Injury Epidemiology: Fourth Edition

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Chapter 8. THE USE AND ABUSE OF CAUSAL ANALYSIS

On an exam in a course on the use of epidemiology for injury control, I asked the students, "What is wrong with this statement: 'Alcohol is consistently associated with unintentional injury deaths, therefore alcohol abuse must be reduced to lower rates of death and disability due to injury?'". Only one in twenty came close to writing a correct answer. Yet in presentations during the course, numerous instances to the contrary were noted. Motor vehicle injuries, for example, have been reduced by improved vehicle crashworthiness, seat belt, and child restraint use laws, lighting dark sections of roads, and channeling traffic, without reducing alcohol or otherwise changing drivers.

In explaining the answer to the students, I rewrote the statement: "Gender is consistently related to injury; therefore gender must be changed to reduce injury". The nervous laughter, particularly from the gender often at higher risk (males), suggests that some might have got the point.

The statement about alcohol on the exam was not original. Prominent leaders in public health published the statement (Brown, et al., 1990). There is an assumption inherent in much public health literature that the specification of a complex interaction of factors -- sometimes called causal webs -- is necessary for injury and disease reduction (Krieger, 1994). On the contrary, while the notion of the causal web may be useful to call attention to the complexities of multiple causes of diseases and injuries, it may disable our minds from thinking about prevention (Renwick, 1973).

The "public health model" is usually presented as follows: 1. identify the problem (surveillance), 2. identify risk factors, 3. develop and test interventions, 4. implement interventions and measure effectiveness (e.g., Powell, et al., 1996). Many interventions and therapies have been successful without the second step. One does not need to know the "risk factors" for a headache to use an analgesic. Focusing on multiple "risk factors" in causal webs may lead astray rather than toward injury control.

Consider the New York Health Department's study of children's fatal falls that resulted in barriers over windows in high-rise buildings and a huge reduction in child deaths (Chapter 7). What would have happened if the researchers had attempted an analysis of numerous "risk factors" that could contribute to children falling from windows, such as no adult present, intoxicated adults, numerous factors that distracted adults, and children's inquisitiveness, hyperactivity, or numerous other characteristics? Would there have been attempts to change those factors rather than install window barriers? If so, it is unlikely that the falls would have declined nearly to the extent produced by the window barriers.

Multiple causations of disease and injury, for which the causal web is the most common metaphor, are repeatedly demonstrated. It does not logically follow, however, that multi-factorial theory or analysis leads to rational prevention policy. This was recognized in the original conceptualization of the "causal web" (McMahon, et al., 1960), but it is commonly forgotten. Those of us who study injuries in the U.S. are not fond of metaphors regarding guns, but there were instances in which "magic bullets" were found despite multiple risk factors for infectious diseases (Evans, 1993).

If we have to deal with causal webs, perhaps we need a metaphor for something more effective than bullets against webs and spiders -- like brooms. To Krieger's (1994) provocative question regarding causal webs: "Has anyone seen the spider?" should be added, "Has anyone seen the housekeepers with the preventive brooms?"

Proponents of the search for complex causes before consideration of preventive action argue that, without the understanding of causes, prevention may fail because of confounding factors or may have unintended consequences. Confounding refers to the attribution of risk to a factor that has little or no effect but is mistakenly thought to have an effect because it is correlated to an important factor.

In rare instances, confounding has misled preventive efforts, and adverse "side effects" have also occurred, but the nature of certain types of causes is such that the potential for unintended consequences is minimized. Complete understanding is an elusive goal that is yet to be accomplished for most phenomena. How much, then, do we need to know for injury control?

To eliminate an injury or anything else that is undesirable, one need only find a controllable necessary condition for the outcome and control that condition. Even the least resilient of human anatomies can tolerate some mechanical energy load (commonly measured in pounds per square inch or kilograms per centimeter). In the road environment, any combination of energy management by vehicle components or the surrounding environment that keeps loads below that tolerance will eliminate injury. While energy management to that extent has not been accomplished, and may not be feasible in the extreme for economic or other reasons, a substantial reduction in motor vehicle death rates occurred due to increased vehicle crashworthiness, increased visibility of side running lights, and high-mounted brake lights, and automatic corrective action by electronic stability control. There are claims of unintended consequences in increased risky driving by those protected, but these claims are not supported by better research (Chapter 13, Appendix 13-1).

Causal analysis of injury can inform preventive approaches when it specifies substantially changeable factors that account for a proportion of a given type of injury and rules out factors that are spuriously correlated to injury. Attention to the nature of types of causes gives guidance to the extent that a given injury might be reduced if a given factor were changed.

In examining data on potential causes of injuries, it is useful to bear in mind the types of statistical distributions of injuries that are found when conditions are necessary, sufficient, or contributing factors. Examples are shown in Tables 8-1 through 8-3 where the designation of “none”, "some" and “more” cases refers to a proportion of all cases in the table.

Table 8-1. Distribution of Observations When A Is a Necessary Condition for B

	B	Not B
A	Some	Some
Not A	None	Some

Table 8-2. Distribution of Observations When A Is a Sufficient Condition for B

	B	Not B
A	Some	None
Not A	Some	Some

Table 8-3. Distribution of Observations When A Increases the Probability of B

	B	Not B
A	More	Some
Not A	Some	More

If A is a necessary condition for B, then B will not occur in the absence of A, and the joint distribution of A and B will look like that in Table 8-1 when the measurement of both is accurate. For example, keeping the height of playground equipment below the level that can produce enough energy in a fall to injure the heaviest users will eliminate such injury (Chapter 2).

If A is sufficient for B, then B will always occur in the presence of A, but the absence of A does not imply the absence of B unless A is also necessary for B (Table 8-2). A competently used guillotine will behead anyone placed under it, but all beheadings are not by guillotine. Occupants of cars that run under trailers of tractor-trailer rigs are sometimes beheaded.

As seen in Table 8-3, if the lower left and upper right cells were both zero, factor A would be both necessary and sufficient for B, but such effects are very rare. In Table 8-3 as shown, A increases the probability of B, and more cases of B are found in the presence of A, but A is neither necessary nor sufficient for B. Some people who drink and drive are not in crashes and many drivers in crashes have not consumed alcohol so alcohol is a contributing factor, neither necessary nor sufficient to cause a crash.

Since prevention of a necessary condition for harm completely reduces the harm, the more closely the distribution of injuries of a given type and severity (B) with the presence of some factor (A) resembles the distribution in Table 8-1, the greater the number of such injuries that will be prevented if A is eliminated.

CRITERIA FOR CAUSATION. The old Henle-Koch criteria for attributing causes to infectious diseases required that a microorganism or other factor be found both necessary and sufficient to produce a given infectious disease, but clearly, those criteria were too stringent. Many microorganisms were found necessary but not sufficient for the infection to occur (Kelsey, et al., 1986). Nevertheless, by controlling the microorganism, its access to the host, or host resistance, the disease could be controlled.

Similarly, as noted in Chapter 2, energy exchange with the human organism is a necessary and specific condition for injury, but the degree of the energy exchange necessary in individual cases may vary by the nature of the tissues affected, which makes them more or less tolerant of energy insults. Nevertheless, if the energy exchange can be kept below the tolerance of the most vulnerable tissue, an injury will not occur. Where it is not possible to reduce energy exchanges to that degree, it is nevertheless possible to greatly reduce severity by reducing energy exchanges.

Several criteria must be met to make a strong inference of causation. First, the cause must precede the effect in time. For example, a study of alcohol measured in the breath of emergency room patients found that some claimed to have consumed alcohol after the injury (Wechsler, et al., 1969). In those cases, if true, alcohol could have not contributed to the incidence, although it might affect recovery.

Second, the hypothesized cause must be correlated to the effect, that is, they must have joint distributions similar to one of those in Tables 8-1 through 8-3, or in the case of non-categorical variables such as blood alcohol concentration, there should be a dose-response correlation. The cliché among some scientists that "correlation doesn't mean causation" is not precise. Correlation is a necessary but not sufficient condition for inference of causation.

The absence of correlation does not totally rule out causation if measurement is unreliable, biased, invalid, or other factors intervene in such a way as to mask the correlation. Occasionally claims of lack of correlation are disproved when better research designs controlling for other factors are used (e.g., Zador, et al., 1984). In other cases, the effect may be under or overestimated for a lack of control for relevant factors. For example, the National Highway Traffic Safety Administration estimated that the placement of fuel tanks in front of the rear axles of passenger cars reduced fatal rear-impact fires by 29 percent (Tessmer, 1994). The analysis did not account for the fact that cars were being reduced in size during the period that gas tanks were being relocated. When vehicle size and tank location were considered simultaneously in a multivariate analysis, forward-located tanks were found to reduce rear-fire fatalities by more than half (Robertson, 1993).

Third, the correlation must be demonstrated as large enough, given the number of observations in the sample, which is unlikely to have occurred from random fluctuations in drawing samples of that size. If the sample size is too small, however, a causal connection may be falsely rejected. Statistical power is a function of sample size. Studies often lack a large enough sample to detect magnitudes of correlation that would have practical use if detected. One study of prominent medical journals, such as the Journal of the American Medical Association, found that 70 percent of articles with statistically insignificant results did not discuss the issue of statistical power, that is, the number of cases studied was often not large enough to detect important differences beyond chance variation (Hebert, et al. 2002). Textbooks in statistics contain criteria for sampling and tests for random fluctuation in samples (e.g., Armitage, 1971, Selvin, 1991). Computer programs such as EPI INFO, a free program that is used for data entry and statistical calculations, include features for estimating statistical power before a study is undertaken (e.g., Dean, et al., 1994).

Fourth, the research design must be adequate to rule out co-variation between the hypothesized cause and other factors that could explain the same variation, or specify how such co-variation represents a causal sequence. This is the previously mentioned problem of "confounding". Research projects that demonstrate a nonrandom correlation between risk factor A and injury B are often criticized by saying: "You didn't control for X." The criticism may be legitimate if X could reasonably be expected to affect both A and B strongly enough to account for their correlation, but some such criticisms have no basis in terms of plausible causal mechanisms or the potential magnitude of the effect.

Fifth, the mechanism of causation should be plausible in terms of what is known about the phenomena in the relevant discipline. Occasionally the theories of physics, chemistry, biology, or behavior are modified by some seemingly implausible research finding, but often such a finding is subsequently shown to be the result of faulty research methodology. See Appendix 8-1 for an analysis of an implausible claim of the cause of divergence in trends of motor vehicle fatality rates among countries.

CAUSAL MODELS. The more plausible hypotheses are those that are deduced from what is known. Some important discoveries occur as a result of hypotheses based on hunches, observations of a few cases in a clinic or morgue, and the like, but the odds of finding something useful are greater when the hypothesis to be tested provides a plausible link in a causal model.

In the case of injury, a hypothesis is more plausible if the proposed cause has a likely connection to the concentration of energy, energy exchange with tissue, or the vulnerability of tissue. If the hypothesized cause could not directly affect one or more of these factors, it is likely to be a relatively weak and indirect contributor to the incidence or severity of the injury. To aid thinking about the research that is needed to fill gaps in knowledge, a diagram of the hypothesized causal paths of a given set of injuries is often useful.

For example, it is well known that age is correlated to severe motor vehicle injuries, but age is merely a measure of how often one has circled the sun. While age indicates a differential risk that can be used to target injury control efforts at overinvolved age groups, such as middle-aged opioid users, it is not a cause of injury. Age is an inexact proxy measure of human limitations and behaviors, including exposure to more or less risky energy sources (knowingly or unknowingly), which result in the concentration of energy or energy exchanges, and physical conditions that affect tissue vulnerability.

Correlating such factors as age and gender to injury rates has been called "black box" epidemiology (Susser and Susser, 1996). Without precise knowledge of the exposures to energy and behaviors in the presence of those exposures, by age and gender, the causal mechanisms involved are lumped in a box, the contents of which are unknown.

Figure 8-1 illustrates some possible paths of causation whereby correlates of age may contribute to the necessary and specific causes of motor-vehicle-occupant injury or severity. It ignores outdoor temperature, precipitation, gas price, or other factors potential road users considered in the decision to be on the road (Appendix 8-1). The direction of the arrows represents the known or hypothesized direction of the effect of a given factor on another.

Energy generated by speed and mass interacts with vulnerable tissue in a crash, exacerbated by insufficient room for vehicle occupants to decelerate as indicated by vehicle size. The power of an engine, measured as horsepower in given increments, is necessary for given increments in speed. Vehicle size and horsepower accounted for about 55 percent of the variation in occupant fatalities per 100,000 passenger cars of particular makes and models in the 1980s (Robertson, 1991). Observations of the age, gender, and speed of drivers at sites where vehicles rolled over indicated younger drivers were driving faster than the average speed at the sites, but there was no correlation between speed and gender. Age, however, was not correlated with rollover, and speed was not correlated to vehicle stability. Therefore, speeding or young drivers did not confound the effect of vehicle stability on rollover (Robertson and Maloney, 1997).

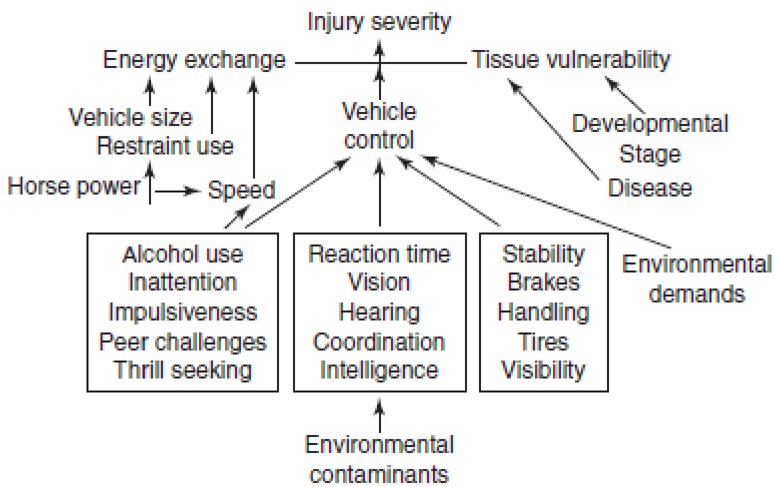


Figure 8-1. Causal Model of Motor Vehicle Injury

Loss of control of one or more vehicles usually precedes a crash. Loss of control is affected by vehicle stability, steering characteristics, braking capacity, and tire characteristics in interaction with inputs from the driver -- speed, steering, braking. Vehicle stability accounted for about 62 percent of the variation in fatal rollover crashes per 100,000 utility vehicles in the 1980s, independent of other major known risk factors. These vehicles had stability less than the vast majority of cars (Robertson, 1989). Automatic Stability Control Systems reduced fatal crashes generally, especially rollovers, but the effect of low stability still accounts for a significant proportion, controlling for the effects of other factors (Appendix 9-1).

Inputs to the vehicle by the driver are known to be altered by impairment or other effects of alcohol and other drugs, and variations in human limitations such as reaction times (time from the signal of the need to change inputs to actual behavior), vision, hearing, intelligence and coordination of senses and motor function. The effect of each is not known precisely and is more or less contingent on the demands of the driving environment. Therefore, the extent of reductions in injuries that could be achieved by changing the changeable factors is in more or less doubt for each one. The other listed behavioral factors may affect speed, reaction time, steering or braking, and degree of vehicle use, but how often is largely unknown. Factors related to age may also affect tissue vulnerability including developmental stages, alcohol or other drugs, and certain diseases.

The variables in Figure 8-1 are only operative if people are using roads as drivers, pedestrians, or bicyclists. Often neglected in research on injury prevention are the factors that increase road use and other dangerous activities. For example, road deaths per capita exposed to varying temperatures are highly correlated to the temperatures (Appendices 8-1 and 10-1). Miles-driven increases in correlation with higher temperatures and colder temperatures very likely reduce the number of people walking or bicycling on roads.

Some injury control efforts in industry use the concept "root cause" and in attempts to identify what should be changed to reduce injuries (Shufutinsky, et al., 2017). One of the purveyors of commercial course offering on "root cause" analysis defines it thusly: "The root cause is the core issue—the highest-level cause—that sets in motion the entire cause-and-effect reaction that ultimately leads to the problem(s)" (ASQ.org, no date). Anyone who has scientific training should know that there is seldom a single "root cause" for any phenomenon. If there is a necessary condition for the outcome and the condition is changeable, there is a root solution, but that condition is unlikely to be found at the outset of a long chain of events in the aggregate.

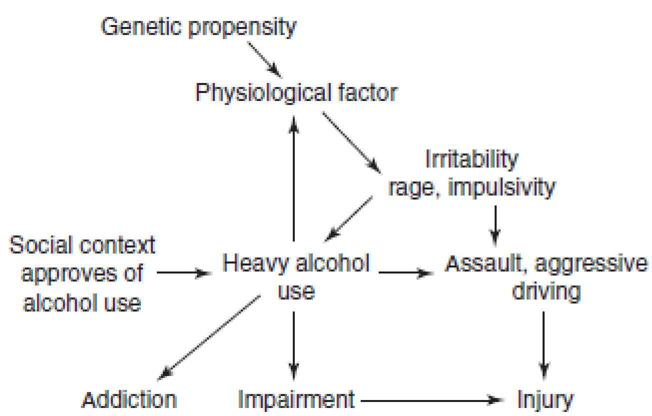


Figure 8-2. A General Theory of Alcohol Use and Effects

As depicted in Figure 8-1, the effects of given variables are straightforward in causal chains, but some of the links among factors may be more complicated. While the effect of alcohol is usually referred to as impairment, there are several aspects of the correlation between alcohol and injuries that suggest a more complex causal pattern. Alcohol in drivers in motor vehicle crashes is more correlated to injury severity than to incidence (Haddon, et al., 1968). Alcohol is found more often in the victims of assault and homicide than in drivers killed in motor vehicle crashes (Wechsler, et al., 1969; Baker et al., 1971). Therefore, it is unlikely that the effect of alcohol is just to impair performance or make the tissue more vulnerable. Based on these and other findings, a more elaborate model of alcohol use is suggested (Robertson, 1983), as displayed in Figure 8-2.

In biological and other systems, the system goes out of control when there is amplifying feedback, that is, factor A increases factor B which, in turn, increases factor A directly or through its effect on other factors. In the model, alcohol use is hypothesized as partly a consequence of inherited biological factors that affect emotions that may then contribute to an increase in drinking in uncontrolled feedback. If such feedback occurs, the individual effects in the loop do not have to be large to have an enormous effect in a few iterations. For example, if B=1.02A, C=1.03B, and A=1.02C, the three factors will about double in 10 cycles and about quadruple in 20 cycles. There is also evidence that unpleasant sensations when drinking (skin flush, increased heart rate) are related to reduced alcohol use (Akutsu, et al., 1989).

If the alcohol model is supported by research evidence, then the elimination of alcohol might not eliminate all of the injuries with which it is associated. The emotions that contributed to drinking may also contribute to the behaviors leading to injuries and would be present in the absence of alcohol, albeit to a lesser degree (McClelland, 1984). Chapter 15 includes an analysis that indicates that the removal of all alcohol from drivers would reduce fatal crashes by about half the amount presumed from data on alcohol "involvement". Too often, the human mind converts "involvement" to "caused".

This dynamic model of the causes and consequences of alcohol use is more comprehensive regarding the possible reasons for the involvement of alcohol in assaults as well as other injuries, and eventual alcohol addiction. It suggests that if the physiological factor possibly intervening between the genes and increased use can be identified and controlled, a variety of problems would be alleviated (Kapoor, et al., 2013).

Amplifying feedback is apparently a factor in violence ranging from interpersonal disputes to war. From the Hatfield and McCoy family feuds to youth in inner cities in the U.S. today, violence led to more violence through retaliation for real or perceived wrongs. Throughout much of the world, the violence of members of one ethnic group against another is often claimed to be justified by past wrongs (McConnell, et al, 2014). And President George W. Bush pushed the invasion of Iraq based at least in part on the allegation that Saddam Hussain tried to have Bush's father murdered. In a U.S. city, retaliatory homicide was found significantly more often in gang and drug killings but not so in those altercations among friends, family, strangers, or those in which alcohol was involved (Kubrin and Weitzer, 2003).

These are relatively simple examples of causal models. Numerous variations have been suggested just for driver factors in motor vehicle injury (Michon, 1985). The value of a model is not necessarily its complexity or completeness, but whether or not it suggests testable hypotheses regarding variables that have a major influence, particularly variables that are controllable for injury prevention or severity reduction.

Appendix 8-1. False Inference of Causation (Adapted from Robertson, 2006, 2018)

A retired General Motors employee, Leonard Evans, published a book in which he compared trends in motor vehicle death rates in the U.S. to other countries (Evans, 2004). The decline in U.S. rates slowed in the 1990s compared to those of several other industrialized countries. As a result, the U.S. no longer holds its historically leading position -- the lowest rate. Evans said the trends indicated a "Dramatic Failure of U.S. Safety Policy". His "analysis" was confined to eyeballing the trends and assertions regarding the cause. The slower decline in the slope of U.S. rates he attributed to personal injury lawyers who, he said, have an interest in focusing on vehicles to the neglect of programs to change driver behavior. He said that the emphasis on vehicle factors that resulted in the Motor Vehicle Safety Act of 1966 was the beginning of the problem and singled out the airbag controversy of the 1970s and 1980s as a subsequent major culprit.

Actually, the Motor Vehicle Act of 1966 was inspired by a book co-edited by a physician and two social scientists, who had no interest in injury lawsuits (Haddon, et al., 1964). That book emphasized that the energy in car crashes (and other injurious events) could be managed by product and environmental modifications to reduce injury severity. Based on that analysis, Senator Abraham Ribicoff began hearings on vehicle manufacturer responsibility to improve vehicle safety. When private detectives hired by General Motors were caught trying to entrap Ralph Nader in a scandal, and GM's Chairman apologized in a Senate hearing, the issue gained wider public attention (McCarry, 1972). Whether the Motor Vehicle Safety Act would have been enacted without that incident is problematic but it was a positive step to reduce vehicle fatalities. The initial regulations substantially improved the crashworthiness of passenger cars. The manufacturers subsequently improved crashworthiness, possibly in response to publicized crash tests (Appendix 13-1).

Evans said that Joan Claybrook (lawyer) in the Carter Administration, egged on by Ralph Nader (lawyer) imposed the "airbag mandate" in the U.S. In fact, the only airbag mandate was introduced by non-lawyers Douglas Toms, head of the National Highway Traffic Safety Administration (NHTSA) and a former state motor vehicle administrator, and John Volpe, Secretary of Transportation, a former owner of a construction company, in the Nixon administration before the Carter Administration. Because their mandate was not a performance standard as required by U.S. law, the standard was later revised to require minimum forces on crash dummies in frontal impacts at 30 miles per hour, which is not an "airbag mandate". The Reagan administration tried to overturn the standard but was overruled by the courts as a result of a lawsuit by insurance companies and others.

Automakers chose to use airbags to meet the standard but were not compelled to do so if they had chosen to design the vehicles differently.

Even more bizarre than Evans' false assertions were the favorable reviews of Evans' book in leading medical and other journals. The reviewer in the Journal of the American Medical Association called the chapter on U.S. policy failure a "show stopper" and devoted most of the review to an uncritical repetition of Evans' falsehoods regarding the history of vehicle regulation (Eisenberg, 2005).

So what are the available data that Evans failed to recognize? One prominent trend in U.S. vehicle sales was the increase in pickup trucks and large "sport utility vehicles" (SUVs) built on pickup truck frames. (Readers outside the U.S. unfamiliar with SUVs may see pictures by typing SUV in any Internet search engine). Vehicles with a center of gravity too high relative to their width have excessive rollover death rates (Robertson and Kelley, 1989). Stiff frames in certain SUVs, pickup trucks and vans are also a factor in increased risk (Gabler and Hollowell, 1996). Evans mentioned a possible effect of SUVs but said they could not make a difference of more than "a few thousand" deaths per year, "up or down". A few thousand deaths per year affect the rankings among countries substantially. He did not mention the fact that pickup trucks with high centers of gravity rapidly increased in sales. It has long been known that vehicles with their weights distributed higher off the ground contribute disproportionately to the deaths of occupants of other vehicles, as well as pedestrians and bicyclists, the latter probably because of longer braking distances of heavier vehicles (Robertson and Baker, 1976).

Using data from the countries that Evan's claimed as superior in safety policy to the U.S., trends in the sales of trucks were examined to see if those countries had a similar increase in truck use compared to the U.S. (Binder, 1990-2003). The major separation in the rates of the U.S. and other countries occurred in the 1990s. Figure 8-3 illustrates the change in truck sales during that period. In the U.S., trucks, and SUVs were 25 percent of sales in 1991 and steadily increased to more than 40 percent of sales in 2002. None of the other countries experienced a parallel increase. In Japan, the percentage of truck sales declined substantially but the trend was relatively flat in most countries. Trends among other European countries mentioned by Evans (Denmark, Finland, Luxembourg, and the Netherlands) were similar to those of their nearby neighbors shown here. The Canadian percentage started lower but rose similarly to that of the U.S. until 1998 and then declined. These trends led to substantial differences in vehicle mix among the countries.

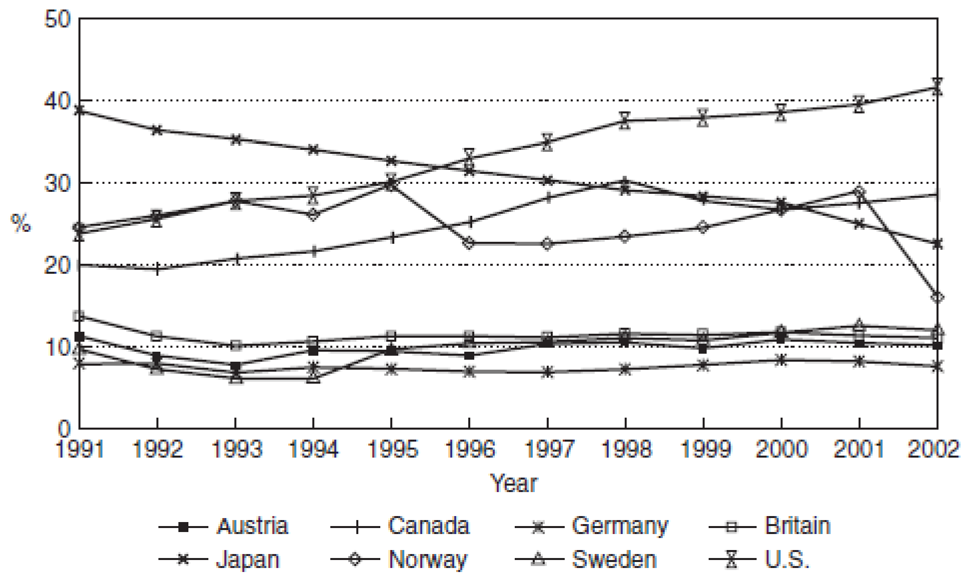


Figure 8-3. Truck Sales 1991–2002—Selected Countries

Of course, we cannot say with certainty that trends in truck sales explain the differences in fatality trends among countries without additional data but the fact that trucks have a higher death rate than other vehicles and truck sales increased in the U.S. disproportionately favors that explanation over Evans' "lawsuit" explanation, which has no plausible causal nexus.

More convincing data is available based on Canadian vehicle sales. Evans gave special emphasis to the decline in Canadian death rates because, he claimed, Canada has the same mix of vehicles and drivers as the U.S. The Canadian motor vehicle death rate per vehicle in use declined 63 percent from 1979 to 2002, similar to other industrialized countries, compared to a reduction of 46 percent in the U.S. rate.

Canada does not keep records of the makes and models of vehicles in fatal crashes but the data from death rates for each make and model in the U.S. can be applied to the same make-models in use in Canada to calculate an expected death rate, adjusted for average miles traveled per vehicle. The following steps were undertaken to obtain an expected death rate for Canada:

1. Count the number of deaths of each make and model in use in the U.S. and divide by the number in use during 2001-2002.
2. Multiply the death rate for each make and model in the U.S. times the number of each make and model in use in Canada during the same period to obtain expected deaths for each make and model in Canada.
3. Sum the number of expected deaths across makes and models and divide the total by the total number of vehicles in use in Canada. Multiply the expected rate per 100,000 vehicles by the ratio of the average annual miles per vehicle in Canada to the average annual miles in the U.S.

The fatalities in the U.S. by make and model of vehicle in the years 2001-2002 were obtained from the National Highway Traffic Safety Administration's Fatal Analysis Reporting System. Only 1991 and later models were included. All deaths in a fatal crash in which a given vehicle was involved were counted. In collisions of two or more vehicles, only the occupants who died in each vehicle were counted for that vehicle to avoid double counting. Vehicles in use in the U.S. and Canada were estimated by tabulating the annual sales by make and model in each country during 1991-2002 and subtracting the number scrapped before 2002, using published data on vehicles remaining after a given number of years of use for cars and trucks separately in each country (Binder, 1990-2003).

Differences in types of vehicles in use in 2001-2002 between the U.S. and Canada are shown in Table 8-4, based on an examination of specific make-model sales from 1991-2002, discounted for scrapped vehicles. U.S. drivers used proportionately larger cars that had somewhat lower death rates than smaller cars while Canadian drivers disproportionately used vans – the vehicles with the lowest aggregate death rates. U.S. drivers used proportionately more SUVs and pickup trucks that have higher total death rates mainly because of their higher involvement in the deaths of road users other than their occupants. The higher weights of trucks and SUVs result in longer stopping distances, contributing to increased deaths of pedestrians, bicyclists, and motorcyclists. Motorcycles are also used more in the U.S. Even with deaths of motorcycle riders counted in the total rate of other vehicles that collided with motorcycles, the single-vehicle death rate of motorcycles is higher than the total death rate of the other classes of vehicles.

Table 8-4. Percent Vehicles in Use in the United States and Canada and U.S. Fatality Rates: 2001–2002

	Percent Use		Total Deaths/100,000 Vehicles	Occupant Deaths/100,000 Vehicles
	U.S.	Canada		
Cars <3,000 lbs	30.4	38.8	18.2	16.5
Cars ≥3,000 lbs	25.9	17.5	16.9	14.0
Vans	9.6	16.5	13.4	9.7
SUVs	12.8	8.4	19.9	14.4
Pickup trucks	18.0	16.3	22.7	15.0
Motorcycles (single vehicle)	3.1	2.4	33.4	33.4

U.S. drivers drove an average of 12,655 miles in 2002 compared to 10,733 miles driven by Canadian drivers. Applying the U.S. rates for each make-model to the number of vehicles of the same make-model in use in Canada, and correcting for mileage differences, results in an expected death rate in Canada of 15.9 per 100,000 vehicles compared to 18.9 per 100,000 in the U.S. That difference is exactly the difference in the total death rates between the countries in 2002.

Contrary to Evan's assertion, the U.S. and Canada did not have the same mix of vehicles. The difference in death rates between Canada and the U.S. is predicted by the difference in vehicle mix and miles that were driven between the two countries. The results suggest that if Canadian drivers had driven the same mix of vehicles the same miles per vehicle as U.S. drivers, they would have the same total death involvement rate as U.S. drivers.

Also, there is no evidence for Evans' claim that the differences between the U.S. and Canada resulted from too little emphasis on behavioral factors in the U.S. Indeed, the U.S. federal government spends large amounts on such programs. According to officials at Transport Canada, there was no Canadian federal government expenditure on behavior programs during the period studied. In addition to the Motor Vehicle Safety Act, in 1966 the U.S. Congress enacted the Highway Safety Act which provides grants to the states for safety programs. A study of the early effects of such grants found an adverse effect on state motor vehicle fatality rates of high school driver education expenditures but a favorable effect of other programs (Robertson, 1984a). Driver education is no longer federally funded. The grants more than doubled in the 1990s. From 1998 to 2002, the grants to the states were incremented from $236.1 million to $556.8 million – targeted substantially toward alcohol abuse, seat belt use, and child restraint use. U.S. states and Canadian Provinces may have spent additional funds but there was certainly no paucity of attention to behavioral factors in the U.S.

Evans ignored all the evidence and in 2014 was allowed to publish a paper in The American Journal of Public Health updating the international trends, asserting that the differences were a result of variations in policies among the countries but again citing no specific policies or evidence of their effectiveness (Evans, 2014). One researcher who matched U.S. states to 43 high-income countries with similar populations found no differences in road deaths per vehicle miles traveled among those matched into four groups by population density, urbanization, and climate (Kahane, 2016).

Ignored by Evans and other claimers of policy failure (Richter, et al., 2001; Shuck, 2015) is research showing otherwise. Combined lap and shoulder belts reduce vehicle occupant deaths by about 45 percent when used. Belt use increased from about 15 percent before laws requiring belt use were enacted in the 1980s to more than 88 percent recently, based on observed belt use by vehicle occupants (National Highway Traffic Safety Administration, 2016), a remarkable change in behavior. Alcohol at 0.08 percent or above by weight in U.S. drivers in fatal crashes declined from 35 percent in 1982 to 20 percent in 1997, rose slightly, and dipped to 20 percent again in 2005. This reduction was associated with various changes in state laws such as the legal minimum drinking age and the lowering of the legally acceptable blood alcohol concentration (Dang, 2008). Licensed teen aged drivers are involved in fatal crashes about 3.5 times as often per mile driven as 30-70-year-olds. Reductions of 16-21 percent of 16-year-old drivers' fatal crashes were associated with laws restricting the conditions for 16-year-olds to be licensed, the percent reduction dependent on the specifics of the restrictions (Chen, et al., 2006). Involvement of drivers 16-19 years old in fatal crashes declined from about 50 per 100,000 people in that age group in the late 1970s to less than 20 per 100,000 in 2014 due to additional declines in licensure perhaps affected by the demise of driver education in many schools and increasing insurance rates.

After a 40 percent reduction in fatalities per registered passenger cars that met the initial federal safety standards required of new cars sold to the government in 1966 models and all cars in 1968 and subsequent models (Robertson, 1984a), airbags were phased in as standard equipment in new cars during the late 1980s as a result of a federal standard for increased frontal crash protection in new passenger cars. Manufacturers also installed side airbags and improved energy management of other vehicle components in numerous makes and models in subsequent years. Energy transfer criteria in the government's vehicle crash tests steadily improved year-to-year through 2010 (National Highway Traffic Safety Administration, 2015). A study of 1999-2005 model years of passenger cars, minivans, and sports utility vehicles assessed the effects of each of various vehicle safety features, controlling statistically for the effects of the others (Robertson, 2007). Vehicles with electronic stability control had about 42 percent fewer fatalities in crashes per vehicle than vehicles without the system. Vehicles that had injury criteria above average on the government's side crash tests had 19.4 percent fewer deaths. Those that scored best on the Insurance Institute for Highway Safety's offset frontal crash tests had 8.6 percent fewer deaths. Electronic stability control installations increased from 29 percent in "light vehicles" of the 2006 model year to 71 percent in the 2011 model year. All such vehicles, including pickup trucks, sold after September 11, 2011, are required by federal safety standards to have electronic stability control.

A review of studies of alteration of the built environment to reduce pedestrian deaths indicates effects from 25 to 75 percent (Retting, et al., 2003) but the extent that these measures have been deployed in the U.S. is unknown. States that implemented statewide trauma response systems had about a 9 percent lower fatality rate, controlling for other factors than those that did not (Nathans, et al., 2000).

The substantial percentage reductions in road deaths related to the noted efforts indicate policy success but they cannot be added together to assess their collective effect because of possible overlapping effects. For example, the correlations of death reductions from seat belt use and driving while intoxicated are each reduced when considered simultaneously because intoxication is associated with lack of belt use (Chapter 15).

Given the well-documented effects of specific laws, regulations, and other efforts, why was the decline in U.S. road deaths not steeper? Research has identified several major factors that would potentially offset preventive efforts to reduce road deaths in the U.S. Warmer temperatures are strongly related to increased road use and death rates (Leard and Roth, 2016; Robertson, 2017). The temperature effect was likely accelerated by the mass migration to warmer areas of the country in recent decades (Cebula, et al., 2014). The large growth in sales of pickup trucks and truck-based "sports utility vehicles" that are heavier than most passenger cars increased the deaths of occupants of other vehicles and pedestrians substantially (White, 2004) and have higher rollover death rates than passenger cars because of too high a center of gravity relative to track width (Appendix 15-1). Motorcycles, pickup trucks, and buses are substantially more involved in pedestrian deaths per mile driven than passenger cars (Paulozzi, 2005). Motorcycles have an occupant death rate 34 times that of other vehicles per mile traveled (Lin and Kraus, 2009) and their registrations increased apace in the 1960s and 1970s, declined in the late 1980s and 1990s, and increased again through 2015. A factor that would increase road deaths in the 1970s and reduce them subsequently is the aging of the large cohort of people born in the decade post-World War II (so-called baby boomers) that were in their teens and twenties in the 1960s and 1970s but were near retirement age in 2015.

Given these facts, I attempted to estimate the number of road deaths that would have occurred during 1968-2015 given the correlation of deaths per population to temperature, precipitation, the median age of the population, and vehicle mix among U.S. states in the early 1960s and the changing prevalence of these factors. The difference between predicted and actual deaths is an estimate of the overall effect of the various mentioned state and federal government prevention policies and other efforts.

The study is confined to the contiguous 48 U.S. states because weather data are not available for all the years in Alaska and Hawaii. State data were used because data on vehicle registrations are not available for smaller geographical areas within states. I obtained counts of road deaths per year in each state from Vital Statistics Mortality Tables National Center for Health Statistics, 1961-1998) and the WISQARS search system for 1999-2015 (Centers for Disease Control and Prevention, 2017). I searched the online files of the Fatal Analysis Reporting System (FARS) from the time it became available to count road deaths per month to relate them to average temperature per month (National Highway Traffic Safety Administration, 2017). FARS includes road deaths only when the death occurred within 30 days of the crash. Road deaths counted in vital statistics are about 1-5 percent more than in FARS from year-to-year.

I copied truck, motorcycle, and total vehicle registrations, estimated miles driven, and linear miles of a roadway by state from Highway Statistics for each year Federal Highway Administration, 1961-2016). I obtained resident population counts per state and the median age of the population from the U.S. Census and estimated the numbers in non-census years by linear extrapolation except for the 2015 estimates obtained from the U.S. Census Bureau's American Community Survey (2017). I downloaded files of monthly average temperature and precipitation by state from the National Oceanic and Atmospheric Administration (NOAA, 2017). It uses different numerical codes for states than the Census Bureau and the death files, so the author matched the data from the various files by a computer routine.

Since temperature varies somewhat among smaller geographic areas within states depending mainly on elevation and aridity, some fidelity of measurement is lost by using state averages. The weather stations that record the temperature and precipitation data are concentrated in populated areas within states so extreme temperatures that occur in thinly populated mountainous and desert areas have less weight on the averages (Weather station map, 2017). NOAA adds and deletes a small percentage of the 7000 monitored weather stations from time to time, creating controversy regarding whether trends are affected, but NOAA officials insist that trends in the data are unaffected (U.S. Government Accountability Office, 2011).

I examined the relationship between average monthly temperature and road deaths per population exposed to temperatures monthly in detail before using the state yearly averages as predictors. I used least squares regression to estimate the relation of deaths per population exposed to specific average temperatures per month in 1975 and repeated in each year 1976-1980 to establish the reliability of the coefficients.

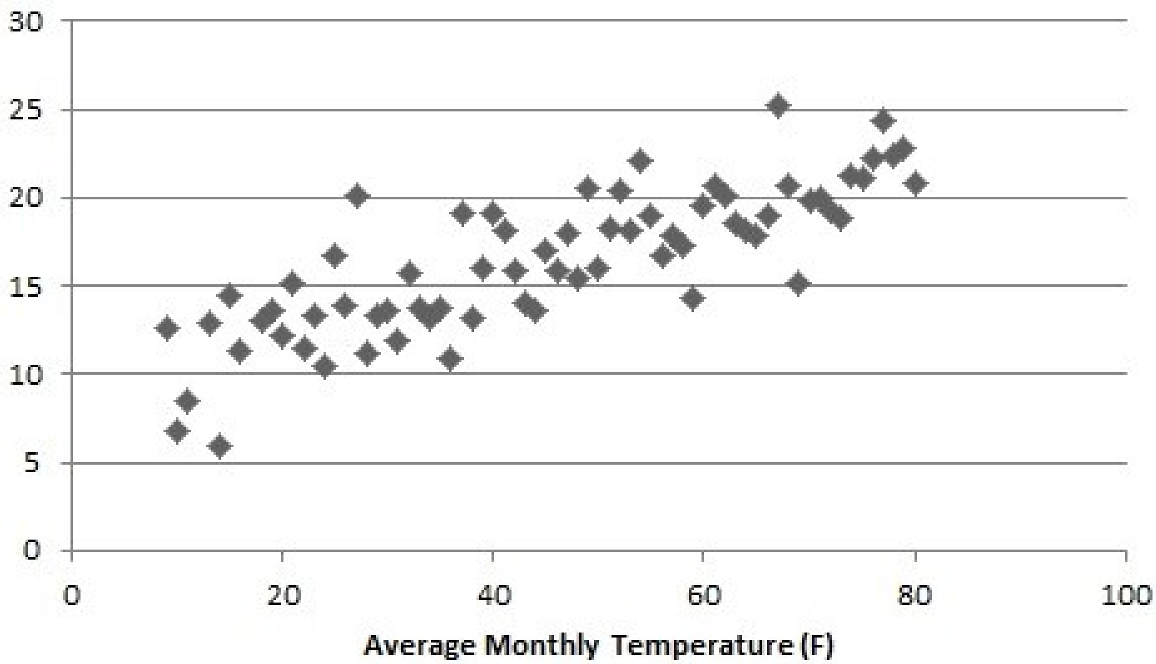
One measure of exposure to death risk is miles traveled per vehicle, found to be related to temperature in urban U.S. counties (Robertson, 2017). The Federal Highway Administration began reporting estimated miles traveled separately in U.S. states in 1994 based on traffic count data at 4000 locations (Federal Highway Administration, 2017). I estimated miles traveled per registered vehicle in 1994 through 1999 correlated to average annual temperature and precipitation using least squares regression to examine the reliability of the coefficients using state-level data.

I employed logistic regression to assess the association of deaths per population with average annual temperature, total annual precipitation, the median age of the population, registered vehicles per population, and percent of registered vehicles that were motorcycles, trucks, or buses using data for each of the 48 states in 1961. I tested the reliability of the coefficients by noting the fit of predicted deaths to actual deaths in 1962 through 1965 before the noted prevention efforts were adopted. I used the regression coefficients to predict the number of deaths that would occur given temperature and precipitation averages and migration to warmer areas, as well as vehicles per population and vehicle mix separately in each state through 2015. I compared the sum of predicted deaths across the states to the actual number of deaths nationally.

Changes in the risk factors during the period studied were remarkable. Nationally, the population increased from 178 million to 314 million (76.4 percent), and vehicles per 100 population increased from 45 to 94 during 1961-2015. Percent trucks and SUVs of all registered vehicles increased from 19 to 55 during the study period and percent busses declined from 0.4 percent to 0.3 percent. Percent motorcycles of registered vehicles rose from 0.8 to nearly 4 percent during 1961-

1984, declined to 2 percent in the mid-1990s, and rose to 3.7 percent in 2015. The average temperature among the states rose about 2 degrees (F) from 1961 to 2015 with a few percent year-to-year variations about the trend line in interim years. Precipitation fluctuated but did not show a major trend. The predominant effect of temperature is due to the migration of populations from cooler to warmer states. For example, the 8 states bordering Mexico and the Gulf of Mexico gained 252.7 percent population (41.2 to 104.1 million) from 1960 to 2015 while the 13 states bordering Canada gained only 31.4 percent (59.8 to 78.6 million).

Figure 8-5. Deaths per Population of Those Exposed to Average Monthly Temperature in the Contiguous 48 U.S. States, 1975.



The association of road deaths per million person-months of exposure to given monthly average temperatures is shown in Figure 8-5. Each point represents the sum of deaths that occurred in a month in any state where the average temperature was at specified degrees divided by the sum of the population in the state every time the temperature was at the same degrees. The data in the graph are similar to that found when deaths on specific days were divided by the number of persons exposed to the temperature on a given day in previous research of the 100 most populous counties in the U.S. (Robertson, 2017). The average death rate per exposed population at average temperatures near 80 degrees (F) is about twice the risk at temperatures in the teens and the rate increases linearly, on average, as temperature increases. Table 8-2 shows the regression coefficient of the association seen in Figure 1 compared to that in each of the subsequent five years. The 1975 coefficient is the second to the lowest and the 1976 coefficient is an outlier but R^2 is very strong in each year. Therefore, there doesn't appear to be much loss in fidelity of the correlation of temperature and road death rate per population using the average monthly temperatures in each state.

Table 8-2. Least-squares Regression Coefficients and 95 Percent C.I. of the Relation of Road Deaths Per Million Person Months and Average Temperature Per Month, 48 Contiguous U.S. States

Years	Intercept	Temperature Coefficient	95% C.I.	R^2
1975	9.051	0.169	0.142-0.196	0.68
1976	12.129	0.113	0.083-0.143	0.59
1977	9.151	0.207	0.173-0.239	0.66
1978	8.812	0.215	0.185-0.245	0.70
1979	10.080	0.184	0.155-0.213	0.68
1980	9.774	0.194	0.162-0.226	0.64

The regression of vehicle miles traveled per vehicle correlated to average annual temperature during the first six years that miles were measured more accurately shows consistently more miles driven per vehicle registered at warmer temperatures (Table 8-3). For each higher degree (F) of temperature, vehicles were driven about 110 more miles per year with some year-to-year variation. The relatively low R^2 may be due to the small sample of sites per state to measure mileage. The results suggest that discretionary driving is more frequent in states with warmer temperatures.

Table 8-3. Least-squares Regression Coefficients and 95 Percent C.I. of the Relation of Vehicle Miles Traveled Per Vehicle and Average Annual Temperature, 48 Contiguous U.S. States

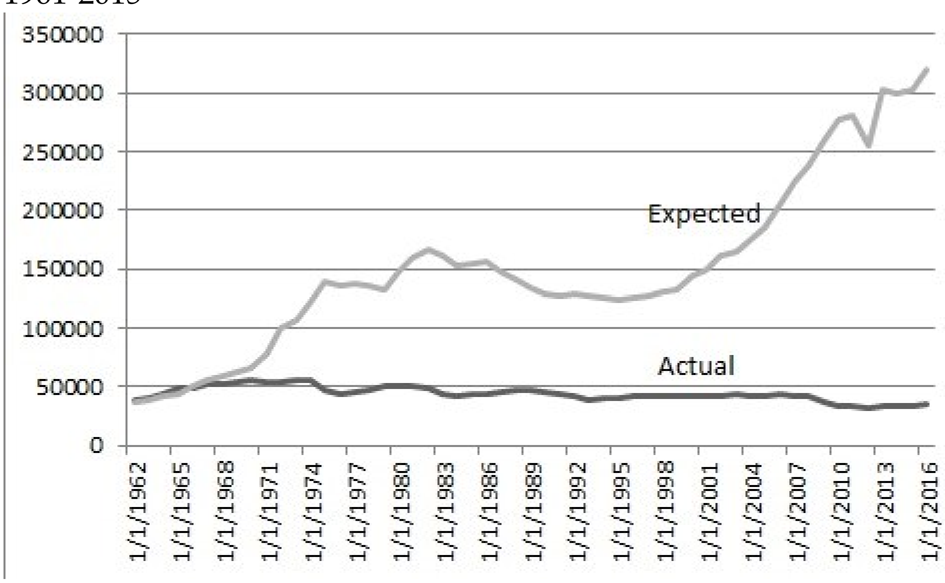
Years	Intercept	Temperature Coefficient	95% C.I.	R^2
1994	6691	103	48-158	0.23
1995	6349	111	60-162	0.27
1996	6679	110	60-161	0.26
1997	6284	121	64-178	0.26
1998	6749	111	50-172	0.20
1999	7071	108	39-177	0.15

Table 8-4 displays the logistic regression coefficients and 95% confidence intervals derived from the analysis of the noted risk factors among the states in 1961. Each of the predictor variables is strongly correlated to death rates per population among the states.

Table 8-4. Logistic Regression Coefficients Relating Road Deaths Per Population to Temperature, Precipitation Vehicle Types, and Median Age of State Populations, 48 Contiguous U.S. States, 1961.

	Coefficient	Lower 95% C.I.	Upper 95% C.I.
Per degree (F) of Average Monthly Temperature	.00702	.00521	.00883
Per Inch of Precipitation	-.00386	-.00508	-.00264
Vehicles/population	.7515	.4369	1.0661
Percent Motorcycles of Registered Vehicles	.2936	.1553	.3553
Percent Trucks of Registered Vehicles	.0175	.0146	.0204
Percent Buses of Registered Vehicles	.4962	.3532	.5593
Median Age of the Population	-.0228	-.0287	-.0169
Intercept	-9.0688		

Figure 8-6. Sum of Expected and Actual Road Deaths:48 Contiguous U.S. States, 1961-2015



The equation in Table 8-4 predicted slightly fewer deaths than occurred in 1962-1966, indicative of a conservative estimate of effects, but predicted deaths increased rapidly relative to actual deaths in subsequent decades except during the late 1980s and 1990s when the baby boomers were leaving their higher risk years and motorcycle registrations declined dramatically (Figure 8-6). By 2015, the deaths expected without interventions were 8.8 times the actual number of deaths. The model predicted about 8 million deaths during 1968-2015 but 2.2 million occurred, suggesting that about 5.8 million road deaths were avoided by preventive efforts. Deaths were 72.5 percent less than expected throughout the period which suggests that some of the deaths prevented by the noted prevention efforts would have been prevented by one or more others. Contrary to the claims of failure of U.S. road safety laws, vehicle safety regulations, and other preventive efforts, these results suggest that they averted a public health disaster. Deaths would likely have been in the hundreds of thousands rather than tens of thousands per year in the twenty-first century without the demonstrably effective federal and state policies and other efforts mentioned in the introduction.

This estimate does not account for factors unmeasured here that are known to affect risk. In the 1960s, vehicle manufacturers engaged in a "horsepower war", competing to make more powerful engines, which may explain why deaths during that period exceed the predictions. Although the driver error rate probably did not change much, if any, during the study period, a distraction from portable cell phones and other electronic devices that increases risk when used while driving grew in numbers rapidly in the 1990s and early 2000s resulting in increased risk (Wilson and Simpson, 2010). Lower gasoline prices correlated to increases in road deaths during 1983-2000 (Grabowski and Morrissey, 2004). Historic gasoline prices are not available for each state so they could not be included in this analysis. In 2015 dollars, the national average price per gallon was $1.83 in 1965 and $2.45 in 2015 with large fluctuations in the interim (U.S. Department of Energy, 2017). U.S. gas prices per gallon are less than half of those in most other wealthy nations (Bloomberg News, 2017) an additional reason for differences in road death rates among countries. Opioid drug use increased substantially in recent decades as indicated by rapidly growing overdose deaths but a large case-control study found no significant effect of various drugs other than alcohol on driver crash risk when other risk factors were controlled statistically (Compton and Berning, 2017).

The inverse correlation of precipitation and deaths per population suggests the probability that some people, especially pedestrians, pedal cyclists, and motorcyclists without a protective shell, also avoid being on roads when the weather is adverse, more than offsetting the increased risk of slick roads. The finding regarding precipitation here is opposite of that noted in the study of urban counties in 2014 (Robertson, 2017). While the seasonality of precipitation is fairly consistent in some sections of the U.S., it is quite variable in others (Finkelstein and Truppi, 1991). Observation of the frequency of road use by type of user under different temperature and precipitation conditions at rural and urban locations would shed light on reasons for the difference. The finding that road use with accompanying deaths increases as temperatures rise is consistent between the studies and suggests that continued warming of the atmosphere related to the emission of greenhouse gasses from motor vehicles and other sources (Tebaldi, et al., 2017) does not portend well for the future of road safety.

Given the apparent effect of rising temperature on increased road use, previous research that compared states or other jurisdictions in cross-sectional or time series studies of road death and injury should be redone to assess whether the inclusion of temperature and precipitation alters the findings.

References – Chapter 8

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