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Injuries in Crashes -- Reported Compared to Actual

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ABSTRACT

Most of our understanding of traffic safety comes from analyzing data. Derivations from data, strictly speaking, tell us only about properties of data sets. They become important only with the assumption that the data reasonably represent reality. Yet what is included in a data set rarely corresponds to exactly what really happened. Cases that should be included are not included, and cases that should not be included are included. The most reliable information is for fatalities, yet even fatality data are far from perfect. For non-fatal crashes the problems are vastly greater. Indirect means can be employed to compare expected and reported injuries. The number of injuries per fatality, and the number of injuries in similar crashes, should remain fairly constant in time and between countries. This is examined using data from the US, Canada, Great Britain, Northern Ireland, Ireland, and Lithuania. Large discrepancies between reported and inferred injuries are found. These suggest that when reporting an injury provides the injured person no benefits, injuries are likely to be underreported. However, when large monetary payments may result from reporting an injury, especially a whiplash injury, large overreporting of injuries occurs.

INTRODUCTION

Most of our understand of traffic safety originates from analyzing data. Derivations from data, strictly speaking, tell us only about properties of data sets. Such findings are rarely of much interest in themselves. They become important only with the assumption that the data reasonably represent what really happened. Yet what is included in a data set rarely corresponds to exactly what really happened. Two types of mistakes occur. Cases that should be included are not included, and cases that should not be included are included. In addition to mistakes (which are not intrinsically inevitable), there are the errors that must intrinsically accompany the measurement of all quantitative variables, such as the time of a crash, or any measure of crash severity. Small numbers of mistakes and errors of small magnitude are generally of little consequence – inferences from the data will apply closely to the real world. However, when the mistakes become more numerous, and the errors larger, conclusions from the data may have little relationship to the real world.

It is rarely possible to investigate directly the reliability of a data set by comparing its content to what really happened, because we do not know what really happened. All we have is the data set. However, various indirect inferences can help bridge the gap between what is in the data set and what really happened.

FATAL INJURIES

While the most reliable data are for fatal injuries, it should not be assumed that fatality data are free of uncertainties. Some problems even with fatalities are treated in some detail to provide background to the much larger problems that occur in data sets for non-fatal crashes.

TWO EXAMPLES UNRELATED TO GROUND TRAFFIC. The two examples below show that even for systems more narrowly defined than a nation's annual traffic toll, uncertainties are still present.

<u>Deaths from the sinking of the *Titanic*.</u> After the *Titanic* sank in 1912, official inquiries were conducted by a special committee of the US Senate (because American lives were lost) and the British Board of Trade (under whose regulations the *Titanic* operated). The total numbers of deaths established by these hearings were:

US Senate committee:	1,517 lives lost
British Board of Trade:	1,503 lives lost

In principle, there is a correct number, but we can never know what it is. It is not necessarily within the range of the above estimates. Such uncertainty for a closed system, with formal passenger and crew manifests, alerts us to the possibility of uncertainties in less controlled situations.

<u>Deaths from 9/11 terrorist attacks</u>. We will never know the precise number of people killed by the terrorists attacks on 11 September 2001. Estimated totals, while widely quoted, are subject to errors of omission and

inclusion. Victims whose whereabouts or lives were not a subject of close interest to others were likely to not be counted in the absence of physical evidence. We are unlikely to find out how many (if any) spouses used the attacks as an opportunity to violently terminate a no-longer loving marriage. During the stress and chaos, one more claimed victim would be unremarkable. A clumsy body disposal would be unlikely to attract police attention even if police had not been preoccupied with other matters. Additional motivation was provided by anticipating generous monetary compensation to family members of those whose names were inputted into the victims list.

FARS DATA

The Fatality Analysis Reporting System (FARS)[1] data have contributed to more to understanding traffic safety than any other data set. While the crashes it documents occur exclusively on US public roads, FARS has been used by researchers in many countries. The more than one million well-documented fatal crashes makes FARS not only a valuable US national resource, but also a valuable world resource conveniently available to all on the NHTSA web without charge.[1]

While FARS is the *gold standard* of traffic safety data sets and has enormous strengths, it has a few deficiencies. Some are intrinsic, while different decisions could have avoided others.

The definition of a traffic fatality in FARS underlines how difficult decisions cannot be avoided, and even for fatalities there is not an obvious include, or do not include, classification in all cases. FARS defines a traffic fatality as a person who dies within 30 days of a crash on a US public road involving a vehicle with an engine, the death being the result of the crash. If a driver has a non-fatal heart attack that leads to a crash that causes death, this is a traffic fatality. However, if the heart attack causes death prior to the crash, then this is not a traffic fatality. If a victim dies many days after a crash, a difficult judgment may be required to decide whether it is a traffic fatality.[2] For example, a frail person may die from pneumonia during hospitalization to treat crash trauma. As we all have some chance of dying at any moment, some people die within 30 days of even the most minor crash.

Date of birth of people involved in fatal crashes is not coded in FARS – just age at time of crash, in one year increments. While of little consequence for adults, the absence of date of birth precludes examining any differences in risk in the first few days of life compared to in the eleventh month. Much is to be learned from traffic fatalities sustained in the first year of life.[3(p 227-230)]

<u>FARS ***not*** a census.</u> FARS is not a census of traffic deaths because cases in which *deliberate intent*, such as suicide or vehicular homocide, can be definitively identified are excluded. Excluding some unknown (but small) percent of traffic suicides makes the file less useful for investigating traffic suicides. All traffic deaths should have been coded, and if deliberate intent was confirmed, or suspected, this should have been coded in additional data elements. Hopefully FARS can correct this deficiency. NHTSA has developed procedures to impute the missing alcohol measurements based on relationships between factors known to correlate with alcohol use.[4] A corresponding approach might be possible to estimate unidentified suicides, but only if the data for confirmed suicides were in the data set.

Indirect methods applied to Finnish data indicate that as many as 5.9% of traffic deaths may be suicides.[3(p 225),5] Such a rate applied to the US implies about 2,500 traffic fatalities attributable to suicide per year in the US. Only a few of these would be excluded, because direct evidence, such as a suicide note, is rare.

If the Finish estimate applies to the US, then suicide has a larger affect on total US traffic fatalities than airbags, which are estimated to have reduced total 2003 fatalities by under 5%.[3(p 381)] It is important to consider the suicide component of traffic deaths because the mix of countermeasures to reduce this source of traffic death will differ for the mix of countermeasures to reduce harm by drunk drivers, which is different from the mix to reduce harm by sober drivers, etc.

<u>Time between fatal crashes.</u> In 2001 there were 37,900 fatal crashes in the US with crash time coded in FARS. This is equivalent to an average rate of 0.07133 crashes per minute, or one fatal crash per 14 minutes. Crashes occurring at random times at this average rate define a Poisson process, with its well known properties.[6] In particular, the number of crashes occurring at a given time, *t*, after the most recent previous crash would follow an exponential decline, as indicated by the straight line on the log-linear plot in Fig 1.

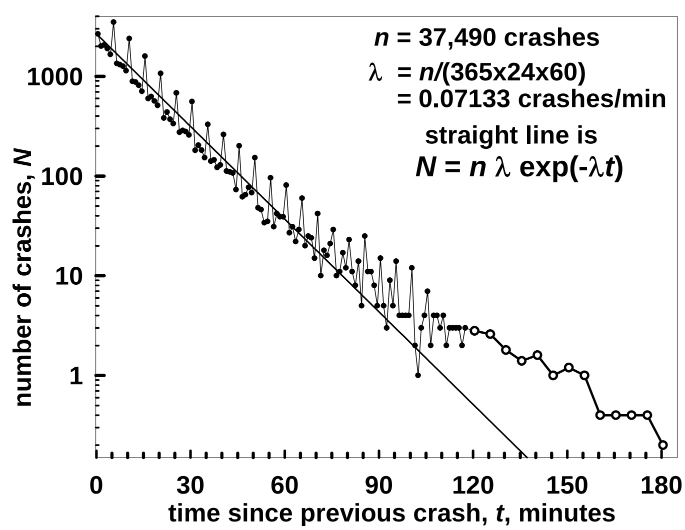

Figure 1. The distribution of the times between consecutive fatal crashes in FARS 2001.[3(p 59)]

If the data were a perfect representation of reality, the largest number of occurrences would be for $t = 0$. As the smallest time unit in the data is one minute, a recording of no time difference between a pair of crashes implies that they occurred within a minute of each other. The observed peak is not at $t = 0$, but at $t = 5$ minutes. The distribution shows a prominent cyclical pattern of peaks at multiples of 5 minutes. This results from the tendency to record times in multiples of 5 minutes, a tendency reinforced by traditional mechanical analogue devices which display time using circular dials marked in five-minute intervals. This tendency might disappear when the digital revolution is complete. The straight line is clearly a truer representation of reality (for $t < 75$ minutes) than the actual data. If the data did fit a perfect Poisson process, Fig. 1 predicts that in a year we should expect one period of 110 minutes to elapse with no crashes anywhere in the US. So, even if crashes were perfectly random, someone is not killed every 14 minutes (when 372 crashes for which the time of crash was not adequately coded are added, the average rate for 2001 becomes one crash per 13 minutes) .[3(p 58)]

Unlike the 5 minute cyclical pattern, the large departures from the Poisson relationship at large values of *t* reflect a real phenomenon. This occurs because risk is not equal at all times. Risk at 2:00 am on Saturday or Sunday is about four times risk at 5:00 am on a weekday.[3(p 58)] No fatal crash occurred between 3:30 am and 7:00 am on Tuesday 6 March 2001, or between 3:00 am and 6:30 am on Tuesday 27 November 2001. In both these cases, three and a half hours elapsed without anyone being killed, illustrating more specifically that someone is not killed every 13 minutes.

DEPENDENCE OF NUMBER OF REPORTED CRASHES ON CRASH SEVERITY.

Figure 2 shows the number of crashed vehicles according to severity, as measured from weighted NASS data.[7] The number increases very steeply with decreasing delta-*v*, reaching a peak at Δv just under 20 km/h.

There have been many comments to the effect that the most common crash delta-*v* is some value, say about 20 km/h. This is not so. The peak is a characteristic of the data set, not a characteristic of crashes. There are compelling reasons to believe that more crashes occur with Δv in the range 0-1 km/h than occur in the range 1-2 km/h, and so on, with the number of crashes increasing systematically with decreasing severity. At below about 20 km/h, the probability that a crash is recorded in the data set declines reaching essentially zero for $\Delta v = 1$ km/h, thus producing the observed pattern in the recorded data. The extrapolation of a straight line fit to the data for delta-v above 25 km/h estimates about 9 times as many crashes in the range 0-1 km/h as in the range 19-20 km/h.[3(p 27)]

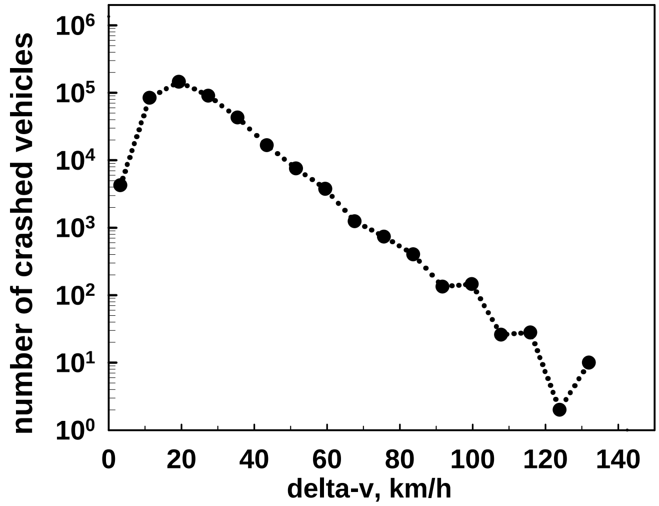

Figure 2. The number of crashed vehicles according to severity, as recorded in weighted NASS data.[3 (p 27)]

NON-FATAL INJURIES

The omission of large numbers of low-severity crashes from the data used to produce Fig. 2 is a feature built into the data set – only crashes above a specified level of severity were supposed to be included (indeed, they were all tow-away crashes). The *missing* cases were not supposed to be included. Because almost no injuries are expected in even very large numbers of sub-threshold crashes, their omission is of little material consequence. Real problems do arise when cases that should be included are omitted, and when cases are included when they should not be.

Problems of the type illustrated above for fatalities and crashes become vastly most acute for injuries. Three examples are presented below which show, by indirect means, that the number of injuries recorded in data sets can depart by large amounts from he actual number of injuries that occurred

FATALITIES COMPARED TO REPORTED INJURIES FROM IRISH DATA

Figure 3 shows the number of traffic fatalities per million population versus road user age for Northern Ireland and for the Republic of Ireland for 1990-1992. Northern Ireland, which is a province of the much larger United Kingdom, and the Republic of Ireland, an independent nation, share the same small island of Ireland. As physical environment, climate, vehicles, and general human behavior are similar in the two jurisdictions, it is not surprising that fatalities show similar characteristics. However, reported injuries per thousand population do not, as indicated by 1991 data for each jurisdiction:[8]

Northern Ireland: 6.9 reported injuries per 1000 pop.

Republic of Ireland: 2.7 reported injuries per 1000 pop

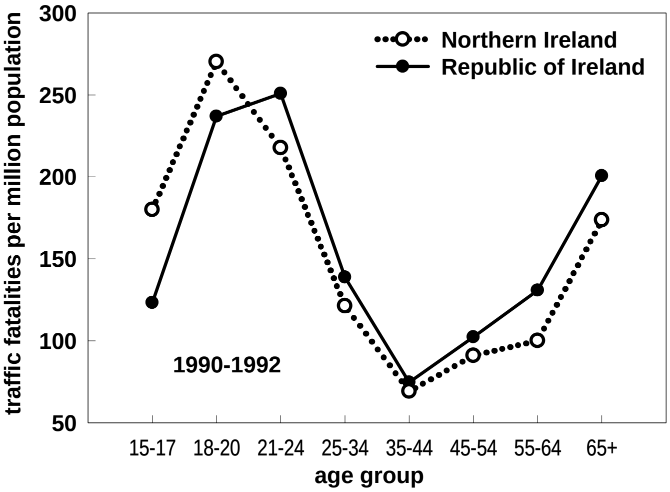

Figure 3. Traffic fatalities per million population in Northern Ireland and the Republic of Ireland.[3(p 29)] Data from Ref. 8.

The authors of the report providing the data comment on the dramatic difference in reported injury rates compared to the lack of difference in fatality rates as follows:

The most likely solution to this conundrum is that reporting practices are very different in the two jurisdictions, with "minor" injuries likely to go unrecorded in the Republic but to be "over-reported" in the North.[8]

The more generous British social welfare benefits available in Northern Ireland provided monetary compensation for genuine injuries. However, the same benefits were available for reporting injuries even if none occurred. Such benefits being less available in the Republic at the time covered by the study may have led some real injuries to go unreported, because those injured did not feel it worth the time and trouble to report them.

WHIPLASH

The term "whiplash" refers to injuries associated with occupants' heads moving rearward relative to their bodies when vehicles in which they are traveling are struck in the rear by other vehicles. Late whiplash syndrome refers to symptoms that persist, or arise, long after the crash. Unquestionably many injuries occur in rear-impact crashes, many of which cause major pain and disability. Such injuries can be difficult to diagnose by objective medical tests, so patients' reports of neck pain are often the only basis of diagnosis.

There are innumerable published estimates of more than a million whiplash injuries in the US each year, with some estimates being as high as 4 million.[9] The total monetary cost is estimated to be 29 billion dollars per year.[10] For Western Europe over a million whiplash injuries are reported, and estimated to cost 8 billion euros a year.[11]

It is common knowledge in the US and Western Europe that a reported whiplash injury can lead to monetary compensation. It is likewise well known that a rear-impact crash has a very high probability of being followed by claims of whiplash injury. The expectation that such injuries are a near inevitable consequence of a rear-impact crash may generate genuine symptoms that, absent such expectation, might not occur.

How widespread would reports of whiplash injuries be if people did not expect to suffer them after rear-impact crashes, or could not receive payment for claiming symptoms? This question was addressed by two studies using similar methodology conducted in Lithuania. In Lithuania, few car drivers and passengers were covered by insurance, and there was little awareness among the general public about the potentially disabling consequences of a whiplash injury.

In the first study, 202 occupants of cars that had been struck in the rear were interviewed 1-3 years after their crashes.[12] A control group of 202 individuals matched in age and gender who had not been involved in any type of traffic crash completed the same questionnaire. Members of the study and control groups were asked to report symptoms associated with whiplash, with the results summarized in Table 1. The authors report that no one in the study group claimed disabling or persistent symptoms as a result of the crash.

Table 1. Comparison of reported whiplash symptoms by occupants of cars struck in the rear 1-3 years earlier topeople not involved in traffic crashes. Data from Lithuania, where few car drivers and passengers are covered by insurance. From Ref. 12.

self-reported complaint	*202 occupants of cars struck in rear*	*202 random people*
neck pain	71	67
headache	107	100
chronic neck pain	17	14
daily headache	19	12
disabling or persistent symptoms as a result of the crash	0	not applicable

The second study used 210 subjects in cars struck in the rear, and 210 crash-free subjects matched in age and gender.[13] Unlike the earlier investigation, study subjects were mailed questionnaires soon after the crash to obtain information about short-term effects. Follow up questionnaires were sent to the study subjects two months after their crashes, and one year after their crashes. A follow up questionnaire was sent to the control subjects a year after they were first identified. The results are summarized in Table 2. The authors conclude:

In a country were there is no preconceived notion of chronic pain arising from rear end collisions, and thus no fear of long term disability, and usually no involvement of the therapeutic community, insurance companies, or litigation, symptoms after an acute whiplash injury are self limiting, brief, and do not seem to evolve to the so-called late whiplash syndrome.[13]

Table 2. Comparison of reported whiplash symptoms by occupants of cars struck in the rear and respondents not involved in traffic crashes. Data from Lithuania, where few car drivers and passengers are covered by insurance. From Ref. 13.

frequency of neck pain	*crash victims before crash n=210*	*controls at identification n=210*	*crash victims after 2 months n=198*	*crash victims after 1 year n=200*	*controls after 1 year n=193*
no neck pain	148 (70%)	146 (70%)	132 (67%)	140 (70%)	114 (59%)
neck pain					
< 1 day per month	37 (18%)	38 (18%)	36 (18%)	27 (14%)	39 (20%)
1-7 days per month	18 (8.6%)	16 (7.6%)	18 (9.1%)	25 (13%)	28 (15%)
8-15 days per month	2 (1.0%)	4 (1.9%)	3 (1.5%)	3 (1.5%)	4 (2.1%)
> 15 days per month	1 (0.5%)	3 (1.4%)	2 (1.0%)	0 (0.0%)	1 (0.5%)
every day	4 (1.9%)	3 (1.4%)	7 (3.5%)	5 (2.5%)	7 (3.6%)

NHTSA estimates that about 1.5 million vehicles are struck in the rear annually in the US.[14] The more than a million reported cases of whiplash injury implies that a rear-end crash has about a 67% chance of generating a reported whiplash injury, so that samples of over 200 occupants struck in the rear would be expected to produce about 130 cases of whiplash. The data in Tables 1 and 2 convincingly reject any possibility that whiplash injuries are nearly that common. In fact, there are no more than minor differences between the self-reported symptoms of occupants of vehicles struck in the rear and people not involved in any type of traffic crash. The conclusion is inescapably clear. It is insurance compensation and litigation that is responsible for most of the whiplash injuries reported in the US and Western Europe, not crash forces.

INJURIES PER FATALITY

In Canada from 1970 to 2001 the number of traffic fatalities decreased by 45%, but the number of injuries increased by 24%. A number of explanations have been offered to account for this dramatic contrast. These include the suggestion that occupant protection has made enormous strides in preventing fatalities, but not in preventing injuries. This is unconvincing. There is no reason to suppose that measures that reduce the forces on the human body in a crash will particularly alter the distribution of injuries by severity. All injury levels are expected to decline by comparable proportions. Such evidence as there is suggests occupant protection improvements will reduce injury risk more than fatality risk. For example, safety belts are probably more effective at preventing injuries than fatalities.[3(p 283)]. Another suggestion is that improved trauma care reduces fatalities, but an injury remains an injury even if given better medical treatment. This is qualitatively correct. But, as more than half of fatalities in FARS 2002 died within an hour of their crashes, the quantitative effect of improved trauma care, while an important contributor, cannot explain more than a small portion of the enormous divergence between the fatality and injury trends.

There are general reasons why the ratio of injuries to fatalities is expected to be fairly robust, and to not depend much on country, safety policy (for example, belt wearing laws) or vehicle design, and to change only gradually in time. Even if vehicle factors did somehow influence the ratio, the effect from year to year could be no more than a percent or so, because 90% of the vehicles on the road in a given year are the same vehicles that were on the road in the previous year.

The data in Fig. 4 defy any plausible interpretation in terms of engineering or medical factors. The number of injuries per fatality should be similar in Canada and Britain, and change only slowly, and similarly, in each country. What Fig. 4 appears to be reflecting is not changes in the risk of injury, but changes in the probability that an injury is reported. The reporting probability depends on politics, medical policy, insurance policy, and law, all factors that can change quickly, and differ from country to country, and from era to era.

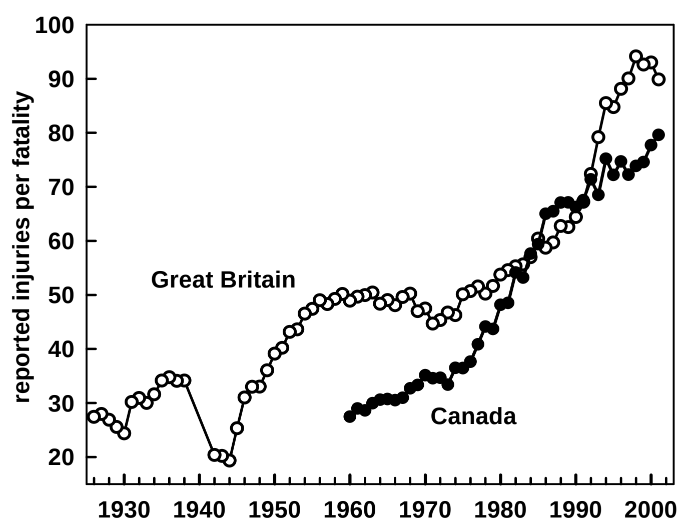

Figure 3. The number of reported injuries divided by the number of reported fatalities in Canada and Great Britain. Figure from Ref. 3 (p 33) using data from Refs. 15 and 16.

In Britain in the Second World War years 1942-1944 there were 20 reported injuries per fatality, compared to 34 in the pre-war years 1935-1938. After the war the number of reported injuries per fatality increased, but stabilized at close to 50 during the prolonged period from 1950 and 1970. This period followed the introduction of the National Health Service in 1948. Everyone requesting health care received it free of cost, but opportunities for additional compensation for injuries were generally unavailable. Beyond the 1970s, opportunities for monetary compensation expanded. Fig. 4 shows a marked increasing trend in the number of reported injuries per fatality after 1970.

In Canada in the 1960s, when medical care was largely paid directly out of patients' pockets, the number of reported injuries per fatality was substantially lower than in Britain. However, it later increased rapidly as Canadian provinces moved more in the direction of public payment for medical care, and later opportunities expanded for compensation in addition to medical care.

There are two effects that can make the number of reported injuries increase even if the actual number of injuries remains constant. First, in the past injuries occurred but were not reported. Direct out-of-pocket expenditures discourage reporting. Because of increased emphasis on health care, someone suffering a cut, scratch, or bump today is more likely to seek medical care than in the past even if cost is not a consideration.

The second way that reported injuries might depart from actual injuries is through injuries being reported when none is present. Providing rewards for reporting injuries encourages such behavior. Transport Canada defines *injuries* to "include all those who suffered any visible injury or **complained of pain**" (bold added).[15]

A BROADER MESSAGE

Data from a number of countries and sources show consistently that reported injuries can depart from actual injuries by large systematic amounts. This finding teaches two principles important to traffic safety. First, clear effects observed in data sets do not necessarily imply real phenomena, but may instead be due to data selection and definition. The second principle is more universal, and is well understood by economists, but often ignored, or even hotly denied, by others. The principle is that as the out-of-pocket cost of an activity increases, less of it occurs, while as the reward for an activity increases, more of it occurs. The empirical data show that this principle explains variations in reported injuries per fatality. The same principle applies to many traffic safety topics. If the cost of crashing increases, fewer crashes occur. If the cost decreases, say, because of insurance, more crashes occur. Any policy that increases the cost of drunk driving, such as increased alcohol taxes, reduces drunk driving.

SUMMARY AND CONCLUSIONS

Data from a number of countries, and spanning many decades, imply that the number of reported injuries may differ by large amounts in either direction from the number of actual injuries. If injury victims must pay for medical treatment themselves, it is likely that injuries go unreported. If reporting an injury can result in compensation, injury reports may be filed even in the absence of injuries.

It is insurance compensation and litigation that is responsible for most of the whiplash injuries reported in the US and Western Europe, not crash forces.

More information on topics in this paper is available in the author's book ***Traffic Safety***[3] described at

http://ScienceServingSociety.com/traffic-safety.htm

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* Chapter 1 -- Introduction
* Chapter 2 -- Data sources
* Chapter 3 -- Overview of traffic fatalities

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