Underreaction

David L. Ikenberry
and
Sundaresh Ramnath

Jones Graduate School of Management
Rice University
6100 S. Main Street
Houston, Texas TX 77005-1892

e-mail: daveike@rice.edu
ramnath@rice.edu

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Abstract

In the last decade, an emerging body of empirical literature examining self-selected corporate news events makes the observation that markets appear to underreact. Other recent papers have tried to provide theoretical frameworks to explain why or how underreaction might occur in markets. However, the notion of underreaction is contentious. Concern over empirical research has focused on two issues - spurious results from unusual time periods and/or misspecified return benchmarks or methods. In this paper, we revisit the issue of underreaction by focusing on the simplest of corporate transactions, the stock split. Using a matched-control firm approach, we continue to find abnormal returns of 9% in the year following the stock split announcement for a recent sample of cases. The results suggest that the conclusions in previous studies from earlier time periods are not spurious. However, the notion of underreaction fundamentally concerns the revision of expectations to news. Therefore, we next examine how earnings expectations are revised subsequent to this type of news event. If the positive drift reported in this study and elsewhere is attributable to benchmark problems one might not expect to find surprising results with respect to revisions in earnings expectations. Yet, this is not the case. Instead, we find that the revisions in earnings forecasts are just as sluggish as stock prices. This gradual revision in earnings expectations explains a significant portion of the drift in abnormal returns. At least with respect to this most simple of news events, it would indeed appear that markets underreact to news.

JEL classification: *G14, M49*

The mechanism through which information is transmitted into stock prices has come under scrutiny in recent years. Early foundations of modern finance presumed that the valuation impact of news was transmitted to the market through buyers and sellers revising their expectations about future firm performance. This, in turn, changed the risk-adjusted value of the firm which eventually became reflected in market prices. This transmission mechanism was argued to operate in both a rapid and unbiased manner and motivated the term "efficiency." Of course, the notion of informational efficiency has never suggested that markets are somehow clairvoyant. No supposition is made as to the absolute degree of precision with which prices should respond to news in any one case. Indeed, because of the continual noise prevalent in markets, one cannot be surprised to find spurious indications of pricing error in many situations, even when, on average, expectations, and thus prices completely react to news.

However in the last decade, a broad-class of papers seriously challenges the notion of informational efficiency. These papers raise questions as to the completeness of the immediate market reaction to news. An extensive body of empirical literature examines a wide-ranging set of specific news events and finds, with rather striking consistency, that markets appear to initially underreact to news. While not true in all cases, positive news events are generally met with a positive market reaction. Yet returns subsequent to the announcement show positive, long-horizon abnormal drifts. Similarly, negative news events are generally met with a negative market reaction and are subsequently followed by negative long-horizon abnormal return drifts.

While numerous concerns have appropriately been raised about the power or quality of these empirical studies, the primary objections that strike to the core of this literature typically fall in two broad areas. First, these papers reiterate concern over becoming errantly excited over spurious results. Given our bias as researchers to explore interesting findings, we run the risk of collectively mining data and circulating spurious findings when in fact this research commits a type one error; rejecting the null when in fact it is true (Merton (1985)). A second over-arching criticism is that the mounting evidence of underreaction is due to the absence of a robust asset-pricing model. In recent years, researchers have been thrust into using ad-hoc models that while having power in explaining cross-sectional stock returns,

are not motivated by positive theory. These models take a variety of forms. Without guidance from theory, one is left to question whether these ad-hoc approaches address all systematic sources risk; a concern that for the true skeptic can never be fully assuaged. Thus arises the famous joint-test hypothesis problem.

To some extent, this concern is reduced when the entire portfolio of underreaction events is assembled. Here, the benchmark problem becomes one of suggesting that not one, but perhaps several factors with cross-sectional power (factors still unknown to the profession) has extreme shifts in exposure that somehow affect asset prices in a manner that leads to the appearance of underreaction. Responding to these concerns is not always straightforward. Absent a coherent model, a conservative approach is to account for as many factors as possible that, to date, have demonstrated cross-sectional power in explaining returns. While this approach conceivably errs in "over-explaining" the sources of returns to various factors (Loughran and Ritter (1999)), it conservatively addresses whether drifts that are observed can be attributable to known empirical relations.

In this paper, we examine this broad question of underreaction by narrowly focusing on the case of stock splits. Of all the possible corporate events where researchers have observed potential underreaction, this particular announcement is perhaps most interesting because of its utter simplicity. Unlike most corporate news events, the split announcement is one situation where the event itself has little or no causal properties that affect the firm in any material way. As such, the impact of a stock split is restricted to the domain of investor expectations about future performance. By following a "cleaner" type of information event, we hope to focus attention on the extent of underreaction and be less distracted by concerns over changing cash flows or risk characteristics. Among the various announcements one might examine, stock splits are rather unique in this regard.

Similar to earlier work on underreaction, we again extensively examine long-horizon returns subsequent to stock splits. Previous papers report anomalous long-horizon returns in the 1970s and 1980s. In this paper, we look at out-of-sample results that focus on the 1990s. To measure abnormal performance, we use a rank-order searching technique that matches a control firm to each split firm on the

basis of market-cap, value/growth, and momentum. To control for concerns of liquidity, we also match on nominal share price.

To focus on the issue of whether the underreaction in stock returns is explained, at least partially, by slowly revised expectations, we shift attention to the earnings expectations of Wall Street analysts. Presumably, these astute observers of financial information have incentives to revise their earnings forecasts to reflect any information that a stock split might convey. Such an examination fundamentally strikes to the heart of the issue with respect to underreaction. If markets underreact, one would hypothesize that earnings expectations should revise slowly, in a manner consistent with the subsequent sluggish price performance observed in previous studies. Conversely, to the extent that flawed return benchmarks are the culprit, one hypothesizes that earnings expectations should be unbiased.

Our results are summarized as follows. Using stock splits from 1988 through 1997 announced by NYSE, ASE and NASDAQ firms, the drift following a split announcement during the 1990s is strikingly similar to results reported in other studies for earlier time periods. Over the year following a split announcement, the mean abnormal return for sample firms is 9.00% (t=7.93). Here, the overall median abnormal one-year return is 6.31% (p-value < .0001). The mean portfolio return apparently is not a consequence of a handful of right-skewed returns, a point sometimes voiced by those concerned with this literature. These findings are generally stable across various dimensions and are not focused in smaller, less widely traded stocks. For example, while mid-cap and small- stocks show evidence of positive drift, even the largest quintile stocks in our sample show significant abnormal performance. Moreover, the results do not appear to be too sensitive to momentum. Between value and growth stocks, no real pattern in abnormal performance is evident.

Turning to earnings expectations, we see evidence that expectations adjust gradually to the news of a stock split. We focus on analysts' forecasts pertaining to the next release of annual earnings and observe how this forecast changes in response to a split announcement. When we compare the forecasted earnings for the split and associated match firms as a percentage of their actual earnings, we find that ten-days prior to a split announcement analysts tend to relatively *underestimate* future earnings of splitting

firms by 7.67%. Ten-days after the split announcement, this gap between the forecasts of splitting and match firms drops to 7.08%. Over the next few months, analysts' expectations for the split and match firms converge to their actual values. This revision process appears to stabilize roughly three months prior to the earnings announcement, when the earnings forecast for the split firms are 3.03% below the forecast for match firms. However, even three days prior to the actual earnings release, earnings expectations for the split firms are 2.68% below the expectations for their matches. Similar results obtain for various sub-sets of this sample as well.

In short, the evidence, at least with respect to stock splits, is consistent with the notion that markets underreact to news. Our findings of underreaction by Wall Street analysts are also consistent with recent theoretical papers such as Daniel, Hirshleifer and Subrahmanyam (1999) who suggest that analysts overweight their own priors when valuing firms, and thus underweight new information as it arrives.

The balance of the paper is organized as follows. In section I, we briefly review the empirical evidence on underreaction and motivate our choice in this study of re-examining out-of-sample evidence for stock splits. Section II discusses how we formed our sample and how we identified control firms. Section III reviews the evidence on long-horizon abnormal returns subsequent to the split announcement. Section IV examines the evolution of earnings forecasts subsequent to the split announcement. In section V, we provide some robustness checks and in section VI, we summarize the paper.

I. The evidence on market underreaction and our focus on stock splits

a. The evidence

Over the last decade, the empirical literature focusing on long-horizon stock returns has grown substantially. A unifying observation in much of this work is that the initial market reaction to a given news event is not complete. Generally speaking, these corporate events can be sorted into two classes. The first set consists of self-selected events where corporate insiders choose to execute a given transaction at a particular point in time. This class of events is interesting because the joint decision of

both if and when to execute a given transaction is at the discretion of management, individuals who may have private insight into the firm's true value and future prospects. The second class of events is non-self-selected. The timing and execution of these events is at the discretion of outsiders to the firm. Although these events may be motivated by decisions insiders may have previously made, they are not conditioned on management's knowledge about the firm. While long-horizon return drifts have also been observed for many non-self-selected events, we focus our discussion here on self-selected events because of their endogenous nature.

Among the set of self-selected events, one of the earliest papers to examine long-horizon performance that received widespread attention was Ritter (1991). That paper reports that managers appeared to be "timing" the market at a relative peak when initially issuing stocks as subsequent long-horizon abnormal returns were negative. Subsequent studies by Loughran and Ritter (1995) and Spiess and Affleck-Graves (1995) reported similar long-horizon findings for seasoned equity offerings. Because market prices could be measured prior to this type of offering, the evidence leaned further toward managerial timing and market underreaction to the news of an offering. In fact, aggregate flows of equity offerings appear to have predictive power for overall market returns (Baker and Wurgler (1999)), thus giving some merit to the notion of the "window-of-opportunity" when companies choose to issue stock.

The transaction that complements equity offerings is an equity repurchase. Lakonishok and Vermaelen (1990) examine long-horizon returns subsequent to fixed price tender offers. For open market stock repurchase announcements, Ikenberry, Lakonishok and Vermaelen (1995 and 1999) report evidence of positive long-horizon returns in the U.S. and, more recently, in Canada as well. This evidence is consistent with the notion that in at least some of the repurchase cases, managers are just as sensitive to underpricing as their counterparts seem sensitive to overpricing. Focusing on value-stocks that announced share repurchases, abnormal performance was particularly high.

Another self-selected event concerns the initiation of dividends, where managers may be signaling confidence. Here, Michaely, Thaler and Womack (1995) find evidence of positive drifts subsequent to dividend initiations. Another self-selected event is the spin-off; a transaction often

motivated to “unlock” value that is otherwise not priced by the market. Miles and Rosenfeld (1983) and Cusatis, Miles and Woolridge (1993) find evidence of positive drifts subsequent to these announcements.

The list of negative return drifts, where managers may be responding to perceived over-pricing, is longer. While these events are also self-selected, here theoretical stories about managers choosing to intentionally signal, of course, carry much less significance. An early paper in this regard is Agrawal, Jaffee and Mandelker (1992) who report negative long-horizon abnormal returns following mergers. Recently, several papers including Loughran and Vijh (1997) and Rau and Vermaelen (1998) extend this work and find that these negative drifts are associated with equity deals, particularly equity deals done by growth companies where managers may be using "overvalued" stock as currency in a given transaction. Other negative self-selected events include dividend omissions (Michaely, Thaler and Womack (1995)), and exchange listings where firms (particularly small- and mid-cap firms) move from one trading market to another (Dharan and Ikenberry (1995)). Recently, new evidence suggests that managers may also be timing the issuance of bonds (Spiess and Affleck-Graves (1999)).

b. Why stock splits

Stock splits are another corporate event also self-selected by managers as to if and when they occur. Among all the possible events one might focus on, the stock split is appealing because it is one of the few corporate decisions that does not directly affect future cash flows or firm risk characteristics. By contrast, the cases mentioned above tend to have ramifications in one or more areas like operating cash flow efficiencies, capital structure, internal capital allocation, managerial incentives or tax liabilities. Because of these dynamic changes around and following these events, concern may exist as to how well the market can be relied upon to digest this more complex information. Specific concerns are also raised as to the impact of these events on the benchmark used to measure long-horizon returns. Recently, for example, the negative drift subsequent to equity offerings has come under reexamination. Eckbo, Masulis and Norli (1999) claim that the new-issue puzzle surrounding equity offerings arises because this particular event reduces leverage-risk and improves trading liquidity. To the extent that these two factors are priced, it may lower the required return for these types of firms. Similar arguments might also be

made for a wide variety of transactions that affect either operating cash flows or financial characteristics of the firm.

Stock splits are thus intriguing because their direct impact on firm cash flows seems negligible. While debate continues as to why managers choose to split their stock, most studies theorize that managers are conveying positive signals to the market. One line of reasoning is that managers are intentionally trying to convey news through the transaction (for example, Brennan and Copeland (1988) and Brennan and Hughes (1991)). However an alternative literature has suggested that these signals may be unintentional and instead splits occur as a consequence of management's desire to solve some other problem. Specifically, managers may split their stock in order to preserve a trading range (for example, Grinblatt, Masulis and Titman (1984) and McNichols and Dravid (1990)).[1] For example, managers may use splits to adjust the market prices downward to keep certain classes of investors (like individuals, for example) from being priced out (Schultz (1999)). Yet, not all firms with high prices may choose to split. In some cases, managers may not be so confident of future growth. Here, managers may have concerns that if they were to split, the firm might encounter situations where a lower price threshold could be violated, thus perhaps imposing some cost on the firm. These managers may choose to refrain from splits. As such, the observed split announcements are indirectly informative, thus motivating the initial positive market reaction to the news of a stock split that has been reported in so many papers.

The question here is whether this initial reaction is complete. Previous studies such as Ikenberry, Rankine and Stice (1996) and Desai and Jain (1997) find evidence of positive long-horizon drifts following split announcements. These papers, however, are not the first to report a drift in post-split stock returns. Nearly 15 years ago, Grinblatt, Masulis and Titman (1984) provide some indication that a post-split announcement drift may be present. Despite all of the varying methods and time periods used in these studies, some researchers have expressed reservation about the evidence specifically relating to

[1] For further discussion of the trading range story, see Ikenberry, Rankine and Stice (1996). They evaluate relative prices before and after two-for-one stock splits and find that the majority of split firms move share prices from extremely high levels to slightly below the median price, a result consistent with preserving a trading range.

stock splits (Fama (1998)). We try to addresses some of these issues using out-of-sample evidence, a variety of estimation approaches and finally by assessing how expectations, as proxied by earnings forecasts, appear to be evolving over time.

One might question why we also do not consider stock dividends. On the surface, these events seem to be mini-versions of stock splits. It is not clear that this really is the case. For whatever reason, the accounting treatment for the two procedures is quite different. Stock dividends can dramatically decrease a firm's retained earnings balance and thus affect numerous accounting ratios, performance metrics and covenants. Splits have no impact in this regard (Grinblatt, Masulis and Titman (1984) and Rankine and Stice (1997a). While financial economists are often wont to discard accounting nuances, this differential impact on retained earnings may be non-trivial. Moreover, there is some evidence the market even responds differently to cases where managers can choose between the "easy" accounting treatment afforded stocks splits as opposed to the "hard" treatment that stock dividends receive (Rankine and Stice (1997b)). Because of this apparent dissimilarity, we look only at splits. While clearly stock dividends arguably have signaling properties, focusing more narrowly on splits gives us greater clarity in examining the issue of underreaction.

II. Sample and Methods

a. Sample

During the period 1988 to 1997, the population of stock splits of 5-for-4 or greater announced by NYSE, ASE and NASDAQ firms totals to 4,154 cases. We start with 1988 because even though the I/B/E/S Detail Tapes go as far as back as 1983, their coverage in the early years is limited. The most recent CRSP tapes end on December 31, 1998 and therefore we end the sample with announcements made in 1997 to permit measurement of returns for the year after the split announcement. Although the time period in this study partially overlaps with previous studies by Ikenberry et al. (1996) and Desai and Jain (1997), the final six years in this sample (1992-97) have not been previously examined and thus serve as a convenient hold-out sample.

Of the total population of split announcements, 3,028 announcements had sufficient data required by our matching procedures (discussed in the next section) and thus comprise our sample.[2] Table 1 provides details by year and by split factor for our sample. Roughly half the cases are for two-for-one or more. Moreover, our sample is slightly tilted towards more recent observations.

b. Methods

Post-split abnormal returns for split-announcing firms were measured based on the matching control firm approach as a benchmark (Barber and Lyon (1996, 1997)). All firms that had not split their stock in the previous 12 months were included in the pool of potential matches for every splitting firm. Based on previous research we control for market-cap, value/growth, and momentum by finding a match firm that is similar to the splitting firm along these dimensions. For every stock split announcing firm in our sample the match firm is selected using the following procedure. First, using the market value of all NYSE firms at the end of the month prior to the split announcement, all firms are assigned to one of five market-cap quintiles. Each market-cap quintile is further divided into five quintiles based on the prior 36-month return of all firms in that group. And finally, within each market-cap by three-year return group, firms are further classified into quintiles based on their 12-month return prior to the split announcement. Thus a firm that announces a stock split in any month could be assigned to one of 125 (5x5x5) portfolios based on its market-cap, three-year return and one-year return.

Each split firm in our sample is matched to a control firm. Control firms are required to be in the same (market-cap x three-year return x one-year return) group as the sample. As may be expected, most of the splitting firms have multiple matches. When a firm has more than one match, the following procedure is used to pick the best match. First, based on the stock price distribution of all NYSE firms, the (post-split) stock price percentile of the split and all potential match firms is determined as of the previous month end. To control for potential differences in liquidity due to nominal share price, all matches that are within 5 percentiles (on either side) of the sample's price percentile range are included in

[2] Most of the firms lost at this stage were firms having less than 36 months of stock returns on the CRSP tape prior to the month of split announcement.

the first round of potential matches.[3] To identify the one match firm, we use a rank order procedure. All eligible candidates are ranked from 1 to n (n being the number of first round matches) based on the closeness in value between the sample and the match firm on each of the three matching dimensions (market-cap, three-year return, and one-year return). Ranks are summed across all three categories and the firm with the lowest cumulative sum is picked as the match firm for that splitting firm. If for any reason the first match ceases to trade at a given point in time, the firm with the second lowest sum of ranks is used and so on. For example, if the first match has a stock split within 12 months of the (sample's) split announcement date, then the second match is used as a replacement from the day the first match made its stock split announcement. This procedure ensures that there is no look-ahead bias in the matching procedure.

Table II presents descriptive statistics for sample and control firms along the matching dimensions of market-cap, three-year return and one-year return. Overall, sample and match firms have similar matching attributes especially in terms of median values. The stock split announcements are fairly spread over all market-cap quintiles. However, as may be expected, most of the splits are associated with growth stocks (quintile five in the three-year return category) and to some extent high momentum stocks (quintile five in the one-year return category).

III. Long-horizon abnormal stock return evidence

a. The overall evidence

Table III reports one-year abnormal returns for the overall sample of 3,028 split announcements. The mean total return for sample firms is 23.29%. This contrasts with the total return for the matched-control firms of 14.29%. The difference of 9.00% is both economically and statistically significant (t=7.93).

Long-horizon returns tend to exhibit positive skewness. While the matching firm approach

[3] A further benefit of matching on post-split price is that the match firm itself is less likely to be a candidate to split its own stock in the near future.

mitigates this issue (Barber and Lyon (1997)), we nevertheless consider two approaches to reduce the impact of outliers in our analysis. A simple technique is to examine median returns. Median returns pose some problems when considering questions of efficiency because of the inconsistency this statistic poses for ex-ante trading strategies. However, medians allow some sense of robustness of the abnormal performance and thus we consider them here. The overall median paired difference is 6.31% and the p-value for the Wilcoxon signed-rank test is less than .0001.[4]

Other approaches for handling skewness involve some ex-post alteration or truncation of the data. However, such an ex-post remedy does not reflect the performance that one expects in a real-time, ex-ante trading environment and as such is not entirely consistent with the context of this paper. Thus we consider an alternative approach that is consistent with what investors might implement in real-time; an approach we label real-time truncation. Here we monitor, on a daily basis from the initial investment date, the excess return of each firm in our sample relative to its paired-match firm. At any given point in time when that paired difference exceeds 100%, we assume that the position is liquidated and the return for the remainder of the year is set to 0%. Using this ex-ante approach, we eliminate all our extreme winners. Yet, we retain all the losses that might be generated from extreme, right-skewed returns coming from a short position in the matched-control firm. For the sake of completeness and as a final check, we also report ex-post evidence by trimming the extreme high and low return differences in our sample to their respective 99% and 1% values.

The evidence using both the real-time, truncation approach as well as the winsorized results is similar. Removing only the extreme winners under the real-time truncation approach does not materially affect the results. The point estimate of the mean abnormal return falls slightly from 9.00% overall to 8.26%. However, eliminating these extreme winners also reduces the standard error; thus the statistical significance is roughly the same. When both the right- and left-tails are trimmed through winsorization, the mean and median abnormal returns are largely unaffected.

[4] Because of the non-parametric nature of this approach, the median paired difference in returns does not equate to the difference in median total returns.

Apparently, the positive drift that Desai and Jain (1997) and Ikenberry, Rankine and Stice (1996) observed for stocks splits in the 1970s and 1980s is not so unique. Abnormal performance of similar magnitude is evident for splits announced in the 1990s as well.

b. Consistency

In this section, we report abnormal performance for various partitions of the sample to examine the pervasiveness of the positive drift. We begin with Table IV by reviewing the evidence across various years in our sample, across the three trading markets and finally by the various split factors.

Because of our interest in evaluating I/B/E/S earnings forecasts, our sample starts in 1988 and thus overlaps with evidence reported by Ikenberry, Rankine and Stice (1996) whose sample ends in 1990 and also with evidence reported by Desai and Jain (1997) whose sample ends in 1991. Both studies find evidence of positive drift during the first year after a split announcement. We see confirming evidence of this in the first panel of Table IV for the sub-periods 1988-89 and 1990-91. However, we also see positive drift in each of the subsequent sub-periods as well. There is little evidence, at least for this sample, that the drift subsequent to split announcements is receding. In fact, the mean and median abnormal returns for the most recent period in our study, 1996-97, are quite similar to the respective mean and median numbers for the entire ten-year period.

The drift is similar for both ASE and NYSE firms, 7.36% (t=2.02) and 7.63% (t=5.05) respectively. For NASDAQ stocks, the point estimate is a little higher,10.27% (t=5.90). One concern might be that our matching approach somehow does not adequately control for differences in returns across exchanges. Reinganum (1990) points out that returns to NASDAQ stocks are generally lower than similar NYSE stocks, thus posing concern about inflated abnormal returns for some of the non-exchange matched NYSE firms. However, Loughran (1993) points out that much of this inter-market difference is driven by the comparatively higher prevalence of initial public offerings among NASDAQ stocks. Fortunately, we impose a three-year seasoning requirement on both sample and matching control firms and thus mitigate at least a portion of this IPO bias.

In the third panel, we see that abnormal performance is evident across the various split factors.

Two-for-one splits are the single most prevalent split factor in our sample and show the lowest point estimate for mean abnormal performance, 6.75% (t=3.94). Split factors less than and more than two-for-one show mean abnormal performance of 10.40% (t=6.58) and 13.74% (t=2.66) respectively.

In Table V, we examine the evidence across the same dimensions that we initially controlled for when identifying match-control firms. While one can never fully allay questions about the quality of the benchmark, we can at least examine whether our findings are driven by unusual sub-sets of firms. The first sub-panel reports abnormal returns by market-cap quintile defined relative to only NYSE stocks. While mean and median abnormal performance is positive and significant for the smaller three quintiles, abnormal performance is not limited to only small firms, a concern voiced in the literature in recent years. For example, even the largest firms (in quintile 5), a set of stocks extensively followed and traded by institutional investors, show evidence of abnormal performance with match-adjusted returns of 4.42% (t=2.25).

Firms which announce stock splits overwhelmingly are classified as growth stocks. In our case, roughly two-thirds of the sample is classified in the highest growth quintile. Not surprisingly, the mean and median abnormal performance for quintile 5 is comparable to that observed for splits in general. Moving toward the other extreme, sample size declines rapidly; thus our estimates of abnormal performance for these quintiles is noisy and statistical power is weak. Nevertheless, the point-estimates suggest a positive drift throughout the value-growth spectrum.

Firms which announce stock splits also tend to have high one-year return momentum. These cases are interesting to examine for these stocks tend to be in the news and draw substantial attention from both investors and analysts. Thus, one might expect to find excess performance primarily in the low momentum quintiles. Yet, in fact, abnormal returns are highest in the high-momentum quintiles 4 and 5 where mean abnormal performance returns is 10.28% (t=5.30) and 10.12% (t=5.02) respectively.

So far, it is not clear from any of these partitions that our evidence is driven by a handful of stocks with peculiar characteristics that are inadequately controlled for by matching firms. Nor do our results appear to be driven by obscure firms that are difficult to trade.

IV. Earnings and earnings forecast revision evidence

Firms which announced a stock split, at least during the period 1988 to 1997, appear to have unusually favorable long-horizon stock returns. In this section, we begin by looking at realized earnings for these companies after the split to get some sense of their relative operating performance. However, stock prices are driven by expectations about future operating performance. Using only stock returns, the conclusion from the previous section suggests that market expectations tend to revise slowly over time. Thus, next we try to directly address this issue by evaluating analysts' earnings forecasts following stock splits. Of course, analysts' forecasts for next year's earnings are probably not a perfect proxy for the market's overall expectation about the future. Moreover, next period's earnings are but one element in the future stream of cash-flows that investors need to consider. Yet, analysts earnings forecasts clearly affect market prices (Womack (1996)). Moreover, these analysts are conceivably engaged in providing some indication of future performance. Perhaps using this data, we can at least objectively evaluate whether this subset of influential market participants shows any systematic bias in its expectation of future operating performance and, if so, see whether it is consistent with the drift evident in stock returns.

Of course, we are not the first to consider these issues. Several papers, including Lakonishok and Lev (1987) and McNichols and Dravid (1990), report evidence on earnings growth and earnings expectations following stock splits. We more thoroughly explore this evidence including the evolution of expectations using more recent data.

To do this, we form a sub-set from our original sample. We begin by looking for cases where both the split firm and its corresponding match have earnings forecasts available on I/B/E/S for the next fiscal year-end. In cases where the next annual earnings announcement is within 125 trading days of the split announcement (roughly six calendar months), we jump ahead to the following fiscal year. This requirement provides us with at least some time to examine the evolution of earnings forecasts after the split announcement.

From our original dataset, we obtain a sub-sample of 948 firms over the period 1988 to 1997 that satisfy the I/B/E/S data requirements. For this group, we obtain their actual operating earnings in the year

prior and following the stock split normalized by price at month-end prior to the split announcement. We use I/B/E/S as our source for realized earnings. I/B/E/S reports actual operating earnings before unusual items. This allows us to make a cleaner comparison of performance between periods. More importantly, this measure is consistent with the target that analysts are trying to forecast.

Table VI reports growth in realized earnings yield for this sub-sample of firms. Split firms are doing well, a result not unexpected given our previous findings. The mean change in earnings yield from before to after a stock split announcement is 1.18%. Nearly 85% of split firms here show positive earnings growth. Interestingly, the matching control firms (which are not intentionally matched on earnings growth) also seem to be doing well. Here, the mean matching firm shows earnings yield growth of .92%, and 73% of these cases are positive. For both sets of firms, earnings growth is strong, yet the difference between the two groups is not so impressive. While, the mean difference in earnings yield growth is .26%, the median difference is only .14%. Only slightly more than half the paired differences are positive. This result is consistent to some extent with Lakonishok and Lev (1987). They form control firms on the basis of industry and size and also report only a modest difference in earnings growth between a sample of split and control firms.

For comparison, we report concurrent growth in earnings yield evident in the S&P industrial index matched in time to each of our cases. Here, we see a more compelling case for earnings growth in both split and matched control firms. Overall, mean earnings growth for both sets of firms is roughly two to three times greater than that observed in the market overall.

Figure 1 plots the distribution of changes in earnings yield for both sample and match-control firms and provides further insight into the operating performance of firms that announce stock splits. Focusing on the right-side of this graph, we see little difference between the two distributions. In fact, over only the growth region above 2%, the frequency of matching firms is slightly *higher* than that of split firms. Splitting firms are not demonstrating unusually skewed operating performance. Instead, the difference in the two distributions clearly arises from a relative absence of negative growth realizations in the split sample. This mass appears shifted slightly to the right. An extremely high density of earnings

growth in our split sample is evident between .5% and 1.5%. In short, it would appear that managers announcing splits may not be anticipating a rapid acceleration in earnings, so much as they sense a low likelihood of a decline in operating performance. This may give managers the confidence to split their shares into a lower "trading-range" with a reduced concern that future stock prices may trade below some desired minimum.

Thus we have some indication that company earnings are rising after a stock split. Before we consider how analysts are forecasting earnings and also how their forecasts change in response to the news of a split, we digress for a moment and consider analyst following. One widely discussed argument for why we see stock splits stock is that this action might cause the firm to receive greater attention and increased analyst coverage (Brennan and Hughes (1991)). Given the earnings growth we saw in the previous table, it would appear that splitting companies, generally speaking, have little to hide. Thus in Table VII, we consider this question by summarizing analyst following for both split and match firms in our sample. Overall, we see that analyst coverage following a stock split, at least in our sub-sample of 948 firms, does indeed increase from a median following of 9 analysts just prior to the split to 13 analysts three days before the subsequent annual earnings announcement. However, we also see nearly the same growth pattern in match-control firms. While analyst coverage after a stock split is higher, it is not clear that the split itself really had much of an impact. Instead, the increase in analyst coverage appears to be more a consequence of how analysts choose to cover new stocks; the evidence suggests when analysts initiate coverage, they may favor high-growth, high-momentum stocks.

Next, we examine earnings forecasts and how they change in response to stock splits. We examine the earnings forecasts for sample and match-control firms at various points in time with reference to their split announcement and earnings announcement dates. While in most cases the earnings announcement dates of the split and match firms are within a few days of each other, they are not perfectly aligned in calendar time. However, we align them in event time by choosing a pseudo-split date for the match firm which is the same number of trading days prior to its earnings announcement date as the sample's split announcement date is from its earnings announcement.

Forecasts for the sample and match firms are expressed as a percentage of the actual (subsequently realized) earnings to measure the differential trend in earnings forecasts between the two groups of firms. At every point in event time, we compute one forecast accuracy measure for sample firms and one measure for their control-match firms. The computation of this measure for each group is as follows:

$$FPA_{I,t} = \frac{\sum_{i=1}^{n} (F/P)_{i,t}}{\sum_{i=1}^{n} (A/P)_{i,t}} \quad (1)$$

$FPA_{I,t}$ is the forecast at time t for group I (I = sample, match) expressed as a percentage of the actual earnings for group I. $(F/P)_{i,t}$ is the forecasted earnings per share (EPS) for firm i at time t scaled by its stock price as of the end of the month prior to the split, and $(A/P)_{i,t}$ is the actual EPS for firm i at time t scaled by the same stock price. We compute a summary measure for the sample and the matched firms to overcome the problem of small denominators and negative earnings. Results of this analysis are presented in Table VIII.

Consistent with prior research that shows optimism in analysts' forecasts, we find that the EPS forecasts of match firms start well above the actual EPS and gradually get revised downward as the year progresses. From an overestimation of roughly 5.50% at the beginning of the event period, the forecasted EPS exceeds the actual EPS by around 2% three days prior to the earnings announcement date. For the stock split firms, on the other hand, the forecasting behavior is markedly different. Ten days prior to the split announcement, analysts *underestimate* annual EPS for these firms by roughly 2.2%.[5] As the year progresses, the mean forecasts for these firms exhibit an upward trend. However, even three days prior to the earnings announcement, mean earnings forecasts are 0.7% below actual reported earnings.

Focusing on the difference between the forecasts for the two groups, this spread declines

[5] Using data from an earlier time period and using a different technique, McNichols and Dravid (1990) report similar conclusions. They also find that earnings forecasts for splitting firms are too low compared to their control firms, where forecasts at the same point in time are actually too high.

monotonically over time. The forecasts of the splitting firms and their matches converge, although not completely, to their actual EPS. This evidence suggests that financial analysts underestimate, in absolute and relative terms, the financial performance of the splitting firms. While they revise their forecasts upward immediately after the split announcement, this adjustment is incomplete.

We also examine whether the cross-sectional variation in the abnormal returns following a split announcement is explained by the gradual revision in earnings expectations by financial analysts. For this purpose, we focus on the period beginning ten days after the split and ending three days prior to the announcement of annual earnings. The match-adjusted return for the splitting firms over this period is comparable to the one-year post-split returns presented in Table III. As shown in Panel A of Table IX, the mean differential return is 8.30% and the median return is 7.52%. Both the mean and median match-adjusted forecast revisions of the sample firms are also positive and significant. In other words, analysts' upward revision in the earnings forecasts of splitting firms is significantly greater than the revisions in forecasts for corresponding match firms over the same time period.

Panel B of Table IX summarizes the association between the match-adjusted forecast revision (*MAFR*) and the match-adjusted returns (*MAAR*) of the splitting firms from ten trading days after the split announcement to three days prior to the earnings announcement. If the positive drift in the returns of splitting stocks can be attributed to the differential revision behavior in the earnings forecasts for these firms, *b* should be positive and roughly equal to the average earnings capitalization factor (P/E ratio) in the following regression:

$$MAAR_i = a + b * MAFR_i + e \qquad (2)$$

As expected, *b* is positive, 10.35, and highly significant. Moreover, the scale of *b* seems reasonable. The median E/P ratio of these firms is roughly .06, suggesting a P/E of about 17. Thus, while our point estimate is lower than we might expect, a host of factors affect the dependent variable here. Overall, the evidence suggests that at least some portion of the positive drift in the abnormal returns is not entirely attributable to misspecified return benchmarks but instead can be explained by bias in analysts forecasts.

This finding is consistent with the notion of sluggish market response to information conveyed in split announcements.

V. Robustness

Of course measuring long-horizon abnormal stock returns is not straightforward. Concern always exists as to just how robust the evidence really is. We address a few of these concerns here. First, despite the fact that it is easy to visualize stocks splits as simplistic news events, we consider whether information from other concurrent events may be affecting our results. Second, as is popular in many studies like this, we adopt a separate technique for measuring abnormal stock return performance. We calculate calendar-time portfolio returns and measure performance relative to the Fama-French (1993) three-factor model. Additionally, we supplement our current dataset that extends from 1988 to 1997 with stock splits during the period 1930 to 1987.

a. Dividends

When companies choose to split their stock, in many cases they consider concurrent changes in their cash dividend policy. Previous studies show that dividend increases, particularly dividend initiations, tend to be good news (Michaely, Thaler and Womack (1995)). While the evidence so far points to underreaction for splits overall, one wonders whether our results may be driven by information revealed when companies choose to simultaneously increase dividends or imply that they may increase dividends in the future. If investors are slow to respond to this news, the information revealed in dividends may be clouding our view of what appears to be underreaction to stock splits. Clearly, managers in firms which choose to both split their stock as well as increase their dividend may be more confident of their prospects in comparison to cases where managers only choose to split their shares. Thus, one might not be surprised to see lower long-horizon returns for firms which only announce a stock split in comparison to cases where dividends are also increasing. Therefore, when viewed more narrowly, are splits really informative and is there any evidence that the market is underreacting to this simple news event?

There are several ways to address this question. We choose a conservative approach. We not only eliminate from our original sample those firms which made concurrent dividend increases, but we also remove any firm that paid a dividend at the time of the announcement. We focus exclusively on companies that had no dividend at the time of the split announcement. Further, we impose a "look-ahead" and remove all firms which subsequently initiate a dividend in the same year as the split announcement. Clearly, we throw away more cases than we probably need to do. However, the remaining cases are situations where investors, when initially reacting to the news of a stock split, are less likely to be considering concurrent changes in dividends. Additionally, the look-ahead approach we apply biases our results slightly downward as this eliminates both the initial positive market reaction and subsequent return drift observed following dividend initiations (Michaely, Thaler and Womack(1995)).

For the 889 firms that survive these requirements, we report long-horizon evidence in a format similar to earlier tables. Generally speaking, the results are the same. This narrowly defined set of splits continues to show evidence of underreaction. In fact, there is no evidence that performance is lower in comparison to cases where dividends are paid and/or are increasing. In Panel A of Table X, we see that point estimates for the difference between our non-dividend sample and their respective matches is 11.77% (t=4.34). Again, although median return differences are not particularly interesting in our study, we see that the median difference is also high, 7.45%. The fact that right-skewed outliers do not drive this result is also verified using the real-time truncation approach.

In Panel B, we examine performance conditional on various sample characteristics. Because our new, non-dividend sample is substantially smaller and some of our former groupings are not highly populated, we collapse several categories. Splitting our ten-year sample period into two sub-periods, we see that abnormal returns in both periods are high. Although smaller, less mature firms often do not pay dividends (Grullon, Michaely and Swaminathan (1999)), our non-dividend results do not appear to be driven by less widely held or followed firms. Mean abnormal returns for exchange-listed stocks is still high, 9.81% (t=2.28). The mean abnormal performance for pooled market-cap quintiles 3 through 5 is 10.73% (t=3.23). The number of value-stocks left in our non-dividend sample is too limited to have any

meaningful commentary, yet the point estimates continue to be high. And finally, as we saw in the overall sample, the results for the non-dividend sub-sample do not appear to be driven by high momentum stocks as mean abnormal returns in the two lowest momentum quintiles is high, 11.86% (t=2.01).

b. More data and a new estimation technique

To this point, we have focused on buy-and-hold returns to measure abnormal stock performance. Numerous studies consider evidence using a calendar-time approach. Thus, we try this technique as well, replicating the returns that a portfolio would experience in real time. Because we use monthly data for this exercise, we modify our technique slightly and assume that investors wait until the end of the announcement month before buying a firm that announces a stock split. We add sample stocks into the portfolio after their announcement, hold them for twelve months and then sell them out. Each month, the portfolio is rebalanced. Researchers have debated as to just how the rebalancing should be done. We focus our discussion on an equal-weighted investment strategy where each stock in the portfolio in a given month receives the same weight. Although using an equal-weighted approach does not assure that each firm has the same impact on our analysis, the resulting monthly portfolio returns benefit from better diversification and thus low idiosyncratic noise. For completeness, we also report two other less diversified investment styles. Given that liquidity can be quite different between large-, mid-, and small-cap stocks, it is common to consider portfolio weights that tilt away from smaller firms. We report evidence for two such strategies. First, we report evidence for a value-weighted investment strategy. Yet given the extreme skewness observed in market equity values, strict cap-weighting can lead to perverse investment weights in some months. Not only does this assumption depart from a realistic or reasonable investment policy, estimation of abnormal performance is less precise and thus has less power because the portfolio returns are more noisy. Thus, we also consider log-value-weighted portfolios as a compromise to handle the extreme skewness in market-cap weights.

For each of the calendar time portfolios, we measure abnormal performance relative to the Fama-French (1993) three factor model. The model takes the form:

$$R_{p,t} - R_{rf,t} = a + b_{mkt}(R_{mkt,t} - R_{rf,t}) + b_{SML}R_{SML,t} + b_{HML}R_{HML,t} + e_t \quad (3)$$

Here, the three factors relate to monthly factor pay-offs for the market overall, a small minus large-cap stock factor and a high minus low book-to-market factor.[6] After regressing excess monthly portfolio returns on these three independent variables, our measure of excess performance is the intercept. A standard approach in this literature is to use ordinary least squares. Such an approach gives each month equal impact in the analysis. Because splits are not uniformly distributed in time, this approach implies that each firm does not have equal impact our analysis. In months of heavy split activity, these observations receive comparatively less weight. Thus, we estimate abnormal performance using weighted least squares where the weights are proportional to the number of firms in the portfolio in a given month. This approach assures that each firm has the same impact on the analysis and produces results that are generally consistent with traditional event studies. Under both the OLS and WLS methods, months where the portfolios held fewer than five firms are dropped from the analysis.

We apply these techniques to our overall split sample of 3,028 firms which span the period 1988 to 1997. Recall that we choose this period because of our need for I/B/E/S data. Additionally, the later years from this time frame have not been examined in previous studies and thus this sample provides some out-of-sample evidence in comparison to earlier work.

However, for completeness, we report evidence for all splits that we can locate. We scan the entire CRSP monthly returns tape from 1930 to 1987 and identify all stocks with split factors of 5-for-4 or greater.[7] This yields us an additional set of 9,364 cases to consider. We arbitrarily divide our new supplemental sample into two sub-periods at 1960. Splits in the 1930s and 1940s were much less frequent, thus the first sub-period is more limited.

Table XI, summarizes the intercepts and factor loadings for the various combinations of estimation methods, investment styles and time periods for the calendar time approach. Overall, the

[6] Our factor returns were supplied to us by Eugene Fama. For a more careful discussion of how these factors are determined, see Fama and French (1993).
[7] From 1930 to 1962, the monthly CRSP tape only has NYSE stocks. After 1962, ASE firms are covered on the tape. After 1974, Nasdaq stocks can be identified.

results are generally consistent with the conclusions derived earlier. For example, if we look at splits over the entire period from 1930 to 1997, our estimate of abnormal monthly performance for each of the three investment styles estimated using both OLS and WLS is strikingly similar, varying between .42% and .50% per month, or roughly 6% per year. This abnormal performance appears significant, although these results are affected by the large number of cases in the 1988 to 1997 sub-period. During this most recent window of time, estimates of monthly excess performance are uniformly lower in scale than that observed using the buy-and-hold approach. For example, focusing on WLS estimates for the equal-weighted investment style, abnormal performance is .45% per month, or roughly 5.5% per year. This compares with the 9% return we observed earlier. Clearly the two basic approaches, (the calendar-time and buy-and-hold approaches), differ in several ways and thus differences are to be expected. Yet in each of the various estimation methods using the calendar-time approach, the alphas are significant at reasonable confidence levels.

Thus, the results we report earlier for the most recent ten-year period in our sample would seem to stand up to the various approaches one might use to estimate calendar-time performance. The results from this last sub-period are also not so unique. We observe similar point estimates for abnormal performance for the middle sub-period, 1961 to 1987.

Focusing attention on the early years between 1930 and 1960, the results are not quite as robust and depend, to some extent, on how performance is estimated. Focusing on WLS, we see that when portfolios are formed on an equal-or log-value-weighted basis, our point estimates of abnormal returns are positive, but not significant. Interestingly, if we use a value-weighted investment style, our point estimate of abnormal performance is meaningful and significant, .30% per month (t=2.69). This point estimate is actually larger than that observed in the most recent 1988-1997 time period using the same technique.

In sum, using both a separate technique and also looking back many decades, we see that the evidence generally seems to hold. Over an extended number of decades, it would seem that the market underreacts to the news contained in stock splits. Moreover, the results do not appear to be driven by small stocks. While the results are not specific to any one time period, there is some sense that the

evidence is not quite as strong (at least for smaller stocks) in the earliest years of our examination. This may be a contributing factor in why the conclusions reported in previous studies of stock splits including Fama, Fisher, Jensen and Roll (1969) differ from what we see here.

VI. Conclusions

Over the last decade, a growing body of empirical literature has focused on long-horizon stock returns and made the common inference that markets do not appear to fully respond to news. These papers generally find that markets underreact to both good and bad news events. Recently, papers have begun trying to find a theoretical motivation for why underreaction might be observed in markets.

In this paper, we address some of the concerns that have been raised about this literature by re-examining new evidence with respect to one of the more simple, self-selected events a corporation can choose to engage in: the stock split. While splitting firms have their own unique properties, this particular transaction is typically not associated with large structural shifts in either operating cash flows or risk characteristics. As such, this study allows us to monitor how the market appears to revise its expectations in response to receiving a common piece of rather innocent news. Previous research focused on stock return evidence. In this paper, we go further and examine in detail the crucial implication of the underreaction hypothesis, the sluggish revision in expectations.

We examine a sample of over 3,000 stocks splits announced between 1988 and 1997. Using a control firm matched on the basis of market-cap, value/growth, momentum and nominal share price , we find abnormal returns in the year following the announcement of 9.00% for firms announcing stock splits. This finding is consistent with the positive drift observed following splits in the 1970s and 1980s and suggests that this return anomaly identified in earlier years with respect to stock splits is not spurious. Roughly one-third of our sample firms have earnings forecasts available on I/B/E/S. Consistent with the sluggish price evolution, we see a strikingly similar evolution in analysts' earnings expectations. Just prior to a split announcement, the relative *underestimation of* annual earnings for splitting firms (relative to their matches) is 7.67%. Just following the announcement, this underestimation narrows only slightly

to 7.08%. Over time, these expectations revise gradually such that just prior to the release of earnings, the forecast bias decreases to roughly 3%. We find that this differential forecasting behavior between the splitting firms and their matches explains a significant portion of the variation in the abnormal returns across splitting firms. At least a portion of the underreaction observed in long-horizon stock returns can be traced to the underreaction in the revision of earnings expectations for firms announcing stock splits.

At least with respect to one of the more simple pieces of news, the stock split, the evidence, using both stock returns and earnings expectations, is consistent with previous research that has documented underreaction to corporate news events.

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Table I
Stock Splits by Year and Split Factor

The table below reports the number of stocks in our sample overall, by year, and by split-factor that announced a forward stock split during the ten-year period 1988 to 1997. All firms on NASDAQ, NYSE and ASE whose split factor was 5-for-4 or greater were first identified (a total of 4,154 cases). From this set, our sample was formed by taking only those firms whose market-cap was available at month-end prior to the split announcement and which had returns data available on CRSP for the prior 36 months.

Year	< 2-for-1	2-for-1	> 2-for-1	Total
1988	103	53	8	164
1989	151	97	14	262
1990	80	75	6	161
1991	139	101	10	250
1992	212	146	21	379
1993	207	173	15	395
1994	133	99	18	250
1995	166	159	11	336
1996	189	185	14	388
1997	211	207	25	443
Total	1,591	1,295	142	3,028

Table II
Descriptive Data for Split Sample and Match Firms

Values for each of the descriptive variables listed below are determined at month-end prior to the split announcement. Although the sample includes non-NYSE stocks, only NYSE stocks are used in determining cut-off values. Market-cap cut-off values are determined based on the market value of equity. Value/growth quintiles (defined as the preceding three-year compounded return) are determined separately for each market-cap quintile. Momentum quintiles (based on the preceding one-year compounded return) are defined separately for each market-cap by value/growth classification.

		Mean		Median	
	n	Sample	Match	Sample	Match
Overall					
Market-cap (in $ millions)	3,028	2,147	1,299	332	320
Value/Growth (3-yr comp. return in %)	3,028	205.9	153.2	123.2	114.3
One-year return (1-yr comp. return in %)	3,028	72.1	63.0	48.1	46.6
Nominal Price (post-split)	3,028	$22.24	$22.69	$20.75	$21.00
Market-cap Quintiles (in $ millions)					
1 (small stocks)	766	50	46	46	42
2	565	169	163	162	153
3	582	413	395	400	374
4	534	1,117	1,071	1,045	965
5 (large stocks)	581	9,519	5,173	4,290	3,557
Value/Growth Quintiles (prior three-year compounded return (in %))					
1 (value stocks)	30	-3.7	-8.5	-1.7	-1.4
2	111	17.8	16.0	22.8	19.9
3	247	40.7	40.6	42.5	40.2
4	530	70.2	67.8	72.2	68.1
5 (growth stocks)	2,110	272.2	197.4	177.4	149.9
Momentum Quintiles (prior one-year compounded return (in %))					
1 (low momentum stocks)	207	0.9	-4.0	0.0	-3.6
2	344	18.2	17.7	18.6	17.8
3	462	33.7	33.3	32.2	32.1
4	688	47.5	46.8	46.7	45.6
5 (high momentum stocks)	1,327	123.4	103.9	88.0	84.0

Table III
One-year post-split returns (in %) for sample and matching control firms

This table reports compounded returns for the period starting two trading days after the split announcement date and ending 250 trading days later for sample firms and matching control firms. Returns are calculated three different ways. First, we report overall returns for all firms in our sample. The second approach, real-time truncation, is based on a strategy of prematurely liquidating the investment in both the sample and match firms on the first day in which the difference in compounded return between that sample firm and its match exceeds 100%. In the third approach, we symmetrically winsorize the sample, ex-post, at the 1% and 99% levels. Numbers in parentheses represent significance levels for t-tests of the means and a Wilcoxon signed-rank test for medians.

	Overall		Real-Time Truncation		Winsorized	
	Mean	Median	Mean	Median	Mean	Median
Split	23.29	16.18	22.52	17.21	22.44	16.18
Match	14.29	11.01	14.26	10.66	13.87	11.01
Paired-Difference	9.00	6.31	8.26	6.88	8.52	6.31
	(t=7.93; p<0.0001)	(p<0.0001)	(t=8.23; p<0.0001)	(p<0.0001)	(t=8.37; p<0.0001)	(p<0.0001)

Table IV
One-Year Post-Split Abnormal Returns by Year, Exchange Listing and Split Factor

This table reports paired differences in compounded returns between split-announcing and matching control firms by year, exchange, and split factor. Returns are for the period starting two trading days after the split announcement date and ending 250 trading days after the announcement. Numbers in parentheses represent significance levels for t-tests of the means and a Wilcoxon signed-rank test for medians.

	N	Mean	Median
By year of split			
1988-89	426	6.82 (t=2.70; p<0.0072)	6.49 (p<0.0045)
1990-91	411	10.84 (t=3.16; p<0.0017)	6.86 (p<0.0026)
1992-93	774	7.48 (t=3.61; p<0.0003)	4.15 (p<0.0003)
1994-95	586	11.02 (t=3.55; p<0.0004)	6.44 (p<0.0015)
1996-97	831	9.19 (t=4.58; p<0.0001)	7.18 (p<0.0001)
By Exchange			
NYSE	1,148	7.63 (t=5.05; p<0.0001)	5.58 (p<0.0001)
AMEX	285	7.36 (t=2.02; p<0.0440)	5.89 (p<0.0344)
NASDAQ	1,595	10.27 (t=5.90; p<0.0001)	7.18 (p<0.0001)
By Split Factor			
< 2-for-1	1,591	10.4 (t=6.58; p<0.0001)	6.64 (p<0.0001)
2-for-1	1,295	6.75 (t=3.94; p<0.0001)	5.81 (p<0.0001)
> 2-for-1	142	13.74 (t=2.66; p<0.0086)	6.21 (p<0.0001)

Table V
One-Year Post-Split Abnormal Returns by Market-cap, Value/growth and Momentum

This table reports compounded returns for the period starting two trading days after the split announcement date and ending 250 trading days later for various groups of sample and matching control firms. Market-cap cut-off values are determined based on the market value of equity of only NYSE stocks. Value/growth quintiles (defined as the preceding three-year compounded return) are determined separately for each NYSE size category. Momentum quintiles (based on the preceding one-year compounded return) are defined separately for each market-cap by value/growth category. Numbers in parentheses represent significance levels for t-tests of the means and a Wilcoxon signed-rank test for medians.

	N	Mean	Median
Market-cap Quintiles (in $ millions)			
1 (small stocks)	766	12.37 (t=4.53; p<0.0001)	9.31 (p<0.0001)
2	565	11.03 (t=4.03; p<0.0001)	6.42 (p<0.0003)
3	582	11.94 (t=4.67; p<0.0001)	11.33 (p<0.0001)
4	534	3.78 (t=1.66; p<0.0982)	2.65 (p<0.2181)
5 (large stocks)	581	4.42 (t=2.25; p<0.0247)	4.17 (p<0.0317)
Value/Growth Quintiles (prior three-year compounded return (in %))			
1 (value stocks)	30	37.47 (t=1.62; p<0.1152)	10.25 (p<0.0984)
2	111	11.00 (t=2.49; p<0.0141)	6.15 (p<0.0290)
3	247	2.13 (t=0.73; p<0.4668)	0.20 (p<0.3467)
4	530	6.83 (t=3.09; p<0.0021)	6.37 (p<0.0001)
5 (growth stocks)	2,110	9.84 (t=6.86; p<0.0001)	6.73 (p<0.0001)
Momentum Quintiles (prior one-year compounded return (in %))			
1 (low momentum stocks)	207	6.11 (t=1.35; p<0.1788)	8.67 (p<0.0336)
2	344	6.96 (t=2.90; p<0.0040)	4.96 (p<0.0098)
3	462	6.66 (t=2.72; p<0.0069)	5.68 (p<0.0071)
4	688	10.28 (t=5.30; p<0.0001)	7.12 (p<0.0001)
5 (high momentum stocks)	1,327	10.12 (t=5.02; p<0.0001)	5.89 (p<0.0001)

Table VI
Change in Earnings Yield for Split and Match-Control Firms and the S&P Industrial Index

This table reports the growth in earnings yield for 948 split announcing firms and their respective matches from the year before to the year following the split. Actual earnings (adjusted for unusual items) are obtained from I/B/E/S. The difference in earnings per share is computed as the annual earnings reported after the split announcement less previous year's earnings, scaled by month-end stock price preceding the split announcement. If the annual earnings are reported within 125 trading days of the split date, we use reported earnings at the next fiscal year-end. EPS differences for match firms are computed similarly by aligning fiscal years as closely as possible with their split-announcing counterparts. For comparison, we also report changes in earnings yield for the S&P industrial index at the same point in time. Numbers in parentheses represent significance levels for t-tests of the means and a Wilcoxon signed-rank test for medians.

	Mean	Median	% positive
Split Sample	1.18 (t=14.55; p<0.0001)	0.95 (p<0.0001)	84.7
Match Firms	0.92 (t=6.65; p<0.0001)	0.91 (p<0.0001)	72.9
S&P Industrial Index	0.38	0.48	---
Difference in Earnings Yield Growth (Split – Match Firms)	0.26 (t=1.66; p<0.0968)	0.14 (p<0.0206)	52.7
Difference in Growth (Split Firms - S&P Index)	0.79 (t=9.19; p<0.0001)	0.59 (p<0.0001)	71.7

Table VII
Analyst Following for Split and Match-Control Firms

This table reports the change in analyst following for split firms and match firms from ten days prior to split announcement to three days before the first annual earnings announcement. In cases where the first annual earnings announcement is within six months of the split announcement date, the next fiscal year is used in the analysis. Match firms are matched to split firms in event time based on their earnings announcement dates. Number of analysts following a firm are measured as the number of individual analysts submitting annual earnings forecasts to I/B/E/S for that fiscal year.

		After Split				**Before Earnings Announcement**			
	10 Days Before Split	**10 days**	**1 month**	**2 months**	**3 months**	**3 Months**	**2 Months**	**1 Month**	**3 Days**
Number of Analysts following split firms									
Mean	12.68	13.06	13.24	13.61	14.05	15.52	15.89	16.16	16.36
Median	9.00	10.00	10.00	10.00	11.00	12.00	12.50	13.00	13.00
Number of Analysts following match firms									
Mean	13.08	13.48	13.69	14.04	14.40	15.94	16.28	16.54	16.74
Median	10.00	10.00	11.00	11.00	11.00	12.00	12.00	13.00	13.00

Table VIII
Revisions in Earnings Expectations Subsequent to Stock Split Announcements

This table reports the evolution of earnings expectations for a subset of 948 split announcements for which forecast data is available on I/B/E/S. We report forecasted earnings per share as a percentage of actual earnings per share normalized by stock price at month-end prior to split announcement. This measure of forecast as a percentage of actual (FPA) is computed for both the sample firms and their matches using the following procedure:

$$FPA_{I,t} = \frac{\sum_{i=1}^{n}\left(\frac{F}{p}\right)_{i,t}}{\sum_{i=1}^{n}\left(\frac{A}{p}\right)_{i,t}}$$

where $FPA_{I,t}$ is the forecasted EPS expressed as a percentage of actual EPS for group I (I=sample, match) as of time t, $(F/P)_{i,t}$ is the mean I/B/E/S forecast for firm i as of time t scaled by its stock price at month-end prior to split and $(A/P)_{i,t}$ is the actual earnings for the year after split, scaled by the same (split-adjusted) stock price. If the first post-split earnings announcement is within 125 days of the split announcement, we jump to the next annual earnings cycle. The difference in FPA is computed as $FPA_{S,t}$-$FPA_{M,t}$ where S is the set of sample firms and M the set of match firms.

		After Split				Before Earnings Announcement			
	10 Days Split	10 days		2 months	3 months		2 Months	1 Month	
Overall									
FPA for Split Firms (in %)	97.83	98.63	98.79	99.42	99.70	99.85	99.71	99.46	99.31
FPA for Match Firms (in %)	105.50	105.71	105.62	105.43	104.93	102.88	102.67	102.11	101.99
Difference in FPA Overall (Sample minus Match)	-7.67	-7.08	-6.82	-6.01	-5.23	-3.03	-2.96	-2.65	-2.68
Difference in FPA for Large-Cap (Q5)	-5.86	-5.34	-5.24	-4.55	-4.29	-1.25	-1.32	-1.20	-1.16
Difference in FPA for Small & Med-cap (Q1 to Q4)	-8.32	-7.71	-7.39	-6.54	-5.57	-3.71	-3.57	-3.20	-3.24
Difference in FPA for High Growth (Q5)	-9.59	-9.07	-9.81	-7.82	-6.82	-4.10	-3.65	-3.15	-3.10
Difference in FPA for Value and medium (Q1 to Q4)	-5.06	-4.38	-4.11	-3.55	-3.06	-1.59	-2.01	-1.98	-2.11
Difference in FPA for High momentum (Q5)	-9.96	-8.64	-8.38	-7.17	-5.95	-3.73	-3.86	-3.17	-3.14
Difference in FPA for Low and medium (Q1 to Q4)	-6.19	-6.07	-5.81	-5.25	-4.74	-2.58	-2.37	-2.31	-2.37

Table IX

The Relation Between Post-split Earnings Forecast Revisions And Stock Returns

Panel A of this table reports changes in earnings forecasts for the next fiscal year from ten days after the split announcement to three days prior to the earnings announcement as well as compounded returns over the same interval for sample and match firms. Panel B reports regression evidence of the association between the two variables. The match-adjusted forecast revision (MAFR) represents the change in mean I/B/E/S earnings forecast for the sample firms, relative to the change in forecast for the match firms over the same event interval. This measure is computed as:

$$\frac{F_{si,ead-3} - F_{si,spdt+10}}{P_{si}} - \frac{F_{mi,ead-3} - F_{mi,spdt+10}}{P_{mi}}$$

si is sample firm *i*, and *mi* is its match. P is the stock price at month-end prior to the split announcement. To handle extreme values of forecast revisions, observations beyond the 1st and 99th percentile are excluded. The match-adjusted abnormal return (MAAR) for sample firm *i* is computed as the compounded daily return for that firm over the period from 10 days after the split announcement to three trading days prior to its annual earnings announcement less the compounded return for its match over the same time period.

Panel A - Descriptive Statistics

Forecast Revision (in %)	n	Mean	Median
Sample	948	0.048	0.033
Match	948	-0.258	-0.040
Paired-difference (MAFR)	948	0.306 (p<0.0001)	0.100 (p<0.0001)
Abnormal Return (in %)			
Sample	948	18.88	13.62
Match	948	10.58	7.71
Paired-difference (MAAR)	948	8.30 (p<0.0001)	7.52 (p<0.0001)

Panel B - Regression Results:

$$MAAR_i = a + bMAFR_i + e$$

a		R^2
10.35 (t=12.20; p<0.0001)		13.74%

Table X
Post-Split Abnormal Returns (in %) for Non-dividend Paying Firms

Panel A reports compounded returns for the period starting two trading days after the split announcement and ending 250 trading days later for 889 sample firms which did not pay a dividend both in the year before and year of the split. Returns are calculated using the same methods detailed in Table III. Panel B reports return differences for various sub-samples. Numbers in parentheses represent significance levels for t-tests of the means and a Wilcoxon signed-rank test for medians.

Panel A: Abnormal stock returns for non-dividend paying split firms

	Overall		Real-Time Truncation		Winsorized	
	Mean	Median	Mean	Median	Mean	Median
Split	24.45	12.78	22.84	15.83	23.24	12.78
Match	12.68	6.15	12.52	5.41	12.11	6.15
Difference	11.77 (t=4.34; p<0.0001)	7.45 (p<0.0003)	10.32 (t=4.62; p<0.0001)	9.71 (p<0.0001)	10.87 (t=4.55; p<0.0001)	7.45 (p<0.0003)

Panel B: Abnormal stock returns by group

	N	Mean	Significance	Median	Significance
By year of split					
1988-92	301	14.00	t=3.03;p<0.0026	10.43	p<0.0034
1993-97	588	10.63	t=3.17; p<0.0016	5.48	p<0.0188
By Exchange					
NYSE & AMEX	293	9.81	t=2.28; p<0.0234	8.29	p<0.0158
NASDAQ	596	12.74	t=3.69; p<0.0002	6.66	p<0.0057
By Split Factor					
< 2-for-1	463	15.73	t=4.06; p<0.0001	9.85	p<0.0003
2-for-1 or greater	426	7.48	t=1.97; p<0.0490	4.58	p<0.1506
By Market-cap Quintiles					
1 and 2	456	12.76	t=3.00; p<0.0028	7.62	p<0.0197
3, 4 and 5	433	10.73	t=3.23; p<0.0013	6.58	p<0.0049
By Value/Growth Quintiles					
1 and 2	22	51.20	t=1.61; p<0.1205	17.62	p<0.1342
3, 4 and 5	867	10.77	t=4.05;p<0.0001	7.12	p<0.0007
By Momentum Quintiles					
1 and 2	126	11.86	t=2.01; p<0.0466	16.22	p<0.0087
3, 4 and 5	763	11.76	t=3.91; p<0.0001	6.31	p<0.0044

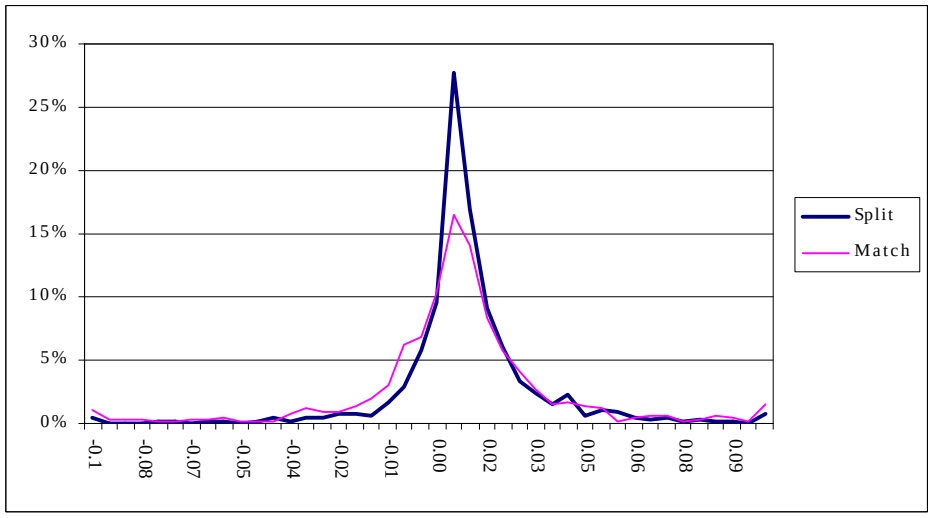
Table XI

This table reports abnormal stock returns for calendar-time portfolios formed using split announcing stocks. Firms are added to the portfolio in the month following the split and are held for 12 months. The intercept represents the overall abnormal monthly return (in %) measured using the Fama-French (1993) three factor model. Our original sample of 3,028 splits announced between 1988 and 1997 is supplemented with 9,354 cases announced between 1930 and 1987. We exclude months where portfolios hold less than five stocks. Portfolio returns are computed assuming an equal-weighted, value-weighted and log value-weighted investment style. The three-factor model is estimated using both OLS and weighted least squares. Numbers in parenthesis are t-statistics. The t-test for the market premium coefficient (β_{market}) assumes a null value of 1. ***, **, and * indicate significance at the 1%, 5% and 10% levels, respectively.

	Ordinary Least Squares Estimates				Weighted Least Squares Estimates			
	Intercept	β_{market}	β_{smb}	β_{hml}	Intercept	β_{market}	β_{smb}	β_{hml}
Equal Weighted Portfolio								
All years	0.47	1.03	0.58	-0.24	0.50	1.04	0.69	-0.19
	(7.69)***	(2.15)**	(24.23)***	(-10.39)***	(10.40)***	(3.22)***	(37.26)***	(-9.67)***
1930-1960	0.27	1.09	0.35	-0.30	0.13	1.10	0.38	-0.11
	(2.16)**	(3.08)***	(6.10)***	(-6.01)***	(1.47)	(4.76)***	(8.14)***	(-2.54)**
1961-1987	0.56	1.02	0.67	-0.25	0.59	1.02	0.71	-0.27
	(7.35)***	(1.05)	(23.68)***	(-8.11)***	(8.40)***	(1.26)	(26.34)***	(-9.54)***
1988-1997	0.46	1.04	0.68	-0.05	0.45	1.04	0.71	-0.06
	(4.21)***	(1.33)	(16.52)***	(-1.05)	(4.15)***	(1.40)	(17.85)***	(-1.21)
Value Weighted Portfolio								
All years	0.44	1.05	-0.02	-0.35	0.42	1.06	-0.03	-0.39
	(5.83)***	(3.11)***	(-0.75)	(-12.23)***	(6.43)***	(3.72)***	(-1.23)	(-14.55)***
1930-1960	0.44	1.09	-0.04	-0.36	0.30	1.09	-0.04	-0.27
	(3.12)***	(2.72)**	(-0.65)	(-6.15)***	(2.69)***	(3.30)***	(-0.64)	(-4.91)***
1961-1987	0.49	1.02	0.02	-0.40	0.54	1.05	0.00	-0.46
	(4.33)***	(0.76)	(0.42)	(-8.97)***	(5.15)***	(2.03)**	(0.09)	(-10.92)***
1988-1997	0.32	1.03	-0.10	-0.32	0.27	1.04	-0.09	-0.30
	(2.47)**	(0.80)	(-2.02)**	(-5.62)***	(2.30)**	(1.00)	(-2.12)**	(-5.86)***
Log-value Weighted Portfolio								
All years	0.47	1.04	0.53	-0.26	0.50	1.05	0.64	-0.21
	(7.88)***	(2.56)**	(22.51)***	(-11.40)***	(10.40)***	(3.86)***	(34.72)***	(-10.93)***
1930-1960	0.28	1.09	0.30	-0.31	0.14	1.10	0.34	-0.12
	(2.36)**	(3.04)***	(5.47)***	(-6.29)***	(1.66)*	(4.82)***	(7.24)***	(-3.00)***
1961-1987	0.57	1.03	0.61	-0.27	0.60	1.03	0.65	-0.29
	(7.40)***	(1.50)	(21.52)***	(-8.62)***	(8.43)***	(1.79)*	(24.14)***	(-10.16)***
1988-1997	0.45	1.05	0.64	-0.09	0.43	1.06	0.67	-0.09
	(4.09)***	(1.67)	(15.68)***	(-1.80)*	(3.98)***	(1.77)*	(16.97)***	(-1.93)*

Figure 1

Frequency Distribution of Earnings Growth



This graph plots the distribution of changes in earnings yield for both split firms and

in earnings is computed as the difference between year-end operating earnings reported

of time between the split announcement and the next annual earnings release is less than

changes in earnings are normalized by price in the month following the split

items.