life years before very advanced ages. That is because those who die from injury, on average, are usually decades younger than those who die from the “leading” causes of death. Some researchers subtract the age of death from 65 to obtain "productive years of life lost", but that practice implies that the only value of life is economic. Such comparison of potential years of life lost has resulted in more attention to injuries but is seldom used in detailed research. Potential years of life lost by type of injury would also be suggestive of the importance of concentrating on certain types of injury that take more years than the total numbers of fatal injuries in a given category would indicate. For example, firearms overtook motor vehicles as the leading cause of years of potential life lost from injuries in 2018-2019 (Klein, et al., 2021)

Figure 5-1. Years of Potential Life Lost Before Age 85 in 2016. Source: Centers for Disease Control and Prevention



Years of Potential Life Lost (YPLL) Before Age 85
2016 United States
All Races, Both Sexes
All Deaths

Cause of Death	YPLL	Percent
All Causes	37,926,177	100.0%
Malignant Neoplasms	8,638,812	22.8%
Heart Disease	6,388,865	16.8%
Unintentional Injury	**5,205,534**	**13.7%**
Suicide	**1,715,998**	**4.5%**
Chronic Low. Respiratory Disease	1,512,726	4.0%
Cerebrovascular	1,154,691	3.0%
Diabetes Mellitus	1,141,119	3.0%
Homicide	**986,441**	**2.6%**
Liver Disease	982,684	2.6%
Perinatal Period	974,906	2.6%
All Others	9,224,401	24.3%

INJURY RATES. Comparing raw numbers of injuries between populations does not take into account the differences in the numbers of people available to be exposed to harmful energy. To correct for exposure, injuries are often reported as rates per population, or some other denominator, such as per miles driven for motor vehicle injuries or per hours flown for injuries in aircraft. The population rate is calculated by dividing the number of people injured by the number of people in the population:

$$\text{Rate} = \frac{\text{Number of injuries in a year in the population}}{\text{Number of people in the population}}$$

This rate is usually multiplied by 100,000 to get the rate per 100,000 persons in the population at risk, or by 10,000 or 1,000 if the injuries of interest occur in larger numbers.

To be accurate, the people injured in the numerator must have come from the population in the denominator and both must be counted accurately. Most such calculations are approximations based on estimates of the population from the census or some other source. Some of the injured during the year, or whatever period chosen, could be visitors from outside the population and some of the population may have been outside the area all or part of the year.

These potential biases may not be important in total population estimates when the visitors' injuries or the numbers of the population that are outside the area are small relative to the total numbers involved. Estimates of injury rates per population for smaller areas such as cities, counties, or even states may be substantially affected by seasonal influxes of tourists or students, roads carrying large amounts of non-local traffic, and the like.

The basic principle in calculating population rates is that cases in the numerator should only come from the population in the denominator. This is a major problem in hospital trauma registries where the population from which the injuries come is often difficult or impossible to define (Waller, 1988). To calculate rates for segments of the population, or using some other denominator such as miles driven in the case of motor vehicle injuries, one must be able to place cases in the numerator and denominator into the same mutually exclusive and exhaustive categories. For example, if the injury cases are classified by age as 0-4, 5-9, 10-14, etc. and the population data are classified by age as 0-3, 3-6, 7-10, etc., the rates for each age group cannot be calculated without reclassifying one or the other.

The use of denominators other than the population is an attempt to obtain a more refined estimate of the rate per exposure. For example, the Federal Aviation Administration reports injuries in aircraft crashes per hour flown. Obviously, a person who doesn't travel in airplanes will not crash in one, although crashing aircraft occasionally injure people on the ground. Aircraft usually crash during attempts to take off or land. In the same make and model of an airplane, a person who flies the same number of hours over shorter distances is at greater risk per hour flown than when flying the same number of hours over longer distances. Therefore, the rate per hour flown can be a misleading indicator of risk. (See http://www.rita.dot.gov/bts/about/bts_programs.html for data on exposure to various forms of transportation).

Hours of exposure become even more problematic when considering numerous activities. For example, the drowning of young children often most often occurs in home swimming pools. How do we estimate hours of exposure of young children to home swimming pools? Hours in or by the pool are inadequate because many wander into the pool from the house or yard at times when the use of the pool is unintended by their adult supervisors. Some wander into a neighbor's pool. Do we count the number of hours the children are awake? Do we multiply the number of hours children are awake times the number of pools in the neighborhood? What constitutes the neighborhood in defining the likelihood of home pool exposure?

Trends in rates of motor vehicle fatalities are sometimes based on total deaths per 100 million total miles driven. The latter was based on self-reports, periodic odometer monitoring, road use surveys, or sales of fuel until 1994. The Federal Highway Administration began reporting estimated miles traveled separately in U.S. states in 1994 based on traffic count data at 4000 locations. The deaths per mile are often decreasing even in periods when the population rate is not changing or increasing. This occurs, for example, when the use of vehicles in urban areas is increasing faster than use in rural areas. In urban areas, the speed limits are lower and congestion of traffic during the misnamed "rush hours" also reduces speeds, thus lowering the energy exchange in crashes. Crashes in urban areas are more frequent per mile but they are less severe, and vehicle occupant death rates per mile are lower than in rural areas.

An auto industry analysis of occupant fatalities in cars and light trucks by the hour of day and day of week noted that 32 percent of such fatalities occurred between 11:00 p.m. and 5:00 a.m. while diaries of personal vehicle use indicate that only 4.5 percent of total mileage was accumulated during those hours (Schwing and Kamerud, 1988). Unfortunately, the authors went on to speculate about "risk-taking behavior", which they did not measure, as the explanation. It is difficult to believe that behavior during those hours is so radically different than during the remainder of the day. As will be further noted in Chapter 7, lighting roads at sites where severe crashes clustered at night virtually eliminated the problem, which suggests that visibility at night may explain a substantial proportion of the diurnal variation in fatalities per mile. Mileage as exposure does not necessarily indicate the quality of exposure.

When considering separately other road users struck by motor vehicles, such as pedestrians and bicyclists, rates per mile of motor vehicle use in urban areas are higher than in rural areas. This is probably because more people walk or use bicycles in urban areas, but miles walked or bicycled are difficult to estimate accurately and are rarely used.

The point here is that there is no absolutely right or wrong measure of exposure. The issue under consideration should determine the choice of denominators to be researched and what can be learned about injury reduction from the results. The use of one or another measure of exposure has various implications for interpretation that may not be obvious.

RELEVANCE AND IRRELEVANCE OF RATES. One focus in the collection of data on incidence, severity, and other aspects of injury is to specify the risk of the injury in question. Risk is the probability that the injury, or a specific level of severity, will occur in the use of a given product or participation in a given activity. Risk is an estimate of what will happen in the future while a rate is an indication of relative frequency in the past. Risk is usually derivable from rates based on the assumption that the previous relative frequency will continue, adjusted for the deaths that eliminate future participation (Kelsey, et al., 1986), but the interpretation is sometimes difficult depending on how accurately the denominator reflects the quality of exposure. When rates are changing, as we saw regarding fatal injuries in Chapter 1, using them to project risk is dubious. For very rare events, such as nuclear-power-plant meltdowns, estimates of risk are based on the probability of various combinations of failures in the technology rather than the previous frequency (Gould, et al., 1988).

The scientist who calculates injury rates using whatever denominators should be aware of poor methodology and tricky interpretations that have been applied to the data. Particularly in regulatory and product liability forums, the word risk is often preceded by adjectives such as "reasonable" or "acceptable". From a technical standpoint, an injury rate is an occurrence per specified unit of exposure and merely reflects the occurrence per exposure during the specified period. Standing alone, the rate does not indicate anything about whether it could be reduced by the manufacturer of an involved product, except by not manufacturing the product, or by the user of the product.

The risk of a product is the expected number of injuries per product during its use. If a given make and model of a motor vehicle has an occupant death rate of 20 per 100,000 vehicles per year and it will be in use for an average of 10 years per vehicle, its occupant fatality risk is (10 x 20)/100000 or 0.002, or stated in the inverse, 1 occupant death per 500 vehicles. I once tried to explain this during a meeting with an Assistant Secretary of Transportation in the Nixon Administration who influenced safety standards. She thought I was saying that risk increased over time, which may or may not be true depending on whether wear and tear on the product or differential use increases or decreases risk as the product ages. But it simply refers to a chunk of time different from a year. Too many "risk analyses" fail to account for the lifetime risks of products or practices. Failure to acknowledge the difference between annual risk and cumulative risk over the life of products has resulted in grotesque errors in estimating the effects of potential preventive measures (Chapter 15, Appendix 15-1).

Manufacturers and others opposed to regulation, or "experts" and attorneys involved in product liability lawsuits, often cite the injury rate from the product in question relative to injury rates from other products as indicative of reasonableness or acceptability. Given the variety of denominators available to calculate rates and the difficulty in measuring exposure, the placement of a given rate in an array of rates of injury by-products or users can be very different, depending on the denominator used and the comparability of the research methods in case finding and exposure measurement.

An interesting case of such arguments about rates and risk occurred regarding the consideration of the regulation of "all-terrain vehicles" by the U.S. Consumer Products Safety Commission (CPSC). These vehicles are propelled by modified motorcycle-type engines at speeds up to 60 miles per hour. They have three or four balloon tires and are steered by handlebars similar to those on a motorcycle. Corrected for other factors, the injury rate is higher for three-wheeled versions and increases substantially with engine size (Rogers and Adler, 2001). The steering is not as easy as it looks; however, requiring weight shifts by the rider that is different from those on a bicycle or motorcycle. The vehicles are unstable when in motion and will roll over at low speeds, sometimes onto the rider if the rider does not apply the weight shift at the moment needed (Deppa, 1986).

During 2014-2020, all-terrain vehicles, recreational off-highway vehicles, and utility task vehicles were involved in more than 600 deaths each year (Consumer Federation of America, 2022). Due to marketing in the U.S. as a recreational vehicle for families, including children, sales of all-terrain vehicles increased rapidly in the early 1980s to the point that about 2.5 million were in the hands of consumers by 1986. CPSC estimates of deaths on ATVs increased from 26 in 1982 to 268 in 1986 and estimates of injuries from a survey of those treated in hospital emergency rooms increased from about 8,600 in 1982 to 86,400 in 1986. Congress and consumer groups pressured CPSC to ban the vehicles or issue safety standards for them.

Arguments ensued among the staff and commissioners of CPSC as to whether or not the rate of injuries to occupants of all-terrain vehicles should be compared to those of occupants of other motorized, off-road vehicles such as snowmobiles and mini/trail bikes. Initial comparisons indicated that the rate of injury per vehicle of all-terrain vehicles was 2 times that of mini/trail bikes and 4-5 times that of snowmobiles. Based on a few anecdotal reports of hours of use per vehicle in public hearings, however, the rates per hour of use for all-terrain vehicles were said to be less than those of the other vehicles (Verhalen, 1986). A majority of the commissioners voted that the comparison was irrelevant to the issue of whether all-terrain vehicles should be regulated (General Accounting Office, 1986), but the industry commissioned research on injuries per hour of participation in other recreational activities in an attempt to justify the injury rates of all-terrain vehicles.

An economist formerly employed at CPSC was hired by the industry to survey hours of use of mini/trail bikes and snowmobiles. Based on these data and CPSC counts of injuries, he argued that the injury rate per hour of use was no different among the three types of vehicles (Heiden, 1986). In subsequent Congressional hearings (Heiden, 1990) and lawsuits against manufacturers, comparisons were also made to injuries per hour of people engaged in other activities such as organized football, use of motorcycles and other on-road vehicles, general aviation, snow skiing, swimming, and other activities. Except for organized football games and general aviation, the estimates of hours of participation were extrapolated from surveys that used categories for the time of participation such as "daily or almost daily", "about once or twice a week", "about once or twice a month", and "less than once per month". These are useless for the estimation of hours of participation and, given the problems of remembering hours of participation, questions regarding more specific hours of participation in most activities are likely to be unreliable or invalid (Robertson, 2006).

Aside from methodological issues, this case illustrates some of the uses or misuses of epidemiological data of which the researcher should be aware. While it may be useful to present the public with information about the risks of engaging in activities that are substitutable one for the other per hour, per person, per vehicle, or product, the comparison of descriptive injury rates per whatever exposure unit among various activities or products does not tell us whether the risks are "reasonable" or "acceptable", or whether products involved are defective.

Two products may have the same injury rate, one of which could have been reduced by design modifications at less cost, no cost, or little cost, while the cost of modification of the other may be very large. The calculation of cost-savings, cost-effectiveness, risk-benefit, or cost-benefit of injury reduction modifications to products, or injury control programs generally, is not based on risk per hour or risk per product or activity. These calculations are based on the total costs of the injuries relative to the total costs of product modifications or programs to reduce them (Chapter 15).

If a product or activity were considered "reasonable" or "acceptable" as long as its injury rate was less than the riskiest products and activities, manufacturers and promoters of products and activities would be free to add one new risk after another with impunity. The fact that more risky products are for sale does not indicate user acceptability of a less risky product when the vast majority of purchasers do not have any way to assemble the data on relative rates.

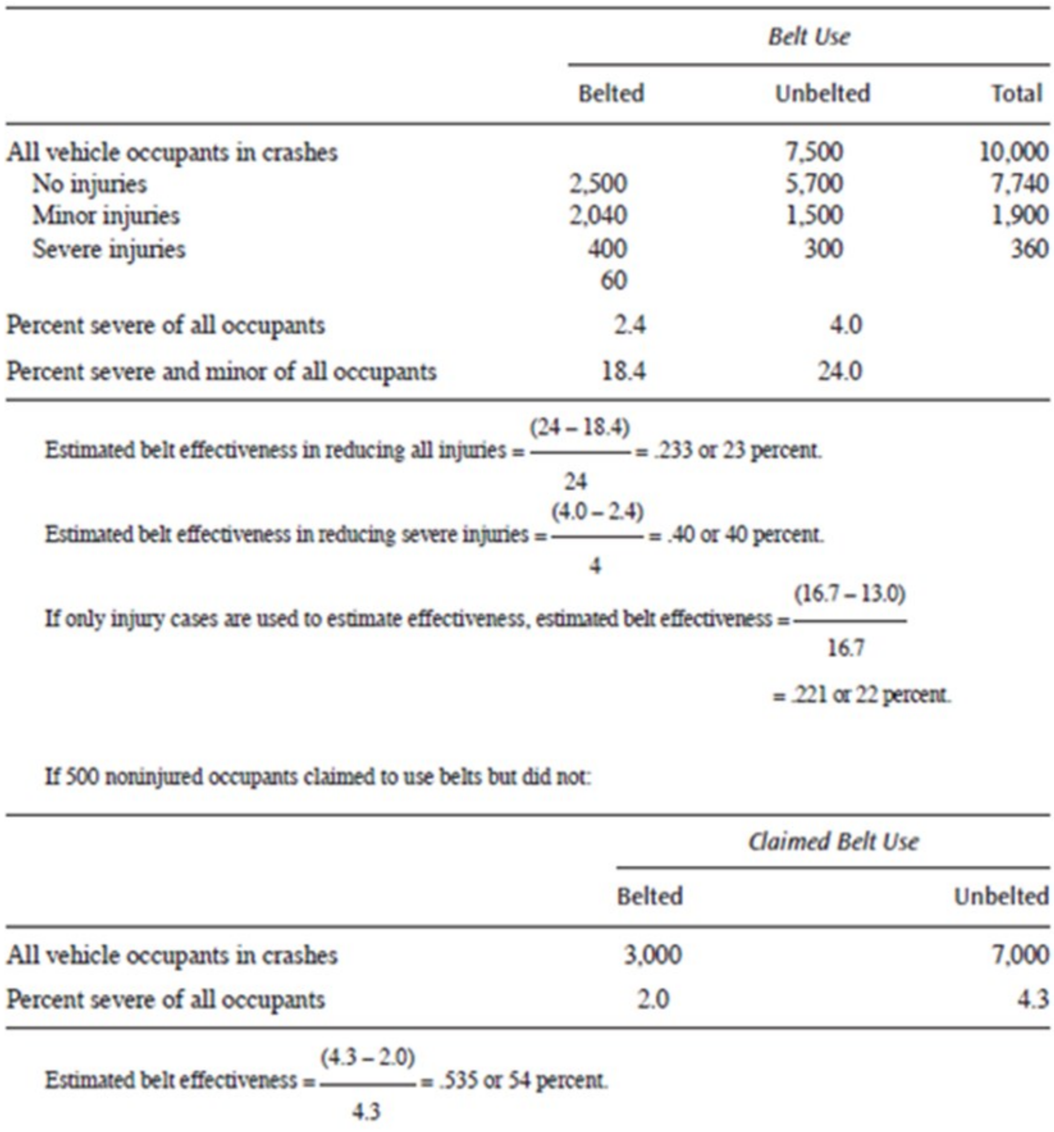
One methodology that has been used to assess public acceptability of risk is to compare the perception of current restrictions or regulations to desired restrictions or standards for motor vehicles, guns, commercial aviation, industrial chemicals, nuclear power generation, and nuclear weapons. In random sample surveys of the population in Connecticut and the Phoenix metropolitan area, the respondents rated desired restrictions and standards far above restrictions and standards prevalent in 1982-83, a period of severe economic recession and strong anti-regulation political sentiment (Gould, et al., 1988).

Epidemiological data on injury rates is relevant to the issue of modifiability of products and activities when and if the specific characteristics of the products and activities that increase or decrease the rates are delineated using sound epidemiological research. The methods for such analytical epidemiological studies are discussed in Chapter 8. Chapters 6 and 7 deal with a special case of descriptive epidemiology that is useful for problem identification and targeting control strategies -- injury surveillance.

Appendix 5-1. Biased Statistics and Estimates of Seat Belt Effectiveness

Nineteen studies of the effectiveness of seat belt use versus nonuse in reducing severe and fatal injuries, available by the mid-1970s, indicated estimates ranging from 7.5 percent to 82.8 percent effectiveness. Obviously, some of the studies were horribly biased. I examined the variation by two potential factors that might explain it -- the seriousness of the injuries included and the potential bias in claimed belt use (Robertson, 1976).

Table 5-1. Hypothetical Data on Belt Use and Effectiveness Estimates



	Belt Use		
	Belted	Unbelted	Total
All vehicle occupants in crashes		7,500	10,000
No injuries	2,500	5,700	7,740
Minor injuries	2,040	1,500	1,900
Severe injuries	400	300	360
	60		
Percent severe of all occupants	2.4	4.0	
Percent severe and minor of all occupants	18.4	24.0	

Estimated belt effectiveness in reducing all injuries = $\frac{(24 - 18.4)}{24}$ = .233 or 23 percent.

Estimated belt effectiveness in reducing severe injuries = $\frac{(4.0 - 2.4)}{4}$ = .40 or 40 percent.

If only injury cases are used to estimate effectiveness, estimated belt effectiveness = $\frac{(16.7 - 13.0)}{16.7}$

= .221 or 22 percent.

If 500 noninjured occupants claimed to use belts but did not:

	Claimed Belt Use	
	Belted	Unbelted
All vehicle occupants in crashes	3,000	7,000
Percent severe of all occupants	2.0	4.3

Estimated belt effectiveness = $\frac{(4.3 - 2.0)}{4.3}$ = .535 or 54 percent.

Table 5-1 illustrates the potential biases that could occur if injuries of different severity were used, or if some uninjured people claimed to use belts when they did not. The latter was known to occur when comparing observed use in traffic and later questioning people about their belt use (Waller and Barry, 1969).

As the table of hypothetical data shows, if five percent (500 of the 10,000 hypothetical occupants) of the persons in crashes claimed to use belts, but did not, the effectiveness of belts would be overestimated. If only injured persons were considered, belt effectiveness would be underestimated. In the 19 studies examined, belt effectiveness was primarily correlated to claimed use, suggesting that the higher estimates of belt effectiveness were influenced by false claims of belt use.

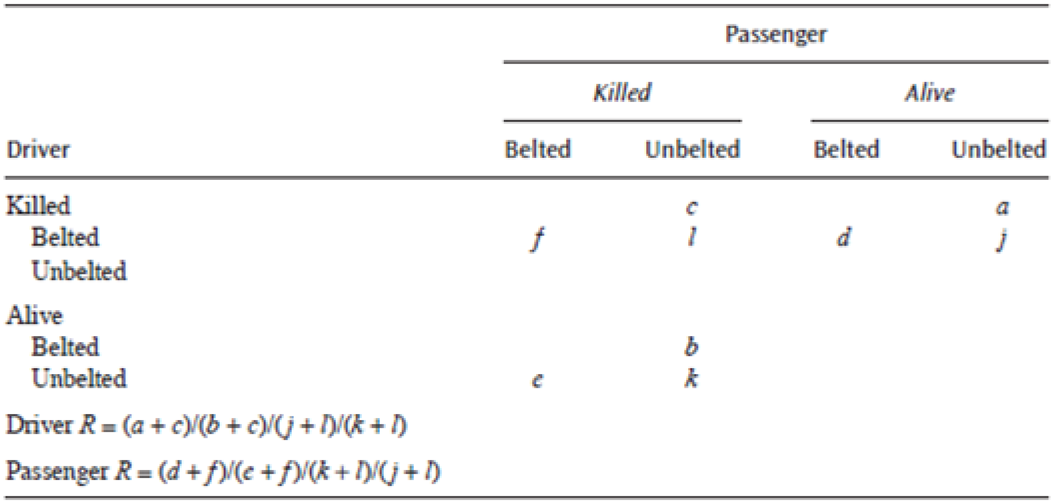
A few researchers combine fatal and "A" injuries in state police files in their studies, for example, in the assessment of seat belt effectiveness (e.g. Reinfurt and Chi, 1981; Streff, 1994). Because "A" injuries are far more frequent than fatalities and nearly half the "A" injuries are non-serious injuries that occur to belted drivers (Chapter 6, Figure 6-1), estimates of belt effectiveness or other factors in such studies are not valid. Researchers who use hospitalized injuries to estimate the effects of belts and other equipment (e.g., Orsay, et al., 1988) should be aware that without the inclusion of the fatally injured and the uninjured, the estimates are substantially biased.

In contrast to claims of 60-65% belt effectiveness estimate using FARS data before 1986, 41% effectiveness in older as well as newer model cars was found using a within-vehicle comparison method. The "double pair comparison" method attempts to control for a variety of factors (Evans, 1986a). It is a variation of a case-control design applied to fatal crashes in which there is more than one occupant. The relative risk (R) of a fatality to a given set of occupants is calculated as a ratio of ratios. For example, the calculation of the relative risk of belt use for drivers and passengers is noted in Table 5-2, using the cross-tabulation of who was using belts and who died in the formula as indicated.

Belt effectiveness in percent is then 100(1-R). Estimates of the effectiveness of belts by this method, using Fatality Analysis Reporting System data for 1975-1984 car models in calendar years 1975-1983 produced a weighted average of belt effectiveness in preventing death at 41 percent, averaged among age groups (Evans 1986b). Apparently when one occupant is belted and the other is not, reported use is more accurate.

An analyst in the National Highway Traffic Safety Administration did a thorough study of subgroups of drivers and passengers to identify potential biases and concluded that an increase in belt effectiveness estimates was biased by self-reported belt use by survivors probably because of the laws requiring use. He indicated that the 45% estimate of belt effectiveness in passenger cars is more realistic given the potential biases in reported belt use in crashes. A telling commentary on police reports in that study is the finding that 65% of dead drivers in multiple-vehicle collisions were judged culpable by police while only 32% of the surviving drivers in the same crashes were considered culpable (Kahane, 2000). The dead tell no tales but apparently the survivors do.

Table 5-2. Double-pair Comparison of Occupants of the Same Vehicle By Claimed Belt Use



	Passenger			
	Killed		*Alive*	
Driver	Belted	Unbelted	Belted	Unbelted
Killed		*c*		*a*
Belted	*f*	*l*	*d*	*j*
Unbelted				
Alive				
Belted		*b*		
Unbelted	*e*	*k*		

Driver $R = (a + c)/(b + c)/(j + l)/(k + l)$

Passenger $R = (d + f)/(e + f)/(k + l)/(j + l)$

Estimates of the effectiveness of seat belts, based on police reports or special investigator reports of use in crashes (NASS-CDS, Chapter 6), have varied substantially, partly because of variations in belts or their effectiveness in crashes of different severity, and partly because of variations in research methodology. Differential misreporting of belt use by survivors of crashes, and by police and NASS investigators assuming higher belt use, became rampant after belt use laws were enacted. This misled a team of researchers to publish repeatedly the claim of 60-65 percent belt effectiveness (Cummings et al., 2002; 2003).

The claim that belt effectiveness is near 60-65 percent was based on an analysis of data from the NASS-CDS, assuming that the NASS investigator recording of use was a "gold standard" (Cummings, et al. 2003; Schiff and Cummings, 2004). Cummings compared belt effectiveness using police reports and investigations by multidisciplinary teams for the National Automotive Sampling System (NASS), the latter supposed better investigators than the police. He concluded that police reports are valid indicators of belt use because the seat belt effectiveness using either police reports or NASS investigations are similar (Cummings, 2002), particularly among the more seriously injured (Schiff and Cummings, 2004). Since his analyses produce implausible belt effectiveness coefficients based on data from each of the two groups, he merely demonstrates that NASS investigators are just as biased as the police. That does not mean that they are intentionally biased but knowing the injury outcome could shade anyone's judgment of whether belts were used or not. Cummings and his colleagues attribute their higher effectiveness estimates primarily to a phenomenon called non-differential misclassification, which means that random error in seat belt use classifications results in an understatement of effectiveness in within-vehicle comparisons when use is low. They claim the theory is supported based on trends in police-reported use in such crashes and a simulation of its effect on effectiveness estimates. What is not explained adequately by the theory is the nonrandom bias in police and NASS-

CDS reported belt use by the dead and survivors that were exacerbated by belt use laws in the mid-1980s.

The political push for belt-use laws during the mid-1980s probably sensitized police and NASS investigators to the importance of belts. Some may have taken the illogical step of assuming that if the person died, the belts were not in use. Perhaps too much emphasis has been placed on the problem of self-reports of crash survivors and not enough emphasis on the potential bias of investigators judging the cause based on the outcome. Of course, that is why we require double-blind studies in assessing the effects and safety of drugs rather than relying on the judgments of physicians and patients who know which drug was taken and the outcome.

An objective measure of belt use and other conditions, such as speed and crash forces in crashes, is now available. The installation of "event data recorders" in vehicles provides a measure of such conditions preserved electronically at the time of a crash. Some 40 million vehicles were equipped with them in the U.S. by 2004. The first data on these vehicles that appeared in the NASS CDS files indicate that in 31 percent of 213 cases where NASS investigators indicate belts as buckled in a crash, the data recorder indicated that the belt was not in use. The investigators reported that 74 percent of vehicle occupants buckled up but only 54 percent of the data recorders indicated belts buckled (Gabler, et al, 2004). Some of those could be buckled while the occupant sat on them. The NASS investigators, like the police, are substantially overestimating belt use, which results in inflated estimates of effectiveness. The assumption that NASS investigators provide the "gold standard" for seat belt use (Cummings, et al. 2003; Schiff and Cummings, 2004) is foolish.

Estimates of the effectiveness of airbags and other countermeasures that controlled for seat belt use using invalid police and NASS investigator reports of belt use are likely invalid as well (e.g., Cummings, et al., 2002). Some researchers that employ police reports of seat belt use note reporting bias in the "limitations" section of their reports but fail to acknowledge that such bias renders their estimates of belt effects on injury, and perhaps other claimed correlates of belt use, untenable (e.g., Allen, et al., 2006).

When belt-use laws were enacted in many states in the United States in the mid to late 1980s, belt use increased substantially and death rates declined commensurate with a belt effectiveness of 45% when used. That estimate controlled for changes in vehicle crashworthiness, alcohol involvement, economic conditions, vehicle size, and vehicle age (Chapter 13). The regression coefficient in Table 13-2 indicates a reduction of .007 in fatal crashes per 100 million vehicle miles for each percentage point increase in belt use. Belt use was approximately 53 percent in 1991 (National Highway Traffic Safety Administration, 1992). If the remaining 47 percent of car occupants had been restrained, the occupant fatality rate of 1.6 per 100 million miles would have been reduced by about 21 percent. This result is obtained by multiplying the coefficient in Table 13-2 by the percentunused belts (.007 x 47 = .329) and dividing the result by the death rate (.329 / 1.6 = 0.21). The implied effectiveness of belts when used is 21/47 or 45 percent. While the study did not account for belt use by the occupants of the vehicles in crashes, it had the advantage that the observations of belt use by the model year of the vehicle in the population were independent of knowledge of the crashes.

As more objective data become available, particularly among the fatally injured, we will at least be able to know more precisely the effectiveness of seat belts in combination with airbags as well as other vehicle factors and equipment. Since there are few, if any, cars that have data recorders and no airbags, an estimate of the effectiveness of seat belt use alone based on accurate belt use data may never be available unless vehicles in countries where airbags are less prevalent are equipped with data recorders. Unless the sampling protocol for NASS is changed from the one now based on inaccurate police characterization of injury severity, research using that system will remain invalid (Chapter 6).

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