CORRESPONDENCE.

"MAXIMUM MORTALITY PERCENTAGES."

To the Editor of the Journal of the Institute of Actuaries.

Sir,—I have carefully read Mr. T. B. Macaulay's letter (*J.I.A.*, xxxii, 117) with the above heading, and I consider it an interesting contribution to the discussion of the important subject of the mortality among insured lives.

In my paper of 1870, I showed, from an examination of the experience collected by the Institute of Actuaries, that, taking all ages together, and comparing the actual deaths with the expected, in suitable groups of years of insurance, the series of ratios exhibited a maximum for the group of years of insurance, 11 to 15. Mr. Macaulay has now conclusively shown that such a maximum may appear in an aggregate for all ages, even when it does not appear in any of the series obtained from the same statistics arranged in quinquennial groups of ages.

I think, however, that Mr. Macaulay attaches too much importance to his results, and that they do not bear out the conclusions he draws from them. I would first point out that it is very incorrect to speak, as he does, of a maximum mortality "theory"; for the question whether there is a maximum in the series of percentages, or not, is a question of fact, and not of theory. So, again, when he speaks of my "opinion" on this point, I have to remind him that this is not a matter of opinion, but a matter of fact. Either there is a maximum, or there is not; any man's mere opinion on such a point is of very little weight, if any; and the question must be decided by an examination of the statistics.

What, then, are the facts? The table given by Mr. Macaulay enables us to answer this question very readily, although if we wish to have the years of insurance correctly, we must refer to the *J.I.A.* for April 1870. My description of the facts was as follows (page 349), "The law * * * of the actual mortality as compared with " the expected, increasing to a maximum, and then again decreasing, " is retained in a very marked manner throughout the interval of 20 " years, from age 56 to 75; and it will be seen further that the " figures involved are so large, and the results are so consistent with " each other, that we are forced to admit the phenomenon as a true " deduction from the facts. At both extremities of the above period, " namely from the age 46 to 55 at the one end, and from 76 to 80 at

" the other end, the same law appears in a less prominent manner;
" while under the age of 46, and above the age of 80, the rate of
" mortality increases continually with the standing of the assurance."
In order to show more clearly the nature of the facts which
Mr. Macaulay endeavours to explain away, I have extracted from
my paper the figures in columns (2) to (7) of the following table,
and added those in the last column.

Present Ages (1)	Number at Risk (2)	Deaths (3)	Maximum Percentage (4)	Years of Insurance (5)	Final Percentage (6)	Years of Insurance (7)	Difference (4)−(6) (8)
46–50	156,180·5	2,255	109·7	19–25	106·8	26–35	2·9
51–55	124,028·5	2,302	113·6	19–22	107·7	23–40	5·9
56–60	90,757	2,312	108·7	12–17	93·6	30–end	15·1
61–65	60,778·5	2,258	106·4	16–21	100·0	32– ,,	6·4
66–70	36,283	1,920	105·8	14–16	99·7	27– ,,	6·1
71–75	19,012·5	1,548	105·2	18–21	90·7	33– ,,	14·5
76–80	7,808·5	937	104·4	27–36	103·1	37– ,,	1·3
	494,848·5	13,532					

These facts are not to be got rid of by arguments such as those
used by Mr. Macaulay. If the law indicated by the figures, namely
the increase of the percentages to a maximum, and their subsequent
decrease, were not a reality; but, as Mr. Macaulay supposes, the
result of calculating the expected deaths by an unsuitable table; this
could be proved by calculating the expected deaths by a more suitable
table: and I trust that Mr. Macaulay will apply this test to the
figures, and publish the results. I am satisfied that, when this is
done, it will be found that the same law still prevails in the series of
percentages. It is certainly true, as pointed out by me in 1870, and
now in greater detail by Mr. Macaulay, that the use of an incorrect
mortality table may, when the expected deaths *at all ages* are
compared with the actual, introduce a maximum into the series of
percentages, which would not be there if a correct mortality table
were used. But, when this comparison is restricted to a small
interval of age, such as five years, inaccuracies in the mortality table,
even of the most serious character, can produce no such effect. If,
for instance, the mortality table employed gave, in a certain
quinquennial group of ages, a mortality 50 per-cent in excess of the
truth, *all* the percentages relating to persons who had attained those
ages, but had been insured for different periods, would be reduced in
the same ratio; each of them being two-thirds of what it would be if
a true table had been used; but the inaccuracy in the table would not
introduce any maximum into the series of percentages.

The law of the figures being such as I have described, it is
a wholly different question how this law is to be explained, or
accounted for. In 1870, I showed how the law might be accounted
for as the effect of the withdrawal of healthy lives; and I believed

that its existence proved that the lives which withdraw are, on the average, better than those which remain. I now entertain considerable doubt on this point. Mr. Chatham claims to have shown (*J.I.A.*, xxix, 81), that, where the withdrawals are most numerous in the early years of insurance, there the mortality is lightest (p. 172); and he concludes, "that in ordinary circumstances the rate of increase in " the mortality during the 10 years after insurance is independent of " the rate of discontinuance": (p. 173). One of the facts on which he relies is that, on comparing the experience of the 10 Scotch offices with the total experience collected by the Institute in 1863, the rate of withdrawal in the Scotch experience was less than in the total, of which it formed a part; but the rate of mortality was, nevertheless, higher. Mr. Chatham's conclusion, if correct, is of great importance; but the difference between the rates of mortality in the Scotch and in the total experience, is small, and the conclusion seems to me to require confirmation from other sources, before it can be accepted without reserve.

The results of a careful examination which I have made of the mortality among the Female Government Annuitants, as given in Mr. A. J. Finlaison's Report of 1882, seem to point in the same direction. I hope to submit the details of my investigation to the Institute at some future time, but I take the present opportunity of stating the general results. I must, however, do this from memory, as I have mislaid my notes on the subject. On treating the annuitant experience in the same way as I treated the experience of assured lives in 1870, I found a very similar law prevailed in the results; that is to say, taking quinquennial groups of ages attained, I found that, in almost all of these groups, as the time elapst since the date of purchase continually increast, the ratio of the actual to the expected claims, first increast to a maximum, and then diminisht. In this case, the law cannot be a result of withdrawals, for there are none; and some other explanation of the law must be sought for. The only possible explanation seems to be a peculiar composition of the body of lives on which annuities are purchast. Suppose, for example, that the nominees of any age on whose lives annuities are purchast, comprise (1) a majority, who are subject to a low rate of mortality at first, gradually increasing in the course of, say, 10 years, to the normal rate corresponding to the age; and (2) a minority, who are also subject to a low rate of mortality at first, but, after the lapse of a few years, are subject to such heavy rates of mortality that they are all dead within, say, 25 years from the date of purchase; then, confining our attention to nominees of a given age at entry, it is clear that during the 10 years after purchase, the ratio of the actual to the expected deaths, as calculated according to the normal rate of mortality, will continually increase: that, after the lapse of the 10 years, it will still increase, in consequence of the heavy mortality among the minority lives, and will for several years exceed unity; but, that, after the lapse of the 25 years, when all the minority lives are dead, the ratio will become equal to unity; in other words, the ratio will attain a maximum and then diminish. This is, of course, not the same case as we have been hitherto considering; for we have here traced a number of entrants of

the same age, say 50, who have gradually grown older; but it is not difficult to see that, under the circumstances supposed, the same law will hold good when we consider nominees of different ages at entry, who attain the same age, say 70, after the lapse of different times.

Further investigation is desirable, in order to ascertain, if possible, whether the composition of the body of annuitant lives is such as I have described above; or, at all events, whether the law of the percentages increasing to a maximum and then diminishing is observed in other collections of data. If so, it may fairly be argued that the same cause which produces this maximum among annuitant lives, produces it, or at all events, contributes to it, in the case of assured lives.

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I am, Sir,

Your obedient servant,
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Edinburgh,   T. B. SPRAGUE.
    28 *August* 1895.