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# Investment Insights

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Is small-cap investing worth it?  
Two decades of research  
on small-cap stocks

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## Is small-cap investing worth it? Two decades of research on small-cap stocks

It has long been part of market folklore that stocks of smaller companies are high-returning investments, due perhaps to aggressive management and the fact that they have room to grow, or because such issues are neglected by many investors.<sup>1</sup> This folklore is consistent with the intellectual constructions of the 1960s and 1970s—the efficient market hypothesis and the Capital Asset Pricing Model (CAPM)—*only* if small stocks have greater risk, with high returns being a compensation for bearing that risk. Because the stakes were high among both academics and practitioners for finding convincing evidence that the market is not efficient, or that the CAPM does not hold, a great deal of effort has been devoted to searching for market “anomalies.”<sup>2</sup> The first big payoff was the discovery (or rediscovery, if one counts the folklore) of high returns earned by small-capitalization stocks in the United States, as documented by Banz (1981).

The astonishment, skepticism and delight that greeted this finding are difficult to overstate. By 1981, the efficient market hypothesis was so deeply ingrained in academic financial thinking that a simple beat-the-market rule (buy an index of small-cap stocks) was almost inconceivable. That it appeared to work on a risk-adjusted basis was contrary to the CAPM, which appeared to many academics to be true by construction and thus unlikely ever to be overturned.<sup>3</sup> Practitioners, for their part, were generally pleased to see respected academics chipping away at their own efficient-market framework, for if one major anomaly was discovered, then others would surely be found, vindicating the active manager’s craft.

Over the period since Banz’s discovery, researching the small-stock effect has become a favorite pastime of academics and research-oriented practitioners so that the volume of writing on this topic is enormous. This paper reviews the most influential papers on small stocks, classifying them into “threads” or intellectual themes that tie them together. Criteria for including a paper here are: (1) authorship by respected academics and research-oriented practitioners; (2) influence on later writing and on portfolio management; and (3) where these criteria are met only marginally, an innovative approach or unexpected conclusion that helps one think about small-capitalization stocks in a new way.

## Threads of research on small-cap stocks

The following are the principal threads by which we classify research conclusions on small-capitalization stocks and the size effect, and will serve as the basis for organizing this paper. Of course, the researchers themselves knew no such neat categories, and a given article may well be categorized in multiple threads.

### THREAD 1

*Discovery and measurement* of a historical small-stock premium

### THREAD 2

Evidence that the small-stock premium is a payoff for the *greater risk* of small stocks

- 2a The premium *is* a payoff for risk that is captured by properly measured beta
- 2b The premium is a payoff for risk that *is not* captured in beta
- 2c An argument that one would observe some small-stock premium even if small stocks are fairly priced

### THREAD 3

Evidence that the small-stock premium is compensation to the investor for *transaction, information and other costs*

### THREAD 4

Evidence that the small-stock premium is *unreliable or does not exist*

- 4a Arguments that the small-stock premium derives from a *single observation or one-time repricing* of these stocks
- 4b Arguments that the small-stock effect is *really a proxy for other effects such as low P/E*

### THREAD 5

Behavioral explanations for the small-stock effect

### THREAD 6

Evidence that the small-stock premium is a *true anomaly* (that is, that small stocks offer an excess expected return after controlling for risks, marketability, and other investor costs)

### THREAD 7

Short-term timing of the small-stock effect (the January effect, etc.)

### THREAD 8

Long-term timing of the small-stock effect

### THREAD 9

The small-stock effect internationally

## THREAD 1

## Discovery and measurement of the small-stock premium

As noted in the introduction, the modern history of research on the small-stock effect begins with Banz (1981). A young Northwestern University professor whose research on the topic was actually finished in 1979, Rolf Banz summarized his predecessors' efforts as follows:

*[R]ecent evidence suggests the existence of additional factors [beyond the CAPM beta] which are relevant for asset pricing. Litzenberger and Ramaswamy (1979) show a significant positive relationship between dividend yield and return of common stocks for the 1936–1977 period. Basu (1977) finds that price-earnings ratios and risk adjusted returns are related. . . . This study contributes another piece to the emerging puzzle.*

Banz then set up a two-factor regression in which one factor is the market or beta factor and the other is the sensitivity of the stock's return to its market capitalization, or size. This is a *joint* test of the efficient market hypothesis and the CAPM; that is, if the CAPM is correctly specified and the market is efficient, the coefficient on the second (size) factor should be zero. Using data for all New York Stock Exchange (NYSE) stocks over the period 1926–1975, Banz found that even after controlling for beta, the smallest firms outperformed the largest ones by about 5% per year, and that the effect is concentrated

among very small firms (the fourth and fifth quintiles of NYSE stocks ranked by size). This is a very large effect, and Banz emphasizes that “the magnitude...during the past 45 years is such that it is of more than just academic interest.”

Banz's speculations at the end of his article are a model of caution in interpreting startling new findings. Noting that “the size effect exists but it is not at all clear why,” he suggests that it may be a proxy for other “true but unknown factors correlated with size.” He also invokes Klein and Bawa (1977), who suggest that securities for which little information is available will be underpriced and offer superior returns; small firms may fit this categorization. Finally, Banz comments that “given the longevity [of the effect], it is not likely that it is due to a market inefficiency but it is rather evidence of a pricing model misspecification,” a view that prefigures the work of Fama and French many years later and that conforms to a position we take later in this paper.

Marc Reinganum is sometimes considered the co-discoverer of the small-stock effect. Actually, Reinganum (1981) takes the existence of a small-firm effect as a given (citing Banz's 1978 PhD dissertation) and attempts to disentangle the size effect from the P/E effect.<sup>4</sup> Because of the need for earnings as well as price and capitalization data, his

study covers only 1963–1977. Reinganum concludes that the size effect subsumes or dominates the P/E (or E/P) effect:

*After controlling returns for any E/P effect, a strong firm size effect still emerged. But, after controlling returns for any [size] effect, a separate E/P effect was not found. While an E/P anomaly and a [size] anomaly exist when each variable is considered separately, the two anomalies seem to be related to the same set of missing factors, and these factors appear to be more closely associated with firm size than E/P ratios.*

Ibbotson and Sinquefeld (1982) extended the measurement of returns on stocks of all capitalization categories and also measured their risk (standard deviation). Taking a “just the facts” approach, they note:

*Over the entire [1926–1981] period, the arithmetic mean of the annual returns was 18.1% for small stocks... [and] ...11.4% for [large] stocks.... Small stock returns were more volatile.... The standard deviation of small stock annual returns was 37.0% [compared to 21.7% for large stocks], while the returns ranged from 142.9% in 1933 to -58.0% in 1937.*

Applying a simple test, then, the greater returns of small stocks were not out of proportion to their greater risk. As other researchers had pointed out,

however, this risk did not show up in the beta as it was then being measured. In [Thread 2](#) of this paper we address the issue of measuring the beta of small stocks more precisely.

Ibbotson and Sinquefeld (1982) also identify, apparently for the first time, the long waves of small-stock outperformance and underperformance that we cover at greater length in [Thread 8](#) of this paper:

*From 1926 to 1931, the compound annual rate of return for [the premium of the smallest NYSE stocks over the largest ones] was -17.0%, and the premium was negative in every year. From 1932 to 1945 the premium had a compound...return of 16.6% and was positive in 12 of the 14 years. From 1946 to 1957, the compound... return was -4.4%.... Finally, the small stock premium returned 7.6% per year from 1958 to 1981. Most of the 1958–1981 gain occurred in the last eight years of the period, in which the compound [relative] rate of return was 18.5% and the premium was positive in each year.*

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We would add that 18.5% is a very high annual rate of outperformance indeed, and it is little surprise that a small-stock effect might be discovered after such a period. Interestingly, the next downward-trending period began almost as soon as the small-stock effect was discovered. From 1984 to 1990, small stocks underperformed at a compound annual rate of -10.5%, and the premium was negative in all but one year. Starting in 1991, the small-stock effect has been mixed, with good performance in 1991–1994 and poor performance thereafter.<sup>5</sup>

The works of Banz, Reinganum, and Ibbotson and Sinquefeld were occasionally criticized as representing a sector of

stocks too small to be representative of small-stock investing as actually practiced. (As of year-end 1997, the index used by Ibbotson and Sinquefeld to represent small stocks had a weighted-average capitalization of \$161 million.) To remedy this, Ibbotson Associates (1998), updating the work of Ibbotson and Sinquefeld, also constructs a spanning set of size-decile portfolios of the NYSE<sup>6</sup> (see [Table 1](#)).

Notably, both the arithmetic-mean return and risk of the portfolios increase monotonically<sup>7</sup> as one moves from larger to smaller stocks. The 15 years of subpar performance of small stocks has not undone the effect accumulated over the period that started in 1926.

Table 1

Size-Decile Portfolios of the New York Stock Exchange: summary statistics of annual returns, 1926–1997.			
Decile	Geometric mean (%)	Arithmetic mean (%)	Standard deviation (%)
1–Largest	10.2	11.9	18.9
2	11.3	13.7	22.3
3	11.7	14.3	24.1
4	11.9	15.0	26.5
5	12.3	15.8	27.3
6	12.1	15.8	28.4
7	12.2	16.4	30.8
8	12.4	17.5	34.6
9	12.5	18.2	37.1
10–Smallest	13.9	21.8	46.1
NYSE total value-weighted index	10.7	12.6	20.2

Source: Ibbotson Associates, *Stocks, Bonds, Bills, and Inflation: 1998 Yearbook*, page 131.

## THREAD 2

## The small-stock premium as a payoff for taking risk

The fact that small stocks have much more risk than large stocks prompted many researchers to speculate that either (1) small stocks have higher betas than large stocks, but the betas were not being properly measured by the methods of Banz, Reinganum, and Ibbotson and Sinquefeld; or (2) small stocks have risk that is unrelated to beta but that is compensable in the market.

### 2a The premium is a payoff for risk that *is* captured by properly measured beta

Unlike large stocks that are traded many times per day, many small stocks are traded infrequently. As a result, their betas are understated by the ordinary-least-squares (OLS) regression technique.<sup>8</sup> Scholes and Williams (1977) and Dimson (1979) pointed out that by summing the betas from led, contemporaneous, and lagged regressions of daily stock returns on daily market returns, betas can be estimated more accurately for stocks that do not trade every day. Further adjustments are also possible. Richard Roll (1981) speculated that infrequent trading might cause the measured betas of small stocks to be much too low, and conducted a simple test suggesting the conjecture was right. He compared an equally-weighted portfolio of all NYSE- and American Stock Exchange-listed stocks (in which small stocks have a large weight) with the value-weighted S&P 500 Index, using returns of different time frequencies:

*[N]otice that when weekly, bi-weekly, monthly, bi-monthly, quarterly, and semi-annual returns are employed, the correlation coefficient of returns stays about the same. In contrast, the beta and the ratio of total variances increases uniformly and materially.*

Remarkably, the ratio of variances rises more than threefold, so that an investor with a long time horizon would regard the equally weighted portfolio as more than three times as risky as the S&P 500. This contrasts with the OLS beta, which suggests that the equally weighted portfolio is less than 1.5 times as risky as the S&P 500. Clearly there is a risk factor for smaller stocks that is not being captured by beta.

Scholes and Williams (1977) and Dimson (1979) pointed out that by summing the betas from led, contemporaneous, and lagged regressions of daily stock returns on daily market returns, betas can be estimated more accurately for stocks that do not trade every day.

The reason that small-stock portfolios are riskier as one lengthens the return measurement period is simple: their returns are *autocorrelated*. (Autocorrelation measures the extent to which one period's return influences the next period's return; in a highly autocorrelated return series, one downturn is likely to be followed by others, creating a risk that is not present when



returns are independent across time. In a highly autocorrelated series, then, good and bad returns tend to have “runs.”) Roll, relying on his initial conjecture, blames infrequent trading for the appearance of autocorrelation in small-stock returns, and concludes:

*At first, one might think there is a knotty theoretical question of how risk is related to the investment “horizon.” A moment’s reflection, however, reveals that no such problem is present. The true riskiness is exactly the same for all data intervals; it is simply underestimated for the shorter ones.*

This is true, however, *only* if the autocorrelation is an illusion caused by infrequent trading or other measurement problems. If, however, small-stock returns are truly autocorrelated (that is, if a perfect measurement existed and small-stock returns were still found to be autocorrelated), then the true riskiness is *not* the same for all data intervals. The risk is that low returns will lead to more low returns, resulting in a significant erosion of investor wealth over time. Returns measured over *whole years* tend (it is hoped) to eliminate biases from infrequent trading. The autocorrelation of annual small-stock returns in excess of S&P 500 returns, from Ibbotson Associates (1998), is 0.38, with a standard error of 0.12, indicating strong statistical significance (as though Ibbotson’s narrative about long periods of over- and underperformance were not enough). Clearly, the long-run autocorrelation of small-stock excess returns poses a risk to the investor that returns

might have a run of underperformance during the investor’s horizon—for which he might demand to be compensated.

In 1982, Reinganum authored a “direct test of Roll’s conjecture,” using Dimson’s (1979) method of measuring beta for stocks that trade infrequently. Roll had been correct in guessing that measured betas would rise if the method were changed to account for infrequent trading. To cite an extreme example, the beta for the smallest decile of NYSE and AMEX stocks measured over 1964–1978 rises from 0.75 using the OLS method to 1.69 using the Dimson method. Even with the much higher beta estimates, the returns of small stocks substantially exceed that which is predicted by the CAPM. (Note that the overall return on the market in excess of Treasury bills was flat over the period studied, so that beta risk went unrewarded in that particular time frame.) Reinganum’s test did not, however, consider the possibility that long-run autocorrelation in the underlying data poses a risk to the investor that is unrelated to beta.

In adjusting the betas of small stocks to capture their true risks, Ibbotson, Kaplan, and Peterson (1997) also build on the work of Dimson. Unlike Reinganum (1982), however, they adjust for “cross-autocorrelation,” which is the correlation of one time series with

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the “led” or “lagged” version of a *different* time series. By introducing autocorrelation into the analysis, they construct a beta that captures some of the risk of repeated underperformance to which we referred in the discussion of Roll (1981).<sup>10</sup> They find that the adjusted betas partially explain the size effect.

## 2b The premium is a payoff for risk that *is not* captured in beta

If one has made all reasonable adjustments to beta and that factor still does not explain the high returns on small stocks, then perhaps the small-stock premium is a payoff for a risk that is unrelated to beta. Such a finding would, of course, overturn the Capital Asset Pricing Model.<sup>11</sup>

Friend and Lang (1988) find that S&P stock ratings are extremely powerful in predicting differential returns and that they in essence subsume the small-stock premium as well as the CAPM beta.

Chan, Chen and Hsieh (1985) use Arbitrage Pricing Theory (APT)<sup>12</sup> to estimate the returns expected by small-stock investors, and determine that the five macroeconomic APT factors they use are sufficient to explain the additional returns of small stocks. The factors are industrial production, unanticipated inflation, changes in expected inflation, the realized excess return of Treasury bonds over short-term bills, and the realized risk premium on low-

grade corporate bonds over Treasuries. They use the market factor, identical to the CAPM beta, as a sixth explanatory variable.

If these APT factors explain the “return to size,” then transaction and information costs need not be invoked as a reason why small-stock investors appear to earn high returns before subtracting these costs. However, in the APT model, the small-firm effect is still a payoff for risk, and there is no free lunch in small-stock investing, even if all costs can be avoided. Moreover, we find the arguments relating small-stock returns to various types of costs to be very compelling (see [Thread 3](#)).

Friend and Lang (1988) take a creative turn in using Standard and Poor’s stock ratings, which are constructed by security analysts, to measure the risk of stocks. They find that these ratings are extremely powerful in predicting differential returns and that they in essence subsume the small-stock premium as well as the CAPM beta. Friend and Lang’s method is intuitively sensible in that if stocks have a type of systematic risk that is not reflected in the beta, security analysts might be able to find it using traditional financial analysis. By reading the description of the analyst’s methods, it seems that their concept of risk is closely aligned with standard deviation, because they are evaluating each stock on its own merits, not calculating the marginal contribution of the stock to portfolio risk (which is what beta does). This is noteworthy in light of our comments on standard deviation in our discussion of Ibbotson Associates (1998).

Unfortunately, there is (at least potentially) an element of circularity in Friend and Lang's approach. If security analysts know that a stock has a small market capitalization, and if they are aware that an expected-return premium attaches to small stocks, they may modify the risk rating of the stock accordingly. Thus, if there is a risk premium specifically for smallness, it will be reflected in security analysts' ratings, and these ratings may appear statistically to subsume or dominate the small-stock effect when all they are doing is mirroring it.

**Fama and French's results lend support to APT rather than the CAPM and bolsters the idea that the small-stock premium is a reward for taking risks that are unrelated to beta.**

The work in the 1980s of Chan, Chen and Hsieh, and of Friend and Lang, prefigures the later literature on the inadequacy of beta as a predictor of cross-sectional differences in stock returns. This literature came into its own with Eugene Fama and Kenneth French's landmark 1992 article, *The cross-section of expected stock returns*. Fama and French found that over 1963–1990, the reward for taking the risk of high-beta rather than low-beta stocks was almost exactly zero.<sup>13</sup> (Much earlier, Fischer Black [1972] predicted that if investors face restrictions on borrowing or selling

short, the CAPM line will be flatter than predicted by the original theory, but it is a stretch to read Fama and French's completely-flat CAPM line into Black's prediction.) Fama and French's results lend support to APT rather than the CAPM and bolsters the idea that the small-stock premium is a reward for taking risks that are unrelated to beta.

The debate in response to Fama and French on whether beta is "dead" is outside the scope of this paper, except to the extent that it sheds additional light on the small-stock premium. Most noteworthy is an article by Kothari, Shanken, and Sloan (1995), in which they use annual data to obtain estimates of small-stock beta that are not tainted by infrequent trading. They find that beta risk was rewarded over the same period as that tested by Fama and French, but that there is still a size effect. The authors also caution that all studies of stock-market "effects" tend to be tainted by data-snooping and hidden biases,<sup>14</sup> so that one should not jump to conclusions.<sup>15</sup>

## 2c **An argument that one would observe some small-stock premium even if small stocks are fairly priced**

A different tack is taken by Jonathan Berk, a young University of Washington professor who came over to economics from the hard sciences and is thus untainted by years of indoctrination. He points out that if markets discount riskier securities at higher discount rates, then firms of identical "physical size"

but differing risk will have differing market capitalizations, with the smaller firms offering higher returns even though they are fairly priced:

*Imagine an economy in which all firms are the same size (i.e., have the same value of assets in place). . . . Assume that all firms, because they have the same size, also have the same expected cash flows. Of course, [that] is not the same as having the same cash flows. Because some firms' cash flows are likely to be riskier than others, [their] discount rates will differ. . . . Consequently, the discounted value of a riskier firm (i.e., its market value) will be lower than the discounted value of a less risky firm's cash flows.*

*A firm's expected return is defined to be its expected cash flows divided by its market value. The assumption that all firms have the same expected cash flows implies that (risky) firms with low market values have high expected returns and vice versa. Thus, even though I have explicitly assumed that all firms have the same size, the so-called "size enigma" exists in this economy—market value is inversely related to return.*

Thus, even if small-cap stocks are fairly priced, one would observe a small-stock "effect" that has no implications for investment management. In other

words, the higher returns on small-cap are not really an effect; they simply mirror the market mechanism working to price riskier companies more cheaply than safe companies.

Market capitalization, then, is not a pure measure of a firm's size; it simultaneously measures size and the discount rate. (The discount rate on a security is also the expected return from holding the security.) To measure a firm's size alone, Berk uses two admittedly imperfect measures—sales and book value. He finds that the size effect is nonexistent when firms are ranked by these

Berk concludes that (1) because the size (market-capitalization) effect is theoretically predicted, we would be surprised *not* to find it in the data; and (2) we cannot use the market-capitalization effect to earn higher risk-adjusted returns, because the size premium is in reality a premium for risk, not for size.

variables. In other words, "the size enigma results from the part of market value that measures the firm's discount rate and not from a relation between the size of firms and returns." When stocks are sorted by these nonmarket measures of size, then, the results are completely consistent with the CAPM, and greater return and greater risk are associated with each other, with nothing left over for capitalization to explain.

Berk concludes that (1) because the size (market-capitalization) effect is theoretically predicted, we would be surprised *not* to find it in the data; and (2) we cannot use the market-capitalization effect to earn higher risk-adjusted returns, because the size premium is in reality a premium for risk, not for size. While Berk's contention that small stocks are fairly priced is hardly unique, his contribution is to take the small-stock effect out of the realm of anomalies. It is simply to be expected from the way markets are organized. The real anomaly, in Berk's view, would be if a small-stock premium were *not* observed.

### Conclusion

The preponderance of the evidence suggests that at least part of the small-stock effect cannot be explained by beta, no matter how accurately beta is measured. The best statistical explanation may be a risk premium for small size (independent of beta) in the style of Fama and French. This is somewhat unsatisfying: at some level, Fama and French have merely attributed to some sort of generalized "risk" the part of the return that beta cannot explain. Perhaps the small-stock effect is a pay-

off to the investor for being forced to pay transaction and information costs. Perhaps behavioral factors (that is, irrationality) are required to explain the small-stock premium. Perhaps, as Berk suggests, a small-size effect is implicit in the mechanism that markets use to price assets and we should be surprised not to find it. Finally, perhaps the premium is a type of free lunch or permanent arbitrage opportunity—a true capital-market anomaly. We explore these possibilities in the following sections.



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## THREAD 3

## Transaction, information, and other investor costs

The idea that the small-stock premium might be a form of compensation for transaction costs is simple, and was understandably raised quite early, by Hans Stoll and Robert Whaley in 1981. They write,

*A simple explanation of [the small-stock premium] is that an investor taking a small-firm portfolio position faces higher transaction costs than he does when he takes an otherwise similar large-firm position. The market maker's spread on a proportional basis is generally higher for small firms because of their infrequent trading activity and risk; and the broker's commission rate is, among other things, an inverse function of the price per share, a variable correlated with the total market value of the stock. In addition, there are other, less-explicit costs such as the cost of investigating and monitoring a firm that may be higher for small firms. ... [Our study suggests that while] small firms find it more costly to attract investment funds, unjustified discrimination against small firms is not necessarily present.<sup>16</sup>*

Unjustified discrimination against small firms is, of course, the prerequisite for the finding of a true small-cap anomaly. If the premium is merely compensation for the higher costs that investors pay to maintain small-firm portfolios, and after-cost returns on small firms are unexceptional, then the market is arguably efficient and the CAPM correct.

Stoll and Whaley go on to demonstrate that, over 1960–1979, if one had sold all the stocks in a small-firm portfolio at the end of each month and then bought the stocks in the newly constituted portfolio, the transaction cost (including brokerage commissions and bid-asked spreads) would have been so onerous as to reverse the small-firm effect. That is, large-firm portfolios won.<sup>17</sup> This is unsurprising because turning over the whole portfolio monthly is a profoundly irrational way for an investor facing transaction costs to behave. Apparently anticipating this criticism, Stoll and Whaley lengthen the holding period to two, three, four, six, and 12 months. They find that four months is the break-even holding period, and that the smallest-firm portfolio had positive abnormal returns after transaction costs for all holding periods of six months or longer. At a holding period of 12 months, the abnormal return is huge (4.53% per month).

While Stoll and Whaley contend that transaction costs at least partly overturn the size effect, that is not necessarily the case over the time period they studied. Even the most active investors are unlikely to turn over their whole portfolios monthly, so the reversal of the small-firm effect for investors having a one-month holding period is a straw man. Moreover, the results suggest that a buy-and-hold approach to small stock investing (with, say, rebalancing every 12 months) would have been very profitable over 1960–1979. Note that buying and holding is differ-

ent from indexing; because small-stock prices fluctuate a lot, substantial turnover is required to track changes in a size-decile index. If small-stock investing were always as profitable before transaction costs as it was over 1960–1979, investors could reap abnormal returns by holding small-firm portfolios and limiting their turnover. (We will argue later that transaction costs go a long way towards negating the small-stock effect for all practical purposes, but Stoll and Whaley did not get that far.)

Schultz (1983) points out many of these problems with Stoll and Whaley's approach, and in addition takes note of some aspects of the size effect that cannot be explained by transaction costs. Most important, the small-stock premium was sharply and persistently nega-

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tive over 1926–1931, 1946–1957, and 1984–1990. Transaction costs were obviously not negative in these periods. Moreover, as Schultz indicates, small-stock returns have been much higher in January (until recently) than in other months of the year. On a per-transaction

basis, transaction costs are not higher in January than at other times. However, there may be more transactions in January due to tax-loss selling and other rebalancing behaviors.

Roll (1983) and Blume and Stambaugh (1983) advance an argument quite similar to that of Stoll and Whaley, although they frame it (somewhat confusingly) in terms of a “bias in mean return calculation.” Roll draws a distinction between arithmetic-mean and buy-and-hold methods of computing mean returns. The arithmetic-mean method is the one used in most academic studies of the small-stock effect, and (though it is rarely admitted) assumes costless rebalancing at the same time interval as the data-sampling frequency. That is, if

actual daily stock-return data for a small stock index are averaged over time using the arithmetic-mean method, the result is an average of a series that is rebalanced regularly to the index design criteria, and thus the averages taken from this series

implicitly assume daily rebalancing. The buy-and-hold method, in contrast, assumes no rebalancing. Realistic rebalancing rules (that is, somewhere between daily and never) can be incorporated by changing the method of mean-return calculation.

Roll's empirical work shows that the size premium is cut in half (from 14.9% per year to 7.45% per year over 1963–1981) if the buy-and-hold method is used instead of the arithmetic-mean method.<sup>18</sup> Blume and Stambaugh arrive at a similar result. The explanation seems to be that it would have been impossible to transact at the closing prices used to construct the return series that implies daily rebalancing. One would have paid the ask and received the bid, and incurred commissions. Thus, the buy-and-hold method gives a more realistic estimate of the profits that can actually be earned by small-stock investing.

A 7.45% annual small-stock premium is still substantial. Moreover, Roll admits that:

*Papers with monthly returns are... much less subject to mean return estimation problems.... The well-known paper by Banz (1981) used monthly data.... Thus, it seems unlikely that the results... will be much affected by the problem investigated here. In a more recent paper, Reinganum (1983) used the buy-and-hold method and found results close to those reported above.*

However, the cost of one full turn of a small-stock portfolio has been reported at as much as 4%. Typical turnover for actively managed small-stock portfolios is around 100% (one full turn) per year (it varies greatly), so that the 7.45% annual premium may fall by more than half—and this premium was measured in a period when small stocks outperformed by a healthy margin. They do

not always do so. The effect of any realistic transaction-cost estimate on the small-stock premium is profound when turnover rates typical of active management are considered. Only an indexed or other low-turnover strategy can mitigate these costs in an effort to capture the larger portion of the premium.

Costs other than those related to transacting have received less attention from researchers, although they are no less important. Perhaps this is because data on transaction costs are more readily measured. Lustig and Leinbach (1983) sensibly assert, without present-

Ibbotson, Diermeier, and Siegel (1984) argue that investors demand and, in equilibrium, expect to receive compensation (that is, a higher before-cost expected return) for all investor costs.

ing evidence, that the cost of obtaining and updating information on small firms is differentially greater and that investors may be demanding and receiving a premium in the before-cost return to compensate them for this burden. Barry and Brown (1983) present a theoretical framework in which the impact of differential information about stocks on their returns can be evaluated, but find the data too scarce to support a strong conclusion.

An interesting discussion of all these issues can be found in Ibbotson, Diermeier, and Siegel (1984). These authors argue that investors demand and, in equilibrium, expect to receive compensation (that is, a higher before-cost expected return) for *all* investor



costs. These costs include risks (including but not limited to beta risk), transaction and information costs, taxes, and other costs not enumerated in the article. After subtracting the costs faced by the marginal investor from the market return, markets may be observed to be efficient. However, investors who can be “off the margin” with respect to these costs can earn above-market returns. In the small-stock sector, an index fund has the best chance of earning the superior returns that small stocks appear to offer on a before-cost basis, particularly for index managers who have the ability to “cross” trades internally.

Because all the investor-cost articles cited thus far are quite old, an update is in order. Wagner and Edwards (1993) point out that the cost of illiquidity is not limited to the explicit cost of trading (consisting of market impact plus commissions and/or spreads). There is also the opportunity cost of *not* trading:

*[Delay] cost [is] the price move prior to being able to trade; it can be thought of as the cost of seeking liquidity...[and]...is defined as price movements between the initial submission to the trade desk and the exposure of that order, mostly in easily digested pieces, to the broker.*

[Missed-trade] cost [is] the cost of failing to find the liquidity to complete the trade. We define opportunity cost as the price change on unexecuted shares from the time of submission to the trade desk until cancellation [by the manager, due to an excessive rise in the market price].<sup>19</sup>

From their firm’s study of various institutional portfolios, Wagner and Edwards find that the missed-trade cost for stocks over \$1 billion in market capitalization was 1.03%, while it was 3.93% for stocks under \$1 billion in market capitalization. (These amounts are per one-way trade; double them for a round turn.) This difference compares with the 1.7% spread of small-stock geometric mean returns over large stocks in the 1926–1997 period studied by Ibbotson Associates. If a small-stock portfolio is subjected to 50% turnover per year, it is legitimate to directly compare these two numbers. Extrapolating (somewhat heroically) Wagner and Edwards’ result to 1926–1997, then, more than all of the small-firm premium is confirmed by opportunity cost alone.

In an update of Wagner and Edwards’ work, Plexus Group (1998) estimates the “implementation cost”—the total of commission, impact, delay, and missed-trade costs—to be 1.01% for stocks over \$1 billion, and a towering 4.49% for stocks under \$1 billion. (Double these amounts for a round turn.) However, an indexed management style keeps turnover low, and impact costs can be minimized through internal crossing and other techniques available by exemption to certain informationless trades. Only then might the expected return premium from investing in small stocks be sufficient to overcome the cost disadvantage of trading them.

## THREAD 4

## Evidence that the small-stock premium is unreliable or does not exist

This section focuses on arguments that the small stock premium is unreliable, nonexistent, or a proxy for some other effect. We start with the observation that much of the historical small-stock premium was earned in a single period from 1974 to 1983.

### 4a Arguments that the small-stock premium derives from a single observation or one-time repricing of these stocks

One of the most damning criticisms of the idea that one can win by buying small stocks was enunciated by Jeremy Siegel (1994) in his best-selling book, *Stocks for the Long Run*:

*[I]f the period from 1974 to 1983 is eliminated, the total accumulation in small stocks falls nearly 25% below that in large stocks. Even for the long-term investor..., if you don't catch the small-stock wave, you miss the boat!<sup>20</sup>*

If the entire small-stock premium as measured over 1926 to the present can be explained by a single, continuous 10-year period (what statisticians call a single observation, even though measured over multiple periods), then one cannot rely on the premium. The best one can do is to try to time the weight of small stocks in the portfolio to correspond with upward relative moves in that sector (see [Thread 8](#)).

What would cause small stocks to experience a tremendous upward move in the 1970s and early 1980s? One explanation is that these neglected and poorly regarded “secondaries” were underpriced (see [Thread 3](#)) at the start of the period, when they sold at a P/E ratio some 50% lower than that of the S&P 500. Then, as the big investment houses became interested in this underpriced sector, they “institutionalized” the process of gathering information about these companies and made massive purchases of them, driving up prices. By 1983, small stocks were selling at a 20% P/E-ratio *premium* compared to the S&P 500. Of course, this 140% upward revaluation of the P/E multiple is insufficient

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to explain the 344% relative return on small stocks (as measured by the Ibbotson index) over 1974–1983. Earnings of smaller companies also boomed. In a period of relatively poor performance by large-corporate America, and in a political climate of deregulation, it was relatively easy for enterprising small companies (as well

as foreign companies) to chip away at oligopolies in businesses as diverse as airlines and financial services. This trend has been sharply reversed, and US industry is now about as concentrated as it has ever been. The current condition may augur well for small stocks in the near future.

Jensen, Johnson, and Mercer (1996, 1997) report a contrasting result. Constructing equally weighted rather than value-weighted portfolios, and controlling for beta so as to isolate the pure stock effect, they find small stocks outperformed in every “decade” (1963–1969, 1970–1979, 1980–1989, and 1990–1995). However, it is difficult for investors to bet on this pure factor return; equally-weighted portfolios do not stay that way, and require costly rebalancing; and if decades were defined in some other way, the premium might not be positive in all of them. We believe that Jeremy Siegel’s argument (echoed by many other authors, before and since) regarding the single-observation character of the small-stock premium casts substantial doubt on the reliability of the premium in the future—even before transaction costs.

## 4b Arguments that the small-stock effect is a really a proxy for other effects such as low P/E

The existence of a small-firm effect (whether at all times or only sometimes, and whether or not it survives transaction costs) does not mean there is necessarily something unique or

mysterious about small stocks that causes them to have high returns. Perhaps the small-firm effect is a proxy for some other effect or combination of effects.

Other than Reinganum (1981), whose work has already been reviewed, Basu (1983) was the first to seriously evaluate this possibility. Basu’s results contrast with those of Reinganum (whom he criticizes on grounds of statistical method), and imply that the P/E and size effects were closely related over 1963–1979. He writes,

*This E/P effect...is clearly significant even after experimental control was exercised over differences in firm size... On the other hand..., the size effect virtually disappears when returns are controlled for differences in risk and E/P ratios... While neither E/P nor size can be considered to cause expected returns..., most likely, both variables are just proxies for more fundamental determinants of expected returns for common stocks.<sup>21</sup>*

This opened a can of worms that could only be contained with a much larger can. More than a dozen other articles by major authors attempt to disentangle the size effect from other identified or suspected effects such as P/E, price-to-book (P/B), and neglect (that is, lack of coverage by brokerage house research staffs). We review them very briefly in chronological order

(some have already been mentioned elsewhere herein).

Cook and Rozeff (1984), in contrast to several other authors cited above, found that size and P/E are two separate effects.<sup>22</sup> Lakonishok and Shapiro (1984) contend that the size effect swamps both beta and variance (total risk) as explainers of the differences in stock returns; they did not look at P/E or other measures of “cheapness.” Downen and Bauman (1986) find that size dominates P/E, although not completely; but that size fully explains the “neglect” effect. Zivney and Thompson (1987) arrive at the startling conclusion (never since confirmed) that “a stock’s relative price ratio, the ratio of the current price to the average of the highest and lowest prices over some holding period, is a better predictor of future stock returns than firm size.” If this is true, then the small-stock effect is a proxy for cheapness and has no independent existence. Keim (1990), using a longer data period than other authors,<sup>23</sup> finds that P/E and size effects are both significant, but that the size effect disappears if one removes data from January of each year (see [Thread 7](#)).

Fama and French (1992, 1995) find that size and P/B (a kind of cheapness) independently explain returns, and they surely have not had the last word on the subject.

What we get out of all this is that size and “cheapness” are deeply intertwined. Stocks have earned excess returns because they are low-priced in many dimensions—price per unit of book value, earnings, or cash flow; price per share; price relative to their own historical price series, as in Zivney and Thompson; and price-times-shares-outstanding, which is the small-stock effect. When statistical analysis reveals that the small-cap effect is at least partially independent of other effects, that finding can be interpreted to mean that “cheap” firms (those bargain-priced by the market) can be identified to some extent by choosing the small ones. Of course, such a procedure will turn up some emerging growth companies that are not cheap by valuation measures, but these do not appear to dominate the small-firm universe.

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## THREAD 5

## Behavioral explanations for the small-stock effect

If small-cap, low P/E, and other “buy cheap” investment strategies work with any degree of consistency, then a behavioral explanation is in order. Behavioral finance is the branch of finance that rejects (or sets aside for the sake of exploration) the assumption that all economic agents are rational. Instead, behavioral finance says, investors engage in all sorts of activity that is irrational from the standpoint of net-present-value (dollar) maximization, although it may be rational in the sense of maximizing psychological utility.<sup>24</sup> Because capital-market efficiency relies on investor rationality, behavioral approaches to finance may help explain inefficiencies.

Behavioral finance has been applied to the small-stock question by several authors, of whom the most noteworthy are Lakonishok, Shleifer, and Vishny (1994). They propose that the small-stock effect is a value effect caused by the preference of investors for safe, well-known companies. As Lakonishok and his colleagues argue, a large fraction of investors has historically disdained risky, unprofitable, poorly capitalized, or otherwise troubled companies. By failing to buy these stocks for their portfolios, these investors have caused the prices of “unloved” stocks to be lower than their fair value, and (because of this low pricing) to be lower in capitalization ranking than the stocks would otherwise be. Then, as the unloved companies become loved or at least accepted in the mainstream of institutional investing, a correlated return-to-value

and return-to-small-size emerges. Many authors have speculated along these lines, but it is difficult to devise empirical tests for love and disdain, so the argument has remained mainly conceptual.

A related (and more testable) idea, supported by some empirical evidence, is that firms neglected by brokerage-house researchers have higher returns because they are initially underpriced. We have already (in [Thread 4b](#)) seen Downen and Bauman’s finding that the neglect effect is subsumed by the size effect. Beard and Sias (1997) confirm this. These works still do not address the question of whether neglect *causes* the size effect. Along with risk and transaction costs, it very well may. Today, few small firms are as neglected as they used to be—small-cap portfolios are part of practically every broker’s and money manager’s repertoire—and this may dampen the small-stock effect in the future.

Not all small stocks are value stocks. In fact, most investors probably care more about small companies for their growth potential. Emerging-growth companies, particularly in technology fields, have had exceptional performance in recent years, taking attention away from “fallen angel” and other value-oriented styles of small-cap investing. However, high earnings growth rates do not have a significant role in the research literature on the causes of historical excess returns in the small-stock universe.

## THREAD 6

## The small-stock effect as a true market anomaly

Perhaps because academic journals rarely publish articles that fail to find an explanation for something, the literature suggesting that the small-stock effect is a true anomaly or profit opportunity is relatively sparse. A couple of notable articles, however, appear to be classifiable only by invoking this thread.

The first is a review article by G. William Schwert (1983) in the *Journal of Finance* issue that focused exclusively on questions of small-stock behavior. Because of the burst of activity that immediately followed the discovery of the size effect in 1979–1981, a surprisingly large number of the explanations with which we are familiar today had already been suggested. At that time, Schwert writes, all had been found inadequate:

*The search for an explanation of this anomaly has been unsuccessful. Almost all authors of papers on the ‘size effect’ agree that it is evidence of misspecification of the [CAPM], rather than evidence of efficient capital markets. On the other hand, none of the attempts to modify the CAPM to account for taxes, transaction costs, skewness preference, and so forth have been successful at discovering the ‘missing factor’ for which size is a proxy. Thus, our understanding of the... causes of the apparently high average returns to small firms’ stocks is incomplete. It seems unlikely that the ‘size effect’ will be used to mea-*

*sure the opportunity cost of risky capital in the same way as the CAPM is used because it is hard to understand why the opportunity cost of capital should be substantially higher for small firms than for large firms.... Therefore, it is unlikely that the ‘size effect’ will be taken into account in teaching capital budgeting or performance evaluation for investment portfolios.<sup>25</sup>*

Schwert, of course, could not predict the emergence of the Fama-French three-factor model (in which size is a risk factor). By saying that the size factor would be unlikely to be used to develop cost of capital estimates in applications requiring an estimate of risk, Schwert reveals his view that the factor is unrelated to risk, and that the size effect is a market anomaly.

Writing in 1987 about the arcane topic of parameter preference,<sup>26</sup> Booth and Smith argue that, all other things being equal, investors should prefer assets which have return distributions that are skewed to the right (that is, having more outlying positive observations than predicted by the normal or lognormal distribution). Small stocks have positive skewness, while medium

Booth and Smith’s elaborate statistical test rejects the hypothesis that the small-stock effect is caused by a preference for assets with positively skewed return distributions.

and large-cap stocks have zero or negative skewness. Whether a preference for positive skewness explains the small-stock effect is a testable proposition.

Booth and Smith's elaborate statistical test<sup>27</sup> rejects the hypothesis that the small-stock effect is caused by a preference for assets with positively skewed return distributions. They write,

*This is an extremely strong conclusion for the following reasons: First, returns are measured monthly so that...problems...with nonsynchronous trading...are minimized. Second, the conclusion is in no way dependent on specification of a...market portfolio, and, thus, benchmark error in the market portfolio [or]...in the risk-free rate is not an issue. Third..., the small-firm effect persists even when January returns are excluded.... Fourth..., the finding of a small-firm effect by third-degree stochastic dominance means that no utility function that exhibits skewness preference will account for the effect. These findings strongly suggest that the resolution of the...paradox will be found in comprehensive analysis of the...institutional factors that affect asset value.*

This powerful and overlooked article, then, falls squarely into the group that advises us to look at the small-firm effect as a market inefficiency, perhaps caused by institutional behavior. Another possibility that Booth and Smith do not mention is transaction costs.

We earlier reviewed a class of articles that indicate the small-stock effect exists and is not a reward for taking risk. Banz (1981), the originating article for this topic, is the most illustrious example. These articles may be interpreted as part of the true-anomaly literature, but that would be a little misleading. Tests of the Banz (1981) type are joint tests of (1) market efficiency and (2) a *specific* model of investor behavior toward risk (say, the CAPM). Thus, one cannot reject market efficiency at the general level—and conclude that a permanent arbitrage opportunity exists—purely on the finding of a small-stock premium unrelated to risk as specified by that particular model. Maybe—likely—the behavioral model is simply inadequate to the task. To conclude that there is a permanent arbitrage, one has to find that small stocks beat other stocks fairly consistently over time, after all risks and costs have been accounted for, and for a large class of investors (not just, say, those who have a seat on the stock exchange). Such a conclusion is difficult to support in light of the accumulated evidence to date.

## THREAD 7

## Short-term timing of the small-stock effect (the January effect, etc.)

One of the most puzzling findings about small stocks is that their returns in excess of large-stock returns are usually concentrated in January of each year, and (some studies find) in the first few days of January. Such a “seasonality” is entirely inconsistent with efficient-market theory, for if a regular pattern of returns becomes known, it should be quickly eliminated by arbitrageurs. As early as 1925, Owens and Hardy had written, “Seasonal variations are impossible.... If a seasonal variation in stock prices did exist, general knowledge of its existence would put an end to it.”<sup>28</sup> Yet the January small-stock seasonality has been widely written about since 1983 at the latest, and it continues to persist. This is very odd.

Because this paper is directed to long-term investors, an explanation of the January effect and a forecast regarding its reliability or continued existence is of secondary interest. Therefore, we keep the review of the literature relatively brief.

The January effect was discovered by Keim (1983). He found that over 1963–1979, a period of high performance for small stocks, about 50% of the extra return was earned in January of each year. Moreover, 10% of the annual size effect for an average year

was earned in the first trading day of the year, and 26% was earned in the first five trading days.<sup>29</sup> Keim suggests that tax-loss selling<sup>30</sup> or the release of information may explain the effect. He also allows for the possibility that there is a data error or bias.

Reinganum (1983) tests for a tax-loss selling effect. First, he identifies “loser” small-cap stocks that are most likely to have been sold to realize tax losses. He compares the January and early-January returns on losers to those of winners, and finds that the losers did

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spring back with unusual vigor after the turn of the year. However, small-cap stocks that had been winners over the previous year, and unlikely to have been sold for tax loss reasons, also outperformed large issues in January. Thus, the tax-loss hypothesis does not explain the entire January effect.

If tax-motivated behavior helps to explain the January effect in the United States, then one should find it on different dates in countries with a different



tax year. In Australia, where the tax year ends on June 30, Brown, Keim, Kleidon, and Marsh (1983) find a substantial July premium—and a January premium which may be caused by American investors. However, the authors point out that:

*The tax-loss selling hypothesis itself relies on an absence of arbitrage from those not forced to sell a particular security for tax purposes. It seems difficult to reconcile an integrated capital market, that functions so well that a US tax-induced January seasonal shows up in even penny stocks in Australia, with simultaneous mispricing of securities to create the original US January seasonal.<sup>31</sup>*

This comment suggests that even four finance professors can occasionally force their views to bow to common sense.

Some researchers have hypothesized that the January effect is a payoff for higher risk levels in January, but the results are unconvincing.<sup>32</sup> Another generally unproductive thread associates the January effect with institutional rebalancing and “window dressing” of portfolios at year end.<sup>33</sup> Taking a different tack, Lakonishok and Smidt (1986) find that most, but not all, of the profits from a trading strategy designed to exploit the January effect for small firms would be consumed by transaction costs. They do not, however, find any evidence that transaction costs *cause* the effect to exist in the first place.

Some observers believe that the small-stock January effect has been disappearing. Addressing this question, Haugen and Jorion (1996) find that “the January effect is still going strong 17 years after its discovery.” Riepe (1997), however, finds this conclusion is applicable only to the very smallest stocks, where transaction costs are highest. Riepe writes,

*The January effect has weakened. . . . Almost no January effect occurs for many deciles [of all US stocks sorted by market capitalization] over the last four years [1994–1997]. . . . [F]or deciles 5 to 8, there is virtually no effect at all.<sup>34</sup> . . . The January effect is still there for the smallest companies.*

Institutional investors with substantial assets under management, however, will probably have a difficult time exploiting the January phenomenon going forward.

## THREAD 8

## Long-term timing of the small-stock effect

The seminal work on predicting long-term trends in the small-stock effect came later than that on other threads. Reinganum (1992) attributes the delay to a view among academics, prevalent in the wake of the efficient-market hypothesis, that predictable trends in the stock market were impossible and thus not worth looking for. Whatever the reason, little follow-up was done on Ibbotson and Sinquefeld's (1982) observation that the small-stock premium was highly autocorrelated and thus somewhat predictable. As noted earlier in this paper, Ibbotson and Sinquefeld recorded periods of small-stock over- and underperformance lasting many years, and having a large magnitude in terms of differential return. This suggests timing approaches basing next period's forecast on last period's return.

Jacobs and Levy (1989) introduced a macroeconomic model relating small-stock returns to six APT-type factors.<sup>35</sup> They isolated the "pure" returns to size; that is, the returns which remain after removing other effects such as P/E and price-per-share. This makes the forecast signals too weak to make much profit from using the signals. Then, Reinganum (1992, 1993) proposed a simple overreaction model in which small-stock underperformance in a given year is predictive of outperformance about five to six years later. The model worked remarkably well:

*[H]istory suggests that the first half of the 1990s should be a boom period for small cap stocks.... By the mid-1990s, however, this research suggests that the small-cap advantage will diminish or perhaps even turn negative.*

We thus have a true out-of-sample test, for Reinganum's article was completed before the end of 1990. Over 1991–1994, the Ibbotson Associates small-stock index rose by 42% in excess of the S&P 500 return. Then, over 1995–1997, it fell by 14% relative to the S&P 500.<sup>36</sup>

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The overreaction model was better at predicting rebounds after periods of poor small-stock performance than it was at the opposite. When small stocks are proxied by decile 2, 6, or 10 of the NYSE ranked by capitalization (where decile 1 contains the largest stocks, and decile 10 contains the smallest), a five-year period of small-stock underperformance was followed by a five-year period of outperformance *every time* over 1926–1989.<sup>37</sup> (When small stocks are proxied by other deciles, the regularity is very much there, but it is

less perfect.) However, periods of out-performance do not predict periods of underperformance nearly as accurately. For example, outperformance over 1974–1978 might be interpreted as predicting underperformance in 1979–1983. This second period was one of the best in history for small stocks, and the overreaction model would have caused investors to miss it (or, much worse, sell small stocks short during the period).

A five-year reversal is less frequent than the signals usually preferred by tactical asset allocators, although that is not to say that it would not be valuable. Macedo (1993), in contrast, has presented a model that relates small-stock relative performance to changes in macroeconomic variables, and that gives much more frequent signals.

Macedo's model identifies five economic variables—the equity risk premium, market volatility, Treasury bill yields, the trend of leading economic indicators, and exchange rate volatility—that are correlated to subsequent relative returns of the S&P 500, small-cap value stocks, and small-cap growth stocks.<sup>38</sup> The variables are said to work as follows:

*A high equity risk premium favors portfolios perceived as more risky, so small cap is indicated over large when the equity risk premium is high. After a period of high [stock market] volatility, small-cap stocks can be expected to outperform large, since small cap tends to be*

*oversold during the “flight to quality” that occurs during turbulent markets. Rising Treasury bill yields favor large cap over small cap. Small companies outperform large at the beginning of a recovery; after there is evidence of a healthier economy, such as a strong year-over-year rise in the leading indicator, large tends to outperform small. Large stocks are more leveraged to overseas earnings growth than small stocks; consequently, large stocks are penalized when the exchange rate is highly volatile.*

The model was backtested over 1981–1992, and 100% of the portfolio was allocated to the most attractive asset (from among the S&P 500, the Wilshire small value index, and the Wilshire small growth index). An allowance of 2% round-trip was made for small-cap transaction costs; for the S&P 500 the allowance was 0.5% round-trip. Forecasts were updated monthly, but allocations were changed only when the expected difference in returns exceeded transaction costs. The benchmark portfolio consisted of 50% in the small value index and 50% in the small growth index.

The results indicate a 6.0% annual return advantage to the timing model, with very modest turnover (19% annually). However, there is an element of retrofitting in this analysis. It is unlikely that the correct variables could have been identified by economic reasoning at the beginning of the period. Because the author had access

(at least subconsciously) to information about how markets behaved during the study period, a model could be constructed that was highly effective in-sample. Future researchers can determine how models such as Macedo's work out-of-sample.

Finally, Jensen, Johnson, and Mercer (1997) identify a number of candidate variables for explaining differences between small- and large-stock returns, and conclude that monetary policy is the critical factor. Specifically,

*We find significant and consistent small-firm and low price-to-book effects only during expansive monetary policy periods. In restrictive periods, neither size nor price-to-book ratio is significantly or consistently related to returns.*

However, when two closely correlated variables (in this case, real economic activity and monetary policy) are subjected to the type of analysis performed by Jensen, Johnson, and Mercer, the variable with the slightly stronger statistical effect appears to

dominate completely. As Kaplan (1997) notes, monetary policy over the past two decades has been conducted in a manner that is highly correlated with real economic activity, but the causal relationship between easy money and a growing economy is dubious. Thus, monetary policy may be an important factor in determining small-stock relative returns, or it may be serving as a proxy for economic growth.

It is worthwhile to note that, for reasons made clearer in Grinold and Kahn (1995), tactical asset allocation across large- and small-capitalization stocks is difficult. There is simply not enough cross-sectional diversification across just two investment alternatives with relatively few independent investment signals during the course of the year, causing the residual risk to be large relative to the expected alpha of the signal.<sup>39</sup> This problem is, of course, general to all problems of choice between two assets, not specific to these two particular assets.

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## THREAD 9

## The small-stock effect around the world

So far, this discussion has been focused on the United States because researchers have been motivated by the superabundance of data to study that market closely. Non-US markets, however, are just as important (more so if total market capitalization is the criterion), so investors can benefit from knowing whether the size effect applies in other countries. The literature on the small-stock effect outside the US is focused primarily on what we referred to as Thread 1—identification and measurement of the effect.

A summary of small- and large-stock returns in developed markets around the world, from Bruce and Leahy (1993), is shown in [Table 2](#).

Table 2

Annualized return differences between small and large stocks in MSCI Country indices, 1978–1991	
Country	Small minus large return (%)
Australia	-2.50
Austria	-1.00
Belgium	6.80
Denmark	-3.49
Finland	7.60
France	6.11
Germany	2.42
Hong Kong	1.49
Italy	5.57
Japan	3.93
Netherlands	-4.94
New Zealand	22.00
Singapore	1.98
Spain	2.15
Sweden	5.16
Switzerland	3.42
United Kingdom	2.53
Value-weighted non-US portfolio	3.89
United States	0.58

Source: Bruce and Leahy (1993).  
Small stocks are defined as the bottom 20% of stocks (ranked by capitalization) in each MSCI country index, and large stocks are the top 80%. Returns are in US dollars and represent capitalization-weighted portfolios.

Bruce and Leahy compare returns on the bottom 20% (sorted by market capitalization) of the stocks in the Morgan Stanley Capital International (MSCI) country indices with returns on the top 80% of the stocks in the same indices.<sup>40</sup> The comparison is over the brief<sup>41</sup> 13-year period from 1978 to 1991. Small-cap stocks outperform in 14 of the 18 countries. Excess returns of small over large issues range from a compound annual rate of 22% in New Zealand to -5% in the Netherlands. Note that portfolios are rebalanced annually and transaction costs are not deducted; because the bottom quintile of an MSCI country index is subject to considerable turnover while the top 80% has much less turnover, transaction costs would be much larger for the small-stock portfolios.

We now review academic literature on the international small-cap effect, country by country. Unfortunately, due to data limitations, the periods covered by most of the studies are even shorter than the 13 years studied by Bruce and Leahy. Thus, it may be better to generalize from US results than to draw distinctions among countries based on these short-period studies. There is no particular reason to expect that the small-cap effect should be different from country to country. Therefore, we may do as good a job at estimating it in any particular country by generalizing from what we have learned from long data series in the US as by relying on much shorter data series from that country. The standard error of the estimate from short-period studies is necessarily quite high.

## Japan

Probably the most thorough investigation of a small-stock effect in countries outside the US is that of Hamao and Ibbotson (1989). Following the method of Ibbotson and Sinquefeld, Hamao and Ibbotson measure monthly, capitalization-weighted returns on Tokyo Stock Exchange (TSE) sections I and II stocks, and on the fifth quintile of the TSE section I. (The authors describe section I as consisting of “large, mature firms” and section II as consisting of “small, young, or troubled firms.” The fifth quintile of section I corresponds more closely to the fifth quintile of the NYSE, containing a good number of fallen angels that had once been large but that became smaller.) The study covers 1971–1988. The shortness of the period and the fact that it essentially covers only rising markets creates problems in interpreting the results, but the authors had limited data with which to work.

They found that the fifth (smallest) quintile of TSE section I beat larger issues at a compound rate of 4.3%. Moreover, the yearly excess returns of small stocks are highly autocorrelated (follow long waves of good and bad performance), as in the US, and are higher in January than in other months, as in the US.

Rao, Aggarwal, and Hiraki (1992) study a slightly more recent period, and also found a “significant” size effect, as well as a seasonal anomaly. They speculated, “The fact that these anomalies behave in a fashion similar to that observed in the United States is sugges-

tive of either an integrated global capital market or the omission of common elements in the pricing process used, or both.”

## United Kingdom

In 1988, Mario Levis found a substantial size effect in the UK over 1966–1982. The smallest stocks outperformed the largest by an arithmetic mean of 6.8% per year. Interestingly, small stocks were less risky (had lower standard deviations of quarterly returns) than large stocks. Small stocks also had lower OLS betas, but these are subject to our earlier comments on infrequent trading and autocorrelation.

Only a year later, Levis (1989) commented that size effect was present but not strikingly important: “[I]nvestment strategies based on dividend yield, price-earnings ratios, and share prices appear as profitable [as], if not more [than], a strategy concentrating on firm size.... [T]here is a large degree of interdependency between all four effects.” By 1997, Jonathan Fletcher found no size effect at all. This contrasts with the work of Bruce and Leahy (1993), whose data ended in 1991.

## The Netherlands

Corhay and Rad (1993) investigated 50 firms comprising most of the capitalization of the Dutch stock market using daily returns over 1987–1992.<sup>42</sup> While their work focuses on the effect of return-measurement frequency on measured beta in the style of Stoll and Whaley, they noted that “the size effect is reduced when the interval length is

increased, although it remains statistically significant.” The contrast between this finding and Bruce and Leahy’s observation of a *negative* size effect in the Netherlands over 1978–1991 shows that any such short period study is very sensitive to the choice of a data period, which is consistent with our expectation based on statistical notions.

### Canada

Calvet and Lefoll (1989) and Elfakhani (1993) both found a small-stock effect in Canada, although the conclusions in the first article are hedged. Calvet and Lefoll find a size effect using the CAPM over 1963–1982, but “[w]ith a multi-parameter model..., size is no longer priced.... Non-systematic risk and size appear highly correlated, and we cannot reject the hypothesis that the non-systematic risk could be, at least in part, behind the size effect.” Elfakhani, who found that P/E, beta, and size effects are all related, assures us that “[t]he results show support for the firm size effect, even after proper adjustment for risk.”

### Mexico

Over the extremely brief period 1987–1992, Mexican small stocks outperformed larger issues, according to Herrera and Lockwood (1994). In addition, there was a beta effect (high-beta firms had higher returns), and beta and size were priced separately.

### Korea

Lee and Chang (1988) found that the firm-size effect and a January effect exist in Korean stock-market data collected over January 1976 to June 1985,

even after adjusting for biases suggested by various hypotheses. Cheung, Leung, and Wong (1994), using data from 1982–1988, reported similar results and, in addition, a P/E effect. The Korean market was completely closed to foreign investors for the duration of these studies, so these findings have limited application to the current investment environment.

### Various emerging markets

The International Finance Corporation’s Emerging Markets Data Base contains stock-by-stock return data on 20 developing markets, with differing starting dates for different countries. Studying their returns, Claessens, Dasgupta, and Glen (1995) find “limited evidence” of small-firm and turn-of-the-year effects.

### International small stocks: conclusion

In none of the countries studied do the data approach the 70-plus years of the US studies. However, taken as a whole, the non-US results tend toward the same conclusion as the US results; namely, that small stocks outperform large ones before adjusting for risks and costs. It is probably safe to generalize that the “first principles” that govern the relative performance of small- and large-capitalization stocks within the US probably also govern them similarly outside the US.

## Summary and conclusions

Having toiled through such a large quantity of material, the reader no doubt expects some wisdom to emerge to provide guidance for investment decisions. What strikes us most vividly, however, is the lack of agreement among researchers on many of the most basic concepts: whether the small-stock effect exists, whether it was a one-time event, whether it is a payoff for risks and investor costs, and so forth.

Obviously, the attraction to small-capitalization stocks arises because they have historically offered a gross return higher than that of large stocks by an amount sufficient to generate substantial and ongoing investor interest. However, there are several reasons why one should be cautious with respect to actually being able to achieve large exceptional small-cap results in a portfolio:

- More than all of the historical excess return was earned in one ten-year period, 1974–1983, under conditions unlikely to be repeated.
- To the extent small stocks have earned higher returns, the premium is at least partly a payoff for taking more beta risk, and likely for taking other risks that are not captured by the beta (but that are potentially identified by Arbitrage Pricing Theory). Thus, the premium is smaller than it first appears or may not exist at all.
- Transaction and information costs, which are likely to be higher for small stocks, have not been subtracted from the returns of either the small-

large-stock indices. These costs also substantially diminish the achievable small-capitalization premium in practice.

So what guidance comes out of all this? Let's look at a few issues:

### Active versus Passive

The active versus passive decision for small stocks tilts in favor of passive. Index funds and other low-turnover strategies will avoid many of the transaction costs that are blamed for diminishing the small-capitalization premium in practice, giving this category of funds a better return rate with respect to the premium than active funds. Further, those index-fund investment management firms having the legal ability to cross trades internally (certain bank collective funds having regulatory approval) will avoid most transaction costs entirely, maximizing the achievement of whatever small-capitalization premium might be available.

What strikes us most vividly is the lack of agreement among researchers on many of the most basic concepts: whether the small-stock effect exists, whether it was a one-time event, whether it is a payoff for risks and investor costs, and so forth.

The active versus passive decision is best made using recently developed approaches for optimizing manager structure, such that the expected alpha is maximized at some acceptable level of residual risk (tracking error). In such



an approach, described by Waring and Castille (1998), active and passive are balanced according to their relative contributions to the expected alpha (after fees and costs) and the residual risk of the portfolio. The substantial cost advantage of passive over active in the small-capitalization sector means that an active management firm has a higher hurdle to get over before successfully persuading the optimizer that it should be held in the portfolio based on its net expected alpha. While we believe there are active small-capitalization managers that have positive expected alphas, the proportion of the manager population that is in that category in the expectancy is probably smaller in the small-capitalization sector than in other sectors.

As always, active managers should never be used merely because they are active, but only if they are expected to create a worthwhile positive alpha after fees and costs. Investors who lack confidence in their ability to identify investment managers with strong positive net alphas will improve their results by allocating more to passive management.

### **Are small stocks an asset class?**

How investors allocate between small and large stocks depends, first, on whether one views small stocks as a true asset class or, more modestly, a *style* of equity investing.

Ideally, an asset class is a set of securities that are highly correlated with each other, but that are less high-

ly correlated with securities not in the set. Of course, no asset class in real life fits this description neatly.

Small stocks certainly met this stringent qualification for an asset class over the historical period studied by Banz, Reinganum, and Ibbotson and Sinquefeld. Acting under these authors' influence, many investors adopted a "barbell" approach that involved buying the largest and the smallest stocks (the latter held in a proportion larger than their market capitalization), to accentuate the small-cap effect (Table 1, p. 5).

This is still the most common configuration. On the active side, moreover, small-stock managers are thought to have skills and industry contacts different from those of large-stock managers, so that a manager search is done separately for the two categories.

As institutions have brought small stocks into the investment mainstream, these securities have become steadily less different from other stocks, giving rise to the contention that they form an investment style within the asset class of all US stocks, rather than a separate asset class. If this is the appropriate way to look at small stocks, then market-cap weights should be held (at least as a base case) across the whole range of capitalization from smallest to largest. A substantial minority of institutional investors acts on this view, using a broad equity benchmark such as the Wilshire 5000 or Russell 3000 indices.

Which decision framework—small stocks as asset class or style—should be used? Certainly the very different return, risk, and correlation behavior of small stocks, and especially the high autocorrelation of that sector, lend support to the asset-class view. The return premium may be less than once thought, but small stocks remain an effective diversifier.

On the other hand, there is a continuum between small, mid-cap and large stocks, and many managers select from these sectors at will, blurring the distinctions among them. Moreover, the smaller one believes the small-stock premium to be, the more justified one is in regarding all capitalizations as belonging to the same asset class. At any rate, it is not a matter of great importance whether one regards small stocks as an asset class or style, because any resulting misallocation is likely to be minor if the small-cap premium is kept at modest levels.

#### **Expected return assumptions for strategic asset allocation**

To the extent that the premium for holding small-capitalization stocks is smaller than indicated in the raw data, the expected return premia of 3% to 6% sometimes used in asset allocation studies are too high. Much smaller premia, perhaps in the range of 0.5% to 2%, are more consistent with the research. A true small-capitalization premium skeptic might well put the premium at zero. However, some premium should probably be included

particularly for investors using index funds or other low-turnover investment strategies in the small-capitalization sector.

#### **International small stocks**

Anything an investor might do with US small stocks, including holding

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broad capitalization exposure, is probably equally sensible in the international arena. International small-cap investment products are not available in wide variety. (MSCI only recently established a developed-country, small-capitalization benchmark; Salomon Smith Barney's benchmark, the oldest in existence, is reconstructed only to 1989.) However, the category is becoming investable as more investment managers provide usable products designed to serve this sector.

#### **Market timing and tactical asset allocation**

The research results from both tactical asset allocation and market-timing approaches are encouraging at first glance. While the return advantage of small stocks over large ones is modest on average over time, it is very sub-

stantial when small stocks are in an up trend, and investors may be able to take advantage of this. Of course, the perfection of the five-year down-to-up reversal signal for some deciles must be regarded as a coincidence. However, overreaction in the relative valuation of small versus large stocks appears to be a fact, and long-term tactical asset allocators can act on this information. With three years of small-stock underperformance behind us, it is possible that investors will soon have an opportunity to profitably increase their allocations to small stocks.

A more aggressive market-timing approach is fruitful in a back-test, but investors should make a realistic allowance for trading costs, and arrangements for their minimization must be made before deciding to transact. Because the superior return of small-capitalization stocks over large-capitalization stocks, and vice versa, appears to run in long secular waves, there is a strong need to be correct when the calls are (infrequently) made. In this situation, residual risk can be cumulatively bad as well as cumulatively good.

Any timing approach based on these ideas should probably be just one aspect of a more complete tactical asset allocation framework. With more asset classes, acceptable signal-to-noise ratios (information ratios) can be generated because there is adequate cross-sectional diversification.

#### **Final word**

Small-capitalization stocks should definitely be held. The investor, however, should probably not expect to achieve the same small-capitalization premium in the future as the index returns suggest might have been achieved in the past. Regardless of the size of the premium, there are clear diversification gains to be had. At a minimum, small-capitalization stocks should be held in a weight proportionate to their representation in the market portfolio; modest levels above that remain defensible. But the allocations justified by risk premia of 2% or more, which have grown to be common in recent years, are likely to be overly aggressive for strategic asset allocation.

Small-capitalization stocks should definitely be held. The investor, however, should probably not expect to achieve the same small-capitalization premium in the future as the index returns suggest might have been achieved in the past.

## Endnotes

1 See, for example, Owens and Hardy (1925).

2 An anomaly with respect to a theory is an occurrence in nature (or in the economy) that is contrary to what the theory predicts. For example, if it is possible to make money (earn excess risk-adjusted returns) consistently by buying a particular set of stocks, then that sector of the market is not efficient, and that set of stocks is considered to be anomalous with respect to the efficient market theory.

3 The CAPM is true if one accepts the assumptions required to derive it, but many of these assumptions are unrealistic in practice.

4 Like Banz, Reinganum attributes the discovery of the P/E effect to Basu (1977). Properly, our review of Reinganum (1981a) belongs in the thread (4b) on disentangling the various effects, but because of the influence that Reinganum's article had in establishing the existence of a small-stock effect, we cover it here instead. Reinganum (1981b) is a précis of the parts of Reinganum (1981a) that deal with the small-firm effect, and is written in practitioner's language.

Reinganum's claim to be the co-discoverer of the small-firm effect would be more widely accepted if he had not generously referred to Banz's dissertation in a footnote, and if his paper had been more clearly focused on demonstrating the existence of a small-firm effect rather than on separating it from a purported price/earnings anomaly.

5 Data in this paragraph are from Ibbotson Associates (1998), in which the small-stock index is proxied by the actual return of the Dimensional Fund Advisors (DFA) Small Company 9/10 (for ninth and tenth deciles) fund. The fund is composed of American Stock Exchange and NASDAQ stocks as well as ninth- and tenth-decile NYSE stocks. To reduce trading costs, the fund is managed somewhat differently from a pure index fund.

6 Moreover, Ibbotson Associates uses names for the sectors—mid-cap (NYSE deciles 3-5), low-cap (deciles 6-8), and micro-cap (deciles 9-10)—that correspond more closely to current portfolio management practice.

7 Counting nonnegative, rather than strictly positive, changes in arithmetic-mean return; that is, the fifth and sixth decile have the same return, so the monotonic relation holds.

8 The ordinary-least-squares regression technique produces a line that "fits" a scatter-plot of data so as to minimize the sum of the squares of the distances of the data points above and below the line. When the term "regression" is used without modification in economic or financial literature, OLS regression is usually meant.

9 Autocorrelation is sometimes called serial correlation or serial dependence. It is measured by calculating the correlation of a series of returns with the same series "lagged" (time-shifted) by one or more periods.

10 We wish they had captured more of the autocorrelation risk. The risk of repeated underperformance can develop over many years, while Ibbotson, Kaplan, and Peterson's (1997) method examines only the one-month lag.

11 To overturn the CAPM one would have to find that there was an expected return (not just a realized one) associated with the non-beta factor. Whether Fama and French (1992) found an expected return, as they claimed, or just a realized return is the subject of amusing debate (see Davis [1994]).

12 APT asserts, contrary to the CAPM, that there can be multiple risk factors that are "priced" in the sense that investors can expect higher returns if they take these risks. Some critics of APT contend that it merely breaks up beta (CAPM) risk into its constituent parts, and that APT therefore offers nothing new. We tend to regard APT as having substance, and if the cross-section of expected stock returns can be better explained with APT factors than with the CAPM beta, then the CAPM has been shown to be incomplete.

13 Over this period, the market return in excess of Treasury bills was substantially positive, so that if the CAPM is correct then beta risk should have been rewarded.

14 Data snooping is the practice of looking at all the variables that might conceivably explain a particular phenomenon and choosing the ones that provide the best statistical fit. When the variables are themselves related (for example, P/E, price-to-book, size, beta, and price-per-share), classical measures of statistical significance will overstate the true economic significance of the variables that "win" in this process of elimination.

15 While this might seem like a natural place to review the rest of the literature on disentangling size, low P/E, and other non-beta effects (or effects appearing to be non-beta effects), this discussion is deferred to Thread 4b, which deals with the possibility that the size effect is a proxy for other attributes that convey "cheapness." The question is important because, if the size and (for example) low P/E effect are one and the same, the investor can earn small-stock-like profits by buying low P/E large stocks.

16 "Unjustified": our emphasis.

17 The portfolios formed by Stoll and Whaley are "arbitrage" portfolios that hold a long position in an equally-weighted portfolio representing a particular decile of the NYSE ranked by market capitalization, and a short position in the equally-weighted NYSE index. This wrinkle does not change the interpretation of their findings.

18 Roll does not form size fractiles. Instead, to proxy the small-firm effect, he compares returns on the AMEX index with those of the NYSE, which lists much larger stocks on average.

19 Delay cost is called timing cost in the original; and missed-trade cost is called opportunity cost in the original. The new wording is from the Plexus Group's (1998) update of Wagner and Edwards. Both types of costs are opportunity costs in the ordinary sense, necessitating a change in the wording.

20 Emphasis on "below" and exclamation point in the original.

21 The E/P effect is the same as the P/E effect (because the P/E ratio is the inverse of the E/P ratio). Because stocks do not sell at zero or negative prices, the E/P ratio is more amenable to mathematical analysis.

22 This view, while unconfirmed by most researchers before or since, survives in consultants' "style maps" that show size and valuation to be separate effects.

23 Earnings data are less easily obtained for distant historical periods than price, return, and market-capitalization data.

24 A primer on behavioral finance can be found in Wood (1995).

25 "Causes": our emphasis.

26 The first four "parameters" or "moments" of a distribution of observations are (1) mean, (2) variance, (3) skewness, and (4) kurtosis. The discussion up to this point has been concerned with mean and various measures (including standard deviation, beta and correlation) that are related to variance.

27 Too complex to describe here.

28 Owens and Hardy (1925), quoted in Riepe (1997).

29 In a later paper, Keim (1987) also notes that 63% of the small-stock premium was realized on Fridays.

30 Tax-loss selling is the selling of securities by taxable investors at a price below the investor's cost, to realize a loss that can be offset against realized gains in other securities for the purpose of calculating income subject to personal income tax. This practice is widespread in non-US markets as well as in the United States.

31 Emphasis on "seasonal" in the original.

32 See Rogalski and Tinic (1986).

33 Window dressing refers to the practice of money managers selling stocks perceived to be risky or undesirable from the client's point of view, and replacing them with safe stocks, just before portfolio contents are reported to the client at quarter end or year end. This could contribute to year-end underpricing of small-capitalization issues.

34 As Riepe notes, deciles 6 to 8 correspond roughly to the Russell 2000 Index.

35 Unexpected changes in (1) the Baa corporate bond rate, (2) the Treasury bond rate, (3) the Treasury bill rate, (4) the S&P 500 return, (5) the Consumer Price Index, and (6) industrial production.

36 There is a hidden bias here, which is that we would probably not have selected Reinganum's articles for review if his out-of-sample predictions turned out to be wrong.

37 The five-year periods include overlapping years; that is, underperformance in 1969-1973 and in 1970-1974 count as separate observations.

38 While Macedo's explanatory variables resemble those of Jacobs and Levy (1989), her model provides stronger forecasts because she is forecasting the

"naive" returns to size (that is, the difference between small- and large-stock returns) while Jacobs and Levy are forecasting the "pure" returns to size (with the effect of all related factors, such as P/E, price-per-share, earnings surprise, etc. removed).

39 Cross-sectional diversification is the extent to which different asset classes have different returns in the same time period.

40 The smallest stocks in these markets are not in the MSCI indices at all; as Bruce and Leahy note, the Salomon Extended Market Indices must be used to access these returns. However, the distinction between bottom-quintile MSCI stocks and the top 80% is sharp enough to test the small-stock effect with reasonable accuracy. Nevertheless, MSCI indices need to be evaluated for bias. For each country, MSCI creates a stratified sample of stocks, diversified by industry, that sums to approximately 60% of the total capitalization of the country's market.

41 By comparison, the Ibbotson data for the US now spans 72 years, a much more robust statistical sample.

42 To be exact, they studied data from October 28, 1987 to March 1, 1992.

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