CORRESPONDENCE

(*To the Editors of the* Journal of the Institute of Actuaries)

DEAR SIRS,

The appearance of a paper covering a novel field of investigation is always an important event and I have some diffidence in writing in a critical vein on the paper by Johnson & Garwood published in *J.I.A.* **83**, 277.

The authors have made an exhaustive analysis of the frequency of claims arising from a limited group of motor policies selected in a special way. As they point out, the data are far from homogeneous and it is therefore not surprising that the analysis reveals this fact. I think it is particularly unfortunate they have used the term 'accident proneness' to describe the heterogeneity arising from the variations in accident frequency of individual policies.

There is considerable literature on the subject of accident analysis, see, for example, the papers by Adelstein (*J.R.S.S.* **115**, 354–410 (1952)) and by Prof. Greenwood (*Biometrika*, **37**, 24–29 (1950)), where there is considerable discussion on the various interpretations and uses of the concept of accident proneness. There is also quite a number of studies relating specifically to motor vehicle accidents. The authors use the term 'accident proneness' as an 'assessment of the proportion of drivers who are likely to be accident repeaters, or more exactly, who are likely to make repeated numbers of claims'. Since about 90% of the policies are issued for any driver, and claims arise from the driving of cars by someone other than the person in whose name the policy is issued, it is clear that the data cannot provide any real information about individual drivers, as is commented by the authors in Section IV. The correct definition would be 'proportion of policies on which repeated claims are likely to arise', whence it follows that all that is being measured is heterogeneity in the data, i.e. the totality of all the different factors entering into the exposure to risk of accident.

The position of the mathematical models is concisely summed up in the remarks of Prof. Greenwood in the above-mentioned paper '...because an aggregation of frequency distributions... gives a negative binomial, the finding is no proof that proneness was responsible', but it is quite pertinent to remark that if differential accident risks do exist in the population being studied, then an analysis made in the form of Table 4 will present the features found, an obvious fact which underlies the basis of review of types of policies in which the benefits arise from accidental events.

For example, if the risk follows the type III form adopted by the authors in Appendix II it is easy to show that the ratio of the expected claim rates of the two sections (*a*) those policies with claims up to duration t, and (*b*) those with none, will be

$$\frac{(1+t\bar{\lambda}/\kappa)^{\kappa+1}-1}{(1+t\bar{\lambda}/\kappa)^{\kappa}-1}.$$

If κ is taken as 1·75, and $\bar{\lambda}$ as ·28, this function takes the values shown in the following table, which also shows the numerical values derived from Table 4. The table provides further evidence that the type III distribution is not a good representation of the risk distribution.

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t	Theoretical value	Observed ratio	
		After 5 years	After t years
1	1·70	1·64	1·94
2	1·83	1·88	1·84
3	1·97	1·72	1·82
4	2·10	1·65	1·68
5	2·25	1·90	1·90

Another feature of interest is derived by noting from Table 6 that the number of claims in the first year is 85, giving a claim ratio of ·310 for the 274 policies exposed. The claim ratio for all years after the first may be calculated from Table 4 as ·139. Having regard to the numbers involved, the difference must be statistically significant. Clearly a reason exists for this divergence and without establishing it, the formulation of mathematical models is a hazardous pursuit.

Yours faithfully,

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DEAR SIRS,

We have read Mr Beard's letter with interest, though we regard it as being only very mildly 'in a critical vein', since it draws attention to points which we have mentioned in our paper, though not always, perhaps, with sufficient emphasis.

In particular, we pointed out—in Section IV and again on p. 286—that the data give information on the proneness of *policies* to *claims* rather than of *drivers* to *accidents*.

Mr Beard's table of the ratio of expected future claim rates for claim-free and non-claim-free policies is useful, and demonstrates departure from the consequences of assuming a type III distribution for λ (in a clearer way than does our Table 6).

Since the publication of our paper more extensive data from the same source have been analysed, and it is hoped to publish the results in due course. These appear to confirm Mr Beard's point in his last paragraph, namely, that the claim rate in the first year is higher, and it is evident that the mathematical model of a distribution of λ, independent of policy duration, is, in itself, inadequate in this respect.

Yours faithfully,

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