THE ACTUARY AND IBNR

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DISCUSSION BY WARREN P. COOPER

This will not be a very spicy review. I have been unable to find the one mandatory flaw which, despite all succeeding praise, puts the logical flow of the paper in question. To the contrary, I find the authors' suggested treatment of Incurred But Not Reported Losses altogether congenial. So much so that I shall simply give four varyingly garrulous comments and pose one question. Hopefully, they will serve to amplify the authors' position, not just expose my own prejudices.

1. The idea of using losses to predict losses is a particularly happy one. A genuinely unscientific telephone survey has convinced me that a minimum of 80% of our industry's total IBNR is established by referencing premiums in one way or another, my own company not excepted. As the authors point out, premiums in force now seem to be in vogue, although I'm not sure I understand all the critical points of the rationalization. In the "longest-tail" lines, such as products and professional liability, the bulk of IBNR claims arise from expired policies, not those in force, and while such claims may not predominate in the usual third-party unreported reserves, they add up to be a substantial portion. Earned premiums, according to some of our sages, do the trick, particularly calendar-year earnings used to predict accident-year IBNR. Certainly all the now-classical arguments in favor of this accounting marriage are convincing, unless we face up to one usually unexpressed assumption: premiums are a proper measure of exposure. Wouldn't that solve a bunch of problems if it were true? We could even stop compromising our definitions of exposure, where we can define it, and cease looking for definitions where we can't. But, alas, the assumption is not necessarily true for many reasons: regulations, individual risk rating, etc. Consider the lively marketplace of 1973, where our favorable underwriting results for the previous two years are generating strong pressures to reduce rates. Better experience is expected to bring rate decreases, but unfortunately, the illogical, non-actuarial work does not always match the individual cost reduction with a cognate reduction in loss exposure. Competition is more generally felt. The same book of, say, commercial multiple peril busi-

ness in 1973 could produce a substantially lower premium flow than it did in 1972. While the severity, perhaps even the frequency, of IBNR it brings along to the insurers should also drop, I doubt it it will go as far. In a highly competitive arena prices tend to over-react. This is just another face of the cyclical swings we find in insurance profits. Alternate speed-ups and lags in pricing are one of the causes of these swings. Do we really want to tie our reserves to the pendulum?

For those lines where earned exposures are realistic and measurable, we should be able to use them to predict a stable, adequate IBNR. I hope someone will devote some time to the relationship and share their findings with the rest of us. But for now, the procedures that Messrs. Bornhuetter and Ferguson have described allow us to calculate reserves that are responsive, as the outside world demands, and efficiently consistent across lines, as our comptrollers demand.

The authors of the paper point out that expected loss ratios are best considered in certain cases and there's no faulting their logic, as most of us involved in "long-tailed" business will testify. "Expected loss ratios" is a not very well hidden reference to premiums and my remarks above caution against premiums in any disguise. However, my arguments do not solve the problems that loss ratio budgeting does in slender-data circumstances. There, they must be used. In other cases, the authors note, the results of calculating IBNR by expected loss ratios on the one hand and extending losses on the other should put our reserves in just about the same condition. Ideally this is so, but realistically it is probably not so. More bears on the real situation than the Central Limit Theorem.

2. I suspect the authors' lumping of classical IBNR and development on known cases together will rouse uneasy feelings in some of our colleagues. There are reasonable arguments for putting the "development-on-known" portion in either the IBNR or the outstanding column. Pragmatically, does it matter as long as the liabilities are fully accounted for? At the very least, the authors have given us a vehicle to get where we want to go and where the law wants us to be. However, I can see reason to separate the two pieces for control purposes, i.e., in order to interpret distortions in development patterns. Under the paper's definition, unreported reserves cover four situations: "true" unreported, reopenings, known but unrecorded items and known case development. In the third-party lines, we find, this last is less accurately predictable than the sum of the first three. Hence the excellent papers we have in the *Proceedings* on

problems with changing reserve margins. Fluctuation in margins is not altogether random; the management of our claims departments has considerable influence upon it. In a primary company, we should know what unusual actions the claims people have taken: extra periodic reviews, wholesale markdowns, special analyses, etc., and what policy changes they may institute. Armed with these bits of information, we can understand strange patterns and make corrective adjustments. For a reinsurer, the case is not so clear, since he is dealing with many claims staffs most of whom are probably strangers to him. The primary carrier might consider two routines, one like that the authors present and another similar one that ignores known case development. The difference is, of course, the latter. As an expression of our belief in the control value of the second routine, we sort accident-year losses by known and unreported to watch their separate development over several years. We suspect the margin change patterns are dissimilar, but we do not know definitely as yet.

3. The crucial step in the authors' IBNR calculation is the selection of the year-to-year development factors, those extrapolated from their Exhibit A and developed on Exhibit B. In the paper, a three-year average, weighted by the earlier years' incurred claims value, is used. Like all averaging procedures, the calculation smoothes out underterminable yearly variations. It also smoothes out known aberrations. I am certain that the authors do not advise slavish use of their formula to derive the factors; in the text they suggest curve fitting, trending and judgment adjustments. I would like to underline their suggestion. One of the elements bunched into the IBNR is what we might call incurred but not recorded (perhaps abbreviated IBNR' or a $_l$IBNR), the load of claims that for one reason or another are clogged in the processing pipeline and don't make it to the drain by the time of the end of period cut-off. This is the kind of imponderable we actuaries like to believe is a constant and can, therefore, be conveniently ignored. To our misfortune, it is not, and it is probably impossible to derive an algorithm to account for it. Let me give you an example. A few years ago our company shifted to an entirely new common master file system for all losses in the house. We expected conversion drags and a strong jump in entry rejects; we got disruption in profusion. The situation stabilized after two or three quarters, but, one of the unstable quarter-ends was also a year-end when we had more than double the expected processing lag. We made exceptional surveys to account for this aberration in the Annual Statement. We couldn't correct entry date, so, on an accident-year argument, the rejects flowed into our reports as late

reportings. Two years later, an analysis very similar to that in the paper
was made of our third-party experience. If we had believed the three-year
average factor, our IBNR would have been overstated by several millions
of dollars; a plus, perhaps, on the solvency side, but a definite minus to
the IRS. If there is a moral to this tale, it is to emphasize the need for in-
spection of the data, isolation of strange patterns and judgment adjust-
ments. After all, we actuaries are clinicians, aren't we? Mathematical
procedures offer extensive help, but, of necessity, they are impersonal
and the data sluggish. Together they might leave you with an IBNR sub-
stantially above, or below, the necessary amount. Experienced diagnosis
and prescription is a critical step in selecting development factors.

4. Throughout the paper, the authors refer to varying ways their
systematic approach may be refined. This flexibility is a prime virtue and
should be explicitly stated. Their concept, after all, is a model for predict-
ing IBNR as defined. Like any model, it may be contracted, or expanded,
as the case warrants. The warranting case might be the peculiarity of a
particular line (witness the choice between using expected loss ratios or
extending incurred losses); or it might be some unique company charac-
teristic. The model can be more, or less, rigorous mathematically, de-
pending on how the company views the value of rigor. As rehearsed
above, it certainly can incorporate the clinical actuary's diligent insights.
Not the least impressive aspect of the system's flexibility is its ability to
monitor the reserve once established and allow for interim adjustments.
The authors develop some alternatives in this area, while most of our
IBNR computational schemes do not serve in this regard at all.

5. Finally the question. Why do the authors express surprise that
IBNR flow in prior accident years is skewed? If the most recent year's
development reflects a constantly diminishing rate, why not the other
years, at least in frequency? Severity, especially in the later years of develop-
ment, will probably be random. Both factors should make the 25% per
quarter distinction unlikely.