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Does Competition Induce Analyst Effort? Evidence from a Natural Experiment of Broker Mergers*

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Abstract

Hong and Kacperczyk (2010) document that decreases in analyst competition due to broker mergers encourage analysts to please managers, leading to greater consensus optimism bias. We propose three additional effects of analyst competition. The analyst effort hypothesis suggests that weaker competition reduces analysts' incentives to collect and analyze information. The herding hypothesis argues that weaker competition reduces analysts' career concerns, which in turn reduces herding incentives. The strategic deviation hypothesis implies that weaker competition alleviates analysts' incentives to strategically deviate from others. We find that after broker mergers, analysts follow fewer firms and switch their coverage from firms with more to those with less R&D expenses. They weigh their private information less when it is unfavorable. At the same time, their forecasts become more dispersed. All these findings appear to be more consistent with the analyst effort hypothesis than the herding or strategic deviation hypothesis.

JEL Classification: G14; G17; G24
Keywords: Analyst forecast activities; Competition; Analyst effort; Herding; Strategy deviation

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Abstract

Hong and Kacperczyk (2010) document that decreases in analyst competition due to broker mergers encourage analysts to please managers, leading to greater consensus optimism bias. We propose three additional effects of analyst competition. The analyst effort hypothesis suggests that weaker competition reduces analysts' incentives to collect and analyze information. The herding hypothesis argues that weaker competition reduces analysts' career concerns, which in turn reduces herding incentives. The strategic deviation hypothesis implies that weaker competition alleviates analysts' incentives to strategically deviate from others. We find that after broker mergers, analysts follow fewer firms and switch their coverage from firms with more to those with less R&D expenses. They weigh their private information less when it is unfavorable. At the same time, their forecasts become more dispersed. All these findings appear to be more consistent with the analyst effort hypothesis than the herding or strategic deviation hypothesis.

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1. Introduction

Information production and processing are key functions of many important markets, such as those for analyst forecasts, credit ratings, and the financial press. In the financial market, sell-side analysts acting as information providers compete to provide earnings forecasts and stock recommendations to the public. Competition among financial analysts is essential, as it affects analysts' incentives, thereby shaping the outcomes of their forecast activities. In a pioneering study, Hong and Kacperczyk (2010) document that competition disciplines analysts so that they have fewer incentives to please managers, thereby leading to lower consensus optimism bias.

The literature also suggests three additional impacts of competition on analysts' incentives. First, the compensation and job security of analysts depend on their performance relative to their peers (Stickle, 1992; Mikhail, Walther, and Willis, 1999). Intense competition puts more peer pressure on analysts and therefore encourages them to exert more effort to achieve better relative performance. Second, due to career concerns, analysts tend to ignore their private information and exhibit herding behavior to avoid reputation damage resulting from potential inaccurate forecasts (Scharfstein and Stein, 1990; Trueman, 1994; Graham, 1999; Hong, Kubik, and Solomon, 2000). Analyst competition increases analysts' career concerns and thereby induces stronger herding incentives. Third, a strand of the literature on strategic forecasting (Laster, Bennett, and Geoum, 1999; Ottaviani and Sorensen, 2005) suggests that the compensation of forecasters depends on both the accuracy of their forecasts and their ability to generate publicity. Competition may strengthen forecasters' incentives to attract public attention and encourage them to issue bold forecasts that strategically deviate from those of others.

We complement Hong and Kacperczyk (2010) by investigating the analyst effort, herding, and strategic deviation channels of analyst competition. Following Hong and Kacperczyk (2010) and Brochet, Miller, and Srinivasan (2014), we use the number of analysts covering a firm as the measure of competition.[1] Intuitively, the more analysts who follow the same firm, the more competitive pressure these analysts impose on each other. To address the endogeneity issue, we use a natural experiment of broker mergers to identify shocks to analyst competition. Wu and Zang (2009) were the first to investigate the effects of broker mergers on the career outcomes of financial analysts. They find that broker mergers usually result in the firing of analysts who provide redundant coverage of the same firm. This decrease in analyst coverage can be treated as plausibly exogenous to firm fundamentals, as suggested by Hong and Kacperczyk (2010), Kelly and Ljungqvist (2012), and Derrien and Kecskés (2013), among others.

Our empirical analysis includes 31 broker mergers between 1980 and 2006. We find that 2,872 observations of the affected firms are simultaneously followed by the acquiring and target brokers before the corresponding mergers. The sample period ends in 2006 because from 2007 onward the Institutional Brokers' Estimate System (I/B/E/S) no longer provides the broker translation file, which we use to link broker names to I/B/E/S broker IDs. We conduct difference-in-differences regressions to examine changes in analyst forecast activities around broker mergers at both the analyst and firm levels.

Specifically, we compare changes in individual analyst coverage, their weighting of private versus public information, and the analyst forecast dispersion for merger-affected analysts or firms with the corresponding changes in the control group. The analyst effort, herding, and strategic deviation hypotheses have different implications for analysts' forecast activities. We first test the analyst effort hypothesis. We examine changes in the coverage of individual analysts around broker mergers. The results show that after broker mergers, merger-affected

[1] In the literature on media markets, the number of media outlets is widely used as a competition measure (Besley and Prat, 2006; Gentzkow and Shapiro, 2006, 2008). In the literature on rating agencies, the entry of Fitch is also treated as an increase in competition (Becker and Milbourn, 2011).

analysts on average reduce the number of firms they follow and switch their coverage from firms with higher to those with lower R&D expenses. This suggests that merger-affected analysts exert less effort after broker mergers, which is consistent with the notion that decreased analyst competition reduces analysts' peer pressure, thereby reducing the effort exerted by merger-affected analysts.

We next examine the herding hypothesis by analyzing the effect of competition on analysts' weighting of public versus private information. Hong et al. (2000), Gleason and Lee (2003), and Clement and Tse (2005) interpret herding as analysts' ignoring their private information, overweighting public information, and revising their forecasts toward market consensus. If weaker competition is associated with less herding, analysts should place more weight on their private information after broker mergers than before. We use a linear regression method similar to that proposed by Chen and Jiang (2006) to test this hypothesis. We find that when private information is negative relative to public information, analysts place more weight on public information in their post-merger forecasts than in their pre-merger forecasts. This result is inconsistent with the prediction of the herding hypothesis.

Finally, to distinguish between the analyst effort hypothesis and the strategic deviation hypothesis, we investigate the changes in forecast dispersion around the broker mergers for merger-affect firms. Under the strategic deviation hypothesis, reduced competition after broker mergers suppresses analysts' incentive to attract public attention, which discourages them to issue bold forecasts. Therefore, earnings forecasts of the merger-affected firms become less dispersed. In contrast, the analyst effort hypothesis predicts that analysts exert less effort and produce more inaccurate forecasts after broker mergers, which increases the dispersion of their forecasts. We find an increase in forecast dispersion after broker mergers, which is consistent with the analyst effort hypothesis and fails to support the strategic deviation hypothesis.

Overall, our results suggest that decreased analyst competition after broker mergers reduces the analyst effort effect of competition. This leads to fewer individual analysts' coverage, less focus on firms with severe information asymmetry, and more dispersed earnings forecasts. All of these outcomes suggest that broker mergers impair the quality of the information environment. Our findings are more consistent with the analyst effort hypothesis than with the herding or strategic deviation hypothesis.

Our paper is related to three strands in the literature. First, we complement Hong and Kacperczyk (2010) by investigating three additional effects of competition that may change analysts' incentives. Hong and Kacperczyk (2010) find that analyst competition reduces analysts' incentives to please managers, thereby resulting in less upward-biased earnings forecasts. This mechanism focuses on the interaction between analysts and firms and emphasizes the role of competition in improving the information environment of the financial market. The analyst effort, herding, and strategic deviation hypotheses instead focus on the interactions between analysts. We identify a different channel through which analyst competition ultimately enhances the information quality of analyst reports. Moreover, Hong and Kacperczyk (2010) focus on consensus forecast bias, which is a firm-level variable aggregated from individual analysts. In contrast, we conduct a battery of analyst-level studies that produce evidence that cannot be generated by analyzing firm-level variables.

Second, broker mergers or closures are widely perceived in the literature as shocks to analyst competition or to the information environments of firms. Researchers have used this natural experiment to establish the causal link between analyst coverage and (i) analyst consensus reporting bias (Hong and Kacperczyk, 2010), (ii) the cost of capital (Kelly and Ljungqvist, 2012; Derrien et al., 2016), (iii) corporate investment and financing (Derrien and Kecskés, 2013), (iv) corporate innovation (He and Tian, 2013), (v) managerial disclosure (Irani and Oesch, 2013), and (vi) corporate governance (Chen, Harford, and Lin, 2015).[2] We contribute to this strand of the literature by examining the effects of broker mergers on a variety of outcomes of analysts' forecast activities, such as individual analysts' coverage, their weighting of private versus public information, and the dispersion of their forecasts.

Finally, our study is also related to the large body of literature that examines the behavior of financial analysts and their role in the capital market. A central question in this field concerns the degree to which analyst behavior is distorted by various conflict-of-interest issues.[3] We complement this literature by shifting the focus from the interaction between analysts and other entities (i.e., firms and brokers) to the interactions between analysts in the form of competition. Our attempt to establish a causal relation between analyst competition and analyst forecast quality can help distinguish the positive effect of competition (i.e., disciplining analysts) from its negative effect (i.e., inducing herding and strategic deviation). Moreover, our findings should help regulators design a better competition structure to facilitate information production and dissemination in financial markets.

2. Hypothesis development

[2] In addition, Balakrishnan et al. (2014) use broker closures to establish the causal relation between managerial voluntary disclosure and liquidity. Fong et al. (2014) use broker mergers to examine the disciplining effect of analysts on credit rating agencies. Purnanandam and Rajan (2018) use broker mergers to establish the causal relation between capital expenditure and firm leverage.

[3] Examples include currying favor with management (Francis and Philbrick, 1993), creating an investment banking business (Lin and McNichols, 1998; Michaely and Womack, 1999), maximizing trading commissions (Cowen et al., 2006), and associated career concerns (Hong et al., 2000; Clement and Tse, 2005). Moreover, Guo, Li, and Wei (2020) have recently found that analysts tend to give more favorable stock recommendations to overvalued stocks, and these highly recommended overvalued stocks subsequently tend to have very large negative abnormal returns.

Our empirical predictions about the effects of analyst competition on analysts' behavior and their related earnings forecast and recommendation outcomes are based on theoretical considerations and recent empirical evidence. Hong and Kacperczyk (2010) document how analyst competition disciplines analysts and reduces their incentives to cater to corporate managers. However, three strands of the literature also suggest that competition may have additional implications for the incentives of sell-side analysts beyond those discussed by Hong and Kacperczyk (2010).

First, Stickle (1992), Mikhail et al. (1999), and Jackson (2005), among others, suggest that competition increases the peer pressure on sell-side analysts, which encourages them to exert more effort and produce more accurate forecasts. This analyst effort hypothesis works in three ways. First, Mikhail et al. (1999) find that the *relative* forecast accuracy, rather than *absolute* accuracy, of an analyst is closely related to the probability of involuntary turnover. An analyst with relatively poor performance (i.e., larger forecast errors) is more likely to lose her job. It is more difficult for her to maintain a high ranking when the number of peer analysts increases. Second, an analyst's compensation also depends substantially on her relative performance and reputation. For example, an analyst on the *Institutional Investor* (*II*) All-American Research Team is better paid than other analysts (Stickle, 1992). Given that the number of *II* All-Americans is limited and relatively constant over time, it is tougher for an analyst to earn this title when the number of competitors increases. The desire for higher pay would encourage her to exert more effort to be selected. Third, sell-side analysts generate revenue for their brokers through the commissions paid by the institutional investors they serve. Analysts with better forecast performance gain a higher reputation in the long run and generate higher future trading volume for their brokers (Jackson, 2005). Given a certain number of institutional investors, the more brokers there are, the more fiercely they compete for commissions from these investors. Brokers therefore have stronger incentives to discipline their analysts and encourage them to produce better outcomes to attract as many intuitional investors as possible.

Second, the models of Scharfstein and Stein (1990), Trueman (1994), and Graham (1999) suggest that reputation concerns may lead decision makers (e.g., analysts) to ignore their private information and exhibit herding behavior. The model implications are confirmed by the empirical evidence of Hong et al. (2000) and Clement and Tse (2005) in the financial analyst industry. They find that analysts tend to follow consensus forecasts because they are likely to be fired if they issue bold forecasts that turn out to be highly inaccurate. The herding hypothesis suggests that analyst competition enhances analysts' tendency to herd. In a more competitive job market, reputational damage resulting from bold inaccurate forecasts is likely to affect analysts' career prospects more adversely.

Third, Laster et al. (1999) argue that the compensation of forecasters depends on not only the accuracy of their forecasts, but also their ability to generate publicity. The strategic deviation hypothesis asserts that the latter incentive encourages forecasters to strategically issue bold forecasts that deviate from others. Obviously, there is a tradeoff between issuing bold forecasts for publicity purposes and producing accurate forecasts. The extent to which a forecaster chooses to deviate from the consensus depends on the benefits and costs of doing so. Ottaviani and Sorensen (2005) directly model the order-ranking contest among forecasters and emphasize that whether competition among forecasters encourages or discourages aggressive forecasts depends on the reward structure. If the cost of inaccurate forecasts is low, then competition encourages forecasters to issue bold forecasts.[4]

In summary, our three hypotheses have different implications for the effects of intensified competition on analysts' forecast behavior. The analyst effort hypothesis suggests that analysts exert more effort and produce more accurate reports to stay at the top of their profession. The herding hypothesis indicates that analysts worry about the costs of inaccurate forecasts, and hence engage in herding behavior. The strategic deviation hypothesis dictates that analysts issue bold forecasts to obtain greater publicity when they are less concerned about the costs of inaccurate forecasts.

To empirically test these hypotheses, we adopt the broker merger event as a shock to analyst competition. We first directly test the analyst effort hypothesis by examining changes in the number and type of firms covered by individual analysts. The idea is that if analyst competition encourages analysts to exert more effort, we would expect individual analysts to cover fewer firms and focus less on firms with more severe information asymmetry after a broker merger. For example, analysts may switch to following firms with lower R&D expenses after a broker merger. The herding and strategic deviation hypotheses focus on analysts' incentives to follow or deviate from the consensus forecast. It does not have a clear prediction on whether analyst competition changes analysts' effort. The preceding discussion leads to our first hypothesis.

Hypothesis 1: *The analyst effort hypothesis predicts that broker mergers cause merger-affected individual analysts to exert less effort, in the sense that they follow fewer firms and focus less on firms with R&D expenses.*

We then directly test the herding hypothesis. If analyst competition induces herding as competition intensifies, analysts will place less weight on their private information when issuing

[4] The empirical evidence of Laster et al. (1999) confirms this prediction by showing that the financial analysts of banks, brokers, and security firms are less likely to issue bold forecasts, as it is costly to lose well-paying jobs. In this paper, we provide additional evidence regarding the strategic deviation hypothesis.

forecasts. However, the strategic deviation hypothesis predicts the opposite. It holds that when competition becomes fiercer, analysts will issue bolder forecasts (because benefit outweighs cost), and therefore place less weight on public signals. The analyst effort hypothesis does not impose any restriction on the weighting behavior of analysts. Specifically, we examine the following hypothesis.

Hypothesis 2: *The herding hypothesis predicts that broker mergers cause a significant decrease in analyst herding behavior, in the sense that analysts place more weight on their private information, whereas the strategic deviation hypothesis predicts the opposite.*

Finally, we investigate the dispersion of analyst forecasts around broker merger events. This analysis helps distinguish between the analyst effort hypothesis and the strategic deviation hypothesis. Analysts who exert more effort obtain more valuable information about the firms they follow, which reduces their forecast errors. As a result, analysts following the same firm should produce less dispersed forecasts with respect to that firm. In contrast, the strategic deviation hypothesis assumes that forecast errors impose limited costs on analysts. They strategically issue bold forecasts deviating from those of their peers to gain greater public attention. Thus, their forecasts tend to be more dispersed. After broker mergers, competition among analysts reduces and as a result, analysts would exert less effort and have less pressure to issue bold forecasts. The above discussions lead to our final hypothesis.

Hypothesis 3: *The analyst effort hypothesis predicts that the dispersion of earnings forecasts is increased after broker mergers, whereas the strategic deviation hypotheses predict the opposite.*

3. Data, sample, and natural experiment

3.1. Natural experiment validity

Ideally, a natural experiment should provide a source of exogenous variation in the level of analyst competition. We have argued up to this point that broker mergers are a valid setting for our natural experiment. However, one may argue that the number of analysts is not a good measure of analyst competition. If this is the case, we can regard our empirical strategy as a joint test, which simultaneously examines whether broker mergers reduce analysts' peer competition and the outcomes of the change in competition.

Moreover, the merger-driven reduction in analyst coverage is not caused by any particular merger-affected firm. In other words, the shocks to analyst coverage are exogenous to the covered firms. Although the exogeneity claim is not testable, the literature provides several arguments to justify it (e.g., Hong and Kacperczyk, 2010). Finally, the merged brokers do not selectively retain analysts whose forecasts are more accurate or more optimistic. This is a crucial condition. We argue that potential selection biases are not likely to invalidate our natural experiment, as the literature finds that analysts who disappear after broker mergers do not necessarily produce less accurate forecasts (Wu and Zang, 2009; Derrien and Kecskés, 2013) or less optimistic forecasts (Hong and Kacperczyk, 2010).

3.2. Natural experiment sample

The analyst forecast and stock recommendation data are from the I/B/E/S, the stock return data are from the Center for Research in Security Prices (CRSP), and the accounting data are from Compustat. We focus on annual earnings per share (EPS) forecasts, as they are the most commonly issued forecasts. To be included in the sample, a firm must be a U.S. firm simultaneously covered by the I/B/E/S, the CRSP, and Compustat. To ensure that outliers do not drive our empirical results, we annually trim the variables at the 1.5^{th} percentile at both tails.

To construct the sample for the natural experiment involving broker mergers, we first search the Securities Data Company (SDC) database for completed merger deals between target firms whose SIC code is 6211 (including but not limited to investment banks and brokerage firms) and acquiring firms belonging to the financial industry. Only acquiring firms that own 100% of the shares of the target firm after the transaction are included in our sample. We further require the merger effective date to fall within the sample period of 1980 to 2006. We choose 2006 as the end of our sample period because that we need the I/B/E/S broker translation file to link merger events to I/B/E/S brokers, and this file has not been updated since 2007.[5] This procedure results in a sample containing 750 merger events.[6]

Starting from these 750 merger events, we manually match the names of the target and acquiring firms in the SDC with the I/B/E/S broker names and estimator IDs, mainly based on the I/B/E/S broker translation file. We use LexisNexis Academic as a complementary news source to verify the links between the SDC merger events and I/B/E/S brokers. This matching procedure identifies 46 merger events, with both the target and acquirer being I/B/E/S brokers.

Not all 46 merger events are valid for the natural experiment. The natural experiment requires (i) some firms covered by the target broker to overlap with those covered by the acquiring broker before the merger and (ii) the merged broker to take material actions to reduce or eliminate redundant coverage. Therefore, we take the following steps to exclude merger events that are not appropriate for the natural experiment.

First, for each merger event, we define merger-affected firms as firms that are covered by both the target and acquiring brokers over the period from 3 to 14 months before the merger effective date recorded in the SDC.[7] The redundant positions are likely to be reduced after the

[5] The broker IDs in the I/B/E/S historical detail file are numerical to facilitate programming. The I/B/E/S has a separate broker translation file that converts the numerical broker IDs in the Historical Detail file into the names of the brokers. We need this broker translation file to link the brokers in the I/B/E/S to those in the SDC through their names, so that we can identify the merger-affected firms. However, the I/B/E/S has not updated the broker translation file since 2007. As such, we cannot identify merger-affected firms after 2006.
[6] The sample screening procedure for the merger deals is similar to that used by Wu and Zang (2009).
[7] The I/B/E/S and the SDC are not always consistent in recording merger dates. In particular, a target broker could stop reporting to the I/B/E/S in a month different from the merger effective month recorded by the SDC. Therefore,

merger. We only keep merger events with at least five merger-affected firms, to rule out the possibility of the broker mergers being driven by any particular merger-affected firms. Second, for each merger event, if at least one merger-affected firm associated with that merger event is still covered by the target and acquiring brokers for the period from 3 to 14 months after the merger effective date, we exclude that merger event from our natural experiment sample. If the merger-affected firms are still covered by both the target and acquiring brokers after the merger, the merger event may not have affected the number of analysts covering these firms. We are not interested in such cases. With all of these restrictions, the sample is reduced to 31 merger events. As a final step, we further group the merger events by month to mitigate any compounding effects of multiple mergers occurring in close succession. The final sample includes 28 merger groups (31 merger events) affecting 2,872 firms for which we have the necessary data for the difference-in-differences tests.[8]

Table 1 reports the merger effective dates, the names of the target and acquiring brokers, and the number of affected stocks associated with each merger event group. Also reported are the changes in analyst coverage of the affected firms by merger event group. In total, there are 31 merger events in the sample period of 1980 to 2006. The longest time lapse between two adjacent merger events is approximately three years: Josephson International merged with Gruntal Financial Corp on September 6, 1985, and the next merger, between Johnson, Lane, Space, Smith & Co and Interstate Securities Inc., did not take place until October 18, 1988. However, some mergers have the same effective date. For example, on November 3, 2000, both

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we always measure analyst coverage before and after the SDC broker merger effective date by skipping three months.

[8] Our final sample includes 28 merger groups, whereas that of Hong and Kacperczyk (2010) only has 15. This discrepancy is probably driven by the use of different screening procedures for the merger deals. Furthermore, our sample period is one year longer than theirs. Following Wu and Zang (2009), we include acquirers with a two-digit SIC code of 60, 62, or 63. Hong and Kacperczyk (2010) do not include firms with a two-digit SIC code of 63. They also do not clearly explain how their merger-affected firms are defined. This may be another reason for the difference in sample size between the two studies.

the merger between Donaldson Lufkin & Jenrette and Credit Suisse First Boston and that between Paine Webber and UBS Group became effective. When two merger events occur in close succession, it is very difficult to examine the effects of each individual merger. To partially address this issue, we group merger events by month and end up with 28 merger event groups, which we treat independently in a later analysis.

The number of firms affected by mergers among brokers in the merger event groups varies substantially. For example, the two mergers among brokers in group 20 affect 624 firms. This is the largest merger event group, as it includes two mergers involving big brokerage firms (CSFB acquiring Donaldson Lufkin & Jenrette and UBS acquiring Paine Webber). The second largest merger group is group 11 (Travelers Group acquiring Salomon). The merger in this group affects 480 firms. The top 10 largest merger groups involve mergers affecting 2,480 firms, which account for approximately 86.35% of the sample of affected firms. However, there are also small merger event groups in which fewer than 10 firms are affected. For example, the merger between E*Offering and Wit Soundview (group 19) only affects five firms.

3.3. Changes in analyst coverage around broker mergers

The last two columns of Table 1 report the average changes in analyst coverage and net-of-control analyst coverage of the affected firms associated with each merger group around broker mergers. The last row reports the averages for all merger groups. For each merger-affected firm, we define pre-merger (post-merger) analyst coverage as the number of analysts issuing annual earnings forecasts in the period from 3 to 14 months before (3 to 14 months after) the merger effective month. We choose a 12-month period to define analyst coverage, as an analyst typically issues at least one annual EPS forecast per year. Analyst coverage of a firm decreases by 2.36 analysts on average. However, the change in analyst coverage (post-merger analyst coverage –

pre-merger analyst coverage) may reflect the trend in analyst coverage for all firms, rather than the effect of broker mergers on merger-affected firms.

To control for this trend, we construct a group of control firms for each group of merger-affected firms. Specifically, for each merger group, we divide the sample of firms into merger-affected and control firms. Each merger-affected firm is assigned to its benchmark portfolio constructed from control firms. To construct the benchmark portfolio, we follow Hong and Kacperczyk (2010) and first sort all of the control firms into terciles by their market capitalization before the corresponding broker merger. Each market capitalization tercile is further sorted into terciles by book-to-market equity ratio, followed by past annual return, and finally analyst coverage before the merger. With this dependent sorting procedure, 81 benchmark portfolios are constructed. Each merger-affected firm is then assigned to its corresponding benchmark portfolio.[9] We then use the average analyst coverage of the benchmark portfolios to adjust that of the merger-affected firms and examine the net-of-control changes in the latter around broker mergers.

After we control for the benchmark trend, we find a net change in analyst coverage of −1.11. Both measures of change are highly significant at the 1% level. Furthermore, this decrease in analyst coverage is not driven by any single merger group. In fact, as shown in the last column, 22 of the 28 merger groups are associated with a negative change in net-of-control analyst coverage. The remaining six merger groups are all small in the sense that none of them affect more than 60 firms. The evidence confirms that broker mergers lead to less analyst coverage of the affected firms, which reduces analyst competition.

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[9] If the merger effective month is January or February of year t, the three sorting characteristics (pre-merger market capitalization, book-to-market equity ratio, and annual stock return) correspond to the year $t-2$ values. If the merger effective month is any other month of year t, the three sorting characteristics correspond to the year $t-1$ values. This is to ensure that all of the sorting characteristics are already available in the merger effective month.

4. Empirical results

Using broker mergers as an exogenous shock, we conduct a regression-type difference-in-differences analysis to examine the changes in analyst forecasting activities around broker mergers at both the analyst and firm levels. For all of the regressions, we include year dummies to control for the time-fixed effect. For the analyst- and firm-level regressions, we further control for analyst- and firm-fixed effects by adding analyst and firm dummies. Finally, the *t*-statistics in all of the regressions are adjusted by double clustering the standard errors.[10]

4.1. Summary statistics

To examine the changes in forecasting activities, we use an event window close to the event date to maximize the relevancy of the broker merger event. Specifically, the pre-merger (post-merger) forecasting activities are measured over the period five to seven months before (five to seven months after) the merger effective month. Using a pre-merger window that is six months before and a post-merger window that is six months after the broker merger event ensures a one-year time difference between the two windows.[11]

We measure the forecasting activities as follows. For each individual analyst who issues at least one forecast over the three-month pre-merger (post-merger) window, we define the pre-merger (post-merger) number of firms covered by that individual analyst as the number of firms

[10] For the firm-level analysis (Table 6), we cluster by firm and year. For the individual analyst analysis (Tables 3 and 4), we cluster by analyst and year. For the forecast-level analysis (Tables 5 and 7), we cluster by analyst and month.

[11] If the time difference is less than one year, the analyst forecasts used to construct the pre-event forecasting activities, such as forecast accuracy, and those used to calculate the post-event forecasting activities would probably have the same forecast period end date. In this case, the post-merger forecast accuracy would be mechanically better than the pre-merger forecast accuracy, as the forecasts within the post-merger window would be closer to the forecast period end date. Our current setting avoids this mechanical bias.

with respect to which she issues at least one forecast during the pre-merger (post-merger) year.[12] Following Diether, Malloy, and Scherbina (2002) and Cen, Wei, and Yang (2017), forecast dispersion is defined as the forecast standard deviation, scaled by the absolute value of the mean consensus forecast. Both the forecast standard deviation and mean consensus forecast are obtained from the I/B/E/S unadjusted summary file. If the mean consensus forecast equals 0, the forecast dispersion is treated as missing. The pre-merger (post-merger) forecast dispersion is defined as the median of the forecast dispersion values corresponding to the I/B/E/S unadjusted summary records 7 to 5 months before (5 to 7 months after) the merger effective month. We use the median of the three dispersion values from three adjacent months to mitigate the impact of outliers. Detailed definitions of the variables used in this paper are provided in Appendix A.

Table 2 reports the summary statistics for the merger-affected analysts (analyst level) and the merger-affected firms (firm level). As mentioned earlier, merger-affected firms are firms that are simultaneously covered by target and acquiring brokers in the period from 3 to 14 months before the merger effective month. An individual analyst is defined as a merger-affected analyst if she covers at least one merger-affected firm. For each merger group, we calculate the mean, standard deviation, and median of analyst forecast activities and firm characteristics. The numbers reported are the averages across merger groups. We highlight two interesting observations. First, the number of firms covered by merger-affected analysts after broker mergers decreases slightly. The median decreases from 15.55 to 15.25 firms. Second, there is an increase in forecast dispersion for merger-affected firms after broker mergers. The median increases from 4.28% to 5.92%.

[12] The pre-merger (post-merger) year is the year of the sixth month before (after) the merger effective month. We set a six-month time difference between the pre-merger (post-merger) window and the event effective month so that the pre-merger and post-merger years are different.

Overall, the summary statistics for the sample of merger-affected firms and analysts provide preliminary evidence that is more consistent with the analyst effort hypothesis than the herding or strategic deviation hypothesis. However, we must conduct more rigorous tests to formally distinguish the three hypotheses.

4.2. Changes in individual analyst coverage around broker mergers

In this subsection, we conduct two formal analyst-level tests to verify H1. Specifically, we examine whether analysts exert less effort after broker mergers that weaken competition. We first investigate whether individual analysts reduce the number of firms they follow after broker mergers by running the following difference-in-differences regression:

$$\begin{aligned} NFirm_{i,t}\ (Ln\big(1+NFirm_{i,t}\big)) &= \alpha+\beta_1 Merger+\beta_2 Affected \\ &+\beta_3 Merger\times Affected+\beta_4 Controls_{i,t-1}+\varepsilon_{i,t}. \end{aligned} \tag{1}$$

This test is based on all analysts who issue at least one earnings forecast during the three-month pre- or post-merger window. Analyst *i* is regarded as a merger-affected analyst before (after) the broker merger if she covers at least one merger-affected firm during the pre-merger (post-merger) window. $Affected$ equals 1 if an analyst is a merger-affected analyst and 0 otherwise. $NFirm_{i,t}$ is either the pre- or post-merger number of firms that analyst *i* follows. $Merger$ equals 1 (0) if the dependent variable is measured after (before) the merger event. The analyst effort hypothesis stated in H1 predicts that β_3 < 0.

Table 3 reports the results. The dependent variable in Columns (1) and (2) is $NFirms_{it}$, but it is $Ln(1+NFirms_{it})$ in Columns (3) and (4). For each version, we run a parsimonious regression that includes only $Merger$, $Affected$, and $Merger\times Affected$ (Columns (1) and (3)) and another that includes two additional analyst characteristics (Columns (2) and (4)). $Accuracy_{i,t-1}$ denotes the average accuracy of analyst i's forecasts issued in the previous year across all of the firms she followed. A larger value of $Accuracy_{i,t-1}$ indicates that the analyst was more accurate in the previous year. $Star_{i,t}$ indicates whether analyst *i* is a star analyst in year *t*. We define an analyst as a star in year *t* if she is ranked among the top four according to the *II Poll* in that year.

The regression coefficient on $Merger \times Affected$ is significantly negative in all four regressions. This suggests that merger-affected analysts on average follow fewer firms after a broker merger than other peer analysts. This is consistent with H1. The regression coefficients on other control variables show that star analysts follow more firms than non-star analysts. However, analysts who issued more accurate forecasts in the previous year cover fewer firms in the following year. This suggests that star analysts prefer to maintain their good track record. Those who were more accurate in the past have a greater incentive to remain accurate.

Next, we examine whether individual analysts on average switch their coverage from firms with high R&D expenses to those with low R&D expenses. For this purpose, we run the following analyst-firm level regression:

$$R\&D_{i,k,t-1}\ \left(R\&D_{i,k,t-3y}\right) = \alpha + \beta_1 Merger + \beta_2 Affected + \beta_3 Merger \times Affected \\ + \beta_4 Controls_{i,t-1} + \varepsilon_{i,k,t}. \qquad (2)$$

Specifically, this regression is based on all analysts and the firms they cover during the pre- or post-merger period. The dependent variable, $R\&D_{i,k,t-1}$, equals 1 if firm *k* covered by analyst *i* had positive R&D expenditures in the previous year and 0 otherwise. $Merger$ and $Affected$ are defined as before. The analyst effort hypothesis stated in H1 predicts that $\beta_3 < 0$.

To verify the robustness of our results, we conduct a similar test based on a different definition of the dependent variable, $R\&D_{i,k,t-3y}$. $R\&D_{i,k,t-3y}$ indicates whether the median value of the R&D expenditures of firm *k* in the previous three years is positive. An analyst may change her coverage after the broker merger event. For each analyst, there are three types of firms: (i) those she covers only before the event, (ii) those she covers only after the event, and (iii) those she covers both before and after the event. We exclude the last type of firm for each analyst. If we kept these firms in our sample, the empirical results might only reflect firms' incentives to change their R&D activities over time, rather than analysts' tendency to change their coverage.

Columns (1) and (2) of Table 4 show the results based on $R\&D_{i,k,t-1}$, whereas Columns (3) and (4) show the results based on $R\&D_{i,k,t-3y}$. The results are quite robust. The coefficient on $Merger \times Affected$ is significantly negative in all four regressions. This implies that merger-affected analysts switch to firms with less R&D expenditures after broker mergers, so that they can exert less effort. Given that U.S. firms generally increase their R&D expenditures over time, our results are quite robust. In general, our results are consistent with H1, which suggests that analysts exert less effort after broker mergers that reduce competition among them.

4.3. Test results for the herding hypothesis

To directly test the herding hypothesis, we examine the effect of broker mergers on analysts' weighting of public versus private information, using the following linear regression method proposed by Chen and Jiang (2006):

$$FE_{,it} = \alpha + \gamma Dev_{i,t} + \varepsilon_{i,t}, \tag{3}$$

where $FE_{i,t}$ is analyst *i*'s forecast error for year *t*, defined as forecast EPS minus realized EPS, scaled by the stock price at the previous fiscal year-end; and $Dev_{i,t}$ is forecast deviation from consensus, defined as forecast EPS minus previous forecast consensus, scaled by the stock price at the previous fiscal year-end. Chen and Jiang (2006) show that a positive (negative) γ suggests that analysts place more (less) weight on their private information than is considered efficient.

Figure 1 illustrates the intuition behind Chen and Jiang's (2006) argument. The market has a consensus of the EPS forecast. Assume that an analyst receives a positive private signal relative to the market consensus (as shown in Panel A) and she plans to revise the forecast accordingly. Clearly, the positive private signal would lead to a positive deviation from the previous market consensus. Furthermore, if the analyst places too much weight on her private information, the new forecast would be too high relative to the Bayesian efficient forecast, leading to a positive expected forecast error. Therefore, regressing forecast error onto forecast deviation from consensus would result in a positive coefficient on the forecast deviation from consensus (i.e., a positive γ). Similarly, if the analyst has received a negative private signal and overweighs the private information (as shown in Panel B of Figure 1), we would observe a negative forecast deviation from consensus and a negative expected forecast error. Therefore, γ should also be positive.

Following Chen and Jiang (2006), we extend the baseline Eq. (3) to examine the effect of broker mergers on analysts' weighting of public versus private information. In particular, we run the following regression:

$$FE_{i,t} = \alpha + \gamma Dev_{i,t} + \gamma_1 Merger \times Dev_{it} + \gamma_2 Affected \times Dev_{i,t} + \gamma_3 Merger \times Affected + \gamma_4 Merger \times Affected \times Dev_{i,t} + \gamma_5 Controls_{k,t} + \varepsilon_{i,t}, \quad (4)$$

where all variables are as defined previously. If reduced competition among analysts after a broker merger reduces their incentive to engage in herding behavior, such that they rely more on their private information when issuing forecasts, we should observe a significantly positive γ_4. However, a significantly negative or insignificant γ_4 would suggest that merger-driven reduction in analyst competition does not reduce analysts' tendency to herd.

Before we implement this empirical strategy, a few issues must be clarified. First, we need an appropriate measure of earnings forecast consensus to calculate the forecast deviation ($Dev_{i,t}$). Following the literature, we define forecast consensus as the weighted average of all valid forecasts. Specifically, for each EPS forecast, its corresponding consensus is the weighted average of forecasts for the same fiscal year issued at least one day before and no earlier than six months before the current forecast. Only the last forecast from each analyst within this six-month period is included in the consensus. Furthermore, three weighting schemes are used for the consensus calculations. The first is an inverse-weighting scheme that assigns higher weights to more recent forecasts. In particular, if there are N forecasts and forecast f_n is issued d_n days before the current forecast date, with $d_N > d_{N-1} > \cdots > d_1$, this weighting scheme assigns weight $w_n = \frac{1/d_n}{\sum_{k=1}^{N} 1/d_k}$ to forecast f_n. The second is a linear order weighting scheme, which assigns weight $w_n = \frac{N-n+1}{\sum_{k=1}^{N}(N-k+1)}$ to forecast f_n. The third is an equal weighting scheme, which assigns weight $w_n = \frac{1}{N}$ to forecast f_n.

Second, as we want to examine the changes in the information weighting schemes of analysts around broker mergers, it is necessary to specify the event windows. For consistency with the previous tests, we only examine forecasts issued five to seven months before and five to seven months after the corresponding broker mergers. On average, the time lapse between the pre- and post-merger forecasts is 12 months, which is a natural choice considering the frequency of firm annual reports. Moreover, for each firm, only the forecasts with the most common forecast period end date in the three-month pre-merger (post-merger) period are retained. If an analyst issues multiple forecasts during the three-month pre-merger (post-merger) period, only the latest forecast is retained. This screening procedure ensures that the sample is not biased in favor of analysts who issue forecasts very frequently, and that there is only one target EPS to be forecasted.

Third, instead of running full sample regressions, we split the sample into forecasts with positive deviation from consensus and forecasts with negative deviation from consensus. Chen and Jiang (2006) provide evidence that how analysts weight public and private information when issuing forecasts that are favorable relative to the consensus differs substantially from how they weight information when issuing forecasts that are unfavorable relative to the consensus. Examining the two subsamples separately can help distinguish the herding effect from other incentive effects caused by the merger-driven reduction in analyst competition. The herding hypothesis stated in H2 predicts that $\gamma_4 > 0$ for both subsamples.

Fourth, the optimism bias mechanism described by Hong and Kacperczyk (2010) may have implications for this test. Analysts have stronger incentives to please the managers of merger-affected firms after broker mergers. They therefore place more weight on more favorable information for merger-affected firms. To control for this possibility, we use the consensus mean bias with respect to the last forecast period, $Consensus\ bias_{t-1}$, to measure the overall tendency of analysts to issue a biased forecast for a particular firm. We include $Consensus\ bias_{t-1}$ and $Dev \times Consensus\ bias_{t-1}$ in Eq. (4) as controls.

Table 5 reports the results. We focus on the first two columns, where the forecast consensus is based on the inverse weighting scheme. The results are quantitatively similar under the other two weighting schemes. The first column reports the estimated coefficients of Eq. (4) for a sample of forecasts that deviate positively from consensus. The coefficient on the triple interaction term $Merger \times Affected \times Dev$ is positive and significant. This implies that if the private information is favorable relative to the consensus, analysts place more weight on their private information in their post-merger forecasts than in their pre-merger forecasts for firms affected by the merger. This seems to be consistent with the herding hypothesis.

However, when we run Eq. (4) again for the sample of forecasts that deviate negatively from consensus, the results in the second column contradict the herding hypothesis. The coefficient on the triple interaction term $Merger \times Affected \times Dev$ is negative (−0.66) and highly significant

(t-statistic = −4.76). This indicates that if the private information is unfavorable relative to the consensus, analysts place less weight on their private information in their post-merger forecasts than in their pre-merger forecasts for firms affected by the merger. This goes against the herding hypothesis, which predicts that analysts always place more weight on their private information in post-merger forecasts, regardless of whether the private information is favorable or unfavorable relative to the consensus. Overall, the results in Table 5 appear to be inconsistent with the herding hypothesis.

The coefficients on the other control variables are also interesting. First, we find that the coefficient on $Consensus\ bias_{t-1}$ is always positive and significant. This means that when analysts following the same firm in general have a stronger tendency to issue optimistically biased forecasts in the previous year, they are likely to issue biased forecasts in the current period. This result suggests that consensus mean bias in the previous period to some extent captures the overall proclivity of analysts to optimistically bias their earnings forecasts. Second, the coefficient on $Dev \times Consensus\ bias_{t-1}$ is in general insignificant when the private signal is favorable and significantly positive when it is unfavorable. Although this term does not capture the difference between the treatment and control groups or the difference between the pre- and post-event periods, it generally measures how analysts weigh their private information when their incentive to issue biased forecasts varies. This result shows that simply because analysts have an incentive to issue biased forecasts does not mean they place more weight on favorable information in general. Thus, the results in Table 5 are unlikely to be driven by the mechanism described by Hong and Kacperczyk (2010).[13]

[13] To verify the robustness of this test result, we re-estimate Eq. (4) by including a quadruple interaction term, $Merger \times Affected \times Dev \times Consensus\ bias_{t-1}$. If the optimism bias incentive described by Hong and Kacperczyk (2010) drives our results, we would expect this quadruple interaction term to have a significantly positive coefficient when the private signal is favorable and a negative coefficient when it is unfavorable. However,

4.4. Changes in forecast dispersion around broker mergers: Testing the analyst effort hypothesis versus the strategic deviation hypothesis

In this section, we examine the changes in forecast dispersion for merger-affected firms around broker mergers by running the following firm-level difference-in-differences regression:

$$DISP_{k,t} = \alpha + \beta_1 Merger + \beta_2 Affected + \beta_3 Merger \times Affected$$
$$+\beta_4 Controls_{k,t-1} + \varepsilon_{k,t}. \quad (5)$$

As discussed in the last section, this test helps distinguish between the analyst effort and the strategic deviation hypotheses. $DISP_{k,t}$ in Eq. (5) is the dispersion of earnings forecasts issued by the analysts covering firm *k* during either the pre- or post-merger period. The coefficient β_3 captures the effect of broker mergers on the dispersion of earnings forecasts for firms affected by the mergers, relative to that of earnings forecasts for firms that are not affected. Based on H3, the analyst effort hypothesis predicts that $\beta_3 > 0$, whereas the strategic deviation hypothesis predicts that $\beta_3 < 0$.

We also include the following control variables: (i) standard deviation of stock returns (*Return stdev*), (ii) mean monthly stock returns (*Mean return*), (iii) return on assets (*ROA*), (iv) standard deviation of ROA (*ROA stdev*), (v) natural logarithm of book-to-market equity (*Ln*(*Book-to-market*)), (vi) natural logarithm of market capitalization (*Ln*(*Market cap*)), (vii) firm age in Compustat (*Firm age*), (viii) natural logarithm of one plus *Coverage*, which is the number of analysts following the firm (*Ln*(1 + *Coverage*)), and (ix) the consensus mean bias (*Consensus bias*). We include the consensus mean bias as a control variable to address the concern that the change in analyst forecast dispersion after broker mergers might be caused by the change in the bias of analyst consensus forecast documented by Hong and Kacperczyk (2010).

the coefficient on this quadruple term is insignificant in all cases. This result indicates that although analysts have a stronger incentive to optimistically bias earnings forecasts, they are not driven to put more weight on more favorable information for treatment firms after merger events.

The results in Table 6 show that the coefficients on $Merger \times Affected$ are all positive and significant at the 10% level or better. To appreciate the economic significance of these results, consider the full regression specification in Column (6). The coefficient β_3 is 0.011 (*t*-statistic = 1.81). From Table 2, the mean pre-merger *DISP* is 0.0816, with a standard deviation of 0.1181. Therefore, the impact of broker mergers on *DISP* for affected firms is approximately 13.48% (= 0.011/0.0816) and 9.31% (=0.011/0.1181) of the mean and the standard deviation of pre-merger *DISP*, respectively. This is economically significant considering that broker mergers on average lead to a reduction in the analyst coverage of merger-affected firms by only one analyst.

Column (6) of Table 6 shows that the coefficient on *Consensus bias* is positive and highly significant, indicating that analyst forecasts for firms with upward-biased consensus forecasts tend to be more dispersed than for those with downward-biased consensus forecasts. More importantly, even after we account for this effect, there is still a significant increase in the forecast dispersion for merger-affected firms after broker mergers, relative to firms that are not affected.

In summary, the results in Table 6 are more consistent with the analyst effort hypothesis than with the strategic deviation hypothesis stated in H3. Indeed, a reduction in analyst competition causes analysts to issue more dispersed earnings forecasts. Moreover, as optimistically biased analysts tend to dominate pessimistically biased analysts, the results in Columns (1) to (5) of Table 6 are also consistent with the mechanism suggested by Hong and Kacperczyk (2010).

4.5. An alternative explanation: the learning channel

An analyst may learn from the private information of other analysts who cover the same firm. The more analysts who cover a firm, the more sources of information an individual analyst can learn from. An increase in the number of analysts leads to improved forecast accuracy through this learning channel, as by learning from more peers all analysts will have a more accurate idea of the true value of earnings. The private information of analysts would eventually be incorporated into the public signal. If an analyst really learned from other analysts' private information, she would behave as if she put more weight on the public signal. However, our analyst effort channel does not predict this pattern. In Table 5, we test whether analysts put more weight on the public signal or their private signals using Chen and Jiang's (2006) method. The results show that analysts do put more weight on their private information, but only when it is more favorable than the consensus forecast. They put less weight on their private information when it is more unfavorable than the consensus forecast. In general, the results are not consistent with the pure learning channel.

6. Conclusions

How competition affects the incentives of analysts is an important issue, as it ultimately determines the quality of information that analysts provide to the public. We test three hypotheses regarding the effects of analyst competition. Analyst competition may benefit the information production process by motivating analysts to exert greater effort in evaluating firms via the analyst effort channel, leading to a better information environment with more precise earnings forecasts. However, competition may also induce analysts to engage in herding behavior or strategically deviate from others, which prevents information from being fully incorporated into analyst earnings forecasts.

Using broker mergers as a natural experiment, we conduct difference-in-differences regressions to examine changes in the variety of analyst forecasting activities around broker mergers. These tests help distinguish between the three hypotheses. Specifically, we find that an exogenous reduction in analyst competition due to broker mergers causes the coverage of individual analysts to decrease after the broker merger and a switch in their coverage from firms with higher R&D expenses to those with lower R&D expenses. This result is consistent with the analyst effort hypothesis, but it cannot rule out the herding and strategic deviation hypotheses.

To directly test the herding explanation, we examine the effect of broker mergers on analysts' weighting of public versus private information. The results indicate that although analysts place more weight on their private information after mergers when private information is favorable relative to public information forecasts, the opposite is true when private information is unfavorable relative to public information. The latter result is inconsistent with the herding explanation.

To distinguish between the analyst effort hypothesis and the strategic deviation hypothesis, we examine the changes in the forecast dispersion for merger-affected firms around broker mergers. The results indicate that after broker mergers, as competition decreases among analysts covering merger-affected firms, the earnings forecasts of these analysts also become more dispersed. This result is consistent with the analyst effort hypothesis, but fails to support the strategic deviation hypothesis.

Appendix A: Variable Definitions

Variable	Definition and data source
Shareholders' equity	Compustat shareholders' equity (SEQ) if not missing. If missing, use total common equity (CEQ) plus preferred stock par value (PSTK) when both are present. Otherwise, use total assets (AT) minus total liabilities (LT) when both are present.
Preferred stock value	Compustat preferred stock redemption value (PSTKRV) if available, or liquidating value (PSTKL) if available, or carrying value (PSTK).
Book equity (*Book equity*)	Shareholders' equity – preferred stock value + balance sheet deferred taxes (TXDITC) – FASB 106 adjustment (PRBA). If either shareholders' equity or preferred stock value is missing, treat book equity as missing.
Shares outstanding	Use Compustat quarterly data if available; otherwise use Compustat annual data.
Market capitalization (*Market cap*)	Shares outstanding × stock price (from CRSP).
Book-to-market equity (*Book-to-market*)	The ratio of book equity for the last fiscal year-end in calendar year *t* divided by market equity in December of calendar year *t*.
Firm age (*Firm age*)	The number of years since the firm first appeared in Compustat.
Return on assets (*ROA*)	Operating income before depreciation (OIBDP) divided by total assets (AT).
Standard deviation of ROA (*ROA stdev*)	Standard deviation of ROA for the past five years, requiring that there are at least three years' ROA available.
Mean monthly stock return (*Mean return*)	Average monthly return for the year, requiring that there are at least six months' returns available.
Return standard deviation (*Return stdev*)	Standard deviation of monthly returns for the year, requiring that there are at least six months' returns available.
Analyst coverage (*Coverage*)	The number of analysts that issue EPS forecasts with respect to a particular firm.
EPS forecast dispersion (*DISP*)	The standard deviation of analyst EPS forecasts for the next year divided by the absolute value of mean EPS forecast for the next year. The standard deviation and mean are both from the I/B/E/S Summary History – Unadjusted Summary Statistics file.
Bias of consensus mean EPS forecast (*Consensus bias*)	The consensus mean EPS forecast minus the realized EPS, scaled by the stock price in the previous fiscal year-end. All the relevant variables are split-adjusted to the same basis using CRSP shares and prices adjustment factors. The consensus mean EPS forecast is from the I/B/E/S Summary History – Unadjusted Summary Statistics file. The realized EPS is from Compustat. The stock price is from CRSP.
Forecast error (*FE*)	Forecast EPS minus realized EPS scaled by the stock price at the end of previous fiscal year.
Average accuracy of an analyst (*Accuracy*)	For a particular firm in a typical year, we sort analysts' latest issued forecasts regarding this firm in the previous year according to their forecast accuracy. An analyst gets a score 1 if her forecast in the previous year is among top 20 percent in term of accuracy, and 0 otherwise. Then for each analyst at each year, we take the average of her scores across all firms she covered in the previous year to obtain *Accuracy*.
Star analyst status (*Star*)	This dummy variable equals 1 if an analyst is ranked among top 4 by Institutional Investor Poll during the year when she issues forecasts, and 0 otherwise.

Individual analyst coverage (*NFirms*)	The number of firms covered by an individual analyst who makes at least one forecast in either the pre-merger period or the post-merger window. If an individual analyst issues forecasts in the pre-merger (post-merger) period, Firms is the number of firms she covers in the year of the 6^{th} month before (after) the broker mergers.

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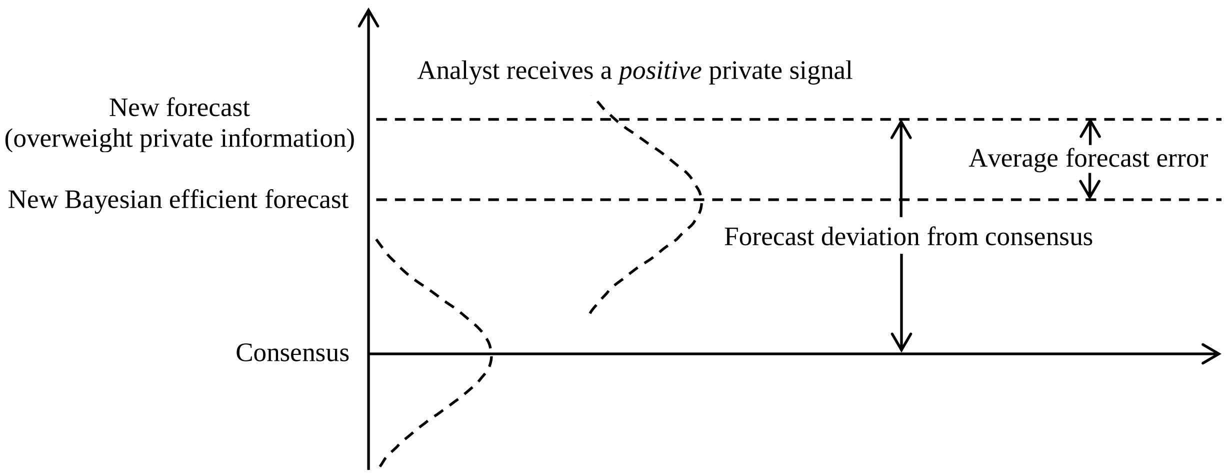
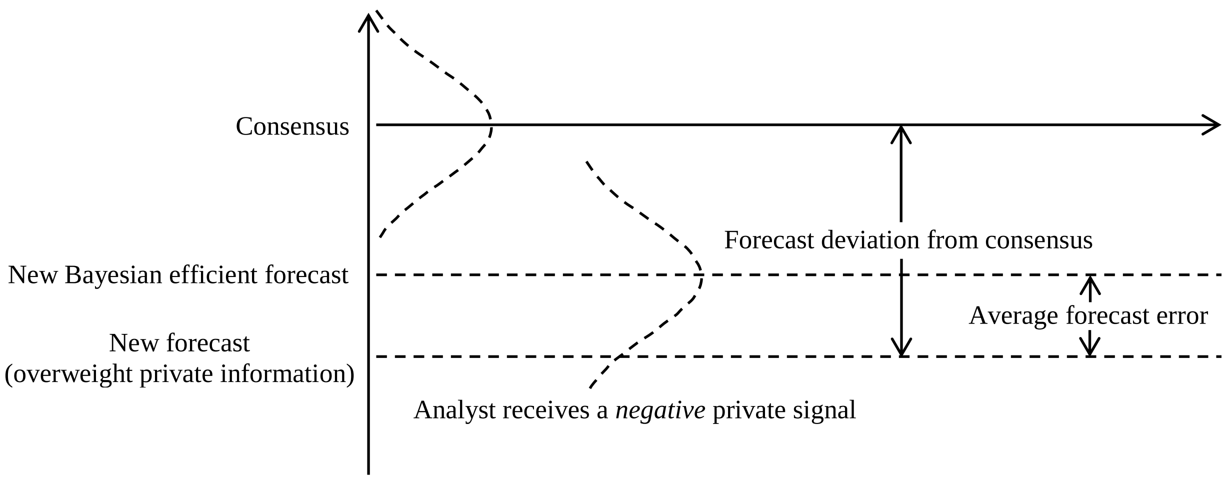


Panel A: The case when analyst receives a *positive* private signal



Panel B: The case when analyst receives a *negative* private signal

Figure 1. The relation between forecast error and forecast deviation from consensus when analysts overweight the private information.

Table 1: Merger events and changes in analyst coverage

This table reports the merger events and the brokerage houses involved. Also reported are the number of stocks affected by each merger event group (stocks simultaneously covered by the acquiring and target brokers from 14 to 3 months before the merger effective month), the average change in analyst coverage around the merger for the stocks, and the average change in net-of-control analyst coverage, where the control benchmark portfolios are matched to the affected stocks based on pre-merger market capitalization, log book-to-market equity, annual stock return, and analyst coverage, following Hong and Kacperczyk (2010). The last row reports the average changes in analyst coverage and in net-of-control analyst coverage across the full sample of affected firms, with *t*-statistics in parentheses. The standard errors are clustered by merger group.

Merger group id	Merger date	Target house	Bidder house	Number of affected stocks	Changes in coverage	Changes in coverage (net of control)
1	9/10/1984	Becker Paribas	Merrill Lynch & Co Inc.	219	-1.77	-0.80
2	2/1/1985	Donaldson Lufkin & Jenrette	Equitable Life Assurance	98	-2.49	-1.12
3	9/6/1985	Josephson Intl – Herzfeld & Stern	Gruntal Financial Corp.	8	-5.00	-2.15
4	10/18/1988 10/31/1988	Johnson, Lane, Space, Smith & Co. Butcher & Co Inc.	Interstate Securities Inc. Wheat First Securities Inc. (WF)	64	-2.03	-1.16
5	12/31/1989	Thomson McKinnon – Ret Brokerage	Prudential – Bache Securities	193	-3.13	-0.95
6	9/5/1991	Hopper Soliday Corp.	Fahnestock Viner Holdings Inc.	30	-1.70	0.42
7	4/28/1994	Foley Mufson Howe – Certain Asst	Janney Montgomery Scott Inc.	5	-2.40	-1.72
8	12/16/1994	Kidder Peabody & Co.	PaineWebber Group Inc.	294	-2.16	-1.06
9	5/31/1997	Morgan Stanley Group Inc.	Dean Witter Discover & Co.	302	-1.23	-0.24
10	9/2/1997 9/2/1997	Alex Brown Inc. Dillon Read & Co (UBS AG)	Bankers Trust New York Corp. SBC Warburg (Swiss Bank Corp.)	36	-1.25	-0.13
11	11/28/1997	Salomon Inc.	Travelers Group Inc.	480	-1.79	-0.87
12	1/9/1998	Principal Financial Securities	EVEREN Capital Corp.	22	-3.91	-2.50
13	2/17/1998	Jensen Securities Co.	DA Davidson & Co.	17	-2.12	-1.00
14	4/6/1998	Wessels Arnold & Henderson LLC	Dain Rauscher Corp.	57	-0.93	0.02
15	2/19/1999	Fifth Third – Brkg Offices (6)	Fahnestock & Co.	8	-2.38	-1.87
16	10/1/1999	EVEREN Capital Corp.	First Union Corp, Charlotte, NC	34	-4.56	-2.83
17	4/28/2000	Black & Co Inc.	First Security Van Kasper & Co.	5	0.40	1.60
18	6/12/2000	JC Bradford & Co.	PaineWebber Group Inc.	29	-2.34	0.59
19	10/16/2000	E*Offering (E*Trade Group Inc.)	Wit Soundview Group Inc.	5	-4.80	-6.33
20	11/3/2000 11/3/2000	Donaldson Lufkin & Jenrette PaineWebber Group Inc.	CSFB UBS AG	624	`-3.29	-1.73
21	12/31/2000	JP Morgan & Co Inc.	Chase Manhattan Corp., NY	83	-3.69	-2.17

22	1/5/2001	Wasserstein Perella Group Inc.	Dresdner Bank AG	17	-4.59	-1.82
23	7/27/2001	Robinson – Humphrey Co.	SunTrust Banks Inc., Atlanta, GA	22	0.00	1.29
24	9/18/2001	Josephthal Lyon & Ross	Fahnestock & Co.	9	-1.22	1.79
25	10/29/2004	Schwab Soundview Capital	UBS AG	123	-2.19	-2.22
26	3/22/2005	Parker/Hunter Inc.	Janney Montgomery Scott LLC	11	-2.91	-1.95
27	12/2/2005	Advest Group Inc. (Mony Group)	Merrill Lynch & Co Inc.	42	-2.67	-1.02
28	12/8/2006	Petrie Parkman & Co Inc.	Merrill Lynch & Co Inc.	35	-2.57	-1.84
All				2,872	-2.36	-1.11
					(-8.54)	(-5.89)

Table 2: Summary statistics of the sample of merger-affected firms

This table reports the summary statistics of the sample of analysts and firms affected by broker mergers. The sample includes 28 merger groups (31 merger events) affecting 2,872 firms for which we have data on market capitalization, book-to-market equity, and annual stock returns before the merger. Affected firms are firms that are simultaneously covered by the target and acquiring brokers from 14 to 3 months before the merger effective month. For each merger group, the mean, standard deviation, and median of each variable are calculated based on all observations of affected analysts or firms associated with that merger group. The numbers reported are the averages across merger groups. For an affected firm, the pre-merger (post-merger) dispersion (*Dispersion*) is the median of the forecast dispersion values corresponding to I/B/E/S unadjusted summary records 7 months to 5 months before (5 months to 7 months after) the merger effective month. Only the forecasts with the most common forecast period end date in the 3-month pre-merger (post-merger) period are used in the calculation of *AECF*. If an analyst issues multiple forecasts during the 3-month pre-merger (post-merger) period, only the latest forecast is used. Number of firms is the number of firms covered by individual analysts. *Return stdev*, *Mean return*, *ROA stdev*, *ROA*, *Ln*(*Book-to-market*), *Ln*(*Market cap*), and *Firm age* are all pre-merger firm characteristics. If the merger effective month is January or February of year t, these firm characteristics correspond to the year $t-2$ values; if the merger effective month is any other month of year t, these firm characteristics correspond to the year $t-1$ values. *ROA stdev* and *ROA* are annually trimmed at the 1.5th percentile at both tails before they are linked to the affected firms.

Variables	# of obs.	Mean	Std dev	Median
Analyst level:				
Number of firms: NFirms (pre-merger)	436	17.63	10.29	15.55
Number of firms: NFirms (post-merger)	428	17.53	9.80	15.25
Firm level:				
Dispersion: DISP (%, pre-merger)	102	8.16	11.81	4.28
Dispersion: DISP (%, post-merger)	96	12.11	19.45	5.92
Return stdev (%)	103	12.11	5.27	10.97
Mean return (%)	103	2.83	3.83	2.49
ROA stdev (%)	99	4.74	4.42	3.42
ROA (%)	100	13.59	9.01	14.22
Ln(Book-to-market)	103	-1.04	0.82	-0.95
Ln(Market cap)	103	7.99	1.55	7.92
Firm age	103	22.94	12.95	21.73

Table 3: Regression results of changes in the number of firms covered by individual analysts around broker mergers: Analyst-level analysis

This table reports the results of the following analyst-level regression:

$$NFirms_{i,t}\ (Ln(1 + NFirms_{i,t})) = \alpha + \beta_1 Merger + \beta_2 Affected + \beta_3 Merger \times Affected$$
$$+\beta_4 Controls_{i,t} + \varepsilon_{i,t}.$$

The construction of the regression sample follows Table 2. The dependent variable ($NFirms_{i,t}$) is the number of firms covered by individual analysts. $Accuracy_{i,t-1}$ in Columns (2) and (4) denotes the average accuracy of analyst i's forecasts issued in the previous year across all firms she covered. A larger value of $Accuracy_{i,t-1}$ indicates that analyst *i* was more accurate in the previous year. $Star_{i,t}$ is the star analyst status of analyst *i*. An analyst is defined as a star in a particular year if she is ranked among top 4 according to the Institutional Investor Poll in that year. $Merger$ equals 1 for the post-merger observations and $Affected$ equals 1 if an analyst covers any treatment firm affected by the corresponding broker merger. For the pre-merger (post-merger) observations of the firm, the month that is 6 months before (after) the merger effective month is used as the reference month. The table reports the regression coefficients with the corresponding *t*-statistics in parentheses. The standard errors are clustered by analyst and year. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

	$NFirms_{i,t}$		$Ln(1 + NFirms_{i,t})$	
	(1)	(2)	(3)	(4)
Merger	0.051	-0.12	0.007	-0.009
	(0.48)	(-0.96)	(0.90)	(-0.95)
Affected	1.480***	1.252***	0.121***	0.097***
	(9.25)	(9.31)	(10.25)	(10.31)
Merger × Affected	**-0.253***	**-0.231***	**-0.021****	**-0.018****
	(-1.72)	**(-1.83)**	**(-1.98)**	**(-2.00)**
$Accuracy_{i,t-1}$		-0.446***		-0.040***
		(-7.38)		(-7.68)
$Star_{i,t}$		2.223***		0.162***
		(10.70)		(10.67)
Year fixed effects	Yes	Yes	Yes	Yes
Analyst fixed effects	Yes	Yes	Yes	Yes
R-squared	0.683	0.695	0.702	0.718
# of obs.	131,128	114,186	131,128	114,186

Table 4: Regression results of changes in R&D intensity of firms covered by individual analysts around broker mergers: Analyst-firm level analysis

This table reports the results of the following analyst-level regression:

$$R\&D_{i,k,t-1}\ (R\&D_{i,k,t-3y}) = \alpha + \beta_1 Merger + \beta_2 Affected + \beta_3 Merger \times Affected + \beta_4 Controls_{i,t} + \varepsilon_{i,k,t}.$$

The construction of the regression sample and the definition of variables follow Table 2. The dependent variable in Columns (1) and (2) is $R\&D_{i,k,t-1}$, which equals 1 if firm *k* covered by analyst *i* in year *t* has positive R&D expenses in year *t*-1, and 0 otherwise. The dependent variable in (3) and (4) is $R\&D_{i,k,t-3y}$, which equals 1 if firm *k* covered by analyst *i* in year *t* has a positive median value of R&D expenses over years *t*-3, *t*-2, and *t*-1, and 0 otherwise. $Accuracy_{i,t-1}$ in Columns (2) and (4) denotes the average accuracy of analyst *i*'s forecasts issued in the previous year across all firms she covered. $Star_{i,t}$ is the star analyst status of analyst *i*. $Merger$ equals 1 for the post-merger observations and $Affected$ equals 1 if an analyst covers any treatment firm affected by the corresponding broker merger. The table reports the regression coefficients with the corresponding *t*-statistics in parentheses. The standard errors are clustered by analyst and year. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively

	$R\&D_{i,k,t-1}$		$R\&D_{i,k,t-3y}$	
	(1)	(2)	(3)	(4)
Merger	0.0032***	0.0031***	0.0033**	0.0030**
	(4.05)	(3.64)	(2.37)	(2.07)
Affected	0.0003	0.0004	0.0009	0.001
	(0.53)	(0.69)	(1.18)	(1.27)
Merger× Affected	**-0.0013***	**-0.0015****	**-0.0017***	**-0.0017***
	(-1.86)	**(-1.97)**	**(-1.71)**	**(-1.66)**
$Accuracy_{i,t-1}$		-0.0001*		-0.0001
		(-1.91)		(-1.53)
$Star_{i,t}$		0.0001		0.0001
		(0.15)		(0.19)
Year fixed effects	Yes	Yes	Yes	Yes
Analyst fixed effects	Yes	Yes	Yes	Yes
R-squared	0.486	0.491	0.505	0.5100
# of obs.	288,854	266,954	278,382	257,212

Table 5: Regressions of forecast errors on forecast deviations from forecast consensus: Testing the herding hypothesis

This table reports the regression results of analyst forecast errors (FE) on the forecast deviations from consensus (Dev) as follows:

$$FE_{i,t} = \alpha + \gamma Dev_{it} + \gamma_1 Merger \times Dev_{i,t} + \gamma_2 Affected \times Dev_{i,t} + \gamma_3 Merger \times Affected + \gamma_4 Merger \times Affected \times Dev_{i,t} + \gamma_5 Controls_{t-1} + \varepsilon_{i,t}.$$

Analyst forecast error (FE) is the forecast EPS minus realized EPS from Compustat, scaled by the stock price at the previous fiscal year-end. Forecast deviation from consensus (Dev) is forecast EPS minus previous forecast consensus, scaled by the stock price at the previous fiscal year-end. For each EPS forecast, the corresponding consensus is a weighted average of forecasts for the same fiscal year issued at least 1 day before and no earlier than 6 months before the current forecast. Only the last forecast from each analyst within this 6-month period is included in the consensus. Three weighting schemes are used for the consensus calculations, as described in Section 5.5. To construct the regression sample, for each of the 28 merger groups, we start with all firms for which data on market capitalization, book-to-market equity, and annual stock returns before the corresponding broker merger are available. Each firm is linked to the "pre-merger" ("post-merger") EPS forecasts, which are forecasts issued 7 to 5 months before (5 to 7 months after) the merger effective month. Only the forecasts with the most common forecast period end date in the 3-month pre-merger (post-merger) period are retained. Furthermore, if an analyst issues multiple forecasts during the 3-month pre-merger (post-merger) period, only the latest forecast is retained. $Merger$ equals 1 for the post-merger observations and $Affected$ equals 1 if an analyst covers any treatment firm affected by the corresponding broker merger. $Consensus\ bias_{t-1}$ is the consensus mean EPS forecast minus the realized EPS, scaled by the stock price in the previous fiscal year-end. For each valid forecast, its forecast error (FE) and forecast deviation (Dev) from consensus are calculated, and the forecast is assigned to either the positive deviation sub-sample or the negative deviation sub-sample. All relevant variables are split-adjusted to the same basis. The table reports the regression results for the two sub-samples, with t-statistics in parentheses. The standard errors are clustered by firm and month. Forecasts whose errors are at the 1.5th percentile at both tails are excluded from the sample and variable constructions. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

	Consensus from inverse weighting		Consensus from linear order weighting		Consensus from equal weighting	
	Dev≥0	Dev<0	Dev≥0	Dev<0	Dev≥0	Dev<0
Dev	1.41***	-0.58***	1.40***	-0.61***	1.32***	-0.63***
	(21.35)	(-11.58)	(17.82)	(-12.98)	(14.94)	(-13.74)
Consensus bias$_{t-1}$	0.17***	0.20***	0.16***	0.21***	0.16***	0.21***
	(10.92)	(10.38)	(10.65)	(9.97)	(10.44)	(9.63)
Consensus bias$_{t-1}$ × Dev	-0.75	2.12***	-0.41	2.34***	0.03	2.33***
	(-1.37)	(5.82)	(-0.68)	(6.60)	(0.06)	(6.07)
Merger × Dev	0.25***	-0.15**	0.25***	-0.16***	0.23***	-0.16***
	(3.68)	(-2.57)	(3.22)	(-2.66)	(2.78)	(-2.82)
Affected × Dev	-0.23***	0.18*	-0.28***	0.14	-0.28***	0.11
	(-3.32)	(1.77)	(-4.00)	(1.25)	(-4.20)	(1.05)
Merger × Affected	-0.00	-0.00**	-0.00	-0.00*	-0.00*	-0.00*
	(-1.25)	(-2.03)	(-1.16)	(-1.78)	(-1.73)	(-1.91)
Merger × Affected × Dev	**0.36****	**-0.66*****	**0.27***	**-0.62*****	**0.22**	**-0.62*****
	(2.10)	**(-4.76)**	**(1.87)**	**(-3.49)**	**(1.51)**	**(-3.85)**
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Analyst fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.208	0.156	0.207	0.157	0.205	0.160
# of obs.	376,232	405,171	375,799	405,604	370,598	410,805

Table 6: Regression results of changes in forecast dispersion around broker mergers: Testing for the analyst effort hypothesis versus the strategic deviation hypothesis

This table reports the results of the following regression:

$$DISP_{k,t} = \alpha + \beta_1 Merger + \beta_2 Affected + \beta_3 Merger \times Affected + \beta_4 Controls_{k,t} + \varepsilon_{k,t}.$$

To construct the regression sample, for each of the 28 merger groups, we start with all firms that are covered by I/B/E/S, Compustat, and CRSP, and for which data on market capitalization, book-to-market equity, and annual stock returns before the corresponding broker merger are available. Each firm is then linked to a pre-merger observation and a post-merger observation. The dependent variable, $DISP_{k,t}$, is the median of the forecast dispersion values corresponding to I/B/E/S unadjusted summary records 7 to 5 months before (5 to 7 months after) the merger effective month. $Consensus\ bias_{k,t}$ is the median of the bias values of the consensus mean forecasts corresponding to I/B/E/S unadjusted summary records 7 to 5 months before (5 to 7 months after) the merger effective month. $Merger$ equals 1 for the post-merger observations and $Affected$ equals 1 if an analyst covers any treatment firm affected by the corresponding broker merger. For the pre-merger (post-merger) observation of the firm, the month that is 6 months before (after) the merger effective month is used as the reference month. *ROA stdev* and *ROA* are annually trimmed at the 1.5th percentile at both tails before they are linked to the firms. Forecast dispersion ($DISP$) and consensus forecast bias ($Consensus\ bias$) obtained from the I/B/E/S unadjusted summary dataset are also trimmed within each merger group at the 1.5th percentile at both tails. The *t*-statistics are in parentheses. The standard errors are clustered by firm and year. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

	Forecast dispersion					
	(1)	(2)	(3)	(4)	(5)	(6)
Merger	0.001	-0.001	0	-0.001	-0.001	-0.001
	(0.11)	(-0.22)	(0.02)	(-0.23)	(-0.32)	(-0.30)
Affected	-0.006	-0.008*	-0.007*	-0.004	-0.005	-0.004
	(-1.59)	(-1.70)	(-1.76)	(-0.93)	(-1.24)	(-0.80)
Merger × Affected	**0.014****	**0.013***	**0.014****	**0.015****	**0.013***	**0.011***
	(2.17)	**(1.86)**	**(2.26)**	**(2.04)**	**(1.87)**	**(1.81)**
Ln(1+Coverage$_{k,t-1}$)	-0.011***				0.006*	0.005*
	(-2.73)				(1.86)	(1.88)
Return stdev$_{k,t-1}$		0.363***			0.282***	0.219***
		(6.42)			(6.94)	(6.18)
Mean return$_{k,t-1}$		-0.767***			-0.462***	-0.363***
		(-6.84)			(-4.30)	(-4.21)
ROA stdev$_{k,t-1}$			0.005		0.019	0.03
			(0.12)		(0.46)	(0.79)
ROA$_{k,t-1}$			-0.413***		-0.290***	-0.269***
			(-6.51)		(-5.37)	(-5.15)
Ln(Book-to-market$_{k,t-1}$)				0.031***	0.012***	0.004
				(7.57)	(2.86)	(1.09)
Ln(Market cap$_{k,t-1}$)				-0.034***	-0.024***	-0.022***
				(-8.36)	(-5.11)	(-5.03)
Ln(Firm age$_{k,t-1}$)				0.020***	0.019***	0.015***
				(2.97)	(3.18)	(2.95)
Consensus bias$_{k,t}$						0.344***
						(4.96)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.332	0.354	0.35	0.353	0.370	0.381
# of obs.	168,025	166,984	153,965	167,166	153,160	142,804