select lives, and throughout their existence must be expected to exhibit heavier rates of mortality. As, however, there is room for difference of opinion here, the point may be investigated further. Assume, therefore, that some of the $\sigma_{x+n}$ lives were included in the $\delta_x$ damaged lives in existence $n$ years earlier. By considering the $l_{[x]}$ select lives and their survivors after $n$ years we find, as before, that the probability of a select life surviving as "select" $= \frac{\sigma_{x+n}}{l_{[x]}}$. Similarly the probability of a select life surviving as damaged $= \frac{\delta_{x+n}}{l_{[x]}}$. Hence out of the $\sigma_x$ select lives there will survive after $n$ years $\sigma_x \times \frac{\sigma_{x+n}}{l_{[x]}}$ select, and $\sigma_x \times \frac{\delta_{x+n}}{l_{[x]}}$ damaged. But we know from the ultimate column of the table that out of $\sigma_x + \delta_x$ lives there will survive after $n$ years $\sigma_{x+n}$ select and $\delta_{x+n}$ damaged. Therefore, by subtraction, we see that out of $\delta_x$ damaged lives there will survive after $n$ years $\sigma_{x+n}\left(1 - \frac{\sigma_x}{l_{[x]}}\right)$ select and $\delta_{x+n}\left(1 - \frac{\sigma_x}{l_{[x]}}\right)$ damaged; that is to say, the survivors of the damaged lives will consist of select lives and damaged lives mixed in the same proportions as are the survivors of a body of select lives, and therefore they will thereafter experience identically the same rates of deterioration and mortality as the survivors of the select lives. This result seems to me quite inconsistent with the fact that the damaged lives are undoubtedly inferior to the select; for if any of the damaged do become select during the $n$ years we should expect them to be a much smaller proportion of the survivors than if we were dealing with select lives. Looking at this point more closely, we see that the only difference in the subsequent histories of the $l_{[x]}$ select lives and the $l_x$ mixed lives is during the first $n$ years, when the mixed lives experience an excess of $l_x - l_{[x]}$ deaths. If we leave these $l_x - l_{[x]}$ lives out of account, the remainder of the $l_x$ mixed lives are found to die, or to survive as select lives, or to survive as damaged lives, in identically the same manner as an equal body of select lives. Now if a body of lives is found to possess identically the same rates of survivance, of deterioration, and of death, as select lives, that body must necessarily be composed entirely of select lives. Hence the $l_x$ mixed lives are composed of two groups, namely, one numbering $l_x - l_{[x]}$ who all die in $n$ years and must be regarded as damaged lives, and the other numbering $l_{[x]}$ who are all select.

In applying this principle it should be remembered that our mortality tables are only approximate, and that selection can in fact be traced (though to a very small extent) for a period longer than is shown by those tables. Further, if the figures in those tables be regarded as approximations of the first order of accuracy, the differences between them will only be approximations of the second order; and as the numbers of damaged lives are found by these differences we cannot place so much reliance upon them as upon the figures relating to select lives.

I am, etc.,

A. E. SPRAGUE.

22 GEORGE STREET,
EDINBURGH, 6*th March* 1907.

*To the Editor of the Transactions of the Faculty of Actuaries,*

SIR,—Dr. Ernest Sprague has been kind enough to show me his letter on this subject. I am much obliged to him for replying so fully to the observations I made, and as the subject is not devoid of interest I should

like, with your permission, to pursue it a little further, in the hope that we may be able to arrive at a clearer understanding with regard to it.

The question at issue is whether, by means of a Select Mortality Table, we can determine the number of "select" lives contained in a body of "mixed" lives at any given age; and in particular whether the number of select lives among the mixed lives shown in the "ultimate" column of the table is the same as the number of select lives entering at that age. Dr. Ernest Sprague, founding on the principle laid down by Dr. T. B. Sprague in a well-known paper, holds this to be true, while I, on the other hand, venture to call in question its accuracy, and to hold, on the contrary, that we cannot by means of a Select Mortality Table determine at all the number of select lives contained in a body of mixed lives.

There are two senses in which a theory or a proposition may be held to be true—(1) it may be demonstrably true in fact, or (2) it may be true only as a deduction or corollary from a hypothesis which, although not demonstrably true, is held to be so for practical purposes. From the language employed by Dr. Ernest Sprague in his paper, I understood him to affirm his proposition as true in the former sense, but it is manifestly not so. It depends not only on the fundamental hypothesis employed by Dr. T. B. Sprague, that the benefit of selection wears off in a definite short period of years, but also on a conclusion drawn by him (whether warrantably or not is now in question) that all "damaged" or non-select lives will die within a period similar to that in which the effect of selection wears off. That this deduction is not true in fact scarcely needs to be proved, as it is contradicted by everyday observation. Formal proof, however, may be had by referring to the statistics of "diseased" or damaged lives, collected at the same time as the material of the $H^M$ Table, where the history of such lives is traced for 20, 30, 40, 50 and more years, and up to practically the same limit of extreme old age as in the case of "healthy" or originally select lives.

Dr. Ernest Sprague's proposition therefore is not true in the first sense above indicated. Whether it is true in the second sense depends on whether Dr. T. B. Sprague's principle is correctly deduced from his fundamental hypothesis. This is what that gentleman himself says on the subject (*J. I. A.*, xxii, 422):—

"Turning to Table No. 2, let us in the first instance consider the 862,820 select lives of 30. We see that out of these there survive at the end of five years 829,800 who are then of the age 35. Some of these will be still select lives and some will be damaged, and the rates of mortality prevailing among these two classes will of course be widely different, but the 829,800 will as a whole be subject to the mortality of the $H^{M(5)}$ Table. (It is true that according to the table the $H^{M(5)}$ mortality is reached at the age of 33, but this does not in any way affect our argument.) After the lapse of five more years, or at the age of 40, the number of survivors will be 786,500. Now, according to the construction of the table, this is also the number of survivors out of 820,928 select lives of 35, that is to say, the 820,928 select lives and the 829,800 mixed lives of 35 will give us after five years the same number of survivors who will thenceforward be subject to the $H^{M(5)}$ mortality. *It is therefore clear* that the difference, 8,872, between these numbers, must be the number of damaged lives included among the latter; and consistently with our supposition that the effect of selection wears off in five years at most, these damaged lives must all be dead before the end of five years, or none of them will attain the age of 40."

The words which I have put in italics indicate that the writer is content to base his conclusion on what precedes. Now, with all deference, I submit that the conclusion does not necessarily follow from the premises. The problem is of this nature:—We have $A$ lives entering at the age of 30,

becoming reduced by death to $B$ at the age of 35, and further reduced to $D$ at the age of 40. We have also $C$ lives entering at the age of 35 and becoming reduced by death to the same number $D$ at the age of 40. From that age onwards both sets of lives are subject to the same rate of mortality. Dr. T. B. Sprague says it is clear that the difference between $B$ and $C$ must be the number of damaged lives included in the former. But is this so? Let $B'$ be the number of select lives and $B''$ be the number of damaged lives among the $B$ mixed lives of 35, and let $p'$, $p''$, and $p$ be the probabilities respectively of a select life, a damaged life, and one of the mixed or ultimate lives—whether select or damaged being unknown—surviving to the end of five years. Then

$$B'p' + B''p'' = Bp = D.$$

But $Cp'$ also $= D$, hence

$$B'p' + B''p'' = Cp'.$$

This is an indeterminate equation, in which $B'$, $B''$, and $p''$ are unknown. Dr. Sprague's solution makes $p'' = 0$ and hence $B' = C$; but, as we have already seen, 0 is an impossible value for $p''$ in point of fact. Any value of $p''$ within the limits of 0 and $p$ would satisfy the equation, and if any conclusion may properly be drawn it would rather seem to be that the value of $p''$ must lie between those limits, the extreme values 0 and $p$ being both inadmissible, and that the value of $B'$ must lie between 0 and $C$.

The danger inherent in Dr. Sprague's line of reasoning will be seen if we apply it to a similar problem where selection operates in another direction. Suppose that a Life Assurance Office opened its membership to all persons of a particular class who might choose to come in, without inquiry being made as to their fitness for assurance. At the outset the quality of the lives admitted would naturally be below the standard of even "ultimate" lives, but it is reasonable to suppose that after five years the worst of those "sub-standard" lives would be eliminated by death, and the rate of mortality would thenceforward be similar to that of the $H^{M(5)}$ table. Suppose we have a table of such lives constructed on a plan similar to that of a Select Table, and we endeavour by means of it to ascertain how many of the ultimate lives of a given age are still of "sub-standard" quality. In this case the rate of mortality for ultimate lives of 35 will be lower than that of sub-standard lives entering at the same age; and hence, in correspondence with a given number of ultimate lives at the age of 40, the number of sub-standard lives at 35 will be larger than the number of ultimate lives at that age. Reasoning, then, by Dr. Sprague's method, the ultimate lives of 35 will consist of $C$ sub-standard lives and *minus* $(C - B)$ others; a result to which we can attach no intelligible meaning.

The truth is, it seems to me, that a Select Mortality Table, constructed on the principle now familiar to us, and based on the assumption that the effects of selection wear off in a definite number of years, has said its last word on those effects when it has traced the lives from entry into the "ultimate" column. If we try to make it say more—to tell us, for instance, how many of the ultimate lives are "select" and how many are "damaged"—we must perforce introduce some new element or assumption not involved in the construction of the table, and we must be prepared to accept any anomaly or incongruity that may be elicited by our effort. We cannot, however, claim that our result is taught by the table, or that it follows from the hypothesis on which the table is based. We can only ascribe it to our requiring from the table more than it professes to teach.

Concerning the lives in the "ultimate" column, the table only tells us that they are no longer select, but are henceforth subject to the ultimate rate of mortality. When Dr. Sprague says of them that some will still be select and some will be damaged, he goes beyond anything that we find in the table, and draws upon experience and observation altogether outside of it

But if experience and observation are to be introduced as factors in the problem, we cannot admit their validity in one direction and deny it in another. Now, as we have seen, they emphatically contradict the conclusion that all lives which are not insurable at the ordinary rate of premium must die within five years. But this is a necessary conclusion from the principle that the number of select lives contained among the $l_{x+5}$ lives in the ultimate column of a Select Mortality Table is the same as the number of lives entering the table as select at $x+5$. We must therefore conclude that that principle is itself erroneous. Dr. Sprague has not proved that it necessarily follows from the fundamental supposition as to the effect of selection wearing off in five years, and I do not think this is capable of proof. If it were, the reasonable view would seem to be, not that we must follow out that supposition to all extremes, but that we should carefully limit its application, and not be led by it to a manifestly impossible conclusion.

Dr. Ernest Sprague in his letter seeks to carry the demonstration further than Dr. T. B. Sprague has done. He dwells more on the identity of the ultimate rate of mortality of the entrants at age $x$ with that of the entrants at age $x+n$, and argues that after the ultimate column is reached, every group of the same attained age—whatever the age at entry—must be held to consist in similar proportions of select and damaged lives. Here again he must be reminded that the presumption that there are select and damaged lives, and not merely a homogeneous body of ultimate lives, after the period of selection has expired, does not arise from the table but is inferred from independent observation and experience which did not enter into its construction. It is read into and not evolved from the table, and therefore neither the table itself nor the fundamental assumption upon which it is constructed can be held responsible for any inadmissible conclusion that may result. Such a conclusion does in fact result, for Dr. Ernest Sprague finds himself immediately at issue with the undoubted fact that damaged lives, so far from all dying off in the period of $n$ years, within which selection is assumed to operate, do in many instances become select lives, and gain admission at the ordinary rate of premium on applying a second time for assurance. Here again the facts of observation, being brought in evidence on one point, must be permitted to bear testimony to another on which they are equally emphatic, and they vitiate the otherwise able argument adduced in Dr. Ernest Sprague's letter.

On the whole matter I submit, that however convenient the principle laid down by Dr. T. B. Sprague may be as a working hypothesis for certain purposes of calculation, it is by no means a proved conclusion—does not necessarily follow from the fundamental assumption of a Select Life Table —and in practice should be employed with caution, and only after careful circumspection as to its tendency in the particular case which is the subject of calculation.

I am, etc.,

GEO. M. LOW.

28 ST. ANDREW SQUARE,
EDINBURGH, 14*th March* 1907.