

It does not therefore ensue that any reason has been adduced for doubting the accuracy of your statement, that, the coin "being true, one result might be looked for at any time just as much as the other"; but if any one has any impression in favour of the opposite opinion, let me recommend him, before embracing it, to view the question in another light. A hundred and one dynamically true coins are fairly tossed, and then one of them is covered. The others, when examined, are all found to have head uppermost. Is not the hidden coin as likely to have head as tail uppermost?

I am, Sir,

Your obedient Servant,

Royal Military Asylum,
Chelsea, August 24th, 1853.

W. J. REYNOLDS, B.A.

THE CHANCES OF PREMATURE DEATH AMONG SELECT LIVES.

To the Editor of the Assurance Magazine.

SIR,—As I presume the paper last read at the Institute, in which my name was somewhat frequently referred to, will be inserted in your present Number, perhaps you will allow me to make some comments upon that paper, in order that such readers as were not present at the Institute, may not imagine, that I thereupon became a convert to Mr. Spens' views.

If I understand that gentleman's notions correctly, he seeks to maintain, that for a single year's insurance, the value of selection neutralizes all distinction of age from 21 to 45, and that the *true* premium per cent. for each age alike, is about 10s. 8d., which is the money value of 0.55, when discounted for a year at 3 per cent. I further understand Mr. Spens to maintain that all other ideas are essentially "fabulous," and more especially those set forth in what he is pleased to call my "elaborate treatise 'On the Chances of Premature Death, and the Value of Selection among Assured Lives.'" Mr. Spens, indeed, does not demur to the correctness of the conclusions, as deduced by me; for he expressly says, "*I have not one word to say against these results being correctly deduced from the data on which they are founded; but it is my argument, that they are the correct deductions, reasoning from the data, assuming them to be correct data.*" Is it then, an unfair presumption to consider, that data collected and arranged, under the joint superintendence and responsibility of ten well known actuaries, may at least have some claim for correctness and consideration? It was, indeed, the obvious justness of this claim that induced me to undertake the laborious task referred to.

True it is, that the experience so collected, was of necessity the experience of policies, and therefore not necessarily that of lives; but it had already been tolerably well ascertained, that in moderately large numbers, as a matter of ratio the number of increased deaths, when reckoned by policies, was sufficiently balanced by that of the increased number of policy survivals, when reckoned on the same principle. As an instance of this: Mr. Griffith Davies' well known Table of the Equitable Experience was founded on Mr. William Morgan's statement of policies (*Essay on Rise and Progress*), not upon Mr. Arthur Morgan's experience of Equitable lives; and yet the two tables did not so materially differ as to render Mr. Davies' table

“worthless.” The same analogy of result was equally expected and found, upon due examination, to exist in the policy experience of the 15 combined Offices. Indeed, no unbiassed actuary can calmly compare the town and country, male and female, and British and Irish, policy experiences, with those of the Equitable lives and the Carlisle and Northampton Tables, without admitting the success of the experiment, so far as these identical materials are concerned, as to the fitness of these policy ratios, at least for practical purposes; especially when it is remembered, that even reckoning by lives does not relieve us from the difficulty, that the amounts to be payable upon the deaths of one set of persons, may be very different from those payable upon others, although equal in numbers.

The general mortalities, then, as deduced from the policies so collected, being at least akin to those derived from other tables of mortality, it became a fair presumption, that the mortality during the first calendar year after selection, reckoning by policies, would also, *pro tanto*, shape out a reasonable scale of what might be expected to occur among lives; and the more usefully, because the Equitable experience, of itself, was not sufficiently extensive to determine this more isolated question. That such experience, or perhaps that of any other single Society, should not be held sufficiently authoritative on such a point, was clearly enough indicated, by comparing the relative mortalities for the first and second years of insurance, which, if carefully deduced, could not be reasonably supposed to materially differ from each other, when large numbers were concerned; whereas, relying on the Equitable experience alone, if the mortality during the first calendar year, be thence taken, merely as 0·5 per cent., that for the second must be taken as 1·15, or in the ratio of about 1 to 2·4—a ratio apparently so excessive, as a general rule, between the first and second years of insurance, as not likely to commonly recur, either in the Equitable or any other Society.

But if we take, not merely a part, by excluding the Irish (which is virtually seeking for a minimum), but the *whole* available experience of the 62,014 assurances, including the Equitable, we find 1·0352 as the collective rate of mortality per cent. for the first calendar year after assurance, and 1·4843 for that of the second, or in the ratio of about 1 to 1·4—a much more likely ratio, and one which indicates that the rate deducible from the second year of the Equitable experience, or 1·15, is better as a general type than that of the first. Indeed, the general rates for the first, sixth, eleventh, sixteenth, twenty-first, and twenty-sixth years after insurance, as deducible from the totality of the policy data, will be found (*Life Contingency Tables*, p. x) to be 1·0352, 1·7984, 2·1350, 2·7463, 3·2968, and 3·9992. Considered as a series, these numbers obviously present types of regularity; and thereby strongly inculcate the canon, that however bizarre the experience of a single Society may be, yet that this is often merely an indication, that some other Society may have had an experience, equally bizarre in an opposite direction, and that it is by combination alone that the true average can be indicated.

On examining further how the collective rate of mortality of 1·0352 per cent. for the first calendar year after assurance is made up as to ages, we find the actual rates to have been, for 21 to 30, 0·62184; 31 to 40, 0·74847; 41 to 50, 1·14139; 51 to 60, 2·13132; and 61 to 70, 4·1874—a series, like the former, of considerable regularity, and sufficiently so, to induce us to believe, that the mortality increases from age to age among recently select lives, on the same principles, as it does among lives in general. Moreover, we are to think that such rates, treated specially, cannot be mate-

rially excessive, if we remember that the Carlisle Table is certainly not a very overcharged table, even for the very best lives, and therefore, that any rates much lower than the Carlisle, might be presumed to be verging, rather towards the limit of deficiency than of excess. Now the rates of mortality per cent. for one year, as indicated by the Carlisle Table, for Mr. Spens' terminal ages of 21 and 45, are 0·6946 and 1·4809, and by the Policy Experience Table, for the first calendar year after assurance, 0·5891 and 1·0877, or about 20 per cent. lower at these ages than by the Carlisle Table—a set off for recent selection, at least prominent enough for all practical purposes, considering the avowedly faulty graduation of the Carlisle Table itself.

With such safe and intelligible inferences, then, before us, it may be fairly asked, how it is that the manager of the Scottish Amicable, can yet delude himself into the belief, that Offices in general will be content to disregard all distinction of ages for one year insurances, and be satisfied to consider 10s. 8d. per cent. as the true and only premium for all ages from 21 to 45? Without wishing to be particularly stringent upon the nature of Mr. Spens' own cogitations, we may presume, that he has first been delighted with the ancient paradox of the nullity of age, as to the relative chances of death among healthy lives, and then taxed his ingenuity for modern arguments to defend it. Thus, in his former paper, referred to by himself in support of his views, having in vain attempted to show, even by the limited experience of the Equitable, that the mortality between 21 and 25 is the same as between 41 and 45, he rather naïvely says, "*Although there is a slight difference in the above in favour of the younger lives, one death more among them, or one death less among the older lives, would have made the proportion almost the same*" (p. 4). The permission, however, to add or subtract a unit at pleasure, when such small numbers as 8 and 10 are in debate, is a logical concession that few could be expected to ask for, and still fewer to grant.

Again: in p. 7 of the same paper the following passage occurs, which is also cited to show that, with Mr. Spens, the preceding is a recognized form of *petitio*:—"While I am quite willing, that this paper should be viewed with the suspicion justly due to the arguments of a party desirous of supporting a particular theory, I will not omit to notice that the limited experience of the Amicable does contradict it; for out of about 224 lives from about 21 to 25, there is 1 death, while among about 500 from about 41 to 45 the deaths are 5. But the disadvantage of the smallness of the numbers, especially in the younger ages, is obvious from this, that one more death among the younger lives would have made the proportion equal." With such an aid as the power of changing, at pleasure, one into two, or of doubling the deaths, what results might not be declared patent?

Further: in his last paper, after having inveighed against the use of policy experience, Mr. Spens does not hesitate himself, to employ the somewhat rougher comparison of merely the sums assured and the amount of claims, without any distinction whatever, either as to lives, policies, numbers, or ages, and thereby arrives (as he conceives by a sufficient process) at his 0·55 per cent. for each age. Partially feeling, however, the intrinsic weakness of such a procedure, Mr. Spens adds: "*It may be asked why I have got the information (as to certain Offices) in reference to the sums assured, and not the individuals. One reason was, that I believed the return could be more easily made; and on the whole, I am inclined to think that no error of any consequence can have arisen, from estimating the rate of mortality in*

this manner." Had an equally wide interpretation been accorded to the reckoning by policies, Mr. Spens would obviously have been obliged to resign his "favourite theory," and your readers have been spared this exposition of the presumed fallacy of it.

It is right however to state, that Mr. Spens does not wholly stand alone in his ideas as to the non-increase of mortality as age progresses. The Government Annuity Table, and the Succession Duty Table which is founded on it, and even the Carlisle Table, present instances of obvious irregularity.

I am too well versed, I hope, in the history of the subject, ever to have stated that the rate of mortality necessarily increases with the age; indeed, the following quotation from my work already referred to, will at once protect me from so wide an assertion:—"If the integral rate of mortality were presumed to be equal at all ages, it is obvious the probabilities of surviving any defined period would also be equal at all ages, or the number of survivors would decrease in geometrical progression. It is now, however, generally admitted by actuaries, that the rates of mortality after infancy, and consequently the premiums of assurance, should be graduated so as to increase with the age, although, in nearly all original data, deviations from this presumed increase in the rate of mortality have been observable. These deviations, however, not being of a fixed and determinate character, but differing in different data, have been gradually disregarded in practice, and a conditional augmentation generally adopted." (*Life Contingency Tables*, p. 2.) Those who object to this conditional augmentation, generally do so upon the plea of occasional experience to the contrary; but such a plea, is commonly allied to an experience so limited, as to pourtray, that the upholders of it have not sufficiently distinguished, between a doctrine of certainty, which is not dependent on numbers, and a doctrine of probability which is. In a doctrine of certainty, the same die, thrown one hundred times, and then another hundred, would present in each hundred the same results. So far, however, is this from being the case in a doctrine of probability, that if one die of 100 faces had 99 white and 1 black, and another 98 white and 2 black, it would require a very considerable number of throws, of each die, to establish finally, and then only with a certain degree of probability, which of the two dice, judging from the throws alone, had the greater number of black faces.

Now, to reconcile the admitted increased chances of mortality, at extreme old ages with those of youth, the conditional augmentation in the annual ratio of mortality need not exceed upon an average 0.02; or, for 100 deaths at one age, that there should be, out of a similar number of living, 102 at the next. Consequently, when fairly represented as a problem of indirect probability, to distinguish between the mortalities at successive ages as a matter of experience, we must have a sufficient number of trials, to collaterally distinguish between, the throws produced by a die of 9,900 white faces and 100 black, and those of another, having 9,898 white faces and 102 black; so that, having regard to the limited extent of data we really possess which set forth the results of mortality at each successive age, except during childhood, it is only for decennial periods, or such as may be presumed to offer very obvious differences in the rates of mortality, that the actual results of any past experience, can be decisively appealed to, as probable types of the future. But it is obviously in every sense, in the absence of more extensive data for each age, *per se*, a mere gratuitous reversal of analogy, to attempt to

set up, as some have done, the petty irregularities of limited data, as so many declared laws of nature, when, by the very amalgamation of the elements of such data, or by their extension, a simple law of increase is the only one fairly observable.

By adopting, then, this law of increase at great intervals, as a type of what might be expected for lesser intervals, were more extensive experience at hand, we not only protect ourselves from paradoxical triflings, but walk in the steps pointed out by the doctrine of probability itself: for if a series of increasing ratios at wide intervals be presented to us, the most probable interpolations, among the infinity of all the possible ones, are those of a cognate character with the general type of the declared series; and it is to detect what this general type is, that all mathematical formulæ essentially tend. Individually, I should be sorry to rob any healthy senior of the solace he may find in thinking he has an equal chance of surviving to his next birthday as his juniors; but, as an actuary, I consider it is merely attempting to sustain an obsolete paradox to pretend, that such is to be taken as the general attribute, displayed to us by a sufficient experience.

In conclusion, I may be allowed to state, that I have not been content to allow Mr. Spens' paper to pass unnoticed (as I might, I conceive, very safely have done, so far as the argument is concerned), but have rather replied to it at full length, not only as an act of courtesy to that gentleman, whom I personally respect, but also because I feel convinced that, however actuaries may differ in opinion, yet that free and open discussion, without ill feeling and without personal imputations, forms one of the surest indications that the public can possess, that the subject of insurance, in which they all have so deep an interest, is being at once honestly and efficiently studied.

Your obedient Servant,

London, Sept. 1, 1853.

EDWIN JAMES FARREN.

ON THE VALUES OF REVERSIONS PAYABLE AT THE INSTANT OF DEATH.

To the Editor of the Assurance Magazine.

SIR,—I beg to thank you for correcting the errors in my letter published in last *Magazine*, and, through you, to apologize to your readers for the obscurity which they necessarily occasioned in the meaning. I hope you will also allow me this opportunity to extend my observations a little on the subject, lest I should not have been sufficiently understood.

The determination of the value of reversions payable at the *instant* of death may not on first view seem to be an inquiry of much practical utility, since it has never been the practice, so far as I know, to pay claims immediately after proof of death; but the fact that some Offices now do pay claims within a few weeks thereafter, renders a decision on the point the more urgent, and the inquiry more inviting to the actuary, irrespective of the interest which usually accompanies investigations of this kind.

It appears to me, that since *one year* has naturally become the unit for measuring the decrements of human life as well as the improvement of money, the practice has arisen, in life calculations, of assuming the reversion to be payable at the *end* of the year in which death occurs, when the conversion of interest takes place, the same being then due—or, in other words,