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Article abstract

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ABSTRACT

A number of papers on the underwriting cycle have appeared in the literature in the last 15 years. Despite the reviewing process many of these papers have potentially serious problems. Subsequent writers have cited the earlier work uncritically and sometimes perpetuated the errors in their own work. This article points out four problem areas and discusses some of the effects. One potentially far-reaching problem has been the neglect of taxation issues in the financial models used to motivate analysis. If the financial models are incomplete by ignoring taxes then the premium to surplus ratio becomes an omitted variable and adding such variables related to it in empirical analyses could lead to spurious regression due to the misspecification. Other problems include the use of dangerous statistical methods without proper safeguards, the use of data that are inappropriate for the intended application.

RÉSUMÉ

Un nombre important d'articles sur les cycles de tarification ont été publiés depuis 15 ans. Malgré le processus d'arbitrage des revues spécialisées, ces articles recèlent des problèmes potentiellement importants. De plus, des auteurs ont cité ces articles sans vraiment les critiquer, répétant les mêmes erreurs dans leurs propres travaux. Un problème potentiel, difficile à cerner, est de négliger la dimension fiscale dans les modèles financiers utilisés pour motiver l'analyse. Comme le fait d'ignorer les éléments fiscaux rend les modèles financiers incomplets, le ratio prime sur surplus devient une variable omise et ajouter des variables qui peuvent y être reliées peut causer des régressions présentant des problèmes de spécification. D'autres problèmes sont reliés à l'utilisation de méthodes statistiques dangereuses, ne comportant pas les restrictions appropriées et utilisant des données non appropriées aux applications désirées.

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INTRODUCTION

A number of studies presenting empirical assessments of the underwriting cycle have appeared in the literature in recent years. They all use statistical and econometric methods to study aspects of the so-called "underwriting cycle", the quasi-cyclical pattern of variation of underwriting profits or their equivalent, combined ratios. Despite the fact that almost every article has cited the previous literature, none of these citations offers a critical appraisal, the citation being limited to reciting the purported findings without any assessment of their validity. Yet I believe that for the most part these studies are seriously flawed in various ways, although some common patterns are apparent. The purpose of this article is to bring out and discuss some of these flaws. This is done in the hope that it will lead to an improvement in the quality of future research and we may learn more about the pattern of fluctuations of underwriting profit or income. It should be noted that in some cases the flaws are due to developments in the state of the art. Econometrics has, after all, developed rather rapidly in the last twenty years or so.

The major common flaws that are encountered include the misspecification by ignoring the theoretical basis of the model, misspecification through the use of extensive variables such as total premium and total losses when intensive variables such as unit price or profit margin are required, the use of potentially dangerous econometric tools, the use of inappropriate data, and less than complete reporting. I will deal with each of these problems in turn[1].

MISSPECIFICATION BY IGNORING THE THEORETICAL BASIS

Two issues of misspecification are important. The first deals with failure to incorporate into the models the financial theory underlying the relation of underwriting margin to other economic variables, or using only over-simplified versions of those models. The second deals with using in the empirical models relations that are different from those implied by the underlying theoretical model.

Ignoring Financial Theory

Since the late 70's it has been known (Fairley 1979, Hill 1979) that in financial equilibrium the competitive underwriting profit margin is linear in the product of interest rate times a complex function of the tax rates, the premium to surplus ratio, and the ratio of investable funds generated by insurance to the surplus of the insurer.

The essence of the relation can be derived by considering an investor who must choose between investing in an insurance company or in a mutual fund. If he invests in the corporation he will experience cash flows from the investment of the capital provided to the insurance company, from the investment of the funds temporarily held by the insurance company because of its insurance operations, and from the underwriting of insurance. Using the fact that this opportunity must, in competitive and capital market equilibrium, be just as good as investing in a mutual fund Leng, Powers and Venezian (2001a,b), showed that

$$E\left(u^*(t)\right)=\frac{r^*(t)}{w^*(t)}\left[\frac{\tau_I^*(t)}{1-\tau_U^*(t)}-\lambda^*(t)\frac{1-\tau_I^*(t)}{1-\tau_U^*(t)}\right]+\beta_U^*(t)M \qquad (1)$$

where $r(t)$ is the risk free rate,

$w(t)$ is the ratio of premiums earned by the insurer to its surplus,

$u(t)$ is the underwriting profit margin.

$\tau_I(t)$ is the tax rate on income from invested capital,

$\tau_U(t)$ is the tax rate on income from underwriting,

$\lambda(t)$ is the ratio of investable funds generated by insurance operations to surplus,

β_U is the ratio of the covariance of the underwriting profit margin and the market rate of return to the variance of market rate of return, and

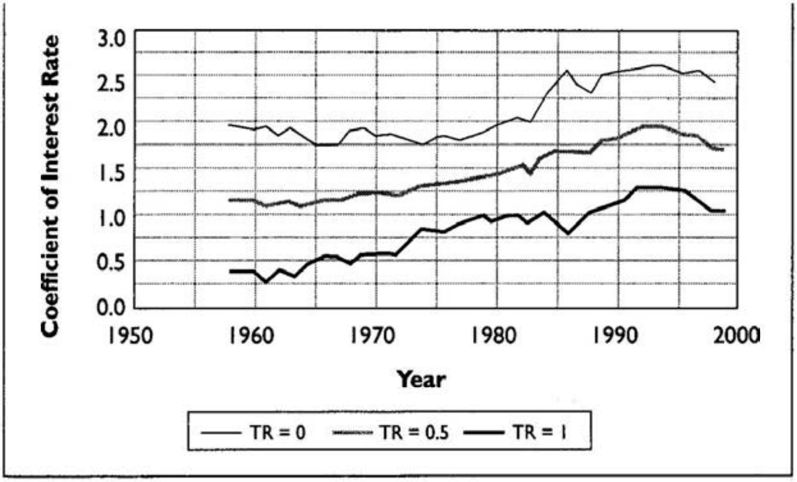
$M(t)$ is the market risk premium.

The asterisk is used to emphasize that these variables apply to estimates of the quantities available at time t as anticipated at the time that the prices for insurance at time t are set. This result is consistent with those derived by Hill and Modigliani (1980), Myers and Cohn (1981), and Kraus and Ross (1982) and reduces to the results of Biger and Kahane (1978), Fairley (1979), Hill (1979) under the proper simplifying assumptions.

Two features of Equation 1 are worth emphasizing. One is that it brings variables that relate to the capital adequacy of the insurer, notably the premium to surplus ratio, into the determination of underwriting profit without appealing to capacity constraints as determinants of profit. This suggests that models which appeal to capacity constraints are misspecified unless they introduce the role of taxation on investment income[2]. The other is that these variables enter as parts of a coefficient of the interest rate, so using the interest rate as a regressor (or as a cointegration variable) is tantamount to assuming that the function that multiplies it is constant over time. If this function varies over time the regression or cointegration will be misspecified in that the variables used in the statistical analysis do not correspond to those suggested by the underlying financial theory. Figure 1 shows the behavior of this function in the United States as inferred from data from the A.M. Best Company, the Federal Reserve Bank of St. Louis, and data on taxes incurred and taxable income of non-financial corporations in the United States[3], and the assumption that the ratio of tax rate on investment income to tax rate on underwriting income is 0, 0.5, and 1. It should be clear that the coefficient has varied appreciably over the period 1959 to 1998.

In view of this variation over time, it appears unwise to neglect this factor. Nonetheless, many articles have neglected it[4]. It is worth mentioning that taking this factor into account does not appear to

FIGURE 1
VARIATION OVER TIME OF THE COEFFICIENT OF INTEREST RATE



affect materially the empirical results when capacity measures are not included, as indicated by the analysis of Leng, Powers, and Venezian (2001a,b).

It may also be worth noting that if all tax rates are zero Equation 1 reduces to:

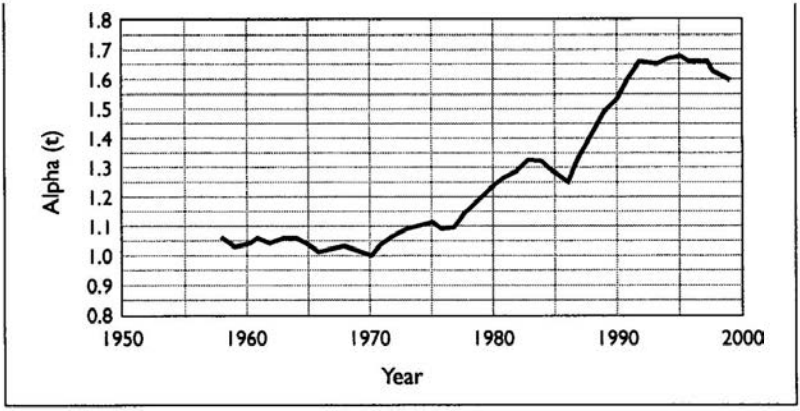
$$E\left(u^*(t)\right) = -r^*(t)\frac{\lambda^*(t)}{w^*(t)} + \beta_U^*(t)M^*(t)$$
$$= -r^*(t)\alpha^*(t) + \beta_U^*(t)M^*(t) \tag{2}$$

where $\alpha(t)$ is the ratio of investable funds provided by the insurance operation to premiums earned.

Even if we ignore taxes, therefore, the coefficient of the interest rate is not necessarily invariant over time. The value of $\alpha(t)$ has not been particularly steady, as shown in Figure 2.

It follows from this that investigations that attempt to use regression or cointegration to relate underwriting income to interest rates are misspecified, at least if data from the United States are used, because they ignore multiplicative variables. The insertion in regression relationships of additive variables closely related to those that enter into the coefficient of the interest rate, such as the ratio of premium to surplus or surplus variables correlated with that ratio, compounds the misspecification. I have no detailed information for data from other countries, but changes in tax rates have been common throughout the world in the last two decades; ignoring the

FIGURE 2
VARIATION OF α(t) OVER TIME



coefficient of interest rates thus appears dangerous unless investigation supports the hypothesis that this is nearly constant and its fluctuations are independent of those in other variables of interest.

Most of the published studies have ignored these theoretical considerations. Some have ignored the potential effects of interest rates entirely[5]. Others have taken into account the interest rate but ignored the variation in the factor that should multiply interest rates if current financial theory is correct[6].

□ Discrepancy between Theoretical and Empirical Models

Cummins and Outreville (1987) presented a model in which underwriting profits are generated through a process involving modifies rational expectations. The theoretical equation resulting from their model was:

$$\Pi_t = \alpha(\varepsilon_t + \varepsilon_{t-1} + \nu_t) + (1-\alpha)(\varepsilon_{t-1} + \varepsilon_{t-2} + \nu_{t-1}) \quad (3)$$

where Π_t is the underwriting profit at time t,
ε_t is the permanent error component,
υ_t is the temporary error component, and
α is a coefficient to account for the overlap between fiscal and rate-making years.

They show that the first two total autocovariances are not zero and use a second order autoregressive process to fit data. Cummins an Outreville (1987) imply that total autocovariances at lags greater than two are zero and this result was proved by Leng and Venezian (2000), who showed, that the process described by Equation 3 implies that covariances at all lags greater than two are identically zero. As almost any book that covers time series analysis points out[7], second order autoregressive processes have partial autocovariances of zero for lags greater than two but total autocovariances that are not zero. It is second order moving average processes that are characterized by total, not partial, autocovariances of zero at lags above two. Thus the empirical use of a second order autoregressive model to draw inferences from the theoretical model is inappropriate and the results of using an AR2 process in the empirical work involves a serious misspecification. Thus the conclusion of Cummins and Outreville (1987) that the rational expectations model implies cycle lengths of between 6 and 8 years is not valid. Notwithstanding these considerations a number of papers have interpreted the presence of second order autoregressive processes as supporting the modified rational expectation hypothesis[8].

MISSPECIFICATION BY USING EXTENSIVE VARIABLES

The term "extensive variable" is used in the physical sciences to refer to variables whose value depends on the size of the system under study. Mass and volume are typical examples of extensive variables, they depend on how much material is under study. In contrast, the term "intensive variable" is used to refer to variables whose value is independent of the size of the system, such as temperature and density, which apply to every unit of material. That terminology is useful also in considering statistics and econometrics. In that context, variables such as aggregate consumption and total deaths from lung cancer are extensive variables whereas per capita consumption and the mortality rate for lung cancer are close to being intensive variables[9].

The problems that arise when we use extensive variables are well known in biometrics and epidemiology. If we study the number of deaths of lung cancer as related to the number of cigarettes consumed in different polities we will, of course, find a strong positive relation. They would be related even if smoking did not contribute to the incidence of cancer. They are certainly related when the study is cross-sectional because population varies across polities and, other things being equal, more population implies both more cigarettes consumed and more cancer deaths. If the study is longitudinal the same effect, covariation of both smoking and deaths with population, will be present, but may be weaker. Ignoring population gives rise to spurious regression arising from misspecification. If we want to find out whether indeed cigarettes cause cancer at a population level we must analyze intensive variables such as cancer mortality and per capita consumption of cigarettes. There is, of course, nothing wrong with using extensive variables if we use a model that explicitly takes into account the size of the system as it goes through time or changes across polities.

The same problems exist in the field of insurance. If we regress net premiums earned on losses incurred, or losses incurred on net premiums earned, the coefficients we measure will be distorted because political units or time intervals with large numbers of risks will tend to have both higher losses incurred and higher premiums earned. The population of risks is an important omitted variable and since it is correlated with both the dependent and the independent variables will create misspecification and biased estimators.

This comment is important because the theoretical models of the cycle that have been proposed view the cycle in terms of the underwriting profit margin or combined ratio. This is very clear in the model of Venezian (1985), since in that article the analysis is based completely on underwriting profit margins. It is somewhat less clear in Cummins and Outreville (1987), where the theoretical model is rather ambiguous about whether the intent is to model profit per unit of risk, profit per dollar of premium, or aggregate profit. The ambiguity should be resolved by the fact that the empirical investigations conducted by the authors is based on variables that are justified on the basis of their strong correlation with underwriting profit margin.

In this context. we are primarily interested in how the price for insurance depends on other factors. The total premiums, however, are the product of this price times the number of units of coverage[10] sold. If we denote the price of coverage per unit by P(t), the losses per unit by L(t), and the number of units provided by N(t), then regressions using extensive variables are attempting to draw conclusions about P(t) and L(t) from regressions of the form:

$$P(t) * N(t) = \sum_{i=1}^{I} \gamma_i P(t-i) N(t-i) + \sum_{j=1}^{J} \eta_j L_j(t-j) N(t-j) \quad (4)$$
$$+ \text{others regressors}$$

without ever introducing any measure of the units of coverage supplied or demanded. Any conclusions that might result will apply to the combination of price and quantity and will say nothing about price by itself. Thus under the best of circumstances the empirical evidence can be interpreted as referring to prices only if we are willing to make the assumption that the number of policies sold is independent of time and circumstances. If interest rates affect the willingness to self-insure, for example, the hypothesis of constant number of policies would be violated; an increase in the number of insured vehicles or home resulting from growth in population or from increasing affluence would have the same result. In contrast, using intensive variables involves drawing inferences on the relationship between these variables from the regression:

$$P(t) = \sum_{i=1}^{I} \gamma_i P(t-i) + \sum_{j=1}^{J} \eta_j L(t-j) + \text{others regress} \quad (5)$$

Of course if the effect of the number of risks is fully modeled, then the inferences from both will be equally valid, but absent such full modeling the inferences from Equation 4 are more likely to be valid than those from Equation 3.

The situation is aggravated by three complicating factors: the definition of lines of insurance is not constant over time, the number of companies on which we have data varies, and the identity of the companies is not always the same. The importance of the first might be illustrated by the creation of homeowners multiple peril as a line in the mid 1950's. This line attracted some existing policyholders that carried fire insurance on their homes and may have carried liability insurance. Thus the creation of the line implies a relative reduction in the premiums for these two lines, among others. Since the effect of diversion of premiums from established lines to newly created lines is not modeled, the creation of new lines will muddy any interpretation of the results for the preexisting lines. For the second factor, the data are usually obtained from sources, such as A.M. Bests, that do not have compulsory reporting, it follows that the number of reporting companies varies from year to year, and that the same companies may enter and exit from the reporting pool during any time interval. If extensive variables are used these changes can easily affect the results. Finally, there is exit from and entry into the business, and companies already in the business may merge, acquire operating units from other companies, or separate previously integrated activities into local subsidiaries. Hence the total premiums and total losses in different years may not even refer to the same population of companies and changes, especially in extensive variables must be interpreted with great care. If the analysis is based on intensive variables such as profit margins these effects disappear provided that the business that migrates to newly introduced lines had profit characteristics not too different from that which remained in the pre-existing lines and that the companies that enter and exit the database are either a small fraction of the total or have comparable characteristics similar to those that remain steadily in the database. If the analysis is based on extensive variables then the effect are very unlikely to be negligible.

Using logarithmic transformations of the variables does not remove the problem. The logarithm of the product of two variables is the sum of the logarithms, so products of price and quantity might be viewed as becoming sums of the logarithm of price and the logarithm of quantity. But the fact is that the relation contains more than one term on the right hand side, so taking logarithms of both sides does not achieve separation. On the other hand, arbitrarily changing the variables to logarithmic form does not conform to the postulated relationship of Equation 3. Even at best, if the population of risks varies across time we would be neglecting a relevant additive variable rather than a multiplicative one, and that has known biasing effects. Taking differences, either of the extensive variables or their logarithms, removes only part of the problem[11], it substitutes population growth or percentage growth for population size or its logarithm. If population growth is correlated with the underlying variables then we have the usual problems that we encounter with omitted variables. But there is good reason to believe that as interest rates rise the percentage of self-insured risks rises. There is also good reason to believe that the number of insurance companies decreases after periods of negative underwriting profits and that it increases following periods of profitable underwriting. It may even be postulated that after times of poor underwriting performance companies may defer reporting to the point that their results are not included in the statistical totals, or may fail to report. Hence there are serious issues that need to be considered in the interpretation of the results.

Several studies rely on inferences drawn from analyses using extensive variables to draw conclusions about the behavior of intensive variables such as prices or profits margins[12]. Given the general nature of these analyses it is not possible to say to what extent price effects and quantity effects affect the empirically determined parameters. Hence they cannot be interpreted as contributing to the resolution of any controversy about price effects unless it can be shown that there are no quantity effects. Yet is all instances the authors go on to discuss the level of support that their empirical results provide for the theoretical models.

■ THE CHOICE OF POTENTIALLY DANGEROUS ECONOMETRIC TOOLS

□ Detrending

In an early study of the underwriting cycle Venezian (1985) noted that the profit margin series exhibited trends and dealt with the issue by "detrending" the series. This procedure involved the fitting of a linear regression against time and using the residuals in the analysis. This procedure has an inherent danger that it may create the appearance of cycles of lengths approximately equal to the length of the data series, though this was not a problem in that study.

A more serious objection to detrending in this manner is that it may remove important sources of variability. For example, it is now known that during the period from which Venezian drew his data interest rates had a secular increase. If the underwriting profit margins had been a function of interest rates, the detrending would have removed much of the effect of interest rates on underwriting profit margins.

Venezian (1985) did not analyze the effect of interest rates, so this is not a major problem in that article, but the same detrending procedure was followed in Fields and Venezian (1989), in which interest rates were used as part of an analysis based on seemingly unrelated regressions. The conclusions of that study with respect to the influence of interest rates are therefore in serious doubt.

□ Cointegration

We regard regression as the method of choice provided the variables involved are stationary. The variables with which we work, notably interest rates, have been noted as not satisfying the statistical criteria for stationarity, they appear to exhibit unit roots. If we knew that the series involved have unit roots, then the method of choice should be cointegration, since regression can, under those conditions, lead to "spurious" results. The results are "spurious" in the sense that the usual statistical criteria for the significance of coefficients do not apply. When the variables have unit roots cointegration places the results in an interpretable statistical context and the results can be interpreted in a fairly straightforward manner.

If the series involved do give the appearance of having unit roots[13] then the choice of econometric tools is important, especially so if we have series with 20 to 100 points. The inherent dangers do not seem to be widely appreciated and need to be discussed.

It is well known that if two variables can be expressed in the form:

$$X(t) = \gamma_X X(t-1) + \varepsilon(t)$$
$$Y(t) = \gamma_Y Y(t-1) + \beta X(t) + \upsilon(t) \qquad (6)$$

then OLS regressions of the form:

$$Y(t) = a + bX(t) + \xi(t) \qquad (7)$$

are inappropriate if $\gamma_X = 1$. The problem is that in this situation the distribution of b around β is not the usual t – distribution and the effect is that we would conclude much too often that the linkage coefficient β is significantly different from zero. This would lead us to conclude meaningful (and possibly causal) relations when indeed there are none. In this particular situation the solution is to use cointegration analysis. This looks for expressions of the form:

$$\xi(t) = Y(t) - bX(t) \quad (8)$$

such that $\xi(t)$ is stationary noise (that is, a stationary variable for with the hypothesis of a unit root can be rejected.

The problem with the use of cointegration can be seen by assuming that $\gamma_X < 1$ and $\gamma_Y < 1$. Under these conditions both $Y(t)$ and $X(t)$ are stationary, so the function $\xi(t)$ is stationary noise for every value of b that we may choose. It follows that for any value of b we would, in the limit of large sample sizes, find that $\xi(t)$ is, indeed, stationary noise. This problem is compounded by the fact that we cannot know whether γ_X and γ_Y are indeed one other than by constructing a fully specified model of the relation. At best we can determine whether some statistical procedure, such as Dickey-Fuller regression, rejects the hypothesis of a unit root. If it does not, however, we should remember that, as Hamilton (1994) points out, the same data would not reject the hypotheses that γ_X and γ_Y are 0.99 or 0.95. With short series such as those usually found in our research the probability that a series with $\gamma_X = 0$ would fail to reject the unit root hypothesis about 20% to 40% of the time, and the rejection rate would be substantially lower if $\gamma_X = 0.5$. Thus the risk of using cointegration in an inappropriate context is high, and the probability of finding that the linear combination is cointegrated is also high. This translates into a high probability of inferring a meaningful relation where there indeed is none.

To guard against inappropriate uses of cointegration we must determine the cointegrating rank of the data. If the cointegrating rank is equal to the number of variables the interpretation is that either the variables do not indeed have a unit root or that the relationships are misspecified. Most papers that use cointegration have paid little attention to these possibilities[14].

An alternative to cointegration would be regression of differenced variables, but as Hamilton (1994) points out, differencing may result in a misspecified regression if γ_X is not exactly one or if the relation is one of cointegration. The danger arises because in these cases differencing causes the error terms to become correlated. Moreover, if differencing is performed, it must be performed in such a way that the theoretical relations are preserved. For example, if the theoretical relations envision an autoregressive process in the current ratio, differencing in logarithmic for disrupts that relation and results in misspecification. The articles that have used differencing have not always taken care of these potential problems[15].

The problems may be even worse than alluded to above. The usual procedures for assessing the likelihood of a unit root and for assessing cointegration are based on the null hypothesis that the process that generates the series (such as Equation 5) is stable, that is, it has time-invariant coefficients. Leng (2000) has shown that there are good indications that combined ratios were not stable during the interval 1973 to 1997. The series seems to have changes from an AR2 process to an AR1 process in 1981. Hence caution would dictate that the analysis be conducted separately for each period of stability.

USE OF APPARENTLY INAPPROPRIATE DATA

A potential problem with data in cointegration or regression analysis is that persistent but large patterns in short term behavior may lead to erroneous conclusions about long term behavior. For example, if births in a city peak in the months in which the storks arrive from their seasonal migration for nesting, the birth rate and the stork population will appear to be correlated or cointegrated if the data are for weeks or months but the relation will not appear if the data are annual. When there are clear indications of the existence of strong seasonal patterns, seasonal adjustments are advisable in cointegration analysis, as indicated by Charemza and Deadman (1997). The alternative is to use reporting periods that eliminate the seasonality. Using quartely data with no seasonal adjustments[16] is inappropriate and may lead toerroneous conclusions.

A second potential problem arises when data are taken from periods in which different regimes seem to generate the results. Sometimes this problem is predictable. The Supreme Court's decision on the South Eastern Underwriters Association case in 1945 and the enactment of the MacCarran Ferguson Act in 1948 could be expected to have affected the way insurance is priced. The creation of new lines of insurance, particularly homeowner's multiple peril and commercial multiple peril, in the late 1950's is likely to have affected underwriting profitability in related lines (such as dwelling and commercial fire) as the underwriting in the new lines developed and selection problems among the lines became settled. The introduction of claims made policies in the late 1970's changed some of the risk characteristics of some insurance coverages and may have affected the overall results. In other cases, regime changes can be seen only after the fact. Recent analyses by Malliaropulos (2000), Leng (2000), and Leng, Powers and Venezian (2001a,b) indicate that the series for interest rates and underwriting margins changed regime in 1981. Yet most analyses have often been conducted as though a single homogeneous period had been under investigation[17]. The articles of Venezian (1985), and Fields and Venezian (1989) escaped these problems primarily by luck.

The worst problem is that, occasionally, the data used are incorrect. Such instances can be prevented by careful consideration of all the issues and, if they are not prevented, are detected only if the authors share information with others. Such errors do exist and there is no routine way in which they can be brought to the attention of scholars generally.

LESS THAN COMPLETE REPORTING

It is generally accepted in modern writing that when the variables being assessed are likely to involve error, some statistical assessment of the magnitude of the error ought to be reported. Yet virtually every article on the subject that has used second order autoregressive techniques reports the cycle lengths as point estimates, that is, the cycle length that would be appropriate if the empirical estimates had zero standard deviation. Given the non-linear relation between the cycle length and the estimated regression parameters the distinction may be important. The one exception is Venezian (1985). That paper used simulation to assess the likelihood that the empirically determined parameters did represent a cyclical pattern, to provide estimates of the cycle length, and to assess whether cycle lengths in different lines were likely to be different or not. Venezian (1985) gives an estimate of the standard deviation of the cycle length for all lines combined of about half a year, and average periods are in some lines are as much as one year longer than the nominal periods, thus citing the nominal cycle length in fractions of a year is important. No article since 1985 has provided any fiducial limits or estimates of the standard deviation of the cycle length.

A similar problem exists in the case of spectral analysis. Smith and Gahin (1981), on the basis of spectral analysis, gave a range of 5.56 to 6.25 for the cycle for all lines combined. Reporting such a range is consistent with the reminder of Box and Jenkins (1976) that periodograms "show an increase in intensity in the immediate vicinity of f." Since 1981, no article on the underwriting cycle that has used spectral analysis has reported a reasonable range for the underlying periods[18].

Theoretically, the number of cointegrating relationships should be smaller than the number of variables included. When the cointegration rank is equal to the number of variables raises the questionarises of whether the relations are misspecified or whether the variables do not, indeed, have a unit root, as discussed by Banerjee, Dolado, Galbraith, and Hendry (1993), Hamilton (1994) and Charemza and Deadman (1997). Thus the finding that the number of cointegrating relations is equal to the number of variables[19] should not be reported without some warning and elaboration.

Selective reporting is another problem of incomplete reporting. Selective reporting cand take many forms. One is ignoring prior literature that provides evidence against the hypothesis proposed and being tested. A second one is focusing on empirical results that do not contradict the hypothesis and emphasizing those results that do not contradict it. In both these cases, it should be remembered that statistical and econometric analysis can, at best, fail to reject a hypothesis. One the other hand, if one test rejects the hypothesis at the one percent level and three others fail to reject it the data have rejected the hypothesis[20].

Yet another form of incomplete reporting arises when the authors neglect or misreport their own results. In the context of research on the underwriting cycle this has taken the form of counting as significant AR2 processes all processes with a significant F-statistic, whether or not the coefficient of the second lag was significant. A related problem has been the failure to take into account the fact that multiple tests have been performed. After all, if we were to perform 20 regressions, we would expect one to be significant at the 5 percent level[21].

DISCUSSION

I have pointed out some of the flaws in the existing literature on the underwriting cycle. Some of the problems cited could be remedied easily. For example, the studies could be redone by including the interest rate in the form required by financial theory rather that by itself. The effect of less than complete reporting would be taken care of if subsequent authors citing those articles would read them critically instead of accepting anything that has been published as gospel. This could also be remedied by a more stringent and consistent editorial process. Others are more difficult to remedy.

The use of extensive variables is not so easy to cure. To begin with, viewing the cycle purely in terms of profit fluctuations may be misguided if there are significant demand effects stemming from the changes in price, as pointed out by Tzeng (1996). Using ratios in the hope that by using losses divided by premiums or profits divided by premiums we have indeed eliminated the problem, is plausible but is not a clear cure. A better solution would be to collect data that include the number and nature of the insured risks, but that seems to be a pipe dream in all lines other than automobile, where the data are available simply because of the exigencies of the residual market[22]. In any event this would not cure the problem unless we had detail about the limits of coverage. Yet another solution would be to develop models in which the number of risks covered is explicitly taken into account. That would mean modeling both the number of insured risks and the limits of coverage as functions of existing economic conditions, a daunting task indeed.

The dual horns of spurious regression and spurious cointegration constitute a dilemma with which we must cope and for which no solution is currently available. Part of the cure is to exercise due care interpreting unit root tests. It is important to remember that the tests do not establish that unit roots are present, they merely fail to reject the hypothesis that unit roots are present. Another part of the cure lies in looking beyond the significance of the eigenvalues of the cointegration analysis and examining the significance of coefficients. This may seem to be an undue burden to place on authors, but the underlying problems are potentially so serious that extra care needs to be taken unless there are valid theoretical reasons for believing that a unit root should be expected. Perhaps bringing this into the open may speed the development of tools to deal with the problem.

My hope is that at the very least this will sensitize us to be more careful both in the data we use, in the analyses we perform, and in how we read the literature.

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Notes

1. This article will not deal with the the theoretical aspects of Lai, Witt, Fung, MacMinn and Brockett (2000). These deserve a more extented treatment.

2. Gron (1994) and Doherty and Garven (1995) present models of this type.

3. The data was obtained from Bureau of Economic Analysis (1985 and 1992) and Slater, C.M. and C.J. Strawser (1999). Insurance data might be obtainable but would be distorting because insurance companies appear to adjust their holdings of taxable and tax preferred bonds in response to anticipated market conditions.

4. Among these are: Fields and Venezian (1989), Haley (1993), Haley (1995), Grace and Hotchkiss (1995), Lamm-Tennant and Weiss (1997), and Fung, Lai, Patterson, Witt (1998).

5. Venezian (1985) and Cummins and Outreville (1987) are examples.

6. See, for example, Fung, Lai, Patterson, and Witt (1998), Grace and Hotchkiss (1997), Haley (1993, 1995), Lai and coworkers (2000), Lamm-Tennant and Weiss (1997), and Niehaus and Terry (1993).

7. See, for example, Gujarati (1995), Box and Jenkins (1976).

8. Niehaus and Terry (1993), Lamm-Tennant and Weiss (1997), and Fung and coworkers (1998) are representative papers.

9. In the behavioral sciences it is not as easy to specify what variables are intensive. Age specific per capita consumption and age specific lung cancer mortality would come closer to the concept of intensive variables. In the physical sciences the same problem exists when we go to the limit of very small systems.

10. This discussion abstracts from reality by eliminating such issues as the depth and quality of coverage. Involving such variables would add to the complexity.

11. And of course it changes the specification of the equation. If the sum of lagged variables is appropriate in the linear equation then it is not appropriate in an equation involving logarithms of the variables or changes in these logarithms.

12. Niehaus and Terry (1993), Lamm-Tennant and Weiss (1997), and Fung, Lai, Patterson, Witt (1998), and Lai, Witt, Fung, MacMinn and Brockett (2000) are among the papers in this category.

13. We never know whether a given series actually has a unit root. I use this phrase to emphasize that all we know is that a unit root test has failed to reject the hypothesis that the series has a unit root. Unfortunately most of the literature uses a less specific terminology that groups together series that actually have a unit root and series for which the hypothesis is not rejected. Much confusion arises from this common practice.

14 Among these articles are Haley (1993), Haley (1995), and Grace and Hotchkiss (1995).

15. Differencing is used by Niehaus and Terry (1993) and by Lamm-Tennant and Weiss (1997).

16. Grace and Hotchkiss (1995) use quartely data in cointegration analysis without seasonal adjustment even though their spectral analisis shows seasonal peaks at one year and six months for both combined ratio and GDP.

17. Niehaus and Terry (1993), Haley (1993) , Lamm-Tennant and Weiss, Grace and Hotchkiss (1995) , and Fung, Lai, Patterson, and Witt (1998) uses the period that include more than one regime.

18. Grace and Hotchkiss (1995) provide enough information for the reader to infer that the peak a "3.93 quarters" is likely to represent a period of between 3.5 and 4 quarters, whereas that at "8.4 years" is likely to represent a period of between 5.5 and 17 years.

19. This is the case in Grace and Hotchkiss (1995).

20. Cummins and Outreville (1987) provide an example of these kinds of incomplete reporting.

21. Chen, Wong, and Lee (1999) is probably the best example in this group.

22. Even in automobile insurance, where information on the number of vehicles insured is usually available, data on the deductibles and limits of coverage are not usually available.