JIA 75 (1949) 0012-0038

THE DISTRIBUTION OF SICKNESS

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[Submitted to the Institute, 22 November 1948]

'The fact that many applications of probability are based on belief or faith should not discourage us; for it is better to do something, though it may not be quite reliable, than nothing.'
J. V. Uspensky

This paper is concerned with the application of probability theory to sickness. An attempt is made to explore the form, and in particular the standard deviation, of the distribution of the sickness experienced by a body of lives. It appears that, in the past, none but empirical investigations have been made in this field, the theoretical foundation being entirely lacking. The only exceptions are Sir George Hardy's paper in *J.I.A.* Vol. XXVII, and the paper in *J.S.S.* Vol. VII by R. E. Beard on *The standard deviation of the distribution of sickness*. This statistic, as Beard points out, appears to have been practically ignored by British actuaries hitherto.

The laws governing chance variations in mortality rates, or in the number of deaths in a group of lives in a certain period, have been extensively elaborated. From these laws have been derived the forms of distribution of the number of deaths, tests of homogeneity of bodies of lives, and tests of mortality table graduations known as probabilistic tests. In almost every case where the standard deviation of deaths is employed, a parallel use could be found for the standard deviation of sickness, even if the practical value of this use were slight at the present time. Probability laws of the type mentioned can be applied to the number of occurrences of any 'simple event' such as birth, death, marriage or falling sick, and the actual observed distributions agree reasonably well with what is expected from the theory. In talking of a 'simple event', it is not meant that the causes leading to the event are not complicated and varied, but that the events happen in discrete units and have something of a 'once-for-all' nature about them. These processes do not appear to have been extended to sickness rates (except in work which rests on a purely empirical basis).

The number of weeks of sickness is a more complex measure than the number of deaths, because it depends not only on the probabilities of falling sick, but also on the probabilities of recovering or dying within a given time from the onset of the sickness. The object of this paper is to show that the observed fluctuations in sickness rates can broadly be accounted for by the classical principles of probability theory.

The essential problem to be solved may be stated thus. Let a group of lives, each subject to given probabilities of falling sick and of recovering at every duration of sickness, be observed for a period of time, and the number of weeks of sickness recorded. This number may be regarded as a sample from a universe of many similar observations. It is desired to find the parameters of the universe distribution (and in particular the standard deviation).

It is not desired to enter into refinements of the definition of probability. If the paper contains somewhat naïve assumptions, for example that probabilities of sickness may exist, these are made in full awareness of the difficulties surrounding the foundations of the classical theory of probability. In view of the theoretical attacks, a suspicion lingers that perhaps we are not strictly entitled to speak of a mortality rate or a sickness rate at all, except as a summary of past history. For practical work, however, we continue to use these notions.

ASSUMPTIONS

Certain assumptions need to be made in order to bring the problem within the scope of mathematical treatment. Throughout this paper sickness is meant to include disablement, unless of course the duration is specified to be under six months. The following assumptions have been made.

(1) *That, as a working hypothesis, the group of lives to be observed is subject to certain sickness probabilities, which may be written down as a table of sickness rates.* We can imagine either that the lives are a random sample from a parent population, in which population each member is destined to have a predetermined but unknown amount of sickness, or that to each life (considered merely as a member of the observed group, and without knowledge of individual health and circumstances) there may be assigned a certain probability of falling sick and recovering at each duration. These two conceptions are not irreconcilable, and lead to the same final results.

(2) *That within one year of observation deaths can, for the present purpose, be treated in the same way as cessations of sickness by recovery.* This may appear to be a drastic assumption. Death is likely to be correlated with sickness and to be selective, by tending to remove those for whom sickness is not merely accidental but a sign or cause of lowered vitality. It should be noted, however, that cessation of sickness by death is of course allowed for in all published tables, and it seems, therefore, that the only error lies in the impossibility of a dead man contracting another illness within the year.

(3) *That within the year of observation a person cannot fall sick more than once.* This assumption is roughly equivalent to a 'linking-up' period of twelve months.

(4) *That there is an even spread of sickness over the year of age,* or to be more precise, that the probability of being in any duration of sickness is uniform in the year of age.

(5) *That the sickness of the lives is independent.* The effects of the differences between the conditions of practice and these assumptions are discussed later in this paper and, where possible, the numerical effect is estimated. Comparisons have been made of actual deviations with those calculated on the basis of the theory. The hypothesis that the observed weeks of sickness can be considered as random samples taken from the assumed model distribution has been tested.

AN URN PROBLEM AND THE VARIANCE OF THE EXPECTATION OF LIFE

It will be helpful first to consider a simple urn problem, the solution of which illustrates the method used in finding the standard deviation of sickness.

Consider a series of k urns containing black and white balls in such proportions that the chance of drawing a white ball ('success') from the rth urn is p_r. Let N drawings be made from the first urn (the ball drawn being replaced after each drawing) and suppose the number of successes is x_1. Now let x_1

drawings be made from the second urn and suppose the number of successes is x_2. If this process is continued a non-increasing series of numbers $x_1, x_2, x_3, \ldots, x_k$, is obtained. What is the mean and variance (standard deviation squared) of the statistic

$$X_k = x_1 + x_2 + x_3 + \ldots + x_k?$$

The distribution of the variate x_r is binomial. If we let $p_1 p_2 p_3 \ldots p_r = P_r$, then the mean of the distribution of x_r is NP_r and the variance is $\sigma_r^2 = NP_r(1 - P_r)$.

We next require the correlation between the numbers of successes from any two urns, that is, between x_r and x_s taking $r < s$. If R_{rs} is the correlation coefficient then we find that

$$\begin{aligned} R_{rs}\sigma_r\sigma_s &= \text{mathematical expectation of } (x_r - NP_r)(x_s - NP_s) \\ &= NP_s(1 - P_r). \end{aligned} \tag{1}$$

Now let us consider $X_k = \sum_1^k x_r$. The mean of X_k is equal to $N \sum_1^k P_r$. The variance of X_k is derived from the usual formula

$$\sigma^2(X_k) = \Sigma\sigma_r^2 + 2\Sigma\Sigma R_{rs}\sigma_r\sigma_s,$$

where the summations extend from 1 to k, and r is always less than s. Hence we have

$$\sigma^2(X_k) = N[2\Sigma r P_r - \Sigma P_r - (\Sigma P_r)^2]. \tag{2}$$

It is interesting to notice that these results are directly applicable to life contingencies. For example, it is not difficult to derive the standard deviation of the total of annuity payments made to a group of annuitants over a given period of years. Further, if the force of mortality is known at every age (integral and fractional), the standard deviation of the complete expectation of life can be found. In fact, for a single life,

$$\sigma^2(\mathring{e}_x) = 2Y_x/l_x - T_x^2/l_x^2, \tag{3}$$

where in the usual notation

$$T_x = \int_0^\infty l_{x+t}\,dt \quad \text{and} \quad Y_x = \int_0^\infty T_{x+t}\,dt.$$

Thus we are able to estimate the variance of the expectation of life of any group of lives of which the mortality rates are known, and of the total annuity payments in a period, and other similar functions.

TIME UNITS

To turn to sickness rates, a slight difficulty arises from the mixed units of time by which sickness is commonly measured. For example, the Manchester Unity (Whole Society) Tables show that the 'First three months' sickness of a man aged 40 years is on average ·840 weeks per annum. This statement involves three different measures of time: weeks, months and years—surely an irrational mixture of units. In the analysis that follows it is very convenient to have one unit by which to measure age, duration and length of sickness, and the unit chosen is a year.

Let us then define $s_{x,t}$ as the *force of sickness*, measured throughout in yearly units, at age x duration t. It is one of our assumptions that the force of sickness is to a sufficient degree of accuracy independent of age over the interval of our observations. Hence, for brevity, the suffix x has in general been dropped and s_t or $s_{x,t}$ denotes the force of sickness of duration t at the age x under consideration. In other words, out of a group of N lives aged x, there will be, on the average, $Ns_t dt$ sick in the duration t to $t+dt$. From a different point of view this expression is the number of people expected to reach the duration t of sickness in the interval of time dt. Thus $Ns_0 dx$ people fall sick, on average, between the ages of x and $x+dx$, and in the whole year a proportion s_0 (sometimes denoted $\bar{z}_x$) of the total lives may be expected to fall sick.

The connexion between the force of sickness in yearly units, and sickness rates as ordinarily tabulated, is clear. For example, the 'First three months' sickness rate is

$$S_x \text{ (First three months)} = 52 \int_0^{\frac{1}{4}} s_{x\ t}\, dt.$$

THE SICKNESS DIAGRAM, CORRELATION BETWEEN THE SICKNESS OF ANY TWO SMALL REGIONS

In the sickness diagram shown in Fig. 1, the sickness of a group of N lives is represented. Any point on the diagram represents a certain age and duration of sickness. If the time scales of age and duration in the diagram are the same, we may imagine that a life who is sick will trace a diagonal line at 45° to the axes, from the top left to the bottom right, until recovery or death takes place.

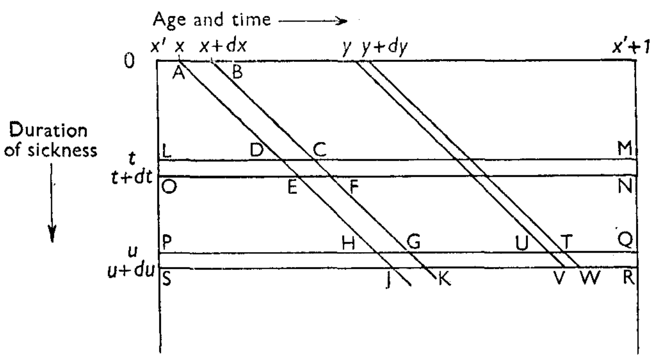

Fig. 1. The Sickness Diagram.

To each area that may be marked out on the sickness diagram there corresponds a certain total of sickness. It is desired to find the variance of the sickness corresponding to the area bounded by ages x' and $x'+1$ and two given durations of sickness. The method employed is to regard the sickness diagram as divided up into very small elementary areas. The sickness in each elementary area is distributed according to the binomial law with known variance. Furthermore, the correlation between the sickness in any two elementary areas can be found. It is, therefore, merely a matter of integration to find the variance of sickness corresponding to any given region, by using the well-known formula for the variance of the sum of a number of correlated variates.

The method of the following pages is to start from first principles and proceed step by step to the formula for the variance. The process (though more analytical and thus perhaps of greater benefit to the student) is lengthy, and those who are familiar with moment-generating functions may find the approach in the Appendix more direct.

The expected (or mean) number who attain duration t of sickness, from among those who first fall sick in the interval of age x to $x+dx$, is $\mathrm{N}s_t dx$, and the variance of this number must be $\mathrm{N}s_t dx\,(1-s_t dx)$ or, ignoring second-order differentials, $\mathrm{N}s_t dx$ (the Poisson variance). Hence in the interval of time dt the mean sickness of these lives is $\mathrm{N}s_t dx\,dt$ with variance $\mathrm{N}s_t dx\,(dt)^2$. We have thus found the mean and variance of sickness corresponding to the small parallelogram CDEF of the sickness diagram (which for brevity we may describe as the sickness 'in the area CDEF').

A life represented by the point A on the diagram must move along the diagonal to J or recover or die in the interval. A chain of correlation exists between the numbers of lives sick at successive points on the line AJ similar to that discussed in the urn problem. There is obviously a large positive correlation between the number of lives at two points on the line, especially if they are separated by only a small time interval. This correlation is given by formula (1) of the urn problem. In fact, the correlation of the numbers of lives is given by

$$\mathrm{R}_{tu}\sigma_t\sigma_u = \mathrm{N}s_u dx\,(1-s_t dx) = \mathrm{N}s_u dx,$$

ignoring, as before, the second-order infinitesimals. Hence the *covariance* (this term denotes expressions of the type $\mathrm{R}_{tu}\sigma_t\sigma_u$) between the sickness in two elementary areas such as CDEF and GHJK on the diagram is

$$\mathrm{N}s_u\,du\,dt\,dx. \tag{4}$$

This expression gives the correlation between the sickness in two elementary areas between the same two top left to bottom right diagonals. There is also a correlation between any other pair of elementary areas. Consider first two elementary areas for which the age is equal. It is clear from general reasoning that, if at any point the number of lives between given durations of sickness is greater than expectation, the numbers at other durations, or for that matter in good health, will on average be less than expectation. This is, in fact, the usual negative correlation between any two ordinates (or cell frequencies) of a frequency distribution.*

To find the correlation between the sickness in *any* two elementary areas, consider the chance that a life, after falling sick at age x to $x+dx$, attains duration t of sickness (i.e. reaches or crosses the line CD in the diagram). The chance is $s_t dx$. Hence the probability that exactly r lives cross CD is

$$p_r = \mathrm{C}_r^{\mathrm{N}}\,(s_t dx)^r\,(1-s_t dx)^{\mathrm{N}-r}.$$

If there are r lives which cross CD, then none of these lives can be included in those which cross TU, and therefore the chance that exactly s lives cross TU is

$$p_s = \mathrm{C}_s^{\mathrm{N}-r}\left(\frac{s_u dy}{1-s_t dx}\right)^s\left(1-\frac{s_u dy}{1-s_t dx}\right)^{\mathrm{N}-r-s}.$$

* See, for example, *An Introduction to the Theory of Statistics*, by Yule and Kendall (11th ed.), para. 21.3.

The covariance of r and s, called $C(r, s)$, is the mathematical expectation of rs less the product of the means of r and s, and hence

$$\begin{aligned} C(r, s) &= \Sigma rsp_r p_s - \Sigma rp_r . \Sigma sp_s \\ &= -\mathrm{N}s_u s_t dx\, dy. \end{aligned}$$

The covariance between the sickness in the areas CDEF and TUVW is, therefore,

$$-\mathrm{N}s_u s_t\, dx\, dy\, dt\, du. \tag{5}$$

DERIVATION OF THE FORMULA FOR THE VARIANCE OF SICKNESS

The variance of the sickness in a strip (width dt) such as LMNO in the diagram is

$$\int_0^1 [\mathrm{N}s_t (dt)^2]\, dx = \mathrm{N}s_t (dt)^2.$$

Furthermore, from (4) and (5) the covariance between two strips (durations t and u where $u>t$) is the sum of the covariances of every possible pair of elementary areas, one from each strip. It is, therefore,

$$C(t, u) = \mathrm{N}(1-u+t)\, s_u\, dt\, du - \mathrm{N}s_u s_t\, dt\, du. \tag{6}$$

The variance of the sum of a number of correlated variates can be, as previously mentioned, obtained from the well-known formula

$$\sigma^2 = \Sigma\sigma_r^2 + 2\Sigma\Sigma \mathrm{R}_{rs}\sigma_r\sigma_s.$$

In the case we are considering, the strips have been regarded as infinitesimally thin and, therefore, the finite summations must be replaced by integrals between appropriate limits. The variates are the total of sickness in the areas such as LMNO and PQRS. Hence, if we put V_x for the variance of sickness of one individual at age x, the variance of sickness between durations a and b, in the whole year of age is

$$\begin{aligned} \sigma^2 = \mathrm{NV}_x &= \int_a^b [\mathrm{N}s_t dt]\, dt + \int_a^b \int_a^u 2\mathrm{N}\,[(1-u+t)\, s_u - s_u s_t]\, dt\, du \\ &= 0 + 2\mathrm{N}\int_a^b [(u-a) - \tfrac{1}{2}(u-a)^2]\, s_u\, du - 2\mathrm{N}\int_a^b \int_a^u s_u s_t\, dt\, du \\ &= 2\mathrm{N}\int_a^b (u-a)\, s_u\, du - \mathrm{N}\int_a^b (u-a)^2 s_u\, du - \mathrm{N}\left\{\int_a^b s_u\, du\right\}^2 \\ &= \mathrm{N}(2\mathrm{U}_1 - \mathrm{U}_2 - \mathrm{U}_0^2), \end{aligned} \tag{7}$$

where
$$\mathrm{U}_r = \int_a^b (u-a)^r s_u\, du.$$

This is the fundamental formula for the standard deviation squared of sickness measured in yearly units. The similarity with formula (3) for the variance of the expectation of life should be noticed. The formula can also be reached by using moment-generating functions (see Appendix), a method which has the advantage of enabling the skewness and kurtosis of the distribution to be estimated.

The variance of sickness has thus been expressed in terms of moments of the sickness rates about a convenient origin and of N, the number of lives (or number of years' exposure). Modifying the formula so that the sickness is measured in weeks (accented symbols denote that the sickness is measured in weeks), we have

$$\sigma'^2 = NV'_x = N\,(2U'_1 - \tfrac{1}{52}U'_2 - U_0'^2), \quad \text{where} \quad U'_r = 52^{r+1}U_r. \tag{8}$$

Formula (8) requires adjustment if the total duration of sickness is greater than the average time of exposure, which we have assumed to be one year. Thus the adjusted formula for 'All sickness' is found to be

$$\sigma'^2 = NV''_x = N\,[52S_x\text{ (after one year)} + 2U'_1\text{ (first year)} - \tfrac{1}{52}U'_2\text{ (first year)} - U_0'^2\text{ (all periods)}]. \tag{9}$$

In the formula (8) it is generally found that the first term is considerably greater than the other two. The variance of sickness, to a first approximation, is, therefore, twice the first moment of the sickness rates. It has not been possible to discover any 'commonsense rationale' as to why this should be so. A simple explanation would be illuminating.

Moreover, it is always true that $U_0 > 2U_1 - U_2$. The inequality merely depends on the fact that $(\overline{u-a}-1)^2 s_u$ is always positive. Hence the variance of sickness is always less than the total expected sickness, when the unit of time is a year throughout. If sickness is measured in weeks, this implies that the standard deviation of sickness, whatever the shape of the curve of sickness rates, is less than $\sqrt{(52)}$ or 7·2 times the square root of the expected sickness. This upper limit is too high to be of much use except perhaps at old ages and the longest durations of sickness.

Attempts have been made in the past to relate the standard deviation to the square root of the expected sickness. Table 1 shows the ratios of these quantities.

Table 1. Manchester Unity sickness rates 1893–97 (Whole Society)

Theoretical values of K, where K is the ratio of the standard deviation of sickness (in weeks) to the square root of expected sickness (in weeks)

Age	First 3 months	Second 3 months	Second 6 months	First year	All periods
20	2·30	3·12	4·32	3·01	2·84
30	2·36	3·17	4·25	3·37	4·00
40	2·45	3·18	4·30	3·66	4·64
50	2·52	3·23	4·35	3·94	5·16
60	2·61	3·22	4·39	4·37	5·61
70	2·69	3·21	4·36	4·71	5·44

Although K may be greatly affected by changes in the *shape* of the sickness curve, it will only be slightly decreased if the sickness rates are increased by a uniform percentage. The constancy of K with age is remarkable, and suggests that a single value of K might be found for each period of sickness which would with sufficient accuracy give the standard deviation of the total sickness ignoring age. The maximum value that K can possibly take has been shown to be just over 7. In the above table the value for 'All periods' is not far below this limit at higher ages. The variances found by Beard in the paper already referred to are fairly consistent with the above figures.

COMPARISON OF ACTUAL AND EXPECTED STANDARD DEVIATIONS

It is very fortunate that there exists published material which can be used to show the degree of closeness with which the results of the foregoing theory correspond to actual fact. The Manchester Unity investigation is sufficiently detailed to enable the agreement between observation and values calculated from the formulas developed earlier in this paper to be tested. The Manchester Unity Tables give both observed and graduated sickness rates, and tests can be made by using the graduated rates as *a priori* probabilities. The fact that the Tables, which are based on the experience of the years 1893–97, are quite out of date does not in any way invalidate the tests, and no subsequent British tables are so convenient for this purpose. Appendix V to the 1912–13 *Report on the administration of the National Insurance Act* (reprinted in Command 6907) subdivides the Manchester Unity (Whole Society) graduated rates into weeks of duration for the first six months of sickness, and gives some subdivisions of sickness after six months. This information has been used to calculate the moments of the sickness rates which are required for our variance formula.

An advantage of using an accepted graduation in this investigation is that there can be no suspicion of bias, in choosing the graduated rates or method of graduation, that might tend to improve or reduce the agreement between theory and observation.

The number of weeks' sickness per year, in any week of duration, which can be obtained by differencing the columns of Table 1 on p. 593 of the Health Insurance Report, is a close approximation to the force of sickness s_t. Using these values of s_t and changing other time units to weeks, approximate values of the moments U'_0, U'_1 and U'_2 were found. These, together with the variance function V'_x calculated therefrom, are given in the Appendix for various sickness periods for age 16 and quinquennial ages from 20 to 70.

Values of the variance function at ages other than the quinquennial ones were next obtained by interpolation. The theoretical variance of the sickness rate at each age in the Manchester Unity (Whole Society) was then found by dividing the variance function by the number of exposures at that age. The difference at each age between the crude and graduated rates for the Manchester Unity (Whole Society) Experience was obtained directly from the Tables. We should expect that these differences, divided in each case by the corresponding theoretical standard deviation, would form a set of standardized variates (with zero mean and unit standard deviation) probably with a distribution not far from normal. If the standard deviation of this set of statistics is found to be close to the expected value of unity, then the assumptions made in the second section of the paper are reasonably well borne out in practice. Should the agreement of actual and expected be poor this probably indicates that the basic assumptions are not applicable to the experience under examination, and efforts should be made to find the reasons for the divergencies.

The results of this calculation are summarized in Table 2. There were very exceptional differences between the crude and graduated rates for ages 18 and 19, at the 'tail-end' of the graduation where the summation graduation formula cannot be applied. The values for these ages were therefore excluded from the totals. The most satisfactory way of testing the hypothesis that our estimates of variance are correct is to employ the χ^2 test. This will indirectly test the hypothesis that the lives in the Manchester Unity Experience were subject to our assumptions (of randomness, even distribution of sickness over the year of age, etc.).

The totals in Table 2 for ages 20 to 69 inclusive are the sums of 50 variates. It would, however, be wrong to apply the χ^2 test with 50 degrees of freedom. The crude Manchester Unity rates were, except at extreme ages, graduated by Spencer's 15-term summation formula. Graduation, in general, by means of a summation formula involves some reduction in the number of degrees of freedom in the χ^2 test. The reduction cannot be accurately assessed, but theoretical considerations and experimental results (of which there is no need to give details here) suggest that in our particular case the reduction in the original 50 should be about 12. The number of degrees of freedom was therefore taken as 38.

Table 2. Manchester Unity (Whole Society) Experience

Summary table—values of actual deviations squared divided by theoretical variance and summed in quinary age-groups

Age-group	Period of sickness				
	First 3 months	Second 3 months	Second 6 months	First year	All periods
18 and 19	39·67	·47	·36	25·20	24·85
20–24	1·98	3·01	4·95	·57	·55
25–29	1·78	5·19	5·83	3·63	2·80
30–34	4·89	1·39	4·58	3·78	2·43
35–39	1·04	1·43	4·73	1·15	·99
40–44	1·85	2·27	3·51	1·80	·58
45–49	2·34	1·91	1·84	1·21	1·39
50–54	8·83	1·12	6·14	1·54	1·19
55–59	3·70	3·16	2·25	3·40	·82
60–64	2·77	1·64	1·34	1·67	1·03
65–69	5·38	1·34	11·47	6·96	2·05
Total for ages 20 to 69 = χ^2 with 38 degrees of freedom	34·56	22·46	46·64	25·71	13·83
P = probability of a lower value of χ^2 than the actual	·365	·025	·841	·068	·00034

DIVERGENCIES FROM THE ASSUMPTIONS

The foregoing results should be considered in the light of the following divergencies of the assumptions from actual facts by which the theory has been tested.

(*a*) In the case of the Manchester Unity sickness rates we have every reason to suppose that there is positive correlation between the sickness at adjacent ages. The average exposure to sickness of each life in the experience was 3·64 years. A specially unhealthy group of lives might increase the sickness rates for four or five successive years. Also, many illnesses lasted from one year into the next. The graduated rates (owing to the weak summation graduation formula used) would to some extent tend to reproduce the 'bump' in the crude rates. Hence the apparent variance would be reduced. Correlation between near ages, therefore, tends in the direction of reduction of the observed variances. It is at old ages that long-term sickness is commonest, and hence this effect is likely to be greatest at old ages and long durations.

(*b*) The assumption that the average duration of exposure is one year leads to a small error. Should the average exposure be k years, the expression for the variance would strictly require to be increased by

$$(1-k)\,(U_0'^2 - U_2'/52k).$$

From the published particulars of the Manchester Unity (Whole Society) Experience the number of lives contributing to the exposures at each age can be found approximately, and hence also the average duration of exposure. These durations all lie between ·925 and ·985 years. For these values of k, the adjustment to the variance is sometimes positive and sometimes negative and only in extreme cases amounts to as much as 1 %.

(*c*) It is not easy to discover what error is introduced by ignoring deaths. The sickness rates on which our theoretical variance is calculated give the number of weeks sickness per year of exposure to sickness. On the average, lives are assumed to be exposed for half a year in the year of death. The only reason why a distinction should be drawn between exits from the state of illness by recovery and by death, is that another attack may, in the former case, occur in the same year of exposure. Also it is likely that a heavy year for sickness would show an unusually large number of deaths. It is debatable whether the theoretical variance would be increased or decreased were it possible to take proper account of deaths.

(*d*) The Manchester Unity (Whole Society) Experience, drawn from different geographical areas and occupations, etc., is heterogeneous. Now heterogeneity in a sample does not affect the variance provided that the sample is chosen at random. If, however, the sample is 'stratified' (that is, includes certain fixed proportions from different universes) then the variance can easily be shown to be reduced. It is well known that the variance of the number of deaths, in a stratified sample from heterogeneous data, is less than that if the probability for each life were the same and equal to the mean of the probabilities of the heterogeneous lives. The same can be proved true in the case of sickness.

It seems possible that a large friendly society may have a little of the character of a stratified sample in that the proportions from different areas, and with different occupations and income levels, may be more nearly constant from age to age than they would be if the lives were chosen at random from an infinite universe. If so, we should expect the observed variances to be rather less than the theoretical ones.

(*e*) The effect of the benefit conditions, such as the exclusion of 'First three days' sickness, the reduction in benefit after six months, the off-period and

* It is interesting to notice that the usual formula Epq for the variance of deaths is incorrect if the average exposure is less than one year. The correct variance then becomes $Eq\,(1-kq)$.

the linking-up provisions, appears to be automatically provided for in the sickness rates themselves. These benefit conditions may affect the average amount of the sickness claims. However, the variance of sickness depends only on the probabilities of being sick at each duration, and not on the factors which built up these probabilities.

(*f*) The effect of assuming that sickness is evenly distributed over the year of age is negligible.

(*g*) The epidemic effect of infectious diseases should also be mentioned as possibly preventing independence in the sickness of the lives.

CONCLUSIONS

The results of applying the χ^2 test to the Manchester Unity (Whole Society) Experience, the expected variances being calculated from our formulas (8) and (9), are shown in the last line of Table 2.

The 'First three months', 'Second six months' and 'First year' sickness are within the 5% limits, that is $\cdot 05 < P < \cdot 95$, and the test is therefore satisfied. The 'Second three months' sickness satisfies the less stringent test $\cdot 02 < P < \cdot 98$. However, 'All periods' sickness completely fails the test as P is found to be less than ·0005.

Of the divergencies of the theoretical assumptions from practice, the most important is the lack of independence between sickness at successive ages in the tables. Could this factor be fully allowed for, the agreement between the expected and observed variances would, except in the case of 'Second six months', be improved. It is of great importance at the longer durations of sickness, and possibly it might account for the strikingly low variances of 'All periods' sickness.

Another possible factor lies in the administration of the Manchester Unity Societies. Where the actual sick claims are much above average, it is likely that they will be closely scrutinized; where the claims are low the management can perhaps afford to be less stringent. Thus there may be a certain levelling effect.

Although the expected variance of the sickness rates has ranged from under 10^{-5} to over 10^{-3}—the greatest value being more than 4800 times the smallest—yet there is little sign of any clear trend in the figures of the Summary Table (except for the low values of the 'All periods' sickness column) either with age or period of sickness.

To recapitulate briefly, a formula for the standard deviation of sickness has been developed from the commonly accepted first principles of probability theory. In the Manchester Unity Tables the deviations are on the whole rather less than we should expect from the formula, most especially in the case of 'All periods' sickness. However, there are a number of reasons which can be assigned for this difference. Whether, bearing these reasons in mind, the figures of the Summary Table can be considered to justify the theory on which this paper is based must be left to the opinions of the readers.

My thanks are due to Mr B. Benjamin, B.Sc., F.I.A., F.S.S., for his valuable help in the preparation of this paper.

APPENDIX

Readers who are familiar with moment-generating functions may find the following derivation of the variance of sickness more straightforward than that given earlier in this paper. The approach by way of moment-generating functions has the further advantage that moments of the sickness distribution beyond the second can be systematically obtained.

Consider first the measure of total sickness that most readily lends itself to statistical treatment. This is the total of sickness up to a certain duration *which commenced in a given year*, including sickness which commenced in the year and continued into the next, but excluding any sickness which commenced in the previous year. The same assumptions are made as before regarding independence, death, linking-up, etc., and the unit of time is a year.

Suppose now that s_y is the force of sickness of duration y, the units of time being uniform throughout. Then the chance that a life has no sickness commencing in the year is $(1-s_0)$, the chance of a sickness period of length over y but under $y+dy$ is $-ds_y$, and the chance of a sickness of length t or over is s_t. This defines the probability distribution for a single life.

The moment-generating function for an individual about zero origin is therefore

$$F(\alpha)=(1-s_0)\,e^0-\int_0^t e^{\alpha y}\frac{ds_y}{dy}\,dy+e \quad {}_t$$

$$=1+\alpha\int_0^t e^{\alpha y}s_y\,dy.$$

If we write $U_r=\int_0^t y^r s_y\,dy$, then the moment-generating function of the total sickness of N random lives is

$$\{F(\alpha)\}^N=(1+\alpha U_0+\alpha^2 U_1+\tfrac{1}{2}\alpha^3 U_2+\dots)^N$$

$$=1+NU_0\alpha+[2NU_1+N(N-1)U_0^2]\,\tfrac{1}{2}\alpha^2+\dots.$$

The first, second, third, etc., moments of the sickness distribution, about zero origin, are given by the coefficients of $\frac{\alpha^1}{1!}, \frac{\alpha^2}{2!}, \frac{\alpha^3}{3!}$, etc., in the expansion of the moment-generating function in ascending powers of α. Transferring the moments to the mean NU_0 as origin, we obtain

$$\mu_2=N(2U_1-U_0^2),$$

$$\mu_3=N(3U_2-6U_0U_1+2U_0^3),$$

$$\mu_4=3\mu_2^2+N(4U_3-12U_0U_2-12U_1^2+24U_0^2U_1-6U_0^4).$$

The expression for μ_2 is identical in form with that for the variance of the expectation of life given in formula (3).

A numerical example will indicate the magnitude of the quantities involved. The following figures relate to the First Year's sickness of N lives aged 40, subject to the Manchester Unity (Whole Society) sickness rates.

Total expected sickness	$=1{\cdot}122N$ weeks,
Variance $=\sigma^2$	$=19{\cdot}9N$,
Skewness $=\mu_3/\sigma^3$	$=7{\cdot}8N^{-\frac{1}{2}}$,
Kurtosis $=\mu_4/\sigma^4$	$=3+72{\cdot}7N^{-1}$.

Thus the standard deviation of the number of weeks sickness is 4·2 times the square root of the total expected sickness; the distribution is skew with the 'long tail' towards high values, and also leptokurtic.

On precisely the same principles we may find the moments of the distribution of the sickness, duration 0 to t, *experienced in a given year of observation.* This is a total of sickness such as would be found at a particular age in any sickness experience compiled in the usual way.

The probabilities that during the year a life should experience no sickness, sickness of length y to $y+dy$, or sickness of length t and over, are built up as follows (see Fig. 2).

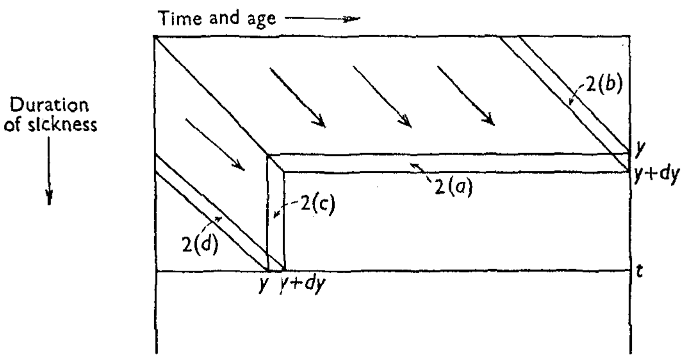

Fig. 2. The analysis of the sickness of length y to $y+dy$ within one year of observation (see (2) below).

(1) The chance of no sickness is:

(*a*) Chance of not falling sick in the year $\quad 1-s_0$

less (*b*) Chance of being sick at the commencement $\quad \int_0^t s_y\,dy$

Chance of no sickness $\quad c_0 = 1 - s_0 - \int_0^t s_y\,dy$

(2) The chance of a sickness length y to $y+dy$ in the given year, and within the durations of sickness 0 to t, is:

(*a*) Chance that sickness commenced in the year and ended by recovery $\quad (1-y)(-ds_y)$

plus (*b*) Chance that the sickness commenced in the given year and continued into the next year, the total sickness in the given year being y to $y+dy$ $\quad s_y\,dy$

plus (*c*) Chance that the sickness commenced in a previous year and ended by recovery in the given year $\quad \int_y^t (-ds_y)\,dy$

As to the valuation of cases of over 5 years' duration, it was probably the case that the rate of recovery was small, but the rates of pay in permanent partial disablement cases were still liable to fluctuate even after five years, as the experience of recent years had shown in a very marked degree. By the time the first five years had elapsed the ratio of diminution had ceased to operate and as the values were based upon the actual weekly rates in force the author's method assumed that the rates would not be appreciably modified in future. The suggested further step of increasing gradually the proportion of the Post Office annuity from 75 % to the 100 % level could be regarded as providing for the approach to normal mortality as duration increased, and also for a possible increase in the weekly payment in later years.

There was a short section at the end of the paper dealing with valuation of 'suspended' cases. Suspended cases might hide a serious liability. The cases which had previously drawn compensation could, he thought, be divided into the following categories: (1) the men who had been certified as fully recovered (it was of interest to note that there had been an increase in recurrences during the last two years), (2) the men who were back at work but had some permanent disability, such as permanently stiff fingers or limbs (he thought such cases would have little difficulty in sustaining a renewed claim, probably at a higher rate of compensation, should economic conditions result in a shortage of employment), and (3) the men who were still on risk, but whose rate of payment at the moment was *nil*.

In the first category the prospect of renewed claims was probably fairly remote, in the second category not so remote, and in the third it might only be a matter of time before claims had to be met.

He would suggest for consideration that the liability for the first group should be included in a general reserve related to claims after 5 years' duration (the cases under 5 years' duration had already been dealt with by the author's negative recoveries). In the second group perhaps there could be a scale of fixed reserves according to the nature of the injury. In the third group the employees were merely in receipt of the lowest possible rate of compensation, and in essence they were no different from other cases receiving a few pence per week. On practical grounds he thought it would be easier to leave such cases on the 'live' register until they were certified by the doctors as having recovered, especially as in some collieries the weekly payments were liable to fluctuate from week to week in sympathy with wages, so that a person on the 'live' register one week might pass to the suspended list in the next week.

The method of valuation suggested by the author gave, of course, satisfactory reserves but his method of dealing with the *nil* cases affected the rate of recovery. If the man went on to the 'suspended' list he presumably appeared in the rate of recovery, and if that were so the rate of recovery took on a special significance as it was then related only to the evaluation of the benefit and not to the medical condition of the workman. If that course were adopted the logical procedure seemed to be to ignore the medical condition and to use a rate of claim which should be an amalgam of the ordinary rate of recovery and the author's ratio of diminution. A rate of claim was, however, the actuary's last hope, to be avoided if possible.

He wished to refer briefly to two other matters. The first was silicosis. The author did not mention it in the paper, probably because it was not found in the collieries with which he was concerned. But it was important in dealing with a colliery where the conditions were conducive to the disease. Hitherto the chances of recovery had been very slight, and although the duration of the cases was relatively short the cost was heavy because full pay might be claimed all the time. The prevalence of silicosis in a colliery rendered unsuitable for use, without appropriate modification, facts drawn from the experience of another colliery.

The second point concerned the age limit. If after the war an increase in the State pension were to materialize, then it would appear to be convenient to stop compensation at the pension age, and to let the man draw his pension. There should be some limiting age, he thought, but, of course, it was necessary to provide an alternative benefit for the man. With an adequate pension there would no longer be any need for compensation, the liability for which should cease as that for sickness benefit ceased.

Mr W. F. Marples agreed with Mr Lancashire that the question of diminution of compensation with duration since the accident tended to fade into insignificance when the difficulty of determining the payment to be valued was considered. The annual payment in the case of total disablement was fixed, subject to review procedure, but that of a partially disabled man varied considerably because it was dependent on the difference between his pre-accident and post-accident earnings and hence was greatly influenced by the degree of activity in the industry.

The use in valuation of the current payment was fraught with considerable danger, in that it not only undervalued the real liability but produced extreme irregularity in reserves. The problem had been solved partially in certain cases by using an estimate of the compensation which would be payable if the man were performing light work for, say, four shifts a week, a method which had the merit of bringing out consistent valuation results and a not unreasonable estimate of the real liability. The payment so adjusted did not take into account any increase in the compensation which might be taking place under Section 11 of the 1925 Act, under which a man could claim to have his pre-accident earnings reviewed if the earnings of a fit man in the same occupation were more than 20% higher than when the disabled man was similarly employed.

When the payment for valuation had been determined the question of the annuity value had to be considered. Section 13 of the 1925 Act prescribed the basis of redemption of compensation payments, namely, 75% of the Post Office annuity value, which might be regarded as the minimum value. But an estimate had to be made of the actual value of the continuing payment, determinable by death, redemption or recovery. That value depended on the actual experience of the company concerned and might vary considerably in different parts of the country. *A priori*, there was no reason to suppose that the Government annuitants' table was actually suitable for the valuation of compensation payments for disabled miners, however satisfactory it might be as a uniform statutory redemption basis. The position should at least be examined carefully before adopting a simple rule, such as that suggested by the author, by which a reserve of the full Post Office annuity value was built up.

The speaker gave details of mortality and commutation experiences relating to miners in various parts of the country with over 3 years' duration of disability. He said that if those experiences were employed to produce annuity values the results would differ widely, and that showed in his opinion how inequitable the operation of a uniform basis of valuation might be, particularly when it was remembered that premiums and the corresponding calls for Mutual Indemnity Associations were based on the valuation results.

Finally, he stressed the need for information with regard to miners' disablement. It had long been a matter for regret that so little information was available with regard to the incidence of the cost of compensation in the mining industry because of the absence of what should be obtainable from statutory returns.

Mr Guy Johnson (a visitor) said that he did not think he ought to allow Mr Lancashire's remarks concerning lump-sum payments to pass without comment. The subject had been investigated by a number of commissions and committees during the last twelve years, and they had failed to agree whether the lump-sum payment was desirable. No general agreement could be gathered from the evidence given on behalf of the insurers, the employers and the trade unions; the Accident Offices Association had been neutral on the subject.

The author had stated that the cost of payments for the first 26 weeks differed with the system of insurance in force, and that where there was a special inducement to get the workman back to work within the 26 weeks' period the effect was to reduce the compensation paid for the first 26 weeks' incapacity. He hoped he had misunderstood the significance of that statement, which was rather disturbing. He doubted very much whether the author would find many people to agree with the idea that the insurer could materially affect the time which would elapse before the man returned to work.

A similar point was raised later, where the author said that the increased compensation under the Workmen's Compensation (Supplementary Allowances) Act had led to an increase in both the short- and long-duration cases. He would like to know how far that was a general experience. He would have expected to find that the increased wage levels exerted a stronger influence, and that therefore there would be a tendency to return to work quickly. He agreed that the cost of the first 26 weeks varied with different industries, and that the composite offices had lower costs because they dealt with a larger proportion of lighter injuries.

In considering the factor used in the undertaking to the Home Office it should be remembered that the factor was not applied to the business of any one company but to the total of the sums reported by all the members.

He proceeded to describe the method by which he understood that the undertaking had been devised by the late Sir Alfred Watson. For the first three years of the undertaking the factor was calculated by finding for the second, third and fourth years previous to the account the proportion of payments on account to actual payments in respect of outstanding claims, plus estimates for claims outstanding at the end of the year of account. To the figures so ascertained a margin for safety was added. It was found that for three years the factor produced was approximately 10 %, and that figure was adopted from 1927 onwards and had been rather successful. The last year which could be taken was 1936, and taking the nine years 1928–36 there had been an excess in every year but one. The average figure was about £63,000 in each of those years, and as the amount of reserve averaged rather over £1,600,000, there was an average excess of about 3·86 % of the reserve. There had been fairly wide fluctuations from year to year, partly due to the fact that the figures reported by the members were the amounts which they had paid to reimburse the employer. That was subject to rather undue external influences. For example, air raids in the winter of 1940 apparently led to delay in presenting and settling accounts, and there was probably less brought in than there should have been.

The Supplementary Allowances Act had affected weekly payments more than other payments, and such payments formed the bulk of what were called the 26 weeks' payments. But it had not increased the payments, other than the weekly payments, to the same extent—it had hardly affected them at all. Common law claims and medical expenses had not been affected, and that meant that the figure which had been multiplied had been increased out of proportion to the total claims which had had to be paid.

Something should be said about the author's remark that uniformity of method was desirable. He would not say that that was an assumption with which he would be prepared to associate himself. It seemed to him that the criteria of the desirability of uniformity were that the result should be easily produced, that it should be better when it was produced, and possibly that the result should be such as enabled a comparison to be made between one method and another. Better results depended upon the extent to which the formula was elaborated and particularly the extent to which it was kept up to date. There were so many changes in the labour market and in general economic conditions that the amount which would have to be paid undoubtedly varied during quite short periods, and as the effect of any system could not be checked in less than 5 years the formula was likely to be out of date.

He did not know how far elaboration would be possible in a composite office. To attempt to arrive at multipliers, and more particularly to keep them up to date, would undoubtedly involve an enormous amount of work.

As regards the comparability of reserves, he thought that the attraction was probably more apparent than real. Such comparisons could usefully be made only by persons versed in the art of making them. The ratio of reserves to premiums of one company could always be compared with that of another, and, as was probably more significant and important, any change in the ratio of reserves to premiums made by a company from one year to another could be noticed and an explanation sought.

The undertaking basis was not in fact an alternative, but a method which was applied to all companies or to a large group of companies. It had proved to be very satisfactory from that point of view, but it was not established to provide adequate reserves. It would be fair to say that it was designed to ensure that the amounts which were brought into account on the reserves were adequate, but not more than adequate, when striking a profit for that year. It seemed to him on balance that the case for uniformity as compared with the individual method had not been established.

Mr D. A. Porteous said that the author had referred to the division of cases between accidents and nystagmus, and had suggested that industrial diseases other than nystagmus might generally be grouped for valuation purposes with accident. He would probably not adopt that course without investigation. In particular, cases of silicosis, if numerous, should certainly be investigated as they presented some distinctive features.

He had been specially interested in the reserve factors on the basis adopted in valuing the outstanding liabilities of the tariff offices. The Accident Offices Association factors were, of course, used to obtain estimates of the aggregate outstanding liabilities of the accident offices as a whole and were not necessarily suitable in an individual case. It was interesting, however, to find that the total factor based on the experience of the author's Association agreed so closely with the A.O.A. standard factor, although the constituent parts for the various durations were very different. A possible explanation of the difference between the factors for the first year might be a greater tendency on the part of the accident offices to clear cases off their books by payment of the lump sum. The factors based on the actual experience of the claims which arose in 1933 and 1934 were about 9·5; 1935 produced 10·2, but that was affected probably to a small extent by the supplementary allowances which started in 1940. He had examined the A.O.A. results to see whether they showed any trace of the emergence of latent liability during the period of heavy unemployment in the years 1931–33. No such result appeared, however, as the payments made in those years on the claims which arose previously produced factors which, if anything, were rather smaller than usual. It might be that the point was not so important in the large aggregation of relatively light risks with which the accident offices dealt.

The author referred to the proportion of payments for the first 26 weeks of disablement which was outstanding at the end of the calendar year, and said that the figure could be taken at about 10 % of the amount paid for new cases. No direct comparison on that point could be made with the A.O.A. figures, but in recent years the amount spent in the year on new claims still in force at the end of the year was 16·5 % of the total amount of the new claims for the year. That included lump-sum payments and death claims. The proportion had shown a rising tendency; in 1924–26 it was only about 12·5 %. As the proportion was to some extent linked with the reserve factor it would be of interest to compare that feature of the A.O.A. experience with the corresponding results of the coal industry if the author could conveniently provide the figures.

Mr S. Townsend (a visitor) said that a reference had been made to the likelihood that a great deal of work would be saved if some actuarial method were found for the valuation of outstanding claims. One of the practical difficulties, however, was that by the time the data necessary to arrive at the method had been obtained, the data were out of date.

The factors governing the amount of claim payments changed very quickly. Since the Factories Act of 1937 the number of common law claims had increased very considerably. Recently a concession had been made by employers and insurance companies with regard to election. It had been agreed that for a period of three months workmen would not be bound by their election, and that they could take Workmen's Compensation without losing their common law rights until the end of that time. That privilege was very important in estimating the cost of a claim. He thought that the Home Office calculation was a method which was more likely to fit the case than any other. He agreed with Mr Lancashire that a reserve for contingencies was required; in other words, if a factor was to be obtained it must be calculated on a broad basis. He thought that the Home Office factor was of such a nature and that it had proved in its results to be very satisfactory, bearing in mind that it was calculated on a very large figure and that offices varied in size, so that it obviously could not be applied to each one. In the examination of the figures of several companies he had noticed that there seemed to be a relation between the amount which was paid each year on account of outstanding claims and the amount which was finally paid; it varied with different companies, but the results suggested that there was some underlying principle. The estimates of the companies concerned were made by the medico-legal method, taking each case separately, and that method of arriving at the figures was in his opinion unquestionably the most accurate.

Mr B. C. Lucena, F.F.A., said that the author dealt chiefly with Mutual Indemnity Associations and particulars had been given concerning the insurance of the risks with which those associations were concerned. But among the minority were the trust funds of individual collieries, which presented additional problems to the actuary. A trust fund established under the Workmen's Compensation (Coal Mines) Act, 1934, required an estimate of the future cost of all claims which would arise during the ensuing 12 months or the new claims liability. The present paper did not cover that point, but any method which the author had devised would be very valuable to the actuary. In two collieries in Staffordshire it was found that over a period of years before the war the number of new claims varied fairly consistently with the tonnage of coal raised, and on that basis it had been possible to estimate the number of new claims and the expected cost. But a disturbing feature during the last 3 years had been a rapid and rising increase in the number of claims per thousand tons raised. Before the war the averages of both collieries were rather below the figures given in Maudling's paper, but by 1941 both averages had risen by about 50% over the 1938 figure. It was also interesting to note that in the colliery which had the higher number of claims the initial expected duration of claim was the less. That increase was not apparently confined to Staffordshire pits, and it would be interesting to know whether the author had the same experience, as both the outstanding and the new claims would be affected, and it was to be expected that the number of claims due to accident would tend to fall when wages were high. Was it more difficult working conditions or the older age of the miners concerned which led to more frequent accidents? Was that borne out by the experience of others? In the case of the men with dependants, the increase due to the supplementary payments granted by the Act of 1940 was proportionately greater than the increase in wages, and it was recognized that the smaller the margin between wages and compensation the more frequent and prolonged would the claims tend to be, so that some increase in the duration of claim might also be expected.

His experience with regard to latent claims was that they seemed to be more important than they were a few years ago; in fact, about one-third of the whole reserve had to be made on account of latent claims.

In the valuation of a trust fund it was necessary to allow for compensation from the outset, and so the duration of claims was divided in separate weeks for the first 26 weeks, then 6 months to 12 months, and thereafter in completed years, and it was found that about one-half of the claims had a duration of 4 weeks or less. Commutations were few in the case of the Staffordshire collieries, and for the purpose of calculating annuities were considered as extending the duration of the claim by the number of weeks' payments represented by such settlements. The resulting values for over six months' were much less than the figures given by the author, which was evidence in support of what had been said about the different conditions in different parts of the country.

On the question of valuation by the medico-actuarial basis, he thought that the method might be dangerous, because to make a proper estimate the actuary sometimes needed also to be a doctor and a lawyer.

Mr G. D. Stockman said that if no attempt were made to obtain a law for the basis of reserves there was no prospect of getting nearer the truth. In exceptional times the procedure should be to go back to the most stable time that could be found, to recognize that it could not be applied without adjustment, and to make the best adjustment possible in the light of what was known about current conditions.

In dealing with the difficulties arising from latent liability, it appeared from what Mr Lancashire had said that he took a cautious view with regard to each aspect of the problem, and fearing that the reserves brought out might still be inadequate he added a substantial margin for contingencies. That course appeared to the speaker to be a travesty of a valuation. What should be done was to try to assess accurately the various factors which came into the calculation, and if any item remained uncovered, to make a reserve for that specific liability.

Absolute uniformity of method was unlikely to be obtained while there were different policies with regard to lump-sum settlements. It seemed to him that the basis of all methods implicitly involved something like a salary scale, and, with a scale, variations could be made in valuing the liabilities of different associations, the same methods being used but the weight of the different factors altered. In that way a dependable method of valuation would be reached and its results could be compared with the results of another valuation.

One speaker had mentioned the handicap which arose from the lack of published statistics with regard to workmen's compensation. The Committee on Compulsory Insurance was similarly handicapped and although it obtained quite a useful volume of information for its own purposes, unfortunately that information could not be published because it was obtained on the understanding that it was solely for the information of the Committee. He supposed that in due course there would be amending insurance legislation, and by that time the Mutual Indemnity Associations might themselves recognize that it was not to their advantage that there should be no published information concerning the way in which their business was being run.

Mr William Penman said that he had a certain amount of sympathy with what Mr Stockman had said. It would be useful if there were in existence something in the nature of a standard for the valuation of claims, even if it were no more than a standard by reference to which the effect of variations of basis might be measured.

He agreed with other speakers that the actuary when dealing with the outstanding liability for a trust fund ought to visualize what the position would be if the colliery went into liquidation and the trust fund had to meet its liabilities without any further support from the colliery. It was quite certain, in that event, that light work would be dealt with in a totally different way, for the successor to the liquidated colliery company would not be greatly interested in providing light work for men injured under the previous regime. So far as trust funds were concerned—and it would presumably apply also to Mutual Associations—the actuary should visualize what the compensation would be if that support were withdrawn, and he should make his estimate of liability on that assumption.

He did not think that the actuary making the valuation was very much concerned with the ethics of lump-sum settlements. Some lump-sum settlements were beneficial to the injured workman and others might be prejudicial, but, in any event, he thought that it would be wrong for the actuary to bring into his calculation any estimate of future profit on lump-sum payments. All that would happen would be that, as a result of payments of that description during the year, a portion of the assets would disappear from one side of the account and a portion of the liability from the other.

The President said that the principles underlying the work which the author had brought forward were familiar to them all, but the practical application of the principles to the problems considered was a matter with which comparatively few actuaries had to deal. In spite of that a good discussion had taken place, to which both actuaries and non-actuaries had contributed, and he was quite sure that the author would be gratified by the reception given to his paper, even though he might not have agreed with all that had been said.

They were all alive to the importance of not underestimating liabilities for outstanding claims in casualty business, and they remembered the opinion of the Cassel Committee that such underestimation had been an important factor in the failure of certain companies in years gone by.

But if that was important to the companies it was equally important to the Mutual Indemnity Associations, which, as the author had pointed out, handled something like 80% of the total claims for Workmen's Compensation in the coal industry. Unfortunately, very little was known about the financial position of those associations. Many of them no doubt were in good hands, well managed and financially sound, but others might not be. He entirely agreed with previous speakers that it was a matter of regret that legislation on the lines proposed by the Cassel Committee, and previously by the Clauson Committee and the Holman Gregory Committee, had not been passed. Actuaries were concerned to see that the beneficiaries of the funds they administered were properly protected and one of the strongest safeguards was publicity. It was a sad criticism of their parliamentary machine that a system condemned so long ago should still remain in existence. After the war one of the many urgent tasks of Parliament would be to find some means of dealing with legislation, which was to a large extent non-controversial, without the interminable delays which had been experienced in connexion with insurance.

Mr Harold Boag, who was unable to attend the meeting, has sent the following communication:

Mr Lancashire expressed the view that the medico-legal method might be dangerous, particularly for small cases. Where the numbers are susceptible to average I prefer the actuarial method, but I would employ the medico-legal method if the number of cases were small and the experience heterogeneous. In the case of an experience of moderate extent I employed a combination of the two methods as a check upon my results. I estimated the amount payable at the 5-year duration point by an examination of each individual case in the light of its medical and legal history and applied rates of recovery, mortality, commutation and discount to the aggregate of such estimated amounts. These results, together with estimates of the discounted value of the weekly payments and the amounts to be paid in commutation up to the 5-year point gave the required reserve values. If practically the same rates of recovery, etc., are used as are employed in the calculation of the actuarial 'multipliers', then the main difference between the two methods is that in one case an aggregate of individual estimates of the reduction in the weekly rates is used instead of a calculated 'ratio of diminution'.

I agree with Mr Lancashire as to the difficulties introduced into the problem of valuation by possible changes in economic conditions. In addition to the special effects on the rates of claim, recovery and weekly payment there is the effect of economic conditions reflected in the rate of interest (and consequently the Post Office Table to be employed) and depreciation of investments. It must be remembered, however, that variations in economic conditions would affect the results of the valuation whichever method were employed, and I do not think this is an argument against a conservatively arrived at 'ratio of diminution'. Also the ratio of diminution is expressed as a percentage of the initial full rates of the actual cases to be valued, and part of the variation (i.e. that due to high or low full rates of compensation) is therefore allowed for in the calculation.

I am particularly interested in Mr Lancashire's remark that the willingness of an employer to provide light work is hardly a matter to be taken into account in insurance. The two Associations for which I act have endeavoured to meet this point by giving a rebate to their members of 75 % of the gain which the Associations make owing to the provision of light work.

With regard to the treatment of lump-sum payments in cases where a policy of commutation is consistently adopted, it would lead to a lack of faith by the layman in the results of the valuation if the gains under this head were to be entirely ignored. One of my Associations commutes from 50 to 75 % of its 6-months' nystagmus cases and about 30 % of its 6-months' accident cases, and to ignore the fact of such commutation would undoubtedly over-estimate the liability. On the other hand, in view of the fact that recent and projected legislation may adversely affect the number of commutations, it is certainly wise to adopt very conservative rates for this factor.

I think it is true that in recent years there has been a tendency for the number of years' purchase paid in commutation cases to increase, and this view is supported by the experience of a north-country association in which the figures for the five years 1936–40 were 3·9, 4·3, 4·8, 4·4 and 5·2 respectively. Care should be taken, however, in interpreting figures derived from overhead averages relating to the coal industry. The number of years' purchase depends chiefly on the age and the nature of the case, e.g. in our Associations accident cases of about age 40 are at present commuted for approximately seven years' purchase, while nystagmus cases of all ages average about three years' purchase. A variation in the age distribution or the proportion of nystagmus cases to the whole may, therefore, account for a change in the average apart from any general tendency of variation in amount.

Mr Lancashire has observed that the rate of recovery would be affected if the *nil* cases are included in the rate of recovery. In our statistics they are so treated on the 'actuarial cards', but when rates of recovery are being calculated for premium purposes the suspended cases are 'written back' and the rates are also adjusted for deaths and recoveries amongst the suspended cases.

Cases of silicosis, being of rare occurrence in the north of England pits, have not been treated as a separate class, but if they were of more frequent occurrence they would undoubtedly, as suggested by Mr Lancashire and Mr Porteous, require special treatment. These remarks will probably also apply to pneumokoniosis when this disease is brought within the Workmen's Compensation Acts.

indefinitely large hypothetical population at the beginning of the year and the death, (if mortality was being studied) could be noted at the end of the year or, alternatively, a sample could be taken at the end of the year from all deaths in the hypothetical population. From a probability or mathematical statistical point of view there was no difference whatever in the results obtained in those two ways. The second way indicated what the author meant when he said 'predetermined but unknown amount of sickness'.

On the first page of the paper, he found it difficult to agree to the use of such language as 'The number of weeks of sickness is a more complex measure than the number of deaths, because it depends not only on the probabilities of falling sick...' and so on. To try to put various statistical functions into an order of complexity was a difficult matter, and he had been pleased to hear it said in the discussion that in fact the sickness case should be looked upon simply as a set of probabilities of falling sick for duration t; i.e. there was the proportion not sick at all, the proportion sick for one day, the proportion sick for two days, the proportion sick for three days and so on. In that form a probability distribution was obtained, from which nearly all the statistical problems could be solved comparatively easily, including the form for the variance.

His last point was with regard to the author's suggestion on p. 21 that it was possible that a large friendly society might have a little of the character of a stratified sample: he thought that the type of stability which the author had in mind in connexion with the branches of a friendly society was of a totally different order from the type of stability involved in a stratified sample.

Mr B. Benjamin remarked that it was noteworthy that papers read before the Institute were more and more concerned with what was currently termed statistical mathematics. That was symptomatic, he thought, of the widening outlook of the Institute, which was reflected in changes in its examination syllabus. With the author it was a fairly early symptom, and it had taken some time for the diagnosis to be confirmed in the pages of the *Journal*, because the paper had been written in 1941, when the author had been unwell and had devoted some of his enforced leisure to studying the variance of sickness.

Since he had encouraged the author to overcome his initial diffidence about the paper, it would be churlish on his part to criticise it, even if he were capable of so doing. He proposed, therefore, merely to refer to one or two points which seemed to arise from it.

It was a practical paper; the practical application had been outlined in the synopsis, though he did not think that it was repeated in the paper. He would like to assure the Opener that the application of the formula was fairly simple, and that the arithmetic was not too formidable. There was also some possibility that the approximate relationships indicated by the values of K on p. 18 might enable the theory to be applied without a great deal of work.

The main benefit which seemed to flow from the paper would be, he thought, to students, because he agreed with Mr Perks that, if they took the trouble to follow through the paper and to digest the mathematics, they would gain a great deal from it. The promise which was given at the top of p. 16, to develop the theory from first principles, was fulfilled, and the paper would encourage and stimulate those students who might wonder about the application of the wider field of statistical mathematics to which they were being introduced.

The diagrammatic treatment of the correlation between the various elements of sickness was commendable. In the paper the correlation between sickness in different periods was not developed, but it was clear, he thought, that the same principles that had been outlined in the paper could be applied to that problem.

Sickness as measured by claims on a friendly society was not, of course, the same thing as sickness as understood by doctors. The administrative interference to which Mr Perks had referred operated to make the variable much more complicated than it would otherwise be. Those who had been acquainted with what was known as quality control would have seen the way in which interference by the engineers led to the reduction in the variance of the particular commodity which was the subject of statistical control, and it seemed to him that, in much the same way, the variance of sickness claims might be affected by the interference of the administration.

Apart from the value to students of the step-by-step development of the mathematics of the paper, he thought that the detailed analysis which the author devoted to accounting for the difference between the expected and actual variance could also be regarded as a model exercise in criticism.

Mr M. Lander also expressed his pleasure at reading the paper. So much statistical work, he said, was being published in journals, such as *Population Studies*, by people other than actuaries, that he thought that it was a good thing for papers such as the present one to be published if actuaries were to maintain their claim to be experts on that side of the work.

Unfortunately, he had not had time to give the paper the detailed study which it deserved, but there were a few minor questions which he would like to put to the author. The author said that the only theoretical work which had been done was by Hardy and by Beard. There was, however, some work published in the *Transactions of the Ninth International Congress*, Vol. III, and he wondered whether the author had taken that into account, or whether he thought that it was not worth mentioning. There was also a paper by Mauritz Sundström in *Skandinavisk Aktuarietidskrift*, 1944, pp. 176–228, which tackled the question of sickness from a theoretical basis. Admittedly the author considered only the National Health Insurance funds in Sweden, but he went into the theoretical formulae underlying the sickness function. It would be of interest to know whether the author had happened to come across that paper, and if so whether he thought that there was anything in it. It was written in German, but he himself could let anyone who was interested have an English translation.

He had read that evening's paper only cursorily, but he thought that he was right in saying that the author did not say very much about the way in which his function s_t was measured from given statistics, except that on p. 19 he said 'The number of weeks' sickness per year, in any week of duration, which can be obtained by differencing the columns of Table 1 on p. 593 of the Health Insurance Report, is a close approximation to the force of sickness, s_t'. He wondered whether the author had investigated how to make consistent and unbiased estimates of the parameters of the distribution, and also questions of sufficiency and efficiency. Some interesting work in a recent paper in *Skandinavisk Aktuarietidskrift* was, it seemed to him, relevant to those questions.

Mr J. H. Gunlake, in closing the discussion, remarked that the closer was happy whose points had not all been made by previous speakers. If he put a few of the points which he had noted in his own way it might escape attention that they had been made already.

Not many weeks had gone by since the President in his Address made the remark that 'sickness is a notoriously unstable thing, affected appreciably, and sometimes violently, by subjective as well as by outward factors'. The President gave some very notable figures to illustrate that remark. Presumably 'outward factors' would include such things as epidemics, malnutrition, and (unhappily, in the modern world) even aerial bombardment, and he wondered whether in mentioning subjective influences the President had in mind something like his own conviction that many illnesses—including, more often than was generally recognized (at least among actuaries), the last fatal illness—had their origin and encouragement in such things as boredom and unhappiness. He felt strongly that it was this interference by the spirit and by human volition that made the statistics of sickness, and even of death, so baffling—because they were baffling; notwithstanding all the paraphernalia of graduation and modern statistical methods, the fact was that very little indeed was known about mortality, and still less about sickness.

The author was perfectly well aware of that and was, quite properly, not in the least deterred by it from constructing and using his microscope; but had the author considered focusing the microscope on accident statistics? Mr Beard had suggested the possibility of working on fire statistics, and accident statistics might provide a happy compromise.

Turning to some of the details in the paper, he could not help recalling a remark made by Mr Redington, in the discussion which followed Daw's paper a few years ago, that the square root of npq, as a measure, was borrowed from the probability theory which underlay such things as coin tossing and urn drawings, and was really a stranger to the study of mortality. Personally, he suspected that it was still more exotic to the study of sickness rates, but Beard made use of it in the paper in *J.S.S.* Vol. VII, and with what might be called a sigh of resignation, if not a sniff of disdain, the author had followed suit. He himself was tempted to ask whether the distilled essence of research must be poured into that particular vessel; in other words, was there no alternative to Table 1?

Attention had already been drawn to the fact that in that Table the 'All periods' figures exceeded those relating to the 'First year', which in turn were greater than those for the 'First three months', at all ages except 20, and there might be some ground for suspicion that the statistics at age 20 (and perhaps also 21) were affected by the same disturbing elements which forced the author to isolate ages 18 and 19 in Table 2.

With regard to Table 2 generally, he would like to ask how far, in the author's view, his conclusions hung upon the particular analysis of sickness in Cd. 6907, and whether he had considered comparing that analysis with the figures given by Rhodes in *J.I.A.* Vol. LXXII.

The main problem which arose in dealing with sickness rates in daily practice was well described by Hardy in the sixth of his lectures on the *Theory of the Construction of Mortality and other Tables*. After mentioning that the actuary was nearly always faced with the choice between using, on the one hand, tables based on extensive data of doubtful applicability to the case in question, and, on the other hand, tables based on more appropriate but more restricted data, Hardy observed that in the latter case it was important to be able to form some opinion of the extent of the probable errors involved in the data and their effect on the financial values. Hardy went on to add that the important question for a life office or similar organization—such as, of course, a friendly society—was the average amount of the annual or quinquennial fluctuation in profit due to deviations of the claims from the normal amount.

To that might be added one other problem, and that a very great one, the problem of interpreting the experience of a friendly society, profoundly fickle as it so often is, as a guide to the valuation basis. In that connexion he was very relieved to hear the remarks which Beard had made on the subject of small societies. The author referred to those practical problems in the last paragraph of the synopsis of his paper, but although the paper did provide the means of examining at least some of them more closely, for anyone like himself, who was engaged in a kind of general practitioner work in dealing with friendly societies, something a little more detailed on those lines would have been helpful. He hoped that the author would be tempted to carry his researches a little further. The technique which the author applied to the complete expectation of life, for instance, suggested that it could be extended to the annuity value, and to other monetary functions.

To come back to sickness problems, he felt when he read the paper—and everything which had been said in the discussion had reinforced his view—that the 'All periods' figures required a good deal more study. He agreed with the remark made in the discussion that they were the most important; the split into various periods was really a financial split, an analysis of the sickness rates according to the rates at which they were compensated. There seemed to be something very peculiar about the 'All periods' figures which needed looking into a good deal more. That brought him back, full circle, to his first suggestion about accident statistics, which might perhaps provide more easily handled material.

The President (Sir George H. Maddex, K.B.E.), in proposing a vote of thanks to the author for his paper, said he thought that the author could congratulate himself on his good luck; he had made a brave effort to develop further the statistical theory which might be considered appropriate to functions of the character of sickness rates, and those who were more competent than he could claim to be to examine that matter seemed to have been on the whole fairly well satisfied with the author's exposition.

Mr Gunlake had quoted what he might have said himself, perhaps in different words. To the practitioner in sickness insurance and friendly society work there were two questions. One was whether the beautiful mathematical implement could really be useful to him, knowing as he did the extraordinary quirks which occurred in sickness experiences, and knowing also that it was on occasion possible to identify them as being due to specific factors, some of which were external in a sense not intended, perhaps, by Gunlake—for example, the imposition of a particular regulation which was almost at once reflected in a change in the amount of sickness recorded in the experience.

The second question would be how he could make use of this implement in his daily work. There were probably possibilities in that direction, but they had yet to be developed. The use of the instrument which had been provided—to adopt the author's simile, the use of that knife or razor or, perhaps it would be better to say, tin-opener—seemed likely to produce devastating results in the hands of an unskilled practitioner.

He would like to say one other word on the question of the particular rate of sickness which should be used—the 'All periods' sickness rate or some other. Clearly the 'All periods' sickness rate, i.e. the total amount of sickness experienced in a given year of age, was a starting point. Sub-dividing that, it seemed to him that there had been a certain amount of confusion, and he would like the author to clear it up a little. There was scope for a good deal of further investigation here. The natural division of sickness might appear to be into durations of attack, the period between becoming sick and ending the given sickness by death or recovery; but in fact in friendly society sickness statistics, and of course in the Manchester Unity experience, no data of that kind were examined; the divisions were not merely arbitrary but artificial, owing to the operation of the linking-up period. He would like to see some statistical examination of the difficulties which arose from those divisions.

The paper was a very interesting one and had given rise to a compact discussion which should be very satisfactory from the author's point of view as well as from that of his audience.

Mr L. E. Coward, in reply, said he would not attempt to make a full reply to the discussion, but he would answer a few of the points which had been raised, without being able to weld what he had to say into a very coherent whole. There seemed to him to be two principal matters which had been raised. First of all, there were a number of questions which really depended on the fact that the paper was a first attempt, and admittedly rough and ready; he had had to make over-simplified assumptions to get his material into a form in which mathematical results could be obtained. That applied, moreover, to the test which he had tried to devise, for which he had had to go back to the Manchester Unity data, which were fifty years old. He had not been able to find anything published since then which was really suitable.

Mr Gunlake had remarked that the binomial theorem was continually cropping up, and seemed to think that something else should be tried for a change. Personally, he agreed that it did often crop up, and on the simplified assumptions that he had been forced to make, it seemed to be the natural and correct thing to use.

He had been interested in the opener's remarks. He thought that perhaps the opener exaggerated the importance of the assumption made in the paper that a person could not fall sick more than once in a year. Having gone through the theory without that assumption and having constructed what he thought were the correct formulae without it, he found their appearance so appalling that he felt that he could not possibly inflict them on the meeting. Moreover, the actual difference, so far as he could determine it, was not very great.

The opener thought that the formulae gave values which were too high, and he himself was certainly forced to that conclusion, if it was meant that the assumptions behind the formulae gave results which were too high. He was most gratified to hear that Mr Perks agreed that the formulae themselves should be satisfactory where the experience satisfied the particular assumptions.

Reference had been made to the question of the actual durations which were included at different times. His own way of looking at it was the following. If one particular entry in a sickness table were considered, the sickness included would, or should ideally, be that occurring during one particular year of age, the period of observation being one year. If only part of an illness occurred during the year of observation, then only that part should be included in the entry. It was the variance of such a total of sickness that he had been investigating.

With regard to the test which he had used, there had been a certain amount of criticism of the use of Appendix V to the National Insurance Act Report, and indeed of using the Manchester Unity rates, but he was forced to use them, because it was the experience for which the most complete analysis so far had been made. He had to admit that he had not gone into all the questions which Mr Lander raised, and he thought that questions of sufficient and efficient statistics were rather beside the point, because he was trying to do a very much simpler and cruder job.

He quite agreed with Mr Beard that there was much yet to be done. It seemed to him that the practical use of the work would be twofold; first of all, in making it possible to decide the size of society giving sickness benefits which could be efficiently run, and, secondly, in interpreting the statistics and in understanding what the results of sampling could tell the actuary about sickness statistics. He felt that the implement which should be used, if it was attempted to apply the method in practice, was Table 1, with such modifications as might be found necessary because of alterations in the assumptions. If better assumptions could be found, perhaps the ratios in Table 1 would be reduced somewhat, but a Table in that form should be of most practical use.

Mr R. H. Daw has sent the following note: In the section *Comparison of actual and expected standard deviations* the author tends to give the impression that he is using the Manchester Unity Experience to test the correctness of the assumptions on which he has based his analysis, but actually he is only testing the graduation of the Manchester Unity table on these assumptions. For, if he found disagreement, the reason could be either faulty assumptions, faulty graduation, or both. He is, of course, well aware of this as shown by his remarks on the advantage of using an accepted graduation, but I think the point requires emphasis.

In order to test how far the results of theory agree with fact, a test independent of the graduation is needed. In the case of mortality tables a test which is nearly independent of any graduation has been proposed by Redington and Michaelson and was used in my paper *On the Validity of Statistical Tests of the Graduation of a Mortality Table* (*J.I.A.* Vol. LXXII, p. 174). The assumption is made that the third differences of the true underlying rates of mortality are small, so that the third differences of the ungraduated rates of mortality will be mainly composed of random errors. If, therefore, these third differences are divided by their theoretical standard deviations, the resulting ratios should be nearly normally distributed with zero mean and unit standard deviation, provided the random errors do actually follow the binomial distribution and the data are homogeneous between ages so that a smooth graduation is justified.

Now that Mr Coward has given us the standard deviation of sickness, this test can be applied to the Manchester Unity data provided we can assume third differences are small. This seems reasonable as sickness rates appear to be of the same nature as mortality, for the Manchester Unity Experience has been graduated by a summation formula, and, like mortality rates, attempts have been made with reasonable success to graduate sickness rates by Makeham's formula (*Biometrika*, Vol. II, p. 503 (1903) and Vol. III, p. 52 (1904)).

I have, therefore, worked out, for ages 20–65, the ratios (r_x) of the third differences of the ungraduated sickness rates (S'_x) to their standard deviation, i.e.

$$r_x = \frac{\Delta^3 (S'_x)}{\sqrt{\left(\dfrac{V'_{x+3}}{E_{x+3}} + 9\dfrac{V'_{x+2}}{E_{x+2}} + 9\dfrac{V'_{x+1}}{E_{x+1}} + \dfrac{V'_x}{E_x}\right)}}.$$

The standard deviation of r_x (σ_r) was then calculated and the results are shown in the table below. If the assumptions on which the standard deviation of sickness has been calculated are in accordance with fact, σ_r will not differ significantly from unity. As shown in my paper, the standard error of σ_r, on the hypothesis that the population value is unity, is about $1\cdot5/\sqrt{(2n)}$ and this quantity is given in brackets in the table.

	Number of values of r_∞	Standard deviation of r_∞ (σ_r)
Manchester Unity: 'First three months' sickness	46	·85 ($\pm$·16)
'All periods' sickness	46	·59 ($\pm$·16)

It will be seen that σ_r for the 'First three months' sickness is not significantly different from unity, but for the 'All periods' sickness it is significantly less than unity. These results agree with those obtained by the author for the Spencer 15-term formula graduation, but the r_∞ test has the advantage that graduated sickness rates are only used in calculating the standard deviations and a different graduation is not likely to alter these greatly.

As regards the 'All periods' sickness, I am inclined to agree with the author that the lack of independence between sickness at successive ages is the principal cause of the abnormally close agreement between theory and fact, but there is scope for research to investigate the nature and magnitude of this correlation.

Mr Perks has sent the following analysis which was referred to in his remarks.

The analysis refers to sickness in the year from duration a to duration b where b does not exceed one year. The frequency function is defined by $\frac{dS_y}{dy} = -n_y$, where y is the duration of sickness at the termination of the sickness. The total sickness can be classified as follows:

1. Sickness terminating in the year at duration y $(a<y<b)$
   (a) which commenced in the year or which was current at the beginning of the year at duration $<a$,
   (b) which was current at the beginning of the year at duration $>a$ and $<b$.
2. Sickness passing through duration b during the year
   (a) as for 1 (a),
   (b) as for 1 (b).
3. Sickness passing through duration y at the end of the year $(a<y<b)$.

The contributions to the chance of sickness and to the first and second moments are in accordance with the following scheme:

	Chance of sickness	First moment	Second moment
1 (a)	$\int_a^b\int_0^{1-y+a} n_y\,dr\,dy$	$\int_a^b\int_0^{1-y+a} (y-a)\,n_y\,dr\,dy$	$\int_a^b\int_0^{1-y+a} (y-a)^2\,n_y\,dr\,dy$
1 (b)	$\int_a^b\int_a^y n_y\,dr\,dy$	$\int_a^b\int_a^y (y-r)\,n_y\,dr\,dy$	$\int_a^b\int_a^y (y-r)^2\,n_y\,dr\,dy$
2 (a)	$(1-\overline{b-a})\,s_b$	$(b-a)(1-\overline{b-a})\,s_b$	$(b-a)^2(1-\overline{b-a})\,s_b$
2 (b)	$\int_a^b s_b\,dr$	$\int_a^b (b-r)\,s_b\,dr$	$\int_a^b (b-r)^2\,s_b\,dr$
3	$\int_a^b s_y\,dy$	$\int_a^b (y-a)\,s_y\,dy$	$\int_a^b (y-a)^2\,s_y\,dy$
	Total	$\int_a^b s_y\,dy = u_0$	$2\int_a^b (y-a)\,s_y\,dy - \int_a^b (y-a)^2\,s_y\,dy = 2u_1-u_2$

$$\text{Variance} = 2u_1 - u_2 - u_0^2.$$

If b is greater than one year, and in particular for 'All periods' sickness, care needs to be taken over the limits of the integrals, but the results obtained are the same as the author's, e.g. formula (9). The above variance applies to a sample of one. For a sample of N the variance is, of course, multiplied by N.

[In comparing with the paper, pp. 24 and 25, it should be noted that Perks uses the more general limits $\int_a^b$ in place of $\int_0^t$. Perks's 1 (*a*), 1 (*b*), 2 (*a*), 2 (*b*), and 3 are the author's (2)(*a*), (2)(*c*), (3), (2)(*d*) and (2)(*b*) respectively. Perks has not included expressions corresponding to the author's (I)—the chance of no sickness—which contributes nothing to the moments. They each deal differently with the moments of Perks's 1 (*b*) and 2 (*b*). Coward first combines the two chances to obtain

$$\int_0^t (s_y - s_t)\, dy + \int_0^t s_t\, dy = \int_0^t s_y\, dy,$$

and uses the moment-generating function $e^{\alpha y}$ for the combined chance. Perks deals with the chances separately obtaining moment-generating functions

$$\int_a^b \int_a^y e^{\alpha(y-r)}\, n_y\, dr\, dy \quad \text{and} \quad \int_a^b e^{\alpha(b-r)}\, s_b\, dr,$$

the sum of which, as can easily be verified, reduces to $\int_a^b e^{\alpha(y-a)}\, s_y\, dy$. Eds. *J.I.A.*]