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Does the Stock Market Overreact?

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As economists interested in both market behavior and the psychology of individual decision making, we have been struck by the similarity of two sets of empirical findings. Both classes of behavior can be characterized as displaying *overreaction*. This study was undertaken to investigate the possibility that these phenomena are related by more than just appearance. We begin by describing briefly the individual and market behavior that piqued our interest.

The term overreaction carries with it an implicit comparison to some degree of reaction that is considered to be appropriate. What is an appropriate reaction? One class of tasks which have a well-established norm are probability revision problems for which Bayes' rule prescribes the correct reaction to new information. It has now been well-established that Bayes' rule is not an apt characterization of how individuals actually respond to new data (Kahneman et al. [1982]). In revising their beliefs, individuals tend to overweight recent information and underweight prior (or base rate) data. People seem to make predictions according to a simple matching rule: "The predicted value is selected so that the standing of the case in the distribution of outcomes matches its standing in the distribution of impressions" (Kahneman and Tversky [1982, p. 416]). This rule-of-thumb, an instance of what Kahneman and Tversky call the representativeness heuristic, violates the basic statistical principle that the extremeness of predictions must be moderated by considerations of predictability. Grether (1980) has replicated this finding under incentive

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compatible conditions. There is also considerable evidence that the actual expectations of professional security analysts and economic forecasters display the same overreaction bias (for a review, see De Bondt [1985]).

One of the earliest observations about overreaction in markets was made by J. M. Keynes: ". . . day-to-day fluctuations in the profits of existing investments, which are obviously of an ephemeral and nonsignificant character, tend to have an altogether excessive, and even an absurd, influence on the market" (1936, pp. 153–154). About the same time, Williams noted in this *Theory of Investment Value* that "prices have been based too much on current earning power and too little on long-term dividend paying power" (1938, p. 19). More recently, Arrow has concluded that the work of Kahneman and Tversky "typifies very precisely the excessive reaction to current information which seems to characterize all the securities and futures markets" (1982, p.5). Two specific examples of the research to which Arrow was referring are the excess volatility of security prices and the so-called price earnings ratio anomaly.

The excess volatility issue has been investigated most thoroughly by Shiller (1981). Shiller interprets the Miller-Modigliani view of stock prices as a constraint on the likelihood function of a price-dividend sample. Shiller concludes that, at least over the last century, dividends simply do not vary enough to rationally justify observed aggregate price movements. Combining the results with Kleidon's (1981) findings that stock price movements are strongly correlated with the following year's earnings changes suggests a clear pattern of overreaction. In spite of the observed trendiness of dividends, investors seem to attach disproportionate importance to short-run economic developments.[1]

The price earnings ratio (*P/E*) anomaly refers to the observation that stocks with extremely low *P/E* ratios (i.e., lowest decile) earn larger risk-adjusted returns than high *P/E* stocks (Basu [1977]). Most financial economists seem to regard the anomaly as a statistical artifact. Explanations are usually based on alleged misspecification of the capital asset pricing model (CAPM). Ball (1978) emphasizes the effects of omitted risk factors. The *P/E* ratio is presumed to be a proxy for some omitted factor which, if included in the "correct" equilibrium valuation model, would eliminate the anomaly. Of course, unless these omitted factors can be identified, the hypothesis is untestable. Reinganum (1981) has claimed that the small firm effect subsumes the *P/E* effect and that both are related to the same

1. Of course, the variability of stock prices may also reflect changes in real interest rates. If so, the price movements of other assets—such as land or housing—should match those of stocks. However, this is not actually observed. A third hypothesis, advocated by Marsh and Merton [1983], is that Shiller's findings are a result of his misspecification of the dividend process.

set of missing (and again unknown) factors. However, Basu (1983) found a significant *P/E* effect after controlling for firm size, and earlier Graham (1973) even found an effect within the thirty Dow Jones Industrials, hardly a group of small firms!

An alternative behavioral explanation for the anomaly based on investor overreaction is what Basu called the "price-ratio" hypothesis (e.g., Dreman [1982]). Companies with very low *P/E*'s are thought to be temporarily "undervalued" because investors become excessively pessimistic after a series of bad earnings reports or other bad news. Once future earnings turn out to be better than the unreasonably gloomy forecasts, the price adjusts. Similarly, the equity of companies with very high *P/E*'s is thought to be "overvalued," before (predictably) falling in price.

While the overreaction hypothesis has considerable a priori appeal, the obvious question to ask is: How does the anomaly survive the process of arbitrage? There is really a more general question here. What are the equilibria conditions for markets in which some agents are not rational in the sense that they fail to revise their expectations according to Bayes' rule? Russell and Thaler (1985) address this issue. They conclude that the existence of some rational agents is not sufficient to guarantee a rational expectations equilibrium in an economy with some of what they call quasi-rational agents. (The related question of market equilibria with agents having heterogeneous expectations is investigated by Jarrow [1983].) While we are highly sensitive to these issues, we do not have the space to address them here. Instead, we will concentrate on an empirical test of the overreaction hypothesis.

If stock prices systematically overshoot, then their reversal should be predictable from past return data alone, with no use of any accounting data such as earnings. Specifically, two hypotheses are suggested: (1) Extreme movements in stock prices will be followed by subsequent price movements in the opposite direction. (2) The more extreme the initial price movement, the greater will be the subsequent adjustment. Both hypotheses imply a violation of weak-form market efficiency.

To repeat, our goal is to test whether the overreaction hypothesis is *predictive*. In other words, whether it does more for us than merely to explain, ex post, the *P/E* effect or Shiller's results on asset price dispersion. The overreaction effect deserves attention because it represents a behavioral principle that may apply in many other contexts. For example, investor overreaction possibly explains Shiller's earlier (1979) findings that when long-term interest rates are high relative to short rates, they tend to move down later on. Ohlson and Penman (1983) have further suggested that the increased volatility of security returns following stock splits may also be linked to overreaction. The present empirical tests are to our knowledge the first attempt to use a behavioral principle to predict a new market anomaly.

The remainder of the paper is organized as follows. The next section describes the actual empirical tests we have performed. Section II describes the results. Consistent with the overreaction hypothesis, evidence of weak-form market inefficiency is found. We discuss the implications for other empirical work on asset pricing anomalies. The paper ends with a brief summary of conclusions.

1. The Overreaction Hypothesis: Empirical Tests

The empirical testing procedures are a variant on a design originally proposed by Beaver and Landsman (1981) in a different context. Typically, tests of semistrong form market efficiency start, at time $t = 0$, with the formation of portfolios on the basis of some event that affects all stocks in the portfolio, say, an earnings announcement. One then goes on to investigate whether later on ($t > 0$) the estimated residual portfolio return $\hat{u}_{pt}$—measured relative to the single-period CAPM—equals zero. Statistically significant departures from zero are interpreted as evidence consistent with semistrong form market inefficiency, even though the results may also be due to misspecification of the CAPM, misestimation of the relevant alphas and/or betas, or simply market inefficiency of the weak form.

In contrast, the tests in this study assess the extent to which systematic nonzero residual return behavior in the period after portfolio formation ($t > 0$) is associated with systematic residual returns in the preformation months ($t < 0$). We will focus on stocks that have experienced either extreme capital gains or extreme losses over periods up to five years. In other words, "winner" (W) and "loser" portfolios (L) are formed *conditional upon past excess returns*, rather than some firm-generated informational variable such as earnings.

Following Fama (1976), the previous arguments can be formalized by writing the efficient market's condition,

$$E(\tilde{R}_{jt} - E_m(\tilde{R}_{jt}|F^m_{t-1})|F_{t-1}) = E(\tilde{u}_{jt}|F_{t-1}) = 0$$

where F_{t-1} represents the complete set of information at time $t - 1$, $\tilde{R}_{jt}$ is the return on security t at t, and $E_m(\tilde{R}_{jt}|F^m_{t-1})$ is the expectation of $\tilde{R}_{jt}$, assessed by the market on the basis of the information set F^m_{t-1}. The efficient market hypothesis implies that $E(\tilde{u}_{Wt}|F_{t-1}) = E(\tilde{u}_{Lt}|F_{t-1}) = 0$. As explained in the introduction, the overreaction hypothesis, on the other hand, suggests that $E(\tilde{u}_{Wt}|F_{t-1}) < 0$ and $E(\tilde{u}_{Lt}|F_{t-1}) > 0$.

In order to estimate the relevant residuals, an equilibrium model must be specified. A common procedure is to estimate the parameters of the market model (see e.g., Beaver and Landsman [5]). What will happen if the equilibrium model is misspecified? As long as the variation in $E_m(\tilde{R}_{jt}|F^m_{t-1})$ is small relative to the movements in $\tilde{u}_{jt}$, the exact specification of the equilibrium model makes little difference to tests of the efficient market hypothesis. For, even if we knew the "correct" model of $E_m(\tilde{R}_{jt}|F^m_{t-1})$, it would explain only a small part of the variation in $\tilde{R}_{jt}$.[2]

Since this study investigates the return behavior of specific portfolios over extended periods of time (indeed, as long as a decade), it cannot be merely *assumed* that model misspecification leaves the conclusions about market efficiency unchanged. Therefore, the empirical analysis is based on three types of return residuals: market-adjusted excess returns; market model residuals; and excess returns that are measured relative to the Sharpe-Lintner version of the CAPM. However, since all three methods are single-index models that follow from the CAPM, misspecification problems may still confound the results. De Bondt (1985) formally derives the econometric biases in the estimated market-adjusted and market model residuals if the "true" model is multifactor, e.g., $\tilde{R}_{jt} = A_j + B_j\tilde{R}_{mt} + C_j\tilde{X}_t + \tilde{e}_{jt}$. As a final precaution, he also characterizes the securities in the extreme portfolios in terms of a number of financial variables. If there were a persistent tendency for the portfolios to differ on dimensions that may proxy for "risk," then, again, we cannot be sure whether the empirical results support market efficiency or market overreaction.

It turns out that, whichever of the three types of residuals are used, the results of the empirical analysis are similar and that the choice does not affect our main conclusions. Therefore, we will only report the results based on market-adjusted excess returns. The residuals are estimated as $\hat{u}_{jt} = R_{jt} - R_{mt}$. There is no risk adjustment except for movements of the market as a whole and the adjustment is identical for all stocks. Since, for any period t, the same (constant) market return R_{mt} is subtracted from all R_{jt}'s, the results are interpretable in terms of raw (dollar) returns. As shown in De Bondt (1985), the use of market-adjusted excess returns has the further advantage that it is likely to bias the research design *against* the overreaction hypothesis.[3] Finally, De Bondt shows that winner and

2. Presumably, this same reasoning underlies the common practice of measuring abnormal security price performance by way of easily calculable mean-adjusted excess returns [where, by assumption, $E(\tilde{R}_j)$ equals a constant K_j], market-adjusted excess returns (where, by assumption, $\alpha_j = 0$ and $\beta_j = 1$ for all j), rather than more complicated market model residuals, let alone residuals relative to some multifactor model.

3. We will come back to this bias in Section II.

loser portfolios, formed on the basis of market-adjusted excess returns, do not systematically differ with respect to either market value of equity, dividend yield or financial leverage.

We will now describe the basic research design used to form the winner and loser portfolios and the statistical test procedures that deter- winner and loser portfolios and the statistical test procedures that deter-
mine which of the two competing hypotheses receives more support from the data.

1.1 Test Procedures: Details

Monthly return data for New York Stock Exchange (NYSE) common stocks, as compiled by the Center for Research in Security Prices (CRSP) of the University of Chicago, are used for the period between January 1926 and December 1982. An equally weighted arithmetic average rate of return on all CRSP listed securities serves as the market index.

1. For every stock j on the tape with at least 85 months of return data (months 1 through 85), without any missing values in between, and starting in January 1930 (month 49), the next 72 monthly residual returns u_{jt} (months 49 through 120) are estimated. If some or all of the raw return data beyond month 85 are missing, the residual returns are calculated up to that point. The procedure is repeated 16 times starting in January 1930, January 1933, . . . , up to January 1975. As time goes on and new securities appear on the tape, more and more stocks qualify for this step.
2. For every stock j, starting in December 1932 (month 84; the "port- folio formation date") ($t = 0$), we compute the cumulative excess returns $CU_j = \Sigma_{t=-35}^{t=0} u_{jt}$ for the prior 36 months (the "portfolio formation" period, months 49 through 84). The step is repeated 16 times for all nonoverlapping three-year periods between January 1930 and December 1977. On each of the 16 relevant portfolio formation dates (December 1932, December 1935, . . . , December 1977), the CU_j's are ranked from low to high and portfolios are formed. Firms in the top 35 stocks (or the top 50 stocks, or the top decile) are assigned to the winner portfolio W; firms in the bottom 35 stocks (or the bottom 50 stocks, or the bottom decile) to the loser portolio L. Thus, the portfolios are formed conditional upon excess return behavior prior to $t = 0$, the portfolio formation date.
3. For both portfolios in each of 16 nonoverlapping three-year periods ($n = 1, \ldots, N$; $N = 16$), starting in January 1933 (month 85, the "starting month") and up to December 1980, we now compute the cumulative average residual returns of all securities in the portfolio, for the next 36 months (the "test period," months 85 through 120), i.e., from $t = 1$ through $t = 36$. We find $CAR_{W,n,t}$ and $CAR_{L,n,t}$. If a security's return is missing in a month subsequent to portfolio formation, then, from that moment on, the stock is permanently dropped from the portfolio and the CAR is an average of the available residual returns. Thus, whenever a stock drops out, the calculations involve an implicit rebalancing.[4]
4. Using the CAR's from all 16 test periods, *average* CAR's are calculated for both portfolios and each month between $t = 1$ and $t = 36$. They are denoted $ACAR_{W,t}$ and $ACAR_{L,t}$. The overreaction hypothesis predicts that, for $t > 0$, $ACAR_{W,t} < 0$ and $ACAR_{L,t} > 0$, so that, by implication, $[ACAR_{L,t} - ACAR_{W,t}] > 0$. In order to assess whether, at any time t, there is indeed a statistically significant difference in investment performance, we need a pooled estimate of the population variance in CAR_t,

$$S_t^2 = \left[\sum_{n=1}^{N}(CAR_{W,n,t} - ACAR_{W,t})^2 + \sum_{n=1}^{N}(CAR_{L,n,t} - ACAR_{L,t})^2\right] / 2(N-1).$$

With two samples of equal size N, the variance of the difference of sample means equals $2S_t^2/N$ and the t-statistic is therefore

$$T_t = [ACAR_{L,t} - ACAR_{W,t}] / \sqrt{2S_t^2/N}.$$

Relevant t-statistics can be found for each of the 36 postformation months but they do not represent independent evidence.

5. In order to judge whether, for any month t, the average residual return makes a contribution to either $ACAR_{W,t}$ or $ACAR_{L,t}$, we

4. Since this study concentrates on companies that experience extraordinary returns, either positive or negative, there may be some concern that their attrition rate sufficiently deviates from the "normal" rate so as to cause a survivorship bias. However, this concern is unjustified. When a security is delisted, suspended or halted, CRSP determines whether or not it is possible to trade at the last listed price. If no trade is possible, CRSP tries to find a subsequent quote and uses it to compute a return for the last period. If no such quote is available because the stockholders receive nothing for their shares, the return is entered as minus one. If trading continues, the last return ends with the last listed price.

can test whether it is significantly different from zero. The sample standard deviation of the winner portfolio is equal to

$$s_t = \sqrt{\sum_{n=1}^{N} (AR_{W,n,t} - AR_{W,t})^2 / N - 1}.$$

Since $s_t/\sqrt{N}$ represents the sample estimate of the standard error of $AR_{W,t}$, the t-statistic equals

$$T_t = AR_{W,t}/(s_t/\sqrt{N}).$$

Similar procedures apply for the residuals of the loser portfolio.

1.2 Discussion

Several aspects of the research design deserve some further comment. The choice of the data base, the CRSP Monthly Return File, is in part justified by our concern to avoid certain measurement problems that have received much attention in the literature. Most of the problems arise with the use of daily data, both with respect to the risk and return variables. They include, among others, the "bid-ask" effect and the consequences of infrequent trading.

The requirement that 85 subsequent returns are available before any firm is allowed in the sample biases the selection towards large, established firms. But, if the effect under study can be shown to apply to them, the results are, if anything, more interesting. In particular, it counters the predictable critique that the overreaction effect may be mostly a small-firm phenomenon. For the experiment described in Section A, between 347 and 1,089 NYSE stocks participate in the various replications.

The decision to study the CAR's for a period of 36 months after the portfolio formation date reflects a compromise between statistical and economic considerations, namely, an adequate number of independent replications versus a time period long enough to study issues relevant to asset pricing theory. In addition, the three-year period is also of interest in light of Benjamin Graham's contention that "the interval required for a substantial underevaluation to correct itself averages approximately 1½ to 2½ years" (1959, p. 37). However, for selected experiments, the portfolio formation (and testing) periods are one, two, and five years long. Clearly, the number of independent replications varies inversely with the length of the formation period.

Finally, the choice of December as the "portfolio formation month" (and, therefore, of January as the "starting month") is essentially arbitrary. In order to check whether the choice affects the results, some of the empirical tests use May as the portfolio formation month.

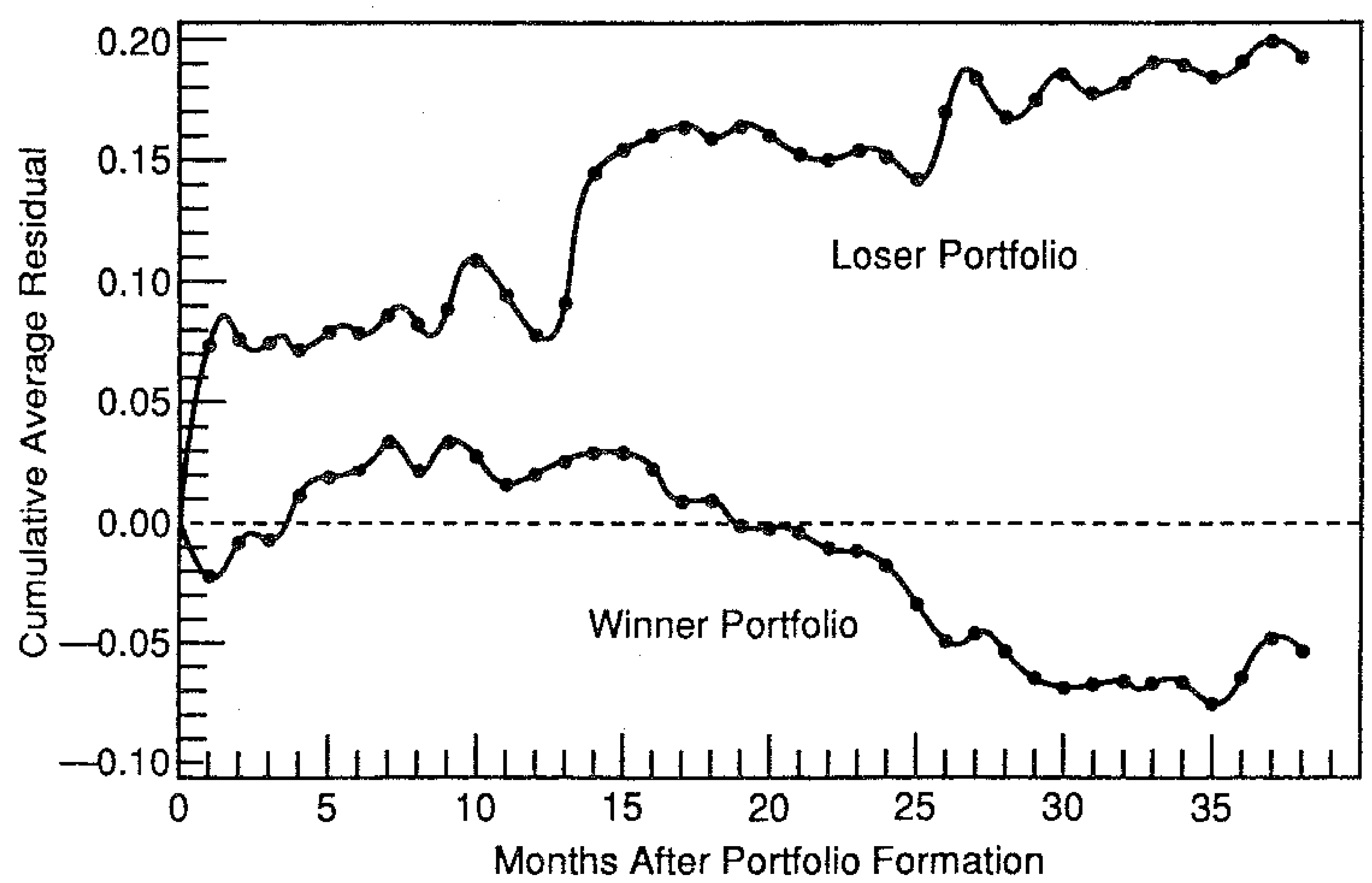
2. The Overreaction Hypothesis: Empirical Results

2.1 Main Findings

The results of the tests developed in Section I are found in Figure 1. They are consistent with the overreaction hypothesis. Over the last half-century, loser portfolios of 35 strocks outperform the market by, on average, 19.6%, thirty-six months after portfolio formation. Winner portfolios, on the other hand, earn about 5.0% less than the market, so that the difference in cumulative average residual between the extreme portfolios, $[ACAR_{L,36} - ACAR_{W,36}]$ equals 24.6% (t-statistic: 2.20). Figure 1 shows the movement of the ACAR's as we progress through the test period.

The findings have other notable aspects. First, the overreaction effect is asymmetric; it is much larger for losers than for winners. Secondly, consistent with previous work on the turn-of-the-year effect and seasonality, most of the excess returns are realized in January. In months $t = 1$, $t = 13$, and $t = 25$, the loser portfolio earns excess returns of, respectively, 8.1% (t-statistic: 3.21), 5.6% (3.07), and 4.0% (2.76). Finally, in surprising agreement with Benjamin Graham's claim, the overreaction

Figure 1 Cumulative Average Residuals for Winner and Loser Portfolios of 35 Stocks (1–36 months into the test period)



phenomenon mostly occurs during the second and third year of the test period. Twelve months into the test period, the difference in performance between the extreme portfolios is a mere 5.4% (t-statistic: 0.77).

While not reported here, the results using market model and Sharpe-Lintner residuals are similar. They are also insensitive to the choice of December as the month of portfolio formation (see De Bondt [1985]).

The overreaction hypothesis predicts that, as we focus on stocks that go through more (or less) extreme return experiences (during the formation period), the subsequent price reversals will be more (or less) pronounced. An easy way to generate more (less) extreme observation is to lengthen (shorten) the portfolio formation period; alternatively, for any given formation period (say, two years), we may compare the test period performance of less versus more extreme portfolios, e.g., decile portfolios (which contain an average 82 stocks) versus portfolios of 35 stocks. Table 1 confirms the prediction of the overreaction hypothesis. As the cumulative average residuals (during the formation period) for various sets of winner and loser portfolios grow larger, so do the subsequent price reversals, measured by $[ACAR_{L,t} - ACAR_{W,t}]$ and the accompanying t-statistics. For a formation period as short as one year, no reversal is observed at all.

Table 1 and Figure 2 further indicate that the overreaction phenomenon is qualitatively different from the January effect and, more generally, from seasonality in stock prices. Throughout the test period, the difference in ACAR for the experiment with a three-year formation period (the upper curve) exceeds the same statistic for the experiments based on two- and one-year formation periods (middle and lower curves). But all three experiments are clearly affected by the same underlying seasonal pattern.

In Section I, it was mentioned that the use of market-adjusted excess returns is likely to bias the research design against the overreaction hypothesis. The bias can be seen by comparing the CAPM-betas of the extreme portfolios. For all the experiments listed in Table I, the average betas of the securities in the winner portfolios are significantly larger than the betas of the loser portfolios.[5] For example, for the three-year experiment illustrated in Figure 1, the relevant numbers are respectively, 1.369 and 1.026 (t-statistic on the difference: 3.09). Thus, the loser portfolios not only outperform the winner portfolios; if the CAPM is correct, they are also significantly less risky. From a different viewpoint, therefore, the results in Table I are likely to *underestimate* both the

5. The CAPM-betas are found by estimating the market model over a period of 60 months prior to portfolio formation.

Table 1 Differences in Cumulative Average (Market-Adjusted) Residual Returns Between the Winner and Loser Portfolios at the End of the Formation Period, and 1, 12, 13, 18, 24, 25, 36, and 60 Months into the Test Period

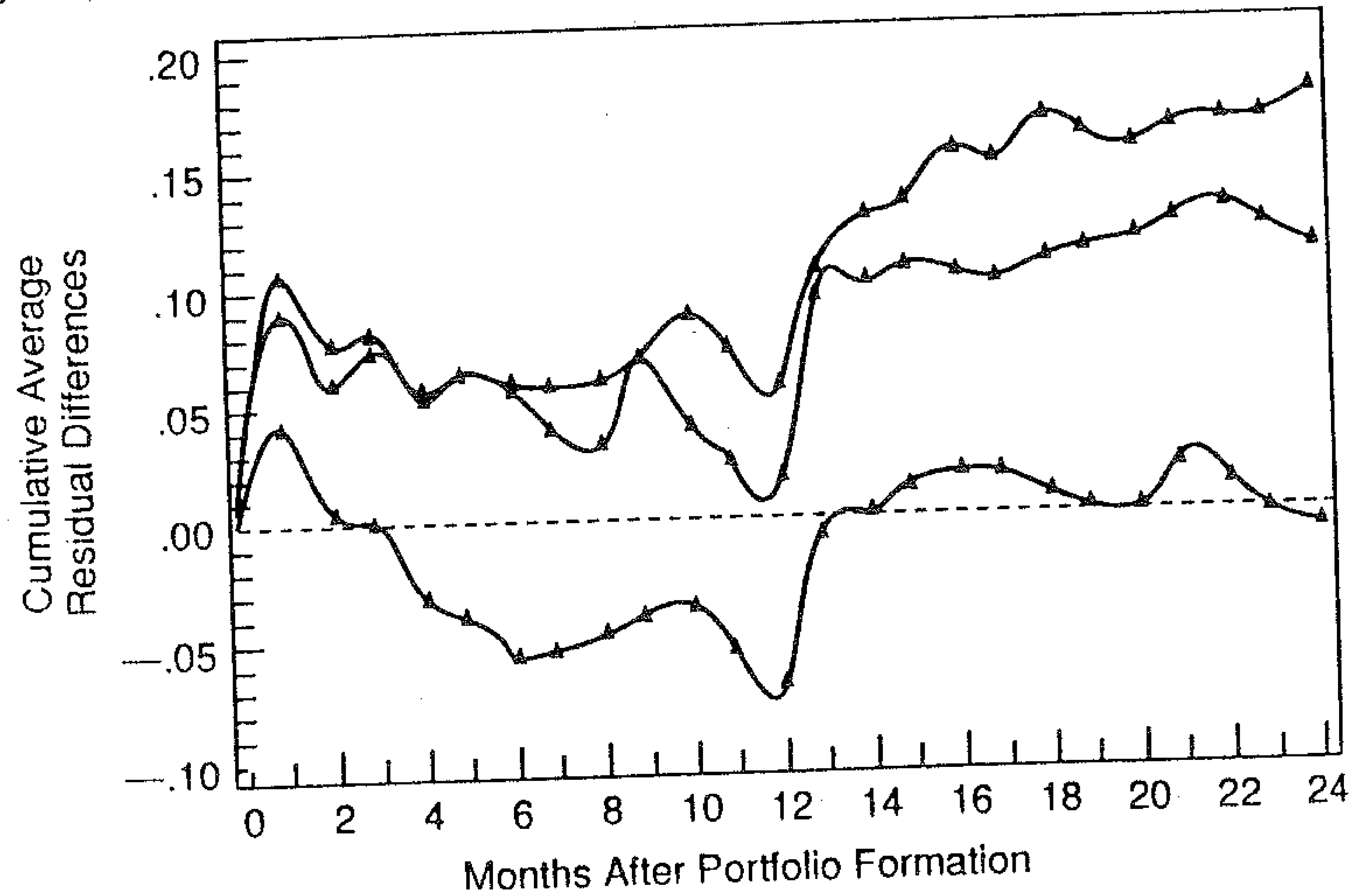
Portfolio Selection Procedures: Length of the Formation Period and No. of Independent Replications	Average No. of Stocks	CAR at the End of the Formation Period		Difference in CAR (t-Statistics)							
				Months After Portfolio Formation							
		Winner Portfolio	Loser Portfolio	1	12	13	18	24	25	36	60
10 five-year periods	50	1.463	−1.194	0.070 (3.13)	0.156 (2.04)	0.248 (3.14)	0.256 (3.17)	0.196 (2.15)	0.228 (2.40)	0.230 (2.07)	0.319 (3.28)
16 three-year periods	35	1.375	−1.064	0.105 (3.29)	0.054 (0.77)	0.103 (1.18)	0.167 (1.51)	0.181 (1.71)	0.234 (2.19)	0.246 (2.20)	NA*
24 two-year periods[a]	35	1.130	−0.857	0.062 (2.91)	−0.006 (−0.16)	0.074 (1.53)	0.136 (2.02)	0.101 (1.41)	NA	NA	NA
25 two-year periods[b]	35	1.119	−0.866	0.089 (3.98)	0.011 (0.19)	0.092 (1.48)	0.107 (1.47)	0.115 (1.55)	NA	NA	NA
24 two-year periods[a] (deciles)	82	0.875	−0.711	0.051 (3.13)	0.006 (0.19)	0.066 (1.71)	0.105 (1.99)	0.083 (1.49)	NA	NA	NA
25 two-year periods[b] (deciles)	82	0.868	−0.714	0.068 (3.86)	0.008 (0.19)	0.071 (1.46)	0.078 (1.41)	0.072 (1.29)	NA	NA	NA
49 one-year periods	35	0.774	−0.585	0.042 (2.45)	−0.076 (−2.32)	−0.006 (−0.15)	0.007 (0.14)	−0.005 (−0.09)	NA	NA	NA

a. The formation month for these portfolios is the month of December in all uneven years between 1933 and 1979.

b. The formation month for these portfolios is the month of December in all even years between 1932 and 1980.

c. NA, not applicable.

Figure 2 Differences in Cumulative Average Residual Between Winner and Loser Portfolios of 35 Stocks (formed over the previous one, two, or three years; 1–24 months into the test period)

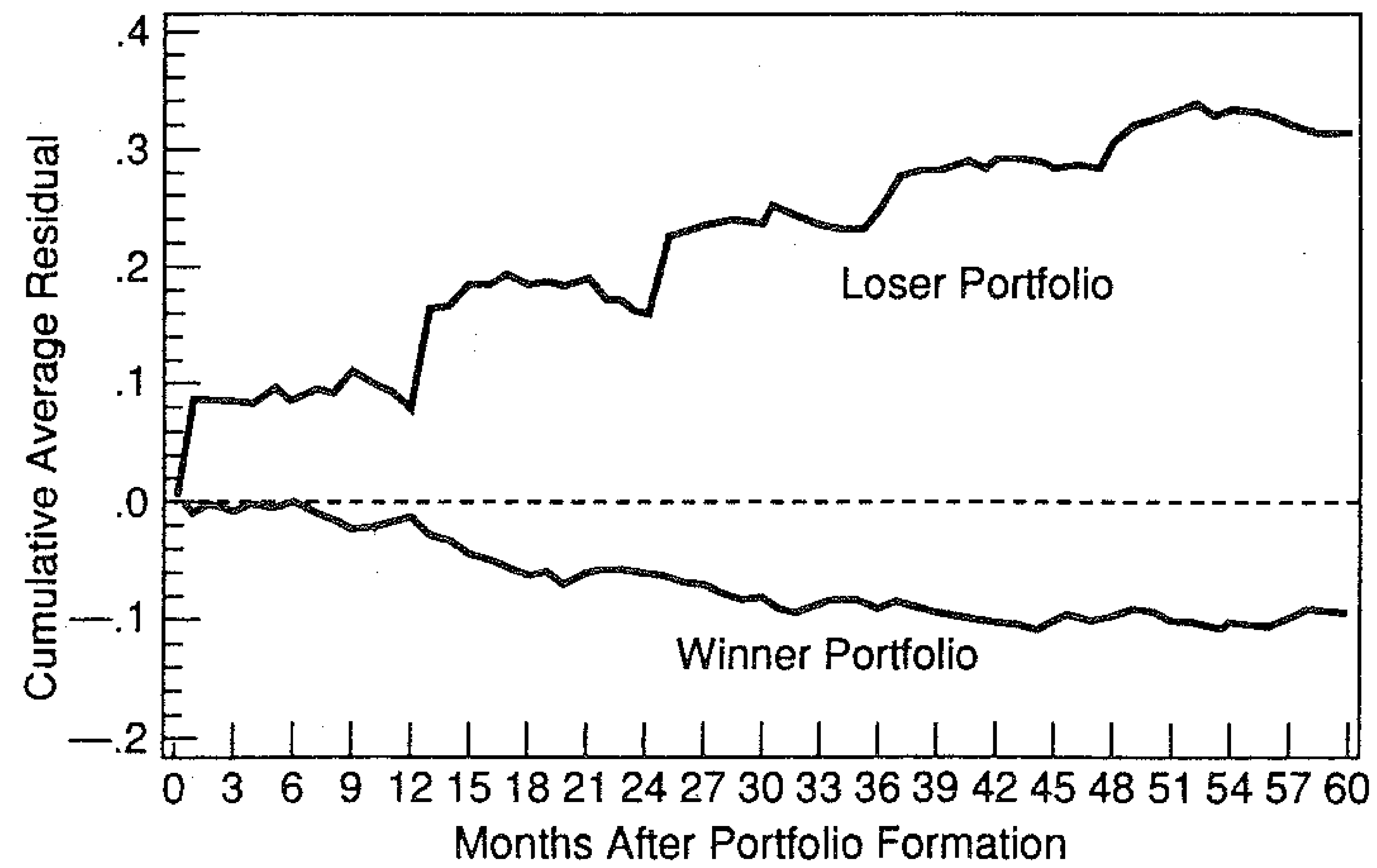


true magnitude and statistical significance of the overreaction effect. The problem is particularly severe with respect to the winner portfolio. Rather than 1.369, the residual return calculations assume the CAPM-beta of that portfolio to equal 1.00 only. This systematic bias may be responsible for the earlier observed asymmetry in the return behavior of the extreme portfolios.

To reiterate, the previous findings are broadly consistent with the predictions of the overreaction hypothesis. However, several aspects of the results remain without adequate explanation. Most importantly, the extraordinarily large positive excess returns earned by the loser portfolio in January.

One method that allows us to further accentuate the strength of the January effect is to increase the number of replications. Figure 3 shows the ACAR's for an experiment with a five-year-long test period. Every December between 1932 and 1977, winner and loser portfolios are formed on the basis of residual return behavior over the previous five years. Clearly, the successive 46 yearly selections are not independent. Therefore, no statistical tests are performed. The results in Figure 3 have some of the properties of a "trading rule." They represent the average

Figure 3 Cumulative Average Residuals for Winner and Loser Portfolios of 35 Stocks (1–60 months into the test period)



(cumulative) excess return (before transaction costs) that an investor, aware of the overreaction phenomenon, could expect to earn following any December in which he chose to try the strategy. The effect of multiplying the number of replications is to remove part of the random noise.

The outstanding feature of Figure 3 is, once again, the January returns on the loser portfolio. The effect is observed as late as five Januaries after portfolio formation! Careful examination of Figure 3 also reveals a tendency, on the part of the loser portfolio, to decline in value (relative to the market) between October and December. This observation is in agreement with the naive version of the tax-loss selling hypothesis as explained by, e.g., Schwert (1983). The winner portfolio, on the other hand, gains value at the end of the year and loses some in January (for more details, see De Bondt [1985]).

2.2 *Implications for Other Empirical Work*

The results of this study have interesting implications for previous work on the small firm effect, the January effect and the dividend yield and *P/E* effects. Blume and Stambaugh (1983), Keim (1982), and Reinganum (1981) have studied the interaction between the small firm and January effects. Their findings largely redefine the small firm effect as a "losing firm" effect around the turn-of-the-year.[6] Our own results lend further credence to this view. Persistently, losers earn exceptionally large January returns while winners do not. However, the companies in the extreme portfolios do not systematically differ with respect to market capitalization.

The January phenomenon is usually explained by tax-loss selling (see, e.g., Roll [1983]). Our own findings raise new questions with respect to this hypothesis. First, if in early January selling pressure disappears and prices "rebound" to equilibrium levels, why does the loser portfolio—even while it outperforms the market—"rebound" once again in the second January of the test period? And again, in the third and fourth Januaries? Secondly, if prices "rebound" in January, why is that effect so much larger in magnitude than the selling pressure that "caused" it during the final months of the previous year? Possible answers to these questions include the argument that investors may wait for years before realizing losses, and the observed seasonality of the market as a whole.

With respect to the *P/E* effect, our results support the price-ratio hypothesis discussed in the introduction, i.e., high *P/E* stocks are "overvalued" whereas low *P/E* stocks are "undervalued." However, this argument implies that the *P/E* effect is also, for the most part, a January phenomenon. At present, there is no evidence to support that claim, except for the persistent positive relationship between dividend yield (a variable that is correlated with the *P/E* ratio) and January excess returns (Keim [1982]).

3. Conclusions

Research in experimental psychology has suggested that, in violation of Bayes' rule, most people "overreact" to unexpected and dramatic news events. The question then arises whether such behavior matters at the market level.

Consistent with the predictions of the overreaction hypothesis, portfolios of prior "losers" are found to outperform prior "winners." Thirty-six months after portfolio formation, the losing stocks have earned about 25% more than the winners, even though the latter are significantly more risky.

6. Even after purging the data of tax-loss selling effects, Reinganum (1983) finds a (considerably smaller) January seasonal effect related to company size. This result may be due to his particular definition of the tax-loss selling measure. The measure is related to the securities' relative price movements over the last *six months* prior to portfolio formation only. Thus, if many investors choose to wait longer than six months before realizing losses, the portfolio of small firms may still contain many "losers."

Several aspects of the results remain without adequate explanation; most importantly, the large positive excess returns earned by the loser portfolio every January. Much to our surprise, the effect is observed as late as five years after portfolio information.

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10

Measuring Abnormal Performance

Do Stocks Overreact?

NAVIN CHOPRA, JOSEF LAKONISHOK, and JAY R. RITTER

1. Introduction

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

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).

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17

The Hubris Hypothesis of Corporate Takeovers

RICHARD ROLL

> Finally, knowledge of the source of takeover gains still eludes us. [Jensen and Ruback 1983, p. 47]

I. Introduction

Despite many excellent research papers, we still do not fully understand the motives behind mergers and tender offers or whether they bring an increase in aggregate market value. In their comprehensive review article (from which the above quote is taken), Jensen and Ruback (1983) summarize the empirical work presented in over 40 papers. There are many important details in these papers, but Jensen and Ruback interpret them to show overall "that corporate takeovers generate positive gains, that target firm shareholders benefit, and that bidding firm shareholders do not lose" (p. 47).

My purpose here is to suggest a different and less conclusive interpretation of the empirical results. This interpretation may not turn out to be valid, but I hope to show that it has enough plausibility to be at least considered in further investigations. It will be argued here that takeover gains may have been overestimated if they exist at all. If there really are no aggregate gains associated with takeovers, or if they are small, it is not hard to understand why their sources are "elusive."

The mechanism by which takeover attempts are initiated and consummated suggests that at least part of the large price increases observed in

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target firm shares might represent a simple transfer from the bidding firm, that is, that the observed takeover premium (tender offer or merger price less preannouncement market price of the target firm) overstates the increase in economic value of the corporate combination. To see why this could be the case, let us follow the steps undertaken in a takeover.

> First, the bidding firm identifies a potential target firm.
>
> Second, a "valuation" of the equity of the target is undertaken. In some cases this may include nonpublic information. The valuation definitely would include, of course, any estimated economies due to synergy and any assessments of weak management et cetera that might have caused a discount in the target's current market price.
>
> Third, the "value" is compared to the current market price. If value is below price, the bid is abandoned. If value exceeds price, a bid is made and becomes part of the public record. The bid would not generally be the previously determined "value" since it should include provision for rival bids, for future bargaining with the target, and for valuation errors inter alia.

The key element in this series of events is the valuation of an asset (the stock) that already has an observable market price. The preexistence of an active market in the identical item being valued distinguishes takeover attempts from other types of bids, such as for oil-drilling rights and paintings. These other assets trade infrequently and no two of them are identical. This means that the seller must make his own independent valuation. There is a symmetry between the bidder and the seller in the necessity for valuation.

In takeover attempts, the target firm shareholder may still conduct a valuation, but it has a lower bound, the current market price. The bidder knows for certain that the shareholder will not sell below that; thus when the valuation turns out to be below the market price, no offer is made.

Consider what might happen if there are no potential synergies or other sources of takeover gains but when, nevertheless, some bidding firms believe that such gains exist. The valuation itself can then be considered a random variable whose mean is the target firm's current market price. When the random variable exceeds its mean, an offer is made; otherwise there is no offer. Offers are observed only when the valuation is too high; outcomes in the left tail of the distribution of valuations are never observed. The takeover premium in such a case is simply a random error, a mistake made by the bidding firm. Most important, the observed error is always in the same direction. Corresponding errors in the opposite direction are made in the valuation process, but they do not enter our empirical samples because they are not made public.

If there were no value at all in takeovers, why would firms make bids in the first place? They should realize that any bid above the market price represents an error. This latter logic is alluring because market prices do seem to reflect rational behavior. But we must keep in mind that prices are averages. There is no evidence to indicate that every individual behaves as if he were the rational economic human being whose behavior seems revealed by the behavior or market prices. We may argue that markets behave as if they were populated by rational beings. But a market actually populated by rational beings is observationally equivalent to a market characterized by grossly irrational individual behavior that cancels out in the aggregate, leaving the trace of the only systematic behavioral component, the small thread of rationality that all individuals have in common. Indeed, one possible definition of irrational or aberrant behavior is independence across individuals (and thus disappearance from view under aggregation).

Psychologists are constantly bombarding economists with empirical evidence that individuals do not always make rational decisions under uncertainty. For example, see Oskamp (1965), Tversky and Kahneman (1981), and Kahneman, Slovic, and Tversky (1982). Among psychologists, economists have a reputation for arrogance mainly because this evidence is ignored; but psychologists seem not to appreciate that economists disregard the evidence on individual decision making because it usually has little predictive content for market behavior. Corporate takeovers are, I believe, one area of research in which this usually valid reaction of economists should be abandoned; takeovers reflect individual decisions.

There is little reason to expect that a particular individual bidder will refrain from bidding because he has learned from his own past errors. Although some firms engage in many acquisitions, the average individual bidder/manager has the opportunity to make only a few takeover offers during his career. He may convince himself that the valuation is right and that the market does not reflect the full economic value of the combined firm. For this reason, the hypothesis being offered in this paper to explain the takeover phenomenon can be termed the "hubris hypothesis." If there actually are no aggregate gains in takeover, the phenomenon depends on the overbearing presumption of bidders that their valuations are correct.

Even if gains do exist for some corporate combinations, at least part of the average observed takeover premium could still be caused by valuation error and hubris. The left tail of the distribution of valuations is truncated by the current market price. To the extent that there are errors in valuation, fewer negative errors will be observed than positive errors. When gains exist, a smaller fraction of the distribution will be truncated than when there are no gains at all. Nonetheless, truncation will occur in every situation in which the gain is small enough to allow the distribution of valuations to have positive probability below the market price.

Rational bidders will realize that valuations are subject to error and that negative errors are truncated in repeated bids. They will take this into account when making a bid. Takeover attempts are thus analogous to the auctions discussed in bidding theory wherein the competing bidders make public offers. In the takeover situation, the initial bidder is the market, and the initial public offer is the current price. The second bidder is the acquiring firm who, conscious of the "winner's curse," biases his bid downward from his estimate of value. In fact, he frequently abandons the auction altogether, allowing the first bidder to win.

In a standard auction, we would observe all cases, including those in which the initial bid was victorious. Theory predicts that the winning bid is an accurate assessment of value. In takeovers, however, if the initial bid (by the market) wins the auction, we throw away the observation. If all bidders accounted properly for the "winner's curse," there would be no particular bias associated with discarding bids won by the market; but if bidders are infected by hubris, the standard bidding theory conclusion would not be valid. Empirical evidence from repeated sealed bid auctions (Capen, Clapp, and Campbell 1971; and Dougherty and Lohrenz 1976), indicates that bidders do not fully incorporate the winner's curse. Unless there is something curative about the public nature of corporate takeover auctions, we should at least consider the possibility that the same phenomenon exists in them.

The hubris hypothesis is consistent with strong-form market efficiency. Financial markets are assumed to be efficient in that asset prices reflect all information about individual firms. Product and labor markets are assumed efficient in the sense that (*a*) no industrial reorganization can bring gains in an aggregate output at the same cost or reductions in aggregate costs with the same output and (*b*) management talent is employed in its best alternative use.

Most other explanations of the takeover phenomenon rely on strong-form market inefficiency of at least a temporary duration. Either financial markets are ignorant of relevant information possessed by bidding firms, or product markets are inefficiently organized so that potential synergies, monopolies, or tax savings are being ineffectively exploited (at least temporarily), or labor markets are inefficient because gains could be obtained

by replacement of inferior managers. Although perfect strong-form efficiency is unlikely, the concept should serve as a frictionless ideal, the benchmark of comparison by which other degrees of efficiency are measured. This is, I claim, the proper role for the hubris hypothesis of takeovers; it is the null against which other hypotheses of corporate takeovers should be compared.

Section 2 presents the principal empirical predictions of the hubris hypothesis and discusses supportive and disconfirming empirical results. Section 3 concludes the paper by summarizing the results and by discussing various objections to the hypothesis.

2. Evidence For and Against the Hubris Hypothesis

If there are absolutely no gains available to corporate takeovers, the hubris hypothesis implies that the average increase in the target firm's market value should then be more than offset by the average decrease in the value of the bidding firm. Takeover expenses would constitute the aggregate net loss. The market price of a target firm should increase when a previously unanticipated bid is announced, and it should decline to the original level or below if the first bid is unsuccessful and if no further bids are received.

Implications for the market price reaction of a bidding firm are somewhat less clear. If we could be sure that (*a*) the bid was unanticipated and (*b*) the bid conveys no information about the bidder other than that it is seeking a combination with a particular target, then the hubris hypothesis would predict the following market price movements in bidding firms:

1. a price decline on announcement of a bid;
2. a price increase on abandoning a bid or on losing a bid; and
3. a price decline on actually winning a bid.

It has been pointed out by several authors, most forcefully by Schipper and Thompson (1983), that condition *a* above is by no means assured in all cases. Bids are not always surprises. As Jensen and Ruback (1983, pp. 18–20) observe, this alone complicates the measurement of bidder firm returns.

The possibility that a bid conveys information about the bidding firm's own operations, that is, violation of condition *b*, is an equally serious problem (cf. Jensen and Ruback 1983, p. 19 and n. 14). For example, the market might well interpret a bid as signaling that the bidding firm's immediate past or expected future cash flows are higher than previously estimated, that this has actually prompted the bid, and that, although the takeover itself has a negative value, the combination of takeover and new information is on balance positive.

Similarly, abandoning a previous bid could convey negative information about the bidding firm's ability to pay for the proposed acquisition, perhaps because of negative events in its own operations. Losing a bid to rivals could signal limited resources. These problems of contaminating information make it difficult to interpret bidding firm price movements and to interpret the combined price movements of bidder and target.

2.1 *The Evidence about Target Firms*

Let us first examine, therefore, the more straightforward implications of the hubris hypothesis for target firms. Bradley, Desai, and Kim (1983*b*) present results for target firms in tender offers that are consistent with the implications. Target firms display increases in value on the announcement of a tender offer, and they fall back to about the original level if no combination occurs then or later.

A similar pattern is observed in Asquith's (1983) sample of target firms in unsuccessful mergers. These firms were targets in one or more merger bids that were later abandoned and for whom no additional merger bids occurred during the year after the last original bid was withdrawn. The original merger bid announcement was accompanied by a 7.0% average increase in target firm value that appears to be almost entirely reversed within 60 days (figure 1, p. 62). By the date when the last bid is abandoned, the target's price decline amounts to 8.1% (Table 9, p. 81), slightly more than offsetting the original increase.

The result may be partially compromised by the following problem. The "outcome date" of an unsuccessful bid is the withdrawal date of the final offer following which no additional bid is received for 1 year. Thus as of the outcome date the market could not have known for certain that other bids would not arrive. However, if the market had known that no other bids would arrive, the price decline would likely have been ever larger, so perhaps this partial use of hindsight was not material. In summary, target firm share behavior, as presented in Bradley et al. (1983*b*) for tender offers and in Asquith (1983) for mergers, is consistent with the hubris hypothesis.

2.2 *The Evidence about Total Gains*

The central prediction of the hubris hypothesis is that the total combined takeover gain to target and bidding firm shareholders is nonpositive.

None of the evidence using returns can unambiguously test this prediction for the simple reason that average returns of individual firms do not measure average dollar gains, especially in the typical takeover situation in which the bidding firm is much larger (cf. Jensen and Ruback 1983, p. 22). In some cases, the observed price increase in the target would correspond to such a trivial loss to the bidder that the loss is bound to be hidden in the bid/ask spread and in the noise of daily return volatility.

In an attempt to circumvent the problem that returns cannot measure takeover gains when bidder and target have different sizes, Asquith, Bruner, and Mullins (1983) take the unique approach of regressing the bidder announcement period return on the relative size of target to bidder. They reason that, if acquisitions benefit bidder firms, large acquisitions should show up as having larger return effects on bidder firm returns. They do find this positive relation for bidding firms. The same relation is not significant for target firms, although, as usual, target firms have much larger average returns. The positive relation for bidding firms is consistent with more than one explanation. It is consistent with the bidding firm losing on average, but losing less the larger the target. Perhaps a more accurate valuation is conducted when the stakes are large and this results in a smaller percentage loss to the bidder. Perhaps large targets are less closely held so that the takeover premium can be smaller relative to the preoffer price and still convince shareholders to deliver their shares. Perhaps bidders for larger targets have fewer rivals and can thus get away with a bidder-perceived "bargain."

The absence of any relation for target firms is puzzling under every hypothesis unless the entire gain accrues to the target firm shareholders (and Asquith et al. [1983] interpret their results to indicate that takeover gains are shared). If synergy is the source of gains, for example, target shareholder's returns would increase with the relative size of its bidder-partner.

Several studies have attempted to measure aggregate dollar gains directly. Halpern (1973) finds average market adjusted gains of $27.35 million in a sample of mergers between New York Stock Exchange–listed firms (p. 569); the gain was calculated over a period 7 months prior to the first public announcement of the merger through the merger consummation month. The standard error of this average gain, assuming cross-sectional independence, was $19.7 ($173.2/$\sqrt{77}$ [see table 3, p. 569]). In 53 cases out of 77, there was a dollar gain.

Bradley, Desai, and Kim (1982) present dollar returns for a sample of 162 successful tender offers from 20 days before the announcement until 5 days after completion. The average combined dollar increase in value of bidder plus target was $17 million, but this was not statistically significant. The $17 million gain was divided between a $34 million average gain by targets and a $17 million average loss to bidders. The authors note that the equally weighted average rate of return to bidders is positive, though the dollar change is a loss; they argue that this can be explained by skewness in the distribution of dollar changes.

In a revision of their 1982 paper, Bradley, Desai, and Kim (1983*a*) present slightly different results. The sample is expanded from 162 and 183 tender offer events, although the underlying data base appears to be the same (698 tender offers from October 1958 to December 1980). The only stated difference in the selection of samples is that the earlier paper excludes offers that are not "control oriented" (cf. Bradley et al. 1982, p. 13; and Bradley et al. 1983*a*, pp. 35–36). This sample change resulted in an average gain to targets of $28.1 million and to bidders to +$5.8 million (table 9). The authors say, however, that "the distributional properties of our dollar gain measures preclude any meaningful inferences about their significance" (p. 58).

Malatesta (1983) examines the combined change in target and bidder firms before, during, and after a merger. Jensen and Ruback summarize Malatesta's results as follows: "Malatesta examines a matched sample of targets and their bidders in 30 successful mergers and finds a significant average increase of $32.4 million ($t$ = 2.07) in their combined equity value in the month before and the month of outcome announcement. . . . This evidence indicates that changes in corporate control increase the combined market value" (1983, p. 22).

Malatesta (1983) himself does not reach so definite a conclusion. In fact, his overall interpretation of the evidence is that "the immediate impact of merger per se is positive and highly significant for acquired firms but *larger in absolute value and negative* for acquiring firms" (p. 155; emphasis added). Jensen and Ruback were referring to smaller samples of matching pairs. Even for this sample, Malatesta says, the results "provide *weak* evidence that successful resolution of these mergers had a positive impact on combined shareholder wealth" (p. 170; emphasis added). In 2 months culminating in board approval of the merger, the combined gain was positive, but "over the entire interval −60 to 0 [months], the cumulative dollar return is a trival 0.29 million dollars" (p. 171). Of course, this could be due to selection bias; bidding or acquired firms or both may tend to be involved in mergers after a period of poor performance. According to Asquith's (1983) results, however, this is true only for targets. The opposite is true for bidders; they tend to display superior performance prior to the merger bid announcement. During the culminating merger months, the acquiring firms' gains in Malatesta's sample were not statistically significant (although the acquired firms' were).

Malatesta's month zero is when the board announced merger approval, not when the merger proposal first reached the public. Even if the merger per se has no aggregate value, the price reaction on approval could be positive because it signals that court battles, further bids to overcome rivals, and other costly events associated with hostile mergers will not take place in this case, although their possibility was signaled originally by the merger proposal. Malatesta does not present evidence about the dollar reactions of the combined firm on the first announcement of the merger proposal.

Firth (1980) presents the results of a study of takeovers in the United Kingdom. In his sample, target firms gain, and bidding firms lose, both statistically significantly. The average total change in market value of the two firms in a successful combination, from a month prior to the takeover bid through the month of acceptance of the offer, is £−36.6 million. No t-statistic is given for this number, but we can obtain a rough measure of significance by using the fact that 224 of 434 cases displayed aggregate losses. If these cases were independent, the t-statistic that the true proportion of losing takeovers is greater than 50% is about .67.

The relative division of losses was examined by Firth (1980) in an ingenious calculation that strongly suggests the presence of bidding errors. The premium paid to the target firm (in £) as a fraction of the size of the bidding firm was cross-sectionally related to the percentage loss in the bidding firm's shares around the takeover period. The regression coefficient was −.89 (t = −5.94). Firth concludes (p. 254), "This supports the view that the stock market expects zero benefits from a takeover, that the gains to the acquired firm represent an 'overpayment' and that the acquiring company's shareholders suffer corresponding losses."

Using dollar-based matched pairs of firms, Varaiya (1985) finds that the aggregate abnormal dollar gain of targets is $189.4 million while the average abnormal dollar loss of bidders is $128.7 million for 121 days around the takeover announcement. The aggregate gain of $60.7 ($189.4 − 128.7) is not statistically significant, on the basis of a parametric test, though a nonparametric test does indicate significance. Varaiya also reports a cross-sectional regression that indicates that, the larger the target's dollar gain, the larger the bidder's dollar loss. The regression coefficient was −.81 (t = −2.81).

To summarize, the evidence about total gains in takeovers must be judged inconclusive. Results based on returns are unreliable. Malatesta's dollar-based results show a small aggregate gain in the months just around merger approval in a small matched sample and an aggregate loss in a larger unmatched sample. The interpretation of Malatesta's results is rendered difficult by the possibility of losses or gains in prior months, after announcement of a merger possibility but before final approval is a certainty. Dollar-based results presented by Bradley et al. (1982, 1983*a*) show a small and insignificant aggregate gain. Firth's (1980) British results show an insignificant aggregate loss. Both Firth (1980) and Varaiva (1985) present persuasive evidence for the existence of overbidding. But, on balance, the existence of either gains or losses to the combined firms involved in corporate combinations remains in doubt.

This mixed and insignificant evidence is made even less conclusive (if that is possible) by potential measurement biases. There is a potential upward bias in the measured price reaction of bidding firms (and thus of the aggregate) caused by contaminating information. There is a potential downward bias due to prior anticipation of the takeover event, as explained by Schipper and Thompson (1983), and another potential downward bias in some studies due to an improper computation of abnormal returns (Chung and Weston 1985). These biases will be discussed in detail next, in connection with the empirical findings for bidding firms.

2.3 *Evidence about Bidding Firms: The Announcement Effect*

The hubris hypothesis predicts a decrease in the value of the bidding firm. As pointed out previously, this decrease may not be completely reflected in a market price decline because of contaminating information in a bid, because the bid has been (partly) anticipated, or simply because the economic loss is too small to be reliably reflected in prices.

The data contain several interesting patterns. Asquith (1983) finds that bidding firm shares show "no consistent pattern" around the announcement date, but, "in summary, bidding firms appear to have small but insignificant positive excess returns at the press day" (p. 66). Some of Asquith's other results are understandable under the hubris hypothesis. Before the first merger bid, for instance, firms who become successful bidders have much larger price increases than firms whose bids are unsuccessful. One would expect a higher level of hubris and thus more aggressive pursuit of a target in firms that had experienced recent good times.

Asquith's results are in conflict with those of Dodd (1980), who finds statistically significant negative returns at the bid announcement. Jensen and Ruback (1983) noted the difference in results, and they asked Dodd to check his data and computer program, which they report (Jensen and Ruback 1983, p. 17, n. 12) he did without finding an error.[1]

1. Recently, Chung and Weston (1985) suggested that part of the difference in results could be explained by an improper calculation of "abnormal" returns around the merger announcement. Chung and Weston point out that the premerger period generally displays statistically significant positive returns for bidding firms. If data from this period are used to estimate abnormal returns at merger announcement, the measured announcement effect will be biased downward. The reported difference between, say, Dodd (1980) and Asquith (1983) would be reduced by a recalculation by Dodd excluding the preannouncement period. However, it probably would not be entirely eliminated; the bias appears to be only a small fraction of Dodd's observed announcement effect.

Negative bidder returns were also found by Eger (1983) in her study of pure exchange (noncash) mergers. Bidding firm stock prices declined, on average by about 4%, from 5 days prior to merger bid announcement to 10 days afterward (Eger 1983, table 4, p. 563). The decline was statistically significant. Eger suggests that the difference between her results and Asquith's (1983) might be attributable to a difference between mergers involving cash and pure stock exchange mergers; and she notes that tender offers, which often involve cash, seem to display more positive bidder stock price reactions (see below).

In his study of United Kingdom takeovers, Firth (1980) reports statistically significant negative bidding firm returns in the month of the takeover announcement. Eighty percent of the bidders had negative abnormal returns during that month, and the t-statistic for the average return was about -5.0 (cf. Firth 1980, table 5, p. 248).

Varaiya (1985) also finds statistically significant negative returns for bidding firms on the announcement day. He reports also that the bidder's loss is significantly larger when there are rival bidders.

A recent paper by Ruback and Mikkelson (1984) documents announcement effects of corporate purchases of another corporation's shares according to the stated purpose of the acquisition (filed on form 13-D with the Securities and Exchange Commission). The 2-day announcement effect for acquiring firms was positive and statistically significant for the 370 firms whose stated purpose was not a takeover. In contrast, for 134 acquiring firms indicating an intention to effect a takeover, the announcement effect was negative and significant (table 4, p. 17).

Studies of individual cases have been mixed. For example, Ruback (1982) argues that DuPont's large stock price decline in announcing a bid to take over Conoco could be an indication that managers (of DuPont) "had an objective function different from that of shareholder wealth maximization" (p. 24). However, he rejects this explanation because of "the magnitude of Conoco's revaluation and the lack of evidence that DuPont's management benefitted from the acquisition" (p. 24). He also rejects every other explanation except inside information possessed by DuPont and not yet appreciated by the market; but even this hypothesis "cannot be confirmed since the nature of the information is unknown" (p. 25).

One interesting aspect of the DuPont/Conoco case is that DuPont's decline was more than offset by Conoco's gain; that is, the total gain was positive (although the bidding firm lost). This suggests that nonhubris factors were indeed present, bringing a total gain to the corporate combination, but that overbidding was present too, resulting in a loss to DuPont shareholders.

The other case study by Ruback (1983) finds only a small negative effect for Occidental Petroleum in its bid for Cities Service. Cities Service's stock price increased by a relatively small amount for a target firm, and the total effect was positive. Apparently, there was little significant hubris evidenced by Occidental (who offered only a small premium). An interesting sidelight was the performance of Gulf Oil, a rival bidder who withdrew. It suffered a loss far in excess of Cities Service's gain.

Schipper and Thompson (1983) find a positive price reaction around the announcement that a firm is embarking on a program of conglomerate acquisitions. Also they observe negative price reactions of such firms to antimerger regulatory events. The two findings are interpreted as at least consistent with the proposition that acquisitions are positive net present value projects for the bidding firm. However, the authors emphasize the tentative nature of their conclusion (pp. 109–11). For example, they note that the announcement of an acquisition program is sometimes accompanied by "announcements of related policy decisions, such as de-emphasis of old lines of business, changes in management, changes in capital structure or specific merger proposals" (p. 89). Even without such explicit contaminating information, announcement of the program could be interpreted as good news about the future profitability of the bidder's current assets rather than about the prospect of an undisclosed future target firm to be obtained at a bargain price.

The possibility of contaminating information is a central problem in interpreting the price movement of a bidding firm on the announcement date of an intended acquisition. Bidders are activists in the takeover situation, and their announcements may convey as much information about their own prospects as about the takeover. To mention one example of the measurement problem, mergers are usually leverage-increasing events. It is well documented from studies of other leverage-increasing events, such as exchange offers (Masulis 1980) and share repurchases (Vermaelen 1981), that positive price movements are to be expected. Thus to measure properly that part of the gain of a bidding firm in a merger that is attributable to the merger per se and not to an increase in leverage, we ought to deduct the price increase that would have been obtained by the same firm through independently increasing its leverage by the same amount.[2]

2. I am grateful to Sheridan Titman for pointing out this possibility.

The measurement problem induced by the disparate sizes of target and bidder is the subject of a paper by Jarrell (1983). Jarrell argues that, when a bidder is several times larger than a target, a gain to the bidder equal in size to the gain observed in the target can be hidden in the noise of the bidder's return variability; that is, the t-statistic for the bidder's effect is likely to be much smaller than for the target's effect. Jarrell suggests solving this problem by adjusting the bidder's t-statistic upward by a factor proportional to the relative sizes of bidder and target. When he makes the adjustment in his sample, bidding firms display significantly positive price movements from 30 days prior to 10 days after the takeover announcement. The mean abnormal return prior to adjustment is 2.3%; after adjustment it is 9.2%. Similarly, the combined bidder and target returns become more statistically significant.

The problem with the Jarrell adjustment is that it can be applied to any sample in order to render a sample mean of either sign statistically significant. For example, if Firth (1980) had adjusted his bidding firm returns downward according to the relative sizes of bidder and target, he could have concluded that British takeovers had significant aggregate negative effects on shareholders. This does not imply that Jarrell's conclusions are incorrect, but we are certainly entitled to remain skeptical. Several studies have reported positive bidder gains, and several others have reported losses. Applying the Jarrell technique indiscriminately to all of them could make the gains or losses more "significant," but this would simply create more confusion since the now "significant" results would disagree across studies.

2.4 *Evidence about Bidding Firms: Resolution of Doubtful Success*

There is some evidence available to help isolate the reevaluation of a bidding firm's own assets induced by the bid but not caused by the proposed corporate combination itself. Asquith's (1983) sample of bidding firms in mergers is separated into successful and unsuccessful bidders, and both samples are examined prior to bid announcement, between announcement and merger outcome, and after outcome. For the successful group, merger outcome is the actual date when the target firm is delisted; this is presumably the effective date of the merger. At the original bid announcement, the market cannot know for sure whether such firms actually will consummate the merger, that is, be in the "successful" group. There is only a probability of success. Between the bid announcement and the final outcome this probability goes to 1.0 for firms in the successful group. Thus if the combination itself has value for the bidder, these bidding firms should increase in value over this interim period. They do not. On average, successful bidding firms decline in value by .5% over the interim period (see Asquith 1983, fig. 4, p. 71; table 9, p. 81). The decrease in value is small and statistically insignificant, but the result has economic significance because the opposite sign must be observed if the corporate combination per se has value. If the combination has substantial value, one might have expected to observe a statistically significant upward price movement between bid announcement and outcome, provided, of course, that the upward revision in probability of success is large enough to show up.

Firms in Asquith's successful bidder group have very large prebid 20 days before the bid announcement. They have small positive returns (.2%) on the announcement date. The entire sequence of returns for successful bidding firms is consistent with the hubris hypothesis. In the prebid period, excellent performance endows management with both hubris and cash. A target is selected. The bid itself signals a small upward revision in the market's estimate of the bidding firm's current assets that is not completely offset by the prospect of paying too much for the target. Then there is a small downward revision in bidder firm value as it becomes more probable and then certain that the target will be acquired (at too high a price).

Eckbo (1983) reports a small and insignificant decline during the 3 days subsequent to the initial merger bid. But Eckbo's "successful" bidder is defined as one who is unchallenged on antitrust grounds; this may be a less relevant representation of actual success for our purposes here.

Eger (1983, p. 563) finds significant negative bidder firm returns averaging −3.1% in the 20 days after the original announcement of a merger that is ultimately successful. Most of this decline occurs in the first 10 days after the merger announcement. The bonds of these firms also decline slightly in price over the same period. This is consistent with a price decline in the total value of the bidding firm as it becomes more certain that the merger will succeed.

The most significant price decline between merger proposal and outcome is reported by Dodd (1980). Successful bidding firms decline in value by 7.22% from 10 days before the bid is announced until 10 days after the merger outcome, where outcome is defined as target stockholder approval of merger bid. The price decline is statistically significant. In the 20 days prior to the outcome date, successful bidder firms in Dodd's sample fall in price by about 2% (p. 124).

Evidence from papers using monthly data is more difficult to interpret, but the patterns do seem consistent with a negative price movement between merger announcement and successful outcome. For example, Langetieg's (1978, p. 377) bidding firms show a significant price decline continuing in the combined firm after the merger outcome. Similarly, Chung and Weston (1982, p. 334) report price declines between merger announcement month and merger completion in pure conglomerate mergers. However, the decline is not statistically significant.

Similar evidence is given in Malatesta (1983, table 4, p. 172). Acquiring firms in this sample have significant negative price performance in the period after the first announcement of a merger proposal. Since the data are monthly, the merger outcome date could be included somewhere in the sample period. This means that part of the puzzling post-outcome negative performance detected by Langetieg (1978) and Asquith (1983) might be included in Malatesta's table 4 results. In tables 5 and 6 Malatesta presents performance results for acquiring firms after the "first announcement of board/management approval of the merger" (p. 170). The returns are strongly negative in this period. This might not be such a puzzle if "board/management approval" still leaves open the possibility of withdrawal, for then the absolute certainty of merger (and the concomitant price drop expected under the hubris hypothesis) would occur sometime after this particular event date.

In summary, during the interim period between initial bid and successful outcome, the average price movement of successful merger bids is small, so it is not possible to draw strong implications. However, the pattern is generally consistent with the hubris hypothesis, which predicts the observed loss in value of bidding firm's shares. The loss is statistically insignificant in Asquith's sample but is significant in the samples of Dodd (1980) and Eger (1983) and in the monthly data samples of Langetieg (1978) and Malatesta (1983).

Evidence about the interim period from tender offer studies is mixed. One study seems to be clearly inconsistent with hubris alone; Bradley's (1980) sample of 88 successful bidding firms show a price rise after the announcement data and before the execution date. The number is not given, but the plot of the mean abnormal price index (p. 366) indicates that the gain is approximately 2%–3%.

The interim price movement of the successful acquiring firm is reported by Ruback and Mikkelson (1984) as −1.07% with a t-statistic of −2.34 (table 6). The sample is not dichotomized by merger vs. tender offers, however, and it probably contains some of both types of takeovers.

The results given by Kummer and Hoffmeister (1978) for a 17-firm matched sample of tender offers are more difficult to interpret because the data are monthly and, apparently because of the small size of the cross-sectional sample, the time series of prices relative to the event data appears to be more variable. Abnormal returns are positive and largest in the announcement month but are also positive in months +1 and +2. If the tender offer is revolved sometime during these 2 months, the results are basically the same as Bradley's (1980). Months +3 to +12 witness a decline of about 4%. If the success of the tender offer is not known until sometime during this period, an interpretation could be made similar to the one discussed above concerning Asquith's and Dodd's samples of successful merger bids.

An identical set of nonconclusive inferences can be drawn from the monthly data of Dodd and Ruback (1977). There appears to be a positive price movement by successful bidders just after the announcement month followed by a price decline later. The decline over the 12 months after a bid amounts to −1.32%, but it is not statistically significant.

Bradley's daily results probably represent the best available evidence against the hubris hypothesis. The detected movement is small, but, unlike the case of merger's, the bidding firm's price does increase on average in Bradley's sample. This is consistent with the proposition that tender offers increase aggregate value and that some of the increase accrues to tender offer bidders. Whether the evidence is sufficiently compelling, particularly when balanced against evidence of an opposite character, is up to further investigation to decide definitely.

One other piece of evidence from the interim period between announcement and outcome is worthy of contemplation. This is the price behavior of the first bidder's stock on the announcement of a rival bid. In their study of unsuccessful tender offers, Bradley et al. (1983*b*) report a significant price drop in the first bidder's stock. In contrast, Ruback and Mikkelson (1984) report a significant price increase (table 5); however, the latter sample consists not only of ultimately unsuccessful bidders in tender offers but of all corporate investors in other stock (including many who are not contemplating a takeover).

A price drop in the first takeover bidder's stock on the announcement of a rival bid is explainable by hubris. The rival bid may set off a bidding war that the market expects to result in a large loss for the winner. It would be extremely informative to observe the price reaction of the first bidder when it becomes evident that the rival bidder has won.

Finally, it should be noted that the price change after the resolution of a successful bid (either merger or tender offer) is almost uniformly negative (cf. Jensen and Ruback 1983, table 4, p. 21) and is relatively large in magnitude. This is a result that casts doubt on all estimates of bidding firm returns because it suggests the presence of substantial measurement problems.

3. Summary and Discussion

The purpose of this paper is to bring attention to a possible explanation of the takeover phenomenon of mergers and tender offers. This explanation, the hubris hypothesis, is very simple: decision makers in acquiring firms pay too much for their targets on average in the samples we observe. The samples, however, are not random. Potential bids are abandoned whenever the acquiring firm's valuation of the target turns up with a figure below the current market price. Bids are rendered when the valuation exceeds the price. If there really are no gains in takeovers, hubris is necessary to explain why managers do not abandon these bids also since reflection would suggest that such bids are likely to represent positive errors in valuation.

The hubris hypothesis can serve as the null hypothesis of corporate takeovers because it asserts that all markets are strong-form efficient. Financial markets are aware of all information. Product markets are efficiently organized. Labor markets are characterized by managers being employed in their best operational positions.

Hubris predicts that, around a takeover, (*a*) the combined value of the target and bidder firms should fall slightly, (*b*) the value of the bidding firm should decrease, and (*c*) the value of the target should increase. The available empirical results indicate that the measured combined value has increased in some studies and decreased in others. It has been statistically significant in none. Measured changes in the prices of bidding firms have been mixed in sign across studies and mostly of a very small order of magnitude. Several studies have reported them to be significantly negative, and other studies have reported the opposite. Target firm prices consistently display large increases, but only if the initial bid or a later bid is successful. There is no permanent increase in value for target firms that do not eventually enter a corporate combination.

The interpretation of bidding firm returns is complicated by several potential measurement problems. The bid can convey contaminating information, that is, information about the bidder rather than about the takeover itself. The bid can be partially anticipated and thus result in an announcement effect smaller in absolute value than the true economic effect. Since bidders are usually much larger than targets, the effect of the bid can be buried in the noise of the bidder's return volatility. There is weak evidence from the interim period between the announcement of a merger and the merger outcome that the merger itself results in a loss to the bidding firm's shareholders; but the interim period in tender offers shows some results that favor the opposite view. Both findings have minimal statistical reliability.

The final impression one is obliged to draw from the currently available result is that they provide no really convincing evidence against even the extreme (hubris) hypothesis that all markets are operating perfectly efficiently and that individual bidders occasionally make mistakes. Bidders may indicate by their actions a belief in the existence of takeover gains, but systematic studies have provided little to show that such beliefs are well founded.

Finally, I should mention several issues that have arisen as objections by others to the hubris idea. First, the hubris hypothesis might seem to imply that managers act consciously against shareholder interests. Several recent papers that have examined nontakeover corporate control devices have concluded that the evidence is consistent with conscious management actions against the best interests of shareholders.[3] But the hubris hypothesis does not rely on this result. It is sufficient that managers act, de facto, against shareholder interests by issuing bids founded on mistaken estimates of target firm value. Management intentions may be fully consistent with honorable stewardship of corporate assets, but actions need not always turn out to be right.

Second, it might seem that the hubris hypothesis implies systematic biases in market prices. One correspondent argued that stock prices would be systematically too high for reasons similar to those advanced in E. M. Miller's (1977) paper. This implication is not correct, however, for the simple reason that firms can be either targets or bidders. If bidders offer too much, their stock price will fall ex post while their target's price will rise. On average over all stocks, this cancels. Unless one can predict which firms will be targets and which will be bidders, there is no bias in any individual firm, and certainly no bias on average over all firms.

Third, an argument can be advanced that the hubris hypothesis implies an inefficiency in the market for corporate control. If all takeovers were prompted by hubris, shareholders could stop the practice by forbidding managers ever to make any bid. Since such prohibitions are not observed, hubris alone cannot explain the takeover phenomenon.

The validity of this argument depends on the size of deadweight takeover costs. If such costs are relatively small, stockholders would be indifferent to hubris-inspired bids because target firm shareholders would

3. See Bradley and Wakeman (1983), Dann and DeAngelo (1983), and DeAngelo and Rice (1983). Linn and McConnell (1983) disagree with the last paper. The possibility that managers do not act in the interest of stockholders has frequently been associated with the takeover phenomenon. For example, in a recent review, Lev (1983, p. 15) concludes by saying, "I think we are justified in doubting . . . the argument that mergers are done to maximize stockholder wealth." Foster (1983) seems to share this view or at least the view that bidders make big mistakes. Larcker (1983) presents interesting results that managers in large takeovers are more likely to have short-term, accounting-based compensation contracts. He finds that, the more accounting-based the compensation, the more negative is the market price reaction to a bid. Larcker also suggests that managers who own less stock in their own company are more likely to make bids.

gain what bidding firm shareholders lose. A well-diversified shareholder would receive the aggregate gain, which is close to zero.

Fourth, and finally, a frequent objection is that hubris itself is based on a market inefficiency defined in a particular way; in the words of one writer, "It seems to me that your hypothesis does not rest on strong form efficiency, because it presumes that one set of market bidders is systematically irrational" (private correspondence). This argument contends that a market is inefficient if some market participants make systematic mistakes. Perhaps one of the long-term benefits of studying takeovers is to clarify the notion of market efficiency. Does efficiency mean that every individual behaves like the rational, maximizing ideal? Or does it mean instead that market interactions generate prices and allocations indistinguishable from those that would have been generated by rational individuals?

The earlier drafts of this paper elicited many comments. It is a pleasure to acknowledge the benefits derived from the generosity of so many colleagues. They corrected several conceptual and substantive errors in the previous draft, directed my attention to other results, and suggested other interpretations of the empirical phenomena. In general, they provided me with an invaluable tutorial on the subject of corporate takeovers. The present draft undoubtedly still contains errors and omissions, but this is due mainly to my inability to distill and convey the collective knowledge of the profession. Among those who helped were C. R. Alexander, Peter Bernstein, Thomas Copeland, Harry DeAngelo, Eugene Fama, Karen Farkas, Michael Firth, Mark Grinblatt, Gregg Jarrell, Bruce Lehmann, Paul Malatesta, Ronald Masulis, David Mayers, John McConnell, Merton Miller, Stephen Ross, Richard Ruback, Sheridan Titman, and, especially, Michael Jensen, Katherine Schipper, Walter A. Smith, Jr., and J. Fred Weston. I also benefited from the comments of the finance workshop participants at the University of Chicago, the University of Michigan, and Dartmouth College, and of the referees.

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