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Investment and access to external finance in Europe: Does analyst coverage matter?*

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Abstract

We aim to determine whether analyst coverage improves European firms' access to capital markets and investment. Based on a data set that includes firms from several European countries between 2000 and 2015, we implement a treatment effect framework and an instrumental variables (IV) approach, in which the intensity of industry-level waves in coverage is used as an instrument for firm-level coverage. We show that analyst coverage is favorable to firms' debt and share issuance and their investment expenses. Our paper emphasizes the key role of financial analysts in improving European firms' financial conditions.

Keywords: investment, debt issuance, share issuance, analyst, coverage

JEL Classification : G23, G31, G32

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Abstract

We aim to determine whether analyst coverage improves European firms' access to capital markets and investment. Based on a data set that includes firms from several European countries between 2000 and 2015, we implement a treatment effect framework and an instrumental variables (IV) approach, in which the intensity of industry-level waves in coverage is used as an instrument for firm-level coverage. We show that analyst coverage is favorable to firms' debt and share issuance and their investment expenses. Our paper emphasizes the key role of financial analysts in improving European firms' financial conditions.

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1 Introduction

Analysts are key information intermediaries in capital markets and their effects for investors and the healthy functioning of financial markets have been well documented. However, less is known about whether they have effects on the real economy. Analysts may have real effects by impacting corporate policies and, eventually, investment expenses. This relationship can best be explained as follows: the production of public information (earning forecasts, investment recommendations, etc.) by financial analysts is expected to reduce information asymmetry for companies and thereby their capital costs (Kelly and Ljungqvist, 2012, Ellul and Panayides, 2018), which does increase ceteris paribus the investment by the issuing of new debts or equity. Several studies have examined this causal relationship between financial analyst and corporate policies. This literature has concluded that analysts matter. Doukas et al. (2008) found that firms with high analyst coverage tend to invest more and engage in more external financing, and Derrien and Kecskés (2013) found that firms that lose an analyst decrease their investments compared to similar firms with unchanged coverage.[1] Further, to finance their investment, firms covered by fewer analysts issue equity less frequently (Chang et al., 2006), issue underpriced equity (Bowen et al., 2008), or raise debt at a higher interest rate (Derrien et al., 2016).

Despite this growing evidence, some challenges remain to be addressed. First, it is difficult to consolidate the findings because the literature has largely been restricted to U.S. data. This restriction could be a problem because results that are valid in the U.S. might not apply in other areas. Actually, the U.S. is not a representative case regarding the functioning of financial markets. It is largely admitted that the U.S. financial system is specific:

[1] Analyst coverage refers to the number of sell-side financial analysts that follow a public company, i.e., that publish regular information about this company.

the financial markets are broader and more liquid, and the information availability on firms is greater (Allen and Gale, 2001, Allen et al., 2004), which could impact the value of information provided by financial analysts. Therefore, a critically important question is to ask whether the evidence has relevance beyond the U.S. and, in particular, in countries where financial markets are less developed and financial systems are more bank based. Second, the estimation of the causal effect of analyst coverage on investment faces empirical difficulties. These difficulties are explained by the fact that firms that are covered by analysts could be different in terms of their potential outcomes (investment rate, for instance) from those remaining uncovered, referring to an issue of endogenous selection by financial analysts. To address this issue, the literature has proposed an instrumental variable strategy (Doukas et al., 2008), as well as quasi-natural experiments (Derrien and Kecskés, 2013, Chen et al., 2015, Yu, 2008, To et al., 2018, Kim et al., 2019, Li, 2020). Although the first approach is attractive, the validity of the proposed instruments is questionable. Actually, they do not really provide exogenous perturbations of analyst coverage. In response, another strand of the literature uses "quasi-natural experiments" to assess their results, but this approach also comes with other flaws. The initial idea is that broker mergers (Hong and Kacperczyk, 2010) or closures (Kelly and Ljungqvist, 2012) entail that some firms will lose coverage without this loss being related to the firm's corporate policy or characteristics. However, this approach does not allow us to analyze increases in or initiation of analyst coverage. Moreover, these events do not totally occur randomly since they are dependent on macroeconomic and financial factors also impacting firms characteristics. Finally, broker mergers or closure are very rare events outside the U.S., rendering this methodology difficult to implement in other areas.

Against this background, the present paper suggests a new evaluation of the causal impact of financial analysts on investment policy for a sample of firms outside the U.S. over the 2000-2015 period. More precisely, we consider a sample of firms from six Euro area capital markets (Belgium, France, Germany, the Netherlands, Italy, Spain), which represents more than 98% of the Euro area stock market capitalization in our period of study. Since there is no perfect empirical strategy for estimating the causal effects of analysts on firms' investment, we propose two different strategies that impose different assumptions. Reassuringly, the two approaches lead to similar conclusions.

We first investigate the effects on corporate policies (investment and funding policies) of the initiation of analyst coverage by comparing the average outcomes in term of corporate policies for firms affected by a coverage treatment (treatment group) with the average outcomes of unaffected firms (control group). Because the selection of firms to initiate coverage is not random and depends on firm characteristics (selection bias), we employ two different estimators to model the selection process and infer the average treatment effect on the treated (ATET): the inverse probability weighting (IPW) and the doubly robust estimator.

We then use an instrumental variables (IV) approach that complements our first strategy by providing a general assessment of the impact of analyst coverage on investment and not only of one particular coverage event (the initiation) and by addressing the possibility that the coverage and the corporate policies might be affected by time-varying omitted variables. In the spirit of Acemoglu et al. (2019), our IV strategy exploits sector-level idiosyncrasies and considers sectoral waves of analyst coverage as an instrument for the number of analysts following a firm. Our assumption is that the intensity of coverage in sector s affects the coverage of firms i belonging to sector s, without directly influencing their corporate policies. We justify the first part of our assumption (relevance condition) because analysts tend to herd in their recommendations (Hong et al., 2000, Clement and Tse, 2005, Jegadeesh and Kim, 2010, Frijns et al., 2018), and stock markets face sector rotation and sector hypes fueled by herding behaviors (Choi and Sias, 2009, Chen et al., 2012, Gavriillidis et al., 2013). Regarding the second part of our assumption (exclusion restriction), it is noteworthy that our sector-level instrument is orthogonal to firm-specific development. Further, the exclusion restriction is strengthened by the inclusion of several firm- and sector-level variables and by the specific way in which we built our instrument.

Our evidence suggests that analyst coverage matters for investment and funding and therefore has real effects. In particular, considering the initiation of analyst coverage for firms as a treatment, we find that the latter has positive and significant effects on investment, share issuance and debt issuance. Furthermore, we show that this positive effect lasts at least two years and is economically sizeable. The potential outcome of the treatment is estimated as approximately 2 percentage points in term of investment rate. Regarding our IV approach, our findings indicate that an exogenous increase of one analyst covering a firm causes an increase of 0.650 percentage points in the investment rate (in our baseline specification). This result is robust to a variety of robustness checks. We check in particular the reliability of our instrument (and the exclusion restriction) in multiple ways. Interestingly, when we compare IV and OLS results, we find that the OLS results are subject to an attenuation bias, justifying the decision to rely on external sources of variation in coverage to infer causal effects from coverage to firms' investment expenses. Finally, our IV results also emphasize the positive effects of coverage on share issuance and debt issuance.

The rest of the paper is organized as follows. The next section presents the literature review. Section 3 presents our data and explains our treatment effects framework, as well as our IV approach. Section 4 presents the results and several robustness checks. Section 5 concludes the study.

2 Literature

In this section, we present the framework of our study. We first present the literature addressing the effect of analyst coverage on firms' financial conditions. We then turn to the endogeneity issue.

2.1 The effect of analyst coverage on firms' financial conditions

Our paper relates to a first strand of literature, which focuses on the effect of coverage on firms' financial conditions.

Because they produce public information about firms, financial analysts alleviate information asymmetry between insiders and outsiders and play the role of informed agents regarding financial markets. As a consequence, information spreads more slowly across financial markets for weakly covered firms (Lin et al., 2014). Being followed by an analyst also reduces a firms stock overevaluation regarding financial markets (Li, 2020) and improves investors' confidence, thus reducing the probability of extreme events and future stock price crashes (Kim et al., 2019). Moreover, Mola et al. (2012) revealed that firms that lost coverage are more likely to lose investor recognition and delist from a public stock exchange. For the same reason, a drop in coverage or coverage termination reduces market participation and liquidity (Brennan and Subrahmanyam, 2006, Irvine, 2003, Roulstone, 2003, Kelly and Ljungqvist, 2012, Mola et al., 2012, Ellul and Panayides, 2018),[2] especially

[2]This liquidity improvement effect is also observed when a former analyst is hired by the firm as an "investor relations officers' (Hope et al., 2021).

when information asymmetry is strong, i.e., for firms with large insider holdings, no external funding and no issue earnings guidance (Ellul and Panayides, 2018).[3] Interestingly, Irvine (2003) found that the impact of coverage initiation on liquidity is larger than that of coverage continuation, suggesting that initial recommendations convey more information than recommendations issued by analysts who already follow firms.

Second, analysts also provide monitoring and reduce conflicts of interest and agency problems (Jensen and Meckling, 1976). Chen et al. (2015) and (Yu, 2008) showed that, when fewer analysts follow a firm, its management is more likely to invest in value-destroying projects and strongly engage in earnings management strategies. Symmetrically, analysts provide managers an incentive to make decisions that enhance firm value, thus increasing firms' market value (Chung and Jo, 1996) and total factor productivity (To et al., 2018). Moreover, in line with pecking order theory (Myers and Majluf, 1984), Chang et al. (2006) reported that, when a firm experiences a reduction in coverage, it is less likely to issue equity as opposed to debt.

Finally, by mitigating information asymmetry between firms and outside investors, analysts decrease capital costs and increase firms' access to external financing. Focusing on the cost of raising equity capital, Bowen et al. (2008) revealed that coverage lowered Seasoned Equity Offerings (SEO) underpricing. Considering the cost of borrowing, Derrien et al. (2016) found that the loss of an analyst causes the cost of debt to increase by 25 basis points and the rate of default to rise from 100% to 150%. Finally, firms with larger coverage raise more external funds and have more investment expenses (Doukas et al., 2008).

[3] Jiang et al. (2011) also established that firms covered by a large number of analysts exhibit wider spreads, lower market quality indices and larger price impacts of trades because analysts attract uninformed trading on stocks that they follow.

Similarly, Derrien and Kecskés (2013) demonstrated that a reduction in coverage reduce firms' capital expenditures.

2.2 The endogeneity issue

Another strand of the literature has focused on the determinants of analyst coverage. Chung and Jo (1996), Das et al. (2006), Lee and So (2017) reported that analysts are more prone to follow large and highly profitable firms.[4] Analyst coverage also depends on the firm information environment: it is larger when the firm provides a management forecast in the IPO prospectus (Chatalova et al., 2016), when past forecast errors or revisions of its EPS are small (Giraldo, 2011) or when it is listed on the main board rather than on the junior board (Hassan and Skinner , 2016). Because firms with good prospects and high information transparency are also more likely to have favorable financial conditions, these results might induce potential reverse causality and bias the estimations concerning the impact of coverage on firms' access to finance and investment expenses. Hence, one important challenge in empirically investigating the effect of analyst following on firm financial conditions is to control for the potential endogeneity of coverage.

One possible approach consists of resorting to two-stage least squares (2SLS) or three-stage least squares (3SLS) to estimate models in which coverage is defined as a function of firm characteristics (Brennan and Subrahmanyam, 2006, Chung and Jo, 1996, Roulstone, 2003, Yu, 2008, Doukas et al., 2008, To et al., 2018, Li, 2020). However, the variables that are correlated with the number of analysts covering a firm are likely to also influence firms'

[4]Note that, in Europe, this effect has been amplified by the implementation of the unbundling rules in MiFID II, which has compelled brokers to clearly divide the fees for brokerage and financial research services (Anselmi et al., 2021, Fang et al., 2020).

corporate policies. Finding instruments that are exogenous to specific firm characteristics (exclusion restriction) and highly correlated with the variable that is instrumented, i.e., analyst coverage (relevance condition), at the firm level is very challenging.

Following Hong and Kacperczyk (2010) and Kelly and Ljungqvist (2012), another empirical strategy consists of considering that external sources of variation in coverage provide quasi-natural experiments, allowing for the inference of causal effects from coverage to firms' investment expenses. As discussed in the literature (Derrien and Kecskés, 2013, Chen et al., 2015, Yu, 2008, To et al., 2018, Kim et al., 2019, Li, 2020), quasi-experiments can be found in broker closures and mergers, which can induce analyst termination and loss of coverage for some firms. This approach is attractive but not without flaws. First, it is exclusively based on discrete external sources of loss of analyst coverage, which does not allow for the effect of an increase in coverage to be assessed. Second, because they are partially driven by common patterns in the financial industry, merger and closure events are correlated over time. Third, as emphasized by Wu and Zang (2009), the analysts who survive to the elimination of redundant workforce members after a merger are those who have the greatest forecast accuracy and the largest stock following. Combined with the idea that coverage itself depends on firm peculiarities, this argument suggests that analyst termination following a merger is not totally exogenous to firm characteristics, such as investment expenses. Finally, considering broker closures and mergers is not easily replicable for samples different from the U.S. market. Indeed, while the U.S. financial industry has experienced a strong consolidation movement in recent decades, the number of mergers and acquisitions in the European financial industry remain quite small (European Banking Authority, 2018).

In the rest of the paper, we thus propose an alternative empirical approach to determine the impact of analyst following on European firms' access to capital markets and investment.

3 Data and Methodology

3.1 Data

The data that we use in this paper come from FactSet. We extract data for listed firms in Belgium, France, Germany, the Netherlands, Italy and Spain from 2000 to 2015. As mentioned above, it is noteworthy that, in contrast to most of the literature on analyst coverage, which focuses on U.S. data, we use data from Continental European countries – members of the Euro area. In 2000 (resp. in 2015), Belgium, France, Germany, the Netherlands, Italy and Spain constituted more than 98% (resp. nearly 100%) of the Euro area stock market capitalization and 81% (resp. 95%) of the number of listed companies (source: World Bank). Our data set is thus highly representative of European financial markets while reflecting their diversity in terms of external finance structure, corporate governance and legal systems (La Porta et al., 1997, 1998). Hence, the countries included in our data appear particularly relevant to exploring the impact of analyst coverage in non-Anglo-Saxon financial systems.

Table 1: List of dependent and explanatory variables

DEPENDENT VARIABLES	
$Inv_{i,t}$	Percentage of net cash flow from investing of firm i on date t over the total assets of firm i on date $t-1$.
$ShareIssuance_{i,t}$	Percentage of net new share issues of firm i on date t over the total assets of firm i on date $t-1$.
$DebtIssuance_{i,t}$	Percentage of net new debt issues of firm i on date t over the total assets of firm i on date $t-1$.
EXPLANATORY VARIABLES	
$Coverage_{i,t}$	Number of analysts following firm i on date t.
$CF_{i,t}$	Ratio of net income and amortization expenses of firm i on date t over total assets of firm i on date $t-1$.
$Q_{i,t}$	Logarithm of (market value + total assets - book value) over total assets, of firm i on date t.
$Size_{i,t}$	Logarithm of total assets in millions of euros, of firm i on date t.
$ROE_{i,t}$	Ratio of net return on equity, of firm i on date t.
$Debt_{i,t}$	Ratio of short-term and long-term debt over total assets, of firm i on date t.
$Growth_{i,t}$	Growth rate of Sales of firm i, Logarithm of net sales on date t minus logarithm of net sales on date $t-1$.
INSTRUMENTS FOR COVERAGE	
$Z_{i,t}$	Average number of analysts following other firms than i in i's sector and within the same class (initially covered/not covered) on date t.

Our initial sample comprises all of the firms in Factset from 2001 to 2015 listed in Belgium, France, Germany, the Netherlands, Italy and Spain. Following Baker et al. (2003) and McLean et al. (2012), we exclude firms that have no positive book value for at least one year. Further, we study non-financial agents and thus exclude financial firms (code 52 from the North American Industry Classification System (NAICS) classification). As a result, we have $1,741$ firms for which we have simultaneously information about analyst coverage and corporate policies. We allow new firms to enter our sample at any time of our study period, meaning that our panel is not balanced. As can be seen on Figure A1 in the appendix, the number of observations by year grows over time.[5] Although our sample contains IPO, the way in which we define our variables will lead us not to examine the effects of analyst coverage on corporate policies in the year of the IPO. Indeed, we assess the impact of analyst coverage for listed firms from at least one year, which ensures more reliable estimates since the year of the IPO is very singular.[6] Further, to reduce the effects of outliers, we winsorize each of the accounting variables at the top and bottom 1%, with the exception of the variable $Size$. All of our variables are described in Table 1, while summary statistics are provided in Table A2 in the appendix. Here, we simply define our main dependent variables, as well as our main variable of interest. We are interested in the effects of analysts on investment policies and financing policies. We use the investment ($Inv_{i,t}$), as well as the share issuance (*Share issuance*$_{i,t}$) and debt issuance (*Debt issuance*$_{i,t}$), to approach these policies. We use the standard definition of these variables. $Inv_{i,t}$ is the percentage of cash flow from the investing activities of firm i on date t over the total assets of firm i on date $t-1$, while $Share\ Issuance_{i,t}$ and $Debt\ Issuance_{i,t}$ correspond to the percentage of new share issues or new debt issues of firm i on date t over the total assets

[5] In Table A1 in the appendix, we also provide information regarding the composition by country of our sample.

[6] For example, IPO might simultaneously allow newly listed firms to invest and encourage analysts to follow these firms, thus biasing our results.

of firm i on date $t-1$. Finally, we measure analyst coverage ($Coverage_{i,t}$) by computing the number of analysts issuing recommendations and forecasts for a firm i on date t.

To gain initial insight into the relationship between analyst coverage and corporate policies, we build a "clean" measure of the firm' investment rate, share issuance and debt issuance and simply plot them against the intensity of coverage, that is, the number of analysts following the firm. Our "clean" measure is obtained by regressing the reported investment rate, share issuance and debt issuance on individual fixed effects, as well as year fixed effects. For ease of interpretation, we group our observations into 20 equal-sized bins of the distribution of the coverage intensity.

Figure 1: Investing and financing policies and analyst coverage

(a) Investment (b) Share issuance (c) Debt issuance

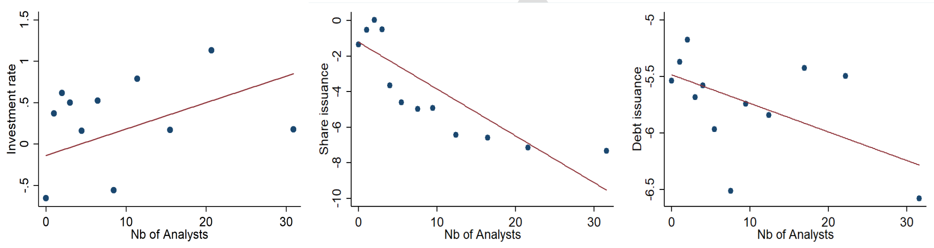

Figure 1 clearly depicts both a positive relationship between analyst coverage and investment and a negative relationship with share and debt issuance. These associations are definitely counter-intuitive and illustrate why simple descriptive statistics are not sufficient to analyze coverage. The next section answers this challenge.

3.2 Methodology

3.2.1 Treatment effect estimates

Our treatment effect framework aims to explore the link between analyst coverage and investment by examining the effects of the transition from not being covered to being covered, meaning that we focus on one particular evolution of analyst coverage: the initiation. In line with Irvine (2003), the analyst coverage effect on investment is likely to be particularly high at this point. In practice, we define a transitioning binary variable, $C_{i,t}$, which equals 1 if the number of analysts following firm i at time t is greater than or equal to one, while this number is equal to zero at $t-1$ and 0 otherwise.[7] Hence, $C_{i,t}$ can be viewed as a "treatment". The challenge is to address the nonrandomization of the treatment and that the firms selected by sell-side analysts might differ across their characteristics. In this section, we assume that the selection process can be modeled as a function of observable variables.[8] In what follows, we define the standard tools that we use to model the selection process and assess the effect of our "treatment" on investment.[9]

We start by denoting by $Inv_{i,t}(C_{i,t})$ the investment rate $Inv_{i,t}$ for a firm i that experiences an initiation of coverage $C_{i,t} \in \{0, 1\}$ at year t. Therefore, the average treatment effect on the treated, denoted by $ATET$, is measured as the difference between the average level of $Inv_{i,t}$ for treated firms and the average level of $Inv_{i,t}$ for treated firms if they were

[7] There are 1,532 observations of firms experiencing such a coverage treatment in our sample. The number of coverage (re)initiations by year is represented in Figure A2.

[8] We relax this assumption in the next subsection.

[9] To explore the channels by which coverage initiation affects firm investment, we also analyze the effects on financing source indicators: $ShareIssuance$ and $DebtIssuance$ (See Table 1 in the previous section for the definition of these variables).

not assigned the treatment. This equation can be written as follows:

$$ATET = E[Inv_{i,t}(1) - Inv_{i,t}(0)|C_{i,t} = 1] \quad (1)$$

As noted above, treatment is not independent of $Inv_{i,t}$. We thus make the (strong) assumption that the selection process can be modeled as a function of observables. This assumption can be expressed as follows:

$$ATET = E[Inv_{i,t}(1) - Inv_{i,t}(0)|C_{i,t} = 1, X_{i,t-1}] \quad (2)$$

where $X_{i,t-1}$ is a set of firm-level observable characteristics at $t-1$. Equation (2) provides the causal treatment effect on the treated if and only if accounting for observables provides the unconditional means of $Inv_{i,t}(1)$ and $Inv_{i,t}(0)$; i.e., any unobserved heterogeneity affects the means of $Inv_{i,t}(1)$ and $Inv_{i,t}(0)$.

To model the selection process, i.e., to obtain unconditional means, we rely on two different approaches. The first, known as inverse probability weighting (IPW), focuses on the treatment assignment. This approach explicitly models the probability $P_{i,t}$ of transitioning into the group of covered firms. To determine the propensity score of each firm, we fit a probit model of $C_{i,t}$ as a function of the lagged values of the outcome, Q, $Size$, the square term of $Size$, and CF (see Table 1 in the appendix for the definition of these variables), as well as a set of sector fixed effects. Our covariates are the same as in Derrien and Kecskés (2013), with the exception of the lagged outcome (which is included to eliminate the pre-trend in the data) and the square term of firm size (to consider the non-linear effect of the size on the selection process). Then, to estimate the value of $ATET$, we regress the outcome of interest (Inv) on the transitioning variable $C_{i,t}$, with each observation of Inv weighted by the inverse of the probability to be covered given the value of observable characteristics. The underlying idea of IPW is to give greater weight to the observations of uncovered firms exhibiting similar characteristics to transitioning firms. The average treatment effect can be expressed as follows:

$$ATET = E(Inv_{i,t} \times w_{i,t}) \tag{3}$$

where the inverse probability weight is given by:

$$w_{i,t} = f(\frac{1}{P(C_{i,t} = 1|X_{i,t-1})}) \tag{4}$$

where f corresponds to the efficient weighting scheme of Hirano et al. (2003) used by the Stata command "teffects", and $X_{i,t-1}$ corresponds to the covariates entered into the probit model.

The second estimator that we use is the doubly robust estimator. This estimator extends the IPW estimator by addressing not only the probability of treatment conditional on covariates ("the propensity score") but also the outcome expectation conditional on treatment and covariates ("the outcome regression"). The covariates that we use to determine the outcome expectation correspond to several variables that can be entered into an investment equation. These variables are CF, and the lagged value of Q, $Size$, $Debt_{i,t}$, ROE and $Growth$ and are grouped in the vector X'. The definition of the variables are provided in Table 1 in the previous section. Formally, we estimate $ATET$ as:

$$ATET = E((Inv_{i,t} - \hat{\beta}X'_{i,t-1}) \times w_{i,t}) \tag{5}$$

where $\hat{\beta}$ denotes a vector of estimates provided by the outcome regression. As can be seen, the only difference from Equation (3) is the expected outcome $\hat{\beta}X'_{i,t-1}$, which is orthogonal to the covariates X'.

The causal interpretation of our estimates rests on different assumptions. The first (unverifiable) assumption is that there are no unobserved variables correlated with outcomes and with the probability of receiving a treatment (conditional independence assumption). The second assumption is that the difference between the treated and control groups would remain constant in the absence of an intervention (parallel trend assumption). Finally, we make a timing assumption regarding the treatment. In particular, we assume that a change in coverage between the end of year $t-1$ and the end of year t can have effects on the investment rate observed at the end of year t.[10]

3.2.2 IV strategy

Our previous empirical strategy examines the treatment effect of analyst coverage initiation. The key assumption was that the selection process can be modeled as depending on a set of observables (Chung and Jo, 1996, Das et al., 2006, Lee and So, 2017). One important concern regarding this strategy is the omission of variables simultaneously correlated with

[10]This assumption is questionable, meaning that there would be only few months (six months on average if the treatment is randomly distributed over the year) between coverage initiation and changes in investment with our assumption. To convince ourselves of the plausibility of our assumption, we used information on coverage observed on a quarterly basis. This information is very useful since we can examine the effect of initiation of coverage in a specific quarter and perform a falsification test. Reasonably, we can consider that an early treatment provided during the first quarter will have an effect during the year. In contrast, we expect no difference in the investment rate in t due to a treatment given in the last quarter of the year. The results that we obtain, which are available upon request, support both expectations.

Coverage and *Inv*. To address this endogeneity bias, we propose an IV strategy. Such an identification strategy relies on external instruments, i.e., variables that are correlated with the number of analysts covering a firm (relevance condition), without directly influencing a firm's corporate policies (exclusion restriction). Finding such variables at the firm level is very challenging. In this paper, we exploit sector idiosyncrasies and use the intensity of sector-level waves in analyst coverage as an instrument for firm-level coverage. This variable satisfies both conditions defined above.

Concerning the relevance condition, the literature emphasizes the key role of sector classifications in firm following. Indeed, it is typical for analysts to specialize according to sectors and to exploit industry-level information when they issue recommendations or forecasts on firms (O'Brien, 1990, Kini et al., 2009). This idea is corroborated by Choi and Sias (2009): "*Analysts, for example, are usually assigned on an industry basis. Institutional Investors (the magazine) annual "All-America Research Team" analyst rankings, for instance, are by industry, e.g., Aerospace and Defense, Autos and Auto Parts, etc.*" (p. 470). Furthermore, it is well established that financial analysts tend to herd in their recommendations (Hong et al., 2000, Clement and Tse, 2005, Jegadeesh and Kim, 2010, Frijns et al., 2018) and that stock markets face sector rotation and sector hype fueled by herding behaviors (Choi and Sias, 2009, Chen et al., 2012, Gavriillidis et al., 2013). One typical example is the internet bubble during the latter half of the nineties; biotechs could provide new evidence of investors' herding. Moreover, the literature has also emphasized the existence of an industry-level price momentum phenomenon induced by the returns of highly covered firms leading the returns of less-followed firms within the same industry (Boni and Womack, 2006, Hou, 2007). Finally, for all of these reasons, the magnitude of industry-level coverage waves appears as a key driver of firm-level coverage.

Regarding the exclusion restriction, it is noteworthy that industry coverage waves are not correlated with specific firm characteristics. First, from a technical point of view, because they are defined at a more aggregated level, industry coverage patterns can be considered exogenous from the perspective of individual firms.[11] Second, as shown by Kadan et al. (2012), industry-level knowledge and firm-level expertise are two orthogonal dimensions of financial information that independently contribute to analyst recommendations. Considered together, these arguments strongly support the view that the intensity of industry coverage waves can be considered a suitable instrument for firm-level analyst following. In practice, we exploit this idea more subtly by considering the *initial* coverage pattern within each industry. This approach aims at strengthening the exclusion restriction assumption. Indeed, the key assumption underlying our empirical strategy is that, conditional on multiple firm-level control variables, firm fixed effects and year fixed effects, sector-level coverage patterns (i.e., our instrument) do not affect firm investment decisions. Obviously, our exclusion restriction assumption is threatened if the sector coverage patterns actually capture the effects of sector-year shocks leading to changes in sector-correlated firm investment. To ensure the reliability of our assumption, the standard approach would consist of adding observed sector-year shocks to our specification. By doing so, we would partially address the issue, and the exclusion restriction would be strengthened. However many shocks are actually unobserved. For example, this lack of observations is the case with sectoral innovations, which could attract analysts to follow the sector and simultaneously encourage firms to invest, thus violating the exclusion restriction. For this reason, it is very important to include sector-year effects from our estimates. The problem that we face is that we cannot

[11]This idea is exploited in several other research areas (see, notably, Manova et al. (2015) for an investigation on the link between finance and trade or Acemoglu et al. (2019) for a study of growth and democracy).

introduce such fixed effects with an instrument characterized by sector-year variations. To address this issue, we follow the idea of Acemoglu et al. (2019) and desegregate the sectoral wave of coverage by computing two waves: a sectoral wave built with all of the firms initially covered in our sample by at least one analyst; and a sectoral wave built with all of the firms initially uncovered. With this approach, the coverage of a firm should be correlated with the average coverage of firms that belong to the same industry and that face the same initial coverage situation.

We thus define two groups based on the observation of their coverage status (covered and uncovered firms) at the start of the sample. Formally, for firm i at time t, our instrument $Z_{i,t}$ is defined as the jackknifed average[12] of the coverage calculated on the set of firms belonging to the same industry s ($s_j = s_i$) and the same initial class (covered or uncovered) g ($g_j = g_i$) as firm i, i.e., the set of firms $I = [j : j \neq i, s_j = s_i, g_j = g_i]$.

$$Z_{i,t} = \frac{1}{N_I} \sum_{j \in I} Coverage_{j,t} \tag{6}$$

where N_I is the number of firms belonging to I, and $Coverage_{j,t}$ is the number of analysts following firm j in period t.

Finally, the 2SLS model that we estimate can be written as follows:

$$Coverage_{i,t} = \beta_1 Z_{i,t} + \sum_{n=2}^{N} \beta_n X_{i,t} + \alpha_i + \delta_t + \varepsilon_{i,t} \tag{7}$$

where α_i, δ_t and $\varepsilon_{i,t}$ denote firm fixed effects, year fixed effect and the error term, respec-

[12]We thus omit firm is coverage observations from the calculations to ensure that the industry-level average is exogenous to firm is characteristics, which is also a way to strengthen the exclusion restriction.

tively, and $X_{i,t}$ is a set of six firm-level control variables ($CF_{i,t}$, $Q_{i,t-1}$, $Size_{i,t-1}$, $ROE_{i,t-1}$, $Debt_{i,t-1}$ and $Growth_{i,t-1}$).

$$Inv_{i,t} = \gamma_1 \hat{Coverage}_{i,t} + \sum_{n=2}^{N} \gamma_n X_{i,t} + \mu_i + \nu_t + \upsilon_{i,t} \tag{8}$$

where $\hat{Coverage}_{i,t}$, μ_i, ν_t and $\upsilon_{i,t}$ denote the predicted value of coverage from Equation (7), firm fixed effects, year fixed effect and the error term, respectively.

In line with the arguments provided above, an increase in $Z_{i,t}$ should induce a rise in $Coverage_{i,t}$. The expected sign of β_1 is thus positive. Moreover, according to the literature mentioned in Section 2, the expected sign of γ_1 is positive. Concerning the sign of γ_n, we make the following predictions. First, since the seminal paper of Fazzari et al. (1988), regressing physical investment expenses on liquidity, measured by cash flow ($CF_{i,t}$), and investment opportunities, measured by Tobin's Q ($Q_{i,t-1}$), has become the standard approach to account for the existence of the financial constraints faced by a firm. In the presence of information asymmetry between firms and external investors, the cost of internal funds is lower than that of external funds, thus making the level of liquidity a key determinant of investment: the higher that it is, the greater that the investment expenses are. Hence, the expected sign of the coefficient for $CF_{i,t}$ is positive. Similarly, because investment should increase with investment opportunities, the expected sign of the coefficient for $Q_{i,t-1}$ is also positive. In addition, because technology decreases returns to scale, large firms could invest less than small firms (Gala and Julio, 2016, Gebauer et al., 2018). The expected sign of the coefficient for firm size ($Size_{i,t-1}$) is negative. We also include $ROE_{i,t-1}$ as a control variable. $ROE_{i,t-1}$ measures the amount of earnings that a company can generate from shareholders' equity. Firms with high returns on equity do not require substantial investments in capital expenditures to be highly profitable. For this reason, the coefficient for $ROE_{i,t-1}$ is expected to be negative. Moreover, firms with a high debt ratio ($Debt_{i,t-1}$) are more likely to be perceived by investors as risky. This perception could reduce their access to external finance and their ability to invest. Finally, we expect that investment increases with the growth in sales, as measured by $Growth_{i,t-1}$. See the appendix for the definitions (Table 1) and the descriptive statistics (Table A2) of all of the variables.[13]

To summarize our methodology section, we posit two research hypotheses.

H1: We hypothesis that the treatment effect on the treated (ATET) of the initiation of analyst coverage on investment is significant and positive.

H2: We hypothesis that idiosyncratic changes in analyst coverage caused by sectoral waves of analyst coverage positively impact the investment rate.

4 Results

4.1 ATET results

Tables 2 and 3 present the results obtained using the IPW estimator and the doubly robust estimator, respectively, for our three variables of interest: Inv, $ShareIssuance$ and $DebtIssuance$.[14]

[13]Note that, $Size$ being the log of total assets in million euros, it is negative when the total asset is less than 1 million euros. $ShareIssuance$ can be negative in the case of share repurchases, and $DebtIssuance$ can be negative in the cases of debt repayments, debt restructuring, and/or the inability to borrow new debt.

[14]Before presenting the estimates of our treatment effects, we must determine whether our matching procedure has successfully balanced the covariates. In this regard, we compute the standardized difference between treatment and control means, the ratio of treatment and control variances, and the kernel density distribution of the covariates after matching in Section 1 of the online appendix. All of the results highlight the quality of the matching.

Table 2: Values of $ATET$ for investment expenses, share issuance and debt issuance after coverage initiation: IPW estimator

Outcome variable	Treatment	t-2	t	t+1
Inv	Coverage initiation	-0.387	2.394***	1.871***
		(0.482)	(0.521)	(0.543)
Shareissuance	Coverage initiation	-0.143	4.126***	1.483**
		(1.143)	(1.098)	(0.680)
Debtissuance	Coverage initiation	0.246	1.054***	1.669***
		(0.461)	(0.380)	(0.361)

Note: Cluster-robust standard errors computed at the firm level using 100 bootstrap repetitions are reported below their coefficient estimates. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

The values of $ATET$ obtained when using the IPW estimator (Table 2) indicates that, in year $t-2$, there is no difference between the average value of Inv for treated firms (i.e., firms that start being covered at t) and for treated firms had they not experienced coverage initiation. This result indicates that, in the absence of treatment, both types of groups would have had the same trends across time. However, in years t and $t+1$, the results reveal that the value of $ATET$ is significant and positive. Hence, firms that obtain analyst coverage at t exhibit an increase in their investment expenses, validating our first research hypothesis. The potential outcome of the treatment is estimated as approximately 2.4 percentage points in year t against 1.9 percentage point in year $t+1$, which is sizeable since the average of Inv is equal to 7.4. Finally, turning to financing source ratios ($ShareIssuance$ and $DebtIssuance$), we observe that, at $t-2$, all of the firms appear similar in terms of share and debt issuance, suggesting that there is no pre-trend in the data. Moreover, coverage initiation significantly improves firms' ability to issue shares and debt at t and $t+1$.[15]

Table 3: Values of $ATET$ for investment expenses, share issuance and debt issuance after coverage initiation: doubly robust estimator

Outcome variable	Treatment	t-2	t	t+1
Inv	Coverage initiation	-0.527	2.138***	1.655***
		(0.737)	(0.582)	(0.576)
$Shareissuance$	Coverage initiation	0.102	3.963***	1.951***
		(0.905)	(0.899)	(0.700)
$Debtissuance$	Coverage initiation	0.048	0.893**	1.671***
		(0.536)	(0.384)	(0.449)

Note: Cluster-robust standard errors computed at the firm level using 100 bootstrap repetitions are reported below their coefficient estimates. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

When using the doubly robust estimator (Table 3), the value of $ATET$ for Inv, $DebtIssuance$ and $shareIssuance$ in year $t-2$ is not significant, suggesting that we do not have a pre-trend in the data. Concerning years t and $t+1$, our results indicate that the value of $ATET$ for Inv, $ShareIssuance$ and $DebtIssuance$ is significant and positive. Considered together, our findings thus confirm that coverage initiation boosts firms' debt issuance and investment expenses in years t and $t+1$. Finally, our treatment effect estimates indicate that, by producing financial information about firms, analyst coverage favors their access to external financing and increases investment expenses (Doukas et al., 2008, Mola et al., 2012, Derrien and Kecskés, 2013, Chen et al., 2015), hence confirming our research hypothesis H1.

[15]In Table A3 in appendix, we assess the effect of coverage termination. The results show that coverage termination reduces significantly firms' investment rate, share issuance and debt issuance.

4.2 Baseline IV results

We now turn to the IV estimate results, which are presented in Table 4. Panel B of Table 4 reports the first-stage regression estimates of Equation 7. The coefficient for $Z_{i,t}$ reported in columns [2]-[9] is significant and positive. This result reveals that sector-level coverage waves are a strong predictor of firm-level analyst coverage. Instrument relevance might also be inferred from the F-stat form of the Kleibergen-Paap statistic reported at the bottom of Panel A in Table 4. All F-stat values are very large and greater than their critical values, confirming the relevance of our instrument.

Panel A presents the results of our estimates in Equation (8). Column [1] reports the results obtained using OLS estimates. As expected, investment expenses increase with cash flow (CF), investment opportunities (Q), and sales growth ($Growth$), and they decrease with firm size ($Size$) and leverage ($Debt$). Turning to our variable of interest $Coverage$, we also observe that analyst coverage has a positive effect on firm investment. However, because an increase in the investment rate might attract brokers, our positive estimates are potentially driven by reverse causality and therefore biased downward. In addition, analyst coverage and investment might be simultaneously determined by time-varying unobservable variables related to future firm conditions. This situation could lead to a positive (resp. negative) bias in our estimates if the omitted variable affects analyst coverage and investment in the same (resp. opposite) direction.

Table 4: Analyst coverage and investment: baseline results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	OLS	IV baseline	IV without covariates	IV with country-year fixed effects	IV with sector-year fixed effects	IV with sector trends	IV with sector firm shocks	IV with sector analyst shocks	IV with year -initial coverage effects

Panel A: Second-stage estimate

Explained variable	*Inv*	*Inv*	*Inv*	*Inv*	*Inv*	*Inv*	*Inv*	*Inv*	*Inv*
Coverage	0.115***	0.650**	0.328***	0.684**	0.638**	0.627**	0.661***	0.651**	0.735***
	(0.043)	(0.253)	(0.108)	(0.270)	(0.267)	(0.254)	(0.253)	(0.253)	(0.270)
CF	0.213***	0.211***		0.211***	0.213***	0.211***	0.212***	0.211***	0.212***
	(0.021)	(0.021)		(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)
Q	6.073***	5.084***		5.118***	5.121***	5.110***	5.099***	5.091***	4.899***
	(0.544)	(0.739)		(0.753)	(0.758)	(0.743)	(0.742)	(0.739)	(0.763)
Size	-3.575***	-4.198***		-4.392***	-4.272***	-4.226***	-4.214***	-4.187***	-4.266***
	(0.381)	(0.482)		(0.514)	(0.498)	(0.487)	(0.483)	(0.482)	(0.496)
ROE	-0.020	-0.156**		-0.148**	-0.152**	-0.144**	-0.157**	-0.155**	-0.172**
	(0.029)	(0.070)		(0.072)	(0.073)	(0.070)	(0.070)	(0.070)	(0.073)
Debt	-0.165***	-0.156***		-0.152***	-0.150***	-0.156***	-0.153***	-0.157***	-0.153***
	(0.015)	(0.016)		(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)
Growth	1.255***	1.257***		1.284***	1.173**	1.254***	1.221**	1.233**	1.224**
	(0.477)	(0.477)		(0.477)	(0.480)	(0.477)	(0.476)	(0.480)	(0.476)
Inv_g							39.724***		
							(11.809)		
CF_g							-0.064		
							(0.061)		
Q_g							-0.967		
							(2.049)		
Reco_g								-1.363	
								(1.378)	
Nb. Obs.	16,246	16,236	20,047	16,236	16,236	16,236	16,236	16,219	16,236
Nb. firms	1,498	1,497	1,691	1,497	1,497	1,497	1,497	1,494	1,497
Exc. Instruments F-stat.		345.8	482.9	274.8	343.9	386.0	349.1	345.5	320.5

Panel B: First-stage estimate

Explained variable		*Coverage*	*Coverage*	*Coverage*	*Coverage*	*Coverage*	*Coverage*	*Coverage*	*Coverage*
$Z_{i,t}$		0.157***	0.391***	0.145***	0.148***	0.157***	0.157***	0.157***	0.148***
		(0.008)	(0.017)	(0.009)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)

Note: Cluster-robust standard errors computed at the firm level are reported below their coefficient estimates. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

We address this issue in columns [2]-[9], in which we report results from our IV strategy. In variant [2], the estimated coefficient from 2SLS for the control variables is consistent with those obtained in column [1]. Moreover, as expected, the coefficient for ROE is now significant and negative. Turning to the coefficient for $Coverage$, we observe that it is positive and highly significant. The magnitude is greater (0.650) than that under OLS estimation (0.115), which might be interpreted as a reduction in a simultaneously downward bias. Specification [3] drops all of the covariates and shows that the positive estimated effect of $Coverage$ is not conditional on the set of observable variables.

Although specifications [1] to [3] include firm and year fixed effects, in columns [4] and [5], we consider other sets of fixed effects: country-year fixed effects (variant [4]) and sector-year fixed effects (variant [5]). Specification [4] allows us to control for shocks at the country level. This ability is important since national business cycles are not synchronized in Europe, thus inducing heterogeneous demand-side shocks for firms (especially those that are highly dependent on their home markets) and/or heterogeneous shocks for the supply of funds since European funding markets are not fully integrated. Specification [5] addresses omitted sector-year variables and allows us to determine that industry-level average coverage does not reflect only economic sector-year shocks. Note that collinearity with our instrument is not an issue because the instrument distinguishes two initial regimes (covered and uncovered). The coefficients reported in column [4] show the robustness of our estimates to including country-year fixed effects. Further, the results in column [5] confirm that our instrument does not consider only sector-year shocks.

Specifications [6], [7] and [8] address concerns about the omission of sector-correlated variables in a less demanding manner. In column [6], we control for observable shocks at the sector-year level by incorporating sector-specific trends, calculated as the interaction between sector dummies and year. Specification [7] includes the industry-level jackknifed average of Inv, CF and Q, denoted Inv_s, CF_s and Q_s, respectively, as new control variables. To be more explicit, the introduction of the sectoral jackknifed average of Q, for instance, allows us to control for the firm investment decisions perhaps being a function of the valuation of its peers, as emphasized by Foucault and Frésard (2014). The results indicate that the coefficient for Inv_s is highly significant and positive. Hence, investment expenses increase with the average level of investment expenses in the firm's industry. The coefficients for the other control variables are not significant. In column [8], we include the industry-level jackknifed average recommendation, denoted by $Reco_s$, as a way to control for analyst views about the firm's sector. Our results indicate that this variable does not affect investment. In specification [9] of Table 4, we introduce year dummies in interaction with firm initial coverage situation dummies (covered or uncovered) to control for different common shocks between covered and uncovered firms at the beginning of the sample. Finally, in all of these specifications, our findings show that the coefficient for *Coverage* remains significant and positive, hereby confirming our research hypothesis H2.

Finally, the results reported in Table 4 are strongly robust across specifications.[16] They are also consistent with those obtained in Tables 2 and 3. Our empirical findings thus provide strong evidence that analyst coverage boosts firms' investment expenses. This finding is in line with the results obtained for U.S. firms (Doukas et al., 2008, Mola et

[16] To save space, we provide an online appendix in which we report a number of robustness checks for our IV estimates. First, Table 2 in the online appendix explores the sensitivity of our IV results to (i) a dynamic specification of our model, (ii) a change in scale in our *coverage* indicator, (iii) the way in which we cluster standard errors and (iv) outliers. In addition, we also use our IV framework to explore the effects of coverage on share issuance and debt issuance and show that *Coverage* significantly improves access to external finance. Finally, we investigated the sensitivity of our IV results to different constructions of the instrument in Table 3 in the online appendix. All of these different sensitivity tests confirm well that exogenous variation in analyst coverage positively impacts firm investment.

al., 2012, Derrien and Kecskés, 2013, Chen et al., 2015) and emphasizes the key role of financial analysts in improving firms' financial access to capital markets in the framework of the progressively market-oriented European financial system.

5 Conclusion

The goal of this paper was to determine whether analyst coverage improves European firms' access to external financing and investment. Using a data set that includes firms from several European countries between 2000 and 2015, we implement a treatment effect estimate and an IV method. In the latter case, we propose an innovative approach that exploits the existence of sector idiosyncrasies using the intensity of industry-level waves in analyst coverage as an instrument for firm-level coverage.

In line with the literature on the favorable impact of analyst coverage on U.S. firms' financial conditions (Doukas et al., 2008, Mola et al., 2012, Derrien and Kecskés, 2013, Chen et al., 2015), our results suggest that coverage increases European firms' investment expenses and the ability to issue debt and shares on financial markets. Our paper thus emphasizes the key role of financial analysts in improving firms' access to external financing in the framework of progressively market-oriented European financial systems. It supports the view that SMEs' admission to trading in public and growth markets, which lies at the heart of the CMU project, should be articulated with the promotion of analyst coverage for newly publicly traded firms.

Our findings also echo the literature on the link between analyst coverage and the degree of investor protection. Indeed, several papers have shown that sell-side analysts issue more accurate forecasts and are more effective in monitoring firms in common-law financial systems with high levels of information disclosure, law enforcement and investor protection (Hope, 2003, Lang et al., 2004). This literature thus suggests that analyst coverage is particularly valuable in Anglo-Saxon countries. Interestingly, our paper suggests that coverage can also be effective in improving firms' financial conditions in Continental European countries.

Finally, our work could be extended by considering the regulation of firms' coverage by analysts. First, as suggested by the Securities Exchange Commission (SEC) in the case of U.S. firms and in line with reforms that applied to bond ratings in 1974, issuers could be compelled to pay to be followed by analysts when they go public. If such a model is implemented in Europe, it would be interesting to analyze the extent to which such a "paid-for system" could amplify or mitigate the favorable effects of coverage compared to a device in which research is paid for by investors. Second, based on a longer post-MiFID II data set, another valuable extension of our work could consist of exploring the extent to which this regulation has reduced the intensity of firm following by analysts and whether this intensity has in turn affected firms' access to capital markets and investments.

Appendix

Table A1: Number of observations and number of firms by country

Country	Nb. obs.	Nb. firms
Belgium	1,256	103
France	7,110	628
Germany	6,556,	546
Italy	2,578	243
The Netherlands	1,211	99
Spain	1,400	122
Total	20,111	1,741

Table A2: Summary statistics for dependent and explanatory variables and instruments

Variables	Nb. obs.	Mean	Sd	Min	Max
Inv (%)	20,111	7.4	13.1	-22.4	80.3
$ShareIssuance$ (%)	18,112	4.6	20.4	-6.6	156.7
$DebtIssuance$ (%)	17,719	1.6	10.1	-20.6	58.2
$Coverage$ (number of analysts)	20,111	5.1	8.0	0	55
CF	19,582	4.7	15.5	-75.4	49.2
Q	18,466	0.26	0.43	-0.61	1.96
$Size$ (logarithm € M)	20,111	5.1	2.3	-14.1	12.91
ROE	20,111	6.2	8.7	0	35
$Growth$ (%)	19,814	0.06	0.33	-1.41	1.67
$Debt$	20,090	24.02	19.58	0	93.1
Z (number of analysts)	20,097	5.1	4.2	0	21.6
Z' (number of analysts)	20,080	5	3.9	0	19
Z'' (number of analysts, weighted by initial size)	20,103	24.5	17.1	-36.6	146.0
Z'' (number of analysts, weighted by initial coverage)	20,103	27.7	45	0	407

Figure A1: Number of firms by year

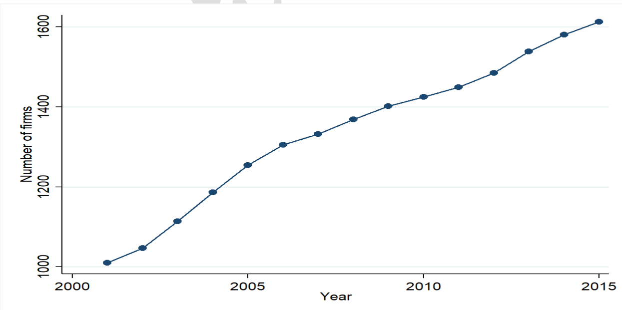

Figure A2: Amount of (re)initiation coverage by year

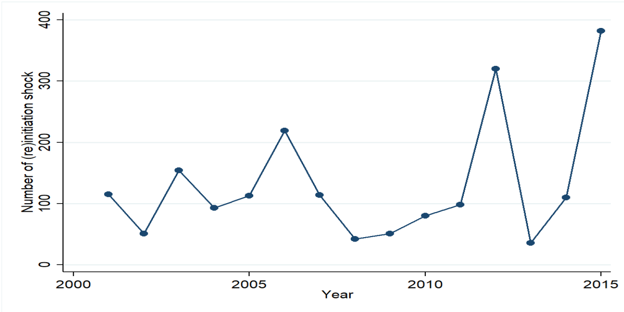

Table A3: Values of $ATET$ for investment expenses, share issuance and debt issuance after coverage termination

Outcome variable	Treatment	IPW	IPWRA
Inv	Coverage termination	-0.899***	-0.982***
		(0.346)	(0.313)
$ShareIssuance$	Coverage termination	-0.652*	-0.645**
		(0.356)	(0.309)
$DetbIssuance$	Coverage termination	-0.801***	-0.288
		(0.284)	(0.330)

Note: Cluster-robust standard errors computed at the firm level using 100 bootstrap repetitions are reported below their coefficient estimates. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

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Highlights:

- The paper assesses whether analyst coverage improves European firms' access to capital markets and investment.
- We implement a treatment effect framework and an instrumental variables (IV) approach, in which the intensity of industry-level waves in coverage is used as an instrument for firm-level coverage.
- We show that analyst coverage is favorable to firms' debt and share issuance and their investment expenses. Our paper emphasizes the key role of financial analysts in improving European firms' financial conditions.

Investment and access to external finance in Europe: Does analyst coverage matter?

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