Analyst coverage and the quality of corporate investment decisions*

Thomas Y. To
University of Sydney
thomas.to@sydney.edu.au

Marco Navone
University of Technology Sydney
marco.navone@uts.edu.au

Eliza Wu†
University of Sydney
eliza.wu@sydney.edu.au

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†Corresponding author. Tel.: +61-2-86274626. Address: University of Sydney, NSW 2006, Australia.

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ABSTRACT

In this paper we examine the effect of financial analysts on the quality of corporate investment decisions. We show that greater analyst coverage leads to higher total factor productivity, a finding that is robust after using both an instrumental variable approach and an experimental design that exploits exogenous reductions in coverage due to broker mergers and closures. We further show that the positive effect of analysts on productivity occurs only in opaque firms and firms with weaker investor protection suggesting that analysts improve investment decisions by playing an important role in information distribution and external monitoring.

JEL classification: G31, G32, G34.

Keywords: equity analysts, productivity, information asymmetry, corporate governance, external finance, corporate investment.

1. Introduction

Financial analysts produce and disseminate firm-specific information across financial markets based on their timely access to information, private knowledge and analytical skills. As such, scholars have found that they help to reduce information asymmetries between corporate insiders (management) and outsiders (investors)(Lys and Sohn, 1990; Brennan and Subrahmanyam, 1995; Frankel and Li, 2004).

While some authors argue that analysts' role in reducing information asymmetry allows firms to have better access to external financing and be able to pursue more aggressive investment policies (Doukas, Kim, and Pantzalis, 2008; Armstrong et al., 2011; Derrien and Kecskés, 2013), others have pointed to a darker side of financial analysts: by focusing investors' attention on quarterly profits they may force managers to forego valuable long-term investments, such as R&D, in order to meet the short-term expectations (Graham, Harvey, and Rajgopal, 2005; He and Tian, 2013; Irani and Oesch, 2016).

Although both camps provide compelling arguments and show robust empirical evidence, the effect of analyst coverage on the quality of corporate investment decisions remains an open empirical question: while an increase in external financing can be valuable it does not automatically translate into good investment decisions, and while the foregone innovative projects may be critical for firms' long-term growth the effect is very difficult to measure since the vast majority of firms do not file patents.

In this paper we take a closer look at the effect of financial analysts on the quality of corporate investments by analyzing their impact on the efficiency of firms' investment decisions as proxied by total factor productivity (TFP). With this approach, rather than looking at the financing or size of firm investments we focus our attention directly on the quality of investment decisions as gauged by firms' efficiency gains.[1]

Our main hypothesis is that firms followed by a larger number of analysts should exhibit higher total factor productivity, and the causal effect comes from two possible channels: on the one hand by providing information about firms' growth opportunities to outsiders analysts can help firms to

[1]We know of two other papers that study financial analysts and firm efficiency. Biddle, Hilary, and Verdi (2009) find a positive association between analyst coverage and over-investment, whereas Chen, Xie, and Zhang (2017) show that analysts' forecast quality is positively related to investment efficiency. Both papers do not examine labor and capital efficiency combined or address the endogeneity of analyst coverage with natural experiments like we do.

obtain the "optimal" amount of external finance to invest in capital and labor in order to undertake productive projects (Doukas et al., 2008; Derrien and Kecskés, 2013), whilst on the other hand due to analysts' ability to reveal negative information they can act as external monitors and ensure that managers will undertake the most productive projects (Yu, 2008; Irani and Oesch, 2013). It is important to note that our main hypothesis, which we term the "*information hypothesis*", differs from the focus of Derrien and Kecskés (2013) in that we do not postulate a positive relationship between analyst coverage and the *size* of corporate investments but rather for the *quality* of corporate investments, as proxied by productivity gains. This is an important distinction as more is not necessarily better in the case of corporate investments. Our measure, TFP, is a particularly appropriate measure to capture this distinction since it is calculated as the difference between expected output, given the inputs used in production, and the actual output produced by the firm. Therefore gains in TFP cannot be simply the result of an increase in the size of corporate investments.

We also formulate an alternative hypothesis, our take on the dark side of analyst coverage, where corporate executives are pressured into pursuing suboptimal investment strategies in order to meet / exceed short term earnings expectations. Here we expect a negative relation between analyst coverage and total factor productivity. Again, it is important to notice that this competing "*managerial pressure hypothesis*" differs from the prior work of He and Tian (2013) and Irani and Oesch (2016) in that we do not focus on a specific driver of long term growth (investment in innovation) but rather on the overall investment policy of the firm.

We test these two competing hypotheses by empirically examining the relation between analyst coverage and firm-level total factor productivity (TFP) with an extensive sample of U.S. listed firms covered by Compustat from 1991 to 2013.[2] Controlling for firm characteristics, our baseline regression results indicate a positive relation between analyst coverage and firm productivity and this lends greater support to the information hypothesis.

However, a major challenge for our study is the potential endogeneity of analyst coverage. Unobservable firm heterogeneity correlated with both analyst coverage and firm productivity could bias the results (i.e., the omitted variable concern), and firms with higher productivity potential could attract more analyst coverage (i.e., the reverse causality concern). Hence, to establish causality, we

[2]TFP measures have also been used in studies by Schoar (2002), Maksimovic, Phillips, and Prabhala (2011), Chemmanur, Krishnan, and Nandy (2011, 2014), and Krishnan, Nandy, and Puri (2015).

use two identification strategies and perform an array of robustness tests.

Following Yu (2008), we first adopt a measure of expected coverage based on the time-varying size of brokerage firms as an instrumental variable in a two-stage least squares model to address the potential endogeneity problem of coverage decisions. The positive effect of analyst coverage on firm productivity remains robust to the use of this instrument.

Our second approach relies on two quasi-natural experiments based on brokerage mergers (Hong and Kacperczyk, 2010) and brokerage closures (Kelly and Ljungqvist, 2012), to address the potential endogeneity problem.[3]For our purpose, these events directly affect firms' analyst coverage but are exogenous with respect to firms' productivity. Using a difference-in-differences approach, we show that an exogenous decrease in analyst coverage leads to a larger relative decrease in firm productivity for the treatment group (i.e. firms that experience a reduction in analyst coverage due to the broker disappearances) compared to the control group (i.e., similar firms not affected by the treatment), and the effect is stronger for firms covered by fewer analysts. Overall, our two identification tests unambiguously indicate that analyst coverage significantly enhances firm productivity, and our results are robust to using an alternative construct of productivity and excluding economic downturn periods.

In the final part of our paper, we attempt to identify the channels through which financial analysts improve firm productivity. First, we test for analysts' role in providing firms with the optimal amount of external finance. We find that the positive effect of analyst coverage on productivity occurs in firms with a higher level of information asymmetry between management and outsiders, i.e., smaller firms, younger firms, and firms with a higher level of intangible assets. This result is not surprising since these firms are harder to analyze and therefore investors are likely to either provide the firms with too little or too much external capital without analysts' independent assessment of firms' growth opportunities, which ultimately leads to either firm over-investment or under-investment. Second, we test for analysts' role as external monitors. We expect the external monitoring role of analysts to be particularly important in ensuring managers' commitment to shareholder value creation in firms where investor protection is relatively weak, as internal gov-

[3]These quasi-natural experiments have been used extensively in the literature studying analyst coverage and analyst reporting bias (Hong and Kacperczyk, 2010), firm valuation (Kelly and Ljungqvist, 2012), corporate governance (Irani and Oesch, 2013; Chen, Harford, and Lin, 2015), cost of capital (Derrien and Kecskés, 2013), firm innovation (He and Tian, 2013), and stock liquidity (Balakrishnan et al., 2014).

ernance mechanisms and threats from the market for corporate control are not sufficient to stop managers from increasing perquisite consumption in these firms. Consistent with our expectations, we find that the positive effect of analyst coverage on productivity occurs in firms where executives are shielded by stronger anti-takeover provisions or where powerful CEOs have gained a strong control on the board of directors.

To the best of our knowledge, this is the first study to examine the causal effect of analyst coverage on firm-level productivity. While other studies have provided causal evidence that analysts help in providing firms with a better access to external financing (Derrien and Kecskés, 2013) and in disciplining managers (Irani and Oesch, 2013; Chen et al., 2015), we find that both of these roles of analysts are responsible in improving a firm's level of productivity.

Our study also contributes to the literature on finance and productivity. Prior literature documents that access to early-stage financing (such as venture capital and angel financing) improves the productivity of young and small firms (Chemmanur et al., 2011; Kerr, Lerner, and Schoar, 2014). In this vein, it has also been shown that productivity of domestic firms benefit from foreign direct investment (Aitken and Harrison, 1999; Javorcik, 2004) and access to financing improves agricultural productivity (Butler and Cornaggia, 2011). However, there have been relatively fewer studies investigating how improved access to mainstream financing within the U.S. affects firms' productivity. Our study is closest in spirit to Krishnan et al. (2015), as they find that an increased access to bank financing improves productivity for small manufacturing firms. While in their paper a lack of access to capital markets due to extreme information asymmetries is remedied by increased access to bank financing, in our paper we focus on the role of information production and dissemination by financial analysts in stimulating financing of productive investments.

The rest of the paper is organized as follows. Section 2 develops the main hypotheses. Section 3 describes the data and sample selection and explains the construction of various variables used in this study. Section 4 presents the baseline results. Section 5 addresses the identification issues. Section 6 analyzes the mechanisms through which financial analysts work to improve investment decisions, and section 7 concludes.

2. Hypothesis development

2.1. Financial analysts and information dissemination

As an important information intermediary in stock markets, financial analysts have the opportunity to interact directly with management on a consistent basis in order to formulate views about firm prospects, and to express their views about through research reports to their clients and through appearances in public media. Focusing on the information dissemination role of analysts, scholars have provided empirical evidence to show that analyst coverage reduces information asymmetry Lys and Sohn (1990); Brennan and Subrahmanyam (1995); Frankel and Li (2004).

Recent studies highlight that the ability of analysts to disseminate information to outsiders allows them to perform two roles in corporations. First, it has been shown that analyst coverage can affect the amount of external finance a firm is able to obtain (Chang, Dasgupta, and Hilary, 2006; Doukas et al., 2008; Derrien and Kecskés, 2013). The rationale comes from the adverse selection problem first introduced by Jensen and Meckling (1976). Due to the separation of ownership and control, managers are likely to have a better knowledge about firm's true value than outsiders and therefore may have the incentive to issue capital when the firm is overpriced. If they are successful in raising funds, the excess capital would enable managers to hire excessive workers, purchase excessive capital, and to build their own empire at the cost of shareholders Jensen (1986). However, outsiders may also detect such behaviors and respond by not providing external financing to the firm, and this can be a pervasive effect that affects all firms including those that do not issue overpriced capital as investors start to price private information Easley and O'hara (2004). We hypothesize that by providing an independent assessment of firm prospects, analysts would enable firms to obtain the "optimal" amount of external capital in order to hire workers and invest in capital to undertake productive projects.

Second, it has been documented that the ability of analysts to discover and reveal managerial misbehavior enables them to act as external monitors to managers. For example, Yu (2008) and Irani and Oesch (2013) find that firms covered by more analysts engage in less earnings management because managers are afraid that analysts would discover and reveal to the public that the firm's earnings are inflated. Similarly, Chen et al. (2015) finds that CEOs receive less excess compensation in firms covered by more analysts. Consistent with the findings of these prior studies, we hypothesize that by acting as external monitors analysts would force managers to invest in the most productive projects.

Overall, our main hypothesis, the *information hypothesis*, predicts that financial analysts should improve firm productivity by providing firms with the "optimal" amount of external capital to avoid over-investment or under-investment and to act as external monitors to force managers to invest in the most productive projects. To summarize, our main hypothesis is:

H1 (Information hypothesis): Higher analyst coverage increases firm productivity.

2.2. Financial analysts and managerial pressure

One of the main negative impacts of financial analysts discussed in the literature is the excessive pressure that they impose on corporate managers to beat short term earnings expectations. Analysts' forecasts are generally overly optimistic, which make them very difficult to meet (Dechow, Hutton, and Sloan, 2000; Ertimur, Muslu, and Zhang, 2011). Empirically, it has been shown that beating forecasts increases short-term returns (Bartov, Givoly, and Hayn, 2002; Bhojraj et al., 2009),while missing forecasts by even a small margin can lead to a reduction in bonuses for the CEO (Matsunaga and Park, 2001) and a decrease in stock prices for firms (Skinner and Sloan, 2002). The survey of 401 U.S. Chief Financial Officers (CFOs) conducted by Graham et al. (2005) confirms the importance of meeting analysts' forecasts as they find that a majority of the executives would forego a project with positive net present value (NPV) if the project would cause them to fall short of the current quarter consensus forecast, with 80% of the executives suggesting that they would decrease discretionary spending, including R&D and advertising expense in order to meet the earnings benchmarks. Empirically, Irani and Oesch (2016) show that higher analyst coverage leads to real earnings management performed predominantly via the offering of price discounts to temporarily increase sales, overproduction to report lower cost of goods sold, and reduction of discretionary expenditures to improve reported margins. Along the same line of inquiry He and Tian (2013) find that firms covered by a larger number of analysts invest less in innovation. Overall, the findings of these studies collectively suggest that analyst coverage provides a strong incentive for corporate managers to behave myopically. Since long term productive investments in product and process innovation and human capital development do not yield short term results, our alternative hypothesis, the *managerial pressure hypothesis*, predicts that analyst coverage would impede firm productivity as the need to beat earnings expectations forces managers to delay (or outright forego) long term productive projects. This view is summarized by Jensen (2005), who writes that *"when real operating decisions that would maximize value are compromised to meet market expectations, real long-term value is being destroyed"* (p.8). Hence, we also access the following competing hypothesis:

H2: (Managerial pressure hypothesis): Higher analyst coverage impedes firm productivity.

3. Sample selection and firm productivity

This section describes our sample and explains the measurement of firm productivity.

3.1. Sample Selection

We obtain firm-specific financial variables from Compustat and analyst information from the Institutional Brokers Estimate System (I/B/E/S) database. Following Imrohoroglu and Tüzel (2014), we omit foreign firms, financial firms, and regulated firms from our sample as they suggest that these firms face different productivity constraints. We also exclude firms that have missing values for total assets, sales, number of employees, gross property, plant, and equipment, depreciation, accumulated depreciation, or capital expenditures, as these are the variables required for our TFP estimation. The final sample used to investigate the relation between analyst coverage and one-year-ahead firm productivity consists of 35,280 firm-year observations between 1991 and 2013. The reason for starting the sample in 1991 is because the I/B/E/S recommendation file used to identify brokerage mergers and closures starts in 1994, and we need to observe a three-year trend before the brokerage mergers or closure events to ensure that the parallel trend assumption is satisfied for our DiD test to be valid.

3.2. Measuring Firm Productivity

We follow the literature closely in constructing TFP measures to capture firm productivity. The key inputs for estimating TFP are output, capital, and employment. We use data from Compustat, investment and output deflators from the Bureau of Economic Analysis, and wage data from the

Social Security Administration. Following Imrohoroglu and Tüzel (2014), Output (y_{it}) is measured using Sales minus materials used in production, deflated by the output deflator; capital stock (k_{it}) is measured using Property, Plant and Equipment, deflated following Hall (1990), and labor (l_{it}) is measured using the total number of employees.[4]

We employ the production function given in:

$$y_{it} = \beta_0 + \beta_k k_{it} + \beta_l l_{it} + \omega_{it} + \epsilon_{it} \tag{1}$$

where y_{it} is the log of output estimated for firm i in period t, ω_{it} is the level of productivity known to the firm and correlated with the inputs and ϵ_{it} is the error term. From this model, the logarithm of firm-level total factor productivity is estimated as the difference between the actual and predicted output, i.e. $TFP_{it} = y_{it} - \hat{y}_{it}$.

However, using this baseline equation to estimate TFP presents endogeneity and selection problems. Firstly, firm productivity is correlated with its input decisions. More productive firms tend to put in more capital and labor due to higher current and anticipated future investment opportunities. Secondly, large firms can continue to operate at a lower level of productivity as they expect higher investment returns, whereas small firms may have to exit the market if they operate at a low level of productivity.

Hence, we employ the semi-parametric procedure suggested by Olley and Pakes (1996) to estimate the parameters in this production function. This approach explicitly addresses the simultaneity and selection biases involved in the estimation of production functions by using a "proxy method" where one uses firms' investment decisions to construct proxies for their unobserved productivity parameters (see Appendix A for details).

Once we estimate the production function parameters ($\hat{\beta}_0$, $\hat{\beta}_l$, and $\hat{\beta}_k$) using the approach suggested by Olley and Pakes (1996), we obtain the logarithm of firm-level total factor productivity estimates by:

[4] We follow Imrohoroglu and Tüzel (2014) and use sales minus materials to proxy for output rather than using sales alone.The underlying economic rationale comes from Foster, Haltiwanger, and Syverson (2008) who show that firm revenue usually confounds the effects of idiosyncratic demand and factor prices with efficiency differences. That is, firms can have high revenue levels because they are efficient, but it can also be driven by high input prices. Detailed variable definitions are provided in Appendix A

$$TFP_{it} = y_{it} - \hat{\beta}_0 - \hat{\beta}_k k_{it} - \hat{\beta} l_{it} \tag{2}$$

We use industry-specific time dummies in the estimation to control for both within industry biases and changes in TFP over time. In order to gauge the accuracy of our TFP measures we compare our production function estimates with the estimates reported in previous studies in the literature, and confirm that our production function estimates are consistent with those previously reported by Olley and Pakes (1996) and Imrohoroglu and Tüzel (2014).

3.3. Measuring analyst coverage and other control variables

We obtain analyst information from the I/B/E/S database. Analyst coverage is calculated as the total number of unique analysts issuing earnings forecasts for a firm during the 12-month period before its fiscal year end. We then take the natural logarithm of this raw measure to construct our main measure of analyst coverage (*LnCoverage*). Guided by recent findings on the determinants of firm-level TFP (Imrohoroglu and Tüzel, 2014; Krishnan et al., 2015; Li, 2015), we control for a vector of firm characteristics that could potentially affect a firm's future TFP. The controls include firm size (the natural logarithm of total assets), book-to-market ratio, firm profitability (return on assets), capital expenditures, asset growth, cash flow, and leverage. We provide detailed variable definitions in Table 1.

[Place Table 1 about here]

[Place Table 2 about here]

To minimize the influence of outliers, we winsorize our independent variables at the first and 99th percentile. Table 2 provides the summary statistics. Panel A shows that on average, a firm in our sample has a TFP of -0.344 (which is similar to that reported in Imrohoroglu and Tüzel (2014)) and is followed by 11 analysts. Regarding other variables, an average firm has book value assets of 2.94 billion dollars, leverage of 21.6%, a book-to-market ratio of 0.548%, and ROA of 14.6%, which are similar to those reported in prior analyst studies (Hong and Kacperczyk, 2010; He and Tian, 2013).

Panel B of Table 2 reports firm characteristics sorted by the level of analyst coverage. First, we notice that firms with a higher level of productivity are covered by more analysts, which is consistent, prima facie, with our information hypothesis. Second, firms with a greater number of analysts following are larger, more profitable, have lower book-to-market ratio and higher cash flows and capital expenditures. While higher cash flows and capital expenditures are consistent with analysts improving access to external finance, this evidence is also compatible with the notion of endogeneity of coverage decisions: analysts prefer to cover large and profitable firms where higher trading fees can be generated for the brokerage house (Bhushan, 1989; McNichols and O'Brien, 1997).

4. Baseline empirical results

To assess how analyst coverage affects firm productivity, we estimate the following model:

$$TFP_{i,t+n} = \alpha + \beta LnCoverage_{it} + \gamma Controls_{it} + Year_t + Industry_k + \epsilon_{it} \tag{3}$$

where t denotes year, i denotes firm, k denotes industries and n equals one, two, or three years ahead. The dependent variable, $TFP_{i,t+n}$, is the productivity for firm i. Since it generally takes time for a firm's productivity to change, we examine the effect of analyst coverage on a firm's productivity up to three years ahead. The analyst coverage measure, $LnCoverage_{it}$, is measured for firm i over its fiscal year t. $Controls_{it}$ is a vector of firm-specific characteristics that could affect productivity. $Year_t$ and $Industry_k$ capture time and industry fixed effects respectively. We cluster standard errors at the firm level.

[Place Table 3 about here]

We start with a parsimonious model that regresses TFP one year ahead only on our key variable of interest, *LnCoverage*, and then add year and industry fixed effects. Results in columns (1) and (2) of Table 3 show that analyst coverage has a positive and significant impact on firm-level productivity. In column (3), we add a set of controls that are important determinants of TFP. The coefficient estimate of analyst coverage remains positive and statistically significant at the 1% level. In terms of the magnitudes, the result suggests that an increase in one standard deviation of *LnCoverage* would increase TFP by 9.4%. Thus, the effect we document is not only statistically but also economically significant. In columns (4) and (5), we consider TFP two and three years ahead, respectively. Estimates of the main coefficient of interest continue to be positive and significant, suggesting that even after controlling for the time-dimension of productivity changes analyst coverage is still associated with higher levels of firm productivity. Overall, our baseline results seem to lend support to the information hypothesis.

5. Identification

A major concern with this baseline estimation is the potential endogeneity of analyst coverage. One could easily argue that analysts may prefer to cover "successful" firms or that some unobserved measure of investment opportunities may affect both coverage decisions and current productivity.

In this section we adopt two different identification strategies to establish causality. We first implement a 2SLS estimation with an instrumental variable for coverage based on exogenous changes in the size of brokerage firms. Our second identification strategy, instead, uses a difference-in-differences (DID) estimation based on two quasi-natural experiments where a number of "treated" firms experience an exogenous drop in analyst coverage due to mergers and closures of brokerage houses.

5.1. Instrumental variable approach

Our first identification strategy to address the endogeneity problem of analyst coverage is to exploit changes in the size of brokerage houses in order to build a valid instrument for analyst coverage. This measure of "expected coverage" was first introduced by Yu (2008) and he argues that the size of brokerage houses (in terms of the number of analysts employed) changes over time, usually depending on revenues or profits. These changes affect coverage decisions but are, at the same time, unrelated to the characteristics of the firms covered by the brokerage house. Hence, this instrument captures the variation in analyst coverage that is exogenous to a firm's productivity.

Following Yu (2008), we use the following equations to calculate expected coverage:

$$ExpectedCoverage_{itj} = \frac{Brokersize_{jt}}{Brokersize_{j0}} * Coverage_{i0j} \tag{4a}$$

$$ExpectedCoverage_{it} = \sum_{j=1}^{n} ExpectedCoverage_{itj} \tag{4b}$$

where $ExpectedCoverage_{itj}$ is the expected coverage of firm i from broker j in year t. $Brokersize_{j0}$ and $Brokersize_{jt}$ are the number of analysts employed by broker j in the benchmark year and year t respectively. $Coverage_{i0j}$ is the number of analysts following firm i from broker j in the benchmark year 0. Finally $ExpectedCoverage_{it}$ is the expected coverage for firm i in year t.

In the spirit of Yu (2008), we use year 2002, the middle year of our sample period, as the benchmark year. We require a firm to be followed by at least one analyst in the benchmark year. A possible concern for the validity of this instrument is that a broker's choice of which firms to stop covering could introduce a potential selection bias problem. However, as Yu (2008) points out, the selection issue affects only the realized coverage but not the expected coverage, because the expected coverage measures the tendency to maintain the coverage before the broker decides which firms to actually keep.

[Place Table 4 about here]

Column (1) of Table 4 shows the result of the first-stage regression using *LnCoverage* as the dependent variable. The result confirms that, "Expected coverage" is positively correlated with realized coverage (the instrumented variable). From column (2), we examine the effect of (instrumented) analyst coverage on firm productivity at different time-horizons. All the models confirm our basic finding showing an unambiguously positive, and highly significant, relationship between analyst coverage and firm productivity.

5.2. Quasi-natural experiments

Our second identification strategy uses two quasi-natural experiments based on exogenous shocks to analyst coverage. The first experiment, pioneered by Hong and Kacperczyk (2010), relies on the assumption that two merging brokerage houses which were both covering the same stock will likely drop one of their analysts after the merger. The authors of the original study argue that the coverage termination is not a decision made by the analysts and is also not related to the characteristics of the firms covered. The second experiment, similar in spirit and first adopted in Kelly and Ljungqvist (2012), relies on the fact that brokerage firms sometimes respond to unfavorable changes in revenues and profitability by closing their research operations. Similar to the previous experiment, the authors argue that the resultant coverage termination is not a decision made by individual analysts and is also not related to the characteristics of the firms covered. Therefore, brokerage mergers and closures capture exogenous variations in analyst coverage that can be exploited to quantify the impact of analyst coverage on firm productivity.[5].

We focus on brokerage mergers and closures that occur between 1994 to 2010 (given that our sample period is from 1991 to 2013 and we need to observe a three-year trend before and after the broker disappearances). Our list of broker disappearances includes all of the closures and 13 out of the 15 mergers considered in the aforementioned seminal papers. We lose two merger events because they fall outside our sample period.[6]

In order to implement our identification strategy, we must calculate the change in our outcome variable (productivity) around the time of the broker disappearance. Since productivity is estimated on yearly data and the closure/merger event can happen anytime during a given calendar year we follow other studies using a similar empirical setup (He and Tian, 2013; Chen et al., 2015; Irani and Oesch, 2016), and define the change in the outcome variable as the difference between the values in $t + 1$ and $t - 1$ where time t indicates the year of the broker merger or closure.

We follow Hong and Kacperczyk (2010) and Kelly and Ljungqvist (2012) in constructing a sample of treatment firms that are covered by broker houses that either merged or closed and then

[5]One may argue that the analysts who lose their job after brokerage disappearances are of lower quality and do not produce relevant information First of all, it is important to notice that this bias would lead to a lack of measurable impact on firm productivity, hence would go against our hypothesis. Secondly, Derrien and Kecskés (2013), using a similar set of brokerage events, show that the analysts dropped do not produce, on average, lower quality research.

[6]For a detailed description of the selection process, see Derrien and Kecskés (2013) and Chen et al. (2015). We thank Francois Derrien and Jarrad Harford for sharing the list of brokerage disappearances with us.

lost an analyst. For brokerage mergers, we identify firms covered by both the target broker and the acquirer broker during the year before the merger and for which one of their analysts disappears during the year after the broker merger date (by not issuing earnings forecasts). For brokerage closures, we identify firms for which the analyst disappears from I/B/E/S during the year after the broker closure date. These procedures ensure that the loss of analyst coverage for these firms is definitely due to the brokerage disappearances and not for other unrelated reasons. For a firm to be classified into our treatment or control group, we also need it to have non-missing matching variables in the matching year (year $t-1$), and non-missing TFP data from year $t-1$ to year $t+3$. We end up with a sample of 1,919 treated firms.

We construct the control group of firms that are matched to the treatment group on important observable characteristics one year prior to the brokerage events to ensure that the difference in productivity changes between the treatment and control firms is not caused by their cross-sectional heterogeneity.

In order to produce a more robust test of our two competing hypotheses we produce four different control samples. For our main matching strategy we follow that employed by Derrien and Kecskés (2013). For every firm in our treatment sample we consider candidate control firms in the same Fama-French 49 industry, same size (total assets) quartile, Tobin's Q quartile, and cash flow quartile in the matching year (year $t-1$). We then retain candidate control firms that have, in the matching year, the smallest difference in the number of analysts compared to the treated firm. Finally, we rank the remaining firms based on the average ranking distance from our treated firm in terms of total assets, Tobin's Q, and cash flow and select as control firm the one with the lowest distance. We are forced to drop 1024 treated firms for which we cannot identify a suitable control firm. We end up with 895 unique pairs of treatment-control matches deriving from 53 broker disappearances, of which 17 are the result of broker closures and 36 are the result of broker mergers.

In order to validate our identification strategy we measure the drop in analyst coverage experienced by our treatment (and control) firms around the broker closures and mergers. We find that firms directly affected by these events experience an average drop in coverage, compared to the matched control firms, of 1.1 analysts (with a t-statistic of 4.08). This figure is consistent with similar studies and show that broker closures and mergers actually entail a significant shock to analyst coverage for the firms affected.

Albeit widely used in the literature, the matching methodology described above is very demanding in terms of data required, leading to a set of possible control firms that is empty for roughly 50% of the firms in the treatment sample. To circumvent this problem we consider three alternative matching strategies where every treated firm is matched with:

- The closest firm in terms of pre-event total factor productivity.
- The closest firm in terms of pre-event total factor productivity within the same industry.
- The closest firm, within the same industry, in terms of propensity score estimated with a probit model on pre-event total assets, Tobin's Q, cash flow, analyst coverage, and productivity.

After obtaining our matched samples of control firms, we use a DID estimation to ensure that the difference in productivity between the treatment and control firms is not caused by a common time trend in productivity. The success of this approach rests on the assumption that the only relevant difference between the two samples is the treatment. While it is impossible to prove that without the treatment the two samples would have behaved similarly, we can look at the productivity trend before and after the treatment. The so-called parallel trend assumption does not require the level of productivity for treatment and control firms to be identical before the treatment because these distinctions will be differenced out in the estimation. Rather, the parallel trend assumption requires similar trends in the level of productivity between treatment and control firms before the treatment. We test this assumption in two ways.

First of all we plot Figure 1 the difference in TFP between treatment and control firms (built with our main matching methodology), over a seven-year window around the exogenous shock to analyst coverage. The graph shows that outside the event window ($t-1$ to $t+1$) the two samples show similar behavior in terms of their productivity trend. As a second test of the parallel trend assumption in Panel A of Table 5 we calculate descriptive statistics for the two samples of firms and show, among other things, that the median growth rate of productivity in the year prior to the treatment is the same for treatment and control firms. Moreover, there are no significant differences in the matching variables between the treatment and control firms. The values are consistent with those reported by (Derrien and Kecskés (2013)). Overall, the univariate comparisons in Panel A indicate that the matching process has effectively removed most of the cross-sectional heterogeneity between the treatment and control firms.

[Place Figure 1 about here]

[Place Table 5 about here]

Panel B of Table 5 reports the result for our DID analysis. We compute the mean change in TFP from year $t-1$ to year $t+3$ for our treatment and control firms and construct our diff-in-diff estimator as the difference between the two. The diff-in-diff estimator calculated is -0.042 and statistically significant at the 5% level. Since TFP is measured in logs, this result suggests that an exogenous loss of the coverage of one analyst causes the affected firm to be 4.2% less productive than a similar firm unaffected by the event.[7]

Next, we examine whether our results are stronger if the drop in analyst coverage is more costly to the firm. Intuitively speaking, the loss of an analyst should make a big difference for a firm that is followed by 10 analysts before the shock, and should not make a big difference for a firm that is followed by 30 analysts before the shock. To examine this conjecture, we classify a firm as having a low (high) level of initial coverage if the firm's level of analyst coverage in year $t-1$ is below (above) the median pre-event analyst coverage of 18. Panel C reports the results. An exogenous loss of one analyst only reduces productivity for firms that were followed by less analysts before the shock, and the result is significant at the 1% level.

Lastly, we examine whether this result is affected by the specific requirements of the matching strategy. Panel D reports the DID estimators built with the alternative control samples. Results are comparable in size and sign and are universally significant at the 1% level. Overall our two identification strategies both indicate a strong positive causal effect of analyst coverage on firm-level total factor productivity.

5.3. Robustness

We conduct two additional robustness checks and report the results in Table 6.[8] First, we examine how our results could be affected by our estimation of firm-level productivity. As mentioned

[7]Results are similar if we cluster standard errors by firm or by industry

[8]We conducted robustness tests for our OLS, IV, and DID analysis and found similar results. For brevity, we only report the robustness tests results for our DID analysis.

before, the approach suggested by Olley and Pakes (1996) controls for the simultaneity and selection problems involved in the estimation of production functions by using a proxy method in which firms' investment decisions are used to construct proxies for their unobserved productivity parameters. On the other hand, Levinsohn and Petrin (2003) suggest to use firms' materials used in production to construct proxies for the unobserved productivity parameters (See Ackerberg, Benkard, Berry, and Pakes, 2007 for a comparison of these two proxies). Hence, we re-estimate productivity following Levinsohn and Petrin (2003) (see Appendix A for details) and run the DID estimations using this measure. Panel A of Table 6 reports the results. The positive relationship between analyst coverage and firm productivity remains highly significant and quantitatively unchanged.

[**Place Table 6 about here**]

Second, we examine whether our findings are driven by specific systemic events. First of all, a significant fraction (23%) of the brokerage disappearances took place after the burst of the so-called dot-com bubble in 2001. Hence, one might be concerned that our results could be driven by this systemic event. Another uncommon time period that could be driving our results are the years of the global financial crisis when the impact of financial analysts on firm productivity could be affected by the general economic conditions. To address these concerns, we exclude the years 2001, 2008, and 2009 in our estimations. Panel B of Table 6 reports the results. We continue to find a significant positive relationship between analyst coverage and firm productivity.

6. Channels through which analysts improve firm productivity

Our evidence so far suggests that analyst coverage improves firm productivity. While this finding on the surface may seem to contradict the findings of He and Tian (2013) as they show that analysts discourage investment in innovation (arguably one of the key drivers of productivity), we find two possible reasons to suggest that the two findings are not in contradiction with each other. First, Clarke, Dass, and Patel (2016) recently show that analysts only impede innovation in the least productive firms and at the same time encourage innovation in the most productive firms, and hence play a beneficial role in resource allocation in the economy. Their finding is consistent with our main hypothesis, that is, analysts increase firm productivity by improving resource allocation (providing firms with the optimal amount of external finance and forcing managers to invest in the most productive projects). Second, we need to remember that our evidence so far reflects only the net effect of analyst coverage on firm productivity. Analysts may force managers to forgo long term innovative investments that could possibly improve firm productivity, but analysts can also improve productivity by allowing firms to invest in the most productive investments. Hence, the positive net effect of analyst coverage on firm productivity we observe could be due to the fact that the effect of the latter dominates the former. In this section, we examine in detail the channels through which analysts help to improve firm productivity.

6.1. External financing

As information providers, financial analysts are in a position to help with reducing the information asymmetries between company insiders and outsiders by producing and sharing their views about the firms' growth opportunities (Lys and Sohn, 1990; Liu, 2011). Researchers (Doukas et al., 2008; Derrien and Kecskés, 2013) have shown that by reducing information asymmetries analyst can affect the amount of external finance a firm can obtain. This activity would be particularly important for firms with limited public information (such as small and young firms) and firms that are more difficult to analyze (such as firms with a high level of intangible assets). Investors simply may not have the resources to develop a deep understanding of these firms and may be reluctant to invest without an independent assessment of their business prospects (Barth, Kasznik, and McNichols, 2001; Easley and O'hara, 2004), or they may over-invest when self-interested insiders (managers) of these firms purposely issue shares when the firm is overvalued with the incentive to build their own empire or to increase perquisite consumption with the excess capital (Jensen, 1986). Hence, analysts' independent assessment of growth opportunities would be extremely important to these opaque firms as it would allow them to obtain the optimal amount of external financing to avoid under- or over-investment. On the other hand, this role of analysts is less important for large and mature firms and firms with a lower level of intangible assets because investors can easily obtain information about these firms and would be able to provide the optimal amount of external finance. In this case, we would expect an exogenous drop in analyst coverage to significantly affect small and young firms and firms with a high level of intangible assets as the reduction in analyst coverage would severely reduce the ability of company outsiders to acquire the necessary information of these firms in order to provide the optimal amount of external finance.

To test this hypothesis, we split the firms into young and old, big and small, and tangible and intangible. Firms are assigned to the two sub-samples based on firm characteristics measured in the year prior to the decrease in analyst coverage to avoid endogeneity issues. Firm age is based on the number of years since the first appearance of the firm in Compustat. Firm size is based on market capitalization. The relevance of intangible assets is based on firms' R&D and advertising expenses. Barth et al. (2001) argue that these expenses generate brand and technology-related intangible assets and offer a better proxy than traditional measures based on the accounting value of firm assets (variable definitions are provided in Table 1). Conditional on each measure, we divide the treatment and control firms into two groups and conduct the DID estimation for the two sub-groups separately. Table 7 reports the results. Consistent with our hypothesis, the reduction in analyst coverage only reduces productivity in firms that are small and young and firms that have a higher level of intangible assets, and all the results are significant at the 5% level.

[Place Table 7 about here]

6.2. External monitoring

In addition to reducing information asymmetries, prior studies have also shown that analysts can act as external monitors and help to reduce agency problems between the shareholders and managers. For example, Dyck, Morse, and Zingales (2010) find that analysts play a bigger role in detecting corporate fraud than the SEC and auditors. Due to the ability of analysts to blow the whistle, it has been found that firms covered by more analysts engage in less earnings management as managers are afraid that analysts would discover and reveal the inflation of earnings to the general public (Yu, 2008; Irani and Oesch, 2013). In a similar vein, we expect that analysts would help to improve firm productivity as their ability to detect and reveal managerial misbehavior would force managers to invest in the most productive projects rather than to increase perquisite consumption. Our expectation is consistent with prior studies that show reducing agency costs improves capital investment efficiency (Biddle and Hilary, 2006; Verdi, 2006; Cheng, Dhaliwal, and Zhang, 2013). In particular, we expect the external monitoring role of analysts to be very important for firms where investor protection is relatively weak, for example in firms where executives are shielded by stronger anti-takeover provisions or where powerful CEOs have gained a strong control on the board of directors. On the other hand, we would expect a smaller effect for firms with less entrenched executives because internal governance mechanisms and threats from the market for corporate control are sufficient to ensure managerial commitment to shareholder value creation in those firms.

To test this hypothesis, we condition our DID estimation upon proxies for managerial power. Following the literature, we measure managerial power using the G-index from Gompers, Ishii, and Metrick (2003), CEO tenure, and CEO/Chair duality (Variable definitions are provided in Table 1). We measure all of our proxies during the year before the exogenous decrease in analyst coverage. Conditional on each proxy, we divide the treatment and control firms into two groups and conduct the DID estimation for the two sub-groups separately. Table 8 reports the results.

[Place Table 8 about here]

The first proxy we use to measure managerial power is the G-index constructed by Gompers et al. (2003). The G-index is based on 24 anti-takeover provisions and higher index levels correspond to greater managerial power. Bertrand and Mullainathan (2003) and Masulis, Wang, and Xie (2007) show that managers working in firms with a higher G-index are less committed to shareholder value creation due to the high level of job security. Thus, we expect that losing analysts' external monitoring is likely to hurt firms with a higher level of G-index a lot since there is an insufficient level of internal monitoring. Consistent with our expectations, the reduction in analyst coverage only reduces productivity for firms with a higher G-index, and the result is significant at the 5% level.

The second proxy we use to measure managerial power is CEO tenure, obtained from the ExecuComp database. Hermalin and Weisbach (1998) predict that longer-serving CEOs are likely to have exerted a stronger influence on board composition and consequently would be subjected to weaker monitoring. Ryan and Wiggins (2004) further suggest that entrenched CEOs often use their power to influence directors' compensation in order to reduce directors' incentives to monitor them. Therefore, we expect that losing analysts' external monitoring is likely to hurt firms with entrenched CEOs. Consistent with our expectations, the reduction in analyst coverage only reduces productivity for firms with longer serving CEOs, and the result is significant at the 5% level.

The third proxy we use to measure managerial power is CEO/Chair duality, and we obtain this information also from the ExecuComp database. We expect the loss of external monitoring by analysts to be vital in firms which the CEO is also the chairman because the dual office structure concentrates power in the CEO's position, which allows him to control the release of information to other board members and thus impedes effective internal monitoring (Jensen, 2003). Consistent with our expectations, the reduction in analyst coverage only reduces productivity for firms which the CEO is also the chairman, and the result is significant at the 5% level.

7. Conclusion

In this paper, we examine the ability of financial analysts to affect the quality of corporate investment decisions by focusing on their impact on firm productivity. Using an extensive sample of U.S. publicly listed firms we show that greater analyst coverage leads to higher total factor productivity. We address the issue of the potential endogeneity of analyst coverage decisions by using a well-documented instrumental variable approach and exogenous shocks to analyst coverage due to the merger and closure of brokerage firms.

Analysts can help to provide firms with the optimal amount of external financing to invest in productive projects and can also act as external monitors to force managers to invest in the most productive projects. We find empirical validation for both channels by showing that the positive effect of analyst coverage on productivity occurs in more opaque firms and firms with weaker investor protection.

However, we need to bear in mind we cannot rule out the potential negative role analysts play in motivating productivity as our evidence reflects only the net effect of analyst coverage on firm productivity. Analysts can play a positive role in improving productivity by allowing firms to invest in the productive projects, but can also play a negative role by forcing managers to forgo valuable long term investments that could possibly improve firm productivity. Our results suggest that the effect of the former dominates the latter.

Overall, we provide novel micro-level evidence to shed new light on the role of financial analysts within the real economy. Specifically, we show despite the frequent criticism that analysts foster short-termism amongst corporate managers, analyst coverage has a positive effect on the quality of corporate investment decisions, and via this effect on firm productivity.

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Table 1: Variable definitions

This table contains the definition of all the variables used throughout the paper.

Panel A: Definition of main and control variables	
Variable	Definition
TFP	Natural logarithm of firm level productivity calculated using the semi-parametric method introduced by Olley and Pakes (1996)
LnCoverage	Natural logarithm of *Coverage* defined as the total number of unique analysts issuing earnings forecasts for firm i during the 12 month period before its fiscal year ending date obtained from I/B/E/S
LnAssets	Natural logarithm of total assets (AT)
Leverage	Book value of debt (DLTT+DLC) divided by total assets (AT)
Book-to-market	Book value of common equity (CEQ) divided by market value of common equity (CSHOxPRCC)
Capex	Capital expenditure (CAPX) scaled by net sales (SALE)
AssetsGrowth	Change of total assets (AT) scaled by lagged total assets
CashFLow	Earnings before extraordinary items (IBC) plus depreciation and amortization (DPC) all divided by total assets (AT)
ROA	Operating income before depreciation (OIBDP) divided by total assets (AT)
Dividends	Dividends to common shares (DVC) plus dividends to preferred shares (DVP), all divided by total assets (AT)
Tobin's Q	Total assets (AT) minus book value of common equity (CEQ) plus market value of common equity (CSHO x PRCC), all divided by total assets (AT).
Panel B: Definition of information asymmetry proxy variables	
Firm Size	Natural logarithm of the market value of common equity (CSHOxPRCC)
Firm Age	The number of years since the firm's first appearance in Compustat
Intangible Assets	Research and development expenses (XRD) plus advertising expenses (XAD), all divided by total operating expense
Panel C: Definition of investor protection proxy variables	
GIM Index	From Gompers et al. (2003), based on 24 antitakeover provisions. Higher levels correspond to lower investor protection.
CEO Tenure	Number of years the executive has been CEO within the firm (obtained from Execucomp).
CEO/Chair duality	Dummy variable that equals one if the CEO is also the Chair, and zero otherwise (obtained from Execucomp).

Table 2: Summary statistics

This table reports the summary statistics for variables constructed based on the sample of U.S. public firms from 1991 to 2013. Definitions of the variables can be found in Table 1.

Panel A: Firm characteristics

Variable	Mean	St Dev	Q1	Median	Q3	N
Coverage	10.922	9.196	4.000	8.000	15.000	35280
LnCoverage	2.013	0.933	1.386	2.079	2.708	35280
TFP	-0.344	0.436	-0.564	-0.369	-0.141	35280
LnAssets	6.524	1.677	5.302	6.396	7.617	35280
Leverage	0.216	0.189	0.041	0.194	0.332	35280
Book-to-market	0.548	0.429	0.282	0.459	0.710	35280
Capex	0.080	0.125	0.023	0.043	0.079	35280
AssetsGrowth	0.152	0.494	-0.005	0.073	0.197	35280
CashFlow	0.094	0.086	0.057	0.096	0.139	35280
ROA	0.146	0.098	0.092	0.138	0.191	35280

Panel B: Firm characteristics by size of analyst coverage

		Number of analysts covering the firm				
Variable		1 - 5	6 - 10	11 - 15	16 - 20	>20
TFP	Mean	-0.467	-0.351	-0.289	-0.265	-0.146
	Median	-0.460	-0.369	-0.328	-0.306	-0.215
LnAssets	Mean	5.309	6.248	7.047	7.732	8.549
	Median	5.254	6.217	7.021	7.705	8.513
Leverage	Mean	0.210	0.214	0.225	0.237	0.213
	Median	0.173	0.190	0.210	0.230	0.200
Book-to-market	Mean	0.679	0.534	0.475	0.442	0.407
	Median	0.580	0.459	0.411	0.373	0.336
Capex	Mean	0.061	0.076	0.083	0.093	0.118
	Median	0.034	0.042	0.045	0.051	0.060
AssetsGrowth	Mean	0.118	0.180	0.175	0.152	0.161
	Median	0.057	0.088	0.086	0.075	0.076
CashFlow	Mean	0.081	0.093	0.096	0.102	0.115
	Median	0.086	0.095	0.097	0.103	0.116
ROA	Mean	0.127	0.144	0.151	0.158	0.184
	Median	0.124	0.137	0.141	0.147	0.166

Table 3: Baseline regression of firm productivity on analyst coverage

This table reports regressions of firm productivity (one, two or three years ahead) on analyst coverage and other control variables. Definitions of variables are in Table 1. Robust standard errors clustered by firm are displayed in parentheses. Statistical significance at the 10, 5 and 1% level is indicated by *, **, and ***, respectively.

Dependent variable	(1) TFP_{t+1}	(2) TFP_{t+1}	(3) TFP_{t+1}	(4) TFP_{t+2}	(5) TFP_{t+3}
LnCoverage	0.110***	0.116***	0.037***	0.039***	0.037***
	(0.006)	(0.005)	(0.006)	(0.007)	(0.007)
LnAssets			0.025***	0.024***	0.025***
			(0.004)	(0.005)	(0.005)
Book-to-market			-0.116***	-0.104***	-0.096***
			(0.011)	(0.011)	(0.011)
ROA			1.526***	1.238***	1.086***
			(0.116)	(0.098)	(0.093)
Capex			0.207***	0.225***	0.280***
			(0.071)	(0.073)	(0.077)
AssetsGrowth			0.087***	0.045***	0.020**
			(0.028)	(0.016)	(0.009)
CashFlow			0.451***	0.275***	0.208**
			(0.093)	(0.083)	(0.083)
Leverage			-0.010	0.004	0.026
			(0.028)	(0.031)	(0.033)
Constant	-0.578***	-0.618***	-0.760***	-0.718***	-0.650***
	(0.010)	(0.117)	(0.089)	(0.108)	(0.103)
Year FE	No	Yes	Yes	Yes	Yes
Industry FE	No	Yes	Yes	Yes	Yes
Number of Obs.	40740	40740	35280	30615	26801
Adjusted R^2	0.051	0.075	0.280	0.194	0.160

Table 4: Instrumental Variable Estimation

This table reports the 2SLS estimation of the effect of analyst coverage on firm productivity. Model (1) reports the result of the first stage where analyst coverage is regressed on a measure of Expected Coverage derived from exogenous changes in broker size following Yu (2008). The remaining models report results for the second stage where productivity (one, two and three years ahead, respectively) is regressed over instrumented coverage and the other control variables. Definitions of variables are shown in Table 1. Robust standard errors clustered by firm are displayed in parentheses. Statistical significance at the 10, 5, 1% level is indicated by *, **, and ***, respectively.

Dependent variable	(1) *LnCoverage*	(2) TFP_{t+1}	(3) TFP_{t+2}	(4) TFP_{t+3}
ExpectedCoverage	0.051***			
	(0.002)			
LnCoverage (instrumented)		0.110***	0.183***	0.209***
		(0.032)	(0.034)	(0.036)
LnAssets	0.290***	-0.005	-0.036***	-0.046***
	(0.007)	(0.012)	(0.013)	(0.014)
Book-to-market	-0.281***	-0.092***	-0.050***	-0.027
	(0.017)	(0.018)	(0.018)	(0.019)
ROA	0.308***	1.373***	1.133***	0.974***
	(0.086)	(0.121)	(0.103)	(0.098)
Capex	0.406***	0.239**	0.220**	0.236**
	(0.081)	(0.094)	(0.093)	(0.095)
AssetsGrowth	0.019	0.076***	0.038***	0.014*
	(0.016)	(0.028)	(0.015)	(0.008)
CashFlow	-0.014	0.575***	0.307***	0.288***
	(0.085)	(0.101)	(0.092)	(0.091)
Leverage	-0.544***	0.070*	0.146***	0.200***
	(0.044)	(0.038)	(0.040)	(0.043)
Constant	-0.047	-0.712***	-0.592***	-0.489***
	(0.081)	(0.106)	(0.125)	(0.119)
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Number of Obs.	28721	25839	23506	21484
Adjusted R^2	0.626	0.28	0.174	0.127

Table 5: Difference-in-Differences Estimation

This table reports the DID estimation results of the causal effect of an exogenous drop in analyst coverage (due to brokerage closures and mergers) on firm productivity. The sample includes 895 firms (1919 in Panel D) that have lost an analyst between 1994 and 2010 because of broker closures and broker mergers. Each treated firm is matched with a control firm from the same industry and with similar size (total assets), Tobin's Q, cash flows, and analyst coverage (see Section 5.2 for the detailed matching procedure). Panel A reports the univariate comparisons between treatment and control firms' median characteristics. Panel B shows the average change in productivity experienced by firms in the treatment and control samples between year $t+1$ and year $t-1$ (where t is the year of the exogenous shock to coverage) as well as the diff-in-diff estimator with the relative T-Stats. Panle C reports the DID test results based on the level of analyst coverage for firms in year $t-1$. Panel D repeats the diff-in-diff analysis with three alternative matching strategies detailed in section 5.2. Statistical significance at the 10, 5, 1% level is indicated by *, **, and ***, respectively.

Panel A: Post-match median characteristics

	Treatment	Control	Difference	P-Value
CashFlow	0.110	0.110	0.000	0.925
Assets	2079.982	2056.879	23.103	1.000
Tobin's Q	1.992	1.860	0.132	0.344
Coverage	18.000	18.000	0.000	0.449
TFP growth	-0.001	0.005	-0.006	0.437

Panel B: Diff-in-Diff estimation

	Δ Treatment	Δ Control	Diff-in-Diff	T-Stat
Quartile Matching	-0.105	-0.064	-0.042**	-2.271
	(0.012)	(0.013)	(0.018)	

Panel C: Diff-in-Diff estimation conditioning on intital coverage

Low initial coverage	-0.144	-0.064	-0.080***	-2.816
	(0.019)	(0.021)	(0.028)	
High initial coverage	-0.077	-0.057	-0.021	-0.829
	(0.017)	(0.018)	(0.025)	

Panel D: Alternative matching strategies

TFP-matched	-0.091	-0.040	-0.051***	-3.624
	(0.009)	(0.011)	(0.014)	
TFP/FF49-matched	-0.090	-0.047	-0.043***	-2.984
	(0.009)	(0.011)	(0.014)	
Propensity-score matched	-0.090	-0.049	-0.040***	-3.284
	(0.009)	(0.008)	(0.012)	

Table 6: Robustness tests

This table reports robustness test results for our diff-in-diff analysis. Panel A presents test results using an alternative TFP estimation methodology derived from Levinsohn and Petrin (2003). Panel B presents test results dropping the years relative to the dot-com bubble and the global financial crisis (2001, 2008 and 2009). Standard errors are displayed in parentheses. Statistical significance at the 10, 5, 1% level is indicated by *, **, and ***, respectively.

Panel A: Alternative productivity measure

	Diff-in-Diff	T-Stat
Quartile matching	-0.042**	-2.253
	(0.019)	
TFP-matched	-0.031**	-2.190
	(0.014)	
TFP/FF49-matched	-0.024*	-1.657
	(0.015)	
Propensity-score matched	-0.036***	-2.917
	(0.012)	

Panel B: Alternative time period

	Diff-in-Diff	T-Stat
Quartile matching	-0.051**	-2.275
	(0.022)	
TFP-matched	-0.052***	-3.076
	(0.017)	
TFP/FF49-matched	-0.048***	-2.798
	(0.017)	
Propensity-score matched	-0.050***	-3.417
	(0.014)	

Table 7: The effect of coverage on productivity conditional upon firm opaqueness

This table shows the effect of an exogenous reduction in coverage on the productivity of firms divided in subsamples according to three different proxies for firm opaqueness. The sample includes 895 firms that lost an analyst between 1994 and 2010 because of broker closure and broker mergers. Each treated firm is matched with a control firm from the same industry with similar size (total assets), Tobin's Q, cash flow, and analyst coverage (see section 5.2 for the detailed matching procedure). For a definition of the information asymmetry variables see Table 1. Standard errors are displayed in parentheses. Statistical significance at the 10, 5, 1% level is indicated by *, **, and ***, respectively.

	Δ Treatment	Δ Control	Diff-in-Diff	T-Stat
Firm Size				
Small	-0.120	-0.064	-0.055**	-1.980
	(0.020)	(0.019)	(0.028)	
Big	-0.093	-0.063	-0.030	-1.268
	(0.016)	(0.018)	(0.024)	
Firm Age				
Young	-0.151	-0.087	-0.065**	-2.027
	(0.020)	(0.025)	(0.032)	
Old	-0.056	-0.042	-0.014	-0.776
	(0.013)	(0.012)	(0.018)	
Intangible Assets				
High	-0.141	-0.015	-0.126**	-2.002
	(0.036)	(0.053)	(0.063)	
Low	-0.073	-0.088	0.015	0.512
	(0.019)	(0.023)	(0.030)	

Table 8: The effect of coverage on productivity conditional upon investor protection

This table shows the effect of an exogenous reduction in coverage on the productivity of firms divided in subsamples according to different proxies for information asymmetries (Panel A) and investors' protection (Panel B). The sample includes 895 firms that lost an analyst between 1994 and 2010 because of broker closure and broker mergers. Each treated firm is matched with a control firm from the same industry with similar size (total assets), Tobin's Q, cash flow, and analyst coverage (see section 5.2 for the detailed matching procedure). For a definition of the different proxy variables see Table 1. Standard errors are displayed in parentheses. Statistical significance at the 10, 5, 1% level is indicated by *, **, and ***, respectively.

	Δ Treatment	Δ Control	Diff-in-Diff	T-Stat
Firm Size				
High	-0.097	0.009	-0.107**	-2.532
	(0.033)	(0.026)	(0.042)	
Low	-0.075	-0.087	0.012	0.216
	(0.040)	(0.038)	(0.056)	
CEO tenure				
Long	-0.135	-0.074	-0.061**	-2.176
	(0.019)	(0.020)	(0.028)	
Short	-0.071	-0.073	0.002	0.099
	(0.016)	(0.017)	(0.023)	
CEO/Chair duality				
Yes	-0.105	-0.055	-0.049**	-2.241
	(0.016)	(0.015)	(0.022)	
No	-0.104	-0.100	-0.005	-0.151
	(0.021)	(0.024)	(0.032)	

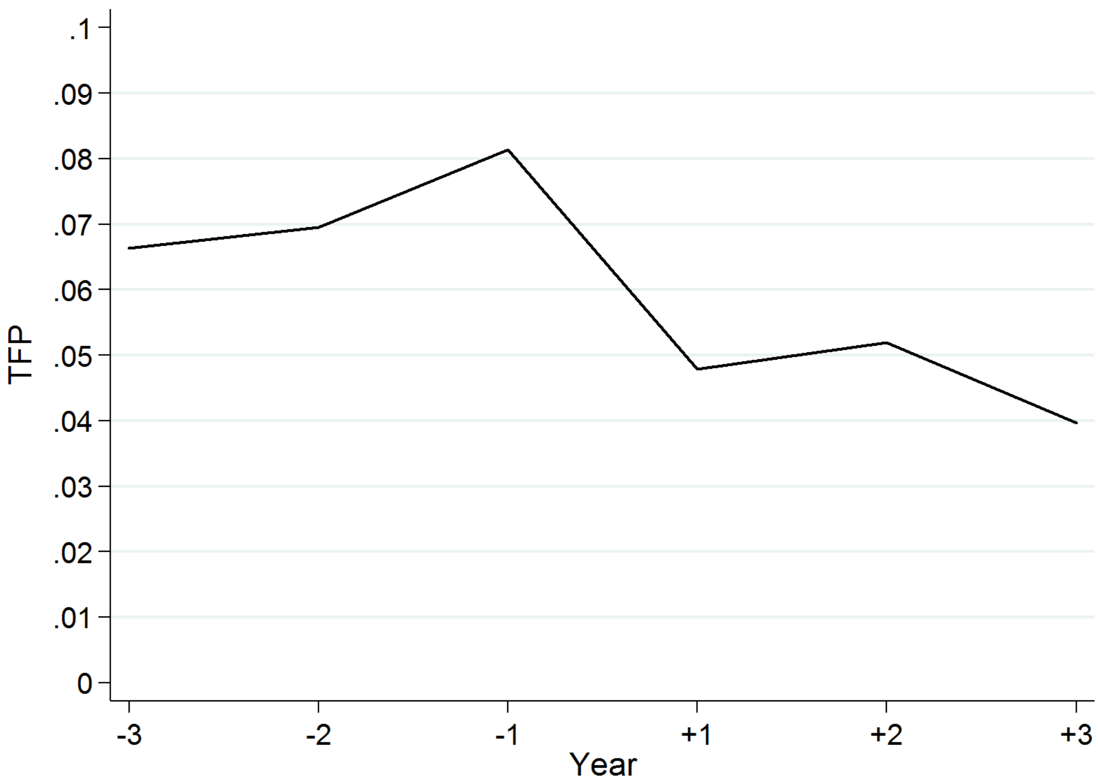

Figure 1: Parallel Trend in TFP
This figure presents the mean difference in TFP between firms in the treatment and control samples during the seven years around the exogenous decrease in analyst coverage. The sample includes 895 firms that lost an analyst between 1994 and 2010 due to a broker closure or broker merger. Each treated firm is matched with a control firm from the same industry with similar size (total assets), Tobin's Q, cash flow, and analyst coverage (see section 5.2 for the detailed matching procedure).

Appendix A. Estimation of Production Functions

To estimate the production functions we start from the Compustat universe and drop financial firms (SIC classification between 6000 and 6999) and regulated firms (SIC classification between 4900 and 4999). We keep all remaining observations with non-missing data on sales, total assets, number of employees, gross property, plant and equipment, depreciation, accumulated depreciation, and capital expenditures. The price index for Gross Domestic Product is used as the deflator for output and the price index for private fixed investment is used as the deflator for investment and capital, both obtained from the Bureau of Economic Analysis. Data on employees' average wages is sourced from the national average wage index from Social Security Administration.

Following Imrohoroglu and Tüzel (2014), output is computed as sales minus materials, deflated by the GDP price deflator.[9] Sales is measured as net sales (SALE). Materials is measured as Total expenses minus Labor expenses. Total expenses is approximated as sales minus operating income before depreciation and amortization (OIBDP). Labor expenses is calculated by multiplying the number of employees (EMP) by average wages from the Social Security Administration. Labor stock is measured by the number of employees.

Capital stock is given by gross property, plant and equipment (PPEGT), deflated by the price deflator for investment following Hall (1990). Investment is made at various times, therefore we need to calculate the average age of capital at every year for each company and apply the appropriate deflator. Average age of capital stock is calculated by dividing accumulated depreciation (DPACT)) by current depreciation (DP). Age is further smoothed by taking a 3-year moving average. The resulting capital stock is lagged by one period to measure the available capital stock at the beginning of the period.

We estimate the production function based on labor and physical capital as inputs. The production function is:

$$y_{it} = \beta_0 + \beta_k k_{it} + \beta_l l_{it} + \omega_{it} + \epsilon_{it} \tag{A1}$$

[9]Sales minus materials is used to proxy for output rather than using Sales alone. The underlying economic rationale comes from Foster et al. (2008) who show that firm revenue usually confounds the effects of idiosyncratic demand and factor prices with efficiency differences. That is, firms can have high revenue levels because they are efficient, but it can also be driven by high input prices.

where y_{it} is the log value of output for firm i in period t, k_{it} and l_{it} are the log values of capital and labor respectively, ω_{it} is the level of productivity and ϵ_{it} is the error term. As argued by Olley and Pakes (1996), productivity, is observed by the firm before the firm makes some of its factor input decisions, and this gives rise to the simultaneity problem. To control for this problem, both Olley and Pakes (1996) and Levinsohn and Petrin (2003) suggest using investment and intermediate inputs respectively to proxy for productivity. Both methods assume monotonic relationship between the proxy variable and the true productivity shocks. Due to this assumption, both methods require the proxy variable to be positive as productivity shocks can rarely be negative in reality. In addition, both methods define labor to be a variable input as firms can adjust this factor in response to current productivity levels and define capital to be a fixed input as it is assumed the capital that the firm uses in period t was decided in period $t-1$.

Olley and Pakes (1996) approach

Let I denote firm investment. Conditional on the firm having positive investment ($I_{it} > 0$), we can write:

$$\omega_{it} = h(I_{it}, k_{it})$$

In other words, conditional on a firm's capital stock, firm investment allows us to uniquely determine productivity. Moreover, conditional on all information available at time t, ω_{it} is a sufficient statistic for predicting $\omega_{i,t+1}$.

Let's define:

$$\phi_{it} = \beta_0 + \beta_k k_{it} + h(I_{it}, k_{it}) \tag{A2}$$

Using equations (A1) and (A2), in the first stage we estimate

$$y_{it} = \beta_l l_{it} + \phi_{it} + \epsilon_{it} \tag{A3}$$

where we approximate ϕ_{it} with a second order polynomial series in capital and investment. This first stage estimation results in an estimate for $\hat{\beta}_l$ that controls for the simultaneity problem. In the second stage, consider the expectation of $y_{i,t+1} - \hat{\beta}_l l_{i,t+1}$ on information at time t and survival of the firm:

$$E_t(y_{i,t+1} - \hat{\beta}_l l_{i,t+1}) = \beta_0 + \beta_k k_{i,t+1} + E_t(\omega_{i,t+1}|\omega_{it}, Survival) \tag{A4a}$$

$$= \beta_0 + \beta_k k_{i,t+1} + g(\omega_{it}, \hat{P}_{Survival,t}) \tag{A4b}$$

where $\hat{P}_{Survival,t}$ denotes the probability of firm survival from time t to time $t+1$. The fitted value of survival probability is estimated via a probit model with polynomial expression containing capital and investment. We fit the following equation by nonlinear least squares:

$$y_{i,t+1} - \hat{\beta}_l l_{i,t+1} = \beta_k k_{i,t+1} \rho \omega_{it} + \lambda \hat{P}_{Survival,t} + \epsilon_{i,t+1} \tag{A5}$$

where ω_{it} is given by $\omega_{it} = \phi_{it}(I_{it}, k_{it}) - \beta_0 - \beta_k k_{it}$. At the end of this stage, $\hat{\beta}_l$ and $\hat{\beta}_k$ are estimated.

Finally, firm level log TFP is measured by:

$$TFP_{it} = y_{it} - \hat{\beta}_0 - \hat{\beta}_l l_{it} - \hat{\beta}_k k_{it} \tag{A6}$$

We estimate equation (A3) with industry specific time dummies and then subtract them from the left hand side of equation (A4). Hence, our firm level TFP measure is free of the effect of industry or aggregate TFP in any given year.

Levinsohn and Petrin (2003) approach

The two stage estimation devised by Olley and Pakes (1996) relies on firm investment to solve the simultaneity problem. Levinsohn and Petrin (2003) suggest that the use of this variable can be problematic. First of all the estimation requires a positive value for investments so, for example, firms that make only intermittent investments will have their zero-investment observations truncated from the estimation. Moreover they also point out that non-convex adjustment costs may lead to kinks in the investment function that affect the responsiveness of investment to economic shocks.

To overcome these problems the authors propose an alternative estimation strategy based on an intermediate input, materials used in production, that should not suffer from the same issues. Now conditional on $m_{it} > 0$, we can write $\omega_{it} = h(m_{it}, k_{it})$. Conditional on a firm's capital stock, materials used in production allow us to uniquely determine productivity.

The derivation of firm-level productivity follows the same two-stage structure introduced by Olley and Pakes (1996) with the substitution of materials used in production m_{it} for firm investments I_{it}.