Accident Proneness, or Variable Accident Tendency?

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INTRODUCTION

The term accident proneness is current in both medical and lay press, but it is not always used in the same sense, nor is one always confident that many who employ it so freely are aware of the exact assumptions contained in their definition To many, although not to all, the designation of a person as accident prone implies that, irrespective of environment, that individual is more likely at all times to incur an accident than his colleagues even though exposed to equal risk, and that this is due to some characteristic or summation of characteristics associated with corporeal dexterity, sensori-motor skill, personality, or higher conative or cognitive function. In short, accident proneness is conceived as an immutable load to which the unfortunate possessor is chained like some Ixion to his wheel The corollary is, that the population can either be dichotomised on the basis of the possession or non-possession of the characteristic, or at least ranked in terms of its severity

How this widely held concept achieved the status of an axiom, as it did for thirty years, is a salutary lesson to all research workers stemming as it does mainly from inadequate appreciation and grasp of the fundamental research In this paper an attempt is made to explain, firstly why such misinterpretations arose, secondly to present an alternative hypothesis of accident distribution among a population at risk, using for this purpose data derived from Public Transport drivers in Northern Ireland, and thirdly to compare the respective abilities of these two hypotheses to explain the data

ARGUMENT

The psychologist, Dr May Smith, wrote in 1943 "When during the last war [First World War] two statisticians had their curiosity aroused by some accident figures in munition works history was made" (Smith, 1943) This might seem an hyperbole, certainly May Smith did not mean to imply that these two statisticians, who can now be identified as Major Greenwood and Udny Yule, were the first to suggest that some people had clumsier fingers, slower reflexes or less alert brains than others , she was referring to the now classic fact that these two were (so far as is known) the first to thoroughly examine the laws of probability governing the distribution of accidents amongst a population whose members are exposed to an equal risk of incurring an accident Greenwood was the dominant partner in this context and it would perhaps be advantageous to follow his reasoning from the beginning

The abnormal industrial conditions of the First World War were associated with an increased accident rate in many branches of industry, and it was frequently observed that when the working hours were reduced accidents decreased disproportionately (Vernon, 1918) This was attributed to the cumulative effect of fatigue which at that time was considered a purely physical phenomenon This explanation did not altogether satisfy Greenwood, who had ample experience of aircraft accidents through his membership of the Committee of the Wartime Flying Service (Greenwood, 1927), and at the War's end he used industrial data, collected by Miss Hilda Woods and her colleagues, to reappraise the theories of accident causation (Greenwood and Woods, 1919).

The first theory he considered was that accidents were accidents in the strictest sense and their allocation to human beings at risk was analogous to the throwing of a six or the dealing of an ace In this event the statistics of multiple accidents would conform to a type of pure chance distribution similar to Poisson's (1837) theoretical distribution which, in the accident situation, is given by —

$$P(r)=e^{-\lambda}.\frac{\lambda^r}{r!}$$

where $P(r)$ is the probability of an individual incurring r accidents, and λ is the mean number of accidents Such a series seems to have been first applied by De Moivre (1718), and it has been obtained independently by other investigators (e g , "Student", 1907 ; Bateman, 1910) ; in fact the path to the limit is simple, and, as von Bortkiewicz (1898) has remarked, it does not need a mathematician of Poisson's ability to reach it Greenwood obtained the Poisson series when using the schema of pigeon-holes being bombarded by billiard balls and then arguing by analogy to accidents being distributed amongst persons There are, of course, *a priori* objections to such an analogy, to which Greenwood (Greenwood and Woods, 1919) readily drew attention, e.g , the assumption is made that a particular number of accidents *must* happen to a particular number of people, the only problem being to allocate them, whereas this just does not hold in the accident situation However, in practice, when the chance of incurring an accident is small, it serves the purpose.

The divergence of each observed accident frequency distribution from that calculated from Poisson's limit, in Greenwood's original data (Greenwood and Woods, 1919), is considerable, as Table I (based on one of the original populations) illustrates.

The first obvious modification of the theory is, that after incurring an accident an individual is subsequently more or less likely to incur another, i e., first accidents distributed at random, subsequent accidents by some other law of probability. In the pigeon-hole schema this is analogous to arguing that the chance of any pigeon-hole receiving a ball is not independent of the number it already contains, i e , it is not constantly equal to the reciprocal of the number of pigeon-holes as it was in the Poisson series Again there are *a priori* objections to the analogy employed, e g , if a pigeon-hole receives a ball, then, by virtue of the theorem, its opening must become bigger (or smaller) so rendering neighbouring pigeon-holes smaller (or bigger) since the total volume of the pigeon-holes is a constant. In the accident situation this is analogous to arguing that the incurrence of an accident will render the other population members *ipso facto* less (or more) liable to injury, which is an absurdity.

In practice the emergent theoretical distribution (Greenwood and Yule, 1920) is troublesome to apply, but even when modified to a more useful statistical form by assuming only two levels of accident liability, viz , that before an accident and that after an accident without variation in either (in this form it successfully graduates some of the observed statistics (Table 2)) this should not be interpreted as substantiation of the theory Greenwood termed this distribution the " Biassed Distribution " ; its main application is in supplying a theoretical fit to the unusual distribution in which the mean exceeds the variance.

The next obvious modification was that of *ab initio* differentiation among the population by which it is supposed that all workers did not start equal, but that some were inherently more liable to incur accidents than others On the pigeon-hole scheme this is analogous to supposing that the size of the opening of the pigeon-hole was a continuous variable But what form should this variation take ? On the purely practical consideration that such a distribution must be skew, Greenwood assumed λ, the parameter of the Poisson, to be distributed in the form of a Pearsonian curve of Type III The selection of this type from the number of skewed curves available was quite arbitrary and was largely for the technical reason that, being in binomial form, its equation was well suited to the mathematical manipulations required. This emphasis on technical considerations has been insufficiently stressed, but Greenwood himself puts it unambiguously : " The choice of the binomial curve to represent the distribution of the continuously varying liabilities throughout the " population " has been dictated by considerations of practical convenience. An infinity of skew curves fulfilling the required conditions might be imagined, but no objective evidence favouring one more than another can be produced " (Greenwood and Yule, 1920).

The resultant distribution was a negative binomial termed, accurately but perhaps unfortunately, the " Distribution of Unequal Liabilities " (Greenwood and Woods, 1919) As a general rule it graduates published data appreciably better than the Poisson (Table 3). In the 14 sets of data published by Greenwood and Woods (1919) the average value of P (for graduation) was 0·38 (" P " represents the probability that the divergence between empirical and theoretical distributions can be due to chance) Thus a high value of P indicates excellent agreement with theory and a very low value suggests that the theory may be inadequate From the magnitude of the value obtained, and other evidence, Greenwood concluded · " The hypothesis comes reasonably well out of the test " (Greenwood and Yule, 1920).

The formula for the proportion of the population expected to have r accidents is given by—

$$P(r)=\left(\frac{c}{c+1}\right)^{p}\cdot\frac{\Gamma(p+r)}{r!\cdot\Gamma(p)\cdot(c+1)^{r}}$$

where p and c are determined from the first two moments Mean$=p/c$, Variance$=(p/c)+(p/c^2)$ Γ=the Gamma function

Greenwood published his results with all the appropriate qualifications Thirty years later he published his only other work on accidents, of which we are aware, an article in *Biometrika* which appeared posthumously and was the last completed article he ever wrote (Greenwood, 1950) Accident statistics were certainly the poorer for this long period of quiescence

Since in Greenwood's original work he obtained satisfactory concordance with observed data using the Distribution of Unequal Liabilities, many investigators, but not Greenwood himself, have assumed that the existence of unequal initial liability to accident is established, and it should therefore be possible to detect persons prone to accident *before* they incurred any accidents at all This in fact has been a major objective of accident studies for the last 40 years. Discriminant tests for the " accident prone " range from the extremes of measuring corporeal dexterity to investigating the psyche, and although statistically significant correlation coefficients between test performance and accident score have been obtained from time to time, no test or battery of tests has so far been identified which will enable one to exclude the highly accident prone *without* also excluding a large proportion whose subsequent accident record suggests that they were not specifically bad risks

The failure of this pragmatic approach initiates two logical trains of thought Firstly, it is an obvious snag because we cannot now, by mathematical methods alone, ascertain the true distribution of the variable which Eric Farmer and E. G Chambers (1926) christened accident proneness, because only the actual distribution of accidents is known ; we can only hazard a guess Miss Ethel Newbold's (Newbold, 1926 ; 1927) considerable theoretical consideration of the Negative Binomial distribution could not establish that the choice of the Pearson Type III, which is in the transition range of Pearson's skewed curves, for the requisite skewness was a good one ; she could only say that it was mathematically reasonable Secondly, since the theoretical implications have not been adequately supported in practice, it is only logical to examine in more detail the known alternative hypotheses by which the Distribution of Unequal Liabilities may arise.

The first alternative is contained in the assumption of a population homogeneous for risk of incurring an accident Although a Negative Binomial can arise in the way described, it can also arise if the components aggregated were Poissons, unless of course they were identical Poissons In practice this means that, if the population in fact comprised several sub-populations each being differently exposed to risk and in each of which accidents were chance determined, a Negative Binomial distribution will emerge In fact the difference in the λ's (the mean number of accidents) need only be a small order. This fact has not been stressed in the literature, but it is of great practical significance because perfect homogeneity to accident in a reasonably-sized group is an almost unattainable ideal.

The second alternative hypothesis is, that if the probability of sustaining an accident is altered by the incurrence of a previous accident, then a Negative Binomial may again arise if the law of change for the probability is suitably chosen (Irwin, 1941) More particularly the same distribution can arise on the hypothesis that people start out alike but that having had one accident makes an individual more likely to have another, but that this change is not the same for all

A third alternative way in which a Negative Binomial distribution can arise is as follows Suppose accidents occur at random, and if they be classified according as they give rise to 1, 2, 3, etc , individual accidents, then the expected frequency of equal time intervals containing 0, 1, 2, 3, etc , individual accidents can on occasions be Negative Binomial in form This was demonstrated by Luders (1934). But this is a distribution of time intervals containing 0, 1, 2, 3, etc , accidents to individuals *not* the distribution of persons having 0, 1, 2, 3, etc , accidents each in a given time interval, which is the present problem However, if the probability of a particular person incurring an accident is proportional to the total number of persons involved, but is otherwise equal and immutable for all, the distribution will still be of Poisson form (Irwin, 1941) There is a fourth alternative hypothesis which will be discussed later

There are doubtless other hypotheses on which a Negative Binomial can arise, but each of the above four has some relevance to the accident situation, and, if a Negative Binomial adequately graduates an observed frequency distribution, the conclusion that " proneness ", i e , the first hypothesis considered, must be operative is a *non sequitur* It is unnecessary to emphasise that statisticians of the calibre of Greenwood, Yule and Newbold did not fail to comprehend the simple logic of inverse probability. To quote Greenwood again : " Our solution of the problem of *a priori* differentiation [accident proneness] was empirical, in the sense that our only justification of the particular choice [of the Pearson Type III curve] was that it ranged from $\lambda=0$ and led to a statistically useful form. We did not suggest that no law connecting λ [proneness] with r [number of accidents] on the other hypothesis would give an identical graduation " This warning is unambiguous, but it is patent from the published work of the last 40 years (and there must be more than 1,000 such works, dealing specifically with accident distribution, over the period in the English language alone) that the statistical bases have been widely misinterpreted

For the present let us suppose that the desiderata of the alternative hypotheses by which a Negative Binomial can arise are not in fact operative, i e , that the group *is* in fact truly homogeneous for risk, that after incurring an accident the subsequent probability does *not* conform to a suitable law of change, and that the probability of sustaining an accident is in no way proportional to the number of persons at risk ; then, if an accident frequency distribution is Negative Binomial in form, there is a possibility that in fact " proneness " not only exists but by itself explains the entire skewness. Accepting this possibility has worried some thoughtful clinicians because, *ex hypothesi*, each person's " proneness " is constant through time, a fact not always consistent with clinical observation, e g , it is now well established that experience and age, acting independently, can influence the probability of incurring an accident * Also, it allows no substantial "within person" fluctuation in individual susceptibility, in fact it is an hypothesis in poor accord with most concepts of the dynamics of human behaviour For this reason the authors have been tempted to favour the view that the concordance of the Negative Binomial to the published accident frequency distributions was due more to the heterogeneous nature of the data than to the Pearson Type III distribution of λ Greenwood, Yule, and Newbold, had all used industrial accidents as ascertained through first-aid departments, such data are often unreliable for four main reasons

1 There is invariably unknown heterogeneity to risk amongst industrial workers, even amongst those listed as being in the same trade or the same shop

2 Especially with minor accidents there is confusion of "tendency to report accidents" with "tendency to have accidents", producing an ascertainment which is a hybrid

3 The effect of absence on the exposure to risk component. The investigators did make some allowance for absence, but their period of observation was often extremely short, e g, as little as 5 weeks in the case of some of Greenwood's munition workers (Greenwood and Woods, 1919)

4 The ascertainment is not independent of severity.

These investigators were of course fully aware of these limitatons, but they conceived them, certainly in public, more as theoretical points than as pertaining to the data which the field workers collected. Such loyalty to colleagues is laudable, but, unless industrial recording of minor accidents was more valid then than now, and the homogeneity of risk rather better than has been supposed, their elegant statistics were being applied to less than elegant data.

Data

Because of the deficiencies of much industrial data for an investigation of the distribution of accidents amongst a population at risk, when such a study was inaugurated in Northern Ireland it was decided to use transport accidents Since the study was retrospective, and only limited funds available, it had to be those incurred by drivers in Public Transport Authorities Here, hours driven are known; accidents are more or less completely ascertained, a fairly long retrospective period of time can be taken depending upon the available records, and such factors as absence, overtime, etc, recorded * These considerable advantages are partly discounted by the facts that accidents to bus drivers are extremely rare events, e g, the average number per driver per year in Northern Ireland is about 0·75, and that the group is a highly selected one. This latter has three main drawbacks; firstly, results obtained cannot logically be extrapolated to the general driving population; secondly, the chance of identifying personal qualities which correlate with accident experience is reduced because the "worst" individuals are syphoned off more rigorously than in general industrial occupations; and thirdly, statistically it is difficult to compare the components of the distribution with any other, i e., to standardise them, unless of course proneness is obliging enough to have a gamma function distribution in which case Pearson's

*For a more detailed discussion see Cresswell and Froggatt (1962)

"Tables of the Incomplete Gamma Function" are of value. But the advantages far outweigh these drawbacks. However, it must be stressed that the findings relate only to the groups studied and in no ways are they to be construed as applying either to the general driving population or to accidents in industry.

It is customary at this stage to describe the data to be used and establish their validity * This is of little interest *per se* and is beyond the scope of this paper, it is sufficient to say that everything possible has been done to satisfy the most stringent criteria The only point demanding reservation is that of equal exposure to risk Without a degree of prospective control over the experimental populations, scarcely attainable in practice, a theoretically desirable experiment, e g, along the lines of a Latin Square, cannot be conducted, but it seems likely that the heterogeneity in the groups selected is small. Also, although *comparaison n'est pas raison* rather more has been done to ensure homogeneity of accident risk than has been attempted in most other investigations

Results

Six populations were isolated, viz., 4 in the U T.A, B.C T. bus and B C T. trolley-bus (Table 4) All drivers who were in continuous employment over the entire four year period 1952–1955 and who were not absent for more than 10 weeks in either two year sub-period, were initially admitted Each population was subsequently further restricted by omitting very inexperienced drivers, i e, those with less than about 18 months experience, and also those near retirement, because of the independent influences of age and experience (Cresswell and Froggatt, 1962) Lastly, two different dichotomies by "type" of accident were recognised and significant correlation coefficients between their incidence obtained (Cresswell and Froggatt, 1962) This logically justified "pooling" of all accidents Negative Binomial distributions were compared with the data derived from each of the populations over the four year period and each two year sub-period, a total of 18 tests Of these the Negative Binomial graduated the data adequately in every single instance Table 5 illustrates the concordance achieved by the theoretical distributions used in this study, with the accident frequency distribution obtained from the largest of the populations

The satisfactory reproduction of the observations by the Negative Binomial was perhaps surprising, because whatever justification there may be for assuming a liability constant through time for the "industrial" situation, and that is slight, there seems none for the traffic situation Consider it carefully. Accidents to bus drivers are broadly of three types · firstly, those entirely the driver's fault; secondly, those partly his fault; thirdly, those entirely someone else's fault. Those in the first, and some in the second category, *may* be attributable to "proneness", but many in the second and all in the third, cannot. There is nothing heretical in these assertions; Greenwood gave them legitimacy when he wrote: "Any statistics of accidents must contain a number of events which have no connection with the personal qualities of the exposed to risk We may safely infer from what we know already that the proportion is not large, but it must vary with

*For a full discussion see Cresswell and Froggatt (1962).

the occupation " (Greenwood, 1950) Although not large, perhaps, amongst Greenwood's groups in what one might call " closed " communities where the number of accidents directly involving a third party is small, on the roads swarming with other drivers, pedestrians, cyclists, etc , the proportion must surely be sizeable Because of this it seems reasonable to argue that the chance element, as applied to each individual, cannot be ignored, and that adopting a λ as a constant through time, as Greenwood had done, would be unreal in the traffic situation. This led to the formulation of two alternative hypotheses. The first, that accidents occur to drivers exclusively in " spells " or periods of time These " spells " as entities are randomly distributed in the population, and such that all accidents incurred are randomly distributed within each spell This is analogous to saying that all drivers are equally likely to incur a spell, and, having incurred a spell, the accidents within it do not conform to any non-random pattern. It is not irrelevant that the limits are from zero, i e , a spell may have no accidents within it. The resultant distribution (The Long)* is a compound Poisson of the following form.

$$P(r)=\exp\left[\lambda\left(e^{-\theta}-1\right)\right]\cdot\frac{\theta^r}{r!}\sum_{k=0}^{r}\left\{\left[\begin{matrix}r\\k\end{matrix}\right]\frac{(\lambda e^{-\theta})^k}{k!}\right\}$$

λ is the probability of a driver suffering a " spell ".
θ is the probability of a driver incurring an accident within a " spell ".

Although this adequately graduated the data derived from the 18 populations in the present study (Table 5) and has a statistically useful form, its acceptance would have involved a violation of principle since it does not adequately describe clinical hypotheses of traffic accident causation, more particularly it allows no accident outside a " spell ", a very severe restriction when there are other drivers on the road. Clearly, chance accidents, i e . those in which the other person's culpability is either complete or in practice very great, must occur, and it seems reasonable to suppose that they occur at random (in an equal-risk population) and completely independent of " spell " accidents This leads to another compound Poisson (the Short) which adequately graduates the present data in 17 of the 18 distributions (Table 5) It has the following form.

$$P(r)=\exp[\lambda(e^{-\theta}-1)-\phi]\sum_{j=0}^{r}\left\{\frac{\phi^j}{j!}\cdot\frac{\theta^{r-j}}{(r-j)!}\cdot[r-j]\right\}$$

where :—

$$[r]=\sum_{k=0}^{r}\left\{\left[\begin{matrix}r\\k\end{matrix}\right]\frac{(\lambda e^{-\theta})^k}{k!}\right\}$$

ϕ = Probability of incurring an accident independent of a " spell " accident.

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*This can be shown (Cresswell and Froggatt, 1962) to be formally equivalent to the Type A distribution of Neyman (1939).

This *embarras de choix* poses a common problem, viz , several hypotheses explaining the data as presented, plus, naturally, the infinite number of unformulated ones which might do so as well or better For the moment discussion is restricted to a narrower field, viz , the respective abilities of the Negative Binomial, which, as derived, supposes constant " proneness ", and the " Short " distribution which does not, to explain the present data Since they are both in satisfactory concord with the observed distributions, the Negative Binomial perhaps apparently slightly more so, other evidence must be considered The principal sources are two, viz , the correlation between accidents in different time periods, and the clinical findings in groups of drivers The correlation between accidents in different situations, e g , at work and at home, although it is logically part of " proneness ", is ignored because relevant reliable data are impossible to obtain. After this the Short distribution is considered in more detail

The Comparison

The pioneer investigators clearly recognised that if " proneness " was constant through time, as they hypothesised, the correlation coefficient (r) between the number of accidents incurred in any two observational periods of reasonable duration, should be significant and reasonably stable provided exposure to risk remained unchanged. Generally the early industrial results confirmed the significance, although not the stability, with values ranging between +0 2 and +0·7 and being many times their probable error (Greenwood and Woods, 1919 , Newbold, 1926). Newbold (1926) in particular stressed this finding as confirmation of the stability of " proneness " Transport data have been more difficult to garner and are generally less remarkable Table 6 displays data from the London Transport Executive (Farmer and Chambers, 1939), Table 7 a sample of those from Helsinki (Hakkinen, 1958), and Table 8 an example of those from the present study. This latter illustrates the order of the coefficients usually obtained, and their moderate variability

Since in the present instance one year is a short period of observation, the two two-year sub-periods, 1952–'53 and 1954–'55, were taken as the units and the correlation coefficients for the 6 populations calculated (Table 9) Column (*c*) is the value which would obtain if the Bivariate Negative Binomial distribution (Arbous and Kerrich, 1951) were the underlying model. Column (*d*) is the standard error derived by customary theory , Column (*e*) the value the standard error would attain if there was zero correlation between the variables. Now it is difficult to evaluate precisely the significance of a correlation coefficient in the present instance (where neither variable is normally distributed), but since the coefficient is, in population order, 6·56, 2 43, 2·65, 4·08, 3·66, and 4·83 times its standard error, and these standard errors are of the same order as those of zero correlation, the coefficients obtained are likely to be real Consequently it may be provisionally assumed that the number of accidents an individual driver sustains in any period is not independent of the number he sustained in the previous period

Employing the two-year sub-periods the regression of the following period's accidents on the preceding period's could be reckoned as linear in each of the 3 populations, viz , U T A., B C.T. bus drivers, and B C T. trolley-bus drivers, and with similarly sloped regression lines (e g , Graph 1) Detailed discussion is necessary but inapposite in this paper , readers are referred to Cresswell and Froggatt (1962) The general conclusions drawn are, that the findings of correlation and regression can be construed as supporting either hypothesis With the former hypothesis this is patent , with the latter, or "spells" hypothesis, it is not so apparent but it must be understood that the concept of spells does not require the number of accidents incurred in one period to be independent of the number in any other Since small correlations or variations may in themselves only indicate an unknown heterogeneity to risk throughout the period of the study, it might be imprudent to place too inflexible an interpretation on the correlation coefficients in the present instance

The second item of evidence lies in the examination of the men themselves. For this purpose two groups of B C T drivers were selected The first comprised those drivers who appeared in the worst accident group in both two-year sub-periods, broadly speaking they had 8 or more accidents in the four years The second group comprised an equal number of drivers who had incurred zero, 1 or 2 accidents over the entire four-year period, each member being individually matched with an " accident " driver for age, Corporation driving experience and type of vehicle driven The response rate was 93% for cases, and 82% for controls, the other controls being made up from second choices The interview was " blind " " Personality " measures were preferred to tests of simple reaction time and sensori-motor skills because of the high group selection , testing complex skills was unfortunately impossible in the circumstances of the investigation The results are summarised in Table 10, and elaborated in Cresswell and Froggatt (1962)

If the rationale of accident studies of the last 40 years is accepted, viz , that " accident proneness " not only exists but its biological correlates can be identified, then the present failure to demonstrate such differences can only be logically construed in two ways (assuming the experimental procedures to be fully acceptable) ; either one has been searching for the wrong things, or else in fact such differences do not exist in the population studied In this investigation either conclusion is possible ; but other investigators of transport drivers, with the possible exception of Hakkinen (1958), have never been able to identify unequivocally such correlates. Hakkinen, in a very good study, demonstrated certain differences in choice reaction times and ambiguous situation tests between his high and low accident groups, but there are points of technique in his methodology which prevent one from accepting his results without qualification Therefore, if constant differences between " good " and " bad " drivers have not been demonstrated as group properties, it is just as logical to conclude that they do not in fact exist, as to conclude that they *must* be present if only one can find them If one accepts the heresy that such differences do not in fact exist, then this seriously undermines the *a priori* position, in this context, of the Negative Binomial as Greenwood derived it, whereas it is consistent with the present hypothesis where there has been no need to assume that men necessarily inherently differ in their liability to spells Spells may in fact be induced by environmental circumstances acting on an array of drivers with a normal variation of human characteristics and exposed to equal risk, the net result of which is to produce an accident frequency distribution of composite

Poisson form If this is so it can be argued that men in the accident group should have more " environmental circumstances " than men in the safer group. It is of course impossible to say what such circumstances might be, but if one followed clinical impression then subnormal clinical health, either physically or mentally induced, might be suspected This is impossible to measure retrospectively, all that can be done is to take circumstances which might reasonably be assumed to worry any human being and about which " hard " information is available The results, checked from records, are summarised in Table 11.

Before leaving the Negative Binomial there is one further point of relevance If, in fact, removal of those individuals with the worst accident experience in one observational period produced a considerable reduction in the accident rate of the remainder compared with the whole group in another observational period, or if some test or tests could be devised which would allow one to identify a group whose subsequent exclusion would disproportionately reduce the accident rate, then a useful step in accident prevention would be made. Neither of these have been achieved to any appreciable extent in practice, which in itself must raise doubts about the stability of " proneness " even assuming it existed Greenwood (1950) understood this fully when he admitted that the failure of this pragmatic approach prevented one from being definite about the distribution of λ because only the distribution of actual accidents was available. One can now go further and enquire whether, in fact, the accident record can be considered as a satisfactory criterion against which to measure " accident proneness ", even after swallowing the *post hoc ergo propter hoc* fallacy implied Now the sample correlation coefficients of the various bivariate frequency distributions arising from the present data differed from zero to such an extent as to suggest that the actual population correlation coefficients were greater than zero Consequently a bivariate analysis might be fruitful. To this end a Bivariate Negative Binomial distribution can be derived and in the present instance the " Marginal Distribution " graduates the data satisfactorily. If the average number of accidents over each two-year period (a) is constant, then λ can be expressed in terms of a formula of constants derived from the moments of the Negative Binomial; or formally

$$\gamma=2[(\rho/a)+2]\lambda$$

which has a χ^2— distribution with $2(\rho+r_o)$ degrees of freedom, where r_0 is the observed number of accidents Consequently the 90% confidence limits of λ for certain r_o values can be obtained. These are shown in Table 12.

As was to be expected λ and r_o increase together suggesting that the greater the number of accidents an individual incurred the greater is his inherent " proneness ", but the overlap in the estimates at the limits adopted between, say, a man with one accident and a man with nine, precludes one from stating this categorically. Adherents to " proneness " and the Negative Binomial will have to find an alternative criterion to the accident rate as a satisfactory measure of accident liability. Their search may well be a protracted one

Finally, to return to the Short distribution This was constructed so that, *ab initio*, its parameters would have readily interpretable meanings If these are then estimated by substituting the sample values for the population moments, one can achieve, for large samples, a dichotomy of accidents into " spell " accidents ($\lambda\theta$) and those independent of such (ϕ) The former have been termed " personal " in the sense that the driver in question may have subscribed to the accident in more than a passive role, and the latter " chance " only in the sense that they are independent of the former, have a Poisson distribution, and can occur outside a spell But when assessing the results it must be stressed that up to a point they are speculative. The price Greenwood paid for a statistically useful form was an unsatisfactory hypothesis ; the price that has been paid here for clinical reality is an unstable third moment producing, especially in small samples, large standard errors and therefore possibly inaccurate estimates of the parameters To overcome this snag the theoretical distribution was modified by assuming that the " chance " parameter was equal to q times the first moment (where q ranged from 0 to 1). This in fact alters the estimates only slightly, but reduces the standard errors. In Table 13 the parameters with their full standard errors are given

If the parameters of the distribution truly have the inherently unique meaning ascribed to them, it must follow that in an environment where accidents are due primarily or exclusively to personal qualities, in their widest connotation, the value of the " chance " component will tend to zero even though the Short provides adequate graduation Such fitting to Greenwood's original data from 647 munition workers gives the " chance " component=—0·2. On reflection this seems reasonable, since in his population the number of accidents due to " extra-personal " reasons would be expected to be small, and moreover the period of observation was often very short Other classical frequency distributions from the literature have been so tested with generally speaking similar results

Data from the largest four populations in the present study, and from Hakkinen (1958) and Krivohlavy (1958), are displayed in Tables 14 and 15. The stability of the " personal " component ($\lambda\theta$) is quite striking, the increase in the accident rate from rural to urban environment being due to an increase in the " chance " (ϕ) component If the estimates of the parameters are accepted then this might appear reasonable in that increase in traffic leads to an increased rate of accidents (other things being equal) for which the driver is not solely culpable

Conclusions

In summary, a comparison has been made between the respective abilities of the Negative Binomial and Short distributions, both of which graduate the observed frequency distributions satisfactorily, to explain other information arising from the data. The evidence, though tenuous in the extreme, might be construed to favour the Short But if the hypothesis of stable " accident proneness " is unrealistic in the traffic situation, why the extraordinary success, in the present instance, of the Negative Binomial to explain what one truly believes to be rigorous data ? It is known that the Negative Binomial has considerable smoothing ability, and a recent paper in *Statistica* (Amato, 1959) suggests that this ability may be greater than is generally supposed It seems likely that even slight heterogeneity to accident risk in a population, and under experimental conditions this is almost inevitable, is sufficient to give rise to a Negative Binomial when the accidents are chance distributed amongst those who are truly equally exposed to risk The wisest conclusion to draw may well be that, since all the theoretical models considered have certain smoothing abilities, and the nature of accident data is such that seldom can one be completely confident of their validity, then the relative merits of the two hypotheses considered cannot be adequately assessed in the present instance, or indeed in any readily envisaged practical circumstance Certainly, with the exception of Zarathustra, tendency to accident is a hazard of living

Fruitful lines of further investigation might be—

(*a*) On the theoretical side, greater knowledge of the hypotheses on which compound Poisson distributions can arise, and their smoothing abilities Such distributions are currently in favour with actuaries, and a paper by the Belgian, Thyrion (1960), illustrates the potential danger to the unwary field worker Thyrion investigated the theoretical case of events occurring in clusters (" les grappes ") where each event happened simultaneously and at least one event occurred in each cluster His model was therefore formally completely different from the Long, Short or Negative Binomial; but on making different assumptions as to the probability law governing the number of accidents within a cluster, the Negative Binomial emerged in one instance, and a distribution of similar order to the Long in another. This is yet another way in which a Negative Binomial can arise.

(*b*) After forty years the picture of " accident proneness ", so called, is more confused than ever It is not a case of more rigorous retrospective studies being required, it is simply that, because of the limitations of all accident data, further retrospective studies will be unenlightening or confuse the picture even further Proper prospective investigations with subsequent retesting of results, would be long, tedious and costly, with no guarantee of producing results of value or practical applicability But if epidemiologists are to attempt to provide the best possible answers to the obvious questions, and 7,000 deaths and 350,000 injuries per year on the roads of Britain do raise questions, such studies are imperative Recently the Minister of Transport allocated £500,000 for a field study designed to ascertain the destination of London drivers ; the distribution of traffic accidents deserves and demands a similar priority Without such work there is a real danger, in fact it has already to be faced, that the results of what are, frankly, unsatisfactory studies, will be accepted by administrators because they have no other choice Already the British Medical Association (1954), the World Health Organisation (1956), and the American Medical Association (1959), have published recommendations for " safe driver " selection Some of the criteria derive from reasoning from propositions, e g , individuals suffering from conditions known to cause unconsciousness, dizziness, etc , should be excluded ; some are arbitrarily chosen, e g , " safe " standards of vision based frequently on just no worthwhile experimental evidence at all ; and some seem prompted by caprice If reputable scientific bodies continue to promulgate recommendations based on such flimsy evidence, even on the excuse that road accidents are a sufficient problem to allow expediency, it must ultimately jeopardise their objectives when offering advice, or recommending action, based on well-established associations For this reason, if for no other, properly designed and executed studies are now more than ever essential

SUMMARY

The concept of "accident proneness", and its statistical basis, are described An alternative hypothesis of accident distribution in a population at risk, is presented The respective abilities of these two hypotheses to explain data derived from Transport Authorities in Northern Ireland, is discussed Rewarding lines of future research are indicated

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TABLE 1.—DISTRIBUTION OF ACCIDENTS TO 647 WOMEN MUNITION WORKERS OVER 5 WEEKS (GREENWOOD & WOODS, 1919)

Number of Accidents	Frequency	
	Observed	Poisson's limit
0	447	406
1	132	189
2	42	45
3	21	7
4	3	1
5	2	0 1

P<0 001.

TABLE 2—DISTRIBUTION OF ACCIDENTS TO 198 MACHINISTS OVER 6 MONTHS. (GREENWOOD AND YULE, 1920)

Number of Accidents	Frequency	
	Observed	Modified " Biassed "
0	69	71
1	54	49
2	43	41
3	15	23
4	13	10
5	1	3
6	2	1
7	1	0.2

P=0·14.

TABLE 3—DISTRIBUTION OF ACCIDENTS TO 414 MACHINISTS OVER 3 MONTH (GREENWOOD AND YULE, 1920)

Number of Accidents	Frequency	
	Observed	Negative Binomial
0	296	299
1	74	69
2	26	26
3	8	11
4	4	5
5	4	2
6	1	2 (6–8)
7	0	
8	1	

P=0 64

TABLE 4.—DATA ON THE POPULATIONS SELECTED

	Drivers		
	U.T A	B C T (Omnibus)	B C.T (Trolley-Bus)
Number of Accidents, 1954–1955	1,602	1,116	979
Average miles driven/year	31×10^6	$9+\times10^6$	$8\ 5\times10^6$
Mean annual number of accidents per million miles	26·1	56 5	57·9
Number of routes or rotas	18	17	10
Number of Populations considered	4	1	1

TABLE 5—U T A. (EXCLUDING BALLYMENA, DERRY AND NEWRY) 1952–5 OBSERVED AND VARIOUS THEORETICAL FREQUENCIES FOR DIFFERING NUMBERS OF ACCIDENTS, (r)

r	Observed	Poisson	Neg Binomial	Long=N T A	Short
0	117	71 5	110 4	116 7	110·4
1	157	164 0	168 5	162 0	169·7
2	158	187·9	156 8	153·1	156·0
3	115	143 6	114 7	115·3	113·9
4	78	82 3	72 5	74 6	72 5
5	44	37 7	41 5	43 2	41 9

TABLE 5 —U T A (EXCLUDING BALLYMENA, DERRY AND NEWRY) 1952–5 OBSERVED AND VARIOUS THEORETICAL FREQUENCIES FOR DIFFERING NUMBERS OF ACCIDENTS, (*r*)—(*contd*)

r	Observed	Poisson	Neg Binomial	Long=N T A	Short
6	21	14·4	22 1	22 8	22 5
7	7	} 6 5	11 2	11 3	11 3
8	6		} 10 3	5 3 } 9 0	5 4 } 9 8
⩾9	5			3 7	4 4
χ^2		64 174 $v=6$	3 436 $v=6$	2 705 $v=6$	3 787 $v=5$
P		P<0 001	0 80>P>0 70	0 90>P>0 80	0 70>P>0·50
Significance .		Very highly significant	Not significant	Not significant	Not significant

TABLE 6 —CORRELATION COEFFICIENTS BETWEEN ACCIDENTS INCURRED IN DIFFERENT YEARS 166 LONDON OMNIBUS DRIVERS (FARMER AND CHAMBERS, 1939)

Years	1	2	3	4
2	0 298			
3	0 235	0 328		
4	0 177	0 176	0·212	
5	0 274	0 265	0 273	0 224

TABLE 7 —CORRELATION COEFFICIENTS BETWEEN THE SIXTH YEAR, AND YEARS 1–5 101 HELSINKI BUS DRIVERS (HAKKINEN, 1958)

Years	Correlation Coefficient
6/5	0 370
6/4	0·191
6/3	0·250
6/2	0 242
6/1	0 034

TABLE 8 —CORRELATION COEFFICIENT (*r*) FOR B C T TROLLEY-BUS DRIVERS, BETWEEN ACCIDENTS INCURRED IN YEAR *x* AND YEAR *y*

y \ x	1951	1952	1953	1954
1952 ..	0 097			
1953 .	0 111	0 266		
1954	0 148	0 136	0 259	
1955 .	0 052	0 216	0 118	0 185

TABLE 9.—CORRELATION COEFFICIENTS (*r*) BETWEEN ACCIDENTS IN 1952–'53 AND 1954–'55

Population (*a*)	'*r*' Observed (*b*)	Expected (*c*)	S E of '*r*' (*d*)	S E of Zero '*r*' (*e*)	(*b*) — (*d*)
U T A Pop 1	0 236	0 200	0 036	0 038	6 56
U T A Pop 2	0 231	0 119	0 095	0 098	2·43
U T A Pop 3	0 244	0 232	0·092	0 094	2 65
U T A Pop 4	0 420	0 237	0 103	0 113	4 08
B C T Bus	0 262	0 267	0 072	0 074	3 66
B C T Tr -Bus	0 297	0 335	0 061	0 064	4 83

TABLE 10.—FINDINGS IN TWO GROUPS OF B C T DRIVERS MATCHED FOR AGE AND DRIVING EXPERIENCE

Test	Mean Values (*ad hoc* units)		*t*	Significance of the Difference
	Cases (38)	Controls (38)		
Personality				
Heron I	4 2	3 9	—	—
Heron II	4 2	3 9	—	—
Body build				
Endomorphy	3 0	3 0	—	—
Mesomorphy	4 1	4 3	0 9	0 4>P>0·3
Ectomorphy	3 5	3 4	0 4	0 7>P>0·6
Androgyny	86 5	88 4	1 5	0·2>P>0 1
Eyesight				
Acuity	8 3	8 3	—	—
Depth Perception	3 3	3 8	0 6	0 6>P>0·5
Horizontal Phoria	1 5	1 9	1 3	0 2>P>0 1
Vertical Phoria	0 7	0 7	—	—
Field Impairment	1 0	0 0	—	—

TABLE 11.—FAMILY ENVIRONMENTAL DATA (1952–1955)

Circumstance	Cases	Controls
Number of children born	13	10
Hospital admissions of all children	8	4
Hospital admissions of wife (all men married)	9	6
Deaths in household	1	0
Total number of children alive on December 31st, 1955	107	97

TABLE 12 —90 PER CENT CONFIDENCE LIMITS FOR λ, GIVEN THAT A DRIVER SUFFERS r_0 ACCIDENTS DURING THE PERIOD 1952–5

r_0 \ λ	B C T Bus Driver	B C T Trolley-Bus Driver
0	0 482 — 2 075	0 417 — 2 123
1	0 621 — 2 358	0 574 — 2 469
2	0 766 — 2 636	0 739 — 2 806
3	0 914 — 2 909	0 911 — 3 136
4	1 067 — 3 178	1 088 — 3 461
5	1 222 — 3 445	1 269 — 3·782
6	1 380 — 3 709	1 454 — 4 099
7	1·541 — 3 970	1 643 — 4·413
8	1·703 — 4 230	1·833 — 4 724
9	1 867 — 4 487	2 026 — 5 033
10		2 222 — 5 340

TABLE 13.— DISTRIBUTION PARAMETERS AND THEIR STANDARD ERRORS

Population \ Distribution	Negative Binomial				Short					
	p	σp	c	σc	λ	$\sigma\lambda$	θ	$\sigma\theta$	ϕ	$\sigma\phi$
U T A (Excluding Ballymena, Derry and Newry), Bus Drivers	4 574	0 804	1 995	0 356	1 393	2 109	0 908	0 719	1 027	0 924
U T A Ballymena Bus Drivers .	8 143	5 557	3 689	2 532	0 055	0 125	3 304	3 704	2 026	0 262
U T A Derry Bus Drivers	4 523	1 760	1 658	0 656	1 215	3 617	1 164	1 827	1 314	2 021
U T A Newry Bus Drivers	5 753	2 584	1 606	0 732	0 454	1·028	2 217	2 686	2 576	1 110
B C T Omnibus Drivers	5 484	1 478	1 371	0 375	0 634	0 939	2 146	1 704	2 640	0 968
B C T Trolley-Bus Drivers	4 413	0 881	0 992	0 203	0 394	0 345	3 372	1 625	3 118	0 552

TABLE 14.—PARAMETERS OF THE SHORT DISTRIBUTION APPLIED TO OBSERVED MEAN NUMBERS OF ACCIDENTS
(FOUR LARGEST NORTHERN IRELAND POPULATIONS, OVER 4 YEARS)

Population	Mean Number of Accidents $m=\phi+\lambda\theta$		
	m (Total)	ϕ (" Chance ")	$\lambda\theta$ (" Personal ")
U.T.A. Pop 1 . .	2 29	1·03	1·26
U.T A. Pop. 2 . ..	2 73	1 32	1·41
B.C.T. Bus	4 00	2·64	1 36
B C.T. Tr -Bus .. .	4 45	3·12	1·33

TABLE 15 —PARAMETERS OF THE SHORT DISTRIBUTION APPLIED TO OBSERVED MEAN NUMBERS OF ACCIDENTS
(HAKKINEN, 1958, KRIVOHLAVY, 1958)

Population*	Mean Number of Accidents $m=\phi+\lambda\theta$		
	m (Total)	ϕ (" Chance ")	$\lambda\theta$ (" Personal ")
582 Prague Tram Drivers	5 02	3 74	1 28
101 Helsinki Bus Drivers	4 53	3 18	1 35
363 Helsinki Tram Drivers .	4 16	1 84	2 32

*Corrected to Four-Year Period

GRAPH 1