DISCUSSION

Daniel E. Smith, National Bureau of Economic Research

Lee Giesecke's paper on the use of the Chi-square statistic to set Whittaker-Henderson smoothing coefficients addresses an important empirical question -- how to estimate the probability of a given event occuring at an interval of time given that only limited data are available for certain intervals. In an actuarial context this problem arises when an actuary must estimate probabilities of mortality, disability and turnover for each age group. Since some age groups may have few members from which to draw such estimates, a process known as smoothing has developed. In the smoothing procedure, the raw estimates of the probabilities of each interval are smoothed to fit some predefined linear function by using information from the surrounding intervals.

The procedure which naturally comes to mind is the fitting of a regression line. The paper mentions that actuaries tend to shy away from the use of regression but it's not clear why this should be the case. While most statisticians use regressions to fit polynomials, one can also estimate logarithmic functions using least squares based techniques.

The paper's chief criticism of regression is the necessity of choosing a correct functional form -- whether or not to include squared and cubed terms, for example. As an alternative it suggests the use of Whittaker - Henderson graduation techniques. The essence of Whittaker-Henderson is to choose a smoothness criteria -- the simplest being the second or third differences -- and then minimizing the squares of these second or third differences.

Unfortunately, it seems that the use of Whittaker-Henderson has the same drawbacks which the paper attributes to regression based techniques. Just as with regression, the need to choose which differences to minimize is equivalent to the choice of a functional form.

A similar technique has been used in econometrics by Robert Shiller in his 1973 Econometrica article. The Shiller lags have found little general application for precisely the reason outlined above. Given the necessity of choosing a functional form with both techniques, there doesn't seem to be any theoretically compelling reason to choose Whittaker-Henderson over the more conventional regression routines.

Finally, the paper notes that Whittaker-Henderson is best for larger samples. This hardly seems to be a compelling rationale for its use since the larger the sample, the less will be the need to smooth in the first place.

On a positive note, the paper does provide a useful comparison of regression and Whittaker-Henderson smoothing techniques and gives the analyst a rough guide to when each technique is most appropriate.

The paper by Emily Andrews and Olivia Mitchell attempts to test for economies of scale in large and medium-size pension plans. Since the methodology used in the paper is familiar and basically sound, my discussion will focus primarily on the shortcomings of the data used in the analysis as well as the implications of these empirical results.

The analysis relies on a data file created from the "5500 forms" which were filed with the Department of Labor pursuant to the reporting requirements of the Employee Retirement Income Security Act (ERISA) of 1974. The forms contained detailed information about employee benefit plans including number of participants, assets, current liabilities and expenses. Unfortunately, there are some serious flaws with these data which -- at the very least -- merit some discussion within the paper.

The first problem with the data relates to the reporting of assets on a consolidated trust basis. Plans which invest their assets in conjunction with other plans in a consolidated trust often report the assets of the entire trust on the ERISA reporting forms rather than breaking out the assets of the individual plan.

In an effort to solve this problem an attempt was made to identify these consolidated trusts by flaging all plans with a large amount of assets per participant on the file. While this technique was effective for identifying trusts which were made up of multiple large plans, it failed to pick up trusts which were composed of large and small plans when only the large plan was in the sample. This is because the addition of the small plan's assets may not be substantial enough to cause the larger plan to hit the "assets per participant" screen.

Just as these consolidated trusts make the reporting of assets dubious, they also raise questions about plan administrative expense numbers. Since plans within a consolidated trust are jointly managed, expenses must be divided among them. Hence, trust managers allocate expenses based on their own perceptions of the economies of scale.

This poses a severe problem for the Andrews and Mitchell analysis. For consolidated trusts, the model is measuring the perceptions of plan administrators about the economies of scale rather than the actual economies of scale.

This highlights a third problem with plan financial data. Very little is known about the expensing process. There is great reason to believe that expensing for small plans is very different than that for larger plans. For example, in a small plan where plan administration consists of a single employee working part-time, the employer often will not expense the employee's salary to the plan. On the other hand, in a large plan where plan administration consists of one or more full-time managers the employer is much more likely to expense the manager's salary. This is consistent with the papers finding of a higher $R^2$ for the multi-employer equation.

This paper could be greatly enhanced by estimating the equations with the separate components of the total expense number -- salaries, commissions, etc.. Since most of the under-expensing relates to salaries, the $R^2$ should be substantially higher when the equations are estimated using non-salary expenses.

Despite these drawbacks, the results presented in the paper are most interesting. It would be a mistake, however, to exaggerate their public policy implications. The paper's findings of significant economies of scale in pension plan administration are not surprising but it would be a mistake to assume that small plans should therefore consolidate. While consolidation may provide benefits for some plans, it may also prove costly and unfeasible for others.

This paper is particularly useful in pointing out the problems inherent in the currently available pension plan data. The results also provide some excellent food for thought for pension plan managers and participants alike.

Sylvester Schieber's paper provides an alternative perspective on the move towards a national retirement income policy. In particular, the paper takes issue with the finding of the President's Commission on Pension Policy (PCPP)

that growth in private pension coverage has stagnated.

In essence, the paper simply presents an alternative macroeconomic forecast without the arbitrary constraints imposed by the PCPP in their estimates. In reality, both forecasts seem to be fundamentally flawed. In both cases, the models seek to forecast future private pension coverage gains based on past growth experience. Given that the greatest potential for coverage growth is in the small service industry sector, this macro approach does not seem to be theoretically justifiable. Small service firms represent a special case and it would be a grave mistake to attempt to forecast their likely coverage experience from the past experience of the aggregate economy.

There are many reasons why the small service sector represents a special case. First, the normal incentive for employers' adoption of pension plans -- the desire to discourage turnover -- is often lacking in service establishments who rely on turnover to keep wages low. Second, this sector is primarily composed of younger workers who aren't particularly interested in accumulating retirement wealth.

The picture is further clouded by the creation of new defined contribution vehicles which make smaller pension plans much more viable. This new uncertainty makes any such macro forecasts very questionable. Clearly, what is warranted here is an analysis which is microeconomic oriented -- one which recognizes that those who are currently not covered represent a peculiar set of circumstances which are very different from the covered sectors of the economy.

One must also wonder about the implications of the Schieber "alternative" forecast. Since it differs from the PCPP forecast by less than six percent it really does not dispute the PCPP claim that vast numbers of the labor force will remain uncovered.