Meet me halfway: Financial analysts and strategic change*

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ABSTRACT

Do financial analysts favor or inhibit firms' strategic change? Two long-standing theories suggest opposite predictions. First, analyst coverage could foster strategic change by decreasing managerial agency costs, a common obstacle to change initiatives. Second, as analysts are reluctant to put in the effort to evaluate novel firm strategies, their presence would discourage change. We argue that these different views can be reconciled introducing firm transparency as a relevant factor: if firms are willing to share information, analysts' search costs decrease and thus analysts are more likely to evaluate change initiatives. When firms are transparent, analysts promote strategic change. Using variation in resource allocation patterns as a proxy for strategic change, we find empirical support for our theory on a sample of 4,187 U.S. public firms.

KEYWORDS: strategic change, strategic renewal, resource allocation, financial analysts, corporate governance

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1. INTRODUCTION

What is the effect of financial analysts on firms' strategic choices and in particular on *strategic change*? While it has been convincingly established that analysts can affect firm strategies, it is still up for debate *how*. Two long-standing different streams of research seem to suggest opposite predictions on the relationship between analyst coverage and firms' propensity to change their strategy.

Research rooted in finance has stressed that analysts are instrumental to information production (e.g., Jensen and Meckling, 1976; Schipper, 1999; Asquith et al., 2005). With specific training and substantial industry background knowledge, analysts track firm actions and results on a regular basis. As a consequence of this information production and dissemination, firms followed by financial analysts are less likely to suffer from asymmetric information between managers, shareholders and the market at large, and therefore are characterized by lower (managerial) agency costs. In turn, this should make long-term, value-enhancing actions like strategic change more likely to be adopted, even if these actions are typically described as to run against short-term managerial interests and career concerns (e.g., Aghion and Stein, 2008; Gentry and Shen, 2013; Zhang and Gimeno, 2016).

Another stream of research, rooted in strategy and organization theory, rather than focusing on the effect of financial analysts on managerial agency costs, has unpacked the role of analysts in developing the 'evaluative frames' that shape stock valuations, and the analysts' own (agency) costs. Analysts do not affect valuations only through their recommendations, but also by developing evaluative frames (Beunza and Garud, 2007) that shape how market participants make sense of corporate strategies and their performance (Benner and Ranganathan, 2017). The role of these narratives is increasingly acknowledged in finance as well (Damodaran, 2017), but one key implication of this work is that not all companies (and strategies) will be equally easy to interpret. And analysts pursue their own private utility, too: they are often unwilling to devote the time and effort needed to truly understand firm strategies and develop novel evaluative frames (Zenger, 2013; Brauer and Wiersema, 2018). When firm strategies deviate from analysts' expectations and existing frames, they might not be willing to invest the time needed to make sense of the novel strategies. Hence, analysts might refrain from that effort and ultimately avoid covering firms that adopt them (e.g., Zuckerman, 1999; Litov et al., 2012; Zenger, 2013). As a consequence of the lack of analyst coverage, novel strategies are often undervalued by the market (Litov et al., 2012), with potential negative consequences for the managers that pursue them (Matsunaga and Park, 2001; Mergenthaler et al., 2012). Therefore, novel strategies are less likely to be adopted also because of managerial career concerns induced by analysts.

In sum, one the one hand analysts are seen as rational actors that act in the interest of the market and produce original and valuable information on the companies they follow. If this is the case, analyst coverage should facilitate firms' strategic change by decreasing managerial agency costs, when managers, acting out of self-interest, avoid change to lower their employment risk, even when this may run counter to shareholders' interest. On the other hand, analysts are described as having also, and perhaps mostly, private objectives. Their analyses of firm strategies involve some costs, that analysts want to minimize. These costs increase in the novelty of firm strategies. If this is the case, then, analyst coverage discourages the adoption of novel strategies, because when firms adopt them, the market punishes them.

In this paper, we argue that these long-standing different views can be reconciled by putting forward the idea that when firms are more willing to share information about their own strategy, analysts' search costs decrease, and, as a consequence, analysts are more likely to evaluate and support firms' change. When firms are transparent, financial analysts' coverage promotes strategic change. By contrast, when firms are opaque, analysts' coverage make strategic change less likely.

Using variation in resource allocation patterns as a proxy for strategic change, as common in strategic management research (e.g., Finkelstein and Hambrick, 1990; Haynes and Hillman, 2010), we examine empirically the relationship between analyst coverage and strategic risk taking using a sample of 4,187 publicly traded U.S. firms during the period from 1993 to 2014. Our baseline results reveal that, on average, firms with higher analyst coverage have a lower likelihood to encourage strategic change as compared to firms with lower coverage. These results are robust to a battery of alternative specifications, controlling also for the endogeneity of analyst coverage. Most interestingly, our results also indicate that when firms are transparent our empirical results overturn. That is, while on average analyst coverage inhibits strategic change, it actually promotes it for firms that provide truthful and reliable information. We thus show that firms' provision of information and the analysis and communication of financial analysts play a complementary role in fostering strategic change.

These results carry important implications for theory and practice. First, we contribute to the literature on financial analysts and their relationship with firm strategy. The substantial previous literature on analysts offered contradicting hypotheses on the role of analysts with regards to strategic change (Brauer and Wiersema, 2018), although nobody had directly tested it. Benner and Beunza (2020) have suggested that these contradicting perspectives can be reconciled by considering the role evaluative frames play in the process, and arguing that the influence of analysts on innovation varies depending on the phase of technological and firm evolution. We argue, and empirically show, that a way to reconcile the opposite perspectives and results of previous literature is to recognize the active role of firms in providing information transparently. Second, we contribute to the literature on strategic change. While strategic change is necessary for companies to succeed and grow (e.g., Zajac et al., 2000), a vast literature documents that strategic change is rare and difficult: there is an observed tendency for strategy to be preserved, rather than changed (e.g., Nelson and Winter, 1982; Ghemawat, 1991). An important driver of firms' observed inertia is precisely managerial short-termism, often exacerbated by financial markets (e.g., Aghion et al., 2013). In this paper, we suggest that commitment to transparency is a potential remedy to this problem.

2. THEORY AND HYPOTHESES

A strategy can be broadly defined as an observed pattern regarding how firms allocate and deploy resources across different areas (Bower, 1972; Mintzberg, 1978). Consistent with this view, we conceptualize strategic change as the variation in a firm's pattern of resource allocation over time (e.g., Haynes and Hillman, 2010). The opposite is strategic persistence, defined as the extent to which a firm's pattern of resource allocation along key dimensions remains stable over time (e.g., Finkelstein and Hambrick, 1990).

The business environment and, therefore, the optimal allocation of firm resources, are continuously changing. Hence, to remain successful, firms must be able to respond to changes in the environment or to anticipate it, by shuffling resources (e.g., Zajac et al., 2000; Kraatz and Zajac, 2001). According to a McKinsey report, 83 percent of executives identify resource reallocation as the top management lever for spurring growth and change.[1]

However, research in strategic management and business practice alike have shown that varying the allocation of resources is a challenge for most firms: there is a general tendency for it to be preserved, rather than changed (e.g., Nelson and Winter, 1982; Henderson and Clark, 1990; Ghemawat, 1991). Prior literature has suggested that companies tend not to change because they are not able to recognize the threats to the status quo, for example because of managerial cognition (e.g., Tripsas and Gavetti, 2000). Others have suggested managers might actually see a threat, but do not know how to respond or cannot immediately respond, as they have invested in specific assets (e.g., Ghemawat, 1991) or have standardized routines that prevent flexibility (e.g., Nelson and Winter, 1982).

While previous research has described a number of mechanisms that prevent firms to change and adapt, whose full discussion is out of the scope of this paper, in this study we specifically focus on one of them, that relates to managerial agency costs and the short-termism induced by financial markets (e.g., Aghion et al., 2013; Flammer and Bansal, 2017; Zhang and Gimeno, 2016). In this case, managers understand the need for change, might be able to implement it, but do not pursue it because of their own private utility (Gentry and Shen, 2013).

This mechanism is based on two key premises. First, change involves uncertainty. Any type of strategic change increases variance in performance and decreases the ability of foreseeing future firm results, at least in the short-run (e.g., Greve, 1998). Second,

[1] Click here for the report.

managers dislike variance in firm performance. Graham et al. (2005) highlight how an overwhelming majority of CFOs prefer smooth earnings (versus volatile earnings), and that almost 80% of CFOs would actually give up economic value in exchange for smooth earnings. This preference is driven by private utility considerations. Uncertainty implies more volatile earnings, a risk premium in the market, and a higher probability of having a 'negative earnings surprise.' Negative surprises, in turn, strongly affect managers in terms of takeover threats as well as reputational and career concerns (Aghion et al., 2013). As a direct consequence of negative surprises, managers may face a decline in compensation (Matsunaga and Park, 2001) and a greater likelihood of turnover (Mergenthaler et al., 2012). Given these large perceived private costs to managers, the model in Brandenburger and Polak (1996) suggests that a decision maker has higher incentives to ignore her own superior information, stabilize performance and follow the market priors, even if this leads to possibly non-value-maximizing actions. Aghion and Stein (2008) similarly show that managers are more prone to continue with the status quo, the strategy that is expected by the market. In sum, the short-term uncertainty induced by strategic change may hurt managers (short-term) private utility, and therefore managers shirk from costly efforts at (long-term) change, even if it would be valuable for shareholders. This is only possible because the market has often only incomplete information about firm strategies and managerial actions (Litov et al., 2012). Against this background, we ask the following question: *Does analyst coverage favor or inhibit firms' strategic change?*

In principle, the effect could go either way. If it is true that managers are able to prioritize their private utility over firm long-term profitability thanks to the asymmetry of information that prevent shareholders to understand the need for change and investors to evaluate the long-term value of change, then having more information available —thanks to a higher analyst coverage —should actually decrease managerial agency costs and, as a consequence, managers' resistance to change. Analysts produce valuable information as they are experts on the firms they cover. By constantly analyzing and disseminating information about the future prospects of a firm, financial analysts have been conceived as information intermediaries that fulfill an important monitoring function in the financial markets (Jensen and Meckling, 1976; Litov et al., 2012; Gentry and Shen, 2013). The information produced by analysts is readily reflected in equity prices. Womack (1996) provides evidence that new analyst recommendations create significant price and volume changes on the market: on the day that a new "buy" recommendation is issued, the target stock appreciates 3%, and its trading volume doubles. Survey evidence obtained by Graham et al. (2005) confirms that managers believe that analysts are important economic agents in setting the stock price of their firm. Empirical research in financial economics further indicates that higher analyst coverage actually implies more (and better) information available. For example, Barth and Hutton (2004) find that stock prices for firms with higher analyst coverage incorporate more rapidly information on accruals and cash flows. Similarly, Ellul and Panayides (2018) document that an exogenous termination of analyst coverage leads to deteriorating liquidity and price efficiency, more informed trading, and higher profitability of insider trades, indicating that the presence of higher analyst coverage restrains insiders from exploiting their informational advantage over outsiders with beneficial effects on market quality. In sum, the literature perceives analysts as performing an important informational role: they provide accurate, non-obvious information on the firms they follow, and that information is efficiently reflected in market prices. Therefore, increasing analyst coverage increases the knowledge available on covered firms, and hence decreases asymmetric information between managers and the market

Coherently, it has been shown that firms followed by fewer analysts are more likely to suffer from information asymmetries and engage in non-value maximizing activities. For example, Chen et al. (2015) show that when analyst coverage decreases, shareholders value their internal cash holding less, CEOs receive higher excess compensation, and they are more likely to engage in value-destroying acquisitions. Similarly, Derrien and Kecskés (2013) document that a decrease in analyst coverage increases the cost of capital, which results in a decrease in firm investments. Taken together, these arguments lead to the following hypothesis:

Hypothesis (H1a). *Firm strategic change increases as the number of analysts following a firm increases.*

It is also possible to argue that higher analyst coverage can make strategic change even more difficult and less likely. Consider the process described above, whereby change introduces uncertainty that managers dislike because of career concerns. First, higher analyst coverage can increase the variance in firm performance induced by strategic change. Research shows that firms have a natural tendency to release information selectively: they are more inclined to share good news rather than bad ones. Inter alia, Graham et al. (2005) provide evidence that poorly performing firms are more likely to delay the communication of bad news, while Hong et al. (2000) show that firm-specific negative information diffuses only gradually across the investing public. Fabrizio and Kim (2019) make similar arguments with respect to environmental ratings: firms with unfavorable news obfuscate their language in information disclosure. An increase in analyst coverage, however, may increase firms' pressure to disclose news more precisely and transparently. For example, Yu (2008) shows that firms followed by more analysts do less accrual-based earnings management. And while good news would travel fast anyhow, even without the support of analysts, higher coverage improves information diffusion particularly when it would be scarce, i.e., in case of bad news. Hence, as the number of analysts grows, while information diffusion on good news might remain relatively constant, information disclosure on bad news increases, thus augmenting the variance in firm expected results.

This effect can also be reinforced by analysts' negative preference for firms' change (Zenger, 2013). Changes in firm strategy implies costly investments for the analysts, who have to acquire new knowledge themselves. These costs temper analysts' incentives for analysis, leaving the market less informed about firms with strategies that are more costly to evaluate (Brauer and Wiersema, 2018). Consistently, Litov et al. (2012) find that more unusual strategies receive less coverage by analysts. Similarly, in the context of uncertain technological change, Benner (2010) finds that firms pursuing more radical technology investments in response to new technology received less attention from analysts than firms adopting incremental extensions of existing technology. And in the context of diversification, Zuckerman (1999) shows that firms are followed by fewer analysts when firms' diversification strategies deviate from the industry categories covered by analysts.

In turn, this lack of attention from analysts lead to a decrease in market value (Litov et al., 2012). As a consequence, analysts might increase the downside risk of strategic change by simultaneously increasing the variance and decreasing the average of firm (expected) future performance.

Second, an increase in analyst coverage increases the probability that, conditional upon negative surprises, managers will face setbacks in their careers. On the one hand, it has been shown that the market reaction to earnings disclosure and surprises increases with analyst coverage, and the effect is driven by firms that miss their analyst earnings forecasts, i.e., the effect of bad news on stock prices is emphasized as analyst coverage increases, while that of good news is not (Huang et al., 2017). On the other hand, and strictly related, the consequences of bad news on managerial career are also magnified under the lens of an increased analyst coverage. Puffer and Weintrop (1991) examining the link between firm performance and CEO turnover, and argue that analyst forecasts, rather than accounting performance, set the expectations and the fate of a CEO. Wiersema and Zhang (2011, p. 1179) claim that financial analysts "*provide the board with third party certification of the CEO's ability and performance.*" Overall, these arguments suggest that a higher analyst coverage reinforces the negative effect of managerial agency costs on strategic change. Thus, we hypothesize:

> **Hypothesis (H1b).** *Firm strategic change decreases as the number of analysts following a firm increases.*

The two competing hypotheses presented above stem from two different perspectives on the nature and the role of analysts, that have longly co-existed in the related literature. One perspective sees analysts as knowledgeable experts, who by constantly analyzing and disseminating information about the future prospects of a company, fulfill effectively a fundamental monitoring function in the financial markets. The other perspective recognizes that analysts are imperfectly rational, potentially biased individuals, who produce evaluative frames that shape how all market participants make sense of corporate decisions and performance. Given that the development of novel frames is costly, analysts do not always exert the needed effort to updated them in order to fully understand firms' strategies. Both perspectives share a common unrealistic assumption on the homogeneity of the interpretive challenges different firms and strategies pose. The finance literature generally assumes there is no significant cost in collecting information for financial analysts. By contrast, even if the 'evaluative frame' perspective acknowledges that developing these frames is costly, it only recognizes that this cost depends on firms' strategy, and not on the way in which a firm communicates it. In other words, it is assumed that it is equally costly to collect information for novel strategies for any firm. In this paper, we propose that the two seemingly contradicting perspectives on analysts discussed above can actually be reconciled in a realistic way by relaxing this assumption.

In reality, it is known that firms vary greatly in their transparency and willingness to share (reliable) information, in all domains, from accounting to environmental performance (Bushman et al., 2004; Fabrizio and Kim, 2019). Similarly, anecdotal evidence suggests that analysts proactively search for valuable information and they are actually often going beyond the financial information provided by the firm, in order to develop novel evaluative frames that can help make sense of the firm's strategy and its market valuation. To cite just one example, a recent report on Tesla reported: "*We have driven a Tesla for seven months in preparation for this report, and after conducting investor meetings with the company last week, we're finally ready to take a stand, [...] we wanted to do a thorough job of due diligence before making an actionable call.*" It is thus likely that when provided with better, reliable information by the firm, analysts would not shirk from exploring in depth the novel strategy the firm is introducing.

We thus propose that analysts' and firms' role in facilitating strategic change are complementary, in that analysts can actually facilitate the market's understanding of the need of strategic change if and when the firm is transparent in the first place, and it shares openly truthful information. By sharing information, firms can reduces analysts' evaluation costs —who can then analyze and diffuse information properly. Formally:

Hypothesis (H2). *At high level of firm transparency, firm strategic change increases as the number of analysts following a firm increases.*

3. DATA AND METHODS

3.1. Sample selection

The sample used in this paper includes information on U.S. public firms for the period between 1993 and 2014. We start with all firms traded on NYSE, Amex, or NASDAQ that are in the Compustat database during the specified period. We exclude financial firms with standard industrial classification (SIC) codes between 6000 and 6999 as well as firms with book value of assets less than $10 million. For the remaining firms, we retrieve accounting information from Compustat and then merge these companies with the information from the other databases. We obtain data on analyst coverage and managerial guidance from the Institutional Brokers' Estimate System (I/B/E/S). We collect institutional ownership information from Thomson's Institutional Holdings dataset (SEC Form 13-F), stock price and volatility information from the Center for Research in Security Prices (CRSP), intraday trades and quotes for constructing stock illiquidity measures from the Trade and Quote (TAQ) database, and corporate governance information from the Institutional Shareholder Services (ISS) governance database. Our final sample for the baseline regressions consists of 39,584 firm-year observations on 4,187 firms. Table 1 provides summary statistics for the main variables used in the empirical analysis.

3.2. Variables

3.2.1. Dependent variable

Our key dependent variable, strategic change, reflects the extent to which a firm's strategy changes over time (Finkelstein and Hambrick, 1990). We conceptualize a firm's strategy as a pattern in a stream of important decisions regarding the allocation of resources which are under the discretion of the top management team. Following common practice in the strategy literature (e.g., Finkelstein and Hambrick, 1990; Zhang, 2006; Chatterjee and Hambrick, 2007; Haynes and Hillman, 2010; Quigley and Hambrick, 2012; Bednar et al., 2013; Oehmichen et al., 2017), we use a composite measure that consists of the following six strategic resource dimensions: (1) advertising intensity (advertising expense/net sales), (2) R&D intensity (R&D expense/net sales), (3) plant and equipment newness (net property, plant and equipment/gross property, plant and equipment), (4) nonproduction overhead (selling, general, and administrative expense/net sales), (5) inventory levels (inventories/net sales), and (6) financial leverage (total debt/stockholders equity). The data for these measures are retrieved from Compustat.

A lack of change in these measures from company's historical pattern represents strategic persistence. In contrast, large changes in these ratios (either an increase or a decrease) indicate important changes to firm's resource allocation decisions and thus change in the underlying strategy of the firm. To calculate change scores over time, we use the approach put forth in Carpenter (2000) and Haynes and Hillman (2010), among others, which uses a combination of exponential smoothing and Euclidean distance calculations. First, we establish baseline variation patterns for each firm (and year) using the exponentially smoothed historical resources allocation ratios over that last five years (i.e., from $t-4$ to $t=0$).[2] The absolute value of simple differences representing divergence from historical resource allocation were calculated between the exponentially smoothed, forecasted amount and the actual resource allocation ratios. The result of the procedures were the divergence of the firms' actual resource allocation profiles from the forecasted resource allocation profiles, based on the firms' previous five years of data, and are then summed. In order to normalize the dependent variable, we calculated the natural logarithm of the differences.

3.2.2. Independent variable

Analyst coverage is the main independent variable in our regressions. We measure analyst coverage using the number of analysts that issue forecasts for a firm. Following the literature, we calculate the number of analysts as the mean of the 12 monthly numbers of earnings forecasts that a firm receives annually from the I/B/E/S summary file. We

[2] As explained in Haynes and Hillman (2010), a five-year window is appropriate to establish variation patterns since it is broad enough to capture strategic change and narrow enough to exclude changes in firm's external environment. Further, in comparison to using simple averages, exponential smoothing enables us to place more emphasis on more recent years while, at the same time, incorporating information gained from previous years.

use this number because most analysts issue at least one earnings forecast for a firm in a year and the majority of them issues at most one earnings forecast each month. The firm-year observations which are not followed by analysts have missing information in the I/B/E/S database; we set those observations to zero (e.g., as in He and Tian, 2013; Guo et al., 2019) We then take the natural logarithm of (one plus) this raw measure to construct our final measure of analyst coverage.

3.2.3. Control variables

Following the strategy and finance literature, we control for a vector of firm and industry characteristics that could affect firm's strategic change behaviour. All control variables are one year lagged with respect to our dependent variable. The usual baseline control variables are firm size, which is the natural logarithm of total assets; firm age, which is the natural logarithm of the number of years since a firm has been included in Compustat; and firm return on assets. We also control for firm Tobin's Q, which captures firm's growth opportunities; capital-labor ratio (K/L), which is the natural logarithm of property, plant, and equipment scaled by the number of employees; capex, which is capital expenditures scaled by total assets; and stock illiquidity, which is the natural logarithm of relative effective spreads (He and Tian, 2013). Further, since the demand for analyst services could be an increasing function a firm's stock return volatility (Chan and Hameed, 2006), and return volatility is, in turn, likely to be correlated with managerial risk-taking preferences (Cassell et al., 2012), we include stock return volatility as an additional control variable. This variable is calculated as the standard deviation of the monthly stock returns for each firm for the fiscal year. Likewise, institutional demands for information about a particular firms could also affect analyst decisions about which firm to follow (O'Brien and Bhushan, 1990). At the same time, the results in Aghion et al. (2013) suggest that the presence of institutional investors has an impact on firm investment decisions. Therefore, we also include the fraction of institutional ownership. Finally, since incentives to change strategy may also be affected by the degree of competition within an industry, we include the Hirschman-Herfindahl index based on market shares, to measure industry concentration, and its squared term to mitigate concerns regarding non-linear effects of product market competition on strategic change.

3.2.4. Firm transparency

We measure firm transparency in three different ways. Our first measure is actually an indirect measure. As has been argued in the literature, managers in well-governed firms have strong incentives to follow close-to-optimal disclosure policies (Irani and Oesch, 2013; Boone and White, 2015). To measure corporate governance, we use the 'governance index' (G-index), which aggregates information on shareholder rights at the firm-level (Gompers, Ishii, and Metrick, 2003). We obtain data on the G-index from the ISS (formerly known as Riskmetrics and IRRC) governance database. This index assigns a value of one to each of 24 firm-level governance provisions and is computed as the sum of these provisions. For a given firm, a high G-index corresponds to the presence of more provisions protecting management, thus leading to more entrenched management. Since the ISS database is restricted to S&P 1500 firms, this variable is not available for all firms included in our sample.[3]

The second variable we use follows Anantharaman and Zhang (2011), and is based on data from managerial guidance. The authors show that the quantity and quality of corporate disclosure, in particular financial guidance from managers, are key drivers for analysts' interest in and willing to cover firms (see also Lang and Lundholm, 1996; Bushee and Miller, 2012). We procure management forecasts from the I/B/E/S Guidance file, which includes information previously available in the Company Issued Guidance file and information from the defunct First Call database. We include forecasts of both annual and quarterly EPS, and drop forecasts before 2001, the period before Regulation Fair Disclosure (Reg. FD) became effective.[4] Following Anilowski et al. (2007), we further remove observations with missing earnings announcement dates and those with guidance

[3]Further note that the G-index is not available for all the years during our sample period. When the G-index is missing, we follow the existing literature and use the G-index from the latest year available.

[4]We exclude the pre-Reg. FD period because pre-Reg. FD, managers had the option to communicate privately with analysts in response to analysts' add/drop decisions. Therefore, managerial guidance is not completely observable (see Anantharaman and Zhang, 2011; Balakrishnan et al., 2014).

dates occurring on or after the actual earnings announcement date. To ensure the highest degree of precision, we restrict our analysis only to forecasting firms, i.e., firms that are included in the I/B/E/S Guidance database and have provided managerial forecasts in the past (e.g., as in Balakrishnan et al., 2014). We measure financial reporting frequency as the natural logarithm of number managerial earnings forecasts made during the fiscal year.

Our third variable of transparency is a common measure of financial reporting quality (FRQ). Specifically, we follow the earnings management literature and construct an accrual-based measure. Accounting adjustments can be used to split earnings up into cash flows and accruals. Both the size and sign of accruals are subject to management's judgement and can be used to manipulate reported earnings. Managerial discretion in the use of accruals can make it harder for firm outsiders to evaluate the true economic content of firms' financial statements (Irani and Oesch, 2013, 2016).

We construct financial reporting quality as follows. First, we estimate the "normal" level of accruals for a given firm, using coefficients obtained from an industry-level cross-sectional regression model of accruals. To estimate the normal level of accruals, we use the Jones (1991) model in its modified version (Dechow et al., 1995). To this end, we first run the following regression for each Fama-French 48 industry and each year:

$$\frac{TA_{i,t}}{Assets_{i,t-1}} = \beta_1 \frac{1}{Assets_{i,t-1}} + \beta_2 \frac{\triangle REV_{i,t}}{Assets_{i,t-1}} + \beta_3 \frac{PPE_{i,t}}{Assets_{i,t-1}} + \varepsilon_{i,t} \tag{1}$$

where $TA_{i,t}$ denotes total accruals of firm i in year t, computed as the difference between net income and cash flow from operations; $\triangle REV$ is the difference in sales revenues; and PPE is gross property, plant and equipment. All variables are all normalized by one-year lagged total assets. The estimated coefficients from Eq. (1) are then used to calculate normal accruals (NA) for each firm:

$$\frac{NA_{i,t}}{Assets_{i,t-1}} = \hat{\beta}_1 \frac{1}{Assets_{i,t-1}} + \hat{\beta}_3 \frac{\triangle REV_{i,t} - \triangle AR_{i,t}}{Assets_{i,t-1}} + \hat{\beta}_3 \frac{PPE_{i,t}}{Assets_{i,t-1}} + \varepsilon_{i,t} \tag{2}$$

where $\triangle AR$ is the change in receivables and the remaining variables are the same as before. As a last step, we compute our measure of financial reporting quality (FRQ) as the the absolute difference between total accruals and the predicted firm-level normal accruals:

$$FRQ_{i,t} = |TA_{i,t} - NA_{i,t}| \tag{3}$$

We interpret large absolute abnormal accruals as an indication of relatively high differences between the cash flows and the earnings of a firm. Large abnormal accruals thus make it harder for investors to learn about the true economic performance of a company and indicate lower financial reporting quality (Irani and Oesch, 2013, 2016).

[Insert Table 1 Here]

4. EMPIRICAL RESULTS

4.1. Testing H1a and H1b

4.1.1. Baseline results

We begin by testing the simple, direct effect of analyst coverage on firms' strategic change, contrasting the two long-standing streams of literature described in Section 2. Once we have convincingly established this direct (average) effect, we turn to the analysis of the potential moderating effect of firm transparency. The idea is to first examine which of the two traditional views expressed in the prior literature applies to strategic change (H1a vs. H1b), and then to see whether it is possible to reconcile them by considering firm transparency (H2).

To assess how analyst coverage is associated with strategic change, we estimate various forms of the following model:

$$Y_{i,t+1} = \beta Analyst\ coverage_{i,t} + \gamma Z_{i,t} + \delta_t + \lambda_i + \varepsilon_{i,t} \tag{4}$$

where the dependent variable, $Y_{i,t+1}$, is the natural logarithm of the change from previous resource allocation decisions, as defined in Section 3.2.1. Because both strategic change and analyst coverage are in logarithmic form, β gives us the elasticity of strategic change to analyst coverage. The remaining control variables, included in $Z_{i,t}$, capture firm and industry characteristics, as described in Section 3.2.3. δ_t are time dummies that account for inter-temporal variation that may affect the relationship between analyst coverage and strategic change; λ_i are firm fixed effects that account for unobserved time-invariant heterogeneity across firms. Because strategic change metrics could be autocorrelated over time, all our models will allow the standard errors to have arbitrary heteroskedasticity and autocorrelation (i.e., clustering standard errors by firm).

We start with a parsimonious (unreported) model that regresses the strategic change measure in the next year on the key independent variable, *Analyst coverage*. We find that the coefficient on analyst coverage is 0.038 (p-value = 0.000), suggesting a positive raw association between analyst coverage and strategic change. We then add δ_t to absorb any aggregate time effect and report the results in column 1 of Table 2. The coefficient on coverage remains positive (p-value = 0.000). Column 2 includes the vector of firm fixed effects, λ_i, and thus, the coefficient can be interpreted as signifying that a positive change in analyst coverage within a firm is associated with a change in strategy. Such change-on-change models with the lagged dependent variable help us from making inferences from possible spurious results from problematic error terms. Consistent with the direction of the bias observed in He and Tian (2013), we find that the coefficient turns negative (p-value = 0.000) when firm fixed effects are included, suggesting that firms that face an increase in the number of analysts following them are less likely to pursue strategies that differ from historical paths. This results also suggests that time-invariant omitted factors, such as unobservable firm quality, might positively affect both analyst coverage and strategic change.

However, omitting time-variant variables that are correlated with analyst coverage and strategic change could bias our estimate, too. In column 3, we therefore add our set of control variables to the model: firm size, age, profitability, capital expenditures, capital intensity, growth opportunities, the Hirschman-Herfindahl index as well as its squared term, institutional investors, stock illiquidity and return volatility. The coefficient on analyst coverage remains negative (p-value $= 0.000$) and the control variables even slightly increase the marginal effect of analysts coverage from -0.043 to -0.045. The coefficients on the controls are consistent with those found in Carpenter (2000). Older and more established firms face bureaucratic momentum and often have severe difficulties effecting change. Firm performance also has the same direction on strategic change that is seen in Carpenter (2000).

In the last column of Table 2, we explore the heterogeneity in the effect by examining different thresholds of analysts coverage. In particular, we create dummies for quartiles of the distribution of analyst coverage. These categories are (almost) no coverage (lowest quartile), low coverage (second quartile), medium coverage (third quartile), and high coverage (fourth quartile). We use the no coverage category as our baseline in the regression. Column 5 shows a large coefficient estimate for medium (-0.062) and high coverage (-0.118) and a much lower estimate for the low coverage dummy (-0.029). In terms of economic significance, the coefficient estimate on the high coverage dummy (p-value $=$ 0.000) implies that firms covered by 11 or more analysts diverge, on average, about 11.8% less from previous resource allocation patterns as compared to firms with fewer than one analyst, and thus, those firms are less likely to change strategy.

Overall, therefore, our results reject *Hypothesis 1a* and are consistent with *Hypothesis 1b*: An increase in analyst coverage makes strategic change less likely.

[Insert Table 2 Here]

4.1.2. Alternative dependent variables

To ensure robustness, we employ several alternative measures of our dependent variable, for example to address the concerns that some resource allocation ratios have several missing values in Compustat and that it could take more than one year for decision makers to implement strategic change. We also assess the possibility that the impact of financial analysts on strategy extends to the degree to which a firm deviates from industry norms (as opposed to its past behavior).

Panel A of Table 3 reports regression results from replacing our main dependent variable with several variations of it. In column 1, we use a two-year time lag between analyst coverage and strategic change (as opposed to one year). In columns 2 and 3, we use a measure composed of the three dimension (1) plant and equipment newness, (2) nonproduction overhead, and (3) financial leverage in one and two years ahead, respectively. Thus, we drop the three ratios that have frequently missing values in the Compustat data. Across all the columns of Panel A, the coefficients on analyst coverage range between -0.047 and -0.051. In Panel B of Table 3, we repeat the same exercise but examine the impact of coverage on the deviation from industry norms. Specifically, we computed the absolute value of the difference between the industry norm of competition and firm's resource allocation pattern. We first classify each firm into Fama-French 48 industries. We then use the top four firms in each industry based on the commonly used four-firm concentration ratio to establish industry resource allocation norms. In particular, the strategic norm for each industry is established as the sum of the average of the top four firms' six sales-weighted resource allocation ratios. Firm-level divergence from the industry norm is then calculated as the natural logarithm of the absolute value of the difference between the composite measure reflecting the industry norm and firm's resource allocation ratios (see Carpenter, 2000; Haynes and Hillman, 2010). Similar to the results on strategic change, we find that the coefficients of analyst coverage are negative in those equations, suggesting that higher coverage is associated also with a tendency to gravitate around industry norms.

[Insert Table 3 Here]

4.1.3. Miscellaneous robustness tests

We conduct several additional robustness tests for our baseline empirical analysis and report the results in Tables 4. First, we repeat the baseline regression by using various sub-samples. We start with the sub-sample of firms that are in the S&P 500 index to check whether our results are driven by small and young firms that are typically covered by fewer analysts and may face distinctive conditions when it comes down to strategic change (Quigley and Hambrick, 2012). We report the results in column 1 of Table 4. The coefficient on analyst coverage remains similar to our baseline estimate (i.e., -0.037 versus -0.045), suggesting that our results hold for large and mature firms. We then address the concern that our sample period overlaps with the dot-com bubble (1995 – 2000) and the financial crisis (2007 – 2008). In particular, it could be questioned whether our results are largely driven by dynamics of resource availability triggered by crisis periods. As can be seen in column 2, however, the coefficient on analyst coverage remains negative with a slightly larger estimate (-0.053) when we exclude those years from our analysis. We further examine a sub-sample including only firms with positive analyst coverage. As emphasized in Section 3.2.2, our baseline includes firms without analyst coverage. In column 3, we obtain a negative but much stronger coefficient on analyst coverage (-0.087) when we restrict to this more homogeneous group of firms.

Second, we check whether our baseline results are robust to an alternative proxy for analyst coverage. In particular, we want to address the concern that analyst coverage is associated with many factors that could also affect strategic change. For this reason, we follow Yu (2008) and construct "residual coverage" to account for confounding effects of some of these factors. We estimate the following model:

$$\begin{aligned} Analyst\ coverage_{i,t} = {} & + \beta_1 Firm\ size_{i,t} + \beta_2 ROA_{i,t-1} + \beta_3 Growth_{i,t} + \beta_4 External \\ & financing_{i,t} + \beta_5 Cash\ flow\ volatility_{i,t} + \delta_t + \varepsilon_{i,t} \end{aligned} \tag{5}$$

where firm size is measured by the natural logarithm of total assets, past performance is measured by the lagged ROA, growth is measured by the growth rate of total assets, external financing activities is measured by the net cash proceeds from equity and debt financing scaled by total assets, and cash flow volatility is measured by the standard deviation of cash flows of a firm in the entire sample period, scaled by lagged assets.[5]

[5]Following Bradshaw et al. (2006), we define net cash proceeds from equity financing as the the cash proceeds from sales of common and preferred stocks less cash payments for purchases of common and preferred stocks less cash payments for dividends. Net cash flow from debt financing is the cash proceeds from the issuance of long-term debt less cash payments for long-term debt reductions plus the net changes in current debt.

We then take the residual from the above regression, and use it as an alternative analyst coverage measure; standard errors are obtained from bootstrapping. In column 4 of Table 4, we find that the coefficient on residual coverage is -0.039 (p-value = 0.000), similar to our baseline estimate.

[Insert Table 4 Here]

4.1.4. Quasi-natural experiment

So far, we have established a robust negative association between analyst coverage and strategic change. While our approach has been to include a wide range of firm characteristics, we are, however, still concerned that omitted variables correlated with analyst coverage and strategic change could bias the results towards the findings reported above. Although the inclusion of firm fixed effects alleviates the concern of omitted variables that remain constant over time, it cannot solve the issue if the omitted variables are time-variant. Moreover, there is the potential reverse causality concern that expected changes in firm's strategy may affect a firm's current coverage, i.e., firms with less variation in their strategic decisions attract more financial analysts.

Our identification approach relies on the quasi-natural experiments used in Hong and Kacperczyk (2010) and Kelly and Ljungqvist (2012), among others. Those experiments are based on brokerage mergers and brokerage closures that generate plausible exogenous variation in the number of analysts following a firm. Brokerage mergers and closures are desirable because (i) the existing literature provides extensive support for the assumption that the resulting loss in coverage is orthogonal to firm characteristics and decisions (e.g., Hong and Kacperczyk, 2010; Kelly and Ljungqvist, 2012; Derrien and Kecskés, 2013), and (ii) those events affect many firms from various industries over several years. This latter feature mitigates concerns that confounding events that coincide with brokerage disappearance drive the results. We conduct difference-in-differences (DiD) tests surrounding the brokerage merger/closure events.

Our treatment sample is a combination of firms affected by the merger events from Hong and Kacperczyk (2010) that occurred during our sample period (13 out of 15) and firms affected by all merger/closure events from Kelly and Ljungqvist (2012). For the brokerage mergers in Hong and Kacperczyk (2010), we obtain the list of firms (identified by PERMNO) from Marcin Kacperczyk's website, and we exclude sample firms with the stop indicator equal to one because those firms have a coverage drop prior to the merger event. For the remaining events (18 brokerage mergers and 21 closures) that are identified in Kelly and Ljungqvist (2012), we follow the approach in Derrien and Kecskés (2013) and He and Tian (2013) to determine the firms whose analyst coverage is affected by the merger/closure events. We proceed as follows. First, for each event, we define the event period as the six months around the broker disappearance month; as emphasized in He and Tian (2013), this accounts for the fact that some mergers spanned several days or even a couple of months. Next, we retrieve all firms covered by the event brokers in the 12-months before the event period $[-15, -3)$ as well as the analysts working for them. We assume that an analyst covers a firm if there is at least one earnings estimate in the I/B/E/S Detail History file for that firm in the pre-event period. Similarly, we assume that an analyst disappears if there is no earnings estimate by him/her in the I/B/E/S records in the 12-months after the event period $(+3, +15]$.[6]

For brokerage closures, we retain firms for which the analyst disappears from I/B/E/S in the post-event period; using those analysts that do not issue any earnings estimate during that period ensures that analysts who transition to other brokerage houses do not continue to cover those firms. For brokerage mergers, we retain firm that are covered by both the acquirer and the target brokerage house before the merger period and for which one of their analysts disappears; this ensures that the resulting loss in coverage is indeed due to the brokerage merger. Following Kelly and Ljungqvist (2012), we further drop firms from the sample that are no longer covered by the acquirer in the period after the event; the reason for this last restriction is that such terminations could be endogenous because the acquiring brokerage house has chosen to stop covering the firm for reasons that are not observable to us.

For each event, we then continue to construct a control sample of unaffected firms

[6]Note that the analyst identifier in I/B/E/S are unique. This allows us to track their careers across the different periods.

(firms that are not covered by both merging brokerage houses or the closing brokerage house before the event) that are present in Compustat and I/B/E/S during the event window. Following other studies that relate brokerage house disappearance and firm-level outcomes (e.g., He and Tian, 2013; Guo et al., 2019), we use a five-year event window consisting of two years before and two years after the event. Hence, we retain firms that are active in Compustat and have coverage in the I/B/E/S file during the five-year window that corresponds to each event. Since we include both treatment and control firms in the empirical analysis, we impose these restrictions on both groups.

There are two additional points that we need to consider. First, because our variables are measured on an annual basis, we have to avoid overlaps in the pre- and post-event period. To do so, we use the last fiscal year that ends in the pre-event period $[-15, -3)$ as year -1 and the first fiscal year that starts in the post-event period $(+3, +15]$ as year $+1$. Second, it is possible that firm-year observations overlap across events. This could happen, for example, if a firm is in the treatment group in a one period and in the control group in the same period but for another event. To address this, we restrict the control sample to firms that are not included in any treatment cohort during the relevant pre- or post-treatment time window. This design implies that some firms serve as treatment firms in one period and control firms in another, although never within a five-year window surrounding treatment.

To implement the DiD identification strategy, one remaining concern is that the treatment and the control sample could differ in important dimensions which could affect the estimate on the coverage loss. For example, it is possible that firms with more volatile stock returns are covered by more brokerage houses (and are thus more likely to be treated) and this could also reflect changes in resource allocation patterns. Thus, it is important to control for such differences in our empirical specifications to ensure that we can attribute the effect to a shock in coverage. We mitigate this concern in the following way. First, we incorporate our baseline control variables into the DiD regression framework. Second, we implement a DiD matching estimator. To take into account multiple brokerage merger/closure events, we estimate the following equation:

$$
Y_{i,e,t+1} = \beta_1 Post_{e,t} + \beta_2 Treated_{i,e} + \beta_3 Post_{e,t} \times Treated_{i,e}
+ \gamma Z_{i,t} + \delta_t + \lambda_i + \theta_e + \varepsilon_{i,t} \tag{6}
$$

where $Y_{i,e,t+1}$ is the change in resource allocation patterns from year t to year $t+1$ for firm i, which is either in the treatment or the control sample for event e. $Post_{e,t}$ is a dummy variable equal to one for a firm in the post-event period for event e, and $Treated_{i,e}$ is a dummy variable that indicates whether firm i is part of the treatment sample for event e. The coefficient of interest is β_3. It captures the impact of a decrease in analyst coverage after a brokerage house merger/closure on changes in strategy of the treated firms relative to the control firms. $Z_{i,t}$ is the vector of firm-specific control variables as detailed in Section 3.2.3. The variables δ_t, λ_i, and θ_e correspond to year, firm, and event fixed effects, respectively. We cluster robust standard errors at the firm level.[7]

Table 5 presents the results. In column 1, we start with the key idea of the experiment: on average, a firm in the treatment sample should loose about one analysts relative to a control firm in the period after the brokerage house merger/closure event. We test this in the regression framework described above but use analyst coverage as the dependent variable. The estimated DiD coefficient is -1.224 (p-value = 0.000) suggesting that our setting indeed captures the effect of a decrease in coverage. In column 2 – 6, we examine the impact of this loss in coverage on strategic change. Column 2 presents the results from estimating Eq. (6) without control variables or event fixed effects. As can be seen, the DiD coefficient, β_3, is positive (p-value = 0.000). This implies that, as the number of analysts decreases (recall the treatment is an exogenous decrease in analyst coverage), firms' strategic change increases. In column 3, we run the same regression model but include our battery of controls, and, in column 4, we further condition on event fixed effects. The latter ensures that the estimate is not due to systematic differences in strategic change behaviour across events. In both specifications, the DiD estimate remains positive (with p-value = 0.000) and with similar magnitude.

[7] Note that we also experiment with clustering standard errors at the firm-event level (e.g., as in Guo et al., 2019). However, clustering at the firm level tends to produce the more conservative standard errors. For example, in an identical specification to column 3 of Table 5, the standard error on the DiD estimate is 0.010 when clustering at the firm-event level.

Next, we examine how the adjustment behaviour in resource allocation patterns varies with initial analyst coverage. We expect firms experiencing a large percentage reduction in coverage to adjust their resource allocation patterns behavior more sharply. This is an important way to test the validity of our identification approach, and is common in the related literature (e.g., Hong and Kacperczyk, 2010; He and Tian, 2013; Irani and Oesch, 2016). Columns 5 and 6 confirm this intuition. We re-estimate Eq. (6) by splitting the sample into firms with 'low' and 'high' initial coverage, depending on whether analyst coverage in the year prior the brokerage house merger/closure events is above or below the sample median. As can be seen, the cross-sectional effect is concentrated among firms in the low initial coverage sub-sample, which are firms where the loss of one analyst represents a larger percentage change in analyst coverage. For those firms, the DiD coefficient is positive (p-value = 0.018), and the parameter estimate, 0.104, is about two times larger in magnitude than the corresponding DiD coefficient for the full sample (i.e., 0.051 in column 4). For firms with high initial coverage, the DiD estimate is small in magnitude and indistinguishable from zero (p-value = 0.889).

We now turn to the second DiD approach in which we account for potential differences between the treatment and the control groups by using a matching estimator similar to that used in Derrien and Kecskés (2013), Irani and Oesch (2013) or Guo et al. (2019). We construct a control sample of matched firms based on observable firm-level characteristics measured in the year prior to each event (i.e., year -1). In particular, we match on firm size, ROA, Q, return volatility and analyst coverage, which correspond to the most commonly used variables in the related literature. To implement the matching scheme, we first estimate a logit model with the dependent variable equal to one if a firm-year is classified as treated (and zero otherwise) on our matching variables. The sample used to estimate this regression consists of 1,848 treatment and 27,857 candidate control pre-event firm-years. This is the sample of treated and control firms with all control variables available in the pre-event year. Second, we use the estimated coefficients to predict propensity scores of treatment, which are then used to perform nearest neighbor matching with replacement using a standard tolerance (0.005 caliper). We allow for up to five matches per treated firm. We end up with 863 treated and 2,568 control firms.

Panel B in Table 5 shows the results. Columns 7 – 10 presents the summary statistics for the treatment and matched control firms. The summary statistics suggest that, at least for the variables we match on, the differences between treated and control firms are small. This is one indication that the matching approach performs well. Another indication is presented in Figure 1, Panel B. It shows the difference in strategic change behaviour between the treatment group and the matched control group over the five-year event window surrounding the exogenous coverage shock. As can be seen, the difference in the years leading up to the drop in coverage between both groups is stable, suggesting that there are no observable pre-trends. Column 11 of Table 5 displays the impact of the brokerage house disappearance events on strategic change. We obtain a DiD matching estimator that is similar, both in terms of economic magnitudes and statistical significance, to the results presented in Panel A. This provides further support for the notion that our results from the DiD estimation are not driven by cross-sectional heterogeneity between treatment and control groups.

Overall, this section has shown that an exogenous decrease in analyst coverage leads to an increase in strategic change initiatives. The evidence from the quasi-natural experiments thus suggest a negative causal effect of coverage on strategic change, consistent with *Hypothesis 1b*.[8]

[Insert Figure 1 Here]

[Insert Table 5 Here]

[8] For robustness purposes, we also considered a second identification approach which is based on an instrumental variable estimation. The instrument is expected coverage, introduced by Yu (2008), and captures the change of brokerage house size (e.g., see also He and Tian, 2013; Guo et al., 2019). Consistent with prior work, we find that the instrument is positive (p-value = 0.000). Moreover, the first-stage F-statistic of the excluded instrument is with 218 large and well above the rule of thumb for weak instruments, indicating that the instrument explains a substantial part of the variation in coverage. Consistent with the results reported above, the IV coefficient on analyst coverage remains negative (p-value = 0.000). Interestingly, the IV estimate is much larger (i.e., more negative) than the OLS estimate (-0.097 versus -0.045). This indicates that omitted time-variant factors simultaneously make firms more likely to engage in strategic change and more intensively covered by analysts.

4.2. Testing H2

4.2.1. Baseline results

Having obtained robust support for the hypothesis that an increase in analyst coverage leads to less strategic change, we next present the results for the empirical test of *Hypothesis 2*. This hypothesis attributes firms an active and important role: if analysts have difficulties in evaluating strategic changes, thereby pushing managers to adopt more stable strategies, we expect the negative effect of analyst coverage on strategic change to be mitigated when firm management is more transparent about those changes and supports analysts in their evaluations. In other words, financial analysts' coverage and firm transparency are complementary when it comes to complex firm initiatives, such as strategic change.

We test this hypothesis in two different ways. We begin by using the baseline specification from Section 4.1.1 and we estimate the following model:

$$\begin{aligned} Y_{i,t+1} = {} & \beta_1 Analyst\ coverage_{i,t} + \beta_2 Firm\ transparency_{i,t} + \beta_3 Analyst\ coverage_{i,t} \\ & \times Firm\ transparency_{i,t} + \gamma Z_{i,t} + \delta_t + \lambda_i + \varepsilon_{i,t} \end{aligned} \tag{7}$$

where $Firm\ transparency$ is measured as described in Section 3.2.4. The interaction term between $Analyst\ coverage$ and $Firm\ transparency$ is the key variable of interest that captures how a firm's information provision alters the marginal effect of analyst coverage on strategic change. We de-mean the variables in the interaction term to facilitate the interpretation of β_1 and β_2.

The results of our tests are shown in Table 6. To preview, we find evidence that is consistent with *Hypothesis 2*, for all the three different transparency measures we use. We first consider the G-index. Recall that a higher value of the G-index corresponds to *weaker* shareholder rights, whereas the findings in Irani and Oesch (2016) suggest that firm disclosure policies are close-to-optimal in the presence of strong shareholder rights (i.e., when the G-index is low). Column 1 reproduces our initial baseline model (column

4 in Table 2), including also the G-index. The coefficient of analyst coverage remains negative (p-value = 0.000). Column 2 introduces the interaction between analyst coverage and the G-index, and the parameter estimate is negative (p-value = 0.015). The economic effect is significant: while the marginal effect of analyst coverage on strategic change is -0.039 if a firm's G-index is at the sample mean, the marginal effect goes down to -0.026 if the firm's G-index decreases of one standard deviation (3.379), and this constitutes a 33% difference, i.e. a 33% increase in strategic change for the same level of coverage as the G-index decreases (and hence firm transparency increases) of one standard deviation below the mean.[9]

In column 3 and 4 of Table 6, we provide evidence from a more direct measure of firm transparency, i.e. reporting frequency, which is based on managerial earnings forecasts (e.g., Anilowski et al., 2007; Anantharaman and Zhang, 2011; Boone and White, 2015). In both columns, the coefficient of analyst coverage remains negative, consistent with our earlier findings. The coefficient of the interaction, β_3, is positive (p-value = 0.028), suggesting that the negative impact of analyst coverage on strategic change is mitigated when firms provide more guidance about their initiatives. To be concrete about the economic significance, if a firm's reporting frequency is at the sample mean, the marginal effect of analyst coverage on strategic change is -0.064; however, if the firm's reporting frequency is one standard deviation (0.673) above the mean, the marginal effect goes up to -0.032. This corresponds to a 50% difference.

Last but not least, in columns 5 and 6, we consider financial reporting quality (FRQ). Notice that a high value of FRQ indicates low reporting quality, whereas a lower value of the measure indicates an increase in reporting quality. Again, the coefficient on analyst coverage remains negative and with similar magnitude. In column 6, the parameter estimate of the interaction between analyst coverage and FRQ is negative (p-value = 0.028), implying that the negative impact of analyst coverage is less pronounced if firms provide better reporting, thereby supporting analysts (and other market participants)

[9] In unreported tests, we also considered the relationship between institutional ownership and firm disclosure behavior (see Boone and White, 2015). We find qualitatively similar results that the negative effect of analyst coverage on strategic change is weakened for firms with a larger share of firm equity is owned by institutional investors.

in their evaluations. Specifically, the coefficients imply that while the marginal effect of analyst coverage on strategic change is -0.042 if a firm's financial reporting quality is at the sample mean, the marginal effect increases to -0.035 if the firm's reporting quality is one standard deviation (0.158) above the mean (i.e. the FRQ measure decreases of one standard deviation), a 17% difference.

[Insert Table 6 Here]

4.2.2. Quasi-natural experiment

To address obvious endogeneity concerns, we also test *Hypothesis 2* using a DiD specification, using the same quasi-experimental setting previously described. We thus estimate the following model:

$$\begin{aligned} Y_{i,e,t+1} = {} & \beta_1 Post_{e,t} + \beta_2 Treated_{i,e} + \beta_3 Post_{e,t} \times Treated_{i,e} \\ & + \beta_4 Post_{e,t} \times Treated_{i,e} \times Transparent\ firm_{i,e} \\ & + \gamma Z_{i,t} + \delta_t + \lambda_i + \theta_e + \varepsilon_{i,t} \end{aligned} \tag{8}$$

where $Transparent\ firm$ is a dummy variable which equals to one if a given firm is classified as 'transparent' in the year prior to the event (i.e., year -1), and zero otherwise. The classification is defined below. Due to the nature of our test, we restrict attention to firms with *low* initial coverage, as discussed in Section 4.1.4.

Table 7 present the results from estimating Eq. (8) on this sub-sample. In column 1, we consider transparent firms those where the G-index is in the bottom quartile in the year prior to the merger/closure event. We obtain a DiD coefficient that is -0.313 (p-value = 0.004) for firms with strong shareholder rights and less entrenched management (and hence better disclosure policies). Notice once again that, in our setting, the shock is an exogenous *decrease* in coverage, hence the coefficient implies that for transparent firms, a sudden increase in coverage fosters strategic change. On the other hand, the basic DiD coefficient is positive (0.094) for the remaining firms (p-value = 0.186).

Column 2 reports results when we define transparent firms as those with reporting frequency in the year prior to the relevant event in the top quartile of the distribution.

The DiD coefficient for those firms firms is -0.334 (p-value = 0.016). For the group of firms that are less transparent, the DiD estimate is positive (p-value = 0.012). Overall, this pattern strongly suggests that the negative effect of analyst coverage on strategic change is only related to firms with medium and low transparency. For the most transparent firms, analyst coverage is actually beneficial, consistent with *Hypothesis 2*.

In our last empirical test, we use financial reporting quality (FRQ) to measure firm transparency. We classify firms as transparent if the reporting quality measure is in the bottom quartile in the year prior the event (recall that a high FRQ implies lower reporting quality and hence lower transparency). The results are presented in column 5, and are very similar to the ones with the reporting frequency measure. The DiD coefficient of analyst coverage on strategic change is -0.237 (p-value = 0.001) for firms with high-quality reporting, consistent with H2. Also the DiD coefficient is again positive in the case of firms with lower reporting quality (p-value = 0.031). Overall, Table 7 presents additional results consistent with *Hypothesis 2*.

[Insert Tables 7 Here]

5. DISCUSSION AND CONCLUSION

In this paper, we have studied the effect of financial analysts on firms' strategic change. Our baseline analysis revealed that, on average, firms covered by a larger number of analysts are less prone to change their strategies. The effect is robust to a battery of robustness tests, including a difference-in-differences approach to control for the endogeneity of analyst coverage. Our baseline result is consistent with the sizeable stream of literature that highlights how analysts are generally unwilling (or unable) to devote the time and effort needed to truly understand novel firm strategies (e.g., Zuckerman, 1999; Benner, 2010; Litov et al., 2012; Zenger, 2013; Brauer and Wiersema, 2018). Nevertheless, our study takes seriously the idea that analysts' ability to develop the necessary 'evaluative frame' (e.g., Beunza and Garud, 2007; Benner and Beunza, 2020) to make sense of these strategies heavily depends on the willingness of the firms to disclose all relevant information. Thus, in line with our theoretical predictions, we find that when firms are more transparent, the effect of analyst coverage on the likelihood of strategic change becomes positive.

Our paper contributes to the theoretical understanding of the influence of financial analysts on strategic decisions, by directly exploring the interplay between analysts and firms in this process. We depart from the two dominant perspectives on the role of analysts, which treat them as either perfectly rational actors or biased lemmings, and starting from the 'evaluative frame' perspective, we recognize that analyst's work goes beyond the production of forecasts but it is primarily about the development of frames that can help investors and other market actors better understand what the firm is doing (and will do) (e.g., Beunza and Garud, 2007; Benner and Beunza, 2020). While our study does not explore directly the role of these frames, our findings do lend support to this theoretical perspective. Furthermore, our contribution to advancing this perspective is to directly theorize the role that firms play in facilitating the information necessary for analysts to do their job. Our key theoretical contribution thus is to suggest that in order to understand how analyst shape strategic decision-making one cannot only theorize the work of analysts and their context, but needs to consider how firms themselves affect their work practices. This insight might be leveraged to revisit existing studies that explore the role of analysts in shaping specific strategic decisions, such as innovation, diversification, and merger and acquisitions. Our theory would suggest that when considering the moderating role of transparency, we might find that analysts are not necessarily such as a conservative force for firms.

Our theory also contributes to the debate on short-termism. Indeed, while the literature on earnings pressure has explored the role of analysts in short-term competitive moves (e.g., Zhang and Gimeno, 2010, 2016), we still "*know little of how analysts can influence managers' time horizons when it comes to long-term strategic investments*" (Brauer and Wiersema, 2018, p. 240). Our findings suggest that analysts (and thus more broadly financial markets) might indeed induce short-termism and inertia, but only for less transparent firms. It is thus possible for firms to engage in a different, more productive, collaboration with analysts and the market at large, but it does require them to open up more.

Unfortunately, we still know very little on the way in which firms decide to communicate their strategic decisions, and despite their increased centrality, investor relations departments have not been studied in-depth. Some preliminary evidence in accounting, consistent with our core findings, showed that financial analysts have less dispersed (or more consistent) EPS forecasts for firms with higher-rated investor relations programs than for firms with lower-rated programs (Farraghe et al., 1994). Bushee and Miller (2012) find that firms initiating investor relations programs exhibit greater increases in ownership by institutional investors and a shift toward investors that normally would not follow the firms. They also find greater improvements in analyst following, media coverage, and the book-to-price ratio. Still, more disclosure might also have competitive consequences, and more research is needed to explore whether firms are indeed better off by disclosing more.

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FIGURES

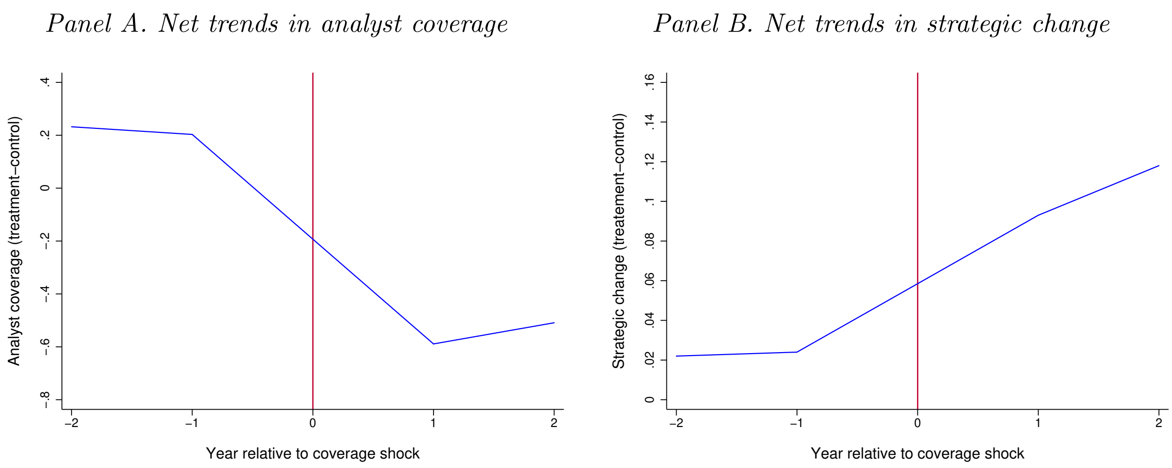

Fig. 1. Net trends in analyst coverage and strategic change (treatment-control group). This figure presents the trends in analyst coverage and strategic change for the treatment sample net of the matched control sample for the two years before and after the brokerage house disappearance events. The measures are normalized by the pre-event averages over treatment and control group.

TABLES

Table 1
Descriptive statistics.

Variable	Mean	Std. dev.	25^{th} percentile	Median	75^{th} percentile	Observations
Strategic change (Log)	-1.692	1.435	-2.475	-1.623	-0.840	39,584
Analyst coverage (Log)	1.641	1.016	0.799	1.740	2.454	39,584
Assets (Log)	6.340	1.923	4.918	6.243	7.631	39,584
Years included (Log)	2.945	0.686	2.398	2.944	3.555	39,584
ROA	0.036	0.161	0.012	0.046	0.085	39,584
Capex	0.058	0.059	0.022	0.041	0.073	39,584
K/L (Log)	3.910	1.445	3.016	3.653	4.503	39,584
Tobin's Q	1.798	1.264	1.095	1.428	2.052	39,584
H-Index	0.121	0.163	0.010	0.058	0.170	39,584
H-Index squared	0.041	0.116	0.000	0.003	0.029	39,584
Institutional ownership	0.554	0.289	0.320	0.586	0.788	39,584
Illiquidity (Log)	-4.460	0.621	-4.913	-4.521	-4.073	39,584
Return volatility	0.122	0.078	0.073	0.105	0.150	39,584
Governance index	7.241	3.379	4.000	7.000	10.000	19,570
Reporting frequency (Log)	1.130	0.673	0.693	1.386	1.609	7,447
Reporting quality	0.096	0.158	0.045	0.072	0.115	33,304

Table 2
Baseline results.
N = 39,584. Number of firms: 4,187. Robust standard errors are clustered by firm (in parentheses). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Estimation method: OLS Dependent variable: Strategic change (Log)	(1)	(2)	(3)	(4)
Analyst coverage (Log)	0.046***	-0.043***	-0.045***	
	(0.006)	(0.007)	(0.007)	
Low Coverage				-0.029***
				(0.010)
Medium Coverage				-0.062***
				(0.013)
High Coverage				-0.118***
				(0.017)
Assets (Log)			0.078***	0.078***
			(0.011)	(0.011)
Years included (Log)			-0.056**	-0.055**
			(0.026)	(0.027)
ROA			-0.024*	-0.024*
			(0.014)	(0.014)
Capex			-0.742***	-0.747***
			(0.095)	(0.096)
K/L (Log)			0.061***	0.061***
			(0.013)	(0.013)
Tobin's Q			-0.010***	-0.010***
			(0.003)	(0.003)
H-Index			-0.367**	-0.371**
			(0.168)	(0.168)
H-Index squared			0.255	0.265
			(0.162)	(0.162)
Institutional ownership			-0.111***	-0.120***
			(0.031)	(0.031)
Illiquidity (Log)			0.069***	0.070***
			(0.011)	(0.011)
Return volatility			0.295***	0.296***
			(0.050)	(0.050)
Firm FE	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Table 3
Alternative dependent variables: Different lag structures and strategic deviation.

Robust standard errors are clustered by firm (in parentheses). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Dependent variable	Panel A: Strategic change (Log)			Panel B: Strategic deviation (Log)			
Strategic dimensions	6	3	3	6	6	3	3
Time lag	$t+2$	$t+1$	$t+2$	$t+1$	$t+2$	$t+1$	$t+2$
Estimation method: OLS	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Analyst coverage (Log)	-0.051***	-0.047***	-0.050***	-0.064***	-0.058***	-0.064***	-0.048**
	(0.007)	(0.007)	(0.008)	(0.019)	(0.018)	(0.020)	(0.019)
Assets (Log)	0.076***	0.087***	0.085***	-0.058**	-0.064**	-0.062**	-0.081***
	(0.012)	(0.012)	(0.013)	(0.029)	(0.028)	(0.032)	(0.030)
Years included (Log)	-0.054*	-0.070**	-0.070**	-0.199**	-0.200***	-0.135	-0.216**
	(0.030)	(0.029)	(0.032)	(0.080)	(0.077)	(0.085)	(0.084)
ROA	-0.039**	-0.017	-0.034**	-0.062	-0.118	-0.046	-0.074
	(0.018)	(0.015)	(0.015)	(0.054)	(0.095)	(0.052)	(0.079)
Capex	-0.737***	-0.740***	-0.748***	-0.380*	-0.098	-0.175	-0.181
	(0.104)	(0.094)	(0.106)	(0.228)	(0.221)	(0.235)	(0.232)
K/L (Log)	0.071***	0.074***	0.086***	0.018	0.035	-0.021	0.029
	(0.013)	(0.013)	(0.014)	(0.031)	(0.030)	(0.032)	(0.031)
Tobin's Q	-0.011***	-0.009***	-0.011***	0.041***	0.034***	0.034***	0.027***
	(0.003)	(0.003)	(0.003)	(0.009)	(0.010)	(0.009)	(0.010)
H-Index	-0.325*	-0.332*	-0.381*	-0.956*	-0.919*	-1.210**	-0.965*
	(0.190)	(0.180)	(0.203)	(0.543)	(0.524)	(0.574)	(0.564)
H-Index squared	0.219	0.194	0.279	1.199**	1.128**	1.439**	1.180**
	(0.191)	(0.178)	(0.203)	(0.557)	(0.547)	(0.579)	(0.582)
Institutional ownership	-0.120***	-0.120***	-0.130***	0.095	0.064	0.095	0.056
	(0.034)	(0.033)	(0.036)	(0.080)	(0.078)	(0.083)	(0.082)
Illiquidity (Log)	0.077***	0.076***	0.081***	0.049	0.035	0.047	0.045
	(0.012)	(0.012)	(0.013)	(0.032)	(0.031)	(0.032)	(0.033)
Return volatility	0.324***	0.327***	0.362***	0.466***	0.424***	0.353***	0.267**
	(0.055)	(0.051)	(0.058)	(0.128)	(0.117)	(0.127)	(0.121)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	31,843	39,584	31,843	39,584	31,843	39,584	31,843
Firms	3,541	4,187	3,541	4,187	3,541	4,187	3,541

Table 4
Miscellaneous robustness tests.
Robust standard errors are clustered by firm (in parentheses). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Sample Estimation method: OLS Dependent variable: Strategic change (Log)	S&P 500 (1)	Excluding crisis (2)	Positive coverage (3)	Residual coverage (4)
Analyst coverage (Log)	-0.037***	-0.053***	-0.087***	
	(0.011)	(0.009)	(0.012)	
Residual coverage				-0.039***
				(0.007)
Assets (Log)	-0.032	0.069***	0.079***	0.059***
	(0.027)	(0.014)	(0.013)	(0.011)
Years included (Log)	-0.027	-0.040	-0.058**	-0.082***
	(0.084)	(0.035)	(0.028)	(0.031)
ROA	-0.119	-0.003	-0.053*	-0.039
	(0.076)	(0.013)	(0.031)	(0.027)
Capex	-0.662**	-0.782***	-0.724***	-0.747***
	(0.290)	(0.146)	(0.109)	(0.092)
K/L (Log)	-0.000	0.063***	0.048***	0.057***
	(0.033)	(0.016)	(0.014)	(0.014)
Tobin's Q	-0.010**	-0.013**	-0.006**	-0.010***
	(0.005)	(0.006)	(0.003)	(0.003)
H-Index	-0.258	-0.469**	-0.387**	-0.394**
	(0.319)	(0.202)	(0.183)	(0.172)
H-Index squared	0.184	0.377*	0.258	0.306*
	(0.271)	(0.195)	(0.171)	(0.170)
Institutional ownership	0.110	-0.105***	-0.096***	-0.102***
	(0.078)	(0.038)	(0.034)	(0.033)
Illiquidity (Log)	0.029	0.074***	0.056***	0.073***
	(0.033)	(0.015)	(0.012)	(0.012)
Return volatility	0.416**	0.343***	0.314***	0.325***
	(0.199)	(0.082)	(0.060)	(0.053)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
N	7,146	23,156	33,260	39,584
Firms	663	3,864	3,884	4,187

Table 5
Controlling for endogeneity: Difference-in-differences estimation.
Robust standard errors are clustered by firm (in parentheses). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Panel A: Basic matching						Panel C: Matched sample				
Sample	Full	Full	Full	Full	Low Initial Coverage	High Initial Coverage	Full	Full	Full	Full	Full
Dependent variable	Analyst coverage	Strategic Change (Log)	Strategic Change (Log)	Strategic Change (Log)	Strategic Change (Log)	Strategic Change (Log)	Mean Treated	Mean Control	Mean Diff.	*t*-value	Strategic Change (Log)
Estimation method: OLS	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Treated × Post	-1.224***	0.054***	0.050***	0.051***	0.104**	-0.002					0.052**
	(0.232)	(0.019)	(0.018)	(0.019)	(0.044)	(0.017)					(0.024)
Matching variables:											
Assets (Log)							7.827	7.735	0.092	1.21	
ROA							0.015	0.018	-0.003	-0.35	
Tobin's Q							2.118	2.194	-0.076	-0.77	
Return volatility							0.131	0.126	0.005	1.04	
Analyst coverage							11.684	11.779	-0.095	-0.37	
Controls	No	No	Yes	Yes	Yes	Yes					No
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes					Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes					Yes
Event FE	No	No	No	Yes	Yes	Yes					Yes
N	97,644	97,644	97,644	97,644	48,848	48,796	863	2,568			13,672
Firms	1,896	1,896	1,896	1,896	974	1,420	724	1,304			2,027

Table 6
Baseline results: Allowing the analyst coverage effect to vary with firm transparency.
Robust standard errors are clustered by firm (in parentheses). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Estimation method: OLS Dependent variable: Strategic change (Log)	(1)	(2)	(3)	(4)	(5)	(6)
Analyst coverage (Log)		-0.004**				
× Governance index		(0.002)				
Analyst coverage (Log)				0.047**		
× Reporting frequency (Log)				(0.021)		
Analyst coverage (Log)						-0.043**
× Reporting quality						(0.021)
Governance index	0.005	0.008**				
	(0.003)	(0.004)				
Earnings Guidance (Log)			0.003	-0.027*		
			(0.013)	(0.014)		
Reporting quality					-0.006	0.026
					(0.015)	(0.022)
Analyst coverage (Log)	-0.041***	-0.039***	-0.056**	-0.064***	-0.046***	-0.042***
	(0.009)	(0.009)	(0.024)	(0.024)	(0.007)	(0.007)
Assets (Log)	0.029*	0.026	0.025	0.026	0.072***	0.072***
	(0.017)	(0.017)	(0.027)	(0.027)	(0.011)	(0.011)
Years included (Log)	-0.051	-0.048	0.107	0.111	-0.044	-0.045
	(0.043)	(0.043)	(0.075)	(0.075)	(0.028)	(0.028)
ROA	-0.094**	-0.097**	-0.055	-0.055	-0.012	-0.036
	(0.040)	(0.040)	(0.055)	(0.055)	(0.016)	(0.022)
Capex	-0.748***	-0.746***	-1.063***	-1.071***	-0.682***	-0.676***
	(0.156)	(0.157)	(0.192)	(0.193)	(0.101)	(0.101)
K/L (Log)	0.016	0.016	0.029	0.027	0.054***	0.053***
	(0.019)	(0.019)	(0.025)	(0.025)	(0.014)	(0.014)
Tobin's Q	-0.015***	-0.014***	-0.013*	-0.013*	-0.009***	-0.009***
	(0.005)	(0.005)	(0.008)	(0.008)	(0.003)	(0.003)
H-Index	-0.406*	-0.395*	-0.825**	-0.811**	-0.353*	-0.355*
	(0.229)	(0.228)	(0.412)	(0.409)	(0.187)	(0.187)
H-Index squared	0.247	0.240	0.644*	0.635	0.294	0.295
	(0.207)	(0.205)	(0.390)	(0.391)	(0.201)	(0.201)
Institutional ownership	-0.017	-0.015	0.049	0.055	-0.101***	-0.100***
	(0.042)	(0.041)	(0.093)	(0.094)	(0.033)	(0.033)
Illiquidity (Log)	0.043**	0.044**	0.054	0.055*	0.074***	0.073***
	(0.018)	(0.018)	(0.034)	(0.034)	(0.012)	(0.012)
Return volatility	0.260***	0.266***	0.231*	0.221	0.271***	0.271***
	(0.088)	(0.089)	(0.140)	(0.139)	(0.049)	(0.049)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	19,570	19,570	7,447	7,447	33,304	33,304
Firms	2,121	2,121	1,518	1,518	4,076	4,076

Table 7
Difference-in-differences estimation: Allowing the analyst coverage effect to vary with firm transparency.

Robust standard errors are clustered by firm (in parentheses). For reasons discussed in Section 4.1.4, the sample is restricted to firms with *low* initial coverage. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Estimation method: OLS Dependent variable: Strategic change (Log)	(1)	(2)	(3)
Treated × Post × 1	-0.313***		
(Low governance index)	(0.107)		
Treated × Post × 1		-0.334**	
(High reporting frequency)		(0.138)	
Treated × Post × 1			-0.237***
(Good reporting quality)			(0.072)
Treated × Post	0.094	0.192**	0.132**
	(0.071)	(0.076)	(0.061)
Controls	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Event FE	Yes	Yes	Yes
N	18,980	22,588	40,760
Firms	394	638	934