This is a textbook which should be recommended to every actuary who wants to get a first introduction into the vast field of risk theory and to the student who in his undergraduate years wishes to learn about a powerful application of probability theory. We ASTIN members—constituting the "in-group" of risk theory—must thank the authors for making our ideas known to a wider circle which hopefully will get interested in this fascinating subject.

HANS BÜHLMANN

Remarks to Seal's review in Astin Bull VI on my paper 'A Review of the Collective Theory of Risk' (Suppl. to Astin Bull. V).

Seal remarks i.a. that in the review a few papers dealing with the individual risk theory rather with the collective risk theory have been included, while other papers related to the former theory have not been even mentioned.

The fact is, that the border line between the individual and collective methods seems to have become rather vague with the modern development of the latter method. For example in a recent paper (Astin Bull V: 3, 1971), it has been supposed that a large group of insurances may be divided into sub-groups for which the view-points of the collective risk theory are applicable, this assumption has been tacitly made in papers dealing, particularly, with motor insurance. As a particular case, was assumed that the risk process of each such sub-group was a compound Poisson process. In this case, the main group was found to be in the same form, with the risk distribution defined by the convolution of the risk distributions in the sub-groups, and the claim distribution by a weighted average of the claim distributions in the sub-groups. If the sub-groups contain only one individual, the problem is principally the same, it means that the individual process shall be treated with the collective method. It seems, therefore, not unnatural to include some papers dealing with the individual theory without giving a complete list of such papers.

Seal remarks, further, that in mentioning papers dealing with pure mathematics rather than with collective risk theory neither with stochastic process theory [60] *), [100], [179], [183], [184], [219] and [355] have not been included. For example [183], [184] deal with distributions generated by Poisson distributions, and with branch processes. These distributions, and processes are, however, of utmost interest for the collective risk theory, so that it does not seem unnatural to consider these items as belonging to the methods of the collective risk theory rather than to pure mathematics, which may also be said with respect to the remaining papers, in the list just given. Seal considers it a disadvantage that the literature list at the end of my review has not been divided into three parts, one referring to stochastic process theory and other pure mathematical items, one to the collective risk theory, and one to individual risk theory, where the latter should be either completed or eliminated. In my opinion, my comments on the development in collective risk theory seem to be sufficiently well illustrated by selected quotations. As the two parts not considering collective risk theory, are

*) The figures within square brackets refer to the list of literature in my review.

incomplete, it seems better not to divide the list. It seems, however, to be unnecessary, on account of the facts stressed here, to change the title to a review of the collective risk theory with selected references to individual risk theory, to the theory of stochastic processes, and, to other fields in pure mathematics.

Queueing processes form a particular group of the stochastic processes. The reason for the queueing processes have not been explicitly dealt with in the review, is that Prabhu by his application of such processes to the Poisson case, where, particularly, the claim distribution is positive with a positive loading, or negative with a negative loading reached the same results in [306], as Arfwedson in [35**] obtained by the application of a Lagrange expansion to $\exp[-s_1x]$, s_1 being the only zero in the interval of interest of the function $1 + cs - p(s)$ in Cramér's notations, to Cramér's deductions in the particular cases here concerned. Arfwedson found also, that the formulae deducted by him, agreed with the corresponding formulae in [111], for the complex Fourier transform of $\psi(u, z)$ in the positive and negative case, (116) combined with the last relation, p. 77, and for $\psi(u)$ the last relations on p. 81, respectively.—However, Prabhu extended his results to hold even for a general time-homogeneous stochastic process with continuous frequency functions by using results given by Kendall. My neglect of Takács was not made intentionally, I admit that a reference to his book would have been appropriate, which may also apply to Beneŝ's book, but this was unknown to me. As I have formulated the remark on my references to items on general stochastic process theory "some studies" into this "theory have been included in the list of literature," it seems, however, not necessary, to refer to the theory of queues, nor to combinatorial methods in the stochastic process theory, as being particular cases of the last-mentioned theory.

Seal's remark to my phraseology and notation as being "impenetrable specialist phraseology and notation" seems not to have been based on my introduction and definitions. In my opinion this section clearly defines my phraseology and my notations, and the connection with the terminology used by other writers, in English, in French, and in German. It seems to me that the difficulty to understand my commentary, is not due to my terminology, but rather to the fact that different authors use different terms for the same concept, e.g. a compound Poisson process, and a non-elementary Poisson process, have been called "a weighted Poisson process" and "a compound Poisson process", respectively. As most authors do not deal with other terminology than their own, it seems to make their texts more easy to penetrate, than in the case, where the differences in terminology must be considered, as in the comparison of different papers.

The real difference, apart from differences in terminology, between the models defined in the introduction to the review is, that the risk distribution (structure function) of a compound Poisson process may depend on time. According to my experience, trials for adjusting statistical results to a compound Poisson process neglecting the possibility of such a dependence are doomed to fail in most cases. For example the time trend in the claim frequency of motor insurance, i.a. due to such a trend in traffic intensity, cannot be neglected. Finally it might here be referred to Cramér, Skand. Akt. Tidskr. 1969, who gives an indication for the construction of the model here concerned. The theory of these models have, further, been treated by Grandell in a paper to the Astin Colloquium in Randers, soon to be published in Skand.Akt.Tidskr. It seems, therefore, as if my ideas in this respect will be followed up by other writers, which—according to Seal's comments—"they should be".

Stockholm, November 12, 1971 CARL PHILIPSON