



American Finance Association

Corporate Capital Investment, Accounting Methods and Earnings: A Test of the Control Hypothesis: Discussion

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Source: *The Journal of Finance*, Vol. 35, No. 2, Papers and Proceedings Thirty-Eighth Annual Meeting American Finance Association, Atlanta, Georgia, December 28-30, 1979 (May, 1980), pp. 565-568

Published by: Blackwell Publishing for the American Finance Association

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Finally, it may be possible to integrate the model of ex post investment-earnings relationship with models of investment decisions in which current investment is a function of current and future expected earnings or cash flows.⁹ Such integration would be particularly useful in interpreting the coefficient β_0 of I_t in equation (3).

⁹ See, for example, Eisner and Strotz [1963] and Jorgenson [1963].

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DISCUSSION

JEROLD L. ZIMMERMAN*: Given the very preliminary state of the paper, the nature of my remarks are intended not so much as criticisms of the current paper

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but rather as suggestions that Professor Sunder may find useful if he pursues this line of inquiry. Also, my remarks are limited to the first half of the paper and do not address the control hypothesis tests. I exclude a discussion of the corporate control tests for several reasons: 1) brevity, 2) the hypothesis being tested is not specified very well,¹ and finally, 3) the results are inconclusive. Thus, my advice to professor Sunder regarding the second half of the paper is "to let sleeping dogs lie."

There are three general areas for improving the study that this note addresses: motivating the study, data definitions, and econometrics. The most substantive improvement that Professor Sunder can provide in any future work in this area is an expanded discussion of why the study is being conducted and what the author hopes to learn from it. The author explains the purpose of the current study as an attempt to see if the so-called "conservative bias" in accounting for investments reduces short-run reported earnings. Even assuming that these measurements can be made, what would they tell us? Accounting is full of biases due to the use of historical costs instead of current market values, lower of cost or market rules, etc. Why is the bias in investment-earnings profiles any more or less important than any of the other biases?

There are two other questions that I have concerning the motivation for this study that should be addressed in any future work by the author:

1) Professor Sunder states that the purpose of the paper is to measure the effect of corporate capital investment decisions on firm's earnings. However, the definition of accounting earnings used in the study is net earnings *after* interest and taxes. Therefore, the present paper is examining the effects of both leverage and investment decisions on earnings, not just the effect of investments on earnings. I suggest that interest after taxes be added back to earnings to at least partially control for leverage.²

2) How can the "conservative accounting bias" be assessed without deriving an expectations model of what earnings should have been? The empirical relationship between accounting earnings and capital investments can be estimated. But I do not understand how any inferences can be drawn without explicitly stating an expectations model as a benchmark.

The next set of questions that subsequent revisions of the paper should address involve data definitions.

1) The question of why earnings are measured after and not before interest expense was raised previously.

2) Why was the data not adjusted for changing price levels? By not adjusting for inflation a higher association among the variables will be observed than can be attributable to the relationship measured in real terms.

¹ In particular, the hypothesis assumes that the compensation committee of the board of directors does not adjust the compensation plans to offset any disincentive effect due to the investment. Also, the hypothesis ignores the endogeneity of the type of corporate control and the investment-financing opportunities—see M. Jensen and W. Meckling, "Theory of the Firm: Managerial Behavior, Agency Costs and Ownership Structure," *Journal of Financial Economics* (October, 1976).

² Controlling for leases is a bit more difficult given that it is a joint financing and investment decision.

3) Why were no adjustments made for different depreciation methods, deferred taxes, the investment tax credit (ITC), etc.? For example, the investment tax credit was adopted about midway in Sunder's time period (1962), suspended in 1966, restored in 1967, repealed in 1969, and reinstated in 1971. These and other changes in the tax laws, such as the adoption of accelerated depreciation in 1954 introduces non-stationarities into the earnings and investments time series.

4) The paper does not draw a clear distinction between accounting earnings and cashflows. Investment decisions, presumably are based on net cashflows, not accounting earnings. Yet the paper seems to be treating cashflows and earnings as equivalent. I would suggest that Professor Sunder provide some discussion as to why he is estimating investment-earnings profiles and not investment-net cashflow profiles.

The author should consult Foster's book³ for an expanded discussion of these and other data-type problems.

The final set of recommendations I offer the author involve two econometric issues. The econometrics literature has long recognized the dangers of applying ordinary least squares to models with lagged dependent variables. The basic econometric problem in these models surrounds the structure of the error term. In Sunder's equation (4), the error is $v_t = u_t - \lambda u_{t-1}$. As Professor Sunder recognizes, for v_t to be serially uncorrelated and thus OLS be an appropriate procedure, u_t must follow an auto-regressive process of the form $u_t = \lambda u_{t-1} + \epsilon_t$. (Where ϵ_t is serially independent). A priori, this seems to be a rather restrictive condition for the error term to meet. Furthermore, lagged dependent variable models, even with random disturbances produce biased coefficients in finite samples. With serially correlated errors and lagged dependent variables, OLS estimators are not even consistent.⁴ Besides these econometric problems, the Durbin-Watson test is biased towards two, which is the value of a random disturbance. Thus specification tests of the model, in particular the structure of the disturbance terms, cannot rely on the Durbin-Watson test and should be more fully explored before any inferences are drawn.

Another econometric issue involves the negative coefficient on investment last period which causes Professor Sunder to conclude that "capital investment has a substantial short-run negative effect on earnings." I question the appropriateness of such an inference based on a univariate t -statistic on β_1 . All of the independent variables and in particular, investment this period and last period (I_t and I_{t-1}) are probably highly collinear. Given multicollinearity, it is not uncommon to observe one of the regressors entering the regression with "opposite signs." Thus, the correct statistical test that Sunder should conduct is the joint test that $\beta_0 > 0$ and $\beta_1 > 0$, or a test on the sum of the betas. Professor Sunder might also want to explore the sensitivity of the estimates to alternative model specifications. In particular, what happens to the estimates of the coefficients as

³ George Foster, *Financial Statement Analysis* (Englewood Cliffs: Prentice-Hall, 1978).

⁴ Zellner, A. and M. Geisel, "Analysis of Distributed Lag Models with Applications to Consumption Function Estimation," European Meeting of Econometric Society, 1968, suggests a search procedure to estimate the generalized least squares regression for the type of lag structure in Sunder's model.

the lag-structure is changed by adding (or deleting) lagged investments? Does the sign of β_1 change if I_3 is added as an unconstrained regressor in the model?

As mentioned in the paper's conclusion, Professor Sunder plans to conduct specification tests. I would encourage this and also suggest conducting joint tests on the coefficients as well as sensitivity tests. However, I suspect that multicollinearity among the independent variables will make it very difficult to establish the precise influence of any one regressor on contemporaneous earnings as Sunder is attempting.

Hopefully, Professor Sunder will find these comments of some help in his future work in this area.