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Measuring abnormal performance

Do stocks overreact?

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A highly controversial issue in financial economics is whether stocks overreact. In this paper we find an economically-important overreaction effect even after adjusting for size and beta. In portfolios formed on the basis of prior five-year returns, extreme prior losers outperform extreme prior winners by 5-10% per year during the subsequent five years. Although we find a pronounced January seasonal, our evidence suggests that the overreaction effect is distinct from tax-loss selling effects. Interestingly, the overreaction effect is substantially stronger for smaller firms than for larger firms. Returns consistent with the overreaction hypothesis are also observed for short windows around quarterly earnings announcements.

1. Introduction

The predictability of stock returns is one of the most controversial topics in financial research. Various researchers have documented predictable returns

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over long and short horizons for both individual securities and indices.[1] While there is now a consensus that returns are predictable, there is widespread disagreement about the underlying reasons for this predictability. Fama (1991) observes that the interpretation of the evidence on return predictability runs 'head-on into the joint-hypothesis problem; that is, does return predictability reflect rational variation through time in expected returns, irrational deviations of price from fundamental value, or some combination of the two?'

One of the most influential, and controversial, papers in this line of research is by De Bondt and Thaler (1985), who present evidence of economically-important return reversals over long intervals. In particular, stocks that experience poor performance over the past three-to-five years (losers) tend to substantially outperform prior-period winners during the subsequent three-to-five years. De Bondt and Thaler interpret their evidence as a manifestation of irrational behavior by investors, which they term 'overreaction'.

Various authors [e.g., Chan (1988) and Ball and Kothari (1989)], however, have argued that these return reversals are due primarily to systematic changes in equilibrium-required returns that are not captured by De Bondt and Thaler. One of the main arguments for why required returns on extreme winners and losers vary substantially follows from pronounced changes in leverage. Since the equity beta of a firm is a function of both asset risk and leverage, a series of negative abnormal returns will increase the equity beta of a firm, thus increasing the expected return on the stock (assuming that the asset beta is positive and does not decrease substantially, and that the firm does not change its debt to fully offset the decline in the value of its equity). Following the same logic, a decrease in the equity beta is expected for winners. Consistent with the prediction of the leverage hypothesis, Ball and Kothari report that the betas of extreme losers exceed the betas of extreme winners by a full 0.76 following the portfolio formation period. Such a large difference in betas, coupled with historical risk premiums, can account for substantial differences in realized returns.

Another reason that has been advanced for why losers outperform winners relates to the size effect. Zarowin (1990) and others have argued that the superior performance of losers relative to winners is not due to investor overreaction, but instead is a manifestation of the size and/or January effects, in that by the end of the ranking period, losers tend to be smaller-sized firms than winners.

In general, attempts to discriminate between market inefficiency and changing equilibrium-required returns are most difficult when long return

[1]Among the many recent studies documenting time-series return predictability for long and short horizons are Rosenberg, Reid, and Lanstein (1985), Keim and Stambaugh (1986), Fama and French (1988), Lo and MacKinlay (1988), Poterba and Summers (1988), Conrad and Kaul (1989), Jegadeesh (1990), Lehmann (1990), Jegadeesh and Titman (1991), and Brock, Lakonishok, and LeBaron (1992).

intervals are used. This is because the measurement of abnormal performance over long horizons is very sensitive to the performance benchmark used, as emphasized by Dimson and Marsh (1986). In this paper, in addition to allowing time variation in betas, as recently applied in this context by Ball and Kothari (1989), we use three methodological innovations that enable us to perform a comprehensive evaluation of the overreaction hypothesis. Our methodology is applicable to any study measuring abnormal performance over long horizons.

First, we use the empirically-determined price of beta risk, rather than that assumed by a specific highly-structured model such as the Sharpe–Lintner capital asset pricing model (CAPM). Since the betas of extreme prior-period winners and losers differ dramatically, large differences in returns between winners and losers can be accounted for by the Sharpe–Lintner CAPM, in which the compensation per unit of beta risk is $r_m - r_f$, where r_m is the return on the market and r_f is the risk-free rate. In the 1931–82 period, $r_m - r_f$ averages almost 15% per year using an equally-weighted index of NYSE stocks for r_m and Treasury bills for r_f. The Sharpe–Lintner CAPM assumption is innocuous in many other studies, where the portfolio betas typically do not differ much from 1.0. But in this study, the betas of winners are markedly different from the betas of losers. Numerous empirical studies, starting with Black, Jensen, and Scholes (1972), find a much flatter slope than that assumed by the Sharpe–Lintner CAPM.[2] Indeed, Fama and French (1992) question whether there is any relation at all between beta and realized returns.

Second, we calculate abnormal returns using a comprehensive adjustment for size. Numerous studies have found a relation between size and future returns. Portfolios of losers are typically comprised of smaller stocks than portfolios of winners. Thus, in order to ascertain whether there is an independent overreaction effect, a size adjustment is appropriate. However, because small-firm portfolios contain proportionately more losers, the common procedure of adjusting for size might overadjust and thus create a bias against finding an independent overreaction effect. To address this possibility, we purge stocks with extreme performance from our size-control portfolios.[3] Our methodology enables us to disentangle the effects of size and prior performance in calculating abnormal returns on winner and loser portfolios.

[2]Black, Jensen, and Scholes (1972), Miller and Scholes (1972), Fama and MacBeth (1973), Tinic and West (1984), Lakonishok and Shapiro (1986), Amihud and Mendelson (1989), and Ritter and Chopra (1989), among others, find flatter slopes than predicted by the Sharpe–Lintner CAPM.

[3]Fama and French (1986) use a nearly identical procedure for controlling for size effects. For size deciles, they compare the average return on prior winners and losers with stocks in the same size decile that were in the middle 50% of returns during the portfolio formation period. They use continuously-compounded returns over three-year periods rather than the annual arithmetic returns over five-year periods that we use, but obtain somewhat similar results to those reported here.

In addition, we explore the generality of the effect in both January and non-January months.

Third, we examine abnormal returns over short periods of time. Abnormal returns calculated over long intervals are inherently sensitive to the benchmark used. Currently, there is no consensus on the 'best' benchmark, and research documenting abnormal returns calculated over long intervals is frequently treated with suspicion. Therefore, in one of our tests, we focus on short windows in which a relatively large amount of new information is disseminated, an approach analogous to that employed by Bernard and Thomas (1989, 1990) in their investigation of abnormal returns following earnings announcements. We compute abnormal returns for winners and losers for the three-day period in which quarterly earnings announcements occur. Positive abnormal returns at subsequent earnings announcements for prior losers, and negative abnormal returns for prior winners, are consistent with the overreaction hypothesis. In drawing our inferences, we are careful in adjusting for size effects and the higher volatility that other researchers [e.g., Chari, Jagannathan, and Ofer (1988)] have documented at earnings announcement dates.

Our results indicate that there is an economically-significant overreaction effect present in the stock market. Moreover, it is unlikely that this effect can be attributed to risk measurement problems, since returns consistent with the overreaction hypothesis are also observed for short windows around quarterly earnings announcements. Depending upon the procedure employed, extreme losers outperform extreme winners by 5–10% per year in the years following the portfolio formation period. Interestingly, the overreaction effect is much stronger among smaller firms, which are predominantly held by individuals; there is at most only weak evidence of an overreaction effect among the largest firms, which are predominantly held by institutions. One interpretation of our findings might be that individuals overreact, but institutions do not.

There is a strong January seasonal in the return patterns, but long-term overreaction is not merely a manifestation of tax-loss selling effects, as captured by the prior year's performance. To examine this issue, we form portfolios based upon prior one-year returns, and examine the performance of these portfolios over the subsequent five years. One-year and five-year formation periods produce dramatically different patterns in returns during the subsequent five years. We find much smaller differences in returns between extreme portfolios when portfolios are formed based upon one-year returns rather than five-year returns. Much of this difference in behavior occurs in the first of the five post-ranking years: portfolios of winners and losers formed on the basis of one-year returns display momentum, rather than immediate return reversals.

The structure of the remainder of this paper is as follows. In section 2, we measure the extent of abnormal performance for portfolios formed on the basis of prior returns while alternately controlling for beta and size effects. In section 3, we present evidence on the abnormal returns for winners and losers while simultaneously controlling for beta and size effects. We also explore seasonal and cross-sectional patterns in the extent of overreaction. In section 4, we present evidence from the market's reaction to earnings announcements. Section 5 concludes the paper.

2. Beta and size-adjusted abnormal returns

2.1. Methodology

For comparability with prior studies [e.g., Ball and Kothari (1989)] we use the CRSP monthly tape of New York Stock Exchange issues from 1926 to 1986. All stocks that are continuously listed for the prior five calendar years are ranked each year on the basis of their five-year buy-and-hold returns and assigned to one of twenty portfolios. Thus, the first ranking period ends in December 1930, and the last one ends in December 1981, a total of 52 ranking periods. The post-ranking periods are overlapping five-year intervals starting with 1931–35 and ending with 1982–86. For each of the twenty portfolios, this procedure results in a time series of 52 portfolio returns for each of the ten event years -4 to $+5$, with the last year of the ranking period designated as year 0. These 52 observations are used to estimate betas and abnormal returns for the ten event years.

Annual portfolio returns for each firm are constructed from the monthly CRSP returns by compounding the monthly returns in a calendar year to create an annual buy-and-hold return. The annual returns of the firms assigned to a portfolio are then averaged to get the portfolio's annual return. If a firm is delisted within a calendar year, its annual return for that year is calculated by using the CRSP equally-weighted index return for the remainder of that year. In subsequent years, the firm is deleted from the portfolio.

To estimate the market model coefficients, we use Ibbotson's (1975) returns across time and securities (RATS) procedure. For each event year $\tau = -4,\ldots,0,+1,\ldots,+5$ and portfolio p, we run the following regression using 52 observations:

$$r_{pt}(\tau) - r_{ft}(\tau) = \alpha_p(\tau) + \beta_p(\tau)[r_{mt} - r_{ft}] + e_{pt}(\tau), \tag{1}$$

where $r_{pt}(\tau)$ is the annual return on portfolio p in calendar year t and event year τ, r_{mt} is the equally-weighted market return on NYSE stocks meeting our sample selection criteria in calendar year t, and r_{ft} is the annual return on T-bills [from Ibbotson Associates (1988)]. The intercept in eq. (1) is known as Jensen's (1969) alpha, and is a measure of abnormal performance.

2.2. Beta-adjusted excess returns

In columns (1)–(3) of table 1, we have formed portfolios by ranking firms according to their prior five-year returns. We report the annual returns, alphas, and betas averaged over the five years following the portfolio formation (ranking) period.[4] Our numbers are slightly different from those reported in Ball and Kothari's (1989) table 1 because of the different sample selection criteria employed. Ball and Kothari require that their firms remain listed on the NYSE for the entire five-year post-ranking period, whereas we do not impose such a requirement. Their sample selection criteria imposes a survivorship bias. In our sample, approximately 22% of the extreme loser portfolio's firms are delisted by the end of the post-ranking period, but only 8% of the extreme winner portfolio's firms are delisted. (In the 1930s, many of the delistings occurred due to bankruptcies, whereas by the 1970s, takeovers are the main reason for delistings. As might be expected, bankruptcies are rare among the extreme winners.)

The most striking result in table 1 is the inverse relation between the past and subsequent returns. Portfolio 1 (the prior-period losers) has a post-ranking-period average annual return of 27.3%, while portfolio 20 (the prior-period winners) has a post-ranking-period average annual return of 13.3%, a difference of 14.0% per year.[5] Over the five-year post-ranking period, even before compounding, this difference cumulates to 70%! The debate revolves around how much of this difference is attributable to equilibrium compensation for risk differences, and how much is an abnormal return. In fact, as demonstrated by Ball and Kothari, much of this difference can be explained by the Sharpe–Lintner CAPM. According to column (3) of table 1, the

[4] Two issues (at least) are raised by the procedure of averaging the returns, alphas, and betas over the five post-ranking years. First, since the last price of the ranking period is the first price of the post-ranking period, negative serial correlation might be induced by bid–ask spread effects. To examine the sensitivity of our results to this issue, in work not reported here, we have also calculated average returns, alphas, and betas using only event years +2 to +5. Our results are nearly identical to those found using event years +1 to +5. This raises the second issue: if the return reversals are due to overreaction, with firms whose market price has deviated from fundamental value eventually reverting, how long does this reversion take? One might expect a stronger reversion in event years +1 and +2 than in years +4 and +5. This is in fact the case: the per-year abnormal returns are slightly greater when a three-year post-ranking period is used rather than a five-year post-ranking period.

[5] De Bondt and Thaler (1985) find a smaller difference in post-ranking-period returns between winners and losers than we (and Ball and Kothari) do. In their fig. 3, they find a difference of about 8% per year for their five-year post-ranking period, compared to our 14% per year. There are a number of reasons for this difference, most notably because the definition of extreme winners and losers is not the same. In most of their work, De Bondt and Thaler define their portfolios as the most extreme 35 firms in each year, whereas the number of firms in each of our portfolios increases from about 20 in the 1930s to about 50 in the 1970s, averaging about 43 firms. Further differences are that our last ranking period ends in 1981, whereas their last ranking period ends in 1978, and they use monthly return intervals versus our annual return intervals.

Table 1

Average annual post-ranking-period percentage returns, alphas, and betas for twenty portfolios formed on the basis of either ranking-period returns or ranking-period betas. Average monthly post-ranking-period percentage returns, alphas, and betas are also reported for portfolios formed on the basis of five-year ranking-period returns.

Alphas and betas are estimated from time-series regressions with 52 observations, for ranking periods ending in 1930–81, for each of the five post-ranking years. The alphas and betas reported are the averages of these five post-ranking-period numbers. In columns (1)–(5) and (9)–(11), portfolio 1 is comprised of stocks with the lowest ranking-period returns, and portfolio 20 is comprised of the stocks with the highest ranking-period returns. In columns (6)–(8), portfolio 1 is comprised of stocks having the highest ranking-period betas, and portfolio 20 is comprised of stocks having the lowest ranking-period betas. EW and VW are, respectively, equally-weighted and value-weighted market indices of NYSE stocks. Columns (1)–(8) are based upon annual returns, whereas columns (9)–(11) are based upon monthly returns.

	Portfolios formed on the basis of ranking-period returns					Portfolios formed on the basis of ranking-period betas			Portfolios formed on the basis of ranking-period returns		
		Computed using EW index		Computed using VW index			Computed using EW index			Computed using EW index	
Portfolio	Average annual return (%) (1)	Alpha (2)	Beta (3)	Alpha (4)	Beta (5)	Average annual return (%) (6)	Alpha (7)	Beta (8)	Average monthly return (%) (9)	Alpha (10)	Beta (11)
1	27.3	−0.2	1.65	2.7	1.95	21.0	−3.0	1.42	2.36	0.26	1.52
2	23.0	0.5	1.31	2.5	1.62	19.4	−3.5	1.34	1.90	0.07	1.30
3	21.0	0.1	1.20	1.9	1.51	20.3	−1.2	1.25	1.80	0.05	1.23
4	21.2	0.9	1.16	2.9	1.45	20.4	−0.8	1.22	1.73	0.09	1.14
5	20.5	1.2	1.09	2.8	1.39	21.0	−0.6	1.24	1.65	0.09	1.07
6	19.9	0.7	1.08	2.2	1.40	20.2	0.0	1.15	1.59	0.06	1.05
7	19.4	0.0	1.09	1.6	1.40	20.2	0.1	1.14	1.52	0.01	1.03
8	18.5	1.5	0.94	2.9	1.24	19.8	0.1	1.12	1.48	0.06	0.95
9	17.6	0.2	0.95	1.7	1.26	18.5	−0.2	1.04	1.41	−0.03	0.98
10	17.8	0.7	0.94	2.1	1.24	18.3	−0.5	1.06	1.43	0.03	0.94
11	16.9	0.2	0.91	1.4	1.22	19.3	0.5	1.05	1.35	−0.04	0.93
12	16.6	0.1	0.89	1.2	1.22	17.3	0.0	0.95	1.34	−0.03	0.92
13	16.7	0.2	0.90	1.4	1.22	17.2	0.4	0.91	1.33	−0.00	0.88
14	16.1	−0.2	0.88	0.8	1.21	17.2	0.8	0.89	1.29	−0.06	0.90
15	15.5	−0.2	0.84	0.9	1.16	16.2	0.9	0.82	1.25	−0.07	0.87
16	15.3	−0.6	0.85	0.3	1.18	15.4	0.6	0.78	1.20	−0.07	0.83
17	14.6	0.1	0.76	1.0	1.08	15.2	1.4	0.72	1.16	−0.05	0.78
18	14.5	−1.3	0.85	−0.5	1.18	14.2	1.7	0.62	1.10	−0.12	0.79
19	14.3	−1.3	0.84	−0.7	1.19	14.4	1.1	0.67	1.11	−0.12	0.79
20	13.3	−2.7	0.86	−2.0	1.21	13.7	2.1	0.56	1.01	−0.24	0.81
Mean	18.0	0.0	1.00	1.35	1.32	18.0	0.0	1.00	1.45	−0.06	0.98
$r_1 - r_{20}$	14.0	2.5	0.79	4.7	0.74	7.3	−5.1	0.86	1.35	0.50	0.71

difference in post-ranking betas between the extreme winner and loser portfolios is 0.79. Given a market risk premium $(r_m - r_f)$ in the 14–15% range using an equally-weighted portfolio of NYSE stocks, the CAPM predicts a difference in returns of approximately 11%, leaving only about 3% of the 14.0% difference unaccounted for. Indeed, using this approach, Ball and Kothari report a difference in alphas between extreme winner and loser portfolios of 3.9% per year, which they view as economically insignificant. Using our sample, we find an even smaller difference in alphas between extreme portfolios: only 2.5% per year.

Although not apparent from the numbers reported in table 1, the beta estimates for winners and losers are very different depending on whether the realized market risk premium $(r_m - r_f)$ is positive or negative. This raises a question, discussed in the appendix, about what beta really is measuring. Table 7 reports the beta estimates for up and down markets separately.

The conclusion that most of the difference in post-ranking returns between winners and losers can be accounted for as compensation for risk bearing is heavily dependent upon the Sharpe–Lintner CAPM's assumption that the return per unit of beta risk provided by the market is $r_m - r_f$. However, numerous empirical studies (see footnote 2) have invariably found a much flatter slope.

In order to estimate the empirical relation between risk and return, we form portfolios on the basis of ranking-period betas, using the same sample and the same methodology as in columns (1)–(3). The ranking-period beta of each firm has been calculated on the basis of a 60-observation regression using monthly returns during the ranking period. For each of the 52 ranking periods, firms are then ranked on the basis of these betas, and assigned to one of twenty portfolios. The post-ranking-period portfolio betas are then estimated using the RATS procedure during each of the five post-ranking years with annual returns. In columns (6)–(8), we report the average annual returns and the average alphas and betas computed using the RATS methodology for the five post-ranking years for portfolios formed on the basis of ranking-period betas. The dispersion in betas between the extreme portfolios reported in column (8) is 0.86, slightly greater than the 0.79 reported in column (3). This large difference in betas in column (8), however, is associated with a difference in returns between the two extreme portfolios of only 7.3%, dramatically less than the 14.0% reported when portfolios are formed on the basis of ranked prior returns. It should be noted that the only difference between columns (1)–(3) and (6)–(8) is in how the portfolios are formed: the universe of firms and the estimation methodology are identical.

Using the twenty post-ranking-period portfolio returns and betas reported in columns (6) and (8), respectively, we estimate the market compensation per unit of beta risk. The resulting regression has an intercept of 8.5% and a slope of 9.5%. These coefficients are consistent with those reported by other

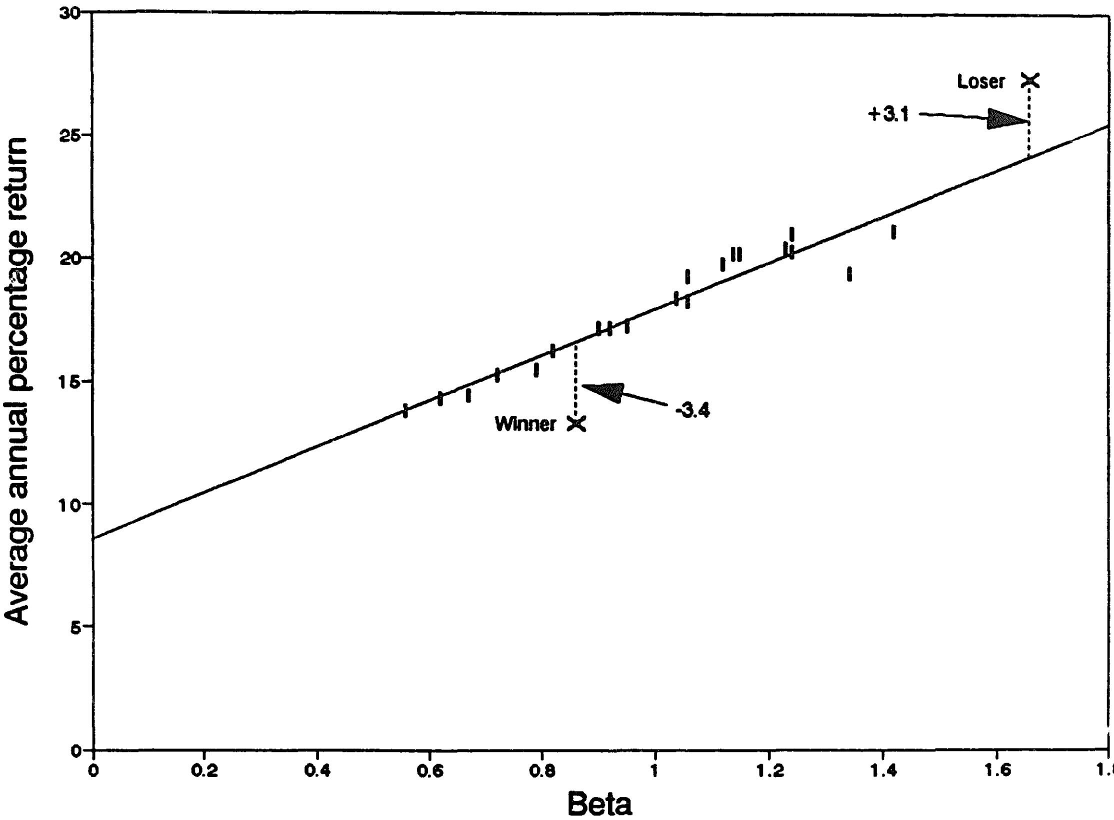

Fig. 1a. Plot of the empirical security market line (SML) calculated using annual data from the realized post-ranking-period returns and betas for twenty portfolios formed on the basis of ranking-period betas, and the realized post-ranking-period return on extreme winner and loser portfolios.

The empirical SML is estimated from the twenty portfolio returns and betas reported in columns (6) and (8) of table 1. The empirical SML has an intercept of 8.5% and a slope of 9.5%. Alphas are calculated as deviations from the empirical SML.

researchers (see footnote 2). Note that the 8.5% intercept is considerably higher than the average risk-free rate during the sample period of about 3.5%, and the slope coefficient of 9.5% is considerably lower than the 14–15% market risk premium. (In fact, the RATS procedure may overestimate the relation between realized returns and beta, because the betas are estimated contemporaneously with the post-ranking-period returns.) In other words, differences in betas do not generate differences in returns during the sample period as great as assumed by the Sharpe–Lintner CAPM.

In figs. 1a and 1b, we have plotted the regression equation estimated from the twenty portfolios formed on the basis of prior betas. The two extreme winner and loser portfolios are also plotted. In fig. 1a, we use annual data from columns (6) and (8) of table 1. In fig. 1b, we use monthly data (not reported in table 1). Using annual data, the extreme winner portfolio underperforms a portfolio with the same beta by 3.4%, while the extreme loser

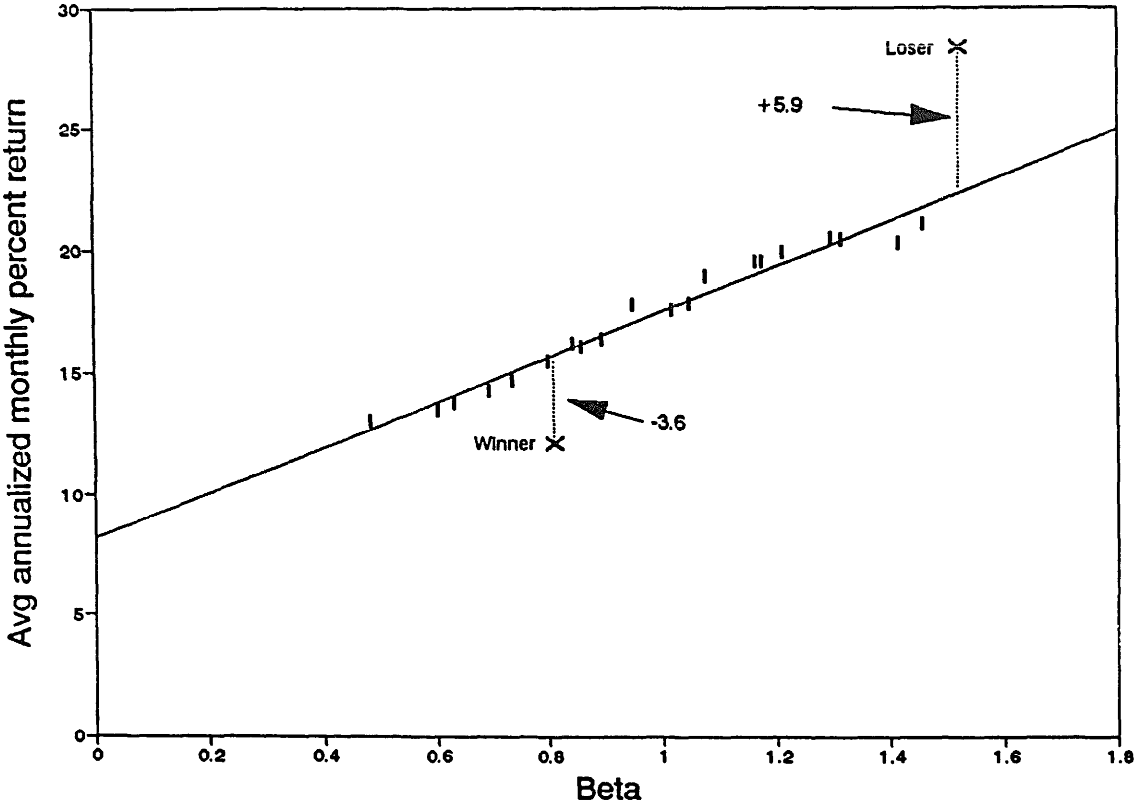

Fig. 1b. Similar to fig. 1a, except that monthly returns are used, which are then annualized by multiplying by 12 before plotting.

The empirical SML has an intercept of 8.2% and a slope of 9.3%. Alphas are calculated as deviations from the empirical SML. The mean annualized return is 17.5% rather than the 18.0% in fig. 1a due to our procedure of multiplying the average monthly returns by 12, rather than compounding them.

portfolio outperforms a portfolio with the same beta by 3.1%. Thus, the difference in abnormal returns is 6.5%, substantially higher than the 2.5% reported in column (2). The difference between these two numbers is attributable to different assumptions about the slope of the security market line (SML). Using the Sharpe–Lintner model's theoretical risk premium results in a lower estimate of the overreaction effect than when the empirical risk premium is used.

To examine the sensitivity of the results to the choice of a market index, columns (4) and (5) present results for annual measurement intervals using a value-weighted market index. The betas are all above 1.0, reflecting the fact that the equally-weighted index itself has a beta of 1.3 with respect to the value-weighted index. The difference in alphas between the extreme winners and losers widens from the 2.5% reported using an equally-weighted market index to 4.7% using a value-weighted index. Using the empirical security market line increases these spreads.

The discussion so far has focused on annual measurement intervals, even though monthly measurement intervals are much more commonly used in financial research. To examine the sensitivity of the results to the use of different measurement intervals, in columns (9)–(11) of table 1 we report monthly returns, alphas, and betas using an equally-weighted index. This procedure produces a slightly smaller spread in betas (0.71 vs 0.79 when annual measurement intervals are used) and a greater difference in abnormal returns (0.50% per month, or 6.0% per year) between extreme winner and loser portfolios. Using the empirical security market line calculated from monthly data with portfolios formed on the basis of ranked prior betas, extreme losers outperform extreme winners by 9.5% per year. With a value-weighted index and monthly data, the difference in alphas between extreme losers and winners is 12% per year using the Sharpe–Lintner model as the benchmark.[6] (These results are not reported here.) Applying a benchmark based upon the empirical security market line yields an even larger difference.

2.3. *Size-adjusted excess returns*

We have focused thus far on adjusting for differences in betas between winners and losers. However, winners and losers differ on another dimension as well. Prior research [e.g., Zarowin (1990)] has found that losers have lower market capitalizations than winners, on average, indicating that measurement of excess returns must be careful to control for size effects. The correlation of size and prior returns is apparent in fig. 2, which plots the percentage of each size quintile that falls into each prior return quintile. (We plot quintile results, rather than the twenty portfolios that we use in the empirical work, to minimize the clutter that would otherwise obscure the figure.) For example, fig. 2 shows that in the smallest size quintile, 40% of the firms are in the extreme loser quintile, while only 10% are in the extreme winner quintile. Because of this correlation between size and prior returns, a simple size adjustment may cause the extent of any overreaction effect to be underestimated.

In fig. 3, we plot the joint distribution of annual raw percentage returns for the same quintile portfolios used in fig. 2. Inspection of this figure shows that, holding size constant, returns are higher the lower are prior returns, and holding prior returns constant, returns are higher the smaller is size. On average, holding size constant, the extreme loser quintile has a 5.4% higher

[6] A caveat is in order, however, in regard to the use of monthly returns. As Conrad and Kaul (1991) discuss, monthly arithmetic returns on low-priced stocks are biased upwards in a manner that overestimates the magnitude of size and prior return effects. This is because small firms and losers are more frequently low-priced stocks. Our annual return measures, however, suffer from minimal bias.

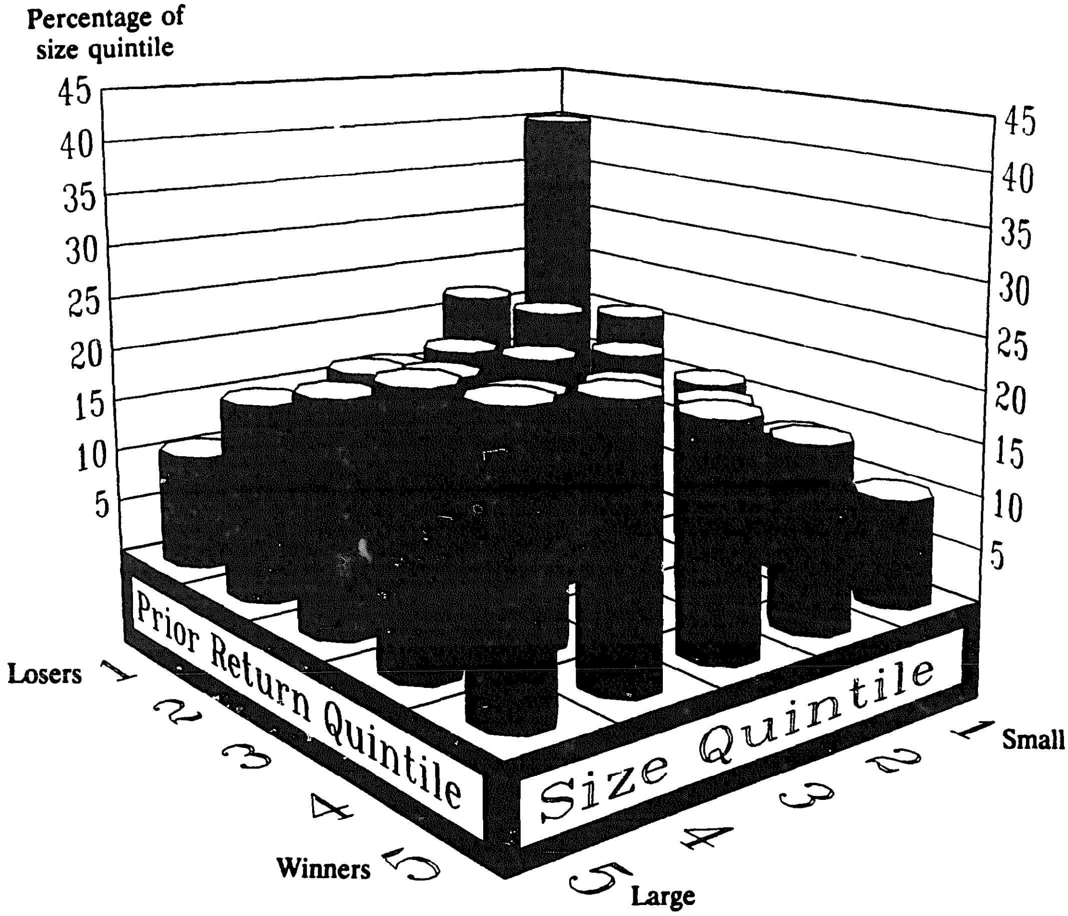

Fig. 2. The joint distribution of firms categorized by size and prior returns.

For each size quintile, the percentage of firms falling in each prior return quintile is plotted. Quintile portfolios are plotted rather than the twenty portfolios used in the empirical work because 400 portfolios (20 × 20) produces too cluttered a figure compared with the 25 portfolios plotted.

average annual return than the extreme winner quintile. On average, holding prior returns constant, the smallest size quintile has an 8.2% higher average annual return than the largest size quintile.

In column (1) of table 2, we report the average annual returns on twenty portfolios [these numbers are the same as in column (1) of table 1]. In column (2), we report the returns on control portfolios formed by matching on size, which we refer to as size-control portfolios. To construct the size-control portfolios, we rank the population of firms at the end of each of the 52 portfolio formation periods on the basis of market capitalization, and then assign the firms to twenty portfolios formed on the basis of size. In computing the average annual returns on the twenty size portfolios, we follow the same procedure used in table 1 with the twenty prior-return portfolios. For each of the twenty prior-return portfolios, we form a size-control portfolio. This

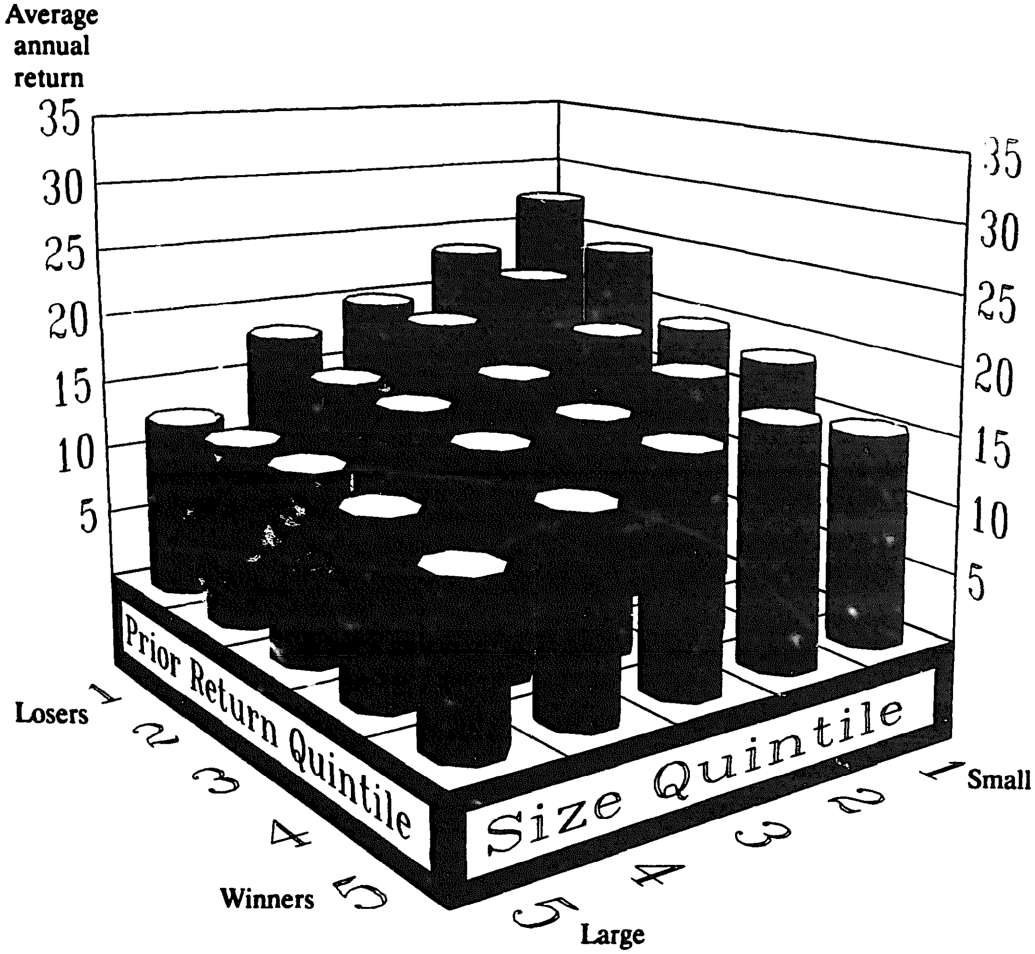

Fig. 3. The joint distribution of average annual returns in the post-ranking period categorized by size and prior returns.

The average annual return on the smallest quintile of losers is 27.37%, while the average annual return on the largest quintile of winners is 11.59%.

size-control portfolio is constructed to have the same size composition as its corresponding prior return portfolio, with the weights being determined by the proportion of the prior-return portfolio that falls in each size classification.

In column (3) of table 2, we report the average annual returns on size-control portfolios formed in a manner identical to that employed in column (2), with the exception that the population of firms from which the size portfolios are drawn has been purged of firms in prior return portfolios 1–5 (losers) and 16–20 (winners). Because of the correlation of size and prior returns, more than 50% of the smallest (and largest) firms are purged, and slightly less than 50% of moderate-size firms are purged. The purpose of this purging is to minimize the confounding of any overreaction effects with size effects.

Table 2

Average annual post-ranking-period percentage returns for twenty portfolios of firms ranked by their five-year ranking-period returns, size-control portfolios with and without losers and winners purged, and the associated size-adjusted returns.

The twenty size-control portfolios are constructed to have approximately the same market values as the twenty ranked portfolios. Excess returns are computed two different ways: (i) size-adjusted returns using all firms (unpurged) and (ii) size-adjusted returns after the portfolios have been purged of all firms in the top five and the bottom five portfolios of prior returns (purged).

	Average annual return (%) in years +1 to +5				Size-adj. returns (%) $e = r_p - r_s$	
	Ranked firms	Control firms				
Portfolio	Ranked firms (r_p) (1)	Unpurged (r_s) (2)	Purged (r_s) (3)	Difference (2) − (3) (4)	Unpurged (1) − (2) (5)	Purged (1) − (3) (6)
1	27.3	23.4	20.4	3.0	3.9	6.9
2	23.0	21.3	19.3	2.0	1.7	3.7
3	21.0	20.6	19.0	1.6	0.4	2.0
4	21.2	20.0	18.8	1.2	1.2	2.4
5	20.5	19.4	18.0	1.4	1.1	2.5
6	19.9	18.8	18.0	0.8	1.1	1.9
7	19.4	18.9	18.1	0.8	0.5	1.3
8	18.5	18.1	17.6	0.5	0.4	0.9
9	17.6	17.9	17.4	0.5	−0.3	0.2
10	17.8	17.5	17.2	0.3	0.3	0.6
11	16.9	17.3	16.9	0.4	−0.4	0.0
12	16.6	17.0	16.7	0.3	−0.4	−0.1
13	16.7	16.9	16.8	0.1	−0.2	−0.1
14	16.1	16.6	16.3	0.3	−0.5	−0.2
15	15.5	16.6	16.4	0.2	−1.1	−0.9
16	15.3	16.6	16.4	0.2	−1.3	−1.1
17	14.6	16.2	16.1	0.1	−1.6	−1.5
18	14.5	16.0	16.1	−0.1	−1.5	−1.6
19	14.3	16.0	15.9	0.1	−1.7	−1.6
20	13.3	16.0	16.1	−0.1	−2.7	−2.8
Mean	18.0	18.0	17.4	0.6	0.0	0.6
$r_1 - r_{20}$	14.0	7.4	4.3	3.1	6.6	9.7

In column (5) of table 2, we report excess returns computed by subtracting the unpurged size-control returns. There is a nearly monotonic decrease in the excess returns as one goes from portfolio 1 (the losers) to portfolio 20 (the winners). The difference in excess returns between the extreme portfolios is 6.6% per year during the five post-ranking years.

In column (6), we report the excess returns computed using the purged size-control portfolios. The pattern in column (5) is accentuated, confirming our conjecture that controlling for size without taking the correlation of size and prior returns into account understates the overreaction effect. The difference between the extreme portfolio excess returns is 9.7% per year during the five post-ranking years. From these numbers, it appears that there is an economically-significant overreaction effect above and beyond any size effect.

2.4. Seasonal patterns in returns, tax-loss selling, and momentum

In table 3, we report the average post-ranking-period raw and size-adjusted (using purged size controls) returns using annual, January, and February–December returns. The February–December returns are 11-month returns, computed by compounding the monthly returns. In columns (1)–(6), the portfolios are formed on the basis of five-year prior returns; the annual numbers are identical to those reported in columns (1) and (6) of table 2. In columns (7)–(12), the portfolios are formed on the basis of one-year prior returns, although the post-ranking period remains five years. The population of returns used in columns (1)–(6) and (7)–(12) are identical; what is different is the ranking criteria to form the twenty portfolios.

Inspection of columns (1)–(6) discloses that the overreaction effect is disproportionately concentrated in January, consistent with the graphical evidence presented in De Bondt and Thaler's (1985) fig. 3. While the differences in average annual and January returns between portfolios 1 and 20 are reliably different from zero, the February–December difference is not significantly different from zero at conventional levels for either raw returns or size-adjusted returns. The January seasonal raises the question of whether there is an independent overreaction effect, above and beyond tax-loss selling effects.

To distinguish between these two effects, in columns (7)–(12) we report returns on portfolios formed on ranked one-year returns, which should produce a cleaner measure of the influence of tax-loss selling effects. The choice of one-year formation periods to examine tax-loss selling effects is consistent with prior work in this area. [Reinganum (1983), Chan (1986), and others form portfolios based upon return intervals that correspond to the short-term capital gains holding period, which has varied from six to twelve months at various times during our sample period, and Roll (1983) uses one-year returns.] In columns (7)–(12), there are much smaller return reversals than in columns (1)–(6), and they are much more concentrated in January. Using annual size-adjusted returns, the difference in returns between the extreme winners and losers is 9.7% per year using five-year ranking periods, but only 3.5% per year using one-year ranking periods. As in columns (1)–(6), only the annual and January return differences are reliably different from zero. Although the return differences ($r_1 - r_{20}$) are generally lower in columns (7)–(12) than in columns (1)–(6), the p-values tend to be similar because there is less time-series variability and less autocorrelation in the portfolio return series when one-year ranking periods are used than when five-year ranking periods are used.

Table 3

Seasonal patterns in raw returns and size-adjusted returns for ranking periods of five years and one year.[a]

	Five-year ranking periods						One-year ranking periods					
	Avg. raw returns (%)			Avg. size-adj. returns[b] (%)			Avg. raw returns (%)			Avg. size-adj. returns[b] (%)		
Portfolio	Annual (1)	Jan. (2)	Feb.–Dec. (3)	Annual (4)	Jan. (5)	Feb.–Dec. (6)	Annual (7)	Jan. (8)	Feb.–Dec. (9)	Annual (10)	Jan. (11)	Feb.–Dec. (12)
1	27.3	13.1	12.9	6.9	7.2	−0.8	23.5	11.2	11.3	2.6	4.7	−2.2
2	23.0	8.8	13.3	3.7	3.5	0.1	20.5	7.4	12.0	1.3	1.9	−1.0
3	21.0	7.4	12.9	2.0	2.5	−0.4	19.8	6.6	12.4	0.9	1.4	−0.5
4	21.2	6.6	13.8	2.4	1.7	0.7	18.4	5.9	11.9	0.1	0.8	−0.6
5	20.5	5.7	14.0	2.5	1.2	1.2	18.7	5.5	12.6	1.0	0.8	0.4
6	19.9	5.6	13.5	1.9	1.2	0.5	18.1	5.3	12.1	0.3	0.7	−0.4
7	19.4	5.1	13.6	1.3	0.9	0.5	17.5	5.0	12.0	−0.3	0.4	−0.5
8	18.5	4.7	13.2	0.9	0.5	0.5	18.2	4.6	12.8	0.7	0.2	0.3
9	17.6	4.5	12.4	0.2	0.4	−0.2	16.8	4.4	11.8	−0.4	0.1	−0.5
10	17.8	4.2	13.0	0.6	0.2	0.5	17.5	4.0	12.8	0.2	−0.2	0.4
11	16.9	4.1	12.3	0.0	0.1	0.1	16.9	4.3	12.3	−0.6	0.0	−0.2
12	16.6	3.9	12.2	−0.1	0.0	−0.1	17.6	4.0	12.8	0.3	−0.1	0.3
13	16.7	3.6	12.6	−0.1	−0.1	0.2	17.0	3.9	12.5	0.0	−0.3	0.3
14	16.1	3.3	12.3	−0.2	−0.3	0.3	16.7	3.8	12.3	−0.5	−0.4	0.0
15	15.5	3.3	11.8	−0.9	−0.3	−0.4	16.8	3.6	12.7	0.0	−0.4	0.5
16	15.3	3.2	11.4	−1.1	−0.4	−0.8	16.9	3.7	12.6	−0.1	−0.3	0.3
17	14.6	3.1	11.0	−1.5	−0.5	−0.9	16.9	3.5	12.7	0.1	−0.6	0.7
18	14.5	3.0	10.8	−1.6	−0.4	−1.2	16.3	3.6	12.2	−1.2	−0.7	−0.3
19	14.3	2.7	10.9	−1.6	−0.7	−1.0	17.5	3.7	13.1	−0.4	−0.8	0.5
20	13.3	2.6	10.0	−2.8	−0.7	−2.1	17.7	4.3	12.8	−0.9	−0.6	−0.1
$r_1 - r_{20}$	14.0	10.5	2.9	9.7	7.9	1.3	5.8	6.9	−1.5	3.5	5.3	−2.1
p-values[c]	0.001	0.001	0.105	0.005	0.004	0.273	0.001	0.001	0.072	0.004	0.001	0.024

Portfolio	Five-year ranking periods						One-year ranking periods					
	Avg. raw returns (%)			Avg. size-adj. returns[b] (%)			Avg. raw returns (%)			Avg. size-adj. returns[b] (%)		
	Annual (1)	Jan. (2)	Feb.–Dec. (3)	Annual (4)	Jan. (5)	Feb.–Dec. (6)	Annual (7)	Jan. (8)	Feb.–Dec. (9)	Annual (10)	Jan. (11)	Feb.–Dec. (12)
$r_1 - r_{20}$ in year +1[d]	15.3	15.1	−0.7	11.0	12.6	−2.6	−7.2	8.6	[illegible]	−8.6	7.2	−15.2
p-values[c]	0.011	0.000	0.425	0.028	0.000	0.176	0.013	0.000	0.000	0.004	0.000	0.000
Tests of the hypothesis that $r_1 - r_{20}$ with five-year ranking periods = $r_1 - r_{20}$ with one-year ranking periods:												
p-values[f]	0.002	0.003	0.007	0.009	0.035	0.037						

[a]All numbers, except for the row labeled '$r_1 - r_{20}$ in year +1', are the equally-weighted averages of the five post-ranking years, for all 52 post-ranking periods beginning in 1931–1982. The January returns are monthly averages. The February–December returns are averages of eleven-month compounded returns.

[b]The size-control portfolios have been purged of extreme winners and losers, using the procedures described in table 2. The purged firms for the one-year ranking periods are those in the bottom 25% and the top 25% of one-year returns.

[c]The p-values test the hypothesis that the mean value of $r_1 - r_{20}$ is zero; p-values are computed adjusting for fourth-order autocorrelation as follows, and the standard deviation of the mean value of $r_1 - r_{20}$ is computed as

$$\text{s.d.} = \frac{\sigma}{T}\sqrt{T + 2(T-1)\rho_1 + 2(T-2)\rho_2 + 2(T-3)\rho_3 + 2(T-4)\rho_4},$$

with $T = 52$, where σ is the standard deviation of the portfolio returns and ρ_n is the estimated nth-order simple autocorrelation coefficient. (Four lags are used because of the five-year overlapping post-ranking periods.) The T observations are the time series of five-year average portfolio returns, expressed as annual numbers.

[d]The numbers in this row represent the average value of $r_1 - r_{20}$ in the first year of the five post-ranking years.

[e]The p-values test the hypothesis that the mean value of $r_1 - r_{20}$ is zero. A time series of 52 nonoverlapping year +1 observations are used to calculate the standard deviation of the mean, adjusting for first-order autocorrelation. The autocorrelation coefficients are as high as 0.406 for the size-adjusted January returns in column (5). For February–December returns, the autocorrelations are insignificantly different from zero.

[f]The p-values are calculated from a time-series of 52 values of $(r_1 - r_{20})_{5,t} - (r_1 - r_{20})_{1,t}$, adjusted for fourth-order autocorrelation, where $(r_1 - r_{20})_{i,t}$ is the average return difference over the five-year post-ranking period starting in year t with ranking period of length i.

In the last row of the table, we report the results of a test of the hypothesis that the return differences $(r_1 - r_{20})$ using five-year ranking periods are the same as those using one-year ranking periods. The p-values of 0.002 to 0.037 indicate that the higher return differences using five-year ranking periods are generally reliably so, even in February–December.

While the portfolios formed on the basis of five-year returns display greater return reversals during the subsequent five years than those formed on the basis of one-year returns, an interesting pattern is obscured. Specifically, the portfolios formed on the basis of one-year returns display return *momentum*, as shown in the row '$r_1 - r_{20}$ in year $+1$'. In this row, we report the average difference in returns on extreme portfolios during the first post-ranking year. Focusing on size-adjusted returns, in the first post-ranking year, prior five-year losers outperform winners by 11.0% in column (4), whereas prior one-year losers *underperform* winners by 8.6% in column (10). This underperformance is entirely in the February–December period, where column (12) reports that one-year losers underperform winners by 15.2%. In plain English, when winners and losers are chosen on the basis of one-year returns, losers continue to lose and winners continue to win during the next year. Similar momentum patterns are also reported by De Bondt and Thaler (1985, table 1), Ball and Kothari (1989, table 5), and Jegadeesh and Titman (1991). These momentum patterns may explain the Value Line anomaly [see Huberman and Kandel (1987)] and the post-earnings announcement drift anomaly [see Bernard and Thomas (1989, 1990)].

3. Multiple regression tests

In the previous section, we controlled for, respectively, beta and size in computing abnormal portfolio returns. In this section, we present multiple regression evidence that simultaneously incorporates the effects of beta, size, and prior returns on post-ranking period returns. This analysis uses 400 portfolios, each containing an unequal number of firms, formed on the basis of independent rankings of firm size and prior returns. For each of these portfolios, a beta is calculated from a pooled (across both post-ranking years and firms) regression, using $r_{it} - r_{ft}$ as the dependent variable and $r_{mt} - r_{ft}$ as the explanatory variable, where r_{it} is the return on firm i in year t. The portfolio excess return is also calculated as the pooled (across both firms and post-ranking years) average excess return.[7]

[7]When annual returns are used, if a given portfolio, e.g., the largest extreme losers (size portfolio 20, return portfolio 1) has a total of 83 firms in it over the entire 52 formation periods (an average of 1.6 firms per formation period), there are up to 83×5 annual returns (if each of the 83 firms lasts for all five post-ranking years).

In table 8 of the appendix, we report results using two alternative procedures for calculating betas and returns for each of the 400 portfolios. In general, the results are qualitatively similar.

In panels A and B of table 4, we report the results of estimating eq. (2) using 400 portfolios constructed on the basis of independent rankings of prior returns and size:

$$r_p - r_f = a_0 + a_1 SIZE_p + a_2 RETURN_p + a_3 beta_p + e_p. \tag{2}$$

The explanatory variables in panels A and B are relative market capitalization (*SIZE*), measured as the portfolio rank (1 small, 20 large), prior five-year returns (*RETURN*), measured as the portfolio rank (1 losers, 20 winners), and the portfolio beta.[8] In panel A, using annual returns, we find that all three explanatory variables are reliably different from zero and the coefficients have the predicted signs. Furthermore, a large fraction of the variation in portfolio returns is explained (the R^2 is 0.68). The *RETURN* coefficient of -0.254 implies that after controlling for size and beta, extreme losers outperform extreme winners by 4.8% per year on average for the five post-ranking years. [Since *RETURN* (and *SIZE*) is measured as the portfolio rank, -0.254 multiplied by (1 minus 20) results in the 4.8% difference.] Also noteworthy is that in panel A, the coefficient on beta of 5.438% is lower than the 9.5% slope reported in fig. 1a. Apparently, estimates of the SML slope from single-variable regressions suffer from an omitted variable bias. Another aspect worth noting is that the magnitude of the overreaction effect is nearly as great as that of the size effect, as can be seen by comparing the two coefficients.

A straightforward approach to estimating the t-statistics for table 4 would be to use the standard errors from the pooled regressions with 400 observations. The resulting t-statistics, however, would be vastly overstated, because the pooled regression standard errors do not account for the time-series variability of the empirical relations. Consequently, the t-statistics that we report in panel A are based upon the time-series variability of the coefficients from 52 annual cross-sectional regressions. In general, these coefficients would be intertemporally dependent. Furthermore, our procedure of using overlapping post-ranking periods will induce strong autocorrelation in the parameter estimates. Thus, in computing the standard errors for the point

[8] We have explored some alternatives to our use of portfolio rankings as measures of prior returns and size. For example, using the actual prior return rather than the portfolio rank produces a slightly better fit and a stronger measured overreaction effect. One reason for our preference for the use of portfolio rankings to measure size is that market capitalizations changed substantially over time during our 52-year sample period. This poses a problem for pooling observations over time. For a detailed discussion of some of the issues involved, see Chan, Hamao, and Lakonishok (1991). We have not attempted to conduct a comprehensive examination of alternative specifications, for this would then introduce data-snooping biases.

Table 4

OLS regressions of average percentage excess returns for the first five post-ranking years for portfolios of NYSE firms formed on the basis of size and prior returns.

For each of the 52 ranking periods ending on December 31 of 1930 to 1981, firms are independently ranked on the basis of their December 31 market value and their five-year prior return, and assigned to one of 400 portfolios. Each portfolio beta is the pooled (over firms and post-ranking years) beta for the firms in the cell, calculated using annual returns and equally-weighted market returns. *SIZE* is measured as the portfolio ranking (1 to 20, with 1 being smallest), and *RETURN* is measured as the portfolio ranking (1 to 20, with 1 being the most extreme prior losers). In panels C and D, *DS* is a dummy variable equal to one if a portfolio is among the bottom 40% of *SIZE* vitiles, *DM* is a dummy variable equal to one if a portfolio is among *SIZE* portfolios 9 to 16 (the middle 40%), and *DL* is a dummy variable equal to one if a portfolio is among the largest 20% of *SIZE* portfolios. *T*-statistics are in parentheses. These are computed using a Fama–MacBeth (1973) procedure adjusted for fourth-order autocorrelation as follows: the *t*-statistic for coefficient a_i is computed as a_i/s.e., where

$$\text{s.e.} = \frac{\sigma}{T}\sqrt{T + 2(T-1)\rho_1 + 2(T-2)\rho_2 + 2(T-3)\rho_3 + 2(T-4)\rho_4}\,,$$

with $T = 52$, where σ is the time-series standard deviation of the coefficient estimates and ρ_t is the estimated *n*th-order simple autocorrelation coefficient. (Four lags are used because of the five-year overlapping post-ranking periods.) The *T* observations are the time series of cross-sectional regression coefficients. The first-order autocorrelations in panel A vary from 0.142 for the intercept to 0.649 for the coefficient on *RETURN*. The R^2 values are based upon the pooled regressions, and do not reflect the year-to-year variability in the regressions.

$$r_p - r_f = a_0 + a_1 SIZE_p + a_2 RETURN_p + a_3 Beta_p + e_p$$

	Coefficient estimates			
Intercept	*SIZE*	*RETURN*	*Beta*	R^2_{adjusted}
Panel A: Annual percentage returns				
14.443	−0.364	−0.254	5.438	0.68
(10.517)	(−3.779)	(−2.996)	(1.707)	
Panel B: Monthly percentage returns, all months				
1.236	−0.031	−0.023	0.369	0.68
(4.671)	(−2.926)	(−3.039)	(1.393)	

$$r_p - r_f = a_0 + a_1 SIZE_p + a_2 DS \cdot RETURN_p + a_3 DM \cdot RETURN_p + a_4 DL \cdot RETURN_p + a_5 Beta_p + e_p$$

	Coefficient estimates					
Intercept	*SIZE*	*DS* · *RETURN*	*DM* · *RETURN*	*DL* · *RETURN*	*Beta*	R^2_{adjusted}
Panel C: Annual percentage returns						
18.113	−0.597	−0.417	−0.182	−0.136	4.364	0.72
(9.915)	(−5.440)	(−4.2[illegible]7)	(−2.009)	(−1.433)	(1.298)	
Panel D: Monthly percentage returns, all months						
1.631	−0.055	−0.039	−0.018	−0.010	0.238	0.73
(6.431)	(−5.675)	(−4.733)	(−2.235)	(−1.326)	(0.898)	

estimates reported in panel A, we have adjusted for fourth-order autocorrelation using the formula reported in table 4. Without these adjustments, the t-statistics from the pooled cross-sectional regressions are approximately three times as large.

To examine the sensitivity of our conclusions to the use of annual returns rather than monthly returns (which are more commonly used in empirical studies), panel B reports results from monthly regressions. (In panels B and D, we use monthly returns to calculate betas, and we use the same procedure to calculate t-statistics as used in panel A.) These results, after multiplying the monthly coefficients by 12, are qualitatively similar to those in panel A. The overreaction effect is slightly stronger using monthly returns, with panel B reporting that extreme losers outperform extreme winners by 5.2% per year, ceteris paribus. The compensation per unit of beta is 4.4% per year using monthly data, a decrease from the 5.4% per year reported in panel A using annual returns.

In panels C and D, we permit the overreaction effect to vary by firm size by estimating three different slope coefficients, depending upon whether a portfolio is comprised of small, middle-size, or large firms. Panel C reveals that the overreaction effect is strongest among smaller firms. The *DS · RETURN* coefficient of -0.417 implies a 7.9% per year abnormal return difference between portfolios 1 and 20 for the smallest (bottom 40%) firms. For middle-size firms, this difference is 3.5%, while for the larger (upper 20%) firms, the difference is 2.6%. This relation between firm size and the extent of overreaction has not previously been emphasized.

To examine the robustness of our table 4 results, we have also run the regressions for the 1931–56 and 1957–82 subperiods. Our results (not reported here) indicate that there is a significant overreaction effect in both subperiods, although the effects are stronger in the second subperiod, in contrast to the evidence on index autocorrelations over three-to-five year periods reported by Fama and French (1988), who find weaker results for subperiods excluding the 1930s.

The evidence in panels C and D of table 4 demonstrates that the overreaction effect is stronger for smaller firms. This finding deserves further analysis. In table 5, we examine the extent of overreaction within each of ten size deciles by reporting regression results with *RETURN* and beta as explanatory variables. Each of the ten regressions uses the 40 portfolios out of the 400 formed for our table 4 analysis that correspond to the appropriate size grouping. In table 5, the coefficient on *RETURN* is generally closer to zero the larger is the size decile. The last column in the table reports the implied annual difference in returns between the extreme winner and loser portfolios, holding size and beta constant. These differences in returns are plotted in fig. 4. The numbers demonstrate that for the smaller firms an overreaction effect on the order of 10% per year (50% per five years, even before compounding)

Table 5

OLS regressions of annual average percentage excess returns on ranking-period returns and beta by size decile.

RETURN is measured 1 to 20 (1 = losers, 20 = winners), where prior returns are measured over the five years prior to the portfolio formation date. Firms are assigned to size deciles (1 = small, 10 = large) on the basis of their market capitalization at the end of the ranking period. The beta of each portfolio is calculated as the pooled (over firms and post-ranking years) beta. Each of the ten regressions uses forty observations (two ranks of size with twenty prior-return portfolios in each size rank). *T*-statistics, computed using the fourth-order autoregressive process described in table 4, are in parentheses.

	$r_p - r_f = a_0 + a_1 RETURN_p + a_2 Beta_p + e_p$ Coefficient estimates				
Size decile	*Intercept*	*RETURN*	*Beta*	R^2_{adjusted}	$-19 \times$ *RETURN* coefficient[a]
1	9.888 (2.463)	−0.578 (−2.119)	9.980 (2.670)	0.76	10.98%
2	27.658 (4.379)	−0.729 (−6.436)	−2.784 (−0.426)	0.74	13.85%
3	21.218 (4.723)	−0.510 (−3.382)	0.402 (0.078)	0.65	9.69%
4	18.942 (6.730)	−0.350 (−3.811)	0.739 (0.242)	0.51	6.65%
5	16.356 (3.715)	−0.140 (−2.629)	−0.641 (−0.101)	0.10	2.66%
6	14.226 (1.982)	−0.293 (−2.242)	2.489 (0.288)	0.52	5.57%
7	9.149 (4.691)	−0.153 (−1.755)	4.838 (2.463)	0.51	2.91%
8	8.018 (3.012)	−0.113 (−0.764)	5.171 (1.000)	0.37	2.15%
9	6.101 (1.634)	−0.016 (−0.149)	4.524 (0.572)	0.01	0.30%
10	5.080 (1.932)	0.040 (0.327)	2.471 (0.466)	0.01	−0.76%

[a]Multiplying the coefficients on *RETURN* by −19 gives the expected difference in annual returns for the five post-ranking years between prior-return portfolios 1 and 20, controlling for beta, for firms categorized by their size decile.

is present, while for the largest 20% of NYSE firms (roughly the S&P 500) no overreaction effect is apparent. Since individuals are the primary holders of the smaller firms, while institutions are the dominant holders of the larger firms, the results are consistent with the hypothesis that individuals overreact, while institutional investors do not.

Our finding that overreaction is concentrated among smaller firms is consistent with results reported in Fama and French (1988), where small-firm

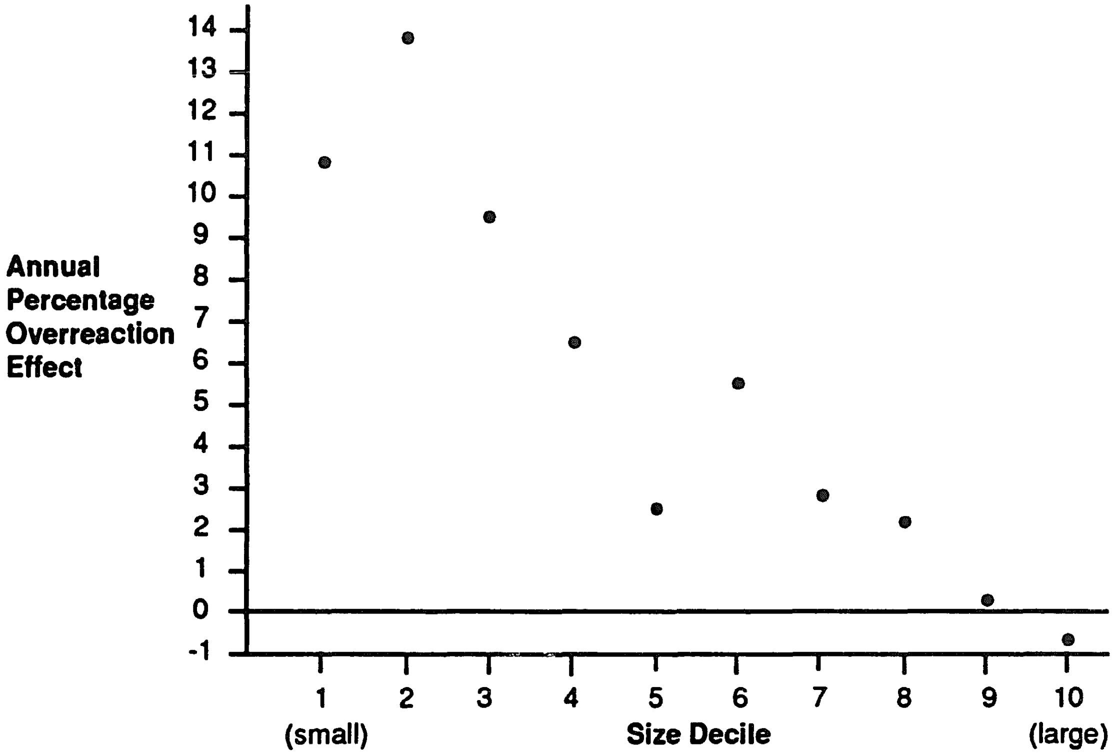

Fig. 4. The difference in annual abnormal returns between extreme loser and winner portfolios by size decile.

The numbers plotted are the coefficients on *RETURN* in table 5 multiplied by -19. This represents the expected difference in annual returns for the five post-ranking years between prior return portfolios 1 and 20, controlling for beta, for firms categorized by their size decile.

portfolios are found to have greater negative serial correlation than large-firm portfolios. Furthermore, Poterba and Summers (1988) provide evidence that there is greater long-term negative serial correlation in countries with less-developed capital markets than in countries such as the U.S. or Britain. Together, this evidence is consistent with the hypothesis that the further one moves away from large-capitalization stocks in well-developed capital markets, the more likely it is that stocks take prolonged swings away from their fundamental value.

Another noteworthy aspect of the table 5 regressions is that in contrast to the importance of the *RETURN* variable, which is statistically significant at the 0.05 level for the six smallest size deciles, the coefficient on beta is highly variable and statistically significant in only two of the ten regressions. For the largest two size deciles, which account for the majority of market capitalization, beta is far from statistically significant. For these two deciles, the compensation per unit of beta risk is substantially below the 5.4% reported in panel A of table 4 and the 9.4% reported in fig. 1a. Also noteworthy is that for these largest two deciles, the R^2s are essentially zero: neither prior returns nor beta are related to realized returns. In other words, a stock is a stock.

4. Evidence from earnings announcements

The evidence presented so far indicates that even after controlling for size and beta effects, there is an overreaction effect. However, because the magnitude of any effect measured over long intervals is sensitive to the benchmark employed, we also present evidence of overreaction around earnings announcements. Focusing on short windows such as the three-day period surrounding earnings announcements minimizes the sensitivity of results to misspecification of controls, which can provide further evidence on the existence of an overreaction effect. However, it cannot shed much light on the exact magnitude because there is no reason why the return towards fundamental value should occur on only a few discrete dates.

For the firms in the ranking periods ending in 1970–81, we searched the Compustat quarterly industrial, historical, and research files for their quarterly earnings announcements during the five years of the post-ranking periods.[9] Our search resulted in 227,522 earnings announcements. For each of the twenty portfolios formed by ranking firms on prior returns, we computed the average raw return for earnings announcements for a three-day window of $[-2, 0]$ relative to the Compustat-listed announcement date. This three-day window is commonly used in the earnings announcement literature [e.g., Bernard and Thomas (1990)].

In fig. 5, we plot the raw three-day earnings-announcement-period returns using the same size and prior-return quintiles as in figs. 2 and 3. The small losers have average returns of 0.958% per three days, while the large winners have average returns of 0.001% per three days.

Returning to the twenty portfolios, the average earnings-announcement-period return for firms in portfolio 1 (losers) is 0.63%. For firms in prior-return portfolio 20, the average earnings-announcement-period return is zero. Thus, the evidence from earnings announcements indicates that the market is systematically surprised at subsequent earnings announcements in a manner consistent with the overreaction hypothesis.

Recent research, however, finds anomalous returns at earnings announcement dates. [Much of the literature on earnings announcements is surveyed

[9] The quarterly industrial file contains only companies that are currently publicly-traded. The research file contains companies that were delisted. Combining these data files gives us a sample that covers almost all of the NYSE firms in our sample, but only for the most recent 48 quarters. Adding the historical data extends the sample back into the 1970s. Compustat's data on quarterly earnings announcement dates becomes progressively less comprehensive for earlier years, which is why we restrict our analysis to the 1970s and 1980s, rather than the full 52 years of data.

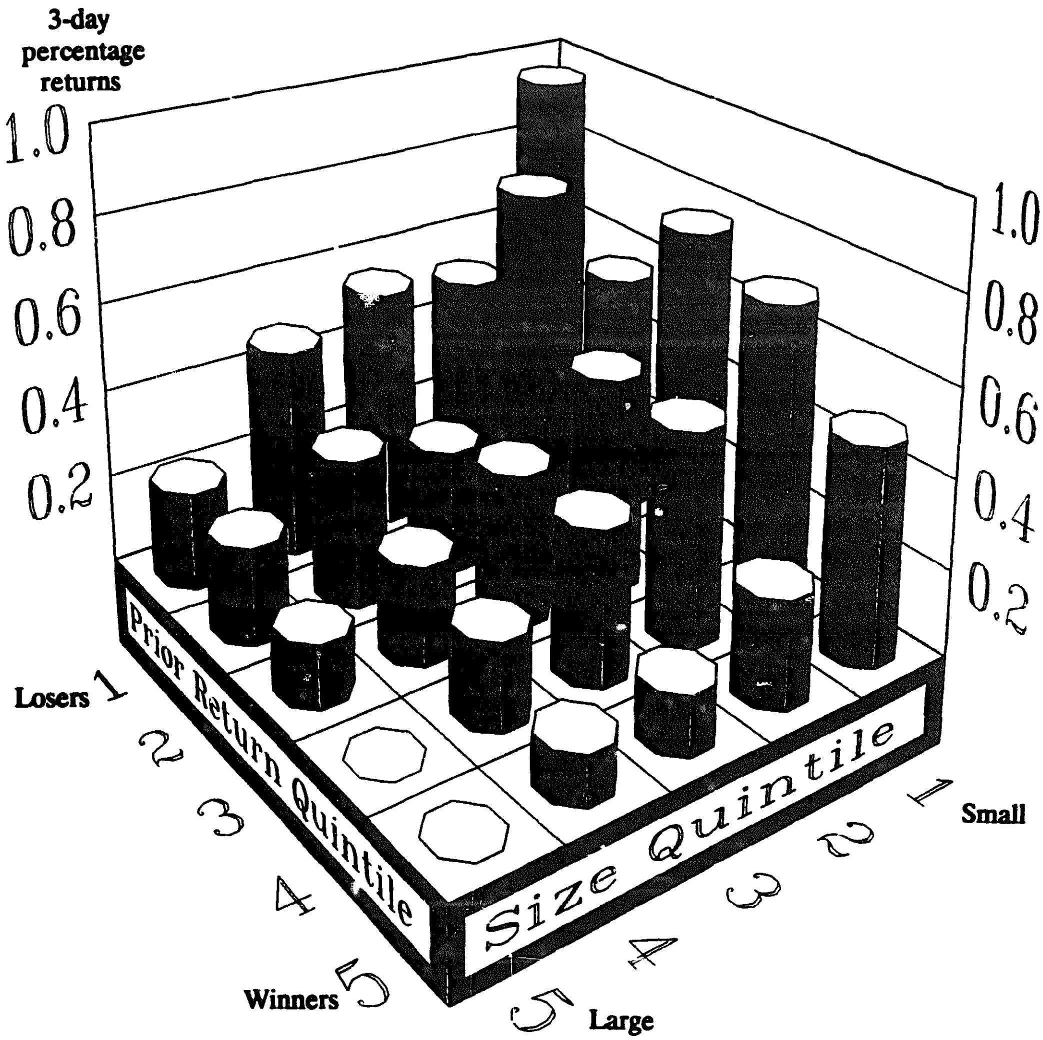

Fig. 5. The joint distribution of three-day earnings announcement returns categorized by market capitalization and prior returns.

Firms are assigned to portfolios based upon independent rankings of size and prior returns. The average three-day raw return at subsequent earnings announcements is computed for Compustat-listed quarterly earnings announcement dates during the five-year post-ranking period. The average three-day raw return is 0.001% for the largest extreme winners and 0.958% for the smallest extreme losers.

in Ball and Kothari (1991).] In particular, Chari, Jagannathan, and Ofer (1988) document that small firms tend to have higher earnings-announcement-period returns than large firms, and in our case, a disproportionate fraction of losers are small. Chari, Jagannathan, and Ofer hypothesize that because of the increased flow of information around earnings announcements, these periods are riskier than nonannouncement periods. Therefore, to examine whether past price changes affect returns around earnings an-

Table 6

Regression of three-day earnings announcement portfolio returns on size, prior returns, and beta.

394 portfolios are used (400 portfolios based on independently ranking firms by size and prior return, with six portfolios deleted which had fewer than 100 earnings announcements). Size is measured with the smallest firms in portfolio 1, and the largest in portfolio 20. Prior returns (measured over the five prior years) are also ranked from 1 to 20, with 1 being the losers. Betas are calculated for each portfolio using all earnings announcement returns for all firms in the portfolio. The dependent variable is measured as the percentage return per three-day announcement period [−2, 0], for earnings announcements made during the first five post-ranking years. Earnings announcement days are from Compustat's industrial, historical, and research tapes, for announcements during the five post-ranking years following the ranking periods ending in 1970–81. There are 227,522 earnings announcements. *T*-statistics, computed using the time-series variance of the cross-sectional regression coefficients, adjusted for first-order autocorrelation, are in parentheses.

	$R_p = a_0 + a_1 SIZE_p + a_2 RETURN_p + a_3 Beta_p + e_i$ Coefficient estimates			
Intercept	*SIZE*	*RETURN*	*Beta*	R^2_{adjusted}
0.641 (3.230)	−0.027 (−7.701)	−0.014 (−2.548)	0.111 (2.018)	0.32

nouncements, we have to control for both size and risk, which we accomplish by using an approach similar to that employed in eq. (2). The analysis uses 400 portfolios formed on the basis of independent rankings of firm size and prior returns. For each of these 400 portfolios, we compute an average raw three-day holding period return. We also calculate a portfolio beta by running a pooled market model regression (over both firms and earnings announcements) using three-day announcement-period returns and three-day market returns.

In table 6 we report the results of a regression based on 394 observations (six portfolios with less than 100 earnings announcements are deleted) where the portfolio three-day return is the dependent variable. Explanatory variables are *SIZE* (as measured by the size portfolio number), *RETURNS* (as measured by the prior returns portfolio number), and beta. The coefficients indicate that, holding beta and firm size constant, the earnings announcement returns are more positive for prior losers than winners. In particular, multiplying the coefficient of −0.0142 by (1 minus 20) is 0.27% per announcement. Since there are four quarterly earnings announcements per year, this is a difference of 1.08% during each calendar year for these 12 trading days alone, reinforcing our earlier results on the existence of an overreaction effect. Corroborating evidence is also found in Hand (1990), where differential earnings announcement effects are found depending upon the proportion of shares held by individuals.

5. Summary and conclusions

One of the most controversial issues in financial economics in recent years is the question of whether stocks overreact. De Bondt and Thaler (1985) present evidence that stocks with poor performance (losers) over the past three-to-five years outperform prior-period winners during the subsequent three-to-five years. This work has received considerable attention because the authors find a very large difference in returns between winners and losers during the five-year post-ranking period (about 8% per year), and they interpret their findings as evidence that there are systematic valuation errors in the stock market caused by investor overreaction.

Subsequent papers suggest that De Bondt and Thaler's findings are subject to various methodological problems. In particular, Ball and Kothari (1989) show that when betas are estimated using annual returns, nearly all of the estimated abnormal returns disappear in the context of the Sharpe–Lintner CAPM. In another paper, Zarowin (1990) argues that the overreaction effect is merely a manifestation of the size effect. It is apparent that the quantitative magnitude of the overreaction effect is highly sensitive to the procedures used in computing abnormal returns, particularly in any study in which abnormal returns are being computed over multiple-year periods.

In this paper, we estimate event time-varying betas but do not use the restrictive assumptions of the Sharpe–Lintner CAPM in computing abnormal returns for winners and losers. The Sharpe–Lintner model assumes that the compensation per unit of beta risk is about 14–15% per year when an equally-weighted market portfolio is used. Given that the betas of extreme winners and losers differ by about 0.8 when annual returns are used, an adjustment for beta risk explains a large portion of the overreaction effect. We rely instead on the estimated market compensation per unit of beta risk, which is substantially smaller than that assumed by the Sharpe–Lintner model. We obtain results that are consistent with a substantial overreaction effect. Using annual return intervals, extreme losers outperform extreme winners by 6.5% per year. Using monthly return intervals, this spread increases to 9.5% per year. Furthermore, we show that the overreaction effect is not just a manifestation of the size effect. We demonstrate that the common procedure of adjusting for size underestimates the spread in abnormal returns between winners and losers, because part of the size effect is attributable to return reversals. After adjusting for size, but before adjusting for beta effects, we find that extreme losers outperform extreme winners by 9.7% per year after purging size-control portfolios of winners and losers.

In general, because size, prior returns, and betas are correlated, any study that relates realized returns to just one or two of these variables suffers from an omitted variable bias. In the context of a multiple regression using all three of these variables, we find an economically-significant overreaction effect of about 5% per year. This overreaction effect, however, has a pronounced January seasonal, consistent with the findings of other authors, which raises the question of whether there is an overreaction effect that is distinct from previously-documented tax-loss selling effects. To address this issue, we construct portfolios based upon prior one-year returns, a common measure of tax-loss selling intensity, and measure their performance over the subsequent five years. We find much smaller differences in returns between extreme portfolios than when portfolios are formed based upon five-year returns.

The overreaction effect, however, is not homogeneous across size groups. Instead, it is much stronger for smaller companies than for larger companies, with extreme losers outperforming extreme winners by about 10% per year among small firms. These smaller firms are held predominantly by individuals. In contrast, there is virtually no evidence of overreaction among the largest firms, where institutional investors are the dominant holders. This suggests that overreaction by individuals is more prevalent than overreaction by institutions.

In common with other studies that examine returns over long intervals, there is always the possibility that what we attribute to overreaction is instead equilibrium compensation for some omitted risk factor (or factors). However, we feel that our results cannot be explained by risk mismeasurement since returns consistent with overreaction are observed for the short windows surrounding quarterly earnings announcement days. We find that even after adjusting for the size effect and the higher risk that is present at earnings announcements, losers have significantly higher returns than winners.

If the return reversals documented here and elsewhere are not merely compensation for risk bearing, then why is it that the patterns do not disappear due to the actions of arbitrageurs? Shleifer and Vishny (1991) argue that 'smart money' investors are exposed to opportunity costs if there is no certainty that mispricing will be corrected in a timely manner. The periodic evaluation of money managers by their clients contributes to their unwillingness to undertake long-term arbitrage positions. For these reasons, 'smart money' will flock to short-term rather than long-term arbitrage opportunities, and resources devoted to long-term arbitrage will be quite limited. The trading strategies discussed in this paper require capital commitments over extended horizons in smaller firms, which may be why these opportunities can persist for so long.

In summary, we have documented an economically-important overreaction effect in the stock market, concentrated among smaller firms. While the underlying reasons for the valuation errors have not been uncovered, the fact that the effect is strongest for smaller stocks may indicate that a productive area for future research is understanding the difference in the investment patterns between individuals and institutions.

Table 7

RATS betas on winner and loser portfolios for each event year from −6 to +5 for ranking periods in all markets, down markets ($r_{mt} - r_{ft} < 0$), and up markets ($r_{mt} - r_{ft} > 0$). Years −6 to −5 are the pre-ranking period, years −4 to 0 are the ranking period, and years +1 to +5 are the post-ranking period.[a]

$$r_{pt} - r_{ft} = \alpha_p + \beta_p(r_{mt} - r_{ft}) + \varepsilon_{pt}$$

Beta coefficient estimates

Year relative to ranking year 0	All 52 years		Years when $r_m - r_f < 0$ only		Years when $r_m - r_f > 0$ only	
	Winners	Losers	Winners	Losers	Winners	Losers
−6	1.15	1.19	1.20	1.03	1.01	1.10
−5	1.21	1.12	1.12	1.07	1.17	1.06
−4	1.58	0.78	1.11	0.96	2.02	0.52
−3	1.52	0.83	0.99	0.87	1.86	0.58
−2	1.47	0.95	0.99	0.75	1.78	0.83
−1	1.48	1.02	0.98	0.86	1.72	0.99
0	1.21	1.06	0.94	0.83	1.13	1.03
+1	0.85	1.54	0.94	0.97	0.63	1.73
+2	0.79	1.63	0.93	1.26	0.56	1.89
+3	0.86	1.54	0.80	1.22	0.74	1.71
+4	0.94	1.55	0.72	1.08	0.95	1.78
+5	0.88	1.61	0.77	0.95	0.88	1.88
Average, −6 to −5	1.18	1.15	1.16	1.05	1.09	1.08
Average, −4 to 0	1.45	0.93	1.00	0.85	1.70	0.79
Average, +1 to +5	0.86	1.57	0.83	1.10	0.75	1.80

[a]Winner and loser portfolios consist of the stocks with the most extreme total returns over the five years −4 to 0. The 50 best and the 50 worst stocks in each ranking are assigned to the winner and loser portfolios. In the first two columns, α_p and β_p coefficients are estimated using a time series of 52 annual portfolio returns, using Ibbotson's (1975) RATS methodology. For years −6 and −5, respectively, 50 and 51 annual returns are used because of the lack of CRSP data for 1924 and 1925. There are between 15 and 21 down-market years and 31 to 37 up-market years, for the years −6 to +5. Riskless annual returns are from Ibbotson Associates (1988). The market return is defined to be the equally-weighted market return on NYSE stocks with at least five years of returns.

Appendix

A.1. Asymmetries in beta changes

Ibbotson's (1975) RATS procedure is ideally suited for estimating event time-varying betas in a situation where the sample firms are experiencing dramatic changes in their market capitalization over relatively short intervals. In the context of this study, substantial differences in betas between winners and losers are observed using this procedure.

One of the attractive features of the RATS procedure is that one can observe on a period-by-period basis how the betas are changing within the ranking or post-ranking periods. Ball and Kothari (1989) present evidence, in their tables 4 and 5 and fig. 1, that the betas of winner and loser portfolios change over time in the direction that would be predicted due to leverage changes. We replicate these patterns in columns (1) and (2) of our table 7. In this table, following De Bondt and Thaler (1985) and Ball and Kothari (1989), we have defined winners and losers to be the 50 stocks with the most extreme ranking-period returns. We have calculated betas for each year of a two-year pre-ranking period (years -6 to -5), for the ranking period (years -4 to 0), and for the post-ranking period (years $+1$ to $+5$). The changes in the betas from the pre-ranking period to the ranking period, and from the ranking period to the post-ranking period, are striking. The ranking-period betas appear to suffer from severe biases. Apparently, the timing of the extreme returns on winners (and losers) is correlated with the market excess return. What is particularly noteworthy is that in the pre-ranking period, the firms that subsequently become the extreme winners and losers have betas that are practically indistinguishable from each other.[10] From year -5 to -4, the beta of the winner portfolio jumps from 1.21 to 1.58, whereas the beta of the loser portfolio falls from 1.12 to 0.78. These dramatic shifts are in the opposite direction to the changes predicted by the leverage hypothesis.

The leverage hypothesis predicts that, since year -4 is part of the ranking period, the beta of winners should fall and the beta of losers should rise. (In the ranking period, the winners have an average annual raw return of 55% for five years, while the losers have an average annual raw return of -9% for five years.) Throughout the ranking period, the betas of the winners remain high and the betas of the losers remain low. As soon as the ranking period ends, there is another huge change in betas. Between years 0 and $+1$, the winners' betas decrease by 0.36 and the losers' betas increase by 0.48, a combined swing of 0.84. One would expect a much smaller change, given that the market capitalizations change by a smaller amount between years 0 and $+1$ than between any two adjacent years during the ranking period. In contrast, the swing in betas during the entire five-year ranking period in which the relative market capitalizations changed dramatically is only 0.65 (0.27 for winners and 0.38 for losers).

These abrupt changes in betas cast doubt on the hypothesis that the changes are primarily due to movements in leverage. Thus, a fundamental question is raised about just what phenomenon is being captured by the betas of the winners and losers. The puzzle deepens when the patterns in betas for

[10] The betas of both the subsequent winners and losers are above 1.0 during the pre-ranking period. Small firms tend to have high betas, and firms with a lot of unique risk are overrepresented among both extreme winners and extreme losers. Large firms are generally more diversified, and are thus less likely to become extreme winners or losers.

Table 8

OLS regressions of average annual percentage excess returns for the first five post-ranking years for portfolios of NYSE firms formed on the basis of size and prior returns.

For each of the 52 ranking periods ending on December 31 of 1930 to 1981, firms are independently ranked on the basis of their December 31 market value and their five-year prior return, and assigned to one of 400 portfolios. *SIZE* is measured as the size portfolio ranking (1 to 20, with 1 being smallest), and *RETURN* is measured as the prior-return portfolio ranking (1 to 20, with 1 being the most extreme prior losers). Annual returns and an equally-weighted market index are used in all three panels. *T*-statistics, computed using the autocorrelation-adjusted Fama–MacBeth procedure described in table 4, are in parentheses.

Panel A reports results using betas that are calculated by pooling observations across both firms and post-ranking event years. This is identical to panel A in table 4. Panels B and C report results using the two alternative procedures. In all three panels, *t*-statistics are based upon variation in the coefficients from a 52-observation time series of cross-sectional regressions, adjusted for fourth-order autocorrelation.

In panel B, the procedure is analogous to that used in table 1: for each of the 400 portfolios we run a time-series regression using (up to) 52 portfolio returns in each of the five post-ranking years, and then compute the portfolio beta as the average of these five numbers. A disadvantage of this procedure is that there are many portfolios that have missing observations in some of the 52 years.

In panel C, the procedure calculates separate betas for each of the five post-ranking years and then averages these five numbers to calculate the portfolio beta.

$$r_p - r_f = a_0 + a_1 SIZE_p + a_2 RETURN_p + a_3 Beta_p + e_p$$

	Coefficient estimates			
Intercept	*SIZE*	*RETURN*	*Beta*	R^2_{adjusted}
	Panel A: Betas computed with pooling over both firms and event years			
14.443	−0.364	−0.254	5.438	0.68
(10.517)	(−3.779)	(−2.996)	(1.707)	
	Panel B: Betas computed using the RATS procedure			
15.637	−0.290	−0.204	7.210	0.70
(4.949)	(−2.007)	(−2.259)	(2.062)	
	Panel C: Betas computed with pooling over firms			
17.838	−0.314	−0.266	5.817	0.67
(6.381)	(−2.194)	(−3.022)	(1.646)	

up and down markets are observed. [De Bondt and Thaler (1987) first documented these differences in betas between up and down markets.] During down markets, defined as years for which $r_m - r_f < 0$, the betas of winner and loser portfolios show little variation between the ranking and post-ranking periods. Furthermore, in the post-ranking period the down-market betas differ by only 0.27 (0.83 for winners, 1.10 for losers). In contrast, during up markets, defined as years for which $r_m - r_f > 0$, the betas of winners fall by roughly half from the ranking period to the post-ranking period, while the betas of losers approximately double. Furthermore, during the post-ranking period, the up-market betas of winners and losers differ by a full 1.05 (0.75 for winners, 1.80 for losers). Thus, the large difference in betas between winners and losers in the post-ranking period emphasized by Ball and Kothari is driven primarily by the extraordinarily high betas on losers during up markets. Thus, while the difference in betas during the post-ranking period between portfolios comprised of the 50 most extreme winners and losers is 0.70 (0.79 using extreme vitile portfolios in table 1), we have serious reservations whether the difference in risk that investors face is actually of this magnitude.

What is beta capturing? This is an open issue that requires further study. Work by Bhandari (1988) and Braun, Nelson, and Sunier (1990) finds only a weak association between changes in leverage and equity betas. Stroyny (1991) finds that heteroskedasticity in the returns distribution induces some of the biases, since percentage variances tend to be asymmetric between up and down markets.

A.2. Sensitivities to alternative measures of beta computation

In table 8, we report the results of alternative beta computation procedures for the table 4 regression using annual returns. As can be seen, the qualitative conclusions are not highly dependent on the procedure employed.

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