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.

I am, etc.,

A. E. SPRAGUE.

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

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

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 supposed the theory now in question to be regarded by its author in any higher light than as a mere working hypothesis, divorced indeed from actual facts, but convenient as a practical basis for certain calculations. Even now, I can scarcely believe that on the main issue my views are seriously divergent from his, for I observe that Dr. Sprague agrees with me as to the necessity for caution in employing the numerical conclusions resulting from his theory. There would be no need for such caution if, as Dr. Ernest Sprague claims, the problem dealt with is capable of definite solution, and has in fact been completely solved.

Your readers would in other circumstances have welcomed with unalloyed satisfaction a contribution to this correspondence by our esteemed Honorary Fellow, but I am sure they will share my regret for any annoyance he may have been caused by the reopening of a question which he doubtless regarded as long settled. As Dr. Sprague intimates that he does not intend to write again on the subject, it will I think be more suitable, and more consistent with my high regard for him, if I do not take up any controversy with Dr. Sprague himself, but continue the discussion with Dr. Ernest Sprague, whose letters cover all the points that are necessary for the elucidation of the subject.

Until we reach the "conclusions" at the end of that gentleman's second letter, it seems to me difficult to apprehend what is his view on the main question at issue, namely, whether we can or cannot, by means of a select mortality table such as we are familiar with, determine the number of "select" in a body of "mixed" lives. In one place he speaks of the problem as capable of a definite solution, and as having been completely solved, but elsewhere he speaks of this "complete" solution as a "fair approximation," and as being deduced from an "erroneous" assumption. Obviously a solution cannot be definite and complete and at the same time be merely approximate and founded on error. I suppose the meaning to be drawn from those apparently conflicting statements is that if the fundamental hypothesis were correct, the solution would be definite and complete, but the hypothesis being inexact, the solution is necessarily only approximate. This accords with his conclusion No. 1:—"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."

Now in spite of all the argument that has been adduced, and with the utmost respect for the high authority invoked, I submit that this conclusion is untenable, and further, that even if it were a proved conclusion, it would by no means follow that we had found a means of determining even approximately the number of select lives in a body of mixed lives. On the contrary, it would follow that we had ascertained the impossibility of solving this problem on the assumption that the effect of selection wears off in a definite short period of years, the attempt to solve it having led to such a conclusion. For consider what "select lives" and "damaged lives" respectively are. Select lives, strictly speaking, are lives which have just been ascertained, after examination and inquiry, to be lives eligible for insurance at the ordinary rate of premium. In this strict sense there are no select lives after *n* years, because there is no fresh selection exercised by the Insurance Office, but for convenience we employ the term "select lives" to denote lives of such quality that they would be entitled to admission as select if they again applied for insurance and again underwent the usual scrutiny. "Select lives," then, may be briefly defined as lives which are eligible for insurance at the ordinary rate of premium. "Damaged lives," on the other hand (since those two kinds compose the whole body of "ultimate" lives), are lives which, although at one time select, are not now eligible for insurance at the ordinary rate of premium.

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Within this description must be embraced all those persons of the "mixed" or "ultimate" class :—

(1) Who are actually the subjects of mortal sickness, or have become the victims of discoverable disease of such a type as to render them unfit for insurance ;
(2) Who have fallen into bad habits ;
(3) Who have contracted or have developed a tendency to any disease or infirmity which increases the risk of insurance, but does not prevent acceptance at an extra premium, large or small according to circumstances ;
(4) Whose family history has developed taints which would either prevent insurance or require the imposition of an extra premium, large or small ;
(5) Who are suffering from illness of a more or less serious nature, rendering them for the time unfit for insurance, or fit only at an extra premium, but who may ultimately recover.

The catalogue could no doubt be extended, but these examples will suffice. To say of those various classes of persons that they must of necessity all die within a limited period, during which the effect of selection is assumed to persist, is so entirely opposed to our knowledge and experience that any argument employed in support of such a proposition may be dismissed as unworthy of serious consideration. I adduced the D^{MF} table as evidence to the contrary, and Dr. Ernest Sprague has thought it worth while to call attention to the fact that the lives embraced in that table differ from the ultimate lives of a select mortality table in respect that the latter include persons in a dying condition, while the former (at the time of entry) do not. Needless to say I had not overlooked so obvious a consideration, but it in no way invalidates the evidence, as my point is established if only a section of the $l_{[x]+n}$ lives resemble those of the D^{MF} table, and this undoubtedly is the case.

Dr. Ernest Sprague, however, cites a passage from Dr. Farr as affording some support to his view. The passage occurs in Dr. Farr's letter to the Registrar-General, introducing the English Life Table No. 2 :—"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." For a writer like Dr. Farr this is a somewhat loose statement, and the precise meaning is not quite apparent. It may, however, be gathered from the context. Dr. Farr gives 100 illustrative cases showing the causes of death and durations of fatal illness as furnished to the Registrar. Extracting from among these the cases of men between the ages of 20 and 60, we find that the average duration of illness was about two years. Now according to the mortality table which Dr. Farr was then introducing, the number of men who die in a year between the ages of 20 and 60 is almost exactly 13·5 per 1000. It may therefore be reasonably inferred that Dr. Farr derived his figure of 27 per 1000 by multiplying the number of deaths by the average duration of fatal illness, and that what he really meant was that at any given time about 27 per 1000 of the male population, within the ages mentioned, were already attacked by their mortal illness. So far therefore as Dr. Farr's evidence goes, it tends to show, not that Dr. Ernest Sprague's figure of 24·5 per 1000 is a "fair approximation" to the true number of damaged lives in a body of mixed lives, but that it is probably wide of the mark, inasmuch as it corresponds with only one of the five classes of damaged lives above detailed, and that possibly not the most numerous, and surely less numerous than all the other classes combined.

On the whole therefore, we may conclude that we cannot arrive at the

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number of damaged lives of any age $x+n$ by the simple process of subtracting $l_{[x+n]}$ from $l_{[x]+n}$; and, as no other means of solving the problem presents itself, we may further conclude that we cannot arrive at any true solution by means of a select life table based on the usual hypothesis that selection wears out in a definite short period of years. This establishes my main proposition, and (I venture to submit) justifies the criticism I offered upon Dr. Ernest Sprague's paper.

There remains the question whether, if the hypothesis that selection wears off in n years were true, it would necessarily entail the conclusion that all damaged lives must die in n years. Supposing this were the case, it does not follow that we may proceed to build upon the conclusion even if for practical purposes we adopt the hypothesis. Dr. Ernest Sprague himself says in his first letter—"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." Now an approximation of the second order may be (as in fact we have seen) very far indeed from accuracy. If, starting from a given point, we wish to reach approximately another point not far distant, it may suit quite well to take a short and easy path that will bring us somewhere near our object; but if, having attained with sufficient nearness our first object, we wish to reach a second that is further distant, we may find the same path lead us far astray, for any divergence from the true path increases with every step we take. Hence the conclusion in question, being based upon a hypothesis admittedly "erroneous," must be received with extreme caution, and cannot be admitted to have even the same degree of validity as the hypothesis itself. In short, if for our convenience we adopt the hypothesis, we are by no means bound to adopt the conclusion, and are not entitled to maintain it as approximately true even if it seems to follow strictly from the hypothesis.

But, in this case, does the conclusion strictly follow? In my previous letter I endeavoured to show that the problem of finding the number of damaged lives on the above hypothesis is an indeterminate one. Dr. Ernest Sprague complains that I left out one of the conditions of the problem, and so rendered it indeterminate, but I think he is mistaken in this, and I observe that Dr. T. B. Sprague (if I may for a moment refer to that gentleman's letter) does not adopt Dr. Ernest Sprague's contention, but on the contrary reaffirms what I had pointed out, that the whole demonstration is contained in the passage quoted in full in my letter, and upon which I founded. We do not transform an indeterminate problem into a determinate one by introducing a new factor or condition unless that serves to give a definite value to one of the quantities hitherto unknown, and Dr. Ernest Sprague does not show that the condition on which he lays stress—the identity of "ultimate" mortality for all ages at entry—has this effect. I consider that in point of fact it has not, because I do not see how by means of it my equation is to be solved, nor has Dr. Ernest Sprague assisted me in this. He did in his previous letter found largely on the condition in question, but if his argument be closely examined, it will be seen that it requires, as an assumption, the very thing that is to be proved—namely, that the increased mortality of mixed lives is due to the presence among them of a small minority of bad lives, so small and so bad that they are all doomed to disappear within the next n years—whereas the condition itself is equally consistent with the contrary assumption, that the "ultimate" lives have all alike become deteriorated. The reasoning, in fact, proceeds in a circle, and ignores some obvious considerations. For example, it leaves no room for the patent fact that damaged lives, so far from all disappearing within a short time, do sometimes regain their place as select lives. Exception is taken to my statement that this happens in "many" cases—my own experience suggests that it does—but if it happens even in "some" the assumption is vitiated and the claim to an exact solution fails.

We may look at the matter in another light, which yet is the same light as that in which it is presented in my indeterminate equation. There are two opposite ways in which the increased mortality among mixed or ultimate lives might be accounted for—(1) it might be due to a general deterioration in quality, so that the surviving lives present individually less resisting power to the forces which make for death, or (2) it might be due to the presence of a small proportion of lives which have already broken down irretrievably and are doomed to early extinction. These are the two extremes, for the one assumes all the l_{x+n} lives to be of a quality inferior to that of select lives—in other words to be "damaged"—while the other assumes the number of "select" lives to be the greatest that is possible. The latter is the alternative we are asked to adopt, but the former is equally consistent with the material afforded by a select life table, and has, moreover, this in its favour, that it is also consistent with the assumption implied in the use we make of every mortality table, that the lives of a given age in the l column are equally exposed to the risk of death. But neither the one alternative nor the other is tenable, because neither is consistent with known facts. The only possible conclusion is that the truth as to the numbers of "select" and "damaged" lives respectively must lie somewhere between the two extremes, but where precisely it does lie the select life table affords no means of ascertaining.

Two other points remain to be noticed. I had said that the proposition which assumes some of the lives in the ultimate column to be select and some to be damaged, is read into and not evolved from the select life table. Dr. Ernest Sprague says this is not so, because the population consists of select and damaged lives, and the rates of mortality of assured lives tend to approximate to those of the population as the effect of selection wears off. But does not this very statement confirm what I have said, and indicate the extraneous source from which (in Dr. Ernest Sprague's view) the proposition is derived? Then he says that I have not applied Dr. T. B. Sprague's reasoning in a correct manner to the case in which selection is supposed to operate in a negative direction. He restates the reasoning in language of his own, and brings out a different though not necessarily a contradictory conclusion. This does not disprove what I have said, and it still remains that the reasoning which I quoted would, if applied in the same manner to the other case, bring out the impossible result to which I pointed.

I submit, with all deference, that the line of reasoning in question is fallacious. It takes the number of lives in the ultimate column, l_{x+n}, finds by proportion how many lives, selected n years before, would be now represented by l_{x+n} lives, and then assumes that the rate of mortality $(l_{[x} - l_{x+n}) \div l_{[x]}$ must have been in operation among $l_{[x]}$ of the $l_{[x-n]+n}$ lives appearing in the ultimate column as l_x. The table does not warrant this. On the contrary it only teaches us that the l_x lives as a whole become l_{x+n} by being subject to the rate of mortality $(l_x - l_{x+n}) \div l_x$. As well might we take the proportionate number of lives at age x by any other table—for in reality a select life table gives a different mortality table for the n years following each age at entry—and then assume that the rate of mortality shown by that table must have been in operation among so many of the lives.

I am sorry that through my having challenged what appeared to me an unwarranted use of language, it has fallen to me to point out what I conceive to be mistaken in the theory we have been discussing, and I should regret still more if it should be thought that I have unjustifiably attacked the theory in question. To my mind that theory is by no means necessarily bound up with the principle of select life tables, in the development of

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which Dr. T. B. Sprague has rendered such distinguished service, and even if the theory were set aside, that would not detract from the great obligation under which he has laid the actuarial profession in regard to all the legitimate uses of such tables.

I am, etc.,

GEO. M. LOW.

28 St. Andrew Square,
Edinburgh, *7th August* 1907.

NOTES ON THE BRITISH OFFICES LIFE ANNUITY TABLES (1893).

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

Sir,—My attention has been called to the fact that the values of the ratio $\mu_{x+\frac{1}{2}} \div m_x$, as tabulated in column (9) of Table XI. of my Paper upon the above subject (p. 323 of the present volume), show irregularities in passing through successive quinquennial values of x, which are not in accordance with the theoretical relations of the quantities m_x and $\mu_{x+\frac{1}{2}}$, when deduced on the basis of Makeham's first modification of Gompertz's formula. These irregularities in the Table appear to arise from the fact that I have not computed the values of colog p_x (from which m_x is deduced) and of $\mu_{x+\frac{1}{2}}$, as given in columns (6) and (8) respectively, to a sufficient number of places. Mr. D. C. Fraser, M.A., F.I.A., very kindly sends me the following Table, in which the values are more fully computed :—

x	$\text{colog}_{10} p_x$	m_x	$\mu_{x+\frac{1}{2}}$	$\frac{\mu_{x+\frac{1}{2}}}{m_x}$
52	·007,894,424	·018,177,082	·018,173,863	·999,823
57	·010,673,839	·024,576,185	·024,571,662	·999,816
62	·014,978,643	·034,486,181	·034,480,679	·999,840
67	·021,645,995	·049,831,430	·049,827,928	·999,930
72	·031,972,501	·073,586,172	·073,598,004	1·000,161
77	·047,966,365	·110,334,501	·110,413,492	1·000,716
82	·072,737,928	·167,094,849	·167,433,931	1·002,029
87	·111,104,536	·254,441,440	·255,748,136	1·005,136

It will be seen that the ratios, given in the last column above, fall to a minimum at age 57, and then steadily increase to the end of life, whilst the values in column (9) of my Table XI. show great irregularities at successive ages. The correction of the values in column (9) necessarily involves consequential alterations in the following columns (10) to (17) of my Table, but the changes in the final values of the constants A, B, a, β are very slight, and need not now be followed out, as the Table was only intended to illustrate a particular method of experimental graduation.

I am indebted to Mr. John Spencer, F.I.A., for the following interesting and useful demonstration of the relations of m_x and $\mu_{x+\frac{1}{2}}$, when Makeham's law applies :—

Taking the usual approximation for m_x, we have

$$m_x = \frac{q_x}{1-\frac{1}{2}q_x} = 2\,\frac{1-p_x}{1+p_x} = 2\,\frac{1-e^{-\text{colog}_e p_x}}{1+e^{-\text{colog}_e p_x}}$$

$$= \text{colog}_e\, p_x - \frac{(\text{colog}_e\, p_x)^3}{12} + \frac{(\text{colog}_e\, p_x)^5}{120} - $$

$$= \frac{\text{colog}_e\, p_x}{1+\frac{(\text{colog}_e\, p_x)^2}{12}} \text{ approximately.}$$

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