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### Title

Accident Proneness: The History of an Idea

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<https://escholarship.org/uc/item/9rh9f29x>

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### Publication Date

2001-08-01

UCI-ITS-WP-01-2

**Accident Proneness:  
The History of an Idea**

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August 2001

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## Introduction

During World War I, the British government established the Industrial Fatigue Research Board (IFRB), later known as the Industrial Health Research Board (IHRB). The board was created because of concern over the large number of accidental deaths and injuries in the British war production industries. In the course of its dozen or so years, the Board investigated myriad aspects of the issue. One of these studies forms the basis of the present discussion.

In one of the volumes published by the Board, mathematicians discovered a curious anomaly in the data. To understand the importance of the discovery, we should remember that mathematical description of random events was at that time less than twenty years old. It began with work by von Bortkiewicz on death by horse-kick in the Prussian Army (Haight, 1967). In essence, von Bortkiewicz showed that the number of such deaths, per year per division, could be almost exactly described by the *Poisson Distribution*, a formula discovered for a different purpose nearly one hundred years earlier by an eminent French mathematician.

After the work of von Bortkiewicz, but before the establishment of the IFRB, the Poisson Distribution was applied to a considerable range of random phenomena: demographic data, gas molecules, telephone traffic, particles in solution, deaths from malaria, enteric fever and so on. With only trivial exceptions, the Poisson Distribution was a success: it fit the data to perfection. Furthermore, this was accomplished with only a single parameter to be estimated : the mean number of occurrences. <sup>1</sup>

By World War I, the Poisson Distribution was universally regarded as a unique characterization -- virtually a definition -- of complete randomness. Because of its theoretical foundations, it still is.

With this background, both theoretical and empirical, we can imagine the consternation among mathematicians working with IFRB when they found a distribution of accidents that was seriously non-Poisson (Table 1).

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<sup>1</sup> For details relating to this early period, see Haight (1967).

Table 1  
*Accidents to Women Working on 6-inch Shells*  
*February 13, 1918 - March 20, 1918.* <sup>2</sup>  
*(Poisson Theory)*

Number of Accidents	How Often Observed	Poisson Theory $\lambda = 57$
0	447	406
1	132	189
2	42	45
3	21	7
4	3	1
5 or more	2	0

From the perspective of statistics, something seems seriously wrong here. Either the accidents are not occurring at random, or the Poisson frequencies are somehow not behaving as theory and earlier data suggest that they should. Naturally, with such a tantalizing puzzle, a solution was soon found. A paper by Greenwood and Woods (1919) showed that a better fit was obtained with a different distribution. Called the “negative binomial” this distribution led to the following tabulation, using the same data (Table 2).

Table 2  
*Accidents to Women Working on 6-Inch Shells*  
*February 13, 1918 - March 20, 1918*  
*(Negative Binomial Theory)*

Number of Accidents	How Often Observed	Negative Binomial Theory
0	447	442
1	132	140
2	42	45
3	21	14
4	3	5
5 or more	2	2

For those who cared about such matters, this was a source of considerable satisfaction. <sup>3</sup> But the puzzle remained. Why wasn’t the Poisson distribution performing in the way it should? About a year later, Greenwood and the prominent

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<sup>2</sup> The periods were a composite of times, factories and perhaps work shifts.

<sup>3</sup> The negative binomial distribution has two parameters, and so would be expected to provide a better fit. Here I am trying to describe the reasoning of 80 years ago, not to justify it.

mathematician Yule discovered the answer and proposed a new theoretical model (Greenwood and Yule, 1920).

A brief description of the model goes as follows. The accident experience of each woman working with six inch shells is indeed Poisson – as everyone agreed it should be -- but with the value  $\lambda$  (the average number of accidents) differing from person to person. It was then a fairly simple calculation (and one that we now give as an exercise to students) to show that the negative binomial probabilities for the entire collection of workers must result. <sup>4</sup>

The whole episode might have been confined to the world of mathematics, but for the invention of an intriguing new term a few years later. The individual values of each woman's  $\lambda$  were now called her '*accident proneness*' and the story becomes more complex. <sup>5</sup>

### The Search for a Measure

'Accident proneness' had a nice ring to it. It seemed to exculpate those who were clumsy, kept the others from gloating and above all amused the public. One difficulty was that no one knew how to determine in advance its value in a given individual. Psychologists were challenged to devise a way to measure it, just as one measures height, weight and perhaps even intelligence. If each individual has a unique  $\lambda$ -value (his or her '*accident proneness*'), it should be a reasonably simple matter to find out what that value is by devising clever tests, perhaps physiological, but more probably psychological.

The search began as early as the work of the IFRB, initially by Farmer and Chambers (1926, 1929). In their first paper, the connection between accident experience and sickness was correlated with nine clinical tests. <sup>6</sup> Their conclusions were that "there is something called accident proneness, related to poor aestheto-kinetic coordination and nervous instability", but only slightly connected with sickness, and further that

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<sup>4</sup> One additional assumption is involved: that the distribution of lambda is what we now call the *gamma*.

<sup>5</sup> I'm not sure who should get credit for the name. My best guess is Ethel Newbold, or perhaps G. Udny Yule.

<sup>6</sup> Called simple reaction, simple coordination, choice reaction, complications, cover up, distraction, changed position, selective and dilemma.

there is a clear correlation between major and minor accidents.<sup>7</sup> The second paper continued with thirteen “psycho-physical” tests.<sup>8</sup> The conclusions deserve to be quoted: “Even when the tests are weighted, they fail to give correlations of sufficient magnitude to warrant the assumption that they measure more than a portion of the factors involved in objective criteria. . . . It is clear that the accident rate for any period less than the whole working life must be an imperfect measure of an individual’s accident proneness. . . . The longer the period of exposure, the nearer does the accident rate tend to be a true measure of accident proneness”.<sup>9</sup>

The search for a reliable measure of 'accident proneness' continued for some thirty years. Hundreds of papers were subsequently published, appearing in such prestigious journals as the *Journal of the Royal Statistical Society*, *Psychometrics*, the *Journal of Applied Psychology* and the *Journal of the Institute of Actuaries*. These authors covered the spectrum of fame up to and including Franz Alexander (Alexander, 1949). (The latter, by the way, decided that all accidents were deliberate, so that the elusive  $\lambda$  was a measure of a death wish rather than of mere clumsiness!).

Regretfully, none of the experiments produced a result in the sense of strong correlation with the accident experience of individuals. Perhaps the most convincing evidence may be found in the many volumes published by the IHRB, reporting on tests that had been proposed, each with its tabulation of results. One that is quite comprehensive (Newbold 1926) has thirty tables, at least half of which bear captions that begin with the words “Correlation between...” .

### **Public Acceptance**

Now the story takes a different turn. The expression 'accident proneness' leaked into the popular press, and thus into public opinion, and then into politics. The latter, in the United States, often means Congressional hearings. By 1938, a mere twenty years after its discovery, hearings before the United States House of Representatives produced

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<sup>7</sup> In the 1960's I was rather struck by this conclusion, and thought that the best clue to future serious accidents might be the scratches and dings on a vehicle.

<sup>8</sup> Called dotting, pursuit meter, choice reaction, interrupted pursuit meter, coordination, steadiness, rectangles, strip, cube, linguistic intelligence, number setting, stereoscopic and dynamometer.

<sup>9</sup> Newbold (1927) meanwhile set off on what one might call the “mathematical track”: trying to use the observed anomaly to suggest interesting mathematics, rather than using mathematics to crack the accident problem.

a sizeable document, *Motor Vehicle Conditions in the United States*.<sup>10</sup> This fat book contains a section "The Accident Prone Driver". The findings were rather far-reaching. For example, "There exists among the general population of drivers, a small group who are definitely accident prone and a much larger group who are just as definitely accident free. . . Given a drivers' histories . . . one can predict their histories in the other half of the experience, either prospectively or retrospectively."

For public agencies, accident proneness was an appealing concept. If accidents could be shown to be confined to this small proportion of "bad drivers", measures to combat accidents could be enormously simplified -- and cheaper, too. All that would be needed was detection and cancellation of drivers' licenses.<sup>11</sup>

The report goes on to justify accident proneness on the basis of contagion, specifically that the distribution of intervals between accidents "was not normal, as it would be if the first accident had no effect on the driver's propensity toward another."<sup>12</sup>

This view, that appeared to be the official U.S. view at the time, dominated political thinking and public opinion, not only in the federal government but also in state and local agencies even though it was almost entirely unsupported by any research available at the time. Since there was little to support the proposition scientifically, the ever-imaginative popular press invented something that I will call "The Great Percentage Fallacy".

This argument claimed, no doubt with good reason, that a small percentage of drivers were involved in a large percentage of accidents. This small number of villains was proclaimed to be the hitherto missing group: the 'accident prone' drivers. Of course, even in the beginning, no knowledgeable person believed this beguiling argument. There are many ways in which The Percentage Fallacy is wrong. I'll briefly mention three.

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<sup>10</sup> How did the obscure experiments of the IFRB cross the Atlantic and reach Congress so quickly and powerfully? I don't know for sure, but I suspect those rascals, the "safety activists."

<sup>11</sup> With the additional assumption, which subsequently proved to be insupportable, that unlicensed drivers would stop driving.

<sup>12</sup> Actually, the distribution of intervals in a Poisson process is exponential, a concept that is often difficult to grasp. Even Rutherford, in an early paper, tried to force the normal distribution on exponential data.

First, *any* model will involve unequal distribution of accidents, except when a supernatural power allots only one per customer. In this light, one might as well call the losers at Monte Carlo "poverty prone"!

The second argument against the fallacy is given by Moore (1956): "Attempts have sometimes been made to justify the concept of accident proneness by statements such as 'one-third of drivers have two-thirds of the accidents' . . . Statements of this type are no proof of the hypothesis, as the following example will show. In 1952, there were roughly 6 million licensed drivers in Great Britain and, in the same year, some 200,000 mechanically-propelled vehicles were involved in personal injury accidents. That is, not more than 200,000 drivers had accidents, from which we deduce that 4% of the drivers had 100% of the accidents. In one *hour* in 1952, on the other hand, an average of 25 drivers had accidents, that is, 0.004% of drivers had 100% of accidents."

With this simple example, Moore shows that the Fallacy depends on the length of the survey period. The supporting argument is time-sensitive; shorter periods give sharper contrasts. The relatively late publication of Moore's paper gives a clue that the official view was still widely accepted as late as the 1950s.

The third difficulty with the official version is even more important because it has implications for the entire 'accident proneness' concept and leads to a quite different model. Let's revisit the Monte Carlo example. For a person to be truly 'poverty prone', it would be necessary for that person to lose money not only today, but also tomorrow and the day after. In fact, in the original negative binomial formulation, this point has been entirely omitted. Suppose the 4% of drivers were involved in 100% of the accidents in 1952; how about 1953, where presumably another 4% of drivers were involved in 100% of the accidents? Was it the same 4% or an entirely new cohort?

In other words, if we plot the number of accidents experienced by an individual during one time period on the  $x$ -axis, and during another on the  $y$ -axis, would the points form an exactly straight line? If not, then what is the correlation coefficient? Is there, in fact, a meaningful correlation between the experience in consecutive time periods? In sports, including chess, there certainly seems to be because individual outcomes do not seem to be independent and random. The sense of 'accident proneness', on the other hand, implies a condition or personal quality that is relatively

stable over time. Otherwise there would be little sense in the exercise of finding its value for an individual.

This 'correlational model' was discussed in one of the last IHRB publications. Farmer and Chambers (1939) proposed the idea, but confessed that it is hard to interpret a correlation for non-normal data. Actually the data is not only non-normal, but based on samples too small to be meaningful, since an individual's accident experience does not usually reach statistically significant quantities.<sup>13</sup>

The argument between correlation and negative binomial models continued for a bit (see, for example, Maritz, 1950) but veered away into mathematical abstraction. The reason may be the conclusion of Blum and Mintz (1951): "The major effort is to reduce accidents, not to wait for successive periods."

There were many mathematical results arising from the 'accident proneness' hypothesis. For example, Irwin (1941) proved that the negative binomial distribution could equally well be obtained from a hypothesis of 'contagion', meaning that each accident increased the probability of another. Arbous and Kerrich (1951) call this the "burnt fingers model" and continue to provide an elaborate mathematical analysis, including its implication for bivariate analysis.

### **Interlude: Why so Popular ?**

The slogan 'accident proneness' was an instant hit with the public in the 1920s and 1930s, and to some extent still resonates today in the popular press. Having been taken up by the public and the media and confirmed by the government, the lack of evidence was irrelevant. Principal investigators could be depended on to provide that.

This might be a good time to point out that the safety profession does seem to be somewhat slogan-prone. In addition to 'accident proneness' we have suffered various other pithy mottos: 'defensive driving', 'road rage', 'killer drunk', 'risk homeostasis', 'Holiday Death Toll', 'zero vision', 'road hog' and so on. Any one of these might be a good topic for a dissertation on popular culture in some department of anthropology.

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<sup>13</sup> At one time I tried to overcome the sample size difficulty by correlating entire towns' accident totals in two consecutive years. As I remember, some 500 towns with populations varying from 10,000 to 25,000 yielded a correlation coefficient of approximately 0.017.

In support of the hypothesis, there were many people both in and out of government willing and able to produce reports of success. Academic scholars have been known take a sample of their students, administer a test to find out which ones were 'accident prone', and then follow up the crash experience of those sampled. The results were typically not positive, and frequently so inconclusive as to indicate that more research was needed to locate this elusive quantity.<sup>14</sup>

In the domain of popular opinion, 'accident proneness' became a household word. It seemed to have the backing of government and was frequently found in press coverage of accidents. By the time it was disproved in the 1950s, 'accident proneness' was an everyday expression. It had the advantage of being "proved" by bar charts and endorsed by impressive mathematical expressions. It seemed a panacea for traffic safety -- the "cheap effective measure". It still lingers as a generic expression for personal injury. If A drops a brick on B, then A is guilty, but B is 'accident prone'.

### **Destruction of Accident Proneness**

Shortly after World War II, authors of scientific papers began to destroy all the ideas on which 'accident proneness' had been based. After all, the entire concept was balanced on the point of a pin - IFRB research. Subsequent papers that supported the idea were clearly vulnerable -- logically, statistically and practically. Several papers of the period consisted mainly of wrangling between proponents of the single variable model (whether Poisson or exponential) and those of the 'correlational' model.

One of the first dissents was by Johnson (1946). In this important paper he critiques and criticizes over two hundred studies. He shows that the statistical analyses were almost all invalid, inappropriate, inadequate or irrelevant and asserts that the conclusions were logically unwarranted. He discusses the weakness and absurdity of psychological tests of driving ability. Very little of the buoyantly confident literature of the pre-war period escaped Johnson.

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<sup>14</sup> "The tendency has always been strong to believe that whatever received a name must be an entity or being, having an independent existence of its own. And if no real entity answering to the name could be found, men did not for that reason suppose that none existed, but imagined that it was something particularly abstruse and mysterious." (*Anon.*)

The 1950s were a rich time for careful analysis of the 'accident proneness' concept. Arbous and Kerrich (1951), although perhaps not as impetuous as Johnson, nevertheless carry on in much the same vein, debunking the literature of the 1920-1938 period. They are aware of the IFRB literature and praise Newbold, Greenwood and Yule; the first of these "must still be regarded as almost complete summaries of our existing knowledge of this phenomenon." The uncritical acceptance of proneness as a proven fact is attributed to Farmer and Chambers; if this is true, one must add that they had many helpers. The relationships between causes (contagion, proneness, chance) and effects (Poisson, negative binomial, correlation) are also analyzed as to the logical structure.

Some of Arbous' conclusions are: " .. the 'accident-prone Percy' is a figment of the imagination resulting from wishful thinking" . . . . "It is to be lamented that statements of this type should be allowed to acquire a mantle of respectability by being accepted for publication in journals of repute." . . . . "Self deceit of this type has never produced any results worth having." Strong words, indeed, but in my view quite justified. Needless to emphasize, he believes that the idea of removing the high accident people from the population will have little or no effect on subsequent accident totals.<sup>15</sup>

Adelstein (1952) covers much the same ground, with many of the same conclusions, but in his case based on analysis of primary data. He chose the most hazardous occupation that would provide an adequate sample size (1,452 accidents): men working as shunters on the South African railways in relatively homogeneous circumstances over an eleven year period. He found that age and experience had an effect on accidents among those recently employed, but that the effects rapidly diminished. Adelstein also studied home accidents of the shunters as well as the correlation between time periods.

In a dense paper of 56 pages, he covers many aspects of the problem: "With respect to injury, during the first five years, chance factors are sufficient to explain the data. Those men who have been observed for five years show more evidence that proneness plays a part, and in those 122 men observed for eleven years, there is stronger evidence for differing degrees of proneness. But the correlations between different periods is small, and the factor of proneness . . . is probably of small practical importance, in comparison with chance factors which play a predominant part".

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<sup>15</sup> Although I did write a paper on the subject (Haight, 1965).

"There is no evidence of correlation between industrial and home injuries, or between industrial accidents which cause injury and those which cause damage to property. There is no evidence of correlation between minor and major injuries. . . . No good evidence is available that the same men tend to repeat the same kind of accident. When men who have most accidents in their first year of exposure are compared during the next three years with the remaining men, it is found that the mean rates do not differ significantly".

### **The Counter-Revolution**

By the late 1950's, 'accident proneness' had fallen into disuse in serious discourse, and into disrepute as well. <sup>16</sup> It is difficult today to use the expression in the title of a paper and expect that paper to be published in a good journal. Let us reflect back on the problems encountered in using 'accident proneness' as an explanatory variable. First, beginning with IHRB material, the "differences", for example, between Table 1 and Table 2 were so small as to be inconsequential. Second, there has never been a clear-cut and successful way to measure proneness. Finally, the craze during its early years was so extreme and then so violently refuted that it is hard to take seriously now, at least in academic circles. But the expression continues in popular usage, and may continue to do so for some time. It seems to me that this may be due, in part, to the reluctance of the vernacular press to feature negative results.

In my opinion, the negative result does not mean that 'accident proneness' does not exist; actually, I would guess that it does. After all, some people do have clumsier fingers than others. I think that perhaps the clumsy ones find a way to compensate so that we can't easily identify them. We would then have a quantity  $\lambda$  that exists but that can't be measured. I don't see any logical difficulty with that. I am sure that there are plenty of things that exist, but can't be measured: love, for example, and duty.

As accident proneness faded, it was struck a fatal blow by the emergence of two charismatic figures, Ralph Nader and William Haddon. Nader, in a best selling polemic showed that vehicle design was important, and by implication, that driver characteristics much less so - and in any case less amenable to manipulation. Haddon followed from

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<sup>16</sup> Once 'accident proneness' was so wounded, the hounds swarmed denying it, deriding it, mocking it, and slamming it in every possible way.

an important government position.<sup>17</sup> In one of his most powerful images, he asked why, if we can ship eggs without damage, we can't do the same with people. (Let's all take a moment to think about the answer to that). One can see these ideas developing in an early paper (Haddon, 1961): "As accidents under present conditions are inevitable and will be to some extent for a long time to come, the vehicle should be designed to be fail safe, that is to be safe to have accidents in." Somewhat later in a 1970 paper, after leaving the Federal government he wrote "It is basic to the reduction of highway losses that there is no present evidence that vehicle crashes can be eliminated or even adequately reduced in numbers in the foreseeable future" (Haddon, 1970). This guess has certainly been borne out thirty years later.

There was also political support. In a glowing tribute to Haddon's theories, the popular senator Daniel Patrick Moynihan wrote in 1964: "Accidents are the only remaining major cause of human death and disablement still substantially viewed by educated and uneducated alike in terms of ignorant superstition" (Moynahan, 1964). This is quite a good summary of 'accident proneness', if that is what he had in mind.

The emphasis on injury avoidance tended to discount human factors in general, and the role of the driver in particular. "Accidents will happen"<sup>18</sup> was the philosophy. Diagnosis and removal of drivers has been shown to be unproductive, so the argument went. Instead, let's focus our efforts on vehicle crashworthiness and occupant protection.

The 1960s, 1970s and 1980s were the heyday of vehicle, roadway and environmental modification. These efforts are undoubtedly well-known to readers and were, it is fair to say, overwhelmingly successful. That story is told in many other places and I don't propose to recapitulate the details.<sup>19</sup>

### **The Driver Again Emerges**

Within the past fifteen or twenty years, the driver as a factor in accidents has gradually started to re-emerge providing us, among other things, with some interesting

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<sup>17</sup> Director of the National Highway Safety Board, forerunner of the present National Highway Traffic Safety Administration.

<sup>18</sup> Curiously enough, also the slogan of the earliest days, before IHRB.

<sup>19</sup> I will, however, insert a published remark of one NHTSA administrator of the period: "I think we can almost eliminate deaths on the highways, except for pedestrians and those caused by dune buggies and motorcycles."

comparisons with the 'accident proneness' period. I believe that there are at least two important trends that reinforced each other to produce the renaissance of the driver.

First, as occupant protection became increasingly successful, and at the same time increasingly costly, the opportunities for improvement became smaller and smaller.<sup>20</sup> By the 1990s, roads and vehicles in industrialized countries were generally in quite good condition. Seat belts, air bags and frangible construction, combined with driving style, maturity, an aging population and increasing urbanization have all lowered injury and fatality measures. This is particularly true for those measures that are based on the distance of travel. Although widely denied, it does seem to me that the twilight of occupant protection is approaching.

The second factor was the re-discovery of a suitable villain that everyone could blame. This time it was the drunken driver, or occasionally even the drinking driver.<sup>21</sup> This tendency was strongest in places where alcohol was already under suspicion as morally unwelcome. Reading the published literature on drunk drivers is strangely reminiscent of the similar literature on 'accident prone' drivers, and makes one speculate about the differences and similarities of these two grievous attributes. What is the difference between the drunk driver and the 'accident prone' driver?

First, we can measure the former but it is difficult, if not impossible, to measure the latter. This means that compensation after drinking is not quite as easy as compensation for clumsy fingers. If I am not gravely mistaken, compensation possibilities after drinking have never been seriously investigated.<sup>22</sup> It may work to increase safety (Drive Carefully!) but cannot always escape alcohol detection devices. The 'accident prone driver' could presumably escape, since no reliable test has been invented.

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<sup>20</sup> Of course, the automobile industries and the safety establishments have new and glamorous devices on their drawing boards. It seems to me unlikely that these devices, even at their best, will change the safety in cars as much as they did in the preceding half-century.

<sup>21</sup> Not entirely, however. The speeding driver, the aged driver, the youthful driver and the discourteous driver have also had their share of attention, and very recently the angry driver. Note that angry, youthful and discourteous are bad people; aged people are 'accident prone'.

<sup>22</sup> There have been many studies using alcohol and steering around traffic cones, but none I believe offering \$1,000 for a successful completion of the trial.

Second, the 'accident prone' individual, if he or she exists does not bring the moral outrage attached to the drunken individual.<sup>23</sup> If anything, people who stumble or who have clumsy fingers are sometimes even looked upon sympathetically. Clumsiness is excusable, drunkenness is not.

Third, the treatment that society sends to the drunk driver is overwhelmingly punitive, whereas the 'accident prone' driver might be told simply to "be more careful next time". It is obvious that the law treats them differently. Punishments for drunk driving are among the most severe, even when no damage or injury is involved.

There is another factor to consider when guilt is assigned after a crash. Police officers are trained to observe the driver<sup>24</sup>, and more specifically the 'alcohol involved' driver. They typically do not look for the cause of accidents in road design, vehicle ergonomics or similar factors. Accident reporting forms have boxes for 'driver' and 'drunk' and these are easy to check off. Further along the path of justice, a drunken driver is usually easy to convict. The incompetent traffic engineer (who by the way might also have been drunk) is not.

## Conclusion

In my view, if we want to construct a model of the causes of road accidents (as distinct from the cause of any individual accident), we could consider a triage: (a) those factors that are internal to the driver, (b) those that are specifically related to road, vehicle and other environmental factors and (c) those that are neither, but purely random. In this context, random means "cause impossible to know".

My experience, both with the concept of accident proneness and with the statistics of the Poisson distribution, convinces me that factor (c) has been greatly underestimated both in public understanding and in research. Unfortunately many people think that if no blame can be attached no countermeasure can be prescribed. This is shown to be false by our experience with lightning rods; we don't know where and when lightning will strike, or what is "at fault". The lightning rod is analogous to the seat belt in ameliorating the effect of a class (c) event.

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<sup>23</sup> The public image of drunk drivers involves injuring others rather than themselves. This image has been ruthlessly exploited in public relations campaigns.

<sup>24</sup> A policeman once told me "I don't care if there is a ten foot hole in the middle of the road, it is the driver's job to stay out of it."

I think that even the IHRB tabulations of eighty years ago are near enough to Poisson to suggest events that are random, and which we therefore call accidental. If this is true, the search for the 'accident prone' person was essentially looking for not only for a needle in the haystack, but also for a scapegoat.

But that was factories. What about road traffic? I believe that especially in modern sophisticated traffic, (c) is a larger factor than we are usually willing to admit. The further we go back in motorization, whether in the history of a specific country or in the collection of countries worldwide, the greater the importance of (a) and (b). The goal would eventually be that 100% of accidents are in category (c), modeled exactly by the Poisson distribution.

*Caveat:* In this paper, to avoid plunging into mathematical digression I have used lambda ( $\lambda$ ) to denote both the average value (in the tables) and an individual value (in later discussion).

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