

Injury Epidemiology: Fourth Edition

Leon S. Robertson, Ph.D.

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Chapter 12. EVALUATION OF LAWS AND RULES DIRECTED AT INDIVIDUAL BEHAVIOR

Governments sometimes impose laws applicable to the public or segments of the public in an attempt to change behavior to reduce injuries. Also, in organizations such as corporations, sports leagues, and summer camps, there are often formal rules that proscribe or require particular behaviors thought or known to be related to injury risk. Laws and rules directed at individual behavior are generally far more effective than attempts at voluntary behavior change (Robertson, 1975). For example, changing certain rules for sports events was found to strongly reduce injury rates (Roberts, et al., 1996). This chapter is primarily focused on the use of epidemiological data in evaluating the effects of laws and their enforcement on injury incidence or severity.

To simplify discussion, the word "law" will be used here to refer to administrative rules as well as governmentally imposed laws directed at individual behavior. Two types of laws have been adopted in attempts to reduce injuries: those that proscribe behavior thought to increase risk and those that require behavior thought to increase protection. Examples of proscribed behaviors are murder, assault, drunk driving and exceeding posted speed limits in motor vehicles. Protective laws include measures such as required use of hardhats at worksites, required use of helmets by motorcyclists and bicyclists, and use of seat belts or child restraints by vehicle occupants.

Epidemiological data may provide lawmakers and administrators with information to justify, change, or repeal laws or rules (Bergman, 1992). For example, the high infant and toddler mortality rates in motor vehicles during the 1970s were used to persuade legislators to enact laws requiring restraint use among younger children. Post-enactment analysis identified gaps in coverage of child-restraint laws, indicating that 39 percent of children 0-5 years old killed as occupants were exempted from the laws and thus demonstrated a need for legislators to consider filling the gaps (Teret, et al., 1986). A study of the laws

effects among the 50 states indicated a reduction of about 4.8 percent in the fatality rate for each additional year of age from 1 to 6 in which child restraint use is required (Houston, et al., 2001).

Required protective behaviors include not only use of protective equipment, such as seat belts in motor vehicles, but may also include procedures to be followed, such as the checklists of airline pilots. Although there is less theory regarding the effects of such laws than that regarding deterrence of proscribed behavior, a body of research does suggest some generalizations whereby the probable success of the laws can be estimated. It can be argued that deterrence theory applies to protective laws as well, but coercing someone into doing something that they would not ordinarily do is not necessarily the same thing as deterring someone from doing something that he or she is wont to do.

Deterrence. The theoretical models regarding the effect of law on proscribed behaviors are called deterrence theories (e.g., Ross, 1982). Such theories focus on several factors that may affect the behavior in question: probability of detection of the proscribed behavior by the enforcement authority, probability of conviction for the offense, time from detection to conviction, and severity of punishment if convicted.

Two types of deterrence are considered: specific deterrence of repetition of the proscribed behavior (recidivism) by those detected, and general deterrence, which includes reduction in the proscribed behavior of those not detected. Research on detected recidivism is relatively easy using police, court, or medical records, but the inferences of the effects of law on general deterrence are more difficult to support with scientific evidence.

The "public health" and "criminal justice" approaches have been falsely characterized as a difference between prevention of first offenses versus reduction of recidivism (Moore, 1995). Most theoretical criminologists have long recognized the possible general deterrent effects of laws and their enforcement and competent public health researchers are well aware of the necessity to reduce repeated incidents of a disease or injury.

The premises of deterrence theory are at least partly dependent on the question of the extent of intent in human behavior related to injury. If all homicides or assaults were well planned by people weighing the costs and benefits, as economic theories of "crime" would have us believe (e.g., Becker, 1968), then enough increase in the cost to the offenders should deter potential offenders from the acts. To the extent that assault is momentarily impulsive or reactive behavior without rational deliberation, or the result of mental illness, weighing of the potential consequences by the assailant may be relatively inconsequential (Smith and Warren, 1978).

Most drivers deliberately or inadvertently violate traffic laws daily and the discretion of the police in enforcement often looks arbitrary and sometimes capricious (Ross, 1960). How much this leads to disrespect for law and police

beyond the road environment is unknown, but it is likely that frequent observation of law violations increases the probability of such behavior. Police may discriminate by race, gender and socioeconomic status with impunity (Cressy, 1975) unless the incident escalates into violence, as several publicized cases have in recent years in the U.S., further increasing the disrespect of police, particularly among those who identify with people singled out.

Once a law is enacted, the simplest question regarding a law is: does it reduce injuries? The simplest, but sometimes misleading, research design to answer the question is to compare injury rates before and after the law. If other factors are changing that would have increased or reduced injuries during the same period of time, the change in injuries can be falsely attributed to the law.

For example, when a supervising judge in Chicago required seven-day jail sentences for drunk driving during a period in 1970-1971, the death rate in the city declined and the rule was declared a success. Aware of the threats to validity in before-after comparisons (Campbell, 1969), researchers designed a quasi-experimental study to examine the effect of Chicago's judicial policy. Comparison of the death rates in Chicago and the nearby city of Milwaukee during the same period indicated that a similar decline in motor vehicle fatalities occurred at the same time in Milwaukee, which did not have a policy of mandatory jail sentences (Robertson, et al., 1973). The reduction in fatality rates was likely the result of recession in the economy or changes in weather patterns that happened concurrent with the judge's sentencing policy.

Since most legislative bodies are unlikely to enact a law for research purposes, the enactment of laws is not necessarily random, and research must be designed to account for pre-law differences and changing conditions among jurisdictions. The use of rules in corporate settings could be studied in randomized experimental-control studies, but if such are conducted, they usually are not reported in the literature. Attempts have been made to study the effects of differing sanctions by having judges randomly assign sentences but many judges would not cooperate, preferring to retain their discretion (Ross and Blumenthal, 1975).

Police departments have been somewhat more amenable to the use of randomized experimental designs in an attempt to find out if resources can be more effectively allocated or if certain procedures are more effective. For example, during more than a year, fifteen police districts in Kansas City were divided into three groups of five each: one set had the usual preventive patrol activity of a patrol car per district; a second had double to triple the usual patrol; and a third had no preventive patrol -- the police responded only when called regarding a problem. Comparison of various crime rates and motor vehicle crashes among the areas, including victimization surveys as well as those reported to police, indicated no differences in assaults and motor vehicle crashes. The three comparisons that were statistically significant -- sex crimes other than

rape, home burglaries, and community vandalism -- did not consistently support a case for more intensive patrols (Kelling, et al., 1974).

Several subsequent experimental studies were undertaken in an attempt to define aspects of police procedure and response that might make a difference, but more recent efforts are focused on first defining the dimensions of the variety of problems that the police encounter and developing tailored prevention programs or responses (Goldstein, 1990). This is no less than the use of epidemiological surveillance and analysis of relatively homogeneous subsets of the problem noted in Chapters 6 and 7. This approach requires a reorientation of policing from treating each incident as a case to be investigated and resolved to a search for patterns that can guide targeted efforts. The research evidence indicates that concentration of police activity in "hot spots" of crime, particularly in unpredictable sequences for limited periods of time, reduces crime in those areas without displacing it elsewhere (Sherman, 1996). For example, randomly assigned "hot spot" areas where foot patrol by police was introduced in Philadelphia experienced a 27 percent reduction in violent crime compared to control areas (Ratcliffe, et al., 2011).

Injuries related to guns have been reduced using the "hot spot" approach. In a controlled experiment in Kansas City, police concentrated in areas with high gun-related crime used traffic violations to increase gun seizures. Shots fired decreased 81 percent in the experimental area compared to 32 percent in the comparison area (Sherman, 1996). While "hot spot" enforcement is not a panacea, a meta-analysis shows consistently effective results in reduced violence and positive community response as well (Braga, et al., 2014).

Some of the literature on targeting countermeasures based on surveillance raises the question of whether the targets should be based on rates per exposure versus total numbers. For example, one report from a police service in Canada compared police-reported motor-vehicle crashes and convictions of drivers by age and gender relative to a survey of kilometers driven. Based on a narrowing of differences in crashes and convictions among age and gender groupings when considering rates per kilometer driven relative to proportionate involvement, the author of that study suggested that less attention should be paid to young and/or male drivers (Mercer, 1989). Yet, from a public health standpoint, one would target the groups with the largest numbers whether those numbers were generated by disproportionate exposure or not.

Particular aspects of the motor vehicle injury problem are known to be concentrated on certain roads at certain times of day. For example, Figure 12-1 presents illegal alcohol involvement in fatally injured drivers in the U.S. by type of road and time of day in counties where alcohol was actually measured in more than 90 percent of fatally injured drivers. While the amount of driving with illegal blood alcohol on these roads might be useful to indicate how other conditions combine with alcohol to increase or decrease individual risk, the data are clear regarding enforcement of law. If the police are to maximize the

apprehension of persons with illegal blood alcohol who will be in severe crashes, they will concentrate their alcohol squads on two-lane roads at night where 61 percent of fatal crashes with illegally intoxicated drivers occur.

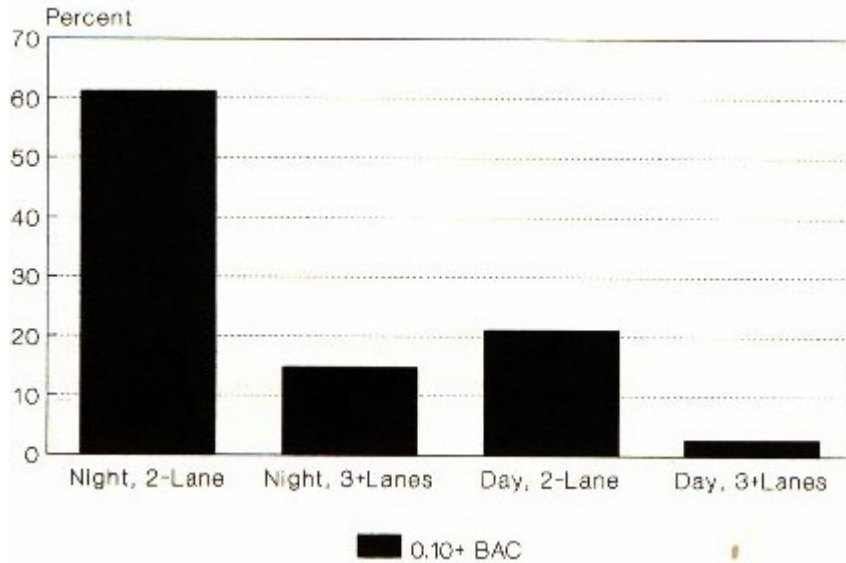


Figure 12-1. Time of Day and Type Road of Fatal Alcohol-related Crashes

Research is needed to identify other clusters of problems that law enforcement could be focused. For example, sales of guns used in felonies have been found concentrated among identifiable dealers, controlling for crime rates and socio-demographic factors (Wintemute, et al., 2005). Do assaults in public occur repeatedly in certain bars, other establishments or on certain streets during certain hours? Do domestic assaults occur repeatedly in definable households at certain times of day and days of week?

Although assault among members of households resulting in injury requiring medical attention is more frequent than assault by strangers, arrest in domestic assault cases in the 1970s was about half as likely as in cases involving strangers (Berk and Loseke, 1980-81). A controlled experiment of the effect of arrest on specific deterrence of spouse battering was conducted in Milwaukee. In response to calls regarding domestic violence, police randomly applied one of three actions: 1. arrest of the assailant, 2. ordered the assailant away from the premises for eight hours, or 3. gave advice or attempted mediation. Follow-up study of police records and interviews with the persons assaulted indicated that arrest was the most effective policy to reduce recidivism (Sherman and Berk, 1984).

A retrospective study of 28 months of police records in a California county, controlling for factors that affected probability of arrest, found the same result (Berk and Newton, 1985). Since time in jail has implications for job or income loss, the authors of the latter study suggested another controlled experiment with treatments such as: 1. a citation (like a traffic ticket) 2. arrest, book and immediate

release, or 3. arrest and hold for a hearing. If the first or second were found as effective in deterring battering as arrest and hold, they would be preferable. A 23-year follow up of those arrested vs. warned in the original Milwaukee study found no differences in various risks except a statistically insignificant higher homicide rate among those arrested (Sherman and Harris, 2013).

While many cities adopted an arrest policy for domestic violence as a result of the research, a substantial debate ensued regarding the methods of these studies, such as reliance on police records, and unmeasured effects, such as more severe injuries from escalation of domestic disputes exacerbated by the arrest (Sherman, 1992). Replication of the Milwaukee experiment in other cities did not produce consistent results but some evidence of efficacy was found when the various studies were compared (Buzawa and Buzawa, 1996; Christopher et al., 2001). This experience illustrates both the difficulty in policing private behavior and in learning whether or not police response has an effect because of questionable accuracy of reporting private behavior.

Historically many police departments reduced foot patrols in favor of motorized patrols. Recent research found that intensified foot patrols in high crime areas decreased violent crimes compared to similar areas without the patrols. There was some spillover of robberies into other areas suggesting that predictability of the patrols be reduced (Piza and O'Hara, 2014).

An example of an experimental study of detailed aspects of the effects on publicly observable behavior of a combined publicity and enforcement campaign is the research on elderly pedestrian behavior in Dade County, Florida. The experimental area was a street in Miami Beach with a high concentration of elderly residents and the control area was a comparable street with similar aged residents in Coral Gables. Cameras mounted high above the street recorded pedestrian and post-crossing interviews were also obtained, before, during and after the increased enforcement of a law prohibiting jaywalking (crossing the street outside designated pedestrian crossing areas).

Prior to increased enforcement, news media coverage and speeches by police officers to 35 groups in the experimental area emphasized the law and the date of increased enforcement. During a two-week special enforcement period, extra police foot patrols were deployed and about 300 tickets for jaywalking were issued, compared to about 40 per month ordinarily.

The results indicated that legal crossings increased during the enforcement period, more so when police were in sight, but declined to the pre-enforcement percent when no police were present in the observation period after the special enforcement. Post-crossing interviews indicated that most people understood the law, but preferred to rely on their own judgment regarding the movement of vehicles (Weiner, 1968). The effect on actual pedestrian collisions was not measured since too few would be expected in a two-week period for statistical power.

Cameras are also used as an enforcement tool. Cameras that identify the vehicles that run red lights and, separately speeding, result in substantial reductions in proscribed behavior compared to control areas without cameras (Rettig, et al., 1999; Rettig and Farmer, 2003, Newstead and Cameron, 2013). Public opinion polls are mixed regarding public support of use of the cameras making them politically difficult in some areas. In Tucson, AZ the locations of radar vans are announced on morning TV news broadcasts, perhaps diluting their effectiveness.

Cameras are commonly used in certain retail outlets as well as on streets in urban areas. The bombers of the Boston Marathon were repeatedly seen on surveillance cameras, obviously not deterred by the presence of cameras but perhaps unaware of the cameras. Comparison of shootings and auto theft in areas with cameras to randomly selected areas without cameras indicated reductions in both, auto thefts more so than shootings, associated with camera installation (Caplan, et al. 2011). An experiment in which body cameras were worn by police during randomly selected experimental periods found that police use of force was reduced by half when cameras were being worn (Barak, et al, 2014).

One of the threats to valid conclusions in before-after research designs without comparison jurisdictions, particularly in smaller jurisdictions where numbers fluctuate substantially in short periods of time, is so-called regression to the mean (Campbell, 1969). That is, if a new law or increased enforcement is initiated in areas based on a high injury rate in a short period, the rate would be expected to trend toward the average in a subsequent period irrespective of the change in policy. For example, the National Highway Traffic Safety Administration allocated 10 million dollars in grants to states in 1973 for intensified law enforcement based on research indicating a reduction in fatalities after such enforcement in six counties. The counties were selected primarily on the basis of unusually high fatality rates. Using data from counties chosen on the basis of similar criteria in the same and another state, subsequent research showed that similar reductions in fatalities occurred in counties comparably chosen, but without special enforcement (Williams and Robertson, 1975).

Therefore, the reduction in the counties with special enforcement probably occurred because of return to normal from an unusual increase prior to the program, not because of special enforcement. While it makes sense to target efforts at areas where injuries are the most acute, claims of the effectiveness of a law or other injury control effort based on application in areas with unusually high rates prior to implementation is highly suspect. Effectiveness should be studied in areas that have no inordinate rates.

When the law applies to a country as a whole, there may not be another country for comparison nearby that has comparable weather, economic or other factors that could affect changes in injury rates. A research methodology that has been employed to evaluate the effect of laws in that circumstance is the

interrupted time series (Campbell, 1969; Hoff, 1983). If one can obtain data on injury rates for an extended period of time before and after a law change, an abrupt and substantial change in the rates coincident with the change in law, enforcement, or punishment, is usually indicative of the effect of the change in policy, particularly if such abrupt changes have not occurred in the rates in the past.

An attempt to deter overdose of opioid poisons involves state laws establishing drug prescription monitoring systems that link the identities of drug buyers among pharmacies. The information is usually available to state and federal law enforcement authorities as well as physicians upon request and is proactively supplied to authorities in some states. Studies comparing mortality rates from opioid overdose among U.S. states over time found no differences in states that had different or no prescription monitoring systems (Paulozzi, et al., 2011; Li, et al, 2014).

Since 2009, the Florida state legislature enacted several laws aimed at physicians and pain clinics prescribing narcotics, a prescription monitoring program, and tougher penalties for violations (Kuehn, 2014). In 2010, authorities in Florida investigated pain clinics dispensing large numbers of opioid prescriptions, arrested 22 individuals and seized cash and vehicles. Figure 12-2 shows that the increase in opioid related overdoses was reversed, declining 27 percent from 2010 through 2012 (Johnson, et al. 2014).

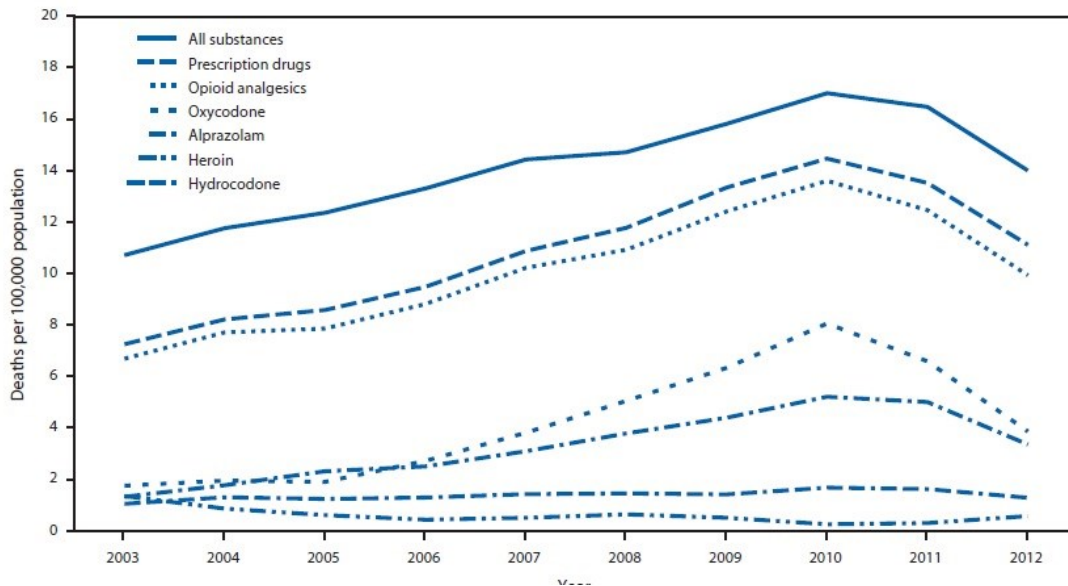


Figure 12-2 Poison Deaths Per 100,000 Population in Florida By Substance

Source:

http://origin.glb.cdc.gov/Mmwr/preview/mmwrhtml/mm6326a3.htm?s_cid=mm6326a3_w

A more detailed study of monthly deaths in Florida showed that an acceleration of the decline in overdoses of oxycodone-caused deaths occurred in October 2011 when the prescription monitoring program was implemented. The study used a sophisticated statistical interrupted time series model that accounted for potential effects of covariates (Delcher, et al., 2015). The Florida experience raises research and enforcement questions: Would publicity about the existence of the prescription drug monitoring program in other states reduce "doctor shopping" in the absence of publicized enforcement of other laws as occurred in Florida (general deterrence)? How can injury control advocates motivate physicians and law enforcement to increase use of the prescription monitoring program (specific deterrence)?

Abrupt changes such as those in Florida are likely because of changes in law or enforcement crackdowns. Sometimes gradual change in injury trends from before to after a change in law or enforcement is attributed to the law, but such inferences are highly questionable without replication in other jurisdictions and times. A trend may change for all sorts of reasons, and a continuation of a previous trend may occur for a reason different from the trend to that point. Lack of a change in trend may mask the effect of an efficacious policy if other factors are changing to offset it. For example, if the rural to urban migration of populations (that is usually accompanied by a downward trend in motor vehicle death rates, due to congestion and lower speeds in urban areas), slows about the time that an effective alcohol countermeasure is introduced, the effect of the alcohol countermeasure could be underestimated unless most of the drivers' blood or breath alcohols are available for analysis.

Both from the scientific and practical points of view, replications of research results are very important. Replication refers to finding a similar result when a study is repeated in a different study population. Particularly when there is no reasonable comparison or control group, replication is needed to assure that change in injuries at a time of change in law, enforcement, or punishment was not for reasons other than the legal effect. In the case of the effect of "crackdowns" on alcohol and driving, interrupted time series in several jurisdictions indicate that a publicized increase in police activity results in a temporary reduction in vehicle-related injuries, the time of the effect varying from a few months to a few years (Ross, 1982).

Most police crackdowns are locally determined and there is little research on why some communities undertake such efforts and others do not. Stimulation of community efforts by grant programs can have an effect, but many communities do not even apply for the funds. In Massachusetts, for example, communities were offered \$70,000 per year for five years to initiate police enforcement and community activities to reduce drunk driving and alcohol use. Thirty responded and six were funded, resulting in reductions in motor vehicle injuries in those six compared to others who applied but were not funded (Hingson, et al., 1996). Dozens of communities did not apply. It would be worthwhile to know why.

Perception of increased probability of arrest often has a deterrent effect, but changes in severity of punishment do not have much if any effect (Ross, 1982; Ross, 1992; Ross and Klette, 1995). The practical implication of the replicated findings is that "crackdowns" that increase the probability of arrest, or at least the perception of increased probability of arrest, will reduce the injury rate for a time, even in countries with somewhat differing cultures and legal traditions. The results leave open questions of how long such "crackdowns" can be maintained in terms of publicity and actual police activity, how often they can be repeated in the same population with the same effect, and whether a long-term change in behavior can be achieved with repetition (Hingson, et al., 1988).

A substantial decline in the proportion of fatally injured drivers with illegal blood alcohol was observed during the late 1980s and early 1990s in the U.S. (National Highway Traffic Safety Administration, 1995), partly due to the reenactment of 21-year-old drinking age laws in several states (Robertson, 1989) and partly due to other laws and enforcement (Zador, et al., 1988). A review of various alcohol countermeasures speculates that they would not have occurred except for a shift in public acceptability of drunk driving spurred by MADD, i.e., Mothers Against Drunk Driving (Hingson and Sleet, 2006). Whether drunk driving was more acceptable before MADD was organized is debatable and impossible to document retrospectively. Certainly MADD and other organizations deserve credit for pushing legislatures to reduce the legal blood alcohol concentration and increasing punishment but severity of punishment is a minor factor compared to other policies.

It should be noted that the 1968 Alcohol and Highway Safety Report (1968) requested by the U.S. Congress was issued more than ten years before MADD's founding and laws against drunk driving date back to 1910. Laws specifying specific alcohol concentrations of blood alcohol were initiated in the 1930s. In 1953, technology was developed for police to measure alcohol in breath at the scene facilitating arrests. Increased taxes on alcohol as well as increased public attention to dieting and the marketing of beverages with less alcohol per volume may have also contributed to the reduction in alcohol-related fatalities, but evaluation of the effect of the latter factors is difficult because of the lack of disaggregated data regarding these factors among drivers in crashes relative to those exposed to the same road and other conditions.

A more complex variation of time-series analysis is the multivariate analysis of the fluctuation of injury rates during a period of time in one or more ecologic units (country, provinces, states, cities) relative to fluctuations in factors thought to affect those rates, including laws or aspects of law enforcement. For example, various changes in laws among U.S. are related to fluctuations in motor vehicle fatalities (Silver, et al., 2013). In addition to the fact that ecological level data do not necessarily reflect the characteristics of the individuals injured (or assailants and drivers in crashes), noted in Chapter 8 (the ecological fallacy), the specification of the statistical models and the inter-correlation of factors at the

ecological level renders the estimates of effects from such analyses highly problematic. These problems have long been recognized in the analysis of economic data for decades (Orcutt, et al., 1968; Leamer, 1983), but economists particularly persist in performing such questionable analyses (Chapter 13).

On the assumption that allocations of budgets for police and equipment are in response to crime as well as possibly having an effect on crime, the regression equations on the time series are sometimes estimated in two or more stages. One review of eleven such studies noted effects on various crimes varying from estimates of substantial effect of police manpower or expenditures, to no effect, to adverse effects (O'Connor and Gilman, 1978). The latter could occur, for example, if the police and certain criminals, or minorities who fear police as much or more than criminals, engaged in an arms race.

The effects of police manpower and police expenditures per capita have been studied with such models and the results vary remarkably depending on the ecologic units used, the assumed causal variables controlled, and the types of outcomes measured. The unit of analysis and the type of crime analyzed may be more or less sensitive to the ecologic fallacy or the direction and degree of effect. Use of smaller jurisdictions, such as cities, narrows the variation in factors such as poverty, concentration of minority populations and other socioeconomic factors. If the increased enforcement merely moved the criminal activity to the suburbs, however, the estimate of effect would be overstated. The latter would not be true if the analysis were confined to domestic violence or crimes of passion, but in the noted studies, these are usually lumped together with all murders and police-reported assaults, or felonies generally. Use of larger ecologic units, such as metropolitan areas or states, might give better estimates of the overall effect, but these estimates might be confounded in different ways by the variation of other factors as well as large differences in concentration of enforcement within smaller segments of such ecologic units.

One of the more controversial issues subjected to a variety of analyses is gun legislation. Gun-related deaths and injuries are rare in most countries where the ownership of guns is prohibited for most people, or tightly controlled (Killas and Markwalder, 2012; Brent et al. 2014). The population rate of all assaults in a region of Denmark in a year was about 75 percent of those in the Northeastern Ohio Trauma Study, but the Danish homicide rate was only 20 percent of that in the Ohio study. The difference in homicide rates is mainly the result of greater gun involvement in assaults in Ohio. Private ownership of guns is allowed only for hunting in Denmark (Baker, 1985).

Even countries that have substantial social and political unrest have lower gun deaths per capita than many cities in the United States. Detroit, Michigan had more gun-related deaths in 1973 than Northern Ireland, with a similar-sized population, during 5.5 years of renewed "troubles" in Northern Ireland in 1969-1974 (Zimring and Hawkins, 1987). Long-term comparisons of trends in violent crime as various countries imposed strict gun control indicates substantial

effectiveness of such laws but the weaker types of such laws typical in the U.S. had no discernible effects (Podell and Archer, 1994). Also, the effects of laws vary among U.S. states. For example, laws regarding child access to guns were found to reduce gun involvement in child deaths in one state but not several others (Webster and Starnes, 2000).

The effects of new gun laws in the United States are limited by the pre-law prevalence of gun ownership, and the effects of extant state and local laws are tempered by the movement of guns from jurisdictions with few restrictions to those with more strict laws. Using an index of strictness of combinations of various types of laws, comparisons of jurisdictions found gun-related deaths lower in jurisdictions with stricter gun laws, and particularly where there is licensure of dealers, licensure by owners, and a waiting period between sale and delivery (Geisel, et al., 1969). As indicated later in this chapter, when persons in the community other than police augment enforcement, in this case dealers, laws tend to be more effective. The argument against studies showing a lower gun death rate related to gun control laws is: "You didn't control for X."

For example, other researchers found that when the region of the country is controlled statistically, the correlation of the gun strictness index and gun death rate is no longer statistically significant. This was interpreted as demonstration of a spurious correlation, i.e., gun death rates were said to be the result of the "frontier culture" of the south and southwest rather than gun control laws, which are more prevalent in the north and east (Magaddino and Medoff, 1984). Since no one has attempted to measure "frontier culture", that is a dubious interpretation. For example, in Texas, a state that has been stereotyped as having a "frontier culture", Dallas and Houston had the highest seat belt use in response to law, more than 15 percentage points higher than the highest use in 17 cities in other states (Goryl and Bowman, 1987). That hardly seems the manifestation of a "frontier culture" thumbing its nose at laws.

The effect of gun legislation on suicide rates is less controversial. Researchers who have questioned the effect on homicide rates found that suicide rates are substantially lower in areas with stricter gun laws (Medoff and Magaddino, 1983).

While gun ownership is curtailed in many countries, guns remain a major source of morbidity and mortality in the U.S. and other countries where gun ownership is relatively unrestricted. Although the vast majority of the U.S. public supports background checks to qualify for gun purchase, the "gun lobby" is strongly influential in state and federal legislatures and has blocked legislation in most jurisdictions. Despite increased frequency of mass shootings in schools, malls, theaters and workplaces, efforts to enact background check laws often fail. Research indicates that background checks on persons trying to buy guns legally at retail dealers and gun shows would reduce firearm related crimes about 25 percent (Wintemute, 2013). That and research on other approaches to reduction

of gun violence were presented at a conference at Johns Hopkins University in 2013 (Webster and Vernick, 2013).

Almost all of the studies of gun laws are of aggregated data. It would be useful to compare individual level data on homicides and suicides with guns in jurisdictions with different gun laws regarding such factors as: How old was the gun? Was the gun legally or illegally possessed? How was the gun obtained? What was the history of the gun (e.g., a gun possessed illegally where the death occurred but purchased legally elsewhere)?

Based on aggregated data among states, one study claimed that laws granting the right-to-carry concealed weapons deter crime (Lott and Mustard, 1997). The hypothesis was that criminals knowing of the law would be deterred by the possibility that potential victims would have a concealed weapon. Of course, people often lie about what they are thinking and aggregated crime data certainly cannot reveal what anyone, potential criminal or otherwise, is thinking. When the minimum age for permits was factored into the analysis, an increase in homicides was associated with right-to-carry laws (Ludwig, 1998). This is consistent with the hypothesis that many assaults are thoughtless results of failure to manage anger or other impulses, often while inebriated. A meta-analysis of homicides found that the killer was inebriated or on drugs in about half the cases (Kuhns, et al., 2014). The debate regarding concealed weapons continued but the brunt of the evidence supports an increase rather than a decrease in homicides associated with concealed handgun carry permits (Ayres and Donohue, 2009).

Studies of severity of punishment of those who use guns in crimes have been limited. There is some evidence that a mandatory sentence for possession of a firearm during various crimes reduces homicides from firearms. Comparison of trends in percent of homicides and suicides from firearms in New Jersey relative to the remainder of the U.S. before and after a mandatory sentencing law in 1981 indicate a substantial reduction in gun involvement in homicides but not suicides (Fife and Abrams, 1989). Since New Jersey had relatively strict gun laws prior to the mandatory sentencing law (Geisel, et al., 1969), the results may not be generalized to states without such combinations of laws.

A meta-analysis of the effects of programs and policies to reduce gun violence noted that claimed effectiveness or lack thereof is related to the quality of the research designs of the studies reviewed (Makarios and Pratt, 2012). Generally, the studies find that gun buyback programs are ineffective and that increased police enforcement of extant laws is most effective.

The violence of domestic terrorist and racist groups, such as self-styled “patriot militias”, has been reduced by successful lawsuits that distributed their assets to their victims (Dees, 2001). Activities of such groups are monitored by the Southern Poverty Law Center, data from which might be useful to researchers (SPLC, 2015).

The augmentation of law enforcement by persons other than police in the community is probably a substantial factor in the effect of several laws known to reduce injuries. Examples are laws that prohibit driving by 16-year olds after a certain time in the evening. Since the police cannot easily see drivers at night, much less make a judgment regarding their ages; it is unlikely that arrest activity by police is effective in enforcement of such curfews. Yet a comparison of matched states with and without the laws indicates 25-62 percent reductions in crashes of 16-year-old drivers during the curfew hours (Preusser, et al., 1984). A comparison of 47 cities with curfews for teenagers and 77 cities without curfews indicates a 23 percent reduction in motor vehicle deaths among 13-17 year olds (Preusser, et al., 1993). It is likely that many parents enforce the law by requiring their youngsters to be home during the hours that the curfew is in effect. Studies of the effect of curfews on injuries other than by motor vehicle find little effect (e.g., Moscovitz, et al. (2000).

Several states have adopted "provisional licenses" or "graduated licenses" for teenaged drivers, the latter based on demonstrated effectiveness of such laws in a few states and other countries. Comparison of teenage driver involvement in fatalities among the states found that reductions were associated with the stronger laws (Li-Hui, et al., 2006; Morrisey, et al., 2006). Laws specifying 18 as the minimum age for school dropouts is associated with a decline of 17 percent in arrests for violent and other crimes of those younger. The author recognized that arrests are an imperfect indication of actual crimes but the results are plausible (Anderson, 2014).

Persons 85 years of age and older are about 31 percent less likely to be drivers in fatal crashes in states where they are required to show up at the Department of Motor Vehicles to renew their licenses (Tefft, 2014). Several other requirements – length of renewal period, a driving test or a knowledge test – were not associated with fatal crash involvement of the elderly. Since this was an ecological study, it did not specify the reason for the effect but knowledge of the impaired aging drivers that impairments will be obvious to the licensing personnel is likely.

When changes occur simultaneously that could affect injury rates, then specification of changes in the behavior and specification of the injuries that should be affected by a law enacted at that time is essential for confidence in the research results. An example of multiple changing events occurred during the oil boycott that began near the end of 1972. The lack of gasoline and increased prices of fuel resulted in reduced driving; economic recession occurred; and the 55-mile-per-hour (mph) speed limit was adopted on roads that had a higher limit, primarily to reduce fuel use, all in a matter of months. In the United States, there was a very notable reduction of about 8,000 motor vehicle-related deaths per year in 1974-75 compared to the average in the five immediately prior years (interrupted time series), but the amount of the reduction that could be attributed to the reduced legal speed limit rather than the other factors was widely argued.

To estimate the effect of the speed limit, researchers noted that average speeds declined, particularly on rural interstates more than other roads. The percentage of drivers exceeding 65 miles per hour decreased from 59 percent in 1973 to 10 percent in 1974 and those exceeding 55 miles per hour declined from 93 percent in 1973 to 68 percent in 1974. The greatest reduction in deaths per miles traveled occurred on interstate and federal-aid primary roads -- those where the speed limit was reduced. On the basis of reductions per miles traveled on relevant roads, about half of the total death reduction was attributed to the 55 mph limit (Kemper and Byington, 1977).

Eventually, due to an equivocal report from the Transportation Research Board (Altshuler, et al., 1984) and an anti-regulation Congress, the speed limits were raised. Deaths on rural Interstate Highways, where the limits were first raised, increased about 17% (Farmer, et al., 1999). This is not as much as expected from the earlier research but more crash worthy vehicles were introduced in the interim (Chapter 13), perhaps moderating the effect.

Occasionally a situation arises that allows the researcher to obtain data regarding a hypothesis that is ordinarily difficult to test. For example, a police strike in Finland provided evidence of the effect on vehicle speeds of public knowledge that police would not be on the road (Summala, et al., 1980). The average speed increased only slightly but the standard deviation increased about 20 percent. Apparently, some drivers drove faster but were almost matched by those who drove more slowly, perhaps for fear of other drivers in a period of no enforcement. Unfortunately, crash data were not included, perhaps because the strike lasted only two weeks and the numbers would have been too small for statistical power.

Generally, the effects of laws on behavior and the evaluation of the effects are enhanced if the behavior is easily observable. If the researcher can observe the behavior, so can the police. It is not surprising, therefore, that laws requiring observable behavior, such as limits on vehicle speed, child restraint use, seat belt use, and motorcycle helmet use are usually more effective than laws directed at phenomena not observable without stopping persons -- such as limits on blood alcohol of drivers or the carrying of concealed weapons (Robertson, 1983). There is also the synergistic effect of laws regarding observable behavior leading to arrests for violation of laws that proscribe less evident behavior. Hingson and Sleet (2006) make the point that increased alcohol enforcement was probably enhanced by belt use laws. Given the correlation of driver's blood alcohol and non-use of belts (Appendix 12-1), that is quite plausible to the extent that belt use laws are enforced.

Required Protective Behavior. To the extent that child restraint use, seat belt use, or whatever protective behavior, is the result of psychological, social or cultural factors that preclude or override concerns about detection or punishment, the effect of protective laws and their enforcement will be

diminished. In some instances, the effects of laws may be diminished because people are exempted for reasons of religion or custom. For example, Sikhs in India and Great Britain are exempted from laws requiring helmet use on motorcycles because the turbans required by their religion do not accommodate commercially available helmets (Berger and Mohan, 1996). Turbans provide some protection but are not as effective as helmets (Sood, 1988). Laws are seldom uniformly adopted among states of the U.S., and some have not been retained once enacted, such as motorcycle helmet use laws. Although hypotheses regarding the social and political processes that lead to enactment or resistance to laws by legislators may not be viewed by epidemiologists as within their purview, those with backgrounds in sociology and political science may find the subject of interest.

One question that has been little explored is: what is the origin and sustenance of the lobbying in opposition to laws to reduce injury? No doubt many of the opponents are sincerely concerned that their rights or economic interests are at stake, but there may be a more cynical element. Collection of dues, subscriptions to magazines and newsletters, and sale of other propaganda can be very profitable. Of course, proponents of laws can be exploited in a similar manner. Some of the lobbyists, or magazine and newsletter publishers, may care little or nothing about the issue in question, but use it to enrich themselves by continuously stirring up opposition with claims that the rights of whatever group (gun owners, motorcyclists) are being violated. For example, the National Rifle Association and other gun oriented organizations display repeated paranoia when any form of regulation of guns is proposed. They claim to be protecting freedom but when gun dealers offer for sale so-called "smart" guns that have technology allowing only the owner to fire them, the alleged freedom-loving gun organizations threaten to put the dealer out of business. (<http://security-today.com/blogs/reaction/2014/05/nra-looks-to-ban-smart-guns.aspx>).

Most laws applied to individual behavior in the U.S. are enacted in state and local jurisdictions. While this is disadvantageous in obtaining the uniform application of laws of known efficacy, it gives the researcher an opportunity to study the effect of laws of unknown or questionable efficacy in jurisdictions that introduce them relative to those that do not. Evaluation of the effect of enactment of laws that were in effect before data systems on relevant injuries were initiated are not possible, although the waxing and waning of enforcement may provide opportunities for study.

Other jurisdictional comparisons have also suggested effectiveness of laws related to risks other than assaults and motor vehicles. Drowning of children in swimming pools were 65 percent less frequent in Honolulu, where pool fencing was required, than in Brisbane, Australia with no requirement. The two cities had similar weather and pool-to-household ratios (Pearn, et al. 1979). Comparison of adjacent affluent counties in Maryland and Virginia, with and

without requirements for household smoke detectors, found 25 percent fewer deaths in house fires where smoke detectors were required (McLoughlin, et al., 1985).

In studies of effects of laws and rules, researchers measure either the proscribed or required behavior, or the incidence and severity of injuries that supposedly affected by the behaviors. Preferably, both should be measured. For example, research on the effect of motorcycle helmet use included observations of helmet use by motorcyclists at selected sites in states with and without helmet use laws, as well as fatal injuries to motorcyclists in states with and without the laws before and after the laws were enforced. Since helmet use was almost universal in the states with laws requiring use, there was no question about differential use by those at differential risk after the law (Robertson, 1976, Appendix 12-1).

Just because a law results in a change in the behavior required does not mean that the law is having proportional success in injury reduction. If compliance is not universal and those who comply with the law are more or less at risk, the ultimate effect of the law on injury will not be proportional to the technical effectiveness of the required behavior. That has apparently been the case with seat belt use laws.

The expected percent change in injury rate as a result of a change in behavior is:

$$P = 100 \frac{E(B - A)}{1 - BE}$$

where

P = expected percentage change in injury rate

B = proportion behaving in the prescribed way before the change

A = proportion behaving in the prescribed way after the change

E = the technical effectiveness of the behavioral change (Robertson, 1983b)

In Australia, where the first belt use law in a large jurisdiction (the state of Victoria) was enacted, belt use increased from 20 to 70 percent from before to after the law. If the one-piece lap and shoulder belts used in Australia were 60 percent effective in reducing death, as claimed at the time, when used, $E = 0.60$, $B = 0.2$ and $A = 0.7$. Therefore, the reduction in deaths should have been 35 percent. It was actually 10 percent in rural areas and 20 percent in urban areas, based on comparison of changes in death rates in Victoria relative to Australian states without the law at the time (Foldvary and Lane, 1974). Even if belts, when worn, were 45 percent effective in reducing deaths, i.e., $E = .45$, the reduction should have been 25 percent without selective use by those at less risk. Estimates of the effects of belt laws in the U.S. are also not compatible with 60 percent belt

effectiveness when used (Cohen and Einav, 2003). See Appendix 5-1 for a critique of studies claiming 60-65 percent belt effectiveness when used.

There is a hypothesis that nonusers of belts are more likely to crash at higher speeds, resulting in an overestimate of the effects of belts. Tests of the hypothesis, however, rely on data that is inadequate to support or reject the hypothesis. For example, Evans (1996) compared "delta-v" in crashes, an estimate of velocity change in the crash, from the National Automotive Sampling System (NASS) during 1982-1991, between belted and unbelted drivers. Delta-v was missing in 60 percent or more of cases in each of those years and the percent missing among the injury severity codes are substantially different. Competent epidemiologists do not rely on data with such huge potential for bias in the selection of vehicles to measure delta-v based on the outcome in injury severity (Farmer, 2006). Evans also used a data set from North Carolina that measured injury by the police codes, combining A+K, which is biased by the overwhelming number of A injuries that are relatively trivial (Chapter 7).

Seat belt use in response to law varies by whether the law provides for citation by police for nonuse of belts exclusively (primary law) or only if drivers first commit some other violation (secondary law). Figure 12-3 shows the difference in observed seat belt use in primary and secondary enforcement states in the U.S. during 2012 and 2013. About two-thirds of U.S. states have primary enforcement laws. Given the correlation of nonuse of belts and driving while intoxicated, nonuse of belts could be considered "reasonable suspicion" for an alcohol breath test. A police officer must have "reasonable suspicion" that a crime has been committed to stop a motorist which, in the case of driving while intoxicated, means illegal or unusual driving behaviors.

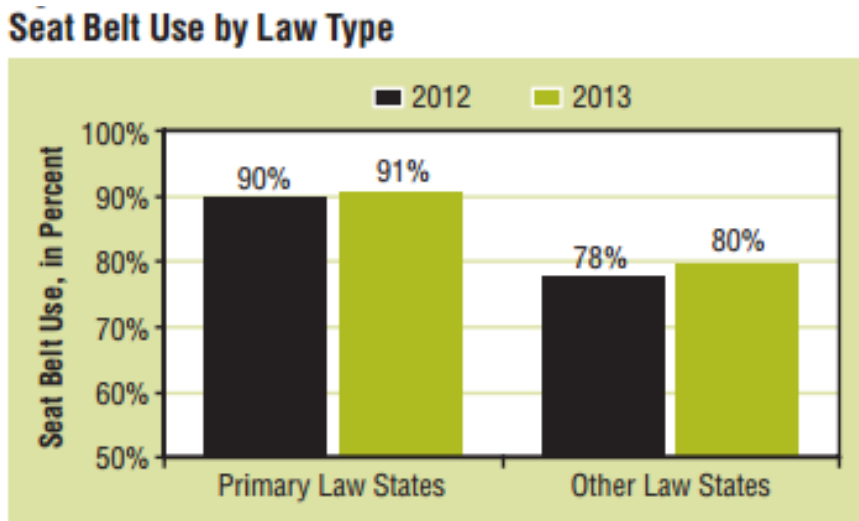


Figure 12-3. Observed Seat Belt Use in States with Primary and Secondary Enforcement.

Source: <http://www-nrd.nhtsa.dot.gov/Pubs/811875.pdf>

UNINTENDED CONSEQUENCES. Generally laws that apply to publicly observable behaviors have proved more effective than non-legal persuasive approaches (e.g., Mock, et al., 1995). Few studies have paid attention to the possibility that laws requiring or proscribing a particular behavior could result in alternative behaviors that could place the individuals affected at more or less risk.

In Japan, comparison of child injuries and deaths before and after a child restraint use law claimed an increase in the child occupant death rate (Desapriya, et al., 2004). The authors of the study compared that rate with the pedestrian rate, supposedly as a control for general trends in death and injury on the road. The pedestrian rate declined. They suggested misuse of child restraints as an explanation of the unexpected finding but the more likely explanation is that the study was done when people were reducing their walking with children and increasing travel by motor vehicle. Although the study included vehicle miles traveled, it did not include number of passengers per vehicle. Without data, we do not know whether there was an increase in transportation of children in motor vehicles and, if so, whether the increase was because parents believed they were safer in child seats. Given the known effectiveness of child seats, it is not plausible that an increase in their use increased child deaths in vehicles.

Laws that proscribe participation in certain activities also raise the issue of the extent to which the alternative activities are more or less dangerous. Numerous studies were done of the effects of the minimum legal drinking age on involvement of teenagers as drivers in fatal crashes without consideration of potential effects on other injuries. Although the estimates of effects on vehicle crashes were somewhat biased toward underestimate of the effect of the laws because proxy measures for alcohol involvement (night and single-vehicle crashes) were not as indicative of alcohol in the younger drivers (Robertson, 1990), the studies usually found reduced fatal motor vehicle crashes of the drivers in the age groups affected by the laws (General Accounting Office, 1987).

These studies left open the question of what teenagers who were drinking in bars or other settings, where drinking was legal, would have been doing had they not been there. Not only did no one consider that those drinking legally were placed at risk of other injuries related to alcohol (e.g., assaults in bars), no one considered that if they could not drink, they might be doing something even more dangerous.

Finally, one research project, based on one state that changed its legal drinking age compared to one that did not, found no statistically significant effect of changing the drinking age on non-motor-vehicle-injury mortality (Hingson, et al., 1985), but the statistical power was limited by the small sample. A larger study, based on all the states, estimated the effect of the legal minimum drinking age on all fatal injuries to persons 16 to 24 years of age, and found a net beneficial effect on all injuries (Jones, et al., 1991). Several potential effects were considered.

Did the law affect injuries to persons younger than the drinking age, presumably because alcohol may be more easily obtained if adjacent aged peers can drink legally? Was there a spurt in injuries at the age that drinking became legal -- the initiation effect? Did injuries decline as persons aged beyond the drinking age -- the experience effect? The data indicated that, for most types of injuries, the higher the legal minimum drinking age, the lower the injury death rates in correlation with the number of years between the individuals' ages and the legal age. The hypothesized initiation effect was found only for homicides, and increased rather than decreased injury death rates occurred in correlation with presumed drinking experience (age at death minus legal drinking age).

Laws that prohibit alcohol sales in a limited geographic area have the potential to increase the risk of injury if the law results in driving or walking by intoxicated persons returning to the "dry" area from the "wet" area. During a brief period of legal alcohol use on an American Indian reservation, the arrests of reservation residents for intoxication in the neighboring county were reduced about one-third with no increase in such arrests on the reservation (May, 1975). Since arrests do not necessarily reflect alcohol use, or the other behaviors associated with its use, the data are inadequate for a strong conclusion, but the issue is worthy of research. One study comparing retail availability of alcohol among states found no correlation between "single-vehicle" fatal crashes at night and alcohol retail outlets when other factors were taken into account (Gruenewald and Ponicki, 1995).

Comprehensive study of a variety of laws among U.S. states found most alcohol laws have some effect (Villaveces, et al., 2003). The imputation of alcohol in drivers in which alcohol was not measured weakens confidence in some of the results.

Following the OPEC oil boycott in the mid-1970s, most states in the United States adopted a law allowing right-turn-on-red at signalized intersections after stopping to assure that the turn could be made safely, unless a particular intersection was specified otherwise. The purpose of the law was to conserve fuel that was being burned during the waits for lights to change. However, observations of driver behavior indicated that the number of drivers who turned right at signalized intersections without stopping increased from 47 percent to 70 percent in one state (Baumgaertner, 1981). The unintended effect was an increase in total crashes and, particularly, injuries to pedestrians and bicyclists (Zador, 1984).

A theory of unintended consequences says that drivers whose crash protection is increased will drive more riskily and endanger other road users -- so-called risk compensation or risk homeostasis theory. Economists claimed to originate this theory in the 1970s (Lave and Weber, 1970; Peltzman, 1975), but it was used to oppose the requirement of safety equipment on trains in the Nineteenth Century (Adams, 1879). The typical study claiming to support the theory employs an ecological design: cross-sectional or time-series data on fatality rates

correlated to aggregated data on seat-belt use or regulation of vehicle characteristics and other factors. The latter studies will be discussed in Chapter 13.

Among the problems in such studies is that the drivers who were more protected were not disaggregated from those less protected and motorcyclists were included as "pedestrians". For example, one study attempted to correlate occupant and other road user fatalities among states to belt use and factors such as vehicle miles driven, percent urban population, average speed on rural roads, alcohol sales per capita, percent youth in the population and income per capita. States with higher belt use, according to the behavioral risk factor survey of the Centers for Disease Control, had higher non-occupant fatalities, controlling statistically for the other factors (Garbacz, 1990).

In the study, "non-occupants" included all motorcyclists, many of whom were killed in single-vehicle crashes in which other drivers, belted or not, were not involved. As noted in Chapter 7, the behavioral risk factor survey on belt use is not valid. Numerous differences among the states that could be confounded with belt use and fatal injuries were not considered, such as child restraint laws, motorcycle helmet use laws, and vehicle mix (large and small cars, tractor-trailer trucks, pickups, SUVs, as well as motorcycles).

The studies that have examined disaggregated data on the effects of belt use laws on behavior of drivers, or belt use in crashes, have not found evidence of risk compensation. Several important driving behaviors were observed on the road before and after the belt use law was enforced in Newfoundland, and in Nova Scotia during the same period without a law. Belt use increased from 16 percent to 77 percent in Newfoundland and remained virtually unchanged in Nova Scotia. Four driver behaviors (speed, stopping at intersections when the control light was amber, turning left in front of oncoming traffic, and gaps in following distance) were measured at various sites before and after the law. Changes in these behaviors in Newfoundland were similar to those in Nova Scotia, except that drivers in Newfoundland drove slower on expressways after the law, contrary to the theory (Lund and Zador, 1984). Study of changes in belt use among states of the U.S. found no effect on non-occupant fatalities (Cohen and Einav, 2003). I have found no study of appropriately disaggregated data that finds an effect on injury to other road users of increased driver belt use.

Detailed data from Suffolk County, New York on all motor-vehicle-related injuries (including non-occupants) seen in emergency rooms (or that were fatal), before and during the first two quarters of the seat belt use law in New York, indicate a substantial reduction in severe injuries. Total injury cases decreased slightly in the first quarter and increased slightly in the second quarter, compared to the same periods in the year before, but larger increases were expected based on trends in Rhode Island without a law (Barancik, et al., 1988; Rockett, et al., 1988). In Suffolk County, the fatalities per emergency room case declined 20 percent in the first six months of the belt use law compared to the

same period a year earlier while in Rhode Island, without a law, the fatalities per case increased 24 percent. The economist (Garbacz, 1990) who claimed that risk compensation behavior offsets the effects of belts did not mention these studies or the previously mentioned studies of the effects of belt use laws.

Changes in laws can have unintended consequences if they result in substitution of a less effective countermeasure for one having some effect. In Nassau County, New York, drivers convicted of driving while intoxicated were assigned randomly to the usual sentence or an education-rehabilitation program. The drivers who had the latter program had worse subsequent driving records than those who received the usual sentences (Preusser, et al., 1976). Contrary findings occurred in an experiment in two Canadian cities. When "accidental" and violent injuries were combined, convicted drunk drivers who received rehabilitation were less likely to die in the subsequent decade (Mann, et al., 1994).

The effect of law on economic incentives or disincentives may also have unintended consequences. In Quebec, Canada, the government did not subsidize driver education as was done in the United States, but required it for licensure before age 18 at a cost to the potential licensee of about \$200. In 1983, the law was amended to require driver education of all newly licensed drivers. Since, thereafter, drivers could not avoid the cost of driver education whatever their ages, the economic incentive to delay licensure to age 18 was removed. Licensure of 16-17 year-olds increased 12 percent among males and 19 percent among females in the year after the law was changed (Potvin, et al., 1988).

Laws and rules regarding work practices have unintended consequences that could be changed to reduce injuries. Comparison of fatalities in the periods around the one-hour change from daylight savings and standard time indicate that some 900 fatal crashes could be avoided annually if daylight savings time were retained year round (Ferguson, et al., 1995). The self-reported crash rate of hospital nurses working in rotating shifts is twice that of those working fixed shifts (Gold, et al., 1992).

Unintended consequences of changes in law are not always adverse to injury control. The death reduction attributed to the 55 mph speed limit is an example. Required deposits on cans and bottles for various drinks and other uses have been required in various jurisdictions to reduce cleanup costs and for conservation. In Maryland, the adoption of such legislation was associated with a 60 percent reduction of glass-related lacerations to children (Baker, et al., 1986).

Replicated quasi-experiments of the effect of alcohol taxes on alcohol consumption and related fatalities indicate substantial effects (e.g., Wagenaar, et al., 2015). Increase in the cost of alcoholic beverages is associated with a reduction in motor-vehicle fatalities as well as mortality due to cirrhosis and violent crime (Cook, 1981, Cook and Durrance, 2013). Replication of that research is needed on the effects of alcohol taxes on other injuries, including suicide.

Appendix 12-1. The effect of motorcycle helmet use laws on behavior and fatalities.

To estimate the effectiveness of a law directed at individual behavior to reduce injury, measurement of the behavior and the injuries is desirable. I was able to accomplish both in a study of the effects of motorcyclist helmet use laws (Robertson, 1976). The data on observed use were collected only after the laws were enforced but data on fatalities before and after the laws were available from the Fatality Analysis Reporting System.

Motorcyclists were observed while riding on roads in Georgia and Maryland when those states had laws and in California and Illinois that did not have laws during the same time period. All but one of nearly 1000 motorcyclists observed in the states with laws was wearing a helmet. Compliance with the law was almost universal. Voluntary helmet use in non-law states varied widely - 61 percent of 1338 motorcyclists in California and 25 percent of 504 seen in Illinois.

To compare fatality rates, 8 states that enacted laws were matched to adjacent states that did not enact laws during the same period. Motorcyclist's deaths were counted in each state for three years - the year before the law was enforced, the year that enforcement began and the year after. The number of registered motorcycles in each state for the years studied was obtained from annually published data (Federal Highway Administration 1974). Fatal motorcycle crashes in each year in each state was divided by the number of registered motorcycles and multiplied by 10,000 to get a rate per 10,000 motorcycles. The deaths per 10,000 registered motorcycles declined about a third in states that enforced helmet laws compared to the comparison states (Figure 12-3).

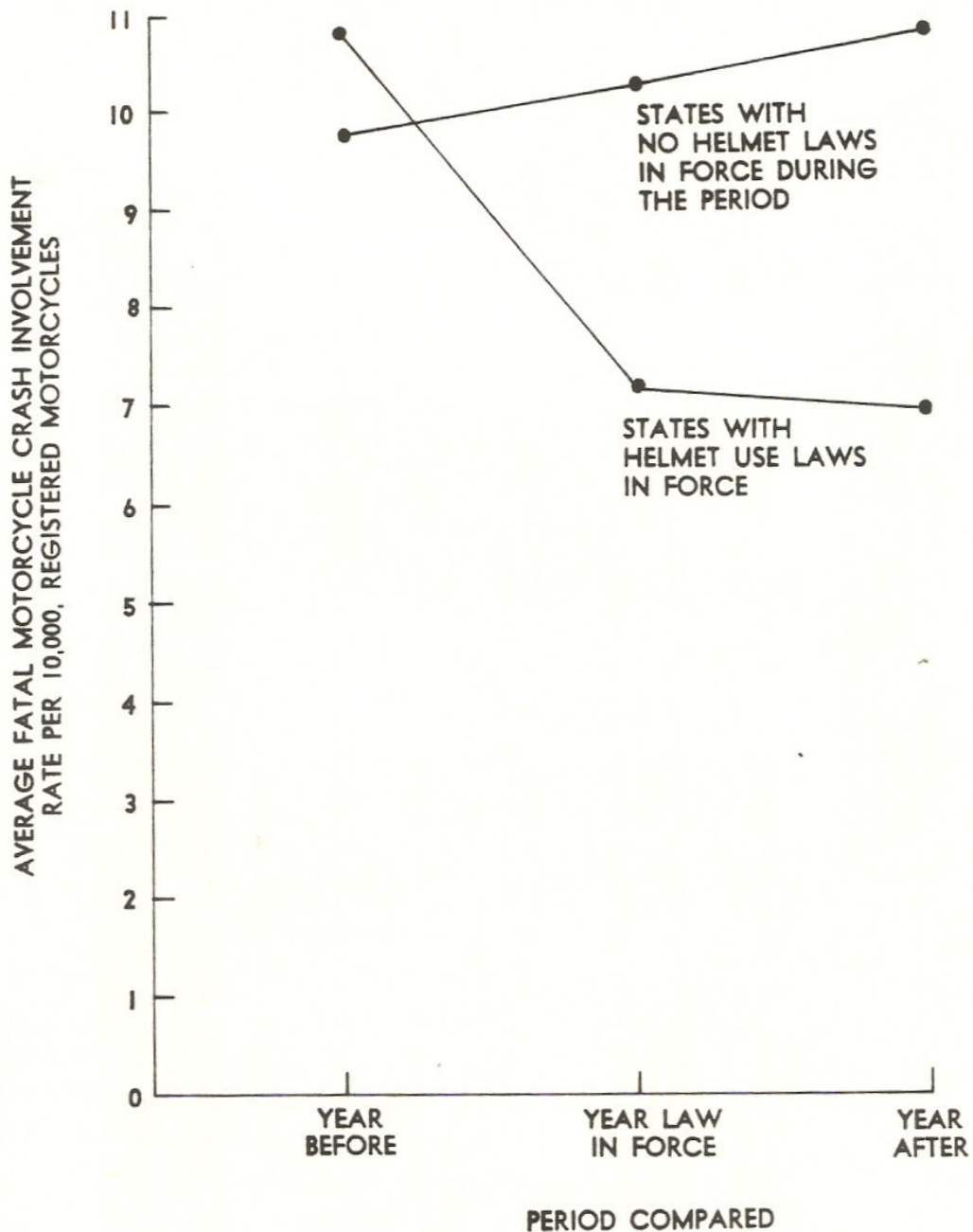


Figure 12-3 Fatal Motorcycle Crashes Before and After helmet Use Enforcement in States that Enforced Laws Regarding Helmet Use and Matched States That Did Not Enforce Helmet Laws During the Same Periods

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