Injury Epidemiology: Fourth Edition

Leon S. Robertson, Ph.D.

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Chapter 13. EVALUATION OF AGENT, VEHICLE AND ENVIRONMENTAL MODIFICATIONS

Product manufacturers and the builders of roads, housing, and other infrastructures and products have the opportunity to modify them to reduce the incidence and severity of injurious energy exchanges (Staunton et al., 2007). The sources of the vast majority of serious injuries are the products of industry and builders in everyday or frequent use: among them, drugs, motor vehicles, road characteristics, guns, agricultural and industrial machines, stairs, cigarettes, matches, propane lighters, stoves and space heaters, clothing, bedding, swimming pools, and watercraft.

Many industrially developing countries are repeating the mistakes that occurred during the industrialization of Europe and the U.S. Replacement of human and animal power by engine power exposes people to vastly increased energy, often with no attention to features or alternatives that would minimize harm (Berger and Mohan, 1996; Krishnan, et al., 1990).

Well-designed epidemiological studies can reveal the injuries associated with the characteristics of products and environments that injure. They can also aid in the evaluation of the effectiveness of modifications of products, protective equipment and environments in injury control.

Decisions that influence the rate of injuries associated with a given product may include design characteristics, quantity per package and type of package, quality control in manufacture or building the product, and extent and target of marketing efforts. Most of the efforts by government to influence these decisions for injury reduction have been focused on standards affecting designs or recalls of products that have design or manufacturing defects. The agreement between the United States Consumer Product Safety Commission and the manufacturers of "all terrain" vehicles that the vehicles would not be marketed for use by children, and the very occasional banning of a product by the Commission and labeling requirements by various agencies, are exceptions to the general lack of attention to marketing and its targets.

PRODUCT DESIGN. Research that relates product or environmental designs to injury rates and patterns can inform manufacturers, builders and governmental regulatory agencies; although there is often resistance to the inference that injury is "caused" by the design. Such research is frequently attacked on the grounds that every conceivable behavioral factor was not controlled or was inadequately controlled. In contrast, when a behavioral factor is correlated to rates of injury, questions regarding controls for characteristics of products involved are rarely raised by scientists, much less by manufacturers or the government.

Not only is analysis of causation sometimes a "long cut to prevention" (Chapter 8), so is the argument about causation with respect to injury from product characteristics. If children are being poisoned by aspirin, what are the causes -- the children’s natural tendency to put things in their mouths, the parents' or others' failure to store the aspirin inaccessible to children, the attractiveness of the aspirin package, the number of pills per package, or the color of the pills?

If the number of pills per package can be reduced or the cap on the bottle can be changed to make it difficult for the child to open it, why argue about primal causation or launch expensive studies to specify the effects of various risk factors, some of which are much less controllable than a product modification? In the case of child poisonings from aspirin, the aspirin manufacturers were informed by pediatricians and consumer groups that they could make a difference in child poisonings by modification of quantity per package and type of packaging and did so. The child poisonings from aspirin declined 80 percent (Done, 1978).

The hazardous characteristics of certain products are obvious but their consequences are often ignored. Examples are points and edges on interior and exterior surfaces of motor vehicles, sharp edges on the ends of roadside guardrails, the height of playground equipment relative to the hardness of the surface under it, the ease of use of handguns, and exposed moving parts of industrial, agricultural, and recreational machines. Product testing can reveal less obvious factors such as vehicle crashworthiness, that is, energy absorption by vehicle parts in crashes before the energy reaches the occupants.

Case studies can be a rich source of hypotheses regarding such characteristics of products, as they were historically in investigations of motor vehicles and aviation (e.g., Woodward, 1948; Gikas, 1972; Snyder, 1975; Champion, et al., 1986; Clark, et al., 1987; Shanahan, 2012). The epidemiologist can draw attention to the extent of a given problem and provide estimates of the effects of modification by correlating the incidence and severity of injuries to variations in relevant characteristics of the products or environments.

Opportunities to study relatively rare phenomena sometimes arise to epidemiologists at the site for other reasons. For example, an area in Guatamala, where nutrition studies were underway, experienced an earthquake. Epidemiologists were able to correlate injury with housing characteristics and other factors (Glass, et al, 1977).

Even products that are sometimes used to injure intentionally may be changed to reduce severity. As noted in Chapter 2, the physics of bullets and shotgun pellets have been described in detail (Sykes, et al., 1988; Ordog, et al., 1988; Karlson and Hargarten, 1997), but their relative contribution to severity of injuries in shootings has only recently been subjected to epidemiological investigation. Trends toward higher caliber weapons that remain small enough to be concealed will probably increase the fatalities per shooting (Wintemute, 1996). The companies that aggressively promoted the cheap "Saturday Night Specials" increased their marketing of more powerful, concealable handguns (Wintemute, 1994).

Even intended injury to self has been found modifiable by changing characteristics of weapon used. Suicide by cutting and piercing declined about 80 percent among males in England in parallel with the replacement of straight razors with safety razors (Farmer, 1992). Apparently no one studied the exact extent of razor involvement, but at least some of the reduction is likely from replacement of a potentially lethal instrument easily at hand. The removal of carbon monoxide from domestic gas was associated with a reduction of about one-third in the net suicide rate in England and Wales, but apparently not so in Scotland or the Netherlands (Kreitman, 1976; Clarke and Mayhew, 1988). In the United States, suicide by motor vehicle exhaust increased among males as that by domestic gas decreased, but there was an apparent net decrease in suicides among females (Lester, 1990). Various types of deliberate “means restriction” to prevent suicide have been demonstrated but there are not applied generally because of inconvenience to the public generally and misconceptions about the intent of suicide attempters (Yip, et al., 2012; Barber and Miller, 2014).

Characteristics of products and environments that may not be obvious, but likely contributors to injury incidence or severity, can be learned by review of elementary physics or chemistry (Chapter 2). Examples are energy-absorbing capability of vehicle components as well as trees, poles, guardrail and other roadside objects, stability of vehicles, cigarette burn rates, flammability of clothing, furniture, and bedding, and toxicity of drugs, household chemicals, and chemicals used in farming and industry. Again, the epidemiologist can learn the variations among products and, by observing the patterns of such variations in relation to relevant injury rates, estimate the effect of potential modifications (See Chapter 9, Appendix 9-1). For example, legislation requiring lit cigarettes to self-extinguish when not being smoked was associated with a 19 percent reduction in rates of death from residential fires as states adopted those laws (Yau and Marshall, 2014)

Biomechanical studies in the laboratory also suggest hypotheses for epidemiological investigation. Laboratory studies of simulated pedestrian collisions using bumpers of varying height indicated that bumpers twenty-five or more inches from the road surface produced more severe injuries (Weiss, et al., 1977). Epidemiologic study of the injuries to people struck on the road showed more severe injury related to higher bumper height (Ashton, 1982).

Collaboration of biomechanical and epidemiological researchers can contribute to understanding of injury tolerances of subsets of the population. For example, examinations of severity and circumstances of injuries to children in free falls and biomechanical simulations of the events were used to estimate tolerances of children to head impacts (Mohan, et al., 1979).

EVALUATION OF PRODUCT CHANGES. It is also important to know whether product modifications intended to reduce risk are functioning as intended. For example, of twenty families who had anti-scald devices attached to bathroom faucets, 19 had removed them within 9 months because of sediment buildup in the devices (Fallat and Rengers, 1993). Antilock braking systems on motorcycles are associated with 31 percent lower deaths per vehicle compared to the same makes and models without the system (Teoh, 2013). New crash avoidance technologies on cars, SUVs and trucks such as automatic brakes, rear cameras, adaptive headlights, lane departure warning, lane departure prevention and better vision of the side “blind spots” are available as options on more expensive vehicles. Evaluation of their effectiveness in reducing crashes and crash severity is needed. Updates on the technology and research on it is posted on the website of the Insurance Institute for Highway Safety (2015).

Most governmental standards for products are performance standards that do not directly dictate design, but set minimum limits for performance in injury reduction or, in the case of worker injuries, set standards and allow for inspections of workplaces. The United States Federal Motor Vehicle Safety Standards, for example, specify criteria for performance of components, such as the energy absorption of steering assemblies in frontal crashes, but do not indicate how the component is to be designed to meet the standard.

The effectiveness of a regulation depends on the technical effectiveness of the regulation relative to the performance of the regulated product, process, or environment prior to the regulation, and the degree of compliance to the regulation by manufacturers (or other relevant organizations). There is also the argument, noted in Chapter 12, that persons whose risk is reduced will behave differently, offsetting the effect of laws or regulation. Another test of the hypothesis that increased protection would be offset by behavior was provided by the introduction of airbags in cars. Those drivers protected by airbags could have reduced their belt use to partly offset the reduced risk provided by the airbags but they did not. Belt use by drivers in airbag-equipped cars was not significantly different than in cars without airbags (Williams, et al., 1990).

The research on the effect of motor vehicle safety regulation includes conflicting conclusions, mainly based on inept research methodology by economists (Appendix 13-1). These issues are instructive regarding the misleading results that are often obtained from insufficiently disaggregated data. Some economists and other researchers apparently do not understand the point that specific regulations are directed at relatively homogeneous subsets of injuries (vehicle occupants, pedestrians, motorcyclists, rollovers, rear-end collisions, etc.) and it is necessary to disaggregate the subset to evaluate the effects accurately. The research should include the possibility of unintended consequences, including effects on other road users, but should not include built-in biases that falsely infer such consequences.

The evidence on the substantial effectiveness of the initial motor vehicle safety standards does not mean that all regulation is effective or that regulation is always the most effective way to achieve injury reductions. The time necessary for unregulated products to be discarded is also a factor in accomplishing the full effects of regulation, or product changes undertaken voluntarily. In the case of passenger cars, the average life of the vehicles is more than 10 years, so it takes that long for the regulations to have full effect as the older vehicles are scrapped. Other products, such as mattresses, may be used longer, and some, such as children's cribs, may be used for generations. Research on trends in child suffocations and strangulations suggest that standards for refrigerator or freezer entrapment and warnings on plastic bags reduced fatalities from those sources, but regulations regarding crib design have had less, if any, effect (Kraus, 1985).

EVALUATION OF PROTECTIVE EQUIPMENT AND ENVIRONMENTS.

Irrespective of the means employed to obtain use of increased protection (persuasion, laws directed at individuals, regulation or voluntary changes in products and processes), the effect of the protection in use is often worthy of epidemiologic study. Engineers who design and test protective equipment can indicate precisely the energy absorbed, reduction of access to moving parts, stability and other product characteristics, but other factors cannot always be anticipated -- the range of uses and factors affecting lack of use, the amounts of energy involved in actual injuries, and the possibility of misuse. Epidemiological data on the use and effectiveness of potentially injurious energy sources can be helpful in modifications of design or attempts at changes in how the equipment is used. Unfortunately, failure to consider plausibility of results misleads some researchers to unwarranted conclusions. In that regard, the decades-long debate on the effectiveness of seat belts is recounted in Appendix 5-1.

When new protective equipment is introduced, it is possible to conduct controlled experiments of effectiveness. Again, the best research design is the experimental-control design where feasible. For modifications of higher-priced products, experimental-control designs are often not considered feasible before sales to the general public, but if large volumes are bought for use by corporations or the government, random assignment to users in those organizations is possible. For example, the effect of the high-mounted brake light on crashes of cars while braking was studied experimentally in corporate and governmental fleets (e.g., Reilly, et al., 1980). A quasi-experimental design was used to study the effect of energy absorbing floors on severity of falls in a nursing facility for the elderly. The "quasi" refers to the fact that the new floors were not strictly randomly assigned among parts of the facility. The severity of fall injuries was reduced by almost half in the areas with energy absorbing floors but the confidence intervals were wide because the numbers on which the estimate was based were too small (Gustavsson, et al. 2015).

If the government or other agencies distribute certain products, it can be done experimentally. The New York Health Department undertook an experimental-control study of potential problems with a type of child-resistant cap for medicine containers (the "Palm-'N'-Turn" cap) by randomly distributing them in municipal hospitals and conducting follow up home visits in experimental and control groups (Lane, et al., 1971). The reduction in injuries to softball players due to introduction of breakaway bases was demonstrated by randomly rotating teams among fields with and without the breakaway bases (Janda, et al., 1988).

Many studies of protective technology are case-control studies done after partial adoption of the technology. This always raises the issue of selection bias; risk-averse people may more often be early adopters. One means of estimating potential bias in selection of cases or controls is the use of more than one control group in a case-control study. For example, in a study of the effect of bicycle helmets in reducing head injuries, cases were persons who were seen at five hospitals for head injuries while bicycling. One comparison group was bicyclists who came to the emergency rooms of the same hospitals for injuries other than to the head. Another comparison group of injured bicyclists was identified from the records of a group health plan. The self-reported helmet use in the two comparison groups was nearly the same, but was substantially lower in the head-injured group. Data obtained on several potential confounding factors in the three groups allowed adjustment for these factors in the estimate of helmet effectiveness given an injury while bicycling, which suggested a reduction in risk of head injury of bicyclists using helmets of about 85 percent (Thompson, et al., 1989).

That estimate, like seat belt use, may be too high to the extent that helmet use is misreported. The researchers had no way of verifying that helmets were actually used in the groups studied. To check on claims of use versus actual use, observed users and nonusers would have to be identified unobtrusively and later questioned regarding use. To control for whether or not bicyclists who are injured are more or less likely to be using helmets, a third control group would be needed -- those bicycling at the same times and places as the injured.

Modifications of environments under the jurisdiction of corporations or governments can usually be studied experimentally, but often are not. Extensive reviews of the literature on road modifications are available (Federal Highway Administration, 1982; Elvik and Vaa, 2009). Much has been learned from cross-sectional studies of road features and before-after studies of modifications. Often the differences in crashes or severity in before-after studies of environmental modifications are so large that the reduction is unlikely due to biased study design, but the lack of control sites and the potential for regression to the mean

(Chapter 11), where the modifications were targeted only at high incident sites, raises doubts about the exact magnitude of some effects.

For example, installation of flashing lights at rural stop signs was associated with an 80 percent reduction in fatal crashes at those sites (Hagenauer, et al., 1982). Lighting of intersections reduces night/day crash ratios by 12 percent (Bullough, et al., 2013). Removal of trees from near roadsides or use of impact attenuators at fixed objects was estimated to reduce fatal crashes by 50-75 percent (McFarland, et al., 1979). Pedestrian injuries in areas of New York's Safe Route to Schools initiative were substantially reduced compared to areas without (Dimaggio and Li, 2013).

An analysis of various studies that claimed large reductions in motor-vehicle crashes at high incidence sites (so-called "black spots") claims that those with controls for regression to the mean show less or no effects (Elvik, 1997). The study did not distinguish between reduced incidence and reduced severity. For example, a guardrail may reduce severity without reducing incidence. While modifications at sites that had temporarily high incidence rates would be expected to return to the average without the modifications, studies that are based on long experience of severe injuries at particular sites show remarkable reductions over similarly long follow-up periods when the modification would be expected to have a long-term effect, such as guardrail installations (Short and Robertson, 1997).

Case-control studies have indicated criteria for selecting high-risk sites for severe crashes where vehicles left the road (Chapter 7). Epidemiologists who become familiar with the literature and gain agreement with highway authorities, park administrators and others in charge of facilities to aid in study designs could make an enormous contribution to increased precision of estimates of the effects of road and other environmental modifications.

The AAA Foundation for Traffic Safety, in collaboration with state highway departments, developed a system for roadway modifications based on severe crashes on sections of roads in various US states (Harwood, et al., 2010). The studies included maps of roads with relatively high and low risks, ratings of roads as to protective features and follow up to see if modifications made a difference in injury risk. Unfortunately they combined fatal and unreliable A-rated "severe or disabling" injuries. The report is devoid of references to previous research. While such an effort may help to motivate states to do more to improve safety of roads, it would likely be more effective if more reliable data were used. As noted in Chapter 6, the A-rated injuries recorded by police are often not serious and serious internal injuries are sometimes not detected by police.

INSPECTING HAZARDS. Regulation that provides for inspections, such as the workplace inspections of the Mine Safety and Health Administration and the Occupational Safety and Health Administration, could have effects analogous to those discussed for legal controls of individual behavior in Chapter 12 -- general deterrence and specific deterrence. If corporate executives in a position to make changes wish to avoid citations and fines, they could make the changes in the absence of actual inspections (general deterrence). Some may make changes only after citation by inspectors (specific deterrence) and others may not respond even under those conditions.

A substantial increase in regulation of coal mines followed the enactment of the U.S. Federal Coal Mine Health and Safety Act of 1969. Death rates of miners in the prior ten years showed no trend, but declined rapidly in the 1970s (Weeks and Fox, 1983). Apparently no attempt has been made to study differential effects in mines inspected versus others to delineate the effects of frequency and quality of inspections. The inspections of coal mines were intense relative to other occupational settings, about 34,641 inspections of 2,131 active underground mines in 1977 for example. Inspections are more frequent in mines whose workers are organized in labor unions (Morantz, 2011).

The probability of inspection of workplaces regulated by the Occupational Safety and Health Administration (OSHA) was much lower than that of mines in the 1970s and declined substantially in the 1980s. A business could expect to be inspected once per 20 years in 1977 if the inspections were random (Mendeloff, 1979). OSHA targeted industries with higher injury rates for more frequent inspections and some plants were inspected every few years. Several econometric analyses of trends in worker injuries found no effect of OSHA (Mendeloff, 1979; Smith, 1976; Viscusi, 1979). Further analysis of disaggregated data found an effect in one instance but not significantly so in another (Smith, 1979).

One of the economists who found no effect of OSHA inspections claimed that increased protection of workers would be offset by behavior changes (risk compensation again), and that fines for plants cited were too low to provide an incentive for workplace modification in any case (Viscusi, 1979). In a project originally intended to study the epidemiology of worker injuries in three metalworking plants during eight years, I and a colleague serendipitously discovered new data on the issue of the effects on injury rates of workplace inspections (Robertson and Keeve, 1983).

Individual differences in injuries in the plants were mainly correlated to the type of work being done in specific departments with some correlations to worker age, formal education, and number of previous employers. Using regression estimates of the effects of these factors at the individual level, the expected injury rate in a given year in a given plant was calculated and compared to the actual rate. The actual rates were separated into injuries that could be observed objectively in the plants' clinics (lacerations, burns, amputations), which we called "objective injuries", and those that were discernable as to incidence or severity primarily by patient complaint (mainly back pain), which we called "subjective injuries". Figure 13-1 is a comparison of the actual and expected rates per year in one of the plants.

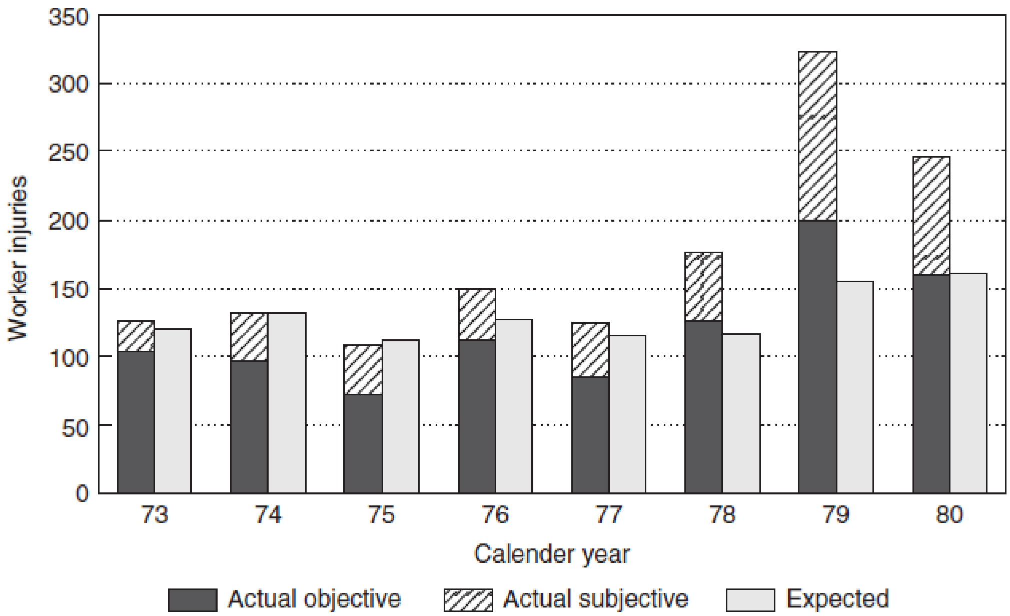

Figure 13-1. Actual Versus Expected Objective and Subjective Worker Injuries in a Metal-Working Plant

Noting that the actual rates varied from the expected rather sharply in certain years, we asked ourselves what other factors might have affected the rates in a given year. Two obvious external factors were OSHA inspections and Workers' Compensation available to workers who missed work because of injury. When the effects of these factors were examined, "objective" injuries declined relative to those expected in the year following OSHA inspections and "subjective" injuries increased more than expected when Workers' Compensation increased above inflation (Robertson and Keeve, 1983).

The lack of the effect of OSHA in the previous studies of aggregated data was apparently due to failure to control for workers' compensation effects. We also examined change in lost workdays from 1975 to 1976 among industries in 20 states, and the injuries decreased in correlation to an increase in OSHA inspections, but increased in relation to increases in workers' compensation. If "risk compensation" occurred at all, it did not offset the effects of OSHA inspections, but actual compensation that allowed the workers to take time off apparently did affect their absence when they could afford to be away from work, particularly for pain and strain, when working and not working is more discretionary.

The economic theory that the fines for OSHA violations were too small for deterrence did not seem a factor in the plants studied. We were given free access to correspondence regarding OSHA citations and it was obvious that management took the citations very seriously, in some cases spending more in time and travel to persuade OSHA to reduce a fine than the amount of the fine.

The deterrent effect of OSHA inspections did appear to be specific and temporary. Lagged correlations indicated no effect of an OSHA inspection beyond the first year following an inspection.

QUALITY CONTROL. Reducing hazards in products during the production phase may also be important for reducing injuries. In any manufacturing process, variances will occur in the products produced. Prudent manufacturers employ an inspection system to identify and correct safety defects from variances in manufacturing processes, as well as other aspects of quality of the product. Some manufacturers install internal checks on operability of safety and other systems even after the product is sold. For example, the airbags and some other systems in motor vehicles are checked electronically each time the vehicle is started.

An important issue in quality control of the manufacturing process, particularly for products or components that can increase injury and death, is how many to inspect and the criteria for rejection of a defective product or inspection of a total batch for defects when one or more is found in a sample. The cost of inspection is a small proportion of the cost of relatively expensive products, such as cars, but adds proportionately more to the price of inexpensive items, such as matches, propane lighters, and hand tools. In some cases, the inspection of every unit produced may not be feasible and a sample of the products in a batch is inspected.

The probability of finding a percent of defects in a batch is highly sensitive to the size of the sample inspected. Websites are available that allow manufacturers to estimate the probability of defects in a batch of products based on the number found in a sample (e.g., https://src.alionscience.com/toolbox/oneshotcalc.htm).

The size of samples used in quality control is proprietary to the manufacturers. The author is aware of no federal or state standards for sample size to detect defects from manufacturing variances. Motor vehicle manufacturers are required to report known design defects or defects due to design or manufacturing variances to the National Highway Traffic Administration, but known defects have not always been reported.

A requirement that sample sizes used for detecting defects be revealed publicly, as well as the numbers found, and the procedure for inspecting the batch given defects in the sample, would likely result in greater manufacturer attention to the problem. It would also give epidemiologists a tool to correlate the types of quality control and sample sizes to injury incidence and severity related to product variations.

Appendix 13-1 Evaluation of U.S. Motor Vehicle Safety Standards

Motor vehicles were essentially unregulated in the United States until the 1960s except for a few consensus standards adopted by the states, such as for headlamps. Installation of lap belts in front outboard seats was required by several states in the early 1960s, and, by 1964, the manufacturers installed them in passenger cars as standard equipment. The federal government required certain standards, such as energy absorbing steering assemblies and windshields, in 1966 and later models sold to the government, and the manufacturers included these features in public sales of certain models that also were sold to the government in substantial numbers. In 1968 and subsequent model years, the federal government required numerous standards, including several to reduce energy exchanges of occupants and vehicles in crashes and several to reduce incidence, such as reduced glare in drivers' eyes, redundant brakes, and side running lights.

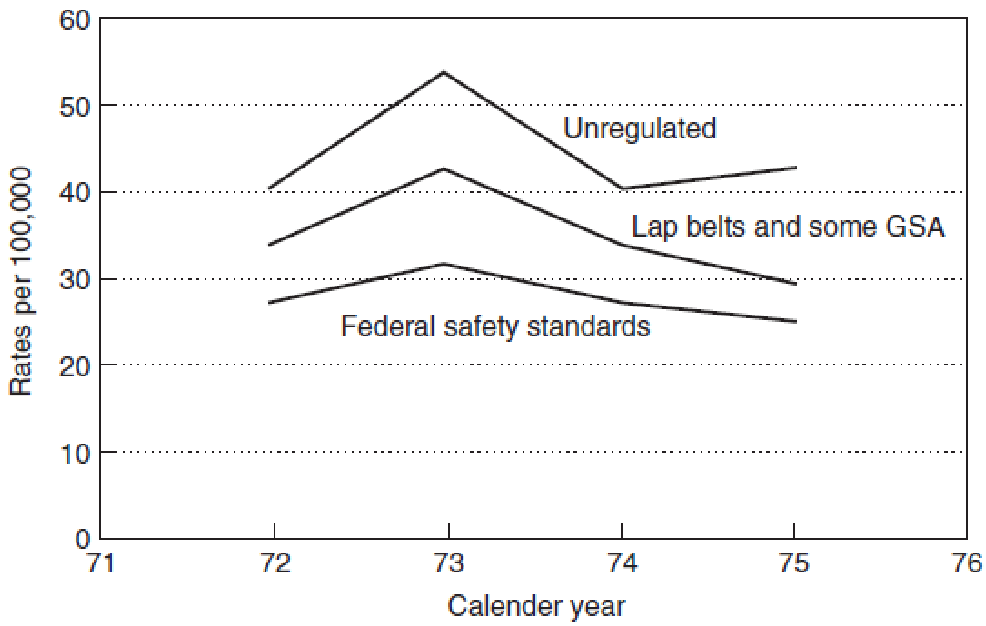

Figure 13-2. Death Rates to Car Occupants in Maryland: 1972–1975.

Since national data disaggregated by type, make and model of vehicles in crashes were not available until FARS (now called the Fatal Analysis Reporting System) began in 1975, it was necessary to assemble state data to obtain such detailed information. I undertook such a study using police reports of fatal crashes in Maryland to evaluate the overall impact of Federal Motor Vehicle Safety Standards. The data indicated that, during the calendar years 1972-1975, occupant deaths averaged 44 fatalities per 100,000 cars in pre-1964 model cars, 35/100,000 for 1964-1967 cars and 27/100,000 for 1968-1975 model cars, a decline of 39 percent from unregulated pre-1964 cars to the federally regulated cars

(Figure 13-2). Deaths to pedestrians struck by the cars, compared by model year, were not significantly different -- 8/100,000 for pre-1964 cars, 10/100,000 for 1964-1967 cars and 9/100,000 for 1968-1975 cars (Robertson, 1977).

Factors such as driver age and vehicle age that later came into contention regarding the effect of regulation were not found in the Maryland study. Percent of youthful drivers (in this case less than 26 years old) was slightly less in the older, un-regulated cars -- opposite to subsequent conjectures that older cars had higher rates because they were driven by younger drivers. The death rate of the pre-regulation cars 1960-1963 model cars during 1972-1975 in Maryland was the same as the national death rate for passenger cars in the calendar years 1960-1963, 44 per100,000 registered. Therefore, age of vehicle did not explain the reductions in deaths to occupants of regulated vehicles.

After completion but prior to publication of the Maryland study, an economist published a study using aggregated trends in motor vehicle fatalities and other factors in an attempt to evaluate the effect of motor vehicle safety regulations on fatality rates. He argued that drivers with increased occupant protection would drive more "intensively" -- the "risk compensation" or "risk homeostasis" hypothesis -- and kill more pedestrians.

His study design involved projections of expected death rates based on correlations of death rates and trends in other aggregated data over time. He included separate regressions of total occupant and "pedestrian" death rates during 1947 to 1965, allegedly using as predictors: alcohol consumption per capita, average speeds on rural roads, linear trend, ratio of younger to older persons in the population, income, and cost of crashes in those years. The regression equations were then used to project the expected death rates in 1966 through 1972 based on year-to-year indicators of the other variables during that time. The actual occupant rates were less than expected but the "pedestrian rates" were greater than expected, the latter offsetting the former. The results, he said, supported risk compensation theory and indicated no net benefit of regulation (Peltzman, 1975).

Having found no effect of regulation on pedestrian deaths in the Maryland study, perhaps because the numbers were too small for statistical power, I obtained the data used in the econometric analysis in an attempt to account for the difference in results of the two studies. The econometric study was laced with methodological errors. Not only were regulated and non-regulated cars not separated, occupants of unregulated trucks and "pedestrians" struck by them were not disaggregated from regulated cars. Motorcyclists were counted as “pedestrians" and single-vehicle motorcycle crashes were not disaggregated from those in collisions with other vehicles. Motorcycle registrations were doubling every five years, guaranteeing a substantial increase in their deaths.

There were also problems with the predictor variables. The alcohol-consumption variable excluded beer. The crash-cost index was based on the Consumer Price Index for auto repair services, which includes such things as oil changes and filters, but not the cost of auto parts damaged in crashes. The "youth" variable was ratio of 15-24 year olds to older persons in the population rather than their percent as licensed drivers or drivers in crashes, which was known for all but three of the earliest years studied.

A regression equation based on 1947 to 1960 data did not predict the rates in 1961-65, a simple check on the validity of the model that would have ruled out its use to evaluate regulation. Some of the predictor variables were virtually substitutes for one another, a condition called multi-collinearity that distorts regression coefficients, and their correlations changed drastically in the pre- and post-regulation periods, guaranteeing invalid projections (Table 13-1). Although just what was considered as "intensive" driving was not specified, the most likely candidate, speed, was used as a predictor variable rather than an outcome variable (Robertson, 1977).

Table 13-1. Correlation Matrix of Data Used by Peltzman (1975) to Project Fatality Rates, 1947–1965 (1966–1972 in parentheses)

	1.	2.	3.	4.	5.
1. Crash cost index	—				
2. Income/capita age 15+	–.87 (.88)				
3. Linear trend	–.92 (.80)	.97 (.95)			
4. Alcohol consumption	–.85 (.72)	.90 (.95)	.91 (.91)		
5. Average rural speed	–.89 (.68)	.98 (.88)	.98 (.85)	.89 (.96)	
6. 15- to 24-year-olds/ older population	–.34 (.78)	.29 (.95)	.37 (.99)	.57 (.91)	.32 (.99)

Since publication of the inept original study, it has been cited frequently as gospel in the economic literature, usually without reference to my critique and other research contradicting it, including one study on similar regulations in Sweden published later in the same journal (Lindgren and Stuart, 1980). A recent book on alleged government failures cited the Peltzman (1975) study as fact rather than reviewing the contradictory research (Schuck, 2014).

When the FARS data became available for a number of years (1975-1981), it was possible to examine separately data on regulated and unregulated vehicles nationally. Using a survey of mileage per vehicle age, the effect of state and federal regulations on death rates per mile of occupants, pedestrians, motorcyclists and pedal cyclists in collisions with specific vehicles were estimated in a regression equation, controlling for age of vehicle and type of vehicle (cars versus trucks). The data indicated that significant reductions in car occupant death rates were associated with state regulations, and deaths both to car occupants and non-occupants struck by cars were lower in those subject to federal safety standards. These results were consistent with the fact that state lap-belt requirements and standards for government cars in 1964-67 models were exclusively aimed at occupant protection in crashes while the 1968 and subsequent federal standards included crash avoidance. The occupant death reduction was similar to that found in the Maryland study, 40 percent (Robertson, 1981).

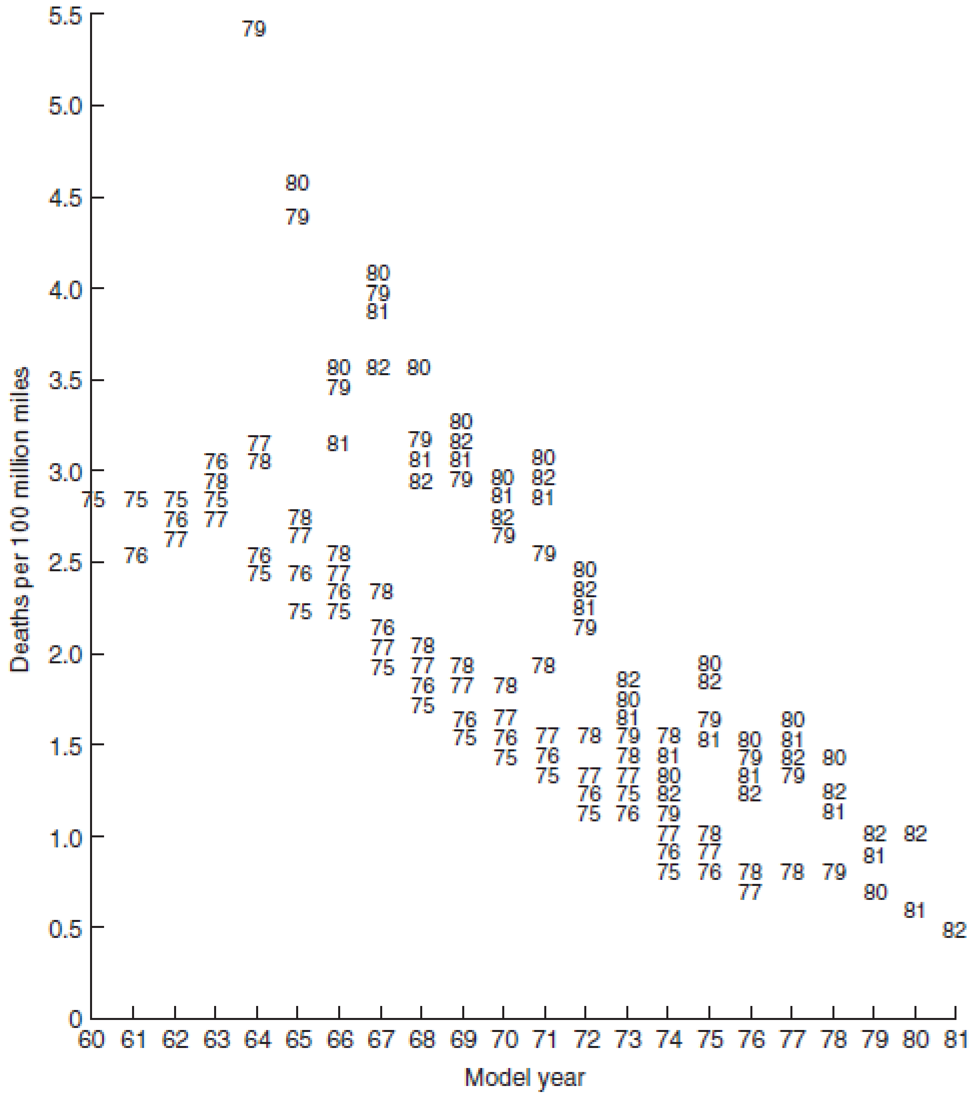

Figure 13-3. Death Rate by Model Year (Calendar Year as Data Points).

The study of the FARS data was attacked by an economist who claimed that it was inappropriate to compare cars to trucks Because age of vehicle and model year was correlated, the critic said that the regulatory effect was greatly reduced mainly by his estimate of a vehicle-age effect (Orr, 1984). His estimate of lives preserved, however, was miscalculated by 38 percent using his own regression coefficients (Robertson, 1984). A simple graph shows that the primary decline in death rates occurred in relation to model year of the vehicles and not vehicle age, as indicated by calendar year in Figure 13-3.

The most misleading characterization of the alleged vehicle-age effect was presented in a conference at General Motors. Graphs of model year differences were separated by calendar year, removing rather than demonstrating the vehicle-age effect, and totally and mistakenly attributing all model-year differences to vehicle age (Adams, 1985).

Since more data is accumulated each year in the Fatal Analysis Reporting System (FARS), the opportunity to evaluate the effects of safety standards and other factors is available as conditions change. Adoption of safety standards virtually ceased from 1978 to 1987. The Reagan Administration rescinded the federal standard that required seat belts that automatically encircle front-outboard occupants or, in the alternative, air bags. It was restored in the late 1980s only after the courts ruled that the administration had acted illegally. The National Highway Traffic Safety Administration did continue frontal crash tests of a selection of cars annually and published the results. Manufacturers were embarrassed enough by the reported forces on occupant test dummies at 35 miles per hour that on occasion they requested a retest after modifying the vehicles.

The crash tests indicated incremental improvement in crashworthiness during the 1980s, as evidenced by reduced forces on crash-test dummies. Research on fatalities to occupants in frontal crashes indicated reduced deaths in frontal crashes of vehicles that performed well in the crash tests (Zador, et al., 1984b; Kahane, 1994), but the research designs of these studies did not allow for the possibility of offsetting behavior -- the alleged increased risky driving. Seat belt use laws, enacted by the states from 1985 to 1990, largely accounted for more than doubling of seat belt use (National Highway Traffic Safety Administration, 1975-1991) and alcohol in fatally injured drivers declined about 20 percentage points during the 1980s (National Highway Traffic Safety Administration, 1995).

By combining FARS data with data from other sources on vehicle use and belt use, I assessed the effects of the various mentioned government policies. Death rates per 100 million miles use in which passenger cars were involved, by model year of the vehicles and whether the death was to one or more occupants of the cars, were tabulated for model years 1961-1990 in each of the calendar years 1975-1991. The miles per vehicles of a given age in a 1988 mileage survey (Energy Information Administration, 1990) were adjusted to other years by multiplying them by the ratio of average miles driven in other years to the 1988 average (Federal Highway Administration, 1975-1992). The mileage survey included periodic calls to the same households to obtain odometer readings, which is probably more valid than asking people their annual mileage. Rates were available on 254 combinations of vehicle model year and calendar year.

Two sets of rates per 100 million miles were analyzed separately: 1. occupant fatalities, 2. crashes fatal to non-occupants (occupants of the other vehicle in multiple-vehicle crashes, pedestrians and bicyclists). The first allows an estimate of the effects of safety standards, improved crashworthiness and the other factors on all occupant deaths. The second examines possible effects on other road users. If there is offsetting behavior, non-occupant death rates should increase in relation to regulation, crash test publicity, or belt use.

Since the General Services Administration imposed some safety standards in 1966 on cars sold to the government and the standards for all cars began to be imposed in 1968, the absence of standards is correlated with vehicle age. Data on the 1975-77 models were available for a full 15 years of vehicle use. The death rates (each of the two sets separately) per mile were calculated for these model years for each year of age and averaged among the three model years. The death rates were neither linear nor monotonic as the vehicles aged (Robertson, 1996). Ten to fifteen-year-old 1975-77 models had substantially lower death rates than the ten to fifteen year old pre-regulation vehicles. The 1975-77 rate for a given aged vehicle was used as an expected rate for a vehicle of that age to control for variation attributable to vehicle age.

Other factors considered were the "downsizing" of vehicles and economic conditions during a given calendar year, both of which have been correlated to death rates, observed seat belt use in a given calendar year, and percent of drivers with alcohol greater than 0.10 percent by weight in a given calendar year. Smaller vehicles have higher occupant death rates because of less interior space to decelerate and deaths per mile are marginally higher in years of greater economic prosperity. Wheelbase, the distance from the front to rear axle, has been shown to be the best predictor of differences in fatality rates due to vehicle size (Robertson, 1991).

To control for vehicle size, the death rates per mile in calendar year 1988 were calculated for seven categories in five-inch increments of wheelbase (from <95.1 to 120.1+), using the 1988 mileage survey and decoded vehicle identification numbers for make and model of vehicle in the fatal file and mileage survey file. Expected fatalities were calculated by multiplying the 1988 rate for each size times the numbers of vehicles of those sizes sold in a given model year, discounted for numbers scrapped as they aged (Ward's Automotive Yearbook, 1960-1992; Flammang, 1992). The expected number was then divided by the mileage previously estimated for each model and calendar year. The index of industrial production was used as an indicator of economic activity (Council of Economic Advisors, 1994). Belt use in a given model-calendar year was included from the annual survey of 19 cities and their environs, extrapolating for a few years in which the survey was not done (National Highway Traffic Safety Administration, 1975-1991). Percent of alcohol in fatally injured drivers for each model-calendar year was obtained from FARS in states that test 80 percent or more of such drivers.

Ordinary least-squares regression was used to estimate the effects of the various factors. The variable for minimum safety standards was zero for pre-1966 models, incremented from one to twelve in 1966-1977 models and assigned twelve for 1978-1991 models. As noted, the reduction in occupant death rates has been shown to be incremental in 1966-1977 models, partly because of the imposition of new standards in various years of that period and partly because of delays in meeting the standards in some models. The publication of crash test (NCAP) results began in 1979, so the NCAP variable is zero for 1961-1979 models and incremented by one, from one to eleven consecutively, for 1980 to 1990 models, based on the assumption that crashworthiness was improved incrementally as the crash test results for particular makes and models became known.

The regression coefficients for the predictor variables are presented in Table 13-2. Controlling for the expected effects of vehicle age and size differences, the incremental model years in which minimum standards were imposed and the model years during which NCAP tests were publicized are strong predictors of reduced occupant death rates and are somewhat related to reduced non occupant fatal crash rates.

Table 13-2. Regression Estimates and 95% Confidence Intervals of the Effects on Passenger Car Death and Fatal Crash Rates per 100 Million Vehicle Miles, 1961–1990 Cars in 1975–1991

Variable	Occupant Deaths	Nonoccupant Fatal Crashes
Standards	–0.260 ± 0.023	–0.055 ± 0.010
NCAP publicity	–0.077 ± 0.033	–0.029 ± 0.020
% Belt use	–0.007 ± 0.006	–0.006 ± 0.004
% Alcohol > 0.10	0.007 ± 0.006	0.007 ± 0.005
Calendar year	–0.017 ± 0.036	–0.016 ± 0.022
Industrial production	0.029 ± 0.014	0.019 ± 0.009
Expected age of vehicle effect	0.444 ± 0.212	0.289 ± 0.096
Expected wheelbase effect	1.066 ± 0.454	1.252 ± 0.299
Intercept	–0.793	–1.950
R^2	0.92	0.86

In the 1966-1977 model passenger cars, the reduction was an average 0.26 death per 100 million vehicle miles (mvm) per model year across 12 model years for a total reduction of 3.12 deaths per 100 mvm (.26 x 12). The reduction in 1980-1990 models was 0.077 per model year, a total of 0.847 per 100 mvm in 11 model years (.077 x 11). The effect of belt use increases and alcohol reductions were much less. A 40-percentage point increase in belt use reduced the rate .28 (40 x .007) and a 20 percentage point reduction in alcohol reduced the rate .14 (20 x .007).

The rates increase marginally in more economically prosperous years as indicated by the predictive coefficient on the Index of Industrial Production.

There is no significant linear trend in the rates independent of the other predictor variables as indicated by the coefficient on calendar year.

The results support the conclusion that vehicle-related fatalities were reduced substantially by increased crashworthiness and somewhat by increased seat belt use and reduced alcohol use. Contrary to offsetting behavior theory, the evidence indicates that the reduction of fatalities per mile attributable to increased crashworthiness of passenger cars and increased seat belt use was not attenuated by increased risk to others from more protected drivers. Indeed, more regulated vehicles were in fewer crashes fatal to other road users.

There are reasonable explanations for the latter. The minimum safety standards included crash avoidance standards (redundant brakes, reduced glare in driver's eyes, and side running lights) as well as crashworthiness standards. The period of NCAP tests and increased seat belt use was also the period of increased aerodynamic designs to save fuel, which may, coincidentally, reduce severity of pedestrian and cyclist impacts. The points and edges on older models were related to increased risk to pedestrians (Robertson, 1990). The "crumple zones" that absorb more energy may help reduce the velocity changes to occupants of other vehicles in crashes with the more crashworthy vehicles.

Although most of the reductions in occupant deaths are model-year specific during the period of regulation or publicized crash tests, and therefore vehicle rather than driver or environmentally based, the data do not allow the conclusion that all of the reduction is attributable to regulation or embarrassment. Some aspects of crashworthiness might have been adopted without government standards had there been no regulation, although the failure of most manufacturers to adopt available technology, until required to do so in the 1960s, and the 18-year battle against the airbag in the 1970s and 1980s, suggests otherwise. And concern for sales lost to manufacturers of vehicles that had better crash indices in the NCAP tests may have motivated improvements, irrespective of embarrassment.

Following that study, another econometric study was published that claimed offsetting behavior (Chirinko and Harper, 1993) apparently from failure to separate passenger cars from pickup trucks and utility vehicles in a time-series analysis. This will give false results on the effects of regulation on passenger cars, not because the regulated cars are more often hitting trucks as claimed by recent proponents of the risk compensation hypothesis, but because the government failed to impose standards to reduce rollover of unstable pickup trucks and utility vehicles that have grown substantially in use. (See Chapter 8, Appendix 8-1).

This discussion may seem to be only an academic quarrel over study designs, but it had more ominous results. The anti-regulation administrators of federal agencies in the 1980s until recent years were often advised by neoclassic economists, and those who attempted some feeble extension of safety regulations were blocked by the neoclassic economists and others in the Office of

Management and Budget in the White House. The long delay in the adoption of airbags was but one of many delays in regulations influenced by these intra governmental debates.

Economic determinists have claimed that reversal in the growth of road deaths among more economically developed countries in the second half of the 20th Century was mainly a function of economic development. They think that such a reversal in developing countries will occur only when those countries reach a level of economic growth similar to the more economically developed countries. In fact, the change among the developed countries occurred when scientists realized that road deaths could be controlled by focusing on vehicle and road characteristics rather than driver error and politicians implemented the laws that included that focus (Bhalla and Mohan, 2016).

References – Chapter 13

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