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Fifty years of capital markets research in accounting: Achievements so far and opportunities ahead

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ABSTRACT

This paper reviews the literature on capital markets research in accounting over the last 50 years. Rather than trying to be comprehensive, the review focuses on selected areas, and strives to be forward-looking. The first major takeaway is that the literature has made great progress, especially on the technical side. The second takeaway is that great opportunities remain, especially in using Big Data, looking more closely into the accrual process, and in issues related to standard setting.

1. Some caveats

I would like to start with some caveats. The first caveat is that this review is not meant to be comprehensive. The existing capital markets literature is simply too large, and so it is nearly impossible to cover everything. To use an analogy from music, what I am going to do here is to present the Selected Hits, rather than the Complete Collection. And since it is the Selected Hits, the choice of what to include is largely driven by personal choice and personal expertise. The idea is to provide a breezy and punchy overview rather than an exhaustive analysis of everything that has been done. Second, I will be attempting to "give an opinion" rather than just "listing what has been done". In other words, the review will take more of a position on certain things. Note that my opinion could be biased or wrong but I feel that this is the right occasion to say something more pointed rather than keep to the safety of just listing accomplishments. Third, the review aims to be big-picture and forward-looking. The important thing is not only what we have done so far but where we go from here. Accordingly, I will try to provide some ideas for future research.

For those who want a more complete coverage, there are some existing reviews of the capital market literature that are helpful. Kothari (2001) is a comprehensive review of capital markets research; it is widely read and cited but it does not reflect more recent findings. Richardson et al. (2010) is more recent but covers mostly

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accounting anomalies and fundamental analysis. Dechow et al. (2014) is also recent and more compact, concentrating mostly on the relation between stock prices and earnings. The most recent review I have seen is Kothari and Wasley (2019), a fairly comprehensive review with a particular emphasis on how the literature evolves from Ball and Brown (1968).

2. Is the earnings number useful?

2.1. An updated replication of Ball and Brown (1968)

Ball and Brown (1968) is widely considered foundational for the capital market literature, tracking the stock returns for separate portfolios of positive and negative earnings surprises. The idea is that the stock market is an efficient aggregator of information, and therefore we can use stock prices and returns as a benchmark for the information content in earnings. Fig. 1 shows an updated replication of Ball and Brown (1968), reproduced from Dechow et al. (2014). An examination of Fig. 1 reveals pretty much the same message as the original Ball and Brown (1968) paper, showing that stock returns correctly anticipate the sign of future earnings surprises up to 12 months in advance. If anything, the spread between the positive and negative portfolio is wider here, exceeding 30% over the full 18-month window. Thus, the original message of Ball and Brown (1968) is emphatically confirmed in more recent samples, showing the continued relevance of their groundbreaking research. The other big takeaway is that there appears to be a market underreaction to earnings information, as the two portfolios continue to drift in the direction of the earnings surprises even after earnings are announced in month 0. This phenomenon was heavily studied in later research, and became known as the Post-Earnings-Announcements-Drift (PEAD).

2.2. Are earnings announcements useful?

The other major study from the early years is Beaver (1968), which shows pronounced spikes in trading volume and stock returns in narrow windows around earnings announcements. This evidence indicates that earnings announcements contain new information, and that the stock market reacts to this new information. Fig. 2 presents the Dechow et al. (2014) replication of Beaver (1968) for several sample periods over time. What is interesting here is that both the trading volume and the stock return reaction have become more pronounced over time. So, it seems that earnings announcements have become a source of increasing information content, although more recent studies find that this information content is due to items beyond earnings (Beaver et al., 2020).

Summing up, early studies like Ball and Brown (1968) and Beaver (1968) marked an exciting start to capital market research in accounting. Earnings especially, and the outputs of the accounting system more generally,

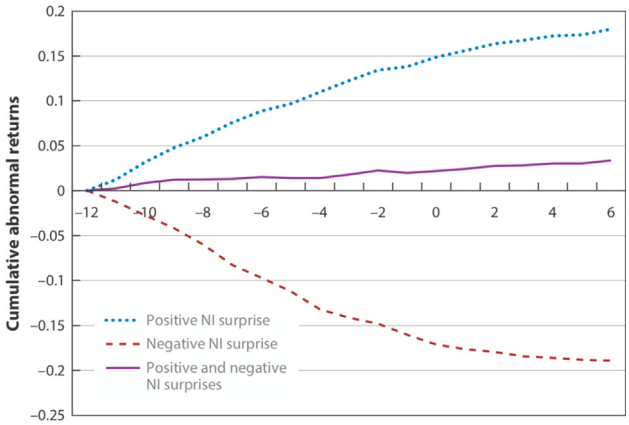

Fig. 1. Month relative to annual earnings announcement.

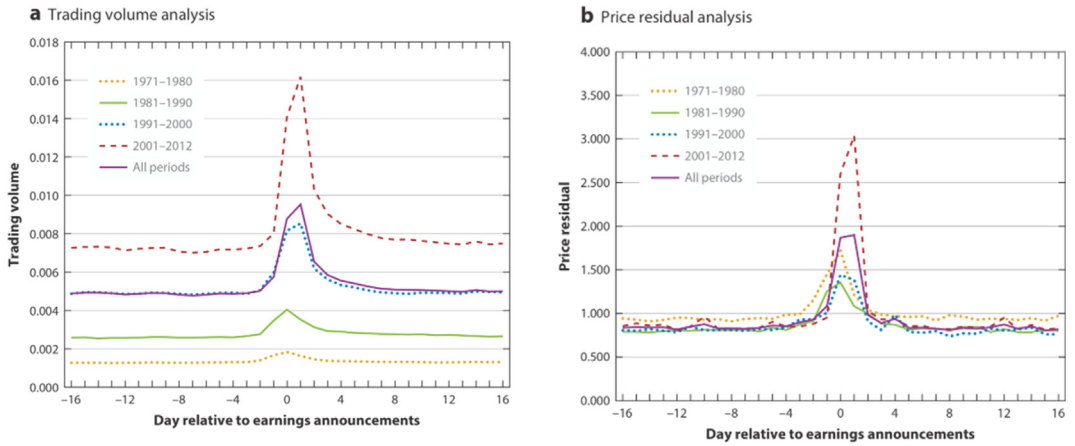

Fig. 2. Day relative to earnings announcements.

seemed to be a powerful source of information for capital markets. Accordingly, this early excitement and promise attracted many researchers, and capital markets quickly became perhaps the dominant area in all accounting research.

2.3. After *Ball and Brown (1968) and Beaver (1968)*

I use the framework and the language of *The Structure of Scientific Revolutions* by Thomas Kuhn to describe what followed the initial successes in capital markets research, as the comparison is apt. Basically, the early studies marked the rise of a new paradigm which captured the minds of accounting academics, and the whole idea and practice of accounting research shifted in a decisive manner. While previous research relied mostly on "armchair" arguments and speculations, the new paradigm brought empirical tests and evidence to the fore, with corresponding attention to data, statistical tests, and borrowing theories and methods from economics and finance. There were some interesting sociological angles as well, with many from the older generations displaced or sidelined by the new way of doing research.

Using the language of the Kuhn book, a great volume of "normal" research on the price/earnings relation appeared after the initial foundational studies. "Normal" research means "mopping up operations", basically studies that explore and advance the new paradigm but do not aim for radical novelty. The basic point of Ball and Brown (1968) and Beaver (1968) is that prices react to earnings, and earnings have information content. But there was a lot that could be done with these initial findings, they could be expanded to new samples, improve the test methodology, and so on. For example, the normal research on the price/earnings relation includes multiple studies on the specification of earnings surprises, such as using time-series models vs. analysts forecasts. Researchers explored event return windows that varied from a few days to several years, and used various definitions of earnings, such as net income, income before non-recurring items, and cash flows.

One useful way to summarize the first two decades of capital market research is provided by Lev's (1989) influential review. Looking at the big picture, Lev (1989) finds that the whole exercise has been somewhat disappointing, especially compared to the enormous effort invested in this literature. His main point is that if we look at the magnitudes of the R^2 of the associations between earnings numbers and returns, they are fairly small, with typical magnitudes of 2–7%. There are, of course, good reasons for this, a major one being that stock returns are very noisy. Thus, the question is how much R^2 can we expect, especially on a firm-level basis where the noise is much higher as compared to portfolio specifications. Lev (1989) also suggests that GAAP rules are probably at fault as well, and perhaps we need to think of ways to improve the accounting to better capture the value of firm operations.

Perhaps even more importantly, around the end of the 1980s and the beginning of the 1990s, the profession came to the critical realization that the basic premise of using formulations like $R_t = a + b^*\Delta E_t + e$ is quite

limiting (Lee, 2001). Notice that we are using market prices or returns as the benchmark, and we judge information content in earnings by the degree to which the right-hand side maps into this benchmark. The problem is that if we always put contemporaneous returns on the left-hand side in these models, we are assuming that the market has already perfectly impounded all available information. But if the market perfectly knows everything already, what is the use of accounting? Where is our value-added then? Does it matter at all how we do the accounting?

3. The rise of fundamentals-based research

There was another new wave of research that soon arose in response to these disquieting questions, and I do think it was very good for accounting. This is the rise of what we can call fundamentals-based research. The critical difference from before is that instead of immediately going to the market for validation of accounting information, we now first study the accounting system itself, and try to understand how it captures the value created in real company operation, including the production of key value metrics like earnings and book value. And it is only after we internalize this knowledge that we turn to the market to see if the fundamentals-based metrics map into observable market prices. It was a very definite and large shift in the way that we think about these things. Perhaps most prominently, James Ohlson and Stephen Penman led the way here in a series of theoretical papers, developing these ideas more formally, see Ohlson (1995) for example. Basically, the idea is to derive equity value as a function of firm fundamentals, especially earnings and book value of equity. And soon after, there was a stream of empirical papers implementing these ideas, prominent examples include Frankel and Lee (1998) and Dechow et al. (1999).

This shift in thinking opened the doors to asking entirely new questions, and addressing new audiences. For example, if we find that fundamental values and observable stock prices differ, now we are not going to necessarily assume that prices are better, and accounting inputs are deficient. Now we may allow for the possibility that prices are deficient, and we may investigate whether prices catch up with fundamental value at some point, consistent with how value investors like Warren Buffett approach the analysis of financial information and make investments. Correspondingly, such research has natural synergies with our teaching, where a lot of our undergraduate and MBA students have an interest not so much in accounting itself but more in how accounting can be used to find hidden value, and lead to better investing decisions. In other words, this new thinking marked a radical shift where market efficiency is now an examined hypothesis rather than just a maintained hypothesis.

Perhaps the biggest single positive from the rise of fundamentals-based research is that this new approach sparked renewed interest in how accounting actually works. I view this as a strong positive because it naturally plays to our expertise in accounting, and allows us to offer insights which would otherwise not be possible for our colleagues in finance and economics who study similar questions. It basically brought us back to such classic questions as "What makes good accounting?" and "What makes earnings useful?".

4. What makes earnings useful?

4.1. Accrual accounting

Dechow (1994) is the paper that both signaled and catalyzed the rebirth of interest in how accrual accounting actually works. It is remarkable that Dechow (1994) appeared a full 26 years after Ball and Brown (1968), while it asks the most basic questions about accrual accounting. Most prominently, is earnings or cash flows better at measuring firm performance? The story in Dechow (1994) is that cash flow accounting is simple and objective: income is just cash flows coming in, while expenses are cash flows going out. But the weakness of cash flow accounting is that there are timing and mismatching problems, which question the use of net cash flow as a measure of firm performance. For example, consider a firm that sells Christmas trinkets. Before the Christmas season, the firm spends a lot of money on inventory, and then the money from the Christmas sales comes much later. So, if we look at the performance of this firm in terms of net cash flow, it looks like it has a lot of losses first and then it makes big profits later on. This does not make much sense because the "early losses" are clearly related to the "late profits" by the logic of the business.

Accrual accounting alleviates such timing and mismatching problems through the use of accruals. Using accruals, the Christmas trinkets firm above will capitalize the cost of inventory, and expense it in Cost of Goods Sold when the inventory is sold. Thus, accrual accounting aligns the cost of Inventory with Sales, consistent with the logic of the business, and so we get a much better measure of firm performance. But the cost of using accrual accounting is that accruals are essentially estimates made by management, which opens the door to estimation errors and subjectivity, and even willful manipulation. For example, capitalizing the cost of inventory at the time of purchase in anticipation of realizing it as Cost of Goods Sold at the time of sale may turn out to be problematic because we may need to write off the inventory before selling it. Thus, recording accruals involves a trade-off of benefits and costs, and it is an empirical question whether the sophistication of accrual accounting is better than the simplicity and objectivity of cash flow accounting.

Using stock prices as the benchmark for value-relevance, Dechow (1994) finds that accrual earnings are more strongly associated with stock returns than cash flows. In other words, the findings of Dechow (1994) indicate that accrual accounting is indeed resolving timing and mismatching problems, and that the stock market understands and endorses the use of accrual accounting. Thus, Dechow (1994) is foundational in terms of establishing the utility of accrual accounting.

A continuation of this line of inquiry is Dechow and Dichev (2002), delving further into the structure of the accrual process. The first message of Dechow and Dichev (2002) is captured in Eq. (1) below, where accounting earnings is expressed as a sum of past, present, and future cash flows, and the origination and reversals of accrual estimation errors. In Eq. (1), the first three terms are the cash flows, where the subscripts indicate when the cash flow occurs, and the superscripts indicate when the cash flow is recognized in earnings. The intuition is that accruals resolve the timing and mismatching problems in the underlying cash flows by moving their recognition in earnings across time, e.g., the revenue from a sale is recognized in earnings at the time of the sale by recording an accounts receivable rather than waiting for sales proceeds to be collected later on. Thus, the benefit of accrual accounting is captured in the first and third cash flow terms, whose recognition in earnings has been shifted across time. However, the benefit of recording accruals comes at the cost of incurring accrual estimation errors, e.g., the recorded Accounts Receivable estimate may be less than the actual cash collections later on. The consequence is that recording an overstated receivable first and writing it off later introduces noise in earnings, creating a false boost in earnings first, and a false decline in earnings later on. In sum, the cash flow terms capture the benefit of accrual accounting, while the error terms capture the cost. For accounting to be "good", we hope that the cash flow terms are large, and the error terms are relatively small.

$$Earnings_t = CF^t_{t-1} + CF^t_t + CF^t_{t+1} + \varepsilon^t_{t+1} - \varepsilon^{t-1}_t \quad (1)$$

Since moving the recognition of cash flows into earnings across time is accomplished by recording accruals, Dechow and Dichev (2002) also show how accruals can be expressed in terms of the underlying cash flows. Unfortunately, the current accounting system does not provide the theoretically required cash flow variables, and instead the empirical version of Dechow and Dichev's model is based on observable variables. Since the model is based on working capital accruals, not surprisingly the empirical version employs cash flow from operations (see Dechow and Dichev, 2002 for further detail):

$$Accrual_t = b_0 + b_1 CFO_{t-1} + b_2 CFO_t + b_3 CFO_{t+1} + \boldsymbol{e} \quad (2)$$

Intuitively, Eq. (2) says that accruals that do not map into their corresponding cash flows represent the accrual estimation errors, e.g., the portion of receivables not eventually collected represents the initial overstatement of the receivable. The empirical tests in Dechow and Dichev (2002) show that the estimates of the accrual estimation errors derived from Eq. (2) are indeed helpful in capturing the quality of accruals and earnings, e.g., high levels of estimation errors are related to low earnings persistence.

While the Dechow and Dichev (2002) model has been widely used, it also has some limitations. So, how can we improve on it? Useful extensions and re-formulations include McNichols (2002), Francis et al. (2004, 2005), Ball and Shivakumar (2006), and others. For example, McNichols (2002) combines the Dechow and Dichev (DD) model with the Jones (1991) model, and that combination has been popular empirically, although it is more questionable on theoretical grounds. I would argue that further work is possible here. Most importantly, notice that the DD model only captures the discretion/estimation of anticipatory accruals mapping into future

cash flows (like Accounts Receivable). But there is a lot of other discretion/estimation in accounting, including depreciable and useful lives, salvage values, interest rates, sales returns, percentage of completion, etc.

The good news is that the basic DD intuition applies to these other estimates as well. The spirit of the DD model is that accounting makes estimates, and the resulting estimation errors represent the cost of using accruals, manifesting as noise in earnings. The very same intuition applies to all other estimates as well, and so it can be used to measure the accrual estimation error noise as well. If you have the estimates, and if you have the realizations, the difference between them will give you the estimation errors. For example, if initial estimates of severance costs are too high and are low at realization, earnings will be initially understated and then overstated at realization. The bad news is that in most cases the accrual estimates and their realizations are not available to outside users of financial information.

But perhaps there could be some workarounds in implementing this intuition. For an idea what is possible, notice that the error term in Eq. (1) has a very specific form. If you re-write Eq. (1) as of time t − 1, and also as of time t + 1, and compare that to the expression as of time t, you would see that the error term is very strongly negatively correlated across time. This negative relation is, of course, not accidental. The error terms in (1) are negatively autocorrelated because the accrual process is self-correcting - if you make an accrual error in some period, it has to be corrected in some future period with an accrual with the opposite sign. The DD model is on working capital, so the errors and their corrections happen within one period, e.g., if at time t the initial estimate of the Accounts Receivable is too optimistic, the uncollected receivable is written off at time t + 1. The initial overestimation has a positive effect on earnings, and the write-off has a negative effect on earnings, so the accrual estimation errors are negatively autocorrelated over time. The same intuition applies to all other estimates. If we make an accrual estimation error, we have to correct it at some point after that with an accrual with the opposite sign. Thus, the signature of accrual estimation errors is that they reverse over time, and thus they induce a negative autocorrelation in earnings. The challenge will be to formalize this intuition, and to distinguish the reversals of all accruals (since all accruals have to reverse at some point) from the reversals of accrual estimation errors. To my knowledge, Dechow et al. (2012) is the only existing study that uses this intuition but I believe that we can get a lot more mileage out of it.

There are many other worthy studies on the properties of accruals but for the purposes of this review, I will limit the discussion to just two more widely influential examples. Sloan (1996) examines Eq. (3) below, and finds that the coefficient b_1 is greater than b_2, indicating that the cash flow component of earnings is more persistent than its accrual component. In other words, the results in Sloan (1996) indicate that firms with high accruals have low earnings persistence, and low earnings quality.

$$E_t + 1 = b_0 + b_1 * CFO_t + b_2 * Accruals_t + e \quad (3)$$

In addition, investors do not seem to understand this property, so portfolios long on stocks with low accruals and short on stocks with high accruals earn abnormal returns on the magnitude of 10% a year. This phenomenon has become known as the "accrual anomaly," and is one of the most widely known and researched stock market anomalies during the last 25 years.

Richardson et al. (2005) extends Sloan (1996), and investigates for differential persistence *within* accruals. The idea is that looking closer at the properties of the accrual process allows one to identify accruals that are more problematic than other accruals. Indeed, Richardson et al. (2005) identify a taxonomy of accruals that is helpful in predicting earnings quality, and also find that the stock market does not seem to fully appreciate these more subtle properties of accruals. But the most important contribution of Richardson et al. (2005) really is that they provide a comprehensive definition of accruals, namely, for a given period accruals can be defined as the changes in all non-cash assets and liabilities during that period. To me at least, the fairly-recent arrival of Richardson et al. (2005) is totally fascinating. Accruals represent the value-added of accrual accounting in the world, the bread and butter of what we do. Yet it took almost 40 years of research after Ball and Brown (1968) to finally have a comprehensive definition of accruals! While there could be a pessimistic read on this fact, I prefer the more positive interpretation. If the Richardson et al. (2005) experience is any guide – and I think it is – it implies that there are a lot of fundamental discoveries still to be made, and it is only our own limitations that prevent us from seeing what they are. What are we going to do in the next 50 years? The good news is there seems to be a lot that can be done, and I try to provide some pointers further below.

4.2. Conservatism

Another major strand of the literature on what makes earnings useful is conservatism, starting with Basu (1997). The story there is that accounting reacts asymmetrically to good and bad news. Specifically, accounting immediately impounds the full effect of bad news, capitalizing the present value of all future implications. For example, on finding that the depreciable life of some asset turns out to be shorter than originally expected, accounting will immediately write down the asset for the full effect of this shortening. In contrast, the book values of assets are not written up under favorable circumstances, and the gain on value is delayed until the asset is sold.

The literature on conservatism has seen rapid growth, finding a number of beneficial effects of more conservative accounting, especially for contracting outcomes. Indeed, the sheer volume of the conservatism literature warrants a separate review in itself, and such reviews actually already exist (Basu, 2009). So, instead of attempting to survey this literature, I would like to make one limited but crucial point. The Basu (1997) measure of conservatism uses stock returns as the benchmark for information content, and stock returns reflect many things, including possibly other explanations for the hypothesized asymmetric relations with earnings. Correspondingly, the Basu measure has been subject to a number of criticisms, and then rebuttals, see for example, Ball, Kothari, and Nikolaev (2013) and references thereof. Personally, I think a possible way forward in this literature is to derive a measure of conservatism which is independent of market prices, essentially a fundamentals-based measure of conservatism.

4.3. The dark side of discretion – earnings management

Earnings management can be defined as intentional adjustments to reported earnings to achieve desirable outcomes such as beating earnings benchmarks. By its very nature, earnings management is harmful to the role of earnings as a measure of firm performance and, correspondingly, research in this area has been a major focus for accounting academics for many years. By now, earnings management represents a very large, and rather mature literature, comprising dozens and perhaps even hundreds of studies.[1] Without getting into detail, I think the weight of the evidence leaves little doubt that earnings management exists. The extent of it, though, is more debatable, and partly depends on definition. It seems that more innocuous forms of earnings management within GAAP rules are widespread, affecting about 20% of U.S. firms in recent years (Dichev et al., 2013). More serious and extreme forms of earning management crossing into fraud seem to be much less prevalent, perhaps on the magnitude of 1–2% of firms, as reflected in studies of formal SEC actions and investor lawsuits (Dechow et al., 2011).

Is there room for more work in this literature? Given the sheer number of existing studies, it has become more difficult to make a meaningful contribution. On a personal level, I think two areas offer room for improvement. First, we need more earnings management studies where the null hypothesis is defined in a much sharper way as compared to that for most existing research. The null hypothesis is a statement of what earnings would be without earnings management. And this is really the key weakness of the majority of studies here: it is not quite clear whether the null is sharply defined enough to offer a powerful test. For example, suppose that there is a study which finds that in bad times managers cut R&D, apparently to hit earnings benchmarks. The question, though, is whether such a finding is truly indicative of earnings management - because in bad times prudent managers would cut R&D down anyway, as they should. So, what is the key to a sharp null hypothesis? There could be different approaches here but basically the key is identifying a setting where the null of no earnings management is very clear and convincing.

Second, it would be good to have more big-picture evidence on the economic prevalence of earnings management. By now there are scores of studies that offer evidence of earnings management in certain settings like equity offerings or for certain earnings components like special items or accruals. But there is less evidence on the broad prevalence of earnings management in the economy. Some authors opine that earnings management

[1] In fact, this literature has been such a major preoccupation of accounting research that one can argue that we have been more successful at identifying the problems of accounting (of which earnings management is a major one) rather than building up the case for what is good in accounting.

is likely confined to isolated pockets of rogue managers and firms, while others suggest that it is a rather pervasive phenomenon affecting perhaps the majority of firms in one way or another. More research could help to narrow down these rather divergent views.

4.4. Fair value

The research on fair values is another significant stream of the literature that investigates what makes accounting numbers useful. The reason is that the U.S. and international standard setting has adopted a balance sheet view of accounting, which emphasizes the valuation of assets and liabilities as the primary role of accounting. An integral part of this view is a broad push for various forms of fair value accounting, especially for financial assets and liabilities. Accordingly, there have been a number of studies that investigate the information content of fair values, usually benchmarking them against stock prices. Barth (1994) finds that the fair values of investment securities of banks provide significant information content beyond that of historical costs. For bank loans, Barth et al. (1996) also documents significant information content for fair values but Eccher et al. (1996) and Nelson (1996) arrive at the opposite result.

Some studies investigate the value relevance of fair values stratified by their reliability. Specifically, existing GAAP establishes the fair value hierarchy of using the so-called Levels 1, 2, and 3 inputs, where Level 1 includes the most cleanly measured assets like regularly traded shares on organized exchanges, and Level 3 includes the most problematic assets such as mortgage-backed securities and private equity shares. The prediction is that assets which are measured more cleanly will likely have more value relevance. This prediction seems intuitive, and is confirmed in the research findings (Choi et al.; Kallapur and Kwan, 2004; Song et al., 2010). The European setting provides some decisive advantages for research in fair values, specifically IFRS allows some PPE-type assets to be revalued up, which is not allowed under U.S. GAAP. The main finding in the European setting is that such upward asset revaluations do map into stock prices, which suggests that they have value relevance (Aboody et al., 1999).

Overall, it seems that fair values are reliably informative for financial assets and liabilities but the picture is more complicated and contentious for operating assets and liabilities. Of course, a key explanation for this difference is that financial assets typically have exchange value independent of the value of the firm and its operations, while operating assets are by definition mostly for synergistic use within the firm.

4.5. The big picture on the usefulness of accounting

In trying to make sense of the accumulated evidence, it is useful to step back and think about the big picture on the usefulness of accounting. The key question here is: what do investors think about the key outputs of accounting? Overall, there is some very good news here. Crucially, investors still consider earnings the single most important number in making their decisions (Graham et al., 2005). So, we can take some comfort in the fact that, despite all problems like earnings management and complicated accounting rules, we still produce the number that is the most used by investors.

But there's also some not-so-good news. There is solid evidence that earnings volatility has doubled or tripled over the last 30–40 years, and earnings persistence is way down, from a near-random-walk of 0.90 down to 0.60 (Givoly and Hayn, 2000; Dichev and Tang, 2008), and perhaps even lower today. These findings are troubling because they question the traditional role of current earnings as a guide to future earnings. Given these results, it is probably not surprising that studies have also shown a sharp deterioration of the relation between stock returns and earnings over time (Collins et al., 1997). In addition, we see a proliferation in non-GAAP definitions of earnings, which suggests that investors are dissatisfied with GAAP earnings, and are looking for alternative and better measures of performance.

Is this deterioration of the information content of GAAP earnings due to changes in the GAAP rules or changes in the real economy? And what can be done about it? Dichev and Tang (2008) point to the increasing balance sheet orientation of GAAP rules as a possible explanation. The story is that this orientation produces frequent asset/liability revaluations, which appear as one-time items on the income statement, and reduce the otherwise high persistence of regular ongoing income. Donelson et al. (2011) confirm the importance of one-time items but find that it is mostly due to economic factors, while Srivastava (2014) points to the confounding

effect of newly listed firms with lots of intangibles. Overall, this is an ongoing debate, and the difficulty is that the accounting and the economics are entangled with each other, so it is hard to cleanly separate their effects. Finally, more research in this area seems highly desirable because of its potential to inform GAAP standard setting.

5. A note on research methodology

In addition to discussing topics and areas of research, I would like to make a brief note on research methodology, using the earlier discussion of the post-earnings announcement drift as an illustration. The big positive is that when you look over the last 30 to 40 to 50 years, the general level of research proficiency is way up, there is just no doubt about it. The younger generations are much more tooled up in terms of statistics and research methodologies, the PhD programs do a better job, the computer equipment is way better, the databases are also better. So, there is no doubt that the technical level is not just a little but significantly better over time. This is all a tremendous achievement, and a great portent for the future.

What, then, are the challenges? In short, a lot of research designs tend to be quite bland. The typical research paper today has some story and hypotheses, and basically what the test boils down to is a prediction that the coefficient on some variable is different from zero, say positive. And in the typical case, the coefficient does turn out to be significantly positive. Such results, however, are rather bland and unconvincing, or "do not change the priors much" if you want to use the scientific jargon. Why? For one thing, samples tend to be quite large nowadays, and that means that most variables show up as statistically significant at conventional levels. In addition, there are growing concerns about various forms of p-hacking and cherry-picking of the results. So, what can be done about this? At the very minimum, we have to be more proactive about establishing economic significance, in addition to statistical significance. That implies, for example, looking at incremental R^2 from including the relevant variable or not, and looking at the change in the dependent variable for a typical change in the independent variable. Formulating hypotheses on the magnitudes of the coefficient rather than just the sign is also a great way to go, while it is close to non-existent today.

I also think that the effort to avoid "accidental significance" should be broader than the minimums identified above. I would use one of my very favorite studies, Bernard and Thomas (1990), to illustrate what I am trying to say. Bernard and Thomas (1990) is a study on PEAD, so the basic story is that stock returns continue to drift in the direction of the earnings surprise for many months after the earnings announcement. So, a bland study in this space will be some kind of regression of the abnormal stock returns on the magnitude of the earnings surprises. But the Bernard and Thomas (1990) study is a lot more interesting, and consequently a lot stronger, than that. By advancing some sharper assumptions, Bernard and Thomas are able to make detailed and intricate predictions about the pattern of abnormal stock returns, including their ordered signs (three positives in a row, one negative), their relative magnitudes, and with the effects manifesting in narrow windows around subsequent earnings announcements. The bottom line is that at the end of that paper one is left with the strong impression that the documented pattern of results is highly unlikely to happen by chance. So, ideally, it would be great to see a lot more of that, sharper and more specific predictions about not just the sign of a coefficient but also about its magnitude, predictions on the pattern of results as opposed to just one result, the timing of the hypothesized effect, and so on. These are the kinds of characteristics that make for a convincing paper, and memorable results.

6. Some possible areas of future research

As Yogi Berra famously said "It's tough to make predictions, especially about the future." So, consider the following as partly a prediction of what is likely to happen, and partly as a personal wish list for what I would like to see happen.

6.1. Prediction of long-term earnings

The literature on valuation leaves little doubt that forecasting of long-term earnings is the key to deriving better estimates of value (Ohlson, 1995). And those who derive better estimates of value can then make better

investing decisions, and earn positive abnormal returns. Given such indications of importance, surprisingly little has been actually done in this space. Most studies that use long-term earnings projections source them from analyst forecasts despite reliable evidence that such forecasts suffer from extreme optimism (McInnis, 2010). Perhaps the best explanation for this state of affairs is also the simplest - long-term prediction of earnings is just difficult. But the case for the value of long-term forecasting of earnings remains unchanged. Perhaps new data and new techniques can re-energize this line of inquiry (e.g., using machine learning).

6.2. Using big data, especially in fundamental analysis and valuation

Using Big Data in fundamental analysis and valuation is already happening, and will not only continue for a while but is almost surely here to stay. There are already a number of papers using various kinds of new and big data, including Glassdoor data on opinions from employees, online customer reviews, cellphone location data, crowdfunding data, photo, speech, and video data on managers, satellite images of parking lots and so on (Huang, 2018; Huang et al., 2020; Katona et al., 2018; Mayew and Venkatachalam, 2012). Further developments along these lines are basically unavoidable, and my guess is that they will happen sooner rather than later.

For those who are looking to get involved in this area, the basic template for research is fairly clear. You look for some new data like employee satisfaction or satellite data on whether parking lots are full. Based on this data, you try to establish some links to future fundamentals (especially earnings), and then you try to see whether this fundamental relation is priced correctly in the capital markets. In other words, at least conceptually, the basic template is fairly straightforward. The challenge is more on the technical side, in learning to program and to manipulate very large datasets or unusual and ill-behaved data. The upshot is that those who are willing and able to make the sizable investment to operate in this area are likely to be well-positioned for the future.

6.3. Paying more attention to the accrual process

The accrual process is our value-added to the world. We need to thoroughly understand and own this space to make a meaningful contribution to knowledge and practice. Some progress has been made, as discussed above. But we still do not quite understand (or at least we have not fully internalized) fairly basic things about the accrual process. For example, consider the following situation: a mature firm has no growth, and so its assets and liabilities stay roughly the same. For such a firm, can the quality of accruals deteriorate over time? Intuitively, the answer must be yes. But in the current literature (Richardson et al. 2005), the definition of accruals is the change in non-cash assets and liabilities. So, for a firm like this which has no change in non-cash assets and liabilities, the accruals are zero, which means there are no accruals. But that sounds strange then, how can you talk about the properties of something when that something doesn't even exist?! So, what is the answer to this puzzle?

To clarify the logic, let's make the question more specific. For a mature firm with zero growth, how can the quality of accounts receivable change? The answer is that for such a firm the *net* receivable accrual is zero (the change in the Accounts Receivable account is zero for the year) but you still have *gross* receivable accruals during the year. Specifically, the firm collects the old receivables, and originates new receivables. And the point is that the new receivables can have entirely different properties from the old receivables, for example they can be from more marginal customers. So yes, the quality of receivables can go down during the year, even though the net receivable accrual is zero for the year. When you think about this question the right way, the answer is obvious.

What I am trying to say here is that the great paper on the difference between net and gross accruals has not been written yet, while I think there will be a paper on that at some point. It does seem to be a key and consequential difference. To illustrate, let's develop this idea further, and say that we are going to regress accruals on cash flows. This is the most basic regression in this kind of research, with variations of it appearing in countless studies. What kind of accruals should we put on the left-hand side of this regression? And to be clear what we are doing, let's make this more specific, let's say that this is about revenue accruals, and we have only accounts receivable and no deferred revenues. So, that implies that we will be putting the accounts receivable

accrual on the left-hand side, while the corresponding cash flows go on the right-hand side, and for revenues that will be cash collections from customers. So, what kind of accruals should we put on the left-hand side? Using the Richardson et al. (2005) definition, it should be the change in accounts receivable because that is the definition of the receivables accrual. However, this answer does not seem quite right. What would make more sense is that we need the accounts receivable to be matched with the cash flows which were collected from these same receivables, which implies that we need some kind of gross accruals on the left-hand side, not net accruals. So, who is putting the gross accruals on the left-hand side for such regressions? Pretty much nobody at this point. The implication is that there needs to be some re-assessment of the very basics of what we do in accrual research. Needless to say, there will be a significant premium attached to the work that can solve such fundamental problems.

Another example of the danger of misunderstandings of the basic properties of accruals is the interpretation of the empirical relation between contemporaneous cash flows and accruals. For example, some studies interpret the high negative correlation between concurrent cash flows and accruals (or equivalently, a high ratio of cash flow to earnings volatility) as indicative of opportunistic earnings smoothing. In addition, Bushman, Lerman, and Zhang (2016) show that the negative correlation between contemporaneous operating cash flows and working capital accruals has declined to just about zero in recent years. These are both fine points, and are well-taken. However, what I'm really concerned about is not so much these studies per se but about a possible misinterpretation and confusion about their results. Even before looking at the empirical results, it would help to be clear that the negative correlation between cash flows and accruals is an *unavoidable* property of accrual accounting, it happens any time the recognition of a cash flow is shifted over time. In fact, this correlation is −100% between properly specified accruals and their associated contemporaneous cash flows (Dichev and Owens, 2020). If you bought Inventory for $200 cash, that is a debit to Inventory of $200 (a positive accrual) and a credit to Cash of $200 (a negative cash flow), a perfect negative correlation of −100%. Of course, things look much messier on the empirical side because in any given period you also have the other side of the Inventory accrual – expensing to COGS which is not related to the Inventory cash flows - and various one-time items and revaluation accruals that weaken the negative association to something less than −100%. But the basic point remains. Having a firm grasp of the unavoidable strong negative correlation between concurrent associated accruals and cash flows helps in the interpretation and calibration of empirical results.

6.4. More attention on standard setting and "what is good accounting?"

Standard setting matters. It sets the tone in financial reporting practices, and whether and how the world sees the value-added of accounting. And to be completely honest, what worries me is that I think standard setting today is going in the wrong direction, for both U.S. GAAP and IFRS (Dichev, 2017). Standard setters espouse a balance sheet orientation of financial reporting, emphasizing the valuation of assets and liabilities, and with little care for the income statement, and the paramount importance of earnings. This balance sheet orientation is at odds with how most companies conduct their operations, and think about value creation. For most companies, assets and liabilities are just the necessary props to ensure the success of operations, and the emphasis is on making various operational bets, essentially advancing expenses to earn revenue and profit. In other words, operations inherently follow an income statement logic for most companies, and the balance sheet orientation of standard setting is at odds with that.

Whether you agree with the above assessment or not, it is probably safe to say that there is relatively little engagement between accounting academia and standard setters today. Standard setters have trouble finding value in the academic literature, and the decisions on standards are rarely driven by research findings. In turn, standard setting-oriented work seems to garner little respect from the research journals, and the research community in general. This situation seems puzzling given the importance of standard setting. The scant engagement of accounting academics in rule-making, and practice in general, also seems at odds with what our sister disciplines do in their fields. For example, economics has a keen interest in policy questions, and there is considerable interaction between practitioners and academics at all levels, including many academics serving in top policy jobs like Chairman of the Federal Reserve and Chairman of the Council of Economic Advisors.

So, what can be done to improve standard setting? This is a subject for a much longer conversation but in a nutshell, I suggest three things. First, we need a clear articulation of the fundamental relation between cash

flows and accruals across the financial statements (complemented by disclosure). For example, we should have clear articulation between "Revenue" on the Income Statement, "Accounts Receivable" and "Deferred Revenues" on the Balance Sheet, and "Cash Collections from Customers" on the Statement of Cash Flows. The idea is that we need a clear link and articulation for all major accruals and their corresponding cash flows across the major financial statements. Right now, this articulation is greatly muddled to impossible depending on the item. Second, and partly related to the first point above, we need clear disclosure about the estimates and their realizations for the most important accounting estimates. Current GAAP already requires firms to make Critical Accounting Policies disclosure in their financial reports. And so the idea is to make this disclosure much more specific, where for each critical accounting policy firms present their estimates and realizations for the current period. Such disclosure already exists for some items, e.g., some firms reconcile their beginning and ending warranty liability with their warranty expense and warranty claims paid for the current period. The point is to extend such disclosure to all important accruals. As one immediate benefit, it will be much harder to manage earnings if this information is readily available. Third, we need a clear separation of the results of operating and financing activities since they have quite different functions and implications for firm value.

7. Conclusion

Fifty years of capital markets research in accounting is really not such a long time considering the centuries of research tradition in older fields of science. So, we can be proud of the enormous progress made in this literature, from fairly humble beginnings to the great sophistication today, especially on the technical side. And yet, I feel that we have only scratched the surface, and that there are great opportunities ahead. Ideally, future accounting research will retain its rigor but move closer to practice, including standard setting.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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