



RESEARCH ON ACCIDENT PRONENESS

THE READINGS:

Accident Proneness	387
The Incidence of Industrial Accidents upon Individuals with Special Reference to Multiple Accidents	389
A Contribution to the Study of the Human Factor in the Causation of Accidents	397
A Study of Accident Proneness Among Motor Drivers	410
The Phenomenon of Accident Proneness Personality Characteristics of Accident Repeating Children	427
The Accident-Prone Automobile Driver	437

THE IDEA THAT SOME PERSONS or groups appear to be especially vulnerable—or invulnerable—to misfortune is very old. It is a fundamental theme in history, art, and literature. It serves as the basis of much of ancient myth and modern fiction and is widely represented in the primitive world of totem and taboo. The story of Achilles shows well both its antiquity and importance, but it is seen in its fullest early development in the Book of Job, in which the interrogations and pronouncements of Job's pious friends illustrate the common inclination to attribute individual misfortune to undesirable personal characteristics that can be expiated, or at least uncovered, by appropriate therapy.

Against this background it is not surprising that the idea of accident proneness as a *scientific* concept concerned with unequal personal liability to accidents was greeted by research worker and layman alike with enthusiasm. In the 1920's, when it was introduced, attempts to explain the everyday world in scientific terms were increasingly numerous, and it is probable that this climate favored its popularity.*

Accident proneness proved to be one of those concepts which revolutionized a field of scientific investigation. And, as so often happens with such classical formulations, the disciples who followed pushed the concept far beyond what its originators intended. Coming at a time when the engineering approaches to accident prevention appeared to be reaching a point of diminishing returns, this brave, supposedly new idea of human vulnerability appeared to offer fresh hope for the conquest of accidents. Fifty years after its initial formulation in Great Britain by Greenwood and Woods, it still exerts a major influence upon accident research. For this reason, we feel it worth while to devote a separate chapter to this behavioral approach.

What is meant by accident proneness? First we have to distinguish between accident repetitiveness and accident proneness. The former simply refers to the descriptive fact that some individuals have more accidents than others, a distribution statistic that is true for a great many other social, psychological, and medical phenomena. There can be no quarreling with this statistical truth, but this is not what is meant by accident proneness, although a large number of studies proceed as if this were the case. Rather, accident proneness is offered as the *explanation* of why this distribution occurs, and it is this point that has aroused first minor, and now major, protest. As a theoretical explanation of repeated accidents on the part of the individual, accident proneness is a *psychological* abstraction and, as such, it is assumed to refer to the existence of an enduring or stable personality characteristic that predisposes an individual toward having accidents.

The critical words in this definition are the adjectives *enduring* and *stable*. Various researchers have attempted to identify such stable personality characteristics and have listed many unvalidated, negative personality traits, foremost among which are aggressiveness, impulsiveness, maladjustment, antagonism toward authority, immaturity, in-

* The notion that increased accident liability could be studied scientifically soon appeared in literature. For example, the plot of Thornton Wilder's Pulitzer Prize novel, *The Bridge of San Luis Rey* (1927), concerned an attempt to find the pertinent characteristics of a group of persons killed in the collapse of a bridge. "If there were any plan in the universe at all, if there were any pattern in a human life, surely it could be discovered, mysteriously latent, in those lives so suddenly cut off. Either we live by accident and die by accident, or we live by plan and die by plan. . . . But these occasions of human woe had never been quite fit for scientific examination. They had lacked what our good savants were later to call *proper control*." [Wilder's italics.]

considerateness, and hostility. Dunbar, one of the prominent exponents of the accident proneness concept, so described the accident-prone individual,^{1,2} as did Tillmann and Hobbs in a study discussed later in this chapter.

It should be obvious to the reader that so indiscriminate a listing of personality traits could hardly be expected to define a *single* type of individual. The list of distinguishing characteristics changes from study to study; the results concerning any single characteristic are inconsistent; and, in general, the correlations between personality characteristics and, hence, their predictive value, are extremely low.³⁻⁵ The bulk of evidence indicates that there is no such thing as a single type of "safe" or "unsafe" individual. Rather, each individual has a *range of behavior*, any portion of which may be safe or unsafe, depending on the environmental hazards to which he is exposed. Thus, instead of a single type of accident-prone individual, there may be many reasons why some individuals may incur more accidents than others.

For example, individuals may vary considerably: (1) in their exposure to hazards; (2) in their sensory, neural, and motor functioning; (3) in their capacity for correctly recognizing and making judgments concerning hazards; (4) in their experience and training; and (5) in the extent to which they are exposed to pertinent social and other environmental stresses. In addition, individual variations in susceptibility to trauma (see Chap. 9) may bias the data when accidents are defined in terms of the occurrence of given degrees of structural damage or personal injury. Finally, because individual variations in accident rates may result completely from chance, it is never adequate to show merely that a small fraction of similarly exposed individuals accounts for a disproportionate share of accidents. Rather, it is necessary to demonstrate that the disproportion observed is greater than that which would be commonly expected on the basis of chance alone. This has been overlooked by many, if not most, of those who have concerned themselves with accident proneness.

From this list it should be clear that accident proneness—as a psychological concept—must be viewed as only one possible explanation for individual variations in accident rates. This is not to deny the occasional usefulness of the concept but to indicate that there are other explanations which must always be considered.

ACCIDENT PRONENESS

—Edward A. Suchman, Ph.D., Alfred L. Scherzer, Ph.D.

The concept of accident proneness has been criticized on statistical, methodological, and theoretical grounds. Some of these criticisms are summarized in the following report.

* * *

ONE IDEA prevalent in accident research is that a high frequency of accidents incurred by certain individuals may be explained in terms of some abnormal personality char-

acteristic of those involved. There is a great deal of confusion and unprofitable debate about this concept of accident proneness. It is our contention that accident proneness does deserve a limited place in accident re-

[Reprinted, with omissions, from *Current Research in Childhood Accidents*. New York: Association for the Aid of Crippled Children, 1960, pp. 7-8.]

search but not the central position it is now given by many research workers.

The concept of accident proneness arose out of the observation that certain adults and children seemed to have more accidents than others. The term implies the existence of a particular personality type which is predisposed toward having repeated accidents. This predisposition is regarded as a psychological abnormality due to some underlying neurotic or psychopathic condition. For example, as summarized in the *Proceedings of the First Conference on Home Accident Prevention*, "The accidents, then, might well be unmotivated, and the defect a developmental one in the ego-control mechanisms."

Certainly a psychiatric point of view justifies the hypothesizing of a neurotic tendency on the part of some adults and children to inflict self-injury through deliberate accidents. Such clinical cases have been reported by various psychiatrists. However, can one use such clinical evidence to justify the use of accident proneness to explain all, or even most, of the cases of individuals who have a high frequency of accidents?

There are several reasons why this extension of a legitimate psychiatric concept is unwarranted. These reasons are statistical, methodological, and theoretical.

The existing evidence has serious statistical shortcomings. Since chance plays a part in many accidents, we would expect to find that during any given period a certain proportion of the population would suffer an inordinately high number of accidents *by chance alone*. The concept of accident proneness is based on the assumption that the membership of this group, in terms of individuals, remains unchanged with the passage of time. Actually, as Schulzinger observes, "The evidence indicates that if the period is sufficiently long, the small group of persons who are responsible for most of the accidents is essentially a shifting group of individuals with new persons constantly falling in and out of the group." Moreover, statistical correlations between present and future accidents are often low; the mathe-

tical models and techniques utilized to establish these relationships are subject to severe criticism by responsible theoreticians; observable injury or damage are not necessarily adequate indicators of the occurrence of accidents; and the multitude of possible intervening factors requires statistical controls which are not generally applied. After considering accident proneness from a theoretical and mathematical point of view, McFarland concludes that the scanty evidence which remains after critical review may hardly be taken as adequately supporting the concept.

From a statistical point of view the problem of prediction becomes extremely important as proof or disproof of the concept of accident proneness. To what extent can we predict that individuals with a high frequency of accidents in the past will continue to have more than their share of accidents in the future? Very few studies have attempted to do this.

* * *

Methodologically, too, there are a number of inadequacies in the current research on accident proneness. Perhaps most important is the failure to control an environmental exposure or risk. Certain individuals are more likely to be exposed to hazardous occupations or environments and thus to incur more accidents. Comparisons, therefore, must attempt to equate individuals on exposure before comparing them on accident frequency. Unless we take this differential exposure into account, we are likely to attribute to personality characteristics what is in fact attributable to the environments that attract such personalities.

There also appears to be some confusion between the concept of predisposing factors and accident proneness. Because certain groups of individuals (*i.e.*, young males) are apt to have more accidents than others, we cannot say that individuals who belong to these groups are accident-prone. There are a great many social factors which contribute to a differential rate of accidents among

subgroups of the population, and these factors have to be taken into account in explaining why some groups have more accidents than others.

Finally, from a theoretical point of view, there is some reason to doubt the existence of any identifiable personality type that could be labeled as the accident-prone personality. Dietrich is not fully convinced of the accident proneness concept because the accident repeater has not been successfully identified as a psychological entity. In fact, most studies have shown highly contradictory results concerning the traits of "the accident-prone personality." For example, in some cases this may be an overly timid individual, whereas in others this type of individual is more likely

to be aggressive. Unless the accident-prone individual can be identified and described as a meaningful type, it would seem to be more useful to identify those separate personality characteristics that are more or less associated with repeated accidents.

This analysis presents only some of the reasons why the concept of accident proneness must be viewed with caution. Certainly the evidence in its support is not conclusive. It seems likely that greater progress will be made in accident research through concentration upon the specific causes of repeated accidents and through restricting the concept of accident proneness to the limited number of psychiatric cases in the total population of accident repeaters.

The foregoing discussion should suffice to indicate that we are dealing here with a greatly misunderstood concept. In view of this, and in view of its considerable influence on accident research, we are reproducing below three of the early reports dealing with the subject. We do this not only for historic purposes but also because the first two remained the best work by far that had been done on the problem until the recent work of Cresswell and Froggatt (see below). We include also two more recent studies, one of which stresses a statistical and the other a clinical approach.

THE INCIDENCE OF INDUSTRIAL ACCIDENTS UPON INDIVIDUALS WITH SPECIAL REFERENCE TO MULTIPLE ACCIDENTS

—Major Greenwood, Hilda M. Woods

We begin with the classic paper in the field. As so often happens with new concepts, its originators state their case with greater moderation than their disciples. Greenwood and Woods did not describe some workers as "prone" to accidents† but developed the thesis that a small minority of individuals have greater numbers of accidents than would be expected on the basis of chance alone. This theoretical formulation led others to undertake hundreds of such studies and hence served as the basis for much of the research on accident proneness.

By studying accidents among workers in a British munitions factory during World War I, Greenwood and Woods found that accidents were not evenly distributed but that a relatively small proportion of the workers had most of the accidents. To explain this phenomenon, they proffered the theory of unequal initial liability—*i.e.*, that some individuals are inherently more likely to have accidents than others. Taking the accident records of a large number of work groups, they compared the observed frequen-

† See the preface to Newbold's work, other portions of which are reproduced in this chapter.

cies with three alternative hypothetical distributions. Further checks on consecutive time periods led them to propose that the presence of individuals with unequal liabilities best explained all the facts.

WHEN A NUMBER OF PERSONS engaged upon a specific task are observed over a period of some weeks or months, they are often found to have sustained a certain number of casualties; if such casualties are so trivial as to permit the victim to continue work, it may also be observed that the same person is injured more than once, so that the statistics of the whole period provide a certain number of persons who have passed through unscathed, some who have been injured once, others who have been injured twice, and so on.

A frequency distribution of this kind arises under various conditions and the proportions of the whole population found in its different subdivisions will be regulated by the group of causes which determine the happening of the event in question. If for instance we distributed amongst a set of families, each containing the same number of members, some source of infection (perhaps a person suffering from influenza might go to reside in each family), then we should ultimately have statistics of multiple cases of influenza, some families having no cases (other than that of the intruder), some having one, two, and so forth. But even without the supposed importation, and if sickening with influenza were as much a matter of chance as the drawing of an ace of spades from a well-shuffled pack of cards which we should do once on the average in every 25 trials, we should still expect to find that the statistics give instances of families with more than one case of influenza.

To take another illustration, let us suppose that 100 equally capacious and equally accessible pigeon holes are bombarded with 20 balls, none of which can fall clear of the

pigeon holes altogether, then the chance of any one ball lodging in any particular pigeon hole is one in a hundred, and at the end of the bombardment the distribution of pigeon holes with 0, 1, 2, etc. balls in each is given by the 21 successive terms of:

$$100 \left(\frac{99}{100} + \frac{1}{100} \right)^{20}$$

But the pigeon holes might not be of equal size. If some were very much larger than others, the former would receive a greater share of the balls and the distribution would be very different from that just given. Similarly if the pigeon holes changed size after the bombardment had commenced, the distribution would be affected. The extreme limits of the two modifications would be reached if either (a) all the pigeon holes save one were covered in, when the final distribution would necessarily be 1 pigeon hole with 20 balls and 99 with none, or (b) if directly a ball entered a pigeon hole a lid fell—as in the trap nest of a poultry fancier—which would lead to an ultimate distribution of 80 pigeon holes with no balls and 20 with one each.

These examples, although their analogy to the subject we are engaged upon is but imperfect, start a train of thought. Knowing the form of the ultimate distribution of pigeon holes with various numbers of balls, it is evidently practicable to form a judgment as to the nature of the causes which have operated in the distribution, since these will completely determine the result. We say advisedly "*form a judgment as to*" and not "*prove what was*" because an inverse problem of this kind presents certain difficulties which we have no space to discuss. Following up this trail might it not similarly be

Reprinted from Report No. 4, 1919, Medical Research Committee, Industrial Fatigue Research Board (Great Britain), with the permission of the Controller of Her Britannic Majesty's Stationery Office. A small portion of the text, 29 tables, and the Appendix have been omitted.

possible, from a consideration of statistics of multiple accidents, to reach a judgment as to factors producing these?

To make our point clear, let us discuss the genesis of accidents more at large. We have not, however, to consider any general influences common to the whole number of persons studied. Such influences will not affect the distribution of accidents as between individuals, but will modify the general scale; they are of course of immense importance because they may determine the total numbers of accidents sustained, but need another method of investigation and have in fact been studied by other workers. We are only dealing with the differentiation of individuals.

The simplest hypothesis is that there is *no* differentiation, that industrial accidents are really accidents in the strictest sense, just as it is an accident if one draws the ace of spades from the well-shuffled pack, an accident if a particular pigeon hole receives a ball at any particular throw and so on. In that event, the statistics of multiple accidents would conform to the type of a pure chance distribution of which the first arrangement of pigeon holes imagined above is one illustration.

The most obvious modification of the pure chance scheme is to suppose that the workers did all start equal, but that an accident having happened to any individual that individual's chance of sustaining a second accident became different from what it was before. Such a train of events is common enough in human life. A person may acquire some disease by the merest accident, but passing through the attack will profoundly modify his chance of acquiring it again when the original conditions are, in all other respects, reproduced; he may be practically immune or conversely he may be much more sensitive to infection. The analogous schema in our sets of pigeon holes is that of the trap nests, although the analogy is imperfect because in that case not only is the future chance of the particular pigeon hole modified, which is correct, but, by the conditions

that all 20 balls must ultimately rest somewhere in the 100 pigeon holes (introduced for simplicity) the chance of the empty pigeon holes is also modified, which is wrong, for the happening of an accident to one person should not generally affect anyone else's chance.

Thirdly we might suppose that all the workers did not start equal, but that some were more liable to suffer casualties than others; suppose there were only two classes, clumsy and careful people, then the analogy would be 100 pigeon holes, 50 having an opening of 1 square foot and 50 having an opening of 2 square feet and we should get another special distribution of multiple accidents.

These three hypotheses correspond to three distinct policies of organisation.

If industrial accidents were found to be allocated upon a pure chance schema, the diminution of their number would be effected by a change of scale through administrative reforms inspired by researches into general conditions, but not into the individual physiology or psychology of the worker. Were the second mentioned hypothesis in better accord with the facts, there would be need for consideration whether the enhanced liability to accident after a first casualty (supposing the bias were in that direction) might not be reduced, perhaps by a compulsory period of rest, possibly by a short interval of different work. If, on the other hand, the third possibility materialised, it would follow that both initial selection of recruits and also a rapid elimination of those sustaining multiple accidents should have a great effect in reducing the casualty rate of the factory.

It seemed therefore possible that an investigation of the statistics of multiple accidents might yield results of some practical importance and we felt justified in making a preliminary survey of the field; a full discussion of the various mathematical questions suggested by the data will be published in a memoir by Mr. G. Udny Yule and one of us at a later date.

STATISTICAL METHODS

When it has been desired to ascertain whether the distribution of multiple events actually observed in any case were such as might arise upon the hypothesis of a pure chance determination, the schema of pigeon holes into which balls are tossed has been adopted by the best modern exponents of applied mathematical probability.

This schema was used by, for instance, Professor Karl Pearson in a study of the occurrence of multiple cases of cancer in houses and in connection with the similar problem of recurrent enteric fever in houses. When the number of pigeon holes is very large but the ratio of balls to pigeon holes finite, the formula admits of rapid computation by means of an approximation.

The *a priori* objections to this schema as a proper representation of what occurs in a chance distribution of accidents are formidable. In effect, the assumption is made that n accidents *must* happen to the N persons, all that we have to do being to distribute them; but it might properly be retorted that a true analogy is that of a platoon of soldiers exposed to fire of whom a certain number are struck once, some twice and so on. Now suppose the number of bullets discharged (irrespective of whether they find billets or not) is M and the number of wounds inflicted (again irrespective of whether they are inflicted upon different bodies) n ; then the chance of being struck being p and of being missed $1 - p = q$, we can determine values of the unknown chance from the mean and second moment of the distribution; we take the chance of being wounded as not given *a priori*.

In practice, however, these two fundamentally different methods do not really lead to widely divergent final distributions. This is illustrated by the following examples. In A, 400 accidents are distributed amongst 10,000 persons by the pigeon hole schema. In B it is assumed that the same number of persons were exposed to risk of accident during 4 units of time, the chance of having an accident in each unit being one in a

hundred and the unit so small that no two successive accidents could occur within it.

We have:—

Accidents per person	Frequency by A	Frequency by B
0	9608	9606
1	384	388
2	8	6
3	0	0

The reason of this accord is that in each case the form of distribution approximates to that obtained by the series mentioned in the first section of the appendix, viz.:—

$$e^{-\lambda} \left(1 + \lambda + \frac{\lambda^2}{2!} + \frac{\lambda^3}{3!} \dots \right)$$

where λ is the number of accidents divided by the number of persons. Consequently whenever the conditions are such that the chance of sustaining an accident must be deemed small (and these conditions are fulfilled in the majority of cases), the unmodified pigeon hole schema is probably a quite adequate test of the likelihood that the distribution is one of pure chance in origin, and this is the test we have uniformly applied.

The second hypothesis to be tested, viz., that in a population initially all equally liable and exposed to risk, those who experience first accidents are by virtue of that experience rendered more or less liable to have another accident, is *not* satisfactorily imitated by the modification of the pigeon hole schema which we indicate in the Appendix [omitted].

There is no algebraical difficulty in devising a much more appropriate schema, one not suffering from the obvious defect that increasing the size of certain pigeon holes diminishes the size of the remainder, the total space within all the pigeon holes together being kept constant. The requisite algebraical formula has been worked out and will be given in the paper by Yule and one of us to which reference has been made. But it was found on trial that this more correct formula produced a distribution very similar to that reached with the modification of the pigeon hole schema just mentioned, while actual fitting to data was extremely

laborious and not in general practicable in terms of the lower moment coefficients. Hence, although fully alive to the imperfections of the modified pigeon hole formula, we have utilised it as some test of the physiological theory.

The third hypothesis, viz., that of *ab initio* differentiation, is we think quite sufficiently tested with the aid of the third formula in the Appendix. We do not of course mean that the assumption involved in equation (5) of that Appendix is necessarily correct, but merely that it provides the kind of distribution which we should naturally anticipate and has the advantage of leading to a series the constants of which can be deduced from the first two moments of the statistics.

These three methods then, that of a Simple Chance Distribution (denominated in our tables by the letters C.D.), that of a Biased Distribution (entered as B.D.), and that of a Distribution of Unequal Liabilities (indicated by U.D.), have been used throughout. A criterion of agreement between the deductions from the formulae and the statistical facts has been obtained by using Professor Pearson's Goodness of Fit Test as modified by him in the under-cited paper.

We shall first set out the tabular results yielded by data recording the numbers exposed to risk (we of course satisfied ourselves that the material conditions of exposure to risk were really approximately constant in each set of data), and the distribution of multiple accidents. We shall then examine some statistics providing more detailed information (Tables I.-IX.).

As will be seen from the headings of the tables, the sources of the data are various; sometimes we had merely a record of the total numbers employed in a particular shop over a given period, on other occasions we were furnished with the records of a number of women chosen at random, e.g., by the fact of their names occurring on particular pages, while in one set of data the method of selection was to take a random sample of women who had, and of women who did not have, an accident during a particular month. It must be noted that the last-mentioned

TABLE I.A.*—750 WOMEN WORKING ON 60-LB. SHRAPNEL

Accidents	No. persons	C.D.	B.D.	U.D.
0	398	422	411	429
1	294	242	258	233
2	43	70	67	70
3	10	14	12	15
4	3	2	2	3
5	2	.23	.01	.4
Total	750	750	750	750

(P=002) (P=0)

* The meaning of the quantity P given in the first 10 tables is this: When a hypothesis requires the numbers of observations falling within a series of classes to be a_1, a_2, a_3, \dots , and one actually finds b_1, b_2, b_3, \dots observations in these classes, the probability of the discrepancies having arisen by "chance" depends upon the value of the sum of such quantities as:— $\frac{(b_1 - a_1)^2}{a_1}$, and is denoted by the letter P. For instance, in Table II., Shop A, P = .13 for the B.D. distribution means that were the hypothesis valid, then in every 100 trials we should get no better agreement than actually observed here 13 times. But for the U.D. hypothesis, the chance is better, viz., 39 in 100.

method, although indifferent if accidents are truly random, is not indifferent if the true cause be varying personal liability; on that hypothesis such selection is differential.

A glance at the tables is enough to show that the C.D. hypothesis is altogether inadequate, while in a majority of cases the other two hypotheses provide good fits; but on a general review of the data (Table X.) it is apparent that the U.D. method is decidedly superior. In five cases the B.D. distribution fails, the U.D. only twice (it is noteworthy that two sets of data which neither hypothesis fits are mere enumerations of totals employed and therefore the guarantee of equal exposure to risk is much slighter than in other cases). The superiority of the method of Unequal Liability is not a mere consequence of using a formula with one more constant; two moments are involved in each calculation; hence it appears just to infer that the hypothesis of the deviation from simple chance being dependent upon unequal initial liability to accident is sustained. We now proceed to examine the point more strictly.

It is evident that if the C.D. principle held, the previous record of any individual

TABLE X.

Data	Values of P	
	B.D.	U.D.
Random sample of 201 women	.60	.15
Random sample of 198 women	.22	.39
62 women having accidents in February	.94	.93
136 women having no accidents in February	.16	.17
55 women having accidents in January	.35	.89
61 women having no accidents in January	.90	.87
50 women having accidents in March	.00	.56
50 women having no accidents in March	.44	.39
648 women working on 6-in. H.E. shell, "A" shop	.13	.39
584 women working on 6-in. H.E. shell, "B" shop	.30	.16
100 women on machine work	.00	.48
414 women on machine work	.00	.57
750 women on 60-lb. shrapnel	.002	.00
580 women on 6-in. H.E. shell	.001	.002
Average of P	.29	.43

would be without influence upon his or her subsequent experience, just as if in one particular set of tosses a certain coin fell heads five times running, that coin would be neither more nor less likely to fall heads five times in a subsequent experience.

But if some one coin were biased in favour of falling heads, then its records in successive experiments would naturally be interrelated. Having in some of our data records of previous experience, we can easily determine which case responds to the reality, and in Tables XI.-XIII. are set out the records of women who in a particular month did or did not have accidents. It will be seen that almost invariably the balance of accidents is heavily against those women who fell victims in the month taken as a criterion of classification.

A yet more striking result is shown by other data which we owe to the zeal of some welfare supervisors in a great National Factory. These data were compiled with exceptional care and relate to random samples of women whose accidents, output and lost time over a trial of three months were carefully recorded, and whose accident experience in the previous three months was likewise available. We first went through these data rejecting all women who had not

been in employment at the factory at least eight months when the record was made and we likewise rejected those who had been absent from work 14 days or more during the trial period. There remained 22 in one and 21 in another shop (the samples related in each case to women employed throughout the trial period upon one and the same lathe operation). We then correlated the individual's accidents in the two successive periods of three months. Tables XIV. and XV. exhibit the results from which it appears that in both processes the correlation is substantial and especially noteworthy in the sample of heavy lathe operatives, who were performing what is considered to be a strenuous task. Tables XVI. and XVII., covering all the records, tell the same story.

Since there is considerable correlation between the records of successive periods, there can be little doubt that the C.D. hypothesis is inappropriate. To discriminate between the two other suppositions a further investigation is necessary. Upon either hypothesis we should expect to find correlation, but, were the B.D. hypothesis correct, the observed correlation would be increased by eliminating from the record individuals who sustained no accidents in the first period because, *ex hypothesi*, these persons are neither more nor less likely to have accidents in the second period than they were before; hence their presence amounts to a dilution of correlated pairs with uncorrelated pairs. On the other hand by the U.D. hypothesis there is correlation between immunity in one period and immunity in the following period. Accordingly if we remove the pairs the first members of which are zeros, the correlation should increase if the B.D. hypothesis is correct, but not otherwise. Table XVII.A shows that in every instance the correlation is reduced, although the difference is hardly significant. The conclusion, then, is that the supposition of unequal initial liabilities better explains the facts, as was suggested by the previous investigation.

These results indicate that varying individual susceptibility to "accident" is an

TABLE XVII.A.—CO-EFFICIENT OF CORRELATION BETWEEN ACCIDENTS IN SUCCESSIVE PERIODS

<i>Data</i>	<i>Observed 3 months and previous 3 months</i>	<i>Not including persons having no accidents in previous 3 months</i>
Women on heavy lathe operation	.69 ± .06	.63 ± .09
Women on heavy lathe operation engaged in 1917 or earlier	.72 ± .07	.61 ± .12
Women on profiling operation	.53 ± .09	.37 ± .14
Women on profiling operation engaged in 1917 or earlier	.37 ± .12	.18 ± .17

extremely important factor in determining the distribution; so important that given the experience of one period it might be practicable to foretell with reasonable accuracy the average allotment of accidents amongst the individuals in a subsequent period (Table XVIII.) This result is in itself of considerable interest, because it shows that by weeding out susceptibles the accident rate would necessarily decline; but before much practical value attaches to it we must be a little clearer as to what one ought to mean by this phrase individual susceptibility.

The naive interpretation is of course that of carefulness or carelessness; as one says, there are people whose fingers are all thumbs and there are others who are neat fingered; or again some people are scatter-witted and others circumspect; do our results amount to more than an arithmetical verification of this? Perhaps not, but there are other possibilities. Industrial accidents are usually held to be a function of output, and also a function of fatigue; the faster one works the greater the number of accidents, and the more weary one is when working at the same rate the greater the risk of misadventure.

We naturally ignore in this investigation any effect of general increase of output or of general fatigue due to conditions affecting all, but individual variations do concern us. Then again, our records of accidents are of reported accidents and, with few exceptions, the accidents are trivial, small cuts, slight burns, foreign bodies in the eye, rarely involving either absence from work or

recourse to a surgeon. But the nervous or ultra careful woman may, for various reasons, report accidents which the average woman would disregard altogether. Consequently we have sheltering under the term individual susceptibility a motley host of motives or factors which will be very difficult indeed to separate and measure. Two variables, however, we can attempt to deal with, viz., output and general sickness.

Tables XXI.-XXIV. relate to the women used in Table VIII., and the records of broken time were not so accurate as we could wish; they show no measurable difference of output between the women who had and those who did not have accidents.

Tables XXI.-XXIV. relate to the women figuring in Tables XIV.-XVII., and here the records of lost time were more satisfactory. Output has been reduced to terms of hours actually worked. It will be noticed that the heavy lathe operatives and the profilers vary in opposite directions; but in view of the absolute smallness of the divergences, and paying attention to the error of sampling, we do not think any stress can be put upon the result; so far as our data go the differentiation of those who do, from those who do not, have accidents cannot be shown to be related to a similar differentiation in the matter of output. Those who sustain many accidents are on the average neither less nor more productive workers than their fellows.

In Tables XXV. and XXVI. time lost by sickness is brought into relation with the accident record. Here there is, perhaps, some indication of a difference, sickness lost time being negatively correlated with accidents. But, as Professor Loveday pointed out, the allocation of lost time to different causes is, unless medical certificates of a trustworthy character are available, extremely unreliable, so that the relation may mean little more than a tendency to credit some sickness or lost time to accidents among women who have had accidents, the real attribution being uncertain.

Accidents have also been correlated with age (Tables XXVII. and XXVIII.), but the results again are of no practical importance.