# Competition and Bias

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**Abstract**

We attempt to measure the effect of competition on bias in the context of analyst earnings forecasts, which are known to be excessively optimistic due to conflicts of interest. Our instrument for competition is mergers of brokerage houses, which result in the firing of analysts because of redundancy (e.g., one of the two oil analysts is let go) and other reasons such as culture clash. We use this decrease in analyst coverage for stocks covered by both merging houses before the merger (the treatment sample) to measure the causal effect of competition on bias. We find the treatment sample simultaneously experiences a decrease in analyst coverage and an increase in optimism bias the year after the merger relative to a control group of stocks, consistent with competition reducing bias. The implied economic effect from our natural experiment is significantly larger than estimates from OLS regressions that do not correct for the endogeneity of coverage. And this effect is much more significant for stocks with little initial analyst coverage or competition.

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## I. Introduction

Reporting bias is an important aspect of economic life. A prominent example is the well-documented excessive optimism of sell-side analysts' earnings forecasts and recommendations (Brown, Foster, and Noreen (1985), Stickel (1990), Abarbanell (1991), Dreman and Berry (1995), and Chopra (1998)). In the aftermath of the collapse of internet stock valuations, Congress blamed the exuberance of well-known analysts, such as Mary Meeker and Jonathan Blodgett for contributing to the losses of individual investors who bought internet stocks on their recommendations (Kane (2001)). Another prominent example as of late is media bias. Some media outlets slant to the right on the political spectrum, while others slant to the left, e.g. Fox News reports on the same event with a more conservative slant than does the New York Times (e.g., Groseclose and Milo (2005)). There is some concern that such bias is leading to polarization of the electorate and failing trust in the media. Recently, the bias of credit rating agencies such as Moody's and Standard and Poor's has been blamed for the subprime crisis. Numerous theories have been advanced on the nature of reporting bias in a variety of contexts (see Ottaviani and Sorensen (2006) for a review).

An important question, both from an academic and a regulatory perspective, arising out of these concerns about bias is the extent to which competition affects bias. There are a number of views on this issue. The one that we focus on, the competitive pressure view, is that competition reduces bias because of the pressure to be accurate. This view implicitly assumes that consumers (e.g., investors or readers of news) want accuracy. Another perspective, or the catering view, is that competition need not reduce and may increase bias if consumers want to hear reports that conform to their priors.

Gentzkow and Shapiro (2006) provide a recent model of bias that is emblematic of the first view, while Mullainathan and Shleifer (2005) provide one for the second view.[1]

In this paper, we attempt to shed light on the competitive pressure view by measuring the effect of competition on bias in the context of analyst earnings forecasts. Our paper takes as its point of departure a large literature that convincingly shows that analyst optimism bias arises out of conflicts of interest – a desire to be objective by producing accurate forecasts (desired by investors) versus the need to curry favor with companies and help their houses bring in investment banking business and sales and trading commissions through the issuance of positive forecasts and recommendations.[2]

Several key findings from this literature shape our analysis. First, a number of papers find that an analyst from a brokerage house that has an underwriting relationship with a stock tends to issue more positive predictions than analysts from nonaffiliated houses (see Dugar and Nathan (1995), Lin and McNichols (1998), Dechow, Hutton, and Sloan (1999), Michaely and Womack (1999)). Sales and trading commissions are also important in generating optimism bias (Cowen, Groysberg, and Healy (2006)). Second, many investors (retail) cannot adjust for this bias (i.e. de-bias) and these optimistic recommendations have an effect on stock prices (see Michaely and Womack (1999), Malmendier and Shanthikumar (2007)). Importantly, analysts' career outcomes depend both on relative accuracy and optimism bias (see Hong and Kubik (2003), Fang and Yasuda (2006)).

[1] Both also provide comprehensive overview of both of these views.

[2] Companies naturally like analysts to be optimistic about their stocks, particularly when they are doing initial or seasoned equity offerings. They would not do business with an investment bank if the analyst were not positive about the stock.

We build on existing models of analyst bias to develop the competitive pressure view more formally in Appendix A. Consistent with the empirical evidence, implicit in models of bias (and hence our model) is that many investors (e.g. retail) cannot easily de-bias and also reward analysts based on relative accuracy (see Laster, Bennett, and Geoum (1999) and Ottaviani and Sorensen (2006) for discussions). Since analysts are rewarded for relative accuracy by investors, under the competitive pressure view, we would expect more competition in the form of analysts covering a stock to lead to less bias. If an analyst has no competition in forecasting a stock, he can easily weigh the rewards from bias while ignoring, at least in the short run, the need for accuracy. However, other analysts through relative performance evaluation provide a check on the sanity or accuracy of an analyst's forecasts. Moreover, to the extent that collusion is not possible, greater competition even in the form of one analyst (who might turn out to be whistleblower) could significantly improve accuracy.

The only complication in our setting is that since analysts are not rewarded solely for accuracy, it might be the case that competition may have no effect on bias and might even increase it to the extent that analysts might have to compete for investment banking business by being even more optimistic. Hence, any causal effect from competition we identify in this setting is really the net of these conflicting interests. So whether or not competition actually improves accuracy is empirical question of great interest. Note also that investors typically want accurate forecasts as opposed to having their priors confirmed. So our setting is ideal to test the competitive pressure view but not necessarily the catering view.

Consistent with the competitive pressure view, existing evidence indicates that the average bias of earnings forecasts is significantly smaller for stocks with more analyst coverage and presumably competition (Lim (2001)).[3] Using data on analyst forecasts from 1980 to 2005, we replicate this finding below by regressing the average forecast bias of a stock on analyst coverage and a number of other covariates including firm size. Henceforth, we will refer to the average or mean bias of a stock as simply the bias of that stock. These regressions are based on a sample of large stocks in the top 25% of the market capitalization distribution. We restrict ourselves to this sample to facilitate a comparison with the results from our natural experiment. The mean analyst coverage of these stocks is about 21 analysts and the standard deviation across stocks is about 10 analysts. Depending on the controls used, we find that a decrease in one analyst leads to an increase in bias of anywhere from 0.0002 (2 basis points) to 0.0006 (6 basis points). The bias for a typical stock is about 0.03 (3 percent) with a standard deviation across stocks of about 0.03 (3 percent). Hence, these estimates obtained from cross-section regressions suggest a modest increase in bias of about 60 basis points to 2 percent as a fraction of the cross-sectional standard deviation of bias as we decrease coverage by one analyst, though they are very precisely measured.

Of course, these cross-sectional regressions are difficult to interpret due to the endogeneity of analyst coverage. For instance, if stocks that attract lots of coverage are those analysts are likely to be excited about, then these OLS estimates are biased downward. We would then expect to find a larger causal effect from competition if we could randomly allocate analysts to different stocks. Alternatively, stocks that attract lots

[3] Note that the focus of Lim (2001) is not on competition and bias. Rather it is to show that bias can be rational because bias helps analysts get access to a firm and hence to provide more accurate forecasts. Analyst coverage ends up being one of his control variables. In contrast, it is our main variable of interest.

of coverage may be stocks that every analyst has to cover and so any given analyst does not have to be optimistic about the prospects of the company to issue a forecast. In contrast, stocks covered by only a few analysts are likely under-the-radar stocks that analysts have to be very excited about to initiate coverage on. In this instance, the OLS estimate of the competition effect would be biased upwards. Existing studies suggest that the first worry – a selection bias in coverage in that analysts tend not to cover stocks that they do not issue positive forecasts about – is more likely to be relevant in the data (e.g., McNichols and O'Brien (1997)).

To more accurately identify the causal effect of competition or coverage on bias, we use mergers of brokerage houses as an instrument for competition. When brokerage houses merge, they typically fire analysts because of redundancy and potentially lose additional analysts for other reasons including culture clash and merger turmoil (e.g., Wu and Zang (2007)). For example, if the merging houses each had one analyst covering oil stocks, they would only keep one of the two oil stock analysts after the merger. We use this decrease in analyst coverage for stocks covered by both merging houses before the merger (the treatment sample) to measure the causal effect of competition on bias. During the period of 1980 to 2005, there are fifteen mergers of brokerage houses that affected 948 stocks (stocks covered by both merging houses) or 1656 stock observations. We measure the change in analyst coverage and mean bias for the stocks in the treatment sample from one year before the merger to one year after relative to a control group of stocks. The control group is stocks with the same market capitalization, market-to-book ratio, and past return features as the treatment sample. The exclusion restriction is that the change in the mean bias of the treatment sample across the merger date is not due to any factor other than the merger leading to a decrease in analyst coverage of those stocks. We think this is a good instrument since the merger-related departures of analysts due to redundancy or culture clash ought not to a priori be related to anything having to do with the bias of the forecasts of the other analysts, particularly those working for other houses.

We first verify the premise of our instrument by measuring the change in analyst coverage for the treatment sample from the year before the merger to the year after. We expect these stocks to experience a decrease in coverage since one of the redundant analysts is typically let go. The exact number depends on a couple of factors. On the one hand, the fired analyst might get a job with another firm and cover the same stock, which means the decrease in coverage might be less than one. On the other hand, a firm might lose or fire both analysts for reasons of culture clash or merger turmoil. In this case, if neither analyst is rehired by another firm, we would see a decrease in coverage of two analysts. It is an empirical question as to what the magnitude turns out to be. We find that the average drop in coverage for the treatment sample (using the most conservative control group) is around 1 analyst with a t-statistic of around 5.3. One can think of this finding as essentially the first stage of our instrumental-variables estimation. The effect is economically and statistically significant in the direction predicted, and hence confirming the premise of our natural experiment.

We then measure the change in the mean bias for the treatment sample across the merger date. We find that the treatment sample simultaneously experiences an increase in optimism bias the year after the merger relative to a control group of stocks. A conservative estimate is that the mean optimism bias increases by 17 basis points (as a result of reducing coverage by 1 analyst). As we mentioned earlier, the sample for the natural experiment is similar to that of the OLS by construction – the typical stock has a bias of around 0.03 and the standard deviation of the optimism bias is also around 0.03. So, this means that the estimate of the competitive effect from our natural experiment is anywhere from three to eight times as large as the OLS estimates. The results are similar when we use median estimates instead of mean estimates for bias in a stock and when we use an alternative regression approach rather than a pure difference-in-differences approach. This is a sizeable difference and suggests that the OLS estimates are biased downwards, consistent with the documented selection bias that stocks that attract lots of coverage are likely to have more optimistic analysts.

We then consider a number of robustness checks. Primarily, we worry that our mean bias effect might be driven by selection due to which one of the two analysts from the merging firms covering the stock gets fired. It might be that the less optimistic analyst gets fired and hence the bias might be higher as a result. Another possibility could be that analysts employed by the merging houses may compete for the job in the new merged house and thus they may strategically change their reporting behavior. To deal with these issues, we only look at the change in the bias for the analysts covering the same stocks but not employed by the merging firms. The findings are similar.

In this paper, we focus on annual earnings forecasts since these are the key numbers that the market looks to and every analyst has to submit such a forecast. For completeness, we also look at how long-term growth forecasts and stock recommendations change for the treatment sample in comparison to the control sample around these mergers. One big downside is that data is more sparse as analysts do not issue as many timely growth forecasts or recommendations. Nonetheless, we find similar results, which is reassuring.

We also test an auxiliary prediction that will further buttress our identification strategy. We check to see whether the competition effect is more pronounced for stocks with smaller analyst coverage. The idea is that the more analysts covering a stock, the less the loss of an additional analyst matters – akin to Cournot competition. (There is a caveat if collusion is possible as we explain below.) We divide initial coverage into three groups: less than or equal to five analysts, between 6 and 20 analysts, and greater than 20 analysts. We find that the effect is significantly smaller with greater initial coverage. This key result of our paper is very comforting as it reassures us that our instrumental-variables estimation is a sensible one.

Finally, we look at how forecast dispersion and mean forecast error change along with the increase in forecast bias. Our competition effect has ambiguous implications for the directional change of these two quantities. We find that both forecast dispersion and accuracy fall across the mergers. We interpret these findings in light of a number of different theories.

The paper proceeds as follows. We describe the data in Section II and replicate the OLS regressions of bias on analyst coverage in Section III. In Section IV, we provide background and statistics on the mergers. We discuss the methodology we use to measure the effect of the mergers on analyst coverage and bias in Section V and describe the results in Section VI. We conclude in Section VII.

## II. Data

Our data on security analysts come from the Institutional Brokers Estimates System (IBES) database. Our full sample covers the period 1980-2005. In our study, we focus on annual earnings forecasts since these types of forecasts are the most commonly issued. For each year, we take the most recent forecast of the annual earnings. As a result, we have for each year one forecast issued by each analyst covering a stock.

Our data on U.S. firms come from the Center for Research in Security Prices (CRSP) and COMPUSTAT. From CRSP, we obtain monthly closing stock prices, monthly shares outstanding, daily and monthly stock returns for NYSE, AMEX, and NASDAQ stocks over the period of 1980-2005. From COMPUSTAT, we obtain annual information on corporate earnings, book value of equity, and book value of assets during the same period.[4] To be included in our sample, a firm must have the requisite financial data from both CRSP and COMPUSTAT. We follow other studies in focusing on companies with CRSP share codes of 10 or 11.

We use the following variables in our study. Analyst forecast bias is the difference between her forecast and the actual *EPS* divided by the previous year's stock price. Given the fact that the values of *EPS* reported by IBES tend to suffer from data errors we follow the literature and use *EPS* from COMPUSTAT. Since our analysis is conducted at the stock level we further aggregate forecast biases and consider the consensus bias expressed as a mean or median bias among all analysts covering a particular stock, which is denoted by $BIAS_{it}$. This is our main dependent variable of interest.

[4] Our results are similar if we use IBES earnings numbers as opposed to those from COMPUSTAT.

We also utilize a number of other independent variables. $COVERAGE_{it}$ is the number of analysts covering stock *i* in year *t*. $LNSIZE_{it}$ is the natural logarithm of firm *i*'s market capitalization (price times shares outstanding) at the end of year *t*. $SIGMA_{it}$ is the variance of daily (simple, raw) returns of stock *i* during year *t*. $RETANN_{it}$ is the average monthly return on stock *i* in year *t*. $LNBM_{it}$ is the natural logarithm of firm *i*'s book value divided by its market cap at the end of year *t*. $ROE_{it}$ is firm *i*'s return on equity in year *t*. *ROE* is calculated as the ratio of earnings during year *t* over the book value of equity. Earnings are calculated as income before extraordinary items available to common stockholders (Item 237), plus deferred taxes from the income statement (Item 50), plus investment tax credit (Item 51). To measure the volatility of *ROE* ($VOLROE_{it}$), we estimate an AR(1) model for each stock's *ROE* using the past 10-year series of the company's valid annual *ROE*s. We calculate $VOLROE_{it}$ as the variance of the residuals from this regression. $PROFIT_{it}$ is firm profitability, defined as operating income over book value of assets. $SP_{it}$ is an indicator variable equal to one if the stock is included in S&P 500 index and zero otherwise. As in earlier studies, stocks that do not appear in IBES are assumed to have no analyst estimates.

We also consider two additional dependent variables. Our measure of analyst forecast error is defined as the absolute difference between her forecast and the actual *EPS* of firm *i* at time *t*. Following Lim (2001), we exclude analyst forecasts whose absolute difference exceeds 10 dollars on the basis that this is likely a coding error. We again express the differences as a percentage of the previous year's stock price. Like for forecast bias, we further aggregate forecast errors and consider the consensus error, expressed as a mean or median error among all analysts covering a particular stock, which we denote by $FERROR_{it}$. $FDISP_{it}$ is forecast dispersion, defined as the standard deviation of all analyst forecasts covering firm *i* at time *t*. Following Lim (2001) we exclude observations (stock-year) in which the stock price is less than 5 dollars or whose mean bias is at the outer tails – 2.5% left and right tails.

**III. OLS Results**

We begin by estimating a pooled OLS regression of the mean and median *BIAS* on lagged values of *COVERAGE* and a set of control variables as in Lim (2001). As we alluded to in the Introduction, the focus of Lim (2001) is not on competition and bias. Rather it is to show that bias can be rational because bias helps analysts get access to a firm and hence to provide more accurate forecasts. His cross-sectional regression emphasizes that analysts from small brokerage houses with limited access or analysts covering firms that have difficulty to predict earnings are those that are more likely to bias their forecasts. Analyst coverage ends up being one of his control variables. In contrast, it is our main variable of interest. But we use the other independent variables in Lim's cross-sectional specification as controls, which include *LNSIZE, SIGMA, RETANN, LNBM, VOLROE,* and *PROFIT*. We additionally include S&P 500 index dummy (*SP500*) as well as potentially time and three-digit SIC industry fixed effects. Standard errors are clustered at the industry groupings.

These regressions are based on a sample of large stocks in the top 25% of the market capitalization distribution. We restrict ourselves to this sample to facilitate a comparison with the results from our natural experiment.[5] The summary statistics for these regressions (time-series averages of cross-sectional means, medians, and standard

[5] Qualitatively, the same results hold even using the entire universe. We have replicated these results, which are consistent with those in Lim (2001).

deviations) are reported in Table I. The cross-sectional mean (median) analyst coverage of these stocks is about 21 (21) analysts and the standard deviation across stocks is about 10 analysts. The cross-sectional mean (median) bias is 0.027 (0.021) with a standard deviation of around 0.03.

The regression results are presented in Table II. We first present the results for the mean bias with just industry fixed effects in column (1) and with both industry and time fixed effects in column (2). In column (1), the coefficient in front of *COVERAGE* is –0.0006 and is statistically significant at the 1% level of significance. In column (2), the coefficient is smaller at –0.0002 but it is still statistically significant at the 5% level of significance. So depending on the controls used, we find that a decrease in coverage by one analyst leads to an increase in bias of anywhere from 0.0002 (2 basis points) to 0.0006 (6 basis points). The bias for a typical stock is about 0.027 (2.7 percent) with a standard deviation across stocks of about 0.03 (3 percent). Hence, these estimates obtained from cross-section regressions suggest only a small increase in bias of about 60 basis points to 2 percent as a fraction of the cross-sectional standard deviation of bias as we decrease coverage by one analyst, though they are very precisely measured. The results using the median bias instead of mean bias are reported in columns (3) and (4). Again, there is little difference in the coefficient on *COVERAGE*.

The other control variables also come in significantly in these regressions. Bias increases with firm size, firm book-to-market ratio, volatility of return on equity, and firms' profits. Bias is lower for firms with high returns and for S&P500 firms. The sign on stock return volatility is ambiguous depending on whether time fixed effects are included. These results are vastly consistent with those reported in Lim (2001), though the magnitudes of the coefficients do not always match those from Lim. This is in part because we use a longer time series of data and we follow slightly different sample selection criteria. Nevertheless, we can largely conclude that our sample is representative in that its qualitative aspects do not differ much from those reported in other studies.

Of course, as we explained in the Introduction, these cross-sectional regressions are difficult to interpret due to the endogeneity of analyst coverage. If stocks that attract lots of coverage are stocks that analysts are likely to be excited about, then these OLS estimates are biased downward. In contrast, if stocks covered by only a few analysts are likely under-the-radar stocks that analysts have to be very excited about to initiate coverage on, then these OLS estimates of the competition effect are biased upwards. Estimating this regression using stock fixed effects is not an adequate solution to the endogeneity critique since analyst coverage tends to be a fairly persistent variable and analysts drop coverage on stocks when the stock is no longer doing well (see McNichols and O'Brien (1997)).[6]

Hence, we need an instrument for competition to sort out these endogeneity issues. We will use mergers of brokerage houses as our instrument on the premise that mergers typically lead to a reduction in analyst coverage on the stocks that were covered by both the bidder and target firms pre merger. If a stock is covered by both firms before the merger, they will get rid of at least one of the analysts, usually the target analyst. It is to these instruments that we now turn.

[6] Indeed, when we run the regression using stock fixed effects, we find that the coefficient in front of coverage is basically zero and statistically insignificant. Since coverage is fairly persistent, it may be that a fixed-effects approach is not picking up the right variation in contrast to the cross-sectional approach. In any event, the endogeneity problem applies unless one can find an instrument.

## IV. Background on Mergers

We begin by providing some background on these mergers. We identify mergers among brokerage houses by relying on information from the SDC Mergers and Acquisition database. We start with the sample of 32,600 mergers of financial institutions. Next, we choose all the mergers where the target company belongs to the four-digit SIC code 6211 ("Investment Commodity Firms, Dealers, and Exchanges"). This screen reduces our sample to 696 mergers. Subsequently, we manually match all the mergers with IBES data. This match identifies 43 mergers with both bidder and target being covered by IBES. Finally, we select only those mergers where both merging houses analyze at least two same stocks – otherwise, there is little scope for our instrumental variables approach below. With this constraint, our search produced 15 mergers, which we break down to parties involved: bidder and target. We provide further details about these mergers in Appendix B.

Of the 15 mergers, six are particularly big in the sense that the merging houses tend to both be big firms and had coverage pre-merger on a large number of similar stocks. The first of these big mergers is Merrill Lynch acquiring in 9/10/1984 a troubled Becker Paribas that was having problems with its own earlier merger to another firm. The second is Paine Webber acquiring Kidder Peabody on 12/31/1994. Kidder was in trouble and had fired a good part of its workforce before the merger and in the aftermath of a major trading scandal involving its government bond trader, Joseph Jett. Kidder's owner, General Electric, wanted to sell the company and Paine Webber (a second tier brokerage house) wanted to buy a top-tier investment bank with a strong research department. The third is Morgan Stanley acquiring Dean Witter Reynolds on

05/31/1997. Morgan-Stanley was portrayed as wanting to get in on the more down-market retail brokerage operations of Dean Witter. The fourth is Smith Barney (Travelers) acquiring Salomon Brothers on 11/28/1997. This is viewed as a synergy play led by Sandy Weill.

The fifth and sixth mergers involved Swiss banks trying to geographically diversify their lines of business into the American market. These mergers happened within a few months of each other. Credit Suisse First Boston acquired Donaldson Lufkin and Jenrette on 10/15/2000. A few months later on 12/10/2000, UBS acquired Paine Webber. These anecdotal descriptions of the motivations for these mergers provide comfort in the exclusion restriction of our proposed instrument, which is that these mergers provide a change in competition that is unrelated to some underlying unobservable of the biases in the stocks.

In Table III, we provide a number of key statistics regarding all fifteen mergers. In Panel A, we summarize the names, dates, and the number of stocks covered by the bidder and target individually and the overlap in the coverage. For instance, in the merger involving Paine Webber and Kidder Peabody, Paine Webber covered 659 stocks and Kidder covered 545 stocks. There was a 234 stock overlap in terms of their coverage. As a result, the merger can potentially lead to a decrease of around one analyst for a large number of stocks. The size of our treatment sample, the number of firms covered by both merging houses, ranges from a low of 5 stocks in the merger involving Fahnestock and Josephthal Lyon and Ross to a high of 327 stocks in the Smith Barner and Salomon Brothers deal. Notice that the big six mergers described above give us much of the variation in terms of the number of treatment stocks. In total, we have a significant treatment sample with which to identify our effect.

To better support the premise that mergers lead to less analyst coverage in the treatment sample via job turnover, we examine career outcomes of analysts employed by merging houses. Panel B presents the results with the breakdown of career outcomes of analysts employed by both the bidder and target house. A few observations can be noted. First, the big mergers affected a very significant number of analysts. The largest of the mergers – between Credit Suisse First Boston and Donaldson Lufkin and Jenrette – concerned almost 200 analysts. The smallest merger in terms of analysts affected is Davidson and Jensen with ten. Given that in our sample the average brokerage house employs approximately 15 analysts, a number of our mergers constituted an important event in the analyst industry.

Second, as expected, mergers generally reduce the number of analysts covering stocks. For example, both brokerage houses, Paine Webber and Kidder Peabody, involved in the first merger employed a total of 101 analysts prior to merger. After Paine Webber acquired Kidder Peabody the employment in the joint entity decreased to 57 analysts. Third, the majority of the employment reduction comes from the closure of the target house. In particular, out of 51 analysts employed by Kidder only 9 of them were retained in the new company, 28 left to a different house, while 14 exited the sample, which we interpret as a firing decision.

In Panel C, we more precisely confirm that for stocks covered by both houses pre-merger, it is usually the analyst in the bidding house that remains while the target analyst is let go. In the first column of Panel C, we report for the treatment sample, stocks covered by both houses, the fraction of that that is covered by the bidder analyst after the merger. In the second column, we report the fraction covered by the target analysts after the merger of the treatment sample. In the Paine Webber and Kidder merger, for stocks covered pre-merger by both houses, it is the target analyst that is indeed the redundant one that gets fired – the corresponding figures are 73.7% for the bidder analysts and only 15.8% for the target analyst. Similarly big gaps exist for most of the mergers. This gap is much smaller in the Davidson and Jensen merger, 50% for the bidder and 50% for the target. Nonetheless, from Panel B, it still appears that there were fewer analysts working for the merged entity than for the sum of the analysts at the two houses beforehand.

## V. Empirical Design

Our analysis of the effect of competition on analyst forecast bias utilizes a natural experiment involving brokerage house mergers. The outcome of such process is the reduction in the number of analysts employed in the combined entity compared to the total number of analysts employed in bidder and target entities prior to merger. As a result, the number of analysts covering a stock that was covered by both houses before the merger (our treatment sample) should drop as one of the redundant analysts is let go or reallocated to another stock (or maybe even both are let go) and thus the competition in the treatment sample decreases. The questions then are whether there is a decrease in competition among analysts around merger events and whether this is associated with an economically significant effect on average consensus bias.

Our empirical methodology requires that we specify a representative window around the merger events. In choosing the proper estimation window we face a trade-off unlike most event studies that would have us focus on a very narrow window. As is the case with most event studies, choosing a window which is too long may incorporate information which is not really relevant for the event in consideration. But in our case, choosing too short of a window means we may lose observations since analysts may not issue forecasts at the same date or with the same frequency. We want to keep a long enough window to look at the change in the performance of all analysts before and after the merger.

To this effect, we decided to use a two-year event window, with one year of data selected for each pre- and post-event period. Most analysts will typically issue at least one forecast within a twelve-month window. Given that in each of the two windows one analyst could issue more than one forecast we retain only the forecast which has the shortest possible time distance from the merger date. In addition, since we are interested in the effect of merger on various analyst characteristics, we require that each stock be present in both windows around the merger. As a result, for every stock we note only two observations – one in each window of the event.

Having chosen this one-year before and one-year after the merger event, one then has to factor in the fact that coverage and the average stock bias may vary from one year to the next one. In other words, to identify how the merger affected coverage in the stocks covered by both houses pre-merger and how the bias in these stocks then also changed, one needs to account for the fact that there may be natural changes from year to year in coverage and bias for these stocks.

A standard approach to deal with these time trends is based on the difference-in-differences (DID) methodology. In this approach, the sample of stocks is divided into treatment and control groups. In the context of our paper, the treatment group includes all stocks that were covered by both brokerage houses before the merger. The control group includes all the remaining stocks. If we denote the average observed characteristics in the treatment (T) and control (C) group in the pre- and post-event period by $C_{T,1}$, $C_{T,2}$, $C_{C,1}$, and $C_{C,2}$, respectively, the partial effect to change due to merger can be estimated as:

$$DID = (C_{T,2} - C_{T,1}) - (C_{C,2} - C_{C,1}) \tag{1}$$

Here the characteristics might be analyst coverage or bias. By comparing the time changes in the means for the treatment and control groups we allow for both group-specific and time-specific effects. This estimator is unbiased under the condition that the merger is not systematically related to other factors that affect $C$.

A potential concern with the above estimator is the possibility that the treatment and control groups may be significantly different from each other and thus the partial effect may additionally capture the differences in characteristics of the different groups. For example, the average stocks in both groups may differ in terms of their market capitalization or value characteristics. To account for such systematic differences across two samples we use the matching technique similar to that used in the context of IPO event studies or characteristic-based asset pricing. In particular, each stock in the treatment sample is matched with its own benchmark portfolio obtained using the sample of stocks in the control group.

We use three different benchmarks. These benchmarks are motivated by the existing literature on the determinants of analyst coverage and bias. For instance, it is well known from Hong, Lim, and Stein (2000) that bigger stocks, higher priced stocks, and stocks with good past return attract more coverage. Indeed, Lim (2001) then finds that these three characteristics are also important for explaining bias as well, as we showed in Table II. We expect our progressively tighter controls to typically do a better job at capturing our true effect by netting out unobserved heterogeneity.

To construct the first benchmark, we sort stocks into tercile portfolios according to their market capitalization. The second benchmark is constructed by sorting stocks into tercile portfolios first according to their market capitalization and subsequently within each size portfolio sorting stocks again according to their book-to-market ratio. This sort results in 9 different benchmark portfolios. Finally, the third benchmark further sorts stocks in each of the 9 portfolios into tercile portfolios according to their past returns, which in turn results in 27 different benchmark portfolios.

For each of the three benchmark specifications, we then construct the benchmark-adjusted *DID* estimators (*BDID*). In particular, for each stock $i$ in the treatment sample the partial effect to change due to merger is calculated as the difference between two components:

$$BDID^i = (C^i_{T,2} - C^i_{T,1}) - (BC^i_{C,2} - BC^i_{C,1}), \tag{2}$$

where the first component is the difference in characteristics of stock $i$ in the treatment sample moving from the pre-merger to post-merger period. The second component is the difference in the average characteristics of the benchmark portfolios that are matched to stock $i$ along the size, size/value, and size/value/momentum dimensions. To assess the average effect for all stocks in the treatment sample, one can then take the average of all individual *BDID*s.

An alternative approach to capture the effect to change in the bias due to merger that we consider is to estimate the following regression model:

$$C_i = \alpha + \beta_1 Merge_i + \beta_2 Affected_i + \beta_3 Merge_i \times Affected_i + \beta_4 Controls_i + \varepsilon_i \qquad (3)$$

where $C$ is the characteristic which may be subject to merger; *Merge* is an indicator variable, equal to one for observations after the merger, and zero, otherwise; *Affected* is an indicator variable equal to one if stock $i$ was affected by the merger, and zero, otherwise; *Controls* is a vector of stock-specific covariates affecting $C$. In this specification, the coefficient of primary interest is $\beta_3$, which captures the partial effect to change due to merger; in the version with additional controls its value is similar in spirit to the DID estimator in equation (2). By including additional controls we account for any systematic differences in stocks, which may affect the partial effect to change due to merger.

One final issue which we need to account for is that a few of the mergers occurred within several months of each other (e.g., the fifth and sixth mergers occurred on 10/15/2000 and 12/10/2000, respectively). As a result, it might be difficult to separate out the effects of these two mergers individually. As the baseline case, we decided for simplicity to treat each merger separately in our analysis. However, we have also played with robustness checks in which we group mergers occurring close together in time and treat them as one merger. For instance, we consider a one-year window before the third merger on 10/15/2000 as the pre-merger period and the one-year window after the fourth merger on 12/10/2000 as the post-merger period. As a result, the treatment sample is the union of 307 stocks jointly covered by Credit Suisse and DLJ and the 213 stocks covered by UBS and Paine Webber. There is potentially some overlap of these two subsets of stocks and hence it might be the case that some of these stocks will experience a greater decline in analyst coverage to the extent that they have more than two redundant analysts. However, these alterations do not affect our baseline results.

Table IV presents summary statistics for the treatment sample in the two-year window around the merger. The characteristics of the treatment sample are similar to those reported in Table I for the OLS sample. For instance, the coverage is about 21 analysts for the typical stock. The mean bias is 2.79% with a standard deviation of around 3.10%. These figures, along with those of the control variables, are fairly similar across these two samples. This provides comfort that we can then relate the economic effect of competition obtained from our treatment sample to the OLS estimates presented in Table II.

## VI. Results

### *A. Analyst Coverage and Optimism Bias*

We first verify the premise of our instrument by measuring the change in analyst coverage for the treatment sample from the year before the merger to the year after. We expect these stocks to experience a decrease in coverage.

Table V reports the results of this analysis. We present the DID estimator for coverage using three different benchmarking techniques – size-matched, size and book-to-market matched, and size, book-to-market, and return matched. Using the size-matched technique, we find an average drop in coverage for the treatment sample of around 1.14 analysts. This effect is significant at the 1% level of significance. The corresponding number for size and book-to-market match is 1.1 analysts, which is also statistically significant at the 1% level. Finally, using the tight matching by size, book-to-market, and past return characteristics, we find a drop in coverage of 1 analyst, which is again very significant. We observe a discernible drop in coverage due to merger using the DID estimator and the level of the drop of between one and two analysts is in line with our expectations.

One can think of this finding as essentially the first stage of our instrumental variables estimation. The effect is economically and statistically significant in the direction predicted, and hence confirming the premise of our natural experiment. We tend to believe the last number, 1 analyst, since it arises from the most conservative benchmarking technique and we will focus on this number in our discussion of the economic effect of competition below.

We next look at how the optimism bias changes for the treatment sample across the mergers. These results are presented in Table VI. We present the findings in the first column for the mean *BIAS* and in the second column for the median *BIAS*. Using the DID estimator with size matching, we find an increase in optimism bias of 0.0019 for the mean bias (significant at the 5% level) and 0.0022 for the median bias (significant at the 1% level). The size and book-to-market matching technique yields slightly smaller estimates of 0.0017 for the mean bias and 0.0020 for the median bias. The mean and median bias estimates are both statistically significant at the 5% level. Finally, using the tighter size, book-to-market, and return match, we find corresponding figures of 0.0017 and 0.0020 for the mean and median bias respectively. These point estimates are not so different from those using only size and book-to-market matches. The statistical significances are also similar.

The results for the effect on bias using an alternative regression approach outlined in equation (3) are presented in Table VII. The first column shows the result using mean bias and the second column show the results for the median bias. The regressions include the usual controls along with merger fixed effects and industry fixed effects. We estimate our regression model using a pooled (panel) regression and calculating standard errors by clustering at the merger level. This approach addresses the concern that the errors, conditional on the independent variables, are correlated within merger groupings (e.g., Moulton (1986)). One reason why this may occur is that the bias occurring in one company may also naturally arise in another company covered by the same house because the broker tends to cover stocks with similar bias pressures.[7]

In the first column, the coefficient of interest in front of *MERGE*×*AFFECTED* is 0.0021, which is significant at the 10% level. The coefficient of interest increases slightly to 0.0022 for median bias and the statistical significance level is 10%. Hence, the results in this table are consistent with those using the DID estimator though the estimates are a bit bigger. Note that the coefficient in front of MERGE is 0.0004 and the one in front of AFFECTED is -0.0007 (both for both mean and median bias). Both are statistically insignificant. This suggests that our treatment sample has very similar bias properties as the control sample and that our natural experiment is a good one. In other words, the bias of the treatment sample really does increase relative to the control sample.

Using the range of the estimates obtained above, a conservative estimate is that the mean optimism bias increases by 17 basis points (as a result of reducing coverage by 1 analyst). As we mentioned earlier, the sample for the natural experiment is similar to that of the OLS by construction – the typical stock has a bias of around 2.7% and the standard deviation of the optimism bias is also around 3%. So, this means that the

[7] We have also considered other dimensions of clustering: clustering by industry, by stock, by time, and by time and industry. All of them produced standard errors that were lower than the ones we report.

estimate of the competitive effect from our natural experiment is anywhere from three times as large to eight times as large as the OLS estimates. This is a sizeable difference and suggests that the OLS estimates are biased downwards, consistent with the documented selection bias that stocks that attract lots of coverage are likely to have more optimistic analysts.

### *B. Robustness Checks*

We conduct a number of robustness checks. The first is to separately estimate our effect using the six biggest mergers. The results are very similar in that the conservative estimates are a 1 analyst drop in coverage associated with a 0.0017 increase in bias. Second, we estimate our effect separately for each of the fifteen mergers. Each of the fifteen mergers experienced a decline in coverage using the most conservative DID estimate. Hence, our result is not driven by outliers – there is a distinct coverage drop with mergers. Clearly, the fact that fifteen out of fifteen mergers experienced a drop suggests that our effect is robustly significant in a non-parametric sense. Similarly, we find that twelve (thirteen) of the fifteen mergers experienced an increase in mean (median) bias using the most conservative DID estimate. It is important to emphasize that since these mergers occur throughout our entire sample our effects are not due to any particular macro-economic event such as a recession or boom.

Third, we worry that our mean bias effect might be driven by selection due to which one of the two analysts from the merging firms covering the stock gets fired. It might be that the less optimistic analyst gets fired and hence the bias might be higher as a result. Another possibility could be that analysts employed by the merging houses may compete for the job in the new merged house and thus they may strategically change their reporting behavior. We deal with these issues in two ways. The first is simply to check whether the merging brokerage houses selectively fire analysts who are less optimistic since we have turnover data. We did not find such a selection bias. The second and more direct way to deal with this is that we only look at the change in the bias for the analysts covering the same stocks but not employed by the merging firms. The findings are in Table VIII. We report the change in bias for the treatment sample but now the bias is calculated using only the forecasts of the analysts not employed by the merging houses. The figures are very similar to the main findings – only slightly smaller in some instances by a negligible amount. Collectively, these findings provide comfort that our main results are not spuriously driven by some outliers or by selection biases.

Finally, we worry that the bias change we are capturing may result from the difference in the timeliness of the forecasts issued pre-merger compared to post-merger. In particular, empirical evidence suggests that analyst bias is more pronounced the farther out is the forecast. Indeed, there is a tendency for an analyst to under-shoot the earnings number for forecasts issued near the earnings date. We checked that there was no difference in the timeliness of the forecasts issued pre-merger as compared to post-merger. We omit these results for brevity.

*C. Key Auxiliary Prediction*

We next test a key auxiliary prediction that will further buttress our identification strategy. We check to see whether the competition effect is more pronounced for stocks with smaller analyst coverage. The idea is that the more analysts cover a stock, the less the loss of an additional analyst matters (i.e. akin to the Cournot view of competition). If collusion is possible, then we might expect a non-linear relationship between bias and coverage. Suppose that collusion is easier when there are only a few analysts. Under this scenario going from one to two analysts may not have an effect because the two can collude. And we might find more of an effect when going from five to six analysts if the sixth analyst does not collude. With collusion, it might be that we expect the biggest effect for stocks covered by a moderate number of analysts – i.e., an inverted u-shape with the effect being the biggest for medium coverage stocks.

We examine this issue in Table IX using the same DID framework as before with the matching done along the size, book-to-market, and return dimensions. We divide initial coverage into three groups: less than or equal to five analysts, between 6 and 20 analysts, and greater than 20 analysts. The first column reports the results using mean bias. We expect and find that the effect is significantly smaller when there are a lot of analysts covering. The effect is greatest for the first group (less than 6 analysts), followed by the second group (between 6 and 20), and the effect is much smaller for the biggest group. The evidence is similar when we use median bias, as presented in the second column. In sum, the evidence in Table IX is remarkably comforting as it conforms well to our priors on competition being more important when there are fewer analysts around. This result reassures us that our instrumental-variables estimation is a sensible one.

*D. Change in Long-Term Growth Forecasts and Recommendations*

In this paper, we focus on annual earnings forecasts since these are the key numbers that the market looks to and every analyst has to submit such a forecast. For completeness, we also look at how long-term growth forecasts and stock recommendations change for the treatment sample in comparison to the control sample around these mergers. One downside is that data in this case is more sparse as analysts do not issue as many timely growth forecasts or recommendations. Moreover, we cannot measure bias in the same way since there are no actual earnings forecasts to make the comparison to. However, we can gauge the extent to which the average long-term forecast or recommendation changes for our treatment sample (provided data is available) across the merger date compared to the control group. To the extent that there is less competition as a result of these mergers, we expect forecasts for percentage growth to be higher after the merger and for there to have more positive recommendations.

The results for the long-term growth forecasts and recommendations are in Table X. Panel A reports the results for long-term growth forecasts (which is the percentage long-term growth in earnings). Using the most conservative benchmark, we see that long-term growth forecasts increase by 56 bps after the merger using mean forecasts and 41 bps using median forecasts. The mean long-term growth forecast in the treatment sample is 14% with a standard deviation of 6%. So, a one-analyst drop in coverage in our treatment sample results in an increase in the mean long-term growth forecast that is about nine percent of a standard deviation of these forecasts. This is both an economically and statistically significant effect.

Panel B reports the results using recommendations. Recommendations are given in terms of the following five rankings: strong sell, sell, hold, buy, and strong buy. We convert these into a score of 1 for strong sell, 2 for sell, 3 for hold, 4 for buy, and 5 for strong buy. We then take the average and median of these recommendation scores and look at how they vary for the treatment sample and the control group across the merger date. Using again the most conservative benchmark, the merger event is associated with an increase in the average recommendation score for the treatment sample of 0.05 using the mean score and 0.09 using the median score. The result using the mean score is not statistically significant, but the result using the median score is statistically significant at the 10% level. However, both estimates imply quite significant economic effects. The mean score for the treatment sample is 3.87 with a standard deviation of 0.44. Hence, we find that a one analyst drop in coverage leads to about a 20% (10%) increase in the median (mean) recommendation score as a fraction of the standard deviation of these recommendations. In sum, we conclude from this table that our baseline results using annual forecasts are robust to different measures of bias.

*E. Change in Forecast Dispersion and Forecast Error*

Finally, we look at how the forecast dispersion and mean forecast error change along with the increase in forecast bias. Our competition effect in theory has ambiguous implications for the directional change in forecast dispersion and error. On one hand, one might expect forecast dispersion to increase as the mean bias increases since there is more leeway for analysts to issue whatever forecast they want. On the other hand, it might be that with less competition, all analysts will issue similarly optimistic forecasts without any checks on what they say. Moreover, there are other theories of competition in analyst forecasts that would predict a strategic reason for why analyst dispersion might decrease with less competition. For instance, Laster, Bennett, and Geoum (1999) and Ottaviani and Sorensen (2005) propose a model in which forecasters in a rank-order contest based on accuracy differentiate themselves in their forecasts strategically a la Hotelling in equilibrium, and so less competition may lead to less differentiation or dispersion.

We find that forecast dispersion falls across the mergers, consistent with the latter theories that predict that dispersion increases with competition. The results are presented in Table XI. Panel A presents the DID estimators. For each of the benchmarks, there is a discernible drop in dispersion. For the size, book-to-market, and return matched benchmark, the DID estimate is –0.0006 and it is significant at the 10% level of significance. The cross-sectional standard deviation of *FDISP* is around 0.94, so this is a drop of about 6% relative to the cross-sectional standard deviation. Panel B presents the results using the regression method. The coefficient in front of *MERGE*×*AFFECTED* is 0.0001 but is not statistically significant. So these findings suggest that as there is less competition, analysts seem to be issuing similarly optimistic forecasts without any checks. But the results are less robust compared to the bias findings since the regression results are not statistically significant. In sum, our findings are consistent with a number of theories that predict that dispersion increases with competition. In particular, it might be tantalizing evidence of strategic behavior to differentiate with more competition in the face of rewards for relative accuracy. This is not an unreasonable perspective and completely consistent with our bias results. However, it is difficult to differentiate among all these competing stories for the effect of competition on dispersion.

The effect of competition on forecast accuracy is also ambiguous in theory. On one hand, we naturally expect the mean forecast error to increase to the extent that the remaining analysts are now more biased. On the other hand, there is heterogeneity in analyst forecasts (i.e., dispersion), with some analysts being persistently negative or pessimistically biased and some being optimistically biased. In this paper, we do not deal with the why there is this heterogeneity. This dispersion might come from strategic reasons outlined above. Or it might come from analysts using different models (see Hong, Stein, and Yu (2007)). If dispersion is decreasing with less competition as found above, this may naturally lead to an association of improved accuracy. In other words, there are two offsetting effects of competition on accuracy: (1) less competition means less accuracy since analysts can be as optimistic as they want; (2) less competition means less forecast dispersion and hence more accuracy.

The results are presented in Table XII. It turns out that forecast accuracy does indeed deteriorate. The economic and statistical significance, however, is sensitive to the estimation procedure. In Panel A, we present the DID estimators for the change in both the mean and median forecast accuracy. For each of our benchmarks, we see a decrease in forecast accuracy, with the effect typically larger and more significant for medians than for means. For instance, using the DID estimator with size, book-to-market, and return benchmarking, there is an increase of about 0.0007 for the median forecast accuracy. Since the cross-sectional standard deviation of forecast accuracy is 0.03, this is a decrease of about 2 percent. These estimates are statistically insignificant. In Panel B, we present the results using the regression approach. Here, the coefficient in front of *MERGE*×*AFFECTED* for mean forecast error is 0.0023 (significant at the 5% level) and the coefficient for median forecast error is 0.0021 (significant at the 10% level).

**VII. Conclusion**

We attempt to measure the effect of competition on bias in the context of analyst earnings forecasts, which are known to be excessively optimistic due to conflicts of interest. Using cross-sectional regressions, the existing literature finds that stocks with more analyst coverage, and presumably competition, have less biased forecasts on average. However, these OLS estimates are biased since analyst coverage is endogenous. We propose an instrument for competition – namely, mergers of brokerage houses, which result in the firing of analysts because of redundancy and other reasons including culture clash or general merger turmoil. We use this decrease in analyst coverage for stocks covered by both merging houses before the merger (the treatment sample) to measure the causal effect of competition on bias. We find the treatment sample simultaneously experiences a decrease in analyst coverage and an increase in optimism bias the year after the merger relative to a control group of stocks. Our findings suggest that competition reduces analyst optimism bias. Moreover, the economic effect from our IV estimates is larger than that from the OLS estimates by a factor of several times.

Our instrument for analyst coverage can also be useful in other settings. Namely, it can be used to identify the effect of analyst coverage on stock prices. There is a large literature in finance and accounting that have tried to pin down whether analyst coverage increases stock prices and the mechanisms through which this might happen. These studies are typically biased because of endogeneity as analysts tend to cover high priced, high performing, or large stocks for a variety of reasons. In other words, the causality might be reversed. Our instrument can hence be used to identify the causal effect of coverage on stock prices. Recent interesting research in the spirit of our instrument is Kelly and Ljungqvist (2007) who uses closures of brokerage houses as a source of exogenous variation in coverage. We anticipate more exciting work will be done along this vein.

## Appendix A

We consider a static set-up in which an analyst trades off the rewards for accuracy versus bias. The empirical motivation for the model below comes from the large literature on conflicts of interest cited in the introduction. Implicit in models of bias (and hence our model) is that many investors (e.g. retail) cannot easily de-bias and also reward analysts based on relative accuracy. In particular, Hong and Kubik (2003) find that analysts' incentives (at least through job separations) depend both on accuracy and also optimism bias. Analysts' career outcomes depend equally on both factors. These findings also accord well with voluminous anecdotes on the incentives of sell-side analysts in the press also cited in the introduction.

To model this conflict of interest, we assume that an analyst's wage is equal to

$$w = \overline{w} - \alpha(n)(\hat{x} - x)^2 - \beta(n)(\hat{x} - B)^2 \qquad \text{(A.1)}$$

where

$\overline{w}$ is her fixed wage, $\alpha(n)$ is the weight of accuracy in determining her wage (which as we discuss below will depend on the number of analysts), $\hat{x}$ is her forecast, $x$ is the earnings, $\beta(n)$ is the weight of bias in determining her wage, and $B$ is an exogenously given target that the analyst has an incentive to hit.

For simplicity, we assume that the earnings $x$ is normally distributed with mean zero and precision $\tau_0$. We further assume that the analyst receives a signal $s = x + \varepsilon$, where $\varepsilon$ is normally distributed with mean zero and precision $\tau$. And finally, we assume that the target $B = E[x \mid s] + \overline{B}$, where $E[x \mid s]$ is the analyst's conditional expectation of earnings given his signal, and $\overline{B}$ is a positive constant.

The analyst maximizes her expected wage given her signal $s$, which is equivalent to her minimizing with respect to $\hat{x}$

$$\alpha(n)E[(\hat{x} - x)^2 \mid s] + \beta(n)E[(\hat{x} - B)^2 \mid s] \qquad \text{(A.2)}$$

It is easy to rewrite this maximization problem as $b$

$$\text{Min w.r.t } b \; [\alpha(n)\{b^2 + Var[x \mid s]\} + \beta(n)(b - \overline{B})^2] \qquad \text{(A.3)}$$

where $b = \hat{x} - E[x \mid s]$ is the conditional bias. The solution is given by

$$b = \frac{\beta(n)}{\alpha(n) + \beta(n)}\overline{B}\,. \qquad \text{(A.4)}$$

Notice that if $\beta(n) = 0$ (only accuracy matters and not bias) then $b = 0$. Alternatively, if $\alpha(n) = 0$ (only bias matters and not accuracy), then $b = \overline{B}$.

As we explained in the introduction, one perspective on competition is that an analyst has to weigh accuracy more because of relative performance evaluation. For instance, imagine that there is no competition. Then, the analyst doesn't have to worry about accuracy since she is the only game in town. An extra analyst who provides an independent forecast will force her to weigh accuracy more to the extent that investors punish an analyst who is too far off from the consensus. Now, analysts could collude on issuing optimistic forecasts. So, to the extent that competition makes it more difficult for analysts to collude and issue optimistic forecasts, then we expect competition to make forecasts less upward biased. These scenarios correspond to $\alpha'(n) > 0$ and $\beta'(n) = 0$. However, competition could have the other effect which is to make analysts issue more upwardly biased forecasts to the extent that it is needed to get customers or access to the firm, i.e. $\beta'(n) > 0$. As such, our empirical estimates are capturing the net of these competing effects.

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## Table I
## Summary Statistics on the IBES Sample

We consider a sample of stocks covered by IBES during the period 1980-2005 with valid annual earnings forecast records. $COVERAGE_{it}$ is a measure of analyst coverage, defined as the number of analysts covering firm *i* at the end of year *t*. Analyst forecast bias ($BIAS_{jt}$) is the difference between the forecast analyst *j* in year *t* and the actual *EPS*, expressed as a percentage of the previous year's stock price. The consensus bias is expressed as a mean or median bias among all analysts covering a particular stock. Analyst forecast error ($FERROR_{jt}$) is the absolute difference between the forecast analyst *j* in year *t* and the actual *EPS*, expressed as a percentage of the previous year's stock price. The forecast error is expressed as a mean or median bias among all analysts covering a particular stock. $FDISP_{it}$ is analyst forecast dispersion, defined as the standard deviation of all analyst forecasts covering firm *i* in year *t*. $LNSIZE_{it}$ is the natural logarithm of firm *i*'s market capitalization (price times shares outstanding) at the end of year *t*. $SIGMA_{it}$ is the variance of daily (simple, raw) returns of stock *i* in year *t*. $RETANN_{it}$ is the average monthly return on stock *i* in year *t*. $LNBM_{it}$ is the natural logarithm of firm *i*'s book value divided by its market cap at the end of year *t*. To measure the volatility of *ROE* (*VOLROE*), we estimate an AR(1) model for each stock's *ROE* using a 10-year series of the company's valid annual *ROE*s. $ROE_{it}$ is firm *i*'s return on equity in year *t*. *ROE* is calculated as the ratio of earnings in year *t* over the book value of equity. We calculate *VOLROE* as the variance of the residuals from this regression. $PROFIT_{it}$ is the profitability of company *i* at the end of year *t*, defined as operating income over book value of assets. We exclude observations that fall to the left of the $25^{th}$ percentile of the size distribution, observations with stock prices lower than \$5, and those for which the absolute difference between forecast value and the true earnings exceeds \$10.

| Variable | Cross-sectional mean | Cross-sectional median | Cross-sectional st. dev. |
|---|---|---|---|
| $COVERAGE_{i,t}$ | 21.45 | 21 | 9.57 |
| Mean $BIAS_{i,t}$ (in %) | 2.70 | 2.10 | 3.10 |
| Median $BIAS_{i,t}$ (in %) | 2.64 | 2.01 | 3.17 |
| Mean $FERROR_{i,t}$ (in %) | 3.31 | 2.39 | 2.93 |
| Median $FERROR_{i,t}$ (in %) | 3.24 | 2.26 | 3.00 |
| $FDISP_{i,t}$ (in %) | 0.75 | 0.41 | 1.02 |
| $LNSIZE_{i,t}$ | 8.38 | 8.38 | 1.62 |
| $SIGMA_{i,t}$ (in %) | 40.72 | 35.04 | 21.03 |
| $RETANN_{i,t}$ (in %) | 1.73 | 1.49 | 4.04 |
| $LNBM_{i,t}$ | -1.02 | -0.92 | 0.88 |
| $VOLROE_{i,t}$ (in %) | 26.53 | 10.43 | 19.79 |
| $PROFIT_{i,t}$ (in %) | 15.48 | 15.29 | 9.38 |

# Table II

## Regression of Consensus Forecast Bias on Company Characteristics

The dependent variable is *BIAS*, defined as a consensus forecast bias of all analysts tracking stock *i* in year *t*. Forecast bias is the difference between the forecast of analyst *j* in year *t* and the actual *EPS*, expressed as a percentage of the previous year's stock price. The consensus is obtained either as a mean or median bias. $COVERAGE_{i,t}$ is a measure of analyst coverage, defined as the number of analysts covering firm *i* at the end of year *t*. $LNSIZE_{i,t}$ is the natural logarithm of firm *i*'s market capitalization (price times shares outstanding) at the end of year *t*. $SIGMA_{it}$ is the variance of daily (simple, raw) returns of stock *i* during year *t*. $RETANN_{i,t}$ is the average monthly return on stock *i* in year *t*. $LNBM_{i,t}$ is the natural logarithm of firm *i*'s book value divided by its market cap at the end of year *t*. To measure the volatility of *ROE* (*VOLROE*), we estimate an AR(1) model for each stock's *ROE* using a 10-year series of the company's valid annual *ROE*s. $ROE_{i,t}$ is firm *i*'s return on equity in year *t*. *ROE* is calculated as the ratio of earnings in year *t* over the book value of equity. *VOLROE* is the variance of the residuals from this regression. $PROFIT_{i,t}$ is the profitability of company *i* at the end of year *t*, defined as operating income over book value of assets. $SP500_{i,t}$ is an indicator variable equal to one if stock *i* is included in the S&P500 index in year *t*. We exclude all observations that fall to the left of the 25th percentile of the size distribution, observations with stock prices lower than $5, and those for which the absolute difference between forecast value and the true earnings exceeds $10. All regressions include three-digit SIC industry fixed effects. Standard errors (in parentheses) are clustered at the industry level. ***, **, * denotes 1%, 5%, and 10% statistical significance.

| Variables\Model | Mean BIAS | | Median BIAS | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $COVERAGE_{i,t-1}$ | -0.0006*** | -0.0002** | -0.0006*** | -0.0002*** |
| | (0.0001) | (0.0001) | (0.0001) | (0.0001) |
| $LNSIZE_{i,t-1}$ | 0.0065*** | 0.0028*** | 0.0065*** | 0.0028*** |
| | (0.0008) | (0.0009) | (0.0008) | (0.0008) |
| $SIGMA_{i,t-1}$ | 0.0098** | -0.0095 | 0.0091* | -0.0097 |
| | (0.0047) | (0.0061) | (0.0047) | (0.0060) |
| $RETANN_{i,t-1}$ | -0.0852*** | -0.1001*** | -0.0827*** | -0.0988*** |
| | (0.0169) | (0.0199) | (0.0162) | (0.0193) |
| $LNBM_{i,t-1}$ | 0.0124*** | 0.0121*** | 0.0121*** | 0.0118*** |
| | (0.0016) | (0.0016) | (0.0016) | (0.0016) |
| $VOLROE_{i,t-1}$ | 0.0058*** | 0.0062*** | 0.0057*** | 0.0060*** |
| | (0.0020) | (0.0019) | (0.0019) | (0.0019) |
| $PROFIT_{i,t-1}$ | 0.0544*** | 0.0577*** | 0.0541*** | 0.0576*** |
| | (0.0100) | (0.0095) | (0.0103) | (0.0098) |
| $SP500_{i,t-1}$ | -0.0116*** | -0.0111*** | -0.0116*** | -0.0110*** |
| | (0.0024) | (0.0025) | (0.0024) | (0.0025) |
| Year Fixed Effects | No | Yes | No | Yes |
| Industry Fixed Effects | Yes | Yes | Yes | Yes |
| Observations | 9313 | 9313 | 9313 | 9313 |

Table III

Descriptive Statistics for Mergers

Panel A includes the names of brokerage houses involved in mergers, the date of the merger, and the number of stocks covered by either brokerage house or both of them prior to the merger. Panel B breaks down the merger information at the analyst level. We include number of analysts employed in the merging brokerage houses prior to merger and after the merger as well as the detailed information on the career outcomes of the analysts after the merger. Panel C calculates the percentage of analysts from the merging houses that cover the same stock after the merger. We restrict our sample of stocks to those which were covered by bother the bidder and the target house.

*Panel A: Mergers Used in the Analysis and Stocks Covered*

| | Merger number | Merger Date | # Stocks (Bidder) | # Stocks (Target) | # Stocks (Bidder and Target) |
|---|---|---|---|---|---|
| Merrill Lynch | 1 | 9/10/1984 | 762 | | 173 |
| Becker Paribas | | | | 288 | |
| Paine Webber | 2 | 12/31/1994 | 659 | | 234 |
| Kidder Peabody | | | | 545 | |
| Morgan Stanley | 3 | 05/31/1997 | 739 | | 251 |
| Dean Witter Reynolds | | | | 470 | |
| Smith Barney (Travelers) | 4 | 11/28/1997 | 914 | | 327 |
| Salomon Brothers | | | | 721 | |
| Credit Suisse First Boston | 5 | 10/15/2000 | 856 | | 307 |
| Donaldson Lufkin and Jenrette | | | | 595 | |
| UBS Warburg Dillon Read | 6 | 12/10/2000 | 596 | | 213 |
| Paine Webber | | | | 487 | |
| Chase Manhattan | 7 | 12/31/2000 | 487 | | 80 |
| JP Morgan | | | | 415 | |
| Wheat First Securities | 8 | 10/31/1988 | 178 | | 8 |
| Butcher & Co | | | | 66 | |
| EVEREN Capital | 9 | 1/9/1998 | 178 | | 17 |
| Principal Financial Securities | | | | 142 | |
| DA Davidson & Co | 10 | 2/17/1998 | 76 | | 8 |
| Jensen Securities | | | | 53 | |
| Dain Rauscher | 11 | 4/6/1998 | 360 | | 26 |
| Wessels Arnold & Henderson | | | | 135 | |
| First Union | 12 | 10/1/1999 | 274 | | 21 |
| EVEREN Capital | | | | 204 | |
| Paine Webber | 13 | 6/12/2000 | 516 | | 28 |
| JC Bradford | | | | 182 | |
| Fahnestock | 14 | 9/18/2001 | 117 | | 5 |
| Josephthal Lyon & Ross | | | | 91 | |
| Janney Montgomery Scott | 15 | 3/22/2005 | 116 | | 10 |
| Parker/Hunter | | | | 54 | |

*Panel B: Career Outcomes of Analysts after Mergers*

| Broker | Merger Number | # Analysts Prior | After | # Analysts After Merger Retained in the House | Left to Another House | Exited Sample (Fired) | New Analysts |
|---|---|---|---|---|---|---|---|
| Merrill Lynch | 1 | 90 | 98 | 84 | 0 | 5 | 13 |
| Becker Paribas | | 27 | - | 1 | 11 | 15 | - |
| Paine Webber | 2 | 50 | 57 | 42 | 1 | 7 | 6 |
| Kidder Peabody | | 51 | - | 9 | 28 | 14 | - |
| Morgan Stanley | 3 | 70 | 92 | 61 | 2 | 7 | 26 |
| Dean Witter Reynolds | | 35 | - | 5 | 16 | 14 | - |
| Smith Barney (Travelers) | 4 | 91 | 140 | 70 | 6 | 15 | 27 |
| Salomon Brothers | | 76 | - | 43 | 20 | 13 | - |
| Credit Suisse First Boston | 5 | 120 | 146 | 93 | 5 | 22 | 35 |
| Donaldson Lufkin Jenrette | | 77 | - | 18 | 17 | 42 | - |
| UBS Warburg Dillon Read | 6 | 94 | 118 | 80 | 5 | 9 | 0 |
| Paine Webber | | 64 | - | 38 | 8 | 17 | - |
| Chase Manhattan | 7 | 64 | 106 | 48 | 5 | 11 | 24 |
| JP Morgan | | 50 | - | 34 | 1 | 15 | - |
| Wheat First Securities | 8 | 13 | 21 | 13 | 0 | 0 | 8 |
| Butcher & Co Inc | | 13 | - | 3 | 3 | 7 | - |
| EVEREN Capital | 9 | 27 | 31 | 21 | 4 | 2 | 8 |
| Principal Financial Securities | | 18 | - | 2 | 6 | 10 | - |
| DA Davidson & Co | 10 | 6 | 8 | 4 | 1 | 1 | 0 |
| Jensen Securities | | 4 | - | 4 | 0 | 0 | - |
| Dain Rauscher | 11 | 39 | 36 | 19 | 9 | 11 | 6 |
| Wessels Arnold & Henderson | | 15 | - | 11 | 0 | 4 | - |
| First Union | 12 | 35 | 54 | 26 | 2 | 7 | 16 |
| EVEREN Capital | | 32 | - | 12 | 0 | 20 | - |
| Paine Webber | 13 | 54 | 55 | 37 | 9 | 8 | 18 |
| JC Bradford | | 22 | - | 0 | 0 | 22 | - |
| Fahnestock | 14 | 14 | 16 | 7 | 1 | 6 | 9 |
| Josephthal Lyon & Ross | | 14 | - | 0 | 0 | 14 | - |
| Janney Montgomery Scott | 15 | 13 | 15 | 11 | 1 | 1 | 3 |
| Parker/Hunter | | 5 | - | 1 | 0 | 4 | - |

*Panel C: Percentage of Stocks Covered by Analysts from Bidder and Target Houses after Mergers*

| Merger | Percentage of Stocks (Bidder) | Percentage of Stocks (Target) |
|---|---|---|
| (1) | 85.7 | 1.1 |
| (2) | 73.7 | 15.8 |
| (3) | 66.3 | 5.4 |
| (4) | 50.0 | 30.7 |
| (5) | 63.7 | 12.3 |
| (6) | 67.8 | 32.3 |
| (7) | 45.3 | 32.1 |
| (8) | 61.9 | 14.3 |
| (9) | 67.7 | 6.5 |
| (10) | 50.0 | 50.0 |
| (11) | 52.8 | 30.6 |
| (12) | 48.1 | 22.2 |
| (13) | 67.3 | 0 |
| (14) | 43.8 | 0 |
| (15) | 73.3 | 6.7 |

Table IV

Summary Statistics for the Treatment Sample

We consider all stocks covered by two merging brokerage houses around the one-year merger event window. $COVERAGE_{it}$ is a measure of analyst coverage, defined as the number of analysts covering firm *i* at the end of year *t*. Analyst forecast bias ($BIAS_{jt}$) is the difference between the forecast analyst *j* at time *t* and the actual *EPS*, expressed as a percentage of the previous year's stock price. The consensus bias is expressed as a mean or median bias among all analysts covering a particular stock. Analyst forecast error ($FERROR_{jt}$) is the absolute difference between the forecast analyst *j* at time *t* and the actual *EPS*, expressed as a percentage of the previous year's stock price. The forecast error is expressed as a mean or median bias among all analysts covering a particular stock. $FDISP_{it}$ is analyst forecast dispersion, defined as the standard deviation of all analyst forecasts covering firm *i* at time *t*. $LNSIZE_{it}$ is the natural logarithm of firm *i*'s market capitalization (price times shares outstanding) at the end of year *t*. $SIGMA_{it}$ is the variance of daily (simple, raw) returns of stock *i* during year *t*. $RETANN_{it}$ is the average monthly return on stock *i* during year *t*. $LNBM_{it}$ is the natural logarithm of firm *i*'s book value divided by its market cap at the end of year *t*. To measure the volatility of *ROE* (*VOLROE*), we estimate an AR(1) model for each stock's *ROE* using a 10-year series of the company's valid annual *ROE*s. $ROE_{it}$ is firm *i*'s return on equity in year *t*. *ROE* is calculated as the ratio of earnings during year *t* over the book value of equity. We calculate *VOLROE* as the variance of the residuals from this regression. $PROFIT_{it}$ is the profitability of company *i* at the end of year *t*, defined as operating income over book value of assets. We exclude observations with stock prices lower than $5 and those for which the absolute difference between forecast value and the true earnings exceeds $10.

| Variable | Cross-sectional mean | Cross-sectional median | Cross-sectional st. dev. |
|---|---|---|---|
| $COVERAGE_{i,t}$ | 21.12 | 20 | 9.45 |
| Mean $BIAS_{i,t}$ (in %) | 2.79 | 2.24 | 3.10 |
| Median $BIAS_{i,t}$ (in %) | 2.74 | 2.21 | 3.19 |
| Mean $FERROR_{i,t}$ (in %) | 3.40 | 2.52 | 2.90 |
| Median $FERROR_{i,t}$ (in %) | 3.33 | 2.43 | 2.99 |
| $FDISP_{i,t}$ (in %) | 0.75 | 0.40 | 0.94 |
| $LNSIZE_{i,t}$ | 8.39 | 8.37 | 1.60 |
| $SIGMA_{i,t}$ (in %) | 41.00 | 35.86 | 21.02 |
| $RETANN_{i,t}$ (in %) | 1.74 | 1.52 | 4.13 |
| $LNBM_{i,t}$ | -1.03 | -0.92 | 0.91 |
| $VOLROE_{i,t}$ (in %) | 25.32 | 9.89 | 43.40 |
| $PROFIT_{i,t}$ (in %) | 15.52 | 15.25 | 9.22 |

## Table V
## Change in Stock-Level Coverage: DID Estimator

We measure analyst coverage as the number of analysts covering firm *i* at the end of year *t*. For all mergers, we split the sample of stocks into those covered by both merging brokerage houses (treatment sample) and those not covered by both houses (control sample). We also divide stocks into pre-merger period and post-merger period (one-year window for each period). For each period we further construct benchmark portfolios using the control sample based on stocks' size (*SIZE*), book-to-market ratio (*BM*), and average past year's returns (*RET*). Our benchmark assignment involves three portfolios in each category. Each stock in the treatment sample is then assigned to its own benchmark portfolio (*SIZE*-matched, *SIZE/BM*-matched, and *SIZE/BM/RET*-matched). Next, for each period, we calculate the cross-sectional average of the differences in analyst stock coverage across all stocks in the treatment sample and their respective benchmarks. Finally, we calculate the difference in differences between post-event period and pre-event period (DID Estimator). Our sample excludes observations with stock prices lower than $5 and those for which the absolute difference between forecast value and the true earnings exceeds $10. Standard errors (in parentheses) are clustered at the merger groupings. ***, **, * denotes 1%, 5%, and 10% statistical significance.

| | |
|---|---|
| DID Estimator (*SIZE*-Matched) | -1.144*** |
| | (0.218) |
| DID Estimator (*SIZE/BM*-Matched) | -1.096*** |
| | (0.187) |
| DID Estimator (*SIZE/BM/RET*-Matched) | -1.034*** |
| | (0.188) |

Table VI

Change in Forecast Bias: DID Estimator

We measure analyst forecast bias ($BIAS_{jt}$) as the difference between the forecast analyst *j* at time *t* and the actual *EPS*, expressed as a percentage of the previous year's stock price. The consensus bias is expressed as a mean or median bias among all analysts covering a particular stock. For all mergers, we split the sample of stocks into those covered by both merging brokerage houses (treatment sample) and those not covered by both houses (control sample). We also divide stocks into pre-merger period and post-merger period (one-year window for each period). For each period we further construct benchmark portfolios using the control sample based on stocks' size (*SIZE*), book-to-market ratio (*BM*), and average past year's returns (*RET*). Our benchmark assignment involves three portfolios in each category. Each stock in the treatment sample is then assigned to its own benchmark portfolio (*SIZE*-matched, *SIZE*/*BM*-matched, and *SIZE*/*BM*/*RET*-matched). Next, for each period, we calculate the cross-sectional average of the differences in analyst forecast bias across all stocks in the treatment sample and their respective benchmarks. Finally, we calculate the difference in differences between post-event period and pre-event period (DID Estimator). Our sample excludes observations with stock prices lower than $5 and those for which the absolute difference between forecast value and the true earnings exceeds $10. Standard errors (in parentheses) are clustered at the merger groupings. ***, **, * denotes 1%, 5%, and 10% statistical significance.

| | Mean BIAS | Median BIAS |
|---|---|---|
| DID Estimator (SIZE-Matched) | 0.0019** | 0.0022*** |
| | (0.0007) | (0.0007) |
| DID Estimator (SIZE/BM-Matched) | 0.0017** | 0.0020** |
| | (0.0007) | (0.0007) |
| DID Estimator (SIZE/BM/RET-Matched) | 0.0017** | 0.0020** |
| | (0.0007) | (0.0007) |

## Table VII
## Change in Forecast Bias: Regression Evidence

The dependent variable is forecast bias (*BIAS*), defined as the difference between forecasted earnings and actual earnings, adjusted for the past year's stock price. For each merger, we consider a one-year window prior to merger (pre-event window) and a one-year window after the merger (post-event window). We construct an indicator variable (*MERGE*) equal to one for the post-event period and zero for the pre-event period. For each merger window, we assign an indicator variable (*AFFECTED*) equal to one for each stock covered by both merging brokerage houses (treatment sample) and zero otherwise. *LNSIZE* is a natural logarithm of the market cap of the stock; $SIGMA_{it}$ is the variance of daily (simple, raw) returns of stock *i* during year *t*; *RETANN* is annual return on the stock; *LNBM* is a natural logarithm of the book to market ratio; *COVERAGE* denotes the number of analysts tracking the stock. To measure the volatility of *ROE* (*VOLROE*), we estimate an AR(1) model for each stock's *ROE* using a 10-year series of the company's valid annual *ROE*s. $ROE_{it}$ is firm *i*'s return on equity in year *t*. *ROE* is calculated as the ratio of earnings in year *t* over the book value of equity. *VOLROE* is the variance of the residuals from this regression. $PROFIT_{it}$ is the profitability of company *i* at the end of year *t*, defined as operating income over book value of assets. *SP500* is an indicator variable equal to one if a stock is included in the S&P500 index. We also include three-digit SIC industry fixed effects and merger fixed effects. We include results based on both mean and median bias. Standard errors (in parentheses) are clustered at the merger groupings. ***, **, * denotes 1%, 5%, and 10% statistical significance.

| | Mean BIAS | Median BIAS |
|---|---|---|
| $MERGE_i$ | 0.0004 | 0.0004 |
| | (0.0009) | (0.0009) |
| $AFFECTED_i$ | -0.0007 | -0.0007 |
| | (0.0015) | (0.0014) |
| $MERGE_i$*$AFFECTED_i$ | 0.0021* | 0.0022* |
| | (0.0011) | (0.0012) |
| $LNSIZE_{i,t-1}$ | 0.0007 | 0.0008* |
| | (0.0004) | (0.0004) |
| $SIGMA_{i,t-1}$ | -0.0048* | -0.0050** |
| | (0.0024) | (0.0022) |
| $RETANN_{i,t-1}$ | 0.0115 | 0.0184** |
| | (0.0076) | (0.0073) |
| $LNBM_{i,t-1}$ | 0.0110*** | 0.0109*** |
| | (0.0005) | (0.0005) |
| $COVERAGE_{i,t-1}$ | -0.0001** | -0.0002*** |
| | (0.0001) | (0.0001) |
| $VOLROE_{i,t-1}$ | 0.0045*** | 0.0045*** |
| | (0.0006) | (0.0006) |
| $PROFIT_{i,t-1}$ | 0.0870*** | 0.0865*** |
| | (0.0039) | (0.0041) |
| $SP500_{i,t-1}$ | -0.0092*** | -0.0090*** |
| | (0.0022) | (0.0021) |
| Merger Fixed Effects | Yes | Yes |
| Industry Fixed Effects | Yes | Yes |
| Observations | 57,100 | 57,100 |

# Table VIII

## Change in Forecast Bias: DID Estimator (w/o analysts from merging houses)

We exclude from our sample all analysts employed in the merging houses. We measure analyst forecast bias ($BIAS_{jt}$) as the difference between the forecast analyst *j* at time *t* and the actual *EPS*, expressed as a percentage of the previous year's stock price. The consensus bias is expressed as a mean or median bias among all analysts covering a particular stock. For all mergers, we split the sample of stocks into those covered by both merging brokerage houses (treatment sample) and those not covered by both houses (control sample). We also divide stocks into pre-merger period and post-merger period (one-year window for each period). For each period we further construct benchmark portfolios using the control sample based on stocks' size (*SIZE*), book-to-market ratio (*BM*), and average past year's returns (*RET*). Our benchmark assignment involves three portfolios in each category. Each stock in the treatment sample is then assigned to its own benchmark portfolio (*SIZE*-matched, *SIZE*/*BM*-matched, and *SIZE*/*BM*/*RET*-matched). Next, for each period, we calculate the cross-sectional average of the differences in analyst forecast bias across all stocks in the treatment sample and their respective benchmarks. Finally, we calculate the difference in differences between post-event period and pre-event period (DID Estimator). Our sample excludes observations with stock prices lower than $5 and those for which the absolute difference between forecast value and the true earnings exceeds $10. Standard errors (in parentheses) are clustered at the merger groupings. ***, **, * denotes 1%, 5%, and 10% statistical significance.

| | Mean BIAS | Median BIAS |
|---|---|---|
| DID Estimator (SIZE-Matched) | 0.0017*** | 0.0020*** |
| | (0.0006) | (0.0006) |
| DID Estimator (SIZE/BM-Matched) | 0.0015** | 0.0017** |
| | (0.0006) | (0.0006) |
| DID Estimator (SIZE/BM/RET-Matched) | 0.0015** | 0.0017** |
| | (0.0006) | (0.0007) |

## Table IX
## Change in Forecast Bias: Conditioning on Initial Coverage

The table below presents our results by cuts on initial coverage. There are three groups: lowest coverage (<=5), medium coverage (>5 and <=20) and highest coverage (>20). We measure analyst forecast bias ($BIAS_{jt}$) as the difference between the forecast analyst *j* at time *t* and the actual *EPS*, expressed as a percentage of the previous year's stock price. The consensus bias is expressed as a mean or median bias among all analysts covering a particular stock. For all mergers, we split the sample of stocks into those covered by both merging brokerage houses (treatment sample) and those not covered by both houses (control sample). We also divide stocks into pre-merger period and post-merger period (one-year window for each period). For each period we further construct benchmark portfolios using the control sample based on stocks' size (*SIZE*), book-to-market ratio (*BM*), and average past year's returns (*RET*). Our benchmark assignment involves three portfolios in each category. Each stock in the treatment sample is then assigned to its own benchmark portfolio (*SIZE*-matched, *SIZE*/*BM*-matched, and *SIZE*/*BM*/*RET*-matched). Next, for each period, we calculate the cross-sectional average of the differences in analyst forecast bias across all stocks in the treatment sample and their respective benchmarks. Finally, we calculate the difference in differences between post-event period and pre-event period (DID Estimator). Our sample excludes observations with stock prices lower than $5 and those for which the absolute difference between forecast value and the true earnings exceeds $10. Standard errors (in parentheses) are clustered at the merger groupings. ***, **, * denotes 1%, 5%, and 10% statistical significance.

| | Mean BIAS | Median BIAS |
|---|---|---|
| DID Estimator (SIZE/BM/RET-Matched) | 0.0059* | 0.0076* |
| (Coverage <=5) | (0.0033) | (0.0043) |
| DID Estimator (SIZE/BM/RET-Matched) | 0.0020* | 0.0022** |
| (Coverage>5 & <=20) | (0.0010) | (0.0011) |
| DID Estimator (SIZE/BM/RET-Matched) | 0.0012 | 0.0014 |
| (Coverage>20) | (0.0014) | (0.0014) |

# Table X
## Change in Alternative Forecast Bias Measures: DID Estimator

We measure analyst forecast bias ($BIAS_{jt}$) using two different measures: the forecast of long-term growth of analyst *j* at time *t* (Panel A), and the analyst's *j* stock recommendation at time *t* (Panel B). For each analyst, the recommendation variable is ranked from 1 to 5, where 1 is strong sell, 2 is sell, 3 is hold, 4 is buy, and 5 is strong buy. The consensus bias is expressed as a mean or median bias among all analysts covering a particular stock. For all mergers, we split the sample of stocks into those covered by both merging brokerage houses (treatment sample) and those not covered by both houses (control sample). We also divide stocks into pre-merger period and post-merger period (one-year window for each period). For each period we further construct benchmark portfolios using the control sample based on stocks' size (*SIZE*), book-to-market ratio (*BM*), and average past year's returns (*RET*). Our benchmark assignment involves three portfolios in each category. Each stock in the treatment sample is then assigned to its own benchmark portfolio (*SIZE*-matched, *SIZE*/*BM*-matched, and *SIZE*/*BM*/*RET*-matched). Next, for each period, we calculate the cross-sectional average of the differences in analyst forecast bias across all stocks in the treatment sample and their respective benchmarks. Finally, we calculate the difference in differences between post-event period and pre-event period (DID Estimator). Our sample excludes observations with stock prices lower than $5 and those for which the absolute difference between forecast value and the true earnings exceeds $10. Standard errors (in parentheses) are clustered at the merger groupings. ***, **, * denotes 1%, 5%, and 10% statistical significance.

*Panel A: Long-Term Growth*

| | Mean BIAS | Median BIAS |
|---|---|---|
| DID Estimator (SIZE-Matched) | 1.019*** | 0.835*** |
| | (0.228) | (0.235) |
| DID Estimator (SIZE/BM-Matched) | 0.866*** | 0.663*** |
| | (0.238) | (0.245) |
| DID Estimator (SIZE/BM/RET-Matched) | 0.561** | 0.406* |
| | (0.224) | (0.232) |

*Panel B: Analyst Recommendations*

| | Mean BIAS | Median BIAS |
|---|---|---|
| DID Estimator (SIZE-Matched) | 0.0785* | 0.1216** |
| | (0.0414) | (0.0576) |
| DID Estimator (SIZE/BM-Matched) | 0.0756* | 0.1195** |
| | (0.0411) | (0.0562) |
| DID Estimator (SIZE/BM/RET-Matched) | 0.0512 | 0.0917* |
| | (0.0409) | (0.0562) |

## Table XI
## Change in Forecast Dispersion

We measure analyst forecast dispersion ($FDISP_{jt}$) as the standard deviation of all analyst forecasts covering firm *i* at time *t.* In Panel A, for all mergers, we split the sample of stocks into those covered by both merging brokerage houses (treatment sample) and those not covered by both houses (control sample). We also divide stocks into pre-merger period and post-merger period (one-year window for each period). For each period we further construct benchmark portfolios using the control sample based on stocks' size (*SIZE*), book-to-market ratio (*BM*), and average past year's returns (*RET*). Our benchmark assignment involves three portfolios in each category. Each stock in the treatment sample is then assigned to its own benchmark portfolio (*SIZE*-matched, *SIZE*/*BM*-matched, and *SIZE*/*BM*/*RET*-matched). Next, for each period, we calculate the cross-sectional average of the differences in forecast dispersion across all stocks in the treatment sample and their respective benchmarks. Finally, we calculate the difference in differences between post-event period and pre-event period (DID Estimator). In Panel B**,** the dependent variable is *FDISP.* For each merger, we consider a one-year window prior to merger (pre-event window) and a one-year window after the merger (post-event window). We construct an indicator variable (*MERGE*) equal to one for the post-event period and zero for the pre-event period. For each merger window, we assign an indicator variable (*AFFECTED*) equal to one for each stock covered by both merging brokerage houses (treatment sample) and zero otherwise. *LNSIZE* is a natural logarithm of the market cap of the stock; $SIGMA_{it}$ is the variance of daily (simple, raw) returns of stock *i* during year *t*; *RETANN* is annual return on the stock; *LNBM* is a natural logarithm of the book to market ratio. To measure the volatility of *ROE* (*VOLROE*), we estimate an AR(1) model for each stock's *ROE* using a 10-year series of the company's valid annual *ROE*s. $ROE_{it}$ is firm *i*'s return on equity in year *t*. *ROE* is calculated as the ratio of earnings in year *t* over the book value of equity. *VOLROE* is the variance of the residuals from this regression. $PROFIT_{it}$ is the profitability of company *i* at the end of year *t*, defined as operating income over book value of assets. *SP500* is an indicator variable equal to one if a stock is included in the S&P500 index. We also include three-digit SIC industry fixed effects and merger fixed effects. We include results based on both mean and median forecast error. Our sample excludes observations with stock prices lower than $5 and those for which the absolute difference between forecast value and the true earnings exceeds $10. Standard errors (in parentheses) are clustered at the merger groupings. ***, **, * denotes 1%, 5%, and 10% statistical significance.

*Panel A: DID*

| | FDISP |
|---|---|
| DID Estimator | -0.0008* |
| (SIZE-Matched) | (0.0004) |
| DID Estimator | -0.0007** |
| (SIZE/BM-Matched) | (0.0003) |
| DID Estimator | -0.0006* |
| (SIZE/BM/RET-Matched) | (0.0003) |

*Panel B: Regression Evidence*

| | FDISP |
|---|---|
| $MERGE_i$ | -0.0001 |
| | (0.0001) |
| $AFFECTED_i$ | -0.0001 |
| | (0.0003) |
| $MERGE_i*AFFECTED_i$ | 0.0001 |
| | (0.0005) |
| $LNSIZE_{i,t-1}$ | 0.0008*** |
| | (0.0001) |
| $SIGMA_{i,t-1}$ | 0.0048*** |
| | (0.0003) |
| $RETANN_{i,t-1}$ | -0.0156*** |
| | (0.0012) |
| $LNBM_{i,t-1}$ | 0.0020*** |
| | (0.0001) |
| $VOLROE_{i,t-1}$ | 0.0024*** |
| | (0.0001) |
| $PROFIT_{i,t-1}$ | -0.0092*** |
| | (0.0005) |
| $SP500_{i,t-1}$ | -0.0023*** |
| | (0.0002) |
| Merger Fixed Effects | Yes |
| Industry Fixed Effects | Yes |
| Observations | 57,100 |

Table XII

Change in Forecast Error

We measure analyst forecast error ($FERROR_{jt}$) as the absolute difference between the forecast analyst *j* at time *t* and the actual *EPS*, expressed as a percentage of the previous year's stock price. The consensus error is expressed as a mean or median forecast error among all analysts covering a particular stock. In Panel A, for all mergers, we split the sample of stocks into those covered by both merging brokerage houses (treatment sample) and those not covered by both houses (control sample). We also divide stocks into pre-merger period and post-merger period (one-year window for each period). For each period we further construct benchmark portfolios using the control sample based on stocks' size (*SIZE*), book-to-market ratio (*BM*), and average past year's returns (*RET*). Our benchmark assignment involves three portfolios in each category. Each stock in the treatment sample is then assigned to its own benchmark portfolio (*SIZE*-matched, *SIZE*/*BM*-matched, and *SIZE*/*BM*/*RET*-matched). Next, for each period, we calculate the cross-sectional average of the differences in analyst forecast error across all stocks in the treatment sample and their respective benchmarks. Finally, we calculate the difference in differences between post-event period and pre-event period (DID Estimator). In Panel B, the dependent variable is *FERROR*. For each merger, we consider a one-year window prior to merger (pre-event window) and a one-year window after the merger (post-event window). We construct an indicator variable (*MERGE*) equal to one for the post-event period and zero for the pre-event period. For each merger window, we assign an indicator variable (*AFFECTED*) equal to one for each stock covered by both merging brokerage houses (treatment sample) and zero otherwise. *LNSIZE* is a natural logarithm of the market cap of the stock; $SIGMA_{it}$ is the variance of daily (simple, raw) returns of stock *i* during year *t*; *RETANN* is annual return on the stock; *LNBM* is a natural logarithm of the book to market ratio; *COVERAGE* denotes the number of analysts tracking the stock. To measure the volatility of *ROE* (*VOLROE*), we estimate an AR(1) model for each stock's *ROE* using a 10-year series of the company's valid annual *ROE*s. $ROE_{it}$ is firm *i*'s return on equity in year *t*. *ROE* is calculated as the ratio of earnings in year *t* over the book value of equity. *VOLROE* is the variance of the residuals from this regression. $PROFIT_{it}$ is the profitability of company *i* at the end of year *t*, defined as operating income over book value of assets. *SP500* is an indicator variable equal to one if a stock is included in the S&P500 index. We also include three-digit SIC industry fixed effects and merger fixed effects. We include results based on both mean and median forecast error. Our sample excludes observations with stock prices lower than $5 and those for which the absolute difference between forecast value and the true earnings exceeds $10. Standard errors (in parentheses) are clustered at the merger groupings. ***, **, * denotes 1%, 5%, and 10% statistical significance.

*Panel A: DID*

| | Mean FERROR | Median FERROR |
|---|---|---|
| DID Estimator (SIZE-Matched) | 0.0008 | 0.0009** |
| | (0.0005) | (0.0004) |
| DID Estimator (SIZE/BM-Matched) | 0.0005 | 0.0006 |
| | (0.0004) | (0.0004) |
| DID Estimator (SIZE/BM/RET-Matched) | 0.0006 | 0.0007 |
| | (0.0005) | (0.0005) |

*Panel B: Regression Analysis*

| | Mean FERROR | Median FERROR |
|---|---|---|
| $MERGE_i$ | 0.0007 | 0.0009 |
| | (0.0010) | (0.0010) |
| $AFFECTED_i$ | 0.0013 | 0.0018 |
| | (0.0013) | (0.0012) |
| $MERGE_i$*$AFFECTED_i$ | 0.0023** | 0.0021* |
| | (0.0011) | (0.0011) |
| $LNSIZE_{i,t-1}$ | -0.0012** | -0.0012** |
| | (0.0005) | (0.0005) |
| $SIGMA_{i,t-1}$ | -0.0022 | -0.0032 |
| | (0.0038) | (0.0039) |
| $RETANN_{i,t-1}$ | -0.0137 | -0.0051 |
| | (0.0091) | (0.0096) |
| $LNBM_{i,t-1}$ | 0.0072*** | 0.0070*** |
| | (0.0007) | (0.0007) |
| $COVERAGE_{i,t-1}$ | -0.0002*** | -0.0002*** |
| | (0.0000) | (0.0000) |
| $VOLROE_{i,t-1}$ | 0.0097*** | 0.0095*** |
| | (0.0011) | (0.0011) |
| $PROFIT_{i,t-1}$ | -0.0179** | -0.0181** |
| | (0.0081) | (0.0082) |
| $SP500_{i,t-1}$ | -0.0056*** | -0.0051*** |
| | (0.0017) | (0.0017) |
| Merger Fixed Effects | Yes | Yes |
| Industry Fixed Effects | Yes | Yes |
| Observations | 57,100 | 57,100 |

## Appendix B: Mergers included in the sample (sorted by date)

| Merger Number | Merger date | Target | Target's industry | IBES No. | Industry code | Bidder | IBES No. | Bidder's industry | Industry |
|---|---|---|---|---|---|---|---|---|---|
| 1 | 9/10/1984 | Becker Paribas | Brokerage firm | 299 | 6211 | Merrill Lynch & Co Inc | 183 | Pvd invest, fin advisory svcs | 6211 |
| 8 | 10/31/1988 | Butcher & Co Inc | Securities dealer; RE broker | 44 | 6211 | Wheat First Securities Inc(WF) | 282 | Investment bank,brokerage firm | 6211 |
| 2 | 12/31/1994 | Kidder Peabody & Co | Investment bank | 150 | 6211 | PaineWebber Group Inc | 189 | Investment bank | 6211 |
| 3 | 5/31/1997 | Dean Witter Discover & Co | Pvd sec brokerage svcs | 232 | 6211 | Morgan Stanley Group Inc | 192 | Investment bank | 6211 |
| 4 | 11/28/1997 | Salomon Brothers | Investment bank | 242 | 6211 | Smith Barney | 254 | Investment bank | 6211 |
| 9 | 1/9/1998 | Principal Financial Securities | Investment bk;securities firm | 495 | 6211 | EVEREN Capital Corp | 829 | Securities brokerage firm | 6211 |
| 10 | 2/17/1998 | Jensen Securities Co | Securities brokerage firm | 932 | 6211 | DA Davidson & Co | 79 | Investment company | 6799 |
| 11 | 4/6/1998 | Wessels Arnold & Henderson LLC | Investment bank | 280 | 6211 | Dain Rauscher Corp | 76 | Investment bank | 6211 |
| 12 | 10/1/1999 | EVEREN Capital Corp | Securities brokerage firm | 829 | 6211 | First Union Corp,Charlotte,NC | 282 | Commercial bank; holding co | 6021 |
| 13 | 6/12/2000 | JC Bradford & Co | Securities brokerage firm | 34 | 6211 | PaineWebber Group Inc | 189 | Investment bank | 6211 |
| 5 | 10/15/2000 | Donaldson Lufkin & Jenrette | Investment bank | 86 | 6211 | CSFB | 100 | Investment bank | 6211 |
| 6 | 12/10/2000 | Paine Webber | Investment bank | 189 | 6211 | UBS Warburg Dillon Read | 85 | Investment bank | 6211 |
| 7 | 12/31/2000 | JP Morgan | Investment bank | 873 | 6211 | Chase Manhattan | 125 | Investment bank | 6211 |
| 14 | 9/18/2001 | Josephthal Lyon & Ross | Security brokers and dealers | 933 | 6211 | Fahnestock & Co | 98 | Securities brokerage firm | 6211 |
| 15 | 3/22/2005 | Parker/Hunter Inc | Pvd invest,invest bkg svcs | 860 | 6211 | Janney Montgomery Scott LLC | 142 | Pvd sec brkg svcs | 6211 |