Distilling the information in S&P500 delistings

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ABSTRACT

We examine price movements around the removal of seven foreign firms from the S&P 500 Index in July, 2002. Unlike other deletions from the S&P 500, because these firms were removed solely due to their nationality, any information effect that is normally present will not be present now. This allows us to isolate the information effect, and casts a shadow on some previous tests of demand curve slopes. Consistent with this theory we find that foreign firms experience a smaller decrease over several different time windows than control firms. This supports an information effect in the S&P 500 index.

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1 I would like to thank seminar participants at UBC for comments and suggestions, and especially Kai Li for excellent support and guidance. All errors remain my own.
I. Introduction

It is a well accepted fact in the literature and in practice that when a company is added to the S&P 500 index, there will be an immediate price reaction. There is, however, some disagreement on whether or not this effect is short term or permanent, though on balance it now seems accepted that at least some of this effect is permanent. This seeming anomaly has been broken down in many ways, but until now there has been no way of directly testing some effects. On the opposite side when companies are removed there is a corresponding decrease in price.

There are several different theories that have been put forward to explain this phenomenon.

The price pressure hypothesis says that when a stock is added to the index, there is a large short-term demand from index funds. This requires them to pay a premium to compensate sellers for selling at an earlier time than they might have wanted.

The downward sloping demand curve hypothesis says that since stocks are not perfect substitutes for each other, an increase in demand (for example from index funds in this case) will also cause an increase in price, all else being equal.

The liquidity hypothesis says that since the liquidity of a stock increases after addition to the index, people are willing to pay a premium for the stock.
Finally, the *information* hypothesis says that there is new beneficial information revealed by inclusion the S&P 500 index. It is this hypothesis that we will focus on in this paper.

Of course these hypotheses are not mutually exclusive. It is likely that each one occurs to varying degrees. The problem has been that it can be difficult to disentangle all of these effects. Many authors have had to assume that one or more hypotheses are false in order to test another. It is only when we get the chance of a natural experiment like the removal of foreign firms studied here that we can isolate a single effect. Since these seven firms under study were removed solely to make the index officially “American”, we argue that the information effect that may be present in normal deletions will not exist in these firms. All of the other effects mentioned above should, however, remain the same, thus any difference between the control sample and the foreign firms will be due to the information effect.

The primary goal of this paper is to determine whether or not there is an information effect in S&P stock delistings, and how important this is relative to other effects. A second contribution of this paper will be to apply the Fama-French 3 factor model to the problem using daily frequency portfolios, and to adjust for risk using industry matched portfolios and stock-matches based on size and industry. All papers to date have used raw returns or market based models. We also contribute by studying in detail a unique event in recent markets where a major rebalancing has occurred for apparently exogenous
reasons. These changes in policy by S&P affect almost all institutional investors, indexers and mutual fund managers who are measured against the popular index.

The remainder of the paper is organized as follows: Section II discusses previous studies in all areas of the relevant literature to date. Section III describes the method for stock selection in the S&P 500, the event we want to study and the method for selecting our control sample. Section IV reports and analyses the results. Section V concludes.

II. Previous literature

The price-pressure hypothesis says that when a stock is added to the index, there is a large short-term demand from index funds. Index funds have grown immensely since Vanguard started its first index fund, the Vanguard 500 Index Fund, in 1976. It is currently estimated that directly indexed assets to the S&P 500 (excluding “closet” indexers) is in excess of US$1 trillion <put a source here>. Since index fund managers are concerned only with tracking error, and not outperformance of the index, they are forced to buy new stocks as close to the inclusion date as possible. Consistent with this hypothesis, Harris and Gurel (1986) find that there is a short-term rise in price after inclusion which is reversed within 30 days. Unfortunately the reversal has been countered by studies conducted by Beneish and Whaley (1996), Dhillon and Johnson (1991), Lynch and Mendenhall (1997), and Shleifer (1986) who all find that while some of the price effect is reversed there is still a permanent price increase associated with inclusion in the index. [Need to write more about these original papers]
A new spin on the price-pressure hypothesis is proposed in a recent paper that looks at a new argument to explain the price swings – taxes. If the price is pushed upward on index inclusion in order to compensate investors for selling now instead of later, then it is reasonable to expect that if an investor also has to trigger capital gains taxes on this sale, the price pressure will be even higher. Blouin, Raedy and Shackelford (2000) examine this effect through the relationship between price appreciation on index inclusion and expected tax effect (this is a combination of recent price activity and the difference between long and short term capital gains taxes). The authors find that there is, as expected, a positive effect relating incremental taxes and price movement. The authors conjecture that this accounts for 0.5% of the daily increase observed. There are several issues with the estimation of the amount which the authors point out. For example although much of the press that indexers get come from such mutual funds as Vanguard or from ETFs like SPDRs. Anecdotal evidence suggests, however, that there is at least as much indexing occurring in institutional portfolios. Many of these such as pension funds and endowment funds are tax-free, and so would not be affected by capital gains taxes.\footnote{As the authors point out the marginal retail investor, through taxable mutual funds, would still have an effect. However, it is not clear that mutual fund managers would worry about the tax situation as much as the end investor.}

The first theory consistent with this permanent price increase is the imperfect substitutes hypothesis of Shleifer (1986) and Scholes (1972). Under the assumption of perfect frictionless markets, demand functions for stocks are perfectly elastic. So if a stock is priced $0.01 above (below) fair value, investors will sell (buy) an infinite amount of it, and can then form a portfolio of assets that has the exact same risk and return...
characteristics to earn an arbitrage profit. Unfortunately markets aren’t perfect and this arbitrage profit is not always available. This means that demand curves will actually slope downwards for stocks, and when a stock is added to the index, the demand curve gets shifted to the right, and thus the equilibrium price increases. Wurgler and Zhuravskaya (2002) look in detail at some of these imperfections in a way that was not possible when they were first suggested. They create portfolios of stocks that are near, though not exact matches of the stocks under investigation to measure how well they might be arbitraged. Surprisingly, even for the median stock in the sample, only a quarter of the daily variation can be explained. This effect is far more notable for large stocks than small stocks. They find that price increases on inclusion are increasing in the ease of arbitraging, strongly supporting the contention that at least some of the price jump is due to a shifting demand curve.

One element that all other studies of this topic have in common is the use of closing price data. Beneish and Whaley (1997) take a new tact by using intraday data. They examine whether or not there are still profits to be made by “playing the S&P game”, or trying to profit from index changes at the expense of inflexible index funds. The good news is the primary conclusion that the profits available have decreased since 1989 as more and more people become aware of the opportunity, even when allowing for the also significant effect of the number of days between announcement and effective dates. Although it isn’t mentioned, this effect has to combat the opposite force of the rapid increase in indexing activity. So even though there are more people (and presumably
more money) chasing the “arbitrage” profits\(^3\), the balance is towards decreasing profits. Also interesting to us is the new intraday data and methodology employed. Since all the information is released the night before it can be acted upon then one would expect most of the price effect to occur overnight rather than spread out through the first day or over the period before the index change actually occurs. Unfortunately with daily close prices it is impossible to test this. To separate the overnight effect from the rest of the 1\(^{st}\) day effect the authors look at the price change from the close the previous night to the opening trade. One new issue is that the S&P index is not directly tradable on the open. Each stock is opened at a different time, so that there is no official index level to compare with. To overcome this problem the authors use the S&P futures as a benchmark. So to determine the profit available from buying companies added to the index they take a long position in the stock, and a corresponding short position in the future. Although this ignores the basis of the futures contract, the strategy *is* tradable, and the basis effect is likely negligible. They find that this overnight effect is 3.06%. This return is, however, not tradable since the markets are closed\(^4\). The abnormal return from this first trade until the close of the first day is 3.70%. This return, however, is available to quick traders.

Liquidity is a relatively new aspect in the finance literature that can move asset prices. If a stock is easily traded in large size at a reasonable price around the bid-ask then an investor might rationally be willing to pay a premium for the stock. Hegde and

\(^3\) Technically this is not an arbitrage profit, though it is often referred to as such. As we will see a few companies do not follow the trend except on the 1\(^{st}\) day after announcement where every company decreases as expected.

\(^4\) Over recent years after hours trading has been a fast growing, though still illiquid option. Some of these companies are also traded on foreign markets. Both of these alternative trading methods should also be examined.
McDermott (2000) test for liquidity changes around stocks being added to the S&P 500. They document a permanent increase in liquidity measured by decreased effective spreads, increased quoted depth, frequency, as well as the well documented increase in volume. Because much of the liquidity demand is due to index funds, there is some concern about time effects. Indexing began in the early 70s, but started its exponential growth much sooner. We will only be looking at data from 1990 onwards, but we also need to look at how the effects change over time. [Need much more here]

The final explanation and the one we will focus on in this paper is the information effect. As Harris and Gurel (1986) state before arguing against an information effect: “The no-information assertion is essential to any interpretation of the post-announcement price change”. Does S&P have some skill in picking companies using public information that others do not have? There are, as in any interesting debate, arguments on both sides. Unfortunately until very recently no one has formally tested the thesis. There are several convincing arguments against any information effect. The most obvious is that there should be no extra returns that can be generated using publicly available information beyond the cost of trading and acquiring the information. The companies should already be fairly priced. A recent study by Denis, McConnell, Ovtchinnikov and Yu (2002) examines the relationship between earnings expectations, realizations and inclusion in the S&P 500. They discover by looking at matched firms that are and aren’t included in the index that earnings expectations increase after inclusion in the index, as do actual earnings. The authors also test the Granger causality of the event. There are two possibilities. One is that addition to the index actually improves either the quality of the
firm, perhaps by making its name known and acceptable to more potential clients\textsuperscript{5}. Also the extra information generated by more analyst coverage (which is confirmed by Denis et al.) and more managers looking at the firm will tend to increase the price regardless of the average expectations. The opposite possibility, and the one that we will study here is that S&P has some ability or skill in picking higher quality firms to be included (or lower quality firms to be excluded). One reasonable reason to expect that they may have superior skill in picking firms, or even possibly non-public information, is that S&P is one of the biggest bond-rating firms in the world, and is likely to have rated the debt of any firm eligible for inclusion in the index. Information from that process may well be used for creating index products.

While this information effect is interesting in its own right, it is key assumption used when estimating whether or not stocks have downwards sloping demand curves. In normal day to day market watching, it is not possible to tell whether the demand is perfectly elastic or not. Previous studies mentioned above have used initial and secondary equity offerings and S&P additions to make this test\textsuperscript{6}. Unfortunately they all assume that there is no information effect at all provided. One paper which does not do this is Kaul et al. (2000). They examine another natural experiment which occurred in 1996 when the weights in the TSE 300 were rejigged after the change in the definition of float. In this case the event was advertised many months in advance, so there was no actual information in the event. On the day of the re-weighting the 31 stocks which had

\textsuperscript{5} Shaw is an example of a local company that uses index inclusion in its marketing material

\textsuperscript{6} There are several papers that deal with block trades, SEOs and IPOs, including Scholes, for example. Is it worth adding an entire section about this? I think it is too far off the track, but might be needed for completeness.
increased weights experienced excess returns of 2.3%. This was not merely a liquidity effect since the gains were not reversed even after the volume had returned to normal.

Liu (2000) also examines a foreign market, this time the Nikkei 500. He studies the price and volume effects around additions and deletions of 92 (86 deletions) to the Nikkei 500 between 1991 and 1999. He finds that while similar effects are found in the Japanese market, the increases (decreases) are not as large (negative) in price are not as large as previous results in the S&P 500. He also finds that the price effect is permanent, with no significant reversals, although volumes seem to return quickly to normal. The author argues that these findings can only support the downward sloping demand curve theory, but is not consistent with the other 3 theories that are normally considered. There are, however, some important differences between the sample used in this paper, and others studying the S&P 500. Firstly, the amounts indexed to the Nikkei are very small compared to the S&P. This may account for the lower effect. Secondly, there is a lot of clustering involved in the dataset. Only approximate methods are used in the estimation procedure. Since almost all of the changes occur around annual reviews there will be a strong correlation between many of the stocks. Also, more than 90% of the additions occur in just 3 of the 9 years, and 3 years have no changes at all. This, combined with the downward trending market make comparison with American experience difficult. Finally, despite the claim that the results support a lack of information effect, the effect is almost surely different in the US. Part of the reason for suspecting an information effect is that Standard and Poor’s has a reputation that it leverages around the world. They are the leading bond rating service and thus would be expected to have superior knowledge
about firms. Also, S&P states that financial viability and low turnover are explicit goals of the index composition. For this reason we hope to determine whether or not there is an information effect in the S&P index deletions.

III. Data

III.A S&P 500 Index construction

Companies are only added to the S&P indices when another company needs to be removed. Firms are removed primarily due to mergers, acquisition or a restructuring that significantly alters the company and makes it no longer suitable including spin-offs\(^7\). Some firms are also removed when they no longer meet the criteria mentioned below under additions in some significant way. Although some firms may seem to “temporarily” be unsuitable, this does not automatically trigger a removal.

S&P has very specific criteria for firms to be added, though it seems that these change over time. The index committee at S&P maintains a pool of companies that are considered to be suitable for inclusion. This list is updated periodically and when a company needs to be removed, a replacement can be chosen quickly. The selection criteria for inclusion are:

1. The company must be American (this was first added at the time of the event under study in this paper).

\(^7\) Bankruptcy, though rare for S&P companies, would count as a restructuring.
2. Liquidity and price. The current liquidity definition states that the dollar volume traded per year must be at least 30% of the market cap. Low dollar value priced companies are not eligible.

3. Market capitalization. The current threshold for the S&P 500 is $3 billion. It must also have a traded float of at least 50% of the company’s equity.

4. Sector balance. S&P endeavour to have its index match the performance of the economy in general as well as the stock market.

5. Operating company. Holding companies, partnerships, closed-end funds, and royalty trusts (except REITs) are not eligible.

6. Financial viability

It is this last point specifically that those supporters of an information effect believe is most important. In their latest report on the topic, S&P states that financial viability means 4 quarters of consecutive positive net earnings. However as Beneish and Whaley (1996) suggest “the firm’s financial and operating conditions are rigorously analyzed to ensure that added firms will have longevity”. This view would also be supported by S&P’s belief “that unnecessary and excessive turnover in index membership should be avoided when possible”. The index committee would thus understandably prefer companies that had good prospects to avoid needing to remove them later.

Prior to October, 1989, S&P announced any changes to the S&P index one evening, and then the changes became effective the next morning. However, in order to alleviate some of the imbalances they decided to pre-announce changes. The standard
policy is to announce the changes 5 days in advance (the mode of the distribution charted below). Circumstances do not allow this policy to always be followed, however. As can be seen from the chart below, while approximately half of the companies do have a 5 day window, there is a range from 1 to 19 days.

![Bar chart showing number of days between announcement and actual removal of a company from the S&P 500. Includes 7 foreign firms.](chart.jpg)

**Figure 1 - Number of days between announcement and actual removal of a company from the S&P 500.** Although the stated target since 1989 is to give 5 trading days notice, this is not always possible or desirable.

III.B So what is the event we are looking at and why is it useful?

On July 19, 2002 Standard and Poor’s announced that seven foreign firms would be removed from the S&P 500 index. The 5 Canadian firms were Alcan, Barrick Corp, Inco, Nortel Networks and Placer Dome. The 2 Dutch firms were Unilever and Royal
Dutch Shell. S&P made the decision to ensure that the index was a proper representation of American firms. All of the firms are still members of their respective country’s S&P index, and of the S&P 1200 World index. There do exist some index funds following these indices, especially the Canadian and Dutch country indices. However, the demand by American index funds, and the assets benchmarked to the S&P 500 dwarfs these amounts and is likely to be the dominant effect.

This event provides perhaps a unique chance to test once and for all whether or not S&P 500 inclusion provides information about the companies it chooses. Although the information hypothesis is interesting in its own right, it is also important for tests of the whether or not the demand curve for stocks is downward sloping. A fundamental assumption of many of the tests that use the S&P 500 index listing and delisting as a control event is that there is no information in the event. If there is, then these effects will generally be confounded and there will be no way of disentangling them. This natural experiment, however, gives us exactly that opportunity. [Spell out the testing strategy here]

III.C Sample Selection

We obtained the names and dates of exclusion from two primary sources. For 1979-2000 we used the data provided by J. Wurgler on his website\(^8\). From 1993-2002 we received dates and names from Sakrit Dash at Standard and Poor’s research. Both of these sources provided a list of companies to search on. For deletions from 1994

\(^8\) http://pages.stern.nyu.edu/~jwurgler/
onwards, we searched the Lexis-Nexis for Standard and Poor’s daily notices for announcements of any changes to the S&P 500. Any companies that were removed from the index due to merger, acquisition, spin-off or bankruptcy were removed from the sample. There is clearly confounding information in these announcements, and in most cases the company is no longer trading after the effective date. This left us with 68 companies for our control sample, plus the 7 firms that we are investigating. Figure 2 - Number of companies in total sample (control and foreign firms combined) broken out by year of announcement

Price and volume information for all companies and indices was obtained from the Centre for Research in Security Prices (CRSP). Portfolio returns for the Fama-French factors were obtained from K. French’s website.

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9 Future versions of the paper will do two things to complete the data. In order to have a consistent membership process throughout, we will focus only on removals post-1989. Also, we will complete the pre-1994 data and add the 2002 data which has only just become available. This is why there are sometimes inconsistent firm numbers in the control sample.

10 http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html
IV Results

IV.A Some summary data on the foreign firms removed

Unlike other deletions in the past (with the exception of the AT&T break-up in 1983), S&P provided a great deal of information about the companies that were removed as part of a 23 page press release on the subject. We argue that the great trouble they went to with these companies supports the idea that unlike the control sample companies that were removed, there is no adverse information in this event. Following are some items of interest:

Table 1 - Background data on 7 foreign firms studied. On July 19, 2002, S&P announced that it would be removing all foreign firms from the S&P 500 index in order to make it more representative of the American economy and a better benchmark for American investors. The following 7 companies were removed and form the basis for the natural experiment being studied in this paper.

<table>
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<th>Company Name</th>
<th>Royal Dutch</th>
<th>Unilever</th>
<th>Nortel Networks</th>
<th>Alcan Inc</th>
<th>Barrick Gold</th>
<th>Placer Dome</th>
<th>Inco, Ltd</th>
</tr>
</thead>
<tbody>
<tr>
<td>Ticker</td>
<td>RD</td>
<td>UN</td>
<td>NT</td>
<td>AL</td>
<td>ABX</td>
<td>PDG</td>
<td>N</td>
</tr>
<tr>
<td>Index weight</td>
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<td>.38%</td>
<td>.07%</td>
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<td>286</td>
<td>172</td>
<td>180</td>
<td>373</td>
<td>381</td>
</tr>
<tr>
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<td>UPS</td>
<td>Goldman Sachs</td>
<td>Prudential Securities</td>
<td>Electronic Arts</td>
<td>Principal Financial</td>
<td>Sungard Data</td>
<td>eBay</td>
</tr>
</tbody>
</table>

IV.B Methodology

The basic methodology follows Campbell, Lo and MacKinlay (1997), with an extension described in Lynch and Mendenhall (1997) to allow for 2 event dates with
different times between the two dates for each security. As mentioned before, there is no set time between announcement of removal from the index and actual removal.

There are two important dates – $T_1$ is the day after the announcement. The announcements are made after the close of the market, so the return the day after is the one of interest. $T_2$ is the date after the removal, i.e. the first day it is traded outside the S&P 500 index. In our sample, the time between these two dates ($T_2-T_1$) ranges from 0 days to 19 days with an average of 4.7 days. (If we exclude the 13 firms with 0 days then the average is 5.7 days.) The mode, as expected, is 5 days. We use the period from 60 days before the initial announcement to the day before the announcement as our estimation window. None of the previous studies suggest that any information is available prior to the announcement, or that any of the reaction occurs before this date in returns or in volumes. In this period we estimate the market model:

$$r_i = \alpha_i + \beta_i r_m + \epsilon_i$$

for each of the securities in our control sample and the foreign firms. The average beta in our sample is 0.6.

Given the estimate of $\alpha$ and $\beta$ we can calculate the excess or abnormal return $r^*$ in subsequent periods as:

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11 These tend to be stocks that have performed poorly, so beta will be low when markets are good. Average beta in initial sample is 0.6. This SHOULD get picked up if we add a momentum factor, and in theory even if we don’t, unless momentum is priced. It may be, however, that it is only the alpha that is negative, and not the beta which is low. We will also test with longer beta estimation periods although it is unlikely to affect the results materially.

12 We will think of $\beta$ as a vector later to give essentially the same methodology for a Fama-French model.
This will give the excess return for a given day. To analyze the effect of the delisting event we need to combine the returns over the entire event window. The cumulative abnormal return \( \text{CAR}(T_1, T_2) \) is defined as the sum of all excess returns over the window of interest. The average abnormal return \( \text{AAR}(T_1, T_2) \) is defined as the \( \text{CAR}(T_1, T_2) \) divided by the number of trading days in the period \((T_2-T_1)\). To test for effects with several firms we construct two average measures – \( \text{MAAR} \) and \( \text{MCAR} \), which are simple averages of \( \text{AAR} \) and \( \text{CAR} \). So for \( N \) companies,

\[
\begin{align*}
\text{AR}_i &= r_i - (\hat{\alpha}_i + \hat{\beta}_i r_m) \\
\text{CAR}_i(T_1, T_2) &= \sum_{t=T_1}^{T_2} r_{it} - \left(\hat{\alpha}_i + \hat{\beta}_i r_m\right) \\
\text{MCAR} &= \frac{1}{N} \sum_{i=1}^{N} \text{CAR}_i(T_1, T_2) \\
\text{MAAR} &= \frac{1}{N} \sum_{i=1}^{N} \frac{1}{(T_2-T_1)_i} \text{CAR}_i(T_1, T_2)
\end{align*}
\]

When there are the same number of days in every event window as in most event studies, these produce the same inferences. However when there are different numbers of days across firms – for example the window between announcement and effective date in our study - the \( \text{MAAR} \) will give greater weight to firm-days where the window is short, whereas the \( \text{MCAR} \) gives identical weight to all firm-days. In this case the \( T_2-T_1 \) would be different for each firm.
To determine whether or not there is an information effect, we can do a two-sample t-test (adjusted for estimation error and heteroscedasticity) to test for differences in the MCAR/MAAR. The information hypothesis predicts that the MCAR/MAAR should be negative for both groups of stocks, but less negative for the foreign sample on removal from the index. Our null hypothesis is that there is no difference between the two groups. If we do see a difference, then we would take that as evidence to reject the null hypothesis that there is no information effect. We also test each event window to see if the effect for both the control sample and the foreign firms to see if the CARs are different from zero.

IV.C Price results

There are several events that we can look at to provide some insights. The first event window that we look at is the period leading up to the announcement. Previous authors with only one exception have not studied this period under the assumption that the information is private to the index committee. This is unusual since in most event studies including, but not limited to takeovers, earnings announcements or restructurings [add references here] the balance of evidence suggests that there is, in fact, a run-up effect to many of these events. Only Liu (2000) examined this time period, but for the Nikkei 500 index. He looked at each of the 15 days preceding the announcements and found that none of the individual days was significant. However, it is important to look at the “big picture”, or the cumulative effect. We have chosen to look at the 30 day

13 We assume throughout this version of the paper that the effects are stationary through time. Some testing still needs to be done regarding changes to the effect over time. In other words if the information effect has just been getting smaller over time, we could observe the same results as we do here.
period immediately preceding the delisting announcement. Given the lack of attention by previous authors, we were surprised to discover a CAR of -7.91% (t-stat of –3.8). For the foreign firms a much smaller CAR of –4.85% (t-stat of –1.0) was observed. This result is not statistically significant, though it is in the correct direction. So the control sample firms did indeed experience poor returns before the removal, but the foreign firms did not. From the chart below it seems that the 7 foreign firms under performed even more up until the final days. What is not clear is the causality of the event. That is, were the companies removed because they did poorly in this short window, or did the market anticipate their removal and discount that fact in advance\textsuperscript{14}. [Check on previous papers’ non-beta adjusted results to see if that’s the problem]

\textsuperscript{14} If we accept the second direction then this provides an interesting possibility for interpreting the results in much of the rest of the paper. If poorly performing and “small” firms in general are anticipated to be removed, that would not have been the case for the 7 foreign firms. So one might argue that the larger foreign firms would have less abnormal losses in the run-up period as we observe, or less of a surprise, but then experience a greater negative return after the event. This interpretation would only strengthen the results below. Thanks to Alan Kraus for suggesting this line of reasoning.
Figure 3 - Preannouncement CARs. This figure charts the CARs of the control sample and the foreign stocks over the 30 day period leading up to the announcement of removal from the S&P 500. The anticipatory decline was surprising given that almost no authors had reported results for this period before the announcement.

The first date that other authors have studied in detail is the announcement date (or, more precisely the first trading day after the announcement date). As mentioned above, we have companies that have essentially zero days between the announcement date and the effective date. All 13 of these companies entered the control sample before the changes to the S&P announcement policy in October 1989. For this test, and for the effective date and the time between the 2 dates (3 event windows in total) we leave out these 13 companies leaving 55 companies in the control sample. Under both the liquidity and the information hypotheses we should observe a negative return on this day. In both the control sample and the foreign firms we would expect liquidity to be a significant issue, but if there is an information effect, it should only appear for the control sample firms and not for the foreign firms. Any difference between the two must therefore
represent the information effect that we are trying to detect and measure. For the control sample of 55 firms we get a one day abnormal return of \(-7.15\%\) (t-stat of \(-18.8\)), which is strongly significantly different from 0. This is similar in magnitude to the \(-6.26\%\) reported by Lynch and Mendenhall (1997). For the foreign firms we get a one day abnormal return of \(-4.62\%\) (-5.3), which is also significantly different from 0. The two results are also statistically different from each other which means that we are unable to reject the alternative hypothesis that there is an information effect. Both negative results also do not allow us to reject any of the other hypotheses.

The next window of interest is the effective date. If there were only index funds involved, or if only index funds knew that there was going to be a change or in the time before there was pre-announcement of changes, this would be the only important date to look at. Under both the liquidity and information hypotheses we would expect a decline on this date. Even though most index funds spread their trades over several days, this introduces tracking error, which is the most meaningful way of measuring index manager performance. Of course it is not necessarily in the interest of shareholders or unitholders, but if liquidity is the only issue, then the prices should return to normal over time. For the control sample of 55 firms (i.e. not including the 13 pre-1989 observations) we find an abnormal return of \(-2.55\%\) (-6.7). For the foreign firms we get a one day abnormal return of \(-1.92\%\) (-2.2).

The literature assumes that information effects should occur almost immediately, whereas liquidity effects can take some time just due to the large volume of trading that
needs to be done. The empirical evidence is not yet compelling for this, so we will use the MAAR. Because companies have differing numbers of days between announcement date and effective date (not including the effective date itself) it is unclear whether AAR or CAR is more important, so we present both statistics for consideration. For example, when Microsoft was added to the index, there were 16 trading days before the effective date. This was to allow portfolio managers a longer time due to the very large amount of stock that needed to be traded. It is unclear whether or not all of the excess return would be spread over time, or whether there would be a greater or lesser overall effect. Looking at the total time the CAR for the control sample firms is –11.27% and for the foreign firms is –11.15%. Although slightly smaller, this effect is statistically similar to the –12.69% reported by Lynch and Mendenhall (1997). These are not statistically different in means from each other, but they are both statistically different from zero. This is inconsistent with the idea that the information effect is present in the control sample but not the foreign firms. To test whether this is possibly due to outliers we ran a Wilcoxon 2 sample test. Although 7 companies is too small for the asymptotic normality assumption to hold [Need to implement the exact test once I have the complete dataset], the observed Z-score is 33, strongly suggesting that the medians of the two distributions are different, with a median in the control sample (foreign) firms of –10.49% (-12.1%). However, the AAR for the sample is only –3.14%, while for the foreign firms it is –1.39%, which simply represents the shorter average event window of the sample firms.

The last event windows that we want to examine are those after the companies are gone from the index. We have seen that there is a strong negative reaction over the
windows that we expect. What we want to see is if this reaction is reversed over time. Studies to date have offered mixed reviews. The balance of evidence currently supports that while there is some price reversal over time, there is also some permanent effect that does not get reversed. If there is new information revealed by the removal then we would not expect it to be reversed over time. We would, however, expect short term liquidity effects to disappear and be reversed. We look at windows 10 (non-inclusive) days after the effective date and 30 days after. These periods are overlapping, so some care needs to be taken interpreting the results. In the control sample firms we find evidence to support that previous studies. In the 10 days after the firm is removed, the cumulative abnormal return is 2.79% (t-stat of 2.3). After 30 days, the MCAR is 7.85% (3.8). Lynch and Mendenhall report a return of 4.6% in the first 5 days after the effective date, and then an additional 1.6% in the next 5 days. This is considerably higher than our figure. It is possible that the effect has decreased with time. This suggests that the permanent effect is greater now than before. So while some of the abnormal losses are reversed, there does seem to be a permanent decrease in the price of the stocks. This would be consistent with several of the hypotheses presented above including the information hypothesis and the market segmentation hypothesis. The 7.85% could be considered the value of the liquidity premium that was demanded in the short run to absorb the excess supply of stock from indexers, while the approximate 3.5% difference\(^{15}\) would be the value to other sources. Very surprisingly this result does not appear in the 7 foreign firms examined. The CAR over the 10 and 30 day windows is –9.93% (-3.6) and –4.32% (-0.9) respectively. We do not as yet have an explanation for this result.

\(^{15}\) A loss of 11.27% from announcement to effective date, and then a rebound of 7.85% in the 30 days after the company has been removed from the index. 11.27% - 7.85% = 3.42%
Figure 4 - Post-removal CARs from the day after the effective date until 30 days after removal. This figure charts the performance of both the control sample firms and the foreign firms for the 30 days following the effective date of removal from the S&P 500. The control sample reaffirms previous research and reverses some but not all of the decrease on removal. A surprising result is that the foreign firms continue to decline after removal. Further work, especially using liquidity measures, still needs to be done.

Table 2 - Summary of price results. Numbers in parentheses are t-stats. It should be noted that with 7 foreign firms, t-stats will be different from standard normal results. Rows marked with ° are significant in that the control sample and the foreign firms are statistically different using a 2 sample t-test. Numbers marked with a * are statistically significant at the 5% level.

<table>
<thead>
<tr>
<th>Time</th>
<th>Control Sample</th>
<th>Foreign Firms</th>
</tr>
</thead>
<tbody>
<tr>
<td>30 days preceding announcement°</td>
<td>-7.91% (-3.8)*</td>
<td>-4.85% (-1.0)</td>
</tr>
<tr>
<td>Announcement date°</td>
<td>-7.15% (-18.8)*</td>
<td>-4.62% (-5.3)*</td>
</tr>
<tr>
<td>Announcement to effective date16</td>
<td>-11.27%*</td>
<td>-11.15%*</td>
</tr>
<tr>
<td>Effective date only°</td>
<td>-2.55% (-6.7)*</td>
<td>-1.92% (-2.2)*</td>
</tr>
<tr>
<td>10 days after effective date°</td>
<td>2.79% (2.3)*</td>
<td>-9.93% (-3.6)*</td>
</tr>
<tr>
<td>30 days after effective date°</td>
<td>7.85% (3.8)*</td>
<td>-4.32% (-0.9)</td>
</tr>
</tbody>
</table>

16 Error terms are tricky, though they are both significantly different from 0. The control and foreign firms are not statistically different using a 2 sample t-test, but are statistically different in median using a Wilcoxon test.
IV.D  Volume results

V. Conclusion

In this paper we looked at returns around the removal of 7 foreign firms from the S&P 500 in July 2002. This event gave us a unique opportunity to separate the information effect from the other effects that have been proposed for S&P deletions. Keeping with the findings of past literature we find an abnormal negative return between the day of announcement and the actual day that the company is removed from the index. During this period the control sample companies decreased by more than the 7 foreign firms on the announcement date, the effective date and the time period in between. This is consistent with theories that suggest that there is information content in S&P removals. Because the 7 foreign firms were removed for exogenous reasons, the difference between the returns is the expected value of information divulged by the S&P.

The surprising, and as yet unexplained result, came after the companies had been removed. In the control sample firms we confirmed previous research which showed that some, but not all, of the losses are recaptured within the 30 trading days after the event. However with the foreign firms, not only does the loss not reverse itself, the companies’
prices continue to decline! It is possible that some explanation will be seen in the volume analysis that has yet to be completed.

Other areas for future research include a look at the a cross-sectional analysis of companies that are removed. Are there company specific factors other than size that determine whether or not a company will be removed? For example some companies are removed when they are 497th largest. But what about the 3 smaller companies? International markets are also an interesting and underdeveloped comparison for some hypotheses. For example it is estimated that there is $3-4 trillion indexed to the MSCI indices, and there have been some major historical rebalancings. Some work discussed above has looked at individual countries, but they are for various reasons not comparable across borders. Another possibility would be to look at companies that move to or from other S&P indices like the S&P small or mid-cap, as opposed to being removed from indices entirely.\footnote{Thanks to Amir Barnea for this last idea.}

Other issues that are still being worked on and will be included in later versions of this paper are volume effects, correlation effects in the error estimates, increasing the number of explanatory factors from just the market to also include the F-F SMB and HML factors, and possibly a factor to allow for correlation with the home market of each of the 7 foreign firms. This last factor will likely have no effect given the global nature of the firms. We will also finalize the control sample to include all eligible firms between 1989 and 2002.
Price effects Figures and graphs

<Actual excess return year by year, including standard deviations and t-stats>

<Amount of funds indexed – use Vanguard at least. Also total?>

<Each event day return from say –30 to +30, including percent >0>

Volume effect and miscellaneous figures and graphs

<Mean increases in volumes around announcements>

<Mean volume trading ratios say –30 to +30 again>

Appendix A.

Error estimates for Abnormal returns.

Dealing with correlation

Although there are some overlapping windows in the control sample, there is little enough that the correlation between returns of different companies can safely be ignored. Unfortunately, our entire set of foreign firms overlaps completely. To check if this is
likely to be a problem we look at the correlation structure over the estimation window and get the following correlation matrix:

<table>
<thead>
<tr>
<th></th>
<th>AL</th>
<th>N</th>
<th>PDG</th>
<th>RD</th>
<th>ABX</th>
<th>UN</th>
<th>NT</th>
</tr>
</thead>
<tbody>
<tr>
<td>AL</td>
<td>1.0000</td>
<td>0.7223</td>
<td>0.0844</td>
<td>0.6315</td>
<td>0.1723</td>
<td>0.4789</td>
<td>0.3323</td>
</tr>
<tr>
<td>N</td>
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<td>0.5162</td>
<td>0.1999</td>
<td>0.4473</td>
<td>0.2838</td>
<td></td>
</tr>
<tr>
<td>PDG</td>
<td>1.0000</td>
<td>0.0003</td>
<td>0.8431</td>
<td>0.0553</td>
<td>-0.0310</td>
<td></td>
<td></td>
</tr>
<tr>
<td>RD</td>
<td>1.0000</td>
<td>0.0637</td>
<td>0.6631</td>
<td>0.3151</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>ABX</td>
<td>1.0000</td>
<td>0.1604</td>
<td>-0.0319</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>UN</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>1.0000</td>
<td>0.1665</td>
<td></td>
</tr>
<tr>
<td>NT</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>1.0000</td>
<td></td>
</tr>
</tbody>
</table>

It is clear that some of these stocks move strongly together, and that this needs to be addressed.

Appendix B – List of companies being studied

<Summary statistics for all control sample firms>

References


Scholes, M. 1972. The market for securities: substitution versus price pressure and the effects of information on share prices, *Journal of Business* 45, 179-211


*Standard and Poor’s Press Release*, October 22, 2002, Criteria for Additions to and Deletions from a U.S. Index