The need for diagnostic assessment of bootstrap predictive models

Glen Barnett and Ben Zehnwirth

The bootstrap is, at heart, a way to obtain an approximate sampling distribution for a statistic (and hence, if required, produce a confidence interval). Where that statistic is a suitable estimator for a population parameter of interest, the bootstrap enables inferences about that parameter. In the case of simple situations the bootstrap is very simple in form, but more complex situations can be dealt with. The bootstrap can be modified in order to produce a predictive distribution (and hence, if required, prediction intervals).

It is predictive distributions that are generally of prime interest to insurers (because they pay the outcome of the process, not its mean). The bootstrap has become quite popular in reserving in recent years, but it's necessary to use the bootstrap with caution.

The bootstrap does not require the user to assume a distribution for the data. Instead, sampling distributions are obtained by resampling the data.

However, the bootstrap certainly does not avoid the need for assumptions, nor for checking those assumptions. The bootstrap is far from a cure-all. It suffers from essentially the same problems as finding predictive distributions and sampling distributions of statistics by any other means. These problems are exacerbated by the time-series nature of the forecasting problem – because reserving requires prediction into never-before-observed calendar periods, model inadequacy in the calendar year direction becomes a critical problem. In particular, the most popular actuarial techniques – those most often used with the bootstrap – don't have any parameters in that direction, and are frequently mis-specified with respect to the behaviour against calendar time..

Further, commonly used versions of the bootstrap can be sensitive to overparameterization – and this is a common problem with standard techniques.

In this paper, we describe these common problems in using the bootstrap and how to spot them.

A basic bootstrap introduction

The *bootstrap* was devised by Efron (1979), growing out of earlier work on the *jackknife*. He further developed it in a book (Efron, 1982), and various other papers. These days there are numerous books relating to the bootstrap, such as Efron and Tibshirani (1994). A good introduction to the basic bootstrap may be found in Moore et al. (2003); it can be obtained online.

The original form of the bootstrap is where the data itself is resampled, in order to get an approximation to the sampling distribution of some statistic of interest, in order to make inference about a corresponding population statistic.

For example, in the context of a simple model $E(X_i) = \mu$, $i = 1, 2, \ldots, n$, where the X's assumed to be independent, the population statistic of interest is the mean, μ, and the sampling statistic of interest would typically be the sample mean, $\bar{x}$.

Consequently, we estimate the population mean by the sample mean ($\hat{\mu} = \bar{x}$) – but how good is that estimate? If we were to collect many samples, how far would the sample means typically be from the population mean?

While that question could be answered if we could directly take many samples from the population, typically we cannot resample the original population again. If we assume a distribution, we could infer the behaviour of the sample mean from the assumed distribution, and then check that the sample could reasonably have come from the assumed distribution.

(Note that rather than needing to assume an entire distribution, if the population variance were assumed known, we could compute the variance of the sample mean, and given a large enough sample, we might consider applying the central limit theorem (CLT) in order to produce an approximate interval for the population parameter, without further assumptions about the distributional form. There are many issues that arise. One such issue is whether or not the sample is large enough – the number of observations per parameter in reserving is often quite small. Indeed, many common techniques have some parameters whose estimates are based on only a single observation! Another issue is that to be able to apply the CLT we assumed a variance – if instead we estimate the variance, then the inference about the mean depends on the distribution again. As the sample sizes become large enough that we may apply Slutzky's theorem, then for example a t-statistic is asymptotically normal, even though in small samples the t-statistic *only has a t-distribution if the data were normal*. Lastly, and perhaps most importantly when we want a *predictive distribution*, the CLT usually cannot help.)

In the case of bootstrapping, the *sample* is itself resampled, and then from that, inferences about the behaviour of samples from the population are made on the basis of those resamples. The empirical distribution of the original sample is taken as the best estimate of the population distribution.

In the simple example above, we repeatedly draw samples of size *n* (with replacement) from the original sample, and compute the distribution of the statistic (the sample mean) of each resample. Not all of the original sample will be present in the resample – on average a little under 2/3 of the original observations will appear, and the rest will be repetitions of values already in the sample. A few observations may appear more than twice.

The standard error, the bias and even the distribution of an estimator about the population value can be approximated using these resamples, by replacing the population distribution, F by the empirical distribution F_n.

For more complex models, this direct resampling approach may not be suitable. For example, in a regression model, there is a difficulty with resampling the responses directly, since they will typically have *different* means.

For regression models, one approach is to keep all the predictors with each observation, and sample them together. That is, if $\underline{X}$ is a matrix of predictors (sometimes called a *design matrix*) and $\underline{y}$ is a data-vector, for the multiple regression model $\underline{Y} = \underline{X}\,\beta + \underline{\varepsilon}$, then the rows of the augmented design matrix [X|y] are resampled. (This is particularly useful when the X's are thought of as random.)

A similar approach can be used when computing multivariate statistics, such as correlations.

Another approach is to resample the *residuals* from the model. The residuals are estimates of the error term, and in many models the errors (or in some cases, scaled errors) at least share a common mean and variance. The bootstrap in this case assumes more than that – they should have a common distribution (in some applications this assumption is violated).

In this case (with the assumption of equal variance), after fitting the model and estimating the parameters, the residuals from the model are computed: $e_i = y_i - \hat{y}_i$, and then the residuals are resampled as if they were the data.

Then a new sample is generated from the resampled residuals by adding them to the fitted values, and the model is fitted to the new bootstrap sample. The procedure is repeated many times.

Forms of this *residual resampling* bootstrap have been used almost exclusively in reserving.

If the model is correct, appropriately implemented residual resampling works. If it is incorrect, the resampling scheme will be affected by it, some more than others, though in general the size of the difference in predicted variance is small. More sophisticated versions of this kind of resampling scheme, such as the second bootstrap procedure in Pinheiro et al. (2003) can reduce the impact of model misspecification when the prediction is, as is common for regression models, within the range of the data. However, the underlying problem of amplification of unfitted calendar year effects remains, as we shall see.

For the examples in this paper we use a slightly augmented version of Sampler 2 given in Pinheiro et al – the prediction errors are added to the predictions to yield bootstrap-simulated predictive values, so that we can directly find the proportion of the bootstrap predictive distribution below the actual values in one-step-ahead predictions.

In the case of reserving, the special structure of the problem means that while often we predict inside the range of observed accident years, and usually also within the range of observed development years, we are always projecting *outside* the range of observed calendar years – precisely the direction in which the models corresponding to most standard techniques are inadequate.

As a number of authors have noted, the chain ladder models the data using a two-way cross-classification scheme (that is, like a two-way main-effects ANOVA model in a log-link). As discussed in Barnett et al. (2005), this is an unsuitable approach in the accident and development direction, but the issues in the calendar direction are even more problematic. Even the more sophisticated approaches to residual resampling can fail on the reserving problem if the model is unsuitable.

Diagnostic displays for a bootstrapped chain ladder

Many common regression diagnostics for model adequacy relate to analysis of residuals, particularly residual plots. In many cases these work very well for examining many aspects of model adequacy. When it comes to assessing predictive ability, the focus should, where possible, shift to examining the ability to predict data not used in the estimation. In a regression context, a subset of the data is held aside and predicted from the remainder. Generally the subset is selected at random from the original data. However, in our case, we cannot completely ignore the time-series structure and the fact that we're predicting outside the range of the data. Our prediction is always of future calendar time. Consequently the subsets that can be held aside and assessed for predictive ability are those at the most recent time periods.

This is common in analysis of time series. For example, models are sometimes selected so as to minimize one-step-ahead prediction errors. See, for example, Chatfield (2000).

Out of sample predictive testing

The critical question for a model being used for prediction is whether the estimated model can predict outside the sample used in the estimation. Since the triangle is a time series, where a new diagonal is observed each calendar period, prediction (unlike predictions for a model without a time dimension) is of calendar periods after the observed data. To do out-of-sample tests of predictions, it is therefore important to retain a subset of the most recent calendar periods of observations for post-sample predictive testing. We refer to this post-sample-predictive testing as *model validation* (note that some other authors use the term to mean various other things, often related to checking the usefulness or appropriateness of a model).

Imagine we have data up to time t. We use only data up to time $t–k$ to estimate the model and predict the next k periods (in our case, calendar periods), so that we can compare the ability of the model to predict actual observations not used in the estimation. We can, for example, compute the prediction errors (or validation residuals), the difference between observed and predicted in the validation period. If these prediction errors are divided by the predictive standard error, the resulting standardized validation residuals can be plotted against time (calendar period most importantly, and also accident and development period), and against predicted values, (as well as against any other likely predictor), in similar fashion to ordinary residual plots. Indeed, the within-sample residuals and "post-sample" predictive errors (validation residuals) can be combined into a single display.

One step ahead prediction errors are related to validation residuals, but at each calendar time step only the next calendar period is predicted; then the next period of data is brought in and another period is predicted.

In the case of ratio models such as the chain-ladder, prediction is only possible within the range of accident and development years used in estimation, so out of sample prediction cannot be done for all observations left out of the estimation. The use of one step ahead prediction errors maximizes the number of out-of-sample cases that have predictions. Further, when reserving, the liability for the next calendar period is generally a large portion of the total liability, and the liability estimated will typically be updated once it is observed; this makes one-step-ahead prediction errors a particularly useful criterion for model evaluation when dealing with ratio models like the chain ladder.

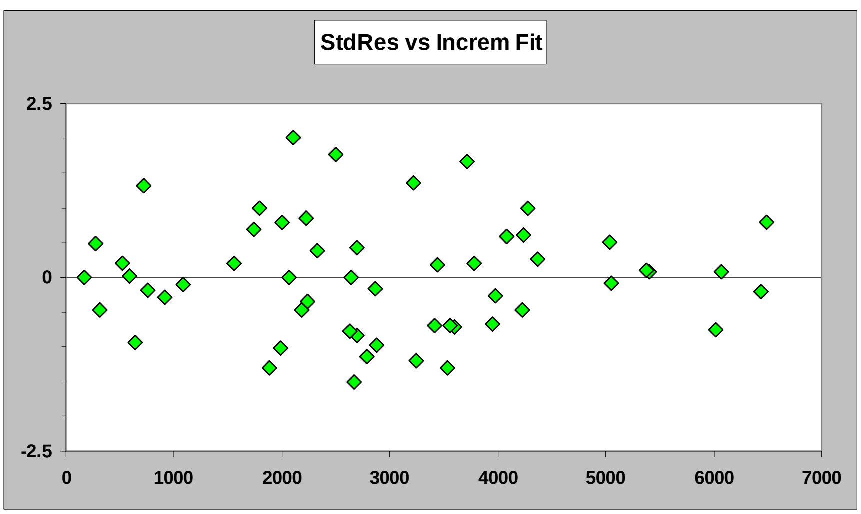
For a discussion of the use of out of sample prediction errors and in particular one-step-ahead prediction errors in time series, see Chatfield (2000), chapter 6.

For many models, the patterns in residual plots when compared with the patterns in validation residuals or one step ahead prediction errors appear quite similar. In this circumstance, ordinary residual plots will generally be sufficient for identifying model inadequacy.

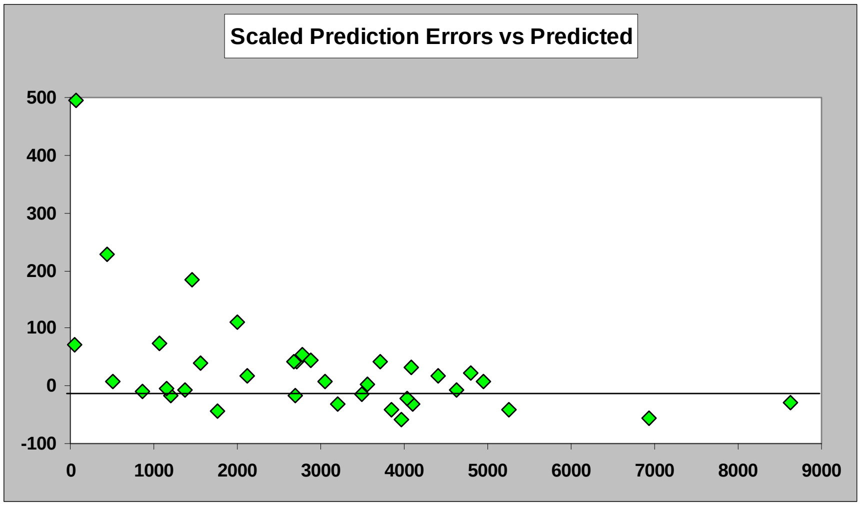
Critically, in the case of the Poisson and quasi-Poisson GLM that reproduce the chain ladder, the prediction errors and the residuals *do* show different patterns.

Example 1:
See Appendix A. This data was used in Mack (1994). The data are incurred losses for automatic facultative business in general liability, taken from the Reinsurance Association of America's Historical Loss Development Study.

If we fit a quasi- (or overdispersed) Poisson GLM and plot standardized residuals against fitted values, the plot appears to show little pattern:

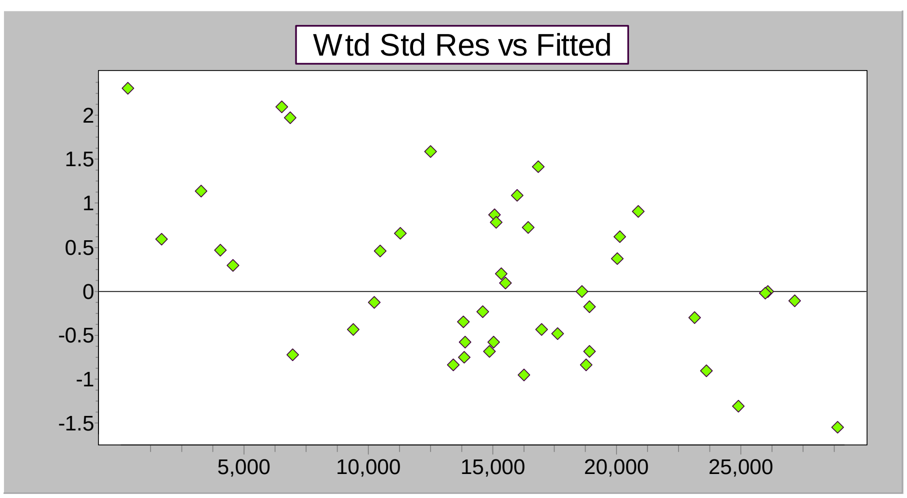


However, if we plot *one step ahead prediction errors* (scaled by dividing the prediction errors by $\hat{\mu}^{1/2}$) against predicted values, we *do* see a distinct pattern of mostly positive prediction errors for small predictions with a downward trend toward more negative prediction errors for large predictions:



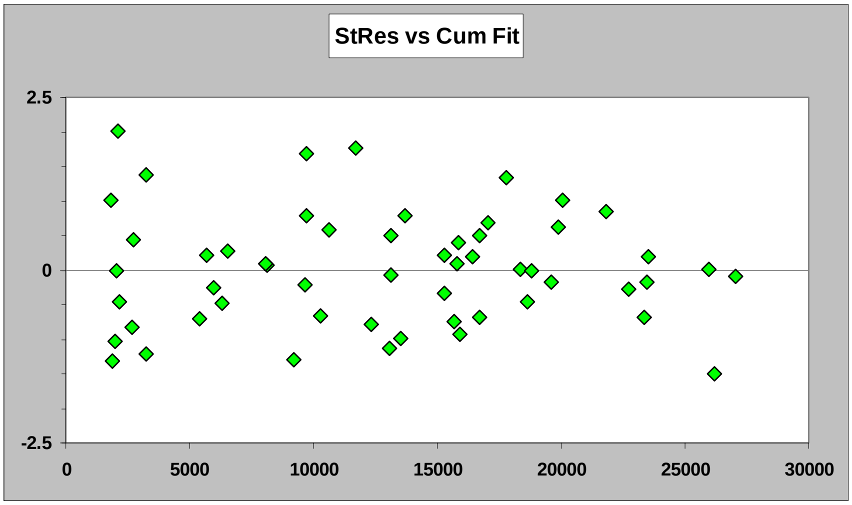
Prediction errors above have not been standardized to have unit variance. The underlying quasi-Poisson scale parameter would have a different estimate for each calendar year prediction; it was felt that the additional noise from separate scaling would not improve the ability of this diagnostic to show model deficiencies. On the other hand, using a common estimate across all the calendar periods would simply alter the scale on the right hand side without changing the plot at all, and has the disadvantage that for many predictions you'd have to scale them using "future" information. On the whole it seems prudent to avoid the scaling issue for this display, but as a diagnostic tool, it's not a major issue.

This problem of quite different patterns for prediction errors and residuals does not tend to occur with the Mack formulation of the chain ladder, where ordinary residuals are sufficient to identify this problem:



As noted in Barnett and Zehnwirth (2000), this is caused by a simple failure of the ratio assumption – it is not true that $E(Y|X) = \beta X$, as would be true of any model where the next cumulative is assumed to be (on average) a multiple of the previous one. (For this data, the relationship between Y and X does not go through the origin.)

The above plot is against cumulatives because in the Mack formulation, that's what is being predicted. For comparison, here are the OD Poisson GLM residuals vs cumulative fitted rather than incremental fitted:



Why is the problem obvious in the residuals for the Mack version of the chain ladder model, but not in the plots of GLM residuals vs fitted (either incremental or cumulative)? Even though the two models share the same prediction function, the *fitted values* of the two models are different.

On the cumulative scale, if X is the most recent cumulative (on the last diagonal) and Y is the next one, both models have the prediction-function $E(Y|X) = \beta X$.

Within the data, the Mack model uses the same form for the fit – $E(Y|X) = \beta X$, but the GLM does not – you can write it as $E(Y) = \beta E(X)$, which seems similar enough that it might be imagined it would not make much difference, but the right hand side involves "future" values not available to predictions. This allows the fit to "shift" itself to compensate, so you can't see the problem in the fits. However, the out-of-sample prediction function is the same as for the Mack formulation, and so the predictions from the GLM suffers from *exactly* the same problem – once you forecast future values, you're assuming $E(Y|X) = \beta X$ for the future – and it *does not work.*

Adequate model assessment of ODP GLMs therefore *requires* the use of some form of out-of-sample prediction, and because of the structure of the chain ladder, this assessment seems to be best done with one-step-ahead prediction errors. For many other models, such as the Mack model, this would be useful but not as critical, since we can identify the problem even in the residuals.

Assessing bootstrap predictive distributions

When calculating predictive distributions with the bootstrap, we can in similar fashion make plots of standardized prediction errors against predicted values and against calendar years. Of course, since the prediction *errors* are the same, the only change would be a difference in the amount by which each prediction error is scaled (since we have bootstrap standard errors in place of asymptotic standard errors from an assumed model); the broad pattern will not change, however, so the plot based on asymptotic results are useful prior to performing the bootstrap.

Since we can produce the entire predictive distribution via the bootstrap, we can evaluate the percentiles of the omitted observations from their bootstrapped predictive distributions – if the model is suitable, the data should be reasonably close to "random" percentiles from the predictive distribution. This further information will be of particular interest for the most recent calendar periods (since the ability of the model to predict recent periods gives our best available indication if there is any hope for it in the immediate future – if your model cannot predict last year you cannot have a great deal of confidence in its ability to predict *next* year).

We could look at a visual diagnostic, such as the set of predictive distributions with the position of each value marked on it, though it may be desirable to look at all of them together on a single plot, if the scale can be rendered so that enough detail can be gleaned from each individual component. It may be necessary to "summarize" the distribution somewhat in order to see where the values lie (for example, indicating 10^{th}, 25^{th}, 50^{th}, 75^{th} and 90^{th} percentiles, rather than showing the entire bootstrap density). In order to more readily compare values it may help to standardize by subtracting the mean and dividing by the standard deviation, though in many cases, if the means don't vary over too many standard deviations, simply looking at the original predicted values on (whether on the original scale or on a log scale) may be sufficient – sometimes a little judgement is required as to which plot will be most informative.

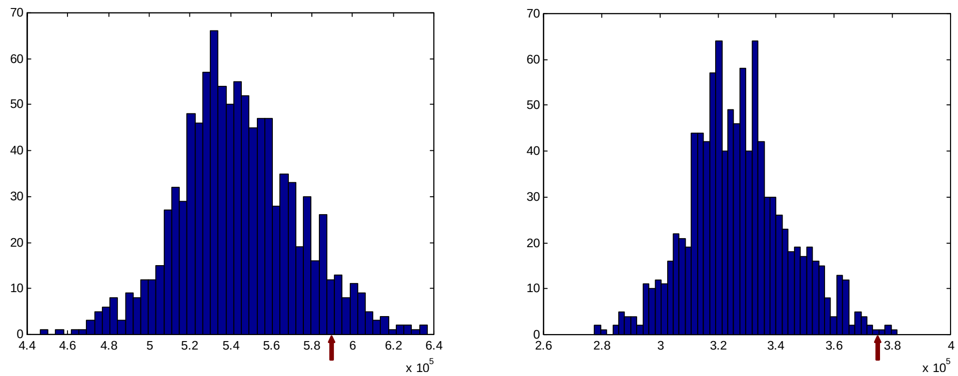
In some circumstances it might also be useful to obtain a single summary of the indicated lack of 'predictive fit'. If the data are "random" percentiles from their predictive distributions, the proportion of the predictive distribution below which each observation falls should be uniform. Of primary interest would be (i) substantial bias in the predictive distribution (both up and down bias are problematic for the insurer), and (ii) substantial error in the variability of the predictive distribution. The first will tend to yield percentiles that are too high or too low, which the second will either yield percentiles that are both too high *and* too low (if the predictive variance is underestimated), or percentiles that are clustered toward the centre (if it is overestimated). If p_i is the proportion of the predictive distribution below the observed value, then $q_i = 2|\, p_i - ½\,|$ should also be uniform, but if there are too many extreme percentiles (either from upward or downward bias, or from underestimating of the predictive variances), the q_i values will tend to be too large, while if there are too few extreme percentiles the q_i values will tend to be too small. There are various ways of combining the q_i into a single diagnostic measure. Once such would be to note that if the q's are uniform, then $2.\exp(-\{1-q_i\})$ has a chi-square distribution with 2d.f. (with large values again indicating an excess of extreme percentiles). Several of these (k, say) may be added if a single statistic is desired and compared with a χ^2_{2k} distribution. Unusually large values would be of particular interest, though unusually small values would also be important. If required this could be used as a formal hypothesis test, but it is generally of greater value as a *diagnostic* summary of the overall tendency to extreme percentiles.

Example 2

ABC data. This is Worker's Compensation data for a large company. This data was analyzed in some detail in Barnett and Zehnwirth (2000). See appendix B.

In this example we actually use the bootstrap predictions discussed in the basic bootstrap introduction above, based on the second algorithm from Pinheiro et al (2003). Below are the predictive distributions for the first two values (after DY0) for the last diagonal, for a ODP GLM fitted to the data prior to the final calendar year, which was omitted. The brown arrows mark the *actual* observation that the predictive distribution is attempting to predict.

ABC Predictive distribution for last diagonal - histograms

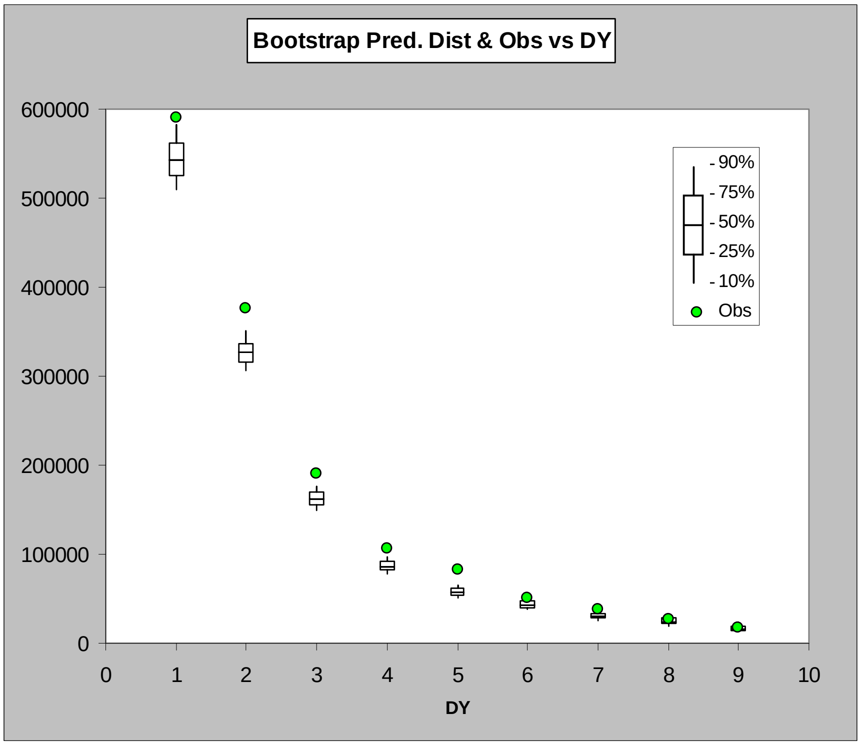


Delay 1 **Delay 2**

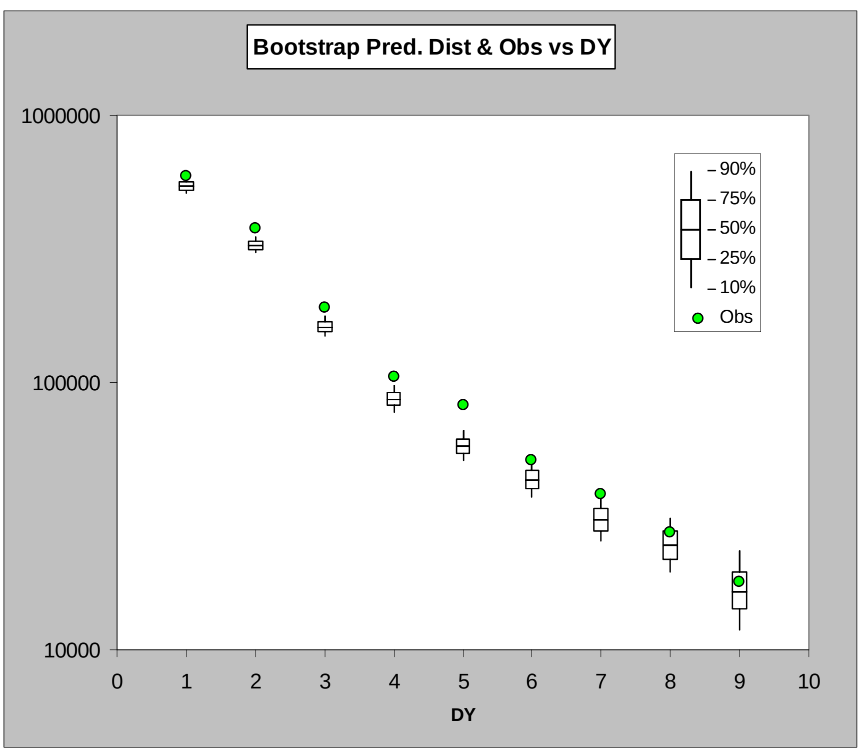
The arrow marks the actual observation for that delay; for the two distributions shown, the observed value sits fairly high. For a single observation, this might happens by chance, even with an appropriate model, of course.

The runoff decreases sharply for this triangle, so most of this information in the histograms would be lost if we looked at them on a single plot. Consequently, for a more detailed examination, the bootstrap results are reduced to a five-number summary of the percentiles:

ABC Predictive distribution for last diagonal – box and whisker plots



As can be seen, the actual payments for the first seven development periods are all very high, but it's a little hard to see the details in the last few periods. Let's look at them on the log-scale:



Now we can see that in all cases the observations sit above the median of the predictive distribution, and all but the last two are above the upper quartile.

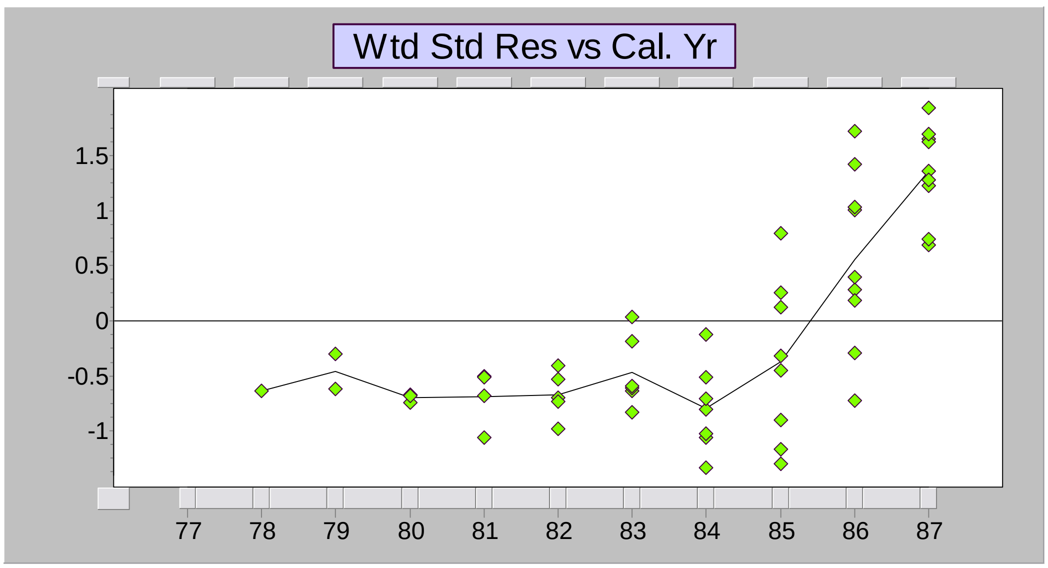
Below is a summary table of the bootstrap distribution for the final calendar year:

ABC: Bootstrap Predictive distributions for last calendar year

DY	Actual	10%	25%	50%	75%	90%	% ≤obs
0	496200						
1	590400	509620	525430	542150	562070	583270	93.9
2	375400	306580	315890	326600	337060	351080	99.6
3	190400	148750	155240	161520	169110	176290	98.9
4	105600	77760	81850	86220	91330	97340	99.2
5	82400	51050	54270	57740	61590	65730	100
6	51000	37440	40300	43360	46950	51380	89.3
7	38000	25490	27940	30770	33680	37110	92
8	27400	19430	21920	24540	27840	30970	72.9
9	18000	11930	14210	16460	19630	23450	63.9
10	12200						

So what's going on? Why is this predicting so badly?

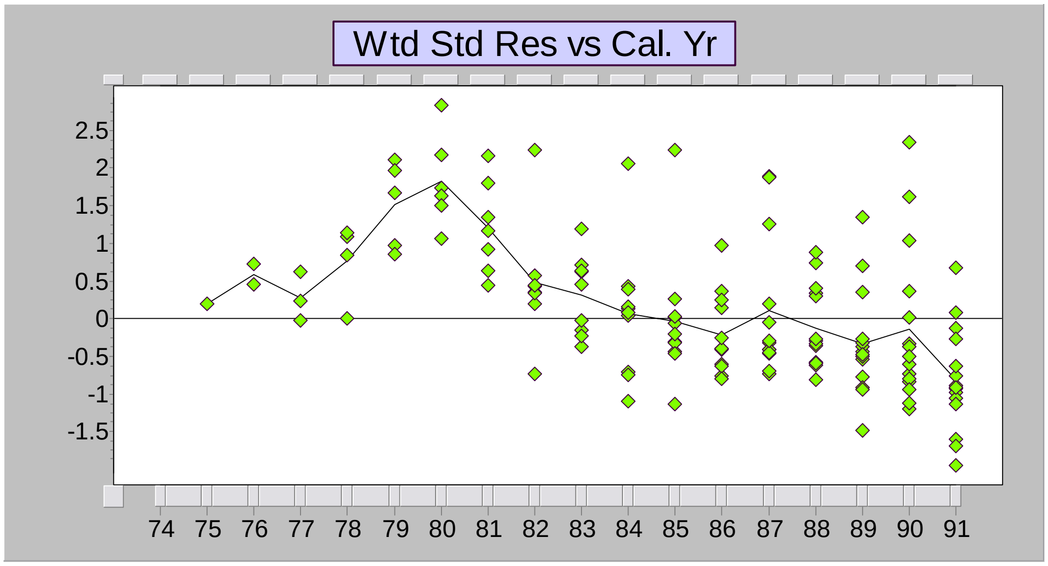
Well, we can see via one-step-ahead prediction errors that there's a problem with the assumption of no calendar period trend; alternatively, as we noted earlier, we can look at residuals from a Mack-style model, and get a similar impression:



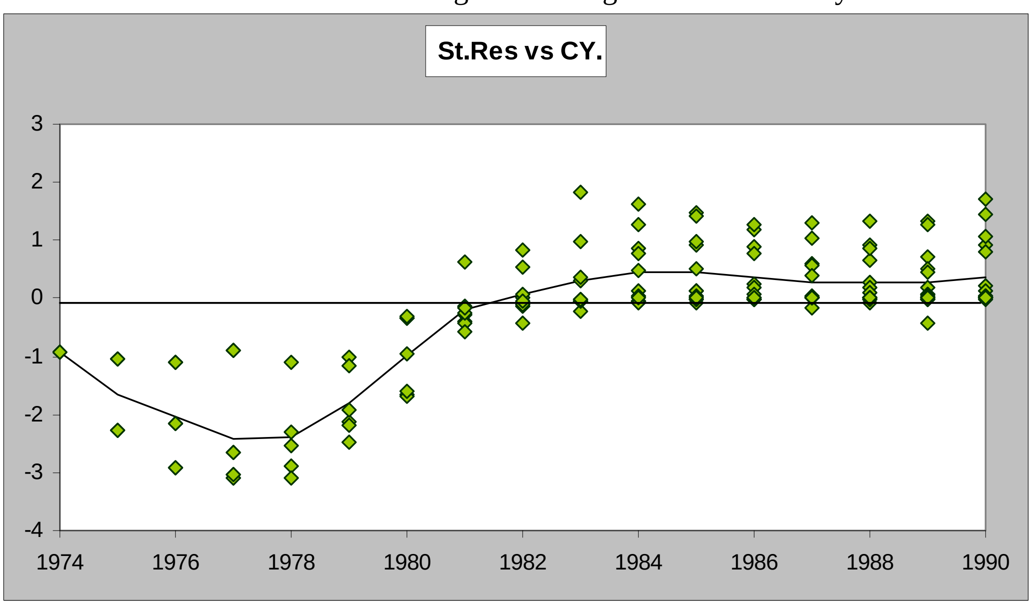
We can see a strong trend-change. Consequently, predictions of the last calendar year will be too low. One major difficulty with the common use of the chain ladder in the absence of careful consideration of the remaining calendar period trend is that there is no opportunity to apply proper judgement of the future trends in this direction, because the practitioner lacks information about the past behaviour for a context in which to even seek information that would inform scenarios relating to the future behaviour.

Example 3 – LR high
The data for this example is available in Appendix C. As we have seen, we can look at diagnostics and assess before we attempt to produce bootstrap prediction intervals whether we should proceed.

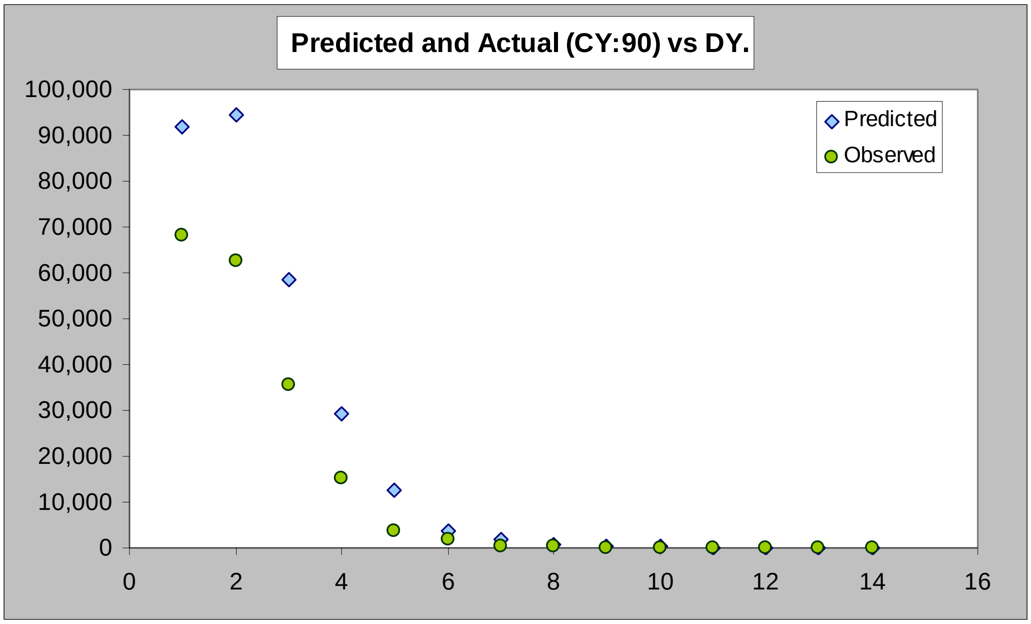
Here are the standardized residuals vs calendar years from a Mack-style Chain ladder fit. As you can see, there's a lot of structure.



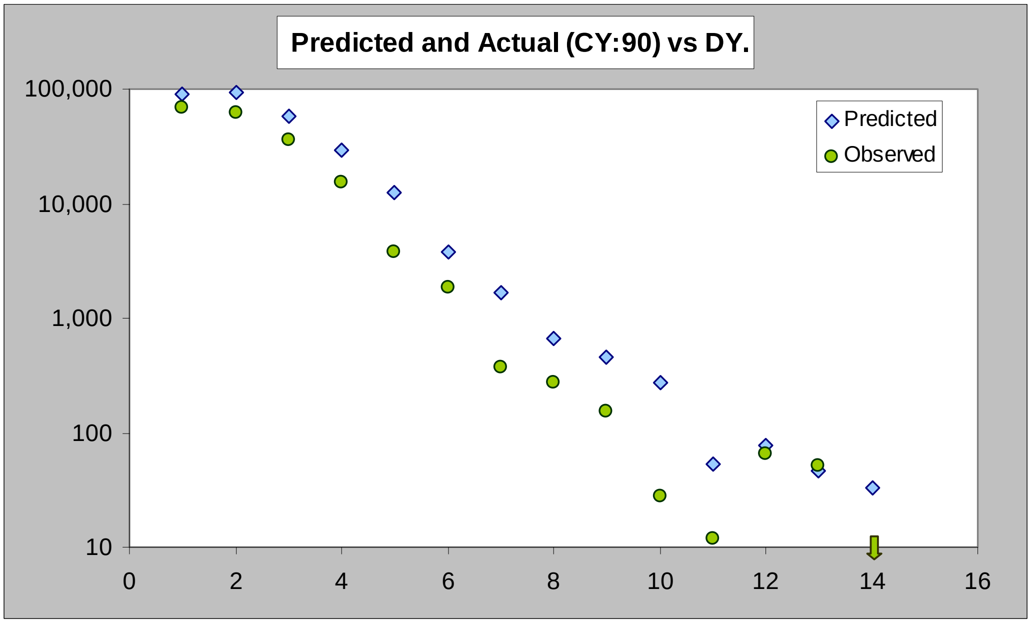
There's similar structure in the overdispersed Poisson GLM formulation of the chain ladder - residuals show there are strong trend changes in the calendar year direction:



However, as we described before, this residual plot gives the incorrect impression that the GLM is underpredicting. This impression is incorrect, as we see by looking at the validation (one step ahead predictions) for the last year:

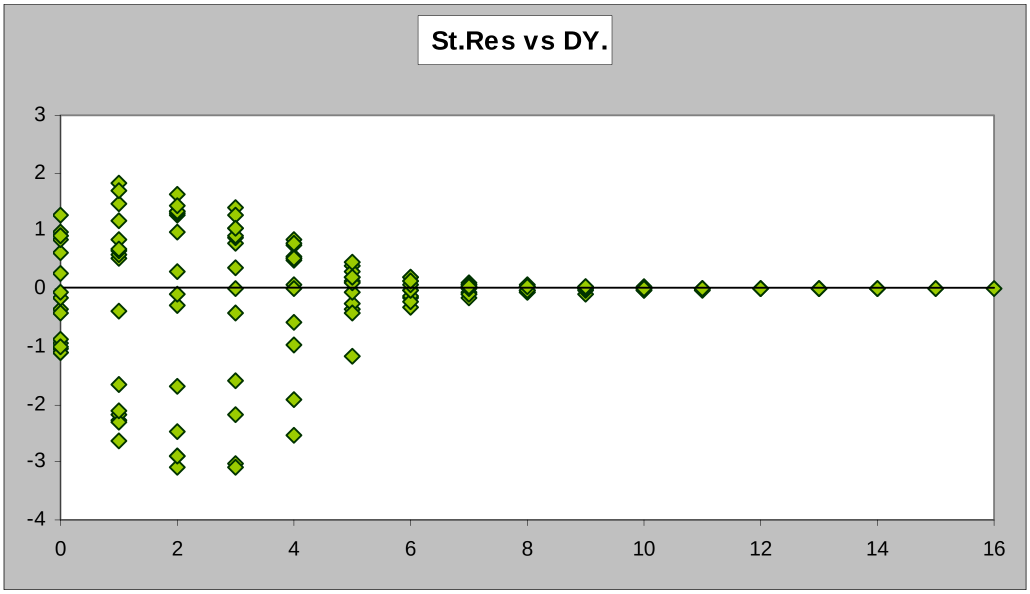


It's a little hard to see detail over on the right, so let's look at the same plot on the log scale:



The Mack-model residual plot gave a good indication of the predictive performance of the chain ladder (bootstrapped or not) for both the Mack model *and* the quasi-(overdispersed) Poisson GLM. It's always a good idea to validate the last calendar year (look at one-step-ahead prediction errors), but a quick approximation of the performance is usually given by examining residuals from a Mack-chain ladder model.

A further problem with the GLM is revealed by the plot of residuals vs development year:



The assumed variance function does not reflect the data.

Example 4
The next example has been widely used in the literature relating to the chain ladder. Indeed, Pinherio et al. (2003) referred to it as a “benchmark for claims reserving models”. The data come from Taylor and Ashe (1983). See Appendix D.

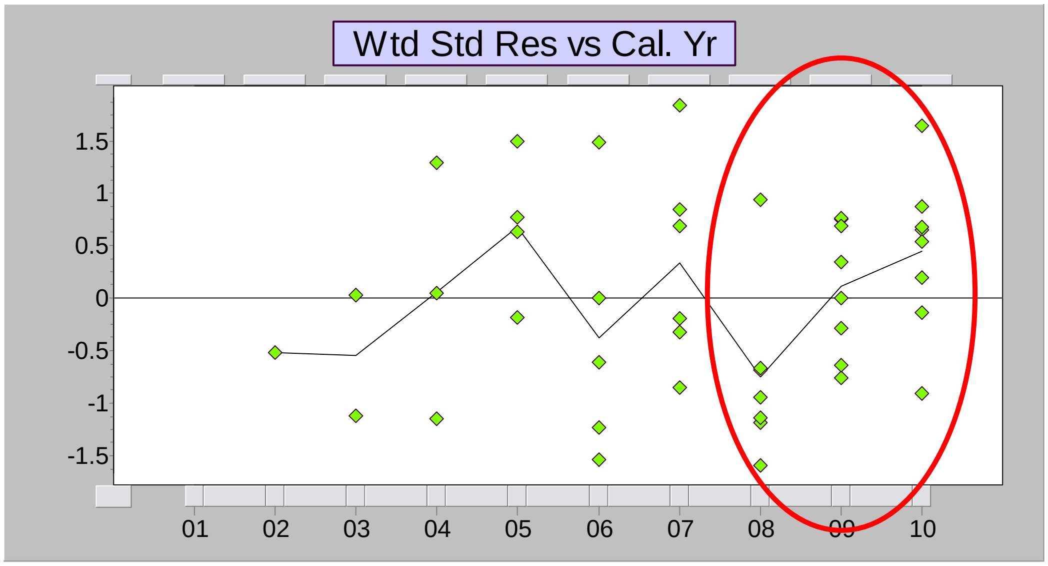
Here are the bootstrap predictive means and s.d.s for the last diagonal (i.e. with that data not used in the estimation) for a quasi-Poisson GLM, and the actual payments for comparison:

DY:	1	2	3	4	5	6	7	8
CL pred	931994	1000686	1115232	482991	325851	443060	231680	309629
mean:	958887	1021227	1114169	490137	328453	452636	242346	327365
stdev:	452285	331706	318026	195225	156289	200080	152170	227644
actual:	986608	1443370	1063269	705960	470639	206286	280405	425046

Firstly, there is an apparent bias in the bootstrap means. The chain ladder predictions sit below the bootstrap means, indicating a bias. Since, the ML for a Poisson is unbiased, if the model is correct, these predictions should be unbiased. This doesn’t *necessarily* indicate a bad predictive model, but is there anything going on?

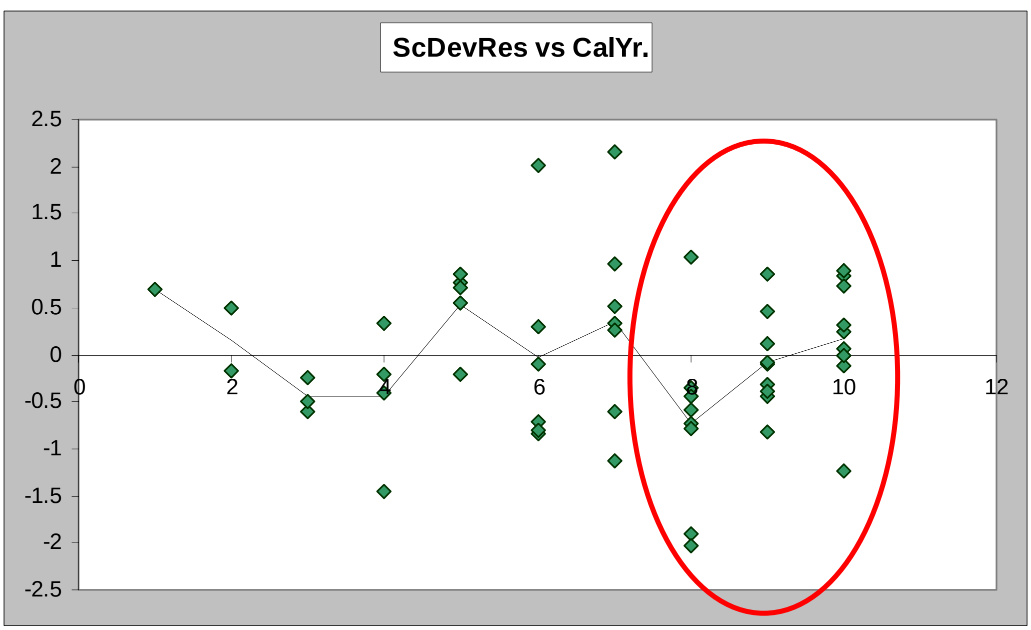
In fact there is, and we can see problem can be seen in residual plots.

Here is a plot of the residuals versus calendar year from a Mack type fit (this is done first because it’s the easiest to obtain – it takes only a couple of clicks in ICRFS-ELRF).



Strong calendar period effects in the last few years. The existence of a calendar period effect was already noted by Taylor and Ashe in 1983 (who included the late calendar year effect in some of their models), but it has been ignored by almost every author to consider this data since. If the trend were to continue for next year, the forecasts may be quite wrong. If we didn't examine the residuals, we may not even be aware this problem is present.

Exactly the same effect appears when fitting a quasi-Poisson two-way cross classification with log-link:



There's little advantage in not examining Mack residuals before fitting a quasi-Poisson GLM – the residuals are easier to produce, and the information in the plot of residuals vs fitted has more information about the predictive ability of the model.

Some other considerations

All chain-ladder reproducing models (including both the quasi-Poisson GLM and the Mack model) must assume that the variance of the losses is proportional to the mean (or they will necessarily fail to reproduce the chain ladder). This assumption is found to be rarely tenable in practice – and for an obvious reason. While it can make sense with claim counts – for example, if the counts are higher on average they also tend to be more spread but often with lower coefficient of variation. If they happen to be Poisson-distributed (a strong assumption), the variance will be specifically proportional to the mean. However, heterogeneity or dependence in claim probabilities can make it untenable even for claim numbers. But with claim payments, the amount paid on each claim is itself a random variable, not a constant, and anything that makes the claim payments variable will make the variation increase faster than the mean. Simple variation in claim size (such as a constant percentage change, whether due to inflation effects or change in mix of business or any number of other effects) will make the variance increase as the square of the mean, while claim size effects that vary from policy to policy can make it increase still faster. Dependence in claim size effects across policies can make it increase faster again. Consequently the chain ladder assumption of variance proportional to mean must be viewed with a great deal of caution, and carefully checked.

The chain ladder model is overparameterized. It assumes, for example, that there is *no* information in nearby development periods about the level of payments in a given development, yet the development generally follows a fairly smooth trend – indicating that there is information there, and that the trend could be described with few parameters. This overparameterization leads to unstable forecasts.

Finally, in respect of the bootstrap, the sample statistic may in some circumstances be very inefficient as an estimator of the corresponding population quantities. It would be prudent to check that it makes sense to use the estimator you have in mind for distributions that would plausibly describe the data.

Conclusions

The use of the bootstrap does not remove the need to check assumptions relating to the appropriateness of the model. Indeed, it is clear that there's a critical need to check the assumptions.

The bootstrap cannot get around the facts that chain-ladder type models have no simple descriptors of features in the data. Note further for triangles ABC and LR-High there is so much remaining structure in the residuals –the bootstrap cannot get around this.

If you do fit a quasi-Poisson GLM, it's important to check the one-step-ahead prediction errors in order to see how it performs as a predictive model – the residuals against fitted values don't show you the problems.

In any case, it should be looked at before bootstrapping a model, and once a bootstrap has been done, you should also validate at least the last year - examine whether the actual values from the last calendar year could plausibly have come from the predictive distribution standing a year earlier.

If it is the predictive behaviour that is of interest, prediction errors are appropriate tools to use in standard diagnostics, and they can be analyzed in the same way as residuals are for models where prediction is within the range of the data.

Checking the model when bootstrapping is achieved in much the same way as it is for any other model – via diagnostics – but they must be diagnostics selected with a clear understanding of the problem, the model and the way in which the bootstrap works..

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Appendix A

Incurred Loss Array for the Mack (RAA AFG) data

Development Year

		0	1	2	3	4	5	6	7	8	9
	1981	5012	8269	10907	11805	13539	16181	18009	18608	18662	18834
	1982	106	4285	5396	10666	13782	15599	15496	16169	16704	
	1983	3410	8992	13873	16141	18735	22214	22863	23466		
Acci-	1984	5655	11555	15766	21266	23425	26083	27067			
dent	1985	1092	9565	15836	22169	25955	26180				
Year	1986	1513	6445	11702	12935	15852					
	1987	557	4020	10946	12314						
	1988	1351	6947	13112							
	1989	3133	5395								
	1990	2063									

Appendix B

ABC - Incremental paid losses triangle and exposures

	0	1	2	3	4	5	6	7	8	9	10
1977	153638	188412	134534	87456	60348	42404	31238	21252	16622	14440	12200
1978	178536	226412	158894	104686	71448	47990	35576	24818	22662	18000	
1979	210172	259168	188388	123074	83380	56086	38496	33768	27400		
1980	211448	253482	183370	131040	78994	60232	45568	38000			
1981	219810	266304	194650	120098	87582	62750	51000				
1982	205654	252746	177506	129522	96786	82400					
1983	197716	255408	194648	142328	105600						
1984	239784	329242	264802	190400							
1985	326304	471744	375400								
1986	420778	590400									
1987	496200										

Accident Year	1977	1978	1979	1980	1981	1982	1983	1984	1985	1986	1987
Exposure	2.2	2.4	2.2	2.0	1.9	1.6	1.6	1.8	2.2	2.5	2.6

Note that the exposures were not used in the chain ladder fits described here. (Since there is already a parameter to each accident year, there is little point in any case.)

Appendix C

LR high data, incremental payments, development years 0-8

	0	1	2	3	4	5	6	7	8
1974	668	4,270	6,530	6,970	8,215	11,428	6,640	1,633	990
1975	775	6,248	7,193	9,250	13,745	14,425	3,408	1,280	608
1976	925	5,935	11,343	15,015	16,215	10,288	3,215	1,613	575
1977	1,443	8,250	14,338	18,375	17,005	7,370	4,203	3,158	458
1978	1,273	10,023	18,873	22,878	14,940	6,058	3,093	965	895
1979	1,575	12,833	26,523	19,333	12,465	8,600	3,368	768	263
1980	2,695	17,470	23,630	21,433	14,290	5,190	2,343	1,655	313
1981	4,115	19,330	21,640	21,545	11,503	5,308	2,278	1,745	343
1982	4,385	23,755	23,420	18,083	8,758	4,928	1,825	415	225
1983	4,993	21,578	25,968	19,998	11,935	6,353	2,793	313	275
1984	5,410	23,435	25,028	19,045	13,183	5,220	1,055	373	
1985	4,805	22,543	26,045	17,828	11,235	5,870	1,823		
1986	4,905	27,728	37,040	26,728	14,753	3,818			
1987	5,823	39,393	50,033	34,635	15,190				
1988	8,358	53,658	68,120	35,373					
1989	9,618	75,810	62,653						
1990	15,225	68,255							
1991	13,628								

LR high data, development years 9-17

	9	10	11	12	13	14	15	16	17
1974	483	148	50	63	0	30	0	0	0
1975	123	553	45	168	0	0	0	80	
1976	328	220	240	18	20	45	0		
1977	263	133	5	0	125	0			
1978	728	283	-137	45	53				
1979	325	148	48	65					
1980	825	50	13						
1981	98	28							
1982	153								

Accident Year	1974	1975	1976	1977	1978	1979	1980	1981	1982
Exposure (000s)	11.00	11.00	11.00	12.00	12.00	12.00	12.00	12.00	11.00

Accident Year	1983	1984	1985	1986	1987	1988	1989	1990	1991
Exposure (000s)	11.00	11.00	11.00	12.00	13.00	14.00	14.00	14.00	13.00

Appendix D

Taylor-Ashe data. Incremental payments

	0	1	2	3	4	5	6	7	8	9
1972	357848	766940	610542	482940	527326	574398	146342	139950	227229	67948
1973	352118	884021	933894	1183289	445745	320996	527804	266172	425046	
1974	290507	1001799	926219	1016654	750816	146923	495992	280405		
1975	310608	1108250	776189	1562400	272482	352053	206286			
1976	443160	693190	991983	769488	504851	470639				
1977	396132	937085	847498	805037	705960					
1978	440832	847631	1131398	1063269						
1979	359480	1061648	1443370							
1980	376686	986608								
1981	344014									

Taylor-Ashe data. Number of claims finalized

	0	1	2	3	4	5	6	7	8	9
1972	**40**	**124**	**157**	**93**	**141**	**22**	**14**	**10**	**3**	**2**
1973	**37**	**186**	**130**	**239**	**61**	**26**	**23**	**6**	**6**	
1974	**35**	**158**	**243**	**153**	**48**	**26**	**14**	**5**		
1975	**41**	**155**	**218**	**100**	**67**	**17**	**6**			
1976	**30**	**187**	**166**	**120**	**55**	**13**				
1977	**33**	**121**	**204**	**87**	**37**					
1978	**32**	**115**	**146**	**103**						
1979	**43**	**111**	**83**							
1980	**17**	**92**								
1981	**22**									

Note that Taylor and Ashe (1983) give the data as payments per claim finalized and number of claims finalized. The original payments have been reconstructed. Note also that the number of claims finalized have not been used in the analysis here, the table is given for the sake of completeness.