

Invited Remarks

Ball and Brown [1968]

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1. Introduction

It is a great honor for me to be at this conference and speaking to you today.

Nick Dopuch was not all that explicit when it came to a topic for this last session. Nick said, "Talk about anything you like." I interpreted that to mean I should talk in general terms about my early work with Ray Ball and its influence on the accounting literature; and in particular, about a paper we wrote about 20 years ago, which is now commonly referred to simply as "Ball and Brown [1968]."

I begin by covering the paper itself: the antecedent conditions, why the paper was written, what we attempted to do in the paper, and what was novel as far as we were concerned. Next, I give my view of the main strands in the accounting literature, since 1968, that are related to our work. I conclude by offering a few thoughts on directions that the capital-markets-based accounting literature might take in the years ahead.

2. Background

2.1 ANTECEDENT CONDITIONS

Ray Ball and I are accounting honors graduates of the University of New South Wales (UNSW). Bill Stewart was my mentor, and it was he who was responsible for me deciding to go to the United States to pursue

* University of Western Australia. This paper, which is based on my plenary address at the 1989 Accounting Association of Australia and New Zealand Conference, is an elaboration of my presentation at the 1989 *JAR* Conference. This version has benefited from comments by George Foster.

a career in accounting education. Bill had been a visiting professor at the University of Chicago's Graduate School of Business, where he had met and been greatly impressed by Sid Davidson and his colleagues. I believe I was the first of what was to become a steady procession of Australians and New Zealanders who went to the States in search of strong accounting doctoral programs, where, incidentally, many of them have excelled. I went to Chicago in 1963; but just before I left UNSW I met Ray, who was a student in a first-year accounting tutorial group I taught.

At Chicago, my basic discipline was economics; and my two fields of concentration were accounting and finance.

Honors students at UNSW studied the accounting classics. We had been exposed to Canning, Gilman, Hatfield, Paton and Littleton, Edwards and Bell, and Chambers, to mention a few. We were also well trained in accounting procedures. So on my arrival at Chicago I was exempted from all accounting courses other than the doctoral seminar which, in any event, was confined to ground I had already traversed. I was, however, programmed into a full complement of courses in Chicago-style economics and finance.

With some notable exceptions, much of Chicago's finance research program was built around the Center for Research into Security Prices (*CRSP*). Jim Lorie had arranged the funding of *CRSP*'s New York Stock Exchange monthly data history, which was the core of *CRSP*; Larry Fisher had the computer expertise to make the *CRSP* data base a reality; Mert Miller and Gene Fama provided inspiration and real scholarly leadership; and a whole group of amazingly able students had been attracted to the finance program. Dick Roll, Mike Jensen, Marshall Blume, and Myron Scholes are a few of the better-known graduate students from those heady days. It did not take long for me to be completely seduced by the sheer vitality of the Chicago finance group which, at that time, was rapidly developing lines of research fundamentally at odds with much of the accounting literature to which I had been exposed. Chicago's research workshops in accounting and finance were lively battlegrounds, to say the least.

The second part to this "formative" story is the role of Chicago's Economics Department. I and many of my doctoral program classmates chose Economics as our basic discipline (the alternatives were Mathematics or Behavioral Science). We then trotted off to the Economics Department where we inevitably were schooled in applied microeconomics and given a heavy dose of so-called positive economics, often taught by Milton Friedman himself. The empirical mindset was so dominant in the 1960s that it influenced almost all of the doctoral students' choices of research topics for a generation or more.

My doctoral dissertation was on Miller and Modigliani's theoretical and empirical work on valuation. That is when I learned about Ordinary Least Squares, instrumental variables, two- and three-stage least squares, Zellner's λ -estimators, reverse regressions, and the like. Forgive me for taking this opportunity to quash a rumor that has lived too long and too

dangerously. I did in fact submit my dissertation. Its title is "Some Aspects of Valuation in the Railroad Industry." Moreover, I graduated on the strength of that dissertation, not on other papers which I wrote, sometimes with Ray, at the same time as I worked on my doctorate--papers which, admittedly, I found more interesting than a dissertation. They addressed such topics as the comparative predictive abilities of alternative income measures, market and industry effects in accounting income series, portfolio theory and accounting, and, of course, an empirical evaluation of accounting income numbers. But I am jumping ahead.

Ray Ball came to Chicago in the autumn of 1966 against the advice of Bill Stewart, who, according to Ray, was not particularly pleased with the direction my own interests had taken by then. I am glad Ray came, because he completed the three C's, which contributed to our success: Chicago, *CRSP*, and colleagues. Incidentally, Ross Watts joined the Chicago accounting doctoral program the same year as Ray.

2.2 THE AIM OF BALL AND BROWN [1968]

King [1966] had pointed to clustering of the off-diagonal commonalities in the stock return variance-covariance matrix, due to what he identified as industry effects. Ray and I conjectured that these same commonalities would be present in earnings, being reflected in the "top down" approach to security analysis, for example. We thus chose industry and market commonalities in earnings as our first major project. We quickly realized, however, that it was leading us to the far more important question of the connection between earnings and stock returns. When we presented our first paper at the 1967 *JAR* Empirical Research Conference, so sure were we that it was merely the precursor to a much more important second paper that we were taken aback when a discussant, Thomas H. Williams, did not obviously share our expectations.

Despite its grandiose title, "An Empirical Evaluation of Accounting Income Numbers," Ball and Brown [1968] had a quite modest aim: to test the null hypothesis that accounting income numbers are "not useful" to share-market investors, against the specific alternative hypothesis that they are. In simple terms, we had had an earful, first in our honors course at UNSW and then in our early Chicago accounting doctoral seminar, about why accounting practice was little more than mere ritual. How could accounting reports have survived for so long, we reasoned, if they cost real dollars to prepare yet had no bearing on the wealth positions of the shareholders to whom they were addressed? The fact that reports did not match exactly what this or that accounting theorist prescribed was, and still is, insufficient grounds for rejecting reports prepared under Generally Accepted Accounting Principles.

Once we rejected the null, we then went on to assess what we called "usefulness," in terms of the "relevance" and "timing" of preliminary net income reports. We concluded as follows: "Of all the information about an individual firm which becomes available during a year, one-half or more is captured in that year's income number. Its content is therefore

considerable. However, the annual income report does not rate highly as a timely medium, since most of its content (about 85 to 90 per cent) is captured by more prompt media which perhaps include interim reports.”

2.3 THE PAPER'S IMPACT

Why did our paper have such an enormous impact? I can offer you only my personal view. I have seven reasons.

1. It was cast in the mold of a traditional experiment: question, hypothesis, data collection, hypothesis testing, conclusion. It was strong on other methodological grounds, e.g., in demonstrating robustness to alternative models and variable definitions. If you read the footnotes you will see one of the first empirical applications of Sharpe's Capital Asset Pricing Model, which was used to demonstrate that our results were robust to our choice of the market model to measure abnormal returns. (Interestingly, we reported that a proxy for cash flow did not outperform accrual income.)
2. It expressed a view than ran counter to the critics of accounting, in that it rejected the null hypothesis in a convincing way.
3. It emphasised the use of data to test a belief. As Bill Voss, a fellow doctoral student, often said when one of us took to pontificating, “That's an interesting hypothesis—why don't you test it?”
4. It reflected the then-thinking in finance: the Efficient Market Hypothesis was a maintained hypothesis.
5. It documented an association between earnings and prices that has proved robust over time and different markets.
6. It introduced a focus on income expectations and on announcements which revised those expectations (in contrast to say the Fama, Fisher, Jensen, and Roll classic study on stock splits, which focused on a split's effective date, which is some time after the split's announcement).
7. It developed an approach to measuring the shareholder wealth effects of information of various types.

2.4 WEAKNESSES

Ball and Brown [1968] must also have had its weaknesses, although I am hardly the person to ask to point them out. It was rejected by the editor of *The Accounting Review* on the grounds that it was not suited to an accounting journal. The editor did offer to reconsider the manuscript if Ray and I wished to cut down the empirical stuff and expand the “bridge” we had tried to build between our paper and the accounting literature, when we argued that our null hypothesis was consistent with a common belief expressed in that literature.

While I am on the subject of weaknesses, Ray and I might well have started the habit, which others continued, of drawing attention to securities market anomalies and then proceeding to ignore them. This habit

culminated in Ray's paper (Ball [1978]), where earnings-related anomalies were brought out into the open once again in a forceful way.

That will have to do for criticisms from me. Let me turn now to the literature since 1968 and how it is connected to our paper.

3. The Literature Since Ball and Brown [1968]

I am no historian, so there is no way I can hope to do complete justice to the literature which has referenced Ball and Brown since 1968. My problem is exacerbated by the fact that Ball and Brown [1968] is one of the most frequently cited articles in the accounting literature since at least as far back as 1956.¹ So if you will allow me to make a highly personalized, selective classification, I shall group the empirical literature into eight main areas:

1. shareholder wealth effects of accounting income numbers
2. shareholder wealth effects of other accounting numbers
3. time-series behavior of accounting numbers
4. preannouncement drifts in share prices
5. earnings-related anomalies in share rates of return
6. earnings response coefficients
7. information transfers
8. multiperiod earnings forecasts and share prices

It is a selective classification and I shall take them one at a time.

3.1 SHAREHOLDER WEALTH EFFECTS OF ACCOUNTING INCOME NUMBERS

An early extension of our work was the research into the "cosmetic" effects of accounting changes, an example of which is Ray's paper, "Changes in Accounting Techniques and Stock Prices" (Ball [1972]). Ray argued that, in the absence of cash flow effects, changes in accounting methods that changed the accounting representations of economic events, but not the events themselves, would be "seen through" by security analysts, because they had almost costless access to alternative information sources. This literature was relatively short-lived, because it begged the question of why a firm would change its accounting methods if change is costly and there are no apparent economic benefits.

The more interesting question that has been asked many times since is "What are the economic incentives that lead managers to choose a particular set of accounting policies?" Accounting changes can have cash flow consequences because of their implications for income tax payments,

¹The citation Ray and I received in 1986 for the American Accounting Association's inaugural Seminal Contribution to the Accounting Literature award reads as follows: "No other paper has been cited as often or has played so important a role in the development of accounting research during the past thirty years."

borrowing costs, political costs, and management compensation agreements (although the connection between management compensation and accounting methods is less rigid in Australia than in the United States). This literature, which often is identified with the Rochester school, has had a major impact on the accounting literature since the mid-1970s.

Ball and Brown compared the predictive powers of operating income and net income both before and after extraordinary items. We concluded that the mean abnormal return from a perfect foresight model was greatest for net income inclusive of extraordinary items. Since then, there have been many studies of the “incremental” share price effects of alternative accounting constructs—e.g., net cash flows, lease capitalization, *GPLA* and replacement cost disclosures, and *LIFO* adoptions, to name just a few. Indeed, it is now hard to imagine any proposal to change U.S. accounting reporting requirements that will not have to run the gauntlet of a market-based test as part of the routine evaluation of its economic consequences.²

Ball and Brown [1968] had measured simply the average share price movement for all firms. Others have questioned whether the nature of the adjustment of prices to information is homogeneous. They have related the adjustment to attributes such as a firm’s size, its industry, the time delay from the end of the fiscal year until the profit report is released, or, as in Skinner [1989], whether there are exchange-traded options on the underlying shares. In general, the answer seems to be that there are important and to some extent predictable differences in the adjustment process across firms.

3.2 SHAREHOLDER WEALTH EFFECTS OF OTHER ACCOUNTING NUMBERS

Ball and Brown focused tightly on the bottom line, yet there are many other numbers in the financial statements. The question of their relevance was put (but not answered) by Brown [1970]: “Of what importance are other items contained in the annual report?” In that paper, I proposed that a multivariate discriminatory technique be used to classify reports into “good” and “bad” news categories, based on financial statement data.

Over the years, several attempts were made to answer this question, one conclusion being that there may not be a whole lot more to be said, once earnings had been accounted for. For example, in his [1974] paper, Nick Gonedes looked at six ratios, including working capital/total assets, financial leverage, the asset turnover rate, cash flow to debt plus preferred stock, two measures of the accounting rate of return, and *EPS*. Nick concluded that the numbers, jointly, were uninformative and that, given *EPS*, the other six explained little.

² A key development in the process was Beaver and Landsman [1983], which won the 1985 Wildman Medal.

However, more recently Ou and Penman [1989] have been pushing this line of enquiry considerably further. Using 68 initial accounting variables,³ they have fitted a multivariate logit model first to predict *future* earnings and then to predict *future* stock market returns, with a success rate that has startled many believers in market efficiency!

3.3 TIME-SERIES BEHAVIOR OF ACCOUNTING NUMBERS

The time-series behavior of earnings was perhaps the first major outgrowth of Ball and Brown [1968]. We had referred to the issue as “the statistical nature of the income process, a process little understood but of considerable interest to accounting researchers” [1968, p. 177]. As Brown [1970] explains more directly, “in the absence of explicit knowledge of investors’ *EPS* forecasts they must be simulated by mechanical rules.”

Ball and Brown had used the classical naive model, which predicts that *EPS* this year will be no different from *EPS* last year, to identify good and bad *EPS* news; and an adaptation of the “market model” popularized by Fama et al. [1969], which has embedded in it a submartingale assumption.⁴ Although Little [1962] had concluded that earnings in the United Kingdom seemed to approximate a random walk, it was important that the efficiency of earnings forecast methods be addressed directly. For if the forecast was inefficient, then the earnings forecast error—in essence, the “surprise” component of the earnings signal—would be measured with error, and the association between earnings and share returns would be understated.

Ray and Ross Watts enrolled in a corporate finance class I taught in the summer of 1967 in Chicago’s evening M.B.A. program. I required a term paper from them and admit to steering them in the direction of the time-series properties of *EPS*; whence came Ball and Watts [1972], one of the ten most-cited articles in the finance journals of the 1970s.

Since then, people have gone on to examine and reexamine the question, mostly arriving at the same result: that Ball and Brown’s choice was perhaps fortuitous, but nevertheless a sound one. It went back to a second-year undergraduate statistics course I took from Dr. Sheila Rowley at UNSW, who impressed upon her students the value of genuinely parsimonious models in empirical applications. It is a lesson I have never forgotten.

Interest in the efficiency of time-series forecasts expanded to encompass the comparative efficiencies of security analysts’ and managers’ forecasts. In many applications that are modeled on Ball and Brown’s,

³ Ou and Penman have reduced the 68 initial variables to 28 plus an intercept, thereby giving new meaning to the term “parsimonious model.”

⁴ That is, when the market effect is zero, predicted *EPS* is last year’s plus the average change in *EPS* since the start of the time series.

time-series extrapolations have been replaced by security analysts' forecasts, since they are more efficient.⁵

The time-series literature too has developed a life of its own, far beyond that Ray and I ever contemplated. To illustrate, George Foster, in his book *Financial Statement Analysis* [1986, p. 212], lists six applications of time-series forecasting:

- A. Performance evaluation of management where a key concern is what percentage of the earnings change is due to non-firm-oriented factors;
- B. Examining allegations that management is "manipulating" earnings to (say) avoid violating restrictive covenants in a bank loan agreement;
- C. Designing a "profit-sharing" component of an executive compensation plan where a central concern is risk sharing between management and other parties associated with the firm;
- D. Management decisions on alternative accounting methods where an important factor is the time-series variability in the reported earnings series;
- E. Litigation where allegations of excess profits have been made and the concern is to explain the sources of a reported earnings series;
- F. Litigation where business operations have been disrupted by a fire or a strike and estimation must be made of the earnings that would "normally" have occurred.

3.4 PREEARNINGS ANNOUNCEMENT DRIFTS IN SHARE PRICES

Ball and Brown [1968] noted the preannouncement drift and concluded that, on average, 85% of the earnings-related change in share prices⁶ could be attributed to events that predate the annual *EPS* announcement month. They then asked [1968, p. 177] "of what help are interim (i.e., quarterly earnings) reports and dividend announcements" in explaining the anticipation?

So it was not long before quarterly or interim profit reports⁷ and dividend announcements⁸ were studied in the United States and in Australia.⁹ Clearly, a variety of signals can result in the contents of an annual report being anticipated. It is only to be expected that share prices in a competitive market will reflect the contents of many reports well before their release.

⁵ For example, Brown, Foster, and Noreen [1985] document that analysts' consensus forecasts are on average 50% more accurate than the Ball and Brown naive model when the forecast is made 12 months prior to the announcement.

⁶ Net of market effects.

⁷ For example, Brown and Kennelly [1972].

⁸ For example, Pettit [1972] and Watts [1973].

⁹ For example, Brown [1972].

Since 1968 this line of research has spread to New Zealand, Canada, the United Kingdom, Europe, and Singapore. I have little doubt that, somewhere in the Soviet Union, there is a researcher hoping that *perestroika* will extend to the introduction of a capital market and that one day he will be able to relate share prices to published accounting reports in the U.S.S.R. . . .

One thing we have learned from all the replications and extensions is that the original Ball and Brown experiment is extremely robust.

3.5 EARNINGS-RELATED ANOMALIES IN SHARE RATES OF RETURN

Ball and Brown observed a *postannouncement* drift which, although not particularly significant from a statistical viewpoint, nevertheless was troublesome because of the maintained hypothesis of market efficiency. The drift was observed on average. It need not be true of any one individual company, as the averaging across firms of jumps which occur at random intervals for individual firms can induce an apparent drift, whether it be downward or upward.

This "anomalous" result was one of the first in what is now a whole series of anomalies. Unusual market behavior has been documented for all of the following:

- (a) *Time of Day*. On the NYSE, for example, the mean return at the end of the day is consistently large and positive, which means you should sell, but not buy, at the end of the day.
- (b) *Day of the Week*. Australian stockmarket returns are typically lower on Mondays, Tuesdays, and Wednesdays and higher on Thursdays and Fridays; hence you should buy on a Wednesday afternoon but sell on a Friday afternoon.
- (c) *Day of the Month*. Australian market returns are below the grand mean up to the last two days of the month, when they become positive; thus, you should buy toward the end of the month, but sell halfway through it.
- (d) *Month of the Year*. In the United States you should buy at the end of December, but not sell before the end of January, because of the turn-of-the-year anomaly.
- (e) *Around Holidays*. In Australia, according to Easton [1989], you should sell after holidays but buy before them, because returns are anomalously high around holiday periods.
- (f) *Shares with High Versus Low Earnings Yields*. Abnormally high returns are available if only you can figure out which stocks command low *P/E* ratios. . . .
- (g) *Shares with High Versus Low Dividend Yields*. Or, if you find it easier to calculate the dividend yield, just stick to the high-yielding shares. . . .
- (h) *Small Versus Large Firms*. Finally, as everyone now knows, all

you have to do is figure out which stocks have low market capitalizations and, if you live in the United States, buy them for Christmas!

I'll stop there, but the list is seemingly endless.

This circumstance, recurring phenomena which are present but not understood, has led to a number of papers that explore research design questions, beginning with Ball's [1978] "Anomalies" paper and extending to Bernard and Thomas's [1989] Conference paper, "Post-Earnings Announcement Drift: Delayed Price Response or Risk Premium?" It may be claiming too much to attribute them to questions raised in Ball and Brown [1968], but in a sense they are a natural extension of the postannouncement drift we found so puzzling.

3.6 EARNINGS RESPONSE COEFFICIENTS

There is a burgeoning literature on what has been called "earnings response coefficients"; but it has a long history. That history dates back at least to Miller and Modigliani's [1966] work on estimating the cost of capital, and it illustrates why R^2 is not a helpful way to summarize the association between earnings and prices.

It is easy to find a high correlation between earnings and prices. Simply choose a relatively homogeneous industry with a wide range of firm size, and correlate the capitalized market value of the firm with Miller and Modigliani's variable, $X(1 - \tau)$, which is just net income plus after-tax interest payments. However, as Miller and Modigliani noted, the resultant estimate of the "earnings response coefficient"—they referred to its inverse as "the cost of capital"—may be nonsensical. Standard statistical procedures, such as deflated and instrumental variable regressions, may yield more efficient estimates of the coefficients and at the same time reduce the *apparent* explanatory power.

Ball and Brown [1968, p. 177] referred to this class of question (i.e., earnings response coefficients), as follows: "The relationship between the magnitude (and not merely the sign) . . . could . . . be investigated." And "be investigated" it was!

You might well wonder why Ray and I did not investigate it ourselves. There were two reasons. First, we did not need to, because we were interested in the mean abnormal return from taking a long or short position in every share, and to do this, we needed to know only the sign of the earnings forecast error. Second, we foresaw "some difficult econometric problems associated with this relationship," some of which we spelled out in a footnote.

3.7 INFORMATION TRANSFERS

Ball and Brown, and Beaver [1968], had very limited views of the financial world: we simply did not contemplate a major role for infor-

mation transfers.¹⁰ For example, there was no industry index in the Ball and Brown earnings regression model, and Bill Beaver steadfastly refused to throw out any observations other than those for which the firm itself made an announcement. So it has been left for others to go down the information transfer track, one that has been trodden well by Australians, including Richard Morris [1980], George Foster [1981], and Greg Clinch and Norm Sinclair [1984]. George Foster [1981], for example, reported that earnings information transfers contributed to positive covariances between the returns on shares of firms in the same industry. To quote Foster again: "Those earnings releases that were associated with the largest increase in security return variability for the announcing firm also were associated with the largest increase in security return variability for the other firms in its industry. Further analysis revealed that earnings releases that were associated with positive/negative price changes for the announcing firm . . . were also associated with positive/negative price changes for the other nonannouncing firms in the same industry. . . . Clinch and Sinclair [1984] report similar results for a sample of 328 announcements by 47 Australian firms in ten industries in the 1977-81 period" [1986, pp. 388-89].

What we seem to have learned so far is that *EPS*-determined information transfers are a second-order phenomenon, a result which George Foster, Greg Clinch, and I are finding holds up at the transaction level as well.

3.8 MULTIPERIOD EARNINGS FORECASTS AND SHARE PRICES

The last strand I wish to mention is multiperiod modeling of the relationship between earnings forecasts and prices. This literature is small and, I believe, undeservedly so. I am referring to the Brown, Foster, and Noreen [1985] AAA Monograph no. 21 and to a paper by Brad Cornell and Wayne Landsman [1989], who explore issues raised in the Brown, Foster, and Noreen monograph.

Brown, Foster, and Noreen asked the question, "Is the market myopic?" Managers have been criticized for acting as if short-run market considerations were paramount. Managers act this way, so the argument goes, because portfolio managers have developed a short-term focus and pressure managers into acting against their long-term interests.

The monograph tried to answer the question by seeing if changes in short- or long-term *EPS* forecasts were more closely associated with stock price movements. Far from finding evidence of market myopia, we found strong evidence of a dominant long-term focus. One possible explanation for this result is that shorter-term forecasts are recognized by analysts as being influenced relatively more by transitory components,

¹⁰ "Information transfers" refer to the process whereby information about one firm has value implications for other firms in the same industry.

whereas longer-term forecasts are more allied in concept to "permanent" earnings and have a closer association with share prices.

Academics are interested in sorting out transitory versus sustainable earnings, to borrow an analysts' phrase. So too are the analysts, and their short- and long-term forecasts are available.

4. Prospect

Let me now turn from my admittedly selective interpretation of the post-Ball-and-Brown [1968] literature to give what is an even more selective view of some likely directions that capital-market-based accounting research will take over the next few years. I have seven of them.

4.1 DERIVATIVE SECURITIES

Patell and Wolfson [1984] used option prices to infer that most of the adjustment of prices to information releases occurs within the first two trading hours, although detectable traces "linger into the following day" [1984, p. 240]. Skinner [1989], in a different tack again, reports that profit announcements have less effect on share prices if options are traded on those shares than if they are not.

Derivative securities, however, have a much wider interest to accountants. To illustrate, they underlie many executive compensation schemes, yet the time value of the option is ignored in accounting for management costs; they affect the balance sheet classification of equities, yet convertible securities may be treated as pure debt; they have been used via put options to manipulate profits from certain well-publicized Australian property transactions; and they are relevant to such contracts as interest-rate swaps and currency hedges, which do affect credit ratings but need not be capitalized.

Derivative securities raise so many accounting issues and the research agenda is so rich that my mind positively boggles.

4.2 MARKET MICROSTRUCTURE

Over the years we have had access to market data for ever-shorter trading intervals: yearly, monthly, weekly, daily, transaction by transaction, and now bid and ask prices in real time. There are some major challenges in the use of the latest data sets, including modeling market equilibrium conditions as well as designing efficient computer-processing methods.

Market microstructure is concerned with details of the trading process, such as the generation and spreading of information, patterns in order arrivals, the distribution or price changes, the variance-covariance structure of security returns, the impact of regulation or deregulation, the institutional rules which translate orders into trades, price pressures exerted by block trades, and so forth. Although interest in these issues

dates back at least a quarter of a century,¹¹ there is an accelerating awareness of the potential implications of market microstructure for making valid inferences about market behavior, including the adjustment of prices to information as it enters the public domain.

Share-markets, such as the New York Stock Exchange (NYSE), are often held out as everyday examples of perfectly competitive markets. While in many cases it may be reasonable to assume away from the NYSE the frictions that characterize most markets, in some cases, particularly when we get down to the transaction level, we simply cannot ignore them.

Market microstructure is thus becoming a key issue as violations of the assumptions of competitive markets become more crucial in experimental design. I am sure we shall see an explosion of interest in microstructure considerations in security market research, as transaction data become more widely available.

4.3 INFORMATION TRANSFERS

Information transfers have not been exploited fully, by any means. At issue is explaining the variance-covariance matrix of abnormal security returns.

I expect to see much more work done on accounting data at a less aggregated level than net income. For example, dollar sales are often modeled in corporate plans as the net result of market size, market share, product life cycles, and a pricing policy. Clearly, one firm's actions and performance can impinge on other firms along each of these sales dimensions. A potentially richer way to model information transfers would be to disaggregate the earnings signal into key components.

4.4 SHAREHOLDER WEALTH EFFECTS

It is unlikely that any new major financial reporting method or mandate will be introduced without it being subject, at some stage, to a market-based study of its wealth effects. In the United States, a hot research agenda item is accounting for the cost of, and liability for, employees' postretirement benefits other than pensions.¹² According to some commentators,¹³ between 400 and 1,000 billion U.S. dollars of unfunded, off-balance sheet liabilities are not being recognized because such costs are being accounted for on a pay-as-you-go basis. I have no doubt that we will read, and some of us maybe will also write, much about this phenomenon in the next few years.

4.5 "QUALITY" OF EARNINGS

Baruch Lev [1989] has called for more research into the "quality" of earnings, and he has a point. We know that analysts make ad hoc

¹¹ For example, Stigler [1964].

¹² Current and future pensioners' medical bills are a prime source of the liability.

¹³ *Australian Financial Review* (April 17, 1989), p. 36.

adjustments to P/E ratios for various matters, including the “quality” of earnings.¹⁴ So far we have done little in security market research applied to accounting questions to look behind the accounting numbers, preferring rather to adopt the view that we are studying the numbers as reported. If I may quote Baruch: “Research on the quality of earnings shifts the focus to an explicit consideration of accounting issues by calling for a systematic examination of the extent to which the specific principles underlying accounting measurements and valuations, as well as managerial manipulations, detract from the usefulness of earnings and other financial variables. Such research has the potential both to further our understanding of the role of financial information in asset valuation and to contribute meaningfully to accounting policymaking” (Lev [1989, p. 176]).

4.6 GLOBALIZATION OF CAPITAL MARKETS¹⁵

International capital markets are now highly integrated. The same securities are often listed on exchanges in several countries and professionally managed portfolios are diversified internationally. And there is much interest in establishing electronic linkages to create a global capital market. These trends must affect our research agenda.

A prime candidate for research is explaining differences in the P/E ratios of companies listed in different countries. An article in the July 17, 1989 issue of *Business Week* (p. 141) tabulated P/E ratios in ten countries, ranging from 100 in Japan (reported to have the largest market capitalization) down to 10 in Australia (which had the smallest capitalization of the ten countries). The P/E ratio in the U.S. was reported to be 21, while the market capitalization of the U.S. was about two-thirds that of Japan.

Are these differences in P/E ratios explained by differences in growth rates, in the quality of earnings, in leverage, in inflationary expectations, in tax regimes, in real returns, or in relative risk? There is a long list of possible explanations which can be probed, now that the necessary data are becoming available. An upsurge in research related to global trading could lead to more substantive interaction between accounting researchers in different countries.

4.7 MARKET EFFICIENCY

Then there is that old chestnut, “Was the security market efficient with respect to . . . ?”—and you can fill in the blanks. There are so many “anomalies” around nowadays that I sometimes wonder if there are more anomalies than instances of efficiency.

However, there is enough disagreement about how prices should behave in an efficient market that I fear the efficiency question will be around for many a year yet. The argument will presumably be sealed for me

¹⁴ See, e.g., Beaver and Morse [1978].

¹⁵ This discussion was added at George Foster's suggestion.

when proponents of inefficiency, such as my good friend Steve Penman at Berkeley, give up their family station wagons and take to traveling in private jets. Until then, I am afraid my Chicago training has left me too skeptical to believe that competitive capital markets could remain so obviously inefficient for so long.

5. Concluding Remarks

It is now 22 years since Ray and I wrote the first draft of Ball and Brown [1968], and it has been a hard act, for me at least, to follow. Nevertheless we both plug on, each in the hope that he still has something to discover, something to say.

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