Revisiting the Index Effect

Evidence from ESG Index Additions and Deletions

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Abstract:

This thesis investigates whether inclusion in or exclusion from an ESG screened index causes abnormal stock returns and abnormal trading volume following the event. It examines if firms experience an increase in trading volume after being added to the MSCI Choice ESG Screened Indices and a decrease in trading volume after deletion. To do this, the study applies an event study methodology, similar to Chen, Noronha and Singal (2004), comparing actual returns after the announcement and rebalancing with expected returns estimated using the CAPM model and comparing actual trading volume with a benchmark volume. While we find no significant impact on returns for either stock additions to or deletions from an ESG index, we do find that there is an increase in trading activity around the time when the change in constituents is implemented. This thesis contributes to the research on the index effect by expanding this to include a growing body of indices with sustainability focus, filling a current gap in the existing literature.

Key words:

Index Effect, ESG, Event Study, Abnormal Returns, Abnormal Trading Volume

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I. Introduction

The index effect is a well-studied phenomenon in finance and refers to the impact on a stock's price, liquidity or trading activity after the stock has been added to or removed from a major stock index (Harris and Gurel, 1986). While this effect has been widely investigated in the finance literature, it has mostly been looked at in relation to major stock indices such as the S&P 500, MSCI indices and FTSE Russell. There is a certain group of indexes that has received comparatively little attention in the discussion of the index effect, ESG indices, which we consider to be overlooked in the existing finance literature. An ESG index can be defined as a stock market index that selects, weights or excludes companies based on environmental, social and governance criteria in addition to traditional financial measures (SIX Group, 2023). These indices give investors exposure that meets certain ethical standards and allow institutional investors and funds to align their portfolios with ESG principles (SIX Group, 2023). This thesis will investigate the index effect in relation to ESG indices and fills a gap in the literature by being among the first to analyze the effects of changes in index composition in the context of ESG indices. Our research question can be formulated as: Does inclusion in (or deletion from) an ESG index lead to abnormal stock returns and abnormal trading volume around the announcement and rebalancing dates?

This thesis contributes to the research on the index effect by expanding this to include a growing body of indices with sustainability focus. By analyzing these effects, we provide insights into whether ESG index revisions have similar impacts on stock returns and trading volume that have been found in studies on indices lacking this focus (Chen et al. 2004; Harris and Gurel, 1986; Shleifer, 1986). Previous research has been highly focused on traditional indices, while ESG indices have not yet been well studied. Since ESG indices is a new phenomenon and the focus on ESG among investors is increasing (OECD, 2020), it will become a relevant field of study for future research and this thesis provides a first step in that process. Understanding these insights can inform asset managers, corporate decision-makers, and policymakers about the market implications of ESG-focused index changes.

Based on the documented index effect and previous studies, we believe that we will see a positive impact on stock prices after a stock has been included on the ESG index, as well as a negative impact after deletion from an ESG index. We expect that the magnitude of the cumulative abnormal returns will vary across firms depending on their idiosyncratic characteristics, which is why we will perform a cross-sectional regression to test for these effects. Further, we also believe that there is an effect on trading volume. Our hypothesis is that a stock experiences a positive abnormal trading volume after being included in an ESG index, while there is a negative impact on abnormal trading volume from ESG index deletion.

Our hypothesis builds on market efficiency. The efficient market hypothesis is widely recognized as a concept in financial theory, proposing that financial markets operate efficiently, making it difficult for investors to consistently achieve abnormal profits without taking on additional risk (Malkiel, 2003). Most empirical finance studies work under the semi-strong form of an efficient market hypothesis, which states that all public information is reflected in prices (Fama, 1970). Therefore, if this hypothesis holds, the stock prices should immediately reflect the new information when index changes are publicly announced (Fama, 1970). This does not only affect prices but also has implications for trading behavior. Under the efficient market hypothesis volume changes are reactions to new public information (Fama, 1970). According to John Maynard Keynes's theory, investors prefer to hold liquid assets rather than illiquid assets, which cannot be quickly converted into cash without losing significant value (Keynes, J.M., 1936). Liquid stocks have a high level of trading activity, which makes it simple to buy or sell the shares quickly without significantly impacting the price (Amihud and Mendelson, 1986). One way to measure the liquidity of a stock is to look at the bid-ask spread, which is the difference between the highest price a buyer is willing to pay and the lowest price a seller is willing to accept (Amihud and Mendelson, 1986). Liquid stocks have a smaller spread, while illiquid stocks have larger spreads, as the spread is an implicit cost for trading (Stoll, H.R., 2000). When a stock is included in an ESG index, the increased demand from, for example, index-tracking funds and increased investor attention could improve the stock's liquidity (Chen, Noronha and Singal, 2004). This makes the stock less costly to trade, thus, attracting more trades and resulting in a positive abnormal trading volume (Chen et al., 2004). Conversely, when a stock is removed from an ESG index, anticipated declines in trading activity and attention increase the stock's illiquidity, leading to lower trading volume (Chen et al., 2004). These insights are considered in our hypothesis.

The ideal way to test our hypothesis would be to randomly assign index inclusion to a set of firms that are all eligible to be on the MSCI Choice screened indices. We would then accurately measure the differences in returns and volumes between the two groups. This ideal method guarantees no selection bias, no confounding variables, no endogeneity and a clear causal identification. In reality, different stocks are included in the screened indices for different reasons and it is thus not randomized. This causes an issue with selection bias as well as omitted variable bias, since there might be factors that are correlated with index inclusion that are affecting stock returns or trading volume. We are using an event study to identify the effect of index inclusion and exclusion on stock returns and trading volume, following the method presented by Chen et al. (2004). Since this method uses abnormal returns and trading volume ratios (comparing the observed values to a benchmark), it is controlling for different omitted variables that are affecting the dependent variables. Since we are using non-overlapping windows across rebalancing events and short event windows, the risk of omitted variable bias is minimalized (MacKinlay, 1997). The index constitution is not randomized, but rather depends on underlying factors, which may give rise to selection bias. One important consideration is thus if abnormal returns/volume is caused by the index inclusion/deletion or the underlying factors that led to it. The event study approach that we are using is handling this issue by using tight event windows, which means that for selection bias to be an issue, the underlying factors must abruptly change in the event window. Since this is not the case (these factors change over time, and the ESG scores are presented far before the index reconstitution), the approach is likely to handle selection bias as well. The timing of ESG index inclusion/deletion can be argued to be quasi random if there is nothing that systematically causes the announcement to happen on a day when returns would have been abnormally high or low anyways. What must be considered to be able to claim quasi randomness is particularly the risk of the market anticipating the index inclusion/deletion before the announcement. This anticipation may be an issue since the ESG rankings and controversy scores are updated beforehand, which means the market may expect that the stock will be included or excluded from the index. If this is the case, the announcement is not an exogenous surprise at announcement date. This risk is going to be tested for in the analysis and the implications will be discussed further.

The results from our study show that stock additions to or deletions from the MSCI Choice ESG Screened Indices does not produce significant abnormal returns around the announcement and rebalancing dates. These findings stand in contrast to earlier studies on traditional indices such as the S&P 500 (Chen et al. 2004; Harris and Gurel, 1986; Shleifer, 1986). Our firm-characteristics regression indicates that idiosyncratic characteristics impact the magnitude of cumulative abnormal returns following ESG index revisions. In terms of trading activity, we observe a clear pattern: spikes in abnormal trading volume around the rebalancing date, followed by a rapid return to normal levels, in line with existing research (Chen et al., 2004).

In their study, Chen et al. (2004) examine the index effect and the effects of additions to and exclusions from the S&P 500 index on stock prices. They find that while stock prices rise significantly when firms are added to the index, the declines following exclusions from the index are considerably smaller. The authors argue that the explanation relates to the trading behavior of index funds, which must immediately purchase shares of newly added firms but can wait before selling those removed from the index. This asymmetry in trading leads to greater positive price reactions for inclusions than negative effects for exclusions. This study is still relevant as it contributes to the understanding of the index effect and offers explanations not solely based on liquidity or investor awareness.

Denis et al. (2003) investigate whether the positive stock price effect after inclusion in the S&P 500 index is caused by price pressure from increased index fund demand or a signal of changed fundamentals and improved earning expectations. In other words, it examines if inclusion is an information-free event or if it leads the market to change its expectations about the stock. This article confirms the significant positive abnormal returns observed by other researchers after S&P 500 index inclusion. It finds that there is no significant improvement in analyst earnings forecast following this inclusion and no evidence that analysts revise their expectations upward. Also, Denis et al. (2003) observe no improvement in actual operating performance nor outperformance compared to a control group. Therefore, this study concludes that the effect on stock prices are primarily driven by pressure from institutional demand rather than fundamental improvement in firm expectations.

In a more recent study, Greenwood, R.M. and Sammon, M. (2024) find that the abnormal returns generally associated with a stock being included in the S&P 500 index has declined from an average of 7.4% in the 1990s to less than 1% over the past decade. The authors offer multiple explanations for their findings. One of their explanations is that there has been a change in the composition of the firms that are included, for example related to the size of the company relative to the index. To reduce tracking error, benchmarked funds tend to purchase added stocks that constitute a larger percentage of the index upon inclusion and might be less inclined to purchase additions that are small relative to the index. Other explanations to why these abnormal returns have largely disappeared is due to increased arbitrage and improved liquidity provision. The findings are similar to what Preston and Soe (2021) concludes in their study after observing a declining pattern across multiple major indices including the Nikkei 225, S&P/TSX 60 and DAX 60. These articles illustrate that the evidence of the effects on index inclusions and deletions includes mixed observations. However, to the best of our knowledge, there are far fewer studies which have looked at stock prices after inclusion or removal from the niched perspective of ESG indices.

Other relevant insights are offered by Bolton, P. and Kacperczyk, M. (2021), who examine if investors price carbon-related risks in equity markets. The study shows that firms with higher carbon risk have lower valuations but higher expected returns, indicating a carbon risk premium. This suggests that carbon emissions, one aspect of ESG considerations, affect stock prices for publicly traded firms. These findings are relevant for this topic because they indicate that the market considers environmental risks in stock pricing. However, these findings are not always consistent in the literature. In their article, Alves, R., P. Kruger, and M. van Dijk (2023) find little evidence that ESG ratings are related to stock returns.

This paper is structured as follows: Section 2 provides a theoretical background and section 3 describes the indices used in our study. Section 4 and 5 outlines the data collection and methodology. Section 6 presents our empirical findings. Section 7 includes a discussion of the results as well as an evaluation of the robustness of our model. Finally, section 8 is our concluding remarks.

II. Theoretical background

A. The index effect

Index tracking

Passive investing through a mutual fund or an ETF is a type of investing aimed at mimicking the performance of a certain stock market index (Wurgler, 2010). By their design and objective, it is necessary for index funds and ETFs to hold all assets of the index which they are tracking (Wurgler, 2010). Part of the rationale behind the index effect is based on the fact that when a firm is added or removed from an index, it triggers major fund flows into or out of that firm from passive funds (Harris and Gurel, 1986). Passive funds must buy and sell stocks after the constituents change. This is likely to cause a larger passive flow, which can drive up demand for stocks when being included in an index, resulting in positive abnormal returns and increased liquidity (Shleifer, 1986). However, this effect should be temporary since prices will eventually return to fundamentals once the mechanical trading is completed (Shleifer, 1986). In the last decades, passive investing has risen in popularity and inflows into index funds have surpassed inflows into actively managed funds (Sabban, 2024). Wurgler (2010) explains how a feedback loop is created when growing inflows into passive funds amplify the index effect, boosting index stock returns and making passive investing even more attractive, which in turn drives further inflows.

Imperfect substitutes

In contrast to the hypothesis above, another possible explanation to the index effect predicts that the price changes after stock inclusion should be permanent and sustained as long as the stock remains included on the index (Shleifer, 1986). In his famous paper, Shleifer (1986) argues that stocks are not perfect substitutes for one another. This implies a downward-sloping demand curve for stocks, which contradicts the traditional belief of flat demand curves, as the absence of perfect substitutes will lead to a reduction in arbitrage activity to flatten the demand curve (Chen et al., 2004). Wurgler and Zhuravskaya (2002) show that arbitrage activity is weaker and mispricings are more likely to occur for stocks that do not have any close substitutes. Thus, arbitrage risk seems to be an important factor for determining the slope of the demand curves. This means that stocks that do not have close substitutes experience higher price jumps after S&P 500 index inclusion.

Signaling

Another possible mechanism behind the observed index effect relates to the conveyance of information (Berkman, O'Brien, See and Yeh, 2023). Due to information asymmetry, investors do not have full insight into a company's ESG related practices or risks. Therefore, ESG index inclusion could be seen as a signal of quality since it confirms that the firm has strong governance, takes social responsibility and follows sustainable practices, which reduces uncertainty and increases investor confidence (Berkman et al., 2023). The study by Dyck, Lins, Roth and Wagner (2019) supports the presence of such signaling effects. They argue that institutional investors, influenced by ESG signals, actively allocate capital toward more sustainable firms. Their study find that higher institutional ownership is associated with high corporate social responsibility since these investors do not only value financial metrics but also sustainability practices. In addition, the study demonstrates that engaged institutional investors motivate firms to improve their ESG practices, which creates a feedback loop in which investor engagement and corporate behavior reinforce one another.

Investor interest, media attention and analyst coverage

Being included in certain indices increases investor interest, media attention and analyst coverage, which contributes to increased volumes and higher prices (Goyal and Wahal, 2025). On the other hand, if a stock is deleted from the index, it could have the opposite effect, leading to lower prices and volumes being traded (Goyal and Wahal, 2025). This effect is related to the behavioral finance literature. Merton (1987) argues that there is a "shadow cost" of incomplete information. Therefore, investors tend to seek value-related information to facilitate their trading decisions, such as financial news coverage around index rebalancing (Merton, 1987). In addition, media coverage increases investor attention and awareness, which has been shown to play an important role in investors' buying behavior (Barber and Odean, 2008). Investor attention or awareness is enhanced by events such as financial news, which can generate buying behavior by investors around index rebalancing, potentially resulting in better price informativeness (Barber and Odean, 2008).

B. ESG in finance

The term "ESG" refers to the consideration of environmental, social and governance criteria to measure an organization's environmental and societal impact, as well as their governance structure (OECD, 2020). The term has become widespread and established in finance as more investors have started using ESG-ratings and ESG data in investing (Pástor, Stambaugh and Taylor, 2021). Motives for investors could for example be related to regulatory constraints, personal or ethical considerations or a potentially higher risk-return tradeoff (OECD, 2020). In recent years, there has been an increase in the number of ETFs and mutual funds with an ESG focus and ESG investing is one of the fastest growing areas within asset management (Pástor et al., 2021). 72 new sustainable funds were launched globally just in the second quarter of 2025, which is up from 57 in the previous quarter (Morningstar, 2025).

ESG indices differ from traditional indices in that their constituents are chosen not only based on financial metrics but also based on ESG criteria (MSCI, 2024). This dual-selection mechanism generates a distinct feature that could influence how these indices are perceived by the market. Furthermore, the motivations and behavior of ESG fund investors may differ from that of passive index investors (Bolton and Kacperczyk, 2021). ESG index investors may respond not only to mechanical trading demands but also to the real social or environmental effects that inclusion signals (Dyck et al., 2019). These additional motives might impact the level and duration of price and volume effects in a certain way (Dyck et al., 2019). With the growing flow of ESG fund exposure and sustainable investing, we address a critical gap by investigating if and how the traditional index effect occurs in ESG indices.

III. MSCI Choice ESG Screened Indexes

The MSCI Choice ESG Screened Indexes are free float-adjusted market capitalization weighted indices that reflect the performance of firms with an above average ESG ratings relative to peers in the same sector. These indices exclude companies that are involved in controversies or in controversial business activities, including controversial weapons, tobacco, and fossil fuels (MSCI, 2024).

The eligible universe for the MSCI Choice ESG Screened Indices includes all the constituents of their respective MSCI underlying parent index. The underlying index for each index is outlined in figure 1 below.

Figure 1

MSCI Choice Screened Indices and the underlying indices

Figure 1 presents the parent, or underlying, index for each of the three versions of the MSCI Choice ESG Screened Index. The constituents of the underlying index represent all stocks that could be eligible for inclusion in the sustainability-focused version, if they meet the sustainability-related criteria.

MSCI Choice ESG Screened Index	Underlying Index
MSCI EAFE Choice ESG Screened Index	MSCI EAFE Index
MSCI EM (Emerging Markets) Choice ESG Screened	MSCI Emerging Markets Index
MSCI USA Choice ESG Screened Index	MSCI USA Index

Based on MSCI ESG Ratings, the MSCI Choice ESG Screened Indices identify companies that have demonstrated a commitment and an ability to successfully manage their ESG risks and opportunities. To be eligible for inclusion, firms are required to have an MSCI ESG Rating of 'BBB' or above. The MSCI ESG Ratings system aims to measure firms' management of environmental, social and governance risks and opportunities and indicates how the firm manages relevant key issues relative to industry peers. MSCI ESG Ratings use a weighted average key issue calculation that is normalized by industry to provide an industry-adjusted ESG score, ranging from 0 to 10, which is then translated to a seven-point scale from 'AAA' to 'CCC' (MSCI, 2024).

In addition, the MSCI Choice ESG Screened Indices use MSCI ESG Controversies Scores to identify companies that are involved in serious controversies with an environmental, social, or governance impact of their operations and/or products and services. For a stock to be eligible for inclusion in any of these indices, the firms are required to have an MSCI ESG Controversies Score of 3 or above (MSCI, 2024).

The MSCI Choice ESG Screened Indices are reviewed and rebalanced on a quarterly basis. Additions and exclusions are implemented as of the close of the last business day of February, May, August and November. However, the index constituents are reviewed on a monthly basis for the involvement in ESG controversies. If a constituent is considered to be involved in ESG controversies, they will be deleted. For a deletion to occur, these controversies should be classified as Red Flags, i.e. have an MSCI ESG Controversies Score of 0. A Red Flag indicates an ongoing, severe ESG controversy affecting the company directly through its actions, products, or operations. Index revisions are usually announced nine business days before becoming effective. The MSCI Choice ESG Screened Indices do not maintain a fixed number of constituents. The number of constituents may fluctuate at each rebalancing as firms are added or removed (MSCI, 2024).

To conduct this study, we chose the MSCI Choice ESG Screened Indices for a number of reasons. These indices are specifically designed to reflect the performance of companies that fulfill certain ESG criteria and offer a transparent methodology for how stocks are selected, added, or deleted from the indices. Second, these indices cover three different global markets; EAFE (Europe, Australasia and Far East), USA and Emerging Markets, which makes it possible to compare and draw conclusions from different geographic regions. Third, the MSCI Choice ESG Screened Indices are among the more widely used ESG benchmarks, making them likely to trigger observable market reactions after index revisions (MSCI, 2024). The prominence of these indices are relevant for investment flows, index-tracking products such as ETFs, and institutional investor behavior (MSCI, 2024).

IV. Data collection

Index membership revisions have been collected separately for the three indices from Bloomberg. We find 275 additions and deletions to the EAFE index during the quarterly rebalancing events in 2023, 2024 and 2025, excluding the last rebalancing event in 2025 since this had not occurred at the time when the data was collected. Out of the sample of 275 actions, we exclude 18 actions that occurred on other dates than the official rebalancings.

This method is following the one used by Chen et al. (2004), which excludes stocks that were added to or deleted from the index due to a merger or takeover, spinoff or change in share type or for which there is insufficient data. In the case of the MSCI Choice ESG Screened indices, another reason for irregular additions and deletions could be that new information has emerged, for example non-compliance with MSCI ESG criteria, leading to immediate exclusion (MSCI, 2024). This filtering leads to the dataset being more representative of the full population of ESG indices, since the stocks that are included/excluded at irregular dates are affected by significant simultaneous events. This introduces a risk of the observed effect on stock returns or trading volume being caused by this event rather than the index inclusion/exclusion, meaning increased worries of omitted variable bias. By excluding these, we are minimizing this risk and thus improving internal validity of the study. However, it also decreases the sample size slightly, which lowers statistical power (Stock and Watson, 2019). It could also create sample selection bias if the excluded firms are systematically different from the included firms. This is not necessarily an issue in this case, but it is important to acknowledge the potential risk. It also has an important implication for the external validity of the study, since it means that the effect that we find cannot be interpreted as the general effect of index inclusion/deletion, but rather only the effect of planned rebalancing. Additionally, the stocks that are irregularly deleted are often removed because of involvement in controversies, meaning our final sample excludes these stocks. This has an effect on the interpretation of external validity of the findings.

We find 133 additions and deletions to the USA index during the quarterly rebalancing events in 2025 and 2024. For this index, we do not include constituent changes as a result of rebalancing in 2023 due to lack of data. Out of the sample of 133 actions, we exclude 19 actions that occurred on other dates than when the official rebalancing occurred for the reasons outlined above.

For the EM index, we find 170 additions and deletions during the quarterly rebalancing events in 2025, including the last rebalancing date in 2024. We include only 2025 and one rebalancing event in 2024 due to lack of data on rebalancing events dating back further than this. Out of the sample of 170 actions, we exclude 13 actions that occurred on other dates than when the official rebalancing occurred.

Our dataset differs from Chen et al. (2004) in the sense that it contains a shorter time period due to lack of available data for rebalancing of the indices further back in time. This shorter time frame increases the risk of time-specific bias in comparison to if the dataset had been longer. If a specific market condition prevailed during 2023 to 2025 (for example regarding macroeconomic conditions, monetary policy or investor sentiment towards ESG), it may distort the index effect observed in our analysis. This means the results may reflect unique market conditions rather than a general index effect, reducing external validity.

Daily stock price and trading volumes for the stocks in our sample are obtained for a time interval extending from the last rebalancing date to the next rebalancing date. This interval stretches from approximately 90 trading days before the actual rebalancing event to 90 trading days after the actual rebalancing event, following the method presented by Chen et al. (2004). It includes the announcement date, which usually takes place around 11 to 15 trading days before the actual rebalancing date.

In our data analysis, another three stocks (two from the EAFE index and one from the USA index) are excluded from the sample due to insufficient data to perform the necessary calculations outlined in the methodology section. After these adjustments, the final sample consists of 525 events.

Figure 2
Number of observations

This figure illustrates the number of stocks on which we have based the analysis. We outline the number of joiners and leavers to or from the index for all three versions of the index as well as the full sample consisting of all three indices. Column 2 is the initial number of stocks. Column 3 and 4 show the number of stocks that have been excluded from the final sample due to either them being irregular events or other factors. The final sample is presented in column 5.

	Initial sample	**Irregular events**	**Other**	**Final sample**
MSCI EAFE	275	18	2	255
Joiners | Leavers	*101|174*	*8|10*	*1|1*	*92 | 163*
MSCI USA	133	19	1	113
Joiners | Leavers	*42|91*	*7|12*	*0|1*	*35 | 78*
MSCI EM	170	13	0	157
Joiners | Leavers	*102|68*	*5|8*	*0*	*97 | 60*
FULL SAMPLE	578	50	3	525
Joiners | Leavers	*245|333*	*20|30*	*1|2*	*224|301*

To complement this analysis, we will also investigate if there is a certain type of companies that are affected more than others by index inclusion or deletion by performing a cross-sectional regression. We obtain data on firm characteristics for the stocks that have been either included in or removed from the MSCI Choice ESG Screened indices through Bloomberg. The firm characteristics chosen are summarized in the table below.

Figure 3
Firm characteristics in the cross-sectional regression

In the first column of figure 3, we show the 5 factors that are considered for each stock in the cross-sectional regression. A brief explanation of the factors are provided in column 2. The label is the short form which we have used in the data collection and analysis. This label appears in column 3. All firm characteristics are taken one day before the announcement date (AD-1) as shown in column 4.

Factor	**Explanation**	**Label**	**Timing**
Momentum	Performance over the last 6 months	momentum	AD-1
Price to book (P/B) ratio	Proxy for growth vs value	px_to_book	AD-1
Market capitalization	Firm size	mkt_cap	AD-1
Average bid-ask spread	Proxy for liquidity	bid_as_spread	AD-1
Industry and sector according to the BICS definition	What industry the firm belongs to	Dummy Tech Dummy Cons Dummy Indust Dummy Bas mat Dummy Finans Dummy Util Dummy Comm Dummy Energy	AD-1

The values for each stock and each factor are collected at exactly one trading day prior to the announcement date (AD-1). We chose this timing because it provides the most recent and accurate information at the time of the event without risking that the factors have been influenced by the event itself. This approach follows standard event study methodology (MacKinlay, 1997), which emphasizes that explanatory variables and control factors should be measured prior to the event to avoid issues of endogeneity and event-related influence.

We chose these specific factors since they can all contribute to explaining the variation in cumulative abnormal return, following the characteristics used by Bolton and Kacperczyk (2021). The momentum factor helps control for pre-existing price trends that could bias our results, since stocks that have performed well in the past tend to continue performing well in the near future, and vice versa (Jegadeesh and Titman, 1993). Therefore, a stock might show high CAR after inclusion simply because it has been trending upward already before inclusion. The price to book ratio can be used to identify growth and value stocks (Fama and French, 1993). Firms with a high P/B ratio are typically classified as growth stocks with higher market expectations and strong anticipated future performance (Fama and French, 1993). In contrast, firms with a low P/B ratio are considered to be value stocks, often undervalued relative to their fundamentals (Fama and French, 1993). Thus, the P/B ratio helps test whether growth and value stocks react differently when being included or excluded from an ESG index. Market capitalization is a measure of firm size and a standard variable in regression models and event studies (Fama and French, 1993). Larger firms tend to have greater analyst coverage, higher liquidity and more stable investor bases, which may affect the price reaction (Banz, 1981). In contrast, smaller firms might experience stronger abnormal returns due to lower liquidity and higher information asymmetry (Barber and Odean, 2008). A narrow bid-ask spread indicates higher liquidity because investors trade the stock more easily without causing large price movements (Amihud and Mendelson, 1986). Conversely, a wide spread reflects lower liquidity and higher trading frictions (Amihud and Mendelson, 1986). Including the bid-ask spread variable can indicate how liquidity affects stocks' reactions to ESG index inclusion and deletions. Finally, industry and sector according to Bloomberg's Industry Classification System (BICS) are included to identify possible patterns for stocks in the same industries.

Figure 4

Summary statistics of dependent and independent variables

This table reports summary statistics for the dependent and independent variables used in the analysis. The first two panels (addition panel and deletion panel) show the cumulative abnormal return (CAR) over the different event windows as well as the trading volume ratio, denoted as Abnormal Volume (AV), at day 0 and day 13. Day 0 is the announcement date (AD) and day 13 is 13 days after announcement. ED denotes "Effective date", which is the same as the rebalancing date. The last panel shows the different firm characteristics.

PANEL: Additions

	Mean	Median	Min	Max
CAR AD	0.0005	0.0014	-0.0560	0.0952
CAR AD_ED	0.0032	0.0076	-0.9552	0.2580
CAR AD_ED+20	-0.0172	-0.0119	-1.0923	0.4849
CAR AD_ED+60	0.0269	0.0235	-0.3981	0.6642
AV Day 0	1.1865	0.9595	0.0000	5.4866
AV Day 13	5.1690	1.1747	0.0000	119.3087

PANEL: Deletions

	Mean	Median	Min	Max
CAR AD	-0.0017	-0.0034	-0.1408	0.2580
CAR AD_ED	-0.0164	-0.0107	-0.2442	0.5033
CAR AD_ED+20	-0.0248	-0.0266	-0.5867	0.4183
CAR AD_ED+60	-0.0294	-0.0324	-0.7730	0.4055
AV Day 0	1.4455	1.1865	0.1664	6.9085
AV Day 13	5.9116	1.7445	0.0000	118.0538

PANEL: Firm Characteristics

	Mean	Median	Min	Max
momentum	2.007920e+01	0.000000e+00	-4.400000e+03	6.800000e+03
px_to_book	2.931200e+00	1.454300e+00	2.810000e-01	7.433860e+01
mkt_cap	6.981051e+11	4.876130e+10	2.217705e+09	1.710007e+13
bid_ask_spread	5.441700e+00	2.860000e-01	7.000000e-04	1.666330e+02

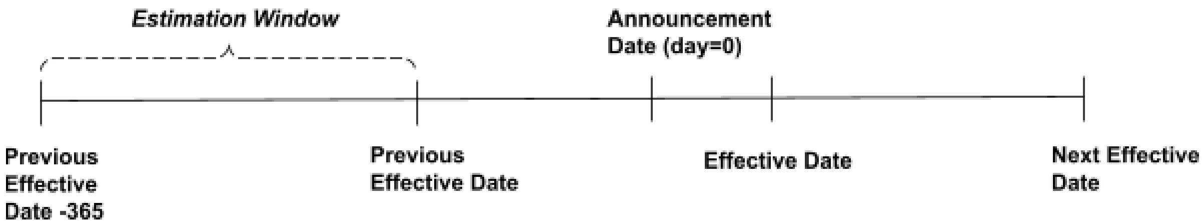
Figure 4 shows summary statistics over the independent and dependent variables used in the analysis. We have chosen day 0 and day 13 because they represent the announcement and most common effective date (ED) used in the analysis. The last panel shows the different firm characteristics. Overall, the table shows small deviations from zero for CAR over the different event windows, but generally positive for additions and negative for deletions. Trading volume is closer to 1 at announcement date than at day 13 for both the mean and median for additions and deletions, but the difference between the mean and the median is larger for day 13. That can be explained by large outliers that are heavily affecting the mean, shown by the large maximum values. The most deviant data regarding the firm characteristics is market capitalization, since it has a very large maximum value and a significantly larger mean and median, pointing to the data being right-skewed.

V. Methodology

We use the event study methodology proposed by MacKinlay (1997) and Chen et al. (2004) to investigate the effect associated with changes of the composition of MSCI Choice ESG Screened Indices on stock prices and trading volume. Day 0 is the inclusion or deletion announcement, which occurs a few days before the actual rebalancing takes place. Depending on the different rebalancing dates and the different indices, the announcement is usually made 11 to 15 days before the changes are implemented. Announcement dates as well as the actual rebalancing dates are included in the event window. A timeline is provided below.

Figure 5
Timeline

This timeline illustrates the estimation window in relation to the announcement date and effective date. The estimation window is 365 days long, starting one year before the previous effective date. The length between two effective dates is approximately 90 days. The effective date takes place around 11 to 15 days after the announcement date, which is denoted as day 0.



Throughout the whole paper, the unit of analysis is firm-event, since we analyze effects at event level (every index inclusion or deletion for a given firm constitutes one unit of analysis). The raw data contains daily stock-level observations, but the analysis is performed at firm-event level.

A. Stock return effect

Abnormal returns are calculated as the difference between the observed stock return and the expected return estimated from a market model following the event study method suggested by MacKinlay (1997). The cumulative abnormal return (CAR) is then obtained by summing the daily abnormal returns over the different CAR windows (figure 6). Statistical significance of the CARs is tested using a two-tailed t-test to evaluate whether they differ significantly from zero. A detailed description of the methodology is provided in this section.

To assess the stock return effect on index inclusion and deletions, we look at different event windows and calculate the cumulative abnormal return (CAR). The CAR periods are based on the periods used by Chen et al. (2004). This methodology provides a well-established ground for evaluating the persistence of price and volume reactions after index revisions. Chen et al. (2004) suggests that stock prices may revert after the event, so extending the event windows until 20 days and 60 days after the effective date allows us to determine whether the effects are temporary or permanent. Also, Beneish and Whaley (1996) find decreasing significance of the effects of stock index inclusion up to 60 days after the ED, which again motivates the longer event windows. By including all different event windows, we are able to capture both short-term and long-term effects.

Figure 6
Cumulative abnormal return (CAR) period

Figure 6 shows the four different event windows during which we calculate the cumulative abnormal returns. The first "period" consists of one single date, meaning that we calculate the abnormal return only on this day. These periods are chosen based on the methodology used in the event study by Chen et al. (2004). For the rebalancing dates in August 2025, we calculate cumulative abnormal returns only over the first two event windows, as sufficient data for the subsequent windows were not yet available at the time of collection. Below we also provide the labels used throughout our analysis when referring to these periods. "AD" stands for "Announcement Date" and "ED" stands for "Effective Date", referring to the date on which the change in index constituents is implemented.

Cumulative abnormal return (CAR) period	Label
Abnormal return on announcement date	AD
Cumulative abnormal return from the announcement date to the effective date	AD → ED
Cumulative abnormal return from the announcement date to 20 days after the effective date	AD → ED+20
Cumulative abnormal return from the announcement date to 60 days after the effective date	AD → ED+60

One of the most crucial assumptions of the event study is that nothing else happened at the same time which affects stock returns, since it in that case would be hard to isolate the effect of inclusion in or exclusion from the index (MacKinlay, 1997). To ensure that this assumption holds, we have used short event windows (both AD and AD to ED are short time periods), which decreases the risk of confounding events. Furthermore, revisions occur at different dates since the index is rebalanced four times per year and we have used data between 2023 and 2025. This frequency means it is unlikely that industry or macro events would happen systematically at the same time as index rebalancing. We are also using abnormal returns, which removes the effect of general market movements (MacKinlay, 1997). Additionally, inclusion or exclusion of the MSCI indices are publicly known processes that are not initiated by the firms themselves (Stock and Watson, 2019). This means they are to the largest extent exogenous shocks, which reduces the concern of endogeneity (Stock and Watson, 2019). However, for index inclusion/exclusion to be an exogenous shock, there cannot be an anticipation effect since that means the treatment is not a shock to the market and there is a risk of bias. Therefore, we assume exogeneity but will more carefully discuss how reasonable this assumption is and the implications of it in the discussion section.

To begin our analysis, we calculate the actual daily returns for each stock in our final sample from the stock prices, which we explain in the data collection section. The actual return ($R_{i,t}$) for each day is calculated by using equation 1:

$$R_{i,t} = \frac{P_{i,t} - P_{i,t-1}}{P_{i,t-1}} \quad (1)$$

The abnormal return ($AR_{i,t}$) is defined as the difference between a stock's actual return and the expected return (MacKinlay, 1997). It is given by equation 2:

$$AR_{i,t} = R_{i,t} - E(R_{i,t}) \quad (2)$$

In equation 2, we use the actual change in stock price from one day to another together with the expected return, which we obtain from estimations using the CAPM, the Capital Asset Pricing Model (equation 3). We chose a market model approach instead of a constant mean return model because the market model represents a refinement by removing the part of a stock's return that is related to variation in the market's return, thereby reducing the variance of abnormal returns (MacKinlay, 1997). MacKinlay (1997) further argues that the incremental benefits of using a multifactor model in event studies are limited as the additional factors only offer a small marginal explanatory power beyond the market factor and that there is thus little reduction in the variance of the abnormal return. We therefore apply a market model based on CAPM. The equation used for the CAPM is as follows (Fama, 1970):

$$E(R_{i,t}) = R_{f,t} + \beta_i\big(E(R_{m,t}) - R_{f,t}\big) \quad (3)$$

To ensure the validity of the CAPM as a benchmark for expected returns, we have to make certain assumptions. First, we assume that the market is efficient, or more specifically, that all publicly available information is quickly reflected in stock prices (Fama, 1970). Another key assumption in the CAPM is that return on assets can be directly explained by their sensitivity to the market portfolio (beta coefficients) (Fama, 1970). In other words, there is a linear relationship between the expected return and beta. We assume that the beta coefficient of a stock relative to the market does not change significantly from the estimation window to the event window. In reality, however, CAPM is a simplification and may not capture all risks, such as risks related to size, value and momentum factors. Therefore, some researchers prefer multi-factor models such as the Fama-French 3 factor or 5 factor model since these models could potentially be more accurate in predicting the expected return (Fama and French, 2015). In this study we chose to work with CAPM for reasons explained above.

The CAPM model requires estimating a beta coefficient for each stock in our sample (Fama, 1970). We estimate the beta coefficients by running an OLS regression over the estimation window (figure 5), following the framework of Fama (1970). The estimation is done by using the daily price in an interval from one year before the last effective date until the last effective date. Beta coefficients are used together with the daily risk free rate ($R_{f,t}$) and the corresponding market return ($R_{m,t}$) to calculate an estimated return for each stock, according to the CAPM (Fama, 1970). The regression is performed in Bloomberg, using the following regression model:

$$R_{i,t} - R_{f,t} = \alpha_i + \beta_i(R_{m,t} - R_{f,t}) + \varepsilon_{i,t} \quad (4)$$

The estimation window is one calendar year, ending around 90 days before the announcement date. To prevent the event from influencing the normal performance model parameter estimates, the event window should not be included in the estimation window (Campbell, Lo and MacKinlay, 1997). Campbell et al. (1997) also recommend having a buffer period between the end of the estimation window and the beginning of the event window, to avoid contamination by pre-event movements. We have chosen to estimate our beta coefficients for the expected return calculations by using daily observation of price during one year. For event studies, it is recommended to use 200–250 daily observations before the event for beta estimation, which is roughly one calendar year considering that the stock market is closed on weekends and holidays (Campbell et al., 1997). The estimation period should be long enough to estimate model parameters with statistical precision, yet short enough to ensure that the parameters remain stable over time (Campbell et al., 1997). We compute beta as the slope coefficient from a regression of the stock's returns on the benchmark's daily returns over the specified period. As the benchmark, we use different indices for our three subsamples. The MSCI ESG Screened Choice Indexes are constructed by applying ESG exclusion screens to an underlying index (MSCI, 2024). These underlying indices are used as a benchmark for the beta regression, as well as for the market proxy (figure 1).

In order to be able to draw the overall interference for the event, the abnormal returns are aggregated over the different event windows (figure 6). We calculate the cumulative abnormal return (CAR) by summing up the abnormal returns over the event windows for each stock (equation 5). The mean abnormal return is calculated for all firms over the event windows, which we do separately for additions and deletions (equation 6). The variance is also calculated for all stocks over the event windows (equation 7). T_1 and T_2 in equations 5 - 8 denote the first and last day of the event windows, respectively. This analysis is done for our three subsamples alone as well as for the whole sample of 525 stocks.

$$CAR_{i,[T_1,T_2]} = \sum_{t=T_1}^{T_2} AR_{i,t} \quad (5)$$

$$\overline{CAR}_{[T_1,T_2]} = \frac{1}{N}\sum_{i=1}^{N} CAR_{i,[T_1,T_2]} \quad (6)$$

$$Var[CAR_{T_1,T_2}] = \frac{1}{N-1}\sum_{i=1}^{N}(CAR_{T_1,T_2,i} - \overline{CAR}_{T_1,T_2})^2 \quad (7)$$

We calculate sample variances and perform a two-tailed t-test to test statistical significance of the CARs. The two-tailed t-test allows both positive and negative CARs, meaning that it tests whether the CARs are significantly higher or lower than expected without assuming a specific direction of the effect. Thus, the null hypothesis is that CAR=0 while the alternative hypothesis is that CAR≠0. We perform a t-test using equation 8:

$$t\text{-stat} = \frac{\overline{CAR}_{[T_1,T_2]}}{\sqrt{\mathrm{Var}(CAR_{[T_1,T_2]})}} \quad (8)$$

To be able to calculate the variance of abnormal returns, we make the assumption that in the cross section the abnormal returns are uncorrelated. This assumption is argued to be valid if the event windows do not overlap, which is the case in our study since we have relatively short event windows and the index is rebalanced quarterly (MacKinlay, 1997). Additionally, stocks come from different industries and countries, which further reduces the inter-correlation (Brown and Warner, 1985). Thus, following prior literature, we make the assumption of zero cross-sectional correlation of abnormal returns. Further, to do the hypothesis testing using the student's t-distribution, we assume the cumulative abnormal return follows the normal distribution (Stock and Watson, 2019). This is reasonable due to the samples being large (almost all samples have N>30) and we assume the cumulative stock returns are identically and independently distributed, which means it follows from the central limit theorem that the cumulative abnormal returns are approximately normal (Stock and Watson, 2019). However, the sample of inclusions for the USA index and exclusions for the EM index have fewer than 30 observations for the longer event windows, meaning these results should be interpreted more carefully. No clustering is used since we compute one CAR per firm per event window, creating a cross-sectional dataset without within-firm serial correlation and thus clustering is not required (Stock and Watson, 2019).

B. Trading volume effect

To examine the effect on trading volume after index revisions, we analyze the abnormal trading volume using MacKinlay's (1997) constant mean model. With this method, we calculate a volume ratio by using the average trading volume for a stock in the estimation window as a benchmark trading volume. This is the same estimation window as used for beta estimation when looking at stock returns (figure 5). We then assess whether trading volume exhibits abnormal behavior after the announcement by applying the same two-tailed t-test used for the CARs. This test evaluates whether the average abnormal trading volume differs significantly from zero. To find the average trading volume we use the following equation:

$$\bar{V}_i = \frac{1}{N}\sum_{t=1}^{N} V_{it} \quad (9)$$

The volume ratio is calculated by comparing the trading volume on a certain day with the benchmark trading volume found in equation 8. The equation for the volume ratio ($AV_{i,t}$) is as follows (MacKinlay, 1997):

$$AV_{it} = \frac{V_{it}}{\bar{V}_i} \quad (10)$$

The volume ratio is calculated separately for our three subsamples as well as for the whole sample over each day in the event windows. To evaluate statistical significance of abnormal trading volume over the different event windows, we calculate sample variances and perform t-tests to determine if the observed results are statistically different from one. The t-tests test the hypothesis that the ratio is statistically different from one, against the null hypothesis that the ratio is equal to one (equation 11). Since we are comparing one observation per firm and day there is no time dimension or serial clustering, thus clustering is not applicable.

$$t\text{-stat} = \frac{\overline{AV}_i}{\sqrt{\mathrm{Var}(AV_i)}} \quad (11)$$

C. Regression on firm characteristics

In this part of our analysis, we investigate if there are a certain type of companies that are affected more than others by index inclusion or deletion by performing a cross-sectional regression. The firm characteristics chosen are momentum, price to book (P/B) ratio, market capitalization, average bid-ask spread and industry and sector. These factors are all summarized and explained in figure 3. We also provide a motivation for why we have chosen these specific factors in our regression in the data collection section above. Based on these factors, we use the following regression model:

$$\mathrm{CAR}_i = \beta_0 + \beta_1 \mathrm{Momentum}_i + \beta_2 \mathrm{PxToBook}_i + \beta_3 \log(\mathrm{MktCap}_i) + \beta_4 \mathrm{BidAskSpread}_i + \sum_{k=1}^{7} \gamma_k \mathrm{IndustryDummy}_{ik} + \varepsilon_i$$

CAR_i is the cumulative abnormal return for each firm given the four different dependent variables; CAR_AD, CAR_AD_ED, CAR_AD_20 and CAR_AD_60. Each coefficient (each beta) describes the effect of that firm characteristic on cumulative abnormal return, holding the other factors constant. We have used the natural logarithm of market capitalization since market capitalization is right skewed (there are some firms that are many times larger than the rest), so by using the natural logarithm we are reducing the influence of outliers and thus better satisfy the assumption regarding no outliers (Stock and Watson, 2019). In this analysis, all stocks have a positive market capitalization. Thus, there is no issue with taking the natural logarithm out of zero. We have then used a dummy variable for every industry, where Technology is omitted to avoid the issue with multicollinearity caused by the dummy variable trap (Stock and Watson, 2019). This means the other industry variables measure performance relative to Technology firms. We have also used robust standard errors, to adjust for the fact that the errors are likely heteroskedastic (Stock and Watson, 2019).

To be able to make the OLS regressions to find which firm characteristics are explaining abnormal returns, we make the standard assumptions of the multiple linear regression. We assume that the error term is not correlated to any of the independent variables, that the observations are identically and independently distributed, that outliers are rare and no perfect multicollinearity (Stock and Watson, 2019). The last assumption is relaxed by excluding one dummy variable of an industry from the regression, since including it would impose perfect collinearity with the intercept term. We further relax the assumption of homoskedasticity by using robust standard errors. We are not using clustering since it is not possible to cluster by firm since each firm only appears once and if we would cluster by rebalancing date we would get the issue with too few clusters (Stock and Watson, 2019). Thus, ordinary heteroskedasticity-robust standard errors are appropriate. The exogeneity assumption is plausible since firm characteristics are pre-determined (before cumulative abnormal returns), which reduces the risk of reverse causality. As previously stated, rebalancing of indices is not initiated by firms and can therefore be argued to be an exogenous shock that is not correlated with other factors that are systematically related to firm characteristics (Stock and Watson, 2019).

VI. Results

A. Stock return

Index inclusion

Panel 1a reports our findings from the stock return analysis for index inclusion across the EAFE, USA and EM subsamples as well as the full sample. We find no significant abnormal returns over the event windows after a stock has been included in the ESG index. For all markets and event windows, the t-statistics are small and most are in fact quite far from significant. This suggests that the observed CARs are indistinguishable from zero. In addition, we also notice that several of the mean CARs are negative, possibly contradicting our hypothesis that being included on the ESG index leads to positive abnormal returns. Even in the windows where mean CARs are positive (e.g. AD → ED+60 for EAFE and EM), the large variances and high standard deviations imply substantial cross-sectional dispersion, making it difficult to draw any conclusions about systematic inclusion effects. Taken together, the evidence suggests that index inclusion does not lead to a significant price effect, and any small movements observed appear to be noise. These results contradict our hypothesis, as we were expecting to find significant effects on stock returns after index inclusion. They also stand in contrast to traditional studies on the index effect (Chen et al., 2004; Harris and Gurel, 1986), which find positive and statistically significant abnormal returns following inclusion in major stock indices.

Panel 1a
Cumulative abnormal return (CAR) for index inclusion

This panel shows the number of observations, mean, variance, standard deviation and result from the t-test for index inclusion. The panel includes all three subsamples as well as the full sample. We present the findings over four different event windows. The first event window is at the announcement date (AD), only including that one date. The other event windows all span from the announcement date and stretch to the effective date (ED), 20 days after the effective date (ED+20) and 60 days after the effective date (ED+60).

Panel A: Inclusion

	No. observations	Mean	Variance	St. deviation	T-test
EAFE					
AD	92	0,000155	0,000342	0,018493	0,008401
AD → ED	92	0,002146	0,004750	0,068918	0,031145
AD → ED+20	85	0,004349	0,008696	0,093252	0,046635
AD → ED+60	85	0,036932	0,022577	0,150255	0,245796
USA					
AD	34	−0,003014	0,000524	0,022888	−0,131692
AD -> ED	34	0,001122	0,006552	0,080946	0,013863
AD → ED+20	26	−0,032549	0,009074	0,095257	−0,341693
AD → ED+60	26	0,016920	0,009314	0,096510	0,175316
EM					
AD	97	0,001592	0,000398	0,019948	0,079810
AD → ED	97	0,001011	0,013306	0,115351	0,008768
AD → ED+20	69	−0,023267	0,028293	0,168206	−0,138327
AD → ED+60	69	0,042053	0,096642	0,310872	0,135273
Full sample					
AD	223	0,000297	0,000393	0,019813	0,014993
AD → ED	223	0,001497	0,008675	0,093140	0,016068
AD → ED+20	180	−0,011567	0,016333	0,127802	−0,090506
AD → ED+60	180	0,036004	0,048676	0,220626	0,163191

Index exclusion

Panel 1b reports our findings from the stock return analysis for index exclusion. We find no significant mean cumulative abnormal return over the event windows. Also here, the t-tests are quite far from significance. These t-statistics indicate that it is not possible to reject the null hypothesis of zero abnormal returns. For all indices, the mean CARs are negative values over the longer windows (AD → ED+20 and AD → ED+60), which suggests that excluded stocks may experience small downward price shifts. However, the wide variances and large standard deviations imply substantial dispersion in firm-level reactions, making these average effects statistically indistinguishable from zero. For example, in the full sample, CARs range from -0.0014 on the announcement date to -0.0345 in the AD → ED+20 window, yet none of these are close to statistical significance. These results contradict our hypothesis, as we were expecting to find significant negative effects on stock returns after index exclusion. Chen et al. (2004) found that the negative effect of deletions is much smaller or non-existent, hence, in the case of index deletion, our result is less contrasting to the existing literature. Our findings are consistent with those of Greenwood and Sammon (2024). Greenwood and Sammon (2024) argue that index exclusions typically do not generate large or persistent price effects because the market impact of deletions has diminished over time. Thus, while our hypothesis of a strong negative effect is not confirmed, the null results are supported by empirical findings that deletion effects have become smaller or insignificant.

Panel 1b

Cumulative abnormal return (CAR) for index exclusion

This panel shows the number of observations, mean, variance, standard deviation and result from the t-test for index exclusion. The panel includes all three subsamples (EAFE, USA and EM) as well as the full sample. We present the findings over four different event windows. The first event window is at the announcement date (AD), only including that one date. The other event windows all span from the announcement date and stretch to the effective date (ED), 20 days after the effective date (ED+20) and 60 days after the effective date (ED+60).

Panel B: Exclusion					
	No. observations	Mean	Variance	St. deviation	T-test
			EAFE		
AD	163	0,005808	0,001065	0,032636	0,177962
AD → ED	163	−0,010441	0,004685	0,068447	−0,152533
AD → ED+20	144	−0,028461	0,012237	0,110623	−0,257279
AD → ED+60	144	−0,035250	0,026785	0,163660	−0,215384
			USA		
AD	79	−0,013565	0,000508	0,022542	−0,601753
AD → ED	79	−0,034753	0,006997	0,083650	−0,415463

AD → ED+20	57	−0,053252	0,010067	0,100336	−0,530730
AD → ED+60	57	−0,045204	0,022067	0,148550	−0,304303
EM					
AD	60	−0,002998	0,000373	0,019310	−0,155271
AD → ED	60	−0,000454	0,012813	0,113193	−0,004008
AD → ED+20	29	−0,027520	0,033190	0,182181	−0,151059
AD → ED+60	29	0,043802	0,048989	0,221334	0,197898
Full sample					
AD	302	−0,001367	0,000845	0,029077	−0,047009
AD → ED	302	−0,020065	0,007030	0,083843	−0,239319
AD → ED+20	230	−0,034486	0,014278	0,119492	−0,288605
AD → ED+60	230	−0,027749	0,028871	0,169916	−0,163312

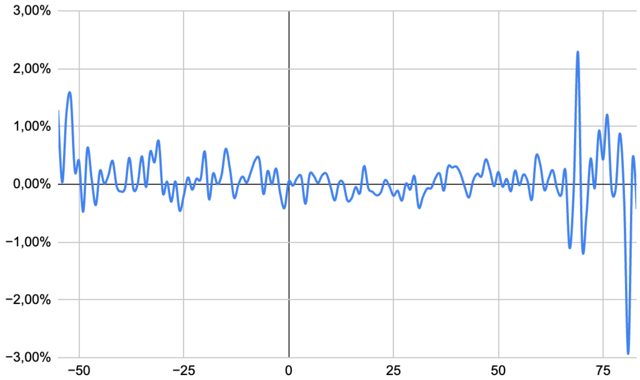
B. Time series plot for stock return

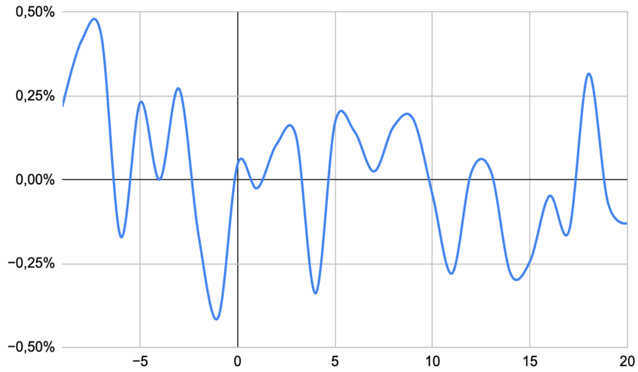
Graphs 1.1 to 2.2 shows the average abnormal return for the full sample over the event window as well as a period before the announcement date. Below are the separate plots for index inclusion and for index exclusion. We have also included a more zoomed in version (Graphs 1.2 and 2.2), clearly showing how abnormal return changes close to the announcement date. We can observe from the plots that there is no clear movement at the announcement date or another date before or after, but rather similar movements along the x-axis. This observation points to the data being noise rather than a clear relationship between abnormal returns and index inclusion or deletion. Thus, we can conclude that there are no obvious effects, neither for index inclusion nor index deletion. Not all stocks in our sample have data stretching more than 75 days after the announcement date, which is why we are careful to draw conclusions about the right most part of the graphs.

Index inclusion

Graph 1.1 and 1.2
Time series of Average Abnormal Return (Additions)

The left graph plots the daily average abnormal return for a period starting 60 days before the announcement date until 80 days after. This is the ARR for the full sample, comprising all three indices. The y-axis is average abnormal return and the x-axis is the day relative to the announcement date. Day 0 is the vertical line and denotes the announcement date. The graph to the right is more focused, starting 10 days before the announcement date and stretching until 20 days after.



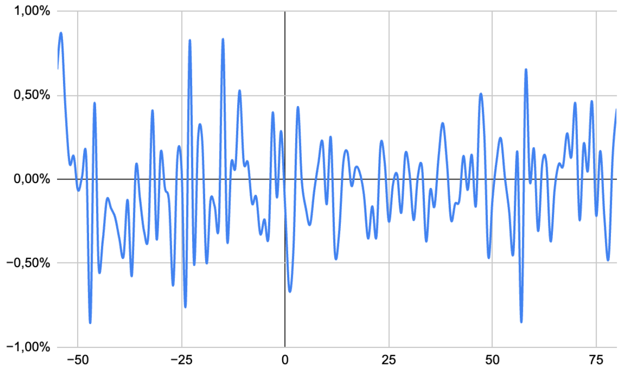


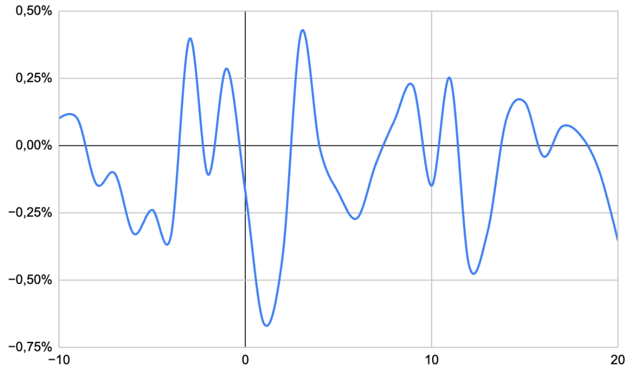
Index deletion

Graph 2.1 and 2.2

Time series of Average Abnormal Return (Deletions)

The left graph plots the daily average abnormal return for a period starting 60 days before the announcement date until 80 days after. This is the ARR for the full sample, comprising all three indices. The y-axis is average abnormal return and the x-axis is the day relative to the announcement date. Day 0 is the vertical line and denotes the announcement date. The graph to the right is more focused, starting 10 days before the announcement date and stretching until 20 days after.





C. Trading volume

Index inclusion

Table 1.1 presents the results from a t-test on the abnormal trading volume for all index inclusions. We have tested each subsamples separately as well as the sample as a whole, from 15 days before the announcement date until 20 days after. There are significant effects, and mostly so for the emerging markets index. However, the USA index contains a fewer number of observations in the deletions sample, which makes it more difficult to get significant results when performing a t-test. We can also note that there are significant results before the announcement date as well as after, even though it seems to be that most significance happens after day 0. This result supports our hypothesis that a stock experiences a significant increase in abnormal trading volume following inclusion in an ESG index.

Table 1.1

T-test results by Day for Additions (H_0: mean = 1, Days -15 to +20)

Table 1.1 presents the volume ratio for each day from 15 days before the announcement date (-15.0) until 20 days after the announcement date (20.0) for index inclusions. The symbols *, **, and *** denote significance at the 10 %, 5 %, and 1 % level respectively. The results are presented separately for the three subsamples (EM, USA and EAFE) as well as for the full sample (Combined).

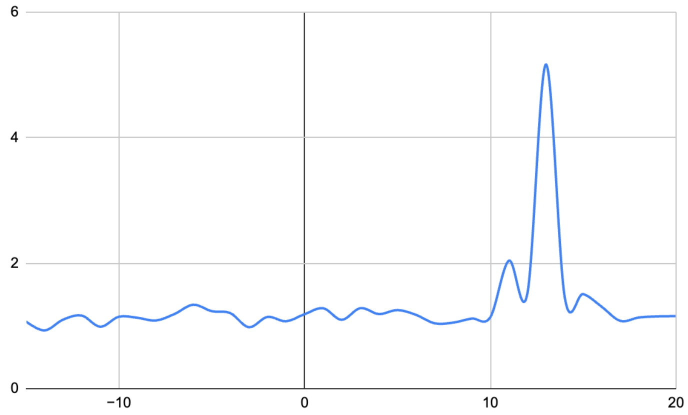
Day	EM (t)		USA (t)		EAFE (t)		Combined (t	
-15.0	1.095		0.125		0.809		1.232	
-14.0	-1.647		-0.016		0.831		-1.536	
-13.0	0.887		-0.75		1.57		1.379	
-12.0	1.708	*	0.585		0.74		1.707	*
-11.0	0.929		-0.573		1.161		-0.161	
-10.0	1.329		0.245		-2.936	***	1.859	*
-9.0	1.331		0.237		0.829		1.476	
-8.0	0.904		0.382		-0.626		1.214	
-7.0	2.103	**	0.522		1.888	*	2.26	**
-6.0	2.697	***	1.615		3.055	***	2.929	***
-5.0	3.768	***	1.192		0.89		3.17	***
-4.0	3.101	***	-1.915	*	1.373		3.31	***
-3.0	1.644		0.27		1.485		-0.433	
-2.0	2.484	**	0.449		1.362		1.759	*
-1.0	2.091	**	0.782		1.908	*	1.391	
0.0	2.621	**	-0.34		-1.077		3.221	***
1.0	2.744	***	-0.652		0.404		4.16	***
2.0	1.377		0.259		0.178		1.478	
3.0	2.216	**	-3.307	***	0.832		2.083	**
4.0	2.325	**	0.443		2.498	**	2.454	**
5.0	3.264	***	0.707		1.506		3.151	***
6.0	2.131	**	0.452		4.048	***	2.827	***
7.0	2.267	**	-0.225		2.147	**	0.747	
8.0	1.109		-2.742	**	2.443	**	1.118	
9.0	2.388	**	-0.717		2.845	***	1.934	*
10.0	2.708	***	1.346		0.09		2.339	**
11.0	2.769	***	2.376	**	1.681	*	3.231	***
12.0	3.57	***	0.616		0.589		3.382	***
13.0	2.304	**	-0.036		1.412		4.401	***
14.0	2.748	***	-0.231		1.26		2.826	***
15.0	3.856	***	0.213		1.862	*	3.408	***
16.0	2.741	***	0.739		2.366	**	3.894	***
17.0	1.709	*	0.61		1.464		1.199	
18.0	1.844	*	0.531		2.536	**	2.573	**
19.0	1.935	*	0.425		3.453	***	1.992	**
20.0	2.214	**	-0.171		0.347		2.665	***

In graph 3.1 we have plotted the abnormal trading volume starting before the announcement date until after the effective date. We see a clear spike in the graph at about 13 days after the announcement date. This can be explained by the fact that the actual inclusion is implemented at the effective date, which takes place at about 11-15 days after the announcement is made. The graph indicates that there is a steep increase in trading activity at the time of the actual index rebalancing. These findings are in line with Chen et al. (2004), as they report a significantly higher turnover ratio at this date. Similar to us, Chen et al. (2004) also find that the abnormal increase in volume turnover tapers off after the effective date.

Graph 3.1

Volume ratio by Day for Additions (H_0: mean = 1, Days -15 to +20)

Graph 3.1 illustrates the daily volume ratio from 15 days before the announcement date (-15) until 20 days after the announcement date (20) for index inclusion. The y-axis is the volume ratio and the x-axis is the number of days relative to the announcement date. This is the volume ratio for the full sample, comprising all three indices. Day 0 is the vertical line and denotes the announcement date.



Index Exclusion

Table 1.2 below shows the t-test for stock deletions from the ESG index during a period -15 to +20 from the announcement date. We observe multiple highly significant results, especially around the effective date (approximately day 13). We also observe that these results occur both before and after the announcement date. However, in line with the findings of Chen et al. (2004), we see no permanent effect on trading volume since it quickly reverts to normal levels after the announcement date. A comparison between trading volume effects after index inclusion (table 1.1) with index exclusion (table 1.2) indicate that we have more significant results for index exclusion.

Table 1.2

T-test results by Day for Deletions (H_0: mean = 1, Days -15 to +20)

This table presents the volume ratio for each day from 15 days before the announcement date (-15.0) until 20 days after the announcement date (20.0) for index deletion. The symbols *, **, and *** denote significance at the 10 %, 5 %, and 1 % level respectively. The results are presented separately for the three subsamples (EM, USA and EAFE) as well as for the full sample (Combined).

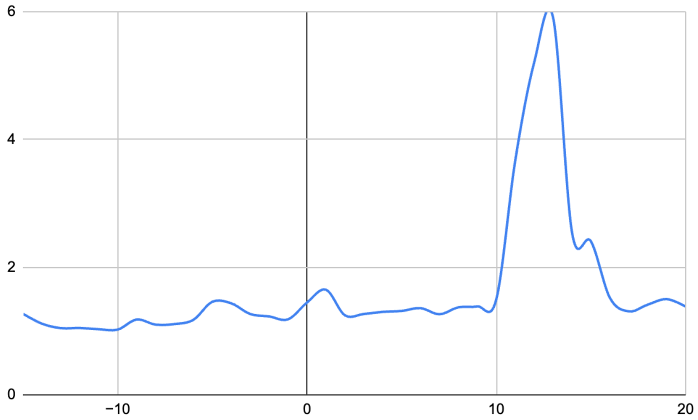
Day	EM (t)		USA (t)		EAFE (t)		Combined (t	
-15.0	1.365		2.285	**	2.067	**	2.314	**
-14.0	-1.792	*	1.265		0.682		2.833	***
-13.0	-2.262	**	0.259		3.202	***	1.314	
-12.0	-0.914		0.362		3.855	***	1.444	
-11.0	-1.836	*	1.112		0.84		0.807	
-10.0	-1.012		2.945	***	1.332		0.539	
-9.0	-0.737		3.788	***	2.876	***	3.716	***
-8.0	-4.999	***	3.159	***	5.289	***	2.589	**
-7.0	0.304		4.467	***	4.774	***	1.818	*
-6.0	1.066		4.082	***	4.119	***	2.77	***
-5.0	1.857	*	4.431	***	3.546	***	4.532	***
-4.0	0.898		3.827	***	7.226	***	6.519	***
-3.0	1.157		1.872	*	8.097	***	5.738	***
-2.0	1.945	*	2.855	***	4.889	***	4.776	***
-1.0	0.34		2.054	**	5.325	***	4.104	***
0.0	2.415	**	4.029	***	5.219	***	7.627	***
1.0	4.082	***	2.741	***	5.197	***	9.514	***
2.0	1.712	*	2.295	**	3.944	***	5.712	***
3.0	0.937		2.253	**	3.466	***	5.528	***
4.0	3.234	***	0.793		4.609	***	6.424	***
5.0	1.892	*	1.688	*	4.254	***	4.263	***
6.0	1.246		1.796	*	6.385	***	3.828	***
7.0	2.08	**	1.05		4.293	***	4.132	***
8.0	3.207	***	3.035	***	5.874	***	5.567	***
9.0	3.578	***	1.616		5.672	***	6.138	***
10.0	3.366	***	2.813	***	5.088	***	6.563	***
11.0	3.552	***	6.098	***	2.493	**	5.591	***
12.0	2.097	**	3.885	***	5.854	***	7.936	***
13.0	3.826	***	4.44	***	4.601	***	7.141	***
14.0	2.858	***	4.109	***	5.718	***	6.86	***
15.0	3.322	***	2.435	**	2.706	***	3.264	***
16.0	3.37	***	3.169	***	3.407	***	6.856	***
17.0	1.35		3.967	***	5.087	***	5.465	***
18.0	2.991	***	3.665	***	5.262	***	6.727	***
19.0	3.212	***	2.556	**	5.896	***	4.474	***
20.0	3.935	***	2.427	**	3.312	***	5.59	***

Graph 3.2 plots the abnormal trading volume for stocks that have been deleted from the ESG index. Again, we see a clear spike in the graph at around 13 days after the announcement date. The actual stock deletion takes place at around this time from the announcement, leading to a higher abnormal trading volume at around the effective date. As illustrated in graph 3.2, the magnitude of the spike is larger than in graph 3.1 for index inclusion, indicating that abnormal trading volume is higher for index deletion than index inclusion. These findings are consistent with Chen et al. (2004), who also report a higher turnover ratio for the deletions sample on the effective date compared to the additions sample, although both are statistically significant.

Graph 3.2

Volume ratio by Day for Deletions (H_0: mean = 1, Days -15 to +20)

Graph 3.2 illustrates the daily volume ratio from 15 days before the announcement date (-15) until 20 days after the announcement date (20) for index exclusion. The y-axis is the volume ratio and the x-axis is the number of days relative to the announcement date. This is the volume ratio for the full sample, comprising all three indices. Day 0 is the vertical line and denotes the announcement date.



D. Cross-sectional regression for stock returns

Index inclusion

In table 2.1 we see the results from the regression of firm characteristics on cumulative abnormal returns. The logarithm of market capitalization has a positive and highly significant coefficient at announcement date, and marginally significant between announcement and effective date. This observation points to larger firms experiencing higher abnormal returns upon index inclusion and particularly immediately. The coefficient for the bid ask spread is negative and highly significant in the first two model specifications, indicating that less liquid stocks tend to experience lower abnormal return close to (or at) announcement date of index inclusion. The value and momentum factors are insignificant through all model specifications, pointing to those characteristics not being driving forces behind abnormal returns related to index inclusion. Regarding the industry dummies, multiple of them are significant in the first model specification (consumer, basic materials, finance, utilities, communications and energy) and all of these have negative coefficients, showing that these industries tend to experience lower abnormal returns compared to the omitted category (Technology). This shows that at the announcement date, technology firms seem to experience a higher abnormal return than other firms. However, these effects seem to weaken over time since they are not significant in the other model specifications.

Table 2.1

Cross-sectional regression for stock return (Additions)

This table presents the cumulative abnormal returns at announcement date (CAR_AD), announcement date to effective date (CAR_AD_ED), announcement date to 20 days after the effective date (CAR_AD_20) and announcement date to 60 days after the effective date (CAR_AD_60) regressed on firm characteristics for all additions in the sample. The coefficients are presented together with standard errors in parentheses and symbols (stars) that represent the p-values. The symbols *, **, and *** denote significance at the 10 %, 5 %, and 1 % level respectively.

	CAR_AD	CAR_AD_ED	CAR_AD_20	CAR_AD_60
const	-0.0004**	-0.0011	-0.0018	-0.0007
	(0.0002)	(0.0008)	(0.0012)	(0.0021)
momentum	-0.0000	-0.0000	0.0000	-0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
px_to_book	-0.0000	-0.0000	-0.0000	-0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
log_mkt_cap	0.0000***	0.0001*	0.0001	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0001)
bid_ask_spread	-0.0000***	-0.0000***	-0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Dummy Cons	-0.0001**	-0.0002	-0.0001	0.0002
	(0.0000)	(0.0003)	(0.0003)	(0.0005)
Dummy Indust	-0.0001	-0.0003	-0.0001	0.0001
	(0.0001)	(0.0003)	(0.0004)	(0.0006)
Dummy Bas mat	-0.0002***	0.0002	-0.0000	0.0002
	(0.0001)	(0.0004)	(0.0007)	(0.0010)
Dummy Finans	-0.0001*	0.0001	-0.0000	0.0002
	(0.0001)	(0.0003)	(0.0003)	(0.0005)
Dummy Util	-0.0002**	-0.0004	0.0005*	0.0005
	(0.0001)	(0.0003)	(0.0003)	(0.0007)
Dummy Comm	-0.0001*	-0.0001	-0.0002	0.0000
	(0.0001)	(0.0003)	(0.0004)	(0.0006)
Dummy Energy	-0.0001*	0.0000	-0.0003	-0.0017***
	(0.0001)	(0.0004)	(0.0006)	(0.0006)
R-squared	0.1135	0.0875	0.0420	0.0565
R-squared Adj.	0.0616	0.0352	-0.0264	-0.0109
N	200	204	166	166

Standard errors in parentheses.
* p<.1, ** p<.05, ***p<.01

Index deletions

In table 2.2 we see a positively significant effect of momentum at announcement date, pointing to firms with recent strong performance being less punished by index removal. This observation could be due to investors perceiving these as strong despite the deletion. Furthermore, the coefficient for value is negatively significant at announcement date, indicating that growth firms face slightly more negative abnormal returns. We also observe a positively significant effect of the logarithm of market capitalization, which shows that larger firms experience more positive (or less negative) abnormal returns after deletion. Liquidity does not explain variation in our data in any of the three model specifications (measured by bid-ask spread), pointing to it not systematically explaining CAR after index deletion in our sample. We further observe the industry dummies having less significant effects in comparison to our sample for additions, but financial firms seem to experience more positive (or less negative) abnormal returns than technology firms after deletions through the first three model specifications. The other dummies are mostly insignificant, showing no clear difference in effects in comparison to technology firms. Similarly as for additions, the effects are observed close to the announcement date, but seem to fade away when increasing the distance from the announcement.

Table 2.2

Cross-sectional regression for stock return (Deletions)

This table presents the cumulative abnormal returns at announcement date (CAR_AD), announcement date to effective date (CAR_AD_ED), announcement date to 20 days after the effective date (CAR_AD_20) and announcement date to 60 days after the effective date (CAR_AD_60) regressed on firm characteristics for all deletions in the sample. The coefficients are presented together with standard errors in parentheses and symbols (stars) that represent the p-values. The symbols *, **, and *** denote significance at the 10 %, 5 %, and 1 % level respectively.

	CAR_AD	CAR_AD_ED	CAR_AD_20	CAR_AD_60
const	-0.0003	-0.0018***	-0.0028***	-0.0020
	(0.0002)	(0.0006)	(0.0009)	(0.0017)
momentum	0.0000**	0.0000	0.0000	-0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
px_to_book	-0.0000**	-0.0000	-0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
log_mkt_cap	0.0000	0.0001**	0.0001**	0.0001
	(0.0000)	(0.0000)	(0.0000)	(0.0001)
bid_ask_spread	-0.0000	-0.0000	0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Dummy Cons	0.0000	0.0005**	0.0002	0.0000
	(0.0001)	(0.0002)	(0.0003)	(0.0004)
Dummy Indust	0.0000	0.0002	0.0002	-0.0001
	(0.0001)	(0.0002)	(0.0003)	(0.0004)
Dummy Bas mat	0.0000	0.0003	0.0001	0.0001
	(0.0001)	(0.0003)	(0.0004)	(0.0008)
Dummy Finans	0.0001*	0.0005**	0.0006*	0.0005
	(0.0001)	(0.0002)	(0.0003)	(0.0004)
Dummy Util	-0.0000	0.0005	0.0011	0.0013
	(0.0001)	(0.0006)	(0.0008)	(0.0012)
Dummy Comm	-0.0001	0.0002	0.0003	0.0008
	(0.0001)	(0.0003)	(0.0004)	(0.0006)
Dummy Energy	0.0001	-0.0003	-0.0000	-0.0008
	(0.0001)	(0.0004)	(0.0009)	(0.0011)
R-squared	0.0646	0.0715	0.1085	0.0741
R-squared Adj.	0.0250	0.0337	0.0595	0.0232
N	272	282	212	212

Standard errors in parentheses.
* p<.1, ** p<.05, ***p<.01

VII. Discussion

A. Analysis of results

The results from our study shows that stock additions to or deletions from the MSCI Choice ESG Screened Indices does not produce significant abnormal returns around the announcement and effective dates. The t-tests reveal that the cumulative abnormal returns do not differ statistically from zero for all event windows and subsamples. These findings stand in contrast to earlier studies on traditional indices such as the S&P 500, in which stocks experienced strong positive abnormal returns following addition and negative abnormal returns following deletion (Harris and Gurel, 1986; Shleifer, 1986). This result implies that the index effect might work differently for indices with a focus on ESG compared to traditional market indices.

One possible explanation to the contrasting results may be that ESG indices capture a smaller portion of passive fund flows compared to traditional market indices (Morningstar, 2025; OECD, 2020). ESG indices make up only a small part of the investment universe and appeal mainly to investors with explicit sustainability preferences, meaning that the total assets under management tracking ESG indices is smaller (Morningstar, 2025; OECD, 2020). There is thus less buying and selling pressure when index revisions occur. Secondly, investors may already be predicting or pricing in ESG-related information, so there is a lower information surprise at the time of index revision (Bolton and Kacperczyk, 2021). Thirdly, the quarterly rebalancing frequency of the MSCI Choice indices might make these events more predictable and thereby reduce the price pressure effects (MSCI, 2024). Investors in ESG indices include institutional and long-term investors with sustained commitments rather than short-term arbitrageurs. Such an investor profile may dampen the mechanical demand shocks that drive index effect returns in traditional indices (Dyck et al., 2019).

Another consideration is related to the composition of the index and the fact that the number of index constituents is not fixed (MSCI, 2024). Because the index size is variable, an inclusion does not automatically require a deletion, and vice versa. Other studies on the index effect have mainly studied indices with a fixed number of constituents (Chen et al., 2004; Harris and Gurel, 1986; Greenwood and Sammon, 2022). In these cases, every addition must be matched with a deletion which means index-tracking funds must both buy the added stock and sell the deleted stock. When the number of constituents is not fixed, there is no forced substitution and trading flows are more dispersed and weaker, which could help explain the insignificant effect on abnormal return (Shleifer, 1986). This relates to the theory of imperfect substitutes (Shleifer, 1986), which states that stocks are not perfect substitutes for one another, leading to downward-sloping demand curves. For indices with a fixed number of constituents, forced substitutions create a stronger demand shock. When the number is variable and a stock does not need to be sold to make room for another, the demand shock is smaller, reducing the price impact according to the imperfect substitutes theory (Shleifer, 1986).

The lack of significant abnormal returns show a similar pattern to the results of Greenwood and Sammon (2024). In their article, Greenwood and Sammon (2024) demonstrate that the traditional index effect has largely disappeared in recent years, which they attribute to smaller net demand shocks from index funds due to “index migration”. This means that when a stock is moving from one index to another, some trackers will buy it while others have to sell it, which creates offsetting demand shocks. They further explain this by increased market efficiency and greater predictability of index changes. These explanations likely extend to ESG indices, which have smaller passive inflows, priced-in information and transparent rebalancing events, as explained above. Consequently, even if the trading volumes increase after index revisions, the impact on stock returns is minimal. Particularly, Greenwood and Sammon (2024) emphasize that the market has become more efficient in the way that liquidity traders are coordinating their trades to the same time (at the rebalancing date) and the liquidity providers know when this date is, which minimizes transaction costs. They find evidence that the proportion of trades at rebalancing dates has increased dramatically between the 1990s and 2010s, pointing to this mechanism existing. This may be highly relevant in the case of ESG indices as well, since our study finds evidence that the trading volume is concentrated at the rebalancing date. This evidence indicates a high level of market efficiency, which can help explain the insignificant effect on abnormal returns.

Another possible explanation to the absence of significant effects on abnormal returns is that the stocks have already been added to a broader, underlying index before (MSCI EAFE/USA/Emerging Markets Index) which means the effect has already occurred. Previous research has investigated the effect of being added to broader indices (such as the S&P 500) and found significant effects for this (Chen et al., 2004), while being added to screened (more flexible) indices has not gained as much attention in the existing research. It may be that while additions to broader indices generate abnormal returns, additions to screened indices show no significant results because the effect has already been absorbed when the stock was included in the underlying index.

From the results for trading volume, however, we observe a clear pattern. The observed spikes in abnormal trading volume around the effective date, followed by a rapid return to normal levels, indicate that market activity temporarily increases when the index changes are implemented. These findings are consistent with the liquidity hypothesis, which claims that index revisions trigger portfolio rebalancing among investors and funds tracking these indices (Harris and Gurel, 1986). We further see that the trading volume differs significantly from the one year average on more days than just at and directly after the announcement date and effective date. Particularly interesting, there are significant trading volumes before the announcement date for both additions and deletions. To be able to distinguish what this comes from, one must consider the different elements that a t-test contains. A larger t-statistic can depend on both a larger mean and a smaller standard error. The standard error consists of both the square root of the variance and the number of observations, which is fixed (since it is the number of stocks, which stays the same over the full period). Thus, a larger t-statistic is either because of a larger mean or a smaller variance. By decomposing these two parts separately, we observe that the mean of the daily trading volume across all stocks is rather close to 1 before the announcement date, but the variance is small for the days where we find significant results. This is further shown in the plots (graph 3.1 and 3.2), since these show average trading volume ratio daily for additions and deletions. This points to the fact that the significant results before that is due to a small variance. The small variance for these particular days mean that all stocks are generally traded relatively equally in comparison to their own previous average during these days. It can be observed that the stocks cluster around the trading volume ratio of 1 for these days, pointing to nothing special happening which is affecting the trading volume, meaning it is similar to the one year average. This leads to large t-statistics, even though the average trading volume ratio is not abnormally high (Stock and Watson, 2019).

The cross-sectional regressions offer additional insights. We find that larger firms and those with a narrower bid-ask spread exhibit slightly higher abnormal returns after inclusion, implying that index inclusion has a stronger impact on these types of firms. This finding is consistent with what has been observed in the literature. Kappou, Brooks and Ward (2008) find that larger firms have a more positive abnormal return after index inclusion than smaller firms, in line with our findings. Greenwood and Sammon (2024) also finds that liquidity is a key moderating factor in modern index effects, which further points to more liquid stocks being more heavily affected by inclusion. We also find that tech stocks are more affected than other types of stocks, due to the significantly negative coefficients for the other dummy variables. This finding aligns with recent literature, such as Tavor (2014), which shows a stronger effect of index inclusion for tech firms. This might be due to tech stocks typically relying more on growth expectations than on current cash flows, which could mean that the signaling effect of index inclusion is more valuable for these stocks (Tavor, 2014). For stock deletions, high recent performance (high momentum) is correlated with lower CAR, indicating that high-momentum stocks are more negatively affected by index deletions. This may be due to investors viewing index deletions as a signal that the strong performance is about to end, creating a larger negative impact for these stocks due to a stronger signalling effect (Shleifer, 1986). To some extent, we also see that market capitalization has an impact. Larger firms are less negatively impacted by index deletion than small firms, in line with our findings for additions. Financial firms also seem to be less negatively impacted by index deletion than other sectors, possibly due to them often being large, stable and closely followed by analysts and institutional investors, which increases efficiency and reduces the impact of index deletion (Merton, 1987). However, the explanatory power of these regressions is relatively limited, which again indicates that the impact of inclusion or exclusion in ESG indices on stock performance is, at most, slight and short-lived.

Taken together, the results demonstrate that there are important differences between ESG and traditional index effects. While index revisions in major indices like the S&P 500 typically result in price adjustments, ESG index revisions seem to affect trading activity without impacting stock returns. These findings contribute to the understanding of ESG investing by showing that indices with a sustainability focus behave and influence the market differently compared to traditional market indices.

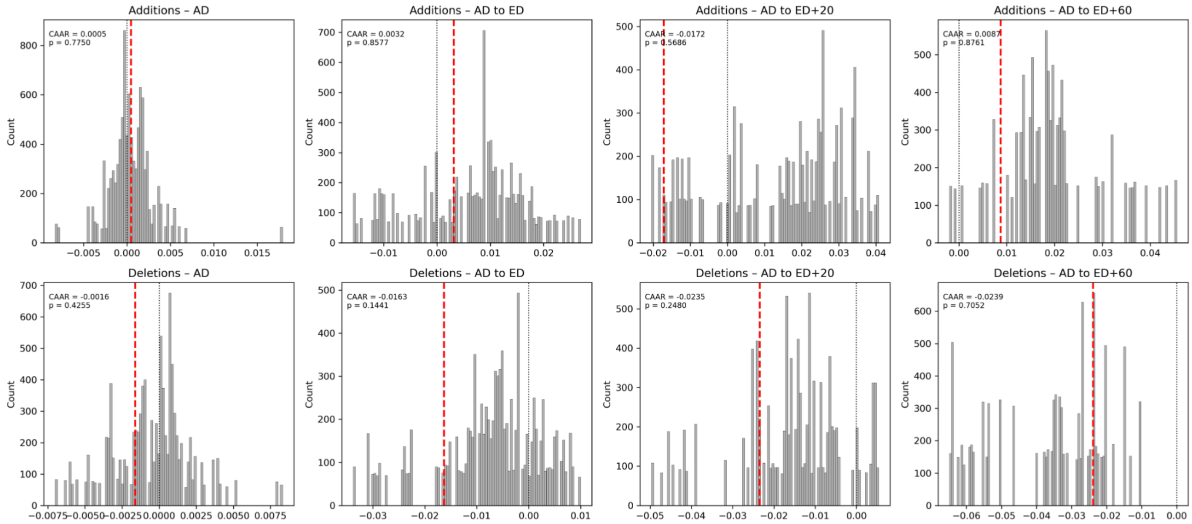
B. Robustness check

To test for the robustness of our results, we have performed permutation tests for cumulative abnormal returns and abnormal trading volume. The test for abnormal returns is performed to help verify that the observed cumulative abnormal returns do not significantly differ from noise. The graphs below show the results.

Graph 4.1

Permutation test results for abnormal returns

These graphs show the permutation tests for additions and deletions separately, and for all the different event windows separately. The four event windows are ED, AD to ED, AD to ED+20 and AD to ED+60. “AD” stands for “Announcement Date” and “ED” stands for “Effective Date”, referring to the date on which the change in index constituents is implemented. The y-axis is the frequency, indicating how often each value of the test statistic occurs across all permutations, while the x-axis is the test statistic. The red line shows the observed cumulative average abnormal return from our analysis and the grey line shows CAAR=0 (null hypothesis).



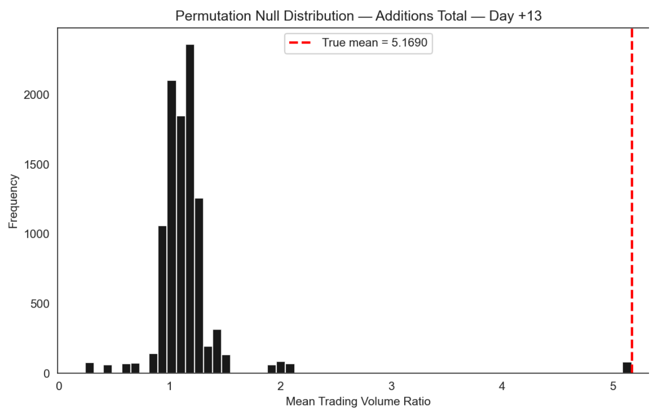
The permutation test finds if there is an effect on abnormal return by random chance, without assuming anything about the return distribution. This is done by randomly reshuffling the data 10,000 times, which gives a distribution of CAARs (cumulative average abnormal returns) in a scenario where index inclusion or deletion has no effect. It is then calculated how extreme the observed CAAR from our data is compared to this distribution, which is presented by how far out in the tails of the distribution the red line is. All tests show insignificant results, which suggests that it is not possible to reject the hypothesis that our results differ from noise. Since the red dashed line is well within the null distribution, the observed CAARs are not extreme compared to what could arise randomly if the effective dates had no real impact. In addition, the p-values all exceed conventional significance thresholds, indicating that the magnitude of observed CAAR is consistent with chance. The permutation test is fully aligned with the results of our t-tests, which also fail to detect statistically significant CAARs after ESG index revisions.

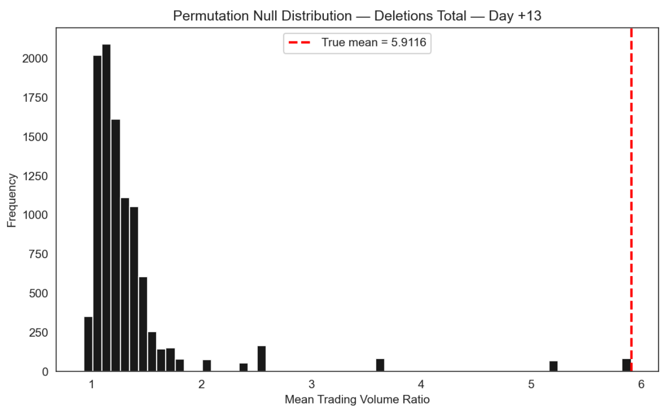
Furthermore, we have performed a permutation test for trading volume at the effective date (ED) to test the robustness of this analysis. The results are shown in the graphs below.

Graph 4.2 and 4.3

Permutation test results for trading volume

These graphs show the permutation test for trading volume. The graph to the left shows the test for additions and the right graph shows the test for deletions. The y-axis is the frequency, indicating how often each value of the test statistic occurs across all permutations, while the x-axis is the test statistic. The red line illustrates the observed mean trading volume ratio for day 13.





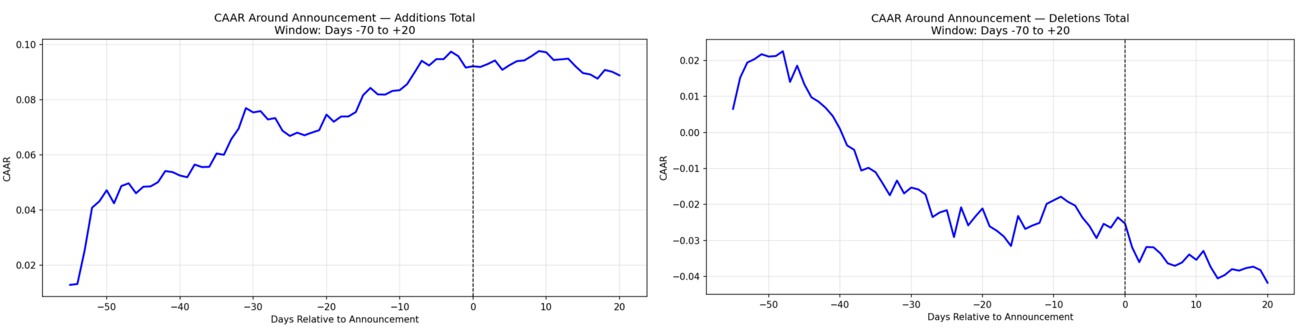
This permutation test finds if the trading volume ratio at the effective date is unusually large compared to if the event would have occurred any other day. The test randomly assigns a fake effective date and computes mean trading volume for this date. This is repeated 10,000 times to create a distribution of what effective date means if the event has no effect at all. The true mean at day 13 is then compared to this distribution, and we see that the true mean abnormal trading volume lies far to the right of the permutation null distribution. This indicates that the trading activity following ESG index addition and deletion is extremely unlikely to be due to random variation. Thus, there seems to be an effect of the event for trading volume, even when relaxing the assumptions regarding distributions. We have chosen to compare day 13 to the randomized distribution because it can be observed from our previous analysis (t-tests and plots) that there seems to be a big effect around day 13, and it is a valid proxy for the effective date. Therefore, it is of particular interest to see if this effect differs from noise.

We have further tested the assumption of no anticipation, following the method presented by Greenwood and Sammon (2024). We have tested the assumption by visualizing cumulative average abnormal returns from -70 days to +20 days for additions and deletions separately. If there is no anticipation of index inclusion/exclusion from the market, CAAR should remain relatively stable before the announcement, and then suddenly increase at announcement. Graph 4.4 shows that this is not the case in our sample. For additions, CAAR is increasing almost constantly before announcement, showing positive abnormal returns generally for the stocks before announcement. The opposite relationship is observed in graph 4.5 for deletions. This result could be driven by an anticipation effect as information about index changes leak before the official announcement (Beneish and Whaley, 1996; Greenwood and Sammon, 2024). If market participants infer this information in advance, stock prices will begin to adjust prior to the announcement date. The gradual price shift before the official announcement challenges the semi-strong form of the efficient market hypothesis, that all publicly available information should be priced in the stock price (Fama, 1970). Under the EMH, we would expect all relevant information to be quickly reflected in the stock price instead of gradually (Fama, 1970). However, the method does not control for other events affecting stock returns simultaneously, which means it is not possible to rule out that the perceived anticipation effect is observed due to other events than anticipation of index inclusion/exlusion.

Graph 4.4 and 4.5

CAAR before announcement (-70 to +20)

The graphs below show cumulative average abnormal returns (CAARs) around the announcement date (-70 days to +20 days). The graph to the left shows the CAAR for stocks additions while the right graph shows the stock deletions. The y-axis is the CAAR and the x-axis is the number of days from the announcement. The vertical line illustrates the announcement date (Day 0).



One dimension of robustness that would have been interesting to further explore is if the results are robust to different time periods. Due to data limitations, our sample contains data from 2023, which means the results may depend on the market conditions that have been seen during this period. Furthermore, it has been argued in recent literature that the index effect has been decreasing during the most recent time (Greenwood and Sammon, 2024). Thus, this may be an explanation to the insignificant effect on abnormal returns that we find. If we would have a larger dataset that spans over a longer time period, we would be able to split this dataset into one part that is more recent and one part that is older. In that way we would be able to see if the effect is robust to different market conditions, and also if the decreasing index effect can be an explanation to the results. However, this is not possible to do since ESG indices are a new phenomenon (OECD, 2020). The absence of such a dataset affects the interpretation of the external validity of our results, since it means we cannot rule out that the index effect exists only in this specific period.

C. Limitations of our study

A limitation concerns our choice of method for the expected return calculation. In fact, this is an issue in most event studies. Fama (1970) refers to this issue as the "bad model problem". According to Fama, this implies that even if markets are efficient, a poorly constructed model can lead researchers to conclude that anomalies exist when they actually reflect model misspecification rather than market inefficiency. By using the CAPM to estimate expected returns, we ignore other systematic factors and only consider market risk to be the determinant of expected performance. Other potential factors such as size, value, momentum are assumed to be irrelevant (Fama and French, 2015). This simplification might bias our estimation of abnormal returns.

In addition, the CAPM assumes a linear relationship between risk and return, which might overlook some of the dynamics in investor behavior and not fully capture investor preferences for ESG investing (Fama, 1970). We also assume beta for the stocks to be stable over time, indicating that a firm's systematic risks remain unchanged. In reality, betas can fluctuate as a result of changed market conditions, firm-specific events or investor sentiment. This assumption might also distort expected return estimates and, consequently, the abnormal returns.

Taken together, the CAPM limitations mentioned above highlight that part of the observed abnormal returns could stem from model misspecification rather than genuine market inefficiencies, as Fama (1970) suggests in the bad model problem. We therefore suggest that further studies use a multifactor model to calculate expected returns (such as the Fama French 3 factor model or 5 factor model), which might lead to more accurate expected returns and thus, abnormal returns (Fama and French, 1993; Fama and French 2015). By including additional risk factors such as size, value, profitability and investment, a multifactor model could provide more accurate estimations and reduce the likelihood of biased estimations (Fama and French, 2015). Additionally, further research could test the robustness of our result by using a different specification of CAPM, which includes alpha instead of the risk free rate.

Another limitation relates to the scope of the study and the limited amount of data. Further research could contribute to and expand the findings in this study by exploring the index effect in other ESG indices, such as indices in the FTSE4Good Index Series or the S&P Global 1200 ESG Exclusions Index. The scope of this thesis limits the analysis to a single family of indices and does not extend to other sustainability indices. The delimitation restricts the external validity of the findings and their applicability, as the results may not be representative of other indices that are a part of the ESG universe. To be able to draw more general conclusions regarding the term "ESG indices", this study should be repeated on other ESG indices. In addition, the data in our sample is collected between 2023 and 2025, which is a rather short period to be able to reliably capture long-term trends or draw robust conclusions about patterns over time. The study would be more robust if we had a larger dataset over a longer time frame, since it would both include more observations which increases the statistical power and reduce the risk of confounding events happening in our sample period.

One assumption we make is that there is no anticipation from the market about the inclusion or deletion of a certain stock. However, as seen in graph 4.4 and 4.5 this assumption likely does not hold. The observed anticipation effect could cause an understated observed effect for our event window and insignificant abnormal returns, even if an effect actually exists. For the MSCI Choice ESG Screened Indices, which follow a predictable rebalancing schedule (MSCI, 2024), some investors may monitor changes in firm ESG ratings or controversies to predict likely additions or deletions. These investors try to anticipate which additions or deletions will occur using publicly available ESG information. As a result, the market reaction may occur gradually, making it difficult to isolate the real announcement effect as part of the effect may occur before the official announcement date. We therefore identify a need to test for anticipation effects, potentially by expanding the event windows prior to the announcement date. By including more trading days before the announcement date, it captures early trading activity by investors anticipating changes in the index composition prior to the official announcement.

A further limitation with regards to the constant mean model methodology for abnormal trading volume is that it does not account for market-wide fluctuations in trading activity. The model assumes that the average trading volume is constant and equal to the historical average computed over the estimation window (Campbell et al., 1997). However, during the estimation period, there could have been macroeconomic events, monetary policy changes, shifts in market structure (e.g. the rise of high-frequency trading or ETFs) or broader market regimes (e.g. bull markets with higher overall liquidity). If the estimation period is characterized by relatively low (high) market-wide trading activity while index revisions occur during a period of high (low) volumes, the model systematically overestimates (underestimates) the abnormal volume (Campbell et al., 1997). To address this issue, further studies should consider using a market-adjusted volume ratio, in which the denominator is the benchmark trading volume standardized by market volume during the estimation period and the numerator is the event period turnover standardized by market volume during the event period (Chen et al. 2004).

VIII. Conclusion

This thesis examines the index effect in the context of ESG screened indices. We use the MSCI Choice ESG Screened Indices to evaluate whether abnormal stock returns are associated with inclusion to or exclusion from an ESG index. The study applies an event study methodology, comparing actual returns after the announcement and implementation dates with expected returns estimated using the CAPM model and comparing actual trading volume with a benchmark volume. The analysis tests for short-term and medium-term cumulative abnormal returns and trading volumes across multiple event windows to determine whether index inclusions and exclusions generate significant market reactions over different time horizons. The study further investigates the effect on trading volume, by examining how trading volume changes upon index inclusion or exclusion. We also consider how firm-specific characteristics such as market capitalization, liquidity and valuation ratios help explain the cross-sectional variation in abnormal returns.

We find no significant impact on returns for either stock additions nor deletions to an ESG index, which contrasts previous research on the traditional index effect for major indices such as the S&P500. The absence of significant abnormal returns suggests that ESG index revisions function differently from those of other indices. Several factors likely contribute to this result, such as smaller passive fund flows into ESG indices, greater predictability of rebalancing events and the structure of the MSCI Choice ESG screened indices, which avoids forced substitutions. Moreover, the anticipation effect causes the market to react gradually, making it difficult to isolate the real announcement effect.

The results also show a sharp increase in trading volume on the rebalancing dates, indicating that ESG index changes influence market behavior primarily through temporary liquidity effects rather than lasting price impacts. The cross-sectional regression shows that firm characteristics such as size, liquidity, and sector can influence these reactions. Robustness checks confirm that the increase in trading volume is extremely unlikely to be due to random variation, while the tests for abnormal returns show that it is not possible to reject the hypothesis that our results differ from noise, aligning with our t-test results.

Several limitations have been acknowledged and points to a need for further studies on the index effect in the context of ESG indices. With that being said, this study highlights important differences between ESG and traditional index effects which are highly relevant for the academic debate. The existing literature on index effects has historically focused on large, traditional benchmarks and studies on ESG indices remains limited. By examining a growing family of sustainability-focused indices, this study broadens the empirical scope of index effect research and highlights structural distinctions between ESG and conventional index mechanics. In addition, the findings support recent evidence that index effects have weakened in modern markets (Greenwood and Sammon, 2024), extending this argument to the ESG context. With this study, we also contribute to a growing understanding of how sustainability-focused indices interact with market dynamics and underlines the need for further research on a broader range of ESG benchmarks and with longer time horizons.

IX. References

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AI Appendix

We have used AI tools (ChatGPT, Grok, Deepseek and Elicit AI) for help with coding and grammar. Equations 1 to 11 have been generated using AI tools.