Post-Earnings-Announcement Drift and Investor Sophistication

Employing buy-side, sell-side and inside proxies in a Swedish setting

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Abstract

This paper studies the post-earnings-announcement drift and its connection to investor sophistication in Sweden over a time period ranging from 2004 to 2013. Using a sample of 215 stocks, it is first hypothesized and shown that a portfolio long (short) in shares with positive (negative) earnings announcement returns yields economically and statistically significant cumulative abnormal returns over a 60-day holding period. Second, it is hypothesized that higher institutional ownership and analyst experience reduce the magnitude of the drift whereas insider trading is expected to lead to a faster drift realization. Yet, while there is some indicative evidence in favour of the latter prediction, no statistically significant relationship between post-earnings-announcement drift and investor sophistication is found.

Keywords: post-earnings-announcement drift, investor sophistication, institutional ownership, analyst experience, insider trading

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TABLE OF CONTENTS

1 INTRODUCTION 3
2 PREVIOUS RESEARCH AND HYPOTHESES 5
2.1 Background 5
2.2 Literature documenting the post-earnings-announcement drift 5
2.3 Literature explaining the post-earnings-announcement drift 7
2.4 Development of hypotheses 9
3 DATA 11
3.1 Sample selection 11
3.2 Data needed for documenting the post-earnings-announcement drift 12
3.3 Data needed for explaining the post-earnings-announcement drift 13
4 METHODOLOGY 15
4.1 Documenting the post-earnings-announcement drift 15
4.2 Using buy-side and sell-side proxies to explain post-earnings-announcement drift 21
4.3 Using the inside proxy to explain the post-earnings-announcement drift 24
5 EMPIRICAL RESULTS 28
5.1 Documenting the post-earnings-announcement drift 28
5.2 Using buy-side and sell-side proxies to explain post-earnings-announcement drift 33
5.3 Using the inside proxy to explain the post-earnings-announcement drift 36
6 DISCUSSION 41
7 CONCLUSION 45
REFERENCES 46
APPENDIX A – SAMPLE SELECTION 49
APPENDIX B – EARNINGS ANNOUNCEMENT SCREENING 50
APPENDIX C – ROBUSTNESS TESTS 52

1 INTRODUCTION

The efficient-market hypothesis postulates that financial markets are efficient – meaning that assets traded in these markets are correctly priced – with regard to the information available at each point in time, thus precluding investors from consistently earning abnormal risk-adjusted returns. More specifically, under weak-form efficiency, asset prices properly reflect all information contained in historical asset prices. Semi-strong-form efficiency implies that asset prices appropriately reflect all such information as well as all other publicly available information, whereas strong-form market efficiency posits that asset prices accurately reflect both public and private information, meaning that not even private information can be used to consistently earn abnormal returns.

However, numerous scholars have shown that an investor buying shares in companies whose announced earnings exceed expected earnings and selling (short) shares in companies whose announced earnings fall short of expected earnings could earn risk-adjusted abnormal returns (Ball and Brown, 1968; Foster et al., 1984; Bernard and Thomas, 1989). Thus, at odds with semi-strong-form market efficiency with regard to publicly available information, it seems that share prices update gradually rather than instantaneously to incorporate the information contained in earnings announcements, a phenomenon commonly labelled the post-earnings-announcement drift.

The phenomenon has been thoroughly documented over the last 45 years, but the research community – albeit having researched it extensively for almost half a century – has hitherto failed to provide "a rational, economic explanation for the drift" (Kothari 2001, p. 196). Among the explanations considered but generally deemed insufficient are (i) the possibility that the results are driven primarily by flaws in the methodologies used and (ii) the view that the drift is a compensation for some omitted risk factor. More promising attempts to illuminate the phenomenon are based on behavioural explanations relying on investors being irrational, for example by failing to understand the full implications of current earnings for future earnings, and market frictions such as limits to arbitrage.

In light of previous research on the topic, the aim of this paper is twofold. First, it aims to provide out-of-sample evidence of Setterberg's (2011) Swedish study, which covered the time period 1990-2005, by documenting the existence of the drift in a Swedish setting over a time period ranging from 2004 to 2013. Second, following some of the more successful attempts at explaining the drift in a US setting, it aims to test whether investor sophistication – proxied by buy-side, sell-side and inside variables – can explain the drift.

Given the persistence of the phenomenon in previous research, it is first hypothesized that the post-earnings-announcement drift can still be found in Swedish equity markets. This hypothesis is tested using an event-study approach where three portfolios – one good-news, one bad-news and one long-short portfolio – are formed two days after the earnings announcement on the basis of the earnings announcement return, defined as the share return on the day of the earnings announcement.

Consistent with the first hypothesis, the good-news and bad-news portfolios yield 60-day cumulative abnormal returns of a positive 1.77 per cent, statistically significant at the one per cent level, and a negative 1.44 per cent, statistically significant at the five per cent level, respectively. These numbers correspond to a 60-day long-short portfolio return in excess of three per cent, which is also significant at the one per cent level, or buy-and-hold abnormal returns of almost 15 per cent on an annualized basis, suggesting that the magnitude of the drift is both statistically and economically significant. In addition, the results are robust to several changes in the way the performance and surprise measures are calculated, lending further support to the conclusion that the drift is still present in the Swedish equity market.

Following research on some of the more promising attempts to explain the drift, it is also hypothesized that investor sophistication reduces the drift or causes a faster drift realization. More specifically, a higher proportion of institutional ownership and a higher degree of sell-side analyst experience, used as *buy-side* and *sell-side* proxies for investor sophistication, are predicted to reduce the drift. Similarly, extensive insider trading, used as an *inside* proxy for investor sophistication, is predicted to lead to a faster realization of the drift. To test these predictions, two different ordinary least squares regression specifications controlling for additional financial variables as well as time-quarter and firm fixed effects are used.

Contrary to the first prediction, no statistically significant relationship is found between the buy-side and sell-side proxies on the one hand and the post-earnings-announcement drift on the other. While this could be because, contrary to US findings, there is no such relationship in a Swedish setting, a more likely explanation is probably that there are some problems with the validity of the measures of institutional ownership and analyst experience. Conversely, in line with the prediction regarding insider trading, it seems that extensive insider buying following a positive surprise causes a faster drift realization. In addition, while not causing a faster drift realization, insider selling seems to lead to a more prolonged downward drift. Still, as these results benefit from limited or no statistical significance they are merely indications; the overall conclusion is that there is no statistically significant relationship between investor sophistication and the drift.

This paper contributes to the research on the post-earnings-announcement drift by providing out-of-sample evidence of the presence of the drift in a Swedish setting. Furthermore, it is the first paper taking a holistic approach to studying the connection between buy-side, sell-side and inside proxies for investor sophistication and the drift in a Swedish and, to the best of the authors' knowledge, European setting. Finally, by presenting indications (i) that there is no clear relationship between the drift and buy-side and sell-side proxies for investor sophistication and (ii) that extensive insider selling after negative surprises causes a prolonged downward drift, it suggests that there might be differences in drift behaviour across countries.

This paper proceeds as follows. Section 2 introduces some of the most influential literature on the post-earnings-announcement drift, with a particular focus on the literature relating to the hypotheses tested in this paper. Sections 3 and 4 describe the data and methodology used in this paper. Section 5 presents the empirical results from the documentation of the post-earnings-announcement drift as well as the testing of the investor sophistication hypothesis, whereas section 6 discusses these results. Section 7 concludes.

2 PREVIOUS RESEARCH AND HYPOTHESES

2.1 Background

Accounting research before the 1960s was almost exclusively normative or value-laden, revolving around what were the most appropriate accounting objectives (Kothari, 2001). Consequently, there was also disagreement on the optimal set of accounting policies, which "led to skepticism about the usefulness of accounting income reported in the financial statements" (Kothari 2001, p. 113). Indeed, Boulding (1962, p. 55), as cited by Lev and Ohlson (1982, p. 258), went so far as to name accounting "a known untruth".

However, Kothari (2001) describes three almost simultaneous developments in the 1950s and 1960s which paved the way for positive accounting and capital market research. First, the 1950s saw a general shift of focus from normative to positive research. Second, Fama (1965, 1970) laid the foundation for the efficient-market hypothesis (EMH) and tested it empirically while the capital asset pricing model (CAPM) was developed independently by, inter alia, Sharpe (1964) and Lintner (1965). Third, Fama et al. (1969) – while not strictly making the first event study (MacKinlay, 1997) – made substantial improvements to the event-study methodology. Thus, the EMH provided researchers with testable hypotheses whereas the CAPM and the refined event-study methodology facilitated the empirical testing of the EMH as well as other hypotheses.

Against the backdrop of the aforementioned disbelief in accounting, many scholars set out to test empirically whether accounting numbers are useful *at all*. While Benston (1967) finds that the relationship between sales and earnings numbers and share prices is statistically but not economically significant, other researchers showed that share prices and trading volumes are affected by both yearly earnings announcements (Beaver, 1968) and quarterly earnings announcements (Kiger, 1972). Moreover, several foreknowledge studies assessing the performance of portfolios formed *ex post* on the basis of foreknowledge of accounting variables documented variables such as earnings- and dividend-price ratios (Latané and Tuttle, 1967) and the sign of changes in annual income numbers (Ball and Brown, 1968) to be relevant for valuation.

2.2 Literature documenting the post-earnings-announcement drift

However, Ball and Brown (1968) also highlight another interesting issue. While they focus on the usefulness of accounting, they also – quite unintentionally – discover what is today labelled the post-earnings-announcement drift (PEAD).[1] Ex post, they form two portfolios characterized by positive and negative earnings changes or "surprises", respectively. The performance of these portfolios is studied over a time period ranging from twelve months before to six months after the earnings announcements, and abnormal performance indices are calculated for both portfolios. These abnormal performance indices are highly positive and negative for the positive and negative earnings surprise portfolios, respectively, and most – but not all – of the abnormal performance occurs before the earnings announcement.

[1] Indeed, Brown (1989), in a discussion of the Ball and Brown (1968) paper, remarks that "Ray [Ball] and I might well have started the habit […] of drawing attention to securities market anomalies and then proceeding to ignore them".

More specifically, whereas the abnormal performance indices increase from 1.00 to around 1.07 over the year preceding a positive earnings surprise, they increase an additional 0.6 to 0.8 percentage points (depending on the earnings variable used) over the three months following the earnings announcement. This result, albeit meagre in comparison to the performance before the earnings announcement, is important for two reasons. First, it challenges semi-strong-form market efficiency, which posits that share prices should respond rapidly and correctly to public information such as earnings announcements, thus precluding abnormal gains from this type of information. Second, it spurred continued research on PEAD.

While Latané et al. (1969) and Brown and Kennelly (1972) contribute to documenting the PEAD effect more thoroughly indirectly in foreknowledge studies investigating the usefulness of accounting, the bulk of the literature relating to PEAD deals more directly with the phenomenon. Two early examples are Jones and Litzenberger (1970), who show that companies whose quarterly earnings exceed predicted quarterly earnings by more than 1.5 standard deviations outperform a broad equity market index over the following six months, and Latané et al. (1970), who undertake sort-rank analysis of earnings-price and earnings-change variables to show that "good" stocks outperform the market over a six-month period.[2]

Latané et al. (1974) made an important contribution when developing the standardized unexpected earnings (SUE) measure, defined as the ratio of reported earnings per share (EPS) less predicted EPS, estimated using a time-series model, to the standard error of estimate of said time-series model. The SUE measure improved upon Jones and Litzenberger's (1970) earnings-surprise model in terms of outperforming the market, and is still the main measure used to calculate earnings surprises. Nevertheless, Latané et al. (1974, p. 131) conclude that even though their results are intriguing, more research is needed as "the empirical evaluation of the efficient market hypothesis must be based on collective information".

Additional studies cast further doubt on the notion of efficient markets by showing that significant abnormal returns could be earned on portfolios formed on the basis of both the SUE measure (Latané and Jones, 1977) and the percentage earnings surprise (Brown, 1978) and, moreover, that the magnitude of the abnormal return is positively related to the magnitude of the percentage earnings surprise (Joy et al., 1977).

Some of the early PEAD studies have been criticized on methodological grounds, as will be described in the next section. Still, Ball (1978) argues that the remarkable stability of the drift over a ten-year period and across methods indicates that the effect is not due solely to methodological biases; indeed, while the results of some studies might be questionable, he concludes that the phenomenon is real and that earnings most likely act as a proxy for some omitted variable. Thus, attempting to reinforce the EMH by explaining the substantial PEAD effect is what the research community turned its attention to beginning in the 1980s.

[2] In a later study, Litzenberger et al. (1971) verify that earnings-price variables can be used to select stocks that subsequently outperform the market, and the literature on earnings-price ratios is extensive. However, since this investment strategy identifies stocks with high earnings-price ratios, it resembles value investing, an approach which typically identifies stocks with high dividend yields and book-to-market ratios and low price-earnings ratios. And, while value investing is interesting as such, it is outside the scope of this paper

2.3 Literature explaining the post-earnings-announcement drift

Early studies documenting the PEAD effect often maintain that markets are efficient. For instance, Jones and Litzenberger (1970), while mentioning that the equity market might not be as efficient as EMH proponents claim, argue that gradual rather than instantaneous incorporation of earnings information into share prices is reasonable and consistent with the EMH given the relatively slow spread of this information from market professionals to other investors. In a similar vein, Latané et al. (1970, p. 428) – despite finding a relatively substantial PEAD effect – maintain that "while the market is very *nearly* perfect, time lags do exist". However, the burgeoning research on the PEAD phenomenon soon called for other explanations.

Data and methodological issues

According to Joy and Jones (1979), many early PEAD studies suffer from severe data problems. For example, early Compustat tapes suffered from a selection bias in that only surviving firms were included, and post-announcement revisions of earnings numbers sometimes replaced the original values. In short, early Compustat tapes suffered from "uncomfortably large error rates" (Joy and Jones 1979, p. 53).

In addition, some of the early studies were methodologically flawed. For example, Foster et al. (1984) criticize Jones and Litzenberger (1970) for assuming that interim results are available two months after the end of the fiscal quarter, which is not always true. Likewise, Holthausen (1983), as cited by Foster et al. (1984), criticizes some studies for forming portfolios after all observations for a period are compared and ranked, which implicitly assumes that an investor knows the distribution of earnings announcements in advance. Foster et al. (1984) also indicate that the estimation of the parameters used to calculate expected returns – such as the risk-free rate, the market risk premium and the exposure to market risk if the CAPM is used – could be biased, thus severely impacting the validity of the results.

Interestingly, however, not all criticism is correct. For example, Griffin (1977), in a paper discussing the time-series properties of quarterly accounting data, criticizes (an earlier version of) Joy et al.'s (1977) paper for not taking several statistical properties into consideration when estimating predicted earnings, thus invalidating the results. However, as Joy and Jones (1979) correctly emphasize, this criticism is in error as a model devising an investment strategy does not have to be statistically correct to contradict market efficiency; failure to take statistical properties into account could explain a potential lack of results, but never the presence of results.

All in all, the early PEAD studies did indeed suffer from data and methodological issues, some of which had the potential to exaggerate the results. For example, a survivorship bias could work to magnify the PEAD results, and so could a hindsight bias by either allowing pre-announcement returns to be included in the post-announcement period or exaggerating the returns that could be earned from utilizing a PEAD investment strategy. Still, as Ball (1978) highlights, not all early PEAD studies suffer from methodological problems, and the consistency with which they document the PEAD phenomenon invalidates any explanation based solely on methodological flaws.

Market frictions

Watts (1978) argues that even though the PEAD effect is an indication of market inefficiency, such a conclusion is somewhat constrained by the fact that a normal investor would have to pay relatively substantial transaction costs to exploit the apparent "arbitrage" opportunity. However, this view has not remained unchallenged, as some studies have shown that the PEAD effect is larger than any transaction costs. More importantly, Bernard and Thomas (1989, 1990) argue that although transaction costs could explain why an individual investor refuses to exploit the apparent mispricing, such costs do not explain why this "mispricing" persists given that some trading actually occurs. Rather, even though some investors might refrain from trading, prices should still adjust as long as at least some investors engage in trading.

However, other scholars have examined the existence of market frictions and its relation to PEAD more thoroughly. For example, Bhushan (1994) shows that the magnitude of PEAD is positively related to measures of direct and indirect trading costs, reinforcing the role of transaction costs and liquidity constraints as explanations for PEAD. Similarly, Mendenhall (2004) defines arbitrage risk as the idiosyncratic part of a share's volatility and finds that this variable is positively related to the magnitude of PEAD. Thus, actual and perceived limits to arbitrage – that is, liquidity constraints and high idiosyncratic risk – seem to limit the feasibility of a PEAD strategy, meaning that the market friction explanation has some merit.

Risk-based explanations

The estimation of the expected-return model parameters could be biased, but it could also be the case that the model as such, albeit correctly estimated, is flawed or fails to adjust properly for risk. Indeed, Ball (1978) argues that earnings proxy for some omitted variable or risk factor, implying that the CAPM is not sufficient. Accordingly, Watts (1978) tests whether deficiencies in the CAPM can explain PEAD, but concludes that they cannot. Likewise, Bernard and Thomas (1989) show that although market betas shift around earnings announcements, failure to consider this when estimating the CAPM parameters does not explain the drift.

In general, omitted risk factors have not been as thoroughly researched as other explanations for PEAD. For example, Bhushan (1994, p. 46) bluntly remarks that "[a]ttempts to explain [PEAD] as compensation for risk […] have been unsuccessful", and Bernard and Thomas (1989) show that none of the most common risk factors used in the asset-pricing literature can help to explain PEAD, suggesting that omitted risk factors is not a satisfactory explanation for the phenomenon.

Behavioural explanations

The persistence of the drift has been viewed as an indication of market inefficiency. This is consistent with the behavioural finance view, which posits (i) that some investors are irrational rather than risk-avert and utility-maximizing and (ii) that there are limits to arbitrage preventing those who are actually rational from exploiting existing "arbitrage" opportunities (Barberis and Thaler, 2003). For example, traits such as *overconfidence* and *belief perseverance* are consistent with PEAD as the former could lead investors to neglect public information on behalf of private assessments, causing them to underreact to an earnings announcement, whereas the latter could cause them to hang on to such a view or investment strategy for too long.

Related to these concepts is that of *underreaction*. Hong and Stein (1999) develop a model characterized by bounded rationality – meaning that individuals have limited information-processing capabilities – where agents are either "newswatchers", which possess private information, or "momentum traders". Since newswatchers cannot extract additional information from share prices, firm-specific information diffuses gradually, causing the market to underreact; when such information spreads, however, momentum traders using "trendchasing" strategies begin to trade on the newly available information, thus causing overreaction. In Hong and Stein's (1999) view, this model is consistent with the return pattern following upon not only earnings announcements, but also share issues and repurchases.

Bernard and Thomas (1989) investigate a number of potential explanations for the PEAD effect. As has been described above, they discard risk-based explanations as well as explanations based on transaction costs, lending them to conclude that prices fail to reflect the full implications of current earnings for future earnings, which is what causes the gradual upward (downward) drift for firms exhibiting positive (negative) earnings surprises. In a later paper, Bernard and Thomas (1990) also test this hypothesis more thoroughly, reaching the same conclusion. Likewise, Mendenhall (2004), while discussing limits to arbitrage in his paper, concludes that his results are supportive of PEAD as an underreaction phenomenon, in line with Hong and Stein's (1999) theoretical model.

2.4 Development of hypotheses

Setterberg (2011) documents the existence of PEAD in Sweden over the time period 1990-2005. Consequently, the first aim of this paper is to provide out-of-sample evidence of the phenomenon in a Swedish setting by trying to document the existence of PEAD over a time period ranging from 2004 to 2013. Accordingly, given the persistence of the phenomenon in existing research, the first hypothesis is that the PEAD effect is still present in the Swedish equity market.

As has been described above, there is extensive research trying to explain the PEAD phenomenon, so far without complete success; indeed, Kothari (2001, p. 196) remarks that "a rational, economic explanation for the drift remains elusive". However, the above discussion has also indicated what hypotheses are currently regarded as the most plausible. In particular, the view that PEAD can be fully explained by flawed methodologies is disregarded in this paper.

Likewise, the risk-based explanation is disregarded. Given that Setterberg (2011) attributes PEAD in part to information uncertainty risk, this decision might require an explanation. If the PEAD effect is due to an omitted risk factor, this risk factor should work in the same direction for all stocks exposed to the particular risk factor by increasing their expected returns. This implies that the usage of an expected-return model which fails to take said risk factor into account should lead to positive abnormal returns for all stocks exposed to the risk factor rather than positive abnormal returns for some stocks and negative for others. Consequently, as Setterberg (2011, p. 123) remarks, "it is not trivial to explain the classic PEAD results with an omitted risk factor".

Since Setterberg (2011), unlike many other scholars, finds that the PEAD effect is driven almost exclusively by the stocks with positive earnings surprises, it is still possible that an omitted risk factor – say, information uncertainty risk – could explain her results. Still, the results of other studies are difficult to reconcile with her theoretical framework. In particular, Setterberg (2011, p. 123) mentions that "the theoretical framework assumes that the long and short positions are equally exposed to the information risk" and that the information risk explanation can be consistent with a downward drift only if this assumption is relaxed.[3] However, as she also remarks that "empirical evidence seems to point towards both good and bad earnings news encompassing high information uncertainty" (Setterberg 2011, p. 124), her theoretical framework seems unable to fully explain the PEAD phenomenon. Therefore, guidance is sought from other explanations.

Accordingly, following evidence provided by Bernard and Thomas (1989, 1990), which indicates that investors are not fully rational when it comes to interpreting the implications of current earnings for future earnings, as well as by Bhushan (1994) and Mendenhall (2004), which indicates that PEAD is related to market frictions in the form of liquidity constraints and limits to arbitrage, a more behavioural view is taken. More concretely, building on Bernard and Thomas's (1989, 1990) research, the second aim of this paper is to shed some light on a potential explanation for PEAD by investigating whether more sophisticated investors – that is, investors presumed to act more rationally – are better able to understand the implications of current earnings for future earnings, thus reducing the magnitude of the PEAD effect.

More specifically, following research by Bartov et al. (2000), who show that the proportion of shares held by institutional investors is negatively related to PEAD, this paper argues that institutional ownership is a useful proxy for investor sophistication as institutional owners invest in companies on the basis of their professional knowledge of that particular company or its industry. Likewise, following Mikhail et al. (2003), who demonstrate that sell-side analyst experience reduces the drift, it is argued here that analyst experience is a suitable proxy for investor sophistication as analysts give advice to investors based on their professional judgement of a company's prospects.[4]

Finally, drawing upon Kolasinski and Li's (2011) conclusions that the occurrence of insider trading after an earnings announcement leads to a faster realization of PEAD, it is argued that insider trading is also a suitable proxy for investor sophistication. More concretely, as these actors, too, base their investment decisions on superior professional knowledge of their particular company and also have to report or "flag" their transactions to regulators, their trading is expected to work as a signal communicating said professional knowledge to the market, thus potentially speeding up the drift. Consequently, the second hypothesis is that investor sophistication, proxied by presumably knowledgeable *buy-side*, *sell-side* and *inside* actors in the equity market, reduces – or, in the case of insider trading, speeds up – the drift in the Swedish equity market.

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[3] In addition, there is an assumption that investors in the long and short portfolios have the same risk appetite, an assumption which, somewhat unrealistically, has to be relaxed to reconcile Setterberg's (2011) framework with a downward drift.

[4] In this paper, the term analysts invariably refers to sell-side analysts.

3 DATA

3.1 Sample selection

The sample period investigated in this paper ranges from 2004 to 2013. Such a recent time period is desirable since it increases the relevance of the study. The starting point of the sample period is chosen because there is no Swedish pre-2004 data available from Thomson Reuters's Institutional Brokers' Estimate System database (I/B/E/S), making it difficult to obtain not only earnings announcement dates but also analyst data, which is central for testing the hypotheses of this paper.

This paper studies Swedish publicly listed companies. The sample is limited to companies presently or previously listed on the Large, Mid and Small Cap lists of the Nasdaq OMX Nordic Stockholm stock exchange as well as the former A and O lists. The sample size could have been increased by including companies listed on other exchanges, such as the Nordic Growth Market or AktieTorget, or other lists, such as First North and First North Premier. However, due to concerns regarding data reliability and availability, especially when it comes to ownership, analyst and insider data, these exchanges and lists are not included in the sample.

All stocks listed on the Large, Mid and Small Cap lists as per December 31, 2013 are included in the sample. In addition, all stocks that have been delisted from these lists or the former A and O lists between 2004 and 2013 are added to the sample, giving a list of 435 stocks. Delisted stocks are added back in order to avoid survivorship bias, an issue which has plagued some early PEAD studies and, according to Brown et al. (1992), can severely impact the results of performance studies. The list of 435 stocks has been subject to the screening procedure described in **Appendix A**. This screening procedure is undertaken in order to exclude all stocks that are not useful for this study, including financial stocks and the least traded stock of companies with more than one class of shares, as well as those for which there is no data available and gives a final list of 215 stocks.[5]

A potential problem with the screening procedure is that some of the screens made to exclude stocks that are not useful may have reintroduced the issue of survivorship bias. Although the initial sample of 435 stocks comprises 275 active stocks and 160 delisted stocks, meaning that some 37 per cent of the stocks are delisted stocks, the final sample includes 174 active stocks and only 41 delisted stocks, meaning that the proportion of delisted stocks is only around 19 per cent. In particular, it seems that the screen excluding stocks for which there is no data in the I/B/E/S database substantially decreases the number of delisted stocks in the sample. However, since approximately 20 per cent of the stocks in the final sample are delisted, the issue of survivorship bias is at least partially mitigated.

[5] A list of all stocks included in the study is available from the authors upon request.

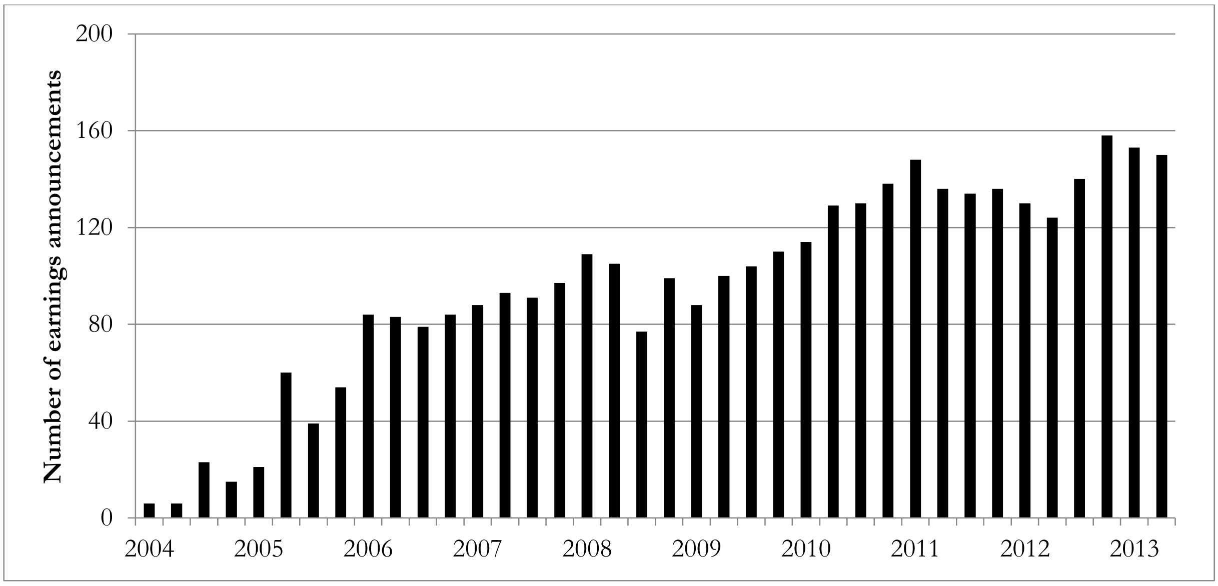
3.2 Data needed for documenting the post-earnings-announcement drift

Market data for the 215 stocks is downloaded from Thomson Reuters's Datastream database (Datastream), a database commonly used in financial market research. In Datastream, share prices are available on a daily basis whereas dividend information is restricted to a yearly basis. Thus, daily returns are obtained using the total return index variable, which adjusts for the effect of dividends and stock splits, making it useful for calculating daily returns. The trading volume data is used to remove dates which Datastream classifies as trading days but which are, in fact, holidays when the Swedish stock exchanges are closed.[6]

In addition, daily returns of a broad market index, MSCI Sweden, is downloaded from Datastream. With 31 constituents, the MSCI Sweden index covers some 85 per cent of the Swedish equity universe (MSCI, 2014), making it a good proxy for the Swedish equity market. Moreover, quarterly earnings announcement data is obtained from I/B/E/S, giving a total of 4,142 quarterly earnings announcements. These 4,142 earnings announcements are subject to a screening procedure, described in **Appendix B**, which is aimed at removing unfeasible or even erroneous data. It results in a final sample of 3,635 earnings announcements, and this sample is considered the main sample for testing the first hypothesis of this paper.

Since not all firms in the sample were listed during the entire sample period, the earnings announcements are not evenly distributed throughout the sample period. Rather, as shown in **Figure 1**, the distribution is skewed towards newer announcements, implying that more recent quarters have a more profound impact on the results. However, there is no reason to believe that this should bias the results in any direction; if anything, it should make them more relevant as they are based mostly on newer earnings announcements.

Figure 1: Sample distribution of earnings announcements



The figure shows the distribution of the 3,635 earnings announcements in the sample, divided by quarter.

[6] Examples of holidays classified as trading days in Datastream include May 1, December 24 and December 31. In order to prevent these holidays from biasing the results, all dates for which the trading volumes of Ericsson, H&M and Volvo – the three most traded stocks in the sample – equal zero are excluded from the sample.

3.3 Data needed for explaining the post-earnings-announcement drift

As will be detailed in the methodology section, the testing of the second hypothesis will be done using two different setups. Thus, the data used in these setups is described separately.

Data on institutional ownership and analyst experience

Ownership data is retrieved from the Swedish database SIS Ägarservice, which is the foremost database for data on ownership in Swedish publicly listed companies. SIS Ägarservice publishes ownership data on a company-by-company and owner-by-owner basis. Lacking a generally accepted definition of institutional ownership, institutional owners are defined as all members of either Institutional Owners Association for Regulatory Issues in the Stock Market (Institutionella ägares förening för regleringsfrågor på aktiemarknaden) or the Swedish Investment Fund Association (Fondbolagens förening) as per 2012 and 2014, respectively.[7]

For each company included in the sample, the proportion of shares owned by each of these institutional owners as per each year-end – or, in some cases, the latest available date for the year – from 2003 to 2012 is obtained on an owner-by-owner basis from the database. The different institutional investors' ownership stakes are aggregated into a measure of the proportion of institutional ownership on a company-by-company basis. Thus, for each company and year, there is a single number representing the proportion of institutional ownership.[8]

However, there are two issues with the data obtained from SIS Ägarservice. First, there is no data available for 25 of the 215 companies, most of which are delisted, which exacerbates the issue of survivorship bias as well as makes it impossible to test the second hypothesis for these companies. A total of 190 earnings announcement observations corresponding to these 25 companies are dropped, leaving the sample at 3,445 earnings announcements.

Second, it seems that there are some problems for companies included in the database as well. For example, as per the end of 2012, company-by-company data for ABB indicates that the Swedish pension fund Alecta is the fifth-largest owner, whereas the owner-by-owner data indicates that Alecta has no ownership stake in ABB.[9] Thus, even though ABB is not included in the sample as the Nasdaq OMX Nordic Stockholm stock exchange is not its primary listing, this example introduces uncertainty as to whether the data obtained from SIS Ägarservice is reliable, suggesting that any results obtained using this data should be interpreted with some caution.

[7] The reason for using different dates is that more recent, reliable information on which funds are members of the Institutional Owners Association for Regulatory Issues in the Stock Market is limited. Presumably, this difference should not affect the results.

[8] For many companies and owners, the ownership data is updated every quarter. However, this is not the case for all companies and owners, which is why yearly data is used. While this choice means that the proportion of institutional ownership will not be as timely as it could be for many companies, it should not bias the results in any particular direction.

[9] Ownership data for Autoliv exhibits essentially the same problem.

Data on analyst experience is obtained from I/B/E/S. In addition to quarterly earnings announcement dates, consensus forecast as well as actual EPS are obtained from the Summary History files in I/B/E/S. Furthermore, estimates made by individual analysts are obtained from the Detail History files. However, such detailed data is unavailable for a total of 304 of the remaining earnings announcements (as well as some of the announcements which were removed due to lack of ownership data), reducing the number of useful earnings announcements to 3,141.

In addition, a host of additional variables are downloaded from Datastream. These include the share price and trading volume as well as the market capitalization. Of these, the latter is denominated in US dollars.[10] For four earnings announcements, there is no data available for market capitalization. These observations are removed, leaving the final sample for testing the institutional ownership and analyst experience variables at 3,137 observations.

Data on insider trading

As described more thoroughly in the methodology section, the data on insider trading is used in a different setup than the data on institutional ownership and analyst experience. The data comes from the Swedish Financial Supervisory Authority (Finansinspektionen). It includes, on an individual-by-individual basis, data on the number of shares traded, the date of the trade and the "flag" date – that is, the date on which the trade is reported – for all insiders required to report to the Swedish Financial Supervisory Authority.

The data spans the time period 2004-2011. Thus, all earnings announcements occurring during the last two years of the sample period are excluded from the sample in order to avoid using them as "control group" when testing the predictions regarding insider trading. This removes no less than 855 earnings announcements, thus reducing the sample size from 3,635 to 2,780.[11] Likewise, since the post-announcement performance is studied over a period of up to 120 days, all earnings announcements occurring in the last two quarters of 2011 are excluded from the sample. This removes another 270 earnings announcements, leaving the sample at 2,510 earnings announcements.

In addition, it seems that there is no data available for a total of six companies, corresponding to 76 earnings announcement observations. While it could be the case that there are no insiders or that no insider trading has occurred in these companies, this seems somewhat unlikely. Thus, these observations are treated as erroneous, resulting in a sample size that is further reduced, from 2,510 to 2,434. Additionally, six more observations are lost as the insider trading amounts are accumulated, and a further 31 observations are lost when the dependent variables for the insider trading regressions, described in the methodology section, are retrieved. Finally, two observations where there is no market capitalization available are dropped, resulting in a final sample of 2,395 observations.

[10] While all stocks in the sample should have their market capitalization denominated in Swedish krona, denominating market capitalization in US dollars facilitates comparison and avoids any currency biases.

[11] As mentioned above, the insider data is used in a different setup than the data on institutional ownership and analyst experience. Thus, the calculation of the number of observations once again starts from 3,635 observations.

4 METHODOLOGY

4.1 Documenting the post-earnings-announcement drift

An event study "measures the impact of a specific event on the value of a firm" (MacKinlay 1997, p. 13). The method – while, according to MacKinlay (1997), first used as early as in 1933 – was substantially improved by Fama et al. (1969) in a paper investigating the impact of stock splits on security returns. The basic approach is to calculate the return, in excess of some benchmark, for a certain security following the event of interest. The advantage of this approach, especially when studying short time horizons or using large sample sizes, is that it is often reasonable to assume that the impact of other events either is negligible or cancels out.

In this paper, the event-study approach is used to investigate whether the announcement of "good" ("bad") earnings figures, as compared to some measure of expected earnings, has a positive (negative) impact on the share price performance of Swedish companies. To this end, four steps are taken. First, a single measure capturing the degree of "surprise" – measured in terms of the direction and magnitude of the deviation from the expectation – contained in the earnings announcement is calculated. Second, based on this measure, a trading or portfolio formation rule defining what is viewed as "positive" and "negative" surprises is determined. Third, the share price performance of companies exhibiting positive and negative surprises, respectively, is compared. Finally, the economic and statistical significance of any performance difference between "good" and "bad" companies is assessed.

Measuring the surprise

In earlier PEAD studies, the most common method used to calculate the surprise is the standardized unexpected earnings (SUE) measure, which standardizes the deviation of (some measure of) actual earnings from expected earnings. Expected earnings are usually obtained using either a time-series model or analyst estimates, but irrespective of this choice SUE is defined as the actual earnings number less the estimated earnings number divided by the standard error of estimate (in the case of a time-series model) or the standard deviation (in the case of analyst estimates). In practice, the most common earnings measure used is the EPS.

However, as Brandt et al. (2008) emphasize, the SUE measure has some limitations. In particular, it requires an estimator of the market's expectation, which is essentially unobservable. In addition, the EPS measure – while typically used in PEAD studies as it is an easily understandable summary measure that typically does not suffer from data unavailability – suffers from the fact that it can be considerably affected by non-recurring items, resulting in substantial deviations from expected EPS even though there are no long-term implications for valuation.

Thus, following Brandt et al. (2008), this paper uses the earnings announcement return (EAR) measure, first used by Foster et al. (1984). The EAR measure, calculated as the return of a share (in excess of some benchmark) on the day of the earnings announcement, does not rely on any estimate of expected earnings. Likewise, it does not use the EPS measure. Still, it captures the degree of surprise as, presumably, a good (bad) earnings announcement leads to an immediate upward (downward) share price reaction, which is then captured by the EAR measure.

More importantly, as highlighted by Brandt et al. (2008, p. 1), "EAR captures the surprise in all aspects of the company's earnings announcement, and not just the surprise in earnings". For example, not only accounting numbers but also complementary financial information as well as statements made in the management discussion and analysis or during analyst presentations are included in the EAR measure, thus potentially making it a better measure of surprise than measures based on estimations of one aspect of earnings. In addition, even though the measure could capture events not related to the earnings announcement, this risk should be limited as it is calculated over a short time horizon. Thus, it is used to measure surprise in this paper.

$$EAR_{i\tau} = R_{i\tau} - E(R_{i\tau}) \tag{1}$$

More concretely, the EAR is calculated in accordance with (1), where $R_{i\tau}$ denotes the return of security i at time τ (which is the earnings announcement date) and $E(R_{i\tau})$ denotes the expected return of the same security. In its simplest form, the expected return is assumed to equal zero, reducing the EAR to the realized return of the security. However, the EAR is usually calculated as the return in excess of either the market return, in which case the expected return of all stocks is the market return, or the security-specific expected return, which takes differences in risk characteristics into account. In this paper, the main EAR measure is the EAR in excess of the security-specific expected return – the calculation of which will be described later – but the other approaches, where the expected return is equal to zero or the market return, are used to test the robustness of the results.

Determining the trading rule

As described in the previous research section, Foster et al. (1984) criticize the use of hindsight information in some early PEAD studies. For example, a strategy where portfolios are formed based on a ranking of all earnings surprises is not practically implementable as the investor would have to know the distribution of earnings surprises already when the first earnings announcement is made. Thus, using such an approach means that any results obtained do not represent the returns that could be earned from employing the particular investment strategy; rather, they are biased by using information which is not available at the time of the investment decision.

To avoid any concerns regarding hindsight information, the surprises are classified as positive or negative depending solely on their EARs. The EAR for each stock is compared to predetermined cut-off values to form three portfolios, GOOD NEWS, BAD NEWS and LONG-SHORT, which is long in GOOD NEWS and short in BAD NEWS. Stocks are included in GOOD NEWS (BAD NEWS) when exhibiting EARs in excess of (below) a positive (negative) three per cent. Using these limits, approximately 24 (26) per cent of the earnings announcements are classified in the GOOD NEWS (BAD NEWS) portfolio, whereas the remaining 50 per cent are classified as containing no surprise and, hence, not included in either of the portfolios. While the cut-off values have been chosen somewhat arbitrarily, the size of the resulting portfolios is close to those used in previous research, where the entire sample is typically divided into quintiles. Moreover, the robustness of the results is tested using both higher and lower cut-off values.

The portfolios are not formed on the day of the earnings announcement. The reason for this is that only the post-announcement performance should be captured; the EAR should not be included when determining this post-announcement performance. Likewise, since it could be the case that some companies announce the interim results after the market has closed, causing their "true" EARs to occur on the trading day following the formal earnings announcement day, the portfolios are not formed on this day either. Rather, even though it is not possible to adjust the EAR measure for those stocks whose "true" EAR occurs the day after the earnings announcement day, the portfolios are formed two trading days after the formal earnings announcement day to mitigate the risk of capturing the announcement effect – and not only the potential post-announcement drift – when calculating the portfolio returns.

Comparing the post-announcement returns

To facilitate comparison of the post-announcement performance of the two portfolios, the abnormal return – that is, the return in excess of some benchmark – has to be calculated for each portfolio. Following MacKinlay (1997), this paper relies mainly on the abnormal return (AR) measure.

$$AR_{i\tau} = R_{i\tau} - E(R_{i\tau}|X_\tau) \tag{2}$$

In (2), $AR_{i\tau}$, $R_{i\tau}$ and $E(R_{i\tau}|X_\tau)$ are the abnormal, actual and normal (or expected) returns for security i at time τ; X_τ is the conditioning information in the model used to calculate the normal return. However, as described by MacKinlay (1997), there is a variety of ways of calculating the normal performance, and the alternatives have different statistical properties and interpretations. Of these, this paper uses the market model, which relates security returns to market returns. This model is theoretically superior to the other main model, the constant mean return model, in that it reduces the variance of the abnormal returns (MacKinlay, 1997).[12] In addition, MacKinlay (1997) argues that the usefulness of employing multi-factor models, such as those including the size and value factors developed by Fama and French (1992), is limited, lending further support to the use of the market model.

[12] Still, MacKinlay (1997) argues that the practical difference between the two models is usually limited.

$$R_{i\tau} = \alpha_i + \beta_i R_{m\tau} + \varepsilon_{i\tau} \qquad E(\varepsilon_{i\tau}) = 0 \qquad Var(\varepsilon_{i\tau}) = \sigma^2_{\varepsilon_i} \tag{3}$$

The market model is defined as in (3), where $R_{i\tau}$ and $R_{m\tau}$ are the returns for security i and the market, respectively, at time τ whereas $\varepsilon_{i\tau}$ is the error term, which is zero in expectation. The market model parameters – that is, the coefficients, α_i and β_i, as well as the residual variance, $\sigma^2_{\varepsilon_i}$ – are usually obtained over an estimation period preceding the event period and then used to obtain the out-of-sample, event-period expected returns. Substituting the expected return into (2), the AR can be calculated.

$$AR_{i\tau} = R_{i\tau} - (\alpha_i + \beta_i R_{m\tau}) \tag{4}$$

In (4), the expected return of a security during a specific date depends on two components. The first term, α_i, is a constant which can take on both positive and negative values. The second term expresses the fact that the expected return of a security also depends on the performance of the market through the security's exposure to market risk. A β_i higher than one indicates a relatively higher exposure to market risk, where the performance of the market is expected to be amplified for the individual security, whereas the opposite is true for firms with β_i values below one. The AR is the period τ return of a portfolio long in security i and short in the theoretical expected return, obtained using the market model, for the same security. Thus, this does not correspond to an implementable trading strategy as the expected return cannot be earned.[13]

In this paper, daily returns are used to estimate expected returns and measure the performance of the different portfolios. Using daily returns has the benefit of allowing a large number of observations to be used in the estimation, while still keeping the estimation period relatively short, thus ensuring that the company has not changed too much when the parameters are used in comparison to when they are estimated. A potential drawback of using daily data to estimate the market model is that the β_i coefficients for illiquid stocks – that is, stocks that are not traded every day – become biased downwards. The reason for this is that these stocks, albeit potentially being heavily exposed to market risk, seem not to co-move with the market on days when they do not move at all (as they are not traded), leading to lower β_i coefficients. Still, since the sample contains firms listed only on major lists this should not be a major cause for concern.

More concretely, following MacKinlay (1997), the market model parameters are estimated using ordinary least squares regressions. The MSCI Sweden index is used as a proxy for the market portfolio, and the parameters are estimated using 240-day estimation periods starting 250 trading days and ending 11 trading days before each earnings announcement. The reason for ending the estimation period a couple of weeks before the actual earnings announcement is that the earnings announcement return – or, indeed, any information leakage shortly before the earnings announcement – should not affect the market model parameters.

[13] In particular, even though it is possible to construct a portfolio that gives the same market risk, as measured by the β_i coefficient, the inclusion of the α_i parameter makes the formation of an expected-return portfolio impracticable.

The AR measure is useful for assessing the performance of a security over a single period. However, most event studies assess performance over a longer time horizon, and the present one is no exception. Thus, the one-period ARs have to be accumulated. There are two main ways of doing this, but this paper relies primarily on the one described by MacKinlay (1997), the cumulative abnormal return (CAR).

$$CAR_i(\tau_1, \tau_2) = \sum_{\tau=\tau_1}^{\tau_2} AR_{i\tau} \tag{5}$$

In (5), the CAR for security i over the time period ranging from τ_1 to τ_2 is obtained by summing the one-period ARs over the same time period. Thus, CAR does not take compounding into account, meaning that it represents the returns to a trading strategy long in security i and short in the (purely theoretical) expected return with periodical rebalancing to the original investment amount and no reinvestment of potential gains. Hence, it represents an accumulated return from a trading strategy that is unlikely to be implemented by any real investor. Therefore, two alternative, more intuitive ways of accumulating the one-period returns are also used.

$$API_i(\tau_1, \tau_2) = \prod_{\tau=\tau_1}^{\tau_2} (1 + AR_{i\tau}) \tag{6}$$

First, the abnormal performance index (API) is used as an alternative measure of accumulated abnormal returns. In (6), the API of security i is calculated by multiplying one plus the one-period ARs over the time period ranging from τ_1 to τ_2. Hence, as described by Barber and Lyon (1997), the API mitigates the bias of CAR stemming from not taking compounding into account. Still, the API also assumes periodical rebalancing, meaning that this measure also might not be a good reflection of a return earned from implementing an actual trading strategy.

$$BHAR_i(\tau_1, \tau_2) = \prod_{\tau=\tau_1}^{\tau_2} [1 + R_{i\tau}] - \prod_{\tau=\tau_1}^{\tau_2} [1 + E(R_{i\tau})] \tag{7}$$

Finally, following Barber and Lyon (1997), the buy-and-hold abnormal return (BHAR) is used as an alternative way of accumulating the abnormal returns. However, as the BHAR for security i over the time period ranging from τ_1 to τ_2 is calculated as in (7), it does not strictly use the AR measure. Rather, it calculates the buy-and hold return (BHR) of security i as well as some benchmark and then compares these to obtain the BHAR. Thus, it not only takes compounding into account, but also makes no unrealistic assumptions regarding periodical rebalancing.

In this paper, the primary holding period is 60 trading days, although a 120-trading-day holding period is also investigated in some of the robustness tests. The GOOD NEWS and BAD NEWS portfolio CARs are obtained as the average 60-day CARs for all stocks exhibiting positive and negative surprises, respectively, thus implicitly assuming equally weighted portfolios. As mentioned above, the CAR measure is considered the main measure in this paper even though it has its flaws. The reason for this is that the differences between CAR, API and BHAR are typically small (Barber and Lyon, 1997), especially when the holding period is relatively short and the ARs are small in magnitude. In addition, even though the API and BHAR measures are used to test the robustness of the results, another reason for choosing CAR as the main measure is that its statistical properties, such as additivity, make it a useful measure for statistical testing.

Statistical testing of the abnormal returns

Under the null hypothesis that the ARs are not statistically different from zero – meaning that there is no drift, which is what is tested with the first hypothesis of this paper – the one-period ARs should be jointly normally distributed with a zero mean and a variance as in (8).

$$\sigma^2(AR_{i\tau}) = \sigma^2_{\varepsilon_i} + \frac{1}{L_1}\left[1 + \frac{(R_{m\tau} - \hat{\mu}_m)^2}{\hat{\sigma}^2_m}\right] \tag{8}$$

In (8), the first component is the error variance from the estimation of the market model whereas the second component is additional variance stemming from the sampling error in α_i and β_i. As L_1 expresses the number of observations used to estimate these parameters, the second component approaches zero as the number of observations becomes larger. The estimation period of 240 days used in this paper should be "large enough to make it reasonable to assume that the contribution of the second component to the variance of the abnormal return is zero" (MacKinlay 1997, p. 21).[14]

As MacKinlay (1997) demonstrates, this assumption makes it possible to obtain the variance of CAR for a single security by multiplying the first component of (8) by the number of periods over which the performance of the particular security is studied. Consequently, the null distribution of both AR and CAR can be derived, in turn making it possible to obtain the variance of the average AR and CAR – that is, the variance of the AR and CAR of an equally weighted portfolio – as well as its distribution under the null hypothesis of zero ARs and, thus, CARs. These distributions are used to derive the test statistics in (9) and (10).[15]

$$\theta_{\overline{AR}} = \frac{\overline{AR}_\tau}{\sqrt{Var(\overline{AR}_\tau)}} \sim N(0,1) \tag{9}$$

$$\theta_{\overline{CAR}} = \frac{\overline{CAR}(\tau_1, \tau_2)}{\sqrt{Var\left(\overline{CAR}(\tau_1, \tau_2)\right)}} \sim N(0,1) \tag{10}$$

[14] Indeed, Per-Olov Edlund, Associate Professor at the Center for Economic Statistics at the Stockholm School of Economics, claims that 60 time periods should be enough to make the impact of the second component negligible.
[15] The formal derivation of all steps taken to obtain the tests statistic in (9) and (10), while not overly complex, are beyond the scope of this paper. The interested reader is referred to MacKinlay (1997).

In this paper, the "true" error variance from the estimation of the market model, which is unobservable, is estimated in accordance with the convention of using the mean squared error, defined as the residual sum of squares divided by the degrees of freedom of the residual sum of squares (Wooldridge, 2009). The test statistics are used to test the statistical significance of (i) the daily average ARs – that is, the portfolio ARs – and (ii) the average CARs; that is, the portfolio CARs. In particular, although the significance of one-day ARs is of some interest, the focus is primarily on the value of the test statistic in (10) as 60-day portfolio CARs significantly different from zero would provide evidence of PEAD.

4.2 Using buy-side and sell-side proxies to explain post-earnings-announcement drift

Definitions of variables

To test whether investor sophistication – proxied by the proportion of institutional ownership and the experience of analysts – reduces PEAD, several variables are needed. The dependent variable in each regression is the 60-day CAR, but both the 120-day CAR and the 60-day API will also be used to test the robustness of the results to the choice of the dependent variable.

The first variable of interest is the proportion of institutional ownership in a company, expected to be inversely related to the magnitude of the drift as institutional investors, typically making investment decisions based on professional knowledge of a company or industry, should be better able to understand the full implications of current earnings for future earnings. Thus, for each earnings announcement observation in the sample, the proportion of institutional ownership – INST – is defined as the proportion of institutional ownership as per the calendar year-end preceding the particular earnings announcement.

The other variable of primary interest is the experience of analysts following the company, likewise expected to be inversely related to the magnitude of PEAD as analysts, scrutinizing companies in order to be able to give good advice to investors, should over time become better at understanding the connection between current and future earnings. This is in line with the findings of Mikhail et al. (1997), who show that analysts improve their analytical abilities and performance with experience. Accordingly, following Mikhail et al. (2003), analyst experience – measured as the number of quarters, including the present, for which a certain analyst has followed and published quarterly earnings estimates for a particular company – is expected to capture analyst performance.[16] The main measure of analyst experience used in the regressions is the median experience – MEDEXP – of all analysts following the particular company, measured in quarters at the time of each earnings announcement.

To test the robustness of the results obtained using MEDEXP, two other measures of analyst coverage are also used. First, the total experience – TOTEXP – is defined as the total experience of all analysts following a particular company, measured in quarters at the time of each earnings announcement. Second, the number of analysts following the company at the time of each earnings announcement, measured as the number of

[16] This definition is not strictly the same as the one used by Mikhail et al. (2003) as they use the number of *prior* quarters as their measure of experience, but this difference should not impact the results.

earnings estimates published – EST – is used to measure analyst coverage. The latter measure does not capture the experience of the analysts, but this is not the intention of the measure. Rather, since I/B/E/S lacks pre-2004 data, the early earnings announcements will most likely not have the correct numbers for MEDEXP and TOTEXP as these measures do not capture analyst experience gained before 2004. Thus, even though EST might not be a good measure of analyst experience it should have some merit as a larger number of analysts should also have some impact on the presence of PEAD.

Along with the two variables of main interest, a number of additional variables are included in the regressions. First, the earnings announcement return – EAR – is included in all of the regressions. However, this variable is not of primary interest as the first part of the paper – the event study – is the part concerned with documenting PEAD. Second, given concerns that PEAD is driven in part by small stocks and following most previous research (Bhushan, 1994; Mendenhall, 2004), a size variable – SIZE – which is defined as the natural logarithm of the market capitalization, measured in US dollars at the end of the previous year, is included in most of the regressions.

Third, a number of variables controlling for transaction costs and limits to arbitrage are included in some of the regressions. Following Bhushan (1994), who argues that the share price is inversely related to direct trading costs, the share price – PRICE – eleven days before the earnings announcement is included in the regressions. Likewise, Bhushan (1994) argues that the yearly dollar trading volume is negatively related to indirect trading costs as higher liquidity means that large trades can be made without much delay and adverse price impacts. Thus, the natural logarithm of the annual dollar trading volume for the previous year – VOLUME – is also included to control for limits to arbitrage. Finally, following Mendenhall (2004), who assumes that arbitrageurs hedge using the market portfolio and argues that the arbitrage risk is higher the less of the security returns can be explained by exposure to market risk, the arbitrage risk – ARBRISK – is defined as the error variance from a regression of the individual security's returns on those of the market over a 240-day period and also included in some of the regressions.[17]

However, the variables as such are not included in the regressions; rather, they are discretely standardized. More specifically, for each quarter and each of the variables described above – INST and MEDEXP (and TOTEXP and EST) as well as EAR, SIZE, PRICE, VOLUME and ARBRISK – the observations are ranked from smallest to largest and classified into quartiles based on this ranking. The quartiles are given values ranging from zero, for the smallest quartile, to three, for the largest quartile. The exception is EAR, where the quartile values instead range from one to four. The quartiles are scaled by three so that all variables – QINST, QMEDEXP, QTOTEXP, QEST, QEAR, QSIZE, QPRICE, QVOLUME and QARBRISK – have a range of one; QEAR from one third to four thirds and the others from zero to one.

[17] The attentive reader might notice that this is the same parameter as the one used to estimate the variance of the ARs.

This approach, albeit seemingly peculiar, is in line with previous research (Bhushan, 1994; Bartov et al., 2000; Mikhail et al., 2003). The approach has some drawbacks in that it makes it relatively difficult to interpret the magnitude of the coefficients and, more importantly, reduces the variation in the explanatory variables. On the other hand, this has the benefit of reducing the impact of statistical outliers, which should make the results more robust. Thus, despite its drawbacks the approach is used in this paper to facilitate comparison to earlier research on the subject.

The focus of this paper is not on the ceteris-paribus effect of investor sophistication on CAR, which would be captured by including the proxies as such in the regressions. Rather, a high degree of the variables of interest – QINST and QMEDEXP – is expected to reduce PEAD, defined as the CAR of a portfolio long in GOOD NEWS and short in BAD NEWS, when there are positive or negative earnings surprises.[18] To capture this effect, all variables are interacted with the QEAR variable. The intuition behind this choice, which is in line with previous research (Bhushan, 1994; Bartov et al., 2000; Mikhail et al., 2003; Mendenhall, 2004), is that high levels of the variables of interest – may it be institutional ownership or analyst experience – multiplied by high (low) QEARs, result in high (low) values of the interaction terms. Multiplying high (low) interaction term values with negative regression coefficients leads to a substantial (unsubstantial) negative impact on CAR, consistent with the hypothesis. Thus, the coefficients on both QEAR×QINST and QEAR×QMEDEXP are expected to be significantly negative.

Similarly, as the drift is usually shown to be more pronounced for small stocks, the coefficient on QEAR×QSIZE is expected to be negative. Likewise, the coefficients on QEAR×QPRICE and QEAR×QVOLUME are expected to be negative as higher amounts of these variables – consistent with lower transaction costs – should make it easier to earn the PEAD returns, thus reducing the drift. Conversely, the coefficient on QEAR×QARBRISK is expected to be positive as higher arbitrage risk is expected to reduce investors' willingness to exploit the "mispricing", thus increasing – or, at the very least, not reducing – the drift. Finally, the coefficient on QEAR is expected to be positive, reflecting the basic PEAD effect.

Specification of regression model

To test the predictions regarding the two proxies, the regression specification in (11) is used.

$$\begin{aligned} Y_{i\tau} = \beta_0 + \beta_1 QEAR_{i\tau} + \beta_2 QEAR_{i\tau} \times QINST_{i\tau} \\ + \beta_3 QEAR_{i\tau} \times QMEDEXP_{i\tau} \\ + \beta_4 QEAR_{i\tau} \times QSIZE_{i\tau} \\ + \beta_5 QEAR_{i\tau} \times QPRICE_{i\tau} \\ + \beta_6 QEAR_{i\tau} \times QVOLUME_{i\tau} \\ + \beta_7 QEAR_{i\tau} \times QARBRISK_{i\tau} \\ + YQ_\tau + FIRM_i + \varepsilon_{i\tau} \end{aligned} \tag{11}$$

[18] Expressed differently, the variables of interest are expected to narrow the gap between the GOOD NEWS and BAD NEWS portfolio drifts, meaning that their impact on positive and negative CARs is not expected to go in the same direction.

The dependent variable, $Y_{i\tau}$, is typically the 60-day CAR, but sometimes the 120-day CAR or the 60-day API, for firm i following the earnings announcement made in quarter τ. QMEDEXP is replaced with QTOTEXP and QEST in some specifications. As described above, the β_1 coefficient is expected to be positive whereas coefficients β_2 through β_6 are expected to be negative. Finally, the β_7 coefficient is expected to be positive.

The regression in (11) is run for the sample of 3,137 earnings observations for different firms over a period ranging from 2004 to 2013. All firms are not included in the sample over the entire sample period as some stocks have entered the public equity market or been delisted throughout the sample period. In addition, some observations have been removed due to potential data errors. Thus, this is a case of unbalanced longitudinal (or panel) data, meaning that unobserved firm-specific effects that are constant over time, denoted $FIRM_i$ in (11), could bias the results if they are not included. For example, following Wooldridge's (2009, p. 489) reasoning, it could be the case that "some units are more likely to drop out of the survey, and this is captured by a_i".[19] It could also be the case that there are time-specific effects – for instance, bullish or bearish market conditions – which are also affecting the results for all firms. These are denoted YQ_τ in (11).

These unobserved firm- and time-specific effects should be included in the regressions. Still, whether to use a random- or a fixed-effects model requires some consideration. As described by Wooldridge (2009), a random-effects model is more efficient than a fixed-effects model when each of the explanatory variables is uncorrelated with the unobserved effects. However, if any of the explanatory variables is correlated with the unobserved effects, the random-effects model causes biased estimations. Conversely, the fixed-effects model, while less efficient than the random-effects model should it be the case that the explanatory variables are uncorrelated with the unobserved effects, always leads to unbiased estimations. Hence, it is the preferred method in this paper. Finally, a Breusch-Pagan test indicates that the regression specification in (11) suffers from heteroskedasticity, meaning that the variance of the error term is not identically distributed throughout the sample. Thus, in all regressions, heteroskedasticity-consistent Huber-White standard errors are used.

4.3 Using the inside proxy to explain the post-earnings-announcement drift

Definitions of variables

To test whether insider trading following earnings announcements leads to a faster drift realization as investors trade upon the potential information contained in the reporting or "flagging" of trades made by presumably knowledgeable insiders, a number of variables are needed. As will be detailed below, the 60-day CARs will be split into different sub-periods to investigate whether PEAD is realized more quickly when insiders trade. The robustness of the results will be tested using the 120-day CAR.

[19] It should be noted that a_i is Wooldridge's (2009) way of denoting unobserved effects. The corresponding notation in (11) is $FIRM_i$.

To investigate the hypothesized relationship, variables capturing surprise and insider trading as well as the interaction between them are needed. However, since the focus is on the signalling value of insider trading – that is, what happens to PEAD once a trade is flagged – and not the exact number of shares traded, variables capturing this qualitative effect are needed. Thus, in line with the approach used by Kolasinski and Li (2011), all variables of interest are defined as binary variables. This not only facilitates comparison to Kolasinski and Li's (2011) results, but also simplifies the interpretation of the coefficients of interest.

Not only insiders, but also their spouses, children and, in some cases, other close relatives are required to flag their transactions. However, for the purpose of this paper, only the actual insiders, including those trading through legal entities, are defined as insiders. As trading by these insiders constitutes the vast majority of all trades flagged, the exclusion of other trades should have no impact on the results. For each flagging, initially quoted as the number of shares flagged, the trading amount is calculated by multiplying the number of shares flagged by the closing share price.

For the 2,395 earnings announcements, the accumulated Swedish krona trading amount over the 25 trading days beginning with the portfolio formation day – which, as discussed above, is two days after the official earnings announcement date – is calculated. Kolasinski and Li (2011) study a 30-calendar-day period and then adds no less than 42 days to allow all insider trades to be flagged. However, this paper uses trading days and – assuming there are 21 trading days each calendar month – thus studies a 21-trading-day period starting the day after the earnings announcement. As the CAR calculation starts from day two following the earnings announcement, the first measure is the 20-day CAR. In addition, five days (rather than 42) are added as that is the number of trading days in which an insider has to flag the transaction in Sweden.

Of all the 2,395 25-day trading amounts in the sample, which could be either positive or negative with a positive (negative) number representing net buying (selling), the top and bottom deciles are classified as net buying and net selling, respectively. The reason for using this relatively narrow definition of what should be regarded as buy and sell signals is that only extensive insider trading is expected to have a signalling value.[20] Two binary variables – BUY and SELL – are generated, taking on the value of one if the accumulated trading amount is classified as a buy and sell transaction, respectively, and zero otherwise.

Furthermore, based on the EARs, two binary variables – POSITIVE and NEGATIVE – are generated, taking on the value of one when the EAR is higher and lower than a positive and negative three per cent, respectively, and zero otherwise. For robustness, two additional binary variables, where the cut-off EARs are instead a positive and negative five per cent, respectively, are also generated. While the binary variables as such are expected to capture the effect on CAR following buy and sell transactions as well as positive and negative surprises, they will not capture any additional effect stemming from the interaction. Thus, the BUY and SELL variables are interacted with the POSITIVE and NEGATIVE variables, resulting in four additional binary variables. All of these binary variables are included in the regressions.

[20] Indeed, the signalling value of an insider buying or selling shares for a small amount of money is most likely limited.

Finally, two control variables measuring the size and book-to-market ratio – SIZE and BTM – are also included in the regressions. SIZE is defined as the natural logarithm of the market capitalization, measured in US dollars at the end of the previous year, and BTM is defined as the book value of equity, measured in US dollars at the end of the previous year, divided by the market capitalization, measured in US dollars at the end of the previous year. These variables are intended to control for the possibility that PEAD is related to either the size of the company or value investing.

Specification of regression model

To test whether insider trading leads to a faster drift realization, the regression specification in (12) is used.

$$\begin{aligned} Y_{i\tau} = \alpha_0 + \alpha_1 BUY_{i\tau} + \alpha_2 POSITIVE_{i\tau} & \\ + \alpha_3 BUY_{i\tau} \times POSITIVE_{i\tau} & \\ + \alpha_4 SELL_{i\tau} + \alpha_5 NEGATIVE_{i\tau} & \\ + \alpha_6 SELL_{i\tau} \times NEGATIVE_{i\tau} & \\ + \alpha_7 BUY_{i\tau} \times NEGATIVE_{i\tau} & \\ + \alpha_8 SELL_{i\tau} \times POSITIVE_{i\tau} & \\ + \alpha_9 SIZE_{i\tau} + \alpha_{10} BTM_{i\tau} & \\ + YQ_\tau + FIRM_i + \varepsilon_{i\tau} & \end{aligned} \tag{12}$$

The dependent variable, $Y_{i\tau}$, alternates in four different setups. More specifically, the 20-day, 25-day and 60-day CARs as well as the CAR over the period ranging from day 26 to 60 are used as regressands to investigate whether insider trading leads to a swifter realization of PEAD. In some robustness tests, the latter two variables are replaced by the 120-day CAR and the CAR over the period from day 26 to 120.

The interpretation of the coefficients is relatively straightforward. For example, the intercept expresses the base-case long-short portfolio CAR should SIZE and BTM (unrealistically) be zero and the observation contain neither a buy or sell transaction nor a positive or negative surprise. Likewise, the effect of buy and sell transactions on the average CAR is captured by the coefficients on these variables, and should it be the case that an observations contains, say, both a positive surprise and a buy transaction, the effect of this is captured by the coefficients on these two variables as well as the corresponding interaction term.

Consequently, as the hypothesis is that insider buying should lead to faster drift realization, the α_3 coefficient, representing the effect on CAR when there is extensive insider buying following a positive surprise, is expected to be positive for the shorter holding periods but negative during the holding period ranging from day 26 to 60. Conversely, the α_6 coefficient is expected to be negative for the shorter holding periods but positive during the period starting on day 26, consistent with the hypothesis that insider trading leads to drift realization already during the beginning of the holding period. However, since Kolasinski and Li (2011) argue that insiders sell shares for many different reasons, only some of which are related to their assessment of the future prospects of the company, it is expected that the effect is less pronounced for insider selling.

Two common issues with unbalanced longitudinal data are unobserved, firm-specific effects which are constant over time and time-specific effects affecting all firms. In (12), these are denoted $FIRM_i$ and YQ_τ, respectively, and to include these in the regressions a fixed-effects regression model is used. This is preferred over a random-effects model as it allows the unobserved effects to be correlated with the explanatory variables without leading to biased estimations. Finally, to avoid incorrect inferences stemming from standard errors biased by heteroskedasticity, all regressions use heteroskedasticity-consistent Huber-White standard errors.

5 EMPIRICAL RESULTS

5.1 Documenting the post-earnings-announcement drift

Descriptive statistics

Panel A of **Table 1** shows that, even though the standard deviation and dispersion of the distributions are relatively large, the average 60-day CAR for the whole sample is a negative 66 basis points whereas the median is a negative 1.29 per cent. The 60-day API and BHAR as well as the 120-day CAR exhibit similar results, indicating a subpar performance in the sample as a whole. A potential explanation for this could be that the sample as a whole is more exposed to negative surprises.

This picture is reinforced by Panels B and C of **Table 1**, which show that when the cut-off EAR values are a positive and negative three per cent there are 876 and 940 observations, respectively. The average 60-day CARs of a positive 1.77 and a negative 1.44 per cent, respectively, indicate an average performance difference between positive and negative surprises in excess of three per cent, suggesting that stocks delivering positive surprises outperform those delivering negative surprises. In addition, the other 60-day measures as well as the 120-day CAR support this picture, warranting further investigation of the statistical significance of these preliminary results.

Table 1: Summary statistics of performance variables

Variable	Mean	Std. dev.	Minimum	25th perc.	Median	75th perc.	Maximum	Obs.
CAR60	-0.0066	0.1890	-1.1472	-0.1064	-0.0129	0.0838	1.8899	3,635
API60	0.9945	0.1993	0.1907	0.8873	0.9783	1.0751	3.5738	3,635
BHAR60	-0.0114	0.1972	-0.9255	-0.1156	-0.0219	0.0775	2.4875	3,635
CAR120	-0.0067	0.3048	-1.7380	-0.1641	-0.0154	0.1324	2.1297	3,635

Panel A: Summary statistics for all observations

Variable	Mean	Std. dev.	Minimum	25th perc.	Median	75th perc.	Maximum	Obs.
CAR60	0.0177	0.1834	-0.7419	-0.0871	0.0043	0.1081	1.3642	876
API60	1.0195	0.2039	0.3757	0.9046	0.9955	1.1038	2.9718	876
BHAR60	0.0146	0.1936	-0.6343	-0.0956	-0.0045	0.1034	1.8160	876
CAR120	0.0245	0.3061	-1.7380	-0.1423	0.0130	0.1585	1.9446	876

Panel B: Summary statistics for observations in the GOOD NEWS portfolio

Variable	Mean	Std. dev.	Minimum	25th perc.	Median	75th perc.	Maximum	Obs.
CAR60	-0.0144	0.2290	-1.1472	-0.1329	-0.0146	0.0900	1.8899	940
API60	0.9879	0.2451	0.1907	0.8606	0.9737	1.0784	3.5738	940
BHAR60	-0.0213	0.2419	-0.8496	-0.1410	-0.0251	0.0795	2.4875	940
CAR120	-0.0031	0.3646	-1.3418	-0.1819	-0.0219	0.1561	2.1297	940

Panel C: Summary statistics for observations in the BAD NEWS portfolio

The table shows summary statistics for the main variables used to measure the performance of the stocks in the sample subsequent to earnings announcements. Panel A shows the summary statistics for the entire sample whereas Panels B and C show the summary statistics for the subsamples of stocks included in the GOOD NEWS and BAD NEWS portfolios, respectively.

Portfolio results

Table 2 shows the performance, in terms of daily ARs and CARs, for the GOOD NEWS and BAD NEWS portfolios as well as the LONG-SHORT portfolio. The first two daily ARs are statistically different from zero for all portfolios. Furthermore, some additional ARs are significantly different from zero.

Table 2: 60-day portfolio CARs

	GOOD NEWS				BAD NEWS				LONG-SHORT			
Event day	AR		CAR		AR		CAR		AR		CAR	
1	0.21%	***	0.21%	***	-0.31%	***	-0.31%	***	0.52%	***	0.52%	***
2	0.20%	**	0.40%	***	-0.24%	***	-0.55%	***	0.43%	***	0.95%	***
3	0.10%		0.50%	***	-0.06%		-0.61%	***	0.16%		1.12%	***
4	0.00%		0.50%	***	0.02%		-0.59%	***	-0.03%		1.09%	***
5	0.02%		0.52%	***	-0.02%		-0.61%	***	0.05%		1.14%	***
6	0.09%		0.61%	***	0.08%		-0.53%	***	0.01%		1.15%	***
7	-0.01%		0.60%	***	0.09%		-0.44%	**	-0.10%		1.04%	***
8	0.08%		0.68%	***	-0.04%		-0.48%	**	0.12%		1.17%	***
9	-0.06%		0.62%	***	0.00%		-0.49%	**	-0.06%		1.11%	***
10	-0.09%		0.53%	**	0.03%		-0.46%	*	-0.12%		0.99%	***
11	0.04%		0.57%	**	-0.10%		-0.55%	**	0.14%		1.13%	***
12	0.00%		0.57%	**	-0.05%		-0.61%	**	0.05%		1.18%	***
13	-0.05%		0.52%	*	-0.07%		-0.68%	**	0.03%		1.21%	***
14	0.11%		0.63%	**	-0.09%		-0.77%	**	0.20%	*	1.40%	***
15	-0.02%		0.62%	**	-0.07%		-0.85%	***	0.06%		1.46%	***
16	0.09%		0.71%	**	-0.08%		-0.93%	***	0.18%		1.64%	***
17	-0.05%		0.66%	**	-0.07%		-1.00%	***	0.02%		1.65%	***
18	0.01%		0.67%	**	-0.11%		-1.10%	***	0.12%		1.77%	***
19	0.05%		0.72%	**	-0.04%		-1.14%	***	0.09%		1.86%	***
20	0.00%		0.72%	**	0.03%		-1.11%	***	-0.03%		1.83%	***
21	-0.01%		0.72%	**	0.16%	*	-0.95%	**	-0.16%		1.67%	***
22	0.02%		0.73%	**	-0.12%		-1.07%	***	0.13%		1.80%	***
23	-0.05%		0.68%	*	0.01%		-1.06%	***	-0.06%		1.74%	***
24	-0.03%		0.66%	*	-0.06%		-1.12%	***	0.04%		1.78%	***
25	0.12%		0.77%	**	-0.01%		-1.13%	***	0.13%		1.90%	***
26	-0.06%		0.71%	*	-0.16%	**	-1.29%	***	0.10%		2.01%	***
27	-0.06%		0.65%		-0.09%		-1.39%	***	0.03%		2.04%	***
28	0.03%		0.68%		0.04%		-1.34%	***	-0.01%		2.03%	***
29	-0.08%		0.60%		-0.18%	**	-1.52%	***	0.10%		2.13%	***
30	-0.07%		0.54%		0.15%	*	-1.37%	***	-0.21%	*	1.91%	***
31	0.00%		0.54%		0.11%		-1.26%	***	-0.11%		1.80%	***
32	0.11%		0.66%		-0.19%	**	-1.46%	***	0.31%	***	2.11%	***
33	-0.12%		0.54%		-0.02%		-1.47%	***	-0.10%		2.01%	***
34	0.07%		0.61%		0.05%		-1.42%	***	0.02%		2.03%	***
35	-0.02%		0.59%		0.21%	***	-1.21%	**	-0.23%	**	1.80%	***
36	0.06%		0.65%		0.01%		-1.20%	**	0.05%		1.85%	***
37	0.02%		0.67%		-0.02%		-1.22%	**	0.04%		1.89%	***
38	0.05%		0.72%		-0.16%	*	-1.38%	***	0.21%	*	2.10%	***
39	0.05%		0.78%		0.02%		-1.36%	***	0.03%		2.13%	***
40	0.01%		0.78%		-0.01%		-1.37%	***	0.02%		2.15%	***

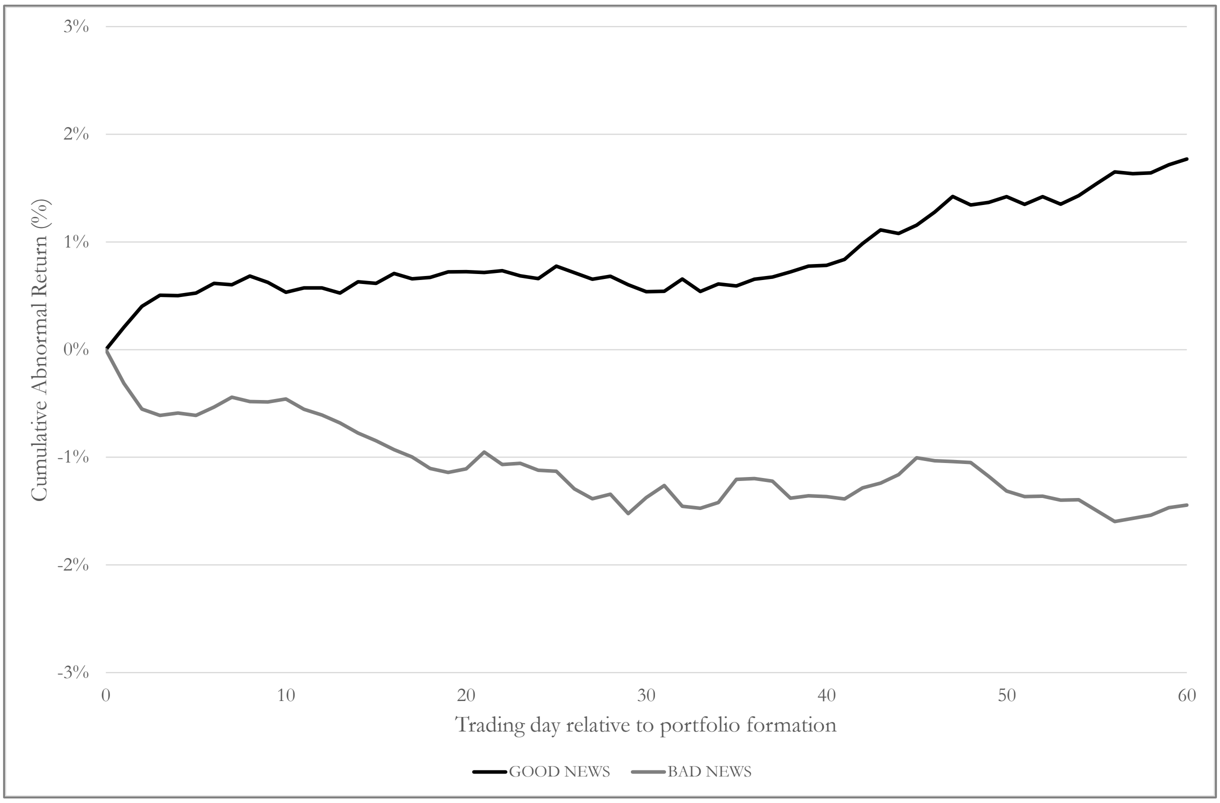
Table 2: 60-day portfolio CARs (cont.)

	GOOD NEWS				GOOD NEWS				LONG-SHORT			
Event day	AR		CAR		AR		CAR		AR		CAR	
41	0.05%		0.84%	*	-0.02%		-1.39%	***	0.08%		2.23%	***
42	0.15%	*	0.99%	*	0.10%		-1.28%	**	0.04%		2.27%	***
43	0.13%		1.11%	**	0.04%		-1.24%	**	0.08%		2.35%	***
44	-0.03%		1.08%	**	0.08%		-1.16%	**	-0.11%		2.24%	***
45	0.08%		1.16%	**	0.15%	*	-1.01%	*	-0.08%		2.16%	***
46	0.12%		1.28%	**	-0.03%		-1.03%	*	0.15%		2.31%	***
47	0.15%	*	1.42%	***	-0.01%		-1.04%	*	0.15%		2.46%	***
48	-0.08%		1.34%	**	-0.01%		-1.05%	*	-0.07%		2.39%	***
49	0.02%		1.37%	**	-0.13%		-1.18%	**	0.15%		2.54%	***
50	0.05%		1.42%	**	-0.13%		-1.31%	**	0.19%	*	2.73%	***
51	-0.07%		1.35%	**	-0.05%		-1.37%	**	-0.02%		2.71%	***
52	0.07%		1.42%	**	0.00%		-1.36%	**	0.07%		2.78%	***
53	-0.07%		1.35%	**	-0.04%		-1.40%	**	-0.03%		2.75%	***
54	0.08%		1.43%	**	0.00%		-1.39%	**	0.07%		2.82%	***
55	0.11%		1.54%	***	-0.10%		-1.50%	**	0.21%	*	3.04%	***
56	0.11%		1.65%	***	-0.10%		-1.60%	***	0.21%	*	3.25%	***
57	-0.02%		1.63%	***	0.03%		-1.57%	**	-0.05%		3.20%	***
58	0.01%		1.64%	***	0.03%		-1.54%	**	-0.02%		3.18%	***
59	0.07%		1.72%	***	0.07%		-1.47%	**	0.01%		3.18%	***
60	0.05%		1.77%	***	0.02%		-1.44%	**	0.03%		3.21%	***

The table shows the abnormal returns (AR) and the cumulative abnormal returns (CAR) over a 60-day holding period for (i) GOOD NEWS, a portfolio of stocks exhibiting positive surprises, (ii) BAD NEWS, a portfolio of stocks exhibiting negative surprises, and (iii) LONG-SHORT, a portfolio long in the GOOD NEWS portfolio and short in the BAD NEWS portfolio. The event days are trading days relative to portfolio formation, which, in turn, is two days after the earnings announcement. The stars *, ** and *** denote statistical significance at the ten, five and one per cent significance levels, respectively.

However, a more interesting feature is that the 60-day CARs are statistically significantly different from zero for both the GOOD NEWS and BAD NEWS portfolios, suggesting a statistically significant abnormal performance in both portfolios. In addition, at a positive 1.77 per cent and a negative 1.44 per cent, respectively, over the 60-day period, this abnormal performance is also economically significant. Likewise, the performance of the LONG-SHORT portfolio is statistically different from zero at the one per cent level, and finishes at a level of 3.21 per cent. For this portfolio, however, the abnormal performance is significant at the one per cent level over the entire holding period, suggesting that abnormal returns can be earned from a portfolio long in stocks exhibiting positive surprises and short in stocks exhibiting negative surprises. As shown in **Figure 2**, this is consistent with the traditional PEAD pattern.

Figure 2: 60-day portfolio CARs



The figure shows the cumulative abnormal return (CAR) over a 60-day holding period for GOOD NEWS, a portfolio of stocks exhibiting positive surprises, and BAD NEWS, a portfolio of stocks exhibiting negative surprises.

Robustness tests

When extending the holding period to 120 trading days, the properties of the drift change somewhat, as shown in **Table C1** and **Figure C1** in **Appendix C**. The GOOD NEWS portfolio continues its upward drift, ending up at a positive 2.45 per cent, significantly different from zero at the one per cent level. The BAD NEWS portfolio, however, exhibits a reversal of the downward pattern, ending up at a statistically and economically insignificant amount of negative 30 basis points. Still, driven by the GOOD NEWS portfolio, the LONG-SHORT portfolio yields a CAR of 2.76 per cent, significant at the five per cent level.

As described in the methodology section, since CAR does not take compounding into account it corresponds to the returns that an investor would actually earn only under certain, quite unrealistic, assumptions. Thus, in order to test whether the drift pattern differs when compounding is taken into account, the performance of the portfolios in terms of API is also investigated. **Table C2** and **Figure C2** in **Appendix C** show that the change to API leaves the results virtually unchanged in quantitative terms. Hence, as MacKinlay's (1997) approach does not allow calculation of test statistics using APIs, calculation of the statistical significance of these virtually identical results using a different approach is deemed unnecessary.

Still, as API also uses quite an unrealistic rebalancing assumption, the performance in terms of BHAR is also investigated. The 60-day BHAR of the GOOD NEWS portfolio is a positive 1.46 per cent, corresponding to an annualized abnormal return in excess of six per cent. This is qualitatively similar to the abnormal return obtained using the CAR and API measures. The corresponding number for the BAD NEWS portfolio is a negative 2.13 per cent, or almost nine per cent on an annualized basis, further supporting the notion of a sizeable drift. In addition, since the API and BHAR results are quantitatively similar to those obtained using CAR, the choice of the CAR measure seems to introduce no severe biases, making it a viable main measure for the rest of the paper.

As discussed in the methodology section, the choice of an EAR cut-off value of three per cent was somewhat arbitrary. However, as shown in Panel A of **Table C3** as well as **Figure C3** in **Appendix C**, varying the cut-off value from one to five per cent does not change the overall result that there is an economically substantial PEAD effect. Even though the downward drift is not statistically significant for some of the lower cut-off values, there seems to be a positive relationship between the cut-off value and the magnitude of the drift; the higher the cut-off value (in absolute terms), the larger the drift.

In addition, using two alternative measures of EAR – those where the market return and a zero return rather than the expected return are used as benchmark returns – it is also shown, in Panel B of **Table C3** and **Figure C4** in **Appendix C**, that the results are qualitatively similar to the main results. However, they are somewhat less pronounced than the main results, most likely because these measures, which do not incorporate expected returns, are somewhat more prone to capture EARs that are not surprising when adjusting properly for risk.

Finally, as the hypothesis regarding investor sophistication is tested using two different, smaller samples, the robustness of the results when changing to these samples is also investigated. The results are shown in Panel C of **Table C3** as well as **Figure C5** and **Figure C6** in **Appendix C**. Using the sample of 3,137 observations used to test the predictions regarding institutional ownership and analyst experience, the magnitude of the results is slightly lower. The GOOD NEWS (BAD NEWS) portfolio yields a positive (negative) 60-day CAR of 1.01 (1.17) per cent, corresponding to a LONG-SHORT portfolio CAR of 2.18 per cent. Even though the 60-day CAR of the GOOD NEWS portfolio is not significant at conventional levels whereas the corresponding BAD NEWS portfolio CAR is significant only at the ten per cent level, the LONG-SHORT 60-day CAR is significant at the five per cent level. Conversely, in the 2,395-observation sample used to test the predictions regarding insider trading, the GOOD NEWS and BAD NEWS portfolio CARs are a positive 2.31 per cent and a negative 2.79 per cent, respectively, both of which are significant at the one per cent level. Consequently, the LONG-SHORT 60-day CAR is a hefty 5.10 per cent, likewise significant at the one per cent level.

5.2 Using buy-side and sell-side proxies to explain post-earnings-announcement drift

Descriptive statistics

Table 3 shows summary statistics for the variables used to test the relationship between the documented drift and investor sophistication, as proxied by institutional ownership and analyst experience. Judging by the standard deviations as well as the minimum and maximum values, several of the variables are quite widely dispersed with some extreme values. For example, the EAR ranges from a negative 39 per cent to a positive 66 per cent, which are indeed quite extreme one-day abnormal returns. Thus, the use of discretely standardized regressors seems appropriate in order to alleviate the concern that results are driven (primarily) by statistical outliers.

Table 3: Summary statistics for regression variables

Variable	Mean	Std. dev.	Minimum	25th perc.	Median	75th perc.	Maximum	Obs.
EAR	-0.16%	6.30%	-39.08%	-3.25%	-0.34%	2.94%	66.16%	3,137
INST	20.31%	12.00%	0.00%	11.82%	20.41%	28.34%	70.10%	3,137
MEDEXP	4.5	3.2	1.0	2.0	4.0	6.0	27.0	3,137
TOTEXP	20.9	26.3	1.0	4.0	10.0	28.0	253.0	3,137
EST	3.7	3.4	1.0	1.0	2.0	5.0	27.0	3,137
SIZE	13.43	1.86	7.90	11.97	13.31	14.81	17.98	3,137
PRICE	101.08	173.21	0.45	35.75	68.00	118.00	3,635.26	3,137
VOLUME	14.86	2.41	7.80	12.98	14.61	16.89	20.65	3,137
ARBRISK	0.000517	0.000525	0.000048	0.000233	0.000371	0.000604	0.006881	3,137

The table shows summary statistics for the main variables used in the regressions testing the relationship between post-earnings-announcement drift and investor sophistication, proxied by institutional ownership and analyst experience.

Regression results

Table 4 shows the main results from regressing the 60-day CAR on QEAR and its interactions with the proxies for investor sophistication, institutional ownership and analyst experience. The size variable as well as the proxies for factors limiting arbitrage – share price, trading volume and arbitrage risk – are added successively to control for these aspects. In all regressions, firm and year-quarter fixed effects are included and heteroskedasticity-consistent Huber-White standard errors are used.

The first column shows the results from a regression of the 60-day CAR on QEAR. Consistent with the documented PEAD effect, the coefficient on QEAR is positive and statistically significant at the five per cent level. In columns (2) to (4), where the QINST and QMEDEXP variables are included one by one and together, the coefficients on these variables are – contrary to what is expected – positive but not significant. However, the statistical significance of the QEAR variable vanishes, at first glance suggesting that there is no PEAD effect once these variables are included in the regression. This would support the notion of investor sophistication as an explanation for PEAD.

Table 4: Regressions of 60-day CAR on investor sophistication variables

Variable	Exp. sign	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
QEAR	+	0.0190 **	0.0123	0.0148	0.0089	0.0557 ***	0.0456 ***	0.0508 ***	0.0415 **	0.1050 ***	0.0959 ***	0.1004 ***	0.0921 ***
		(0.0090)	(0.0132)	(0.0102)	(0.0139)	(0.0148)	(0.0170)	(0.0151)	(0.0172)	(0.0184)	(0.0208)	(0.0187)	(0.0210)
QEAR×QINST	–		0.0133		0.0123		0.0256		0.0244		0.0202		0.0193
			(0.0165)		(0.0165)		(0.0163)		(0.0163)		(0.0164)		(0.0164)
QEAR×QMEDEXP	–			0.0094	0.0087			0.0125	0.0114			0.0106	0.0098
				(0.0100)	(0.0100)			(0.0101)	(0.0101)			(0.0099)	(0.0099)
QEAR×QSIZE	–					-0.0741 ***	-0.0797 ***	-0.0757 ***	-0.0809 ***	-0.0471	-0.0509 *	-0.0481	-0.0516 *
						(0.0192)	(0.0191)	(0.0194)	(0.0192)	(0.0293)	(0.0296)	(0.0295)	(0.0297)
QEAR×QPRICE	–									-0.1270 ***	-0.1263 ***	-0.1267 ***	-0.1261 ***
										(0.0188)	(0.0188)	(0.0187)	(0.0188)
QEAR×QVOLUME	–									0.0342	0.0337	0.0339	0.0335
										(0.0274)	(0.0273)	(0.0274)	(0.0273)
QEAR×QARBRISK	+									-0.0395 ***	-0.0382 ***	-0.0389 ***	-0.0378 ***
										(0.0143)	(0.0143)	(0.0142)	(0.0143)
INTERCEPT		-0.2438 ***	-0.2415 ***	-0.2417 ***	-0.2397 ***	-0.2866 ***	-0.2853 ***	-0.2847 ***	-0.2836 ***	-0.2713 ***	-0.2702 ***	-0.2696 ***	-0.2687 ***
		(0.0204)	(0.0206)	(0.0205)	(0.0207)	(0.0225)	(0.0223)	(0.0224)	(0.0222)	(0.0223)	(0.0221)	(0.0222)	(0.0221)
Firm FE		X	X	X	X	X	X	X	X	X	X	X	X
Year-quarter FE		X	X	X	X	X	X	X	X	X	X	X	X
Observations		3,137	3,137	3,137	3,137	3,137	3,137	3,137	3,137	3,137	3,137	3,137	3,137
R-squared		0.1765	0.1767	0.1767	0.1769	0.1810	0.1817	0.1814	0.1821	0.1975	0.1980	0.1978	0.1982

The table shows the results from the ordinary least squares regressions of the 60-day cumulative abnormal return (CAR) on the earnings announcement return, the proxies for investor sophistication and a host of control variables. QEAR is the quartile portfolios for each quarter of the earnings announcement return (EAR), scaled to range from one third to four thirds, and QINST, QMEDEXP, QSIZE, QPRICE, QVOLUME and QARBRISK are the quartile portfolios for each quarter of the institutional ownership, the median analyst experience, the natural logarithm of the US dollar market capitalization, the share price, the annual US dollar trading volume and the residual variance from a regression of each security's return on those of a market index, respectively, scaled to range from zero to one. The predicted sign is included for each coefficient except the intercept. The table exhibits the regression coefficients, with heteroskedasticity-consistent Huber-White standard errors in parentheses, as well as the number of observations and the R-squared values. The stars *, ** and *** denote statistical significance at the ten, five and one per cent significance levels, respectively.

However, such a conclusion is premature. Rather, the explanation for the results in columns (2) to (4) is most likely an omitted-variable bias. In ordinary least squares regressions, an important assumption is that the error term is uncorrelated with the explanatory variables (Wooldridge, 2009). However, if either QINST or QMEDEXP is correlated with QSIZE – which is not included in the regressions in columns (2) to (4) and, thus, included in the error term – and QSIZE also affects the dependent variable, QSIZE is a confounding factor causing an omitted-variable bias in the regression as well as violating the aforementioned assumption. Thus, failure to include QSIZE in the regression causes biased and inconsistent OLS estimators (Wooldridge, 2009), in turn invalidating any inferences drawn from the regressions in columns (2) to (4).

Hence, QSIZE is included in columns (5) to (8). In all columns, the coefficient on QSIZE has the expected sign and is significant at the one per cent level. In addition, the significance of QEAR reappears, suggesting that even after controlling for QSIZE, there is a significant PEAD effect. The QINST and QMEDEXP variables are still positive, which is inconsistent with the prediction, but insignificant. However, their coefficients, just as the QEAR coefficients, are economically larger in columns (5) to (8), suggesting that the exclusion of QSIZE did indeed cause a downward bias of the other regression coefficients.

Finally, columns (9) to (12) show the results from also including the variables controlling for transaction costs and limits to arbitrage. When these variables are included, the significance of the QSIZE variables evaporates in two of the four regressions in line with Bhushan's (1994) finding that proxies for transaction costs subsume the relationship between size and PEAD. However, even though the increase in the coefficient on QEAR suggests that the models in columns (5) to (8) might still have suffered from omitted-variable biases, the inclusion of the additional control variables does not change the interpretation of the QINST and QMEDEXP variables. Rather, compared to prediction, these variables still have the "wrong" sign but are statistically insignificant, suggesting no clear relationship between PEAD and investor sophistication, at least not when the latter is proxied by institutional ownership and sell-side analyst experience.

Robustness tests

To test the robustness of the above results, the regression specification is altered in a number of ways. First, the dependent variable is changed from the 60-day to the 120-day CAR. These results are shown in **Table C4** in **Appendix C**, and are virtually the same. The slightly higher explanatory power of the model as a whole, as measured by the R-squared, is not associated with higher statistical significance; apart from QEAR becoming slightly less significant in some specifications – but still significant at the one per cent level in the full specification in column (12) – the results are qualitatively similar. The coefficients on the investor sophistication proxies are somewhat higher and still have the wrong sign, but are also still insignificant.

Likewise, the results from using the 60-day API as regressand, shown in **Table C5** in **Appendix C**, are similar. As API centres on one rather than zero, the intercepts have changed dramatically, but the coefficients on all variables except QSIZE – where all coefficients are significant at the ten per cent level – are quantitatively and qualitatively similar to those obtained using the 60-day CAR as regressand. Once again, the coefficients on the variables of interest have the "wrong" sign but are statistically insignificant.

In order to test whether the use of a measure incorporating more information about the experience of analysts changes the results, a regression where QMEDEXP is replaced by QTOTEXP is run. In addition, due to concerns about the reliability of the experience measures arising from the lack of Swedish pre-2004 data in I/B/E/S, regressions where QMEDEXP is replaced by QEST are also run. These regression results are shown in **Table C6** and **Table C7** in **Appendix C**, respectively.

In both specifications, the introduced analyst variable exhibits negative, but far from significant, coefficients in columns (3) and (4). However, as discussed above these specifications are most likely plagued by an omitted-variable bias, and the negative sign on the introduced analyst variables also disappears when controlling for size and the factors limiting arbitrage. Thus, these results – and, indeed, the results of all of the robustness tests – lend further support to the notion of no clear relationship between investor sophistication, proxied by institutional ownership and various measures of analyst experience, and PEAD.

5.3 Using the inside proxy to explain the post-earnings-announcement drift

Descriptive statistics

Panel A of **Table 5** shows that, for the 577 earnings announcements delivering positive surprises, 48 are followed by extensive insider buying whereas 529 are not. After 25 days – that is, when all trades occurring the month following the earnings announcement have been flagged – the former have an average CAR of 2.14 per cent whereas the corresponding number for those earnings announcement that are not followed by extensive insider buying is only 0.88 per cent. Both of these numbers are significantly different from zero at the five per cent level. After 60 days, however, the average CARs are 2.89 and 2.26 per cent, respectively, of which the latter is significant at the one per cent level whereas the former is not significant at conventional levels. Thus, even though there is no statistically significant difference between either the 25-day CARs or the 60-day CARs of the two groups, these results provide some preliminary indications (i) that the drift is higher for those earnings announcements that are followed by extensive insider trading and (ii) that this drift is also, to a larger extent, realized during the earlier part of the holding period.

Similarly, Panel B of **Table 5** shows that of the 614 announcements offering negative surprises, 38 are followed by extensive insider selling whereas 576 are not. For these two groups of stocks, the first 25 days of the holding period yield CARs of a negative 5.64 and 1.15 per cent, respectively, both of which are significant at the one per cent level. After 60 days, the CARs for the two groups are a negative 10.32 and 2.29 per cent, respectively, both of which are likewise significant at the one per cent level. In addition, there is a statistically significant difference between the performance of the two groups after both 25 and 60 days. Thus, even though these results are not indicative of any difference in the speed of the drift realization, they suggest that earnings announcements followed by extensive insider selling yield lower CARs than those where no extensive insider selling takes place. Hence, the results warrant further investigation of the relationship between insider trading and PEAD.

Table 5: Descriptive statistics for performance following insider trading

	25-day CAR						60-day CAR				
	Mean		Std. dev.	Std. err.	Obs.		Mean		Std. dev.	Std. err.	Obs.
Buying	2.14%	**	8.52%	1.23%	48	Buying	2.89%		15.73%	2.27%	48
No buying	0.88%	**	10.20%	0.44%	529	No buying	2.26%	***	19.21%	0.81%	529
Difference	1.27%			1.31%		Difference	0.63%			2.42%	

Panel A: Performance of stocks in which insider buying occurs following positive surprises

	25-day CAR						60-day CAR				
	Mean		Std. dev.	Std. err.	Obs.		Mean		Std. dev.	Std. err.	Obs.
Selling	-5.64%	***	10.63%	1.73%	38	Selling	-10.32%	***	24.57%	3.99%	38
No selling	-1.15%	***	11.88%	0.50%	576	No selling	-2.29%	***	20.95%	0.87%	576
Difference	-4.49%	***		1.79%		Difference	-8.03%	**		4.08%	

Panel B: Performance of stocks in which insider selling occurs following negative surprises

The table shows descriptive statistics for the performance, measured as the 25-day and 60-day cumulative abnormal returns (CAR), of stocks exhibiting positive or negative surprises. Panel A shows the performance of those stocks exhibiting positive surprise, divided into those in which insider buying occurs and does not occur. Panel B shows the performance of those stocks exhibiting negative surprises, divided into those in which insider selling occurs and does not occur. The standard errors are used to test whether the mean CARs are statistically different from zero. The stars *, ** and *** denote statistical significance at the ten, five and one per cent levels, respectively.

Regression results

Table 6 shows the main results from regressing the 20-, 25-, 60- and 25-to-60-day CAR on the surprise and insider trading variables, their interactions and two control variables. In all regressions, firm and year-quarter fixed effects are included and heteroskedasticity-consistent Huber-White standard errors are used.

Columns (1) and (2) show the regressions of the 20- and 25-day CAR on the earnings announcement, insider trading and interaction dummies as well as the size and book-to-market variables. In column (2), the coefficient on BUY×POSITIVE (SELL×NEGATIVE) is expected to be positive (negative), significant and more pronounced than any opposite-direction effect from the BUY (SELL) variable, reflecting an immediate upward (downward) impact of insider trading on CAR after a positive (negative) surprise has occurred. Both coefficients have the predicted sign and magnitude, but are insignificant at conventional levels.

In addition, since the sum of the coefficients on BUY and BUY×POSITIVE reflects the total impact of extensive insider buying on CAR given that a positive surprise has occurred, it is also predicted that the sum of the coefficients on BUY and BUY×POSITIVE should be the same in columns (2) and (3). The reason for this is that, if insider buying does indeed cause a faster drift realization, all drift should be realized already on day 25, meaning that the 60-day drift should not differ from the 25-day drift.[21] The same should be true for the coefficients on SELL and SELL×NEGATIVE. Contrary to this prediction the sums differ markedly for both the buy and sell dummies, and most of them are not statistically significant at conventional levels.

[21] An alternative way to express this condition is that the sum of the coefficients on BUY and BUY×POSITIVE in column (4) is expected to be zero, consistent with no additional drift being realized after day 25.

Table 6: Regressions of 60-day CAR on insider trading variables

Variable	(1) CAR20		(2) CAR25		(3) CAR60		(4) CAR26–60	
BUY	-0.0044		-0.0043		-0.0079		-0.0036	
	(0.0087)		(0.0096)		(0.0173)		(0.0135)	
POSITIVE	0.0080		0.0081		0.0270	***	0.0189	**
	(0.0053)		(0.0060)		(0.0100)		(0.0079)	
BUY×POSITIVE	0.0218		0.0249		0.0372		0.0123	
	(0.0142)		(0.0154)		(0.0288)		(0.0226)	
SELL	-0.0137	*	-0.0173	*	-0.0351	**	-0.0177	
	(0.0078)		(0.0090)		(0.0150)		(0.0119)	
NEGATIVE	-0.0151	***	-0.0155	**	-0.0259	**	-0.0105	
	(0.0053)		(0.0063)		(0.0107)		(0.0080)	
SELL×NEGATIVE	-0.0231		-0.0166		-0.0319		-0.0153	
	(0.0168)		(0.0205)		(0.0415)		(0.0311)	
BUY×NEGATIVE	0.0187		0.0208		0.0465	*	0.0257	
	(0.0146)		(0.0156)		(0.0266)		(0.0200)	
SELL×POSITIVE	0.0157		0.0147		-0.0027		-0.0174	
	(0.0124)		(0.0138)		(0.0238)		(0.0191)	
SIZE	-0.0101		-0.0108		-0.0366	*	-0.0258	*
	(0.0093)		(0.0104)		(0.0198)		(0.0156)	
BTM	0.0261	**	0.0308	**	0.0709	***	0.0400	***
	(0.0110)		(0.0126)		(0.0212)		(0.0136)	
INTERCEPT	0.1134		0.1364		0.5296	*	0.3932	**
	(0.1172)		(0.1380)		(0.2737)		(0.1989)	
Firm FE	X		X		X		X	
Year-quarter FE	X		X		X		X	
Observations	2,395		2,395		2,395		2,395	
R-squared	0.1892		0.1690		0.2430		0.2426	

The table shows the results from the ordinary least squares regressions of the 20-day, 25-day, 60-day and 26-to-60-day cumulative abnormal return (CAR) on the binary surprise and insider trading variables as well as their interactions and size and the book-to-market ratio. BUY and SELL are binary variables taking on the value of one when there is extensive insider buying and selling, respectively, and zero otherwise. POSITIVE and NEGATIVE are binary variables taking on the value of one when there is positive and negative surprises, respectively, and zero otherwise. BUY×POSITIVE, SELL×NEGATIVE, BUY×NEGATIVE and SELL×POSITIVE are interactions of these binary variables. SIZE and BTM are the size and book-to-market ratio for each observation. The table exhibits the regression coefficients, with heteroskedasticity-consistent Huber-White standard errors in parentheses, as well as the number of observations and the R-squared values. The stars *, ** and *** denote statistical significance at the ten, five and one per cent significance levels, respectively.

Even so, there are once again some limited indications of insider trading causing a faster drift realization. The coefficient on POSITIVE (NEGATIVE) has the expected positive (negative) sign, reflecting the basic PEAD effect. More importantly, given that a positive (negative) surprise has occurred, the impact on CAR from extensive insider buying (selling), captured by the coefficients on BUY and BUY×POSITIVE (SELL and SELL×NEGATIVE), is positive (negative), suggesting that insider trading leads to more drift. In addition, at least for extensive insider buying following positive surprises, it seems that the bulk of the drift occurs within 25 days from portfolio formation, whereas the majority of the drift occurs after day 25 if there is no extensive insider trading following a positive surprise.

More specifically, the sum of the coefficients on BUY and BUY×POSITIVE are 0.0206 and 0.0293 over the 25- and 60-day holding periods, respectively, suggesting that about 70 per cent of the drift has occurred after 25 days. Conversely, the 25- and 60-day holding period coefficients on POSITIVE are 0.0081 and 0.0270, respectively, suggesting that when there is no extensive insider buying, only 30 per cent of the drift is realized within 25 days. Still, as these results benefit from no statistical significance, they cannot be viewed as evidence – but only indications – in favour of insider buying leading to a faster drift realization. When extensive insider selling takes place after negative surprises, there is no such indication of a faster drift realization.

Robustness tests

Since the documentation of the PEAD effect indicated a reversal effect in the BAD NEWS portfolio, the robustness to changing from the 60-day to the 120-day CAR is first investigated. The results are in **Table C8** in **Appendix C**. However, as only the regressions in columns (3) and (4) change, the interpretation and magnitude of the coefficient on BUY×POSITIVE remains unchanged. Likewise, the interpretation of the sum of the coefficients on BUY and BUY×POSITIVE as well as SELL and SELL×NEGATIVE remains unchanged; the numbers, however, do not.

The sum of the coefficients on BUY and BUY×POSITIVE is still 0.0206 over the 25-day holding period whereas it is 0.0441 over the 120-day period, corresponding to almost half the drift being realized within 25 days when a positive surprise is followed by extensive insider buying. When there is no extensive insider trading, however, only about one fourth of the total drift is realized after 25 days, as witnessed by the 25-day and 120-day coefficients on POSITIVE. Thus, even though these numbers, too, do not benefit from any statistical significance at conventional levels, they reinforce the indications provided by the main results.

Conversely, for the negative surprises it seems that virtually all drift is realized by day 25 when there is no insider selling whereas only one fifth is realized by day 25 when there is extensive insider selling. This contrasts the prediction of a faster drift realization following insider trading, but also indicates that whereas there is a reversal effect similar to the one in the main sample when there is no insider selling, such reversal does not occur when extensive insider selling follows a negative surprise.

In **Table C9** in **Appendix C**, the EAR cut-off value used to determine what is a positive (negative) surprise is changed to a positive (negative) five per cent in order to test whether the results become more pronounced when a more restrictive cut-off is used. The dependent variables in columns (3) and (4) are the 60-day and 25-to-60-day CARs. The negative coefficient on BUY×POSITIVE in column (4) indicates that, after a positive surprise followed by extensive insider buying, all drift – indeed, too much drift – has been realized after 25 days. Thus, this is an indication in line with the prediction of a faster drift realization, but just as before there is no statistical significance on conventional levels. Conversely, the pattern for negative surprises remains unchanged as it seems that the drift continues after day 25 when negative surprises are followed by extensive insider selling. Interestingly, the statistically significant coefficients on NEGATIVE in columns (2) and (3) also indicate that when the lower EAR cut-off value is used, there is no clear reversal effect.

Finally, **Table C10** in **Appendix C** displays the results from regressions using the entire 120-day holding period and the higher EAR cut-offs used in **Table C9**. In this specification, the sum of the coefficients on BUY and BUY×POSITIVE in column (2) is close to the corresponding sum in column (3), suggesting that when an extremely positive surprise, defined using a high EAR cut-off value, is followed by extensive insider buying, most of the additional drift occurring as a result of the insider buying is realized after 25 days. However, these results benefit from no statistical significance, making them mere indications.

For negative surprises, it once again seems that most of the drift is realized after 25 days when there is no subsequent extensive insider selling. Conversely, when there is such insider selling the drift continues over the entire 120-day period, suggesting that insider selling leads to a prolonged downward drift when following a negative surprise.

6 DISCUSSION

The first aim of this paper is to investigate the post-earnings-announcement drift in the Swedish equity market. The main results presented in the first part of the previous section suggest that PEAD exists in Sweden, and that the drift is both statistically and economically significant. In addition, the effect is robust to changing the way the surprise in the earnings announcement is measured as well as the abnormal performance measure used for the 60-day holding period. When extending the holding period from 60 to 120 days, the GOOD NEWS portfolio continues its upward drift whereas there seems to be a reversal effect for the BAD NEWS portfolio. This reversal effect, however – while interesting in itself – does not change the fact that there seems to be a delayed share price reaction to the earnings announcement, which is inconsistent with semi-strong-form market efficiency. In addition, it does not overthrow a PEAD-based trading strategy.[22]

Yet, there are some limitations to utilizing such a trading strategy. First, although the apparent mispricing might be viewed as an arbitrage opportunity for the investor, such an interpretation is flawed; attempting to profit from the drift is not an arbitrage opportunity in the true sense of the word as it involves risk. Indeed, as can be seen from the summary statistics presented in **Table 1**, the CARs in the sample are quite dispersed, indicating that while a PEAD trader would earn abnormal returns *in expectation*, some of the stocks will generate extremely good returns while others will incur substantial losses. Still, as these extreme returns should offset each other, an investor taking positions in all firms whose earnings announcements are classified as either positive or negative surprises should be able to diversify away much of this risk, making this a less likely explanation for the documented drift.

Second, this paper does not consider transaction costs. Implementing the aforementioned diversified PEAD trading strategy would incur a considerable amount of trading as, on average, roughly half of all earnings announcements in a certain quarter would be classified as either a buy or a sell candidate. In addition, each position would incur two trades; first when taking the position and then when closing it. Even so, given the magnitude of the drift – which, in the main sample, amounts to an annualized BHAR of almost 15 per cent for the LONG-SHORT portfolio – a PEAD-based trading strategy may well be attractive, at least for the professional investor. Furthermore, applying Bernard and Thomas's (1989, 1990) reasoning – that is, that transaction costs could prevent trading from take place but not explain mispricing once trading actually occurs – trading costs also seem unlikely to explain the documented drift.

As neither transaction risk factors nor transaction costs are likely to explain the prevalence of the drift, other explanations have to be considered. To this end, the second aim of this paper is to test the relationship between PEAD and investor sophistication, where the latter is proxied by the proportion of institutional ownership, the degree of sell-side analyst experience and the magnitude of insider trading.

[22] Indeed, the savvy investor could even be expected to profit from the reversal by taking a long position in the BAD NEWS portfolio towards the end of the 120-day holding period.

Of these variables, the proportion of institutional ownership and the degree of analyst experience are both expected to reduce the magnitude of PEAD as these presumably knowledgeable buy-side and sell-side players should be able to detect mispricing or, at the very least, act – directly and by giving advice to investors, respectively – to make pricing in the equity market more efficient. However, no significant relationship between PEAD and the two proxies is found, which is interesting as it is in contrast to prediction as well as evidence from the US equity market. There are several potential factors which could, at least partially, explain these deviating results, three of which are discussed below.

First, the lack of a clear relationship could be due to the quality of the explanatory variables used. More specifically, even though the intention is to use variable definitions similar to those used in previous research on the topic while simultaneously ensuring that they capture what they are supposed to proxy for, some concern regarding the institutional ownership and analyst experience variables may be warranted.

For example, as there is neither consensus on what owners should be considered institutional nor any data on the aggregate institutional ownership in Swedish public companies readily available, such a measure is defined and constructed for the purpose of this paper. Hence, it is possible that this measure does not capture what it purports to capture. For example, some funds classified as institutional investors might apply a passive portfolio strategy tracking an index rather than chasing mispriced securities, meaning that they might not make pricing more efficient. Conversely, the definition of institutional investors used in this paper might not encompass all funds actively chasing mispricing, making the institutional ownership variable a relatively poorer proxy for investor sophistication. Likewise, even though the measure of analyst experience utilized in this paper has been used in previous research, it could be the case that it does not capture the degree to which analysts make pricing more efficient.

Second, it could be the case that while the explanatory variable definitions as such are decent, the data used to calculate the variables is flawed. For example, as discussed in the data section, the ownership data from SIS Ägarservice seems somewhat dubious at times. More importantly, however, the lack of Swedish pre-2004 analyst data is problematic as any experience gained before 2004 is disregarded when calculating the analyst experience measure, which hence becomes biased downwards, at least during the early years of the sample period.

Still, replacing this measure by the number of analysts following a certain company does not change the results in a meaningful way, suggesting that analyst coverage does not affect PEAD. Alternatively, the number of analysts following a company might possibly proxy for the size of a company – indeed, the correlation between the number of analysts following a firm and firm size, as measured by the natural logarithm of the market capitalization, is 0.67 and statistically significant on all conventional levels – meaning that it could still be the case that the lack of pre-2004 data partially explains the lack of a significant relationship between analyst experience and PEAD.

Third, the possibility that the relationship between investor sophistication and PEAD found in previous research is absent in Sweden cannot be ruled out. Indeed, as Setterberg (2011) finds that the PEAD pattern in Sweden differs from that found in previous, mostly US-based studies, the findings in this paper could be viewed as another indication that the Swedish equity market might differ from the one in the US. For example, potential differences between Sweden and the US in terms of ownership concentration or the role of sell-side analysts in financial markets could explain differences between previous research and the present paper when it comes to the relationship between institutional ownership and analyst experience on the one hand and PEAD on the other.

The third proxy for investor sophistication – insider trading – provides some interesting indications of a relationship between investor sophistication and PEAD. First, positive surprises which are followed by extensive insider buying seem to experience a larger total drift than positive surprises which are not followed by extensive insider buying, lending some support to the notion that insiders have superior insight into their companies and are therefore able to spot market underreaction.

Second, most of the drift following positive surprises accompanied by extensive insider buying occurs during within the first 25 days of the holding period, whereas the drift occurring after positive surprises not followed by extensive insider buying occurs to a larger extent during days 26 to 60, suggesting that extensive insider buying might make the drift realization swifter. The latter indication is in line with previous research, which has concluded that insider buying has informational value to the market, thus causing the drift to occur more quickly.

Regarding insider selling, Kolasinski and Li (2011) argue that it should have limited implications since an insider may have numerous reasons for selling shares, most of which are unrelated to potential mispricing of the firm.[23] In this paper, however, there is indicative evidence suggesting otherwise. In particular, negative surprises followed by extensive insider selling exhibit a much more pronounced downward drift, both during the first 25 days and beyond. However, since there is no faster drift realization, it seems that the market – potentially based on arguments similar to those presented by Kolasinski and Li (2011) – does not pay any particular interest to insiders selling following negative surprises.

As discussed above, there is a reversal effect in the downward drift when extending the holding period to 120 days. However, it seems that this reversal does not take place when negative surprises are followed by extensive insider selling; rather, the downward drift continues over the full 120-day holding period. This interpretation, however – as well as those offered above – are constrained by the fact that they benefit from limited or no statistical significance at conventional levels.

[23] For example, an insider may have been allotted shares in an incentive scheme which are sold to free up cash or diversify risk.

The lack of statistical significance is problematic as firm conclusions about the relationship between insider trading and PEAD cannot be comfortably drawn. The relatively small sample of 2,395 observations used in this paper is most likely one factor severely limiting the statistical significance. In contrast, Kolasinski and Li (2011) have a sample size of 182,057, meaning that their sample is approximately 76 times larger. Everything else equal, a sample that much larger corresponds to approximately nine times smaller standard errors and, consequently, nine times larger t-statistics. Hence, such a large sample size presumably makes it easier to obtain statistically significant regression coefficients.

Apart from the sample size limitation, the insider trading data and measures seem to suffer from no major problems. Thus, the measures most likely capture what they are intended to capture – that is, instances of extensive insider trading – meaning that the insider trading measures as well as the results obtained using these measures are highly valid. Likewise, the results documenting the PEAD effect are seemingly valid as there are no apparent concerns regarding the data and variables used. In contrast, as was discussed above there are some concerns regarding the validity of the other two measures of investor sophistication, institutional ownership and analyst experience. More specifically, as there is some uncertainty whether these variables actually measure what they purport to measure, the validity of the results using these measures might be somewhat constrained.

Albeit being of a somewhat limited validity, the results regarding institutional ownership and analyst experience are most likely reliable insofar as another researcher should be able to follow the approach used in this paper and obtain similar results. Likewise, the results documenting PEAD as well as those connecting it to insider trading are considered reliable as they should be fairly easy to replicate. These results are also generalizable as they – although, in the case of the results regarding insider trading, benefiting from limited statistical significance – are in line with previous research, suggesting that they should be relevant, especially to settings similar to the one in Sweden.

In contrast, since the results regarding the connection between institutional ownership and analyst experience on the one hand and PEAD on the other indicate no statistically significant relationship between the two, which is inconsistent with previous research on the US equity market, these results are arguably less generalizable. On the other hand, as there are some indications that the Swedish PEAD pattern differs from the one in the US, similar results could be expected in studies of the PEAD phenomenon and its connection to investor sophistication in settings similar to the Swedish. Nevertheless, while there is a statistically and economically significant and robust PEAD effect in the Swedish equity market as well as some indications of insider trading causing a faster drift realization, it seems that the relatively lower validity of the other two investor sophistication proxies – institutional ownership and analyst experience – leads to results being somewhat less generalizable for these two proxies.

7 CONCLUSION

The first aim of this paper is to provide out-of-sample evidence of Setterberg's (2011) findings of a post-earnings-announcement drift in the Swedish equity market by documenting whether or not there was a drift in Sweden over the time period ranging from 2004 to 2013. Since the drift has been thoroughly documented not only in Sweden but also proven persistent in other countries, it is first hypothesized that there is a drift in the Swedish equity market. Consistent with this hypothesis, this paper provides evidence of a statistically and economically significant as well as robust drift.

Following some of the more promising attempts at explaining the drift in a US setting, the second aim of this paper is to test the relationship between investor sophistication – measured using buy-side, sell-side and inside proxies – and the documented drift. Accordingly, the second hypothesis is that a higher degree of institutional ownership and analyst experience reduces the drift while a larger amount of insider trading subsequent to earnings announcements is predicted to cause a faster drift realization. Whereas neither institutional ownership nor analyst experience has a statistically or economically significant impact on the drift, there are some indications of extensive insider trading leading to a larger, but also quicker, drift. However, as these results benefit from limited or no statistical significance, they are indications rather than evidence.

This paper contributes to the research on post-earnings-announcement drift by using another measure of surprise and sample period than Setterberg (2011), thus providing out-of-sample evidence of the existence of a Swedish drift. Thereby, it supports the notion of a drift persistent across regions and over time. Moreover, it is the first Swedish – and, to the best of the authors' knowledge, European – paper to take a holistic approach by using the same sample to test buy-side, sell-side and inside proxies for investor sophistication.

Finally, some of the results – or, in some cases, the lack of results – presented in this paper are contributions insofar as they differ from findings in previous, US-based research. For instance, the absence of a significant relationship between institutional ownership and analyst experience on the one hand and post-earnings-announcement drift on the other contrasts evidence of such a relationship found in earlier research. Likewise, the documented impact of extensive insider selling on the downward drift seems to be new evidence, suggesting that there might be differences in drift behaviour across countries.

A couple of serendipitous findings in this paper could also provide material for further research. First, when excluding the eight last quarters from the sample to test the predictions regarding insider trading, the drift increased in magnitude, suggesting that it might be worthwhile to investigate whether the properties of the Swedish drift change over time. In addition, given the limited explanatory power of the investor sophistication proxies and the significance of some of the control variables relating to transaction costs and limits to arbitrage, an investigation of the impact of limits to arbitrage on the drift might prove fruitful. Finally, further examination of the indications from the insider trading results, potentially using a larger, European sample, could provide interesting insights into not only its relationship to insider trading, but also the drift more generally, which remains a conundrum.

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APPENDIX A – SAMPLE SELECTION

The 435 stocks that have at some point during the sample period been listed on the Large, Mid or Small Cap lists or the former A or O lists of the Nordic OMX Nordic Stockholm stock exchange have to fulfil a number of additional criteria in order to be included in the final sample. The screening procedure is summarized in **Table A1**.

Table A1: Sample selection

	Observation loss	Remaining observations	Remaining percentage
Stocks listed during 2004-2013		435	100.0
Financial firms	87	348	80.0
Non-primary listings	17	331	76.1
Non-major classes of shares	25	306	70.3
I/B/E/S ticker unavailable from Datastream	11	295	67.8
Data unavailable from I/B/E/S	80	215	49.4
Final sample		215	49.4

The table describes the screening procedure undertaken to arrive at the final sample of firms. The first row shows the starting point for the screening procedure, whereas the next five rows show excluded stocks as well as the remaining sample, both in numbers and as a percentage of the original sample. The last row shows the size of the final sample, both as an absolute number and as a percentage of the original sample.

First, in accordance with earlier PEAD studies, a total of 87 financial firms are excluded from the sample. More specifically, utilizing the Industry Classification Benchmark (ICB) as provided by Thomson Reuters's Datastream database (Datastream), all stocks belonging to the Financials industry, meaning that they have ICB codes starting with 8, are excluded from the sample. This removes companies from the banks, insurance, real estate and financial services sectors, among them Investor and Svenska Handelsbanken. In addition, the real estate company D. Carnegie & Co, reported as an industrial company in Datastream, is removed manually, leaving a sample of 348 stocks.

Second, a total of 17 stocks, including ABB and Astra Zeneca, are excluded from the sample as the listing on Nasdaq OMX Nordic Stockholm is not their primary listing. Third, shares other than ordinary shares, such as preference shares, are removed, and so are the less liquid classes of shares for those companies that have more than one class of shares traded on the Nasdaq OMX Nordic Stockholm stock exchange. This screen removes 25 stocks, among them Eniro's preference share and Scania's class A share, and, together with the screen on primary listings, leaves the sample at 306 stocks.

Fourth, for the list of 306 stocks, the ticker used by Thomson Reuters's Institutional Brokers' Estimate System database (I/B/E/S) is obtained from Datastream. However, the fact that Datastream fails to provide an I/B/E/S ticker for eleven companies reduces the sample to 295 stocks. Finally, the 295 I/B/E/S tickers are used to obtain data on, among other things, earnings announcement dates from I/B/E/S. However, I/B/E/S is unable to provide data for 80 of the 295 stocks, meaning that these stocks cannot be used. Thus, these stocks are removed, leaving a final sample of 215 stocks.

APPENDIX B – EARNINGS ANNOUNCEMENT SCREENING

The 4,142 earnings announcements obtained from Thomson Reuters's Institutional Brokers' Estimate System database (I/B/E/S) have to fulfil a number of additional criteria in order to be included in the final sample of earnings announcements. The screening procedure is summarized in **Table B1**.

Table B1: Earnings announcement screening

	Observation loss	Remaining observations	Remaining percentage
Earnings announcements available from I/B/E/S		4,142	100.0
Earnings announcements in Q3 and Q4 2013	301	3,841	92.7
Pre-announcement return data unavailable	59	3,782	91.3
Two announcements in the same quarter	32	3,750	90.5
Announcements in weekends/holidays	5	3,745	90.4
Actual EPS data unavailable	110	3,635	87.8
Final sample		3,635	87.8

The table describes the screening procedure undertaken to arrive at the final sample of earnings announcements. The first row shows the starting point for the screening procedure, whereas the next five rows show excluded stocks as well as the remaining sample, both in numbers and as a percentage of the original sample. The last row shows the size of the final sample, both as an absolute number and as a percentage of the original sample.

First, as the post-announcement performance of the portfolios of stocks is measured over as much as 120 trading days, all earnings announcements occurring in the third and or fourth quarter of 2013 – 150 and 151 earnings announcements, respectively – are removed. After this adjustment, the sample consists of 3,841 earnings announcements. Second, as the expected return of each security is estimated using the market model – described thoroughly in the methodology section – historical return data is needed. More specifically, as this paper uses 240 days of historical return data to estimate the market model, 59 earnings announcements for which such data is not available are excluded from the sample, reducing the sample size to 3,782 earnings announcements.

Third, there are 31 cases where there are two earnings announcements occurring in the same quarter. Of these, some might be correct as it could be the case that a company publishes more than one interim report in a given calendar quarter, but it could also be the case that these announcements represent data errors. Even if they do not, however, overlapping data should be avoided if possible, as pointed out by MacKinlay (1997). Thus, in 30 of the cases the second earnings announcement is excluded from the sample, whereas both of the earnings announcements are removed in one case where the earnings announcements occur on two consecutive days.[24] Fourth, five of the earnings announcements seem to have occurred at weekends or during holidays, meaning that the earnings announcement return, described more thoroughly in the methodology section, cannot be calculated for these firms. These two adjustments leave the sample at 3,745 earnings announcements.

[24] For Aspiro, earnings seem to have been announced on two consecutive days in May 2010.

Finally, a total of 110 earnings announcements are excluded as I/B/E/S lacks information on actual earnings per share (EPS). Alternatively, since this paper does not use the EPS measure, only the 56 observations where there is no announcement date available could have been removed. However, visual inspection of the 54 observations where there is an announcement date but no actual EPS indicates that about 25 per cent are erroneous. Thus, for consistency and in order to avoid biases arising from haphazard treatment of potentially erroneous data, all of these earnings announcements are excluded. After this final adjustment, the sample comprises 3,635 earnings announcements.

APPENDIX C – ROBUSTNESS TESTS

Table C1: 120-day portfolio CARs

	GOOD NEWS				BAD NEWS				LONG-SHORT			
Event day	AR		CAR		AR		CAR		AR		CAR	
1	0.21%	***	0.21%	***	-0.31%	***	-0.31%	***	0.52%	***	0.52%	***
2	0.20%	**	0.40%	***	-0.24%	***	-0.55%	***	0.43%	***	0.95%	***
3	0.10%		0.50%	***	-0.06%		-0.61%	***	0.16%		1.12%	***
4	0.00%		0.50%	***	0.02%		-0.59%	***	-0.03%		1.09%	***
5	0.02%		0.52%	***	-0.02%		-0.61%	***	0.05%		1.14%	***
6	0.09%		0.61%	***	0.08%		-0.53%	***	0.01%		1.15%	***
7	-0.01%		0.60%	***	0.09%		-0.44%	**	-0.10%		1.04%	***
8	0.08%		0.68%	***	-0.04%		-0.48%	**	0.12%		1.17%	***
9	-0.06%		0.62%	***	0.00%		-0.49%	**	-0.06%		1.11%	***
10	-0.09%		0.53%	**	0.03%		-0.46%	*	-0.12%		0.99%	***
11	0.04%		0.57%	**	-0.10%		-0.55%	**	0.14%		1.13%	***
12	0.00%		0.57%	**	-0.05%		-0.61%	**	0.05%		1.18%	***
13	-0.05%		0.52%	*	-0.07%		-0.68%	**	0.03%		1.21%	***
14	0.11%		0.63%	**	-0.09%		-0.77%	**	0.20%	*	1.40%	***
15	-0.02%		0.62%	**	-0.07%		-0.85%	***	0.06%		1.46%	***
16	0.09%		0.71%	**	-0.08%		-0.93%	***	0.18%		1.64%	***
17	-0.05%		0.66%	**	-0.07%		-1.00%	***	0.02%		1.65%	***
18	0.01%		0.67%	**	-0.11%		-1.10%	***	0.12%		1.77%	***
19	0.05%		0.72%	**	-0.04%		-1.14%	***	0.09%		1.86%	***
20	0.00%		0.72%	**	0.03%		-1.11%	***	-0.03%		1.83%	***
21	-0.01%		0.72%	**	0.16%	*	-0.95%	**	-0.16%		1.67%	***
22	0.02%		0.73%	**	-0.12%		-1.07%	***	0.13%		1.80%	***
23	-0.05%		0.68%	*	0.01%		-1.06%	***	-0.06%		1.74%	***
24	-0.03%		0.66%	*	-0.06%		-1.12%	***	0.04%		1.78%	***
25	0.12%		0.77%	**	-0.01%		-1.13%	***	0.13%		1.90%	***
26	-0.06%		0.71%	*	-0.16%	**	-1.29%	***	0.10%		2.01%	***
27	-0.06%		0.65%		-0.09%		-1.39%	***	0.03%		2.04%	***
28	0.03%		0.68%		0.04%		-1.34%	***	-0.01%		2.03%	***
29	-0.08%		0.60%		-0.18%	**	-1.52%	***	0.10%		2.13%	***
30	-0.07%		0.54%		0.15%	*	-1.37%	***	-0.21%	*	1.91%	***
31	0.00%		0.54%		0.11%		-1.26%	***	-0.11%		1.80%	***
32	0.11%		0.66%		-0.19%	**	-1.46%	***	0.31%	***	2.11%	***
33	-0.12%		0.54%		-0.02%		-1.47%	***	-0.10%		2.01%	***
34	0.07%		0.61%		0.05%		-1.42%	***	0.02%		2.03%	***
35	-0.02%		0.59%		0.21%	***	-1.21%	**	-0.23%	**	1.80%	***
36	0.06%		0.65%		0.01%		-1.20%	**	0.05%		1.85%	***
37	0.02%		0.67%		-0.02%		-1.22%	**	0.04%		1.89%	***
38	0.05%		0.72%		-0.16%	*	-1.38%	***	0.21%	*	2.10%	***
39	0.05%		0.78%		0.02%		-1.36%	***	0.03%		2.13%	***
40	0.01%		0.78%		-0.01%		-1.37%	***	0.02%		2.15%	***

Table C1: 120-day portfolio CARs (cont.)

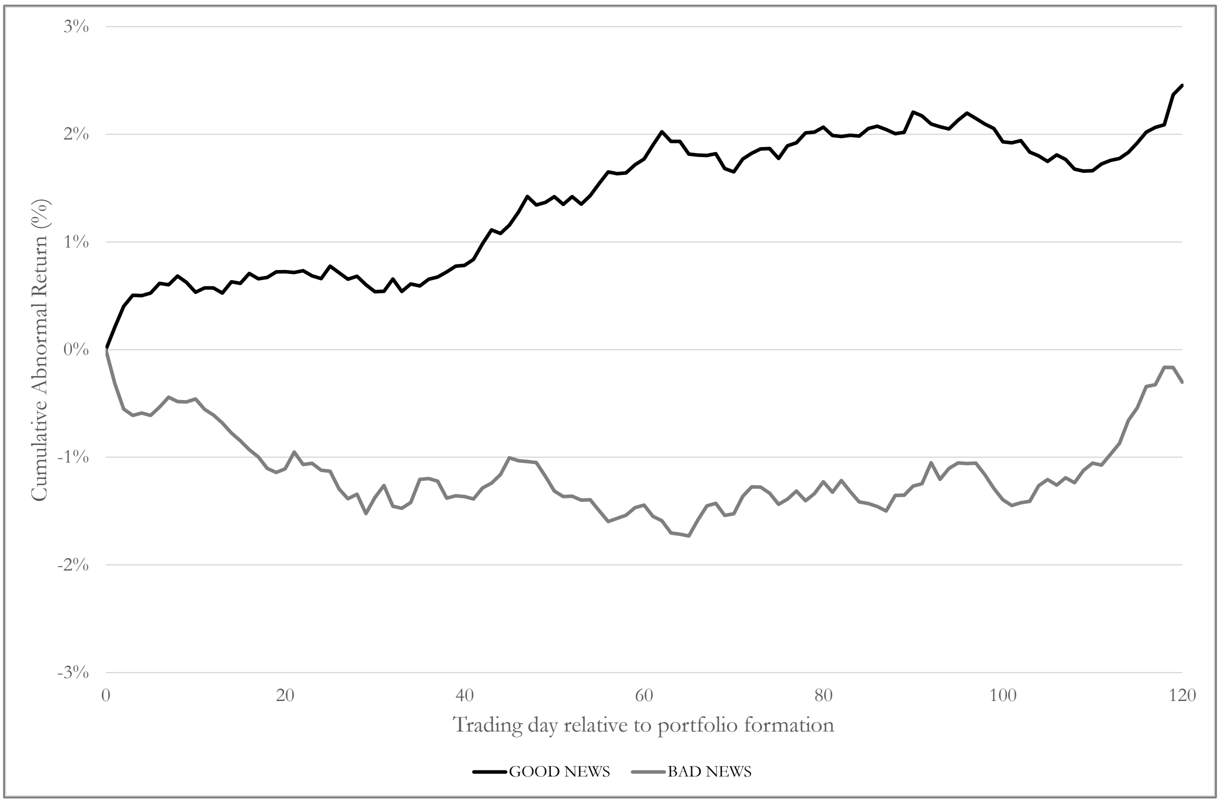
	GOOD NEWS				BAD NEWS				LONG-SHORT			
Event day	AR		CAR		AR		CAR		AR		CAR	
41	0.05%		0.84%	*	-0.02%		-1.39%	***	0.08%		2.23%	***
42	0.15%	*	0.99%	*	0.10%		-1.28%	**	0.04%		2.27%	***
43	0.13%		1.11%	**	0.04%		-1.24%	**	0.08%		2.35%	***
44	-0.03%		1.08%	**	0.08%		-1.16%	**	-0.11%		2.24%	***
45	0.08%		1.16%	**	0.15%	*	-1.01%	*	-0.08%		2.16%	***
46	0.12%		1.28%	**	-0.03%		-1.03%	*	0.15%		2.31%	***
47	0.15%	*	1.42%	***	-0.01%		-1.04%	*	0.15%		2.46%	***
48	-0.08%		1.34%	**	-0.01%		-1.05%	*	-0.07%		2.39%	***
49	0.02%		1.37%	**	-0.13%		-1.18%	**	0.15%		2.54%	***
50	0.05%		1.42%	**	-0.13%		-1.31%	**	0.19%	*	2.73%	***
51	-0.07%		1.35%	**	-0.05%		-1.37%	**	-0.02%		2.71%	***
52	0.07%		1.42%	**	0.00%		-1.36%	**	0.07%		2.78%	***
53	-0.07%		1.35%	**	-0.04%		-1.40%	**	-0.03%		2.75%	***
54	0.08%		1.43%	**	0.00%		-1.39%	**	0.07%		2.82%	***
55	0.11%		1.54%	***	-0.10%		-1.50%	**	0.21%	*	3.04%	***
56	0.11%		1.65%	***	-0.10%		-1.60%	***	0.21%	*	3.25%	***
57	-0.02%		1.63%	***	0.03%		-1.57%	**	-0.05%		3.20%	***
58	0.01%		1.64%	***	0.03%		-1.54%	**	-0.02%		3.18%	***
59	0.07%		1.72%	***	0.07%		-1.47%	**	0.01%		3.18%	***
60	0.05%		1.77%	***	0.02%		-1.44%	**	0.03%		3.21%	***
61	0.13%	*	1.90%	***	-0.10%		-1.55%	**	0.23%	**	3.45%	***
62	0.12%		2.02%	***	-0.04%		-1.59%	**	0.16%		3.61%	***
63	-0.09%		1.93%	***	-0.12%		-1.70%	***	0.03%		3.64%	***
64	0.00%		1.93%	***	-0.01%		-1.71%	***	0.01%		3.65%	***
65	-0.12%		1.82%	***	-0.02%		-1.73%	***	-0.10%		3.55%	***
66	-0.01%		1.81%	***	0.15%	*	-1.58%	**	-0.16%		3.39%	***
67	0.00%		1.80%	***	0.13%		-1.45%	**	-0.14%		3.26%	***
68	0.02%		1.82%	***	0.02%		-1.43%	**	-0.01%		3.25%	***
69	-0.14%	*	1.68%	**	-0.11%		-1.54%	**	-0.02%		3.22%	***
70	-0.03%		1.65%	**	0.02%		-1.53%	**	-0.05%		3.18%	***
71	0.12%		1.77%	***	0.16%	*	-1.36%	**	-0.04%		3.13%	***
72	0.05%		1.82%	***	0.09%		-1.28%	*	-0.04%		3.10%	***
73	0.04%		1.86%	***	0.00%		-1.28%	*	0.04%		3.14%	***
74	0.00%		1.87%	***	-0.06%		-1.33%	*	0.06%		3.20%	***
75	-0.09%		1.78%	***	-0.10%		-1.44%	**	0.01%		3.21%	***
76	0.12%		1.89%	***	0.05%		-1.39%	*	0.07%		3.28%	***
77	0.03%		1.92%	***	0.08%		-1.31%	*	-0.05%		3.23%	***
78	0.09%		2.01%	***	-0.09%		-1.40%	*	0.18%		3.41%	***
79	0.01%		2.02%	***	0.07%		-1.34%	*	-0.06%		3.36%	***
80	0.04%		2.07%	***	0.11%		-1.23%	*	-0.07%		3.29%	***
81	-0.08%		1.99%	***	-0.10%		-1.32%	*	0.02%		3.31%	***
82	-0.01%		1.98%	***	0.11%		-1.22%		-0.12%		3.20%	***
83	0.01%		1.99%	***	-0.10%		-1.32%	*	0.12%		3.31%	***
84	-0.01%		1.98%	***	-0.10%		-1.41%	*	0.09%		3.40%	***
85	0.07%		2.05%	***	-0.01%		-1.43%	*	0.08%		3.48%	***
86	0.02%		2.07%	***	-0.03%		-1.46%	*	0.05%		3.53%	***
87	-0.03%		2.04%	***	-0.04%		-1.50%	*	0.01%		3.54%	***
88	-0.04%		2.00%	***	0.14%	*	-1.35%	*	-0.18%		3.36%	***
89	0.01%		2.02%	***	0.00%		-1.35%	*	0.01%		3.37%	***
90	0.19%	**	2.21%	***	0.09%		-1.27%		0.10%		3.47%	***

Table C1: 120-day portfolio CARs (cont.)

	GOOD NEWS				BAD NEWS				LONG-SHORT			
Event day	AR		CAR		AR		CAR		AR		CAR	
91	-0.04%		2.17%	***	0.02%		-1.25%		-0.06%		3.42%	***
92	-0.08%		2.09%	***	0.20%	**	-1.05%		-0.27%	**	3.14%	***
93	-0.03%		2.07%	***	-0.16%	*	-1.21%		0.13%		3.27%	***
94	-0.02%		2.05%	***	0.10%		-1.10%		-0.12%		3.15%	***
95	0.08%		2.13%	***	0.05%		-1.05%		0.03%		3.18%	***
96	0.07%		2.20%	***	-0.01%		-1.06%		0.07%		3.25%	***
97	-0.05%		2.15%	***	0.00%		-1.05%		-0.05%		3.20%	***
98	-0.05%		2.10%	***	-0.11%		-1.16%		0.05%		3.26%	***
99	-0.04%		2.05%	***	-0.13%		-1.29%		0.09%		3.34%	***
100	-0.12%		1.93%	**	-0.10%		-1.39%	*	-0.02%		3.32%	***
101	-0.01%		1.92%	**	-0.05%		-1.45%	*	0.04%		3.37%	***
102	0.02%		1.94%	**	0.03%		-1.42%	*	0.00%		3.36%	***
103	-0.11%		1.83%	**	0.01%		-1.41%	*	-0.12%		3.24%	***
104	-0.03%		1.80%	**	0.15%	*	-1.26%		-0.18%		3.06%	***
105	-0.05%		1.75%	**	0.06%		-1.21%		-0.11%		2.95%	**
106	0.06%		1.81%	**	-0.05%		-1.26%		0.11%		3.07%	***
107	-0.04%		1.77%	**	0.07%		-1.19%		-0.11%		2.96%	**
108	-0.09%		1.68%	**	-0.05%		-1.24%		-0.05%		2.91%	**
109	-0.02%		1.66%	**	0.11%		-1.12%		-0.13%		2.78%	**
110	0.00%		1.66%	**	0.07%		-1.05%		-0.06%		2.72%	**
111	0.06%		1.72%	**	-0.02%		-1.07%		0.08%		2.80%	**
112	0.03%		1.76%	**	0.10%		-0.97%		-0.07%		2.73%	**
113	0.02%		1.77%	**	0.10%		-0.87%		-0.08%		2.65%	**
114	0.06%		1.83%	**	0.21%	***	-0.66%		-0.16%		2.49%	**
115	0.09%		1.92%	**	0.12%		-0.54%		-0.03%		2.46%	**
116	0.10%		2.02%	**	0.20%	**	-0.34%		-0.10%		2.36%	*
117	0.04%		2.06%	**	0.02%		-0.33%		0.03%		2.39%	*
118	0.02%		2.09%	**	0.16%	*	-0.16%		-0.14%		2.25%	*
119	0.28%	***	2.37%	***	0.00%		-0.17%		0.28%	**	2.53%	**
120	0.09%		2.45%	***	-0.14%		-0.30%		0.22%	*	2.76%	**

The table shows the abnormal returns (AR) and the cumulative abnormal returns (CAR) over a 120-day holding period for (i) GOOD NEWS, a portfolio of stocks exhibiting positive surprises, (ii) BAD NEWS, a portfolio of stocks exhibiting negative surprises, and (iii) LONG-SHORT, a portfolio long in the GOOD NEWS portfolio and short in the BAD NEWS portfolio. The event days are trading days relative to portfolio formation, which, in turn, is two days after the earnings announcement. The stars *, ** and *** denote statistical significance at the ten, five and one per cent significance levels, respectively.

Figure C1: 120-day portfolio CARs



The figure shows the cumulative abnormal return (CAR) over a 120-day holding period for GOOD NEWS, a portfolio of stocks exhibiting positive surprises, and BAD NEWS, a portfolio of stocks exhibiting negative surprises.

Table C2: 60-day portfolio APIs

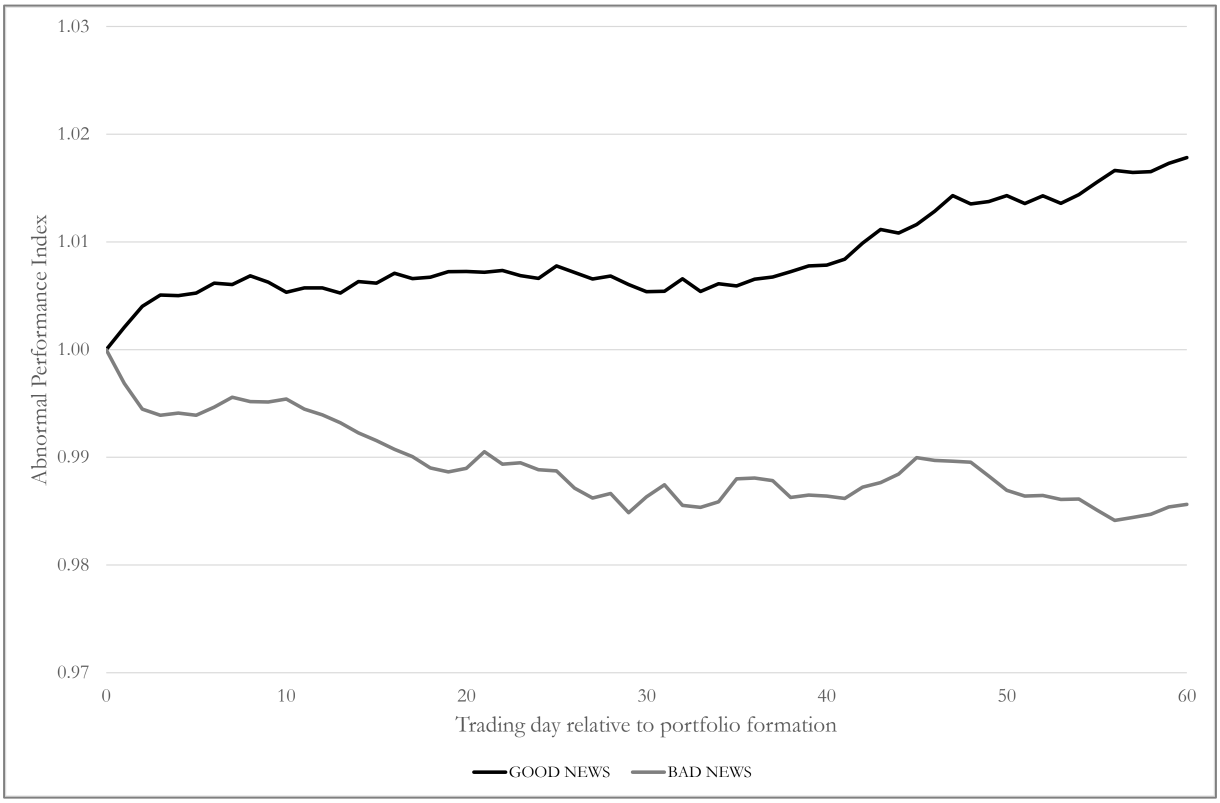
	GOOD NEWS		BAD NEWS		LONG-SHORT	
Event day	AR	API	AR	API	AR	API
1	0.21%	1.0021	-0.31%	0.9969	0.52%	1.0052
2	0.20%	1.0040	-0.24%	0.9945	0.43%	1.0096
3	0.10%	1.0051	-0.06%	0.9939	0.16%	1.0112
4	0.00%	1.0050	0.02%	0.9941	-0.03%	1.0109
5	0.02%	1.0053	-0.02%	0.9939	0.05%	1.0114
6	0.09%	1.0062	0.08%	0.9947	0.01%	1.0115
7	-0.01%	1.0060	0.09%	0.9956	-0.10%	1.0105
8	0.08%	1.0069	-0.04%	0.9952	0.12%	1.0117
9	-0.06%	1.0063	0.00%	0.9951	-0.06%	1.0111
10	-0.09%	1.0053	0.03%	0.9954	-0.12%	1.0099
11	0.04%	1.0057	-0.10%	0.9945	0.14%	1.0113
12	0.00%	1.0057	-0.05%	0.9939	0.05%	1.0118
13	-0.05%	1.0053	-0.07%	0.9932	0.03%	1.0121
14	0.11%	1.0063	-0.09%	0.9923	0.20%	1.0141
15	-0.02%	1.0062	-0.07%	0.9916	0.06%	1.0147
16	0.09%	1.0071	-0.08%	0.9907	0.18%	1.0165
17	-0.05%	1.0066	-0.07%	0.9901	0.02%	1.0166
18	0.01%	1.0067	-0.11%	0.9890	0.12%	1.0179
19	0.05%	1.0072	-0.04%	0.9887	0.09%	1.0188
20	0.00%	1.0072	0.03%	0.9890	-0.03%	1.0184

Table C2: 60-day portfolio APIs (cont.)

	GOOD NEWS		BAD NEWS		LONG-SHORT	
Event day	AR	API	AR	API	AR	API
21	-0.01%	1.0072	0.16%	0.9905	-0.16%	1.0168
22	0.02%	1.0073	-0.12%	0.9894	0.13%	1.0181
23	-0.05%	1.0069	0.01%	0.9895	-0.06%	1.0175
24	-0.03%	1.0066	-0.06%	0.9888	0.04%	1.0179
25	0.12%	1.0078	-0.01%	0.9887	0.13%	1.0192
26	-0.06%	1.0072	-0.16%	0.9871	0.10%	1.0203
27	-0.06%	1.0066	-0.09%	0.9862	0.03%	1.0206
28	0.03%	1.0068	0.04%	0.9866	-0.01%	1.0204
29	-0.08%	1.0060	-0.18%	0.9849	0.10%	1.0215
30	-0.07%	1.0054	0.15%	0.9863	-0.21%	1.0193
31	0.00%	1.0054	0.11%	0.9874	-0.11%	1.0181
32	0.11%	1.0066	-0.19%	0.9855	0.31%	1.0213
33	-0.12%	1.0054	-0.02%	0.9854	-0.10%	1.0203
34	0.07%	1.0061	0.05%	0.9859	0.02%	1.0205
35	-0.02%	1.0059	0.21%	0.9880	-0.23%	1.0181
36	0.06%	1.0065	0.01%	0.9881	0.05%	1.0186
37	0.02%	1.0067	-0.02%	0.9878	0.04%	1.0191
38	0.05%	1.0072	-0.16%	0.9863	0.21%	1.0212
39	0.05%	1.0078	0.02%	0.9865	0.03%	1.0215
40	0.01%	1.0078	-0.01%	0.9864	0.02%	1.0217
41	0.05%	1.0084	-0.02%	0.9862	0.08%	1.0225
42	0.15%	1.0099	0.10%	0.9872	0.04%	1.0229
43	0.13%	1.0112	0.04%	0.9877	0.08%	1.0237
44	-0.03%	1.0108	0.08%	0.9884	-0.11%	1.0226
45	0.08%	1.0116	0.15%	0.9900	-0.08%	1.0218
46	0.12%	1.0128	-0.03%	0.9897	0.15%	1.0233
47	0.15%	1.0143	-0.01%	0.9896	0.15%	1.0249
48	-0.08%	1.0135	-0.01%	0.9895	-0.07%	1.0242
49	0.02%	1.0137	-0.13%	0.9883	0.15%	1.0257
50	0.05%	1.0143	-0.13%	0.9869	0.19%	1.0277
51	-0.07%	1.0136	-0.05%	0.9864	-0.02%	1.0275
52	0.07%	1.0143	0.00%	0.9865	0.07%	1.0281
53	-0.07%	1.0136	-0.04%	0.9861	-0.03%	1.0278
54	0.08%	1.0144	0.00%	0.9861	0.07%	1.0286
55	0.11%	1.0155	-0.10%	0.9851	0.21%	1.0308
56	0.11%	1.0166	-0.10%	0.9841	0.21%	1.0329
57	-0.02%	1.0165	0.03%	0.9844	-0.05%	1.0325
58	0.01%	1.0165	0.03%	0.9847	-0.02%	1.0322
59	0.07%	1.0173	0.07%	0.9854	0.01%	1.0323
60	0.05%	1.0178	0.02%	0.9856	0.03%	1.0326

The table shows the abnormal returns (AR) and the abnormal performance indices (API) over a 60-day holding period for (i) GOOD NEWS, a portfolio of stocks exhibiting positive surprises, (ii) BAD NEWS, a portfolio of stocks exhibiting negative surprises, and (iii) LONG-SHORT, a portfolio long in the GOOD NEWS portfolio and short in the BAD NEWS portfolio. The event days are trading days relative to portfolio formation, which, in turn, is two days after the earnings announcement.

Figure C2: 60-day portfolio APIs



The figure shows the abnormal performance index (API) over a 60-day holding period for GOOD NEWS, a portfolio of stocks exhibiting positive surprises, and BAD NEWS, a portfolio of stocks exhibiting negative surprises.

Table C3: Additional robustness tests

EAR cut-off	Expected return	Obs.	GOOD NEWS		BAD NEWS		LONG-SHORT	
± 1%	Market model	3,635	0.98%	**	-1.07%	**	2.06%	***
± 2%	Market model	3,635	1.34%	**	-1.22%	**	2.56%	***
± 3%	Market model	3,635	1.77%	***	-1.44%	**	3.21%	***
± 4%	Market model	3,635	2.15%	***	-0.91%		3.05%	***
± 5%	Market model	3,635	3.40%	***	-0.97%		4.37%	***

Panel A: Robustness to varying the EAR cut-off value

EAR cut-off	Expected return	Obs.	GOOD NEWS		BAD NEWS		LONG-SHORT	
± 3%	Market model	3,635	1.77%	***	-1.44%	**	3.21%	***
± 3%	Market return	3,635	1.33%	**	-0.79%		2.13%	**
± 3%	Zero return	3,635	1.57%	***	-1.29%	**	2.86%	***

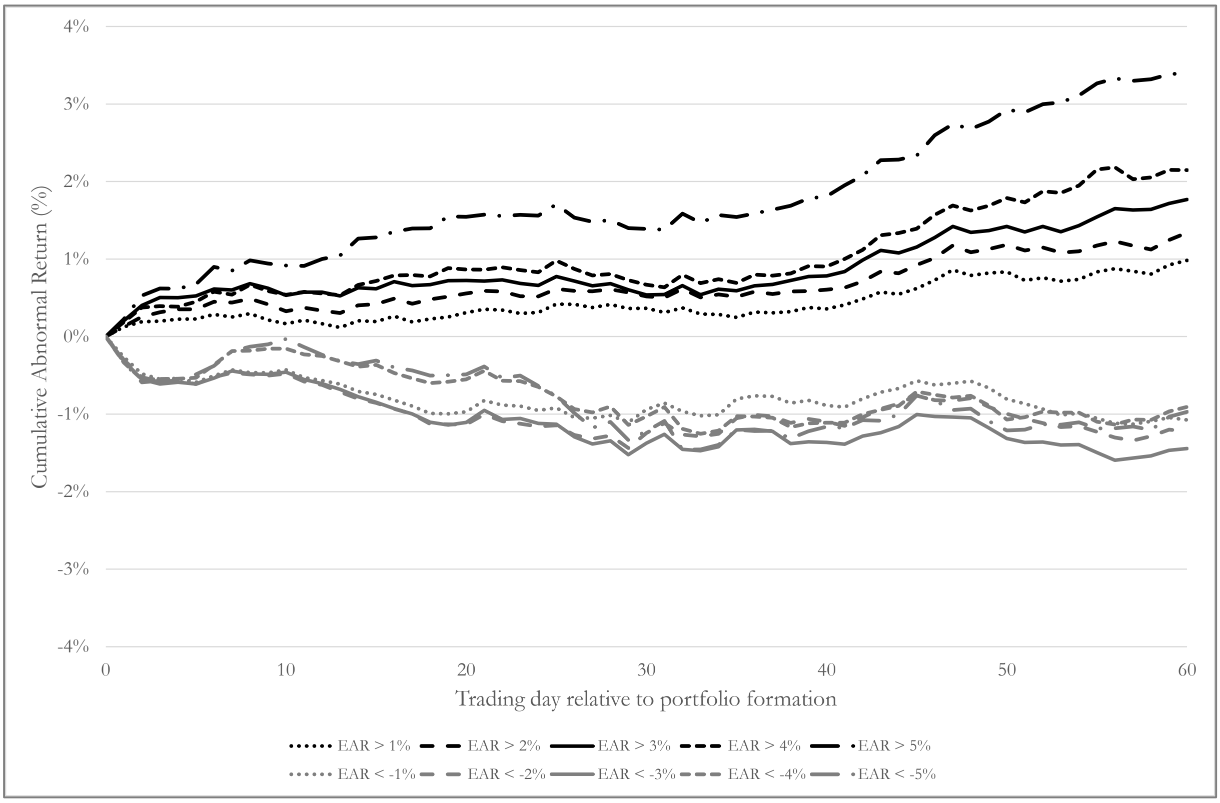
Panel B: Robustness to varying the expected return in the EAR calculation

EAR cut-off	Expected return	Obs.	GOOD NEWS		BAD NEWS		LONG-SHORT	
± 3%	Market model	3,635	1.77%	***	-1.44%	**	3.21%	***
± 3%	Market model	3,137	1.01%		-1.17%	*	2.18%	**
± 3%	Market model	2,395	2.31%	***	-2.79%	***	5.10%	***

Panel C: Robustness to varying the sample used

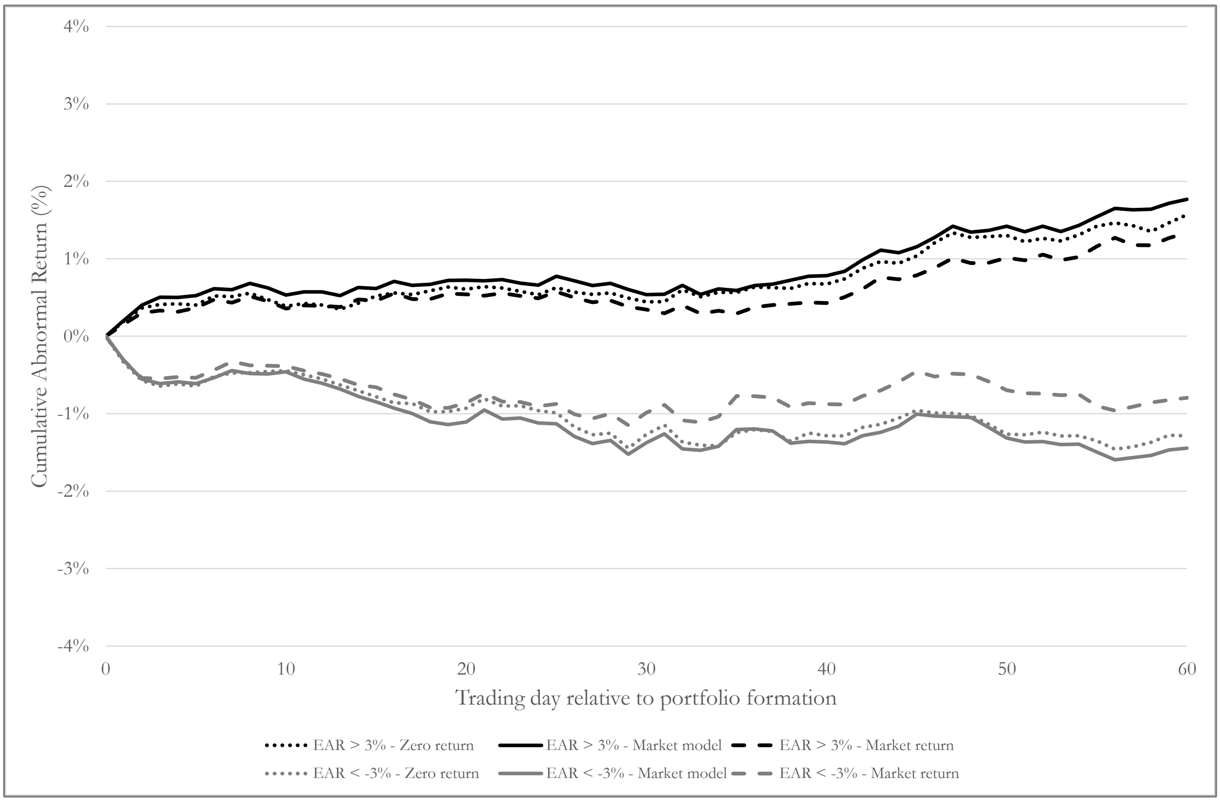
The table shows a number of robustness tests. Panel A shows the performance of the three portfolios, measured as cumulative abnormal returns (CAR), when changing the earnings announcement return (EAR) cut-off values. Panel B shows the performance, measured as CAR, when changing the way the expected return on the day of the earnings announcement is calculated. Panel C shows the performance of the three portfolios, measured as CAR, when changing the sample to the two samples used to test the hypothesis regarding the connection between investor sophistication and post-earnings-announcement drift. The stars *, ** and *** denote statistical significance at the ten, five and one per cent significance levels, respectively.

Figure C3: 60-day portfolio CARs with different EAR cut-offs



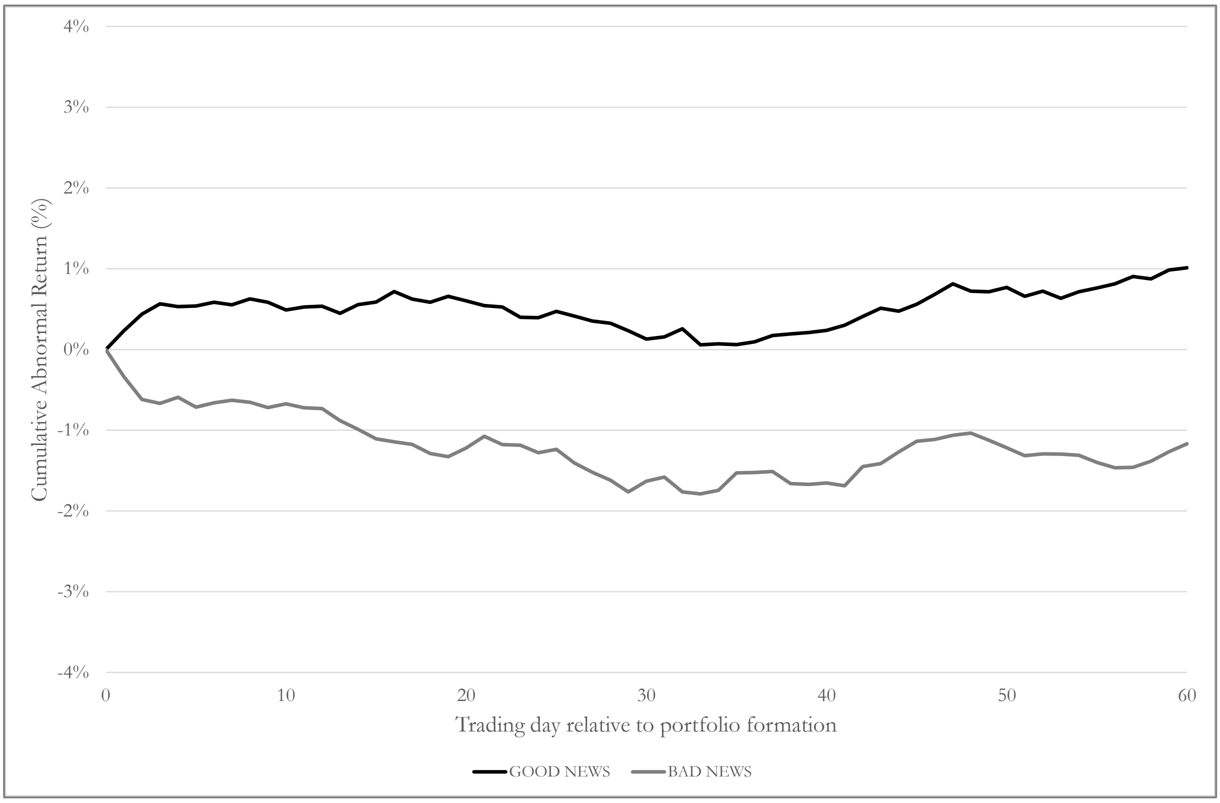
The figure shows the cumulative abnormal return (CAR) over a 60-day holding period for portfolios constructed on the basis of the earnings announcement return (EAR), with cut-off values ranging from ±1 per cent to ±5 per cent.

Figure C4: 60-day portfolio CARs with EARs based on different expected returns



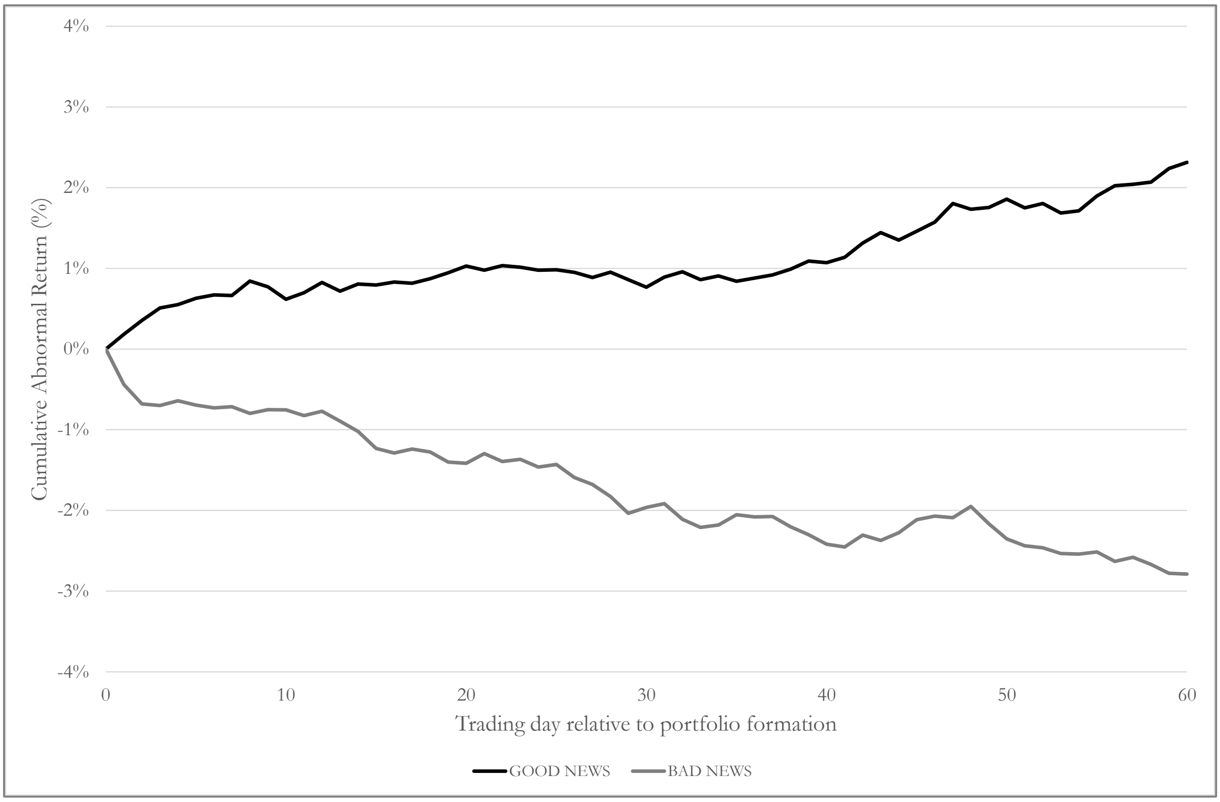
The figure shows the cumulative abnormal return (CAR) over a 60-day holding period for portfolios constructed on the basis of the earnings announcement return (EAR). For all portfolios, the EAR cut-off value is ±3 per cent, but the expected return differs between (i) a zero return, (ii) the market model return and (iii) the market return.

Figure C5: 60-day portfolio CARs for institutional ownership and analyst experience sample



The figure shows the cumulative abnormal return (CAR) over a 60-day holding period for GOOD NEWS, a portfolio of stocks exhibiting positive surprises, and BAD NEWS, a portfolio of stocks exhibiting negative surprises, for the sample of 3,137 observations used to test the predictions regarding institutional ownership and analyst experience.

Figure C6: 60-day portfolio CARs for insider trading sample



The figure shows the cumulative abnormal return (CAR) over a 60-day holding period for GOOD NEWS, a portfolio of stocks exhibiting positive surprises, and BAD NEWS, a portfolio of stocks exhibiting negative surprises, for the sample of 2,395 observations used to test the predictions regarding insider trading.

Table C4: Regressions of 120-day CAR on investor sophistication variables

Variable	Exp. sign	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
QEAR	+	0.0157	0.0047	0.0118	0.0018	0.0753 ***	0.0587 **	0.0700 ***	0.0546 *	0.1731 ***	0.1592 ***	0.1689 ***	0.1559 ***
		(0.0143)	(0.0212)	(0.0163)	(0.0223)	(0.0246)	(0.0281)	(0.0251)	(0.0285)	(0.0299)	(0.0336)	(0.0302)	(0.0339)
QEAR×QINST	–		0.0218		0.0209		0.0418		0.0406		0.0312		0.0304
			(0.0261)		(0.0261)		(0.0257)		(0.0257)		(0.0254)		(0.0254)
QEAR×QMEDEXP	–			0.0085	0.0073			0.0135	0.0117			0.0098	0.0085
				(0.0157)	(0.0157)			(0.0158)	(0.0157)			(0.0152)	(0.0151)
QEAR×QSIZE	–					-0.1204 ***	-0.1295 ***	-0.1222 ***	-0.1308 ***	-0.0257	-0.0315	-0.0266	-0.0322
						(0.0319)	(0.0315)	(0.0321)	(0.0317)	(0.0434)	(0.0436)	(0.0436)	(0.0437)
QEAR×QPRICE	–									-0.2997 ***	-0.2987 ***	-0.2995 ***	-0.2985 ***
										(0.0283)	(0.0283)	(0.0283)	(0.0283)
QEAR×QVOLUME	–									0.0570	0.0563	0.0568	0.0561
										(0.0413)	(0.0412)	(0.0413)	(0.0412)
QEAR×QARBRISK	+									-0.0638 ***	-0.0618 ***	-0.0633 ***	-0.0614 ***
										(0.0230)	(0.0230)	(0.0229)	(0.0229)
INTERCEPT		-0.4992 ***	-0.4953 ***	-0.4972 ***	-0.4938 ***	-0.5687 ***	-0.5665 ***	-0.5666 ***	-0.5648 ***	-0.5319 ***	-0.5302 ***	-0.5303 ***	-0.5289 ***
		(0.0310)	(0.0314)	(0.0312)	(0.0316)	(0.0363)	(0.0362)	(0.0363)	(0.0362)	(0.0341)	(0.0340)	(0.0341)	(0.0340)
Firm FE		X	X	X	X	X	X	X	X	X	X	X	X
Year-quarter FE		X	X	X	X	X	X	X	X	X	X	X	X
Observations		3,137	3,137	3,137	3,137	3,137	3,137	3,137	3,137	3,137	3,137	3,137	3,137
R-squared		0.1971	0.1973	0.1972	0.1974	0.2018	0.2025	0.2019	0.2027	0.2359	0.2363	0.2360	0.2364

The table shows the results from the ordinary least squares regressions of the 120-day cumulative abnormal return (CAR) on the earnings announcement return, the proxies for investor sophistication and a host of control variables. QEAR is the quartile portfolios for each quarter of the earnings announcement return (EAR), scaled to range from one third to four thirds, and QINST, QMEDEXP, QSIZE, QPRICE, QVOLUME and QARBRISK are the quartile portfolios for each quarter of the institutional ownership, the median analyst experience, the natural logarithm of the US dollar market capitalization, the share price, the annual US dollar trading volume and the residual variance from a regression of each security's return on those of a market index, respectively, scaled to range from zero to one. The predicted sign is included for each coefficient except the intercept. The table exhibits the regression coefficients, with heteroskedasticity-consistent Huber-White standard errors in parentheses, as well as the number of observations and the R-squared values. The stars *, ** and *** denote statistical significance at the ten, five and one per cent significance levels, respectively.

Table C5: Regressions of 60-day API on investor sophistication variables

Variable	Exp. sign	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
QEAR	+	0.0186 *	0.0117	0.0131	0.0072	0.0598 ***	0.0489 **	0.0537 ***	0.0438 **	0.1064 ***	0.0965 ***	0.1004 ***	0.0915 ***
		(0.0098)	(0.0149)	(0.0111)	(0.0156)	(0.0164)	(0.0190)	(0.0166)	(0.0192)	(0.0201)	(0.0232)	(0.0205)	(0.0235)
QEAR×QINST	–		0.0137		0.0123		0.0274		0.0260		0.0222		0.0210
			(0.0187)		(0.0187)		(0.0186)		(0.0185)		(0.0187)		(0.0186)
QEAR×QMEDEXP	–			0.0122	0.0115			0.0157	0.0145			0.0138	0.0129
				(0.0105)	(0.0105)			(0.0106)	(0.0106)			(0.0104)	(0.0104)
QEAR×QSIZE	–					-0.0832 ***	-0.0892 ***	-0.0852 ***	-0.0907 ***	-0.0587 *	-0.0629 *	-0.0601 *	-0.0639 **
						(0.0207)	(0.0206)	(0.0209)	(0.0208)	(0.0315)	(0.0321)	(0.0317)	(0.0322)
QEAR×QPRICE	–									-0.1270 ***	-0.1263 ***	-0.1267 ***	-0.1261 ***
										(0.0215)	(0.0216)	(0.0215)	(0.0216)
QEAR×QVOLUME	–									0.0396	0.0391	0.0393	0.0388
										(0.0294)	(0.0293)	(0.0294)	(0.0293)
QEAR×QARBRISK	+									-0.0370 **	-0.0356 **	-0.0363 **	-0.0350 **
										(0.0148)	(0.0148)	(0.0148)	(0.0148)
INTERCEPT		0.7782 ***	0.7807 ***	0.7810 ***	0.7830 ***	0.7302 ***	0.7316 ***	0.7326 ***	0.7337 ***	0.7471 ***	0.7483 ***	0.7492 ***	0.7502 ***
		(0.0213)	(0.0216)	(0.0214)	(0.0217)	(0.0236)	(0.0235)	(0.0235)	(0.0233)	(0.0233)	(0.0232)	(0.0232)	(0.0231)
Firm FE		X	X	X	X	X	X	X	X	X	X	X	X
Year-quarter FE		X	X	X	X	X	X	X	X	X	X	X	X
Observations		3,137	3,137	3,137	3,137	3,137	3,137	3,137	3,137	3,137	3,137	3,137	3,137
R-squared		0.1771	0.1773	0.1774	0.1776	0.1822	0.1829	0.1827	0.1834	0.1968	0.1973	0.1973	0.1977

The table shows the results from the ordinary least squares regressions of the 60-day abnormal performance index (API) on the earnings announcement return, the proxies for investor sophistication and a host of control variables. QEAR is the quartile portfolios for each quarter of the earnings announcement return (EAR), scaled to range from one third to four thirds, and QINST, QMEDEXP, QSIZE, QPRICE, QVOLUME and QARBRISK are the quartile portfolios for each quarter of the institutional ownership, the median analyst experience, the natural logarithm of the US dollar market capitalization, the share price, the annual US dollar trading volume and the residual variance from a regression of each security's return on those of a market index, respectively, scaled to range from zero to one. The predicted sign is included for each coefficient except the intercept. The table exhibits the regression coefficients, with heteroskedasticity-consistent Huber-White standard errors in parentheses, as well as the number of observations and the R-squared values. The stars *, ** and *** denote statistical significance at the ten, five and one per cent significance levels, respectively.

Table C6: Regressions of 60-day CAR on investor sophistication variables – changing QMEDEXP to QTOTEXP

Variable	Exp. sign	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
QEAR	+	0.0190 **	0.0123	0.0223 *	0.0158	0.0557 ***	0.0456 ***	0.0512 ***	0.0421 **	0.1050 ***	0.0959 ***	0.1007 ***	0.0926 ***
		(0.0090)	(0.0132)	(0.0118)	(0.0148)	(0.0148)	(0.0170)	(0.0148)	(0.0170)	(0.0184)	(0.0208)	(0.0185)	(0.0209)
QEAR×QINST	–		0.0133		0.0148		0.0256		0.0240		0.0202		0.0189
			(0.0165)		(0.0165)		(0.0163)		(0.0163)		(0.0164)		(0.0164)
QEAR×QTOTEXP	–			-0.0068	-0.0089			0.0219	0.0199			0.0193	0.0178
				(0.0141)	(0.0141)			(0.0164)	(0.0164)			(0.0164)	(0.0164)
QEAR×QSIZE	–					-0.0741 ***	-0.0797 ***	-0.0862 ***	-0.0903 ***	-0.0471	-0.0509 *	-0.0524 *	-0.0555 *
						(0.0192)	(0.0191)	(0.0224)	(0.0223)	(0.0293)	(0.0296)	(0.0302)	(0.0304)
QEAR×QPRICE	–									-0.1270 ***	-0.1263 ***	-0.1276 ***	-0.1269 ***
										(0.0188)	(0.0188)	(0.0187)	(0.0188)
QEAR×QVOLUME	–									0.0342	0.0337	0.0283	0.0283
										(0.0274)	(0.0273)	(0.0277)	(0.0276)
QEAR×QARBRISK	+									-0.0395 ***	-0.0382 ***	-0.0378 ***	-0.0367 **
										(0.0143)	(0.0143)	(0.0144)	(0.0144)
INTERCEPT		-0.2438 ***	-0.2415 ***	-0.2476 ***	-0.2461 ***	-0.2866 ***	-0.2853 ***	-0.2817 ***	-0.2809 ***	-0.2713 ***	-0.2702 ***	-0.2677 ***	-0.2669 ***
		(0.0204)	(0.0206)	(0.0219)	(0.0219)	(0.0225)	(0.0223)	(0.0226)	(0.0224)	(0.0223)	(0.0221)	(0.0223)	(0.0222)
Firm FE		X	X	X	X	X	X	X	X	X	X	X	X
Year-quarter FE		X	X	X	X	X	X	X	X	X	X	X	X
Observations		3,137	3,137	3,137	3,137	3,137	3,137	3,137	3,137	3,137	3,137	3,137	3,137
R-squared		0.1765	0.1767	0.1766	0.1768	0.1810	0.1817	0.1815	0.1822	0.1975	0.1980	0.1979	0.1983

The table shows the results from the ordinary least squares regressions of the 60-day cumulative abnormal return (CAR) on the earnings announcement return, the proxies for investor sophistication and a host of control variables. QEAR is the quartile portfolios for each quarter of the earnings announcement return (EAR), scaled to range from one third to four thirds, and QINST, QTOTEXP, QSIZE, QPRICE, QVOLUME and QARBRISK are the quartile portfolios for each quarter of the institutional ownership, the total analyst experience, the natural logarithm of the US dollar market capitalization, the share price, the annual US dollar trading volume and the residual variance from a regression of each security's return on those of a market index, respectively, scaled to range from zero to one. The predicted sign is included for each coefficient except the intercept. The table exhibits the regression coefficients, with heteroskedasticity-consistent Huber-White standard errors in parentheses, as well as the number of observations and the R-squared values. The stars *, ** and *** denote statistical significance at the ten, five and one per cent significance levels, respectively.

Table C7: Regressions of 60-day CAR on investor sophistication variables – changing QMEDEXP to QEST

Variable	Exp. sign	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
QEAR	+	0.0190 **	0.0123	0.0244 **	0.0174	0.0557 ***	0.0456 ***	0.0536 ***	0.0442 ***	0.1050 ***	0.0959 ***	0.1031 ***	0.0948 ***
		(0.0090)	(0.0132)	(0.0116)	(0.0147)	(0.0148)	(0.0170)	(0.0149)	(0.0171)	(0.0184)	(0.0208)	(0.0185)	(0.0209)
QEAR×QINST	–		0.0133		0.0159		0.0256		0.0244		0.0202		0.0189
			(0.0165)		(0.0164)		(0.0163)		(0.0163)		(0.0164)		(0.0164)
QEAR×QEST	–			-0.0126	-0.0148			0.0172	0.0153			0.0195	0.0180
				(0.0144)	(0.0143)			(0.0162)	(0.0161)			(0.0163)	(0.0162)
QEAR×QSIZE	–					-0.0741 ***	-0.0797 ***	-0.0849 ***	-0.0890 ***	-0.0471	-0.0509 *	-0.0524 *	-0.0556 *
						(0.0192)	(0.0191)	(0.0218)	(0.0218)	(0.0293)	(0.0296)	(0.0301)	(0.0303)
QEAR×QPRICE	–									-0.1270 ***	-0.1263 ***	-0.1285 ***	-0.1278 ***
										(0.0188)	(0.0188)	(0.0186)	(0.0187)
QEAR×QVOLUME	–									0.0342	0.0337	0.0266	0.0267
										(0.0274)	(0.0273)	(0.0278)	(0.0277)
QEAR×QARBRISK	+									-0.0395 ***	-0.0382 ***	-0.0382 ***	-0.0371 ***
										(0.0143)	(0.0143)	(0.0143)	(0.0143)
INTERCEPT		-0.2438 ***	-0.2415 ***	-0.2500 ***	-0.2482 ***	-0.2866 ***	-0.2853 ***	-0.2845 ***	-0.2835 ***	-0.2713 ***	-0.2702 ***	-0.2699 ***	-0.2690 ***
		(0.0204)	(0.0206)	(0.0214)	(0.0214)	(0.0225)	(0.0223)	(0.0226)	(0.0225)	(0.0223)	(0.0221)	(0.0223)	(0.0222)
Firm FE		X	X	X	X	X	X	X	X	X	X	X	X
Year-quarter FE		X	X	X	X	X	X	X	X	X	X	X	X
Observations		3,137	3,137	3,137	3,137	3,137	3,137	3,137	3,137	3,137	3,137	3,137	3,137
R-squared		0.1765	0.1767	0.1767	0.1770	0.1810	0.1817	0.1813	0.1820	0.1975	0.1980	0.1979	0.1983

The table shows the results from the ordinary least squares regressions of the 60-day cumulative abnormal return (CAR) on the earnings announcement return, the proxies for investor sophistication and a host of control variables. QEAR is the quartile portfolios for each quarter of the earnings announcement return (EAR), scaled to range from one third to four thirds, and QINST, QEST, QSIZE, QPRICE, QVOLUME and QARBRISK are the quartile portfolios for each quarter of the institutional ownership, the number of analyst estimates, the natural logarithm of the US dollar market capitalization, the share price, the annual US dollar trading volume and the residual variance from a regression of each security's return on those of a market index, respectively, scaled to range from zero to one. The predicted sign is included for each coefficient except the intercept. The table exhibits the regression coefficients, with heteroskedasticity-consistent Huber-White standard errors in parentheses, as well as the number of observations and the R-squared values. The stars *, ** and *** denote statistical significance at the ten, five and one per cent significance levels, respectively.

Table C8: Regressions of 120-day CAR on insider trading variables

Variable	(1) CAR20		(2) CAR25		(3) CAR120		(4) CAR26–120	
BUY	-0.0044		-0.0043		-0.0169		-0.0126	
	(0.0087)		(0.0096)		(0.0298)		(0.0258)	
POSITIVE	0.0080		0.0081		0.0345	**	0.0264	*
	(0.0053)		(0.0060)		(0.0160)		(0.0145)	
BUY×POSITIVE	0.0218		0.0249		0.0610		0.0361	
	(0.0142)		(0.0154)		(0.0432)		(0.0387)	
SELL	-0.0137	*	-0.0173	*	-0.0625	***	-0.0452	**
	(0.0078)		(0.0090)		(0.0241)		(0.0216)	
NEGATIVE	-0.0151	***	-0.0155	**	-0.0146		0.0008	
	(0.0053)		(0.0063)		(0.0169)		(0.0144)	
SELL×NEGATIVE	-0.0231		-0.0166		-0.1001		-0.0836	
	(0.0168)		(0.0205)		(0.0659)		(0.0565)	
BUY×NEGATIVE	0.0187		0.0208		0.0736	*	0.0529	
	(0.0146)		(0.0156)		(0.0423)		(0.0365)	
SELL×POSITIVE	0.0157		0.0147		-0.0116		-0.0263	
	(0.0124)		(0.0138)		(0.0376)		(0.0338)	
SIZE	-0.0101		-0.0108		-0.0418		-0.0309	
	(0.0093)		(0.0104)		(0.0308)		(0.0262)	
BTM	0.0261	**	0.0308	**	0.1387	***	0.1079	***
	(0.0110)		(0.0126)		(0.0315)		(0.0237)	
INTERCEPT	0.1134		0.1364		0.7878	*	0.6515	*
	(0.1172)		(0.1380)		(0.4075)		(0.3409)	
Firm FE	X		X		X		X	
Year-quarter FE	X		X		X		X	
Observations	2,395		2,395		2,395		2,395	
R-squared	0.1892		0.1690		0.2788		0.2859	

The table shows the results from the ordinary least squares regressions of the 20-day, 25-day, 120-day and 26-to-120-day cumulative abnormal return (CAR) on the binary surprise and insider trading variables as well as their interactions and size and the book-to-market ratio. BUY and SELL are binary variables taking on the value of one when there is extensive insider buying and selling, respectively, and zero otherwise. POSITIVE and NEGATIVE are binary variables taking on the value of one when there is positive and negative surprises, respectively, and zero otherwise. BUY×POSITIVE, SELL×NEGATIVE, BUY×NEGATIVE and SELL×POSITIVE are interactions of these binary variables. SIZE and BTM are the size and book-to-market ratio for each observation. The table exhibits the regression coefficients, with heteroskedasticity-consistent Huber-White standard errors in parentheses, as well as the number of observations and the R-squared values. The stars *, ** and *** denote statistical significance at the ten, five and one per cent significance levels, respectively.

Table C9: Regressions of 60-day CAR on insider trading variables – EAR cut-off is ± 5%

Variable	(1) CAR20		(2) CAR25		(3) CAR60		(4) CAR26–60	
BUY	-0.0007		0.0031		0.0033		0.0002	
	(0.0074)		(0.0082)		(0.0147)		(0.0114)	
POSITIVE	0.0180	***	0.0205	***	0.0457	***	0.0252	**
	(0.0068)		(0.0077)		(0.0130)		(0.0103)	
BUY×POSITIVE	0.0143		0.0060		-0.0217		-0.0277	
	(0.0212)		(0.0219)		(0.0393)		(0.0287)	
SELL	-0.0097		-0.0118		-0.0251	*	-0.0133	**
	(0.0068)		(0.0077)		(0.0131)		(0.0100)	
NEGATIVE	-0.0166	**	-0.0181	**	-0.0305	**	-0.0124	
	(0.0069)		(0.0083)		(0.0138)		(0.0100)	
SELL×NEGATIVE	-0.0278		-0.0278		-0.0666		-0.0388	
	(0.0199)		(0.0256)		(0.0533)		(0.0419)	
BUY×NEGATIVE	0.0257		0.0203		0.0648	*	0.0445	**
	(0.0172)		(0.0186)		(0.0294)		(0.0204)	
SELL×POSITIVE	0.0051		-0.0003		-0.0438		-0.0434	*
	(0.0155)		(0.0170)		(0.0317)		(0.0259)	
SIZE	-0.0100		-0.0104		-0.0363	*	-0.0259	*
	(0.0093)		(0.0105)		(0.0198)		(0.0156)	
BTM	0.0257	**	0.0307	**	0.0708	***	0.0402	***
	(0.0111)		(0.0127)		(0.0213)		(0.0136)	
INTERCEPT	0.1139		0.1322		0.5243	*	0.3920	**
	(0.1171)		(0.1378)		(0.2726)		(0.1983)	
Firm FE	X		X		X		X	
Year-quarter FE	X		X		X		X	
Observations	2,395		2,395		2,395		2,395	
R-squared	0.1890		0.1702		0.2441		0.2435	

The table shows the results from the ordinary least squares regressions of the 20-day, 25-day, 60-day and 26-to-60-day cumulative abnormal return (CAR) on the binary surprise and insider trading variables as well as their interactions and size and the book-to-market ratio. BUY and SELL are binary variables taking on the value of one when there is extensive insider buying and selling, respectively, and zero otherwise. POSITIVE and NEGATIVE are binary variables taking on the value of one when there is positive and negative surprises, respectively, and zero otherwise. BUY×POSITIVE, SELL×NEGATIVE, BUY×NEGATIVE and SELL×POSITIVE are interactions of these binary variables. SIZE and BTM are the size and book-to-market ratio for each observation. The table exhibits the regression coefficients, with heteroskedasticity-consistent Huber-White standard errors in parentheses, as well as the number of observations and the R-squared values. The stars *, ** and *** denote statistical significance at the ten, five and one per cent significance levels, respectively.

Table C10: Regressions of 120-day CAR on insider trading variables – EAR cut-off is ± 5%

	(1)		(2)		(3)		(4)	
Variable	CAR20		CAR25		CAR120		CAR26–120	
BUY	-0.0007		0.0031		0.0003		-0.0028	
	(0.0074)		(0.0082)		(0.0240)		(0.0209)	
POSITIVE	0.0180	***	0.0205	***	0.0545	***	0.0341	*
	(0.0068)		(0.0077)		(0.0210)		(0.0188)	
BUY×POSITIVE	0.0143		0.0060		0.0120		0.0060	
	(0.0212)		(0.0219)		(0.0542)		(0.0492)	
SELL	-0.0097		-0.0118		-0.0577	***	-0.0459	**
	(0.0068)		(0.0077)		(0.0206)		(0.0184)	
NEGATIVE	-0.0166	**	-0.0181	**	-0.0216		-0.0035	
	(0.0069)		(0.0083)		(0.0218)		(0.0181)	
SELL×NEGATIVE	-0.0278		-0.0278		-0.1160		-0.0881	
	(0.0199)		(0.0256)		(0.0879)		(0.0733)	
BUY×NEGATIVE	0.0257		0.0203		0.0865	*	0.0661	*
	(0.0172)		(0.0186)		(0.0468)		(0.0393)	
SELL×POSITIVE	0.0051		-0.0003		-0.0670		-0.0667	
	(0.0155)		(0.0170)		(0.0522)		(0.0469)	
SIZE	-0.0100		-0.0104		-0.0421		-0.0317	
	(0.0093)		(0.0105)		(0.0308)		(0.0262)	
BTM	0.0257	**	0.0307	**	0.1384	***	0.1077	***
	(0.0111)		(0.0127)		(0.0318)		(0.0240)	
INTERCEPT	0.1139		0.1322		0.7888	*	0.6566	*
	(0.1171)		(0.1378)		(0.4066)		(0.3403)	
Firm FE	X		X		X		X	
Year-quarter FE	X		X		X		X	
Observations	2,395		2,395		2,395		2,395	
R-squared	0.1890		0.1702		0.2789		0.2859	

The table shows the results from the ordinary least squares regressions of the 20-day, 25-day, 120-day and 26-to-120-day cumulative abnormal return (CAR) on the binary surprise and insider trading variables as well as their interactions and size and the book-to-market ratio. BUY and SELL are binary variables taking on the value of one when there is extensive insider buying and selling, respectively, and zero otherwise. POSITIVE and NEGATIVE are binary variables taking on the value of one when there is positive and negative surprises, respectively, and zero otherwise. BUY×POSITIVE, SELL×NEGATIVE, BUY×NEGATIVE and SELL×POSITIVE are interactions of these binary variables. SIZE and BTM are the size and book-to-market ratio for each observation. The table exhibits the regression coefficients, with heteroskedasticity-consistent Huber-White standard errors in parentheses, as well as the number of observations and the R-squared values. The stars *, ** and *** denote statistical significance at the ten, five and one per cent significance levels, respectively.