

Why Did the Index Effect Disappear? A Pre-Emption Perspective

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Abstract

The index effect refers to the abnormal price movements that occur when stocks are added to or removed from major equity indices. In the S&P 500, this effect peaked in the 1990s and had largely disappeared by the 2010s. We examine whether improved anticipation of index changes has shifted part of the price impact into the pre-announcement window, thereby contributing to the decline. To study this mechanism, we investigate the relationship between each stock's pre-announcement and post-announcement cumulative abnormal returns and interpret a negative association as evidence of pre-emption. Our results suggest that pre-emption has strengthened over time as the index effect has diminished, although its magnitude indicates that it accounts for only a small share of the overall decline.

Keywords: Index effect, S&P 500, pre-announcement returns, anticipatory trading, passive investing

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1 Introduction

Decades of research document the existence of an index effect: when firms are added to or deleted from the S&P 500, their stock prices exhibit sizable abnormal reactions (Shleifer, 1986; Harris and Gurel, 1986; Beneish and Whaley, 1996; Lynch and Mendenhall, 1997). Early studies attributed these price movements to inelastic demand from index-tracking investors, while later work introduced complementing information-based explanations (Harris and Gurel 1986, Wurgler and Zhuravskaya 2002, Cai 2007, Aghaee 2022). Recent evidence, however, indicates that the magnitude of the index effect has declined substantially over time and has become nearly negligible in the 2010s (Greenwood and Sammon, 2024; Kamal et al., 2012; Aghaee, 2022; Bennett et al., 2020). Understanding why the index effect has faded is important for evaluating market efficiency and for assessing the economic costs borne by passive investors who trade in response to index changes.

Several explanations have been proposed, including increased market liquidity and the growing importance of migrations from other indices. One prominent yet insufficiently examined explanation for the diminishing index effect is the possibility that market participants have become increasingly adept at anticipating index changes. If investors can better forecast forthcoming additions and deletions, a portion of the associated price adjustment should occur before the announcement, reducing the residual reaction observed on the announcement day. While prior research, including Greenwood and Sammon (2024), documents rising pre-announcement abnormal returns, these studies stop short of formally testing whether anticipatory price movements systematically substitute for post-announcement returns at the individual-stock level. Practitioner efforts to forecast S&P 500 changes further suggest that predictability may be economically meaningful, yet the empirical link remains largely unexplored.

This paper aims to fill this gap by examining whether higher pre-announcement abnormal returns (*PreCAR*) are associated with weaker post-announcement abnormal returns (*PostCAR*) for S&P 500 additions and deletions. Building on the idea that predictable index changes create opportunities for informed investors to trade ahead of index funds' mechanical rebalancing flows, we model the relationship between each stock's *Pre-* and *PostCAR* and interpret a negative relationship as a proxy for pre-emption. We estimate this relationship while controlling for momentum, short-term reversal and firm characteristics. To ensure that the controls capture normal return dynamics rather than patterns specific to index events, we estimate all control variables using a broad sample of large U.S. non-event firms. Overall, our design aims to isolate the relationship between *PreCAR* and *PostCAR* from confounding influences, allowing it to serve as a cleaner proxy for pre-emption.

Examining a sample of S&P 500 index changes between 1980 and 2020, we find a significant but economically small negative association between *PreCAR* and *PostCAR* when using a 20-trading-day pre-announcement window. The estimated coefficients are -0.008 for additions and -0.006 for deletions. To assess the economic magnitude, we evaluate the implied effect at the average *PreCAR*. For additions, the coefficient implies a 0.012 percentage point reduction in *PostCAR*, which is about 0.4% of the average *PostCAR*. For deletions, the corresponding effect is 0.032 percentage points, or roughly 0.9% of the average *PostCAR*. These values indicate that the economic magnitude of pre-emption over the full sample period is small¹.

Next, we investigate how pre-emption has evolved over time. We hypothesize that pre-emption of the index effect has increased as the index effect has disappeared, consistent with a contribution to

¹Economic magnitudes presented in the introduction uses abnormal returns calculated using the CAPM. In Section 6 we also report results from using FF3, FF5 and simple market-adjusted returns.

the disappearing index effect. For additions, we find supporting evidence. *PreCAR* has no significant relationship to *PostCAR* in the 1990s, when the index effect peaked, but is negatively associated in the 2010s when the index effect had largely disappeared. In the 2010s, the estimated coefficient of -0.076 implies a 0.14 percentage point reduction in *PostCAR* when evaluated at the average *PreCAR*, or about 14% of the average *PostCAR* in that decade. The effect is, however, not fully robust to the choice of market model for calculating abnormal returns.

In contrast, we find no evidence of pre-emption for deletions in any era or market model. This asymmetry weakens the interpretation that increased pre-emption contributed to the diminishing index effect. However, we theorize about two possible explanations for this outcome; lower incentives to pre-empt deletion events due to short selling costs, and limited statistical power due to the smaller sample of discretionary deletions.

When using longer pre-announcement windows, 50 and 100 trading days, we generally find more robust evidence of pre-emption for additions, especially in the 2010s. By contrast, a shorter window of 10-trading days attenuates the estimated relationship. We interpret this as consistent with the dual prediction problem a trader faces, reflecting both selection and timing predictability. Since timing appears to be harder to predict, the price adjustment is less likely to be concentrated in the immediate pre-announcement period. For the most predictable candidates, proxied by market capitalization rank, we find less pre-emption in our windows. We interpret this as consistent with the idea that for firms whose inclusion is highly foreseeable, part of the price adjustment may occur even before the earliest window we consider. Under this interpretation, our measured *PreCAR* may understate the extent of pre-emption for the most predictable firms.

To assess whether increased pre-emption may have contributed meaningfully to the disappearance of the index effect, we compare the implied magnitude of our pre-emption estimates with the historical decline in post-announcement cumulative abnormal return. Using pre-announcement windows ranging from 20 to 100 trading days, the 2010s coefficients multiplied by that decade’s average *PreCAR* imply reductions in *PostCAR* of 0.14 to 0.64 percentage points. This may be viewed in relation to the observed 4.13 percentage point decline in average *PostCAR* from the 1990s to the 2010s. Based on our estimate, this corresponds to roughly 3-15% of the total decline. Moreover, the underlying demand shock associated with S&P 500 inclusions has increased over this period, implying that the true downward pressures on the index effect must be even stronger than the naïve decline suggests. We therefore conclude that increased pre-emption, while present, likely accounts for only a small part of the declining index effect, consistent with [Greenwood and Sammon \(2024\)](#).

At the same time, we emphasize that pre-emption is an equilibrium outcome, with incentives to pre-empt diminishing as the index effect decreases. Therefore, the fact that magnitudes remain similar or higher in the 2010s compared to the 2000s, despite weaker incentives, underscores that anticipatory trading has become more systematic.

This paper makes several contributions. First, we provide the first formal stock-level test of whether pre-announcement returns substitute for post-announcement returns in S&P 500 index changes, extending the descriptive evidence in [Greenwood and Sammon \(2024\)](#). Second, we present evidence that pre-emption has increased as the index effect has declined. Yet, the magnitude appears small relative to the overall decline, confirming the view of [Greenwood and Sammon \(2024\)](#). Third, we find that anticipatory trading is not fully captured by the narrow pre-announcement windows used in prior work by [Greenwood and Sammon \(2024\)](#) and [Petajisto \(2011\)](#), suggesting that longer windows may be needed to measure pre-announcement price adjustments accurately. Finally, our evidence

of pre-emption suggests that passive index investors bear higher costs than implied by the post-announcement index effect.

The remainder of the paper is structured as follows. Section 2 reviews the related literature. Section 3 outlines the research design and develops the hypothesis. Section 4 describes the data and variable construction, and Section 5 details the empirical methodology. Section 6 presents the results, followed by a discussion in Section 7. Section 8 concludes.

2 Literature Review

This section reviews the relevant literature. We begin by summarizing the index effect and its theoretical explanations. We then review the literature documenting the decline of the index effect over time. Thereafter, we discuss proposed explanations for this decline. Finally, we review the literature on the predictability of index changes and outline the gap our paper addresses.

2.1 The Index Effect

Early studies of S&P 500 index changes were the first to document the “index effect.” They found that stocks added to the index typically saw their prices rise, while those removed saw their prices fall (Shleifer, 1986; Harris and Gurel, 1986; Beneish and Whaley, 1996; Lynch and Mendenhall, 1997). Shleifer (1986) and Harris and Gurel (1986) found average price increases of about 3 percent on the effective date, which at the time also served as the announcement date. When S&P began pre-announcing index changes in 1989, Lynch and Mendenhall (1997) found that abnormal returns on the announcement day were even higher, between five and seven percent. Broadly, explanations fall into two groups: demand-based and information-based.

2.1.1 Demand-Based Theories

Demand-based theories argue that index changes trigger shifts in investor demand unrelated to fundamentals. This contrasts with the efficient market hypothesis, which assumes that demand is perfectly elastic. Wurgler and Zhuravskaya (2002) show that downward-sloping demand curves can arise from limits to arbitrage and imperfect substitutability between stocks. They argue that arbitrageurs face risks when trying to offset index demand shocks because there are no perfect substitutes. Petajisto (2009) shows that such inelastic demand can also arise in equilibrium when only a limited share of active capital is available to absorb non-informational demand shocks. Pavlova and Sikorskaya (2023) further show that stocks with larger changes in institutional benchmarking intensity have stronger index effects, which is consistent with the idea of inelastic demand from benchmark-constrained investors.

2.1.2 Information-Based Theories

In contrast, information-based theories suggest that the announcement of a stock’s addition to (or deletion from) an index conveys positive (or negative) information about the asset (Chen et al., 2004). One explanation within the information-based view is that a firm’s inclusion may signal positive information about its future prospects. This is particularly relevant for the S&P500, where additions are chosen discretionarily rather than mechanically. Denis et al. (2003) find that newly added firms experience increases in both earnings forecasts and realized earnings. Cai (2007) supports this by

showing that when a company is added, other industry- and size-matched firms also experience positive price reactions, suggesting that information is shared through the event. Another information-based theory is that index inclusion increases investor awareness, which may incentivize management to improve performance (Denis et al., 2003). Chen et al. (2004) refine this view by showing that the information effect is asymmetric. Firms added to the index gain investor awareness, whereas deletions do not experience a comparable decline because awareness does not simply disappear. On the other hand, Bennett et al. (2020) find that the increased public scrutiny following an index addition can have a negative effect on firm performance.

The numerous different explanations for the index effect highlight the difficulty of attributing it to a single cause. Aghaee (2022) takes a more holistic view by analyzing the index effect through both demand- and information-based perspectives. He uses a novel identification strategy by analyzing the incumbents of the S&P 500 index to isolate the demand-based price impact. He finds that informational effects dominated before the year 2000, but that demand-driven forces became the main driver after 2000.

2.2 The Disappearance of the Index Effect

Given demand-based explanations of the index effect, a natural expectation is that the index effect would have increased along with the sharp rise in capital devoted to index tracking over the last four decades (Chinco and Sammon, 2024), assuming a constant elasticity of demand. Recent studies, however, document that the effect has declined substantially over the same period.

Central to this paper is Greenwood and Sammon (2024), who shows that the announcement day index effect for additions (deletions) was 3.4% (−4.6%) in the 1980s, peaked at 7.4% (−16.1%) in the 1990s, declined to 5.2% (−12.4%) in the 2000s, and nearly disappeared at 0.8% (−0.6%) in the 2010s. Their findings are in line with several recent studies that also report a declining index effect (Kamal et al., 2012; Aghaee, 2022; Bennett et al., 2020).

2.2.1 Reasons for the Disappearing Index Effect

Greenwood and Sammon (2024) examine four possible explanations for the disappearing index effect: changing composition of additions and deletions, increase in migrations from other indices, increased liquidity, and pre-emptive trading.

First, Greenwood and Sammon (2024) conclude that changes in the composition of additions and deletions explains only a small part of the disappearing index effect. Second, they examine the role of migrations from other indexes. They find that migrations can partly explain the decline in the index effect, since stocks that migrate from other indexes show smaller index effects, and the proportion of such migrations has increased over time. This migration effect is also stronger for additions than for deletions, which is consistent with Chen et al. (2004) and their findings on asymmetric information effects. Third, increased market liquidity around index changes is found as a contributing factor of the disappearing index effect. They find that the increase in liquidity is mainly driven by (1) higher overall market liquidity and lower trading costs, (2) more coordinated trading activity around index changes, and (3) greater flexibility by active managers in adjusting to index revisions.

Complementing these explanations, Kamal et al. (2012) finds that the decline in information asymmetry in the post-2000 period is a major reason for the reduction in announcement-day abnormal returns for S&P 500 additions. He attributes this to improvements in the information environment

following regulatory changes.

Our paper is closest related to the fourth explanation in [Greenwood and Sammon \(2024\)](#), which investigates whether increased predictability of index changes could help explain the decline. They test this idea in two ways. First, they examine whether investors anticipate index changes before the announcement. If market participants have become better at predicting which stocks will be added, prices should rise in advance as investors pre-empt the event. To study this, they examine cumulative abnormal returns before announcements and find that pre-announcement returns have increased over time when using a longer 100-day window. However, they note that this could also mean the S&P 500 committee is simply getting better at picking the best performing stocks, so the results are not conclusive. When they examine shorter pre-announcement windows, such as 10 or 20 days before the announcement, they do not find large enough differences to support the idea that predictability has increased. Second, they test whether it has become easier to predict additions using a simple size-ranking model. They find that predictability based on firm size has increased, since the S&P committee now tends to select larger firms within a narrower size range. However, they find significant randomness in the size rank of the firm getting added as the interquartile range is about 40 ranks. In conclusion, [Greenwood and Sammon \(2024\)](#) find only limited evidence that predictability has contributed to the decline of the index effect. However, their findings should be seen as suggestive rather than definitive, as they are mainly based on visual inspection of average pre-announcement cumulative abnormal returns and a simple forecasting model using firm size. Importantly, they do not examine whether pre-announcement and announcement-day abnormal returns are related at the stock level. Our paper addresses this gap by analyzing this stock-level relationship directly.

Other research also points to the possibility that S&P 500 revisions can be anticipated. [Agrawal et al. \(2024\)](#) uses machine learning to predict S&P 500 membership changes and achieves an F1-score of 0.85 with a random forest model. This suggests that even though the index changes are discretionary, they show a non-trivial degree of predictability. Similarly, [Geppert et al. \(2010\)](#) use survival analysis and neural network methods to study the probability of discretionary deletion of a firm from the S&P 500 index, and find that deletions might be predictable. There is also anecdotal evidence that market participants actively seek to predict index changes for financial gain. For example, FactSet has developed a quantitative model to help clients anticipate potential changes to the S&P 500 by predicting which companies are likely to be added or removed within a six-month window. From 2018 to 2021, their model correctly predicted 60% of additions and 83% of deletions, on average ([Segerstrom, 2021](#)).

In summary, prior literature suggests that the decline of the index effect may be explained by both demand-based factors and information-based factors. However, both academic studies and industry practice suggest that increased predictability of index changes may also play a role. Yet, to the best of our knowledge, no study has directly tested whether higher pre-announcement abnormal returns are systematically associated with lower post-announcement returns at the stock-level. This mechanism is important to understand, because if part of the price impact is now absorbed before the announcement, then the decline in the index effect may reflect a shift in timing rather than a true reduction in price pressure. Such a shift would have implications for interpreting market efficiency and for assessing the trading costs borne by passive investors. To address this gap, we empirically examine whether higher pre-announcement abnormal returns are associated with lower post-announcement abnormal returns for S&P 500 index changes. The next section outlines our research design and develops the hypotheses we test.

3 Research Design and Hypothesis

We argue that the relationship between pre-announcement and post-announcement abnormal returns provides a practical way to assess predictability. The intuition is as follows. If index changes are unpredictable, most of the price adjustment should occur after the announcement. However, if changes become more predictable, some investors will try to get ahead of the announcement. They do this because the announcement signals that, at the upcoming effective date, index funds will be required to buy or sell to match the new index composition and to maintain low tracking error (Pavlova and Sikorskaya, 2023), creating predictable demand or supply.

The fact prices move more on announcement day than on the effective day (Petajisto, 2011), already suggests that a front-running mechanism plays a role. Active traders act as liquidity providers and trade immediately after the announcement to profit from the anticipated impact of index-fund flows (Greenwood and Sammon, 2024; Madhavan, 2003). This front-running motive is precisely why S&P began pre-announcing index changes: to give the market time to absorb the trades and smooth the demand shock (Petajisto, 2011). Our measure of pre-emption extends this same mechanism further back in time.

If some investors can anticipate changes even before the announcement, part of the price impact will occur earlier, leaving less for the post-announcement period. This pre-emption implies a negative relationship between pre-announcement ($PreCAR$) and post-announcement abnormal returns ($PostCAR$).

Our research design builds on the logic in Greenwood and Sammon (2024), who note that under perfect anticipation one would expect no abnormal returns at the announcement date. While they point to this substitution mechanism, they assess it only by comparing average $PreCAR$ and $PostCAR$ over time. By modeling the relationship between these two periods directly, we provide a formal test of the substitution effect that Greenwood and Sammon describe but do not empirically examine. While we are, to our knowledge, the first to model the $PreCAR$ - $PostCAR$ relationship as a proxy for pre-emption in the index-recomposition setting, Jin (2000) employs an analogous proxy to capture pre-emption from anticipation of dividend-initiation announcements.

Prior research has highlighted endogeneity concerns when linking $PreCAR$ to pre-emption, primarily due to selection bias Greenwood and Sammon (2024). Specifically, pre-announcement returns may reflect the types of firms the Index Committee tends to select, firms that have recently appreciated, rather than true investor anticipation. We argue that our design is less exposed to this issue because our proxy for pre-emption relies on the substitution pattern between a firm’s pre- and post-announcement returns rather than on $PreCAR$ levels. However, selection can still matter to the extent that the same firm-level characteristics influencing selection also affect the $PreCAR$ - $PostCAR$ relationship. To mitigate this, we control for observable patterns related both to selection and to short-run return dynamics, namely momentum, short-term reversal, and firm characteristics. Although unobservable elements of the selection process may still add noise, such as the Committee’s qualitative assessment of firms, we argue that remaining bias is unlikely to materially affect the estimated relationship between $PreCAR$ and $PostCAR$.

We test this idea with the following regression:

$$\text{PostCAR}_i = \alpha + \beta \text{PreCAR}_i + \theta' X_i + \varepsilon_i$$

where PreCAR_i is the cumulative abnormal return before the announcement, and PostCAR_i is the cumulative abnormal return after the announcement. The vector X_i includes controls for momentum, short-term reversal, and firm characteristics. Based on the above reasoning, we formulate the following hypotheses for S&P 500 additions and deletions:

(i): There is pre-emption of the index effect, proxied by a negative association between pre-announcement cumulative abnormal return (PreCAR) and post-announcement cumulative abnormal return (PostCAR)

(ii): Pre-emption of the index effect has increased over time, as the index effect has declined, proxied by an increasingly negative association between pre-announcement cumulative abnormal return (PreCAR) and post-announcement cumulative abnormal return (PostCAR)

4 Data

In this section, we first provide an overview of the S&P 500 index and how the committee operates. We then detail the assembly of our dataset, including event selection and the exclusion criteria applied.

4.1 The S&P 500

Over recent decades, passive index tracking has increased substantially, driven by large inflows into index mutual funds and ETFs. The S&P 500 covers about 80% of the U.S. equity market capitalization and is one of the most widely followed indices globally. Eligibility for inclusion requires that a firm meets minimum thresholds for size, liquidity, free float, and profitability and that it is U.S.-listed and U.S.-domiciled ([S&P Dow Jones Indices, 2024](#)). Since more than 500 companies can meet all eligibility criteria simultaneously, the final selection is at the discretion of the S&P 500 Index Committee, which also considers factors like sector balance and overall market representation. Thus, index changes are not purely rule-based, in contrast to indices such as the Russell 2000. Since October 1989, index changes have been pre-announced after the market close at 5:15 PM Eastern Time, with press releases posted on the S&P's website ([S&P Dow Jones Indices, 2024](#)).

The rules for staying in the index are asymmetric compared to those for initial inclusion. Companies must meet all requirements to be added, but they are not removed automatically when they fall below thresholds. The S&P 500 Index Committee seeks to avoid unnecessary turnover and only removes companies when necessary to maintain the stability and representativeness of the index ([S&P Dow Jones Indices, 2024](#)).

Deletions fall into two main categories: non-discretionary and discretionary. Non-discretionary deletions are those forced by corporate events, such as mergers or acquisitions, that affect a stock's eligibility. These account for about 70 percent of all deletions ([Preston, 2025](#)). Each such event creates a vacancy that must be filled, meaning that many additions are reactive responses to deletions rather than proactive promotions. Discretionary deletions happen when the committee removes a company that has become too small or less representative, which is less common. This asymmetry means that deletions often trigger the additions, and the timing of many additions can be partly anticipated when a corporate event makes a deletion likely. Although the index is formally rebalanced every

quarter, about 90 percent of all constituent changes since 1995 have occurred outside these scheduled rebalances, highlighting the reactive nature of most index changes (Preston, 2025).

4.2 Data Construction

We obtain data on S&P 500 additions and deletions between 1980 and 2020 from Greenwood and Sammon (2024). Specifically, we extract their event specification data from the online appendix, which contain the complete set of S&P 500 additions and deletions during this period. In this dataset, Greenwood and Sammon provide both the date the index change was announced (announcement date) and the date the change was implemented (effective date). We focus on the announcement date, as after that point there is no longer a matter of predicting which firms will enter the index. The data consolidated by Greenwood and Sammon combines sources: Sibilis Research for the period 1990 to 2020 and Barberis et al. (2005) for announcement dates prior to 1990.

Drawing from Greenwood and Sammon (2024), we apply several exclusions to ensure a consistent sample. Firstly, we remove index additions that coincide with major confounding events in a ± 100 trading day window around the announcement. Specifically, we exclude cases where the firm (i) was newly listed (IPO), created via spin-off within ± 100 trading days, (ii) acquired or delisted for reasons other than an acquisition (i.e. non-discretionary deletions), or (iii) acted as an acquirer in a merger within ± 100 trading days. For acquisition activity, we cover acquisitions appearing as an ACPERM in CRSP, i.e. acquisitions where the target was also a US-listed security. Secondly, we remove firms that cannot be matched on CUSIP, from where we obtain the analyst coverage control. Table A.1 in the Appendix specifies the sample selection process from the dataset’s total observations.

We merge the event specifications with CRSP data on PERMNO to measure returns and construct control variables. From CRSP, we also construct daily market-wide statistics to normalize our control variables, including daily turnover across all ordinary common shares traded on major exchanges and the total daily market capitalization of S&P 500 firms. In addition, we match the event specifications to analyst forecast data from I/B/E/S using CUSIP.

Finally, we obtain risk-free returns, market returns and Fama-French factors from Kenneth R. French’s online data library, which we use to estimate expected returns under CAPM, FF3 and FF5 model specifications and calculate market-adjusted returns.

5 Methodology and Empirical Design

In this section, we first motivate our choice of pre-announcement and post-announcement windows. Thereafter, we describe the market models used to compute abnormal returns. We then explain our construction of the estimation sample, followed by presenting the firm-characteristic and return-based controls included in the regressions. Next, we introduce the interaction variables. Finally, we outline our empirical strategy by presenting the regression specifications and their extensions.

5.1 Event Window

We define our main event window as the trading days surrounding the announcement, spanning from 20 days before to one day after ($-20, +1$), to capture both anticipatory trading and the immediate post-announcement reaction.

5.1.1 Pre-Announcement Window

The pre-announcement period covers the 20 trading days before the announcement (-20, -1), to capture potential anticipatory trading while limiting unrelated noise. About 90 percent of index changes occur outside scheduled rebalances (Preston, 2025), creating timing uncertainty since there is no fixed timetable for announcements. Because of this uncertainty, investors who expect an index change cannot time their trades precisely and are likely to build positions gradually rather than on a single day. A very short pre-announcement window could therefore miss relevant anticipatory trading, while a much longer one would likely include more unrelated noise. Greenwood and Sammon (2024) analyze pre-announcement windows of 10, 20, 50, and 100 trading days and find that cumulative abnormal returns increase with longer windows. We use 20 days as our main pre-announcement window, which we view as a reasonable balance between capturing anticipatory trading and limiting unrelated noise, given the timing uncertainty of index changes. This is also the window Greenwood and Sammon (2024) uses as the main window to balance the trade-off. For robustness, and because the appropriate pre-announcement window is inherently uncertain, we also use alternative windows of 10, 50, and 100 trading days (-10 to -1, -50 to -1, and -100 to -1) trading days.

5.1.2 Post-Announcement Window

Following Greenwood and Sammon (2024), we begin by defining the announcement return as the cumulative abnormal return from the day before to the day after the announcement. We then exclude the day before the announcement because it belongs to our pre-announcement window. Our post-announcement window therefore covers two trading days (0, +1), where day 0 is defined as the first day the announcement can be traded on. A two-day window limits noise while being long enough to capture the immediate market reaction.

5.2 Market Models

Earlier studies of the index effect employ two main approaches to measure abnormal returns: (1) estimation-based market models (e.g., Bennett et al., 2020; Aghaee, 2022), and (2) simple market-adjusted returns (e.g., Greenwood and Sammon, 2024; Kasch and Sarkar, 2014). The choice between these approaches reflects a trade-off between bias and estimation error: model-based adjustments correct for heterogeneity in systematic risk but require parameter estimation, whereas simple adjustments avoid estimation error at the cost of assuming identical market sensitivity across firms. The method to calculate abnormal returns are henceforth referred to as “market models”.

Estimation based market models

We primarily use estimation-based abnormal returns for two reasons. First, because our analysis focuses on the firm-specific relationship between pre- and post-announcement returns, we aim to remove systematic variation that could obscure this relationship. Estimating factor loadings allows us to remove return variation driven by common risk factors and isolate firm-level movements more accurately. Second, our pre-announcement windows, ranging up to 100 trading days, make differences in factor exposures economically meaningful. Over such horizons, ignoring variation in risk factors could confound factor-driven co-movements with anticipatory trading.

Following Bennett et al. (2020), we estimate abnormal returns using several asset-pricing specifications to ensure robustness to model choice: the CAPM, Fama-French three-factor (FF3), and

Fama-French five-factor (FF5) models. Incorporating multiple factors is also motivated by Kasch and Sarkar (2014), who show that S&P 500 index inclusions depend on the firms’ pre-inclusion market and fundamental characteristics, and therefore are not exogenous. We exclude the momentum factor from the asset-pricing models and instead control for momentum separately in the cross-sectional regressions. This specification treats momentum effects more flexibly by not constraining their influence through the factor model.

Abnormal returns are estimated over a one-year estimation window ending 20 trading days before each event window (Aghaee, 2022). The market return is proxied by the CRSP value-weighted market portfolio (Shleifer, 1986; Harris and Gurel, 1986; Kasch and Sarkar, 2014). We prefer the CRSP portfolio to the S&P 500 return (as in Greenwood and Sammon, 2024) because the S&P 500 index is mechanically affected by additions and deletions, both directly through constituent changes and indirectly through anticipatory trading. Although this effect is likely small, the CRSP portfolio provides a more exogenous proxy for the aggregate market.

Finally, we acknowledge that abnormal returns are conditional on the chosen asset-pricing model. As noted by Fama (1970), any test based on abnormal performance jointly depends on market efficiency and the correctness of the benchmark model. Accordingly, we use several models as a robustness check against potential model-driven bias.

Simple Market-Adjusted Model

Despite the advantages of estimated models, much of the index effect literature employs a simple market-adjusted measure, defined as the stock’s return minus the market return, typically using the CRSP value-weighted or S&P 500 index (e.g., Kasch and Sarkar, 2014; Petajisto, 2011; Greenwood and Sammon, 2024). This approach implicitly assumes a market beta of one for all firms and removes the need to estimate firm-specific parameters, thereby eliminating estimation error. For very short event windows, such as one or two trading days, the differences in expected returns across firms are small, and the additional estimation error from fitted models may exceed the theoretical correction. Consequently, when measuring short-horizon index effects, many studies favor the simple approach.

To enhance comparability with recent studies and to verify that our findings are not sensitive to model estimation errors, we also report results using market-adjusted (henceforth “MA”) returns defined as the difference between the firm’s return and the CRSP value-weighted market return.

5.3 Estimation Sample Construction

We estimate our control variables using a broad set of large U.S. firms (henceforth, “estimation sample”) rather than only the firms added to or deleted from the index (henceforth, “event firms”). This serves two purposes. First, estimating the controls only on additions and deletions would risk controlling for part of the treatment effect itself, since these firms are selected precisely because they experience unusual dynamics around index changes. Including a broad sample of large firms averages out effects common to all firms, ensuring that the estimated control relationships are not biased by the event-specific behavior of treated firms. Second, pooling the event firms and estimation sample to estimate our controls allows us to calculate momentum and short-term reversal even when their estimation windows overlap with the event window, since the event firms only make up about 0.4 percent of the total sample used to estimate the controls.

To construct our estimation sample, we rank all U.S.-traded firms in the CRSP daily stock file for each event and select firms ranked between the 300th and 700th largest by market capitalization. The

ranking and selection are performed 20 trading days prior to the start of each *PreCAR* event window. This timing ensures that the event under examination has not yet influenced firm size rankings, maintaining the exogeneity of the estimation sample composition. For this estimation sample, we collect the same variables and apply the same exclusions as for our event firms. Additionally, we exclude any firm that was itself subject to an index change within ± 100 trading days from the event to avoid contamination of the estimation sample by effects related to the event.

We acknowledge that, for this setup to yield accurate estimates, we implicitly assume that the relationship between returns and our control variables is comparable for event firms and the firms in our estimation sample in the pre-event period.

For our firm-characteristic controls presented in Section 5.4.1, the assumption of a common slope between event- and estimation firms is relatively innocuous. These variables are used as cross-sectional controls intended to absorb level differences, they are measured well before the event window and there is no theoretical or empirical reason to expect S&P 500 candidates to exhibit systematically different return sensitivities to these characteristics beyond their levels. Using a pooled slope for such controls is also standard in difference-in-differences settings, where similar firm-level variables capture cross-sectional heterogeneity under comparable identification assumptions (e.g., Bennett 2020). While our design is not a difference-in-differences, the underlying rationale is conceptually similar.

By contrast, our main momentum and short-term reversal controls are signal-based time-series measures, for which this assumption is stronger. We therefore explicitly examine the validity of this assumption in Section 5.4.2.

5.4 Control Variables

We first define our firm-characteristic controls, following Greenwood and Sammon (2024). Next, we introduce our return-based control variables and document heterogeneity in momentum and reversal exposures between our event firms and the estimation sample. This leads us to include each firm’s factor loadings in the regression and to interact them with the momentum and reversal signals.

5.4.1 Firm-Characteristic Control Variables

We include the same four firm-characteristic controls as Greenwood and Sammon (2024) and define each variable following their methodology to maintain consistency with prior work. *Turnover* is the stock’s average daily trading volume divided by shares outstanding, minus the value-weighted average turnover across all ordinary common shares traded on major U.S. exchanges over the same period. *Turnover* captures potential liquidity effects that could influence how returns evolve both before and after index announcements. *Size* is the firm’s market capitalization relative to the total market capitalization of the S&P 500. Larger firms are generally more visible and easier to anticipate as index candidates, which may strengthen pre-announcement price adjustments. At the same time, their additions trigger larger benchmark-driven buying due to tracking error concerns. Arbitrage risk (*WZ*) follows Wurgler and Zhuravskaya (2002) and is measured as the variance of residuals from a CAPM regression estimated over the previous year, capturing firm-specific volatility. Firms with higher idiosyncratic volatility face stronger limits to arbitrage, which can hinder price corrections and lead to weaker adjustments both before and after the announcement. Analyst coverage (*Coverage*) is the number of analysts with active earnings forecasts for the firm, based on I/B/E/S data. Broader analyst coverage increases information production and visibility, potentially improving the market’s

ability to anticipate index changes.

All controls are measured before the pre-announcement window so they capture normal market conditions and are unaffected by the event (MacKinlay, 1997). In line with Aghaee (2022), all controls are measured using information available up to 20 trading days before the start of each *PreCAR* window. Specifically, *Turnover* is measured over one month (30 calendar days) ending 20 days before the start of the *PreCAR* window. *Size* is measured as point-in-time values 20 days before the start of the *PreCAR* window, and *Coverage* is the most recent quarterly figure available prior that date. *WZ* is estimated from a CAPM regression using daily returns over one year ending 20 days before the start of the *PreCAR* window. The 20-day buffer moves dynamically based on the chosen *PreCAR* window ranging from $[-100, -1]$ to $[-10, -1]$ to maintain a consistent distance between the control-variable measurement and the start of each event window.

5.4.2 Return-Based Control Variables

In addition to the firm-characteristic controls, we include return-based controls for time-series momentum and short-term reversal to account for normal return dynamics that could otherwise confound the relationship between *PreCAR* and *PostCAR*.

Momentum

Momentum refers to the tendency of recent winners to keep outperforming and recent losers to keep underperforming (Jegadeesh and Titman, 1993). This dynamic is important to control because firms added (deleted) to the S&P 500 often experience strong price appreciation (depreciation) before the announcement. Greenwood and Sammon (2024) show that additions have experienced increasingly large positive 100-day pre-announcement abnormal returns, while deletions have exhibited increasingly negative ones. This implies that a larger fraction of index candidates are momentum stocks in recent decades. Kasch and Sarkar (2014) further find that the permanent index effect previously attributed to the index inclusion itself can instead be explained by momentum and is independent of index membership. These results suggest that omitting momentum could bias our estimates, as firms with strong recent performance may continue to outperform after inclusion, mechanically inflating post-announcement returns and biasing our *PreCAR* coefficient upward.

Following Kasch and Sarkar (2014), we include the daily raw return in the pre-inclusion year as a control for the firms' extraordinary pre-inclusion performance. Our main momentum control is therefore the time-series momentum signal, which captures positive predictability from a security's own past returns (see Moskowitz et al., 2012). This measure is particularly relevant in our setting given the strong changes in market value that precede index changes, making continuation in the same stock's price path the primary concern. Following Carhart (1997), we exclude the most recent month to avoid short-term reversal contamination. This control will be referred to as Ret^{-2-12} .

Short-Term Reversal

Short-term reversal reflects temporary price pressure or liquidity effects that lead to partial return reversion over short horizons (Jegadeesh, 1990; Lehmann, 1990). Controlling for this is important to avoid misattributing mechanical mean-reversion in returns with event-related predictability. In that case, our estimated *PreCAR* coefficient would be biased downward, overstating the degree of substitution between pre- and post-announcement returns.

We measure short-term reversal as the stock's return over the most recent month before the announcement, consistent with the convention in Carhart (1997), who excludes this period from the

momentum signal to avoid contamination. Controlling for short-term reversal in this way is consistent with e.g. [Konstantinidi \(2022\)](#), [Binsbergen and Ma \(2022\)](#) and [Atilgan \(2014\)](#). This control will be referred to as Ret^{-1} .

Overlap between PreCAR and Return-Based Controls

We mitigate the concern that our momentum and short-term reversal measures overlap with some of the *PreCAR* windows by estimating the control variable coefficients jointly on the full sample, where treated firms represent only about 0.4 percent of observations. This approach ensures that the coefficients on the control variables are not materially biased from event related behaviour of treated firms, and that the coefficient on *PreCAR* can be interpreted as the incremental effect of pre-announcement returns *beyond* that explained by these short-term factors. Nonetheless, the overlap introduces some multicollinearity, which increases the standard errors but does not bias the results.

Heterogeneity in Momentum and Reversal Exposures

As discussed in Section 5.3, estimating the momentum and short-term reversal controls on the full sample implicitly assumes that the relationship between each signal and pre-event returns is the same for treated and control firms. In other words, a one-unit increase in a momentum or reversal signal should have the same expected effect on returns for treated and control firms. While treated firms are expected to differ in their momentum and reversal signals, since additions are typically recent winners and deletions recent losers, our estimation requires them to not systematically differ in how these signals translate into returns. If this assumption is violated, e.g., if treated firms have systematically higher exposures to the momentum factor (*UMD*) than their control firms, then a single pooled coefficient on the momentum variable would be inaccurate. In that case, our regression might not fully remove the effect of momentum for treated firms, leaving some unaccounted bias, which would likely bias our *PreCAR* coefficient upward. Similarly, heterogeneous exposures to the short-term reversal factor (*STR*) would likely generate a downward bias.

While we consider this assumption to be reasonable *ex ante*, given that (i) both treated and control firms are highly liquid, operate under similar trading conditions and attract comparable levels of investor attention, (ii) there is no reason to believe that the S&P 500 Index Committee’s selection decisions are systematically related to firms’ exposures to factors such as momentum or reversal, we formally test its validity.

We obtain the *UMD* and *STR* factor returns from Kenneth R. French’s online data library. Each firm’s exposure to these factors, together with the three Fama-French factors, is estimated over a one-year window ending 20 trading days before the start of the main pre-announcement window (following [Aghaee, 2022](#)). We then test for equality of factor loadings between treated and control firms. The results, reported in Table A.14 in the Appendix, show that additions exhibit significantly higher loadings on the *UMD* factor relative to their estimation sample, while deletions exhibit significantly more negative *UMD* loadings. For *STR*, we find that deletions have significantly higher loadings than their estimation sample, while deletions have no significant differences compared to their estimation sample.

To mitigate potential bias arising from these differences, we include the estimated factor exposures directly in our regressions. We thereby draw from [Lou and Polk \(2012\)](#), in controlling for both the time-series momentum signal (i.e. prior returns), and the cross-sectional momentum exposure (i.e. sensitivity to the broader momentum factor) in the same regression. Although the *STR* exposures are balanced for additions, we include this control in the specification as well for completeness and

to maintain consistency across all model specifications. Furthermore, we interact each firm’s factor exposures with the respective signal controls Ret^{-2-12} and Ret^{-1} to allow the effects of these signals to vary proportionally with the firm’s own sensitivity to the corresponding factor.

5.5 Predictability-Interaction

While our main test examines whether pre-announcement returns substitute for post-announcement returns on average, the degree of substitution is likely to depend on how predictable an index change is. If an index change is easier for investors to anticipate, a larger share of the price adjustment should occur before the announcement. To explore this, we introduce two interaction variables that proxy for different dimensions of predictability: mechanical predictability, based on a firm’s *SizeRank* among S&P-eligible firms, and informational visibility, based on *Coverage*.

We measure mechanical predictability using each firm’s market-capitalization rank among all U.S.-traded firms (*SizeRank*). Greenwood and Sammon (2024) show that the Index Committee has gradually shifted toward adding larger firms, although there remains considerable randomness in which firms are chosen. This suggests that the rank variable provides some degree of predictability, as larger firms, other things equal, tend to have a higher likelihood of being added to the index. We measure *SizeRank* using data from 20 trading days before the start of the *PreCAR* window, so that it is ex ante relative to our event period and unaffected by potential price movements within the pre-announcement window. This allows us to test whether substitution between pre- and post-announcement returns is stronger for firms that are mechanically easier to predict.

We measure informational visibility using the number of analysts with active earnings forecasts for each firm, based on I/B/E/S data. Analyst coverage serves as a proxy for a firm’s visibility, since firms followed by more analysts tend to attract greater investor attention and have less information asymmetry (Frankel and Li, 2004). Higher *Coverage* might make index changes easier to anticipate, as such firms are more closely monitored by market participants. We therefore expect substitution between pre- and post-announcement returns to be stronger for firms with higher *Coverage*. *Coverage* is defined as in Section 5.4.1.

5.6 Model Specification

To investigate whether there is pre-emption of the index effect and whether it has increased over time, we structure our research models as follows. Equation (1) tests for pre-emption over the full sample period, proxied by a negative relationship between *PreCAR* and *PostCAR*. Equation (2) then examines how this relationship has changed over time. Thereafter, Equation (3) determines whether changes in the relationship are significant. Finally, Equation (4) explores how mechanical or visibility predictability interacts with pre-emption, proxied by market-capitalization rank and analyst coverage.

5.6.1 Baseline Regression

We begin by estimating a baseline regression across the full sample period between 1980 and 2020 to test whether pre-announcement returns substitute for announcement-window returns on average:

$$\text{PostCAR}_i = \alpha + \beta \text{PreCAR}_i + \theta' X_i + \varepsilon_i \quad (1)$$

where PostCAR_i is the cumulative abnormal return over the $[0, +1]$ window, PreCAR_i is the cu-

mulative abnormal return over the pre-announcement window [20, -1]. X_i are firm-characteristic and return-based controls defined in Section 5.4 and estimated together with the estimation sample defined in Section 5.3. This provides an overview of the general *PreCAR-PostCAR* relationship, where a negative β indicates predictability.

5.6.2 Era-Interaction Regression

Next, we examine how this relationship has evolved over time. We do this to examine whether anticipation has increased over time, which would indicate that anticipation may be a driver of declining index effect post 1990s (Greenwood and Sammon, 2024; Aghaee, 2022; Bennett et al., 2020). In line with Greenwood and Sammon (2024), we divide our sample into decade-based eras to capture the relationship of *PreCAR* to *PostCAR* over time. We also allow our controls to vary by decade:

$$\text{PostCAR}_i = \sum_e \beta_e (\text{PreCAR}_i \times D_e) + \sum_e \theta'_e (X_i \times D_e) + \text{FE}_{\text{year}(i)} + \varepsilon_i \quad (2)$$

where D_e are dummies for the decades, where $e \in \{1980s, 1990s, 2000s, 2010s\}$ and $\text{FE}_{\text{year}(i)}$ are year fixed effects. All other variables are defined as in Equation (1).

5.6.3 Wald Test

We then formally test whether the substitution coefficients differ across eras by using pairwise Wald tests. For each pair of eras e and e' , we test the null hypothesis $H_0 : \beta_e = \beta_{e'}$:

$$F_{(e,e')} = \frac{(\hat{\beta}_e - \hat{\beta}_{e'})^2}{\text{Var}(\hat{\beta}_e - \hat{\beta}_{e'})} \quad (3)$$

where β_e and $\beta_{e'}$ are the decade-specific coefficients.

5.6.4 Predictability-Interaction Regression

Finally, we explore what drives differences in substitution across firms by extending the model with interaction terms. We interact *PreCAR* with two predictability proxies: mechanical predictability (*SizeRank* among all US-traded firms) and informational visibility (*Coverage*). This allows us to test whether substitution is stronger for firms that are mechanically easier to anticipate (larger firms) or more visible to investors:

$$\begin{aligned} \text{PostCAR}_i = \sum_e \bigg[& \beta_e (\text{PreCAR}_i \times D_e) \\ & + \beta_{s,e} \underbrace{(\text{PreCAR}_i \times \text{SizeRank}_i \times D_e)}_{\text{mechanical predictability}} \\ & + \beta_{c,e} \underbrace{(\text{PreCAR}_i \times \text{Coverage}_i \times D_e)}_{\text{informational visibility}} \\ & + \theta'_e (X_i \times D_e) \bigg] + \text{FE}_{\text{year}(i)} + \varepsilon_i \end{aligned} \quad (4)$$

where β_s and β_c capture how the degree of mechanical predictability or visibility affects the relationship between pre-announcement and announcement returns. A more negative coefficient implies stronger substitution for that dimension. All other variables are defined as in Equation (2).

In conclusion, we study (i) the association between *PreCAR* and *PostCAR* in the pooled sample between 1980 and 2020; (ii) how this association has evolved between the decades; and (iii) whether firm-level predictability proxies amplify the association between *PreCAR* and *PostCAR*.

5.6.5 Alternative Pre-Announcement Windows

In addition to the regressions above, we examine the robustness of our findings by re-estimating the effects using alternative *PreCAR* windows of 10, 50, and 100 trading days before the index change announcement.

6 Empirical Results

In this section, we begin by presenting descriptive statistics for the main variables used in our analysis. We then report the results from the baseline regression examining the association between *PreCAR* and *PostCAR* in the pooled sample across eras. Next, we present the era-specific results and the corresponding Wald tests that assess whether the association between *PreCAR* and *PostCAR* differs across eras. We then analyze the interaction specifications, which evaluate whether predictability proxies strengthen or weaken this association. Finally, we assess the robustness of our findings by re-estimating the models using alternative pre-announcement windows.

6.1 Descriptive Statistics

We report descriptive statistics for all main variables used in the analysis. We begin by showing the summary statistics for the control variables for event firms over the full sample period in Table 1. We then summarize *PreCAR* and *PostCAR* in Table 2 by era and for the full sample, across market models and pre-announcement windows of 10, 20, 50, and 100 days. Table A.2 in the Appendix covers the descriptive statistics of our estimation sample for the full sample period. Table A.3 through A.6 in the Appendix report descriptive statistics for the event firms by era, and Table A.7 through A.10 provide the corresponding statistics for the estimation sample.

Table 1 presents the summary statistics for the control variables for our main sample for additions and deletions. We want to highlight two observations. First, the sample of additions is more than twice as large as that of deletions. This is driven by the exclusion of deletions that happened because of non-discretionary reasons, such as M&A. Second, additions are more than twice as large as deletions when *Size* is measured as market capitalization relative to the total S&P 500. This is expected given the S&P 500's asymmetric rules, where additions must meet eligibility criteria but deletions are discretionary.

Table 2 shows that there is a clear difference in pre- and post-announcement cumulative abnormal returns for additions and deletions. For additions, the average 20-day *PreCAR* is 1.4 to 1.9 percent and the *PostCAR* is 3.1 to 3.2 percent, depending on market model. For deletions, the corresponding averages are -5.7 to -5.8 percent and -3.4 to -3.5 percent, respectively.

For additions, Table 2 shows that the average *PreCAR* increases over the eras in all market models, with the exception of the 2000s to 2010s transition where this holds only for the 50 and 100 trading day windows. In contrast, the average *PostCAR* peaks in the 1990s and declines in the 2000s and 2010s. For deletions, Table 2 shows that the *PreCAR* development over the eras follow a similar trend as additions, however, the 2010s *PreCAR* are usually less negative than the 2000s. *PostCAR* peaks in

the 1990s and declines in the 2000s before disappearing in the 2010s, mirroring the pattern observed for additions. Figure 1 visualizes the *PreCAR* for additions and deletions across eras over the 100-day pre-announcement horizon, using CAPM abnormal returns. The *PostCAR* development for additions and deletions is visualized in Figure 2.

The cumulative abnormal return for additions and deletions, using the market-adjusted model, differs slightly from the results reported by Greenwood and Sammon (2024), though the differences are not economically large. These small differences arise because we employ CRSP as the market benchmark rather than the S&P 500, and because our sample composition differs partly to Greenwood and Sammon (2024) as they apply additional exclusions required to match observations with mutual fund holding data.

Correlation tables are reported in Table A.11 in the appendix. We also report variance inflation factor tests in Table A.12 in the appendix, which indicate that multicollinearity is not a concern.

Table 1: Summary Statistics for Control Variables

Additions						
	N	Mean	SD	Median	P25	P75
<i>Size</i>	722	0.0008	0.0005	0.0006	0.0005	0.0009
<i>Turnover</i>	722	0.0024	0.0070	0.0002	-0.0015	0.0036
<i>WZ</i>	722	0.0005	0.0004	0.0003	0.0002	0.0006
<i>Coverage</i>	722	11.5900	8.8334	12.0000	1.0000	18.0000
$Ret^{-2,-12}$	722	0.3816	0.5031	0.2593	0.0611	0.5657
Ret^{-1}	722	0.0220	0.0918	0.0209	-0.0362	0.0802
<i>UMD</i>	722	0.1499	0.5120	0.1173	-0.1767	0.4140
<i>STR</i>	722	0.0429	0.2938	0.0330	-0.1344	0.2173
$UMD \times Ret^{-2,-12}$	722	0.1195	0.3341	0.0280	-0.0201	0.1370
$STR \times Ret^{-1}$	722	0.0029	0.0286	0.0006	-0.0070	0.0105

Deletions						
	N	Mean	SD	Median	P25	P75
<i>Size</i>	286	0.0003	0.0005	0.0002	0.0001	0.0003
<i>Turnover</i>	286	0.0072	0.0122	0.0020	-0.0009	0.0116
<i>WZ</i>	286	0.0008	0.0009	0.0006	0.0003	0.0010
<i>Coverage</i>	286	7.8252	8.6199	5.0000	0.0000	14.0000
$Ret^{-2,-12}$	286	-0.1459	0.3743	-0.1622	-0.4191	0.1221
Ret^{-1}	286	-0.0429	0.1351	-0.0214	-0.1163	0.0426
<i>UMD</i>	286	-0.5036	0.6996	-0.4047	-0.8548	-0.0258
<i>STR</i>	286	0.1222	0.4204	0.0420	-0.1191	0.3172
$UMD \times Ret^{-2,-12}$	286	0.1843	0.3936	0.0453	-0.0177	0.2946
$STR \times Ret^{-1}$	286	-0.0084	0.0584	-0.0014	-0.0189	0.0073

Notes: This table presents summary statistics for the event firms, covering all firm-characteristic and return-based control variables used in the empirical analysis. Firm-characteristic control variables include firm size (*Size*), measured as the firm's market capitalization relative to the total market capitalization of the S&P 500 on day $[-40]$; past turnover (*Turnover*), defined as average daily trading volume divided by shares outstanding over the calendar month ending at $[-40]$, adjusted by the value-weighted market average; arbitrage risk (*WZ*), defined as the variance of CAPM regression residuals over the year ending at $[-40]$; and analyst coverage (*Coverage*), measured as the number of analysts covering the stock in the most recent earnings report available at $[-40]$. Return-based control variables capture both return patterns and factor exposures. Time-series momentum ($Ret^{-2,-12}$) is measured as the stock's cumulative return over the prior calendar year excluding the most recent month, anchored relative to the announcement date by counting the window back from day -1 . Short-term reversal (Ret^{-1}) is measured as the stock's return during the most recent calendar month ending at day -1 . Factor exposures to the *UMD* momentum factor and the *STR* short-term reversal factor are estimated over a one-year window ending 20 trading days before the beginning of the main pre-announcement window. To allow the predictive effects of return signals to vary with a firm's sensitivity to the corresponding factors, we additionally include the interaction terms $UMD \times Ret^{-2,-12}$ and $STR \times Ret^{-1}$.

Table 2: PreCAR and PostCAR by Era and Market Model

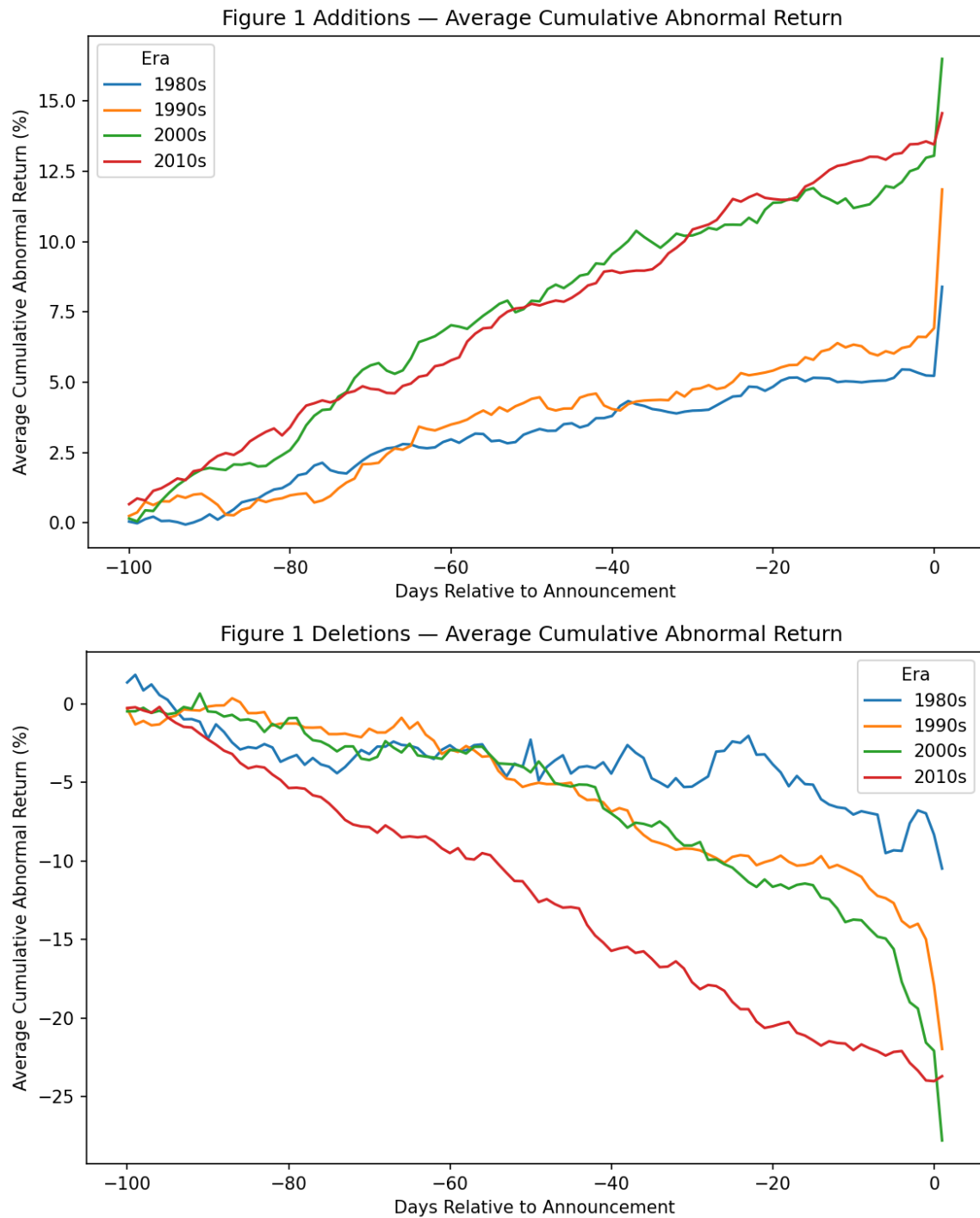
CAPM							
Time relative to announcement			Mean CAR (%)				
			1980s	1990s	2000s	2010s	Full sample
Additions							
-100	-1		5.318	5.930	12.802	13.047	9.462
-50	-1		1.976	2.492	5.327	5.764	3.957
-20	-1		0.679	1.290	2.121	1.891	1.509
-10	-1		0.206	0.406	1.680	0.829	0.824
0	1		3.257	5.145	3.486	1.010	3.163
Deletions							
-100	-1		-6.247	-13.345	-21.997	-23.634	-19.268
-50	-1		-4.496	-8.004	-16.996	-12.077	-12.139
-20	-1		-5.238	-3.872	-10.222	-2.715	-5.728
-10	-1		-1.396	-3.231	-7.713	-1.908	-4.040
0	1		-3.545	-6.904	-5.925	0.385	-3.498
FF3							
Time relative to announcement			Mean CAR (%)				
			1980s	1990s	2000s	2010s	Full sample
Additions							
-100	-1		5.497	6.842	11.060	12.105	8.957
-50	-1		2.123	2.875	4.978	5.312	3.863
-20	-1		0.454	1.572	2.188	1.738	1.486
-10	-1		0.115	0.462	1.710	0.706	0.790
0	1		3.196	5.206	3.331	1.005	3.112
Deletions							
-100	-1		-8.277	-12.542	-25.263	-18.340	-18.484
-50	-1		-4.401	-8.014	-17.819	-9.751	-11.545
-20	-1		-5.113	-3.804	-10.502	-2.577	-5.744
-10	-1		-1.479	-3.120	-7.996	-1.367	-3.923
0	1		-3.622	-6.882	-5.743	0.119	-3.540

(continued on next page)

FF5							
Time relative to announcement			Mean CAR (%)				
			1980s	1990s	2000s	2010s	Full sample
Additions							
-100	-1		5.273	8.045	11.051	12.325	9.182
-50	-1		1.841	3.502	5.054	5.621	4.005
-20	-1		0.279	1.803	2.075	1.697	1.441
-10	-1		-0.047	0.488	1.549	0.599	0.679
0	1		3.218	5.286	3.320	1.001	3.129
Deletions							
-100	-1		-6.925	-12.790	-23.525	-18.526	-17.863
-50	-1		-3.318	-8.764	-17.209	-10.771	-11.733
-20	-1		-4.763	-3.920	-10.235	-2.890	-5.752
-10	-1		-1.364	-3.101	-7.545	-1.531	-3.817
0	1		-3.542	-6.934	-5.499	0.087	-3.471
MA							
Time relative to announcement			Mean CAR (%)				
			1980s	1990s	2000s	2010s	Full sample
Additions							
-100	-1		5.032	6.780	13.925	14.697	10.271
-50	-1		1.965	2.925	6.268	6.632	4.520
-20	-1		0.758	1.682	2.403	2.289	1.784
-10	-1		0.381	0.761	1.712	1.042	1.002
0	1		3.240	5.017	3.442	0.958	3.108
Deletions							
-100	-1		-3.870	-14.549	-24.368	-21.015	-19.047
-50	-1		-3.390	-8.232	-19.636	-10.222	-12.256
-20	-1		-5.688	-3.500	-11.244	-1.974	-5.778
-10	-1		-1.583	-3.217	-7.729	-1.343	-3.854
0	1		-3.486	-6.894	-5.747	0.368	-3.436

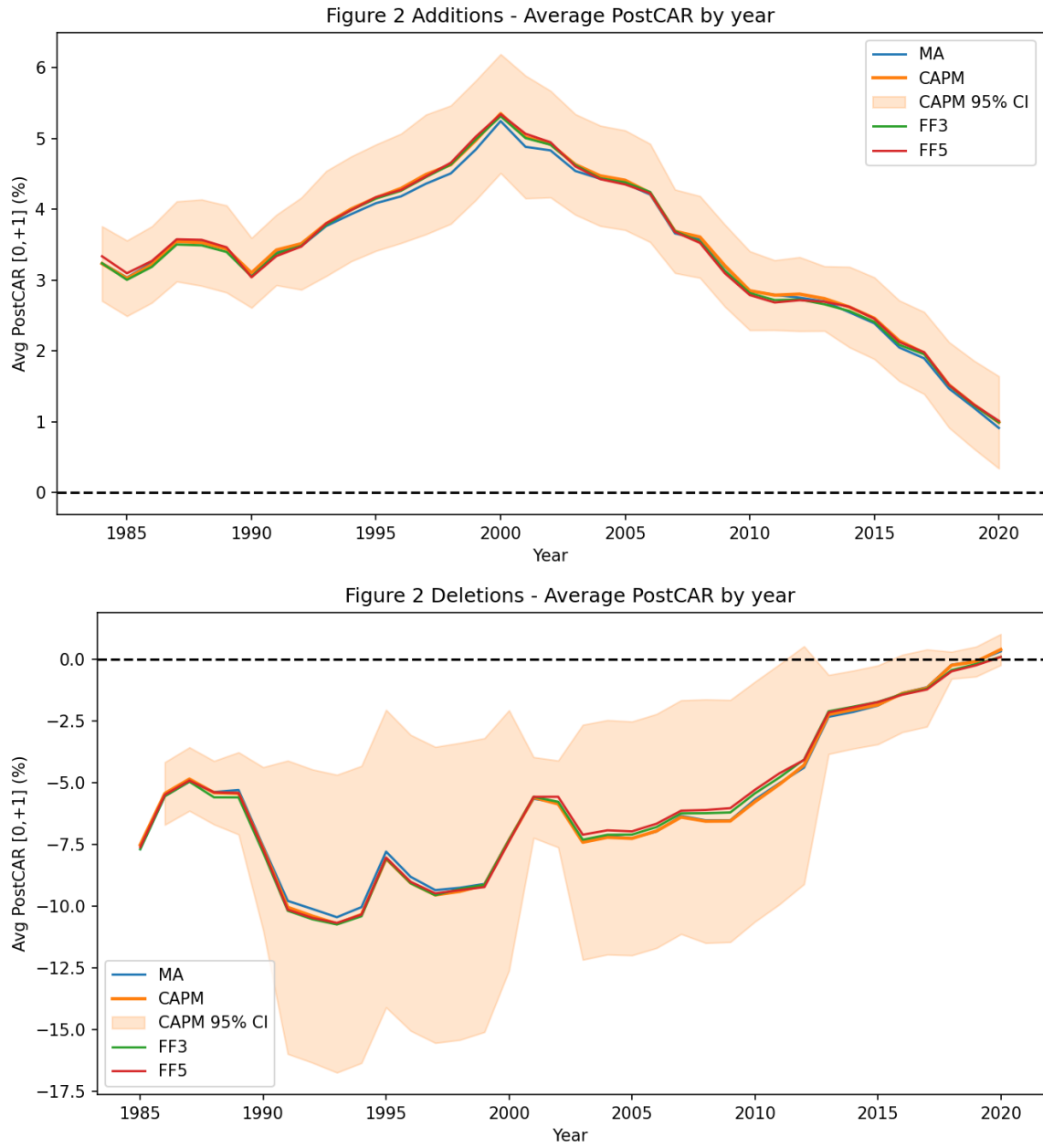
Notes: This table presents mean cumulative abnormal returns from $t = n$ to $t = -1$ relative to the announcement date, where n takes values -100 , -50 , -20 , and -10 . Results are reported separately for additions and deletions and shown under four market models: CAPM, FF3, FF5 and MA.

Figure 1: Cumulative abnormal return for S&P 500: Additions and Deletions



Notes: This figure shows the average cumulative abnormal returns (*PreCAR*), under CAPM, for S&P 500 additions (upper panel) and deletions (lower panel) across four decades: the 1980s, 1990s, 2000s, and 2010s. The horizontal axis measures trading days relative to the index announcement, with day 0 marking the announcement date. All return paths are normalized to start at zero 100 trading days before the announcement.

Figure 2: Post-Announcement Cumulative Abnormal Return: Additions and Deletions



Notes: This figure shows the average post-announcement cumulative abnormal return (*PostCAR*) for S&P500 additions (upper panel) and deletions (lower panel) from 1980 to 2020. Each line represents a 10-year moving average of the annual mean *PostCAR*. Results are shown for four market models: MA, CAPM, FF3, and FF5. Shaded areas represent 95% confidence intervals based on the CAPM.

6.2 Regression Analysis

In the first part of this section we present the baseline results (Equation (1)), followed by the era specific-results (Equation (2)). We then report the Wald tests results (Equation (3)), and finally the interaction results (Equation (4)).

6.2.1 Baseline Results

Table 3 and 4 show the results of Equation (1), which investigates the relationship between *PreCAR* and *PostCAR* over the full sample period from 1980 to 2020, using a pre-announcement window of 20 days.

For additions, we find a statistically significant negative association between *PreCAR* and *PostCAR* under the CAPM, with a coefficient of -0.008, indicating the presence of pre-emption. This means that a 1 percentage point increase in *PreCAR* is associated with a 0.008 percentage point lower *PostCAR*. The sign and significance of the coefficient are informative, but the magnitude is harder to interpret because the *PreCAR* and *PostCAR* windows differ in length and are therefore not directly comparable. To provide an economic sense of scale, we evaluate the impact at the average *PreCAR*. The average *PreCAR* is 1.51% (Table 2), implying a reduction in *PostCAR* by 0.012 percentage point. Given that the average *PostCAR* is 3.16% (Table 2), this corresponds to about 0.4% of the average *PostCAR*. Thus, the implied economic magnitude of this pre-emption appears small.

For the FF3, FF5 and MA, the association remains negative and statistically significant, with similar coefficients of -0.009, -0.008, and -0.008, respectively.

For deletions, the results are similar to those for additions. We find a statistically significant negative association between *PreCAR* and *PostCAR* under the CAPM, with a coefficient of -0.006, indicating the presence of pre-emption. This means that a 1 percentage point increase in *PreCAR* is associated with a 0.006 percentage point lower *PostCAR*. The average *PreCAR* is -5.73% (Table 2), implying a reduction in *PostCAR* by 0.032 percentage points. Given that the average *PostCAR* is -3.50% (Table 2), this corresponds to about 0.9% of the average *PostCAR*. Thus, the implied economic magnitude of this pre-emption appears small for deletions as well.

For the FF3, FF5 and MA models, the relationship remains negative and statistically significant, with coefficients of -0.008, -0.007, -0.010 respectively.

Overall, these findings provide evidence of pre-emption of the index effect, though the economic magnitude of the relationship is modest over the sample period. These results are consistent with hypothesis (i), which predicts a negative association between pre-announcement and announcement-day abnormal returns for S&P 500 additions and deletions.

Regarding the return-based control variables for additions, under the CAPM, Ret^{-2-12} , and its interaction with *UMD* are significant with the expected positive sign, while the exposure to *UMD* on its own is insignificant. None of the corresponding variables for *STR* are significant. For the FF3, FF5 and MA models, the signs are mostly similar.

Among firm characteristics, only *WZ* is significant and positive, while *Size*, *Turnover*, and *Coverage* are insignificant. These controls are, however, sensitive to market models.

Regarding the return-based control variables for deletions, under the CAPM, Ret^{-2-12} , is marginally significant with the expected positive sign. Its interaction with *UMD* is marginally significant and negative, likely reflecting the negative exposure to *UMD* by the deletions, thereby correcting for positive attenuation with the expected sign. *UMD* on its own is insignificant. Ret^{-1} is significant with an unexpected positive sign, while both its interaction with *STR* and *STR* itself are insignificant. Under the other market models, similar signs are observed, with the notable exceptions that momentum becomes insignificant in the FF3 and FF5 models, and the exposure to *STR* turns negative.

Among the firm characteristics, *Turnover* is significantly negative. *Size*, *WZ* and *Coverage* are insignificant. Under the other market models, the results are largely consistent, except that *Coverage* is significant and positive under the MA, and *Turnover* is not significant in the FF5 model.

Overall, the behaviour of the control variables shows some variation across the different return models, with a few coefficients shifting in significance when the benchmark changes. We do not view this as a major issue, since the main *PreCAR* coefficient remains stable across CAPM, FF3, FF5, and MA, which suggests that our main result is not sensitive to how abnormal returns are measured.

One result that warrants attention is the positive coefficient on the Ret^{-1} variable for deletions and its insignificance for additions. This is unexpected, as short-term reversal is typically negative. A likely explanation is that reversal unfolds over a longer horizon than the two-day post-event window used in our case. Earlier studies, such as [Lehmann \(1990\)](#) and [Jegadeesh \(1990\)](#), measure reversal over weekly or monthly intervals, and our short post-event window may be too narrow to capture the standard reversal effect. We nevertheless retain Ret^{-1} to control for recent return dynamics prior to the announcement that are not captured by the return model, ensuring that such movements are not absorbed by *PreCAR* and mistakenly attributed to the main effect, even though its sign differs from expectations.

Table 3: Baseline Regression: Additions

Variable	MA	CAPM	FF3	FF5
<i>PreCAR</i>	-0.008*** (0.000)	-0.008*** (0.000)	-0.009*** (0.000)	-0.008*** (0.000)
<i>Size</i>	-0.034 (0.920)	0.434 (0.187)	0.749** (0.022)	0.690** (0.034)
<i>Turnover</i>	-0.026* (0.059)	-0.005 (0.712)	-0.007 (0.576)	-0.007 (0.602)
<i>WZ</i>	-0.536** (0.045)	0.682*** (0.009)	0.225 (0.383)	0.356 (0.167)
<i>Coverage</i>	0.000* (0.069)	0.000 (0.997)	0.000 (0.850)	-0.000 (0.863)
$Ret^{-2,-12}$	0.001*** (0.000)	0.001*** (0.000)	0.001*** (0.000)	0.001*** (0.000)
Ret^{-1}	0.001 (0.523)	0.001 (0.137)	0.001 (0.309)	-0.000 (0.962)
<i>UMD</i>	0.000 (0.513)	-0.000 (0.136)	-0.000** (0.040)	-0.000 (0.278)
<i>STR</i>	-0.000 (0.844)	-0.000 (0.976)	-0.000 (0.539)	-0.000* (0.081)
$UMD \times Ret^{-2,-12}$	0.001** (0.036)	0.002*** (0.000)	0.001** (0.012)	0.001*** (0.003)
$STR \times Ret^{-1}$	0.004 (0.188)	0.002 (0.544)	0.001 (0.840)	-0.000 (0.908)

Notes: This table reports the results from Equation (1), which examines the relationship between *PreCAR* and *PostCAR* over the full sample period from 1980 to 2020. The dependent variable is the cumulative abnormal return over the $[0, +1]$ window (*PostCAR*), and the main independent variable is the cumulative abnormal return over the $[-20, -1]$ window (*PreCAR*). The results are reported using MA, CAPM, FF3, and FF5 as market models in the calculation of abnormal returns. Firm-characteristic control variables include firm size (*Size*), measured as the firm's market capitalization relative to the total market capitalization of the S&P 500 on day $[-40]$; past turnover (*Turnover*), defined as average daily trading volume divided by shares outstanding over the calendar month ending at $[-40]$, adjusted by the value-weighted market average; arbitrage risk (*WZ*), defined as the variance of CAPM regression residuals over the year ending at $[-40]$; and analyst coverage (*Coverage*), measured as the number of analysts covering the stock in the most recent earnings report available at $[-40]$. Return-based control variables capture both return patterns and factor exposures. Time-series momentum ($Ret^{-2,-12}$) is measured as the stock's cumulative return over the prior calendar year excluding the most recent month, anchored relative to the announcement date by counting the window back from day -1 . Short-term reversal (Ret^{-1}) is measured as the stock's return during the most recent calendar month ending at day -1 . Each firm's exposure to the momentum (*UMD*) factor and the short-term reversal (*STR*) factor is estimated, together with the FF3 and FF5 factors, over a one-year window ending 20 trading days before the beginning of the main pre-announcement window. To allow the predictive effects of return signals to vary with a firm's sensitivity to the corresponding factors, we additionally include the interaction terms $UMD \times Ret^{-2,-12}$ and $STR \times Ret^{-1}$. All control variables are estimated together with the estimation sample defined in Section 5.3, with all variables winsorized at the 2.5% level and all firm characteristics demeaned. Parentheses report p-values computed using robust standard errors, and ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Table 4: Baseline Regression: Deletions

Variable	MA	CAPM	FF3	FF5
<i>PreCAR</i>	-0.010*** (0.000)	-0.006*** (0.000)	-0.008*** (0.000)	-0.007*** (0.000)
<i>Size</i>	-0.533 (0.287)	-0.612 (0.215)	-0.365 (0.455)	-0.303 (0.531)
<i>Turnover</i>	-0.055*** (0.002)	-0.036** (0.037)	-0.028* (0.092)	-0.016 (0.320)
<i>WZ</i>	-0.261 (0.487)	-0.129 (0.728)	-0.319 (0.381)	-0.173 (0.631)
<i>Coverage</i>	0.000** (0.013)	0.000 (0.101)	0.000 (0.214)	0.000 (0.176)
$Ret^{-2,-12}$	0.001** (0.020)	0.001* (0.054)	0.000 (0.324)	0.000 (0.291)
Ret^{-1}	0.006*** (0.000)	0.003** (0.017)	0.003** (0.011)	0.004*** (0.003)
<i>UMD</i>	0.001** (0.011)	0.000 (0.476)	0.000 (0.279)	0.000 (0.687)
<i>STR</i>	0.001** (0.023)	0.000 (0.675)	-0.001* (0.072)	-0.001* (0.085)
$UMD \times Ret^{-2,-12}$	-0.001* (0.085)	-0.001* (0.055)	-0.001 (0.142)	-0.000 (0.708)
$STR \times Ret^{-1}$	0.003 (0.467)	0.003 (0.531)	0.006 (0.144)	0.009** (0.038)

Notes: This table reports the results from Equation (1), which examines the relationship between *PreCAR* and *PostCAR* over the full sample period from 1980 to 2020. The dependent variable is the cumulative abnormal return over the $[0, +1]$ window (*PostCAR*), and the main independent variable is the cumulative abnormal return over the $[-20, -1]$ window (*PreCAR*). The results are reported using MA, CAPM, FF3, and FF5 as market models in the calculation of abnormal returns. Firm-characteristic control variables include firm size (*Size*), measured as the firm's market capitalization relative to the total market capitalization of the S&P 500 on day $[-40]$; past turnover (*Turnover*), defined as average daily trading volume divided by shares outstanding over the calendar month ending at $[-40]$, adjusted by the value-weighted market average; arbitrage risk (*WZ*), defined as the variance of CAPM regression residuals over the year ending at $[-40]$; and analyst coverage (*Coverage*), measured as the number of analysts covering the stock in the most recent earnings report available at $[-40]$. Return-based control variables capture both return patterns and factor exposures. Time-series momentum ($Ret^{-2,-12}$) is measured as the stock's cumulative return over the prior calendar year excluding the most recent month, anchored relative to the announcement date by counting the window back from day -1 . Short-term reversal (Ret^{-1}) is measured as the stock's return during the most recent calendar month ending at day -1 . Each firm's exposure to the momentum (*UMD*) factor and the short-term reversal (*STR*) factor is estimated, together with the FF3 and FF5 factors, over a one-year window ending 20 trading days before the beginning of the main pre-announcement window. To allow the predictive effects of return signals to vary with a firm's sensitivity to the corresponding factors, we additionally include the interaction terms $UMD \times Ret^{-2,-12}$ and $STR \times Ret^{-1}$. All control variables are estimated together with the estimation sample defined in Section 5.3, with all variables winsorized at the 2.5% level and all firm characteristics demeaned. Parentheses report p-values computed using robust standard errors, and ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

6.2.2 Era-Interaction Results

Next, we turn to the development of the relationship between *PreCAR* and *PostCAR* over time. Table 5 presents the results from Equation (2) for additions and deletions and Table A.15 through A.22 in the Appendix presents the results of the control variables from Equation (2).

For additions, we find evidence of pre-emption in the 1980s, no evidence in the 1990s, and partial evidence in the 2000s and 2010s, with the latter being marginally significant.

Under the CAPM, we find that the *PreCAR* is negative and statistically significant during the 1980s, with a coefficient of -0.066. For the 1990s, the coefficient is positive and insignificant. Further, for the 2000s, the coefficient is negative and significant, with a coefficient of -0.047. In the 2010s, the coefficient remains negative and is marginally significant, at -0.076.

When we use the FF3, FF5, and MA models, the overall pattern of coefficients is similar, but the statistical significance is generally weaker. Under the FF3 model, the coefficient is significant only in the 1980s (-0.063) and marginally significant in the 2010s (-0.077), but not significant in the 1990s or 2000s. Under the FF5 model, the coefficient is only significant in the 1980s (-0.065) and marginally significant in the 2000s (-0.039). Under MA returns, the coefficient is significant in the 1980s (-0.060) and 2000s (-0.067), but not in the 1990s or 2010s.

In terms of magnitudes, the significant coefficients are substantially larger than in the full-sample estimates. For example, under CAPM, the baseline *PreCAR* beta is about -0.008, while in the era regression it is -0.047 in the 2000s and -0.076 in the 2010s, which is about six to nine times as large. In the 2010s, since the average *PreCAR* is 1.89% (Table 2), Equation (1) indicates that the average *PostCAR* decreases by 0.14 percentage points. Given that the average *PostCAR* is 1.01% (Table 2), this corresponds to about 14% of the average *PostCAR*. Thus, the implied economic magnitude of this pre-emption is non-negligible.

For deletions, we observe no statistically significant relationship between *PreCAR* and *PostCAR* in any decade or market model. The coefficients are positive for the 1980s and 1990s across models, negative in the 2000s, and mixed in the 2010s, but none are statistically significant. Overall, we find no evidence of pre-emption for deletions in any decade.

The majority of control coefficients are statistically insignificant and some display instability across eras and market models, as shown in Table A.15 through A.22. In particular, the momentum control is negative and significant in the 2010s under the CAPM and MA, which differs from the pooled sample, where it had a positive relation with post announcement returns. We do not have a good explanation for this pattern. However, when we re-estimate the model using a pooled control specification across eras, in which the controls are constrained to have a single coefficient common to all eras as in Regression (1), the estimated *PreCAR* coefficient remains very similar in both sign and magnitude, as reported in Table A.13 in the Appendix. This suggests that the era specific instability of the controls, including the unexpected sign on momentum, is not driving our main pre-emption result.

Table 5: Era Regression Results**Additions**

Decade	MA	CAPM	FF3	FF5
1980s	-0.060** (0.031)	-0.066** (0.014)	-0.063** (0.018)	-0.065** (0.013)
1990s	0.036 (0.244)	0.030 (0.293)	0.013 (0.648)	0.008 (0.764)
2000s	-0.067*** (0.000)	-0.047** (0.017)	-0.031 (0.187)	-0.039* (0.079)
2010s	-0.052 (0.205)	-0.076* (0.058)	-0.077* (0.060)	-0.057 (0.174)

Deletions

Decade	MA	CAPM	FF3	FF5
1980s	0.059 (0.452)	0.054 (0.482)	0.018 (0.801)	0.012 (0.871)
1990s	0.017 (0.738)	0.019 (0.715)	0.021 (0.689)	0.018 (0.711)
2000s	-0.035 (0.383)	-0.028 (0.471)	-0.027 (0.504)	-0.036 (0.369)
2010s	0.013 (0.676)	0.001 (0.987)	-0.000 (0.988)	0.003 (0.904)

Notes: This table reports the results from Equation (2), which examines the development of the relationship between *PreCAR* and *PostCAR* over time. The model estimates decade-specific coefficients on *PreCAR* by interacting it with decade dummy variables (D_e) for the 1980s, 1990s, 2000s, and 2010s, while controlling for year fixed effects. The dependent variable is the cumulative abnormal return over the $[0, +1]$ window (*PostCAR*), and the main independent variable is the cumulative abnormal return over the $[-20, -1]$ window (*PreCAR*). The results are reported using MA, CAPM, FF3, FF5, as market models in the calculation of abnormal returns. The specification includes the same firm-characteristic and return-based control variables as defined in Table 3. This set comprises firm size (*Size*), past turnover (*Turnover*), arbitrage risk (*WZ*), analyst coverage (*Coverage*), time-series momentum ($Ret^{-2, -12}$), short-term reversal (Ret^{-1}), factor exposures (*UMD*, *STR*), and the interaction terms between return signals and the corresponding factor loadings ($UMD \times Ret^{-2, -12}$, $STR \times Ret^{-1}$). All control variables are estimated together with the estimation sample defined in Section 5.3, with all variables winsorized at the 2.5% level and all firm characteristics demeaned. Parentheses report p-values computed using robust standard errors, and ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

6.2.3 Wald Test

Given the differences in the sign of the *PreCAR* coefficient across eras, we next conduct Wald tests to assess whether the *PreCAR* coefficient differs significantly over time. Table 6 presents the results from Equation (3) for additions and deletions.

For additions, we find that the *PreCAR* coefficient becomes significantly more negative from the 1990s to both the 2000s and 2010s under the CAPM. This pattern indicates an increase in pre-emption of the index effect over time, consistent with the timing of the documented decline in the announcement-window index effect. The FF3 and FF5 specifications exhibit similar directional results but weaker statistical significance, showing no significant differences after the 1990s, except for a decrease from the 1990s to the 2010s being marginally significant under the FF3. Under MA returns, the decrease from the 1990s to the 2000s is significant, and to the 2010s is marginally significant.

Overall, we find partial evidence of a significant increase in pre-emption between the 1990s and the later decades.

For deletions, we find no statistically significant differences across decades under any of the market models. Thus, we do not observe changes that would indicate that pre-emption has contributed to the disappearance of the index effect for deletions.

Table 6: Wald Test

Additions				
Comparison	MA	CAPM	FF3	FF5
2000s vs. 1990s	-0.103*** (0.004)	-0.077** (0.026)	-0.043 (0.233)	-0.047 (0.187)
2010s vs. 2000s	0.015 (0.743)	-0.029 (0.515)	-0.046 (0.324)	-0.018 (0.705)
2010s vs. 1990s	-0.088* (0.087)	-0.106** (0.031)	-0.090* (0.071)	-0.065 (0.196)
1990s vs. 1980s	0.096** (0.021)	0.096** (0.014)	0.076* (0.050)	0.073* (0.057)
2000s vs. 1980s	-0.007 (0.836)	0.019 (0.561)	0.033 (0.358)	0.026 (0.447)
2010s vs. 1980s	0.008 (0.873)	-0.010 (0.844)	-0.014 (0.777)	0.008 (0.868)
Deletions				
Comparison	MA	CAPM	FF3	FF5
2000s vs. 1990s	-0.052 (0.423)	-0.047 (0.466)	-0.049 (0.468)	-0.054 (0.392)
2010s vs. 2000s	0.047 (0.344)	0.029 (0.569)	0.027 (0.593)	0.039 (0.414)
2010s vs. 1990s	-0.004 (0.942)	-0.018 (0.763)	-0.022 (0.721)	-0.015 (0.788)
1990s vs. 1980s	-0.042 (0.653)	-0.036 (0.701)	0.003 (0.974)	0.006 (0.944)
2000s vs. 1980s	-0.094 (0.287)	-0.083 (0.340)	-0.046 (0.583)	-0.048 (0.567)
2010s vs. 1980s	-0.046 (0.582)	-0.054 (0.520)	-0.019 (0.811)	-0.009 (0.911)

Notes: This table reports the results from Equation (3), which formally tests for statistical differences in the relationship between *PreCAR* and *PostCAR* across decades. The model follows the specification in Equation (2), interacting *PreCAR* with decade dummy variables (D_e). The dependent variable is the cumulative abnormal return over the $[0, +1]$ window (*PostCAR*), and the main independent variable is the cumulative abnormal return over the $[-20, -1]$ window (*PreCAR*). The results are reported using MA, CAPM, FF3, and FF5 as market models in the calculation of abnormal returns. The specification includes the same firm-characteristic and return-based control variables as defined in Table 3. This set comprises firm size (*Size*), past turnover (*Turnover*), arbitrage risk (*WZ*), analyst coverage (*Coverage*), time-series momentum ($Ret^{-2, -12}$), short-term reversal (Ret^{-1}), factor exposures (*UMD*, *STR*), and the interaction terms between return signals and the corresponding factor loadings ($UMD \times Ret^{-2, -12}$, $STR \times Ret^{-1}$). Reported values show the estimated coefficient differences across decades. Parentheses report the corresponding p-values from Wald tests computed using robust standard errors, and ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

6.2.4 Predictability-Interaction Results

Finally, we explore how the relationship between *PreCAR* and *PostCAR* varies with firm-level predictability proxies by extending the model with interaction terms. Table 7 shows the results from Equation (4) for additions and deletions.

Our proxy for informational visibility (*Coverage*) has changed over time for additions. Under the CAPM, the interaction between *PreCAR* and analyst coverage is negative and statistically significant in the 1980s, implying that firms with greater *Coverage* show stronger pre-emption during this period. The interaction becomes insignificant in the 1990s and 2000s, and turns positive and significant in the 2010s. Under FF3, FF5 and MA the results are broadly similar. Overall, analyst coverage appears to strengthen pre-emption in the 1980s and weaken it in the 2010s.

Turning to our proxy for mechanical predictability (*SizeRank*), we again observe changes over time for additions. Under the CAPM, the interaction term is insignificant throughout the 1980s, 1990s, and 2000s, but becomes negative and significant in the 2010s. Because smaller firms receive higher (worse) size ranks, the negative interaction implies that smaller firms exhibit stronger pre-emption, whereas larger firms weaken the effect. Under FF3, FF5 and MA the results are similar.

For deletions, the interaction results show limited statistical significance. Given the absence of significant *PreCAR* coefficients in Section 6.2.2-6.2.3, we place limited interpretive weight on these findings.

Table 7: Predictability-Interaction Results

Additions					Deletions				
<i>Coverage</i>					<i>Coverage</i>				
Era	MA	CAPM	FF3	FF5	Era	MA	CAPM	FF3	FF5
1980s	-0.006** (0.024)	-0.007*** (0.008)	-0.005* (0.074)	-0.006** (0.026)	1980s	-0.038*** (0.000)	-0.041*** (0.000)	-0.033*** (0.005)	-0.033** (0.011)
1990s	-0.002 (0.541)	-0.003 (0.314)	-0.004 (0.214)	-0.004 (0.182)	1990s	0.001 (0.884)	0.002 (0.849)	-0.000 (0.995)	0.003 (0.724)
2000s	-0.001 (0.439)	0.000 (0.961)	-0.002 (0.437)	-0.003 (0.192)	2000s	0.005 (0.295)	0.006 (0.257)	0.006 (0.254)	0.008* (0.075)
2010s	0.012*** (0.006)	0.010** (0.015)	0.008* (0.064)	0.009** (0.043)	2010s	0.001 (0.652)	0.001 (0.653)	0.003 (0.261)	0.002 (0.333)
<i>SizeRank</i>					<i>SizeRank</i>				
Era	MA	CAPM	FF3	FF5	Era	MA	CAPM	FF3	FF5
1980s	0.000 (0.369)	0.000 (0.318)	0.000 (0.584)	0.000 (0.632)	1980s	-0.000 (0.406)	-0.001 (0.124)	-0.001** (0.038)	-0.001* (0.081)
1990s	-0.001 (0.153)	-0.000 (0.582)	-0.000 (0.750)	-0.000 (0.860)	1990s	-0.000 (0.504)	-0.000 (0.482)	-0.000 (0.556)	-0.000 (0.536)
2000s	0.000 (0.319)	0.000 (0.746)	-0.000 (0.973)	0.000 (0.827)	2000s	0.000 (0.610)	0.000 (0.430)	0.000 (0.501)	0.000 (0.584)
2010s	-0.001*** (0.001)	-0.001*** (0.003)	-0.001*** (0.002)	-0.001*** (0.001)	2010s	-0.001 (0.326)	-0.001 (0.129)	-0.001 (0.164)	-0.001 (0.156)

Notes: This table reports the results from Equation (4), which extends the analysis by including interaction terms that capture cross-sectional differences in the relationship between *PreCAR* and *PostCAR*. The model interacts *PreCAR* with proxies for mechanical predictability (*SizeRank*) and informational visibility (*Coverage*), allowing the substitution effect to vary across firms and decades. The dependent variable is the cumulative abnormal return over the $[0, +1]$ window (*PostCAR*), and the main independent variable is the cumulative abnormal return over the $[-20, -1]$ window (*PreCAR*). The results are reported using MA, CAPM, FF3, and FF5 as market models in the calculation of abnormal returns. *SizeRank* represents mechanical predictability. Analyst coverage (*Coverage*) captures informational visibility. The interaction coefficients ($\beta_{s,e}$ and $\beta_{c,e}$) indicate how these characteristics moderate the relationship between pre-announcement and announcement-window returns. All control variables are estimated together with the estimation sample defined in Section 5.3, with all variables winsorized at the 2.5% level and all firm characteristics demeaned. Parentheses report p-values computed using robust standard errors, and ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

6.3 Alternative Pre-Announcement Windows

A key concern in this study relates to the timing of potential pre-emptive trading activity. In the main regressions, we use a 20-trading-day window prior to the index change, which is intended to balance the risk of capturing unrelated price movements with the likelihood that anticipatory trading occurs closer to the event. Because the timing of such trading is inherently uncertain, we extend this window and re-estimate Equations (1)-(4) using alternative pre-announcement windows of 10, 50, and 100 trading days before the index announcement. For simplicity, we focus on the CAPM result and note any meaningful differences in the other market models.

6.3.1 Alternative Pre-Announcement Windows – Baseline Results

In the baseline regression for the full sample period, we find a consistent negative relationship between *PreCAR* and *PostCAR* for additions across all window lengths and market models, as shown in Table 8. The magnitudes are of similar limited size as in the 20-day window.

For deletions, the relationship is also significantly negative across all window lengths, with limited magnitude. Hence, the baseline findings appear robust to the choice of event window for both additions and deletions, and the choice of market model.

Table 8: Alternative Pre-Announcement Windows - Baseline Results

Additions				
PreCAR	MA	CAPM	FF3	FF5
<i>t-10</i>	-0.018*** (0.000)	-0.016*** (0.000)	-0.016*** (0.000)	-0.014*** (0.000)
<i>t-50</i>	-0.006*** (0.000)	-0.006*** (0.000)	-0.005*** (0.000)	-0.003*** (0.000)
<i>t-100</i>	-0.003*** (0.000)	-0.003*** (0.000)	-0.002*** (0.000)	-0.001** (0.018)
Deletions				
PreCAR	MA	CAPM	FF3	FF5
<i>t-10</i>	-0.019*** (0.000)	-0.015*** (0.000)	-0.014*** (0.000)	-0.014*** (0.000)
<i>t-50</i>	-0.006*** (0.000)	-0.005*** (0.000)	-0.005*** (0.000)	-0.005*** (0.000)
<i>t-100</i>	-0.005*** (0.000)	-0.004*** (0.000)	-0.002*** (0.003)	-0.002*** (0.005)

Notes: This table reports the results from Equation (1) for the alternative pre-announcement windows analysis, in which the pre-announcement window used to construct *PreCAR* is varied across three alternative specifications of 10, 50, and 100 trading days before the index announcement. In each specification, *PreCAR* is defined as the cumulative abnormal return over the respective $[n, -1]$ window, where $n \in -10, -50, -100$. The dependent variable remains the cumulative abnormal return over the $[0, +1]$ window (*PostCAR*). Abnormal returns are calculated using MA, CAPM, FF3, and FF5 models. Firm-characteristic control variables are defined analogously to the baseline specification, with measurement ending at day $[n - 20]$, corresponding to 20 trading days before the start of each alternative pre-announcement window. Firm size (*Size*) is measured as the firm's market capitalization relative to total S&P 500 market capitalization on day $[n - 20]$. Past turnover (*Turnover*) is the average daily trading volume divided by shares outstanding over the calendar month ending at day $[n - 20]$, adjusted by the value-weighted market average. Arbitrage risk (*WZ*) is the variance of CAPM residuals over the year ending at day $[n - 20]$. Analyst coverage (*Coverage*) is the number of analysts covering the firm in the most recent earnings report available at $[n - 20]$. Return-based control variables are constructed using the same timing conventions as in the main specification. Time-series momentum ($Ret^{-2, -12}$) is measured as the cumulative return over the prior calendar year excluding the most recent month, anchored to the announcement date by counting the window back from day $[-1]$. Short-term reversal (Ret^{-1}) is the return during the most recent calendar month ending at day $[-1]$. Factor exposures to the *UMD* momentum factor, the *STR* short-term reversal factor, and the FF3 factors are estimated using a one-year rolling window ending 20 trading days before the beginning of each *PreCAR* window. To allow predictive return signals to vary with factor sensitivities, interaction terms $UMD \times Ret^{-2, -12}$ and $STR \times Ret^{-1}$ are included. All control variables are estimated together with the estimation sample defined in Section 5.3, with all variables winsorized at the 2.5% level and all firm characteristics demeaned. Parentheses report p-values computed using robust standard errors, and ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

6.3.2 Alternative Pre-Announcement Windows – Era-Interaction Results

When we estimate the model separately by era, the pattern of a more negative coefficient coinciding with the declining index effect remains significant for the longer windows but weakens for the shorter one, as shown in Table 9.

For the 1980s and 1990s, the coefficient is statistically insignificant across all windows and market models. In the shorter 10-day window, it is also insignificant for both the 2000s and 2010s.

For the 2000s, the coefficients are negative in the 50-day window under CAPM and MA, but not under the remaining market models, and are mostly insignificant in the 100-day window. In the 2010s, the coefficient is significantly negative for both the 50-day and 100-day windows across all market-model specifications, at least with marginal significance. Under the CAPM specification, for example, the coefficients are -0.073 and -0.051 in the 50-day and 100-day windows, respectively. Since the average *PreCAR* is 5.76% and 13.05% (Table 2), this indicates that the average *PostCAR* decreases by 0.42 and 0.66 percentage points. Given that the average *PostCAR* is 1.01% (Table 2), these effects correspond to roughly 42% and 65% of the average *PostCAR*, respectively. Thus, the economic magnitude of this pre-emption effect is larger than the 14% implied by the main 20-day window, as shown in Section 6.2.2.

6.3.3 Alternative Pre-Announcement Windows – Wald Test

Formal Wald tests are shown in Table A.23 in the Appendix and provide partial evidence that anticipatory trading has strengthened for additions in recent decades when longer pre-announcement windows are used, whereas shorter windows capture weaker effects.

Relative to the 1990s, the Wald tests show that the *PreCAR* coefficient becomes more negative in the following cases. For the 50-day window, this holds under the CAPM and MA in the 2000s, and across all market models in the 2010s, at least with marginal significance. For the 100-day window, this does not hold in the 2000s and for most market models, not in the 2010s.

For deletions, the results remain insignificant across nearly all decades and window lengths. Overall, there is no consistent evidence of increasing anticipatory behavior for deletions, in line with the findings from the main event window.

6.3.4 Alternative Pre-Announcement Windows – Predictability-Interaction Results

Table A.24 in the Appendix reports that the informational visibility interaction, proxied by analyst coverage, shows a broadly similar pattern as the main window, with a negative interaction in the 1980s, gradually moving to a positive interaction in the 2010s. However, significance varies across windows and market models. The mechanical predictability interaction, proxied by *SizeRank*, remains similar to the main window. It becomes negative and significant only in the 2010s, indicating stronger pre-emption for smaller firms whereas larger firms weaken the effect.

Table 9: Alternative Pre-Announcement Windows – Era-Interaction Results

Additions					Deletions				
Window	CAPM	FF3	FF5	MA	Window	CAPM	FF3	FF5	MA
1980s					1980s				
10	-0.051 (0.229)	-0.053 (0.197)	-0.065 (0.119)	-0.039 (0.352)	10	0.070 (0.408)	0.061 (0.457)	0.068 (0.429)	0.086 (0.336)
50	-0.026 (0.246)	-0.018 (0.421)	-0.014 (0.509)	-0.016 (0.505)	50	0.025 (0.504)	0.013 (0.728)	0.008 (0.838)	0.034 (0.382)
100	-0.008 (0.634)	-0.000 (0.992)	-0.003 (0.814)	-0.003 (0.841)	100	0.041 (0.197)	0.024 (0.481)	0.019 (0.563)	0.053* (0.092)
1990s					1990s				
10	0.021 (0.526)	0.010 (0.770)	0.005 (0.865)	0.025 (0.473)	10	-0.003 (0.972)	-0.013 (0.869)	-0.015 (0.854)	0.005 (0.956)
50	0.002 (0.907)	-0.002 (0.928)	0.000 (0.981)	-0.003 (0.896)	50	0.055* (0.084)	0.081*** (0.000)	0.076*** (0.000)	0.051 (0.108)
100	-0.004 (0.778)	-0.007 (0.568)	-0.010 (0.397)	-0.011 (0.376)	100	0.019 (0.398)	0.023 (0.328)	0.019 (0.413)	0.015 (0.516)
2000s					2000s				
10	0.019 (0.558)	0.003 (0.931)	-0.008 (0.799)	-0.013 (0.698)	10	-0.072 (0.184)	-0.068 (0.221)	-0.089 (0.106)	-0.072 (0.199)
50	-0.034*** (0.009)	-0.017 (0.265)	-0.019 (0.199)	-0.050*** (0.000)	50	0.025 (0.288)	0.017 (0.500)	0.016 (0.504)	0.012 (0.625)
100	-0.014 (0.171)	-0.002 (0.890)	-0.010 (0.392)	-0.019* (0.061)	100	0.017 (0.363)	0.017 (0.389)	0.014 (0.478)	0.002 (0.923)
2010s					2010s				
10	-0.047 (0.492)	-0.044 (0.515)	-0.001 (0.991)	-0.021 (0.759)	10	0.071 (0.140)	0.061 (0.155)	0.058 (0.190)	0.078* (0.091)
50	-0.073*** (0.008)	-0.076*** (0.006)	-0.058** (0.037)	-0.068** (0.015)	50	0.009 (0.658)	0.007 (0.723)	0.009 (0.645)	0.013 (0.544)
100	-0.051*** (0.007)	-0.045** (0.019)	-0.035* (0.075)	-0.045** (0.019)	100	0.015 (0.379)	0.011 (0.410)	0.012 (0.382)	0.015 (0.399)

Notes: This table reports the results from Equation (2) for the alternative pre-announcement windows analysis, in which the pre-announcement window used to construct *PreCAR* is alternately defined over the $[n, -1]$ window, where $n \in -10, -50, -100$. The model examines how the predictive relationship between *PreCAR* and *PostCAR* varies across decades by interacting *PreCAR* with decade dummy variables (D_e) for the 1980s, 1990s, 2000s, and 2010s, while controlling for year fixed effects. The dependent variable is the cumulative abnormal return over the $[0, +1]$ window (*PostCAR*). Abnormal returns are calculated using MA, CAPM, FF3, and FF5 models. The specification includes the same firm-characteristic and return-based control variables as defined in Table 3. This set comprises firm size (*Size*), past turnover (*Turnover*), arbitrage risk (*WZ*), analyst coverage (*Coverage*), time-series momentum ($Ret^{-2, -12}$), short-term reversal (Ret^{-1}), factor exposures (*UMD*, *STR*), and the interaction terms between return signals and the corresponding factor loadings ($UMD \times Ret^{-2, -12}$, $STR \times Ret^{-1}$). All control variables are estimated together with the estimation sample defined in Section 5.3, with all variables winsorized at the 2.5% level and all firm characteristics demeaned. Parentheses report p-values computed using robust standard errors, and ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

7 Discussion

This section interprets the empirical findings in light of the paper’s hypotheses and the broader literature. We begin by discussing the evidence for pre-emption and how it varies across eras and window lengths. We then evaluate the extent to which pre-emption may have contributed to the disappearance of the index effect. Thereafter, we consider interpretational considerations related to our research design. We subsequently examine the broader implications for passive investors and market efficiency. Finally, we explore potential reasons for the asymmetry between additions and deletions.

7.1 Evidence of Pre-Emption

Main Window

Across the full sample from 1980 to 2020 and using a 20-day *PreCAR* window, Equation (1) shows a statistically significant negative association between *PreCAR* and *PostCAR*. This is consistent with our proxy for pre-emption, supporting hypothesis (i). However, when evaluated at the average level of *PreCAR*, the implied economic magnitude of the relationship is small compared to the average *PostCAR* in the sample. Thus, the overall extent of pre-emption appears limited over the full period. This finding is not surprising, given the lower predictability of S&P 500 changes in earlier decades, as suggested by [Greenwood and Sammon \(2024\)](#) and [Petajisto \(2011\)](#).

We next examine how pre-emption develops over time, using the same window. Equation (2) shows no association between *PreCAR* and *PostCAR* in the 1990s, when the index effect is at its peak. The coefficient becomes significantly negative in the 2000s and remains negative, albeit marginally significant, in the 2010s, as the index effect declines and eventually disappears. For additions, this provides partial support for Hypothesis (ii).

For deletions, however, we find no corresponding evidence to support Hypothesis (ii). The disappearance of the index effect has been close to symmetric for additions and deletions. If anticipation was an important driver of the overall disappearance, the asymmetry we observe would be unexpected. Accordingly, the deletion results weaken the robustness of the interpretation that increased pre-emption contributed to the diminishing index effect. We discuss potential reasons for this asymmetry in Section 7.5. Given the lack of evidence across eras, window length, and market-model specifications for deletions, we henceforth focus on additions.

Moreover, even for additions, evidence from the 20-day window is not uniformly robust across market model specifications. The estimated relationship between *PreCAR* and *PostCAR* weakens under richer models (FF3, FF5), which may reflect either increased measurement error or limited economic strength of the underlying mechanism. This sensitivity tempers the strength of the evidence for Hypothesis (ii) in the main window. At the same time, given the substantial timing uncertainty inherent in S&P 500 index reconstitutions, it is natural to examine alternative *PreCAR* windows.

Timing and Window Length

When extending the pre-announcement window, the evidence for pre-emption generally strengthens. For instance, the 50-day window yields significant pre-emption in the 2010s across market models. In contrast, shortening the pre-announcement window eliminates the significance of the effect, suggesting that short windows miss part of the pre-emption.

We interpret this pattern as consistent with anticipatory trading that unfolds gradually. Traders

predicting S&P 500 changes face a two-dimensional problem: they must assess both the likelihood of eventual inclusion (selection predictability) and the timing of the Committee’s announcement (timing predictability). The selection component can become predictable well in advance for firms whose characteristics clearly satisfy index eligibility criteria. In contrast, the timing component is inherently uncertain, as approximately 90% of S&P 500 changes occur outside regular rebalancing cycles. Given this uncertainty, it is unlikely that price adjustment will be concentrated immediately before the announcement, implying that short windows may fail to capture pre-emption that has already occurred. For illustration, FactSet treats a forecast as correct if it identifies a change within a six-month window or approximately 125 trading days (Segerstrom, 2021). For comparison, the longest pre-announcement window we use is 100 days.

Timing uncertainty also helps explain our findings on mechanical predictability. We initially expected larger firms, i.e. those that are most mechanically predictable as index additions, to exhibit stronger measured pre-emption. Instead, in the 2010s, where evidence for pre-emption is strongest overall, we find that high-rank firms display weaker measured pre-emption. A plausible explanation is that for highly predictable candidates, much of the anticipatory trading occurs before our pre-announcement windows begin. If pre-emption takes place earlier, we observe lower *PostCAR* for these stocks without a correspondingly high *PreCAR* in our window, mechanically weakening our proxy for pre-emption. This interpretation is consistent with the idea in Greenwood and Sammon (2024) that index prediction became more systematic and transparent in the 2010s, making the earliest stages of anticipation particularly important.

Under this interpretation, the moderation by *SizeRank* does not reflect genuinely weaker pre-emption among large, mechanically predictable firms. Rather, *SizeRank* captures how predictable a stock is as a candidate for inclusion, but it does not capture how predictable the timing of that inclusion is. When selection predictability is high but timing predictability is low, anticipatory trading may occur well before our measurement window. We predict that if timing uncertainty is reduced, anticipatory trading should shift closer to the announcement, and the observed rank-based moderation should diminish. To test this, we restrict the sample to additions occurring on dates with at least one M&A-driven deletion (approximately 70% of additions) which provides a precursor signal of an imminent index change. In this subsample, the moderation attenuation of the size-rank interaction disappears, as shown in Table A.25 in the Appendix, consistent with anticipatory trading moving nearer to the announcement when a timing signal is present.

Taken together, these patterns provide suggestive evidence that the strength of pre-emption is sensitive to the timing dimension of predictability, and that our windows may understate pre-emption for the most mechanically predictable cases.

7.2 Pre-Emption and the Disappearance of the Index Effect

When considering the longer pre-announcement windows, the evidence for increased pre-emption over time for additions is robust. The simultaneous rise in pre-emption and decline in announcement-day abnormal returns is consistent with pre-emption contributing to the disappearance of the index effect. Evaluated at the average *PreCAR* levels, the implied economic magnitude appears non-negligible relative to 2010s *PostCAR*, ranging from 14% to 65% of the average *PostCAR* depending on window length².

²Magnitudes discussed in this section refers to CAPM figures.

However, compared with the historical decline of the index effect, pre-emption can only explain a smaller share of the change. First, relating our implied reduction of 0.14-0.67³ percentage points to the decline in *PostCAR* of 4.13 percentage points corresponds to about 3-15% of the total decline, indicating a partial contribution to the disappearing index effect. Second, Greenwood and Sammon (2024) show that the underlying demand shock associated with S&P 500 inclusions increased by about a factor of 1.96 from the late 1990s to 2010s, after adjusting for migrations. Under the standard demand-curve mechanism, where larger index-fund demand induces larger price pressure, we would expect the index effect to increase. The fact that the index effect has declined even as demand forces have increased implies that the downward pressures must be substantially stronger than the 4.13 percentage point decline we observe. This means that the forces driving the disappearance of the index effect are materially stronger than what the naïve decline suggests, which makes the contribution of pre-emption even smaller in relative terms.

In this sense, our findings both confirm and formalize the argument in Greenwood and Sammon (2024). They argue for a pronounced decline in the price-pressure multiplier M (where $PostCAR = M \times Demand$), consistent with improved market liquidity. Further, they argue that pre-emption cannot explain the decline in M , since even when adding the full 20-day *PreCAR* to the *PostCAR*, M still declines. One concern is that a 20-day window may miss earlier pre-emption, for which we find supporting evidence. However, we show that even when extending the *PreCAR* window up to 100 trading days, the implied pre-emption effect remains small in relation to the historical decline of the announcement effect.

7.3 Interpretation Considerations

Equilibrium outcome

A conceptual complication in assessing pre-emption together with the declining index effect is that the two forces are jointly determined. Pre-emption mechanically compresses announcement-day returns, but a smaller index effect in turn lowers the incentives for traders to pre-empt. In addition, other structural changes identified in the literature, such as rising liquidity and the growing share of index migrations, have independently reduced the index effect. Because traders' incentives depend on the net effect of all these forces, the observed level of pre-emption in any period should be viewed as an equilibrium outcome rather than a clean causal driver.

Viewed through this lens, the fact that pre-emption in the 2010s is of similar, or in some windows and market models, higher significance than that in the 2000s, despite much smaller announcement-day returns in the later decade, is informative. It suggests that anticipatory trading has become increasingly systematic, allowing pre-emption to persist even as the index effect has declined.

Informational Timing

A challenge in interpreting our proxy as pre-emption is isolating it from other forces that may also shift part of the index effect into the pre-announcement window. A potential confound is the timing of the informational component of the index effect. Historically, S&P 500 additions have reflected not only demand-based price pressure but also information about firm quality conveyed by the Committee's selection decision, as shown by Aghaee (2022). If price-relevant information is incorporated into prices at different times across firms, this heterogeneity can generate a negative relationship between *PreCAR* and *PostCAR*, even without anticipatory trading.

³0.14 percentage points corresponds to the 20-day window and 0.67 percentage points to the 100-day window.

Suppose that:

$$\begin{aligned} \text{PostCAR}_i &= (1 - a_i) \text{Demand}_i + (1 - b_i) \text{Information}_i, \\ \text{PreCAR}_i &= a_i \text{Demand}_i + b_i \text{Information}_i. \end{aligned}$$

where b_i represents the share of informational value incorporated before announcement. Variation in b_i across firms generates a negative *PreCAR-PostCAR* relationship that is observationally indistinguishable from pre-emption (variation in a_i). Thus, informational timing could contaminate our proxy for a_i ⁴.

Crucially, [Aghaee \(2022\)](#) shows that the informational component of index inclusions was substantial before 2000 but becomes negligible thereafter, when the index effect is dominated by mechanical demand flows. We therefore consider it unlikely that our main pre-emption results for additions post-2000 to be driven by heterogeneity of informational timing⁵.

This framework, however, helps in interpreting the surprising result that our pre-emption proxy is highly significant in the 1980s in our main window, when we would not have expected widespread anticipatory trading. [Aghaee \(2022\)](#) shows that the informational component was substantial in the 1990s, and similar evidence appears in earlier work such as [Cai \(2007\)](#), whose sample includes the 1980s. In such settings, informational timing would generate strong variation in b_i . Our proxy for informational visibility reinforces this interpretation. The 1980s is the only era in which higher analyst coverage is associated with a significantly more negative relationship between *PreCAR* and *PostCAR*. This pattern is consistent with informational timing: firms with greater analyst coverage likely had more information produced and disseminated before the announcement, increasing the share of the informational component incorporated early. Such heterogeneity would naturally generate a more negative *PreCAR-PostCAR* relationship. In contrast, firms with low analyst coverage would have little information incorporated prior to the announcement, leaving *PreCAR* uniformly small and *PostCAR* largely reflective of the entire informational component, with minimal variation attributable to informational timing.

7.4 Implications

We consider two areas of implications of our findings: passive index investors and market efficiency.

In the 2010s, announcement day abnormal returns for S&P 500 additions are very small. Taken at face value, this suggests that passive index funds no longer pay a meaningful premium at reconstitution⁶. However, our evidence of increased pre-emption qualifies this conclusion slightly. According to [Petajisto \(2011\)](#), pre-emption spreads demand over time, reducing effective-day price pressure compared to a scenario in which all demand arrives at once⁷. However, insofar as pre-emption is reflected

⁴The informational component of the index effect can reflect different sources, including certification, investor awareness as well as fundamental news about firm prospects. Any trading that anticipates this informational value is genuine pre-emption. Our concern instead arises when fundamental information is incorporated at different times for reasons unrelated to the index event. Such variation in informational timing can generate early price movements that imitate pre-emption in our *PreCAR-PostCAR* proxy.

⁵We re-estimate the baseline regression in Section 6.2.1 for the main *PreCAR* window and test excluding the 1980s, the 1990s, and both. As reported in Table A.26 in the Appendix, the results are unchanged in sign or significance for additions, and broadly stable for deletions, with only a slight reduction in significance in some market models.

⁶Our event window extends to two trading days after the announcement and does not include the effective date when most index funds rebalance. We therefore do not observe the exact price at which passive investors enter their positions.

⁷Although we do not measure effective-day prices, post-announcement returns capture the same mechanism; price movements after the announcement indicate how much of the demand shock has already been absorbed by pre-emptive trading.

in pre-announcement price increases, which is what our proxy captures, passive investors should still face a residual premium, only shifted earlier in time. Our measure of pre-emption, in this sense, represents a redistribution of abnormal returns that are no longer visible in the announcement-day effect.

In terms of market efficiency, our findings indicate that not all components of the index effect have disappeared to the extent *PostCAR* implies. The presence of pre-emption implies that some inefficiency remains relative to a setting where migrations and liquidity fully eliminate it. At the same time, our results do not imply that anticipation must persist in the future. As discussed in the equilibrium argument, pre-emption should decline as the residual index effect diminishes and incentives weaken. Nevertheless, in equilibrium, pre-emptive trading may continue to exploit the last portion of index-related mispricing that migration flows and liquidity provision do not eliminate.

7.5 Potential Reasons for Asymmetry Between Deletions and Additions

As mentioned, the asymmetry between additions of deletions weaken the robustness of the interpretation that increased pre-emption contributed to the diminishing index effect. However, the absence of an effect for deletions may be consistent with structural differences between additions and deletions, as well as the smaller sample size for deletions. While this exercise is more speculative, we below explore two possible reasons.

First, economic frictions may make anticipatory trading around deletions harder. Profiting from anticipatory trading around deletions requires taking and maintaining short positions, and a large literature shows that short selling is costly, constrained, and exposed to recall and funding risks (Ofek et al. (2004)). These frictions often limit arbitrage on the short side and can sustain return anomalies that do not persist on the long side, e.g. the accrual anomaly (Hirshleifer et al., 2011), the short-interest puzzle (Engelberg et al., 2018) and the distress puzzle (Kim et al., 2019). In the context of index changes, this implies that even if a deletion is predictable, the incentives for pre-event trading may be weaker than for additions. That short-selling costs create asymmetric effects between S&P 500 additions and deletions is also argued by Aghaee (2022), who shows that it creates asymmetric volatility. Timing uncertainty may amplify this, since holding a short position for an extended period amplifies its risks and costs, as shown by Ofek et al. (2004). Moreover, timing uncertainty itself may differ, since many additions are triggered by M&A-driven deletions, providing traders with an advance signal not available for discretionary deletions.

Second, statistical power is lower for deletions. After excluding non-discretionary events, the sample of discretionary deletions is much smaller than the addition sample. This reduces cross-sectional variation and makes modest pre-announcement effects difficult to detect, even if present.

8 Conclusion

This study examines whether the disappearance of the S&P 500 index effect can be partly explained by increasing predictability of index changes. More specifically, we investigate if pre-emption of the announcement day abnormal returns has increased concurrently with the declining index effect. We proxy this by whether higher pre-announcement abnormal returns (*PreCAR*) are associated with weaker post-announcement abnormal returns (*PostCAR*) for S&P 500 additions and deletions.

Using a sample of 722 additions and 286 deletions from 1980 to 2020, we find a statistically significant relationship between *PreCAR* and *PostCAR* across the full sample. This suggests that

some anticipatory trading has historically occurred, but the estimated economic magnitude is small relative to the size of the index effect over the same period.

When we examine this relationship across eras, we find partial evidence for increased pre-emption over time. For additions, pre-emption is absent during the 1990s, when the index effect reached its historical peak, but becomes stronger in the 2000s and remains present in the 2010s. This pattern aligns with the timing of the declining index effect and is consistent with increased pre-emption having contributed to the diminishing index effect. The results are, however, not uniformly robust, since under richer market models, the estimated pre-emption in later decades is weaker.

For deletions, we find no evidence of pre-emption in any era or specification. This asymmetry weakens the case for predictability as a general explanation for the disappearance of the index effect. We argue that two reasons possibly contribute to this pattern: (i) economic frictions from higher costs and risks on short positions, reducing incentives to pre-empt deletion events, and (ii) the smaller sample of discretionary deletions, which limits statistical power.

Using longer pre-announcement windows of 50 and 100 trading days yields stronger and more consistent evidence of pre-emption for additions, particularly in the 2010s, whereas a shorter 10-day window obscures the effect. This pattern is consistent with the two-dimensional prediction problem inherent in S&P index events: selection predictability of a firm’s eventual inclusion and timing predictability of the announcement. Given high timing uncertainty, price adjustment may unfold gradually over extended horizons rather than concentrating immediately before the announcement.

While we interpret the magnitude of pre-emption for additions to be non-negligible in comparison to the observed index effect in the 2010s, it appears small in comparison to the decrease of the index effect. Our results therefore align with contemporary contributions, most notably [Greenwood and Sammon \(2024\)](#), which concludes that predictability is, at most, a secondary contributor to the disappearing index effect.

We acknowledge that our paper has limitations. First, our paper relies on a proxy for pre-emption based on the relationship between *PreCAR* and *PostCAR*. Although this proxy is conceptually grounded, it remains an indirect measure. Second, our findings are sensitive to the choice of pre-announcement window, and there is no clear benchmark for the correct window length. Longer windows in our setting yield stronger evidence of pre-emption but also accumulate more unrelated return noise, whereas shorter windows reduce noise but can miss a substantial part of pre-emptive price adjustment. Third, the sample of discretionary deletions is much smaller than the sample of additions, which limits our statistical power, especially in the decade-level analyses.

We have two suggestions for future research. First, one could broaden the setting to include additional indices. For example, focusing on more mechanically predictable indices, such as the Russell 2000, would provide a useful setting for understanding how predictability shapes both the magnitude and timing of pre-emption. Second, a natural direction for future research is to investigate the sources of the asymmetry between additions and deletions. We speculate about these mechanisms, but a formal assessment of why pre-emption differs between additions and deletions would be a meaningful contribution.

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A Appendix

Table A.1: Sample Construction

	Additions Excluded	Additions Sample	Deletions Excluded	Deletions Sample
Full sample		933		933
(i) was newly listed (IPO), created via spin-off within ± 100 trading days	-55	878	-0	933
(ii) acquired or delisted for reasons other than an acquisition	-0	878	-619	314
(iii) acted as an acquirer in a merger within ± 100 trading days	-131	747	-10	304
Cannot be matched to I/B/E/S data, have missing returns or other control data around the index announcement	-25	722	-18	286
		722		286

Notes: This table details the exclusion criteria used to construct the event-firm sample for S&P 500 additions and deletions. The resulting sample includes 722 additions and 286 deletions.

Table A.2: Estimation Sample Descriptive Statistics

Additions						
	N	Mean	SD	Median	P25	P75
<i>Size</i>	155208	0.0006	0.0002	0.0005	0.0004	0.0007
<i>Turnover</i>	155208	0.0014	0.0062	-0.0004	-0.0020	0.0025
<i>WZ</i>	155208	0.0004	0.0005	0.0003	0.0002	0.0005
<i>Coverage</i>	155208	10.0641	8.1207	10.0000	1.0000	16.0000
$Ret^{-2,-12}$	155208	0.1674	0.3776	0.1181	-0.0735	0.3372
Ret^{-1}	155208	0.0058	0.0949	0.0074	-0.0478	0.0612
<i>UMD</i>	155208	-0.0496	0.4930	-0.0389	-0.3331	0.2375
<i>STR</i>	155208	0.0467	0.2829	0.0300	-0.1285	0.2073
$UMD \times Ret^{-2,-12}$	155208	0.0486	0.2078	0.0066	-0.0283	0.0781
$STR \times Ret^{-1}$	155208	0.0002	0.0267	0.0000	-0.0075	0.0078

Deletions						
	N	Mean	SD	Median	P25	P75
<i>Size</i>	82806	0.0006	0.0002	0.0005	0.0004	0.0007
<i>Turnover</i>	82806	0.0018	0.0066	-0.0003	-0.0020	0.0032
<i>WZ</i>	82806	0.0004	0.0005	0.0003	0.0002	0.0005
<i>Coverage</i>	82806	11.0891	8.2591	11.0000	3.0000	17.0000
$Ret^{-2,-12}$	82806	0.1499	0.3592	0.1122	-0.0795	0.3200
Ret^{-1}	82806	0.0076	0.0967	0.0107	-0.0466	0.0653
<i>UMD</i>	82806	-0.0398	0.4832	-0.0313	-0.3203	0.2385
<i>STR</i>	82806	0.0435	0.2757	0.0286	-0.1274	0.2002
$UMD \times Ret^{-2,-12}$	82806	0.0451	0.1922	0.0068	-0.0270	0.0746
$STR \times Ret^{-1}$	82806	0.0003	0.0263	0.0000	-0.0075	0.0079

Notes: This table presents summary statistics for the estimation sample for additions and deletions, covering all firm-characteristic and return-based control variables used in the empirical analysis. Firm-characteristic control variables include firm size (*Size*), measured as the firm's market capitalization relative to the total market capitalization of the S&P 500 on day $[-40]$; past turnover (*Turnover*), defined as average daily trading volume divided by shares outstanding over the calendar month ending at $[-40]$, adjusted by the value-weighted market average; arbitrage risk (*WZ*), defined as the variance of CAPM regression residuals over the year ending at $[-40]$; and analyst coverage (*Coverage*), measured as the number of analysts covering the stock in the most recent earnings report available at $[-40]$. Return-based control variables capture both return patterns and factor exposures. Time-series momentum ($Ret^{-2,-12}$) is measured as the stock's cumulative return over the prior calendar year excluding the most recent month, anchored relative to the announcement date by counting the window back from day -1 . Short-term reversal (Ret^{-1}) is measured as the stock's return during the most recent calendar month ending at day -1 . Factor exposures to the *UMD* momentum factor and the *STR* short-term reversal factor are estimated over a one-year window ending 20 trading days before the beginning of the main pre-announcement window. To allow the predictive effects of return signals to vary with a firm's sensitivity to the corresponding factors, we additionally include the interaction terms $UMD \times Ret^{-2,-12}$ and $STR \times Ret^{-1}$.

Table A.3: Event Firms Descriptive Statistics 1980s

Additions						
	N	Mean	SD	Median	P25	P75
<i>Size</i>	198	0.001	0.001	0.001	0.000	0.001
<i>Turnover</i>	198	0.000	0.002	-0.000	-0.001	0.001
<i>WZ</i>	198	0.000	0.000	0.000	0.000	0.000
<i>Coverage</i>	198	10.444	8.539	11.000	0.000	15.750
$Ret^{-2,-12}$	198	0.278	0.377	0.196	0.032	0.479
Ret^{-1}	198	0.016	0.089	0.010	-0.046	0.075
<i>UMD</i>	198	0.080	0.465	0.108	-0.237	0.397
<i>STR</i>	198	0.029	0.318	0.011	-0.175	0.218
$mom_ts \times \beta^{UMD}$	198	0.042	0.173	0.023	-0.024	0.094
$STR \times \beta^{STR}$	198	0.001	0.026	0.000	-0.009	0.009

Deletions						
	N	Mean	SD	Median	P25	P75
<i>Size</i>	32	0.001	0.002	0.000	0.000	0.001
<i>Turnover</i>	32	0.001	0.004	-0.001	-0.001	0.001
<i>WZ</i>	32	0.001	0.002	0.000	0.000	0.001
<i>Coverage</i>	32	4.594	5.690	1.500	0.000	10.000
$Ret^{-2,-12}$	32	0.081	0.350	0.127	-0.059	0.257
Ret^{-1}	32	0.031	0.182	0.024	-0.022	0.071
<i>UMD</i>	32	-0.294	0.872	-0.189	-0.840	0.192
<i>STR</i>	32	0.102	0.419	-0.023	-0.214	0.330
$mom_ts \times \beta^{UMD}$	32	0.221	0.586	0.045	-0.008	0.165
$STR \times \beta^{STR}$	32	0.029	0.120	-0.000	-0.006	0.013

Notes: This table presents summary statistics for the event firms in the 1980s, covering all firm-characteristic and return-based control variables used in the empirical analysis. Firm-characteristic control variables include firm size (*Size*), measured as the firm's market capitalization relative to the total market capitalization of the S&P 500 on day $[-40]$; past turnover (*Turnover*), defined as average daily trading volume divided by shares outstanding over the calendar month ending at $[-40]$, adjusted by the value-weighted market average; arbitrage risk (*WZ*), defined as the variance of CAPM regression residuals over the year ending at $[-40]$; and analyst coverage (*Coverage*), measured as the number of analysts covering the stock in the most recent earnings report available at $[-40]$. Return-based control variables capture both return patterns and factor exposures. Time-series momentum ($Ret^{-2,-12}$) is measured as the stock's cumulative return over the prior calendar year excluding the most recent month, anchored relative to the announcement date by counting the window back from day -1 . Short-term reversal (Ret^{-1}) is measured as the stock's return during the most recent calendar month ending at day -1 . Factor exposures to the *UMD* momentum factor and the *STR* short-term reversal factor are estimated over a one-year window ending 20 trading days before the beginning of the main pre-announcement window. To allow the predictive effects of return signals to vary with a firm's sensitivity to the corresponding factors, we additionally include the interaction terms $UMD \times Ret^{-2,-12}$ and $STR \times Ret^{-1}$.

Table A.4: Event Firms Descriptive Statistics 1990s

Additions						
	N	Mean	SD	Median	P25	P75
<i>Size</i>	142	0.001	0.001	0.001	0.001	0.001
<i>Turnover</i>	142	0.002	0.005	-0.000	-0.002	0.002
<i>WZ</i>	142	0.001	0.000	0.000	0.000	0.001
<i>Coverage</i>	142	12.209	8.568	13.000	6.500	18.750
$Ret^{-2,-12}$	142	0.429	0.541	0.289	0.081	0.614
Ret^{-1}	142	0.028	0.095	0.038	-0.039	0.096
<i>UMD</i>	142	0.139	0.565	0.141	-0.151	0.444
<i>STR</i>	142	0.130	0.326	0.133	-0.085	0.331
$mom_ts \times \beta^{UMD}$	142	0.118	0.353	0.035	-0.025	0.131
$STR \times \beta^{STR}$	142	0.009	0.033	0.005	-0.006	0.021

Deletions						
	N	Mean	SD	Median	P25	P75
<i>Size</i>	52	0.000	0.001	0.000	0.000	0.000
<i>Turnover</i>	52	0.000	0.003	-0.000	-0.002	0.001
<i>WZ</i>	52	0.001	0.000	0.001	0.000	0.001
<i>Coverage</i>	52	4.336	5.910	1.000	0.000	6.250
$Ret^{-2,-12}$	52	-0.070	0.346	-0.026	-0.363	0.167
Ret^{-1}	52	-0.018	0.081	-0.013	-0.049	0.023
<i>UMD</i>	52	-0.396	0.634	-0.275	-0.860	0.030
<i>STR</i>	52	0.029	0.493	-0.025	-0.263	0.303
$mom_ts \times \beta^{UMD}$	52	0.092	0.269	0.003	-0.026	0.169
$STR \times \beta^{STR}$	52	-0.006	0.048	-0.002	-0.011	0.005

Notes: This table presents summary statistics for the event firms in the 1990s, covering all firm-characteristic and return-based control variables used in the empirical analysis. Firm-characteristic control variables include firm size (*Size*), measured as the firm's market capitalization relative to the total market capitalization of the S&P 500 on day $[-40]$; past turnover (*Turnover*), defined as average daily trading volume divided by shares outstanding over the calendar month ending at $[-40]$, adjusted by the value-weighted market average; arbitrage risk (*WZ*), defined as the variance of CAPM regression residuals over the year ending at $[-40]$; and analyst coverage (*Coverage*), measured as the number of analysts covering the stock in the most recent earnings report available at $[-40]$. Return-based control variables capture both return patterns and factor exposures. Time-series momentum ($Ret^{-2,-12}$) is measured as the stock's cumulative return over the prior calendar year excluding the most recent month, anchored relative to the announcement date by counting the window back from day -1 . Short-term reversal (Ret^{-1}) is measured as the stock's return during the most recent calendar month ending at day -1 . Factor exposures to the *UMD* momentum factor and the *STR* short-term reversal factor are estimated over a one-year window ending 20 trading days before the beginning of the main pre-announcement window. To allow the predictive effects of return signals to vary with a firm's sensitivity to the corresponding factors, we additionally include the interaction terms $UMD \times Ret^{-2,-12}$ and $STR \times Ret^{-1}$.

Table A.5: Event Firms Descriptive Statistics 2000s

Additions						
	N	Mean	SD	Median	P25	P75
<i>Size</i>	211	0.001	0.000	0.001	0.000	0.001
<i>Turnover</i>	211	0.005	0.011	0.002	-0.002	0.008
<i>WZ</i>	211	0.001	0.001	0.000	0.000	0.001
<i>Coverage</i>	211	10.500	8.994	11.000	0.000	18.000
$Ret^{-2,-12}$	211	0.376	0.628	0.278	-0.038	0.611
Ret^{-1}	211	0.008	0.105	0.015	-0.048	0.069
<i>UMD</i>	211	0.197	0.645	0.066	-0.228	0.429
<i>STR</i>	211	0.026	0.262	0.005	-0.131	0.198
$mom_ts \times \beta^{UMD}$	211	0.206	0.627	0.018	-0.043	0.185
$STR \times \beta^{STR}$	211	0.002	0.028	-0.000	-0.008	0.009

Deletions						
	N	Mean	SD	Median	P25	P75
<i>Size</i>	96	0.000	0.001	0.000	0.000	0.000
<i>Turnover</i>	96	0.006	0.013	0.001	-0.002	0.009
<i>WZ</i>	96	0.001	0.001	0.001	0.000	0.002
<i>Coverage</i>	96	5.695	7.601	1.500	0.000	9.000
$Ret^{-2,-12}$	96	-0.187	0.449	-0.234	-0.548	0.132
Ret^{-1}	96	-0.094	0.176	-0.060	-0.168	0.020
<i>UMD</i>	96	-0.527	0.732	-0.395	-0.791	-0.025
<i>STR</i>	96	0.090	0.264	0.041	-0.091	0.273
$mom_ts \times \beta^{UMD}$	96	0.227	0.525	0.050	-0.019	0.326
$STR \times \beta^{STR}$	96	-0.016	0.057	-0.002	-0.024	0.007

Notes: This table presents summary statistics for the event firms in the 2000s, covering all firm-characteristic and return-based control variables used in the empirical analysis. Firm-characteristic control variables include firm size (*Size*), measured as the firm's market capitalization relative to the total market capitalization of the S&P 500 on day $[-40]$; past turnover (*Turnover*), defined as average daily trading volume divided by shares outstanding over the calendar month ending at $[-40]$, adjusted by the value-weighted market average; arbitrage risk (*WZ*), defined as the variance of CAPM regression residuals over the year ending at $[-40]$; and analyst coverage (*Coverage*), measured as the number of analysts covering the stock in the most recent earnings report available at $[-40]$. Return-based control variables capture both return patterns and factor exposures. Time-series momentum ($Ret^{-2,-12}$) is measured as the stock's cumulative return over the prior calendar year excluding the most recent month, anchored relative to the announcement date by counting the window back from day -1 . Short-term reversal (Ret^{-1}) is measured as the stock's return during the most recent calendar month ending at day -1 . Factor exposures to the *UMD* momentum factor and the *STR* short-term reversal factor are estimated over a one-year window ending 20 trading days before the beginning of the main pre-announcement window. To allow the predictive effects of return signals to vary with a firm's sensitivity to the corresponding factors, we additionally include the interaction terms $UMD \times Ret^{-2,-12}$ and $STR \times Ret^{-1}$.

Table A.6: Event Firms Descriptive Statistics 2010s

Additions						
	N	Mean	SD	Median	P25	P75
<i>Size</i>	171	0.001	0.000	0.001	0.000	0.001
<i>Turnover</i>	171	0.003	0.008	0.000	-0.002	0.004
<i>WZ</i>	171	0.000	0.000	0.000	0.000	0.000
<i>Coverage</i>	171	13.782	8.894	14.000	7.000	20.000
$Ret^{-2,-12}$	171	0.472	0.444	0.338	0.156	0.655
Ret^{-1}	171	0.041	0.085	0.034	-0.014	0.085
<i>UMD</i>	171	0.202	0.387	0.158	-0.011	0.390
<i>STR</i>	171	0.007	0.255	0.004	-0.123	0.193
$mom.ts \times \beta^{UMD}$	171	0.155	0.306	0.039	-0.000	0.204
$STR \times \beta^{STR}$	171	0.001	0.025	0.000	-0.005	0.008

Deletions						
	N	Mean	SD	Median	P25	P75
<i>Size</i>	106	0.000	0.000	0.000	0.000	0.000
<i>Turnover</i>	106	0.014	0.014	0.011	0.003	0.021
<i>WZ</i>	106	0.001	0.000	0.000	0.000	0.001
<i>Coverage</i>	106	12.350	9.271	13.000	5.000	18.000
$Ret^{-2,-12}$	106	-0.208	0.302	-0.234	-0.409	-0.014
Ret^{-1}	106	-0.029	0.117	-0.024	-0.113	0.048
<i>UMD</i>	106	-0.601	0.650	-0.498	-0.970	-0.134
<i>STR</i>	106	0.205	0.499	0.097	-0.076	0.421
$mom.ts \times \beta^{UMD}$	106	0.207	0.356	0.091	-0.001	0.297
$STR \times \beta^{STR}$	106	-0.007	0.057	-0.002	-0.029	0.006

Notes: This table presents summary statistics for the event firms in the 2010s, covering all firm-characteristic and return-based control variables used in the empirical analysis. Firm-characteristic control variables include firm size (*Size*), measured as the firm's market capitalization relative to the total market capitalization of the S&P 500 on day $[-40]$; past turnover (*Turnover*), defined as average daily trading volume divided by shares outstanding over the calendar month ending at $[-40]$, adjusted by the value-weighted market average; arbitrage risk (*WZ*), defined as the variance of CAPM regression residuals over the year ending at $[-40]$; and analyst coverage (*Coverage*), measured as the number of analysts covering the stock in the most recent earnings report available at $[-40]$. Return-based control variables capture both return patterns and factor exposures. Time-series momentum ($Ret^{-2,-12}$) is measured as the stock's cumulative return over the prior calendar year excluding the most recent month, anchored relative to the announcement date by counting the window back from day -1 . Short-term reversal (Ret^{-1}) is measured as the stock's return during the most recent calendar month ending at day -1 . Factor exposures to the *UMD* momentum factor and the *STR* short-term reversal factor are estimated over a one-year window ending 20 trading days before the beginning of the main pre-announcement window. To allow the predictive effects of return signals to vary with a firm's sensitivity to the corresponding factors, we additionally include the interaction terms $UMD \times Ret^{-2,-12}$ and $STR \times Ret^{-1}$.

Table A.7: Estimation Sample Descriptive Statistics 1980s

Additions						
	N	Mean	SD	Median	P25	P75
<i>Size</i>	42066	0.001	0.000	0.001	0.000	0.001
<i>Turnover</i>	42066	0.000	0.002	-0.001	-0.001	0.001
<i>WZ</i>	42066	0.000	0.000	0.000	0.000	0.000
<i>Coverage</i>	42066	8.043	8.004	7.000	0.000	14.000
$Ret^{-2,-12}$	42066	0.197	0.330	0.158	-0.034	0.373
Ret^{-1}	42066	0.010	0.082	0.007	-0.044	0.060
<i>UMD</i>	42066	-0.015	0.504	-0.008	-0.312	0.311
<i>STR</i>	42066	0.048	0.326	0.033	-0.165	0.246
$mom_ts \times \beta^{UMD}$	42066	0.044	0.182	0.007	-0.032	0.087
$STR \times \beta^{STR_{Rev}}$	42066	-0.001	0.026	-0.000	-0.009	0.008

Deletions						
	N	Mean	SD	Median	P25	P75
<i>Size</i>	13011	0.001	0.000	0.001	0.000	0.001
<i>Turnover</i>	13011	0.000	0.002	-0.001	-0.001	0.001
<i>WZ</i>	13011	0.000	0.000	0.000	0.000	0.000
<i>Coverage</i>	13011	8.991	8.302	9.000	0.000	15.000
$Ret^{-2,-12}$	13011	0.186	0.311	0.151	-0.033	0.359
Ret^{-1}	13011	0.012	0.082	0.011	-0.039	0.064
<i>UMD</i>	13011	-0.016	0.509	-0.006	-0.328	0.323
<i>STR</i>	13011	0.046	0.341	0.032	-0.177	0.255
$mom_ts \times \beta^{UMD}$	13011	0.033	0.172	0.006	-0.035	0.082
$STR \times \beta^{STR_{Rev}}$	13011	-0.001	0.027	-0.000	-0.010	0.008

Notes: This table presents summary statistics for the estimation sample in the 1980s, covering all firm-characteristic and return-based control variables used in the empirical analysis. Firm-characteristic control variables include firm size (*Size*), measured as the firm's market capitalization relative to the total market capitalization of the S&P 500 on day $[-40]$; past turnover (*Turnover*), defined as average daily trading volume divided by shares outstanding over the calendar month ending at $[-40]$, adjusted by the value-weighted market average; arbitrage risk (*WZ*), defined as the variance of CAPM regression residuals over the year ending at $[-40]$; and analyst coverage (*Coverage*), measured as the number of analysts covering the stock in the most recent earnings report available at $[-40]$. Return-based control variables capture both return patterns and factor exposures. Time-series momentum ($Ret^{-2,-12}$) is measured as the stock's cumulative return over the prior calendar year excluding the most recent month, anchored relative to the announcement date by counting the window back from day -1 . Short-term reversal (Ret^{-1}) is measured as the stock's return during the most recent calendar month ending at day -1 . Factor exposures to the *UMD* momentum factor and the *STR* short-term reversal factor are estimated over a one-year window ending 20 trading days before the beginning of the main pre-announcement window. To allow the predictive effects of return signals to vary with a firm's sensitivity to the corresponding factors, we additionally include the interaction terms $UMD \times Ret^{-2,-12}$ and $STR \times Ret^{-1}$.

Table A.8: Estimation Sample Descriptive Statistics 1990s

Additions						
	N	Mean	SD	Median	P25	P75
<i>Size</i>	33474	0.001	0.000	0.000	0.000	0.001
<i>Turnover</i>	33474	0.001	0.006	-0.001	-0.002	0.001
<i>WZ</i>	33474	0.000	0.000	0.000	0.000	0.001
<i>Coverage</i>	33474	9.751	8.003	10.000	0.000	16.000
$Ret^{-2,-12}$	33474	0.227	0.419	0.149	-0.039	0.381
Ret^{-1}	33474	0.009	0.097	0.008	-0.051	0.066
<i>UMD</i>	33474	-0.071	0.520	-0.058	-0.375	0.226
<i>STR</i>	33474	0.051	0.314	0.038	-0.152	0.239
$mom_ts \times \beta^{UMD}$	33474	0.042	0.253	0.003	-0.039	0.072
$STR \times \beta^{STR_{Rev}}$	33474	0.001	0.031	0.000	-0.009	0.010

Deletions						
	N	Mean	SD	Median	P25	P75
<i>Size</i>	17851	0.001	0.000	0.001	0.000	0.001
<i>Turnover</i>	17851	0.001	0.005	-0.001	-0.002	0.001
<i>WZ</i>	17851	0.000	0.000	0.000	0.000	0.000
<i>Coverage</i>	17851	10.067	7.961	11.000	0.000	16.000
$Ret^{-2,-12}$	17851	0.201	0.351	0.145	-0.028	0.357
Ret^{-1}	17851	0.022	0.090	0.019	-0.036	0.075
<i>UMD</i>	17851	-0.029	0.516	-0.029	-0.336	0.259
<i>STR</i>	17851	0.051	0.325	0.040	-0.158	0.247
$mom_ts \times \beta^{UMD}$	17851	0.044	0.219	0.004	-0.036	0.067
$STR \times \beta^{STR_{Rev}}$	17851	0.002	0.031	0.000	-0.009	0.011

Notes: This table presents summary statistics for the estimation sample in the 1990s, covering all firm-characteristic and return-based control variables used in the empirical analysis. Firm-characteristic control variables include firm size (*Size*), measured as the firm's market capitalization relative to the total market capitalization of the S&P 500 on day $[-40]$; past turnover (*Turnover*), defined as average daily trading volume divided by shares outstanding over the calendar month ending at $[-40]$, adjusted by the value-weighted market average; arbitrage risk (*WZ*), defined as the variance of CAPM regression residuals over the year ending at $[-40]$; and analyst coverage (*Coverage*), measured as the number of analysts covering the stock in the most recent earnings report available at $[-40]$. Return-based control variables capture both return patterns and factor exposures. Time-series momentum ($Ret^{-2,-12}$) is measured as the stock's cumulative return over the prior calendar year excluding the most recent month, anchored relative to the announcement date by counting the window back from day -1 . Short-term reversal (Ret^{-1}) is measured as the stock's return during the most recent calendar month ending at day -1 . Factor exposures to the *UMD* momentum factor and the *STR* short-term reversal factor are estimated over a one-year window ending 20 trading days before the beginning of the main pre-announcement window. To allow the predictive effects of return signals to vary with a firm's sensitivity to the corresponding factors, we additionally include the interaction terms $UMD \times Ret^{-2,-12}$ and $STR \times Ret^{-1}$.

Table A.9: Estimation Sample Descriptive Statistics 2000s

Additions						
	N	Mean	SD	Median	P25	P75
<i>Size</i>	47002	0.001	0.000	0.000	0.000	0.001
<i>Turnover</i>	47002	0.002	0.009	-0.000	-0.003	0.005
<i>WZ</i>	47002	0.001	0.001	0.000	0.000	0.001
<i>Coverage</i>	47002	9.799	7.196	10.000	3.000	15.000
$Ret^{-2,-12}$	47002	0.120	0.466	0.059	-0.171	0.300
Ret^{-1}	47002	-0.007	0.121	0.003	-0.064	0.062
<i>UMD</i>	47002	-0.047	0.525	-0.066	-0.356	0.229
<i>STR</i>	47002	0.044	0.240	0.031	-0.101	0.179
$mom_ts \times \beta^{UMD}$	47002	0.082	0.276	0.010	-0.027	0.102
$STR \times \beta^{STR_{Rev}}$	47002	-0.000	0.029	0.000	-0.007	0.008

Deletions						
	N	Mean	SD	Median	P25	P75
<i>Size</i>	26610	0.001	0.000	0.000	0.000	0.001
<i>Turnover</i>	26610	0.003	0.009	-0.000	-0.003	0.006
<i>WZ</i>	26610	0.001	0.001	0.000	0.000	0.001
<i>Coverage</i>	26610	10.016	7.318	10.000	3.000	15.000
$Ret^{-2,-12}$	26610	0.107	0.478	0.032	-0.203	0.307
Ret^{-1}	26610	-0.012	0.129	0.000	-0.075	0.064
<i>UMD</i>	26610	-0.061	0.518	-0.074	-0.361	0.195
<i>STR</i>	26610	0.039	0.226	0.029	-0.101	0.169
$mom_ts \times \beta^{UMD}$	26610	0.074	0.266	0.011	-0.026	0.101
$STR \times \beta^{STR_{Rev}}$	26610	-0.001	0.029	-0.000	-0.008	0.008

Notes: This table presents summary statistics for the estimation sample in the 2000s, covering all firm-characteristic and return-based control variables used in the empirical analysis. Firm-characteristic control variables include firm size (*Size*), measured as the firm's market capitalization relative to the total market capitalization of the S&P 500 on day $[-40]$; past turnover (*Turnover*), defined as average daily trading volume divided by shares outstanding over the calendar month ending at $[-40]$, adjusted by the value-weighted market average; arbitrage risk (*WZ*), defined as the variance of CAPM regression residuals over the year ending at $[-40]$; and analyst coverage (*Coverage*), measured as the number of analysts covering the stock in the most recent earnings report available at $[-40]$. Return-based control variables capture both return patterns and factor exposures. Time-series momentum ($Ret^{-2,-12}$) is measured as the stock's cumulative return over the prior calendar year excluding the most recent month, anchored relative to the announcement date by counting the window back from day -1 . Short-term reversal (Ret^{-1}) is measured as the stock's return during the most recent calendar month ending at day -1 . Factor exposures to the *UMD* momentum factor and the *STR* short-term reversal factor are estimated over a one-year window ending 20 trading days before the beginning of the main pre-announcement window. To allow the predictive effects of return signals to vary with a firm's sensitivity to the corresponding factors, we additionally include the interaction terms $UMD \times Ret^{-2,-12}$ and $STR \times Ret^{-1}$.

Table A.10: Estimation Sample Descriptive Statistics 2010s

Additions						
	N	Mean	SD	Median	P25	P75
<i>Size</i>	32666	0.001	0.000	0.001	0.000	0.001
<i>Turnover</i>	32666	0.003	0.008	0.001	-0.002	0.005
<i>WZ</i>	32666	0.000	0.000	0.000	0.000	0.000
<i>Coverage</i>	32666	13.413	8.746	14.000	6.000	19.000
$Ret^{-2,-12}$	32666	0.146	0.291	0.116	-0.041	0.290
Ret^{-1}	32666	0.012	0.076	0.013	-0.034	0.057
<i>UMD</i>	32666	-0.071	0.413	-0.024	-0.281	0.190
<i>STR</i>	32666	0.045	0.251	0.020	-0.112	0.176
$mom.ts \times \beta^{UMD}$	32666	0.026	0.133	0.005	-0.018	0.053
$STR \times \beta^{STR_{Rev}}$	32666	0.001	0.019	0.000	-0.005	0.006

Deletions						
	N	Mean	SD	Median	P25	P75
<i>Size</i>	25334	0.001	0.000	0.001	0.000	0.001
<i>Turnover</i>	25334	0.003	0.007	0.001	-0.002	0.005
<i>WZ</i>	25334	0.000	0.000	0.000	0.000	0.000
<i>Coverage</i>	25334	14.060	8.724	14.000	8.000	20.000
$Ret^{-2,-12}$	25334	0.147	0.266	0.125	-0.026	0.291
Ret^{-1}	25334	0.013	0.078	0.014	-0.036	0.060
<i>UMD</i>	25334	-0.033	0.418	-0.003	-0.260	0.227
<i>STR</i>	25334	0.041	0.255	0.020	-0.121	0.185
$mom.ts \times \beta^{UMD}$	25334	0.030	0.129	0.006	-0.019	0.057
$STR \times \beta^{STR_{Rev}}$	25334	0.001	0.020	0.000	-0.006	0.006

Notes: This table presents summary statistics for the estimation sample in the 2010s, covering all firm-characteristic and return-based control variables used in the empirical analysis. Firm-characteristic control variables include firm size (*Size*), measured as the firm's market capitalization relative to the total market capitalization of the S&P 500 on day $[-40]$; past turnover (*Turnover*), defined as average daily trading volume divided by shares outstanding over the calendar month ending at $[-40]$, adjusted by the value-weighted market average; arbitrage risk (*WZ*), defined as the variance of CAPM regression residuals over the year ending at $[-40]$; and analyst coverage (*Coverage*), measured as the number of analysts covering the stock in the most recent earnings report available at $[-40]$. Return-based control variables capture both return patterns and factor exposures. Time-series momentum ($Ret^{-2,-12}$) is measured as the stock's cumulative return over the prior calendar year excluding the most recent month, anchored relative to the announcement date by counting the window back from day -1 . Short-term reversal (Ret^{-1}) is measured as the stock's return during the most recent calendar month ending at day -1 . Factor exposures to the *UMD* momentum factor and the *STR* short-term reversal factor are estimated over a one-year window ending 20 trading days before the beginning of the main pre-announcement window. To allow the predictive effects of return signals to vary with a firm's sensitivity to the corresponding factors, we additionally include the interaction terms $UMD \times Ret^{-2,-12}$ and $STR \times Ret^{-1}$.

Table A.11: Correlation table

Additions	$PreCAR_{[-20,-1]}^{CAPM}$	$PostCAR_{[0,+1]}^{CAPM}$	<i>Size</i>	<i>Turnover</i>	<i>WZ</i>	<i>Coverage</i>	$Ret^{-2,-12}$	Ret^{-1}	<i>UMD</i>	<i>STR</i>	$UMD \times Ret^{-2,-12}$	$STR \times Ret^{-1}$
$PreCAR_{[-20,-1]}^{CAPM}$	1.000	-0.147	-0.201	0.057	0.133	0.024	0.216	0.468	-0.020	-0.040	-0.021	0.051
$PostCAR_{[0,+1]}^{CAPM}$	-0.147	1.000	0.149	0.037	0.148	0.033	-0.012	-0.127	-0.036	0.093	0.053	0.032
<i>Size</i>	-0.201	0.149	1.000	-0.043	-0.230	0.116	0.002	0.016	0.014	-0.014	-0.015	-0.002
<i>Turnover</i>	0.057	0.037	-0.043	1.000	0.433	0.129	0.086	-0.044	0.039	0.043	0.162	-0.015
<i>WZ</i>	0.133	0.148	-0.230	0.433	1.000	0.025	0.097	-0.068	-0.078	0.098	0.150	-0.002
<i>Coverage</i>	0.024	0.033	0.116	0.129	0.025	1.000	-0.067	0.017	-0.094	-0.001	-0.006	0.002
$Ret^{-2,-12}$	0.216	-0.012	0.002	0.086	0.097	-0.067	1.000	0.015	0.215	-0.131	0.077	0.009
Ret^{-1}	0.468	-0.127	0.016	-0.044	-0.068	0.017	0.015	1.000	-0.014	-0.007	-0.021	0.191
<i>UMD</i>	-0.020	-0.036	0.014	0.039	-0.078	-0.094	0.215	-0.014	1.000	-0.073	0.312	-0.022
<i>STR</i>	-0.040	0.093	-0.014	0.043	0.098	-0.001	-0.131	-0.007	-0.073	1.000	0.015	0.037
$UMD \times Ret^{-2,-12}$	-0.021	0.053	-0.015	0.162	0.150	-0.006	0.077	-0.021	0.312	0.015	1.000	0.013
$STR \times Ret^{-1}$	0.051	0.032	-0.002	-0.015	-0.002	0.002	0.009	0.191	-0.022	0.037	0.013	1.000
Deletions	$PreCAR_{[-20,-1]}^{CAPM}$	$PostCAR_{[0,+1]}^{CAPM}$	<i>Size</i>	<i>Turnover</i>	<i>WZ</i>	<i>Coverage</i>	$Ret^{-2,-12}$	Ret^{-1}	<i>UMD</i>	<i>STR</i>	$UMD \times Ret^{-2,-12}$	$STR \times Ret^{-1}$
$PreCAR_{[-20,-1]}^{CAPM}$	1.000	0.083	0.279	-0.225	-0.258	-0.143	0.349	0.600	0.330	-0.072	-0.269	0.160
$PostCAR_{[0,+1]}^{CAPM}$	0.083	1.000	0.099	0.277	-0.145	0.193	0.078	0.055	0.121	-0.011	-0.072	0.043
<i>Size</i>	0.279	0.099	1.000	-0.037	-0.222	0.146	0.020	0.007	0.029	-0.018	-0.022	-0.003
<i>Turnover</i>	-0.225	0.277	-0.037	1.000	0.438	0.125	0.040	-0.031	0.039	0.063	0.162	-0.014
<i>WZ</i>	-0.258	-0.145	-0.222	0.438	1.000	0.007	0.047	-0.050	-0.070	0.115	0.165	-0.009
<i>Coverage</i>	-0.143	0.193	0.146	0.125	0.007	1.000	-0.043	0.029	-0.082	0.008	-0.004	0.005
$Ret^{-2,-12}$	0.349	0.078	0.020	0.040	0.047	-0.043	1.000	0.053	0.201	-0.140	0.052	0.017
Ret^{-1}	0.600	0.055	0.007	-0.031	-0.050	0.029	0.053	1.000	-0.002	-0.003	-0.018	0.199
<i>UMD</i>	0.330	0.121	0.029	0.039	-0.070	-0.082	0.201	-0.002	1.000	-0.082	0.270	-0.020
<i>STR</i>	-0.072	-0.011	-0.018	0.063	0.115	0.008	-0.140	-0.003	-0.082	1.000	0.006	0.072
$UMD \times Ret^{-2,-12}$	-0.269	-0.072	-0.022	0.162	0.165	-0.004	0.052	-0.018	0.270	0.006	1.000	0.016
$STR \times Ret^{-1}$	0.160	0.043	-0.003	-0.014	-0.009	0.005	0.017	0.199	-0.020	0.072	0.016	1.000

Notes: This table reports pairwise correlations among all variables used in the baseline regressions (Equation (1)), shown separately for additions (top panel) and deletions (bottom panel). All control variables are estimated together with the estimation sample defined in Section 5.3. PostCAR is the cumulative abnormal return over the $[0, +1]$ window. PreCAR is the cumulative abnormal return over the $[-20, -1]$. Firm size (*Size*) is measured as the firm's market capitalization relative to the total market capitalization of the S&P 500 on day $[-40]$; past turnover (*Turnover*) is defined as average daily trading volume divided by shares outstanding over the calendar month ending at $[-40]$, adjusted by the value-weighted market average; arbitrage risk (*WZ*) is defined as the variance of CAPM regression residuals over the year ending at $[-40]$; and analyst coverage (*Coverage*) is measured as the number of analysts covering the stock in the most recent earnings report available at $[-40]$. Return-based control variables capture both return patterns and factor exposures. Time-series momentum ($Ret^{-2,-12}$) is measured as the stock's cumulative return over the prior calendar year excluding the most recent month, anchored relative to the announcement date by counting the window back from day -1 . Short-term reversal (Ret^{-1}) is measured as the stock's return during the most recent calendar month ending at day -1 . Factor exposures to the *UMD* momentum factor and the *STR* short-term reversal factor are estimated over a one-year window ending 20 trading days before the beginning of the main pre-announcement window. To allow the predictive effects of return signals to vary with a firm's sensitivity to the corresponding factors, we additionally include the interaction terms $UMD \times Ret^{-2,-12}$ and $STR \times Ret^{-1}$.

Table A.12: VIF

Additions		Deletions	
Variable	VIF	Variable	VIF
<i>WZ</i>	1.368	<i>WZ</i>	1.362
<i>Turnover</i>	1.278	<i>Turnover</i>	1.283
<i>UMD</i>	1.198	<i>UMD</i>	1.156
$UMD \times Ret^{-2,-12}$	1.157	$UMD \times Ret^{-2,-12}$	1.129
$Ret^{-2,-12}$	1.089	<i>Size</i>	1.081
<i>Size</i>	1.077	$Ret^{-2,-12}$	1.072
<i>Coverage</i>	1.048	<i>Coverage</i>	1.052
Ret^{-1}	1.044	$STR \times Ret^{-1}$	1.049
$STR \times Ret^{-1}$	1.041	Ret^{-1}	1.048
<i>STR</i>	1.034	<i>STR</i>	1.044

Notes: This table reports variance inflation factors (VIFs) for all regressors included in Equation (1), shown separately for additions and deletions. The variable definitions follow Table 3.

Table A.13: Era Regression Results with Pooled Controls Across Eras
Additions

Decade	MA	CAPM	FF3	FF5
1980s	-0.060** (0.032)	-0.065** (0.017)	-0.064** (0.018)	-0.065** (0.013)
1990s	0.038 (0.219)	0.032 (0.252)	0.014 (0.623)	0.009 (0.752)
2000s	-0.068*** (0.000)	-0.047** (0.016)	-0.030 (0.201)	-0.038* (0.085)
2010s	-0.053 (0.207)	-0.079* (0.050)	-0.079* (0.055)	-0.059 (0.158)

Deletions

Decade	MA	CAPM	FF3	FF5
1980s	0.046 (0.546)	0.054 (0.505)	0.019 (0.802)	0.013 (0.864)
1990s	0.011 (0.826)	0.015 (0.771)	0.018 (0.734)	0.016 (0.738)
2000s	-0.027 (0.500)	-0.025 (0.538)	-0.025 (0.544)	-0.035 (0.387)
2010s	0.013 (0.671)	0.001 (0.986)	-0.002 (0.943)	0.000 (0.988)

Notes: This table reports the results from Equation (2) and the main pre-announcement window of 20 trading-days, under the restriction that all control variables are constrained to have a single, era-invariant coefficient. The model estimates decade-specific coefficients on *PreCAR* by interacting it with decade dummy variables (D_e) for the 1980s, 1990s, 2000s, and 2010s, while controlling for year fixed effects. The dependent variable is the cumulative abnormal return over the $[0, +1]$ window (*PostCAR*), and the main independent variable is the cumulative abnormal return over the $[-20, -1]$ window (*PreCAR*). The results are reported using MA, CAPM, FF3, FF5, as market models in the calculation of abnormal returns. The specification includes the same firm-characteristic and return-based control variables as defined in Table 3. This set comprises firm size (*Size*), past turnover (*Turnover*), arbitrage risk (*WZ*), analyst coverage (*Coverage*), time-series momentum ($Ret^{-2, -12}$), short-term reversal (Ret^{-1}), factor exposures (*UMD*, *STR*), and the interaction terms between return signals and the corresponding factor loadings ($UMD \times Ret^{-2, -12}$, $STR \times Ret^{-1}$). Parentheses report p-values computed using robust standard errors, and ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Table A.14: Factor Exposure UMD and STR: Event firms and Estimation sample

Factor	Exposure		Test of equality
	Event firms	Estimation sample	p -value
Panel A: Additions			
UMD	0.152	-0.037	0.000***
STR	0.044	0.049	0.650
Panel B: Deletions			
UMD	-0.496	-0.034	0.000***
STR	0.116	0.048	0.001***

Notes: This table reports differences in factor exposures between event firms and the estimation sample for the momentum (*UMD*) and short-term reversal (*STR*) factors. Exposures are estimated for each firm using daily returns over a one-year window ending 20 trading days before the start of the pre-announcement period, following [Aghaee \(2022\)](#). Equality of mean factor loadings between event firms and the estimation sample is assessed using a Welch two-sample t -test, which accounts for unequal variances across groups. These comparisons evaluate the identifying assumption that momentum and reversal signals have comparable return implications for treated and control firms. As discussed in Section 5.3, systematic differences in factor exposures could bias the coefficients on the return-based controls, particularly the *PreCAR* estimate.

Table A.15: Regression (2) Control Variables – 1980s: Additions

	Main Event Window $[-20, +1]$			
	CAPM	FF3	FF5	MA
<i>Size</i>	-1.143* (0.063)	-0.234 (0.702)	-0.340 (0.579)	-0.698 (0.262)
<i>Turnover</i>	-0.098** (0.046)	-0.076 (0.116)	-0.069 (0.152)	-0.042 (0.396)
<i>WZ</i>	-0.414 (0.540)	-0.237 (0.726)	-0.250 (0.712)	0.712 (0.297)
<i>Coverage</i>	-0.000** (0.013)	-0.000** (0.037)	-0.000** (0.018)	-0.000* (0.071)
$Ret^{-2,-12}$	0.001** (0.034)	0.001* (0.090)	0.001 (0.107)	0.001* (0.062)
Ret^{-1}	0.002 (0.267)	-0.002 (0.351)	-0.002 (0.194)	0.001 (0.713)
<i>UMD</i>	0.000 (0.509)	-0.000 (0.616)	0.000 (0.383)	0.000 (0.978)
<i>STR</i>	-0.000 (0.280)	-0.000 (0.561)	-0.000 (0.936)	-0.000 (0.526)
$UMD \times Ret^{-2,-12}$	0.000 (0.599)	0.001 (0.348)	0.001 (0.276)	0.001 (0.171)
$STR \times Ret^{-1}$	-0.002 (0.713)	-0.002 (0.775)	-0.002 (0.746)	0.001 (0.901)

Notes: This table reports the control variable results from Equation 3. The main results from this regression is reported in Table 3. The results are reported using MA, CAPM, FF3, FF5, in the calculation of abnormal returns. The specification includes the same firm-characteristic and return-based control variables as in Table 3. This set comprises firm size (*Size*), past turnover (*Turnover*), arbitrage risk (*WZ*), analyst coverage (*Coverage*), time-series momentum ($Ret^{-2,-12}$), short-term reversal (Ret^{-1}), factor exposures (*UMD*, *STR*), and the interaction terms between return signals and the corresponding factor loadings ($UMD \times Ret^{-2,-12}$, $STR \times Ret^{-1}$). All control variables are estimated together with the estimation sample defined in Section 5.3, with all variables winsorized at the 2.5% level and all firm characteristics demeaned. Parentheses report p-values computed using robust standard errors, and ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Table A.16: Regression (2) Control Variables – 1990s: Additions

	Main Event Window $[-20, +1]$			
	CAPM	FF3	FF5	MA
<i>Size</i>	1.105 (0.192)	0.800 (0.341)	0.593 (0.478)	0.481 (0.577)
<i>Turnover</i>	0.138*** (0.001)	0.093** (0.025)	0.088** (0.031)	0.110** (0.010)
<i>WZ</i>	0.372 (0.543)	-0.945 (0.120)	-0.806 (0.183)	-0.832 (0.182)
<i>Coverage</i>	-0.000 (0.902)	0.000 (0.315)	0.000 (0.237)	0.000 (0.793)
$Ret^{-2,-12}$	0.001** (0.013)	0.001*** (0.007)	0.001** (0.016)	0.001*** (0.005)
Ret^{-1}	0.004* (0.055)	0.001 (0.441)	0.001 (0.694)	0.003 (0.103)
<i>UMD</i>	-0.001 (0.154)	-0.000 (0.225)	-0.001* (0.070)	-0.001** (0.043)
<i>STR</i>	0.001 (0.211)	0.001 (0.129)	0.000 (0.447)	0.000 (0.959)
$UMD \times Ret^{-2,-12}$	0.001 (0.467)	0.000 (0.726)	0.000 (0.658)	0.000 (0.727)
$STR \times Ret^{-1}$	-0.004 (0.540)	-0.004 (0.484)	-0.003 (0.584)	0.001 (0.913)

Notes: This table reports the control variable results from Equation 3. The main results from this regression is reported in Table 3. The results are reported using MA, CAPM, FF3, FF5, in the calculation of abnormal returns. The specification includes the same firm-characteristic and return-based control variables as in Table 3. This set comprises firm size (*Size*), past turnover (*Turnover*), arbitrage risk (*WZ*), analyst coverage (*Coverage*), time-series momentum ($Ret^{-2,-12}$), short-term reversal (Ret^{-1}), factor exposures (*UMD*, *STR*), and the interaction terms between return signals and the corresponding factor loadings ($UMD \times Ret^{-2,-12}$, $STR \times Ret^{-1}$). All control variables are estimated together with the estimation sample defined in Section 5.3, with all variables winsorized at the 2.5% level and all firm characteristics demeaned. Parentheses report p-values computed using robust standard errors, and ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Table A.17: Regression (2) Control Variables – 2000s: Additions

	Main Event Window $[-20, +1]$			
	CAPM	FF3	FF5	MA
<i>Size</i>	0.006 (0.993)	0.135 (0.845)	0.156 (0.820)	-1.002 (0.165)
<i>Turnover</i>	-0.032 (0.164)	-0.032 (0.166)	-0.028 (0.214)	-0.072*** (0.003)
<i>WZ</i>	0.526 (0.187)	0.712* (0.073)	0.946** (0.016)	-1.184*** (0.004)
<i>Coverage</i>	0.000** (0.046)	0.000 (0.219)	0.000 (0.325)	0.000*** (0.005)
$Ret^{-2,-12}$	0.002*** (0.000)	0.002*** (0.000)	0.002*** (0.000)	0.003*** (0.000)
Ret^{-1}	0.000 (0.787)	0.003* (0.078)	0.002 (0.227)	-0.001 (0.385)
<i>UMD</i>	-0.001** (0.026)	-0.001*** (0.003)	-0.001** (0.018)	0.001*** (0.007)
<i>STR</i>	-0.000 (0.457)	-0.001 (0.304)	-0.002*** (0.004)	-0.001** (0.026)
$UMD \times Ret^{-2,-12}$	0.002** (0.010)	0.002** (0.017)	0.002*** (0.002)	0.000 (0.994)
$STR \times Ret^{-1}$	0.002 (0.701)	-0.001 (0.872)	-0.004 (0.557)	0.005 (0.446)

Notes: This table reports the control variable results from Equation 3. The main results from this regression is reported in Table 3. The results are reported using MA, CAPM, FF3, FF5, in the calculation of abnormal returns. The specification includes the same firm-characteristic and return-based control variables as in Table 3. This set comprises firm size (*Size*), past turnover (*Turnover*), arbitrage risk (*WZ*), analyst coverage (*Coverage*), time-series momentum ($Ret^{-2,-12}$), short-term reversal (Ret^{-1}), factor exposures (*UMD*, *STR*), and the interaction terms between return signals and the corresponding factor loadings ($UMD \times Ret^{-2,-12}$, $STR \times Ret^{-1}$). All control variables are estimated together with the estimation sample defined in Section 5.3, with all variables winsorized at the 2.5% level and all firm characteristics demeaned. Parentheses report p-values computed using robust standard errors, and ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Table A.18: Regression (2) Control Variables – 2010s: Additions

Main Event Window $[-20, +1]$				
	CAPM	FF3	FF5	MA
<i>Size</i>	2.183*** (0.000)	2.224*** (0.000)	1.974*** (0.001)	2.001*** (0.001)
<i>Turnover</i>	-0.034** (0.033)	-0.018 (0.250)	-0.019 (0.239)	-0.026 (0.113)
<i>WZ</i>	1.568** (0.017)	0.140 (0.829)	-0.165 (0.802)	0.755 (0.254)
<i>Coverage</i>	-0.000 (0.674)	-0.000 (0.795)	-0.000 (0.715)	0.000 (0.549)
$Ret^{-2,-12}$	-0.002*** (0.000)	-0.001 (0.155)	-0.000 (0.344)	-0.002*** (0.000)
Ret^{-1}	-0.001 (0.526)	-0.000 (0.822)	-0.002 (0.332)	0.002 (0.359)
<i>UMD</i>	-0.000 (0.991)	0.000 (0.838)	0.000 (0.214)	-0.000 (0.769)
<i>STR</i>	0.001 (0.171)	0.000 (0.966)	-0.000 (0.639)	0.001** (0.011)
$UMD \times Ret^{-2,-12}$	0.002 (0.107)	-0.000 (0.853)	-0.001 (0.507)	0.004*** (0.001)
$STR \times Ret^{-1}$	0.005 (0.482)	0.003 (0.648)	-0.000 (0.999)	0.003 (0.672)

Notes: This table reports the control variable results from Equation 3. The main results from this regression is reported in Table 3. The results are reported using MA, CAPM, FF3, FF5, in the calculation of abnormal returns. The specification includes the same firm-characteristic and return-based control variables as in Table 3. This set comprises firm size (*Size*), past turnover (*Turnover*), arbitrage risk (*WZ*), analyst coverage (*Coverage*), time-series momentum ($Ret^{-2,-12}$), short-term reversal (Ret^{-1}), factor exposures (*UMD*, *STR*), and the interaction terms between return signals and the corresponding factor loadings ($UMD \times Ret^{-2,-12}$, $STR \times Ret^{-1}$). All control variables are estimated together with the estimation sample defined in Section 5.3, with all variables winsorized at the 2.5% level and all firm characteristics demeaned. Parentheses report p-values computed using robust standard errors, and ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Table A.19: Regression (2) Control Variables – 1980s: Deletions

Main Event Window $[-20, +1]$				
	CAPM	FF3	FF5	MA
<i>Size</i>	-1.354 (0.253)	-0.772 (0.513)	-0.816 (0.488)	-0.282 (0.813)
<i>Turnover</i>	-0.031 (0.724)	0.002 (0.984)	0.035 (0.687)	-0.008 (0.929)
<i>WZ</i>	-1.525 (0.243)	-2.726** (0.037)	-2.324* (0.074)	2.916** (0.028)
<i>Coverage</i>	-0.000 (0.763)	0.000 (0.894)	-0.000 (0.958)	0.000 (0.802)
$Ret^{-2,-12}$	0.002*** (0.005)	0.002*** (0.006)	0.002** (0.029)	0.001 (0.187)
Ret^{-1}	-0.002 (0.654)	-0.002 (0.590)	-0.001 (0.684)	-0.001 (0.819)
<i>UMD</i>	0.001 (0.157)	0.000 (0.641)	0.001 (0.159)	0.001** (0.035)
<i>STR</i>	-0.002*** (0.001)	-0.002** (0.014)	-0.002** (0.039)	-0.001* (0.075)
$UMD \times Ret^{-2,-12}$	-0.003** (0.038)	-0.003* (0.085)	-0.002 (0.127)	-0.002 (0.131)
$STR \times Ret^{-1}$	-0.004 (0.672)	-0.003 (0.728)	-0.005 (0.592)	-0.001 (0.891)

Notes: This table reports the control variable results from Equation 3. The main results from this regression is reported in Table 4. The results are reported using MA, CAPM, FF3, FF5, in the calculation of abnormal returns. The specification includes the same firm-characteristic and return-based control variables as in Table 4. This set comprises firm size (*Size*), past turnover (*Turnover*), arbitrage risk (*WZ*), analyst coverage (*Coverage*), time-series momentum ($Ret^{-2,-12}$), short-term reversal (Ret^{-1}), factor exposures (*UMD*, *STR*), and the interaction terms between return signals and the corresponding factor loadings ($UMD \times Ret^{-2,-12}$, $STR \times Ret^{-1}$). All control variables are estimated together with the estimation sample defined in Section 5.3, with all variables winsorized at the 2.5% level and all firm characteristics demeaned. Parentheses report p-values computed using robust standard errors, and ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Table A.20: Regression (2) Control Variables – 1990s: Deletions

	Main Event Window $[-20, +1]$			
	CAPM	FF3	FF5	MA
<i>Size</i>	-0.390 (0.718)	-0.129 (0.904)	-0.176 (0.869)	0.494 (0.652)
<i>Turnover</i>	-0.084 (0.164)	-0.055 (0.350)	-0.036 (0.536)	-0.054 (0.383)
<i>WZ</i>	-2.494** (0.010)	-2.509*** (0.009)	-2.167** (0.022)	-1.045 (0.286)
<i>Coverage</i>	0.000 (0.607)	0.000 (0.374)	0.000 (0.217)	0.000 (0.354)
$Ret^{-2,-12}$	0.001 (0.478)	0.000 (0.888)	0.000 (0.641)	0.001 (0.401)
Ret^{-1}	-0.001 (0.675)	-0.001 (0.780)	0.001 (0.677)	0.000 (0.883)
<i>UMD</i>	0.000 (0.724)	0.001 (0.289)	0.000 (0.628)	-0.000 (0.568)
<i>STR</i>	0.001* (0.063)	0.001 (0.165)	0.001 (0.249)	0.002*** (0.001)
$UMD \times Ret^{-2,-12}$	0.002 (0.283)	0.001 (0.348)	0.001 (0.390)	0.002 (0.134)
$STR \times Ret^{-1}$	-0.006 (0.485)	-0.003 (0.729)	0.002 (0.796)	-0.008 (0.371)

Notes: This table reports the control variable results from Equation 3. The main results from this regression is reported in Table 4. The results are reported using MA, CAPM, FF3, FF5, in the calculation of abnormal returns. The specification includes the same firm-characteristic and return-based control variables as in Table 4. This set comprises firm size (*Size*), past turnover (*Turnover*), arbitrage risk (*WZ*), analyst coverage (*Coverage*), time-series momentum ($Ret^{-2,-12}$), short-term reversal (Ret^{-1}), factor exposures (*UMD*, *STR*), and the interaction terms between return signals and the corresponding factor loadings ($UMD \times Ret^{-2,-12}$, $STR \times Ret^{-1}$). All control variables are estimated together with the estimation sample defined in Section 5.3, with all variables winsorized at the 2.5% level and all firm characteristics demeaned. Parentheses report p-values computed using robust standard errors, and ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Table A.21: Regression (2) Control Variables – 2000s: Deletions

	Main Event Window $[-20, +1]$			
	CAPM	FF3	FF5	MA
<i>Size</i>	-0.476 (0.665)	-0.665 (0.544)	-0.229 (0.832)	-0.930 (0.406)
<i>Turnover</i>	-0.050 (0.111)	-0.048 (0.115)	-0.029 (0.329)	-0.089*** (0.006)
<i>WZ</i>	0.478 (0.357)	0.755 (0.140)	0.659 (0.191)	-0.220 (0.677)
<i>Coverage</i>	0.000* (0.050)	0.000 (0.457)	0.000 (0.421)	0.000** (0.032)
$Ret^{-2,-12}$	0.001** (0.012)	0.001 (0.196)	0.001 (0.133)	0.002*** (0.002)
Ret^{-1}	0.009*** (0.000)	0.008*** (0.000)	0.009*** (0.000)	0.013*** (0.000)
<i>UMD</i>	0.001*** (0.005)	0.001*** (0.008)	0.000 (0.687)	0.002*** (0.000)
<i>STR</i>	0.000 (0.636)	-0.001 (0.292)	-0.001 (0.114)	-0.000 (0.806)
$UMD \times Ret^{-2,-12}$	-0.002** (0.048)	-0.001 (0.377)	0.001 (0.517)	-0.003*** (0.006)
$STR \times Ret^{-1}$	-0.010 (0.239)	-0.001 (0.869)	0.003 (0.756)	-0.007 (0.406)

Notes: This table reports the control variable results from Equation 3. The main results from this regression is reported in Table 4. The results are reported using MA, CAPM, FF3, FF5, in the calculation of abnormal returns. The specification includes the same firm-characteristic and return-based control variables as in Table 4. This set comprises firm size (*Size*), past turnover (*Turnover*), arbitrage risk (*WZ*), analyst coverage (*Coverage*), time-series momentum ($Ret^{-2,-12}$), short-term reversal (Ret^{-1}), factor exposures (*UMD*, *STR*), and the interaction terms between return signals and the corresponding factor loadings ($UMD \times Ret^{-2,-12}$, $STR \times Ret^{-1}$). All control variables are estimated together with the estimation sample defined in Section 5.3, with all variables winsorized at the 2.5% level and all firm characteristics demeaned. Parentheses report p-values computed using robust standard errors, and ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Table A.22: Regression (2) Control Variables – 2010s: Deletions

	Main Event Window $[-20, +1]$			
	CAPM	FF3	FF5	MA
<i>Size</i>	0.043 (0.952)	0.063 (0.928)	-0.104 (0.881)	0.209 (0.773)
<i>Turnover</i>	-0.054*** (0.006)	-0.041** (0.030)	-0.030 (0.110)	-0.065*** (0.001)
<i>WZ</i>	1.340 (0.137)	-0.450 (0.615)	-0.534 (0.548)	0.540 (0.551)
<i>Coverage</i>	0.000 (0.331)	0.000 (0.131)	0.000 (0.120)	0.000 (0.369)
$Ret^{-2,-12}$	-0.002*** (0.005)	-0.001 (0.113)	-0.001* (0.097)	-0.002*** (0.001)
Ret^{-1}	-0.002 (0.226)	-0.002 (0.294)	-0.003 (0.125)	0.000 (0.980)
<i>UMD</i>	-0.002*** (0.000)	-0.002*** (0.000)	-0.000 (0.267)	-0.001* (0.063)
<i>STR</i>	-0.000 (0.463)	-0.001* (0.058)	-0.001 (0.104)	0.001 (0.405)
$UMD \times Ret^{-2,-12}$	-0.000 (0.893)	-0.002 (0.234)	-0.002* (0.076)	0.001 (0.355)
$STR \times Ret^{-1}$	0.039*** (0.000)	0.034*** (0.000)	0.034*** (0.000)	0.034*** (0.000)

Notes: This table reports the control variable results from Equation 3. The main results from this regression is reported in Table 4. The results are reported using MA, CAPM, FF3, FF5, in the calculation of abnormal returns. The specification includes the same firm-characteristic and return-based control variables as in Table 4. This set comprises firm size (*Size*), past turnover (*Turnover*), arbitrage risk (*WZ*), analyst coverage (*Coverage*), time-series momentum ($Ret^{-2,-12}$), short-term reversal (Ret^{-1}), factor exposures (*UMD*, *STR*), and the interaction terms between return signals and the corresponding factor loadings ($UMD \times Ret^{-2,-12}$, $STR \times Ret^{-1}$). All control variables are estimated together with the estimation sample defined in Section 5.3, with all variables winsorized at the 2.5% level and all firm characteristics demeaned. Parentheses report p-values computed using robust standard errors, and ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Table A.23: Alternative Pre-Announcement Windows – Wald Test

Additions					Deletions				
Window = 10	MA	CAPM	FF3	FF5	Window = 10	MA	CAPM	FF3	FF5
2000s vs. 1990s	-0.039 (0.432)	-0.001 (0.979)	-0.007 (0.892)	-0.014 (0.763)	2000s vs. 1990s	-0.077 (0.443)	-0.070 (0.481)	-0.054 (0.578)	-0.074 (0.441)
2010s vs. 2000s	-0.008 (0.918)	-0.066 (0.382)	-0.047 (0.538)	0.007 (0.923)	2010s vs. 2000s	0.151** (0.039)	0.143** (0.048)	0.129* (0.066)	0.147** (0.037)
2010s vs. 1990s	-0.047 (0.548)	-0.067 (0.371)	-0.054 (0.476)	-0.006 (0.936)	2010s vs. 1990s	0.074 (0.437)	0.074 (0.440)	0.075 (0.415)	0.073 (0.423)
1990s vs. 1980s	0.065 (0.240)	0.072 (0.180)	0.062 (0.235)	0.070 (0.177)	1990s vs. 1980s	-0.081 (0.506)	-0.073 (0.536)	-0.074 (0.518)	-0.082 (0.481)
2000s vs. 1980s	0.026 (0.633)	0.071 (0.191)	0.056 (0.303)	0.057 (0.281)	2000s vs. 1980s	-0.158 (0.134)	-0.143 (0.157)	-0.129 (0.193)	-0.156 (0.124)
2010s vs. 1980s	0.018 (0.824)	0.004 (0.956)	0.009 (0.913)	0.064 (0.428)	2010s vs. 1980s	-0.007 (0.943)	0.000 (0.998)	0.000 (0.997)	-0.010 (0.921)
Window = 50	MA	CAPM	FF3	FF5	Window = 50	MA	CAPM	FF3	FF5
2000s vs. 1990s	-0.047* (0.051)	-0.037 (0.124)	-0.016 (0.503)	-0.020 (0.385)	2000s vs. 1990s	-0.039 (0.324)	-0.030 (0.450)	-0.064* (0.058)	-0.059* (0.070)
2010s vs. 2000s	-0.018 (0.557)	-0.039 (0.207)	-0.058* (0.065)	-0.039 (0.220)	2010s vs. 2000s	0.002 (0.957)	-0.016 (0.623)	-0.010 (0.754)	-0.008 (0.808)
2010s vs. 1990s	-0.065* (0.059)	-0.075** (0.027)	-0.074** (0.023)	-0.058* (0.073)	2010s vs. 1990s	-0.037 (0.333)	-0.046 (0.235)	-0.074** (0.014)	-0.067** (0.021)
1990s vs. 1980s	0.013 (0.674)	0.029 (0.342)	0.016 (0.564)	0.014 (0.594)	1990s vs. 1980s	0.017 (0.738)	0.030 (0.547)	0.067 (0.134)	0.067 (0.138)
2000s vs. 1980s	-0.034 (0.211)	-0.008 (0.763)	0.001 (0.981)	-0.005 (0.844)	2000s vs. 1980s	-0.022 (0.625)	-0.000 (0.996)	0.003 (0.943)	0.008 (0.862)
2010s vs. 1980s	-0.052 (0.156)	-0.047 (0.193)	-0.058 (0.103)	-0.044 (0.211)	2010s vs. 1980s	-0.021 (0.645)	-0.016 (0.714)	-0.007 (0.879)	0.001 (0.989)
Window = 100	MA	CAPM	FF3	FF5	Window = 100	MA	CAPM	FF3	FF5
2000s vs. 1990s	-0.008 (0.596)	-0.011 (0.511)	0.005 (0.759)	-0.000 (0.994)	2000s vs. 1990s	-0.014 (0.645)	-0.002 (0.944)	-0.006 (0.852)	-0.005 (0.861)
2010s vs. 2000s	-0.026 (0.238)	-0.037* (0.088)	-0.043* (0.053)	-0.025 (0.269)	2010s vs. 2000s	0.013 (0.602)	-0.002 (0.926)	-0.006 (0.805)	-0.002 (0.921)
2010s vs. 1990s	-0.034 (0.133)	-0.047** (0.036)	-0.038* (0.088)	-0.025 (0.266)	2010s vs. 1990s	-0.001 (0.984)	-0.004 (0.876)	-0.012 (0.666)	-0.008 (0.775)
1990s vs. 1980s	-0.007 (0.726)	0.005 (0.821)	-0.006 (0.728)	-0.007 (0.705)	1990s vs. 1980s	-0.038 (0.338)	-0.022 (0.575)	-0.001 (0.983)	0.000 (0.996)
2000s vs. 1980s	-0.016 (0.427)	-0.006 (0.778)	-0.001 (0.938)	-0.007 (0.700)	2000s vs. 1980s	-0.051 (0.156)	-0.024 (0.516)	-0.007 (0.865)	-0.005 (0.893)
2010s vs. 1980s	-0.041 (0.106)	-0.042 (0.101)	-0.044* (0.063)	-0.032 (0.177)	2010s vs. 1980s	-0.038 (0.288)	-0.026 (0.464)	-0.012 (0.729)	-0.007 (0.833)

Notes: This table reports the results from Equation (3) for the alternative pre-announcement windows analysis, which formally tests for statistical differences in the relationship between *PreCAR* and *PostCAR* across decades under the alternative pre-announcement windows of 10, 50, and 100 trading days. In each specification, *PreCAR* is defined as the cumulative abnormal return over the $[n, -1]$ window, where $n \in \{-10, -50, -100\}$. Following the structure of Equation (2), *PreCAR* is interacted with decade dummy variables (D_e) for the 1980s, 1990s, 2000s, and 2010s, and F-tests are applied to evaluate whether the estimated decade-specific coefficients differ significantly across eras. Abnormal returns are computed using MA, CAPM, FF3, and FF5 specifications. The specification includes the same firm-characteristic and return-based control variables as defined in Table 3. This set comprises firm size (*Size*), past turnover (*Turnover*), arbitrage risk (*WZ*), analyst coverage (*Coverage*), time-series momentum ($Ret^{-2, -12}$), short-term reversal (Ret^{-1}), factor exposures (*UMD*, *STR*), and the interaction terms between return signals and the corresponding factor loadings ($UMD \times Ret^{-2, -12}$, $STR \times Ret^{-1}$). Parentheses report p-values computed using robust standard errors, and ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Table A.24: Alternative Pre-Announcement Windows – Predictability-Interaction Results

Additions					Deletions				
Window = 10					Window = 10				
<i>Coverage</i>					<i>Coverage</i>				
Era	MA	CAPM	FF3	FF5	Era	MA	CAPM	FF3	FF5
1980s	-0.008* (0.084)	-0.008 (0.107)	-0.007 (0.155)	-0.008* (0.095)	1980s	-0.037** (0.030)	-0.037** (0.023)	-0.034* (0.051)	-0.033* (0.077)
1990s	-0.001 (0.833)	-0.001 (0.732)	-0.002 (0.530)	-0.002 (0.670)	1990s	0.005 (0.649)	0.004 (0.751)	0.004 (0.760)	0.005 (0.659)
2000s	-0.002 (0.647)	0.003 (0.460)	0.002 (0.638)	0.002 (0.661)	2000s	0.006 (0.490)	0.006 (0.395)	0.007 (0.363)	0.010 (0.156)
2010s	0.003 (0.668)	0.002 (0.803)	-0.004 (0.673)	-0.001 (0.888)	2010s	0.005 (0.258)	0.005 (0.312)	0.006 (0.148)	0.004 (0.353)
<i>SizeRank</i>					<i>SizeRank</i>				
Era	MA	CAPM	FF3	FF5	Era	MA	CAPM	FF3	FF5
1980s	0.000 (0.951)	0.000 (0.864)	-0.000 (0.653)	-0.000 (0.658)	1980s	0.000 (0.794)	0.000 (0.505)	0.000 (0.734)	-0.000 (0.990)
1990s	-0.000 (0.137)	-0.000 (0.388)	-0.000 (0.438)	-0.000 (0.395)	1990s	0.001 (0.450)	0.001 (0.444)	0.001 (0.443)	0.000 (0.645)
2000s	0.000 (0.238)	0.000 (0.716)	-0.000 (0.669)	-0.000 (0.904)	2000s	0.001 (0.240)	0.001** (0.041)	0.001* (0.087)	0.001 (0.205)
2010s	-0.002*** (0.001)	-0.001*** (0.006)	-0.001*** (0.007)	-0.002*** (0.001)	2010s	-0.001 (0.481)	-0.001 (0.277)	-0.001 (0.359)	-0.001 (0.327)
Window = 50					Window = 50				
<i>Coverage</i>					<i>Coverage</i>				
Era	MA	CAPM	FF3	FF5	Era	MA	CAPM	FF3	FF5
1980s	-0.004* (0.093)	-0.004* (0.065)	-0.003 (0.193)	-0.003 (0.116)	1980s	-0.010 (0.183)	-0.009 (0.250)	-0.008 (0.325)	-0.008 (0.296)
1990s	-0.003 (0.192)	-0.003 (0.104)	-0.003* (0.082)	-0.004** (0.026)	1990s	0.003 (0.362)	0.003 (0.373)	0.002 (0.333)	0.002 (0.285)
2000s	-0.002 (0.143)	-0.001 (0.405)	-0.002 (0.128)	-0.002 (0.173)	2000s	0.003 (0.309)	0.002 (0.432)	0.001 (0.679)	0.002 (0.426)
2010s	0.001 (0.607)	0.001 (0.664)	0.001 (0.795)	0.002 (0.572)	2010s	-0.001 (0.504)	-0.001 (0.493)	0.000 (0.833)	0.000 (0.825)
<i>SizeRank</i>					<i>SizeRank</i>				
Era	MA	CAPM	FF3	FF5	Era	MA	CAPM	FF3	FF5
1980s	0.000 (0.835)	0.000 (0.574)	0.000 (0.500)	0.000 (0.477)	1980s	0.000* (0.077)	0.000** (0.042)	0.000* (0.075)	0.000* (0.097)
1990s	-0.000 (0.248)	-0.000 (0.435)	0.000 (0.811)	0.000 (0.945)	1990s	0.000 (0.786)	0.000 (0.909)	-0.000 (0.574)	0.000 (0.952)
2000s	0.000 (0.210)	-0.000 (0.894)	-0.000 (0.606)	0.000 (0.924)	2000s	-0.000 (0.936)	0.000 (0.566)	0.000 (0.705)	0.000 (0.682)
2010s	-0.001*** (0.000)	-0.001*** (0.002)	-0.001*** (0.001)	-0.001*** (0.001)	2010s	-0.000*** (0.000)	-0.000*** (0.000)	-0.000*** (0.000)	-0.000*** (0.000)

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Additions (continued)

Window = 100

Coverage

Era	MA	CAPM	FF3	FF5
1980s	-0.002 (0.303)	-0.002 (0.235)	-0.001 (0.291)	-0.002 (0.115)
1990s	-0.001 (0.327)	-0.002 (0.134)	-0.002 (0.212)	-0.002 (0.105)
2000s	-0.002 (0.119)	-0.001 (0.321)	-0.002 (0.189)	-0.001 (0.324)
2010s	0.003 (0.166)	0.002 (0.199)	0.002 (0.265)	0.002 (0.267)

SizeRank

Era	MA	CAPM	FF3	FF5
1980s	0.000 (0.893)	0.000 (0.847)	0.000 (0.560)	0.000 (0.500)
1990s	-0.000 (0.152)	-0.000 (0.177)	-0.000 (0.905)	-0.000 (0.976)
2000s	0.000 (0.390)	-0.000 (0.937)	-0.000 (0.556)	0.000 (0.992)
2010s	-0.001*** (0.000)	-0.001*** (0.000)	-0.000*** (0.000)	-0.001*** (0.000)

Deletions (continued)

Window = 100

Coverage

Era	MA	CAPM	FF3	FF5
1980s	-0.011* (0.059)	-0.011* (0.063)	-0.010 (0.127)	-0.008 (0.225)
1990s	0.003 (0.274)	0.003 (0.225)	0.004* (0.082)	0.004* (0.077)
2000s	0.002 (0.345)	0.001 (0.594)	0.002 (0.264)	0.003* (0.052)
2010s	-0.001 (0.531)	-0.001 (0.587)	-0.001 (0.572)	-0.001 (0.604)

SizeRank

Era	MA	CAPM	FF3	FF5
1980s	0.000 (0.182)	0.000 (0.136)	0.000 (0.222)	0.000 (0.375)
1990s	0.000 (0.225)	0.000 (0.274)	0.000 (0.713)	0.000 (0.509)
2000s	-0.000 (0.936)	0.000 (0.341)	0.000 (0.572)	0.000 (0.552)
2010s	-0.001*** (0.001)	-0.001*** (0.000)	-0.001*** (0.001)	-0.001*** (0.003)

Notes: This table reports the results from Equation (4) for the Alternative Pre-Announcement Windows analysis, which extends the specification by introducing interaction terms that capture cross-sectional differences in the relationship between *PreCAR* and *PostCAR*. The pre-announcement window is alternately defined over the $[n, -1]$ interval, where $n \in \{-10, -50, -100\}$. In each case, *PreCAR* is interacted with proxies for mechanical predictability (*SizeRank*) and informational visibility (*Coverage*), allowing the substitution effect to vary across firms and over time. The dependent variable is the cumulative abnormal return over the $[0, +1]$ window (*PostCAR*). Abnormal returns are calculated using CAPM, FF3, and FF5 specifications. *SizeRank* measures each firm's relative market capitalization within the cross-section of U.S.-listed firms and captures mechanical predictability. Analyst coverage (*Coverage*) serves as a proxy for informational visibility. The interaction coefficients ($\beta_{s,e}$ and $\beta_{c,e}$) indicate how these characteristics moderate the link between pre-announcement and announcement-window returns, with more negative values reflecting stronger substitution effects. The specification includes the same firm-characteristic and return-based control variables as defined in Table 3. This set comprises firm size (*Size*), past turnover (*Turnover*), arbitrage risk (*WZ*), analyst coverage (*Coverage*), time-series momentum ($Ret^{-2,-12}$), short-term reversal (Ret^{-1}), factor exposures (*UMD*, *STR*), and the interaction terms between return signals and the corresponding factor loadings ($UMD \times Ret^{-2,-12}$, $STR \times Ret^{-1}$). Parentheses report p-values computed using robust standard errors, and ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Table A.25: Predictability-Interaction Results – Prompted Additions

<i>Coverage</i>				
Era	MA	CAPM	FF3	FF5
1980s	-0.009** (0.013)	-0.009** (0.014)	-0.005 (0.145)	-0.005* (0.091)
1990s	-0.006 (0.283)	-0.008 (0.156)	-0.007 (0.151)	-0.008 (0.127)
2000s	-0.001 (0.831)	-0.000 (1.000)	-0.003 (0.506)	-0.003 (0.451)
2010s	0.002 (0.717)	0.002 (0.750)	-0.001 (0.808)	-0.001 (0.741)
<i>SizeRank</i>				
Era	MA	CAPM	FF3	FF5
1980s	0.000 (0.463)	0.000 (0.551)	0.000 (0.693)	0.000 (0.652)
1990s	-0.000 (0.487)	0.000 (0.958)	0.000 (0.573)	0.000 (0.645)
2000s	0.000 (0.423)	0.000 (0.499)	0.000 (0.732)	0.000 (0.484)
2010s	-0.000 (0.321)	-0.000 (0.279)	-0.000 (0.465)	-0.000 (0.222)

Notes: This table reports the results from Equation (4) and the main pre-announcement window of 20 trading-days, for the restricted additions sample, with the additions occurring on dates with at least one M&A-driven deletion (approximately 70% of additions). The model interacts *PreCAR* with proxies for mechanical predictability (*SizeRank*) and informational visibility (*Coverage*), allowing the substitution effect to vary across firms and decades. The dependent variable is the cumulative abnormal return over the $[0, +1]$ window (*PostCAR*), and the main independent variable is the cumulative abnormal return over the $[-20, -1]$ window (*PreCAR*). The results are reported using MA, CAPM, FF3, and FF5 as market specifications in the calculation of abnormal returns. The specification includes the same control variables as in defined Table 3. Parentheses report p-values computed using robust standard errors, and ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Table A.26: Baseline Regression - Selected Sample Exclusions

Additions				
Sample exclusions	<i>PreCAR</i>			
	MA	CAPM	FF3	FF5
Excluding 1980s and 1990s	-0.006*** (0.000)	-0.006*** (0.000)	-0.009*** (0.000)	-0.006*** (0.000)
Excluding 1990s	-0.014*** (0.000)	-0.015*** (0.000)	-0.013*** (0.000)	-0.014*** (0.000)
Excluding 1980s	-0.006*** (0.000)	-0.005*** (0.000)	-0.008*** (0.000)	-0.006*** (0.000)

Deletions				
Sample exclusions	<i>PreCAR</i>			
	MA	CAPM	FF3	FF5
Excluding 1980s and 1990s	-0.010*** (0.000)	-0.005** (0.017)	-0.007*** (0.000)	-0.005*** (0.004)
Excluding 1990s	-0.008** (0.029)	-0.001 (0.677)	-0.003 (0.478)	-0.006* (0.069)
Excluding 1980s	-0.009*** (0.000)	-0.005*** (0.005)	-0.007*** (0.000)	-0.005*** (0.001)

Notes: This table reports the results from Equation (1) and the main pre-announcement window of 20 trading-days, which examines the relationship between *PreCAR* and *PostCAR* over the full sample period from 1980 to 2020, with selected period exclusions. *PostCAR* is the cumulative abnormal return over the $[0, +1]$ window. *PreCAR* is the cumulative abnormal return over the $[-20, -1]$. The results are reported using MA, CAPM, FF3, FF5, as market models in the calculation of abnormal returns. The specification includes the same firm-characteristic and return-based control variables as defined in Table 3. This set comprises firm size (*Size*), past turnover (*Turnover*), arbitrage risk (*WZ*), analyst coverage (*Coverage*), time-series momentum ($Ret^{-2, -12}$), short-term reversal (Ret^{-1}), factor exposures (*UMD*, *STR*), and the interaction terms between return signals and the corresponding factor loadings ($UMD \times Ret^{-2, -12}$, $STR \times Ret^{-1}$). All control variables are estimated together with the estimation sample defined in Section 5.3, with all variables winsorized at the 2.5% level and all firm characteristics demeaned. Parentheses report p-values computed using robust standard errors, and ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Artificial Intelligence Disclosure

Artificial intelligence tools, specifically ChatGPT and Sana, were used to support the writing and coding processes in this paper. For writing, these tools assisted solely with language refinement and clarity. We retain full responsibility for the originality and authorship of all written content. For coding, the AI tools provided limited technical assistance in reviewing and refining code, primarily by identifying potential errors and inefficiencies. All methodological decisions and computational procedures were developed and implemented independently.

The primary risk identified in using AI tools was the possibility of hallucinated or inaccurate information. To mitigate this risk, we did not use AI tools for factual statements, references or the development of academic arguments. We also carefully reviewed all refinements assisted by AI for accuracy.

Our experience from the process suggests AI tools are useful for linguistic refinement and code review once the underlying content has been developed, but they do not substitute for any of the methodological work presented in this paper.