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A selective critical review of financial accounting research☆

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ABSTRACT

This essay provides a selective critical review of the financial accounting literature focusing primarily on accounting valuation including implied costs of equity capital, empirical accounting proxies, and frictions in accounting theory. In the opinion of this author, accounting research in these areas is often too complacent, suffering from a lack of critical reasoning. Complacency distorts research innovation and hinders the long-run sustainability of accounting academe in the area of financial accounting. The examples discussed in this essay include (but are not limited to) the issue of structural modeling and model falsifiability; determining whether a firm is over or underpriced based on valuation models that do not allow for such phenomena; arbitrarily "merging" two disparate models one for valuation and one for the discount rate; failing to appreciate the empirical limitations induced by risk-neutral valuation models in estimating costs of capital; employing the same proxies over and over again that ostensibly have no underlying theoretical bases; estimating regressions that necessarily yield biased coefficients when the econometrics literature provides ready solutions; and generating complex models absent the frictions that are essential to the issue being researched.

1. Introduction

This essay provides a selective critical review of the financial accounting literature focusing primarily but not exclusively on empirical archival research. Selectivity is necessary given the many topics that comprise financial accounting research. Criticality is also called for because the field in my view is overly complacent regarding its scientific methodology, its many dubious proxy constructs and the rather cavalier attitude that financial accounting empiricists (and sometimes even theorists) take to financial accounting theory. Lest I be accused of excessive hubris, let me state at the outset that my own work is not immune from the criticisms raised in this essay.

I will focus my review primarily on three financial accounting research topics: accounting valuation including implied costs of equity capital, empirical accounting proxies, and frictions in accounting theory. I concentrate on these topics because they are central to accounting research and because they are related to my own research interests. The penultimate section discusses more general issues.

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In what follows, Section 2 reviews the accounting valuation and implied cost of capital, literature. Section 3 comments on proxies in financial accounting research. Section 4 focuses on accounting theory and un-modeled frictions. Section 5 discusses the relation between financial accounting research and the refereeing process. Section 6 briefly concludes.

2. Accounting valuation and implied costs of capital

North American accounting regulators, such as the FASB, have traditionally focused on providing investors with information relevant for making informed investment decisions and allocating capital efficiently. For better or worse, their view of the role of accounting numbers has had a major impact on North American financial accounting research, especially on the centrality of valuation and the cost of capital.[1]

2.1. Accounting valuation or accounting allocation

Almost all accounting valuation models to date are based on some variant of Ohlson (1995a,b) and Feltham and Ohlson (1995, 1996, 1999).[2] By accounting valuation models, I mean models that value the firm's equity based on accounting numbers such as earnings (however defined), book value of equity, the book to market ratio, etc. As far as I can tell, the large empirical literature surrounding these models, especially Ohlson (1995a,b) and the related Residual Income Model (RIM), is motivated primarily by the relative simplicity with which these models can be estimated. In fact, simplicity of execution seems to be a strong motivator for much of the work done in empirical financial accounting research. Simplicity of execution may be a desideratum but surely not at the expense of a fundamental understanding of the research issues involved.

In my experience, financial accounting empiricists often fail to understand the underlying nature of Ohlsonian-type models.[3] First, Ohlsonian models are really accounting allocation models rather than accounting valuation models per se. In these allocation models, the value of the firm is derived from its dividend dynamic based on non-arbitrage arguments. In other words, the value of the firm is known ex ante, given expected dividends, and accounting plays no role in this valuation process. Subsequently, accounting is added (essentially artificially) through the clean surplus (or a similar) relation, with accounting numbers being substituted for dividends.[4] Because the value of the firm is already known (given non-arbitrage), all that one accomplishes here is allocating the known value of the firm to accounting numbers. In the original Ohlson (1995a,b) model, disregarding "other value relevant information", the value of the firm is allocated to book value (with a weight of one) and to abnormal earnings with a weight of $\omega/(R_F - \omega)$ where ω is the abnormal earnings persistence parameter and R_F is (one plus) the risk-free rate. By contrast, in a Feltham and Ohlson (1995) model, the firm's known value is allocated to book value (or net financial assets) with a weight of one, to abnormal operating earnings with a weight of $\omega_{11}/(R_F - \omega_{11})$ and to operating assets with a weight of $\omega_{12}R_F/(R_F - \omega_{11})(R_F - \omega_{22})$ where ω_{11} is the abnormal operating earnings persistence parameter, ω_{12} is the parameter linking operating assets to abnormal operating earnings and ω_{22} is the operating assets persistence parameter. The difference between Ohlson (1995a,b) and Feltham and Ohlson (1995) is that the former assumes neutral accounting, that is, accounting that is neither conservative nor aggressive, while the latter assumes that accounting is conservative (and the firm is growing). Firm value in the Feltham and Ohlson (1995, 1996) model includes operating assets as an additional valuation factor to account for the assumption that conservative accounting earnings understate the firm's future growth. In other words, the allocation of firm value to specific accounting variables depends on knowing, ex ante, the underlying accounting of the firm. In the previous example, the more conservative the firm, given growth, the more of the firm's known value that is *allocated* to operating assets relative to operating earnings. The valuation problem is exacerbated by the undefined generic "other value relevant information" because these variables too will have the known value of the firm partially allocated to them as well. But, how are we to know, ex ante, which other variables are value relevant—the model does not specify them—and what if the variables that are value relevant differ among firms or across industries?[5]

One could argue that because Ohlsonian models allocate the known value of the firm to accounting variables, these models cannot provide meaningful insights into valuation at all. Although I do not subscribe to this view, it does imply correctly that Ohlsonian models cannot be used to determine which firms are over or undervalued nor estimate intrinsic

[1] The contracting role of accounting numbers has taken on more importance in financial accounting research in recent years thanks to the Chicago/Rochester school of thought. As a result of the burgeoning governance literature, the (non-contractual) stewardship role of accounting numbers has also taken on a more prominent role. Normative accounting issues continue to be severely under-researched in North America.

[2] Accounting valuation models based on Vuolteenaho (2002) are reviewed elsewhere (see Callen, 2009).

[3] Ohlsonian models and their estimation have been criticized before in the literature. See, for example, Lo and Lys (2000), Ryan (2000), and Beaver (2002) among others. By and large, the arguments expressed in this paper differ from these earlier papers, mainly in terms of the focus on conceptual issues.

[4] The artificiality arises because there is no demand for accounting in these models. Dividends do the trick just as well. Numerous papers have added accounting and other variables (beyond earnings and book value) to Ohlsonian models in arbitrary fashion to proxy for "other value relevant information".

[5] The rationale for including "other value relevant information" variables in the model is that we know that firm value is typically a function of variables other than just book values of equity or earnings. The weakness of this approach is that these variables are not specified a priori by the model and, hence, potentially make the model non-falsifiable. After all, if the model predicts poorly it could be because the wrong empirical proxy was chosen to represent "other value relevant information".

values based on accounting numbers *different* from market prices.[6] If the accounting numbers yield a value other than market value, it just means that the known value of the firm has not been allocated "correctly" to the accounting numbers. The attempt by many to use Ohlsonian models to measure intrinsic value or equivalently, under or over valuation of the firm relative to market values is conceptually flawed.[7]

I do believe that one can in theory integrate backwards from accounting variables to firm value, at least for linear Ohlsonian models, but the integration process requires (i) knowing ex ante the underlying accounting policies of the firm, such as the extent of the firm's accounting conservatism and (ii) knowing which variables comprise "other value relevant information" or replacing the latter by expectational variables.[8] However, the fact that the conservatism policies are essentially unobservable to outsiders raises substantive empirical difficulties in using Ohlsonian models for valuation purposes at the firm level. For example, how are we to know the extent to which operating assets are value relevant for a given firm unless we know the firm's conservatism policy and its degree of conservatism ex ante?[9]

The latter discussion abstracts from the fact that most Ohlsonian models are based on the assumption of risk-neutrality. Since the world is decidedly not risk neutral, it is hard to know what to take away from the empirical estimation of such models and what to make of their relative popularity. In an insightful paper, Feltham and Ohlson (1999) extend the basic Residual Income Model (RIM) valuation model to include risk so that the value of the firm equals its book value, weighted abnormal earnings (as in risk-neutral models) and a sum of covariance risk-adjustment terms. Few empirical accounting papers have attempted to deal with risk in the context of model estimation. One exception is Nekrasov and Shroff (2009) who estimate the Feltham and Ohlson (1999) extended RIM model. However, while they account for risk, they do not account for the empirical fact that risk is time-varying. Recently, Lyle et al. (forthcoming) incorporate an extended system of dynamics, including risk dynamics, in the Feltham and Ohlson (1999) RIM model, similar conceptually to Ohlson's (1995a,b) extension of the standard RIM model. In addition to yielding a closed-form linear solution that is amenable to empirical estimation, their dynamic risk structure and empirical results are consistent with the extensive empirical evidence in the accounting and finance literatures that costs of capital (expected returns) are time varying.[10] Equally importantly, their *model-derived* cost of capital is *solely* a function of observable accounting and other fundamental firm characteristics that are well-defined (such as size and the book to market ratio) rather than unobservable covariances and unknowable "value relevant" information. Still, their model is preliminary and does not as yet address many important accounting issues such as conservatism for example.

2.2. Accounting valuation and structural modeling

With a few notable exceptions, empirical financial accounting research is not based on structural models despite the fact that accounting procedures force a linear structure on accounting numbers that are interrelated by construction.[11] The Clean Surplus identity is a case in point. However, even when accounting empiricists invoke a structural model to motivate testable hypotheses, almost inevitably they will abandon that structure to run a rather "carefree" regression that incorporates the model-determined variables of interest but not the structure of the model.

The original Ohlson (1995a,b) model is a case in point. The Ohlson model is comprised of two structurally interrelated equations, an abnormal earnings dynamic, and a price dynamic. Crucially, the structure of the abnormal earnings dynamic (partially) determines the structure of the price dynamic. Thus, in the standard Ohlson model, abnormal earnings at time t are a linear autoregressive function of abnormal earnings in $t-1$. As a consequence of this latter dynamic, price is a function of contemporaneous earnings only (in addition to book value). Now, suppose we change the structure of the abnormal earnings dynamic. Specifically, suppose that the structure of the dynamic is such that abnormal earnings at time t are a linear function of abnormal earnings at time $t-1$ and abnormal earnings at time $t-2$. It is then straightforward to show that price will be a linear function of abnormal earnings at time t and abnormal earnings at $t-1$.[12] This simple point is not always appreciated by the empirical literature. For example, in an influential empirical study, Dechow et al. (1999) assume an abnormal earnings dynamic involving five variables in addition to abnormal earnings, yet they assume that the pricing equation remains unchanged from the conventional model.[13] Similarly, the Lo and Lys (2000) critique of Bar-Yosef et al.'s (1996) and Morel's (1999) direct testing of the Ohlson dynamics misses the point that the dynamics and the pricing relation are not independent of each other. In particular, the multivariate lag structure of the dynamics determines the variables of interest in the pricing equation and their lag structure. Relatedly, efficient estimation of the Ohlson model really demands

[6] This view implies that Ohlsonian accounting valuation models are not useful for publicly traded firms with known market values but are potentially useful for the valuation of non-traded firms provided the issues raised in the next paragraph are solvable.

[7] See Lee et al. (1999) for one prominent example that uses the RIM to estimate intrinsic value.

[8] Liu and Ohlson (2000) insightfully finesse the "other value relevant information" variables by replacing them with expectations of future values, raising in the process other thorny empirical issues (Myers, 2000; Callen and Segal, 2005).

[9] In theory, a long enough time series at the firm level for each of the model variables could solve this conundrum providing of course we are willing to abstract from potential structural change. In any case, such time series are rarely available.

[10] See Callen and Segal (2004) and Callen et al. (2005, 2006) in the accounting literature.

[11] This raises serious econometric issues as well (Christodoulou and McLeay, forthcoming).

[12] See Callen and Morel (2001). This result generalizes. If the structure of the dynamic is such that abnormal earnings at time t are a linear function of abnormal earnings at time $t-1$, $t-2$,..., $t-n$ then price will be a linear function of abnormal earnings from time t to time $t-n-1$.

[13] This issue was noted by Myers (1999) and Morel (2003).

that both the pricing equation and the earnings dynamic should be estimated simultaneously. Yet, I am only aware of one paper that does so (Morel, 2003).

I believe the paucity of structural modeling in empirical accounting research is to some extent self-serving. Accounting researchers tend to emphasize the importance of incentives but rarely as it relates to their own research. Rejecting the null hypothesis based on a regression lacking structure is a far more likely scenario than rejecting the null in a regression based on the model's structure. In the former, only the signs of the coefficients tend to matter whereas in the latter magnitudes matter as well. Again, Ohlsonian models are illustrative. It is fairly conventional to estimate a pricing dynamic that yields a positive coefficient on book value, thereby rejecting the null that book value is not valuation relevant. It is far more challenging to come up with a coefficient on book value insignificantly different from *one* as demanded by the structure of the model.[14] Indeed, the evidence strongly suggests that the coefficient on book value is three and significantly different from *one* (Callen and Segal, 2005).

Model falsifiability is an important issue. The philosophy of science has long established that a model, formal or informal must be falsifiable. Yet, almost every proposed model in the accounting literature whether formal or based on informal intuition, fails to be rejected. Almost every robustness check in the literature appears to validate the main results. I can only conclude from this that accounting scholars are uniformly more prescient than most scientists. Further, even when the model is evidently rejected by the data, there is apparently still a possible way out. For example, one could argue that Callen and Segal (2005) did not reject Feltham and Ohlson (1995) by finding a significant book value coefficient of three (instead of one) because the higher book value coefficient may simply reflect unobservable off-balance sheet financing. But if one cannot falsify the model, why are we so intent on empirical testing? After all, the model (formal or intuitive) is always right under some circumstance. The answer of course is that the data do reject Feltham and Ohlson (1995) because they reject a book value coefficient of one. If off-balance sheet assets matter to valuation, one has to develop a new model which allows for off-balance sheet financing and see what the new model predicts about the book value coefficient. But, as for Feltham and Ohlson (1995) model, it is rejected by the data.

2.3. Accounting valuation and costs of capital

Accounting cost of capital research is potentially important both from a practical point of view and in terms of motivating accounting research. For example, at the practical level, costs of capital can be used to value investments and provide a benchmark for evaluating CEO performance. At the research level, a substantial amount of research on accounting disclosure policy is motivated by the assumption that disclosure reduces firms' costs of capital.

In accounting research, costs of capital are often implied; that is, computed as the internal rate of return relating current known price to estimated future cash flows where cash flows are evaluated by some (typically Ohlsonian) model. Most empirical studies assume that the resulting internal rate of return number measures the firm's cost of capital. However, if cash flows in the numerator are risk-adjusted properly, the resulting implied cost of capital will then necessarily be the risk-free rate. But then what is the point of such an exercise? If the cash flows are not risk-adjusted then only under very restrictive assumptions will the resulting estimate approximate the cost of capital, as noted long ago by Samuelson (1965) and later by Ohlson (1990).

Irrespective of whether the valuation model accounts for risk properly, it is common for empirical accounting valuation studies to use Ohlsonian type models to value the firm's cash flows and a CAPM-type model to empirically determine the relevant cost of capital. This dichotomy appears to be driven by the conundrum that if an Ohlsonian model is being used to value the firm's cash flows, one cannot then turn around and reverse engineer an estimate of the firm's cost of capital from the same model. Alternatively, if one reverse engineers the model to derive an implied cost of capital estimate, one cannot then turn around and use that model to value the firm. Yet, in order to properly use Ohlsonian models for valuation purposes some estimate of the cost of capital estimate is de rigueur—for example, to compute abnormal earnings. The common approach whereby two models are "merged" so that one model is used for valuation and another for estimating costs of capital, is very problematic for two reasons. First, a firm's value and its cost of capital are jointly determined as made explicit by the implied cost of capital literature. After all, that literature assumes that price encapsulates both future cash flows (earnings) and the discount rate. Estimating firm value from one model and cost of capital from another model cavalierly disregards this simultaneity. Second, and even more crucially, estimating price from an Ohlsonian type model and the cost of capital from a CAPM-type model presupposes that the two models are essentially equivalent, which they are not. Neither model necessarily implies the other.[15]

There are two potential solutions to this conundrum. First, it is possible to estimate both value and costs of capital simultaneously at the firm level using a nonlinear structural model as in Morel (2003). Alternatively, as in Lyle et al. (forthcoming), one can substitute both stochastic discount factor dynamics and abnormal earnings dynamics into the

[14] Ryan (2000) makes a similar point in his criticism of Ahmed et al. (2000). Yet, in a contradictory stance, Ryan (2000) also argues against adherence to structural models. He says: "I think this strict adherence to the structure of the models is at best an inefficient way to exploit the models' insights and often likely to miss these insights altogether. In my view, a more useful approach is to take the propositions and equations in Ohlsonesque models as a motivation and starting point rather than a recipe for empirical analysis." The problem with the latter "loose" approach in my opinion is that almost any result is then potentially justifiable and almost nothing is falsifiable.

[15] See Feltham and Ohlson (1999) and Morel (2003) for further discussion of this issue.

Feltham and Ohlson (1999) RIM framework. In this model, the dynamics of the valuation equation explicitly drive the dynamics of the return equation and, hence, the dynamics of the cost of capital (expected return) equation. Both firm value and cost of capital can then be estimated from the same model.

2.4. Validating implied costs of capital

It is difficult to say what makes for a good cost of capital model given that the firm's true cost of capital is unobservable.[16] The early accounting literature tried to validate their cost of capital estimates by correlating them with variables presumed to be related to the firm's cost of capital (e.g., Botosan, 1997; Botosan and Plumlee, 2002). This argument is circular and has little to recommend itself (Easton, 2009).

Generally speaking, some implied costs of capital models can be rejected out of hand. The PEG model (Easton, 2004) is an obvious example because the model is rejected whenever second period analyst forecasts are less than first period analyst forecasts or whenever analysts forecast consecutive losses. These situations occur typically about 25% of the time. A model that discards 25% of the observations *mechanically* cannot possibly be meaningful.[17] It is therefore with some discomfort that I note the recent claim by Botosan et al. (2011) that the PEG model is one of the two best implied costs of capital models based on a horse race of a number of implied costs of capital models.[18]

The asset pricing finance literature measures the efficacy of cost of capital (expected return) models by reference to realized returns. In other words, the cost of capital model that is more highly correlated with or better at predicting next period's realized returns (either at the firm or portfolio levels) whether in sample or out of sample is the superior model. One criticism of this approach is that realized returns are not a good measure of expected returns in that realized returns incorporate both cash flow news effects and expected return news effects (Elton, 1999; Callen and Segal, 2004; Botosan et al., 2011). Thus, it has been suggested that one should adjust for cash flow news and discount rate news effects and then see which cost of capital model best predicts realized returns adjusted for news effects (Easton and Monahan, 2005; Ogneva, 2012; Larocque, 2009). The problem here is that news effects, by definition, are mean zero and unpredictable ex ante. Moreover, ex post estimates of cash flow news and discount rate news are not model-free. Indeed, adjusting realized returns for ex post estimated news items simply yields another modeled estimate of the cost of capital. Thus, the criterion by which news-adjusted return estimates can be used to judge other cost of capital models is unclear.

Despite recent claims that realized returns adjusted (ex post) for news items yield superior results to realized returns (Botosan et al., 2011; Ogneva, 2012), as determined by asset pricing tests, considerable doubt lays over the value of such a finding. For example, using Vector Autogressive (VAR) estimation, Larocque (2013) finds a significant relation between realized returns and implied costs of capital after controlling for both expected return news and cash flow news.[19] Nevertheless, both Larocque (2009, 2013) and Ogneva (2012) obtain expected return estimates, computed as realized returns adjusted for cash flow news and expected return news, that are often less than the risk-free (and oftentimes even negative) which contradicts any meaningful asset pricing equilibrium.[20]

2.5. Time varying costs of capital

As mentioned earlier, there is an enormous literature in finance and accounting showing that costs of capital are time varying. This means that static estimates of the cost capital, such as implied costs of capital, are necessarily biased. Given the firm's current stock price and a valuation model, the implied cost of capital is an *averaged* yield, a melange, that equates estimated future cash flows with the current stock price.[21] The notion is identical to that of the yield to maturity of a bond. But, the yield to maturity is and does not generally reflect any expected future return unless the term structure is flat. To see why, suppose the firm has expected future cash flows (dividends) of 800 next year and 1000 the year after. No more cash flows are expected thereafter. Assume next period's expected cost of equity is 2.5% and 5% the year after. The firm's current stock price is $1709.64.[22] The implied cost of capital is 3.375% both in year one and year 2 by assumption. In other words, in year one the implied cost of capital significantly overstates the true cost of capital by 35% and in year two understates the true cost of capital by 48%. In this example, not only would static implied cost of capital severely distort decisions regarding whether to undertake future real investments but it would be distortive for many other calculations such as benchmarking executive compensation or valuing pensions.

One method suggested for estimating time varying costs of capital is to employ traded assets such as futures to infer future cash flow risks (Ang and Liu, 2004). Unfortunately, futures do not ordinarily trade on individual firms or on the firm's

[16] Despite being unobservable, there is obviously practical value in seeking to best estimate the firm's cost of capital if only to make rational investment decisions.

[17] Note the problem here is not lack of data but the data itself.

[18] The target-price based implied cost of capital is also somewhat problematic insofar as it uses Value Line target forecasts and Value Line only covers larger firms.

[19] See Callen (2009) for a survey of the VAR valuation approach.

[20] Ogneva (2012) adjusts for cash flow news only.

[21] In general, the implied cost of capital is not the average yield.

[22] $1709.64 = 800/(1.025) + 1000/(1.025)(1.05) = 800/(1.03375) + 1000/(1.03375)^2$. This example is obviously somewhat dramatic.

dividend streams. Callen and Lyle (2010) use forward looking put and call options to create synthetic futures which are then used, in turn, to estimate the term structure of implied costs of capital (and risk premia) at the firm level. Interestingly, they find that term structure of implied costs of capital for most years and industries is upward sloping and concave—like the concavity of the term structure of interest rates—with the exception of the 2008–2009 crisis years for which (some of) the term structure is downward sloping and even convex. These estimates are not a complete panacea because options are typically traded only on large firms and the maximum option term is realistically no more than two years. Still, being able to estimate firm-level costs of capital robustly for large firms up to two years forward should be of benefit to capital market professionals, regulators, and researchers.[23]

3. Proxies, proxies everywhere

Another important issue is the willingness of financial accounting empiricists to accept certain proxy measures as the "truth" without subjecting these proxies to rigorous theoretical validation. Instead, hand-waving "intuition" seems to rule the roost. Intuition, as scientists and mathematicians know well, is often misleading. The utilization of the Jones or the Dechow–Dichev metrics of discretionary accruals by the literature as proxies for earnings management/quality is a well-known case in point. These proxies have been around awhile and yet, to the best of my knowledge, they have not been derived from any modeling process despite the fact there are no end of earnings management/quality models out there.[24] I believe that our willingness to accept these measures without rigorous theoretical validation is driven by complacency. After all, these proxies of earnings management are easy to compute, easy to apply and we know their properties. We are comfortable with them and so we use them without subjecting them to rigorous checks. Other cases follow.

3.1. Conditional conservatism

There is a profound gap between models of conditional accounting conservatism and the proxies for conditional conservatism used by the empirical literature. Not one of the proxies employed by the empirical literature to date obtains formally from underlying primitives. By that I mean that there is no rigorous development linking the definition of conditional conservatism to the (even approximate) form of the proxy. In fact, none of the proxies seem to be based on a formal rigorous definition of conditional conservatism at all. The ubiquitous Basu (1997) measure (and its many variations) is a case in point. Basu (1997) defines conditional conservatism informally as "capturing accountants' tendency to require a higher degree of verification for recognizing good news than bad news in financial statements. Under my interpretation of conservatism, earnings reflect bad news more quickly than good news." But, Basu (1997) never rigorously defines what he means by "verification" or "quickly" nor does he rigorously develop metrics of "a higher degree of verification" and "more quickly". Crucially, Basu (1997) fails to show formally how his definition of conditional conservatism leads to the regression "model" that he uses to estimate conditional conservatism.[25] Furthermore, he leaves unclear the measure by which one firm is more or less conservative than another. In other words, how should one measure the degree of conservatism in the Basu "model" given his definition of conditional conservatism? On intuitive grounds, Basu (1997) measures the degree of conservatism by the relative regression coefficients on the return variables which, as shown by Callen and Segal (forthcoming), is not generally correct.

The Basu measure is not the most problematic of the conditional conservatism proxies because at least Basu tries to define what he means by conditional conservatism, albeit informally. Other commonly used proxies for conditional conservatism such as non-operating accruals or skewness seem to be based on little more than wishful thinking and ease of computation. To date the only published proxy for conditional conservatism that appears to have some theoretical underpinnings is the CR metric developed by Callen et al. (2010b). Nevertheless, the nonlinear relation between earnings news, discount rate news and unexpected returns that forms the basis for their metric was not developed formally by them nor do they rigorously derive a measure of the degree of conservatism from underlying primitives.[26]

In recent years there has been much criticism of the Basu (1997) measure on empirical grounds (Dietrich et al., 2007; Givoly et al., 2007; Patatoukas and Thomas, 2011). Nevertheless, because of its computational simplicity, it would appear, the Basu measure (and its variants) is still the conservatism measure of choice. Arguably, the most devastating empirical criticism of the Basu measure is the one by Dietrich et al. (2007) because their criticism implies that the Basu regression necessarily yields biased coefficients. In other words, even if the Basu measure would have theoretical underpinnings, the current empirical estimates of the Basu regression are unusable. Their criticism it should be noted applies equally to the

[23] Ubiquitous static implied costs of capital estimates based on analyst forecasts suffer from similar limitations. They too are based on at most two-year forward analyst forecasts and firms with analyst followings are large.

[24] This issue is distinct from empirical criticisms of these proxies as expressed by Stubben (2010) and the references therein.

[25] Pope and Walker (1999) make a valiant effort to model the Basu regression. Unfortunately, price in their model is determined without reference to recognized earnings or to the impact of conservatism on the time series properties of recognized earnings so that the relation that they derive between recognized earnings and returns is ad hoc. Also, they do not model the degree of conservatism.

[26] For such a development and its empirical implementation, see Callen and Segal (forthcoming). Unlike the simplicity of the Basu regression, the empirical implementation of their model requires fairly sophisticated econometrics. It is not difficult to prophesy which approach will dominate if complacency is pervasive.

Basu-like conservatism measures of Qiang (2007) and Khan and Watts (2009). In a nutshell, Dietrich et al. (2007) point out inter alia that to the extent that returns are endogenous, any regression which conditions returns on their being positive or negative necessarily induces a sample selectivity bias. This is because whether returns are positive or negative is not exogenous but a function of firm characteristics. Dietrich et al. (2007) do not suggest a solution to this issue. However, problems of this type have been analyzed extensively in the econometrics literature by Heckman and Maddala (1983, 1986), among others. One potential solution is to estimate Basu-type regressions using a switching regression methodology as is done by Callen et al. (2010b) and Callen and Segal (forthcoming) for their piece-wise measures of conservatism. In the context of Basu (1997), this methodology would have one simultaneously estimate three regression equations: the Basu equation for positive returns, the Basu regression for negative returns and a selectivity regression that characterizes the relation between conservatism and firm characteristics.[27]

To understand the importance of potential selectivity issues as they affect the Basu regression, consider the paper by Jayaraman and Shivakumar (2013). They hypothesize that the passage of U.S. state anti-takeover laws resulted in an increase in conservatism—as measured by a Basu metric—for firms with higher debt-based contracting pressures. Debt-based contracting pressure is measured by (changes in) leverage. They then proceed to show that the estimated coefficient on negative returns is positive after the enactment of state anti-takeover laws especially for firms with debt-based contracting pressures, consistent with increased conservatism in the Basu-sense.[28] The problem, of course, is that firms with high (or increased) leverage are riskier than their less levered counterparts potentially generating more negative returns in tandem with lower earnings. In other words, it is not necessarily increased conservatism that is generating their results but the fact that they failed to control for sample selectivity.[29]

Why is Basu (1997) still the accounting flavor of choice given such fundamental criticisms of this metric? In my opinion, simplicity of execution is the motivator. What can be simpler than running a linear regression?

3.2. The market to book ratio—a proxy for everything, a proxy for nothing

The market to book ratio (or its inverse) seems to proxy for many things in the literature: growth and growth options, (normalized) firm value, monopoly power, uncertainty about average profitability, information asymmetry, a risk factor, and unconditional conservatism. A proxy that covers so much territory is probably a poor proxy for any given specific construct. As a measure of firm value, monopoly power, growth, and uncertainty about average profitability, the market to book ratio is problematic if only because the denominator (book value) is a function of accounting policies such as conservatism (whether conditional or unconditional). I know of no theory based on primitives that relates the market to book ratio to information asymmetry or unconditional conservatism—although it is likely affected by both conditional and unconditional conservatism. Worse yet, the market to book ratio is not an exogenous primitive and yet the numbers of papers that employ this variable as a regressor are a legion.

3.3. Real earnings management—what are the exogenous variables?

Measures of real earnings management in the accounting literature do not appear to be derived from theoretical underpinnings either. Beyond theory, however, these measures also suffer from severe econometric problems. Basically, it is not at all clear what is exogenous and what is endogenous. For example, in an influential paper, Roychowdhury (2006) proxies for real earnings management by regressing cash flow from operations on contemporaneous sales and the change in sales to determine normal levels of cash flows. But, cash flows and sales are simultaneously determined and neither one is exogenous to the other. The same issues affect his other real earnings management proxies. In fact, endogeneity is an issue for almost any study relating accounting-based variables to each other.

3.4. Investment efficiency and under- and over-investment

Biddle and Hilary (2006) investigate the relation between accounting quality and investment efficiency, where by investment efficiency they mean the cash flow sensitivity of investment. Biddle and Hilary measure cash flow sensitivity either by regressing investment on contemporaneous cash flows or by using the cash-flow-weighted time-series average investment relative to un-weighted arithmetic time-series average investment. Both measures are problematic. The

[27] Another option to address selectivity in the Basu context is offered by Beaver et al. (2012). However, they do not model conservatism beyond Basu.

[28] In their attempt to measure conservatism via a Basu reverse regression format, Jayaraman and Shivakumar (2013) also fail to execute a proper difference-in-differences analysis making their results un-interpretable (Callen et al., 2012). The editorial process appears to have failed here which speaks potentially to some of the issues raised in Section 5.

[29] In a tendentious footnote that should also have raised editorial concerns, Jayaraman and Shivakumar (2013) manage to selectively dismiss two large sets of literatures out of hand. First, not only do they dismiss the concerns of Dietrich et al. (1997) but also, apparently unknowingly, the entire sample selectivity literature of Heckman (1976, 1979), Lee (1982, 1983) and Maddala (1983, 1986), among many others. Instead, they selectively reference the lone study by Ball et al. (2010). Second, they also manage to dismiss the entire accounting VAR decomposition literature begun by Volteenaho (2002) by reference to the lone study by Chen and Zhao (2006). Not only do they fail to acknowledge that the latters' results have been disputed by Engsted et al. (2010), they also manage to misunderstand the implications of Chen and Zhao (2006) for accounting research (Callen, 2009).

regression measure disregards the fact that investment is a multi-period decision and current investment is likely affected by both past investments and past cash flows (profitability).[30] Furthermore, these measures presuppose that investment is endogenous to cash flows and cash flows are exogenous, disregarding the likelihood that cash flows are endogenous to investment. By contrast, a more general approach that not only provides a comprehensive measure of investment efficiency but also addresses the potential endogeneity of the cash flow/investment relation is provided by Hu and Schiantarelli (1998).[31]

Biddle et al. (2009) investigate the relation between accounting quality and investment efficiency where the latter is measured by over- and under-investment. Over-and under-investment are measured in one case by the residual from a regression of next period's investment on this period's sales growth. Such a regression implicitly makes extensive simplifying assumptions about the optimal investment strategies of firms. For example, investment is a multi-period decision and it is heroic to assume that one can measure under- or over-investment over a one period horizon. As we learned from the real options literature, both the amount and timing of investment is a function of the firm's growth (or abandonment) options, yet these play no part in their analysis.

4. Financial accounting theory and frictions

Financial accounting theory is supposed to provide testable hypotheses and direct empirical research in financial accounting. Nevertheless, there is an unfortunate tendency for theory to disregard the very frictions that make the theory meaningful.[32] True, it is often difficult to model frictions but sometimes the frictions are the heart of the issue. This point has been raised by theorists before—see, for example, Hemmer's (2008) discussion of un-modeled frictions in the paper by Plantin et al. (2008)—but, to my mind, the issue is important and bears emphasis. Two examples should suffice.[33]

Gigler et al. (2009) elegantly model the effect of accounting conservatism on debt covenants. In the context of their model, they assume that level of debt is positive but exogenous to the firm. In other words, Gigler et al. (2009) do not model the leverage decision. This in and of itself is not unusual or necessarily problematic. One cannot endogenize everything in a model and even important decisions cannot always be incorporated especially where the model is complex otherwise. But, here is the rub. In their model, by assumption, debt has no positive value. Indeed, debt is costly with no balancing offsets. Logically then, the firm in this model should be all equity but of course one cannot analyze debt covenants for an all equity firm. In short, it makes no sense to assume an exogenous level of debt when the model itself drives an optimal capital structure that is all equity. How can one reasonably explain debt covenants in a model in which only irrational firms issue debt?

One potential response to such a criticism is that the model could be extended by adding, say, tax deductibility of interest which would create an optimum level of debt in the model. But, given the extant model's complexity, including taxes would likely make the model intractable and, anyway, it would not change the qualitative model results. In my opinion, this response is inadequate. There is no guarantee that the authors' results regarding the effect of conservatism on debt covenants would still hold if they incorporated tax frictions in the model. For example, it is quite possible that the assumed exogeneity of debt, where no firm would be holding debt optimally in the first place, is what drives the paper's result that accounting conservatism is essentially a bad. In my opinion, a proof with taxes (or other) frictions is mandatory if we are to believe the model's results.

There are cases where some frictions are modeled whereas others are not, and it is precisely the un-modeled frictions that are particularly relevant. In an interesting paper, Beyer and Guttman (2012) consider a model of voluntary endogenous disclosure prior to the firm issuing new shares in order to finance new investment opportunities. The issue is that new shareholders appropriate part of the benefits from current assets in place. This gives management an incentive to misreport upwards the value of current assets in place so that new investors will be willing to pay more for each new share, thereby effectively reducing share dilution. The model is quite elegant and the implications for voluntary disclosure new. Nevertheless, the crucial implicit assumption in the model is that new shareholders share benefits from assets in place with old shareholders. However, to the extent that assets in place distort managerial incentives, firms will often do project financing rather than raising straight equity, which separates the returns from the new investment and the returns from assets in place. In other words, project financing makes their problem go away. Beyer and Guttman do not consider this option or what frictions arise that might make the project financing approach too costly.

5. Financial accounting methodology, journal referees and the editorial process

The discussion above focused on a few central areas of financial accounting research. In this section, I would like to discuss more general concerns that have implications for the refereeing process in accounting journals. Similar concerns were raised in the past by Lee (1995) and Williams and Rodgers (1995) among others.

[30] For empirical evidence, see Bar-Yosef et al. (1987). As shown by Aivazian and Callen (1979), the optimal path of firm-level investment is also affected by industry competition. In their regression proxy, Biddle and Hilary (2006) estimate industry parameters effectively controlling for competition. No such control obtains for their alternative proxy. Other criticisms of the investment efficiency literature are a legion beginning with Kaplan and Zingales (1997).

[31] This measure may not be useful for a cross-country analysis given the paucity of data but is surely doable in a firm-level analysis.

[32] Frictions include transactions costs, taxes, short-sales constraints and so on.

[33] Space restrictions preclude a more complete analysis.

In the opinion of this author, characterizing the field of financial accounting research as being complacent becomes more obvious as one surveys cognate fields such as finance (and economics). Financial accounting research seems to suffer from poor scientific methodology. How often do we see accounting papers that model a phenomenon and then proceed to test the model? I am struck by the fact that in contrast to financial accounting, papers in top finance journals often (but not always) model the phenomenon at issue, use these models to drive the empirical hypotheses and then validate these models rigorously by testing the hypotheses. The highly cited asset pricing papers by Easley et al. (2002), Bansal and Yaron (2004), and Pástor and Veronesi (2003) are prime examples of important papers that model asset prices and then test the model empirically. There are many others. In contrast, the accounting asset pricing literature does theory and empirical work but rarely the two together. The absence of integration between the theory and the empirics in financial accounting research often leads to empirical hypotheses that are either based on intuitive hand-waving or, worse yet, on misapplied models that cannot describe the empirical phenomenon being studied.

It is rare indeed to see an accounting paper that has both a model and empirical work. Perhaps, this is no accident. In my experience, papers of this sort get short shrift from the accounting refereeing process because accounting referees fail to appreciate the subtle interplay between the models that can be used to derive testable hypotheses and the empirical analysis. Usually, papers with both theory and empirics are evaluated by both a theorist and empiricist. Theorist referees typically want to see more elegance in the models and will reject on that basis, although on balance they are more sympathetic to the endeavor than empiricists because they realize that elegance often comes at the expense of deriving clear cut hypotheses. By contrast, I find that empiricist referees fail to fully appreciate that every model is an abstraction from reality and, therefore, as we learned long ago from Milton Friedman and others, one cannot judge a model by its assumptions which are necessarily "unrealistic". Rather, one judges a model by subjecting its conclusion to rigorous empirical testing, and not by judging its assumptions. Evaluation by these two referee types operating in their own world view results in the usual rejection outcome. The heart of the problem is that very few accounting scholars, or editors for that matter, are comfortable with both theory and empirics. Interestingly, our cognate fields seem not to suffer from this weakness.

I am often struck by the fact that financial accounting empiricists are fixated on linear regression models. Referees just follow suit. There are two problems here. First, although accounting papers are often replete with robustness tests, rarely are the linear regression forms themselves subjected to much specification analysis, even as simple as estimating a log linear form instead. Second, whatever happened to other methods of empirical analysis such as Monte-Carlo simulation, Bayesian (MCMC) analysis, path analysis, non-linear regressions, linear and nonlinear programming, case study/qualitative data analysis, and so on? One only has to go to the finance and, especially, the economic literatures to see how varied are the empirical tools used by these discipline as compared to ours.

I find it distressing that what I believe to be some of my more innovative papers have failed to make it into the so-called top accounting journals. I believe that the refereeing process is often to blame. Referees are too willing to review papers that are beyond the confines of their expertise rather than sending these papers back to the editor. Because of perceived reputational damage, editors are focused on preventing Type 2 errors (accepting fundamentally "bad" papers) and often seem to give short shrift to the cost to our profession of Type 1 errors (rejecting fundamentally "good" papers). Indeed, how often will editors take a chance on an interesting idea if the referees are negative? I personally only know of one such accounting editor who, perforce, must remain nameless. Worse yet, I often get the feeling that editors do not even read the paper when the reviewers are negative. From my personal experience, the cavalier attitude of accounting editors stands in stark contrast to editors at the top finance and economics journals (such as the *Journal of Finance* and the *American Economic Review*) whose responses are often very insightful *especially* when the referees are negative. I would rather receive a churlish acceptance than a sweet rejection, as a former co-author of mine once wrote, but if it is to be a rejection at least I want to learn from it beyond a simple rehash of the referees' reviews. The upshot is that the accounting journal refereeing process often yields pedestrian results even in the top accounting journals.

More recently, the editor of at least one major accounting journal refused to allow author rebuttals except under egregious circumstances. Of course, this policy makes life easier for those involved in the editorial process but surely results in extensive Type I errors. Scholarly referees and editors are not baseball umpires. They can be mistaken. I personally have successfully rebutted referees at such journals as the *Journal of Finance* and the *Journal of Accounting Research*. A related issue is that even when rebuttal is allowed, editors rarely seem to take sides. From my experience, their attitude is that the author must kowtow to the referees and just maybe referees will agree to change their mind. Surely, if the editor is worth his/her salt, that editor should be intellectually involved.

Another editorial issue is failure to refer to studies outside of accounting and finance that speak to the topic being researched. One blatant example involves recent accounting papers on nonprofit governance.[34] These papers fail to cite relevant nonprofit literature that deals with nonprofit governance.[35] One would think that the first order of business prior to investigating the relation between accounting and nonprofit governance is to be aware of what the nonprofit literature has to say about nonprofit governance.

There is also an important issue that is much discussed privately but rarely publicly. Accounting scholars appear to be afraid to discuss the issue publicly presumably because of the perceived sanctioning power that certain journals will have on

[34] See Aggarwal et al. (2012) and Yetman and Yetman (2012), for example.
[35] See Callen et al. (2003, 2010a) and the references cited therein.

the publication of their future output. The issue of course is the perceived "incestuous" relationship between some influential accounting journals and the authors whose papers are accepted by these journals. If you are connected (current faculty and former students) then you are invited to the appropriate journal conferences. If you are connected, your chances of making it through the refereeing process appear to be significantly greater than those who are not connected.[36] I personally discount papers which I believe are the outcome of such a relationship. Not only is this phenomenon an embarrassment to the profession but it distorts research innovation to the detriment of all of us. Journals are a public trust even when they are managed by private universities.

6. Conclusion

This essay provides a selective critical review of the financial accounting literature focusing primarily on three research topics: accounting valuation including implied costs of capital, empirical accounting proxies, and un-modeled frictions in accounting theory. In the opinion of this author, accounting research in these areas is often too complacent, primarily in its lack critical reasoning. Empiricists often fail to understand the limitations of the available models and end up misapplying (abusing) them. The examples discussed in this essay include structural modeling and model falsifiability; determining whether a firm is over or underpriced based on valuation models that do not allow for such phenomena; arbitrarily "merging" two disparate models—one for valuation and one for the discount rate; and failing to appreciate the empirical limitations induced by risk neutral valuation models in estimating costs of capital. Other examples of lack of critical reasoning include employing the same proxies over and over again that ostensibly have no underlying theoretical bases; estimating regressions that necessarily yield biased coefficients when the econometrics literature provides solutions; and generating complex models absent the frictions that are essential to the issue being researched.

The intent of this essay is not simply to be critical but to raise needed discussion and, perhaps, even generate new tools and concepts that will move the profession forward. However, I am pessimistic. After all, consider the minimal impact that other critical papers have had on the financial accounting literature status quo. Nevertheless, the long run offers hope. I believe it is incumbent on those of us who are involved in PhD programs to ensure that our empirically-oriented students are not only able to read theory but to do theory as well. At a minimum, we would do well to train our students to appreciate the subtle interplay between theory and empirics and to explain to them why we should test model implications rather than model assumptions. We also must teach them to be more critical of published papers and to question the status quo. Finally, we must ensure that our students understand scientific methodology such as why hypotheses need to be falsifiable, and to be aware that in accounting, as in other fields, scholars are loath to reject their pet theories even when the empirics seems to suggest that they should be rejected. Hopefully, if we do our teaching job properly, these students, as they become journal referees and editors, will turn the system around for the better.

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[36] See also Williams and Rodgers (1995).

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