PRICING EXCESS-OF-LOSS CASUALTY WORKING COVER REINSURANCE TREATIES

by Gary Patrik and Russell John

Discussion by Jerry A. Miccolis

GENERAL COMMENTS

This is an interesting paper. It presents a progress report on the analytical approach one large reinsurer is developing toward the pricing of excess casualty coverage. The approach is an analytical one, in that pricing decisions are made on the basis of information generated by a theoretical pure premium distribution fitted to sample data.

The authors illustrate their techniques via two examples: one a new doctors' mutual (for which there is precious little historical data) and the other an excess-of-loss treaty between a reinsurer and a large primary insurer (for which there is a wealth of detailed pricing information).

A note on format. The authors present their work in phases: description of the coverages, the pricing approach, the model, parameter estimation, results, and conclusions. Within most phases, the two examples are presented separately with much of the technical detail left to appendices. The order of presentation is a matter of personal preference, but I had a much easier time following the flow of the paper by reorganizing it so that I could trace the complete development of first one example through all phases, and then the other.

PRICING PHILOSOPHY

In Section III, the authors mention five items to consider in pricing a reinsurance treaty: 1) the distribution of aggregate loss of the treaty, 2) the distribution of the cash flow of the treaty, 3) a number of corporate criteria (including other treaties in the reinsurer's portfolio, surplus, assets, investment opportunities, corporate goals, and corporate views on risk vs. rate-of-return), 4) "needed surplus" to support the treaty, and 5) distribution of the rate of return on "needed surplus" for each treaty in the reinsurer's portfolio. The paper concentrates on item (1), the distribution of aggregate loss to the reinsurer under the treaty, citing it as "the least ambiguous and most important" item of the five.

I balk slightly at the term "most important". I would select any of the other four items as being more important than item (1), and I suppose that when the perfect pricing model is someday developed, all five items will be thoroughly treated. However, given the present state of our art/science, I grant that knowledge of item (1) is a prerequisite to intelligent formulation of a model treating the latter four items, and in this sense, then, it may be the most important, and deserves our current attention.

AGGREGATE LOSS MODEL

The conceptual meat of the paper is contained in Section IV. The portfolio is assumed to consist of several groups of "independent" risks. (I'll explain the use of the quotation marks shortly.) *Each group has its own distribution of number of claims, and its own* distribution of size of loss for each claim. Further, the specification of these distributions is contained in a "parameter vector", θ, for each group. This is a convenient formulation, since anything that might cause the losses in different groups to move together (e.g., inflation) can be parameterized and thrown into the parameter vector. This allows one to state that the conditional distributions (given θ) for all groups are mutually independent (and hence the quotes above). *The authors show how the necessary conditional* distributions are derived and their moments computed, and then describe how to weight these moments together to arrive at the moments of the unconditional distribution of aggregate loss for the entire portfolio.

SOME TECHNICAL POINTS

There are some errata in the version of the paper that I received. They are itemized in the Appendix following.

Below are some random thoughts of a somewhat technical nature arranged in no particular order:

Section IV

1. It should be noted that unless some grouping of the portfolio is found such that the authors' three assumptions preceding equation (4.3) are met (or at least approached), then there is no advantage to grouping.

2. A simple description of the convolution concept before equation (4.3) might be useful to the lay reader.

3. I'm not sure the presentation would suffer if the notion of "cumulants" was never introduced. Equations (4.5) could be derived without them.

4. Equations (4.7) might deserve derivation in an appendix.

Section V

5. The next-to-last paragraph, last sentence, mentions low, medium and high loss-amount c.d.f.'s. On what basis are these c.d.f.'s characterized low, medium and high? (unlimited mean? coefficient of variation?)

Section VII

6. It is interesting to note that the structure function (i.e., the subjective distribution function of the parameter vector θ) in Example A does not permit much mixing of the frequency and

severity distributions. That is, the "low" claim-count c.d.f. always occurs in conjunction with the "low" loss-amount c.d.f. and similarly for the medium and high c.d.f.'s.

7. To a casual reader of the risk theory papers in the authors' bibliography, it would appear controversial that Patrik and John claim good results for the NP-approximation when the coefficient of skewness is fairly large (i.e., $2 < \gamma_1 < 8$). I think further elaboration by the authors on their position and its apparent conflict with the views of some of risk theory's pioneers would be extremely enlightening.

8. I wonder if a simulation approach would not produce more cost-effective results. In particular, I wonder if it would eliminate recourse to the "Chebyshev-like bound" of equation (7.3).

9. Despite the above comments, I certainly agree with the authors that too much concern over an approximation technique may miss the point. There is so much opportunity for error (the specification of trend and loss development, the choice of the general form of the c.d.f.'s, the use of broad industry data in Example A) that perhaps nothing more than ballpark estimates should be strived for.

Appendix A

10. Page A2: One would expect a smooth progression of parameters for the loss-amount c.d.f. as one moves from low to medium to high. This is not the case for the XP parameter for physicians nor

for the β parameter for surgeons. A rationale for these apparent reversals might be instructive.

<u>Section VIII</u>

11. In the discussion of item (2), mention is made of discounting the future cash flow. I think treatment of this topic is incomplete without consideration of the potentially offsetting phenomenon of inflation on outstanding losses.

12. The discussion of items (4) and (5) contain some ad-hoc measures of supporting surplus and the expected return on such surplus. These measures are elegant in their simplicity and usefulness.

13. The next-to-last paragraph claims that the paper has application beyond excess-of-loss reinsurance. I'd like to issue a warning against using the model (in particular the four-parameter loss amount c.d.f. of Appendix D) for pricing coverage at limits near the truncation point, t. The four-parameter c.d.f. was derived in the context of increased limits pricing where the truncation point was well below basic limits. In this context, the shape of the c.d.f. to the left of t is immaterial, and the form chosen in Appendix D for $G_s(x|\alpha, \beta, t, XP)$ for $x \leq t$, is as arbitrary as any other choice. Indeed, all that matters is that $G_s(x|\alpha, \beta, t, XP)$ reach XQ by the time x reaches t from the left, and the route $G_s(\cdot)$ takes to get to XQ is quite irrelevant. As it happened, the ISO Increased Limits Subcommittee only decided to use a truncation point

in the first place because no theoretical c.d.f. could be found to fit empirical data from first dollar. Further, since in increased limits pricing the concern is with the "tail" of the distribution, it was only necessary to find a distribution which fit the empirical data to the right of a chosen truncation point. This background should be kept in mind when applying the authors' model to anything other than increased limits or excess-of-loss pricing.

SUMMARY

This is an important paper, as are all intelligent attempts at modelling an insurance process. It takes some high-powered techniques and applies them in a practical way. It claims application for risk theory techniques beyond the boundaries imposed by the originators of those techniques. In the "spirit of a Call for Papers" it should generate much response among actuaries and hopefully some suggestions for future enhancement.

I commend the authors on their progress thus far.

Appendix
Sheet 1

ERRATA

1. Section IV, first paragraph: the first reference should be "Bühlmann (1970)".

2. Section IV, group definition: the passage reads, "... For example A, our groups will be defined by year of coverage and ISO doctor class ...". Since the authors are only dealing with the first year of coverage in example A, the groups are defined solely by ISO doctor class.

3. Section IV, definition of cumulant following equation (4.4): "... the moment generating function of L evaluated at 0 ..." should read "... the moment generating function of L given θ evaluated at 0 ...".

4. Section IV, following equation (4.10): "When θ is unknown, equations (4.3) - (4.7) usually no longer hold. In particular, equation (4.5) now holds only for the first moment ..." should read "When θ is unknown, the <u>unconditional</u> <u>counterparts</u> to equations (4.3) - (4.7) usually do not hold. In particular, equation (4.5) would hold <u>unconditionally</u> only for the first moment ...".

5. Section IV, equation (4.14): As it stands, the equation imparts a positive probability to the event of a negative claim. The equation should read:

$$G_x(x|\theta) = \begin{cases} 0 & \text{if } x < 0 \\ G_s(x + r|\theta) & \text{if } 0 \leq x < b - r \\ 1 & \text{if } b - r \leq x \end{cases}$$

6. Section V, next-to-last paragraph: The reference, "NAIC (1977, 1979)" should read "NAIC (1977, 1978)".

7. Appendix A, page A1: Footnotes (2) and (3) are confusing and do not seem to match their respective columns.

8. Appendix D, page D1: The specification of the Negative Binomial density function is wrong. It should be:

$$f(x|p,\alpha) = \binom{\alpha + x - 1}{x} p^{\alpha} (1 - p)^{x} \quad \text{for } x = 0, 1, 2 \ldots$$

where $0 < p < 1$ and $\alpha = 1, 2, 3 \ldots$

The density may be generalized to the case of non-integer α as follows:

$$f(x|p,\alpha) = \frac{\Gamma(x + \alpha)}{x!\ \Gamma(\alpha)} p^{\alpha} (1 - p)^{x} \qquad \text{for } x = 0, 1, 2 \ldots$$

where $0 < p < 1$ and $\alpha > 0$

Appendix
Sheet 3

9. Bibliography: The following reference is missing:

Weissner, Edward (1978) "Estimation of the Distribution of Report Lags by the Method of Maximum Likelihood", PCAS, Volume 65.