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YOU GET WHAT YOU PAY FOR! EVIDENCE ON HOW RESEARCH UNBUNDLING UNDER MIFID II IMPACTS THE QUALITY OF STOCK ANALYST FORECASTS

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Abstract

Until recently, a common practice among investment banks was to provide stock analyst research to clients for free as part of a "bundle" of services. This practice was completely stopped, literally overnight, when a major European directive, MiFID II, mandated that research must be unbundled from other services and priced separately. These new rules that transformed the market for stock analyst research have raised considerable concerns about analysts losing their jobs, substantial drops in analyst coverage, and a subsequent decrease in the quality of analyst reports. The goal of this paper is to investigate these concerns. We use difference-in-differences models to isolate MiFID's effect on coverage and research quality (bias and forecast accuracy) in European stocks. Our results show that the regulation significantly decreased analyst coverage, but, contrary to the concerns, *increased* research quality (lowered bias and increased accuracy). Using mediation models, we find that the increase in research quality is partly due to the decrease in coverage from low-performing analysts that are dropped once clients have to pay for the coverage and partly due to remaining analysts becoming more accurate, which we attribute to increased pressure to produce high-quality reports. Our findings imply that the research unbundling in MiFID II lowered conflicts of interest and had positive spillover effects on research quality and market competition. Ultimately, when it comes to stock analyst research, you get what you paid for.

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1. Introduction

The Markets in Financial Instruments Directive is a European Union regulation aimed at increasing the competitiveness and transparency of financial markets by harmonizing the regulation that financial actors are subject to. The end goal of the directive is to guarantee market integrity and to improve the protection of investors trading financial instruments such as shares, bonds, and derivatives.

One of the most discussed introductions in MiFID II, which came into effect on January 3, 2018, is the law on unbundling commissions, according to which sell-side firms (e.g., investment banks) must separate their trade execution commissions from their research services fees. Financial firms that want to continue to charge their clients for research have to do it through a Research Payment Account (RPA) and prepare the research budget *ex-ante*, in such a way that end-investors are always informed for what and how much they are paying. Alternatively, firms can absorb the cost, i.e., cover the cost of research from their own profits. These recent regulations reflect a major change is how stock analyst research is provided to end-users, with the most significant change being that end users now typically pay an explicit fee for the analyst research that they wish to consume.

While unbundling is certainly bringing more transparency to the market, both the buy- and sell-side firms have raised considerable concerns about the negative side-effects of the new regulation. More and more investment houses choose to cover research in-house (CFA Institute, 2019), which leads to budget constraints and potentially higher fees for the end-investors. Demand for sell-side research risks to fall, which means that a number of analysts will lose their positions. Thus, firms, especially the small- and mid-capitalization ones, risk losing coverage. The impact on research quality is also highly debated, with some claims that the drop in coverage will negatively impact the quality of coverage.

Given the intense dialogue on research unbundling and the benefits of MiFID II, in this paper, we attempt to measure the respective effects. We first examine the change in analyst coverage following the implementation of MiFID II in 2018, which we hypothesize, based on previous literature, leads to a change in the level of analyst optimism (i.e., analyst bias). There are plenty of studies presenting evidence that analyst recommendations often tend to be over-optimistic due to various reasons, the most common one being the presence of conflicts of interest. We are therefore interested in investigating whether MiFID II has had a positive or negative effect on analyst bias and forecast accuracy by addressing the problems of transparency and investor protection. In this sense, the research question that we seek to answer is *How has MiFID II affected research quality?* To do so, we employ both OLS regressions and a model of mediation, which will allow us to observe whether the implementation of MiFID II had an impact on bias and, if so, whether some of its effect has occurred through the decrease in analyst following. Next, we conduct a difference-in-differences (DiD) analysis in order to isolate the variation that originates from the new regulation from the natural time trends in bias and accuracy. Additionally, we evaluate whether the changes hold for firms with different market capitalizations. Finally, we look at whether there are significant changes in the market reaction to forecast revisions in the post-MiFID II period compared to pre-MiFID II period.

Our results indicate that the directive led to significant decreases in coverage and to an increase in research quality for European companies of all market sizes. Our mediation model shows that MiFID II led to decreases in bias and forecast error both directly and indirectly through the decrease in coverage. The cut in analyst coverage led to low-performing analysts quitting or being dismissed (i.e., the coverage channel), while the remaining analysts became more incentivized to produce quality reports under the pressure of keeping their jobs. Our DiD analysis shows that MiFID II, rather than a common time series trend, is responsible for the reduction in coverage, analyst bias, and forecast error for European firms.

The novelty of the research comes from the fact that MiFID II has been implemented very recently, and it is primarily relevant for EU countries. To the authors' knowledge, no peer-reviewed academic papers that would evaluate the early effects of the research unbundling reform have been published in the last two years. Moreover, our analysis is timely, considering that MiFID's effects are already spreading outside the EU (Riding, 2019). This is particularly beneficial for the construction of the DiD model, for which we need to compare our sample with one that has not yet been affected by MiFID II. Most importantly, the implementation of MiFID II gives us the possibility to analyze a unique natural experiment to better understand the effect of payment for research on its quality.

In terms of relevance, this research is of interest to legislators and policymakers as we provide an opportune evaluation of MiFID II's effects. We examine whether the directive's goal of diminishing conflicts of interest has been achieved by analyzing the change in analysts' optimism bias, and we inspect the potential unintended consequence of decreasing research quality. Notably, reducing analyst optimism has welfare implications. As Hong and Kacperczyk (2010) state, retail investors, compared to institutional investors, are unable to account for analyst optimism when making investment decisions, therefore being more vulnerable to biased forecasts. In addition, since retail investors cannot adjust to analyst bias, their investment decisions will be equally biased, which could bring more noise to the market. This is where MiFID II comes in with an increased focus on investor protection, which could, ultimately, improve the efficiency of the market.

This paper is structured as follows. Section 2 provides an overview of the analyst industry and reviews the relevant previous research regarding the link between analyst coverage and forecast bias. Section 3 enumerates the sources of data and explains how the variables used throughout the paper were created. Section 4 describes in detail the tests that were conducted and the obtained results. Finally, Section 5 concludes.

2. Literature Review and hypotheses

2.1 Analyst industry and research bundling

The research available to investors is classified into *sell-side* research, typically produced by brokerage houses, investment banks, and available to both institutional and individual investors, and *buy-side* research, available exclusively to asset managers and their crew. Although both sell-side and buy-side analysts fundamentally perform the same activity, the difference in their audiences determines the scope of their work, as well as their incentives. The sell-side employs a significantly larger number of analysts compared to the buy-side, which enables them to produce more focused and detailed research. A major contribution to the content of sell-side research comes from analysts' communication with a firm's senior management - information that is not as easily accessible for buy-side analysts (Groysberg, Healy, & Chapman, 2008). Brown, Call, Clement, and Sharp (2016) find that "sell-side analysts' experience following a company and the frequency of their communication with senior management" (p. 140) are the main reasons for buy-side analysts to use sell-side research. Sell-side analysts' in-depth industry knowledge is equally important. Essentially, the value investors receive from sell-side research is derived from comprehensive reviews at the firm and industry levels, summaries of the companies' quarterly performance, due diligence done with the firms' management, and compelling guidance for making investment decisions (Stowell, 2012). Most importantly, sell-side research contributes to reducing information asymmetries and improving market efficiency.

An important difference between buy-side and sell-side analysts is their compensation. According to Groysberg et al. (2008), since buy-side analysts are expected to come up with critical analysis and good stock recommendations, their compensation is based on providing the best possible assistance to portfolio managers. Sell-side analysts' compensation is closely linked to their reputation and the ratings they receive from institutional investors (Ljungqvist, Marston, Starks, Wei, & Yan, 2007; Groysberg, Healy, & Maber, 2011; Hong & Kubik, 2003), as well as commissions and soft-dollar arrangements. Soft dollar practices are agreements between an advisor and a broker-dealer, under which the advisor commits to directing transactions to the broker-dealer in exchange for products or services beyond the execution of the transactions, such as research products, administrative, or distribution services, i.e., on a *quid pro quo* basis (SEC, 2000). Such practices had benefited both the sell-side and the buy-side for decades (Horan & Johnsen, 2008). Bundling various services with trading commissions allows fund managers to bypass the expense ratio[1] since commissions are not included in operating expenses. According to SEC (2000), their absence is justified by the fact that there is no standard way of measuring spread costs. Including trading commissions in the reported expense ratio would thus be unfair to brokerage houses with higher commissions but lower spread costs. In this vein, bundling research and other services leads to a lower and hence more attractive expense ratio for investors. The sell-side, in turn, can provide institutional investors with all the research they produce, taking advantage of less scrutiny from asset managers who treat research as free goods (Bogle, 2009). Institutional investors themselves might be lightly monitored by largely dispersed fund shareholders, as is the case for mutual funds (Horan & Johnsen, 2008). This is especially profitable for the sell-side, considering that a notable part of the brokerage commissions goes to the payment of research (Groysberg et al., 2011; Livingston & O'Neal, 1996). However, not all the research that managers buy with soft-dollars is beneficial, nor is it necessary for making investment decisions, which means that the soft-dollar arrangement merely leads to a funding of biased

[1] The expense ratio is a measure for evaluating the costs of running a fund. It is calculated by dividing the fund's operating expenses by its total assets under management.

research, the actual price of which is hard to determine (Logue, 1991). Managers of big mutual funds, in particular, were overpaying for research due to their huge trading volumes.

One of the benefits of soft-dollar commissions is that they should lead to higher risk-adjusted returns due to premium brokerage services (e.g., more efficient order execution, swift delivery of research reports). Livingston and Zhou (2015), as well as Horan and Johnsen (2008), provide evidence that supports this view and find a significant positive relation between premium brokerage services and mutual fund performance. On the other hand, Edelen, Evans and Kadlec (2012) document a negative relation, pointing out increased agency costs and inefficient fund expenditures. Another potential benefit is the reduction in management fees paid by investors since the cost of research is covered by the bundled commissions. Nevertheless, Livingston and O'Neal (1996) find a positive relation between brokerage commissions and the expense ratio, which could indicate that the opaqueness of the bundling mechanism leads to a breach of asset managers' fiduciary duty. Erzurumlu and Kotomin (2016) find contradicting evidence to both of the alleged benefits of soft-dollar commissions. Bogle (2009), in fact, infers that research should be paid for internally or with hard dollars, simply because it is "the very service of professional management" (p. 50) that motivates investors to invest in mutual funds.

2.2 The quality and informativeness of sell-side research

A large stream of academic literature has examined the benefits investors get from analysts' work. Specifically, analysts' forecast revisions have been shown to affect investors' decisions, hence influencing stock price dynamics. Womack (1996), Stickel (1995), Jegadeesh and Kim (2006) find that positive analyst reviews lead to positive returns, with price drifts that last months after the recommendation. Hirst, Koonce, and Simko (1995) and Asquith, Mikhail, & Au (2005) establish that investors value downgrades the most in analyst reports, while Francis and Soffer (1997) find the opposite.

Frankel, Kothari, and Weber (2006) show evidence that research is generally informative, based on the annual average stock price reaction to forecast revisions. The same study by Frankel et al. (2006) finds that analyst informativeness increases with the number of analysts covering a firm, supporting the view that higher coverage leads to better information production about a firm. Nonetheless, the authors admit the possibility of a negative relation between coverage and analyst informativeness in cases where analysts reproduce existing information or tend to release misleading information for personal interests. Generally, Chen, Francis, and Jiang (2005) find that investors' reaction to forecast revisions depends on their learning about analysts' abilities and past performance.

Barber, Lehavy, McNichols, and Trueman (2001), and Mikhail, Walther, and Willis (2004) show that despite the abnormal returns generated by investing in stocks with the most favorable recommendations, after accounting for trading costs, net returns equal zero. From another perspective, Chen, Harford, and Chen (2015) find that sell-side analysts, thanks to their connections with companies' management, contribute to monitoring their decisions and potentially decrease agency costs for investors.

Forecast accuracy has been shown to be a predictor of investors' reaction to analyst forecasts, both in theory (Abarbanell, Lanen, & Verrecchia, 1995) and in empirical research (Stickel, 1992; Gleason & Lee, 2003; Park & Stice, 2000). Clement and Tse (2003) advance this topic by stating that "investors do not respond to analyst forecasts revisions as if forecast accuracy is all that matters" (p. 230) but show that they highly value timely forecasts and reports from large brokerage houses. Among others, forecast accuracy is positively correlated with firm-specific experience (Mikhail et al., 1997), employer size (Clement, 1999; Jacob, Lys, & Neale, 1999), and decrease with the number of firms an analyst has to follow (Clement, 1999).

Despite analysts' contribution to capital market efficiency and liquidity, there are numerous concerns regarding their impartiality, which consequently affects their bias. Conflicts of interest generate optimism bias in situations when sell-side analysts are pressured to issue optimistic recommendations to attract external corporate financing (Michaely & Womack, 1999), or give positive reviews for companies that have an established relationship with the analyst's investment bank. Hong and Kubik (2003) find that optimism is rewarded more than accuracy when it comes to evaluating the bank's underwritten stock. Brokerage commissions are another driver of optimism bias (Jackson, 2005; Agrawal & Chen, 2012). Specifically, 'buy' recommendations are more profitable since they attract new investors and, hence, more trading activity. Besides, research bundling had created propitious conditions for such over-optimistic forecasts to circulate freely and unaudited by fund managers. Furthermore, analysts are pressured to issue positive reviews by the firms themselves to avoid hostility from companies' management and preserve their access to the management's private information (Chen, Novoselov, & Wang, 2018).

Cognitive biases, such as overconfidence and representativeness, have also been shown to cause optimistic judgments (Kahneman & Lovallo, 1993; Mokoteli, Taffler, & Ryan, 2006). Finally, McNichols and O'Brien (1998) suggest selection bias as a reason for optimism, which implies that the observed over-optimism is not added artificially to the analysts' views, but rather it arises from the analysts' choice to cover stocks for which they have higher expectations.

Reducing the conflicts of interest faced by asset managers and the optimism in analysts' recommendations has welfare implications. While institutional investors are able to adjust for the bias present in analysts' forecasts, retail investors are not in the same position (Hong & Kubik, 2010), which means that the optimism in analysts' recommendations will continue to be reflected in price changes (Michaely & Womack, 1999). Therefore, keeping analyst bias in check is crucial for regulators in order to create a more transparent environment for unsophisticated investors.

2.3 MiFID II and research unbundling

MiFID I was a first step introduced by the European Securities and Markets Authority (ESMA) to harmonize rules governing investment services and activities in Europe. The directive also put a big accent on investor protection, transparency, and market integrity. The first set of rules (Directive 2004/39/EC) came into effect on April 30, 2004 and had been applied between January 31, 2007 and January 2, 2018. One of the cornerstones of MiFID I is Article 21, which stipulates that investment firms have to deliver the best possible results for their clients, i.e., the best execution principle. Its assessment would be based on price, speed, costs, and other factors that are part of the order execution (European Commission, n.d.(a)). Two conducted reviews - by the Financial Services Authority (FSA) in 2012 and by the Financial Conduct Authority (FCA) in 2014 - found that the best execution principle has been considerably shirked by institutional investors.

The revision of MiFID I led to the adoption of MiFID II (Directive 2014/65/EU) on July 2, 2014, which has been applied since on January 3, 2018. MiFID II puts an even stronger emphasis on the previous goals. Among many others, the regulation imposes changes to the securities market structure. It extends the "best execution" principle and the reporting requirements in order to avoid inducements and conflicts of interest. Special attention has been paid to Article 24(7) and (8) of the directive, which prohibits financial institutions from bundling the payment for research with trading commissions (European Commission, n.d.(b)). First, portfolio managers have the option to cover the research costs from their own funds, against the firm's profit and loss. Second, if they want to continue charging their clients, they have to do it through a special research payment account (RPA), which charges the client a specific amount based on a research budget agreed upon with the client. Only in these two cases, the research received from third-party entities would not constitute an inducement, according to MiFID II.

2.4 The effect of research unbundling

Research unbundling, effectively, disciplines institutional investors, who can no longer overlook the way research is obtained. Having to pay for it from their own funds or with hard dollars incentivizes managers to filter their sources of research or produce it more efficiently, fulfilling their "best execution" obligation. Further, conflicts of interest that managers have towards brokers and clients should be diminished due to the execution-only commission payment, i.e., will engage in arm's length transactions. More scrutiny in research selection from buy-side should stimulate the sell-side to produce high-quality and unbiased research, which can potentially reduce conflicts of interest that analysts face towards brokers and investors. In addition, more transparency should lead to lower agency costs and more efficient fund expenditures, as Edelen et al. (2012) argue.

However, critics of the regulation claim that the new regulations could have substantial unintended consequences. First, there is a risk that the coverage and, hence, the quality of the sell-side research have been negatively affected. If most asset managers decide to use the P&L method for funding their research, budgets will be cut, lowering demand for sell-side research. According to McLannahan (2019), banks have already dismissed hundreds of analysts. Because of their rapidly decreasing number of colleagues, the remaining analysts now have to cover more securities, spending less time on analyzing each of them individually, or completely dropping the coverage of certain companies. Academics have shown that analyst coverage is negatively linked with bias and forecast error (Hong & Kubik, 2010; Merkley, Michaely, & Pacelli, 2017; Derrien & Kecskes, 2013; Kelly & Ljungqvist, 2012). In other words, lower competition created through decreased coverage could lead to reduced information dissemination and higher bias.

However, there are reasons for why the quality of research should improve and the bias should decrease after a loss of coverage. If a decrease in coverage is the observed trend, it is likely that the analysts dropped will be the ones who previously produced reports of the lowest quality. Lower-ranked analysts already suffer from less interaction with their clients (Murphy, 2018a). Moreover, as more buy-side firms will choose to cover research in house, they will resort to paying for sell-side research only if it is perceived as having higher quality. In this context, previous studies show that institutional investors impact sell-side analysts' estimates (Gu, Li, & Yang, 2013). Therefore, the competition to stay in business due to the cost-management pressure and buy side's higher standards will incentivize the remaining sell-side analysts to provide precise predictions with less over-optimistic recommendations. As Murphy (2018a) points out, MiFID II might "give rise to a new generation of star analysts," as the ones remaining will be forced to make sure their work brings extra value in order to stay relevant. Further on, research unbundling aims to eliminate the conflicts of interest between asset managers and analysts, which are a major driver of optimism. Prokop & Kammann (2018) have already shown that MiFID I had succeeded in combating conflict of interests among affiliated financial analysts.

Another worry is that the sell-side analyst coverage of companies with smaller market capitalization has significantly dropped (Schulte, 2018; Murphy, 2018b). In contrast, it is expected that a drop in coverage caused by sell-side analyst layoffs should have a less negative impact on big corporations, which are covered by dozens of analysts. Meanwhile, smaller companies, whose coverage is already scarce, stand to lose more. Lower coverage can expose these firms to great risks, such as increasing share price volatility, decreasing trading volume, higher cost of capital, and the creation of a hostile environment for capital formation. In the long term, this can negatively impact innovation and economic growth. These companies are actually threatened by a complete loss of coverage, in which case they risk underperforming their peers - by 4.2% on average, as indicated by Jefferies Group (2017). According to other researchers, these fears are unfounded. They point out that the decline in trading volumes that smaller companies experienced in 2018 is the continuation of a longer trend, caused by a more general reluctance of market participants to invest in smaller and riskier enterprises. Some argue that small-cap firms do not need as much analyst attention since they rely on other strategies to attract investors (McLannahan, 2019). Gervais Williams, who is a small-cap fund manager at Miton Group, notes a different trend - that investors are interested in diversifying portfolios with more holdings in small caps, which might actually offset the negative effects on costs and revenues (Murphy, 2018c).

The CFA Institute has conducted a survey between December 6-19, 2018, with respondents coming from 449 different firms across 25 European countries. The survey includes respondents who work for both the buy-side (68%) and the sell-side (20%). As expected, the main findings show that most fund managers chose to internalize the costs of research. Already in August 2017, the second-largest fund manager, Vanguard, was the first to announce that it will cover research costs from its own profit and loss account (Marriage, 2017). Not only is this option preferred because asset owners expect portfolio managers to do so, i.e., competitive pressure, but it is also more convenient from an administrative and regulatory point of view. Another explanation for the avoidance of the RPA method is that it would create a free-rider problem, i.e., investors who join the fund later and/or choose smaller research budgets will receive the same benefits as those who are ready to contribute more (Tata, 2019). Generally, management fees have not been increased to cover the new additional costs, which shows that the end-investor indeed benefits from the new regulation (CFA Institute, 2019).

The survey results indicate that the average decrease in the buy side's research budgets is 6.3%. The higher the number of assets-under-management is, the higher the budget reduction, which may suggest that big investment funds were indeed spending too much on research due to trading commissions and that it is relatively easy for them to replace external research. After MiFID II, the CFA Institute points out that 57% of the respondents outsource less sell-side research, and that 34% use more in-house research. Independent research houses have not benefited from MiFID II, considering that competition has toughened, and investment banks have significantly reduced their price quotes on research products. On the bright side, research markets have benefited from price discovery in the months before MiFID II went into effect (CFA Institute, 2019). As far as research quality is concerned, the buy side does not see any changes, whereas 44% of the sell side believe it has decreased. Less than 10% of respondents on both the buy side and sell side hold the view that quality has improved. Regarding research coverage, around half of the respondents on both sides agree that coverage of small- and mid-cap equities has decreased, while the coverage of large-cap stocks seem unaffected.

A positive outcome of the regulation is that it has limited the supply of low-quality research products. The majority of both buy and sell side believes that the research marketplace has become more competitive as a result of MiFID II, which means that the directive has reached one of its main goals. A more competitive research market enables greater discipline and examination from asset managers in their decision to acquire research, which could have positive spillover effects over the quality of analyst reports. There were no inquiries in the survey regarding the perception of analyst bias, but in terms of market transparency, only the sell side thinks that equity markets have become more transparent (31%), while buy-side representatives believe that the market remained unchanged in this matter (CFA Institute, 2019).

2.5 Hypotheses

The focus in our evaluation of MiFID II is contained in the following research question: *How has MiFID II affected research quality?* By research quality, we infer two attributes of analyst reports: analyst bias and aggregate forecast accuracy. Both of them are important concerns of MiFID II, given that conflicts of interest - hence, analyst bias - are directly addressed by the research unbundling reform, and it is unclear how forecast accuracy will be affected as a side-effect of the regulation. As we surmise that coverage is a mediator of the relation between MiFID II and the attributes of research quality, we perform a two-step analysis to answer the research question: first, we look at the effect of MiFID II on analyst coverage; second, assuming MiFID II is a proxy for the change in the number of sell-side analysts, we observe its effect on forecast bias and accuracy. In other words, in this paper we look at coverage as a channel through which the implementation of MiFID II affects analyst performance.

To begin with, we hypothesize that in the post-MiFID II period, the coverage of European companies should decrease, considering that most investment firms will choose to cover research from their own expenses and reduce their research budgets. Therefore, we expect that:

Hypothesis 1: On average, analyst coverage decreases after MiFID II for EEA companies.

Additionally, we will look at whether the change in coverage is different for large, midsize, and small companies. On the one hand, we could find that, proportionally, coverage decreases more for mid- and small-caps than for large-caps, as their coverage is already scarce (Table I, Panel B & C). On the other hand, finding the opposite would offer supporting evidence for the oversupply of analysts covering large companies (McLannahan, 2019), which would explain why these companies might suffer from a relatively sharper cut in analysts.

Further on, we hypothesize that the decrease in coverage will lead to the dismissal of analysts with low performance (Murphy, 2018a), considering that institutional investors will become more aware of the research they are paying for and will value more accurate earnings forecasts as a result of the unbundling reform. According to CFA Institute (2019), the research marketplace is already perceived to be more competitive by both the buy side and the sell side, which should consequently put pressure on analysts to provide high-quality research. Therefore, we add the following hypotheses:

Hypothesis 2: On average, analyst bias decreases after MiFID II in EEA countries.

Hypothesis 3: On average, forecast error decreases after MiFID II in EEA countries.

Currently, there are ongoing debates (McLannahan, 2019; Murphy, 2018c) on whether small- and mid-cap equities have been hit significantly by MiFID II. At the same time, it is assumed that large-cap companies have been affected only mildly or not at all because they are prioritized in analysts' reports due to their higher liquidity (Murphy, 2018b). Therefore, if the overall trend observed after MiFID II is a cut in the analysts producing poor research, leading to increased forecast quality, we attempt to examine whether that is the case for companies with different market capitalizations. On the one hand, we could find that post-MiFID II forecasts for European mid- and small-cap firms are more erroneous, confirming the aforementioned concerns. On the other hand, we could find that the quality of forecasts increases likewise for equities of all market sizes, which will corroborate the positive effects of research unbundling.

As an alternative to *Hypotheses 2* and *3*, Hong and Kacperczyk (2010) show an inverse relationship between coverage and optimism bias, i.e., higher competition through increased coverage leads to more discipline and, hence, less bias. In this vein, lower coverage after MiFID II could lead to higher bias and error for smaller firms for the following reasons: first, the remaining analysts might have to cover more stocks, which would lower their focus and accuracy; second, it is easier for firms to pressure analysts to issue more optimistic forecasts when their number is smaller. Small and midsize companies are endangered by low coverage or a complete loss of coverage, which will lead to less information about them being available and, consequently, to less accurate predictions.

Finally, to strengthen our results, we hypothesize that forecasts of higher (lower) quality should be more (less) informative and hence should produce a stronger (weaker) market reaction (Merkley et al., 2017; Stickel, 1992). If we observe that research quality increases post-MiFID II, we expect stronger market reactions to forecast changes:

Hypothesis 4: Market reaction to changes in analyst earnings forecasts is stronger in the post-MiFID II period.

As previously discussed in the literature review, there is no conclusive evidence regarding the market reaction to upward and downward forecast revisions. Thus, we additionally look at the market reaction to upgrades and downgrades in forecasted earnings and whether this reaction changes significantly in the post-MiFID II period compared to the pre-MiFID II one.

3. Data and variables

To estimate the effect of MiFID II on the quality of the forecasts issued by sell-side analysts, we focus on three analyst measures: the number of analysts who follow each company in the sample over the sample period, the amount of analyst bias contained in the forecasts of each company and how it varies over time, as well as the forecast error of these observations. Aggregated analyst data is obtained from the Institutional Brokers Estimates System (I/B/E/S) database, where information about both the number of analysts covering a stock and the mean value of their estimates is available. Accordingly, we retrieve the mean and median value of one-year earnings (EPS) forecasts and the total number of estimates that contributed to these values on a monthly basis. Our choice to rely on one-year EPS forecasts is prompted by the fact that this is the type of forecast that analysts provide most frequently.

For the purpose of this paper, our sample includes only the countries for which MiFID II is applicable, which are the ones belonging to the European Economic Area. Financial data for individual companies is obtained from the Datastream and Worldscope databases. Our final sample consists of 334,749 firm-month observations from 25 EEA countries. The sample period ranges from 2006 to 2019. The distribution of stock-month observations by country can be found in Panel E, Appendix B.

Given the proposed research question, our main variables of interest are the number of analysts covering stock i at time t $\left(Coverage_{i,t}\right)$, the analyst forecast bias ($Bias_{i,t}$), and forecast error ($FError_{i,t}$). For each month, we calculate $Bias_{i,t}$ as the difference between the mean consensus EPS forecast and the actual EPS, relative to the absolute value of the actual EPS. Following Merkley et al. (2017), we remove the observations for which the absolute value of the actual EPS is less than 0.10 EUR per share to avoid obtaining skewed results for observations whose actual EPS is near or equal to zero. Additionally, since coding errors can be found in the analyst data reported by IBES (Hong & Kacperczyk, 2010), we exclude observations for which the absolute difference between the actual and forecasted EPS is either greater than 10 EUR or represents more than 50% of the actual value. As our sample consists of companies whose earnings differ considerably, the former rule is more appropriate for eliminating data errors among companies with low EPS, while the latter deals more effectively with errors among companies with high EPS. Furthermore, the resulting measure of analyst bias is winsorized at the 2.5th and 97.5th percentiles to make it more robust to extreme values.

$$Bias_{i,t} = \frac{Consensus\ EPS_{i,t}\ -\ EPS_{i.t}}{\left|EPS_{i,t}\right|} \quad (1)$$

By computing $Bias_{i,t}$ in the above-mentioned way, we can observe both analyst optimism, i.e., $Bias_{i,t} > 0$, and analyst pessimism, i.e., $Bias_{i,t} < 0$. Consequently, the absolute value of analyst bias ($\left|Bias_{i,t}\right|$) can be used as a proxy for forecast error ($FError_{i,t}$), according to Merkley et al. (2017).

As we are interested in observing how the attributes of forecast quality changed after MiFID II was put into effect, we employ an indicator variable ($MiFID_t$) that takes the value of 1 for reporting periods that follow after January 3, 2018, which is the date of the implementation of MiFID II, and 0 for the periods that predate the new regulation. Additionally, certain firm characteristics are included in the regressions to control for the variation of the dependent variables that is not related to the introduction of MiFID II. Namely, we add the natural logarithm of company i's market capitalization at the end of month t ($LnSize_{i,t}$), the natural logarithm of firm i's book-to-market ratio at the end of month t ($LnBM_{i,t}$), the return on stock i for month t ($Return_{i,t}$), the profitability of firm i in month t ($Profit_{i,t}$), the variance of daily simple returns of stock i during month t ($Sigma_{i,t}$), and the volatility of stock i's ROE in month t ($VolROE_{i,t}$). A detailed explanation of the construction of each variable is provided in Appendix A. The control variables are lagged by one year to ensure that they correspond with the period in which the one-year EPS forecast for firm i was issued. Descriptive statistics for the dependent and independent variables employed in the regression analyses are presented in Table I. The mean (median) number of analysts covering stocks in EEA countries is 13 (11) analysts, with a standard deviation of approximately nine analysts. The cross-sectional mean bias in the sample is equal to 18%, with a standard deviation of 53%.

Furthermore, we intend to test whether any observed effect varies for companies with different market capitalizations, given the concern present in the marketplace that small caps have been disproportionately affected by the new directive. To this end, we classify the observations in our sample into three groups, based on the company's market capitalization in 2017, the year immediately before the discussed legislative framework came into force: the firms whose market capitalization is higher than the 85th percentile are identified as large-cap firms, those with market capitalizations between 60th and the 85th percentile represent the mid-cap firms, while the remaining companies are classified as small-cap. Subsequently, we create a three-level categorical variable ($MCAP_i$), whose levels ($MCAPlarge_i$, $MCAPmid_i$, and $MCAPsmall_i$,) correspond to large-caps, mid-caps, and small-caps, respectively. Appendix B, Panel A, presents statistics for the analyst coverage of European companies by market capitalization group. We note a considerable difference in the average analyst following of each group, with large stocks being covered by approximately 24 analysts, mid-sized stocks - by 16 analysts, and small stocks - by only eight analysts.

Table I.
Summary Statistics on the IBES Sample, EEA countries

	Entire sample			Pre-MiFID II			Post-MiFID II		
	Cross-sectional mean	Cross-sectional median	Cross-sectional st. dev.	Cross-sectional mean	Cross-sectional median	Cross-sectional st. dev.	Cross-sectional mean	Cross-sectional median	Cross-sectional st. dev.
$Coverage_{i,t}$	13	11	8.98	13.35	12	9.21	11.67	10	7.78
$LnCoverage_{i,t}$	2.4	2.48	0.73	2.42	2.56	0.73	2.32	2.4	0.69
$Mean\ Bias_{i,t}$ (%)	18	6	53	18	7	54	16	6	50
$Median\ Bias_{i,t}$ (%)	18	6	53	18	6	54	16	6	50
$Mean\ FError_{i,t}$ (%)	33	15	48	33	15	49	30	15	45
$Median\ FError_{i,t}$ (%)	33	15	48	33	15	49	30	14	45
$Return_{i,t}$	0.2	0.11	4.97	0.21	0.11	5.52	0.17	0.11	0.43
$LnSize_{i,t}$	14.54	14.52	1.96	14.43	14.38	1.99	15.03	15	1.78
$LnBM_{i,t}$	-0.68	-0.63	0.76	-0.65	-0.61	0.76	-0.77	-0.73	0.75
$Profit_{i,t}$	0.09	0.07	0.08	0.09	0.07	0.08	0.08	0.07	0.08
$Sigma_{i,t}$	0	0	0.02	0	0	0.02	0	0	0.01
$VolROE_{i,t}$	0.03	0	0.37	0.04	0	0.38	0.02	0	0.29

Note. We consider a sample of stocks covered by IBES, Datastream, and Worldscope databases during the period 2006-2019 that have valid monthly EPS forecast records. Our sample comprises a total of 334,749 firm-month observations. All the variables listed above are explained in Appendix A. We exclude observations for which the absolute difference between the actual and forecasted EPS is either greater than 0.10 EUR or represents more than 50% of the actual value. The resulting measures of analyst bias and forecast error are winsorized at the 2.5th and 97.5th percentiles.

4. Method and results

We begin by conducting informal tests to check the premise of our research question. Specifically, we run a difference of means test to see whether there is a significant difference in the number of analysts ($Coverage_{i,t}$), level of analyst bias ($Bias_{i,t}$), and forecast error ($FError_{i,t}$) before and after the implementation of MiFID II. We find that the change in coverage following the implementation of MiFID II is statistically significant at less than 1% and is equal to a decrease of 1.69 analysts on average (Appendix C, Panel A). Both analyst bias and forecast error experience a significant change, decreasing by around 2.41 pp and 2.82 pp, respectively (Appendix C, Panel B & C). We have thus shown that the premises for conducting our research are sound, as all three of our variables of interest are significantly impacted by the implementation of MiFID II.

4.1 OLS regressions

To estimate the impact of MiFID II on the quality and number of forecasts, we run the following model:

$$Dependent\,Var_{i,t} = \alpha_0 + \beta_1 MiFID_t + \sum_{j=1}^{6} \delta_j Control_{j,i,t-12} + \varepsilon_{i,t} \quad (2)$$

where $Dependent\,Var_{i,t}$ stands for one of the three aggregated analyst characteristics: $Coverage_{i,t}$, $Bias_{i,t}$, or $FError_{i,t}$. The vector of $Control_{j,i,t-12}$ represents the independent variables that control for firm characteristics: $LnSize_{i,t}$, $LnBM_{i,t}$, $Return_{i,t}$, $Profit_{i,t}$, $Sigma_{i,t}$, $VolROE_{i,t}$ (Appendix A). We estimate each regression using two-way cluster-robust standard errors in order to account for potential heteroskedasticity in our dataset, following Thompson's (2011) and Cameron, Gelbach, and Miller's (2011) formulas for calculating double-clustering robust covariance matrix estimators. The observations are clustered by firm and month.

If there is a collective trend among asset managers to reduce their spending for research in response to the research unbundling reform and, as a consequence, the sell-side dismisses part of its analysts, we expect to see a significant decrease in the coverage of European companies driven by MiFID II. Therefore, running regression (2) on $Coverage_{i,t}$, a negative β_1 implies that post-MiFID II coverage has decreased significantly.

Table II reports the results of regression (2) for coverage, bias, and forecast error, run on the entire sample of European firms. In column (1), the coefficient on $MiFID_t$ indicates that, in general, the analyst coverage of European stocks dropped by about four analysts after the regulation was implemented, the effect being statistically significant at the 1% level. To understand this change in relative terms, we log-transform the dependent variable, $Coverage_{i,t}$. Column (2) presents the results for $LnCoverage_{i,t}$. We find that after MiFID, companies lost, on average, around 24%[2] of their analysts. These results are in line with our expectations. The research unbundling reform requires asset managers to pay for research either from their own resources or charge their shareholders through an RPA account. Given the legal complexity of setting RPA accounts, the difficulty of putting a separate price on research that investors might not be willing to pay for, and the "peer-pressure" coming from other funds that took the decision to cover research internally, the P&L method has turned out to be the most employed method (CFA Institute, 2019). This implicitly led to reductions in their research budgets and to a more selective choice of research products from the sell side, which, in turn, had to adjust by laying off analysts.

If following a decrease in coverage the analysts dismissed were the ones with low ratings, and competition pressures the remaining analysts to produce quality research (Murphy, 2018a), we expect bias and forecast error to be lower after MiFID II. When we use $Bias_{i,t}$ or $FError_{i,t}$ as the dependent variable in regression (2), $\beta_1 < 0$ indicates that lower coverage after MiFID II is associated with less biased and more accurate analyst reports, respectively. On the other hand, $\beta_1 > 0$ implies that decreased coverage led to lower competition. This diminishes the disciplining effect among analysts and leads to less firm-related information being available, which fosters more biased and erroneous forecasts as a consequence of the new regulation.

After establishing that MiFID II is associated with a decrease in coverage, the results in Table II, Column (3) show that analyst bias decreases on average by about 3 pp (at the 5% significance level). Coherent results should indicate that forecast accuracy increases post-MiFID II. Accordingly, $FError_{i,t}$, as reported in Column (4), decreases by roughly 2 pp after MiFID II (at the 1% significance level). Relative to the unconditional mean of the dependent variables $Bias_{i,t}$ and $FError_{i,t}$, these effects represent changes of about 16% and 6%, respectively. These results together confirm our expectations. Bias is no longer fueled through bundling of services

[2] According to Hardy (1993), in semi-logarithmic regressions, the coefficient β in front of a dummy variable should be estimated using the following formula: 100*[exp(β)-1] (p. 57-58).

and over-optimistic reports to attract business, i.e., lower conflicts of interest. Additionally, the results support the prediction that the type of research cut was of poor quality and that remaining analysts tend to issue less biased and more accurate forecasts.

TABLE II

The effect of MiFID II on firm coverage and forecast quality. OLS model.

Note. This table presents the results from panel regressions of coverage and forecast quality (i.e., bias and error) based on the following model:

$$Dependent\ Var_{i,t} = \alpha_0 + \beta_1 MiFID_t + \sum_{j=1}^{6} \delta_j Control_{j,i,t-12} + \varepsilon_{i,t} \quad (2)$$

where $Dependent\ Var_{i,t}$ stands for $Coverage_{i,t}, LnCoverage_{i,t}, Bias_{i,t}$, or $FError_{i,t}$. $Coverage_{i,t}$ is the total number of estimates of the one-year forward EPS for stock i in month t. $LnCoverage_{i,t}$ is the natural logarithm of company i's analyst coverage in month t. Our proxy for analyst bias is the difference between the mean consensus forecasted EPS and the actual EPS of company i for month t, scaled by the absolute value of actual EPS. Our proxy for forecast error is the absolute value of analyst bias. $MiFID_t$ is an indicator variable that takes the value of 1 for periods after January 2018, and 0 otherwise. Other control variables include $LnSize_{i,t-12}$, $LnBM_{i,t-12}$, $Profit_{i,t-12}$, $Return_{i,t-12}$, $Sigma_{i,t-12}$, and $VolROE_{i,t-12}$ as defined in Appendix A. Data is obtained from IBES, Datastream, and Worldscope. All regression results are based on monthly measures of variables across 334,479 firm-month observations from EEA countries, between 2006-2019. Each regression uses two-way cluster-robust standard errors (by firm and month). *t*-statistics are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	$Coverage_{i,t}$		$LnCoverage_{i,t}$		$Mean\ Bias_{i,t}$		$Mean\ FError_{i,t}$	
	(1)		(2)		(3)		(4)	
$MiFID_t$	-3.7630	***	-0.2733	***	-0.0292	**	-0.0201	***
	(-18.97)		(-19.33)		(-2.47)		(-2.24)	
$LnSize_{i,t-12}$	3.5654	***	0.2871	***	0.0130	***	-0.0004	
	(67.10)		(71.13)		(5.51)		(-0.15)	
$LnBM_{i,t-12}$	0.4654	***	0.0063		0.0351	**	0.0874	***
	(3.78)		(0.59)		(2.72)		(7.47)	
$Profit_{i,t-12}$	5.3701	***	0.3299	***	-0.1907	**	-0.5397	***
	(4.80)		(3.63)		(-2.52)		(-6.22)	
$Return_{i,t-12}$	-0.0106		-0.0008		0.0002		-0.0001	
	(-1.38)		(-1.05)		(0.67)		(-0.36)	
$Sigma_{i,t-12}$	1.0865		0.1133	**	-0.0061		0.0924	**
	(1.52)		(2.25)		(-0.11)		(2.08)	
$VolROE_{i,t-12}$	0.5537	***	0.0378	***	-0.0217	***	0.0171	***
	(3.86)		(3.02)		(-3.13)		(2.98)	
Observations	334,479		334,479		334,479		334,479	
R^2	59.43%		59.04%		0.78%		4.17%	

Further on, we test whether the effects hold and whether their magnitude varies for companies with different capitalizations. Among the concerns regarding the implementation of

MiFID II, there was a fear that the regulation had a more negative impact on smaller firms, because they risk undergoing a larger drop in analyst following or even losing coverage completely and becoming less visible to investors. Additionally, it is assumed that large-capitalization equities would be the least impacted, as they are already "covered by dozens of analysts" (Murphy, 2018b). Thus, a decrease in coverage would not significantly hurt them. What is more, the remaining analysts, being forced to cover more companies at the same time, would focus on the most liquid stocks, paying less attention to small- and mid-cap equities. While we have found that, overall, research quality improved post-MiFID II, it is worth examining if that is the case for companies with different market capitalizations. We are able to compare the absolute and proportional decrease in coverage across large-, mid-, and small-capitalization companies, and the change in the quality of forecasts (i.e., aggregate forecast bias and error) for these companies.

To check the aforementioned aspects, we amend our initial model to include a classification of the observations based on their market capitalization:

$$Dependent\ Var_{i,t} = \alpha_0 + \beta_1 MiFID_t + \beta_2 MCAP_i + \gamma MiFID_t \times MCAP_i + \\ + \sum_{j=1}^{6} \delta_j Control_{j,i,t-12} + \varepsilon_{i,t} \quad (3)$$

where $MCAP_i$ is a three-level categorical variable whose coefficient will indicate the change in the dependent variable for small and mid-sized firms relative to large ones.

A positive γ implies that the effect of MiFID II on the particular dependent variable we test is higher for either small or midsize companies compared to large companies. For example, if the dependent variable in regression (3) is $Coverage_{i,t}$, a $\delta > 0$ for the interaction between $MiFID_t$ and $MCAPsmall_i$ implies that the incremental effect of MiFID II on the coverage of small-capitalization equities is higher than for large ones.

Table III reports the results for regressions with the same set of dependent variables ($Coverage_{i,t}$, $LnCoverage_{i,t}$, $Bias_{i.t}$ and $FError_{i,t}$), including the interaction between $MiFID_t$ and $MCAP_i$. We find that large firms lost around four analysts in the post-MiFID II period, while the coverage of midsize firms shrank by about two analysts. Small firms were dropped by one analyst on average (Column 1). In absolute terms, large companies have lost approximately two more analysts than mid- and small-capitalization companies (Column 1). Percentage-wise, the results in column (2) show that large companies have about 13% less coverage after MiFID (p <

0.01). In relative terms, we can confidently state ($p < 0.05$) that large-caps have lost more analysts than mid-caps by 4.12% after MiFID II (Column 2).

First, these findings confirm that the coverage of all three groups of equities has declined after the introduction of MiFID II. Second, they counter buy and sell sides' expectations that the analyst following of large-cap equities has remained the same (CFA Institute, 2019). On the contrary, these firms have lost even more coverage than mid-cap equities, for instance. However, this does not necessarily imply that large-caps were more affected by MiFID II than companies of smaller size since they were covered by more analysts, to begin with (Appendix B, Panel A).

We continue with inspecting the implications of decreased coverage on forecast quality for companies with different market capitalizations. Interestingly, the results in column (3) show that before MiFID, analyst bias in the consensus forecast for small companies was lower than bias for large companies by 6.42 pp. Small-caps also had 17 fewer analysts on average than large-caps before the regulation, as presented in Column (1). These findings challenge the theory stating that more competition through more coverage lowers the bias of analysts' reports (Hong & Kacperczyk, 2010; Merkley et al., 2017). Out of the three groups of companies, large companies have lost the most analysts (in absolute numbers) and experienced the greatest improvement in forecast accuracy post-MiFID II, as reported in Column (4) (i.e., forecast accuracy for large-caps improved by 4.45 pp more than for small-caps). This suggests that before MiFID II, the research market might have been oversupplied with low-quality reports, and sharp reductions in coverage, such as the one observed for large companies, indirectly lead to improved forecast quality by getting rid of poor research.

Although the significance levels of the coefficients of our control variables are not consistent across our regressions, we generally find that coverage, bias, and error increase with firms' sizes, book-to-market values, profits, and volatility of ROE. The effect of the daily variance of returns is ambiguous; we only find that the higher the stock volatility, the higher its forecast error ($p < 0.05$). We find no significant evidence that a stock's monthly return affects any of the dependent variables.

TABLE III

The effect of MiFID II on firm coverage and forecast quality. OLS model.

Note. This table presents the results from panel regressions of coverage and forecast quality (i.e., bias and error) based on the following model:

$$Dependent\ Var_{i,t} = \alpha_0 + \beta_1 MiFID_t + \beta_2 MCAP_i + \gamma MiFID_t \times MCAP_i + \sum_{j=1}^{6} \delta_j Control_{j,i,t-12} + \varepsilon_{i,t} \quad (3)$$

where $Dependent\ Var_{i,t}$ stands for $Coverage_{i,t}, LnCoverage_{i,t}, Bias_{i,t}$, or $FError_{i,t}$. $Coverage_{i,t}$ is the total number of estimates of the one-year forward EPS for stock i in month t. $LnCoverage_{i,t}$ is the natural logarithm of company i's analyst coverage in month t. Our proxy for analyst bias is the difference between the mean consensus forecasted EPS and the actual EPS of company i for month t, scaled by the absolute value of actual EPS. Our proxy for forecast error is the absolute value of analyst bias. $MiFID_t$ is an indicator variable that takes the value of 1 for periods after January 2018, and 0 otherwise. $MCAP_i$ is a three-level categorical variable that distinguished large, mid, and small companies. Other control variables include $LnSize_{i,t-12}$, $LnBM_{i,t-12}$, $Profit_{i,t-12}$, $Return_{i,t-12}$, $Sigma_{i,t-12}$, and $VolROE_{i,t-12}$ as defined in Appendix A. Data is obtained from IBES, Datastream, and Worldscope databases. All regression results are based on monthly measures of variables across 334,479 firm-month observations from EEA countries, between 2006-2019. Each regression uses two-way cluster-robust standard errors (by firm and month). *t*-statistics are reported in parentheses. ***,**, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	$Coverage_{i,t}$		$LnCoverage_{i,t}$		$Mean\ Bias_{i,t}$		$Mean\ FError_{i,t}$	
	(1)		(2)		(3)		(4)	
$MiFID_t$	-3.5616	***	-0.1362	***	-0.0225		-0.0454	**
	(-12.70)		(-9.61)		(-0.98)		(-2.41)	
$MCAPmid_i$	-8.3582	***	-0.4214	***	-0.0172		0.0250	
	(-21.17)		(-17.81)		(-1.01)		(1.59)	
$MCAPsmall_i$	-17.046	***	-1.1789	***	-0.0642	***	0.0207	
	(-48.10)		(-50.33)		(-4.58)		(1.58)	
$LnBM_{i,t-12}$	-0.3302	**	-0.0597	***	0.0328	***	0.0870	***
	(-2.51)		(-5.09)		(2.58)		(7.59)	
$Profit_{i,t-12}$	1.7014		-0.0075		-0.1996	***	-0.5478	***
	(1.43)		(-0.07)		(-2.66)		(-6.36)	
$Return_{i,t-12}$	-0.0078		-0.0007		0.0002		-0.0001	
	(-1.23)		(-1.08)		(0.65)		(-0.56)	
$Sigma_{i,t-12}$	0.3260		0.0426		-0.0096		0.0905	**
	(0.50)		(0.97)		(-0.17)		(2.04)	
$VolROE_{i,t-12}$	-0.3127	*	-0.0366	**	-0.0249	***	0.0162	***
	(-1.89)		(-2.14)		(-3.36)		(2.89)	
$MiFID_t \times MCAPmid_i$	1.7820	***	0.0404	**	-0.0280		0.0022	
	(6.19)		(2.42)		(-1.14)		(0.10)	
$MiFID_t \times MCAPsmall_i$	2.3387	***	0.0148		0.0147		0.0445	**
	(8.34)		(0.77)		(0.65)		(2.31)	
Observations	334,479		334,479		334,479		334,479	
R^2	52.92%		45.38%		0.78%		4.25%	

4.2 Mediation model

Following our results, we infer that the most likely explanation for the improvement in research quality post-MiFID II is the reduction in poor research, along with the increased pressure on remaining analysts to deliver high-quality reports. In this context, we hypothesize that MiFID II has an indirect effect on the bias and accuracy of analyst forecasts because the regulation impacts the number of analysts who follow a stock, which in turn has an influence on the quality of forecasts produced for that stock. In other words, we assume that $Coverage_{i,t}$ is a channel through which MiFID II could lower the level of analyst bias. To test whether this is indeed the case, we employ a method used by Malceniece, Malcenieks, & Putniņš (2018) and estimate a simple mediation model. The purpose of a mediation model is to provide more insight into the causal relationship between an independent and a dependent variable. Specifically, such models are applied in situations when there are reasonable grounds to believe that the regressor has not only a direct influence on the outcome variable but also an indirect one through an unobserved variable that "mediates" the relationship between them. With the help of a mediation model, we can obtain estimates for both the direct and indirect pathways by simultaneously running regressions with and without the mediator. If the result indicates that the total effect is equal to the indirect effect, while the direct effect of the predictor on the outcome becomes insignificant, we can conclude that the relationship is fully mediated. If instead both effects are significant and of the same sign, we can say that the relationship is partially mediated.

In our case, we estimate a mediation model by running the following set of regressions:

$$Coverage_{\,i,t} = \alpha + \beta_1 MiFID_t + \sum_{j=1}^{6} \delta_j Control_{j,i,t-12} + \varepsilon_{i,t} \quad (5)$$

$$Dependent\ Var_{i,t} = \alpha + \beta_0 MiFID_t + \beta_2 Coverage_{i,t} + \sum_{j=1}^{6} \delta_j Control_{j,i,t-12} + \varepsilon_{i,t} \quad (6)$$

where $Dependent\ Var_{i,t}$ stands for $Bias_{i,t}$ or $FError_{i,t}$, while $Control_{j,i,t-12}$ represents the same set of control variables described earlier (Appendix A). Here, β_0 provides an estimation for the direct effect of MiFID II on either analyst bias or forecast accuracy, while the product of β_1 and β_2 denotes the indirect effect. We expect all three coefficients to be significant, which would mean that a change in the bias and accuracy of analysts is partly due to the unmediated effect of the new regulation and partly due to the regulation's impact on analyst coverage.

The results from the mediation analysis are shown in Figure 1. Our findings support the hypothesis that coverage acts as a mediator in the relationship between MiFID II and analyst bias. We obtain significant coefficients for the indirect pathway, while the coefficient for the direct one becomes insignificant, suggesting that the relationship between bias and MiFID II is closer to a fully mediated relationship than a partially mediated one. In fact, 90.65% of the effect of the research unbundling reform on bias occurs through the decrease in the number of analysts who follow a company.

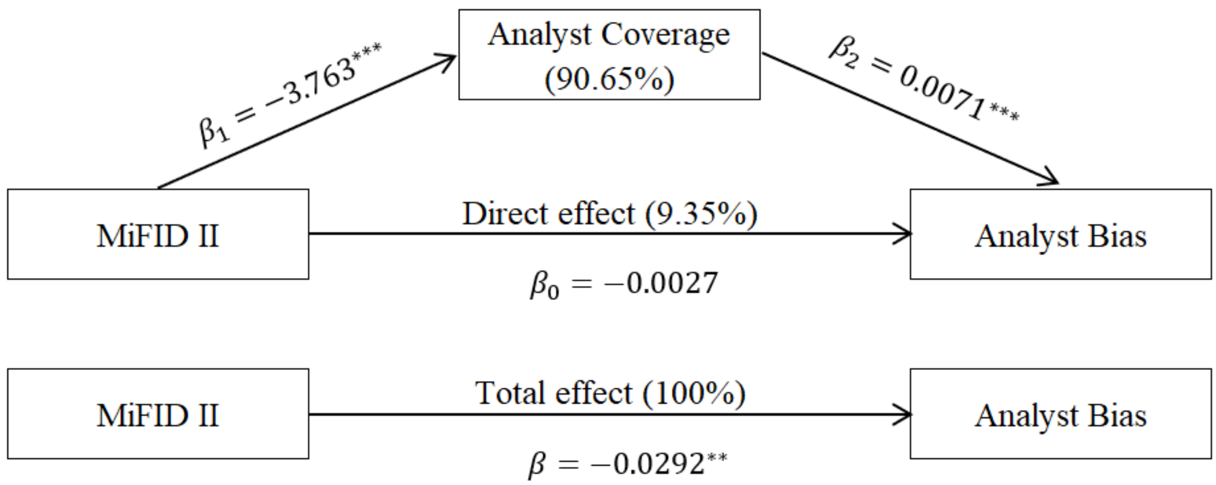

Figure 1. Direct and mediated effects of MiFID II on analyst bias. The graph illustrates the result of a simple mediation analysis of the channels through which MiFID II affects the bias in analyst forecasts. To derive the estimates, we simultaneously run the following set of panel regressions using stock-month (i, t) observations:

$$Coverage_{i,t} = \alpha + \beta_1 MiFID_t + \sum_{j=1}^{6} \delta_j Control_{j,i,t-12} + \varepsilon_{i,t}$$

$$Bias_{i,t} = \alpha + \beta_0 MiFID_t + \beta_2 Coverage_{i,t} + \sum_{j=1}^{6} \delta_j Control_{j,i,t-12} + \varepsilon_{i,t}$$

where $Bias_{i,t}$ is a measure of analyst bias, $MiFID_t$ is a categorical variable that takes the value of 1 for the years included in the post-MiFID II period, $Coverage_{i,t}$ is the number of analysts following stock i in month t, $Control_{j,i,t-12}$ is a set of six control variables: $LnSize_{i,t-12}$, $LnBM_{i,t-12}$, $Profit_{i,t-12}$, $Return_{i,t-12}$, $Sigma_{i,t-12}$, and $VolROE_{i,t-12}$. The regressions are estimated using heteroskedasticity-consistent standard errors. The percentages indicate what proportion of the total effect is expressed by each pathway. *, **, *** denote significance at 10%, 5%, and 1%, respectively.

A different result is reached when $FError_{i,t}$ acts as the dependent variable, as presented in Figure 2. The significant coefficients for both the direct and indirect pathways suggest a partially mediated effect on forecast error, of which 46.63% can be attributed to lower analyst following and 53.37% - to the direct influence of MiFID II on accuracy.

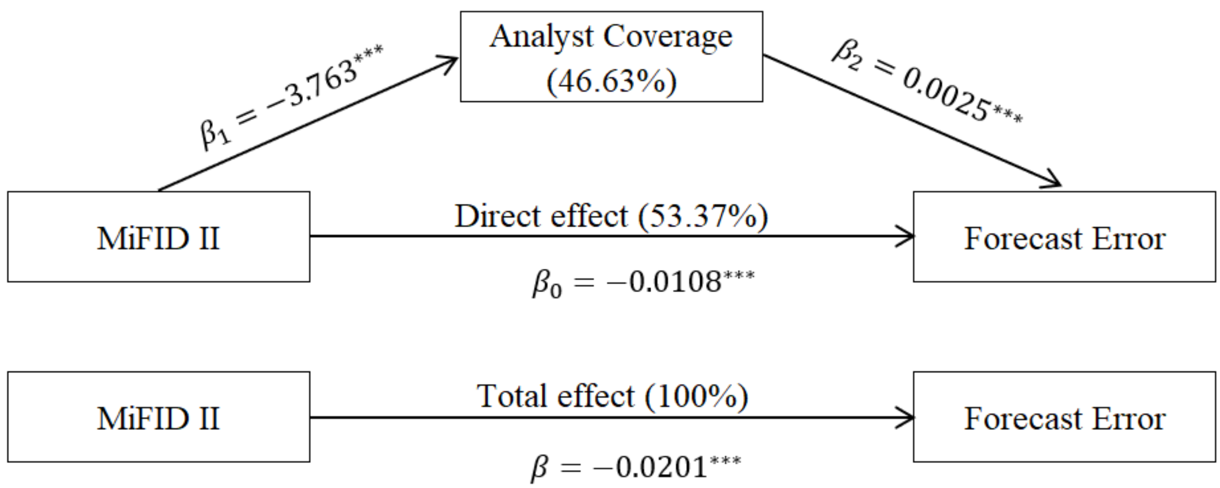

Figure 2. Direct and mediated effects of MiFID II on forecast error. The graph illustrates the result of a simple mediation analysis of the channels through which MiFID II affects the accuracy in analyst forecasts. To derive the estimates, we simultaneously run the following set of panel regressions using stock-month (i, t) observations:

$$Coverage_{\ i,t} = \alpha + \beta_1 MiFID_t + \sum_{j=1}^{6} \delta_j Control_{j,i,t-12} + \varepsilon_{i,t}$$

$$FError_{i,t} = \alpha + \beta_0 MiFID_t + \beta_2 Coverage_{i,t} + \sum_{j=1}^{6} \delta_j Control_{j,i,t-12} + \varepsilon_{i,t}$$

where $FError_{i,t}$ is a measure of forecast error, $MiFID_t$ is a categorical variable that takes the value of 1 for the years included in the post-MiFID II period, $Coverage_{\ i,t}$ is the number of analysts following stock i in month t, $Control_{j,i,t-12}$ is a set of six control variables: $LnSize_{i,t-12}$, $LnBM_{i,t-12}$, $Profit_{i,t-12}$, $Return_{i,t-12}$, $Sigma_{i,t-12}$, and $VolROE_{i,t-12}$. The regressions are estimated using heteroskedasticity-consistent standard errors. The percentages indicate what proportion of the total effect is expressed by each pathway. *, **, *** denote significance at 10%, 5%, and 1%, respectively.

The mediation model has, therefore, helped us quantify the role of coverage as a channel through which MiFID II led to a decrease in bias and forecast error. It is evident that coverage has substantially decreased specifically because of the research unbundling reform. We conjecture that the indirect effect of this decrease on improved research quality happened because the dismissed analysts were the ones producing the most biased and erroneous reports (Murphy, 2018a). The effect of MiFID II outside the coverage channel comes from the remaining analysts. Taking into account that research is explicitly paid for and that their peers are dismissed because of their poor-quality coverage, current analysts are more strongly incentivized to improve their forecasts.

4.3 Difference-in-Differences model

A significant drawback of the OLS regressions is that they do not allow us to disentangle the variation in the dependent variable that originates from confounding time series. A change in a certain measure is not necessarily the consequence of an external shock, such as MiFID II. It could be the effect of natural fluctuations in the level of the variable. To take into consideration these time trends, a common strategy is to apply a difference-in-differences (DiD) methodology (Merkley et al., 2017; Hong & Kacperczyk, 2010). This is an approach that allows us to separate the variation in an outcome variable caused by an external event from the variation that is a part of a common trend. The DiD estimator measures the partial effect of a change in the level of a variable, such as analyst coverage or bias, due to MiFID II, by calculating the difference between the change over time in a treatment group and the change over time in a control group. For the purpose of this paper, the treatment group represents the sample of European stocks, and the control group consists of US stocks, observed over the same time period. While the sample of US stocks does not represent a perfect control group, it consists of firms that are generally similar to their European counterparts from an economic standpoint but are not directly regulated by MiFID II.

A critical assumption that the DiD method relies on is the parallel trend assumption, which states that the outcome variable in the treatment and control groups should vary by a fixed amount over time and their variation should exhibit common period-specific patterns. Therefore, in order to obtain a meaningful DiD estimator, the treatment and control groups should be similar in terms of the movement of variables in the pre-treatment period, i.e., the covariates should have similar distributions. To ensure that, we employ a matching technique introduced by Iacus, King, and Porro (2011) called Coarsened Exact Matching (CEM). Each European firm is matched with its nearest US neighbor with respect to the pre-MiFID II values for $Bias_{i,t}$, $FError_{i,t}$, $Coverage_{i,t}$, $LnSize_{i,t}$, and $LnBM_{i,t}$. The summary statistics for the treatment and control groups before and after the matching process are presented in Panels C & D, Appendix B.

Using the sample of matched observations from the control and treatment groups, we can estimate the following model:

$$Dependent\ Var_{i,t} = \alpha_0 + \beta_1 MiFID_t + \beta_2 TREAT_i + \beta_3 MiFID_t \times TREAT_i + \\ + \gamma Controls_{i,t-12} + \varepsilon_{i,t}, \qquad (7)$$

where $TREAT_i$ is a categorical variable that is equal to 1 for European firms, which captures the distinction between the control and the treatment group. The interaction between $MiFID_t$ and $TREAT_i$ is the DiD estimator, whose coefficient indicates the difference between changes in the dependent variable over time.

If the drop in coverage, forecast accuracy, and analyst bias is caused by the implementation of MiFID II rather than by an overall downward trend, we expect to see a stronger decrease in the sample of European companies. When we run regression (7) on each of the dependent variables, a negative β_3 coefficient for the DiD estimator would indicate that the effect of MiFID II is incrementally lower for European firms than for US firms, which is what we expect to find.

The results from the DiD regressions on the entire sample of companies in the EEA and the US are reported in Table IV. The negative interaction coefficients in Columns (1) and (2) suggest that, overall, coverage of European firms after MiFID II is incrementally lower by about one analyst and 6.5%, respectively, compared to the coverage of US firms. These results show that the decrease in the coverage of European firms is indeed driven by MiFID II and does not reflect a general time trend. Additionally, in columns (3) and (4), we find that both $Bias_{i,t}$ and $FError_{i,t}$ are lower in Europe than in the US by 11 pp and 12 pp, respectively. All the coefficients hold at the 1% significance level. We can conclude that, overall, after MiFID II, the European research market has become less biased and more accurate than its US counterpart. The research unbundling reform has successfully addressed the conflicts of interest analysts were subject to and incentivized them to produce more accurate forecasts.

Finally, we check whether our expectation that market capitalization is a crucial factor in determining the impact of MiFID II on a firm's forecasts will be confirmed after controlling for the variation that is generated by a common trend. To avoid having to interact the DiD estimator with $Mcap_i$ and running a regression with a three-way interaction, we divide our full sample instead into three separate samples based on the classification offered by $Mcap_i$. Thus, we can estimate model (3) for samples of large-cap, mid-cap, and small-cap companies, which enables us to observe whether the coefficient of the DiD estimator remains significant for firms of different market sizes.

Table IV

The effect of MiFID II on firm coverage and forecast quality. DiD model.

Note. This table presents the results from panel regressions of coverage and forecast quality (i.e., bias and error), on the entire matched sample of EEA and US companies, based on the following model:

$$Dependent\ Var_{i,t} = \alpha + \beta_1 MiFID_t + \beta_2 TREAT_i + \beta_3 MiFID_t \times TREAT_i + + \gamma Controls_{i,t-12} + \varepsilon_{i,t}, \quad (7)$$

where $Dependent\ Var_{i,t}$ stands for $Coverage_{i,t}, LnCoverage_{i,t}, Bias_{i,t}$, or $FError_{i,t}$. $Coverage_{i,t}$ is the total number of estimates of the one-year forward EPS for stock i in month t. $LnCoverage_{i,t}$ is the natural logarithm of company i's analyst coverage in month t. Our proxy for analyst bias is the difference between the mean consensus forecasted EPS and the actual EPS of company i for month t, scaled by the absolute value of actual EPS. Our proxy for forecast error is the absolute value of analyst bias. $MiFID_t$ is an indicator variable that takes the value of 1 for periods after January 2018, and 0 otherwise. $TREAT_i$ is an indicator variable that takes the value of 1 for companies from EEA countries, and 0 for US companies. Other control variables include $LnSize_{i,t-12}$, $LnBM_{i,t-12}$, $Profit_{i,t-12}$, $Return_{i,t-12}$, $Sigma_{i,t-12}$, and $VolROE_{i,t-12}$ as defined in Appendix A. Data is obtained from IBES, Datastream, and Worldscope databases. All regression results are based on monthly measures of variables across 469,352 firm-month observations from EEA and US countries, between 2006-2019. Each regression uses two-way cluster-robust standard errors (by firm and month). *t*-statistics are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	$Coverage_{i,t}$		$LnCoverage_{i,t}$		$Mean\ Bias_{i,t}$		$Mean\ FError_{i,t}$	
	(1)		(2)		(3)		(4)	
$MiFID_t$	-2.0918	***	-0.1974	***	0.1147	***	0.1489	***
	(-13.20)		(-15.17)		(7.03)		(9.51)	
$TREAT_i$	2.3476	***	0.1584	***	-0.017	**	0.0096	
	(13.77)		(11.34)		(-2.34)		(1.38)	
$LnSize_{i,t-12}$	3.4838	***	0.2903	***	0.0055	***	-0.0064	***
	(80.54)		(86.45)		(3.48)		(-3.99)	
$LnBM_{i,t-12}$	0.1468		-0.0099		-0.0217	***	0.0368	***
	(1.48)		(-1.14)		(-3.28)		(6.10)	
$Profit_{i,t-12}$	5.6062	***	0.4077	***	-0.3855	***	-0.5408	***
	(6.23)		(5.34)		(-7.42)		(-9.77)	
$Return_{i,t-12}$	-0.0097		-0.0007		0.0002		0.0001	
	(-1.37)		(-1.06)		(0.76)		(0.28)	
$Sigma_{i,t-12}$	0.3057		0.0777		-0.0144		0.0895	***
	(0.59)		(1.58)		(-0.36)		(2.80)	
$VolROE_{i,t-12}$	0.2542	***	0.0138		-0.0275	***	0.0031	
	(2.74)		(1.61)		(-5.19)		(0.88)	
$MiFID_t \times TREAT_i$	-1.3859	***	-0.0669	***	-0.1116	***	-0.1248	***
	(-7.79)		(-4.75)		(-5.73)		(-7.49)	
Observations	469,352		469,352		469,352		469,352	
R^2	59.16%		58.76%		0.98%		3.14%	

Appendix D reports the results from regressions on samples with large, midsize, and small companies. As for the results on the entire matched sample, we expect the interaction coefficients to be negative and significant, suggesting that our dependent variables are incrementally lower in the EU than in the US post-MiFID II. Subsequently, we find such coefficients in the regressions for all three groups of companies. If we compare the magnitude of MiFID's effect on coverage, we observe that large companies lose more analysts than midsize and small companies, both in absolute values (Column 1) and percentage changes (Column 2). Specifically, the coverage of large firms falls by around 16%, while mid-sized firms lose around 10% of their analysts and small firms - around 7%. Similarly, for the change in bias and forecast error in Europe compared to the US, we obtain negative significant interaction coefficients across all samples, showing that MiFID II led to an incrementally bigger reduction in erroneous forecasts of European companies of all market sizes. It is worth noting that conducting regressions separately for each group of companies does not capture the relative effect based on market capitalization. Thus, we refrain from comparing the extent of the effects across the three samples. Nevertheless, the results of the DiD model allows us to conclude that the drop in coverage of European companies and the increase in research quality have been conditioned by MiFID II and cannot be attributed to widespread movements in the level of the variables.

4.4 Market consequences

Our results show that analyst bias and forecast error have decreased or, in other words, that the quality of research has improved in EEA countries as a consequence of MiFID II. In this context, several studies show that superior analyst performance, among other factors, is linked to higher informativeness and a stronger impact on price dynamics (Stickel, 1992; Clement & Tse, 2003; Merkley et al., 2017). Therefore, our goal is to observe whether market reactions to forecast revisions in EEA countries are stronger after MiFID II by comparing the effect of changes in EPS estimates on stock returns in the pre- and post-MiFID II periods.

Unlike in previous sections, here we employ a daily time series. For each company in our sample, we retrieve the daily stock price and aggregated EPS estimate from 2010 to 2019. Using this data, we estimate the following first-difference estimator model, which helps us control for time-invariant unobserved heterogeneity (Wooldridge, 2001):

$$CumRet_{i,t} = \beta_1 MiFID_t + \beta_2 \Delta ForecastedEPS_{i,t} + \beta_3 MiFID_t \times \Delta ForecastedEPS_{i,t} + \varepsilon_{i,t} \quad (8)$$

where $CumRet_{i,t}$ represents the cumulative daily returns of company i from day $t-1$ to day $t+1$, $\Delta ForecastedEPS_{i,t}$ stands for the first-differenced (day t less day $t-1$) aggregated EPS forecast of company i, and $MiFID_t$ is an indicator variable that takes the value of 1 for periods after January 2018, and 0 otherwise. We include stock fixed effects to the regression.

The coefficient β_2 is the first-difference estimator, which we interpret as the analyst informativeness in the pre-MiFID II period since it measures the effect of forecast revisions on stock returns. Our coefficient of interest is β_3, which denotes the incremental change in analyst informativeness after the implementation of MiFID II. A significant β_3 would suggest that in the post-MiFID II period, earnings forecast changes are more informative than they were before the directive came into effect, i.e. there is a stronger market reaction to such changes.

Panel A of Table V reports the results of regression (8). Consistent with *Hypothesis 4*, we obtain a significant coefficient ($p < 0.01$) for the interaction term (Column 1), which indicates that, generally, revisions in earnings forecasts are more informative in the post-MiFID II period. These results support our previous evidence on the improved quality of research after MiFID II. We find that earnings forecasts bring new information content to the market and have a more prominent effect on prices.

Additionally, we test whether the price reaction is conditional on the type of forecast revisions. Thus, we categorize forecast revisions in two groups: upgrades (UP), if $\Delta ForecastedEPS_{i,t} > 0$, and downgrades ($DOWN$), if $\Delta ForecastedEPS_{i,t} < 0$ and estimate the following model:

$$CumRet_{i,t} = \delta_0 + \beta_1 MiFID_t + \beta_2 RevisionType_{i,t} + \beta_3 MiFID_t \times RevisionType_{i,t} + \varepsilon_{i,t} \quad (9)$$

where $RevisionType_{i,t}$ is a categorical variable that indicates whether the change in the analyst forecast is an upgrade or a downgrade.

Once again, we focus on the coefficient of the interaction term, β_3. A significant positive β_3 would imply that, after MiFID II, upward earnings forecasts revisions elicit a stronger market reaction than downward ones. Similarly, a negative β_3 corresponds to a stronger market reaction after MiFID II to downward revisions relative to upward ones. Panel B of Table V reports the results of regression (9). We observe that the coefficient of interest is negative (Column 1), leading us to conclude that the informativeness of downgrades is higher than that of upgrades in the post-MiFID II period. The coefficient is significant at the 1% significance level. This result suggests that investors tend to value downgrades more than upgrades, which is in line with the main conclusions of Hirst et al. (1995) and Asquith et al. (2005). According to Ramnath, Rock, and Shane (2008), downgraded analyst estimates are more trustworthy than upgraded ones due to the analysts' well-known tendency to issue positively biased forecasts. This perception seems to persist among investors even after MiFID II, despite the observed trend of decreasing bias. As investors learn more about the effects of the new directive, this perception could be subject to change (Chen et al. 2005).

Table V

The change in market reaction to forecast revisions after MiFID II

Note. This table provides panel regressions of daily stock returns on first-differenced aggregated EPS forecasts for each stock-day observation. Panel A reports the coefficients for the following model:

$CumRet_{i,t} = \beta_1 MiFID_t + \beta_2 \Delta ForecastedEPS_{i,t} + \beta_3 MiFID_t \times \Delta ForecastedEPS_{i,t} + \varepsilon_{i,t}$ (8),

where $CumRet_{i,t}$ represents the cumulative daily returns of company i from day $t-1$ to day $t+1$, $\Delta ForecastedEPS_{i,t}$ stands for the first-differenced (day t less day $t-1$) aggregated EPS forecast of company i, and $MiFID_t$ is an indicator variable that takes the value of 1 for periods after January 2018, and 0 otherwise. Panel B refers to the model:

$CumRet_{i,t} = \delta_0 + \beta_1 MiFID_t + \beta_2 RevisionType_{i,t} + \beta_3 MiFID_t \times RevisionType_{i,t} + \varepsilon_{i,t}$ (9),

where $RevisionType_{i,t}$ is a categorical variable that indicates whether the change in the analyst forecast is an upgrade or a downgrade. In each panel, Column (1) and (2) present the results for the mean and median consensus EPS forecast, respectively. In column (3), we run the regression using mean consensus EPS forecasts on the "cleaner" version of the corresponding sample, i.e. excluding years 2016 and 2017. All regressions include stock fixed effects.

Panel A: Market reaction. Entire sample.

	Cumulative daily stock returns					
	(1)		(2)		(3)	
$MiFID_t$	-21.857	***	-21.599	***	-24.069	***
	(-55.72)		(-54.95)		(-54.20)	
$\Delta ForecastedEPS_{i,t}$	-0.0143	***	-0.0133	***	-0.0120	***
	(-8.39)		(-7.98)		(-3.09)	
$MiFID_t \times \Delta ForecastedEP$	0.0159	***	0.0148	***	0.0135	***
	(5.58)		(5.56)		(2.98)	
Observations	4,530,703		4,498,461		3,513,420	
Stock-fixed effects	Yes		Yes		Yes	
R^2	0.07%		0.07%		0.08%	

Panel B: Market reaction. Upgrade vs Downgrade samples.

	Cumulative daily stock returns					
	(1)		(2)		(3)	
$MiFID_t$	-0.0019	***	-0.0019	***	-0.0023	***
	(-36.34)		(-35.31)		(-38.78)	
$RevisionType_{i,t}$	0.0052	***	0.0053	***	0.0049	***
	(139.09)		(140.03)		(106.19)	
$MiFID_t \times RevisionType_{i,t}$	-0.0005	***	-0.0005	***	-0.0001	***
	(-6.61)		(-7.27)		(-1.50)	
Observations	4,530,703		4,498,461		3,513,420	
Stock-fixed effects	Yes		Yes		Yes	
R^2	0.63%		0.64%		0.58%	

4.5 Robustness checks

To check the robustness of our coefficients, we estimate the proposed models with some slight modifications. First, we run the regressions using the median values of the analysts' estimates as the consensus EPS forecast instead of the mean value. The results from the first robustness check are reported in Panel A and C of Appendix E. Second, although MiFID II was applied to the EU member states in 2018, it was formally adopted by EU institutions as early as 2014. Consequently, some sell-side companies could have started to react to the impending regulation before it came into effect, which could lead to a decrease in the significance of our coefficients. Therefore, we remove observations from the years 2016 and 2017 in order to obtain a "cleaner" sample on which to run the initial regressions. The results from the second robustness check are reported in Panel B and D of Appendix E. Following both of our analyses, we conclude that our main results for testing changes in research quality using mean values of our measures across the full sample are robust to different measures and sample adjustments.

We run the same robustness checks on our market reaction models. The results from regressions using the median values of consensus EPS forecasts are reported in Column (2) of Table V, while Column (3) shows the results of the regressions run on a "cleaner" sample. We find that the coefficient for the interaction between $MiFID_t$ and $\Delta ForecastedEPS_{i,t}$ keeps its significance as we modify the specifications of our sample. Therefore, we conclude that our results are robust and can say with confidence that the market reaction to forecast revisions is stronger after MiFID II.

5. Conclusion

We provide one of the first assessments of how the MiFID II research unbundling reform, which requires investment firms to separate the charges related to research from the charges related to securities dealing, impacted the quantity and quality of stock analyst research.

Our results show that MiFID II resulted in a substantial reduction in analyst coverage of European stocks, partly due to low-rated analysts being dropped. As the remaining analysts work under stronger pressure of producing high-quality reports, we find that both earnings forecast error and analyst bias decrease for large-, mid-, and small-capitalization equities. To clearly distinguish the effect of the regulation from time-trends in research quality, we estimate difference-in-differences models using a matched sample of US companies, where MiFID II is not applicable. We find that the downward pressure on coverage, analyst bias, and forecast error in Europe is attributed to the MiFID II unbundling reform. Moreover, as research quality is improved in the post-MiFID II period, we find evidence that the market reaction to forecast revisions is significantly stronger.

Our study is conducive to the success of MiFID II at lowering conflicts of interest and making the research market more competitive. The unbundling reform has overall improved investors' protection and created conditions for higher quality research production. These findings imply that the free research distributed as part of the bundled services before MiFID II was often of poor quality. Now, when investors are asked to pay for it, they will generally do it only if it is worth their money.

6. References

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7. Appendices

Appendix A. *Variable definitions*

Variable	Definition	Source
Dependent variables		
$\boldsymbol{Bias_{i,t}}$	Measure of analyst bias, calculated as the difference between the consensus forecasted EPS and the actual EPS of company i for month t, scaled by the absolute value of actual EPS: $$Bias_{i,t} = \frac{Consensus\ EPS_{i,t} - EPS_{i.t}}{\lvert EPS_{i,t} \rvert}$$	I/B/E/S Datastream
$\boldsymbol{FError_{i,t}}$	Measure of forecast accuracy, calculated as the absolute value of analyst bias: $$FError_{i,t} = \lvert Bias_{i,t} \rvert$$	I/B/E/S Datastream
$\boldsymbol{Coverage_{i,t}}$	Total number of estimates of the one-year forward EPS for stock i for month t	I/B/E/S
$\boldsymbol{LnCOV_{i,t}}$	The natural logarithm of company i's analyst coverage in month t	I/B/E/S
Independent variables		
$\boldsymbol{MiFID_t}$	Indicator variable that takes the value of 1 for periods after January 2018, and 0 otherwise	Datastream
$\boldsymbol{TREAT_i}$	Indicator variable that takes the value of 1 for companies from EEA countries, and 0 for U.S. companies	Datastream
$\boldsymbol{LnSize_{i,t}}$	The natural logarithm of company i's market capitalization in month t, where market capitalization is the product of the share price ($P_{i,t}$) and the number of shares outstanding ($NOSH_{i,t}$) for that month: $$LnSize_{i,t} = log\ log\ (P_{i,t} \times NOSH_{i,t})$$	Datastream
$\boldsymbol{LnBM_{i,t}}$	The natural logarithm of company i's book-to-market ratio, calculated as the book value divided by the market capitalization in month t: $$LnBM_{i,t} = log\left(\frac{Book\ Value_{i,t}}{Market\ Capitalization_{i,t}}\right)$$	Datastream
$\boldsymbol{Profit_{i,t}}$	The ratio of company i's operating income to the total value of its assets in month t: $$Profit_{i,t} = \frac{Operating\ Income_{i,t}}{Total\ Assets_{i,t}}$$	Worldscope
$\boldsymbol{Return_{i,t}}$	The return on stock i in month t:	Datastream

	$$Return_{i,t} = \frac{P_{i,t} - P_{i,t-1}}{P_{i,t-1}}$$	
$\boldsymbol{Sigma_{i,t}}$	The variance of daily returns on stock i during month t: $$Sigma_{i,t} = \frac{\sum_{j=1}^{n}\left(r_{j,i,t} - \overline{r_{i,t}}\right)^2}{n-1}$$	Datastream
$\boldsymbol{VolROE_{i,t}}$	The volatility of $ROE_{i,t}$, where $ROE_{i,t}$ is the ratio of net income available to common stockholders divided by the book value of equity in month t. We run an AR(1) model for each company's ROE: $$ROE_{i,t} = \alpha + \varphi_1 ROE_{i,t-1} + \varepsilon_{i,t}$$ Then for each observation, we calculate $VolROE_{i,t}$ as the variance of the residuals over the last year obtained from this regression: $$VolROE_{i,t} = \frac{\sum_{t=1}^{n}\left(residuals_{i,t} - \overline{residuals_{i,t}}\right)^2}{n-1}$$	Worldscope
$\boldsymbol{MCAP_i}$	A three-level categorical variable, that classifies firms into 3 groups according to their market capitalization. The firms whose market capitalization is higher than the 85th percentile are identified as large-cap firms, those with market capitalizations between 60th and the 85th percentile represent the mid-cap firms, while the remaining companies are classified as small-cap	Datastream
$\boldsymbol{CumRet_{i,t}}$	The cumulative daily returns of company i from day $t-1$ to day $t+1$	Datastream
$\boldsymbol{\Delta ForecastedEPS}$	The first-differenced (day t less day $t-1$) aggregated EPS forecast for stock i	I/B/E/S
$\boldsymbol{RevisionType_{i,t}}$	The type of the forecast revision: upgrades (UP), if $\Delta ForecastedEPS_{i,t} > 0$, and downgrades ($DOWN$), if $\Delta ForecastedEPS_{i,t} < 0$.	I/B/E/S

Appendix B. *Descriptive statistics for the EEA and US samples.*

Panel A. Coverage by company size (EEA). Entire sample.				
	Number	Mean Coverage	Median Coverage	SD
Large	61,452	24.33	25	7.49
Midsize	87,718	16.32	16	6.76
Small	185,309	7.72	6	5.44
Panel B. Coverage by company size (US). Entire sample.				
	Number	Mean Coverage	Median Coverage	SD
Large	28,208	21.31	21	7.71
Midsize	48,498	13.38	13	7.11
Small	89,347	6.74	6	4.45

Panel C. Sample sizes for matching

	Treated	Control
All	135,879	270,561
Matched	125,519	250,638
Unmatched	10,360	19,833
Discarded	0	90

Note. This table presents the sizes of the European and American samples. The *Treated* sample includes European companies. The *Control* sample includes American companies. Data is obtained from IBES and Datastream databases.

Panel D. Summary of balance for all data and matched data

	All data			Matched data		
	Means Treated	Means Control	SD Control	Means Treated	Means Control	SD Control
$Bias_{i,t}$	0.18	0.25	0.58	0.15	0.15	0.46
$FError_{i,t}$	0.33	0.36	0.55	0.28	0.28	0.41
$Coverage_{i,t}$	13.35	11.15	8.00	12.84	12.82	8.83
$LnSize_{i,t}$	14.43	14.51	1.79	14.37	14.41	1.90
$LnBM_{i,t}$	-0.65	-0.81	0.78	-0.68	-0.69	0.69

Note. This table presents summary statistics for the variables used for matching the sample of European and American companies. The *Treated* sample includes European companies. The *Control* sample includes American companies. Data is obtained from IBES, Datastream, and Worldscope databases.

Panel E: Distribution of European stock-month observations by country

Country	Full sample	
	Frequency	Percent
Austria	8,667	2.59%
Belgium	10,728	3.21%
Bulgaria	40	0.01%

Croatia	205	0.06%
Czech Republic	1,603	0.48%
Denmark	9,479	2.83%
Estonia	136	0.04%
Finland	13,860	4.14%
France	54,250	16.22%
Germany	49,240	14.72%
Greece	3,271	0.98%
Hungary	1,263	0.38%
Iceland	46	0.01%
Ireland	5,045	1.51%
Italy	19,740	5.90%
Latvia	52	0.02%
Lithuania	113	0.03%
Luxembourg	1,646	0.49%
Netherlands	14,062	4.20%
Norway	12,581	3.76%
Poland	3,909	1.17%
Portugal	3,325	0.99%
Romania	413	0.12%
Slovenia	327	0.10%
Spain	17,770	5.31%
Sweden	27,257	8.15%
United Kingdom	75,451	22.56%
Total	**334,479**	**100.00%**

Note. This table presents the sample of stock-month observations for countries from the European Economic Area. Data is obtained from Datastream.

Appendix C. *Tests in the differences-of-means.*

Panel A: Coverage

	n	Mean	SD	t-value	p	Decision
Pre-MiFID	270,561	13.3521	9.21	47.494	2.2×10^{-16}	Reject
Post-MiFID	63,918	11.6664	7.78			

Note. This table reports the results of a test in the difference-of-means of coverage before and after MiFID II, on a sample of 334,479 EEA firm-month observations. The null hypothesis (*True difference is equal to 0*) is rejected. Data is obtained from IBES.

Panel B: Mean Bias

	n	Mean	SD	t-value	p	Decision
Pre-MiFID	270,561	0.1847	54.00	10.73	2.2×10^{-16}	Reject
Post-MiFID	63,918	0.1606	50.00			

Note. This table reports the results of a test in the difference-of-means of analyst bias before and after MiFID II, on a sample of 334,479 EEA firm-month observations. The null hypothesis (*True difference is equal to 0*) is rejected. Data is obtained from IBES.

Panel C: Mean Forecast Error

	n	Mean	SD	t-value	p	Decision
Pre-MiFID	270,561	0.3321	49.00	13.925	2.2 x 10^{-16}	Reject
Post-MiFID	63,918	0.3039	45.00			

Note. This table reports the results of a test in the difference-of-means of analyst forecast error before and after MiFID II, on a sample of 334,479 EEA firm-month observations. The null hypothesis (*True difference is equal to 0*) is rejected. Data is obtained from IBES.

Appendix D. *Difference-in-differences models.*

Panel A: LARGE companies

	$Coverage_{i,t}$		$LnCoverage_{i,t}$		$Mean\ Bias_{i,t}$		$Mean\ FError_{i,}$	
	(1)		(2)		(3)		(4)	
$MiFID_t$	0.2901		0.0158		0.1386	***	0.1524	***
	(0.70)		(0.76)		(4.22)		(4.90)	
$TREAT_i$	4.3018	***	0.1856	***	-0.027	*	-0.022	
	(8.79)		(6.63)		(-1.76)		(-1.55)	
$LnSize_{i,t-12}$	2.1432	***	0.1006	***	-0.0181	***	-0.0254	***
	(10.62)		(8.56)		(-2.77)		(-4.14)	
$LnBM_{i,t-12}$	0.8527	***	0.0390	**	-0.0052		0.0162	***
	(2.65)		(2.19)		(-0.42)		(1.41)	
$Profit_{i,t-12}$	17.1374	***	0.8340	***	-0.437	***	-0.568	***
	(5.31)		(4.77)		(-4.29)		(-6.01)	
$Return_{i,t-12}$	0.8194	**	0.0395	**	-0.0259		-0.0398	**
	(2.56)		(2.27)		(-1.57)		(-2.57)	
$Sigma_{i,t-12}$	-2.4131		-0.0830		0.0867		0.1704	***
	(-1.49)		(-1.17)		(0.85)		(4.90)	
$VolROE_{i,t-12}$	0.6924		0.0328		-0.0418	*	-0.031	*
	(0.85)		(0.90)		(-1.87)		(-1.73)	
$MiFID_t \times TREAT_i$	-4.1294	***	-0.1711	***	-0.0876	**	-0.103	***
	(-8.78)		(-7.09)		(-1.97)		(-2.64)	
Observations	77,850		77,850		77,850		77,850	
R^2	15.68%		10.78%		1.61%		3.32%	

Panel B: MIDSIZE companies

	$Coverage_{i,t}$		$LnCoverage_{i,t}$		$Mean\ Bias_{i,t}$		$Mean\ FError_{i,t}$	
	(1)		(2)		(3)		(4)	
$MiFID_t$	-1.4732	***	-0.0945	***	0.1181	***	0.1243	***
	(-5.64)		(-4.88)		(4.92)		(5.87)	
$TREAT_i$	2.8495	***	0.2173	***	-0.0088		0.0030	
	(7.97)		(7.97)		(-0.61)		(0.22)	
$LnSize_{i,t-12}$	2.9081	***	0.2102	***	-0.0120	**	-0.0126	**

	(20.62)		(19.11)		(-2.14)		(-2.32)	
$LnBM_{i,t-12}$	-0.0074		-0.0107		-0.0172	*	0.0160	*
	(-0.04)		(-0.72)		(-1.74)		(1.72)	
$Profit_{i,t-12}$	6.7650	***	0.5304	***	-0.3748	***	-0.6728	***
	(3.52)		(3.69)		(-5.14)		(-9.04)	
$Return_{i,t-12}$	-0.0028	*	-0.0001		0.0002		0.0000	
	(-1.68)		(-0.68)		(0.58)		(-0.16)	
$Sigma_{i,t-12}$	0.4846		0.0462		-0.0529		-0.0065	
	(0.80)		(1.16)		(-1.59)		(-0.16)	
$VolROE_{i,t-12}$	0.0305		-0.0185		-0.0372	***	0.0079	**
	(0.29)		(-1.53)		(-3.39)		(2.17)	
$MiFID_t \times TREAT_i$	-1.7464	***	-0.1091	***	-0.1328	***	-0.1176	***
	(-5.85)		(-5.05)		(-4.87)		(-5.07)	
Observations	128,424		128,424		128,424		128,424	
R^2	19.29%		18.66%		0.87%		2.74%	

Panel C: SMALL companies

	$Coverage_{i,t}$		$LnCoverage_{i,t}$		$Mean\ Bias_{i,t}$		$Mean\ FError_{i,t}$	
	(1)		(2)		(3)		(4)	
$MiFID_t$	-1.7279	***	-0.2126	***	0.1187	***	0.1517	***
	(-11.82)		(-13.09)		(6.57)		(8.51)	
$TREAT_i$	1.5124	***	0.1438	***	-0.0152		0.0296	***
	(9.06)		(8.19)		(-1.56)		(3.13)	
$LnSize_{i,t-12}$	2.3920	***	0.2927	***	0.0104	***	0.0183	***
	(38.73)		(47.82)		(2.91)		(5.55)	
$LnBM_{i,t-12}$	-0.0924		-0.0076		-0.0283	***	0.0607	***
	(-0.91)		(-0.67)		(-2.98)		(7.31)	
$Profit_{i,t-12}$	3.1299	***	0.3147	**	-0.3755	***	-0.4809	***
	(3.90)		(3.51)		(-5.51)		(-6.72)	
$Return_{i,t-12}$	-0.0590	*	-0.0067	*	0.0006		0.0011	*
	(-1.74)		(-1.82)		(0.80)		(1.83)	
$Sigma_{i,t-12}$	0.6450		0.1061		-0.0176		0.1115	***
	(1.03)		(1.46)		(-0.29)		(3.14)	
$VolROE_{i,t-12}$	0.1790		0.0210		-0.0247	***	0.0037	
	(1.57)		(1.47)		(-4.28)		(0.67)	
$MiFID_t \times TREAT_i$	-0.7758	***	-0.0697	***	-0.1187	***	-0.1344	***
	(-5.08)		(-4.07)		(-5.90)		(-7.44)	
Observations	263,078		263,078		263,078		263,078	
R^2	33.52%		37.55%		1.03%		3.62%	

Note. This table presents the results from panel regressions of coverage and forecast quality (i.e., bias and error), on the entire matched sample of EEA and US companies, based on the following model:

$$Dependent\ Var_{i,t} = \alpha + \beta_1 MiFID_t + \beta_2 TREAT_i + \beta_3 MiFID_t \times TREAT_i + + \gamma Controls_{i,t-12} + \varepsilon_{i,t}, \quad (7)$$

where $Dependent\ Var_{i,t}$ stands for $Coverage_{i,t}, LnCoverage_{i,t}, Bias_{i,t}$, or $FError_{i,t}$. $Coverage_{i,t}$ is the total number of estimates of the one-year forward EPS for stock i in month t. $LnCoverage_{i,t}$ is the natural logarithm of company i's analyst coverage in month t. Our proxy for analyst bias is the difference between the mean consensus forecasted EPS and the actual EPS of company i for month t, scaled by the absolute value of actual EPS. Our proxy for forecast error is the absolute value of analyst bias. $MiFID_t$ is an indicator variable that takes the value of 1 for periods after January 2018, and 0 otherwise. $TREAT_i$ is an indicator variable that takes the value of 1 for companies from EEA countries, and 0 for US companies. Other control variables include $LnSize_{i,t-12}$, $LnBM_{i,t-12}$, $Profit_{i,t-12}$, $Return_{i,t-12}$, $Sigma_{i,t-12}$, and $VolROE_{i,t-12}$ as defined in Appendix A. Panel A presents regressions run on the sample of large companies. Panel B – on the sample of mid-sized companies. Panel C – on the sample of small companies. Data is obtained from IBES, Datastream, and Worldscope databases. All regression results are based on monthly measures of variables firm-month observations from EEA and US countries, between 2006-2019. Panel A contains 77,850 firm-month observations. Panel B contains 128,424 firm-month observations. Panel C contains 263,078 firm-month observations. Each regression uses two-way cluster-robust standard errors (by firm and month). *t*-statistics are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Appendix E. *Robustness checks.*

Panel A: OLS regressions using median EPS consensus forecast

	$Median\ Bias_{i,t}$				$Median\ FError_{i,t}$			
	(1)		(2)		(3)		(4)	
$MiFID_t$	-0.0282	**	-0.0203		-0.0195	**	-0.0443	**
	(-2.39)		(-0.88)		(-2.19)		(-2.35)	
$MCAPmid_i$			-0.0177				0.0253	
			(-1.04)				(1.60)	
$MCAPsmall_i$			-0.0676	***			0.0185	
			(-4.83)				(1.41)	
$LnSize_{i,t-12}$	0.0134	***			0.0001			
	(5.68)				(0.02)			
$LnBM_{i,t-12}$	0.0343	***	0.0321	**	0.0879	***	0.0875	***
	(2.69)		(2.54)		(7.57)		(7.70)	
$Profit_{i,t-12}$	-0.1739	**	-0.1820	**	-0.5277	***	-0.5357	***
	(-2.32)		(-2.45)		(-6.15)		(-6.29)	
$Return_{i,t-12}$	0.0002		0.0001		-0.0001		-0.0001	
	(0.66)		(0.62)		(-0.79)		(-1.07)	
$Sigma_{i,t-12}$	-0.0108		-0.0144		0.0852	*	0.0832	*
	(-0.19)		(-0.25)		(1.87)		(1.83)	
$VolROE_{i,t-12}$	-0.0215	***	-0.0247	***	0.0176	***	0.0166	***
	(-3.17)		(-3.40)		(3.05)		(2.94)	
$MiFID_t \times MCAPmid_i$			-0.0315				0.0001	
			(-1.28)				(0.01)	
$MiFID_t \times MCAPsmall_i$			0.0146				0.0449	**
			(0.65)				(2.33)	
Observations	334,479		334,479		334,479		334,479	
R^2	0.75%		0.78%		4.14%		4.22%	

Note. This table presents the results from panel regressions of coverage and forecast quality (i.e., bias and error) based on the following models:

$$Dependent\ Var_{i,t} = \alpha + \beta_1 MiFID_t + \sum_{j=1}^{6} \delta_j Control_{j,i,t-12} + \varepsilon_{i,t} \quad (2)$$

$$Dependent\ Var_{i,t} = \alpha + \beta_1 MiFID_t + \beta_2 MCAP_i + \gamma MiFID_t \times MCAP_i + \sum_{j=1}^{6} \delta_j Control_{j,i,t-12} + \varepsilon_{i,t} \quad (3)$$

where $Dependent\ Var_{i,t}$ stands for $Coverage_{i,t}, LnCoverage_{i,t}, Bias_{i,t}$, or $FError_{i,t}$. $Coverage_{i,t}$ is the total number of estimates of the one-year forward EPS for stock i in month t. $LnCoverage_{i,t}$ is the natural logarithm of company i's analyst coverage in month t. Our proxy for analyst bias is the difference between the median consensus forecasted EPS and the actual EPS of company i for month t, scaled by the absolute value of actual EPS. Our proxy for forecast error is the absolute value of analyst bias. $MiFID_t$ is an indicator variable that takes the value of 1 for periods after January 2018, and 0 otherwise. $MCAP_i$ is a three-level categorical variable that distinguished large, mid, and small companies. Other control variables include $LnSize_{i,t-12}$, $LnBM_{i,t-12}$, $Profit_{i,t-12}$, $Return_{i,t-12}$, $Sigma_{i,t-12}$, and $VolROE_{i,t-12}$ as defined in Appendix A. Data is obtained from IBES, Datastream, and Worldscope. All regression results are based on monthly measures of variables across 334,479 firm-month observations from EEA countries, between 2006-2019. Each regression uses two-way cluster-robust standard errors (by firm and month). t-statistics are reported in parentheses. ***,**, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel B: OLS regressions using mean EPS consensus forecast. Cleaner sample (excluding observations from 2016 and 2017)

	$Coverage_{i,t}$		$LnCoverage_{i,t}$		$Mean\ Bias_{i,t}$		$Mean\ FError_{i,t}$	
	(1)		(2)		(3)		(4)	
$MiFID_t$	-4.5397	***	-0.3296	***	-0.0314	**	-0.0206	**
	(-24.22)		(-24.66)		(-2.45)		(-2.09)	
$LnSize_{i,t-12}$	3.5839	***	0.2838	***	0.0132	***	0.0010	
	(61.68)		(64.88)		(5.03)		(0.39)	
$LnBM_{i,t-12}$	0.3714	***	-0.0044		0.0521	***	0.1002	***
	(2.91)		(-0.41)		(3.62)		(7.95)	
$Profit_{i,t-12}$	5.1868	***	0.3223	***	-0.1334		-0.4670	***
	(4.32)		(3.49)		(-1.62)		(-5.76)	
$Return_{i,t-12}$	-0.0107		-0.0007		0.0002		-0.0001	***
	(-1.52)		(-1.11)		(0.70)		(-0.50)	
$Sigma_{i,t-12}$	1.0105		0.0951		-0.0082		0.1192	*
	(0.95)		(1.29)		(-0.11)		(1.94)	
$VolROE_{i,t-12}$	0.6027	***	0.0382	***	-0.0147	*	0.0219	***
	(4.12)		(3.11)		(-1.70)		(3.79)	
Observations	262,365		262,365		262,365		262,365	
R^2	60.40%		59.50%		1.00%		4.40%	

Note. This table presents the results from panel regressions of coverage and forecast quality (i.e., bias and error) based on the following model:

$$Dependent\ Var_{i,t} = \alpha + \beta_1 MiFID_t + \sum_{j=1}^{6} \delta_j Control_{j,i,t-12} + \varepsilon_{i,t} \quad (2)$$

where $Dependent\ Var_{i,t}$ stands for $Coverage_{i,t}, LnCoverage_{i,t}, Bias_{i,t}$, or $FError_{i,t}$. $Coverage_{i,t}$ is the total number of estimates of the one-year forward EPS for stock i in month t. $LnCoverage_{i,t}$ is the natural logarithm of company i's analyst coverage in month t. Our proxy for analyst bias is the difference between the mean consensus forecasted EPS and the actual EPS of company i for month t, scaled by the absolute value of actual EPS. Our proxy for forecast error is the absolute value of analyst bias. $MiFID_t$ is an indicator variable that takes the value of 1 for periods after January 2018, and 0 otherwise. Other control variables include $LnSize_{i,t-12}$, $LnBM_{i,t-12}$, $Profit_{i,t-12}$, $Return_{i,t-12}$, $Sigma_{i,t-12}$, and $VolROE_{i,t-12}$ as defined in Appendix A. Data is obtained from IBES, Datastream, and Worldscope. All regression results are based on monthly measures of variables across 262,365 firm-month observations from EEA countries, between 2006-2015 and 2018-2019. Each regression uses two-way cluster-robust standard errors (by firm and month). t-statistics are reported in parentheses. ***,**, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel C: DiD regressions with median EPS consensus forecast

	$Median\ Bias_{i,t}$		$Median\ FError_{i,t}$	
	(1)		(2)	
$MiFID_t$	0.1143	***	0.1490	***
	(7.01)		(9.57)	
$TREAT_i$	-0.0196	***	0.0096	
	(-2.71)		(1.38)	
$LnSize_{i,t-12}$	0.0058	***	-0.0061	***
	(3.68)		(-3.82)	
$LnBM_{i,t-12}$	-0.0218	***	0.0376	***
	(-3.32)		(6.28)	
$Profit_{i,t-12}$	-0.3749	***	-0.5325	***
	(7.30)		(-9.72)	
$Return_{i,t-12}$	0.0002		0.0000	
	(0.76)		(0.11)	
$Sigma_{i,t-12}$	-0.0200		0.0813	**
	(-0.53)		(2.40)	
$VolROE_{i,t-12}$	-0.0270	***	0.0034	
	(-5.15)		(0.95)	
$MiFID_t \times TREAT_i$	-0.1106	***	-0.1246	***
	(-5.70)		(-7.54)	
Observations	469,352		469,352	
R^2	0.99%		3.12%	

Note. This table presents the results from panel regressions of coverage and forecast quality (i.e., bias and error), on the entire matched sample of EEA and US companies, based on the following model:

$$Dependent\ Var_{i,t} = \alpha + \beta_1 MiFID_t + \beta_2 TREAT_i + \beta_3 MiFID_t \times TREAT_i + + \gamma Controls_{i,t-12} + \varepsilon_{i,t}\ , \quad (7)$$

where $Dependent\ Var_{i,t}$ stands for $Coverage_{i,t}, LnCoverage_{i,t}, Bias_{i,t}$, or $FError_{i,t}$. $Coverage_{i,t}$ is the total number of estimates of the one-year forward EPS for stock i in month t. $LnCoverage_{i,t}$ is the natural logarithm of company i's analyst coverage in month t. Our proxy for analyst bias is the difference between the median consensus forecasted EPS and the actual EPS of company i for month t, scaled by the absolute value of actual EPS. Our proxy for forecast error is the absolute value of analyst bias. $MiFID_t$ is an indicator variable that takes the value of 1 for periods after January 2018, and 0 otherwise. $TREAT_i$ is an indicator variable that takes the value of 1 for companies from EEA countries, and 0 for US companies. Other control variables include $LnSize_{i,t-12}$, $LnBM_{i,t-12}$, $Profit_{i,t-12}$, $Return_{i,t-12}$, $Sigma_{i,t-12}$, and $VolROE_{i,t-12}$ as defined in Appendix A. Data is obtained from IBES, Datastream, and Worldscope databases. All regression results are based on monthly measures of variables across 469,352 firm-month observations from EEA and US countries, between 2006-2019. Each regression uses two-way cluster-robust standard errors (by firm and month). t-statistics are reported in parentheses. ***,**, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel D: DiD regressions using mean EPS consensus forecast. Cleaner sample (excluding observations from 2016 and 2017)

	$Coverage_{i,t}$		$LnCoverage_{i,t}$		$Mean\ Bias_{i,t}$		$Mean\ FError_{i,t}$	
	(1)		(2)		(3)		(4)	
$MiFID_t$	-2.4624	***	-0.2305	***	0.1166	***	0.1514	***
	(-14.29)		(-16.52)		(6.92)		(9.37)	
$TREAT_i$	2.7009	***	0.1808	***	-0.0138	*	0.0117	
	(15.12)		(12.11)		(-1.73)		(1.54)	
$LnSize_{i,t-12}$	3.4763	***	0.2869	***	0.0068	***	-0.0048	***
	(73.10)		(78.10)		(3.94)		(-2.84)	
$LnBM_{i,t-12}$	0.0596		-0.0181	**	-0.0164	**	0.0404	***
	(0.58)		(-2.05)		(-2.25)		(6.27)	
$Profit_{i,t-12}$	5.4014	***	0.4057	***	-0.3694	***	-0.5330	***
	(5.76)		(5.26)		(-6.92)		(-9.82)	
$Return_{i,t-12}$	-0.0090		-0.0006		0.0003		0.0001	
	(-1.51)		(-1.11)		(0.83)		(0.30)	
$Sigma_{i,t-12}$	-0.3542		0.0268		-0.0238		0.1330	**
	(-0.54)		(0.40)		(-0.42)		(2.48)	
$VolROE_{i,t-12}$	0.3016	***	0.0144	*	-0.0258	***	0.0030	
	(3.15)		(1.75)		(-4.13)		(0.91)	
$MiFID_t \times TREAT_i$	-1.7259	***	-0.0882	***	-0.1155	***	-0.1273	***
	(-9.04)		(-5.66)		(-5.77)		(-7.41)	
Observations	373,896		373,896		373,896		373,896	
R^2	59.55%		58.79%		1.07%		3.24%	

Note. This table presents the results from panel regressions of coverage and forecast quality (i.e., bias and error), on the entire matched sample of EEA and US companies, based on the following model:

$$Dependent\ Var_{i,t} = \alpha + \beta_1 MiFID_t + \beta_2 TREAT_i + \beta_3 MiFID_t \times TREAT_i + + \gamma Controls_{i,t-12} + \varepsilon_{i,t}, \quad (7)$$

where $Dependent\ Var_{i,t}$ stands for $Coverage_{i,t}, LnCoverage_{i,t}, Bias_{i,t}$, or $FError_{i,t}$. $Coverage_{i,t}$ is the total number of estimates of the one-year forward EPS for stock i in month t. $LnCoverage_{i,t}$ is the natural logarithm of company i's analyst coverage in month t. Our proxy for analyst bias is the difference between the mean consensus forecasted EPS and the actual EPS of company i for month t, scaled by the absolute value of actual EPS. Our proxy for forecast error is the absolute value of analyst bias. $MiFID_t$ is an indicator variable that takes the value of 1 for periods after January 2018, and 0 otherwise. $TREAT_i$ is an indicator variable that takes the value of 1 for companies from EEA countries, and 0 for US companies. Other control variables include $LnSize_{i,t-12}$, $LnBM_{i,t-12}$, $Profit_{i,t-12}$, $Return_{i,t-12}$, $Sigma_{i,t-12}$, and $VolROE_{i,t-12}$ as defined in Appendix A. Data is obtained from IBES, Datastream, and Worldscope databases. All regression results are based on monthly measures of variables across 373,896 firm-month observations from EEA and US countries, between 2006-2015 and 2018-2019. Each regression uses two-way cluster-robust standard errors (by firm and month). *t*-statistics are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.