SSE Riga Student Research Papers
2022 : 1 (243)

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ISSN 1691-4643
ISBN 978-9984-822-67-9

May 2022
Riga

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Abstract

On August 17, 2021, US SEC charged Matthew Panuwat with insider trading in an acquisition deal. Mr Panuwat, however, did not possess direct material non-public information of the company whose stock he bought – it was neither the acquirer nor the target of the merger, but a competitor company's stock that he traded. While this remains the only insider trading court case of such kind, the term "shadow trading" has been coined by Mehta, Reeb, and Zhaol (2020) to describe using inside information about one company to trade the stock of a different but economically linked company. In this thesis, we examine whether shadow trading also occurs in exchange-traded funds (ETFs), which we hypothesise could be attractive to insiders because they often are more liquid than the underlying companies. Using mergers and acquisitions of US companies from 2009 to 2021 and bootstrap methods that draw on randomised placebo samples, we find that around 3-6% of the suspected same-industry ETFs experience positive abnormal volumes ahead of official M&A announcements, consistent with shadow trading. We estimate the total shadow trading activity in ETFs at around $2.8 billion during our sample period.

Acknowledgement

We would like to express our sincere gratitude to our supervisor Tālis J. Putniņš for the invaluable contribution to the methodology of the thesis and the guidance throughout the whole writing period. We would also like to thank Konstantīns Beņkovskis for the helpful input on our thesis's empirical part.

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1. Introduction

Market integrity is crucial for the proper functioning of financial markets. The US Securities and Exchange Commission state that a part of their mission is “protecting investors, maintaining fair, orderly, and efficient markets,” (SEC, n.d.). Despite the consensus on the desirability of regulation that ensures fairness for all market participants, the definition of illegal security trading is ambiguous. The rules of insider trading are changing as the markets adjust to new financial innovations, and the old standards of illegal conduct no longer reflect all insider trading possibilities in the financial markets.

The conventional form of insider trading can be described as follows: an insider learns material non-public information about some company, and either trades that company’s stock, or leaks the information to an outsider who then trades on the information. Alternatively, the information can be revealed to a corporate outsider who provides professional services to the company. The corporate outsider who has become aware of insider information then trades the company’s stock and reaps personal benefits. In both cases, the insider information is used to trade the equities of the company directly affected by the information (SEC, n.d.).

A recent academic example that expands the insider trading definition is the “shadow trading” phenomenon proposed by Mehta, Reeb, and Zhaol (2020). It states that upon receiving material non-public information related to some company, insiders can exploit the information indirectly. Instead of trading the stock of the companies that are directly impacted by the news (the source firms of the information), insiders can purchase stock in economically linked companies based on their predictions of the impact the information will have on the peers and the partners. Mehta et al. (2020) present evidence consistent with shadow trading in partner and competitor firms before earnings announcements.

In our thesis, we further expand the theory of shadow trading by quantifying insider information-driven trading in exchange-traded funds (ETFs). We justify the possibility of shadow trading in ETFs by presenting academic research on these investment vehicles. First, they are among the most traded assets in the US secondary markets (Buckle, Chen, Guo, & Tong, 2018), thus should be easily available to insiders. Second, theoretical and empirical evidence shows that illegal traders prefer highly liquid assets to hide their trades among noise traders (Kyle, 1985; Lei & Wang, 2014; Patel & Putniņš, 2021) and ETFs are known to be more liquid than the underlying securities (Ben-David, Franzoni, & Moussawi, 2014; Marshall, Nguyen, & Visaltanachoti, 2018). Combining the highly liquid nature of ETFs with the diversity of portfolios these financial investments trace, we conclude that ETFs offer a cost-effective way of utilizing material non-public information.

Unlike Mehta et al. (2020), we propose focusing on M&A announcements. First, these corporate events have been shown to have the most material price impact, creating the strongest financial incentives for illegal insider trading (Huang & Walking, 1987; Masulis & Simsir, 2018; Lee, 2020; Patel & Putniņš, 2021). Second, they are mostly unanticipated events, unlike earnings announcements. Due to their scheduled nature, earnings announcements can create abnormal activity in ETFs that is not attributable solely to insiders, introducing significant noise to the results. We also limit our scope to peer companies only, i.e., to sector ETFs, as we are not able choose partners individually for each acquired firm because ETFs are mostly positioned as diversification vehicles and invest in large baskets of companies.

The diversification function of ETFs also needs to be considered from shadow trader's perspective. As ETFs invest fractions of assets in their portfolio companies, the effect of larger returns in any of the constituents is muted by construction. Further, Mehta et al. (2020) in one of their robustness checks find no evidence of shadow trading in peer companies prior the M&A using price-driven metrics, suggesting that mergers have heterogenous effects on industry structure, i.e., they are not necessarily improving or worsening competitors' prospects. This finding from stock shadow trading sets an expectation that sector ETFs are not very actively used by insiders as buying or selling such ETFs is effectively like trading peer stock portfolios.

It is important to underline, however, that Mehta et al. (2020) do not explicitly consider ETFs as shadow trading instruments. Moreover, assuming that some deals are positive news for the industry and some are negative, we can expect zero price run-up on average, but not necessarily zero shadow trading. If insiders have strong signals about how the peer stocks would react, they can successfully trade in different directions in different deals, which, if shadow trading is sizeable, is going to be visible as an increased trading volume in industry ETFs prior the M&As.

We formulate our research question as follows: ***How widespread is shadow trading in ETFs before related firms' M&A announcements?***

For each M&A announcement in the sample period 2009-2021 we obtain information on sector ETFs that track the industry of the M&A target firm. Then, by calculating correlations between the ETF and the target firm, we arrive at a data-driven estimate of the ETFs that are most closely related to the target firm. We further narrow the sample of related ETFs by imposing filters suggested by previous research. We expect that insiders would be more likely to trade the closely related ETFs, as it would allow to utilize their information in the most fungible way. Then, we adapt shadow trading proxies from insider trading literature and estimate shadow trading in previously detected related ETFs. We run regressions and perform bootstrapping analysis to estimate the extent of shadow trading in related ETFs prior source firm's news announcements.

We find that shadow trading happens in around 3-6% of the suspected ETFs. We find stronger evidence with abnormal volumes than with price run-ups, consistent with a hypothesis that ETFs are very liquid assets and insiders' activity does have a pronounced price effect. Our estimate for shadow trading activity ranges between \$163m and \$15.37bn, with a central estimate of around \$2.75bn. We also show that shadow trading happens disproportionately in the health care and technology industries, as well as shows peaks in 2015 and 2016.

Our thesis contributes to the literature on insider, and more specifically, shadow trading. We study insider-information driven trading in ETFs: a financial instrument which, to the best of our knowledge, has not been considered in the context of insider trading. We also add to the growing literature on ETFs, their impact on financial markets, and the ways informed traders use ETFs. Finally, our thesis has policy implications. If insiders shadow trade ETFs to reap benefits from insider information, the trading policies of corporate insiders should be revisited.

The rest of the paper is structured as follows. Section 2 presents an overview of available academic literature related to our topic. Section 3 describes the data used in the empirical analysis, and Section 4 explains the methodology of the research. Section 5 describes and discusses the results of our research. Section 6 concludes.

2. Literature review

In this section, we motivate our research question by reviewing previous academic studies on insider trading. We highlight the ambiguities in insider trading regulation and introduce the concept of shadow trading. Then, we describe the characteristics determining the presence of insider and shadow traders. Finally, we present evidence suggesting that shadow trading could also happen in ETFs related to the companies releasing news announcements.

2.1. Insider trading

2.1.1. The definition of insider trading

In the US, the Securities Exchange Act of 1934 defines a corporate insider as any director, corporate officer, or beneficial owner who holds more than 10% of a company's equity securities (SEC, 2021). Insider trading refers to corporate insiders' purchases and sales of their company's equity or derivative instruments (SEC, n.d.). Two forms of insider trading can be recognized. The legal form of insider trading includes trades motivated by either publicly available information, or routine trades (caused by e.g., portfolio rebalancing or liquidity reasons). The second form is illegal insider trading: it relates to trades that are based on insiders' knowledge of material nonpublic information about their company's prospects (hereinafter in text referred to as "insider trading"). Insider trading is generally seen as detrimental to markets, as it decreases the fairness of market outcomes, and can increase adverse selection costs, negatively affecting liquidity. In the US, insider trading is not extensively covered in legislation and is usually prosecuted based on anti-fraud legislation and insider trading court case precedents (Eisenberg, 2017). Some attempts have been made to change the legislation – most recently, on May 18, 2021, the US House of Representatives passed a bill that would explicitly address and prohibit insider trading, called the Insider Trading Prohibition Act. According to the bill's sponsor Jim Himes, "the legislation does not expand insider trading law but simplifies and codifies the law as articulated by courts through decades of opinions" (Godoy, 2021). The bill has not yet been passed by the Senate, thus the rules related to insider trading are still dictated by court precedents.

An important aspect to note is that the scope of conduct that is recognized as insider trading has been expanded over the years. A person can be liable for insider trading under one of the two insider trading legal theories. First, the classical theory concerns the corporate insider defined in the previous paragraph (more than 10% equity-holder), who becomes aware of material non-public information and breaks their fiduciary duty to shareholders by trading their own company's stock for personal gains (Gubler, 2016). On the other hand, the misappropriation theory of insider trading extends the scope of liability. It applies to any person who becomes aware of material non-public information concerning a company and trades that company's stock (*United States v. O'Hagan*, 1997). Generally, corporate outsiders can acquire insider information through (a) providing services to the company (for example, employees of law or brokerage firms), or (b) being 'tipped off' by the insiders (for example, family members, friends, business partners, employees) (SEC, n.d.). The misappropriation theory of insider trading helps protect market integrity against corporate outsiders who cannot be prosecuted based on a breach of fiduciary duty to the company whose equity securities the outsider illegally trades (*United States v. O'Hagan*, 1997).

2.1.2. The determinants of insider trading

Although limiting insider trading and protecting market integrity is generally seen as a desirable goal among regulators, insider trading is a difficult area to research due to its nature – only the detected part of all illegal trading activity can be studied precisely, and estimating the full extent of insider trading in the markets is not a straightforward task. The US SEC claims that trading on material nonpublic information is common practice in markets, but it is difficult to prove due to convictions largely relying on circumstantial evidence (Cline & Posylnaya, 2019). Patel and Putniņš (2021) estimate that the actual number of insider trading cases in the US is at least four times higher than the number of cases prosecuted by the SEC. With the prevalence of illegal trading, it is of importance to characterize the decisions driving insider trading.

First, we should expect that high regulatory attention decreases incentives to trade on insider information. According to Becker (1974), market participants' decision to engage in illegal activity is driven by the perceived profits or losses of such action; it is a purely economic decision. Consistent with this view, Cline and Posylnaya (2019) apply probabilistic analysis to model the dynamics of detected and undetected insider trading in the US markets and find that increased insider trading litigation is associated with lower illegal trading in the next year. The shift away from illegal trading depending on the level of regulation is rational: Patel and Putniņš (2021) find that the probability of illegal insider detection and prosecution increases along with regulatory resources allocated to enforcement of insider trading rules.

Further, according to Kyle's (1985) batch auction model, informed traders are more likely to trade in more liquid securities to hide their trades among noise traders and earn greater profits. The prediction is applied to the insider trading setting and empirically supported by Lei and Wang (2014) and Patel and Putniņš (2021), indicating that Kyle's model holds, and insiders (among other informed traders) are more likely to trade in liquid securities to exploit their material non-public information.

The third important motivator to trade on private information is the value of the information the insider possesses. Referring to Becker (1974), a higher value of the information is expected to increase the payoff of insider trading. Consistent with theory, Kacperczyk and Pagnotta (2018) find that illegal insiders consider information value in trades and *ceteris paribus* are less likely to trade if the value of information is lower.

2.2.Shadow trading

2.2.1. The definition of shadow trading

The definition of illegal insider trading is still fuzzy despite the attempts of courts to develop common rules, and the theories of insider trading can be interpreted in ever-changing ways. One of the cases which illustrates the ambiguity of the law began on August 17, 2021, when the US SEC charged Matthew Panuwat, an employee of a pharmaceutical company who, according to the SEC, broke the law by trading stock prior an M&A announcement while in possession of insider information (SEC, 2021). The part that is unique in this case is the interpretation of the misappropriation theory (Anagnosti et al., 2021). M. Panuwat did not possess direct material non-public information on the company whose stock he bought; it was neither the acquirer nor the target of the M&A, but an economically linked company to the target. M. Panuwat moved to dismiss the case before trial based on a lack of precedents indicating that his trade was against the law, but the motion was dismissed by the judge who found SEC's charges justified (Levine, 2022). The case is still yet to be settled in court, but its existence proves the changing nature of insider trading regulation in the US.

In popular discourse, the type of trading described in the previous paragraph has been titled "shadow trading". The term was coined by Mehta, Reeb and Zhao in their 2020 research article titled *Shadow trading*, where they aim to prove that insiders exploit material non-public information to trade in economically linked companies' stock. To the best of our knowledge, it is the only research on illegal insider trading in related firms' stock so far. In the article, Mehta et al. (2020) construct firm pairs of source firms and linked firms, where a source firm is a firm releasing news announcements, and linked firms are the source firm's competitors or business partners. They develop the following price-based shadow trading proxies: abnormal short sales, option/stock ratio, and order imbalance. They then apply the proxies to study the relationship between shadow trading in related firms and source firm's abnormal returns around the news announcement date. They find statistically significant changes in their shadow trading proxies associated with abnormal returns of source firm's stock, providing evidence that shadow trading is an undocumented phenomenon happening in financial markets.

2.2.2. The determinants of shadow trading

The findings of Mehta et al. (2020) are consistent with most of the conclusions from the literature on determinants of insider trading. First, we should note that shadow trading is associated with lower regulatory costs than conventional insider trading, since it was not explicitly highlighted as illegal by SEC until the charges against M. Panuwat in August 2021. Thus, based on Becker's (1974) theory of all criminal activity being an economic choice, shadow trading should be more widespread when conventional insider trading becomes potentially more expensive. The effect of increased regulatory attention is tested by Mehta et al. (2020). They document that increased regulation on conventional insider trading in the state where the firm operates increases the likelihood of insiders engaging in shadow trading. In other words, the higher the regulatory cost, the higher likelihood to substitute insider trading with shadow trading.

The second aspect consistent with previously mentioned determinants is the value of information. We should expect that shadow trading is stronger when insiders have more fungible information on the economically linked companies' prospects. Mehta et al. (2020) compare the increase in shadow trading among different information events and find significant differences. Before earnings announcements, insiders are equally likely to trade both business partners' and competitors' stock, as they have greater clarity about the effect on the stock price – their competitors and business partners are either expected to lose or gain from the source firm's earnings announcement. However, before M&A announcements, the increase in shadow trading is smaller in competitors' stock due to possible ambiguous effects of M&As on the market structure. Similarly, before new product announcements, only an increase in business partner shadow trading is observable, again indicating that the effects on competitors are not clear for insiders. Throughout all tests, the authors use price-based proxies, therefore, their results do not capture shadow trading volume. Rather, they find either the presence or absence of a price impact attributed to shadow traders. Nevertheless, their findings show that, as suggested by previous research, the value of the insider information can influence the decision to shadow trade.

Overall, the findings by Mehta et al. (2020) suggest that insider trading can happen in more subtle ways than is conventionally assumed. Insiders can switch to instruments other than the stock of the company about which they possess insider information. We propose quantifying the shadow trading happening in other securities linked to the source firms of insider information. The security we focus on is Exchange-traded funds (ETFs). The next section summarizes the evidence pointing towards a research gap on this topic.

2.2.3. Exchange-traded funds as an instrument for shadow trading

Exchange-traded funds (ETFs) are open-ended investment funds, the shares of which are traded on secondary market exchanges (Lettau & Madhavan, 2018). They are gaining popularity among investors due to the low trading costs, diversification benefits, and increasing variability of offered investment options. With ETFs, investors can track market indexes, specific sectors, asset classes and follow various investment strategies (Lettau & Madhavan, 2018). Buckle et al. (2018) note that the ETF trading volume has been rapidly increasing ever since the financial crisis of 2008, and nowadays constitutes nearly 50% of stock trading in the US. In December 2021, the average daily volume of ETF shares traded was 1.83 billion across 2,793 ETFs listed in the US, and average daily transaction value was around $160 billion (NYSE, n.d.). Deutsche Bank (2017) notes that interest for mutual, hedge and index funds has been decreasing in the last decade, replaced by interest in ETFs, which is largely driven by poor active fund performance and the convenience offered by ETF solutions.

Naturally, the rapid growth of ETFs and their trading patterns have attracted the attention of researchers. As noted by Subrahmanyam (1991), ETFs (or any basket of securities) can be expected to be more liquid than the underlying securities due to lower adverse selection costs. Chelley-Steeley and Park (2010) test the hypothesis, and by decomposing the spreads observed in markets, find evidence in favor of lower adverse selection costs in ETFs, indicating higher liquidity in ETFs than in the underlying assets. The literature also suggests that ETFs attract the attention of high-frequency traders who seek liquidity that is superior to the liquidity offered by the underlying assets (Ben-David et al., 2014; see also Marshall et al., 2018). Recently, Khomyn, Putniņš, and Zoican (2021) find that the perceived value of ETF liquidity leads to clientele segmentation, with more liquid ETFs *ceteris paribus* setting higher fees. Overall, the literature suggests that ETFs are liquid, and market participants value the liquidity according to their investment needs.

A long-standing debate concerns the amount of informed trading in ETFs. On the one hand, ETFs by construction track indexes and thus can be viewed as purely passive investment vehicles. On the other hand, the growing diversity of tracked indexes and the liquidity offered by ETFs should offer cost-effective means of trading on information for informed traders. In literature, there is contrasting evidence on the informational efficiency of ETF-tracked indexes. Glosten, Nallareddy, and Zou (2021) state that the ability to trade a basket of securities at a low price enhances price discovery in the underlying securities and leads to quicker information incorporation for a broad cross-section of stocks. A similar sentiment is expressed by Xu, Yin, and Zhao (2019), who emphasize that for prices to converge to the true value, there should be enough informed traders in the ETF who have contrasting views on the fundamental value of the index (two-sided ETF trading). Glosten et al. (2021) document increased price efficiency of the ETF-tracked index, and further find that the increased efficiency is attributable to the incorporation of systemic fundamental information, not firm-specific fundamental information. Bhattacharya & O'Hara (2018) add that the introduction of ETFs is likely to increase informational efficiency in easily accessible securities, while the ETF information spillovers in low-accessibility markets (bond, commodity markets) might lead to increased efficiency on the macro level and distortion at the micro price level. Thus, we should expect that in stock markets the trading of ETFs is associated with price efficiency across individual securities, indicating that informed traders participate in ETF trading.

Huang, O'Hara, and Zhong (2021) look at the issue of informed trader presence in ETF markets from another point of view. They study whether informed traders include ETFs in hedging strategies. More specifically, they focus on industry ETFs and provide evidence that informed investors implement *long-the-stock/short-the-ETF* strategies to hedge industry risk. They find that the long-short activity surges before positive earnings announcements, suggesting that informed traders can exploit the strategy to trade on their information on specific firms, contrasting the view expressed by Glosten et al. (2021) on the incorporation of solely systematic information.

To further test the presence of a firm-specific information component in ETF trading, we propose quantifying insider trading in ETFs before firm news (M&A) announcements. Literature suggests that ETFs are traded by informed investors, and we propose that the liquidity of ETFs and the diversity of tracked portfolios offered by these investment vehicles could be attractive not only to informed investors in general but also to individuals possessing material non-public information on specific firms. Our focus on M&As is explained in detail in the Methodology section.

Based on the reviewed academic research, we formulate the following hypotheses:

Hypothesis 1: Because ETFs are often more liquid than the individual stock that is the target of the takeover, we expect some shadow trading will occur in ETFs.

Hypothesis 2: Shadow trading is more likely to be observed in M&A deals where the target is a large index constituent, sizeable announcement returns are expected, and closely related ETFs are widely traded.

Hypothesis 3: Shadow trading is expected to be evident as abnormal levels of trading volume in the ETF before the M&A announcement.

Hypothesis 4: Given the higher level of liquidity of ETFs, price run-ups are expected to be small and not significant.

Hypothesis 5: Since ETF liquidity has increased over time, shadow trading is more likely to be observed in M&A deals in the later years of our sample.

3. Data

In this research, we use Refinitiv Eikon and Datastream as our data sources. From the SDC database (available in Eikon as Deal Screener), we obtain all M&As of US domiciled public companies since 2009 where at least 50% of the target's equity is acquired. The total number of such M&As is 3,396.

For the 3,396 M&As, we filter out those database records that have no Datastream code for a target, resulting in 3,311 deals. We use these codes to uniquely identify stocks and obtain price and volume data from Datastream. We obtain daily data from January 2007 to December 2021. We go back as far as 2007 as some of the variables we construct require more than a full calendar year of trading data prior the M&A announcement day. We manage to obtain non-zero amount of Datastream data for 3,209 targets. To deal with data errors and outliers, we apply return filters proposed for this data source by Kumar, Nguyen, and Putniņš (2021) based on Ince and Porter (2006). These filters result in 3,150 stocks in our sample. Return filters are described in Appendix 1.

Next, we obtain ICB industry codes for the targets.[1] The choice of ICB is determined by the data availability, as we could not obtain more widely used GICS industry records for our sample companies in Datastream.[2] ICB codes are available for 3,140 targets in the clean sample.

[1] We obtain static data from Datastream for industry codes, i.e., the latest available data as of February 2, 2022. We assume these data correctly reflects the industry information at M&A announcement, i.e., that the classification does not change after the deal.

[2] It is worth mentioning that 11 ICB industries unambiguously match 11 GICS sectors, so we see these two classifications as perfect substitutes at the highest hierarchy level, which is the only level we consider.

As we are particularly concerned with quality data available around M&A announcement, we find two (from pre-announcement to post-announcement) day return for 2,711 targets, which corresponds to 2,734 deals. 23 stocks are targets of two M&A deals. The summary statistics on various deal characteristics are presented in Appendix 2.

We then use Eikon Search Tool (Screener) to screen relevant ETFs. In the Screener, it is possible to use Lipper Classification to obtain a list of sector ETFs, i.e., ETFs that track particular industries in the economy. We find that Lipper industries closely match ICB 11 industries; this provides an opportunity to link stocks and ETFs as we already have ICB industries for M&A targets from Datastream. We select all ETFs that are classified as sector ETFs by Lipper, that are US domiciled and are primary issues. This returns a list of 1,411 ETFs and their RIC codes. The list includes both listed and delisted ETFs, eliminating survivorship bias in our data.

We apply Kumar, Nguyen, and Putniņš (2021) filters to ETFs as well, which results in 1,207 ETFs. We then use RICs to obtain ETF price and volume data from Datastream from January 2007 to December 2021, the same period we use for stocks. To calculate cumulative abnormal returns in targets and related ETFs, we use Russell 3000 index from January 2007 to December 2021. We use Datastream return index (RI) for all price observations. Compared to the raw price, it is adjusted for dividends and capital changes, e.g., share issues/buybacks and splits/reverse splits.

Finally, we obtain target stock price-to-book ratio and market capitalization at monthly frequency to use as controls. We also obtain market capitalization as a proxy for the net asset value of ETFs at the end of each month.

4. Methodology

4.1. Choice of news announcements

To study shadow trading, we utilize events where information previously held only by corporate insiders is released to the financial markets. More specifically, we choose M&A announcements as the unexpected news events. Literature highlights the two main advantages of this approach.

First, M&As serve as truly unexpected and unscheduled news events. As they are unexpected, information-rich, and fundamental, they are more likely to be material in terms of market returns compared to earnings announcements, which are released on pre-determined dates and are accompanied by speculation and increased noise trading. M&As offer the most lucrative opportunities for large profit driven illegal insider trading, thus any abnormal activity around these events is more likely attributable to insider trading than market speculation. Among prosecuted insider trading cases in the US between 1996 and 2016, 50% relate to M&As, and only 20% to earnings announcements (Patel & Putniņš, 2021). Augustin, Brenner, and Subrahmanyam (2019) estimate that more than 25% of the M&As in their sample period are connected to abnormal options trading volumes. Similarly, Patel & Putniņš (2021) find evidence indicating that insider trading can be observed in 20% of M&As (for comparison, insiders trade before 5% of earnings announcements).

The second advantage of M&As relies on overwhelming evidence of significant target-firm abnormal returns around announcement days. Huang and Walking (1987) report an average target cumulative abnormal return (CAR) of 23.4% for the two-day announcement period ($CAR\ (-1,+1)$ = 23.4%) for 1977–1982 deals, Masulis and Simsir (2018) calculate a close $CAR(-2,+2)$ of 26.4% for a 1997–2012 M&A sample, and Lee (2020) reports a lower but nevertheless sizable 15.8% $CAR(-1,+1)$ using a 1985–2014 sample. Similarly, we observe an average target $CAR(-1,+1)$ of 20% in our sample period 2008-2021 (see Appendix 2). This evidence supports the argument that M&As offer profitable trading opportunities for insiders.

Despite the M&A target returns being large on average, we should remember that the focus of our study is shadow trading rather than conventional insider trading. The only published paper which studies shadow trading mainly focuses on earnings announcements rather than M&As. Mehta et al. (2020) do not find evidence of shadow trading in competitor stocks prior M&A announcements. They put forward the theory that M&As can have ambiguous effects on the competitors and industry structure in general, thus insiders see no clear way of profiting from their information. We propose that the lack of evidence in their findings could be explained by the proxies they construct, which are all price-based – they test abnormal short sales, stock/option ratio and order imbalance. We suggest a volume-based proxy as our main test, complimented by two price-based proxies, since the lack of price impact is not equivalent to the absence of shadow trading altogether. A volume-based measure is of even higher importance in the context of ETFs, where the high liquidity and large daily trading volume might obstruct any price impact of shadow trading.

4.2.Shadow trading proxies

To detect shadow trading in related ETFs, we use proxies based on insider trading literature. We draw on a study by Foley et al. (2021) where the authors test the validity of various insider trading metrics against a sample of prosecuted insider trading cases. They find that M&A target stock abnormal trading volume, abnormal returns and order imbalance significantly predict insider trading, corresponding to the consensus in academia on the validity of these measures. We therefore apply these measures to ETFs that are related to M&A targets to study shadow trading.

In the interpretation and analysis of the results, we focus most on abnormal volume. First, it is the proxy where we are most likely to see abnormality, given the high liquidity of ETFs and share of insiders versus other market participants, and second, it is the only measure which is directly adapted from Foley et al. (2021). The price run-up measure is modified according to the nature of ETF returns, and we lack high frequency trading data necessary for constructing an order imbalance measure which would catch short-term price movements. Nevertheless, we include the two price-based measures in our tests, as they might provide evidence for shadow trading.

4.2.1. Abnormal volume

Our first shadow trading proxy measures abnormal volumes in the related ETFs prior to the M&A announcement. Meulbroek (1992) finds evidence that the presence of insider trading is associated with larger-than-expected volume. Fishe & Robe (2004) add that in addition to increased trader by insiders, the abnormal volume is also driven by mimicking and momentum traders who notice the presence of informed traders. Following Foley et al. (2021), we measure abnormal volume as follows:

$$Abnormal\_Volume_i = \frac{V_i(-5,-1) - V_i(-30,-11)}{V_i(-5,-1) + V_i(-30,-11)} \quad (1)$$

where $V_i(-5,-1)$ is the average daily volume traded in the linked ETF i from 5 days before the announcement to one day before the announcement, and $V_i(-30,-11)$ is the average daily volume traded from 30 to 11 days before the announcement. Abnormal volume is bound in the range [-1, +1] with larger values indicating higher likelihood of shadow trading.

In addition, to estimate shadow trading in monetary terms, we calculate:

$$Abnormal\_USD\_Volume_i = Abnormal\_Volume_i * Volume_i(-5,-1) * P_i(-30,-1) \quad (2)$$

where $Volume_i(-5,-1)$ is the total volume traded in the linked ETF i from 5 days before the announcement to one day before the announcement, and $P_i(-30,-1)$ is the average closing price of ETF i from 30 to 1 day before the announcement.

4.2.2. Abnormal returns

Our second shadow trading proxy measures abnormal returns of the related ETF prior to the M&A announcement. Research shows that the price run-up before major firm announcements is determined by the extent to which market participants trade on private information. For example, Tang & Xu (2016) find that price run-ups before M&A announcements are associated with unreported insider trading and cannot be predicted by public information. Frino et al. (2013) find that price run-ups can be predicted by insiders' traded volume, and the run-ups tend to be smaller in high sanctions environment, indicating that insiders are less likely to use their private information as the associated cost increases. Thus, we should expect that more shadow trading would result in higher run-up in the related ETF prior to the source firm's news announcement date.

We measure price run-up before M&A announcements in a related ETF by modifying the measure proposed in Foley et al. (2021). In their paper, the abnormal target stock return for the five days prior the M&A announcement ($CAR_i(-5,-1)$) is expressed as a proportion of the total abnormal returns around the announcement ($CAR_i(-5,+2)$). The measure assumes that both the pre- and post-announcement returns are positive for M&A target firms, as empirical evidence points towards the validity of this assumption. However, for the purpose of researching ETFs we should modify the calculation. Since ETFs track numerous firms besides the M&A target and are highly liquid, it is not given that the ETF experiences positive returns around a portfolio firm's M&A announcement. We modify the measure to allow for a negative run-up. Thus,

$$RunUp_i = CAR_i(-5,-1) \tag{3}$$

where $CAR_i(-5,-1)$ is the cumulative abnormal return in a source-firm-related ETF i from five days before the M&A announcement day to one day before the announcement. Daily abnormal return is calculated as the mid-quote return of the ETF minus the mid-quote return of the corresponding value-weighted market index, Russell 3000 in our case.

4.2.3. Order imbalance

Our third shadow trading proxy is related to the direction of trades prior to the M&A announcement. Fishe & Robe (2004) find that during periods of insider trading the trades are mostly buyer-initiated. Ahern (2018) tests the power of various financial indicators to predict insider trading and finds that order imbalance has significant predictive power for categorizing insider trading cases.

Since we cannot collect data on buyer- and seller-initiated volume, we employ two methods to categorize the pre-aggregated daily trading volume.

First, we categorize trade volume into buyer- and seller-initiated trades using the bulk volume classification (BVC) method developed by Easley, López de Prado, and O'Hara (2016). It is a probabilistic method used to assign direction to pre-aggregated volume over a chosen period. We apply the following formula to ETF data to calculate buyer-initiated volume:

$$V_{i,t}^{B} = V_{i,t} * Z\left(\frac{log\left(Close_{i,t}\right) - log\left(Close_{i,t-1}\right)}{\sigma_{\Delta P}}\right) \tag{4}$$

where $V_{i,t}$ is the volume traded in ETF i at day t, Z is the CDF of the standard normal distribution, $log(Close_{i,t})$ – $log(Close_{i,t-1})$ is the log daily price change, and $\sigma_{\Delta P}$ is a measure of the volume-weighted standard deviation of the security's log price changes over the sample period. The method relies on the fact that buyer-initiated trades increase the price, while seller-initiated trades decrease the price of the ETF. By looking at the distribution of returns of ETF i over the sample period, we estimate what share of volume on day t is buyer initiated depending on what the price change has been over the day. Seller-initiated volume is then:

$$V_{i,t}^S = V_{i,t} - V_{i,t}^B \tag{5}$$

where $V_{i,t}$ is the volume traded in ETF i at day t, and $V_{i,t}^S$ and $V_{i,t}^B$ are seller- and buyer-initiated volumes in ETF i at day t, respectively.

While BVC is typically applied to intraday data, it can also be used with daily data, which we do in our study. As a robustness test, we categorize the volume via a simpler method, which is also based on the daily price changes. The process is as follows: if ETF i has a positive return on day t, all daily volume is classified as buyer initiated, thus $V_{i,t}^B = V_{i,t}$. If ETF i has a negative return on day t, all daily volume is classified as seller initiated, thus $V_{i,t}^S = V_{i,t}$. If the return on day t is zero, the daily volume is split equally, and $V_{i,t}^S = V_{i,t}^B = 0.5 * V_{i,t}$.

After the volume categorization, order imbalance calculation is the same for both methods. Following Foley et al. (2021):

$$Imbalance_i = \frac{B_i(-5,-1) - S_i(-5,-1)}{B_i(-5,-1) + S_i(-5,-1)} \tag{6}$$

where $B_i(-5,-1)$ and $S_i(-5,-1)$ are cumulative buyer- and seller-initiated volumes ($V_{i,t}^B$ and $V_{i,t}^S$) from five days ($t = -5$) to one day ($t = -1$) before the M&A announcement in an ETF i. The measure is bound within the value range [-1, +1], with larger values indicating a proportionately larger amount of buyer-initiated volume, and thus larger likelihood of insider (and shadow) trading. In further text we will refer to the calculated proxy which uses BVC-categorized volume data as $BVC\ Imbalance$, and the proxy which uses the simpler method to categorize volume will be referred to as $Simple\ Imbalance$.

4.3.Stock-ETF relatedness

To determine relevance of an ETF to an M&A target from insider's perspective, we use two filters:

1. ETF must be tracking the industry of the target firm according to Lipper and ICB classification.
2. ETF historical returns must be closely correlated with the target firm returns.

We continuously calculate 180-day rolling correlations of target firms with same-industry ETFs and use the value at the day prior the announcement as a stock-ETF relatedness measure. We suppose insiders will perceive highly correlated same-industry ETFs as the “safest” vehicles for shadow trading among all other ETFs. We see rolling correlations as the second-best alternative to holdings data, which is unavailable in our data sources.

4.4.Bootstrapping

The main challenge of our analysis is that given a sufficiently large sample of related ETFs, we will observe some abnormal activity in these ETFs around M&A announcement date purely by statistical chance. An example of a similar challenge is to separate luck and skill when studying fund manager performance – given a large sample of professionals, some will beat their benchmarks by chance, even over multiple consecutive periods. If Type 1 error is ignored in such tests, spurious conclusions can be drawn, e.g., that all the managers that beat benchmarks are more skillful than others. In our setting, explaining all cases of abnormal activity in ETFs by shadow trading would be a similar mistake. A common practice used to quantify impacts beyond statistical randomness in statistics is bootstrapping (see, e.g., Kosowski et al., 2006; Putniņš & Barbara, 2020; Augustin et al., 2019; Reeb et al., 2014).

To test the presence of shadow trading, we use three separate bootstrap specifications to draw random samples. Through random sampling we find the level of shadow trading proxies we would expect to see purely by chance (due to randomness) and compare the random distributions against the distributions of ETFs where we suspect shadow trading.

For all bootstrap specifications, the first three steps are the same. We start by drawing the sample of ETFs where we suspect the presence of shadow trading:

1. Based on our chosen filters (to be discussed in more detail in the Results section), draw a subsample of M&A deals where we expect shadow trading.
2. For each chosen M&A deal, find all ETFs that track the industry of the M&A target.
3. Based on filters (to be discussed in more detail in the Results section) draw a sample of ETFs we expect to be shadow traded from the sample of all ETFs tracking the M&A target’s industry.

After choosing the ETFs where shadow trading is suspected, we create three random samples to compare the suspected ETFs against. The following steps differ in each random sample creation process. We develop the sampling process based on the following logic. First, it is important to recognize that we have a panel dataset with multiple ETFs (both suspected and unsuspected in shadow trading) observed at multiple time periods (including M&A dates and non-M&A dates). Therefore, we can introduce randomness in our samples by either a) randomly choosing the date (comparing a suspected ETF on M&A date against the same ETF on random days), b) randomly choosing the ETF (comparing a suspected ETF on M&A date against random ETFs on the same date), c) randomly choosing both the date and ETF (comparing a suspected ETF on a M&A date against random ETFs on random days).

In the first specification (random days approach), the procedure is as follows:

4. Calculate shadow trading proxies *as of the actual announcement date* for each ETF chosen in step 3.
5. Calculate shadow trading proxies *as of some random date* for each ETF chosen in step 3, i.e., use a random date instead of the true announcement date. We impose a filter that the random date must be at least one month before the true M&A announcement date, as we are not certain about the exact time the insider acquires information and chooses to shadow trade. Similarly, we exclude dates one month after the M&A as those can be impacted by market incorporating the news. Additionally, we exclude periods when the chosen ETF is suspected in another M&A deal.
6. Repeat the 5th step 1,000 times in total, each time choosing dates randomly with replacement. As a result, we have 1,000 observations at random dates for each ETF chosen in step 3.

In the second specification (random ETF approach), the procedure is as follows:

4. Calculate shadow trading proxies as of the actual announcement date for each *ETF chosen in step 3*.
5. For each ETF chosen in step 3, calculate shadow trading proxies as of the actual announcement date for a *randomly chosen ETF that tracks a different industry*. We impose a filter that the randomly chosen ETF cannot be suspected in another M&A deal at the same time (suspected period for that ETF includes 30 days before and after the respective M&A).

6. Repeat the 5th step 1,000 times in total, each time choosing an ETF from another industry randomly with replacement. As a result, we have 1,000 randomly chosen ETFs from another industry on the M&A announcement date for each ETF chosen in step 3.

In the third specification (random both approach), the procedure is as follows:

4. Calculate shadow trading proxies *as of the actual announcement date* for each *ETF chosen in step 3.*
5. For each ETF chosen in step 3, calculate shadow trading proxies as of *some random date* for *a randomly chosen ETF that tracks a different industry*. Again, we impose the filter that the randomly chosen ETF cannot be suspected in another M&A deal at the same time.
6. Repeat the 5th step 1,000 times in total for each ETF, each time choosing a different ETF at a different date, sampling with replacement. As a result, we have 1,000 random observations (random ETF at a random date) for each ETF chosen in step 3.

For all specifications, the next step is comparing the distribution of shadow trading proxies for the suspected ETFs on the actual M&A date to the randomly drawn sample. The intuition behind the test is the following: if the ETFs chosen in step 3 are shadow traded, we should see more abnormality in them than in the randomly drawn samples. We calculate the shadow trading proxy values for the suspected ETFs and check them against the random distributions. Then we count how many of the suspected ETFs have a higher proxy value than some percentile in the random distributions. As we do not expect very large abnormal volumes created by insiders (due to high liquidity of ETFs), in our tests we focus on the median and the 60th percentile. If there was no shadow trading, then 50% of ETFs chosen in step 3 should have a proxy value higher than the median in the random distribution (the distribution would perfectly fit the random distribution), and 40% of the ETFs should have a value higher than the 60th percentile in the random distribution. If, however, more than 50% of ETFs have a proxy value above the median in the random distribution, there is abnormality above statistical randomness – we establish that some share of ETFs is being shadow traded.

The procedure is as follows:

7. Pool together the 1,000 randomly chosen observations to create a placebo distribution for each shadow trading proxy for each ETF chosen in step 3.
8. Determine the median and the 60th percentile of each placebo distribution.
9. For each ETF, check if its value of each of the shadow trading proxies is above the thresholds estimated in step 8, i.e., where the actual abnormality level is compared to a randomly drawn sample.

10. Count how many ETFs from the sample drawn in step 3 have abnormality measures higher than the chosen percentiles in the placebo distribution.
11. Calculate the proportion of all ETFs drawn in step 3 that have shadow trading proxies higher than the chosen percentiles in the placebo distribution.
12. Test if the share of ETFs that have shadow trading proxies higher than each percentile in its placebo distribution is statistically significantly higher than expected, e.g., for the median test if more than 50% of ETFs are above the median in their random distribution, and for the 60th percentile test if more than 40% of ETFs have a proxy value higher than the value corresponding to the 60th percentile in the random distribution. We use one-sample mean t-test.

4.5. Linear regression

By running the bootstrapping tests, we establish a level of abnormality above randomness in the suspected ETF subsample and infer what share of ETFs is shadow traded. Our next test is linear regression, which we use to estimate the scale of shadow trading – the impact insiders have on ETF abnormality characteristics. Thus, while bootstrapping helps estimating the frequency of shadow trading, linear regressions add the size of the shadow trading effect.

We estimate the following equation:

$$\begin{aligned} ShadowTrading_{i,x,t} &= \beta_0 + \beta_1 * Related_{i,x,t} + \sum \beta_j * Controls_{j,i,t} + \sum \beta_k * Controls_{k,x,t} \\ &+ Fixed\ Effects_{i,t} + \varepsilon_{i,x,t} \end{aligned} \tag{7}$$

where $ShadowTrading_{i,x,t}$ is measured as one of the proxies (abnormal returns, abnormal volume, or order imbalance) for ETF i paired with target stock x at day t. Stock, ETF, and day uniquely identify a shadow trading "case". $Related_{i,x,t}$ is a dummy variable taking value 1 if ETF i is suspected for shadow trading coming from insiders of stock x on a day t according to our chosen criteria (to be discussed further in the Results section). $Controls_{j,i,t}$ and $Controls_{k,x,t}$ include multiple characteristics of target firm x and ETF i on day t. We control for firm market capitalization, price-to-book value, firm's 2-day CAR around the M&A announcement, ETF market capitalization and share turnover (see Appendix 3 for the full list of variables we use in our analysis). A statistically significant β_1 is an indicator of shadow trading.

To build the data sample on which the regression is run, we again follow the bootstrapping logic. For each suspected ETF-stock pair (that has $Related_{i,x,t} = 1$) we draw 20 random observations following the ‘random both’ approach described previously, i.e., we choose 20 random ETFs on random dates and mark them as unsuspected for the same stock x ($Related_{i,x,t} = 0$). The ratio of a constant number of random observations for each suspected observation ensures that each suspected ETF-stock pair (“case”) is given an equal weight in the dataset. In our regressions, we use ETF and month fixed effects but do not cluster errors at ETFs as our samples are very imbalanced (the data for each ETF is not continuous).

5. Results and discussion

5.1. Summary statistics

Table 1. ETF summary statistics

Statistic	N	Mean	St. Dev.	Min	Max
Panel A. Same-industry ETFs on M&A announcement dates, sample period 2009-2021					
ETF market capitalization (million USD)	69,301	1,111.29	3,244.78	0.46	53,487.08
ETF CAR (-1;1)	69,630	0.00	0.01	-0.31	0.17
180-day rolling correlation	69,640	0.29	0.23	-0.27	0.95
ETF abnormal volume	68,556	-0.05	0.31	-0.99	1.00
ETF run-up	69,640	-0.0004	0.02	-0.26	0.26
ETF simple imbalance	67,701	0.08	0.53	-1.00	1.00
ETF BVC imbalance	67,701	0.02	0.24	-0.99	0.96
Panel B. All ETFs, sample period 2009-2021					
ETF market capitalization (million USD)	928,551	1,104.72	2,949.18	0.42	53,777.24
ETF abnormal volume	928,551	-0.06	0.31	-1	1
ETF run-up	900,388	-0.0005	0.02	-0.70	0.38
ETF simple imbalance	911,020	0.06	0.53	-1	1
ETF BVC imbalance	911,024	0.01	0.25	-1	1

Note. Panel A shows same-industry ETF summary statistics on M&A announcement dates. Panel B shows summary statistics for the whole ETF universe over our sample period. For computational efficiency we only merge rolling correlations with our sample of ETFs on M&A days. Metrics are calculated by the authors using data from Refinitiv Eikon and Datastream.

Table 1 presents summary statistics of all same-industry ETFs on the target M&A announcement date, regardless of their size and correlation with the target (we later refer to this sample as full sample). Return of the same-industry ETF is on average zero percent with a standard deviation of 1%. This implies that the impact of acquisition on peer companies is on average zero, in line with Mehta et al. (2020) who argue that mergers tend to have heterogenous effects on industry structure.

We also document that ETF abnormal volume is on average negative -0.05 with a standard deviation of 0.31. Negative abnormal volume in the full sample indicates that shadow trading is at best not widespread, generally in line with our hypotheses. Run-up prior the merger announcement is very close to zero, while imbalance is slightly positive on average and ranges between 0.02 for the BVC imbalance and 0.08 for simple imbalance.

We compare the M&A announcement date ETF metrics to the same metrics over the whole sample period. The average values of all shadow trading proxies are slightly lower than on M&A announcement dates, but the difference is not economically large when comparing to the standard deviation of the variables.

5.2.Shadow trading in the suspected ETFs

We start our main tests by zooming into cases where we may reasonably expect insiders to be more likely to shadow trade. We introduce several filters using insights from the Literature Review, in line with our Hypothesis 2.

1. Correlation between target stock and ETF is high, indicating larger weight of the target in ETF portfolio. We require that the 180-day rolling correlation is above the third quartile in the full sample (all same-industry ETFs per deal), which corresponds to 0.46.
2. Target announcement return is above median in the full sample year.[3] High return potential motivates insider trading. Although insiders cannot perfectly predict returns, they can be assumed to have a signal based on synergies, valuation, and other relevant deal characteristics.
3. Target market capitalization is above median in the full sample year. This filter reinforces the correlation filter by attempting to separate weight-driven correlations from spurious correlations.
4. ETF market capitalization is above median in the full sample year. As suggested by Kyle (1985), informed traders are more likely to trade in more liquid stocks (ETFs) to hide their trades among noise traders. While market size is not a direct measure of liquidity, we believe it is well-suited for our shadow trading scenario. We argue that shadow traders are not likely to calculate liquidity measures; rather, when looking for an ETF to trade, they

[3] Sample year refers to a sub-sample of the full sample based on M&A date. For example, for a deal announced on April 13, 2020, we would look at the distribution of target returns in 2020. We use sample-years to account for dynamic market conditions.

are likely to specifically look for or find “big names” in ETF industry, like Vanguard or BlackRock, which tend to manage the largest industry ETFs.

5. Deal is not rumored. Rumored deals have on average lower returns, as found in Alperovych et al. (2021) and confirmed in our sample – we calculate a mean announcement target CAR of 9.9% for rumoured deals and of 21.8% for non-rumoured deals. Lower potential return disincentivises insider trading. In addition, if a considerable price movement on rumours in a target stock is not reflected in an ETF, an insider may more critically assess potential shadow trading returns.

The screens cut our focus from 2,734 to 341 deals or roughly 12.5% of the full sample. For each deal, we look at up to 10 largest ETFs. The choice of ETF market capitalization is motivated by the findings of Kyle (1985) – we assume that given a choice of more than 10 highly-correlated, big ETFs, a shadow trader would still choose the largest to hide their trading volume. On the other hand, we consider 10 ETFs instead of just three or five ETFs as market capitalization is dynamic, and if there is a fairly short list of candidates, insiders may choose based on the unobserved individual preferences. At the end, we have 2,734 suspected deal-ETF pairs, picking 8 ETFs per deal on average.

The results of a percentile test and an OLS regression are both presented in Table 2. We obtain a highly statistically significant evidence of shadow trading in 3.8% to 6.2% of suspected ETFs based on abnormal volume median (50th percentile) test, depending on the bootstrap. 60th percentile test indicates shadow trading in 2.8%-4.9% of suspected ETFs. These ETFs are estimated to have 0.025 higher abnormal volume, all else equal, which is a non-negligible amount of around 8% of a standard deviation of abnormal volume measure in our full sample. Its economic significance is further amplified by high liquidity of ETFs – it is more difficult to create abnormal volume in large ETFs (one of our screens) than in target stock (classic insider trading).

We find significant evidence of, all else equal, a 0.1 pp. lower run-up in suspected ETFs, suggesting short selling. The effect is economically small, 5% of a standard deviation of ETF run-up in our full sample, but it is non-negligible given high liquidity of ETFs and a relatively small number of insiders compared to other market participants. Negative run-up is also confirmed by the percentile

Table 2. Results from the main analysis

Panel A. Percentile tests

Percentile	Random days (N=2734)		Random ETF (N=2734)		Random both (N=2734)	
	Estimated proportion	p-value	Estimated proportion	p-value	Estimated proportion	p-value
50						
Abnormal volume	0.5388 ***	0.0000	0.5563 ***	0.0000	0.5618 ***	0.0000
Run-up	0.4627 ***	0.0001	0.4854	0.1260	0.4792 **	0.0292
Order imbalance	0.4642 ***	0.0002	0.4941	0.5406	0.4678 ***	0.0008
Order imbalance (BVC)	0.4788 **	0.0265	0.4835 *	0.0852	0.4901	0.3018
60						
Abnormal volume	0.4396 ***	0.0000	0.4279 ***	0.0032	0.4495 ***	0.0000
Run-up	0.3625 ***	0.0000	0.3731 ***	0.0036	0.3628 ***	0.0001
Order imbalance	0.3672 ***	0.0004	0.3778 **	0.0169	0.3665 ***	0.0003
Order imbalance (BVC)	0.3998	0.9813	0.3855	0.1199	0.4060	0.5232

*Note: *p<0.10; **p<0.05; ***p<0.01*

Panel B. OLS regression

	Dependent variable:			
	Abnormal volume	Run-up	Order imbalance	Order imbalance (BVC)
Related	0.025***	-0.001***	-0.033***	-0.010**
	(0.007)	(0.0005)	(0.011)	(0.005)
log (Target market cap.)	0.001	-0.0001	-0.002	-0.001
	(0.001)	(0.0001)	(0.002)	(0.001)
log (ETF market cap.)	-0.003**	-0.001***	-0.006**	-0.009***
	(0.002)	(0.0001)	(0.003)	(0.001)
ETF share turnover	-0.011***	0.0001	0.0010	-0.001
	(0.001)	(0.0001)	(0.002)	(0.001)
PTBV	-0.0001	0.0000	0.00001	0.00003
	(0.0001)	(0.00001)	(0.0001)	(0.0001)
Target 2-day CAR	-0.007	-0.0004	-0.004	-0.009
	(0.010)	(0.001)	(0.017)	(0.008)
Constant	-0.034	0.011***	0.113	0.079**
	(0.048)	(0.003)	(0.082)	(0.037)
ETF fixed effects	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes
Observations	56,236	56,236	56,236	56,236
R^2	0.068	0.036	0.077	0.104
Adjusted R^2	0.057	0.025	0.066	0.094
Residual Std. Error (df = 55600)	0.303	0.022	0.517	0.234
Note:				$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Note. The table presents the main results of the analysis described in Section 5.2. Panel A shows the results of the percentile test. We report the proportion of cases in the M&A sub-sample that have their M&A shadow trading proxy above different percentiles in their random (bootstrap, non-M&A) 1000 observation samples. The process of the bootstrap sample creation – Random days, Random ETF, Random both – is described in Methodology Section 4.4. Panel B shows the results of the linear regression described in Equation 7. In Panel B standard deviation is reported in brackets under the coefficient.

tests, where we see significantly less than 50% of suspected ETFs having a proxy value above the median in a placebo sample.

We also find that suspected ETFs are expected to have by 0.03 lower order imbalance. This corresponds to around 5.5% of the standard deviation of order imbalance in our full sample. We see this as a direct consequence of a negative price run-up, imposed by the construction of the variable.

In Appendix 4, we include distributions of shadow trading proxies in the sub-sample used for tests presented in Table 2 compared with their respective placebo distributions. We see a noticeable deviation of the distribution to the right for abnormal volume and less pronounced shift to the left for the run-up and imbalance measures.

Overall, we find strong evidence of abnormal volume in a fraction of suspected ETFs, confirming the Hypothesis 1 that some shadow trading does take place in ETFs. However, it does not necessarily mean that the ETFs outside our suspected sample are systematically less likely to be shadow traded, as implied by the Hypothesis 2. To understand if our screens are robust, we perform the same tests as in Table 2 for all the non-suspected ETFs. For computational efficiency, we randomly select up to 5 ETFs per deal. Results, in a format analogous to Table 2, are presented in Table 3.

We find little evidence of any abnormal activity in the non-suspected sample. In the percentile tests, we observe some indications of negative abnormal volume and positive run-ups and imbalances, but we do not confirm them with regressions. Being a part of the non-suspected sample is not useful for predicting abnormal volume and imbalance. While there appears to be a statistically significant relation between the outside-filter ETFs and run-ups, the effect is twice as small as in the suspected sample and is inconsistent with the percentile tests. We confirm our Hypothesis 2 – shadow trading is more likely to be observed in M&A deals where the target is a large index constituent, sizeable announcement returns are expected, and closely related ETFs are widely traded.

We can also confirm Hypothesis 3 – we indeed see abnormal levels of trading volume in the ETFs before M&A announcements. We cannot, however, confirm Hypothesis 4, as price run-ups are small but significantly negative. We thus find it relevant to discuss potential reasons for the unexpected finding.

As we consider the largest half of the targets every year, one possible explanation is that insiders in larger companies often expect mergers to be bad news for their industry as their companies are further reinforced by a strong parent, making competition for peers more difficult. Another relevant source of short selling may be target announcement CAR – if an insider expects very strong benefits to their firm from the deal, they can consider that other firms will have particularly hard time competing.

Table 3. Results from the placebo (anti-join) test

Panel A. Percentile tests

	Random days (N=13806)		Random ETF (N=13806)		Random both (N=13806)	
Variable	**Estimated proportion**	**p-value**	**Estimated proportion**	**p-value**	**Estimated proportion**	**p-value**
50						
Abnormal volume	0.5064	0.1298	0.4925 *	0.0796	0.4970	0.4853
Run-up	0.4984	0.7081	0.5066	0.1214	0.5070 *	0.0987
Order imbalance	0.5072 *	0.0887	0.5118 ***	0.0055	0.5134 ***	0.0016
Order imbalance (BVC)	0.5002	0.9593	0.5136 ***	0.0014	0.5106 **	0.0123
60						
Abnormal volume	0.4087 **	0.0384	0.4011	0.7999	0.4040	0.3437
Run-up	0.3887 ***	0.0063	0.4058	0.1625	0.3854 ***	0.0004
Order imbalance	0.4082 **	0.0492	0.4181 ***	0.0000	0.4176 ***	0.0000
Order imbalance (BVC)	0.4006	0.8813	0.4164 ***	0.0001	0.4101 **	0.0157

*Note: *p<0.10; **p<0.05; ***p<0.01*

Panel B. OLS regression

	Dependent variable:			
	Abnormal volume	**Run-up**	**Order imbalance**	**Order imbalance (BVC)**
Related	0.001	-0.001^{***}	-0.005	-0.003
	(0.003)	(0.0002)	(0.005)	(0.002)
log (Target market cap.)	0.0002	0.00001	-0.0002	0.0001
	(0.0002)	(0.00002)	(0.0004)	(0.0002)
log (ETF market cap.)	-0.006^{***}	-0.001^{***}	-0.008^{***}	-0.007^{***}
	(0.001)	(0.0001)	(0.001)	(0.001)
ETF share turnover	-0.008^{***}	0.0001^{*}	-0.001	-0.001^{*}
	(0.0004)	(0.00003)	(0.001)	(0.0003)
PTBV	-0.0000	-0.0000	-0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Target 2-day CAR	0.0003	-0.00003	-0.003	-0.001
	(0.002)	(0.0001)	(0.003)	(0.001)
Constant	-0.015	0.006^{***}	0.059	0.065^{***}
	(0.021)	(0.002)	(0.037)	(0.017)
ETF fixed effects	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes
Observations	268,385	268,385	268,385	268,385
R^2	0.066	0.030	0.071	0.095
Adjusted R^2	0.064	0.028	0.068	0.093
Residual Std. Error	0.294 (df = 267746)	0.023 (df = 267868)	0.513 (df = 267564)	0.234 (df = 267564)

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Note. The table presents the results of the non-suspected sample analysis described in Section 5.2. Panel A shows the results of the percentile test. We report the proportion of cases in the M&A sub-sample that have their M&A shadow trading proxy above different percentiles in their random (bootstrap, non-M&A) 1000 observation samples. The process of the bootstrap sample creation – Random days, Random ETF, Random both – is described in Methodology Section 4.4. Panel B shows the results of the linear regression described in Equation 7. In Panel B standard deviation is reported in brackets under the coefficient.

To test these explanations, in Appendix 5 we modify our screens for target returns and target size. We only report results for abnormal volume and run-up for brevity (note that our imbalance proxies are to a large extent run-up determined, as seen in Table 2). First, instead of requiring above-median target CAR, we look at below-median target CAR (all other screens, including target size, are left unchanged). We find much weaker evidence of shadow trading in low-return announcements. Abnormal volume is significantly different from chance in two out of three bootstrap tests, but it is not confirmed by the regression. Run-up is indistinguishable from zero. This suggests that insiders are not incentivized to shadow trade when they do not expect big returns in their stock, consistent with our motivation for introducing this filter in the first place.

Among the below-median target CAR cases, there is a subsample that might be of particular interest – negative returns announcements. There are only 50 negative target CAR deals that have at least one related ETF after satisfying our screens, and, similarly to the above/below median split, we cannot confirm shadow trading neither using abnormal volumes nor run-ups (Appendix 5). While it may appear that run-ups are positive and are unsignificant only due to the small sample size, this is a bold assumption and so we refrain from making it. Nevertheless, from these two tests we can conclude that it is mostly when anticipated target returns are big would insiders look for shadow trading opportunities, and in these cases, they tend to short sell industry ETFs.

Then, we modify our filter for the target size, keeping the target CAR and other screens in place. We look at targets below the median target market capitalization in a full sample-year (Appendix 5) and find no shadow trading – similarly to the return split, abnormal volumes are not confirmed by one of the bootstrapping procedures and the regression, while run-ups are not statistically different from zero using any approach. We conclude that even when anticipated returns are large, insiders generally do not shadow trade in ETFs if their firm is small. The most intuitive explanation we provide is that in such situations, insiders do not think that the announcement will be material for their peers, or, even more simple, there is no ETF where the stock is included or has any meaningful share of total holdings.

Finally, decision to shadow trade might realistically be impacted by some unobserved factors, like industry structure. In this case, or even if the factors we have considered are the most material, it is important to understand if insiders on average can predict ETF CAR. We thus perform another test in Appendix 5, where we do not change any filters, but look separately at the outcomes based on positive and negative ETF announcement returns. First, we confirm shadow trading using abnormal volumes in all three bootstrap tests and the regressions. What is more surprising, run-up estimates are almost identical and negative in both cases, although confirmed only by the random day samples and the regressions. The findings are strongly consistent with the conclusion that insiders cannot predict positive ETF CARs, and, more generally, that their informational signals are not strong.

A question emerges why insiders would trade if they do not have a high-quality signal. One explanation is inspired by the behavioral finance literature. If, due to overconfidence, greed, or limited cognitive capacity, an insider strongly believes they can very well predict ETF announcement CAR, they would still shadow trade. In other words, it is not what insiders possess but what they think they possess that is decisive.

Another hypothesis that could explain such behavior is that the main motivation of insiders trading ETFs before M&A deals is not abnormal return to be earned on the announcement, but rather unwillingness to be exposed to the increased volatility following the announcement. According to this hypothesis, insiders use the non-public information to avoid uncertainty rather than make profit, and exit their existing long ETF positions. Importantly, as we have weak evidence of shadow trading when the target is small or target returns are small, it seems reasonable that such motive is more relevant for insiders of big firms that also expect a lot of movement in the stock price on the announcement.

The set of explanations we provide may not be exhaustive. As we cannot confirm which explanation is the most valid with our data, we avoid any further speculation on this matter. We also note that while it is important to discuss potential reasons for negative run-ups, the evidence for non-zero run-ups is in general weaker than for non-zero abnormal volumes. This becomes more evident after looking at the results of robustness checks that we present later.

5.3. Shadow trading by years

This far, our findings have been describing the ‘big picture’: the amount of shadow trading in related ETFs over the whole sample period. Now we move on to the analysis of sample subsets. First, we look at the time dynamics. We apply the filters described previously and run the analysis for year groups to ensure a sufficient number of observations in each subsample. The results are presented in Table 4. We focus on abnormal volume since it is being directly adapted from insider trading literature with no further modifications and allows us to look deeper into the confirmed Hypothesis 3.

Table 4. Abnormal volume analysis by years

	Bootstraps			Regression	Sample size	
	Random days	Random ETF	Random both	Random both		
Year	**Estimated proportion**	**Estimated proportion**	**Estimated proportion**	**Estimated effect**	**ETFs**	**Deals**
2009, 2010, 2011	0.5032 (0.8436)	0.5595 *** (0.0003)	0.5281 * (0.0871)	0.0042 (0.7011)	924	109
2012, 2013	0.5230	0.5209	0.5383	0.004	392	50

Year	Bootstraps: Random days — Estimated proportion	Bootstraps: Random ETF — Estimated proportion	Bootstraps: Random both — Estimated proportion	Regression: Random both — Estimated effect	Sample size: ETFs	Sample size: Deals
	(0.3639)	(0.5641)	(0.1299)	(0.8091)		
2014, 2015	0.6597 *** (0.0000)	0.5707 * (0.0505)	0.6911 *** (0.0000)	0.0647 ** (0.0113)	191	26
2016, 2017	0.5895 *** (0.0004)	0.5816 *** (0.0014)	0.6105 *** (0.0000)	0.0527 *** (0.0058)	380	52
2018, 2019	0.6127 *** (0.0001)	0.5683 ** (0.0152)	0.6444 *** (0.0000)	0.0349 ** (0.0474)	315	38
2020, 2021	0.4644 (0.1825)	0.5043 (0.8730)	0.4872 (0.6316)	-0.0065 (0.6962)	351	42

*Note: *p<0.10; **p<0.05; ***p<0.01*

Note. The table presents the abnormal volume analysis by year, as described in Section 5.3. Suspected ETFs are chosen based on the filters described in Section 5.2. The process of the bootstrap sample creation – Random days, Random ETF, Random both – is described in Methodology Section 4.4. The Regression column shows the results of the *Related* variable from linear regression described in Equation 7. p-value is reported in brackets under the coefficient.

The years with the strongest evidence for shadow trading are in the middle of our sample period, from 2014 to 2019. Depending on the specification, around 7%-19% of ETFs were shadow traded in 2014 and 2015, 8%-11% in 2016-17, and 6%-14% in 2018-19. The estimated effect is decreasing over time: in 2014 suspected ETFs had by 0.06 higher abnormal volume, which decreased to 0.03 in 2018-19.

We explain the absence of shadow trading evidence during the first four years of our sample (2009-2013) by the novelty of ETFs. Since these investment vehicles started gaining popularity around 2008, we should naturally expect some time to pass before insiders consider them as safe vehicles for informed trading. One of the features increasing the attractiveness of ETFs over time is the growing assets under management – as more funds were invested into ETFs starting from 2008, a higher variety of investment options was offered. Consequently, competition and economies of scale drove down the ETF fees (Ben-David et al., 2022; Johnson, 2021), providing insiders with liquidity.

We first find evidence of shadow trading in the 2014-15 subsample. Later, the estimated effect from shadow trading gradually declines over the years. We do not, however, find evidence that there are less opportunities to shadow trade. The average target CAR around the M&A date remains around 25%-30% throughout our sample period, indicating that the value of deals on average is not decreasing. Similarly, the number of ETFs in the market is growing. Looking at the two facts together, we can infer that in the later years insiders should have similar, perhaps even more opportunities to utilise their material non-public information. We can explain the decrease in shadow trading from two perspectives. First, we should consider the learning component of shadow trading. As the mean ETF return around a related firm's M&A date is 0% in our sample, we propose that the average insider could try the shadow trading strategy, earn a return of 0% or even lose due to transaction costs, and not repeat the strategy, thus driving down the abnormality we observe over the years. A second, alternative explanation assumes that the number of insiders who shadow trade on average remains constant or is increasing, but the daily traded volumes of ETFs is increasing at a faster rate. In other words, the relative share of shadow traders in the ETF clientele declines. Therefore, by construction, the same absolute shadow trading volume creates smaller abnormality in our proxies and can be deemed statistically insignificant in our tests. The second explanation would be especially relevant for our suspected ETF sample, as we look at the most liquid ETFs with large daily trading volumes.

Further, we find no statistical significance of shadow trading in the last 2 years of our sample. As 2020 and 2021 were years of record high ETF volume, we may again argue that insiders' traded volume creates negligible abnormality against the large trading volume. We may also consider a different scenario. The second half of 2019 was when regulators first proposed the Insider Trading Prohibition Act, which is the first attempt to codify the insider trading court precedents into a statute (Mukhi et al., 2019). The bill has not been passed by the US Senate, but it has not been dismissed either; in May 2021 the bill was approved by the US House of Representatives (Godoy, 2021). The work on the bill shows increased regulatory attention on illegal trading. According to Patel and Putniņš (2021), higher regulatory attention increases the probability of insider trading detection and prosecution, which, as argued by Mehta et al. (2020), should incentivize insiders to switch from conventional illegal trading vehicles to others. We find the opposite evidence – after 2019, ETF shadow trading decreases. If we also consider the shadow trading charges brought against M. Panuwat by the US SEC in August 2021, we could conclude that insiders are aware of the legal ambiguity of shadow trading and do not want to risk prosecution. We should, however, note that 2020 and 2021 are years of the COVID-19 pandemic and extreme market turmoil, therefore any conclusions drawn from this sample period might not be applicable to standard market conditions.

To sum up, we find strong evidence of shadow trading in ETFs a few years after the instrument started gaining popularity in financial markets. We observe declining shadow trading effect over the years, rejecting our Hypothesis 5, and provide two alternative explanations to the finding. First, it is possible that the shadow trading volume has decreased over the years as the strategy brings insiders negligible returns, accompanied with increased regulatory attention on both insider and shadow trading. As an alternative perspective we propose that the absolute shadow trading volume has not declined. The relative share of shadow traders in the ETF market, however, might be decreasing as non-shadow traded volume grows quicker than the shadow-traded volume.

5.4.Shadow trading by industries

We move onto analysing the presence of shadow trading across different industries. We start by comparing how often each industry's M&A deals appear in the suspected sample and compare to the full M&A sample. If an industry appears more frequently in the suspected sample, we could conclude that it presents better opportunities for its shadow traders.

Table 5. Weight of industries in the suspected versus full sample

Industry	**Share among suspected M&A deals Sample size = 341**	**Share among all M&A deals Sample size = 2,734**
Health Care	73 (21%)	395 (14%)
Technology	63 (18%)	387 (14%)
Industrials	53 (16%)	278 (10%)
Financials	37 (11%)	695 (25%)
Consumer Discretionary	35 (10%)	345 (13%)
Energy	26 (7.6%)	195 (7.1%)
Basic Materials	18 (5.3%)	87 (3.2%)
Telecommunications	13 (3.8%)	116 (4.2%)
Real Estate	9 (2.6%)	84 (3.1%)
Consumer Staples	8 (2.3%)	98 (3.6%)
Utilities	6 (1.8%)	54 (2.0%)

Note. The table compares the weight of industries in the suspected sample to the weight of industries in the full sample. The suspected sample is chosen according to the filters described in Section 5.2.

We present the comparison in Table 5. Three industries stand out. First, the Health Care sector is represented in 21% of the suspected sample's deals, and only 14% in the full sample. The second industry is Technology, which accounts for 18% of the deals in the suspected sample and 14% in the full sample. Last, the Industrials sector comprises 16% of the suspected sample and 10% in the full sample. These three industries together account for around 50% of the suspected sample (up from 38% in the full sample), while the remaining eight industries together comprise the other 50%. Therefore, we draw our first inference about shadow trading in different industries and note that Health Care, Technology and Industrials industry seem to offer better opportunities for insiders to shadow trade. There are two other industries with a higher share in the suspected subsample than the full sample, yet their absolute weight is not as significant. The share of Energy sector deals increases from 7.1% in the full sample to 7.6% in the suspected subsample, and Basic Materials industry shows an increase from 3.2% to 5.3%.

After our initial screen of the industries where more shadow trading should be expected, we run our full tests on each industry separately. The results are reported in Table 6. Our previous finding is reflected in the results. In the Health Care sector, we find statistically significant abnormal volume in two out of three bootstrap specifications, which points towards shadow trading in 4.9%-5.82% of Health Care sector ETFs, with an average higher abnormal volume of 0.0439. The Technology sector provides strong evidence for shadow trading in 5.84%-12.23% of ETFs, with suspected ETFs showing on average by 0.0586 higher abnormal volume.

Table 6. Abnormal volume analysis by industry

	Bootstraps			**Regression**		
	Random days	**Random ETF**	**Random both**	**Random both**	**Sample size**	
Industry	**Estimated proportion**	**Estimated proportion**	**Estimated proportion**	**Estimated effect**	**ETFs**	**Deals**
Health Care	0.5287 (0.1828)	0.549 ** (0.0225)	0.5582 *** (0.0066)	0.0439 *** (0.0022)	541	73
Real Estate	0.5797 (0.1874)	0.6087 * (0.0706)	0.6232 ** (0.0398)	0.0825 (0.1024)	69	9
Consumer Discretionary	0.5598 * (0.0671)	0.5214 (0.5145)	0.5556 * (0.0892)	0.0089 (0.6427)	234	35
Industrials	0.5221 (0.3358)	0.5411 * (0.0735)	0.5411 * (0.0735)	0.0204 (0.1760)	475	53
Technology	0.5584 *** (0.0062)	0.6223 *** (0.0000)	0.5876 *** (0.0000)	0.0586 *** (0.0000)	548	63
Telecommunications	0.5227 (0.7669)	0.5455 (0.5526)	0.5000 (1.0000)	0.0263 (0.6055)	44	13
Financials	0.4754 (0.3608)	0.487 (0.6287)	0.4957 (0.8720)	-0.022 (0.1592)	345	37
Energy	0.5785 ** (0.0143)	0.5207 (0.5215)	0.6157 *** (0.0003)	0.0362 * (0.0688)	242	26
Basic Materials	0.5592 (0.1449)	0.5526 (0.1953)	0.5724 * (0.0743)	-0.0396 (0.1988)	152	18
Utilities	0.6889 *** (0.0096)	0.6444 * (0.0515)	0.7111 *** (0.0035)	0.0821 (0.3505)	45	6
Consumer Staples	0.5385 (0.6371)	0.4615 (0.6371)	0.4872 (0.8752)	-0.1273 (0.1703)	39	8

*Note: *p<0.10; **p<0.05; ***p<0.01*

Note. The table presents the abnormal volume analysis by industry, as described in Section 5.4. Suspected ETFs are chosen based on the filters described in Section 5.2. The process of the bootstrap sample creation – Random days, Random ETF, Random both – is described in Methodology Section 4.4. The Regression column shows the results of the *Related* variable from linear regression described in Equation 7 (we do not include ETF fixed effects as in each industry sample we observe ETFs of this industry only in a treated (suspected) state). p-value is reported in brackets under the coefficient.

In the other industries we find mixed results. In Industrials sector we observe statistically significant abnormality in two out of three bootstrap specifications, but the result is not reflected in the regression. Therefore, we conclude that the existing evidence showing shadow trading is not sufficient.

Next, two out of three bootstrap specifications show that 7.85%-11.57% Energy ETFs are shadow traded with the average abnormal volume being higher by 0.0362. We see no evidence of shadow trading in the Basic Materials industry. There is another industry which at first appears suspicious in the test results. Namely, the Utilities sector shows high abnormal volume in bootstrap specifications and no statistical significance in the regression analysis. We should, however, note that only 6 deals from the Utilities industry are represented in the suspected sample, and we consider the number too low to draw conclusions about shadow trading activity.

After finding the industries with higher levels of shadow trading, we now turn to the question of why exactly these industries provide a better environment for shadow trading. We study the Health Care, Energy, and Technology sectors in more detail. First, we find that there are more ETFs tracking these industries than any other industry. 132 ETFs in our full sample track the Technology sector, 75 track Energy and 58 track Health Care, while the average ETF per sector is 44. Therefore, we could expect more shadow trading due to the availability of ETFs to shadow trade. With a higher diversification among the sector ETFs, there is a higher probability for the insider to find an ETF which would fit their shadow trading strategy.

Second, we turn to the nature of companies in these industries. Previous research reveals that insider trading is more likely when the value of information is high. Rahman, Kabir and Oliver (2021) note that firms with a higher level of trade secrecy and higher R&D levels are more often insider traded due to the increased information asymmetry between insiders and investors. Naturally, we should expect that modern Health Care and Technology companies are among the most innovative and information asymmetric companies in our sample (OECD, 2017), therefore offering highly lucrative earning opportunities for insiders. Further, we find that these industries have some of the most valuable M&A deals in the whole sample: in the Health Care sector the average target return around the M&A announcement date is 28.6%, and in Technology sector it is 23.8%, compared to the sample average of 20%. Combined with the wide array of sector specific ETFs on the market, we can see an environment which is well fit for shadow trading.

As for the Energy sector, we propose a different explanation. The industry is highly concentrated due to the cost structure, and few M&A deals occur within it. At the same time, Electric Power Generation, Transmission and Distribution, as well as Oil and Gas Extraction are among the most regulated industries in the U.S. (Lellis, 2022). We should thus expect that this industry experiences higher regulatory attention per each deal, motivating insiders to shadow trade ETFs rather than trade the target stocks.

5.5. Monetary estimate of shadow trading

To provide a tangible measure of shadow trading in ETFs, we calculate abnormal volume in USD as in Equation 2 for each ETF-day in our sample. We then perform an identical bootstrapping procedure as for the shadow trading proxies used in previous tests and obtain a distribution of abnormal dollar volume for the suspected sample and three placebo samples. Total shadow trading dollar volume is in general calculated as:

$$ShadowTrading = \sum_{i=1}^{i}\sum_{x=1}^{x}\sum_{t=1}^{t} Suspected_{i,x,t} - \frac{\sum_{k=1}^{k}\sum_{x=1}^{x}\sum_{m=1}^{m} Placebo_{k,x,m}}{1000} \quad (8)$$

where $Suspected_{i,x,t}$ is stock-ETF pair i, x suspected on day t, and $Placebo_{k,x,m}$ is ETF k paired with the same stock x on a day m. Put simply, we sum all the abnormal volumes in the suspected sample and subtract one thousandth of the sum of abnormal volumes in one of the three placebo samples. The sum in the placebo sample is divided by one thousand because for each suspected ETF-stock-day we sample one thousand placebo ETF-days, which results in thousand times more observations in the placebo samples. Also note that for random day and random ETF approaches ETF and time dimensions are respectively irrelevant.

An important difference between relative abnormal volume and absolute abnormal volume is that the latter is much more prone to outliers – extremely large volumes that appear because of high price, high volume, or high abnormal volume. Therefore, in Table 7 we provide three more estimates, trimming the abnormal dollar volume in ETF-day universe at the 1st and 99th percentile, 5th and 95th percentile or 10th and 90th percentile. Outlier adjustment in the ETF-day universe ensures that both suspected and placebo samples are drawn from the same data.

We find that all estimates are positive and range from \$163m to \$68,137m. Except for the random day approach, untrimmed estimates are several orders of magnitude larger than the trimmed estimates, confirming our concerns about the impact of large abnormal dollar volumes. We also note that all three samples converge at an estimate of around \$2.75bn when we trim abnormal dollar volumes at 5th and 95th percentile.

Table 7. Total sample period shadow trading estimates, USD million

Trimming percentile	Random day	Random ETF	Random both
No trimming	6112	48588	68137
1 and 99	8808	13712	15337
5 and 95	2954	2410	2890
10 and 90	638	163	262

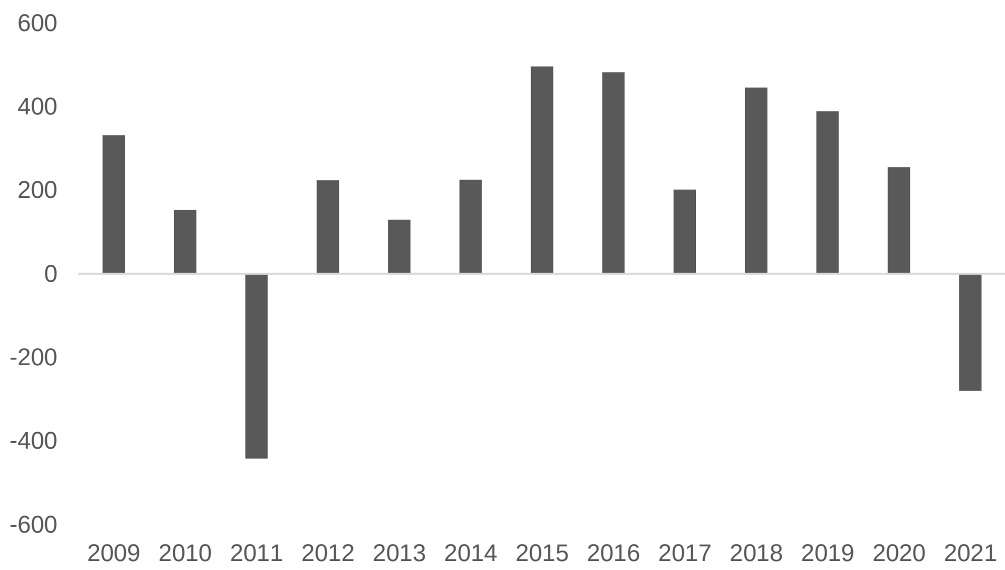
Note. This table presents the abnormal dollar volume estimates according to various trimming percentiles

As the estimated range is very broad, we perform a back-of-the-envelope calculation using one of the most popular ETFs in our suspected sample, IGV (iShares Expanded Tech-Software Sector ETF), satisfying the conditions of our screens 52 times. As of April 3, 2022, its 50-day average trading volume is 678,264 shares and the share price is \$345 according to Datastream. Based on our findings for the main specification (Table 2), 3.8% to 6% of 2,734 suspected ETF-deals are shadow traded, and suspected ETFs have on average 0.023 higher abnormal volume. As shadow trading cases are not disclosed, the exact volume impact of these cases is not known, but we can assume for the calculation that it ranges between 0.025 and 0.05. Given the assumptions, shadow trading may range from $678{,}264\ shares * \$345 * 5\ days * 0.025 * 3.8\% * 2734 = \$3bn$ to $678{,}264\ shares * \$345 * 5\ days * 0.05 * 6\% * 2734 = \$9.6bn$. A similar calculation for VIS (Vanguard Industrials ETF), suspected 50 times, results in a range from \$360m to \$1.1bn; for VHT (Vanguard Health Care ETF), suspected 47 times, it results in estimates between \$800m and \$2.5bn.

While the back-of-the-envelope calculation does not confirm a particular estimate, it points to ranges similar to the empirically obtained bounds when trimming is applied. Our baseline estimate is thus \$2.75bn, an average of the three bootstrap approaches when trimming at 5 and 95%, with a minimum shadow trading volume at around \$163m and maximum at \$15.37bn. We find untrimmed estimates using random ETF and random both approaches hardly justified.

We also perform the calculation of abnormal volume splitting both the suspected and the placebo samples by years and industries. In line with our findings in the previous sections, we find mixed evidence of shadow trading during the first three years of our study period, mainly due the

Figure 1. Shadow Trading Estimates by Years (USD Million)



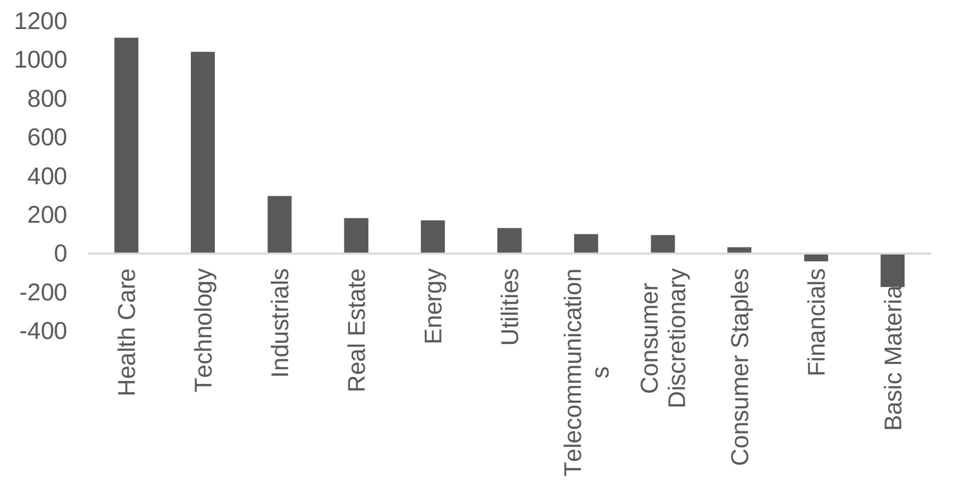
Note. The estimates are obtained using random day approach, trimming abnormal dollar volumes at the 5th and 95th percentiles.

negative estimate in 2011 (Figure 1).[4] For the next 9 years, shadow trading is consistently estimated at above $100m, reaching peaks of close to $500m/year by 2015-2016. Since then, the abnormal volumes have been generally decreasing and even turned negative in 2021.

Given that the first shadow trading court case was brought in August 2021 (SEC, 2021), a tempting argument for the negative volume is that shadow trading activity in ETFs reduced due to increased perceived costs. Unfortunately, our suspected sample contains only 3 deals after July 2021, which does not enable any opportunities for statistical inference. We find, however, that the negative abnormal volume in 2021 does not come mainly from these deals, with most months in 2021 recording negative estimates. This suggests that the negative volume in 2021 is either a delayed result of the Insider Trading Prohibition Act proposed in 2019 or a coincidence. It ultimately remains a question of time to see if the Panuwat's case has had any effect on the shadow trading in ETFs.

Finally, assessing shadow trading by industries, our earlier findings are reconfirmed in dollar terms. There are two industries that are responsible for around 73% of shadow trading volume – Health Care and Technology. Their share of abnormal volume is much larger than their share of the suspected sample, around 39%, which is explained by them being both large constituents of the suspected sample and prone to shadow trading.

Figure 2. Shadow Trading Estimates by Industries (USD Million)



Note. The estimates are obtained using random day approach, trimming abnormal dollar volumes at the 5th and 95th percentiles.

[4] In Figure 1, year 2011 looks suspicious, but closer examination of the deals in 2011 does not reveal any data issues. Also note that in absolute terms the estimate for 2011 is comparable to the estimates for 2016, 2018, and 2019.

5.6. Robustness tests

5.6.1. Relaxing the assumptions

Although we show that little to no shadow trading activity takes place outside the suspected sample, this placebo test does not reveal how sensitive the subsample is to each of the assumptions we make about where shadow trading is expected. Appendices 6 and 7 present a number of tests for abnormal volume and run-up. We omit imbalance metrics for brevity as they are to a large extent run-up-determined.[5]

First, we remove each of the screens one-by-one and find little impact on the results for abnormal volume. This presents strong evidence for shadow trading and our Hypothesis 3. Price run-up is almost never confirmed by all three bootstrap approaches but remains confirmed by the regression. Importantly, when we do not use target CAR screen, the effect associated with the suspected sample is around two times smaller than in the main specification, or around -0.05 pp. This is in line with one of the explanations we provide for negative run-up in our main tests in the subsection 5.2, namely that insiders consider shadow trading only when the anticipated target return is large.

We then remove assumptions in groups, for example, we ignore all screens that are related to market capitalization or all screens that are related to target company. Results still largely hold for the abnormal volume, although the economic effect is reduced gradually from 0.02 reported in the main tests to around 0.01. Importantly, shadow trading is revealed also by using the correlation as the only screen. If we use all screens except correlation, we are also able to confirm abnormal volume, suggesting the complementarity of our filters.

The same procedure for run-up, however, largely removes evidence of the price impact of insiders' shadow trades. When we abandon both filters related to the target company, target CAR and target market capitalization, we find almost no evidence of price run-up, again, in line with what we find in our modified filter tests in the subsection 5.2.

It should be noted that the main purpose of these robustness checks is not to analyse the impact of individual variables on our results. While we may get some indications of these impacts, our goal when relaxing assumptions is to show that our results are not a result of a mechanical setup that spuriously results in evidence consistent with shadow trading. We confirm this, and we also document that abnormal volume, our main variable of interest, is much more robust to screen modifications than

[5] The results are available upon request.

the run-up. We interpret vulnerability of run-up to model modifications as an indication of the liquidity argument – as ETFs are highly liquid assets, insiders' trades leave little price impact.

5.6.2. Alternative proxy periods

The next assumption we test is the shadow trading proxy period. Throughout the thesis, we have assumed that we are likely to see shadow trading 5 days before the M&A announcement. Of course, we cannot predict how early insiders become aware of the M&A and decide to trade. Therefore, we extend our proxy periods to 7 and 10 days before the M&A announcement and check if our main results change. We find no changes in our results: abnormal volume is statistically significant across all tests, while price run-up and imbalance remain negative. The results are reported in Appendix 8.

5.6.3. Alternative correlation periods

Due to the unavailability of ETF holdings data, throughout the study we have used rolling correlations as one the main filters to detect stock-ETF relatedness. We recognize that the results may be sensitive to the chosen calculation period, thus stress-test our main results by introducing different correlation periods. We substitute 180-day daily correlation to 180-day weekly correlation, as well as 30-day, 90-day, and 360-day daily correlation. The results are reported in Appendix 9. Our conclusions remain unchanged. We find evidence for abnormal volume across tests, and mixed evidence for price run-up and order imbalance. Filtering suspected ETFs based on 360-day daily correlation results in statistically significant negative run-up across all tests, while for other correlation periods there is no clear evidence for either positive or negative price run-up and order imbalance.

As an alternative correlation robustness test, we choose based on correlation as of a different date. In our main tests, we use correlation 1 day prior the M&A announcement as a filter. As a robustness test, we try to filter based on 180-day correlation as of 2, 3, 5, and 10 days prior the M&A announcement. Abnormal volume is significant across all tests, while price run-up and order imbalance lose significance in most bootstrap tests.

We conclude that stress-testing our main stock-ETF relatedness filter results in more evidence for abnormal volume, and weak evidence for ETF price run-up or order imbalance.

5.7. Limitations

We see at least three areas of improvement for the testing procedure we have implemented in our research. First, our stock-ETF relatedness measure, 180-day rolling correlation, is not a perfect substitute for the share of ETF assets invested in a stock. While holdings data is not available from Datastream, such data would benefit the precision of the findings by enabling us to isolate and analyse the impact of larger portfolio weight on shadow trading decisions. Second, intra-day data would significantly improve our order imbalance proxies by removing their direct link between daily returns. Third, expansion of the sample period to the pre-2009 M&As would enable us to test if the lack of adoption argument for years 2009-2013 holds. If less evidence of shadow trading could be found in the years prior to 2009, this would serve as additional evidence that insiders indeed did not consider these vehicles when they were not widely adopted.

6. Conclusions

The purpose of this research was to analyse how widespread shadow trading is in ETFs. Based on the findings of the seminal work on shadow trading by Mehta et al. (2020), we hypothesise that prior to merger and acquisition announcements, the potential for insider trading in ETFs is limited due to mixed effects these deals have on target industries. At the same time, ETFs are very liquid and accessible financial instruments, and high liquidity has been shown to be attractive to insiders. Taking into consideration these somewhat opposing effects, we construct a sample of target-ETF pairs that are expected to be especially prone to shadow trading during M&A announcements from 2009 to 2021 and find that shadow trading happens in around 3-6% of suspected ETFs. We estimate this activity at around \$163m to \$15.37bn, with a central estimate of around \$2.75bn. We also show that shadow trading happens disproportionately in the Health Care and Technology industries, as well as shows peaks in 2015 and 2016. We explain the former with broader ETF availability and high levels of R&D activity and trade secrecy, and the latter with early adoption period, learning effects, legislative changes (Insider Trading Prohibition Act) and, perhaps most importantly, liquidity of ETFs growing faster than shadow trading.

Throughout our tests, we find that abnormal volume is the most consistent proxy of irregular activity in ETFs. This finding is in line with our hypothesis that the volume created by insiders does not materially impact prices due to high ETF liquidity. While our subsample of suspected deals is associated with negative price run-ups, we estimate they are economically small, around -0.01%, and are not confirmed by some of our robustness checks.

Our findings contribute to the very novel, and, with the first formal accusations by SEC in August 2021, increasingly relevant research area of shadow trading. While Mehta et al. (2020) show that insiders are not limited to the source firms when trading on the material non-public information and can use economically linked stocks, we demonstrate that the potential pool of insider trading vehicles is even broader and includes ETFs. Together with Mehta et al. (2020), this suggests that the amount of insider trading may be underestimated if only classic (direct) insider trading is considered. This then has potential legal implications as it appears that related securities provide a loophole to insider trading provisions.

We also see the thesis as a valuable addition to the research on ETF use cases. By documenting insider trading activity in ETFs, we show that ETFs are not purely passive vehicles that “free-ride” on the price discovery that happens in the underlying stocks, but that they too can potentially contribute to price discovery.

Our suggestions for future research include (1) a use of ETF holdings and intra-day trading data for improved precision of stock-ETF relatedness and order imbalance measures, and (2) a longer sample period to include years when ETFs were not widespread to verify the early adoption hypothesis. In addition, an inclusion of more exotic ETF types, in particular leveraged ETFs, may be useful at further explaining ETF shadow traders’ behaviour.

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8. Appendices

Appendix 1: Return filters

First, we adjust data to account for data errors in Datastream following Ince and Porter (2006). We set the daily stock and ETF returns of both days d and $d - 1$ to be missing if $R_{i,d}R_{i,d-1} \leq 50\%$, where $R_{i,d}$ is the gross return of stock/ETF i on day d, and at least one of the two returns is 200% or greater. To avoid rounding errors, the daily return is set to be missing if the return index (RI) in Datastream for either the previous day or the current day is smaller than 0.01.

Then, we follow Kumar et al. (2021) and exclude non-trading days. They define non-trading days as days on which if more than 90% of stocks listed on a given exchange have zero returns. We do this separately for stocks and ETFs and assume all stocks and all ETFs trade on the same exchange (non-trading days should be the same for all US exchanges).

Finally, following Kumar et al. (2021), a stock/ETF is excluded if the number of zero-return days is more than 80% in a given month.

Appendix 2. Summary statistics

This appendix reports summary statistics for our dataset. The data is retrieved from Refinitiv Eikon and Datastream and the data cleaning process is described in the Data section and Appendix 1.

Panel A: M&A deals summarized across various categories

M&A deals	**N = 2,734**	**M&A deals**	**N = 2,734**
Acquiror Nation		**Industry**	
United States	2,340 (86%)	Financials	695 (25%)
Canada	97 (3.6%)	Health Care	395 (14%)
United Kingdom	54 (2.0%)	Technology	387 (14%)
Japan	38 (1.4%)	Consumer Discretionary	345 (13%)
France	27 (1.0%)	Industrials	278 (10%)
Germany	20 (0.7%)	Energy	195 (7.1%)
China (Mainland)	17 (0.6%)	Telecommunications	116 (4.2%)
Switzerland	17 (0.6%)	Consumer Staples	98 (3.6%)
Bermuda	13 (0.5%)	Basic Materials	87 (3.2%)
Netherlands	13 (0.5%)	Real Estate	84 (3.1%)
Other	98 (3.6%)	Utilities	54 (2.0%)

M&A deals	**N = 2,734**	**M&A deals**	**N = 2,734**
Deal form		**Rumored deal**	
Merger	2,564 (94%)	Yes	303 (11%)
Acquisition Of Majority Interest	150 (5.5%)	No	2,431 (89%)
Acquisition Of Assets	14 (0.5%)		
Acquisition	4 (0.1%)		
Acquisition Of Remaining Interest	2 (<0.1%)		

Panel B: Characteristics of M&A deals

M&A deal descriptive statistics

Statistic	**N**	**Mean**	**St. Dev.**	**Min**	**Max**
Share acquired	2,734	97.49	8.86	50	100
Target market capitalization (million USD)	2,692	1,491.13	4,330.68	0.01	66,912.25
Target CAR (-1;1)	2,734	0.20	0.37	-3.51	5.66

Panel C: Characteristics of ETFs related to M&A targets

Related ETF descriptive statistics

Statistic	N	Mean	St. Dev.	Min	Max
ETF market capitalization (millions USD)	69,301	1,111.29	3,244.78	0.46	53,487.08
ETF CAR (-1;1)	69,630	0.00	0.01	-0.31	0.17
180-day rolling correlation	69,640	0.29	0.23	-0.27	0.95
ETF abnormal volume	68,556	-0.05	0.31	-0.99	1.00
ETF run-up	69,640	-0.0004	0.02	-0.26	0.26
ETF simple imbalance	67,701	0.08	0.53	-1.00	1.00
ETF BVC imbalance	67,701	0.02	0.24	-0.99	0.96

Panel D: Median target and ETF market capitalization

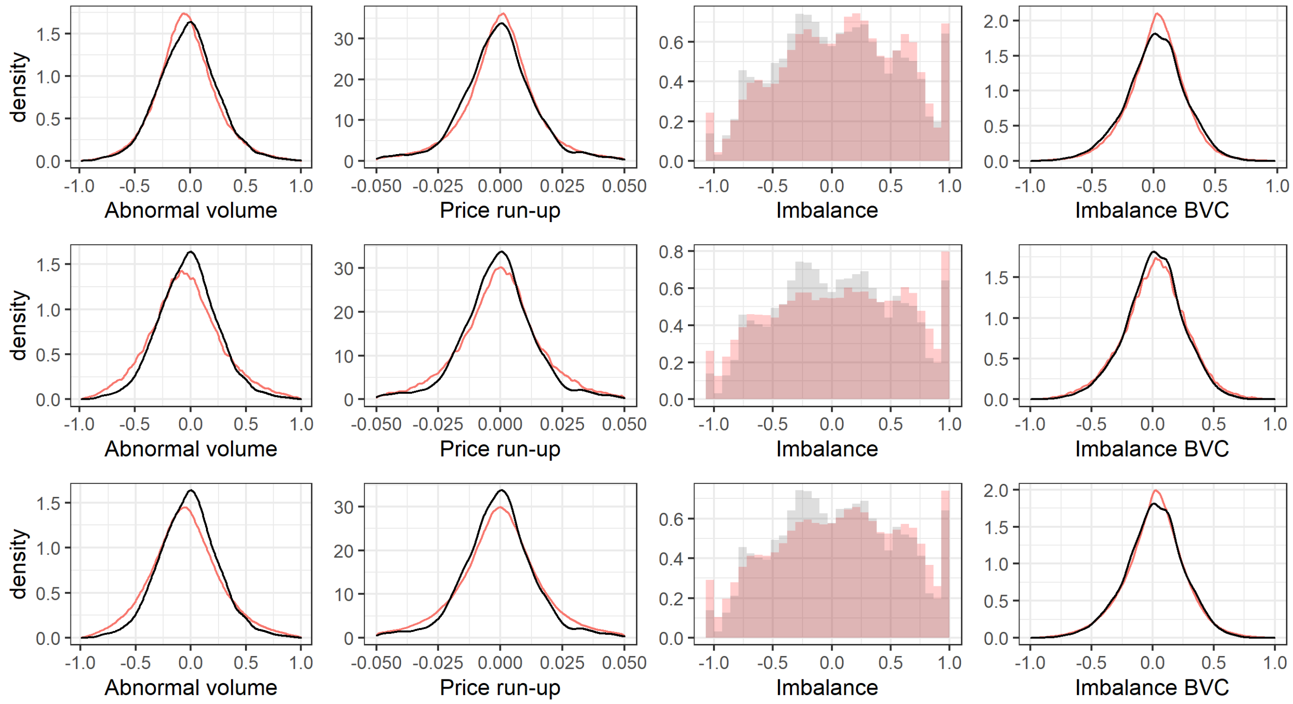
Year	Median target market capitalization (millions USD)	Median target CAR	Median ETF market capitalization (millions USD)
2009	36.72	16.89%	97.91
2010	155.07	22.53%	123.01
2011	220.94	23.61%	142.00
2012	131.67	25.63%	110.47
2013	216.66	19.62%	143.20
2014	277.29	18.88%	205.37
2015	355.48	16.13%	211.28
2016	494.19	17.28%	150.74
2017	374.00	14.09%	192.48
2018	777.00	13.71%	226.17
2019	430.23	17.26%	159.46
2020	402.83	16.77%	192.56
2021	560.12	17.43%	323.39

Appendix 3. Variables used in the research

This table describes the variables used in the research

Variable	Description	Calculated
Rolling correlation	Continuously calculated correlation of returns with a fixed window (180 day for the main tests)	Yes
Market capitalization	Market value of equity, millions USD	No
CAR (-1;1)	(First lead of price / First lag of price - 1) less the same for market (Russell 3000 index)	Yes
ETF abnormal volume	See section 4.2.1.	Yes
ETF run-up	See section 4.2.2.	Yes
ETF simple imbalance	See section 4.2.3.	Yes
ETF BVC imbalance	See section 4.2.3.	Yes
Price-to-Book Ratio (PTBV)	Market value of assets / Book value of assets	No
Share turnover	Monthly share volume / Average number of shares in a month	Yes

Appendix 4. Distribution of proxies in the suspected sample



These histograms show distributions of proxies in the M&A subsample described in Section 5.2. Black line represents M&A subsample and red line represents random (bootstrap) sample. First row of histograms corresponds to random days approach, second row corresponds to random ETF approach, and third row corresponds to random both (day and ETF) approach.

Appendix 5. Abnormal volume and run-up analysis after filter modification

		Bootstrap			Regression	Sample size	
		Random days	Random ETF	Random both	Random both		
	Variable	Estimated proportion	Estimated proportion	Estimated proportion	Estimated effect	ETFs	Deals
Target CAR							
Below median	Abnormal volume	0.5075 (0.4288)	0.5458 *** (0.0000)	0.5408 *** (0.0000)	0.009 (0.1651)	2816	351
Below median	Run-up	0.4929 (0.4511)	0.5174 * (0.0648)	0.4979 (0.8211)	0.0003 (0.5597)	2816	351
Negative	Run-up	0.5231 (0.3365)	0.6042 *** (0.0000)	0.5231 (0.3365)	0.0020 (0.1021)	432	50
Negative	Abnormal volume	0.5301 (0.2113)	0.5556 ** (0.0207)	0.5440 * (0.0675)	0.0053 (0.7706)	432	50
Positive	Abnormal volume	0.5246 *** (0.0004)	0.5553 *** (0.0000)	0.5516 *** (0.0000)	0.0195 *** (0.0000)	5141	645
Positive	Run-up	0.4758 *** (0.0005)	0.4929 (0.3087)	0.4900 (0.1509)	-0.0008 ** (0.0182)	5141	645
Target size							
Small	Abnormal volume	0.4542 ** (0.0235)	0.5065 (0.7467)	0.4641 * (0.0753)	0.0058 (0.6928)	612	90
Small	Run-up	0.5016 (0.9356)	0.5049 (0.8086)	0.5065 (0.7467)	0.00001 (0.9871)	612	90
ETF CAR							
Positive	Abnormal volume	0.5279 ** (0.0243)	0.5395 *** (0.0014)	0.5456 *** (0.0002)	0.0315 *** (0.0002)	1633	306
Negative	Abnormal volume	0.5631 *** (0.0000)	0.5731 *** (0.0000)	0.5767 *** (0.0000)	0.0204 ** (0.0423)	1101	244
Positive	Run-up	0.4611 *** (0.0017)	0.4807 (0.1190)	0.4825 (0.1584)	-0.0015 ** (0.0105)	1633	306
Negative	Run-up	0.4641 ** (0.0172)	0.5077 (0.6086)	0.4759 (0.1102)	-0.0012 * (0.0995)	1101	244

Note. The table presents the analysis described in Section 5.2 when modified filters are applied. The process of the bootstrap sample creation – Random days, Random ETF, Random both – is described in Methodology Section 4.4. The Regression column shows the results of the Related variable from linear regression described in Equation 7. p-value is reported in brackets under the coefficient. Order imbalance tests are excluded for brevity and are available upon request.

Appendix 6. Abnormal volume tests with relaxed assumptions

	Bootstraps			Regression	Sample size	
	Random days	Random ETF	Random both	Random both		
Changed assumption	**Estimated proportion**	**Estimated proportion**	**Estimated proportion**	**Estimated effect**	**ETFs**	**Deals**
Filtering on median correlation value, not 75th percentile	0.5225 *** (0.0044)	0.5574 *** (0.0000)	0.5532 *** (0.0000)	0.0191 *** (3e-04)	4006	447
Removing target CAR filter	0.5234 *** (0.0005)	0.5528 *** (0.0000)	0.551 *** (0.0000)	0.0166 *** (0.0003)	5573	695
Removing target market cap. filter	0.5256 *** (0.0030)	0.5471 *** (0.0000)	0.5423 *** (0.0000)	0.0221 *** (0.0005)	3356	432
Removing ETF market cap. filter	0.5333 *** (0.0003)	0.5427 *** (0.0000)	0.551 *** (0.0000)	0.0181 *** (0.0056)	2889	346
Do not filter out rumored deals	0.5248 *** (0.0057)	0.5437 *** (0.0000)	0.5508 *** (0.0000)	0.0233 *** (0.0001)	3110	386
Choose TOP-10 ETFs randomly, not based on market cap.	0.5331 *** (0.0005)	0.5382 *** (0.0001)	0.5437 *** (0.0000)	0.0235 *** (0.0002)	2734	341
Choose TOP-5 ETFs based on market cap.	0.5291 ** (0.0229)	0.5643 *** (0.0000)	0.5643 *** (0.0000)	0.0220 *** (0.0056)	1531	341
Remove all ETF market cap. filters	0.5276 *** (0.0031)	0.5203 ** (0.0301)	0.5262 *** (0.0050)	0.0166 *** (0.0072)	2889	346
Remove all target-associated filters (target CAR, market cap.)	0.5192 *** (0.0016)	0.5491 *** (0.0000)	0.5476 *** (0.0000)	0.0196 *** (0.0000)	6755	856
Remove all ETF and target market cap. filters	0.5188 ** (0.0261)	0.5145 * (0.0856)	0.5213 ** (0.0114)	0.0103 * (0.0848)	3557	441
Filter based on correlation only	0.5098 * (0.0685)	0.5172 *** (0.0014)	0.5148 *** (0.0060)	0.0108 *** (0.0034)	8640	1066
Remove correlation filter	0.524 *** (0.0006)	0.5532 *** (0.0000)	0.5542 *** (0.0000)	0.0124 *** (0.0075)	5041	536

Note: *p<0.10; **p<0.05; ***p<0.01

Note. The table presents the abnormal volume analysis if some the assumptions of Section 5.2 are relaxed. The process of the bootstrap sample creation – Random days, Random ETF, Random both – is described in Methodology Section 4.4. The Regression column shows the results of the *Related* variable from the linear regression described in Equation 7. p-value is reported in brackets under the coefficient.

Appendix 7. Run-up tests with relaxed assumptions

	Bootstraps			Regression	Sample size	
	Random days	Random ETF	Random both	Random both		
Changed assumption	Estimated proportion	Estimated proportion	Estimated proportion	Estimated effect	ETFs	Deals
Filtering on median correlation value, not 75th percentile	0.474 *** (0.0010)	0.4943 (0.4674)	0.4963 (0.6356)	-0.0012 *** (0.0000)	4006	447
Removing target CAR filter	0.4807 *** (0.0040)	0.5022 (0.7377)	0.4902 (0.1443)	-0.0005 * (0.0758)	5573	695
Removing target market cap. filter	0.4648 *** (0.0000)	0.4881 (0.1673)	0.4878 (0.1570)	-0.0015 *** (0.0000)	3356	432
Removing ETF market cap. filter	0.459 *** (0.0000)	0.4826 * (0.0624)	0.4823 * (0.0573)	-0.0014 *** (0.0000)	2889	346
Do not filter out rumored deals	0.4662 *** (0.0002)	0.4945 (0.5422)	0.4945 (0.5422)	-0.0013 *** (0.0000)	3110	386
Choose TOP-10 ETFs randomly, not based on market cap.	0.4636 *** (0.0001)	0.4885 (0.2282)	0.483 * (0.0752)	-0.0016 *** (0.0000)	2734	341
Choose TOP-5 ETFs based on market cap.	0.454 *** (0.0003)	0.4925 (0.5568)	0.4847 (0.2298)	-0.0010 ** (0.0498)	1531	341
Remove all ETF market cap. filters	0.4615 *** (0.0000)	0.4829 * (0.0669)	0.4843 * (0.0924)	-0.0019 *** (0.0000)	2889	346
Remove all target-associated filters (target CAR, market cap.)	0.4909 (0.1345)	0.5106 * (0.0819)	0.4962 (0.5350)	0.0005 (0.9679)	6755	856
Remove all ETF and target market cap. filters	0.4812 ** (0.0261)	0.4974 (0.7616)	0.5026 (0.7616)	-0.0014 *** (0.0000)	3557	441
Filter based on correlation only	0.5027 (0.6125)	0.5217 *** (0.0001)	0.5164 *** (0.0023)	0.0001 (0.6164)	8640	1066
Remove correlation filter	0.4772 *** (0.0012)	0.5008 (0.9102)	0.4982 (0.7998)	-0.0009 *** (0.0018)	5041	536

Note: *p<0.10; **p<0.05; ***p<0.01

Note. The table presents the run-up analysis if some the assumptions of Section 5.2 are relaxed. The process of the bootstrap sample creation – Random days, Random ETF, Random both – is described in Methodology Section 4.4. The Regression column shows the results of the *Related* variable from linear regression described in Equation 7. p-value is reported in brackets under the coefficient.

Appendix 8. Robustness test with different proxy periods

Panel A. Abnormal volume tests

	Bootstraps			Regression		
	Random days	Random ETF	Random both	Random both	Sample size	
Proxy period	Estimated proportion	Estimated proportion	Estimated proportion	Estimated effect	ETFs	Deals
Seven days	0.5368 *** (0.0001)	0.5458 *** (0.0000)	0.5439 *** (0.0000)	0.0232 *** (0.0001)	2734	341
Ten days	0.5248 ** (0.0105)	0.5316 *** (0.0011)	0.5282 *** (0.0036)	0.0150 *** (0.0037)	2734	341

Note: *p<0.10; **p<0.05; ***p<0.01

Panel B. Run-up tests

	Bootstraps			Regression		
	Random days	Random ETF	Random both	Random both	Sample size	
Proxy period	Estimated proportion	Estimated proportion	Estimated proportion	Estimated effect	ETFs	Deals
Seven days	0.4354 *** (0.0000)	0.464 *** (0.0002)	0.4598 *** (0.0000)	-0.0015 *** (0.0011)	2734	341
Ten days	0.4275 *** (0.0000)	0.4654 *** (0.0004)	0.4515 *** (0.0000)	-0.0034 *** (0.0000)	2734	341

Note: *p<0.10; **p<0.05; ***p<0.01

Note. The table presents the robustness tests for the analysis described in Section 5.2 if the proxy calculation period is changed. The process of the bootstrap sample creation – Random days, Random ETF, Random both – is described in Methodology Section 4.4. The Regression column shows the results of the *Related* variable from linear regression described in Equation 7. p-value is reported in brackets under the coefficient. Order imbalance tests are excluded for brevity and are available upon request.

Appendix 9. Robustness test with different correlation assumptions

Panel A. Different correlation periods

		Bootstrap			Regression	Sample size	
		Random days	Random ETF	Random both	Random both		
Correlation period	Variable	Estimated proportion	Estimated proportion	Estimated proportion	Estimated effect	ETFs	Deals
30-day	Abnormal volume	0.5282 *** (0.0022)	0.5387 *** (0.0000)	0.5465 *** (0.0000)	0.0174 *** (0.0044)	3022	379
90-day	Abnormal volume	0.5276 *** (0.0035)	0.5398 *** (0.0000)	0.5456 *** (0.0000)	0.0130 ** (0.0256)	2944	347
360-day	Abnormal volume	0.5149 (0.1350)	0.5324 *** (0.0011)	0.5284 *** (0.0043)	0.0152 ** (0.0430)	2517	315
180-day weekly correlation	Abnormal volume	0.5256 ** (0.0144)	0.5282 *** (0.0070)	0.5400 *** (0.0001)	0.0142 ** (0.0300)	2289	254
30-day	Run-up	0.4694 *** (0.0009)	0.498 (0.8250)	0.5010 (0.9120)	0.0000 (0.1286)	3022	379
90-day	Run-up	0.4753 *** (0.0089)	0.5039 (0.6770)	0.5068 (0.4718)	-0.001 *** (0.0004)	2944	347
360-day	Run-up	0.4497 *** (0.0000)	0.4776 ** (0.0243)	0.476 ** (0.0158)	-0.001 *** (0.0000)	2517	315
180-day weekly correlation	Run-up	0.4766 ** (0.0253)	0.4919 (0.4394)	0.5011 (0.9168)	-0.001 *** (0.0035)	2289	254

Note: *p<0.10; **p<0.05; ***p<0.01

Panel A. Different correlation dates

		Bootstrap			Regression	Sample size	
		Random days	Random ETF	Random both	Random both		
Correlation date	Variable	Estimated proportion	Estimated proportion	Estimated proportion	Estimated effect	ETFs	Deals
2 days prior M&A announcement	Abnormal volume	0.5374 *** (0.0001)	0.5501 *** (0.0000)	0.5550 *** (0.0000)	0.0247 *** (0.0001)	2740	343
3 days prior M&A announcement	Abnormal volume	0.5423 *** (0.0000)	0.5558 *** (0.0000)	0.562 *** (0.0000)	0.0268 *** (0.0000)	2733	340
5 days prior M&A announcement	Abnormal volume	0.5406 *** (0.0000)	0.5582 *** (0.0000)	0.5615 *** (0.0000)	0.0246 *** (0.0001)	2723	342
10 days prior M&A announcement	Abnormal volume	0.5377 *** (0.0001)	0.5514 *** (0.0000)	0.5581 *** (0.0000)	0.0219 *** (0.0005)	2693	336
2 days prior	Run-up	0.463 ***	0.4921	0.4854	-0.0011 ***	2740	343

Correlation date	Variable	Bootstrap: Random days — Estimated proportion	Bootstrap: Random ETF — Estimated proportion	Bootstrap: Random both — Estimated proportion	Regression: Random both — Estimated effect	Sample size: ETFs	Sample size: Deals
M&A announcement		(0.0001)	(0.4168)	(0.1315)	(0.0034)		
3 days prior M&A announcement	Run-up	0.4665 *** (0.0005)	0.4896 (0.2757)	0.4896 (0.2757)	-0.0011 *** (0.0010)	2733	340
5 days prior M&A announcement	Run-up	0.4671 *** (0.0006)	0.4932 (0.4784)	0.4884 (0.2274)	-0.0013 *** (0.0000)	2723	342
10 days prior M&A announcement	Run-up	0.4705 *** (0.0022)	0.4961 (0.6858)	0.4928 (0.4524)	-0.0015 *** (0.0000)	2693	336

Note. The table presents the robustness tests for the analysis described in Section 5.2 if the correlation assumptions are changed. The process of the bootstrap sample creation – Random days, Random ETF, Random both – is described in Methodology Section 4.4. The Regression column shows the results of the *Related* variable from linear regression described in Equation 7. p-value is reported in brackets under the coefficient. Order imbalance tests are excluded for brevity and are available upon request.