"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 of the age of 40, the number of sub-standard lives at 35 will be larger "than the number of ultimate lives at that age"; that is, C is greater than B. There is, I think, a good deal of confusion here. It is, in fact, impossible to make out what is the exact problem which Mr. Low here proposes for solution. What does he mean by "still of sub-standard quality"? Does he mean at the age of 40? Apparently not, for he presently speaks of the ultimate lives of 35. By his supposition the entrants, taken as a whole, are of sub-standard quality; but they are a body of mixed lives, containing a certain number of what he calls the "worst" lives, who all die off in five years; and others, whom he calls ultimate lives. It is therefore incorrect to speak of sub-standard lives of any age as different from the entrants at that age. Nevertheless, Mr. Low says that, reasoning by my method, the ultimate lives of 35 will consist of C sub-standard lives and *minus* (C − B) others. (It is to be noticed that he deals here with only one age—35, or the age at entry; whereas my problem deals with the number of survivors at 35, out of entrants at the age of 30.) The proper conclusion from the facts supposed by Mr. Low, is that in C, the number of sub-standard lives who enter at 35, there are included B ultimate lives, and C − B "worst" lives who all die within five years; and the absurd conclusion drawn by Mr. Low, is not got by any reasoning at all analogous to mine, as explained above. The "danger" is not due to my method of reasoning, but to the application of my formula to a totally different problem, to which it is not applicable.

To sum up:—the assumption that the benefit of selection wears off after *n* years, is a convenient one to make in the investigation of the problems of selection; but, when we suppose that *n* is equal to 5 or even 10, this is only approximately, and not strictly true. If it were strictly true, then all the damaged lives would, as I have proved above, die off in the five or ten years after entry; but this, as Mr. Low points out, is not the case. I think, however, that Mr. Low greatly exaggerates when he says that "many" of the damaged lives become select, and are admitted at the ordinary rate of premium on applying a second time for insurance. In forming a Select Table and in drawing conclusions from it, we are entitled to use any facts that observation and experience furnish us with; and one of the most important of these, to the use of which Mr. Low, strangely enough, objects, is the fact that, after the lapse of any number of years, a body of select lives will cease to be select, not in consequence of any deterioration that affects them all, but because a comparatively small number of them have been attacked by various diseases and infirmities which lessen their chances of life, while the majority are still select lives. In conclusion, although a select table cannot be trusted to give us the exact number of damaged lives after the lapse of any number of years, the number given by the table is approximately correct; and this is all that can be said about any conclusion deduced from even the best mortality tables.

I am, etc.,

T. B. SPRAGUE.

EDINBURGH, 26*th May* 1907.

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

SIR,—Mr. Low, in his letter of 14th March, refers to the D^{MF} experience as affording formal proof that damaged lives do not all die in five years, and

lest this reference should give rise to some misunderstanding I think it well to point out that the lives forming that experience are not a fair sample of the class of damaged lives. The whole business of insurance is based upon averages ; and when speaking of the rate of mortality among a body of select lives of a given age we mean the rate of mortality that will on the average be experienced by such a body. The individuals composing that body do not all possess the same vitality, but range from the life who has barely been accepted without an extra premium, to the most perfect possible specimen of mankind ; and it is probably correct to say that no two individuals have exactly the same prospects of life. For the purpose of our business, however, we group all these different lives together under the heading of select lives and treat them as if their prospects of life were identical. Similarly, the class of damaged lives includes some who are very nearly select and are accepted with a small extra ; some who are charged a large extra ; some who are actually suffering from diseases more or less acute, and are therefore uninsurable ; and all who are on their deathbeds. It will therefore be seen that the class of damaged lives comprises a vastly greater variety than the class of select lives ; and the differences between special sections of it, and the general average of damaged lives, may therefore be very wide. In particular the section entering into the D^{MF} experience resembles select lives much more closely than it resembles the average of the damaged lives. Those who are on their deathbeds, or who are known to be seriously ill, are not proposed for assurance (save in rare cases of fraud), and the worst of the damaged lives are thus weeded out. Many others are eliminated by the medical examinations and other inquiries made by the Offices, and there is thus a double process of selection, the result being that only the very best of the damaged lives are accepted for assurance and enter into the D^{MF} experience. We are therefore not entitled to draw from that experience any conclusion as to the rates of mortality that prevail in the class of damaged lives as a whole.

Considering now Mr. Low's mathematical investigation, it must be remembered that the problem of finding n unknown quantities from n conditions connecting them, is determinate or indeterminate according as those n conditions are independent or not ; but if one of the conditions be discarded and an attempt made to find the n quantities from the remaining $n-1$ conditions, an indeterminate equation is the result whether the problem be in fact soluble or indeterminate. The problem at present in question is to find the two unknown quantities B'' and p'' (using Mr. Low's notation) from two conditions, namely, (1) that the survivors of the two groups B and C after 5 years are equal in number, and (2) that those survivors are subject to the same rates of mortality. But Mr. Low has in his investigation discarded the second condition, and accordingly his resulting equation must of necessity be indeterminate whether the problem be soluble or not. Therefore no conclusion whatever can be drawn from the fact that he has arrived at an indeterminate equation. It is true that he has introduced into his formulas a third quantity, B' ; but as it is by definition equal to $B - B''$, there are really only the two above-mentioned unknown quantities to be determined from the two conditions. These conditions are easily seen to be independent, for it is obviously possible to have two groups containing equal numbers but subject to different rates of mortality, or containing different numbers but subject to the same rates of mortality ; and it follows that the problem is not indeterminate, but is capable of a definite solution. Dr. T. B. Sprague's solution is therefore not a mere particular case of an infinite number of possible solutions, but represents the complete solution of the problem.

It does not appear to me that Mr. Low has applied Dr. T. B. Sprague's reasoning in a correct manner to the case where selection is supposed to operate in a negative direction. The lines of that reasoning, stated in general terms, are somewhat as follows :—If there are two groups of lives containing different numbers B and C and subject to different rates of mortality, and if after *n* years these two groups are found to contain precisely the same number of survivors who are thereafter subject to pre- cisely the same rates of mortality, then it follows that the larger of the two original groups is made up of (1) a body numerically equal to the smaller group and subject to the same rates of mortality as that group, and (2) a body numerically equal to the difference between the two groups, and subject to such rates of mortality that they will all die in *n* years.

Applying this to the hypothetical case put by Mr. Low, we see that Dr. T. B. Sprague's reasoning leads to the conclusion that the entrants aged 35 will consist of B lives subject to the same rates of mortality as the "ultimate" lives, and C – B "damaged" lives who will all die in 5 years. This part of Mr. Low's argument, therefore, seems to be based upon a misapprehension.

With reg rd to the constitution of the lives in the "ultimate" column of the Select tables, it must be remembered that select lives differ from the population owing to the rejection of a minority of bad lives. It is a funda- mental fact, therefore, that the population consists of mixed lives, and this must also (after the outset) be true of assured lives, whose rates of mortality tend to approximate to those of the population as the effect of selection gradually wears off. The proposition that some of the lives in the ultimate column of the Select tables are "select" and others "damaged" is, there- fore, not read into the tables from an extraneous source ; but is one of the fundamental facts embodied in the data upon which the tables are based.

I do not know what authority Mr. Low has for his statement that damaged lives "do in many instances become select lives and subsequently gain admission at the ordinary rate of premium," and my own impression is that such instances are rare. Even if the statement be literally correct, the use of the word "many" is likely to mislead some readers by conveying the impression that a substantial *proportion* of damaged lives become select. The whole theory of life contingencies is based on relative numbers or proportions, not upon absolute magnitude ; and even if a considerable *number* of damaged lives were proved to become select, the conclusions to be drawn therefrom would depend entirely upon the *proportion* that number bears to the total number of damaged lives. This proportion might be (and I think it probably is) so small, that it would not seriously disturb the general conclusions to be drawn regarding the damaged lives Further, it is probable that some of these cases would, under modern conditions, have originally been passed as select lives, and that their apparent improvement merely represents an error in the original classification. But admitting that some damaged lives survive for more than five years, it does not follow that Dr. T. B. Sprague's proposition is wrongly deduced from the assumption that selection wears off in five years—for the error may lie in the original assumption and not in the process by which the proposition was deduced therefrom. In point of fact, that assumption is known to be erroneous, and the effect of selection has been shown to persist for more than ten years, though after that time it is only faintly traceable ; and this is, I submit, the true explanation of any discrepancy between Dr. T. B. Sprague's proposition and the observed facts.

It is to be observed that Mr. Low bases his criticisms largely on the ground that the proposition leads to a conclusion greatly at variance with the results of experience and observation, and I will now show that this

variation is much less than Mr. Low supposes, by putting the proposition to the test of actual facts regarding the number of damaged lives.

We know on the high authority of Dr. Farr (*J. I. A.*, xix, 413) that "it may be broadly stated that 27 in 1000 men of the population of the age of 20 and under 60 are suffering from some kind of disease or other." Now, applying Dr. T. B. Sprague's proposition to the $O^{[M]}$ tables between the ages mentioned, we find that there are 3,214,435 "ultimate" and 3,135,690 "select" lives, and the difference between these numbers, namely, 78,745, is the number of "damaged" lives. The proportion of damaged lives is therefore 24·5 to every 1000 "ultimate" lives, which is about 10 per cent. lower than the proportion of damaged lives in the male population as estimated by Dr. Farr. The class from which ordinary insured lives are drawn is, however, not a fair sample of the whole male population, but represents a higher standard of living and healthier surroundings, so that we should expect it to exhibit a lower proportion of damaged lives. Allowing for this, and for the fact that selection has not entirely worn off in ten years, we see that Dr. T. B. Sprague's principle gives a fair approximation to the true number of damaged lives, and this furnishes independent evidence of the strongest possible nature, of the truth of that principle.

From the foregoing considerations I submit that the following conclusions may be drawn :—

    I. If the effect of selection wears off in a definite period of n years, it necessarily follows that all the damaged lives must die in n years.

    II. The fact that the effect of selection is traceable for more than the 10 years shown by the $O^{[M]}$ table accounts for the cases where the damaged lives may not all die in 10 years.

    III. As the effect of selection is only faintly traceable after 10 years, the number of damaged lives who survive that period must be small compared with the total number of damaged lives.

    IV. As the $O^{[M]}$ table is believed to give very close approximations to the true rates of mortality, the conclusions drawn from it regarding damaged lives should on a broad average be in fair accordance with the facts.

<div align="center">I am, etc.,</div>

<div align="right">A. E. SPRAGUE.</div>

22 George Street,
Edinburgh, 31*st May* 1907.

<div align="center">*To the Editor of the Transactions of the Faculty of Actuaries,*</div>

Sir,—I was lately favoured with a perusal of Dr. T. B. Sprague's letter on this subject, dated 26th May, and I had previously seen Dr. Ernest Sprague's second letter, dated 31st May.

When I took exception to a portion of the paper read by the last named gentleman in January, it did not occur to me that I was raising a question on which there could be serious controversy. I considered that the author of the paper had by certain unqualified expressions elevated to the position of an acknowledged principle what at the best was a conclusion based upon mere assumption, and my hope was that he would so modify those expressions as to bring them into accord with reality. To my surprise it has appeared that he maintains all that the language seemed to imply, and to my great regret I find myself at seeming variance not only with the author of the paper, but with his honoured and distinguished father. I had not