CORRESPONDENCE

(To the Editor of the Journal of the Institute of Actuaries)

It has been drawn to the attention of the Secretary of the Continuous Mortality Investigations Bureau that there is an inconsistency in the paper: 'Considerations Affecting the Preparation of Standard Tables of Mortality', presented to the Institute on 28 January 1974 (*J.I.A.* **101**, 135). He has asked me to let you know the corrections that should be made.

The parameters for durations 5 and over in Table 8 are inconsistent with the values of q_x in Table 9. In fact the parameters are wrong, having been transcribed by me incorrectly from an earlier trial graduation. The correct values for durations 5 and over are:

Y	24·798937	not	24·798921
$B \times 10^3$	0·76654669	not	0·76666901
c^5	1·5726555	not	1·5726325
$H \times 10^4$	1·0340378	not	1·0343051

The values in the paper that purport to be calculated from the parameters are nevertheless correct, having been calculated from the correct set of parameters as quoted above.

A further misprint should be noted: in Table 9 the correct value for $q_{(x-1)+1}$ for age 40 is 0·00121095 not as printed ·00121905.

Yours faithfully,

27 *January* 1975

A. D. WILKIE

In his note 'Optimizing the Term of an Investigation into Decremental Rates' (*J.I.A.* **99** (1973), 69) G. C. Taylor makes an attempt to quantify a matter which has so far been considered largely qualitatively. He states explicitly at the beginning that he is assuming that 'the object of a mortality investigation is to estimate a set of current mortality rates' and his method and comments are on this basis. For example on page 76 he says that the five-year term of the Australian Assured Lives, 1958–63, experience is significantly greater than his optimal term of about 1½ years 'for an investigation *aiming to calculate 1963 mortality*' (my italics).

I will first consider whether Mr Taylor's method takes account of the significant features of mortality data. I think it fails to do so in several respects:

(i) The long series of graphs in *J.I.A.* of the annual mortality rates in Great Britain (e.g. *J.I.A.* **96** (1970), 393) shows that, at the ages important to life assurance, the year-to-year variations in the level of mortality due to epidemics, weather conditions, etc., are substantially larger than any general trend. The Joint Mortality Investigation Committee clearly recognized this in the passage about the A 1949–52 quoted by Mr Taylor on page 82, yet his method assumes a general trend and ignores the year to year variations in the general level.

(ii) Assured lives data usually contain duplicates which increase the variance of the mortality rates, often substantially. An estimate is given in *J.I.A.* **101** (1974), 419, that the duplicates in the Australian 1958–63 experience, to which Mr Taylor applies his method, increase the variance of the mortality rates by an average of 80% and allowance for this would increase the optimal term he obtains.

(iii) The passage from page 76 of Mr Taylor's note mentioned above seems to imply that he would consider using a term which is not an integral number of years; this would almost certainly give biased estimates of mortality rates because of the seasonal variation. Over the four years 1967 to 1970 the male deaths in England and Wales in the successive quarters of a year average 28·9%, 23·4%, 21·4%, and 26·3% respectively.

The next question is whether Mr Taylor's stated object is relevant to mortality investigations.

I do not think it is, except perhaps in rare special circumstances. I have yet to meet an actuary who expects a mortality investigation to give an estimate of the mortality at the more recent end of the period covered. Surely the aim of, say, the Australian 1958–63 experience was to estimate the average mortality over the whole period covered or perhaps to obtain rates broadly applicable to about the centre of the period.

Because of the substantial year-to-year variation I do not think that the mortality of one year can necessarily be taken as an estimate of the average level of mortality at that time. To make such an estimate it is necessary to have regard to the experience of adjacent years, as was done in the case of the A 1949–52 experience.

While Mr Taylor's note may be of mathematical interest and perhaps have relevance to other decremental rates, I consider that it has little, if any, practical application to mortality rates. Also it should be realized that the resulting estimate using the optimal term, although it has minimum mean square deviation, is still a biased estimate of the mortality at the more recent end of the period covered. Had Mr Taylor's object been to find the optimal term for an investigation to estimate the rate applicable to the *centre* of the period covered and had a linear trend been assumed he would have found that his equation (1) gave an unbiased estimate of the rate he was seeking and equation (4) gave the optimal term as infinite.

Yours faithfully,

2 *January* 1975 R. H. DAW

Although I am indebted to Mr R. H. Daw for his comments on my paper 'Optimizing the Term of an Investigation into Decremental Rates' (*J.I.A.* **99**, 69) I am afraid that they do not force me to the conclusion that the paper was of no practical interest to those responsible for compiling tables of mortality rates.

Mr Daw's criticisms come under two headings: firstly, he questions my assumption that 'the object of a mortality investigation is to estimate a set of current mortality rates', and secondly, he points out three significant features of mortality data which he thinks are not taken into account adequately in my analysis. I shall attempt to deal with these separate criticisms in this order.

Mr Daw's idea is that, in contradiction of my assumption quoted above, the object of mortality investigation is 'to obtain rates broadly applicable to about the centre of the period'. I am unable to accept this for the simple reason that if it were true that actuaries were indifferent to the date to which the results of a mortality investigation applied, then, of course, they would be indifferent to the period over which the same mortality rates were measured. In fact, there would seem to be little reason for ever producing another mortality table since we already have the 1949–52 table which is broadly applicable to the beginning of 1951. Clearly such a position would be unacceptable to most actuaries who would claim that the 1949–52 table was gradually becoming outdated and that a more up-to-date table was necessary. By the phrase 'up-to-date' surely they would mean current, as suggested in my paper.

In fact, if Mr Daw's object of an investigation is acceptable, the position is even worse than I have described in the previous paragraph, for actuaries would not be *indifferent* to the period of an investigation but (in the case where mortality rates were changing linearly with time) would simply include all mortality data that had ever been recorded on the grounds that this reduces the variance of the results and they do not mind to which point of time these results relate anyway. This absurd result is pointed out by Mr Daw himself in his final paragraph. In that same paragraph he points out that 'the resulting estimate using the optimal term, although it has minimum mean square deviation, is still a biased estimate of the mortality at the more recent end of the period covered'. I would prefer (and consider it more meaningful) to reverse his statement and say that 'the resulting estimate . . ., although a biased estimate . . ., still has minimum mean square deviation'.

Before commencing an examination of Mr Daw's second type of criticism, namely his points numbered (i), (ii) and (iii), I should perhaps make some general comments in defence of

the optimization procedure which I have suggested. This procedure arose when I examined a table of mortality rates produced from the data of a single Australian Life Office. This office, which commanded something like 10% of the Australian market had produced a table of mortality rates based upon the 4-year investigation period 1950–53. It occurred to me that it might be very wrong for a single office of this size on the one hand, and virtually the whole of the British life insurance market on the other, to be conducting mortality investigations over the same number of years. This point is made obliquely at the foot of page 82 of the paper where it is stated that 'the smaller a life office (say), the less particular should it be about the data which it includes in an investigation of its own experience'. This much is obvious from very little thought, but just how the term of the investigation should be related to the volume of experience is not clear without some quantitative analysis.

This brings me to Mr Daw's point (i). Here he refers to the year-to-year variation of mortality in Britain. His point is, of course, a valid one, i.e. that an experience of the size of say the 1949–52 experience should contain just as many years as are necessary to make the mortality rates 'representative', whatever meaning we are choosing to attach to this. However, there are two reasons why the analysis given in my paper can still be relevant. Firstly, if one refers to the graphs (*J.I.A.* **96**, 396 and 397) cited by Mr Daw and examines for example that of the age group 45–64, one might well regard the most recent year as 'representative' on the grounds that its results are nearly the same as those of 4 out of the preceding 5 years. Thus without wishing to become unduly preoccupied with this particular set of mortality data, I would suggest that in a given investigation of mortality data one might be justified in taking the most recent period as representative. Secondly, even where this is not possible, the optimization procedure still has relevance to smaller investigations where it produces an optimal term larger than 3 or 4 years. In this case the optimal term for practical purposes is not bounded below by the number of years necessary to produce a representative experience, and it may well be of interest to smaller offices conducting mortality investigations of their own to know that they are quite justified in using periods of investigation of perhaps 7 or more years.

Mr Daw's point (ii) alludes to the error which arises in my formulae as a result of the existence of duplicates in assured lives data. As he points out, the result of this is to increase the optimal term of the investigations but it can be seen from an examination of pages 26–27 of the paper, 'Considerations Affecting the Preparation of Standard Tables of Mortality' (*J.I.A.* **101**, 133) that the variances of mortality rates are increased by a factor which varies, according to age, between limits of 1·10 and 1·67. As the optimal term is approximately a linear function of variance, the existence of duplicates increases it by some factor in roughly the same range—not a major adjustment.

Although I did not intend to recommend that the term of a mortality investigation could be chosen to be an integral number of years without consideration of the practical ramifications, this is not stated explicitly in the paper and I must forgive Mr Daw for placing this interpretation upon it in his point (iii). In the passage on page 76 which he quotes, I should perhaps have sacrificed conciseness and been more circumspect (albeit very pedantic) and said 'the term of the investigation was 5 years, which is significantly greater than the best practical derivate of the theoretical optimal term'.

Yours faithfully,

23 *January* 1975

G. C. TAYLOR